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Lagged Relationships in Economic Forecasting

By P. J. COEN, E. D. GOMME and M. G. KENDALL

Scientific Control Systems Ltd, London

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the President, Mr F. A. COCKFIELD, in the Chair]

INTRODUCTION

1. FOR some years econometricians have been aware of the critical dependence of economic systems on the lag between cause and effect. It has been shown that the very stability of a system may depend on the length of time taken for a stimulus to pass through the transient to full response. However, not much is known about the actual lengths of these lags, at least in the U.K. economy, and surprisingly little effort has been devoted to determining them. Such writers as are concerned with dynamic systems usually write in lags of one or two time units (and occasionally up to four in quarterly data to describe seasonal effects) and hope that autoregression will take care of any important lag effects. We felt that work in this area being done, for example, in the U.S.A. (e.g. by the National Bureau of Economic Research in *Business Cycle Indicators*) might not be applicable in the U.K. and decided to approach the British problem *ab initio*.

2. In December 1966 Mr Gomme approached Scicon to undertake, in the first place, a critical appraisal of the lag concept for predicting economic events, because he had already, after two years' study, had some success in developing a basic equation which predicted the July 1966 crisis in real time. He was faced with pronounced scepticism in the City and in the academic world, but held to the conviction that the whole concept should be critically examined. It was decided to proceed with a joint project which would systematically explore the existence of relationships among a wide range of economic variables. We were in a favourable position to do this because of our access to powerful computers and our program for determining optimum regression equations. Before very long some rather remarkable relationships emerged and our work, though not yet terminated, has reached a stage at which we feel that the results are worth reporting. At this stage we offer the results as empirical phenomena requiring explanation. We are not, for the present, concerned with what that explanation might be.

INITIAL EXPLORATION

3. We began by considering whether a Stock Exchange index of securities, namely the *Financial Times*† ordinary share index, could be related to one or more of the following variables: the *F.T.* commodity index, Reuter's commodity index, bank advances, sterling liability, Standard and Poor‡ share index (425 industrials on Wall Street), U.K. car production, value of U.K. cars produced, expenditure on consumer durables, bank rate, 2½ per cent consols yield, government expenditure, Stock Exchange activity, weekly wage rates, unemployment, vacancies outstanding, U.S. car production, production of bricks and electricity consumed. The data for the last 20 years for variables used in the equations here are exhibited in Table 1. All the data displayed here and used in this work are quarterly.

† *Financial Times* = *F.T.* ‡ Standard and Poor = *S.* and *P.*

TABLE 1

Year and quarter	F.T. index	F.T. commodity index	Reuter's commodity index	Total bank advances (£ million)†	2½ per cent Consols yields (%)	Sterling liability (£ million)	S. and P. index	U.K. production of cars seasonally adjusted	Government securities index	U.K. bank rate (%)
1948/1	118.2		368.7				14.18	82,361	113.0	
1948/2	118.4		380.5				15.92	82,499	112.1	
1948/3	113.4		358.2				15.83	84,896	112.4	
1948/4	119.8		404.1				15.33	84,820	113.4	
1949/1	118.9		404.3				14.84	97,131	113.8	
1949/2	110.8		401.4				14.29	93,840	113.1	
1949/3	104.2		408.8				14.93	108,522	107.3	
1949/4	103.4		465.4				15.92	116,137	105.4	
1950/1	105.0		470.2				16.83	131,092	104.9	2.0
1950/2	109.8		487.6				18.18	123,583	105.8	2.0
1950/3	113.4		532.9				18.32	135,620	106.5	2.0
1950/4	116.3		561.6				20.01	135,889	108.1	2.0
1951/1	120.5		609.1		3.64	2,932	21.81	122,481	105.6	2.0
1951/2	133.7		622.3		3.78	3,119	22.14	115,384	102.9	2.0
1951/3	133.5		601.0		3.74	3,255	23.21	120,588	101.9	2.0
1951/4	129.1		592.9		3.98	3,281	23.57	121,576	99.3	2.33
1952/1	113.0		576.0	2,031	4.14	3,299	24.33	111,172	95.0	3.0
1952/2	108.7		545.4	1,966	4.32	3,164	24.19	109,355	93.0	4.0
1952/3	112.7	96.21	544.1	1,849	4.29	3,128	25.36	105,761	93.0	4.0
1952/4	115.0	93.74	517.9	1,871	4.24	3,099	25.34	121,874	94.7	4.0
1953/1	121.4	91.37	508.0	1,850	4.21	3,063	26.23	126,260	95.3	4.0
1953/2	118.4	86.31	498.2	1,830	4.10	3,121	24.66	145,248	97.4	4.0
1953/3	122.7	84.98	491.1	1,786	4.03	3,118	24.04	160,370	98.1	3.9
1953/4	128.8	86.46	480.6	1,792	3.92	3,073	24.44	163,648	100.4	3.5
1954/1	135.7	90.04	485.6	1,831	3.88	3,060	26.13	178,195	100.6	3.5
1954/2	148.5	94.74	490.0	1,855	3.76	3,067	28.87	187,197	102.6	3.25
1954/3	165.4	92.43	484.2	1,922	3.63	3,091	30.33	195,916	105.2	3.0
1954/4	178.5	92.41	492.7	2,013	3.74	3,153	34.62	199,253	105.4	3.0
1955/1	187.3	91.65	503.3	2,090	3.92	3,244	37.50	227,616	100.6	3.67
1955/2	195.9	89.38	490.9	2,149	4.13	3,232	39.79	215,363	96.6	4.5
1955/3	205.2	91.05	495.3	2,200	4.39	3,250	45.46	231,728	91.1	4.5
1955/4	191.5	89.89	485.0	2,034	4.40	3,237	46.83	231,767	90.9	4.5
1956/1	183.0	90.16	483.7	1,964	4.54	3,216	48.20	211,211	87.3	5.0
1956/2	184.5	86.78	484.7	1,948	4.67	3,191	50.13	185,200	86.4	5.5
1956/3	181.1	88.45	474.9	1,985	4.79	3,082	51.45	152,404	84.2	5.5
1956/4	173.7	90.69	493.3	1,979	4.85	3,265	49.41	156,163	83.7	5.5
1957/1	185.4	86.03	489.0	2,007	4.56	3,366	47.13	151,567	88.0	5.17
1957/2	201.8	84.85	472.8	2,040	4.84	3,329	49.82	213,683	85.2	5.00
1957/3	198.0	84.07	449.8	2,038	5.21	3,403	49.86	244,543	81.2	5.33
1957/4	168.0	81.96	426.9	1,976	5.46	3,038	43.71	253,111	78.6	7.00

† The "Total bank advances" are the figures given in Table 151 of the *Monthly Digest of Statistics*. There was an alteration in the basis in the first quarter of 1967. An adjustment of £2,000 million has been made to take account of this change.

TABLE 1—*continued*

<i>Year and quarter</i>	<i>F.T. index</i>	<i>F.T. commodity index</i>	<i>Reuter's commodity index</i>	<i>Total bank advances (£ million)†</i>	<i>2½ per cent Consols yields (%)</i>	<i>Sterling liability (£ million)</i>	<i>S. and P. index</i>	<i>U.K. production of cars seasonally adjusted</i>	<i>Government securities index</i>	<i>U.K. bank rate (%)</i>
1958/1	161.6	80.03	413.5	1,948	5.13	2,924	44.32	266,580	80.7	6.87
1958/2	170.2	79.80	412.4	1,995	4.96	2,825	46.41	253,543	83.2	5.50
1958/3	184.5	80.19	416.6	2,078	4.87	2,812	50.82	261,675	84.1	4.67
1958/4	211.0	80.13	415.9	2,270	4.86	2,811	55.92	249,407	84.6	4.25
1959/1	218.3	80.42	407.5	2,473	4.74	2,788	59.13	246,248	86.4	4.00
1959/2	231.7	82.67	413.9	2,633	4.63	2,972	61.59	293,062	85.7	4.00
1959/3	247.4	82.78	418.9	2,846	4.79	2,990	63.06	285,809	85.6	4.00
1959/4	301.9	82.61	425.8	3,064	4.93	3,156	62.02	366,265	86.3	4.00
1960/1	323.8	82.47	430.2	3,246	5.23	3,238	60.19	374,241	83.5	4.67
1960/2	314.1	81.86	423.7	3,401	5.42	3,255	59.79	375,764	81.6	5.90
1960/3	321.0	79.70	422.3	3,503	5.48	3,243	59.06	354,411	79.6	6.00
1960/4	312.9	77.89	408.7	3,642	5.56	3,199	58.67	249,527	80.0	5.50
1961/1	323.7	77.61	408.7	3,713	5.83	3,248	65.58	206,165	78.6	5.00
1961/2	349.3	78.90	419.6	3,832	6.08	3,272	69.82	258,410	77.7	5.00
1961/3	310.4	79.72	419.6	3,903	6.52	3,316	70.58	279,342	74.1	6.33
1961/4	295.8	78.08	411.6	3,762	6.51	3,241	73.98	264,824	75.5	6.17
1962/1	301.2	77.54	408.5	3,841	6.28	3,128	77.14	312,983	77.9	5.75
1962/2	285.8	76.99	409.3	3,866	6.09	3,089	65.43	300,932	78.1	4.67
1962/3	271.7	76.25	413.5	3,987	5.72	3,039	60.32	323,424	82.2	4.50
1962/4	283.6	78.13	421.8	4,113	5.73	3,038	62.05	312,780	85.4	4.50
1963/1	295.7	80.38	446.6	4,312	5.81	3,040	68.54	363,336	84.0	4.00
1963/2	309.3	81.78	464.6	4,457	5.48	3,104	73.31	378,275	85.7	4.00
1963/3	295.7	82.81	460.4	4,563	5.43	3,175	74.30	414,457	87.8	4.00
1963/4	342.0	84.99	484.3	4,650	5.72	3,268	77.39	459,158	86.3	4.00
1964/1	335.1	86.31	479.9	4,801	5.90	3,650	82.15	460,397	83.8	4.33
1964/2	344.4	85.95	468.9	4,867	6.01	3,503	85.28	462,279	82.7	5.00
1964/3	360.9	90.73	470.5	5,091	6.05	3,622	87.72	434,255	82.1	5.00
1964/4	346.5	92.42	476.0	5,345	6.22	3,813	89.61	475,890	81.1	5.67
1965/1	340.6	87.18	453.1	5,307	6.27	3,895	91.38	439,365	79.0	7.00
1965/2	340.3	85.20	454.9	5,427	6.53	4,077	92.65	431,666	77.5	6.67
1965/3	323.3	85.44	451.3	5,474	6.43	4,235	92.18	399,160	78.0	6.00
1965/4	345.6	87.85	451.7	5,458	6.40	4,139	97.63	449,564	79.7	6.00
1966/1	349.3	89.95	466.1	5,590	6.53	4,351	97.90	437,555	78.4	6.00
1966/2	359.7	90.20	462.4	5,749	6.72	4,502	94.39	426,616	77.0	6.00
1966/3	320.0	88.89	450.1	5,658	7.08	4,580	87.15	399,254	74.0	6.90
1966/4	299.9	83.25	426.7	5,391	6.85		84.87	334,587	76.8	7.00
1967/1	318.5	81.21	424.2	5,315	6.40		93.03	367,997	80.0	6.50
1967/2	343.1	79.70	435.5	5,421	6.51		98.53	393,808	80.5	5.67
1967/3	360.8	78.70	430.0	5,697	6.82		102.15	375,968	78.4	5.50
1967/4	397.8	81.50	465.0	5,921	7.01		103.09	381,692	76.9	7.00

† The "Total bank advances" are the figures given in Table 151 of the *Monthly Digest of Statistics*. There was an alteration in the basis in the first quarter of 1967. An adjustment of £2,000 million has been made to take account of this change.

4. We tried many relations among these variables with different lags; there is no space to report them all and we shall concentrate most of our attention on the forecasting of the Stock Exchange index, which until recently could be regarded as a fair barometer of the country's economy. One feature which became apparent at an early stage was that good regressions were obtainable with only two or three explanatory variables—bringing in more, rarely if ever improved the representation as measured by R^2 . This is not because the economy or the stock market is a simple process which is nearly completely determined by one or two factors, but because there are groups of highly correlated factors of which these are representatives. This raises a practical difficulty when using these equations for forecasting. If variable A , which has not been included in an equation because of its high correlation with variable B which has been included, has a real effect on the dependent variable and ceases to be correlated with B , then the forecasts produced can be misleading. However, some of the regressions on one or two variables were astonishingly good. The best set of equations for the *F.T.* ordinary share index which we had established early in 1967 is shown in Table 2.

TABLE 2
*Coefficients in regressions of F.T. ordinary share index
on the three variables shown*

Equation No.	Period of fit (year and quarter)	Constant	Coefficients			R^2
			(U.K. car production) $_{t-6}$	<i>F.T.</i> (commodity index) $_{t-7}$	(Reuter's commodity index) $_{t-7}$	
(1)	1948/1-1958/1	82.06	0.00063 (10.34)		-0.05 (-1.54)	0.75
(2)	1954/2-1960/4	750.96	0.00039 (2.99)	-7.07 (-5.65)		0.81
(3)	1954/2-1962/1	757.64	0.00041 (4.89)	-7.16 (-5.96)		0.85
(4)	1954/2-1964/1	716.87	0.00042 (5.83)	-6.74 (-7.66)		0.88
(5)	1954/2-1965/1	717.88	0.00044 (7.30)	-6.78 (-8.42)		0.90
(6)	1958/1-1965/4	897.51	0.00035 (7.879)	-8.675 (-8.49)		0.87
(7)	1954/2-1966/4	653.16	0.00047 (14.114)	-6.127 (-9.89)		0.90

Values of t are in brackets under the coefficients.

5. Much of this early work was done by graphing the series on transparencies to a roughly comparable scale and then superposing them, sliding them along the time-axis to see whether there was any fairly obvious coincident variation. This method has dangers, and we refer later to the steps we took to avoid false conclusions. But as a preliminary screening device it has many advantages, especially where a number of series are involved and one wishes to compare them in many different ways,

sometimes superposing several. The method brought to light some very unexpected effects. Fig. 1 shows (to comparable vertical scale) (1) the S. and P. index from 1951 to 1967 and (2) the production of motor cars in the U.K. (seasonally adjusted) displaced 6 quarters to the right (that is, cars lead the stock market index by 6 quarters).

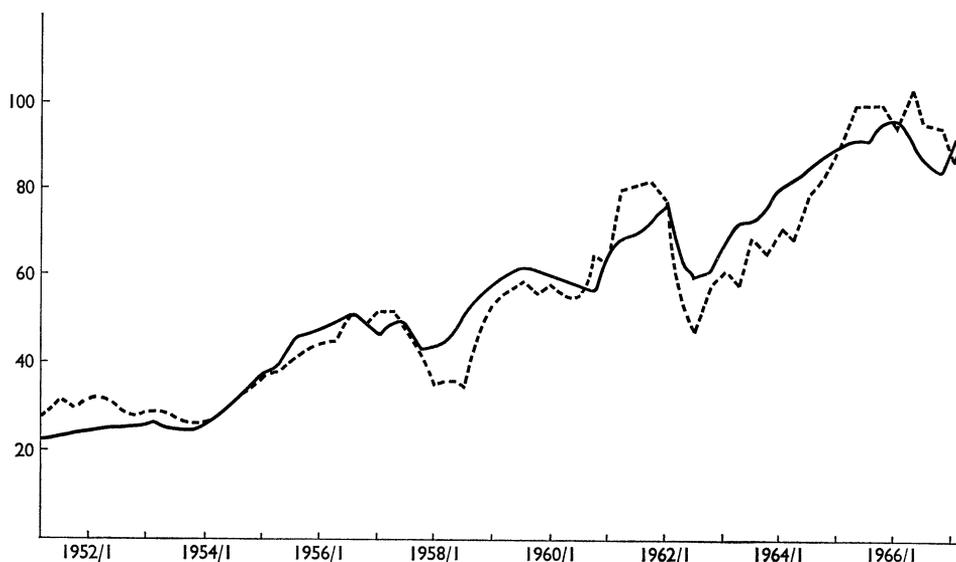


FIG. 1. ———, S. and P. index; - - - - -, U.K. car production (leading by 6 quarters).

It is, we think, difficult to believe that the fidelity with which one series follows the fluctuations in the other is a chance phenomenon. The actual figures are given in Table 1.

6. We also calculated cross-correlation coefficients between the dependent variables (principally *F.T.* share index for London and the S. and P. index for Wall Street) and a number of possible indicator variables at a range of positive lags.

The results for the *F.T.* index and other variables in detrended terms are displayed graphically in Fig. 2. Most of the lagged correlations observed by using transparencies were confirmed and attention was directed to some possible new relationships. The statistical meaningfulness of these relationships is discussed in the Appendix and paragraphs 20–22 below.

The lags at which the maximum values of the cross-correlation coefficient, r_k , occur may be considered to mark the stages of a business fluctuation. Commodity prices tend to be at a peak 7 to 9 quarters before a trough in the *F.T.* index. Sterling liability, which depends, in part, on commodity prices, tends to reach a peak about 7 quarters before the *F.T.* index low point and for bank rate the best correlation is 2 to 4 quarters ahead of the *F.T.* index.

7. We also calculated the cross-correlations of some of the series, notably the *F.T.* index and the S. and P. index, differenced at periods of one year (for instance, the values were 1961/1–1960/1, 1961/2–1960/2, 1961/3–1960/3, 1961/4–1960/4, 1962/1–1961/1, etc.). These are shown in Figs. 3 and 4.

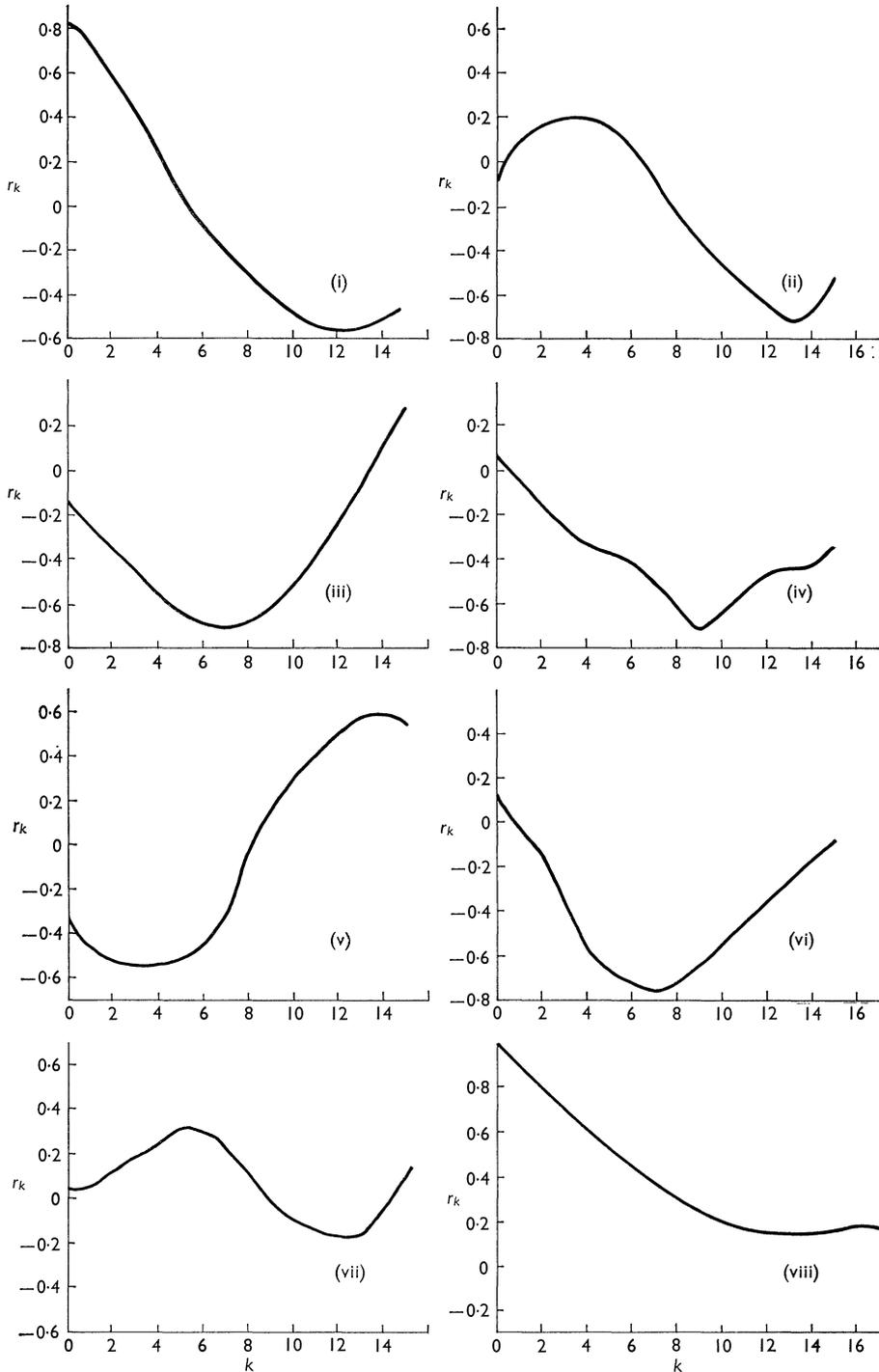


FIG. 2. Values of the cross-correlation coefficients r_k between the *F.T.* share index (detrended) and the following variables (detrended):

- | | |
|-----------------------------------|---------------------------------|
| (i) Bank advances, | (v) Bank rate, |
| (ii) Government securities index, | (vi) Sterling liability, |
| (iii) Reuter's commodity index, | (vii) U.K. car production, |
| (iv) <i>F.T.</i> commodity index, | (viii) <i>F.T.</i> share index, |
- plotted against k , the lag of the *F.T.* index.

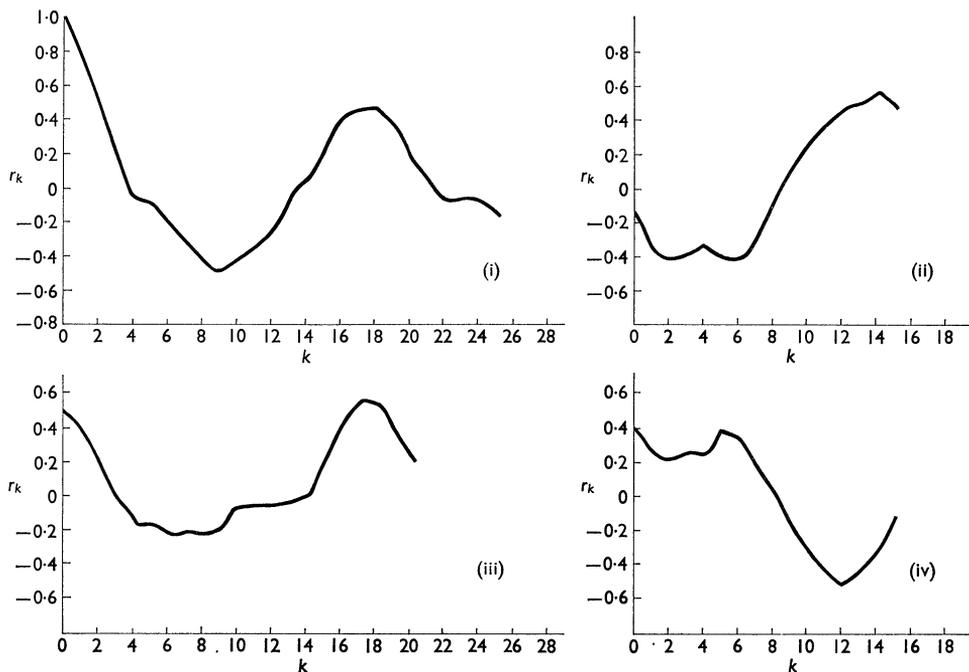


FIG. 3. Values of the cross-correlation coefficients r_k between the *F.T.* share index and (i) *F.T.* share index, (ii) Bank rate, (iii) *S.* and *P.* index, (iv) U.K. car production, plotted against k , the lag of the *F.T.* share index. (All variables are in four-quarter difference terms.)

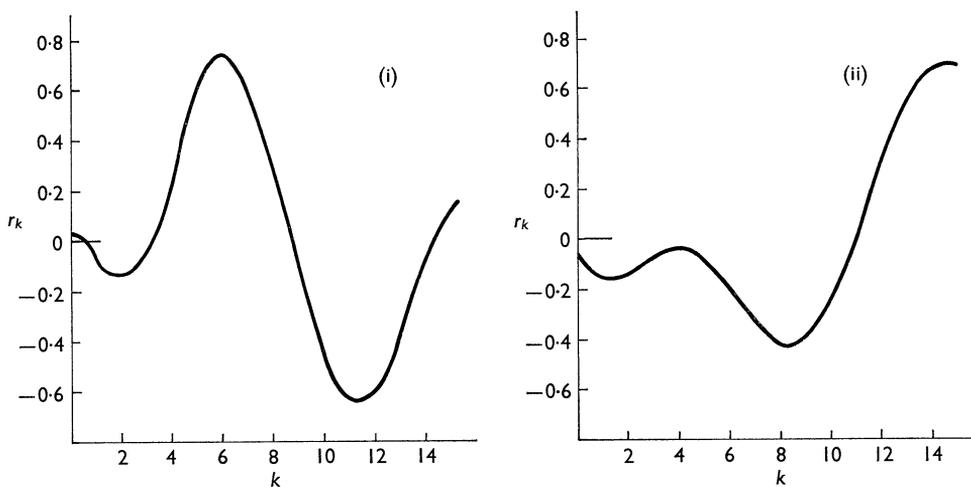


FIG. 4. Values of the cross-correlation coefficients, r_k , between the *S.* and *P.* index and (i) U.K. car production, (ii) U.K. bank rate, plotted against k , the lags of the *S.* and *P.* index. (All variables are in four-quarter difference terms.)

8. The graphs of the cross-correlations indicate the length of lags most likely to yield good correspondence in regression analysis. We then proceeded to the regression stage. This was conducted by including among the regressors a variable at several lags around the value where the cross-correlation was a maximum in absolute value, for example, if y_t had a high correlation with x_{t+u} we might include among the regressors $x_{t+u-4}, x_{t+u-3}, \dots, x_{t+u+4}$. The program for rejecting redundant variables (Beale *et al.*, 1967) will then tell us which are the best to retain. Thus the regression analysis effectively determines the lags to be included in the final equations.

9. The equations shown in Table 2 have only two regressor variables and “explain” between 75 and 90 per cent of the variation in the *F.T.* index, depending on the period used in fitting. Fig. 5 shows the predictions which these equations could have been used to produce beyond their periods of fit; that is to say, they give the forecasts which could have been made with the information then available. These results were quite good for the period 1961 and 1967. Equation (1), fitted from 1948 to 1958, would have predicted the start of the 1958/59 boom but would have missed the second part of the boom in 1959/60. Even that equation, which was fitted over a period of lower variation than the post-1958 period, could have been used to forecast the shape of the *F.T.* index curve up to 1964, although it underestimated the level. The other equations which have used data up to 1960 for fitting give good forecasts of level and shape up to 1967. These forecasts could be produced for almost 6 quarters ahead (that is, the shortest lag, 6, minus the time taken for data to become available). A crude equation had been developed by one of us (E. D. G.) in autumn 1965 and successfully forecast in advance the fall in the index which followed “the July 1966 measures”.

10. We arrived at equation (7) early in 1967 and decided to use it to forecast the *F.T.* index over the next year, by quarters. The resulting prediction was quite unambiguous: before the end of the year a downward swing would start on the stock market and would be equivalent to a serious recession.

As things turned out the market went on rising until the most optimistic bulls had doubts about its stability. It looked as if our first attempts at forecasting were a spectacular failure. We had, however, correctly forecast a major sterling crisis of such gravity that we were forced to devalue and the ensuing budget, to quote the Chancellor, Mr Jenkins, called for “greater tax increases than had ever before been imposed in peace or war and the incomes policy had been backed by legal powers to an extent which is unparalleled in western countries”. The anomaly of this equation is discussed more fully in paragraphs 13 and 14 below.

11. The equations shown in Table 2, even when fitted over 1959–61, underestimate the level of the index at the top of that boom. This led us to consider other variables which might give a better fit over this period. In particular we tested the hypothesis that when the index is rising it gains a certain momentum which will keep it rising (this is probably becoming increasingly important with the development of unit trusts) until some major event such as a budget or bank rate adjustment destroys the market’s confidence. We defined an artificial variable called “the euphoria index” which had a value zero in the decreasing part of a fluctuation and had a value in the increasing part equal to the rise since the last trough multiplied by the number of quarters since the last trough. Troughs were considered to have occurred in 1958, 1962 and 1966. The inclusion of the euphoria index in an equation of the Table 2 type improved the

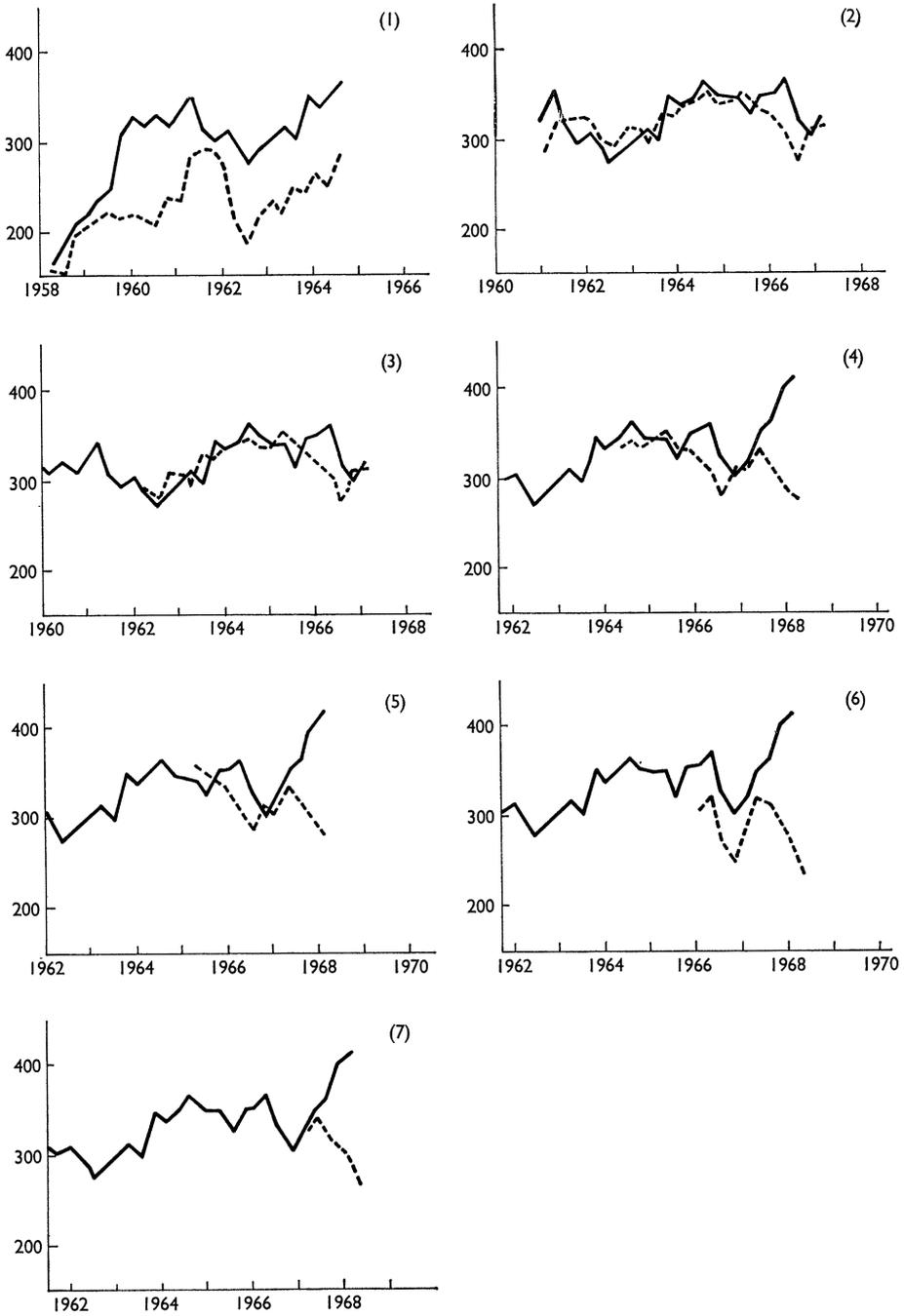


FIG. 5. Forecasts produced by the equations shown in Table 2. —, actual *F.T.* share index; ----, forecast. Equations are identified by the number shown in brackets.

R^2 from 0.91 to 0.95. The equation was

$$\begin{aligned} (F.T. \text{ index})_t = & 541.17 + 0.00046 (\text{car production})_{t-6} \\ & (19.1) \\ & - 4.88 (F.T. \text{ commodity index})_{t-8} \\ & (9.5) \\ & + 0.0080 (\text{euphoria index})_t. \\ & (6.0) \end{aligned} \quad (8)$$

Fig. 6 shows the fit of this equation to the *F.T.* index.

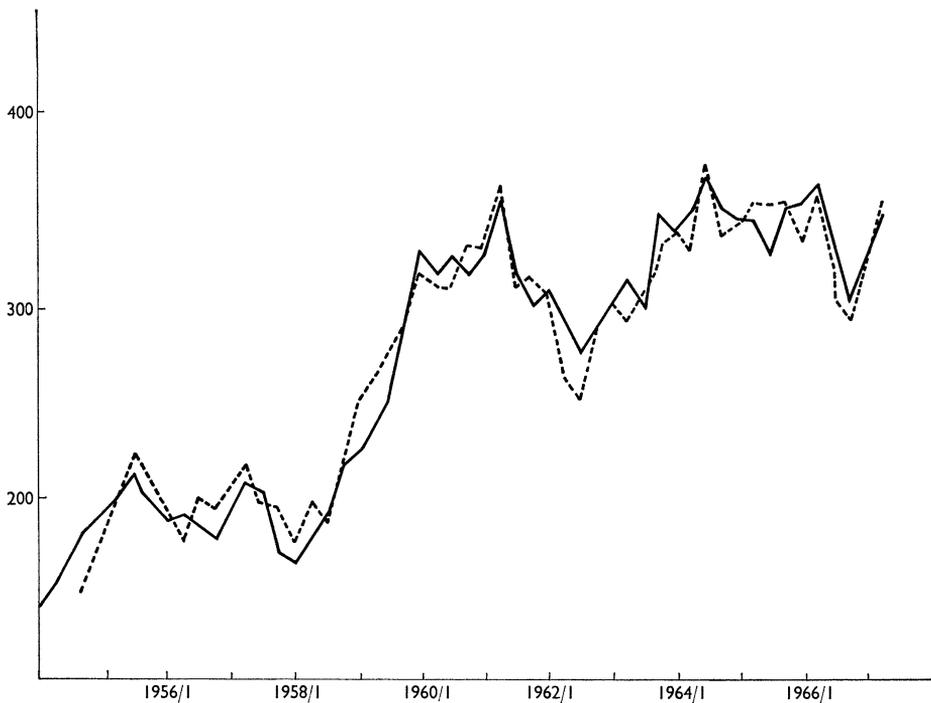


FIG. 6. ———, actual *F.T.* share index; - - - - -, fitted values (for equation (8)).

12. Two other equations with high values of R^2 are shown in Table 3. Both of these depend to some extent on the existence of an oscillatory movement in the *F.T.* index. However, the influence of commodity prices at about 8 quarters and government securities index or interest rates at 13 quarters has been observed in other equations. The fit of one of these equations is shown in Fig. 7.

Both of these equations forecast the recovery in the *F.T.* index in 1967 and although they continued to indicate the direction of movement they were out of control by 1968/1 and 1968/2. At the time we had tended to favour equations such as equation (7) because it had shorter lags and because it required no assumption about a cycle of constant length in stock market prices.

TABLE 3
Fitting equations for (F.T. share index)_t

Equation No.	R^2	Constant	Coefficients			
			(F.T. index) _{t-17}	(S. and P. index) _{t-17}	(Reuter's commodity index) _{t-8}	(Government securities index) _{t-13}
9	0.957	681.66	0.334 (6.89)		-0.09 (4.48)	-5.00 (18.93)
10	0.975	501.39		2.241 (12.21)	-0.06 (4.4)	-3.44 (12.5)

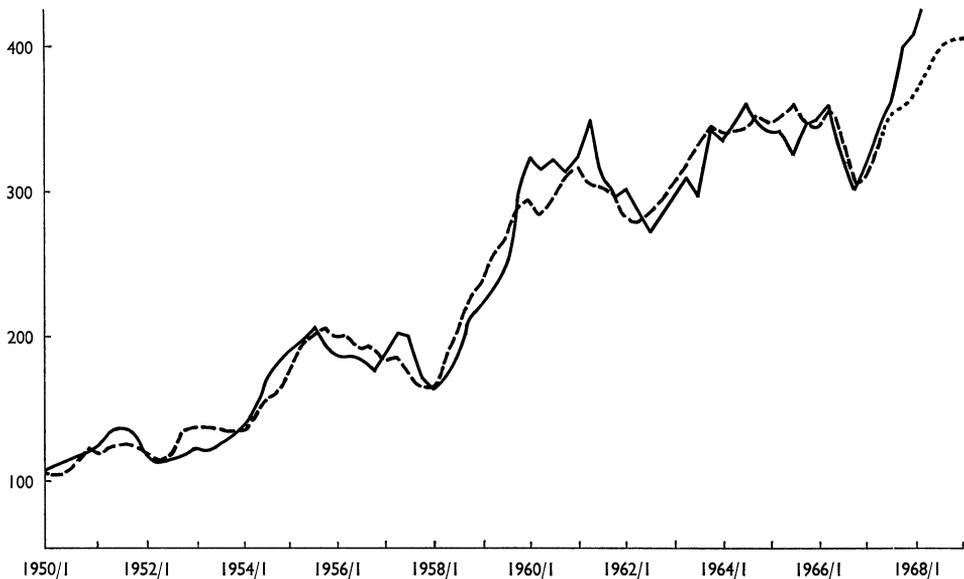


FIG. 7. ———, Actual *F.T.* share index; - - - - -, fitted values (for equation (10)).

13. As mentioned in paragraph 10, the equation type shown in Table 2 produced forecasts for the *F.T.* index for 1967/4 and early 1968 which were badly wrong. In the previous twenty or so years two variables had been sufficient to “explain” and predict the movement of the *F.T.* index. These were car production and a commodity price index. (The commodity indices are in fact highly correlated with sterling liability.) There is little doubt that if an attempt had been made to bring the U.K. trade account into balance without resort to devaluation, drastic deflation would have been necessary and this would bring a serious setback in equity share prices. But this did not happen and the market has continued to rise through the first half of 1968. The economy was now being managed in a different way. After devaluation there was no immediate deflationary action, there was a consumer-spending spree, and a stock market boom and imports rose to a level which caused acute concern. We continued our study and concentrated on looking for further explanatory variables.

We had been aware all along of the danger that a variable, which had not seemed important during the period used for fitting the equation, could suddenly become a key factor. This is a problem for all model builders and there is no easy solution. One can monitor the levels of variables which are suspected of being potentially important but there is still the problem of identifying the potentially important.

14. We re-examined several variables and particularly their recent levels. The 1967/68 boom on the London market was not accompanied by any corresponding movement on Wall Street and the two markets often move together. There were no very definite signs of improvement in the economy. Interest rates were at a very high level which usually means a bad time for equity prices. High interest rates are usually the signs of a restriction on consumer demand and we were being told by the government that we would have to consume less and that the next two years would be very difficult.

The deflationary package used by Chancellors since 1950 has usually involved an increase in bank rate and a brake on consumer spending through reductions in money supply and total bank advances. This can be seen in Fig. 11. However, the high interest rates following November 1967 were not accompanied by any reduction in total bank advances. In fact, as can be seen from Fig. 9, bank advances rose quite sharply.

This led us to reconsider total bank advances at a short or zero lag as a possible explanatory variable. Fig. 10 shows the fit of bank advances to the *F.T.* share index; both are in four-quarter first differences. The value of R^2 for the equation is 0.53, but the patterns of movement are closer than is suggested by this value. The fit can be improved by including other explanatory variables. See, for example, Fig. 11; the fit is given by:

$$\begin{aligned} (F.T. \text{ index})_t &= 11.218 + 0.0565 (\text{bank advances})_t && (4.57) \\ &- 0.396 (F.T. \text{ commodity index})_{t-9} && (3.92) \\ &- 0.00016 (\text{U.K. car production})_{t-12}, && (11) \\ &&& (2.92) \end{aligned}$$

$$R^2 = 0.61.$$

All variables are expressed as four-quarter differences.

Bank advances and money supply are the only series which we have investigated so far which “explain” even statistically the strong upward movement in share prices in 1968. We do not pretend to know the mechanism by which bank advances or money supply might influence share prices, whether it is through increased purchasing power of investors or whether it is through a stimulation of consumer spending which is, in the short term at least, good for trade and is seen as being good for share prices. It may even be that there is some other factor to which share prices and bank advances both respond.

THEORETICAL CONSIDERATIONS

15. The series which we used were all taken from official sources or published data like the *F.T.* index. We had no control over their accuracy or the interval of observation. There were four points in particular we had to consider in some detail:

- (a) Monthly and quarterly data are often subject to seasonal variations. This affects short-term autocorrelations and hence autoregressions. It may also affect lags in cross-correlations.
- (b) It may be better to use logarithmic transforms of some series, for the usual reasons, e.g. that price changes are relative, or that the combined efforts of different variables are multiplicative.
- (c) A great many of our series contained trend. We have to be careful that our lagged relations are not just the kind of covariation which one would expect from two qualities trending in the same direction. On the other hand, if we eliminate trend we have to be equally careful that relations between residuals are not contaminated by the trend-removal process (see, for example, Kendall and Stuart, 1967, Vol. 3, 46.14).
- (d) In particular, we tried taking four-quarter differences of some of the series to eliminate linear trend. This is equivalent to taking a moving average with weights (1, 0, 0, 0, -1).

16. As regards seasonal effects, we removed them in every case where we thought, on general grounds, that they might exist. The program used was the one developed in the U.S.A. by Julius Shiskin (1960). In effect, it fits a moving average to the series, works out the ratio of actual to the average and constructs a factor for each month or quarter which is divided into the actual to give the deseasonalized figure. The program allows for slow changes in seasonal pattern by a moving average of seasonal indices which, in effect, discounts remotely past history. This method, in one form or another, is commonly used for the elimination of seasonal variation and appears to work quite well in practice. The last word on deseasonalization has not been said (there are no last words in time-series analysis), but we think it very unlikely that the method we used has seriously distorted the data.

17. In some cases we did not attempt any further manipulation of the data. Table 2 is a case in which one variable is regressed on the others with no corrections except for seasonality. Most of our series, however, contained trend and we felt it necessary to extract the trend element before embarking on the detection of comparatively short-term relationships.

18. In economic relationships there is a good deal to be said, on theoretical grounds, in favour of using logarithms of the variable rather than the variables themselves. When we construct linear regressions of the usual type, it may look rather remote from reality to express a price, say, as the sum of a physical quantity of production, an interest-rate and a monetary measure of bank advances. To a physicist such an equation would be suspect as being dimensionally heterogeneous. Economic equations with this kind of complexity are often more realistically expressed as products—for example the Cobb–Douglas equation. Equivalently, they are better expressed as linear in the logarithms of the variables involved. This reinforces what has already been mentioned, that it is relative changes which are often of greater interest. The same thought lies at the basis of a good deal of current projective forecasting which assumes an exponential rate of growth or a fixed percentage rise in gross national product.

19. In many cases, then, we worked with the logarithms of quantities, at least in the first instance. But in practice there is an easy escape from the danger of oversophisticating the data: when in doubt do it both ways. On a number of occasions we reworked equations expressed one way in the other, and relied on the reality of the effects only when both methods gave the same kind of result.

The following, for example, is the equivalent of equation (7) in terms of logarithms:

$$\begin{aligned}
 (\log F.T. \text{ index})_t &= 7.355 + 0.512 (\log \text{ U.K. cars production})_{t-6} \\
 &\quad (12.76) \\
 &\quad - 1.847 (\log F.T. \text{ commodity index})_{t-8}, \quad (12) \\
 &\quad (-8.04) \\
 R^2 &= 0.89.
 \end{aligned}$$

20. Trend-elimination is a more serious problem for the theoretician. The cases we considered were either trend-free—so far as the eye could see—or were over a period in which the trend was approximately linear. As a general rule, therefore, we simply fitted a linear regression on time. This is not the usual method, which prefers a moving average to a polynomial fitted over the whole course of the data, but for our purposes it had two important advantages concerning the possible distribution of residuals:

- (a) If there are short-term fluctuations of a pseudocyclical kind the method will not seriously affect the residuals, provided that over the course of experience there has been time for several swings to take place. Failure of the fitted line to correspond exactly to the real line may somewhat affect the amplitudes of the fluctuations in the observed residuals, but will not affect their turning points; and it is the point of reversal of movements which is often of major interest.
- (b) Likewise, if random (unautocorrelated) residuals are imposed on a linear trend, the fitting of a linear regression will not seriously affect the general behaviour of the observed residuals.

21. The subject is considered in more detail from the theoretical and experimental viewpoint in the Appendix.

22. The taking of first differences eliminates linear trend. In a series of random residuals it will generate a serial correlation of about 0.5 but no serial correlations of higher order. Its effect on oscillatory movements depends on the pseudo-period, but for those of more than two time-intervals it will reduce the amplitude, and may do so seriously. For most purposes we have preferred the fitting of a linear trend by regression but, as noted above, have tried the variate-difference method on occasion, particularly in equations using bank advances. Once again, when there was doubt about the reality of the results, several methods were tried.

23. The apparent relationship between the S. and P. index and U.K. car production shown in Fig. 1 was tested by regression; the equation with $R^2 = 0.94$ is

$$\begin{aligned}
 (\text{S. and P. index})_t &= 3.34 + 0.00021 (\text{U.K. car production})_{t-6}. \quad (13) \\
 &\quad (34.9)
 \end{aligned}$$

This was also tested in four-quarter first-difference terms and the fit is shown in Fig. 8 (i). The fit is particularly good up to 1966. The deviation led us to look for other and perhaps more meaningful leading variables. We had never suggested that the level of car production in the U.K. directly influenced the level of the Wall Street stock market but believed that both variables depended on some other monetary or trade factor to which U.K. car production reacted sooner. We tried several American series including car production but without success. Finally, we observed the relationship, displayed in Fig. 8 (ii), between the S. and P. index and the U.K. bank

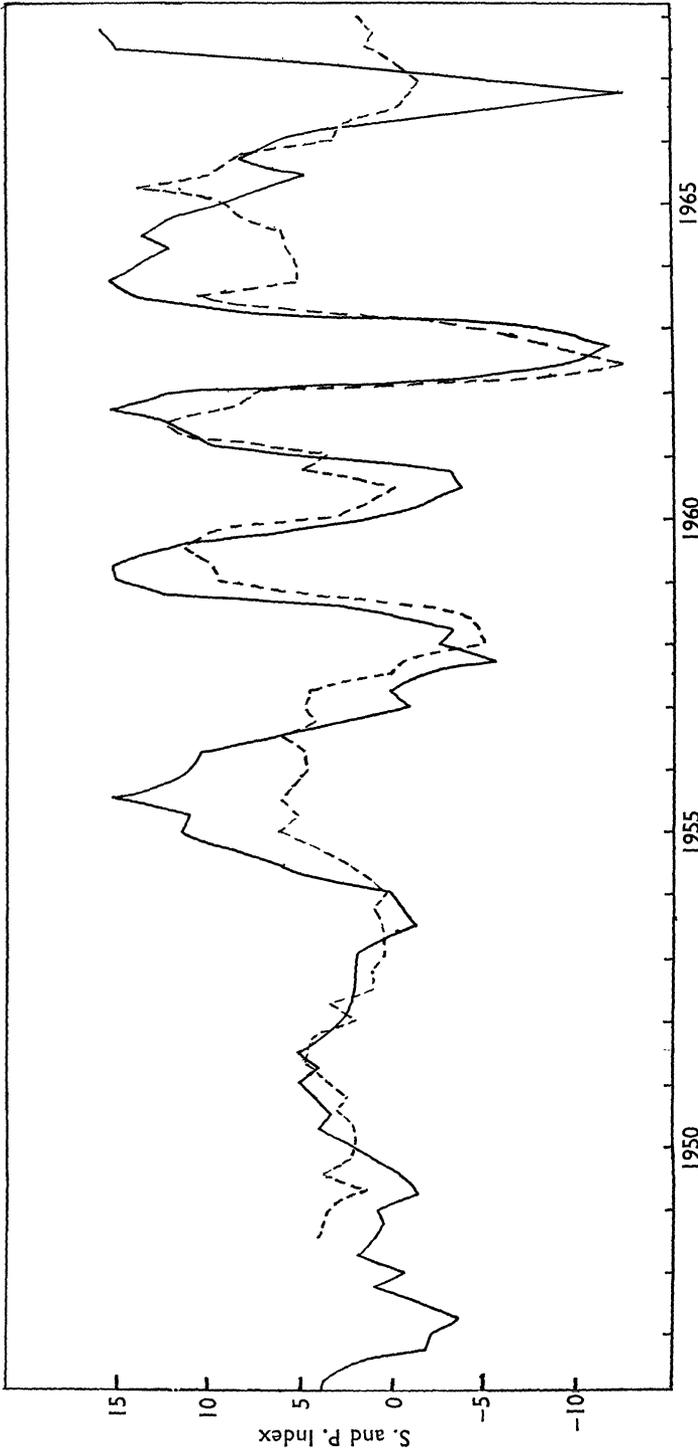


FIG. 8 (i). ———, S and P index (annual differences); - - - - -, U.K. car production (annual differences) leading by 18 months—fitted values.

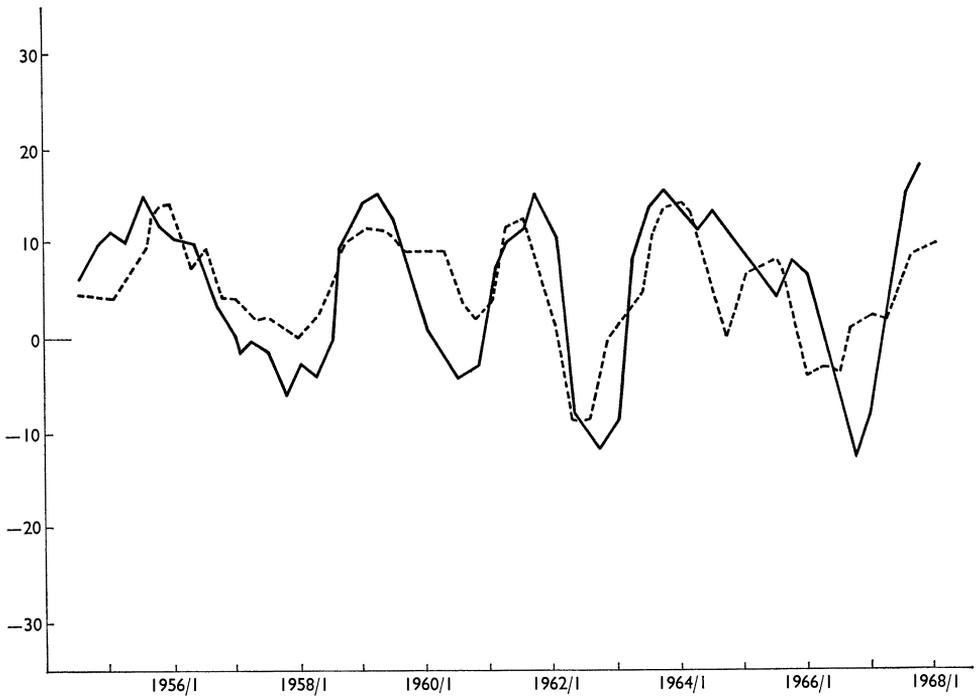


FIG. 8 (ii). —, S. and P. share index (annual differences); ----, fitted values (for equation (15)).

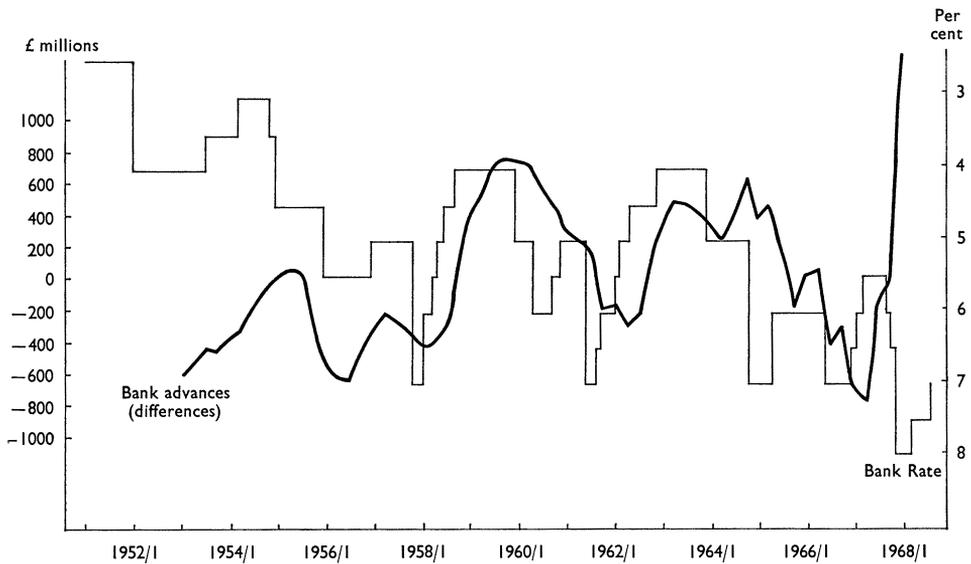


FIG. 9.

rate which still seems to hold good up to the present time. The equations for the fits shown in Figs 8 (i) and (ii) are:

$$(\text{S. and P. index})_t = 3.30 + 0.000092 (\text{U.K. car production})_{t-6}, \quad (14)$$

(7.6)

$$R^2 = 0.52,$$

and

$$(\text{S. and P. index})_t = 4.7 + 4.71 (\text{U.K. bank rate})_{t-14}, \quad (15)$$

(6.5)

$$R^2 = 0.44.$$

All variables are in four-quarter difference terms.

24. We have reported here a selection of the results obtained and of the regression runs attempted. Space and the wish to avoid confusion prevent us from exhibiting them all, but in presenting a selected set we have to guard against two further possible theoretically based criticisms. The first is that if one calculates a mass of relations on a large number of variables, some of them are likely to turn out "significantly" by sheer chance. This is a difficult point to meet with the precision which a statistician likes to bring to bear on his significance tests. There are no such tests available to condition our judgment whether the various relations to which we call attention are significant in any ordinary sense of the word. It seems to us, however, that they are significant in the statistical sense, namely that they are not effects which one would get in unrelated series of a stochastic kind.

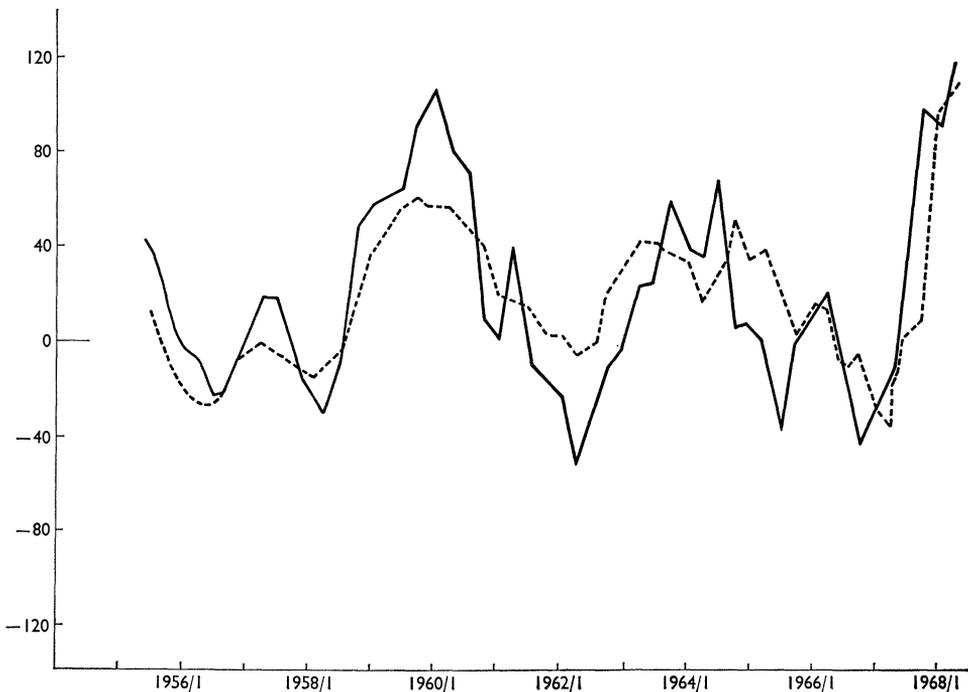


FIG. 10. ———, *F.T.* index (annual differences); - - - - -, fitted values.

25. The second criticism is that, over the time-period examined, there are short-term fluctuations which, not having had time to get out of step in the usual way, may generate autocorrelations and hence lag-effects which are not permanent. From the purely empirical viewpoint of short-term forecasting this may not be important, but from the explanatory point of view it is.

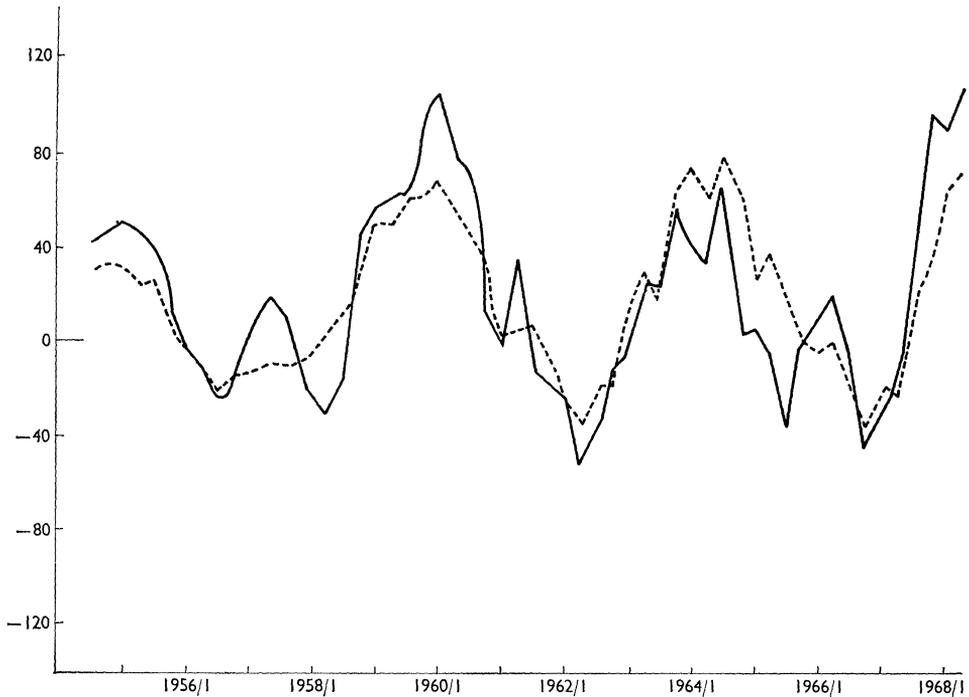


FIG. 11. ———, *F.T.* index (annual differences); - - - - -, fitted values (for equation (11)).

26. The results reported here were essentially empirically determined although we, of course, had some hypotheses in mind in selecting variables for inclusion. It is of the nature of empirically determined relationships that the statistics alone tell us nothing about the causality underlying them. We do not propose at this stage to advance any well-developed theory to explain these statistical relationships. To do this would require more general economic analysis and probably the construction of a model in causal and deterministic terms.

27. Even for forecasting some caution is necessary in the use of these relationships as indeed it is for any forecasting method. Socio-economic systems may be undergoing change either by ordinary evolutionary processes or by the deliberate intervention of human agencies. Thus any prediction must be hedged around by government regulation of the economy, by changes in consumers' or producers' behaviour and by technological development. Changes, however, may not be so quick as some people think. The evidence so far suggests that no rapid change took place in the factors affecting the general level of the stock markets in the last fifteen or so years. While equations such as these may sometimes be wrong for the various reasons mentioned,

some of these reported here have been shown to produce good forecasts and did forecast the major turns in the *F.T.* index in 1966 and the 1966 and 1967 economic crises in real time. It seems to us that the method of lagged relationships deserves serious consideration for short-term (one or two years ahead) economic forecasting.

ACKNOWLEDGEMENTS

We should like to acknowledge the very useful help we have had in the analysis and preparation of diagrams from Mr John Rees, Mr Philip Hurst and Mrs Elizabeth Rawlings of Scientific Control Systems Ltd.

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APPENDIX

The Effect on Random Residuals of Eliminating Trend by Regression Methods

1. We owe the following general investigation to Professor E. M. L. Beale. Assume that we have a model

$$y_i = \sum_r x_{ir} \beta_r + \epsilon_i,$$

where the ϵ 's are independently distributed with variance σ^2 and zero means.

Assume further that we fit the model

$$y_i = \sum_r b_r x_{ir} + e_i$$

by conventional least squares, e_i being the actual residual.

To calculate the b_r we set up the normal equations

$$\sum_s a_{rs} b_s = \sum_i y_i x_{ir},$$

where the matrix $A = (a_{rs})$ has elements

$$a_{rs} = \sum_j x_{jr} x_{js}.$$

We then find that

$$b_s = \sum_r \sum_j a_{rs}^* y_j x_{jr},$$

where $(a_{rs}^*) = A^{-1}$.

This is equivalent to

$$b_s - \beta_s = \sum_r \sum_j a_{rs}^* x_{rj} \epsilon_j.$$

Further, since

$$e_i = \epsilon_i - \sum_s (b_s - \beta_s) x_{is}$$

we see that

$$e_i = \epsilon_i - \sum_r \sum_s \sum_j a_{rs}^* x_{jr} x_{is} \epsilon_j$$

$$= \sum_k c_{ik} \epsilon_k,$$

where

$$c_{ik} = \delta_{ik} - \sum_r \sum_j a_{rs}^* x_{kr} x_{is}$$

and δ is the Kronecker delta.

It follows that

$$\text{corr}(e_i e_j) = \frac{\sum_k c_{ik} c_{jk}}{\{\sum_k c_{ik}^2 \sum_k c_{jk}^2\}^{\frac{1}{2}}}.$$

In the particular case when there are $N = 2n + 1$ values of the time variable $-n$ to n , and we fit (about means)

$$y = \beta_0 + \beta_1 t,$$

we find

$$e_t - \epsilon_t = - \left[\bar{\epsilon} + \frac{\sum t \epsilon}{S_2} \right] t,$$

where $S_2 = \sum t^2 = \frac{1}{3}n(n+1)(2n+1)$.

Now t and ϵ are independent. The expectation of $\sum t \epsilon$ is zero and its variance is σ^2/S_2 . Its range of variation is then of the order of $\sigma/\sqrt{S_2}$. t never exceeds n in absolute value, so the difference between e and ϵ is of the order of σ/\sqrt{N} . Hence the serial correlations of ϵ will not differ much from those of e unless N is small. For our particular purposes we wished to observe the behaviour of the serial correlations of residuals in a time series with linear trend. We therefore constructed four series of the type

$$u_t = t + \epsilon_t \quad (t = 1, 2, \dots, 40).$$

In all series ϵ was a discrete integer variable chosen from a table of random numbers. For series 1 the range of ϵ was 0 to 9; for series 2, 0 (2) 18; for series 3, 0 (3) 27; and for series 4, 0 (4) 36. For each of the four series there was calculated a regression on t , the residuals, and the first 30 serial correlations of these residuals. We also calculated the 10 cross-correlograms from -30 to $+30$ lags but omit them to save space. They have the same irregular appearance as the serials. The diagrams are sufficiently unlike the smooth curves of Fig. 6 to indicate that the latter are not artefacts created by the trend-removal process, at least so far as concerns random residuals.

DISCUSSION ON THE PAPER BY MR COEN, MR GOMME AND DR KENDALL

Professor J. DURBIN (London School of Economics): I do not know which gives one greater pleasure on this sort of occasion, to welcome an old friend or to welcome new authors. Dr Kendall is, of course, an old friend, and Mr Gomme and Mr Coen are, I believe, new authors as far as the Society is concerned, so I am in the happy position of being able to enjoy both pleasures simultaneously.

There are, it seems to me, essentially two basic views on the role of statistics in scientific investigation. In the first of these, one regards statistics as a substantial subject in its own right. According to this view, one would in any particular situation assemble as much as possible of the relevant information and then examine it dispassionately and

rigorously for evidence of statistical regularities and relationships, trying to keep one's mind free from preconceived ideas and expectations. This approach is therefore essentially pragmatic and empirical.

An alternative view of statistics is that it is essentially a humble subject, playing a rather subsidiary role in any particular investigation. According to this view, one would first go to a specialist in the field under review usually a non-statistician, and one would ask him to provide the theoretical model or class of models for the phenomena under investigation. The statistician's job is then to fit the models, carry out tests of significance and so on, the object being either to evaluate the extent to which the evidence supports or refutes the specialist's hypotheses, or alternatively to emerge with the best-fitting model.

As stated, these two views are extreme and many of us hold somewhat intermediate positions. My own approach is that, although on the whole I am generally sympathetic to the latter view, I am quite happy to see both approaches tried and to let the results decide which approach is better on any particular occasion.

As far as short-term economic forecasting is concerned, my feeling is that it is not clear at present whether one does better to fit economic models based on postulated relationships between the variables, or to use statistical forecasting of a frankly *ad hoc* character. This is not to say that an econometric approach to the subject does not have considerable value in other directions—for example, in understanding the nature of the causal forces at work or in predicting the effects of a change of the structure of the system. The difficulty in using the econometric approach in practice is often that there is not sufficient independent variation in the data during the period for which observations are available to enable one to estimate all the desired relationships with sufficient accuracy, and it may well be that one can actually gain greater forecasting accuracy by throwing away some of the theoretical structure and using a more empirical approach.

The basic approach of the authors is to take as their forecasting relation the multiple regression or best fit. This approach has its dangers as they themselves admit since if the relationship is not based on a well-founded causal connection, there is no guarantee at all that it will continue to hold outside the period of fit. Many have pointed out that where there are trends in two variables which are unconnected causally, one can obtain a high degree of apparent correlation between the variables concerned. It is not quite so obvious that if one adopts the frequency approach to time series analysis the same phenomenon holds. Whenever there is oscillation in the data which has a period longer than the period of observation, then by moving two series along the time axis relative to each other until the observed correlation is highest it is possible to obtain a high degree of spurious correlation between variables. In other words, low frequency oscillation as well as trends can induce nonsense correlation between time series.

Having said this, I must confess that the degree of fit achieved by the authors in predicting the share index such a long period of time ahead was a surprise to me. I would be interested to know whether one could do any better by fitting a well-formulated econometric model using the same data, and hope that some econometrician will rise to the challenge.

I notice that the regression relations in the paper contain lagged values only of the explanatory variables and I would like to ask the authors why no lagged values of the dependent variables are included. If all that is required is a forecasting relation, I would expect the fit to be improved by including variables of this type as predetermined variables.

Another feature of this exercise is that the analysis is done entirely in the time domain. When lagged relations are under study there are some advantages in using frequency-domain concepts and I wondered whether the authors had considered this approach at all. For example, using spectral concepts it is a fairly easy matter to allow for linear filtering operations such as a trend-elimination or the elimination of seasonal effects where this is done by linear or approximately linear methods. Essentially, all one has to do is make a Fourier transformation of the data, divide through by the transfer function of the filter and then either stay in the frequency domain or transform back to the time domain.

Finally, there is the question of the serial-correlation of the disturbances in the regression model. In time series regression problems there is often a certain amount of serial-correlation present, and if this is so, it is advisable to allow for this in the fitting procedure. I wonder if the authors have considered this. I would be interested to see what the effect of allowing for it would be on the results.

I have much pleasure in moving the vote of thanks to the authors for their most interesting paper.

Mr J. P. BURMAN (Bank of England): The way in which economists nowadays tackle the analysis of our economic system is painstaking construction of models of different parts of it; discussion of the formulation of these with the statistician; great care to indicate the direction of causation; testing; rejecting; testing again; and finally assembling the parts into a coherent set of equations. Often non-linearities appear in the relationships. This is the humbler of the two roles for the statistician described by Professor Durbin.

As I understand them, Dr Kendall, Mr Coen and Mr Gomme want us to treat the economic system as a "black box". We read various inputs into the box and see whether the output resembles one of the inputs at some earlier time, or a linear combination of inputs at different times. We are to take no notice of the nature of the series or the probable direction of causation; and most of the equations estimated in this paper are based on only 40 or 50 quarterly observations of series whose cycles are 16–20 quarters in length. I suggest we would not be at all surprised to find some apparently significant R^2 in a random collection of such series. But even if we found an R^2 as high as 0.9, in the absence of any causal explanation we would do well to be very sceptical of its future performance.

To give an example, several years ago I published a regression with 20 observations on one variable that gave a correlation of 0.995 and had a good causal connection. Yet in later years the prediction errors in the equation became much larger and it had to be abandoned.

In Sections 1 and 2, the paper refers to long lags as if econometricians had ignored them: there has been a great deal of work in the United States on accelerator models for fixed investment in manufacturing industry, that is, depending on a distributed lag of growth of output; and there is evidence that a distributed lag averaging two years gives a good fit for U.K. data.

As one would expect from Dr Kendall, this paper displays a mastery of statistical techniques and a thorough analysis of a large range of figures, but one needs to examine them from the viewpoint of an economist.

The authors "predict" the *Financial Times* share price index from past values of other economic and financial series, but nowhere is there any mention of the fact that a share price index reflects people's expectations about the *future*. Stock Exchange lore suggests about 18 months forward, but this is perhaps exaggerated. Investors may have tended to be guided in the past by the publication of annual profits, somewhat out of date. But the modern institutional investor looks at the latest national income estimates and is much more closely in touch with current trends. This will tend to upset any lag correlations based on the past. Dr Kendall mentioned that they had tested some of the series for lag correlations in the reverse direction. Equation 15 is an obvious candidate for this treatment, replacing a lag of 14 quarters by a lead of 4 quarters. Professor Durbin referred to the use of cross-spectral analysis for estimating lags and I would strongly support this. I have recently made a first essay into this field with money supply and the index of production multiplied by a price index. The result is a uniformly high coherence and no evidence of phase shift. But the fact that the series is synchronous does not mean—as Dr Kendall suggested—that control of money will affect production. It depends on the direction of causation and it may well be that money is the dependent variable. That is what the current monetary argument is about.

The authors' selection of explanatory variables seems curious: why not look at Gross Domestic Product or the index of industrial production? A glance at a chart of industrial production suggests that the troughs of 1956, 1963 and 1966, lagged the *F.T.* index by between 1 and 3 quarters (bearing in mind the delay in publication of production). One also wonders what cross-correlations exist between the "independent" variables. And would it be unreasonable to ask why the commodity price index always has a negative coefficient in Table 2? Apart from its effect on the shares of commodity producers, none of which are in the 30-share index, high prices generally indicate expanding world trade, which is probably good for the U.K.

In Section 10 the authors say that early in 1967 they forecast a marked downward swing in the share index before the end of the year. "We have, however, correctly forecast a major sterling crisis. . . ." This seems to imply several unstated assumptions: that car production is being used to predict the balance of payments cycle; that when the latter is in deficit, restrictive monetary or fiscal actions have in the past been taken; and that the latter produce a fall in the share index. Why not then regress the balance of payments against lagged car production, etc. and the share index against, say, bank rate and the Central Government's net balance of surplus or deficit?

In Section 12, the authors have lapsed into the use of causative language by saying that "the influence of commodity prices on the *F.T.* index has been observed".

In Section 13 they seem to class themselves among the model builders, but their concept of a model is completely different from that of economists. The latter would insist that each equation be supported by rational argument.

In Section 14, after the two-variable equation has broken down, they try to restore the situation by bringing in a series called "bank advances"; and even then R^2 becomes only 0.61 and the coefficient of car production has now changed sign. Moreover, I am afraid that Dr Kendall's conclusion, that a change in monetary policy has occurred since devaluation, is based on a misunderstanding of the bank advances series. This contains large loans to foreigners—mainly in foreign currency—the so-called Euro-dollars (see last column of the table in the *Monthly Digest* to which the authors refer). These loans to foreigners account for £500 million of the total increase of nearly £1,000 million between November 1967 and May 1968 (not seasonally adjusted) and they largely represent foreign deposits in London being on-lent abroad and revaluation adjustments due to the devaluation of sterling. Also, because of the change of coverage of the series at the end of 1966, which raised its level by some 40 per cent (see footnote to Table 1), the official statisticians have not yet attempted to seasonally adjust it. The seasonal movement is certainly upward between November and May, so the underlying increase in advances to U.K. residents was probably quite modest and would not go far in explaining the rise in share prices. A more appropriate series for this paper would be London clearing banks' "advances to customers and other accounts" as published in Table 35 of *Financial Statistics*.

I am strongly tempted to ask Dr Kendall a question, which is always appropriate for those who make forecasts about the stock market. Has he risked any money on them yet?

To sum up, Mr President, in view of Dr Kendall's distinguished reputation, both in this Society and outside, and his great textbook from which many of us have learnt so much of statistical method, I think it is a great pity that he should have presented his paper in a form which cannot but provoke a hostile reaction from all those who believe that in economics rational explanation should always accompany high correlations. As Dr Kendall said on a previous occasion, "We clearly cannot be satisfied with the mere eliciting of pattern, even if we go about the process systematically and discover a good deal more regularity than has been observed in the past. For a number of very obvious reasons we must go behind the manifest phenomena to see whether some model of the generating process can be set up. Not until we have a satisfactory model can we be said to understand the process fully or to retain control over it in changing circumstances."

I have pleasure in seconding the vote of thanks to Dr Kendall.

The vote of thanks was put to the meeting and carried unanimously.

Dr K. F. WALLIS (London School of Economics): The opening sentence of this paper tells us one thing of which econometricians are aware. It is probably worth while to mention some other relevant concepts from econometrics, first for practical reasons in the interpretation of this study, second to exemplify the earlier discussion (Professor Durbin's first view of the role of statistics) of measurement without theory, and, in general, to persuade students of econometrics that the route so thoroughly pursued by Mr Coen, Mr Gomme and Dr Kendall is not the only road to take.

A useful idea in econometrics is that of the *distributed lag*. It is often the case that the reaction of a dependent variable to a change in an explanatory variable is not concentrated at some particular point in time, but is spread over an interval. In general

$$y_t = \sum_{j=0}^{\infty} \beta_j x_{t-j} + u_t.$$

Irving Fisher realized that restrictions on the lag coefficients, β_j , would be necessary for successful estimation, and problems of multicollinearity would still remain if the infinite upper limit were replaced by some $n < \infty$. Koyck used the assumption that, after some point, the coefficients β_j decline geometrically; if this decline begins with β_0 we have

$$y_t = \beta_0 \sum_{j=0}^{\infty} \lambda^j x_{t-j} + u_t,$$

which gives the estimating equation

$$y_t = \lambda y_{t-1} + \beta_0 x_t + v_t.$$

This simple equation can be generalized by incorporating additional lagged values of either variable. Jorgenson (1966) shows that such "rational distributed lag functions" can approximate any lag distribution; an application in a slightly different context is contained in the work of Box and Jenkins. (It is not clear whether this is what the authors mean when they speak of autoregression taking care of any important lag effects. Little use of these techniques is made in *Business Cycle Indicators*, in any case.) For the general case, the calculation of such summary statistics as the mean lag is straightforward. A useful discussion of this whole area is given by Griliches (1967); note especially his remarks on "theoretical ad-hocery".

Also useful in econometrics is the distinction between endogenous variables (whose variations are explained by the system) and exogenous variables (which are determined outside the system). Most economists would regard the explanatory variables used by the authors to be endogenous; in which case their forecasting equations could not be regarded as reduced forms of some underlying structure. Within a single market, for example, where price is regarded as endogenous, specification of an underlying structure would require demand and supply functions to be spelled out, for price is determined by the interaction of demand and supply.

Taking an example from the paper, if we were to set up an economic model relating, *inter alia*, U.K. car production, U.K. bank rate and a Wall Street index, and we were able to reduce it to some simple form as equations (13)–(15) by not worrying about the endogenous/exogenous distinction, then the dependence, as Mr Burman indicates, could just as well be the other way round. (And, of course, cyclical variations in the two series make quite possible a relation between U.K. bank rate and the lagged S. and P. index.)

Share prices seldom appear in macro-econometric models (of the 10 U.S. models described by Nerlove (1966), only the Tinbergen (1939) model contains share prices). Concern is generally centred on the "real" economy, and interest is concentrated on forecasting income, consumption, investment, and so forth. A well-known *ex-ante* forecasting model is that of the Research Seminar in Quantitative Economics at the University of Michigan, and one might expect the authors to rise to the challenge implied

in the statement by Suits (1967) that "The proper evaluation of the accuracy of a forecast is to compare it, not with the outcome of events, but with the *other forecasts* that were actually made at the same time. In this comparison econometric forecasts have compiled a remarkable record", in that forecasting errors are smaller than those produced by other methods.

Macro-econometric models are also used for *policy analysis*, whereas the methods in this paper do not permit an evaluation of, for example, the probable effects of change in tax rates, government expenditures or institutional arrangements. See, for example, the discussion of paragraphs 13–14; also note the absence of any explicit consideration of institutional changes in and around the stock market, regularly discussed in such journals as *The Economist*. "This is [indeed] a problem for all model builders"—a group from which the authors presumably exclude themselves at the present time. Progress towards a solution is surely to be made by explicitly considering all elements of the causal chain, and closely examining all the interlocking relationships. The possibility of incorporating structural changes, such as those referred to in the closing paragraph, is then always available.

Two technical points. First, with regard to seasonality, the Shiskin methods have been shown to remove from a series more variation than could reasonably be called seasonal. Not much is known about the effect of separate seasonal adjustment of a number of series on the relationships between them—except that these procedures preserve neither sums nor ratios. In these circumstances, the usual practice in econometrics is to work with unadjusted data, adding seasonal variables to the equations if necessary. This also admits the possibility that seasonal variation in the dependent variable is explained, along with other kinds of variations, by the variations in the regressors.

Second, with regard to trend. The same regression coefficients are obtained either when variables are expressed as deviations from a linear regression on time or when a trend variable is added to a regression in the original observations. The regression residuals will also be the same, but R^2 in the former case will be decreased, and appearances are worsened. It would be more consistent with the general spirit of the authors simply to add a trend variable to all equations, and let the data determine whether or not it is needed.

Since stock market operators work in a non-detrended, non-deseasonalized world, predictions which would be useful to them would have trend and seasonality added back in. Is it the case that the summary statistics reported here can be interpreted as describing the accuracy of such forecasts of the actual variables, or are these R^2 for regressions in transformed variables?

Perhaps a compact summary to these remarks can be achieved by referring to a paper given to the Society by Dr Kendall some 16 years ago, entitled "The analysis of economic time series". Among the discussants was H. S. Houthakker now, if not then, an econometrician of repute. All the remarks which he made on that occasion, about the absence in the reported work of an acceptable theoretical framework which can be utilized in interpreting the results, are relevant today.

Dr S. ROSENBAUM (Central Statistical Office): I am glad of the opportunity to comment on the paper by Beale *et al.* (1967) on which this analysis of lag relationships has been based. It appeared in *Biometrika* and has not been discussed at a meeting of the Society. Beale *et al.* gave an economical method for selecting a best set of regression variables but, like its predecessor—the article by Efroymson (1960)—it did not consider the effect this would have on significance levels.

In the special case of analysis of variance with orthogonal components, an appropriate test entails the distribution of the largest F -ratio, namely the ratio of the largest mean square to an independent mean square. An approximation due to Hartley (1938) permits one simply to alter the significance level in the ordinary F -tables by a factor equal to the number of treatment mean squares. An exact test, tabled by Nair (1948), is available

for the case where each mean square has 1 degree of freedom. This would therefore be suitable for a multiple regression if the regressor variables were independent, but the presence of correlation affects the significance level. Recently Chien-Pai Han (1968) showed that the effect of correlation on the distribution of F_{\max} , defined differently as the ratio of the largest to the smallest sample variance, was to introduce the factor $\sqrt{1 - \rho^2}$ into the test criterion.

When there are lag relationships they may perhaps be considered as falling into K groups, in each of which there is a choice of different lags. The size of K , and the correlation if any between groups, determines the level of significance in selecting them, while within a group one would use an independent criterion to find the best lag.

Professor D. E. BARTON (Institute of Computer Science): Two or three years ago Mr E. D. Gomme and Mr M. R. W. Burrows introduced me to an earlier version of the present data and the analysis we made then is of interest in that it provides to some degree an independent study of the problem. In particular we looked at one or two of the questions which Professor Durbin and Dr Wallis have raised and it would be interesting to know whether our answers to these coincide with Dr Kendall's. It should be made clear, however, that not only were we using a set of regressor variables which differed by several items from the present set but, more critically, we did not have the advantage of the Beale-Kendall-Mann programme (or equivalent) to pick out the best regressors. Thus our conclusions may differ in detail from those here because of these limitations. Broadly, they were very similar.

We found that autoregression of the linear-trend-corrected log $F.T.$ index on previous values, even so few as the two immediately preceding, gave as good prediction as multiple regression on the linear-trend-and-seasonal-corrected regressions. This was a point Professor Durbin raised.

Generally, all those variables expressed in cash terms had logarithms showing marked linear trend but no significant higher-order terms. This is, presumably, mostly inflation but also increased affluence, productivity and working population, since items like employment and electricity consumption show similar but less marked behaviour. This relates to Dr Wallis's remarks.

Dr Kendall says he has taken the data as given. This has unfortunate consequences though it is probably unavoidable. The regressors, like the $F.T.$ index itself, are highly complex indices whose components have different trends and seasonalities. This introduces more variability than "purer" figures would, since one cannot remove seasonalities as effectively from composite series. Mr Burman's differentiation between foreign-currency and sterling bank advances is a case in point.

The remarkable phenomenon described in this paper [of the marked (lagged) correlations on particular regressors] may well be as much political as economic. That is, it may be largely generated by the artificially stimulated pre-election boom with its painful post-election disinflationary hangover. Mr Burrows has argued this more fully elsewhere; in his Chairman's address to the Industrial Section of the Market Research Society in July 1966 (which received some newspaper publicity at the time) and in *The Investment Analyst*, November 1966 ("Computer forecasting and the politico-economic trade cycle"). It could well be that the decision to hold the last election so soon after its predecessor has interrupted the rhythm of the cycle and that it is this as much as anything which has made recent predictions go astray.

Professor M. S. BARTLETT (Oxford University): No doubt this problem is of particular interest in relation to the economy but, as previous speakers have indicated, this is a general problem of methodology and statistics of interest in many fields. It may be illuminating to consider what the situation is, say, in the field of forecasting in meteorology. I am not an expert in meteorology, but remember some years ago, when in the United States, discussion with a meteorologist examining the problem of forecasting by using

statistical methods. I got from him the information that at that time the success of the empirical statistical method of forecasting on the basis of statistical patterns in previous data was running parallel with forecasting based on a detailed mathematical model which you have then to estimate as best you can and run through a computer to elicit what will happen to the weather in future. You had these two methods, one empirical and one theoretical, which at that time—I do not know the present position—were rather neck and neck. This is rather intriguing because surely if you have two quite different methods like that which are producing rather similar results, somehow you ought to be able to strike a mean and use both methods more efficiently to give even better results, but I do not know how to suggest that it should be done.

I do think it worth bearing in mind, although I am in favour of model building myself to a certain extent, that even if you do have the perfect model, if you know what the model is but it involves rather complicated equations with lots of coefficients, you have to estimate these, and it by no means follows that you will do better than with the more empirical methods. This means you must distinguish between the problem of interpreting what is going on and the problem of predicting what is going to happen. Prediction is one thing, and the problem of interpreting is another, and it does not always mean that procedures should synchronize for the two purposes.

Professor G. B. WETHERILL (Bath University of Technology): I am grateful to the authors for tackling this problem, because it seems to me that there is a great deal to be learned about the methodology of this kind of an analysis.

My first point concerns the use by the authors of the Beale *et al.* (1967) multiple regression programme. Now in any multiple regression analysis there will usually be a whole set of regression equations which attain nearly as high a sum of squares as the optimum set. It seems to me that the choice within this group has to be made with great care, and in particular an equation which made sense physically may be preferred, even if it were not the best.

Suppose one has several different sets of data. Then if an equation were fitted to each, there will be (in general) a different optimum equation for each. If there is an equation which does reasonably well for all the different sets of data, this equation would be preferred to other equations which did very well in some, but badly in others.

Now suppose our different sets of data are combined in some way, then it is always possible that because of some fluke variation, a single optimum equation is produced which in fact is spurious.

Finally, it always seems to me that situations of this kind are really more suitable for analysis as a functional relationship, and I wonder if the authors have considered it. I would also like to know if the authors used multiple regression because they think it really is appropriate or because there is no other technique which can be conveniently used.

The AUTHORS thanked the participants in the discussion for their remarks, and later replied in writing, as follows:

1. We are well aware, as several speakers from Professor Durbin onwards pointed out, that the empirical detection of relationships is only part of the scientific process. But several speakers, especially Mr Burman and Dr Wallis, seem to think that no empirical results should be published except when accompanied by rational argument (Mr Burman) or presented in an acceptable theoretical framework (Dr Wallis).

In short, Kepler should not have published the laws of planetary motion because he could not explain them; he should have presented them in the acceptable framework of Ptolemaic epicycles. Newton should not have published the *Principia* because he could not explain gravity; Jenner, Prout, Michelson and Morley, Doll and Bradford Hill should have kept quiet until they could explain what they observed.

2. The crux of the argument is whether our results are reproducible. We have followed them through long enough to suggest that they may be, but we acknowledge quite freely

that the only valid test is the experimental one. We strongly rebut any criticism based on the rejection of evidence simply because it cannot be explained. Mr Burman says that we want to treat the economic system as a "black box". We are as anxious as anyone to look inside the "black box" to find out how it works. All we would say is that, until such a thing is possible, it is worth while examining the stimulus-response relationships exhibited by the economy; or the lead-lag relationship where even "stimulus-response" may be thought tendentious.

3. Mr Burman is slightly less than fair in his reference to the length of experience. He has compared the shortest length of experience from one equation with the longest length from another: see Fig. 12 for the comparison of bank advances and share price over 18 years. The apparent relationship between U.K. car production and the S. and P. index, about which we do not wish to make any issue, is shown in Fig. 8(i) for 1948-68.

4. Both Professor Durbin and Mr Burman advise the use of spectral methods in time-series analysis. We think that in econometrics such methods are largely a waste of time. But we are willing to be convinced and will repeat a challenge which one of us made many years ago: can somebody tell us, by the use of spectral methods, something about economic time-series which was not already known or derivable more simply from correlation analysis?

5. Mr Burman questions our choice of explanatory variables. We have in fact tested the variables Mr Burman mentions and found that they are not as good as those in our equations at "explaining" the variation in the *F.T.* index.

6. We have been unable to find any convincing evidence that the stock market is able to anticipate. The market probably is based on people's expectations of the future but examination of any London share price index will suggest that these expectations are often not realized.

7. We were aware of the significant proportion of the increase in bank advances which was accounted for by lending to overseas residents. We would like to point to the similarity in movements in money supply and equity prices: see Fig. 13. We do not think that London clearing banks' advances, as suggested by Mr Burman, are an appropriate series for the more recent years because lending by these banks is becoming a less important part of total bank lending.

8. Dr Wallis's discussion on econometrics is not really our concern in this paper. What we have to say about distributed lags, the estimation of parameters in mixed systems, identifiability problems and so forth will be said on some later occasion. We do not, as Mr Burman and Dr Wallis both suggest, exclude ourselves from the group of model builders, only from the group of bad model builders.

9. Dr Wallis's quotation from Suits is one we wish we had found ourselves, for nothing reveals the invincible resistance to fact so much as this. One is, apparently, to judge of the *accuracy* of a forecast, not by whether it was anywhere near the eventuality, but by whether it agrees with other forecasts made at the same time. Now it is possible to be right for a bad reason just as it is possible, and even creditable, to be wrong for a good reason. But one must not forget that whole classes of forecasters may be wrong because their reasons are bad.

10. We agree with most of Professor Barton's comments. It is true that an autoregressive scheme may produce high values of R^2 , but since one of the primary purposes of the type of forecasting equation which we have been trying to develop is to indicate major turning points we have not pursued autoregressive schemes with one or two terms. There are some interesting theoretical points here which we intend to develop in later studies.

11. Certain pressures probably do exist to increase economic activity before elections and to hold elections when conditions are favourable. However, the coincident movements in bank advances, money supply and equity share prices have not been upset around any recent election. We have also developed equations, which we do not propose to publish here, to predict interest rates. These have not been significantly off course in

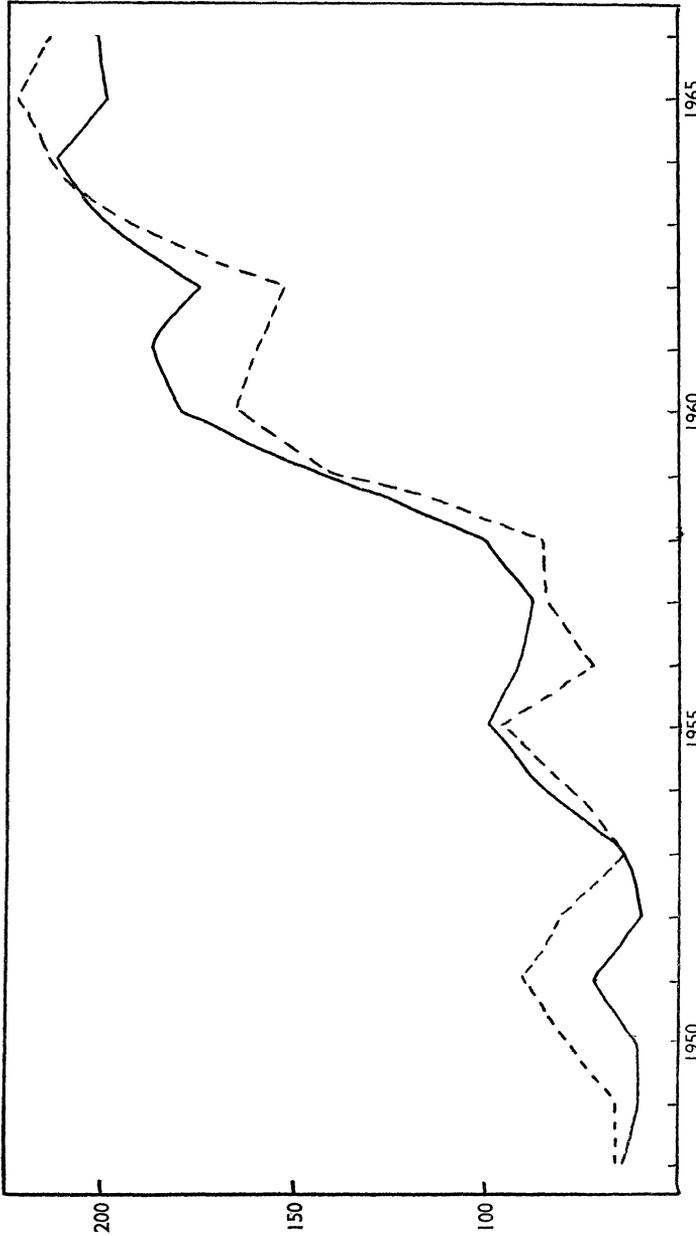


FIG. 12. ———, *F.T.* actuaries share price index; - - - - -, London clearing banks advances (fitted values).

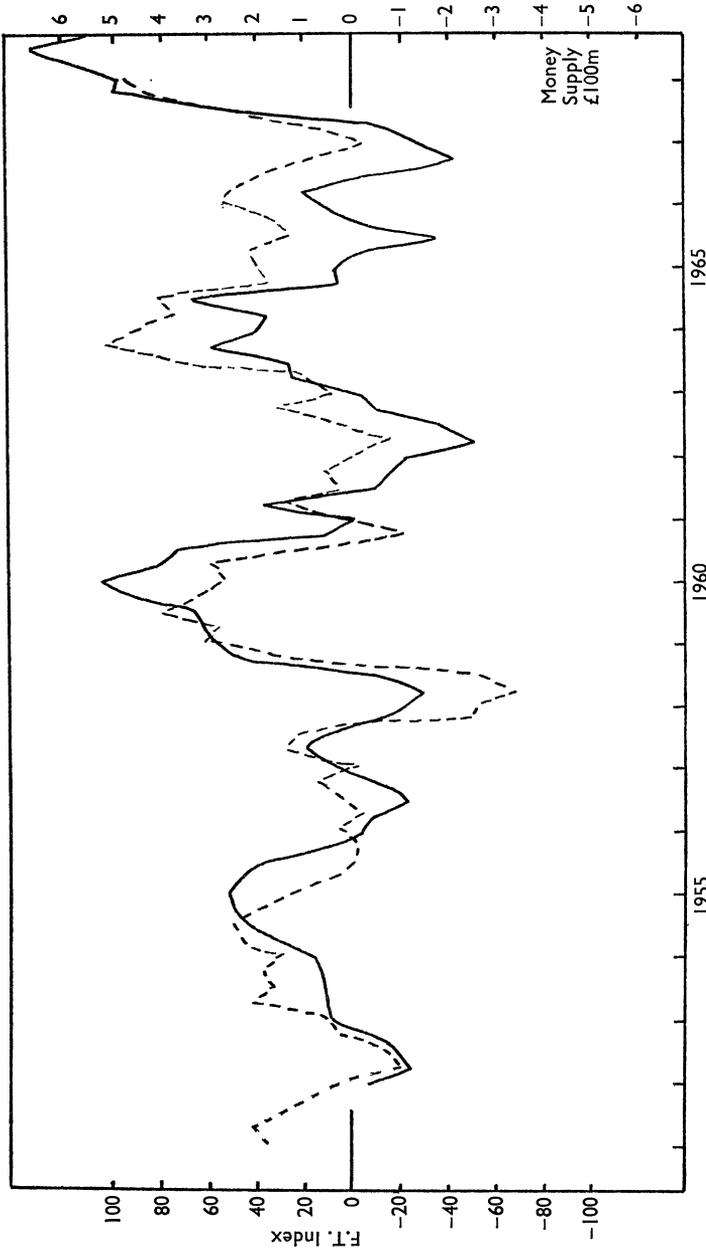


FIG. 13. ———, F.T. index (annual differences); - - - - -, U.K. money supply (annual differences).

the past ten years and, incidentally, produced forecasts nine months ago for 1969/1 which are very close to the actual levels.

12. We should like to thank Professor Bartlett for his very helpful analogy with meteorological forecasting. We entirely agree with his comments.

13. Professor Wetherill is quite right about the solution of variables in regression analysis. Optima being by nature flat, there is often a fairly wide degree of variation for the regression variables in the neighbourhood of the optimal set. A good program should print out the values in the neighbourhood to enable similarity to be gauged.

14. We would agree with Professor Wetherill about functional relationship, but used the regression because of its greater computational simplicity. Even with linear systems the estimation of functional relationship depends on assumptions about scale or sampling variance; and with lagged variation of the autoregression type there are further problems. It would be interesting to see some theoretical work on this subject.

Note: At the time of reading our paper attention was already being focused on bank advances although no reduction had yet been achieved. We would have expected, on the basis of the fits shown in Figs. 10 and 11, that a reduction in bank advances would be accompanied by a fall in equity prices and we were aware that special efforts were being made to reduce bank advances and money supply. In fact the *F.T.* index had fallen by 100 points to 423 on May 13th.

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As a result of the ballot taken during the meeting, the following were elected Fellows of the Society:

- | | |
|-----------------------------------|--------------------------------------|
| BOLDY, Duncan Peter, M.Sc. | PEACOCK, Robert Avenel, B.A. |
| EVANS, Edward Andrew, Ph.D. | PEARE, Charles William Derek, B.A. |
| FREEMAN, Stanley Roger, M.Sc. | PEPPER, Jon Vivian, M.Sc. |
| GROVE, Daniel Michael, B.Sc. | PEPPER, Michael Peter Gregory, B.Sc. |
| IMBER, Valerie, B.A. | ROBINSON, Peter Michael, B.Sc. |
| JOYCE, Peter John, B.Sc. | ROE, Peter Alan, B.Sc. |
| KANITKAR, Yechwant Gajanan, M.Sc. | SHANAHAN, Michael Francis, M.Sc. |
| KU, Hsien Hsiang, Ph.D. | SMITH, Bernard John Alan, B.A. |
| MARTIN, Keith Baillie, M.Sc. | SOLOMON, Vethamani, M.Sc. |
| ORTON, Clive Robert, B.A. | |