Patterns of Instability in Socialist Countries: Do They Call for Internationally Coordinated Stabilization Measures?

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When I settled on the above title, I was certainly overoptimistic. The answer to the question whether an internationally coordinated stabilization policy is needed in socialist countries presupposes the knowledge of (1) what kind of instabilities there exist in socialist countries, (2) how they are generated, (3) whether, and if so, how their mechanisms are linked internationally.

In fact, there is not very much known on these issues. It would therefore be too ambitious to tackle the title question. Even discussion on (3) seems premature. Answers to (1) and (2) have to be provided first. If for nothing else, they will give us an opportunity to see whether and how internal instabilities are reflected in the existing transmission models. Only very tentative suggestions will be made in other directions.

In Section I, an analysis of medium-term cycles, mainly due to fluctuations of agricultural and investment activities, is presented. Section II deals with short-run instability caused by aggregate demand in socialist countries. Finally, Section III examines price instability, more specifically interindustry movements of wage rates, in Yugoslavia.

I. Agricultural and Investment Cycles

The main sources of instability in socialist countries are agricultural and investment cycles. While agricultural fluctuations (including their international transmission) have been taken for granted as wholly exogenous (changing weather conditions), investment cycles have attracted some attention of economists.

The Czech economist J. Goldman (1964) can be credited for introducing investment cycles to the academic audience. Several other economists,

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Czech, Polish, and Yugoslav particularly, contributed to their understanding. The author of this paper published a review article on these efforts in the *Journal of Economic Literature* (1971). Thus this discussion can be brief.

For purposes of this conference research on investment cycles in Eastern (E) European socialist economies (ESE) has been extended to 1950-70. Since the degree of instability does not tell anything about the type of instability, frequencies have been estimated in addition. Results appear in Tables I and II.

Table I gives standard deviations of the yearly rates of growth for investment, construction, industrial production, agriculture and gross social product (GSP) all for 1950-70, 1950-60 and 1960-70. They measure instability of growth in ESE. I do not see sufficient reasons for accepting coefficients of variation as the measure of instability. On *a priori* grounds agricultural fluctuations are independent of growth. This also holds for GSP (high share of agriculture). Cross country rank correlations between average rates of growth and standard deviations are virtually zero (investment 0.095, and construction — 0.097) or negative (manufacturing — 0.607) for the 1950-70 and results for the two subperiods are still worse.

During the whole 20-year period instability is highest in agriculture. It is particularly high in the four south-east (SE) ESE, two to four times as high as in the north-east (NE) ESE. The difference has to do with periodic droughts in SE Europe that have not been adequately counteracted by technological and/or institutional measures. This, however, applies only to the first subperiod. In the second subperiod agricultural fluctuations weakened. Since in NE ESE they increased slightly, differences between the two groups of countries almost vanished. Yugoslavia and Romania are the only exceptions.

Investment is the second main destabilizor in socialist countries. This is surprising as one would expect that planning would try to achieve sustained growth exactly through stably growing investment. Instability of investment is the highest in SE ESE and Czechoslavakia (CSSR). The Soviet Union and Poland display the lowest amplitudes. In Germany and Yugoslavia amplitudes are about two times, and in all other countries three to four times as large as in Soviet Union and Poland. Since Soviet, Polish and German figures are for total investment, they might be biased downward.

Amplitudes of investment cycles were approximately halved in all countries from the first to the second 10-year period, the only exception being Yugoslavia where they retained the same order of magnitude. Since the amplitudes of agricultural fluctuations were further reduced, investment has become the main factor of instability in SE ESE and CSSR. Only in Soviet Union and Poland agriculture is still leading.

Construction comes third. Its fluctuations display similar amplitudes as investment, which is understandable. The main difference is that amplitudes in construction of the three NE ESE are as a rule larger and in the remaining countries substantially smaller than in investment. With CSSR

Table I

Standard deviations of the yearly rates of growth
1950-1970 (a), 1950-1960 (b), 1960-1970 (c)

	Investment			Construction			Manufacturing			Agriculture			Social Product		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Soviet Union	4.17	3.53	2.39	4.74	4.09	2.87	2.16	1.80	0.89	5.83	5.55	5.60	2.46[3]	2.05	1.74
Poland	5.34	6.63	2.63	6.09[3]	7.93	2.69	3.66	4.17	1.25	4.69	4.09	5.22	2.21[3]	2.21	1.97
Germany	8.18	8.71	4.13	9.88	12.98	3.57	4.22	4.61	0.73	n.a.	n.a.	n.a.	4.86	5.37	1.21
Czechoslavakia	10.16[1]	11.24	7.71	5.65	4.52	5.21	3.86	3.22	2.67	4.56	4.04	5.03	3.19	2.03	3.38
Romania	16.15[1]	21.41	7.03	13.04	17.13	3.82	3.70	5.05	1.40	20.60[2]	27.43	7.57	7.81[3]	10.71	2.01
Hungary	20.78[1]	26.80	11.85	9.41	12.76	3.70	6.60	8.72	2.04	10.63	14.25	4.77	6.98[3]	9.60	2.20
Yugoslavia	9.74[1]	10.10	9.28	12.30[2]	15.33	7.15	5.86	6.72	4.67	21.18[2]	28.36	7.93	7.51	10.01	3.53
Bulgaria	12.56[1]	16.67	6.05	7.21	8.49	5.04	3.19	3.45	1.55	12.50	16.45	5.87	9.24[3]	12.55	2.39

Notes: [1]Industrial fixed investment (instead of total)
[2]Gross social product — upward biased amplitude
[3]Net social product

as the only exception, amplitudes are more than halved from the first to the second subperiod.

Fluctuations in GSP are higher than in manufacturing and much lower than in other aggregates. GSP of the three NE countries grows more stably than elsewhere. In the four SE ESE with a large share of agriculture in the GSP, agricultural cycles are a major determinant of instability in the growth of GSP. In the second subperiod the leading role is left to investment, and in Soviet Union and Poland to agriculture.

Although there is no need to define investment cycles as upcross cycles, we adopted this view for two reasons: First, one needs a measure of the frequency of investment cycles. They might be just year-to-year fluctuations. Second, as with Kuznets' cycles (Howrey, 1968; Klotz, 1973), the existence of investment cycles has been questioned. B. Klotz, 1971, points out that investment cycles might be a statistical artifact, produced by three-year moving averages used in our graphical description (Bajt, 1971). He has found that even with zero first-order autocorrelation of residuals of a regression of logs of a certain macro-aggregate on a linear time trend three-year moving averages produce 7.5 year "upcross" cycles, and that data for western countries favor his point. It will be shown that Klotz' fear has not been justified. Autocorrelation is rather high. The only exception is agriculture (and because of it GSP) in the four SE ESE.

Our results are presented in Table II. First order autocorrelation coefficients (with t-values in parentheses) appear as the first two figures, and the corresponding upcross periods in years are added as the third figure for each country. While in SE ESE the length of upcross cycles in agriculture is much below the critical value (7.5 years), including NE ESE agriculture, the length of upcross cycles in investment is (arithmetic mean) 14.29, in construction 11.05, and in manufacturing 14.34 years (GSP cycles are shorter because of agriculture). This corroborates our earlier conclusion that investment cycles are about 8 years in length. Namely, in a sample of 20 the true cycles are about 55 percent of upcross cycles.

Several economists proposed their explanation of investment cycles. J. Goldman explains them by periodically launched "investment drives" in central five-year plans. Socialist development strategy is industrialization of the "predominant role of the producer goods production" type, that is a strategy of rapidly growing producer goods industries and lagging consumer goods production. Designated to catch up with western economies, the extremely high investment, particularly in iron and steel, mining, power generation and heavy chemistry (all these branches are included in "manufacturing" in our tables) produced in a few years all kinds of bottlenecks like raw materials, foreign exchange, labor force, consumption and food production (Goldman and Fleck, 1967). In my view the last two bottlenecks that generated popular discontent (1956 events in Poland and Hungary) should be regarded as the decisive ones. As a result planners periodically had to revise their plans by giving temporary priority to consumer goods production (Lange, 1964). Thus, exaggerated investments, followed by a negative response of the population, seem to be the main

Table II

Upcross period (P) of yearly rates of growth (in years)
(with first order auto-correlation (C) of residuals
from the regression of the yearly rates of growth on a linear time trend

	Investment			Construction			Manufacturing			Agriculture			Social Product		
	C	(t)	P	C	(t)	P	C	(t)	P	C	(t)	P	C	(t)	P
Soviet Union	0.878	(9.4)	22.3	0.473	(2.4)	10.7	0.822	(9.7)	18.3	0.624	(3.5)	12.3	0.819[3]	(8.8)	18.2
Poland	0.762	(9.6)	15.2	0.371[3]	(2.6)	9.8	0.673	(7.5)	13.5	0.413	(4.2)	10.1	0.775[3]	(6.4)	16.3
Germany	0.677	(5.8)	13.6	0.267	(1.4)	9.0	0.660	(7.2)	13.3	n.a.	n.a.	n.a.	0.638	(7.2)	12.9
Czechoslavakia	0.717[1]	(4.4)	14.6	0.735	(5.6)	15.1	0.791	(7.2)	16.9	0.437	(2.1)	10.4	0.756	(6.3)	15.7
Romania	0.455[1]	(2.9)	10.5	0.198	(1.1)	8.6	0.748	(4.3)	15.4	-0.500[2]	(2.8)	4.8	-0.287[3]	(1.7)	5.9
Hungary	0.534[1]	(2.8)	11.4	0.589	(3.1)	12.2	0.395	(2.3)	10.0	-0.336	(1.5)	5.7	0.097[3]	(0.4)	8.0
Yugoslavia	0.642[1]	(3.9)	13.1	0.581[2]	(4.1)	12.0	0.531	(3.9)	11.4	-0.458[2]	(2.4)	4.2	0.161	(0.8)	8.4
Bulgaria	0.676[1]	(4.0)	13.6	0.499	(2.6)	11.0	0.763	(4.9)	15.9	-0.250	(1.2)	6.1	-0.091[3]	(0.5)	7.1

Notes: [1]Industrial fixed investment (instead of total)
[2]Gross social product
[3]Net social product

forces that generated what is labeled investment cycles. Factors like farmers' reluctance to accept collectivization and forced deliveries, misfortunes with weather conditions, unexpected political implications of lagging consumption, might have helped. The facts that outside of agriculture investment cycles are most violent in investment which is the main tool of planners, that they are synchronized among countries (medium-term plans roughly coincide), that investment peaks mainly fall in the odd numbered while investment troughs in the even numbered five-year periods (counted from the first non-Soviet plans on), that fluctuations are the mildest in the Soviet Union (with largest planning experience), that they decreased from the first to the second decade (increased experience of planners) seem to favor our interpretation.

The purpose of what follows is to look for some quantitative arguments in favor of our interpretation. The capacity effect of investment will be studied by looking at the growth of industrial production and construction, GSP may not prove useful because of its agricultural component. Depressing effects on consumption will be studied by looking at the growth of agriculture. Consumer goods production could be another possibility. Yet, there are no reliable data at hand. Moreover, at the prevailing levels of living, particularly in the first postwar period, consumer reactivity to agricultural supply must have been particularly high. Table III summarizes our results. Simple correlation coefficients between rates of growth of investment and rates of growth of construction, industrial production, agriculture, and GSP, lagged as indicated in the first row, appear in the second row of the first column for each aggregate and country. Lags are the highest correlation lags. Since all these aggregates may, and with yearly data as a rule do, accelerate and decelerate simultaneously with investment, their simultaneity being a result of the accelerating and decelerating general activity, zero lag correlations do not tell very much. Simultaneous correlation is therefore eliminated and residuals of the rates of growth of construction, industrial production, agriculture, and GSP, lagged up to three years in each direction, with residuals of the rate of growth of investment correlated instead. The obtained coefficients $r_{Y_t X_{t-i} \,.\, X_t}$ are labeled partial lag-correlation coefficients. They are given for the best-fit lag in each direction (indicated in the first row) in the second row of the second and third column of each aggregate. Low coefficients are omitted. Parentheses show some supplementary, and brackets some substitutional, results of correlation with lagged investment residuals $r_{X_t Y_{t-i} \,.\, Y_t}$ — resulting in slightly different samples).

[Editors' note: Professor Portes in his comment below describes the author's procedure in more detail. The author first regresses the rate of growth of construction, $CONSTR_t$, for example, on the rate of growth of investment, INV_t, and obtains a series of residuals, $(e^C{}_t)$. He then regresses $INV_{t+\tau}$ on INV_t separately for $\tau = -3,\ldots, +3$, and obtains a series of residuals, $(e^I{}_{t+\tau}), \tau = -3,\ldots, +3$. Finally, he regresses $(e^C{}_t)$ on $(e^I{}_{t+\tau})$ for each value of τ and looks for the best fit. The end result of Professor Bajt's

Table III

Highest simple and partial correlation coefficients of the rate of growth of fixed investment with rates of growth of construction, industrial production, agriculture and GSP

	Construction			Industrial Production			Agriculture			GSP			
	-1	-1	1	-1	-3		0	3	-1	0	3	-3	
Soviet Union	0.87	0.69	0.40	0.73	0.71		0.52	0.36	-0.42	0.76	0.60	0.38	
	0	(-3)	3	1	-2		1	1	-3	0	(2)	-3	
Poland	0.68	(0.30)	0.64	0.24	0.25		0.36	0.47	-0.40	-0.38	(0.65)	-0.37	
	0	-3		0	-3	(3)				0	[1]	3	(-3)
Germany	0.64	0.55		0.57	0.64	(0.72)		n.a.		0.66	[0.73]	0.73	0.27
	0	(-1)	(2)	0	-1		3	3	-3	0		1	-1 [-1]
Czechoslovakia	0.81	(0.17)	(0.19)	0.63	0.61		0.31	0.24	-0.55	0.45	0.23	0.19	[0.36]
	0	(-1)	3	0	-2	(1)	1	1	-2	1	1	-1	
Romania	0.15	(0.33)	0.39	0.81	0.53	(0.54)	0.51	0.67	-0.43	0.52	0.54	0.22	
	-1	-1	1	-2	-2	(1)	1	1	-2	1	1	-1	
Hungary	0.24	0.57	0.40	(0.61)	(0.60)	(0.50)	0.57	0.62	-0.42	0.61	0.61	0.34	
	0	-3		0	-3		0	(1)	1	0	3	-3	
Yugoslavia	0.69	0.66		0.54	0.33		0.31	(0.21)	-0.36	0.54	0.18	0.17	
	0	-1	(3)	1	2		0	2	-1	0	2	(-2)	
Bulgaria	0.82	0.48	(0.36)	0.44	0.55		0.44	0.43	-0.48	0.72	0.44	(-0.71)	

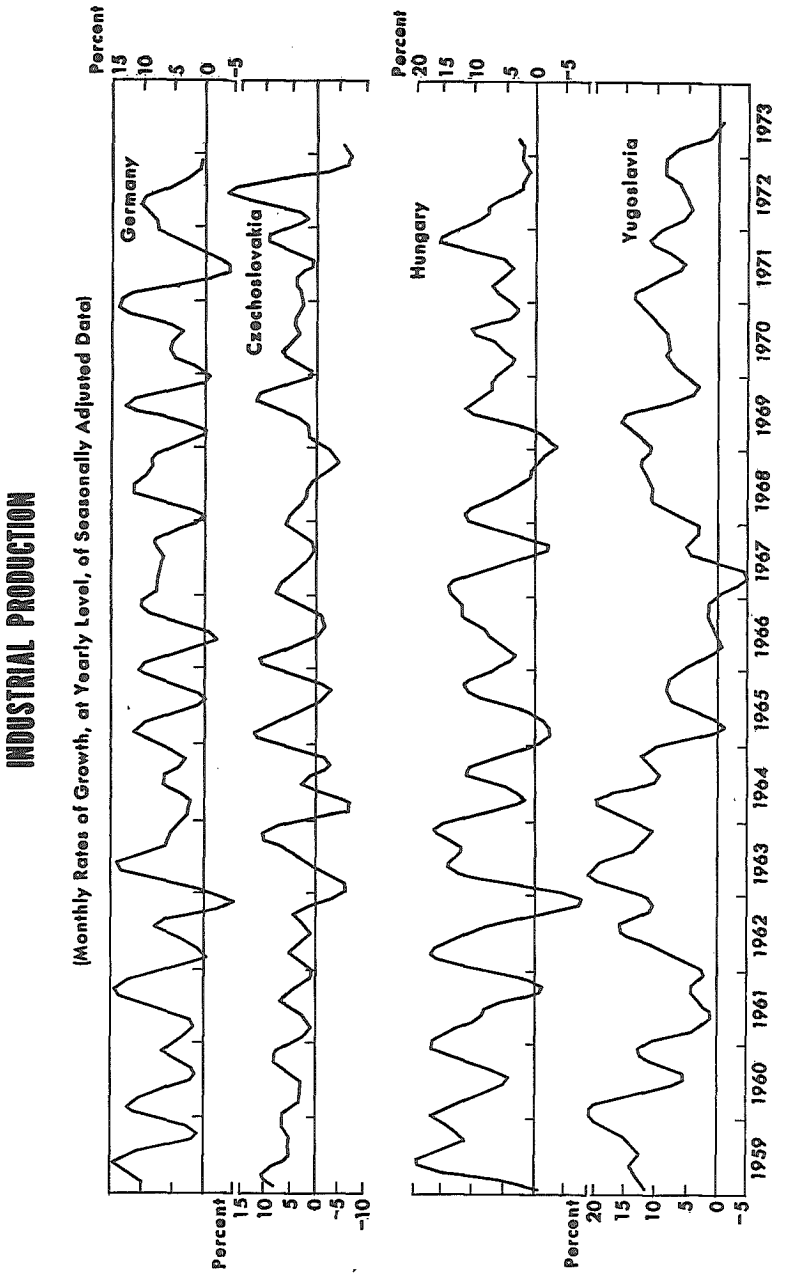
calculations is an approximation to the partial correlation coefficient $r_{Y_t X_{t-i} \cdot X_t}$ (where $Y_t = CONSTR_t$, $X_t = INV_t$) referred to in the text.]

Partial lag-correlations (also simple lag-correlation) indicate the direction of impulses between the corresponding variables, together with their strength and time distance. Impulses in either of the two directions usually exist within any zero simple lag-correlation. Partial lag-correlation allows a rudimentary splitting of statistical variables into individual impulses and contributes to the correct specification.

Results for construction and industrial production had to be expected. The main reason is that investment is mainly exogenous, determined by plans. Pulsation from investment to industrial production is most pronounced in the Soviet Union and Hungary. Partial impulses in the same direction exist in all countries but Bulgaria. The lag between one and three years roughly agrees with the empirically known gestation periods. Impulses in the opposite direction, from production to investment, are detected only in four instances. If impulse from investment to production is identified as capacity effect and the one in the opposite direction as accelerator effect, then it is possible to state that the capacity effect of investment is much stronger than the accelerator effect of production. This is consistent with the planned character of socialist economies. Moreover, it substantiates our belief that investment is mainly exogenous.

It is plausible to assume that a large part of pulsation from investment to construction is covered by zero lag simple correlation. As expected, correlation coefficients are as a rule higher than for industrial production and not a single one is for positive lags (these would indicate impulses from construction to investment). Soviet Union and Hungary manifest particularly strong capacity effects. Partial impulses also are mainly of the capacity type. The Polish and Rumanian exceptions are weakened by the alternative correlations of lagged investment residuals. From the remaining four cases the accelerator effect comes from the same alternative correlation in two of them.

Results for agriculture are the most relevant for our interpretation of investment cycles. Pulsation from investment to industrial production and construction merely describes the transmission of cycles from investment to other aggregates. In view of these results agriculture may not be as exogenous as it is generally believed, even not in short periods of time. Simple correlation coefficients, indicating positive pulsation from agriculture to investment in no less than four instances, might be dismissed on *a priori* grounds. It is difficult to believe that such impulses could exist in centrally planned economies. However, partial lag-correlations are too much in unison. In all countries but Yugoslavia positive impulses from agriculture to investment are at work. That is, an acceleration of agricultural growth favors acceleration of investment and vice versa. We may recall that it has been argued that with a different agricultural policy in ESE a large part of industrialization could have been financed by exporting agricultural produce. Balance-of-payments difficulties usually stem from agricultural failures.



However, the really important result is the uniformly negative partial lag-correlation coefficients for negative lags. In other words, a negative pulsation runs from investment to agriculture in all ESE. Not only does investment not foster, it actually hurts agricultural production. It is not possible to trace the chain of causation from investment to agriculture with the data at hand. Reallocation of population from agriculture to industry, forced deliveries, increased taxes, the general political attitude towards farmers, forced collectivization, impaired farmers' expectations and similar factors may be links in it.

Welcome as the above results may be, a large number of questions remain. For yearly data, not always for the same aggregates, not checked for any possible kind of errors, exposed to spurious correlation, they are too good a performance. The method itself needs careful examination, particularly in its application to yearly data. Nonetheless, the results reinforce the belief that by pushing investment beyond its optimum rate planners have been the main generator of cycles.

Simple and partial lag-correlation method was also used to see whether investment cycles were transmitted internationally (within ESE). No evidence has been found to confirm this hypothesis. The established coincidence of investment cycles most likely comes from roughly coincident medium-term plans and from coincident agricultural setbacks.

II. Demand-generated Instability in Planned Economies?

We now part with yearly data. We are left with Germany, Czechoslavakia, Hungary and Yugoslavia, and with industrial production only.

Figure 1 reproduces monthly rates of growth of industrial production (yearly level), seasonally adjusted (with random and trading day disturbances eliminated), for the four socialist economies, 1959-1973. Graphs display considerable degree of instability (industrial production can be taken as being representative of general economic activity). While average rates of growth 1960 through 1970 are 5.71, 6.43, 6.59 and 8.04 percent (yearly level) for Germany, Czeckoslavakia, Hungary and Yugoslavia, respectively, their standard deviations are 3.79, 9.10, 5.74 and 5.54. Since standard deviations in yearly rates amount to 0.73, 2.67, 2.04 and 4.67 (Table I, period 1960-70), respectively, we have an indication that fluctuations in the first three ESE may be of a predominantly seasonal character.

This appears to be confirmed by our graphs. Almost regular yearly subcycles occur in all of the four series of data despite seasonal adjustment (Method II, X-11, Bureau of the Census). These might be partly the result of a Slutskyan process. Only Yugoslav manufacturing displays undisputable cyclical behavior.

Spectral analysis (data detrended by taking rates of growth from seasonally unadjusted absolute data) confirms this. In the seasonal band of frequencies 10-month cycles persist in Yugoslavia and Germany, and 15-month cycles in the two remaining ESE. The existence of the Yugoslav

10-month cycles is supported by weak remnants of their 3.3-month harmonic. German data display some remnants of 15-month, and Czechoslavakian and Hungarian data of about 6 and 9-month cycles, respectively. All three, however, may be multiples of three-month cycles, found in seasonally unadjusted data. As shown by Nerlove, 1964, these might come from trading-day composition.

Spectral analysis of seasonally adjusted data shows significant (Howrey's criterion, 1968) 90-month cycles in Yugoslavia. I dismiss this possibility on ground of too short time series (Granger and Hatanaka, 1964). The graph indicates that short cycles are shorter. Upcross cycles for seasonally adjusted monthly rates of growth are 45 months long (autocorrelation of residuals 0.972) and upcross cycles for monthly rates of growth of seasonally adjusted indices 21.3 months long (autocorrelation 0.869). While the latter are seasonal, the frequency of the former is cyclical. Spectral analysis of seasonally adjusted data does not reveal any of these cycles. For the other three ESE spectral analysis does not reveal any cycles in the cyclical band of frequencies either for adjusted or for unadjusted data, and visual inspection of graphs does not reveal them either. Upcross cycles for monthly rates of growth of seasonally adjusted data are of about 12 months long in all three ESE. In any case, if short cycles exist, they must be rather weak.

Since our time series are too short for short cycles to be ascertained by spectral analysis, we must rely on visual inspection and on 45-month upcross cycles. We thus believe that short cycles exist in Yugoslavia but that there is no evidence of their existence in the other three ESE.

If this is so, short cycles may have to do with the structure of the decision-making process, the Yugoslav being highly decentralized. Central planning seems able to cope successfully with problems of stable growth. In a rather short time it not only weakened investment cycles (in fact, in manufacturing it decreased them below the capitalist average) but prevented short cycles from turning up. Unless ESE embark on decentralization, their nonagricultural oscillations are likely to become very mild.

The fact that Yugoslav economic growth was highly unstable has been known for quite some time. The two decelerations in 1961 and 1965-67 produced severe political problems (unemployment, insolvency, lagging wages). They both were aggravated by economic reforms that introduced tight money policy.

To show exactly how decentralization leads to instability, a forecasting model based on autonomous expenditures is presented. Induced consumption is usually regarded as a function of disposable income, and induced investment as a function of the increase in consumption. They may, therefore, be estimated from adequate consumption and investment functions. Total consumption and investment being known, autonomous consumption and investment are obtained as the difference between total and induced consumption and investment. This difference equals residuals which are (seasonally adjusted monthly data) highly serially correlated. Of

course, these residuals do not say anything as to the absolute level of autonomous consumption and investment. Fortunately this is not really needed. What is needed is autonomous consumption and investment impulses, and positively correlated residuals seem to serve this purpose quite well.

Government expenditure is treated similarly. With a tax system that rests heavily on personal incomes and with about 6,000 independent federal, republican, and local budgets, government revenues are approximated by personal disposable incomes and their autonomous part estimated within a consumption function.

The model, estimated for 1960-73 (OLS), is as follows:

$$\underset{(-5.1)\quad(214.8)}{CPD_t = -119.85 + 0.879\ Y_t} \tag{1}$$

$$\bar{R}^2 = 0.996, \quad T = 49 - 214$$

$$CPA = CP/CPD - 1 \tag{2}$$

$$\underset{(18.1)\quad(33.1)}{ID_t = 900.67 + 1.610(CP_{t-1} - CP_{t-13})} \tag{3}$$

$$\bar{R}^2 = 0.87, \quad T = 49 - 210$$

$$IA = ID/I - 1 \tag{4}$$

$$\underset{(7.4)\quad(47)}{GD_t = 146.4 + 0.171\ Y_t} \tag{5}$$

$$\bar{R}^2 = 0.93, \quad T = 49 - 210$$

$$GA = GD/G - 1 \tag{6}$$

$$\underset{(8.3)\ (9.7)\qquad(5.8)\qquad(6.3)\qquad(2.5)}{rQA = 10.9 + 55.9\ CPA_{t-9} + 11.1\ IA_{t-7} + 10.1\ GA_{t-5} - 0.023\ T} \tag{7}$$

$$\bar{R}^2 = 0.57, \quad T = 58 - 214$$

All nominal variables (CP = expenditures of the population on goods, I = investment expenditures for fixed and circulating capital, G = government expenditures — federal, republican, and local, D = derived — induced, A = autonomous, Y = disposable personal incomes, QA = economic activity measured by the index of industrial production, 100 = ∅ 1972, and using autonomous expenditures as independent variables, T = linear time trend with T = 1 = January 1956 — 49 = January 1960, r = monthly rate of growth at the annual level) are in millions of current dinars. The period from 1960 onwards was chosen as 1960 is a benchmark in

the development of selfmanagement type of the decision-making decentralization. Equation (7) shows a very good fit (consumption function (1), estimated for monthly rates of growth, has R^2 equal to 0.37). The variable CPA, lagged for nine months, is the decisive one. It determines the general course of the rate of growth of economic activity.

Lags in (7) seem to be acceptable. The differences, particularly between lags of CPA and GA, embrace unknown transmission mechanisms from retail trade to raw materials and capital equipment production. Wholesale trade, consumer goods production, inventories of finished goods both in retail and wholesale trade, and in industry, different calendar lengths of Marshall's short and long-period reacting of production to demand impulses may be links in it. All lags, if looked at as distributed (e.g., Almon polynomial lags), are unimodal with the highest and most significant coefficient estimates as specified and with virtually zero simultaneous correlation.

Forecasting records of the model and its variable CPA in particular are very good. For instance, ex post forecasts produce negative rates of growth in 1967, exactly when they occurred in fact. Ex ante unconditional forecasts have been published since 1968, with great success. Thus the model is considered as a proof that medium-term instability is really demand originated and that demand of the population is the prime mover in the process.

For stabilization purposes the "autonomy" of expenditures, consumer expenditures in the first place, though irrelevant for forecasting, is of greatest interest. If these expenditures were autonomous, counteracting policy would be necessary; if dependent on some other variable, especially an economic policy variable, stabilization efforts could also act through influencing that particular variable.

The answer was tried in two directions. Dependence on various monetary variables was tried first — without success. In particular, consumer credits turned out to be counteracted by population by savings account deposits. On the average, consumer credit expansions are followed by expansions of savings account deposits. Their main influence is on the structure of expenditures.

Dependence on the structure of the economy, that is the interplay of behavior parameters within a structural macroeconomic model, was tried next. Suppose that the basic structure of the economic mechanism is represented by a three-equation structural recursive model à la Samuelson's accelerator-multiplier model of 1939 (Samuelson, 1939) of the following form:

$$\underset{(-19)\quad(88.8)\qquad(7.2)}{GPSGD_t = -0.627 + 0.681\ GSP_{t-1} + 0.331\ (CPSG_{t-1} - CPSG_{t-13})}$$

(1)

$$\bar{R}^2 = 0.998, \quad T = 74 \text{ to } 212, \quad D.W. = 0.10$$

$$ID_t = 1208.63 + 0.799\ (GSP_{t-1} - GSP_{t-13}) \quad (2)$$
$$(30.3)\quad (43.3)$$
$$\bar{R}^2 = 0.937, \quad T = 86 \text{ to } 212, \quad D.W. = 0.07$$

$$GSP_t = CPSGD_t + ID_t + A_t \quad (3)$$

(CPSG = expenditures of population and government on goods and services, GSP = domestic gross social product, I = total investment, A = total autonomous expenditures, D.W. = Durbin-Watson statistic).

All variables are in millions of current dinars. The variable $CPSG_{t-1} - CPSG_{t-13}$ is added to the consumption function in order to account for the empirical fact that CPSG develops ahead of GSP in the predominant part of the estimation period (the best-fit consumption function being thus a function that regresses consumer expenditures on future income). Inclusion of a consumption accelerator is understandable since the economy does not spend CPSG only on non-durables but on durables as well (there are no adequate data on these categories of goods separately). This agrees with Hamburger's finding that expenditures for consumer durables should be treated as investment (1967). CPSG is included instead of disposable personal income only to keep the number of variables low. Cycles in GSP are very similar to those in industrial production forecast by our first model.

For purposes of simulation total autonomous expenditures were defined similarly as in our forecasting model, more specifically as the difference between GSP and induced consumption and investment expenditure,

$$A = (GSP - CPSGD - ID)$$

but included in the system with their linear time trend values (after a slight *ad hoc* correction of the direction of the trend):

$$A_t = (GSP_t - CPSGD_t - ID_t) = -1372 + 16.119T \quad (4)$$
$$(-10.5)\ (18.9)$$
$$\bar{R}^2 = 0.74, \quad T = 86 \text{ to } 212, \quad D.W. = 0.06$$

Simulation experiments with only A exogenous and T = 86 to 98 as the initial values of endogenous variables (with GSP adjusted to stable growth within T = 86 to 98) lead to the following set of conclusions:[1]

1. The consumption function (1) produces with its accelerator term oscillations of consumption ahead of income. It therefore explains the fact observed in the Yugoslav economy (and elsewhere) that the best-fit consumption function is the one that regresses current consumption on future income. The performance of (1) is particularly satisfactory since without

[1]Characteristic roots of the implied difference equations were not computed. The particular part of the solution seems to be particularly cumbersome.

$(CPSG_{t-1} - CPSG_{t-13})$ values of CPSGD, estimated on GSP_{t-1} alone, would systematically lag behind GSP for two or more months. However, $(CPSG_{t-1} - CPSG_{t-13})$ does not explain all oscillations of consumption that are independent of GSP_{t-1}, as clearly shown by the low D.W. of (1).

2. The model produces cycles in GSP, CPSG and I of a period of about 11 to 12 months. Cycles of a similar period really exist in Yugoslav economic activity. However, simulated amplitudes are much milder than in reality. Even without this evidence it is hard to believe that simulated cycles correspond to reality. The reason for our distrust is that even extremely small departures of GSP or A from the determined paths cause the model to explode in a few months. Since obviously only divine forces could keep real initial values of GSP and real course of A on the determined paths, our model suggests that the structure is highly explosive. Moreover, simulation relies on stable point estimates and disregards both other values within confidence intervals and the possibility of changing structural parameters — all rather restrictive and unrealistic assumptions (Wishwakarma, 1974).

3. We were able to simulate the course of GSP, CPSG and I by linear increases in A only for about 30 months. Simulation over a longer period calls for (repeated) adjustments of the increase in A. Since this breaks linearity, thus possibly introducing cycles, we did not pursue this course. Towards the end of the 30-month period simulation seriously underestimates real GSP values. Any attempts to approach them apparently puts GSP in the acceleration part of the cycle (in the sense of short cycles) and leads to explosion. Very likely, short cycles are not implied in the estimated parameters.

Summarizing, partly at least "autonomous" expenditures are not autonomous. Stabilization policy most likely should not confine itself to direct interventions (investment and government expenditures) but try to control incomes of the economy as well.

III. Price Instability Generation

In discussing inflation western scholars usually point at phenomena like queues in front of stores, poor retail trade assortment, deficient quality of goods, high farm prices and second-hand market prices, and similar factors. These, in my view, are rather marginal phenomena. Much more relevant is the fact that retail prices are stable over long periods and that not infrequently they are decreased. The mechanism of the economy allows the central planner to set prices at any level he wishes. Spillovers of demand to free markets are explained by deficiencies in planning. So are unsold surpluses, frequent in foreign trade sectors. Compared with western economies, EESE may be regarded as essentially inflation-proof.

The only real exception is Yugoslavia. From the last economic reform (1968) on, Hungarian prices also have been slipping from their planned course (the case of Czechoslovakia was similar in 1967 and thereafter — Janda, 1970) but it is far too early to contend that they escaped the control of planners, (Zala, 1973). In 1973 the rate of inflation reached slightly

over 3 percent. Only in Yugoslavia inflation (20 percent in 1973) has grown into an unmanageable process that continuously presses the government into a trade-off between stability and growth thus decreasing growth below its potential.

In what follows I shall concentrate on a specific mechanism of inflation, that is wage inflation. This is justified on two grounds. First, wage inflation has been a major gear in price instability in all modern market economies which are characterized by their inability to use monetary and fiscal policy both ways. Second, being essentially a redistribution process, wage inflation mechanism is a prototype of many modern inflation processes, international crude-oil inflation included.

We shall first hypothesize the wage inflation mechanism (1) and subsequently try to test it by investigating the behavior of the interindustry wage structure.

(1) Yugoslav enterprises are independent in their wage policy. Their incomes set the upper limit to wages paid, and minimum wages set the lower limit. Because of differences in capital per worker, productivity, natural resources (skills and intensity of labor are assumed equal) and state intervention, incomes per worker are different in different enterprises. Political power may be added for services (administration, health, instruction, research etc.). Though high-income enterprises invest more per worker, their wages also are higher. This results in inequality of wages between enterprises.

While workers cannot be fired under selfmanagement, they have the right to quit their jobs any time. Since getting a new job is not always easy, pressures for wage increases are more commonly and effectively exerted by decreasing labor efforts or by using political influence. Anyhow, low-wage enterprises usually find ways and means by which to adjust their wages either without changing prices (lower investment per worker, decreased taxes) or by increasing prices (administratively increased price ceilings, increased prices in imperfect markets).

The wage inflation proceeds therefore as follows. High incomes that flow out of strong market (production) or political (services) monopoly power result in high wages. Low-wage enterprises, trying to catch up, necessarily exert an upward pressure on prices that are much less flexible than prices of high-income enterprises (price ceilings, stronger competition). Thus wage inflation is hypothesized as a process starting with a run ahead (demand pull) and finishing with a catch up (cost push) phase.

Despite this process wages never get equalized. The ranking of production processes according to their relative wages remains fairly stable over long periods of time, no matter how they are grouped (two- or three-digit industries, regionally, according to capital intensity, monopoly power). The explanation seems obvious. Increased wages of low-wage industries (together with deficit financing of other categories of final demand) allow, via increased aggregate demand, incessant price and wage increases of high-income enterprises thus making the run ahead (demand pull) a permanent process.

For this reason testing of the described process is likely a difficult task. Whereas it would be easy to trace it by studying a limited number of carefully selected, some low- and some high-income enterprises, wage increases in low- and high-income enterprises of the whole economy are most likely hopelessly intermingled. Cyclical growth may serve as an uncommissioned help. Changing business conditions have discriminatory effects on high- and low-income enterprises. In periods of slow growth business conditions deteriorate for all enterprises but, because of more perfect competition, they deteriorate more for low-income enterprises. For this reason, and as their price-wage margin is narrow anyhow, their wages grow at lower rates (if at all) than wages of high-income enterprises. Conversely, in periods of fast growth, business conditions improve for all, but relatively more for low-income enterprises. Thus they have an opportunity of catching up with high-income enterprises. This time, wages of low-income enterprises grow at higher rates than wages of high-income enterprises.

Three testable hypotheses are implied in the above dynamized run ahead (demand pull) — catch up (cost push) mechanism:

a) Interenterprise wage differentials are neither constant nor do they develop smoothly over time. Rather, they oscillate;
b) Wage differentials oscillate together with business conditions;
c) The correlation is negative between the two, that is, wages in low-income enterprises grow faster in improved, and slower in impaired, business conditions.

(2) Although every industry comprises enterprises with both low- and high-incomes per worker, some are predominantly high-income and some predominantly low-income industries. With monthly data published for three-digit industries, both in production and services, interenterprise structure will be approached through interindustry structure. Any other criterion of grouping, more promising in principle, would require data on individual enterprises.

Our sample period will be January 1964 to April 1970, the period from the beginning of the published time series to the introduction of state interventions in wage formation that transformed (rather than crippled) the predominantly interindustry mechanism into an interregional one. In that period, 1964 to 1965 and 1968 to 1969 were years of fast, and 1966 to 1967, with the end of 1969, years of slow growth. The longest and most pronounced growth cycle is included. That is most important. It is very likely that with short and mild oscillation of growth the proposed test would not have yielded significant estimates.

In order to test (a), coefficients of variance of average industrial wages were computed for each month of the period according to the formula:

$$KV_w = \frac{\Sigma_1^N (w_i - W)^2}{NW}$$

where KV_w = coefficient of variance of average industrial wages, w_i = monthly average wage in the i-th industry, W = monthly average wage of the whole economy, and N = number of industries (54). Yearly rates of change of five-month moving averages (used to decrease the erratic movement of KV_w) of these KV_w are given in Table IV. Cyclical behavior is beyond doubt. Regressed on a linear time trend the yearly rates of change of KV_w give a D.W. value of 0.12, confirming cyclicality. Yearly rates of change of KV_w are used to minimize the risk of introducing seasonal cycles.

Table IV

Yearly rates of change of KV_w

(five-months moving averages)

Month \ Year	1965	1966	1967	1968	1969	1970
I	-4.7	-2.5	8.1	4.2	-3.9	-0.1
II	-2.7	-3.6	8.6	3.5	-2.7	-1.4
III	-1.2	-3.8	9.7	0.8	-1.6	
IV	-1.3	-6.4	10.9	1.3	1.0	
V	-0.4	-7.1	10.4	0.5	0.9	
VI	3.0	-7.5	10.1	-0.9	1.8	
VII	0.0	-6.5	12.1	-0.3	-0.5	
VIII	-0.7	-3.2	10.2	0.5	0.3	
IX	-0.7	-0.6	8.1	-1.8	-0.7	
X	-2.4	0.4	8.5	-1.8	-1.7	
XI	-3.9	3.8	7.0	-2.3	-1.6	
XII	-2.2	6.7	3.2	-1.8	1.6	

In order to test (b), we will regress yearly coefficients of growth of KV_w on yearly rates of growth of three growth variables (plus a time trend): industrial production and unemployment (output and input proxies for real growth — unemployment is used instead of employment to avoid collinearity with industrial production), and retail prices (proxy for nominal growth). Results confirm (b), that is, interindustry wage differentials are really correlated with growth. The best-fit equation is for lags that are indicated (in months) behind variables:

$$kKV_w = \underset{(30)}{123.4} - \underset{(18.8)}{103.3}\, sQ_{t+3} - \underset{(11.7)}{42.7}\, sP_{t-5} + \underset{(5.4)}{32}\, sU_{t-9} - \underset{(3.1)}{0.9}\, T$$

$$\bar{R}^2 = 0.89, \quad F = 124, \quad D.W. = 0.90$$

where Q = index of industrial production, P = retail price index, U = unemployed workers, t = linear time trend, k = yearly coefficient, and s = yearly rate of growth. The lag +3 of sQ together with the lag -5 of sP probably indicate (with their mean lag of about -1) that kKV_w are dependent on business conditions rather than real growth as such. The difference between their mean and the lag of sU (together with their t-values) shows that reactions of kKV_w to product markets are prompter than to labor markets.

Signs of regression coefficients in the kKV_w equation also corroborate (c). This essentially is Wachter's test, (Wachter, 1970.) A new test, intended to show the time path of the wage inflation mechanism, has been designed as follows. In an exponential function, regression coefficients measure the elasticity of the dependent variable with respect to independent ones. Therefore, if interindustry wage structure in the month t is regressed on the wage structure in the month t-12 (yearly rates are chosen to smooth out increases) according to the formula

$$w_t = a\, w_{t-12}^{b},$$

which is linearized to

$$\ln w_t = \ln a + b \ln w_{t-12}$$

where w is a cross-section series of average wages of all industries, b coefficient should have a value greater than 1 in periods of stretching and a value smaller than 1 in periods of shrinking interindustry wage differentials. That is, during periods of impaired business conditions, b should be higher, and during periods of improved conditions lower than 1.

Two qualifications are needed. First, errors of measurement in the independent variable w_{t-12} bias b coefficient towards 0 (Johnston, 1963, p. 148 ss). Second, variable w_{t-12} that gradually shifts along different phases of the cycle thus intermingling slow and fast growth of low- and high-wage industries, biases b coefficient towards 1 and decreases its oscillations over time. It is difficult to say what might be the joint result of these two biases. A rough solution is to take instead of 1 the average value of b over the whole period as the criterion; as for amplitudes, no such simple rule can be applied.

Result of regressing average industrial wages w_t on w_{t-12} according to the above formula, for all successive months from January 1965 to April 1970, appear in Table V. All symbols are known from before. Constant terms that are not significant at 99 percent level are marked with asterisks.

In my view, the results are remarkably good, even if one forgets that the critical value of b coefficient is around 0.9427. If time series of b is regressed on monthly rates of growth of economic activity (which is a poor substitute for business conditions) and on a time trend (that indicates linear shifts of b value over the whole period) the result is:

$$b = \underset{(12)}{0.82} - \underset{(4.2)}{9.06}\, rQ_{t-3} + \underset{(2.3)}{0.001}\, T$$

$$\bar{R}^2 = 0.23, \quad D.W. = 1.71$$

This is not exactly excellent but still satisfactory in view of the many factors that influence wage increases of individual industries and particularly of the fact that data for industries are obtained by quite arbitrary aggregation of those for individual processes.

Table V

Regressions of ln w_t on ln w_{t-12}

Year and month	ln a(t)	b(t)	R^2	D.W.
1965/I	0.62(3.0)	0.94(27)	0.93	1.37
II	1.0 (3.7)	0.88(18)	0.86	2.12
III	0.91(3.5)	0.90(21)	0.89	1.48
IV	0.43(1.4)x	0.98(19)	0.88	2.09
V	0.72(2.4)x	0.93(19)	0.87	1.38
VI	0.48(1.8)x	0.97(21)	0.90	1.74
VII	0.7 (2.2)x	0.93(18)	0.86	1.24
VIII	0.73(3.6)	0.93(28)	0.93	2.28
IX	1.0 (4.0)	0.89(21)	0.90	1.87
X	1.1 (3.7)	0.87(18)	0.85	1.90
XI	0.9 (3.8)	0.91(23)	0.91	1.69
XII	2.2 (6.6)	0.70(13)	0.76	2.09
1966/I	0.32(1.6)x	1.0 (30)	0.95	1.79
II	0.63(1.7)x	0.96(16)	0.83	1.89
III	1.1 (5.2)	0.87(25)	0.92	1.92
IV	1.6 (3.4)	0.81(11)	0.70	2.08
V	1.7 (5.6)	0.79(16)	0.83	1.79
VI	1.6 (5.5)	0.8 (17)	0.85	1.73
VII	0.68(1.9)x	0.95(17)	0.85	1.34
VIII	0.9 (2.7)	0.9 (17)	0.85	1.62
IX	0.53(1.5)x	0.96(17)	0.85	1.57
X	-0.3 (0.1)x	1.05(15)	0.81	2.15
XI	0.1 (.27)x	1.02(16)	0.83	2.10
XII	0.06(.16)x	1.02(18)	0.86	1.70

Table V (cont.)

Year and month	ln a(t)	b(t)	R^2	D.W.
1967/I	0.22(.75)x	1.00(22)	0.90	2.13
II	0.34(.88)x	0.98(17)	0.84	1.62
III	-0.2 (0.5)x	1.06(18)	0.86	1.93
IV	1.1 (2.6)	0.85(13)	0.77	1.68
V	-0.16(.4)x	1.05(19)	0.97	2.04
VI	-0.4 (1.1)x	1.09(19)	0.88	2.05
VII	-0.33(1.0)x	1.07(21)	0.90	1.92
VIII	-0.31(0.8)x	1.07(19)	0.87	2.27
IX	-0.3 (0.8)x	1.06(19)	0.88	1.46
X	-0.07(0.2)x	1.03(19)	0.88	1.32
XI	0.37(0.1)x	0.96(18)	0.86	2.03
XII	-0.4 (1.6)x	1.07(26)	0.93	1.16
1968/I	0.4 (1.1)x	0.96(19)	0.87	1.71
II	0.95(2.7)	0.87(17)	0.84	1.55
III	0.61(1.6)x	0.92(16)	0.83	1.84
IV	0.07(0.2)x	1.0 (22)	0.90	1.52
V	0.22(0.9)x	0.98(27)	0.93	1.45
VI	0.06(0.2)x	1.0 (26)	0.93	1.78
VII	1.3 (5.0)	0.82(22)	0.90	1.98
VIII	0.55(2.5)x	0.93(28)	0.94	1.51
IX	0.59(2.2)x	0.92(23)	0.91	1.99
X	0.94(4.3)	0.88(27)	0.94	1.86
XI	1.1 (3.5)	0.85(19)	0.87	2.30
XII	1.4 (5.)	0.82(21)	0.89	2.09
1969/I	0.75(2.6)	0.90(21)	0.89	1.64
II	-0.17(0.5)x	1.04(23)	0.91	2.15
III	1.1 (2.7)	0.86(15)	0.82	1.70
IV	0.41(1.6)x	0.96(25)	0.93	1.74
V	0.72(28.)	0.91(24)	0.92	2.31
VI	0.96(3.2)	0.88(20)	0.88	1.71
VII	0.05(.17)x	1.02(22)	0.91	1.58
VIII	0.64(.22)x	0.92(22)	0.90	1.47
IX	0.6 (3.0)	0.94(33)	0.95	2.21
X	0.37(1.2)x	0.97(22)	0.91	1.70
XI	0.77(1.5)x	0.91(12)	0.75	2.28
XII	0.09(0.5)x	1.01(39)	0.97	2.05
1970/I	0.76(2.6)	0.91(22)	0.90	2.22
II	0.96(3.8)	0.88(24)	0.92	1.74
III	-0.11(0.4)x	1.04(30)	0.94	2.25
IV	0.67(2.1)x	0.93	0.89	2.00

IV. Stabilization Policy Implications

The most reliable information we have on instability in ESE is the one on investment cycles. There may be differing views on how they are generated. The fact, however, that investment cycles considerably decreased their amplitudes from the first to the second postwar decade and that planning was apparently the only stabilization policy pursued (Yugoslavia excluded), warrants the conclusion that, even if they had not been generated by plans, as my interpretation suggests, central planning of the ESE type is capable of achieving stable growth. Moreover, in the degree to which investment cycles were responsible for agricultural cycles, planning is likely to mitigate these as well. In other words, there seems to be no need for a specific stabilization policy in regard to investment cycles. As they are supply-generated, the supply-type central planning seems to be an adequate stabilization policy.

In Yugoslavia business cycles are most likely demand-generated. The fact that we could not detect them in any of the three examined ESE does not guarantee that they do not exist either in these three or in any other ESE. However, if business cycles exist in the three investigated ESE, they are likely very mild, that is, of small and irregular amplititudes. Moreover, changes in the general price level are small, infrequent and irregular in their direction. Since no stabilization policy beyond central planning is in use, we may conjecture that central planning is capable of insulating the course of the economy from the disturbing effects of "autonomous" forces. For, while a stable growth of government and investment expenditures, both in the long and in the short run, is imaginable, it is difficult to believe that no unplanned impulses were transmitted to production from population.

Quite different is the Yugoslav case. The independence of Yugoslav enterprises together with an inefficient central plan and a multitude of independent, largely uncoordinated budgets has made her economy operate similarly to classical market economies. Moreover, international instabilities, particularly those working through raw material prices, are transmitted most directly. Any measure leading to greater international stability would contribute to internal stability. However, as instability is predominantly her domestic product, two internal-policy recommendations are appropriate:

First, an incomes (wage) policy that would link wages to labor markets and free them from the impact of product markets, and make labor markets perfect, could possibly contribute to a decrease in the rate of inflation. While I do not make any proposal of this sort for capitalist economies (any incomes policy confined to wages would worsen the economic conditions of workers; if extended to profits, it would perpetuate the existing social system) it is strongly recommendable for a country like Yugoslavia. With socialist ownership of the means of production, linking of wages to labor markets simply amounts to "equal pay for equal work."

In a socialist economy this should be an economic-policy objective even if departures from it did not result in generating inflation.

Second, an adequate incomes policy may also contribute to securing stable growth. While our short-cycle analysis calls for an active budgetary policy, both on its revenue and expenditures side, it suggests that business investment has been overemphasized and expenditures of the population underestimated as sources of instability of growth.

Let me conclude by observing that the above was a discussion of stability and not efficiency problems.

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Discussion

Richard Portes*

Despite his modest disclaimer in the face of a difficult title, Professor Bajt has in fact given us three papers. The first considers patterns of instability in "East European Socialist Economies,"[1] with particular attention to medium-term investment cycles, and some evidence against the existence of short cycles in the CPEs. The second deals with Yugoslav short cycles, a forecasting equation, and a small structural model. The third tests a theory of Yugoslav wage structure.

The total product is substantial and contains much interesting material which deserves detailed comments. Given the subject of the Conference, however, the proportions of my consumption differ somewhat from those of Professor Bajt's production. Moreover, if only because of the supposed importance of the "great grain raid" by the U.S.S.R. as a prime mover in recent U.S. and world instability, I think it worth speculating (in a stabilizing way, I hope) on potential linkages between the socialist economies and the rest of the world.

Bajt dismisses the possibility that such linkages might propagate instability, because he believes investment cycles are disappearing from socialist economies and short cycles have never existed in the CPEs. He may in practice be correct here, even for the medium-term future. I should nevertheless have preferred a more explicit discussion of the international economic relations of the socialist countries, especially the role of the foreign sector in generating and transmitting instability among socialist countries themselves and between them and the rest of the world.

Bajt's basic premise (perhaps tautologous) is that if we find instabilities in an economy directed by central planning, we should expect them to arise out of imperfections in the planning system. He goes on to argue that investment (and to some extent agricultural) cycles *have* been

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*I have had helpful discussions with my colleagues Hugh Davies and John Muellbauer on some of the points discussed below.

[1]We shall distinguish in his sample between the centrally planned economies (CPEs) and Yugoslavia. In many respects, post 1967- Hungary lies in between.

generated by specific characteristics of planning and the planners; that the planners' performance clearly improved from the 1950s to the 1960s; and that this just demonstrates that these cycles have been supply-determined phenomena, which properly run supply planning will eliminate. Short cycles and inflation, on the other hand, he simply finds non-existent in the CPEs. He therefore concludes that central planning insulates the economy from autonomous shocks (and prevents it from transmitting them?), so that internationally coordinated stabilization policies between CPEs and the rest of the world — and within CMEA — are unnecessary.

To discuss investment cycles, Bajt extends the data from Bajt (1971a) to cover 1950-1970 and considers the two decades separately. He defines instability in effect as deviations from trend growth. The Conference did not standardize definitions. Bajt's here is purely descriptive of the data. He then goes to a *cyclical* concept, and later, discussing his model of Yugoslavia, he also considers the sensitivity of the economy to shocks. In any event, instability as first defined diminished significantly for all aggregates studied in the second decade.[2] In the 1950s agriculture was the least stable variable, while in the 1960s this was investment (except in the USSR and Poland). Construction, gross social product, and industrial output follow in increasing degree of stability. Thus investment is now "the main factor of instability." But even to put the point this way implies a model in which investment is exogenous. Although he argues this case later, maintaining that investment was determined "by the planners," the data alone — without testing any structure — cannot tell us this.

Bajt then considers a cyclical interpretation of the observed instability. He first denies that the cyclical behavior apparently shown by the data in Bajt (1971a) is spurious, deriving only from the serial correlation introduced by the use of moving averages. In fact, he finds substantial first-order autocorrelation in each of his 20-year series, unaveraged.

He therefore postulates a mechanism of the cycle in CPEs, incorporating elements of Goldmann's (1964) explanation of the upswing and Olivera's (1960) explanation of the downswing. This extends the views in Bajt (1971a, 1971b) by making agriculture at least partly endogenous. The distinctive feature is the stress on the effect of "excessive" growth of investment in drawing resources away from consumption, and the consequent (lagged) response of the population, generating political pressure on the planners to cut back investment. By contrast, Goldmann would emphasize here the balance of trade and raw material supply constraints, while Olivera gives less weight than Bajt to the negative effects of investment on food supplies. I find that Bajt's process has much in common

[2]His statistical measure, however, is the *standard deviation* of annual rates of growth rather than their coefficient of variation. As Seton and I maintained in Bronfenbrenner (1969), the latter is preferable here. It would slightly soften the contrast between the 1950s and 1960s, because average growth rates fell somewhat (these should at least be included in the Table).

with Hicks's (1967) reinterpretation of the Hayekian cycle in contemporary terms. The shift of resources into investment must reduce the supply of consumption goods, and if a *further* increase in the savings ratio is not forthcoming, there will be a "crisis," and real wages must fall. Of course, the Hayek story requires that the rise in investment, its bias towards the producer goods sector, and its gestation period should together be sufficiently great that the supply of consumption goods will not recover and expand quickly enough to avert the crisis.

Bajt seeks empirical support for his hypothesis in correlations between rates of growth of fixed investment and those of construction, industrial production and agricultural output (denoted here by INV, CONSTR, etc.). No model is proposed. Instead, in his "partial lag correlations," he first regresses (say) $CONSTR_t$ on INV_t and gets a series of residuals, $[e^C_t]$. He then regresses $INV_{t+\tau}$ on INV_t, separately for $\tau = -3, \ldots, +3$, and gets a set of series of residuals, $[e^I_{t+\tau}]$, $\tau = -3, \ldots, +3$, Finally, he regresses $[e^C_t]$ on $[e^I_{t+\tau}]$ for each value of τ and looks at the best fit. Table III gives values of τ and r for the best-fit lags. Generalizing across countries, he finds that investment leads construction and industrial production positively ($\tau < 0$, $r > 0$), investment leads agriculture negatively ($\tau < 0$, $r < 0$), and agriculture leads investment positively ($\tau > 0$, $r > 0$).

Bajt makes the following main points in interpreting these results: (i) "investment is mainly exogenous, determined by plans," and there is a "capacity effect" of investment on construction and industrial production which is "much stronger than the accelerator effect"; (ii) "agriculture may not be as exogenous as generally believed," although "it is not possible to trace the chain of causation from investment to agriculture"; (iii) accelerations in agriculture allow subsequent accelerations in investment.

I can see no justification for inferring causality from this work. One cannot expect Bajt to produce fully specified structural models of these economies — but in the absence of *any* specification of structure and appropriate tests, I remain unconvinced. Nor can I take it as evidence for the pervasive and cumulative qualities connoted by "cyclical" behavior. We are given no data on employment, foreign trade, inventories, consumption, real wages, etc; and there are no reasons to suppose that any multiplier or accelerator mechanisms operate in these economies.

Moreover, even taken at face value, his results offer little support for his view of cycles in CPEs. The "capacity effect" of investment is hardly surprising, but it is irrelevant to Bajt's hypothesis about the relation between investment and consumption. Indeed, I should think he would have to show precisely that the capacity effect is too little and too late to relieve that pressure on consumption which forces the cutback in investment. Here the negative effect of investment on agriculture is more interesting, but how is it supposed to operate? I am skeptical of the effect of investment in drawing labor out of agriculture; Hamermesh and Portes (1972) show that in the Hungarian case, the outflow of labor from agriculture is best explained by the vicissitudes of collectivization policy. In

any event, should he not seek evidence of this effect on real wages and total consumption?

Similarly, the point about agricultural surpluses financing investment booms is plausible enough, but if Bajt believes this operates through foreign trade, surely he should be looking at the links between the trade balance in agricultural products and imports of machinery. I have discussed the relation between Hungarian investment fluctuations and the balance of trade in Portes (1971); in a highly open economy holding small reserves, one expects that after a good year in trade, the planners will push up investment in the following year. And they do. In this respect, investment certainly is not exogenous. Nor is his main argument for the "exogeneity" of investment very strong. Until 1966-70, the coincidence of Five Year Plan periods across countries was very rough indeed (Kaser, 1967, p. 66), and FYPs were frequently abandoned in mid-course. Thus to attribute the synchronization of investment cycles across countries to the synchronication of plans, and then to infer that the planners "autonomously" determine investment, seems unwarranted. Again, why not look for synchronization in intra-CMEA trade?

I therefore should have liked to see the results of Bajt's attempts "to see whether investment cycles have been transmitted internationally (within ESE)." This work is particularly germane to the Conference theme, and even inconclusive results might be very informative.

Investigating short cycles, Bajt is constrained by lack of quarterly or monthly data on anything but industrial production, which is available only for three CPEs and Yugoslavia. These monthly data for the CPEs show much greater variance than the yearly data, but I fully agree with his conclusion that these fluctuations have "a predominantly seasonal character." I should think this is simply the effect of the quarterly "plan cycle" ("storming"). Both spectral analysis and looking at serial correlation in residuals suggest that there are short cycles in Yugoslavia, but not in the CPEs.

Bajt therefore concludes that "central planning seems to be able to successfully cope with problems of stable growth . . . it weakened investment cycles from 1950-60 to 1960-70 and prevented short cycles . . . Non-agricultural oscillations are likely to become very mild." I accept this view, but I think Bajt has neglected some of the most important and interesting questions. In what ways has planning improved over the period? Is it just that the planners are more "realistic" and better judges of how tightly they can limit consumption, or are planning techniques and plan implementation better? What are the functional and behavioral mechanisms, the policy instruments, the institutions which have moderated investment cycles and eliminated short cycles and inflation in the CPEs? Are they all inseparable from physical allocation ("supply planning"), or might some offer lessons to mixed economies?[3] Central planning cannot

[3]Kotowitz and Portes (1974) discuss one such device, the "tax on wage increases," which is related to a plan-implementation technique originally introduced in Hungary in 1957.

in practice control *all* economic variables — there are unplanned phenomena generated by the system itself, by unregulated relationships between variables, and by exogenous shocks (foreign trade, shifts in behavioral functions). How do the planners deal with them, and what kinds of stabilizers can they rely on? How do the planners themselves *react* to changes in economic variables, i.e., can we estimate "planners' behavioral equations"?[4] Other problems, more closely related to the subject of this Conference, are perhaps less amenable to quantitative or theoretical analysis, but some observations on them might have been in order all the same. I shall return to them below.

In the remainder of his paper, Bajt deals only with Yugoslavia. I shall be briefer here, in part because I am less confident than he of the relevance of the Yugoslav case to predicting what might happen if other socialist economies decentralize. Yugoslavia is still less developed than most CPEs, and none will adopt its workers' control nor suffer such sharp regional conflicts.

The main purposes of the section on Yugoslav short cycles is to support the conjecture in Bajt (1971b) that "it is the consumer sector which seems to be responsible for the short cycles in Yugoslavia (p. 68)." There is as yet no structural model for the Yugoslav economy, so he discusses his forecasting equation, which he believes reveals the important role of "autonomous" consumption expenditures in propelling the cycles.

The construction of the forecasting equation is straightforward. To get proxies for the impulses transmitted by "autonomous expenditures," he takes residuals (which are highly autocorrelated) from estimated consumption, investment, and government expenditure functions. That is, using monthly data, he estimates $C_t(Y_t)$, $I_t(C_{t-1} - C_{t-13})$, and $G_t(Y_t)$, where Y is disposable income and government spending is taken as a function of revenues, proxied by Y. Note that there are no lags (with monthly data) in C(.) and G(.), and only a one-month lag in the accelerator. He then uses the three series of residuals and a trend to explain economic activity as measured by the index of industrial production. On the residuals, he searches for the best-fit single lags ($\tau = -9$ on the consumption residual variable, -7 on investment, and -5 on government expenditure). The consumption residual is the "decisive" variable (by what criterion?).

As I understand this technique, its main advantage over a more conventional approach is to cut down the work of finding an acceptable lag structure. The corresponding approach would be to estimate C, I, and G as functions of their own lagged values and lagged values of Y, putting a lot of effort into getting the right lag structure in each equation. One would then forecast Y with $\hat{C}$, $\hat{I}$, $\hat{G}$ (estimated from the structural equations, adjusting constants with moving averages of recent residuals). Now the explanatory variables (the consumption, investment, and government residual series) in Bajt's equation are of course also functions of lagged

[4]For a planner's labor demand function (wage-setting equation), see Hamermesh and Portes (1972).

values of C, I, G and Y, and by assuming simple lag structures in the original equations yielding the residual variables he reduces the work of finding acceptable lags to looking at three Almon lag distributions.

Bajt says that "for stabilization purposes, the 'autonomy' of . . . consumer expenditures . . . is of greater interest." Perhaps, but I suspect he is more concerned to back up his interpretation of the Yugoslav short cycles. In either case, I cannot see how his three-equation accelerator-multiplier model and its behavior under simulation have any bearing whatsoever on this structural question. This model, he finds, "suggests the structure [of the economy] is highly explosive." One would of course expect the *model* to be explosive with Durbin-Watson statistics implying first-order autoregressive parameters almost equal to unity, but surely this tells us nothing about the Yugoslav economy.

Quite rightly, Bajt dismisses the conventional view that there is significant inflation in the CPEs, which in view of their negligible open inflation he therefore calls "essentially inflation proof." Current work of my own in this area finds no sign of the two main effects which theory (e.g., Barro and Grossman, 1974) predicts from repressed inflation: forced saving, and reduction in labor supply.

The Yugoslav case is of course quite different. Bajt postulates a mechanism of wage inflation in Yugoslavia which adds to models such as Wachter's (1970) the particular forces generated by worker's control and socialist job security. The basic hypothesis is that the spread of inter*enterprise* wage differentials will be negatively correlated with the state of business conditions. In any case, he has three separate tests (using monthly wage data on 54 *industries* over 1964-1970), all of which confirm the proposition.

I have myself recently used an equation similar to Bajt's (and Wachter's) to explain intertemporal movements in the coefficient of variation of the inter-industry wage structure in several countries, and my results are consistent with his. I am somewhat dubious, however, about concluding from the lag structure of his equation that "reactions of the KV to product markets are prompter than to labor markets," and basing on this his recommendation about incomes policy in Yugoslavia.

Turning finally to some broader issues, while I agree with Bajt that the CPEs are unlikely to become significantly less stable than in the recent past, I think we must bear in mind some arguments to the contrary. The first, mentioned by Bajt (but not with the same emphasis here as in Bajt 1971a, 1971b), is that decentralization and greater scope for market relations and demand influences in the CPEs would introduce more instability. But I myself do not believe that "economic reforms" in Eastern Europe will go very far in this direction. Nor will the rise in standards of living and consequent increasing importance of discretionary consumer expenditure have a significant destabilizing effect (as was suggested by Seton in Bronfenbrenner, 1969). The planners are capable of adjusting the structure of consumer supplies tolerably well and maintaining aggregate

equilibrium. This is shown by the absence of repressed inflation and of any evidence that private savings are "excessive" or especially volatile.

On the external side, I would judge convertibility within CMEA to be about as likely as "economic and monetary union" in the EEC — not very, and certainly not soon. A fortiori, no CPE currency will become convertible in the West. Any form of convertibility is fundamentally inconsistent with physical planning and fixed domestic prices. This will of course set limits to the expansion of East-West commodity trade (though not necessarily to the increasingly popular "cooperation agreements"). CPE trade dependence will continue to rise gradually, and the share of trade with the West will increase somewhat, but we should not expect anything dramatic, if only because of the limited capacity of the CPEs to supply exports acceptable in the West.[5]

The socialist countries are therefore unlikely to become a significant destabilizing influence on the rest of the world. But Soviet purchases of grain do highlight the continuing instability and generally poor performance of agriculture in the USSR. Presumably the United States has learned enough not to be "schnookered" (Nixon, 1973) again, but two general points emerge. First, the socialist countries will play a more important role on world markets for primary commodities than for industrial goods. As buyers, they will no doubt show as much hostility towards incipient LDC cartels as the advanced capitalist countries, but we may expect their influence on world prices to grow. The major potential seller is of course the USSR, but I would regard the immense development projects which would be necessary as a very uncertain and long-run prospect. Second, although we can disregard most of the conventional transmission mechanisms between CPEs and the rest of the world, we should not forget Soviet gold, which does enable the USSR to transmit short but sharp impulses to the world economy.

[5]Even the Chinese, whose trade with the United States this year is projected to rise from $500 mill. in 1973 to $1.25 bill. (and thus to exceed Soviet-American trade), cannot keep it up unless American generosity becomes unbounded (the projected 1974 volumes breaks down to $1.15 bill. Chinese imports, $100 mill. exports). But it does seem unlikely that we shall again witness another eastwards rush of New York bankers, competing to offer the most favorable terms for large long-term loans. Socialist countries may of course borrow more on the Eurodollar market, as the Hungarians have done.

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Reply to Professor Portes' Discussion

A. J. Bajt

My basic reaction to Professor Portes is that of a general agreement with his discussion.

In the first place, I also should like to study ESE international relations, including the transmission of instabilities. Yet, unless one goes in for transmission mechanisms as such, in which case transmission of an occasional shock à la great grain deal is as good as any other, then structural instabilities, regularly reproduced by economic mechanisms of the individual countries, are the first step. After having attended this conference for two days I can add a new reason to those given in the paper. With transmission models that have been developed in the West and which also comprise socialist countries, a direct concentration on transmission problems would be an unnecessary duplication of efforts, amounting almost to suicide in view of their powerfulness.

I also agree with Portes that "speculating on potential linkages between the socialist economies and the rest of the world," judging the likelihood of convertibility and predicting the future course of economic reforms in CPE (to take a few examples) is an appealing approach. But I find this kind of work either obsolete (models not only specify but estimate linkages) or too difficult (econometric forecasters have problems with quarters) and beyond the scope of our profession (how can an economist tell that workers' control will not be adopted in any of CPE?), and in any case placing too much relevance on assumptions rather than facts. Many predictions are just guesses, no matter how right they may prove. For instance, CPE and Yugoslavia did not "show as much hostility towards" the recent Arab oil supply and price handling "as the advanced capitalist countries." Although they had to adapt their economies to a painful change, there was no hostility in their reactions. Moreover, they welcomed the new policy on *a priori* grounds, and they would have had to welcome it even if there were no capitalist economies.

I am by no means surprised if Portes remains unconvinced by my interpretation of investment cycles. In the sense of being 100 percent sure, I am not convinced either. If for nothing else, they must differ from cycle to cycle, and from country to country. The postulated interpretation is

just the one that best and most generally fits into facts — economic, social, political etc. — established statistically and in many other ways. But I am surprised when he accepts my eight-year cycles without any comment, in fact without mentioning them, including Howrey's and Klotz' method of measurement. For planners' one-year lagged reactions to foreign-exchange-reserve fluctuations, which in turn depend on agricultural production and exports, advanced by Portes, should have produced year-to-year investment cycles (see upcross periods in Table II of my paper), which obviously fall outside the range of cyclical fluctuations.

I also share Portes' preference for "fully specified structural models," although he seems to be quite a bit too optimistic about them. First, I do not believe that structural models, and econometric models in general, are the only road to truth: economics existed centuries before such models were invented. There is considerable ESE literature on investment cycles and their interpretation but without econometrics. Even Lange, 1964, does not use econometrics when he discusses them. Second, I would be very reluctant to build any structural investment-cycle model with yearly data. Excercises on international links of investment cycles convinced me that diametrically opposed theories could be "appropriately tested" with equally good results. Highly aggregated data, both across sectors and in time, allow wonderful things. In the best case they yield "great economic ratios" and rather inconclusively describe the actual working of the economy. To approach the right specification including lag structure, V. Bole has proposed the "partial lag-correlation" and used it to study investment cycles. I fully agree that lag-correlation does not stand for cause-effect relationships. But cause-effect relationships do go on in time and sometimes can be traced by appropriate techniques. One may be partial lag-correlation, particularly with yearly data. However, simple lag-correlograms, that is distributed lag structures, can be misleading because real interaction of economic agents takes place in shorter time periods than years.

Finally, in principle I have nothing against introducing real wages and total consumption. But I find the approximation of consumption by agricultural production quite satisfactory. For improvement I would prefer including food exports and imports rather than indices of real wages. Those data may not be reliable and require settlement of several utility and price questions.

There are a few minor technical points on which I would like to add comments.

(l) I wonder about Portes' insistence on endogenous investment. By what mechanism do investment rates endogenously approach the 40 percent (of GSP) mark?

(2) When he said that I should have "to show precisely that the capacity effect is too little," Portes appears to mistake correlation coefficients for regression coefficients. The established correlation coefficients may stand for any value of capacity or accelerator effects. More

important, the "too little capacity effect" is shown by the negative correlation between investment and lagged agriculture. Steel is irrelevant anyhow.

(3) I cannot accept Portes' belief that CPE fluctuations (monthly data) are predominantly seasonal, because data are seasonally adjusted. Quarterly plan "storming," suggested as an explanation, is clearly a seasonal phenomenon and should not have shown up in adjusted data. If quarterly plan storming were the reason for the persistence of seasonal oscillations, one would have to show that they are seasonally irregular and account for the reason. I would retain this possibility as an emergency exit. Since 1960 there has been no plan storming, quarterly or otherwise, in Yugoslavia but an irregular season exists just as in other ESE. It seems more likely that seasonal adjustment of data includes some hidden Slutskian processes which come to the fore when data are transformed into monthly rates of growth. Harmonics of trading day cyclicallity is another possibility.

(4) The coincidence of medium-term plans was certainly not perfect. But the first medium-term plans, most relevant for initiating the process, were launched in the same year (1947) in five countries (Yugoslavia, Czechoslovakia, Hungary, Poland, and GDR), with no country deviating for more than one year.

(5) Portes' argument that the Yugoslav case is irrelevant for what might happen if other ESE decentralize ("Yugoslavia is still less developed than most CPEs, and none will adopt its workers' control nor suffer sharp regional conflicts") is a *non sequitur,* since decentralization with its economic consequences is neither confined to LDC nor necessarily assumes the form of workers' management nor is dependent on regional conflicts. Later on Portes himself argues that decentralization increases demand-generated instability. Post-1967 Hungary, if lying between CPE and Yugoslavia, must have been moving yugowards, despite being better developed and suffering no regional conflicts. How far such processes will go is obviously not an economic question.

(6) Portes' belief that "the main advantage /of the forecasting equation/ ... is to cut down the work of finding an acceptable lag structure" is not warranted. First, it is rather easy to estimate any lag structure with available computer techniques. It can be inferred from our paper that we did estimate Almon lags. Although I like them, I somehow dislike the recent Almon-lag fashion. The simple and partial lag-correlograms give a very exact picture of unconstrained lag structures. The specified lags come from this technique. Portes' proposal of estimating Y as a sum of estimated C, I and G, is equivalent to my three-equation model which he finds of no interest. The proposal "to estimate C, I and G as functions of their own lagged values and lagged values of Y" implies a very restrictive (Koyck's) lag structure, highly unrealistic in the case of our inverted-V-shaped lag structure. The real advantage of our model is that it identifies impulses which lead economic activity for quite a number of months thus

allowing efficient forecasting, particularly of the turning points. Portes' proposal limits the forecasting horizon to one month, at least at a first glance. However, by generating a systematic delay in forecast timing, which is up to three months in the case of Yugoslavia, he is able only to forecast the past. Adjustments of constant terms could help, but not if handled the way Portes proposes and it would be rather queer to rely in real forecasting on adjustment of constant terms anyhow.

(7) Portes "suspects that in the three-equation structural model" I am "more concerned to back" my "interpretation of the Yugoslav short cycles." But its main result is precisely to oppose my "autonomous expenditures" interpretation since oscillations are partly explained endogenously. To grasp the meaning of this and similar results one should look at Hickman, 1972.

(8) As for explosiveness of the model, it entirely depends on the estimated parameters (Samuelson, 1939) and on exogenous data. The Durbin-Watson statistic has no bearing on simulation whatever. Since high positive serial correlation is necessarily present in models with seasonally adjusted monthly data, one is tempted to ask whether building annual models (with no autocorrelation) is the way to increase economic stability.

In our case simulation was performed, in order to avoid transmission of cycles from outside, on an extremely restrictive assumption of linearly increasing government expenditures as the only exogenous variable. The really surprising fact is that explosion occurs only after 30 months.

(9) Portes implies that models and their structures (values of parameters) do not tell anything about the respective economies. It is true that nobody has ever seen an economy explode. But have explosive models really nothing to tell about the stability of an economy? What, then, are they built for?

Let me finally agree with Portes that some of the "most important questions ... bearing on investment cycles" which I neglected to discuss are really highly attractive. Would he agree that they represent a very small sample of the whole population of interesting questions about investment cycles? (All references apply to the bibliography given at the end of my paper.)