

## CHAPTER 4

### INFLATION AND MARKET STRUCTURE

The 20th century was marked by substantial changes in the nature of firms, industries and markets, yet most of these institutional developments left little or no impression on mainstream theories for inflation. Of course, no reasonable macroeconomist would deny that the modern corporation is a far cry from Marshall's 'family firm,' or that the complexity of modern industry is distinct from the simplicity of agricultural markets of early capitalism. Macroeconomists have not disputed that major structural changes occurred but their approaches suggest these and similar developments are simply immaterial for the explanation of inflation. The reason is fairly simple: in order for the aggregate price level to rise, the total demand for commodities must exceed their total supply and, since this requirement is quite independent of underlying structures, the specific nature of such structures is inconsequential for the purpose of analysis. From this perspective, 'perfect competition' should be regarded merely as a convenient instrumental assumption. While inflation might occur in a variety of structures, its ultimate cause is always excess demand and this can be best illustrated by resorting to a competitive framework.

The disregard for real structures and the emphasis on competitive market forces can be explained, to some extent, by noting that the formative years of modern macroeconomics coincided with the long post-war boom in advanced capitalist economies. Growth in that period was always accompanied by rising prices and that seemed to vindicate demand-pull theory. Given these circumstances, it was hardly surprising that most macroeconomists felt they could safely ignore the difficult intricacies of concrete structures and institutions. In this respect, economic growth arrested theoretical progress.

Stagnation, on the other hand, operated as a theoretical catalyst and kept bringing structures and institutions back into the macroeconomic centre-stage. This first happened during the 1930s when, after a half century of neglect for changing structures, the economics profession was woken up by the

clamour of the Great Depression. The discovery that ‘administered’ or ‘full-cost’ prices were not very sensitive to demand pressures was sufficiently persuasive as to provide, at least during the 1930s, a serious alternative to the *a*-structural macroeconomic approach promoted by Keynes. Yet at that period, rising prices were hardly a pressing problem and even the *avant-garde* saw no reason to incorporate these new structural insights into a broader theory for inflation. When the Second World War finally revived the economy and inflation started to appear, it was already too late to achieve such a theoretical breakthrough. Encouraged by the brisk post-war growth, macroeconomists forgot recent findings about modern structures. The familiar microeconomic idea of ‘excess demand’ was now successfully integrated into the aggregate Keynesian framework and provided the necessary explanation for rising prices!

Growth was not continuous, however, and when stagnation or recession reappeared, they again revived interest in structures and institutions. The positive effects of stagnation on structural awareness were felt particularly in the 1950s, when the United States experienced its first bouts of stagflation and, subsequently, during the severe worldwide stagflation of the 1970s and early 1980s. The experience of stagflation produced many explanations and served to heighten the basic difference between structural theories and the macroeconomic approach to inflation. As long as prices were rising with output, macroeconomics had no use for ‘superfluous’ structural complications. Only when the conventional demand-pull theory failed (that is during periods of stagflation), was there a pressing need for institutional insight. Hence, at the risk of some oversimplification, we can say that, while macroeconomics is geared toward *growth inflation*, structural theories relate primarily to *stagflation*.

The link between stagflation and structure is also evident in macroeconomics itself. As we illustrated in Chapter 2, mainstream explanations for stagflation are invariably based on some institutional amendments to the perfectly-competitive ideal but this *ad hoc* approach is quite different from the one followed in structural theories. While macroeconomists often view most institutional factors as unfortunate *imperfections* which can be ignored once stagflation disappears, structural theorists take such institutions as their *fundamental* starting point.

For that reason, the structural literature on inflation -- though much smaller than its macroeconomic counterpart -- is difficult to review and evaluate. In Chapter 3, we argued that the rejection of neoclassical motivational assumptions created a flood of alternative rules-of-conduct for modern firms. Furthermore, the departure from standard macroeconomic methodology meant that most structural theorists did not feel obliged to 'close' their models. These theorists were commonly preoccupied with one or few 'crucial' questions, such as what created the 'spark' of inflation or how it was 'transferred,' and the broader implications of their explanations were often ignored or left for 'future studies.' While it is not at all clear that such 'openness' is necessarily a deficiency, the relaxation of so-called 'consistency requirements' obviously broadened the range of possible theories. Under these circumstances, even Scherer, a prominent authority on industrial structure, cautioned his readers that 'any attempt to summarize *the* state of knowledge is risky, for virtually every conclusion [regarding the effect of structure on inflation] has been contradicted somewhere' (1980, p. 355).

Our examination of structural theories is intentionally selective.<sup>1</sup> Much of the attendant literature is dominated by the notion of 'markup pricing,' and the different interpretations for this practice provide a convenient basis for classification. Two broad categories can be discerned. The majority of explanations use markup pricing to emphasize the *passive* role of firms in the inflationary process. Another, much smaller, group of theories use markup pricing to suggest that firms play an *active* inflationary role. The bulk of this chapter is devoted to exploring the former category of theories. First, we deal with the way in which firms respond to changes in demand and cost. In the first section, we examine the idea that, given their markup-pricing practices, large firms tend to respond slowly to variations in demand. In the context of long-term growth, this behaviour is said to create a moderate (but persistent) inflationary bias and prices continue to rise even during cyclical recessions. The two following sections deal with the way in which industrial firms respond to cost. The second section explores the underpinning of 'normal-pricing,' a hypothetical framework in which fixed markups are added not to current cost but to 'normalized' cost. The third section develops the 'normal-price' hypothesis further by examining how industrial structure and competition affect the extent of 'price

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<sup>1</sup> Surveys of important studies are provided by Blair (1972, chs. 16 and 17), Mueller (1974), Beals (1975) and Dalton and Qualls (1979).

smoothing.’ With markups insensitive to demand conditions, ‘markup pricing’ (in general) and ‘normal pricing’ (in particular) point to cost as the prime mover of inflation. The fourth section integrates commodity prices into the structural framework for inflation, while the fifth section focuses on the potential impact of labour costs. Proponents of ‘cost inflation’ often point to the stability of markups as an indication that firms merely pass their cost increases onto the final price. This reliance on stable markups is a double-edged sword, however. Fixed markups are also consistent with the proposition of ‘profit inflation’ provided the initial rise in profits is followed by subsequent increases in costs. We deal with this idea in the sixth section of the chapter.

#### **4.1 Persistent Demand Inflation: Slow Giants and Unliquidated Monopoly Gains**

During the 1950s, economists in the United States were baffled by the increasing significance of counter-cyclical price movements during recessions. The Federal Government tried to ‘cool’ the economy with restrictive monetary policies, but these were apparently unsuccessful. In an early influential interpretation for the phenomenon, Galbraith (1957) argued that the confusion arose mainly from a basic structural misconception. The divorce of macroeconomics from microeconomic considerations caused policy makers to ignore important heterogeneities in the movement of individual price series and neglect the bearing of market structure on aggregate questions. The positive overall rate of inflation, Galbraith indicated, was affected mainly by prices for steel, steel-mill products, metal products and machinery. Those prices continued to increase despite the slack in activity and substantial excess capacity. On the other hand, prices for commodities such as farm products, synthetic textile products and apparel, behaved pro-cyclically and fell during the recession. According to Galbraith, this contrast in price behaviour during recessions was associated with differences in underlying structures: pro-cyclical price movements were typical to markets which approximated pure competition, while counter-cyclical price changes occurred primarily in markets where oligopolies were dominant.

The situation during the 1950s differed from earlier experience. Many who repudiated suggestions that, during the Great Depression, concentrated industries lowered their prices by less than competitive industries, found it more difficult to ignore how concentrated industries raised their prices

in the midst of recessions (see Chapter 3). Unlike some of his contemporaries who identified oligopolistic inflation as a cost-push phenomenon, Galbraith insisted that price movements in *both* competitive and oligopolistic industries were primarily demand determined. The divergency arose not from cost differences, but rather from a fundamental dissimilarity in the way firms in each industry responded to demand. For a firm operating under perfect competition, the rise in demand appeared as an increase in the ongoing market price. In other words,

The adaptation of prices to the increase in demand is automatic; in the nature of the competitive market no individual has the power to halt the adaptation. The price adaptation proceeds *pari passu* with the increase in demand; it is completed *pari passu* with the completion of the movement in demand. . . . In sum, in these markets price adaptation to changing demand is *contemporaneous* and, hence always *complete*. In all cases the rate of adaptation is market controlled; none of the aggregate effect is subject to the discretion of the individual firm. (p. 127, emphases added)

Hence, under conditions of pure competition, prices always move pro-cyclically with demand. The situation was different in the case of oligopoly. Here, prices were set by firms and not by the market and, thus, the rise in demand was first revealed to those firms in the form of increased orders or sales. Consequently,

The price adaptation must always come *later* and as a result of specific entrepreneurial decision. This adaptation is not automatic as in the competitive market; again in all but the most exceptional cases there will be some *time interval*. (*ibid.*, emphasis added)

Based on his assumption of 'delayed response,' Galbraith drew two related conclusions. First, during the interval in which price was adjusted to increased demand, the oligopolist did not maximize his short-term profits. Second, because the price adjustment during the expansion was 'incomplete' (in other words, short-term profits were not maximized) the oligopolist could *and would* continue to raise his price, even when the increases in demand subsided or disappeared:

With inflation, the demand curves of the firm and industry are moving persistently to the right. Under these circumstances there will normally be an incomplete adaptation of oligopoly prices. Prices will not be at profit-maximizing levels in any given situation, for the situation is continuously changing while the adaptation is by deliberate and discrete steps. This means that at any given time there will ordinarily be a quantum of what may be called *unliquidated monopoly gains* in the inflationary context. The shift in demand calls for a price increase for maximization; since the adaptation is currently incomplete, prices can at any time be raised and profits thereby enhanced. (*ibid.*, emphasis added)

Furthermore, Galbraith argued that 'under quite commonplace conditions the lag in adaptation will be considerable and the unliquidated short-run monopoly gains substantial' (*ibid.*). This assumption was necessary in order for inflation to spill from the upswing over to the downswing. The overall result in

the oligopoly sector was a moderate but continuous inflation throughout the business cycle. Since oligopoly was the dominant sector, the phenomenon overshadowed the different performance of competitive industries and appeared also as a macroeconomic anomaly.<sup>2</sup>

The main argument here is that oligopolies are slow to react to changing conditions. According to Galbraith (pp. 127-8), interdependency between oligopolies introduced caution into price changes. Also, wages often rose with prices but rarely fell with them, so firms tried to refrain from hasty price increases which might cause an irreversible swelling of costs. Finally, oligopolies were attentive to their public image and tried to avoid the appearance of short-term opportunism. Hence, these firms tended to adopt a longer view toward profit maximization, by 'smoothing' short-term price oscillations into a more steady trend. The result, according to Galbraith (p. 128), was that during expansions, oligopoly prices would constantly undershoot the prices implied by short-term profit maximization. When an increase in demand could have allowed them to make larger short-term profits by rapidly raising their prices, oligopolies gracefully waived their claim over this extra income and, thus, moderated the rate of inflation. The oligopolists' sacrifice was only temporary, however. As long as demand continued to increase, large firms continued to accumulate unliquidated monopoly gains, but when the trend was reversed, they carried on with price increases and slowly 'liquidated' those gains. Viewed somewhat differently, the process of moderating the rate of inflation during expansions was not a free service and the community must pay for it by tolerating rising prices also in recessions.

Galbraith's theory for 'perverse' inflation built on a basic difference in the way in which competitive and oligopolistic firms reacted to demand. Yet, in a more fundamental sense, the two types of firms were similar since they both merely *responded* to external stimulus. In changing their prices, both types of firms acted as *intermediaries*, while the ultimate cause for inflation was *exogenous*. This perception on the passive role of firms constituted the cornerstone of many 'transmission studies.' These studies were mainly preoccupied with the effect of market structure on the speed at which exogenous changes in demand or costs were translated into final prices. As we demonstrate in subsequent sections

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<sup>2</sup> For subsequent elaborations on this 'lag thesis,' see Adelman (1961), Scherer (1970, pp. 127-8) and Ross and Wachter (1973).

of this chapter, the ‘transmission mechanisms’ specified in many such studies were rigid in their format and axiomatic in their bases. The seeds of some of these shortcomings were already present in Galbraith’s article and we examine them now.

First, Galbraith indicated that oligopolies had *discretion* over their actions, but then assumed there was a certain *regularity* in exercising this discretion: during expansions oligopolies accumulated unliquidated monopoly gains which they liquidated during recessions. Given a short-run profit-maximizing price, the oligopolist would set his own price lower than this yardstick in expansions, and higher in recessions. One serious problem with this rationale is the lack of any meaningful estimates for ‘profit-maximizing prices’ and, hence, for ‘maximum profit.’ Without these benchmarks, ‘potential monopoly gains’ have no clear meaning and, hence, it becomes rather difficult to show how oligopolies ‘hoard’ and then ‘realize’ such gains.

Second, the logical basis underlying the existence of ‘unliquidated monopoly gains’ is not clear. Galbraith’s ‘catch-up’ thesis seems to rely on the dual assumption that oligopolies only *react* to changes in demand and that they do so by changing their prices in *steps*. Yet, these presumptions are still insufficient to explain why, during the expansion, the average rate of inflation in the oligopoly sector should be lower than what is necessary to maximize profits. As Galbraith (p. 127, emphasis added) acknowledged, ‘There is an, obvious, although I think outside, possibility that although adaptation is by discrete steps, there will be *anticipatory* adaptation in each move.’ Furthermore, the price steps during the expansion could be large enough to leave no ‘unliquidated’ gains for the following recession. Yet, Galbraith discounted these possibilities, arguing that ‘anticipatory’ price changes and ‘high’ price steps were not very likely to happen for a fairly simple reason. In his opinion, oligopolies would prefer to maintain relatively ‘low’ prices during the expansion in order to circumvent wage demands and public protests. But if this were true, should the oligopoly not keep ‘low’ prices also in recessions? It is not clear how large firms in the steel industry, for instance, could hope to prevent wage demands and public criticism by setting ‘excessive’ prices during a slump and blaming the extra profits on sacrifices they made in an earlier expansion. Galbraith also argued that oligopolies, because of intricacies in their interdependence, developed certain inhibitions toward a fast response, but this seems to imply a *lack* of

discretion! In this context, the oligopolist appears as a slow giant whose size and power constitute a fetter rather than advantage.

These criticisms lead back to the methodological dilemma raised in the preceding chapter. The 'regularity' assumptions made by Galbraith are not necessarily wrong, but if they are correct, they indicate that large oligopolies do not have much more discretion and autonomy than their purely-competitive counterparts. Discretion and autonomy mean more than just an ability to *not* maximize profits. They imply a freedom to *alter* one's course of action. If we insist that oligopolistic firms always smooth prices in one particular way, we cannot, at the same time, maintain that these firms exercise discretion. It should be emphasized that Galbraith (p. 127) stopped short of specifying any precise smoothing 'mechanism' for prices and indicated only that, although the oligopolist's response pattern is 'subject to alteration by individual entrepreneurial decision . . . the regularities are more than sufficient for the solution of the present problem.' Unfortunately, these generalities merely blur the basic methodological contradiction arising when the economist, in the name of entrepreneurial discretion, first emancipates firms from the reign of profit maximization and, then, enslaves them to his own dictum of how they should act. The significance of this contradiction is stressed in subsequent sections.

#### **4.2 The 'Normal-Price Hypothesis': In Search for Standard Cost**

With renewed concern over price behaviour since the 1950s, economists exercised great latitude in specifying their own pricing procedures for firms and the number of different models grew rapidly. The best choice among competing hypotheses, so it was hoped, would emerge through rigorous econometric testing, but this has failed to happen. After more than a decade of econometric research into price behaviour, Nordhaus (1972a, p. 34) admitted in a disconcerted tone that '[u]nfortunately, it is not clear that the studies have been fruitful.' Disagreement over the 'proper' model intensified through the 1970s and 1980s. According to Earl (1973, p. 7), the lack of meaningful progress in the econometrics of price formation was hardly surprising, for most models had no 'clear theoretical basis.' Moreover, because of spatial and temporal instability, the analysis of price behaviour had 'no solid econometric foundation' (1975, p. 83). Most researchers chose to ignore the possible non-stationarity of underlying

processes and continued to assume that firms exercised ‘inflexible discretion’ in their pricing. Many studies imposed rigid ‘pricing rules’ on entire industries, sectors and even the economy as a whole, and these rules were assumed to be valid for time periods of arbitrary length. When the econometric results were disappointing, the economist commonly amended or replaced the old specifications, and the process of estimation began anew.

One major approach in this empirical literature has been to consider pricing as a ‘normalizing’ process. The terminology is quite natural in this context. Since one assumes that firms merely respond to external circumstances and that they are unable to take initiative and deviate from their ‘standard’ pricing practices, it seems only logical to label their procedure as ‘normal’ pricing. The ‘normal-price hypothesis’ has several variants. In this section we examine an important series of studies conducted by Nordhaus and Godley (1972), Coutts, Godley and Nordhaus (1978) and Coutts, Godley and Moreno-Brid (1987), all focused on the manufacturing sector in the United Kingdom.<sup>3</sup> We consider these studies in chronological sequence and illustrate how, over a 15-year period, this group of economists has dramatically altered its definitions for ‘normal pricing.’ The changes have blurred the very meaning of ‘normal pricing’ and greatly reduced the scientific stature of attendant statistical tests.

Unlike Galbraith (1957), Nordhaus and Godley (1972) argued that prices responded only to long-run, ‘normal’ changes in direct cost and were insensitive to short-run, ‘temporary’ fluctuations in either cost or demand. Also unlike Galbraith, the two authors specified the pricing process in precise terms. They began by stipulating that ‘output price is set by taking a constant percentage over average normal historical current cost’ (p. 854). The ‘normal value of a variable,’ they wrote, was ‘the value that variable would take, other things equal, if output were on its trend path’ (*ibid.*). The test for this hypothesis involved 3 basic steps: (1) normalizing direct unit cost by removing its cyclical components; (2) estimating the lag profile between costs and prices under the assumption that firms used historical-cost accounting for their pricing; and (3) predicting the price by first imposing on each item of normal cost its corresponding lag profile, and then adding the results using the weights of each cost

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<sup>3</sup> For earlier works on the ‘normal-price hypothesis,’ see Godley (1959), Neild (1963) and Schultz and Tryon (1965). A later study by Gordon (1975) applied the hypothesis for prices in the United States.

item in some particular base year. We explore each of these steps in turn.

The first stage of analysis consisted of deriving the normal, or standard values for direct cost. Nordhaus and Godley argued that only labour cost should be normalized. Non-labour cost (for materials, fuel, services and indirect manufacturing taxes), they explained, had no normal trend and, hence, did not required any special transformation. The definition for unit labour cost involved 4 variables: average weekly hours, hourly wage rates, employment and total output. The value for each of these variables was assumed to be the sum of a normal, long-term component and a temporary, cyclical element. In order to remove cyclical elements from each actual series, Nordhaus and Godley followed a standard two-stage procedure. First, they regressed the variable against a collection of ‘trend’ and ‘cyclical’ carriers. Then, by using the estimated coefficients and values for only the trend carriers, they ‘predicted’ the normal series for the variable of interest. We examine the details of this procedure below.

The variable for average weekly hours ( $H^*$ ) was specified as a function of a constant, standard weekly hours determined by law or national negotiations ( $HS$ ), the rate of capacity utilization ( $CU$ )<sup>4</sup> and a time trend ( $t$ ), such that

$$(1) \quad H^* = \alpha_0 + \alpha_1 HS + \alpha_2 CU + \alpha_3 t + u ,$$

where  $u$  was an error term and  $\{\alpha_i\}$  were unknown coefficients to be estimated. Assuming that  $\alpha_2$  represented the cyclical impact of capacity utilization, the definition for normal weekly hours ( $HC$ ) was given by Equation (2):

$$(2) \quad HC \equiv a_0 + a_1 HS + a_3 t ,$$

where  $\{a_i\}$  were the estimated coefficients for  $\{\alpha_i\}$ . Next, the natural logarithm for average weekly earning ( $AWE$ ) was expressed as a function of a constant, time ( $t$ ), the basic official hourly wage rate

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<sup>4</sup> Capacity utilization was defined as the ratio of actual output ( $X$ ) to ‘normal’ output ( $XN$ ). ‘Normal’ output was obtained by first regressing the natural logarithm of output on a time trend and then using the estimated coefficient to predict the trend in output. See Nordhaus and Godley (1972, p. 875).

(*BHR*), standard hours (*HS*) and the relative deviation of actual from standard hours [ $(H - HS) / HS$ ], reflecting the impact of overtime hours:

$$(3) \quad \ln AWE = \beta_0 + \beta_1 t + \beta_2 \ln BHR + \beta_3 \ln HS + \beta_4 (H - HS) / HS + u ,$$

where  $u$  was an error terms and  $\{\beta_i\}$  were unknown coefficients to be estimated. Using coefficient estimates from this equation, the normal average weekly earning (*AWEN*) was defined implicitly in the following equality:

$$(4) \quad \ln AWE \equiv b_0 + b_1 t + b_2 \ln BHR + b_3 \ln HS + b_4 (H - HS) / HS ,$$

where customary hours (*HC*) replaced actual hours (*H*) and  $\{b_i\}$  were the estimated coefficients for  $\{\beta_i\}$ . In the following step, Nordhaus and Godley distinguished between operative employment, ( $L_{op}$ ) and employment for administrative, technical and clerical workers ( $L_{atc}$ ). They specified one regression for each type:

$$(5) \quad \ln L_{op} = \gamma_0 + \gamma_1 \ln X + \gamma_2 \ln HC + \gamma_3 t + \gamma_4 t^2 + u$$

and

$$(6) \quad \ln L_{atc} = \delta_0 + \delta_1 \ln X + \delta_2 t + \delta_3 t^2 + u ,$$

where  $X$  was output,  $HC$  was customary weekly hours,  $t$  was time,  $u$  was an error term and  $\{\gamma_i\}$  and  $\{\delta_i\}$  were unknown coefficients to be estimated. The corresponding implicit definitions for the normal-employment variables were given by equations (7) and (8):

$$(7) \quad \ln L_{op}N \equiv c_0 + c_1 \ln XN + c_2 \ln HC + c_3 t + c_4 t^2$$

$$(8) \quad \ln L_{atc}N \equiv d_0 + d_1 \ln XN + d_2 t + d_3 t^2 ,$$

where normal output ( $XN$ ) was substituted for actual output ( $X$ ),  $\{c_i\}$  and  $\{d_i\}$  were the estimated coefficients for  $\{\gamma_i\}$  and  $\{\delta_i\}$ , respectively. Finally, normal unit labour cost for operative labour ( $ULCN_{op}$ ) and for administrative, technical and clerical workers ( $ULCN_{atc}$ ) were defined by identities (9) and (10), respectively:

$$(9) \quad ULCN_{op} \equiv (AWEN \cdot L_{op}N) / XN$$

$$(10) \quad ULCN_{atc} \equiv (S \cdot L_{atc}N) / XN,$$

where  $S$  denoted salaries per head.

Note that this process of constructing normal variables for direct unit cost was wholly axiomatic and had little to do with what *firms* might have considered to be 'normal.' First, the definitions depended solely on the perception of Nordhaus and Godley, who alone specified the list of carriers, classified them as reflecting either trend or cyclical influences and provided the functional forms for the different equations. Choosing the time period presented a second problem. Nordhaus and Godley estimated the trend in direct unit labour cost on the basis of actual data for the period between 1953 and 1969. Unfortunately, this estimated trend could not have been very useful for pricing *decisions* made in that period. For instance, how could firms in 1953 (the first year in the sample) determine their normal price on the basis of a *future* trend? Clearly, during the early years of the sample, businessmen had no way of knowing what the subsequent trend would be, and that would have been true even if they happened to meet Nordhaus and Godley at the time! Furthermore, if current pricing could be based on future developments, why should firms in 1969 (the last year in the sample) be satisfied with data for the 1953-1969 period and not wait until they have a more 'complete' data set extending until the year 2000, for example? Also, why should companies operating in the 1950s and 1960s insist not to rely on data for years prior to 1953? And if they used earlier data, how far back did they go? By choosing 1953 as a starting point for the trend, Nordhaus and Godley imposed their own bias with very little explanation for why this should have been preferred over alternative dates such as 1920 or 1880, for instance. Finally,

the authors did not explain how a single pricing procedure could be adequate for every firm in the British manufacturing sector. They also failed to clarify why the relative size of firms or the industrial structure in which they operated were extraneous for pricing.

The derivation of normal variables was also plagued by technical problems. Following their specification and estimation for these variables, Nordhaus and Godley (p. 861) concluded that

We can be confident that all reversible cyclical effects have been purged from these series; the only variables entering normal cost are basic weekly rates, standard hours, salaries per head and time.

This confidence was unwarranted for several reasons. First, the regressions specified by equations (1), (3), (5) and (6) were not the ones estimated with the actual data! In practice, the authors felt free to amend their original specifications. Equation (1) was estimated twice, for men and women. In the equation for women, 2 lagged terms for capacity utilization were added 'on grounds of plausibility' (p. 857); Equation (3) was specified in levels, but then estimated in first differences (p. 858); Equation (5) contained one variable for output and one variable for customary hours, but the estimated equation included 5 additional lagged variables for output and 3 additional lagged variables for customary hours (p. 860); finally, Equation (6) had one variable for output, while the estimated version had 7 additional lagged variables for output (p. 860). These transformations were the outcome of extensive econometric experimentation and the authors' 'preferred equations' were chosen on the basis of unclear econometric criteria. Most importantly, both the transformations and the final selections had no apparent relation to *actual* pricing processes in the British manufacturing sector.

Second, even if we neglected the process of 'data mining,' the empirical results still left much to be desired. The goal of purging *all* cyclical components from the time series was not really achieved. By using the standard least-squares method of estimation, Nordhaus and Godley assumed that the mean value for the error term in each regression was zero and, hence, that the impact of this term was entirely cyclical. The assumption was obviously arbitrary but its potential effect on the estimation of trend could have been ignored, provided the average size of the error was sufficiently small. One way to evaluate this decomposition into trend and cyclical components, is to examine the coefficient of multiple correlation reported for each equation (Nordhaus and Godley reported values for  $\bar{R}^2$ , the

coefficient adjusted for degrees of freedom). Note that, while this coefficient should not be used as a criterion in the testing hypotheses, in this case, where the aim was to 'decompose' the series into trend and cyclical components, it might be quite useful. The figures indicated that only one regression (for customary hours) 'explained' over 95 percent of the total sum of squared deviations in the dependent variable (adjusted for degrees of freedom). In the other regressions, the value for  $\bar{R}^2$  varied between 0.79 and 0.10. For these latter regression we cannot share the confidence of Nordhaus and Godley in having 'purged' *all* cyclical variations. It is possible that alternative specifications with greater 'explanatory power' would have generated different estimates for the coefficients and altered the predicted trend.

Third, the decision to interpret actual non-labour items in prime cost as equivalent to their 'normal' values was justified by noting that, between 1954 and 1968, the volume of materials and services used per unit of output remained approximately constant. Unfortunately, the authors did not provide data to support this observation so it is hard to evaluate its plausibility.<sup>5</sup>

In the following stage of the analysis, Nordhaus and Godley attempted to estimate the lag between the incurring of cost and the setting of price. First, they assumed that markup prices were based on *historical* normal cost. In their opinion, this was a 'natural' assumption to make, partly on the basis of 'the widespread practice of evaluating stocks at cost on a FIFO basis' and, mostly, because that assumption 'has the particular advantage of enabling us to produce, by direct reference to facts, quantitative estimates of the lag structure which can then be imposed on the constructed cost series without any fitting procedure' (p. 862). In adopting the said assumption, the question of whether real firms indeed followed this practice was not even considered. Second, they assumed that costs of some materials entered the price in bulk at the beginning of the production process, while costs of other materials as well as fuel, bought-in services, indirect taxes and labour, entered progressively and evenly throughout the process. Gross profits were also assumed to enter progressively into the final price. On

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<sup>5</sup> In their subsequent study, Coutts, Godley and Nordhaus (1978) chose to alter their explanation for this decision. There they argued that a 'firm has no means of telling what is and what is not normal about changes in its raw materials. Although their costs are vaguely cyclical, they are *not reversible*; they do not automatically fall as the *firm's* capacity utilization falls, nor are they in any way under the firm's control' (p. 34).

the basis of these two assumptions, they demonstrated how the period of production ( $\Theta$ ) could be expressed as a function of total stocks ( $S$ ), quarterly sales ( $X$ ), the share of materials in sales ( $\alpha$ ) and the share of material cost entering in bulk at the beginning of the process ( $\beta$ ), such that

$$(11) \quad \Theta = 2S / X (1 + \alpha\beta) .$$

Values for  $S$ ,  $X$  and  $\alpha$  could be obtained directly from Census data. The value for  $\beta$ , on the other hand, was unknown to Nordhaus and Godley and they assumed it was equal to 2/3rds. They further presumed that, within each category of cost, prices for all inputs moved together. With these arbitrary assumptions they derived estimates for the period of production in each main industry group and, after accounting for inter-industry flows, computed the distributed-lag structure of price behind cost. Finally, by imposing this lag structure on normal unit costs, the authors derived '*historical* normal unit cost' for the sample period.

The last phase of analysis consisted of predicting the normal price ( $PN_t$ ) by using the following formula:

$$(12) \quad PN_t = (1963 \text{ mark-up}) \cdot (\text{Historical Normal Unit Cost})_t ,$$

where the '1963 mark-up' was computed as the 'ratio of total value of output in 1963 to total historical normal current cost in 1963.' The particular choice of 1963 as the benchmark year was not explained, perhaps because the authors felt it should not matter: 'The profits counterpart of the normal price hypothesis,' Nordhaus and Godley (p. 866) wrote, 'is that normal gross profits (that is profits at normal output, employment, etc.) should be a constant fraction of total value of sales.' 'If this theory of profit is precisely correct,' they added, 'the ratio of predicted price to actual price would remain constant.' With this in mind, the empirical results emerging from their detailed study seemed to have rejected the normal-price hypothesis. The data indicated not only that the markup of price over normal cost varied cyclically, but that it also experienced a long-term decline. After examining the actual and predicted series for both the price level and its rate of change (Figure 4, p. 867 and Figure 5, p. 868), Nordhaus

and Godley concluded that ‘The most striking fact is that the mark-up of price over normal cost has *fallen* over the period especially since 1961’ (p. 866, emphasis added). In other words, it appeared that the normal-price hypothesis was not even *approximately* correct!

The authors (p. 869) acknowledged they were unable to explain the secular decline in profit margins and, given this admission, one would have expected that the normal-price hypothesis would be rejected or at least modified. This did not happen, however, and Nordhaus and Godley proceeded to test the effect of demand on actual prices, presuming that the normal-price hypothesis was in fact correct! They estimated 100 different regressions where the actual price was regressed against a constant, the normal price and a demand variable.<sup>6</sup> Their categorical conclusion was that the average effect of demand on prices over the business cycle was ‘uncertain but small’ and probably did not exceed 0.1 percent of the price. The evidence in support of this conclusion were not very solid, however. Consider, for example, the authors’ ‘preferred test’ for the impact of demand as given in Equation (13):

$$(13) \quad \ln P_t = 0.001399 + 0.6248 \ln PN_t + 0.000238 \ln (X / XN)_t ,$$

where  $P_t$  was the actual price,  $PN_t$  was the normal price,  $X$  was output and  $XN$  was normal output. Since the test was conditional on accepting the normal-price hypothesis, the weakness of that hypothesis was manifest here too. Nordhaus and Godley were disturbed by the positive intercept which suggested that the actual rate of inflation was, to some extent, independent of the ‘normal’ rate of inflation. They were even more troubled by noting that the coefficient associated with the normal-price variable was significantly lower than unity:

The coefficient of predicted price is somewhat a puzzle. Our tentative hypothesis is that it is reduced below its *assumed correct value* of unity (a) because of incorrect lag estimates which mean, in effect, that  $[PN]$  is measured with error and (b) because of special factors in the second half of the period -- in particular incomes policy, nationalization of steel, and devaluation -- which *threw price from its normal relation*. (emphases added)

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<sup>6</sup> The specifications combined 10 alternative variables for demand together with 10 functional forms, where the variables were expressed in levels, first differences, linear and logarithmic forms and the equations were written with or without a first-order adjustment of prices.

The explanations provided in this quotation are interesting. First, the notion of having of ‘incorrect’ estimates for the lag structure is unclear. The only correct lag structure was the one employed by *actual* firms in the British manufacturing sector, but this was never explored by Nordhaus and Godley. Their notion of ‘correctness’ seemed to indicate consistency with the data rather than relevance to actual pricing procedures. The problem is that, with sufficient experimentation, we can *always* discover some lag structure which will be consistent with the normal-price hypothesis. Indeed, that was one way in which the researchers later attempted to fit their hypothesis to the data. But as Godley was later to recant (see below), the arbitrary way in which normal cost was defined, stripped the adjective ‘normal’ from any clear meaning. The second explanation is even more intriguing. If exogenous forces can operate to ‘throw’ price from its normal relation with cost for a *substantial* period of time, what is the meaning of ‘normal’ in this context? Can we insist on the assumption of a fixed markup when the ratio of price to normal cost keeps changing? How could we talk about an ‘assumed correct price’ here? Nordhaus and Godley do not address these question, but this is hardly surprising. Since the framework for normal pricing rests on the assumption of a fixed markup, such framework cannot be very useful in explaining why the markup changes.

The normal-price hypothesis for inflation is essentially a *technical* relationship between price and cost. Since the focus is on rates of change rather than levels, we can conveniently ignore the size of the markup and assume it does not change. The practice seems acceptable because our ultimate aim is not to discover how firms actually set their prices, but simply to *predict* the observed rate of inflation. The issue is not merely technical, however. A fixed markup means that we can explain inflation without explaining the markup itself and, thus, avoid the issue of distribution. It is hence hardly surprising that when the markup does change, advocates of the normal-price hypothesis often label such a change as ‘temporary,’ ‘autonomous’ or ‘exogenous.’ When changes persist, the tendency is not to reject the normal-price hypothesis but rather to *redefine* normality. Indeed, when their axiomatic model failed to produce sufficiently accurate predictions for actual price developments, Nordhaus and Godley sought to retain their general approach but alter its particular specifications.

In their subsequently study, Coutts, Godley and Nordhaus (1978) introduced two central amendments into their basic procedure. First, they expanded their sample by breaking the non-food manufacturing sector into 7 broad industry groups to be examined separately. Second, they declared that the assumption of historical-cost pricing used in the 1972 study was an extreme one. Instead of imposing the lag structure, they now proposed to 'test' it, by contrasting historical-cost pricing with alternative specifications for replacement-cost and average-cost methods.<sup>7</sup> The first amendment had the general effect of shortening the time lag of price behind cost because it eliminated the effect of inter-sectoral flows. The second change increased the flexibility of the authors in choosing the 'appropriate' lag profile. Unfortunately, these modifications failed to generate major improvements in the 'goodness-of-fit' of the normal-price hypothesis and did not eliminate the 'autonomous' drift in the markup.

The failure was indicated in Table 3.3 (p. 48), where the authors presented the estimated results for the following regression:

$$(14) \quad \Delta \ln P_t = \alpha_0 + \alpha_1 \Delta \ln PN_t + u_t,$$

where  $\Delta$  denotes first difference,  $P_t$  was the actual price,  $PN_t$  was the predicted 'normal' price,  $u_t$  was an error term and  $\{\alpha_j\}$  were unknown coefficients to be estimated. For each of the 7 industries, the authors estimated 3 equations where  $PN_t$  was constructed on the basis of either replacement cost, average cost or historical cost. For 4 of the industries, the regressions covered the period of 1957 to 1973, while for the remaining 3 industries, the data extended between 1963 and 1973. Coutts, Godley and Nordhaus (p. 48) felt that 'If we apply the test of goodness of fit and the closeness of  $\alpha_1$  to unity, the average-cost specification is very clearly superior to either of the other two.' This was only a relative assessment, however. The 'absolute' performance of the amended model, based on disaggregated

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<sup>7</sup> Under historical-cost pricing, a change in the price of an input affects only those units of input purchased after the change has occurred. Costs will be transmitted to the price only when the affected inputs emerge as part of the finished product at the end of the production process. Under replacement-cost pricing, a change in the price of an input affects units of that input throughout the production process and is, hence, transmitted immediately to the final price. The method of average-cost pricing is a hybrid of the previous two. The percent increase in the product price is computed by taking the ratio between the replacement value of all 'work-in-progress' before and after the change. For instance, if the replacement cost of work-in-progress is valued at \$200,000 before the cost increase and \$300,000 after, the rise in unit cost is said to have been 50 percent.

industries and 'normal average cost,' was still disappointing. The 'goodness-of-fit' which they measured by the value of  $\bar{R}^2$  was not very impressive (the average for the 7 industries was 0.59) and the estimated values for  $\alpha_1$  were generally significantly lower than unity. Yet the authors were in the opinion that the low estimated values for  $\alpha_1$  were 'not a matter for serious concern' (p. 49). The discrepancy, they contended, could be easily explained by the presence of measurement errors, mis-specifications of the lag structure and, most importantly, by 'missing variables.'

Hence, they devoted the remainder of their monograph to examine how demand, the shifting of corporate taxes, government prices policies, competing imports or world demand might affect the markup. It should be emphasized, that all of these tests for the impact of 'missing variables' were constructed on the assumption that the normal-price hypothesis (this time in its 'average-cost' version) was correct. Unfortunately, the inclusion of additional variables still did not seem to improve the 'goodness-of-fit' or provide a convincing explanation for the long-term decline in the markup. After a lengthy examination, Coutts, Godley and Nordhaus (p. 72) concluded that 'The effect of short-run changes in demand through the period of a typical business cycle as a separate influence on price, if it exists at all, is almost certainly no greater than 0.5 per cent from trough to peak.' The results concerning tax shifting were at best unclear: 'The most emphatic conclusion to be drawn,' they wrote, 'is that extremely little tax shifting occurs in the short term, defined as a (mean) lag of one year or less'; as for the long term, 'the data cannot resolve the question how much tax shifting occurs and over what period of time' (p. 96). They also concluded (p. 124) that although direct price controls have had some restraining effect on the markup (and, hence, inflation), their impact was only temporary and sporadic. Finally, they found conclusive evidence that, for the period examined, 'world demand has had no effect on prices relative to costs' and that 'the behaviour of competing import prices has had no significant effect on the price of domestically produced manufacturers' (p. 135).

The initial inability to explain much of the short or long-term changes in the markup, and the apparent insensitivity of the markup to a host of external stimuli were disturbing to Coutts, Godley and Nordhaus. Yet, since the normal-price hypothesis was presented as a technical explanation for prices and was independent of underlying social and power structures, the 'markup mystery' must also be reasoned

as a technical phenomenon. For that purpose, the authors returned to the field of corporate anthropology. Business firms, they argued, could be characterized in reference to 3 'ideal types':

[W]e shall call a firm 'neoclassical' if its objective is the maximization of its net worth; 'managerial' if its objective is broader, including objectives like safety, growth, or size; and 'behavioural' if it has an *inconsistent* set of objectives, or perhaps *no well defined objectives at all*. (p. 96, emphases added)

Given this taxonomy, Coutts, Godley and Nordhaus felt their own results for British manufacturing were consistent with the 'behavioural' model:

In particular, firms appear to have very limited and specific rules about their processes of short-run price behaviour, rules based essentially on their average normal cost of production. The rules do not appear to be complex in that they do not respond automatically and in a significant way to the state of demand, . . . to the price of competitive imports, or to corporate taxation. (p. 96)

The *a priori* presumption of fixed markups clearly pushed Coutts, Godley and Nordhaus into a methodological corner. Given that firms could not decide on changing their own markups, the only remaining explanation was that they simply failed to react. The problem was that such rationale was inconsistent with the very thrust of normal pricing. The latter was a theory of how firms *responded* to external changes but the 'behavioural' firm was defined here as a firm which, to a considerable extent, *failed* to respond! Coutts, Godley and Nordhaus were not deterred by this apparent inconsistency, however. Instead, they chose to explain how the 'response instinct' somehow generated inaction.

The 'behavioural' corporation, they suggested, operated under the stresses and challenges of a hectic business environment. In order to cope with these complex demands, the corporation employed a computer program (or behaved 'as if' it used one) which told its officers what to do. The program contained variables which changed frequently (like wage rates and capacity utilization) but excluded variables which did not change very often (like government anti-trust policies or corporate tax rules). Despite the power of modern computers, the authors (p. 98) maintained that 'the typical computer routine for pricing is very simple and not responsive in an optimizing way to fairly frequent environmental shocks.' Furthermore, 'The response of the firms will be different for those variables which are included in the computer programs from those that are not, and indeed the observed response may change over time as certain decisions move into and out of the computer programs.' For example, the long lag of tax shifting (8 to 10 years, in their opinion) could not be explained by 'corporate drowsiness' because firms were very responsive to changes in other variable such as wage rates. Instead,

The best explanation for this discrepancy is that firms simply are not 'programmed' in a consistent way to react to changes in company taxation, and that it is not until they are woken up by some other events -- such as inability to finance investment or pay out dividends, or low rates of return -- that they react in their pricing and investment policy so as to raise their net profit margin. (pp. 98-9)

Note that this fantastic computer fairy-tale did not necessarily mean that firms resorted to 'sub-optimal' behaviour. If we were to remove their 'behavioural disguise' we might have found what Coutts, Godley and Nordhaus called 'superoptimizers,' firms which in fact

calculate what to include in their programs and what should be excluded, taking into account the costs of decision making and the uncertainties of their environment, but once these programs are 'written' firms may behave in *apparently* non-optimizing ways. (p. 99, emphasis added)

Given its *ad hoc* nature, the concept of 'programmed inaction' by 'behavioural' firms was adopted only as a temporary rationale for unexplained variations in the markup. It was abandoned during the 1980s, after Coutts, Godley and Moreno-Brid (1987) were able to redefine their normal-price hypothesis in a more successful way.

The relative tranquillity of the 1950s and 1960s was followed by the turbulent period of the 1970s and 1980s, and the authors (p. 3) felt it was time to use 'new concepts of costs and profits' in order to bring their earlier studies 'up to date.' First, they were no longer sure about what exactly constituted *the* trend. They observed that, while until the mid 1970s, output, employment and hours had all fluctuated closely around 'well established long term trends,' this were no longer true in the subsequent period. From the mid 1970s onward, the relation between output and productivity was no longer 'consistent' with earlier experience. Since firms were assumed to view 'trends' as being in some sense 'normal,' the question now arose of 'what for the purpose of making their price decision, can firms have regarded as normal during the period since 1974?' (p. 5). Coutts, Godley and Moreno-Brid admitted that 'unfortunately there can be no clear answer to this question, because the deviations from earlier trends have been so large and prolonged' (*ibid.*). Consequently, there was also no point in hiding behind econometric estimation for trends, and the authors simply resorted to an 'as-if' assumption. In particular, they stipulated that 'firms considered as normal the costs which would have obtained had productivity moved smoothly between 1974 and 1985' (*ibid.*). Since the arbitrary basis of normal cost was now an open secret, there was no reason to refrain from making further arbitrary, yet highly convenient improvements in the model.

The second amendment concerned the proper time lag of price behind cost. After moving from historical cost in Godley and Nordhaus (1972), to average cost in Coutts, Godley and Nordhaus (1978), the present authors took the next logical step and adopted 'replacement cost' as the adequate basis for pricing. The justifications for earlier choices were now conveniently disposed of:

[W]e can now see no good reason to suppose that the markup will be on historical costs. The whole notion of markup-pricing does, after all, imply a high degree of price administration. Business firms should be in an excellent position to measure, and often accurately to forecast, the movement of most of their own costs. Does it really make sense to suppose that any systematic lag arises because of inertia? Why should there be any lag at all? Should we not rather expect that changes in price sometimes precede changes in costs? (p. 6)

Unfortunately, this seemingly plausible explication also serves to undermine the normal-price hypothesis: If we assume that firms can accurately predict future developments, that they have a high degree of administrative power and that they can raise prices before cost increases, why should we assume that these firms have to follow 'normal' cost and maintain a fixed markup? The authors did not address these questions. Instead, they moved ahead with additional 'improvements' to their normal-price hypothesis.

The third amendment was in the definition of costs and profits. While earlier the authors insisted that prices were marked over *direct* cost only, now they proposed that the markup was set over *total* costs which included -- in addition to direct costs -- also depreciation, inventory valuation and interest charges. The relation between price and costs was expressed by the following equation:

$$(15) \quad P = (1 + k) \cdot (1 + \sigma r) C ,$$

where  $P$  was unit price,  $k$  was the markup,  $\sigma$  was the inventory/output ratio,  $r$  was the real rate of interest and  $C$  was total normal cost per unit of output. To test this hypothesis, the authors used the following semi-logarithmic equation [assuming that  $\sigma r$  was approximately equal to  $\ln(1 + \sigma r)$ ]:

$$(16) \quad \ln P = \alpha_0 + \alpha_1 \ln C + \alpha_2 \sigma r + u ,$$

where  $\{\alpha_i\}$  were unknown coefficients to be estimated and  $u$  was an error term. For the period between 1967 and 1985, the least-squares estimate for  $\alpha_1$  was 0.97 and Coutts, Godley and Moreno-Brid felt this number was sufficiently close to what the normal-price hypothesis suggested. They also tested and found that, despite its violent fluctuations, demand had no effect on the relationship between price and normal cost.

This seemed to have finally provided the long-sought vindication for the normal-price hypothesis, yet, to their dismay, the authors discovered that a parallel model, containing a variable for *actual* instead of *normal* unit cost, produced a better fit with the data! Furthermore, with actual costs as a carrier, demand changes seemed to have had a positive and statistically significant effect on unit price. The discovery again reshuffled the anthropological cards:

Some people may prefer to interpret this result to mean that firms set prices on actual costs . . . and add a flexible mark-up which adjusts with the state of demand. But on any interpretation our results say that demand has a very small influence on price compared with that of costs. The limitation of our methodology is that although our tests of normal cost pricing imply that demand effects are no larger than the impact of the cycle on unit costs, it cannot at the aggregate level establish whether our interpretation of how firms set prices is correct. (p. 26)

Frustrated with their results, Coutts, Godley and Moreno-Brid pondered on the prospects of ever 'proving' the normal-price hypothesis. After 15 years of research, they discovered that real firms might not share the researchers' own perspective of 'normality' and concluded it was quite unhelpful to presume they did:

Our suggestion as to how entity profit should be defined and measured stands independently of any empirical results. On the other hand we find ourselves unable to draw conclusions as strong as we would wish about how prices are determined, probably because *we* have not been able to define and produce estimates of 'normal' costs which we can be confident were the costs which *manufacturers firms themselves* took to be normal. . . . It looks very much as though by dint of data mining we could find some estimate or other of normal cost which would follow fairly closely the movement of actual costs and which, as a result, would at once perform satisfactorily in a regression competition with actual costs and also be smooth enough to avoid any effects coming from demand. But the results of such excavation would not really add anything to knowledge. (p. 31, emphases added)

It seems that statistical tests for 'normal pricing' involve a joint hypothesis about business behaviour and price behaviour. First, these tests suggest that, on the aggregate, the conceptions of manufacturing firms about what constitutes 'normal' cost correspond to definitions supplied by the

researcher. Second, they state that, on the aggregate, manufacturing prices are set at a fixed percent markup above 'normal' cost. Clearly, the second part of the hypothesis is meaningful only if the first part is correct but, since this cannot be demonstrated by conventional statistical tests, the entire hypothesis becomes impossible to prove. The methodological difficulties explored in this section have failed to deter most structural theorists, however. Indeed, over the years, the normal-price hypothesis has been integrated into a broader framework where it was linked with the underlying structure of individual industries. We examine one such study in the following section.

#### 4.3 'Price Smoothing' and Industrial Structure

Many researchers felt that the aggregate treatment of manufacturing prices left much to be desired. While most manufacturing firms operated under conditions of 'imperfect competition,' the extent of 'imperfections' varied widely across industries. It was thus important to go beyond the aggregate view and examine whether interindustry variations in the *degree* of competition had a systematic effect on price dynamics. In the voluminous empirical literature on the issue, researchers have usually followed the footsteps of Means' original 'concentration thesis' and used some measure of sellers' concentration as an index for 'competitiveness.' (Other proxies for competition have also been used but only to a lesser extent.) Based on their empirical results, the majority of scholars tended to conclude that concentration reduced the 'responsiveness' of prices to both demand and cost.<sup>8</sup> Dalton and Qualls (1979, p. 26) summarized the prevalent view on the demand issue in following words:

In the short run, firms in highly concentrated industries tend to lag behind firms in less concentrated industries in adjusting prices to changes in market demand conditions. Having "lagged" behind, prices in concentrated industries may be adjusted later, even though the initial demand movements may have been halted.

Similarly, Scherer (1980, p. 356) concluded that, with respect to cost,

there is reason to believe that, at least since 1960, [the] price change sluggishness may have come from a tendency for concentrated industries to pass on, in the year they occurred, a smaller fraction of cost increases, and especially labor cost increases, than atomistically structured industries. Although the evidence is not as well developed as

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<sup>8</sup> Some of the important studies on the link between concentration and price responsiveness include Aaronovich and Sawyer (1981), Barrett, Gerardi and Hart (1973), Cagan (1975), Dalton (1973), Depodwin and Selden (1963), DeSilva (1971), Dixon (1983), Domberger (1979), Earl (1973), Eckstein and Fromm (1968), Eckstein and Wyss (1972), Laden (1972), Lustgarten (1975), Ripley and Segal (1973), Sellekaerts and Lesage (1973), Weiss (1966; 1971), Weston and Lustgarten (1974) and Wilder, Williams and Singh (1977).

it might be, this does not necessarily mean that such cost increases are not *eventually* reflected in higher prices; it only means that transmission lags may be longer in concentrated industries.

These conclusions proved puzzling to some extent. If industrial concentration was indicative of market power, should it not allow firms a *greater* flexibility in their response to demand and cost changes?<sup>9</sup> To settle the apparent inconsistency, many economists started to argue that price inflexibility was indicative of 'price-smoothing policies' and that, in a dynamic framework, such policies were in fact 'optimal.'<sup>10</sup> Clearly, the ability to pursue 'optimal-smoothing' policies depended on the market power of firms and this seemed to have shed a new light on the whole issue. The apparent positive association between industrial concentration and price inflexibility was no longer a theoretical embarrassment to those economists. It merely demonstrated how greater market power enabled a more optimal smoothing of prices.

Yet, these attempts to assign an aura of 'optimality' to sluggish price behaviour may have been somewhat misdirected. The attempts focused mainly on how firms reacted to market conditions and largely ignored the possibility that firms initiated price changes. The ensuing methodological difficulties are illustrated here in reference to a recent study by Encaoua and Geroski (1984) who examined the relationship between price dynamics and competition in Canada, Japan, Great Britain, U.S.A. and Sweden for the period of the 1970s.

According to Encaoua and Geroski, price smoothing could be viewed in terms of adjusting the current price toward some 'moving target.' The policy proceeded in two stages: one in which current

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<sup>9</sup> This question was raised in a series of studies by Qualls (1978; 1979; 1981). According to Qualls, the impact of competition on price responsiveness was highly nonlinear. Prices in atomistic industries responded quickly to current changes because firms lacked the market power to counter the invisible hand. In moderately concentrated industries, the mutual distrust and uncertainty about conjectural variations outweighed the potential for concerted action, so firms preferred the less risky course of price stabilization. In highly concentrated industries, however, the centripetal forces toward closer coordination outweighed the centrifugal forces of distrust and uncertainty. Since firms felt confident in pursuing short-term profit maximization, their prices became very responsive toward cyclical variations in cost and demand. Qualls (1979) examined the behaviour of price-direct cost margins for 79 U.S. industries over the period between 1958 and 1970. He found that, contrary to conventional views, the cyclical variability of those margins indeed tended to *increase* with the level of concentration.

<sup>10</sup> See for example recent works by Amihud and Mendelson (1983), Blinder (1982), Carlton (1979) and Philips (1980; 1983).

changes in cost and demand were translated to changes in the target price and, another, in which changes in the target affected the actual price. The extent of smoothing depended on the time-horizon for profit maximization which, in turn, depended on the state of ‘competition.’ Firms which could take a longer view (because they faced less competition) would smooth their prices extensively and respond mainly to changes in ‘normal’ cost and demand. On the other hand, firms which were forced to maximize short-run profits (because they confronted stronger competition) would hardly smooth their prices and respond mainly to current cost and demand. We begin by exploring the general, two-stage model for smoothing and, then, examine how it was used to identify the link between market structure and price dynamics.

The model for price smoothing included two basic equations. First, in any particular industry, the rate of change of the actual price ( $p_t$ ) was said to be a function of the rate of change of the ‘target price’ ( $tp_t$ ) and the rate of change of price in the earlier period ( $p_{t-1}$ ):

$$(1) \quad p_t = \delta_t tp_t + (1 - \delta_t) p_{t-1} ,$$

where the variable coefficient  $\delta_t$  denoted the ‘speed of adjustment of prices toward the target’ (p. 9). Second, the rate of change of the ‘target price’ ( $tp_t$ ) was defined as the sum of the rate of change of the desired markup ( $dm_t$ ) and the rate of change of normalized unit costs ( $nc_t$ ):

$$(2) \quad tp_t = dm_t + nc_t$$

Both  $dm_t$  and  $nc_t$  could not be observed and had to be replaced with ‘satisfactory proxies.’ Encaoua and Geroski asserted that the change in the desired markup ‘clearly depends in the first instance on demand conditions (appropriately normalized)’ and described this dependency with the following equation:

$$(3) \quad dm_t = \tau_t DEM_t ,$$

where  $DEM_t$  was the ratio of the change in inventories to the sum of production and stocks and  $\tau_t$  was an unknown variable coefficient. According to the authors,  $DEM_t$  provided ‘reasonably decent information on the larger current period demand shocks that firms face.’ The coefficient  $\tau_t$  in this equation captured the impact of demand variations on the rate of change in the desired markup. Similarly, the rate of change in normalized unit cost ( $nc_t$ ) was defined as a function of current cost and ‘other variables’:

$$(4) \quad nc_t = \beta_t + \alpha_t c_t .$$

In this function,  $c_t$  denoted the rate of change in current unit costs,  $\alpha_t$  was an unknown variable coefficient reflecting the impact the rate of change in current unit cost had on the rate of change in normalized unit cost, and  $\beta_t$  was the rate of change in normalized unit cost attributed to ‘all other factors’ (p. 10).

The model was developed as an axiomatic set of mathematical definitions and, in order to convert it into a convenient statistical format, several changes had to be implemented. The original specification with variable parameters indicated that smoothing coefficients could change over time. This plausible formulation was now abandoned, however, and all variable parameters were replaced by fixed coefficients! (Encaoua and Geroski did not furnish any explanation for this change of heart.) The original equations included non-observable variables and these were now eliminated by backward substitution of equations (3) and (4) into (2) and subsequently into (1). Finally, the researchers added an error term ( $u_t$ ) and obtained the following statistical function:

$$(5) \quad p_t = \Theta_0 + \Theta_1 p_{t-1} + \Theta_2 c_t + \Theta_3 DEM_t + u_t ,$$

where  $\{\Theta_i\}$  were unknown coefficients to be estimated.<sup>11</sup> Encaoua and Geroski (p. 12) argued that, by using coefficient estimates from this equation, the theoretical coefficients for the smoothing mechanism

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<sup>11</sup> Notice that if errors were added to equations (3) and (4), prior to substitution, the interpretation of  $u_t$  might differ, especially with pooled data.

could 'easily be identified.' Thus, we could estimate the 'speed of adjustment of current prices to the target' ( $\delta = 1 - \Theta_1$ ), the 'sensitivity of the target to current cost variations' [ $\alpha = \Theta_2 / (1 - \Theta_1)$ ], the 'sensitivity of the target to current demand pressures' [ $r = \Theta_3 / (1 - \Theta_1)$ ] and, finally, the 'rate of growth of the target independent of current cost variations' [ $\beta = \Theta_0 / (1 - \Theta_1)$ ].

Given this model, Encaoua and Geroski moved to the next task of assessing the impact of market structure on 'price responsiveness.' In each of the 5 countries, industries were grouped on the basis of one or more of the following criteria for competition: concentration ratios, the degree of foreign ownership and the extent of import penetration.<sup>12</sup> Equation (5) was then estimated separately for every 'industry group,' using the pooled time-series data of all industries in that group. The estimated parameters for each 'industry group' were tabulated as a basis for evaluating the significance of market structure for price smoothing. Based on this analyses, Encaoua and Geroski concluded that competition (as approximated by their 3 criteria) indeed made price changes more responsive to changes in current demand and cost:

[O]n the whole, price adjustment through *both* channels (the conversion of current shocks into targets, and the adjustment towards these targets) is slower in less competitive sectors. It appears that firms in less competitive industries are both slower to incorporate new information into their plans, and slower to adjust to whatever plans are made on the basis of this information. (p. 28)

In our opinion, these conclusions may be misleading for several reasons which we now consider.

The model contains serious flaws which make it hard to assign meaning to the different coefficients. First, Equation (1) stipulates that current price inflation (for the industry's product) is a function of current 'planned' inflation ( $tp_t$ ) and last period's inflation ( $p_{t-1}$ ), but the reason for this formulation is unclear. Why should firms in any particular year be concerned with the rate of price change in the previous year? Encaoua and Geroski argued that the extent of smoothing depended *only* on the relative impact of 'normal' as opposed to 'current' demand and cost changes, so how could it also

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<sup>12</sup> For instance, Japanese industries were classified into 4 groups on the basis of their four-firm concentration ratios. Thus, 17 industries were classified as having low concentration (0-40 percent), 16 were allocated to a low-medium group (40-60 percent), 18 were clustered in the high-medium category (60-80 percent) and 16 were designated as having high concentration (80-100 percent). Similar classifications were used to group industries in the other countries and, in some cases, more than one criterion for competition was used.

depend on the past rate of inflation? As it stands, Equation (1) implies that the rate of inflation in any industry has its own momentum, independent of whether price smoothing is extensive or not. Such momentum might very well exist, but it has no theoretical basis in Encaoua and Geroski's argument about *optimal* smoothing. In the context of this model, it could only be interpreted as a *non-optimal* component of current inflation.

Second, given that last period's inflation has an independent effect on current inflation, why should this impact be proportional to  $1 - \delta_1$ ? For that matter, why should the impact of the target rate of inflation and of last-period's inflation be related in any particular way? The imposition of this arbitrary constraint has interesting ramifications. In this model, a lower sensitivity to demand and cost increases (either current or normal) does not necessarily mean lower inflation. It only implies that a larger proportion of the on-going inflation must be attributed to unexplained 'inertia'!

Finally, equations (1) to (4) were written as axiomatic definitions, not as statistical functions with distinct and specific stochastic properties and, furthermore, they all contain non-observable variables. For example, the assertion expressed in Equation (2), whereby the rate of change in the 'desired' markup is a linear function of some ratio of inventory to stock, can be accepted or rejected as an article of faith. It can not be proven or refuted by resort to empirical evidence. The definition of 'normal cost' given in Equation (4) suffers from the same shortcoming. Consequently, the interpretation of  $tp_1$  as the rate of change in the 'target price,' and of  $\alpha, \beta$  and  $\tau$  as separate 'adjustment coefficients' toward such a target, are also axiomatic.

These observations lead to the simple question of whether we can in fact use Equation (5) to 'test' the link between market structure and 'price smoothing' as Encaoua and Geroski suggested. Note that the theoretical variables for 'price target,' 'normalized cost' and 'target markup,' disappeared from Encaoua and Geroski's final statistical equation. Instead, Equation (5) consists of a simple expression, where current inflation is written as a function of a constant, last period's inflation, current cost, current 'demand pressures' and an error term. Given the criticisms in the preceding paragraphs, it is hard to see how we can use estimates from this equation to 'easily identify' the various 'smoothing parameters.' The

criticisms do not imply, however, that the estimated parameters for Equation (5) are useless.

The summary tables indicated that, in industries with higher concentration ratios, greater foreign ownership or smaller import competition, inflation was commonly less 'responsive' to changes in current demand or costs, and these results appeared to be consistent with the idea of 'price smoothing.' Yet, being interested only in the impact of market structure on price 'responsiveness,' Encaoua and Geroski failed to notice the another important result emerging from their tables. Inflation in the less competitive industries seemed to have had a 'life of its own.' Indeed, in all 5 countries, the impact of last period's inflation on current inflation ( $\Theta_1$ ) and the rate of inflation attributed to 'all other factors' ( $\Theta_0$ ) increased dramatically as the degree of competition decreased. This behaviour is somewhat puzzling. One may ask why, as firms became less responsive to cost and demand, their inflation became increasingly 'autonomous'? What was the source for this 'extra' inflation in less competitive industries? The answer to these questions may require us to transcend the scope of Encaoua and Geroski's framework.

The emphasis of this and similar models on 'responsiveness,' serves to blur another possible link between market structure and inflation, namely, the ability of firms with market power to *initiate* price increases. It is possible that firms in concentrated industry appear insensitive to increases in current demand and costs simply because their price increases preceded rather than followed those changes. But under these circumstances, traditional analyses focused on reaction cannot identify initiative. Instead, such initiatives will be mistakenly interpreted as 'irresponsiveness' or unexplained inflation attributed to 'other sources.'

#### 4.4 'Pull-Push' Spirals

The stagflation episodes in the 1950s created a renewed interest in 'administered prices' and revived the old controversy between 'demand pull' and 'cost push' theorists. Commenting on the debate, Ackley (1959) argued that the very distinction between demand and cost inflation was quite unhelpful

toward understanding the inflationary process in modern capitalism.<sup>13</sup> The demarcation between the two varieties, he noted, hinged on a presumed causal sequence between cost and prices:

In our model of demand inflation . . . buyers of final output are attempting to procure a larger total supply than can be produced. As a result, prices are bid up. To be sure, wages and other cost-prices may promptly rise, too; but it is important that the causal sequence is this: prices are bid up, costs follow. If the causal sequence is reversed -- if costs rise, and therefore prices rise -- we have the case of cost inflation. (p. 420)

Now, in most finished-goods industries, prices were administered by sellers' discretion on the basis of some cost-markup formulae and, hence, according to the above definitions, it would appear as if such industries experienced only cost inflation. Unfortunately, these standard definitions were misleading according to Ackley, because they considered only the direct impact of demand on prices and completely ignored the potential *indirect* effect of demand on administered prices. When demand for finished goods increased, Ackley wrote, firms attempted to purchase more raw materials and semi-finished goods and tried to hire more labour in order to quicken the pace of production:

Now if the materials of which sellers are trying to buy extra quantities are also priced by our administrative rule, their prices will not rise either unless their costs rise. This means that the excess demand for materials is passed backward through a chain of administrative prices until it meets one of the markets where excess demand cannot exist because price rises to eliminate it. . . . Thus we might have the result that, while demand inflation pressures do not directly raise prices which are administered by a markup rule, the effect appears to be much the same, at least to the extent that the pressures focus back on markets where prices do respond to excess demand. (p. 421)

The direct influence of excess demand on prices was particularly pronounced in markets for agricultural commodities and some raw materials, yet the precise impact of such demand pressures was hard to predict for two main reasons. First, prices in those markets were influenced by speculative activities and, second, adjustments in production, especially of agricultural commodities, were subject to cyclical patterns which were often independent of current market pressures. Excess demand also affected labour costs but not in the same manner that it influenced the prices of physical inputs. According to Ackley (p. 423), the money wage was 'one of the most clearly administered' of all prices and, hence, rising demand for labour (following an increased demand for commodities) had an only limited direct effect on wage costs. The more important impact was indirect and came through the 'strong tendency of wages, either by automatic formula or otherwise, to follow the cost of living.'

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<sup>13</sup> Similar arguments were expressed in the mathematical model of Duesenberry (1950) and in the analysis of Moulton (1958).

Thus, far from having no effect on inflation, a general excess demand for goods tended to raise administered prices. That, according to Ackley (pp. 424-5), occurred

when and to the extent that an excess demand for labor causes wage rates to rise faster than they otherwise would; when, and to the largely unpredictable extent, that increased market-determined agricultural prices raise the cost of living and thus wage rates; when, and to the largely unpredictable extent, that market determined prices for a few key raw materials are bid up.

Furthermore, the interaction of 'demand pull' and 'cost push' often tended to develop into a 'pull-push' inflationary spiral:

[T]o the extent that these three forces combine to raise administered prices, the cost of living will be further affected, leading to further wage increases, further marking up of goods prices, and so on. The increased money incomes associated with inflation may also tend to cause those prices which are market-determined to rise further, as higher money prices may be needed to keep these markets cleared. (p. 425)

Although their arguments were different, Ackley concluded much like Galbraith (1957), that the combination of modern oligopolistic structure and excess demand bred moderate but continuous inflation. Because the indirect effect of demand on prices was generally not very rapid,

the process may continue for some considerable period after the original source of excess demand had been eliminated; and, further, that the movement has large element of irreversibility, since money wage increases, once granted, will tend to support a generally higher level for the market-determined prices. Of course, if an excess demand for raw materials is replaced by an excess supply, their prices will fall; but they are not likely to fall as far as they had previously risen. (p. 425)

In the 1950s, most economists viewed stagflation as a perplexing yet atypical phenomenon. Theorists like Ackley felt it was necessary to explain why inflation could *coexist* with stagnation but, in general, they did not attempt to establish *causal* relationships between the two. This line of thinking started to change during the 1970s, when stagflation seemed to become the norm rather than the exception in mature capitalist economies. Increasingly, prominent economists such as Hicks (1975), Kaldor (1976; 1983), and Sylos-Labini (1982) suggested that the same structural forces which generated 'pull-push' inflation were also responsible for stagnation.

According to Kaldor (1976), the simultaneous outbreak of inflation and recession in all major industrial countries during the 1973-75 period, indicated that the roots of the crisis were international in nature. The key toward comprehending these international aspects, Kaldor argued, was a proper structural perspective for the world economy. His analysis began by identifying two broad sectors: a

'primary' sector which provided agricultural staples, energy and basic materials, and an 'industrial' sector which included both 'secondary' industries for consumer and producer goods and 'tertiary' industries for services.<sup>14</sup> The source of contemporary instability, Kaldor claimed, stemmed from the relationship between these two sectors:

Continued and stable economic progress requires that the growth of output in these two sectors should be at the required relationship with each other -- that is to say, the growth of saleable output of agriculture and mining should be in line with the growth of demand, which in turn reflects the growth of the secondary (and tertiary) sectors. (p. 704)

But, then, from a *technical* standpoint,

there can be no guarantee that the rate of growth of primary production, propelled by land-saving innovations, proceeds at the precise rate warranted by growth of production and incomes in the secondary and tertiary sectors. (pp. 704-5)

According to conventional theory, the synchronization of growth rates in the two sectors should have been brought about through changes in the 'terms of trade' (relative prices) between primary and industrial commodities:

The more favourable are the terms of trade to agriculture and mining, the more current technological advance will be exploited through new investment, and the faster the growth of output. If the growth of primary production runs ahead of the growth of industrial demand, the terms of trade will move in favour of industry: this, in theory, should stimulate industrial growth and thereby the demand for primary commodities, whilst retarding the growth of production of primary commodities. (p. 705)

Unfortunately, this desired adjustment often failed to occur because the price mechanism did not perform its task. The reasons for the malfunctioning could be clarified by examining the nature of pricing in each sector. Industrial prices were generally administered by markup formula and, hence, were insensitive to changes in demand conditions. This meant that the necessary adjustments in the 'terms of trade' could be achieved only through changes in the prices of primary commodities. Yet even this could not be accomplished because the price mechanism failed here again. Instead of inducing the necessary alignment, the behaviour of commodity prices in the primary sector constituted a detriment to growth and contributed toward aggravating the inflationary process.

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<sup>14</sup> This distinction between two main sectors as a basis for analyzing macroeconomic developments began with Means (1935a) and then reappeared with slight variations in many important writings such as Kalecki (1943), Hicks (1965; 1974), Sylos-Labini (1969), Robinson (1977) and Okun (1981).

The adjustment problem arose for two principal reasons. First, when prices for primary commodities fell, they moved the terms of trade against primary producers, but when they rose, the improvement in the terms of trade for primary producers was only short-lived. In the latter case, industrial producers increased their own prices to cover rising material costs, and these increases were 'blown up' by the successive compounding of profit markups. Moreover, the accompanying increase in consumer prices exerted pressures on wage demands, which were further strengthened by the rising share of profit in industrial value added. Given those forces, the original increase in relative prices for raw materials was fairly quickly reversed by the consequent onset of administered-price inflation in the industrial sector.

Second, as already indicated by Ackley (1959), commodity prices were subject to variable time lags in adjusting to excess demand or supply. More importantly, their movements often reflected the additional influence of speculative expectations on the holding of stocks. Those factors contributed to make commodity prices fairly erratic and, according to Kaldor, such instability constituted a serious impediment to industrial growth. Consider, for instance, the impact of a sudden and substantial jump in commodity prices, followed by a rapid inflation of administered prices. If the resulting pull-push process redistributes income in favour of the industrial sector, it will cause a decline in the primary sector's demand for industrial output. Furthermore, the severity of inflation is likely to push governments toward restrictive demand policies with the repercussion of further declines in overall industrial demand. On the other hand, when there is a significant income redistribution in favour of the primary sector (like the initial accumulation of petrodollars by oil-producing countries during the 1970s), only part of this redistributed income will be used to demand industrial output. Again, the effect on industrial growth is negative.

For these two reasons, Kaldor (p. 706) argued that *any* large change in commodity prices (whether it was in favour or against the primary sector) was potentially harmful for industrial growth:

The emergence of commodity surpluses which should, in principle, lead to accelerated industrialization may have a perverse effect by diminishing effective demand for industrial products. Similarly the emergence of shortages which should accelerate the growth of availabilities of primary products through improvements in the terms of trade may lead instead to an inflation of manufacturers' prices which tends to offset the improvement in the terms of trade, and by its dampening effect on industrial activity,

worsens the climate for new investment in both the primary sector and the industrial sector. (p. 707)

Hence,

If the above analysis is correct, the market mechanism is a highly inefficient regulator for securing continuing adjustment between the growth of availabilities and the growth in requirements for primary products in a manner conducive to the harmonious development of the world economy. (*ibid.*)

For Kaldor, the basic structural cause for international economic disharmony rested with the malfunctioning of 'price mechanisms.' Furthermore, the latent danger of maladjustment tended to increase with global economic integration.

Given this assessment, it was now necessary to explain the relative post-war stability and why the international crisis erupted only in the 1970s. According to Kaldor, the relatively smooth growth of industrial countries from after the Second World War and until the early 1970s was largely contingent on the remarkable stability of commodity prices. While agricultural technology advanced rapidly, the instituting of government price-support policies and national stock-piling programs in that period prevented the collapse of primary commodity prices, secured a modest growth of real income in the primary sector and, hence, supported the continuous expansion of primary sector's demand for industrial goods. But while real income in the primary sector was growing, industrial inflation acted to curtail the pace of that growth. Early inflationary pressures emanated from wage demands in industrial countries. Since the 1950s, workers began to set their income aspirations on the basis of 'comparability' with more successful labour groups. As a result, wage rates experienced ratchet-like increases and inflation started to rise slowly. The rate of price inflation rose further with the so-called 'wage explosion' during the 1968-71 period.<sup>15</sup> According to Kaldor, deductions from gross wage payments have been rising for a long period of time, and the consequent built-up of labour frustrations exploded during the late 1960s in an outburst of union militancy.<sup>16</sup> The acceleration of wage inflation was accompanied with an even faster growth in administered-price inflation of manufactured products. Inflation during the 1950s and 1960s was moderate but persistent and it gradually worsened the terms of trade enjoyed by primary producers.

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<sup>15</sup> See Nordhaus (1972b) for an early use of this term.

<sup>16</sup> See also Jackson, Turner and Wilkinson (1972).

This relatively stable process of redistribution ended abruptly in 1972. Prices for many primary commodities doubled and even tripled within a year and then started to fluctuate with unprecedented amplitudes. According to Sylos-Labini (1982), the new instability was brought by a 'structural change' which occurred in 1971, and transformed the relationship between commodity prices and industrial production. While the relative fluctuations in global industrial output exceeded those in commodity prices between 1956 and 1971, the situation was dramatically reversed during the subsequent period between 1972 and 1980. Sylos-Labini (pp. 150-1) estimated that the elasticity of commodity prices with respect to industrial production was only 0.9 in the first period but, in the following period, it rose to 2.4!

What caused this 'structural change' from relative stability to marked instability? Both Kaldor (1976; 1983) and Sylos Labini (1982) believed the crisis began in August 1971, when President Nixon officially ended the gold convertibility of the dollar and brought the Bretton-Woods system of fixed exchange rates to an end. The elimination of gold as the ultimate 'anchor' for value, Kaldor and Sylos Labini asserted, led to a marked increase in commodity speculation which, in turn, operated to amplify fluctuations in commodity prices. This latter point deserves some elaboration. According to Kaldor (1983), when professional traders held firm expectations regarding the 'normal' price of a commodity (in terms of gold, say), their buying and selling were counter-cyclical and, hence, tended to *lessen* price oscillations. The end of dollar convertibility impaired this general belief in 'normal' prices for primary commodities. The resulting uncertainty about future price levels enhanced the volume of speculative activity and, given the lack of a stable currency, traders increasingly turned to primary commodities as a hedge against inflation. Under these circumstances, the direction of commodity speculation became pro-cyclical and tended to *aggravate* price fluctuations. For instance, when prices of primary commodities were on the rise, speculators, seeking to hedge against this inflationary tendency, moved to increase, not decrease, their stocks, causing prices to rise even further. Given the international monetary instability, commodity speculation now became the driving force of inflation:

The very jumpiness of commodity prices shows that they are increasingly under the influence of inflationary expectations. The absence of any stable monetary medium which could serve as a hedge against inflation may well lead to spectacular increases in commodity prices, fed by speculations. (Kaldor, 1976, p. 712)

Furthermore, Kaldor predicted that

the problem of keeping inflation at bay will increasingly be at the centre of preoccupations of *all* industrialized countries, with untoward consequences in terms of waste of resources and unemployment. (*ibid.*)

According to Kaldor and Sylos Labini, stagflation during the 1970s and early 1980s resulted primarily from faulty 'market mechanism.' Given this assessment, it was clear that by rectifying these unfortunate 'mechanical' defects, we could go a long way toward solving the problem. Furthermore, since the problem was only technical, the solution could be effectively achieved by government intervention:

The primary need is to strengthen the adjustment mechanism between the growth of supply and demand for primary products. This requires that governments (or international bodies) acting singly or in concert should be prepared to carry much larger stocks than private traders are willing to carry on their own; and be ready to intervene in markets in a price-stabilizing manner. (Kaldor, 1976, p. 712)

The details of such proposal were already elaborated by Keynes. During the war years, he recommended the creation of an International Commodity Control Agency, which would act to stabilize the (then) chaotic arena of primary commodities.<sup>17</sup> Although Keynes' proposal was never seriously considered by international bodies, Kaldor (1976; 1983) believed it has remained the most viable solution for the problem at hand:

I remain convinced -- as I have been for a long time -- the most promising line of action for introduction of greater stability into the world economy would be to create international buffer stocks for all the main commodities, and to link the finance of these stocks directly to the issue of international currency, such as the S.D.R.s, which could thus be backed by, and directly convertible into, major commodities comprising foodstuffs, fibres and metals. Assuming these buffer stocks cover a sufficiently wide range of commodities, their very existence could provide a powerful self-regulating mechanism for promoting growth and stability in the world economy. (Kaldor, 1976, p. 713)

The principal operations of this agency were to be relatively straightforward. When there was excess supply for a particular primary commodity, the agency would increase its purchases and build up its stocks. This would support the price of that commodity and the income of its producers. The commodity purchases would be financed by the issuance of new international money (such as S.D.Rs) and be considered as net additions to world investment. The process would work in reverse when excess demand for the commodity developed. When this happened, the agency would sell some of its stocks.

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<sup>17</sup> See Keynes (1980, ch. 3).

As a result, there would be a corresponding reduction in net world investment and outstanding S.D.Rs, the rise in commodity prices would be checked and the redistribution of income from the industrial to primary sector would be moderated. According to Kaldor (1983, p. 30), linking the buffer stock with the issuing of S.D.Rs was particularly appealing for it would provide the world with a *basic money unit* that was stable in terms of *basic commodities*. In his opinion, reaching mutual stability for both basic money and basic commodities would be a 'tremendous achievement' because it 'would largely deal with the problem of chronic world-wide inflation.'

In summary, the 'pull-push' framework elaborated by Ackley, Kaldor and Sylos-Labini attributed the severe stagflation since the 1970s to a combination of structural deficiencies and exogenous misfortunes. The crisis was sparked by the end of dollar-convertibility, enhanced by commodity speculations and maintained by an asynchronous adjustment mechanism between the 'primary' and 'industrial' sectors. Yet, while this framework provides some valuable insights into global aspects of inflation and stagnation, its excessive emphasis on 'mechanisms' can be highly misleading. Once started, 'pull-push' inflation becomes a simple 'reaction process.' Industrial firms set prices in strict observance for fixed markup-rules. Their inflation is a mere reaction to cost increases emanating from the primary sector. Firms in the primary sector are equally submissive. Their prices obey the invisible hand and rise whenever demand exceeds supply. Hence, in both sectors, firms simply carry out the inflationary process, they do not initiate it. This interpretation raises two important issues to which we now turn.

First, according to the 'pull-push' framework, the inflationary surge in the early 1970s should have occurred regardless of 'autonomous' actions taken by the OPEC cartel and the 'Seven Sisters' (the 7 largest petroleum companies which dominated international oil at the time). Kaldor and Sylos-Labini would of course agree -- indeed they emphasized -- that activities of these actors were central to the onset of inflation in the 1970s, but this emphasis was extraneous to their basic theoretical setting. In the 'pull-push' framework, prices for primary commodities are demand-determined because the underlying markets are competitive. This is also what makes such prices so susceptible to the unsettling impact of speculation. Only when prices are determined by the free play of supply and demand could we expect the end of dollar convertibility to generate a speculative fervour. No such instability was observed in

oligopolistic prices. Hence, the competitive nature of the primary sector is quite crucial for the propagation of 'pull-push' inflation. In this sense, the oligopolistic feature of crude-oil pricing was not only extraneous, but also inconsistent with the basic theoretical framework advanced by Kaldor and Sylos-Labini.

The 'pull-push' framework can be deceiving because it only differentiates the 'primary' from the 'industrial' sector and fails to carry the disaggregation further. It is wholly inadequate to lump Exxon or Royal-Dutch/Shell together with a small mining firm or a tiny agricultural community, as comparable members of the same 'primary' sector. The former can and do take initiative in their pricing policies, while the latter cannot and do not affect prices; energy is a 'key industry' (to use Veblen's terminology) and affects every process of production, while most other primary commodities affect only one or few processes; the large petroleum companies have considerable political sway, while smaller primary producers are relatively powerless. These observations are particularly significant when we consider the suggestions made by Kaldor (and supported by Sylos Labini) to 'solve' world inflation by supplementing the market mechanism with an International Commodity Control Agency. Kaldor and Sylos-Labini may be quite wrong to believe that an international scheme to stabilize petroleum prices is a simple extension of agricultural price-support policies or stock-piling programs. Farmers are likely to welcome government attempts to stabilize prices over which they have no control to begin with; but Mobil, Exxon or British Petroleum will undoubtedly object to an international endeavour to stabilize the price of crude oil. The instituting of such a Commodity Control Agency would constitute a direct challenge to large petroleum corporations, OPEC countries and, in general, to any primary producer with substantial market power.

During the 1970s, there were several international attempts to control the prices for key primary commodities. One fundamental reason why the OPEC cartel was successful where other organizations failed, was its ability to secure the cooperation of all large petroleum companies. In 1974, bauxite-producing countries formed the International Bauxite Association, began to tax the transfer of ores by the multinational mining companies and, in some cases, acquired stakes in their local subsidiaries. The Association never became an effective cartel, however, partly because the large

aluminum oligopoly headed by Alcoa, Reynolds, Kaiser, Alcan, Pechiney and Aluswisse, remained hostile to its cause. Indeed, Australia and Brazil, the largest members of the Association, promoted moderate policies for the International Bauxite Association from fear of confronting the U.S.-based aluminum companies. Another illustration is the effort by the Council of Copper Exporting Countries (which included Chile, Peru, Zambia and Zaire) in 1974 to raise the price of copper by cutting world production. The copper oligopoly, dominated by Kennecott, Anaconda, Revere and Phelps Dodge, declined to cooperate and prices collapsed within a year.<sup>18</sup>

The significant role that large companies play in the 'primary' sector could shed some light on why industrial countries -- who have been able to cooperate on the issue of exchange rates -- have never agreed on the question of international commodity stabilization. Much like small farmers who welcome agricultural price policies, large firms in the primary sector are likely to embrace the international stabilization of exchange rates over which they have no control. The stabilization of their own prices by an International Commodity Control Agency is a different matter, however. While Kaldor and Sylos Labini view the creation of such agency as a desirable improvement to a faulty market 'mechanism,' large petroleum, copper and aluminum firms interpret it as a direct assault on their own sovereignty. Their objections -- latent or blatant -- may have contributed to the long stalemate in this area of international price stabilization.

Given these comments, we cannot accept the emphasis 'pull-push' theorists place on faulty 'mechanisms.' Speculative activity has most likely *exacerbated* the inflationary bias in commodity prices, but the discretionary actions taken by large corporations and by associations of commodity-exporting countries might have had an equally decisive impact on the course of commodity prices. In particular, the overall behaviour of commodity prices since the early 1970s seems to have been greatly influenced by events in the petroleum arena. In this industry, market power and international politics exert a much greater influence on prices than the free play of supply and demand. For this reason, it is important that we go beyond the conventional 'competitive' aggregation for the primary sector and consider the activities of principal actors.

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<sup>18</sup> See Barnett (1980, ch. 5).

The second weakness of the 'pull-push' theory stems from its emphasis on fixed markups for the industrial sector. According to Ackley (1959, p. 425), the important point was not that markups never changed, but only that such changes had no significant effect on inflation:

In determination of the individual seller and product markups, demand and competitive conditions play a major role. . . . But these demand and competitive factors operate primarily on the *internal structure* of markups rather than on their *average level*; and they operate *slowly*. At any given time, some markups may be gradually increasing, other narrowing; but this process of individual readjustment is, in my argument, largely independent of aggregate demand in the economy and of whether the price level as a whole is rising or falling. (emphases added)

Put somewhat differently, this explanation implied that, because markup pricing was merely a 'reaction mechanism' with prices being set as a linear transformations of costs, and because the average markup was relatively stable, industrial firms could play only a *passive* role in the inflationary process. Indeed, Sylos Labini (1979, pp. 198-200) lent further support to this view, by asserting that industrial firms generally lost from inflation. In periods of inflation, he argued, there was only a partial shifting of labour cost onto prices (because wage increases were not uniform around the world); furthermore, unit overhead cost, which, according to Sylos-Labini, was part of the markup, tended to swell during inflationary periods. In his opinion, both of these tendencies led to a progressive erosion of net profit margins in the industrial sector and proved that, under modern conditions, 'inflation is not normally advantageous to the firm.'

These views on the passive behaviour of industrial firms are of course common. They are nevertheless disturbing because Ackley, Kaldor and Sylos-Labini all recognize that in the *primary* sector, large firms can have an 'autonomous' impact on their price-markup. It is not clear why we should accept that petroleum companies could push prices 'on their own,' but still assume that large corporations involved with the production of processed food, automobiles, steel, or armament do not take similar initiatives. This popular conviction that industrial firms merely react to cost increases, or that their price initiatives are too insignificant to affect the course of inflation, is based, at least in part, on the apparent long-term stability of industrial markups. Unfortunately, the use of such evidence demonstrates a basic confusion between causes and consequences, a misunderstanding which also plagues 'wage-push' theories. We turn to these theories in the next section and explain why the relative long-term stability

of markups cannot provide evidence on the source of cost inflation.

#### 4.5 Money-Income Inflation: 'Wage Push' or 'Profit Push'?

While Kaldor and Sylos-Labini concentrated on the role of commodity prices in contemporary stagflation, Weintraub (1978) identified the source of malaise in workers' greed and 'impatient aspirations.' The model on which he based his conclusion was fairly simple and could be summarized with several key equations. In a closed business sector, the dollar value for the gross product, or money income ( $Y$ ), could be written as a function of real output ( $Q$ ) and the implicit price deflator ( $P$ ):

$$(1) \quad Y = P \cdot Q ,$$

or

$$(2) \quad P = Y / Q .$$

Equation (2) could also be rewritten as

$$(3) \quad P = y / A ,$$

where  $y$  was the gross money income per employee and  $A$  was the gross real output per employee, or average labour productivity. Hence, the rate of inflation (as measured by the percent change in the implicit price deflator) could be expressed both as the difference between the rates of growth of money income and real output, or as the difference between the rate of change of gross income per employee and the rate of change of average labour productivity:

$$(4) \quad (\Delta P / P) = (\Delta Y / Y) - (\Delta Q / Q) = (\Delta y / y) - (\Delta A / A) .$$

Based on these definitions, Weintraub (pp. 44-5) concluded that,

Regardless of money supply, money velocity, government expenditures, monopoly practices, import prices, or the volatility of inflationary expectations,  $P$  cannot be subjugated unless  $Y$  matches the  $Q$  tempo.

This, in his opinion, suggested that 'theoretical eminence and emphasis must be assigned to the imbalance of money incomes to physical output volume' as the general 'price-level destabilizer.' To further illuminate the cause of inflation, Weintraub went beyond this overall imbalance, focusing not on aggregate income in general, but on *workers' income* in particular. The reason for this emphasis was twofold. First, employee payments were the largest element of business cost, as well as the source for consumer demand and, second, labour was hired and paid *in advance* of sales, so the incurring of costs preceded the setting of prices. In this context, the ratio between average labour income and average labour productivity became the generator for 'price-level sparks' and this, in Weintraub's words, was 'the essence of the money-income theory of inflation' (pp. 39-40).

To persuade the reader of the validity of his approach, Weintraub reformulated the equations so they conveyed the crucial role wages and salaries played in the inflationary process. The level of nominal income could be expressed as a function of employment ( $N$ ), average labour income ( $w$ ) and the average markup of prices over unit labour costs ( $k$ ):<sup>19</sup>

$$(5) \quad Y = k \cdot w \cdot N .$$

Dividing both sides by  $Q$ , we obtain

$$(6) \quad P = k \cdot w / A ,$$

and hence

$$(7) \quad (\Delta P / P) = (\Delta w / w) - (\Delta A / A) + (\Delta k / k) .$$

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<sup>19</sup> Note that the markup  $k$  was also the reciprocal of the share of labour in total income, such that  $k = Y / wN$ .

Equation (7) contained the main ingredients of the so-called 'Wage-Cost Markup' theory of inflation (WCM). The equation indicated that any changes to the price level must operate through  $w$ ,  $A$  or  $k$  and, according to Weintraub (p. 62), this made the WCM theory sufficiently general to 'absorb all other explanations in a consistent way.'

Weintraub's main presumption was that the markup of price over labour cost was more or less fixed:

The WCM theory builds on the hypothesis of  $k = \bar{k}$ , or nearly so. Practically,  $k$  changes very little year-to-year or over the long run, so we may ordinarily ignore any fluctuations as inaudible  $P$ -noise. It is not vital that  $k$  holds rigid; what matters is that its annual variations are generally too minuscule to explain the  $P$ -surges that have occurred. Variations in  $k$  cannot account for the trebling of the United States (GNP) price level since 1946, or the 75 percent climb since 1967. Indeed, over the long term  $k$  has been falling and there is evidence over the last decade that  $k$  has slumped in the United States and in the United Kingdom, especially in recent years. This should have fostered *falling* prices rather than an intense surge in prices in that beleaguered country. (p. 46)

Given that  $k$  was 'practically subdued,' Weintraub (p. 62) concluded that the final cause behind persisting inflation in the post-war era must have been the increase in average labour income in excess of labour productivity. Yet this explication of wage increases outstripping productivity gains brought us only half way toward a full answer. A full explanation required that we go beyond the *how* and also explain the *why*. In answering this question, Weintraub did not embrace endogenous explanations and, unlike commodity cost-push theorists, preferred to disassociate his WCM hypothesis from market 'mechanisms.' In his opinion, the recent 'unruly income binge' was rooted in the 'autonomous' but rather decadent behaviour of workers:

[P]art of the explanation is undoubtedly attributable to the more permissive life-styles and the more hedonistic drives for instant gratification in material goods, sex, drugs, easy education, and rewarding careers. . . . To the ordinary citizen the obvious means to material riches consists in fingering 'more' in the pay envelope; while the quest for 'more' has never been absent in the economic person or the labor movement, it has been magnified to 'more and more' -- and more quickly. (p. 63)

A similar view was expressed by Wiles (1973), who argued forcefully against 'closed,' or 'determinate' models for inflation. Such models were deficient for they left no room for discretionary action by economic actors in general, and workers in particular. In Wiles' opinion, the price level did not emerge from some 'objective' economic forces, but rather depended on 'what numbers the trade union leaders pick out of the air when they make wage claims' (p. 392). Since those claims were 'entirely subjective,'

the price level was in fact unpredictable. Inflation in this context was triggered by 'absurd wage claims.' The nature and extent of such claims were greatly affected by slow changes in what Wiles called the 'national character':

In nations where governments mainly succeed each other by *coup d'état* we must expect wild conduct at (or under) bargaining tables. In a nation where the national character is plainly changing -- rising crime, sex-and-drug permissiveness, less self-discipline in dress speech and deportment, less respect for hard work, less religion, loosening of the nuclear family, breakdown of a deferential class structure, etc., etc. -- we must also expect less restraint at the bargaining table, less concern for consequences. (p. 392)

Weintraub (p. 63) maintained that his explanation for rising money incomes was not a 'blanket indictment' for workers. It was merely a 'recognition' of facts. Wiles (pp. 392-3) was similarly cautious: 'I do not disapprove *per se* of most of the changes listed, quite the contrary,' he insisted. His only claim was that 'good or bad, they raise prices.' This emphasis placed on the primary role of workers in generating inflation is disturbing. Our concern is not with ideological overtones but with shaky conceptual foundations which we now turn to examine.

Weintraub (pp. 54-5) distinguished between 'wage inflation,' which occurred when the rate of change in  $w$  exceeded the rate of change in  $A$ , and 'profit inflation,' which took place when the value for  $k$  was rising. The aggregate data indicated that the  $w/A$  ratio has been rising while  $k$  has remained stable and, according to Weintraub, this empirical evidence vindicated his theory of 'wage inflation,' while refuting counter-proposition of 'profit inflation.' Unfortunately, the proof was based on inconsistent definitions and was hence quite invalid. Throughout his book, Weintraub had repeatedly stressed that, because the average markup was more or less stable, 'wage inflation' had practically no effect on the aggregate distribution of income between workers and business firms. 'One way and the other,' he wrote, 'the wage share holds constant while inflation is recorded; real incomes, to be sure, still follow the productivity course' (p. 64). 'If the class struggle is the relentless issue,' he added, 'the war is fought over the wrong things in the wrong place and the wrong time,' for '[a]fter the smoke clears the only change is in  $P$ ' and 'labor wins nothing' (p. 110). But if there was no redistribution of income, nominal wages and profits must have been increasing at the *same pace*, so how could Weintraub insist there was only 'wage inflation' and no 'profit inflation'?

The answer to this question is quite simple. Weintraub mistakenly employed the term ‘profit inflation’ where alternative expressions such as ‘markup inflation’ or ‘income-share inflation’ would have been appropriate. It is rather trivial that if inflation has no marked effect on the distribution of income between firms and workers, the markup of price over wage cost must remain relatively stable. Indeed, if the markup does not change, Weintraub’s ‘wage inflation’ can be portrayed as the mirror image of ‘profit inflation.’ To illustrate this point, we rewrite Equation (5) in the following way:

$$(5a) \quad Y = l \cdot \Pi \cdot N ,$$

where  $Y$  denotes income,  $N$  is employment,  $\Pi$  is the average non-labour income (‘profit’) per employee [such that  $\Pi = (Y - wN) / N$ ] and  $l$  is the average ‘markup’ of price over unit profit, or the reciprocal of the share of profit in total income [such that  $l = Y / (Y - wN)$ ]. Dividing both sides by  $Q$ , we obtain

$$(6a) \quad P = l \cdot \Pi / A ,$$

and so

$$(7a) \quad (\Delta P / P) = (\Delta \Pi / \Pi) - (\Delta A / A) + (\Delta l / l) .$$

In reference to his own model as expressed here in equations (5), (6) and (7), Weintraub (p. 45) wrote that

[b]y positing (1)  $k = \bar{k}$ , or  $\Delta k = 0$ , primarily year-to-year as reenforced by factual evidence, and (2) imputing causal significance from right to left, from unit labor costs ( $w/A$ ) to  $P$ , the truism is transformed into a theoretical conjecture.

This same rationale, when applied to equations (5a), (6a) and (7a) with proper changes in the variables, yields the following explanation:

By positing (1)  $l = \bar{l}$ , or  $\Delta l = 0$ , primarily year-to-year as reenforced by factual evidence, and (2) imputing causal significance from right to left, from unit ‘profit’ ( $\Pi/A$ ) to  $P$ , the truism is transformed into a theoretical conjecture.

Based on this revised reasoning, it would seem that the recent ‘unruly income binge’ stemmed not from workers’ excessive demand but rather from the persisting ‘profit push’ of businessmen! How do these

two interpretations differ? Under 'wage inflation' the increase in unit labour cost occurs first. It reduces the markup of price over unit labour cost and this leads to a *subsequent* price increase which restores the markup to its previous 'normal' level. Under 'profit inflation' (not to be confused with 'markup inflation'), the order of events is reversed. First there is a price increase and a reduction in  $l$ . This is *followed* by a wage increase which raises unit labour cost and restores  $l$  to its previous 'normal' level. The two processes differ in their *causal* sequence, yet this difference is not always easy to identify in practice. When prices and wages change only occasionally, we may be tempted to use empirical observations as evidence for causality. During periods of inflation, however, when prices and costs chase each other in a seemingly endless spiral, cause and effect are welded into a closed circle and can no longer be distinguished by simple empirical observations.

Weintraub may be right in arguing that labour demands rather than profit aspirations provide the continuous spark for inflation. Yet, such a proposition cannot be proven by showing (as he attempted to do in Figure 3.4 on page 56 of his book) that the wage rate has been rising while the markup of price over labour cost remained stable. Using the same methodology, we can show that unit profit was rising while the share of labour income remained stable. We can then take this as evidence that there was only 'profit inflation' and no 'wage inflation.' Clearly, both of these 'proofs' are inadequate in dealing with a non-observable causal sequence. Changes in the markup can be used to illustrate the *consequences* of inflation but, in themselves, they provide insufficient information about the *causes* of inflation. Workers can initiate the process with their 'excessive' wage demands and end up with the same income share they started with. Similarly, firms can push prices in the hope of raising their profits, only to incur even larger wage increases which, eventually, reduce their profit markups below their original levels!

Given that *ex-post* markups provide little causal evidence, the essence of Weintraub's theory of 'wage inflation' is reduced to a simple *a priori* presumption about enterprising workers and inert businessmen. Like many other structuralists, Weintraub is also convinced that, whereas workers take initiative, businessmen merely 'act to *protect* their own profits from being eroded, and *counter* by raising administered prices directly *after* tabulating the wage pressures' (p. 64, emphases added).

#### 4.6 More on 'Profit Inflation'

The contention that price inflation is unlikely to emanate from a 'profit push' is quite pervasive. Bronfenbrenner and Holzman (1963), for example, devoted less than one page of their 68-page 'survey of inflation theory' to that possibility. Invoking the authority of Haberler (1959) and Hague (1962), they explained that

a profit-push is likely to be smaller than a wage-push partly because profits constitute a smaller part of price and because such a push is more likely to be 'once and for all,' whereas wage-pushes are more likely to be continuous. (p. 622)<sup>20</sup>

More than a decade later, Laidler and Parkin (1975) found the question of 'profit-inflation' sufficiently marginal to condense its discussion even further, into a 2-line footnote. No references were provided by Laidler and Parkin for, in their opinion, the issue has remained largely unexplored:

[A] question which has been raised but not answered is: do monopolistic firms exert an independent push on prices in a similar manner to that in which it is suggested that trade unions affect wages? (p. 766fn)

Although the literature on this question is indeed limited, it is quite important for our purpose and deserves more than a passing comment. Most explanations examined in previous sections shared the explicit or implicit assumption that oligopolistic pricing practices merely transmit inflation and do not create it. In this section we look at alternative theories which focus on the primal role of oligopolistic initiative and profit. The works of Blair (1974), Eichner (1973) and Kotz (1982) are particularly interesting and we consider them in turn.

The empirical literature on pricing practices commonly suggested that oligopoly price leaders set 'full-cost' prices in order to meet their target rate of return as a long-term average. The 'full-cost' price was set so that sale revenues would cover all costs and target profit when the company was producing its 'standard' volume. Assuming that the average volume over the cycle would equal this predetermined 'standard,' the company could ignore transitory changes in demand and still achieve its long-term objective for profit. This practice seemed to explain why oligopoly prices declined less than competitive prices in recessions and rose less in expansions. Since the early 1950s, however, oligopoly

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<sup>20</sup> See Scherer (1980, p. 353) for a similar expression of this view.

prices tended to rise not only in expansions, but also during recessions, and this latter 'perverse price flexibility' could not be easily explained by the long-run target principle. As an alternative, Blair (1974, p. 468) suggested a 'short-run target return model,' where a price leader would 'seek to attain its target objective not simply over the long run, with good and bad years averaging out around the target, but in each year.' This change of emphasis was significant for pricing practices:

In most manufacturing industries, of course, demand and thus volume do not remain unchanged over any considerable period of time, and it is when output is falling below the standard volume that oligopolistic price behaviour assumes its most anomalous form. An explanation therefore requires something more than a simplistic adjustment of price to reflect cost changes at a constant volume; it also must reflect the effect of changing volume on costs, profit margins, and price.

What was the relation between total unit cost and capacity utilization for a typical oligopoly? Blair argued that, as capacity utilization increased, total unit cost decreased continuously until a certain 'turning point' -- say 90 percent of capacity -- was reached. When capacity utilization surpassed this point, unit cost started to rise. This particular behaviour for total unit cost resulted from the separate effects of changing volume on the cost of materials, labour and overhead. As output increased toward the 'turning point,' the cost of raw material per unit remained unchanged. Unit labour cost, on the other hand, tended to fall, because increases in output levels raised labour productivity. Unit overhead cost also declined as total overhead expenses were spread across a larger output. Beyond the 'turning point,' all three elements of unit costs started to rise. This occurred because very high rates of capacity utilization were usually associated with an overall economic expansion when tight markets for raw materials, labour, and capital brought higher factor prices.

According to Blair, the 'standard volume' for the oligopolist was typically lower than the 'turning point' in unit costs, say at 80 percent of capacity. In a recession, when output fell below the standard volume (for example, to 70 percent of capacity), there was a narrowing of profit margins and a substantial reduction in total profit because both the markup and sales volume have declined. In order for the firm to realize its short-term profit target, the price had to be raised. Furthermore, the new profit-markup must be higher than before because the profit target had to be attained at a lower volume of sales. If operating volume continued to fall, the firm had to raise its price again.<sup>21</sup> While prices

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<sup>21</sup> Blair stressed that a price leader would usually wait until it was convinced the decline in the markup was not short-lived. Consequently, price increases were not continuous and happened in 'steps.' This kind of price behaviour was consistent with numerous observations made since the publication of

tended to rise in recessions, they did not fall in expansions. Instead, they either remained constant or increased. Starting from the 'standard volume,' an increase in capacity utilization caused unit cost to fall, but this did not induce a price reduction. Although the short-run target-pricing principle would have called for a lower price, the danger of triggering a price war was too serious to be ignored. Under these circumstances, the price leader would not lower the price and let its profit markup rise. The increase in profit margins and the fear of 'spoiling the expansion' would in turn work against temptations to ride the tide and raise prices. As a consequence of these countervailing forces, prices during the early stage of expansion would tend to remain stable. When output continued to rise beyond the 'turning point,' however, cost started to increase, putting a squeeze on profit margins. If the squeeze became sufficiently severe to endanger the attainment of target, prices would be raised.

The implication of this model contrasted with conventional views about structural inflation. In reviewing some of the structural literature, Beals (1975) concluded that the relative price inflexibility in concentrated as opposed to atomistic industries occurred in *both* the upward and downward directions. Although prices in concentrated industries fell less than atomistic prices in recessions, they also rose less in expansions and this, according to Beals, implied similar *long-run* behaviour for the two series. Blair rejected this conclusion because, in his model, oligopoly prices did not fall at all. Both competitive and oligopoly prices tended to rise during expansion, but while competitive prices changed their course and fell during recessions, oligopoly prices continued to increase. According to Blair (p. 466), this divergent price behaviour meant that, over the *long-run*, competitive prices would change by very little while oligopolistic prices would display a pronounced upward trend (p. 466). To support his argument, Blair demonstrated that over the cycle extending between December 1969 and December 1971, prices in concentrated industries (having a 4-firm concentration ratio higher than 50 percent) increased by 8.7 percent, while prices in atomistic industries (having a 4-firm concentration ratio lower than 50 percent) declined by 0.1 percent. Hence, contrary to common beliefs, the model and evidence seemed to suggest that the impact of oligopoly on long-run inflation was far from neutral. Indeed, according to Blair, the very cause for long-run inflation tendencies was the uncompromising exertion by oligopolies to meet their profit targets during recessions.

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Means' original study.

A closer look at Blair's conclusion reveals a certain inconsistency with his original assumptions about oligopolistic pricing practices. Given that oligopolistic industries use inputs produced in competitive industries, the cost of such inputs must enter into the oligopolist's calculations. If, as Blair concluded, prices of those inputs remained relatively stable over the long run, while prices for oligopoly output experienced a long-run rise, the rate of return for oligopolies could not remain 'on target' as hypothesized and must increase. This inconsistency could be easily resolved, however, if we recognized that the long-term upward trend in oligopoly prices spelled a positive trend in cost for competitive industries. These cost increases should then lead to at least some positive trend in competitive prices. Given these observations, a more plausible conclusion should be that both oligopoly and competitive prices would rise over time, only that the long-term rate of increase for oligopoly would outstrip that of atomistic industries. The criticisms do not change Blair's basic conclusion, however. Even when the long-term trend of all prices is positive, the source of that trend is the anomalous price behaviour exhibited by oligopolies during recessions.

It should be noted that, although Blair emphasized the role of oligopoly *profit* in the onset of inflation, the role he assigned to oligopoly *firms* was largely passive. Such firms changed their prices in response to changes in unit costs and they did so in order to meet some 'predetermined' target rate of return. Blair did not talk about 'profit-inflation,' perhaps because he implicitly assumed that the *size* of the profit target affected only the absolute level of prices and not their rate of change. Interestingly, this common assumption -- while valid for long-run target rate of return models -- was incorrect for the short-run version developed by Blair. In the former case, the firm aimed to meet its target at some 'standard volume' and, hence, the impact of 'normal-cost' inflation on price inflation was indeed independent of the target rate of return itself. When the firm tried to meet its profit target profit in the short run, however, the size of target exerted a *positive* impact on the rate of inflation, particularly when price increases occurred as a result of declining demand. To illustrate this point, consider a firm which produces 100 million units of a certain good at a unit cost of \$1. Suppose further that the short-run target for profit is \$100 million, so the firm needs to earn a profit of \$1 per unit and, hence, the price is set at \$2. Now consider a fall in volume to 50 million units with an accompanied rise in unit cost to

\$1.5. At this lower volume, the firm would need \$2 in unit profit in order to meet its short-run target and it would increase its price by 75 percent to \$3.5. Suppose now that instead of \$100 million as a short run target for profit, the firm wanted to earn a higher profit of \$200 million. In that case, the original price would have been \$3 (\$1 for unit cost and \$2 for unit profit at output of 100 million) and this would be increased by 83 percent to \$5.5, after the fall in volume (\$1.5 for unit cost and \$4 for unit profit at output of 50 million units). In other words, the higher target led to a higher *increase* in price. It is interesting to note that in a much earlier paper, Blair (1959, pp. 442-4) emphasized this impact of the short-run profit target on inflation. Drawing on the then-popular examples of U.S. Steel and General Motors, he suggested that attempts by these corporations to achieve their target rate of return at lower operating volume were equivalent to an increase in the target itself. In other words, 'perverse price flexibility' during recessions was at least partly affected by 'profit inflation.'

The view that increases in profit targets were the primary spark of inflation was explicitly developed by Eichner (1973; 1976). The key toward understanding how oligopolies affected inflation, he argued, was the 'plus' factor in their cost-plus pricing formulas. In his opinion, empirical evidence, particularly the hearings of the Kefauver Committee and the study by Kaplan, Dirlam and Lanzillotti (1958), clearly indicated that

the pricing decision, when some degree of market power exists, is ultimately linked to the investment decision; that indeed, under the circumstances, prices are likely to be set so as to assure the internally generated funds necessary to finance a firm's desired rate of capital expansion. It is this insight which makes it possible not only to provide the long-missing determinate solution to the oligopolistic pricing problem but also to reintegrate micro with macroeconomic theory. (1976, p. x)

How could this insight into the link between investment plans and the price level explain the onset of inflation? According to Eichner, the answer could be found by exploring how large firms financed an *expansion* in their investment projects. An increase in investment by such firms could be financed externally or internally. Additional external funds (over and above what was currently available) could be obtained by issuing new equity or by borrowing. The cost of such funds were determined by prevailing rates of interest. Additional funds could also be obtained internally, by increasing the flow of profit. This was done by raising the price (and the markup) above their previous level. The 'cost' involved with this latter method were more difficult to calculate and depended on the dynamic consequences following the price increase. The initial impact was a rise in revenues and profits over

their previous levels. With the passage of time, however, revenues and profits were likely to decline, mainly because consumers substituted for alternative products and also because new firms, lured by higher profits, entered the industry and reduced the market shares of existing firms. Eventually, profits would drop below the original level prevailing prior to the price increase. These 'foregone' earnings constituted the implicit cost of raising internal funds in the manner described. Given that the flow of both internal funds and implicit cost could be reasonably identified, the company could compute the implicit rate of interest associated with such fund-raising policy. This rate would be equal to the ratio of funds 'lost' in latter periods (properly discounted) to funds 'raised' in early periods (properly discounted). Note that the implicit rate of interest was not fixed and tended to increase with the amount of additional internal funds. The reason was fairly simple: progressive increases in the markup would yield diminishing returns in terms of additional funds being raised while, at the same time, aggravate the effect of substitution and entry on subsequent foregone earnings.

In deciding on its method of financing, the firm would chose the least costly method, namely the one with the lower rate of interest. This choice, according to Eichner, could explain the link between investment and inflation. While the minimum rate of interest on external funds was generally fixed at some positive rate, the minimum implicit rate of interest for internal funds was zero and increased with the amount of desired funds. This assumption was crucial. It meant that, *up to a certain point*, raising the markup was cheaper than raising external funds. Consequently, higher investment would be at least partially financed by higher profit and that called for higher prices. This was how growing investment sparked inflation. Eichner (1973, p. 1195) emphasized his model did not explain the price level for this was 'historically determined.' It only explicated the 'change in the margin above costs from one pricing period to the next.' This, he argued, was quite sufficient to resolve the issue of oligopolistic price movements, particularly after we took into account concurrent increases in wage rates which turned the original 'profit-push' impetus into a wage-price spiral.

Eichner developed his model before the severe stagflation of the 1970s and early 1980s and hence tended to view inflation as a *growth* phenomenon:

A change in the secular growth rate will, according to post-Keynesian theory, require an increase in the aggregate savings rate. As the 'cost-plus' pricing model just elaborated suggests, this increase in the aggregate savings rate is most likely to be

achieved through an increase in the margin above costs set by price leaders in the oligopolistic sector, the higher prices then being matched by the other firms in their respective industries. (1973, p. 1197)

Stagnation, on the other hand, tended to aggravate the inflationary process:

because the direct or 'out-of-pocket' costs of production account for only part of the price, the internal savings being generated in the form of cash flow will be highly sensitive to any difference between the expected sales volume and the actual sales volume. What this means is that while prices in the oligopolistic sector will be set so as to achieve a balance between planned savings and investment, actual savings and investment are quite likely to diverge depending on the extent to which the economy has been pushed off its secular growth path. (*ibid.*)

Yet, this incorporation of stagnation into the framework is rather forced. According to Eichner, large firms which found their financing plans hindered by unforeseen stagnation, would increase their markups again in order to obtain the still-missing capital. This scenario may be relevant when stagflation is viewed as an occasional dent in a vigorous trend of long-term growth, but it is not highly plausible for a period of prolonged stagnation. Large corporations are simply unlikely to pursue aggressive expansionary policies under the latter circumstances. Since inflation in this model is generated not by investment but rather by an *increase* in investment, Eichner's explanation must be viewed as inadequate for a protracted period of slump.

The works of both Blair and Eichner were criticized by Kotz (1982), primarily for their treatment of the 'profit motive.' Blair's model was found to be deficient for several reasons. First, it assumed that firms had the power to raise their prices but waited for recessions in order to exercise it. Blair explained this behaviour by arguing that, during expansions, firms were merely seeking 'satisfactory' profits but this was not very persuasive, according to Kotz. Second, the proposition that firms sought to achieve *short-term* targets was at odds with empirical evidence about large firms in concentrated industries. Finally, the size of the target profit and its determinants were left unspecified. Eichner overcame some of these shortcomings by emphasizing long-term investment strategies, but his model was still deficient because it explained only changes in the markup and not the markup itself. According to Kotz, Eichner also left open the question of what caused firms to suddenly seek a faster expansion.

Kotz agreed that target-return pricing was a dominant practice in the oligopolistic sector but insisted that, by itself, this practice provided only a partial basis for inflation theory. In order to ‘close’ the model, he argued, we must also explain the target itself. In his search for ‘determinacy’ (to use Wiles’ term), Kotz then brushed doubts which haunted the literature since the late 1930s and suggested we recognized -- as most Marxists and neoclassicists did -- that capital was ‘always seeking the maximum possible profit’ (p. 3). There was, of course, some ambiguity regarding uncertainty and time spans but, in his opinion, the ‘rough idea of pursuing the maximum possible profit, over some suitably defined long-run period, does seem applicable to large corporations.’ Given this presumption, the task now was to explore the objective determinants of this ‘maximum possible profit’ and how they affected the inflationary process.

Kotz constructed his model for inflation using the common dual-market framework for monopoly<sup>22</sup> and competitive industries.<sup>23</sup> Monopolistic industries enjoyed higher rates of profit than their competitive counterparts for two related reasons. First, firms in the monopoly sector colluded to set their prices above comparable competitive levels and, second, the resulting profit differentials were maintained by barriers to entry. According to Kotz, the general relationship between the rates of profit in the two sectors could be reduced to the following expression:

$$(1) \quad (100 + r_m) / (100 + r_c) = \delta ,$$

where  $r_m$  was the percent rate of profit in the monopoly sector,  $r_c$  was the percent rate of profit in the competitive sector and  $\delta$  was the ‘height of entry barriers,’ a ‘structural variable that determines the extent to which a monopolist can gain extra profits’ (p. 6). According to Kotz, this variable, which denoted the ‘*degree of monopoly power*,’ closed the ‘critical gap’ in markup-pricing theories for inflation.

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<sup>22</sup> The noun ‘monopoly’ is used by Kotz in reference to both oligopoly and monopoly. We follow the same convention in this section.

<sup>23</sup> Unlike many other inflation theorists, Kotz (p. 14, note 8) explicitly recognized the alternative dual-market framework based on *firms* rather than *industries*. The latter framework was preferred because price was seen as an ‘industry variable.’ (In the second part of this work, we argue to the contrary, that inflation is an enterprise-based phenomenon.)

Given the rate of profit in competitive industries ( $r_c$ ) and the height of entry barriers ( $\delta$ ), the *maximum* attainable rate of profit for monopolistic firms was given by the following expression:

$$(1a) \quad r_m = \delta (100 + r_c) - 100 ,$$

Any attempt to obtain a rate of profit higher than  $r_m$  would invite entry and defeat its own purpose. Settling for a lower rate, however, was equally irrational for more could be gained under the circumstances. Thus, according to Kotz, monopolistic firms would set their *target* rate of return ( $tr_m$ ) to equal the maximum attainable profit ( $r_m$ ) and, hence,

$$(2) \quad tr_m = \delta (100 + r_c) - 100 .$$

Given this target, the implications for pricing were straightforward:

[T]he monopolist, in order to gain the maximum profit rate compatible with deterring entry (and thus the maximum profit rate that is sustainable over the long run), would follow the 'limit pricing' principle: it would set the price just below the level that would induce entry. (p. 6)

Kotz was careful to stress that, since entry was associated with long-run rather than short-run 'excess profit,' the target rate of return would be perceived as a long-run goal. Consequently, the 'limit price' set to cover 'full-cost' would not be sensitive to temporary fluctuations of the actual rate of profit around the long-run target.

According to both Kotz and Eichner, inflation was ignited when monopolistic firms increased their target rate of profit. But while for Eichner the increase in profit targets occurred in the context of long-term growth, Kotz viewed such increases as resulting from long-term stagnation. Following 'limit-pricing' practices, large oligopolies would increase their prices when long-run barriers to entry tended to rise and that, in Kotz's opinion, occurred during 'long-run crises.'<sup>24</sup> During a prolonged expansion, the creation of additional capacity by new entrants was facilitated because demand was

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<sup>24</sup> Kotz (p. 9) distinguished such crises from short-term recessions. 'In addition to the short-run business cycle,' he wrote, 'capitalism appears to undergo long waves of activity, with prolonged periods of relatively vigorous accumulation alternating with prolonged periods of feeble accumulation. We will refer to such depressed periods as "long-run crises."'

growing. This was no longer the case during a long-run slack. With an inveterate stagnation in demand, new entrants could find buyers for their output only by luring them away from existing oligopolies and this was much more difficult to do. The retaliatory power of established firms and the will to use it against intruders was greatly enhanced under those latter circumstances. Furthermore, financial institutions, who were deeply involved in financing the monopoly sector and benefitted from its higher rate of profit, were unlikely to support new entry which could further aggravate an already difficult situation. Hence, 'the constraint which sets an upper limit to monopoly price is loosened in a period of stagnation' and since 'the entire monopoly sector capital finds that entry barriers rise as a consequence of the crisis . . . the response is to raise monopoly sector prices' (p. 10).

The theory explained how a long-run crisis prompted monopolistic firms to raise their *price markups*. This initial price-spark turned into a general process of *inflation*, first because it induced subsequent increases in both competitive prices and wages and, second, because banks and the monetary authorities, reluctant to aggravate the crisis, were driven to accommodate inflation with expanding credit and money. Inflation was not a stationary process, however, and its nature and intensity tended to change as the long-run crisis lingered.

Beyond the 'front window' of changing prices, Kotz explained, inflation acted to redistribute income between monopoly firms, competitive firms and workers. The inflationary process began because monopoly firms attempted to use their increased 'degree of monopoly' in order to obtain higher rates of profits. Since monopoly power was defined in differential terms, these higher rates of profits could be attained only through a redistribution of income from the competitive sector or the working class. Workers and competitive firms 'resisted' to this attempted redistribution by raising their own prices, but given the increase in entry barriers, their counter-strikes could only prolong the process of redistribution, not prevent it. As the inflationary spiral continued, the superior power of monopoly firms would slowly manifest itself in higher rates of profit and, as those rates approached the maximum set by entry barriers, the inflationary process would wane. In this way, redistribution acted to lessen the very inflation which created it. There was also another, perhaps more important link between redistribution and inflation. Since the turn of the century, the competitive sector has been continuously shrinking

relative to the monopoly sector, primarily due to the ceaseless process of capital concentration and centralization. As the 'income requirements' of monopoly firms increased and the 'income base' provided by competitive firms decreased, the redistribution *via* inflation between the two sectors became increasingly harder to attain. Thus, while individual inflationary cycles may die down, 'the tendency for monopoly pricing to ignite inflation during prolonged stagnation grows stronger and such inflations become longer lived' (p. 12).

Kotz's emphasis on profit inflation and redistribution is highly illuminating, but his central structural thesis contains a potential methodological flaw which must be addressed. According to Kotz, the variable  $\delta$  summarized the combined influence of all factors affecting the ease of entry into the monopoly sector. He also insisted that  $\delta$  could be estimated from data on 'cost differences' or the 'risk of failure of large scale entry' (p. 6). Given Kotz's reasoning, one would expect that the ratio of profit rates in the monopoly and competitive sectors be a *function* of entry barriers  $\delta$ , but in Equation (1), this ratio is written as being *equal* to the height of those entry barriers. For Kotz's model, a number of implications follow.

If we retain the identity of Equation (1), the theory becomes a simple tautology. We can replace Equation (1) with a functional relationship, but this does not solve the problem either. Consider, for example, the relationship expressed in Equation (1b), where the ratio of profit rates is a function of barriers to entry:

$$(1b) \quad (100 + r_m) / (100 + r_c) = f(\delta) + u ,$$

where  $u$  is an unknown error term reflecting the combined influence of 'other factors' on the profit-rates differential. This formulation is still problematic because Kotz (p. 6) defines  $\delta$  to include any element which affects barriers to entry, including those 'whose source is unspecified.' In other words, any institutional or technical feature suspected of having an effect on entry barriers could be included as a component of  $\delta$ . We may be able to find numerous variables whose values increased during the 1970s and early 1980s, and which display a positive correlation with the left-hand side of Equation (1b).

Designating these variables as ‘barriers to entry,’ however, remains quite axiomatic.

Even if we can somehow overcome these difficulties, the significance of profit maximization in this context remains unclear. Note that both Equation (1) or its alternative, Equation (1b), are specified in terms of *actual* rates of profit in the two sectors. The equations suggest that barriers to entry affect the actual rates of profit, but they say nothing about the *maximum* rates. In this light, the move from Equation (1a) to Equation (2) implies that the ‘target rate of profit’ for monopolistic firms is equal to whatever their actual rate of profit happens to be! The possibility that higher rates of profits are attainable in principle yet are not attained in practice is simply assumed away. Hence, it seems that despite his other insights, Kotz failed to fill the ‘critical gap’ in structural theories for inflation, and the *target* rate of return remains elusive as ever.<sup>25</sup>

#### 4.7 Final Remarks

Structural theories for inflation overcome the distaste of macroeconomics for real structures and institutions. Facets of economic reality which macroeconomists may regard as unfortunate ‘imperfections’ often constitute basic building blocs to structural theorists. The rejection of perfect competition and the resort to alternative frameworks have enabled structural theorists to unveil and analyze important aspects of modern inflation. Yet the structural approach is still limited in certain important respects.

First, like macroeconomic theories, structural explanations for inflation are also built around ‘ideal types’ for economic actors. Macroeconomists may prefer to see inflation as arising from actions of ‘short-run profit maximizers,’ while structural theorists like to emphasize the role of businessmen seeking a ‘target rate of return,’ firms that follow ‘full-cost conventions,’ or giant corporations which aim to ‘maximize their long-run profits subject to entry barriers.’ Theories of inflation depend crucially on the way they treat individual motivation. Thus, the similarity among alternative macroeconomic theories should not be surprising in light of their common assumption about ‘profit maximization.’ Structural

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<sup>25</sup> For other criticisms of Kotz’s model, see Foster (1985). A reply is given in Kotz (1985).

explanations, on the other hand, are much more heterogeneous because structural theorists often disagree on what motivates economic actors. Given that the fundamental difference between structural theories concerns the issue of individual motivation, the initial choice among alternative explanations should be based on the relevance of their motivational assumptions.<sup>26</sup> Alas this is easier said than done because the ‘true’ psychological drives behind economic behaviour cannot be observed! The axiomatic substitution of ‘ideal types’ for actual human beings means that the structural literature is not immune from the presence of myth.

The structural literature is limited in yet another way. Note that while structural theorists reject the universal validity of perfect competition, their explanations are still based on the existence of equilibrium between desired and actual outcomes. For those theorists, economic outcomes are not necessarily *stable* but they do reflect the *chosen* positions of economic actors. Consider for instance Blair’s model for inflation, in which oligopolies are motivated by their desire to obtain a ‘short-run target rate of return.’ When demand drops, firms *should* increase their prices in order to maintain their short-run target for profit; but the expected increase in prices *will* occur only if firms are indeed successful in achieving their goal. In other words, the theory would provide reasonable predictions regarding the effect of stagnation on prices only when firms achieve an equilibrium between their desired and actual rates of return. Another illustration is provided by the ‘normal-price’ literature. Here, inflation occurs when firms apply their desired fixed markups to what they perceive as ‘normal cost.’ Put somewhat differently, inflation ensues when firms fulfil their desires. Because they rely on motivational hypotheses, all of the structural theories examined in this essay assume an equilibrium between desired and actual outcomes. Naturally, whenever economic agents fail to fulfil their goals, in other words, when there is a ‘disequilibrium’ between desires and outcomes, the theories break down.

Note that we do not suggest that human drives do not affect economic outcomes in general or inflation in particular. On the contrary. All economic phenomena are social and, as such, they always result from human desires. We do say, however, that the present resort by theorists to individual motivation of ‘ideal types’ may not be the most fruitful way of approaching the question of inflation. The

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<sup>26</sup> Despite Friedman’s perspective on unrealistic elements.

focus on individual motivation as a basis for theory requires that people do not alter their economic goals or that changes in those goals be known to researchers; it demands that economic agents share similar aspirations so that they could be approximated by 'ideal types'; it also necessitates that agents succeed in achieving their targets. These are extremely rigid requirements. In our opinion, such presuppositions may be useful in examining narrow aspects of our complex reality but they should not constitute the methodological basis for wider analyses. It is our belief that a broad investigation of modern inflation must allow considerable heterogeneity in the profile of economic actors; instead of stipulating universal 'ideal types' acting in some prescribed regularity, we must describe *actual* behaviour and seek to identify how it changes. If, like Georgescu-Roegen (1979) claims, broad economic phenomena emerge from a process of *qualitative* change, we must look for those changes which underlie the process of inflation. In the presence of continuous inflation, we expect that because some economic agents fulfil their goals, many others remain 'frustrated.' To characterize such process as a movement from one chosen equilibrium position to the next may be quite unhelpful. These concerns must be addressed if we want to better understand the broader causes and implications of inflation.