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PRICE BEHAVIOUR AND BUSINESS BEHAVIOUR

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This paper is a draft of work in progress

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Contents

Abstract	1
Introduction	2
1. The Administered Price Controversy: Beginnings	4
2. Price Flexibility: Fact or Fancy?	8
3. 'Full-Cost' Pricing	14
4. The Marginalists' Counterattack	18
5. The 'Target' Rate of Return	24
6. The Anthropology of Business Behaviour: An Interpretation	29
References	31

Abstract

The present essay is the second in a series of three papers which examine alternative approaches to inflation. Here we identify some of the principal criticisms expressed against neoclassical views on price behaviour and business behaviour. These challenges grew from the early discovery of 'administered prices' by Means and the subsequent findings by Hall and Hitch regarding 'full-cost' pricing. The notions that industrial prices were relatively inflexible and that businessmen set those prices by imprecise rules-of-thumb stood in sharp contrast to the pristine simplicity of neoclassical models. Yet these attempts for greater realism seemed to undermine the prospects of constructing a coherent theory for prices.

Introduction

The economic and political turbulence of the 1930s spawned a number of serious challenges to the hegemony of classical economic doctrines. Of these challenges, only Keynes' 'new economics' was broadly accepted and assimilated into the mainstream of economic thinking. Keynes was successful partly because his policy propositions sought to reform capitalism while preserving its underlying structure. According to Keynes, the malfunctioning of the system stemmed primarily from a chronic lack of synchronization between the 'propensities' of consumers and the 'animal spirits' of investors. The ultimate problem was rooted not in the *structure of capitalism* but in fundamental psychological tendencies stemming from *human nature* itself.¹ In this context, his call for government intervention appeared to be fairly conservative: policies were needed not to alter basic power relationships among specific economic groups but merely to overcome an unfortunate gap between abstract saving and investment 'tendencies.'

As an orthodox student of Marshall, Keynes rarely questioned the basic structural tenets of neoclassical microeconomics and, indeed, he saw no apparent reason to do so. In his opinion, the macroeconomic problem of unemployment arose *despite* the efficiency of individual markets and, furthermore, the solution for the problem could be achieved by broad policy measures which need not interfere with the functioning of these individual markets.² The apparent success of early Keynesian policies during and after the Second World War further strengthened the conviction that macroeconomics was quite independent of underlying microeconomic structures. This legacy of Keynesian macroeconomics has proven more powerful than Keynesian theory itself, for while the primacy of Keynesianism has been subsequently challenged by competing schools, macroeconomics as a whole continues to neglect significant aspects of real structures and institutions.

The eventual divorce of mainstream macroeconomics from the dynamics of real economic structure was established only in the post-war era, however. During the 1930s, before the apparent triumph of Keynesian policies, economists were seeking answers also in alternative directions. While Keynes was elaborating the psychological reasoning for his *General Theory*, some of his contemporaries were trying to identify structural causes for the general economic distress. Their subject of

¹ See for example Keynes (1936), p. 97 and p. 161. Keynes was of course very much aware of contemporary structures and institutions but these were significant for his *General Theory* only in so far as they *enhanced* the tendency for stagnation or instability. The primary *cause* for these tendencies remained human nature.

² Being aware of contemporary research, Keynes (1936, pp. 268, 270-1) was careful to stress that his theory abstracted from 'administered' or 'monopoly' prices. Half a century later, Tobin (1983, p. 299) expressed retroactive regret for this turn of events: 'It is unfortunate that Keynes, in spite of the Chamberlin-Robinson revolution that was occurring in microeconomics at the same time he was making his macro revolution, chose to challenge orthodoxy on its own microeconomic grounds of competitive markets.'

inquiry concerned basic convictions about ‘price behaviour’ and ‘business behaviour.’ First, the pioneering work by Means (1935a) and by the National Resources Committee (1939) under his direction questioned the monolithic approach to price dynamics. Means suggested that there were in fact two types of prices – those which were relatively flexible and those which were relatively inflexible. More importantly, he argued that this basic difference was rooted in the structure of modern capitalism. Second, the research by Hall and Hitch (1939) challenged accepted assumptions regarding pricing decisions by firms. Their interviews with businessmen indicated that the latter determined their prices by imprecise rules-of-thumb and were quite indifferent to the notion of ‘profit maximization.’ These studies launched a prolonged controversy which has not yet been ‘resolved’ and, because it involves basic methodological issues, perhaps could not be resolved. The purpose of this essay is not to provide a review of this literature but rather to examine key methodological questions arising from it. Given our limited goal and the availability of numerous surveys, we find it appropriate to focus only on some of the important contributions to the debate.

Briefly, the link between ‘price behaviour’ and ‘business behaviour’ involves questions of ‘structure’ and economic or business ‘power.’ The neoclassical notion of ‘pricing power’ suggested that a firm could set its own price but, since the firm was assumed to maximize profit, economists could still ‘determine’ what that price would be. The increasing emphasis since the 1930s on the significance of oligopolistic interdependency did not prove to be detrimental for price theory. With sufficiently restrictive assumptions and a complicated mathematical reasoning, economists often succeeded in finding an ‘optimal solution’ for their game theory. The literature following Means and Hall and Hitch undermined this logical simplicity. The existence of relative price inflexibility in markets other than pure competition did not imply that such prices were ‘optimal’ for firms. It only suggested that prices were ‘administered’ and this was precisely the problem. If these were ‘monopoly prices’ in the neoclassical sense they should have been perhaps higher than comparable competitive prices, but there was no reason to expect them to be less flexible. The fact that administered prices were relatively inflexible implied that firms might not have been acting ‘optimally.’ The writings on business behaviour strengthened this doubt when they pointed to substantial ambiguities and considerable discretion in the way firms set their pricing policies.

Ironically, by emphasizing the significance of structure for *actual pricing*, the new empirical literature operated to undermine the methodological basis for *price theory* itself. It was implied that firms operating in non-perfectly competitive markets had the privilege not only to determine their own prices, but also to set these prices in a rather ‘arbitrary’ manner. Prices were still influenced by ‘objective’ conditions such as cost, demand, the specific structure of the industry, or the intensity of competition. However, since firms enjoyed substantial discretion over their own goals, the ‘mapping’ of these objective conditions into prices was obscured from the economist.

Obviously, these issues have considerable bearing on ‘structural’ theories of inflation which we explore in an accompanying paper (Nitzan, 1990).

The first and second sections of this paper deal with the early contributions to the administered-price controversy and the criticisms they elicited. The third section explores the early literature on ‘full-cost’ pricing while, in the fourth section, we deal with the marginalists’ counterattacks against that literature. The fifth section examines the aspects concerning the ‘target’ rate of return and the last section offers some observations on the anthropology of business behaviour.

1. The Administered Price Controversy: Beginnings

The controversy over the relationship between market structure and price behaviour was triggered in 1935 by the work of Means on *Industrial Prices and Their Relative Inflexibility*.³ Means raised two basic questions concerning (1) the apparent anomaly in the behaviour of numerous industrial prices, and (2) the causes behind this behaviour. First, he argued that comprehensive price indices, such as the Wholesale Price Index published by the Bureau of Labor Statistics (BLS), were potentially misleading because they failed to distinguish between ‘market prices’ and ‘administered prices.’ Market prices were defined as prices which were ‘made in the market as a result of the interaction of buyers and sellers.’ Administered prices, in contrast, were ‘set by administrative action and held constant for a period of time’ while sales fluctuated with demand at the rigid price (Means, 1935b, p. 401). This distinction was highly significant because market and administered prices ‘behaved’ quite differently in terms of both frequency and amplitude of change.

The evidence for such divergent behaviour was based on an analysis of monthly prices for individual commodities included in the BLS Wholesale Price Index. Means classified 747 such items according to the number of times their price changed during the eight-year period between 1926 and 1933 and demonstrated that prices for the majority of items changed either very frequently or very infrequently.⁴ His inference that these were in fact ‘quite different types of prices’ was further enhanced by illustrating that ‘items which changed frequently in price showed a large drop

³ Although he initiated the debate, Means was not the first to draw attention to price inflexibility and to discuss its potential causes. Stigler and Kindahl (1970, pp. 11-12) cited earlier works by Berlund and by Jones on the rigidity of steel prices during the early 20th century. In 1927, Mills published a comprehensive study on *The Behavior of Prices* where he found, much like Means’ later discovery, that industrial prices appeared to be either flexible or inflexible in their frequency of change. Another study by Tinter (1935) on price behaviour in Germany, England and the United States, suggested that the frequency of price changes in monopolized industries was appreciably smaller than in competitive ones.

⁴ Of the 747 item prices, 50 percent changed very infrequently (between 0 and 24 price changes during a period of 96 months), 24 percent changed very frequently (between 80 and 94 times), while 26 percent fell in the intermediate range (between 25 and 79 times over the period). See Means (1935b) Chart I, p. 402.

during the depression while those having a low frequency of change tended to drop little in price' (Means, 1935b, p. 402 and p. 403). Additional evidence published 4 years later by the National Resources Committee under the direction of Means indicated that, as prices recovered between 1933 and 1937, frequency and amplitude of price changes were again positively related.

Writing during the depression of the 1930s, Means was primarily attentive to the broad economic implications of this distinction between market and administered prices. Based on the observation that a substantial number of commodities (over one half) had administered prices, he argued that relative price inflexibility became a major disruptive factor in the American economy:

We have always relied in the past on the automatic balancing of economic activity through price changes. This is all right where prices are flexible, since a general drop in demand such as occurred in the depression would result in a drop of prices and maintained production. If all prices had been flexible it is doubtful if we would have had a serious depression after the stock crash of 1929. Where prices are rigid, however, a general drop in demand has quite different and most disastrous result. Instead of producing lower prices, the drop in demand produces a drop in sales and in production. Workers have less to spend, thus amplifying the original drop in demand. In this manner, rigid prices can expand an initial small fluctuation of industrial activity into a cataclysmic depression. (1935b, p. 405)

Means went on to illustrate that between 1929 and the spring of 1933 there was a marked inverse relationship between the relative drop in prices and the relative decline in production for a sample of ten major industries. When prices fell substantially, like in the case of agricultural commodities or petroleum for example, the decline in output was below 20 percent, while when prices remained stubbornly rigid like in agricultural implements or motor vehicles, production levels dropped by as much as 80 percent!

Given the prevalence of administered prices and given the disruptive effect their relative inflexibility had on macroeconomic performance, Means set to address the second issue, namely the cause behind the phenomenon. In his opinion, administered prices emerged primarily (though not exclusively) as a consequence of industrial concentration. Although he expressed this conviction forcefully already in 1935, empirical support for his 'concentration thesis' was first provided only in the National Resources Committee monograph published in 1939. There Means examined price changes between 1929 and 1932 for a subset of 37 out of the 282 manufacturing industries included in the Census' universe, and contrasted them with four-enterprise concentration ratios associated with each individual industry. In selecting the sample, Means sought to eliminate the possible influence factors other than concentration might have had on price changes. Consequently, he excluded

industries where (1) products were not relatively homogenous, (2) more than 2/3rds of the product value originated outside of manufacturing, possibly in demand-sensitive industries such as agriculture or some raw materials, (3) products were not produced for national or international markets, so national concentration ratios were misleading, and (4) reasonably reliable price data were not available. Based on a scatter diagram between percent change in price and concentration ratios for the 37 industries, Means concluded that 'When the depression drop of prices in these industries is compared with the proportion of value of product which in each was produced by the four largest enterprises, a rough relation is apparent between concentration and price insensitivity' (National Resources Committee, 1939, p. 142).

Means repeatedly emphasized that the existence of administered prices was not synonymous with 'monopoly profits' and that the process of industrial concentration did not necessarily mean a growing 'monopolization':

It is . . . abundantly clear that a considerable degree of administrative control is inherent in the narrowing of markets and the willingness of buyers to accept the one-price system of American merchandising. Further administrative controls is implicit if the efficiencies of modern technology are to be realized. Only to the extent that administrative controls arise from collusion between enterprises or through the bringing of production under common control beyond the extent necessary for efficient operation is there an opportunity to reduce the existing degree of administrative control without incurring a cost of decreased efficiency in the use of resources. (National Resources Committee, 1939, p. 145)

In this context, economic 'power' was perceived not so much as an attribute of broader *social relationships* but more as a facet of *industrial organization*. The power to determine prices did not denote the ability of one group to redistribute income from another, but rather the ability of 'organizations' to overcome the 'market.' Thus, the apparent link between industrial concentration and the administration of prices was rooted primarily in the growing 'bureaucratization' of economic activity:

. . . the last century has seen a steadily increasing shift from market coordination to administrative coordination. Gradually, as our great corporations have been built up, more and more of the coordination of individual economic action has been brought about administratively. . . . As a result of this shift from market to administration, the area of coordination remaining to the market has been greatly reduced while the increased bargaining power of the big administrative units has induced the counter concentration in the form of cooperative bargaining organization, farm cooperatives, labor unions and to a small extent consumer cooperatives,

thus further reducing the number of separate units interacting through the market. (Means, 1935b, p. 407)

To a significant extent, then, the adverse consequences of administered prices were the inescapable price we must all pay for technological progress:

Thus a considerable degree of administrative control over prices appears to be *inherent in the modern economy*. Administrative prices and their depression insensitivity seem to be an *integral part* of the structure of economic activity. With the century-long transition of this country from a predominantly agriculture to a predominantly industrial country, the administration-dominated prices of industry have gradually displaced the market-dominated prices of agriculture as the more characteristic form of prices. (National Resources Committee, 1939, p. 145, emphases added)

Although Means pointed to a structural cause underlying the overall economic depression, he recommended not to alter but accommodate that structure. Breaking up large scale enterprises in order to revive price flexibility, he argued, would be immensely wasteful in terms of foregone output and hence he suggested we 'accept inflexible prices as inherent in our modern economy and build our economic institutions around them in such a manner that inflexible administered prices will cease to be a disruptive factor' (Means, 1935b, p. 408). To that end, he proposed we adopt expansionary monetary policies, but although his recommendations were macroeconomic in nature, his analytical framework was too controversial to be unanimously accepted as a basis for such policies.

The idea that firms administered their prices with a considerable disregard to 'market signals' challenged basic theoretical convictions about 'optimal' behaviour. Furthermore, Means' presupposition that such administrative control was largely unrelated to conventional notions of economic 'power' and 'monopoly profits' was not sufficiently persuasive to defuse public concern. A series of Congressional hearings on administered prices in general and on steel prices in particular began in the late 1940s and extended through the 1950s. Guidelines on wage and price policies were issued by the Council of Economic Advisors in 1962 and were aimed particularly at concentrated industries such as steel, copper and aluminum. The announcement of these Guidelines was followed by heightened confrontation between the subjected industries and the Presidential office and the debate over administered prices received considerable public attention. Means' interpretation that price inflexibility was predominately a 'technical' outgrowth of modern 'industrial organization' and his suggestion that public policy could 'overcome' the problem of administered prices presupposed that, in itself, the administration of prices *served no particular interests*. Given the public turmoil over the issue, this was not a very convincing assumption. There was a growing atmosphere of crisis among economists

and many mainstream scholars who felt as if the 'sky were falling' became receptive toward evidence or explanations which would discredit the administered price thesis. Not surprisingly, then, the publication of *Industrial Prices and Their Price Inflexibility* generated an enduring controversy surrounding both the existence of administered prices and their relation to economic structure.

2. Price Inflexibility: Fact or Fancy?

A most promising line of assault on Means' thesis was to deny the very inflexibility of administered prices, for if 'administered prices' were flexible they were no longer a cause for concern for either economists or politicians. This could have been done by either demonstrating that 'administered prices' changed *frequently* (in other words, that they were not really 'administered') or by showing that even if they changed only infrequently, the *amplitude* of such changes was sufficiently large. Let us briefly examine these criticisms beginning with the 'frequency' issue.

In a staff paper for the Price Statistic Review Committee, headed by Stigler in 1961, McAllister demonstrated that the frequency of change in BLS price indices was positively related to the number of reporting companies. This meant that a price index which combined information from all reporting companies changed more frequently than the price for each of the individual companies.⁵ Following this study, Stigler (1962, p. 5) concluded that

Means' tabulations of frequency of price change are unknown mixture of the actual behavior of quoted prices and the number of firms reporting such prices. By increasing the number of price reporters, the B.L.S. can reduce price inflexibility by the same order of magnitude as the increase in the number of reports. The major development which Means believes to have outmoded neoclassical economic theory is the "development" of collecting a number of price quotations inappropriate to the measurement of short-run flexibility.

Although Blair (1964) promptly indicated that this criticism was based on a simple misunderstanding of Means' procedure, Stigler and Kindahl (1970, p. 20) still insisted that 'The McAllister analysis effectively destroys the entire body of work resting upon frequency of price change.' The offensive crumbled a year later, when Stigler (1971) finally realized that Means had access to the raw data of the BLS and that he in fact analyzed the number of price changes reported by the individual companies.

⁵ 'Government Price Statistics,' Hearings before the Subcommittee on Economic Statistics of the Joint Economic Committee, 87th Congress, 1st Sess., 1961. Cited in Stigler and Kindahl (1970), p. 19. See also McAllister (1961).

While the infrequency of administered-price changes was indeed quite remarkable when compared to market prices, this, in itself, was a relatively minor matter for concern. The crux of Means' discovery was that when prices were 'administered' (changing only infrequently), their *amplitude* of change was unduly small and it was this aspect of his findings which drew the heaviest fire. The criticism, first expressed by Thorp (1936), was that BLS price series were based on *quoted* prices and hence failed to reflect the array of secret or undisclosed rebates, discounts and concessions included in actual *transaction* prices. As Thorp later argued, this discrepancy by itself could explain the mysterious inflexibility of the BLS series:

Frequently a commodity will be quoted at an unchanged price over a period of years and thus to the extent that indexes include this type of quotations they will remain relatively unchanged. Actually, the manufacturers of the product may have shaved or cut the price of the item drastically, in periods when business was slow and boosted it as economic conditions improved without the change being recorded in the quoted price. (Thorp and Crowder, 1941b, p. 406)

As a matter of fact, Means was aware of this potential inaccuracy of the BLS series and, as already indicated, he excluded industries for which price data seemed inappropriate. Furthermore, he expressed his confidence that, despite their shortcomings, BLS data reflected actual prices:

I have become convinced the bulk of their quotations represent net prices. The exceptions seemed unlikely to falsify seriously the picture which I presented. Consultation with the technical staff of the Bureau of Labor Statistics supports this view. So far as this question is concerned, I am confident that the statistical picture is not seriously faulty. (Means, 1936, p. 28)

The adequacy of BLS data was subsequently evaluated in an appendix to the National Resources Committee report, where Nelson compared these data to 'realization' prices of the Census of Manufacturing. 'Realization' prices were taken as an approximation for actual 'unit prices' because they were derived as a ratio between the total dollar value of the industry's sales and a corresponding index for total physical quantity. The analysis of price movements between 1929 and 1933 for 28 commodities indicated the existence of positive relationships between the two indices for most but not all products in the sample. Nelson (1939, p. 185) concluded that there was room for caution in using the BLS wholesale price data, yet

after all due allowance is made for the factors demanding caution, very marked and significant differences still remain between the behavior of rigid and flexible prices. For the statement and interpretation of such different types of price behavior, Bureau of Labor Statistics series can be regarded as furnishing an acceptable basis.

Unfortunately, such comparisons with Census data proved rather ineffective in resolving the dispute. Critics of Means still argued that disparities between the indices were sufficiently large to disqualify the BLS series, while supporters maintained that the discrepancies arose mainly because the Census data constituted a current-based index and, hence, were not strictly comparable to the fixed-based indices published by the BLS.⁶

The most serious attack on the empirical basis underlying Means' thesis was launched by Stigler and Kindahl (1970) in their NBER study on *The Behavior of Industrial Prices* between 1957 and 1966. The authors argued that in the reality of a modern industrial system, many products had a complex 'price structure' which could not be approximated by a single number. The price structure for a commodity was affected by the various physical characteristics the product could be sold at (such as size, finish, or packaging) and by the many possible 'terms of sale' associated with different transactions (for example, when price is related to the quantity purchased or credit terms). The BLS series were based on selected 'typical' products with pre-specified characteristics and given terms of sale, but changes in the list price of such products need not reflect the heterogeneous experience of a multitude of buyers who bought other varieties under different terms of sale. As an alternative to the BLS method, Stigler and Kindahl constructed price indices for some 68 commodities which they obtained from 279 different public-sector and private-sector buyers. The commodities represented approximately 19 percent of the value of all products included in the BLS Wholesale Price Index and were limited mainly to 'widely used staple individual materials' in order to bypass the measurement difficulty presented by quality changes. The buyers were mostly large companies and institutions which, according to the authors, were most likely to enjoy secret rebates.

Stigler and Kindahl declared that, after analyzing their new data, they have found 'a predominant tendency of prices to move in response to the movement of general business' and no evidence 'to suggest that price rigidity or "administration" is

⁶ Ross (1964) suggested for example, that during a contraction, the Census index would appear more flexible mainly because it allowed shifts to lower-priced items within Census categories. The Census price index would be inaccurate also because it reflected changes in both price and product mix for multi-product industries and was further contaminated by inter-company non arm's length transactions. (These observations made by Ross are cited in Blair [1972], p. 434.) Other researchers such as Thorp and Crowder (1941a, pp. 391-92) felt that these theoretical incompatibilities were not very serious and the two indices should move fairly closely together.

a significant phenomenon' (p. 9). In their opinion, the lack of a generally accepted theoretical explanation for inflexible 'administered prices' was no coincidence, for the very existence of such inflexible prices was largely a statistical artifact. Yet the apparently definitive language of these conclusions stood in sharp contrast to the body of evidence on which they presumably rested. If anything, the new NBER data and the analysis by Stigler and Kindahl seemed to both reinforce the validity of the BLS series and reconfirm the administered price thesis!

Stigler and Kindahl compared the behaviour of BLS and NBER series for individual commodity groups and, instead of great disparities, they found that in all but one of the cases (rubber and rubber products), the two indices had similar trends and close cyclical movements. They also found close similarity in the behaviour of the comprehensive index which included all the individual commodities. These observations were contrary to what one would expect if the new NBER data were to put the administrative-price thesis to rest: 'In view of the parallelism between the two series,' Blair (1972, p. 463) commented, 'it is difficult to see how the administered-price doctrine could find support in the BLS indexes but be disproved by the new transaction prices.' The apparent contradiction was resolved by Means (1972) who, feeling personally under attack, sought to strike Stigler and Kindahl with their own data.⁷ According to Means, their study was seriously flawed for two basic reasons. First and foremost, the two researchers simply misunderstood his administered price thesis. Second, Stigler and Kindahl's conclusions had no basis in their own data which, in fact, supported both their own incorrect interpretation of the administered-price thesis as well as the correct version of Means.

The conclusion of Stigler and Kindahl that there were no evidence for significant price rigidity was supported by two summary tables (pp. 8-9), where the 68 price indices were classified according to their average behaviour over the contractions of 1957-58 and 1960-61 and over the expansions of 1958-60 and 1961-1966. The tables indicated that in 56 percent of the cases prices move pro-cyclically, in 17 percent of the cases they remained the same and in 27 percent of the cases prices moved counter-cyclically. Means observed that pro-cyclical movements in just over half of the cases could not be considered a very robust proof for classical price flexibility, but accentuated the deficiency was much more serious. A key problem was that Stigler and Kindahl (p. 3) attributed to Means the notion that administered prices were '*wholly* unresponsive' to cyclical market movement, while Means' original thesis (and the very title of his first 1935 study) emphasized their '*relative* inflexibility.' The difference between the two interpretations was clarified in reference to a broader taxonomy:

⁷ The personal tone was rather evident in Means' reply to Stigler and Kindahl: 'Nor can there be any question that the authors are aiming to test the Means' thesis. Means is indicated as the source of the "doctrine" being tested. The name "Means" appears seventeen times in the first eighteen pages. And no other source is given for the doctrine' (Means, 1972, p. 294).

Basically, the administered-price thesis holds that a large body of industrial prices do not behave in the fashion that classical theory would lead one to expect. . . . This departure from classical behavior in a business cycle could theoretically take any one of three forms. In a recession an administered price might fall substantially less than classically competitive market prices; it might show no substantial change; or it might rise contracyclically. These can be referred to, respectively, as *relatively inflexible*, *rigid*, and *contracyclical behavior*. Any one of these three reactions to a general fall in demand would be classically unexpected except as some noncyclical factor intervened such as a trend of technical change. Similarly, in a recovery, an administered price might rise less, show no change, or actually fall. (Means, 1972, pp. 292-3, emphases added)

Using this classification, Means distinguished between the ‘full’ administered-price thesis which included all 3 deviations from classical behaviour and the ‘truncated’ version of Stigler and Kindahl, which treated *all* pro-cyclical price movements – including those which were *relatively inflexible* – as being in conflict with the thesis and considered only rigid and counter-cyclical behaviour as supporting it. Evidently, the ‘truncated’ version adopted by Stigler and Kindahl was only a subset of the ‘full’ version as expressed by Means.

Moreover, in their zeal to discredit Means’ thesis, Stigler and Kindahl committed several methodological errors and hence failed to properly interpret their own data. By correcting these errors, Means showed that the new data in fact supported the ‘truncated’ version of the administered-price thesis, and the ‘full’ version as well! First, Stigler and Kindahl identified the period between January 1960 and November of 1966 as a period of *cyclical* recovery while the data indicated that the cyclical recovery in fact ended in March of 1962 and was followed by 56 months of a non-cyclical expansion. Second, of the 63 commodities for which NBER data were actually provided, 13 were either produced in competitive markets or had a substantial portion of their input costs determined in competitive markets for raw-materials. Third, by classifying individual indices according to their *average* change over the two recoveries or two recessions rather than according to their behaviour in *each* of the 4 periods, Stigler and Kindahl attributed uniform ‘tendencies’ to many commodities which in fact exhibited none. (For example, a commodity for which the price moved pro-cyclically in one expansion and counter-cyclically or not at all in the second expansion should not be said to have a ‘tendency’ based on its *average* price change over the two expansions.) In reexamining the NBER data, Means concentrated on the 50 commodities which were relevant for the administered price thesis, restricted himself to periods of unambiguous cyclical movements and considered each of the 4 cyclical phases in isolation. The indices for these 50 commodities had 200 opportunities to change over the 2 contractions and 2 recoveries and in 69 percent of these cases their changes

conformed with the 'truncated' version (Means, 1972, Table 1, p. 296). Means also used another classification in which he separately defined indices that either moved counter-cyclically or did not move at all in 3 or 4 of the cyclical movements as 'tending to support the truncated version,' from those indices which move pro-cyclically in 3 or 4 of the cyclical movements as 'tending not to support the truncated version.' A test based on this classification indicated that 60 percent of the indices tended to support the 'truncated' version, 36 percent were neutral and only 4 percent tended to reject it. These data also supported the 'full' version when Means accounted for relative price inflexibility (Table 2, p. 296). The 'neutral' indices (36 percent of the total) which showed no tendency to either support or reject the 'truncated' version according to this test, dropped by an average of only 2 percent in the two contractions compared with close to 7 percent for the 13 market-dominated indices excluded from the sample. In the two recoveries, market-dominated indices increased by 3.5 percent, while the average for the neutral indices *declined* by 0.8 percent! Means (p. 297) also reviewed the analysis by Stigler and Kindahl for specific cycles. The authors had examined price movements for specific commodities in 66 instances where the demand for these individual commodities experienced a sharp cyclical change. According to the data, price behaviour in 85 percent of the cases supported the 'truncated' administered price thesis, yet Stigler and Kindahl described this test as a 'relatively unsuccessful investigation,' characterized its results as 'unprepossessing' and failed to even mention them as part of their main findings!⁸

The debate over the adequacy of BLS data was complicated by further questions. Blair (1956, p. 429) argued that secret rebates need not be limited to administered prices and could be found in market prices as well. In other words, BLS data could be underestimating the true extent of price flexibility for *both* types of prices and in order to test whether or not administered prices were *relatively* inflexible we needed to compare transaction data for the two indices. To refute Means' thesis, one had to show not only that actual administered prices were 'flexible' but also that they were *as flexible as actual market prices*. Of even greater consequences for the debate was the emergence since the 1950s of significant *counter-cyclical* price movements during downswings. The development was particularly disconcerting for those who considered administered prices to be a statistical mirage, for the occurrence of this new phenomenon effectively invalidated their argument. As Adams and Lanzillotti noted, one could reasonably speculate that, facing a recession, companies would reduce their quoted prices by less than they reduce their actual prices, but it was not very convincing to argue that as demand fell, firms *raised* their list prices only to compensate for such increases by even larger secret rebates and discounts.⁹

⁸ For more on the debate, see Blair (1972, pp. 461-6), Moore (1972), Ross and Wachter (1973) and Stigler and Kindahl (1973).

⁹ On this Adams and Lanzillotti wrote: '[If] Stigler is correct about the illusion of quoted prices, why in the spring of 1962 did United States Steel not simply raise its transaction prices to the level of its quoted prices? Why did Roger Blough, who is certainly conversant with the

In general, attempts to deny the existence of administered prices or their ‘perverse’ behaviour were more reassuring than convincing.¹⁰ The criticisms, particularly when voiced by eminent economists, helped to reduce the anxiety and justify the continued theoretical neglect of the issue, but never succeeded in eradicating it.¹¹ Continued concern with administered prices was also fuelled by a related debate which began at about the same time and which focused on how individual firms actually set their prices.

3. **‘Full-Cost’ Pricing**

While Means (1935a) initiated a controversy over ‘price behaviour,’ the Oxford Economists’ Research Group, and in particular Hall and Hitch (1939), helped to launch a related debate over ‘business behaviour.’ The conventional theory of the firm, argued Hall and Hitch, stipulated that firms attempted to maximize their profits and that they did so by choosing the output-price combination (or output in the case of perfect competition) such that marginal revenue was equal to marginal cost. This approach yielded theoretical solutions for equilibrium in the case of pure competition, pure monopoly or monopolistic competition, but when the structure was oligopolistic or when monopolistic competition was mixed with oligopoly, the theoretical method broke down. In those latter instances, interdependency between firms meant that individual demand and marginal revenue curves were indeterminate and, hence, could not be used to determine the output-price combination for maximum profits. Economists commonly chose to either ignore the difficulty by considering oligopoly as an ‘exception’ or to bypass it by using some ad-hoc explanations. According to Hall and Hitch, these two solutions were directed

facts of life in the steel industry, insist on raising a fictitious price? Did he not know that a simple revision of transaction prices would have served his purpose and also saved him from detection by the B.L.S. (and its henchmen)? In short, given Stigler’s model, Mr. Blough was either a fool or a provocateur, hankering for a joust with the President of the United States. Both these interpretations of Mr. Blough’s behavior tax credulity’ (88th Cong., 1st Sess., Senate Subcommittee on Antitrust and Monopoly, Senate Committee on the Judiciary, *Administered Prices: A Compendium on Public Policy*, 1963, pp. 6-7). Quoted in Blair (1972, p. 436).

¹⁰ For example, Weiss (1977) concluded that over long period of times, the Wholesale Price Index of the BLS, the ‘realization’ price index based on the Census of Manufacturing and the buyers’ index developed by the NBER were highly correlated and conveyed the same *general* movements. Coutts, Godley and Nordhaus (1978) compared list and transaction prices for non-food manufacturing industries in the U.K. and concluded that ‘There was little evidence found to support the view that the wholesale price indices, being composed of listed quotations, do not accurately measure transaction prices’ (p. 138).

¹¹ Commenting on the title of Blair’s article, ‘Administered Prices: A Phenomenon in Search of a Theory,’ Bailey (1959, p. 460) brushed aside the entire debate as irrelevant and suggested it was in fact ‘A Theory in Search of a Phenomenon.’ Since then the phrase has been often cited as a summary statement on the insignificance of administered prices.

toward the wrong problem. In their opinion, the interesting question was not so much how firms *should* set their price and output in order to maximize profit, but whether firms *indeed* set prices and output in order to maximize profit. Their concern was not with what firms *ought* to be doing but rather with what they were *actually* doing.

In an attempt to address this latter question, Hall and Hitch conducted interviews with 38 British entrepreneurs of which 33 were involved in manufacturing, 3 were retailers and 2 were builders. Based on these interviews, they pointed to a wide gap between the presumptions of conventional analysis and the reality of business practices:

For the above [neoclassical] analysis it is necessary that entrepreneurs should in fact (a) make some estimate (even if implicitly) of the elasticity and position of their demand curve, and (b) attempt to equate estimated marginal revenue and estimated marginal cost. We tried, with very little success, to get from the entrepreneurs whom we saw, information about elasticity of demand and about the relation between price and marginal cost. Most of our informants were vague about anything so precise as elasticity, and since most of them produce a wide variety of products we did not know how much to rely on illustrative figures of cost. In addition, many, perhaps most, apparently make no effort, even implicitly, to estimate elasticity of demand or marginal (as opposed to average prime) cost; and of those who do, the majority considered the information of little or no relevance to the pricing process save perhaps in very exceptional conditions. (p. 18)

It seemed that the *theoretical* distinction between monopoly or monopolistic competition (where the demand curve facing the firm was assumed to be known) and oligopoly (where the individual demand curves were indeterminate) was not very important for the issue of *practical* price determination. In reality, businessmen operating in all of these markets simply did not 'know' their demand curve and, furthermore, they did not care to 'discover' this demand curve even when they could have done so:

Only where oligopoly elements are present is the demand curve 'indeterminate' in the economist's sense, but in the other cases it is *unknown to the entrepreneur*, and *this seems to be the essential point*. It is true that in the case of monopoly or monopolistic competition the possibility of finding his demand curve by experimenting is open to the entrepreneur; but there are objections to experimentation, and the prospect of a quiet life seems in many cases to have a greater appeal. (pp. 30-1, emphases added)

The revelation that firms neglected their demand led to an even more ‘stunning’ conclusion, namely, that firms did not try to maximize their profits as suggested by standard theory:

The most striking feature of the answers was the number of firms which apparently *do not aim* in their pricing policy, at what appeared to us to be the maximization of profits by the equation of marginal revenue and marginal cost. (p. 18, emphasis added)

Instead of equating marginal revenue and marginal cost in an attempt to maximize profits, Hall and Hitch (p. 18) suggested that businessmen were ‘thinking in altogether different terms.’ While under certain circumstances, pricing behaviour could be explained by reference to ‘long-term’ profit maximization, in most cases businessmen applied a simple ‘rule-of-thumb’ which Hall and Hitch called ‘full-cost’ pricing:

The formula used by the different firms in computing ‘full cost’ differ in detail . . . but the procedure can be not unfairly generalized as follows: prime (or ‘direct’) cost per unit is taken as the base, a percentage addition is made to cover overheads (or ‘oncost’, or ‘indirect’ cost), and a further conventional addition (frequently 10 per cent.) is made for profit. Selling costs commonly and interest on capital rarely are included in overheads; when not so included they are allowed for in the addition to profits. (p. 19)

Firms justified their submission to the practical norm of ‘full-cost’ pricing in a variety of different ways. Some argued it was the ‘right price,’ other considered its application as a ‘fair’ practice toward their competitors, while still others noted that experience ‘proved its advisability.’ When asked why they did not charge a price higher than that implied by the ‘full-cost’ principle, most entrepreneurs cited their uncertainty regarding the response of competitors. When requested to explain why they would not charge a price lower than ‘full-cost,’ the businessmen mentioned primarily the fear that competitors would match the lower price, the unresponsiveness of demand and moral objections to selling below costs. As reasons for not changing prices (however fixed), businessmen explained that they wished not to ‘disturb’ the stability of market prices and also that buyers had a ‘conventional’ price in mind and ‘disliked’ price changes. Hall and Hitch (p. 22, emphasis added) felt that ‘All of these reasons militate against *changing* the price from the conventional level,’ yet they stressed that the ‘full-cost’ principle was insufficient to explain this ‘conventional level’ itself.

The simplicity of the ‘full-cost’ principle was potentially deceiving. ‘It would be useful for economic analysis,’ Hall and Hitch (p. 19-20) wrote, ‘if the magnitude of “full cost” in any case could be deduced from the technical conditions of production

and the supply prices of the factors,' but in practice this was impossible for four principal reasons. First, costs varied with the size of the firm but firms were rarely operating at an 'optimal' size which economists could presumably determine; instead, their size apparently was the consequence of a 'historical accident' which economists found very difficult to 'predict.' Second, overhead cost per unit depended on the 'normal' output level used as a divisor in the 'full-cost' formula, but this benchmark for output was set by arbitrary accounting conventions. Third, selling expenses were included in costs but were often depended on demand. Fourth and most importantly, the way in which entrepreneurs set the magnitude of 'conventional' profit, or the reasons why they changed it were not at all clear.

For the businessmen, the 'full-cost' principle was a straightforward technical matter yet, because of the many 'arbitrary' factors involved, the economist could not anticipate the final price with any reasonable accuracy. Surprisingly, then, getting closer to reality did not seem to enhance our understanding of the pricing process. Hall and Hitch questioned the usefulness of neoclassical price theory because its preoccupation with what firms ought to be doing turned this theory into a *normative* doctrine. They suggested we explain prices by embarking on a *positive* scientific inquiry into actual pricing decisions made by real businessmen but, unfortunately, substituting the businessman's practice for the economist's postulate did not seem to solve the price question. The explanation provided by businessmen appeared 'arbitrary' and were hardly more revealing than the theories of neoclassical economists. Instead of adhering to rigid pricing procedures shaped by necessity, entrepreneurs seemed to follow loose 'conventions' and 'norms of conduct' which did not appear to have a solid 'objective' rationale. Hall and Hitch discarded the normative approach embraced by economists, but their own 'full-cost' principle seemed to reflect the normative ethic adopted by businessmen.

One could have removed the deadlock by seeking psychological explanations for the behaviour of businessmen but this, of course, would have constituted a retreat from the empirical road into the normative twilight. Instead, Hall and Hitch (p. 33) emphasized that 'There is usually some element in the prices ruling at any time which can only be explained in the light of the history of the industry.' The rule-of-thumb for pricing included conventions on what constituted 'normal output,' conventions on how to estimate costs, conventions on how to react or cooperate with competitors and, most importantly, conventions on how to set 'adequate' profit margins. Yet these conventions were shaped by *history*, not by the erratic fancy of businessmen and only by accounting for the specific historical evolution of these conventions could one hope to shed some light on current prices.

The totality of beliefs and conventions prevailing in any one time were encompassed in what Hall and Hitch (p. 28) called the 'community of outlook' of businessmen, and it was within this context that 'full-cost' pricing reinforced a tendency toward price stability:

We cannot say precisely what this price will be, for reasons already explained; if it is set anywhere over a fairly wide range it will have a tendency to stay there. The nearest that we can get to an exact statement is that the price ruling where these conditions obtain is likely to approximate to the full cost of the representative firm; and that this price is reached directly through the *community of outlook* of business men, rather than indirectly through each firm working at what its most profitable output would be if competitors' reactions are neglected, and if the play of competition then varied the number of firms. (pp. 27-8, emphasis added)

In a similar way, price instability was not a direct consequence of changes in underlying conditions but was rather created indirectly when such changes led individual entrepreneurs to question the prevailing 'community of outlook':

Prices in an industry become 'unstable' as soon as any of the competitors form an idea of a profitable price which is markedly different from the existing prices. (p. 28)

'Full-cost' pricing implied that prices would likely be altered in response to significant changes in the cost of labour or raw material but that, normally, businessmen would not question the existing price structure as a result of moderate or transitory changes in demand. As Heflebower (1955, p. 361) indicated, the new heresy of 'full-cost' pricing provided an appealing explanation for relative price stability during the Depression, especially after the findings of Hall and Hitch were supported by subsequent studies like Saxton (1942), Lester (1946), Dean (1951) Oxenfeldt (1951), Fog (1960), Cyert and March (1963) and Skinner (1970).¹² Nevertheless, the imprecise nature of the new approach left it open to criticism from mainstream economists who were quick to respond.

4. The Marginalists' Counterattack

The proposition that businessmen did not try to maximize their profits but rather were content with the quiet life of 'full-cost' pricing was not universally accepted by economists. Leading the neoclassicists' counterattack, Machlup (1946) argued that the rejection of marginal analysis by empirical researchers such as Hall and Hitch (1939) and Lester (1946) was in fact baseless.¹³ In his opinion, Hall and Hitch and their followers erred because their research suffered from one or more of the following shortcomings: (1) a failure to properly understand the essence of marginal

¹² For surveys of 'full-cost' pricing, see Heflebower (1955) and Silberston (1970).

¹³ Similar criticisms of 'full-cost' pricing appeared in Robinson (1939) and Kahn (1952).

analysis, (2) faulty research techniques, and (3) mistaken interpretations of empirical 'findings.' Let us consider these criticisms in some detail.¹⁴

According to Machlup (p. 521), the emphasis Hall and Hitch put on the 'history of the industry' in determining current conditions and in shaping behaviour was 'by no means denied by marginal analysis.' Contrary to common beliefs, he insisted, neoclassical theory recognized the role of history and, hence, did not really seek to explain how an individual firm determined the *levels* for its output, prices and employment. Rather, the theory focused on how the firm *altered* these variables in response to changing conditions. The overriding principle which guided the firm in its actions was the aim of maximum profit and marginal analysis was merely a *technique* used to achieve this goal.

Machlup emphasized that the procedure whereby the firm equated marginal revenue and cost must be interpreted with great care. First, the magnitudes for the relevant variables were 'subjective estimates, guesses and hunches.' They reflected the perceptions, opinions, and beliefs of the businessman and were not necessarily equal to the corresponding 'objective' magnitudes as they might be observed by 'outside' parties. Second, the businessmen need not be engaged in tedious data collection and complicated calculations in order to equate marginal revenue and cost. In most cases he could rely on his intimate knowledge of his own business and follow an imprecise 'routine' which nevertheless accounted for all crucial factors:

The business man who equates marginal net revenue productivity and marginal factor cost when he decides how many to employ need not engage in higher mathematics, geometry, or clairvoyance. Ordinarily he would not even consult with his accountant or efficiency expert in order to arrive at his decision; he would not make any tests or formal calculations, he would simply rely on his sense or his feel of the situation. There is nothing very exact about this sort of estimate. On the basis of hundreds of previous experiences of a familiar nature the business man would "just know," in a vague and rough way, whether or not it would pay him to hire more men. The subjectivity of his judgements is obvious. (p. 535)

Thus, contrary to the inference of Hall and Hitch (1939) and others, the observation that businessmen could not or simply did not know all the objective data, and the fact that they did not perform complicated computations failed to demonstrate that firms did not *seek* to maximize profit.

Hall and Hitch further suggested that entrepreneurs did not make use of concepts such as 'demand elasticity,' 'marginal revenue' and 'marginal cost,' and in many cases did not even understand them but, according to Machlup, this also did not

¹⁴ For further replies and rejoinders see Lester (1947), Machlup (1947) and Stigler (1947). Later comments can be found in Machlup (1967).

invalidate the standard theory. While entrepreneurs might have failed to understand the marginal concepts as presented to them by Hall and Hitch, they have not necessarily failed the crucial test of marginalism. The marginal theory did not stipulate that businessmen must use the *jargon* of marginal analysis as developed by economists, only that they follow the marginal *principles*. Businessmen had no interest in the equality of marginal cost and revenue *per se* but only insofar as it helped them evaluate how their action might affect total profit. For that purpose they could also use many other guidelines which, although expressed in a different language, had practically the same meaning. For instance, a firm might decide to raise its price because it expected unit profit to rise by a greater percentage than the fall in quantity sold. The decision was based on ‘averages’ and ‘totals’ yet the logic was *marginal* for the focus was on the expected *change* in profit.

Given that Hall and Hitch misunderstood the thrust of marginal analysis, and given that they baffled the entrepreneurs with academic jargon, it was hardly surprising that the two researchers also derived erroneous conclusions from their data. To explain this latter point, Machlup (p. 545) summarized the findings of Hall and Hitch in their own words:

“A large majority” of them [of businessmen] explained that they charged the “full cost” price. Some, however, admitted “that they might charge more in periods of exceptionally high demand”; and a greater number reported “that they might charge less in periods of exceptionally depressed demand.” Competition seemed to induce “firms to modify the margins for profits which could be added to direct costs and overheads.” Moreover, “the conventional addition for profit varies from firm to firm and even within firms for different products.”

According to Machlup (*ibid.*) these findings, which apparently ‘shook the researchers’ confidence in the marginal principle and convinced them that business men followed the “full cost principle” of pricing regardless of profit maximization,’ were exactly what one would expect to hear on the basis of *marginal* analysis! Indeed, in the neoclassical framework:

we should expect for most industries that price in the long run would not deviate too much from average cost, yet that the firm would attempt to get better prices when it could safely get them and would not refrain from cutting prices when it believed that this would increase its profit or reduce its losses. (*ibid.*)

The observation that different firms behaved differently and that their experience also vary over time proved, in Machlup’s opinion, that firms paid close attention to

variables other than average cost and, in particular, to those variables which affected their demand. In general, he summarized,

there is little or nothing in the findings of this inquiry [by Hall and Hitch] that would indicate that the business men observed an average-cost rule of pricing when such observance was inconsistent with the maximization of profit principle. On the other hand, there is plenty of evidence in the findings that the business men paid much attention to demand elasticities – which to the economist is equivalent to marginal revenue considerations. (p. 546)

Marginalists attacked ‘full-cost’ pricing on methodological grounds and hence it is interesting to note that their own criticisms suffered from similar methodological shortcomings. One important complaint against Hall and Hitch was that views of businessmen were no substitute for economic theory. Kahn (1952, p. 126), for instance, stated that

the fundamental doubt is whether these business men, and other business men in similar predicaments, did not feel called upon to devise and present to the Oxford intellectuals, a theory of business behaviour which is primarily a rationalization and, in considerable measure a false rationalization of behaviour based on instinct rather than reasoning. It is with business men’s behaviour not with their thoughts, that we have to reckon. The economic *theory* of a business man may be based on the concept of a *fair price*, which is the price which, it is believed, in the absence of special circumstances, *ought* to rule. But very often this theory is a theory of ethics rather than of economics, and the business man takes the best price that he can get (through if this is higher than the fair price he may be reluctant to extort it to the full).

The marginalists rejected the explanations of businessmen for ‘full-cost’ pricing as mere ‘ethics,’ ‘rationalization’ and even ‘false rationalization,’ yet their dismissal of evidence appeared to be quite selective. When the same businessmen reported on *deviations* from ‘full-cost’ pricing, Kahn and Machlup were only too eager to cite them as decisive confirmation of profit maximization. The basis for this selective use of evidence is not clear. Machlup (p. 538) wrote that ‘It takes an experienced analyst to disentangle actual from imaginary reasons and to separate relevant from irrelevant data and essential from decorative bits of information furnished,’ but he failed to enumerate the criteria he himself followed in screening the evidence provided by Hall and Hitch. If, as Kahn so forcefully asserts, we have to reckon with ‘behaviour’ rather than ‘thoughts’ then *every* interpretation provided by businessmen – whether it is consistent or inconsistent with the economic theory under examination – is simply extraneous for our purpose.

Beyond this double standard toward evidence, the citation from Kahn raises an even more serious difficulty concerning our ability to prove or refute the norm of profit maximization. A 'historical' approach to economic theory could emphasize forces beyond the particular inclinations of individuals and claim that, to a large extent, individual opinions and convictions are shaped by these forces. Hence, the empirical basis for testing such a historical theory for prices can indeed be independent from the 'business creed.' This conclusion does not hold for neoclassical price theory, however. The latter is a theory based on *motivation* and as such can be tested only by resorting to direct evidence on motivation. To say that business behaviour is governed by the *aim* of maximum profit and then to argue that the stated goals of businessmen cannot be used as evidence in testing the theory seems to us quite inconsistent. Without such direct evidence on motivation the neoclassical theory of profit maximization amounts to either a normative recommendation for businessmen on how they *should* act, or else it is simply an axiomatic construct.

The marginalists could of course claim that, while they did not have direct support for the motivational theorem of profit maximization, the observation of business performance could provide an indirect test for this basic neoclassical postulate. This, however, is easier said than done. For example, Kahn (1952, p. 127) concludes that observed performances do not lend clear support to either 'full-cost' pricing or profit maximization:

The *actual* behaviour of prices and profits – as revealed by comparisons of different firms and products and of different points of time – fails to support the "full-cost" principle in its undiluted form. But it fails equally to support, in its undiluted form, a narrow interpretation of the operation of the profit motive. (emphasis added)

Yet the bases for such conclusions are not clarified by Kahn. We do not have an empirical yardstick for 'maximum profit' so we cannot really determine whether firms obtained this maximum or not. Furthermore, we cannot use business performance as evidence for business motivation. Even if we somehow knew what maximum profit were and even if we observed that firms indeed *obtained* this maximum, there would be nothing in this observation to demonstrate that firms *sought* maximum profit. Firms could obtain maximum profit by accident or even despite their efforts to attain another goal. Alternatively, firms could strive toward maximum profits but persistently fail to achieve them. In short, the goal for maximum profit can be demonstrated by interviews with businessmen or can be simply stipulated by the economist, but it cannot be proven or refuted by business performance.

The second important criticism against Hall and Hitch was that businessmen acted not on the basis of objective circumstances, but rather on the basis of their own subjective interpretations of these conditions. In particular, it did not matter that

entrepreneurs did not know the objective demand curve as long as they acted on the basis of their subjective notion of that curve. This explication of the neoclassical theory is also problematic because profit maximization becomes consistent with *every* course of action. If, facing an increase in demand, businessmen increase their prices we can argue that profit maximization is vindicated, but we can derive the same conclusion if businessmen lower their prices instead! In this latter case, we can simply argue that businessmen attempted to maximize profits on the basis of erroneous interpretations of current conditions. If for some reason they believed that demand fell or was just about to fall, a policy to reduce prices would have been quite consistent with profit maximization, despite the 'objective' increase in demand. Thus, it would appear that when profit maximization is based on subjective perceptions of businessmen and when these perceptions cannot be accurately observed because we cannot rely on what businessmen tell us, the theory becomes irrefutable.

Both adherents of 'full-cost' pricing and advocates of profit maximization argued that their theories explained business behaviour. They also acknowledged that these theories could not be used to predict prices. According to Robinson (1966), the two doctrines faced the same barrier mainly because they were unable to explain the *profit margin*. In the 'full-cost' approach, price was determined by adding to observed unit cost a certain profit markup but this addendum was admittedly 'arbitrary':

The gross profit margin, or rake-off on price cost . . . probably depends very much upon historical accident or upon conventional views among business men as to what is reasonable. And any conventional pattern of behaviour which established itself amongst an imperfectly competitive group provides a stable result. So long as all adhere to the same set of conventions each can enjoy his share of the market, and each can imagine that he is acting according to the strict rules of competition, though in fact the group as a whole, by unconscious collusion, are imposing a mild degree of monopoly upon the market. . . . Where outright monopoly rules, or where a group of commodities is produced by a few powerful firms, there is great scope for individual variations in policy, and it is hard to make any generalization at all as to what governs the margin of profit per unit of output. (Robinson, 1966, pp. 78-9)

For the neoclassicists, on the other hand, the price was determined when the businessmen attempted to maximize his profit by equating marginal revenue and marginal cost. The magnitudes for marginal revenue and cost, however, were not as clear in practice as they were in theory. The businessmen did not use the 'true' value from marginal revenue but rather his subjective interpretation for it. Furthermore, marginal cost included, in addition to observed expenses, an unspecified figure of 'normal' profit which the entrepreneur presumably added to cover his 'opportunity

cost.’ With both marginal revenue and cost thus obscured, the explanatory power of the theory was dramatically reduced. By using observed costs and *ex-post* prices, we could still predict the subjective elasticity of demand but, in doing so, we merely explained what we were supposed to know by using what we were supposed to explain!:

The gross profit margin, however it is determined, can always be expressed in terms of a formula $e/(e-1)$ [where e denotes the elasticity of demand]. For instance, if, in a certain case, price is found to be equal to prime cost *plus* 50 per cent of prime cost, we may say that the producer concerned acts as though he believed the elasticity of demand in his market to be equal to 3. But by saying so, we add nothing whatever to our knowledge of how the gross margin is determined. (Robinson, 1966, p. 78)

This led Robinson to ponder the dismal prospects of ever answering the ‘first problem’ of economics:

All this makes a serious breach in the smooth surface of the orthodox theory of value, and it seems that economic science has not yet solved its first problem – what determines the price of a commodity? (p. 79)

5. The ‘Target’ Rate of Return

Although the marginalists insisted that ‘full-cost’ pricing was no more than the everyday incarnation of profit-maximization policy, sceptics continued to look for alternative, hopefully more plausible explanations for ‘rule-of-thumb’ pricing. One of the first serious studies on pricing objectives of large U.S. firms was conducted by Kaplan, Dirlam and Lanzillotti (1958) as part of their Brookings Institution research on *Pricing in Big Business*. Based on this investigation, Lanzillotti (1958) tended to reject both the notion that pricing was motivated by an effort to ‘maximize profit’ and the idea that firms simply followed ‘conventions.’ Instead he suggested that, in many big companies, ‘full-cost’ pricing was adopted as part of a broader strategy to obtain a ‘target’ rate of return on investment.

Kaplan, Dirlam and Lanzillotti conducted interviews with officials of 20 large U.S. companies.¹⁵ At the time, all of these firms were among the 200 largest industrial corporations and over 10 were among the 100 largest corporations. Some (like Johns Manville, U.S. Steel, International Harvester and Union Carbide) were dominant price leaders, while others (like Swift and A&P) faced tough competition

¹⁵ Alcoa, American Can, A&P, du Pont, General Electric, General Food, General Motors, Goodyear, Gulf, International Harvester, Johns-Manville, Kennecott Copper, Kroger, National Steel, Sears, Standard Oil of Indiana, Esso, Swift, Union Carbide and U.S. Steel.

and, despite their large size, could not 'decide' for the market. The remaining companies fell between these two extremes. Company officials were asked detailed questions concerning formal and informal commercial goals, procedures for implementing and evaluating goals, techniques of price setting, and functions of pricing executives and committees. When asked about their pricing objectives, officers often cited several goals but in over 10 of the 20 firms surveyed, the primary objective was to achieve a target rate of return. According to Lanzillotti (1958, p. 923fn),

Target-return is defined as the building up of a price structure designed to provide such a return on capital employed for specific products, product groups, and divisions, as to yield a predetermined corporate average return. In most cases management referred to stockholders' equity (net worth) plus long-term debt. Usually, a standard cost system is used as a means of allocating fixed cost to various product divisions, with the standards premised on an assumed rate of production, typically about 70 per cent to 80 per cent of capacity, and an assumed product-mix as "normal."

Kaplan *et al.*, it should be noted, were not the first to unveil the practice of target-return pricing. The essential aspects of this pricing practice were described already in the early 1920s by Brown, who was a vice president of General Motors at the time. Brown (1924) explained that General Motors customarily began with a target of achieving a 20 percent rate of return on stockholders' equity and geared its pricing policies toward that end. The price calculations were based on the assumption of a certain 'standard' volume of production (usually 80 percent of practical annual capacity which represented the 'normal average rate of plant operation'). Cost items were classified as being either variable or fixed and the corresponding average cost per unit were determined by dividing the total figures by the 'standard' volume. Unit profit was similarly determined by dividing the target for total profit by the 'standard' volume. The 'benchmark' price was then set as the sum of unit variable cost, unit fixed cost and unit profit. Brown noted that although fluctuations in actual production would affect unit fixed cost, it was not really practical or even desirable to alter the price whenever there was a change in production in order to continuously maintain the profit margin at the pre-specified level. Since output was expected to oscillate around the 'standard' volume, maintaining the 'benchmark' price despite fluctuations in production would enable the firm to achieve its target as an *average* rate of return over time. Hence, in this early explanation, Brown already provided a simple rationale for both the practice of 'full-cost' pricing and for the relative inflexibility of 'administered prices' which were to be discovered by economists more than a decade later.

In 1955, Bradley who, together with Brown, developed the target return method for pricing in General Motors testified in front of a Congressional committee that the

same principles were still vigorously applied by his company.¹⁶ The research by Kaplan *et al.* (1958) and the summary by Lanzillotti (1958) were important because they indicated that a target rate of return has been a principal pricing objective not only for General Motors, but for many other leading U.S. firms as well. The studies were significant also because they pointed to certain *limitations* on what we could learn from exploring pricing 'objectives.'

Lanzillotti (1958) emphasized that pricing toward a target rate of return was a primary objective only for firms which had substantial market power and occupied a price-leading position in their industry. Most other large firms cited alternative goals such as the need to 'meet' (or prevent) competition, the desire to stabilize prices and margins, an aim to realize a certain market share, the wish to resolve conflict of interests between the different firms and a desire not to arouse public protest and prevent adverse political and legal backlashes. In most cases, there was one paramount objective but it was evident that, in many situations, pricing was also influenced by a combination of subsidiary goals. Even when target-return pricing was the dominant objective, it was hard to separate this from other justifications provided by company officials:

A variety of explanations was given by the companies to justify the particular size of the profit target used as a guide in pricing decisions. The most frequently mentioned rationalizations included: (a) fair or reasonable return, (b) the traditional industry concept of fair return in relation to risk factors, (c) desire to equal or better the corporation average return over a recent period, (d) what the company felt it could get as a long-run matter, and (e) use of a specific profit target as a means of stabilizing industry prices. At least one of the foregoing, and most frequently the first, was mentioned by the companies interviewed, and in a few cases the entire list was offered as justification for the company profit goal. This reinforces the observation that *no single objective* or policy rules all price-making in any given company. In fact, in many companies a close interrelationship exists among target-return pricing, desire to stabilize prices, and target market-share (either a minimum or a maximum objective). (p. 931, emphases added)

The complexity of these considerations indicated that knowing the objectives of the firm still provided the economist with little or no clue about the *causes* for adopting these objectives.

The key question of what determined the target rate of return remained open and Lanzillotti suggested that this target and the pricing practices used to achieve it were

¹⁶ Bradley's testimony is included in 84th Congress, 1st Sess., Senate Subcommittee on Antitrust and Monopoly, Senate Committee on the Judiciary, *Hearings on General Motors*, 1955, Pt. 7, p. 3593, and is reported in Blair (1972), p. 470.

determined together with the firm's investment decisions. He began his explanation by noting that large firms appreciated the complexities of modern business and paid close attention to their implications. These companies realized the delicacy of their dealings with external suppliers and customers, as well as the complexity of interrelationships between the various units within the firm itself. They also acknowledged the intricacy of corporate rivalries in individual markets and in the economy as a whole. Furthermore, because of their size, the actions of large firms were potential targets for the media, legislators and the justice system, so the political cost of 'irresponsible' behaviour might far exceed their immediate pecuniary benefits. Finally, all large firms viewed price competition as a dangerous policy alternative that should be avoided as much as possible. Under these circumstances, argued Lanzillotti (p. 936), business executives viewed the *market* as a *creature of the firm*. They felt it was their responsibility to perpetuate the firm's position and to preserve its different relationships. From their perspective, prices should not be left to anonymous and potentially destabilizing 'market forces' and must be set in accordance with a 'pricing policy.'

In this light, the ideal notion of 'profit maximization' took a new meaning. Large firms that sought to 'maximize' profits could not afford to obey erratic market signals and must follow their own price planning. In fact, the stronger the drive for profits and accumulation, the more compelling was the imperative for a *careful pricing policy*. Of course, firms did not have to stress the priority of profit and accumulation. In a corporatist environment where the market was perceived as a 'subset' of the firm, company executives tended to advance a philosophy which stressed 'responsibility,' 'leadership' and 'cooperation' and to present corporate policies as striving for a 'just prices' and a 'fair return.' Lanzillotti noted that company officials habitually claimed that their products faced a wide array of substitutes and that, consequently, their price discretion was in fact minimal. Under these circumstances, the executives argued, the administration of prices by 'price leaders' merely 'approximates the market equilibrium.' Instead of deciding to engage in cutthroat competition (which was a price 'policy' in itself), large firms chose to 'administer' prices and in this sense they provided an advantageous public service. Both price competition and price administration would lead to similar long-run price trends, they contended, but the latter arrangement saved the system from the destabilizing effects of severe price fluctuations.

Lanzillotti rejected this common rhetoric. Price 'leadership,' he argued, could not be taken as a proxy for equilibrium unless we took the latter to denote whatever the firm happened to decide. In his view, the data overwhelmingly demonstrated that firms based their pricing upon '*planned* profits' (p. 938). Instead of 'administered prices' he suggested the concept of 'administered profits' which were set to meet specific investment plans:

The company proceeds on the assumption of the need for a certain amount of capital to undertake the investment in plant expansion and new facilities which are envisaged for the long haul in order to maintain and/or improve market position. . . . The only way in which price policy can be viewed in such companies as these, with their wide variety of products and selling in a large number of different markets, is in terms of profits-investment ratios. This criterion serves as an effective guide for pricing decisions at divisional and departmental levels. If we are to speak of “administrative” decisions in the large firm, it is perhaps more accurate to speak of administered *profits* rather than administered *prices*. (p. 938)

Yet the administration of profit was a process no less complex than the administration of prices. Lanzillotti argued further that

(a) the large company has a fairly well defined pricing goal that is related to a long-range profit horizon; (b) its management seeks – especially in multiproduct operations – a simultaneous decision with respect to price, cost and product characteristics; and (c) its pricing formulas are handy devices for checking the internal consistency of the separate decisions as against the general company objectives. Under this hypothesis no single theory of the firm – and certainly no single motivational hypothesis such as profit maximization – is likely to impose an unambiguous course of action for the firm for any given situation; nor will it provide a satisfactory basis for valid and useful predications of price behavior. (*ibid.*)

This suggested that the debate over administered prices was partially misdirected. The notion that prices were stable or ‘inflexible’ because they were based on rigid pricing procedures was potentially misleading because these procedures were themselves subject to change. This point was clarified by noting the bureaucratic dichotomy between middle and top management:

Managerial specialists down the line are given a framework of requirements that must be met, while managers at the top, of course, are free to and do change these requirements to meet particular situations. (p. 939)

The recognition that firms may have a complex set of objectives and that these are inherently dynamic presents a serious methodological difficulty if we want to use such objectives as bases for price theories. Many company executives candidly endorse corporatist ideals whereby pricing procedures reflect ‘responsibility’ and ‘leadership,’ but this rhetoric does not provide the researcher with much insight. More importantly, the way in which firms determine their ‘requirements’ for expansion and profit must be understood as part of a dual process. While corporate

decisions may affect the performance of firms, these decisions themselves are formulated in the context of the firm's own history and the general business climate. Corporate *objectives* may guide the short term behaviour of large firms but these objectives themselves will be continuously modified by actual *performance* and corporate projections regarding the firm's evolving environment. In this sense, the success or failure in achieving company objectives – whether they are formulated to 'maximize profits' or to achieve a 'target rate of return' – can never be tested because the causal relationship between any such goal and its realization is *double sided*.

With this in mind, it is hardly surprising to find that large firms 'succeed' in meeting their 'targets.' For example, Blair (1972, p. 482-93) compared the target and actual rates of return for a group of 5 leading companies over the 16-year period between 1953 and 1968. (The group included General Motors, U.S. Steel, Alcoa, Standard Oil and Du Pont.) He concluded that 'Over the 16-year period the success of the 5 leaders in meeting their profit objectives is little short of remarkable' (p. 482) but this, of course, does not establish a clear line of causality between target and actual rates. Under stable conditions, targets rarely change simply because they are achieved, but under dynamic conditions firms might be tempted to alter their targets in light of new circumstances or can be compelled to do so in order to accommodate new realities. For instance, during the 1950s, U.S. Steel apparently attempted to raise its target rate of return from 8 to 12 percent but subsequently it had to reverse this decision when its large price increases were accompanied by an unacceptable fall in volume. Another illustration is provided by General Motors. The company, which from the 1920s has managed to realize an average target rate of return of 20 percent, had to reduce its target in face of intensified competition since the 1970s. In both of these cases, it appears the target was amended, at least in part, in order to enable the firm to successfully achieve it. Hence we should not be surprised to find that the firms succeeded in meeting revised targets.

6. The Anthropology of Business Behaviour: An Interpretation

The ideas of 'administered prices,' 'full-cost pricing' and 'target rate of profit' seemed to have undermined simplistic convictions about business behaviour. The ambiguity was intensified when prominent neoclassicists, in an attempt to shield their paradigm, introduced 'subjectivity' into profit maximization and 'confessed' that the theory was not intended to explain the actual price but only the direction of its movement. The heightened debate over how businessmen 'behaved' questioned the usefulness of adhering to a single 'ideal type.' It was suggested that firms did not necessarily obey an *externally* imposed goal, such as the achievement of maximum profit and, in many cases, followed their *own* objectives which they defined and occasionally altered. Furthermore, it appeared that business behaviour in general, and pricing practices in particular, did not always conform to unique *procedures* and

could not be described by simple *mechanisms*. This presented a serious methodological difficulty for the theory of price movements.

The success of neoclassical price theory was contingent, to a large extent, on the ability of this theory to abstract from underlying dynamics of economic and other social relations. The focus on price as the ultimate variable of interest was required to reduce social relations and aspects of economic structure into a simple static framework. Firms are commonly assumed to operate in one of 4 possible market structures, which are fixed for the purpose of analysis. The structure affects the way firms set their prices but, since this structure is assumed to be fixed, it cannot be altered by price behaviour. Note that the static framework is not merely the first step toward a broader dynamic theory as neoclassicists often like to stress. If we allow price behaviour by firms such as IBM, General Motors or Exxon to alter the economic environment in which they operate, we introduce a fundamental 'non-stationarity' that is likely to undermine our ability to 'predict' such price behaviour. For this reason, the assumption that structure affects prices but prices do not affect structure is quite fundamental to neoclassical theory. This stationarity requirement also explains why it is necessary to assume that a businessman is a slave to a single fixed goal such as 'profit maximization.' Without a clear goal, the functional link between objective conditions and price behaviour is severed and prices become 'arbitrary.'

The new ambiguities regarding the autonomy and diversity of business behaviour in modern capitalist economies removed much of the stationarity necessary for a solid price theory. In the neoclassical paradigm, the theorist could ignore the axiomatic nature of 'profit maximization' because this assumption was deeply embedded within the model and was rarely questioned. With the enlarged menu for potential patterns of business behaviour, things became more complicated. The observation that pricing goals and practices were not really fixed and changed with business conditions suggested that we could not ignore structural dynamics in our explanation for prices. It also insinuated that the behaviour of prices could operate to affect underlying structures.

Many economists, it must be noted, failed to realize that the expanding field of 'business anthropology' created a methodological minefield. Instead of shying away from arbitrary assumptions about 'business motivation,' many preferred to ignore the potential hazard and actually welcomed their new freedom to choose. The result has been a flood of alternative models for inflation which could be distinguished mainly on the basis of their arbitrary behavioural assumptions. We consider some of these models in Nitzan (1990).

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