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DISTRIBUTION, EFFICIENCY, AND MARKET POWER

A STUDY OF THE U.K. MANUFACTURING SECTOR,  
1954-73

Tim Hazledine  
Ph.D. Thesis  
University of Warwick  
Department of Economics

Submitted April 1978

TABLE OF CONTENTS

	<u>Page</u>
0: <u>Introduction and Summary</u> .....	1
I. <u>A Critique of the Received Theory of Industrial Organization</u>	
1. Introduction.....	13
2. Oligopoly Theory.....	18
3. Conduct and Structure in the Long-run (the Entry Threat).....	44
4. Summary and Qualifications.....	57
II. <u>Attempting A New Synthesis</u>	
1. Introduction.....	66
2. The Property Right of Market Power.....	67
3. The Price-setting Process.....	85
4. Summary of Chapters II.2 and II.3.....	111
5. Defining Profitability.....	118
III. <u>The Model Tested and Compared</u>	
1. Specifying the Model.....	115
2. Estimating the Model.....	142
3. A Survey of Previous Work on UK Structure-Profitability Relations.....	173
IV. <u>Extensions and Implications</u>	
1. The Dimensions of Industrial Performance in the Long Run: Salter Extended.....	193
2. Some Implications for Policy and Welfare...	231

Footnotes

Bibliography

APPENDIX A: Productivity Fluctuations in the Short-Term

APPENDIX B: Extensions of the Price-Setting Model

APPENDIX C: Data and Variable Definitions.

## SUMMARY

The Thesis is an investigation into the relationship between market structure and profitability. First, existing oligopoly and entry-threat theories are examined and found not to provide a plausible basis for a durable structure-profitability link. Then an alternative model is set up, based on bargaining concepts. This model is successfully tested with data on UK manufacturing industries from five Censuses of Production between 1954 and 1973. Market power is found to be responsible for durable monopoly profits.

Finally, an analysis is undertaken, following Salter, of rates of change of some dimensions of market performance, and the implications outlined for policy and economic welfare of these and the previous results.

### Acknowledgements

The author of this Thesis owes a very considerable debt to his supervisor Keith Cowling for his comments, criticisms, suggestions and, not least, patience.

### Declaration

Appendix A has been published, with Ian Watts as co-author, in the November 1977 issue of the Oxford Bulletin of Economics and Statistics. Ian Watts collaborated in the collection and preparation of the data, and calculated the peak productivity series used in that paper.

### List of Figures

	<u>Page</u>	
II.1	Line of Market Territory	90
IV.1	The Partial Equilibrium Analysis of Monopoly Welfare Loss	252

### List of Tables

III.1	Goodness of Fit ( $r^2$ ) of Predicted versus Actual Profits: Coarse Grid of R-parameters	148
III.2	Goodness of Fit ( $r^2$ ) of Predicted versus Actual Profits: Finer Grid of R-parameters	149
III.3	Profitability Regressions: Averaged Data	152
III.4	Profitability Regressions: Stacked Data	163
III.5	Durability of Monopoly Profits	170
III.6	UK Structure-Profitability Studies: Tabulation of Results	175
IV.1	Means and Standard Deviations of Variables	194
IV.2	Correlation Coefficients between 1958-73 Rates of Change	201
IV.3	Rate of Price Change Regressions, 1958-73	204
IV.4	Correlation Coefficients between 1958-63 Rates of Change	205
IV.5	Correlation Coefficients between 1963-68 Rates of Change	206
IV.6	Correlation Coefficients between 1968-73 Rates of Change	207

## 0. Introduction and Summary

This Thesis is concerned with identifying the effects that the structure of markets have on the economic performance of firms. It is presented in four Parts, each including two or more chapters. In Part I, the existing theoretical underpinnings of the structure-performance relationship are surveyed and evaluated. From this critique, in Part II, is developed a 'New Synthesis' -- a theoretical approach to the modelling of market behaviour which provides, I argue, both a plausible rationale for the interpretation of observed structure-performance characteristics, and a better chance of more precisely specifying empirically such relationships.

These claims are put to the test in Part III, in which the new model is estimated with data on UK manufacturing industries, and the results compared with those of other studies of the same phenomena.

Part IV elucidates some extensions and implications, in two chapters. In the first of these, the dynamic performance of the UK manufacturing sector is examined, building on the work of Salter. In the second chapter, I outline some of the implications for economic welfare and policy of the results of the Thesis.

As well, there are three Appendices. The first reproduces a recently published paper that contains evidence, relevant to the assumptions used in the traditional specification

of structure-performance models, on the nature and extent of 'short-term' (quarterly) fluctuations in labour productivity as employment varies. Appendix B generalizes somewhat the model of the pricing process put forward in Chapter II.3, and Appendix C gives definitions of all the variables used in the Thesis, sources of data, and Tables showing the actual values of the more interesting calculated series.

In what follows, I attempt to summarize, in point form, my analysis and results, with the exception of the final chapter, IV.2, in which the implications for welfare and policy are sketched out in a fairly speculative and cryptic manner which should not require any further summarizing.

1. Although, in empirical work on the structure-profitability relationship, estimating equations are typically only casually connected with economic theory, there does, in fact, exist two quite sophisticated and distinct bodies of relevant theorizing.

2. First, there are the models of oligopolistic pricing -- pricing when each firm's decisions are perceived to have an effect on other firms in the same market. In this tradition, profits earned by an industry depend on the member firms' success in co-ordinating towards setting the industry-profit maximizing (monopoly) price, and this is supposed to depend on the numbers of firms, basically because, the fewer the



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firms the less incentive each has to 'chisel' on a price agreement, as the price-elasticity of demand of each seller becomes a larger fraction of the total market elasticity. Thus, oligopoly models predict a relationship between industry structure, reflected in some measure of the size distribution of firms in an industry, and profitability.

3. Such models do not provide a plausible rationale for a durable structure-profits relation. If the industry's product is homogeneous, it should not be difficult for firms to achieve a consensus on price, given that it is manifestly in their joint interest to do so. Apart from explicit collusion, there are a number of methods, such as price leadership by one firm, whereby this can be done. If each firm produces a differentiated, or heterogeneous product, the problem of interdependency with a few rivals fades away -- each seller essentially is competing with all other sellers for the customer's money, so co-ordination is infeasible, and firms will set price more-or-less independently.

4. The second class of theoretical models has price-setting constrained by the threat of entry into a market by new competitors, should unusually high profits be therein observed. Curiously, and disturbingly, this literature assumes away the oligopoly problem of co-ordination between existing firms by positing a monopolist or price-leader. Entry-threat models come in two variants:

5. Models of 'smooth' entry -- industries with no economies of scale -- have all supracompetitive profits eventually eliminated by new entrants, with existing firms having only some power to influence the adjustment path along which such entry takes place. These models do not yield very surprising or interesting results.

6. Models of 'lumpy' entry give existing firms some power to permanently set price above the competitive level if production cannot efficiently take place below some minimum scale of output, so that any firm entering at such a scale may add so much to industry capacity that the market is 'flooded' -- market-clearing price falls to a level that is unprofitable for all sellers. Within this approach, scale economies and the industry elasticity of demand determine profits; other structure variables, such as the size distribution of firms, do not matter.

7. In any case, all the entry-threat modelling is crucially weakened by the implausibility of its basic assumption that it is the price presently charged by existing firms that is taken by potential competitors as an indication of what would happen after entry takes place. Of all the instruments available to firms, price is surely one of the most flexible -- there is no reason why a firm should not quickly change its price, should it find it advantageous to do so. Thus, existing firms need only to threaten to lower their price, should entry occur, to indefinitely deter such

an action. To make credible such a threat, they need only have sufficient capacity on hand, or be able to add capacity more quickly than outsiders can, so that they could meet the additional demand generated by a lower, post-entry price.

8. In summary; neither oligopoly nor entry-threat theories generate any reasonable rationale to expect to find any firms not charging a monopolistic profit-maximizing price; in particular, they do not establish a durable a priori relationship between differences in market structure and variations in profitability.

9. However, if we therefore put aside the traditional models, we are faced with another problem -- estimated price elasticities of demand are too low to be consistent with monopolistic behaviour, when this is interpreted, in orthodox fashion, to be the choice of a profit-maximizing price and quantity from a fixed demand curve.

10. The key, I suggest, to resolving this paradox is to question the fundamental notion of a 'demand curve' as a locus of price-quantity combinations on which the seller unilaterally chooses his preferred position -- that is, to question (a) whether price is, in fact, the important instrument whereby firms affect their sales, and (b) whether sellers freely choose the price at which they sell.

11. There is, indeed, evidence that price is not an important marketing instrument -- that firms typically increase their sales not by moving down the demand curve, but by pushing it out; by developing new products, 'opening up' new markets, establishing customer 'goodwill', strengthening distribution networks, and so on. Firms cannot sell any quantity they choose at the going price; they must devote resources to achieve and maintain a share of the market.

(12. Note that the implicit assumption that selling output is a trivial matter of adjusting price underpins both the oligopoly and the entry-threat 'problems' -- in oligopoly models because any firm, by slightly shading its price, can greatly increase its market share at the expense of its rivals, and in the entry-threat context since any new firm supposedly can instantly and costlessly sell whatever quantity it wishes upon entering a market).

13. How, then, is price constrained, if not by its effect on demand? I propose that price and output are best (in a general analysis) viewed as being separately determined. Output, is set, for individual firms, by their success in the marketing activities mentioned above, and for all firms by shifts in the composition and magnitude of aggregate demand. These factors are not analysed further here.

Price is seen to have primarily a distributive, rather than allocative, function, and to elucidate and identify empirically the factors that determine the distribution of

income in markets is the main aim of this Thesis.

14. I propose that the distribution of the gains from exchange is most usefully perceived as a bargaining process between sellers and buyers.

15. The outcome of the bargaining process (the price charged) is determined by the relative power of each party, defined as the costs one party can impose on the other by not trading.

16. Market power is shown to depend (a) on the 'territory' owned by sellers and buyers; conceived as the 'distance', in product characteristic space, that a trading partner would need to travel to reach the next best alternative, should the exchange not be consummated, and (b) on the price at which that alternative is traded.

17. A firm's territory is determined by such factors as its ownership of patents, its knowledge of special techniques, its reputation for reliability, its control over supply and distribution networks, the skill of its managers, and locational advantages. Such factors cannot be dissipated by new entry or oligopolistic price chiselling -- they are property rights held by the firm, often with legal title attached, but in any case under the firm's control. Therefore, they provide the basis for a concept of market power as a durable phenomenon.

18. The price of the next-best alternative -- the price charged at the 'border' of a firm's territory -- depends on the market power of the firms found there. There exists in the economy large numbers of small or 'fringe' firms selling products which are in some degree substitutable for those offered by larger firms with substantial market power. These fringe firms are too small to possess sufficient market power to justify, alone, a price that would keep them in business. Their prices are therefore set at levels which reflect the opportunity cost of self-employed entrepreneurs -- the risk-adjusted wage they can earn by working for someone. This opportunity cost is thus determined by factors such as the real wage, and society's attitudes to risk, which are formed outside the market-power system, and so are exogenous to it. Accordingly, the presence of a competitive fringe sector provides an ultimate constraint on the prices that can be charged by firms with significant market power, so that our model can easily be made determinate (whereas oligopoly models in which all prices are endogenous, are more difficult to squeeze stable solutions from).

19. The bargaining process is not, however, mediated by market power alone. Notions of empathy and fairness may qualify the exercise of brute market forces. This is particularly likely to be so in what I call the explicit bargaining situation, of firms trading with other firms in uniquely negotiated deals.

In these cases, networks of trading partners, relying substantially on trust and custom, develop, and soften the division of the gains from exchange when the market powers of buyers and sellers differ.

20. Even in the situation of firms selling to final consumers, or large firms selling to (or buying from) many small firms, in which the value of each trade is too low to make it worthwhile to customize each, empathy and fairness may still play a part in what might be called a process of implicit bargaining. However, we expect that selling at a 'take-it-or-leave-it' price will still be more profitable than negotiating explicitly with other firms.

21. These theoretical considerations lead to the specification of a model of profitability as a function of market structure factors. This model is tested against data on 51 UK manufacturing industries averaged over five Census of Production years -- 1954, 1958, 1963, 1968, and 1973.

(22. A feature of the estimating equation is the definition of profitability as the ratio of profits actually earned in an industry to the profits that would be earned by all the resources committed to that industry were these deployed instead in the competitive, or fringe, sector of the economy. It is suggested that this variable, called 'Surplus', will be a better measure of profitability than either of those used in previous work, since one of the latter -- the



profit margin on sales -- ignores the capital input (and attaches a spurious significance to the stage of production at which an industry operates), and the other -- the rate of return on capital stock -- allows no markup on the throughout of variable factors such as labour and materials.)

23. Estimation of the model is a success. The goodness of fit is quite satisfactory, and most coefficients show comfortably significant t-statistics. Features of the result are:

- (a) Surplus outperforms the profit margin on sales as a dependent variable.
- (b) Seller concentration is not a significant regressor, but the average size of plant is.
- (c) Buyer market power factors are important. In particular, it is more profitable to sell to consumers than to other firms, as expected, and the 'piggyback' effect dominates the countervailing power of higher market concentration of buying industries in their reselling markets -- some of the monopoly profits buyers earn in their own selling activities are passed back down to their suppliers.

24. We also support the hypothesis that market power is a durable phenomenon -- industries earning higher than average profits at the beginning of the sample period revealed no tendency to have these profits competed away by the end of the period.

25. These results are compared with those of other studies of the UK manufacturing sector. The apparently substantial improvement achieved over the earlier work is probably due to:

- (a) The use of the Surplus dependent variable.
- (b) The incorporation of buyer-power factors.
- (c) The reduction in cyclical 'noise' through the use of data averaged over five Census years.

We may note that, looked at as a whole, the previous work also finds little evidence of seller concentration having a significant influence on profits.

26. Although market power appears to account for a good deal of variation, across industries, in profitability, no relationship could be discerned between such power and changes in profitability over time. Over the period 1958-73, changes in industry prices matched very closely changes in the opportunity costs of the labour, materials, and capital per unit of output.

27. It seems that the effect of plant size on profitability cannot be attributed to economies of scale across industries, although particular industries that did increase their scale of plant between 1958 and 1973 by more than the norm, apparently did so because of associated cost savings.

PART I: A CRITIQUE OF THE RECEIVED THEORY  
OF INDUSTRIAL ORGANIZATION

1. Introduction

Although some of the work to be surveyed in this Part was carried out over a century ago, the search for general theoretical propositions in the field of industrial organization can be placed within the now familiar structure-conduct-performance paradigm of Mason (1949) and Bain (1959). Structural, or slow-changing factors, such as market concentration, demand elasticities, scale economies, and degree of openness to international trade, determine firms' behaviour or conduct -- advertising programs, price-setting policies, attempts to collude and to lobby, expenditure on research and development -- with results that can be measured by various interesting performance variables, such as profitability, rate of technological advance, propensity to minimize costs in the short run, and macroeconomic concerns about inflation and the level of employment.

Of these measures of performance, and their relationship, through conduct, with structure, it is the first -- profitability -- that has received by far the most theoretical and empirical attention in the industrial organisation literature (cf. Weiss' survey (1971)). This emphasis, which probably has its roots in the relative analytical tractability and empirical accessibility of the structure-

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Of these measures of performance, and their relationship, through conduct, with structure, it is the first -- profitability -- that has received by far the most theoretical and empirical attention in the industrial organisation literature (cf. Weiss' survey (1971)). This emphasis, which probably has its roots in the relative analytical tractability and empirical accessibility of the structure-

profits relation, has meant that, of the two great themes of economics -- the distribution of income and efficiency -- industrial organization has predominantly concerned itself with the former. The question typically asked is 'Does the existence of certain structural characteristics in an industry allow its member firms to make more money than the norm?'

Of course, the implication for allocative inefficiency of a divergence between price and cost has not been ignored, and will not be here (Chapter IV.1), and I will also devote some attention to the behaviour of costs (Chapter IV.1) and to macroeconomic questions (Chapter IV.2), but in the core of the thesis -- this and Parts II and III -- we will be concerned with Distribution; that is, with the existence of a stable structure-profits relationship.

A search for the 'theory' of the structure-profits link is not necessarily straightforward. In the empirical econometric literature, one often comes across sentences of the sort -- 'economic theory tells us to include variables X, Y, and Z in our regression equation'<sup>1</sup> -- but such claims have never, until the recent Warwick work (returned to in Chapter I.2) been supported by an explicit exposition of this 'theory', or even, in most cases, references to where such an exposition can be found.

Indeed, many industrial organization economists might be prepared to admit, with Weiss, that

"Work on the parameters of the concentration profits relationship has been woefully free of theory" (1971, p. 374).

that is, that the theory doesn't actually exist.

Nevertheless, there is, indeed, a quite rich body of industrial economics theorizing, the practitioners of which, as we shall see, have settled in one or the other of two non-communicating camps, which are, however, built on the same traditional territory, borrowed from orthodox microeconomics and General Equilibrium theory.

This common ground is the fundamental concept of an 'industry' or 'market', defined by the existence of a 'demand curve' -- a downward-sloping locus of points in the price-quantity plane, showing for each quantity of output of an industry the price at which all that output will be sold -- the market clearing price.

The demand curve is a concept so familiar to economists that we hardly think to question it. But consider its implications as used:

- 1) The industry sells a homogeneous good -- the product of all firms can be aggregated, and is sold at a common price;

- 2) Price is an effective and customary instrument to alter sales -- within the limits of the curve there exists a price that will sell each quantity;
- 3) There is no contact with consumers, who are an undifferentiated mass of price-takers;
- 4) The demand curve is invariant to the actions of existing firms.

In the next two chapters, I will attempt to argue that these implications are unrealistic as general propositions, and that their weaknesses crucially undermine the validity of the theories built on the foundation of the demand curve concept.

The demand curve serves as a taxonomic device to divide all the firms in an economy into two subsets -- those within and those outside of a particular industry. We can make the promised division of industrial organization theorizing into two camps according to the assumptions made about the relative importance of the two subsets.

First, beginning with Cournot (1963), there are the oligopoly theorists who attend to the problems of the subset of firms behind the demand curve, and ignore the others. These will be considered in Chapter I.2.



Second, we have a younger literature on the threat of entry, which does concern itself with the actions of firms outside an industry, but assumes away any internal oligopoly problems by postulating a monopolist or dominant firm already in place. This we look at in Chapter I.3.

The division of firms into the two sets is quite arbitrary -- there are no inherent differences assumed between firms inside and outside a market, only in whether or not they collude with each other. Accordingly, the split of theorizing into two camps is also quite arbitrary, as will be seen, and seems to have arisen solely out of the analytical problems involved in formal model-building under more general assumptions.

Finally, Chapter I.4 summarizes the arguments of Part I, and also lists five qualifications to the generality of these arguments.

## I.2 Oligopoly Theory

In a recent survey, Phillips claims that there is a 'paucity of theory' to support the hypothesis that industry profitability is a function of market concentration (1976, p. 241), but gives rather a long list of exceptions (as many as seven distinct theories or 'quasi-theories') and fails to mention the compact but cohesive body of work, beginning with Cournot in 1838, and culminating, to date, in the tightly specified models of Cowling and Waterson (1976) and Waterson (1976), which do attempt to set out a formal basis for the structure-profitability hypothesis.

In this literature, the standard microeconomic analyses of perfect competition and perfect monopoly are accepted as polar cases, delimiting the lower and upper bounds of industry profits. The problem is to explain the observed continuum of profit rates between these bounds, and the key to so doing is to note that behind each demand curve we typically find a sizeable proportion, perhaps a majority, of output produced by a few -- say, less than ten -- firms. This is not monopoly, but it may plausibly be considered not a good approximation to the 'atoms' of perfect competition, either. This situation is called 'oligopoly'.

With such numbers, it becomes likely that firms are interdependent -- that what one firm does will significantly affect the others -- and Cournot's contribution was to

build a model incorporating interdependence along with an assumption about firms' reaction to it. His result was that the margin of price over cost depends on the number of firms (Cowling and Waterson, 1974, p. 3), but in getting to this, he made the assumption that firms actually assumed no reaction by their rivals to any change in their own output. Cowling and Waterson have generalized the Cournot model to allow for non-zero expected reaction (or 'conjectural variation', as it is known), as well as differences in cost curves among firms, and arrive at the following equation to explain the ratio of profits ( $\pi$ ) to total revenues (R):

$$\pi/R = - \frac{H}{n}(1+\lambda) \quad \dots (1.1),$$

where  $n$  is the industry price elasticity of demand,  $H$  is the Herfindahl measure of market structure

$$H = \sum (X_i/X)^2 \quad \dots (1.2),$$

and  $1+\lambda$  is the conjectural variation of total industry output  $X$ , in response to a unit increase in a particular firm's output,  $X_i$ .

The Cournot solution ( $\lambda = 0$ ) is a special case of (1.1); in general, industry profitability will be greater or less than the Cournot value according to whether  $\lambda$  is greater or less than zero; that is, whether each firm expects the others to match or to accommodate its change in output.

In sharp contrast to the vague or non-existent appeals to 'theory' endemic in the empirical industrial organization literature, The Cowling-Watson equation gives a remarkably tight specification of the structure-profits relation, with even the intercept term constrained a priori to be zero. Indeed, the specification is almost uncomfortably tight -- as we shall see in Chapter III.3, data problems force some rather strong assumptions before (1.1) can be estimated.

There are other oligopoly pricing models (cf. Silberston, 1970, for a survey), making other assumptions about the nature of conjectural variations, and thus reaching results which differ in detail; but since all the models share the basic assumption that somehow it is not possible for firms to collude perfectly (that is, to charge the price that a monopolist would charge) and get the result that the ability to get close to the monopoly price is a function of the number of firms (more generally, of some index weighting the number of firms by their relative size, such as the Herfindahl), I shall not here survey the literature, and will take the Cowling-Watson expression (1.1), with its convenient specificity, as representative of the results that can be achieved by the oligopoly price-setting approach.

In our appraisal of the oligopoly pricing model, we shall first look directly at the empirical plausibility of the profit rates implied by (1.1), given reasonable values of the structural parameters.

Note, first, that formula (1.1) does not do badly in replicating average profit/sales ratios. The mean value of the Herfindahl for UK manufacturing industries is probably about 0.12.<sup>2</sup> Deaton's values for price elasticities for the manufacturing industries in his sample average to -0.53 (1975, pp. 60-61). If we take an intermediate value of  $\lambda$ , say 0.5, and plug these numbers into (1.1), we get a figure for the 'average'  $\pi/R$  of about 0.3. While this is rather greater than the actual manufacturing average  $\pi/R$  of around 0.2, it is not drastically out of line -- alternative assumptions about  $\lambda$  or about a downward bias in the elasticity estimates will easily bring (1.1) down to 0.2.

However, although it is quite possible that oligopoly models can be developed which yield reasonable  $\pi/R$  ratios,<sup>3</sup> I will argue next that they should not be; that models built on assumptions implying that factors limiting the ability of oligopolists to collude are the factors limiting industry profitability are fundamentally misconceived.

I will suggest that, if one wishes to make the least unreasonable general assumption possible (that is, the assumption that, although not true without exception, is widely out of line for the fewest possible number of industries), one should go to the other extreme and assume that the price collusion problem is insignificant, either because price collusion is easy or because it is unnecessary.

Consider first the easiness of colluding. Within the orthodox industry-demand curve paradigm, it is essential that reaching a collusive agreement on price not be easy. For if it is, the oligopolists will presumably agree to set price at the joint-profit maximizing level -- the price a monopolist would charge. But, most empirical evidence implies that this monopolistic price is indeterminate, since most industry price elasticities are absolutely less than one. All of Deaton's estimates for UK manufacturing industries are less than one (loc. cit.) and similar results come out of studies for the US and Canada (Houthakker and Taylor (1970), Hassan, Johnson, and Green (1977)). These elasticity estimates may be biased towards zero by failure to control for quality change or inadequate allowance for lagged responses of quantity demanded to price changes. But the bias we would have to impute in order to make the data consistent with the observed average profits/revenue ratio of about 0.2 under monopoly conditions is implausibly large. Substituting the monopoly

values of  $H$  and  $\lambda$  (1 and 0, respectively) into equation (1.1) shows that an elasticity of -5 is needed to generate the average ratio of profits to revenue. Thus, it is crucial that co-ordination be imperfect.

However, the evidence does not, in my interpretation, support this. Even without explicit collusion, which is, anyway, not illegal in the UK, there seem to be a number of effective ways in which the big firms in an industry can allow a co-ordinated industry price to emerge, as Scherer's survey illustrates (1970, Chapter 6). The acceptance of one of the firms in an industry (usually the biggest) by the others as a 'price leader', whose price changes the other firms reliably follow, is perhaps the most important co-ordinating device. Scherer's list of examples (p. 167) -- cigarettes, steel, aluminum, farm machinery, synthetic fibres, metal cans, and automobiles -- accounts for about 15 per cent of US industrial added value, and is not intended to be exhaustive.

Really, it is not very surprising that oligopolistic firms should be able to come up with ways to avoid frittering away profits by unnecessary price competition, given that it is manifestly in their interest to do so. Economists may have been misled by their own difficulties in formally modelling price formation into assuming that firms find it just as hard to actually do it.

The illusory modelling problem stems from the demand curve assumptions. It is not that the oligopolists are supposed to have any difficulty in knowing what the joint profit-maximizing price is, as might reasonably be thought an important real-world problem. On the contrary, demand and cost curves are typically assumed to be known and fixed. The problem is really not with price at all. It is with quantity; with market shares. Under the conditions of the one-market demand curve, any firm slightly undercutting its rivals' price can sell as much output as it wishes (within the limit of total industry demand). This extreme price responsiveness of market share gives each member of a cartel or collusive agreement an incentive to try and cheat his colleagues by price-chiselling. All the oligopoly models can be seen as attempts to formulate assumptions about rivals' reactions which imply a stable distribution of market shares, to counter the inherent slipperiness of the oligopoly situation as it is initially set up. That these models predict a higher price as the number of firms diminishes is just due to the smaller incentive to cut price as a firm's market share increases (that is, as a firm's demand curve becomes less elastic), rather than to a concrete accretion of market power as firms become bigger.

Is, then, sharing the market really a basic problem limiting collusive success (given the evidence of Scherer, cited above, that the process of achieving price



co-ordination does not seem generally to be too difficult)? Such information that is available suggests that it is not. From a recent survey to which 205 'medium to large' UK companies responded, Atkin and Skinner concluded that

"pricing is an area of marketing which is totally neglected. Pricing as a marketing tool is rarely practised" (1975, ii).

Only 17 per cent of their respondents reported price to be 'vital' in overall marketing strategy (p. 74). To be consistent with the orthodox oligopoly model, 100 per cent should have so replied. Porter, in a study which builds on both the normative management science and positive industrial organization approaches, contrasts the economists' preoccupation with problems of interdependence with the Business Schools' propensity to focus almost exclusively on the individual firm in its solitary quest for 'excellence' (1976, p. 70). Since the livelihood of Business Schools depends rather more than that of economics faculties on the realism of their analyses of the corporate economy, it may not be too unreasonable to suppose that the former group are the more likely to be on the right track.

On reflection, I do not find this conclusion too hard to take. I would suggest that 'marketing' -- the improving or defending of market share -- is typically far from a trivial matter of adjusting price, even when there is

a relatively high substitutability between the products of different firms in the same industry (as, for example, in all the dominant firm-pricing industries cited by Scherer, above). There are many extremely difficult problems to be overcome -- formulating a promotional and advertising strategy, establishing and maintaining distributive, wholesale and retail outlets, building up goodwill and a reputation for reliability, maintaining supplies of output, financing expansion -- by a successful marketing operation, even when 'the price is right'.

To draw together the argument so far: in industries producing what may be regarded as relatively standardized products, price co-ordination should not present great problems, and, indeed, does not seem to do so. I expect that it is most reasonable, in modelling industrial behaviour, to assume that firms in such markets are able to arrive at a price structure which best serves their interests as a group. Such co-ordination does not founder on the problem of assigning market shares because prices are not typically a major marketing instrument. Market shares, given the price structure, are what firms do compete on, using all the instruments mentioned above, and others, too, no doubt. Price competition is a negative-sum game for the industry group, and so is not indulged in; market share competition by other means is zero-sum, and therefore permissible.<sup>4</sup>

An 'economic' objection to this argument would be that firms have an incentive, analogous to the price-chiselling incentive, to each buy for themselves an increase in market share by purchasing more of the inputs that affect it. Certainly, there is evidence that oligopolistic industries do spend more on advertising than would a monopolist (Cable 1972). However, I suggest that most of the market-share-determining activities -- building goodwill, sales networks, maintaining production, and so on -- should be seen fundamentally not as purchased inputs in elastic supply (though to a degree they can be bought, through the services, say, of management consultants), but rather as the basic fixed factors of business enterprise; namely, its entrepreneurial and organizational skills.

If we do not adopt this point of view, it becomes very difficult to rationalize the persistent differences in market shares of different firms observed in so many industries. Is it really plausible that General Motors outsells Ford or Chrysler year after year simply because it happens to purchase more market share-augmenting inputs than its rivals; or is it more reasonable for the economist to simply assume that General Motors is just better at selling automobiles than the others, and leave it at that.

I have considered the situation when collusion or co-ordination on price is easy. There are, however, many markets in which the necessary assumption of standardized

products cannot be maintained; in particular, capital goods industries, in which production runs tend to be small and products varied to suit the needs of particular customers, and those consumer industries, in particular in the service sector, in which the costs of transporting the product and/or the consumer are a significant aspect in the product specification.

In these cases, products are so multidimensional that agreement, especially non-collusive agreement -- on a price structure may readily be admitted a hopelessly difficult task. But, of course, in such a situation, price co-ordination will typically be unnecessary -- each seller's particular characteristics will dominate most of his market area and it will only be at the 'boundary' of his territory (a concept to be more fully explored in the next Part) that he finds himself in competition, in the traditional sense of marketing close substitutes, with other firms in the industry.

This sort of market is more akin to the 'monopolistic competition' and 'imperfect competition' regimes first analysed by Chamberlin (1933) and Joan Robinson (1933) than to orthodox oligopoly.

Naturally, in this case, market sharing is not a major problem as firms are only at their interfaces competing for the same customers.<sup>5</sup> Instead, firm's marketing efforts will be directed towards shifting out the demand curve within

their turf -- to diverting consumer expenditure towards them at the expense of all other uses for the money. The impact of such marketing behaviour will be too diffusely spread amongst other firms in other industries to induce any sort of retaliatory response.

That this situation of firms conducting their business, particularly with respect to 'short run' decision-making on such variables as prices, largely independently of other firms in the same industry is a quite common feature of market behaviour is supported by one of the more striking results from the Atkin and Skinner survey. There, a remarkable ignorance about the industry price structure is suggested by the small proportion of respondents (7 per cent) who thought that their prices are 'lower than average', compared with 41 per cent who put them 'higher than average' (op. cit., p. 68). Though it is arithmetically possible, given sufficient difference between the median and the mean, that these replies are consistent with generally accurate perceptions of the price structure, the large difference in the percentages does not make such a situation seem very likely.

To another question (p. 64), more than half the respondents reported that the 'percentage by which highest competitive price exceeds lowest' is greater than 10. Of course, the use of the undefined term 'competitive' prevents

any precise conclusions being drawn from the figures, but it may be reasonable to infer from the answers to these two questions that (a) in most industries there is not a unique price, and (b) many firms in such industries do not even know accurately the prices charged by their 'competitors', presumably because it is not worth devoting resources to finding out.

We can muster more direct evidence on the plausibility of the homogeneous good assumption. Stigler and Kindahl assembled and analysed a large amount of data on prices actually paid by buyers, and concluded that 'The Unique Price ... is a myth. Differences among prices paid or received are almost universal' (1970, p. 88). Kravis and Lipsey (1971), in a study of OECD country trade in manufactured metal products (which made up 46 per cent of total OECD exports in 1963) found that 'rarely were any two items identical in the degree required by prevailing price collection methods' because of the 'enormous variety of conditions attached to a sale, other than those usually subsumed under the heading of price' (p. 15). These 'conditions' included discounting, credit terms, delivery time, and additional services. Other evidence that 'industries' do not in general produce homogeneous goods comes from the existence of 'intra-industry trade' -- the situation in which an economy imports and exports the 'same'

product simultaneously. Grubel and Lloyd (1975) measured a good deal of intra-industry trade in the European Common Market at the 3-digit industry level, and found, in a case study of Australian data, that the phenomenon persists at the 7-digit level of disaggregation. In an industry producing a homogeneous product, of course, we would observe imports or exports, but not both.

Thus, there are good grounds for taking product heterogeneity to be the general case, though industries undoubtedly differ in the degree to which their member firms depart from offering a standardized product.

To sum up the argument of the chapter to this point : in industries selling relatively standardized products, limits on the ability to collude on price do not seem to be acceptable as the factor limiting profitability -- a number of instruments, such as price leadership, exist and appear typically to deal well with the co-ordination problem. In markets in which product characteristics are more highly differentiated, the collusion problem becomes irrelevant -- firms are just not interdependent enough for it to matter.

However, it is not now possible to sweep aside market structure analysis, and simply assume 'as if' monopoly price setting at the industry level in the standardised product case, and firm by firm when products are differentiated,

at least while still building upon the traditional demand-curve assumption.

This is because the assumption of monopoly pricing also leads to results that jar strikingly with the empirical evidence. We have already noted the consensus from demand studies that estimated industry price elasticities imply that profits are not being maximized by equating marginal revenue to marginal cost. Nor can this be reasonably explained away by attacking the maximization assumption -- inelastic demand at current prices implies that profits can unambiguously be increased by raising the price; something that even the most stolid of 'satisficers' must eventually become aware of and act on.

In the non-standardized product case, a more appropriate elasticity is that facing individual firms; here there is less evidence (because of the difficulties of getting hold of time-series data at the firm level), but in a study of 107 brands from 16 product classes in 8 western European countries, for which availability of price data permitted the estimation of 43 brand-price elasticities, Lambin found long-run price elasticities for which the mean was 1.813, well below the value of five, noted above, needed to be consistent with observed profit margins (1976, p. 103).



How can these discrepancies be explained? Again, I suggest that the problem arises from the too-unquestioning carryover of the demand curve concept from microeconomics; in this case, the third and fourth of the assumptions listed in Section 1, namely that firms sell to an undifferentiated mass of price-taking customers, and that the curve is invariant to actions taken within the industry.

Assumption (3) is most readily questioned in the context of firms selling intermediate goods to other firms. Since, at least, Galbraith's (1952) development of the concept of 'countervailing power', or buyer market power (cf. Scherer, 1970, Chapter 9, for a survey), it has been accepted as reasonable to suppose that greater market power on the buyers' side of the market will allow these buyers to purchase at a lower price, analogously to sellers' market power enabling them to charge a higher price. However, there is another possibility; in a given industry which purchases, processes and then resells, greater market power will allow it to sell at a higher price, and some of the resulting additional profits may be passed on to suppliers. Waterson, (1976) in fact, develops a model due to Cournot with just this 'piggyback' property, in which buyer market power is unambiguously beneficial to intermediate sellers.

Whatever the net effect of buyer power (which will be empirically examined in Chapter III.2), it seems reasonable to propose that when exchange takes place between small

numbers of sellers and buyers, so that each transaction is 'lumpy' (of significant value to both parties) the terms of such transactions, involving not just price, but, as well, all the other dimensions of product heterogeneity mentioned above (delivery dates, service guarantees, payment terms, and so on), will be settled by bargaining between buyer and seller, rather than via the impersonal mediation of the 'market'.

At issue in the bargaining process, and settled according to the relative power of the parties, is the division of the gains from trade -- the profits earned by each party. The actual quantity traded may not enter into the process at all, or at least only in an 'all or nothing' sense, as when a manufacturer invites a number of firms to tender to supply a given quantity of some material input, with the quantity fixed in proportion to an already decided level of the manufacturer's final output.

The bargaining situation does provide, then, one reasonable rationale for a constraint on the profit-making opportunities of a concentrated selling industry. But in the cases of industries selling to small firms or to consumers without market power, when it would be too expensive to haggle over each individual transaction, the problem of inconsistency between demand elasticities and observed profitability remains.

The key to resolving this may lie in extending the 'bargaining' concept beyond its usual application to the resolution of conflict between a pair of agents (or of the representatives of two groups of agents, as in the unionized wage determination process), which might be called explicit bargaining, to what I call implicit bargaining -- the situation in which the usual bargaining parameters of threats and fairness act to mediate the price determination process, even in a market in which a monopolist sells to thousands or millions of (to him) indistinguishable and unknown customers, with whom he may have no formal contact at all, apart from the actual transaction of the sale.

The notion of implicit bargaining is important to the model developed in Part II, and is analysed more fully there: to avoid repetition, I will not now anticipate this discussion. We should, however, note the implication of the concept for this Critique; if implicit or explicit bargaining determines price setting, then the price-taking assumption (3) must go. For what the latter implies is that the selling firms are able to move freely up and down their demand curves with no customer reaction other than the passive response of changing the quantity purchased. Under the bargaining regimen, in contrast, we may observe (for example) a firm putting up its price with little effect on sales following, say, an increase in its raw materials prices, but we would

not conclude that the firm had been irrational in not raising its price before the materials cost increase, given the observed inelasticity, as the seller might simply not be permitted, by the constraints of the bargaining situation, to raise its price without some justification accepted by both parties.

There has been criticism of the domination of price theory by the demand curve since the studies on 'full' cost and 'cost-plus' pricing (Hall and Hitch, Andrews), though the evidence put forward has not typically been uncritically accepted by 'marginalists' (cf. the debate between Lester and Machlup). Silberston (1970) surveys this literature. Of Atkin and Skinners' respondents, 51 per cent reported that they priced by adding a percentage to costs, 39 per cent by fixing the required gross profit margin on selling price (essentially a full-cost procedure) and 21 per cent used some other, non-cost related, method.<sup>6</sup>

While thus observing prevalence of some sort of cost-plus pricing gives us no information about the determination of the size of the markup, and how this may differ across firms and over time, possibly in response to demand factors, the numbers do, I feel, at least put the burden of proof on those who would continue to build on the simple supply-demand model to demonstrate empirically the

consistency of the 'marginal revenue = marginal cost' predictions with the cost-plus vocabulary predominantly employed by businessmen when they describe their pricing procedures. This, to my knowledge, has not been done.<sup>7</sup>

Equally, it is reasonable to make a positive, attempt, as I do in the next Part, to build a theoretical pricing model in which cost factors enter with some independence of demand curve considerations. Indeed, such an attempt is certainly necessary, if the margin of price over costs is to be explained by something more interesting and useful than recourse to 'rules of thumb' or 'common practice'.

We turn now to the fourth and last of the demand curve postulates; the assumption that the curve is invariant to whatever actions are taken by existing firms within it. We have already noted that firms can deliberately use expenditure on advertising to shift the curve outwards; however, this represents a fairly straightforward extension of the simple price-quantity profit maximizing model -- it presents no fundamental problems to these models as they are presently built.

Much more troublesome is the suggestion, which has been made in the 'threat of new entry' literature, that the pricing decisions of existing firms influence the rate at which outside firms decide to enter an industry, and thus

shift the demand curve faced by the original industry member. Extending the oligopoly analysis to potential firms may be possible, in principle, but, in fact, appears to present analytical difficulties that have so far prevented the generalization from being successfully achieved.

Thus, we find two quite distinct bodies of industrial organization theorizing; one, the oligopoly pricing models considered in this chapter, which ignore other than presently operating firms, and second, the entry-limiting price work which, as will be noted in the next chapter, reduces its analytical problem to manageable proportions only by assuming away all oligopolistic co-ordination problems amongst existing firms. The two theories give two independent predictions of what price will be charged by a particular industry which will, in general, differ -- surely, an unsatisfactory state of affairs, but one which does not seem to have concerned empirical researchers, who typically sprinkle their regression equations with variables pertaining, some to oligopolistic co-ordination, others to entry barriers; and bless the specification with a call to 'The Theory', as though an all-encompassing theoretical framework did, in fact, exist.

An exception is the work of Cowling and Waterson (1976). These authors draw on the results of Spence (1974, 1977) in whose model excess capacity is the entry-limiting

instrument, which leaves the pricing decision to be made independently of long-run (demand curve-shifting) consideration. Accepting this model (on which more will be said in the next chapter) would justify the limited scope of oligopolistic co-ordination models, and imply that empirical testing should exclude entry-limiting variables from the regression equation -- this Cowling and Waterson, rather boldly, do.

Of course, from the point of view of this thesis, in which it has already been argued that oligopolistic ability to co-ordinate is not, in general, the factor constraining price, and in which, in the next chapter, qualified approval will be given of the Spence model, it must be concluded that inconsistency of the two bodies of theories is a non-problem, not because one theory dominates the other, but because neither provides an acceptable formulation of the price setting process. To follow these criticisms with more substantive suggestions is the task undertaken in Part II.

An empirical regularity which may be awkward for oligopoly theory is the relationship that has been observed, mainly in the Business School literature, between the profitability and market share of individual firms. In a study of 620 'businesses' of 57 North American corporations for the years 1970-72, Buzzel et. al. found that 'a difference

of 10 percentage points in market share is accompanied by a difference of about 5 points in pretax ROI [rate of return on investment]' (1975, p. 97). Delcombe and Bruzelius (1977) found a similar relation for 18 'product centres' of a multinational engineering company. Dalton and Levin (1977, p. 34), using US data, found that 'profit rates rise with market share' (though only when the four-firm concentration ratio was greater than 45 per cent in a firm's industry).

Miller claims that the relationship cannot be explained as being due to market power within the framework of oligopoly theories, since these have the feature that the 'increased profitability of concentrated industries is due to higher prices, which are received by the small firms as well as the large ones' (1978, p. 476). Mancke suggests that the observed relationship could just be due to luck (1974). This point of view is attacked by Caves et. al. (1977), who refer to Gale's (1972, p. 413) suggestion that a larger market share may be expected to yield high profitability within oligopolies by:

- 1) giving a product differentiation advantage;
- 2) allowing firms to participate in an oligopolistic group;
- 3) increasing firms' bargaining power in this situation;
- 4) allowing economies of scale.



All of these rationalizations are awkward. Product differentiation seems, indeed, to be an important factor -- Buzzel et. al. discovered that businesses with more than 40 per cent of their market were judged to have markedly higher product quality, and slightly higher prices, than the rest -- but, as I have argued above, this takes us away from the oligopoly situation where in a homogeneous product makes it necessary for firms to co-ordinate their actions.

The participation argument is not convincing -- in oligopoly theories, with price the only marketing instrument, all any firm need do to 'participate' in an oligopoly is to set its price according to the assumed decision rule. Nor is it clear why larger firms should have more bargaining power in the oligopoly situation.

Indeed, the opposite may be more likely if, as Miller suggests (1978, p. 476) the 'leading firm in a concentrated industry frequently has to restrict production to maintain the price, leaving it with higher overhead costs per unit of production. The smaller firms, operating under this price umbrella, may be able to ensure capacity operation simply by shading price to the extent necessary, a strategy which would bring about a collapse of the price structure if predicted by one of the leading firms'.

As for economies of scale, oligopoly models do not tell us why some firms will succeed in capturing them while others remain small.

A reasonable explanation for the correlation between market share and profitability may be that both variables are influenced by the distribution of the fixed factors of business enterprise -- the basic effectiveness of entrepreneurs and organizations. I suggested earlier in the chapter that such factors, as reflected in differences in marketing skills, may account for the inter-firm distribution of market shares, and it seems plausible that organizations that are able at marketing may also tend to be good at producing. As well, a low-cost producer, especially when such factors as favourable location, or ownership of important patents contribute to its cost advantage, may find it profitable to sell over a wider market area than will its less favoured rivals.

While inter-firm differences in costs can be squeezed into the oligopoly framework, it seems hardly worthwhile doing so, since their thrust is towards recognizing the heterogeneity of firms and products, and away from the slippery interdependencies underlying the oligopoly problem.

Powerful empirical support for these arguments that we should move away from considering market power as an industry level phenomenon is given, for a US sample of 245 large corporations, by the results of Shepherd (1976), who found that when market share and industry concentration variables are included together in a regression explaining the rate of return on equity, the former variable is strongly significant, and the latter very insignificant (Table 1, p. 40). When just industry concentration is used as a regressor, it is quite significant (Table 2, p. 47), which suggests that it acts as a proxy for market share, and should always be interpreted as such.

### I.3 Conduct and Structure in the Long Run (the Entry Threat)

As we noted in the previous chapter, another strand of industrial organization theorizing, concerned with the likelihood of new firms entering an industry, and with the proper response to the entry threat by the firms already in place behind the demand curve, has developed independently of the oligopoly pricing models, which take as given the number of firms.

Interest in modelling this situation was, initially at least (that is, before mathematicizing set in) nourished by the traditional and deeply felt belief in the strength of the forces of competition as an ultimate constraint on the exercise of monopoly power. Scherer writes that

"It is (the) entry phenomenon, more than long-run substitution between different products, which prompted J. M. Clark, Sir Roy Harrod, P.W.S. Andrews, and others, to insist that the long-run demand curves confronting monopolists and oligopolistic groups tend to be highly elastic, approaching the horizontal" (1970, pp. 200-21).

That is, monopoly profits are a short run, transitory phenomenon. This doctrine has, of course, powerful laissez-faire implications, although the possibility remains of the competitive forces working sufficiently slowly to justify some public intervention to hurry along the process.

Formal modelling has proceeded along two paths, depending on whether entry is 'smooth' or 'lumpy'. When the long-run average cost curve is horizontal, so that firms can enter efficiently at any scale (the smooth case), the modeller's problem is basically just to explain why monopoly profits are not wiped out instantly. This is done by introducing some sort of friction into the entry process, which slows down entry, and by giving interest to the story by assuming that the existing firms have some control over the friction. The usual assumption made is that the rate of entry is a function of the excess of price ( $p$ ) over some normal or competitive level ( $\bar{p}$ ) (Gaskins (1971), Pashigian (1968), Lee (1975), Quandt and Howrey (1968)), and the usual result is that it is optimal for the existing firms to follow some trajectory in adjusting price from  $p$  to  $\bar{p}$ . Some twists have been added; for example Jacquemin and Thisse (1972) allow the existing firms some power to mould market structure, Gaskins suggests expenditures to change the barriers, and Kamien and Schwartz (1971) and de Bondt (1976) introduce uncertainty, but the problem solved remains the same -- essentially to determine the most profitable way for existing firms to delay the inevitable competing away of all their monopoly profits.

Even if its assumptions were valid (and below it will be argued that they are not) I would find it difficult to argue that the dynamic limit pricing literature has

contributed insights that are sufficiently surprising or useful to justify the quite substantial resources of mathematical ability and journal pages that have been committed to it.

Of more interest are the models in which, due to some indivisibility such as a minimum efficient scale of production, entry is likely to take place either at some lumpy rate large enough to affect the demand curve facing existing firms, or not at all. This situation was studied first by Bain (1956) and Sylos (1962), and their work synthesized and extended by Modigliani (1958) in a famous paper. These are comparative static models, yielding a price, to be charged indefinitely, which will optimally forestall entry. In a survey paper, Bhagwati reports that

"The premium that can be charged, consistent with the prevention of entry, varies directly with the minimum scale of the entrant's plant, and inversely with both the size of the total market and price elasticity of industry demand," (1970, pp. 306-7).

These conclusions, though not surprising, are certainly more interesting, and lead to more empirically testable hypotheses, than those of the dynamic limit pricing theorists.

However, both groups of models share two basic assumptions, one restrictive, the other unrealistic, which, in my opinion, must be considered seriously to limit their usefulness.

The restrictive assumption is that there is no co-ordination problem for the existing firms -- either there is just a monopolist, or there is a price leader to take the decision on the correct response to the entry threat. As Stigler puts it, this solves the oligopoly problem (the sole concern of the models discussed in the previous section) by 'murder' (1968, p. 21). Of course, from the point of view being put forward here, such an assumption should not matter too much -- if, as argued above, a limited ability to co-ordinate price setting is not a plausible factor constraining profits in the absence of potential entrants, it is not likely to be much more of a problem when potential entrants are to be considered.

Nevertheless, it is a disturbing characteristic of the whole field of industrial economics theorizing that two such blithely independent paths should have been followed with so little concern for their inconsistency.

Of substantive importance is the second basic assumption made in the entry literature; namely that it is the excess of price or profits ( $p$ ) above 'normal' or 'competitive' price ( $\bar{p}$ ) that induces entry.

This assumption appears in, and, indeed, is fundamental to, all the entry threat models, with the exception, returned to below, of Spence's work.

The rationale of the assumption is that an excess of  $p$  over  $\bar{p}$  is a signal, in fact the only signal, received by potential entrants concerning the profitability of being in an industry. It is assumed that

"The entrant is likely to read the current price policies of established firms as some sort of a 'statement of future intentions' regarding their policies after his entry has occurred," (Bain 1956, p. 95; quoted by Scherer, 1970, p. 229).

I find this unconvincing, for two reasons. First, of all the important variables determining the profitability of an industry, price is possibly the least permanent. Prices may be changed overnight, whereas other factors, such as the rate of technological advance, the efficiency of management, the rate of growth of the market, and some government policies, change only over years or decades. That is, of all the information that a potential entrant will amass concerning current conditions in an industry (and any firm contemplating a sizeable investment will, of course, commit resources to finding out a great deal about its prospective rate of return) that on current price is likely to be amongst the least valuable.

What is important, at least in the oligopoly context, is what will happen to price after entry occurs, and since price is an exceptionally flexible variable, there need to be no connection between the pre- and post-entry prices



that would be set by the existing firms. These firms need only threaten to lower price after entry, and thus eliminate monopoly profits, if they desire to prevent entry. If their cost competitiveness, and/or financial reserves, and/or control over marketing outlets, dominate those of the potential rival, the threat will be effective; if not, entry will occur, but in neither case is there any reason for the firms not to 'make hay while the sun shines', and charge what the market will bear in the short run.

We may go further, and suggest that, if anything, a price closer to  $\bar{p}$  than might be supported by existing market conditions will encourage the ultimate act of entry -- takeover -- since the firm charging such a price would be valued lower on the stock market than the potential earning power of its assets justifies. Kuehn (1975, p. 15) reports that more than 43 per cent of all UK public companies existing sometime during the period 1957-69 were taken over during that period; thus we can believe that the threat of a takeover bid is significant.

The one empirical study of which I am aware looking directly at the relationship between entry and profits found that

"Large-firm entry is not associated with relatively high profit levels. On the contrary, those industries that experienced the largest number of entries have the lowest profitability, as measured by the price-cost margin," (Zimmerman and Honeycutt, 1977, p. 73).

Scherer, after citing the passage from Bain reproduced above, notes that he finds it 'not entirely convincing, given the role bluff, counterbluff, and irrational response play in deterrence' (1970, p. 229), yet only one of the papers referred to in this section attempts to justify the assumption that current price will be read as a reliable indicator of post-entry price. Pashigian (1968) points out that if the 'monopolist's' threat to lower the post-entry price is to be credible

'he must be prepared to produce the larger output required to meet demand at the limit price with a plant primarily designed for efficient production of the smaller monopoly output ... In contrast, the entrant is able to design a plant for the optimum rate of output, with a cost advantage over the monopolist. 'These are formidable difficulties, which will not often be overcome' (1968, p. 166).

These 'formidable' difficulties in fact quickly dissipate when the alternative of deliberately carrying excess capacity in order to make credible a threat to lower the post-entry price is formally examined. Spence (1974, 1977) has analysed in some detail a model in which excess capacity, not price, is the entry-limiting instrument. The excess capacity strategy has the property, which surely must be considered quite devastating, that it dominates the use of a limit price, as Waterson has observed:

'For fixed costs are no higher than under a static limit pricing policy yet output is at a more profitable level,' (1976, p. 91).

since the limit-pricer must also carry the 'excess' capacity to meet the extra demand that his lower price generates, yet he never reaps the profits of a higher price enjoyed by the industry which deliberately uses excess capacity as a strategy.

Spence's excess capacity hypothesis undoubtedly generates a richer and more realistic model that can be put forward using the assumption that price is the proper entry limiting instrument. However, he may have fallen into the same trap that caught the earlier literature, namely of assuming that things are more difficult than they really are, in order to build an interesting model of the optimal responses to the assumed constraint. I have suggested that there is no plausible reason why current price will be taken as an indicator of future price, but, similarly, should it not also be asked why future output need be proxied by current capacity? Surely existing firms, with their greater experience and contacts in their industry, will be able to add new capacity at least as quickly as any newcomer can manage? If so, then, again, only the threat of post-entry action need be made to forestall the would-be predator on the industry's monopoly profits -- there is no need actually to carry excess capacity, and even Spence's model becomes irrelevant.

In fact, it is indeed true that industry has typically a margin of spare capacity on hand. Calculations of the ratio of actual to 'full capacity' output rates for 14 UK manufacturing industries over the period 1964-73 average to 0.967 (Appendix A). Taking the average of Deaton's (1975) industry price elasticities of about -0.5 as representative, we could conclude that, typically, the existing firms in an industry have the capacity to lower price by around 7 per cent, and still meet the added demand, even without allowing for the additional capacity brought in by a new entrant. Since such a price reduction would mean a substantial reduction in profit margins, probably to below 'competitive' levels (the average margin of price over costs in UK industry over the period 1954-73 was 20 per cent), it seems safe to suppose that sufficient capacity is on hand to give credibility to an excess-capacity strategy.

However, I would suggest that the presence of this margin of unproduced output serves not as a defence to keep at bay potential rivals, since, as noted above, extant firms are likely to be at least as quick off the mark in adding to their capital stock as any outsiders, should the occasion arise, but rather as an offensive weapon, in the battle between existing firms for market shares. In this struggle, which is, I suggested in the previous section, likely to be the only acceptable area in which oligopolistic rivalry can

take an active form, the ability to consolidate any gains made in market penetration will depend on the firm being able to supply promptly the additional demand; therefore any firm with hopes of increasing market share will keep on hand a suitable margin of spare capacity.

Thus, we find that, although the entry-limiting literature has developed independently of, and is, indeed, inconsistent with, the oligopoly pricing models, its relevance is also limited by the ubiquitous 'demand curve' assumption; that is, the postulate that there exists a unique industry price, the setting of which is the prime decision problem as each firm's price determines its market share and so its profits. I have argued that, on the contrary, price-setting is not the major problem -- in the oligopoly situation because it is so clearly not in the firms' interests to compete on price, and in the entry-preventing strategy case because current price, being quite freely variable, need not be used as an instrument to threaten potential rivals -- and that it is selling output that is the problem. Maintaining or improving market share does not, I believe, depend uniquely on price, as the orthodox literature almost invariably assumes. Indeed, given the sort of elasticities that have been estimated, price-cutting would seem generally to be a very unprofitable means towards 'buying' an increase in sales. Rather, firms will devote sizeable, and relatively permanent, flows of

resources in the form of marketing and advertising expenditures, new product development, expansion into wholesaling or retailing, and holding excess capacity, towards increasing their profits at a given price, by increasing sales.

This non-triviality (in contrast to the demand curve assumption that quantity is simply 'set' by the price chosen) of actually clearing the market, means too that 'large scale' entry, even if the elasticities were not such that any sizeable increase in industry capacity would ruin everyone, is not really a sensible concept (except as a takeover bid for an existing firm), as it will just not usually be possible to 'enter' a market suddenly on a large scale -- it takes much time, resources and entrepreneurial flair, to build up a market share.

In summary, I have argued in this section that 'lumpy' or large-scale entry that adds to the capacity of an industry is not likely to be an active constraint on the pricing policies of existing firms. If the potential firms have no cost advantage over the current operators, then the threat of lowering price and adding capacity will be a sufficient deterrent. If the newcomers do possess some cost, or other marketing advantage, then they will enter anyway, and drive out the least efficient of the old capacity, the owners of which then may as well enjoy such short-term profits as they can glean.

A very common method of entering an industry on a large scale is to take over some existing firm's assets; however, none of the papers surveyed in this section have made the effort to analyse the takeover phenomenon.

The literature on smooth, or small-scale entry (that is, entry when there are not significant economies of scale) assumes an elastic supply of entrepreneurs ready to squeeze under an industry's demand curve at the least sign of 'excess' profits being earned, and then grinds out its results by looking at the implications of existing firms being given some breathing space by the presence of frictions, which slow down but do not prevent, the achievement of the eventual zero-profit equilibrium. Again, we find all depending on the definition of an industry demand curve allowing market shares to be freely varied simply by the adjustment of price. In fact, getting under a demand curve is not in general the trivial exercise that is tacitly assumed in the Marshallian/Walrasian tradition of microeconomics and industrial organization, when all that the newcomer need do is set up shop in the 'market place', and leave his offers-to-sell with the auctioneer, who will arrange the actual transaction. In reality, in almost all industries, firms do their own selling, in an environment characterized by a great deal of uncertainty. Then, the advantages held by existing firms, especially large ones, of established (perhaps even vertically integrated)

networks of wholesale and retail outlets, of reputations based on past performance, and of 'goodwill' of existing clients, whose special needs are known and accommodated, are likely to be such that the absence of economies of scale of production is far from a sufficient condition for small firms to be able to enter an industry and compete away its excess profits.



#### I.4 Summary and Qualifications

On surveying industrial organization theory we have found it divided into two camps; models of oligopoly pricing and models of entry-limiting pricing. The two groups of models are both concerned to explain the same performance variable -- the level of prices relative to costs -- but they have developed quite independently, and, indeed, conflict in their initial postulates. The oligopoly models ignore the possibility of new firms being attracted to an industry; the entry-limiting work assumes away the oligopolistic co-ordination problem by dealing only with monopolists or dominant firms as representatives of the existing industry membership.

However, it was argued that this inconsistency, though a quite disturbing immanent feature of the literature, is not of substantive importance, since neither group of models paints a realistic or useful picture of the industrial price determination process.

In both cases, it was found that the models' inadequacies can be traced to shortcomings in the underlying demand-curve postulates.

In the oligopoly models, firms have no market power per se, but as a group in an industry their profits are limited by difficulties of colluding, which are proportional to the number of firms involved in collusive agreements. I suggested

that this confidence in collusive difficulties is misplaced -- even when explicit collusion is not possible (because, for example, it is illegal), effective co-ordination on price is either easy, through such procedures as recognition of a price leader, or unnecessary; in industries in which the firms sell products that are sharply differentiated (for example due to the location of the seller mattering) they will not be effectively in competition with each other over most of their market, and so will not need to co-ordinate their prices.

The implication of this is that it is best to assume that oligopoly prices do not differ from the prices that would be charged by a monopolist. Accepting this brings us to a further difficulty -- the overwhelming tendency for estimated industry price elasticities to be too low to be consistent with the existence of a monopolistic profit-maximizing price. To deal with this anomaly, it was suggested that the typical assumption of a demand curve as a locus of points in the price-quantity plane from which the monopolist is free to choose unilaterally that one that best suits his profit-maximizing ends should be questioned. In fact, it is more plausible to suppose that sellers typically find themselves in a situation in which movements of price and quantity are on the agenda of a bargaining process, either explicitly, when the selling firm is selling to another firm, or implicitly, when firms sell to 'atomistic' consumers, but are constrained by considerations

such as possibility of entry, of fairness, and of public policy action on behalf of the consumers, in their freedom to move along the demand curve.

The literature on entry-limiting pricing is itself divided onto two sub-groups models considering the case of 'smooth' entry, when, in the absence of economies of scale, new firms enter an industry at any rate of operation, and 'lumpy' entry, in which an assumed minimum efficient scale of operations simplifies the entry decision to a discrete in-or-out action.

In the former case, existing firms are assumed to have no genuine market power -- it is just a question of time before any excess of profits above costs are eliminated by the entry of new firms who squeeze inwards the demand curve faced by the existing firms, who are assumed, however, to have some power to influence the frictions that slow the inevitable decay of their profits. The optimal calibration of these frictions is just what is analysed in the models of smooth entry. I do not find persuasive the implication of this work that all monopoly profits are transitory, to be competed away eventually; but, in any case, it is a proposition which may be tested empirically, and this I do in Part III.

The basic problem, I believe, with these models, as well as with the oligopoly literature, is that in neither case is are firms, however large they are relative to the industry, supposed to possess any real market power, in the sense of an actual income-earning property right that goes with being big. Excess profits are either the fragile outcome of collusive agreements with other firms, or a purely transitory benefit, eventually to be inexorably competed away by a flood entry of small firms. It will be the task of the next two Parts to attempt to develop and test a model which builds on a concept of market power as a concrete and durable phenomenon.

When technical conditions in an industry are such that, to be able to produce at a cost that is competitive with existing firms, an entrant must be prepared to set up at or above a certain minimum scale of output, the existing firms can supposedly permanently forestall entry by choosing their price to reflect a judicious balancing of minimum efficient scale and demand elasticity.

Two relatively minor objections to the lumpy entry models are (1) in many industries actual minimum efficient scale and elasticity of demand are such that entry would always flood the market -- the extra capacity would result in the industry's total output being unsaleable except at a loss, and (2) the actual business of setting up on a sizeable

scale is not the trivial matter that is implied by the demand-curve assumption, by which price alone is sufficient to generate any desired level of sales (within the overall constraints of the industry demand curve). In fact, selling output involves many more decisions and resources than just getting the price right.

The difficulties in actually selling output are also important in obviating the self-imposed problem in oligopoly models of colluding on market share which make the incentive to cheat so destructive of price-co-ordinating agreements. However, the major problem with the entry-prevention literature is its assumption that current price is the effective entry-limiting instrument, because potential firms read current pricing policies as reliable indicators of post-entry profitability. As price is one of the most flexible of variables, this does not seem a reasonable assumption. Why should existing firms not charge whatever price they can get in the short run, and just threaten to lower it to the no-excess profits level should any new firm dare enter? All that is needed to make credible this threat is sufficient excess capacity to meet the added demand should they lower prices; this, however, is no added burden, since any industry keeping current prices down to forestall entry would have to carry the same capacity in any case.

This line of argument can be carried further, to criticize even the assumption of a need to carry excess capacity to furnish an anti-entry weapon -- so long as existing firms can add to capacity as fast as new firms can invest in the same sort of plant, the threat alone to do so should suffice.

Finally, I should note several qualifications to the style and generality of the argument that has been developed in this critique:

- (1) Parts of the analysis have been conducted in terms of polar cases -- 'substitutable' versus 'differentiated' products, 'smooth' and 'lumpy' entry. This has allowed us to highlight the qualitative differences between these extremes of structure and conduct; in reality, of course, many, perhaps most situations cannot be clearly placed at one or other pole, so that actual behaviour will often be a mixture of the extreme behaviour patterns discussed above.
- (2) There is an important class of situations in which firms selling similar products do not find co-ordinating price easy. This is when sales orders are won through sealed-bid tenders, and explicit price collusion is

illegal. Then, the 'one-off' nature of the buyer's requirements, especially when transactions are of intermediate and capital goods, may make it difficult for price-leadership or other non-collusive price-co-ordinating devices, to develop, and the threat of criminal prosecution may make explicit conniving unattractive. As a response to this, a sort of market-sharing may develop; not necessarily in the sense of splitting up a given quantity of sales, but rather by assigning areas of the market (often through quite conventional devices such as patents and licensing) to individual firms, within which they are more or less free to set their own price. Scherer (1970, Chapter 6) discusses these matters, with some examples from US industrial experience.

- (3) I have found the roots of most of the problems of industrial organization theory to lie in the fundamental demand curve postulates on which all the models are built. I intend no implication that these criticisms of simple price-quantity locuses carry over to the field of microeconomics from which the curve

was borrowed. In microeconomics -- the study of the economic behaviour of individual agents -- the simple demand curve concept has proven to be of great power, allowing the technical development of the marginal analysis and its elucidation of the most fundamental economic-efficiency concept of opportunity costs. It is only when the analysis is extended to interdependent agents that the fluidity of the assumption of price-elastic demand becomes the embarrassment that leads to the strawmen of over-difficult collusion and over-easy entry that have so misled the industrial theorists and the empiricists and policy-makers who have followed them.

- (4) Throughout the chapter I have been rather loose about the performance variable that is to be explained, referring sometimes to 'profitability', and at others to the 'margin of price over cost', without defining these variables and evaluating their appropriateness. In fact, although the matter has not often been discussed explicitly, there is not a consensus on just what is determined by industry structure, and the matter is important; however I will postpone my



discussion of it until Chapter II.5, when it will be raised in the context of my own efforts to justify a new formulation of the dependent variable in the structure-performance relation.

## II Attempting a New Synthesis

### 1. Introduction

If the previous chapter's criticisms of the received industrial organization theories be accepted, then little remains of them of use to the substantive part of this thesis. In this chapter, I attempt to put together a theory of industrial pricing behaviour that is both usefully realistic and internally consistent. I call this an attempt at a 'synthesis', because most of the elements of the new theory have respectable antecedents in the economics literature. They have not, however, to my knowledge been assembled and tested as the market structure-performance model; to do so is the job of this and the following chapter.

The main problem set us by the arguments of Part I is to come up with a concept of market power as a convincingly durable profit-making force, in contrast to the fragility of oligopolistic collusion, and the temporary, perhaps even negative, benefits of entry-delaying pricing. Two other important features of the model should be in ability to deal at once with actual and 'potential' competition, in contrast to the schism that exists between orthodox oligopoly and entry-threat modelling, and consistency with the observed price-inelasticity that seems to characterize most industrial markets.

The theory is developed as follows. In Chapter II.2, a concept of market power as a property right possessed in varying degree by all firms above a certain size is defined and supported. In Chapter II.3 it is proposed that the mechanism whereby market power is mediated into pricing performance is better viewed as a process of explicit and implicit bargaining, rather than as the more usual notion of seller price-making in markets. Chapter II.4 summarizes the preceding arguments, and Chapter II.5 examines the implications of this theory for the proper specification of the profitability variable to be explained by the structure-performance hypothesis.

## II.2 The Property Right of Market Power

In this Chapter I propose and try to justify an acceptable definition of market power, as follows:

"The market power of one *party* vis-à-vis another is the costs it can impose on the other *party* by not trading with it."

This definition is quite general -- it allows differences in a firm's market power between different trading partners, and it allows buyers and sellers to have power.

Under the typical oligopoly model assumptions, such market power is non-existent -- the firms in an industry sell a homogeneous product so that a buyer is indifferent between (suffers no costs in) trading with one firm rather than another.

In fact, though, the list of product characteristics that matter to traders is in general quite lengthy, including such things as reliability, availability of servicing, delivery dates, credit terms, 'image', and many other factors as well as the more obvious physical dimensions of products, and I believe that, given that each firm is itself a unique collection of individuals, physical plant, operating procedures and traditions, we should best assume that complete concordance on all the relevant product characteristics is a (most unusual) special case, rather than the generality.

This heterogeneity means that the typical firm is not competing on equal terms with all other firms producing the 'same' product, but rather offers, in addition to the characteristics in common (i.e., that it is selling or buying 'cars' or 'typewriters' or 'machine tools' or 'haircuts') its own unique bundle of attributes that are of value, in varying degree, to its trading partners. The consequences of this were recognized at least as long ago as Marshall, who wrote:

"When we are considering an individual producer, we must couple his supply curve -- not with the general demand curve for his commodity in a wide market, but -- with the particular demand curve of his own special market ..." (Principles, p. 458n, quoted in Richardson, 1960, p. 63).

Much later, Kaldor (1935) suggested the concept of 'scale' of products, with closeness on the scale a function of the cross-elasticity of demand between two products. In the same decade, Joan Robinson (1933) and Edward Chamberlin (1933) came out with their famous books on 'imperfect' or 'monopolistic' competition, which developed in some detail the implications of the assumption of generally heterogeneous, or differentiated products.

Recognition of heterogeneity does not, however, imply at once the market power concept -- in the Robinson-Chamberlin world, free entry leads to the competing away of all profits -- their main result was that the resulting equilibrium was one of general excess capacity, so that the unregulated market economy, though inefficient, was not necessarily unfair. This conclusion follows from considering only equally small firms; Robinson and Chamberlin did not consider, as we shall do here, the implications of heterogeneity for the profitability of size of firm, and thus for the incentives for firms to attempt to grow relative to their industry's market.

A particular dimension of product heterogeneity, which, once observed as a commonplace factor in the real world can hardly further be ignored, is of the physical location of firms in the economy. Interesting models

exploring some implications of locational heterogeneity of firms selling otherwise identical products have been analysed, first by Hotelling (1929) and Smithies (1941), and recently for example, by Greenhut and Greenhut (1975) and Eaton and Lipsey (1975). These models provide an analytical tradition within which some of my own simple mathematical modelling in Section II.3 proceeds. Lancaster (1966) developed the natural generalization of the notion of geographical differences into n-dimensional differences between firms in 'product space', distinguishing each seller according to the n-dimensional bundle characteristics of its products.

In what follows, I shall adopt the product-space analogy, and examine its implications for the concept of market power defined at the head of the section.

To fix ideas, suppose a market for a product in which the customers are distributed along a line, as in the classic location models, and consider a particular firm owning all the sales outlets in a certain segment of the line. This segment we call the firms' market 'territory'. At the edges or borders of the territory the product can be purchased from other 'fringe' sellers at price  $\bar{p}$ . Customers within the territory incur transport costs, a function of their distance from the nearest border. The firm's market power over customer  $i$  is then  $\bar{p} + d_i$ , where  $d_i$  is  $i$ 's distance from the border, and we assume, for simplicity, that

transport costs are proportional to distance, and units are chosen so that there is a cost of 1 of travelling 1 unit of distance.

The concepts of territory and distance generalize quite readily to other differentiated dimensions of products. Firms, being heterogeneous, produce products which differ from those of other firms: these differences will suit the special needs of a subset of buyers (buyers being also, of course, heterogeneous), who would therefore suffer a loss of utility if they were forced to purchase the product from another firm; this loss is a function of the 'distance' in product-characteristic space of the nearest substitute from the preferred firm's product.

A most important, though neglected, source of differences between firms does not spring from differences in the objective qualities of products, however subtly measured, but rather from the nature of the exchange process itself. The general infeasibility of the purely anonymous exchange regimens assumed in the oligopoly literature means that networks of buyers and sellers grow up. To break these networks, which are discussed more fully in the next section, where their implications for price-setting are examined, imposes costs on both buyers and sellers -- the costs of searching out in an uncertain world reliable new trading partners.

Thus, the possession of some territory or market power may be seen as commonplace, for all but those firms selling or buying unusually homogeneous products in auction markets, which, in the nature of things, are not often observed past the primary processing stage of industrial production.

This notion of territory is not, I hope, particularly surprising or controversial. It does not in itself apparently contain any 'power-leading-to-profits' connotations. But these, indeed, are immediately implied by the concept. To show this, consider again the simple product line analogy. If a firm owning as its selling territory a segment of a given length takes over an adjacent segment of the same length, it is true that the average distance from the border of customers within the territory increases. This result can easily be shown mathematically, and we can appreciate it intuitively by observing that customers nearer the borders of the old territories that have disappeared as the result of the merger must now travel further, to one of the two remaining borders, to purchase from the fringe.

That is, market power, as defined here, increases as the size of the firm increases; this is the basis of the link between structure and profits that is worked through in the next chapter.



The reader familiar with the spatial competition literature, to which some references were made above, may wonder why some such concept of market power does not come out of these models, given their similarity in the use of the concept of location and distance. The reason is the spatial competition approach has remained fundamentally faithful to the assumptions of oligopoly theory; it considers firms' (most often, duopolists, because of the analytical problems involved in further generalization), reactions to their supposed interdependency. It just adds choice of location to choice of price as a competing instrument, which leads, typically, to results such as Hotelling's famous 'minimum differentiation' principle (cf. Eaton and Lipsey, 1975), predicting that the two duopolists will crowd as close together as possible on the market line.

In my view, the assumptions of these models must face the same sort of criticism that was brought, in the previous chapter, to bear on the traditional oligopoly theories; namely, that they suppose too much smoothness in the wrong places. In the oligopoly work, market shares were assumed, wrongly, I argued, to be elastically responsive to price, with no recognition of the problems of 'marketing' that seem, in fact, to preoccupy the lives of the oligopolists (businessmen) themselves. The spatial competition modellers have added to market-share price responsiveness the assumption that firms can freely locate anyway they choose in product space, a postulate that seems to me at least equally unrealistic, for the following reasons:

First, much product space territory is owned legally, through such devices as licensing, patents, and government regulation at all levels.

Second, even without legal restrictions, the actual heterogeneity of firms, based on the quality of their share of the economy's ultimately rather fixed managerial and entrepreneurial skills, and in the characteristics developed in their own right, by organizations implies that rivals are often simply not capable of encroaching on each other's territory. To exemplify this by the familiar automobile industry analogy; there is nothing in law preventing Ford and Chrysler from producing a car as popular as General Motor's Chevrolet, but they are just not good enough to do it.

Third, the 'networks' built up by a continuity of trading experience between firms, cannot, by their very nature, be appropriated by a newcomer. They disappear as the trading linkage is broken.

Thus, legal and customary limitations, and the very nature of business enterprise, all go to make the possession of market power, as defined here, a quite secure property right. The jittery location games played by rival firms in the spatial competition literature are not typical of the real world.

What of the collusive price-setting problem?

Scherer writes;

"When products are heterogeneously differentiated, the terms of rivalry become multidimensional, and the co-ordination problem grows in complexity by leaps and bounds" (1970, p. 187).

Scherer apparently has in mind a collection of rather different products being sold together in the same market, as in the non-differentiated oligopoly situation. But what heterogeneity means, in effect, is that firms are not competing in the same 'market'. Customers who find most attractive a particular bundle of characteristics offered by one firm are 'distanced' from other sellers, so that the latter could alter their prices, within limits, without inducing most of the first firm's clientele to move -- the distance between characteristics, reinforced by the inertia of existing trading networks means that feasible (non-loss making) price cuts may just not be attractive enough to tempt most customers to change.

Only customers at the border between two price-setting firms will be indifferent enough to be fickle, and it is where firms meet that we do find price wars and other forms of rivalry. But we should note (a) that the bigger firms become, the smaller will be the border territory relative to the indisputed interior (in a circle, for example, area goes up as the square of circumference), and (b) it is precisely

at the border that products are not differentiated, so co-ordination on price, where it is needed, may not be too difficult to achieve, given our arguments in Chapter I.2.

This, I hope, deals with the oligopoly problem. Firms controlling substantial slices of market territory will not be particularly concerned with, and certainly not obsessed by, as in the oligopolistic co-ordination literature, the prices charged by other sizeable firms. Those prices may set constraints on a firm's actions, but there is no reason not to believe that, within limits, the market power of a firm, as defined here, can affect price.

What of the threat of new firms entering? The arguments of Chapter I.3 against large scale entry, as in the Bain-Sylos-Modigliani models, being a plausible danger still apply. Given the difficulties, in the real world, of actually achieving a significant market share, the process itself of entering on a large scale of sales will generally be a non-trivial problem, and, in any case, given the apparently low industry demand elasticities usually observed, such entry, once achieved, might just result in swamping the market-wiping away everyone's (including the new entrant's) profits.

In addition, to the extent that the market power of existing firms is 'owned' by them through law, contracts, and custom, as suggested in this chapter, then there may simply be

just not enough available territory for a new firm to set up in, except by taking over the market power of an existing firm or firms.

The act of takeover can be consummated either by the quite common method of actually purchasing the assets of an existing firm (including a sum for the firm's market power, conventionally termed a 'goodwill' payment), or by entering the market with a superior bundle of product characteristics and/or the capability of producing a non-dominating bundle at a lower cost than existing firms can manage. In either case, there is no reason for existing firms not to charge the price that their market power can support, so long as they are in operation. Indeed, I have suggested that charging less than this price could encourage financial takeover bids, by undervaluing the firm's earning power on the stock market. And since a superior firm will enter anyway, the doomed operators might as well make what profits they can, in the short run.

A rider to this is that there may often be an additional long-term return to adding to one's market power -- for example, through adding to holdings of patents and licensing agreements, and by becoming big enough to exploit economies of scale in lobbying with government for protective legislation and regulation -- if this gives existing firms a weapon to block or make more difficult the innovation by other

firms of superior products or lower cost technologies at a rate that would reduce the value of the industry's current stock of capital and property rights.

A situation in which we may expect to observe new but not necessarily superior firms entering occurs when the total market is growing, due, say, to changes in tastes or increases in per capita incomes. The growth of a market generates new property rights (such as newly profitable locations) which can be appropriated by whoever is quickest to spot them. Even in this situation, however, existing firms may have an advantage, based, for example, on better market information, or claims of precedence in the allocation of new licenses, or the exploitation of economies of scale (a case analysed theoretically by Eaton and Lipsey, 1976), over newcomers. If so, we would expect to see a link between growth in aggregate demand and increases in market concentration; something that is certainly consistent with the evidence for the UK (see, for example, Hannah, 1976) that this century's overall upward drift in industrial concentration was halted or even reversed in the generally depressed period of the 1920s and 1930s.

What of small-scale or 'smooth' entry, and, in general, the relationship between large and small firms selling products with some substitutability? The reader may

have been worried by the lack of justification for the, implicitly exogenous, 'fringe' price,  $\bar{p}$ , used in the definition of market power. In the next few paragraphs I try to make good this neglect in answering these questions.

It is proposed that in most if not all markets there exists a number of small firms whose margin of price over costs is set not by their market power but by conditions of demand and supply of entrepreneurs or small businessmen. These are operators whose territory is so small that a price determined by market power alone would not sufficiently compensate them for the risks and alternative wage or salary income foregone to keep them in business. In these small firms price will be set to give the owners an income equal to what they would earn as someone else's employees, adjusted for risk and for the psychic rewards of doing business on one's own account. Equilibrium in the small firm sector is maintained by small variations around this price, reflecting changes in product market conditions, inducing the required entry and exit of operators.

Thus we have a sector of the economy selling goods at prices that are not determined by market power considerations, and so can be taken as exogenous to our analysis of price-setting where market power is significant. These are the 'fringe' firms selling at the exogenous price denoted as  $\bar{p}$  above.

Of course, the performance in aggregate of large firms affects, through the level of the real wage in the economy, and also, perhaps, its macroeconomic stability, conditions of supply and demand for small businessmen. But, with possible exceptions such as mining and car making, individual industries do not contribute enough to the aggregate to make the assumption of an exogenous  $\bar{p}$  faced in each market an unreasonable simplification.

In many industries, the fringe firms are engaged in producing products quite similar to the output of larger firms. However, even in industries where the exploitation of sizeable economies of scale has made small scale production uneconomic, we may still find small firms doing business as importers from foreign suppliers of the product. When this does not occur (because, for example, domestic large producers have gained control over imports) we have to go to the industry making the nearest substitute product to find our fringe. Such a move implies, naturally, that the 'distance' component of big firms' market power will be larger than when they face a fringe on their own doorsteps.

We can note that the relationship between firms with market power and small fringe firms is likely to be unbalanced. Firstly, small firms will usually find it difficult to profitably enter the territory of market-power firms, even when



the property rights of the latter are not firmly enough held to rule out such an advance. This is because an invader will find itself surrounded by the product characteristics of the larger firm, which need only reduce price sufficiently in these neighbouring activities (supporting this with its profits on the market power remaining in its other territories) to prevent the interloper from being able to sell at a profit.

On the other side, fringe firms are never secure from depredations, as it may often pay the larger firm to lose money on price-cutting at its border, in order to force the adjacent fringe out of business and then add its territory to the large firm's market power property. Even without engaging in price wars, the fact that the market power is worth something to the larger firm and nothing to the fringe operator provides a motive for agreement on a takeover that benefits both, at the expense of customers.

Thus, it is easy to understand the relentless tendency to increasing market concentration that has indeed been observed in the oldest of industrial economies -- Great Britain. The process may have limits, however -- to the extent that owner-controlled small firms have a comparative advantage in small scale, specialized production, and are, perhaps, readier to undertake risky projects such as new product and process innovation, large firms will often find

it preferable to let the fringe carry on.

The implication of this analysis that there is a force, stemming from different values being placed by large and small firms on the latter's market power, which pushes up concentration in the economy, is not present, oddly enough, in the orthodox oligopoly models. In these theories, market price is determined by market structure (measured, for example, by the Herfindahl index) to which most individual firms contribute rather little. That is, there is a free-rider problem -- if there are any costs to expansion, such as needing to engage in price wars to force rival firms to sell out, each firm has an incentive to let the other firms go ahead and build up the industry's concentration, and just sit back and enjoy the resulting rise in everyone's price. There is no strong market power reason for a firm to wish to be bigger.

In this chapter I have tried to deal with two of the three major problems, noted in the introduction to the chapter, that in the previous chapter were found to weaken received industrial organization theorizing.

First, and most important, a definition of market power has been proposed, and justified in terms of firms' 'territory' in product space, which provides the basis for predicting a durable linkage between market structure and profitability.

Second, the uncomfortable dichotomy between oligopoly and entry-threat models has disappeared. Indeed, we can now see that the basic idea of the entry problem is mis-stated. To a given firm all other firms are only potential entrants, in the obvious sense that none of them are at present making the trades that the given firm is undertaking. And looked at in this way, the distinction between rivalry removed in time (entry-threat) and in space (oligopoly) disappears. 'Distance', in our product space can be dated, with no increase in analytical complication. Thus, for example, the fringe operators in a market need not actually exist; it is sufficient that they would be prepared to set up shop, should a demand for their services arise, for the fringe price to constrain market-power firms' price-setting. (Of course,  $\bar{p}$  would be higher, the longer it takes new fringe firms to set up shop -- future prices will be discounted at some interest rate).

The third problem, of inconsistency of the fundamental market assumptions, given the unpersuasiveness of orthodox oligopolistic co-ordination and entry threat as constraints on price, with typically observed market demand elasticities, is dealt with in the next chapter.

Finally, it should be noted that, although our discussion of market power has so far concentrated on the market power of sellers, the concept applies equally to buyers,

who can also impose costs on their trading partners by forcing them to look elsewhere for custom. This is surely a natural extension of the concept -- if both parties gain from trade, then both parties lose by not trading -- and will also be important in our analysis of the price-setting process in Chapter II.3.

### II.3 The Price-Setting Process

Most economic analysis, of both capitalist and socialist economies, has sought to explain the allocation of resources by means of one of the two mechanisms of centralized, fiat, planning, or decentralized impersonal markets. It may be, however, that both these systems of analysis are too extreme to provide a realistic basis for a description of those economies, both 'command' and 'market', in which the division of labour has proceeded far enough for the co-ordination of productive activity to be a non-trivial matter. There is a broad middle ground of what Richardson (1971, 1972) calls 'piecemeal planning'.

Richardson proposes that, although there are some goods which are sufficiently standardised so that producers can rely on the law of large numbers to smooth out demand fluctuations and can produce to stock (that is, produce for the market) without much contact with their customers, most demands for goods are sufficiently varied and varying that purely speculative production would be prohibitively wasteful and inefficient. For the same reason, pure centralized administrative fiat would also fail -- the planning authority would be quite unable to collect and process the enormous quantity of specialized information needed to co-ordinate all production activities.

In fact, industrial societies develop myriad networks of subsidiaries, trading agreements, sub-contracting, marketing associations, licensing, and so on, which lower the risks involved in undertaking heterogeneous transactions, and which follow patterns of custom and goodwill that are not greatly influenced by market forces, or by the edicts of planners. This seems to be so not just with respect to sales of intermediate goods between manufacturers, but also with relations between manufacturers and their wholesalers and retailers.

The network system is a natural consequence of the product differentiation that I argued in Chapter I.2 to be the norm in industrial economies. Richardson's concept has important implications for the way we should look at the setting of prices in an economy such as the UK. If they are predominantly neither the result of obedience to governmental directives, nor of submission to the impersonal authority of the market, then price-quantity contracts must be settled by direct agreement between the concerned agents themselves. That is, most prices (including, of course, many wage rates) are settled by bargaining between the interested parties.

Next, in this chapter, we will consider the situation that is conventionally brought to mind by the notion of bargaining, namely; a one-to-one confrontation between two agents; typically, two firms. This I call explicit bargaining.

Then the analysis will be extended to cover the case of a firm trading with a large number of customers, resulting in some standardization of product and price, so that the firm does not engage in direct negotiation with any of its trading partners. This is the situation covered in the orthodox firm-selling-in-a-market analysis, but I will suggest that even here we find important, if usually unstated and subtle, elements of the bargaining process, which will be called implicit bargaining.

There have been many bargaining models built, but the differences between them tend to be of rather second-order magnitude of importance, Laidler and Parkin (1975) note the conclusion of survey by De Menil of a number of bargaining models, that they share in common

"Prediction of maximization of the gains from trade and a sharing of those gains in proportion to the relative marginal disutility that each party could and would, but never does, inflict upon the other" (p. 757).

The 'marginal disutility that each party could ... inflict upon the other' is, of course, effectively the same as the concept of market power defined in the previous chapter, which thus fits neatly into a bargaining analysis framework.

We shall consider only successful and efficient trades; that is, it will be assumed that each trader finds the party offering the bundle of product characteristics

most suited to his needs, and successfully closes a deal with that party. This seems reasonable enough, at least as an equilibrium situation. In fact, deals are constantly falling through in the real world, but we can look upon such failures as information-generating search processes, which do not preclude from being efficient the trades that eventually are consummated ('maximizing the gains from trade'). In any case, the census data on which the model will, in the next chapter be tested, measure only the results of successful trading -- deals that fall through do not result in the generation of industrial output.

Assume, first, two firms exchanging a given quantity of a good. The assumption of fixed quantity is often defensible even in a market framework as a reasonable simplification when the good is an intermediate input, amongst many others, making a small contribution to a process of further adding-value, so that its price could have only a small effect on the total costs of the final product, and so on the demand for the latter.

However, more fundamentally, the assumption will be seen to be consistent with one of the major thrusts of this thesis, namely that price and quantity setting are most realistically and usefully seen to be separate processes -- price being arrived at through the endogenous bargaining procedures to be analysed below, and quantity traded being rather insensitive to generally observed ranges of price



fluctuations, but determined overwhelmingly by factors such as aggregate demand, tastes, and technology, which are exogenous, or nearly so, to individual firms.

Suppose that the good has  $k$  dimensions of interest to buyers, but that  $k-1$  of these are fixed (e.g., by government regulations) and only the  $k$ th dimension differs between sellers. The restriction to one dimensional variation will be maintained in the present analysis, which is simple and intuitively accessible in consequence. The analysis could be generalized to  $k$ -dimensional variation, but, I conjecture, this would not alter the substance of the results. Therefore, I prefer the simplest option in the trade-off between generality and complexity.

The particular selling firm being considered owns a segment of length  $2M$  (that is, has sole selling rights over) on the line on which is measured the  $k$ th dimension. For simplicity, and without loss of generality, we will consider just the right-hand half of the segment, and will set the bisection point to be the origin.

Suppose that the market is initially in equilibrium, with all buyers purchasing the good with the most preferred  $k$ -dimension value, given the other  $k-1$ , and that contract re-negotiation time has come up for buyer  $i$ , who has been purchasing the good with  $k$ -characteristic value  $m_i$ , from our

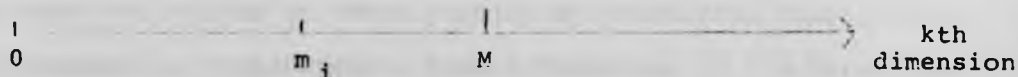
selling firm,  $m_i$  is thus within the seller's segment, a distance  $M - m_i$  from the border, as shown on Figure 1.

At the border, fringe sellers, as defined and discussed above, offer the good with  $k$ -characteristic  $M$  at a price  $\bar{p}$ .

A firm's market power was defined in the previous section as the costs it could impose on its partner by not trading with it. In this model, these costs are, for both buyer and seller, the cost of going to the boundary,  $M$  and of trading there at  $\bar{p}$ . The buyer has to go to the boundary and purchase from the fringe since all points between  $m_i$  and  $M$  are owned by the selling firm; the seller must travel to the boundary, and join the fringe in selling at  $\bar{p}$ , given a) our assumption of initial equilibrium, so that all customers who wish to purchase between  $m_i$  and  $M$  are already doing so, and b) the assumption of inelastic demand.

However, although the distance to be travelled is the same for buyer and seller, the costs of the trip to the boundary will not in general be so. For the buyer, there are the transaction costs of setting up a new trading arrangement -- of re-establishing a network -- plus the loss of

Figure 1: Line of Market Territory



utility in not getting the most preferred k-characteristics, which will depend on the particular consumption technology of the buyer; that is, the cost of substituting values other than  $m_i$  in the uses to which buyer i puts the good.

The seller will also incur some transaction costs by not trading with i, and may, as well, suffer by having to sell a k-characteristic M that is more costly to produce than was  $m_i$ .

We should enumerate some of these costs more concretely. Most obviously, location in geographical space can generate market power -- to travel to the border to buy or sell involves costs of transporting the good to where it is needed; from the production point to the border for the seller, and from the border back to the original trading location for the buyer.

Then there are the basic differences that distinguish all firms and consumers to some degree -- the differences, noted above, in entrepreneurial skill, in organizational effectiveness, in the past development of firms, that generate comparative advantages in carrying out some activities rather than others; and the differences in tastes and needs of consumers. Given that the result of successful trading (which we have assumed initially) is that traders are linked to partners most suited to their particular production capabilities and consumption requirements, then a breakdown in the bargaining process will impose costs on both parties.

A third source of market power is the ownership of patents and licensing agreements. By their nature, these seem to be sellers' rights rather than buyers', and so their presence may result in asymmetry between the magnitude of seller and buyer market power.

It will be useful to distinguish between potential and realized market power. Potential market power is the power that each trader brings with him to the bargaining situation; for the seller it is the cost, or loss in utility, incurred by the buyer in purchasing a less desirable bundle of characteristics from the fringe, and the price,  $\bar{p}$ , of buying from the fringe; for the buyer, it is the costs, if any, born by the seller if he is forced to trade at the fringe, less the price received by so doing.

Realized market power is the after-trade market power -- the costs of going to the fringe, given the price,  $p_i$ , at which trade between seller and buyer  $i$  has been taking place. Let us call the realized market power of the seller  $F_s$ , and of the buyer  $F_b$ . Then the realized market powers of each party -- the cost each can impose on the other by not trading at a price  $p_i$  -- are

$$F_s = \bar{p} - p_i + f_b(M - m_i) \quad (\text{II.1})$$

$$F_b = p_i - \bar{p} + f_s(M - m_i) \quad (\text{II.2})$$

where  $f_b$  and  $f_s$  give the costs incurred by buyer and seller respectively in travelling to the border as functions of the distance from the border. For example, the seller can impose the costs of buying at  $\bar{p}$ , plus foregone utility  $f_b$ , minus  $p_i$  saved.

The bargaining process sets price so that a certain distribution of realized market power, or gains from trading at a certain price, is achieved. Just to see what happens, let us suppose that realized market powers are equated. That is

$$F_s = F_b, \text{ or}$$
$$\bar{p} - p_i + f_b(M-m_i) = p_i - \bar{p} + f_s(M-m_i) \quad (\text{II.3})$$

which reduces to

$$p_i = \bar{p} + \frac{f_b(M-m_i) - f_s(M-m_i)}{2} \quad (\text{II.4})$$

That is, if gains from trade are shared equally, price is set by adding a margin to the fringe price equal to half the difference between the travelling cost part of potential seller and buyer market power, or at half the difference between seller and buyer potential power.

Generally, if realized gains are distributed according to  $F_s = \theta F_b$ , (II.5),

where  $\theta$  is any positive number, price is given by

$$p_i = \bar{p} + \frac{f_b - \theta f_s}{\theta + 1} \quad (\text{II.6})$$

This reduces to (II.4) when  $\theta = 1$ , and is greater or less than (II.4) as  $\theta$  is smaller or larger than one.

Equation (II.6) can be generalized easily to the case when sellers and buyers face different fringe prices  $\bar{p}_s$  and  $\bar{p}_b$ . Then the potential market powers are  $\bar{p}_s + f_b$  and  $\bar{p}_b + f_s$  and realized price satisfies

$$P_i = \frac{\bar{p}_s + \theta \bar{p}_b}{\theta + 1} + \frac{f_b - \theta f_s}{\theta + 1} \quad (\text{II.7})$$

Note that there is no reason why  $p_i$  in these formulae should exceed  $\bar{p}$ . We should, however, impose a lower bound constraint on  $\bar{p}$ , namely

$$p_i > c_i + (\bar{p} - \bar{c}) \quad (\text{II.8})$$

where  $\bar{c}$  and  $c_i$  are the unit production costs of fringe firms and seller  $i$ . If potential market powers are such that bargaining would not yield a price that satisfied (II.8), the average entrepreneur would choose to go out of business rather than trade, since  $\bar{p} - \bar{c}$  is, by assumption, that return which just balances the risks and rewards of entrepreneurship against the opportunity cost of being self-employed.

If  $f_b$  is less than  $\theta f_s$ ,  $p_i$  will be less than  $\bar{p}$ , so long as  $i$ 's costs of production are less than those of the fringe.  $f_b$  could even be negative -- seller  $i$ 's product inherently less desirable than the product available from the

fringe firms. This may often happen when large firms introduce large scale mass assembly or continuous process techniques, at a cost of a loss in quality or variety, but with substantial savings in direct production costs over the small scale methods available to the fringe firms.

We should have some discussion of the distribution parameter  $\theta$ , since it is the formal analysis of the exact division of the gains from trade that preoccupies the bargaining theory literature. However, there are some signs of ennui developing in the profession with respect to further analysis of this 'problem'. We have already noted Laidler and Parkin's report (1975) of De Menil's findings of basic similarity in results of a number of bargaining models, and Vanderkamp writes of 'this addition to the long list of well-meaning bargaining theories', in a review (1975, p.1348) of a paper by J. Johnston.

I would agree that, judging by the lack of variety of useful and distinctive results generated, the bargaining problem has been over-analysed theoretically. In deals between firms, much or most of the variation in  $\theta$  is probably accountable for by differences in the cunning and persuasiveness of the businessmen concerned, and these differences, I would propose, fall into the category of those basic resources of entrepreneurial skills which are not in elastic supply, and so, as argued in Chapter I.2, do not need to be explained in economists' price-setting models set up to explain differences in average behaviour.<sup>8</sup>

Nevertheless, we can make some testable conjectures about factors determining  $\theta$  that are of relevance to our analysis. First, it may be true that the ability of a small firm to realize in full its market power when it is trading with a much larger partner is limited by the plausible threat that the larger firm may be able to make, given its larger financial resources, to set up its own operation to produce the small firm's product, or to trade with the fringe, at a loss if need be, until the small firm is forced out of business.

Secondly,  $\theta$  may be a function of the difference between  $f_b$  and  $f_s$ , such that the effect on price of substantial differences is reduced -- the exercise of market power is restrained. This is because businessmen are not, in general, indifferent to each other's well-being. There may be instances when rival firms selling the same good take pleasure in each other's misfortunes, but I expect that the typical relationship between trading partners being discussed here, involving as it does networks of co-operation and co-ordination between the parties, generates significant empathy between those concerned, so that, after a point, at least, the pleasure received from a dollar gained is diluted if that dollar comes from the profits of the trading partner.



This hypothesis would seem almost trivially obvious to someone accustomed to analysing economic behaviour from a Marxist point of view, to which the cohesiveness, and consequent tendency to empathetic behaviour, of the capitalist class is almost a datum.

A factor reinforcing empathy as a constraint on the exercise of market power (though not one that a Marxist would wish to call on) is the concept of fairness. Even if businessmen are less honourable than their critics, it is unreasonable to expect them to be totally unmoved by the ideas of morality and natural justice that play some role in the lives of other citizens -- 'right' as well as 'might' will matter in the bargaining process, and may include a notion of a 'fair' rate of profit, to earn much more than which is felt to be unseemly.

To the extent that ideas of fairness are important, the distribution parameter  $\theta$  will also depend on the seller's margin of price over costs. Thus, we may observe exogenous increases in costs being passed on in higher prices with no loss in sales. This does not imply, as would the orthodox monopoly analysis, that the seller was being irrational in not putting up price before the cost increase, to take advantage of the demand inelasticity, since, in the bargaining situation, neither party is free to unilaterally set price;

they must agree jointly on the proper price, according to their respective potential market power, and to their ideas of fairness and the goodwill they feel for each other.

I have tried generalizing the analysis of explicit price bargaining to permit some elasticity of demand. Unfortunately, even with the simplest assumption about the demand curve (that it is linear), it does not seem possible to arrive at a concise, readily interpretable expression for price, which turns out to be one of the roots of a quadratic equation. However, I do not consider the ability to be neatly modelled in mathematics a sine qua non of a respectable analytical problem. Any mathematical model too simple to exactly depict reality, and the proper question is just how far it is useful to go with them. When it leads to surprising and significant predictions not available from verbal analysis mathematicizing is justified, and before devoting one's own resources to building mathematical models, the researcher should solve first, informally, a decision model trading off the time and effort involved with the prior probability assigned to the importance of the results.

Just such a preliminary decision model seems to have been lacking from most of the theoretical work in Industrial Organization, otherwise we would not have such a large corpus of oligopoly, entry-limiting pricing, and bargaining models,

each adding so little to what was obvious, or known already, and often built on assumptions chosen for analytical tractability rather than realism, to the consequent detriment of the degree of usefulness of the results.

In the present case of demand elasticity, I conjecture that a fully worked through algebraic analysis (or a comprehensive arithmetic simulation exercise) would just confirm my intuitive prediction that the presence of some elasticity will lower the price relative to the inelastic-demand price, since the seller will find it profitable to concede some quantity discounts in order to increase total revenues, at least so long as there is sufficient supply elasticity to make the increased sales worthwhile. Some results are given in Appendix B.

So far in the chapter, we have built up a model of price-setting as the result of explicit bargaining between firms. The distributive role of prices has been concentrated on -- indeed, in the formal analysis, there was assumed to be no allocative problem at all (demand assumed inelastic) -- through the vertical linkage between buyer and seller. In so focusing the analysis, I have drawn on the arguments of the previous chapter that the allocative bias, taken over from microeconomics, or of previous industrial economics theorizing is misleading, and that the traditional concern with problems

of oligopolistic co-ordination, or horizontal linkages between firms is implausible as a major constraint upon profitability. This model will, I hope, be useful in interpreting the evidence on structure and performance in intermediate goods market presented in the next Part.

However, our analysis of industrial price-setting is certainly still incomplete. Explicit, one-to-one, bargaining is a process to which considerable managerial resources must be devoted by both parties. Yet many industrial firms sell not in large, lumpy, orders to other manufacturers, but directly, or through distributors and retailers, to thousands or millions of final consumers. In such markets, matching products to individual requirements, as well as haggling over the terms of each transaction, is uneconomically expensive, relative to the average value of sales per customer. Sellers will offer a standardized product, at a take-it-or-leave-it price. Indeed, a movement towards mass production, and mass consumption has often been seen as the key feature in the transition from a 'traditional' to a 'modern' industrial economy, as Scitovsky, for example, notes (1951).

In analysing price-setting in consumer goods markets (or markets in which customers are sufficiently numerous firms buying small quantities), we will again use our concept of market power as the basic building block, and will eventually

argue that elements, though they be 'implicit', of the bargaining process may usually be relevant to the process mediating market power into price.

The difference that matters between firm-firm and firm-consumer price setting is that, in the latter situation, with a unique price offered to all customers, each buyer's sanction that can be applied to the seller is limited to anonymously leaving the market and buying from the fringe. Even if a determined customer were able to fight his or her way through to head office to bargain with the selling firm's management, no concession could be negotiated, since a lower price offered to one customer would have to be offered to all (if not, favoured customers would be able to make a quick profit by re-selling to the others). In Hirschman's (1970) increasingly well-known paradigm, private or small firm buyers lose the 'voice' option that is available to important customers; they can only 'exit'. And even if re-selling can somehow be ruled out, the threat of exit posed by single consumers will not usually be significant -- the selling firm should be able to find another consumer to sell to at low cost.

We may examine price-setting in consumer goods markets formally with a simple model.

Suppose, as before, a seller owning a segment of half-length  $M$  of a linear market bordered by a fringe selling at  $\bar{p}$ , that each consumer's demand for the good is perfectly

inelastic, and that, in particular, at a distance  $(M-m_1)$  from the border there is a unit demand. The only difference from the bargaining set-up is that the unit of sales is now split between a number of 'ith' consumers -- too many to negotiate with individually, so that price-setting is now the sole responsibility of the seller.

Clearly, price can be set at any level up to  $p_1'$ , where

$$p_1' = \bar{p} + f_b(M-m_1) \quad (\text{II.9}),$$

the price at which customers are just ready to move to the border to trade. The seller can appropriate all of the fruits of his market power, barring only a sufficient residue to dissuade his customers from transferring their custom to the fringe.

This analysis applies symmetrically to the case of atomistic sellers facing buyers with market power, such as is often found in markets for farm produce, when concentrated 'agribusiness' food processors buy at auction prices from individual farmers.

In both situations, the only way that the small traders can improve their situation is by themselves combining and trading as a single unit. Then they can threaten, by withdrawing altogether their custom or output, to impose costs on the large firm, which is likely to find it not possible to

place elsewhere such a quantity of sales or purchases without incurring significant additional expenses. Farmers, whose objectives are fairly simple -- to sell for the highest possible prices a rather small number of different products -- have often been able to do this, through marketing boards, but consumers, who are usually involved in purchasing a great variety of goods according to very different tastes and incomes, have not generally been successful in coalescing against industry.

In consumer goods market, customers may often not be evenly distributed throughout product characteristic space. This is most obviously so in the geographic dimension, in which there are concentrations of consumers in towns and cities, separated by more sparsely populated suburban and rural areas, but we may also find modalities, based, for example, on class, or income, or ethnic differentials, in other dimensions of the product space.

When a firm with market power can distinguish such groupings, it will find it profitable to differentiate its price. Thus, for example, if there were, in addition to the mass of consumers  $(M-m_i)$  from the border treated above, another group distanced  $(M-m_i+1)$ , the price that can be charged to the latter consumers is upper bounded by  $p_i''$  such that

$$p_i'' = \bar{p} + f_b(M-m_i+1) \quad (\text{II.10})$$

Such price discrimination is stable, so long as the actual prices charged are at least a little below their upper bounds given by (II.9) and (II.10), since then, given the difference in distances in product space, none of the  $(M-m_1)$  group will be able to profitably resell to the customers further away from the boundary paying the higher price.

Will, in fact, the seller usually price as close to  $p_1'$  and  $p_1''$  as is possible without driving away customers? I think not. The forces of empathy and notions of fairness may be expected to play a part in dealings between firms and consumers, and between big and small firms, as they do between firms of comparable economic power. Businessmen are citizens, and not usually especially wicked ones -- their desire for personal advancement and their ideas of reasonable behaviour may differ in degree from those of their customers, but not generally so much as to nullify the common bonds of citizenship and society. Reinforcing morality, too, is the fact that in a non-feudal society all but the richest managers and entrepreneurs have to rub along, day to day, in the society of their consumers, and may find it irksome to be reviled and ostracized for their business rapacity, especially, if like most decision-makers, they do not personally own the capital they are deploying, and thus do not receive in full the profits it earns.



Small businessmen in particular often live among and know personally some of their customers and so will be prone to succumb to peer group pressures not to be overgreedy.

There are additional reasons why it may benefit firms not always to charge literally what the market will bear. Given the uncertainty of the business environment, it may be worthwhile to buy a capital stock of customer loyalty with restrained pricing policies. Government may from time to time step in on behalf of consumers and place controls on firms and industries that it feels have been pricing, or acting otherwise, unfairly towards consumers. Importers may be encouraged by unusually high profits to devote resources towards establishing a large-scale presence in the industry.

Thus we have, when firms sell to atomistic consumers, a situation in which, even though there may be no direct contact between price-setters and customers, sanctions may be brought to bear on the latter's behalf, and ideas of fairness may qualify the actions of both parties, just as in the case of explicit, firm-to-firm bargaining analysed previously. We may call this implicit bargaining. However, despite the characteristics in common to the two sorts of bargaining, we should still expect firms selling to firms to make less money than firms selling to atomistic consumers, since firm buyers, due to the greater importance their purchases have relative to the selling firm's total market, can impose a greater no-trade threat than can individual, uncoordinated consumers.

In summary, we can write that the price charged under conditions of implicit bargaining is:

$$p_i = \bar{p} + \mu f_b(M-m_i), \quad 0 < \mu < 1 \quad (\text{II.11}),$$

for the market segment distanced  $(M-m_i)$  from the border.

When  $\mu = 1$ , we have the same situation as when  $\theta = 0$  in (II.6) - all the gains from trade appropriated by the seller; otherwise the two expressions differ.

I will not be concerned to try to determine, more precisely than was done above, the value to be taken by  $\mu$ . It surely will differ in different economies and industries according to their history, social structure, system of government, and other factors. It may be conjectured that empathy will be more strongly felt by smaller firms, and by those which are owned and controlled in the country in which business is being transacted, due to differences in the strength of the link between managers and their customers' social environment. Thus, for example, there have been some instances in recent years of British firms, not noted for being outstandingly greedy in the home market, being accused of badly exploiting their customers and workforce in their foreign subsidiary operations, especially in South Africa -- an economy with notably weak links of empathy between employing and (manually) employed classes.

We can easily find out if these models (if models be not too grand a term) of industrial price-setting have the property that profitability is an increasing function of firm size. For suppose that a firm doubles the length of its market segment from  $M$  to  $2M$ . Its market power over the new customers will be the same as its previous market power with respect to the original customers, and so too will be the distribution of prices charged. However, the firm's market power over the original customers has now increased, as they are now all distanced an additional  $M$  from the border.

In the implicit bargaining equation (II.11), this results unambiguously in an increase in the price charged the original customers, and so an increase in profitability. In the explicit bargaining case of (II.6), we cannot, in principle, rule out  $f_s$  increasing more than  $f_b$ , so that price falls.

This would occur if the selling firm's size become such that the supply of alternative customers outside its current market began to dry up. However, we should probably regard this possibility as a curiosum, to be observed rarely, if at all. This is because of differences in the  $f_s$  and  $f_b$  functions;  $f_b$  is likely to be a more steeply increasing function of  $(M-m_1)$ , for two reasons. First, product characteristics may often be less costly to alter in

production than in consumption. It will be of no concern to a car manufacturer whether he paints a car blue or red, but to the customer, the difference in colours may mean a lot. In the geographic dimension of product space, the unit cost of moving goods to the border may be less for the seller, who can benefit from economies of of consignment size (since he is already shipping to just before the border, where his territory ends) not accessible to buyers.

Secondly, sellers are more likely to be able to turn to a quite different market to trade in than are buyers. For example, a buyer of machine tools does not have much alternative to using machine tools in his production process -- he is rather locked in to buying goods from the machine tool industry. Manufacturers in the latter industry, on the other hand, sell their product to many industries, and so are locked in to none.

A similar story can be told for many capital goods, intermediate inputs such as packaging and transportation, and industrial materials -- they all have a number of alternative uses, in each of which they are significantly more appropriate than is the nearest substitute.

This implies that the market power of a particular buyer depends largely on the distance of the seller from alternative markets, which does not increase when its size in the buyer's market increases.

These justifications for expecting average price received, or profitability, to increase with increases in the seller's size in no way conflict with the suggestion made earlier that the seller's market power could be negative, if the product of the fringe firms were inherently more desirable. For, although for a particular value of  $(M-m_1)$   $f_b$  might be negative, we should still expect it to increase as  $M$  increases -- in the very act of expansion the selling firm will be taking over or driving out of business the fringe firms whose product was more attractive.

A factor which probably affects the buyer's market power more than the seller's is the size of the transaction between the two. For the seller, the bigger the transaction, the more difficult it will be to dispose of it elsewhere. A small sale may be placed easily enough given the natural turnover of customers from other sellers, and the growth, if any, in the total market, but to make a large sale, the firm will have to disrupt existing trading arrangements between other firms, to lure away some other seller's custom -- this will not usually be accomplished without costs.

On the other hand, a roving buyer is welcomed by the other sellers, who are eager to increase their sales (although a really large order may run into some difficulties, at least in the short run, depending on the supply elasticity of the industry) and is not likely to upset other buyers.

These arguments suggest that it is the size of buyer rather than seller that predominantly determines the former's market power in a transaction. If we measure the buyer's size as the amount of territory he owns in his selling market, we should modify (II.6) to

$$p_i = \bar{p} + \frac{f_b(M^S - m_i) - \theta f_s(M^i)}{\theta + 1} \quad (\text{II.12}),$$

where  $M^S$  is the length of the seller's market segment, and  $M^i$  is the segment of buyer  $i$ , in the market in which he resells (after processing) the product.

However, we should not expect this measure of buyer market power to necessarily be inversely correlated with  $p_i$ . This is because the buyer's power will also, in general, be used in his own selling market to raise the price there received, and some of the resulting profits may be passed back to suppliers, for reasons of empathy, fairness, or simple fear that these suppliers might otherwise be tempted to integrate forward into the next stage of processing. This 'piggyback' effect of intermediate sellers gaining from the market power of final market sellers, may well outweigh the countervailing power effect, so that  $f_s$  becomes a negative function of  $M^i$  in (II.12).

#### II.4 Summary of Chapters II.2 and II.3

In the preceding two chapters I have attempted to provide a model of industrial price-setting with market power that is realistic, testable, and that meets the objections to previous work that surfaced in the critical survey of Part I. The three most important of these criticisms were (1) the fragility of market power as therein conceived, (2) the inconsistent dichotomy between oligopoly and entry-threat models, and (3) the inconsistency, given arguments against oligopolistic co-ordination problems being an effective constraint on price, of observed industry price elasticities with price setting in the orthodox market framework.

The proposition that a good model should build on a concept of market power as a durable force is not uncontroversial -- we noted the laissez-faire school of thought which does believe that any firm's market power is transitory, eventually to be competed away, and which underpins the literature on small-scale entry. Therefore, we will be concerned to reinforce whatever plausibility the concept has, as developed in Chapter II.2, with explicit empirical tests in Part III of the thesis.

Market power was defined as the costs one firm or consumer can impose on another by not trading with it. The basis of such power is suggested to be differences in the characteristics of products, which give some attractiveness

to the continuation of existing trading partnerships, given that partners were appropriately matched in the first place. Three sources of market power were distinguished: 1) search costs, due to uncertainty, of dealing with new trading partners, 2) differences in product characteristics that are legally owned by firms, through patents, licensing and so on, 3) differences in basic (i.e., nonpurchasable) qualities of businessmen and organizations that give all firms comparative and some absolute advantages over one another.

The first and third of these are inherently durable; the second is given durability by the legal system reinforcing contracts and patents, and by all sorts of governmental licensing arrangements. Thus, although in my illustrative formal modelling I draw on concepts from the literature on location theory, I certainly do not believe that the 'problem' posed by these models -- of where rival firms will choose to locate in characteristic space -- is well put. If market power is a durable property right, firms are simply not free to set up where ever they wish in characteristic space; unless they possess a superior product or technique, new firms must buy their way into an industry, with the approval of existing operators.

Market power is determined not just by the distance, in product characteristic space, to the boundary of a firm's market territory, but by the prices there charged for the



nearest substitutes to the firm's product. Here we distinguish two cases -- 1) other firms with market power at the border, and 2) 'small' firms at the border.

In the first instance we have the oligopoly problem of co-ordinating to achieve the best possible price -- a problem which, it was argued in Part I, should not be too difficult for the parties concerned to solve. This is especially so when we drop the one-market assumption of orthodox models; it is only the proportion of output traded at and near the border on which an agreement on price need be reached, and this is made relatively easy by the similarity, ipso facto, of competing products at the border, and by the probable characteristic that these border interfaces are generally between pairs of firms -- we do not have an n-firm oligopoly problem.

Thus, borders between firms with market power should not be relevant to price setting and structure. However, firms also have borders, at some distance, with 'small' firms. By 'small' I mean firms whose product territory is not large enough to generate for them sufficient market power to alone force a price high enough to make them want to stay in business. For this sector of firms, prices will be set to balance the supply and demand of entrepreneurial capital and labour, which is determined, exogenously to any market-power firm, no matter how big, by the structure of attitudes towards risk-taking in the economy, and the income that entrepreneurs can earn in alternative (i.e., not self-employed) occupations. Therefore,

the presence of small border, or 'fringe', firms sets an exogenous constraint on the price-setting power of firms with significant market power.

The preceding two paragraphs have important and potentially testable implications for the relationship between structure and profitability. First, the traditional market structure variables -- concentration ratios, Herfindahl indices -- motivated as they are by the postulate of a unique price determined by the ease of colluding, may, at best, be rather weakly related empirically to profitability, because they ignore differences in the absolute size of firms. In our model, an industry of ten small firms would, ceteris paribus, earn a lower rate of return on its resources than an industry of ten big firms with the same concentration ratio or Herfindahl, since the former group, being nearer in size to fringe firms, should also tend to be closer in 'distance' to them, as well. This is because the likelihood of a small businessman being able to command the financial and managerial resources to set up in competition (either as producer or importer) with an industry is greater, the smaller the scale of firms in that industry.

Second, in the model of this Thesis, firms of different size in the same industry will, in general, charge different prices and make different profits. Under the oligopoly model assumptions, there is a unique industry price

charged by all firms, big or small. We have already noted the evidence (cf. Chapter I.2) that this just is not so; it is good to have a model consistent with the reality of such an important aspect of market conduct.<sup>9</sup>

These points suggest that a structure-performance link be best looked for at the level of individual enterprises, with a firm's profitability related both to its absolute size and its market share. This is not possible with the Census of Production data available for this Thesis, so that the market share effect will have to be omitted, and absolute firm size proxied by average size in each industry.

In Chapter II.3, the implications of market power for industrial price-setting were developed. It was found useful to distinguish between different orders of magnitude of the scale of transactions. Two trading partners involved in a deal that is for each of them a significant proportion of their total business will, it is suggested, find it worthwhile to maximize the gains from trade by tailoring any variable factors to suit their particular requirements, and will apportion these gains according to their respective market powers, in a process of 'explicit bargaining', which may be qualified by the empathy between the traders, and their notions of fair play.

On the other hand, for a firm with market power selling to (or buying from) many separate traders in each segment of its market, the set-up costs of individually

matching each transaction will be prohibitive relative to the value of business involved, and the firm will standardize its product and sell (or buy) at a take-it-or-leave-it price, determined only by its own market power (since the market powers of each of its small customers is insignificant unless aggregated by co-ordination), subject again to the constraints of empathy and morality.

It is proposed that this approach resolves the second and third problems treated in the industrial organization literature. The oligopoly situation can appear only at the borders between large firms, so that it is both a less pervasive and less troublesome to these concerned than is postulated in the orthodox, whole-market oligopoly models, and so can reasonably be assumed away in the optimal trade-off between simplicity and generality of analysis. The entry 'threat' as a factor influencing pricing is also not relevant. Firms offering a superior product or technology will enter anyway (at least, they will not be restrained by the pricing policies of existing firms), and their competitors will do best to make what money they can, in the time left to them. Firms with no innate superiority over existing operators (and these are assumed in all the entry-threat literature) will be unable to enter, without consent, the territory of the latter, since this is postulated to be, in general, owned by them. The dichotomy in the literature between oligopoly and entry-threat theorizing is thus resolved by arguing that both classes of

problem are mis-conceived. When this is set right, the inconsistency becomes irrelevant. However, although larger firms are safe from territorial invasions, they compete always with the fringe of small firms, whose price is not determined by market power considerations.

Therefore, even if total industry demand is quite inelastic (as assumed in our simple formal models), the price charged by individual firms with market power and no problems of collusive co-ordination with other firms is still determinate.

### II.5 Defining Profitability

So far, in both the Critical Survey of Part I, and the model building of Part II, I have used terms such as 'profitability' and 'margin of price over costs' for the dependent variable to be explained by structure-performance analysis, without specifying just what these concepts mean. This omission is dealt with in the present chapter, which looks first into industrial organization received practices, finds them wanting, and goes on to attempt to resolve the found anomalies by developing a serviceable definition of profitability to be used in Part III, in the empirical specification of the structure-performance relationship. The material in the chapter could thus have been divided between Parts I and II; but since the topic can be treated independently of other material, and because the chapter is rather short, I have preferred to maintain cohesiveness by presenting it together.

Not surprisingly, the two independent approaches of oligopoly- and entry threat-pricing imply two separate measures of profitability, both of which are found in the empirical literature, though not usually with any recognition of the particular theoretical source.<sup>10</sup>

In the oligopoly models, with their firm base in the traditional microeconomic framework of short-run market equilibrium, the appropriate dependent variable is the markup of price over marginal variable costs (see, for a clear

statement, Cowling and Waterson, 1976). The divergence of price from marginal cost is Lerner's 'degree of monopoly', bounded by perfect competition (divergence zero), and perfect monopoly (divergence such that marginal revenue equals marginal cost), and its level is determined by the oligopolists' success in co-ordinating their price-setting in their common interest.

There are problems with this measure, in practice and in theory. First, data on marginal costs have never been available for a sizeable sample of industries. Cowling and Waterson explicitly (and others implicitly) assume constant marginal costs, which therefore equal average costs, which can be easily measured by Census data on wage and salaries and material expenses.

This assumption has been supported by calling on the evidence from statistical cost studies of particular plants (e.g., Johnston, 1960) which typically seem to find L-shaped cost curves, flat in the relevant range, rather than the U-shaped curves drawn in microeconomic texts.

However, more direct evidence on the behaviour in the short term of labour productivity in UK manufacturing industries (Appendix A) reveals a quarter-to-quarter variations which are systematically related to fluctuations in output in a way that supports, in general, the U-shaped cost curve hypothesis, and that appears to uncover substantial differences between

industries in the 'tightness' of the U. These results imply that the relationship between marginal and average costs differs across industries, so that, for example, two industries could be observed with the same average costs per unit but have quite different marginal costs -- the assumption that measured average costs are a close proxy for marginal costs is not valid.

A further check on the (im)plausibility of the variable cost markup as the proper profitability measure is the magnitude of the quarter-to-quarter fluctuations in labour productivity that are implied by the productivity functions for the sample in Appendix A, and which would imply, in the oligopoly model, quarter-to-quarter changes in price of one or two per cent. This does not seem plausible given Atkin and Skinner's finding that most firms never change prices by less than 3 per cent (1975, p. 86).

It might be argued in response to this that it is a rather longer-term, 'normal' notion of costs to which oligopolists adjust their prices. This is reasonable, but, if a longer term is to be used, the definition of variable costs must be extended to include capital plant and equipment, which are not fixed over a longer time horizon.

The general failure to allow for a return to capital in the oligopoly price-setting models burdens them with the quite unreasonable prediction that, for example, two industries with



the same market structure and the same short-term variable costs per unit of output would achieve the same price-variable cost markup, even if one of them had a capital stock costing ten times as much as the other's; implying that the latter industry would earn a rate of return on its capital ten times higher than the former, for no reason other than an accident of technology leading to different capital/labour ratios.

A similar problem with the use of markup on variable costs as the variable to be explained by industry structure is that it implies that an industry's position on the chain of production affects its profitability. Consider the case of two industries with identical market structures, labour forces and capital stocks, but differing in the value of materials they purchase. The oligopoly model predicts that each will achieve the same percentage markup on all variable costs; that is, that the industry further up the chain (processing more valuable materials) will make more profit. This property may result in empirical tests of the oligopoly model showing spuriously good fits, if, as was suggested in the previous chapter, industries of firms selling to final consumers generally are more profitable than of firms selling to firms, due to the former group of customers having less bargaining power than the latter.

Some studies (e.g., McFetridge, 1973) use the markup on value added (net output) rather than on total value of sales. This, in fact, does avoid assigning an arbitrary importance to an industry's position on the chain of production (though the problem of industries with different capital stocks remains), which may cause it to 'fit' the data better. However, it has no grounding in any theory, as far as I am aware.

The use of a markup variable to be explained is in direct contradiction with the other common practice of empirical industrial organization work (at least in the US-data is a problem in the UK) of measuring as dependent variable profits as a ratio of the value of the industry's capital stock. The markup attributes nothing to capital; the ratio of profits to stock attributes everything to capital, and nothing to variable costs.

The profits/stock ratio is usually justified as the proper dependent variable implied by entry-threat models -- it is the rate of return on capital that supposedly attracts or repels potential competitors -- though in empirical work we typically find an unresolved mish-mash of 'barriers to entry' and 'ability to collude' explanatory variables. Only Cowling and Waterson (1976), as noted in Part I, have been bold enough to recognize the inconsistency and, working from an oligopoly model, to exclude entry barrier variables from their regression equations.

The natural definition of profitability that follows from the model developed in Chapters II.3 and II.4 is:

"The profits that can be earned by the resources of a firm, given its position in characteristic space (i.e., given its market power) relative to the opportunity cost of these factors,"

with the natural measurement of opportunity costs the return the factors would earn if stripped of their market power, namely the return they would earn if distributed in the fringe, or competitive, sector of the industry. This is consistent with our models in which market power determines a firm's price relative to the price charged at the fringe.

This definition would be equivalent, up to a multiplicative constant equal to the competitive rate of return on capital, to the profits/capital stock ratio measure if fixed capital were the only resource committed to production.

In general, however, this will not be so. First, inventories of materials, goods in process, and finished goods, have an opportunity cost equal to the funds tied up in them multiplied by the going rate of interest.

Secondly, we should allow returns to the managerial-entrepreneurial resources committed to firms by their owners and/or the managers who act on their behalf. It is not sufficient to simply purchase capital, materials and labour services in order to generate a profit-making operation: the factors of production must be organized and supervised if they

are to produce output, and therefore the input required to achieve this earns a return. We will not have a direct measure of the quantity of managerial factors supplied, but it may be reasonable to suppose that they are proportional to the quantity of other factors -- capital, inventories, labour -- that are committed to the production process. Certainly, it seems plausible that, of two industries with similar structure and capital stocks and labour forces of different sizes, the industry with more labour will produce more and will make larger profits.

Therefore, the measure of profitability used in the empirical work of this thesis will be defined as the ratio of the profits (excess of revenues over operating costs) actually earned to the profits that the resources committed to the industry would earn if deployed in the competitive fringe sector. I call this measure the surplus earned by an industry.

Part III: The Model Tested and Compared

III.1 Specifying the Model

In this chapter the theoretical model developed in Part II is specified for subsequent testing with data for the UK manufacturing sector. We are here looking for a 'short-term' relationship; that is, a relationship at any point in time between structure and profitability. In the second chapter, the model is estimated, and in the third, our results are compared with those of other researchers. Chapter III.2 also looks at the 'long-term' dynamics of the relationship, by which is meant the durability over time of the structure-performance link for particular industries. There are few previous results to compare with.

We may begin the specification procedure by writing equations (II.12) and (II.11)

$$p_i = \bar{p} + \frac{f_b(M^S - m_i) - \theta f_s(M^I)}{(\theta + 1)} \quad \dots \text{(III.1)},$$

when explicit bargaining sets price, and

$$p_i = \bar{p} + \mu f_b(M^S - m_i) \quad \dots \text{(III.2)},$$

which holds when implicit bargaining is the rule.

To get from these equations to a model that could be tested on our industry-level data, we would have to aggregate three times -- first over all the customers in each market of each firm, then over all the markets of each firm, and finally over all the firms.

To devote resources to such a tedious process would be to misplace the concreteness of equations III.1 and III.2 -- they were developed only as illustrations, based on particularly simple assumptions, of the essentially verbal analysis of the previous chapters.

In any case, we can anticipate the important features of the aggregation process. The dependent variable to be explained at the industry level is the ratio of the profits earned by the industry to the profits that the resources committed by the industry would earn in market-powerless, fringe activities. We can get to this as follows. Consider first just the aggregation of all customers in one market of one firm under explicit bargaining. Rewrite equation 1 as:

$$p_i - c = \bar{p} - \bar{c} + (\bar{c} - c) + \Delta f^i \quad \dots \text{(III.3)},$$

where  $\Delta f^i$  is a summary notation for the second term on the right hand side of (III.1) and  $c$  and  $\bar{c}$  are the unit direct production costs of the firm and the fringe.

Denoting as  $x_i$  the firm's sales to buyer  $i$ , and summing over all sales;

$$\sum_i x_i (p_i - c) = \sum_i x_i (\bar{p} - \bar{c}) + \sum_i x_i (\bar{c} - c + \Delta f^i) \quad \dots \text{(III.4)}.$$

Equation III.4 further rearranges to

$$\pi_i / \bar{\pi}_i = 1 + \frac{\bar{c} - c}{\bar{p} - \bar{c}} + \frac{\sum_i x_i \Delta f^i}{\sum_i x_i} \quad \dots \text{(III.5)}$$

-- the ratio of the firm's profits,  $\pi_i$ , to the profits,  $\bar{\pi}_i$ , that would be earned by fringe firms selling the same output is determined by the cost advantage of the firm and by a weighted average of the market power functions,  $\Delta f^i$ . We could carry on to aggregate over markets and firms, but will not do so -- Equation III.5 is intended to be illustrative only.

What is clear is that the result of full aggregation would be to find the industry/fringe profit ratio determined by market power, empathy, and cost differences in different markets and for different firms, weighted by the proportions of output being sold under conditions of explicit and implicit bargaining.

Such an aggregated function would no doubt be complicatedly nonlinear, and would differ for each industry. However, it is usually so in empirical work that, when there are firm relationships between variables that persist across industries, much of the variation in the data can be accounted for with a function containing just first, and some second order terms. Accordingly, the estimating equation to be specified here will be linear, with quadratic terms added for two of the most interesting variables. I could have been more energetic in testing different forms -- cubics, semi-log, double-log, for example -- and might, indeed, have slightly increased the ex post goodness of fit by so doing; however, I took the position that the dangers of 'data-mining' -- of choosing a specification that is twisted to best fit the particular error

structure in the observed data, at the cost of being relatively poor at predicting over a new sample of data and errors -- are such that extensive specification trials are not advisable.<sup>11</sup> A strong hypothesis should so reveal itself no matter what the functional form imposed on it.

Selecting the explanatory variables to be inserted in the estimating equation again entails approximations to reality. In a case study approach, such as employed by the Monopolies Commission, factors determining market power, costs, even empathy, can be uncovered and measured directly. Many such factors will be peculiar to the particular industry being studied. In econometric cross-sectional study, however, we are limited by data availability, time, and degrees of freedom, to considering just a few variables which may be important across all industries.

Of course, the challenge being accepted in a work such as this is precisely to show that such generalizations are possible; that our theoretical model is useful in that the simplified relationships it proposes do have some general validity.

The variable found in all structure-performance studies, and the main focus of interest in most of them, is some measure of the size distribution of the firms within each industry, reflecting their difficulties in colluding on price.



I have argued in this thesis that these difficulties are greatly exaggerated -- in the model proposed in Part II, large firms only need to collude where their territories abut, and should not generally there have much difficulty in doing this. Therefore, I do not expect measures of size distribution to contribute significantly to the explanation of profitability, and will test this by including the five-firm concentration ratio (SCR) as an explanatory variable.

What does help determine profitability in the model here proposed is not the relationship between large firms, but between each large firm and the competitive fringe; in particular, the 'distance' between the firm and the fringe. An obvious factor affecting distance is the size of the market-power firms, which determines the amount of territory they own. In an industry for whose product there are no substitutes, a measure of size relative to total industry size, such as is given by the SCR, could be appropriate. However, I expect that the no-substitute assumption is much too extreme to be a reasonable simplification; in general, we should assume the presence, for each product, of other products with significant cross-price elasticities, extending the concepts of territory and distance beyond the boundaries of an industry's Census definition.

We don't have yet the own- and cross-price elasticity estimates for all UK manufacturing industries that might permit us to derive market area measures (by weighting the sizes of

substitute goods industries by cross elasticities) against which to compare firm size to obtain an appropriate market power variable. Failing this, we can proxy the numerator of this ratio -- firm size -- by the Census data on the average net output per enterprise in each industry (NO/ENT), and hope that differences in this number are sufficiently independent of differences in market area for variations in the numerator to be significantly associated with variations in the true ratio, so that NO/ENT may be a useful explanatory variable.

An absolute size measure such as NO/ENT will be particularly relevant to the extent that the control of distribution and retailing outlets is an important ingredient in market power and that the costs of establishing such control do not vary much across different industries and products.

Differences in costs are another market power variable that we expected to affect profitability -- if one firm can produce more cheaply than another it may make more money. It will be able to do so for any length of time only if its cost advantage is durable; that is, if it is not accessible to fringe firms. This will be so either when the low costs are a rent-producing resource accruing from the particular skills or legal property rights (such as a favourable location) of some firms and entrepreneurs, or when the cost advantage is dependent on large scale production. The chances that these conditions are met may be proxied, in all three cases, by some measure of firm

size. We could expect that skill advantages would be associated not just with higher per unit profit margins, but also with more units sold -- if some of the lower costs are passed on to customers, the firm's market will expand relative to higher cost competitors. The number of legal property rights owned may also, on average, be correlated with size, and the exploitation of economies of scale will obviously be linked to the rate of output.

The first two of these factors might again be well-proxied by NO/ENT. Economies of scale, however, may be more closely associated with the size of plants rather than firms -- of establishments rather than enterprises, in the UK Census terminology -- which will be measured here by the average net output per plant in each industry -- NO/EST. Both size variables will be tried out.

Two variables which may be associated with seller market power, and which appear quite often (especially the first) in structure-performance regressions, though with no record of uniform success, are market growth rate, and the degree of exposure to import competition. The rationale for including the first of these may be interpreted, within the theoretical framework of this thesis, as distance in time, analogously to distance in other dimensions of product space, in a growing market, between existing firms and new entrants, giving the former some added temporary market power which they may exploit to increase their profits.

The statistical significance of market growth may be further, though spuriously, reinforced by the typical practice of measuring it by the actual growth in revenues of each industry, since any measurement errors in this number will appear also in the figure for profits on the left hand side of the regression equation. The proper measures of market growth would be, for consumer and intermediate goods industries, the change in personal disposable income weighted by an independently estimated income elasticity, and for capital goods industries, a function specifying investment expenditures, but such data are not so far available at anything like the level of disaggregation needed for a cross-section study of UK manufacturing.<sup>12</sup>

In any case, as we shall see, market growth variables as measured sometimes show negative signs, and are usually insignificant. There are at least two reasons why this should occur; a) if market growth is anticipated, new capacity should be added to keep pace, with no particular tendency for a profits bonus to accrue to anyone, and b) unusually fast growing industries may, in non-planned economies, attract an oversupply of eager entrepreneurs, resulting in price wars and bankruptcies. Two well-known examples of this are the colour television and electronic calculator industries.

Susceptibility to foreign competition is, in principle, an appealing variable. In Part II it was suggested that the ability to import from outside suppliers might be one of the

instruments used by fringe operators that constrains the profitability of market power firms -- but, as with market growth, the available measured proxies are blunted by opposing influences which may cancel out. Tariff protection in some industries may provide a shield behind which unusually large profits can be earned, but, in others, may be imposed to save unusually high-cost domestic industries from extinction, and so may be observed along with lower than average profits.

The other widely available measure -- the share of imports on total domestic sales -- is also two-edged. A high import ratio may indeed reflect a keenly competitive market, but it may also be due to an industry's profitability being unusually attractive, or simply to differing comparative advantages.

A third 'theoretical' factor, which is less ambiguously measurable than the first two, and so has shown more success in empirical work, is the susceptibility of consumers to various efforts firms may make to persuade them to purchase their products. The typical proxy for this is the ratio of advertising expenditures to sales value.

There is a problem in principle with using a simple measure of promotional expenditures, connected with the distinction between purely intra-industry advertising, used by each firm as a market-share weapon, which may not increase total

industry profits, and the externalities of advertising affecting total demand conditions for all firms in a market, which does affect profitability. In general, though, it should not be unreasonable to expect to find that industries which persistently advertise more do so because it pays off.

Some interesting and ingenious theoretical and empirical effort has gone into specifying the advertising-profits relationship (see the book by Cowling et. al., 1975) but I shall not here go further than simply including the ratio of advertising to sales as an explanatory variable, with a positive sign expected. I take this simple-minded approach because I rather doubt that the measured advertising/sales ratio is really up to supporting any sort of elaborate theoretical framework. Much, perhaps most, of promotional efforts are not captured in the measure of bought-in advertising services. The firm itself must typically devote resources to new product development, to its own market research, and to assisting in the choice of marketing strategy, before it gets to the stages of purchasing space and time in the media. A striking, though probably extreme (because of the high unit value of a sale) example of this is provided by the famous study of Fisher, Griliches, and Kaysen (1962) who found the costs of cosmetic model changes in the US car industry to be about \$700 per car per year, compared with just \$14 spent on advertising. Usually advertising will be complementary to the other unmeasured

marketing inputs, and so be a useful statistical proxy, but, as such, cannot legitimately be used, for example, in tests for profit-maximizing behaviour through calculation of marginal value productivities.

This completes the listing of empirically accessible seller market power variables. We turn next to measures of power on the side of the buyers in the bargaining situation.

In the case of firms selling intermediate goods to other firms, where the terms of the transaction are set by explicit bargaining, the power of the buyer's no-trade threat is a function of the size of the transaction, since this affects the ease with which the seller can relocate his output. A large order may take time or price-discounting to place in alternative markets, both involving a loss of profitability to the seller. A natural measure of buyer size is its importance in the industry in which it sells. Accordingly, to get a measure of the buyer, or countervailing power facing an industry selling intermediate goods, I aggregate the seller concentration ratios of all the purchasing industries, weighting each by the proportion they take of the selling industry's output. This, and all other variables are defined and sourced precisely in the data Appendix C.

The buyer concentration ratio (BCR) would measure just what it is supposed to if there were no substitutability between an industry's products sold in different markets. In

general, then, we should weight further by estimates of cross-elasticities of demand. These are not available; therefore we must hope that our rather blunt proxy for buyer power will yet be sensitive enough to pick up the relationship hypothesized.

Both BCR and its squared value will be included in the regression, to allow for any non-linearity. As we noted in Chapter II.2, we cannot predict a priori the signs of the buyer power coefficients, since countervailing power may be dominated by the 'piggyback' effect.

Output not sold as intermediate input goes to final users. The Input-Output tables allow us to distinguish four groups of these -- consumers, public authorities proportion of output sold to each of these final users (BUYC, P, K, E) as explanatory variables. Their coefficients will estimate the net effect of buyer power and empathy in the bargaining process in each of these markets. Of course, it may be rather much to assume that buyer conditions are the same within each group; again, it is to be settled empirically if our simplification is viable -- that is whether between-group variation is sufficiently larger than within-group so that the proportion-of-sales variables can show independent significance.<sup>13</sup>

Our theoretical framework suggests some a priori, predictions about the relative size of coefficients. The coefficient of the proportion sold to consumers, BUYC, should be relatively large, since most consumer sales take place under



conditions of implicit bargaining, which, it was suggested in Part II, is more profitable than explicit bargaining -- the buyers' power is small and uncoordinated. The BUYE coefficient may also be large, if sellers are less constrained than in domestic markets by empathy considerations when they sell to foreigners. The coefficient on BUYK should be smaller than the others, since firms purchasing capital goods will be mostly in an explicit bargaining situation, with their buyer power often reinforced, compared to intermediate goods buyers, by the relative unimportance of time -- an intermediate purchaser who fails to close a deal with one firm may have to find an alternative supplier rather rapidly, or face costly production scheduling disruptions, whereas a capital goods buyer may quite easily be able to continue operating with its existing capacity, at no great cost.

About the coefficient on BUYP we should perhaps best be agnostic in a priori expectations. Public authorities may tend, on average, to purchase in greater bulk than the private sector, which would make selling to them less profitable -- their buyer power would be greater. On the other hand, the public sector may not be so strenuous in obtaining the best possible deal as profit-motivated private sector buyers.

To complete the specification of the right hand side of the market power-profitability equation to be estimated we should note that a constant term will probably not show

significance. To see this, write the profit rate  $r_{ij}$  earned by industry  $i$  from its sales to sector  $j$

$$r_{ij} = a_j + bS_i \quad (\text{III.6}),$$

where  $a_j$  is the effect of the buyer's market power, and  $S_i$  and  $b$  are vectors of seller market power and the associated coefficients. The observed rate of profit  $r_i$  is a weighted average of all the  $r_{ij}$ , with, as weights, the proportion of sales made to each sector,  $\theta_{ij}$ :

$$r_i = \sum_j r_{ij} \theta_{ij}, \quad \sum_j \theta_{ij} = 1, \quad (\text{III.7}),$$

$$= \sum_j a_j \theta_{ij} + bS_i \quad (\text{III.8}).$$

In a regression, the  $\theta_{ij}$  are our BUY proportions, and the  $a_j$  their estimated coefficients -- that is, in a specification with just buyer proportions, no constant is needed, even if there is a common, or constant, element included in all the  $a_j$ 's. The specification used in the next chapter is not quite equivalent to (III.8), since intermediate good sales proportions are already weighted with concentration ratios, but we should probably not be surprised if this adulteration is insufficient to generate a need for an intercept term.

Finally, we go to the left hand side of the equation, to put the profitability variable in a measurable form. In (III.5) the appropriate measure was defined as the ratio of

profits actually earned by an industry to the profits that would have been earned by fringe firms selling the same physical output. Direct measurement of this would require data on actual fringe prices in each industry; data which, of course, we do not have. However, this does not create a problem, since we can make use of our theoretical framework, which suggested that the rate of return to fringe firms is determined by aggregate conditions in the supply and demand for small entrepreneurs which are exogenous to each industry -- that is, fringe prices are determined as markups on the resources committed which are the same (or, at least, tend to be the same -- there will be some errors and adjustments due, for example, to differences in riskiness) in all industries.

Thus, we do not need to be told the actual fringe price  $\bar{p}_i$  in each industry; we can calculate it, if we can measure a) the resources committed to each industry, and b) the average fringe rates of return earned per unit of each factor resource.

Economic factor input resources fall naturally into two classes -- tangible and intangible. First, there is the capital tied up in plant, machinery, and buildings, and in inventories of materials and finished goods. For some economies (the US, Canada) data are available that directly measure capital assets at a disaggregated level; for the UK we will need to devise proxies.

Secondly, there is the intangible entrepreneurial/managerial input that induces capital and labour to combine to produce useful output. Again, direct measurement is not possible, but it may be reasonable to propose that the size of organizational input required is a function of the quantity of resources -- capital and labour -- tied up in an industry, and so to proxy the return to this input that is expected in fringe firms as some markup on the quantity of tangible factors involved.

We will call these markups on the three measurable categories of inputs -- capital stock, inventories, workforce -- that are expected as normal in the fringe sector of manufacturing, NORMRK, NORMRI, NORMRL. We then calculate the normal profits or returns that fringe operators would expect to earn from the factors deployed in the  $i$ th industry, NRET,

$$\begin{aligned} \text{NRET}_i &= \text{NORMRK} * \text{Capital Stock}_i + \text{NORMRI} * \text{Inventory Stock}_i \\ &+ \text{NORMRL} * \text{Wage Bill}_i \end{aligned} \quad (\text{III.9}).$$

The profitability variable is then the ratio of actual profits earned in industry  $i$ , PROFITS $_i$ , to NRET $_i$ . This will be called the 'Rate of Surplus', or just 'Surplus', and denoted by the symbol  $r_i$ ;

$$r_i = \text{PROFITS}_i / \text{NRET}_i \quad (\text{III.10}).$$

The NORMR parameters will have to be estimated along with the right hand side coefficients -- we do not have any direct measures for them. NORMRK and RI will bear some relation

to observed interest rates, but will certainly not tend to equal them -- they incorporate, as well, the riskiness of tying up capital in a business, the psychic income of being an entrepreneur, and the returns to managerial effort. Riskiness will differ between RK and RI depending on the relative liquidity of capital and inventories. NORMRL takes into account the managerial resources required to keep a workforce working productively, which we assume is best taken as markup on the wage bill rather than numbers employed; the former giving a 'quality-adjusted' measure of labour resources committed.

Apart from their inherent differences, the NORMR numbers will vary to adjust for the time period chosen -- NRET and the Wage Bill are flows, whereas the capital and inventory variables are stocks.

### III.2 Estimating the Model

The estimating equation suggested in the previous chapter can be written

$$r_i = \text{PROFITS}_i / \text{NRET}_i = a_1 X_{1i} + \dots + a_m X_{mi},$$
$$i = 1, \dots, n \quad (\text{III.11});$$

surplus is determined as a linear function of  $m$  structural seller and buyer power variables in each of the  $n$  industries in the sample. The only departure from linearity will be to experiment with including the squared values of some of the more important variables. The equation cannot be estimated as it stands, since the denominator of  $r_i$  contains parameters -- normal rates of return -- which themselves must be estimated:

$$\text{NRET}_i = \text{RK} * \text{KS} + \text{RI} * \text{IS} + \text{RL} * \text{W} \quad (\text{III.12}),$$

abbreviating (III.9).

Equations (III.11) and (III.12) can be rewritten as

$$\text{PROFITS}_i = (\text{RK} * \text{KS} + \text{RI} * \text{IS} + \text{RL} * \text{W}) (a_1 X_{1i} + \dots + a_m X_{mi}) \quad (\text{III.13})$$

All the unknown parameters in this equation could be estimated together by a suitable non-linear least squares algorithm. The 'algorithm' that I in fact have used is simply to search over a 'grid' of R-values. Of course, the grid over which I search is much coarser than the grid which would be

surveyed by a proper nonlinear least squares computer program. Setting up the latter would, on the other hand, involve a lot of computer time and programming resources. My justification for not incurring these costs was the conjecture that the benefits of the added precision would not be worthwhile -- I expected the  $R^2$ -maximization hill to be rather flat at its summit, such that quite large changes in R values do not raise or lower much the goodness of fit. In this case, the added 'precision' of a formal algorithm would not be very meaningful; indeed, the less pretentious one-digit precision of my grid search would convey a more appropriate impression of the significance of this aspect of the results.

Before this conjecture can be tested by estimating (III.13), one remaining obstacle must be cleared away. This is the problem set by the lack of data on capital stocks at the Minimum List Heading level of aggregation for UK manufacturing industries. The only available information for the sample period is on investment expenditures, and this only for census years. A fringe firm in a steady state, investing the same amount each year, would expect returns equal to  $(1 + RK)$  times the investment flow each year. I shall adopt this result to calculate a figure for normal returns, hoping, in so doing, that error due to firms not being in steady states is reduced to insignificance by using data averaged over five census years. Averaging should also reduce the effect on the data of any

cyclical link between profits earned and investment expenditures in any particular year. To the extent that such a relation is not averaged out, the calculated significance of the coefficients (the a's) of the structural variables will be biased downwards by the simultaneity between PROFITS and by measure of  $RK*KS$ ,  $(1+RK)GINV$ , where GINV stands for gross investment expenditures.

The database is extracted from the individual reports at the Minimum List Heading level of aggregation for each of the full census years 1954, 1958, 1963, 1968, and 1973. These data were aggregated, where necessary, to the level of the 1963 Input-Output tables, so that the buyer power variables could be calculated. This gives a breakdown of the manufacturing sector into 57 industries. Six of these were dropped from the sample because price indices, which are needed for the analysis of Chapter IV.1, could not be calculated. No other grounds for exclusion were entertained, in contrast to several of the studies to be surveyed in the next chapter, in which some industries were omitted because they were 'outliers', or because their 'specialization' or 'exclusiveness' ratios were judged to be low. The latter justification for restricting the sample is, perhaps, consistent with oligopoly-based models, in which it is the price set in a particular market which determines the dependent variable, price-cost margin, so that an industry with a low exclusiveness or specialization ratio, (i.e., one that



operates in several markets) might show a rather blurred relationship between structure and the average margin, if there are any non-linearities involved in aggregation.

However, such a criterion is not valid within the framework being urged in this thesis, in which it is the rate of return to factor inputs which is to be explained, which is not expected to be particularly affected by orthodox classifications of census firms into 'markets'. In fact, the present model might as well be tested by any arbitrary aggregation of firms into industries -- I expect a non-market-based structural variable such as size of plant to be a more appropriate explanatory variable than concentration ratios based on the Standard Industrial Classification.<sup>14</sup>

Nevertheless, concentration measures will be given as good a chance as possible to show their importance -- the gaps in the published figures have been filled using information reported in the book of case-studies by Walshe (1974). Where aggregation was necessary, it was done by weighting the concentration ratios of constituent industries by the proportion of sales they accounted for in 1963 in their Input-Output industry group. Details on all the data are given in Appendix C.

As noted, the buyer power variables were calculated using the 1963 I-O tables, and so are for that year. Concentration and advertising data are widely available from two censuses -- 1963 and 1968, and are calculated as averages for

those years. Other variables, such as net output per establishment, and the dependent variables, are averaged over all five census years.

The estimation strategy followed was, first, to take the specification of structural variables that had apparently emerged as 'best' from a preliminary series of R-value comparisons, and to use this as the base for performing the quasi-nonlinear-least-squares algorithm; using power to explain total profits as the measure of goodness of fit. The set of R's giving the best fit is then retained for the second stage, in which some alternative combinations of structural variables are tried out. There is no particular justification, so far as I know, for preferring this strategy to others; it does have the property, relative to more exhaustive and exhausting procedures, of limiting the number of regressions to be run to a number that may preserve some of the degrees of freedom available from our sample. Of course, the best ex post justification for a limited specification search can be offered if the model being estimated proves to be rather robust, so that coefficients and their significance levels do not change much with marginal specification alterations.

Of interest will be a comparison of the performance of my surplus variable with that of the conventional gross price-cost margin measure. Since gross margin also has total

profits as its numerator, we can compare goodness of fit by comparing ability to predict profits.

Table III.1 shows the goodness of fit of predicted against actual profits for the initial specification (as in equation 1 in Table III.3) over a 'coarse' grid of R-parameters. To restrict the search to two dimensions, rates of normal returns to fixed capital and inventory stocks were assumed to be equal. The net effect of the differences in liquidity and perishability between fixed and circulating capital cannot be signed a priori; no doubt it differs between industries, hopefully by less, in most cases, than the degree of precision which is reasonably attainable.

In assessing correlation coefficients, what is important is not their absolute levels, which are largely determined by the extent to which variables fluctuate relative to their means, but the relative sizes of the changes in goodness of fit that result from changes in specification. We may see that the poorest-fitting R-set has an  $r^2$  of about 0.83, and the best an  $r^2$  just above 0.90. The worst fit is given by the extreme hypothesis that all the R's are zero; that is, that the proper measure is simply the ratio of profits to the capital stock (as proxied by average investment flows). Softening this extreme case by allowing fringe firms to earn some return on either their inventories (row 1) or their management of a workforce (column 1) results in relatively substantial improvements in explanatory power.

TABLE III.1 GOODNESS OF FIT ( $r^2$ ) OF PREDICTED VERSUS ACTUAL PROFITS: COARSE GRID OF R-PARAMETERS

RK, RI RL	0.00	0.10	0.20	0.30	0.40	0.50
0.00	0.8307	0.8750	0.8786	0.8750	0.8700	0.86
0.10	0.8630	0.8987	0.8999	0.8950	0.8889	0.88
0.20	0.8665	0.8982	0.9027	0.9004	0.8961	0.89
0.30	0.8594	0.8912	0.8990	0.8996	0.8974	0.89
0.40	0.8511	0.8826	0.8930	0.8961	0.8959	0.89

NOTES: Predicted values of profits are from the least squares regression of the AVSURP specification with independent variables as in regression (1), Table 3, and with NRET calculated using the RK, RI, RL values given above. The numbers are the squares of the simple correlation coefficient between predicted and actual profits.

TABLE III.: GOODNESS OF FIT ( $r^2$ ) OF PREDICTED VERSUS ACTUAL PROFITS: FINER GRID OF R-PARAMETERS

RK, RI RL	0.10	0.15	0.20	0.25	0.30
0.10	0.8987	0.9007	0.8999	0.8978	0.8950
0.15	0.8999	0.9026	0.9026	0.9011	0.8989
0.20	0.8982	0.9019	0.9027	0.9020	0.9004
0.25	0.8951	0.8996	0.9013	0.9014	0.9005
0.30	0.8912	0.8965	0.8990	0.8999	0.8996

NOTES: As for Table 1.

These results have two encouraging implications for the theoretical and empirical methodology of the thesis. First, they support the argument that the proper profitability ratio measures actual profits against 'normal' rates of return to all the resources that firms and their owners commit to the production process, not just to capital or just to variable inputs.

Second, they justify the decision made to not devote resources to devising a full nonlinear least squares algorithm to estimate equation (III.13) -- the flatness of the  $r^2$  surface when the R's are varied, so long as all are taken to be non-zero, suggests that any attempts at further precision would be spurious.

Nevertheless, it seemed just worthwhile to fine down the grid somewhat around the apparently best-fitting choice, which assigned a value of 0.20 to all the R's. The results of so doing appear in Table III.2. The slight supremacy of the (0.20, 0.20, 0.20) set remains when the grid is fined down by a factor of two.

Therefore, the Surplus variable taken as dependent in all the following tests of different structural specifications is calculated using these values of normal, or fringe, rates of returns.<sup>15</sup>

An independent check of the reasonableness of these parameters, and of our whole hypothesis that Surplus, as defined here, is the proper dependent profitability variable, can be made

by looking at the actual values so calculated. We would expect the variable to be generally greater than one, with this number being the lower limit to be observed, normally, only in industries made up entirely of competitive fringe firms -- industries with no market power firms at all. Indeed, it can be seen (Appendix C) that of the fifty-one industries in the sample, only two -- Industrial Engines, and Cotton, etc., Spinning and Weaving show surplus values (slightly) less than one. So few such observations could easily be accounted for by data errors, or by an industry experiencing unusually depressed trading conditions in the census years.

The results of limited experimentation with the specification of market power variables are shown in Table III.3. In this, and Table III.4, the numbers shown are the coefficients estimated by ordinary least squares regressions, with t-ratios in parentheses. The means of the variables are also given, and reveal that some scaling was done, to roughly eliminate disparities in the number of significant digits to be tabulated.

The first specification (regression 1) is the one used in the R-parameter search. It includes eight explanatory variables, of which all but BUYP achieve statistical significance at conventionally accepted levels. As hypothesized, the average size of plant in an industry has a positive relationship with profitability, as does the other seller power variable, the proportion advertising expenditures are of total revenues.

TABLE III.3 Profitability Regressions; Averaged Data

Dependent Variable	SCR 5	NO/EST	(NO/EST) <sup>2</sup>	ADV <sub>R</sub>	% DGB	BCRINT	(BCRINT) <sup>2</sup>	BUYC	BUYP
Variable Means	61.37	3.43	48.1	1.27	0.187	20.28	6.48	34.60	5.82
1. AVSURP		0.0227 (3.2)		0.192 (6.9)		0.0413 (5.0)	-0.0426 (-2.5)	0.0145 (11.0)	-0.0090 (-1.4)
2. AVSURP <sup>1</sup>				0.191 (6.8)		0.0411 (4.8)	-0.0412 (-2.4)	0.0149 (11.2)	-0.0081 (-1.2)
3. AVSURP	0.00389 (1.8)			0.199 (6.7)		0.0415 (4.5)	-0.0487 (-2.7)	0.0124 (7.0)	-0.0134 (-1.9)
4. AVSURP	-0.0001 (-0.0)	0.0229 (2.5)		0.192 (6.8)		0.0414 (4.7)	-0.0426 (-2.5)	0.0146 (7.8)	-0.0089 (-1.3)
5. AVSURP		0.0216 (3.1)		0.185 (6.6)	0.360 (1.3)	0.0412 (5.0)	-0.0461 (-2.7)	0.0142 (10.7)	-0.0113 (-1.7)
6. AVGMARG		-0.00795 (-2.1)	0.000287 (2.7)	0.0119 (2.4)	0.132 (2.7)	0.00561 (4.0)	-0.00728 (-2.5)	0.00142 (5.9)	0.00063 (0.5)
7. AVGMARG		-0.00690 (-1.9)	0.000219 (2.1)	0.0119 (3.0)	0.138 (2.6)	0.00112 (0.5)	-0.00518 (-1.8)	-0.00086 (-0.9)	00.00080 (-0.6)
8. AVGMARG	-0.000813 (-1.7)	0.00370 (2.3)		0.0155 (3.0)		0.00662 (4.1)	-0.00694 (-2.2)	0.00169 (5.5)	0.00134 (1.1)
9. AVSURP		0.0205 (2.7)		0.192 (6.9)		0.0332 (2.3)	-0.0384 (-2.1)	0.0106 (1.8)	-0.0112 (-1.5)
10. AVXSURP		0.0174 (1.5)		0.274 (6.1)		0.0539 (4.0)	-0.0514 (-1.9)	0.0200 (9.3)	0.0052 (0.5)
11. AVSURP <sup>2</sup>		0.0211 (3.1)		0.176 (6.3)	0.610 (2.3)	0.0430 ( 5.4)	-0.0525 (-3.2)	0.0138 (11.1)	-0.0113 (-1.7)
12. AVSURP <sup>3</sup>		0.0167 (2.7)		0.179 (7.1)	0.517 (2.1)	0.0400 (5.6)	-0.0441 (-3.0)	0.0136 (12.2)	-0.0058 (-0.9)

<sup>1</sup> Regression with GO/ENT and GO/EST had R<sup>2</sup>'s of 0.641 and 0.624.

<sup>2</sup> Same as equation (5) except for agricultural machinery buyers have been reallocated from BUYK to BUYC

<sup>3</sup> Same as equation (11) with sugar, mineral oil refining, and Iron and Steel deleted.



BUYK	BUYE	Mean of Dependent Variable	R <sup>2</sup>	$\bar{R}^2$	r <sup>2</sup>
8.76	17.21				
0.0035 (2.6)	0.0114 (2.8)	1.607	0.671	0.618	0.903
0.0088 (2.6)	0.0119 (2.9)	NO/ENT (mean = 7.05) 0.006 (3.0)	0.662	0.607	
0.0069 (1.9)	0.0095 (2.1)		0.621	0.559	
0.0085 (2.4)	-0.0115 (2.6)		0.671	0.609	
0.0059 (1.5)	0.0123 (3.0)		0.684	0.624	0.905
0.00100 (1.5)	0.0024 <sup>5</sup> (3.5)	0.197	0.372	0.234	
-0.00109 (-1.0)	-0.00021 (-0.2)	Constant (mean = 1.0) 0.228 (2.3)	0.445	0.307	0.863
0.00201 (3.2)	0.00237 (3.0)		0.240	0.095	
0.00491 (0.8)	0.00674 (0.9)	Constant 0.407 (0.7)	0.675	0.613	
0.0178 (3.3)	0.0164 (2.5)	2.333	0.544	0.470	
0.009 (0.2)	0.0132 (3.3)		0.699	0.641	
0.0013 (0.4)	0.0135 (3.8)	1.606	0.733	0.679	

Buyer power in intermediate goods markets has a strongly, though diminishing, positive effect on Surplus, supporting the 'piggy-back' postulate.

The BUY proportions should be considered relative to each other -- it is not the sign of the coefficients, but the distance between them that matters. Thus, a shift of one percentage point in output from public authority buyers to consumers would apparently be associated with an increase in surplus of about 0.024. Consumer markets appear to be the most profitable to sell in, followed by foreign buyers, capital purchasers, and public authorities. The relative position of the last group may be surprising to some.

These results are quite robust, in the sense that when one variable is tinkered with, or a new one added, the coefficients on the rest of the specification do not change much.

Regression 2 differs from 1 in including output per enterprise (firm) rather than per plant as the main seller power variable. The new variable does not do so well as NO/EST; nor did Gross Output per establishment or per plant (footnote 1). These less successful variables are more appropriate to models based on market price-setting structure-performance links, which I criticized as probably unrealistic in Part I, than to the competitive fringe-constrained hypothesis that I have put forward in Part II, under which size of plants is a quite acceptable market power proxy.

Regressions 3 and 4 show what happens when seller concentration is introduced; first by itself, then in competition with NO/EST. Alone (regression 3) seller concentration barely achieves significance, and this is wiped out completely when the two variables appear together, suggesting that SCR's significance by itself is just because it then acts as a proxy for size of plant (the simple correlation coefficient, between SCR and NO/EST is 0.525). That is, these results give no support to the oligopoly theorists' main prediction that market concentration is the appropriate seller power structural variable.

Three variations on the specification of seller power were tried. Neither the square of the advertising ratio nor the proportion imports were (in 1963) of total domestic sales showed any significance at all, and the regressions are not listed here. The insignificance of import penetration is consistent with the results of other UK studies, as will be seen in the next chapter, and is consistent with much of importing being controlled by the same firms (themselves foreign owned, in many cases) who produce within the UK.

The third variable experimented with was the current price growth in sales between census periods (regression 5). We have already noted a source of possible spurious correlation between this variable and profitability, in that, in a single period analysis both will include the same measurement errors; this, however, may not be a problem in the present study, in which both variables are averaged over four or five censuses.

Therefore, the moderate contribution that sales growth makes to the explanatory power of our structure-performance hypothesis (it slightly increases the corrected  $R^2$  from that of regression 1) is accepted as valid, and regression 5 is settled on as the final specification of the thesis; subject only to small changes in coverage and variable measurement to be discussed below. Adding a quadratic term did not improve explanatory power at all.

The prediction deduced above that a constant term should not be needed, since its role is usurped by the BUY ratios, is supported by the results of regression 9, in which the inclusion of an intercept adds nothing to explanatory power, and disturbs the BUY coefficients.

Next, the Surplus variable was compared to the conventional Gross Margin measure of profitability, the best-fitting specifications for which are shown here in regressions 6 and 7 (the latter with a constant term). It may be noted a) that the standard errors of coefficients in the GMARG regressions are usually not as good as those found with SURP dependent, and b) that Gross Margin specifications do not predict variations in profits as well as those with Surplus. The hypothesis that it is the returns to all productive factors that is the proper measure of profitability to be explained by market power factors is given empirical support.

Regression 10 introduces a variation, 'XSURP', on the profitability measure. XSURP is defined as the ratio of profits plus salaries paid to a measure of net returns which excludes salaries from the wage bill on which the normal markup is calculated. This regression was run to test the 'managerial discretion' hypothesis (cf. Williamson, 1963) that, in large manager-controlled firms, the managers are able to divert some of the surplus into their own pockets, so that it is not declared as profits, and that will show up as a larger and/or better paid managerial class.

Neither numerator nor denominator are the same as for the standard surplus measure, so that there is no way of comparing simply goodnesses of fit between the two, but as all but one of the estimated t-ratios are smaller in regression 10 than in the comparable Surplus regression (1), it does not seem possible to assign any support from these results to the managerial discretion hypothesis. Of course, this may just be due to data inadequacies -- in the Census, employees are divided into wage and salary earners, but the latter group includes many secretaries, clerks, and low- and middle-level managers who may not be in positions of sufficient power to expropriate surplus on their own account.

Finally, in Table III.3 I relax somewhat the rule that the only exclusions permitted from the sample should be those imposed by data unavailability. Examining the residuals

from regression 5, I found that ten industries had errors (ratio of residual to actual Surplus) greater than 20 per cent. These industries were: Grain Milling, Sugar, Mineral Oil Refining, Paint, Iron and Steel, Agricultural Machinery, Industrial Engines, Radio and Telecommunications, Cotton, etc., Spinning and Weaving, and Paper Products. Without searching too hard, it seemed that there were plausible grounds for treating at least four of these outliers as special cases.

First, the Agricultural Machinery industry supplies fixed capital to the agricultural sector; consequently, the bulk of its domestic sales are classified in the Input-Output tables to Capital Formation, and show as BUYK in my data. However, most agricultural machinery is still purchased by individual farmers, whose buyer market power may be less than that of the industrial capital purchasers who carry out the bulk of private fixed capital formation. Indeed, the power of individual farmers may be closer, on average, to that of private consumers than to industrial firms. Therefore I experimented with transferring the whole of the PBUY ratio for this industry to BUYC.

The result (regression 11) is a noticeable improvement in  $R^2$  over regression 5, with little change in coefficients except that of BUYK, which is sharply lowered. If the ex post rationalization is convincing, this change in the data should be therefore accepted permanently.

The final regression, number 12, modifies the sample further by dropping three industries -- Sugar, Mineral Oil Refining, and Iron and Steel -- which are known to have had unusually disruptive dealings with the Public sector over the period; Sugar with a subsidy payment system, Refining with heavy indirect taxes, and Iron and Steel through nationalization.

These deletions produce a further appreciable improvement in goodness of fit.<sup>16</sup>

We should note that the positive effect of buyer power does not imply that it is necessarily more profitable to sell to other firms than to final consumers. The BCRINT coefficients estimate the effect of a ceteris paribus change in this variable. Since the other variables held constant include all the BUY percentages, and therefore the proportion sold as intermediate goods as well (since this is just the residual from subtracting all the BUYS from 100), such a change can only come about through a change in the seller concentration ratios of the intermediate buyers, and this, through the 'piggyback' effect, is positively related to profits.

Since BCRINT appears quadratically, and is, in general, the weighted sum of a number of SCRs, there is no unique number than can be assigned to the effect of increasing BCRINT through the proportion sold to intermediate buyers, holding constant their SCRs. However, we can carry out an illustrative calculation. Suppose that an industry sells one-third of its

output to just one intermediate buying industry, of which the four-firm concentration ratio in its selling market is 60 per cent -- about average for our sample. Then BCRINT equals 20 - about its average value in the sample -- and its squared value, divided by 100, is 4. The partial effect on profitability of increasing the proportion sold to the intermediate buyer by 10 per cent is about

$$0.0400 * 2 - 0.0441 * 0.84 = 0.0430$$

using the BCRINT and  $(BCRINT)^2$  coefficients of regression 12 in Table III.3

The net effect depends on where the increase in intermediate sales is taken from. Suppose that it is the proportion of sales to final consumers that falls by 3.3 percentage points, to accommodate the increase by this amount in the proportion sold as intermediate goods. The partial effect on profits of this equals

$$- 0.0136 * 3.3 = - 0.0449,$$

so that the net effect on profitability --

$$0.0430 - 0.0449 = - 0.0019$$

- is small and negative. Similarly, one could calculate that the difference in Surplus between selling all one's output as intermediate goods to an industry with an SCR4 of 60, and selling it all to final consumers, is about

$$- (2.400 - 1.584) + 1.360 = 0.544$$



greater for the consumer goods industry -- about one-third of the mean value of Surplus in the sample.

In comparison with previous work on structure-performance relationships within UK manufacturing, the model tested above seems to have achieved significantly better empirical results, on both criteria of individual coefficient significance and overall goodness of fit. Of course, this is as it should be -- there would be no point in retracing the ground covered by earlier researchers if their results could not be built on to achieve improvements in our understanding of the empirical regularities observable in the structure-performance relation.

The improvements can be attributed to three types of innovation. First, the theoretical framework advanced in this thesis lead to a more appropriate definition -- 'Surplus' -- of the profitability variable to be explained.

Second, the framework also took us away from the orthodox concern with market concentration towards a variable, average size of plant, which, although crude, performs better empirically; and towards a concern to articulate fully buyer market power factors.

Third, the use here of data averaged over five census years may have eliminated much of the statistical 'noise', due to random and cyclical factors affecting the profitability of

particular industries in particular years, that may be presumed to afflict the previous studies which used information from only one census year, usually 1963 or 1968.

The latter point is demonstrated in Table III.4, in which regressions 3 through 7 show the results of estimating the basic specification, shown in Table III as regression 1, separately for each for the five census years. Significance levels of all coefficients, most drastically for NO/EST, are reduced sharply from those achieved by the same observations when averaged, or when, as in regressions 1 and 2, the data for each year are stacked to give series of 255 (= 51x5) observations on each variable. As might be expected, the stacking process, since although it greatly increases the number of degrees of freedom of the regressions, also retains random and cyclical 'noise', gives lower overall explanatory power and a tendency to lower t-ratios than can be achieved by averaging the data.

An interesting implication that can be drawn both from the census-year dummies of regression 1 and from the intercepts of regressions 3 to 7, is that there seems, since 1958, to have been an autonomous upward drift in the surplus that an industry extracts from a given set of market power factors. This result, which apparently conflicts with the claims, by Glyn and Sutcliffe (1972) and others, that the rate

TABLE III.4 Profitability Regressions; Stacked Data

Dependent Variable	Estimated Period	NO/EST	(NO/EST) <sup>2</sup>	ADVR	BCRINT	(BCRINT) <sup>2</sup>	BUYC	BUYP	BUYK	BUYE	DUM58
Variable means		0.356	0.665	1.270	20.28	6.48	34.60	5.82	8.76	17.21	0.2
1. SURP	all five years	0.303 (2.2)	-0.058 (-2.2)	0.198 (8.5)	0.0455 (6.3)	-0.0542 (-3.8)	0.0120 (8.6)	-0.0134 (-2.4)	0.0063 (2.2)	0.0094 (2.6)	-0.125 (-1.1)
2. MARG	all five years	0.00976 (0.6)	-0.00346 (-1.2)	0.0163 (6.1)	0.00556 (6.8)	-0.00715 (-4.4)	0.000981 (6.1)	0.000339 (0.5)	0.00140 (4.3)	0.00170 (4.1)	0.00215 (0.2)
3. SURP	1954	0.164 (0.9)		0.177 (5.9)	0.0341 (3.8)	-0.0303 (-1.7)	0.0120 (7.9)	-0.0004 (-0.6)	0.00709 (2.0)	0.0130 (2.7)	
4. SURP	1958	0.258 (1.3)		0.183 (6.0)	0.0226 (2.3)	-0.0091 (-0.5)	0.0129 (7.8)	-0.0084 (-1.1)	0.0121 (3.1)	0.0059 (1.2)	
5. SURP	1963	0.0673 (0.8)		0.237 (7.2)	0.0445 (4.5)	-0.0497 (-2.5)	0.0127 (7.7)	-0.0037 (-0.5)	0.0083 (2.1)	0.0111 (2.2)	
6. SURP	1968	0.0414 (0.3)		0.153 (1.6)	0.0822 (2.9)	-0.1202 (-2.1)	0.0152 (3.2)	-0.0241 (-1.1)	0.0058 (0.5)	0.0114 (0.8)	
7. SURP	1973	-0.0383 (-0.4)		0.235 (4.4)	0.0621 (3.9)	-0.0680 (-2.1)	0.0189 (6.9)	-0.0222 (-1.8)	0.0072 (1.1)	0.0141 (1.7)	

DUM 63	DUM 68	DUM 73	Mean of Dependent Variable	R <sup>2</sup>	$\bar{R}^2$	r <sup>2</sup>
0.2	0.2	0.2				
0.146 (1.3)	0.272 (2.4)	0.476 (4.3)	1.607	0.388	0.358	0.880
0.0359 (2.8)	0.0514 (4.0)	0.0629 (4.9)	0.198	0.228	0.189	0.884
			1.461	0.470	0.384	
			1.309	0.547	0.473	
			1.596	0.593	0.527	
			1.732	0.187	0.054	
			1.934	0.472	0.386	

of profit in the UK has been declining recently, might be worth further investigation. The explanation for the decrease may lie in a tendency for industries to invest less over time, if this tendency a) exists and b) has not (yet) been reflected in a decline in productivity, due perhaps to the long average life of capital equipment, so that industry is still earning profits on plant installed decades ago. This would reduce normal returns, as measured here, without correspondingly cutting back the numerator, profits, of the surplus variable.

Finally, we examine the durability of the relationship between structure and performance. The results reported so far give quite solid support to the hypothesis that, in any time, there is a correlation between market power and profitability. However, the regression equations estimated explain by no means all of the variation in Surplus. The observed residuals could be due to measurement error, to crudities in specification, and to omission of significant market power variables.

As well, they could reflect a tendency in the market system for the profits earned by market power to be whittled away over time. If a given set of market structure parameters become less profitable the longer they are observed, my regression specification, which is not time dependent, will be in error.

That monopoly profits are transitory in this sense follows from both orthodox theoretical approaches to structure-performance modelling. In the oligopoly theories, emphasis is placed on the fragility of the collusive agreements that are supposed to be the source of above-normal profits. In the entry-threat models, it is just a question of time before 'excess' profits are competed away by new competitors.

In contrast, the theory proposed in this thesis has profitability determined by durable market power property rights, which are, quite literally, 'owned' by firms. Therefore, it is important to try and examine directly the dynamics of the market power-profitability relationship.

There are surprisingly few, given its importance, precedents for such an investigation. Sullivan (1977) has shown, for the US, evidence that the returns to market power -- i.e., the ability to earn revenues greater than the opportunity cost of the factors committed -- are fully capitalized into the stock market valuation of firms, but this just implies that it is the originators -- entrepreneurs, innovators, clever businessmen -- of firms' market power who capture the rewards therefrom, rather than the essentially passive participants in the market for shares of existing enterprises. It does not as Sullivan emphasizes, imply that the capital market is 'efficient' in the sense that it acts to break down excesses of price over costs:

"The data in this study suggest that capital is being allocated efficiently in the sense that investors receive expected returns sufficient to compensate for risk ... Nothing in the capital market forces the powerful firm to reduce its output price and increase the volume of its output" (p. 111).

Another study, by Orr (1974), of Canadian manufacturing industries, attempts to explain average annual increases in the number of corporations operating in each industry over the period 1964-67. Only variables measuring the scale of each industry are significant; in particular, past profit rates have no discernible effect. In any case, Orr's dependent variable tells nothing about what happens to profits after entry takes place.

One recent study, by Mueller (1977), of data on 472 US firms, reports measures of the proportion of firms remaining in each of eight profitability groups after twenty four years (from 1949 to 1972). The proportion keeping their place in the highest profitability group is 0.34, which is significantly higher than the proportion to be expected if all monopoly profits tended to be dissipated over the period so that the highest-profit firms were evenly redistributed over the eight groups by 1972.

However, it is not clear whether Mueller's bottle, so to speak, is 'half-empty' or 'half-full'. A commentator with laissez-faire tendencies might reasonably claim that the figure of 66 per cent of the highest-profit group losing their position indicates that market discipline has functioned rather

well. To settle the matter, we need a model which properly isolates the effects on levels and changes in profitability of market structure from cyclical and other sources of year-to-year variation.

One reason why more empirical attention has not been paid to this most important matter is probably the tendency of researchers to use just one, or at most two, census years of observations to test their hypotheses. The more extensive sample assembled here, however, does lend itself to direct testing of the durability of market power.

This is done by comparing average 1968-73 Surplus with the average for 1958-63. We do not use the 1954 data lest the difference in the definition of the output variables for this census is a dangerous source of inaccuracy to inter-temporal comparisons. The result of Table III.4 that 1954 is apparently out of line with the upward trend in Surplus does suggest that, despite my attempt to correct for it (see Appendix C), redefinition does affect systematically the data with which surplus is measured.

Even without the earliest observations, the distance between the midpoints of the two pairs of census observations is ten years, and a decade should be long enough for the effects of entry on competition to show themselves, if they do exist.



Averaging of pairs of census observations is done following our experience earlier in the chapter that averaging does tend to eliminate a significant amount of cyclical and other sources of 'noise' which tend to obscure the structure-performance relation.

In Table III.5 are shown first regressions of the Surplus-Structure specification for each of the two periods. These are given just as a check that the hypothesis appears valid for these sub-periods of our total sample. There are no major upsets -- multicollinearity between NO/EST and its square does prevent either from showing significance in the earlier period (NO/EST by itself was comfortably significant), but the quadratic specification is retained to maintain exact comparability with the 1968-73 specification.

Next, we report an attempt to identify any process of erosion of monopoly profits over time. Believers in this process should predict that a given set of structure parameters would be associated with lower profits in the latter period the higher profits had been earlier, since higher profitability in the past would have encouraged entry of new competitors. We test this by adding to the standard 1968-73 Surplus regression specification a variable measured for each industry as the excess of 1958-63 profits over average normal returns in those years, with an expectation, if the competitive discipline is active, of a negative sign.

TABLE III.5 Durability of Monopoly Profits

Dependent Variable	NO/EST	(NO/EST) <sup>2</sup>	ADVR	BCRINT	(BCRINT) <sup>2</sup>	BUYC	BUYP	BUYK	RIYE	SURP-NRET, 58-63	SURP-SURP*, 58-63	p <sup>2</sup>	p̄ <sup>2</sup>
1. SURP 58-63	-0.160 (-0.6)	0.144 (1.4)	0.0176 (7.3)	0.0329 (4.4)	-0.0271 (-1.8)	0.0142 (11.7)	-0.0009 (-0.1)	0.0072 (2.4)	0.0107 (2.9)			0.710	0.650
2. SURP 68-73	0.275 (1.7)	-0.0215 (-1.2)	0.0215 (5.6)	0.0517 (4.9)	-0.0576 (-2.7)	0.0144 (8.2)	-0.0017 (-0.2)	0.0032 (.08)	0.0129 (2.5)			0.627	0.551
3. SURP 68-73	0.322 (2.2)	-0.0309 (-1.9)	0.0053 (1.0)	0.0457 (5.0)	-0.0519 (-2.8)	0.0110 (6.3)	0.0024 (0.3)	0.0031 (0.9)	0.0130 (2.9)	0.785 (3.9)		0.733	0.670
4. SURP 68-73	0.295 (2.3)	-0.0237 (-1.6)	0.0210 (6.9)	0.0519 (6.3)	-0.0581 (-3.4)	0.0144 (10.4)	-0.0014 (-0.2)	0.0031 (0.9)	0.0129 (3.2)		0.886 (5.0)	0.775	0.722

NOTES 1) For SURP 58-63 regression, NO/EST is 1958-63 average, ADVR is 1963; for SURP 68-73 regressions, these variables are for 1968-73 and 1968, respectively.

2) Coverage same as for regression 12, Table III.3

The result is shown as regression 3 in Table III.5. The added variable in fact is positive, and has a comfortably significant coefficient, which gives no support at all to the competitive discipline hypothesis.

This sign must itself be explained. I expect that it is due to the omitted structural variables, measurement error, or whatever, that were the source of the unexplained residuals in the 1958-63 regression, persisting over time, to help generate the residuals over the latter period. A direct test of this is made by adding the first period's residuals to the 1968-73 regression, as in regression 4 in the table. This variable is even more significant than the excess of Surplus over normal returns, suggesting that the latter was just acting as a proxy for the residuals, with which it is correlated.

Of course, it could be true that the gross residual carry-over effect is greater than the coefficient in regression 4 would imply, with the difference made up by a competitive discipline process. However, the coefficient is not significantly different from one, which value it would take if carryover were complete.

Even if the coefficient is indeed 0.886, as estimated, and if all the difference between this figure and one is due to the competitive process, the numbers imply that only about 8 per cent of an excess of Surplus is eliminated in a decade, which suggests that the date when even half of the profits are competed

away is very much in Keynes' long run. Even then, the evidence of these regressions is that the rate of profitability being approached is the rate, including market-power profits, which is suggested by the Surplus-Market Power hypothesis, not the 'normal' rate of return earned by fringe firms.

The hypothesis that the gains from market power are durable seems to be strongly supported by these results.

III.3 A Survey of Previous Empirical Work on UK  
Structure Profitability Relations

In this chapter, we look at the seven previous studies of the structure-profitability relationship across UK manufacturing industries that use census of production data, and one (Cowling and Kelly, 1975) using data on individual firms in the UK food processing and distribution sector.

With the exception of the Warwick work, these studies exemplify all too well the casual approach towards the theoretical underpinnings of the specified regression models that seems endemic to the field of Industrial Organization. It is worth demonstrating this with passages from each of the five non-Warwick articles:

"This relationship [between market power and price-cost margins] may be regarded as merely reflecting the deeper relation between market power and rates of return on investment" (Shepherd, 1972, p. 47).

"The anticipated relations ... [signs of the partial derivatives of profitability with respect to concentration, entry barriers, market demand] require no explanation" (Phillips, 1972, p. 178).

"The hypotheses to be tested cannot be derived from economic theory without making many unacceptably restrictive assumptions ... Nevertheless, they are all intuitively reasonable and there is nothing in economic theory to suggest that they are false" (Holtermann, 1973, p. 121).

"Traditional theory suggests that monopolies and oligopolies tend to have higher profits and prices than competitive industries, ceteris paribus" (Hart and Morgan, 1977, p. 177).

Such vague and passing acknowledgement to 'theory' contrasts with the procedure in most other areas of empirical economic research -- for example, on consumption functions, investment functions, the demand for money -- in which models are specified a priori in a reasonably tight fashion by explicit theoretical reasoning.

The industrial organization practice would be defensible if the existence of a well formulated and uncontroversial theoretical framework made restatement in every empirical study superfluous. This, of course, is not the case. In Part I of this thesis, I did uncover two distinct, and quite sophisticated strands of theoretical modelling of the determinants of differences in profitability, but argued a) that each class of models suffers from immanent inadequacies, and b) the two purport to explain the same phenomenon and so should be at least mutually consistent, but in fact are not so.

(a) Five Orthodox Structure-Performance Regression Analyses

Looking at the results of the eight studies summarized in Table III.6<sup>17</sup> we may find revealed the costs associated with undertaking empirical econometric work without a sound theoretical base. We shall leave aside for the moment the three 'Warwick' studies associated with Cowling, Kelly, and Waterson, and consider the five articles from which passages were quoted above. All of the latter group have in common that

Table III.6: UK STRUCTURE-PROFITABILITY STUDIES: TABULATION OF RESULTS

	Shepherd	Phillips	Holtermann	Khalilzadeh-Shirazi	Hart and Morgan	Cowling and Kelly	Cowling & Waterson	Waterson
Coverage	3-digit (22 small industries excluded)	71 3-digit (availability of CRs limits coverage)	113 MLH industries	60 MLH industries	113 MLH industries	88 food companies	94 MLHs (changes in MLH descriptions limits coverage)	51 input-output level industries <sup>2</sup>
Dependent variable	gross price cost margin, 1958-63 average	gross margin, 1951 (on gross output)	gross margin, 1963 (on sales)	gross margin, 1963 (on sales)	net margin, 1968 (on net output)	gross margin (on sales) 5 year average 1964-69	ratio 1973-6R gross margins (on sales)	as for Cowling and Waterson 1968-73
Concentration	SCR5, 1958-1963 average, adjusted for imports (+1.6)	SCR3, 1951 (+2.2)	SCR5, 1963 (-1.1)	SCR5, 1963 (+0.3)	- (dropped as insignificant) -	SCR4, 1963 (cf fn 4)	ratio 1963-1958 employment Herfindahls (+2.9)	ratio 1968-63 employment Herfindahls (+3.4)
Entry Barriers	sales, average 1958-63 (+2)	average employment size of plant 1951 (+2.2)	average employment size of largest plant (-1.0)	median plant net output/industry net output (+3.9)	median employment size (-1.5)	net assets, average 1965-69 (+0.1)	-	-
Sales Growth	% change in sales, 1958-63 current prices (+2.0)	% change in gross output 1948-54 (-1.5)	% change in sales, 1958-63 current prices (+3.6)	% change in sales, 1958-63 current prices (+1.5)	% change in sales, 1963-68, current prices (+0.9)	-	-	-

	Shepherd	Phillips	Holtermann	Khalilzadeh Shirazi	Hart and Morgan	Cowling and Kelly	Cowling & Waterson	Waterson
Advertising	-	advertising/sales, 1948	advertising/sales, 1963	advertising dummy <sup>3</sup>	advertising/sales, 1968	advertising/sales, average 1965-69	-	-
	-	(+2.8)	(+5.9)	(+3.0)	(+4.1)	(+3.1)	-	-
Buyer Power	-	-	-	exports/total output 1963	-	-	-	ratio 1968-73 constructed buyer Herfindahl
	-	-	-	(+2.1)	-	-	-	(+2.5)
Capital Intensity	average 1958-63 capital expenditure/output	-	asset value/gross output	average 1969-70 net assets/sales	capital expenditure/employment 1968	-	-	-
	(-0.5)	-	(+2.1) average 1958-63 capital expenditure/output 1963 (+4.1)	(+2.3)	(+6.0)	-	-	-
Other variables	-	SCR * advertising/sales	-	imports/total output 1963	imports/domestic sales, 1968	additional advertising variables: total adv. exp. 1965-69 (t=+2.5) and the square of this (t=-1.8)	durable good dummy	-
	-	(+1.5)	-	(-1.3)	(+0.2)	(+0.6)	(+0.6)	-
		'effective price-fixing dummy'					ratio 1968-73 prop'n union membership	
		(+1.3)					(+1.5)	
		producer goods dummy						
		(+1.0)						
$R^2$ ( $R^2$ )	(0.114)	0.260	0.454	0.544	(0.462)	0.342	(0.096)	(0.267)



FOOTNOTES TO TABLE III.6

(Numbers in brackets are the t-ratios of the estimated coefficients.)

1. Excluded from the 119 industry MLH sample
  - a) milk, sugar (non-comparable price-cost margins)
  - b) industries with concentration ratios published for less than 80 percent of their principal products
  - c) industries with specialization index less than 80 percent
  - d) margarine and compound fats (an 'outlier')
2.
  - a) includes mining, construction
  - b) excludes seven industries (MLHs 211, 331, 390, 334, 338, 342 and 349, 362) with 'out of line' specialization and exclusiveness ratios. When not excluded 'results remain significant in many cases but the explanatory power is poorer' (1975, p. 13).
3. Advertising dummy = 1 when advertising is one percent or more of sales, zero otherwise.
4. Concentration included quadratically. Linear and squared coefficients had t-ratios of +3.1 and -2.8, respectively.

they attempt to explain variations in some measure of price-cost margins over a sample of UK manufacturing industries, using Census of Production data at the Minimum List Heading level of aggregation. The following points may be made:

- 1) All studies blandly include variables for both concentration and entry barriers even though the oligopoly models underlying the concentration-profits hypothesis ignore outside firms, and the entry threat literature assumes away oligopoly problems by considering only monopolistic, or quasi-monopolistic (price leadership), conditions within the industry -- the implications for specifying profitability of integrating the two approaches have not, to my knowledge, been uncovered. Nor, of course, do I believe that resources should be devoted to 'generalizing' the existing theoretical modelling efforts; rather, I have argued for a different framework, which downplays oligopolistic co-ordination difficulties, and re-specifies the entry threat in terms of small fringe firms.
- 2) Another problem generated by the dichotomy in received theory is the choice of dependent variable. As we pointed out in Part I, oligopoly models work with the price-cost

margin, whereas entry threat is determined by the return on resources. Four of the five studies being looked at now use the gross price-cost margin, and one (Hart and Morgan) the net margin (profits as a ratio of net output or value added); only one writer (Shepherd, in the passage quoted above) even recognizes any sort of difficulty with this.

- 3) A rather striking feature of the results is that concentration is not a generally significant variable in multivariate regressions, though when left to itself it does show a correlation with margins (Hart and Morgan). Only in Phillip's equation, and then marginally, does the seller concentration ratio have a statistically significant coefficient. This is in line with my own results of the previous chapter, and is consistent with concentration only showing significance in regressions when it is acting as a proxy for other, omitted, variables.
- 4) A variety of entry-barrier proxies are used; only Khalilzadeh-Shirazi's shows any substantial significance. The generally poor showing contrasts with the results of this thesis, that a rather crude measure (net output/establishment)

is strongly related to profitability; the differences are probably due to the more appropriate dependent variable that I use, and to the beneficial (noise-reducing) effects of averaging data over five census years. Four of the five studies explain one year's margins, and one (Shepherd) averages over two.

- 5) All the studies include a current-price sales growth variable. The reservations expressed in Chapter III.1 about the use of such a variable seem to be born out by the generally poor results.
  
- 6) In contrast to the dismal picture of mediocre significance of supposedly important factors seen so far, all four studies which include an advertising variable find it to be quite strongly significant. (It would be interesting to know just how much of the explanatory power in each of these structure-performance regression was contributed by this variable, which most researchers would probably expect to be of second order importance compared with concentration and entry barriers).

- 7) Only Khalilzadeh-Shirazi of the five includes a variable (exports/total output) which, within our framework, could be interpreted as a buyer power factor. He finds moderate significance.
  
- 8) In four cases, (three successfully) a measure of capital intensity appears as an 'explanatory' variable. This requires careful interpretation. The rationale appears to be the realization that price-cost margins relate profits only to variable costs, and that this is not sufficient -- fixed capital earns its return, too. This is a position with which, of course, I am wholly in agreement, but it is important to realize that what the capital intensity variable is doing in these studies is simply correcting the profitability variable, so that its statistical significance should be netted out of the measured goodness of fit to get a true picture of the explanatory power of the market structure-profitability hypothesis that is being tested. Since none of these studies do this, the  $R^2$ 's they report are over-estimates of the power of their hypotheses. It is easy, but spurious, to 'explain' variations in a variable by first measuring it incorrectly, then including a correction factor as a regressor.

9) Two other factors may have contributed to the significance of the capital intensity measures. First, in two cases (Holtermann, Khalilzadeh-Shirazi) output, or sales, appears as the denominator of the variable. Since output is also the denominator in the dependent variable, a source of spurious significance is thereby introduced (which may also have exaggerated the t-statistics of the advertising sales coefficients).

Second, there may be an upward simultaneity bias to calculated significance when current capital expenditure is the numerator (Holtermann, Hart, and Morgan), due to the accelerator relationship leading from profits to investment, so that an industry which earned unusually (because of cyclical fluctuations) high or low profits in the sample year would show unusually high or low capital expenditure in that year.

These observations may explain the contrast between the often quite high apparent significance in three of the studies with the failure of Shepherd's expenditure/output variable, since in the latter study the variables are averaged over two census years, which may be sufficient to eliminate both sources of spurious significance noted above.

- 10) Neither Khalilzadeh nor Hart and Morgan had any more success than did I in discerning a significant relationship between profitability and the share of imports in the domestic market.
  
- 11) In summary, we will look at the results as a whole of the five studies. We may do this because each attempts to explain a similar measure of profitability over a similar sample (UK manufacturing) with data taken from post-war Censuses of Production -- a period over which, according to my own results, a quite stable relationship between market structure and profitability does appear to exist. The lack of stability of coefficients across these studies rather suggests, then, that differences are the outcome of extensive mining of the data that has churned out large numbers of regression equations, of which the five shown are just the choicest examples.

Therefore, the only defensible way of drawing any conclusions from these studies is to treat them as drawings from the same population, and consider only the 'mean' of the results. Thus, if we find a similarly-defined explanatory variable achieving 'significance' in only one or two of the

five regressions, we should conclude, if anything, that the variable is not a determinant of profitability.

The results of applying this procedure to the variables in common are:

- a) market concentration is not an important determinant of profitability;
- b) entry barriers are probably important;
- c) sales growth may be important (caveat measurement error bias);
- d) advertising is important.

These conclusions are consistent with my own findings.

- 12) As a whole, the results are poor. Despite the data-mining, both t-statistics (excepting for advertising) and  $R^2$ 's are low. Further, the measured goodness-of-fit is biased upwards by a) the use of capital intensity (a correction factor), b) the presence of the same variables on both sides of equations, and c) simultaneity, in two studies, from the use of current capital expenditure as an explanatory variable.

(b) The Warwick Studies

We now look, in turn, at the three market structure-profitability studies by Cowling and Kelly (1975), Cowling and Waterson (1976), and Waterson (1975, 1976), which have recently



surfaced from the work being done at the Department of Economics and the Centre for Industrial, Economic, and Business Research at the University of Warwick.

Cowling and Kelly's article differs from the others in that it uses data on individual companies, and is restricted to the food industry. The results are good. The problem of cyclical 'noise' is dealt with by averaging data over the five year period 1965-69. The study is one of series focusing on the economics of advertising, and particular attention was paid to the specification of this factor, including the use of a concept of advertising as an investment in generating a 'stock' of goodwill, which is semi-durable.

Cowling and Kelly find concentration to be significant, while an absolute size measure (net assets of each firm) is not. This seems to conflict with my own findings. However, a firm-by-firm measure of size does, I expect, vary a great deal more than the average industry plant size variable that I have made use of, due to historical factors of firms being of different ages, and to differences in the number of markets in which a firm operates -- factors which will not particularly affect profitability, and which will thus be 'noise' obscuring the market power relationship between size and profits. If so, then concentration (an industry measure) may be a better proxy than total firm assets.

As well, two sources of differences in profitability between industries which play an important role in my model -- degree of exposure to fringe firms, and buyer market power -- may not surface in a study limited to explaining profits within the food sector, since the high cross-elasticities of demand linking food products will tend to equalize the threat from small operators, and since food is sold almost entirely to one group -- consumers -- through the same retail outlets.

It would be most interesting to see a structure-profitability study that uses a sample of individual company data drawn from all UK manufacturing industries.

The work of Cowling and Waterson has already been discussed, in Part I, as the only example of an attempt to specify a structure-performance model explicitly from theory. Their's is an oligopoly model, and their equation, it will be recalled, has the profit margin as the dependent variable:

$$\pi/R = \frac{H}{\eta}(1 + \lambda) \quad (\text{III.14}),$$

where  $\pi/R$  is the ratio of (gross) profits to revenues,  $H$  is the Herfindahl concentration index,  $\eta$  the price elasticity of demand, and  $\lambda$  the 'conjectural variations' parameter -- the response of all other firms to a unit change in one firm's output.

The problem with estimating (III.14) is that data are not available on the last two variables. Cowling and Waterson get around this by explaining changes in profitability, and assuming  $\eta$  and  $\lambda$  to be constant over time, so that their estimating equation has just the ratio of  $\pi/R$ , 1968-63, determined by the 1963-58 ratio of Herfindahl's (the five-year lag to allow for slowness in adjustment of performance to changes in structure, and to eliminate any problem of identifying causality (p. 269).

I find their assumption dangerous, albeit empirically necessary -- it seems quite likely that  $\eta$  and  $\lambda$  too change over time, and that they may do so, indeed, as a result of changes in concentration. If so, we cannot expect the estimated coefficient on the Herfindahl to be an unbiased estimate of this variable's series ceteris paribus influence on profits.

In any case, Cowling and Waterson estimate their change-in- $\pi/R$  equation, and find a comfortably significant coefficient for the Herfindahl ratio. This is consistent with findings that there is a simple correlation between levels of concentration and profitability (Cowling and Waterson's analysis is essentially bivariate -- two additional variables are included ad hoc, but without much significance), but does not, of course, rule out concentration just being a proxy for omitted factors, in particular the distance from fringe firms of the market power firms in an industry, which dominate when all are included in a regression,

as I find in this thesis. Cowling and Waterson do not include any change-in-entry-barriers variable because of data unavailability (p. 269), but in later work (the joint paper first appeared as Warwick Economic Discussion Paper No. 44 in April 1974) Waterson goes onto the offensive, armed with the results of Spence (1974) that the entry threat is independent of the pricing decision, and argues (1976, p. 92) that entry variables should not, even if available, be included in the structure-performance model. This is certainly consistent with the oligopoly assumptions, and avoids the unresolved messiness of the earlier studies surveyed, which include both concentration and entry barrier variables in an ad hoc fashion, but it is also misleading, since it results in regressions being run which appear to give some support to the oligopoly model; support which seems to disappear in multivariate analysis when other factors are included.

Hart and Morgan have questioned the robustness of Cowling and Waterson's result. They find that, when 18 'noncomparable' industries are excluded (industries for which sales reported for 1963 under the 1968 SIC differed by more than 5 per cent from the 1958 SIC figure for the same year) the relationship loses its significance (p. 187, footnote 2). This does not seem very fair, since, as Hart and Morgan admit, it is precisely those industries in which drastic change has forced a major SIC revision which will show the changes in

structure that Cowling and Waterson need to test their hypothesis. 'Noncomparability' will introduce a source of noise into the empirical relation, but Hart and Morgan give no reason, and nor can I think of any, why it should also introduce a bias towards making a Type 1 error.

Waterson has extended his joint work with Cowling to allow for the effects on profitability of buyer, or 'bilateral', power (1975, 1976). Again, the original model is Cournot's, generalized by Waterson to the many-firm, many-market case. As I write, I do not have available Waterson's 1976 thesis (though I have read this), so I will use as my reference his earlier Discussion Paper.

The Cournot bilateral oligopoly model may be called a 'piggyback' model (cf. Chapter II.3) -- selling firms ride on the market power of their customers in the latter's selling markets -- monopoly profits in final goods markets are shared out down the production chain, according to the market power of the firms at each stage. Thus intermediate buyer power is supposed to increase seller profitability. In my own model, I did not rule out a possibility that the stronger effect of buyer power would show as its use to force down the price paid to suppliers, but found the piggyback effect to predominate.

Waterson's assumptions imply a specific and rather complicated estimating equation (equation (28), p. 10) which includes the price elasticity of both seller and his customers.

He gets rid of the former as in the earlier joint work, by taking ratios over time and assuming the elasticity to stay constant, but the customer elasticities are inextricably imbedded in the expression ( $H_{BUY}$ ) representing the effect of buyer power. Waterson therefore assumes these to be all equal as well as unchanging, and tries out three values (2.0, 1.5, 1.0). The smallest elasticity value gives the best results, and is used in the results reported in Table III.6. (It would be interesting to know if goodness of fit continue improving if elasticity were further reduced into the realistic range of inelastic demand).

Waterson's buyer power variable has a positive sign, consistent with the piggyback effect and my own results. There are problems, though. His formula requires him to assign, a priori, Herfindahl numbers to consumer and nationalized industry customers. Consumers are given a Herfindahl of zero. This is a mistake. If consumers were really firms, themselves reselling, their lack of concentration would mean that they would have little market power, and so would earn low profits, so that their suppliers would not get much either. But, of course, consumers do consume the product, not resell it, and the price that can be squeezed out of them (and other final buyers) is the source of the monopoly profits that are then distributed back down the production chain. A lack of organization on the part of final buyers allows higher, not lower, prices to be charged by sellers.

One might expect, given the strongly positive sign of the coefficient for proportion sold to consumers found in my own regressions, that the mistake in Waterson's formula would prevent his HBUY variable from showing much significance. That it does have a quite high t-statistic suggests that we look for a source of possible spurious significance. Such is not hard to find. The variable used is the change between 1963 and 1968 of the constructed buyer Herfindahl, which is basically an index number of the proportions of output sold to each industry weighted by that industry's own seller Herfindahl. Nearly all these Herfindahls are small--for consumers zero, for 49 of the 51 producing selling industries less than 0.1 -- compared with the Herfindahl assigned a priori to nationalized industries and, presumably, public authority buyers, of 0.9. This suggests that a) for the bulk of industries selling a small and/or constant fraction of output to the public sector, there will be very little change in buyer Herfindahls, simply because index numbers tend not to change much when weights don't differ a lot; b) there may be a few outliers, dominating the rest, in industries which did happen to alter appreciably the proportion of sales to the public sector. These outliers will be responsible for the size and significance of the observed coefficient on HBUY, which thus contains no information on the importance of consumer or intermediate purchaser buyer power.

These conjectures appear to be supported by Waterson's Table A1 (1976, p. 231), giving his data. Thirty-four of the fifty-one Bilateral Power Indices changed by less than 5 per cent, and nineteen of these by less than 1 per cent. All the fifteen industries which one might classify as consumer goods industries (say, at least 40 per cent of sales to consumers) had indices which changed by less than 5 per cent; twelve of these less than 1 per cent. A few industries -- Other mining, Coke Ovens, Engineers' Small Tools -- showed changes of thirty or forty per cent, and it is these industries which were almost certainly responsible for the 'significance' of the buyer power coefficient.

To summarize this survey of the Warwick work: the study by Cowling and Kelly was a success. The data were averaged over a five-year period to remove cyclical noise, particular attention was paid to the specification of the effects of advertising on profits, and a new data source was used to provide data for individual companies. These were all in the food industry, and there are reasons why the results achieved looking at this sector alone may not generalize across all manufacturing. A study using a broader cross-section of company data would be of great interest.

The studies by Cowling and Waterson and by Waterson represent a considerable methodological advance on the five papers surveyed in this first section of this chapter.



Regression equations are developed explicitly from a tight theoretical model. However, in neither case (given the probable spuriousness of Waterson's buyer power coefficient) does the regression testing amount to much more than a simple correlation between changes in margins and changes in concentration, which cannot be regarded as conclusive, given my own and others' finding that this correlation disappears when other variables, consistent with other models, are added.

IV. Extensions and Implications

IV.1 The Dimensions of Industrial Performance  
in the Long Run: Salter Extended

Thus far, the analysis of the Thesis has been static -- we have looked for, and apparently found, a statistically stable relationship between the level of profitability and the level of variables measuring market power for the sample of fifty-one industries observed from 1954 to 1973.

The ability of the data to establish such a relationship is perhaps all the more impressive when we note that the stability of the structure-profits link holds alongside a pattern of marked changes in the distribution between the sample industries of output, employment, productivity, and relative prices and wages. Table IV.1 shows the mean, standard deviation, and other statistics of the ratios, industry by industry, of the 1973 to the 1958 values taken by these variables. (We will not use 1954 as a starting year for the dynamic analysis because of the possibility that our adjustment to the different treatment in the 1954 Census of Merchantable Goods is not very accurate. While errors in this should not have mattered too much to the structure-performance regressions, in which the 1954 numbers were averaged with four other years, they could affect calculations of rates of change using data for just two years).

Table IV.1  
Means and Standard Deviations of Variables

Ratio 1973/1958 of	Mean	S.D.	S.D/Mean
Real Gross Output	2.02	1.02	0.51
Total Employment	1.03	0.31	0.30
Real Gross Output/Total Employment	1.92	0.56	0.29
Earnings/Employee	3.01	0.24	0.08
Earnings/Real Gross Output	1.66	0.37	0.22
Materials/Real Gross Output	1.59	0.37	0.23
(Earnings + Materials)/Real Gross Output	1.48	0.28	0.19
Unit Price	1.70	0.32	0.19

The table shows that, on average, all these variables grew between 1958 and 1973, and that there was a good deal of variation in the rates of change for individual industries. Real output doubled, on average, over the fifteen years, and did so with almost no assistance from increased labour input. Labour productivity, thus, about doubled, too. Earnings per employee showed by far the biggest tendency to increase of all the variables in the table, and did so at the most uniform rate -- the ratio of dispersion (measured by the standard deviation of the 1973-58 ratios) to mean is much smaller than for any of the other variables. Unit labour and material costs, and their sum, unit variable costs, showed quite similar means and standard deviations, and their growth was

more than matched, on average, by growth in unit prices, as measured here. Data and precise definitions of all variables are given in Appendix C.

Patterns of industrial growth are of a great deal of interest and importance. To study them, in this chapter, I will, in essence, follow the analysis of Salter (1966). Salter's book, Productivity and Technical Change, is thought of as a 'classic', but, as is often the case with classics, the profession, having awarded the honour, seems thereby to feel itself absolved from the duty of actually reading the work. At any rate, Salter's results are not widely known, and his methodology of examining movements in cross-sections of individual industries remains 'unfashionable' (p. 2) as it was when he wrote in 1959 -- the great bulk of what we think we know about inflation, employment, and growth still comes from the analysis of time-series of aggregated data.

This is a pity. The results of applying Salter's method are striking, both in themselves, and in the conflicts they often reveal with the results from orthodox time-series models. I will not, in this Thesis, attempt to resolve all these conflicts, and, indeed, as an advocate of the Salterian approach, do not feel that the onus is on me to do so. Cross-sections of rates of change, with their substantial variance as shown in Table IV.1, form a sturdier foundation for statistical inference than do quarterly or annual time series of economic aggregates, riddled as these data are with problems

of serial correlation, multicollinearity, measurement error, and fewness of observations. If so, then surely the duty falls on those who put up with these statistical pitfalls to explain away the discrepancies. If, as I expect, the conflicts cannot be resolved in favour of the traditional approach, then this should be modified. A useful synthesis may be found in the method of pooling of time-series and cross-sections of data. I have made an attempt to apply a pooling methodology in specifying employment functions for a cross-section of Canadian Food and Beverage manufacturing industries (1978b).

Salter correlated interindustry rates of change with a sample of 29 UK industries over the period 1928-50. He was able to replicate his results for a sample of US industries, as was Reddaway, in an Addendum to the 1966 edition which analysed post-war (1954-63) data. There is thus some overlap between Reddaway's sample and the 1958-73 data made use of here. Our results should still be of interest, however, since, as well as lengthening the period of study by another decade, we extend the Salter method in the following directions:

- 1) Towards more complete coverage. Given data on the value of output, we can calculate real output if we know price, or vice versa. Price series were not available to Salter, so he made use of the real output indices from

the Census of Production. Availability of these output indices was largely responsible (cf. p. 104, fn1) for limiting his sample to 29 industries covering 'approximately 30 per cent of the industrial sector' (p. 188). By using, instead, the wholesale price indices (and so calculating real output as value deflated by price) available for the post-war period, I was able to include 51 of the 57 manufacturing industries (as disaggregated in the 1963 Input-Output tables) in the sample. Of course, it would be best to have both price and quantity measured independently, since there are statistical problems, noted below, that can arise with the use of constructed variables. This is not possible for a wide coverage of UK industries.

- 2) By extending the regression analysis; in particular by running multivariate regressions. Probably because of the primitive computing facilities available to him, Salter restricted himself to simple correlations and regressions with just one explanatory variable.
- 3) By asking if market structure can explain any of the variation in cross-sectional rates of change.

- 4) We introduce capital as a factor of production in the analysis.

The results can be divided under three headings; those pertaining to the product market, to the labour market, and to productivity and growth. We consider these in turn, then examine the sensitivity of the conclusions to splitting up the 1958-73 period into three five-year sub-periods.

1. Product Market (Pricing)

We will first establish a model of price changes as a framework for the analysis of Salter's results and of our own data. Suppose that price,  $P_i$ , per unit of the  $i$ th industry's output is a function,  $f_i$ , of the cost  $C_{ji}/X_i$  of each factor input  $j$  per unit of output. Thus  $C_{ji}$  is the total expenditure on input  $j$  in industry  $i$ , and  $X_i$  is the quantity of  $i$ 's output. That is,

$$P_i = f_i(C_{1i}/X_i, C_{2i}/X_i, \dots) \quad (\text{IV.1})$$

On the basis of the results in earlier chapters, we expect  $f_i$  to itself be a function of market structure variables as well as, perhaps, product demand conditions. To get something that can easily be estimated, we will restrict (IV.1) to be linear:

$$P_i = a_{1i}(C_{1i}/X_i) + a_{2i}(C_{2i}/X_i) + \dots \quad (\text{IV.2}),$$

where the  $a_{ji}$  are multiplicative markups applied to each input. Differentiating with respect to time,  $t$ ;

$$dP_i/dt = da_{1i}/dt \cdot (C_{1i}/X_i) + a_{1i} \cdot d(C_{1i}/X_i)/dt + \dots (\text{IV.3})$$

- 4) We introduce capital as a factor of production in the analysis.

The results can be divided under three headings; those pertaining to the product market, to the labour market, and to productivity and growth. We consider these in turn, then examine the sensitivity of the conclusions to splitting up the 1958-73 period into three five-year sub-periods.

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$$P_i = f_i(C_{1i}/X_i, C_{2i}/X_i, \dots) \quad (\text{IV.1})$$

On the basis of the results in earlier chapters, we expect  $f_i$  to itself be a function of market structure variables as well as, perhaps, product demand conditions. To get something that can easily be estimated, we will restrict (IV.1) to be linear:

$$P_i = a_{1i}(C_{1i}/X_i) + a_{2i}(C_{2i}/X_i) + \dots \quad (\text{IV.2}),$$

where the  $a_{ji}$  are multiplicative markups applied to each input. Differentiating with respect to time,  $t$ ;

$$dP_i/dt = da_{1i}/dt \cdot (C_{1i}/X_i) + a_{1i} \cdot d(C_{1i}/X_i)/dt + \dots (\text{IV.3})$$



It will be assumed that the  $a_{ji}$  do not change over time. This should not be too unreasonable, given the stability found earlier in the parameters of the structure profitability relationship over the sample period. (IV.3) then simplifies to

$$\begin{aligned} dP_i/dt &= a_{1i} \cdot d(C_{1i}/X_i)/dt + a_{2i} \cdot d(C_{2i}/X_i)/dt \\ &+ \dots \end{aligned} \quad (IV.4)$$

To get rates of change, we divide through by  $P_i$ ;

$$\begin{aligned} (dP_i/dt)/P_i &= (a_{1i} \cdot d(C_{1i}/X_i)/dt)/P_i \\ &+ (a_{2i} \cdot d(C_{2i}/X_i)/dt)/P_i + \dots \end{aligned} \quad (IV.5)$$

or,

$$P_i = a_{1i} \alpha_{1i} (c_{1i} - x_i) + a_{2i} \alpha_{2i} (c_{2i} - x_i) + \dots \quad (IV.6),$$

where, for simplicity, we use lower case letters for rates of change, and where  $\alpha_{ji}$  is the proportion unit costs of input  $j$  are of the unit price (i.e.,  $\alpha_{ji} = (C_{ji}/X_i)/P_i$ ). If we know the quantity,  $x_{ji}$ , of an input  $j$ , we can expand (IV.6) using

$$c_{ji} - x_i = (c_{ji} - x_{ji}) + (x_{ji} - x_i) \quad (IV.7)$$

This is only possible for the labour input, for which we have a reasonable proxy in the level of employment (differences in hours per worker will not count for much compared with changes in the number employed over the long

periods here considered). For labour, then, we can separate the change in unit labour costs into the change in the wage per employee and the change in the number of employees per unit of output.

Looked at within the framework of equation (IV.6), Salter's empirical procedure of calculating simple correlations or regressions between pairs of variables seems rather crude. It risks (and, indeed, suffers from—cf. pp. 119-120) omitted variable bias of the coefficient of the included variable, assumes that the coefficient of this variable is the same across industries, and introduces a source of error by leaving out the weighting parameter  $\alpha$ . Despite all this, Salter's correlation analysis (p. 110) yields some striking results which we can replicate for the 1958-73 sample, and which will turn out to be robust when more sophisticated specifications are tested. The full set of correlations for 1958-73 rates of change across the 51 industries is shown in Table IV.2

The variables correlated in Table IV.2 are the rates of change of real output ( $x$ ), employment ( $e$ ), real output per employee ( $x-e$ ), wage per employee ( $w-e$ ), wage per unit of output, or unit labour cost ( $w-x$ ), unit materials cost ( $m-x$ ), unit labour + materials cost ( $c_v-x$ ), unit capital cost ( $k-x$ ), unit total (labour + materials + capital) costs ( $c_t-x$ ), and price ( $p$ ). Values, definitions, and sources of

all variables are given in Appendix C. Significant values for the first order correlation coefficient,  $r$ , are  $\pm 0.275$  at the 95 per cent level, and  $\pm 0.233$  at the 90 per cent level.

Table IV.2

Correlation Coefficients between 1958-73 Rates of Change

	e	x - e	w - e	w - x	m - x	$c_v - x$	k - x	$c_t - x$	P
x	0.751	0.826	0.065	-0.732	-0.482	-0.590	-0.503	-0.628	-0.623
e		0.276	-0.179	-0.329	-0.156	-0.236	-0.270	-0.244	-0.294
x - e			0.221	-0.857	-0.550	-0.672	-0.538	-0.717	-0.677
w - e				0.181	-0.161	-0.088	0.094	-0.081	-0.072
w - x					0.437	0.625	0.575	0.656	0.612
m - x						0.950	0.390	0.932	0.903
$c_v - x$							0.477	0.977	0.938
k - x								0.619	0.582
$c_t - x$									0.959

In this section we are interested in the correlations between price changes,  $p$ , and the other variables. We can note two observations:

1. There is no correlation between price and wage rate changes;
2. There is a strong correlation between price changes and changes in each of the unit cost variables.

These findings are similar to Salter's<sup>18</sup>. The lack of correlation between prices and wages might be taken to contradict the pervasive notion of a 'wage-price spiral' whereby changes in an industry's wage rates cause it to put up its prices. However, it could be true that, although prices are increased when wages increase, the rate of wage increase is so similar across industries that, statistically, this variable has no power to explain differences in price performance. In support of this, recall that in Table IV.1 the dispersion of  $w$  is less than that of all the other variables, including prices. Still, there is some dispersion in rates of wage rate changes, and it may therefore be surprising, if one did believe in wage-markup pricing at the industry level, to find absolutely no evidence of positive correlation. The evidence certainly must be interpreted as giving no support to the hypothesis that industry-level labour market conditions have anything to do with price inflation. In particular, the 'structuralist' interpretation of the Phillip's curve, originating with Lipsey (1960), and featured since in much of the time-series analysis of inflation, which postulates that, in the presence of downward money wage rigidity, uneven dispersion of the demand for labour leads to increases in wages and then in prices, is inconsistent with the independence of industry wage and price variations. We will have more to say on this in the section on the Labour Market.

The correlations between price changes and unit costs reveal the expected association between factor productivity and product price. Physical labour productivity does about as well as unit labour costs, which is not surprising, since the difference between them (cf. equation IV.7) is just the wage rate, which we have found to be uncorrelated with prices. Unit materials costs and prices are even more strongly related statistically -- we do not have the materials price data that would enable us to distinguish between the price and physical productivity components of materials costs, and discover whether materials prices (which surely varied much more industry by industry than did the wage rate) have an independent effect, in contrast to the price of labour. The two input costs added together show the closest correlation of all with price.

Thus, we are easily able to replicate Salter's finding that price changes are quite closely matched with unit cost changes, along with his qualification that, for labour at least, it is the physical productivity rather than the price of the input that matters. The next interesting question concerns the magnitude of the price-unit cost relationship. To answer this, we run a number of regression specifications of the price change equation IV.6. These are shown in Table IV.3.

TABLE IV.3 RATE OF PRICE CHANGE REGRESSIONS, 1958-73

Variable	Constant	(w-e)	$\alpha_1$ (w-e)	(x-e)	(m-x)	(w-x)	$\alpha_m$ (m-x)	$\alpha_1$ (w-x)	$\alpha_{vc}$ (c <sub>v</sub> -x)	S	Z	Z $\alpha_{tc}$ (c-x)	Z.S	$\alpha_k$ (k-x)	$\alpha_{tc}$ (c <sub>t</sub> -x)	$\bar{R}^2$
Mean	1.000	2.004	0.543	0.931	0.597	0.650	0.356	0.132	0.482	2.300	1.600	0.727	3.679	0.036	0.508	
1	0.875 (2.26)	-0.083 (-0.43)														-0.018
2	0.803 (5.87)		-0.172 (-0.73)													-0.010
3	1.083 (16.61)			-0.402 (-6.73)												0.485
4	0.457 (6.82)			-0.149 (-3.81)	0.655 (10.90)											0.855
5	0.153 (4.05)				0.672 (12.64)	0.238 (4.49)										0.868
6	0.163 (4.75)						1.032 (15.76)	1.358 (6.46)								0.892
7	0.187 (5.90)								1.083 (19.19)							0.887
8	0.179 (6.35)								1.022 (19.27)					1.056 (3.63)		0.910
9	0.193 (6.83)														1.015 (21.26)	0.906
10	0.192 (3.74)								1.082 (18.49)	-0.002 (-0.13)						0.884
11	0.189 (1.89)								1.082 (16.79)	-0.002 (-0.12)	0.002 (0.03)					0.881
12	0.189 (3.40)								0.965 (5.54)	0.018 (0.54)		0.075 (0.67)	-0.011 (-0.63)			0.880

- 204 -

The first of these regressions reaffirms the absence of correlation between  $p$  and  $(w-e)$ . Weighting wage rate changes with labour share in total output, as required by equation IV.6, does not alter the result. Regression 3 illustrates the correlation between price and labour productivity change. In regressions 4 and 5, we go beyond Salter's limit of one independent variable and pair changes in unit materials costs first with labour productivity, then with unit labour costs. All variables are comfortably statistically significant. The fit is improved further when each input is weighted by its share  $\alpha$  in total value, in regression 6. With this specification we have estimates of two of the markup parameters  $a_{ji}$  of equation IV.6. Although the markup on materials is close to one, the wage markup is not. The difference is significant enough to cause the corrected goodness of fit to fall a few points when equal markups are imposed by regressing price change on the change in total variable costs (regression 7). However, these coefficients may be biased by the omission of the third factor of production -- capital. When the rate of change of fixed plus inventory capital multiplied by the 'normal returns' factor settled on in Chapter III.1 (cf. Appendix C for a full definition) is added to the specification, its coefficient, and that on variable costs, are both insignificantly different from one (regression 8). Again, though, the  $\bar{R}^2$  falls slightly when equality of coefficients is imposed (regression 9).

The size of the coefficient on the capital variable gives an independent check on the validity of the normal returns concept of Part III -- the significance and closeness to one of this coefficient are therefore encouraging, since we would expect that if measured normal returns are indeed as they were defined in principle -- the opportunity cost of capital committed to production -- a change in the quantity of capital committed, weighted by the normal rate of return, should be matched by a similar change in revenues earned.<sup>19</sup>

In regression 10, we look for an effect of the rate of growth of sales revenue on price change. There seems to be none, which is not surprising, since (as was noted in Part III), a change in sales must be due to a shift in the demand curve that is not matched by a shift in the supply schedule if it is to affect price (and thus profits), and such a disequilibrium situation is hardly likely to persist, in even the most sluggish of industries, for fifteen years. More evidence that the change in actual sales, over the long run considered here, is dominated by supply conditions, is given in the much greater variability shown in Table IV.1 by changes in real output compared to changes in price. This is further discussed below in the section on productivity and growth.

Next, a variable for the level of market power, measured by the profitability predicted for each industry by specification number 12 in Table IV.3, according to its market structure characteristics, is added and fails to show any



significance (regression 11). Finally, in regression 12, sales growth and market power are introduced multiplicatively. This is theoretically tidier, since it is equivalent to making the  $a_{ji}$  in IV.6 functions of these variables, which makes more sense than having them included in an ad hoc fashion in a linear regression. Despite this, sales growth and market power retain their statistical insignificance.

The absence of a market power effect on the rate of change of prices implies that none of the market power-profitability relationship can be attributed to a widening of price-cost margins in the more powerful industries relative to the others over the 1958-73 period. Although this result means that we are unable, in this long-term analysis, to throw any light on the inflation mechanism, it also provides a test of the likely stability, outside the observation period, of the power-profits relation -- since a positive relationship between the level of market power and the rate of change of price relative to cost cannot persist indefinitely (or else all profits would eventually accrue to just one firm -- the most powerful), we would, had we found such a relation, have had to interpret it as a special feature of the 1958-73 data generating some monopoly profits which could not be expected to persist outside the period, and, indeed, might even be compensated for by a following period of especially low returns to market power.

In all the regressions of Table IV.3, the constant term is significant. This is worrying, since there is no intercept in equation IV.6. This constant may be summarizing the effect on price changes of variables omitted from the regression specifications -- possibilities are that the markup parameters have increased over the sample period, and that labour, materials and capital are not the only relevant productive inputs. Another plausible explanation is that the constant term is generated by an index number problem. Our price variable is a Laspeyre index number; it measures the change in the value of the base-period bundle of commodity output in each industry. Such an index is an upwardly-biased measure of price change so long as there is some substitutability possible between the components of the industry's output bundle of goods, since it weights too heavily those commodities whose price has increased the most, by ruling out substitution for them of relatively lower priced commodities. Salter (pp. 151-52) demonstrates, for productivity change indices, the magnitude of index number bias when rates of change are taken over a long period of time; in view of his figures, it may even be reasonable to attribute all of the 18 per cent or so upward drift in the price cost margin implied by the regression equations in Table IV.3 to this source of measurement error.

In summary, the results of replicating and extending Salter's analysis of price changes to a 1958-73 sample are 1) industry wage rates have no effect on prices, 2) physical labour productivity and unit materials and capital costs explain most of the variation in relative price changes, and imply constant price-cost markups, 3) there is no evidence that demand disequilibrium can persist over the fifteen-year period, 4) there is no evidence that the level of market power affects the rate of change of price-cost margins, 5) there remains a significant autonomous upward shift in the price-cost margin, which may be due to omitted variables, and/or to the index number problem.

## 2. The Labour Market (Employment, Wages, and Inflation)

Of of the most interesting of Salter's results is the absence of any significant statistical correlation between changes in earnings per employee and output per employee -- productivity. This holds for both his UK and US samples (pp. 110, 166, 167, footnote 2), and for Reddaway's extension to post-war UK data (p. 202).

For the 1958-73 sample of this Thesis, we find a correlation (Table IV.2) coefficient  $r = 0.107$ , which is not significant at the 95 per cent probability level (the critical value for a sample of 51 observations is  $r = 0.276$ ).

Salter was 'heartened' (p. 157) by his failure to find a correlation for two reasons. First, it suggested that there is no 'tendency for productivity gains to be appropriated

at their source by strong trade unions' so that the lucky workers receive a pay differential that is not matched by intrinsic differences in labour effort or ability (p. 157). Secondly, if some of productivity gains were captured in higher wage rates, 'the interindustry structure of costs and prices would be less responsive to unequal productivity movements. This would seriously inhibit the structural changes which make such an important contribution to increases in aggregate productivity' (ibid.). That is, monopoly power in labour markets would reduce the economy's rate of growth. Salter's remarks seem still pertinent to the 1958-73 experience.

A second correlation coefficient in Table IV.2 that is relevant to the labour market is between changes in earnings per employee and in the level of employment. The r-value is negative and not significant. For Salter's 1924-50 sample, the equivalent correlation coefficient is -0.270 (not shown by Salter, but calculable from his data as presented in Table 14, p. 107). The negative sign could be spurious, due to the appearance of employment in the numerator of one variable and the denominator of the other; the interesting point, though, is that there is no trace of a positive correlation -- that is, for relatively large increases in employment to be associated with relatively high rates of earnings increase. Reddaway (1959), looking at 1951-56 changes, found evidence of positive correlation between, but not within II-digit industry groups.

These results imply (at least if industries tend to draw their labour force from the same 'pool' as do other industries within the same II-digit Order) that expanding industries do not need to bid up their relative wage in order to attract labour away from other industries. The very considerable amount of employment redistribution that went on over the 1958-73 period (cf. Table IV.1) bore no observable relationship to changes in the wage structure (which, as we have already noted, are relatively small in magnitude, on average).

Thus, the 'structuralist' rationalization for the Phillips curve is dealt another blow -- whether or not there is downward money-wage rigidity, the re-allocation of the labour force appears to be accomplished without the use of relative wage changes (presumably through such instruments as advertising and by simply accepting a larger proportion of the flow of job applicants that firms receive whether or not they notify vacancies).

This finding, and our finding in Section 1 that industry wage and price changes are not related, appear to thoroughly discredit the model -- one might almost say the 'paradigm' given the pervasiveness of the doctrine -- that has sought to link aggregate demand with inflation by means of a detour through the labour market -- an increase in demand in product markets translates to an increase in the demand for labour, which cannot be effected without an increase in wage

rates, which, in turn, forces firms to put up their prices. Two of the three links in this chain of reasoning do not exist, according to the correlations of Table IV.2

I conjecture that the key to constructing a valid theory of price inflation may be found by questioning, too, the first of the links in the traditional model. Increased demand by customers for goods may not be passed on, undiluted, into increased demand by firms for factor inputs, such as labour (that is, the desire to increase real purchases may not be perfectly matched by sellers' willingness to increase real output). Rational firms will prefer to translate growth in nominal demand into an increase in price, not output, since by so doing they will make higher profits. Their ability and willingness to do this will depend on their expectations of future market conditions, and of the reactions to the situation of other firms. These expectations may well depend, inter alia, on market structure.

Recent time series studies of US aggregates (Gordon, 1975, Maccini, 1978) have found evidence of product market demand and expectations factors determining changes in prices. The failure of many earlier studies to find such a link may simply be attributable to a lack of the will to succeed -- it is easy enough to mis-specify a relationship, and to readily accept the resulting negative results, if one's heart is not in

the job, because one does not believe the underlying hypothesis to be true. Possible sources of mis-specification suggested by Maccini are failure to model properly the lag structure (which may be very important in models using quarterly data) and the use of poor proxies for demand variables. For example, the well-known study by Godley and Nordhaus (1972), of UK manufacturing price changes, tests one hundred alternative specifications of models purporting to include 'demand' factors, finds almost no evidence of statistical significance, and so concludes that demand does not affect price. Since, however, none of their proxies necessarily measures demand pressure (cf. Appendix A of this Thesis), the absence of significance of these variables does not at all contradict the hypothesis they claim to test (rather, it gives some support to my arguments above that the relationship between demand and prices does not go through the labour market). Indeed, the failure of their own 'normal cost pricing' model to explain much more than a third of the variation in price changes (p. 869) hardly justifies Godley and Nordhaus' conclusion, tentative though it is, that the 'normal' price hypothesis is correct (p. 873).

The question of the relationship, if any, between market structure and price changes has been at issue since Means introduced the concept of 'administered prices' in the 1930s (cf. Sherer, 1970, Chapter 12). Even the recent evidence does not lead to a consensus position on this. Domberger (1977),

using quarterly data on twenty-one UK manufacturing industries, found that more concentrated industries were relatively quicker to pass on unit cost increases. On the other hand, Ripley and Segal (1973), who used a database of 1959-69 rates of change of prices and other variables for 395 US manufacturing industries, discovered that 'high' concentration industries had passed on relatively less of the change in their unit labour costs, though there was no difference in their response to materials costs, compared with less concentrated industries.<sup>20</sup> Given the difficulties, noted above in the discussion of the absence of a significant market power effect in the regressions of Table IV.3, of interpreting an equation in which the level of one variable has a permanent effect on the rate of change of another, and given the assymetry of their results (concentration affecting price only through labour costs), I do not think that Ripley and Segal's finding can be taken very seriously as other than a peculiar correlation holding over their particular sample period. Since price adjustments take place in the short run of years or quarters, they should be analysed using yearly or quarterly data. The interesting question is what determines the split of short run changes in nominal demand for goods and services into changes in real output and changes in prices.

Thus, we recommend Domberger's methodology of comparing the coefficients estimated for a number of industries by separate time-series regressions, or, better still, the explicit combination of time-series and cross-sectional hypotheses



in a 'pooled' database (e.g., Hazledine, 1978a; for an application to employment functions).

The model here suggested, in which inflation is a product market phenomenon, would include two 'spin-off' equations to explain employment and wage changes. Levels of employment are determined through an employment function by real output (cf. Hazledine, 1978c, for employment functions for UK manufacturing industries at the II-digit level), and wage changes follow changes in actual and expected prices through cost-of-living adjustments in the wage-bargaining process. Thus wages and employment are determined by the same product market factors, so that we should not be surprised to find a statistical correlation between them, as in the many Phillips curve regression studies. However, this association may change when conditions in product markets change, so we should also not be surprised at the notorious instability of estimated Phillips curves,<sup>21</sup> and, of course, even in periods of stability, we should not interpret the correlation as evidence for a causal relationship running from (un)employment to wage inflation.

### 3. Productivity and Growth

In the long run, what determines the physical well-being of an economy is the efficiency with which it utilizes its primary factors -- labour and natural resources. Although goods are produced with the aid of capital and intermediate materials as well as labour and natural resources, capital goods and

materials are themselves produced -- ultimately the value of all output can be accounted for by the present and past labour and natural resources embodied in it.

Therefore, it is of interest to examine and attempt to explain the variations in changes in labour productivity between industries over a long time period (we have no data that would allow us to also study natural resource-use productivity). Salter correlated differences between industry in labour productivity (real gross output per employee) growth and a number of other variables, and used the results to evaluate four possible explanations for these differences:

- (i) Salter rejected the hypothesis that differential increases in labour productivity can be explained by differential increases in the personal efficiency (skill, effort, intelligence) of labour, on the grounds a) that the magnitude of the differences is too great to plausibly be attributed to efficiency change differences, b) that one would expect increases in personal efficiency to be rewarded by increases in earnings, so that, if the hypothesis were true, earnings growth and productivity growth should be correlated, but in fact were not so in Salter's sample, and c) that 'there is no reason of substance' (p. 129) to expect unit materials costs to be affected by changes in

personal efficiency to the extent implied by the high correlation between changes in material costs and in labour productivity.

For our 1958-73 sample, the variations in changes in labour productivity (Table IV.1) do seem too large to be attributed all to changes in personal efficiency, especially since we find no correlation between productivity and earnings changes, which might be expected to accompany changes in personal efficiency.

I do not find particularly convincing Salter's third objection to the personal efficiency hypothesis. It seems quite plausible to me for a good deal of the productivity improvements that would follow an upgrading of the skill level of a workforce to be realized through greater efficiency in the use of material inputs.

(ii) Salter considered the 'classical prescription' (p. 130) for increased labour productivity -- capital-labour substitution. Since factor substitution involves the substitution of one expense for another we would not thereby expect much change in total costs, and, thus, in price, when factor proportions alter. However, in both Salter's samples, and for the 1958-73 data, there is a highly significant negative correlation between changes in labour productivity and changes in product price. Of

course, this does not entirely rule out differences in factor substitution contributing to differences in productivity changes, but we can probably agree with Salter that the results 'give little support to the idea that increases in labour productivity are largely attributable to factor substitution' (p. 132). The correlation coefficient between changes in unit labour and unit capital costs (Table IV.2) over 1958-73 is actually positive -- industries with the best labour cost performance tended also to do better with capital expenses, whereas a negative association would be expected if savings in one factor were paid for by more intensive use of the other.

Of course, across the manufacturing sector as a whole there has been capital-labour substitution, in the sense that the capital/labour ratio has risen -- while total employment in manufacturing grew by only 3 per cent over the fifteen years, the real capital stock nearly doubled. What our correlations imply, however, is that differences across industries in the rate of capital accumulation do not explain the observed differences in labour productivity. This is probably because industries have faced roughly similar changes in relative factor prices -- Table IV.1 showed the relatively small

dispersion in changes in earnings, and it seems reasonable to expect that changes in interest rates and other ingredients of the cost of capital move together across industries -- so that their responses in terms of factor substitution have also tended not to differ.

Therefore, the evidence for the 1958-73 period, which augments Salter's empirical analysis by including a measure of the change in the capital stock, appears to support his conclusion that:

"Speaking loosely, the increases in labour productivity appear to have been costless; they have not been achieved by bribing labour to greater effort, nor by saving labour at the expense of using more of other factors. This suggests that any satisfactory explanation must be one where savings in labour and labour costs are part of a wider process that extends to all factors of production. Two causes of increased productivity meet this requirement: economies of scale and improved techniques arising out of increasing knowledge" (pp. 132-33).

We look next at each of these two suggested explanations.

(iii) Economies of Scale

One of the strongest correlations, in all the samples, is between the growth of productivity and of total output. For 1958-73, the correlation is 0.826. What is the reason for this close association? One possibility is that causation runs from

output growth to productivity growth through the phenomenon of economies of scale -- increased scale of production leads, ipso facto, to lower costs. The many possible sources of scale economies are well-surveyed by Pratten 1971, Chapters 2 and 3), and are, a priori, plausible enough, but it must be doubted whether they can bear all the burden of explaining the output productivity correlation. A simple regression of output growth on productivity growth gives

$$(x - e) = \begin{matrix} 0.455 + & 0.454x, & \bar{R}^2 = 0.676 & \text{(IV.8)} \\ (7.12) & (10.26) & & \end{matrix}$$

(The rate of change of productivity is calculated directly from X/E's computed for 1958 and 1973, rather than by simply subtracting the rate of growth of employment from that of output, to reduce the risk of spurious correlation). Regression (IV.8) implies that a one hundred per cent increase (i.e., a doubling) in an industry's output has been associated with nearly a fifty per cent increase in the productivity of its labour force. This seems too high an elasticity to be attributed all to economies of scale. For the twenty-one of our industries for which Pratten computes the number, the average decrease in unit costs yielded by a doubling of plant size to 'minimum efficient scale' from half that volume of

output is about 7½ per cent.<sup>22</sup> There are problems with this comparison -- the capital/labour ratio may have increased with output and so accounted for some of the productivity increase, and Pratten's estimates are for the growth in individual plant, not industry, scale.<sup>23</sup> We can make a more satisfactory direct test by regressing the change in total unit costs on the change in plant size (net output per establishment):

$$c_t - x = \begin{matrix} 0.825 & - & 0.204 & \% \Delta \text{RNO/EST}, & \bar{R}^2 = 0.447 \\ (15.51) & & (-6.44) & & \end{matrix} \quad (\text{IV.9})$$

Regression (IV.9) implies that a doubling in plant size has been associated with a 20 per cent fall in unit costs -- a substantial figure, but probably not big enough to account for all the increase of 50 per cent in per capita output.

Another interesting test of the nature of scale economies is to estimate a production function. I calculated the average for each industry, for the four census years 1958, 1963, 1968, 1973, real net output per establishment, total employment per establishment, and the capital proxy, real gross fixed capital formation per establishment, and estimated a Cobb-Douglas specification.

$$\begin{aligned} \log(\text{RNO/EST}) = & \begin{matrix} 1.444 & + & 0.575 \log(\text{EMP/EST}) \\ (12.63) & & (7.26) \end{matrix} & (\text{IV.10}) \\ & + \begin{matrix} 0.443 \log(\text{RGCF/EST}) \\ (8.24) \end{matrix} & \bar{R}^2 = 0.954 \end{aligned}$$

This regression is successful statistically -- t-values and overall goodness of fit are all high. Its most striking feature is that the sum of the employment and capital coefficients just about exactly equals one -- there is no suggestion of interindustry economies of scale. That is, industries with bigger plants, on average, do not get more output from a given bundle of inputs.

Of course, this finding does not rule out intra-industry scale economies of the sort implied by regressing productivity growth on output growth, but strongly suggests that, if such relationships do exist, they are not stable across industries. That is, even if it is true for each of two industries, one with small, the other with large establishments, that they would lower unit costs by increasing the size of their plants, it is not, in general, true, according to (IV.10), that the industry with smaller plants will have higher unit costs than the other.

This has implications for the structure-performance model estimated in Chapter III.2, wherein it was found that plant size has a positive influence on profitability. In that chapter it was left open whether this association was due to plant size being a market power proxy or because of economies of scale. We see now that the evidence does not support the economies of scale interpretation.<sup>24</sup>



(iv) If economies of scale are unable to account for all of the correlation between productivity and output growth, it may be that there is some causation running in the other direction, from costs to output, through the process of technical change. That is, inventions, innovations, and development lower unit costs, which, as we know from the analysis of pricing in section (1), leads to lower prices, which, in turn, encourages sales, over the long run.

To fit this hypothesis, we would replace (IV.8) with

$$x = \begin{matrix} -0.360 + & 1.504(x-e), & \bar{R}^2 = 0.676 & (IV.11) \\ (-2.28) & (10.26) & & \end{matrix}$$

the regression of output on productivity growth. Using our earlier result that cost changes are fully passed on in price changes, (IV.11) implies a long-run price elasticity of about 1.5, on average, which is a reasonable sort of number.<sup>25</sup>

Salter finds a rank correlation coefficient of 0.83 between movements of labour productivity in eleven comparable US and UK industries (1966, p. 165), which he takes to support the hypothesis that differences in productivity change within an economy are due to interindustry differences in the rate of technical advance, which 'might reasonably be expected to appear in both countries' (p. 166).<sup>26</sup> However, it could also be true that differential rates of growth in demand could be

correlated across the two countries, so that economies of scale could be responsible for Salter's correlation.

Thus, we should probably restrict ourselves to Salter's earlier (Chapter 10) agnostic conclusion that both economies of scale and technical change may matter, and that their effects cannot be neatly unravelled with the data at hand. This is a pity, since the two hypotheses differ both in the direction of causation they imply in the relationship between output and cost changes, and in what they take as exogenous -- demand shifts and/or propensity to scale economies in one, and susceptibility to technical improvements in the other. Sorting them out might best be done through the case-study approach, using data collected on cost curves (such as Pratten's), elasticities, and technical innovations for individual industries.

Such studies might indeed find that scale economies and technical change are related. There may often be 'bias' to technical change -- the search for new techniques may be concentrated in areas where their shadow price is highest. In particular, it may be true that 'a great deal of process improvement goes towards increasing the optimal size of machinery' (Nordhaus, 1969, p. 19; cf. also Levin, 1977). That is, that research and development is biased towards generating economies of scale. The economic rationale for this could be that although firms wish to grow for market

power reasons (to charge higher prices), the rewards from so doing can be dissipated by loss of control over costs when the number of plants owned by a firm increases. Thus, expanding firms will wish to 'rationalize' their production into larger-scale plants, and will guide the search for techniques towards this end.

To introduce the possibility of technical change being directed towards certain problems is to imply that there is a choice problem -- that the resources that produce technical advances have alternative uses, so that these advances are not 'costless' as Salter thought. (If there were no opportunity costs, there would be no need to 'direct' technical change -- it could proceed in all directions at once).

If there are costs to achieving lower unit costs, we would expect relatively more such activity to be found, in industries in which either the returns to research and development are highest, or in which the availability of funds to undertake R and D is greatest. These may explain the following regression result:

$$(c_t - x) = 1.053 - 0.301 \text{SURP}^* \quad \bar{R}^2 = 0.116 \quad (\text{IV.12})$$

(5.66)    (-2.67),

the industries with the most market power tended to reduce their total unit costs compared with the others, possibly because the market power of firms in these industries either

a) allowed them more opportunity to enjoy the fruits of

technical advance (to prevent competitors from free-loading) or  
b) generated the profits needed to finance R and D programs.

Finally in this chapter, we will look quickly at the results of performing the correlation analysis for each of the three five-year sub-periods available from the Censuses of production from 1958 to 1973.<sup>27</sup> Reddaway noted that:

"On the whole, if the theory suggests a fairly high value of a correlation coefficient, then a rather lower value is to be expected over [a shorter rather than longer period], because chance and trade-cycle factors are more likely to 'spoil' the correlation. On the other hand, where theory suggests an absence of correlation (as with the movements in earnings per operative and output per operative) chance might produce a small correlation over a short period" (Salter, Addendum, p. 201).

In Tables IV.4, IV.5, and IV.6, we find that the important correlations that were found to be significant with the 1958-73 data -- between  $x$  and  $(x-e)$ ,  $x$  and  $p$ ,  $p$  and  $(x-e)$ ,  $p$  and  $(c-x)$  -- are slightly smaller, on the whole, over the five-year period. The signs of all the correlations do not change; nor do any lose their significance.

The two correlations -- between  $p$  and  $(w-e)$ , and  $e$  and  $(w-e)$  -- for which it was of interest that no significance turned up over the fifteen-year period, show no tendency in particular to increase their apparent degree of association over the shorter periods. However, the possibility of chance correlations over shorter periods noted by Reddaway did appear

to be realized when the price change regressions (cf. Table IV.3) for the 1958-73 specifications) were estimated for the five-yearly rate of change data. I do not show these regressions, since they take up space without being of any particular interest in themselves, but can report that, although the main feature of the long-period result -- namely; that unit cost changes were passed on in prices with no significant tendency for the percentage markup to alter -- also shows itself in each sub-period -- over the 1958-63 period the rate of change of sales showed a 'significant' association with price change, as did the market power variable (with a negative sign) for price changes between 1963 and 1968. Since neither of these variables showed any significance in the other periods, nor over the full fifteen years, it seems reasonable to conclude that their 'significance', where found, was spurious, due to chance patterns in the movements of the variables.

Thus, the analysis for the sub-periods, even though these cover a length of time -- five years -- which is long by the standards of quarterly and annual time series econometrics, suggests that it is in general worthwhile to make use of the widest temporal spread that one's database will permit, when estimating the coefficients of economic models.

Table IV.4

Correlation Coefficients between 1958-63 Rates of Change

	e	x - e	w - e	w - x	m - x	c - x	p
x	0.663	0.725	-0.008	-0.746	-0.377	-0.555	-0.380
e		-0.032	-0.327	-0.144	-0.030	-0.054	0.044
x - e			0.272	-0.875	-0.464	-0.685	-0.539
w - e				0.199	-0.232	-0.104	-0.069
w - x					0.338	0.640	0.525
m - x						0.914	0.791
c - x							0.853

Table IV.5

Correlation Coefficients between 1963-68 Rates of Change

	e	x - e	w - e	w - x	m - x	c - x	p
x	0.835	0.538	0.114	-0.523	-0.118	-0.165	-0.321
e		-0.004	-0.128	-0.034	0.027	0.011	-0.059
x - e			0.330	-0.936	-0.225	-0.296	-0.470
w - e				-0.016	0.085	0.103	-0.005
w - x					0.283	0.371	0.521
m - x						0.985	0.887
c - x							0.895

Table IV.6

Correlation Coefficients between 1968-73 Rates of Change

	e	x - e	w - e	w - x	m - x	c - x	p
x	0.799	0.778	0.029	-0.686	-0.328	-0.423	-0.501
e		0.252	-0.039	-0.243	-0.261	-0.295	-0.297
x - e			0.107	-0.856	-0.277	-0.393	-0.513
w - e				0.410	0.105	0.165	-0.006
w - x					0.281	0.423	0.450
m - x						0.977	0.575
c - x							0.606



#### IV.2 Some Implications for Policy and Welfare

In this final chapter of the Thesis, I will attempt to sketch out some implications for economic policy and welfare of my results. I will not here fully develop and document my proposals, since to do so properly would probably require at least another monograph. We will consider, in turn, implications for policy-making towards monopoly profits, changes in relative prices, inflation, unemployment, and productivity, and for welfare.

##### 1. Monopoly Profits

The results of Chapter III.2 do allow us to set aside as untrue and mischievous the laissez-faire proposition that no action against monopoly profits is needed because these are transitory phenomena, which are efficiently dealt with by unaided market forces. For the United Kingdom, at least, monopoly profits persist.

In the model in Part II, monopoly profits depend on the power of traders to impose on each other costs of not trading. One source of differences in this power is differences in the personal luck, ability, and energy of individual entrepreneurs and managers. To the extent that government considers the resulting distribution of income to be too extreme it may wish to implement some redistributory policy. Such a policy should work through the system of the personal income and wealth taxation, rather than through

industrial policy, since the latter discriminates between income earned in industry and in other activities.

In any case, my industry level data do not allow us to estimate the extent of variations in profitability due to inter-firm differences. What the empirical work of Part IV does do is support the proposition that a good deal of the interindustry differences in market power are generated by differences in the size of establishments, and in the structure of their customers' markets -- because size measures the amount of 'territory' a firm owns, and customers' market structure affects, in the case of intermediate buyers, the money they can make on reselling the good after further processing, and in the case of final buyers, the extent to which they can organize effectively in the bargaining process that determines price.

Since this sort of monopoly power has no moral justification, and since costs may be incurred (e.g., managerial diseconomies of scale, reduced product variety) to achieve it, which have, therefore, no compensating social return, industrial policy may, and has, been directed against it. In considering the different policies, we will follow the traditional structure-conduct-performance paradigm of industrial economics. Our examples of British policies come from Scherer (1970), and Pickering (1974), and so do not reflect the most recent developments.

(i) Structural Policies

A form that action against monopolies can take is to break them up. In 1948 the UK Monopolies Commission was established to investigate industries in which one firm controls a third or more of output, and in 1965, under the 'Monopolies and Mergers Act' was given the responsibility to review changes in structure, due to mergers, which would involve a third or more of industry output. (Note that, in focusing on firms' market share, the policy-makers were a step ahead of the oligopoly theorists, to whom only industry market structure is relevant). There are a number of problems with the structuralist approach:

- 1) A large market share does not necessarily imply abuse of monopoly power. As we noted above, large sales and profits can be won by excellence -- by producing a better product than one's competitors. To break up a firm which has succeeded through superior performance could be both unfair and inefficient; the latter because to take away the fruits of excellence would be to destroy the socially beneficial incentive to achieve it.
- 2) In some cases, a large market share may be necessary if economies of scale are to be fully exploited. Indeed, in 1966 the British government set up the 'Industrial Reorganization Corporation' to

encourage mergers towards this end. The conflicts in purpose of the IRC and the Monopolies Commission were apparently resolved by political decision, usually in favour of letting a proposed merger proceed. The IRC has some notable failures to its debit (in particular, British Leyland); nor can it be said that the post-war merger boom was, on the whole, successful in realizing economies of scale (cf. Pickering, 1974, pp. 127-33).

- 3) By undertaking a detailed and competent inquiry, the Monopolies Commission can weigh up the costs and benefits, such as those mentioned above, to determine whether a particular level of, or change in, market structure is in the public interest, under its terms of reference. However, the resources needed to do this properly are such that the Commission can investigate a rather small number of individual cases,<sup>28</sup> whereas our results are that there are thousands of firms who earn a surplus over the 'normal' or competitive rate of return on the inputs they commit to production.

- 4) There is no guarantee that to control structure is to control conduct. Firms that have been prevented from gaining monopoly power by merger may find other ways of co-ordinating their actions.

An area in which our model and results imply that structural policy would be an effective instrument against the exercise of market power is in the encouragement of the small-firm sector. In the model of Part II, market power is measured by the 'distance' of a large firm from the fringe of small businesses with whom it ultimately competes. If that 'distance' could be reduced, by policies encouraging the variety and viability of the small business sector, then so too would be monopoly profits. (We need not fear that policies to increase the number of small firms would also increase each one's profitability, since the latter is controlled by the ease of entry and exit into the sector). I am not proposing programs with a social cost to them, such as subsidies for otherwise unprofitable small business ventures (though these may, of course, be appropriate means of achieving other ends, such as reducing regional inequality), but rather institutional changes which would, for example, make it easier for small business to borrow, or to have access to new technology, at the expense of larger firms. The orthodoxy in Great Britain might not too unfairly be expressed as 'small firms are good to the extent that some of them grow into large firms' (cf. the

Bolton Committee Report on Small Firms, reviewed by B. S. Yamey, 1972) whereas, in my opinion, the small business sector is valuable not only as a distributive system within which all monopoly profits are quickly competed away, but also as a control on the power of large firms to set prices that generate monopoly profits.

Another form of structural policy that may be effective, often through its effect on the viability of small firms, may be laws which discourage vertical integration. Whereas it is often possible to argue that horizontal mergers or large market shares may bring benefits in lower costs to set against any increase in market power, it seems to be much more difficult to find efficiency rationalizations for the forward or backward integration of firms, whereas the market power implications may be substantial. A good example of harmful vertical integration can be found in the UK Brewing industry. By gaining control of Public Houses the big brewers have increased their market power both by cutting out the retail outlets of local or 'fringe' brewers and by reducing the size of the market area where they compete with each other, by eliminating the 'Free House' system.

(ii) Conduct Policies

The other main thrust of traditional industrial policy has been aimed at the conduct of firms; in particular at collusive agreements between them on price and market sharing, and at 'unfair' competitive practices. In the UK, these come under the 'Restrictive Trade Practices Act' of

1956. As I have noted in Chapter I.2 and elsewhere in this Thesis, acts of overt collusion are probably not the mainstay of monopoly power, so that, although the authorities may be successful in isolated cases, even the most diligent and thorough application of the Trade Practices Act will not eliminate more than a small proportion of the monopoly profits that we observe in the UK manufacturing sector. Like the structural policies, conduct regulation is aimed at individual firms, each of whose case must be decided on its merits, with a great deal of expense and bother, which further limits the scope of the policy. Because of this, too, both orthodox structural and conduct policies are likely to be unfair in their application, which can hardly encompass all the cases falling directly under the Acts, and leave unscathed the great majority of firms who are fortunate enough to be able to exercise monopoly power without becoming ostentatiously large or needing to form explicit alliances with their competitors.

(iii) Performance Policies

The traditional approach attempts to affect industry structure and conduct. It does not do so because dominant firms or cartels are judged to be Bad Things in themselves. Rather, these are supposed, according to the orthodoxy of industrial economics, to determine industrial performance -- in this case monopoly profit (too-high prices) -- which is important per se. I have argued that the policies are inadequate because a) they cover only a small subset of

monopolistic price-setting, and b) even where directed they may be circumvented or counterproductive. The lessons that I think can be learned from this are:

- a) we should not design policies that have to be applied to individual firms (with the possible exception of extreme cases, such as British Steel). There are just too many of them, each knowing a lot more about its business than government can or, perhaps, should know, so that the public authorities are always at a disadvantage when dealing in this way with the private sector.
  
- b) we (economists) should not be so naive as to assume that structure-conduct-performance relationships are invariant to public policy-making. The existence of a close-fitting relationship between, say, concentration and profits in a laissez-faire period may mean just that increased concentration is one effective way of increasing profitability. It may not be the only effective instrument available to firms, who therefore, if forced to abandon it by the application of structural policies, will turn to other means of achieving market power -- a forced reduction in concentration will then not



have an effect symmetric to that associated with increases in concentration in the no-policy period.

To summarize, dealing with the behaviour of individual firms is too big and too difficult a game for government to play effectively.

These considerations suggest that we look for a policy tool which would a) operate at a more general level of aggregation than of the firm, b) circumvent the vagueness and fickleness of our structure-conduct-performance models (including my own) by working directly on performance.

So far as I know, the only policy that would meet both these criteria is a system of direct controls over prices charged for final consumption goods, at the level of the industry, or product. Such a system would have the following features:

- a) By acting only on final (retail) prices, the policy would keep the amount of variety to be handled down to manageable level. Of course, I have tried to stress in this Thesis the general heterogeneity of transactions, even within a given industry. However, heterogeneity is undoubtedly less at the retail level, when the one-many relationship between seller and customers forces a good deal of standardization into the terms of transactions (in contrast to exchange of intermediate goods between firms,

which is mediated by explicit bargaining processes, in all their variety).

- b) In any case, it would not be necessary to set prices for all retail commodities and services. At Agriculture Canada we found that, from the total set of about five thousand food commodities sold in supermarkets, we could extract a subset of fifty items which took about half of the total expenditure on food, whose objective characteristics could be quite closely specified, and which included good substitutes for each of nearly all the other thousands of food products. Control over these fifty items, therefore, would act, through their importance and high cross-elasticities, to control all food commodity prices. Food takes up about one quarter of consumer expenditure, so we might expect to have to control two or three hundred items in total; a large but probably not infeasible number. The job of monitoring prices would be considerably simplified if the controlled price set overlapped substantially the set of prices monitored at present for the consumer price index (indeed, the two sets, in principle, should be the same).

- c) Further, it would not be necessary, or even desirable, to attempt to specify all the relevant characteristics of the controlled goods and services. With price and a number of the important physical dimensions of a product standardized, consumers could shift their attention to the less tangible aspects of product quality. As a result of this focusing of interest they would become more expert in evaluating the uncontrolled dimensions of quality, thus increasing the elasticity of demand with respect to these, and so inducing firms to compete on quality, since the returns in higher sales to achieving a quality edge over competitors would be higher than it is at present.
- d) A qualification to this is that it would probably be desirable to make the controlled price a ceiling, so that the most efficient firms could increase their profits by competing on price, too.
- e) Retail price controls would be a policy instrument capable of dealing directly with one of the most important dimensions of market performance; namely, the effects of market power on the distribution of income between private business and the consumer. But what of the distribution of income within the

business sector? Would not a system of final-goods price controls be unfair to sellers in final goods markets, in that it would affect them, but not their suppliers of materials and intermediate goods? I expect not. We found, in Chapter III.2, evidence of a 'piggyback effect' -- of a tendency for monopoly profits earned in final goods markets to be shared back to suppliers in intermediate industries. It seems reasonable, then, to expect that a squeeze on profits, through prices, at the final sales level would also be distributed back through the Input-Output system. Final goods sellers might still be unequally affected, but such an effect may well be in the direction of a more fair interindustry distribution of income, since we also found a significant tendency for industries selling to private consumers to make more Surplus than others.

- f) As a redistributory instrument, price controls would be operated as follows: first, 'Normal Returns', Profits, and, thus, Surplus, would be calculated for each industry (as I have calculated them in this Thesis, but with better data, especially on capital, allowance for factors affecting normal returns such as riskiness, and any other improvements that come to mind). Then, the

Input-Output tables would be used to calculate the rate of Surplus embodied in each commodity, by summing the Surpluses of each input weighted by the proportion of the input in a unit of output of the commodity (having first corrected the published Input-Output coefficients, which at present differ because of differences in Surplus, as well as differences in physical production requirements). Next, the price controllers would begin by squeezing the retail prices of those commodities with the highest rate of embodied Surplus. How far they should go in lowering the general level of prices I do not at present know. I suspect that the Surplus numbers calculated in this Thesis are too high, perhaps because of the omission of some important factor(s) from the computation of Normal Returns.

## 2. Relative Prices

An implication of the considerable interindustry variation in Surplus that we have measured in this Thesis is that the price structure is not particularly closely related to the cost structure -- that prices do not reflect the opportunity costs of the primary inputs embodied in output. A benefit of the price control system would be that, by reducing the variability of Surplus, it would move the economy towards to a price structure more closely reflecting

opportunity costs, with a consequent improvement in the allocation of scarce resources.

However, in contrast to the scatter of price-cost relationships found across industries, we found, in the previous chapter, a rather impressive tendency for all industries to respond fully to changes in input costs over time. It should be ensured, then, that a price control system would not interfere with this desirable property. That is, the price controllers should lower prices as technical advances and other sources of productivity improvement allow, and raise or lower them according to changes in the prices of those materials inputs that are purchased at world prices, which may be taken to be exogenous to the domestic economy. To do so, they would need to keep abreast of developments in best practice technology, and in world market conditions, as do the officials of the firms producing the output. Price changes would not need to be calculated and implemented very often. 80 per cent of the respondents in Atkin and Skinner's sample (1975, p. 86) never change their price by less than 3 per cent. About two-thirds of the fifty-one industries in my sample showed a change, up or down, in price relative to the mean, of 15 per cent, or less, over the fifteen-year period from 1958 to 1973. These two numbers imply a typical frequency of price changes of only one every three years. Although this is almost certainly an underestimate (we have not allowed for inflation, nor for the intra-industry variation in prices that is probably ironed out in the industry indices), it does

perhaps allow us to expect that the price controllers would not be overwhelmed by the need to continually revise their price ceilings.

The price controllers could serve a useful function by filling the gap left by the absence, from almost all real-world industrial and commercial markets, of the 'auctioneer' of competitive theory. By setting ceiling prices, and making public forecasts of future price trends, the controllers could assist firms, especially smaller ones with fewer planners and forecasters of their own, to predict the cost levels they should aim for to remain viably in business, and aid them in planning their investment decisions, especially if the controllers co-operated with trade associations in disseminating information about new technological developments, and on the innovation and investment decisions of other firms.

However, we should not make too much of the role of price as a 'signal' to investors (and so nor of the need to get prices 'just right' at all times). Our 1958-73 data revealed no association between changes in nominal sales and in prices, whereas we would expect a positive relationship if shifts in the demand curve signaled producers to change supply through changes in prices. In a generally Keynesian (demand-constrained) world, quantity signals, such as changes in inventory stocks, or in the rate of new orders, are probably quite adequate to induce the required output response from producers.

### 3. Inflation

By controlling retail prices, government would, of course, have a direct instrument to apply to the rate of inflation, which is just the rate of change of retail prices. Now, it is generally thought that inflation is due to the relationship between prices and wages, with causation running in both directions, and that these variables must be controlled together or not at all. I suggest that this is not so, that there is no need to control inflation other than by acting directly on prices. In Chapter IV.1 it was found that there is no relationship between differences in price and wage changes in individual industries; that is, that there is no evidence of individual groups in the labour force, through strong unions or whatever, being able to exert independent pressure on prices in particular industries, so that a general price control system would not break down into a conflict with particular sections of the workforce and their unions. The absence of a price-and-wage change correlation also implies, in the other direction, that industries which, due to an increase in market power had succeeded in raising their relative prices, did not share the spoils with their employees, so that the latter would have no reason to resist the program of equalizing of Surplus advocated in section (1) above.

Nor is it likely that there is some other factor or factors, omitted from the analysis of the previous chapter, that would explain the interindustry pattern of wage changes, since, as can be seen from Table IV.1, there just is not very



much interindustry variation to be explained. Our conclusion was that wage changes move, in the long run, in a rather uniform pattern, across all industries. Of course, economy-wide increases in wages must affect prices in each individual industry, but the wage changes themselves are determined by changes in consumer prices, and expectations of future changes (if there are costs to renegotiating rates frequently). Were there a firm and permanent (that is, a believable) program of retail price controls in operation, the pressure to achieve rates of earnings increase above the overall rate of productivity growth would be dissipated. The wage-price spiral would be broken, and it would be redundant -- indeed, counterproductive, given the resentment typically thereby caused -- to attempt to fix wages as well.

This proposal should be sharply distinguished from the so-called 'price' (actually profit margin) controls that many of the industrial economies have recently resorted to, in company with attempts to directly fix wage rates. Margin controls, in common with all other extant anti-inflation policies, do not actually act directly on prices. In fact, they provide no more than a formula standardizing the rate at which cost changes can be passed on in higher prices. They are imposed at the level of individual firms, and so encourage diversion of resources towards special pleading and wasteful accounting practices, penalize honest firms, and remove from all firms some of their incentive to reduce costs through

improvements in productivity. The resulting effect on the rate of growth of per capita real output will actually increase inflationary pressure, to the extent that labour expects wage changes to incorporate an allowance for a certain increase in real incomes.

#### 4. Unemployment

As well as a complete lack of correlation between price and wage changes across industries, we found no relationship, in Chapter IV.1, between wage and employment changes, so that there is no evidence that movements in relative wage rates serve to reallocate labour between industries when patterns of demand shift. This implies that a pool of unemployed labour cannot be justified as assisting non-inflationary structural adjustment -- whatever 'Phillips Curve'-type correlation may have been observed over the period was not due to larger changes in relative (and thus absolute, given downward money wage rigidity) wages being needed to facilitate the reallocation of resources in periods when unemployment was relatively low. To put it baldly, unemployment serves no useful purpose<sup>29</sup>. If inflation is really, as I have suggested, a product market phenomenon best dealt with by product market price controls, then it should be possible, by means of consumer demand-expansionary policies coupled with price controls, to expand real output levels up to and beyond what we have become resigned to accepting as 'full' employment, and to maintain such high levels of activity permanently, with cyclical fluctuations manifesting themselves in variations

in the stocks of inventories and unfilled orders, rather than in the 'queue' of unemployed workers and the margin of unused capacity.

Further, the increased profitability that would be associated with high and stable rates of capacity utilization, with lower-risk capital formation, and with less resistance to labour-saving innovations and structural change, would perhaps at least compensate business for having imposed on it, on equity and efficiency grounds, the general lowering and evening out of rates of Surplus proposed in section (1) above.

#### 5. Productivity

Apart from the maintenance of a full and steady level of economic activity, what else might government policy contribute to the economy's rate of growth of productivity? In the previous chapter we found evidence that, ex post, increases in plant size had been associated with improvements in unit costs over the 1958-73 period. Perception of such a correlation may have contributed to the 'big is better' sentiments activated by the Labour Governemnt of the day in setting up the Industrial Reorganization Corporation in 1966. This organization did much of its work in secret, which makes difficult, evaluation of its effectiveness. Pickering's conclusion is tentative but gloomy: 'The danger ... is that excessive enthusiasm for the promotion of mergers may have been achieved at the cost of detriments which the IRC had not considered' (p. 151). As he noted earlier: 'Many estimates of the proportion of successful mergers indicate that well

under 50 per cent fall into that category' (p. 127). This is a rather surprisingly low success rate for transactions supposedly entered into willingly by the parties concerned. One explanation may be that managers' interests are generally well-served by mergers (better chances of promotions and salary increases in a bigger bureaucracy), and that this class has been able to bamboozle the usually ill-informed shareholders into approving the deal, aided and abetted, in some cases, by government, through the IRC.

It is certainly tempting to generalize the ex post correlation between changes in plant size and unit costs into a normative proposition for policy-making, but our other relevant evidence suggests that such a generalization is invalid -- in Chapter IV.1 I found no evidence of inter industry economies of scale. That is, although the relatively large increases in plant size that were actually undertaken in particular industries were profitable, a general increase in size of this magnitude across all industries would not have yielded general economies of scale -- we cannot say there is an observable tendency for industries with bigger plants to have lower costs.

In the previous chapter we also found a positive correlation between unit cost performance and market power -- industries with more market power tended to show a relatively lower rate of increase of total costs (i.e., including capital costs) per unit of output (equation IV.12). Again, we should

beware of invalid generalization of an ex post correlation into a normative policy position, such that market power should be left alone since it 'delivers the goods' of superior productivity growth rates. In an economy in which there are differences in market structure, it could be that control over the production and diffusion of technical progress tends to be one of the property rights that make up market power. As well, the higher profits generated by greater market power, and, possibly, market power-related capital market imperfections, may make it relatively more difficult for more competitive industries to attract financing of cost-reducing research and development activities.

That is, we should not infer from equation (IV.12) that the aggregate rate of growth of productivity would be raised by allowing, or encouraging, the general degree of monopoly power in the economy to increase. A relevant snippet of evidence is that the percentage of industrial employment in 'small' (fewer than 500 workers) firms is 67 per cent in Italy, 62 per cent in Holland, 57 per cent in Belgium, 52 per cent in West Germany, 49 per cent in France, and only 32 per cent in Britain (The Economist, February 25, 1978, p. 46).

However, neither can we infer that a more monopolistic economy would not show a better productivity performance.

#### 6. Welfare and Market Power

In the textbook microeconomic (static, partial) approach, monopoly reduces total welfare by 'restricting output' -- by

raising price above the competitive level where it equals marginal cost, the monopolist drives away some customers

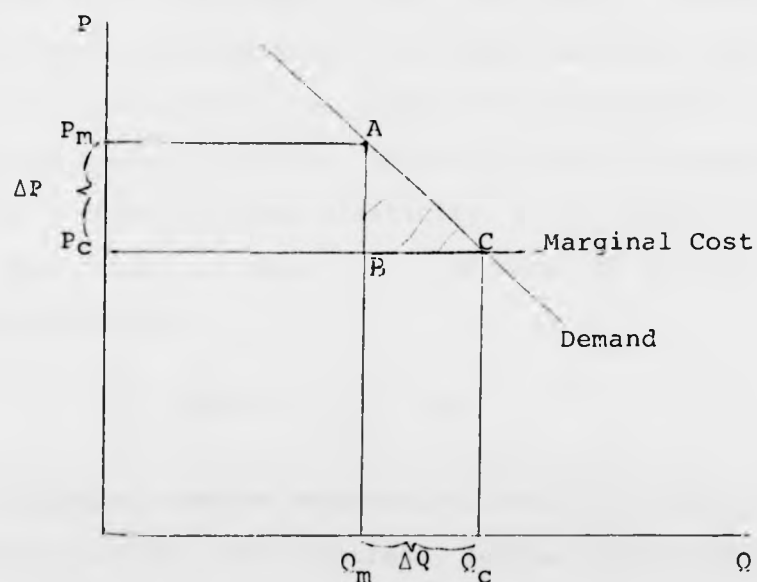


Figure IV.1: The Partial Equilibrium Analysis of Monopoly Welfare Loss

to whom the good is not worth the higher price, but who still received utility from the good that was greater than the cost of producing it. This loss of 'consumer surplus' is given by the triangle ABC in Figure IV.1, for the simplest, and most commonly analysed situation of a linear demand curve and constant marginal costs. Of course, the customers who remain loyal to the good also suffer a welfare loss equal to the rectangle  $P_m A B P_C$  since they are charged more; but this is just handed over to the monopolist. The resulting effect on total welfare of the community will be positive or negative depending on whether a redistribution of income from consumers to

monopolist is judged to be a good or a bad thing. Economists usually feel only able to chalk up the consumer surplus triangle ABC as a definite welfare cost of monopoly, and, beginning with Harberger (1954), have made a number of attempts to estimate its magnitude. The usual approach has been to infer the divergence,  $\Delta p$ , between actual monopoly prices and marginal costs by observed rates of return on capital, to assume a value of price elasticity,  $\eta$  and thus to calculate the restriction in output,  $\Delta Q$  and then the welfare loss,  $\Delta W$ , from the formula

$$\Delta W = \frac{1}{2} \Delta p \Delta Q = \frac{1}{2} \frac{(\Delta p)^2}{p} \eta p Q \quad (\text{IV.13})$$

The resulting numbers have always been rather small fractions of total output. To illustrate, let us, quite typically, suppose a value for the proportion monopoly profits are of price of 0.1, and an elasticity of 1. Then monopoly welfare losses are just  $(0.1)^2 = 0.01$ , or 1 per cent, of the total value of output.

Cowling and Mueller (1976) have criticized this methodology by pointing out, quite correctly, that it is inconsistent with profit-maximizing behaviour by monopolists, under which price and elasticity are not independent numbers, being linked by the profit-maximizing formula (cf. Chapter I.2).

$$\frac{\Delta p}{p} = \frac{p - \text{marginal cost}}{p} = \frac{1}{\eta} \quad (\text{IV.14})$$

(IV.14) can be substituted into (IV.13) to eliminate price elasticity,  $\eta$

$$\Delta W = \frac{1}{2} \left( \frac{P\Delta P}{P} \right) PQ = \frac{\pi}{2} \quad (\text{IV.15})$$

so that the welfare loss becomes simply one half of observed monopoly profits. If these are, as in the previous example, 10 per cent of revenue, then welfare loss will be 5 per cent -- a rather larger fraction than that arrived at through the incorrect method. Cowling and Mueller's work is valuable in exposing the slipshod analysis of the Harberger school, but, from the view of the economy put forward in this Thesis, their own methodology is open to criticisms:

- 1) The values of  $\eta$  implied by actual profit rates are unrealistically large, at least for their U.S. data. For Cowling and Mueller's samples of U.S. and UK large firms, the ratios of the difference between mean and competitive rates of return to the mean rate is about 0.2 for the US and 0.6 for the UK, (cf. Cowling and Mueller, p. 29, footnote 13), implying elasticities of 5 and 1.66. While the UK number is not implausible, that for the US is well above the range of elasticities generally found in demand function studies (cf. Chapter I.2). Even in the UK, there would be firms earning a return less than the mean rate, for whom the implied price elasticity would be out of line with the empirical estimates.



In my chapter I.2, I suggested that this sort of inconsistency with other evidence forces an examination of the validity of the assumptions that underpin the orthodox market demand curve-based model, and in Chapter II.3, I proposed a bargaining model as an alternative, in which observed (ex post) price elasticities that are less than one in absolute value are quite consistent with rational behaviour. In such a model, the allocative consequences of monopoly pricing are not expected to be substantial; indeed, the sort of numbers arrived at by Harberger and company may well be good approximations, though for the wrong reason.

- 2) The formula (IV.14) implies that monopoly output is always half competitive output, no matter what the elasticity. I do not find believable the magnitude (or the constancy) of this restriction of output attributed to market power, and suggest that it reveals another misleading implication of the parametric demand curve assumption underlying the orthodox model. I suggest that it is more likely, in a Keynesian (demand constrained) world, that when an industry becomes monopolized, it uses some of its market power to expand sales, by pushing out its demand curve. That is, the elasticities we observe from price and quantity change for given market structures, cannot be applied to a situation when the price change

follows a change in market structure. As an example, we found in Chapter III.2 that some monopoly profits can be accounted for by higher advertising/sales ratios. Since advertising shifts outwards the demand curve, the associated monopoly profits, in this case, go along with an expansion, not a restriction, of output.

Cowling and Mueller also make a pioneering attempt to account for, in their measures of monopoly welfare loss, the ingenious suggestion of Tullock (1967) and Posner (1975) that monopoly profits will be dissipated by the costs incurred in a competitive struggle to capture them; that 'obtaining a monopoly is itself a competitive activity' (Posner, p. 807). This seems to suggest that we should never in fact actually observe any profits above competitive levels, which is, of course, not in accordance with reality. Cowling and Mueller try to get around this by supposing that there are winners and losers in the struggle for monopoly rents, and assume that the costs incurred by the losers equal, in sum, the observed monopoly profits, which were the winners' reward. However, if this hypothesis were correct, we would expect to find evidence of these costs incurred by the losers, who would be firms earning sub-competitive rates of return. In fact, over the sample of forty firms shown in Cowling and Mueller's tables 2 and 4, only four of the US, and none of the UK firms earned less than the competitive return on their capital

No doubt it is reasonable to assume that a firm will be willing to pay a price to achieve some market power, up to the expected value, in higher trading profits, of that power. However, to the extent that market power is distributed as freely-negotiable property rights, what are costs to the purchasing firm are just transfers of assets from society's point of view, not necessarily using up any resources above the transaction costs. Thus, for example, bribery to obtain a government licence is just a transfer of income, and may, indeed, be quite efficient, to the extent that the bribe system works like an informal auction market, in which the winning bidder is the one to whom the licence is worth the most.

As another example in the terminology of the model of Part II, one firm may take over another in order to increase its product 'territory', and do so in a transaction that is beneficial to both parties and incurs no social costs.

However, when market power can be 'produced', as by the expenditure of money on advertising, and when existing property rights cannot be transferred costlessly, as when two firms combine to increase their market territory, but in so doing incur some diseconomies of scale,<sup>30</sup> there will indeed be welfare losses generated by monopolies. It is difficult to see, however, how these true social costs can be separated out from the transfer payments, and from the continual flow of new non-'produced', or rent-earning property rights

generated by the discovery of new sources of natural resources, by the birth of new ideas and inventions, by the debuts of new entrepreneurs, and so on.

In the model of Part II, market power was actually defined in terms of the real costs each party to a transaction can impose on the other by not trading. However, in this model, these threats served just to determine the distribution of the gains from trade, and were hardly ever supposed to be actually carried out. One exception occurs in the case of a firm with market power selling a product to customers between whom it is unable to discriminate on the price charged. Then, some customers near the border of the firm's territory would find it worthwhile to incur some costs by 'travelling' to the border to purchase from fringe firms at their lower price. However, depending on the relationship between market power and price, such customers may actually eventually become a smaller proportion of all customers as market territory increases, and the fringe recedes in importance.

We have so far discussed only the short-term, or static, welfare implications of market power. We have little evidence on questions of the long-term effects of monopoly -- on its influence on the rates of growth of employment and output. In Chapter IV.1, I did report a correlation between market power and the rate of change of unit costs, but, as I warned in the previous section, such a correlation does not

tell us whether a generally competitive economy would show inferior cost performance to an economy that was monopolistic. Surely, these are questions which would be most fruitful for future Industrial Economics research.

Footnotes

- 1 (p. 14) cf. for example, the quotations beginning Chapter III.3.
- 2 (p. 21) For 33 Canadian industries, for which I have product-market Herfindahl and seller Concentration data, a regression between the two variables showed that an SCR value of 60 (about the mean value for the UK sample) would be associated with a Herfindahl index of about 0.11.
- 3 (p. 21) More problematic may be the tendency of formula (1.1) to predict too large a spread of profit/sales ratios. The values taken by SCR one standard deviation above and below its mean are 83 and 39, respectively. Using the formula estimated from the Canadian data suggests that the associated Herfindahls are about 0.027 and 0.031, which, in (1.1) imply profit margins of 0.621 and 0.093. This variation is probably too big to be reasonable, since the + and - one-standard-deviation-from-the-mean values of the profit/sales ratio in the UK sample are 0.259 and 0.140.
- 4 (p. 26) More accurately, efforts to increase market shares will have a positive effect on profits if there are some spillovers to total industry sales. Against this, the various marketing expenses are themselves costs, of course, and so subtract from profits. The net effect may be in either direction. There can be no doubt, however, of the effect of price-cutting on profits when demand is inelastic.
- 5 (p. 28) And since these boundary situations occur between products with the highest substitutability, they may often involve just two or three of the firms in an 'industry', so that price-co-ordination becomes a series of more-or-less independent small group decision problems, rather than the more difficult matter of finding a single 'price' that is acceptable to all firms.
- 6 (p. 36) Some respondents used more than one pricing procedure, so that the sum of the percentages is greater than one.
- 7 (p. 37) A possible exception (cf. Silberston, p. 49) is Early's (1956) study of 110 'excellently managed companies' in the US, who were found to make extensive use of marginalist principles in their pricing, marketing, and new product policies. What one would like to know is whether these principles were as well-ensconced amongst all the less excellently managed firms.

- 8 (p. 95) The quality of an economy's stock of entrepreneurial resources is, of course, extremely important -- more so, no doubt, than the industrial structure factors being examined here -- in determining the level of economic well-being achieved by a society. However, it is probably legitimate to take entrepreneurial capacity as given to our present analysis, since the political, cultural, and legal factors that account for it are surely outside the scope of this Thesis.
- 9 (p. 115) It is true that unique price does not necessarily imply equal rates of profit between firms -- costs could differ with scale, for example. However, erroneously attributing all profit differences to cost differences leads to unjustified laissez-faire policy conclusions, as in Demsetz (1973).
- 10 (p. 118) Examples are the two studies, published together, of the structure-performance relationship in Canadian manufacturing (Jones et. al., McFetridge, 1973). Each uses a different profitability variable; neither justifies their choice or recognizes that any problem may exist.
- 11 (p. 128) Cf. Freund and Debertin (1975) for a demonstration of the dangers of data mining.
- 12 (p. 132) In work on the Canadian food and beverages processing industry (Hazledine, 1978b), in which I pooled cross sections of annual time series data, I was able to use an elasticity-weighted income growth variable, and found it to be significant in explaining year-to-year fluctuations in profitability.
- 13 (p. 136) Of course manufacturing industries selling products classified into 'Final Consumption' in the Input-Output tables normally make use of wholesalers and retailers. To the extent that there are interindustry differences in the buyer power of these operators, our simplification will be blurred.
- 14 (p. 145) Concern about the proper classification of firms into 'markets' has lead some (e.g., Khalilzadeh-Shirazi, 1974, Hart and Morgan, 1977, Waterson, 1976) to omit from their sample MLHs for which the 'specialization' or 'exclusiveness' ratios are less than some arbitrary number such as 0.8, on the grounds that these industries do not form a homogeneous market in the sense defined. Unfortunately, the excluded industries are, as one would expect, predominantly in the capital goods sector, so that their omission reduces the generality of the results by weakening the representation of capital goods industries in the sample.

- 15 (p. 150) It is probably not correct to assume that normal rates of return have stayed constant over the twenty-year sample period. Changes in real wage rates, in risk, and in the rate of interest, may have had a net effect of changing the R's by a few percentage points. However, given the insensitivity of the  $r^2$ 's to changes in the R's, it is probably so that any estimate of differences in normal rates, census-to-census, would be statistically spurious.
- 16 (p. 159) Three industries -- Sugar, Tobacco, and Man-made Fibres -- have particularly large plants, measured by their Net Output per Establishment (cf. Appendix C). If the second and third of these are also removed from the sample (Sugar is already out) of specification 12, the regression equation becomes

$$\begin{aligned} \text{AVSURP} = & 0.008\text{NO/EST} + 0.180\text{ADV} \\ & (0.30) \quad (6.97) \\ & + 0.464\% \text{DGC} + 0.039\text{BCRINT} - 0.040(\text{BCRINT})^2 \\ & (1.61) \quad (5.10) \quad (-2.60) \\ & + 0.014\text{BUYC} - 0.005\text{BUYP} \\ & (11.24) \quad (-0.71) \\ & + 0.003\text{BUYK} + 0.013\text{BUYF} \quad \bar{R}^2 = 0.670 \\ & (1.84) \quad (3.77) \end{aligned}$$

-- the significance of the NO/EST variable disappears, suggesting that, at least so far as our imperfect industry-average data can discern, it is only for very large plant sizes that the variable influences profits. Removal of the outliers has no disruptive effect on the magnitude or significance of any of the other coefficients.

- 17 (p. 174) In this table I report the results of what seemed to be the most successful regression specification in each paper, following Hart and Morgan's survey.
- 18 (p. 202) Except that Salter did not include a measure of unit capital costs in his analysis. As well, he did not show the correlation between price and wage rate changes. This, however, can be calculated from the data in his Table 14 (p. 107). The correlation,  $r$ , between the two variables is 0.044.



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-- the significance of the NO/EST variable disappears, suggesting that, at least so far as our imperfect industry-average data can discern, it is only for very large plant sizes that the variable influences profits. Removal of the outliers has no disruptive effect on the magnitude or significance of any of the other coefficients.

- 17 (p. 174) In this table I report the results of what seemed to be the most successful regression specification in each paper, following Hart and Morgan's survey.
- 18 (p. 202) Except that Salter did not include a measure of unit capital costs in his analysis. As well, he did not show the correlation between price and wage rate changes. This, however, can be calculated from the data in his Table 14 (p. 107). The correlation,  $r$ , between the two variables is 0.044.

- 19 (p. 206) In a study of quarterly time-series data for five UK manufacturing industries over time periods quite similar to the 1958-73 coverage of this Thesis, Sherif (1977), finds a significant relationship between capital costs and prices. He also finds that changes in wage rates or earnings are determinants of price changes. Note that this does not contradict our finding that interindustry differences in price changes over the period are not related to earnings changes. The interpretation to be placed on the two sets of results is that, although a wage rate change in a given industry is associated with a change in that industry's price, actual wage changes have been so similar across industries that they cannot explain any of the differences between industry's price performance.
- 20 (p. 214) Ripley and Segal do find (p. 267) that wage rate (compensation per man-hour) changes are significantly related to price changes, in contrast to the UK findings. Further work is needed to examine this apparent major behavioural difference between the US and UK economies.
- 21 (p. 215) In a recent simulation study with the three macroeconomic models of the UK economy, Laury et. al. write: "It should be stressed ... that there is little faith in the existence of a stable Phillips Curve for the UK and these [Phillips curve] equations are not used in actual forecasting (1978, p. 55, footnote 1).
- 22 (p. 221) Calculated from the tables at the end of each of Pratten's chapters on individual industries. Of course, if industries tended in 1958, to have plants of less than half the 'minimum efficient scale', doubling size of their plants would, on average, reduce costs by more than 7½ per cent. On the other hand, industries in which doubling took them past minimum efficient scale would tend to show a smaller cost reduction.
- 23 (p. 221) Real Net Output per Establishment and Real Net Output did grow at quite similar rates, on average, between 1958 and 1973, but with by no means an identical pattern across industries. The regression relationship is

$$\% \Delta \text{RNO/EST} = 0.561 + 0.523 \% \Delta \text{RNO}, \quad \bar{R}^2 = 0.459 \\ (3.57) \quad (6.59)$$

so there was a good deal of industry growth not associated with plant size growth, and vice versa.

- 24 (p. 221) Indeed, since NO/EST is biased upwards as a measure of real output by the higher prices associated with large-plant industries, regression (IV.10) could be interpreted to show that there are diseconomies of scale, across industries.

- 25 (p. 223) Cf. Lambin's evidence (1976), p. 103) cited above in Chapter I.2.
- 26 (p. 223) Although eleven industries do not make up a large sample, they are better than just one. Kaldor (1957) demonstrates the dangers of too-casual empiricism by claiming that the fact that in the UK and German automobile industries productivity grew at different rates whereas they might be presumed to have access to similar technology, implies that economies of scale, not technical advance explain differences in productivity growth.
- 27 (p. 226) I did not correlate rates of change for unit 'capital' costs, since the proxy for capital -- investment expenditure -- is probably too affected by cyclical 'noise' to give reliable five-yearly comparisons.
- 28 (p. 234) Pickering (p. 146) reports that only 'about 3 per cent of all qualifying mergers' were the 'subject of a reference to the Commission' over the 1965-73 period.
- 29 (p. 248) A recent twist, from the so-called 'new micro-economists', is to justify all unemployment as voluntary job search behaviour. For this to be plausible, search from unemployment must be more profitable to the worker than on-the-job search (in fact, it must be considerably more profitable, in terms of higher wage eventually captured, since on-the-job search has the advantage that the searcher is being paid while he/she looks for another job). There is no evidence that this is generally so.
- 30 (p. 257) The typically rather low realized profitability of merger activity in the UK referred to in the previous section may be due to the generation of costs -- diseconomies of scale, in particular -- incurred in order to win some additional product market power.

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APPENDIX A : Productivity Fluctuations in the Short-term

(For reasons of space, this Appendix has not been included in the text of the Thesis. It has been published in the Oxford Bulletin of Economics and Statistics, Volume 39, Number 4, November 1977, under the title 'Short-term Production Functions and Economic Measures of Capacity for UK Manufacturing Industries', with Ian Watts as co-author. A reprint will be supplied on request.)

## Appendix B: Price Setting with Some Demand Elasticity

In this appendix, I attempt to generalize the price-setting models of Chapter II.3 to allow some price elasticity of demand. It will be assumed that:

1. buyers are evenly distributed along a line, at unit distance apart;
2. the cost to a buyer of moving a unit distance along the line is constant,  $d$ ;
3. each buyer has the same log-linear demand curve for the product.

We will also begin by allowing non-zero, but constant, unit costs of production,  $c$ , but we will find that this prevents us from reaching analytically tractable expressions for price.

Two cases will be examined: unique and discriminating price-setting.

### 1. Unique Price

The demand curve is:

$$q = p^{-\alpha}, \quad \alpha > 0 \quad (1)$$

for each buyer, where  $q$  is the quantity purchased, and  $p$  is the price.



Each buyer is some distance  $m$  from the centre of the seller's territory, the border of which is at  $M$ . A buyer will purchase from the firm rather than from the fringe firms at  $M$  so long as

$$(M-m)d \geq p - \bar{p} \quad (1),$$

where  $\bar{p}$  is the price charged by fringe firms. Call the  $i$ th buyer the one distanced  $(M-m_i)$  from the fringe who is just indifferent about from whom he buys. For this buyer, then

$$(M-m_i)d = p - \bar{p} \quad (3)$$

We will assume that the  $i$ th buyer, as well as all buyers further from the fringe deals with the firm being examined so that there are  $m_i$  buyers. This firm's problem is to maximise profits,  $\pi$ ,

$$\pi = (p - c)qm_i \quad (4),$$

such that (3) holds

Rearranging (3) and substituting into (4) gives

$$\begin{aligned} \pi &= (p^{1-\alpha} - cp^{-\alpha}) \left( M - \frac{p-\bar{p}}{d} \right) \\ &= -\frac{1}{d} p^{2-\alpha} + \left( M + \frac{\bar{p}}{d} + \frac{c}{d} \right) p^{1-\alpha} - c \left( M + \frac{\bar{p}}{d} \right) p^{-\alpha} \quad (5) \end{aligned}$$

Differentiating with respect to the decision variable,  $p$ :

$$\begin{aligned} \frac{d\pi}{dp} &= -\frac{(2-\alpha)}{d} p^{1-\alpha} + (1-\alpha) \left( M + \frac{\bar{p}}{d} + \frac{c}{d} \right) p^{-\alpha} \\ &\quad + \alpha c \left( M + \frac{\bar{p}}{d} \right) p^{\alpha-1} \quad (6) \end{aligned}$$

Equating to zero, (6) gives

$$(2-\alpha)p^2 - (1-\alpha)(Md+\bar{p}+c)p - \alpha c(Md+\bar{p}) = 0 \quad (7)$$

Unfortunately, the roots of (7) are very complicated functions of the parameters. To get something tractable, I will assume zero costs (following in a tradition beginning with Hotelling, (1929), in the spatial competition literature). Then (7) simplifies to

$$p = \frac{(1-\alpha)}{(2-\alpha)}(Md+\bar{p}) \quad (8)$$

We must find out if (8) gives a maximum or a minimum value for price:

$$\frac{d^2 \pi}{dp^2} = - \frac{(1-\alpha)(2-\alpha)}{d} \bar{p}^{-\alpha} - \alpha(1-\alpha) \left(M + \frac{\bar{p}}{d}\right) p^{-\alpha-1} \quad (9)$$

We look at three possible ranges of values for  $\alpha$ :

(i)  $0 < \alpha < 1$

This is the inelastic demand case. (9) is negative for all positive values of  $p$ , including the extremum, which is therefore a maximum. Note that  $p$  in (8) is an increasing function of  $\bar{p}$ ,  $M$ , and  $d$ , and a decreasing function of  $\alpha$ , as we would expect.

(ii)  $1 < \alpha < 2$



In this, elastic demand, case,  $p$  in (8) is negative.  $d\pi/dp$  (6) is negative, too, so that it would seem to pay to reduce price without limit. However, price in this case will then be set equal to  $\bar{p}$ , since any firm always has the option of selling whatever quantity it wishes in the fringe (competitive) market at this price, and so would not change anything less.

(iii)  $2 < \alpha$

When demand is still more elastic,  $p$  in (8) becomes positive, but the sign of  $d^2\pi/dp^2$  can be shown to also be positive, so that (8) gives a profit-minimizing price. This suggests that either the competitive price,  $\bar{p}$ , or some very high price will be changed. The latter is a puzzling property of the particular model used here.

However, since  $\alpha$  measures the industry price elasticity, and since the evidence cited in Chapter I.2 points overwhelmingly towards industry price elasticities being less than 2 (in absolute value), if not less than one, cases (i) and (ii) are the relevant ones.

## 2. Discriminating Prices

We next consider the case where the seller takes advantage of the property that the 'travelling' costs that distance buyers from the fringe also distance them from each other -- the  $i$ th buyer incurs costs of  $d$  in travelling to

the (i+1)th -- so that price discrimination is possible. We look at two situations when no explicit bargaining takes place and when it does.

(a) Price Discrimination Without Bargaining

This situation is plausible when there are actually a large number of similar customers at each distance in from the fringe, as when, for example, a consumer good is sold in different towns.

The seller's problem is to maximize

$$\pi_i = (p_i - c)x_i \quad (10)$$

Subject to

$$p_i \leq \bar{p} + (M - m_i)d \quad (11)$$

for each group i

If demand is inelastic ( $0 < \alpha < 1$  in (1)),

The seller will simply set the highest price he can get away with without driving the customers to the fringe -- that is, the price given by (11) when the strict equality holds:

$$p_i = \bar{p} + (M - m_i)d \quad (12)$$

The difference in prices charged to different groups equals the travelling costs between them, so there is no incentive for buyers to disrupt the discriminatory price structure by reselling.

If demand is elastic, the seller may not choose to price up to  $\bar{p} + (M-m_i)d$ . The value of  $p_i$  that maximizes (10) is

$$P_i = \frac{cC}{\alpha-1} \quad (13)$$

For small  $(M-m_i)d$  -- customers close to the fringe -- the constraint (11) may be active, and price will be given by (12). However, for customers well inside the firm's territory, (12) may be higher than (13) -- it will pay the firm not to use all its market power, since to do so would reduce sales unprofitably. Then, (13) gives the profit-maximizing price.

(b) Price Discrimination with Explicit Bargaining

If there is only one buyer at each distance  $(M-m)$  from the fringe, a discriminating seller is setting a different price for each transaction. Under these circumstances, as I suggested in Chapter II.3, an explicit bargaining situation will probably develop -- the seller's no-sale threat to the buyer will be countered with the buyer's threat to take his custom elsewhere. In the terminology of Chapter II.3, we wish to measure the 'realized market power' of seller,  $F_s$  and buyer  $F_b$  -- the costs one party can impose on another by not trading at any given price. We have

$$F_s = \int_{\bar{p}}^{\hat{p}} p^{-\alpha} dp \quad (14)$$

$$\text{where } \hat{p} = \bar{p} + (M-m)d \quad (15)$$

$F_s$  equals the money costs of buying at the higher real (i.e., including transport costs) fringe price  $\hat{p}$ , plus the consumer surplus loss at the lower level of consumption.

The buyer's penalty costs are

$$F_b = (p - c)p^{-\alpha} \quad (16)$$

-- the loss in profits to the seller if the deal does not go through. Suppose (as in equation II.5) that the realized gains from trade are distributed according to

$$F_s = \theta F_b \quad (17)$$

where  $\theta$  is any positive number. Substituting (14) and (16) into (17) gives, eventually,

$$\frac{1}{1-\alpha} (\hat{p}^{1-\alpha} - \bar{p}^{1-\alpha}) = \theta (p^{1-\alpha} - cp^{-\alpha}) \quad (18)$$

which is not, in general, analytically solvable. To get something tractable, let us assume again zero costs. Then (18) leads to

$$P = [\theta(1-\alpha) + 1] \frac{-1}{1-\alpha} \hat{p} \quad (19)$$

-- the proportion  $\rho$  is of  $\hat{p}$  depends on  $\theta$  and  $\alpha$ .

If we assumed a linear demand function, the equivalent expression to (18) would be a quadratic function,

for which we could find the roots, even with non-zero costs. Other assumptions would lead to different expressions. The point of this Appendix was just to work through some simple examples to show ways in which market structure can be related to demand and price, rather than to emerge with generally applicable formulae. Given the tendency for the analysis to become intractable with even quite modest attempts at generalization, I do not expect that such formulae are attainable.

The models do illustrate that the presence of fringe sellers constrains the price of a profit-maximizing firm when this is inelastic, but in a way that depends systematically on the market power of the firm. They also show how the demand curve facing a firm is not in general invariant with respect to that firm's market power.

## Appendix C: Data and Variables

### 1 Coverage

The data are at the level of aggregation used for the 1963 Input-Output Tables. All manufacturing industries in the I-0 tables (I-0 numbers 5-61) are included, with the exception of numbers

- 14 -- (Coke Ovens)
- 28 -- (Office Machinery)
- 39 -- (Ships)
- 41 -- (Aircraft)
- 42 -- (Other Vehicles)
- 47 -- (Textile Finishing)

for which no price data were available. Thus there are 51 industries in the full sample.

### 2. Sources

Data on the proportion of output going to different end-users are from the 1963 Input-Output Tables, and are for that year.

Data on Gross Output, Net Output, Wage and Salaries, Number of Employees, End-year Stocks, Capital Expenditures, Concentration Ratios, Expenditures on Advertising, and Numbers of Enterprises and Establishments are from various issues of the Report on the Census of Production. Data for 1973 are 'provisional'. These data are presented at the 'Minimum List Heading' (MLH) level, and so were aggregated, where necessary,

to the level of the Input-Output tables. The coverage of the published Concentration Ratio data has been augmented by information contained in Walshe (1974).

Data on wholesale Price Indices are from various issues of the Department of Trade and Industry's publication Trade and Industry.

### 3. Manipulation of 1954 Data

1. The 1954 Census asked respondents to exclude 'merchanted and factored' goods. Merchanting and factoring were included in 1958 and subsequent Censuses. Therefore, I attempted to adjust the 1954 data to include merchanting and factoring, as follows. The 1958 Census does break-down Gross Output, but not net Output, Wage and Salaries, and Employment, into Merchanting and Manufacturing activity. Therefore, I calculated Gross Output (G0) for 1954 by assuming that the ratio of merchanting to manufacturing gross output observed in 1958 also held in 1954. Thus

$$G054 = NMG054 * \left( \frac{G058 + MERC58}{G058} \right)$$

where 'NM' means 'not-merchanted', and MERC58 is the gross output of merchanted and factored goods. (For simplicity, I use equality signs throughout this Appendix, though all the relationships are identities.)

To get numbers for Net Output (NO) and Employment of 'Operatives' (EOP) and 'Others' (EOT), I assumed that the ratios of Net Output to Gross Output and of Employment to Gross Output observed in the industry classified as 'Distribution' under the Standard Industrial Classification would hold for merchanting and factoring (which is essentially a service activity), and be the same for Operatives and Others. These ratios were 0.23 and 0.22, respectively. Thus, using the already calculated value of gross output in 1954:

$$NO_{54} = NMNO_{54} + 0.23(GO_{54} - NMG_{54})$$

$$EOP_{54} = NMEOP_{54} + 0.22(GO_{54} - NMGO_{54})$$

$$EOT_{54} = NMEOT_{54} + 0.22(GO_{54} - NMGO_{54})$$

Wages and Salaries of Operatives (WSOP) and Others (WSOT) were then calculated by assuming that they were the same for manufacturing and merchanting employees:

$$WSOP = NMWSOP_{54} * \left( \frac{EOP_{54}}{NMEOP_{54}} \right)$$

$$WSOT_{54} = NMWSOT_{54} * \left( \frac{EOT_{54}}{NMEOT_{54}} \right)$$

#### 4. Change in Standard Industrial Classification

During the sample period there was a change from the 1958 to the 1968 Standard Industrial Classification (SIC). In the 1963 Report on the Census of Production, data were given for the 1958 SIC. In the 1968 Report, data were under the new SIC, and were given thus for 1968 and 1963. In the Structure-Performance regressions of Part III, the 1963 data



from the 1968 Report are used, but no attempt was made to adjust the 1954 and 1958 data to allow for changes caused by the change in classification. In the five-yearly rates-of-change correlations reported in the last section of Chapter IV.1 the earlier (1958 SIC) data are used for 1958-63 rates of change, and the 1968 SIC for the 1963-68 analysis.

#### 5. Tables

There are 89 variables in the primary database, which, for reasons of space, I do not show in this Thesis. A printout of the database can be supplied on request. In Table C.1 are shown the computed variables used in the Structure-Performance regressions of Part III.

Table C.2 gives the rates of change, cost shares, and market power data computed for the analysis of Chapter IV.1. The 'rates of change' are not in fact presented as such, but as the ratio of 1973 to 1958 values of each variable, to make their magnitudes easier to perceive visually.

#### 6. Definitions of Variables Computed for Structure-Performance Regressions

WAGES = WSOP + WSOT (Wage Bill)

EMP = EOP + EOT (Employment)

INSTK = Sum of end-year stocks of materials, fuel,  
finished goods, and work in progress  
(Inventory Stock)

GINV = Capital expenditures net of disposals  
(Gross Investment)

PROFITS = NO-WAGES (Net Output less the Wage Bill)

GMARG = PROFITS/GO (Profit Margin on Gross Output)

NRET = RK\*GINV + RI\*INSTK + RL\*WAGES (Normal Return  
to Resources)

RK = 1.2

RI = 0.2

RL = 0.2

'Normal' rates of return to capital  
stock, inventories, and management.

SURP = PROFITS/NRET (Rate of Surplus earned)

XSURP = (PROFITS + WAGES)/(NRET - RL\*WAGES)  
(adjusted Surplus)

The above variables are calculated for each of the 51 industries and for each of the sample years 1954, 1958, 1963, 1968, and 1973. For the regressions of Table III.4, the data are 'stacked' giving series with 255 observations for each variable. For the regressions of Table III.3, the data are simply averaged across the five sample years. Averaged values of SURP, XSURP, and GMARG are prefixed by the letters 'AV'.

NO/EST = Net Output/(Numbers of Establishments)

GO/EST = Gross Output/(Numbers of Establishments)

NO/ENT = Net Output/(Numbers of Enterprises)

GO/ENT = Gross Output/(Numbers of Enterprises)

These variables could be measured only for three years -- 1958, 1963, and 1968. They were stacked according to the formula

$$X_{i,t} = \begin{cases} X_{i,1958}, & t = 1954, 1958 \\ X_{i,1963}, & t = 1963 \\ X_{i,1968}, & t = 1968, 1973 \end{cases}$$

where  $i$  is the industry subscript, and are simply averaged for the Table III.3 runs.

SCR = (Sum of the sales of the five largest firms in each MLH industry included in the I-0 industry for which the information was available)/(total sales of the available MLH industries)

(so SCR is a weighted average concentration ratio)

ADVR = (Expenditure on Advertising Services)/GO

SCR and ADVR could be calculated only for 1963 and 1968, and were stacked according to

$$X_{i,t} = \begin{cases} X_{i,1963}, & t = 1954, 1958, 1963 \\ X_{i,1968}, & t = 1968, 1973 \end{cases}$$

and averaged for Table III.3 regressions.

$$\%DGO = 0.25 \frac{GO73-GO68}{GO68} + \dots + 0.25 \frac{GO58-GO54}{GO54}$$

(average inter-census rate of change in Gross Output. Used only in Table III.3 regressions)

$$DUM_t = \begin{cases} 1, & \text{year } t \\ 0, & \text{otherwise} \end{cases}$$

(for stacked regressions)

BCRINT = Sum of (SCR of each manufacturing industry buying more than 1 per cent of the total output of the selling industry) \* (proportion of the total output bought by that buyer), plus (total proportion of total output bought by all buyers purchasing less than 1 per cent of the total output)\* 64.1, 1963.

Notes

1. BCRINT is thus a weighted average of the SCRs of manufacturing buyers of intermediate goods from the selling industries.
2. 64.1 is the average SCR for the sample. Industries buying less than one per cent were lumped together for computational simplicity.
3. Sales to final users (see below) accounting for less than 1 per cent of an industry's output are also included in BCRINT.

BUYC = Sum of proportion of sales to Agriculture, Forestry and Fishing, Construction, Consumers, Distributive trades and (Proportion of Sales to Private Consumption).

BUYP = Sum of proportion of sales to Coal mining, Other Mining and Quarrying, Gas, Electricity and Water, and All Transport and Communications. (Proportion of Sales to Public consumption).

BUYK = Proportion of Sales to Fixed Capital Formation.

BUYE = Proportion of Sales to Exports.

IMP/GO = Ratio of Imports to Gross Domestic Output.

Note: Sales are measured as Gross Output minus the difference between end-year and beginning-year Inventories over the census year. BUY ratios are for 1963.

7. Definitions of Variables Computed for Chapter IV.1

P = Wholesale Price Index, 1963 = 1,000

RGO = GO/P (Real Gross Output)

RNO = NO/P (Real Net Output)

UMATC = (GO-NOS/RGO (Unit Materials Cost)

UWAGC = WAGES/RGO (Unit Wage Cost)

UVCOST = UMATC + UWAGC (Unit Variable Cost)

$RGINV_{1958} = GINV_{1958} * 1.080$

$RGINV_{1963} = GINV_{1963} * 1.000$

$RGINV_{1968} = GINV_{1968} * 0.830$

$RGINV_{1973} = GINV_{1973} * 0.587$

(Real Gross Capital Expenditures. The deflator is the deflator used in Economic Trends (HMSO) to deflate total manufacturing capital expenditures. There was little difference between this deflator and the deflator that is available for Industry 'Orders' at the II-digit level)

URGINV = RGINV/RGO (Unit Capital Cost)

UTCOST = URGINV + UVCOST (Unit Total Cost)

X/E = RGO/EMP (Labour Productivity)

W/E = WAGES/EMP (Earnings per Employee)

Rates of change of these variables and Peal Net Output per Employee (RNO/EMP) were calculated as

$$\%DX = \frac{X_t - X_{t-1}}{X_{t-1}}$$

$$PWAGE = 0.5 \left( \frac{WAGES}{GO} \right)_{1958} + 0.5 \left( \frac{WAGES}{GO} \right)_{1973}$$

$$PMAT = 0.5 \left( \frac{GO-NO}{GO} \right)_{1958} + 0.5 \left( \frac{GO-NO}{GO} \right)_{1973}$$

$$PK = 0.5 \left( \frac{GINV}{GO} \right)_{1958} + 0.5 \left( \frac{GINV}{GO} \right)_{1973}$$

$$PVC = PWAGE + PMAT$$

$$PTC = PK + PVC$$

Z = Rate of Surplus predicted by Regression 12  
of Table III.3 ('Market Power').

TABLE C-1 DATA FOR PROFITABILITY REGRESSIONS, 51 INPUT-OUTPUT INDUSTRIES

1963 INPUT- OUTPUT NUMBER	AvgMARG	SURPLUS	NET OUTPUT/ ESTABLISHMENT	ADVR	BUYER CONCENTRATION RATIO	SELLER CONCENTRATION RATIO
		AVSURP	NO/EST	ADVR	BCRINT	SCR
1	0.158	2.707	1.527	2.177	30.380	80.850
2	0.140	1.943	1.190	1.417	17.980	45.950
3	0.190	2.781	15.293	0.110	23.180	98.950
4	0.227	2.003	2.122	4.939	4.280	47.580
5	0.149	1.418	1.401	2.956	6.180	23.150
6	0.201	2.036	2.402	1.901	1.180	49.180
7	0.075	1.985	18.204	1.555	0.580	69.450
8	0.095	1.033	4.088	0.598	24.080	95.980
9	0.263	2.603	1.452	2.804	25.580	52.250
10	0.371	3.197	3.258	10.534	7.580	41.450
11	0.105	2.300	2.191	5.054	30.080	82.250
12	0.275	1.981	5.231	0.742	40.780	76.750
13	0.244	1.503	3.465	0.748	30.980	41.050
14	0.145	1.010	3.401	0.147	44.580	72.050
15	0.125	1.052	2.259	0.401	59.080	52.550
16	0.109	1.419	2.107	0.134	47.080	72.080
17	0.209	1.548	0.846	1.095	4.180	45.180
18	0.212	1.207	1.874	0.891	11.880	35.850
19	0.268	1.427	0.574	0.444	18.480	21.980
20	0.170	0.936	6.084	0.338	22.480	41.050
21	0.259	1.574	1.074	0.805	46.280	59.750
22	0.209	1.225	2.175	0.448	5.480	45.950
23	0.233	1.409	1.540	0.505	11.080	50.050
24	0.198	1.103	1.951	0.507	10.580	28.050
25	0.253	1.231	0.629	0.987	18.180	41.280
26	0.241	1.420	1.004	1.259	9.080	40.080
27	0.205	1.170	3.244	0.444	15.780	46.080
28	0.244	1.171	7.554	0.171	20.080	42.080
29	0.226	1.422	3.354	0.974	8.980	74.780
30	0.220	1.742	1.933	2.744	20.480	70.380
31	0.157	0.157	2.064	0.158	59.580	80.080
32	0.167	1.243	0.503	0.401	17.580	58.080
33	0.142	1.278	4.060	0.551	4.180	47.450
34	0.313	2.062	39.103	1.492	10.480	99.250
35	0.121	0.986	1.201	0.529	21.580	45.950
36	0.139	1.197	1.041	0.237	10.480	47.450
37	0.145	1.340	0.948	0.809	4.580	34.450
38	0.177	1.194	0.844	0.809	13.880	40.880
39	0.141	1.308	0.339	0.542	17.280	28.550
40	0.167	1.407	0.387	1.074	1.780	29.380
41	0.174	1.415	1.005	1.252	1.080	32.150
42	0.225	1.709	5.883	0.301	14.080	89.080
43	0.230	1.517	0.584	0.598	13.180	54.280
44	0.247	1.514	1.472	0.503	25.180	75.380
45	0.165	1.621	0.441	1.458	10.880	22.250
46	0.171	1.513	0.294	0.551	14.780	23.450
47	0.197	1.597	4.644	0.401	38.480	56.950
48	0.205	1.459	1.459	0.474	40.580	51.080
49	0.249	1.830	0.644	1.405	5.080	33.050
50	0.204	1.414	3.178	1.175	35.980	80.550
51	0.230	1.613	0.658	1.458	22.080	47.080

TABLE C-1, (continued)

1963 INPUT- OUTPUT NUMBER	PROPORTION OF SALES TO CONSUMERS	PROPORTION OF SALES TO GOVERNMENT	PROPORTION OF SALES TO INVESTMENT	PROPORTION OF SALES TO EXPORTS	PERCENT OF GROSS OUTPUT	PERCENT OF GROSS OUTPUT IMPTS/GO
1	5.	0.294	0.0	1.339	0.604	0.604
2	7.	0.174	0.0	1.550	0.225	0.007
3	8.	0.400	0.0	8.903	0.113	0.111
4	9.	0.429	0.0	5.592	0.273	0.544
5	10.	0.040	0.0	16.125	0.061	0.539
6	11.	0.0	0.0	7.491	0.007	0.007
7	12.	0.265	0.019	25.444	0.107	0.061
8	13.	0.598	0.058	10.385	0.212	0.111
9	14.	0.590	0.348	17.010	0.011	0.000
10	15.	0.588	0.588	17.010	0.011	0.000
11	16.	0.588	0.588	10.433	0.202	0.0
12	17.	0.588	0.588	14.808	0.363	0.000
13	18.	0.588	0.588	26.127	0.285	0.003
14	19.	0.588	0.588	25.071	0.708	0.001
15	20.	0.588	0.588	19.539	0.284	0.010
16	21.	0.588	0.588	19.539	0.361	0.013
17	22.	0.588	0.588	19.539	0.361	0.013
18	23.	0.588	0.588	19.539	0.361	0.013
19	24.	0.588	0.588	19.539	0.361	0.013
20	25.	0.588	0.588	19.539	0.361	0.013
21	26.	0.588	0.588	19.539	0.361	0.013
22	27.	0.588	0.588	19.539	0.361	0.013
23	28.	0.588	0.588	19.539	0.361	0.013
24	29.	0.588	0.588	19.539	0.361	0.013
25	30.	0.588	0.588	19.539	0.361	0.013
26	31.	0.588	0.588	19.539	0.361	0.013
27	32.	0.588	0.588	19.539	0.361	0.013
28	33.	0.588	0.588	19.539	0.361	0.013
29	34.	0.588	0.588	19.539	0.361	0.013
30	35.	0.588	0.588	19.539	0.361	0.013
31	36.	0.588	0.588	19.539	0.361	0.013
32	37.	0.588	0.588	19.539	0.361	0.013
33	38.	0.588	0.588	19.539	0.361	0.013
34	39.	0.588	0.588	19.539	0.361	0.013
35	40.	0.588	0.588	19.539	0.361	0.013
36	41.	0.588	0.588	19.539	0.361	0.013
37	42.	0.588	0.588	19.539	0.361	0.013
38	43.	0.588	0.588	19.539	0.361	0.013
39	44.	0.588	0.588	19.539	0.361	0.013
40	45.	0.588	0.588	19.539	0.361	0.013
41	46.	0.588	0.588	19.539	0.361	0.013
42	47.	0.588	0.588	19.539	0.361	0.013
43	48.	0.588	0.588	19.539	0.361	0.013
44	49.	0.588	0.588	19.539	0.361	0.013
45	50.	0.588	0.588	19.539	0.361	0.013
46	51.	0.588	0.588	19.539	0.361	0.013
47	52.	0.588	0.588	19.539	0.361	0.013
48	53.	0.588	0.588	19.539	0.361	0.013
49	54.	0.588	0.588	19.539	0.361	0.013
50	55.	0.588	0.588	19.539	0.361	0.013
51	56.	0.588	0.588	19.539	0.361	0.013
52	57.	0.588	0.588	19.539	0.361	0.013
53	58.	0.588	0.588	19.539	0.361	0.013
54	59.	0.588	0.588	19.539	0.361	0.013
55	60.	0.588	0.588	19.539	0.361	0.013
56	61.	0.588	0.588	19.539	0.361	0.013
57	62.	0.588	0.588	19.539	0.361	0.013
58	63.	0.588	0.588	19.539	0.361	0.013
59	64.	0.588	0.588	19.539	0.361	0.013
60	65.	0.588	0.588	19.539	0.361	0.013
61	66.	0.588	0.588	19.539	0.361	0.013
62	67.	0.588	0.588	19.539	0.361	0.013
63	68.	0.588	0.588	19.539	0.361	0.013
64	69.	0.588	0.588	19.539	0.361	0.013
65	70.	0.588	0.588	19.539	0.361	0.013
66	71.	0.588	0.588	19.539	0.361	0.013
67	72.	0.588	0.588	19.539	0.361	0.013
68	73.	0.588	0.588	19.539	0.361	0.013
69	74.	0.588	0.588	19.539	0.361	0.013
70	75.	0.588	0.588	19.539	0.361	0.013
71	76.	0.588	0.588	19.539	0.361	0.013
72	77.	0.588	0.588	19.539	0.361	0.013
73	78.	0.588	0.588	19.539	0.361	0.013
74	79.	0.588	0.588	19.539	0.361	0.013
75	80.	0.588	0.588	19.539	0.361	0.013
76	81.	0.588	0.588	19.539	0.361	0.013
77	82.	0.588	0.588	19.539	0.361	0.013
78	83.	0.588	0.588	19.539	0.361	0.013
79	84.	0.588	0.588	19.539	0.361	0.013
80	85.	0.588	0.588	19.539	0.361	0.013
81	86.	0.588	0.588	19.539	0.361	0.013
82	87.	0.588	0.588	19.539	0.361	0.013
83	88.	0.588	0.588	19.539	0.361	0.013
84	89.	0.588	0.588	19.539	0.361	0.013
85	90.	0.588	0.588	19.539	0.361	0.013
86	91.	0.588	0.588	19.539	0.361	0.013
87	92.	0.588	0.588	19.539	0.361	0.013
88	93.	0.588	0.588	19.539	0.361	0.013
89	94.	0.588	0.588	19.539	0.361	0.013
90	95.	0.588	0.588	19.539	0.361	0.013
91	96.	0.588	0.588	19.539	0.361	0.013
92	97.	0.588	0.588	19.539	0.361	0.013
93	98.	0.588	0.588	19.539	0.361	0.013
94	99.	0.588	0.588	19.539	0.361	0.013
95	100.	0.588	0.588	19.539	0.361	0.013



TABLE C-2 1958-1973 DATA, \$1 UN INPUT-OUTPUT INDUSTRIES

1963 INPUT	REAL GROSS OUTPUT	EMPLOYMENT	LABOR PRODUCTIVITY	FABRICS/FABRIQUE	FABRICS/FABRIQUE	FABRICS/FABRIQUE	CAPITAL/UNIT OUTPUT	TOTAL OUTPUT
1	0.892	0.565	1.474	2.084	1.469	1.869	1.395	1.395
2	1.310	0.945	1.374	2.160	1.495	1.905	1.401	1.401
3	1.072	0.846	1.562	1.962	1.562	1.495	1.401	1.401
4	1.271	0.712	1.712	1.764	1.764	1.495	1.401	1.401
5	2.605	1.500	1.722	1.764	1.764	1.495	1.401	1.401
6	1.762	1.034	1.719	1.719	1.719	1.495	1.401	1.401
7	1.125	0.842	1.261	1.261	1.261	1.495	1.401	1.401
8	1.054	0.804	2.158	2.158	2.158	1.495	1.401	1.401
9	1.354	0.824	2.154	2.154	2.154	1.495	1.401	1.401
10	3.804	1.424	2.471	2.471	2.471	1.495	1.401	1.401
11	1.260	0.714	1.753	1.753	1.753	1.495	1.401	1.401
12	5.926	1.414	3.472	3.472	3.472	1.495	1.401	1.401
13	3.044	0.949	3.079	3.079	3.079	1.495	1.401	1.401
14	1.251	0.867	1.844	1.844	1.844	1.495	1.401	1.401
15	1.251	0.867	1.772	1.772	1.772	1.495	1.401	1.401
16	1.472	1.004	1.921	1.921	1.921	1.495	1.401	1.401
17	1.745	1.075	1.825	1.825	1.825	1.495	1.401	1.401
18	1.221	0.912	1.912	1.912	1.912	1.495	1.401	1.401
19	2.024	1.042	1.942	1.942	1.942	1.495	1.401	1.401
20	0.867	0.525	1.451	1.451	1.451	1.495	1.401	1.401
21	1.059	0.794	2.454	2.454	2.454	1.495	1.401	1.401
22	3.052	1.711	1.711	1.711	1.711	1.495	1.401	1.401
23	1.903	1.135	1.477	1.477	1.477	1.495	1.401	1.401
24	1.044	1.126	1.500	1.500	1.500	1.495	1.401	1.401
25	2.422	1.457	1.457	1.457	1.457	1.495	1.401	1.401
26	1.563	0.942	1.423	1.423	1.423	1.495	1.401	1.401
27	2.422	1.457	1.457	1.457	1.457	1.495	1.401	1.401
28	1.279	0.841	1.445	1.445	1.445	1.495	1.401	1.401
29	3.841	1.580	1.445	1.445	1.445	1.495	1.401	1.401
30	2.423	1.204	2.384	2.384	2.384	1.495	1.401	1.401
31	1.040	1.040	1.848	1.848	1.848	1.495	1.401	1.401
32	1.040	0.949	1.571	1.571	1.571	1.495	1.401	1.401
33	2.494	1.423	1.423	1.423	1.423	1.495	1.401	1.401
34	5.104	1.207	1.207	1.207	1.207	1.495	1.401	1.401
35	0.452	0.397	1.442	1.442	1.442	1.495	1.401	1.401
36	2.577	0.550	1.442	1.442	1.442	1.495	1.401	1.401
37	2.094	0.747	1.442	1.442	1.442	1.495	1.401	1.401
38	2.094	0.747	1.442	1.442	1.442	1.495	1.401	1.401
39	2.094	0.747	1.442	1.442	1.442	1.495	1.401	1.401
40	2.094	0.747	1.442	1.442	1.442	1.495	1.401	1.401
41	1.044	0.944	2.217	2.217	2.217	1.495	1.401	1.401
42	1.217	1.044	1.501	1.501	1.501	1.495	1.401	1.401
43	2.065	0.944	2.217	2.217	2.217	1.495	1.401	1.401
44	1.492	0.944	1.943	1.943	1.943	1.495	1.401	1.401
45	2.094	0.747	1.442	1.442	1.442	1.495	1.401	1.401
46	2.094	0.747	1.442	1.442	1.442	1.495	1.401	1.401
47	1.457	0.764	1.764	1.764	1.764	1.495	1.401	1.401
48	2.442	1.124	1.124	1.124	1.124	1.495	1.401	1.401
49	2.442	1.124	1.124	1.124	1.124	1.495	1.401	1.401
50	1.548	1.010	1.010	1.010	1.010	1.495	1.401	1.401
51	1.958	1.126	1.126	1.126	1.126	1.495	1.401	1.401

TABLE C-1 (Continued)

	1963 T.P.U. COSTS PER UNIT	PRICE	LABORING PROPORTION	MATERIALS PROPORTION	CAPITAL PROPORTION	TOTAL COSTS PROPORTION	REAL NET OUTPUT /EMPLOYEE	CHANGE, REAL NET OUTPUT/ ESTABLISHMENT	MARKET PRICE
1	5.	1.703	0.069	0.760	0.036	0.869	3.001	1.157	1.707
2	6.	2.075	0.153	0.649	0.042	0.844	1.204	1.259	1.714
3	7.	1.450	0.072	0.907	0.059	0.934	4.075	3.531	1.717
4	8.	1.719	0.174	0.579	0.059	0.816	1.543	1.939	2.017
5	9.	1.642	0.108	0.731	0.053	0.892	1.706	2.059	2.057
6	10.	1.745	0.044	0.679	0.040	0.764	2.412	2.432	1.723
7	11.	1.672	0.051	0.879	0.037	0.947	3.194	2.800	2.017
8	12.	1.757	0.042	0.809	0.049	1.009	3.004	1.774	1.717
9	13.	1.400	0.170	0.553	0.060	0.783	2.624	1.804	2.017
10	14.	1.274	0.157	0.401	0.074	0.710	3.000	2.074	2.017
11	15.	1.434	0.075	0.764	0.045	0.867	2.700	0.459	2.017
12	16.	0.890	0.151	0.532	0.064	0.769	3.946	3.472	1.723
13	17.	1.227	0.157	0.507	0.108	0.851	2.934	1.801	1.707
14	18.	1.400	0.169	0.464	0.111	0.904	1.625	0.127	1.707
15	19.	1.904	0.202	0.675	0.077	0.850	1.450	-0.120	1.707
16	20.	2.545	0.169	0.774	0.047	0.932	1.041	-0.154	1.707
17	21.	1.754	0.215	0.575	0.074	0.804	1.555	0.157	1.707
18	22.	2.025	0.305	0.445	0.067	0.800	1.534	-0.101	1.707
19	23.	1.620	0.060	0.444	0.040	0.533	1.500	0.725	1.707
20	24.	1.722	0.204	0.507	0.135	0.970	1.070	0.100	1.707
21	25.	1.942	0.107	0.360	0.077	0.645	1.503	1.700	1.707
22	26.	1.404	0.259	0.574	0.042	0.904	1.777	0.177	1.707
23	27.	1.705	0.244	0.469	0.074	0.840	1.620	0.104	1.707
24	28.	1.416	0.234	0.545	0.130	0.930	1.607	0.100	1.707
25	29.	1.542	0.146	0.433	0.100	0.867	1.626	0.203	1.707
26	30.	1.077	0.329	0.441	0.097	0.867	1.204	0.404	1.707
27	31.	1.400	0.302	0.405	0.047	0.804	1.425	0.254	1.707
28	32.	2.215	0.174	0.006	0.001	0.902	1.007	0.292	1.707
29	33.	1.557	0.205	0.504	0.072	0.802	1.574	1.600	1.707
30	34.	1.305	0.270	0.532	0.067	0.809	1.524	0.493	1.707
31	35.	1.544	0.174	0.679	0.059	0.912	1.254	1.271	1.707
32	36.	1.704	0.225	0.503	0.063	0.871	1.471	0.979	1.707
33	37.	1.579	0.217	0.454	0.063	0.934	1.709	0.404	1.707
34	38.	0.922	0.142	0.531	0.114	0.841	3.024	0.901	1.707
35	39.	2.024	0.200	0.404	0.081	0.946	0.929	0.467	1.707
36	40.	1.032	0.160	0.700	0.074	0.734	1.244	0.509	1.707
37	41.	1.152	0.241	0.503	0.065	0.904	1.201	2.203	1.707
38	42.	1.701	0.199	0.615	0.084	0.901	1.244	1.359	1.707
39	43.	2.423	0.162	0.482	0.057	0.921	1.104	0.453	1.707
40	44.	1.450	0.265	0.506	0.041	0.871	0.913	2.174	1.707
41	45.	1.004	0.240	0.530	0.064	0.864	1.120	1.355	1.707
42	46.	1.731	0.115	0.444	0.142	0.771	3.300	1.044	1.707
43	47.	1.751	0.265	0.405	0.045	0.845	1.854	1.901	1.707
44	48.	1.697	0.337	0.390	0.097	0.824	1.000	1.211	1.707
45	49.	1.411	0.272	0.534	0.062	0.860	1.021	1.522	1.707
46	50.	2.254	0.210	0.414	0.050	0.870	1.451	1.702	1.707
47	51.	1.534	0.169	0.444	0.081	0.844	1.849	0.464	1.707
48	52.	1.703	0.214	0.577	0.074	0.809	1.444	1.402	1.707
49	53.	2.107	0.332	0.411	0.063	0.800	1.005	0.604	1.707
50	54.	1.540	0.253	0.530	0.047	0.900	1.005	1.404	1.707
51	55.	1.400	0.242	0.425	0.060	0.850	1.547	1.011	1.707