"Price Cost Margins and Market Structure" Revisited

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October 2002

(Chapter for a book "Competition, Monopoly and Corporate Governance: Essays in Honour of Keith Cowling", edited Michael Waterson, Edward Elgar, forthcoming 2003.)

1. Introduction

In the summer of 1972, I forget whether for six or eight weeks, Keith Cowling employed me on a project examining oligopoly. His original idea, inspired by Means (1962), was that there was a link between oligopoly behaviour and the rate of inflation. However, upon developing the model, it became clear that the link in theory was between the *change* in the degree of oligopoly and the change in pricing. I recall, when I returned to my parental home, telling my mother what I had been doing over those weeks- developing a model. She thought it pretty poor value. In retrospect, I cannot agree; indeed I wish I had more times spent a few weeks so productively! Eventually, this translated itself, via my PhD and a lot of work by Keith, into our joint paper, "Price-cost margins and market structure" (1976) which remains clearly the most-cited piece either of us have.

The model was one of the early elements to provide a rigorous theoretical underpinning for the cross- sectional structure- performance work so popular in industrial economics at that stage. It made two things clear, that the appropriate margin was a margin on revenue, not capital, and that an arguably appropriate measure of industry concentration was the Herfindahl. This latter finding led to a great deal of policy- driven work (e.g. Dansby and Willig, 1979; Farrell and Shapiro, 1980) and thus may have been one of the elements contributing to the use of the Herfindahl (or Hirschmann- Herfindahl, HHI) index in the landmark US Horizontal Merger guidelines, published in 1982 and revised 1984, 1992 and 1997[2].

The empirical work was also somewhat innovative. We explicitly recognised (albeit to a lesser extent than is nowadays considered the case) that structure- performance

studies were plagued by omitted variable problems, one of the most severe being the own price elasticity of demand. This we finessed by an approach, taking ratios of the formula across two years, which has echoes in the first-differencing that is used empirically these days to remove fixed effects.

Thus both theoretically and empirically, the study drew attention rather widely and has been cited nearly 200 times. It also provided an element in the construction of what, in Keith's view, is his most important work, *Monopoly Capitalism* (1982).

Of course, things move on. The "game theory revolution" has significantly impacted upon theoretical modelling in the area of oligopoly. Empirical studies of oligopoly behaviour still use in small part the basic framework (Genesove and Mullin, 1998). However, cross-sectional work has now gone rather out of style in favour of individual industry studies, in part for good reason. The policy pronouncements coming from cross-section studies were really rather heroic. The purpose of this short paper is essentially to put these two areas, empirics and policy, into the context of modern work in Industrial Economics.

2. Empirical Analysis

The relationship Cowling-Waterson used related the profit (plus fixed cost) to revenue ratio, $(\Pi + F)/R$, to the Herfindahl index, H, and the elasticity of demand (written as a positive number), e, as follows

$$(1)\ \frac{\Pi + F}{R} = \frac{Hm}{e}$$

Here m is related to q, the degree to which firms' actions are co-ordinated. Equation (1) was derived using a generalised Cournot-type model of fixed firm numbers, homogeneous products and constant marginal cost and is an intra-industry aggregation of the first order profit maximising condition for firm i

$$(2)\ p = c_i - q_i.P'(Q)q_i$$

where p is price, c_i is marginal cost, q_i is this firm's output and $P(Q)$ the inverse market demand curve. Under Bertrand behaviour, q (m) is zero, under Cournot one, whereas under joint profit maximisation, it is Q/q_i (1/H).

It is clear in general that a series of cross-sectional observations on profits and on concentration along the lines of

$$(3)\quad \frac{\Pi + F}{R} = a + bH$$

with the numerator of the left hand side measured as revenue minus variable costs, will yield a poor and uncertain fit given this model (even assuming industries with products approximating to homogeneous can be found)[3].

As we (amongst others) recognised, the coefficient b will be influenced by variations in the elasticity of demand and the degree of co-ordination across industry. If instead we take a ratio of (3) at two points in time

$$\left(\frac{\Pi + F}{R}\right)_t \Big/ \left(\frac{\Pi + F}{R}\right)_{t-1} = \frac{H_t m_t}{e_t} \Big/ \frac{H_{t-1} m_{t-1}}{e_{t-1}}$$

then if the elasticity of demand does not change over the period (or if all elasticities change proportionately) and behaviour does not change, but concentration does significantly, then we may hope to identify the relationship more precisely. Probably, this is a big "if", in particular as regards behaviour. But even if behaviour changes, but does so in a manner proportionate to the change in concentration, then we may hope to identify a relationship between changes in concentration and changes in the margin and to gain some insight into how powerful the relationship is. Thus, by taking the ratio, we eliminate some sources of unwanted variation across the sample.

More recently, concern has been expressed about the left-hand side of (3) and its meaning. The problems are related both to measurement and to interpretation. In terms of measurement, there is a difficulty over marginal costs and their relationship to variable costs. The upshot is that there is some unwillingness to view average

variable cost as equal to marginal cost. As Bresnahan (1989, p 1012), talking about the "new empirical industrial organization" [NEIO] puts it "Firms' price-cost margins are not taken to be observables…". Thus costs are commonly estimated or approximated using output data and factor prices as explanatory variables. Porter's (1983) study of a railroad cartel is a classic example of this approach.

More significant for the prevailing climate is the feeling that "Individual industries are taken to have important idiosyncrasies." (Bresnahan, 1989, p1012). Thus, cross-sectional studies are far too broad brush in their approach. Individual industries, or small, connected groups of industries, have become the common focus of study. In this context, especially when intra-industry data are available[4], conduct can be examined more directly.

Let us examine these arguments in a little more detail. The general relationship

$$(2)\; p = c_i - q_i.P'(Q)q_i$$

coming from a generalised Cournot homogeneous oligopoly model can be viewed as a form of supply relationship, dependent of course on demand through the term $P¢(Q)$ and also on behaviour through q. This suggests an approach of estimating demand in the industry jointly with the supply relationship. Estimation is ideally performed using data for individual firms, but can also be employed using time series data at industry level (e.g. Porter, 1983). In such approaches, marginal costs (c_i) are sometimes estimated as a function

$$c_i = c(w_i, ...)$$

of factor prices, rather than being assumed or gathered from recorded data.

In order to identify the supply relationship, it is necessary that there are factors which shift the demand curve. However, under at least some theories of oligopoly, the value of q that is expected itself depends upon the state of demand. Therefore, in such cases, the conduct parameter is not independent of the instruments used to estimate it, resulting in a biased estimate. This is the essence of the Corts (1999) critique of the "supply-demand" type estimation procedure for obtaining an average conduct parameter. This issue is investigated by Genesove and Mullin (1998) in the context of the US sugar refining industry. They compare direct estimates of q from (2) (at the

industry level) using known cost information with indirect estimates, where cost is estimated as a function of the input price of raw sugar, and find that in the latter there is some underestimation of q due to correlation between the margin and a demand instrument, High Season. However, they view their results as "reassuring" so far as the NEIO approach is concerned since the effects are minor.

Furthermore, Genesove and Mullin investigate recovered estimates of cost where a particular value of the conduct parameter is assumed[5]. Clearly, from (2), if a more monopolistic form of conduct than is warranted by actual behaviour is assumed, estimated costs will be too low, and vice versa. Indeed, they find in their sample there is a significant bias to estimated costs as a result of inappropriate assumptions regarding conduct.

Of course, although there has traditionally been significant emphasis on structure-performance studies to provide an underlying rationale for competition policy by investigating the degree of market power present in industry, this has never been the only agenda for industrial economists. The newer techniques, such as the supply-demand or NEIO estimation methodology discussed above, are amenable to examining quite different issues. Interesting examples include Petrin's (2002) evaluation of the welfare impact of the introduction of the "minivan" in the US, or Davis' (2001) study of provision of movie theatres relative to the optimum. In these cases, adoption of a specific demand structure in which differentiated products can be placed enables an explicit social welfare evaluation to be made.

But the fact that there are new and interesting questions does not eliminate older issues from consideration. Really, it seems to me, one of the reasons for the almost total abandonment of the broad cross-section study is a widespread lack of belief in the importance or immutability of structure[6]. This follows on from a game-theoretic revolution in which structure has been very much downplayed at the expense of the "anything can happen" demonstrations of game theory. It is only really Sutton (e.g. 1990, 1991) who has taken a consistent line in promoting the idea of looking to establish broad-brush predictions valid for a wide variety of games rather than being concerned with the minutiae in modelling of individual cases. Thus Sutton (together

with former students such as Symeonidis, 2002), keeps alive the broad cross-sectional study, although chooses to use it to examine the determinants of concentration, not its effect. Nevertheless, since the mirror of the distribution of concentration levels is the pricing structure of the industries concerned[7], his work has implications for the relationship between structure and performance.

The other major and perhaps more significant reason for abandonment of the broad cross-sectional study is probably the recognition that Census industries are not good approximations to markets. This issue will be discussed in the section below.

3. Policy- Relevance

As Schmalensee (1988) points out in his survey of the field as a whole, "Despite [their] problems, inter-industry studies have an important role to play. It is difficult to design broad public policies, such as antitrust and tariff policies, without a feel for the main economy-wide relations (structural or otherwise) among affected markets." (p.649). This point is worth dwelling upon and is particularly true as regards the guidelines relating to competition policy, which are now a feature in many jurisdictions, having spread from the United States. However broad-brush the relationship when evaluated across industries, without the background of an overall cross-industry framework, it is difficult to see how such a structure for policy could have been developed.

Unfortunately, in real life the occupants of industries are not symmetrically- sized firms. Thus, a summary statistic for the industry needs construction. Like any such summary measure, the Herfindahl, used in the US merger guidelines, conceals as well as reveals information about industry structure. In particular, as Farrell and Shapiro (1990) point out, an increase in an industry's Herfindahl is not equivalent to an increase in the monopoly welfare loss. This is essentially because improvements in efficiency can increase the Herfindahl. Taking the simple example of the Cournot model, and measuring social welfare in the conventional manner, without regard to distribution, they show that the following relationship holds true

$$(4)\ \operatorname{sgn} dW = \operatorname{sgn}\left[d \ln Q + 1/2 d \ln H\right]$$

Thus an increase in welfare is generated by an *increase* in the Herfindahl, all other things equal, because in the Cournot model, a fall in costs for one firm causes it to gain share at the expense of the other players.

This itself leads to problems of interpretation of any cross-sectional aggregate relationship between (changes in) profitability and (changes in) concentration. The link may be from co-ordination to high profits for all, or it may be through superior efficiency on the part of some players in the market, leading to their domination of the industry. In the first case (where (4) is not the appropriate formula, although it illustrates the issues), an increase in the Herfindahl is accompanied by a fall in industry output, whereas in the second case, this is not necessarily so.

Really, this is the focus of the Demsetz (1973) criticism of these cross industry structure-performance estimations, although he did not put it into an explicitly welfare-theoretic framework. Without examining what happens *within* an industry, it is not possible in general to come to a conclusion regarding the social welfare impact of any relationship estimated. Demsetz did look at intra-industry relationships (large and small firm groups), but his results are not conclusive since he claimed that higher profits amongst large firms demonstrated they were more efficient, whereas in fact other explanations are equally plausible (Scherer, 1979). Thus his results are consistent with, but not determinative of, an efficiency explanation.

An at least partially successful attempt to use a cross-sectional approach alongside intra-industry data is provided by Clarke, Davies and Waterson (1984). They used Census size class distributions to provide intra-industry variation and combined this with a cross-sectional investigation of the results from the margin-size regressions, finding some evidence for a positive relationship between the degree of concentration and the level of apparent collusion. The approach was to take intra- industry size distribution information across a panel of years, with which to estimate the following relationship for each industry j

$$(5)\left(\frac{p - AVC}{p}\right)_i = a + bs_i + cs_i^2 + u_i$$

where the i subscript now refers to the size group, rather than an individual firm. A simple rewriting of (2) suggests the following theoretical link
where a is the "degree of implicit collusion", since a value of 1 corresponds to

$$(6)\left(\frac{p-c_i}{p}\right)=\frac{a}{e}+\frac{(1-a)s_i}{e}$$

collusion, whereas a value of zero coincides with Cournot behaviour. Thus estimates of a were retrieved from the equations (5) and employed in a subsequent cross-sectional estimation relating a to the Herfindahl index (finding some evidence of a positive link).

Of course, this approach has a major weakness, namely that it takes as given that the Census SIC categories correspond to economically meaningful markets. Indeed, in their work, Clarke et al. find that only a minority of their industries are amenable to the second step (relating a to the Herfindahl) of the above analysis. In many cases, the results of estimating equation (5) are not explainable either in terms of the theory underlying (6) or extensions of it to cover economies of scale and product differentiation. For example, in some industries it is the smallest firm sizes that have the largest margins. Therefore the implication is that in many cases either the industry does not operate at all along the lines of the generalised Cournot-type structure underpinning (2), or the Census industry is not coincident with a single "real" market. This widespread view that Census industries have serious problems from the standpoint of meaningful markets is a major factor in the decline in popularity of Census- based cross-sectional work.

A more direct, but narrower, approach to competition issues is to estimate the degree of co-ordination in behaviour directly within a supply-demand relationship of the type (2) described earlier designed for a specific industry. In either case, the welfare interpretation is more straightforward than from Cowling-Waterson, since unambiguously the higher the degree of collusion the greater the welfare loss.

More recently, there has been a trend to estimate structural supply-demand relationships then simulate the social effects of a merger directly, commonly under the assumption of given behaviour. Assuming the demand relationship examined has an

explicit interpretation in terms of consumer utility, meaningful welfare analysis can take place even for example in industries where products are differentiated (so concentration measures are ambiguous). A good example of this genre is Nevo (2000)[8], who having estimated the demand and supply relationships, simulates the impact of mergers in the US ready-to-eat breakfast cereal industry, some of which have taken place. To estimate demand, he uses data across most major (24) brands across 45 cities and 20 quarterly time periods, using the approach to differentiated products popularised by Berry, Levinsohn and Pakes (1995). He then recovers estimates of marginal costs from the supply relationship[9] and calculates the post-merger equilibrium following the simulated merger. It is then possible for instance, to see how great a reduction in marginal cost would be required in order to offset the price rise caused by the merger. In principle at least, such an econometric investigation could take place over the period within which the decision by competition authorities regarding the merger takes place.

However, intriguing though they are, the question examined by such studies is fundamentally different from the agenda underlying Cowling and Waterson (1976), Clarke et al. (1984) and the many similar studies. The more recent writings are concerned with answering a question: Given that a particular merger is to be investigated, should it be allowed to take place? The earlier work was addressed to a much broader question: How might one decide *which if any* merger proposals to investigate further? This was the issue that concerned the authors of the landmark 1982 Merger Guidelines in the US (guidelines that have significantly influenced jurisdictions outside the US)[10]. The key principles these enunciated come through in the following passage

"A merger is unlikely to create or enhance market power or to facilitate its exercise unless it significantly increases concentration and results in a concentrated market, properly defined and measured." (US DOJ/ FTC, 1997, at 1.0).

Thus market definitions, and a filter relating to market concentration, are at the heart of the guidelines. The early work was not very careful about the definition of market, but it was clearly addressed to the question of establishing if there was a broad link between concentration and performance. In that sense, given the continued importance of the Guidelines, it retains the significance of an underlying influence.

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[1] I am grateful to Margaret Slade and Lawrence White for useful discussions on some of the issues presented here.

[2] See e.g. Willig (1991) for a discussion of the intellectual antecedents and Muris (2002) for example regarding the impact of the guidelines. One clear influence on the guidelines was Stigler (1964).

[3] Notice that we assumed the link was from structure to performance, despite the fact that (2) derives from an equilibrium relationship, with both sides endogenous.

[4] This is true in by no means all examples of this genre.

[5] This, in more complex form, is one thing Berry, Levinsohn and Pakes (1995) do.

[6] Another, less legitimate, reason may be that issues arising in particular antitrust cases in which they develop a consulting interest drive the agendas of key individuals.

[7] A point exploited to great effect by Bresnahan and Reiss (e.g. 1989) and others in their inferences about structure derived from observations of distributions of outlets across markets.

[8] See also Pinkse and Slade (2002) and Smith (2002), for example, for alternative approaches to the same issue.

[9] This requires an assumption regarding behaviour, justified in this case on the basis of previous modelling.

[10] The current Chairman of the FTC, Timothy Muris (2002), writes: "The Justice Department's 1982 Merger Guidelines fundamentally changed the way we think about mergers and about how we should formulate competition policy. Not simply a matter of national significance, the analytical ripples of the 1982 Merger Guidelines have covered the globe."