DISCUSSION PAPER SERIES

No. 2816

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ISSN 0265-8003

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Discussion Paper No. 2816
May 2001

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CEPR Discussion Paper No. 2816

May 2001

ABSTRACT

Price Competition, Innovation and Profitability: Theory and UK Evidence*

This Paper examines the effect of price competition on innovation, market structure and profitability in R&D-intensive industries. The theoretical predictions are tested using UK data on the evolution of competition, concentration, innovation counts and profitability over 1952–77. The econometric results suggest that the introduction of restrictive practices legislation in the UK had no significant effect on the number of innovations commercialized in previously cartelized R&D-intensive manufacturing industries, while it caused a significant rise in concentration in these industries. In the short run profitability decreased, but in the long run it was restored through the rise in concentration.

JEL Classification: L11, L13, O31
Keywords: cartels, innovation, manufacturing, market structure, price competition, profitability, UK

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* I thank seminar participants at CEPR, the London Business School, Nuffield College (Oxford), Universidad Carlos III (Madrid) and the University of Athens for helpful comments on earlier versions of this paper. This research is part of a forthcoming book on the effects of cartel policy on firm strategy and market structure in British industry (Symeonidis, G., 2001, *The Effects of Competition*, MIT Press).This Paper is produced as part of a CEPR Research Network on 'Product Markets, Financial Markets and the Pace of Innovation in Europe', funded by the European Commission under the Research Training Network Programme (Contract No. HPRN-CT-2000-00061).

Submitted 14 March 2001

NON-TECHNICAL SUMMARY

This Paper examines the effect of price competition on market structure, technological innovation and profitability in R&D-intensive industries. The theoretical predictions are tested using econometric evidence from a unique 'natural experiment' that occurred in the UK between the late 1950s and the early 1970s. As a result of the 1956 Restrictive Trade Practices Act, restrictive agreements between firms covering a wide range of industries were cancelled. This caused an intensification of price competition in many industries during the 1960s. These can be compared to a 'control' group of industries which had not been subject to agreements significantly restricting competition and were therefore not affected by the Act.

This Paper is part of a larger research project that combines game-theoretic models of competition with econometric and case-study evidence on the effects of competition on firm strategy, market structure and industry performance following the introduction of cartel legislation in the UK (see Symeonidis, 2001). This natural experiment provides us with a way to bypass two very difficult problems that have been endemic in empirical studies of the effects of competition. The first problem is how to measure the intensity of competition. The second problem is how to unravel the complex links between competition and other variables, such as market structure, innovation and profitability, given that all these variables may simultaneously affect one another, thus making the identification of one-way causal effects very difficult.

The present set-up allows us to bypass these difficulties because a change in the intensity of competition across a wide range of industries was in this case induced by an exogenous and measurable institutional change. Thus there is no need to measure the intensity of competition directly. All that is required is a clear distinction between industries affected by the shift in cartel policy and industries not affected. Moreover, the exogeneity of the institutional change allows us to largely overcome any concerns about potential biases in the estimated impact of competition caused by the existence of complex links between competition and other variables. In other words, the identification of one-way causal effects is a feasible task in the present context.

The Paper begins by introducing a theoretical framework for the analysis of competition in R&D-intensive industries. In contrast with much of the previous literature on the links between market power and innovation, I do not regard concentration as a proxy for market power. In fact, I explicitly treat market structure, innovation and profitability as jointly determined endogenous variables. The model is sufficiently general to allow for a variety of possible outcomes regarding the effect of price competition on innovation. This approach is motivated by the inconclusiveness of much of the existing literature on the links between market power and innovation and by the idea that strong results on this question can only be obtained by imposing a considerable amount of structure on theoretical models. Instead, I focus on weaker predictions that can be derived from a more general model and relate to the *joint* behaviour of innovation, market structure and profitability following an intensification of price competition in industries with significant R&D expenditures. The theory predicts that (i) if the effect of price competition on innovation is not negative, then its effect on concentration must be positive, and (ii) if price competition has no significant effect on innovation but causes concentration to rise in the long run, then it should have a negative effect on profitability in the short run (i.e. before any change in market structure occurs) and an ambiguous effect in the long run.

The empirical results are consistent with the theory. I find that the intensification of price competition following the introduction of the 1956 Act in the UK had no significant effect on firms' innovative output and a strong positive effect on concentration in R&D-intensive industries. Also, profitability declined in the short run, but in the long run it was restored through the rise in concentration.

The lack of any overall effect of competition on innovation across industries is perhaps not very surprising in light of the mixed theoretical results on this much debated issue. It should also be clarified that I am referring here only to innovations *produced* by firms. This Paper does not examine whether price competition may have an effect on variable costs and thus on productivity through increased effort by managers and workers, more efficient internal firm organisation, the adoption of better technologies, or otherwise.

The links between price competition, market structure and profitability identified in this Paper are not unique to R&D-intensive industries. Furthermore, the rise in concentration across industries following the introduction of cartel policy in the UK was to a large extent driven by a fall in the number of firms. (See Symeonidis, 2001 for a detailed analysis of these issues.) An interesting implication of these results is that free entry is not incompatible with collusion. In fact, the results suggest that in long-run equilibrium most cartels are likely to result in excess entry rather than excess profits (relative to the absence of collusion), and hence cartel laws will reduce the number of firms rather than their profits.

1. Introduction.

The relationship between market power and innovative activity has been a much-debated issue ever since Schumpeter's pioneering work. On the one hand, large firms in concentrated markets are often seen as the main engines of technological progress, for reasons that relate to the optimal scale for R&D and innovation, appropriability conditions, and the presence of financial constraints. On the other, it is often argued that the lack of competition may lead to inertia and managerial slack, and hence to a reduced level of innovative activity. The view of competition as a stimulus to innovation is probably the majority position among policy-makers today. However, the theoretical and empirical support for this view is far from clear. In fact, despite a large literature on the subject, the issue of the links between market power and innovation is still not settled.

In particular, while some theoretical studies on the interaction between short-run and long-run decisions in oligopoly have found that less short-run competition may result in, or be associated with, more investment in the long-run variable (such as R&D expenditure), others have shown this result to depend on particular functional forms or the specification of the collusive technology (see Yarrow 1985, Fershtman and Gandal 1994, Ziss 1994, Fershtman and Muller 1986, Davidson and Deneckere 1990, Fershtman and Pakes 2000).[1] Recent endogenous growth models of innovation

[1] A common assumption in this context is that firms cannot collude in the long-run variable, whatever their short-run conduct. This is often justified in theoretical studies by a reference to the fact that long detection and retaliation lags hinder the stability of collusion. As long-term decisions take time to implement, the reaction to rivals' behaviour is relatively slow, i.e. there are relatively long retaliation lags; hence collusion in long-run variables will be relatively difficult to achieve. In the case of R&D, an additional argument is that deviations from agreed levels of R&D expenditure are difficult to observe, at least as long as R&D

have produced mixed results, although they have succeeded in identifying specific conditions that may determine whether competition is associated with more or less innovation (see Aghion and Howitt 1997, 1998, Aghion et al. 1999).

On the empirical side, recent surveys (e.g. Cohen 1995, Symeonidis 1996) suggest that there is no strong general relationship between market power and innovation, and that industry characteristics such as appropriability conditions, demand, and especially technological opportunity explain much more of the cross-industry variation in R&D intensity and innovation than market power or market structure. It is therefore all the more interesting that the results from a number of UK studies of innovation seem to be less ambiguous. In particular, both Geroski (1990) and Blundell et al. (1995) have found evidence of a positive link between competition and innovation in British industry.

One of the key methodological problems in the empirical literature on the determinants of innovation has been the relative neglect, until recently, of the endogeneity of market structure. Thus an implicit assumption in many studies has been that market power is greater in concentrated markets. This implies that to analyse the effect of market power on innovation one can simply examine the effect of concentration or firm numbers on innovation. This approach is not justified in a framework where market structure is seen as endogenous. For example, there is evidence that more intense price competition can result in higher concentration (Sutton 1991, Symeonidis 2000a, 2001). This and other recent work suggests that (i) market structure cannot be taken as a proxy for market power in cross-industry studies and (ii) both innovation and market structure must be seen as jointly

cooperation does not take the form of a joint venture; in other words, detection is also problematic, and this further hinders collusion on R&D.

determined endogenous variables, a view which is consistent with much recent work on technological change and the evolution of market structure in R&D-intensive industries (see Utterbach and Suarez 1993, Klepper 1996, Klepper and Simons 1997, Sutton 1996, 1998, among others).

There have been numerous attempts to address one or the other of these issues in econometric work. Some studies of innovation have used measures of competitive pressure or market power other than market structure, such as import penetration. Others have tried to address the endogeneity issue by using instrumental variable techniques or by estimating simultaneous equation systems. However, none of these studies has explicitly modelled innovative output and concentration as jointly determined by some exogenous measure of competition (as well as other variables).

This paper takes this later approach. In addition, it introduces a third endogenous variable of interest, namely profitability. The paper mainly focuses on industries where R&D and technological innovations are important for competition. Equilibrium in such industries is seen as involving the joint determination of market structure, R&D expenditure, innovative output, and profits. The exogenous factors in this framework include short-run conduct, which is referred to as 'price competition' irrespective of whether firms set prices or quantities. The intensity of price competition, as defined here, can be thought of as an inverse measure of the degree of collusion, but it is not equivalent to the price-cost margin, which is endogenous.

I begin by presenting a theoretical framework. The model is sufficiently general to allow for a variety of possible outcomes regarding the effect of price competition on innovation. This approach is motivated by the idea that strong results on this question can only be obtained by imposing a considerable amount of structure on theoretical models. Such results are often of limited generality or depend on features of the model that are difficult to observe or measure across industries. My approach is also motivated by the inconclusiveness of much of the existing literature on the links between market power and innovation. Instead, I focus on weaker predictions that can be derived from a more general model and relate to the joint behaviour of innovation, market structure and profitability following an intensification of price competition in industries with significant R&D expenditures.

The second part of the paper analyses empirical evidence on the evolution of competition, concentration, innovation counts and profitability in the UK over 1952-1977. This particular period offers the opportunity to study a unique natural experiment. The introduction of the 1956 Restrictive Trade Practices Act led to the registration and subsequent abolition of restrictive agreements between firms and the intensification of price competition across a range of manufacturing industries. These can be compared to an equally large number of industries which had not been subject to agreements significantly restricting competition and were therefore not affected by the legislation.

To carry out this comparison I explicitly model market structure, innovative output and profitability as endogenous variables in reduced-form equations derived from the game-theoretic model. I make no attempt to analyse the interaction between these variables using a simultaneous-equation approach, since the available data are simply not sufficient for obtaining proper identification of the equations in such a system. I use the panel structure of the data to control for industry-specific effects as well as for key determinants of innovation, such as technological opportunity, that are likely to be correlated with measures of competition but are relatively stable over time. Moreover, I explicitly focus on long-run effects, although I also compare these to short-run effects. Finally, and perhaps most importantly, I by-pass the need to measure or proxy the intensity of price competition, since I use information on a major exogenous institutional change that significantly affected the competitive environment facing UK firms in several industries.

The econometric results from the analysis of a panel data set of manufacturing industries suggest that the intensification of price competition following the 1956 legislation had no significant effect on the number of innovations commercialised by firms in R&D-intensive industries affected by the Act, while it caused a rise in concentration in these industries. It also caused profitability to decline in the short run, i.e. prior to any adjustment in market structure; in the long run profitability was restored through the rise in concentration.

2. Theoretical framework.

Consider an R&D-intensive industry where each firm produces one or more varieties of a differentiated product. Competition can be modelled by means of a three-stage game (Sutton 1991, 1998, Symeonidis 2000b, 2000c). At stage 1 firms decide whether or not to enter the industry at an exogenously given cost of entry *f*. At stage 2, each firm *i* chooses to incur a cost R_i, which represents expenditure on process or product R&D. The results of R&D are measured by an index of "innovative output" I_i, which is also realised at stage 2 of the game. A rise in I_i enhances product quality or reduces the marginal cost of production; either way, it affects the firms' objective functions at the final-stage subgame. Finally, at stage 3 firms simultaneously set prices or quantities.

The equilibrium of the third-stage subgame can be represented by a vector of payoffs $\pi_i(S, N, h, t, I_1, ..., I_i, ..., I_N, \boldsymbol{c_1}, ..., \boldsymbol{c_i}, ..., \boldsymbol{c_N})$, where S is market size, N is the number of firms that have entered at stage 1, h is a measure of the degree of horizontal product differentiation, $\boldsymbol{c_i}$ is a vector of parameters specific to firm i and independent of N and the I_i's, and, finally, t is a measure of the intensity of price competition.[2] The interpretation of t is that, for any given N, π_i will depend on the firms' pricing strategies, which will in turn partly depend on exogenous institutional factors such as the climate of competition policy or the degree of economic integration. The assumption of the exogeneity of the intensity of price competition t is motivated by the empirical context of the present study, which provides a clear empirical test of the theory by way of a reduced-form analysis of the effects of a change in the main determinant of t during the period under study, namely the 1956 Restrictive Trade Practices Act.[3]

In the benchmark case of symmetric firms the equilibrium gross profit of firm i can be written as $\pi_i(S, C, h, t, I_i, I_{-i})$, where C is any concentration measure whose

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[2] In R&D-intensive industries the degree of horizontal product differentiation depends not only on demand characteristics, such as the extent of diversification of users' preferences and the level of transport costs, but also on technological factors – namely the availability of alternative research paths leading to different varieties of the product or associated with different sub-markets within the industry, as well as the extent of scope economies in R&D across the various research paths (see Sutton 1998). For our present purposes, it will be sufficient to subsume all these influences – which can be regarded as exogenous – within the concept of horizontal product differentiation. This is not inconsistent with Sutton's emphasis on the trade-off between spending on R&D to enhance the quality of existing products and spending to develop new products, and the implications of this trade-off for market structure in technologically progressive industries. In the present model, the incentive to provide variety increases (and the equilibrium level of concentration should normally fall) as h rises.

[3] Thus the exogeneity of the intensity of price competition is probably justified in the present context for two reasons. First, the key determinant of changes in firms' short-run conduct during the period under study was the exogenous change in cartel policy (see section 3 below). Second, the state of competition prior to the introduction of the 1956 Act is likely to have been mainly a function of exogenous industry-specific factors (see the discussion in section 4).

value increases in $1/N$ for given I_i's (and depends only on N when $I_i = I_j$, $\forall i, j, j \neq i$), such as the concentration ratio. I will assume that π_i is increasing in C, S and h, and decreasing in t. Moreover, I will make two assumptions about the effect on π_i of changes in own and rival innovative output:

Assumption R1. π_i is increasing and strictly concave in I_i.

Assumption R2. π_i is non-increasing in I_j, $\forall j \neq i$.

At stage 2 each firm chooses R_i to maximise its net profit $\pi_i - R_i - f$, given the choices of the other firms and the number of firms that have entered at stage 1. Now assume, for simplicity, that the choice of a level of expenditure R_i by firm i entirely determines the index of innovative output of that firm, given the fundamental (and exogenous) technological characteristics of the industry. That is, let innovative output be determined by the function $I_i = I(e, R_i)$, where e is a measure of technological opportunity (hence I_i is increasing in e for any given value of $R_i > 0$). Note that this function implies that there are no R&D spillovers. This assumption is made for simplicity and could be relaxed without changing the key results of the model provided that spillovers are not very large so that the gross profit π_i remains non-increasing in R_j, $\forall j \neq i$.[4] A key implication of the above innovation production function

[4] To see this, note that the R&D of firm j has two opposing effects on the gross profit of firm i. On the one hand, an increase in the R&D of firm j raises its innovation index and thus increases its market share and gross profit at the expense of firm i's market share and gross profit. This negative R&D externality is always present in the model. On the other hand, when there are positive R&D externalities (spillovers), there is also another effect: an increase in the R&D of firm j raises the innovation index of firm i and thus causes the gross profit of that firm to increase at the expense of firm j's own gross profit. The results of the model are not altered as long as this second effect does not dominate the first, i.e. as long as

is that R and I are essentially interchangeable choice variables. In other words, it makes no difference whether the equilibrium of the second stage subgame is defined in terms of a set of conditions for the optimal choice of the R_i's or in terms of a set of conditions for the optimal choice of the I_i's. Finally:

Assumption R3. I_i is increasing and weakly concave in R_i, with $dR_i/dI_i < d\pi_i/dI_i$ at $R_i = 0$, and there exists a level of R_i such that $d\pi_i/dI_i = dR_i/dI_i$ for any given N and set of R_j's.

The weak concavity of the innovation function is in fact a stronger condition than what is required, but it is consistent with the empirical evidence that suggests the presence of constant or diminishing returns to scale in the production of innovations (see, for example, Kamien and Schwartz 1982, Bound et al. 1984, Hausman et al. 1984). The second part of Assumption R3 ensures that there exists a level of $R_i > 0$ such that $d\pi_i/dR_i = 1$, $\forall i$; this is a necessary condition for the existence of an interior solution to the firm's problem of choosing the optimal innovation index (or the optimal level of R&D expenditure).

The first-order condition for the optimal choice of R_i by firm i is $d\pi_i/dI_i = dR_i/dI_i$. At any symmetric equilibrium, we have $I_i = I$ (and hence $R_i = R$), $\forall i$, so the first-order condition for firm i can be written as:

$$\frac{d}{dI_i}\pi_i(S, C, h, t, I_i, I_{-i}) = \frac{d}{dI_i}R_i(e, I_i) \qquad \text{at} \quad I_i = I_{-i} = I. \tag{1}$$

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spillovers are not too large. It is interesting in this respect that Geroski (1994) found no evidence of significant R&D spillovers in British industry during the period that I analyse in this paper.

Equation (1) defines the level of R&D expenditure incurred by each firm, and the level of innovative output, as a function of the number of firms that have entered at stage 1. Note that at stage 2 of the game the I_i's and R_i's are the Nash equilibrium outcomes of a non-cooperative game, whatever the value of t at the final stage. This assumption is consistent with the empirical evidence, which suggests that collusion with respect to R&D is rare, even in industries where firms collude on price (see section 3 below).[5]

At stage 1, the free-entry condition is assumed to hold whatever the value of t and is given by

$$\pi_i = \pi_i(S, C, h, e, t, R_i, R_{-i})\Big|_{R_i = R_{-i} = R} = R + f, \qquad \forall i, \tag{2}$$

where, for simplicity, C is treated as a continuous variable. Note that the gross profit has been written as a function of the R_i's and e (rather than the I_i's) in equation (2). This way of expressing the profit function will often be convenient in the present analysis. Concentration C^*, the number of firms N^*, R&D expenditure R^* and innovative output I^* at the symmetric equilibrium are defined by the two necessary

[5] It is, of course, true that firms may find it easier to collude on R&D when R&D cooperation takes the form of a joint venture. However, as pointed out in section 3 below, there was hardly any R&D cooperation of that form in British industries that practiced price-fixing in the 1950s. More generally, allowing for collusion on R&D in the present model would probably increase the likelihood that a breakdown of collusion (affecting all choice variables) leads to a rise in R&D and innovations. A rise in R&D in this case might also imply a rise in the net profit of each firm, for a given number of firms. Proposition 1 below, which is a prediction on the joint effect of price competition on innovations and market structure, would have to be modified to take that possibility into account. If, however, a breakdown of collusion had no effect on R&D in the more general model, Proposition 1 should still unambiguously hold.

conditions (1) and (2). Since I am here concerned with comparative statics results, I will assume the existence of a unique equilibrium with $N^* \geq 2$ and $R^* > 0$, $I^* > 0$.

Little can be said in general about the comparative statics of I^* and C^* individually without imposing more structure on the model. In particular, an intensification of price competition can lead to more, less or no change in R&D expenditure and innovative output. This is because a rise in t can increase, decrease, or leave unchanged the incentive to spend on R&D. More specifically, a firm's incentive to spend on R&D depends on the total derivative $d\pi_i/dR_i$, and the effect of a rise in t on this derivative is ambiguous. Moreover, the effect on market structure can also be uncertain: since both sides of the free entry condition will be affected by a change in t, it is not clear in what direction C^* should change to restore the free entry condition. Nevertheless, it is possible to derive a general result regarding the *joint* behaviour of I^* and C^* following a change in t, as the following proposition (adapted from Symeonidis 2000b) shows.

Proposition 1. *If an increase in the intensity of price competition t causes either an increase or no change in R* (and therefore also in I*), then concentration C* must rise.*

Proof. It will be convenient to write gross profit directly as a function of the R_i's in what follows. From assumptions R1 and R3, we know that π_i is strictly concave in R_i. Moreover, from R2 and R3, π_i is non-increasing in R_j, $\forall j \neq i$. The gross profit of firm i at the initial equilibrium can be written as

$$\pi_0^* = \pi(R_i = R_0^* \mid S, C_0^*, h, e, t_0, R_{-i} = R_0^*) = R_0^* + f,$$

while the gross profit at the new equilibrium is

$\pi_1^* = \pi(R_i = R_1^* \mid S, C_1^*, h, e, t_1, R_{-i} = R_1^*) = R_1^* + f$.

Also, the profit of firm i at the initial equilibrium values of t and N^* when firm i sets $R_i = R_1^*$ while all other firms set $R_j = R_0^*$ is:

$\pi' = \pi(R_i = R_1^* \mid S, C_0^*, h, e, t_0, R_{-i} = R_0^*)$.

The proof is by contradiction. Assume that $C_1^* \leq C_0^*$ and $R_1^* \geq R_0^*$ following a rise of t from t_0 to t_1. Since π_i is increasing in C, decreasing in t and non-increasing in R_j, $\forall j \neq i$, we obtain $\pi_1^* < \pi'$. Also, since the function $\pi_i(R_i \mid S, C_0^*, h, e, t_0, R_{-i} = R_0^*)$ is concave in R_i and its slope is equal to 1 at $R_i = R_0^*$, its slope is smaller than 1 at all points between R_0^* and R_1^*. Hence $\pi' - \pi_0^* < R_1^* - R_0^*$. Combining the two inequalities, we obtain $\pi_1^* - \pi_0^* < R_1^* - R_0^*$. This is, however, impossible, since net profit must be zero at equilibrium and therefore we must have $\pi_1^* - \pi_0^* = R_1^* - R_0^*$. Hence it must be the case that C^* falls if R^* rises or does not change following a rise in t. ⊠

The intuition for Proposition 1 is as follows. Starting from a zero-profit symmetric equilibrium, an increase in t unambiguously reduces net profit below zero, for given C and R_i's. For the zero-profit condition to be restored at a new symmetric equilibrium, net profit must rise. Now suppose that the increase in t has also caused all firms to spend more or exactly the same on R&D. This cannot increase net profit because (i) an increase in own R&D has a stronger effect on fixed cost than on gross profit when starting from a long-run equilibrium with $d\pi_i/dR_i = 1$, $\forall i$ (owing to the concavity of the gross profit function with respect to R_i), so it reduces gross profit, (ii) if own R&D does not change, there is no effect on profit, and (iii) an increase or no change in rival R&D cannot increase own profit. In these circumstances, then, there is only one way net profit can increase, namely through a rise in concentration.

Note that Proposition 1 is a prediction on the joint effect of price competition on *firm* innovation and market structure. Can anything be said on the joint effect of price competition on *industry* innovation and market structure? In particular, is it possible to rule out the joint outcome $\Delta(N^*I^*) \geq 0$ and $\Delta N^* \geq 0$ ($\Delta C^* \leq 0$), where ΔI^*, ΔN^* and ΔC^* denote the change in I^*, N^* and C^*, respectively? This joint outcome can occur in one of two ways: either through $\Delta I^* \geq 0$ and $\Delta N^* \geq 0$; or through $\Delta I^* < 0$ and $\Delta N^* \geq 0$, with the rise in N^* offsetting the fall in I^*, so that N^*I^* rises. The former case is ruled out by Proposition 1. The latter case cannot be ruled out on the basis of the assumptions we have made, but it does seem very improbable. To obtain $\Delta N^* \geq 0$ following a rise in t, R^* needs to fall considerably. If it falls only by a small amount, it will not offset the direct negative effect of the rise in t on gross profit, so N^* will not rise. But since a significant fall in R^* is required for even a small rise (or no change) in N^*, it is very difficult to obtain $\Delta(N^*I^*) \geq 0$ together with $\Delta N^* \geq 0$ ($\Delta C^* \leq 0$) following a rise in t.

Thus two implications can be drawn for the empirical analysis of innovation and market structure at the industry level. First, if industry innovative activity increases or does not change following a rise in the intensity of price competition, then it is very likely that concentration has risen. Second, if we actually observe that more price competition causes a joint effect *other than* $\Delta(N^*I^*) \geq 0$ and $\Delta C^* \leq 0$, then Proposition 1 will have been confirmed, since the joint outcome $\Delta(N^*I^*) \geq 0$ and $\Delta C^* \leq 0$ is a necessary condition for the joint outcome $\Delta I^* \geq 0$ and $\Delta C^* \leq 0$. The empirical results on the evolution of industry innovation and market structure in section 4 can therefore be used both to test Proposition 1 and to examine whether it is possible to establish an empirical regularity which implies a slightly stronger constraint on the space of outcomes than Proposition 1.

I now turn to the competition effect on the price-cost margin, which, given the symmetry of the model, is the same for each firm and for the industry as a whole. The results derived below are for the special (but quite plausible) case where price competition has no significant effect on *industry* innovative output and a positive long-run effect on concentration.

Proposition 2. *(i) If an increase in the intensity of price competition t has no effect on industry innovation in the short run, i.e. before any change in C*, then gross profit must fall in the short run. (ii) If an increase in t has no effect on industry innovation and causes C* to rise in long-run equilibrium, then the effect on the price-cost margin in long-run equilibrium is ambiguous.*

Proof. Consider first the effect of a rise in t in the short run, i.e. prior to any change in C^*. If N^* and N^*I^* are unchanged, then gross profit must fall due to the rise in t. In the long run, C^* rises, and since N^*I^* remains unchanged, I^*, and hence R^* must rise. From the free-entry condition, it follows that firm gross profit must also rise. At the same time, sales revenue at the firm level will change. If it rises, this may or may not offset the rise in gross profit. Thus the price-cost margin can either rise or fall. ⊠

It should be emphasised that, although part (i) of Proposition 2 focuses on gross profit, a similar result can be expected for the price-cost margin, since sales revenue should normally rise, for given N and R, following a rise in t. Part (ii) of the proposition is largely driven by the requirement that firms make zero net profit in long-run equilibrium irrespective of firm conduct. Note that it is precisely through the adjustment of market structure following a rise in t that gross profit and the price-cost margin can begin to rise after their short-run fall until firms again cover their fixed costs in long-run equilibrium.[6]

The theory should now be confronted with the empirical evidence. The first question to be asked is what was the effect of the 1956 Act on innovations in British industry. This is a question on which the present theory is deliberately silent. Provided that we observe a non-negative effect, the next question will then be whether concentration did indeed rise, as suggested by the present theory. The final question relates to the effect of competition on profitability in the short run and in the long run.

3. Competition in British manufacturing industry.

Explicit restrictive agreements between firms were widespread in British industry in the mid-1950s: nearly half of manufacturing industry was subject to price-fixing. As a result of the 1956 legislation, these agreements were abandoned. This section briefly comments on the evolution of price competition in UK manufacturing and describes the construction of the competition data. A fuller account can be found in Symeonidis (1998, 2001).

The 1956 Act required the registration of restrictive agreements, including verbal or even implied arrangements, on goods. Registered agreements should be abandoned, unless they were successfully defended by the parties in the newly created Restrictive Practices Court as producing benefits that outweighed the presumed detriment (or unless they were considered by the Registrar of Restrictive

[6] Clearly, these results rely heavily on the symmetry assumption. However the mechanism that drives the results will also operate in the presence of asymmetries, and it is not clear how it would be offset by other effects. In any case, this can be left as an open question, to be tested against the empirical evidence.

Trading Agreements as not significantly affecting competition). Because the attitude of the Court could not be known until the first cases had been heard, the large majority of industries registered their agreements rather than dropping or secretly continuing them. The first agreements came before the Court in 1959 and were struck down. This induced most industries to voluntarily abandon their agreements rather than incur the costs of a Court case with little hope of success. Most agreements were cancelled between 1959 and 1963.

Many agreements provided for minimum or fixed producer prices. In general, there were no restrictions on media advertising or R&D expenditure. In some industries there was patent pooling or exchange of technical information between the parties, but only in one case is there any evidence that these schemes may have involved the joint determination of R&D (this industry is not in my sample). Also, there were no significant restrictions on entry in most cartelised industries.

To what extent did the intensity of price competition increase following the abolition of cartels? Case-study evidence (for example, Swann et al. 1973, 1974) suggests that most agreements had been effective prior to cancellation, the parties typically accounted for a large fraction of the market, and there were a number of factors that limited outside competition in many industries. This evidence also indicates that prices generally fell and/or trade discounts increased in previously cartelised industries, although in many cases this occurred several years after the formal cancellation of the agreement. Hence, while the impact of the 1956 Act on competition was not equally significant in all previously cartelised industries, most of these industries did experience, sooner or later, a significant increase in the intensity of price competition as a result of the legislation. It seems then legitimate to think of this evolution as a change of competition regime induced by an exogenous institutional change.

The main source of data on competition were the agreements registered under the 1956 Act. A number of other sources were also used to identify unregistered agreements or agreements modified before registration, including various Monopolies Commission reports, the Board of Trade annual reports from 1950 to 1956, and unpublished background material for the Political and Economic Planning (1957) survey of trade associations.

The approach to modelling the competition effect in the present paper involved distinguishing between those industries with a change of competition regime following the 1956 Act and those without a change in regime. All industries in the sample were classified according to their state of competition in the 1950s on the basis of three criteria: the reliability of the data source; the types of restrictions; and the proportion of an industry's total sales covered by products subject to agreements and, for each product, the fraction of the UK market covered by cartel firms.

In particular, the various types of restrictions were classified as significant, non-significant or uncertain, according to their likely impact on competition. Next, an industry was classified as collusive in the 1950s if the products subject to *significant* restrictions accounted for more than 50% of total industry sales. It was classified as competitive if the products subject to *significant or uncertain* restrictions accounted for less than 20% of industry sales. And it was classified as ambiguous in all remaining cases.[7] All industries with ambiguous state of competition in the 1950s (as

[7] In fact, most industries classified as competitive were free from any restrictive agreements. Similarly, most industries classified as collusive had agreements covering all industry products. Small variations in the cut-off points (in particular using 10% instead of 20%, or using 40% or 70% instead of 50%) do not significantly affect the results reported in section 4,

well as a few with ambiguous state of competition in the late 1960s and early 1970s) were then excluded from the sample, and the dummy variable *CHANGE* was defined, which takes the value 1 for industries with a change in competition regime sometime after 1958 and 0 otherwise.

4. Econometric models and results.

The econometric analysis in this paper is based on a comparison of those industries affected by the 1956 Act with a control group of industries not affected. I will use several different samples of industries in separate regressions for the various endogenous variables. The reason for this approach is that industry definitions across the different statistical sources are often difficult to match.[8]

The theoretical analysis of section 2 suggests estimating the following reduced-form models:

$$Innovations = I(S, f, h, e, t),$$

$$Concentration = C(S, f, h, e, t),$$

$$\Pr ofitability = P(S, f, h, e, t),$$

where the variables are as defined in section 2. In addition to the variables included in the above reduced forms, other factors, including macroeconomic fluctuations, may have affected innovations and concentration during the period examined in a more or

or indeed the samples used. The use of a continuous competition measure instead of cut-off points has proved impractical for a variety of reasons (see Symeonidis 2001 for an extensive discussion).

[8] A joint estimation would not increase efficiency, given that the equations include the same explanatory variables, and is impractical given that it is difficult to match the industry definitions across statistical sources.

less uniform way across industries. Time dummies will be included among the regressors to control for these. One variable not explicitly included in the model but generally thought to be an important determinant of innovation is the degree of appropriability of the outcome of R&D (which is related to the degree of spillovers). This variable and two others explicitly included in the model, namely the degree of horizontal product differentiation h and technological opportunity e, are very difficult to measure. However, it is not unreasonable to assume that these variables will be relatively stable over a period of 10 or 20 years for the large majority of industries, and so they will be largely captured by the industry effects in the panel data models estimated below. As far as technological opportunity is concerned, the ranking of manufacturing sectors in terms of R&D intensity tends to be very similar between countries and across time periods, which is consistent with the view that technological opportunity is relatively stable at the sector level. This is certainly less obvious for three-digit industries and even less so for four-digit industries, but then again changes in technological opportunity will only tend to increase the 'noise' in the results, as long as they are not correlated with changes in the intensity of competition or other variables.

The innovations data come from the SPRU survey of significant innovations commercialised by UK firms during 1945-1983 (see Townsend et al. 1981, Robson et al. 1988, Geroski 1994, for extensive discussions of these data). The level of industry aggregation is usually the three-digit, and sometimes the four-digit level (see the Appendix for further details on these data). Of course, any attempt to count the number of significant innovations is subject to some arbitrariness and possible biases in the evaluation procedure. Fortunately, in the case of the SPRU data, these biases seem to affect more the comparability of innovative activity across industries than the comparability over time for any given industry. This is not a serious problem for the present analysis because I use panel data, so differences across industries in the measurement of innovative output become part of the industry-specific effects and do not affect the results of interest. One issue that arises with respect to the use of the SPRU data is the choice of time periods. Although the data record the number of innovations for every year, it is necessary to group the innovation counts into somewhat longer periods so that they can be matched with Census-based figures on sales revenue that are available only at roughly five-year intervals during the 1950s and 1960s. Using five-year periods to group the innovation data is therefore an obvious choice, but this still leaves open the question of exactly which periods to use. Data on sales revenue are available for 1954, 1958, 1963, 1968, and then from 1970 onwards. Now it seems reasonable to assume that sales revenue in any given year t is a measure of average market size between year t – 2 and t + 2. It is also reasonable to assume that R&D expenditure in any given year is influenced by market conditions in that year. So if sales revenue in year t is a measure of average market size between t – 2 and t + 2, it can also be thought of as a determinant of R&D spending between t – 2 and t + 2. The key question is: what is the time lag between R&D spending and the commercialisation of innovations?

It is well known that the time lag between the beginning of a research project and the commercialisation of innovations varies greatly. Estimates of the *average* lag across industries range from one to four years (see Mansfield et al. 1971, Pakes and Schankerman 1984, Acs and Audretsch 1988). There is also evidence, at least for the UK, that R&D expenditure tends to increase toward the end of a research project (Schott 1976). One way of linking R&D spending and innovation counts which is often used in the empirical literature is to hypothesise that the bulk of R&D spending for innovations commercialised at year t takes place at a certain year t – x, where x is an average across industries and types of innovations. What should be the value of x? If the average lag between the beginning of a project and the commercialisation of innovations is 1-4 years, the average lag between the bulk of R&D spending and the commercialisation of innovations would be 1-2 years. On the assumption of a one-year lag, the number of innovations commercialised in year t is determined, on average, by market conditions in year t – 1. Thus sales revenue in year t (which, as already pointed out, is a measure of average market size between year t – 2 and year t + 2) should be matched with the number of innovations commercialised between year t – 1 and year t + 3.

The time periods chosen for grouping the innovations data in this paper are therefore 1952-1956, 1957-1961, 1962-1966, 1967-1971, and 1972-1976. These were matched with data on sales revenue (and capital intensity) for 1953 (my estimates), 1958, 1963, 1968, and 1973, respectively. While the assumption of a one-year innovation lag seems reasonable and has also been used in previous studies using the SPRU data (e.g. Blundell et al. 1995), I also experimented with an alternative set of time periods for grouping innovation counts, chosen on the assumption of an average two-year lag between the bulk of R&D spending and the commercialisation of innovations. This implied matching innovation periods 1953-1957, 1958-1962, 1963-1967, 1968-1972 and 1973-1977 with sales revenue data for 1953, 1958, 1963, 1968 and 1973, respectively. Both sets of results will be reported below, although they are, in fact, very similar.[9]

[9] Note that the introduction of an innovation in the UK by a domestic firm does not necessarily imply that this firm has actually *produced* the innovation; often it may have simply imported it. The SPRU data report, for each innovation, the country of origin, and, although there are serious doubts regarding the reliability of this information (see Townsend

The basic econometric model for innovation counts is

$$Inn_{it} = f(\alpha_i + \beta_1 \ln Sales_{it} + \beta_2 \ln(K/L)_{it} + \beta_3 Y58 + \beta_4 Y63 + \beta_5 Y68 + \beta_6 Y73 + \beta_7 CHANGE * Y58 + \beta_8 CHANGE * Y63 + \beta_9 CHANGE * Y68 + \beta_{10} CHANGE * Y73).$$

The dependent variable is the total number of innovations introduced by firms in any given industry and period, defined in two different ways. In particular, *INN1* is defined assuming a one-year innovation lag; thus the time periods used for grouping innovations to construct *INN1* are 1952-56, 1957-61, 1962-66, 1967-71 and 1972-76. *INN2* is defined on the basis of two-year innovation lag; i.e. the time periods for grouping the innovations are in this case 1953-57, 1958-62, 1963-67, 1968-72 and 1973-77.

The independent variables are defined as follows. 'Sales' is either total sales revenue by UK firms deflated by the general producer price index (*SS*) or total sales revenue deflated by an industry-specific producer price index (*DS*). *K/L* is the capital-labour ratio, defined at the three-digit industry level; this is a proxy for setup cost.[10] *Y58*, *Y63*, *Y68* and *Y73* are time dummies corresponding to time periods 1952-56, 1957-61, etc when *INN1* is used, or to 1953-57, 1958-62, etc when *INN2* is used. Finally, the interaction terms capture any differences in the evolution of innovation

et al. 1981), the country-of-origin indicator can be used to check the robustness of the results to alternative definitions of the dependent variable. The results from regressions excluding innovations reported in the SPRU data as having originated outside the UK were very similar to the results reported in Table 1 below.

[10] An alternative proxy, namely the capital stock divided by the number of plants, *K/N*, gave similar results to those in Table 1. Note that, since the model includes industry-specific effects, one need not assume that the *K/L* or *K/N* are adequate measures of setup cost. All that is required is that the *change* in *K/L* or *K/N* is an adequate measure of the *change* in setup cost, an assumption that seems quite plausible.

counts after 1958 between industries with a change in competition regime (*CHANGE* = 1) and industries without such a change (*CHANGE* = 0). Thus the coefficient on *CHANGE**Y58* measures the effect of the 1956 Act on the introduction of innovations during the period 1957-61 (or 1985-62); the coefficient on *CHANGE**Y63* measures the effect of the 1956 Act on the introduction of innovations during the period 1962-66 (or 1963-67); and so on. The benchmark period is 1952-56 (or 1953-57). Details on variable definition and data sources are provided in the Appendix.

The potential endogeneity of *CHANGE* is an obvious cause for concern. Put simply, the potential problem is that whatever difference one may observe in the evolution of industries with *CHANGE* = 1 and industries with *CHANGE* = 0 during the 1960s may be to some extent due to unobserved characteristics that differ between the two groups of industries rather than to any effect of the legislation. Ideally, one would want to be able to test formally for exogeneity, but this is impossible here since there are no appropriate instruments for *CHANGE*. How serious is the problem? At first sight, it may seem quite serious in the present case, since the initial conditions regarding innovative activity are very different in two groups. For instance, the mean number of innovations introduced between 1952 and 1956 is 1.8 (with a standard deviation of 2.2) in 10 R&D-intensive industries with a subsequent change in regime and 5.4 (with a standard deviation of 4.9) in 20 R&D-intensive industries without such a change. However, the picture is still very much the same 10 and even 20 years later, when competition had generally been established in previously collusive industries. For instance, the mean number of innovations introduced between 1972 and 1976 is 2.3 in the 10 industries with *CHANGE* = 1 and 7.9 in the 20 industries with *CHANGE* = 0. This suggests that cartelisation in the 1950s is correlated with some variable that strongly influences innovative activity but remains relatively stable over time in any given industry. An obvious candidate is technological opportunity. This, as indeed any other time-invariant industry-specific characteristic, is captured in the present model by the industry-specific effects. Hence that should not lead to any endogeneity bias, provided that one uses a specification that accounts for the correlation between regressors and industry effects.

Moreover, there is an additional indirect check: one can examine the way the two groups of industries evolve between the period 1952-1956 and the period 1957-1962. Because of the time lag between the launch of an R&D project and the commercialisation of innovations and the fact that in several R&D-intensive industries price-fixing agreements were not abandoned until the early 1960s or sometimes even later, any differences in the evolution of innovative activity between the two groups of industries during these years cannot be attributed to the effect of the 1956 Act. If there is an endogeneity problem with *CHANGE* that might affect the regression results, then there should be evidence that the two groups of industries evolved in different ways during the 1950s. The simplest way this can be checked is by looking at the descriptive statistics. The mean number of innovations introduced in 10 R&D-intensive industries with a subsequent change in competition regime is 1.8 during 1952-1956 and 3.1 during 1957-1961. The respective figures for 20 R&D-intensive industries without a change in regime are 5.4 and 7.6. Thus there is no evidence that innovative activity changed in different ways in the two groups of industries during the 1950s: the increase in innovations between 1952-1956 and 1957-1961 can be attributed to a common time trend. As we will see below, this is confirmed in the context of the regression results by the fact that the coefficient on

*CHANGE*Y58* is not statistically significant.[11] We may therefore conclude that any estimated difference between the two groups in later periods should be due to the 1956 legislation. Alternatively, if it turns out that there is no difference between the two groups in later periods, we may conclude that the legislation had no effect on innovation.

Three key features of the innovations data have directed the choice of econometric specification. First, it is evident from the raw data that there is very considerable overdispersion (cf. the means and standard deviations reported above). The overdispersion is so pronounced that it will certainly persist in any Poisson regression: the conditional variance of the dependent variable, although somewhat reduced through the inclusion of regressors, will remain larger than the conditional mean. In such circumstances, the standard Poisson model is not appropriate, even if one can partly control for heterogeneity through the use of fixed industry-specific effects (see Cameron and Trivedi 1998, Hausman et al. 1984). On the other hand, a random-effects Poisson model does allow for overdispersion, and a negative binomial model can provide valid results under overdispersion in all circumstances. Second, there is no 'excess zeros' problem in the present data: only in 18 out of 150 observations in the basic sample does the dependent variable *INN1* take the value 0. And third, as already mentioned, cartelised industries had, on average, a much lower mean number of innovations throughout the period than non-cartelised industries. This implies that *CHANGE* is probably correlated with the industry-specific effects,

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[11] A valid objection to this second type of check is that, if *CHANGE* were indeed endogenous, then the coefficient on *CHANGE*Y58* would itself be biased. However, this check is intended here as a mere confirmation of the picture that emerges from the descriptive statistics, not as a formal test.

so a random-effects model cannot be used. To obtain consistent estimates, the conditional fixed-effects negative binomial specification proposed in Hausman et al. (1984) will be used below.

A potential limitation of my specification for innovation counts is that, although it controls for fixed industry effects, it does not allow for any effect of past innovative activity within an industry or by firms in other industries on current innovation. A dynamic specification cannot be used here because of data limitations. However, the effect of past on current innovation – unlike the impact of industry-specific characteristics, which is captured by the industry effects – may not be as important in industry-level data as it often is in firm-level data. Moreover, while the production of innovations in an industry may be generally influenced by knowledge generated in other industries, Geroski (1994) – who worked with annual firm-level and industry-level innovations data for much of the period that I analyse in this paper – found no evidence of significant cross-industry (or indeed within-industry) R&D spillovers in UK manufacturing.

The first four columns of Table 1 present results for the basic sample of 30 R&D-intensive industries, i.e. industries with average R&D-sales ratio (RDS) during the period examined here higher than 1%. The main reason for focusing on R&D-intensive industries was the expectation that any systematic impact of price competition on the production of innovations might be more difficult to identify or less relevant in industries where R&D is not a key strategic variable. In any case, the last four columns of Table 1 contain results for a larger sample of 42 industries. For this sample, I have added to the basic sample a number of industries with average RDS < 1% but with relatively high numbers of innovations in the SPRU database.

There is no evidence of any significant effect of the intensification of price competition following the 1956 Act on innovations introduced in British industry, either in the short run or in the long run. The coefficients on all the interaction terms are statistically insignificant, even at the 20% level, in all regressions. An interesting feature of the results presented in Table 1 is the almost general failure of the explanatory variables in these regressions. Time-invariant industry-specific characteristics seem to account for much of the cross-industry variation in innovation counts. As already mentioned above, one variable that is almost certainly picked up by the industry effects is technological opportunity. In addition, a lot of variation in innovation counts seems to be due to variables difficult to measure or to observe, including random events. It is nevertheless reassuring that the coefficients on the market size proxies are often statistically significant at the 5% or 10% level in regressions using the larger sample. This suggests that, for the market size variables, the high standard errors may be due to the smallness of the samples used. A similar argument cannot be made with respect to the competition variable.[12]

Given these results, theory predicts that concentration must have risen in R&D-intensive industries affected by the 1956 Act. The impact on concentration across classes of industries, including R&D-intensive industries, has been analysed elsewhere (Symeonidis 2000a), so only a summary of the results will be given here. Concentration data at the four-digit level of aggregation are available for 1958, 1963,

[12] The rate of growth has sometimes been used instead of market size as a regressor in empirical models of innovation. To see whether this makes any difference to my results, I estimated a model including $\Delta \ln SS$ or $\Delta \ln DS$ among the regressors, defined for industry i and year t as the change in $\ln SS$ or $\ln DS$, respectively, in the five-year period preceding year t. The coefficients on these variables were everywhere highly non-significant, and the rest of the results did not change much. Note that, in order to use this alternative specification, the first-year observation for each industry had to be dropped.

1968 and 1975. The sample contains industries with an average or typical R&D-sales ratio (RDS) of more than 1% over the relevant period, excluding industries with ambiguous state of competition in 1958 (or, in a few cases, ambiguous state of competition in the late 1960s and early 1970s), as well as industries with a switch of regime but for which concentration data were not available for at least 1958, 1963 and either 1968 or 1975. This is an unbalanced sample, chiefly as a result of the fact that concentration data for 1958 are not available for a large number of R&D-intensive industries.

Descriptive statistics for the five-firm sales concentration ratio *C5* in 1958 for industries with and industries without a change in competition regime after 1958 suggest that there was no significant difference between the two groups: the mean *C5* is 0.704 for the 10 cartelised industries in the sample and 0.683 for the 23 non-cartelised industries with available data for that year. To compare the evolution of the two groups after 1958, I use the econometric model

$$\begin{aligned} Conc_{it} &= \alpha_i + \delta_1 \ln Sales_{it} + \delta_2 \ln(K/x)_{it} + \delta_3 Y63 + \delta_4 Y68 + \delta_5 Y75 + \\ &\quad \delta_6 CHANGE * Y63 + \delta_7 CHANGE * Y68 + \delta_8 CHANGE * Y75 + u_{it}, \end{aligned}$$

where ‘conc’ is either the four-digit industry five-firm concentration ratio *C5* or its logistic transformation logit*C5* = ln[*C5*/(1–*C5*)], ‘sales’ is either *SS* or *DS* (see above), *K/x* is either the three-digit industry capital-labour ratio *K/L* or the three-digit industry capital stock of the average plant *K/N*, *Y63*, *Y68* and *Y73* are time dummies for 1963, 1968 and 1975 respectively, and the interaction terms capture any differences in the evolution of concentration counts after 1958 between industries with a change in competition regime (*CHANGE* = 1) and industries without such a change (*CHANGE* = 0). Thus the coefficient on *CHANGE**Y63* measures the effect of the 1956 Act on concentration by 1963; the coefficient on *CHANGE*Y68* measures the effect by 1968; and the coefficient on *CHANGE*Y75* measures the effect by 1975. The benchmark year is 1958.

Table 2 presents results using a random effects specification. There are 10 industries with cancelled agreements in these samples. There is a strong positive competition effect on concentration in R&D-intensive industries: the coefficients on *CHANGE*Y68* and *CHANGE*Y75* are everywhere positive and statistically significant at the 5% level. Note that this is in contrast with the market size effect, which breaks down: the coefficients on the sales variables have conflicting signs and are nowhere significant at the 5% level. Most of the competition effect was realised between 1963 and 1968, although there seems to have been some effect even after 1968. Moreover, there has been practically no effect between 1958 and 1963.

The timing of these changes in market structure is very interesting since it has clear implications for the analysis of the evolution of profitability, to which I now turn. Industry price-cost margins for 1954, 1958, 1963, 1968 and 1973 can be constructed from information available in the individual reports of the Census of Production. The level of aggregation is sometimes the three-digit level and sometimes the four-digit level. Because of changes in industry definitions over time, the sample of industries for the analysis of profitability is unbalanced. The econometric model is

$$\begin{aligned} PCM_{it} &= \alpha_i + \gamma_1 \ln Sales_{it} + \gamma_2 \ln(K/x)_{it} + \gamma_3 Y54 + \gamma_4 Y63 + \gamma_5 Y68 + \gamma_6 Y73 + \\ &\quad \gamma_7 CHANGE*Y54 + \gamma_8 CHANGE*Y63 + \gamma_9 CHANGE*Y68 + \gamma_{10} CHANGE*Y73 + e_{it}. \end{aligned}$$

The price-cost margin *PCM* is defined as net output minus wages and salaries divided by sales revenue.[13] Note that this specification is very different from those typically used in 'traditional' studies of profitability, as it does not include a measure of market structure among the regressors. This is because it is a reduced-form equation derived from a theoretical model treating both market structure and profitability as endogenous. The theoretical predictions regarding the effect of price competition on profitability depend on allowing market structure to change to restore the long-run equillibrium. It is therefore important for testing these predictions that one does *no*t control for changes in market structure when specifying the profit equation.

Note that a simultaneous-equations approach cannot be used here because it is simply very difficult to find any variable that affects the number of firms and does not also influence profitability. Nevertheless, the reduced-form equations can still provide important insights on the interaction between market structure and profitability through a comparison of short-run and long-run effects of competition, as will be shown below.[14]

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[13] Using net output as the denominator of *PCM* gave results similar to those reported here.

[14] Let me also point out again that a dynamic panel data model cannot be used in the present context because of data limitations. As the years in my panel are separated by periods of four to five years, however, it is not clear that there should be any significant effect of lagged values of the endogenous variables because of adjustment lags or for other reasons. Also, note that the econometric specification that I use allows the competition effect to operate with a lag, and that setup costs are measured somewhat imprecisely anyway, so some of the effect of changes in setup costs is captured by the time effects. Finally, it could also be argued that, if lagged profitability was indeed an important explanatory variable in the model, its omission would show as significant serial correlation in the residuals. But there is no such evidence in the model estimated below.

The independent variables are the same as in the concentration regressions. *Y54*, *Y63*, *Y68* and *Y73* are time dummies for 1954, 1963, 1968 and 1973 respectively, and the interaction terms capture differences in the evolution of *PCM* after 1958 between industries with a change of competition regime and industries without such a change. Thus the coefficient on *CHANGE*Y63* (*CHANGE*Y68*, *CHANGE*Y73*) measures the effect of the 1956 Act between 1958 and 1963 (1968, 1973). The benchmark year is 1958, as the Act had little effect before then. The coefficient on *CHANGE*Y54* serves as a partial check of this presumption, as well as a check that the evolution of market structure during 1954-58 was not significantly different between the two groups of industries. As mentioned above in the context of the innovation regressions, this is an indirect check against the possibility of endogeneity bias caused by *CHANGE*.

Table 3 reports regression results for profitability from a random effects specification.[15] The coefficients on *CHANGE*Y68* and *CHANGE*Y73* are nowhere statistically significant, even at the 10% level. On the other hand, the coefficient on *CHANGE*Y63* is negative and statistically significant at the 5% level.[16] In other words, the price-cost margin declined, on average, between 1958 and 1963 in R&D-intensive industries with a change in regime, before recovering during 1963-1968. As mentioned above, it was between 1963 and 1968 that nearly all of the restructuring of

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[15] The Hausman test typically favours the random effects model. In any case, the results of the fixed effects model were similar.

[16] Moreover, the coefficient on *CHANGE*Y54* is nowhere statistically significant, even at the 20% level, suggesting that there was no difference in the evolution of market structure and profitability between the two groups of industries before 1958 (the benchmark year). Note that the coefficient on *CHANGE*Y63* has everywhere the same sign as the coefficient on *CHANGE*Y54*, which implies that, even if a differential trend between the two groups of

previously cartelised R&D-intensive industries occurred. The results reported in Table 3 are therefore fully consistent with those presented in Table 2: no significant effect on concentration by 1963, at which date several industries must have been in short-run disequilibrium with reduced margins (including industries where the adjustment of concentration to its long-run value was being delayed by a slow rate of depreciation of the capital stock); then a significant positive effect on concentration between 1963 and 1968, leading to a rise in margins in those industries.[17]

It could be argued that the profit equation estimated above does not adequately control for industry-specific factors that may cause departures from long-run equilibrium. Hence I also ran regressions including the variable $\Delta\ln SS$ among the regressors, defined for industry i and year t as the change in $\ln SS$ in the five-year period preceding year t. A disadvantage of this alternative specification was that the first-year observation for each industry could not be used, and this implied dropping all 1954 observations. In any case, the coefficient on $\Delta\ln SS$ was everywhere positive and sometimes statistically significant, but the rest of the results did not change.

5. Concluding remarks.

This paper has analysed the impact of price competition on innovative output, market structure and profitability in R&D-intensive industries. I have argued that strong theoretical predictions on the effect of competition on innovation cannot be made; however, it is possible to derive predictions on the evolution of market structure and

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industries had existed before 1958 and is imprecisely measured (because the 1954-1958 period is too short, say), this trend was, if anything, reversed during 1958-1963.

[17] A similar link between the evolution of market structure and that of profitability has been observed in all industries affected by the 1956 Act, not only in R&D-intensive industries. See Symeonidis (2001).

profitability that are conditional on the behaviour of innovative activity following a rise in the intensity of competition. The introduction of the 1956 Restrictive Trade Practices Act has provided a unique opportunity to address these issues in an empirical context. An econometric analysis based on a comparison between those industries affected by the legislation and those not affected has produced no evidence of any significant effect of price competition on innovation at the industry level and clear evidence of a strong positive effect on concentration in R&D-intensive industries. Also, profitability has fallen in the short run, but it has been restored in the longer term through the rise in concentration. The empirical evidence is consistent with the theory.

The lack of a clear effect of price competition on innovation is consistent with the overall picture emerging from those few previous empirical studies of the determinants of R&D or innovation that have used measures of competitive pressure other than market structure, such as Scherer and Huh (1992), Bertschek (1995), Geroski (1990), and Broadberry and Crafts (2000). However, the present paper has adopted a different approach than these previous studies. I have explicitly modelled market structure, innovative output and profitability as endogenous variables in reduced-form equations derived from a game-theoretic model. Moreover, I have by-passed the need to measure or proxy the intensity of price competition, since I have used information on a major exogenous institutional change that significantly affected the competitive environment facing UK firms in several industries. Of course, while the use of reduced-form equations is a powerful way to reveal overall effects, it may fail to capture some potentially interesting interactions between the variables of interest. For instance, it could be the case that innovations are often introduced by new entrants (as argued by Geroski 1994). An increase in the intensity of price competition could then cause innovations to fall by reducing new entry and thus the pool of potentially important innovators. At the same time, the intensification of price competition could induce existing firms to innovate more in order to avoid bankruptcy and exit. Hence a zero overall effect of competition on innovation could be consistent with a more complex mechanism than the one identified in this paper. Or it could be the case, as suggested by recent theoretical results on the competition-innovation relationship based on theoretical models of endogenous growth (Aghion and Howitt 1997, 1998), that competition reduces innovation under certain circumstances and increases it under different circumstances. Again, this could be consistent with the zero overall effect suggested by our analysis.[18] These various possible mechanisms and theoretical predictions from more specific models can only be tested with data far richer and more detailed than the data used in this paper.

The results on concentration and profitability are consistent with models that endogenise market structure by means of a free-entry condition and consequently emphasise the effect of firm conduct on structure rather than on performance, such as Selten (1984) and Sutton (1991). They suggest that, in long-run equilibrium and in

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[18] A third possibility might be that high concentration leads to low (high) innovation, everything else being equal, so that an increase in the intensity of competition, by causing concentration to rise, leads to a fall (rise) in innovations. At the same time, the intensification of price competition could induce firms to innovate more (less) at any given level of concentration. Again the overall effect could be zero, but the interaction of the various factors would be more complex than what is implied by the present analysis.

However, this interpretation does not seem to be consistent with the facts. Because market structure was slow to adjust in R&D-intensive industries affected by the 1956 Act, as shown in the concentration regressions of this paper, some insight on the interaction of innovation and market structure can be gained by comparing the short-run effect with the long-run effect of the 1956 Act. The results of Tables 2 and 3, taken as a whole, provide little evidence that the short-run effect of the Act on innovation, i.e. the effect before the adjustment of market structure, was any different than the long-run effect.

the absence of any institutional barriers to entry, cartels do not result in higher profits, but rather allow for excessive entry. Legislation prohibiting cartels will therefore reduce the number of firms rather than their profits.

It is also worth emphasising that this paper did not provide any direct test of the free-entry zero-profit condition. What has been tested here is not whether net profit is approximately zero in the long run for the marginal firm; this is a very difficult task without firm-level data on profits and capital costs. Moreover, as pointed out by Scherer and Ross (1990), the zero-profit condition is a rough approximation – especially for R&D-intensive industries, where firms' capabilities often change at a rate slower than the rate at which technology and demand conditions shift. In such industries, and perhaps also in others, it may be questionable whether a zero-profit equilibrium is actually achieved at any point in time.

What this paper has tested is whether a change in firm conduct has any effect on profit in the short run and in the long run. The results presented here (and in Symeonidis 2001) suggest that in R&D-intensive industries, as in almost all other industries, the level of excess profits in the long run does not depend on firms' pricing conduct, because of forces such as entry and exit that push industries towards the zero-profit equilibrium. It is mainly in this sense, I think, that the free-entry zero-profit condition is a useful approximation for the study of industries.

Table 1. Regression results for innovation counts. (Negative binomial conditional fixed effects estimation.)

	Dep. variable: *INN1*		Dep. variable: *INN2*		Dep. variable: *INN1*		Dep. variable: *INN2*	
ln*SS*	0.272 (0.155)	-	0.187 (0.162)	-	0.272 (0.138)	-	0.194 (0.142)	-
ln*DS*	-	0.166 (0.136)	-	0.112 (0.143)	-	0.183 (0.118)	-	0.134 (0.122)
ln*K/L*	0.154 (0.257)	0.073 (0.259)	0.072 (0.260)	0.025 (0.260)	0.188 (0.208)	0.128 (0.208)	0.097 (0.213)	0.060 (0.214)
Y58	0.255 (0.165)	0.272 (0.165)	0.334 (0.173)	0.336 (0.173)	0.190 (0.140)	0.209 (0.140)	0.243 (0.143)	0.251 (0.142)
Y63	0.190 (0.190)	0.249 (0.189)	0.191 (0.202)	0.229 (0.200)	0.223 (0.163)	0.279 (0.178)	0.224 (0.169)	0.261 (0.163)
Y68	0.098 (0.256)	0.204 (0.256)	0.263 (0.249)	0.326 (0.249)	0.117 (0.213)	0.204 (0.203)	0.244 (0.206)	0.298 (0.198)
Y73	-0.108 (0.331)	0.042 (0.331)	0.136 (0.321)	0.231 (0.319)	-0.117 (0.271)	0.005 (0.256)	0.059 (0.262)	0.134 (0.249)
*CHANGE*Y58*	0.254 (0.360)	0.257 (0.360)	0.282 (0.368)	0.292 (0.368)	0.114 (0.267)	0.119 (0.266)	0.333 (0.269)	0.339 (0.269)
*CHANGE*Y63*	-0.263 (0.406)	-0.285 (0.403)	-0.107 (0.412)	-0.122 (0.410)	-0.052 (0.273)	-0.073 (0.274)	-0.033 (0.287)	-0.047 (0.287)
*CHANGE*Y68*	0.075 (0.377)	0.031 (0.376)	-0.067 (0.388)	-0.093 (0.387)	-0.171 (0.279)	-0.189 (0.280)	-0.074 (0.283)	-0.086 (0.284)
*CHANGE*Y73*	0.180 (0.386)	0.112 (0.387)	0.065 (0.392)	0.022 (0.392)	-0.125 (0.290)	-0.151 (0.293)	-0.155 (0.301)	-0.169 (0.303)
constant	-0.799 (1.960)	0.508 (1.775)	-0.157 (2.009)	0.774 (1.781)	-0.615 (1.672)	0.427 (1.442)	0.059 (1.693)	0.779 (1.453)
Log likelihood	-233.01	-233.87	-241.91	-242.30	-333.94	-334.75	-340.31	-340.67
No. of industries	30	30	30	30	42	42	42	42
No. of industries with *CHANGE*=1	10	10	10	10	16	16	16	16
No. of observations	150	150	150	150	210	210	210	210

Note: Standard errors in parentheses.

Table 2. Regression results for concentration in R&D-intensive industries. (Random effects GLS estimation.)

	Dep. variable: *C5*				Dep. variable: logit*C5*			
ln*SS*	-0.019 (0.014)	-	-0.014 (0.014)	-	0.07 (0.12)	-	0.13 (0.12)	-
ln*DS*	-	-0.014 (0.014)	-	-0.010 (0.013)	-	0.12 (0.12)	-	0.18 (0.12)
ln*K/N*	0.075 (0.015)	0.075 (0.015)	-	-	0.61 (0.13)	0.61 (0.13)	-	-
ln*K/L*	-	-	0.064 (0.017)	0.065 (0.017)	-	-	0.54 (0.15)	0.54 (0.16)
Y63	-0.012 (0.021)	-0.013 (0.021)	-0.008 (0.021)	-0.009 (0.021)	-0.11 (0.16)	-0.13 (0.16)	-0.10 (0.16)	-0.12 (0.17)
Y68	-0.008 (0.024)	-0.010 (0.024)	-0.001 (0.024)	-0.003 (0.025)	-0.15 (0.18)	-0.18 (0.18)	-0.12 (0.19)	-0.16 (0.20)
Y75	-0.019 (0.027)	-0.020 (0.028)	-0.012 (0.029)	-0.014 (0.030)	-0.28 (0.19)	-0.32 (0.20)	-0.28 (0.21)	-0.34 (0.22)
*CHANGE*Y63*	0.005 (0.035)	0.005 (0.034)	0.005 (0.035)	0.006 (0.035)	0.09 (0.24)	0.08 (0.23)	0.13 (0.23)	0.12 (0.23)
*CHANGE*Y68*	0.088 (0.032)	0.089 (0.032)	0.081 (0.033)	0.082 (0.033)	1.08 (0.71)	1.08 (0.71)	1.07 (0.71)	1.06 (0.70)
*CHANGE*Y75*	0.110 (0.031)	0.111 (0.031)	0.103 (0.033)	0.104 (0.033)	0.88 (0.32)	0.89 (0.32)	0.87 (0.33)	0.88 (0.32)
constant	0.866 (0.138)	0.814 (0.131)	0.750 (0.145)	0.703 (0.135)	0.21 (1.20)	0.28 (1.20)	-1.01 (1.27)	-1.48 (1.28)
Hausman statistic	4.03	3.41	3.58	2.94	3.60	4.47	1.83	2.34
Prob-value	0.85	0.91	0.89	0.94	0.89	0.81	0.99	0.97
R^2	0.30	0.30	0.20	0.20	0.30	0.30	0.21	0.21
No. of industries	65	65	65	65	65	65	65	65
No. of industries with *CHANGE* = 1	10	10	10	10	10	10	10	10
No. of observations	208	208	208	208	208	208	208	208

Note: Heteroskedasticity-consistent standard errors in parentheses.

Table 3. Regression results for profitability in R&D-intensive industries. (Random effects GLS estimation.)

	Dep. variable: *PCM*			
ln*SS*	-0.012	-	-0.013	-
	(0.012)		(0.012)	
ln*DS*	-	-0.010	-	-0.010
		(0.010)		(0.011)
ln*K/L*	0.021	0.021	-	-
	(0.011)	(0.011)		
ln*K/N*	-	-	-0.001	-0.001
			(0.011)	(0.011)
Y54	0.005	0.005	0.002	0.002
	(0.011)	(0.011)	(0.011)	(0.011)
Y63	0.029	0.029	0.034	0.033
	(0.009)	(0.009)	(0.009)	(0.009)
Y68	0.028	0.027	0.040	0.039
	(0.014)	(0.014)	(0.014)	(0.014)
Y73	0.003	0.002	0.022	0.021
	(0.020)	(0.019)	(0.018)	(0.018)
*CHANGE*Y54*	-0.011	-0.012	-0.012	-0.013
	(0.017)	(0.018)	(0.018)	(0.018)
*CHANGE*Y63*	-0.034	-0.034	-0.038	-0.038
	(0.017)	(0.017)	(0.018)	(0.018)
*CHANGE*Y68*	-0.008	-0.007	-0.014	-0.014
	(0.015)	(0.015)	(0.015)	(0.015)
*CHANGE*Y73*	-0.001	-0.001	-0.007	-0.007
	(0.022)	(0.023)	(0.022)	(0.022)
constant	0.349	0.325	0.380	0.352
	(0.137)	(0.120)	(0.138)	(0.120)
Hausman statistic	4.90	7.14	2.28	2.91
Probability value	0.90	0.71	0.99	0.98
R^2	0.09	0.08	0.12	0.09
No. of industries	40	40	40	40
No. of industries with *CHANGE* = 1	6	6	6	6
No. of observations	152	152	152	152

Note: Heteroskedasticity-consistent standard errors in parentheses.

APPENDIX

Data on the number of innovations come from the SPRU survey of significant innovations commercialised in UK industry between 1945 and 1983. There are a number of problems with these data, two of which are worth mentioning here. The first is that the comparability of innovation counts over time is not very good in some industries, in particular some low-innovation industries and two R&D-intensive industries, namely textile machinery and instruments. None of the industries with obvious time inconsistencies were included in the samples used in this paper.

The second problem relates to the way innovations are assigned to the various three-digit industries. In addition to a short description of each innovation, the data report the *principal* three-digit industry classification for the firm producing the innovation as well as the industry classification of the innovation. None of these is entirely satisfactory. The former may be misleading in the case of diversified firms, which are numerous in this dataset. The latter is misleading in the case of process innovations, which are a considerable part of the total in the SPRU data - although probably less than 25%. To minimise these problems, I have reclassified the innovations in the SPRU database, so that product innovations are classified according to the industry of the innovation, while process innovations are classified according to the industry in whose production process the innovation was made. In several cases I have been able to classify innovations to four-digit, rather than three-digit industries.

The concentration data were taken from three official publications: (i) Summary Table 5 of the 1963 Census of Production; (ii) Summary Table 44 of the 1968 Census of Production; and (iii) *Statistics of Product Concentration of UK Manufactures for 1963, 1968 and 1975*, Business Monitor PO 1006 (HMSO, 1979).

Data on gross and net output at current net producer prices, wages and salaries were obtained from the individual industry reports of the UK Census of Production (various years). In most cases the industry definitions for the profitability regressions are the "principal products" within any three-digit "minimum list heading" industry, as defined in the Census. Whenever a "minimum list heading" industry is not further subdivided in the Census reports, it is used as the industry definition for this paper.

Industry gross profit is defined as net output minus wages and salaries, and therefore it includes fixed costs, such as advertising expenditure, R&D expenditure and capital costs. Sales revenue data at current net producer prices at the four-digit (or "product group") level of aggregation were obtained from the industry reports of the Census of Production (various years) and from Business Monitors. Data for 1953 are not available, but I have constructed estimates for 1953 sales revenue on the assumption that the 1953-1954 growth rate was equal to the annual growth rate between 1954 and 1958. A series of general producer price indices was obtained from the *Annual Abstract of Statistics*; industry-specific price indices were obtained from the *Annual Abstract of Statistics* or computed from Census data on volume of sales reported together with sales revenue.

Estimates of the capital stock at the three-digit level of aggregation (i.e. for Census "minimum list headings") were taken from O'Mahoney and Oulton (1990). Capital stock was defined as plant and machinery. Data on employment and plant numbers at the three-digit level of aggregation were taken from the relevant Summary Tables and from individual industry reports of the Census of Production. Some of these figures were adjusted to ensure comparability over time in the light of changes in the definition of a number of three-digit industries, the treatment of very small plants, and the definition of "establishment".

Finally, the procedure for constructing R&D-sales ratios is the same as in Symeonidis (2000a); see that paper for details and a listing of the sources used.

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