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# Unprofitable Cartels: Evidence from a Natural Experiment in the UK

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**Abstract**
I analyse the effect of collusion on profitability with the use of evidence from a natural experiment of policy reform: the introduction of anti-cartel legislation in the UK in the late 1950s. This caused an intensification of price competition in previously collusive industries but did not affect industries that had not been collusive. Three main results are established: First, a comparison of the two groups of industries shows that the breakdown of collusion had no significant overall effect on profitability in the long run—although it had a strong negative effect on firm numbers. Second, the larger was the decrease in firm numbers, the smaller was the decline of profitability in previously collusive industries; however, no such correlation exists in industries that were not affected by the cartel law. Third, most cartels did not restrict entry; but in industries where entry had been restricted, profitability rather than firm numbers decreased when collusion broke down. These results are consistent with theories that emphasise the role of entry conditions for profitability: Cartels are often unprofitable because of entry even when collusion is effective.

**Keywords** Cartels · Free entry · Market structure · Profitability · UK manufacturing · Antitrust policy

**JEL Classification** L10 · L60

## 1 Introduction

An influential line of research in oligopoly theory has questioned the conventional wisdom on the economic effects of cartels and has shed new light on the much-debated issue of the links between firm conduct, market structure and performance. In a study entitled "Are cartel laws bad for business?", Selten (1984) predicted that a shift from collusive to non-collusive behavior that was caused

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by cartel policy in an industry would not reduce firms' profits in the long run—although it would cause a decrease in the number of firms. A similar idea was put forward by Sutton (1991, 1997, 1998), expressed as a relationship between the "toughness of price competition" and the level of concentration and cast within his general theory of the determinants of market structure in exogenous sunk cost and endogenous sunk cost industries.

On the other hand, some of the empirical literature on cartels has reported significant positive effects of many cartels on prices and profits (Boyer & Kotchoni, 2015; Connor, 2014; Connor & Bolotova, 2006; Griffin, 1989; Levenstein & Suslow, 2006); but see Asch and Seneca (1976) for evidence of a negative link between collusion and profits. As the information on cartels often comes from case studies or antitrust investigations, it is difficult to rule out selection bias in some studies. What one measures also matters; and Levenstein and Suslow (2004) have emphasised that in assessing a cartel's success in raising profits above the competitive level one should take into account not only the collusive periods but also any periods of price wars or other breakdowns. Nevertheless, and despite these limitations, some of the empirical research on cartels seems to support the traditional view of a positive effect of collusion on profits and to cast doubt on the empirical relevance of Selten's theoretical prediction.

The present research seeks to reconcile theory and evidence on the effect of collusion on profitability by revisiting a natural experiment that occurred in the UK in the 1960s. As a result of the introduction of the 1956 Restrictive Trade Practices Act, collusive agreements among firms were cancelled. This caused an intensification of price competition across a wide range of manufacturing industries during the 1960s. These can be compared to a control group of industries that had not been subject to collusive agreements and were therefore not affected by the 1956 law. The resulting data set is free from sample selection bias. A comparison of the two groups of industries over a 20-year period—with the use of data both before and after the implementation of the 1956 legislation—can provide important insights on the links between collusion and profitability.

In previous research I have analysed the effect of the 1956 Act on profitability with the use of a data set of four-digit industries (Symeonidis, 2002, ch. 7). I have found no evidence of a significant long-run effect of the breakdown of cartels on profitability. One limitation of this work was that the panel that was used for the profitability regressions was very unbalanced, and this may have affected some of the coefficients of interest. Furthermore, in my earlier work I did not attempt to control for the potential endogeneity of collusion.

The first part of the present analysis confirms the lack of any long-run overall effect of the abolition of British cartels on profitability—as well as a strong negative effect on firm numbers—by using an almost-balanced panel of about 100 three-digit manufacturing industries. I also check the robustness of this result much more fully than in my previous work and confirm some of the findings with additional results from an instrumental variable estimation that seeks to address a criticism of studies that use a difference-in-differences methodology: the potential endogeneity of the natural experiment itself. The econometric evidence is

consistent with the fact—which was described in several official reports and case studies—that most British cartels did not deter entry.

The second part of the paper goes further and documents how the effect of the abolition of cartels on profit margins is closely linked with the evolution of market structure in previously collusive industries. The use of an almost-balanced panel is now essential for my approach. I show that profit margins declined in the long run in some previously collusive industries that did not experience a significant long-run fall in firm numbers: In fact, the smaller was the fall in firm numbers, the larger was the decline of profitability. Crucially, I find no evidence of a similar link in industries that had not been collusive.

I interpret these findings as strong indirect evidence for the role of entry conditions in collusive industries: Cartels are often unprofitable because of entry even when collusion is effective—although they can make supra-normal profits if they successfully deter entry.

## 2 Theoretical Framework and Empirical Predictions

The mechanism that underlies the Selten-Sutton theoretical result can be summarised as follows: The number of firms in an industry is determined by a free-entry condition that requires that net profit be driven to (almost) zero irrespective of firm conduct and the competition regime that firms face.[1] If entry is not deterred in a collusive industry, the cartel can still set price above the competitive level, and even achieve perfect collusion, but the firms do not make excess profits. An increase in the intensity of price competition—caused by the introduction of anti-cartel laws or some other exogenous institutional change—will cause price and gross profit to fall, given the initial number of firms. Since some or all firms will be making negative net profits, there will be mergers and exit that lead to a recovery of price–cost margins until the gross profit of each surviving firm rises sufficiently to cover its fixed cost, which has not changed. At the new long-run equilibrium, the number of firms will be smaller, but there will be no significant change in firms' gross or net profits.[2]

This mechanism relies on the assumption that incumbent firms under a collusive regime cannot prevent the entry of new firms into an industry any more than in the

[1] It may be asked why collusion should occur if profits are to be driven to (almost) zero by entry. One reason is that there are short-run gains from collusion among a given number of firms, even if these gains are eventually eliminated by entry. Also, collusion may persist once it is established, even when it no longer generates supra-normal profits for the average cartel member, because its breakdown will result in short-run losses or even exit for many firms.

[2] The Selten-Sutton result is valid even if the degree of collusion varies across cartels in a non-random way. If entry reduces the highest price that is sustainable under collusion, then cartels that fail to deter entry will not collude as effectively as entry-restricting cartels. However, the breakdown of a free-entry cartel should still have no effect on long-run profitability – irrespective of the extent to which the net profits of the cartel firms had been dissipated directly by entry or indirectly through a reduction in the degree of collusion. Moreover, since the price must fall in the short run after a cartel breakdown which leads to negative short-run net profit for the marginal firm, we should still observe a decrease in the long-run number of firms.

absence of collusion. If this is not the case, and collusive firms can make significant supra-normal profits because of entry barriers, the breakdown of collusion will reduce profitability but will not necessarily result in negative net profits in the short run, so there may be little pressure on firms to merge or exit. And in the absence of a decrease in firm numbers, profitability will not recover in the long run—in line with the conventional wisdom on the effects of collusion. Thus the crucial factor that will determine the empirical relevance of the Selten-Sutton result is the extent to which cartels deter entry. Differing entry conditions across collusive industries may help interpret the mixed empirical evidence on cartel profitability.[3]

There are two ways to test the above predictions. A direct test would require detailed information on entry across collusive industries. As I discuss in Sect. 3, case-study evidence suggests that entry was not restricted by most British cartels. However, although entry conditions are known for several cartels, they are not observable or easily measurable for a sufficiently large sample. An alternative, indirect test of the theory relies on examining the link between profitability and market structure.

More specifically, suppose that we observe, on average, a decrease in firm numbers and no significant effect on long-run profitability after the breakdown of cartels. This is consistent with the theory if we know that the cartels were generally successful in increasing price and did not usually restrict entry. However, it is not a decisive test, since we cannot rule out the possibility that some other mechanisms are jointly or independently driving these results.

But now suppose that we also observe in a subset of previously collusive industries a small or non-existent effect of competition on firm numbers and at the same time a decline in long-run profitability. Or we observe that across all previously collusive industries, the larger is the decrease in firm numbers, the smaller is the decline of profitability. This is a stronger result, because it confirms that the movements of the two variables over time are related in the particular way that is predicted by the theory. Again a potential objection is that another mechanism might be at play. For instance, a negative link between the change in firm numbers and the change in industry profitability is also predicted by the traditional structure-conduct-performance approach when market structure is largely exogenous.

The next and final test therefore is to examine whether the negative correlation between the change in firm numbers and the change in profitability is also observed outside the set of previously collusive industries. If it is not observed, then it becomes difficult to think of a mechanism that creates this set of results other than the one proposed here: that the abolition of cartels generated a negative link between

[3] Mechanisms to deter entry in a collusive industry include vertical restraints to hinder the use of existing distribution channels by potential entrants (Levenstein and Suslow, 2014); and price wars as a response to entry (Scott Morton, 1997). Hyytinen et al. (2019) report that price-fixing Finnish cartels were less likely to have contract clauses that relate to entry than did market-sharing cartels – which is not inconsistent with the UK evidence that is described in Sect. 3. The question of why some cartels might deter entry more successfully than others is outside the scope of the present work. Here I take it as given that there are differences in the extent of successful entry deterrence across collusive industries and I focus on the consequences of this for profitability.

the change in firm numbers and the change in profitability because of differing entry conditions across collusive industries. Additional support for this interpretation can be obtained from case-study evidence on entry conditions in particular industries.

## 3 The Competition Data

Explicit restrictive agreements among firms were widespread in British industry in the mid-1950s: Nearly half of manufacturing industry was subject to price-fixing. The agreements were not enforceable at law, but they were not illegal. Most of them provided for minimum or fixed producer prices. There were generally no restrictions on longer-term decisions such as investment in capacity, advertising or R&D expenditure. A description of the institutional changes and the evolution of competition from the 1950s to the early 1970s and a detailed survey of restrictive agreements across all British manufacturing industries can be found in Symeonidis (2002). In what follows I summarise the evidence and describe the construction of the data set for this paper.

The 1956 Restrictive Trade Practices Act required the registration of restrictive agreements—including verbal or even implied arrangements—on goods. Registered agreements should be abandoned, unless they were either successfully defended by the parties in the newly created Restrictive Practices Court as producing benefits that offset the presumed detriment or cleared by the Registrar of Restrictive 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 voluntarily to abandon their agreements rather than incur the costs of a Court case with little hope of success.

Was entry generally free in these collusive industries? The evidence from the agreements that were registered under the 1956 Act, various industry reports published by the Monopolies and Restrictive Practices Commission during the 1950s (see also Guenault & Jackson, 1974; Rowley, 1966) and a large number of case studies that are discussed in Swann et al., (1973, 1974) indicates that entry was not significantly restricted in most, although not all, collusive industries.

Most agreements were operated by trade associations and there were usually no significant restrictions on association membership, so that entry would not be difficult if the entrant was willing to become a party to the agreement. For instance, for only four out of 14 collusive industries investigated by the Monopolies and Restrictive Practices Commission in the 1950s—that operated some of the most restrictive agreements across all British industries—did the Commission report evidence of restricted cartel membership.[4] Furthermore, both the reports of the Monopolies

[4] Free cartel membership is not the same as free entry into the collusive industry. In many cases, however, the former implies the latter because many of the entry deterrence mechanisms that can be used by cartels – such as collective exclusive dealing or aggregated discounts to distributors – are essentially attempts to restrict competition from outside firms and are not very effective if cartel membership is free. In fact, such practices were used by several British cartels, but they were often not effective in deterring

Commission and the case studies in Swann et al., (1973, 1974) contain information on profitability of firms in collusive industries: Profits are usually described as "reasonable" rather than "excessive"—which is consistent with cartel profits being eroded by entry.

Were the agreements effective? Or could the lack of excessive profits be due to ineffective cartels rather than free entry? Evidence from the registered agreements, the Monopolies and Restrictive Practices Commission reports, the Political and Economic Planning (1957) survey of industrial trade associations and Swann et al., (1973, 1974) suggests that in most industries the agreements had been operated effectively before cancellation, the parties typically accounted for a large fraction of the market and contained the largest and best-known domestic firms, and outside competition was usually weak.

For instance, Swann et al. report cartel market shares of 90% or higher in about two thirds of the 40 cases that they examine, and 75% or higher in all but two cases. Competition from imports was often limited because of tariffs and quantitative controls, differing technical standards, transport costs or international restrictive agreements. Finally, the legality of the agreements and the institutional role of the trade associations that operated them had facilitated the coordination, monitoring and enforcement of collusion by the cartels.

To what extent did collusion break down following the abolition of cartels? Evidence from Heath (1961, 1963), Swann et al., (1973, 1974) and the annual reports of the Registrar of Restrictive Trading Agreements indicates that price competition intensified in the short run in many industries. However, in many others, agreements to exchange information on prices or price changes replaced the former explicit collusive arrangements, and price competition emerged only after these information agreements were abandoned in the mid-1960s, following adverse decisions of the Restrictive Practices Court. Price wars occurred in several previously collusive industries in the second half of the 1960s, and the final blow came with the provisions of the 1968 Restrictive Trade Practices Act. In many industries, therefore, competition emerged more than a decade after the introduction of the 1956 legislation. Overall, sooner or later the large majority of industries with collusive agreements in the 1950s did experience a breakdown of collusion as a result of the 1956 Act.

Although my main source of data on competition are the agreements that were registered under the 1956 Act, I also use other sources to identify unregistered agreements: the industry reports of the Monopolies Commission; the 1955 Monopolies Commission report on collective discrimination; the 1949 report of the Lloyds' Committee on resale price maintenance; industry studies in Burn (1958) and in Hart et al. (1973); the Board of Trade annual reports from 1950 to 1956; and the Political and Economic Planning (1957) survey of trade associations (including unpublished

Footnote 4 (continued)
entry. One reason for this may be the fact that although in some industries the existing association members might reject certain applications for membership, they would normally accommodate any powerful non-member firm rather than face strong outside competition.

background material for this survey). The use of a diverse range of sources makes it likely that any potential measurement error that might be caused by ineffective agreements or unknown cases of collusion in the data should be small.

All manufacturing industries were classified as collusive, competitive, or ambiguous 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 revenue that was covered by products that were subject to agreements and, for each product, the fraction of the UK market that was covered by cartel firms.

In particular, the various types of restrictions were classified as significant, not significant or uncertain, according to their likely impact on competition. Next, the products that were subject to agreements were assigned to the industry categories that were mentioned above. An industry was classified as collusive in the 1950s if the products that were subject to *significant* restrictions accounted for more than 50% of total industry sales. It was classified as competitive if the products that were subject to *significant or uncertain* restrictions accounted for less than 20% of industry sales. And it was classified as ambiguous in all remaining cases. I have used the 50% cut-off point because in some cases most core industry products were subject to price-fixing, although some were not. I have used the 20% cut-off point because in some cases secondary industry products were subject to restrictive agreements, although core products were not. Variations in these cut-off points (for instance, using 70% instead of 50%, or 10% instead of 20%) do not significantly affect the results.[5]

To ensure that my panel of industries is balanced I have used the three-digit level of aggregation, although I have sometimes used available data at the four-digit industry level. The sample consists of 93 industries: 38 with a change of competition regime; and 55 that were not significantly affected by the 1956 Act. A binary variable, *CHANGE*, was defined, which takes the value 1 for industries with a change in competition regime after 1958 and 0 otherwise.

My data on profits and firm numbers are for five years: 1954, 1958, 1963, 1968 and 1973. The first four years are the only ones in the 1950s and 1960s for which such data are available, while 1973 is the last year before the oil crisis. Although the anti-cartel law was introduced in 1956, it was not until 1959 that industries started cancelling their agreements: both 1954 and 1958 are therefore years when the cartels where still in place. Since competition was often slow to emerge, the 1956 Act had a significant impact well into the late 1960s and even early 1970s.

[5] Out of 38 industries that were classified as collusive, 20 had agreements that covered all or nearly all industry products and 28 had agreements that covered 70% or more of industry sales. Out of 55 industries that were classified as competitive, 25 were free from any significant collusion and 45 had agreements that covered less than 10% of industry sales. The use of a continuous competition measure is impractical at the three-digit level of aggregation for a variety of reasons (see Symeonidis 2002). I have dropped from my sample a few industries that remained collusive or partially collusive until the early 1970s and those (sugar, steel, aircraft, locomotives) with significant government participation or intervention in the period under study.

**Table 1** Initial conditions (1954, 1958) in industries that were affected by the 1956 Act and in industries that were not affected

| | Mean (Standard deviation) of NFIRMS | Mean (Standard deviation) of FIRMPROFIT | Mean (Standard deviation) of PCM |
|---|---|---|---|
| *Industries with CHANGE = 1 (N = 37)* | | | |
| 1954 | 219.8 (265.7) | 161.2 (110.5) | 0.173 (0.047) |
| 1958 | 191.0 (227.4) | 204.8 (146.5) | 0.175 (0.045) |
| *Industries with CHANGE = 0 (N = 50)* | | | |
| 1954 | 205.2 (215.8) | 121.7 (128.9) | 0.191 (0.072) |
| 1958 | 176.6 (185.4) | 150.6 (153.0) | 0.193 (0.076) |

The figures are based on industries with available data for both 1954 and 1958. N indicates the number of industries. *FIRMPROFIT* is measured in £1000. The small differences in the numbers of industries across different tables are caused by a few missing 1954 or 1973 observations

## 4 Endogenous Variables and Descriptive Statistics

To analyse the effect of competition on profitability I will estimate reduced-form equations; I will treat both market structure and profitability as endogenous. Several profitability measures that are based on gross profit will be used. Gross profit is defined as the net value of output (or value added) minus wages and salaries, and includes fixed costs, such as capital costs, advertising and R&D expenditure other than salaries of R&D personnel. A measure of net profit—gross profit minus fixed costs—cannot be constructed because of data limitations: The capital stock data are estimates; and, although the estimated proportional changes over time in any given industry are reasonably accurate, the estimated levels must be treated with caution (Oulton and O'Mahony, 1990).

To address concerns about profitability measures that are closely related to gross profit, many of my results will be derived with the use of the industry price–cost margin: *PCM*. Any limitations of *PCM* itself are alleviated by the use of panel data and the fact that my results are based on changes in profitability—not on levels.

Descriptive statistics on initial levels of profitability and market structure are presented in Table 1. In particular, the table reports means and standard deviations of three different variables in 1954 and 1958, for industries that experienced subsequently an intensification of competition as well as for industries that were not affected by the anti-cartel law. The variables are: the number of UK producers with at least 25 employees in any given industry, *NFIRMS*; the gross profit of the average firm, *FIRMPROFIT*, which is defined as the industry gross profit deflated by an industry-specific price index and divided by the number of firms in the industry; and the price–cost margin, *PCM*, which is defined as the net value of output minus

**Table 2** Descriptive statistics for *NFIRMS*, *FIRMPROFIT*, and *PCM*, 1958, 1963, 1968 and 1973

| | Mean (Standard deviation) of NFIRMS | Mean (Standard deviation) of FIRMPROFIT | Mean (Standard deviation) of PCM |
|---|---|---|---|
| *Industries with CHANGE = 1 (N = 36)* | | | |
| 1958 | 197.5 (233.8) | 197.8 (147.9) | 0.176 (0.045) |
| 1963 | 163.8 (201.2) | 313.8 (230.8) | 0.201 (0.052) |
| 1968 | 139.6 (184.0) | 447.2 (342.8) | 0.214 (0.053) |
| 1973 | 132.9 (184.0) | 522.2 (372.8) | 0.213 (0.048) |
| *Industries with CHANGE = 0 (N = 55)* | | | |
| 1958 | 168.5 (179.6) | 170.2 (173.6) | 0.194 (0.075) |
| 1963 | 151.4 (152.8) | 280.6 (303.2) | 0.224 (0.072) |
| 1968 | 141.4 (145.6) | 368.0 (376.2) | 0.225 (0.072) |
| 1973 | 152.2 (167.3) | 472.4 (577.9) | 0.223 (0.066) |

The figures are based on industries with available data for all four years. N denotes the number of industries. *FIRMPROFIT* is measured in £1000. The small differences in the numbers of industries across different tables are caused by a few missing 1954 or 1973 observations

wages and salaries divided by sales revenue.[6] The data were obtained from the Census of Production.

There is little difference in initial conditions between industries that were affected by the 1956 Act (*CHANGE* = 1) and those in the control group (*CHANGE* = 0). Note that *FIRMPROFIT* is higher but *PCM* is lower for collusive industries than for competitive industries. One should not read too much into a comparison of the levels in any event. For instance, capital-intensive industries have, on average, a higher incidence of collusion and higher profitability than do labour-intensive industries; whereas low-advertising industries have, on average, a higher incidence of collusion and lower profitability than do advertising-intensive ones. Therefore average profitability could be higher or lower in collusive than in competitive industries for reasons that are not directly linked to the competition regime.

Much more informative is a comparison of the evolution of collusive and competitive industries over 1954–1958: before the implementation of the anti-cartel law. The 1954–1958 evolution is very similar across the two groups for all three variables and practically identical for *PCM*. This will be confirmed by the econometric results in Sect. 5. It would seem on the basis of this evidence that any differences observed after 1958 between industries with *CHANGE* = 1 and those with *CHANGE* = 0 are not biased by any divergent prior trends in the two groups.

Table 2 presents descriptive statistics for each of the three endogenous variables for 1958, 1963, 1968 and 1973. For 1958–1973 as a whole, the average value of

[6] Other measures of market structure, such as concentration ratios, are not available for the industry categories used here. I follow the bulk of the literature in using sales revenue as denominator of *PCM* (see Collins and Preston, 1969; Cowling and Waterson, 1976; Machin and Van Reenen, 1993). The results from regressions using net output as denominator of *PCM* are similar to those reported here.

*NFIRMS* decreased by about 33% in industries with a change of competition regime, whereas it decreased by only about 10% in the control group.

With respect to the profitability measures, a different picture emerges. A first look at the data reveals a puzzle, which can be seen most clearly in competitive industries: the average *PCM* was relatively stable over time (and even for 1954–1958, as can be seen in Table 1), except for an increase of about three percentage points between 1958 and 1963. This increase is difficult to explain by reference to any exogenous factors and may be due to some unknown change in the way that the information was collected and certain variables (especially net output) computed for the 1963 and later Censuses as compared to the 1958 and earlier Censuses.[7] I will therefore treat it as a time effect and control for it through the use of year dummy variables. It will not affect any of the econometric results of interest under the plausible assumption that it is uncorrelated with *CHANGE* and with the change in firm numbers across industries.

Subject to this qualification, I can now compare the evolution of profitability in the two groups of industries. The average *FIRMPROFIT* and *PCM* increased by slightly more in previously collusive than in competitive industries between 1958 and 1973. All in all, the effect of the 1956 law on long-run profitability does not seem to be significant.

It may also be of interest to compare two sub-groups of collusive industries: I distinguish between those industries with a relatively large decrease and those with a smaller decline (or sometimes an increase) in firm numbers. In particular, I have divided the collusive industries into two sub-groups according to whether the change in *NFIRMS* in any given industry was lower or higher than the median over 1958–1968. (The results are similar for 1958–1973.) *PCM* increased over 1958–1968 by 4.5 percentage points, on average, in collusive industries with a relatively large decrease in firm numbers, whereas *PCM* increased by only 2.9 percentage points in those industries with a smaller decrease (or even an increase) in firm numbers. Recall that much of the change in *PCM* across industries in my data set is a time effect, so what is important is the difference between the two sub-groups. The comparison suggests that a larger decline in firm numbers is associated with a smaller decline (or an increase) in *PCM* after the abolition of cartels—as would be expected on the basis of my theoretical discussion in Sect. 2.

[7] UK profitability estimates based on company accounts do not show any shift in 1958–1963; see the aggregate statistics that were published in the November 1974 issue of *Economic Trends*. I had not been aware of this feature of the data in my previous work on profitability in British industry (Symeonidis 2002, ch. 7). Only my interpretation of some of the descriptive statistics and the coefficients on the year dummy variables in my regressions were affected – not the main results.

## 5 Econometric Evidence I: General Results

I now turn to the econometric analysis. The specifications in this section are panel data models with industry fixed effects. My approach is based on the difference-in-differences methodology and consists in comparing the difference between the average change in the variable of interest in the treatment group and the average change in the same variable in the control group to estimate the "average treatment effect on the treated". Year dummy variables are also included among the regressors to control for other factors that may have influenced market structure and profitability over the period examined more or less equally across the two groups of industries: changes in the tax system that are thought to have encouraged mergers, the UK government's prices and incomes policies and macroeconomic fluctuations.

With respect to foreign competition, there is no evidence that any changes during the 1960s were different in the two groups. As shown in Symeonidis (2003), cartelisation was not correlated with the intensity of foreign competition in the 1950s. Moreover, estimates of effective tariff protection for 1963 and 1968 at a level of aggregation between the two-digit and the three-digit that were provided by Kitchin (1976) suggest that effective protection increased in 6 out of 12 industries or sectors that were mostly collusive in the 1950s and decreased in the other 6. For industries or sectors that were mostly competitive, the respective numbers were 8 and 10. Tariff protection changes before 1963 were much smaller.

In any case, I will also include the import penetration ratio as a regressor in some specifications. Although import penetration does not capture the effect of the mere threat of imports and is endogenous, this approach could provide additional reassurance that the main results of interest are not biased by any potential correlation between *CHANGE* and the intensity of foreign competition.

My benchmark specification for the number of firms is:

$$\begin{aligned}\ln NFIRMS_{it} =& \alpha_i + \beta_1 \ln SALES_{it} + \beta_2 \ln(K/L)_{it} + \beta_3 IMPORT_{it} + \beta_4 Y54 \\ &+ \beta_5 Y63 + \beta_6 Y68 + \beta_7 Y73 + \beta_8 CHANGE \times Y54 + \beta_9 CHANGE \\ &\times Y63 + \beta_{10} CHANGE \times Y68 + \beta_{11} CHANGE \times Y73 + \beta_{12} ADV \\ &\times Y54 + \beta_{13} ADV \times Y63 + \beta_{14} ADV \times Y68 + \beta_{15} ADV \times Y73 \\ &+ \beta_{16} RD \times Y54 + \beta_{17} RD \times Y63 + \beta_{18} RD \times Y68 + \beta_{19} RD \\ &\times Y73 + u_{it},\end{aligned}$$

and similarly for the profit measures: ln*FIRMPROFIT*, and *PCM*:

$$\begin{aligned}\text{Profit measure}_{it} =& \alpha_i + \beta_1 \ln(K/L)_{it} + \beta_2 UNION_{i,t-1} + \beta_3 IMPORT_{it} + \beta_4 Y54 \\ &+ \beta_5 Y63 + \beta_6 Y68 + \beta_7 Y73 + \beta_8 CHANGE \times Y54 + \beta_9 CHANGE \\ &\times Y63 + \beta_{10} CHANGE \times Y68 + \beta_{11} CHANGE \times Y73 \\ &+ \beta_{12} ADV \times Y54 + \beta_{13} ADV \times Y63 + \beta_{14} ADV \times Y68 \\ &+ \beta_{15} ADV \times Y73 + \beta_{16} RD \times Y54 + \beta_{17} RD \times Y63 \\ &+ \beta_{18} RD \times Y68 + \beta_{19} RD \times Y73 + \varepsilon_{it}.\end{aligned}$$

This specification for profitability has one important difference from the specifications that are often used in the competition-profitability literature: A measure of market structure is not included among the regressors. Recall that the theoretical predictions with regard to the effect of competition on profitability depend on allowing the number of firms to adjust to restore the long-run equilibrium. It is therefore important not to control for market structure in the profitability equation.

*SALES* is the industry sales revenue deflated by the general producer price index for all manufacturing and serves as a proxy for market size. I expect a positive effect of *SALES* on firm numbers, at least in exogenous sunk cost industries.[8] *K/L* is the capital-labour ratio and is often used in profitability studies to control for the fact that the endogenous variable—the price–cost margin or the rate of return on capital—includes the gross return to capital. Thus *K/L* should have a positive effect on profitability. The capital-labour ratio can also be seen as a proxy for the cost of setting up a plant of minimum efficient scale. *UNION* is union density, which is defined as the number of unionised employees over the total number of employees, and has often been found to have a negative effect on profitability. I will use *UNION* with a one-year lag.[9] *IMPORT* is the ratio of imports to total sales in the domestic market. Finally, I include as an additional variable ln*PLANTSIZE*, which is defined as industry employment divided by the number of plants, or average plant size, in the *PCM* regressions in particular, in order to confirm it is not a significant determinant of *PCM*, since it will be used below as an instrument in a two-stage model for *PCM*.

*Y54*, *Y63*, *Y68* and *Y73* are time dummy variables for 1954, 1963, 1968 and 1973, respectively. The interaction terms with *ADV* and *RD* are meant to capture any differences between exogenous and endogenous sunk cost industries. Note that *ADV* and *RD* each take the same value for all years in any given industry: $ADV = 1$ for industries with typical advertising-sales ratio over the period higher than 1% and 0 otherwise; and $RD = 1$ for industries with typical R&D-sales ratio higher than 1% and 0 otherwise. The 1% cut-off point is commonly used to classify industries according to their advertising or R&D intensity (see Sutton, 1991; Lyons et al., 2001; Symeonidis, 2002).[10] Details on data sources are given in the Appendix.

[8] Measuring market size by net output instead of sales revenue or using industry-specific price indices as deflators gives similar results.

[9] Although union power might be influenced by the competitive environment, this applies much less to union density. For instance, whereas the monetary benefits from union membership may decrease if a union's bargaining power in wage negotiations falls following an intensification of competition, workers may also think that being union members could reduce their chances of being laid off. Union density is seen here as a variable that picks up primarily exogenous influences on union power. The results are similar if *UNION* is omitted.

[10] These interaction terms are not endogenous: Whether the typical advertising-sales or R&D-sales ratio in an industry over a 20-year period is higher or lower than 1% is largely determined by exogenous characteristics: the extent to which advertising is effective in raising consumers' willingness to pay or the scope for technological innovation from within the industry. In fact, a comparison of advertising-sales and R&D-sales ratios over time revealed only few instances where an industry had moved from below 1% to above 1% or vice versa. Omitting the interaction terms with *ADV* and *RD* or using 2% instead of 1% as the cut-off point for the classification of industries does not significantly affect the results.

The interaction terms with *CHANGE* will pick up any differences between industries with *CHANGE* = 1 and those with *CHANGE* = 0 that are due to the different competitive regimes for the two groups. Thus the coefficient on *CHANGE* × *Y63* (*CHANGE* × *Y68*, *CHANGE* × *Y73*) measures the effect of the 1956 anti-cartel law on the dependent variable between 1958 and 1963 (1968, 1973). The baseline year is 1958, as the law did not have a significant effect on competition before then. Note that these regressions are not meant to indicate anything about the *level* of firm numbers or profitability. My approach relies on comparing the *evolution* of firm numbers and profitability between the two groups of industries.

The identifying assumption is that any difference in the evolution of market structure or profitability between industries with *CHANGE* = 1 and industries with *CHANGE* = 0 during the 1960s and 1970s is not due to unobserved characteristics that differ between the two groups of industries. Recall that Table 1 suggests that the difference in the evolution of market structure and profitability over 1954–1958 between industries that would subsequently be affected by cartel policy and those that were not affected is minimal. This will be confirmed by the econometric results: the coefficient on *CHANGE* × *Y54* is nowhere statistically significant. Therefore the comparison between the two groups *after* 1958 should not be biased by any pre-existing trend differentials and should provide an accurate measure of the effect of cartel policy.

Furthermore, even if *CHANGE* is influenced by omitted variables that are correlated with market structure or profitability, these variables are likely to be part of the industry-specific effect, given that the large majority of restrictive agreements were in operation for many years before the introduction of the 1956 law. Any correlations between the industry-specific effects and the endogenous variables will not bias the econometric coefficients in fixed effects specifications.

For one of my dependent variables—*PCM*—I am able to address the concern of potential endogeneity of collusion formally by instrumenting *CHANGE*. I will therefore also report results from a two-stage model where *CHANGE* is replaced by the estimated probabilities of collusion across industries in the 1950s as determined by a first-stage regression of the incidence of collusion on a set of exogenous industry characteristics.

In particular, the estimated probabilities are the predicted values from the following cross-section probit regression:

$$COLL_i^* = \alpha_i + \beta_1 \ln(K/L)_i + \beta_2 \ln PLANTSIZE_i + \beta_3 ADV_i + \beta_4 RD_i + \beta_5 UNION_i + u_i,$$

where instead of the "propensity to collude" *COLL**, an unobserved latent variable, we observe the dichotomous variable *CHANGE*—which for the purposes of this regression is equal to 0 for competitive and 1 for collusive industries in the 1950s. *PLANTSIZE* can be seen as a largely exogenous technological characteristic and an element of market structure (which makes it unsuitable as an instrument for *CHANGE* other than in *PCM* regressions), and on both counts a potential determinant of collusion. In particular, it can be seen as a proxy for entry costs, which are thought to facilitate collusion. For *K/L*, *PLANTSIZE* and *UNION*, I use 1958 data; the results are similar if 1954 data are used.

**Table 3** Regression results for ln*NFIRMS*, ln*FIRMPROFIT*, and *PCM*—all industries

| | ln*NFIRMS* | | ln*FIRMPROFIT* | | *PCM* (one-stage) | | *PCM* (two-stage) | |
|---|---|---|---|---|---|---|---|---|
| ln*SALES* | 0.458 | (5.55) | – | | – | | – | |
| ln(*K*/*L*) | −0.018 | (−0.20) | 0.055 | (0.59) | 0.022 | (2.17) | 0.021 | (1.95) |
| *UNION* | – | | −0.185 | (−0.50) | −0.113 | (−2.77) | −0.068 | (−1.21) |
| ln*PLANTSIZE* | – | | – | | −0.002 | (−0.11) | – | |
| *IMPORT* | 0.133 | (0.70) | −0.207 | (−0.70) | 0.009 | (0.31) | −0.002 | (−0.10) |
| *Y54* | 0.139 | (5.71) | −0.137 | (−2.64) | 0.005 | (1.18) | −0.005 | (−0.50) |
| *Y63* | −0.214 | (−6.45) | 0.499 | (10.97) | 0.029 | (5.25) | 0.021 | (1.63) |
| *Y68* | −0.331 | (−5.52) | 0.770 | (11.65) | 0.028 | (3.86) | 0.017 | (1.73) |
| *Y73* | −0.448 | (−4.93) | 0.996 | (11.28) | 0.036 | (3.67) | 0.043 | (2.53) |
| *CHANGE* × *Y54* | −0.010 | (−0.42) | −0.045 | (−0.67) | −0.001 | (−0.16) | 0.017 | (1.07) |
| *CHANGE* × *Y63* | −0.021 | (−0.59) | −0.061 | (−1.14) | −0.008 | (−1.14) | 0.007 | (0.35) |
| *CHANGE* × *Y68* | −0.081 | (−1.61) | −0.030 | (−0.45) | 0.006 | (0.89) | 0.016 | (0.97) |
| *CHANGE* × *Y73* | −0.142 | (−2.43) | −0.019 | (−0.21) | 0.010 | (0.92) | −0.011 | (−0.36) |
| *ADV* × *Y54* | 0.081 | (2.81) | −0.210 | (−2.56) | −0.013 | (−1.82) | −0.009 | (−1.05) |
| *ADV* × *Y63* | −0.089 | (−2.19) | 0.028 | (0.40) | −0.006 | (−0.71) | −0.003 | (−0.23) |
| *ADV* × *Y68* | −0.142 | (−3.40) | 0.067 | (0.82) | −0.012 | (−1.09) | −0.007 | (−0.74) |
| *ADV* × *Y73* | −0.121 | (−1.99) | 0.077 | (0.80) | −0.012 | (−1.09) | −0.009 | (−0.91) |
| *RD* × *Y54* | −0.013 | (−0.47) | 0.061 | (0.72) | 0.004 | (0.59) | 0.006 | (0.92) |
| *RD* × *Y63* | 0.114 | (2.85) | −0.045 | (−0.64) | 0.002 | (0.18) | 0.002 | (0.28) |
| *RD* × *Y68* | 0.104 | (2.15) | 0.016 | (0.19) | −0.001 | (−0.17) | −0.002 | (−0.24) |
| *RD* × *Y73* | 0.108 | (1.59) | −0.075 | (−0.71) | −0.027 | (−2.38) | −0.031 | (−2.70) |
| $R^2$ | 0.69 | | 0.85 | | 0.39 | | 0.39 | |
| $R^2_{LSDV}$ | 0.98 | | 0.96 | | 0.88 | | 0.88 | |
| Hausmann Statistic | 86.20 | | 92.82 | | 45.91 | | 45.98 | |
| Prob-value | 0 | | 0 | | 0 | | 0 | |
| No. of industries | 93 | | 93 | | 93 | | 93 | |
| No. observations | 457 | | 457 | | 460 | | 460 | |

t-statistics based on cluster-robust standard errors (columns 1–3) or bootstrapped standard errors (column 4) in parentheses

Table 3 contains the results from fixed effects regressions with cluster-robust or bootstrapped standard errors. To help address any concerns with regard to endogeneity of some of the control variables, Table 4 confirms that the main results of interest are robust when using a parsimonious specification with only industry fixed effects, year effects and the interactions of these with *CHANGE*. The first-stage estimation for the two-stage model is shown in Table 5, where it can be seen that ln*PLANTSIZE* has a strong and statistically significant effect on *CHANGE*. Since I am relying on a single instrument, an overidentification test for instrument exogeneity cannot be performed; but I will confirm that ln*PLANTSIZE* is highly non-significant in regressions with *PCM* as dependent variable.

The first column in each of Table 3 and 4 shows that the 1956 anti-cartel law had a strong and statistically significant negative effect on the number of firms. The size

**Table 4** Regression results for ln*NFIRMS*, ln*FIRMPROFIT*, and *PCM*—all industries, alternative specifications

| | ln*NFIRMS* | | ln*FIRMPROFIT* | | *PCM* (one-stage) | | *PCM* (two-stage) | |
|---|---|---|---|---|---|---|---|---|
| *Y54* | 0.154 | (6.70) | −0.213 | (−6.69) | −0.003 | (−0.68) | −0.012 | (−1.98) |
| *Y63* | −0.115 | (−4.02) | 0.494 | (13.96) | 0.029 | (6.90) | 0.023 | (4.06) |
| *Y68* | −0.187 | (−5.50) | 0.805 | (17.72) | 0.031 | (5.81) | 0.025 | (3.09) |
| *Y73* | −0.168 | (−3.69) | 0.992 | (15.89) | 0.028 | (4.13) | 0.025 | (2.06) |
| *CHANGE* × *Y54* | −0.038 | (−1.26) | −0.006 | (−0.11) | 0.001 | (0.08) | 0.022 | (1.64) |
| *CHANGE* × *Y63* | −0.048 | (−1.07) | −0.063 | (−1.12) | −0.006 | (−0.91) | 0.008 | (0.67) |
| *CHANGE* × *Y68* | −0.140 | (−2.33) | −0.041 | (−0.55) | 0.006 | (0.86) | 0.020 | (1.29) |
| *CHANGE* × *Y73* | −0.256 | (−3.54) | −0.024 | (−0.25) | 0.009 | (0.89) | 0.015 | (0.60) |
| $R^2$ | 0.49 | | 0.82 | | 0.31 | | 0.31 | |
| $R^2_{LSDV}$ | 0.96 | | 0.97 | | 0.87 | | 0.87 | |
| No. of industries | 93 | | 93 | | 93 | | 93 | |
| No. observations | 457 | | 457 | | 460 | | 460 | |

t-statistics based on cluster-robust standard errors (columns 1–3) or bootstrapped standard errors (column 4) in parentheses

**Table 5** Probit regression with dependent variable *CHANGE*

| ln(*K/L*) | *ADV* | *RD* | ln*PLANTSIZE* | *UNION* | Constant |
|---|---|---|---|---|---|
| 0.307 | −0.585 | −0.920 | 0.621 | 1.564 | 0.340 |
| (1.61) | (−1.78) | (−2.27) | (2.23) | (1.29) | (0.40) |
| | | $1 - \ln L/\ln L_0 = 0.17$ | | | |
| | | No. of observations: 98 | | | |

t-statistics based on robust standard errors in parentheses. The sample includes five collusive industries in the 1950s where some cartels continued to operate throughout the 1960s (and are therefore not included in the sample for Table 3 and 4)

of the coefficient on *CHANGE* × *Y73* implies that the breakdown of cartels caused the number of firms in previously collusive industries to fall, on average, by 15% or more over 1958–1973. The coefficient on ln*SALES* is large, positive and statistically significant, as expected. The next two columns in each of Table 3 and 4 present one-stage results for ln*FIRMPROFIT* and *PCM*, whereas the last column contains the two-stage estimates for *PCM*.

Consistent with the theory, there is no evidence of any effect of cartel policy on long-run profitability: The coefficients on *CHANGE* × *Y68* and *CHANGE* × *Y73* are nowhere statistically significant. As expected, ln(*K/L*) has a positive and statistically significant effect on *PCM*, and there is some evidence of a negative effect of *UNION*. The coefficients on the year dummy variables are capturing the effect of economy-wide factors, as was already mentioned. Furthermore, *K/L* was increasing throughout the period. To the extent that it is an imperfect proxy for setup cost or is measured with error, the effect of setup cost is partly picked up by the time dummy variables. Finally, in the profitability regressions the year effects reflect in part the

1958–1963 jump in the data that was discussed above. Although part of the change over time in firm numbers or profitability may have been due to factors that are not explicitly included in the model, this does not invalidate the comparison between industries that were affected by the 1956 Act and those that were not affected to the extent that these factors are not correlated with *CHANGE*.

I have performed a variety of robustness checks. These included: 1) applying slightly different criteria to classify the industries as collusive or competitive; 2) replacing *PCM* by ln*PCM* or using net output rather than sales revenue as the denominator for *PCM*; 3) replacing in profitability regressions the capital-labour ratio by the capital stock of the average plant; 4) replacing *UNION* by ln*UNION* and *IMPORT* by ln*IMPORT*; and 5) using the 2% advertising-sales and R&D-sales ratios to classify the industries according to their advertising and R&D intensity or interacting *ADV* and *RD* with *SALES* or dropping the interaction terms with *ADV* and *RD*. The negative effect of cartel policy on firm numbers and the lack of any significant long-run effect on profitability persisted in all specifications.

A potential objection is that some of the empirical measures used here may not be ideal for a decisive test of the Selten-Sutton theory, and that what is preferable (but not available) is a measure of profit net of fixed costs. For instance, one could argue that, unlike net profit, *PCM* should still fall in the long run when collusion breaks down. There are two possible reasons why a decrease in *PCM* is not observed in the present data. First, the collusive price need not be the monopoly price: Several models of collusion predict that a collusive price that is lower than the monopoly price may be necessary to sustain collusion. Therefore the industry price–cost margin that follows the breakdown of a cartel need not be smaller than the collusive price–cost margin *once market structure has adjusted to a new equilibrium*. Second, price–cost margins may have increased during the 1960s in some previously collusive industries to the extent that more intense competition helped low-cost firms to expand at the expense of high-cost rivals. Since low-cost firms have higher margins than high-cost firms, this could have led to an increase in overall profitability in industries with large efficiency differences among firms.

These potential complicating factors should not be exaggerated. It is difficult to think of any interpretation of the stability of *PCM* in the long run that does not also imply stability of net profit in previously collusive industries. Furthermore, the results for ln*FIRMPROFIT* are similar to those for *PCM*, and the evolution of these measures of gross profit must closely match the evolution of net profit—at least when fixed costs are not significantly affected by the competitive regime. All in all, the lack of a significant effect of cartel breakdown on profitability seems consistent with the theory that was outlined in Sect. 2.

## 6 Econometric Evidence II: a Closer Look at Collusive Industries

To provide stronger evidence for the role of entry conditions in shaping the link between collusion and profitability, I will now turn to a more detailed analysis of the British collusive industries. Since entry was not effectively restricted by most cartels and the collusive arrangements had generally been long-standing and stable,

entry had been taking place over many years as long as there were profit opportunities in those industries. Therefore most cartels were not earning supra-normal profits, and the breakdown of collusion that followed the introduction of the 1956 Act did not lead to a permanent reduction in profitability, as was documented in the previous section. On the other hand, case-study evidence suggests that some cartels were effective in deterring entry. The more successful that any specific cartel was in deterring entry, the larger should be the departure from the Selten-Sutton benchmark. As discussed in Sect. 2, differences in entry conditions across collusive industries should generate a link between the evolution of market structure and the evolution of profitability: The larger is the decrease in firm numbers after the breakdown of a cartel, the smaller should be the decline in long-run profitability, other things being equal. In this section I provide an econometric analysis of this link.

The use of *PCM* (rather than net profit) as the dependent variable is unlikely to be a cause for concern here, for two reasons. First, it is difficult to think of an interpretation of the link between the change in firm numbers and the change in *PCM* other than a corresponding association between the change in firm numbers and the change in net profit. Second, I am not arguing that market structure should have any effect on the *collusive* price–cost margin.

The theory that I am testing relies on the *competitive* price–cost margin being a function of market structure: This is a standard result across oligopoly models. The mechanism I propose to test is as follows: The more successful is any specific cartel in limiting entry, the higher is the net profit of each cartel firm; and therefore the smaller is the decrease in the number of firms after the breakdown of collusion, and the larger is the decline in the long-run industry price–cost margin (and, by implication, net profit).

To test this mechanism, I will run a regression of the change in *PCM* between 1958 and 1973 (or 1968) on the change in ln*NFIRMS* and control variables between 1958 and 1973 (or 1968). I define Δ*PCM7358* (Δ*PCM6858*) as the difference between the 1973 (1968) value of *PCM* and the 1958 value of *PCM* in any given industry, Δln*NFIRMS7358* (Δln*NFIRMS6858*) as the difference between the 1973 (1968) and the 1958 value of ln*NFIRMS* in any given industry; and I construct control variables in the same way. The larger is the decrease in *NFIRMS* in the 15-year (10-year) period, the more negative is the value of Δln*NFIRMS7358* (Δln*NFIRMS6858*)—and for a few industries with an increase in firm numbers, Δln*NFIRMS7358* (Δln*NFIRMS6858*) is positive.

Since the 1956 Act appears to have had some effect even after 1968, a regression of Δ*PCM7358* on Δln*NFIRMS7358* may be preferable to a regression of Δ*PCM6858* on Δln*NFIRMS6858*. On the other hand, using the longer period might increase the weight of any unobservable factors that might influence the results. Results for both specifications will be presented below.[11] Note that working with an

[11] The mean of Δln*NFIRMS6858* across all previously collusive industries is −0.33, with a standard deviation of 0.30, and corresponds to a ratio of *NFIRMS* in 1968 to *NFIRMS* in 1958 of 0.75 (minimum 0.38, maximum 1.32). The mean of Δln*NFIRMS7358* is −0.43, with a standard deviation of 0.34, and corresponds to a ratio of *NFIRMS* in 1973 to *NFIRMS* in 1958 of 0.69 (minimum 0.36, maximum 1.12). Δln*NFIRMS7358* and Δln*NFIRMS6858* are strongly correlated, with a correlation coefficient of 0.91.

**Table 6** Regression results for the change in *PCM* in industries with a change of competition regime (*CHANGE* = 1) and in competitive industries (*CHANGE* = 0)

| | Industries with *CHANGE* = 1 | | Industries with *CHANGE* = 0 | |
|---|---|---|---|---|
| | Dependent variable: Δ*PCM6858* | Dependent variable: Δ*PCM7358* | Dependent variable: Δ*PCM6858* | Dependent variable: Δ*PCM7358* |
| Δln*SALES6858* | 0.012 (0.58) | – | −0.008 (−0.31) | – |
| Δln*(K/L)6858* | 0.049 (2.49) | – | 0.019 (0.85) | – |
| Δln*UNION6858* | −0.162 (−3.24) | – | −0.067 (−2.01) | – |
| Δ*IMPORT6858* | 0.117 (1.84) | – | 0.094 (1.27) | – |
| Δln*NFIRMS6858* | −0.046 (−2.91) | – | 0.022 (0.75) | – |
| Δln*SALES7358* | – | 0.024 (1.34) | – | 0.009 (0.27) |
| Δln*(K/L)7358* | – | −0.027 (−0.99) | – | 0.016 (0.97) |
| Δ ln*UNION7358* | – | −0.270 (−4.76) | – | −0.074 (−3.02) |
| Δ*IMPORT7358* | – | −0.168 (−2.75) | – | 0.061 (0.79) |
| Δln*NFIRMS7358* | – | −0.084 (−3.43) | – | −0.023 (−0.88) |
| Constant | 0.012 (0.80) | 0.118 (3.71) | 0.028 (1.40) | 0.016 (0.69) |
| $R^2$ | 0.40 | 0.39 | 0.36 | 0.39 |
| No. of observations | 38 | 36 | 55 | 55 |

t-statistics based on robust standard errors in parentheses. The lower number of observations in column 2 is due to two industries with missing values of *NFIRMS* for 1973

almost-balanced panel is essential for constructing these variables. The very unbalanced panel of four-digit industries that I used in Symeonidis (2002, ch. 7) would not be suitable for my present purposes.

My hypothesis is that for previously collusive industries Δ*PCM7358* (Δ*PCM6858*) is negatively correlated with Δln*NFIRMS7358* (Δln*NFIRMS6858*). In line with the theory, the coefficients on Δln*NFIRMS7358* and Δln*NFIRMS6858* will be interpreted as correlations, not causal effects. My specification for 1958–1968 is:

$$\Delta PCM6858_i = \alpha_i + \beta_1 \Delta \ln SALES6858_i + \beta_2 \Delta \ln (K/L)6858_i + \beta_3 \Delta \ln UNION6858_i + \beta_4 IMPORT6858_{it} + \beta_5 \Delta \ln NFIRMS6858_i + u_i,$$

and similarly for 1958–1973. *SALES*, *K/L*, *UNION* and *IMPORT* are the same as in Sect. 5. I include here the change in ln*SALES* as a control variable because it is likely to be correlated with the change in ln*NFIRMS*. I will report results with ln*UNION* instead of *UNION* because the latter was sometimes not statistically significant in preliminary regressions but its log transformation was. The remarks that were made in Sect. 5 on the control variables apply here as well and will not be repeated. Any time effects on *PCM* will be captured by the constant term of the regression.

The results from OLS regressions with robust standard errors are shown in Table 6. The first column is for 1958–1968, the second column for 1958–1973. It can be seen that the coefficients on Δln*NFIRMS6858* and Δln*NFIRMS7358* are

always negative and statistically significant: The larger is the decrease in firm numbers, the smaller is the decline in profitability.[12] The size of the coefficient varies across the two specifications, with an average of about −0.065: a 0.1 unit increase in $\Delta\ln NFIRMS7358$ is associated with a 0.65 percentage point decrease in *PCM*. To appreciate better what this number implies, let us compare two hypothetical collusive industries.

Suppose that in industry A, where entry barriers had been effective, the number of firms did not change in the long run after the breakdown of collusion: $\Delta\ln NFIRMS = 0$. This is a plausible benchmark, since in such an industry the firms would have been making significant excess profits under collusion and would be under little pressure to merge or exit when faced with more intense competition. Suppose also that in industry B, where entry had been unrestricted, the number of firms declined by 40% in the long run—a figure that is slightly higher than the average decline across all of the 38 industries in the sample and therefore probably close to the experience of a collusive industry with free entry: $\Delta\ln NFIRMS = \ln(60/100) \approx -0.51$.

My estimates imply that if the long-run price–cost margin of industry B stayed the same after the breakdown of collusion—as is suggested by the econometric evidence—the price–cost margin of industry A would decrease by $0.51/0.1 \times 0.65 \approx 3.3$ percentage points. Since the average value of *PCM* across manufacturing industries was about 0.18 in 1958, according to Table 1, collusion with restricted entry would imply a price–cost margin about 20% higher than the competitive *PCM*: a large effect.

Additional robustness checks included: 1) applying slightly different criteria to classify industries as collusive or competitive; 2) replacing *PCM* by ln*PCM* or using net output rather than sales revenue as the denominator of *PCM*; 3) replacing the capital-labour ratio by the capital stock of the average plant as a measure of entry costs; 4) replacing *IMPORT* by ln*IMPORT*; and 5) adding the change in average plant size as an additional regressor. The results were similar to those in Table 6.

Although the evidence that has been presented so far in this section is consistent with the theoretical predictions, a potential concern is that the correlation between the change in firm numbers and the change in profitability that is described in Table 6 may be driven by some factor other than the breakdown of collusion. To address this concern, I have run as a "placebo" test a similar set of regressions for the control group of 55 industries that did not experience a change of competition regime in the 1960s because they had never been subject to collusive agreements to any significant degree. If the correlations in the first two columns of Table 6 are driven by some factor other than entry conditions in collusive industries, one would expect to see similar correlations in the competitive group. The more dissimilar are the results for the two groups, the stronger is the case for an interpretation of the results in accordance with the theory of Sect. 2.

[12] *PCM* could even rise in a previously collusive industry with a substantial decrease in *NFIRMS*: some mechanisms that could generate this result were discussed in the previous section.

The results for competitive industries are presented in the last two columns of Table 6. The coefficients on $\Delta\ln NFIRMS6858$ and $\Delta\ln NFIRMS7358$ are very different for the control group than for collusive industries. Neither of the relevant control-group coefficients is statistically significant, and one is even positive. I regard this as fairly strong evidence that the results that are described by the coefficients on $\Delta\ln NFIRMS6858$ and $\Delta\ln NFIRMS7358$ in the first two columns of Table 6 are indeed driven by the breakdown of cartels.

## 7 Case Studies

Several of the industry case studies of the effect of the 1956 Act collected in Swann et al., (1973, 1974) read like variations on a theme that can be described as follows. Once a cartel breaks down, price competition intensifies and profits fall. The industry then goes through a period of restructuring through mergers and exit of firms, after which prices and profits rise.

Here I am interested in deviations from this pattern, which are not very many. Two cases will be discussed: secondary batteries and wallpaper. These illustrate how Selten's insight may sometimes be wrong and the conventional wisdom with respect to the effects of cartels correct—so that the breakdown of collusion may lead to a decline in long-run profitability and no significant change or even an increase in firm numbers. I will argue that the reason for this outcome—which goes against the average effects estimated in Sect. 5, but provides further support for the theoretical discussion of Sect. 2—is that entry was restricted in these two industries, unlike most other British collusive industries in the 1950s.

The secondary battery industry was subject to a variety of restrictive practices in the 1950s, including: fixed prices for replacement batteries; an understanding between the two leading producers that they would not canvass each other's customers in the automotive initial equipment market; and collective exclusive dealing, whereby buyers were required to deal only with cartel firms if they were to be supplied at all (Monopolies Commission, 1963). These arrangements were gradually abandoned, and by the mid-1960s competition had intensified. The prices of a leading producer increased by only 20% between 1963 and 1970, and some smaller manufacturers were selling at lower prices in 1970 than in 1963 even though the cost of materials more than doubled (Swann et al., 1973). But, although profits were falling during the 1960s and early 1970s and demand growth was moderate, the number of producers of automotive batteries (the main product of the industry) increased from 11 in 1963 to 16 in 1968 before falling back to 11 in 1973. This can be compared to an average fall in firm numbers of almost 20% between 1963 and 1973 in previously collusive industries, as is shown in Table 2.

The evolution of market structure and profitability in this industry, which was so different from that of most other industries that were affected by the 1956 law, can be explained by the initial conditions: the system of collective exclusive dealing, which had hindered entry and expansion by smaller firms, and the long-standing relationships of the leading producers with the car manufacturers. Because of these

entry barriers, the cartel firms had been earning supra-normal profits in the 1950s, so the fall in profit that was caused by the breakdown of collusion could be absorbed without a major restructuring. And without a major restructuring, profitability in the early 1970s showed little signs of a recovery.

The wallpaper industry had been dominated for most of its history by a single firm—Wall Paper Manufacturers Ltd. (WPM)—which had a market share of about 70% in 1958; the five-firm concentration ratio was 91.6% (Monopolies Commission, 1964). For much of the 1950s WPM, two other manufacturers and a large independent merchant agreed on the retail prices of wallpaper that was distributed by specialist merchants—although not on the prices of wallpaper that was sold through retail shops (about half of their sales). Price-fixing was reinforced by agreed discounts to distributors conditional on the volume of their purchases and an undertaking to buy exclusively from cartel firms.

Competition intensified gradually as the various restrictions were abandoned and the chemical firm ICI entered wallpaper manufacture in the early 1960s. Although WPM's market share was still 79% and the five-firm concentration ratio was 95.1% in 1962–1963, these plummeted in the years that followed. WPM was taken over by Reed Paper in 1965 and its share collapsed to 52% by 1977; whereas ICI's share rose to 25% by the late 1970s (Utton, 1986).

Several new firms entered after the mid-1960s, pushing up the total number of UK producers to 12 in 1968 and 15 in the mid-1970s, even though sales of wallpaper increased at a modest rate—in fact, the industry was described as stagnant by Hart et al. (1973). The five-firm concentration ratio fell to 89.7% in 1968, then 74.1% in 1975. This can be compared to the substantial increase of more than 10 percentage points, on average, in the five-firm concentration ratio across all British manufacturing industries during 1958–1975 (Symeonidis, 2000a, 2002). Profit margins fluctuated around 0.25 during the 1960s, then declined to 0.20 in the early to mid-1970s. By comparison, the average profit margin across all manufacturing industries was stable at about 0.22 over the same period.

The fall in concentration and profitability in the wallpaper industry in the 1960s and 1970s is the opposite of what happened in most other previously collusive industries. Part of the explanation must be the cartel's success in reinforcing entry barriers, which allowed cartel firms to enjoy excess profits in the 1950s and early 1960s. The removal of the collective entry restrictions was instrumental to the erosion of market power of WPM and the fall in concentration and profitability.

## 8 Discussion

The large majority of the British cartels of the 1950s did not control entry into their industries. The breakdown of collusion after the introduction of the 1956 Restrictive Trade Practices Act caused the number of firms in previously collusive industries to decline by about 15–20%, on average, but had no significant overall effect on long-run industry profitability. Crucially, the larger was the decrease in firm numbers, the smaller was the decline of long-run profit margins in previously collusive industries, but there is no such link in industries that had not been collusive.

These results are strong despite data limitations that could potentially "blur" them—in particular, the fact that I cannot capture variations in the "degree of collusion" or the exact timing of cartel breakdown across industries, since these cannot be determined from the available information. Case-study evidence from counter-examples suggests that, in industries where the cartels had successfully restricted entry, profitability declined and firm numbers did not decline when collusion broke down.

I believe that this evidence supports theories that emphasise the role of entry conditions for profitability. Cartels may often be unprofitable because of entry even when collusion is effective and prices are set well above what would have been competitive prices given the market structure of the collusive industry—although the cartels can make supra-normal profits to the extent that they successfully deter entry.

The large cartel price overcharges that are reported in the literature are typically derived by comparing observed cartel prices to estimates of the prices that would prevail in the absence of collusion. Even if the observed cartel prices are not subject to selection bias, the overcharges will be upward-biased if they fail to allow for the fact that, when a cartel cannot restrict entry, the long-run equilibrium number of firms will be smaller under competition than under collusion, and therefore the actual competitive price may be higher than the estimate. The overcharges will also be upward-biased if the cartel prices are observed during collusive periods for cartels that are subject to frequent price wars that act as a deterrent to entry.

Alternatively, the overcharges that are reported in the literature need not be biased and may occur when entry to a collusive industry is restricted by institutional factors or by the strategic behaviour of cartel firms. My estimates imply a value for the price–cost margin of a collusive industry that successfully restricts entry that is about 20% higher than the competitive level. This is not very different from the 23% median overcharge for all cartels or the 18% median overcharge for national cartels reported by Connor (2014). It is not much larger than the 15.5% "bias-corrected" mean overcharge estimate of Boyer and Kotchoni (2015).

How relevant are my findings for contemporary cartels? It is well known that although entry deterrence by cartel members is possible in principle (Harrington, 1989, 1991), many cartels have difficulty in effectively deterring entry in practice (Levenstein & Suslow, 2006). For instance, in a survey of 16 major cartels, Levenstein and Suslow (2004) report that entry occurred and was accommodated in 10 out of the 12 cases for which they had relevant information.

Thus my findings seem relevant not only for the relatively cartel-friendly environment of post-war Britain but also for any contemporary cartels that are long-standing, relatively stable and cannot easily deter entry—although they might be less relevant for secret cartels with only a few members in industries with significant barriers to entry. An important policy implication follows: The main welfare loss from collusion may often be, in addition to high prices and low output, the duplication of fixed costs through excess entry and possibly the survival of inefficient firms—rather than excess profits of cartel firms.

One issue that has not been emphasised here is the fact that price–cost margins can change when either prices or unit costs change. In this paper I have played down

this distinction and focused on the restructuring of previously collusive industries as the main reason for the absence of a long-run decline in profitability.

However, my results are not inconsistent with the view that another reason why profitability did not fall in most previously collusive industries was that some of the less efficient firms could not survive in the more competitive conditions of the 1960s and/or competition led to productivity gains for many surviving firms. In fact, this mechanism may have also been at work—I have shown elsewhere that collusive industries experienced significantly slower productivity growth in the 1950s than after the abolition of cartels, when compared to the control group of industries that were not affected by the 1956 law (Symeonidis, 2008). Still, to the extent that a fall in unit costs would have been largely passed through to prices, it is not clear how profitability could have been restored in the long run after the breakdown of cartels without the restructuring of previously collusive industries.

Another potentially relevant factor is the effect of the 1956 law on non-price variables such as advertising and R&D expenditure. Several studies (for instance, Fershtman & Gandal, 1994) have shown that collusion on price may lead to inefficient investment in non-price variables such as capacity or R&D. Overinvestment in non-price variables cannot in itself explain the link between changes in market structure and changes in price–cost margins that are documented in Table 6; but could it be an important reason for the absence of a significant average effect of cartel breakdown on profitability in the British case?

I think that the answer is no. Although data on capacity building before and after the breakdown of British cartels are not available, few collusive industries operated market-sharing schemes, so the allocation of quotas according to capacity that would lead to overproduction as in Röller and Steen (2006) cannot be a significant factor in the present context. Furthermore, I have shown in Symeonidis (2000b, 2002, 2019) that the effect of more intense price competition on advertising and the production of innovations in British industry in the 1960s and early 1970s was small or negligible. Therefore these factors are unlikely to explain the absence of any effect of the 1956 legislation on profitability.

In a recent study of EU mergers, Davies et al. (2015) discuss two possible explanations for why cartel breakdowns are often followed by merger activity in the affected industries (see also Dong et al., 2019 for an analysis of mergers as substitutes for cartels): One explanation is that firms try to restore market power and relax competition through mergers once a cartel is no longer an option. Another explanation is that mergers (and exit) are a response to negative net profits in the face of tougher competition. In earlier work (Symeonidis, 2002) I have called these explanations "behavioural" and "structural", respectively. Under the behavioural interpretation, the net profit of the marginal firm is always positive because firms either collude or merge to enhance their market power, so the restructuring of an industry after a cartel breakdown can be seen as a choice. Under the structural interpretation, restructuring is a necessity, because the net profit of the marginal firm is driven to zero in the long run by entry regardless of the competitive regime. It is not easy to distinguish empirically between the two, as Davies et al. recognise.

In this paper I have not carried out a direct test of the zero-profit condition—this is a difficult task without firm-level data on profits and fixed costs. The evidence

presented here suggests that an intensification of competition will have no effect on long-run profitability when there is free entry under different competition regimes. This could be consistent in principle with either of the two interpretations that I mentioned above. One concern with the behavioural interpretation is that it is not easy to think of a mechanism that could account for the existence across industries of excess profits for the marginal firm that are more-or-less constant irrespective of the competitive regime. In the absence of such a mechanism, my results imply that the zero-profit condition is a valid description of most industries—including many collusive industries. If one also accepts that free entry tends to eliminate excess profits in most competitive industries, at least in the absence of significant efficiency differences among firms, it follows that many cartels are unprofitable too.

## Appendix: Data Sources and Construction of Variables

The data sources on competition were described in the text. Data on output and sales revenue at current net producer prices, firm numbers and employment were obtained from the industry reports of the Census of Production (various years) and from Business Monitors. The figures are for all firms with at least 25 employees. Small corrections were sometimes made to ensure comparability over time. Producer price indices were obtained from the *Annual Abstract of Statistics*, the *Board of Trade Journal* and *Trade and Industry*, and Business Monitors. Some industry-specific price indices were constructed on the basis of data on volume of sales that were published, along with data on sales revenue, in the individual industry reports of the Census of Production.

Estimates of capital stock, defined as plant and machinery, at the three-digit level of aggregation are available from O'Mahony and Oulton (1990). These are net stock estimates constructed on the assumption of fixed and "short" asset lives and exponential depreciation rates. For a few cases where employment data were available at a more disaggregated level, I adjusted the O'Mahony and Oulton estimates on the basis of Census of Production data on the fraction of investment on plant and machinery that are accounted for by each "principal product" within any given three-digit industry.

Import penetration ratios were constructed from data that were obtained from the *Annual Abstract of Statistics*, the *Annual Statement of the Trade of the United Kingdom* (various years), Hughes and Thirlwall (1977) and surveys that were published in the February 1975 and August 1977 issues of *Economic Trends*. Because of difficulties in matching certain product definitions in the trade statistics with those in the Census of Production, some of the import penetration figures are approximate. Data on union density for the period examined here are available at a level of aggregation between the two-digit and the three-digit level and were taken from Bain and Price (1980).

R&D expenditure data were taken from *Research and Development Expenditure*, Studies in Official Statistics no. 21 (HMSO, 1973); *Research and Development: Expenditure and Employment*, Studies in Official Statistics no. 27 (HMSO, 1976); *Industrial Research and Development Expenditure and Employment*, Business

Monitor MO14 (various years); *Industrial Research in Manufacturing Industry: 1959–60* (Federation of British Industries, 1961); *Estimates of Resources Devoted to Scientific and Engineering Research and Development in British Manufacturing Industry, 1955* (HMSO, 1958); and *Industrial Research and Development Expenditure 1958* (HMSO, 1960). A comparison of the various sources suggests that there have not been many significant changes in R&D intensity between the late 1950s and the mid-1970s, so all of the sources were used to classify three-digit industries according to their typical R&D intensity, measured as the ratio of company-funded R&D to sales.

Data on advertising expenditure were obtained from the *Statistical Review of Press and TV Advertising*, published by Legion Information Services Ltd. until the early 1970s; and the *MEAL Monthly Digest of Advertising Expenditure*, published since 1968. These were matched with data on sales in the UK market that were derived by adding to the Census of Production sales figures the value of retained imports and subtracting the value of exports. The Legion/MEAL figures are sometimes incomplete and do not take into account discounts from published rates. To reduce any errors from these factors I adjusted the figures on the basis of data on aggregate annual advertising expenditure that were published in *Advertising Expenditure 1960* (Advertising Association, 1962) and, subsequently, in the *Advertising Quarterly*. Industries for which Legion/MEAL data were not available were generally easy to classify as low-advertising industries.

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