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The Effect of Competition on Wages and Productivity : Evidence from the UK

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Abstract: This paper examines the impact of competition on wages and productivity using a panel data set of UK manufacturing industries over 1954-1973. The introduction of cartel law in the UK in the late 1950s caused an intensification of price competition in previously cartelized manufacturing industries, but it did not affect those industries which were not cartelized. The econometric results from a comparison of the two groups of industries before and after the introduction of cartel law provide strong evidence of a negative effect of collusion on labour productivity growth. There is no evidence of any effect of collusion on wages. These results are robust to controlling for the potential endogeneity of collusion and are further strengthened by a comparison with US data.

JEL classification: L1, L6, J2, J3, O4

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1. Introduction.

It is often argued that the lack of competitive pressure may lead to inertia, low worker effort and managerial slack, and hence reduce the productivity growth of firms, or that it may result in monopoly rents being shared by workers through higher wages. The lack of competition may also allow an excessive number of firms to survive in an industry, including inefficient firms that would not survive under more competitive conditions, and hence further reduce productivity at the industry level. But although the view of competition as a stimulus to productivity growth is probably the majority position among policy-makers today, the theoretical and empirical support for this view has not, at least until recently, been very strong (see Nickell 1999 for a survey).

This paper provides an econometric analysis of the impact of competition (or rather the lack of it) on wages and labour productivity using evidence from a unique natural experiment that occurred in the UK in the 1960s. As a result of the introduction 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 not affected by the 1956 Act. A comparison of the two groups of industries over a twenty-year period, using data both before and after the implementation of the 1956 Act, can provide important insights on the links between the intensity of competition, wages and productivity.

The introduction of cartel legislation in the UK provides us with a way to by-pass two 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

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variables, including productivity, given that these variables may simultaneously affect one another, thus making the identification of one-way causal effects very difficult.

The present setup allows us to by-pass these difficulties because a change in the intensity of competition across a wide range of industries was 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 that were collusive and were therefore affected by the shift in cartel policy and industries that were competitive throughout the period examined. Moreover, the intertemporal structure of the data and the exogeneity of the institutional change allow 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. Any remaining concerns can be addressed by using information about the exogenous industry characteristics facilitating or hindering collusion across British industries in the 1950s in the context of a two-stage econometric procedure designed to control for the potential endogeneity of collusion. In other words, it is possible to address the most common criticism of studies using a difference-in-differences methodology, namely the potential endogeneity of the 'natural experiment' itself (see, for instance, Besley and Case 2000).

There is a large empirical literature on the effect of competition on wages and productivity. Many of these studies have relied mainly on measures of market structure and profitability as proxies for the intensity of competition (Nickell 1996; Nickell et al. 1994; Hay and Liu 1997; Disney, Haskel and Heden 2003). A potential difficulty with this approach is that the links between competition, market structure and profitability are complex and ambiguous. For instance, while most people would agree that an *exogenous* increase in industry concentration or a firm's market share will soften competition, it is also the case that concentration and market share may rise *as a result* of more intense competitive pressure (Symeonidis 2000a, 2002). Furthermore, profitability may fall in the short run but recover in the longer term

following an intensification of competition if there is free entry and exit and market structure is allowed to adjust (Symeonidis 2002). In other words, there is no simple relationship between market structure or profitability variables and the intensity of competition, so these variables are rather imperfect proxies for the intensity of competition.

To avoid these complications, some studies have focused on the effects of regulatory or economic reform in various countries (for instance, Djankov and Murrell 2002; Nicoletti and Scarpetta 2003). Others have examined the effects of trade liberalisation policies in developing countries or the EU single market program. Such work is particularly useful when significant policy changes are implemented within a short period of time, and even more so when firms or industries can be classified into groups according to whether they were more or less likely to be affected by the policy change – depending, say, on the degree of protection they enjoyed prior to the change (Tybout and Westbrook 1995; Bottasso and Sembenelli 2001; Griffith 2001). Most, but not all, of these studies have found positive effects of economic integration or deregulation on productivity, but since these policies often involve more than just an intensification of competitive conduct, it may not always be easy to draw clear implications for the competition-productivity relationship from these results. Schmitz (2005), who by-passes these difficulties by focusing on a very specific instance of foreign competition intensification in the US iron ore industry, has found a strong effect of competition on productivity. Competitive pressure may also intensify as a result of a significant decline in demand. For instance, the collapse of the steel market in the 1980s has led to substantial productivity improvements among iron-ore producers in several countries (Galdon-Sanchez and Schmitz 2002).

There have been very few statistical analyses of the links between collusion and wages or productivity growth. The only such study for the UK is the one by

Broadberry and Crafts (1996).[1] They found that productivity growth during 1954-1963 was slower in industries with restrictive agreements in the 1950s than in competitive industries. A potential limitation of the Broadberry and Crafts study derives from the use of cross-section data and the fact that they did not examine how the two groups of industries evolved after 1963, i.e. after competition intensified in those industries which were collusive in the 1950s. As pointed out above, it is generally very difficult to unravel the two-way link between competition and productivity growth using cross-section analysis. This difficulty can, however, be overcome by examining the evolution of the two variables over time – in particular, by examining the evolution of productivity across industries both before and after the implementation of the 1956 legislation. I carry out this analysis in this paper using a comprehensive data set on competition, explicit criteria to classify industries according to their competitive status, and a sample that extends over a long time period and covers the whole of manufacturing industry. In addition, this paper is, to the best of my knowledge, the first one to carry out a statistical analysis of the effect of collusion on wages.

My results provide strong evidence of a substantial negative effect of collusion on labour productivity growth. In particular, I find that labour productivity was growing much more slowly in collusive industries than in non-collusive industries before the implementation of the cartel legislation – after controlling for changes in market size, capital intensity, average plant size and differences in technological

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[1] None of the other existing studies of the effects of cartels and cartel policy in the UK has focused on the evolution of wages and productivity. Swann et al. (1973, 1974) conducted a series of industry case studies, focusing on the effects of the 1956 legislation on competition. The case-study evidence they report to support the hypothesis of a positive effect of the breakdown of British cartels on the efficiency of firms is very sketchy. Elliott and Gribbin (1977) examined the effect of the 1956 Act on concentration, while O'Brien et al. (1979) focused on the impact of the Act on mergers and profitability. Symeonidis (2000a, 2000b, 2002) provides a detailed econometric analysis of the impact of the 1956 Act on market structure, advertising, innovation and profitability, but not on wages and productivity.

opportunity across industries. Once the cartels were abolished, however, there was no longer any significant difference in the rate of growth of labour productivity between the two groups of industries. On the other hand, there is no evidence of any effect of collusion on wages of manual or non-manual workers. These results are robust to controlling for the potential endogeneity of collusion. I also compare the evolution of wages and productivity of UK industries with that of the same industries in the US, and I find further evidence for a negative effect of collusion on labour productivity growth.

2. The competition data.

Explicit restrictive agreements between firms were widespread in British industry in the mid-1950s: nearly half of manufacturing industry was subject to price-fixing. A detailed description of the institutional changes and of the evolution of competition in UK manufacturing from the 1950s to the early 1970s can be found in Symeonidis (1998, 2002). In what follows I briefly summarise the evidence and describe the construction of the competition data for this paper.

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 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 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.

Were the agreements effective? And to what extent did the intensity of price competition increase following the abolition of cartels? Case-study evidence (Swann et al. 1973, 1974) suggests that in most industries the agreements had been operated effectively 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. The evidence also indicates that price competition intensified in the short run in many industries following the abolition of cartels. However, in several cases agreements to exchange information on prices, price changes and so on replaced the former restrictive arrangements in the short run, and price competition emerged only after these information agreements were abandoned in the mid-1960s, i.e. about a decade after the 1956 Act was passed, following adverse decisions of the Restrictive Practices Court. In sum, while one cannot rule out cases of ineffective agreements in the 1950s or cases of collusion continuing secretly in the 1960s, the available evidence suggests that such cases were not numerous. The large majority of industries with collusive agreements in the 1950s did experience, sooner or later, an intensification of price competition as a result of the 1956 Act, and so it is, on the whole, legitimate to think of this evolution as a change of competition regime induced by an exogenous institutional change.

My 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. 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, not 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. Appendix 1 illustrates the classification of industries using particular examples.[2] All industries with ambiguous state of competition in the 1950s (and a few with ambiguous state of competition in the late 1960s and early 1970s) were excluded

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[2] In fact, out of 55 industries classified as competitive 25 were free from any significant restrictive agreements. I have used the 20% cut-off point because in some cases secondary industry products were subject to restrictive agreements, although core industry products were not. Furthermore, out of 38 industries classified as collusive 20 had agreements covering all or nearly all industry products. 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; clearly, one would expect a significant impact of the 1956 Act in such cases. The use of a continuous competition measure instead of cut-off points has proved impractical for a variety of reasons (see Symeonidis 2002 for an extensive discussion). However, 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 5. Symeonidis (2002) includes a detailed survey of restrictive agreements across all British manufacturing industries.

from the basic sample.[3] The dummy variable *COLL* was then defined: this takes the value 1 for industries that were collusive in the 1950s and experienced a change in competition regime sometime after 1958 and 0 for industries which were competitive throughout and therefore experienced no change in regime.

The panels used in this paper consist of 132 three-digit industries and five years: 1954, 1958, 1963, 1968 and 1973.[4] Note that, although the Restrictive Trade Practices Act was introduced in 1956, it was not until 1959 that industries, on the whole, started cancelling their agreements. Moreover, in several industries where competition did not break out immediately the Act did not have an impact until well into the 1960s.

3. Endogenous variables and descriptive statistics.

To study the effect of competition on wages and productivity, I estimate in this paper reduced-form equations for the determinants of labour productivity, *GPPER*, the average real wage of manual workers, *WOP*, and the average real wage of non-manual workers, *WOTH*, across British manufacturing industries over 1954-1973. In particular, *GPPER* is gross yearly output deflated by industry-specific price indices and divided by the average number of employees during the year. *WOP* is total yearly earnings of manual workers deflated by the retail price index and divided by the average number of manual workers during the year. And *WOTH* is total yearly

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[3] One industry (cement) that remained collusive throughout the period examined in this paper was also excluded from the sample to facilitate the interpretation of the results.

[4] The first four of these years are the only ones in the 1950s and 1960s for which comparable data on gross output, and hence on productivity, are available. 1973 is the last year before the oil crisis of the 1970s. I have excluded from my sample a few industries with significant government participation or intervention for part or all of the period under study (sugar, steel, aircraft, locomotives). In very few cases two or more three-digit industries were merged into one to ensure comparability over time.

earnings of non-manual workers deflated by the retail price index and divided by the average number of non-manual workers.

There are two reasons why I have chosen to focus on labour productivity rather than total factor productivity in this paper. First, the data on capital stock are estimates rather than primary data, and may therefore be subject to measurement error. Even though capital intensity must be included as a regressor in any econometric model of labour productivity, the use of labour productivity rather than total factor productivity as dependent variable implies that at least there will be no measurement error in the dependent variable itself. Second, constructing estimates of total factor productivity always involves making rather restrictive assumptions about the production function, and these assumptions are not innocuous. The reason for using gross rather than net output for my measure of labour productivity is simply that there are no reliable data on prices of inputs at the three-digit industry level for the period under study in this paper, so it is not possible to use appropriate deflators for net output.

GPPER, *WOP* and *WOTH* are all based on a measure of labour input that is not adjusted for hours worked. Data on hours worked during a typical week in any given industry and year are available only for manual workers and are often not fully comparable with the output data. In addition, data on weeks worked during any given year are not available for individual industries. However, to verify that my empirical results are not significantly affected by the lack of adjustment for hours worked I also constructed a series of labour productivity and wage rate estimates on the basis of the available data on hours worked and a number of simplifying assumptions. In particular, I assumed that hours worked of non-manual workers were the same as those of manual workers, that the number of weeks worked in any given year was the same across industries, and I also adjusted some of the figures to ensure comparability

across data sources and over time. The results I obtained using my alternative measures of productivity and wages were very similar to those reported below.[5]

Some descriptive statistics on the evolution of labour productivity and real wages over 1954-1973 are reported in Table 1. In particular, the table presents statistics on the average and median change in each of the three endogenous variables of interest over 1954-1958, 1958-1963, 1963-1968 and 1968-1973, separately for industries with $COLL = 0$ (i.e. industries that were competitive throughout the period) and industries with $COLL = 1$ (i.e. industries that were initially collusive but became competitive in the 1960s). There is some evidence of a negative effect of collusion on productivity. In particular, between 1954 and 1958, a period when the cartels were still in place, labour productivity increased two times faster in industries with $COLL = 0$ than in industries with $COLL = 1$ (average change of 9.8% versus 4.9%, median change of 9.4% versus 4.5%). On the other hand, between 1968 and 1973, when the cartels had generally been abolished, there is only a small difference in the rate of productivity growth between the two groups in proportional terms (average change of 23.5% versus 20%). However, the difference in absolute terms is not much smaller than in the 1950s. The statistics for real wages are more ambiguous: between 1954

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[5] The data on hours worked per week used for these calculations were taken from the *Ministry of Labour Gazette* and its successors, the *Employment and Productivity Gazette* and the *Department of Employment Gazette*. The data on weeks worked per year were taken from O'Mahony and Oulton (1994). Strike activity was not taken into account due to incomplete data, but the resulting measurement error in the productivity figures is likely to be small, since, even in the most strike-prone industries in my sample, the number of working days lost per employee in any given year due to strikes was typically not higher than two in the 1950s and 1960s and not higher than four in 1973. Furthermore, there was a tendency for the same small group of industries to exhibit intense strike activity over time, so part of the measurement error in the productivity figures caused by strikes will be picked up in a panel regression by the industry effects.

and 1958 the average wage of non-manual workers increased more in industries with *COLL* = 1 than in industries with *COLL* = 0, while the reverse was the case between 1968 and 1973. This would be consistent with the hypothesis of a positive effect of collusion on wages. However, exactly the opposite pattern can be observed for manual workers, whose average wage increased less in industries with *COLL* = 1 than in industries with *COLL* = 0 between 1954 and 1958, while the reverse was the case between 1968 and 1973. Clearly, it is difficult to draw any conclusions on the basis of these descriptive statistics, as the comparisons do not control for changes in other variables. To unravel any links between competition, wages and productivity, it is necessary to turn to the econometric analysis.

Some potential concerns can nevertheless be addressed on the basis of the descriptive statistics alone, before turning to the econometrics. It is well known, and is also clear from comparing columns 1 and 4 of Table 1, that productivity growth is procyclical. In the present data, there is a strong positive correlation between ln*GPPER* and ln*DS*, where *DS* is deflated sales revenue. There is also evidence from Table 1 of slow growth across all British manufacturing industries during the period 1954-1958; in fact the British economy was in a mild recession in 1958. (The figures for gross output are very similar to those for sales revenue throughout.) Could it be the case that the difference we observe in labour productivity growth between collusive and competitive industries during 1954-1958 is due to a difference in sales/output growth in the two groups of industries? This could happen if for some reason collusive industries in the UK were more sensitive to cyclical fluctuations and hence experienced a more significant slowdown in sales/output growth during 1954-1958 than competitive industries.

Table 1 suggests that this is not the case. Sales revenue was growing faster for industries with *COLL* = 0 than for industries with *COLL* = 1 during most of the period examined in this paper. This is not surprising given that the industries with *COLL* = 0 tended on the whole to be more advertising-intensive, more R&D-intensive, less unionised and less capital-intensive (see Symeonidis 2002, 2003, and Table 5 below), and such industries typically experience relatively fast growth. However, the difference in the sales growth rate between the two groups of industries was more pronounced *after* the collusive agreements had generally been abandoned (1963-1973) than in the period when the cartels were in place (1954-1958). In fact, the median change in *DS* was essentially the same during 1954-1958 across the two groups (5.5% versus 5.6%), so the period 1954-1958 was the only one when the sales growth rate did not differ much across the two groups. In other words, the collusive UK industries appear to have been affected less, not more, than the competitive industries by the general slowdown in economic growth during 1954-1958, but they nevertheless experienced significantly lower labour productivity growth than the competitive industries. If the differences in labour productivity growth between the two groups of industries were largely driven by cyclical fluctuations, one would expect this difference to be (i) very small or non-existent during 1954-1958, and (ii) significantly larger in later periods – in other words, exactly the reverse of what we observe. Clearly, the figures in Table 1 cannot be explained by cyclical differences across industries and/or periods.

A second potential concern is that whatever difference we observe in labour productivity growth between collusive and competitive industries during 1954-1958 may not be due to the competitive regime but to some fundamental characteristic that differs between the two groups of industries. For instance, collusive industries tended

to be more capital-intensive and less advertising-intensive than competitive industries. Could it be the case that these or some other industry characteristics rather than collusion were the reason for the relatively lower average productivity growth of these industries during 1954-1958? This seems unlikely, since it is not then easy to explain why the difference in the productivity growth rates is much smaller after 1963 – the previously collusive industries did not become less capital-intensive or more advertising-intensive after that date. In any case, one can attempt to address this concern by comparing the evolution of the two groups of industries in another country, such as the US, over the same period: industry characteristics such as relative capital intensity or advertising intensity were generally not very different across the two countries, while competition laws were. In addition, one can compare the UK collusive industries with the same industries in the US, effectively using that group of US industries as an alternative control group. US manufacturing industry was largely free from collusion during the 1950s, and it is possible, on the whole, to construct 3-digit US industry definitions that match the UK 3-digit industry definitions by aggregating 4-digit industry US data.

The data for these comparisons are presented in Table 2. The variables are defined in the same way as for Table 1. Let us first compare two groups of US industries: those that correspond to UK industries with $COLL = 1$ and those that correspond to UK industries with $COLL = 0$. Clearly, there are no significant differences in the evolution of labour productivity or real wages between the two groups during 1954-1958 or in any other period between 1954 and 1973. This implies that there are probably no fundamental industry characteristics that may have caused one group to have generally higher productivity growth than the other over specific periods. Now let us also compare the UK collusive industries with the same group of

industries in the US.[6] Average and median labour productivity growth for these industries in the UK were less than half the US figures during 1954-1958, and they were also significantly lower than the US figures during 1958-1963. However, this pattern is reversed after 1963 and even more so after 1968, when the UK cartels were no longer in place: average and median productivity growth were then higher in the previously collusive UK industries than in the same group of industries in the US. It is very difficult to think of an explanation for this overall pattern other than a significant negative effect of collusion on labour productivity growth. All in all, Table 2 strongly suggests that the presence of collusion was indeed the reason for the relatively low labour productivity growth of the collusive industries in the UK during 1954-1958 (or 1954-1963).

4. Econometric model.

The econometric specifications used in this paper are panel data models with individual-specific (industry) effects. My approach is essentially based on the difference-in-differences methodology, which consists in comparing the difference between the average change in the variable of interest in the experimental group and the average change in the same variable in the control group. Time dummies are also included among the regressors in an attempt to control for other factors that may have influenced the evolution of wages and labour productivity over the period examined, such as human capital accumulation, the progressive opening of the British economy, the UK government's prices and incomes policies between 1965 and 1973, and

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[6] The fact that the number of US industries is slightly smaller than the number of UK industries (33 versus 36) does not affect this comparison. For instance, if we exclude the UK collusive industries for which there are no comparable US data, the UK figures for average $\Delta \ln GPPER$ become: 4.9 in 1954-1958, 14.5 in 1958-1963, 17.5 in 1963-1968 and 19.5 in 1968-1973.

macroeconomic policy. It is difficult to measure these factors at the industry level, but it is plausible to assume that their effect would have been more or less equally realised across all industries, or at least that there would not be a systematic difference between industries with $COLL = 1$ and those with $COLL = 0$ with respect to these factors.

Although I do not attempt to control for the intensification of foreign competition caused by the gradual opening of the British economy during the 1960s other than by the use of time dummies,[7] it is also the case that the extent of foreign competition does not seem to have been different in the two groups of industries, i.e. the group with $COLL = 1$ and the one with $COLL = 0$, during the period examined here. As pointed out in Symeonidis (2003) and confirmed in Table 5 below, there is no evidence of any difference between the two groups with respect to the intensity of foreign competition in the 1950s. Moreover, there is no evidence that changes in the intensity of foreign competition were any different between the two groups in the 1960s. In particular, Kitchin (1976) provides estimates of effective protection for 1963 and 1968 at a level of aggregation between the two-digit and the three-digit industry level. Effective tariff protection increased, according to these figures, in 6 out of 12 industries/sectors that I could classify as collusive in the 1950s and decreased in the other 6. For industries/sectors that I could classify as competitive throughout the

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[7] Ideally, one would need some measure of the extent of foreign competition for each industry across time. Two possible candidates are the import penetration ratio and the rate of effective protection. However, there are serious problems, theoretical and practical, with both of these measures. Estimates of effective rates of protection are available at a high level of aggregation and only for a few of the years in my sample; also, they are often subject to measurement error. The import penetration ratio, on the other hand, is a poor proxy for the extent of foreign competition, since it cannot capture the effect of the mere threat of competitive imports, it does not take into account imports by domestic producers (which may not be in competition with domestic products), and it is itself endogenous. Moreover, the industrial classification used in the foreign trade statistics during the period examined here has been subject to changes over time and is difficult to match with the one used in the Census of Production.

period under study the respective numbers were 8 and 10. Tariff protection changes before 1963 were far less pronounced. In summary, it is not unreasonable to argue that the estimated effect of collusion from my regressions in this paper is not biased by the lack of a better control for the effect of foreign competition.

My basic specification for labour productivity is

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\begin{aligned}
\ln GPPER_{it} = \alpha_i &+ \beta_1 \ln DS_{it} + \beta_2 \ln(K/L)_{it} + \beta_3 \ln PLANTSIZE_{it} \\
&+ \beta_4 Y54 + \beta_5 Y58 + \beta_6 Y68 + \beta_7 Y73 \\
&+ \beta_8 RD*Y54 + \beta_9 RD*Y58 + \beta_{10} RD*Y68 + \beta_{11} RD*Y73 \\
&+ \beta_{12} AGREE*Y54 + \beta_{13} AGREE*Y58 + \beta_{14} AGREE*Y68 + \beta_{15} AGREE*Y73 \\
&+ \beta_{16} COLL*Y54 + \beta_{17} COLL*Y58 + \beta_{18} COLL*Y68 + \beta_{19} COLL*Y73 + u_{it},
\end{aligned}
$$

and similarly for $\ln WOP$ and $\ln WOTH$. *DS* is industry sales revenue deflated by an industry-specific producer price index and serves as a proxy for market size. The idea here is that market size may have a positive effect on productivity through static or dynamic scale economies or changes in factor utilisation, and a positive effect on wages through its effect on labour demand. In fact, we have already seen that labour productivity is procyclical in the present dataset.[8] *K/L* is the capital-labour ratio, while *PLANTSIZE* is industry employment divided by the number of plants and is used as a measure of average plant size. One reason for including *PLANTSIZE* among the regressors is the idea that the effects of unionisation may be more pronounced in

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[8] The potential endogeneity of prices may be a cause for concern. However, using the general producer price index as a deflator to construct an alternative measure of sales revenue gave broadly similar results to those reported here. If there were an endogeneity problem, one would expect the two sets of results to differ, and the fact that they don't suggests that the potential endogeneity of prices is probably not a serious problem in the present case. A different cause of concern may be the possibility of spurious correlation between *DS* and *GPPER*. While this is possible, note that *DS* is included here as a control variable. The question of interest is the effect of collusion on *GPPER*. Controlling for market size is preferable to not including *DS* in the regressions, as this would bias the results if, as seems likely, *DS* is correlated with other exogenous variables.

larger plants than in smaller ones (Prais 1981). *Y54*, *Y58*, *Y68* and *Y73* are time dummies for 1954, 1958, 1968 and 1973 respectively. *AGREE* is the percentage of employees in any given industry covered by collective bargaining agreements (whether national, local or firm-level) in 1973. Any effect of collective bargaining on the evolution of wages and productivity should be at least partly captured by the interaction terms *AGREE*Y54*, *AGREE*Y58*, *AGREE*Y68* and *AGREE*Y73*.[9] *RD* is a dummy which takes the value 0 for industries with average or typical R&D-sales ratio lower than 2% over 1954-1973 and 1 otherwise. To the extent that the effect of innovation on productivity growth over time is more pronounced in R&D-intensive industries than in low-R&D industries, it should be at least partly captured by the interaction terms *RD*Y54*, *RD*Y58*, *RD*Y68* and *RD*Y73*.[10] Details on variable definition and data sources are provided in Appendix 2.

The interaction terms *COLL*Y54*, *COLL*Y58*, *COLL*Y68* and *COLL*Y73* should pick up any differences between industries with *COLL* = 1 and those with *COLL* = 0 that are due to the different competitive regimes facing the two groups of

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[9] The year 1973 is the first year for which data on collective bargaining arrangements at the three-digit industry level are available. The implicit assumption here is that, although the percentage of the workforce covered must have changed over time in any given industry, inter-industry differences in coverage probably did not change very much during 1954-1973 and were primarily determined by exogenous industry characteristics. In preliminary regressions I also included among the regressors union density, measured at a level of aggregation between the two-digit and the three-digit industry level. Unlike data on collective bargaining coverage, union density data are available for the whole period under study, thus allowing for an analysis of the effect of *changes* in union density on the evolution of wages and productivity. This variable was nowhere statistically significant; since it was also available only at a low level of aggregation, it was dropped from the final specification.

[10] These interaction terms are not endogenous: whether the typical R&D-sales ratio in each particular industry will be higher or lower than 2% is determined by exogenous industry characteristics (see also footnote 12 below). In contrast, including a measure of innovations as a regressor would be problematic because of concerns with potential endogeneity.

industries. For instance, the coefficient on *COLL*Y54* allows for a comparison of the evolution of wages and productivity in the two groups of industries between 1954 and 1963, the coefficient on *COLL*Y73* allows for a similar comparison between 1963 and 1973. The benchmark year is 1963. The choice of 1963 as the benchmark year may require some justification in light of the fact that the shift in cartel policy may have had some effect in several previously collusive industries before 1963. However, preliminary results suggested that a shift in the evolution of labour productivity in the group of industries with a change of competition regime occurred around 1963 rather than 1958. This is not surprising, given that competition emerged only slowly in many of these industries following the implementation of the 1956 Act, and it is also consistent with the fact that most of the restructuring of previously collusive industries occurred during 1963-1968 (see Symeonidis 2000a, 2002). I have therefore chosen to use 1963 as the benchmark year in my econometric specification.

Note that the above specification is not meant to tell us anything about *the level* of productivity or wages in the two groups of industries in any given year, since industry effects are included among the regressors. My difference-in-differences approach relies on comparing *the evolution* of wages and productivity in the two groups and indicating what part of the difference is due to the type of competitive regime facing firms. Furthermore, this specification is different from most other studies of the effect of competition on wages and productivity in that a measure of market structure is not included among the regressors. This is because my specification is a reduced-form equation and market structure must clearly be regarded here as endogenous. In particular, market structure is itself a function of the competitive regime: I have shown elsewhere (Symeonidis 2000a, 2002) that the intensification of price competition following the abolition of the British cartels caused a rise in concentration in previously collusive industries.

Let me also point out that it is not possible to use a dynamic panel data model in the present context. Such an approach would imply 'losing' the first one or two

periods in the panel, and would therefore amount to eliminating most of the variation picked up by the interactions of *COLL* with the time dummies. Given that the years in the panel are separated by periods of four to five years, however, it is not clear why there should be any significant effect of lagged values on the endogenous variables in my regressions because of adjustment lags or for other reasons.

A rather more serious objection to the above specification is the potential endogeneity of *COLL*. More specifically, the objection is that whatever difference one may observe in the evolution of wages and productivity before or after 1963 between industries with *COLL* = 1 and industries with *COLL* = 0, after controlling for changes in the other explanatory variables, may be to some extent due to unobserved characteristics that differ between the two groups of industries rather than to the degree of competition. Of course, the fact that the evolution of productivity and wages in the two groups is examined here both before and after the effective implementation of the cartel legislation constitutes in itself an indirect check of the exogeneity of *COLL*. If, for instance, the two groups were found to be similar after 1963, then it would probably be safe to conclude that any observed difference between them before 1963 was due to collusion.

To address the potential endogeneity concern more formally, however, one needs to instrument *COLL*. I will therefore also report results from a two-stage procedure where *COLL* 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 across industries in the 1950s on a set of exogenous industry characteristics. An additional advantage of the two-stage estimates is that they will not be affected by any measurement error in my collusion variable due to unidentified or ineffective collusive agreements or agreements continuing well into the 1960s. In particular,

these estimated probabilities are the fitted values from the following cross-section probit regression[11]:

$$COLL_i^* = \alpha_i + \beta_1 \ln DS_i + \beta_2 \ln K/L_i + \beta_3 \ln PLANTSIZE_i + \beta_1 AGREE_i$$
$$+ \beta_5 RD_i + \beta_6 ADV_i + \beta_7 FOREIGN_i + u_i$$

where instead of the "propensity to collude" COLL*, an unobserved latent variable, we observe the dichotomous variable *COLL* which takes the value 0 for industries without collusive agreements in the mid- and late 1950s and 1 for cartelised industries.[12] *ADV* is a dummy variable which is equal to 0 for industries with advertising-sales ratio lower than 2% in the mid-1950s and 1 otherwise, and *RD* is a dummy which takes the value 0 for industries with R&D-sales ratio lower than 2% and 1 otherwise. These dummies are intended to capture the effect of advertising effectiveness and technological opportunity, respectively, on the likelihood of collusion.[13] *FOREIGN* is a dummy variable which takes the value 0 for industries

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[11] Using a linear regression for the first stage estimates produces second-stage results which are almost identical to those reported in Table 4.

[12] Five industries which were collusive in the 1950s and had ambiguous state of competition in the late 1960s and early 1970s were included in the sample for the probit regression, as was one industry that remained collusive throughout the period examined in this paper.

[13] The idea here is that while the advertising-sales ratio and the R&D-sales ratio are endogenous, it is generally exogenous industry characteristics that will determine whether these ratios are above or below 2%. Thus, for an industry below the 2% cut-off point, advertising/R&D is not a very important strategic variable: in such an industry, advertising is not very effective in raising consumers' willingness to pay or there is little scope for technological innovation from within the industry. On the other hand, in an industry above the 2% cut-off point, advertising/R&D "works". Of course, whether such an industry has an advertising-sales ratio or R&D-sales ratio of 5% or 10%, say, may be largely determined endogenously. But my binary variables *ADV* and *RD* are not very sensitive to endogenous factors that affect advertising and R&D intensity. The assumption of exogeneity of *ADV* and *RD* is also consistent with the fact that a comparison of advertising-sales ratios and R&D-

with relatively high protection in the mid-1950s and the value 1 for industries with relatively low protection. It was constructed on the basis of the Kitchin (1976) estimates of effective protection rates for 1963 and other available information for the 1950s. (Tariff protection changes between the mid-1950s and 1963 were not very significant.) All the other variables are as previously defined. For *DS*, *K/L* and *PLANTSIZE*, I use 1958 data. However, the results turn out to be very similar when using 1954 data instead.

5. Results.

The estimation results for ln*GPPER*, ln*WOTH* and ln*WOP* are presented in Tables 3 and 4, while the results of the first-stage probit model for *COLL* are presented in Table 5.[14] The results in Tables 3 and 4 are for a fixed-effects specification. Robust standard errors were derived for Table 3. The standard errors for the two-stage model (Table 4) were derived using a bootstrap procedure with 1000 replications.

Table 3 contains the results from the one-stage regressions, while Table 4 contains the two-stage results. For the regressions in Table 3, industries with ambiguous state of competition in the 1950s or in later years are excluded. In contrast, Table 4 makes use of nearly all the industries in the data set, excluding only those with ambiguous state of competition in the late 1960s and early 1970s as well as one industry that remained collusive throughout.[15]

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sales ratios across various years revealed very few instances where an industry had moved from below 2% to above 2% or vice versa; and in most cases this was due to an exogenous institutional change, namely the introduction of TV advertising in the UK in the mid-1950s.

[14] The results in Table 5 are consistent, on the whole, with those reported in Symeonidis (2003), where the determinants of collusion are analysed in greater detail using data at the four-digit industry level.

[15] I also ran two-stage regressions using the same sample as in Table 3. The results were very similar to those reported in Table 4.

All the results provide clear evidence of a negative effect of collusion on labour productivity growth. The coefficient on *COLL*Y54* is positive and statistically significant in all regressions with ln*GPPER*, and it is larger in the two-stage than in the one-stage regression results. In particular, the one-stage regression results suggest that the net influence of collusion was a six to eight percentage point difference in labour productivity growth between non-collusive and collusive industries over the period 1954-1963. The two-stage regression results, on the other hand, suggest that the influence of collusion was a 20 to 30 percentage point difference in labour productivity growth between the two groups of industries over the period 1954-1963, and that most of the negative effect of collusion on productivity occurred during 1954-1958, i.e. before industries started abandoning their restrictive agreements. Note that the difference between one-stage and two-stage results is smaller in the specification without the interaction terms *AGREE*Y54*, *AGREE*Y58*, *AGREE*Y68* and *AGREE*Y73*.[16] After 1963, however, there was no longer any significant difference in the rate of growth of labour productivity between the two groups of industries: the coefficients on *COLL*Y68* and *COLL*Y73* are nowhere statistically significant at the 5% level.

To get a better perspective on the magnitude of the effect of collusion on labour productivity growth, note the average change in labour productivity in non-collusive industries during 1954-1963, after controlling for market size, the capital-labour ratio, average plant size, the interactions of the R&D dummy with the year dummies and fixed industry effects. This is given by the coefficient on *Y54*, using 1963 as the benchmark: an increase of 11% according to the one-stage results or 16%

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[16] These terms are not statistically significant in the productivity regressions, and might even bias the coefficients on *COLL*Y54* and *COLL*Y58* to the extent that union power and inefficient work practices are one channel through which the lack of competition slows down productivity growth (see Schmitz 2005). Dropping the unionisation terms from the wage regressions has no significant effect on the coefficients on *COLL*Y54* and *COLL*Y58*.

according to the two-stage results. On the other hand, the sum of the coefficient on *Y54* and the coefficient on *COLL*Y54* is a measure of the average change in labour productivity in the collusive industries during 1954-1963, again after controlling for the same set of explanatory variables. This turns out to be an increase of just 3% according to the one-stage results or a decrease of 2% according to the two-stage results. By contrast, during 1963-1973 labour productivity increased in both groups of industries by about 16%, after controlling for the same set of explanatory variables. These results suggest a substantial negative effect of collusion on labour productivity growth, even more pronounced than the one suggested by the descriptive statistics in Table 1.

There is no evidence of any effect of collusion on real wages of manual or non-manual workers in the present data. One possible explanation is that wages were not very sensitive to competitive pressure from the product market during the period examined in this paper since they were to a considerable extent determined by institutional factors (including centralised bargaining and Wage Council orders) and government policy (including incomes policy). This interpretation is supported by the fact that the variation in productivity change across industries was much larger than the variation in earnings change throughout the period under study – see Table 1 and Wragg and Robertson (1978). Another possible interpretation is that, while collusion may generally have a positive direct effect on wages, it also reduces productivity growth at the industry level, and hence has a negative indirect effect on wages. A third possibility is that less efficient firms pay lower wages than more efficient firms in any given industry, so when an intensification of competition leads to the exit of less efficient firms, the effect on the average industry wage is ambiguous even if the wage in each surviving firm has fallen. I should also emphasise that the absence of any effect of collusion on wages is not inconsistent with the results from a number of

theoretical models of bargaining under oligopoly.[17] For instance, Dhillon and Petrakis (2002) have shown that, under centralised union-oligopoly bargaining, the wage is independent of the degree of market power for fairly general conditions. Under decentralised bargaining, on the other hand, the effect of collusion on wages is ambiguous because of two opposing effects: on the one hand, collusion increases profit margins and hence the ability of unions to push for higher wages; on the other hand, collusion reduces output and increases competition between unions for shares in employment, and this tends to push wages down (Dowrick 1989). Note that wage bargaining in the UK during the period under study was generally a mixture of centralised and plant-level bargaining (Ulman 1968).

An interesting feature of the results in Tables 3 and 4 is the magnitude of the coefficients on the year dummies. After controlling for market size, the capital-labour ratio, plant size, cross-industry differences in technological opportunity, the effect of collusion and industry effects, both labour productivity and real wages were rising rapidly during 1963-1973 and possibly also during 1954-1963 as well. To some extent this must reflect technological progress and human capital accumulation during this period. It may also be partly due to measurement error in the capital-labour ratio, K/L. Recall that the capital stock figures are estimates rather than primary data. As K/L was increasing across industries throughout the period, it is correlated with the time dummies; so to the extent that there is measurement error in K/L, the effect of capital intensity could be partly picked up by the time dummies. In any case, even though some of the rise in wages and productivity during 1954-1973, and especially during 1963-1973, seems to have been due to factors not explicitly included in the present specification, this does not invalidate the comparison between industries collusive in

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[17] While these theoretical results have been derived in models with a fixed number of firms, the mechanisms driving the results would still operate in a context with endogenous market structure.

the 1950s and industries competitive throughout the period to the extent that these other factors are not correlated with the variable *COLL*.

Most of the other explanatory variables in Tables 3 and 4 have the expected signs and are usually statistically significant. In particular, there is strong evidence that capital intensity increases labour productivity and wages of manual workers (although it has no clear effect on the wages of non-manual workers). Market size increases both wages and productivity, as expected. An increase in average plant size is associated with a fall in productivity and an increase in the wages of manual workers, which is consistent with the view that average plant size is positively correlated with union power. There is evidence from the one-stage model that an increase in the proportion of the workforce covered by collective bargaining agreements tends to increase wages; however, the effect is no longer statistically significant in the two-stage model. On the other hand, there is little evidence that R&D-intensive industries tend to exhibit higher productivity growth. This could be at least partly due to the fact that product quality improvements in R&D-intensive industries are not adequately captured by the productivity measure used in this paper, but it is also consistent with the idea that the effect of innovation on productivity works mainly through the use rather than the production of innovations.

6. Concluding remarks.

The introduction of cartel laws in the UK in the late 1950s caused an intensification of price competition in previously collusive manufacturing industries, but it did not affect industries which were not collusive. The econometric results from a comparison of the two groups of industries provide strong evidence of a negative effect of collusion on labour productivity growth. There is no evidence of any effect of

collusion on wages. These results are robust to controlling for the potential endogeneity of collusion and are further strengthened by a comparison with US data.

There are several possible mechanisms that could explain the negative effect of collusion on labour productivity, but in the absence of firm-level data it is difficult to distinguish between these mechanisms. Thus, it is difficult to say to what extent productivity improvements under more competitive conditions were brought about through larger effort by managers or through the more efficient use of capital or labour. One mechanism which does not appear to have played a significant role in the present case is innovation, at least innovations produced (as opposed to innovations adopted) by firms: I have shown elsewhere (Symeonidis 2002) that the introduction of cartel legislation in the UK had no significant effect on the production of innovations by firms in previously collusive industries. On the other hand, the significant restructuring of these industries through merger and exit (Symeonidis 2000a, 2002) must be part of the explanation for the positive effect of competition on labour productivity: collusive agreements seem to have allowed inefficient firms to survive or maintain significant market shares, and this was no longer possible for many of these firms once competition intensified.

Table 1. Average change in ln*GPPER*, ln*WOP*, ln*WOTH* and ln*DS*: 1954-1958, 1958-1963, 1963-1968 and 1968-1973.

	Δln*GPPER*	Δln*WOP*	Δln*WOTH*	Δln*DS*
Industries with *COLL* = 1 (n = 36)				
1954-1958	0.049 [0.045] (0.066)	0.072 [0.073] (0.036)	0.048 [0.046] (0.066)	0.057 [0.055] (0.135)
1958-1963	0.144 [0.147] (0.109)	0.117 [0.114] (0.033)	0.101 [0.105] (0.040)	0.153 [0.128] (0.173)
1963-1968	0.171 [0.186] (0.099)	0.131 [0.127] (0.036)	0.091 [0.100] (0.053)	0.097 [0.096] (0.147)
1968-1973	0.201 [0.188] (0.123)	0.194 [0.189] (0.056)	0.123 [0.123] (0.072)	0.100 [0.139] (0.132)
Industries with *COLL* = 0 (n = 52)				
1954-1958	0.098 [0.094] (0.087)	0.084 [0.088] (0.054)	0.032 [0.033] (0.077)	0.074 [0.056] (0.196)
1958-1963	0.181 [0.163] (0.122)	0.109 [0.109] (0.055)	0.096 [0.098] (0.054)	0.219 [0.206] (0.202)
1963-1968	0.233 [0.221] (0.116)	0.146 [0.143] (0.045)	0.111 [0.115] (0.057)	0.232 [0.209] (0.246)
1968-1973	0.235 [0.231] (0.184)	0.175 [0.171] (0.070)	0.130 [0.126] (0.070)	0.203 [0.172] (0.192)

Notes: Figures based on industries with available data for all five years. The figures in brackets are medians, those in parentheses are standard deviations. n denotes the number of industries.

Table 2. US industries: Average change in ln*GPPER*, ln*WOP*, ln*WOTH* and ln*DS*: 1954-1958, 1958-1963, 1963-1968 and 1968-1973.

	Δln*GPPER*	Δln*WOP*	Δln*WOTH*	Δln*DS*
US industries corresponding to UK industries with *COLL* = 1 (n = 33)				
1954-1958	0.117 [0.123] (0.085)	0.087 [0.093] (0.040)	0.053 [0.040] (0.063)	0.076 [0.050] (0.146)
1958-1963	0.195 [0.192] (0.099)	0.118 [0.114] (0.036)	0.114 [0.111] (0.046)	0.188 [0.172] (0.139)
1963-1968	0.144 [0.138] (0.066)	0.068 [0.060] (0.037)	0.089 [0.091] (0.055)	0.246 [0.228] (0.158)
1968-1973	0.143 [0.131] (0.113)	0.055 [0.062] (0.055)	0.039 [0.035] (0.054)	0.178 [0.199] (0.155)
US industries corresponding to UK industries with *COLL* = 0 (n = 46)				
1954-1958	0.129 [0.137] (0.092)	0.087 [0.083] (0.051)	0.050 [0.035] (0.111)	0.116 [0.088] (0.210)
1958-1963	0.181 [0.183] (0.116)	0.095 [0.105] (0.068)	0.109 [0.119] (0.076)	0.239 [0.233] (0.180)
1963-1968	0.142 [0.129] (0.080)	0.070 [0.063] (0.039)	0.091 [0.090] (0.081)	0.264 [0.240] (0.184)
1968-1973	0.161 [0.151] (0.127)	0.023 [0.039] (0.060)	0.032 [0.034] (0.095)	0.144 [0.215] (0.258)

Notes: Figures based on industries with available data for all five years and that could be matched with UK industries. The figures in brackets are medians, those in parentheses are standard deviations. n denotes the number of industries.

Table 3. Regression results for ln*GPPER*, ln*WOP* and ln*WOTH*: one-stage estimation

	Dep. variable: ln*GPPER*				Dep. variable: ln*WOP*		Dep. variable: ln*WOTH*	
ln*DS*	0.406	(12.72)	0.404	(12.78)	0.042	(3.59)	0.039	(2.55)
ln*K/L*	0.241	(4.87)	0.243	(4.91)	0.094	(6.32)	0.025	(1.57)
ln*PLANTSIZE*	−0.188	(−3.87)	−0.187	(−3.90)	0.082	(3.61)	0.047	(1.89)
Y54	−0.228	(−3.40)	−0.109	(−3.37)	−0.057	(−1.80)	0.082	(1.93)
Y58	−0.047	(−0.73)	−0.064	(−3.37)	−0.006	(−0.24)	−0.011	(−0.32)
Y68	0.103	(2.01)	0.084	(3.87)	0.116	(4.51)	0.097	(2.76)
Y73	0.159	(1.95)	0.155	(4.25)	0.153	(4.15)	0.111	(2.47)
*RD*Y54*	−0.044	(−0.74)	−0.038	(−0.64)	0.045	(2.45)	−0.027	(−0.83)
*RD*Y58*	−0.028	(−0.57)	−0.030	(−0.64)	−0.012	(−0.64)	−0.015	(−0.65)
*RD*Y68*	−0.0001	(−0.00)	−0.001	(−0.03)	−0.006	(−0.37)	0.027	(1.27)
*RD*Y73*	0.108	(1.44)	0.108	(1.53)	−0.003	(−0.16)	0.013	(0.69)
*AGREE*Y54*	0.171	(1.83)	–	–	−0.123	(−2.81)	−0.256	(−4.33)
*AGREE*Y58*	−0.024	(−0.27)	–	–	−0.098	(−2.78)	−0.095	(−2.17)
*AGREE*Y68*	−0.027	(−0.39)	–	–	−0.014	(−0.39)	−0.011	(−0.25)
*AGREE*Y73*	−0.005	(−0.04)	–	–	0.131	(2.63)	0.134	(2.20)
*COLL*Y54*	0.060	(2.16)	0.080	(3.20)	0.023	(1.72)	−0.005	(−0.29)
*COLL*Y58*	0.033	(1.30)	0.030	(1.42)	−0.001	(−0.03)	−0.004	(−0.32)
*COLL*Y68*	−0.007	(−0.29)	−0.010	(−0.46)	−0.005	(−0.50)	−0.009	(−0.74)
*COLL*Y73*	0.022	(0.74)	0.021	(0.78)	0.001	(0.03)	−0.032	(−1.83)
R^2	0.91		0.91		0.96		0.91	
Hausman statistic	105.9		55.7		26.3		58.1	
Prob-value	≈0		≈0		0.12		≈0	
No. of industries	93		93		93		93	
No. of observations	460		460		460		460	

Note: t-statistics based on robust standard errors in parentheses.

Table 4. Regression results for ln*GPPER*, ln*WOP* and ln*WOTH*: two-stage estimation

	Dep. variable: ln*GPPER*				Dep. variable: ln*WOP*		Dep. variable: ln*WOTH*	
ln*DS*	0.376	(12.52)	0.377	(12.69)	0.054	(4.90)	0.054	(4.13)
ln*K/L*	0.235	(5.02)	0.234	(5.08)	0.100	(6.43)	0.017	(1.12)
ln*PLANTSIZE*	−0.219	(−4.65)	−0.217	(−4.61)	0.083	(3.80)	0.038	(1.65)
Y54	−0.019	(−0.24)	−0.158	(−4.77)	−0.080	(−1.91)	0.012	(0.19)
Y58	−0.012	(−0.17)	−0.058	(−2.19)	−0.004	(−0.11)	−0.028	(−0.62)
Y68	0.153	(2.17)	0.086	(3.20)	0.098	(2.85)	0.097	(1.87)
Y73	0.285	(3.05)	0.155	(3.95)	0.163	(3.38)	0.093	(1.59)
*RD*Y54*	−0.001	(−0.02)	−0.032	(−0.55)	0.023	(0.82)	−0.039	(−1.06)
*RD*Y58*	−0.054	(−1.00)	−0.065	(−1.31)	−0.013	(−0.59)	−0.011	(−0.41)
*RD*Y68*	0.051	(0.95)	0.036	(0.71)	−0.009	(−0.47)	0.031	(1.22)
*RD*Y73*	0.158	(2.10)	0.128	(1.91)	−0.007	(−0.26)	0.012	(0.46)
*AGREE*Y54*	−0.250	(−1.87)	–	–	−0.090	(−1.13)	−0.140	(−1.24)
*AGREE*Y58*	−0.083	(−0.65)	–	–	−0.106	(−1.62)	−0.056	(−0.67)
*AGREE*Y68*	−0.121	(−0.99)	–	–	0.011	(0.18)	−0.025	(−0.27)
*AGREE*Y73*	−0.236	(−1.46)	–	–	0.092	(1.02)	0.148	(1.41)
*COLL*Y54*	0.292	(3.52)	0.186	(3.33)	0.021	(0.40)	−0.042	(−0.65)
*COLL*Y58*	0.044	(0.58)	0.009	(0.20)	0.004	(0.13)	−0.026	(−0.53)
*COLL*Y68*	0.061	(0.85)	0.010	(0.24)	−0.009	(−0.25)	0.024	(0.47)
*COLL*Y73*	0.152	(1.58)	0.050	(1.18)	0.036	(0.67)	−0.004	(−0.06)
R^2	0.90		0.90		0.96		0.90	
Hausman statistic	348.4		32.5		56.5		47.4	
Prob-value	≈0		0.005		≈0		0.0003	
No. of industries	132		132		132		132	
No. of observations	650		650		650		650	

Note: t-statistics based on bootstrap standard errors in parentheses.

Table 5. Regression results for the determinants of *COLL*.

	ln*DS*	ln*K/L*	ln*PLANTSIZE*	*AGREE*	*RD*	*ADV*	*FOREIGN*	constant
Coeff.	0.04	0.17	0.43	2.19	−1.36	−1.32	0.07	−1.57
	(0.24)	(0.84)	(1.43)	(1.52)	(−2.33)	(−2.39)	(0.24)	(−0.71)

$$1 - \ln L / \ln L_0 = 0.21$$

No. of observations: 98

Note: t-statistics based on robust standard errors in parentheses.

APPENDIX 1: The classification of industries according to their competitive status

As pointed out in the text, 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. Small variations in the cut-off points do not significantly affect the results. I illustrate here the classification of industries in cases where some but not all products within an industry were affected by cartels by way of a few examples. A detailed survey of restrictive agreements across all British manufacturing industries from the early 1950s to the mid-1970s is given in Symeonidis (2002).

Bread and Flour Confectionery (MLH 212)

The two main products of this industry are bread (about 60% of total industry sales) and flour confectionery. The price of bread was under government control until the mid-1950s. It then became subject to various agreements by national, regional and local associations. The most important agreements were a pricing agreement by the Federation of Wholesale and Multiple Bakers, whose members were 'plant bakers' in England and Wales, and price agreements by the Wholesale and Retail Bakers of Scotland Association, the Scottish Association of Master Bakers, and a Joint Costing Committee of the two associations, respectively. All these agreements were abandoned in 1959-1960. A range of regional agreements in the industry were also terminated at that time, as were agreements by various associations of 'master bakers' and numerous local collusive arrangements. Competition emerged gradually in the bread industry, and there was parallel pricing and exchange of information about discounts, but the industry was competitive in the 1970s. On the other hand, flour

confectionery was not subject to any national agreements. Some of the regional or local pricing arrangements among bakers covered flour confectionery as well, but most did not. In sum, about 70-80% of MLH 212 was subject to collusion in the 1950s, hence MLH 212 was classified as a collusive industry in the 1950s.

Carpets (MLH 419)

There are three subdivisions in this industry: woven carpets, carpeting and carpet floor rugs; tufted carpets, carpeting and carpet floor rugs; and other carpets, carpeting and carpet floor rugs and pile fabric rugs. A series of agreements by the Federation of British Carpet Manufacturers provided for minimum prices for certain types of woven carpets, notification of price changes, trade and quantity discounts, conditions of sale and resale price maintenance. According to Swann et al. (1973), competition intensified after these agreements were abandoned in 1960 despite parallel pricing and the exchange of price information. On the other hand, there is no evidence of any restrictive agreements in tufted carpets, carpeting and carpet floor rugs. Finally, some products that fall into the category "other carpets, carpeting and carpet floor rugs and pile fabric rugs" were covered by the agreements of the Federation of British Carpet Manufacturers described above. Moreover, a series of registered agreements by the Association of Manufacturers of Mohair and Pile Floor Rugs and Mats provided, until 1961, for specified trade discounts, the maintenance of manufacturers individual prices, agreed prices for comparable qualities, and no changes in individual prices without prior discussion with other members. The products that were affected by collusive agreements in the carpet industry accounted for more than 50% of total industry sales, hence MLH 419 was classified as a collusive industry in the 1950s.

Scientific and industrial instruments and systems (MLH 354)

This industry includes a variety of product categories: optical instruments and appliances, industrial control systems and equipment, instruments for measuring and testing non-electrical magnitudes, instruments for measuring and testing electric magnitudes, instruments for physical and chemical analysis, recorders and controllers, and instruments not elsewhere specified (including gas and other domestic meters). Most of these product categories were entirely free of collusive agreements. However, the Meter Manufacturers Association set minimum prices for electricity meters. This agreement was formally abandoned in 1960 and was replaced by an information agreement, which was apparently successful in reducing competition in the industry until about 1970. The Summation Meter Manufacturers Association set minimum prices for summation meters until 1965. The prices of various other types of instruments for measuring electrical magnitudes were regulated by the 'subsidiary power instrument agreement' and the 'patterns group agreement' until 1958-1960. In addition, the Commercial Instruments Conference fixed the prices of certain types of ammeters and voltmeters until 1961. Finally, the prices of gas meters were set, until 1957, by the Gas Meter Makers Conference. The share of all the products affected by these agreements in total sales of MLH 354 was less than 10%. Hence MLH 354 was classified as a competitive industry.

Footwear (MLH 450)

This industry includes leather footwear, rubber footwear, and parts of shoes. Leather footwear accounts for more than 90% of total industry sales, and there is no evidence of collusion in this part of the industry. On the other hand, an agreement of the Rubber Footwear Manufacturers Association related to prices charged for rubber boots and certain other standardised products, trade discounts, the maintenance of

manufacturers' resale prices, and collective exclusive dealing. This agreement was presumably abandoned in 1956, since it was not registered. Also, the Shoe Tip Association fixed the prices of heel tips and toe plates until 1961 and a group of manufacturers of cut leather soles agreed, until 1959, specified trade discounts and aggregated rebates. There is also some evidence of collective exclusive dealing arrangements in boot and shoe repairing. But since the primary product of MLH 450, covering more than 90% of industry sales, was not subject to collusion, MLH 450 was classified as a competitive industry.

APPENDIX 2: Data sources and construction of variables

The industry definitions used in this paper are typically at the three-digit level of aggregation, i.e. they are the 'minimum list heading' (MLH) industries of the UK Census of Production. In a few cases, data are available at a more disaggregated level and have been used.

Information on competition was taken from the agreements registered under the 1956 Act, the various 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 contained in Burn (1958) and 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 background material for this survey). Symeonidis (2002) describes the competition data in considerable detail.

Data on gross output and sales revenue at current net producer prices, wages and salaries, and employment were obtained from the industry reports of the Census of Production (various years). The figures are for all firms employing at least 25 persons. No distinction is made between full-time and part-time employees or

between men and women. Small corrections were made to some of these figures to ensure comparability over time. In addition, some figures were adjusted to correct for subsidies and customs and excise duties: subsidies were added to, and customs and excise duties subtracted from Census of Production sales and gross output figures. A series of retail price indexes was obtained from the *Annual Abstract of Statistics*. Industry-specific producer price indexes were obtained from various sources, including the *Annual Abstract of Statistics*, various issues of the *Board of Trade Journal* and *Trade and Industry*, the *Annual Bulletin of Construction Statistics* and Business Monitors. Sometimes they were constructed on the basis of data on volume of sales often reported, along with data on sales revenue, in the individual industry reports of the 1958, 1963 and 1968 Censuses of Production and in Business Monitors.

Estimates of capital stock, defined as plant and machinery, are available from O'Mahony and Oulton (1990) at the three-digit level of aggregation, i.e. for Census MLH industries. These are net stock estimates constructed on the assumption of fixed and 'short' asset lives and exponential depreciation rates. For those few cases where gross output and employment data were available at a more disaggregated level, I adjusted the O'Mahony and Oulton capital stock estimates on the basis of Census of Production data on the fraction of investment on plant and machinery accounted for by each 'principal product' within any given three-digit MLH industry. A very simple adjustment was applied: the three-digit industry capital stock was in each case multiplied by the ratio of principal product investment to MLH industry investment, averaged over two years. Capital stock was divided by total employment (including smaller firms) to construct capital-labour ratios.

The data for the percentage of employees covered by collective agreements in 1973 come from the *New Earnings Survey* of that year. Finally, the procedure for constructing *RD* and *ADV* was essentially the same as in Symeonidis (2000a, 2000b, 2002, 2003); see these references for details and a list of the sources used.

Finally, US data for Table 2 were taken from three sources. For the years 1958 to 1973, the data were obtained from the NBER manufacturing productivity database, which was constructed by Eric J. Bartelsman and Wayne Gray and is available online on the NBER website. For 1954, data on sales, employment and wages were taken from the 1954 and 1958 Censuses of Manufactures, while commodity price indexes were obtained from the website of the Bureau of Labour Statistics, as was a series of the consumer price indexes. Aggregating US 4-digit industries to match the UK 3-digit industries was relatively straightforward in most cases. In several cases the match is not perfect, in the sense that the US aggregate corresponds to a substantial part but not the whole of the UK industry. However, there was almost always sufficient comparability for my purposes; in a few instances where this was not the case, the industries in question were excluded from Table 2.

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