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Abstract: Using a comprehensive dataset on the incidence of price-fixing across British manufacturing industries in the 1950s, I compare collusive and competitive industries and find evidence of a negative relationship between collusion and the labour productivity of larger firms relative to smaller firms. In particular, collusion is associated with a reduction or even a reversal of the productivity gap between larger and smaller firms. This result is robust to controlling for the potential endogeneity of collusion.

Keywords: Competition, collusion, productivity.

JEL classification: L1, L6, J2, O4

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1. Introduction.

Empirical studies of the links between competition and productivity face two difficult problems: how to measure the intensity of competition and how to unravel the complex links between competition and productivity, two variables which may simultaneously affect one another. A common approach, which involves using measures of market structure or profitability as proxies for the intensity of competition, is often problematic, since these variables are endogenous with respect to competition. More recently, some authors have tried to by-pass these problems by focusing on exogenous institutional determinants of competition, such as regulatory reform or trade liberalisation policies (Bottasso and Sembenelli 2001, Nicoletti and Scarpetta 2003, Schmitz 2005). However, almost none of these studies have examined the links between competition and productivity for different groups of firms across industries. An exception is Fernandes (2007) who has shown that the positive impact of trade liberalisation on productivity in Colombia was stronger for larger plants than for smaller ones. On the other hand, Dutz (1996) has found that smaller firms were more likely than larger ones to exit their industries as a result of liberalisation in Morocco. Thus the relation between competition and the productivity differential between larger and smaller firms is very much an open question.

In a recent study of the effects of competition on productivity I examined evidence from a unique natural experiment of policy reform. Using data on the British price-fixing cartels of the 1950s both before and after their abolition, as well as data on a control group of competitive industries over the same time period, I showed that the cartels significantly slowed down industry productivity growth in the 1950s (Symeonidis

2008).[1] Since the evidence also suggests that the cartels induced excessive entry of firms into the collusive industries but did not raise firms' profits (Symeonidis 2000a, 2000b, 2002), it appears that the survival of inefficient firms may have been the most important source of welfare loss from collusion in the UK. Other studies have found a significant positive effect of competitive pressure caused by deregulation or foreign competition on productivity (Bottasso and Sembenelli 2001, Nicoletti and Scarpetta 2003, Schmitz 2005). If, as seems to be the case, the dynamic inefficiency caused by the lack of competition is a significant source of welfare loss, it is important to understand the economic mechanisms and firm characteristics that generate this inefficiency.

In this paper I examine one aspect of the productivity performance of the British cartels in more detail. In particular, I focus on productivity differences between larger and smaller firms and I attempt to address the following question: which firms were mainly responsible for the slowdown in productivity growth in collusive relative to competitive industries?[2] This question cannot be answered by examining only collusive industries, since productivity differences across firms in any industry are driven by a variety of factors. In what follows, I compare collusive to competitive industries and I find evidence of a negative relationship between collusion and the labour productivity of larger firms relative to smaller ones in 1950s Britain. In particular, the results suggest that collusion is associated with a reduction or even a reversal of the productivity gap between larger and

[1] An earlier study of the links between collusion and productivity in the UK, using cross-section data before the abolition of the cartels, is Broadberry and Crafts (1996).

[2] The answer to this question is unlikely to be "the very small and inefficient firms that survived under the cartel umbrella" since these firms did not affect the aggregate industry productivity very much.

smaller firms. These results are robust to controlling for the potential endogeneity of collusion.

I should point out that it is not possible to examine the link between collusion and relative productivity over time because panel data on relative productivity are not available. The present analysis, which is based on cross-section data for the early 1950s, may still provide significant insight into the factors that explain the low productivity performance of British collusive industries relative to competitive industries during the 1950s. In particular, the negative relationship between collusion and the labour productivity of larger firms relative to smaller ones suggests that the low productivity of larger firms was probably one of the factors behind the slow productivity growth of collusive industries relative to competitive industries.

To the best of my knowledge, this is the first statistical study of the relationship between collusion and the relative productivity of firms,[3] and one of very few studies that examine, more generally, the links between competition and the relative performance of firms. In fact, there is very little empirical evidence on industry-level determinants of the relative productivity of large and small firms. Caves and Barton (1990) have focused on the technical efficiency of firms, and much of their discussion does not apply directly to labour productivity. Most studies of the relative productivity of firms of different size either focus on simply identifying a relationship between size and productivity or examine how this relationship depends on (largely endogenous) factors that differ across firm size (see, for instance, Brush and Karnani 1996, Idson and Oi 1999, Dhawan 2001).

[3] On the other hand, there is a large empirical literature on the effects of cartels on prices (see Levenstein and Suslow 2006, Connor and Bolotova 2006 for recent surveys).

In contrast, my focus is on exogenous industry characteristics, including the intensity of competition.

2. The data.

Explicit price-fixing agreements between firms were operating in about half of British manufacturing industries in the 1950s. Some dated from the 1880s and 1890s, many others had been stimulated by government policies for the control of industry during the two world wars, and still others were the result of the depression of the inter-war years (Swann et al. 1974). Concern over the extent of cartelisation in the 1940s led to the introduction of the Monopolies and Restrictive Practices Act in 1948, giving powers to the newly created Monopolies and Restrictive Practices Commission to investigate cartels on a case by case basis. Although the Act provided for remedies if it was thought necessary, the procedure was slow, the recommendations of the Commission mixed and government action ambiguous. As a result, the 1948 Act did not have a significant impact on competition in British industry (Swann et al. 1974).

The agreements were not enforceable at law, but they were not illegal. Case-study evidence suggests that most of them were effective, the parties accounted for a large fraction of the relevant market, and there were a number of factors that limited outside competition – although entry to the industry was usually not restricted (Swann et al. 1973, 1974; Symeonidis 2002, 2003).[4]

[4] The effectiveness of outside competition was limited in many industries because the cartels tended to contain most or all of the largest and best-known domestic firms; because practices intended to limit outside competition, such as aggregated rebates and collective exclusive dealing, were common; and because competition from imports was

In this paper I use data from the early 1950s, a period when the cartels were still in place. The most comprehensive source of data on collusion in UK manufacturing in the 1950s is the Register of Restrictive Trading Agreements created under the 1956 Restrictive Trade Practices Act. 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.

While my main source of data on competition are the agreements registered under the 1956 Act, the large majority, if not all, of these had been in force for a long time, and were certainly effective in the early 1950s.[5] Furthermore, I also use other sources to identify unregistered agreements or agreements modified before registration, including 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

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often limited as a result of tariffs and quantitative controls, differing technical standards, transport costs or international restrictive agreements.

[5] Since comprehensive collusion data for the 1940s are not available, one cannot rule out the possibility that a few of the cartels became effective after 1951. This would, if anything, tend to bias toward zero the estimated coefficient of collusion on relative productivity in the OLS regressions of section 3.

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). The use of a diverse range of sources is one reason why any potential measurement error caused by ineffective agreements or unknown cases of collusion in the dataset is likely to be very small. A detailed discussion of this issue is provided in the Appendix.

Although the "degree of collusion" must have varied across cartelised industries – depending on the type of restrictions, the extent of outside competition, the balance of interests within the cartel, and so on – it is possible to split the UK manufacturing sector in the 1950s into a group of collusive industries and a group of industries without restrictive agreements. Going beyond this – for instance, by classifying industries with respect to the "degree of collusion" – is very difficult, given the information available. Still, the binary classification adopted here is sufficient for analysing the links between collusion and the relative productivity of large and small firms in 1950s Britain. Note that, in any case, the two-stage econometric models that will be used in this paper will address concerns of measurement error caused by ineffective agreements, unknown cases of collusion or differences in the degree of collusion across collusive industries.

Manufacturing industries were classified according to their state of competition 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 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 used. Since the data on productivity

and other variables are available at the four-digit industry level, several industries consist of subdivisions with different competitive regimes. An industry was classified as collusive if the products subject to *significant* restrictions accounted for more than 80% of total industry sales.[6] 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. 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. I have also used the 80% cut-off point because in some cases most core industry products were subject to price-fixing, although some or some secondary products were not. Small variations in the cut-off points (in particular using 10% instead of 20%, or using 50% instead of 80%) do not significantly affect the results, as will be shown below. Industries with ambiguous state of competition were then excluded from the sample and the dummy variable *COLL* was defined: this takes the value 1 for industries that were collusive in the 1950s and 0 for industries which were competitive.

Note that the use of a continuous competition measure, such as the fraction of sales revenue covered by products subject to agreements, instead of cut-off points has proved impractical. First, the link between the fraction of sales revenue covered by products with agreements and the state of competition is blurred by a variety of factors,

[6] The proportion of an industry's total sales subject to significant restrictions is for 1951, the same year as the productivity data. This proportion may change over time but rarely is this change so large during the 1950s as to cause an industry to move above or below the relevant cut-off point (and even then, it won't move much above or below). Furthermore, the results are robust to using different cut-off points, as will be shown below.

including the types of restrictions, the extent of outside competition, and so on. Second, it is often the case that some products within an industry were subject to significant restrictions, while other products were subject to uncertain restrictions. It is not clear how to deal with such cases if one wants to construct a continuous measure of competition. Third, some industries consist of subdivisions with very different market structures, and, although some of these subdivisions were collusive, most were not. The use of cut-off points has the advantage of treating such industries for what they really are, namely ambiguous, rather than trying to fit them into a continuum of states of competition.

The dataset is a cross-section of 187 four-digit manufacturing industries, 87 of which were collusive.[7] Information is available on a number of industry variables, the most important of which for my present purposes are the aggregate gross output, net output and employment of the three largest firms (in terms of the number of employees) in each industry. This information is only available for 1951 and reported, along with three-firm concentration ratios, in Evely and Little (1960). Thus my dependent variable in the next section is *RELPROD*, defined as the aggregate labour productivity of the three largest firms divided by the aggregate labour productivity of all the other firms in each industry (excluding, due to lack of data, very small plants employing less than 11 persons – these typically account for a very small fraction of industry output and employment).

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

[7] The sample excludes industries with significant government participation and includes two non-manufacturing industries – these do not significantly affect the results.

though capital intensity is included as a regressor in some of the models estimated below, 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. Second, constructing estimates of total factor productivity always involves making rather restrictive assumptions about the production function, and these assumptions are not innocuous.

The results of the present paper are based on a single measure of the relative productivity of larger to smaller firms. Note that the definition of "larger" and "smaller" is relative in the sense that it is based on a comparison of firms within each industry rather than a given level of firm size. I believe this is an advantage since it facilitates a focus on competitive interactions between firms as opposed to technological determinants of productivity. Using an absolute level of firm size would be impractical anyway because of the large variation in average firm size across industries. The split between the three largest firms and the rest is the only one for which there are available data. However, the split is not a very asymmetric one. The average value of the 1951 three-firm concentration ratio is 0.41, and in exactly one third of all industries in the sample the three largest firms account for more than half of industry output. Thus, the performance of the three largest firms is usually an important determinant of the aggregate performance of large firms under any definition of the word "large". Furthermore, the average number of firms in an industry in the sample is 125, and hence smaller firms have, on average, much lower market shares than the three larger firms: most of them are indeed "small". On the other hand, since the large firms in the dataset have an average

market share of 14% in their industry, they are not unusually large and their performance does not reflect any characteristics specific to such firms.

3. Econometric model and results.

To study the links between collusion and relative productivity, I estimate regressions for *RELPROD,* the aggregate labour productivity of the three largest firms divided by the aggregate labour productivity of smaller firms in each industry. In principle, an alternative approach would be to run separate regressions for the labour productivity of larger and smaller firms. The problem with that approach is that a whole range of industry characteristics which have no effect on the *relative* productivity of firms within an industry do have an effect on productivity *levels* of large and small firms. Not only would this make omitted variable bias more likely, but it would also make it much more difficult to find valid and strong instruments for *COLL* in two-stage least-squares regressions.

Labour productivity is defined in two different ways: as gross yearly output divided by the average number of employees during the year (this definition is used to construct *RELPROD1*) and as net output divided by the average number of employees (this definition is used to construct *RELPROD2*).[8] The mean value of *RELPROD1* (*RELPROD2*) for all the competitive industries is 1.12 (1.09), while its mean value for the collusive industries is 1.07 (1.07). Thus, while larger firms were on average more productive than smaller ones in both groups of industries, the productivity gap was

[8] Gross output is defined as the total value of sales and work done during the year adjusted for changes in the value of stocks. Net output is gross output minus: the cost of materials and fuel, payments for work given out and transport payments.

slightly narrower in the presence of collusion. However, this direct comparison may be misleading to the extent that the incidence of collusion is correlated with other variables that affect the productivity gap between large and small firms. Table 1 provides descriptive statistics for all the variables separately for the two groups of industries. Collusive industries were, on average, more concentrated, more capital-intensive, less advertising-intensive and R&D-intensive, more likely to sell producer goods and better protected from foreign competition than competitive industries.

Economic theory provides little guidance regarding the industry characteristics that determine *RELPROD* other than the prediction that it will be higher in the presence of economies of scale or scope. The idea is that larger firms will be able to exploit such economies much more than smaller firms. So the more significant the scale and scope economies to be exploited in an industry, the wider the productivity gap between large and small firms. Moreover, as pointed out in the Introduction, there is very little empirical work on industry-level determinants of the relative productivity of large and small firms. I will therefore begin with a parsimonious specification:

$$RELPROD_i = \alpha_i + \beta_1 COLL_i + \beta_2 CONC_i + Sector\ \ dummies + u_i, \qquad (1)$$

where *COLL* was defined in the previous section and *CONC* is the 1951 three-firm concentration ratio. *CONC* is expected to have a positive effect on *RELPROD* to the extent that a concentrated market structure reflects the presence of significant economies of scale and scope.[9] A potential complication is that the concentration ratio could perhaps

[9] The correlation coefficient between *CONC* and *COLL* is a modest 0.17. I also experimented with other available measures of market structure, such as the average size of the largest firms divided by the average size of the smaller firms or the total number of

also be regarded as a measure of market power. I will return to this issue when discussing the results below. The inclusion of sector dummies among the regressors serves as a control for the presence of industry effects.[10]

An objection to the above specification is the potential endogeneity of both *CONC* and *COLL*. With respect to *CONC*, it can be argued that market structure is itself a function of the competitive regime. Also, *CONC* may be influenced by the relative productivity of firms if more efficient firms have larger market shares. With respect to *COLL*, a possible objection is that any difference in *RELPROD* between collusive and competitive industries may be to some extent due to unobserved characteristics that differ between the two groups rather than the state of competition. Moreover, the direction of causality could be the opposite of what is assumed here. To address these concerns I will also estimate (1) using a two-stage least-squares model where both *CONC* and *COLL* are treated as endogenous. An additional important advantage of the two-stage estimates is that they are not affected by any measurement error in *COLL* due to unidentified or ineffective collusive agreements or cases of tacit collusion.

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firms, but these were not statistically significant. It is not surprising that *CONC* performs better: unlike the other two variables, it is not affected by the number or the size of very small firms in an industry. A measure of minimum efficient scale based on the median plant size would be a better measure of scale economies but could not be constructed because of data limitations.

[10] Ten sectors are distinguished: food and drink; coal products and chemicals; basic metals; mechanical engineering and vehicles; instruments and electrical engineering; metal products; textiles, leather and clothing; building materials, pottery, glass and wood products; paper products; and other manufacturing (the benchmark in equation (1)).

For the two-stage regressions the instruments include ln*GROUT*, ln(*K/L*), *ADV*, *RD*, *PRODCON*, *UNION* and *FOREIGN*.[11] All of these variables potentially affect *CONC*, *COLL* or both. In particular, *GROUT* is industry gross product, as reported in the 1951 Census of Production, and serves as a measure of market size, an important determinant of concentration. *K/L* is the capital-labour ratio, another important determinant of concentration as well as collusion in 1950s Britain (Symeonidis 2002, 2003).[12] *ADV* and *RD* are dummy variables which are equal to 0 for industries with advertising-sales ratio and R&D-sales ratio, respectively, lower than 1% and equal to 1 otherwise. These variables are intended to capture the effect of advertising effectiveness and technological opportunity on the likelihood of collusion: both had a negative effect on the incidence of collusion among British firms in the 1950s.[13] *PRODCON* takes the

[11] Note that although several of these variables may directly affect labour productivity, there is no reason to expect that they might affect relative labour productivity. I check this below both by reporting the results of overidentification tests for the two-stage least-squares regressions and by running OLS regressions of *RELPROD* with the entire set of instruments used as regressors. Either way, there is no evidence of any direct effect of the instruments on *RELPROD*.

[12] The data on capital stock are estimates at the three-digit level of aggregation rather than primary data and were taken from O'Mahoney and Oulton (1990). They were not available for 1951, so 1954 estimates were used instead and combined with employment data from the 1954 Census of Production.

[13] 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 1% (or 2%). Thus, in an industry below the 1% cut-off point, 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 1% cut-off point, advertising/R&D "works". Of course, whether such an

value 0 for producer goods industries and 1 for consumer goods industries; the former were more likely to be cartelized than the latter in 1950s Britain. *UNION* is union density, measured at a level of aggregation between the two-digit and the three-digit industry level and obtained from Bain and Price (1980). Finally, *FOREIGN* is a dummy variable which takes the value 0 for industries with relatively high protection in the mid-1950s and the value 1 for industries with relatively low protection and is negatively correlated with *COLL* (Symeonidis 2003).[14]

To check the robustness and validity of the results I will also estimate with OLS a model that includes the above variables as additional regressors:

$$RELPROD_i = \alpha_i + \beta_1 COLL_i + \beta_2 CONC_i + \beta_3 \ln GROUT_i + \beta_4 \ln(K/L)_i + \beta_5 ADV_i + \beta_6 RD_i + \beta_7 PRODCON_i + \beta_8 UNION_i + \beta_9 FOREIGN_i + Sector\ \ dummies + u_i . \quad (2)$$

Table 2 contains the main results.[15] The gap in labour productivity between the three largest and all the other firms in each industry is considerably narrower in the

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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 procedure for constructing *RD* and *ADV* involved combining information from various official and market research sources; see Symeonidis (2003) for details and a list of the sources used.

[14] The group of industries with high protection contains the engineering industries, instruments, vehicles, finished metal goods, some chemicals, paper and paper products, furniture, pottery and glass, most finished textile goods, rubber products, and various other finished manufactures. The low-protection group contains most food and drink industries, some chemicals, basic metals, clothing and footwear, wood products, publishing, leather and most textile semi-manufactures, and building materials.

[15] The difference in n between the various columns of Table 2 (or Table 4) is due to missing data for some of the additional variables used.

presence of collusion and may also be reversed in certain sectors: *RELPROD* is 15% lower in the presence of collusion in the OLS regressions and 30% lower in the 2SLS regressions and this effect is statistically significant in all models except the OLS regressions with *RELPROD2*. Note that several tests suggest that the instruments are valid and strong. Thus the coefficients on ln*GROUT* and ln*(K/L)* are statistically significant at the 1% level in the first-stage regression for *CONC*, those on ln*(K/L)* and *FOREIGN* are statistically significant at the 5% level in the first-stage regression for *COLL*. First-stage results are shown in Table 3. Moreover, Shea's partial R^2 in the first-stage regression is 0.21 for *CONC* and 0.16 for *COLL*. Finally, the Sargan test of overidentifying restrictions does not reject the null hypothesis of instrument validity in any of the regressions.

While the coefficients on *COLL* in the two-stage regressions are large and should perhaps be treated with some caution, they suggest that there is a strong negative and statistically significant effect of collusion on relative labour productivity. The coefficients on the sector dummies (not reported) are everywhere jointly significant at the 5% level. *CONC* has a positive effect on *RELPROD*, as expected for a measure of scale and scope economies.[16] On the other hand, if *CONC* were to be regarded as a measure of market power, its positive coefficient would be more difficult to interpret. In any case, the positive link between *CONC* and *RELPROD* does not contradict the negative association

[16] When either *CONC* or the sector dummies are omitted, the coefficient on *COLL* is still everywhere negative but smaller (and statistically significant at the 10% level at best). This is not surprising: both *CONC* and the sector dummies are correlated with *COLL* as well as having a direct effect on *RELPROD*, so their omission causes a bias in the estimated coefficient on *COLL*.

between *COLL* and *RELPROD*. Of the two variables, *COLL* is a clear measure of collusive conduct, while *CONC* is only indirectly and ambiguously associated with the intensity of competition.[17] None of the other variables has any effect on *RELPROD*.

I also performed a variety of further robustness tests. Some of these are presented in Table 4, where the dependent variable throughout is *RELPROD1*. First, I used somewhat modified criteria for classifying industries as collusive or competitive. For instance, the first three columns of Table 4 report results using *COLL2* as the collusion variable. To construct *COLL2*, an industry was classified as collusive if the products subject to significant restrictions accounted for more than 50% of total industry sales and it was classified as competitive if the products subject to significant or uncertain restrictions accounted for less than 10% of industry sales. This resulted in 98 industries being defined as collusive and 84 as competitive (with *COLL*, the numbers were 87 and 100, respectively). Second, I used the 2% instead of the 1% cut-off point for the advertising-sales ratio and the R&D-sales ratio to construct dummies for advertising effectiveness and technological opportunity (*ADV2* and *RD2*). Third, I replaced the three-digit capital-labour ratio with estimates of *K/L* at the four-digit industry level, derived by multiplying three-digit capital stock by the ratio of four-digit investment to three-digit investment and dividing this by four-digit employment. The fourth and fifth columns of Table 3 contain results when *ADV*, *RD* and the 3-digit *K/L* are replaced by *ADV2*, *RD2* and the 4-digit *K/L*, respectively. In all cases, the results were not much affected. I also

[17] The abolition of cartels and the resulting intensification of competition caused concentration to rise in previously collusive industries. On the other hand, there is no evidence of concentration facilitating collusion after controlling for capital intensity (Symeonidis 2002, 2003).

estimated two-stage models with only *CONC* or *COLL* treated as endogenous, and again the results were robust.

4. Concluding remarks.

While previous studies have found that the lack of competition, and collusion in particular, slow down productivity growth, little is known on the economic mechanisms and firm characteristics that drive this effect. This paper takes a step towards answering this question, by focusing on the links between collusion and the relative productivity of larger and smaller firms. Since I can only observe cross-section data in this paper, unravelling the links between competition and relative productivity relies on the use of instrumental variable analysis that takes into account the potential endogeneity of collusion.

The results from the analysis of a comprehensive dataset on the incidence of price-fixing across British manufacturing industries in the 1950s reveal a negative association between collusion and the labour productivity of the largest relative to the smaller firms, which persists when controlling for the potential endogeneity of collusion. This is consistent with the view that the productivity slowdown in collusive relative to competitive industries that was documented in Symeonidis (2008) was driven to some extent by the relatively poor performance of the largest firms in collusive industries. It is interesting that even though the cartels induced excessive entry of firms into the collusive industries, the productivity slowdown was probably not primarily caused by the entry of too many small inefficient firms.

The results of this paper are based on cross-section data and the relative productivity data are for 1951, while the productivity slowdown in collusive relative to competitive industries documented in Symeonidis (2008) refers to the period after 1951 (for reasons of data availability). However, it seems likely that the underperformance of the larger firms in collusive industries is related to the relatively slow productivity growth of collusive industries. While plausible, this is not the only interpretation of the evidence, and it is also possible that the negative effect of collusion on productivity growth after 1951 was an industry-wide effect which was independent of the underperformance of larger firms in collusive industries in the early 1950s.

One possible reason for the negative association between collusion and the labour productivity of the largest relative to the smaller firms is that larger firms may be more likely than smaller ones to see their costs rise when competition is not intense. For instance, it is well known that unionisation is more pronounced in larger firms than in smaller ones and that unions are more likely to engage in restrictive practices when competition is not strong. Also, larger firms may face more inelastic demand, so they may be more willing to pass on costs to prices and less concerned about rising costs.

Another reason may have to do with the internal organisation of many cartels, both in Britain and elsewhere, or, more generally, with the way prices are set by firms in collusive industries. If prices tend to be set by the leading firms or determined in negotiations between high-cost and low-cost producers, larger firms will usually be in a stronger position than smaller ones and may find it easier to pass on their costs to prices, so they will be less concerned about cost increases than smaller firms.

Table 1. Descriptive statistics.

	Collusive (n = 87)	Non-collusive (n = 100)
RELPROD1	1.07 (0.25)	1.12 (0.47)
RELPROD2	1.07 (0.22)	1.09 (0.38)
CONC	0.45 (0.22)	0.37 (0.26)
Ln(GROUT)	10.20 (1.12)	10.05 (1.13)
Ln(K/L)	1.03 (0.61)	0.54 (0.84)
ADV	0.08 (0.27)	0.26 (0.44)
RD	0.13 (0.33)	0.20 (0.40)
PRODCON	0.24 (0.39)	0.59 (0.47)
UNION	48.5 (12.3)	43.9 (12.5)
FOREIGN	0.31 (0.46)	0.52 (0.50)

Note: The figures are means of the variables for collusive and non-collusive industries, respectively (with standard deviations in parentheses). n indicates the number of industries.

Table 2. Regression results for the determinants of *RELPROD*.

	Dependent variable: *RELPROD1*			Dependent variable: *RELPROD2*		
	OLS	*OLS*	*2SLS*	*OLS*	*OLS*	*2SLS*
COLL	–0.16 (–2.61)	–0.14 (–2.04)	–0.27 (–2.16)	–0.11 (–1.72)	–0.08 (–1.37)	–0.30 (–2.10)
CONC	0.61 (4.76)	0.67 (4.11)	0.48 (1.45)	0.52 (4.50)	0.50 (4.32)	0.68 (2.35)
ln*GROUT*	-	0.02 (1.00)	-	-	0.02 (1.01)	-
ln*(K/L)*	-	–0.04 (–0.63)	-	-	0.03 (0.78)	-
ADV	-	0.03 (0.35)	-	-	0.04 (0.60)	-
RD	-	0.04 (0.42)	-	-	0.10 (0.80)	-
PRODCON	-	0.08 (0.91)	-	-	0.18 (2.41)	-
UNION	-	–0.01 (–0.60)	-	-	–0.01 (–0.91)	-
FOREIGN	-	–0.08 (–0.77)	-	-	–0.06 (–0.86)	-
constant	0.81 (8.83)	0.51 (1.66)	0.89 (5.66)	0.83 (12.56)	0.52 (2.54)	0.79 (6.73)
Sector dummies	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.16	0.18	0.14	0.17	0.24	0.11
Overidentification test	-	-	$\chi^2(5) = 1.99$ ($p = 0.85$)	-	-	$\chi^2(5) = 7.24$ ($p = 0.20$)
No. of observations	187	178	178	186	177	177

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

Table 3. First-stage regressions.

	Dependent variable: *COLL*	Dependent variable: *CONC*
ln*GROUT*	–0.004	–0.06
	(–0.12)	(–3.79)
ln*(K/L)*	0.20	0.13
	(3.27)	(4.47)
ADV	–0.19	0.08
	(–1.49)	(1.78)
RD	–0.02	0.10
	(–0.14)	(1.74)
PRODCON	–0.11	–0.04
	(–0.95)	(–0.89)
UNION	0.005	–0.002
	(1.76)	(–1.48)
FOREIGN	–0.18	0.27
	(–2.14)	(0.72)
constant	0.08	0.96
	(0.24)	(6.19)
Sector dummies	Yes	Yes
R^2	0.35	0.37
No. of observations	178	178

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

Table 4. Robustness checks.

	Dependent variable: *RELPROD1*				
	OLS	*OLS*	*2SLS*	*OLS*	*2SLS*
COLL	-	-	-	–0.16 (–2.23)	–0.26 (–2.01)
COLL2	–0.15 (–2.35)	–0.14 (–1.96)	–0.22 (–1.86)	-	-
CONC	0.54 (4.17)	0.57 (3.53)	0.46 (1.52)	0.60 (4.08)	0.70 (2.84)
ln*GROUT*	-	0.03 (1.25)	-	0.02 (0.71)	-
ln*(K/L)*	-	–0.01 (–0.16)	-	0.03 (0.60)	-
ADV	-	0.02 (0.20)	-	-	-
ADV2	-	-	-	0.07 (0.56)	-
RD	-	0.05 (0.53)	-	-	-
RD2	-	-	-	–0.16 (–1.76)	-
PRODCON	-	0.08 (0.92)	-	0.12 (1.25)	-
UNION	-	–0.01 (–1.08)	-	–0.01 (–0.46)	-
FOREIGN	-	–0.05 (–0.46)	-	–0.10 (–0.94)	-
constant	0.85 (8.62)	0.54 (1.73)	0.90 (6.08)	0.60 (1.93)	0.79 (6.52)
sector dummies	Yes	Yes	Yes	Yes	Yes
R^2	0.14	0.15	0.13	0.19	0.15
Overidentification test	-	-	$\chi^2(5) = 2.72$ ($p = 0.74$)	-	$\chi^2(5) = 5.55$ ($p = 0.35$)
No. of observations	192	183	183	171	171

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

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APPENDIX

The most comprehensive source of data on collusion in UK manufacturing in the mid- and late 1950s is the Register of Restrictive Trading Agreements created under the 1956 Restrictive Trade Practices Act. The reports of the Monopolies and Restrictive Practices Commission (MRPC) before 1956 and those of the Monopolies Commission (MC) after that date also contain detailed information on the operation of cartels in particular industries. It is clear from these reports that the industries investigated (some of which did not register any agreements under the 1956 Act) had been practicing collusion effectively during the 1950s. Other fully reliable sources on the state of competition in the 1950s include the MRPC report on collective discrimination (MRPC 1955), and several industry studies contained in Burn (1958). Finally, two very important but not fully reliable sources are the Board of Trade annual reports from 1949 to 1956 (Board of Trade 1949-1952, 1953-1956) and the Political and Economic Planning (1957) survey of industrial trade associations, as well as unpublished background material for this survey. These two sources provide information on industries alleged to be collusive; most of these registered agreements, although some did not. This information must be treated with caution because it is mainly based on complaints or reports given by buyers, and buyers may wrongly deduce the existence of a price-fixing agreement from price uniformity or parallel pricing. On the other hand, it may well be the case that some at least of the industries that were alleged to be collusive and did not register any agreements were in fact collusive. Thus, there is usually some uncertainty regarding the state of competition for products contained in the Board of Trade annual reports or the P.E.P. survey but not mentioned as being the subject of restrictive agreements in any of the more reliable sources.

As pointed out in the text, these data sources allow us to distinguish three groups of products: a group with explicit restrictive agreements, a group without explicit restrictive agreements, and a group with uncertain state of competition in the 1950s. In this Appendix I argue that any potential measurement error caused by ineffective agreements or unknown cases of collusion in the dataset is likely to be very small.

One possible objection is that the data on British cartels relate to explicit collusion, but not to tacit collusion. Could it be the case that some of the industries classified as non-collusive actually practiced tacit collusion? Although one cannot rule out the possibility that firms in an industry colluded tacitly in the absence of any explicit arrangement, it is difficult to understand why this would have occurred given that explicit collusion was legal and widespread in the 1950s. Furthermore, the 1956 Act required the registration of informal and even "implied" understandings as well as formal agreements, and this seems to cover cases of tacit collusion. For all the above reasons and given that collusive arrangements of all kinds were not enforceable in the courts, the distinction between tacit and explicit collusion is not very important in the present context.

Another possible objection is that some of the agreements may have not been effective at the time they were registered, so classifying these industries as collusive introduces measurement error. Is this argument valid? I think that it is not, and I will offer several different arguments to support this claim.

First, the case-study evidence discussed in the previous section strongly supports the view that the large majority of the agreements had been effective. Second, the Register is not the only source of information on collusion in British industry. Several industries were investigated by the MRPC during the 1950s, and several more defended their agreements before the Restrictive Practices Court. The available information leaves no doubt as to the effectiveness of these agreements, which are a significant part of the total number. Third, all the sources of information on the effects of the 1956 legislation emphasise that competition was slow to emerge in many industries, and that this was often due to the fact that information agreements replaced the former price-fixing arrangements in the short run. This is not consistent with the view that these arrangements were not effective before 1956. Fourth, a weak agreement could not expect to gain much from a favourable Court decision, because it would still not be enforceable at law. So it is not at all clear why industries with weak agreements would have a strong incentive to register. On the contrary, one might argue that it was industries with strong agreements that had the strongest incentive to register and try to maintain collusion, because of the potentially large cost of a cartel breakdown. Fifth, the evidence from Lydall (1958) and Board of Trade (1946) discussed below also suggests that there is no

serious measurement error or selection bias in the construction of the collusive group of industries in the present study.

The above discussion suggests that the assumption that the existence of an explicit price-fixing agreement is a good overall indicator of collusive conduct is not an unreasonable one in the present context. There is, however, one final difficulty, and this relates to the issue of non-registration. In particular, non-registration of agreements, if widespread, would lead to serious measurement error in the data.

More precisely, one can distinguish between two possible reasons for non-registration: firms may simply suspend an agreement or they may switch to secret or tacit collusion. Take, first, the former case. A reasonable conjecture in this case is that very weak agreements would be more likely to be dropped immediately than stronger ones. Even if that were true, we would not be losing much by failing to identify such cases, since we are interested in effective agreements, not ineffective ones. But it is not even clear why the decision to immediately cancel an agreement rather than register it should have occurred in certain types of industries more than in others: there is not much to be lost by registering an agreement, even a weak one, when the alternative is cancellation. In fact, many of the agreements that were not registered were those that had been condemned by the MRPC. Clearly, these were not weak agreements, but the parties must have thought that they had practically no chance of success in the Court and wished to avoid further adverse publicity. This leaves us with the second reason for non-registration mentioned above. One might argue, for instance, that industries where tacit or secret collusion would be easier to sustain or less easily detected after 1956 had less of an incentive to register.

An important thing to note with respect to the issue of non-registration is the historical context of the introduction of the 1956 Act and, in particular, the uncertainty about the way the legislation would be implemented. Because the attitude of the Court could not be known until the first cases had been heard, firms were prompted to register their restrictive agreements rather than drop or secretly continue them, although in some cases they redrafted their agreements or even removed some of the restrictions in an attempt to increase the likelihood of a favourable Court decision (see Swann et al. 1974, Hunter 1966). It seems therefore that firms genuinely thought that they had a good chance

of success in the Court, which also explains why several agreements were defended in the Court despite the first few unfavourable Court decisions. An additional factor in persuading most industries to register pricing agreements may have been the rather ambiguous attitude of the MRPC towards price-fixing (as opposed to other types of restrictions, such as collective exclusive dealing or market sharing). In particular, between 1948 and 1956 the MRPC investigated restrictive practices in several industries; however, only in some of these did it find price-fixing to be unambiguously against the public interest. To these arguments one could add that the 1956 Act gave the Registrar powers of investigation. Being an officer at the Board of Trade, the Registrar would certainly have access to all the complaints made throughout the 1950s by buyers claiming the existence of restrictive agreements in particular industries. Thus it would be difficult for many industries to collude secretly and go unnoticed for a long time. For all these reasons, it seems safe to conclude that non-registration was not a widespread phenomenon.

This conclusion is supported by a comparison of the Register of Restrictive Trading Agreements with a list of industries subject to restrictive practices published in the 1955 MRPC report on collective discrimination. Swann et al. (1974, pp. 153-154) mention that out of 60 industries with restrictive agreements listed in the report, 8 did not register their agreements. This is a non-negligible percentage, but it has to be borne in mind that in some of these industries the agreements had comprised only the collective enforcement of resale price maintenance or collective exclusive dealing, without any fixing of common prices or market shares. Many of these would normally not be registered, because the 1956 Act contained an outright prohibition of the collective enforcement of resale price maintenance. Moreover, collective exclusive dealing had been consistently condemned by the MRPC and was therefore dropped by several industries prior to registration of their agreements. Once this is taken into account, the incidence of non-registration of registrable agreements appears to have been rather low.

Moreover, and most important, in this paper I do not rely solely on information about registered agreements. As pointed out above, several of the sources examined allow us to identify industries that either were certainly collusive or were alleged to be collusive, and did not register any agreements. Although some of these sources are not

perfect, it would really be surprising if there were a significant number of cases that escaped all of them.

Finally, additional evidence suggesting that there should not be any significant sample selection bias in the data from ineffective or unregistered agreements comes from two other sources. The first is a questionnaire survey of competition in UK manufacturing in the 1950s (Lydall 1958). This study, which used a sample of 876 manufacturing firms from all sectors, did not specifically examine collusion; however, some of the information provided suggests that firms that perceived their condition as being characterised by "no strong competition" were primarily in industries which had a high incidence of explicit collusion, according to my classification, while firms that thought that they were facing "strong competition" were chiefly in industries without many agreements. The second source is a survey of UK cartels carried out in the mid-1940s by the Board of Trade (Board of Trade 1946). Although the survey was not fully comprehensive, the industries chosen spanned the whole spectrum of manufacturing industries, covering capital-good as well as consumer-good industries. Despite this large coverage, and the fact that most of the British cartels of the 1950s had already been active at the time of the Board of Trade survey (see Swann et al. 1974), there are virtually no industries reported as being subject to restrictive agreements in the early 1940s which are not mentioned as collusive in at least one of my data sources for collusion in the 1950s. Furthermore, nearly all of these industries registered agreements under the 1956 Act.

One can conclude, on the basis of the above discussion, that the issue of potential non-registration of agreements does not cause any significant measurement error in the present data.