Abstract: This paper examines the impact of cartel policy on wages and productivity using a panel data set of UK manufacturing industries over 1954-1973. The introduction of cartel laws in the UK in the late 1950s caused an intensification of price competition in previously cartelised manufacturing industries, but it did not affect those industries which were not cartelised. 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 of manual or non-manual workers. These results are robust to controlling for the potential endogeneity of collusion.

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1. Introduction.

It is often argued that the lack of competitive pressure may lead to inertia and managerial slack, and hence reduce productivity growth. In its simplest version, this view essentially implies that managers and workers of firms with market power can capture some of the monopoly rents in the form of slack or lack of effort. More recent versions of this idea have focused on the fact that competition may improve the ability of firms’ owners to provide appropriate incentive schemes to managers. Furthermore, it is often argued that the lack of competitive pressure may result in monopoly rents being shared by workers through higher wages. Finally, the lack of competition may also allow an excessive number of firms to survive in any given industry, including a number of relatively inefficient firms that would not survive under more competitive conditions, and hence result in lower 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 is not very strong (see Nickell 1999 for a recent survey).

This paper provides an econometric analysis of the impact of competition 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 therefore 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 competition, wages and productivity.

The introduction of cartel policy in the UK provides a very rare opportunity for a systematic and comprehensive empirical analysis of the effects of competition. The most important feature of this natural experiment is that it provides us with a way to by-pass two very difficult problems that have been endemic in empirical studies of the effects of competition. The first problem is how to measure the intensity of competition. The second problem is how to unravel the complex links between competition and other variables, 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 in this case induced by an exogenous and measurable institutional change. Thus there is no need to measure the intensity of competition directly. All that is required is a clear distinction between industries that were collusive and were therefore affected by the shift in cartel policy and industries that were competitive and were therefore not affected. Moreover, the intertemporal structure of the data set 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. Thus, the identification of one-way causal effects is a feasible task in the present context.
There is a large empirical literature on the effect of competition on wages and productivity. Most of these studies have used measures of market structure or profitability as proxies for the intensity of competition (see, for instance, Nickell 1996; Nickell et al. 1994; Hay and Liu 1997). The difficulty with this approach is that the links between competition, market structure and profitability are complex and potentially ambiguous. For instance, while most people would agree that an exogenous increase in concentration (or a firm's market share) will relax competition, concentration (or market share) may in fact rise as a result of more intense competitive pressure (Symeonidis 2000a, 2002a). 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 2002a).

To avoid these complications, some studies have focused on the effects of deregulation of particular industries or of economic reform in ex-communist countries. Other studies have examined the effects of exogenous changes in the degree of economic integration, such as the implementation of trade liberalisation policies in developing countries or the EU single market program, on wages or productivity. Such work is especially 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 (see, for instance, Tybout and Westbrook 1995; Bottasso and Sembene 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 typically involve more than just an intensification of competitive conduct, it may not be easy to draw clear implications for the competition-productivity relationship from these results. Finally, competitive pressure may also intensify as a result of a significant decline in demand. Galdon-Sanchez and Schmitz
(2001) have found substantial productivity improvements among (surviving) iron-ore producers that were affected by the collapse of the steel market (the principal market for iron ore) in the 1980s, but little productivity improvements among a control group of firms that were not so affected.

The present paper by-passes many of the problems faced by previous studies by focusing on a natural experiment of competition policy reform. The direct effect of the introduction of restrictive practices legislation in the UK was to cause explicit cartels to break down and competition to intensify in previously collusive industries. Industries that were not collusive were not affected by the legislation. None of the previous studies of the effects of the introduction of cartel laws 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 legislation on competition. The case-study evidence they report, although consistent with the hypothesis of a positive effect of the breakdown of British cartels on the efficiency of firms, is rather 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, 2002a) 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.

The only previous statistical analysis of the links between collusion and productivity growth in British industry is the study by Broadberry and Crafts (1996). 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 Crafts and Broadberry 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 avoided 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 dataset 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.

The results provide strong evidence of a negative effect of collusion on labour productivity growth. In particular, labour productivity was growing 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 opportunity and union power 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. The negative effect of collusion on productivity growth can be contrasted with the absence of any link between collusion and the production of innovations in the present data (see Symeonidis 2002a). 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.

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, 2002a). 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 considered 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 (for example, Swann et al. 1973, 1974) suggests that in most industries the agreements had been operated honourably prior to cancellation, the parties typically accounted for a large fraction of the market, and there were a number of factors that limited outside competition in many industries. This evidence also indicates that 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 etc 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 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.

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

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

In particular, the various types of restrictions were classified as significant, non-significant or uncertain, according to their likely impact on competition. Next, an industry was classified as collusive in the 1950s if the products subject to significant restrictions accounted for more than 50% of total industry sales. It was classified as competitive if the products subject to significant or uncertain restrictions accounted
for less than 20% of industry sales. And it was classified as ambiguous in all remaining cases.\(^1\) 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 from the basic sample.\(^2\) 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 and therefore experienced no change in regime.

The panels used in this paper consist of some 130 three-digit industries and five years: 1954, 1958, 1963, 1968 and 1973.\(^3\) 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, competition did not break out immediately in several industries, so the impact of the Act was felt at least until the late 1960s in very many industries.

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\(^1\) In fact, most industries classified as competitive were free from any 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. Similarly, most industries classified as collusive had agreements covering 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. Small variations in the cut-off points (in particular using 10% instead of 20%, or using 40% or 70% instead of 50%) do not significantly affect the results reported in section 4. The use of a continuous competition measure instead of cut-off points has proved impractical for a variety of reasons (see Symeonidis 2002a for an extensive discussion).

\(^2\) 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.

\(^3\) 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.
3. Empirical models and results.

To study the effect of competition on wages and productivity, I estimate in this section 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 average employment during the year; \( WOP \) is 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 yearly earnings of non-manual workers deflated by the retail price index and divided by the average number of non-manual workers.

There are two main 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 main 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.

Note that \( GPPER, WOP \) and \( WOTH \) are all based on a measure of labour input that is not adjusted for hours worked. The main reason for this is once again data problems. Data on hours worked during a typical week in any given industry and year are available for manual workers only and are not always fully comparable with the
output data. In addition, data on weeks worked during any given year are not available for individual industries. Nevertheless, my empirical results are very unlikely to be affected by the lack of adjustment for hours worked. To verify this, I constructed a series of labour productivity and wage rate estimates on the basis of the available data on hours worked. 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.\footnote{4}

Some descriptive statistics on the evolution of labour productivity and wages over 1954-1973 are reported in Table 1. In particular, the table presents statistics on the average change in each of the three endogenous variables of interest over 1954-1958, 1958-1963, 1963-1968 and 1968-1973, separately for industries with $COLL = 0$ and industries with $COLL = 1$. There is some evidence of a negative effect of collusion on productivity: 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 $COLL = 1$ (9.8\% versus 4.9\%); on the other hand, between

\footnote{4 The data on hours worked per week used for these calculations were taken from the \textit{Ministry of Labour Gazette} (and its successors, the \textit{Employment and Productivity Gazette} and the \textit{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.}
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 (23.5% versus 20%). In absolute terms, however, the changes over time do not
appear very significant. The statistics for wages are even more ambiguous: between
1954 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 seem to suggest 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, I now turn to the
econometric analysis.

The econometric specifications used in this paper are panel data models with
individual-specific 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. This methodology is here applied in
the context of regression analysis in order to control more effectively for industry-
specific effects, time effects and changes in other variables. The industry effects
control for any industry characteristics that are important for wages and labour
productivity but are relatively stable over time. 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 the progressive opening of the British economy, the UK government’s prices
and incomes policies between 1965 and 1973, and macroeconomic fluctuations. It is very 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.

Of all these factors, the one whose omission from my empirical specification is the most regrettable is the intensification of foreign competition caused by the gradual opening of the British economy during the 1960s and 1970s. Unfortunately, it is difficult to control for this in a more satisfactory way.\(^5\) It should be emphasised, however, that there is no reason to think that foreign competition may have had a differential effect across the two groups of industries, i.e. the group with \( COLL = 1 \) and the one with \( COLL = 0 \). As pointed out in Symeonidis (2002b) and confirmed in Table 4 below, there is no strong evidence of any difference in initial conditions between the two groups with respect to foreign competition. Moreover, although tariff reductions occurred throughout the 1960s and the 1970s, they became in fact more pronounced after 1967, when the Kennedy Round was completed. This may be part of the reason why Kitchin (1976) was not able to identify any overall pattern of falling or rising effective protection between 1963 and 1968 in UK manufacturing. On the

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\(^5\) 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 some 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 clearly endogenous. Moreover, the industrial classification used in the foreign trade statistics during the period examined in this book has been subject to changes over time and is often difficult to match with the one used in the Census of Production.
other hand, the effect of the 1956 restrictive practices legislation was mostly realised between 1958 and 1968, i.e. before the first stage of the Kennedy round tariff cuts. Finally, there is no evidence that changes in the level of effective protection were any different between industries with a change in competition regime and industries in the control group, at least between 1963 and 1968. Kitchin provides estimates of effective protection for both these years 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 industry groups that I could classify as having experienced a change in competition regime, and decreased in the other 6. For industry groups that I could classify as having experienced no change in competition regime the respective numbers were 8 and 10. In summary, it is not unreasonable to argue that the estimated effect of the 1956 Act from my regressions in this paper is not biased by the failure to control for foreign competition.

The basic specification for labour productivity is

\[
\ln GPPER_{it} = \alpha_i + \beta_1 \ln DS_{it} + \beta_2 \ln (K / L)_{it} + \beta_3 \ln PLANTSIZE_{it}
\]

\[
+ \beta_4 Y54 + \beta_5 Y58 + \beta_6 Y68 + \beta_7 Y73
\]

\[
+ \beta_8 RD * Y54 + \beta_9 RD * Y58 + \beta_{10} RD * Y68 + \beta_{11} RD * Y73
\]

\[
+ \beta_{12} AGREE * Y54 + \beta_{13} AGREE * Y58 + \beta_{14} AGREE * Y68 + \beta_{15} AGREE * Y73
\]

\[
+ \beta_{16} COLL * Y54 + \beta_{17} COLL * Y58 + \beta_{18} COLL * Y68 + \beta_{19} COLL * Y73 + u_{it},
\]

and similarly for \(\ln WOP\) and \(\ln WOTH\). \(DS\) is the 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, and a positive effect on wages through its effect on labour demand.\(^6\) \(K/L\) is the capital-labour ratio, while \(PLANTSIZE\) is industry employment

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\(^6\) The potential endogeneity of prices may be a cause for concern. However, using the general producer price index as a deflator gave broadly similar results to those reported here. If there were an endogeneity problem, one would expect the two sets of results to differ. The fact that
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 are more pronounced in larger plants than in smaller ones. *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.*

*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.*

Details on variable definition and data sources are provided in the Appendix.

The results are similar suggests that the potential endogeneity of prices is probably not a
serious problem in the present case.

7 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 union density (measured at a level of aggregation between the
two-digit and the three-digit industry level) among the regressors. 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, even at the 20% industry
level; since it was available only at a low level of aggregation and also raised concerns for
potential endogeneity, it was dropped from the final specification.

8 The assumption here is that 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
The interaction terms $COLL*Y54$, $COLL*Y58$, $COLL*Y68$ and $COLL*Y73$ should pick up any differences between industries with a change in competition regime and industries without such a change. Thus the coefficient on $COLL*Y68$ ($COLL*Y73$) measures the effect of the 1956 Act between 1963 and 1968 (1973). The benchmark year is 1963. The coefficient on $COLL*Y54$ ($COLL*Y58$) allows for a comparison of the evolution of wages and productivity in the two groups of industries between 1954 (1958) and 1963. The choice of 1963 as the benchmark year may seem somewhat surprising, in light of the fact that the 1956 Act may have had some effect on wages and productivity of previously collusive industries between 1958 and 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, 2002a). I have therefore chosen to use 1963 as the benchmark year for the econometric specification in this paper.

Note that the above 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 must be itself a function of the competitive regime: I have shown elsewhere (Symeonidis 2000a, 2002a) that the intensification of price competition following the abolition of the British cartels caused a rise in concentration. (A simultaneous equations approach cannot be used here because it is difficult to find a variable that affects market structure and does not also influence footnote 8 below). On the other hand, including innovation as a regressor would be more problematic because of concerns with potential endogeneity.
productivity or wages. This would be difficult even with much more detailed data than the data available for the present study.)

Let me also point out that it is not possible to use a dynamic panel data model in the present context. This would imply losing the observations for the exogenous variables for the first two periods, and would 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 or 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 possible 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 a powerful indirect check of the exogeneity of $CHANGE$. 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.
In particular, these estimated probabilities are the fitted values from the following cross-section probit regression:

\[
\text{COLL}_i^* = \alpha_i + \beta_1 \ln DS_i + \beta_2 \ln K/L_i + \beta_3 \ln PLANTSIZE_i + \beta_4 \text{AGREE}_i \\
+ \beta_5 \text{RD}_i + \beta_6 \text{ADV}_i + \beta_7 \text{FOREIGN}_i + u_i
\]

where instead of the "propensity to collude" \(\text{COLL}^*\), an unobserved latent variable, we observe the dichotomous variable \(\text{COLL}\) which takes the value 0 for industries without collusive agreements in the mid- and late 1950s and 1 for cartelised industries.\(^9\) \(\text{ADV}\) 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 \(\text{RD}\) 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.\(^10\) \(\text{FOREIGN}\) is a dummy variable which takes the value 0 for industries

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\(^9\) 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.

\(^10\) The idea here is that while the actual level of the advertising-sales ratio and the R&D-sales ratio is endogenous and may depend on many variables, including the intensity of price competition, it is generally exogenous industry characteristics that will determine whether this ratio is 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% may be largely determined endogenously. But my binary variables \(\text{ADV}\) and \(\text{RD}\) are not very sensitive to endogenous factors that affect advertising intensity and R&D intensity. The assumption of exogeneity of \(\text{ADV}\) and \(\text{RD}\) is also consistent with the fact that a comparison of advertising-sales ratios and R&D-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
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.\footnote{Note that this variable does not capture other factors that may have influenced the extent of foreign competition across industries such as transport costs or the scope for restrictive practices aimed at reducing outside competition.} All the other variables are as previously defined. (For $DS$, $K/L$ and $PLANTSIZE$, I use 1958 data; however, the results are very similar when using 1954 data instead.) As it turns out, the coefficient on $ADV$ (but not that on $FOREIGN$) in the above probit model is statistically significant. It is also reasonable to assume that $ADV$ is not correlated with the error term in the productivity or the wages regressions, and is therefore a valid instrument for $COLL$.

The estimation results for $\ln GPPER$, $\ln WOTH$ and $\ln WOP$ are presented in Tables 2-3, while the results for $COLL$ are presented in Table 4.\footnote{The results in Table 4 are consistent, on the whole, with those reported in Symeonidis (2002b), where the determinants of collusion are analysed in greater detail using data at the four-digit industry level.} The results in Tables 2-3 are for a fixed-effects specification. The reported standard errors are heteroskedasticity-consistent, adjusted for small sample bias following MacKinnon and White (1985). In addition, the standard errors in the two-stage model are derived using a bootstrap procedure (with 1000 replications). Note that two different $R^2$’s are reported: the first does not include the fixed industry effects, while the second ($R^2_{LSDV}$) does.

Table 2 contains the results from the one-stage regressions, while Table 3 contains the two-stage results. For the regressions in Table 2, industries with ambiguous state of competition in the 1950s (or in later years) are excluded. In contrast, Table 3 makes use of nearly all the industries in the data set, excluding only exogenous institutional change, namely the introduction of TV advertising in the UK in the mid-1950s.
those with ambiguous state of competition in the late 1960s and early 1970s as well as one industry that remained collusive throughout.\textsuperscript{13}

All the results provide clear evidence of a negative effect of collusion on labour productivity growth. In particular, the coefficient on $\text{COLL} \ast Y54$ is positive and statistically significant in all regressions with $\text{lnGPPER}$, although it is much larger in the two-stage than the one-stage regression results. In particular, the one-stage regression results suggest that the net influence of collusion was a 6\% 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 30\% 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. 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 $\text{COLL} \ast Y68$ and $\text{COLL} \ast Y73$ are nowhere statistically significant at the 5\% level.

There is no evidence of any effect of collusion on wages (of manual or non-manual workers) in the present data set. One possible explanation is that wages were largely determined by institutional factors during the period examined in this paper and were not very sensitive to competitive pressure from the product market. This interpretation is supported by the fact that the variation in productivity change across industries was quite larger than the variation in earnings change throughout the period under study – see Table 1 above and Wragg and Robertson (1978). Another possible interpretation is that, while collusion may have a direct positive effect on wages, it also reduces productivity growth at the industry level, and hence has an indirect negative effect on wages. A third possibility is that less efficient firms pay lower

\textsuperscript{13} I also ran two-stage regressions using the same sample as in Table 2. The results were very similar to those reported in Table 3.
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, however, 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.¹⁴ 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 different and 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 (see Dowrick 1989).

An interesting feature of the results in Tables 2 and 3 is the magnitude of the coefficients on the year dummies, especially those for 1968 and 1973. After controlling for market size, the capital-labour ratio, plant size, cross-industry differences in the extent of collective bargaining and technological opportunity, the effect of cartel policy and industry effects, both labour productivity and real wages were rising rapidly during 1963-1973 (they seem to have also been rising during 1954-1963, but the evidence there is not strong). To some extent this must reflect technological progress during this period. It may be also 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 $K/L$, its effect could be partly picked up by the time dummies.

¹⁴ 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.
Still, it is difficult to escape the conclusion that some of the rise in wages and productivity during 1963-1973 was due to factors not explicitly included in the present theory. This, of course, does not invalidate the comparison between industries affected by the 1956 Act and industries not affected to the extent that these other factors are not correlated with the variable \textit{COLL}.

Finally, note that most of the other explanatory variables in Tables 2 and 3 have the expected signs, although they are not always 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. An increase in average plant size is associated with a fall in productivity and an increase in the wages of manual workers (but not in the wages of non-manual workers); this is consistent with the view that average plant size is positively correlated with union power. There is some evidence from the one-stage model that an increase in the proportion of the workforce covered by collective bargaining agreements tends to increase wages (although it has no effect on labour productivity); however, the evidence is not strong, and in particular the effect disappears 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 is not necessarily surprising, given that the effect of innovation on productivity may work mainly through the use rather than the production of innovations.


The introduction of cartel laws in the UK in the late 1950s caused an intensification of price competition in previously cartelised manufacturing industries, but it did not affect those industries which were not cartelised. The econometric results from a
A comparison of the two groups of industries provides 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.

There are several possible mechanisms that could explain the positive effect of competition 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 were brought about through larger effort by managers or through the more efficient use of 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 2002a) that the introduction of cartel legislation in the UK had no significant effect on the production of innovations. Another mechanism which does not appear to have been very important is changes in relative wages (or the rate of growth of wages). On the other hand, the significant restructuring of the previously collusive industries through mergers and exit (Symeonidis 2000a, 2002a) must be part of the explanation for the positive effect of competition on labour productivity.

<table>
<thead>
<tr>
<th></th>
<th>$\Delta \text{lnGPPER}$</th>
<th>$\Delta \text{lnWOP}$</th>
<th>$\Delta \text{lnWOTH}$</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Industries with $\text{COLL} = 1$</strong> (n = 36)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1954-1958</td>
<td>0.049 (0.066)</td>
<td>0.072 (0.036)</td>
<td>0.048 (0.066)</td>
</tr>
<tr>
<td>1958-1963</td>
<td>0.144 (0.109)</td>
<td>0.117 (0.033)</td>
<td>0.101 (0.040)</td>
</tr>
<tr>
<td>1963-1968</td>
<td>0.171 (0.099)</td>
<td>0.131 (0.036)</td>
<td>0.091 (0.053)</td>
</tr>
<tr>
<td>1968-1973</td>
<td>0.201 (0.123)</td>
<td>0.194 (0.056)</td>
<td>0.123 (0.072)</td>
</tr>
<tr>
<td><strong>Industries with $\text{COLL} = 0$</strong> (n = 52)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1954-1958</td>
<td>0.098 (0.087)</td>
<td>0.084 (0.054)</td>
<td>0.032 (0.077)</td>
</tr>
<tr>
<td>1958-1963</td>
<td>0.181 (0.122)</td>
<td>0.109 (0.055)</td>
<td>0.096 (0.054)</td>
</tr>
<tr>
<td>1963-1968</td>
<td>0.233 (0.116)</td>
<td>0.146 (0.045)</td>
<td>0.111 (0.057)</td>
</tr>
<tr>
<td>1968-1973</td>
<td>0.235 (0.184)</td>
<td>0.175 (0.070)</td>
<td>0.130 (0.070)</td>
</tr>
</tbody>
</table>

Notes: Figures based on industries with available data for all five years. The figures in parentheses are standard deviations. n denotes the number of industries.
<table>
<thead>
<tr>
<th>Dep. variable:</th>
<th>Dep. variable:</th>
<th>Dep. variable:</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \ln GPPER )</td>
<td>( \ln WOP )</td>
<td>( \ln WOTH )</td>
</tr>
<tr>
<td>( \ln DS )</td>
<td>0.406 (12.72)</td>
<td>0.042 (3.59)</td>
</tr>
<tr>
<td>( \ln K/L )</td>
<td>0.241 (4.87)</td>
<td>0.094 (6.32)</td>
</tr>
<tr>
<td>( \ln PLANTSIZE )</td>
<td>(-1.188 (-3.87))</td>
<td>0.082 (3.61)</td>
</tr>
<tr>
<td>( Y54 )</td>
<td>(-0.228 (-3.40))</td>
<td>(-0.057 (-1.80))</td>
</tr>
<tr>
<td>( Y58 )</td>
<td>(-0.047 (-0.73))</td>
<td>(-0.006 (-0.24))</td>
</tr>
<tr>
<td>( Y68 )</td>
<td>0.103 (2.01)</td>
<td>0.116 (4.51)</td>
</tr>
<tr>
<td>( Y73 )</td>
<td>0.159 (1.95)</td>
<td>0.153 (4.15)</td>
</tr>
<tr>
<td>( RD*Y54 )</td>
<td>(-0.044 (-0.74))</td>
<td>0.045 (2.45)</td>
</tr>
<tr>
<td>( RD*Y58 )</td>
<td>(-0.028 (-0.57))</td>
<td>(-0.012 (-0.64))</td>
</tr>
<tr>
<td>( RD*Y68 )</td>
<td>(-0.0001 (-0.00))</td>
<td>(-0.006 (-0.37))</td>
</tr>
<tr>
<td>( RD*Y73 )</td>
<td>0.108 (1.44)</td>
<td>(-0.003 (-0.16))</td>
</tr>
<tr>
<td>( AGREE*Y54 )</td>
<td>0.171 (1.83)</td>
<td>(-0.123 (-2.81))</td>
</tr>
<tr>
<td>( AGREE*Y58 )</td>
<td>(-0.024 (-0.27))</td>
<td>(-0.098 (-2.78))</td>
</tr>
<tr>
<td>( AGREE*Y68 )</td>
<td>(-0.027 (-0.39))</td>
<td>(-0.014 (-0.39))</td>
</tr>
<tr>
<td>( AGREE*Y73 )</td>
<td>(-0.005 (-0.04))</td>
<td>0.131 (2.63)</td>
</tr>
<tr>
<td>( COLL*Y54 )</td>
<td>0.060 (2.16)</td>
<td>0.023 (1.72)</td>
</tr>
<tr>
<td>( COLL*Y58 )</td>
<td>0.033 (1.30)</td>
<td>(-0.001 (-0.03))</td>
</tr>
<tr>
<td>( COLL*Y68 )</td>
<td>(-0.007 (-0.29))</td>
<td>(-0.005 (-0.50))</td>
</tr>
<tr>
<td>( COLL*Y73 )</td>
<td>0.022 (0.74)</td>
<td>0.001 (0.03)</td>
</tr>
</tbody>
</table>

\( R^2 \) | 0.00 | 0.00 | 0.00 |
\( R^2_{\text{LSDV}} \) | 0.98 | 0.98 | 0.94 |

| | 105.9 | 26.3 | 58.08 |
| Hausman statistic | \( \approx 0 \) | 0.12 | \( \approx 0 \) |
| Prob-value | | | |
| No. of industries | 93 | 93 | 93 |
| No. of observations | 460 | 460 | 460 |

Note: t-statistics based on heteroskedasticity-consistent standard errors in parentheses.
Table 3. Regression results for lnGPPER, lnWOP and lnWOTH: two-stage estimation

<table>
<thead>
<tr>
<th></th>
<th>Dep. variable: lnGPPER</th>
<th>Dep. variable: lnWOP</th>
<th>Dep. variable: lnWOTH</th>
</tr>
</thead>
<tbody>
<tr>
<td>lnDS</td>
<td>0.376 (12.52)</td>
<td>0.054 (4.90)</td>
<td>0.054 (4.13)</td>
</tr>
<tr>
<td>lnK/L</td>
<td>0.235 (5.02)</td>
<td>0.100 (6.43)</td>
<td>0.017 (1.12)</td>
</tr>
<tr>
<td>lnPLANTSIZE</td>
<td>−0.219 (−4.65)</td>
<td>0.083 (3.80)</td>
<td>0.038 (1.65)</td>
</tr>
<tr>
<td>Y54</td>
<td>−0.019 (−0.24)</td>
<td>−0.080 (−1.91)</td>
<td>0.012 (0.19)</td>
</tr>
<tr>
<td>Y58</td>
<td>−0.012 (−0.17)</td>
<td>−0.004 (−0.11)</td>
<td>−0.028 (−0.62)</td>
</tr>
<tr>
<td>Y68</td>
<td>0.153 (2.17)</td>
<td>0.098 (2.85)</td>
<td>0.097 (1.87)</td>
</tr>
<tr>
<td>Y73</td>
<td>0.285 (3.05)</td>
<td>0.163 (3.38)</td>
<td>0.093 (1.59)</td>
</tr>
<tr>
<td>RD*Y54</td>
<td>−0.001 (−0.02)</td>
<td>0.023 (0.82)</td>
<td>−0.039 (−1.06)</td>
</tr>
<tr>
<td>RD*Y58</td>
<td>−0.054 (−1.00)</td>
<td>−0.013 (−0.59)</td>
<td>−0.011 (−0.41)</td>
</tr>
<tr>
<td>RD*Y68</td>
<td>0.051 (0.95)</td>
<td>−0.009 (−0.47)</td>
<td>0.031 (1.22)</td>
</tr>
<tr>
<td>RD*Y73</td>
<td>0.158 (2.10)</td>
<td>−0.007 (−0.26)</td>
<td>0.012 (0.46)</td>
</tr>
<tr>
<td>AGREE*Y54</td>
<td>−0.250 (−1.87)</td>
<td>−0.090 (−1.13)</td>
<td>−0.140 (−1.24)</td>
</tr>
<tr>
<td>AGREE*Y58</td>
<td>−0.083 (−0.65)</td>
<td>−0.106 (−1.62)</td>
<td>−0.056 (−0.67)</td>
</tr>
<tr>
<td>AGREE*Y68</td>
<td>−0.121 (−0.99)</td>
<td>0.011 (0.18)</td>
<td>−0.025 (−0.27)</td>
</tr>
<tr>
<td>AGREE*Y73</td>
<td>−0.236 (−1.46)</td>
<td>0.092 (1.02)</td>
<td>0.148 (1.41)</td>
</tr>
<tr>
<td>COLL*Y54</td>
<td>0.292 (3.52)</td>
<td>0.021 (0.40)</td>
<td>−0.042 (−0.65)</td>
</tr>
<tr>
<td>COLL*Y58</td>
<td>0.044 (0.58)</td>
<td>0.004 (0.13)</td>
<td>−0.026 (−0.53)</td>
</tr>
<tr>
<td>COLL*Y68</td>
<td>0.061 (0.85)</td>
<td>−0.009 (−0.25)</td>
<td>0.024 (0.47)</td>
</tr>
<tr>
<td>COLL*Y73</td>
<td>0.152 (1.58)</td>
<td>0.036 (0.67)</td>
<td>−0.004 (−0.06)</td>
</tr>
<tr>
<td>R²</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>R²_LSDV</td>
<td>0.98</td>
<td>0.98</td>
<td>0.94</td>
</tr>
<tr>
<td>Hausman statistic</td>
<td>348.4</td>
<td>56.54</td>
<td>47.35</td>
</tr>
<tr>
<td>Prob-value</td>
<td>≈0</td>
<td>≈0</td>
<td>0.0003</td>
</tr>
<tr>
<td>No. of industries</td>
<td>132</td>
<td>132</td>
<td>132</td>
</tr>
<tr>
<td>No. of observations</td>
<td>650</td>
<td>650</td>
<td>650</td>
</tr>
</tbody>
</table>

Note: t-statistics based on bootstrap heteroskedasticity-consistent standard errors in parentheses.
Table 4. Regression results for the determinants of $COLL$.

<table>
<thead>
<tr>
<th>lnDS</th>
<th>lnK/L</th>
<th>lnPLANTSIZE</th>
<th>AGREE</th>
<th>RD</th>
<th>ADV</th>
<th>FOREIGN</th>
<th>constant</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.04</td>
<td>0.17</td>
<td>0.43</td>
<td>2.19</td>
<td>−1.36</td>
<td>−1.32</td>
<td>0.07</td>
<td>−1.57</td>
</tr>
<tr>
<td>(0.24)</td>
<td>(0.84)</td>
<td>(1.43)</td>
<td>(1.52)</td>
<td>(−2.33)</td>
<td>(−2.39)</td>
<td>(0.24)</td>
<td>(−0.71)</td>
</tr>
</tbody>
</table>

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

No. of observations: 98

Note: t-statistics based on heteroskedasticity-consistent standard errors in parentheses.
APPENDIX: 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 (2002b) describes these 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 indices was obtained from the Annual Abstract of Statistics. Industry-specific producer price indices 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, 2002a, 2002b); see these references for details and a listing of the sources used.
REFERENCES

Publications used only as data sources are detailed in the text and are not included here.


