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# The Relationship between Firm Births and Job Creation

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# The Relationship between Firm Births and Job Creation: Did this change in Britain in the 1990s?


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***Abstract:*** This paper examines the relationship between firm births and job creation in Great Britain. We use a new data set for 60 British regions, covering the whole of Great Britain, between 1980 and 1998. The central theme of the paper is that, with the exception of a recent paper by Audretsch and Fritsch for Germany, the relationship between new-firm startups and employment growth has previously been examined either with no time-lag or with only a short period lag. The current paper examines short-run as well as long-run relationships and provides results for Great Britain similar to those for Germany. We find that the short-run employment impact of new-firm startups in British regions has been bigger in the 1990s compared to the 1980s. Concerning long-run effects, we find that the employment impact of new-firm startups is strongest after about five years, but the effect disappears after a decade.



***Keywords:*** new-firm startups, employment growth, entrepreneurship, Great Britain

***JEL-classification:*** J23, L10, M13, R11



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## 1. Introduction

This paper examines the relationship between new-firm startups and employment change in Great Britain. This relationship is of considerable policy importance, since national and sub-national governments in Britain have, for more than two decades, sought to raise business startup rates in order to enhance wealth- and job-creation. An example of a central government policy was the Enterprise Allowance Scheme (EAS). At its peak in 1987-88, public expenditure on EAS was virtually £200 million, to subsidise more than 106,000 unemployed people to start a new business. A second example is the Business Birth Rate Strategy initiated in Scotland in the early 1990s, which sought to raise new-firm formation rates. A third example is the announcement by the Welsh Assembly in 2001 of its Entrepreneurship Action Plan for Wales. The assumption of a strong positive relationship between increased new-firm startup rates and subsequent employment growth underpinned all such policies.

The paper tests for that underpinning. It begins by presenting the theoretical arguments for the presence of a relationship between startups and job creation, going on to provide an overview of current evidence. The central theme of the paper is that, with the exception of a recent paper by Audretsch and Fritsch (2002) for Germany, the relationship between startups and job creation has previously been examined either with no time-lag or with only a short period lag.

The current paper claims to make six advances on prior work. The *first* is to construct and use a long-run (1980-98) data set, that facilitates a valid comparison between the results for Great Britain and Germany. A *second* innovation is the attention given to two measurement issues. The first is whether business stock or labour force should be used to normalize the startup rates of different regions, and the second involves a shift-share adjustment, to take account of the impact of different sectoral structures on the relationship. *Thirdly*, the paper explicitly incorporates tests for spatial autocorrelation and specification-error which virtually all models pass. *Fourthly*, the paper shows the impact of excluding employment change in the public or quasi-public sector from the analysis, so that a "pure" private-sector equation is estimated. *Fifth*, the paper explicitly corrects for multicollinearity caused by strong intertemporal correlations between startup rates from different periods. *Sixth* and finally, it utilises the concept of the "Upas Tree" to see whether Scotland and Wales differ from England in the relationship between start-ups and job creation.

The key finding of the paper is that new-firm birth rates are associated with subsequent employment growth at a regional level. This relationship appears to be strongest with a five year lag but to have evaporated after a decade. We also find the (short-run) relationship to be much stronger in the 1990s than in the previous decade. As a crude rule of thumb, however, one new firm started between 1984 and 1990 on average created 2.3 net new jobs between 1991 and 1998. This is subject to the reservation that there is a clear "Upas Tree" effect, with the job creation impact of a startup in Scotland/Wales being significantly lower than in England.

Despite the presence of a demonstrable short and long-run relationship between new-firm births and employment change, the paper concludes that this does not constitute a justification for public policies to raise new-firm birth rates.

## 2. The Issues

This section reviews the theoretical basis for believing a relationship exists between the extent to which a geographical area is "entrepreneurial" and the extent to which it is "economically successful". We show there are a priori reasons for expecting a positive relationship, but that there are also reasons for expecting no relationship, or for expecting the relationship to vary according to the type of entrepreneurship.

There are three reasons why more "entrepreneurial" areas might generate more jobs- where jobs are a measure of "economic success". The first is that if "entrepreneurial" is reflected in "new-firm formation" then these new firms themselves create jobs directly and so add to the stock of jobs. The second is that the new firms constitute a (real or imagined) competitive threat to existing firms, encouraging the latter to perform better. Finally, new firms provide a vehicle for the introduction of new ideas and innovation to an economy, which has been shown to be a key source of long-term economic growth [Romer (1986)]. Indeed Audretsch and Thurik (2001) argue that the role of new firms in technological development has been enhanced by a reduced importance of scale economies and an increasing degree of uncertainty in the world economy, creating more room for innovative entry.

The reasons for not expecting firm formation rates to be related to job creation are also three-fold. The first is that new firms directly contribute only a very small proportion of the stock of jobs in the economy [5.5% of the stock of UK employment in 1989 was in firms that had been born in the previous two years- Storey (1994)]. Secondly, most new firms were merely displacing existing firms without any observable gain either to the customer or to the economy [Storey and Strange (1992) show that 78% of sales of new firms are to firms in the same administrative county]. Finally, innovation is very much the exception rather than the rule amongst new firms. For example, during the 1990s, twice-yearly Surveys were taken of (primarily) small firms in the West Midlands.The proportion of firms claiming to have introduced a product or service new to the marketplace in the prior twelve months varied from 4% to 17% [Price Waterhouse Coopers (1999)].

The final set of arguments is that the scale of job creation that takes place in new firms varies considerably from firm to firm. Storey and Strange (1992) show that 2% of all new firms created 33% of jobs in new firms, reflecting the extent of skewness in the distribution of employment. This skewness is taken to reflect differences in the human capital of founders [Frank (1988)] or their ability to learn [Jovanovic (1982)]. For these reasons job creation, even in new enterprises, may be more strongly influenced by the human capital of the founders, than by the absolute number of startups [Cooper, Woo and Dunkelberg (1989), Van Praag and Cramer (2001)].

## 3. The Evidence

Prior empirical studies of the relationship between "entrepreneurship" and "economic success" have adopted different approaches, yielding very different results. Three studies, albeit using very different dependent and independent variables, find a positive relationship. GEM (2000) examines the relationship across 21 countries between "total Entrepreneurial Activity" and per cent growth in GDP. They show that "Entrepreneurship is strongly associated with economic growth. Amongst nations with similar economic structure, the correlation between entrepreneurship and economic growth exceeds 0.7 and is highly significant". Second, Johnson and Parker (1996) find "robust evidence that growth in births (and reductions in deaths) *significantly* lowers unemployment".[1] In EIM (1994), no relation is

[1] Their italics.

found between employment growth and firm dynamics for the Netherlands in the period 1987-90. Finally, taking the period 1981-89, Ashcroft and Love (1996), find new-firm formation to be strongly associated with net employment change in Great Britain.

Fritsch (1996), however, obtains more ambiguous results. In a pioneering study that can be considered as the fore-runner to this study, he examines 74 (former) West German planning regions, 1986-89. He finds “a positive statistical relationship between entry rates and employment change for manufacturing in the longer run, …(but)... this relationship proves to be negative for the service sector as well as for all sectors together” [Fritsch (1996), p. 247].

A recent paper by Audretsch and Fritsch (2002) provides new insights for (West) Germany. Taking the same 74 planning regions, they present three key findings. First, confirming the Fritsch (1996) findings, startup rates in the 1980s are found to be unrelated to employment change. Second, in the 1990s, those regions with higher startup rates have higher employment growth. Third, and perhaps most interesting, is that regions with high startup rates in the 1980s had high employment growth in the 1990s.

## 4. Modelling Issues

The relationship to be modelled is of the simple form of Equation (1) below

$$\Delta EMP_t = F\left(BIR_{t-1}, CON\right) \qquad (1)$$

where $\Delta EMP_t$ = change in employment,
$BIR_{t-1}$ = firm birth rates at start of period,
$CON$ = control variables.

Whilst, in principle, the model is simple to estimate there are five clear problems. The *first* relates to the measure of BIR to be used. Given that the units of account are geographical areas that vary in size, BIR needs to be normalised by a size measure. The denominator should both control for the different absolute sizes of the regions concerned, and represent the source from which startups or firm formations are most likely to come [Ashcroft, Love and Malloy (1991)]. The two variables normally used, as denominators are the stock of existing firms, and the size of the regional workforce [Keeble, Walker and Robson (1993)]. This is called the Business Stock (BS) approach and the Labour Market (LM) approach, respectively. The BS approach assumes new firms arise from existing ones, whereas the LM approach assumes that new firms arise from (potential) workers.[2] The choice of measure can be highly significant. For example, for a given number of startups, regions which are equally large in terms of workforce but which are different in terms of average firm size, will have the same startup rate according to the LM approach but different startup rates according to the BS approach.[3] Garofoli (1994) makes a robust case in favour of LM over BS. The latter, he argues, is misleading in areas with small numbers of (generally large) firms. Here small numbers of new firms would provide an artificially high birth rate, primarily because of the small denominator. Audretsch and Fritsch (1994) also show that, in West Germany, the statistical relationship between unemployment and startup activity crucially depends on the BS or LM methods used to measure startup rates.[4] Whilst favouring LM from a theoretical viewpoint, both approaches will be used in the paper.

The *second* key problem relates to the lag structure specified in Equation (1). The case for the lag is that the employment impact of new firms is unlikely to be maximised immediately. Storey (1985), for example, shows that new manufacturing firms are generally eight or nine years old by the time they reach their peak employment, at which time they have about twice the number they had at the end of Year 1. However, because of their high exit rates, total employment in a cohort of new firms is lower in Year 5 than in Year 1. This means that the maximum employment impact of a cohort depends on the scale of these two influences and is an empirical, rather than theoretical, issue.

The above discussion is framed in terms of simple arithmetic, but more complex social processes could also influence the lag. For example, new businesses started in time period t may stimulate the formation of other new firms in period t+1. This may be because the t period firms constitute a market for the t+1 firms; alternatively the success of the t firms stimulates individuals to seek to emulate them, so the t firms become “role-models”. In turn, the t+1 firms stimulate more firms in later time periods, with the result that employment in that economy in t+n

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[2] In Ashcroft and Love (1996), total population is used as denominator. However, this assumes that new firms may arise from children or elderly persons as well. This seems less plausible.

[3] In Van Stel, Dielbandhoesing, Van den Heuvel and Storey (2002) the (differences between the) two approaches are illustrated in detail by means of a numerical example for actual GB data.

[4] In Audretsch and Fritsch (1994) the business stock approach is called the ecological approach.

is stimulated. Theory, again however, is not helpful in specifying the value of n. Nevertheless it seems clear that this is likely to be a period of at least a decade.

The above theoretical arguments discourage the use of contemporaneous startup rate variables in the model, i.e., employment growth in period t being explained by new-firm startups in period t. Although correlations might be significant, the implied causal relation from births to (immediate) growth is misleading. Positive correlations between startup rates and growth in the same period are often due to reversed causality, i.e., regions with high growth attracting new firms.[5] In our empirical work we will include lagged startup rates only.

A *third* problem relates to differences in industrial structure between regions. This raises the question of whether the different sectoral structures of regions should be taken into account, since this influences the number of startups and hence job creation. Taking only the difference between services and manufacturing, startup rates are higher in service industries than in manufacturing [Audretsch and Fritsch (2002)], partly because entry barriers are lower, Minimum Efficient Scale (MES) is lower and because, for some services, demand is high. So, regions with a high share of services in the local economy are more likely to have higher startup rates than regions with a low service share.

But this does not necessarily mean these regions are also more "entrepreneurial", in the sense that startup rates are higher for each sector of the local economy (or most sectors of the local economy). Therefore, to correct for different sectoral structures, the Ashcroft, Love and Malloy (1991) shift-share procedure is applied to derive a measure of sector-adjusted startup activity. This sector-adjusted number of startups is defined as the number of new firms in a region that can be expected to be observed if the composition of industries was identical across all regions. Thus, the measure adjusts the raw data by imposing the same composition of industries on each region [Audretsch and Fritsch (2002)]. In this paper the results from using both the unadjusted and adjusted startup rates are presented.[6]

[5] Even if there is a lag in this reversed causality process, the measured correlation is often still positive, because of path dependency in the growth performance of regions.

A *fourth* issue relates to the choice of control variables (CON) used in Equation 1. Previous studies have shown urban and rural areas differ in both employment change and in new-firm formation rates. In their review of regional variations in firm birth rates, Reynolds, Storey and Westhead (1994) pointed to urban areas consistently having higher formation rates in the 1980s than non-urban areas. Employment change, however, has been more mixed, with an urban-rural shift in the 1970s and 1980s [Fothergill and Gudgin (1979)] but a more mixed picture in more recent times [Green and Turok (2000)]. Account of urban/rural differences is taken by the inclusion of a population density variable, and by Standard Region dummies.

A second control factor that has to be taken into account is the nature of the labour market. Models of self-employment choice [Rees and Shah (1986)] assume the welfare maximising individual chooses between self-employment, paid employment and unemployment. They predict that an exogenous rise in unemployment leads to a rise in self-employment, when alternative employment opportunities fall, all else equal. Whilst local labour market conditions influence self-employment choice, so also do national macro-economic conditions. Recessionary conditions, across the whole economy, reduce the migration incentive and so may encourage local self-employment. To establish the impact of macro-economic conditions, we will analyse recession and boom periods separately.

A third (set of) control factor(s) relates to the earlier discussed problem of reversed causality. Even if we include lagged startup rates only, the employment impact of new-firm startups might be overestimated, due to positive path dependency in the economic performance of regions (i.e., the business cycle effect). We correct for this by including lagged performance indicators of regions. We include both a dynamic performance indicator (lagged growth) and a static indicator (share of population having a job).

*Fifth* and finally, the major cultural differences within the UK in attitudes towards enterprise and self-employment are recognised. We call this the Upas Tree effect. The term was originally used by Checkland (1976) to describe economic change in

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[6] In Van Stel, Dielbandhoesing, Van den Heuvel and Storey (2002) the shift-share procedure is

the city of Glasgow, and was derived from a description of the Upas Tree that was native to Java. According to legend, the Upas Tree was able to destroy other growths for a radius of 15 miles, and Checkland viewed it as analagous to the destructive effect that the heavy engineering sector had upon the growth of other industries in Glasgow for much of the twentieth century.[7] We use it to characterise Scotland and Wales, both of which appear to have a long-standing antipathy to "entrepreneurship".

## 5. Variables and Data Sources

The data used is at the spatial aggregation level of NUTS3 regions in Great Britain. This is county level in England and Wales, and local authority region level in Scotland. In this partitioning, Great Britain comprises 60 regions, each disaggregated by six sectors. This facilitates correction for sectoral differences between regions, i.e., to apply the shift-share procedure described below. Different regional and sectoral classifications in the original data files meant some linking operations were performed to ensure uniformity for the whole period 1980-98. These linking operations and the exact classification schemes employed are reported in Appendix 1. The agricultural sector is excluded, as this sector is fundamentally different from the rest of the economy, having, during this period, exceptionally low startup and death rates.

Variable definitions and their sources are now provided:

*(Lagged) Employment change.* This is the relative change in regional employment, excluding agriculture, expressed in percentages. Data on employment are taken from the Census of Employment and the Annual Employment Survey and are supplied by Nomis. Employment figures include both full-time and part-time employees, and exclude self-employed workers and unpaid family workers. Employment is measured in September of each year.

illustrated in detail by means of a numerical example for actual GB data.

[7] To our knowledge Lloyd and Mason (1984) were the first to use Checkland's analogy in this context.

*Sector adjusted startup rate.* This is the sectoral startup rate, weighted by stock of businesses per sector (business stock approach) or employment per sector (labour market approach) for Great Britain as a whole. Using this weighting implies an identical sector structure for each region. Regional employment, rather than regional workforce, is used as the denominator for the LM approach, because of greater data reliability. Startups in the agricultural sector are again excluded. Startups and stock of businesses are measured as VAT registrations and stock of VAT registered enterprises, respectively, and these data are supplied by Small Business Service. The consistency and general availability of this data source make it the most generally useful source of data on firm formation for the UK as a whole [Ashcroft, Love and Malloy (1991)]. Startup rates are expressed as the number of startups per hundred of existing firms (BS approach) or per thousand workers (LM approach).

*Startup rate.* The sectoral startup rate, weighted by the appropriate denominator (stock of businesses or employment) *for the region under consideration*. Again, agricultural startups are excluded.

*Population density.* Data on both population and area of the regions are obtained from the Office for National Statistics. The variable is expressed in thousands of inhabitants per square kilometre.

*Share of population having a job.* This variable is equal to employment divided by total population, where definitions and sources of employment and population are as described above. The variable is expressed as a fraction (i.e., a number between zero and one).

*Scotland/Wales dummy.* This dummy variable has value of 1 for Scottish or Welsh regions and value of 0 for England.

Wage rates and output levels by region were also considered for inclusion. Unfortunately, a suitable time series, at the level of spatial disaggregation required, was not available for the full period. Nevertheless we are reassured by the findings of Ashcroft and Love (1996). They also estimated a model in which employment change at the British county level is explained by startup activity and various control variables, albeit that they employ a different lag structure and consider only the period 1981-89. However, they found insignificant parameter estimates for wage level and output level. The insignificant effect of wage level might be explained by the fact that for many industries, collective bargains are concluded at the national level, causing regional variations in wage levels to be small. Based on these findings we suggest that the exclusion of these two variables from our model does not lead to omitted variable bias.

## 6. Results

The model is estimated using OLS. Each regression is estimated cross-sectionally, i.e., using 60 observations (one for each region). Because of missing (employment) data, the region Orkney/Shetland/Western Isles had to be dropped, generating a total of 59 observations. To test whether startup activity has a different impact on employment growth in different time periods several models are estimated.

Recalling that the key objective is to test for short or long-run relationships this section begins by examining the relationship between startups, 1980-83, on employment change 1984-91; then it examines startups in the period 1987-90 on employment change 1991-98. This provides an initial assessment of whether the short-term impact of startups differed between the 1980s and the 1990s.

It then examines the relationship between startups in the early and mid 1980s and employment growth in the 1990s to see whether a long-term effect exists. Each time, both the BS approach and the LM approaches, and the unadjusted and the sector-adjusted startup rate, are used. Three important regression diagnostics are presented. First, the Jarque-Bera test on normality of the disturbances. Second, the Lagrange Multiplier test on heteroscedasticity. Third, the Ramsey RESET test on general misspecification of the model. To facilitate direct evaluation of these tests p-values are shown. For all three tests the null hypothesis corresponds to "correct estimates", i.e., normality at the Jarque-Bera test, no heteroscedasticity at the Lagrange Multiplier test and no sign of misspecification at the Ramsey RESET test.

Finally, the fact that the data relate to spatial variations raises the potential problem of spatial autocorrelation, an issue "which has been widely ignored in the econometric literature, including most previous work on spatial variations in new firm formation" [Keeble, Walker and Robson (1993), p.34]. Following Keeble, Walker and Robson (1993) account is taken of this by including Standard Region dummies in the equations.[8] Some of these regional dummies, such as those for Scotland and Wales, also have a specific economic interpretation, as noted earlier (Upas Tree effect).

**(i) Startups and employment change in the 1980s**

Table 1 excludes the share of population having a job (in 1981), lagged employment change (measured over the period 1981-84) and all Standard Region dummies, because of their non-significance.[9] The last three rows show all diagnostic tests are passed (p-values are well above 0.05). The estimated parameters for the startup variables are consistently positive, but only significant for the unadjusted startup rate using the LM approach (t-value 2.60). The coefficient on the sector adjusted startup rate is however non-significant (t-value 1.56) suggesting this is mainly a sectoral effect. Regions with higher shares of services generally have more startups, because of the smaller scale of production of firms in the service sectors. Because services grew faster than manufacturing in the 1980s, these higher growth rates are ascribed to the higher startup rates in the estimation procedure.[10] But, controlling for sectoral structure, causes the size and significance of the estimated parameter to be much lower. This suggests no significant observable relationship between firm birth rates in the early 1980s and employment change later in the decade- supporting Fritsch (1996) for (West) Germany.

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[8] For this purpose the county Greater London is added to the South East region. This is because there is only one county within the London region in our data set.

[9] The parameter estimates of the remaining variables are not affected by this exclusion.

[10] Of course, this might be right as service industries might have performed better just because of the smaller scale of production in services and the associated higher startup rates. However, to avoid the possibility of erroneous conclusions due to regional differences in sector structure, we prefer to focus on the sector adjusted startup rate in this study.

Table 1: Determinants of regional employment change in the period 1984-1991 (%), short-term equation (t-values in parentheses)

| | Business stock approach | | Labour market approach | |
|---|---|---|---|---|
| | Adjusted | Un-adjusted | Adjusted | Un-adjusted |
| Constant | 8.50<br>(1.12) | 8.28<br>(1.16) | 3.65<br>(1.09) | 1.10<br>(0.363) |
| Average sector adjusted startup rate, 1980-1983 | 0.017<br>(0.029) | | 0.647<br>(1.56) | |
| Average startup rate, 1980-1983 | | 0.033<br>(0.061) | | 0.951<br>(2.60) |
| Population density 1981 | -4.03<br>(-4.10) | -4.05<br>(-4.11) | -3.81<br>(-4.27) | -3.52<br>(-4.02) |
| Adjusted $R^2$ | 0.236 | 0.236 | 0.268 | 0.318 |
| JB test: p-value | 0.482 | 0.482 | 0.679 | 0.742 |
| LM het. test: p-value | 0.368 | 0.368 | 0.937 | 0.806 |
| RESET test: p-value | 0.130 | 0.126 | 0.111 | 0.189 |

### (ii) Startups and employment change in the 1990s

The estimation results for the 1990s are shown in Tables 2-4. These tables present regressions where employment change 1991-98 is explained by the numbers of startups in the periods 1987-90 (short-term), 1984-87 (mid-term), and 1980-83 (long-term), respectively. Lagged employment change (measured over the period 1984-91) turned out to be non-significant, and was excluded. Furthermore, when the Standard Region dummies were included, all dummy coefficients had low t-values, except for those of Scotland and Wales. Closer inspection revealed that the inclusion of a combined Scotland/Wales dummy resulted in the best statistical fit. Again, the Jarque-Bera test and the Lagrange Multiplier test on heteroscedasticity are easily passed. The Ramsey RESET test is not always passed though, indicating this test is quite sensitive to small changes in specification.

Table 2: Determinants of regional employment change in the period 1991-1998 (%), short-term equation (t-values in parentheses)

| | Business stock approach | | Labour market approach | |
|---|---|---|---|---|
| | Adjusted | Un-adjusted | Adjusted | Un-Adjusted |
| Constant | -32.8<br>(-2.59) | -31.6<br>(-3.38) | -32.1<br>(-4.50) | -32.2<br>(-4.57) |
| Average sector adjusted startup rate, 1987-1990 | 1.42<br>(2.24) | | 1.66<br>(5.92) | |
| Average startup rate, 1987-1990 | | 1.70<br>(3.51) | | 1.53<br>(6.06) |
| Population density 1988 | -3.48<br>(-3.22) | -3.32<br>(-3.26) | -1.63<br>(-1.77) | -1.67<br>(-1.84) |
| Share of population having a job, 1988 | 49.9<br>(2.44) | 33.9<br>(1.68) | 61.0<br>(3.70) | 65.7<br>(4.00) |
| Dummy Scotland/Wales | -6.28<br>(-2.87) | -5.41<br>(-2.69) | -3.76<br>(-2.16) | -4.34<br>(-2.59) |
| Adjusted $R^2$ | 0.385 | 0.453 | 0.592 | 0.600 |
| JB test: p-value | 0.905 | 0.980 | 0.861 | 0.749 |
| LM het. test: p-value | 0.620 | 0.593 | 0.955 | 0.891 |
| RESET test: p-value | 0.472 | 0.422 | 0.020 | 0.003 |

Table 3: Determinants of regional employment change in the period 1991-1998 (%), mid-term equation (t-values in parentheses)

| | Business stock approach | | Labour market approach | |
|---|---|---|---|---|
| | Adjusted | Un-adjusted | Adjusted | Un-adjusted |
| Constant | -27.8<br>(-2.90) | -25.4<br>(-3.13) | -29.8<br>(-4.26) | -30.4<br>(-4.32) |
| Average sector adjusted startup rate, 1984-1987 | 1.73<br>(2.87) | | 2.01<br>(5.82) | |
| Average startup rate, 1984-1987 | | 1.87<br>(3.71) | | 1.83<br>(5.83) |
| Population density 1988 | -3.44<br>(-3.27) | -3.39<br>(-3.36) | -1.83<br>(-1.99) | -1.86<br>(-2.03) |
| Share of population having a job, 1988 | 34.0<br>(1.60) | 22.6<br>(1.08) | 55.9<br>(3.37) | 62.4<br>(3.75) |
| Dummy Scotland/Wales | -6.50<br>(-3.27) | -6.06<br>(-3.21) | -3.95<br>(-2.27) | -4.61<br>(-2.73) |
| Adjusted $R^2$ | 0.417 | 0.464 | 0.587 | 0.588 |
| JB test: p-value | 0.744 | 0.927 | 0.957 | 0.968 |
| LM het. test: p-value | 0.793 | 0.748 | 0.857 | 0.859 |
| RESET test: p-value | 0.264 | 0.261 | 0.025 | 0.003 |

Table 4: Determinants of regional employment change in the period 1991-1998 (%), long-term equation (t-values in parentheses)

| | Business stock approach | | Labour market approach | |
|---|---|---|---|---|
| | Adjusted | Un-adjusted | Adjusted | Un-adjusted |
| Constant | -21.8<br>(-2.03) | -23.7<br>(-2.36) | -29.0<br>(-3.83) | -30.9<br>(-4.08) |
| Average sector adjusted startup rate, 1980-1983 | 1.11<br>(1.58) | | 2.27<br>(4.89) | |
| Average startup rate, 1980-1983 | | 1.41<br>(2.07) | | 2.07<br>(5.15) |
| Population density 1988 | -3.74<br>(-3.28) | -3.83<br>(-3.43) | -2.00<br>(-2.05) | -1.89<br>(-1.97) |
| Share of population having a job, 1988 | 48.2<br>(2.25) | 42.7<br>(1.99) | 57.0<br>(3.23) | 65.9<br>(3.78) |
| Dummy Scotland/Wales | -7.67<br>(-3.75) | -7.18<br>(-3.52) | -4.14<br>(-2.20) | -4.68<br>(-2.62) |
| Adjusted $R^2$ | 0.357 | 0.377 | 0.534 | 0.549 |
| JB test: p-value | 0.919 | 0.919 | 0.997 | 0.961 |
| LM het. test: p-value | 0.737 | 0.859 | 0.743 | 0.880 |
| RESET test: p-value | 0.329 | 0.281 | 0.140 | 0.006 |

In marked contrast with Table 1, the estimated parameters for the startup variables in Table 2 are all highly significant, with t-values ranging from 2.24 to 6.06, implying that regions with higher startup rates at the end of the 1980s had higher employment growth rates in the 1990s. Table 3 also shows a significantly positive relation between 1984-87 startups and 1991-98 growth

Table 4 presents even more striking findings. Although Table 1 showed early-1980s startups were unrelated to employment change in the 1980s, Table 4 shows the startups from this period are positively and significantly related to employment change in the period 1991-98, at least for the labour market approach. It suggests that, while there is no short-term effect of 1980-83 startups, (no effect on growth 1984-91) there does seem to be a long-term positive effect on growth 1991-98. However, this could reflect (high) intertemporal correlations between startup rates from different periods – an issue to be examined in more detail in Section 6.(viii).

Overall, the four tables suggest the late-1980s births had a different impact on subsequent employment, compared with early-1980s births. The latter had no clear "short-term" effect, whereas the former did. However, the early- and mid-1980s births did seem to have a longer term effect on employment; whether the same will be the case for the 1990s births will only be apparent in future years.

**(iii) Recession births versus boom births**

One possible explanation for the different short-term impacts of startups in the early and late 1980s is that the 1980-83 startups may be a different type of startup, compared with the 1987-90 startups. The obvious difference is that, while 1980-83 were recession years, 1987-90 was a "boom" period. During recessions, as noted earlier, a higher proportion of startups may be from individuals with lower human capital, who find employment in the employee labour market more difficult [Cressy (1996)]. These startups may be less likely to generate jobs. On the other hand, during a period of economic prosperity, it may be the more "entrepreneurial" type of person who starts a business. This type of startup may be more likely to generate jobs in the short and the long-run. In short, while recession births may be the result of "push"-factors being at work (possibly creating fewer jobs), boom births may be more "pull-factor" in nature (possibly creating more jobs).

To test this we examine in Table 5 the relationship between firm births in the 1990s recession and short-term employment change. Using the same control variables as those reported in Tables 2-4, we estimate a regression in which employment change in the period 1993-98 is explained by the average (sector adjusted) startup rate over the period 1990-93. The results are similar to those reported in Table 2: we find a significant positive impact, implying that the lack of a relationship in the 1980s is not because of the choice of recessionary years.

Table 5: Determinants of regional employment change in the period 1993-1998 (%), short-term equation (t-values in parentheses)

| | Business stock approach | | Labour market approach | |
|---|---|---|---|---|
| | Adjusted | Un-adjusted | Adjusted | Un-adjusted |
| Constant | -12.7<br>(-1.22) | -11.1<br>(-1.30) | -13.9<br>(-2.34) | -15.2<br>(-2.54) |
| Average sector adjusted startup rate, 1990-1993 | 1.34<br>(1.58) | | 1.98<br>(6.19) | |
| Average startup rate, 1990-1993 | | 1.58<br>(2.12) | | 1.90<br>(6.25) |
| Population density 1990 | -2.17<br>(-2.03) | -2.26<br>(-2.17) | -0.48<br>(-0.58) | -0.53<br>(-0.65) |
| Share of population having a job, 1988 | 21.4<br>(1.03) | 10.3<br>(0.47) | 16.6<br>(1.08) | 23.1<br>(1.52) |
| Dummy Scotland/Wales | -7.69<br>(-4.32) | -7.69<br>(-4.42) | -3.40<br>(-2.16) | -4.00<br>(-2.63) |
| Adjusted $R^2$ | 0.305 | 0.329 | 0.575 | 0.578 |
| JB test: p-value | 0.912 | 0.838 | 0.481 | 0.498 |
| LM het. test: p-value | 0.986 | 0.740 | 0.474 | 0.267 |
| RESET test: p-value | 0.082 | 0.023 | 0.155 | 0.106 |

Instead, it seems to be the case that (new) firms in the late 1980s contribute more to employment change than in the early 1980s. This analysis implies businesses started during the period 1987-93 contribute significantly to subsequent employment change, irrespective of whether or not the businesses were started during recession or boom years. Furthermore, regions with higher startup activity in the early and mid 1980s subsequently have higher employment growth rates in the period 1991-98, suggesting that in the 1990s a high number of firms *in general* (i.e., not necessarily a high number of startups) is conducive to the economic growth of a region.

The bigger employment impact of 1987-93 births compared to 1980-83 births *might* reflect that the importance of new and small firms in the process of innovation and economic growth has increased in the last two decades of the 20th century. In this interpretation Great Britain would have moved from a more "managed" type of economy toward a more "entrepreneurial" type of economy [Audretsch and Thurik (2001)]. However, perhaps a more plausible explanation is that the increased employment impact reflects “Enterprise Policy” changes, with public policy switching from being quantity-oriented in the 1980s towards being more quality-oriented in the 1990s [Greene (2002)].

### (iv) The Upas Tree effect

Also included in the models in Tables 2-4 are the Upas Tree effect. We included a dummy that is 1 for Scottish or Welsh regions, and 0 for England. The Scotland/Wales dummy is non-significant (and therefore, not included) in the model explaining regional employment change in the period 1984-91 (Table 1), but the dummy is significantly negative for all three tables covering the period 1991-98.[11] This implies that in the 1984-91 period, the economies of Scotland and Wales were not very different from the English economy, whereas the two diverged in the 1990s.

Interestingly, since October 1993, there has been an active public policy in Scotland with the objective of increasing business startups, and in particular business startups that create jobs. This initiative is called the Business Birth Rate Strategy (BBRS) and implemented by Scottish Enterprise. In a recent review of the policy, some empirical support is presented for a positive effect of the BBRS on the number of VAT registrations per head of adult population in Scotland relative to the UK [Fraser of Allander Institute (2001)]. Although the periods studied in the current paper do not entirely coincide with the period during which the BBRS is active (from 1994 onwards), the negative value for the Scotland/Wales dummy indicates that the BBRS has yet to contribute positively to job creation in Scotland.

### (v) The LM approach outperforms the BS approach

This section reviews the results using the BS and LM approaches. The two differ markedly: the t-values of the estimated parameters of the startup rate variables are quite different, especially for the 1984-91 period. A comparison of adjusted $R^2$ values in the various tables also reveals that, without exception, these are higher for the LM approach. For both these reasons, regional workforce appears a more appropriate choice of denominator in measuring startup rates than the stock of businesses. This implies that new firms spring from people rather than from existing firms.

**(vi) Shift-share adjustment matters**

Using the shift-share procedure to adjust startup rates leads to different outcomes for the 1980s estimates according to the LM approach (Table 1). The significant impact of the unadjusted startup rate turns out to be mainly a sectoral effect, once the sector adjustment is made. Tables 2-4 show that, in the period 1991-98, the sectoral adjustment differences between the coefficients and t-values are generally smaller, especially for the LM approach. However, given the big impact of sectoral adjustment in Table 1, presenting results, with and without adjustment, is more insightful.

**(vii) Controlling for the contribution of the non-private sector to regional growth**

Ideally, analysis should be restricted to private sector enterprises and private sector employment. Unfortunately, however, both private and state-owned enterprises can be present within some SIC groups, so this section eliminates the SIC groups dominated by state-owned enterprises.[12]

Table 6 shows the births coefficients for the same regressions as in Tables 1 to 5, the only difference being the employment change variables in the public sectors. Only the (sector adjusted) LM approach results are shown, since this approach has consisitently produced a better statistical fit than the BS approach (see also section 6.v). The tests on normality and heteroscedasticity are easily passed, and so are not reported in Table 6.

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[11] Recall that the inclusion of the Scotland/Wales dummy is the outcome of the earlier described Standard Region dummy procedure to correct for spatial autocorrelation.

[12] For this purpose, employment change 1991-98 and 1993-98 is computed exclusive of SIC92 industries L, M, and N (Public administration, defence and compulsory social security; Education; and Health and social work, respectively), while employment change 1984-91 is computed exclusive of SIC80 industry 9 ("other services"); recall that we utilise employment data according to different SICs before and after 1991, see Table A1b.

Table 6: Regressions with employment change measured exclusive of the non-private sector (labour market approach) *

| Dependent variable, Main explanatory variable | Coefficient sector adjusted startup rate (t-value) | Adjusted $R^2$ | RESET test: p-value |
|---|---|---|---|
| I Empl. change 1984-91, Startup rate 1980-83 | 0.65 (1.56)<br>*1.11 (2.09)* | 0.268<br>*0.233* | 0.111<br>*0.263* |
| II Empl. change 1991-98, Startup rate 1987-90 | 1.66 (5.92)<br>*1.99 (6.12)* | 0.592<br>*0.619* | 0.020<br>*0.097* |
| III Empl. change 1991-98, Startup rate 1984-87 | 2.01 (5.82)<br>*2.33 (5.64)* | 0.587<br>*0.593* | 0.025<br>*0.141* |
| IV Empl. change 1991-98, Startup rate 1980-83 | 2.27 (4.89)<br>*2.70 (4.94)* | 0.534<br>*0.555* | 0.140<br>*0.472* |
| V Empl. change 1993-98, Startup rate 1990-93 | 1.98 (6.19)<br>*2.87 (7.47)* | 0.575<br>*0.652* | 0.155<br>*0.138* |

* For each of the five regressions, the first row is taken from Tables 1-5. The second row (in italics) presents selected results from the corresponding regression with employment change measured exclusive of the non-private sector: SIC80 industry 9 (regression I) or SIC92 industries L, M, and N (regressions II to V). Startup rates are all sector adjusted.

Table 6 shows these adjustments have several important consequences. First, the coefficients of the startup rate variables increase in all five model specifications. This suggests that, in the regressions of Tables 1-5, high growth in non-private sectors was partly associated with low startup rates and vice versa, resulting in a downward bias on the startup rate coefficient. Most important, the t-value of the adjusted startup rate 1980-83 in regression I is now 2.09, implying a positive impact of early-1980s startups in the short-term. Apart from this, the coefficients in Table 6 confirm our earlier findings that the short-term impact of 1980-83 startups is smaller than the short-term impact of 1987-90 startups (compare regressions I and II). They also suggest the long-term effect of 1980-1983 startups is bigger than the short-term effect (compare regression IV and I). A more detailed analysis of the lag structure will be provided in Section 6.(viii).

Finally, in four out of the five regressions in Table 6, the adjusted $R^2$ increases when the non-private sector is excluded, and the p-value of the RESET test increases, so that all five specifications now pass this test. We conclude that correcting for non-private enterprise-induced regional growth is important.

**(viii) A long-term effect?**

To test for the relative magnitude of the long and short-run effects, Table 6 shows direct comparisons. In regressions II, III and IV, using the same control variables, employment change 1991-98 is explained by startup rate 1987-90, 1984-87, and 1980-83 respectively. The coefficient of these startup rates decreases over time: 2.70 for 1980-83, 2.33 for 1984-87, and 1.99 for 1987-90 (private sector). This seems to indicate that the long-run effect exceeds the short-run effect.

However, we must be cautious in comparing these coefficients. To avoid multicollinearity we estimated the impact of the startup rates from different periods in separate regressions. A disadvantage of this approach is that, because of the strong intertemporal correlation between startup rates, the estimated startup rate coefficient may pick up some of the effect of startup activity from other periods. This means comparing coefficients of the long-term and short-term equations is complex.

A better way of establishing the individual impacts of startup rate variables from different periods draws upon the distributed lag literature [Stewart (1991)]. By including startup rates from different periods in one regression, but imposing restrictions on the individual parameters, an accurate approximation of the shape of the lag response can be obtained. In the Almon method, parameter restrictions are imposed in such a way that the coefficients of the lagged variables are a polynomial function of the lag length. In this way the startup rate coefficients are reparameterized in a "smooth" way.

We apply the Almon method for a quadratic polynomial function (i.e., a polynomial of second degree). This choice corresponds to imposing one parameter restriction.[13] The results are shown in Table 7, with further details presented in Appendix 2.

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[13] This can be seen as follows. In the unrestricted regression three startup rate variables are included in the model, while in the first unrestricted regression column, only two variables are included (COMBI1 and COMBI2 in Table 7). In the second unrestricted regression column, only one startup rate variable is included (COMBI3), and this corresponds to two parameter restrictions. The startup rate coefficients in the restricted regressions are linear combinations of the combinatory variable coefficients. See equation (A3) in Appendix 2.

Table 7: Regressions examining the lag structure (LM approach; t-values in parentheses)

| | Including non-private sector | | | Excluding non-private sector | | |
|---|---|---|---|---|---|---|
| | Unrestricted regression | Restricted regression (one restr.) | Restricted regression (two restr.) | Unrestricted regression | Restricted regression (one restr.) | Restricted regression (two restr.) |
| COMBI1= $X_{-1}+2X_{-2}+3X_{-3}$ | | 1.77 (1.87) | | | 2.17 (1.94) | |
| COMBI2= $X_{-1}+4X_{-2}+9X_{-3}$ | | -0.64 (-1.49) | | | -0.79 (-1.57) | |
| COMBI3= $-2(X_{-1}+X_{-2})$ | | | -0.46 (-5.93) | | | -0.54 (-5.96) |
| Startup rate 1987-90 ($X_{-1}$) | 1.32 (0.97) | **1.13 (2.17)** | **0.92 (5.93)** | 3.18 (2.02) | **1.38 (2.25)** | **1.09 (5.96)** |
| Startup rate 1984-87 ($X_{-2}$) | 0.72 (0.43) | **0.98 (4.57)** | **0.92 (5.93)** | -1.22 (-0.63) | **1.17 (4.64)** | **1.09 (5.96)** |
| Startup rate 1980-83 ($X_{-3}$) | -0.42 (-0.39) | **-0.44 (-0.42)** | | -0.36 (-0.29) | **-0.61 (-0.50)** | |
| Adjusted $R^2$ | 0.579 | 0.587 | 0.593 | 0.609 | 0.605 | 0.610 |
| **P-values:** | | | | | | |
| JB test | 0.952 | 0.976 | 0.985 | 0.053 | 0.230 | 0.226 |
| LM het. test | 0.894 | 0.878 | 0.877 | 0.909 | 0.945 | 0.951 |
| RESET test | 0.017 | 0.016 | 0.019 | 0.101 | 0.088 | 0.104 |
| **Validity Almon restrictions:** | | | | | | |
| F-test statistic | | 0.024 | 0.100 | | 1.531 | 0.891 |
| Critical value (5% level) | | 4.04 | 3.19 | | 4.04 | 3.19 |

Dependent variable is employment change 1991-98, measured including and excluding the non-private sector, respectively. All regressions use population density 1988, employment share 1988, a Scotland/Wales dummy and a constant as additional explanatory variables. Startup rates are all sector adjusted. Null hypothesis for JB test, LM het. test and RESET test is “correct model specification”. Null hypothesis for F-test is “valid restrictions”. Critical values for F-tests are according to F(1;52) and F(2;52) distributions.

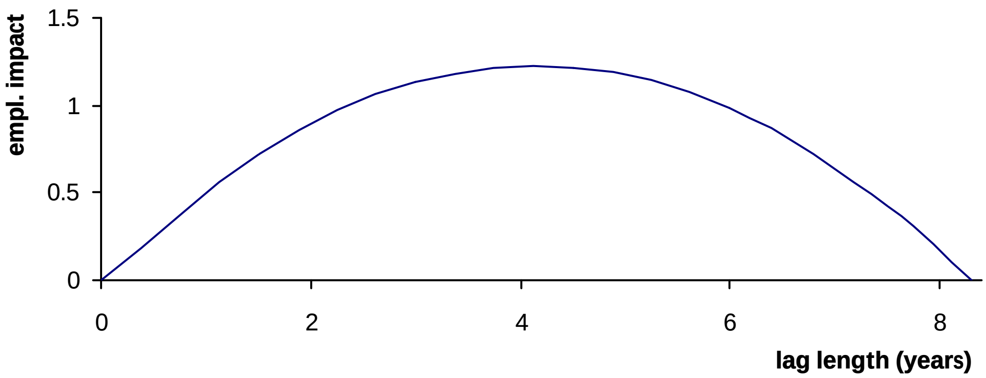
In Table 7, regression results using unrestricted regression (i.e., free estimation) and restricted regressions (i.e., using the Almon method) are presented, both for employment growth variables measured including and excluding the non-private sector (see Section 6.vii). Recall that when only one startup rate variable is included, the coefficients on the startup rate variables are highly significant, irrespective of the period chosen (see Table 6). As noted earlier, this may be due to high intertemporal correlation between startup rates from different periods, i.e., the startup rate from a certain period picks up some of the effect of startup rates from other periods. For the *unrestricted regression* we see that t-values of the separate startup rates are low (except for startup rate 1987-90 in the regressions excluding the non-private sector), and this is due to multicollinearity. In the first *restricted regression* column a corrected lag pattern is presented. We see that the impact of the startup rate 1987-90 is strongest, and the impact of 1984-87 startups is also positive and significant. The impact of 1980-83 startups, however, is zero: absolute t-values are low.[14] This pattern suggests that the lag is approximately 4 to 7 years. The results for 1980-83 startups clearly demonstrate the necessity to take account of intertemporal correlations between the different lags of the startup rate. The validity of imposing the Almon restriction is formally confirmed by applying a standard F-test on the parameter restriction. In Table 7, the null hypothesis of valid restrictions is not rejected. More details are again found in Appendix 2.

Using the estimation results from the first unrestricted regression column in Table 7, the employment impact of the startup rate can be written as a function of the lag length of the startup rate as $b_{i^*} = 1.77\left(i^*/3\right) - 0.64\left(i^*/3\right)^2$, where $i^*$ is the lag length in years.[15] The employment impact of startup rates is maximised after 4.1 years and completely extinguished after 8.3 years, counting backwards from 1991. So, according to this formula, startups from 1987 contribute most to employment growth 1991-98, whereas new-firm startups founded in 1983 or earlier do not contribute to employment growth beyond 1991. This lag structure is displayed in Figure 1.

[14] Recall that in the restricted regression columns in Table 7, the coefficients of the startup rate variables 1987-90, 1984-87, and 1980-83 are linear combinations of the coefficients of the combinatory variables COMBI1, COMBI2, and COMBI3. In other words, the bold-printed coefficients are *restricted* parameter estimates.

[15] The lag length in years is denoted as $i^*$. One unit in $i$ corresponds to a period of three years, i.e., $i = i^*/3$. Again, details are in Appendix 2.

Figure 1: Startup rates and employment growth: the lag structure




Graph of $b_{i^*} = 1.77\left(i^*/3\right) - 0.64\left(i^*/3\right)^2$, where $b_{i^*}$ represents the employment impact of the startup rate of lag $i^*$. The lag length $i^*$ is expressed in years.

It is, however, not a correct interpretation of Figure 1 to suggest that, for lags of 8.3 years and longer, the employment impact of new-firm startups is negative. Instead, the low t-value of the estimated coefficient of the startup rate 1980-83 in the first unrestricted regression column suggests the employment impact for longer lags does not significantly differ from zero. We can also formally derive this by testing the (additional) restriction that the coefficient of startup rate 1980-83 is zero. This extra restriction, which can be written as $b_3 = 0$, also implies that the employment impacts of 1987-90 startups and 1984-87 startups are equal.[16] The regression results with the extra restriction imposed are in the second restricted regression column in Table 7. The F-test of valid restrictions is again not rejected. We therefore conclude that the employment impact of 1980-83 startups is zero and that the employment impacts of 1987-90 startups and 1984-87 startups are equal and significantly positive.

[16] This is clear when the restriction $b_3 = 0$ is substituted in equation (A3) in Appendix 2: this results in $b_1 = b_2 = -2g_2$. Again, we refer to Appendix 2 for further details.

**(ix) Size of the effects**

We now examine the size of the effects. Because the LM approach estimations produce a better statistical fit than the BS approach (see section 6.v), we only present the former results. The coefficients from "separate regressions" overestimate the employment effect as these coefficients partly reflect the impact of new-firm startups from different periods, as was shown above. To establish the correct average impact of one new-firm startup, we use the coefficients in the second "restricted regression" columns. For example, for the results including non-private sector, Table 7 shows that the estimated parameter of the sector adjusted startup rate 1987-90 is 0.92. But this requires interpretation. The dependent variable equals $100\left(Empl_{1998} - Empl_{1991}\right) / Empl_{1991}$, where Empl stands for employment. The independent variable equals $1000 \sum_{i=1987}^{1990} NFF \Big/ \left(4\, Empl_{1987}\right)$, where NFF stands for new-firm formation.

Due to data limitations we use four times 1987-employment, instead of the sum of employment over the years 1987-1990. For simplicity we assume that employment in 1987 equals employment in 1991, so the impact of one new-firm startup on absolute employment change is (0.92×(1000/4))/100=2.3. So, ceteris paribus, one new firm started in the period 1987-90 on average created 2.3 net new jobs in the period 1991-98.[17] The employment impact of 1984-87 is also 2.3 jobs per startup. Note that these jobs are additional to the jobs created by the 1987-90 startups.[18]

**(x) Comparing these results with those from other studies**

Our findings for Great Britain show similarities to those of Audretsch and Fritsch (2002) for German regions. They find no short-term effect on employment of startups in the early to mid 1980s, but they do find a short-term employment effect of the early 1990s startups.

[17] It is important to realize that these 2.3 jobs do not necessarily have to be created in the new firms themselves. It is also possible that (part of) these jobs are created in incumbent firms, but that this is induced by competitive pressure from the new entries. In other words, the 2.3 jobs is the total net effect; we cannot distinguish between direct and indirect employment effects.

[18] Analogously, the coefficient 1.09 for the estimations excluding non-private sector would imply an employment impact of 2.7 net new jobs per startup, both for 1984-87 and for 1987-90 startups.

These are the same results as derived in this paper, prior to account being taken of public sector employment. However the public sector adjustments mean that, for Great Britain, there is also a short-term employment effect of the early to mid-1980s startups. The coefficients and the t-statistics in this paper are also generally higher than reported by Audretsch and Fritsch. The common finding, for both Britain and Germany, is that the short-term effect of new-firm startups is higher in the 1990s than in the 1980s.

Our results for the 1980s, however, differ from those of Ashcroft and Love (1996) for virtually the same British counties. As noted earlier, they find a strong positive effect of new firms started in the period 1980-88 on net employment change in the period 1981-89. They employ a model in which both employment change and new-firm formation are explained with only a one year lag, allowing for interdependencies between these two variables. The employment effect in their study is certainly stronger than our short-term result for the 1980s.

One possible explanation of the differences may again be the different lag structures employed in the two models. In their model Ashcroft and Love relate new-firm formation 1980-88 to net employment change 1981-89, whereas in this paper the lags are of a minimum of three years, taking the mid year of our startup rate variables as reference year.[19] Given the findings of this paper that the relationship strengthens over time (see Figure 1), we believe our results to be more robust.

## 7. Discussion and Implications

This paper has examined the relationship between new-firm startups and employment change in British regions between 1980 and 1998. Our central concern is, if such a relationship exists, whether it is strongest in the short or the longer term.

[19] Note that a lag of three years in the present paper is not comparable with the one year lag used by Ashcroft and Love. In their method, the one year lag is counted backward from the *end* year of the employment change period, whereas we count back from the *start* year of the employment change period. So the lags in the present paper are considerably larger than the difference between 3 and 1 year suggests. In fact, in Ashcroft and Love, the years in which employment change and startup activity are measured display an 80% overlap, possibly resulting in the reversed causality problems described earlier. In the present paper we deliberately choose non-overlapping periods.

We find evidence for a clear positive short-term effect of startup activity in the late 1980s and early 1990s on subsequent employment change, irrespective of macro-economic conditions (i.e., whether the new firms were founded during recession or boom periods). We also find a short-term employment effect for new businesses started in the early 1980s, but it is considerably smaller, and only identifiable once account is taken of public sector employment. Concerning long-run effects, we find that the employment impact of new-firm startups is strongest after about five years, while the effect has disappeared after a decade.

Two other key results emerge. The first is that the broad findings for Great Britain are similar to those for Germany, except that from the German data, Audretsch and Fritsch were unable to link early-1980s births to short-term employment change. Second, we find evidence for the Upas Tree effect, with new-firm births in Scotland and Wales having a smaller employment effect than new-firm births in England.

These findings will resonate clearly with public policy makers for several reasons. First, the considerably bigger short-term (and possibly long-term) employment impact of 1990s births, compared with 1980s births, is likely to reflect “Enterprise Policy” changes. As Greene (2002) argues, the 1980s in Britain was a decade in which the key objective was to maximise the number of business startups. In contrast, the 1990s saw a shift towards policies to improve the “quality” of the SME sector as a whole. Given that major policy shift it is unsurprising - although reassuring- to observe bigger employment impacts in the 1990s, than in the previous decade.

Nevertheless this paper makes it clear that increases in birth rates do lead to additional job creation in the short and medium term. Much less clear is whether a public policy-induced increase in birth rates is a cost-effective way of enhancing employment in the medium term. Indeed our interpretation of our findings is that it is not for two reasons. The first is that the only area, in the 1990s, with a clear (public) policy to promote new-firm births was Scotland. Yet it was Scotland, (along with Wales), where the job creation impact of a new startup was significantly lower than elsewhere [Fraser of Allander Institute (2001)].[20]

Secondly, the key finding is that startups had a much greater impact on job creation in the 1990s than in the 1980s, even though raising the startup rate was the key policy objective in the 1980s. Our interpretation is that "birth rate policies" lead to individuals with limited human capital -who are often unemployed- being encouraged to start in business. Such individuals are likely to be very transitory business owners and very unlikely to start and develop businesses with employees [Storey and Strange (1992)]. This suggests that, if the objective is to enhance employment, implementing old- fashioned "birth rate" policies is difficult to justify from this research.

[20] In 2002 Scottish Enterprise announced the effective abolition of its Business Birth Rate Strategy, replacing it with a greater focus on SMEs with potential for growth. However, in 2001, an Entrepreneurship Plan for Wales was announced with a £300 million budget, one key element of which was to raise birth rates of firms in Wales to the UK average by 2006 [National Assembly for Wales (2001)].

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## APPENDIX 1: Data sources

The various startup rate and employment change variables that are used in this report are all constructed from a data base which contains four basic variables: startups, closures, number of enterprises, and employment. This database was constructed by EIM. These four variables are available at the sectoral (1-digit) and regional (NUTS3) aggregation level for the period 1980-99. By and large, each of these four variables is available on a yearly basis according to uniform regional and sectoral classifications, for the whole period 1980-99. Achieving this uniformity is not straightforward, since the crude data were delivered according to different regional and sectoral classifications. In this appendix the exact regional and sectoral aggregation levels, at which the four variables are available in the EIM-data set, are presented. Furthermore, the data sources and some characteristics of the variables are described.

### Basic data

In Tables A1a and A1b, we give an overview of the different classifications (regional and sectoral), according to which the four variables are available in the basic data files. Also, the exact years for which the variables are available (for employment there are some missing years), are tabulated.

Table A1a: Available years and classification schemes in basic data files: startups, closures and number of enterprises [a]

| Period | Available years | Regional classification | Sectoral classification |
|---|---|---|---|
| 1980-1993 | All | pre-LGR [b] | VTC [c] |
| 1994-1999 | All | post-LGR | SIC92 |

[a] The figures of these variables are supplied by Small Business Service.
[b] LGR = local government reorganisation 1995-98.
[c] VTC = VAT Trade Classification. This is effectively SIC68.

Table A1b: Available years and classification schemes in basic data files: employment [a]

| Period | Available years | Regional classification | Sectoral classification |
|---|---|---|---|
| 1980-1991 | 1981; ’84; ‘87; ’89; ‘91 | pre-LGR [b] | SIC80 |
| 1991-1999 | 1991; ’93; ’95-‘98 | pre-LGR | SIC92 |

[a] The figures of this variable are supplied by Nomis.
[b] LGR = local government reorganisation 1995-98.

**Startups, closures and number of enterprises: source and description**

The figures on startups, closures, and number of enterprises are supplied by Small Business Service (SBS). This organisation publishes yearly figures on VAT registrations, VAT deregistrations, and the stock of VAT registered enterprises, based on data from the Inter-Departmental Business Register (IDBR; this register is administered by the Office for National Statistics). See SBS (2000). The VAT-registrations and VAT-deregistrations represent the number of enterprises registering and de-registering for VAT each year. Because there is a turnover threshold for VAT (£52,000 in 2000, for example), the very smallest one person businesses are excluded from the figures. The stock of VAT registered enterprises represents the number of enterprises registered for VAT at the start of the year.

**Employment: source and description**

The figures on employment are taken from the Census of Employment (until 1993) and the Annual Employment Survey (from 1995 onwards) and are supplied by Nomis. The employment figures only relate to employees. Self-employed workers and unpaid family workers are thus excluded from the data. The employment figures include both full-time and part-time employees, and relate to the situation in September of each year.

**Regional aggregation level and classification schemes**

The regional aggregation level employed in our data set is the British NUTS3 level. This is county level in England and Wales, and local authority region level in Scotland. We thus have data at the level of the 64 regions which are listed in Table 2 of Ashcroft, Love and Malloy (1991, p. 397). In the period 1995-98, a local government reorganisation took place in Great Britain. The five tier NUTS level classification was reviewed, and the so-called unitary authorities (UAs) were introduced. As a result, geographical boundaries of some regions have changed. This implies that we have to adjust the data from before and after the reorganisation so that they become comparable (see Table A1a). For the English regions, this is easy, since the data in the basic file are given in terms of both the new and the old regions ("former counties"). But for Wales and Scotland no variables for the period 1994-99 are given in terms of the old classification. Closer inspection of the boundaries of the unitary authorities reveals that the Scottish regions can remain unchanged but that some Welsh regions have to be aggregated into larger regions, due to overlapping "new" and "old" areas. In particular, the "old" counties Gwynedd, Clwyd, and Powys are combined into one region (which might be labeled North/Mid Wales), and the "old" counties Mid Glamorgan, South Glamorgan, and Gwent are also combined (South/East Wales). This implies that the total number of Welsh regions reduces from eight to four (Dyfed and West Glamorgan remain unchanged), and the total number of British regions in our data set from 64 to 60. These 60 regions comprise 46 English counties, 4 Welsh regions, and 10 Scottish local authority regions. In the latter group of regions, the Orkney, Shetland and Western Isles are combined into one region. The 60 regions cover the whole of Great Britain.

**Sectoral aggregation level and classification schemes**

At the regional aggregation level described above, the four variables are all available at the sectoral 1-digit level. However, from Tables A1a and A1b, we see that three different sectoral classifications circulate: SIC68, SIC80, and SIC92. These classifications are all different, see Table A2.

Table A2: Three Standard Industrial Classifications: 1-digit level labels [a]

| **SIC68** | **SIC80** | **SIC92** |
|---|---|---|
| agriculture, forestry and fishing | **0** agriculture, forestry and fishing | **AB** agriculture; forestry and fishing |
| production | **1** energy/water supply industries | **CE** mining and quarrying; electricity, gas and water supply |
| construction | **2** extraction/manufacture: minerals/metals | **D** manufacturing |
| motor trades | **3** metal goods/vehicle industries, etc | **F** construction |
| wholesale | **4** other manufacturing industries | **G** wholesale, retail and repairs |
| retail | **5** construction | **H** hotels and restaurants |
| catering | **6** distribution, hotels/catering; repairs | **I** transport, storage and communication |
| transport and communication | **7** transport/communication | **J** financial intermediation |
| finance and professional services | **8** banking, finance, insurance, leasing, etc | **K** real estate, renting and business activities |
| business and other personal services | **9** other services | **LO** public administration; other community, social and personal services |
| | | **MN** education; health and social work |

[a] In this table, similarities in covered parts of the economy across columns are coincidental.

As was the case for the regions, some sectors have to be combined to make sectors comparable across different SICs. This results in the six-sector classification in Table A3. In this table, corresponding parts of economic activity across SICs are in the same rows. *By and large*, there are no overlapping sectors in this six-sector classification. As mentioned earlier, we do not use the data for agriculture, forestry and fishing in our analyses.

Table A3: Relation SIC68-SIC80-SIC92 classifications (1-digit level)

| **SIC68-sectors** | **SIC80-sectors (codes)** | **SIC92-sectors (codes)** |
|---|---|---|
| agriculture, forestry and fishing | 0 | AB |
| production | 1, 2, 3, 4 | CDE |
| construction | 5 | F |
| trade and catering [a] | 6 | GH |
| transport and communication | 7 | I |
| other services [b] | 8, 9 | JKLMNO |

[a] This is an aggregate of four SIC68 sectors: motor trades; wholesale; retail; catering.
[b] This is an aggregate of two SIC68 sectors: finance and professional services; business and other personal services.

To summarize, the EIM-data set for Great Britain contains the four variables startups, closures, number of enterprises and employment. Apart from some missing years for employment, these variables are available on a yearly basis for the whole period 1980-1999, at relatively disaggregated sectoral and spatial aggregation levels (6 sectors, 60 regions), and according to uniform sectoral and regional classifications.

## APPENDIX 2: The Almon method [21]

The Almon method is a reparameterization method that corrects for correlation between different time lags of an exogenous variable (distributed lags). Correlation between exogenous variables in a regression model is not desirable as it causes multicollinearity. This problem is often prevalent in the context of distributed lags. When the distributed lag variables are highly correlated, it is difficult to estimate individual response coefficients accurately and regular t-tests on the significance of individual parameter estimates are unreliable. The Almon method assumes that there is some "smoothness" in the lag distribution. By imposing a specific structure in the lag distribution, the multicollinearity problems inherent to free estimation can be solved. In particular, the Almon method suggests approximating the lag structure by a polynomial function. This is explained below.

Suppose we have a model of the form represented by equation (A1).

$$Y_t = a + b_0 X_t + b_1 X_{t-1} + \ldots + b_s X_{t-s} + d Z + u_t \qquad \text{(A1)}$$

where the $X$ variables are the distributed lags, with maximum lag length $s$, and $Z$ is a vector of other exogenous variables (either lagged or unlagged). It is clear that in our model the distributed lag variables correspond to the startup rate variables from the various periods.

Due to high correlation between the $X$ variables with different lags, free estimation of (A1) suffers from multicollinearity. In the Almon method a "smooth" lag distribution is obtained by imposing restrictions on the parameter vector $b$. In particular, the Almon method suggests approximating the graph of $b_i$ against the lag length $i$ by a continuous function of the form

$$b_i = g_0 + g_1 i + g_2 i^2 + \ldots + g_r i^r ; r \le s \qquad \text{(A2)}$$

where $r$ is the degree of the polynomial (A2) and $s$ is the maximum lag length.

Imposing a structure like (A2) on the estimated parameters is implemented by estimating a restricted model. The restricted model is obtained by writing explicit expressions for (A2), and rearranging the distributed lag variables, as we will show below for our employment growth model. *First*, we establish the time periods that correspond to the lags 0, 1, ..., $s$. A straightforward application of our model suggests that lag 0 corresponds to the period 1991-1998, while the lags 1, 2, and 3 correspond to the periods 1987-1990, 1984-1987, and 1980-1983, respectively. So $s$ equals 3. Taking the mid years of these periods, i.e., 1988, 1985, and 1982, we see that in terms of equation (A2), the values $i$=1, 2, and 3 correspond to time lags of 3, 6, and 9 years, respectively, measured from 1991 backwards. In

[21] This appendix is based on Stewart (1991, pp. 180-182).

other words, one unit of $i$ corresponds to a lag length of three years. *Second*, we have not included a startup rate with lag 0 in our model, so $b_0$=0. This restriction reflects our argument that startup rates do not have an immediate (i.e., contemporaneous) effect on growth and inclusion of an unlagged startup rate in the model leads to problems of reversed causality. *Third*, we choose $r$=2, i.e., a quadratic polynomial form.[22] Writing out (A2) with $r$=2, $s$=3, and $b_0$=0 results in

$$b_0 = g_0 \equiv 0; \quad b_1 = g_1 + g_2; \quad b_2 = 2g_1 + 4g_2; \quad b_3 = 3g_1 + 9g_2. \qquad \text{(A3)}$$

Substituting (A3) in (A1) and rearranging terms results in

$$Y_t = a + g_1 (X_{t-1} + 2X_{t-2} + 3X_{t-3}) + g_2 (X_{t-1} + 4X_{t-2} + 9X_{t-3}) + d\,Z + u_t \qquad \text{(A4)}$$

Equation (A4) can be estimated using OLS. The (restricted) parameters of the startup rate variables are obtained by substituting the estimates of $g_1$ and $g_2$ back into equation (A3). The corresponding standard errors are obtained using the ANALYZ command in TSP 4.5.

To test the validity of the parameter restrictions imposed by the Almon method a standard F-test of the form

$$F = [(S_R - S)/(s-r)]/[S/(n-k)] \qquad \text{(A5)}$$

can be applied, where $S_R$ and $S$ are the restricted and unrestricted residual sum of squares, respectively, $r$ is the degree of the polynomial (A2), $s$ is the maximum lag length in equation (A1), $n$ is the number of observations, and $k$ is the number of regressors in the unrestricted model. Under the null hypothesis of valid restrictions, the test statistic under (A5) has an $F$ distribution with $s-r$ and $n-k$ degrees of freedom.

In our first application, the number of restrictions $s-r$ equals 3-2=1, while the expression $n-k$ equals 59-7=52. The critical value of the F(1;52) distribution at 5% level is 4.04. From Table 7 we see that the values of the test statistics equal 0.024 and 1.531 for the two cases, so the null hypothesis of valid restrictions is not rejected.

In our second application, where we put the employment impact of 1980-83 startups on employment growth 1991-98 equal to zero, the number of restrictions equals two. The extra restriction can be written as $b_3 = 0$. Substitution in equation (A3) results in $g_1 = -3g_2$; $b_1 = b_2 = -2g_2$. So, the extra restriction also implies that the employment impacts of lags 1 and 2 (startups 1987-90 and 1984-87) are equal. Another implication is that the optimum lag is 1.5 (or 4.5 years). In this case the F-test statistic has an F(2;52) distribution (critical value 3.19). The test statistics equal 0.100 and 0.891. So, the restriction $b_3 = 0$ is valid.

[22] We consider a first degree polynomial (i.e., a straight line) too restrictive.