Regulating Retail to Town Centres: The price paid in productivity

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Abstract

We exploit a unique dataset from a major UK clothing retailer to investigate the impact of 'Town Centre First' (TCF) – a policy that regulates retail location and competition and that tightened sharply in England and Wales in 1996 – on productivity. Using store-level panel data on labour, land and capital inputs and exploiting differential changes in the policy across UK countries, we find that the tightened land use regulation had significant adverse effects on total factor productivity and local competition. The effects on productivity are quantitatively meaningful: Our most rigorous point estimate suggests that TCF may have reduced clothing retail productivity among stores in England and Wales that started up between 1996 and 1998 by an accumulated 47% by 2010.

JEL classification: D2, L51, L81, R32, R33.

Key words: planning, policy evaluation, productivity, retail.

1 Introduction

In 1996 England introduced a strict new policy to strongly redirect retail and other traditional town centre uses to 'town centres' – as defined by planners. This policy has come to be known as 'Town Centre First' (TCF). In this paper we explore the impact this radical policy change has had on overall total factor productivity (TFP) in the clothing retail sector.

One would anticipate that land (or space) would be an important input in the production function of a supermarket chain or a retailer of bulky goods such as furniture and one might therefore expect that forcing such stores onto more central and smaller sites would have substantial adverse effects on output and productivity. Land is a less obviously important input for retailers of comparison goods, such as clothing or jewellery, so it is less obvious that TCF adversely affects retailers in these sectors. Previous empirical research has little to say about whether and how important these detrimental effects may be in the comparison goods sector. On the one hand, one might expect that store size may be less crucial for these stores. On the other hand, their micro-location may be even more crucial from a productivity point of view.

In order to answer the question whether the policy intervention in 1996 has adversely affected retailers of comparison goods, we exploit a unique panel dataset from a major UK clothing retailer, who wishes to remain anonymous. Specifically, we have access to very detailed store-level information for 385 stores located across the UK and the Republic of Ireland on an annual basis between 2007 and 2010. Using this store-level panel data and exploiting differential changes in the policy across UK countries we find that TCF has very substantially reduced clothing retail productivity in England (and Wales, which closely followed England's policy): The point estimates of our most rigorous specifications indicates that the 1996 policy intervention may have reduced clothing retail productivity among stores in England and Wales that started up between 1996 and 1998 by an accumulated 47% until 2010. Our results also suggest that TCF has reduced store size and use of labour inputs per store, which is consistent with the policy forcing retailers to central locations where commercial properties are typically smaller compared to out-of-town locations. Finally, the results suggest that stores that started up under TCF policy faced fewer local competitors in their own sector.

Our study makes two important contributions to the existing literature. Firstly, our findings provide evidence that the introduction of the strict TCF policy in England and Wales in 1996 adversely affected productivity in comparison goods retailing, in which tightened planning regulation might have been expected to be less harmful than in other retail industries, such as groceries. This adds to the existing evidence of the detrimental impact of TCF policy on retail productivity and is in line with the view that tighter land use regulation is likely to adversely affect the whole retail sector. This finding implies that while space has a significant role in the productivity of the retail sector, the loss in productivity may be to a good extent driven by the fact that in England and Wales, since around 1996, store locations are essentially determined by planners rather than by profit-maximising retailers. In other words not just

space but its micro-location matters for retail productivity and this may be particularly true for retailers of comparison goods.

Our second main contribution relates to the fact that our panel dataset has a number of features that make it particularly suitable for exploring the link between planning induced constraints and retail productivity: (i) it comes from one single company and thus avoids many problems that arise when comparing establishments of different companies (see Griliches and Mairesse, 1995, for example); (ii) the panel feature of the data allows us to employ store fixed effects that capture all unobserved time-invariant characteristics unique to the store (our findings suggest that controlling for store fixed effects is important); (iii) the dataset not only provides precise details on the sales figures of the various stores but also includes direct measures of labour, space and capital inputs; (iv) we know the precise location (i.e., the street address) of all the stores in the sample. This allows us to geographically match our primary dataset with other spatial information. Specifically, the location information of the stores allows us to identify the number of local competitors at various distance bands from the stores. This in turn allows us to explore whether stores that opened up right before 1996 and could choose their location more or less freely, face a similar degree of local competition compared to stores that opened up in the aftermath of the policy change and were severely constrained in the choice of their location.

Estimating the impact of the strict TCF policy on store-level productivity is challenging for a number of reasons. First, the policy mainly affected the location and size of entering establishments, and, as a result, *stores that had entered before the policy was introduced* were unlikely to be directly affected by it. This implies that a standard Differences-In-Differences approach comparing before-after changes in outcomes between existing stores that were located in the area where regulation was tightened and existing stores that were located in an area unaffected by the policy is unlikely to yield good estimates of the treatment effect. In order to account for this issue, we compare stores that started up before and after the introduction of the strict TCF policy between the countries where the policy was tightened (England and Wales) and the countries where regulation was essentially unchanged (Scotland, Northern Ireland and the Republic of Ireland). In other words, we examine the difference in performance between store cohorts that started up before and after the TCF policy was introduced in England and Wales and as a counterfactual for this change we use the corresponding difference in Scotland, Northern Ireland, and the Republic of Ireland where regulation did not change notably or did not change at all in 1996. Second, it may take some time before a new store achieves its productivity potential and thus comparisons between incumbent and entering companies are unlikely to yield good estimates. However, our branch level input and output data covers the years 2007 to 2010, which facilitates the recovering of estimates for productivity long after the introduction of the policy change.

Another concern is that stores that opened up say in the 1960s and in the 2000s are quite different from each other. A store built in 1960 may be more likely to be on an inherently more productive site than a store that opened up in 2010 since in 2010 the inherently most productive sites may no longer be available. To address this particular concern, in order to estimate the effect of the treatment, we choose quite narrow time windows around the

treatment year of 1996 to select start-up stores for our regression sample. Interestingly, choosing a narrow window strengthens our adverse findings, both in terms of statistical significance and quantitative magnitude. Finally, we discuss the concern that our results may be driven by a survival bias. A survival bias may exist if inherently more productive stores are more likely to survive, while less productive stores die. Hence, older stores in our sample may be more productive. We discuss this concern but conclude that, under reasonable assumptions about the regulator's behaviour, in our particular empirical setting, any such survival bias will likely bias against finding an adverse effect of TCF on productivity.

Our study ties into an existing literature that explores the enduring puzzle of the differential productivity growth in the US versus that in Europe and the UK. The poor performance of European economies in general and that of the UK in particular has been a focus of attention for at least two decades. Over-regulation has been a frequently cited cause. This suspicion of over-regulation has mainly been aimed at labour markets and environmental standards, however. Few commentators have suspected land use regulation. Yet there is mounting evidence that land use regulation at least in the UK is an important source of costs to firms. Cheshire and Hilber (2008) already showed that the costs of office space in the UK were typically subject to an equivalent of a tax on marginal construction costs of 400 to 800 percent in the most prosperous cities and even in a depressed and declining context such as Birmingham estimated this 'regulatory tax at 250 over the period 1999-2005. The McKinsey Global Institute (1998) report was the first to raise the suspicion in the context of the retail sector but their analysis was based on circumstantial evidence. Bertrand and Kramarz (2002 produced rigorous evidence that in France regulatory barriers to entry reduced retail employment growth in those areas where they were most stringently applied. Schivardi and Viviano (2011) demonstrate for the Italian retail trade sector that in areas where retail entry regulation is more stringent, lower productivity coupled with larger margins results in higher consumer prices.

The productivity growth divide between the US and the UK is particularly stark in the retail sector. In a seminal paper, Basu *et al.* (2003) pointed out that between the first and second halves of the 1990s, US retail value-added TFP growth *rose* by 4.5 percentage points per year, whereas, during the same time period, UK retail TFP growth *fell* by 1.9 percentage points per year – a very substantive gap in productivity growth of an annualized 6.4%.

Griffith and Harmgart (2005) point out that planning in the UK may affect productivity in a number of ways and may possibly be partly responsible for the emerging productivity gap in the retail sector between the US and the UK. They suggest that to the extent that retail productivity growth is generated by firms closing older stores that are less productive and opening newer stores that are more productive to replace them, regulation, by hindering the opening of new stores and closure of old ones may lower productivity growth. Planning regulation may also result in retail stores operating below minimum efficient scale (i.e., small format stores), thus lowering productivity levels. On the other hand, restricting entry in the grocery retail sector may increase product prices and therefore sales and measured productivity. Griffith and Harmgart (2008) evaluate the impact of restrictive planning regulation on entry into the UK grocery industry by estimating a model with multiple store

formats. They find that (i) more restrictive planning regulation reduces the number of large format supermarkets in equilibrium but (ii) this impact is overstated if variation in demographic characteristics across markets is not properly controlled for.

Haskel and Sadun (2012) document a shift to smaller shops following the 1996 regulatory change that increased the cost of opening large stores. Their analysis emphasizes the link between regulation and store size and suggests that the effect of the policy via reducing store size in the retail sector was associated with a loss in total factor productivity (TFP) of 0.4 percent per annum, or 40 percent of the post-1995 slowdown in UK retail TFP growth.

Cheshire *et al.* (2012) document that the TCF policy was introduced much earlier and more restrictively in England compared to Scotland and Northern Ireland. They exploit cross-sectional data for a major UK supermarket chain and estimate that since the late 1980s planning policies in England have imposed a loss of output of at least 20 percent – more than a "lost decade's" growth. They argue that the TCF policy and local planning constraints reduced output, both by forcing stores onto less productive sites and reducing store sizes.

Our paper is structured as follows. In Section 2 we review the main features of England's 'Town Centre First' policy and briefly discuss how other UK countries and the Republic of Ireland differ in timing and restrictiveness. In Section 3 we describe the data. Section 4 discusses the econometric framework and presents results. Section 5 concludes.

2 The treatment: strict 'Town Centre First' policy

The construction of the motorway (highway) system from about 1960 as well as growth of car ownership and use[1] associated with residential decentralisation created strong forces on the British Isles favouring the development of out-of-town, large format supermarkets and shopping centres from the 1960s. However policy responses to these market forces differed starkly between the different countries of the British Isles: England, Wales, Scotland, Northern Ireland and Republic of Ireland.[2] Whereas two countries (England and Wales) responded by eventually introducing strict 'Town Centre First' policies redirecting retail stores to town centres, the remaining countries kept a planning system in place that was broadly neutral or in favour of edge- and out-of-town development (at least until recently).

2.1 The treatment group: England and Wales

The idea of 'Town Centre First' (TCF) took hold in English policy in the late 1980s as a response to the market forces described above. Policy which had previously accepted the commercial logic of out-of-town retail changed in 1988 to direct new out-of-town retail development to Brownfield or 'regeneration sites'. However, it was only in 1996 when a *strict* TCF policy was introduced through the so called PPG6 (PPG stands for Planning Policy Guidance, replaced with Planning Policy Statements, now abolished and incorporated in the National Planning Policy Framework) (Department of the Environment, 1996). PPG6

[1] Total car miles increased by 39 percent from 1970 to 1980; by 56 percent during the 1980s; 12 percent during the 1990s and a further 7 percent from 2000 to 2008 (Department for Transport, 2012).
[2] The constituent countries of the United Kingdom are England, Wales, Scotland and Northern Ireland.

crucially, brought in both the *'needs test'* and the *'sequential test'* and dropped any mention of 'avoiding unnecessary regulation'.

The *needs test* required the potential developer to demonstrate, according to prescribed formulae, that the community 'needed' more shopping space and that their proposed development would not undermine the viability of other local shopping facilities. It can be argued this erected a barrier to entry into local markets and can be expected to reduce local competition.

The *sequential test* was designed to rule out all possible sites before allowing an out-of-town site even to be considered. A potential developer had to show that suitable sites in 'town centres' were not available and, subsequently, that sites in a 'district centre' or 'neighbourhood centre' were also not available before proposing to develop an edge-of or out-of town site. A site was only defined as 'suitable' if it was identified for retail use in the local plan. The fact that such a site might be owned by a rival retailer did not render it 'unsuitable'. As ODPM (2004) stated: "PPG6… (was) increasingly used by LPAs as a development control tool to prevent out-of-centre development, instead of as a basis for positive planning for town centres. It became all but impossible to develop large-format out-of-town stores in England."

Summing up; policy towards out-of-town retail development in England gradually tightened from 1988 with the radical change in policy coming in 1996. This strongly redirected retail to 'town centres', as defined by planners. Far from attempting to avoid 'unnecessary regulation' as previous policies had done, it put the emphasis firmly on 'town centre first'.

Policy in Wales followed changes in England closely, except that guidance gave more emphasis to the aim of a competitive retail sector and enforcement at the local level appears to have been rather more flexible. The nature of the Welsh policy is such that we concluded that stores located in Wales arguably ought to be part of the treatment group – but, as we demonstrate below, all our results are very similar if we drop Wales from the treatment group.

2.2 The control group: Scotland, Northern Ireland and Republic of Ireland

Scotland introduced a form of TCF policy in 1996. However, it was significantly weaker than that in England. There was an explicit aim of maintaining a 'competitive and innovative retail sector' and a statement that it was not the role of planning to 'protect existing interests or restrain competition' but did steer local planners to favour town centre locations for new retail by introducing a form of the 'sequential' test. A revised policy in 1998 gave more emphasis to directing retail development to town centres but the guidance continued to instruct planners to assist in maintaining 'an efficient, competitive and innovative retail sector offering consumer choice'; the 'sequential' test was maintained but the 'needs' test was not introduced. While the policy was largely non-constraining until 2005, in 2006 it became slightly more restrictive towards development of out-of-town retail while remaining significantly more flexible than that in England. There was no 'needs' test introduced and out-of-town development was permitted when there was access by public transport. Note that in our estimates of treatment effects, the largest (least conservative) time window selects only

stores that opened up between 1987 and 1998, arguably a time period before policy in Scotland was seriously redirecting retail into town centres.

Northern Ireland also introduced a form of TCF policy in 1996. However, this remained significantly more flexible than in England. There was emphasis given to new developments not leading to a significant loss of investment in existing centres and accessibility by transport other than cars but the policy was much less restrictive even than in Scotland.

Finally, the Republic of Ireland never introduced any form of TCF policy. The need for a 'National Spatial Strategy' (NSS) was only recognised by the Irish Government with the publication of the '2002-2006 National Development Plan (NDP). The NSS and NDP are almost completely relaxed about where retail should locate. It is much more concerned with assisting growth and diverting it away from Dublin. There is one statement about the need for retail locations to be accessible for public transport and a general statement about the need for development to be 'sustainable'. However, overall land use planning with respect to retail appears much more relaxed than that in any other country of the UK. (See: http://www.irishspatialstrategy.ie/, last accessed on 30/10/2012, for details.)

Cheshire *et al.* (2012) provide a more in depth discussion of the history of the British planning system and the differences in timing and rigidity of the various policies in England, Wales, Scotland and Northern Ireland. We refer the interested reader to this study for details.

2.3 Some stylized facts about the impact of the *strict* TCF policy in England

The policy change in England sharply affected the volume of applications for major new retail developments. These had more than doubled from the bottom of the economic cycle in 1983 to its peak in 1988 and by 1992 had begun to recover from the 1990 recession. Following the introduction of the full blown TCF policy in 1996, however, development applications fell sharply despite the continuing economic recovery, so that even by 2002 the volume of applications was little greater than in 1983. Since the revised PPG6 of 1996 applied only to new developments, however, applications for store extensions boomed. The Competition Commission (2000) reports – based on its sample of LPAs – that in 1997 and 1998 there was nearly a fivefold increase in applications for food store extensions compared to the preceding five year period. The sample of LPAs surveyed in ODPM (2004) shows an increase from zero extension-applications per LPA in 1994 to 10 in 1998. There must, therefore, be a presumption this favoured incumbents by restricting entry. At the same time the strategic policy of major store groups was revised. Tesco and Sainsbury in particular developed smaller, in town, formats: in 1994 some 25 percent of Tesco's new openings were in town but by 2000 all new openings were defined as 'in town'; Sainsbury went from some 12 percent 'in town' in 1995 to 85 percent in 1999.

A further point is that the sharp reduction in store development has come to be reflected in an older stock of buildings in the retail sector than in any other economic sector. As Barker (2006) shows, an astonishing 90 percent of retail space dates from 1980 or before: this compares to some 75 percent of office space or 70 percent of warehouse space. Older buildings tend to be less productive and also less energy efficient.

3 Data

Our primary data consists of individual store-level information for 385 stores – the full sample of stores of our major UK clothing retailer – and for the years (2007 to 2010). Two (clearance) stores are dropped from the regression sample due to lack of information on the number of tills. Another 35 store-year observations are dropped because the store opened up in 2007 and had incomplete annual data for that year (we do use the data from 2008 to 2010 for these stores). Hence, our regression sample consists of 1497 store-year observations, derived from 383 stores, and we proceed to use these in our empirical analysis described in the next section.

Variables for each store include: sales, labour hours, floor space, number of tills (as a proxy measure of capital), the year the store opened and the precise store location and type of store. The store type is classified into 10 categories: 'out of town', 'covered shopping centre', 'regional shopping centre', 'uncovered shopping centre', 'high street', 'clearance high street', 'clearance outlet', 'clearance retail park', 'staff shop' and 'home standalone store'. Summary statistics for all variables are presented in Table 1. We aggregate the 10 store type-categories into four overarching ones and report the share of each of these overarching categories. We do this so as not to potentially infringe the identity of our data provider. We note however, that in our regressions, we include one dummy variable for each of the 10 categories.

We augment the primary data by geographically matching the 383 stores of the regression sample to their closest competitors in 2011. Our competition variable 'number of competitors within distance band 0 to x km' was constructed as follows. First we identified the 8 closest competitors (in terms of market segment covered) to the company who gave us access to their data. We identified these competitors based on conversations we had with our data provider. (We cannot list the names of these competitors as this would infringe upon the anonymity of our data provider.) For each of our 383 stores we then geocoded the four nearest stores for each competitor by postcode and calculated straight line distances between our store's and the competitor's postcode centroids. Using these distances we calculated the number of competitors within certain distance bands.

4 Empirical analysis

4.1 Econometric framework

Retail production function

We consider a retail unit ('a store') *i* that uses labour L_{it}, floor space F_{it} and working capital K_{it} to produce output y_{it}. Assuming Cobb-Douglas technology, a standard log-linear specification of the production function is

$$\ln y_{it} = \beta \ln L_{it} + \gamma \ln F_{it} + \theta \ln K_{it} + \varepsilon_{it} \quad (1)$$

where y_{it} is sales; labour input is measured as hours worked; floor space is measured as gross acreage of the store; and working capital is approximated by the number of tills in a store.

Identification of the parameters in equation (1) are subject to numerous difficulties (e.g., Griliches and Mairesse 1995). Omitted variables correlated with inputs and output would confound estimates of input elasticities. Another potential source of bias is the simultaneity of output and input decisions: A shock to productivity is likely to alter the optimal input mix.

We rely on a panel estimators used in numerous other studies (e.g. Moretti 2004, Haskel and Sadun 2012) by including store fixed effects, α_i, in the model:

$$\ln y_{it} = \beta \ln L_{it} + \gamma \ln F_{it} + \theta \ln K_{it} + \alpha_i + u_{it} \tag{2}$$

This approach is robust to differences in time-invariant attributes across stores. Estimation of equation (2) is complicated by the fact that the number of tills did not change in any of the stores during the sample period and floor space only changed very marginally for a handful of stores but also was essentially unchanged during the sample period. Thus, identification of γ and θ can be based only on cross-sectional variation. In order to account for this *and*, at the same time, fully exploit within-store variation in labour, we take a two-step approach. The first step estimates an equation omitting time-invariant F_{it} and K_{it}:

$$\ln y_{it} = \beta \ln L_{it} + d_i + v_{it} \tag{3}$$

Here d_i can be interpreted as the average store-specific composite productivity of labour. This equation recovers elasticity estimates on labour. The second step estimates

$$\hat{d}_i = \gamma \ln F_{it} + \theta \ln K_{it} + \tilde{u}_{it} \tag{4}$$

where d_i is an estimate of d_i. Estimating equation (4) using cross-sectional variation recovers unbiased estimates of γ and θ provided that floor space F_{it} and working capital K_{it} are uncorrelated with the fixed effect α_i (e.g., Hausman and Taylor 1981). Based on the input elasticities recovered by estimating equations (3) and (4), we calculate *total factor productivity* as $\ln y_{it} - \ln\left(f\left(L_{it}, F_{it}, K_{it}\right)\right)$.

Identification of net impact of TCF on TFP

In order to identify the net impact of the tightened land use regulation on store performance, we exploit differences in the timing of the introduction of tighter restrictions on land use across UK countries. As explained in section 2, planning policies tightened in England and Wales considerably in 1996, while Scotland, Northern Ireland, and the Republic of Ireland introduced more binding policies only later or, in the case of the Republic of Ireland, not at all. This allows us to use the performance of stores in these three countries where the regulation was not markedly increased as a counterfactual for English and Welsh stores that were affected by the strict tightening in 1996. More specifically, we examine the difference in

the average productivity (and other outcome measures: the various inputs and the number of local competitors) between stores that started up right before and after the introduction of the TCF policy in England and Wales and use the corresponding average difference for stores in the unaffected countries as a counterfactual by estimating the following equation:

$$z_{i,2010} = \alpha + \delta_1 EW_i + \delta_2 I(c_i > 1995)_i + \tau EW_i \times I(c_i > 1995)_i + \eta_i \quad (5)$$

where $z_{i,2010}$ is the outcome of interest for store *i* in 2010 (or 2011 for the variable local competitors); EW_i is a binary indicator equal to one if a store is located in England or Wales; $I(c_i > 1995)$ is a binary indicator equal to one if the entry year c_i is later than 1995 and zero otherwise; η_i is an error term. Here δ_2 is the average difference in $z_{i,2010}$ between stores that have entered by and after 1995, while τ is the treatment effect that tells us how much larger (or smaller) this difference is in England and Wales relative to the unaffected countries.

One concern with this approach is that stores that opened up at different time periods may be quite different from each other in terms of their inherent productivity. For example, it could be that stores that were built later in time could be built on inherently less productive sites as the more productive sites were no longer available. Moreover, this effect could be more or less pronounced in the treatment group versus the control group, for example because more open land is available in Scotland, Northern Ireland and the Republic of Ireland compared to England. So if we chose a very large pre- and post-treatment time window the concern is that our estimates may be biased by some confounding factors that were different for quite old and quite young stores and these factors may have differed between treatment and control group. Therefore we only select stores that started up within a relatively narrow time window around the treatment year of 1996. Our most conservative (baseline) approach is to use only 3 years pre- and post-treatment (1993-1995 plus 1996-1998). The downside of choosing such a narrow window is that our sample size of stores becomes very small and coefficients can therefore be less precisely estimated. We therefore also report results for two somewhat larger time windows: 1990 to 1998 and 1987 to 1998.

A related concern is that new stores are technologically more advanced compared to older stores and therefore inherently more productive. However, we note that in our cohort-based approach trends in technology are differenced out assuming the same technologies were adapted across countries. As our data is from a one fixed format clothing retailer, same technologies were available for all stores as a result of which asymmetric technology adaption is unlikely to be a major source of bias. Furthermore, our results are robust to allowing for different time trends between the treatment and control groups which adds credibility to the assumption that potential differences in adopted technologies across countries are not driving the results.

A last concern relates to potential 'survival bias'. In order to identify the likely direction of such a bias we need to explore the implications of different survival probabilities for different types of stores prior to and after the introduction of strict TCF policy. The focus of our cohort-based approach, in the most rigorous specification, is on stores that started up between 1993 and 1998. We can assume that *all* stores that opened up between 1993 and 1995 did

survive until the beginning of 1996. This is because (i) stores are typically given at least 3 years to get off the ground and (ii) the mid-1990s was an economic recovery period on the British Isles and hence stores can generally be expected to have had a low exit probability (exits occur mainly during economic downturns). This implies that selection based on the free market mechanism was unlikely to have had a noticeable impact for stores that opened up during the pre-reform period between 1993 and 1995. Importantly, this argumentation applies equally to the treatment and control group. Hence, to the extent that there was selection of more productive start up stores during this short time period, despite (i) and (ii), we would expect the survival probabilities to be the same in the treatment and control group, so they should be differenced out.

What is the likely impact of the policy reform on the survival probability of the stores that opened up between 1993 and 1996? One obvious effect of the policy change is that it introduced barriers to entry through the 'needs test'. As a result, less productive stores can be expected to survive compared to the free market case since the productivity threshold for survival can be expected to be lower in the regulated case. We would expect that this threshold is lower since the 'sequential test' rules out the most productive sites (i.e., potential store entries at the top of the productivity distribution determined by the site location and size). If we then plausibly assume that planners select new entrants in our sample independent of their inherent productivity (i.e., randomly)[3], this effect is *not* differenced out and introduces a downward bias. That is, we underestimate the adverse effect of the strict TCF policy since less productive stores that opened up prior to the policy change are more likely to survive, hence lowering any estimate of a productivity loss post reform.

4.2 Parameter estimates

The main purpose of our empirical analysis is to explore the impact of the introduction of strict TCF policy on various store-level specific measures: total factor productivity, labour-, space- and capital-inputs and number of local competitors. In order to derive *total factor productivity*, we must first estimate production function parameters as outlined above, using the data described in Section 3.

Main results: production function estimates

Table 2 displays the production function parameters and standard errors for six different specifications. Columns 1 to 3 show estimates for three different specifications that do not include store-level fixed effects (equation 1), so these panel estimates are cross-sectional in

[3] The assumption that the selection of regulators is random is plausible for two reasons. First, policy implementation in all countries in our sample is the responsibility of local jurisdictions (Local Planning Authorities, LPAs, in England). In all countries of our sample planning policies are implemented by means of 'development control' – that is, each proposed development is considered individually by the LPA and is then either permitted or refused (in contrast to systems in force in the USA or continental Europe where what plans permit can be built). There is then a process of appeal against local decisions. Importantly, inherent productivity advantages of the retailer are not relevant for the selection decision. Second, our sample only consists of stores of one single company. Hence, even if local planners (the selectors) were favouring certain retailers over others for whatever reason, this would not be relevant in our case.

nature. Since we use a panel of stores and our dependent and some of our independent variables vary over time by store we cluster standard errors by store.

Column 1 displays estimates of a specification that includes only the three inputs – labour, floor space and tills – plus a limited set of controls. Our output measure 'sales' as well as the three input variables are all denoted in logs and hence we can interpret the coefficients on the inputs directly as elasticities. The list of control variables includes year dummies, dummy variables for each of the ten store types (listed in the data section) as well as the age and age squared of the store. Whereas the year dummies capture macro-economic shocks that affected all countries of the British Isles, the type dummies control for all time-invariant characteristics unique to the store type. We include the age and age squared of the store to capture the fact that store specific productivity tends to first increase (during the learning phase) and then decrease as the store gets older and store-specific technology tends to get outdated. The coefficient on labour is 1.302 and statistically highly significant, while the coefficients on floor space and tills are both negative and significant.

Column 2 adds Travel to Work Area (TTWA) fixed effects and column 3 furthermore includes TTWA × year fixed effects to account for time-invariant and, respectively, time-variant labour market specific factors that may affect store-level productivity. These fixed effects may also capture variation in demographic characteristics across markets, which Griffith and Harmgart (2008) have pointed out may influence the number of competitors and productivity. The coefficients and significance levels on the three input variables remain virtually unchanged when these various fixed effects are included. The negative estimates of two of the three input elasticities strongly suggest that estimates based on cross-sectional variation are driven by unobservable attributes. Furthermore, because estimates do not change noticeably when area fixed effects are included in the model, it is likely that these unobserved attributes work at the store level.

Columns 4 to 6 present panel estimates that control for store fixed-effects (equation 2), thereby accounting for unobserved time-invariant attributes correlated with the output and the inputs. As discussed above, our measure of working capital (number of tills) does not vary at all over the sample period and our measure of land input (floor space) only varies very marginally for a handful of stores. Thus we cannot recover a coefficient on these input variables in the fixed effects model. However, provided that store fixed effects account for land and working capital, the coefficient on labour (estimated as in equation 3) is not affected if floor space and/or number of tills are excluded from the estimating equation.

In order to recover the coefficients on the remaining two inputs, we estimate equation (4) where we regress store fixed effects on floor space (measured in 2010) and number of tills (this two-step approach is discussed in detail in section 4.1). Similar to column 1, in column 4 we only include year dummies, type of store dummies and age of store and age of store squared as additional controls. In columns 5 and 6 we add TTWA dummies and TTWA × year fixed effects, respectively. Time-variant continuous variables and dummy variables which can be identified separately from store fixed effects are included in the 1st stage regressions. The remaining control variables and dummy variables are included in the 2nd stage regressions. The outcome in the 2nd stage regressions is the estimated store FE from the

corresponding 1st stage regression. The 1st stage regressions in columns 4 and 5 control for store fixed effects, year dummies and age squared. The 1st stage specification in column 6 includes TTWA-year interaction dummies. 2nd stage regressions in columns 4 and 6 control for year dummies and age of store, which is not included in the 1st stage regression as its coefficient cannot be separately identified when year and store FEs are included. The 2nd stage specification in column 5 includes TTWA dummies. Standard errors in columns (4) to (6) are clustered by store in the 1st stage and by TTWA in the 2nd stage.

The parameter estimates of the three input variables are very similar across the three specifications displayed in columns 4 to 6 and we therefore only discuss the estimates of the most rigorous specification reported in column 6. (We subsequently proceed to use the estimate of total factor productivity derived from the parameter estimates reported in column 6 to explore the impact of the strict TCF policy.) Similar to the cross-sectional estimates, the first-stage coefficient on labour is positive and highly statistically significant, however, unlike in the cross-sectional estimates, the elasticity is now below 1, with 0.837. Moreover, the second-stage coefficient on floor space is now positive (0.170) and highly statistically significant, consistent with our conjecture that the cross-sectional estimates reported in columns 1 to 3 are hampered by store-level specific unobserved attributes. The coefficient on number of tills is now also positive (0.034) albeit insignificant. The sum of the three input elasticities (1.04) implies roughly constant returns to scale.

Main results: identifying impact of treatment on TFP and other outcome measures

In order to identify the net impact of the strict TCF policy, we next examine whether our outcome variables of interest – the estimated total factor productivity (as defined in Section 4.1 and backed out from the parameter estimates shown in column 6), the use of the three inputs labour, land and capital, and the number of local competitors – decreased for English and Welsh stores that opened up after 1996 compared to those that opened up just before 1996. To do this we estimate equation (5) using our cohort based approach by comparing English and Welsh stores that opened up during a narrow time window prior to and after the policy intervention in 1996 with stores that started up in Scotland, Northern Ireland or the Republic of Ireland (our control group) during the same time period, both before and after the policy intervention.

Table 3 displays parameter estimates of the treatment effect (τ in equation 5) for the various output measures of interest (TFP, input variables, number of local competitors) and for three different time windows. Columns 1 to 3 show results for a sample that includes stores that started up during the years 1993 to 1998 (i.e., 3 years before and after the policy was tightened sharply in England and Wales). These are our most rigorous specifications since we define the time window very narrowly around the treatment year. One drawback of this approach is that the sample size becomes quite small and coefficients cannot be as precisely estimated. We therefore also report specifications for wider time windows. Columns 4 to 6 display results for a sample of stores that started up between 1990 and 1998. Columns 7 to 9 finally present results for the widest time window: stores that started up between 1987 and 1998. We do not extend the time window beyond 1998 as, arguably, TCF policies have become tighter in some of our control countries after that year. For each of the three time

windows we report parameter estimates for three different specifications. The baseline specification (columns 1, 4 and 7) only includes the variables outlined in equation (5). The second specification (columns 2, 5 and 8) adds a linear time trend and a linear time trend interacted with a dummy that equals one if the start-up store is located in England or Wales. These additional controls capture different trends between the treatment and control group during the relevant time window. The third specification (columns 3, 6 and 9) additionally includes country fixed effects to capture time invariant characteristics that are unique to the country and not just to the treatment- and control-group.

For each point estimate we report three different measures to gauge statistical significance: classic standard errors, standard errors clustered at country level and bootstrap p-values based on a wild bootstrap of t-statistics and 500 bootstrap replications. Classic standard errors are problematic given that our treatment effect is country specific. Clustering at country level would in principle be the appropriate correction, yet, with only 5 countries we have far too few clusters for the standard cluster adjustment to be reliable (Angrist and Pischke, 2009, pp. 319-325). Cameron *et al.* (2008) propose a bootstrap-based improvement that works well with a small number of groups. We follow their guidance and report bootstrap p-values as our best available indicator of statistical significance.

The treatment effect for *TFP*, our key outcome variable of interest, is negative and (based on bootstrap p-values) statistically significant at least at the 8%-level in all nine specifications. The parameter estimate for the most rigorous specification reported in column 3 (narrowest time window and all controls) is -0.632. This estimate suggests that strict TCF policy has reduced clothing retail productivity among stores in England and Wales that started up between 1996 and 1998 by an accumulated 46.8 percent until 2010.[4] The results are very similar for the specifications estimated in columns 1 and 2 – the implied reductions in productivity are 46.1% and 46.4%, respectively.

The magnitude of this adverse effect is very large, yet finding a very substantial adverse impact is consistent with Basu *et al.* (2003), who document a huge gap in retail productivity growth (of an *annualized* 6.4%) between the US and the UK between the first and second halves of the 1990s. Nevertheless we should note two caveats. First, we identify the adverse impact of the strict TCF policy among start-ups (between 1993 and 1998) only and not among the entire universe of clothing stores. A notable part of aggregate productivity shift likely works through entry and exit (Griffith and Harmgart, 2005) and thus finding a larger impact of the policy intervention by looking at start-up stores than when looking at the aggregate productivity shift in the entire sector, is quite plausible. In this light, it is also worth noting that our findings are in line with estimated productivity losses identified in earlier studies for the grocery sector (Haskel and Sadun, 2012; Cheshire *et al.*, 2012). Second, the sample size of start-up stores, on which the three point estimates are based, is quite small and the point estimates can therefore not be very precisely estimated.

[4] Denoting productivity under treatment by a_1 and without treatment by a_0, we have $\log(a_1) = -0.632+bX$ and $\log(a_0) = bX$. Then $\log(a_1)-\log(a_0) = -0.632$. It follows that $a1/a0 = \exp(-0.632) = 0.532$. The latter implies a loss in productivity of 46.8% (= 1-0.532).

It seems sensible therefore, as a sensitivity check, to widen the time window around the treatment year to include additional stores in order to further increase the sample size. When we widen the time window, the point estimates become somewhat smaller, although the implied quantitative effects are still very substantial: The implied productivity loss is between 30 and 33% when the time window includes stores that started up between 1990 and 1998 and it is between 15 and 16 percent, when the time window includes stores that started up between 1987 and 1998. The fact that the effect becomes smaller when we widen the time window may be indicative of a downward bias introduced by stores that opened up prior to 1993. Specifically, as discussed above, our estimates may be biased by confounding factors, which were different for older stores and which we do not perfectly capture with our control variables. The downward bias introduced by the 'survival bias' – discussed above – may also be more pronounced when we include older stores prior to 1993.

The remainder of Table 3 displays the results for the other outcome measures of interest. Considering again first our most rigorous specifications reported in columns 1 to 3 we find that the introduction of a strict TCF policy in 1996 may have significantly reduced labour- and space-inputs in English and Welsh stores that opened up after 1996. The coefficient on the treatment variable is between -0.537 and -0.572 when the outcome measure is log of labour hours and it is between -0.29 and -0.4 when the outcome measure is log of floor space. All these estimated adverse effects are, based on bootstrap p-values, statistically significant at least at the 8 percent level. The effect of the treatment on log of tills is not statistically significant in any of the specifications, consistent with the findings reported in Table 2. The results reported towards the bottom of Table 3 suggest that the treatment may have reduced the number of local competitors in England and Wales in areas where new stores settled after 1996. The coefficient on the treatment variable is between -3.4 and -4.4 if we consider the number of competitors within a distance band of 0 to 5 kilometres (1 kilometre = 0.62 miles) and it is between -5.9 and -6.8 when we consider competitors within a band of 0 to 10 kilometres. The parameters of the treatment effect are statistically quite significant with p-values of 0.054 and 0.024 in column 1 but are less precisely estimated in columns 2 and 3 (with p-values between 0.213 and 0.229).

Next, we consider the remaining specifications reported in columns 4 to 9 of Table 3. Overall, these results appear to confirm the negative impact of the strict TCF policy on the number of local competitors. The point estimates with respect to the impact on competition are similar to columns 1-3 in all specifications. Moreover, at least when we consider the impact on the number of competitors within the narrower of the two distance bands, the estimated adverse effects are, again based on bootstrap p-values, always statistically significant at least at the 7 percent level. Overall, these results strongly suggest that the policy intervention in 1996 had an adverse effect on the number of local competitors that stores, which opened up between 1996 and 1998, are facing in 2011. Whether having fewer competitors implies less competition intensity is not quite clear. To the extent that the latter is indeed the case, we would expect that the treatment increased sales prices, which in turn would misleadingly imply higher productivity (Griffith and Harmgart, 2005). It would also imply that our estimated adverse net effect of the strict TCF policy on TFP may be an under-estimate of the true gross cost associated with the policy intervention. Lastly, when we use wider time

windows, we no longer find statistically significant effects of the treatment on the use of labour- and space-inputs (and the result with respect to capital inputs remains insignificant).

Additional robustness check: drop Wales from treatment group

When we discussed the various land use planning policies in Section 2, we argued that the policies in Wales followed those in England closely. However, we do acknowledge that the two countries did not have identical policies and the treatment in 1996 may have been somewhat weaker in Wales than in England. Hence, as a robustness check, we remove Wales from the treatment group and re-estimate all specifications reported in Table 3. We present results in Table 4. The results are almost identical in all cases, which is not particularly surprising as the narrowest time window in Table 3 only consists of one Welsh store and even the widest time window only includes three Welsh stores. We have also experimented with altering the control group. Arguments could be made to remove stores located in the Republic of Ireland from the control group (since the Republic of Ireland is not part of the UK) or to only include stores located in the Republic of Ireland (since it did not introduce any form of TCF policy; although the number of stores in the Republic of Ireland is small, so that leaves a very small control group). In all these cases, the quantitative effects are of a similar magnitude, although statistical significance levels are somewhat lower.

5 Conclusions

Our empirical analysis demonstrates a clear causal relationship from the introduction of a *strict* 'Town Centre First' (TCF) policy in England and Wales in 1996 to a reduction in store-level total factor productivity (TFP). Given that our data provider is quite representative for the clothing retail sector as a whole and, arguably, even for the broader comparison goods sector, we interpret our findings as indicative that the strict TCF policy has also had a substantial adverse effect on retailers of comparison goods, not just the grocery sector as demonstrated in earlier studies.

Our findings are indicative that the adverse effect of strict TCF policy on TFP may be to a good extent driven by planners at the local level micromanaging store locations. We derive this conclusion from the facts that (i) the implied quantitative adverse effects of strict TCF policy on productivity are very large for our clothing retailer and (ii) it is commonplace in the industry to argue that location is very important for companies that, like our clothing retailer, sell 'comparison' goods, whereas floor space is seen as a less obviously important input compared to the grocery sector. If the results were purely driven by strict TCF policy reducing store size, we should see small rather than large quantitative adverse effects of strict TCF policy based on our sample of clothing stores.

Our findings also emphasize the relevance of unobserved time-invariant and time-varying characteristics at the store level that may hamper cross-sectional estimates. Using panel data and controlling for store fixed effects does alter the sign of our coefficient on space input and arguably leads to less biased estimates of the impact of strict TCF policy on TFP and other outcome measures.

All our results of course relate only to costs in terms of lost productivity. There may be benefits of TCF policies. Their supporters claim these take the form of reducing the energy use in retail by facilitating the use of public transport and linked trips. They also argue that retaining retail in town centres helps poorer families who may not have access to cars. However there is no actual evidence as to the quantitative magnitude of such benefits. In this context it would seem helpful at least to have some measure of their costs.

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Tables

TABLE 1
Summary Statistics

Store characteristics (store panel data 2007-2010)	**All** *(N=1,497)*		**England and Wales** *(N=1,236)*		**Rest of British Isles (Scotland, Northern Ireland, Republic of Ireland, Channel I. and I. of Man)** *(N=261)*	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Sales	4,480,119	3,056,891	4,429,005	3,111,876	4,722,175	2,774,552
Labour hours (L)	56,617	34,602	56,778	35,677	55,854	29,022
Floor space (S)	9,960	5,950	10,046	6,140	9,552	4,939
Number of tills (T)	9.76	4.13	9.74	4.22	9.88	3.64
Residual: sales - f(L, S, T)	4.47	0.16	4.46	0.15	4.53	0.19
Year opened	2000.4	6.661	2000.2	6.789	2001.0	5.986
Location of store (binary variables):						
England	0.79					
Wales	0.034					
Scotland	0.084					
Northern Ireland	0.031					
Republic of Ireland	0.053					
Islands: Channel I. and I. of Man	0.0054					
Type of store (binary variables):						
Out of town stores	0.41					
Shopping centres [1)]	0.29					
High street stores	0.19					
Other stores [2)]	0.11					
Number of competitors in 2011	**All** *(N=383)*		**England and Wales** *(N=316)*		**Rest of British Isles** *(N=67)*	
No. of competitors: 0–5km	5.26	4.85	5.56	4.76	3.84	5.08
No. of competitors: 0–10km	8.80	7.31	9.46	7.23	5.73	6.96

Notes: [1)] Shopping centres include: 'covered', 'regional' and 'uncovered'. [2)] Other stores include: 'clearance high street', 'clearance outlet', 'clearance retail park', 'staff shop' and 'home standalone store'.

TABLE 2

Production Function Parameters: OLS and Fixed Effects Specifications (2007-2010)

	Dependent variable: log(Sales)					
	OLS			Store Fixed Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	TTWA Dummies	TTWA*Year Dummies	Baseline	TTWA Dummies (in 2nd stage)	TTWA* Year (in 1st stage)
				1st Stage Estimates		
log(Labour hours)	1.302***	1.290***	1.308***	0.903***	0.903***	0.837***
	(0.026)	(0.029)	(0.038)	(0.042)	(0.042)	(0.058)
				2nd stage estimates		
log(Floor space)	-0.119***	-0.078**	-0.094**	0.134***	0.179***	0.170***
	(0.035)	(0.034)	(0.043)	(0.039)	(0.055)	(0.037)
log(Tills)	-0.093**	-0.096**	-0.095*	0.009	0.003	0.034
	(0.043)	(0.042)	(0.052)	(0.058)	(0.083)	(0.055)
Additional controls [1)]	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes (1st)	Yes (1st)	Yes (1st)
TTWA Dummies	No	Yes	Yes	No	Yes (2nd)	No
TTWA-Year Dummies	No	No	Yes	No	No	Yes (1st)
Adj. R-Squared	0.972	0.983	0.978	0.782	0.782	0.845
Within R-Squared				0.782	0.782	0.893
Adj. R-Sq. (2nd stage)				0.511	0.550	0.542
Obs.	1497	1497	1497	1497	1497	1497
Groups				383	383	383
Obs. (2nd Stage)				383	383	383

Notes: [1)] Additional controls include age of store, age of store squared and type of store dummies. Store FE models (columns 4-6): Time-variant continuous variables and dummy variables which can be identified separately from store fixed effects are included in the 1st stage regressions. The remaining control variables and dummy variables are included in the 2nd stage regressions. The outcome in the 2nd stage regressions is the estimated store FE from the corresponding 1st stage regression. The 1st stage regressions in columns 4 and 5 control for store fixed effects, year dummies and age squared. The 1st stage specification in column 6 includes TTWA-year interaction dummies. 2nd stage regressions in columns 4 and 6 control for year dummies and age of store, which is not included in the 1st stage regression as its coefficient cannot be separately identified when year and store FEs are included. The 2nd stage specification in column 5 includes TTWA dummies. 35 store observations were dropped because stores opened in 2007 and had incomplete annual data for that year. For those stores we use three (2008-2010) instead of four observations (2007-2010). Two additional (clearance) stores were dropped because they did not contain any information on tills. *, **, and *** correspond to 90, 95 and 99 percent confidence levels, respectively. Standard errors in columns (1) to (3) are clustered by store. Standard errors in columns (4) to (6) are clustered by store in the 1st stage and by TTWA in the 2nd stage and are in parenthesis.

TABLE 3
Impact of 'Town Centre First' on Productivity and other Outcome Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Time window:	1993-1998			1990-1998			1987-1998		
Treatment area trends:	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Country FEs:	No	No	Yes	No	No	Yes	No	No	Yes
Dependent variable:	Coefficient on Post 95 x Treat (Classic standard errors in parenthesis) [Standard errors robust to clustering at country level are in brackets] {Bootstrap p-values based on a wild bootstrap of t-statistics and 500 bootstrap replications in braces} *Implied loss in total factor productivity in % in italics (quantitative effect)*								
Productivity (residual)	**-0.618**	**-0.623**	**-0.632**	**-0.36**	**-0.371**	**-0.398**	**-0.171**	**-0.161**	**-0.168**
	(0.178)	(0.189)	(0.191)	(0.178)	(0.184)	(0.180)	(0.105)	(0.108)	(0.109)
	[0.106]	[0.115]	[0.110]	[0.194]	[0.193]	[0.238]	[0.058]	[0.066]	[0.059]
	{0.004}	**{0.017}**	**{0.018}**	**{0.047}**	**{0.065}**	**{0.081}**	**{0.032}**	**{0.065}**	**{0.023}**
	-46.1%	*-46.4%*	*-46.8%*	*-30.2%*	*-31.0%*	*-32.8%*	*-15.7%*	*-14.9%*	*-15.5%*
		N=30			N=34			N=99	
Log(Labour hours)	**-0.572**	**-0.537**	**-0.563**	0.085	0.046	-0.021	0.112	0.134	0.115
	(0.563)	(0.596)	(0.489)	(0.516)	(0.522)	(0.476)	(0.350)	(0.358)	(0.361)
	[0.148]	[0.177]	[0.278]	[0.473]	[0.444]	[0.643]	[0.173]	[0.220]	[0.202]
	{0.002}	**{0.023}**	**{0.028}**	{0.720}	{0.751}	{0.447}	{0.727}	{0.773}	{0.759}
		N=31			N=35			N=100	
Log(Floor space)	**-0.29**	**-0.371**	**-0.4**	0.228	0.222	0.177	0.264	0.209	0.208
	(0.644)	(0.652)	(0.531)	(0.563)	(0.565)	(0.553)	(0.395)	(0.403)	(0.407)
	[0.173]	[0.192]	[0.302]	[0.596]	[0.597]	[0.745]	[0.329]	[0.370]	[0.366]
	{0.070}	**{0.023}**	**{0.076}**	{0.819}	{0.751}	{0.725}	{0.879}	{0.749}	{0.765}
		N=31			N=35			N=100	
Log(Number of tills)	0.096	0.145	0.053	0.461	0.416	0.341	0.39	0.387	0.403
	(0.503)	(0.528)	(0.412)	(0.444)	(0.447)	(0.432)	(0.306)	(0.312)	(0.316)
	[0.146]	[0.209]	[0.307]	[0.306]	[0.272]	[0.404]	[0.202]	[0.217]	[0.226]
	{0.759}	{0.797}	{0.810}	{0.960}	{0.942}	{0.812}	{0.980}	{0.981}	{0.986}
		N=30			N=34			N=99	
No. of competitors: 0–5km	**-4.429**	-3.429	-3.441	**-6.367**	**-6.45**	**-5.992**	**-4.338**	**-5.269**	**-5.033**
	(3.901)	(3.262)	(3.328)	(4.578)	(4.362)	(4.140)	(3.207)	(3.195)	(3.217)
	[2.989]	[4.198]	[4.338]	[3.432]	[3.120]	[3.518]	[2.999]	[3.053]	[3.112]
	{0.054}	{0.213}	{0.229}	**{0.019}**	**{0.016}**	**{0.068}**	**{0.048}**	**{0.050}**	**{0.064}**
		N=31			N=35			N=100	
No. of competitors: 0–10km	**-6.847**	-5.938	-5.906	-5.514	**-5.824**	-5.271	-3.994	-4.967	-4.676
	(6.883)	(6.548)	(6.675)	(6.655)	(6.354)	(6.176)	(4.671)	(4.626)	(4.662)
	[4.587]	[6.408]	[6.441]	[5.734]	[5.146]	[4.465]	[3.782]	[4.022]	[4.109]
	{0.024}	{0.213}	{0.229}	{0.227}	**{0.070}**	{0.104}	{0.206}	{0.211}	{0.204}
		N=31			N=35			N=100	

Notes: Coefficients with bootstrap p-values <0.1 and corresponding bootstrap p-values are highlighted in **bold**.

TABLE 4
Robustness check: Drop Wales from Treatment Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Time window:	1993-1998			1990-1998			1987-1998		
Treatment area trends:	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Country FEs:	No	No	Yes	No	No	Yes	No	No	Yes
	Coefficient on (England and Wales) × Post-1995 (Classic standard errors in parenthesis) [Standard errors robust to clustering at country level are in brackets] {Bootstrap p-values based on a wild bootstrap of t-statistics and 500 bootstrap replications in braces}								
Dependent variable:	*Implied loss in total factor productivity in % in italics (quantitative effect)*								
Productivity (residual)	**-0.605**	**-0.623**	**-0.632**	**-0.348**	**-0.371**	**-0.398**	**-0.160**	**-0.154**	**-0.161**
	(0.177)	(0.189)	(0.191)	(0.179)	(0.184)	(0.180)	(0.107)	(0.110)	(0.111)
	[0.107]	[0.117]	[0.112]	[0.200]	[0.196]	[0.242]	[0.058]	[0.067]	[0.062]
	{0.007}	**{0.024}**	**{0.027}**	**{0.064}**	**{0.074}**	**{0.064}**	**{0.023}**	**{0.059}**	**{0.072}**
	-45.4%	*-46.4%*	*-46.8%*	*-29.4%*	*-31.0%*	*-32.8%*	*-14.8%*	*-14.3%*	*-14.9%*
	N=29			N=33			N=96		
Log(Labour hours)	**-0.530**	**-0.537**	**-0.563**	0.127	0.046	-0.021	0.154	0.171	0.152
	(0.561)	(0.596)	(0.489)	(0.515)	(0.522)	(0.476)	(0.353)	(0.359)	(0.362)
	[0.138]	[0.180]	[0.282]	[0.485]	[0.451]	[0.655]	[0.166]	[0.219]	[0.203]
	{0.003}	**{0.027}**	**{0.030}**	{0.748}	{0.761}	{0.472}	{0.897}	{0.807}	{0.804}
	N=30			N=34			N=97		
Log(Floor space)	**-0.256**	**-0.371**	**-0.400**	0.263	0.222	0.177	0.300	0.239	0.239
	(0.648)	(0.652)	(0.531)	(0.567)	(0.565)	(0.553)	(0.397)	(0.403)	(0.407)
	[0.170]	[0.195]	[0.307]	[0.614]	[0.608]	[0.758]	[0.335]	[0.377]	[0.375]
	{0.056}	**{0.027}**	**{0.093}**	{0.796}	{0.761}	{0.738}	{0.864}	{0.776}	{0.767}
	N=30			N=34			N=97		
Log(Number of tills)	0.117	0.145	0.053	0.481	0.416	0.341	0.409	0.401	0.419
	(0.510)	(0.528)	(0.412)	(0.450)	(0.447)	(0.432)	(0.308)	(0.313)	(0.316)
	[0.147]	[0.212]	[0.312]	[0.315]	[0.276]	[0.411]	[0.207]	[0.222]	[0.233]
	{0.790}	{0.799}	{0.821}	{0.986}	{0.955}	{0.814}	{0.999}	{0.987}	{0.999}
	N=29			N=33			N=96		
No. of competitors: 0–5km	**-4.179**	-3.429	-3.441	**-6.117**	**-6.450**	**-5.992**	**-4.083**	**-5.052**	**-4.816**
	(3.909)	(3.262)	(3.328)	(4.624)	(4.362)	(4.140)	(3.250)	(3.222)	(3.242)
	[3.066]	[4.263]	[4.405]	[3.527]	[3.175]	[3.580]	[3.073]	[3.119]	[3.185]
	{0.056}	{0.230}	{0.220}	**{0.025}**	**{0.026}**	**{0.067}**	**{0.060}**	**{0.069}**	**{0.110}**
	N=30			N=34			N=97		
No. of competitors: 0–10km	**-6.387**	-5.938	-5.906	-5.054	**-5.824**	-5.271	-3.630	-4.742	-4.453
	(6.885)	(6.548)	(6.675)	(6.684)	(6.354)	(6.176)	(4.728)	(4.682)	(4.717)
	[4.687]	[6.507]	[6.540]	[5.884]	[5.236]	[4.544]	[3.867]	[4.119]	[4.216]
	{0.059}	{0.230}	{0.220}	{0.243}	**{0.078}**	{0.101}	{0.199}	{0.225}	{0.239}
	N=30			N=34			N=97		

Notes: Coefficients with bootstrap p-values <0.1 and corresponding bootstrap p-values are highlighted in **bold**.