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Ownership and competition: Finding Performance Breaks for Great Britain's Power Plants*

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Agency theories predict that private ownership and competition increase firm productivity. We empirically assess these predictions for Great Britain's electricity industry restructuring and privatisation, using US plants as a control group. We estimate unknown break dates and the associated input productivity changes for labour and fuel at the plant level. We exploit the sequence of reform steps to separately identify the effects of ownership change and competition. Privatisation dramatically increased labour productivity. The effect is driven by incentives as well other mechanisms but is not primarily due to selection. Fuel productivity only increased with later increases in competition, probably due to divestitures.

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1. Introduction

In economics two important drivers of firm performance are competition and ownership or governance. The organisation of markets and firms influences incentives, performance, and welfare.

Agency theories suggest that both private ownership and competition provide strong incentives to improve technical efficiency (the main source of static welfare gains besides allocative efficiency). Managers of privately owned firms in competitive markets face high powered incentives to increase firm productivity. Utility industry reforms throughout the 1980s and 1990s were influenced by these theoretical predictions (Winston, 1993). Utility industry reforms also provide good cases for economists to assess these theoretical predictions empirically, because often panel data on physical inputs and outputs is available, because control groups are often present, and because treated plants usually continue operation, which allows for the identification on incentive effects as opposed to selection effects (Fabrizio et al., 2007, p. 1253).

We study the effects of privatisation and competition on plant productivity for Great Britain's (GB) electric industry reforms, starting with restructuring and privatisation (R&P) in 1990/1991 and following competition reforms throughout the 1990s. The GB case is of interest, because it was one of the earliest and most pervasive utility industry reforms in the world. Also, an early reform focus on privatisation and a later focus on effective competition allow us to assess the relative importance of both reform steps.

Although there is a large number of studies assessing utility industry reforms, including UK privatisation programmes, (for an overview of the reform literature see Pollitt (2012)) there are few detailed plant or firm level studies on productivity performance. Two important exceptions are Fabrizio et al. (2007) and Gao and Van Biesebroeck (2014) on US and Chinese electric industry reform, respectively. In particular, there are few firm or plant level studies of actual privatisations (e.g. Waddams Price and Weyman-Jones (1996)). Most studies analyse ownership effects using either before-after or cross-sectional comparisons only (Shirley and Walsh, 2000; Pollitt, 1995). To our best knowledge ours is the first study to analyse the causal effects of one of the UK's privatisation programmes at the plant-level. Newbery and Pollitt (1997), using a simulated counterfactual, were the first to perform a social cost-benefit analysis for the privatisation of the UK's electricity supply industry.

Our empirical approach is to model plant-level productivity as a set of input demand functions derived from a behavioural model of cost minimisation, following Biesebroeck (2003) and Fabrizio et al. (2007). Plant-level, short-run productivity proxies for management performance. The reform effects can only be disentangled at the plant-level, because other factors like fuel mix cannot be controlled for when using aggregate data. Also, our plant-level analysis allows us to control for selection using plant fixed effects as well as contrasting results for an unbalanced and balanced sample. Last, we instrument for the potentially endogenous supply variable in input demand. We analyse the causal effects of the reforms on performance using a standard difference-in-difference approach. Most previous studies assume that the break date is known. In contrast, we estimate unknown break dates, because the timing of the economic impact might be different from the official reform dates or there might be no official reform date at all as for instance, when collusion breaks down. We search for structural breaks in technical efficiency for a window of years around R&P using the criterion suggested by (Bai, 2010). We then map the estimated break dates to the known reform history (Freeman, 2005). Our control group is the performance of comparable, publicly owned, US power plants. This cross-country comparison minimises the risk that treatment effects spill over to the control group.

We collected unique plant-level input-output data for a sample GB electricity generation plants for the years 1981 to 2004 and matched it with an extended version of the data used by Fabrizio et al. (2007) for US, publicly owned plants.

We find that privatisation in 1990/1991 increased labour productivity by about 50 per cent but did not increase fuel productivity at all. The large increase in labour productivity is not predominantly driven by selection but is also too large to be due to incentives only. As discussed in previous studies it also reflects changed management objectives as well as the loosening of political constraints; in particular lower union power. Privatisation probably did not increase fuel productivity because the publicly owned firm had a culture that valued fuel productivity and because competitive pressure was weak right after privatisation. Increased competition, probably through divestitures, increased fuel productivity by up to 15 per cent in the second half of the 1990s. Competition also increased labour productivity but the effect is strongly dominated by privatisation.

The outline is as follows. Section 2 provides some background information on GB electricity R&P. Section 3 summarises the relevant literature. Section 4 describes our empirical approach. Section 5 describes the data. Section 6 gives the results and section 7 concludes.

2. Background

Before R&P, the generation and transmission of electricity in England & Wales was the responsibility of the Central Electricity Generation Board (CEGB), a horizontally and vertically integrated state-owned monopoly. Distribution in England & Wales was the responsibility of 12 regional monopolies, the Area Boards. In Scotland and Northern Ireland vertically integrated, regional monopolies were responsible for generation, transmission and distribution.[1] Even though the Scottish reform process was slightly different, we include Scottish plants in this study. Northern Irish plants are excluded, because Northern Ireland was not interconnected with Great Britain during our sample period.

A first attempt to liberalise the market was undertaken in 1983 when the Energy Act required the Area Boards to buy energy from independent (non-CGEB) generators at avoided cost. We do not have a long enough sample to assess this reform. As the Act did not protect entrants from anti-competitive behaviour by the incumbent the reform was ineffective (Vickers et al., 1991). Eventually, restructuring of English and Welsh companies began in March 1988 and was completed in March 1990. And they were privatised the following year in March 1991. In Scotland the industry was also privatised in 1991 but was not restructured immediately.[2] Restructuring in England and Wales comprised the vertical unbundling of generation and transmission as well as the horizontal separation of generation.[3] Generation assets were separated into four companies: all fossil-fuel plants were divided between PowerGen and National Power (our sample), the new transmission company National Grid obtained pumped storage plants, and Nuclear Electric the nuclear plants. Sixty percent of the shares in PowerGen and National Power were sold to the public in 1991. The remaining shares were sold in February 1995 but there is no evidence that the government kept any direct control after 1991. Part of Nuclear Electric was only privatised in 1996 (as British Energy) and is not included in this study. The Area Boards and the transmission company became private, regulated monopolies.

Even though a deregulated wholesale market was established at R&P, competition amoung electricity generators was not effective, because National Power and PowerGen formed a duopoly for fossil-fuel generation (Sweeting, 2007). Government gave the newly private firms market power to increase the sales price at privatisation (Henney, 2010, p. 37). Moreover, the rules of the newly established wholesale market, referred to as the Pool, facilitated the exercise of market power (Green, 2006). Also, at R&P only sales to industrial (above 10 MW) but not retail customers were deregulated (partly to burden retail customers with the high cost of British coal). Prices for smaller customers were deregulated in stages until retail was fully deregulated in 1999.

A few years after R&P high electricity prices and profits shifted the focus from ownership to competition. Between 1994 to 1996 the regulator set a wholesale price cap. Full privatisation

[1] Whereas Scotland had two companies, Northern Ireland only had a single electricity company.

[2] See Pollitt (1997) and Pollitt (1999) for a discussion of the Northern Irish and Scottish cases, respectively.

[3] In the US restructuring often refers to the introduction of competition, i.e. market liberalisation.

in 1995 certainly removed any conflict of interest for the government concerning competition. The main tool to increase competition was to reduce market concentration through divestitures which came in two rounds in 1996 and 1999. There is evidence that only the second round changed firm behaviour. In the first round plants were leased to a single competitor and lease contracts were such that the new owner had no incentive to compete (Henney, 2010, p. 37). Sweeting (2007) provides evidence that firms (tacitly) colluded throughout the second half of the 1990s. In 1999 the incumbents sold plants to various (mostly US) investors in return for regulatory permission to vertically integrate with the supply function. For example, Edison Mission bought two plants in 1999 and increased output by 30% (Newbery, 2004, p. 18), which is unlikely to be the result of efficiency increases alone. Probably, the entry of US firms in the late 1990s ended tacit collusion between the generators. Market concentration for generation decreased considerably throughout the 1990s. The Herfindahl Hirschman index, a measure of market concentration, for coal fired plant had dropped by 1999 to a fifth of its value in 1990. Nevertheless, the regulator judged that in the Pool "prices had failed to properly reflect a more competitive generation market and falling generation input costs" (Ofgem, 2002, p. 1) and changed the wholesale market rules. The new market design, referred to as the New Electricity Trading Arrangements (NETA), was introduced in 2001. There is evidence that allocative efficiency increased around the year 2000 but there is a debate about whether reduced market concentration or the new market rules were responsible. Whereas Newbery (2004) attributes lower wholesale prices to the change in market structure just before the introduction of NETA, Evans and Green (2003) argue that behaviour changed in anticipation of the new market rules.

In addition to the reforms that directly affected ownership and competition other policies affected generators. Henney (2010, p. 37) argues that the cause for efficiency improvements was neither ownership change nor increased competition but the "freedom [...] from the politics that had traditionally enmeshed [the industry]." After R&P, two major constraints on plant's input decision were removed. First, union power was greatly diminished (both at the coal mines and power plants). Second, constraints on the overall fuel mix, mainly to protect the domestic coal industry, were gradually removed throughout the 1990s. From R&P until 1994 and to a lesser degree till 1998 the incumbent generators were committed to buying certain amounts of British coal at above world market prices, the cost of which could be passed on to captive retail customers (Newbery, 1998). It was intended to let these coal contracts expire once retail competition was extended to all customers in 1998. Starting in about 1993 gas was increasingly substituted for coal (the "dash for gas"). Low capital cost and low gas prices made gas the fuel of choice (Newbery, 1994). As new gas capacity grew much faster than demand many coal-fired

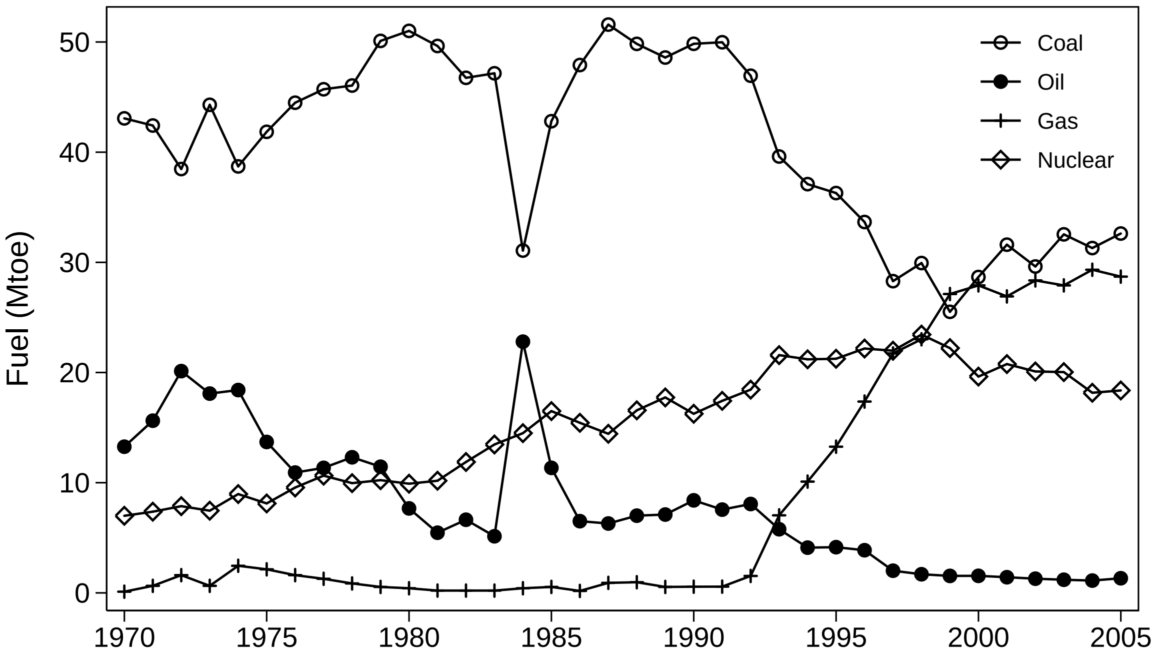


Figure 1: The UK's Fuel Use in Electricity Generation

Notes: The graphs plots aggregate UK fuel input (measured in million tons of coal equivalent). The data source is the Digest of UK Energy Statistics (DUKES).

plants were closed prematurely. To increase the comparability between GB and US plants we focus on coal in the analysis below. Figure 1 illustrates these changes in the aggregate fuel mix for the period 1970 to 2005 using data from the Digest of UK energy statistics (DUKES). There seem to be no major changes in fuel mix in the decade before R&P (except the substitution of oil for coal during the miner's strike in 1984). After R&P, between 1993 and 1999 gas grows rapidly at the expense of coal. The substitution of gas for coal stopped in 1999. Between 1998 and 2000, out of concern for dwindling British coal sales and increasing dependence on gas the government made it more difficult to obtain licenses for new gas-fired stations (the gas "moratorium").

3. Literature

The general argument about the effect of competition on performance goes as follows (Nickell, 1996; Shirley and Walsh, 2000). For manager-owned firms, more competitive pressure increases incentives to operate more efficiently. If a firm's efficiency is too low it will be driven out of the market by its more efficient competitors. This is often referred to as the *incentive* effect. The incentive effect also operates for firms that separate ownership and control. But here part of the effect is indirect via improved *information* for owners. Owners rely on competitors (as comparators) to control the rent seeking of employed managers. Of course, also the owners of monopoly firms want to control rent seeking by managers but with asymmetric information and no competitive benchmark they struggle to do so. Agency theory recognises the importance of asymmetric information and incentives under the separation of ownership and control for management behaviour. Management is considered intrinsically "effort-averse" (Fabrizio et al., 2007).

As such, these considerations apply equally to privately and publicly owned firms in competitive markets.[4] But is it possible that even in competitive markets public firms perform worse? Or, that private monopolies perform better than public monopolies? The controversy around privatisations suggests that ownership affects performance independently. First, taking into account public choice theory, public owners might not have profit maximisation as their (only) objective. Technical inefficiency might arise, because public owners have a preference for more labour intensive operation (machines do not vote) or favour the intensive use of local or national fuels. Newbery (1995) provided anecdotal evidence, that after R&P there was a "change in culture of CEGB, where being base and max. thermal efficiency must change to value flexibility". It is debatable whether efficiency changes due to changes in objectives should be counted as such.

Second, Alchian (1965) argued, that even if public owners maximise profit, more dispersed ownership (all voters) and the inability to sell (except through privatisation) gives public owners relatively little influence over management.[5]

Shirley and Walsh (2000) summarised the literature on the performance effects of competition and ownership. They concluded, that more competition and private ownership positively affect firm performance. And, that on balance, both the theory and the empirical evidence point towards a dominance of the ownership effect over the competition effect.

There is a sizable literature on the effects of public ownership and privatisations on firm performance. Whereas studies that make cross-sectional comparisons across ownership types (Pollitt, 1995; Arocena and Waddams Price, 2002) find little difference D'souza and Megginson (1999) found that actual privatisations increased performance including labour productivity for a sample of 85 privatisations over the years 1990 to 1996. But there is a question about

[4] Ownership and competition are not necessarily independent. When competition is not feasible public ownership is a possible response to the regulatory problems associated with asymmetric information.

[5] Public ownership might affect allocative efficiency because public owners more likely to internalise any externalities from power plant operation. That is increases of technical efficiency at the plant level do not necessarily correlate with welfare improvements. Two externalities that private owner might disregard are effects on system stability (including optimal investment in the long-run) and pollution.

whether the effect is due to actual privatisation or related changes to management and firm or market structure (Pollitt, 2000) which highlights the importance of searching for an unknown break date as we do below. Also, Newbery (1998, p. 3) argued that it is unlikely that performance improvements were due to ownership change. He observed, that Nuclear Electric, which was exposed to competition in 1990/91 but itself only privatised in 1996 experienced similar performance improvements as other generators. Pollitt (1997) found a similar result for the privatisation of the electricity industry in Northern Ireland.

Recently, a number of papers have investigated the effects of ownership and competition for utility industries. Fabrizio et al. (2007) studied the impact of US electric industry restructuring (the introduction of wholesale and possibly retail competition) on plant productivity. They analysed the impact of ownership and competition on the efficiency of US electricity generation plants and found that investor-owned utilities (IOUs) in restructuring states reduced non-fuel expenses by up to 5 percent, labour input by 3 percent, and fuel input by up to 1.4 percent (the latter being statistically insignificant) in comparison to firms in non-restructuring states. They also used an alternative counterfactual, municipality owned plants (we use the same data below), and found that for labour and non-fuel expenses the effect of restructuring is about twice as large. This implies either, that IOUs in non-restructuring states are not a good control group, because restructuring had spill-over effects or, that the effect of ownership adds to the effect of competition and both effects are about the same size. Somewhat surprisingly, they found no economically or statistically significant effect for fuel efficiency. This might be due to the omitted effects of incentive regulation. Bushnell and Wolfram (2005) found that both divestiture, i.e. the sale to non-utility owners and incentive regulation, which tries to mimic the effects of competition, for plants that remain in utility ownership increased fuel efficiency by about 2 per cent. Hiebert (2002) analysed the effects of ownership and restructuring on cost efficiency for a sample of US generation plants for the years 1988-1997. He found that restructuring increased efficiency for coal-fired but not for gas-fired power plants. He found only mixed evidence for the effect of ownership. Gao and Van Biesebroeck (2014) found that after restructuring in 2002, publicly as opposed to privately owned Chinese generation plants saw larger increases in labour and material (including fuel) efficiency. Although, the effect occurred with a lag of at least two years. We take their results as evidence that publicly owned firms, respond to incentives at least as well as privately owned firms.

4. Empirical Model

4.1. Production

The empirical analysis of productivity change requires a model of the production technology. We apply the model developed by Biesebroeck (2003) and Fabrizio et al. (2007), because it represents accurately the short-run production function of an electricity generation plant. In particular, it accurately models the temporal sequence of input decisions and the resulting constraints on short-run input substitution. Here short-run productivity is a proxy for management performance.

We model actual production as the minimum of planned (or "probable") output, which is a function of capital and labour, and fuel input. Actual output (observed by the researcher) could be more or less than planned output, because actual demand differs from expected demand or, because of unexpected changes in plant availability. Actual output Q^A for plant i in year t takes the following Leontief form:

$$Q_{it}^A = \min[g\left(E_{it}, \Gamma^E, \epsilon_{it}^E\right), Q_{it}^P\left(K_i, L_{it}, \Gamma^P, \epsilon_{it}^P\right)\exp\left(\epsilon_{it}^A\right)]. \tag{1}$$

Actual output is a function of probable or planned output Q^P and fuel input $g(\cdot)$. Γ denotes coefficient vectors and ϵ represents error terms. If planned output is not constrained, $Q^A = Q^P \exp\left(\epsilon_{it}^A\right)$, i.e. actual output equals probable output multiplied by a shock that is observed by the plant manager at the time of actual production. ϵ_{it}^P is observed by the plant manager but at the time of planning. As errors are not observed by the researcher we have to take into account simultaneity bias. Planned output is a function of capital K and labour L, but capital is not indexed by t to reflect the short-run nature of the production function. Planned output is not a function of fuel, because the amount of fuel is determined by actual output. Intuitively, whereas the amounts of non-fuel inputs are chosen before production takes place fuel input depends on actual production. This production function allows for medium-run decisions on labour but does not allow labour to substitute for fuel in the short-run. Also, neither in the medium nor the short-run can the plant manager substitute capital.

From this production function we now derive factor demands for labour and fuel. The Leontief form allows us to derive these demands separately. We derive labour demand assuming cost-minimisation behaviour constrained by a Cobb-Douglas production function for probable output $Q_{it}^P = Q_0\left(K_i\right) L_{it}^{\rho} exp\left(\epsilon_{it}^P\right)$.[6] Given a short-run objective function that minimises labour cost,

[6]The assumption of cost-minimisation might be restrictive as it is likely that the CEGB (and possibly the

$\min W_{it} \times L_{it}$, we can derive the following labour demand equation:

$$\ln L_{it} = \alpha_0 + \ln Q_{it}^A - \ln W_{it} - \epsilon_{it}^A \,, \tag{2}$$

where labour L is a function of actual output Q^A, the wage W, and a constant $\alpha_0 = \ln(\lambda\rho)$, which has two components: the Lagrangian multiplier or shadow value of changes in the output constraint λ and the labour parameter ρ or marginal productivity from the production function.

The derivation of the fuel demand follows a different strategy as the fuel input is not part of the optimisation problem used to derive the demand for labour. We only need to assume that $g(\cdot)$ is monotonically increasing in fuel E and inversion produces the following fuel demand equation:

$$\ln E_{it} = \gamma_Q \ln Q_{it}^A + \epsilon_{it}^E \,, \tag{3}$$

where γ is the output coefficient. Unlike for labour the fuel demand function does not depend on price as the price of fuel only enters indirectly through the output the plant is dispatched to produce as the fuel price affects the merit order. We need to expand these theoretical demand equations by a set of indicators to perform our difference-in-difference estimation.

As we are interested in comparing productivity before and after treatment and between treatment and control group we use the following identification strategy. We control for plant fixed effects α_i to control for any cross-sectional differences across plants like technology or regulatory regimes. We control for year fixed effects to control for technological progress for all plants α_t. And we use an indicator if a plant is in the treatment group and treatment period $\delta_{gt} \forall i \in g$. Accordingly, we expand the subscript by g to indicate whether a plant is treated. We also add an error term ϵ^L to capture measurement error for the input. The resulting labour demand equation is:

$$\ln L_{igt} = \alpha_i + \alpha_t + \delta_g + \ln Q_{igt}^A - \ln W_{igt} - \epsilon_{igt}^A + \epsilon_{igt}^L \,. \tag{4}$$

We add further controls to the reduced form equation, in particular proxies for the output constraint λ. We include a variable for plant age (AGE) to control for vintage. Joskow and Schmalensee (1987) found that plant performance "deteriorates significantly" with age. However, Hiebert (2002) showed that length of service might actually increase performance as plant

privatised plants) did neither maximise productive nor allocative efficiency. Also the focus on the short run might underestimate the benefits of R&P as Arocena and Waddams Price (2002) find that private generators have a higher allocative efficiency in the long run.

management learns to better operate the plant. Pollitt (1995, p. 132) found no significant age effect for a sample of base load plants. We control for capital using net capacity (CAP) and the presence of SO2 (FGD) abatement technology. We also control for load factor (LF), as the scale at which a plant operates affects productivity. After R&P plants might have been called upon in a different order (their position in the merit order changed), which would affect performance (Knittel, 2002). The load factor might also control for different operating patterns. Bushnell and Wolfram (2005, p. 2) state that "irregular operating patterns motivated by attempts to exercise market power and the disruption of an ownership change could diminish operating efficiency at least in the short-run". The reduced form labour demand equation is

$$\begin{aligned} \ln L_{igt} &= \alpha_i + \alpha_t + \delta_g + \beta_1^N \ln NET\,GWH_{igt} + \beta_2 \ln W_{igt} \\ &+ \beta_3 FGD_{igt} + \beta_4 AGE_{igt} + \beta_5 LF_{igt} + \beta_6 \ln CAP_{igt} + e_{igt}\ , \end{aligned} \tag{5}$$

where the error term e combines the deviation from planned output ϵ^A and the input specific error ϵ^L. The reduced form for fuel (not shown) is identical but omits the wage variable.

Due to unobserved productivity shocks supply is probably not exogenous. Following Fabrizio et al. (2007) we use state level sales, a demand proxy, as an instrument for plant supply. For the US we extend the Energy Information Agency's series for state level sales used by Fabrizio et al. (2007). For GB we have to use national sales as regional sales data are not available. We use the final electricity consumption series from the National Statistics' "Energy consumption in the United Kingdom" publication. Our IV estimator is a two-stage least squares within estimator. Another source of endogeneity is attrition even though for electricity plants it is unlikely that negative productivity shocks lead to exit. A greater worry is that the reform led to the exit of low productivity (time invariant) coal-fired plants, especially as in GB after privatisation gas became an attractive fuel. To analyse the influence of selection we also use a balanced sample as described below.

4.2. Structural break

Unlike many previous studies we do not assume that the timing of the effects is known. The reason is that even though the dates of the reform steps are known there are likely to be implementation lags or leads. Also, for a given reform step, like increasing competition, there are multiple dates. Using a wrong date would lead to a wrong estimate for the effect size. We identify the effects of R&P and subsequent changes to market structure and competition as unknown breakpoints across time as proposed by Piehl et al. (2003).

Although there is a vast literature on the estimation of structural breaks for time series data the literature on structural breaks for panel data is surprisingly small. We follow the recent work of Bai (2010) and estimate common breaks across panels (i.e. plants). The estimator for the break is the global minimum of the sum of squared residuals (SSR) of the unrestricted model (i.e. including a break) in a least squares framework. Inference for a break relies on the assumption that errors are independent in the time series and in the cross-section. Also, this procedure assumes that there is a break. That is we cannot formally test the Null hypothesis of no break for a given candidate break date because unlike for time series data or for cases where the break date is known, the critical values are not known. But as the GB reforms were drastic this assumption should not be restrictive. Also, there is no econometric theory that allows us to test for the number of breaks for panel data. We will search for two breaks, a primary and a secondary break. To identify multiple breaks Bai (1997) proposed an iterative procedure where a first break is identified, then the sample is split at the break, and each sub-sample is tested for additional breaks. Across the two sub-samples the break with the largest absolute drop in SSR is chosen as the secondary break. Once a second break date is determined we can go back to investigate whether is first break date is still valid when the sample is split at the secondary break date. This procedure, referred to as refinement, assesses the robustness of the (first) break date. Bai (2010) pointed out that panel data is particularly suited for estimating multiple breaks, because the additional cross-sectional information allows for the robust estimation of relatively short regimes (the period between two breaks). Bai (2010) also provided confidence intervals for the break dates.

Our window for candidate break dates is 1983 to 2002 leaving two years out at each end of the sample. As explained by Bai (2010) the additional cross-sectional information allows for the robust identification of breaks close to the extremes and does not require a substantial trim factor as with pure time series data. The window opens almost a decade before R&P and closes one year after the introduction of NETA in 2001.

Endogeneity might bias the coefficient estimate of the effect size, δ in (5), but it does not bias the test for structural change (Perron and Yamamoto, 2008). Actually, break tests based on instrumental variable (IV) regressors are less precise than tests based on standard OLS regressors as IV regressors have less quadratic variation. Therefore, we use a least squares estimator to estimate the break date and size. And we test whether the estimate of the size (but not the timing) of the break is robust to the use of an IV estimator as described above.

5. Data and Summary Statistics

5.1. Great Britain

We collected generation plant data for physical inputs and outputs from various sources. First, we collected all data that is publicly available, e.g. from industry statistical yearbooks, firm reports, government. Second, we contacted firms directly to obtain additional data. Third, we intrapolated missing observations for supply and fuel using engineering science as detailed in the appendix. Though filling gaps this way requires some assumptions we believe it is better than having missing observations. Where all variables are missing gaps in the data remain. There is a gap in the number of observed plants during restructuring (1988 and 1989), because the CEGB stopped publishing statistical yearbooks. There is also a smaller gap in 1997. We cannot say that the missing values are entirely random in the time-series. But we have no reason to believe that the same is true for the cross-section. That is, for instance, even though the gap before R&P is endogenous, it is unlikely that only low or high productivity plants have missing values. The resulting sample is an unbalanced panel for the years 1981 and 2004. We have a total of 909 plant-year observations for the fuel demand equation and 535 for the labour demand equation. About a third of all observations are for the US. We restrict our sample to coal fired plants to maximise comparability over time and with the US. Coal fired electricity generation is a well defined technology that should not vary much over time or across the two countries. To investigate the effects of selection we also use a "balanced" sample where the requirement is that we observe a plant, possibly with gaps for individual variables, for at least 20 out of 24 years (there are too few plants observed for the entire sample length). All variables and their measurements are listed in Table 1.

Some variables are corrected. Whereas some firms report for financial years others report for calendar years. We correct for this by constructing calendar year data from the weighted financial year data (weights are simply the number of months). We drop observations just after the plant is installed or just before it is shut down because in these years plants do not operate regularly. We measure the presence of FGD by plant level dummies. Plants typically consist of several units and the fitting of abatement technology typically occurs over several years, unit by unit. The FGD dummy takes the value 1 if the first unit is fitted.

Wage data is not available at the plant level. We take gross, weekly, regional wages from the New Earnings Survey (published by the UK Office for National Statistics). As the series only goes back to 1986 we use linear extrapolation to fill the gaps to 1981.

Table 1: Variables and Measurement

Variable	Definition
Fuel	Mtce/year
Labour	number of employees/year
Output	net GWh/year
Capacity	net MW
FGD	1 if FGD fitted; 0 otherwise
Age	number of years since first unit commissioned
Load factor	Proportion
Wage	regional, gross, weekly, pounds at market exchanges rates
POST	structural break indicator: 1 if year >= year of break and country==GB; 0 otherwise

5.2. United States

For the US, we start with the data used by Fabrizio et al. (2007) which is available as an online appendix to their paper. As our control group we use cooperatively and publicly (municipal or federal) owned plants in both restructuring and non-restructuring states. According to Fabrizio et al. (2007, p. 1259) US restructuring did not change the competitive environment for public utilities, except in Arizona and Arkansas which we exclude. Excluding restructuring states decreases the number of plants in the control group from 19 to 11 and does not change the quality of our results. Also, we restrict our US sample to states that have a climate comparable to GB. That is, we only include states whose average summer temperature is below 22 °C and whose annual average temperature is between 6 and 12 °Celsius.[7] We then extend their data to 2004 using commercially available data from Platts. We also extend all other variables using publicly available data. We extend the wage data using the Bureau of Labor Statistics' Quarterly Census of Employment and Wages series. Doing so we have to deal with a change from the SIC to the NAICS industry classification system. The original series is for SIC 4911 (Electric Services). We extend it using NAICS 221112 (Fossil Fuel Electric Power Generation).

Table 2 compares the means between GB and the US plants. In terms of capacity GB plants are about twice as large as US plants but have a lower load factor. Plants have a similar vintage. Wages are about double in the US (at market exchange rates).

[7] US plants are from the following states: Colorado, Michigan, New Mexico, Ohio, Pennsylvania, Utah, and Wisconsin.

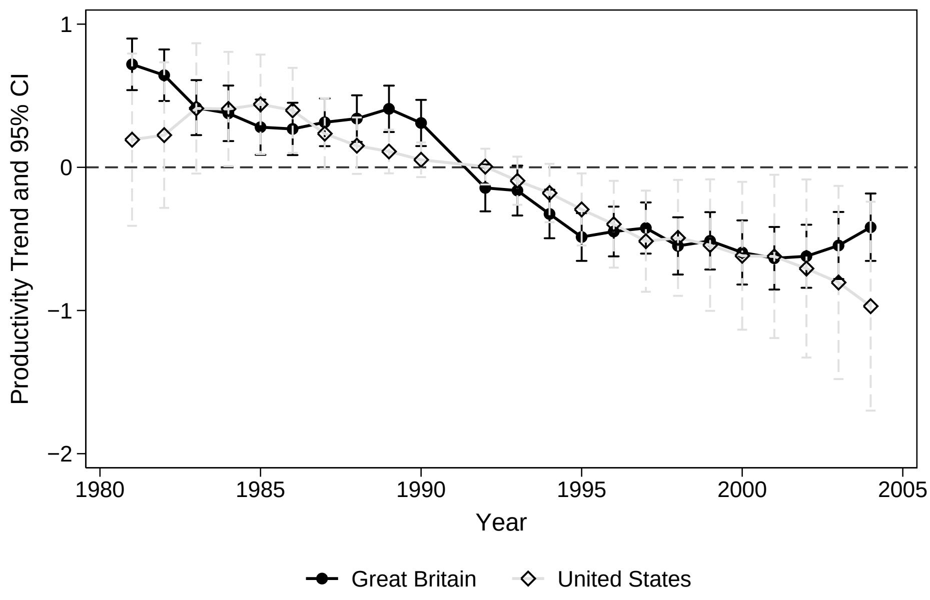
Table 2: Comparison of Means

	US	GB	$Difference$ ($t-statistic$)
Fuel (mtce)	1.19	2.11	0.92^{***}
			(0.13)
Output (net GWh)	3204.70	5401.54	2196.84^{***}
			(338.08)
Capacity (net MW)	604.42	1178.32	573.90^{***}
			(56.31)
Load factor	0.59	0.47	-0.12^{***}
			(0.01)
Wage (pound, gross weekly)	481.72	242.23	-239.49^{***}
			(7.63)
Age (years)	24.17	24.56	0.39
			(0.71)

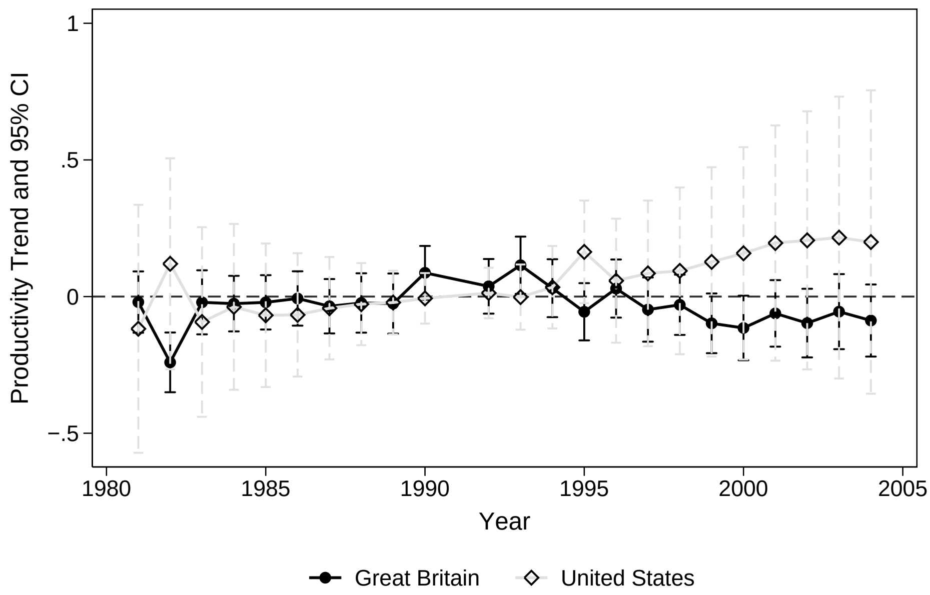
6. Results

Our difference-in-difference identification strategy assumes that factor productivities for the treatment and control groups follow a common trend. To investigate whether this assumption holds we estimate our two input demand equations without regime indicators but including group specific year fixed effects (with base year 1991). Figure 2 plots the estimated year effects which give the input productivities relative to the base year. We see in the upper panel that for labour the productivity trends are similar for the two groups. We also see that there is a clear productivity increase in GB in 1990. In the bottom panel we see that the pre-privatisation productivity trends are similar for fuel, too.[8] For fuel, productivity breaks are not so apparent but probably GB's fuel productivity decreased in 1989 and increased in 1993. The following regression analysis that models breaks explicitly, largely confirms these observations.

[8]These findings also corroborate the common trends assumption made by Newbery and Pollitt (1997).



(a) Labour



(b) Fuel

Figure 2: Trend comparison

Notes: The graphs plot the coefficient estimates and their 95% confidence intervals for group specific fixed effects (relative to the base year 1991 by country). Great Britain is the treatment group and the United States is the control group. A positive (negative) estimate represents a decrease (increase) in productivity relative to the base year.

Now we present the results for the estimate of the break date and the corresponding estimate of the effect size, i.e. the input efficiency difference between any two regimes as separated by the break date. As we estimated input demands, a positive (negative) intercept change implies a decrease (increase) in input efficiency. Following our theoretical model we estimated breaks separately for each input: labour and fuel.

Figure 3a gives the results for the labour demand equation for the unbalanced sample. For each candidate break date that separates two regimes the graph plots the sum of squared residuals (left axis), where the minimum is our estimate for the primary break date. The graph also plots, for each possible break date, the estimated percentage effect for δ using the approximation $[\exp(\delta - 1)] \times 100$. The percentage effect is given for both the OLS and, allowing for the endogeneity of supply, the IV estimator. For the labour demand equation the F-statistic for the excluded instrument in the first stage is 6.88 (significant at a 1 per cent level).

The estimate of the primary break date is the year of R&P, 1991. This estimate is robust to refinement, i.e. the estimate is the same when we split the sample at the secondary break date and repeat the search. This estimate of the break date is subject to uncertainty; Bai (2010) showed that the 95 per cent confidence interval for the break date (as well as for all dates below) is 5 periods, that is 2.5 years either side of the break. The break in 1991 is associated with an increase of labour productivity of about 50 per cent (irrespective of whether supply is considered exogenous or not).

Table 4 in the Appendix reports all coefficient estimates for the labour demand equation for break date 1991. The statistical significance of the treatment effect (POST1991) is high, which is relevant only assuming the break date is known. As we assume that the true break date is *unknown* we are unable to test whether a break exists at all or not. Note that the Cobb-Douglas form assumes that the supply coefficient is 1, which is clearly not the case although the IV estimate is much higher (but also estimated less precisely) than the OLS estimate. The higher IV estimate implies a negative correlation between input shocks and output. For instance, a output reducing plant break down might require more labour. The same negative correlation holds for the fuel demand equation below.

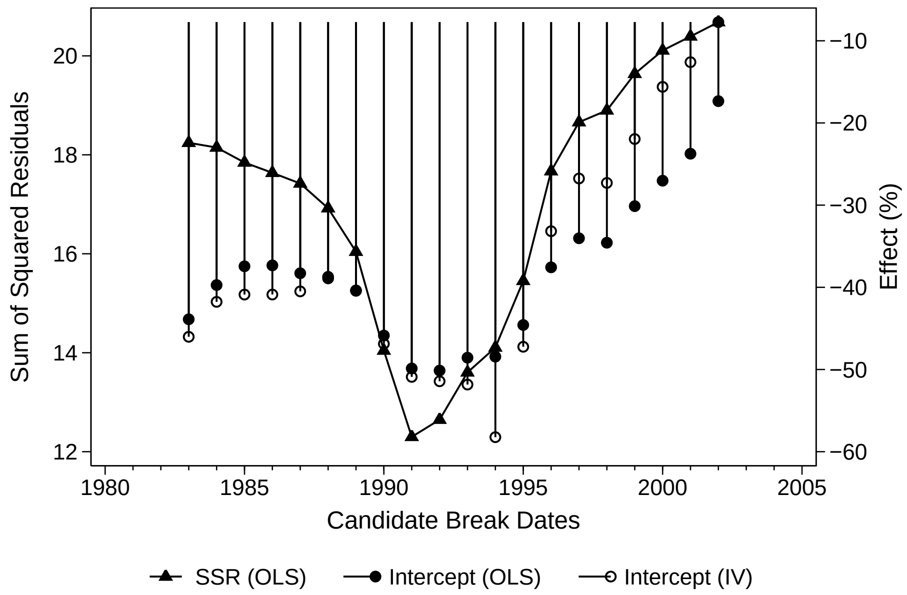
These results might be driven by plant selection, in particular the exit of low productivity plants after R&P. Figure 3b presents the labour productivity results for a balanced sample (remember our definition of “balanced” above). We see that for the year 1991 the effect size is not much lower, which suggest that the effect is not mainly driven by selection. But instead of investigating the robustness of the effect size only we repeat the entire analysis for the balanced sample. The estimate of the break date is 1992 (one year later) and the estimated effect size is lower, especially for the IV estimator (about 38 per cent).

Our hypothesis is that there are two breaks due to the differential timing of privatisation and competition. To estimate a secondary break date we split the sample at the primary break date.

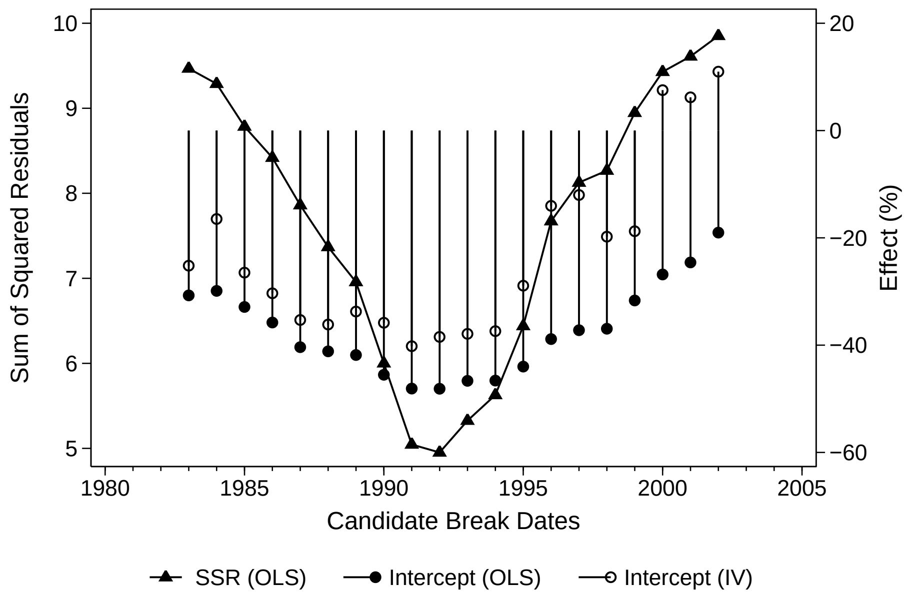
Across the two resulting sub-samples the break date with the largest drop in SSR is the estimate of the secondary break. For the unbalanced sample that is 1994, one year before plants where fully privatised (see Table 3). Note that using the 5 year confidence interval we cannot statistically tell the primary and secondary break dates apart. This secondary break is associated with a 25 per cent increase in labour productivity (21% for the IV estimator) suggesting that the overall increase in labour productivity of 50 per cent occurred over a number of years. The secondary break date for the balanced sample is 1990 associated with an productivity increase of about 19 per cent. Table 3 at the end of this section summarises all the estimates for the break dates and associated effects.

What do these breaks tell us about the likely effect of ownership change and competition on labour productivity? There is clear evidence for a break associated with the initial restructuring and privatisation in 1990/1991. In accordance with the pervasive nature of GB reform, compared for instance to the US reform, the labour productivity effect is large but consistent with firm aggregate data (Newbery and Pollitt, 1997, Table 1). However, the large size suggests that the incentive effect is not the only relevant mechanism. There are other mechanism whose effects we cannot separate. For instance, it is likely that management's objective changed from labour input maximisation to profit maximisation. Also, with privatisation union power was reduced making it much easier to lay off staff.[9] And, our measure of head count might underestimate the true labour input after privatisation. For instance, working hours might have increased and more outsourced labour employed. None of our estimated break dates for labour productivity coincides with changes to market structure and competition. But when we assume that the date for increased competition is 1999, the year when fuel productivity increased (see below), we find a positive effect on labour productivity. Nevertheless, for labour any competition effect is dominated by the privatisation effect.

[9]From anecdotes we also know that voluntary redundancy and early retirement arrangements were very generous. But these should be included in the cost-benefit calculation of the firm.



(a) Unbalanced Panel



(b) Balanced Panel

Figure 3: Test for Structural Change: Labour Demand

Notes: The graphs plot the Sum of Squared Residuals (SSR) for the labour demand equation including a break between two regimes on the left axis. The droplines represent the (OLS and IV) point estimate of the break indicator as an estimate of the average change in input efficiency between any two regimes. The SSR and coefficient estimates are plotted over the different candidate break dates.

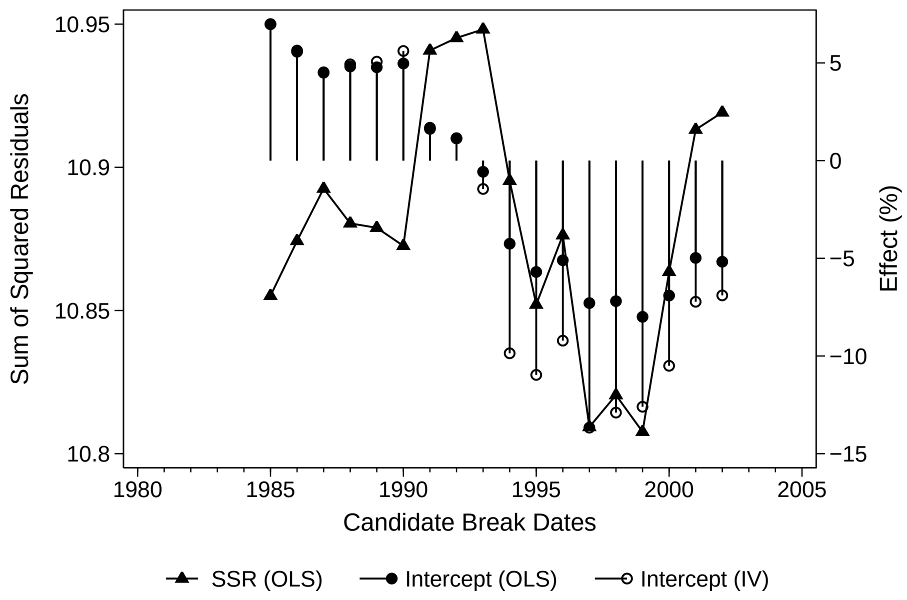
Next, we repeat the analysis for fuel demand. Figure 4a gives the results for the unbalanced sample. Note that for fuel the break window starts 2 years later in 1985. The reason is that an initial analysis suggested both 1983 and 1984 as primary break dates. But as both dates are associated with decreases in fuel efficiency which are unlikely to be related to the reforms of the 1990s we decided to ignore them and start the analysis later. The estimate of the primary break date is the year 1999, the year when market concentration decreased, retail was fully deregulated, and new market rules announced. The break is associated with a 7.2 per cent increase in fuel efficiency. There is doubt about this break date, because it is not robust to refinement. Refinement suggests that the primary break date is 1995, the year of full privatisation and the year before the first round of divestitures. Also unlike for labour the IV estimator produces a much larger effect: about 13 per cent productivity increase. For the fuel demand equation the F-statistic for the excluded instrument in the first stage is 17.44 (significant at a 0.1 per cent level). The significantly higher supply elasticity for the IV estimator than the OLS estimator in Table 5 suggests that the IV estimate might be closer to the true effect. This is as expected, because fuel input decisions are much more likely to be affected by unobserved productivity shocks than labour input decisions.

For the balanced sample in Figure 4b we see that the effect size in 1999 is slightly smaller at 10 per cent, again suggesting that the effect is not mainly due to selection. However, when we also re-estimate the break date, the year is 1995, suggesting there might be a selection effect after all. The break is associated with a fuel productivity increase of 7.5 and 14.7 per cent for the OLS and IV estimators, respectively. But again this primary break is not robust to refinement. Refinement suggests that the primary break is 1990 associated with a *decrease* in fuel productivity of about 6 per cent.

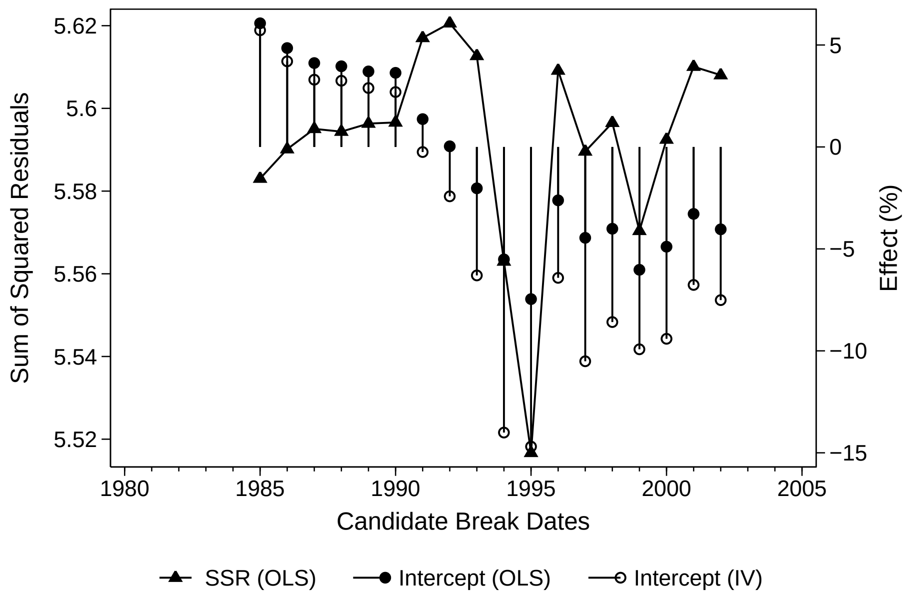
For the unbalanced panel the estimate of the secondary break is the year 1988, the year restructuring began. Surprisingly, it is associated with a *decrease* in fuel productivity between 5 and 6 per cent depending on the estimator. One possible explanation is that this break is an artifact of the drop in the number of GB observations in 1988 (Dukpa Kim, private communication). That we do not find the same break for our balanced sample is evidence for this hypothesis. A possible substantial explanation is that the decrease reflects a loss of economies of scope with the beginning of restructuring. Last, given the uncertainty around the timing of the break, it is possible that the newly created private generators' strategic interaction reduced productivity due to frequent output changes. But we discount this last possible explanation. For the balanced sample the estimate of the secondary break date is 1999, associated with productivity increases of 8.9 and 15.4 per cent for the OLS and IV estimators, respectively.

Before interpreting these results for fuel productivity we should note that overall the results are much less robust than for labour. We believe this reflects the fact that fuel productivity is more affected by the competition related reforms. As competition was enhanced through several smaller reform steps it might be more difficult to pin-point two break dates. Ignoring robustness, what do these breaks tell us about the likely effects of ownership change and competition on fuel productivity? Unlike for labour, privatisation did not increase fuel productivity. Probably this is because the CEGB valued engineering excellence and in particular fuel efficiency. Moreover, there is some evidence fuel productivity decreased when restructuring began before privatisation. We believe this reflects a loss of economies of scope.

We find evidence that fuel productivity increased around the dates when competition was strengthened in 1995 and 1999. The competition effect is strong, up to 15 per cent, and much higher than the effect found by for instance Fabrizio et al. (2007). One possible explanation is that for our quasi-experiment it is less likely that there is spill over from the treatment to the control group. Also, in the US the switch was from a (well) regulated regime to competition, whereas in our case the switch was from an unregulated duopoly to effective competition. That the breaks coincide with both rounds of divestiture suggests that market concentration is an important mechanism, but ultimately we cannot separate this mechanism from the deregulation of retail access in 1999 or the change in market rules in 2001.



(a) Unbalanced Panel



(b) Balanced Panel

Figure 4: Test for Structural Change: Fuel Demand

Notes: The graphs plot the Sum of Squared Residuals (SSR) for the fuel demand equation including a break between two regimes on the left axis. The droplines represent the (OLS and IV) point estimate of the break indicator as an estimate of the average change in input efficiency between any two regimes. The SSR and coefficient estimates are plotted over the different candidate break dates.

Table 3: Results Overview

Input	Unbalanced panel						Balanced panel					
	Primary break			Secondary break			Primary break			Secondary break		
	OLS		IV	OLS		IV	OLS		IV	OLS		IV
	Year	*Effect*	*Size*	*Year*	*Size*	*Size*	*Year*	*Size*	*Size*	*Year*	*Size*	*Size*
Labour	1991	−49.87	−50.90	1994	−24.67	−20.98	1992	−48.12	−38.46	1990	−18.86	−17.64
Fuel	1999	−7.19	−12.90	1988	5.90	5.24	1995	−7.46	−14.70	1999	−8.85	−15.38

Notes: This Table gives all the break date estimates and the estimated of the associated effect sizes. Effects are approximated using $[\exp(\delta - 1)] \times 100$.

7. Conclusion

Ours is the first study to analyse the effects of GB's electricity industry reforms on plant-level productivity. And, it is one of a handful of studies that look at the causal effects of privatisation and competition on plant-level productivity. Agency theories predict that a change from public to private ownership and increased competition improve firm performance via improved managerial incentives and effort. The reason is that without private ownership and competition employed managers can appropriate rent in the form of slack. Newbery and Pollitt (1997) also highlight that technical efficiency gains played a much larger role than allocative efficiency gains in the welfare effects of the GB reform.

We collected plant-level data on output and input quantities for Great Britain and the United States, our control group. We derived input demand functions for labour and fuel to be able to identify independent effects for these two short-run inputs. And indeed, the timing and the effect sizes are different. We employed a standard difference-in-difference approach to estimate the break dates and the associated effects. Finally, we mapped the estimated break dates to the history of industry reforms. Instead of assuming that break dates are known we search for them, because the economic effect might occur at a date different from the recorded reform date. If firm behaviour includes anticipation (as in models of dynamic collusion) effects might be leading. On the other hand, if adjustment or learning is necessary effects might be lagging. Additionally, reform might consist of several small steps, like the introduction of effective competition throughout the 1990s. Searching for a break might result in a better estimate of the true overall effect than picking one or several of the competing reform dates.

We found that at privatisation labour productivity increased dramatically but the effect is not predominantly driven by selection. The large effect makes it unlikely that improved management incentives are the only mechanism. Although, we cannot identify the separate mechanisms the previous literature suggests that two other mechanism were important. Privatisation changed management's (and owners') objectives. It is conceivable that under public ownership labour input maximisation was part of management's objectives whereas after privatisation the most important objective was economic efficiency. Also, privatisation coincided with the lessening of union power, actually it was one of the objectives of privatisation. Labour productivity gains due to lower union power are only an indirect effect of privatisation. When allowing two breaks we find no change in labour productivity associated with steps to increase competition in the second half of the 1990s. However, when we assume the reform dates related to competition are known we do find positive effects. It seems that competition has the expected effect but it is strongly dominated by the effect of privatisation in our sample.

Unlike for labour, privatisation did not increase fuel productivity. Actually, we find a decrease in fuel productivity when the industry was restructured. It seems that public owners encouraged fuel productivity through a culture that valued engineering excellence (Newbery, 1995). This was expressed by the practice to switch to a fuel efficiency based merit order during fuel emergencies. The actual decrease in fuel productivity might be explained by a loss of economies of scope after restructuring. Also, improving economic efficiency was maybe not the first objective of R&P. Political objectives like lessening of union power and widening share ownership were probably equally important (Green and Haskel, 2004, p. 65). The onset of effective competition in the second half of the 1990s is associated with strong increases in fuel efficiency. Although we cannot distinguish between the different mechanisms that increased competition, the timing of the effects provides some evidence that divestitures were important. When controlling for entry and exit effects are somewhat smaller, but generally effects do not seem to be driven by survival.

Privatisation and competition increased productivity as predicted by theories of agency. But if competition is weak foregone economies of scope might dominate and public owners might be able to substitute a culture of excellence (combined with internal competition) for market competition.

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A. Tables

Table 4: Labour Input Demand Estimates for Break Year 1991

	(1)	(2)
	OLS	*IV*
ln(NET GWH)	0.121**	0.629
	[0.00]	[0.10]
ln(CAP)	0.830***	0.406
	[0.00]	[0.23]
LF	0.114	−1.246
	[0.34]	[0.23]
AGE	0.086**	0.079*
	[0.00]	[0.03]
ln(WAGE)	−0.173**	−0.152*
	[0.01]	[0.05]
FGD	−0.120	−0.222*
	[0.06]	[0.04]
POST1991	−49.869***	−50.896***
	[0.00]	[0.00]
Constant	0.056	−1.715
	[0.94]	[0.10]
Plant Fixed Effects	*Yes*	*Yes*
Year Fixed Effects	*Yes*	*Yes*
Observations	535	535
R^2	0.970	

p-values in brackets

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The dependent variable is the log of labour input. The IV estimator is 2-stage least squares within.

Table 5: Fuel Input Demand Estimates for Break Year 1999

	(1)	(2)
	OLS	*IV*
ln(NET GWH)	0.944***	1.206***
	[0.00]	[0.00]
ln(CAP)	−0.121*	−0.396
	[0.02]	[0.09]
LF	−0.162*	−0.889
	[0.01]	[0.14]
AGE	−0.007	−0.010
	[0.73]	[0.67]
FGD	0.060	0.036
	[0.09]	[0.40]
POST1999	−7.993**	−12.598**
	[0.00]	[0.01]
Constant	−6.511***	−6.284***
	[0.00]	[0.00]
Plant Fixed Effects	*Yes*	*Yes*
Year Fixed Effects	*Yes*	*Yes*
Observations	909	909
R^2	0.991	

p-values in brackets

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The dependent variable is the log of fuel input. The IV estimator is 2-stage least squares within.

B. Variable construction

Where possible we filled missing values as follows. Supply is derived from CO2 emissions using generic efficiency measures where plant specific efficiency measures are not available. The formula is:

$$GWh(Supply) = kt(CO2) * EF * Eff, \tag{6}$$

where EF is the emissions factor, Eff is thermal efficiency where the values are either plant specific or if unavailable generic value, and kt stands for thousand tonnes.

Fuel input is derived from CO2. Note that we do not derive fuel input from supply directly. The formula is:

$$kt(Fuel) = kt(CO2) * EF * CF * CV, \tag{7}$$

where CF is a fuel dependent conversion factor and CV is the heat content.