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Abstract

The case for including agglomeration benefits within transport appraisal rests on an assumed causality between access to economic mass and productivity. Such causality is difficult to establish empirically because estimates may be subject to sources of bias from endogeneity and confounding. They may also be sensitive to the range of sample variance in agglomeration being used. The purpose of this paper is to demonstrate some of the key difficulties that the researcher faces in estimating agglomeration economies and to show how these can affect the calculation of agglomeration benefits for the appraisal of transport projects. The results show a high degree of sensitivity to treatment for unobserved heterogeneity and to differences in the sample variance of agglomeration. A key conclusion is that we are unable to distinguish agglomeration effects from other potential explanations for productivity increases, most notably functional heterogeneity. Consequently, the agglomeration effects of transport investments cannot be interpreted causally.

Keywords: Agglomeration, transport, causality, heterogeneity, confounding.

JEL classification: R42, R12.

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I Introduction

Recent thinking on the appraisal of transport infrastructure projects shows an increased interest in the "wider economic benefits" of transport infrastructure. Improved transport infrastructure is thought to generate benefits that standard cost-benefit analysis overlooks by assuming perfect competition and absence of market failures in the broader economy. Reliance on such assumptions evidently means that the model underlying standard cost-benefit analysis is an approximation to real-world conditions. Whether the approximation is sufficiently accurate has been called into question with emerging evidence on the size of agglomeration economies.

Agglomeration economies exist when the spatial concentration of economic activity gives rise to increasing returns in production that are external to the market (cf. e.g. Fujita et al. 1999; Fujita and Thisse 2002; Duranton and Puga 2004), and they feature prominently in the list of wider economic benefits from transport infrastructure investment. If more or better transport infrastructure boosts agglomeration economies through reductions in the generalized price of travel, these benefits are over and above those contained in a standard cost-benefit analysis, so they should be accounted for separately in order to arrive at an accurate prioritization of transport projects and at decision on the total amount of transport infrastructure funding. Venables (2007) and the Eddington Study (Eddington 2006) are well-known examples of this line of reasoning. However, the case for inclusion of agglomeration benefits in routine project appraisal is not iron-clad (see e.g. ITF-OECD 2008), because of the uncertainty over the transferability of empirical evidence and because the increased burden placed upon assessment may jeopardize its timely delivery.

This paper further investigates the case for including agglomeration economies in project appraisal. We do not question the conceptual argument, but ask if available empirical measurements of agglomeration economies and their relation to transport infrastructure are suitable for inclusion in applied project assessment. To obtain a measure of agglomeration economies, econometric analysis focuses on estimating the elasticity of productivity with respect to firms access to economic mass (cf. Rosenthal and Strange 2004, Melo et al. 2009, for reviews), where access is co-determined by transport facilities. However, reverse causation, unmeasured confounding factors, and sensitivity to the range of sample variance pose problems for estimation. The impact of these potential problems is illustrated in this paper.

We find that estimates of the elasticity are highly dependent on the econometric specification. In particular, including area- or firm-fixed effects leads to lower elasticity-numbers or even to estimates that are indistinguishable from zero. This essentially means that we cannot distinguish agglomeration effects from other potential explanations for productivity increases, most notably functional heterogeneity. Hence, the econometric tests do not say that agglomeration economies are weak or inexistent, but that other factors affecting productivity may be picked up by standard estimates of the agglomeration elasticity. This suggests that more effort is needed on identifying these other factors before the estimated elasticities are suitable for use in applied project appraisal, unless one believes that these other factors always occur simultaneously with agglomeration economies. Either way, obtaining more reliable estimates is as much conditional on further theoretical refinements as on better statistical analysis. Our conclusion is strengthened by the fact that other factors influencing productivity are robustly measured across specifications; while the effect of accessibility to economic mass is not.

The paper is structured as follows. Section 2 provides a brief review of the case for including agglomeration benefits within transport appraisal and illustrates how agglomeration elasticities can be used to make the relevant calculations. It also explains the key challenges faced in attaining a valid estimate of agglomeration economies. Section 3 provides a description of the data available for estimation and sets out the measure of agglomeration used. Section 4 outlines our empirical model and discusses assumptions underlying the different estimators we apply. Results are presented in section 5. Section 6 interprets the results and discusses implications for the assessment of agglomeration benefits within transport appraisal. Conclusions are drawn in the final section

II Agglomeration and transport investment: measurement and estimation issues

Agglomeration is a characteristic of the environment in which a firm exists. It is determined by the level of access that firms have to 'economic mass', that is, to markets for inputs and outputs and to factor markets[1]. More agglomeration is a good thing in so far as it generates

[1] A distinction is sometime made between intra-industry and inter-industry agglomeration, referred to respectively as localization and urbanization. It can be argued that the basic mechanisms underpinning the advantages derived from agglomeration are common to each (Duranton and Puga 2004). Since changes to the transport system affect accessibility in general, and not in some industry specific way, it is sufficient here to consider the concept of agglomeration in general as being derived from either broad class of externality.

positive externalities. The sources of these externalities include increased opportunities for labour market pooling, scope for industry specialization, greater efficiency in knowledge or technology sharing, and improved opportunities for input-output association[2]. Theory tells us that agglomeration economies will be manifest in tangible benefits such as lower average costs for firms and higher productivity. Thus, for some firm producing output y using a vector of inputs X and experiencing level of agglomeration A, we can define a general production function

$$y = f(X, A) \tag{1}$$

in which we hypothesise that $\partial \log y / \partial \log A = \eta_{y,A}$ will be positive.

Venables (2007) develops a theoretical model that links agglomeration and transport provision. The argument, as outlined in the introduction is straightforward: transport investment increases the access that firms have to economic mass, which, if agglomeration economies exist, induces a source of increasing returns that is not captured in a standard transport appraisal. Venables goes on to show that we can attain an estimate of the 'agglomeration benefits' of transport investment if we know: a) the change in access to economic mass that will result from making some transport intervention; and, b) the amount by which productivity will rise in response to an increase in agglomeration (i.e. $\eta_{y,A}$).

The UK Department for Transport (DfT) have requested that agglomeration benefits be assessed as an additional component of transport scheme appraisal (e.g. DfT 2005). If we can estimate the level of agglomeration after some transport intervention of size $b - a$ has been made, then the associated productivity change can in general be calculated using

$$\Delta y = y_b - y_a = \left[\left(\frac{A_b}{A_a} \right)^{\eta_{y,A}} - 1 \right] y_a. \tag{2}$$

with the unknown y_b given by the compound growth expression

$$y_b = y_a e^{\eta_{y,A} [\log A_b - \log A_a]}. \tag{3}$$

Given a point elasticity, this expression provides a reasonably consistent calculation for large changes in agglomeration as for small. To evaluate (5) we therefore need an estimate of A_b and of the elasticity $\eta_{y,A}$. Methods for estimation of A_b are well developed since this value is required for conventional calculations of the value of travel time savings. The elasticity

[2] It is possible that the level of agglomeration can exceed some 'optimal' amount. See for instance Graham 2007.

of productivity with respect to agglomeration is a relatively new parameter in the field of transport appraisal and its estimation forms the subject of this paper.

Several empirical studies in the urban economics literature over the last 40 years have explored the relationship between agglomeration and productivity. Most of these have been concerned with the effects of agglomeration on manufacturing industries and have used measures of city and industry size to represent urban and industrial agglomeration. Generally, urban scale or density is found to have a positive and significant effect on productivity with agglomeration elasticities for manufacturing industries typically found to be somewhere between 0.02 and 0.10 (for reviews see Rosenthal and Strange 2004, Eberts and McMillen 1999).

There are three issues surrounding the estimation of agglomeration economies that could hinder a causal interpretation in the context of transport appraisal.

i **Potential for reverse causality** - it is not clear that the direction of causality should run strictly from agglomeration to productivity. It seems reasonable, for instance, that high productivity could induce higher levels of agglomeration if mobile factors move to the most productive locations. Existing urban economic theory has paid little attention to the direction of causality that runs from productivity to agglomeration. Consequently, there is no well developed framework upon which to derive a system of structural equations for estimation. But if reverse causality does exist then it implies estimation with endogenous regressors which could give biased and inconsistent estimates of the agglomeration-productivity effect. This issue is clearly of first-order importance for the evaluation of transport investments. If agglomeration economies are endogenous, if the direction of causality runs substantially the opposite way proposed by theory, then we may not realise the benefits we expect by increasing accessibility through investment.

ii **Unobserved confounders** - empirical work on agglomeration must acknowledge the potential for 'confounding' which can inhibit us from identifying the 'true' productivity effect of changing agglomeration alone. When we measure variance in 'agglomeration' we need to be clear about what this actually represents. Does it capture the effect of access to economic mass alone, or could it also include other omitted variables or 'confounding' effects which are systematically correlated with the observed agglomeration-productivity relationship? The most commonly cited source of confounding in the context of agglomeration is unobserved functional heterogeneity (or variance in labour 'quality'). The argument here is that the occupations performed by workers tend to vary systematically with city size, the distribution being skewed such that higher-productivity jobs tend to be found disproportionally in the most urbanised locations. There is quite a bit of empirical evidence supporting this effect (e.g. Duranton and Puga 2005, Combes et al. 2008b). It is therefore very important to have some idea about the extent to which identification of the agglomeration elasticity is inhibited by confounding, because we know that the case for assessing the agglomeration benefits of transport rests on an assumed causality between productivity and access to economic mass, but not necessarily with the unobserved confounders.

iii **Dependency on range of sample variance in agglomeration** - agglomeration elasticities are typically estimated using observations from across the entire urban system. In others words, they are point estimates which tell us on average how productivity changes with agglomeration for the whole economy. For these point elasticity estimates to be relevant for the assessment of agglomeration benefits within transport appraisal, however, they have to satisfy two conditions: 1) they must be independent of the magnitude of change in agglomeration; and 2) they must be constant over levels of agglomeration. So we require that the agglomeration elasticities be reasonably stable across population sub-samples. This is important, because changes to the transport system will typically result in only minor shifts in access to economic mass rather than trigger an aggregate shift in the level of agglomeration experienced by a city or an industry. So we need to know whether small changes in agglomeration have the same proportional effect on productivity as large changes do.

The first two issues outlined above, endogeneity and confounding, have received attention in the recent empirical literature (e.g. Ciccone and Hall 1996; Ciccone 2002, Rosenthal and Strange 2004, Combes et al. 2008a, Duranton and Puga 2004, and Rice et al. 2006), but have proven difficult to deal with in a satisfactory way. This is largely due to problems in finding relevant and exogenous instruments for agglomeration, but also to an absence of measures that can adequately represent any functional heterogeneity that is distributed systematically with levels of agglomeration. The third issue, concerning potential heterogeneity within sub-samples, has to our knowledge received no prior attention in empirical work other than in attempts to estimate diminishing returns to agglomeration through a quadratic specification of the agglomeration variable (e.g. Graham 2007). However, this is a quite separate issue. Our concern here is whether small changes in agglomeration have the same proportionate impact on productivity as large changes appear to, not whether large changes have different effects depending on the initial level of agglomeration.

Our objective in this paper is to test the stability of agglomeration estimates to treatment for endogeneity and confounding and to differences in sampling variance. The underlying question is whether the type of elasticities we typically find reported in the literature can be used to provide a reliable guide to the agglomeration effects that may arise from transport investments. In the next section we describe the data available for estimation and the measure used to represent agglomeration.

III Data and the measurement of agglomeration

We have panel data available for estimation on the production characteristics of registered UK companies in 2 digit sectors over the period 1995 to 2004. Under UK legislation each registered company is required to provide accounting and other data about their operations to an executive agency of the Department of Trade and Industry know as Companies House. These data are made available in a commercial software package called Financial Analysis Made Easy (FAME), which is produced jointly by Jordans and Bureau Van Dijk (BVD 2003). The production data relate to companies, some of which have plants in a number of different locations. For our analysis it is important that the productivity measures relate to production at one location. It is, however, possible to identify and remove multi-plant firms from the sample because they report more than one trading address.

The FAME data are available for a number of years, although the reporting for individual firms is irregular. We have derived an unbalanced panel of 17,668 firms over 10 years. To ensure that the temporal dimension of the data is sufficient for estimation our sample include only firms which have at least five years of available data. The basic input data we have on each firm includes measures of capital stock and the number of employees. Capital stock is the value of assets possessed by the firm and includes 'fixed assets' such as the depreciated value of buildings, plant, machinery and equipment; 'current assets' such as stocks and various debts owed to the company; and 'current liabilities' or the amount owed by the company as a result of normal trading. Sales is used as a proxy for output.

The FAME data provide us with an extensive sample of variance in productivity of firms within different industries across the UK. We have the full postcode information of each firm in the sample. To construct measures of the agglomeration 'experienced' by each firm we use employment data at the postcode sector (PCS) level taken from the Annual Business Inquiry[3].

To represent agglomeration we use a 'market potential' measure

$$A_i = \frac{E_i}{d_i} + \sum_j \frac{E_j}{d_{ij}} \tag{4}$$

where E_i is employment in PCS i, d_{ij} is the distance between PCS i and j, and d_i is an approximation to the internal distance of PCS i[4]. In fact, the geographic information we have relates to full postcodes rather than to truncated PCSs. To find the centroid of each PCS we have taken the average of all the full postcode x and y coordinates. Applying Pythagoras proposition we use these centroids to calculate distances between PCSs. To obtain an approximation of the internal distance of each PCS we take the average of the distances between each pair of full postcodes that are contained within that PCS.

We estimate agglomeration separately for different industry groups. Table 1 below shows a breakdown of the sample of firms by industry, listing the number of firms and the total number of observations. To allow for a concise presentation of results we estimate using samples aggregated into five industry groups: manufacturing (SIC 14-40), construction (SIC 45), wholesale & retail (SIC 50-52), transport & communications (SIC 60-64), and business services (SIC 65-75). Within each industry group we allow for unobserved heterogeneity associated with distinct industrial activities by including a set of dummy variables corresponding to 2 digit industries.

IV Model and estimation

To analyse the relationship between agglomeration and productivity we specify a production function for the ith firm ($i = 1, ..., N$) producing output Y at time t ($t = 1, ..., T$),

$$\log Y_{it} = \beta_L \log L_{it} + \beta_K \log K_{it} + \beta_A \log A_{it} + \eta_t + f_i + \varepsilon_{it}. \tag{5}$$

[3]There are 11,344 postcode sectors defined in our data for which there are extensive detailed employment data that allow us to construct measures of access to economic mass. The Annual Business Inquiry (ABI) is the official census of employment for Britain.

[4]This measure, which is the most common form used in empirical work on agglomeration, is essentially akin to the traditional measure of accessibility used in transport analysis.

Table 1: Data description: firms and no. of observations by industry.

	firms	obs
manufacturing (SIC 15-40)	4,661	35,686
construction (SIC 45)	1,472	10,442
wholesale & retail (SIC 50-55)	3,545	25,956
transport & communications (SIC 60-64)	1,081	7,834
business services (SIC 65-75)	6,909	48,353
Total	17,668	128,2713

The firm uses labour (L) and capital (K) inputs and is located in an environment with a level of agglomeration measured by A. The term η_t is a time specific effect that allows for unobserved shocks which are common across firms and f_i represents unobserved individual time-invariant heterogeneity. We introduce dynamics by specifying a potentially autoregressive productivity shock $\varepsilon_{it} = \rho\varepsilon_{it-1} + \nu_{it}$, with $|\rho| < 1$ and $\nu_{it} \sim IID\left(0, \sigma^2\right)$ representing serially uncorrelated white noise error. The fundamental problem presented in estimating (5) is that due to the potential endogeneity of agglomeration and of the production function itself (i.e. endogenous inputs), all regressors are potentially correlated with the unobserved individual effects f_i.

To address this problem we estimate equation (5) in a number of different ways. First, assuming no correlation between the regressors and the individual effects we apply a feasible GLS estimator which allows for serial autocorrelation and random individual effects. Estimates obtained using this approach make no attempt to control for endogeneity or confounding, and so provide a sort of base case against which we can compare our other results.

To address the potential problems of endogeneity, following Blundell and Bond (2000), we can also specify (5) as an ADL(1,1) dynamic model

$$\begin{aligned}\log Y_{it} = \rho \log Y_{it-1} + \beta_L \log L_{it} - \rho\beta_L \log L_{it-1} + \beta_K \log K_{it} - \rho\beta_K \log K_{it-1} \\ +\beta_A \log A_{it} - \rho\beta_A \log A_{it-1} + (\eta_t - \rho\eta_{t-1}) + f_i (1 - \rho) + \varepsilon_{it}.\end{aligned} \tag{6}$$

In addition to potential endogeneity of the factor inputs and the level of agglomeration, this model also features correlation between the lagged endogenous term and the individual effects. When the assumption of zero conditional mean of the error term given the regressors is not satisfied, the GMM estimator provides a potential solution. The principle underpinning GMM is that given a set of instrumental variables which are correlated with the regressors but orthogonal to the errors, we can define and solve a set of moment conditions which will be satisfied at the true value of the parameters to be estimated. In the context of dynamic panel data models, the time series nature of the data is used to derive these instruments and establish the moment conditions (e.g. Arellano and Bond 1991; Arellano and Bover 1995; Blundell and Bond 1998).

For instance, taking differences of equation (6) to remove the individual effects, we then have $Y_{i,t-2}$ as an available instrument since it is correlated with $\Delta Y_{i,t-1}$ and, in the absence of serial autocorrelation, orthogonal to $\Delta\varepsilon_{it}$. Similarly, this can be done for any other endogenous explanatory variables in the model giving rise to an instrument matrix Z_i from which we can define the moment conditions for the *difference* GMM estimator

$$E\left[Z_i'\Delta\varepsilon_i\right] = 0 \tag{7}$$

where $\Delta\varepsilon_i = (\Delta\varepsilon_{i3}, \Delta\varepsilon_{i4}, ..., \Delta\varepsilon_{iT})'$.

When the data available for estimation are highly persistent, as is characteristic of our data, we can supplement the moment conditions given in (7) with the additional moment conditions

$$E\left[\Delta Z_{i,t-1}\left(f_i + \varepsilon_{it}\right)\right] = 0. \tag{8}$$

Using both sets of moments conditions together, (7) and (8), gives rise to the so called *system* GMM estimator with first-differenced and levels equations (e.g. Arellano and Bover 1995; Blundell and Bond 1998; Blundell and Bond 2000; Blundell et al. 2000). This estimator has been shown to offer much increased efficiency and less finite sample bias compared to the difference GMM estimator alone[5].

The consistency of GMM relies on the assumptions that there is no first-order serial autocorrelation in the errors of the level equation and that the instrument matrix is truly exogenous and therefore valid to define the moment conditions. The Arellano and Bond test for serial autocorrelation (Arellano and Bond 1991) tests the hypothesis that there is no second-order serial correlation in the first differenced residuals, which in turn implies that the errors from the levels equations are serially uncorrelated. The standard test for validity of the instrument matrix is the Hansen test of overidentifying restrictions. The assumption of exogeneity of the

[5] A good summary of this literature is given in Baltagi (2005). For a full discussion of GMM in the context of dynamic panel models see Arellano and Bover (1995), Arellano and Honore (2001), Blundell and Bond (1998), Blundell and Bond (2000), Blundell et al. (2000).

instruments implies that we have a set of moment conditions which will be satisfied at the true value of our parameter estimates. If the model is overidentified (i.e. if there are more moment conditions than there are parameters to be estimated) the GMM framework allows us to test the validity of the additional moment conditions (overidentifying restrictions) in terms of whether they are set close enough to zero at the optimal GMM parameter estimates. Essentially, this is akin to testing for correlation between the model residuals and a subset of the instruments used[6].

Another useful approach to the estimation of equation (5) is to apply feasible GLS to deal with temporal autocorrelation, but impose fixed rather than random firm level effects. The fixed effects (FE) estimator allows for differences in the mean of the Y_{it} series across firms and could therefore provide a way of capturing any unobserved heterogeneity in functional characteristics that we believe may be a confounder in the agglomeration productivity relationship. In addition, the FE approach identifies the parameters purely from within firm variation, or in other words, from the temporal rather than cross-sectional nature of the data. Since agglomeration is a highly persistent variable, this substantially restricts the sample variance and thus provides an explicit test of how small changes in agglomeration effect productivity.

Finally, as a kind of compromise between the FGLS RE and FGLS FE approaches we use another estimation method which imposes fixed effects at the level of 'area types' rather than firms. The logic in adopting this approach is as follows. We believe that there may be confounding factors which vary in some systematic way with the level of agglomeration. By introducing dummies based on a classification that defines broadly homogeneous area types, we condition the estimates on the mean values for each category to provide some correction for any confounding factors may vary across levels of the urban system. The UK Department for Transport has developed a useful classification of non-rural areas of the UK that we use to define our area types. This is shown in table 2 below.

If there are confounding factors, connected for instance to a skewed spatial distribution of occupations or skills, then we would expect these to be manifest in higher productivity across area types. Thus, the inclusion of area dummies provides a basic test for the presence of

[6] For the endogenous linear regression model $\mathbf{y} = \mathbf{X}\beta + \mathbf{e}$, with instrument matrix $\mathbf{Z}$, the two step GMM estimator is $\tilde{\beta} = [\mathbf{X}'\mathbf{Z}\hat{\mathbf{S}}^{-1}\mathbf{Z}'\mathbf{X}]^{-1}\mathbf{X}'\mathbf{Z}\hat{\mathbf{S}}^{-1}\mathbf{Z}'\mathbf{y}$, where $\hat{\mathbf{S}} = N^{-1}\sum_{i=1}^{N}\mathbf{Z}_\mathbf{i}'\hat{\mathbf{e}}_\mathbf{i}\hat{\mathbf{e}}_\mathbf{i}'\mathbf{Z}_\mathbf{i}$ with estimated residuals, $\hat{\mathbf{e}} = \mathbf{y} - \mathbf{X}\hat{\beta}$, is derived using a consistent estimate of β from some initial first stage estimation. With K parameters to be estimated and r instruments, there are $(r - K)$ overidentifying restrictions. The Hansen test of overidentifying restrictions is formed as $V = \left[\sum_{i=1}^{N}\tilde{\mathbf{e}}_\mathbf{i}'\mathbf{Z}_\mathbf{i}\right]\left(N\hat{\mathbf{S}}\right)^{-1}\left[\sum_{i=1}^{N}\mathbf{Z}_\mathbf{i}'\tilde{\mathbf{e}}_\mathbf{i}\right]$, where $\tilde{\mathbf{e}}_\mathbf{i} = \mathbf{y}_\mathbf{i} - \mathbf{Z}_\mathbf{i}'\tilde{\beta}$. The Hansen test has an approximate χ^2 distribution.

Table 2: Classification of area types in Britain.

		area type
1	National centre	Central London
2		Inner London
3		Outer London
4	Regional centres	Inner Conurbation
5		Outer Conurbation
6	Sub-Regional centres	Urban Big (pop > 250,000)
7		Urban Large (pop > 100,000)
8	Other urban centres	Urban Medium (pop > 25,000)
9		Urban small (pop > 10,000)

unobserved confounders, but allows us to retain degrees of freedom and does not involve the sacrifice in sampling variance required for the firm level FE approach.

V Results

Table 3 below shows production function estimates obtained using a feasible GLS random effects estimator with AR(1) errors (FGLS-RE), difference and system GMM (diff-GMM and sys-GMM), and feasible GLS but with firm level fixed effects (FGLS-FE). In some cases the FGLS-FE model failed with inclusion of all 2 digit industry dummies due to excessive collinearity. In these cases, we replace the fixed effects estimator with one based on first differences (FGLS-FD) which, like the FE estimator, is also consistent under the assumption of unobserved correlated individual effects and uses within firm variation for parameter estimation. We also found that the ADL(1,1) GMM specification given in (3) suffered from multicollinearity and so we instead opted for the ADL(1,0) partial adjustment model

$$\log Y_{it} = \rho \log Y_{it-1} + \beta_L \log L_{it} + \beta_K \log K_{it} + \beta_A \log A_{it} + \eta_t + f_i + \varepsilon_{it}, \tag{9}$$

which is a more parsimonious specification that still allows us to distinguish short from long run effects. Table 3 shows results for all sectors of the economy pooled and for five industry groups; manufacturing, construction, wholesale & retail, transport & communications, and business services. All models are estimated with a set of dummy variables at the 2 digit industry level.

Table 3: Production function estimates

	all industries				manufacturing				construction			
	FGLS-RE	diff-GMM[a]	sys-GMM	FGLS-FE	FGLS-RE	diff-GMM[a]	sys-GMM	FGLS-FD	FGLS-RE	diff-GMM	sys-GMM	FGLS-FE
$\log Y_{t-1}$	-	0.328**	0.670**		-	0.195***	0.301***	-	-	0.185**	0.451**	-
	-	0.031	0.03		-	(0.026)	(0.029)	-	-	(0.059)	(0.058)	-
$\log L_t$	0.700**	0.125**	0.269**	0.693**	0.674**	0.304**	0.470**	0.717**	0.649**	0.266**	0.468**	0.703**
	(0.003)	0.036	0.022	(0.004)	(0.005)	(0.050)	(0.035)	(0.006)	(0.010)	(0.083)	(0.060)	(0.014)
$\log K_t$	0.360**	0.372**	0.131**	0.285**	0.361**	0.388**	0.304**	0.285**	0.460**	0.610**	0.213**	0.401**
	(0.002)	0.19	0.018	(0.003)	(0.004)	(0.063)	(0.030)	(0.006)	(0.009)	(0.118)	(0.052)	(0.015)
$\log A_t$	0.105**	0.19	0.045**	0.058	0.077**	-0.193	0.061**	-0.132	0.095**	0.267	0.134**	-0.16266
	(0.008)	0.155	0.009	(0.042)	(0.013)	(0.336)	(0.024)	(0.082)	(0.034)	(1.233)	(0.051)	(0.200)
LR η_L	0.700	0.186	0.815	0.693	0.674	0.378	0.672	0.717	0.649	0.326	0.852	0.703
LR η_K	0.361	0.554	0.397	0.285	0.361	0.482	0.435	0.285	0.46	0.682	0.388	0.401
LR η_A	0.105	-	0.136	-	0.077	-	0.087	-	0.095	-	0.244	-
R^2	0.870	-	-	0.797	0.89	-	-	0.43	0.87	-	-	0.83
Baltagi-Wu LBI	1.650	-	-	1.644	1.59	-	-	2.44	1.811	-	-	1.811
AR(1)	-	0.000	0.000	-	-	0.000	0.000	-	-	0.000	0.000	-
AR(2)	-	0.249	0.069	-	-	0.184	0.475	-	-	0.073	0.022	-
Hansen	-	0.000	0.000	-	-	0.000	0.000	-	-	0.000	0.000	-
N	133,461	41,188	47,072	115,073	35,686	25,864	30,525	30,525	10,442	7,280	8,752	8,970

Notes: Numbers in parentheses are standard errors; FGLS-RE - random effects with AR(1) errors, diff-GMM - difference GMM, sys-GMM - system GMM, FGLS-FE - fixed effects with AR(1) errors, FGLS-FD - first differences with AR(1) errors; a - model rejects industry dummies due to multicollinearity; ** - significant at 1%, * - significant at 5%; LR η_L, LR η_K and LR η_A are the long run elasticities of labour, capital and agglomeration; LBI is the Baltagi-Wu test for serial autocorrelation; AR(1) and AR(2) are the Arrelano and Bond tests for first-order and second-order serial autocorrelation.

Table 3: (continued):Production function estimates

	wholesale & retail				transport & communications				business services			
	FGLS-RE	diff-GMM[a]	sys-GMM	FGLS-FD	FGLS-RE	diff-GMM[a]	sys-GMM	FGLS-FE	FGLS-RE	diff-GMM[a]	sys-GMM	FGLS-FE
$\log Y_{t-1}$	-	0.202**	0.498**	-	-	0.299**	0.527**	-	-	0.317**	0.376**	-
	-	(0.031)	(0.035)	-	-	(0.051)	(0.052)	-	-	(0.021)	(0.019)	-
$\log L_t$	0.710**	0.283**	0.293**	0.736**	0.725**	0.366**	0.278**	0.740**	0.713**	0.162**	0.447**	0.681**
	(0.006)	(0.060)	(0.037)	(0.008)	(0.012)	(0.129)	(0.061)	(0.048)	(0.005)	(0.052)	(0.023)	(0.008)
$\log K_t$	0.364**	0.520**	0.403**	0.260**	0.286**	0.190**	0.246**	0.266**	0.361**	0.415**	0.275**	0.318**
	(0.005)	(0.073)	(0.038)	(0.006)	(0.010)	(0.074)	(0.048)	(0.032)	(0.004)	(0.052)	(0.019)	(0.005)
$\log A_t$	0.064**	0.970**	0.009	0.025	0.116**	0.978	0.086*	0.345	0.127**	1.212**	0.095**	0.04
	(0.016)	(0.395)	(0.023)	(0.101)	(0.035)	(0.556)	(0.038)	(0.380)	(0.013)	(0.487)	(0.016)	(0.104)
LR η_L	0.710	0.355	0.584	0.736	0.725	0.522	0.588	0.740	0.713	0.237	0.716	0.681
LR η_K	0.364	0.652	0.803	0.26	0.286	0.271	0.520	0.266	0.361	0.277	0.441	0.318
LR η_A	0.064	1.216	-	-	0.116	-	0.182	-	0.127	-0.764	0.152	-
R^2	0.87	-	-	0.4145	0.83	-	-	0.36	0.84	-	-	0.29
Baltagi-Wu LBI	1.578	-	-	2.426	1.499	-	-	-	1.675	-	-	2.526
AR(1)	-	0.000	0.000	-	-	0.000	0.000	-	-	0.000	0.000	-
AR(2)	-	0.439	0.167	-	-	0.170	0.880	-	-	0.226	0.121	-
Hansen	-	0.000	0.000	-	-	0.000	0.000	-	-	0.000	0.000	-
N	25,956	18,346	21,891	21,891	7,834	5,546	6,627	6,627	48,353	33,414	40,323	40,323

Notes: Numbers in parentheses are standard errors; FGLS-RE - random effects with AR(1) errors, diff-GMM - difference GMM, sys-GMM - system GMM, FGLS-FE - fixed effects with AR(1) errors, FGLS-FD - first differences with AR(1) errors; a - model rejects industry dummies due to multicollinearity; ** - significant at 1%, * - significant at 5%; LR η_L, LR η_K and LR η_A are the long run elasticities of labour, capital and agglomeration; LBI is the Baltagi-Wu test for serial autocorrelation; AR(1) and AR(2) are the Arrelano and Bond tests for first-order and second-order serial autocorrelation.

We focus first on results from the FGLS-RE models, which provide the base against which we compare models that incorporate some explicit treatment for endogeneity and confounding, or that make use of different sampling variance in agglomeration. The R^2 values for the FGLS-RE models indicate reasonably high degrees of explanatory power with all values falling in the range 0.8 to 0.9. The Baltagi-Wu locally best invariant (LBI) statistic for serial autocorrelation, which is a suitable diagnostic for unbalanced panels (Baltagi and Wu 1999), rejects the null hypothesis of no first-order serial autocorrelation for all models. FGLS-RE estimates relating to the output elasticities and returns to scale (RTS) are broadly similar across industries and indicate labour share in the range of two-thirds to three-quarters, and constant or slightly increasing RTS.

For the economy as a whole and for all five industry groups we estimate positive and significant agglomeration economies using the FGLS-RE estimator as follows: all industries (0.105), manufacturing (0.077), construction (0.095), wholesale & retail (0.064), transport & communications (0.116), and business services (0.127). Thus, we find evidence of substantial agglomeration economies consistent with the orders of magnitude typically found in previous literature with the largest effects for business services (see for example Melo et al. 2009).

The rationale for the use of the dynamic panel GMM specifications is to provide instrumentation for endogenous regressors. For both the difference and system GMM models, the key diagnostic statistics are the tests for first and second order serial autocorrelation and the Hansen test for overidentifying restrictions. In all cases the GMM models shown in table pass the Arellano-Bond tests, AR(1) and AR(2), for autocorrelation in the errors of the levels equations. None of the models, however, pass the Hansen test of overidentifying restrictions. The failure of the models to pass the Hansen test indicates that the instrument matrix may not be not truly exogenous. The results on agglomeration for diff and sys-GMM do not generally correspond well. As mentioned in section 4 above, sys-GMM estimation should be less susceptible to finite sample bias if both sets of moment conditions are satisfied, and in fact the sys-GMM estimates do appear more plausible and are similar in the long run, though somewhat larger, to the FGLS-RE estimates for manufacturing (0.087), transport & communications (0.182), and business services (0.152).

This could be taken as evidence that the influence of endogeneity is small for these sectors, but equally, it could be indicative of weak instruments which tend to give estimates that are biased in the same direction as least squares[7]. There are in fact other problems which question the validity of dynamic panel GMM results. For instance, we find that estimates of the autoregressive parameters from the diff-GMM models are smaller than those obtained using pooled OLS. This suggests biased estimates which can result when the instruments are only weakly correlated with the endogenous regressors, or when the instruments themselves are not orthogonal. Furthermore, we also find that the estimates are highly sensitive to any changes in the lag structure used for instrumentation. A key problem with dynamic panel GMM specifications in our context is that the data on production and agglomeration are so highly persistent, in fact nearing unit root in AR(1) specifications as shown in table 4 below, that the problem of weak instrumentation can become extreme. For this reason, and given the failure of the models to pass the Hansen test, it is not possible to draw any substantive conclusions on the role of endogeneity from the GMM models.

Table 4: AR(1) specification for agglomeration, OLS and sys-GMM estimates.

	OLS	sys-GMM
manufacturing	1.002	0.997
construction	1.005	0.996
wholesale & retail	0.999	0.985
transport & communications	1.002	0.996
business services	1.000	0.998
all industries	1.000	0.995

We next turn to results from the FGLS-FE and FGLS-FD models. These are the models which provide consistent estimation under the assumptions of unobserved firm level heterogeneity which is correlated with the regressors, and which also on within group rather than between group variance. The first point of interest from these results is that estimates relating to the elasticities of labour and capital, and therefore to RTS, are very similar to those obtained using the FGLS-RE specification. Labour shares range from 0.68 to 0.74 with estimates of RTS all very close to 1.0, though in general slightly less than the FGLS-RE estimates. The second and key point of interest, however, is that we find no evidence of agglomeration economies for any of the five industries listed in the table. Thus, conditional on individual firm effects, there is no evidence that changes in accessibility over time have affected productivity for firms.

[7]Bound et al. (1995) provide some good examples of the problems associated with the use of inappropriate instruments.

Agglomeration economies effectively disappear.

Two possible factors that might explain this stark contrast between the RE and FE models are the treatment, or lack thereof, of unobserved heterogeneity (i.e. confounders); or the differences in sample variance between model specifications (i.e. between and within variance versus only within variance). It is worth noting that the mean annual change in the agglomeration variable is only 1.25% (with a standard deviation of 2.3%) so the range of sample variance over time is actually very small. As mentioned in section 4, as a compromise between the assumptions of the RE and FE models we also run the models with individual fixed effects for area types rather than firms. This specification still allows for substantial sampling variance in agglomeration, but places some constraints, albeit rather blindly, on unobserved heterogeneity. Key results based on this approach are shown in table 5 below.

Table 5: Production function estimates, GLS random effects with AR(1) errors and area type fixed effects.

	all industries	manufacturing	construction	wholesale & retail	transport & communications	business services
η_L	0.699**	0.674**	0.651**	0.709**	0.775**	0.713**
	(0.003)	(0.005)	(0.010)	(0.006)	(0.009)	(0.005)
η_K	0.359**	0.361**	0.461**	0.364**	0.286**	0.361**
	(0.002)	(0.004)	(0.009)	(0.005)	(0.090)	(0.003)
η_A	0.105**	0.060*	0.193**	0.050	0.170	0.110**
	(0.019)	(0.027)	(0.062)	(0.039)	(0.090)	(0.040)

Notes: ** - significant at 1%, * - significant at 5%,

Estimates of labour and capital shares do not differ greatly from those given in table 3 above. Again, we find that RTS are generally close to 1.0 with labour taking a share in total product of somewhere between 65% and 80%. Regarding agglomeration economies, we find evidence of positive and significant effects in the pooled model for all industries and for manufacturing, construction, and business services; but not for transport & communication or wholesale & retail firms. The agglomeration elasticities for manufacturing and business services are somewhat lower than those estimated without area type dummies, a results which is in principle consistent with the existence of unobserved confounders that are positively correlated with productivity. For construction firms, on the other hand, we estimate a substantially larger and significant elasticity with area type effects included. A similar result was found for this sector using sys-GMM. The data do not provide an explanation for this result, but it could be explained by the existence of effects on productivity arising from the heterogeneous nature of construction required in each area type.

It is interesting that with the inclusion of area type fixed effects we lose significant agglomeration economies in the two industries that provide a direct service to consumers: transport, storage & communications and wholesale & retail. This could again be explained by an occupational skew such that a disproportionate share of skilled jobs are located in areas with a high level of agglomeration. But it may also be indicative of the key role played by access to market share for these sectors, such that by capturing broad differences in market potential by area type we leave little additional effect from urban agglomeration. Either way, the key conclusion is that we cannot reject the null hypothesis of no agglomeration effects on productivity for consumer services.

VI Using agglomeration estimates in transport appraisal

What do our results imply for the inference we can draw on agglomeration economies and for the use of such estimates in transport appraisal? The paper has identified three estimation issues that present obstacles in drawing a causal interpretation of agglomeration effects: reverse causation, confounding, and heterogeneity within population sub-samples. In this section we discuss the implications of our findings on these three issues.

On reverse causation, we are not able to offer any solid conclusions because the dynamic panel GMM methods have proven unreliable. An alternative approach could be to address the endogeneity by constructing exogenous instruments. In the existing literature instruments commonly proposed include long lags on population density (e.g. Ciccone and Hall 1996; Mion 2004; Mion and Naticchioni 2005; Hanson 2005; Rice et al. 2006; Combes et al. 2007) or even geological features (e.g. Rosenthal and Strange 2005 and Combes et al. 2008a). For our data it would be hard to defend such instruments as either relevant or exogenous, and in fact the literature gives little convincing guidance on their validity[8]. Given the difficulties faced in constructing valid instruments, and the failure of dynamic panel models to offer a plausible alternative, our belief is that it will not be easy to identify the true role of reverse causality

[8] A key problem here is that the commonly used diagnostic test for instrument exogeneity, the Hansen test, has poor finite sample properties (see for example Andersen and Sorensen 1996 and Bowsher 2002). To quote Hahn and Hausman (2003), even using the standard tests for instrument validity "the researcher may estimate 'bad results' and not be aware of the outcome" (p 118).

in future empirical work.

On the issues of confounding and heterogeneity in sub-samples, clearly, the results show an absence of stability in agglomeration effects across our different model specifications. We find a high degree of sensitivity to treatment for unobserved heterogeneity and to differences in the sample variance of agglomeration. Agglomeration economies effectively disappear when we condition on firm effects, but they are still evident for manufacturing, construction and business services when we impose area type effects. Interestingly, we do not observe any major discrepancies in estimates of the labour and capital output elasticities. So the basic production function parameters are robust to model specification, but the agglomeration estimates are not. One key implication of the different results presented above is that the observed agglomeration effect is capturing something other than simply access to economic mass alone. This is most obviously indicated by the comparison of FGLS-RE results with and without area type effects, which show substantial differences in the magnitude of the agglomeration elasticities.

There are therefore some complex estimation issues to which this paper has drawn attention. The main implication for transport appraisal is that the agglomeration effects of investments cannot currently be interpreted causally, not on the basis of the estimates reported here or those available in the wider literature. The reasons for this is that we cannot pin down the productivity response that will arise from a change in accessibility *alone*. Furthermore, because of the nature of the problem, the extent to which differences in the response of productivity to agglomeration can be attributed to the influence of confounding factors or the sampling variance used for estimation is unclear. This is important because it leaves open the question as to whether small changes in agglomeration will actually have any discernible productivity impact. Even large transport investments, tend to bring about relative modest changes in agglomeration. For instance, in assessing the benefits of a major mainline rail infrastructure project for central London which would cost around £16 billion, the UK Department for Transport (DfT) estimates changes in employment densities from the scheme of 1.8%, 5.9% and 0.6% in financial, business services and 'other sectors', respectively[9]. These are very small changes and we do not currently know much about the productivity benefits that might result from shifts of this order of magntitude. The FE estimates given in this paper suggest they may have little impact.

Approaching a causal interpretation of agglomeration effects will prove difficult. A key prob-

[9]The full methodology and a background to Crossrail can be found in DfT 2005.

lem, one demonstrated in the estimation approaches used in this paper, is that the highly persistent nature of agglomeration can render dynamic panel GMM and FE approaches untenable. This is because a trade-off exists between corrections for unobserved heterogeneity and the retention of sampling variance. While we want to eliminate confounding by differencing or by imposing time invariant effects, this results in a lack of variance in agglomeration due to the near unit root properties of this variable. The empirical literature on agglomeration tends to think about productivity differentials *across* different places. It is equally important to find ways of testing for a link between agglomeration and productivity given changes that occur within the same places, small changes as well as large. We will need to look beyond conventional panel data approaches to find the the most fruitful opportunities to do this.

Another interesting question is is whether changes in accessibility adequately represent changes in agglomeration. The empirical work in this paper has used *effective density* or *market potential* representations of agglomeration. It is possible that other measures could be devised that are more sensitive to any behavioral differences that result from changes in agglomeration. Drawing on the well developed theory of the microfoundations of agglomeration, future empirical research should seek to find how we can best capture the mechanisms underpinning agglomeration externalities. By identifying the actual sources and their relative effects on productivity, we will obtain a better understanding of how improvements in transport accessibility might offer advantages for the performance of the spatial economy.

VII Conclusions

Current thinking on the wider economic benefits of transport investments draws on the theory of increasing returns to urban scale to argue for the existence of agglomeration benefits. These benefits are referred to as ‘wider’ or ‘additional’ because they are believed to be extraneous under the conventional value of travel time saving approach which assumes constant returns and perfect markets. To assess the magnitude of agglomeration benefits we need to be able to establish that there is in fact a casual process running from improvements in accessibility to increased productivity.

In this paper, we identify three key issues to be addressed in approaching such a causal interpretation: reverse causality, confounding, and dependency of the estimate on the range of sampling variance. We examine the relationship between access to economic mass and productivity using estimation techniques that instrument for potential endogeneity and that use different approaches to represent unobserved heterogeneity. We also compare results that draw on different sampling variances. A key aim of the paper is to illustrate the analytical difficulties faced in estimating agglomeration economies and to assess whether results are sufficiently stable and robust to be used as a guideline in the appraisal of transport projects.

The results show a high degree of sensitivity to treatment for unobserved heterogeneity and to differences in the sample variance of agglomeration. We find that agglomeration economies are effectively indistinguishable from zero when we condition on firm effects and can change substantially when we include area type effects in the models. A key conclusion is that we cannot distinguish agglomeration effects from other potential explanations for productivity increases, most notably functional heterogeneity. Under this condition, the agglomeration effects of transport investments cannot be interpreted causally.

A key issue that would improve our understanding of the relationship between access to economic mass and productivity concerns the relative roles of different sources of agglomeration. Empirical evidence on the role of sources, and their relative productivity effects, would provide a useful test of the theory and improve our understanding of the mechanisms that actually drive agglomeration economies. For transport applications, this is particularly important because the ease of making different types of trips matches well to sources. Thus, economies associated with labour markets will be affected by the efficiency of commuting trips, knowledge spillovers economies by the ease of business travel, and externalities of input-output association by provision for freight movement. By identifying the actual sources and their relative effects on productivity, we will obtain a better understanding of how improvements in transport accessibility might offer advantages for the performance of the spatial economy.

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