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Estimating Alternative Technology Sets in Nonparametric Efficiency Analysis

Restriction Tests for Panel and Clustered Data

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Abstract Nonparametric efficiency analysis has become a widely applied technique to support industrial benchmarking as well as a variety of incentive-based regulation policies. In practice such exercises are often plagued by incomplete knowledge about the correct specifications of inputs and outputs. Simar and Wilson (2001) and Schubert and Simar (2011) propose restriction tests to support such specification decisions for cross-section data. However, the typical oligopolized market structure pertinent to regulation contexts often leads to low numbers of cross-section observations, rendering reliable estimation based on these tests practically unfeasible. This small-sample problem could often be avoided with the use of panel data, which would in any case require an extension of the cross-section restriction tests to handle panel data. In this paper we derive these tests. We prove the consistency of the proposed method and apply it to a sample of US natural gas transmission companies from 2003 through 2007. We find that the total quantity of gas delivered and gas delivered in peak periods measure essentially the same output. Therefore only one needs to be included. We also show that the length of mains as a measure of transportation service is non-redundant and therefore must be included.

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1 Introduction

Nonparametric efficiency analysis has become increasingly important for sound decision-making in a variety of economic research fields. In addition to industrial benchmarking, the regulation of network industries, among them natural gas transmission, is a considerable field of application. Regulatory decisions are often directly contingent on the results of such analyses, e.g. in Norway, Germany, and Austria. Because the decisions have strong financial implications for both customers and firms, it is critical that the models underlying the analyses are specified correctly. In the context of efficiency-estimation this means that the correct inputs and outputs are accounted for.

Restriction tests, proposed by Simar and Wilson (2001) and Schubert and Simar (2011), allow for the testing of hypotheses regarding the inputs and outputs. Nevertheless, the practical value of these tests is limited when the cross-section sample size is small. This is typically the case in monopolized and oligopolized producer markets. One way to solve this small sample problem is to observe firms over time by using panel data. The regular cross-section tests are then, however, no longer applicable, because of the *i.i.d.* assumption. Therefore an extension that allows for correlation across the time dimension is required. These tests are developed in the course of this paper and proven to be consistent if the production frontier is constant over time.

The paper is organized as follows: Section 2 provides an expository overview of the role of efficiency measurement and benchmarking as a regulatory tool. We explain some of the benefits of nonparametric techniques as well as major difficulties that arise from small cross-section sample sizes, which often render reliable estimation impossible. We argue that restriction tests for clustered data could help solve this problem in many contexts. In Section 3 we describe the proposed test procedures. Section 4 describes our data set and presents the results. Section 5 concludes.

2 Efficiency Measurement as a Decision-Making Tool

Data Envelopment Analysis (DEA) is a nonparametric method for efficiency analysis and is closely related to the classical models of activity analysis.¹ It offers an alternative way to evaluate the performance of production entities and is capable of expressing productive efficiency in a multiple-input-multiple-output framework. In efficiency analysis, the performance of a production unit is determined by comparing it to a group of production entities that have access to the same

¹ For more details about the methodological linkages of activity and efficiency analysis see Färe and Grosskopf (2005). Homburg (2001) gives detailed insights on how nonparametric efficiency analysis can contribute to activity-based management.

transformation process (technology) through which they convert the same type of resources (inputs) into the same type of products (outputs). From the observed input-output-combinations, a best practice (frontier) is constructed against which each entity is individually assessed. The distance to that frontier reflects the production unit's ability to transform inputs into outputs, relative to what empirically is found and therefore assumed to be feasible.

Hence, efficiency analysis provides a quantitative measure of the existing potential for improvement. As pointed out by Bogetoft and Otto (2011), the scope of application of the DEA method is rich, since conceivable production entities include firms, organizations, divisions, industries, projects, decision-making units (DMUs), and individuals. Empirical analysis investigates, for example, industrial entities such as warehouses (Schefczyk, 1993) and coal mines (Thompson et al, 1995). As noted by Schefczyk (1993) industrial benchmarking serves as a tool to generate measures by which corporate decision-making can be brought in line with the corporate goal of operating efficiently. DEA in combination with Malmquist indices is also commonly applied to determine technical efficiency change, technical change, and total factor productivity change, see e.g. Jamasb et al (2008), all of which are useful tools to evaluate a particular sector and regulatory changes. In addition, DEA is widely used in the regulation of network industries in order to overcome disincentives and distortions related to monopolistic market structures.

2.1 Benchmarking and DEA in Regulation

It is well known that the private sector draws on comparative analyses, such as activity analysis, to improve its performance. Starting in the 1990s, regulatory authorities are making increasing use of benchmarking techniques in order to facilitate the incentive regulation of network utilities; see e.g., Jamasb and Pollitt (2003). In particular, electricity and natural gas transmission and distribution utilities are subject to regulatory activities; see e.g., Jamasb et al (2004); Cullmann (2012); Farsi et al (2007); Sickles and Streitwieser (1998); Hollas et al (2002). Applying benchmarking methods allows the regulator to simulate competitive market structures (quasi-competition), thus helping to pursue and implement regulatory objectives, e.g., reducing monopolistic power and promoting the efficient use of resources.

To foster an efficient use of resources, regulators frequently rely on cost models that allow the determination of cost-reducing targets for each of the firms. In the regulatory practice, the most important cost models are: total cost (TOTEX) benchmarking, capital cost (CAPEX) benchmarking, and operating expenditure (OPEX) benchmarking. The TOTEX approach includes both cost types, i.e. CAPEX and OPEX, while the CAPEX and OPEX benchmarking models only relate to capital costs and operating expenditures, respectively. Including capital costs in the regulatory cost model involves a significant assumption: it assumes that capital is fully substitutable with other input factors, e.g., labor, and easily to adjust in the short-run. Accepting this assumption is questionable when network companies are considered.

The different models are vividly discussed in both academic literature and regulatory practice. Strongly referring to the limited flexibility of managers to adjust CAPEX in the short-run, e.g., Stone & Webster Consultants (2004) and

Saal and Reid (2004) advocate the OPEX benchmarking approach. However, their proposed models do not completely ignore capital input. Instead of treating capital as an individual and flexible input factor, capital is introduced as an OPEX-determining factor.

In practice, however, the choice of the cost model and, thus, the associated selection of input and output variables, is not solely driven by economic theory. Particularly with respect to capital costs and capital stock measures, regulators often have limited information resulting in lack of data on capital or hardly comparable measures. Additionally, regulatory benchmarking is a highly politicized process. Irrespective of the chosen cost model, regulators often apply parametric frontier models, e.g. Stochastic Frontier Analysis (SFA) and DEA in order to establish benchmarks for target determination (Haney and Pollitt, 2009).

Due to the market structure in network industries, many regulatory benchmarking applications rely on a small number of observations; see e.g., Jamasb et al (2008). Larger sample sizes can generally be obtained in two ways: First, using cross-country analysis and, second, using cross-sectional data across multiple time periods. When pooling observations across countries, simple cross-section tests can be used as long as it is reasonable to assume that all countries have access to the same technology. However, when comparing the same individuals across time, the additional problem emerges that a firm's present and past observations are generally not independent. So pure cross-section methods will lead to false inference, even if the technology did not change over the respective time period.

2.2 A Need for Specification Analyses

As a nonparametric method, DEA has, on the one hand, appealing characteristics (Simar and Wilson, 2008); in addition to its great flexibility and easy computability, it requires only few assumptions on the technology set and its frontier. Particularly, it neither assumes a distribution for the inefficiency term nor does it impose a functional form to express the production process generating the observed input-output-combinations (Haney and Pollitt, 2009; Simar and Wilson, 2008). On the other hand, the DEA estimator has drawbacks that are highly relevant for both regulatory and industrial performance analysis.

In addition to its outlier sensitivity, the non-parametric nature of DEA dramatically reduces its asymptotic convergence rate when the dimensionality of the production possibility set is high. The dimensionality is directly linked to the upward bias of DEA efficiency scores occurring when the *true* technology is unknown. The estimated frontier can never be better than the *true* frontier and is likely to provide a less strict benchmark. Consequently, the efficiency of DMUs is overestimated, i.e. upward-biased, which is amplified when the dimensionality of the production possibility set increases. Apparently this is particularly problematic when there are a limited number of observations and, hence, some argue that DEA is not an ideal tool for regulatory purposes; see e.g. Shuttleworth (2005). The critique also extends to cases where the analysis of total factor productivity change in regulated and non-regulated sectors is of primer interest.

However, theoretical developments originating with Simar and Wilson (1998), are overcoming the upward-bias of DEA estimates by means of bootstrapping. These methods not only provide bias-corrected efficiency scores but also options

for drawing statistical inference. To the best of our knowledge, in Europe only Germany uses bootstrapping for this purpose in regulatory practice; see Bogetoft and Agrell (2007) and Agrell et al (2008).

As an alternative to bootstrapping firm-specific efficiency scores, one may consider a technology set that, by definition and economic reasoning, only includes a minimum number of inputs and outputs. However, in most situations it is uncertain what the correct specification of the technology set is. For example, uncertainty may occur as a result of information asymmetries, when the analyst lacks full information about the precise production process.² Then statistical inference about alternative specifications is desirable in order to make sound decisions about the reasonable choice of variables.

Simar and Wilson (2001, 2011) propose different restriction tests for nonparametric efficiency analysis that would facilitate the investigation of whether certain variables can be excluded (exclusion restriction) or summed up (aggregation restriction). Schubert and Simar (2011) extend these tests by introducing a subsampling procedure (a special kind of bootstrap) that relaxes the homogeneity assumption³ and, therefore, allows for tests in input and output directions within the same dataset. Although the benefits of restriction tests on production process formulations are obvious, in the applied literature they receive only scant attention. Restriction tests notably improve nonparametric benchmarking, because they increase the confidence in the chosen representation of the production process by providing statistical inference. The risk of overestimating the performance due to the 'curse of dimensionality' is reduced when variables are identified as irrelevant and, consequently, excluded from further investigation.⁴ Yet the existing implementations of the proposed tests are restricted to cross-sectional data only and are, therefore, not applicable to (unbalanced) panel data.

We aim to present a test procedure that is able to account for correlation that is likely present when panel (respectively, clustered data) is used.⁵ The contribution of the paper at hand is twofold: First, we further develop the theoretical underpinnings of the restriction tests in order to enhance their applicability to (unbalanced) panel and clustered data in general. This requires accounting for intra-observational dependencies. Second, we demonstrate the relevance of the proposed test procedure for benchmarking by applying the method to a data set of US natural gas transmission companies.⁶ Clearly, the main benefits of the pro-

² The information asymmetries in regulation mainly result from *adverse selection* and *moral hazard* problems (Joskow, 2006).

³ The homogeneity assumption is comparable to the parametric homoscedasticity assumption and means that the distribution of the inefficiencies does not depend on inputs or the outputs. The problem is that it will not generally hold in both the input and the output direction, prohibiting tests based on it in both directions.

⁴ Alternatively, variables could be omitted or aggregated. Omitting variables based on correlations should be avoided for translation invariant DEA models (Dyson et al, 2001) and aggregating variables based on principal components might be inappropriate for radial efficiency measurement (Simar and Wilson, 2001). However, the restriction tests proposed by Simar and Wilson (2001) and Schubert and Simar (2011) provide statistical inference procedures for the investigation of aggregates.

⁵ Note, that panel data is just one example of clustered data and that, therefore, the applicability of the proposed test is even more comprehensive.

⁶ This industry is subject to analysis concerned with total factor productivity growth and technical change in the light of changing regulation; see e.g. Sickles and Streitwieser (1992, 1998); Jamasb et al (2008).

posed approach are improving the efficiency estimation and overcoming lacks of information regarding the production process. Although our demonstration relates to the regulatory framework, it is straightforward to apply the technique to any other setting where the aforementioned problems arise.

3 Methodology

3.1 Technology Estimation using the DEA Estimator

We start by presenting the analytical framework. It introduces the concepts necessary for the later proofs of consistency for the test statistics.

Let $x^i \in \mathbb{R}_+^p$ and $y^i \in \mathbb{R}_+^q$ denote the vectors of p inputs and q outputs. The technology set Ψ represents the feasible input-output-combinations available to firm i , $i = 1, \dots, n$, (Bogetoft and Otto, 2011) and can be defined as

$$\Psi = \left\{ (x, y) \in \mathbb{R}_+^{p+q} \mid x \text{ can produce } y \right\}. \quad (1)$$

For Ψ we assume free disposability and convexity. The boundary of Ψ , denoted by Ψ^δ , describes the efficient production frontier, i.e. the technology, and can be defined as

$$\Psi^\delta = \left\{ (x, y) \in \Psi \mid (\gamma x, \gamma^{-1} y) \notin \Psi \text{ for any } \gamma < 1 \right\}. \quad (2)$$

where γ corresponds to the maximal achievable contraction of inputs or expansion of outputs, respectively; see e.g., Simar and Wilson (2011). According to Equation 2, a firm that employs a production plan that belongs to Ψ^δ , is regarded as efficient and its input-output-combination cannot be improved. Companies that operate at points in the interior of Ψ exhibit inefficiencies (Simar and Wilson, 2001), which can be diminished by moving toward the efficient frontier. Being able to handle multi-input and multi-output settings, the Debreu-Farrell measure⁷ quantifies the respective firm-individual degree of efficiency. For any particular coordinate $(x^0, y^0) \in \Psi$, the Debreu-Farrell efficiency score is determined by the radial distance from (x^0, y^0) to the efficient frontier Ψ^δ . It expresses the maximal proportional contraction of all inputs x that allows for the production of output level y for input-orientation, and the maximum proportional expansion of all outputs y that is feasible with the given inputs x , for output-orientation, respectively.

We restrict ourselves to the input-orientated firm-specific efficiency measure, which can formally be expressed as

$$\theta(x^0, y^0) = \inf \left\{ \theta > 0 \mid (\theta x^0, y^0) \in \Psi \right\}. \quad (3)$$

Hence, if $\theta(x^0, y^0) = 1$, the company is efficient and operates along the frontier Ψ^δ . If $\theta(x^0, y^0) < 1$, the company can improve its performance by reducing its input quantities proportionally. Together with the imposed assumptions, Equations 1

⁷ This measure is based on the work of Debreu (1951) and Farrell (1957). Alternatively, the concept proposed by Shepard (1970) can be used.

and 2 set up the *true* economic production model and characterize the data generating process \mathbb{P} (DGP).⁸ However, the *true* technology set Ψ , and hence, the *true* efficient technology Ψ^δ against which observations are compared to, are unknown and both need to be estimated from the observed input-output-combinations.

To approximate Ψ , we apply the DEA estimator proposed by Banker et al (1984), which incorporates the assumptions of free disposability, convexity and variable returns to scales. Thus, the linear program estimating the unknown input-oriented efficiency score θ becomes:

$$\begin{aligned} \hat{\theta}(x^0, y^0) = \min_{\theta, \lambda^1, \dots, \lambda^n} \{ & \theta > 0 \mid \theta x^0 \geq \sum_{i=1}^n \lambda^i x_k^i; k = 1, \dots, p \\ & y^0 \leq \sum_{i=1}^n \lambda^i y_l^i; l = 1, \dots, q \\ & \sum_{i=1}^n \lambda^i = 1; \lambda^i \geq 0 \forall i = 1, \dots, n \} \end{aligned} \quad (4)$$

where λ indicates the weights of the linear combination; the individual inputs and outputs are indicated by the subscripts k and l , respectively. It is well known that the rate of convergence for nonparametric estimators, such as DEA, is small compared to parametric estimators (Simar and Wilson, 2008). The consistency of this estimator is proven by Kneip et al (1998). But like most nonparametric estimators it suffers from the 'curse of dimensionality,' which implies that the rate of convergence (i.e. the speed by which the estimation errors are reduced in sample size) decreases as the number of inputs and outputs increases. Additionally, the DEA estimates are upward biased. This implies that the *true* efficiency is lower than the one estimated in finite samples. The precision of the estimation results is significantly affected by the ratio of observations to the number of variables and a considerable interest arises to test for the relevance of particular inputs and outputs. Reducing the dimensionality of the technology set Ψ by removing possibly irrelevant variables can offer substantial gains in estimation efficiency and decrease finite sample biases.

3.2 Testing Restrictions

Having specified the estimation approach, we formulate the restrictions on the technology set that we aim to test. It is our objective to test whether particular outputs are relevant for modeling the technology set appropriately. Although we focus on the relevance of outputs in this paper, we note that the method is broader. Alternatively, the relevance of input variables can be considered. Further, it can be tested whether inputs and outputs are individually relevant contributors to production or if they can be aggregated. We extend a test procedure suggested by Simar and Wilson (2001) to panel data while, following Schubert and Simar (2011), using subsampling procedures. The formalism of proofs of consistency in the appendix is independent of whether restriction is due to an exclusion or due to an aggregation restriction.

⁸ To comprehensively define the DGP, assumptions on the statistical model are necessary. Due to space limitations, we omit the discussion and refer the reader to e.g., Simar and Wilson (2001).

The basic idea of the original approach is to compare efficiency estimates obtained from a technology set including all potential outputs with efficiency estimates obtained from a restricted technology set that excludes at least one output (or aggregates at least two outputs). For the remainder of the paper, we refer to the model that includes all potential outputs (corresponding to an unrestricted technology set) as the *full model (FM)*. The model including only a subset of all potential outputs (corresponding to a restricted technology set) is denoted as the *nested model (NM)*. The rationale behind assigning a particular output as possibly irrelevant is the uncertainty regarding its relationship to the considered input(s). An output is identified as redundant if the difference between the estimates of both technology sets, where one is nested in the other, does not differ significantly. Conceptually this implies that the irrelevant output is not produced by the firm. The main benefit of this approach is twofold: First, selecting outputs can be based on statistical tests improving the technology specification's quality. Second, when outputs do not need to be included, thus yielding fewer dimensions, the estimation's quality improves, ultimately leading to an increase in the speed of convergence and a reduction in the finite sample upward bias.

To formalize this reasoning, we respecify the output vector y into two subsets of outputs, i.e. $y = (y^1, y^2)$, where $y^1 \in \mathbb{R}^{q-r}$ denotes the vector of $q-r$ outputs that are assumed to be relevant outputs of the production process under consideration, and $y^2 \in \mathbb{R}^r$ denotes the vector of r possibly redundant outputs. The hypothesis then is that x influences the level of y^1 but not of y^2 . The null and alternative hypothesis can therefore be written as

$$\begin{aligned} H_0: & x \text{ influences the level of } y^1 \text{ (} y^2 \text{ is redundant)} \\ H_1: & x \text{ influences the level of } y^1 \text{ and } y^2 \text{ (} y^2 \text{ is relevant)}. \end{aligned} \quad (5)$$

For any given input-output-combination $(x, y) = (x, y^1, y^2) \in \Psi$, the corresponding reformulated input-oriented Farrell efficiency scores in Equation 3 are:

$$\begin{aligned} \theta_{full}(x, y) &= \inf \{ \theta \mid (x, y^1, y^2) \in \Psi \} \\ \theta_{nested}(x, y) &= \inf \{ \theta \mid (x, y^1) \in \Psi \} \end{aligned} \quad (6)$$

where θ_{full} and θ_{nested} represent the efficiency for the FM and the NM. If the outputs in y^2 are truly redundant, θ_{nested} equals θ_{full} . If outputs in y^2 contain relevant outputs, then θ_{nested} would be smaller than θ_{full} . From that we can derive the following inequalities:

$$\begin{aligned} \text{if } H_0 \text{ is true: } & 1 \geq \theta_{full}(x, y) = \theta_{nested}(x, y), \text{ for all } (x, y) \in \Psi \\ \text{if } H_1 \text{ is true: } & 1 \geq \theta_{full}(x, y) > \theta_{nested}(x, y), \text{ for some } (x, y) \in \Psi \end{aligned} \quad (7)$$

According to Equation 4, θ_{full} and θ_{nested} can be estimated from the sample, denoted by \mathcal{X}_n , as follows:

$$\begin{aligned} \widehat{\theta_{full}}(x, y) &= \min_{\theta, \lambda^1, \dots, \lambda^n} \{ \theta > 0 \mid \theta x \geq \sum_{i=1}^n \lambda^i x_k^i; k = 1, \dots, p \\ & y^1 \leq \sum_{i=1}^n \lambda^i y_l^{1,i}; l = 1, \dots, (q-r) \\ & y^2 \leq \sum_{i=1}^n \lambda^i y_l^{2,i}; l = 1, \dots, r \\ & \sum_{i=1}^n \lambda^i = 1; \lambda^i \geq 0 \forall i = 1, \dots, n \}. \end{aligned} \quad (8)$$

and

$$\begin{aligned} \widehat{\theta}_{nested}(x, y) = \min_{\theta, \lambda^1, \dots, \lambda^n} \{ & \theta > 0 \mid \theta x \geq \sum_{i=1}^n \lambda^i x_k^i; k = 1, \dots, p; \\ & y^1 \leq \sum_{i=1}^n \lambda^i y_l^{1,i}; l = 1, \dots, (q-r) \\ & \sum_{i=1}^n \lambda^i = 1; \lambda^i \geq 0 \forall i = 1, \dots, n \}. \end{aligned} \quad (9)$$

where the relationship $1 \geq \widehat{\theta}_{full}(x, y) \geq \widehat{\theta}_{nested}(x, y)$ holds by construction.

In order to test H_0 , we have to find a valid test statistic that appropriately compares the estimated efficiencies under both technology sets. The quantity depending on the generic DGP \mathbb{P} that is proposed by the literature (Simar and Wilson, 2001) is:

$$t(\mathbb{P}) = \mathbb{E} \left(\frac{\theta_{full}(X, Y)}{\theta_{nested}(X, Y)} - 1 \right). \quad (10)$$

From Equation 7 we know that the ratio is equal to zero, i.e. $t(\mathbb{P}) = 0$, if H_0 is true, whereas it is strictly positive otherwise, i.e. $t(\mathbb{P}) > 0$. Empirically, the ratio can easily be obtained by the sample empirical mean that is a consistent estimator (Simar and Wilson, 2001; Schubert and Simar, 2011). Therefore, the empirical equivalent of $t(\mathbb{P})$ is:

$$t_n(\mathcal{X}_n) = \frac{1}{n} \sum_{i=1}^n \left(\frac{\hat{\theta}_{full}(X_i, Y_i)}{\hat{\theta}_{nested}(X_i, Y_i)} - 1 \right). \quad (11)$$

As mentioned before, by construction $t_n(\mathcal{X}_n) \geq 0$. Thus, the important question is how big it should be to be reasonably sure that H_0 is not true, i.e. y^2 is likely to be a relevant output of x . The usual approach is to use critical values corresponding to the distribution of the term in Equation 11. However, although this distribution can be shown to be non-degenerate, it is complicated and depends on local parameters. So far the only way to determine critical values is through bootstrap-based simulation techniques. A particularly comfortable as well as flexible way is to use the subsampling approach, as suggested by Schubert and Simar (2011). This approach is described and extended to clustered data in the next subsection.

To answer the question of how large the test ratio must be in order to reject the null hypothesis, we need to compute a p -value or a critical value. This requires the approximation of the unknown (asymptotic) sampling distribution of $\tau_n(t_n(\mathcal{X}_n) - t(\mathbb{P}))$, i.e. the convergence of the test statistic $t_n(\mathcal{X}_n)$ against the true population parameter $t(\mathbb{P})$ at rate τ_n , where t_n is a function of the sample size. Note that $t_n(\mathcal{X}_n)$ is the estimate of $t(\mathbb{P})$ that discriminates between the H_0 and H_1 .

The subsampling approach is a special kind of bootstrap. It differs from the normal procedure of generating pseudo samples of the original size n in that the samples here are of size $m < n$ such that $m/n \rightarrow 0$ when $n \rightarrow \infty$. This easy adjustment makes the subsampling approach robust to deviations from the assumptions necessary for the consistency of the bootstrap. Like the smoothed bootstrap the subsampling approach evades the problem of inconsistency pertinent to the

naive bootstrap. The naive bootstrap is proven to be consistent if and only if the asymptotic distribution of the estimator is normal, which is not the case for most efficiency estimators including DEA. The asymptotic non-normality results from the fact that the efficiencies depend on the frontier, and thus, on the boundaries of the support of the distribution of the production possibility sets. That is why the inconsistency is also referred to as the frontier problem. The subsampling does not only solve this problem. It also relaxes the homogeneity assumption by the smoothed bootstrap that may be highly problematic in the case restriction tests.

However, the use of subsampling, while consistent in variety of situations when the naive bootstrap fails, also has disadvantages. Since the number of used observations is lower it does not come as a surprise that the subsampling bootstrap is less efficient than the regular naive bootstrap. This strongly suggests that subsampling procedures should be avoided when the naive bootstrap is applicable. Nonetheless, our setting precisely suffers from the failure of these assumptions such that the loss of precision implied by the subsampling bootstrap represents a necessary evil to achieve consistency.

To derive an approximation of the sampling distribution of $\tau_n t_n(\mathcal{X}_n)$, we follow Schubert and Simar (2011) and use the algorithm based on subsampling proposed by Politis et al (2001).⁹ According to the algorithm, a sufficiently large number of subsets $b = 1, \dots, B$, denoted by $\mathcal{X}_{m,b}^*$, are constructed,¹⁰ each producing a test statistic, $t_{m,b}(\mathcal{X}_{m,b}^*)$, as defined in Equation 11. The large number of estimated test statistics approximate the sampling distribution for which a *critical value*, \hat{t}_m^c , can be derived. The *critical value* depends on m and the $(1 - \alpha)$ quantile. At the significance level α , the test rejects H_0 if and only if the observed value is greater than the critical value, i.e. $\tau_n t_n(\mathcal{X}_n) \geq \hat{t}_m^c(1 - \alpha)$, where τ_n equals $\sqrt{nn^{2/(p+q+1)}}$; for details see Schubert and Simar (2011).

The original procedures in Schubert and Simar (2011) are, however, pure cross-section tests and cannot be applied directly to clustered data (such as panel data). The main problem is that a test based on simple pooling of the observations (i.e. treating the clustered sample as pure cross-section) would disregard the fact that the observations within a cluster are not generally independent of each other (in our case: firms are usually not independent of their past). If this dependence is ignored, the most likely setting is that the significance of the tests is overestimated, leading to rejections in cases where there should not be one. An alternative based on cross-sectional tests only would be to run tests separately for each year. While this is clearly consistent, there are two problems. First, we would have a test for each year, which could lead to conflicting results when a test rejects in one year but not in another. Second, since we effectively split the sample into cross-sectional slices, the efficiency of such a test is likely to decline. For example, it is possible that none of the single-year tests rejects, but the full panel-robust test does.

⁹ Other bootstrap methods, e.g. the homogeneous bootstrap proposed by Simar and Wilson (1998) and further developed by Simar and Wilson (2001) or the double smooth bootstrap proposed by Kneip et al (2008) are not applicable in our setting because we need a method that allows for heteroscedasticity and that is valid for all data points considered simultaneously (Schubert and Simar, 2011). The aforementioned alternatives are, therefore, excluded.

¹⁰ A large number of subsets, and hence, of subsampling replications is required in order to reconstruct the behavior of the unknown parameter. Usually, the number of replications B is set to 2,000; see e.g., Daraio and Simar (2007) and Simar and Wilson (2000).

With these drawbacks of pure cross-section analyses in mind, we further develop the work by Schubert and Simar (2011) in the sense that we extend the applicability of the algorithm by Politis et al (2001) to clustered data, including panel data. The panel is allowed to be unbalanced, however year-wise missing observations are assumed to be completely random. Thus, we assume away (non-random) panel selection, such as attrition. Let n be the total number of observations and n_{PD} be the number of different companies in the panel; comparably, m and m_{PD} are defined for the subsample case. Obviously, then $n_{PD} \leq n$. Furthermore, for a balanced panel, L is the time length of the panel, $n_{PD} = n/L$. In an unbalanced panel, the number of observations per company is a random integer, say Z_i , such that it has support on $0, 1, \dots, L$. To distinguish between the overall sample and the panel data cases, we use the subscript PD whenever referring to the latter.

For company i , the test statistic in Equation 11 is then expressed as the intra-observational sum of the company-individual yearly estimates and can be rewritten as:¹¹

$$t_{n_{PD}}(\mathcal{X}_{n_{PD}}, Z) = \frac{1}{n_{PD}} \sum_{i=1}^{n_{PD}} \sum_{t=1}^L \left(\frac{\hat{\theta}_{full,it}(X_i, Y_i, Z_i)}{\hat{\theta}_{nested,it}(X_i, Y_i, Z_i)} - 1 \right). \quad (12)$$

where a zero is added, if a cross-section unit is not observed in a particular year. Equation 12 differs from Equation 11 in two respects. First, the subsampling has to account for the dependence among the observations, because observations belonging to the same unit are likely to be correlated. This problem is solved by clustering the companies across time and subsampling them block-wise as suggested by Davison and Hinkley (1997). The subsampled version of Equation 12 is then defined as

$$t_{m_{PD},b}(\mathcal{X}_{m_{PD},b}^*, Z^*) = \frac{1}{m_{PD}} \sum_{i=1}^{m_{PD}} \sum_{t=1}^L \left(\frac{\hat{\theta}_{full,it}(X_i, Y_i, Z_i)}{\hat{\theta}_{nested,it}(X_i, Y_i, Z_i)} - 1 \right). \quad (13)$$

Second, an additional random variable that captures the random panel response is introduced. The consistency requirement for the subsampling is that $\tau_{n_{PD}} t_{n_{PD}}(\mathcal{X}_n, Z)$ converges to a non-degenerated distribution (Schubert and Simar, 2011). This proof is presented in Appendix A of this paper.

Irrespective of the cross-sectional or panel data case, the test procedure is sensitive to the choice of m_{PD} , which implies a trade-off between too small and too large values. Too much information is lost if m_{PD} is too small; if m_{PD} is too large, the subsample size almost corresponds to the sample size n_{PD} inducing additional biases due to inconsistency of the naive bootstrap (Daraio and Simar, 2007). Therefore, an intermediate level of m_{PD} is supposed to balance the costs of both extremes. We use the data-driven approach, by which m_{PD} is chosen such that the volatility of the resulting measure of interest is minimized. As volatility index we calculate the standard deviation of the 95 percent quantile of the test statistic on a running window from $m_{PD}-2$ to $m_{PD}+2$.¹² Simar and Wilson (2011) show that this data-driven approach allows for tests on m_{PD} and on desirable

¹¹ We could also normalize the inner sum by dividing by Z_i , but this will have no asymptotic effect.

¹² This corresponds to the selection rule proposed by Simar and Wilson (2008), which selects a value of m for which the resulting sample distribution and some of its features, e.g., relevant moments, are stable with respect to deviations from this particular value.

power properties, e.g. rejecting H_0 with high probability when H_0 does not hold (Schubert and Simar, 2011). In order to evaluate the test statistic's volatility with respect to the choice of m_{PD} , a grid of values that m_{PD} can reasonably take is defined. These values belong to the interval $[m_{PD,min}, m_{PD,max}]$. For each of these values $\hat{t}_{m_{PD}}^c(1 - \alpha)$ can be calculated and investigated with respect to their volatility. Therefore, a plot of the critical values $\hat{t}_{m_{PD}}^c(1 - \alpha)$ against the possible values of m_{PD} reveals a first impression of where the interval's region exhibiting stable results (smallest volatilities) lies. For further details see Schubert and Simar (2011).

4 Application to US Natural Gas Transmission Companies

4.1 Technology Specification and Variable Selection

The introduced method is applied to the sector of natural gas transmission, which is frequently subjected to regulatory benchmarking activities. As pointed out by Jamasb et al (2008), regulation schemes vary across countries, with the most obvious differences between European countries and the US. Regulating natural gas transmission traditionally relies on cost-of-service or rate-of-return in the US; overviews of the implemented scheme are given, e.g., by Sickles and Streitwieser (1992, 1998) and, more recently, by O'Neill (2005). In contrast, European regulators are increasingly shifting toward incentive regulation, an approach discussed by e.g. Vogelsang (2002). Incentive regulation aims to introduce a company-inherent production cost reducing behavior by delegating pricing decisions to them, while giving the opportunity to gain profits from additional cost reductions. For this purpose, incentive-based regulation typically sets price or revenue caps using the RPI-X formula (Littlechild, 1983; Beesley and Littlechild, 1989) where X is the expected saving in efficiency. The extent of the expected efficiency saving can be deduced from frontier analysis. As shown by Haney and Pollitt (2009), European regulators frequently use DEA for incentive-based regulation of the natural gas transmission companies. Although frontier analysis is currently not used to regulate US natural gas transmission companies, it is useful in this context to investigate, for instance, the total factor productivity change and technical change of the industry, particularly in the context of changing regulation; see e.g. Sickles and Streitwieser (1992); Granderson (2000); Jamasb et al (2008).

A crucial part of both regulatory benchmarking and the evaluation of total factor productivity, *etc.*, is to specify the technology set. Consequently, extensive attention is usually devoted to the choice of variables. In their analysis on US natural gas transmission companies, Jamasb et al (2008), for example, select the relevant variables via a comprehensive econometric cost-driver analysis. In real life applications, the conflict related to the choice of variables arises from the uncertainty about the correct specification of the technology and, in regulatory frameworks additionally, from the opposing interests of regulating authorities and regulated firms: On the one hand, firms seek to increase the number of the considered variables in order to make the model as detailed as possible and, therefore, increase the dimensions of the technology set. In the case of high dimensionality, nonparametric efficiency analysis as an regulatory instrument is compromised because no meaningful efficiency estimates can be obtained due to the 'curse of

dimensionality'. Regulators, on the other hand, focus on only a few variables that appropriately model the technology set. We draw on discussions in the literature in order to establish alternative specifications of the technology set that we use to perform the proposed restriction test.

The primary task of natural gas transmission companies is to transfer natural gas from other upstream facilities¹³ to city gates, storage facilities and some large industrial customers. From the city gates on, the commodity is distributed to all other customers via local distribution systems that do not belong to the transmission system. To accomplish this task, natural gas transmission companies essentially employ pipelines, compressor stations, natural gas as fuel, and personnel.

We first specify the variables representing the inputs involved in the production process for natural gas transmission.¹⁴ Similar to other sectors, the commonly considered input factors are i) labor; ii) "other inputs" such as e.g., fuel, materials, and power (Coelli et al, 2003); and iii) capital. The expenses on labor and "other inputs" basically constitute the operating expenses, whereas investment spending relates to capital expenses. Since compressor stations require a notable amount of fuel and maintenance, the relative share of "other inputs" is large in natural gas transmission compared to other technologies. The crucial contributors to the pipeline operating costs are, therefore, the number of compressor stations and labor expenses (IEA, 2003). With unknown factor prices, we use operating and maintenance expenses (*O&M*) as an aggregated input measure, which sufficiently covers expenses for labor and "other inputs".

The aggregated input measure implies that factor prices are identical for all firms. This is a strong assumption and must be carefully considered in each application. Unfortunately, a lack of available data prevents an analysis including individual input factor prices. The absence of accurate input factor prices, however, frequently occurs in regulatory practice. In addition authorities find it difficult to obtain accurate physical input quantities (Jamasb et al, 2008). Against this background and from an analyst's perspective, the monetary aggregate has the advantage of overcoming information asymmetries while ensuring to account for all employed inputs.¹⁵

We do not include capital costs since in our simple input-output-specification this would imply the capital input to be fully substitutable with other input factors. In the context of natural gas transmission this seems to be an unreasonable assumption. Hence, we apply an OPEX benchmarking approach explained earlier in the paper in which the pipeline network does not constitute an individual input.¹⁶

¹³ These mainly include gas storage facilities, gas processing and treatment plants, as well as liquefied natural gas storage and processing plants.

¹⁴ For a general overview of commonly considered inputs and output of network industries, the reader is referred to Coelli et al (2003); a comprehensive discussion on the variable selection in the context of gas transmission is given by e.g., Jamasb et al (2008).

¹⁵ Note that the legitimacy of input (or output) aggregation should also be tested, e.g. by means of restriction tests; however, this is outside the focus of the present work.

¹⁶ Alternatively an OPEX model could have been implemented, which makes capital a determinant of some variable input factor as discussed previously. This would involve the specification of an input requirement function. To our best knowledge, there is no empirical analysis dealing with regulatory benchmarking of natural gas transmission companies applying this

There is a broad consensus about the plurality of outputs in network industries. The most obvious and frequently used measure to include is the natural gas delivered (*deliv*) (Coelli et al, 2003). Additionally, we consider the amount of natural gas delivered during peak times (*peak*), since the difference across firms is particularly relevant when regional characteristics vary. The provision of infrastructure (or the service supplied by using this infrastructure) itself can be considered a distinct output. Unlike other studies that incorporate the length of mains (*length*) as a capital measure, e.g., Jamasb et al (2008), we use it as proxy for transportation service. As consumers pay for delivered natural gas, both the gas volume and the distance covered in delivery are meaningful dimensions. Thus *length* is a potential candidate measure of output. In addition, including *length* improves the comparability among the investigated pipeline companies. Typically, larger (existing) networks are associated with higher operational costs: Compressor stations, installed to maintain the network pressure,¹⁷ determine a large part of personnel expenditures and maintenance costs (including fuel consumption). Not considering this technical aspect leaves companies with high *O&M* due to large networks at a disadvantage, *per se*. The network length appears to be a suitable proxy for the number of installed compressor stations since they occur at rather regular intervals of 150-200 km, corresponding to about 93-124 miles (Natgas.info, 2011).¹⁸ Another frequently considered measure is the number of customers supplied, which accounts for the multiplicity of output. However, the number of connections seems to be of minor importance in natural gas transmission networks. We therefore exclude it from consideration. Furthermore, pollution (as a bad output) is sometimes taken into account (Coelli et al, 2003) but it is not considered here.

Test	Input	Outputs	H_0	H_1	Description
I	<i>O&M</i>	<i>deliv, peak</i>	<i>deliv</i>	<i>deliv, peak</i>	<i>peak</i> is redundant
II	<i>O&M</i>	<i>deliv, peak</i>	<i>peak</i>	<i>deliv, peak</i>	<i>deliv</i> is redundant
III	<i>O&M</i>	<i>deliv, length</i>	<i>deliv</i>	<i>deliv, length</i>	<i>length</i> is redundant
IV	<i>O&M</i>	<i>peak, length</i>	<i>peak</i>	<i>peak, length</i>	<i>length</i> is redundant

Table 1: Test strategy

The above-mentioned discussion suggests that three potential candidates for output variables to be analyzed: *deliv*, *peak*, and *length*.¹⁹ Given the input variable

specification. We leave this to further research and present our proposed method with a simple input-output-specification.

¹⁷ The transport of natural gas is based on a pressure differential at the inlet and outlet.

¹⁸ However, we are aware of the fact that the length of mains cannot fully explain the differences of total operational costs of the compressor station since these also depend on the engineering characteristics. Further, length of mains likely reflects the geographical reach of services. An alternative view of its importance might result from the notion that companies active in rural areas naturally need greater *length* to deliver the same amount of gas than firms in metropolitan areas. This is simply because the customers are more dispersed. In this interpretation *length* would be rather a conditioning variable than an input or output. However, if *length* reflects an exogenous and monotonous cost disadvantage, it can also be included as an additional output. Our results are consistent with both qualifications of the variable *length* and corroborate its importance.

¹⁹ One can see clearly that we make *a priori* assumptions about the partition of the technology set into inputs and outputs. This partition is not always unambiguous. For example, one

OBM, we develop our model in terms of a stepwise enlarging set-up as illustrated in Table 1. A potential concern with this cascade of tests stems from the observation that we base our decision to drop a certain variable on the non-rejection of the Null (instead of dropping variable only if a reversed Null is rejected). From the regulator's point of view there is however a good argument to use our specification, because regulated firms have a strong incentive to include as many inputs and outputs as possible in order to reduce the effectiveness of the regulation. Indeed this is frequently observed also in practical settings. Since this would counteract the sense of any regulation exercise, our approach might be better suited because it accepts new inputs/outputs only when there is strong evidence for them.

In Test I we test whether *peak* needs to be included in the technology set when this already includes *deliv*. In Test II we also test this, vice versa, i.e. whether *deliv* is redundant in the presence of *peak*. We find that in each of the two tests, the additional variable is redundant, but it is difficult to tell which. This finding also suggests that one measure of the delivered natural gas should be included, but it is relatively unimportant (at least empirically) which one. Given *deliv* as output variable, Test III analyzes whether *length* is an additional relevant output variable. The same is evaluated in Test IV, except for the fact that *peak* is the baseline output variable. In both latter tests we consistently reject the Null hypothesis, i.e. we find sufficient evidence to add *length* to the set of relevant outputs.²⁰

4.2 Data

We employ data on US natural gas transmission companies provided by the Federal Energy Regulatory Commission (FERC). FERC Form No. 2 includes all natural gas companies whose combined gas transported or stored for a fee exceed 50 mn Dth. Given that we assume that the technologies of onshore and offshore pipelines differ, we only consider companies operating onshore facilities. Some missing values and data irregularities are excluded from the data set. The remaining sample contains information on 43 natural gas transmission pipeline companies that are

can think of arguments that would suggest that *length* is an input rather than an output. If this was, true the cascade of tests in Table 1 would change as well. This is a potential weakness of the procedure presented here, because these assumptions remain untested. Assessing them would require the use of some sort of goodness-of-fit criteria that are, to the best of our knowledge, not available in non-parametric frontier models. Another point concerns a delicate issue in terms of interpretation of the results. Using our tests we do not confirm that an input or output dimension is redundant. Rather we show that there is no conclusive evidence that it is not. Based on the failure to reject the Null we recommend excluding certain outputs/inputs. What might seem unwarranted at first sight can, however, be justified by the regulation context. In fact, because regulated firms have a strong incentive to include as many inputs and outputs as possible in order to reduce the effectiveness of the regulation, from the regulator's point of view it seems adequate to exclude those inputs or outputs that have not proven to be highly relevant. Otherwise the regulator would most likely be forced to use models with large dimensionality rendering the regulation exercise utterly ineffective.

²⁰ Additionally, we conducted the test where the Null incorporating *length* as the sole output variable is tested against the two alternatives *length, deliv* and *length, peak*, respectively. Both tests confirm the presented results. To save space, we present the detailed results in Appendix B.

observed with unequal frequency over a five-year time period (2003-2007).²¹ In total, the unbalanced panel includes 191 observations.

Table 2 presents the characteristics of the data. All variables are related to the companies' transmission branch. In general, all variables exhibit high standard deviations, indicating notable differences between the sample companies. Median values are consistently below corresponding mean values suggesting that the sample consists of relatively more small-size firms. For *O&M* we use the reported sum of transmission expenses for operation and maintenance. Other *O&M* items, e.g., related to production, storage or customer accounts, are not considered making output measures e.g., of storage activities unnecessary. The monetary values of *O&M* are inflation adjusted to 2003 dollars for comparability purposes. On average, the pipeline companies spend 42 mn USD on *O&M*. *Deliv* represents the account for the total quantity of natural gas delivered by the respective company and ranges from about 20 mn to 3 bn Dth. In order to ensure comparability with peak period information, we transformed this variable into Dth per day. The corresponding measure of supplied quantity then has a minimum and maximum value of 0.06 to 8.6 mn Dth a day, respectively. For *peak*, we use the single day account of the amount of natural gas delivered during system peak period. This measure also accounts for potential differences in the transmission network characteristics, e.g., pressure stages of the pipelines, and, therefore, ensures comparability of the analyzed companies in this respect. The sample companies report peak deliveries between 0.1 and 7 mn Dth (per day). *Length* represents the total length of transmission mains, which varies widely between the companies. The smallest pipeline network has 80 miles of pipeline and the largest has over 9,000 miles.

Since the DEA estimator envelops all observed data points to construct the frontier, it is not robust against extreme values and data errors, further referred to as outliers; see e.g., Simar (2003); Simar and Wilson (2008). Before testing the restrictions on the technology set, we perform an outlier detection procedure, using the approach suggested by Pastor et al (1999) to identify suspicious observations. The outlier detection routine is based on a technology set that incorporates all three potential outputs simultaneously²² and performed on a yearly base. 20 observations were identified as outliers and excluded from the original sample. The subsequent analysis is thus conducted with a reduced sample of 171 observations.²³

By using the cross-sections over multiple years, we assume that all observations have access to the same technology, meaning that technical change is absent dur-

²¹ Note that we want to empirically apply our proposed method and are, therefore, not concerned about the exact period under consideration.

²² Calculations are conducted using the statistical software *R* with the additional package "FEAR" version 1.12 by Wilson (2008).

²³ The outlier analysis was conducted before the restriction tests implying that the restrictions tests are run on the unrestricted model. This approach is consistent, because even if an outlier is identified on the basis of an ex post redundant dimension, under the Null the restricted and the unrestricted model converge to the same probability limit. In finite samples, if H_0 is true, this approach is less efficient, because observations may potentially be dropped on the basis of unnecessary dimensions. To assess this, we ran an ex post outlier analysis on the restricted set. We found that largely the same units are identified as outliers irrespective of whether we use the unrestricted or the restricted model. Also the restriction tests were rerun based on this sample. The decisions on the restriction tests did not change, with almost constant numerical test statistics. Thus, we can conclude that in our application the results are robust with respect to the order in which restriction tests and the outlier analysis are performed.

ing the considered time span. The assumption of a constant frontier over time is necessary for the pooling approach to be applicable since pooling implies that the reference set of an efficient unit may consist of observations from all years. However, if the frontier changes, evaluating an inefficient observation from one year to an (efficient) unit from the reference set from another year yields meaningless results. This is because it would be unclear whether the inefficiency just results from an ignored change in the frontier. Thus, it is necessary to exclude the possibility of a shift in the frontier. This assumption can be tested using Malmquist indices (Färe et al, 1992). More precisely, the Malmquist index is a measure of total factor productivity calculated from DEA measures. The Malmquist index itself can be decomposed in various ways of which the three-source decomposition is most common. The first component is change in scale efficiency occurring when units operating under increasing or decreasing returns to scale move closer to constant returns to scale. The second refers to change in pure efficiency, which occurs when an inefficient unit moves closer to the frontier. Third, efficiencies can change when the frontier changes, i.e. if there is technological change. The first two components are uncritical for the applicability of our approach, because they do not imply a change in the frontier. Rather, they result from changes of the positions of units evaluated against a constant production possibility set. However, if the technological component is not equal to one, this would prevent the applicability of this method. In Table 3 we present the upper and lower bounds for the technological component of the Malmquist indices for the units between 2003 and 2007. For all units, except for two, the lower confidence bound is below unity (one indicating no technological change) while the upper is above unity. This means that except for two out of 29 units the hypothesis that there is technological change is not statistically supported. In fact, even in the two cases where there is an indication of a change in the frontier, this does not seem very pervasive as the lower bound is close to one. For efficiency estimation, pooling over time is consequently valid in our case. Note that even though individual efficiency scores are estimated using pooled data, the tests regarding the technology set account for the panel structure by means of block-wise subsampling.

Variable	Min	Mean	Median	Max	Std.dev.
Opex (O&M) [thsd USD ^a]	268	42,421	20,593	244,284	50,632
Total deliveries (deliv) [thsd Dth ^b]	55	1,389	994	8,597	1,381
Peak deliveries (peak) [thsd Dth]	122	1,614	1,303	7,124	1,328
Length of mains (length) [miles]	80	2,379	1,402	9,627	2,505

Source: US FERC. Notes: observations=191, n=43, years=2003-2007, onshore pipeline companies included only. ^a Yearly operating and maintenance expenses are deflated to 2003. ^b Per day measures derived by dividing the total amount of natural gas delivered by 365 days.

Table 2: Descriptive statistics for US natural gas transmission companies

DMU	lower confidence interval (0.005)	upper (0.995)
1	0.6550	1.6710
2	0.9822	2.5057
3	0.7420	2.2977
4	0.2455	1.5239
5	0.3020	1.1728
6	0.5599	1.6946
7	0.6807	1.6229
8	0.4123	1.5637
9	0.5457	1.4202
10	0.2830	1.1000
11	0.2976	1.2316
12	0.4371	1.3456
13	0.5085	1.5655
14	0.4437	1.5074
15	1.0445	3.7676
16	0.9564	1.4677
17	0.4564	1.7341
18	0.4452	1.9881
19	0.6022	1.5135
20	0.6457	2.7563
21	0.5801	2.4130
22	0.6879	1.8920
23	0.4990	2.6042
24	0.5788	2.0197
25	0.5191	2.8096
26	0.7962	2.7978
27	0.5661	2.8805
28	0.7519	3.1450
29	1.0867	3.6763

Table 3: Lower and upper bounds for technological component of Malmquist indices

4.3 Results

4.3.1 Restriction Tests

Turning to the main interest of this paper, Figure 1 illustrates the results of the restriction test for our sample based on subsampling for clustered data. The horizontal dashed lines represent the respective, actually observed values of $t_{n_{PD}}(\mathcal{X}_{n_{PD}}, Z)$ obtained from Equation 12. The observed test statistic is 0.3707 in Test I, 0.1907 in Test II, 2.3491 in Test III, and 2.0337 in Test IV. We reject the respective H_0 if this statistic exceeds the determined critical value.

To derive the empirical approximations of the sampling distributions and the corresponding critical values $\hat{t}_{m_{PD}}^c(1 - \alpha)$, we calculate 2,000 replications of the test statistic $t_{m_{PD},b}(\mathcal{X}_{m,b}^*, Z^*)$, for each of the four tests, using the proposed subsampling procedure. Since the critical values depend on the respective subsample sizes, the replications of the four test statistics are calculated for different values of m_{PD} . The solid lines in the graphs illustrate the obtained corresponding critical values at the preferred level of significance ($\alpha = 5$ percent), as a function of the subsample size m_{PD} .

The vertical dashed lines indicate the respective optimal subsample size determined by the smallest measured volatility index. This corresponds to a region where the test statistic graphically appears to remain stable when slightly deviating from the identified optimal value of m_{PD} . Note that in our applied approach

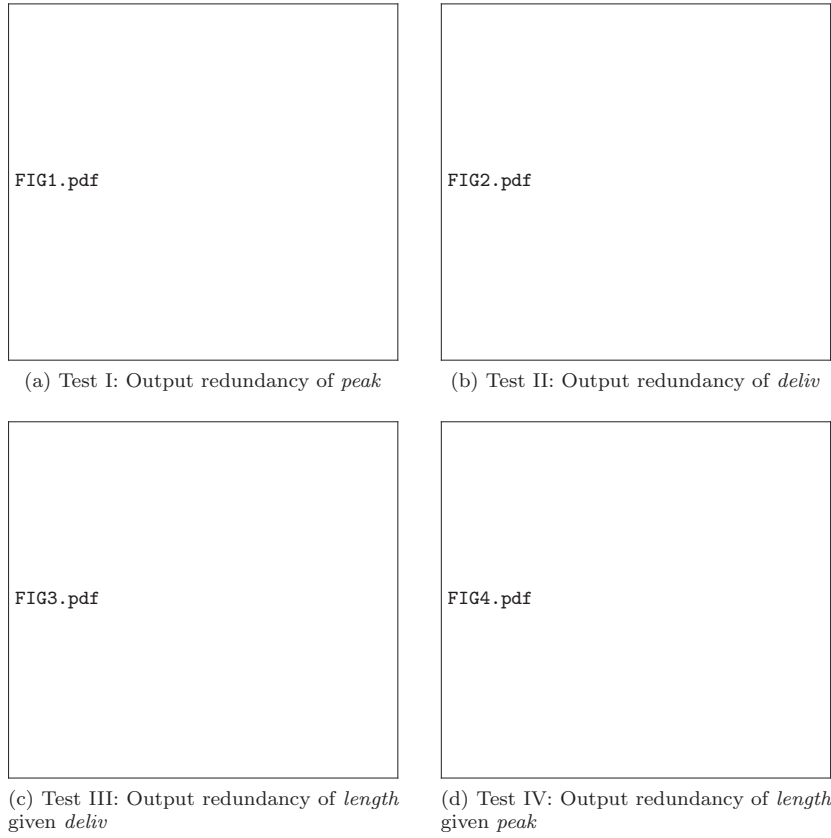


Fig. 1: Results of restriction tests

of subsampling for clustered data, m_{PD} , refers to the number of cross-section covered by the subsample, not to the total number of observations. As shown by the respective panels of Figure 1, the optimal subsample sizes for our tests correspond to 39 (panel a), 36 (panel b), and 34 (panel c and d); they are the reference points where the observed values of the test statistic are compared to the critical values in order to reach a test decision.

As evident from panel (a) in Figure 1, the critical value clearly exceeds the observed value of the test statistic obtained from Test I at the subsample size of 39. Therefore, we do not reject the Null hypothesis meaning that we do not find sufficient evidence to include the variable *peak* to the technology set, given that *deliv* is an output variable. The corresponding p -value (not depicted in the graph) of this test is 46 percent, which is obviously larger than our preferred significance level of 5 percent.

Likewise there is not enough empirical evidence to reject H_0 , if we run the test the other way around (Test II). At the subsample size of 36, panel (b) of Figure 1 shows that the critical value is again larger than the observed test statistic.

Test	Optimal subsample size	Test statistic (panel)	p -value	Test statistic (pooling)	p -value
I	39	0.3707	46 %	0.4049	2%
II	36	0.1907	40 %	0.2128	11%
III	34	2.3491	4%	2.5271	0%
IV	34	2.0337	7%	2.2247	0%

Table 4: Test statistics for Tests I to IV

Thus, given the output variable *peak*, the variable *deliv* is redundant to define the technology set under consideration. The p -value of Test II is slightly smaller (0.40), but roughly of the same magnitude. The results of Tests I and II indicate that we can drop either *peak* or *deliv*, if we control for the respective other one. However, the comparable significance levels show that it is relatively unimportant which is dropped. For Test III, we proceed with *deliv*, for Test IV with *peak* as the baseline output variable.

Test III compares the technology set *deliv* (H_0) against *deliv* and *length* (H_1). Indicated by panel (c) in Figure 1, we can indeed reject the Null hypothesis at the significance level of 5 percent level. In Test IV the relevance of *length* is marginally less pronounced at the 5 percent level. However, with a calculated p -value of 7 percent (not visible in the graph) we can reject the Null hypothesis of Test IV at the 10 percent significance level, thus favoring the alternative. Therefore, *length* represents a further, relevant output for the purpose of modeling the technology set of the considered companies.

Table 4 summarizes the presented results of the tests, i.e. the optimal subsample sizes, the test statistic obtained by subsampling for panel data, and the p -values. In addition, the last two columns of the table show an interesting point of comparison, i.e. the test statistics and the p -values we obtained for the presented tests, if we falsely assume that each observation is independent of each other. In this case we ignore the panel structure of the data and run the subsampling procedure without taking it into account. Technically, dependence leads to a duplication of information, because one observation already provides information about any other dependent observation. Consequently, ignoring dependence should generally overestimate the informational content of a sample leading to underestimated p -values. Indeed this is what we observe. Three out of the four tests (Tests I, III and IV) would now reject the Null hypothesis and the only one that does not is very close to rejection, with a p -value of 11 percent. If we based our decision on these tests, we should include any of the variables, eventually sticking with the full model of *deliv*, *peak* and *length*. Note, that our results depend on the sample and do not provide general evidence for the sector. With the reduced set of outputs, we improve the efficiency estimation by reducing the dimensionality and hence, the risk of overestimating the performance.

4.3.2 Differences in the Efficiency Estimates

In practical regulation settings, the estimated efficiencies have important financial implications for the companies. Each decrease in estimated efficiency will potentially cost the companies large amounts of money. Therefore, it is interesting to

Test	Difference between specifications	Min	Mean	Median	Max	Std.dev.
I	$\hat{\theta}_{H_1} - \hat{\theta}_{H_0}$	0.0000	0.0575	0.0154	0.4405	0.0921
II	$\hat{\theta}_{H_1} - \hat{\theta}_{H_0}$	0.0000	0.0271	0.0000	0.2744	0.0531
III	$\hat{\theta}_{H_1} - \hat{\theta}_{H_0}$	0.0000	0.2417	0.1247	0.9469	0.2766
IV	$\hat{\theta}_{H_1} - \hat{\theta}_{H_0}$	0.0000	0.2180	0.1286	0.9642	0.2493

Table 5: Differences of efficiency estimates

know which effects the proposed restrictions actually have on the companies' efficiency estimates.

Table 5 shows for each of the four tests how the company-individual efficiency scores respond to the differences in the technology set specification. We estimate the performance of each company using Equations 8 and 9 and calculate the differences of the achieved efficiency scores. The full models, i.e. the efficiency estimation using the technology sets under H_1 , by definition provide performance measures that are equal or greater than their corresponding nested models. For Tests I and II the technology set under H_1 is the same, i.e. involves both outputs *deliv* and *peak*. Whereas the technology set under H_0 incorporates only *deliv* in Test I and *peak* in Test II. The minimum differences of zero in both tests can be explained by the fact that there are some observations for which the efficiency score is only determined by the variable that is not excluded in the alternative technology set. Hence, excluding the other output measure does not change their performance measure. However, for some of the companies, the efficiency score changes considerably, with the exclusion of output *peak* yield, for example, a maximum difference of 44 percentage points in Test I. The maximum difference in Test II is, with 27 percentage points, lower. The table further shows that not including the output *peak* has, on average, greater impact than not including the output *deliv*, i.e. the mean of the difference is 0.0575 compared to 0.0271. Given that the tests results allow for the exclusion of either *deliv* or *peak*, from the regulator's perspective this provides a strong argument for using *peak* instead of *deliv*. In either case, discriminatory power is increased.

As a reference point we also present the results for Tests III and IV. However, we note that H_0 is rejected in both cases. Therefore, any potentials in higher discriminatory power are based on the fact that we would falsely restrict the technology set.

5 Conclusions

Industrial and regulatory benchmarking are commonly applied to all kinds of industries in order to improve company performance. Conducting such analyses requires the modeling the technology of the companies under investigation, which in practice is often a mere guess. This paper develops an approach to support the model specification of technology sets in nonparametric efficiency analysis based on statistical inference for clustered data.

To reach a decision on alternative model specifications, we propose approximating the sampling distribution of the test statistic of interest, i.e. the ratio of

the efficiency estimates obtained from alternative technology sets, using a block-wise subsampling procedure. This approach ensures that the dependency between observations is properly accounted for. The corresponding critical value of the sampling distribution can subsequently be used as the decision criteria. Due to the block-wise subsampling, the applicability of restriction tests, previously only proposed for the cross-sectional case, is extended to (unbalanced and balanced) panel data structures and any other kind of dependent observations.

Panel data is, for example, particularly interesting when the relative performance is measured for a small number of units. Due to monopolistic market structures, this is the case with regulatory benchmarking of network industries. Observing the units over multiple time periods can sufficiently enlarge the sample size to obtain meaningful efficiency measures and to apply restriction tests. In addition, regulatory benchmarking involves the issue of uncertainty about the correct specification of the technology, which requires objective modeling.

Therefore, we apply and demonstrate the proposed restriction test in a regulatory framework, considering the natural gas transmission sector. Our consecutive analysis involves four alternative technology sets for this sector, where the variable selection is based on the respective literature and regulatory practice. All technology sets in question contain operating and maintenance expenditures as input, while they differ in the output measures. The analysis is undertaken using an unbalanced panel data set of US natural gas transmission pipelines between 2003 and 2007.

First, we test whether the amount of natural gas delivered during peak times is a redundant output measure, if the technology set already included the total amount of natural gas delivered as an output. The second test deals with the reverse case, i.e. it tests whether the total amount of natural gas delivered is redundant, if the amount of natural gas delivered during peak times is defined as output. The test results suggest that in each case the respective additional output variable is dispensable, meaning that the technology set is sufficiently determined by one of the output variables. Although, the test is not designed to answer the question which of the alternative output variables is the correct one to choose, further analyses on discriminatory power provide some tentative indication that peak deliverables rather than total deliverables should be included. The efficiency estimates are more sensitive toward omitting the peak amount of natural gas delivered than omitting the total amount of natural gas delivered.

Based on the first two tests, the subsequent two tests both suggest including the length of mains as an output from the technology set if the initial output variable was either given by the total amount of natural gas delivered or the amount of natural gas delivered during peak times, respectively. Deleting the length of mains affects the efficiency estimates strongest indicating its importance for modeling the technology set.

For our sample, the test provides an objective tool to reduce the number of variables, which prevents overestimating the performance of the companies by including redundant variables in the specification. In general, the proposed test is a sound and reproducible method that helps remove the information asymmetry between the analyst and the production entity delivering the data that is possibly subject to regulatory benchmarking.

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Appendix A: Proof of Consistency

A robust approach to obtain corrected standard errors with clustered data is to sub-sample block-wise (Davison and Hinkley, 1997). This allows for arbitrary dependence between the observations belonging to the same cross-section unit.

We show that this procedure meets the essential consistency requirements set out in Politis et al (2001). Let sample size n_{PD} be defined by the number of different cross-section observations. Although we used the more easily interpretable Farrel-Debreu measure so far, for actual calculations it is preferable to use the inverse $\lambda = 1/\theta$ because it is truncated only once.

Proposition: Let $n(Z) = \sum_{i=1}^{n_{PD}} Z_i$ where *iid* random variables Z_i give the number of time observations per cross-section unit with distribution function F_Z defined on the support $S_Z = 1, \dots, L$ and expectation $e \in [1, L]$, then for the test-statistic $t_{n_{PD}}(X, Y, Z)$ the asymptotic distribution of $\sqrt{n_{PD}} n_{PD}^{2/(p+q+1)} t_{n_{PD}}(X, Y, Z)$ is non-degenerate with expectation zero.

Proof: If we reformulate the time subscripts to take only consecutive integers, we can use the following definition:

$$t_i(X, Y | Z = z) = \sum_{t=1}^{z_i} \left(\frac{\hat{\lambda}_{full_{it}}(X_i, Y_i, | Z_i = z_i)}{\hat{\lambda}_{nested_{it}}(X_i, Y_i, | Z_i = z_i)} - 1 \right).$$

It follows from the results of Kneip et al (2008) that

$$n^{2/(p+q+1)} \left(\frac{\hat{\lambda}_{full_{it}}(X_i, Y_i, | Z_i = z_i)}{\hat{\lambda}_{nested_{it}}(X_i, Y_i, | Z_i = z_i)} - 1 \right) \xrightarrow{d} H_n$$

for any fixed z_i , where H_n is a random variable with an asymptotic distribution function Q that is non-degenerate and has mean 0 under H_0 . Furthermore we can rewrite $n = n_{PD}e$, where e is the expectation of Z . Replacing and rearranging yields

$$n_{PD}^{2/(p+q+1)} \left(\frac{\hat{\lambda}_{full_{it}}(X_i, Y_i, | Z_i = z_i)}{\hat{\lambda}_{nested_{it}}(X_i, Y_i, | Z_i = z_i)} - 1 \right) \xrightarrow{d} \frac{1}{e^{2/(p+q+1)}} H_n.$$

Since the right-hand-side is a scaled version of H_n , also

$$n_{PD}^{2/(p+q+1)} \left(\frac{\hat{\lambda}_{full_{it}}(X_i, Y_i, | Z_i = z)}{\hat{\lambda}_{nested_{it}}(X_i, Y_i, | Z_i = z)} - 1 \right)$$

has a non-degenerate distribution. This implies that the conditional distribution of $n_{PD}^{2/(p+q+1)} t_i(X, Y | Z = z)$ is non-degenerate. Call this distribution $D(z)$.

Furthermore, we obtain the distribution of $t_i(X, Y, Z)$ by marginalizing out Z : $D(\cdot) = \int_{z \in S_Z} D(z) dF_Z$. Obviously, if $D(z)$ is non-degenerate with a given scaling factor, then $D(\cdot)$ must be non-degenerate with the same scaling factor. In order to complete the proof, since $t_{n_{PD}}(X, Y, Z)$ is an empirical mean of the $t_i(X, Y, Z)$, it follows by using the redefinition $n = n_{PD}e$ that $\tau_{n_{PD}} t_{n_{PD}}(X, Y, Z)$ with $\tau_{n_{PD}} = \sqrt{n_{PD}} n_{PD}^{2/(p+q+1)}$ is non-degenerate and additionally has an asymptotic expectation equal to zero under H_0 , because the mean associated with the asymptotic distribution Q is zero. As a consequence of this result, the subsampling methods proposed by Politis et al (2001) are consistent, when subsampling is conducted block-wise along the cross-section dimension. The sub-sampling size m_{PD} is as usually defined as the integer part of n_{PD}^k for $0 < k < 1$. It should be noted that these results include the case of ordinary cross-section data and a balanced panel setting. In the former case $z_i = 1$ and $n = n_{PD}$ yielding just the formulae in Schubert and Simar (2011). In the latter case $z_i = L$ implying that z_i cannot affect the asymptotic distribution because it is non-random.

Appendix B: Additional Test Results

In this appendix we report the additional tests performed for *length* as single output variable. Panel (a) of 2 presents the visual analysis in which *length* is tested against the alternative technology set with *length* and *deliv*. At the optimal subsample size of 21 the observed test statistic of 0.468 is very close to the critical value. The corresponding *p*-value of this test is 8 percent when the panel structure is accounted for and 1 percent otherwise. Panel (b) of this figure shows the visual results for testing *length* against *length* and *peak*. In this case the observed test statistic takes the value 0.593 and clearly exceeds the critical value at the subsample size of 23. The *p*-value is 3 percent (0 percent) when the subsampling accounts (ignores) the panel structure of the data.

Consequently, we find empirical evidence that the technology set should include either *deliv* or *peak* if *length* is chosen as initial output variable. Therefore, these tests support the previous findings reported in 1. Also they show that the test procedure does not depend on the order of variable selection.

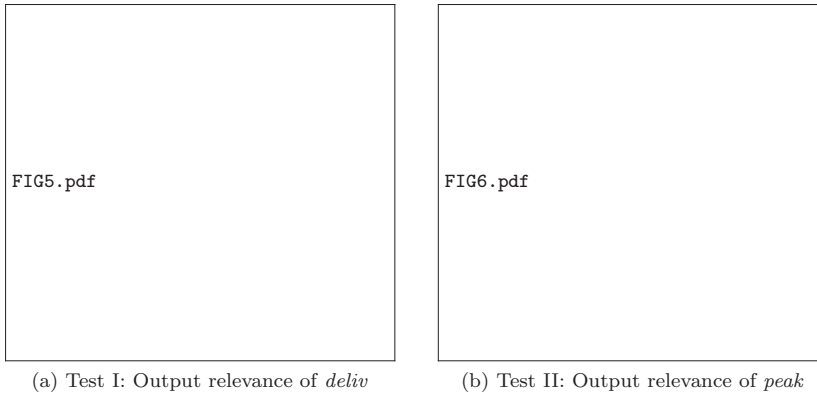
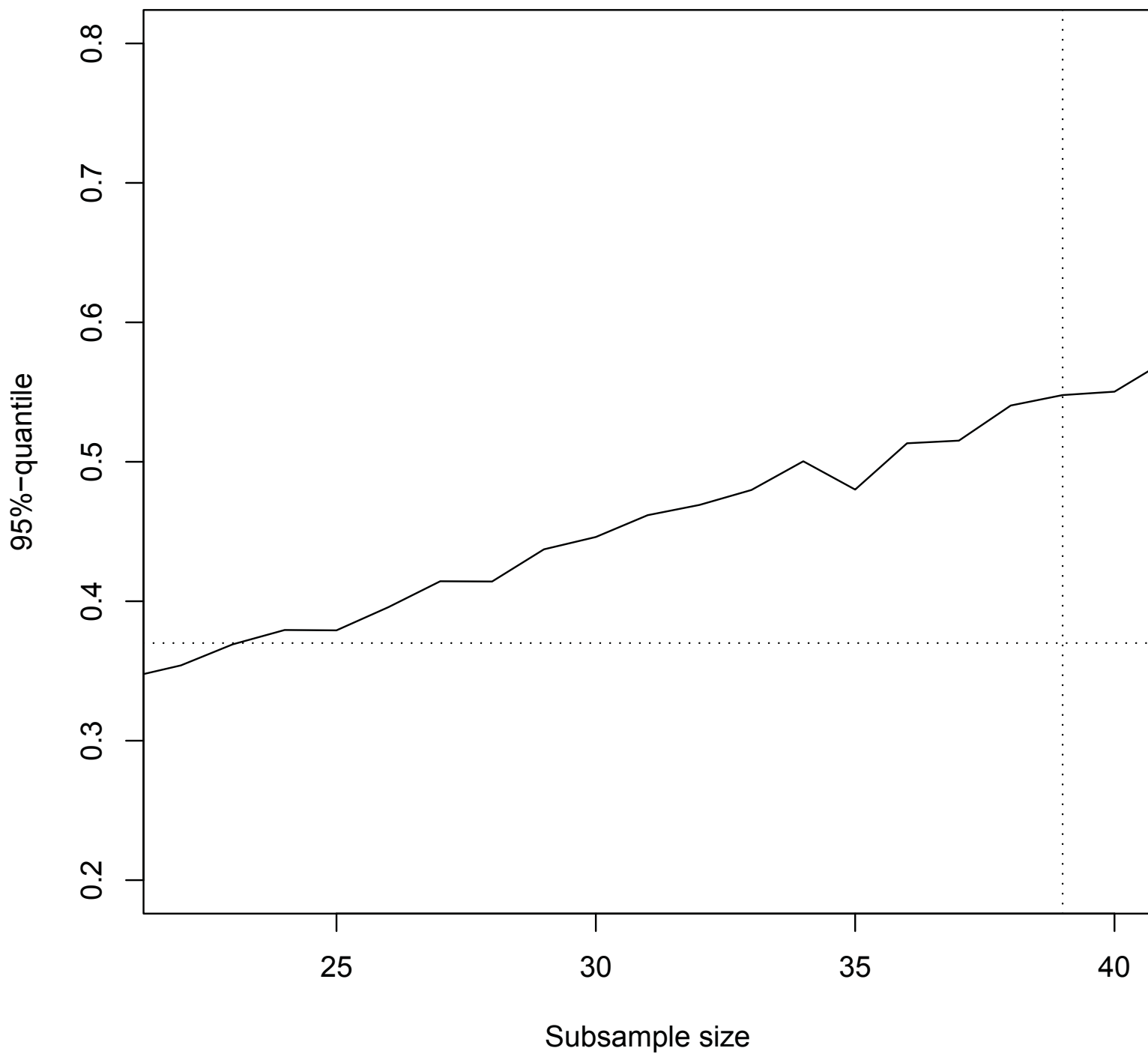
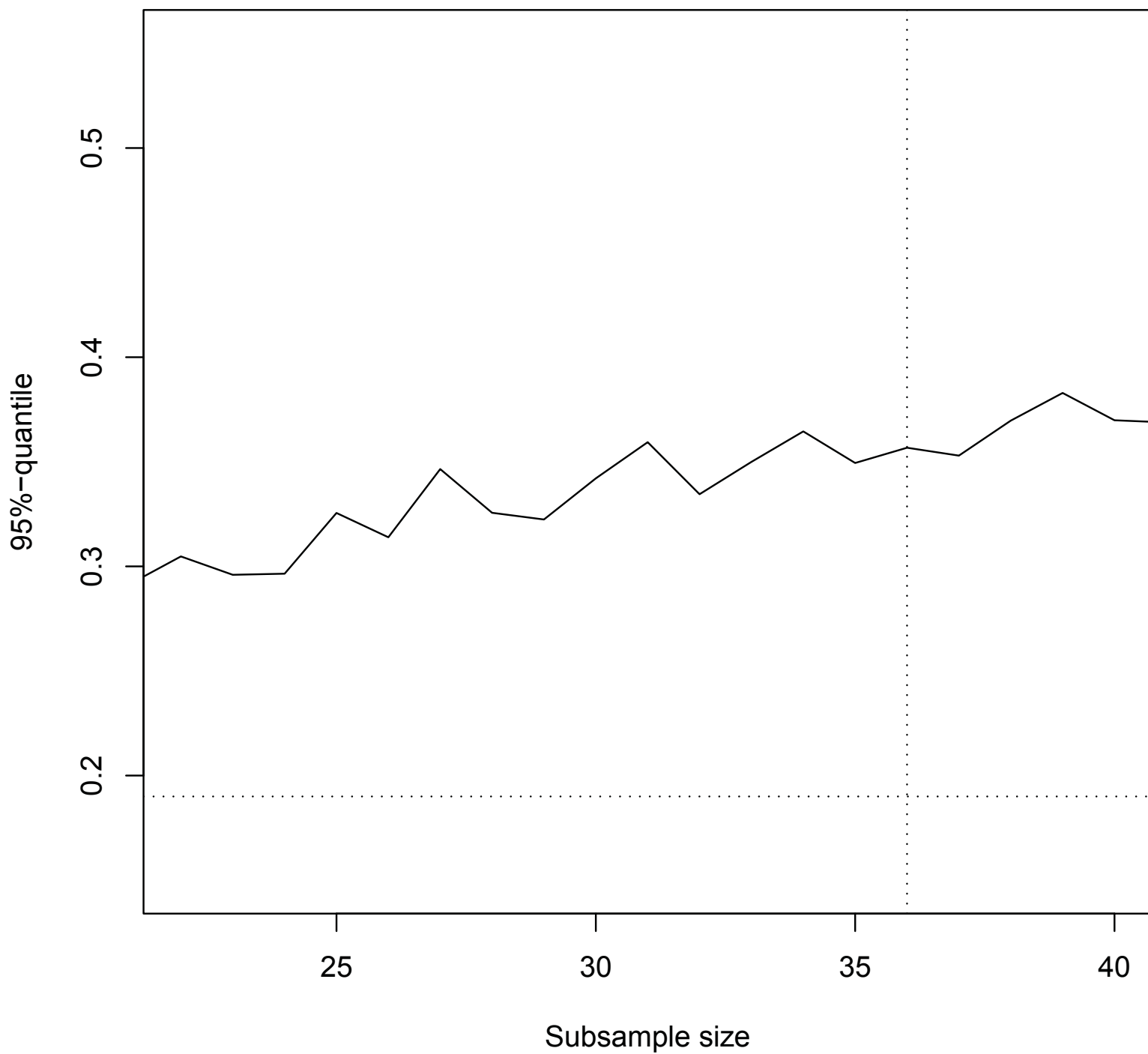
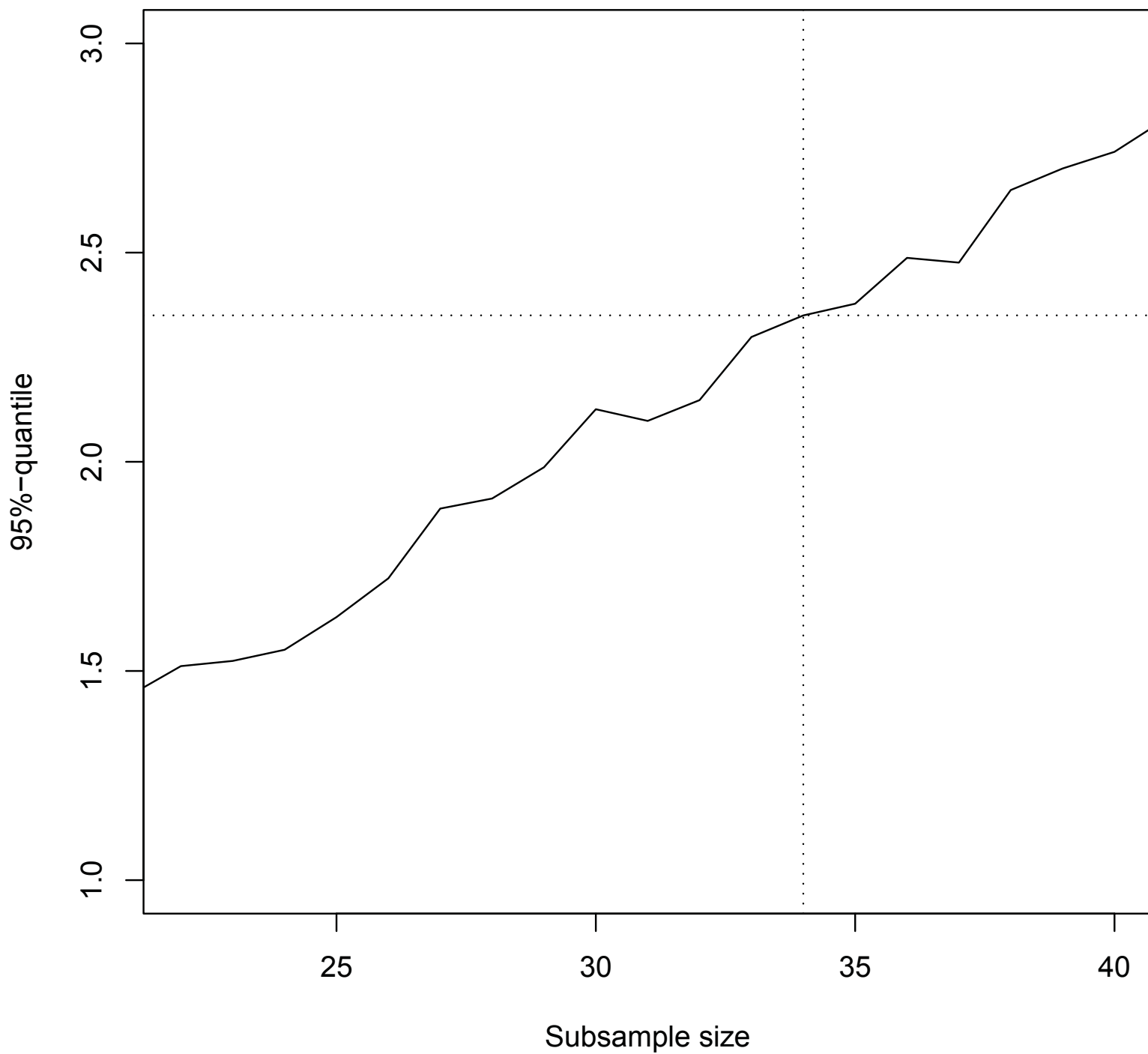


Fig. 2: Results of restriction tests for *length* as initial output variable

Output redundancy peak deliverables

Output redundancy total deliverables

Output redundancy mains

Output redundancy mains