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# Is Rising Returns to Scale a Figment of Poor Data?*

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**Abstract**

While using detailed firm-level data from the private business sector, this study identifies two empirical puzzles: (i) returns-to-scale (RTS) parameter estimates rise at higher levels of data aggregation, and (ii) estimates from the firm level suggest decreasing returns to scale. The analysis shows that, although consistent with rising estimates, the Basu-Fernald (1997) aggregation-bias effect does not drive this result. Rather, rising and too low returns-to-scale estimates probably reflect a mixture of random errors in factor inputs. It turns out, in fact, that a 7.5-10 percent error in labor (hours worked) can explain both puzzles.

**JEL classification:** D24, L60

**Keywords:** Business cycles, Data aggregation, External economies, Factor hoarding, Firm-level data, Monte Carlo simulation, Random errors, Returns to scale

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# 1 Introduction

In recent years there has been a massive amount of research on the procyclicality of measured productivity. This strand of research is of considerable importance since it has large implications for macroeconomic modeling in general and the relative merits of different models of the business cycle. Fernald and Basu (1999), for example, tried to determine the empirical relevance of a number of competitive explanations for why productivity normally rises in business cycle upturns and falls in downturns. Using a standard production-function framework and U.S. manufacturing data, they found that varying factor use and resource reallocation are of crucial importance for procyclical productivity. A similar result was found by Basu and Kimball (1997).

Caballero and Lyons (1992) showed that empirical estimates of the degree of internal returns to scale (RTS) are, in general, larger for manufacturing as a whole than for two-digit industries. Caballero and Lyons interpreted this finding as supportive of the claim that productive external effects (e.g., knowledge spillovers), which are gradually internalized at higher levels of data aggregation, are the driving forces behind procyclical productivity. Basu and Fernald (1997), likewise, confirmed that empirical estimates of RTS in U.S. production typically rise at higher levels of aggregation. However, Basu and Fernald argued that, rather than being caused by external economies, rising RTS estimates at higher levels of aggregation might result from data aggregation bias. The reason, they argued, is that heterogeneity across firms and sectors, in terms of RTS and factor input cyclicality, can result in upward-biased parameter estimates at higher levels of data aggregation.[1]

This article is closely related to the empirical literature on procyclical productivity. It draws on Caballero-Lyons (1992), and tries to determine to what extent – and, if so, why? – RTS parameter estimates differ between various levels of data aggregation. An essential difference between this study and the above-referenced studies is that they used industry-level national-accounts data, while here we use detailed firm-level accounting data. The analysis is based on a fairly new and, in this context, unusually large data set including information on output and factor inputs for the complete population of firms with at least 20 (50) employees in the business (manufacturing)

[1] This heterogeneity across firms may also give rise to a downward-biased RTS parameter estimate at higher levels of data aggregation.

sector.[2] The data include about 7,900 firms in the business sector and 4,300 firms in the manufacturing sector observed annually from 1979 through 1995.

In the first part of the analysis, we find that ordinary-least-squares (OLS) estimates of the RTS parameter rise in stages at higher levels of aggregation. In particular, we find that in the manufacturing sector, the point estimates rise from 0.68 at the firm level to 1.0 at the two-digit level, and in the business-sector the estimates rise from 0.64 at the firm level to 1.0 at the one-digit level. This rising pattern of RTS parameter estimates is thus coherent with the empirical work by Caballero-Lyons (1992) and Basu-Fernald (1997).

Our principal focus of the analysis is a world with poorly measured data. In particular, we examine if the empirical pattern of rising (and too low) RTS parameter estimates has anything to do with random errors on factor inputs that are gradually cancelled out at higher levels of aggregation. Indeed, if such errors are present – and, as we will argue below, we have every reason to believe this is the case – this rising pattern is exactly what we should expect to see in the data.

The standard way of dealing with random errors in (the right-hand side) variables is to use some kind of instrumental-variable (IV) estimation technique.[3] However, lots of studies have reported that it is, in general, difficult to find useful instruments for factor inputs in production-function regressions, and working with firm-level (micro) data certainly amplifies this problem. The instruments must be sufficiently correlated with the right-hand side variables and, at the same time, be valid in the sense of not being correlated with the error term. In fact, sometimes the cure (i.e., the use of instruments) turns out to be worse than the disease (the bias) – that is, the bias resulting from correlation between regressors and the error term is sometimes less harmful to the estimation result than is the use of poor instruments. This study is no exception in this respect; it is, as a rule, difficult to find useful instruments, especially at lower levels of aggregation, and trying to do comparable analysis at different levels of aggregation brings with it additional difficulties.

In this study, we report both OLS and IV estimation results. We find that the rising pattern of RTS estimates is, in general, less apparent when using IV techniques. This result is just what we should expect when instruments adjust, partly or completely, for

[2] A stratified sampling procedure has been used for the remaining smaller firms.
[3] Another reason that often justifies the use of instruments in production-function regressions is that productivity growth (i.e., the residual) correlates with capital and labor inputs.

random errors in factor inputs. Yet, it is difficult to draw any far-reaching conclusions from this result, in particular since the IV technique is in itself vulnerable to a number of issues, such as, for example, the mode of implementation and the size of the data.

In order to determine if the random-error hypothesis is plausible, it is essential to know the error magnitudes implied by the obtained RTS estimates. For that reason, we expand the analysis to include a Monte Carlo simulation allowing for a calculation of the likely downward bias from random errors in factor inputs. These simulations are performed in two steps. In the first, we add random errors of different magnitudes on capital and labor inputs, proportional in size to the levels, and then calculate the asymptotic RTS parameter bias in this new data. This exercise suggests that average errors in labor equivalent to 7.5 percent of actual working hours cause a 0.3 downward bias in the firm-level RTS estimate. Random errors in capital yield a smaller bias, which has to do with capital's share in output being smaller than labor's share.

In the second step, we go from probability limit theory to standard empirical analysis while performing the same pre-analysis data work (i.e., identifying invalid and outlier values) and estimations on the new data as we did on the original data. This exercise backs up the asymptotic results from the first step, although now the downward bias is a bit smaller. It suggests, for example, that random errors in labor equivalent to 10 percent of working hours yield a 0.3 downward bias in the firm-level RTS estimate.

Hence, taken at face value, it appears as if random errors in labor equal to 7.5-10 percent of actual working hours are consistent with the actual estimates. It accords, for example, surprisingly well with the finding that the RTS estimates rise from 0.68 (0.64) at the firm level to 1.0 at the two-digit (one-digit) level in the manufacturing (business) sector.

There are also other (not necessarily mutually exclusive) possible reasons for why the RTS estimates rise at higher levels of data aggregation – none of which, however, can explain the finding of decreasing returns at the firm level. One is the Basu-Fernald (1997) reflection that simple data aggregation may result in biased RTS estimates. Other possible reasons include external effects and factor hoarding tied to aggregate activity. However, while addressing all these possibilities in detail, we find that none is really challenging the random-error hypothesis.[4]

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[4] The present study makes uses of a simple first-order production-function approach. However, there are, of course, other ways of modeling the behavior of producers. For example, Morrison and Siegel

# 2 Analytical Framework

Caballero and Lyons (1989, 1992) postulated an industry-specific value-added production function and derived an expression for the change in industry-level output as a function of the change in industry-level inputs, aggregate activity, and technology. Thus, their model compares movements in output with movements in inputs and, accordingly, relates to the growth accounting literature originating from Solow (1957). In this analysis, we follow their approach at a lower level of data aggregation. Consider a general production function $Y = F(K, L, V)$ for a single firm, where $Y$ is value-added output (that is, gross output net of intermediate inputs).[5] Capital and labor inputs are denoted by $K$ and $L$. $V$ is an index of the level of technology.

Let the production function $F$ be homogenous of degree $\gamma$ in capital and labor and of degree one in $V$. Logarithmic differentiating of $F$ yields equation (2.1):

$$dy = \gamma dk + \left(\frac{F_L L}{Y}\right)(dl - dk) + dv, \tag{2.1}$$

where $dy$, $dk$, $dl$, and $dv$ are the growth rates of $Y$, $K$, $L$, and $V$. $F_L$ is the marginal product of labor. We have used the homogeneity conditions $(F_K K + F_L L)/Y = \gamma$ and $F_E E/Y = F_V V/Y = 1$ in the derivation of (2.1).

Equation (2.1) can be further simplified by using the first-order conditions:

$$\begin{aligned} P\mu^{-1}F_L &= w, \\ P\mu^{-1}F_K &= r. \end{aligned} \tag{2.2}$$

The price level of the firm's output is denoted by $P$ and $\mu$ is the markup factor. The wage rate $w$ and the capital cost $r$ are taken as given by the firms. Now, let $\alpha_v$ be labor's share in total output, that is $\alpha_v = wL/PY$, and use the first relation in (2.2) to obtain $\mu\alpha_v = F_L L/Y$.[6] By combining the two first-order conditions with the

(1997, 1999) applied a more structural dynamic-cost-function approach, which facilitates an appraisal of scale economies and so-called agglomeration externalities while controlling for and identifying other factors (such as changes in the utilization of inputs, non-homotheticity in production, and short- and long-run substitution among internal and external inputs). This approach, however, is beyond the scope of the present study.

[5] Note that we take no account in this study of potential problems related to the improper use of value added as an output measure, even though we do not doubt that value added may sometimes fail to fully account for the productive contribution of the intermediate inputs.

[6] When output and input markets are competitive, the necessary conditions for producer equilibrium are that the share of every input in the value of output equals the output elasticity with respect to that

homogeneity condition for $\gamma$, the product $\mu\alpha_v$ can, in turn, be rewritten in terms of the RTS parameter $\gamma$ and labor's share in total factor costs $\alpha_c$:

$$\frac{PY}{wL + rK} = \frac{\mu}{\gamma} \quad \Leftrightarrow \quad \mu\alpha_v = \gamma\alpha_c, \tag{2.3}$$

where $\alpha_c \equiv wL/(wL + rK)$. Substitution of $\gamma\alpha_c$ for $F_L L/Y$ in (2.1) yields:

$$dy = \gamma dx + dv, \tag{2.4}$$

where $dx$ is a weighted index of input growth:

$$dx_i \equiv \alpha_{ci} dl_i + (1 - \alpha_{ci}) dk_i. \tag{2.5}$$

In what follows, we will refer to this specification as our baseline equation, and we choose to emphasize with a star superscript the possibility that the RTS parameter may include other effects, such as the Basu-Fernald aggregation bias, external economies, or unmeasured variation in factor inputs:

$$dy = \gamma^* dx + \varepsilon. \tag{2.6}$$

This completes the description of the model.

# 3 Empirical Analysis

## 3.1 Ordinary Least Squares

Table 3.1 presents OLS estimates of the baseline equation (2.6) from different levels of aggregation. The table has seven columns. The first shows the level of aggregation; the lowest level is thus the firm level and the highest level is the one-digit level (i.e., total manufacturing). Column 2 shows the number of observations, and column 3 and 4 the RTS parameter estimates and related standard errors. The last three columns give the goodness of fit, judged by either the adjusted R-square (column 5) or the Lagrange Multiplier (LM) test statistic (column 6), and the probability of observing this statistic when the null hypothesis of a correctly specified model is true (column 7).

input. It follows that under constant returns to scale (the elasticities sum to one) the value of output is equal to the total cost of the factors, and hence the share of labor in output then equals the share of labor in total factor costs (see Jorgenson (1986)).

The first row shows that the RTS parameter estimate is 0.76 at the one-digit level. This model, however, is diagnosed with a bad fit – only 4 percent of the variation in output growth is accounted for by the factor inputs. The reason for this is probably that there are simply too few data points at this level to get any useful estimates. The second row shows that the two-digit level is characterized by about constant returns.[7] The RTS parameter then goes down to 0.92 at the three-digit level and 0.68 at the firm level.[8] Another thing that stands out from the table is that the LM test rejects the firm-level specification (see the last two columns). The reason for the bad firm-level specification is, however, not that easy to appraise (we return to this in Section 3.2 and 4).[9]

**Table 3.1** OLS estimates of the RTS parameter using firm-level data
The manufacturing sector (ISIC 3), 1980-1995

| Agg. level | # of obs. | Gamma | Std. error | Adj. $R^2$ | LM-stat. | *P*-value |
|---|---|---|---|---|---|---|
| 1-digit | 16 | 0.765 | 0.756 | 0.040 | 2.37 | 0.306 |
| 2-digit | 128 | 1.025 | 0.110 | 0.403 | 0.56 | 0.755 |
| 3-digit | 448 | 0.919 | 0.060 | 0.399 | 1.44 | 0.488 |
| Firm | 51,116 | 0.676 | 0.011 | 0.165 | 153.64 | 0.000 |

Note: The estimates of the constant are not reported and standard errors are robust with respect to heteroscedasticity (White's procedure).

In order to improve the precision of the estimates from the one-digit level, we expand the data to the whole private business sector. Hence, in addition to firms in the manufacturing sector (ISIC 3), we include firms in the mining and quarrying sectors (ISIC 2), the electricity, gasworks, and water supply sectors (ISIC 4), the building and construction sectors (ISIC 5), and the wholesale and retail trade sectors (ISIC 6). The

[7] The number of observations at this level represents data on eight two-digit sectors (31, 32,…,38) observed annually over 16 years.

[8] Decreasing returns have been reported before in the literature, albeit not (at least to our knowledge) at the firm level. For example, Caballero and Lyons (1992) found international support for decreasing returns in three-digit manufacturing; their point estimates ranged from 0.3 (France) to 0.8 (the United Kingdom). In addition, Caballero and Lyons (1992) found RTS parameter estimates between 0.75 and 1.05 in U.S. manufacturing.

[9] One possible reason for why the firm-level model is rejected by the test is that firms differ in ways not captured by measured inputs. For that reason, we have tried a number of empirical settings, such as models with firm- and time-specific dummy variables. We have also tried the so-called dynamic panel data (DPD) estimation method by Arellano and Bond (1998). However, none of these alternatives did improve the firm-level specification. Another, and probably more likely, reason for the bad firm-level specification is that the data suffer from random errors (see Section 3.2 and 4).

most striking result of Table 3.2 is, as expected, the higher and more precise one-digit RTS parameter estimate. The somewhat larger standard errors at the two- and three-digit levels in Table 3.2 probably reflect increased heterogeneity among the firms in the larger sample. The firm-level model is still rejected.

**Table 3.2** OLS estimates of the RTS parameter using firm-level data
The private business sector (ISIC 2-6), 1980-1995

| Agg. level | # of obs. | Gamma | Std. error | Adj. $R^2$ | LM-stat. | *P*-value |
|---|---|---|---|---|---|---|
| 1-digit | 80 | 1.019 | 0.194 | 0.753 | 3.07 | 0.216 |
| 2-digit | 240 | 0.996 | 0.161 | 0.676 | 3.16 | 0.206 |
| 3-digit | 707 | 0.929 | 0.073 | 0.465 | 5.08 | 0.079 |
| Firm | 82,908 | 0.642 | 0.009 | 0.152 | 284.27 | 0.000 |

Note: The estimates of the constant are not reported and standard errors are robust with respect to heteroscedasticity (White's procedure).

An alternative empirical route to improve the one-digit parameter estimates is to make use of additional data that span a longer time period. This, however, may bring up questions about the comparability of the results. Still, however, and without ignoring this complexity, we want to emphasize here that the rising pattern of parameter estimates is present also in a different two-digit national-accounts data set (see Table 3.3).

**Table 3.3** OLS estimates of the RTS parameter using national-accounts data
The manufacturing sector (ISIC 3), 1968-1993

| Agg. level | # of obs. | Gamma | Std. error | Adj. $R^2$ | LM-stat. | *P*-value |
|---|---|---|---|---|---|---|
| 1-digit | 26 | 1.220 | 0.049 | 0.949 | 3.14 | 0.208 |
| 2-digit | 208 | 1.135 | 0.030 | 0.907 | 3.39 | 0.184 |

Note: The estimates of the constant are not reported and standard errors are robust with respect to heteroscedasticity (White's procedure).

Several points are noteworthy. The first is that the firm-level model is always rejected by the LM test. This may, as pointed out in footnote 9, be the result of an incomplete model specification due to large heterogeneity in the data. For example, if individual differences in the distribution of firms are not completely accounted for, inference and specification are likely to be problematic. We have responded to this test outcome – which seems to mandate a change in the model specification – by testing the standard specifications, such as the fixed and random effects regressions (combined with time dummies). This did not help the firm-level model.

A second issue has to do with the capital data; specification problems may certainly arise because it takes time to build capital, and it also takes time for potential external benefits of capital accumulation to be felt. That is, it is possible not only that there are building-time delays, but that externalities affect output with a time lag. There is also the closely related question of how long capital stays productive after it has been built.[10]

A third prospective problem surrounds our implicit assumption in equation (2.6) about input quality and use; as now, capital and labor are assumed to be homogenous, and their rates of utilization are not allowed to change over time. The theoretical ideal should, of course, be input measures adjusted for quality differences and time-varying utilization rates.[11]

All these remarks, which hence draw attention to the familiar problems of the simple linear production model, call for deeper analysis. This could mean, for example, that more data efforts have to be done, or that the model specification should be checked more rigorously. However, the current data do not allow for much as regards input quality and use, and more work with the empirical specification will probably, in the end, lead the way to more structural models (such as the Morrison-Siegel (1997, 1999) dynamic cost function approach) which, due to data limitations and space, lie outside the scope of this study.[12] For these reasons, we do not pursue this route. Instead we take the following alternative.

## 3.2 Instrumental Variables

In the previous section, we found that the degree of RTS rise at higher levels of data aggregation. One potential reason for this – albeit, of course, not the only one – is that the data suffer from random measurement errors. In particular, if firm-level capital or labor inputs are measured with random errors, a rising pattern of RTS estimates (OLS) is just what we should expect to get. The reason is that these errors are likely to be cancelled out at higher levels of aggregation. The LM model specification test is

[10] In order to check if the capital data is especially problematic, we have constructed real capital from gross investments (i.e., we have used the perpetual investment method (PIM)). This, however, did not change any results.

[11] A number of studies have pointed out that factor inputs are in general rather difficult to measure (see, e.g., Bernanke and Parkinson (1991), Benhabib and Jovanovic (1991), and Griliches (1994)).

[12] See also footnote 4.

yet another sign of random errors: the gradual rise of the P-values at higher levels in tables 3.1 to 3.3 may, in fact, reflect a gradual improvement of the model specification due to receding errors. The standard response to errors in variables is to use an IV-estimation technique.

Table 3.4 shows the two-stage least squares (2SLS) estimation results of the baseline equation (2.6) (the OLS analogues are Table 3.1 and 3.2). The choice of instruments is always a difficult one, and here this difficulty is amplified by the fact that it is not clear from the outset if the same instruments should be used at each level of aggregation or if the same selection procedure for the instruments should instead be used. In the present analysis, we have used the same selection procedure. The procedure starts with the same set of prospective instruments at each level of data aggregation, and then sequentially leaves out those who are most correlated with the residuals. The procedure stops when the instruments are jointly independent of the second-stage residuals (according to an *F*-test statistic below 2).

**Table 3.4** 2SLS parameter estimates of the RTS parameter using firm-level data
The manufacturing (ISIC 3) and private business sector (ISIC 2-6), 1990-1995

| Agg. level | # of obs. | Gamma | Std. error | Adj. $R^2$ | *F*-stat. | *P*-value |
|---|---|---|---|---|---|---|
| ISIC 3 | | | | | | |
| 1-digit | 14 | 1.164 | 0.826 | 0.070 | 1.80 | 0.21 |
| 2-digit | 112 | 1.220 | 0.241 | 0.182 | 1.73 | 0.07 |
| 3-digit | 392 | 0.687 | 0.161 | 0.042 | 0.95 | 0.50 |
| Firm | 28,552 | 0.939 | 0.058 | 0.010 | 1.51 | 0.17 |
| ISIC 2-6 | | | | | | |
| 1-digit | 70 | 0.922 | 0.095 | 0.575 | 1.33 | 0.22 |
| 2-digit | 210 | 0.884 | 0.083 | 0.347 | 1.46 | 0.13 |
| 3-digit | 617 | 0.887 | 0.157 | 0.048 | 1.19 | 0.28 |
| Firm | 41,850 | 1.137 | 0.055 | 0.011 | 0.81 | 0.56 |

Note: The estimates of the constant are not reported and standard errors are robust with respect to heteroscedasticity (White's procedure). The *F*-statistic in column 6 (and the associated *P*-value in column 7) refers to a joint test of the instruments that are used in the regression (see the main text); since the 2SLS regression is robust with respect to heteroscedasticity, this *F*-value can be interpreted as a model specification test (for details, see White (1980)).

The main conclusions from Table 3.4 are as follows. First, 2SLS yields, as expected, a less apparent pattern in the RTS parameter estimates. It thus appears as if the gradual rise in these estimates at higher levels of aggregation that was found in Section 3 has been washed away by the instruments used in the 2SLS.[13] Second, the firm-level model specification is now much better than before; in fact, the LM test does not any longer reject the firm-level model – a finding that probably also spring from smaller errors in the right-hand side variables.

It is, however, not possible to draw any far-reaching conclusions from these results. In particular, although the RTS estimates in Section 3 seem to be largely in line with the random-errors hypothesis, they cannot say anything – at least in isolation – about the error characteristics, such as their frequency distribution and magnitude. In fact, in order to judge if the random-error hypothesis is at all plausible, it is essential to know the error magnitudes implied by the obtained RTS estimates. For that reason, we expand the analysis to include a Monte Carlo simulation allowing for a calculation of the likely RTS parameter bias caused by random errors.

# 4 The Impact of Random Errors

Our starting point in this section is a world with poorly measured data. Suppose that output and input growth are measured with error according to:

$$\begin{aligned} d\tilde{y} &= dy + \eta_1, \\ d\tilde{x} &= dx + \eta_2, \end{aligned} \tag{4.1}$$

where the variables with a tilde designate the observed data at hand. The difference between observed and true output and input growth rates is denoted by $\eta_1$ and $\eta_2$. Substitution of the expressions in (4.1) into the baseline equation (2.6) yields:

$$d\tilde{y} = \gamma^* d\tilde{x} + (\varepsilon - \gamma^* \eta_2 + \eta_1). \tag{4.2}$$

Assume that the random terms $\varepsilon$, $\eta_1$ and $\eta_2$ are (i) from zero mean distributions, (ii) mutually independently distributed, and (iii) unrelated to $dx$. Their variances are denoted by $\sigma_\varepsilon^2$, $\sigma_{\eta_1}^2$, $\sigma_{\eta_2}^2$. The covariance between $d\tilde{x}$ and the error term $(\varepsilon - \gamma^* \eta_2 + \eta_1)$ is $\gamma^* \sigma_{\eta_2}^2$. Hence, when inputs are measured with random errors, one of the essential requirements for unbiased OLS estimates is violated. It can be shown that the RTS parameter estimate converges in probability to:

[13] We have tried a number of settings in order to see to what extent the IV-technique does in fact provide a simple way out of the problem with random errors in the right-hand side variables. It turned out to be difficult to find useful instruments, especially at the firm level, and it was, as a consequence, quite easy to get different estimation results. This finding is in line with most other micro data work we are aware of, and it suggests, for example, that even though IV provides an intuitively appealing solution to the problem with stochastic regressors, it might be less useful in real applications.

$$\hat{\gamma}_i^* \underset{p\lim}{\rightarrow} \gamma^* - \frac{\gamma^* \sigma_{\eta_2}^2}{\sigma_{dx}^2 + \sigma_{\eta_2}^2} = \gamma^* \frac{\sigma_{dx}^2}{\sigma_{dx}^2 + \sigma_{\eta_2}^2}, \tag{4.3}$$

where $\sigma_{dx}^2$ is the variance in $dx$. According to equation (4.3), random errors in factor inputs will hence produce a downward bias in the RTS estimates. The extent of this bias, in turn, depends on the ratio of the variance of the random errors ($\sigma_{\eta_2}^2$) to the variance of the weighted inputs ($\sigma_{dx}^2$). The larger random error variance there is, the larger is the downward bias.[14] The problem hence is that the regressor in equation (4.2) is stochastic and, as such, not independent of the residuals.

## 4.1 Monte Carlo Simulation

Imagine now a world with no measurement errors in the variables. In order to obtain such a hypothetical firm-level data we impute output growth by weighted input growth for each firm

$$dy \equiv \alpha dl + (1-\alpha)dk. \tag{4.4}$$

By constructing output growth according to equation (4.4), we obtain experimental (hypothetical) firm-level input and output growth data that can be taken to mean both that the firms' production is subject to constant RTS and that inputs and output move in complete parallel.[15] This hence implies that the data do not anymore suffer from random errors, and that a simple regression of firm-level output growth on weighted input growth (i.e., a regression of the baseline equation (2.6)) would give a 1.0 point estimate of the RTS parameter. We then add uniformly and independently distributed random errors (*Z*) to the level of inputs and output according to:

$$Z = Z(1 + \delta \, \mathrm{U}[-0.5, 0.5]), \;\; Z = Y, K, L, \tag{4.5}$$

where $\delta$ is a scaling factor, and U[-0.5,0.5] is a function that generates a uniformly distributed random number between -0.5 and 0.5. The errors are hence assumed to be proportional in size to the levels. Uniformly distributed errors in the levels translate into triangularly distributed errors in the growth rates.[16] The resulting distribution in

[14] The effect of biasing the estimate toward zero in this way is known as *attenuation*.

[15] In this section, we simulate the effects of random errors in factor inputs while using experimental data constructed from the original firm-level manufacturing data that were used in Section 3. The same simulations have been made also on experimental data based on the original firm-level business-sector data. The simulation results were roughly the same.

[16] We have experimented with other distributions in order to make sure that this choice does not qualitatively affect the results (see also section 4.2).

the weighted inputs (i.e., in the right-hand-side variable in equation (2.6)), is more complicated to compute. In the simulations below, the scaling factor is set to equal 0.05, 0.1, 0.2, 0.3, … 0.8. Table 4.1 shows how the scale parameter translates into average and maximum errors in the levels and growth rates.

**Table 4.1** Size of differently measured random errors as a function of the scale parameter $\delta$

| $\delta$ | Errors in the level | | Errors in the growth rate | |
|---|---|---|---|---|
| | Maximum abs. | Average abs. | Maximum abs. | Average abs. |
| 0.05 | 2.5 % | 1.2 % | 5 % | 1.7 % |
| 0.10 | 5.0 % | 2.5 % | 10 % | 3.3 % |
| 0.20 | 10.0 % | 5.0 % | 20 % | 6.7 % |
| 0.30 | 15.0 % | 7.5 % | 30 % | 10.0 % |
| 0.40 | 20.0 % | 10.0 % | 40 % | 13.3 % |
| 0.50 | 25.0 % | 12.5 % | 50 % | 16.7 % |
| 0.60 | 30.0 % | 15.0 % | 60 % | 20.0 % |
| 0.70 | 35.0 % | 17.5 % | 70 % | 23.3 % |
| 0.80 | 40.0 % | 20.0 % | 80 % | 26.7 % |

Note: The numbers in the table are, for ease of presentation, reported with a maximum of one decimal.

Diagram 4.1 shows this variance ratio, as calculated by equation (4.3), as a function of the *average absolute* error in labor. The *maximum error* is twice as large (see Table 4.1). The data inherent in the diagram originate from a Monte Carlo simulation with 1,000 replicates.[17] The asymptotic bias in the RTS parameter estimate can easily be calculated from the diagram as one minus the variance ratio. According to the diagram, average errors in labor equivalent to 1.2, 2.5, 5, 7.5, 10, 12.5, and 15 percent of hours worked result in a firm-level downward bias that converges in probability to 0.01, 0.04, 0.15, 0.29, 0.43, 0.55, and 0.64. Hence, given that the firm-level RTS estimates of Section 2 are 0.64-0.68, this simulation result suggests a 7.5 percent average absolute random error in working hours.

[Diagram 4.1]

Diagram 4.2 shows the similar estimates for random errors on capital. These errors give rise to smaller bias, which has to do with capital's share in the production being

[17] Using the experimental data, we first add the errors on each firm's labor input and then calculate the variance ratio according to equation (4.3) for different levels of data aggregation. This procedure is then repeated 1,000 times. Diagram 4.1 shows the average variance ratio over all these replicates.

smaller than labor's share (this means that errors on capital play – per construction – a smaller role in production-function regressions than do similar errors on labor). Random errors on capital corresponding to 1.2, 2.5, 5, 7.5, 10, 12.5, and 15 percent generate a downward bias of 0.01, 0.01, 0.02, 0.02, 0.04, 0.05, and 0.07.

[Diagram 4.2]

In order to appraise the real impact of random errors on the estimated RTS parameter, we next perform exactly the same data processing and production-function regressions as in Section 3 (on the new data). Diagram 4.3 and 4.4 show the results. This exercise supports the asymptotic results of Diagram 4.1 and 4.2, although the downward bias is now a bit smaller. According to Diagram 4.3, random errors on labor equal to 1.2, 2.5, 5, 7.5, 10, 12.5, and 15 percent generate a downward bias on the firm-level RTS estimates equivalent to 0.01, 0.03, 0.11, 0.20, 0.30, 0.38, and 0.44, respectively. Thus, the 0.64-0.68 RTS estimate of Section 3 now indicates random errors in labor equivalent to 10 percent of the actual hours (rather than 7.5 percent).

[Diagram 4.3]

[Diagram 4.4]

Diagram 4.5 shows the analogue to Diagram 4.3 and 4.4 when output is subject to errors. This is the same as including the standard model error term. As suggested by equation (4.3), there is now no systematic pattern in the RTS parameter estimates, and the reason why the bias is at all different from zero has to do with the pre-analysis data work (i.e., the removal of invalid and outlier observations) rather than the added errors.

[Diagram 4.5]

There is, of course, little reason to believe that merely random errors in labor are present in the data. On the contrary, a widely shared view is that capital is in general more difficult to measure than labor. However, although this is often the case, due to a number of difficulties as regards the measurement of real capital, it does not imply that *random* errors are larger for capital than for labor.

Hence, to sum up, this section shows that random errors in factor inputs are probably one important reason for why RTS parameter estimates (OLS) rise at higher levels of aggregation. Random errors may also explain the finding of decreasing returns at the firm level. A qualified guess is that errors in labor equal to 7.5-10 percent of actual working hours generate a 0.3 downward bias in the RTS estimate.

## 4.2 Is this reasonable?

It certainly is a legitimate question to ask if the magnitude of these random errors is really plausible. For example, random errors equal to 7.5-10 percent of actual working hours imply that even though firms on average make use of, say, 40 hours a week per employee, they report 36-37 or 43-44 hours. Is this too much imprecision to be credible? This is, of course, a tricky question to answer, and although it warrants further investigation, we want to stress here that we think not. Yet, as real-world data probably suffer also from random errors in capital, we also want to stress here that the RTS estimates in Section 3 are, in fact, likely to reflect a combination of errors in capital and labor. The role played by the errors in labor is, however, much larger since labor's share in production is larger than capital's share.

# 5 Other Explanations

There are other potential reasons why RTS parameter estimates rise at higher levels of aggregation – none of which, however, can explain why firm-level RTS estimates are so small. In this section we shortly present these alternatives.

## 5.1 Aggregation Bias

Basu and Fernald (1997) showed that heterogeneity across sectors in terms of RTS and factor cyclicality may have cyclical implications at higher levels of aggregation. This effect represents an omitted-variable bias which, in principle, can generate either rising or falling RTS estimates at higher levels of aggregation. The bias is procyclical – that is, it causes RTS estimates to rise at higher levels – if firms with above-average input cyclicality also are characterized by above-average RTS.[18] To see this, consider aggregate output and weighted inputs computed as Divisia indices:

$$\begin{aligned} dy_t &= \sum_i \lambda_{it} dy_{it}, \\ dx_t &= \sum_i \lambda_{it} dx_{it}. \end{aligned} \tag{5.1}$$

[18] In addition, this bias will contribute positively to output growth if firms with higher than average RTS have higher than average growth in inputs.

The share of firm $i$'s nominal value added in one-digit industry value added is denoted by $\lambda_i$. Now, substitute firm-level output growth ($dy_{it} = \gamma_{it}\, dx_{it} + \varepsilon_{it}$) into the first equation in (5.1) to obtain $dy_t = \sum_i \lambda_{it}(\gamma_{it} dx_{it} + \varepsilon_{it})$. Some algebraic operations on this expression and the use of the second equation in (5.1) yield:

$$dy_t = \bar{\gamma}_t\, dx_t + R_t + \varepsilon_t, \tag{5.2}$$

where $\bar{\gamma}_t$ is a weighted average of RTS and $R_t$ is determined by:

$$R_t = \sum_i \lambda_{it}(\gamma_{it} - \bar{\gamma}_t) dx_{it}.^{19} \tag{5.3}$$

Hence, according to (5.3), the aggregation effect is procyclical if firms with above-average input cyclicality have above-average RTS. In order to control for this, we have adjusted output for $R_t$, and then re-estimated all regressions. This did not wipe out the rising pattern of RTS estimates (see Appendix B).

## 5.2 External Economies

Assume that the change in firm-level productivity evolves according to $dv_{it} = dv + \varepsilon_{1it}$. Hence, productivity growth equals the sum of a constant term ($dv$) and a random term ($\varepsilon_{1it}$).[20] Also, let the change in the firm-level externality be generated by an aggregate variable ($de_t$) and an error term ($\varepsilon_{2it}$) according to $de_{it} = \beta_{it} de_t + \varepsilon_{2it}$. Substitution of $de_{it}$ and $dv_{it}$ into equation (2.6) yields the left-hand side of equation (5.4):

$$dy_{it} = \gamma_{it} dx_{it} + \beta_{it} de_t + \varepsilon_{it} \quad \Rightarrow \quad dy_t = \frac{1}{1-\beta_t}(\gamma_t dx_t + \varepsilon_t), \tag{5.4}$$

where $\varepsilon_{it} = \varepsilon_{1it} + \varepsilon_{2it}$. The right-hand side is obtained by replacing the external-effect variable ($de_t$) with the change of aggregate output ($dy_t$) and then taking the sum over all firms. Equation (5.4) hence shows that the RTS parameter will rise at higher levels of aggregation if beta lies between 0 and 1. The logic is, of course, that the externality is gradually internalized at higher levels.

[19] Note again that because the current data lack information on intermediate inputs, we formulate this model in terms of value-added (rather than gross output). According to Basu and Fernald (1997), equation (5.2) should include an additional term capturing the difference between the growth of value-added output and the growth of intermediate inputs. However, because this difference in growth rates cannot be computed here, we implicitly assume that value-added output moves one-to-one over time with intermediate inputs.

[20] Fluctuations in output are here seen as arising solely from supply disturbances. For a discussion of both demand- and supply-determined macroeconomic fluctuations, see Blanchard and Quah (1989), and Blanchard (1989).

However, while using roughly the same data as we do in this study, Lindström (2000) found that a technology-shocks model statistically outperforms the externality model. In this study, we reach the same conclusion.[21] There must hence be more to the story than external economies.

## 5.3 Cyclical Errors

Sbordone (1996) stressed that economy-wide activity may provide information about sector-level factor utilization rates. The argument is that aggregate variables, through their information content about the future economic prospects, may affect the use of inputs. Thus, cyclical errors, which are by definition positively related to aggregate activity, may cause RTS estimates to rise at higher levels of aggregation.

In order to study the impact of cyclical errors on the RTS estimate, we have estimated equation (2.6) on a number of sub-periods. It turned out that although the period used for estimation mattered, at least to some extent, this did not, in general, remove the pattern of rising RTS estimates.[22]

# 6 Concluding Remarks

This study begins with the observation that production-function RTS parameter estimates (OLS) rise at higher levels of aggregation and that firm-level estimates are smaller than one. We find that RTS estimates rise from 0.64-0.68 at the firm level to about 1.0 at the two-digit (one-digit) level in manufacturing (the private business sector). These empirical regularities are interesting in their own right and are also consistent with other work.

Our starting point in the analysis is a world with poorly measured data. In particular, we try to see to what extent the obtained regularities originate from random errors in the factor inputs. The reason for this concern is that is it well-known by now that this kind of errors can easily bias parameter estimates toward zero. In the literature, this is often called an attenuation effect.

[21] A joint test of the linear restrictions imposed by the left-hand side of (5.4) on the technology-shocks model is always rejected by the data (these results are available on request).

[22] This does not mean, however, that factor hoarding is not present in the data; on the contrary, we found a sharper rise in the RTS estimates in the first half of the 1990s (the period 1991-1993 has been identified as the largest recession since the 1930s) than in the 1980s (see Appendix C).

The standard way of dealing with errors in variables is to use some kind of IV estimation technique. This, however, is not always that easy to do since it may be difficult to find useful instruments, in particular at lower levels of data aggregation. Sometimes, in fact, the cure (i.e., the instruments) is worse than the disease (the bias resulting from stochastic regressors). In this study, we report both OLS and IV. It turns out that the rising pattern of RTS estimates is less apparent when using IV, a finding that is just what we should expect to get when instruments adjust, partly or completely, for the errors in the right-hand side variables.

In order to determine if the random-error hypothesis is credible, however, it is vital to know the error magnitudes implied by the obtained RTS estimates. For that reason, we perform a Monte Carlo simulation, which allows for a rough calculation of the likely downward bias in the RTS estimates. This exercise suggests that errors in labor equivalent to about 7.5-10 percent of actual working hours produce a 0.3 downward bias. Random errors in capital yield smaller bias, which has to do with capital's share being smaller than labor's share. Hence, the simulation results accord surprisingly well with the real estimates.

There are also other potential explanations to the finding of rising RTS parameter estimates (none of these, however, can explain why the firm-level estimates are below one). For example, it may reflect the Basu-Fernald (1997) aggregation bias, external effects in production, or cyclical errors tied to aggregate activity. This study addresses all these possibilities, but finds that none is really challenging the random-error hypothesis.

One quite remarkable implication of this study is that it seems to support the idea that RTS may better be estimated at a high rather than a low level of aggregation. Thus, if this proposal is true, this study seems to conflict with much of the current thought in the microeconomic literature, which says that the theory of the firm applies only to disaggregated units and that aggregate estimates of production characteristics (such as RTS) are questionable as long as there is no well-behaved theory of data aggregation allowing for the representative-firm paradigm.

# Appendix A – Data Description

The present study is based on detailed input and output measures from the firm level. The data set represents a sub-sample of the Financial Accounts Database, provided by Statistics Sweden.[23] The data include annual time-series book value records of value-added, capital and labor inputs, and factor costs for Swedish firms from 1979 through 1995.[24] The data include the complete population of firms with at least 20 (50) employees in the business (manufacturing) sector. A stratified sampling procedure has been applied for the smaller firms. The data include about 7,900 firms in the business sector and 4,300 firms in the manufacturing sector observed annually from 1979 through 1995.

Total labor compensation (that is, total wage expenses, social security contributions, and mandatory insurance fees) is used for the labor cost. Capital is measured by the book values of the stocks of machinery and equipment, and buildings and land. Labor is measured by the average number of employees per year. Value added is deflated by a two-digit producer price index (PPI) and capital by a two-digit investment price index (IPI). Capital costs are deflated by the same two-digit PPI and labor costs by a one-digit labor cost index (LCI).[25] In order to derive an indicator of firm-level input activity, $dx_{it}$, capital and labor are, according to (2.5), weighted by their shares in total factor costs. Following Hall and Jorgenson (1967), firm $i$'s user cost of asset $j$ is computed according to:

$$r_i^j = \frac{1 - ITC_i^j - \Gamma^j}{1-\tau}(\delta^j + \rho - \pi^j). \tag{A.1}$$

[23] These data have been used previously by Forsling (1996), who investigated the degree of utilization of tax allowances in Swedish manufacturing, and Hansen (1999), who studied the influence of credit market conditions on firm's investment behavior. In addition, Lindström (2000) used roughly the same data when analyzed the procyclicality of measured productivity.

[24] These book values are established by accountants and reported on the firms' balance sheet.

[25] These deflators are obtained from the Statistical Yearbook of Sweden. Note that the current data set does not include price information for individual firms, implying that firm-specific deflators cannot, unfortunately, be used. Abbot (1991) found that estimates of production function parameters using firm-specific deflators may yield different results than estimates using industry-wide deflators. Note also that although the producer price index is a gross-output deflator, it is used in this study to deflate value-added. The reason for this is that there are no obvious alternatives to this deflator. If the price on intermediate inputs moves one-to-one with the price on value-added output, the gross-output deflator is the same as the value-added deflator.

We assume that the rate of annual depreciation $\delta^j$ equals 0.123 for machinery and equipment, and 0.036 for buildings and land.[26] The real rate of return required on capital is measured by subtracting the inflation rate from the required nominal (tax-adjusted) rate of return on capital, that is $\rho - \pi^j$. The investment tax credit $ITC_i^j$ measures the proportion of the original investment cost that is subsidized (this variable is obtained directly from the data). The present value of depreciation allowances for an investment is captured by $\Gamma^j$.[27] The required payment for the *j*th asset equals $r^j K^j$, where $K^j$ is the current value of the stock of this particular asset. The total cost of employing capital, broadly measured as the sum of machinery and equipment, and buildings and land, hence equals the sum of the required payment for each of the four types of assets.

**Table A.1** Summary statistics of the manufacturing data (ISIC 3), 1980-1995

| Variable | # of obs. | Mean value | Std. dev. | Minimum | Maximum |
|---|---|---|---|---|---|
| 1-digit level | | | | | |
| $dy_a$ | 16 | 0.013 | 0.075 | −0.052 | 0.258 |
| $dk_a$ | 16 | 0.018 | 0.044 | −0.064 | 0.073 |
| $dl_a$ | 16 | −0.016 | 0.036 | −0.101 | 0.051 |
| $dx_a$ | 16 | −0.007 | 0.032 | −0.067 | 0.056 |
| $\alpha_a$ | 16 | 0.785 | 0.054 | 0.691 | 0.868 |
| Firm level | | | | | |
| $dy_i$ | 51,116 | 0.028 | 0.279 | −1.939 | 1.995 |
| $dk_i$ | 51,116 | 0.042 | 0.363 | −1.000 | 1.999 |
| $dl_i$ | 51,116 | 0.004 | 0.166 | −0.993 | 1.979 |
| $dx_i$ | 51,116 | 0.015 | 0.167 | −0.993 | 1.938 |
| $\alpha_i$ | 51,116 | 0.855 | 0.121 | 0.021 | 1.000 |

Table A.1 reports descriptive statistics on growth rates of value added, capital, labor, weighted inputs, and labor's share in total factor costs. The firm-level variables are marked by subscript *i* and the one-digit analogues by subscript *a*. Weighted inputs at the aggregate level is defined as a weighted average of the percentage change in aggregate capital and labor, that is, $dx_a \equiv \alpha_a dl_a + (1-\alpha_a) dk_a$, where $\alpha_a$ is the average of labor's share across firms.

[26] These depreciation rates are the same as in Hansson (1991).
[27] Note that due to various measurement difficulties, estimates of this user cost of capital are at best approximations to the true cost of capital. However, because capital generally is less cyclical than labor, it should be safer to underestimate this cost than the opposite. The reason is that spurious cyclical errors in the baseline equation (2.6) are less likely to show up when labor's share in total costs is large.

# Appendix B – Aggregation Bias

In order to control for the Basu-Fernald (1997) aggregation effect, one can estimate aggregation-bias-corrected value-added output (i.e., an output measure acquired by subtracting the aggregation term $R_t$ from actual value-added growth) on the growth of primary inputs. However, to calculate the aggregation effect for the three-digit level, firm-level estimates of RTS are needed (see equation (5.3)).[28] Firm-level estimates are difficult to obtain when many firms are observed only a few years (the data is unbalanced). Therefore, we now restrict the sample to firms observed over the whole time period 1980-1995. Table B.1 and B.2 below show that the RTS parameter estimates do not change much when $R_t$ is subtracted from output growth (compare the baseline estimates with the adjusted estimates).

**Table B.1**: Aggregation-bias adjusted OLS estimates of the RTS
Swedish manufacturing (ISIC 3) balanced data, 1980-1995

| Agg. level | # of obs. | Gamma | Std. error | Adj. $R^2$ | LM-stat. | *P*-value |
|---|---|---|---|---|---|---|
| Baseline | | | | | | |
| 1-digit | 16 | 0.242 | 0.390 | –0.063 | 1.64 | 0.439 |
| 2-digit | 128 | 0.982 | 0.076 | 0.530 | 0.83 | 0.659 |
| 3-digit | 448 | 0.866 | 0.034 | 0.610 | 0.09 | 0.955 |
| Firm | 6,304 | 0.568 | 0.029 | 0.146 | 20.83 | 0.000 |
| Adjusted | | | | | | |
| 1-digit | 16 | 0.248 | 0.383 | –0.062 | 1.69 | 0.429 |
| 2-digit | 128 | 1.022 | 0.080 | 0.565 | 0.32 | 0.853 |
| 3-digit | 448 | 0.871 | 0.034 | 0.624 | 0.33 | 0.847 |
| Firm | - | - | - | - | - | - |

Notes: The estimates of the constant are not reported and standard errors are robust with respect to heteroscedasticity (White's procedure).

[28] Analogously, in order to compute this effect for the two-digit level, three-digit level estimates of the returns to scale are needed.

**Table B.2**: Aggregation-bias adjusted OLS estimates of the RTS

Swedish business sector (ISIC 2-6) balanced data, 1980-1995

| Agg. level | # of obs. | Gamma | Std. error | Adj. $R^2$ | LM-stat. | *P*-value |
|---|---|---|---|---|---|---|
| Baseline | | | | | | |
| 1-digit | 80 | 0.316 | 0.105 | 0.387 | 3.04 | 0.219 |
| 2-digit | 240 | 0.436 | 0.151 | 0.373 | 2.83 | 0.243 |
| 3-digit | 698 | 0.601 | 0.137 | 0.416 | 2.71 | 0.258 |
| Firm | 8,400 | 0.563 | 0.026 | 0.151 | 26.12 | 0.000 |
| Adjusted | | | | | | |
| 1-digit | 80 | 0.316 | 0.105 | 0.389 | 3.04 | 0.218 |
| 2-digit | 240 | 0.431 | 0.148 | 0.381 | 2.85 | 0.240 |
| 3-digit | 698 | 0.603 | 0.1490 | 0.411 | 2.45 | 0.294 |
| Firm | - | - | - | - | - | - |

Note: The estimates of the constant are not reported and standard errors are robust with respect to heteroscedasticity (White's procedure).

# Appendix C – Estimations in Sub-Periods

To show the impact of cyclical errors on the RTS estimates, we estimate the baseline equation (2.6) on a number of sub-periods. These periods are overlapping, which implies that the estimates are correlated. Table C.1 shows that although the period used for estimation matters, this do not, in general, remove the pattern of rising estimates.

**Table C.1** OLS estimates of the returns-to-scale parameter in sub-periods

| Level of | Gamma, estimated value | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Aggregation | 1987 | 1988 | 1989 | 1990 | 1991 | 1992 | 1993 | 1994 | 1995 |
| SIC 3 | | | | | | | | | |
| 1-digit | -0.05 | 0.05 | -0.22 | -0.68 | 0.73 | 0.62 | 0.94 | 2.35 | 1.03 |
| 2-digit | 0.95 | 0.97 | 0.91 | 0.88 | 1.09 | 1.11 | 1.07 | 1.23 | 1.09 |
| 3-digit | 0.91 | 0.90 | 0.87 | 0.85 | 0.90 | 0.97 | 0.95 | 0.97 | 0.93 |
| Firm | 0.65 | 0.66 | 0.65 | 0.64 | 0.64 | 0.64 | 0.66 | 0.69 | 0.70 |
| SIC 2-6 | | | | | | | | | |
| 1-digit | 0.51 | 0.48 | 0.39 | 0.31 | 0.38 | 1.09 | 1.24 | 1.25 | 1.24 |
| 2-digit | 0.66 | 0.63 | 0.58 | 0.54 | 0.62 | 1.07 | 1.20 | 1.22 | 1.20 |
| 3-digit | 0.86 | 0.86 | 0.84 | 0.83 | 0.86 | 1.00 | 0.97 | 1.00 | 0.98 |
| Firm | 0.62 | 0.63 | 0.62 | 0.61 | 0.61 | 0.61 | 0.64 | 0.66 | 0.66 |

Note: The columns show which eight-year period is used in the estimation: the heading 1987 means the period 1980-1987, the heading 1988 means the period 1981-1988, … , the heading 1995 means the period 1988-1995. The estimates of the constant are not reported and standard errors are robust with respect to heteroscedasticity.

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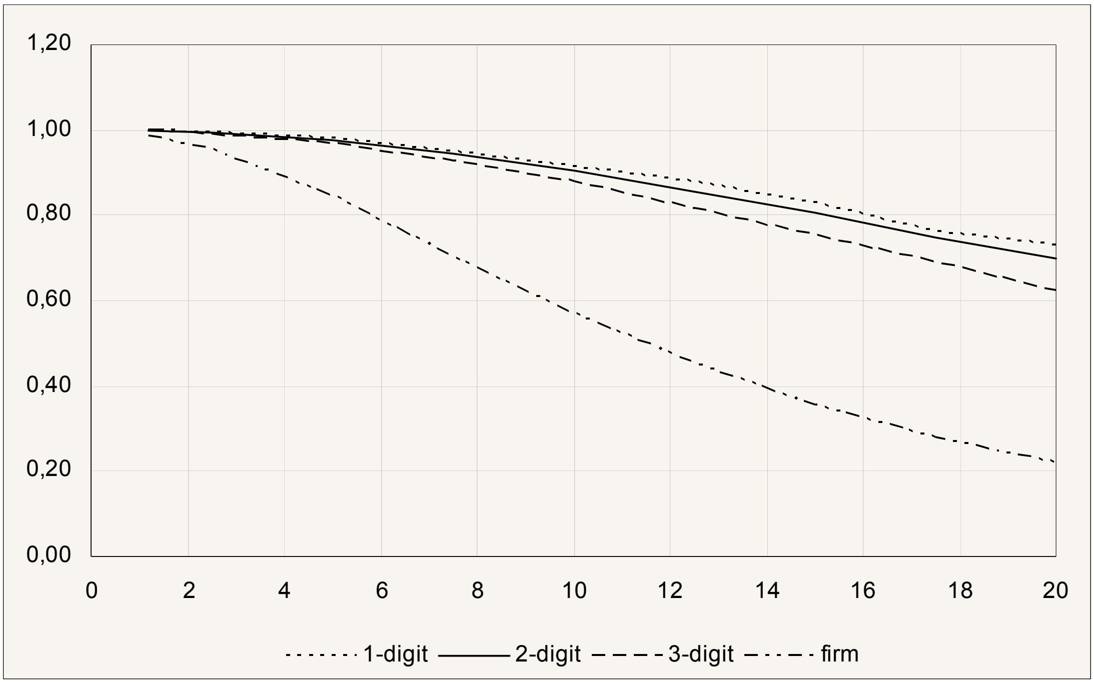
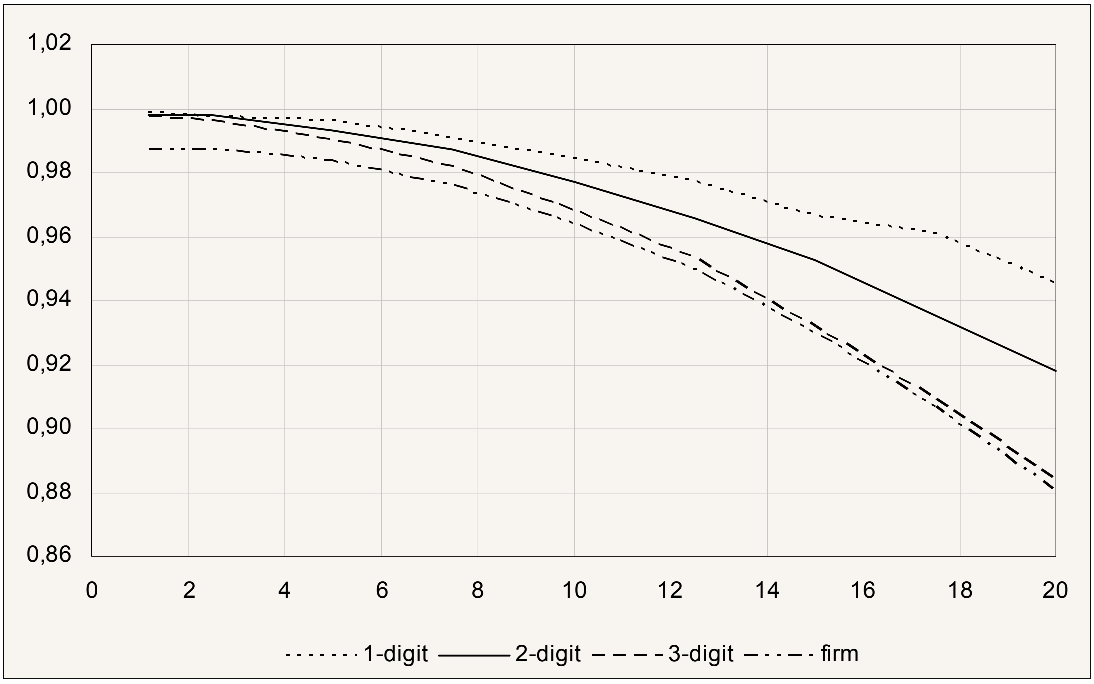
**Diagram 4.1:** Variance ratio as a function of average absolute error in labor



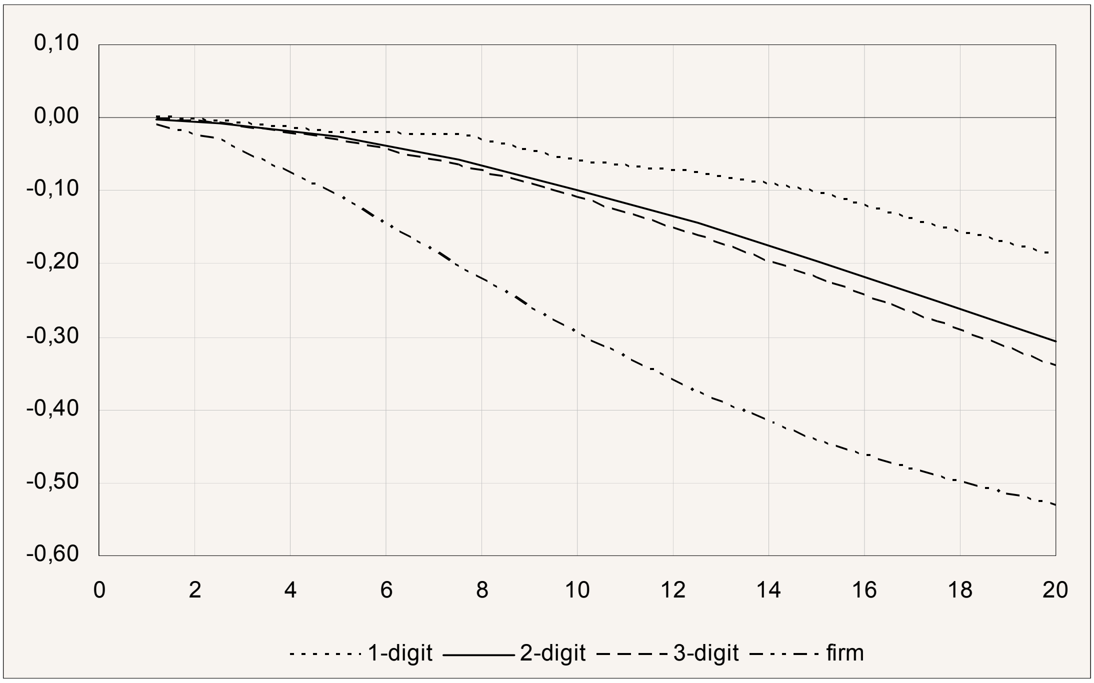

Note: The diagram shows the variance ratio $\sigma_{dx}^2/(\sigma_{dx}^2+\sigma_{\eta_2}^2)$ as a function of the average absolute error in the level of labor (working hours). The downward bias can be calculated from the diagram as one minus the variance ratio. The data in the diagram come from Monte Carlo simulations.

**Diagram 4.2:** Variance ratio as a function of average absolute error in capital



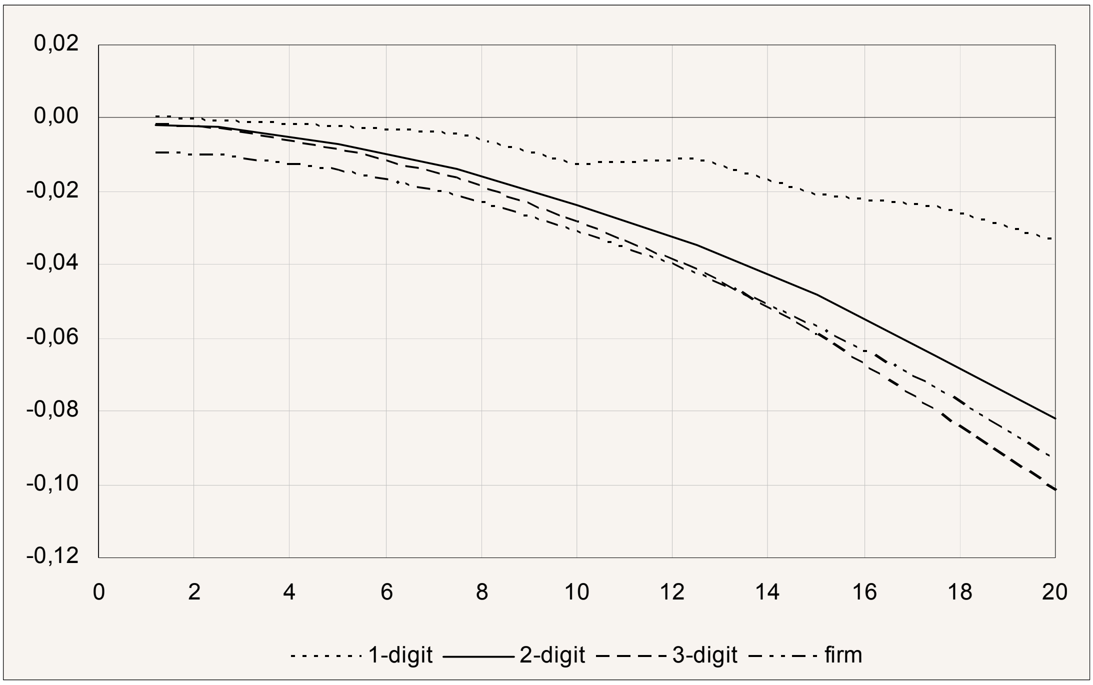

Note: The diagram shows the variance ratio $\sigma^2_{dx}/(\sigma^2_{dx}+\sigma^2_{\eta_2})$ as a function of the average absolute error in the level of capital. The downward bias can be calculated from the diagram as one minus the variance ratio. The data in the diagram come from Monte Carlo simulations.

**Diagram 4.3:** Estimated RTS bias as a function of average absolute error in labor



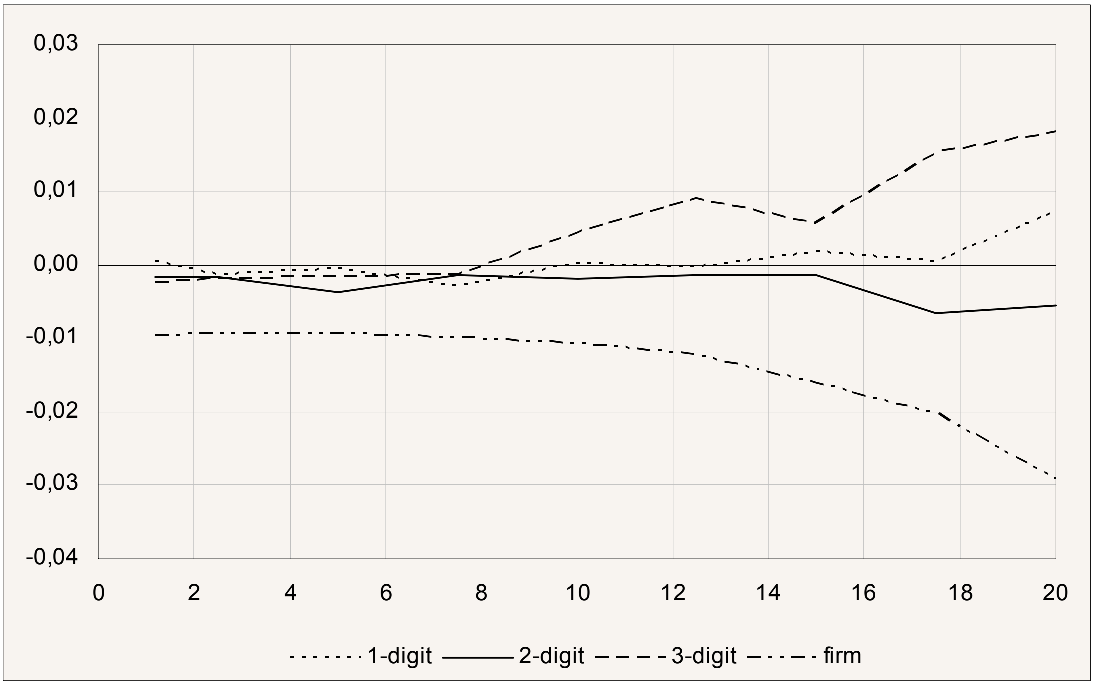

Note: The diagram shows the estimated RTS parameter bias as a function of the average absolute error in the level of labor (working hours). The data in the diagram come from Monte Carlo simulations.

**Diagram 4.4:** Estimated RTS bias as a function of average absolute error in capital




Note: The diagram shows the estimated RTS parameter bias as a function of the average absolute error in the level of capital. The data in the diagram come from Monte Carlo simulations.

**Diagram 4.5:** Estimated RTS bias as a function of average absolute error in output




Note: The diagram shows the estimated RTS parameter bias as a function of the average absolute error in the level of value-added output. The very small bias comes solely from the data processing (i.e., the omission of invalid and outlier values) and not from the errors in output (see equation (4.3), which shows that if the only source of measurement error is in measuring output, there would be no RTS bias provided that there is no other source of bias).

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