The Economic Journal, **126** (*September*), 1739–1773. Doi: 10.1111/ecoj.12214 © 2014 Royal Economic Society. Published by John Wiley & Sons, 9600 Garsington Road, Oxford OX4 2DQ, UK and 350 Main Street, Malden, MA 02148, USA.

WAGE ADJUSTMENT AND PRODUCTIVITY SHOCKS*

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We study how workers' wages respond to changes in firm-level physical productivity using Swedish data. We find that technology shocks affect workers' wages through both internal and external forces. Wages respond three times as much to physical productivity shocks that are shared with outside firms within the same sector as they do to firm-level physical productivity shocks. The larger impact of sectoral physical productivity is related to the degree of within-sector labour mobility, suggesting that the productivity evolution among firms that draw their labour from the same market segment is a crucial determinant of the wage growth of incumbent workers.

A large body of literature shows that real wages and productivity are intimately related at the aggregate level.[1] In contrast, much less is known about the relationship between technological efficiency and wages at the micro level, although productivity growth originates within establishments and firms. Notably, recent studies have shown that the dispersion of physical total factor productivity between firms operating in the same sectors is substantial (Hsieh and Klenow, 2009) and within-sector wage dispersion across firms is large and increasing (Nordström Skans *et al.*, 2009; Card *et al.*, 2013).[2]

A unifying result spanning across a large family of wage-setting models is that incumbent workers' wages should respond to changes in firm-specific technological efficiency, as well as to changes in the productivity of other firms that draw their labour from the same market segment.[3] However, separating the impact on wages of firm idiosyncratic movements in technical efficiency from that of productivity developments that are shared across firms imposes significant data requirements.

The study of the role of firm idiosyncratic technical change on wages gains importance in the presence of incomplete labour contracts, which can lead to a hold-up problem (Grout, 1984). When the investments needed to shift the technological frontier are sunk, workers may be able to extract part of the rents

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We thank three anonymous referees, John Abowd, Colin Cameron, Per-Anders Edin, Peter Fredriksson, John Haltiwanger, Marcel Jansen, Francis Kramarz, Rafael Lalive, Jean-Marc Robin, Nuria Rodríguez-Planas and seminar participants at SciencePo, the Helsinki Center of Economic Research (HECER), the Euro-System WDN network, the 6th ECB/CEPR Labor Market Workshop, IFAU, University of Oslo, Stockholm University, the Swedish National Conference in Economics, the 'Labor Market and the Macroeconomy' conference at Sveriges Riksbank, the LMDG workshop, the UCLS Industrial Relations workshop and the ESSLE and EEA, EALE/SOLE, and LACEA meetings for their useful comments. Carlsson and Nordström Skans gratefully acknowledge funding from the Ragnar Söderberg Foundation. The views expressed in this article are solely the responsibility of the authors and should not be interpreted as reflecting the views of the Executive Board of Sveriges Riksbank or the World Bank.

[1] In a recent example Carneiro *et al.* (2012) find an elasticity equal to unity of aggregate productivity on individual wages. For references to this literature, see also Carneiro *et al.* (2012).

[2] For the US, the large and increasing dispersion in productivity and wages across firms within-sectors is documented by Dunne *et al.*, 2004)

[3] In contrast, in the standard neoclassical model, firm idiosyncratic productivity does not affect wages; and in theories of internal labour markets (Doeringer and Piore, 1971) workers' wages are relatively insulated from outside disturbances.

from firms' technological progress in the form of higher wages. This may be particularly relevant in unionised labour markets, where workers have a higher bargaining power. The hold-up problem has long been a concern for economists because it is likely to reduce the incentives for firms to improve their technological efficiency through innovations or investments. Moreover, the empirical literature on rent-sharing has typically established a link between firm profitability and wages (see Card *et al.*, 2014 for a recent application). However, firms' profits may change due to changes in technical efficiency but they may also respond to other shocks (e.g. shifts in consumer preferences). Isolating the role of firm-level technical efficiency on wage developments provides a more direct estimate of the potential for hold-up channel to reduce the rate of technological improvements.

In this empirical study, we provide evidence of the distinct impact on incumbent wages of shocks to physical productivity that:

(*i*) are purely idiosyncratic to the employing firm; and
(*ii*) are shared with the firms that represent the workers' relevant outside options.

We identify the wage effects of these productivity shocks from shifts in the production function. This means that we use an empirical approach that handles confounding elements arising from endogenous adjustments along the production function, as well as relative price adjustments. To the best of our knowledge, this is the first study that documents that shocks to sector-specific outside options are empirically relevant for incumbent workers' wages after accounting for idiosyncratic firm-level shocks.[4] In addition, we document that the estimated magnitudes are closely related to empirical measures of intra-sectoral worker mobility.

Our analysis takes careful steps to identify the wage effects from changes in physical total factor productivity – that is, shifts in firm-level production functions. These are the closest empirical counterparts of the exogenous productivity processes postulated in most theoretical models focusing on the relationship between firm-level technical efficiency and wages (Smith, 1999; Postel-Vinay and Turon, 2010; Eeckhout and Kircher, 2011; Bagger *et al.*, 2013; Lamadon, 2014). However, the role of physical productivity is seldom isolated in the literature. A likely reason for this gap in the existing literature is that few data sets (see e.g. Foster *et al.*, 2008 and the survey by Syverson, 2011) allow the researcher to remove the impact of relative prices from measured productivity, which is needed to derive measures of firm-level physical total factor productivity. Notably, firm-level prices tend to be a function of factor prices, including wages (Carlsson and Nordström Skans, 2012). Firms with high costs (for instance, due to high wages) and thus high prices and firms with high physical productivity can therefore be separated from each other only if we can remove relative-price differences between them.

Empirically, our article is related to a large literature studying how various measures of firm performance affect the wages of incumbent workers. Some recent examples

[4] Importantly, analysing the direct wage impact of sectoral shocks without accounting for firms' idiosyncratic shocks does not allow the researcher to distinguish between the impact of sectoral outside options and the direct impact of idiosyncratic productivity.

include Van Reenen (1996), Guiso *et al.* (2005), Kramarz (2008) and Bell and Van Reenen (2011).[5] With the exception of Van Reenen (1996) none of these studies, however, isolates the impact of physical productivity shocks stemming from shifts in the firm-level production function. In addition, we are the first to disentangle the wage impact of purely idiosyncratic physical productivity shocks from that of shocks that are shared with other firms hiring from the same labour market segment.

Our analysis draws on a rich matched employer–employee panel data set from the Swedish manufacturing sector. Data include detailed information on worker and firm characteristics linked to firm-level price indices for the compound of goods that each of the firms sells.[6] This allows us to handle a number of key empirical challenges. Most notably, we are able to derive measures of physical total factor productivity (TFPQ, in the terminology of Foster *et al.*, 2008) using a firm-level production-function approach.[7] Once TFPQ is derived, we dissentangle purely firm idiosyncratic shocks from shocks that are shared with other firms within sectors, and study their joint impact on individual wages. These data additionally allow us to deal with worker sorting and fixed firm-specific factors in the empirical specification. Throughout, we exploit the matched employer–employee nature of our panel and estimate the wage impact of changes in technical efficiency in models with employer-by-employee (match) fixed effects. This implies that inference is made from time-varying firm-level productivity for ongoing matched worker–firm pairs, effectively allowing us to abstract from fixed firm-level wage policies, assortative matching and endogenous match quality. Although our analysis can handle all permanent differences across firms in terms of wage-setting policies, the analysis rests on the assumption that changes in firm-specific wage policies are not driving changes in TFPQ within firms.[8]

To preview our results, we find that both workers and firms benefit from firm-level technological advancements but workers benefit substantially more if the advancements are shared with other firms in the same sector – the elasticity of wages to shocks that are shared with other firms within the same sector is three times larger than the elasticity with respect to purely idiosyncratic shocks. This conclusion is robust across a wide set of exercises but crucially hinges on identification from movements in TFPQ, rather than movements in revenue-based total factor productivity (i.e. TFP derived using sectoral deflators, rather than firm-level prices). Notably, however, the variance in idiosyncratic shocks is three times as large as the variance in sectoral shocks, which

[5] Also, see Manning (2011) or Card *et al.* (2014) for thorough reviews of the rent sharing literature and, in particular, Abowd and Lemieux (1993) and Abowd and Allain (1996), who provide detailed empirical explorations of the impact of product market competition shocks in settings with bargaining and heterogeneous firms.

[6] Our main sample is composed of single-establishment firms, although we provide robustness checks including multi-establishment firms. Hence, through most of the article we use the terms ‘establishment’ and ‘firm’ interchangeably.

[7] We use a strategy similar to that of Smeets and Warzynski (2013) and deflate the (nominal) firm-level output series with firm-level price indices and use the term TFPQ for the ensuing TFP series. When calculating revenue-based TFP (TFPR), we use sector-level price indices to deflate firm-level output. However, in a strict sense we are not measuring physical output units of a homogeneous good (Foster *et al.*, 2008), but our empirical strategy handles cross-firm differences in quality by relying on firm fixed effects throughout.

[8] A complementing analysis, which investigates how the firm-specific skill composition and the returns to skills within firms relate to changes in TFPQ, shows that other aspects of firms’ personnel policies appear unrelated to our TFPQ shocks, lending some indirect support to this assumption.

implies that idiosyncratic and sectoral shocks to the firms' physical productivity are equally important as explanations for variations in wage growth across workers.

We also show that the larger impact of sectoral shocks on workers' wages is likely to be driven by market forces stemming from sector-specific outside options. Notably, sectoral shocks are found to matter more for workers who are more closely tied to their sector of employment. In sharp contrast to this result, we find that the impact of idiosyncratic shocks is completely independent of how closely tied the worker is to the original sector. Furthermore, in order to interpret the results, we derive a simple model of wage determination that allows for a distinct role for sectoral and idiosyncratic productivity movements on wages. Using the guidance of the model, we find that the relative magnitudes of the estimated impacts of sectoral and idiosyncratic productivity developments on wages are well in line with observed intra-sectoral mobility patterns of workers who change jobs. Finally, we show that the wage impacts of both idiosyncratic and sectoral shocks are shared equally across workers with different skills within the firm and we find no systematic relationship between the shocks and changes in the firms' skill composition.

Overall, our results show that changes in technological efficiency transmits into workers' wages through both internal and external forces, and that external forces have an important sector-specific component. Our results also imply that the role of sector-specific outside options can be assessed by exploring the degree to which workers' mobility patterns are sector-specific.

The rest of the article is organised as follows: first, we present our empirical strategy in Section 1. Section 2 provides details regarding the data and the measurement of TFPQ. Section 3 presents the main empirical results in the article and Section 4 discusses variations and robustness exercises. Finally, Section 5 provides a concluding discussion.

1. Empirical Strategy

The purpose of this study is to document separately the sensitivity of wages to movements in physical productivity that are shared within a sector and to movements that are truly idiosyncratic to each firm (i.e. orthogonal to sectoral productivity). In essence, this means that we think of the log wage (w) of worker i as being a function of a measure (a) of physical productivity (to be precisely defined below) at firm j during period t. Further, the firm-level productivity a_{jt} can be decomposed into a time-specific sectoral mean a_{st}^{S} and an orthogonal idiosyncratic residual a_{jt}^{I} with potentially different impacts on wages:

$$w_{ijt} = \eta_1 a_{st}^{S} + \eta_2 a_{jt}^{I} + h(\Phi_{ijt}), \tag{1}$$

where other determinants (to be discussed below) of individual wages that may vary with firm, worker, match and time are denoted by Φ_{ijt}.[9] In the remainder of this

[9] This simple framework is very similar to that of Guiso *et al.* (2005), with the key difference that they provide an elaborate discussion regarding the time-series dimension of the shocks, whereas our focus is on the nature of the shocks and the extent to which they are shared across firms. Although our basic framework is static, we show results from an extension that allows for dynamics in subsection 4.4.

Section, we outline the basic empirical strategy and the key empirical challenges that are faced when bringing (1) to the data, while deferring details about data and measurement to the following subsection.

1.1. *Firm-level physical productivity*

In order to estimate (1), we need a measure of sector and idiosyncratic productivity. To fix ideas, let firm j revenues R_{jt} at time t be determined by its output Y_{jt} and a firm-specific price P_{jt}. Further, assume a production function where gross output is produced using capital (K), labour (L), energy (V) and materials (M).[10] The firm-specific level of technology is captured by physical total factor productivity ($TFPQ_{jt}$), so that:

$$R_{jt} = P_{jt}Y_{jt} = P_{jt} \times TFPQ_{jt} \times f(K_{jt}, L_{jt}, V_{jt}, M_{jt}). \tag{2}$$

For reasons that will become clear later, our analysis uses two measures of physical productivity: Labour productivity and *TFPQ*. Labour productivity (LP_{jt}) is defined as

$$LP_{jt} = Y_{jt}/L_{jt} = TFPQ_{jt} \times f(K_{jt}, L_{jt}, V_{jt}, M_{jt})/L_{jt}. \tag{3}$$

As long as we can measure physical output, labour productivity is directly computable. However, it will be a function of processes such as changes in the use of other inputs, or the scale of production if the firm operates under non-constant returns to scale; processes that have little to do with the technology shocks that we have in mind. The second measure (*TFPQ*), on the other hand, is the residual productivity after accounting for changes in inputs, which implies that movements in *TFPQ* are less susceptible to alternative interpretations. However, it needs to be estimated.[11]

Our main empirical strategy is, therefore, to derive TFPQ at the firm level and use it as an instrument for labour productivity in an empirical specification that builds on (1). This strategy parallels the macro approach proposed by Haefke *et al.* (2013) but at the micro level. Using TFPQ as an instrument is convenient in our case, since the inference of the IV estimator is robust even when the instrument is a generated regressor (Wooldridge, 2002).[12] In addition, we use measures of labour input from two very different data sources when calculating physical labour productivity and TFPQ (see the data Section for details). This reduces the scope for classical measurement error bias, which could have generated spurious differences in estimates between the idiosyncratic and sectoral productivity measures.[13] In the end, however, neither qualitative nor quantitative conclusions change if we analyse the impact of TFPQ directly.

Notably, many firm-level data sources contain information about firm-level revenues but in order to measure physical output (and hence physical productivity), we need to

[10] Note that by defining a gross-output production function instead of a value added specification we are able to properly consider cases with non-constant returns to scale and imperfect competition (see Basu and Fernald, 1995 for a discussion).

[11] In practice, we do this using a gross production function approach with estimated returns to scale. See subsection 2.2 for details.

[12] In contrast, using TFPQ directly implies that the estimator suffers from the well-known generated regressor problem.

[13] Remaining sources of potential measurement errors are discussed in subsection 1.3 below.

deflate firm-level revenues by a firm-specific measure of prices. As noted by Klette and Griliches (1996), deflating gross output by a sectoral price index (P^S_{st}) generates a mix of output and relative prices since $\ln(Y_{jt}P_{jt}/P^S_{st}) = \ln Y_{jt} + \ln(P_{jt}/P^S_{st})$ and thus $\ln(TFPR_{jt}) = \ln(TFPQ_{jt}) + \ln(P_{jt}/P^S_{st})$, where *TFPR* demotes the total factor revenue productivity. Hence, standard firm-level *TFPR* series include technology shocks as well as everything else that affects the firm's relative price.

To illustrate the problems of using *TFPR* instead of *TFPQ* as an instrument in the wage equation, think of a simple monopolistic competition model in which firms face a constant-elastic demand function and set their prices as (constant) markups over marginal costs. Because marginal cost, under standard assumptions, is proportional to unit labour cost, the relative price will be a function of wages (for direct empirical evidence see Carlsson and Nordström Skans, 2012). To mitigate this problem, in the article we use a firm-level output-price index aggregated from unit prices for each good the firm produces to deflate firms' gross output, which allows us to obtain estimates of *TFPQ*.[14]

Relying on carefully constructed *TFPQ* series as an instrument of the wage equation allows us to identify the impact of changes in productivity on wages, while minimising the exposure of the analysis to this potential source of endogeneity. Importantly, using *TFPR* instead would affect the relationship between wages and our two shock measures (idiosyncratic and sectoral) differently. If, as expected, positive wage shocks are transmitted into higher prices and revenues, the estimated impact of firms' idiosyncratic productivity on wages using *TFPR* as an instrument would be upward biased, while the sectoral component is less likely to be affected. As we show, this prediction is confirmed by the data. This implies that using *TFPR* would invalidate the contrast between the impacts of the two types of shocks on wages.

1.2. *The empirical wage equation*

Following (1), our key empirical equation explains the log wage (w_{ijt}) of worker i in firm j at time t by the log physical labour productivity (i.e. output per worker) at the sector (lp^S_{st}) and idiosyncratic level (lp^I_{jt}):

$$w_{ijt} = \eta_1 lp^S_{st} + \eta_2 lp^I_{jt} + \rho_t + \alpha\theta_{lt} + \boldsymbol{\lambda}\chi_{it} + v_{ij} + \epsilon_{ijt}, \tag{4}$$

where η_1, η_2, α are coefficients, $\boldsymbol{\lambda}$ is a vector, and ϵ_{ijt} is a worker-firm-year specific disturbance. Since lp^S_{st} is an aggregate covariate, which only varies at the level of the sector, we cluster standard errors at the sectoral level (thus also allowing for serial correlation). Time effects are captured by ρ_t, and a θ_{lt} is a measure of local labour market tightness (letting l index local labour markets). Match-specific fixed effects are denoted by v_{ij} and controls for observable time-varying individual heterogeneity by χ_{it}. In our preferred specification, idiosyncratic and sectoral TFPQ ($TFPQ^I_{jt}$ and $TFPQ^S_{jt}$ respectively) are used as instruments for the sector and idiosyncratic physical labour productivity.

[14] See also Smeets and Warzynski (2013) for a similar strategy.

1.3. *Key identifying assumptions*

To account for aggregate confounders, we include time dummies and a measure of local labour market tightness in (4). Further, the model controls for individual, firm and match-specific fixed effects and time-varying individual heterogeneity. The model thus allows for an arbitrary set of (time-constant) firm characteristics, worker abilities or match quality to be correlated with firm-level productivity. To be precise, the firm fixed effects, which are present in all our models (and embedded in the match fixed effects), eliminate all firm-specific characteristics that remain constant over the period of observation, thus removing possible omitted variable biases stemming from time-constant working conditions or wage-setting policies that may affect both firm-level productivity and wages.[15] Worker fixed effects (also embedded in the match effects) eliminate possible composition biases associated with systematic changes in unobserved characteristics of the employees, for example sorting of more productive workers into more productive firms. Our most stringent specifications, which (4) displays, interact the firm and worker fixed effects into a set of match-specific fixed effects. This allows us to estimate the effect of interest even if, for example, poor matches are the first to be dissolved in response to negative productivity shocks.

Our identifying assumption is that, after controlling for aggregate factors, match-specific fixed effects and observable worker heterogeneity, shocks to TFPQ are structural, and hence uncorrelated with other fundamental shocks which may affect workers' wages.[16]

A key element of this assumption is that we assume that wage shocks have a negligible impact on TFPQ, conditional on time-varying worker and firm observable characteristics and the match-specific (employer-by-employee) fixed effects. This assumption would not be invalidated by, for instance, firms' use of efficiency wages, as long as the use of this instrument is kept fixed over time. Note, however, that our assumption implies that within-firm changes in the usage of efficiency wages over the sample period, if present, have a negligible impact on the estimates. Although we are unable to test this assumption, we do present some complementary exercises that analyse the relationship between the TFPQ series and other aspects of firms' personnel policies (the skill structures and skill returns within firms) in subsection 4.1.

A second key assumption is that differences in the estimated impacts of sectoral and idiosyncratic shocks are not due to differences in the level of measurement error in the two variables. Our IV strategy outlined above allows us to handle measurement errors arising from the labour input side and from the generated TFPQ series. We are, however, still potentially exposed to measurement errors arising from measured firm-level output. To provide some additional insights into the likelihood that measurement errors are driving our results, we empirically explore in detail the rationales for a different response of individual wages to sector and idiosyncratic productivity shocks. In particular, we analyse the extent to which differences between the estimated effects

[15] For a discussion, see Daniel and Sofer (1998).
[16] Including, for example, product innovations or other market expanding demand shocks.

are related to workers' outside options, as summarised by the observed workers' mobility patterns in subsection 3.4.

It should also be noted that, although we exploit the unique features of our data sets to minimise the exposure of the estimated impact of productivity on wages to a large set of possible confounder factors, some concerns may remain if our corrections are imperfect. We, therefore, present multiple robustness checks to assess the importance of aspects including measurement errors in our firm-specific prices (see subsection 3.1), lack of precision in the estimated returns to scale that are needed to derive the TFPQ measures (subsection 4.3) or alternative definitions of the estimation sample, including multiplant firms in some specifications and reducing it to single-product firms in others (subsection 4.3).

2. Data and the Measurement of TFPQ

2.1. *Data*

We combine four data sources covering the Swedish manufacturing industry to construct our used sample. The raw data, covering all manufacturing and mining plants with 10 employees or more, come from the Industry Statistics Survey (IS). They contain annual information for the years 1990–6 on inputs and output as well as geographical location of each plant. To this we add a firm-specific producer price index (PPI) constructed by Statistics Sweden[17] and a matched employer–employee database (register-based labour market statistics, or RAMS) originally compiled by the Swedish Tax Authority. Unemployment and vacancy data at the local labour market level are collected from the National Labour Market Board.

Statistics Sweden's firm-level price index combines firm-specific unit values and detailed disaggregate producer-price indices using Paasche links (to measure the price change between years), which are then used to construct a chained price index. If a firm-specific unit-value price is missing, for example when the firm introduces a new good, Statistics Sweden uses a price index for similar goods defined at the minimal level of aggregation (starting at the four-digit goods code level). Except in the TFPR analysis, which serves as a contrast to our main analysis (to get TFPR, we use a three-digit sectoral PPI as the deflator instead), we deflate output by the firm-level prices throughout the article.

We use the labour-input measure available in the IS to compute labour productivity, whereas the labour-input measures used when estimating TFPQ are taken from RAMS. This ensures that the first-stage relationship between labour productivity and TFPQ is not due to common measurement errors in the labour-input measures.

We derive a measure of monthly wages from information on annual earnings and the duration of employment spells, closely following the procedures of Nordström Skans *et al.* (2009) and Carlsson and Nordström Skans (2012). The data lack information on actual hours, so to restrict attention to workers reasonably close to

[17] In order to construct the PPI, Statistics Sweden uses data from the Industrins Varuproduktion Survey.

full-time workers we only include workers whose (monthly) earnings exceed 75% of the minimum wage.[18] The data also include information on age, gender, education and immigration status of the individual workers.

Our baseline analysis focuses on continuing single-plant firms, which offer the cleanest identification but the robustness Section relaxes this restriction. We focus the main analysis on the balanced sample because it allows us to abstract from potential differences in wage-setting practices across plants within the same firm, potential issues with firms consisting of plants belonging to different sectors and potentially heterogeneous productivity shocks across plants within the same firm. The focus on continuing plants can also mitigate possible selection effects due to firm demographics associated with productivity shocks. In the robustness Section, we analyse an unbalanced sample with multi-plant firms to address potential concerns regarding the external validity of the results.

Finally, we turn to a description of the creation of our used data sets (Appendix A provides further details). As explained in the next subsection, our empirical models require the use of three lags within our differenced fixed effects setting, and the raw data set consists of 5,141 plant-sequences that are long enough to fit into these models. Out of these, 2,325 plants are single-plant firms existing throughout the data period. Matching between data sets leaves us with 1,298 plants, where the bulk of lost observations (958 plants) is lost when matching on the required information (workers belonging to each education group) from the RAMS data to the IS data.

Our main data set used, after removing extreme values, is a balanced panel of 1,136 firms observed over the years 1990–6 and 472,555 employee/year observations distributed over 106,050 individuals, covering 11% of the full-time employees in the manufacturing sector. In robustness exercises (in subsection 4.3), we also show results for an unbalanced version, also including observations from multi-plant firms, of our data set. These data cover about twice as many plants (2,444) observed over the same years, and 2,048,555 employee/year observations distributed over 452,562 individuals, covering 48% of full-time employees in the manufacturing sector. Subsection 4.3 also presents robustness exercises for the one-tenth of the preferred sample (135 firms) that are mono-product firms, and Appendix E shows results for a sample which includes extreme values.

2.2. *Measuring of TFPQ*

This subsection explains how we calculate TFPQ in practice. The calculations are based on observed inputs, cost shares and estimated returns to scale while accounting for varying factor utilisation (details can be found in Appendix B). Postulating a well-behaved physical gross output production function and invoking cost minimisation, we write firm *j*'s change in log physical output (Δy_{jt}) as a function of the change in the log

[18] The minimum wage is defined by the wages of janitors. Using the same procedure with RAMS data, Nordström Skans *et al.* (2009) found that this gives rise to a computed wage distribution that is close to the direct measure of the wage distribution taken from the 3% random sample in the Longitudinal Individual Data Base (LINDA), where hourly wages are the measure of pay.

of cost-share-weighted inputs ($\Delta\tilde{x}_{jt}$) and changes in firm-specific level of technology ($\Delta tfpq_{jt}$) while denoting the overall returns to scale by (ψ_j) as follows:

$$\Delta y_{jt} = \psi_j \Delta\tilde{x}_{jt} + \tau_t + \Delta tfpq_{jt}, \tag{5}$$

where τ_t denotes time dummies intended to capture any aggregate trends in technology growth. The input index includes labour and capital services as well as intermediate inputs. It accounts for differences in worker composition by separating workers by their level of education.[19] We further follow Burnside *et al.* (1995) and rely on measures of energy consumption to proxy the flow of capital services. This procedure is well suited for our manufacturing-sector data where increases in the flow of capital services are likely to be matched by changes in electricity consumption. We do, however, also provide a robustness exercise using a direct capital-stock measure in Appendix E.[20]

Our measure of technology shocks ($\Delta tfpq_{jt}$) reduces to a physical gross-output Solow residual if $\psi_j = 1$ and if there are no economic profits.[21] Hence, $\Delta tfpq_{jt}$ is a physical Solow residual purged of the effects of non-constant returns, imperfect competition and varying factor utilisation.[22]

Empirically, we treat the output elasticities as constants, following the logic of log-linearisation around the steady-state path as in Basu *et al.* (2001) although we discuss variations in the robustness section. Treating elasticities as constants precludes variation in wages from spilling into the TFPQ measure if, for any reason, the output elasticity of labour input is mismeasured. To get the cost shares, we use the average cost share by two-digit industry across the sample period but we also present results from robustness exercises using three-digit industry shares in Appendix E. The cost share of capital and energy is calculated as one minus the sum of the cost shares for all other factors, under the assumption that firms make zero profit in the steady state.[23] As noted by Basu and Fernald (1995), zero profits in equilibrium are consistent with a markup if the markup is equal to the returns to scale.

Details regarding the estimation of returns to scale are presented in Appendix C. The estimate of the returns to scale equals 0.99 for the durables sector and 0.88 for the non-durables sector (see Table 1) but both are somewhat imprecisely estimated (SE = 0.19 and 0.22 respectively). Reassuringly, the point estimates are very similar to

[19] Changes in unobservable skills or match quality will affect the technology measures and their estimated distributions but not the results of primary interest since (4) accounts for match-specific fixed effects.

[20] Throughout, we assume that labour utilisation is constant. Notably, Carlsson (2003) estimates production functions that are similar to (5) on Swedish two-digit manufacturing industries using various proxies for labour utilisation (hours per employee, overtime work per employee and the frequency of industrial accidents per hour worked) and finds that these have no impact on the results. He also reports that the growth rate of hours per employee is acyclical in the same data. Thus, we are not likely to leave out any important variation in labour input by looking only at the growth rate of the extensive margin.

[21] The zero-profit condition implies that the factor-cost shares in total costs equal the factor-cost shares in total revenues, which are used when computing the Solow residual.

[22] To make the measurement of technology consistent with an imperfectly competitive labour market, we implicitly assume a timing sequence where the wage is determined first and then the firm takes the wage as given when making its input and production choices. Moreover, wage-bargaining agents are assumed to recognise this right-to-manage set-up.

[23] Using the sectoral level data underlying Carlsson (2003) we find that the time average (1968–93) for the share of economic profits in aggregate Swedish manufacturing revenues is about −0.001, thus supporting the assumption made here.

Table 1

Returns to Scale

Industry	RTS
Durables	0.986
	(0.194)
Non-Durables	0.882
	(0.224)
Observations	5,680
Firms	1,136
AR(2)	[0.210]
AR(3)	[0.886]
Hansen	[0.296]

Notes. Sample 1991–6 with 1,136 firms. Difference GMM second-step estimates with robust Windmeijer (2005) finite-sample corrected standard errors in parenthesis. See main text for instruments used. Regression includes time dummies and firm fixed effects. p-values for diagnostic tests inside square brackets.

those of previous studies.[24] In the robustness Section, we also address the sensitivity of the results to the estimated returns to scale.

Conditional on estimated returns to scale and the cost shares, we get $\Delta tfpq_{jt}$ from (5). We then integrate these growth rates into a log-level technology series using a straightforward recursion, leaving behind a set of unobserved firm-specific constants (see Appendix B for details). These constants are captured by firm fixed effects in the wage regressions.

Finally, to obtain the idiosyncratic and sectoral components of labour productivity and TFPQ, we run regressions of these measures on sector-specific time dummies. The projections from the sector-specific time dummies in this regression provide measures of lp_{st}^{S} and $tfpq_{st}^{S}$, whereas the residuals measure lp_{jt}^{I} and $tfpq_{jt}^{I}$.[25] For our preferred definition of a sector, we use data on the employer federation of the firm because these federations are set up to handle homogenous segments of the labour market, but we present results from several alternatives.[26]

2.3. *Summary Statistics*

Summary statistics for measured wages and labour productivity as well as estimated TFPQ are presented in Table 2. The dispersion of productivity is much larger than the dispersion of wages but this relationship is largely driven by permanent differences between firms. The variance (over time) within an employment spell (i.e. a match between a worker and a firm) is about equal for the two variables. In the analysis, we

[24] Basu *et al.* (2001) report estimates of 1.03 and 0.78 for durables and non-durables respectively using US sectoral data.

[25] We use employee weights when running this decomposition, such that sector-specific productivity is the average employee-weighted productivity and idiosyncratic productivity is the firm-level deviation from this average.

[26] In practice, we allocate the firm to the most common employer federation among firms in the same five-digit industry according to the standard SNI92 (NACE) classification.

Table 2

Summary Statistics

	All	
	Mean	SD
Wages:		
w_{ijt}	9.615	0.313
w_{ijt} (within match)	–	0.146
Productivity:		
lp_{jt}	6.835	0.667
lp_{jt} (within match)	–	0.155
lp^{S}_{st}	–	0.049
lp^{S}_{st} (within match)	–	0.036
lp^{I}_{jt}	–	0.669
lp^{I}_{jt} (within match)	–	0.130
TFP		
tfp^{S}_{st} (within match)	–	0.022
tfp^{I}_{jt} (within match)	–	0.092
Worker characteristics:		
Age_{ijt}	39.8	11.8
Share of male	0.794	
Share of HE	0.511	
Share of TE	0.122	
Share of non-immigrants	0.895	
Firm-size	212.6	
Observations	472,555	

Notes. The ‘within match’ rows show the dispersion within a combination of person and firm. All statistics are weighted according to the number of employees.

distinguish between sectoral and idiosyncratic components, as discussed above. The sectors are identified following the 16 employer federations that sign collective agreements in the manufacturing sector. When decomposing productivity within and between sectors, we see that the within-match standard deviation of idiosyncratic firm-level productivity is more than three times larger than the variance in sectoral productivity.

3. Results

3.1. *Main Results*

We proceed by estimating (4) to investigate the role of sectoral and idiosyncratic productivity for the evolution of individual wages. The first three columns of Table 3 show the main results. These results all stem from regressions using idiosyncratic and sector-level TFPQ as instruments to isolate the effects of shifts in the physical production function. Column (1) only accounts for firm fixed effects and year dummies, whereas column (2) presents our preferred, and most stringent, specification which includes match-specific fixed effects. As sectoral productivity is shared by all observations at the sector-year level, and since we want to allow for serial correlations in these shocks, we cluster all standard errors at the sectoral level which means that they

Table 3

The Impact of Productivity on Individual Wages

	(1)	(2)	(3)	(4)	(5)
Estimation method:	IV	**IV**	OLS	OLS	IV
Instrument:	TFPQ	**TFPQ**	–	–	TFPR
Regressor:	Prod	**Prod**	TFPQ	Prod	Prod
lp_{st}^{S}	0.123**	**0.149****		0.043	0.112**
	(0.057)	**(0.057)**		(0.042)	(0.046)
lp_{jt}^{I}	0.032***	**0.051*****		0.040***	0.092***
	(0.009)	**(0.006)**		(0.004)	(0.014)
tfp_{st}^{S}			0.124**		
			(0.043)		
tfp_{jt}^{I}			0.042***		
			(0.004)		
Firm FE	Yes	–	–	–	–
Worker FE	No	–	–	–	–
Worker characteristics	No	**Yes**	Yes	Yes	Yes
Worker by Firm FE	No	**Yes**	Yes	Yes	Yes
Observations	472,555	**472,555**	472,555	472,555	472,555
Firms	1,136	**1,136**	1,136	1,136	1,136
Worker by firm matches	–	**107,086**	107,086	107,086	107,086
p-value	0.067	0.049	0.035	0.467	0.314

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses. All specifications include time effects and labour market tightness. Worker characteristics includes age, age squared and age cubed. Specification in bold (column 2) represents the preferred (baseline) specification which we rely on for the rest of the article. 'p-value' denotes tests of the hypothesis that firm-level shocks that are shared within a sector have a larger impact than purely idiosyncratic shocks. Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom.

are robust to arbitrary correlations within sectors. To account for the fact that we have relatively few (16) clusters, we calculate p-values according to the student-t distribution with 15 degrees of freedom as suggested by Cameron and Miller (2014).[27] Table 4 shows that the first-stage regressions are well behaved.

The overall message is that both firm-level idiosyncratic labour productivity movements and movements that are shared within a sector matter for wage determination, but the impact of the latter is much larger. Column (1) of Table 3 reports an elasticity of wages to sectoral productivity of 0.123, which is substantially larger than the estimate of 0.032 for the elasticity with respect to idiosyncratic productivity. Both estimated coefficients are statistically different from zero at the 5% level (at least) and the p-value shows that the sectoral impact is significantly larger than the idiosyncratic effect. The results become marginally larger when we account for worker heterogeneity and match quality in column (2) which accounts for individual observed and unobserved heterogeneity by means of an age polynomial and match fixed effects. The idiosyncratic component increases to 0.051, while the sectoral

[27] In regressions where there is within-sector variation in all variables of interest, we instead cluster at the firm level.

Table 4

First-stage Regressions Corresponding to Table 3

	(1)	(2)	(3)	(4)
Estimation method:	OLS	OLS	OLS	OLS
Dependent variable:	lp^S_{st}	lp^I_{jt}	lp^S_{st}	lp^I_{jt}
tfp^S_{st}	0.854**	0.000	0.842**	−0.020
	(0.291)	(0.002)	(0.289)	(0.013)
tfp^I_{jt}	−0.000	0.846***	0.000	0.838***
	(0.000)	(0.041)	(0.001)	(0.042)
Firm FE	Yes	Yes	–	–
Worker FE	No	No	–	–
Worker characteristics	No	No	Yes	Yes
Worker by firm FE	No	No	Yes	Yes
Observations	472,555	472,555	472,555	472,555
Firms	1,136	1,136	1,136	1,136
Worker by firm matches	–	–	107,086	107,086
F-Stat ($tfp^S_{st} = tfp^I_{jt} = 0$)	4.38**	338.75***	4.26**	233.76***

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses. Regressions also include time effects and labour market tightness. Worker characteristics (columns 3–4) include age, age squared and age cubed. F denotes the F-statistic for excluded instruments. Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom.

component increases to 0.149. The impact of sectoral shocks is significantly larger than that of idiosyncratic shocks also in this case.

In column (3), we instead show the direct effect of TFPQ on wages, paralleling the specification in column (2) but using OLS with TFPQ as the regressor instead of as the instrument. We find very similar, but marginally smaller (0.124 and 0.042), effects compared to column (2). This is not surprising considering that the first stages of the IV regressions (Table 4) are close to, but somewhat smaller than, unity.[28] Since TFPQ is a generated regressor, the model which uses TFPQ directly as the regressor becomes sensitive to potentially attenuating measurement errors and we therefore consider the IV specification as the preferred model.

To highlight the role of TFPQ for identification, columns (4) and (5) show results from regressions using alternative identification strategies. First, using a simple model where we estimate the direct impact of labour productivity on wages without the instrument (column 4) would have suggested that idiosyncratic productivity shocks have the same impact as shocks that are shared within a sector (0.040 *versus* 0.043) and the impact of sectoral shocks is not statistically different from zero. Second, and more importantly, we argued in Section 1 that not accounting for relative price differences within a sector is likely to generate a positive bias in the estimated impact of idiosyncratic productivity if relative prices are endogenous to changes in firm-level costs or product demand. This conjecture is supported by results presented in column (5), where we use three-digit PPI deflators instead of firm-level prices to derive gross

[28] That the first stages are close to unity suggests that the endogenous response of labour productivity through changes in input usage is not very large. This is reassuring since we are using a balanced panel and, because of the relatively short period available, we are unable to model exits of firms. Note, though, that Section 4 shows robustness checks with a larger, unbalanced panel.

output, thus effectively replacing TFPQ with TFPR. The estimated elasticity of idiosyncratic productivity is now almost twice the size (0.092) of the benchmark and very close to the sectoral estimate (0.112). Thus, as when relying on endogenous labour productivity without instruments, relying on TFPR would have led us to underestimate the relative importance of sectoral shocks. Although the prices used for calculating TFPR are considerably more crude than the imputed values used for parts of our firm-level price data, this result also suggests that any potential biases arising from imputed values in the firm-level price index should lead us to overestimate the impact of idiosyncratic shocks and, consequently, would reduce the difference with respect to the impact of the sectoral shocks. In that sense, our main results present a lower bound of the differences between the idiosyncratic and sectoral shocks.

In Section 4 below, we present a number of alternative specifications using alternative measures of productivity and different sample selection criteria. To preview our results, in all cases we find substantially larger effects for shocks that are shared within a sector as long as we focus on shocks that stem from changes in TFPQ.

3.2. *Magnitudes*

Regarding the magnitudes of the estimates, it is clear that elasticities for both idiosyncratic and sectoral shocks are far from unity. For the interpretation of the estimates, it is, however, crucial to note that we are analysing productivity shocks at the firm level rather than match-level shocks as in, for example the standard Mortensen-Pissarides model (Pissarides, 2000). This implies that the productivity advances remain with the firm if the worker is replaced, thus effectively reducing the workers' ability to extract rents relative to the one-worker-per-firm case with match-specific productivity (for a discussion see Faberman and Nagypál, 2008). It is also important to bear in mind that the variance in the underlying productivity processes is relatively large. This is especially true in the case of idiosyncratic firm-level productivity. Removing variation between firms and using our preferred estimates in column (2) of Table 3, we find that an increase of 1 SD in either of the productivity measures (sector or idiosyncratic) raises wages by about one-quarter of the average real wage growth in our sample.[29]

3.3. *Market Forces or Sectoral Bargaining?*

In Sweden, as in many other OECD countries, wage bargaining has a multitiered structure with negotiations at the industry level as well as at the firm level. Thus, the larger elasticity of wages with respect to sectoral shocks may be explained by relatively higher union bargaining power during sectoral negotiations. To test whether the sectoral effects stem from higher bargaining power at the sectoral level or from market

[29] Note that our estimated elasticities net out aggregate productivity effects, since all regressions include time dummies and labour market tightness. Considering that average real wage growth within the manufacturing establishments included in the sample is 2.4%, the estimated impact of 1 SD idiosyncratic productivity on wages amounts to 28% ($0.051 \times 0.130/0.024$) of this average real wage growth, while the impact of 1 SD sectoral productivity is 22% ($0.149 \times 0.036/0.024$).

Table 5

Market Forces Versus Bargaining Power

Estimation method:	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV
lp_{st}^{S}	0.149**	0.029			0.141***
	(0.057)	(0.170)			(0.037)
$lp_{st}^{S,Weighted}$		0.199	0.244**		
		(0.291)	(0.100)		
$lp_{st}^{S,NACE}$				0.203**	
				(0.093)	
$lp_{st}^{S}\hat{\phi}_{ijt}$					1.807***
					(0.694)
lp_{jt}^{I}	0.051***	0.051***	0.051***		0.050***
	(0.006)	(0.006)	(0.006)		(0.010)
$lp_{jt}^{I,NACE}$				0.043***	
				(0.008)	
$lp_{jt}^{I}\hat{\phi}_{ijt}$					−0.016
					(0.153)
Worker characteristics	Yes	Yes	Yes	Yes	Yes
Worker by firm FE	Yes	Yes	Yes	Yes	Yes
Observations	472,555	472,555	472,555	472,555	472,555
Firms	1,136	1,136	1,136	1,136	1,136
Worker by firm matches	107,086	107,086	107,086	107,086	107,086

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses for columns (1)–(4). In column (5), the variable of interest varies on the individual level and standard errors are therefore clustered on firms instead. All specifications include time effects and labour market tightness. Worker characteristics include age, age squared and age cubed. The regression in column (5) also includes the main effect from the predicted return probability (ϕ). 'p-value' denotes tests of the hypothesis that firm-level shocks that are shared within a sector have a larger impact than purely idiosyncratic shocks. Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom for columns (1)–(4).

forces, we derive an alternative measure of sectoral productivity that uses workers' mobility patterns (instead of the firms' employer federations) to measure the relevant outside labour market. More precisely, we calculate a transition-probability-weighted productivity index for each sector. To get the value of the index for sector *A*, we take the fraction of workers moving from sector *A* to sector *B* as a weight for the productivity of sector *B*, then repeat for all 16 sectors in the sample (including inbreeding within sector *A* itself) and, finally, calculate the weighted sum of the sectoral productivities.

Using these transition probabilities, we get a mobility-weighted productivity index that allows us to separately estimate the impact of:

(*i*) worker outside options as measured by the mobility-weighted sectors; and
(*ii*) the impact of productivity developments within the bargaining sector.

As can be seen in column (2) of Table 5, adding this mobility-weighted sectoral labour productivity index, $lp_{st}^{S,Weighted}$, in the baseline regression (using a similarly mobility-weighted sectoral TFPQ index as an instrument) brings down the point estimate of the bargaining sector to close to the point estimate of the idiosyncratic effect (from 0.149 to 0.029). Moreover, the point estimate of the mobility-weighted sectoral index is about four times larger than the point estimate of the idiosyncratic effect (0.199 *versus* 0.051). Thus, the results are more in line with an outside-option interpretation than with a sectoral wage-bargaining story, although the statistical precision becomes an issue since the two measures are highly correlated because of the high frequency of intra-sectoral mobility (running a regression of one on the other yields a within-sector R^2 of 0.93).

In the third column of Table 5, we drop the sectoral productivity index and obtain a statistically significant estimate of the effect from the mobility-weighted productivity index (0.244), which is larger than that of the baseline specification with only the bargaining sector. The fact that the point estimate is larger for the mobility-weighted variable than for the bargaining sector variable lends additional support to the market forces interpretation of the results, because shifting to the mobility-weighted sectoral index should merely add measurement errors to the sectoral variable if the underlying driving force is sectoral wage-bargaining.

Similarly, in the fourth column we instead rely on two-digit industry codes according to the SNI92/NACE classification to capture the sectors and find somewhat larger sectoral results than in the baseline specification. In Appendix E, we also show results for three-digit SNI92/NACE classifications finding similar results but (as expected) with smaller but statistically significant differences between the sector and the idiosyncratic shocks.[30] Our overall conclusion, therefore, is that the additional impact from shocks that are shared within a sector are primarily driven by effects that work through workers' outside options.

3.4. *Relationship to Mobility Patterns*

To get a sense of how the relative magnitudes of the sectoral and idiosyncratic estimates relate to alternative measures of how sector-specific the labour markets are, it is useful to think of wages being set as a weighted sum of idiosyncratic firm-level productivity (where the weight is denoted by β) and outside options in the spirit of empirical rent-sharing models (Manning, 2011). We divide outside options into a sectoral and an aggregate component. Here, the weight on the sectoral component (ϕ) should be interpreted as the additional importance placed on outside options within the own sector (e.g. due to sector-specific human capital as in Rogerson, 2005). Thus, individual wages are expressed as follows:

$$W_{jt} = \beta LP_{jt} + (1-\beta)[\phi W^S_{st} + (1-\phi)\Omega_t], \tag{6}$$

where W_{jt} is the wage of workers employed by firm j at time t, β captures workers' ability to extract rents from firm-specific productivity advancements and LP_{jt} denotes the firm's productivity level. Letting LP^S_{st} denote the average productivity in sector s; defining the average wage in the sector as the average wage in the relevant firms;[31] and defining the firms' idiosyncratic productivity component as the

[30] The three-digit specification is also interesting since it allows us to rely on a larger number of clusters for the standard errors, avoiding potential concerns regarding small sample properties.

[31] More specifically, $W^S_{st} = \beta LP^S_{st} + (1-\beta)[\phi W^S_{st} + (1-\phi)\Omega_t]$.

deviation from the sector average ($LP_{jt}^{I} = LP_{jt} - LP_{st}^{S}$) as in our empirical analysis, we get

$$W_{jt} = \left[\frac{\beta}{1-(1-\beta)\phi}\right] LP_{st}^{S} + \beta LP_{jt}^{I} + \left[\frac{(1-\phi)(1-\beta)}{1-(1-\beta)\phi}\right]\Omega_t. \quad (7)$$

Intuitively, the impact of sectoral shocks should be larger the more closely tied a worker is to the sector of employment (i.e. the larger ϕ is in terms of (7)). To investigate whether this prediction is matched in the data, we interact the variables of interest with measures of worker-specific predicted probabilities of returning to the same sector. To this end, we predict the probability of returning to the same sector for job movers using very detailed information on field and level of education alongside basic demographic variables, using all job changes in the RAMS data within our 16 sectors during the sample period 1990–6 (120,652 observations).[32] We then interact the out-of-sample prediction of intra-sectoral mobility with the variables of interest. The results are displayed in the last column of Table 5. Notably, sectoral shocks are found to have a significantly larger impact for workers who are more closely tied to their sector. The magnitude of the estimated interaction effect (1.81) implies that the sectoral effect varies from 0.074 to 0.208 within the range of two standard deviations of predicted intra-sectoral mobility.[33] In sharp contrast, we find that the impact of idiosyncratic shocks is independent of predicted intra-sectoral mobility. The results thus suggest that workers who are more closely tied to their sector of origin receive a larger additional wage impact from sectoral shocks in both absolute and relative terms. We believe that these results are reassuring in the sense that it seems unlikely that differences between idiosyncratic and sectoral shocks would be systematically related to the predicted cross-sectoral mobility patterns of the workers if the main results primarily were driven by measurement errors in firm-level output.

The simple model also provides a quantitative baseline for the relationship between how sector-specific the labour market is, on the one hand, and the relative wage impact of sectoral and idiosyncratic shocks, on the other hand. Obviously, the key parameter is ϕ; if $\phi = 0$, idiosyncratic and sectoral shocks are of equal importance. To get a sense of the estimated magnitudes, we calculate the (average) empirical counterpart of ϕ, that is the excess probability of returning to the same sector among moving workers. Using all job changes in the RAMS data within our 16 sectors during the sample period 1990–6 (120,652 observations), we calculate the difference between the conditional probability of staying within the same sector when changing employer and the unconditional probability of entering each sector when changing employer (regardless of the sector of origin). This generates an estimate of the excess probability of returning to the same sector (corresponding to ϕ) of 0.54.[34]

[32] We predict the probability of returning to the same sector using three-digit field and two-digit level (both ISCED 97) of schooling alongside gender, an age polynomial of order three, immigration background and year dummies. The model also includes dummies for the sector of origin, but the impact of these is not included in the prediction.

[33] The standard deviation of the prediction is 3.7%.

[34] Defining a residual bargaining sector for all workers outside the 16 sectors of the sample and using all data on transitions (660,859 observations) yields a very similar estimate of $\phi = 0.52$.

Table 6

Complementarities and Sorting

	(1)	(2)	(3)
Estimation method:	IV	IV	IV
Dependent variable:	w_{ijt}	w_{ijt}	$\widehat{\omega}_i$
lp^S_{st}	0.170**	0.152***	−0.024
	(0.062)	(0.039)	(0.016)
$lp^S_{st} \times HE_{it}$	−0.054		
	(0.037)		
$lp^S_{st} \times TE_{it}$	0.065		
	(0.144)		
$lp^S_{st} \times \hat{\omega}_i$		−0.017	
		(0.021)	
lp^I_{jt}	0.047***	0.052***	−0.003
	(0.011)	(0.010)	(0.004)
$lp^I_{jt} \times HE_{it}$	0.005		
	(0.007)		
$lp^I_{jt} \times TE_{it}$	0.010		
	(0.020)		
$lp^I_{jt} \times \hat{\omega}_i$		0.005	
		(0.005)	
Firm FE	–	–	Yes
Worker FE	–	–	No
Worker characteristics	Yes	Yes	Yes
Worker by firm FE	Yes	Yes	No
Observations	472,555	417,870	417,870
Firms	1,136	1,136	1,136
Worker by firm matches	107,086	85,188	–

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses for columns (1) and (3). In column (2), the variable of interest varies on the individual level and standard errors are therefore clustered on firms instead. All specifications include time effects and labour market tightness. Worker characteristics include age, age squared and age cubed as well as education and immigration controls in the model without match specific controls. Interactions in column (2), and the dependent variable in column (3), are based on predictions from a pre-dated AKM model (see main text for details). Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom for columns (1) and (3).

If we use (7) and the estimates of column (2) in Table 3 to calculate the implied value ϕ, we get an estimate of 0.70, with a 95% confidence interval ranging from 41% to 98 %.[35] Thus, the differential impact on wages of sectoral and idiosyncratic shocks is consistent with measures of sector specificity derived from observed mobility patterns.

This analysis provides an illustrative insight regarding the magnitude of the bias incurred when using sectoral instruments to assess the importance of firm-level shocks for individual wages. While the potential presence of this bias is acknowledged in the literature (Bell and Van Reenen, 2011; Manning, 2011; Card *et al.*, 2014), our results suggest that the bias can be substantial but, more importantly, the results also show that observed intra-sector mobility patterns can serve as a useful statistic to assess the likely magnitude of the bias.

[35] Note that (4) can be seen as a log-linearised reduced form of (7), with additional controls added.

4. Heterogeneity, Robustness and Variations

4.1. *Worker Heterogeneity*

To analyse the extent to which technological improvements affect workers differently depending on their skill levels, we first interacted the productivity and TFP measures with the worker's level of education. The results presented in column (1) of Table 6 show that all interactions are insignificant although the precision is fairly poor. To construct a more precise measure of human capital, we first estimate a two-way fixed effects model with wages as the dependent variable, including person and firm fixed effects along the lines of the 'AKM-model' of Abowd *et al.* (1999), relying on data for the universe of full-time primary employments in the Swedish private sector during the period 1985–9, that is the available years before our sample.[36] Denoting the person fixed effect by ω_i, the firm fixed effects by $\Psi_{J(i,t)}$ (following the conventions of the AKM literature) and using x_{it} to denote an age polynomial and time dummies and u_{ijt} for the residual, we estimate

$$w_{ijt} = \omega_i + \Psi_{J(i,t)} + \gamma x_{it} + u_{ijt}, \tag{8}$$

where γ is a vector of coefficients, and extract the person effect as our measure of human capital. We then proceed by estimating interaction models, where we let the impact of our technology shocks vary with the level of human capital as measured by the estimated person effect ($\hat{\omega}_i$). The results, displayed in the second column of Table 6, again show no statistically significant heterogeneity, suggesting that changes in TFPQ are unrelated to the returns to portable individual skills (within firms). Here, it should be noted that, although Card *et al.* (2013) provide recent findings supporting the model, the use of AKM-models to extract measures of human capital is far from uncontroversial (Eeckhout and Kircher, 2011). We have, therefore, also explored alternative models using predicted human capital from estimated Mincer-type wage equation including detailed information about the field and level of education and basic demographics, as in the mobility analysis above.[37] Interacting the technology shocks with this alternative measure of human capital does, however, convey the exact same message suggesting a fairly homogenous wage impact of both technology shocks.[38]

4.2. *The Skill Composition of Firms and Productivity Shocks*

Turning to issues related to the skill composition within the firms, it is first worth noting that our main estimates displayed in Table 3 are only marginally affected when we allow for individual-specific fixed effects. This suggests that compositional effects through firm recruitment and firing policies in response to technology-induced

[36] All in all, this amounts to 8,776,223 linked employer–employee observations.

[37] Three-digit field dummies and two-digit level dummies (both ISCED 97) of schooling alongside gender, an age polynomial of order three and immigration background. The model also includes a firm fixed effect and year dummies, but the impact of these variables is not included in the human capital prediction. The prediction from this model explains 40% of the within-firm wage dispersion

[38] Splitting the data according to the median or quartiles of predicted human capital does not alter this conclusion.

changes in firm-level productivity should be minor. However, to investigate this issue further, we estimate how our measures of firm and sector-level productivity affect the skill composition of the firms. To this end, we use the predicted human capital of the workers ($\hat{\omega}_i$) constructed from the person effects extracted from the AKM-model (as explained above) as the outcome in regressions that are displayed in column (3) of Table 3.[39] This analysis has two virtues. First, we use pre-dated data to estimate the person effects. Thus, the person effects are clearly exogenous to our innovations in technology. Second, our skill measure has the same scale as the wage, and the size of the selection responses can thus be compared to the endogenous wage responses. Note also that any noise in the estimated person effects will be in the residual of the second-stage regressions and, thus, only affect precision and not the point estimates. The estimates, presented in column (3) of Table 6, show no signs of changes in measured human capital in response to shocks in the TFPQ series. This is also well in line with Haltiwanger *et al.* (1999), who use matched employer–employee data for the US, finding that although the skill distribution within establishments is tightly linked to the average sales per worker, there is virtually no relationship between changes in labour productivity and changes in the worker mix. An important aspect of our analysis is that the metric is comparable to that of the main analysis. Thus, given the confidence interval of the estimates, we are able to conclude that any effects on the sorting of workers must be substantially smaller than the endogenous wage responses that we focus on in the main analysis.[40]

Overall, the general impression is therefore that shocks to both idiosyncratic and sectoral technology have similar effects across the skill distribution and that the skill distribution remains unaffected by these shocks. The estimates thus suggest that these aspects of firms' personnel policies are (largely) unrelated to our measured technology shocks. The results, therefore, also lend some indirect support to the notion that our measured changes in TFPQ are primarily driven by processes other than time-varying wage policies within firms.

4.3. *Robustness*

In this subsection, we discuss a number of robustness checks in four different dimensions. First, for reasons discussed in subsection 2.1, our main analysis focuses on single-plant continuing firms, which gives us a non-representative distribution of plants relative to the economy. To evaluate the empirical significance of this particular choice of sample, we have redone the analysis for both a wider and a more restricted sample. The broader data build on an unbalanced panel of plants including plants belonging to multi-plant firms. This gives us a sample of twice as many firms (2,444) and four times as many worker/time observations (2,048,555). As can be seen in column (2) of Table 7, the results are qualitatively unchanged. As an additional robustness check, we

[39] Under the null hypothesis of no sorting, we can estimate the model using predictions from a model without time-varying labour productivity. We have re-estimated the model including time-varying productivity as well, finding identical results.

[40] As above, we have also explored the impact on sorting relative to workers' observable human capital using three-digit field dummies and two-digit level dummies (both ISCED 97) of schooling alongside gender, an age polynomial of order three, and immigration background, finding very similar results.

Table 7

Robustness Variations

	(1)	(2)	(3)
Variation:	Baseline	Unbalanced, multi-plant	Mono-product firms
Estimation method:	IV	IV	IV
lp^S_{st}	0.149**	0.097*	0.271**
	(0.057)	(0.053)	(0.117)
lp^I_{jt}	0.051***	0.018***	0.047
	(0.006)	(0.006)	(0.029)
Worker characteristics	Yes	Yes	Yes
Worker by firm FE	Yes	Yes	Yes
Observations	472,555	2,048,555	50,364
Firms/plants	1,136	2,444	134
Worker by firm matches	107,086	465,760	11,884
p-value	0.049	0.078	0.044

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses. All specifications include time effects and labour market tightness. Worker characteristics includes age, age squared and age cubed. 'p-value' denotes tests of the hypothesis that firm-level shocks that are shared within a sector have a larger impact than purely idiosyncratic shocks. Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom. Column (2) uses an unbalanced panel of plants that also includes plants that belong to multi-plant firms. Column (3) only includes the subset of single-plant firms that produce a single product.

have redone the analysis without any trimming of the data (explained in Appendix A) and the results are, again, qualitatively unchanged; see Appendix E for detailed results.

Another potential concern is that our price index may struggle to handle cases where firms produce multiple products, or change their product mix (e.g. by introducing new products). Although the index is constructed through an aggregation from individual product prices and therefore, in principle, should handle these cases, zooming in on the set of firms that only produce a single product provides a useful robustness exercise. Notably, though, the very detailed definitions of products in our data leave us with a very small sample of 135 mono-product firms and the results, therefore, become less precise. But, they clearly show a very similar overall pattern as in our preferred data (see column 3 of Table 7).

Second, our main analysis keeps the cost shares and the overall returns-to-scale parameter constant, which implies that we rely on a log-linear approximation of the production technology. A second-order approximation could be accomplished by constructing a Törnqvist index by using the average of observed cost shares of adjacent observations (current and lagged) combined with an assumption that the overall returns-to-scale parameter is time-invariant.[41] We have, therefore, redone the analysis using a Törnqvist index and find very similar results; see Appendix E for detailed results. However, note that, as pointed out by Basu and Fernald (2001), to legitimise the use of cost shares at all, factor prices must equal the shadow values of the factors to the firm. In a world with adjustment costs or long-term relationships, this will be true

[41] Diewert (1976) shows that a Törnqvist approximation is 'superlative'. In other words, it is exact if the underlying production function is translog; otherwise it provides a second-order approximation to any functional form.

only in the long run, or on average, which motivates the use of average cost shares as the baseline specification. Appendix E also presents results where we use three-digit (instead of two-digit) industry averages to calculate the cost shares, again with little impact on the results.

Third, we examined the sensitivity of the results to the estimated returns-to-scale parameter. Results in Appendix E build on models which perturb the estimates up or down with 0.1 or impose constant returns-to-scale in both the durables and the non-durables sectors. None of these exercises affects our conclusions.[42]

Finally, we have experimented with alternative ways to estimate the productive contribution of capital by relying on explicit estimates of the capital stock instead of using electricity to proxy the flow of capital services (Appendix D details the construction of the ensuing, alternative TFPQ measure). Estimating capital relaxes the assumption of perfect complementarity between the flow of capital services and electricity use we made in our preferred series. Nevertheless, measuring the flow of capital services with the stock of capital has its own problems: first, this approach ignores variation in capital utilisation. Second, measuring the stock of capital is problematic when the sample is short in the time dimension and the measure of the initial stock is imprecise. However, it is reassuring that the two approaches deliver a very similar message, with the estimated elasticities only slightly smaller than those reported above. As presented in Appendix E, we find an elasticity of 0.043 (SE = 0.007) with TFPQ based on capital for the idiosyncratic productivity component and 0.115 (SE = 0.036) for the sectoral component.

The overall impression is that the results are robust to large variations in the estimated models. In particular, we find that the measured impact for sectoral shocks remains larger than the impact of idiosyncratic shocks throughout a wide set of variations.

4.4. *The Role of Dynamics*

The specifications we have presented so far are static, that is they assume that the wage impact of technology-driven innovations in productivity is immediate. In reality, highly persistent technology shocks may require some time to be absorbed by wages. To assess the importance of potential delays in the impact of productivity, we have estimated models capturing the impact of current as well as lagged productivity.

Estimates from specifications with lagged productivity are presented in Table 8. We concentrate on our preferred specification (including match-specific fixed effects) and

[42] Because TFP series derived from value added are valid only under perfect competition and constant returns, as noted by Basu and Fernald (1995), we use gross output series rather than value added throughout. In the case of decreasing returns-to-scale, a TFP measure derived from value added would be negatively correlated with the growth rate of primary inputs. Positive demand shocks will, therefore, push down measured TFP based on value added, which will exert a negative bias in the wage regressions if labour supply is upward-sloping. In line with this prediction, results from value added-based measures are smaller than those based on TFPQ. The negative bias is somewhat larger for the sectoral elasticity. We have also estimated the model separately for durables and non-durables, finding very similar results in both subsectors when using our preferred model. The results also suggest that the importance of proper measurement (instrumentation and using gross output instead of value added) is most pronounced in the non-durables sector where we estimate the returns to scale to be decreasing.

Table 8

Dynamic Effects

Estimation method:	(1) IV	(2) IV	(3) IV
lp^S_{st}	0.149**	0.052	0.226
	(0.057)	(0.147)	(0.187)
lp^S_{st-1}		0.104	0.198
		(0.149)	(0.486)
lp^S_{st-2}			−0.122
			(0.283)
lp^I_{jt}	0.051***	0.035***	0.033***
	(0.006)	(0.008)	(0.009)
lp^I_{jt-1}		0.034***	0.032***
		(0.011)	(0.010)
lp^I_{jt-2}			0.026***
			(0.006)
Total sector effect	0.149**	0.157**	0.303
	(0.057)	(0.056)	(0.243)
Total idiosyncratic effect	0.051***	0.068***	0.091***
	(0.006)	(0.009)	(0.010)
Worker characteristics	Yes	Yes	Yes
Worker by firm FE	Yes	Yes	Yes
Observations	472,555	402,058	335,291
Firms	1,136	1,136	1,136
Worker by firm matches	107,086	99,473	93,316

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses. All specifications include time effects and labour market tightness. Worker characteristics include age, age squared and age cubed. 'p-value' denotes tests of the hypothesis that firm-level shocks that are shared within a sector have a larger impact than purely idiosyncratic shocks. Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom.

proceed incrementally, first introducing one lag in column (2) and then two lags in column (3).[43] The bottom of Table 8 shows the long-run accumulated effect and its associated level of significance. The results show that there is a role for lagged productivity in shaping current wages. The precision, however, deteriorates fairly rapidly for the sectoral productivity, and we never find the individual lags to be statistically significant. Although the individual lags are estimated with poor precision, the long-run elasticities portray a very similar picture in all cases. The magnitude of the long-run impact is about twice that of the contemporaneous impact for both the idiosyncratic effect (0.091 *versus* 0.051) and the sectoral effect (0.303 *versus* 0.149) when two lags are considered.

4.5. *Additional Variations*

Our data do not allow us to control for part-time work properly but, since part-time work in Sweden is rare among males in the manufacturing sector, we have re-estimated the model using only males. The estimates show that the response of male wages to

[43] Given the short nature of our panel, we were not able to estimate models with more than two lags with any precision.

changes in productivity is very similar to that obtained in the overall sample. The elasticity of wages to sectoral productivity is almost three times as large as the elasticity to idiosyncratic movements in productivity, both estimates being statistically significant at standard levels of testing.

A very active literature (Hall, 2003; Shimer, 2005; Pissarides, 2009) discusses whether search models can be reconciled with the large fluctuations we see in unemployment over the cycle. A key element in this debate is the exact modelling of how wages for new hires react when productivity changes (Pissarides, 2009; Haefke *et al.*, 2013). We have analysed the impact on incumbents and new hires of sectoral and idiosyncratic productivity but found no significant differences in their impact. However, it should be acknowledged that the interacted estimates were quite imprecise.

Finally, we have analysed whether productivity has a differential impact depending on whether the shocks are positive or negative, where one might suspect that negative shocks have a smaller effect because of downward nominal wage rigidity. We find no evidence of such asymmetries. Although this may seem surprising, it should be noted that the magnitudes of the estimated elasticities are such that the wage impact of any 'normal' shock is smaller than the average nominal wage increase among incumbent workers. Thus, there is indeed scope for a symmetric impact of positive and negative productivity shocks, even if nominal wages never fall.

5. Conclusions

We have studied how individual wages are affected by the changes in productivity of the firms where the workers are employed and how those impacts vary when technology is shared with other firms operating within homogeneous sectors. By relying on a carefully constructed measure of physical total factor productivity (TFPQ), we isolate changes in firm-level physical productivity that are caused by shifts in the firms' production functions and analyse how these changes affect individual wages. Using an IV strategy we minimise exposure to measurement errors. In addition, we use matched employer–employee data to purge the analysis of sorting on both the supply and demand side.

The results suggest that both idiosyncratic and sectoral productivity shocks are important for the wages of incumbent workers. However, the impact of the sectoral shocks is substantially more pronounced than the impact of idiosyncratic shocks. Our results are robust to various alterations of the model, as long as we rely on TFPQ for identification. Using a revenue-based measure of TFP (TFPR), which introduces relative price adjustments into the measure of idiosyncratic shocks, would however have led us to find an effect of idiosyncratic shocks that is almost as large as that of sectoral shocks, a result that highlights the importance of proper measurement in this context.

We also link the analysis to observed mobility patterns of workers, under the hypothesis that these patterns provide a proxy for how sector-specific the labour market is. We provide a stylised reduced-form model of wage-setting where sectoral outside options matter and we show that our results are well-aligned with the observed degree of excess mobility within sectors in the data. A worker who changes employer has a 54% age points higher probability of finding a new job in the original, narrowly defined sector, which provides a straightforward rationale for sector-specific outside options to affect the wages in ongoing employment relationships. We also show that sectoral shocks matter more for workers who are more closely tied to the original sector, while idiosyncratic firm-level shocks are unaffected by the mobility patterns.

We believe that these results provide useful guidance for future empirical research on the relationship between firm-level shocks and wage adjustments. Our main findings underpin well-known concerns that it may be inappropriate to use instruments that vary at an aggregate level – for example, defined according to industries (even if narrowly defined) – when analysing firm-level processes as long as outside options matter (see, e.g. the discussion in Manning, 2011). Despite these concerns, sectoral instruments and other instruments with aggregate components, are commonly used for identification in many different strands of the literature where firm characteristics are linked to the wages of employees, including studies of exporter wage premiums (see Greenaway and Kneller (2007) for a survey and Verhoogen (2008) for a recent example), studies of the wage effect of firm-level offshoring (Hummels *et al.*, 2011) or studies of the links between firm performance and the within-firm pay structure (Bell and Van Reenen, 2011). Our results moreover provide a useful way of assessing the likely magnitude of the bias arising from the additional impact through outside options since the size of the bias is found to be related to cross-sectoral mobility patterns. This implies that the existence or magnitude of the outside-option bias could be assessed *a priori* by exploring the extent to which the instrument is correlated with workers' mobility patterns.

Our results also suggest that the impact of both idiosyncratic and sectoral technology shocks is shared equally across the skill distribution. Furthermore, we find that the skill mix of the firms remains unchanged in the face of both types of technology shocks, a finding which is well in line with Haltiwanger *et al.* (1999), who find a very small relationship between changes in labour productivity and changes in the worker mix despite pronounced cross-sectional differences. We interpret these results as somewhat reassuring in terms of our identifying assumptions since they imply that within-firm movements in TFPQ are unrelated to (at least these aspects of) firms' personnel policies.

Overall, our findings suggest that firm-level physical productivity has an impact on workers' wages, which contrast with both simple frictionless competitive models (where individual wages depend only on aggregate labour market conditions and individual skills) and models of internal labour markets (where workers are perfectly insulated from external market forces). The fact that changes in physical productivity that are shared within a sector have an impact on wages that is much larger than purely idiosyncratic shocks in both the short run and the long run suggests not only that:

(*i*) both workers and firms benefit from firm-level technological advancements, but also that; and
(*ii*) workers extract a substantially larger fraction of the rents generated by productivity advancements that are shared with other firms within the sector.

Since the standard deviation of idiosyncratic (within-match) productivity is about three times larger than that of sectoral productivity, the two productivity shocks play similar roles in shaping workers' wage increases: a one standard deviation increase in either sector-specific or idiosyncratic productivity has a short-term (long-term) impact on wages that amounts to about one-quarter (half) of the average yearly wage growth of incumbent workers.

Appendix A. Data Construction

The firm data set we use is primarily drawn from the Industry Statistics Survey (IS) and contains annual information for the years 1990–6 on inputs and output for all Swedish manufacturing plants with 10 employees or more.[44] The data are matched to the Swedish Tax Authority's register-based labour market statistics (RAMS), which add individual wages and worker characteristics of each employee of the manufacturing plants included in the sample. In the baseline sample, we focus on continuing single-plant firms.

Wages cover end-of-the-year employees (working in November). We focus on primary jobs; therefore, for workers with multiple jobs we only keep the job resulting in the highest wage.

Tightness is calculated for local labour markets relying on Statistics Sweden's 1993 definition. The definition, which is based on commuting patterns, divides Sweden into 109 geographic areas.

When computing labour productivity, $LP_{jt}(= Y_{jt}/N(IS)_{jt})$, labour input, $N(IS)_{jt}$, is measured as the average number of employees during the year and is taken from the IS. To compute the input index, $\Delta\tilde{x}_{jt}$, which is used to estimate the returns to scale and change in technology, real intermediate inputs, M_{jt}, are measured as the sum of costs for intermediate goods and services collected from the IS deflated by a three-digit (SNI92/NACE) producer price index collected by Statistics Sweden. Moreover, energy, V_{jt}, is measured as the plants' electricity consumption in megawatt-hours taken from the IS.

When computing the (overall) cost shares, we also need a measure of the firms' labour cost, which is defined as total labour cost including, e.g. payroll taxes available in the IS. Also, to calculate the cost shares by education in (3) as well as the growth rate for respective category of labour input, we use the RAMS data (see discussion in subsection 2.1 above). Here, we define *LHE* (less then high school education) as individuals with a one-digit ISCED 97 level code smaller than or equal to two; *HE* (high school education) as individuals with a one-digit ISCED 97 level code equal to three; and *TE* (tertiary education) as individuals with a one-digit ISCED 97 level code larger than or equal to four.[45] Moreover, since Sweden experienced a boom-bust cycle in the late 1980s and early 1990s we do not use observations from firms experiencing large losses when calculating the two-digit (SNI92/NACE) cost shares (as averages over time within each sector). In the calculations, we drop observations for firms where the (residual) capital share is below -10% of sales. This procedure gives rise to aggregate manufacturing cost shares that are similar to those obtained using the data underlying Carlsson (2003).[46] Data cleaning and matching between data sets leaves us with 1,298 plants. This is out of 2,325 plants in the raw balanced data, where the bulk, that is 958 firms, is lost when matching on the required information from the RAMS data to the IS data.

[44] The data also include a small sample of plants with fewer than 10 employees but as these observations are sampled using a year-specific sample frame they are only useful for purely cross-sectional excercises. Hence, they are not used in this article.

[45] We exclude individuals with missing information on education from the calculations.

[46] The aggregate manufacturing shares in Carlsson (2003) (our sample) equals $C^M = 0.65(0.66)$, $C^L = 0.25(0.20)$, $C^K = 0.07(0.12)$ and $C^V = 0.03(0.03)$.

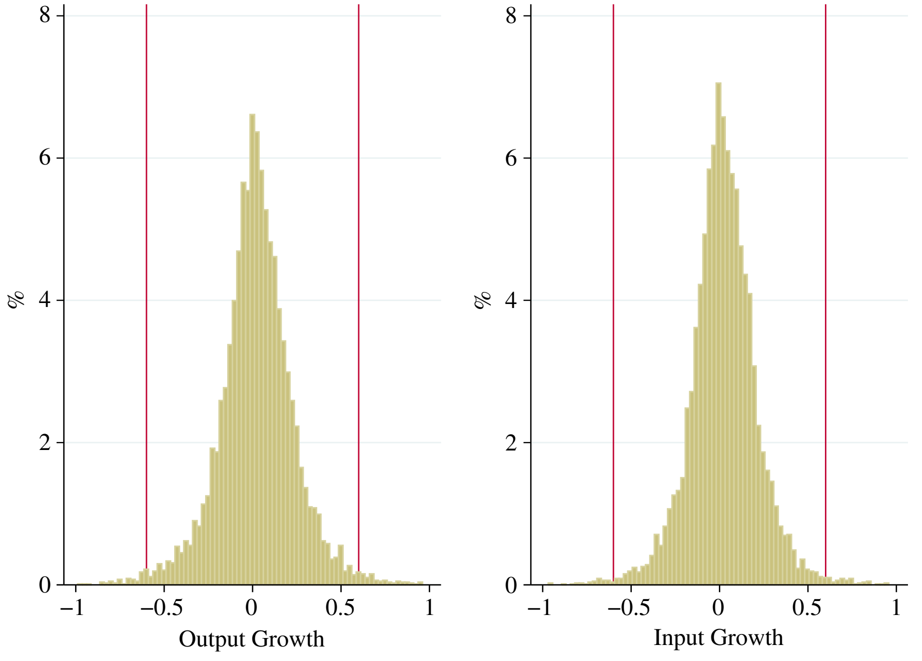

Fig. A1. *Distribution of Output and Input Growth Rates*

Although we have removed obviously erroneous observations, the firm data set still contains very large observations in Δy_{jt} and $\Delta \tilde{x}_{jt}$. To avoid our returns to scale estimates being affected by firms subject to episodes of extreme conditions, these observations are removed (see below). In Figure A1, the data distributions are plotted for the relevant variables for estimating returns to scale and technology change (truncated at ± 1 in log-difference space).

Since the main mass of the data seems to be well captured in the interval ± 0.6 for all variables, we limit the data set to contain firms with observations only within this interval. Note that, for example $\Delta y_{jt} = 0.6$ corresponds to an annual increase of 82% in real output. This procedure removes 160 firms from the sample, leaving us with 1,138 firms.[47,48] To decompose the technology series into a sectoral and an idiosyncratic part, we need to drop two additional firms because they are the only firms in the sample pertaining to a particular sectoral agreement. This then leaves 1,136 firms in the data set we then use to estimate (5).

After merging the final firm-level data with the employee data in RAMS, we arrive at 474,528 employee observations across the remaining 106,815 individuals. Removing observations where education information is missing we have 472,555 observation across 106,050 individuals left. This data set covers 11% of the full-time employees in the manufacturing sector.

Finally, unemployment and vacancy data on the local labour market level are collected from the National Labour Market Board (AMS). The data contain information on the number of registered vacancies and the number of individuals registered as openly unemployed at an unemployment office in November. We use the (1993) definition of homogenous local labour markets constructed by Statistics Sweden using commuting patterns, which divides Sweden into 109 areas.

[47] We do not remove observations with large movements in labour productivity because this variable will be instrumented in the econometric procedure.
[48] Robustness exercises do, however, show that the baseline results, presented in the main text, are not sensitive to this trimming of the data.

Appendix B. Derivation

Let firm j's technology be described by

$$Y_{jt} = TFPQ_{jt} f(\tilde{K}_{jt}, \tilde{L}_{jt}, V_{jt}, M_{jt}), \tag{B.1}$$

where gross output Y_{jt} is produced combining capital services $\tilde{K}_{jt}$ (i.e. the stock of capital K_{jt} times capital utilisation Z_{jt}), labour services $\tilde{L}_{jt}$ (i.e. labour input L_{jt} times labour utilisation H_{jt}), energy V_{jt} and intermediate materials M_{jt}.[49] Finally, $TFPQ_{jt}$ is the index of technology that we want to capture.

Using small letters to denote logs, taking the total differential of the log of (B.1) and invoking cost minimisation, we arrive at:

$$\Delta y_{jt} = \psi_j(\Delta x_{jt} + \Delta u_{jt}) + \Delta tfpq_{jt}, \tag{B.2}$$

where Δy_{jt} is the growth rate of gross output and ψ_j the overall returns to scale. Let C_j^F denote the cost share of factor F in total costs, where $F \in \{K,L,V,M\}$. Then, Δx_{jt} is a cost-share weighted input index defined as $C_j^K \Delta k_{jt} + C_j^L \Delta l_{jt} + C_j^V \Delta v_{jt} + C_j^M \Delta m_{jt}$. Similarly, the change in utilisation of capital and labour is denoted by $\Delta u_{jt} = C_j^K \Delta z_{jt} + C_j^L \Delta h_{jt}$.[50] Thus, given data on factor compensation, changes in output, input and utilisation, and an estimate of the returns to scale ψ_j, the resulting residual $\Delta tfpq_{jt}$ provides a time series of technology growth for the firm. Note that $\Delta tfpq_{jt}$ reduces to a gross-output Solow residual if $\psi_j = 1$, $\Delta u_{jt} = 0, \forall j$, and there are no economic profits. Hence, $\Delta tfpq_{jt}$ is a Solow residual purged of the effects of non-constant returns, imperfect competition and varying factor utilisation.

To properly identify the contribution of technology, it is also important to distinguish between employees with different levels of education (Jorgenson *et al.*, 1987). Hence, using the same logic as above, we define Δl_{jt} as

$$\Delta l_{jt} = C_j^{LHE} \Delta n_{jt}^{LHE} + C_j^{HE} \Delta n_{jt}^{HE} + C_j^{TE} \Delta n_{jt}^{TE}, \tag{B.3}$$

where Δn_{jt} is the growth rate of the number of workers, superscripts LHE, HE and TE denote workers with less than high school education, high school education and tertiary education, respectively, and C_j^{EDU} denotes the cost share of category EDU workers in total labour costs, where $EDU \in \{LHE,HE,TE\}$. Hence, our labour input index will capture changes in the skill composition of the workforce of the firm.

The main empirical problem associated with expression (B.2) is that capital and labour utilisation are unobserved. A solution to this problem is to include proxies for factor utilisation. As a baseline, we follow the approach taken by Burnside *et al.* (1995), who use energy consumption as a proxy for the flow of capital services. This procedure, which is well suited for our manufacturing sector data, can be legitimised by assuming that there is a very low elasticity of substitution between energy and the flow of capital services. This, in turn, implies that energy and capital services are highly correlated (we also experiment with using a direct measure of the capital stock instead in the TFPQ calculation). Assuming that labour utilisation is constant, and including a set of time dummies to capture any aggregate trends in technology growth, τ_t, we arrive at (5) of the article, that is $\Delta y_{jt} = \psi_j \Delta \tilde{x}_{jt} + \tau_t + \Delta tfpq_{jt}$, where input growth, $\Delta \tilde{x}_{jt}$, is defined as $(C_j^K + C_j^V)\Delta v_{jt} + C_j^L \Delta l_{jt} + C_j^M \Delta m_{jt}$. Note that $\Delta tfpq_{jt}$ encompasses any firm-specific constant.

[49] An alternative is to measure TFPQ departing from value added. As we show in Carlsson *et al.* (2011), however, the use of value added in combination with deviations from non-constant returns will result in a measure of TFP that is not independent from the use of intermediate inputs and factor input growth.

[50] Here, the cost shares are assumed to be constants.

Once the series of technical change has been obtained following (5), the next step consists of integrating the growth rates in technology into a log level technology series using the following recursion $tfpq_{jt} = tfpq_{j0} + \sum_{r=1}^{r=t} \Delta tfpq_{jr}$. Note that the initial level of technology ($tfpq_{j0}$) is a firm-specific constant that is not observed, but will be captured by firm fixed effects in the second stage estimation.

Appendix C. Estimating Returns to Scale

When empirically implementing specification (5), we take an approach akin to the strategy outlined by Basu *et al.* (2001). First, the specification is regarded as a log-linear approximation around the steady-state path. Thus, the products $\psi_j C_j^F$ (the output elasticities) are treated as constants. Note that using constant cost shares (including the cost share[s] of labour) precludes variation in wages from spilling into variation in the TFP measure if, for any reason, $\psi_j C_{jt}^L$ is an imperfect measure of the output elasticity of labour input. Second, the steady-state cost shares are estimated as the time average of the cost shares for the two-digit industry to which the firm belongs (SNI92/NACE). Third, to calculate the cost shares, we assume that firms make zero profit in the steady state. Importantly, as noted by Basu and Fernald (1995), zero profits in equilibrium are consistent with a markup if the markup is equal to the returns to scale. Taking total costs as approximately equal to total revenues, we can infer the cost shares from factor shares in total revenues. The cost share of capital and energy is then given by one minus the sum of the cost shares for all other factors.

Note that the estimation of (5) cannot be carried out by OLS, because the firm is likely to consider the current state of technology when making its input choices.[51] Here, we exploit the panel nature of our firm-level data to use internal instruments, as previously described in Section 3.

We first estimate the technology disturbances relying on the empirical specification (5) outlined previously in subsection 2.2. Here, we allow the returns-to-scale parameter ψ_j to vary across durables and non-durables sectors as suggested by Basu *et al.* (2001).[52] The models include firm fixed effects, which capture any systematic differences across firms in average technology growth. Since the firm is likely to consider the current state of technology when making its input choices, we need to resort to an IV technique. Following Marchetti and Nucci (2005) and Carlsson and Smedsaas (2007), we use the difference generalised method of moments (GMM) estimator developed by Arellano and Bond (1991) and report robust, finite-sample corrected, standard errors following Windmeijer (2005). Here, we use $\Delta\tilde{x}_{jt-s}$, for $s \geq 3$, as instruments and collapse the instrument set to avoid overfitting (see Roodman, 2009).[53]

In Table 1, we present the estimation results for (5). The estimate of the returns to scale for the durables sector equals 0.99, and 0.88 for the non-durables sector, but both are somewhat imprecisely estimated (SE = 0.19 and 0.22, respectively). It is reassuring, however, to see that the point estimates of the returns to scale are very similar to estimates reported by earlier studies. For example, Basu *et al.* (2001) reports estimates of 1.03 and 0.78 for durables and non-durables,

[51] This is the so-called transmission problem in the empirical production-function literature (Marshak and Andrews, 1944). Technology change (the residual) represents a change in a state variable for the firm and changes in the level of production inputs (the explanatory variables) are changes in the firm's control variables, which should react to changes in the state variable. In this case, there will be a correlation between the error term and the explanatory variable, hence the need of IV methods.

[52] The data do not allow us to identify the returns-to-scale parameter separately across (SNI92/NACE) two-digit industries, because many sub-samples become too small.

[53] Given that we use a difference GMM estimator, the second and higher ordered lags of $\Delta\tilde{x}$ should be valid instruments under the null hypothesis of no serial dependence in the residual. However, when including the second lag in the instrument set, the Hansen test of the over-identifying restrictions is significant at the 5% level.

respectively, using US sectoral data. Moreover, the Hansen test of over-identifying restrictions cannot reject the joint null hypothesis of a valid instrument set and a correctly specified model.

Table 1 shows that the AR(2) test of the differenced residuals (Arellano and Bond, 1991) shows no sign of any important serial dependence in the estimated technology change series.[54] This implies that the innovations have a highly persistent effect on the level of technology. When estimating an AR(1) process for the level of technology, as in, for example Eslava *et al.* (2004), we find a persistence estimate of 0.88 (SE = 0.012).[55] This estimate is in between the Colombian estimate of 0.92, presented in Eslava *et al.* (2004), and the US estimate of 0.79, presented in Foster *et al.* (2008).

Appendix D. Constructing a Measure of the Capital Stock

We calculate the capital stock using investment data and book values (for the starting values). When using a measure of the capital stock, the input index is defined as $\Delta\tilde{x}_{jt}^{C} = C_j^K \Delta k_{jt} + C_j^V \Delta v_{jt} + C_j^L \Delta l_{jt} + C_j^M \Delta m_{jt}$. The capital stock, K_{jt}, is computed using a variation in the perpetual inventory method which utilises all the information we have available in the data.

We calculate the capital stock in two steps. In the first step, we calculate the forward recursion

$$K_{jt} = \max[(1-\delta_s)K_{jt-1} + I_{jt}, BookValue_{jt}], \tag{D.1}$$

where δ_s is a sector-specific depreciation rate (two-digit SNI92/NACE) and is computed as an asset share-weighted average between the depreciation rates of machinery and buildings (collected from Melander, 2009, Table 2), I_{jt} is real net investments in fixed tangible assets (deflated using a two-digit [SNI92/NACE] sector-specific investment deflator collected from Statistics Sweden); $BookValue_{jt}$ is the real book value of fixed tangible assets (computed using the same deflator as for investment); and

$$K_{j0} = \begin{cases} 0 \text{ if } BookValue_{j0} \text{ is missing,} \\ BookValue_{j0} \text{ otherwise.} \end{cases} \tag{D.2}$$

Since the firm has an incentive to keep the book values low for tax reasons, we use the book values as a lower bound of the capital stock. In a second step, we calculate the backward recursion

$$K_{jt-1} = \frac{K_{jt} - I_{jt}}{(1-\delta_s)}, \tag{D.3}$$

where the ending point of the first recursion, K_{jT}, is used as the starting point for the backward recursion. This is done to maximise the quality of the capital-stock series, given that we do not have a very reliable starting point and the time-series dimension is short. Taking account of missing data when calculating the capital stock, we can project the technology levels for 944 firms using $\Delta\widetilde{x}_{jt}^{C}$ instead of $\Delta\widetilde{x}_{jt}$.

[54] This is important for the validity of the instruments used in the estimation of the returns to scale. Note also that we take an additional lag of the instrument set than would be required under the null of no serial dependence in the technology change series, which further safeguards from any deviations from the null.

[55] Note that this standard error is not appropriate to use to test the null of a unit root, since such an hypothesis would imply a different parameter distribution.

Appendix E. Additional Results

In this Appendix, Tables E1 and E2 present additional robustness exercises, as mentioned in the main text.

Table E1

Additional Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variation:	Baseline	T. V. C_J	Three-Digit Cost Shares	RTS-0.1	CRS	RTS + 0.1	Capital
Estimation method:	IV	IV	IV	IV	IV	IV	IV
lp^S_{st}	0.149**	0.170*	0.152**	0.125***	0.168*	0.184	0.115***
	(0.057)	(0.085)	(0.063)	(0.038)	(0.081)	(0.108)	(0.036)
lp^I_{jt}	0.051***	0.044***	0.049***	0.061***	0.042***	0.038***	0.043***
	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)
Worker characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Worker by firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	472,555	472,682	472,262	472,555	472,555	472,555	451,729
Firms/plants	1,136	1,137	1,132	1,136	1,136	1,136	944
Worker by firm matches	107,086	107,140	107,027	107,086	107,086	107,086	102,125
p-value	0.049	0.074	0.055	0.058	0.067	0.093	0.032

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses. All specifications include time effects and labour market tightness. Worker characteristics include age, age squared and age cubed. 'p-value' denotes tests of the hypothesis that firm-level shocks that are shared within a sector have a larger impact than purely idiosyncratic shocks. Column (20) allows for time varying cost shares. Column (3) calculates the cost shares at the three-digit (instead of two-digit) level. Column (4) pulls the estimated returns to down by 0.1. Column (5) imposes constant returns to scale. Column (6) pushes the estimated returns to up by 0.1. Column (7) uses the estimated capital stock described in Appendix D. Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom.

Table E2

Additional Results

	(1)	(2)	(3)
Variation:	Baseline	No triming	Three-Digit NACE defines the sector
Estimation method:	IV	IV	IV
lp^S_{st}	0.149**	0.147**	0.129***
	(0.057)	(0.054)	(0.039)
lp^I_{jt}	0.051***	0.063***	0.038***
	(0.006)	(0.014)	(0.010)
Worker characteristics	Yes	Yes	Yes
Worker by firm FE	Yes	Yes	Yes
Observations	472,555	519,644	472,555
Firms/plants	1,136	1,296	1,136
Worker by firm matches	107,086	119,091	107,086
p-value	0.049	0.101	0.015

Notes. * (**) (***) denotes significance at the 10(5)(1)% level. Standard errors clustered on sector reported inside parentheses. All specifications include time effects and labour market tightness. Worker characteristics include age, age squared and age cubed. 'p-value' denotes tests of the hypothesis that firm-level shocks that are shared within a sector have a larger impact than purely idiosyncratic shocks. Column (2) estimates the base model without trimming. Column (3) uses three-digit NACE dummies to define the sectors. Hypothesis tests are relative to the student t-distribution with 15 degrees of freedom for columns (1) and (2).

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Accepted: 22 July 2014

Additional Supporting Information may be found in the online version of this article:

Data S1.

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