Wage Adjustment and Productivity Shocks*

Mikael Carlsson[†], Julián Messina[‡] and Oskar Nordström Skans[§]

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Abstract

We study how workers' wages respond to TFP-driven innovations in firms' labor productivity. Using unique data with highly reliable firm-level output prices and quantities in the manufacturing sector in Sweden, we are able to derive measures of physical (as opposed to revenue) TFP to instrument labor productivity in the wage equations. We find that the reaction of wages to sectoral labor productivity is almost three times larger than the response to pure idiosyncratic (firm-level) shocks, a result which crucially hinges on the use of physical TFP as an instrument. These results are all robust to a number of empirical specifications, including models accounting for selection on both the demand and supply side through worker-firm (match) fixed effects. Further results suggest that technological progress at the firm level has negligible effects on the firm-level composition of employees.

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†Research Department, Sveriges Riksbank E-mail: mikael.carlsson@riksbank.se.

‡Office of the Chief Economist for Latin America and the Caribbean, World Bank. E-mail: jmessina@worldbank.org

§Institute for Labour Market Policy Evaluation (IFAU), UCLS, and IZA. E-mail: oskar.nordstrom_skans@ifau.uu.se.

1 Introduction

At the aggregate level, real wages and labor productivity are intimately related.[1] However, productivity growth originates at the firm level. Prominent theories of the labor market, including search and matching models with endogenous job destruction in the Mortensen and Pissarides (1994) tradition, wage posting models in the spirit of Moen (1997) and Burdett and Mortensen (1998), and on-the-job search models with counteroffers à la Cahuc, Postel-Vinay, and Robin (2006), suggest a direct role for idiosyncratic productivity on individual wages. However, there is limited knowledge in the empirical literature about the relative impact of firm-specific factors versus market forces in the process where technological advances are transmitted into wages. Understanding how idiosyncratic technology shocks and shocks that are shared between firms at the sectoral level affect individual wages sheds light on the relative importance of firms vs. market forces in the determination of the joint distributions of labor productivity and individual wages.

The recent empirical literature, building on Abowd, Kramarz, and Margolis (1999), has established that some firms consistently pay higher wages than others, even to identical workers. Yet, surprisingly little is known about the deep determinants of the persistent differences in firms' pay, and about how these differences evolve when firms' economic conditions change.[2] Mortensen (2003) argued that pro-

[1] Recent empirical applications assessing the relationship between aggregate productivity and individual wages include Haefke, Sonntag, and Van Rens (2008) and Carneiro, Guimarães, and Portugal (forthcoming). The later finds a one-to-one relationship between labor productivity and individual wages.

[2] A large literature has established an empirical association between wages and firm-level profits. See Abowd and Lemieux (1993), Blanchflower and Oswald (1996) and Van Reenen (1996) for some of the earlier work and Card, Devicienti, and Maida (2009) for a recent application. Differences in profits across firms, just like the case of revenue or sales discussed in the paper, are likely to be driven by demand, productivity and factor input shocks.

ductivity differences between firms are closely linked to wage dispersion, and the association between measured labor productivity and individual wages is by now also well documented (Lentz and Mortensen (2010)). However, identifying causal effects is not straightforward and, as we argue in this paper, standard identification problems are typically exacerbated by a lack of adequate data on the firm side.[3]

Assessing the causal impact of firm-level productivity on individual workers' wages poses three key identification challenges. The first is a measurement issue. Since firm-level prices rarely are observed, most productivity studies measure firm-level output as revenue divided by an industry-level deflator. This implies that output and productivity measures reflect price differences between firms operating in the same industry. Importantly, firm-level prices tend to be a function of factor prices, including wages (see e.g. Carlsson and Nordström-Skans (Forthcoming)). In order to differentiate between firms with high costs (e.g. due to high wages) and firms with high productivity it is therefore necessary to account for price differences between firms within industries. When assessing the impact of firm productivity on wages, measures of productivity based on firm-level revenues deflated by an industry-level price index will suffer from reversed causality.

The second challenge stems from firms' optimizing behavior, which generates a relationship between wages and labor productivity that differs from the causal impact of productivity on wages we want to capture. Intuitively, shocks to wages, other factor prices or product demand will alter the scale of production and/or the capital-labor ratio as well as individual wages. A positive association between wages and productivity may arise if labor productivity responds to idiosyncratic firm-level wage shocks. Think about a positive wage shock. Firms' optimizing

[3] Fox and Smeets (Forthcoming) and Irarrazabal, Moxnes, and Ulltveit-Moe (2009) have recently looked at the reverse of our question of interest: the impact of unobserved human capital characteristics on productivity differences across firms within sectors.

behavior suggests a substitution of labor for capital, which should increase labor productivity. Sometimes this relationship may be triggered by other shocks which are not easily observed by the econometrician. In a context of decreasing returns to scale, a positive demand shock that results in an upscale of production reduces labor productivity. If the local labor supply is upward sloping, the increase in the demand for labor will push up wages, resulting in a spurious negative association between labor productivity and wages.

The final challenge is associated with worker sorting. More able workers may move from less productive to more productive firms, if the latter pay higher wages. In addition, poor matches between workers and firms may dissolve when firm productivity declines. Hence, assessing the causal impact of productivity on individual wages requires that sorting is properly accounted for.

This paper studies the response of workers' individual wages to purely idiosyncratic firm-level productivity and productivity developments that are shared across firms within sectors. Our empirical strategy exploits unique features of our data to overcome the three challenges previously discussed. We draw on a very rich matched employer-employee panel data set of the manufacturing sector in Sweden. A crucial feature of our data is that, on top of having detailed information on worker and establishment characteristics, we are able to access highly reliable firm-level price indices for the compound of goods that each of the firms sells.[4] This helps us deal with the first problem outlined above. We use firm-level prices to construct proper labor productivity measures, which are clean from movements in relative prices across firms within sectors. To separate out the impact of technology-driven innovations in labor productivity from demand and factor price shocks, we instrument labor productivity with physical total factor productivity (TFP) derived through a firm-

[4] Our sample is composed of single establishment firms. Hence, we use the terms establishment and firm interchangeably in the paper.

level production-function approach.[5] Since an appropriately measured TFP isolates shifts in the production function from movements along the production function, we thereby obtain an instrument for labor productivity that allows us to estimate the impact of productivity on wages. In order to deal with worker sorting and fixed firm specific factors, we exploit the matched employer-employee nature of our panel and estimate models with employer by employee fixed effects. This implies that inference is made from time-varying firm-level productivity for ongoing matched worker-firm pairs, which effectively allows us to abstract from both fixed firm-level wage policies, assortative matching and endogenous match quality.

Empirically, our paper is most closely related to Guiso, Schivardi, and Pistaferri (2005), which uses Italian data to show that transitory shocks in firms' sales are not transmitted into workers' wages, while permanent shocks are not fully transferred. Thus, firms appear to provide partial insurance to workers, in the spirit of Azariadis (1975). An alternative interpretation is that firms share rents with their workers, a point also made in the replication study on Portuguese data by Cardoso and Portela (2009). Obviously, changes in firms' sales are influenced by productivity shocks, demand shocks, shocks to wages and shocks to other factor-prices, and these shocks may influence wage setting in varying degrees. We take a more narrow approach and aim to isolate the effects of technology-driven movements in labor productivity on individual wages, once firm and worker heterogeneity have been accounted for. Importantly, our paper is silent about the role of wage contracts as an insurance mechanism, since the statistical properties of the TFP series we use to instrument labor productivity suggest that the technology shocks we capture are of a permanent

[5] In the paper, we will use the term physical TFP when we deflate the firm level output series with firm level price indices, as opposed to revenue-based TFP, which is based on sectoral deflated output. However, in a strict sense we are not measuring physical output units of a homogeneous good, as in Foster, Haltiwanger, and Syverson (2008).

nature.

We allow for shocks to firm productivity to differ in their wage impact depending on whether the shocks are purely idiosyncratic or if they are shared with other similar firms. This has two motivations. The first is that the outside options of workers, or competing bids, are likely to be affected by productivity shocks if these shocks are shared with other firms operating in the sector. The second motivation is the fact that an important element of wage bargaining in most OECD countries (including Sweden) takes place at a level higher than the firm; either at the sector or aggregate level. In this context, it is crucial to understand how purely idiosyncratic shocks are transmitted into wages and how the effects of these shocks differ from shocks that are shared within a larger bargaining unit.

To preview our results, we show that wages are causally affected by changes in both idiosyncratic firm-level productivity and sectoral productivity. The elasticity of wages to shocks that are shared within a narrow (bargaining) sector is about three times larger than the elasticity with respect to purely idiosyncratic shocks. However, since the variance of idiosyncratic productivity is higher than the variance of sectoral productivity, the actual estimated impact on wages is about the same. We find that an increase of one (within match) standard deviation of either productivity measure (sector or idiosyncratic) raises wages of incumbent workers by about one quarter of the average yearly wage growth.

We also document that the measurement issues discussed above are quantitatively important. Deflating revenues with 3-digit producer price indices instead of using firm-level prices gives estimates of idiosyncratic productivity that are almost twice as large as our baseline estimates. Accounting for the endogeneity of labor productivity also proved to be crucial. The impact of sectoral shocks is vastly understated unless labor productivity is instrumented with a properly identified TFP measure. As suggested by theory, the difference between OLS and IV estimates appears to be due to endogenous adjustments in the part of the economy where returns to scale are decreasing.

We find that accounting for match quality has a relatively minor impact on our results. To investigate this further, we assess how the average quality of the firm's labor force is affected by firm-level productivity shocks. We proceed in two steps. First, we estimate worker fixed effects (FE) for the whole economy in the six years that precede the period of our main analysis. Then, we relate sectoral and idiosyncratic firm-level shocks to these (pre-dated) worker FE in each of the manufacturing firms of our sample. Our results show that the quality of the typical firm's workforce is largely unaffected by changes in its productivity, suggesting that there is little assortative matching between workers' previous portable earnings capacity and the time-varying productivity of firms and sectors. This is perhaps surprising, considering that the shocks we study are permanent in nature. Note however that we are studying the relationship between changes in firm-level productivity and the skill mix, and not the assortative matching between workers and jobs, which lies at the essence of models of sorting featuring search frictions (see Eeckhout and Kircher (forthcoming) for an overview). A possible interpretation of our results is that the personnel policies of firms are rigid in the short and medium run, and only react to changes in technology in the long run. In line with this interpretation, Haltiwanger, Lane, and Spletzer (1999) find in matched employer-employee data for the US that while the skill distribution within establishments is tightly linked to the average sales per worker, there is virtually no relationship between changes in productivity and changes in the worker mix.

Our main results imply that the productivity component shared with other firms within narrowly defined sectors has a larger impact on individual wages than the productivity within the own firm, suggesting that it is crucial to account for the interdependency between firms when assessing the links between firm productivity and workers' wages. We also provide tentative results suggesting that about half of the difference between purely idiosyncratic shocks and sectoral shocks can be accounted for by changes in the outside option of workers (and hence the reservation wage) and conjecture that the other half most likely is related to the structure of wage bargaining in Sweden.

The rest of the paper is organized as follows. First we present our empirical strategy in Section 2. In particular, we discuss in this section the likely endogeneity of labor productivity in the wage regressions, and the advantages and pitfalls of using different TFP series as an instrument. Details on the construction of the data are provided in Section 3. The empirical results in the paper are presented in Section 4 and Section 5 concludes.

2 Method

This section is divided into two parts. First, we outline our estimated wage equation, discussing the different identification challenges that arise in the attempt to interpret the impact of labor productivity on wages as a causal relationship. We stress the potential importance of selection, the endogeneity of labor productivity and the virtues of using physical TFP as an instrument. Then, we describe the estimation strategy to derive physical TFP, and discuss the importance of obtaining an appropriately defined physical TFP series in order for TFP to be a valid instrument for labor productivity.

2.1 Estimating the Wage Impact of Productivity

Conceptually we start from a model of wage setting which allows the wages of workers to depend on individual productivity (human capital), firm-specific productivity, and local labor market conditions. All factors are allowed to be time-varying. Formally,

$$w_{ijt} = F(lp_{jt}, \theta_{lt}, x_{it}, u_{ijt}), \tag{1}$$

where w is the log wage of worker i, working for firm j at time t, and lp_{jt} denotes the log of labor productivity ($\ln(Y_{jt}/N_{jt})$). Moreover, θ_{lt} denotes the tightness (vacancies/unemployed) of the local labor market l to which firm j belongs. Worker human capital is represented by the vector x_{it} (including measures of gender, immigration status, education, age and tenure) and u_{ijt} is a measure of other factors affecting wages (treated as noise).

The specification (1) is general enough to comprise the predictions of most wage-setting models. It is evident that a spot labor market without frictions or bargaining leaves no role for firm-specific productivity to affect wages, once local labor market conditions have been accounted for. In contrast, wage bargaining models or models featuring search and matching frictions predict a positive effect from firm productivity on wages. If wage negotiations between worker and employer associations take place at a sectoral level, productivity developments at the sector level are likely to have a different impact on wages than purely idiosyncratic productivity. Similarly, productivity shocks that are shared across firms within sectors may change workers' outside option, thereby altering their bargaining position inside the firm. Hence, we allow for both, idiosyncratic firm level productivity and sectoral productivity to have a different impact on wages.

Conceptually, we can think of a two-stage, reduced-form model where sectoral wages (w_{st}) are set according to the average productivity in the sector (lp_t^S). Thereafter, firm-level wages (w_{jt}) are determined by the firms' idiosyncratic deviations from the sectoral means ($lp_{jt} - lp_t^S$). In order to account for other factors, we may allow for common time effects (ρ_t), time-invariant sector specific (z_s) and firm-specific (z_j) effects, and local labor market tightness ($\theta_{l(j)t}$). In (log) linear form:

$$\begin{aligned} w_{st} &= lp_t^S \eta_1 + \rho_t + z_s + u_{st} \\ w_{jt} &= w_{st} + (lp_{jt} - lp_t^S)\eta_2 + \theta_{l(j)t}\gamma + z_j + u_{jt}. \end{aligned} \tag{2}$$

The system naturally decomposes movements in labor productivity into a sectoral and a firm-specific component with potentially different effects on wages. In order to arrive at the empirical specification which we estimate on worker-level wage data, we add individual characteristics (x_{it}) and allow the firm-specific fixed effect to vary over individuals. Using the notation lp_{jt}^I for the firm's purely idiosyncratic productivity component (i.e., $lp_{jt}^I = lp_{jt} - lp_t^S$), we propose the following empirical specification:

$$w_{ijt} = lp_{jt}^S \eta_1 + lp_{jt}^I \eta_2 + \theta_{l(j)t}\gamma + x_{it}\beta_{x_i} + \rho_t + z_{ij} + \epsilon_{ijt}, \tag{3}$$

our parameters of interest being η_1 and η_2, which measure the responses of wages to sectoral (lp_{jt}^S) and idiosyncratic labor productivity (lp_{jt}^I), respectively. Note that the match-specific fixed effect (z_{ij}) also captures all sector and firm-level fixed factors. We estimate different versions of this model with different sets of control variables, for various subsamples, and we also let the effects of productivity vary across different types of firms and workers.

All our specifications include time effects, time-varying individual characteristics and labor market tightness at the local labor market. Similarly, we always include firm- (or match-) level fixed effects, since the TFP series we use to instrument labor productivity are derived from integrated firm-level changes, which produce an unknown constant for each firm (see details below). Firm effects also take care of any firm-specific characteristic that remains constant over the period of observation, helping to eliminate possible omitted variable biases at the firm level. For instance, good working conditions may have a positive impact on firm-level productivity, while at the same time these amenities would have a negative impact on wages if compensating wage differentials are important.[6] This would then introduce a downward bias in our estimates of productivity on wages. To the extent that working conditions and other amenities do not change in the short period of time we are studying, they would be captured by the firm-level fixed effects.

Worker fixed effects eliminate possible composition biases associated with systematic changes in the labor force of the firm that are unobservable to the econometrician. For instance, if high productivity firms tend to attract high ability workers, and high ability workers are paid higher wages, our estimates of the effects of productivity might be upwardly biased without individual fixed effects. The simultaneous account for worker and firm fixed effects therefore eliminate possible biases from sorting on either side of the market, but the average quality of retained matches can also change as a consequence of changes in productivity. For instance, poor matches may be the first to be dissolved in response to negative productivity shocks. In this case, what we interpret as the effects of productivity on wages might be driven by the sorting of bad and good quality matches that goes together with movements in productivity. We therefore include worker by firm match fixed effects in our most stringent specification. These eliminate observed and unobserved components of worker-, firm-, or match- specific heterogeneity and thus fully account for the sorting and matching of workers and firms.

When estimating the impact of productivity on wages it is important to note that movements in labor productivity will capture both movements along the pro-

[6] See Daniel and Sofer (1998) for a discussion.

duction function and shifts in the production function. Changes in product demand and factor prices may alter the scale of production and the capital-labor ratio and thereby affect labor productivity. Hence, OLS estimation of equation 3 would suffer from omitted variables and reverse causality problems. Demand shocks in combination with decreasing returns would simultaneously alter productivity and wages if the labor supply curve is upward sloping. Wage shocks are likely to change the capital labor ratio, and hence labor productivity. Technological progress, if appropriately mapped by physical TFP, shifts the production function providing a source of fluctuations in labor productivity which is unaffected by changes in input usage or the scale of production induced by e.g. shocks to product demand or factor prices. Our maintained assumption is that the impact of wage shocks on physical TFP is negligible conditional on time-varying worker and firm observable characteristics and match (employer by employee) specific fixed effects. Hence, we estimate equation 3 by IV, using physical TFP to instrument for labor productivity. Next, we turn to the derivation of the physical TFP series.

2.2 Measuring TFP

We use a production-function approach to derive our technology series. The underlying idea is that technology can be measured as the residual from a production function once changes in both stocks and variable utilization of the production factors are accounted for. We start by postulating the following production function for firm j:

$$Y_{jt} = F(Z_{jt}K_{jt}, H_{jt}N_{jt}, V_{jt}, M_{jt}, TFP_{jt}), \tag{4}$$

where gross output Y_{jt} is produced combining the stock of capital K_{jt}, labor N_{jt}, energy V_{jt} and intermediate materials M_{jt}. The firm may also adjust the level of utilization of capital, Z_{jt}, and labor, H_{jt}. Finally, TFP_{jt} is the index of technology that we want to capture.

2.2.1 Measuring TFP: Derivation

Using small letters to denote logs, taking the total differential of the log of (4) and invoking cost minimization, we arrive at:

$$\Delta y_{jt} = \psi[\Delta x_{jt} + \Delta u_{jt}] + \Delta tfp_{jt}, \tag{5}$$

where Δy_{jt} is the growth rate of gross output and ψ the overall returns to scale, which we will allow to vary between durables and nondurables, following standard results in the literature (e.g. Basu, Fernald, and Shapiro (2001)). Denote C_{jJ} as the cost share of factor J in total costs. Then, Δx_{jt} is a cost-share weighted input index defined as $C_{jK}\Delta k_{jt} + C_{jN}\Delta n_{jt} + C_{jV}\Delta v_{jt} + C_{jM}\Delta m_{jt}$. Similarly, the change in utilization of capital and labor is denoted by $\Delta u_{jt} = C_{jK}\Delta z_{jt} + C_{jN}\Delta h_{jt}$.[7] Thus, given data on factor compensation, changes in output, input and utilization, and an estimate of the returns to scale ψ_j, the resulting residual Δtfp_{jt} provides a times series of technology growth for the firm. Note that Δtfp_{jt} reduces to a gross-output Solow residual if $\psi_j = 1$, $\Delta u_{jt} = 0$, $\forall\ j$, and there are no economic profits.[8] Hence, Δtfp_{jt} is a Solow residual purged of the effects of non-constant returns, imperfect competition, and varying factor utilization.

In order to properly identify the contribution of technology, it is also important to distinguish between employees with different levels of education. Hence, using the same logic as above, we define Δn_{jt} as

$$\Delta n_{jt} = C_{jN}^{LHE}\Delta n_{jt}^{LHE} + C_{jN}^{HE}\Delta n_{jt}^{HE} + C_{jN}^{TE}\Delta n_{jt}^{TE}, \tag{6}$$

[7] Here, the cost shares are assumed to be constants. We will return to this assumption later.

[8] 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.

where superscript LHE, HE and TE denotes workers with less than high school education, high school education and tertiary education, respectively, and C_{jN}^{EDU} denotes the cost share of category EDU workers in total labor costs, where $EDU \in \{LHE, HE, TE\}$. Hence, our labor input index will capture changes in the skill composition of the workforce of the firm.[9]

The main empirical problem associated with (5) is that capital and labor utilization are unobserved. A solution to this problem is to include proxies for factor utilization. Here, we follow the approach taken by Burnside, Eichenbaum, and Rebelo (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 legitimized by assuming that there is a zero elasticity of substitution between energy and the flow of capital services. This, in turn, implies energy and capital services to be perfectly correlated.[10] Assuming that labor utilization is constant,[11] and including a set of time dummies to capture any aggregate trends in technology growth (τ_t), we arrive at the empirical specification used to estimate technology shocks

$$\Delta y_{jt} = \psi_j \Delta \widetilde{x}_{jt} + \tau_t + \Delta tfp_{jt}, \tag{7}$$

where input growth, $\Delta \widetilde{x}_{jt}$, is defined as $(C_{jK} + C_{jV})\Delta v_{jt} + C_{jN}\Delta n_{jt} + C_{jM}\Delta m_{jt}$. Note that Δtfp_{jt} encompasses any firm-specific constant/drift term.

[9] We are, however, not accounting for the contribution to production of the unobservable skills of workers or match quality. Note though that although this will affect the technology measures and their estimated dsitributions, it is not a problem for the estimation of eq. (3) as long as the specification includes match specific fixed effects.

[10] In Section 4.6 we examine the impact of using an alternative TFP series derived from estimates of the capital stock, where we can relax the Leontief assumption.

[11] In a related paper, Carlsson (2003) experiments with using various proxies for labor utilization (hours per employee, overtime per employee and the frequency of industrial accidents per hour worked) when estimating production functions like equation (7) on Swedish two-digit manufacturing industry data. Including these controls has no discernible impact on the results **of that paper**.

2.2.2 Measuring TFP: The Importance of Getting the Measures Right

Our key identifying assumption in the IV estimation of equation (3) is that physical TFP is exogenous to individual wages, and only affects wages through labor productivity conditional on time-varying worker and firm characteristics and employer by employee specific fixed effects. Here we illustrate some of the details of the empirical implementation of TFP, and we discuss why some of these details are crucial for this condition to be met. Fundamentally, we show how using alternative measures of TFP based on sector deflated output or value-added rather than gross output would yield an invalid instrument.[12]

A first point is that it is crucial that nominal output is deflated by appropriate firm-level prices and not by sectoral price indices as is customary. We use firm-level prices aggregated from unit prices for each good the firm produces (see Section 3 for further details), allowing us to derive true volume measures from gross output at the firm-level. Following Klette and Griliches (1996), the problem with the usual approach, which uses a sectoral price index (P_{Sector}) instead of a firm-level price index (P_j), can easily be seen by noting that the measure of real output deflated by sectoral prices would be $\ln (Y_j P_j / P_{Sector}) = \ln Y_j + \ln(P_j / P_{Sector})$. Hence, real output deflated by sectoral prices would be a function of relative prices. Assume next that the firm faces a constant elastic demand function, and sets its price as a (constant) markup over marginal cost as in the standard monopolistic-competition model. Since marginal cost, under standard assumptions, is proportional to unit labor cost, the relative price will be a function of wages (see Carlsson and Nordström-Skans (Forthcoming), for direct empirical evidence). Importantly, this implies that sales deflated by sectoral prices $\ln (Y_j P_j / P_{Sector})$, and consequently also the labor productivity and TFP measures derived from it, will respond to idiosyncratic wage

[12] Similar problems would, of course, emerge if we studied the direct effects of TFP on wages.

shocks. The relationship between sector-deflated labor productivity (or TFP) and wages would then produce upwardly biased estimates of the causal impact of productivity on wages, even if firms are wage takers and produce according to a constant returns to scale technology (in which case marginal cost is independent of the scale of the production).

A second point is that gross output, as opposed to value-added, should be used as the output measure. TFP series derived from standard measures of value-added are only valid under perfect competition and constant returns. Instead, as shown in Appendix C, 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. The drawback from this negative correlation can be easily illustrated in an example. Suppose there is a positive demand shock and the firm has decreasing returns. Profit maximizing firms are likely to respond by increasing production, pushing up the demand for labor, electricity and other intermediate goods. As a consequence of decreasing returns, measured TFP based on VA will decline. If the demand shock has a positive impact on wages, for instance due to an upward sloping labor demand curve, we then expect a negative bias in the wage regressions.

Finally, we use electricity consumption to proxy for variations in the use of capital services as suggested by Burnside, Eichenbaum, and Rebelo (1995). Although this may not be optimal in all settings, it should provide a good approximation for the manufacturing firms we study. The alternative would be to estimate capital stocks using the perpetual inventory method. A disadvantage of this alternative, in finite samples, is that it would require book values as starting values and these may be poor proxies for physical capital since they tend to be strategically constructed for tax purposes. Using electricity flows also has the advantage that it accounts for the actual use of the capital stock, i.e. the flow of productive services from capital, since electricity consumption responds to both capital utilization and changes in the stock of capital.

The empirical importance of these measurement issues are all thoroughly examined in Section 4.8.

2.2.3 Measuring TFP: Empirical Implementation

When empirically implementing specification (7), we take an approach akin to the strategy outlined by Basu, Fernald, and Shapiro (2001). First, the specification is regarded as a log-linear approximation around the steady state. Thus, the products ψC_{jJ} (i.e. the output elasticities) are treated as constants.[13] Note that using constant cost shares (including the cost share of labor) precludes variation in wages to spill into variation in the TFP measure if, for any reason, ψC_{jNt} is an imperfect measure of the output elasticity of labor 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.[14] Importantly, as noted by Basu and Fernald (1995), zero profits in equilibrium are consistent with a mark-up if the mark-up 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

[13] Given that we cannot observe the firms' capital stock to any precision, we cannot construct a credible direct measure of the firms' total capital cost, either. This, in turn, precludes the use of a Törnqvist-type (second-order) approximation relying on time-varying cost shares. However, the negligible effects reported **from sectoral level data** in Carlsson (2003) from using time averages relative to time-varying cost shares indicate that this is not crucial.

[14] Using the **sectoral level** data underlying Carlsson (2003) we find that the time average (1968–1993) for the share of economic profits in aggregate Swedish manufacturing revenues is about -0.001. Thus, supporting the assumption made here.

the cost shares for all other factors.

Note that the estimation of equation (7) cannot be carried out by OLS, since the firm is likely to consider the current state of technology when making its input choices.[15] We exploit the panel nature of our firm-level data to use internal instruments, as described in Section 4.

Once the series of technical change has been obtained following equation (7), the next step consists of integrating the growth rates in technology into a (log level) technology series using the following recursion

$$tfp_{jt} = tfp_{j0} + \sum_{i=1}^{i=t} \Delta tfp_{ji}. \tag{8}$$

Note that the initial level of technology (tfp_{j0}) is a firm/sector-specific constant that is not observed, but will be captured by firm fixed effects in the second stage estimation.

2.3 Sector vs. Idiosyncratic Productivity

In the empirical specification of equation (3) we distinguish between sectoral $\left(lp_{jt}^{S}\right)$ and idiosyncratic $\left(lp_{jt}^{I}\right)$ labor productivity. These measures can easily be obtained by running a regression of firm-level labor productivity, measured as gross output per worker, on sector-specific time dummies. The projection from the sector-specific time dummies in this regression is then a measure of lp_{jt}^{S}, and the residuals are a measure of lp_{jt}^{I}. We use employee weights when running this decomposition, such that sector-specific productivity is the average employee-weighted productivity,

[15] This is the so-called transmission problem in the empirical production function literature. Technology change (i.e. 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.

and idiosyncratic productivity is the firm-level deviation from this average. In an analogous fashion, we decompose the TFP series derived from (8) into a sectoral and an idiosyncratic TFP component.

3 Data

We combine three data sources to construct our sample. The employer side of the data set is primarily drawn from the Statistics Sweden Industry Statistics Survey (IS) and contains annual information for the years 1990-1996 on inputs and output as well as geographical location for all Swedish industrial (manufacturing and mining) plants with 10 employees or more and a sample of smaller plants (see Appendix B for details).[16] We focus on continuing single-plant firms in order to maximize the quality of our TFP-series: excluding multi-plant firms avoids the problem of identifying in which establishment of the firm technological change originates. The focus on continuing plants helps us deal with possible selection effects due to firm demographics associated with productivity shocks.

A crucial feature of IS is that it includes a firm-specific producer price index constructed by Statistics Sweden. The firm-specific price index is a chained index with Paasche links that combines plant-specific unit values and detailed disaggregate producer-price indices (either at the goods level, when available, or at the most disaggregate sectoral level available). Note that in the case in which a plant-specific unit-value price is missing (e.g., 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 4-digits goods code level). The disaggregate sectoral producer-price indices are only used when a plausible goods-price index is not available. Thus, the

[16]The availability of detailed factor input data, specifically electricity consumption, which are crucial for the present study, limits the sample years to 1990-1996.

concern raised by Klette and Griliches (1996) regarding biased returns to scale estimates when sectoral price deflators are used in the computations of real gross output should not be an issue here. We use this price index to deflate output both when constructing labor productivity and when deriving the TFP-series.

The employee side of the data is obtained from the Register Based Labor Market Statistics data base (RAMS) maintained by Statistics Sweden. This data contains information on annual labor earnings for all privately employed workers in Sweden. The raw data was compiled by the Swedish Tax Authority in order to calculate taxes. The data includes information on annual earnings, as well as the first and last remunerated month received by each employee from each firm. We use this information to construct a measure of monthly wages for each employee in each of the firms in our sample, closely following the procedures of Nordström Skans, Edin, and Holmlund (2009) and Carlsson and Nordström-Skans (Forthcoming). The data lacks information on actual hours, so in order to restrict attention to workers reasonably close to full time workers we only consider a person to be a full-time employee if the (monthly) wage exceeds 75 percent of the mean wage of janitors employed by municipalities.[17] We only include employment spells that cover November following the practice of Statistics Sweden. We focus on primary jobs and therefore only keep the job resulting in the highest wage for workers with multiple jobs. The data also includes information on age, gender, education, and immigration status of the individual workers.

Unemployment and vacancy data at the local labor market level for November is collected from the National Labor Market Board (AMS). Here, we rely on the

[17] Using a similar procedure with RAMS data, Nordström Skans, Edin, and Holmlund (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 percent random sample in the LINDA database, where hourly wages are the measure of pay.

1993 definition of homogenous local labor markets constructed by Statistics Sweden using commuting patterns, which divide Sweden into 109 geographic areas.

Note that we use the labor input measure available in IS to compute labor productivity, whereas the labor input measures used when estimating TFP are taken from RAMS. As mentioned above, the IS employment data is based on a survey collected by Statistics Sweden, whereas the RAMS employment data is based on the income statements that employers are, by law, required to send to the Swedish Tax Authority. Since the IS and RAMS measures of labor input are independently collected it is very unlikely that any measurement errors are common in the two. This, in turn, is important for ruling out that any observed relationship between labor productivity and technology is only due to common measurement errors in the labor input measures.

Both RAMS and IS provide unique individual and firm identifiers that allow us to link the employees to each of the firms in the sample. Since the RAMS data covers the universe of workers, we observe every worker employed in each of the IS firms during the sample period. Given the restrictions mentioned above and after standard cleaning procedures (see Appendix B for details), we are left with a balanced panel of $1,136$ firms observed over the years 1990-1996 and $472,555$ employee/year observations distributed over $106,050$ individuals. Our used data set cover about 10 percent of the total manufacturing sector.

4 Estimation Results

4.1 Estimating TFP

We first estimate the technology disturbances relying on the empirical specification (7) outlined in section 2 above. Here, we allow the returns to scale parameter ψ_j to vary across durables and non-durables sectors as suggested by Basu, Fernald, and Shapiro (2001). 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 Carlsson and Smedsaas (2007) and Marchetti and Nucci (2005), we use a difference GMM estimator developed by Arellano and Bond (1991) and report robust, finite-sample corrected, standard errors following Windmeijer (2005). Here we use $\Delta\widetilde{x}_{jt-s}$, for $s \geq 3$, as instruments and collapse the instrument set in order to avoid overfitting (see Roodman, 2006).[18]

In Table 1, we present the estimation results for equation (7). 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 (s.e. of 0.19 and 0.22, respectively).[19] It is reassuring to see that the point estimates of the returns to scale are very similar to estimates reported by earlier studies. For example, Basu, Fernald, and Shapiro (2001) reports estimates of 1.03 and 0.78 for durables and non-durables, respectively, using U.S. 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.

Importantly, Table 1 show that the AR(2) test of the differenced residuals (see Arellano and Bond (1991)) indicates that there is no serial correlation in the estimated technology change series. This implies that we can regard these changes as permanent innovations to the technology level**.** The fact that our shocks in general

[18] Given that we use a difference GMM estimator, the second and higher ordered lags of $\Delta\widetilde{x}$ 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 five-percent level.

[19] The data does not allow us to identify the returns to scale parameter separately across two-digit industries since many sub-samples become too small.

appear to be of a permanent nature is consistent with the view that changes in TFP are capturing shifts in the production function. This should however be kept in mind when comparing our results with previous literature (e.g. Guiso, Schivardi, and Pistaferri (2005)), where the role on wage determination of temporary vs. permanent shocks to sales has been evaluated.

4.2 The Impact of Productivity on Wages

Before moving into the main results of the paper, we provide a brief description of the distribution of wages, TFP and labor productivity. Summary statistics are available in Table 2. First of all note that the dispersion of productivity is much wider than the dispersion of wages, but that this relationship is to a large extent driven by large differences between firms. The variance (over time) within an employment spell (i.e. a match between a worker and a firm) is about equal. In the analysis we distinguish between a sectoral and an idiosyncratic component as discussed above. The sectors are identified following the 16 employer federations that sign collective agreements in the manufacturing sector.[20,21] When decomposing productivity within and between sectors we see that the within-match standard deviation of idiosyncratic firm-level productivity is nearly three times larger than the variance of sectoral productivity.

We proceed by investigating the role of sectoral and firm idiosyncratic productivity on individual wages, following equation 3. The first column in Table 3 shows the results of estimating a simple OLS regression that relates labor productivity to individual wages controlling for firm-level fixed effects, but excluding worker controls. Column 2 shows the same specification, now using idiosyncratic and sector-level TFP as instruments for the two labor productivity measures. Column 3 adds a

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

[21] The Appendix provides further details on the Swedish institutions and the bargaining system.

third-order age polynomial and worker fixed effects. Column 4 presents our most stringent specification, including match-specific fixed effects. Column 5 repeats the last exercise for males. Standard errors are robust to intra-firm correlation.

Both firm-level idiosyncratic labor productivity and sectoral labor productivity matter for wage determination. However, in order to obtain this conclusion it is fundamental to instrument the labor productivity measures, at least with regards to the sector-specific productivity. The OLS results in column 1 suggest a positive and statistically significant impact of idiosyncratic labor productivity on wages, with an elasticity of 0.033. The estimated coefficient of the sector-specific productivity presents a similar magnitude (0.027), but is not statistically different from zero. In sharp contrast, when we use TFP to instrument the labor productivity measures in column 2, we find an elasticity of wages to sectoral productivity that is substantially larger than the elasticity with respect to idiosyncratic productivity, 0.123 compared to 0.032. Both estimated coefficients in column 2 are statistically significant at the 1% level.[22] We will return to a discussion below of potential explanations for why the sectoral OLS results may be downwardly biased. Table 4 shows the first-stage regressions. The values of the F statistics are well above 10, suggesting that our instruments are not weak. More importantly, the Kleibergen-Paap rk LM statistic (see Kleibergen and Paap (2006)), presented at the bottom of Table 3, clearly reject the null hypothesis of underidentification.[23]

The rest of Table 3 shows that the results are somewhat larger when we include covariates that capture the skills and qualities of workers and indicators of match

[22] In a similar vein, Fuss and Wintr (2009) finds that aggregate wages per employee at the firm-level are more reactive to sectoral than firm level TFP shocks in Belgium.

[23] Note that the fact that the first stages are close to unity suggests that the endogenous response in input usage is small. This is reassuring since we are using a balanced panel and, due to the relatively short period available, are unable to model exits of firms.

quality. Column 3 accounts for individual observed and unobserved heterogeneity by means of an age polynomial and individual fixed effects. The idiosyncratic component increases to 0.050, while the sectoral component increases to 0.149. The results are virtually identical if worker and firm effects are replaced by worker-firm match fixed effects, as shown in column 4. The latter may be a result of the fact that we are using a fairly short panel and only a subset of the economy, which means that the individual fixed effects in many cases are identified from single spells (i.e. that the match fixed effects are already captured in the model with worker and firm fixed effects).[24]

Our data does not allow us to properly control for part-time work, but since part-time work in Sweden is very rare among males in the manufacturing sector we have reestimated the model using only males. Column 5 presents results for the male sub-sample. The estimates show that the response of male wages to 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.

Albeit the estimated elasticities are far from unity, one must bear in mind that the variance of 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 4 of table 3, we find that

[24] In regressions not reported in the text, we have also analyzed the direct effect of TFP on wages. For this purpose, we parallel the specification in column 4 of Table 3 and estimate the impact of TFP on wages using OLS. We find marginally smaller effects on wages than those reported in Table 3 (0.124 and 0.042, respectively). This is not surprising, considering that the the first stage of the IV regressions (Table 4) showed estimates of the TFP components somewhat smaller than unity. This model is, however, more sensitive to potentially attenuating measurement errors.

an increase of one standard deviation in either of the productivity measures (sector or idiosyncratic) raises wages by about one quarter of the average *real* wage growth in our sample.[25]

4.3 Returns to scale, OLS and IV

The estimated impact of sectoral productivity developments on wages in the IV specifications is much larger than in the OLS regressions. A simple yet plausible explanation for such differences is attenuation bias due to measurement errors, but in that case it would be expected that there is a similar gap between IV and OLS estimates for the idiosyncratic productivity shocks. As this bias was not found, our results are most likely indicative of an endogenous negative association between labor productivity and wages at the sectoral level, which we try to examine next.

One straightforward explanation is a combination of decreasing returns to scale and an upward sloping labor supply curve, the intuition being that when firms choose to scale up production (e.g., in response to demand shocks) they will endogenously lower labor productivity if returns to scale are decreasing. The resulting increase in demand for labor will lead to higher wages if the supply curve facing the sector (or firm) is upward sloping. This may explain why instrumentation matters specifically at the sectoral level and not at the idiosyncratic firm level, since wages may be pushed up more in response to sectoral adjustments if firms within a sector compete over a restricted set of workers. Hence, increased demand for labor within a sector

[25] Average real wage growth within the manufacturing establishments included in the sample is 2.4%. Considering that our estimates are conditional on time effects, the estimated elasticities should be read as the impact of the different productivity components on real wages. Hence, the estimated impact of one s.d. idiosyncratic productivity on wages amounts to 28% (0.051*0.130/0.024) of the average real wage growth, while the impact of one s.d. sectoral productivity is 22% (0.149*0.036/0.024)

may raise wages while single firms may be allowed to hire freely without affecting wages in the market. Naturally, differential wage responses to increases in firm labor demand versus sectoral labor demand may be reinforced by sectoral bargaining.

While the combination of decreasing returns with an upward sloping wage setting curve is consistent with our main results, we also try to provide a piece of somewhat more direct evidence by tentatively investigating the role of returns to scale. As shown previously, estimated returns to scale vary between the manufacturing plants producing durable goods (decreasing returns) and those producing non-durables (almost constant returns). Although our estimates of the returns to scale are imprecise, similar differences between durables and non-durables have been previously found in the literature (e.g. Basu, Fernald, and Shapiro (2001)). Hence, we expect the gap between IV and OLS estimates for sectoral productivity to be larger in firms operating in durable goods sectors than in firms operating in the non-durables sectors.

In Table 5 we estimate our preferred model (i.e. with match-specific fixed effects) using OLS and IV separately, for firms with decreasing and constant returns. The results are consistent with the proposed hypothesis. The entire difference between the sectoral OLS and IV estimates stems from the firms facing decreasing returns in our sample, while differences within firms facing constant returns are negligible and non-statistically significant. In the case of firms with decreasing returns, we see that the elasticity of wages to sectoral shocks becomes highly significant and more than 4 times larger in the IV specification (0.14 vs. 0.027 in OLS). Interestingly, we also see that instrumentation leads to a non-negligible increase in the estimate of the idiosyncratic productivity effects on wages (from an elasticity of 0.033 in the OLS specification in column 1 to 0.052 in column 2).[26] This suggests that aggregation

[26] The differences between the IV and OLS elasticities in columns (1) and (2) are statistically significant at the 5% level. The p-values of one-sided tests are 0.044 in the case of idiosyncratic

over the sectors also blurred an important role for scale adjustment in response to idiosyncratic productivity, in the sectors where returns are decreasing.

4.4 Bargaining Power, Outside Options and Sectoral Productivity

The difference in the estimated impact on wages between sectoral and idiosyncratic productivity implies that workers extract more rents when productivity advancements are shared within a bargaining sector.[27] This can be explained by two different mechanisms. Firstly, workers' bargaining power may differ depending on the level of negotiation. In practice, workers may have more bargaining power during sectoral negotiations, since strikes are illegal during local bargaining but not during sectoral bargaining (see Appendix A for a discussion). Secondly, the shocks that are shared within a sector also affect the outside option of workers (or equivalently, the quality of counterbids in a poaching game, as in Cahuc, Postel-Vinay, and Robin (2006)), strengthening their bargaining position. This may occur if workers are mobile within sectors clustered in certain geographic areas. In this case, an increase in the bargaining position of workers is expected to be higher if technology improves in all of the firms operating in the same sector.

In order to disentangle the two forces outlined above, we re-estimated the model controlling for predicted outside wages.[28] If the higher elasticity of wages to sectoral shocks is due to an improvement in the outside option of workers, we expect the estimated elasticities to decline once an estimate of the outside option of workers is included in the regression.

productivity and 0.004 in the case of sectoral productivity.

[27] Interestingly, this result does not seem to be related to the particular definition of the sector we use. We have experimented using standard definitions of sectors, following the NACE classification at a two-digit level instead of the employer confederation of the firms, and find very similar results.

[28] These estimates, however, might be endogenous, so the interpretation of these results should be read with a grain of salt.

We estimate the outside wages as the predictions from 763 local labor market (109 areas) and year-specific (7 years) wage regressions using information about age, gender, immigration status (7 regions) and education (both four-digit field and three-digit level codes building on ISCED 97). We do these first-stage regressions for the universe of full-time primary employments in the private sector, which amounts in our sample to $11,523,194$ observations for $2,653,639$ workers. As expected, re-estimating equation (3) controlling for these predicted wages (highly significant with an elasticity of 0.58) in the regressions reduces the impact of sectoral productivity (from 0.149 to 0.096), but a substantial difference between the idiosyncratic and sectoral estimates remains (0.057). Moreover, the elasticity of wages to idiosyncratic productivity is much less affected by the inclusion of outside wages. The difference between the two elasticities is marginally statistically significant (p-value of 0.11). Taken at face value, these estimates suggest that the workers' ability to extract larger rents from sectoral than idiosyncratic shocks is equally driven by the two mechanisms we postulated: stronger bargaining power and better outside options.

4.5 The Role of Dynamics

The specifications we have presented so far are static, i.e., they assume that the wage impact of technology-driven innovations in productivity is immediate. In reality, permanent technology shocks might require some time to be absorbed by wages, e.g., if wage bargaining takes place biannually. In order to assess the importance of potential delays in the impact of productivity, we have estimated models with lagged productivity.

Estimates from specifications with lagged productivity are presented in Table 6. We concentrate on our preferred specification (including match-specific fixed effects) and proceed parsimoniously, first introducing one lag in column 2 and two lags in column 3.[29] The bottom of the table 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 effect does, however, seem to deteriorate fairly rapidly and in the case of sectoral productivity we never find the individual lags to be statistically significant. Although the individual lags are estimated with poor precision, the long-run elasticity remains statistically significant in all cases. The magnitude of the long-run impact is about twice as large as the contemporaneous impact for both the idiosyncratic effect (0.091 vs. 0.051) and the sectoral effect (0.303 vs. 0.149) when two lags are considered.

4.6 Variations

A very active literature (Shimer (2005), Hall (2003), Pissarides (2007)) discusses why unemployment fluctuates so dramatically over the business cycle compared to the smooth movements in aggregate productivity. A key element in this debate is the exact modeling of how firms react in their setting of wages for incumbents and new hires when productivity changes (Haefke, Sonntag, and Van Rens (2008)). We have analyzed 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 are quite imprecise. We have also investigated if productivity advances affect workers with different skills differently, again finding no significant heterogeneity, although with poor precision. Finally, we have analyzed 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 due to downward nominal wage rigidity. We find no evidence of such asymmetries. Although this may seem surprising, it should be noted that

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

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. This implies that there is indeed scope for a symmetric impact of positive and negative productivity shocks, even if nominal wages never fall.

4.7 Productivity and the Selection of Workers

Our main estimates are only marginally affected by the inclusion of individual specific fixed effects. This suggests that compositional effects through firm recruitment and firing policies as a response to technology-induced changes in firm-level productivity should be minor. In order to make this point more precise, in this section we relate our measures of firm and sector level productivity to measures of the employed workers' earnings capacity.[30]

To assess the impact of firm level productivity on worker sorting we proceed in two steps. First, we estimate a two-way fixed effects model with wages as the dependent variable, including person and firm fixed effects along the lines of the Abowd, Kramarz, and Margolis (1999) model (also including an age polynomial and year dummies), relying on data for the *universe* of full-time primary employments in the Swedish private sector during the period 1985-1989, i.e. the available years *before our sample*.[31] All in all, this amounts to $8,776,223$ linked employer-employee observations. From these pre-sample estimations we extract the person effects. In a second step, we use the estimated person effects as dependent variables in the same specifications that we used for our wage regressions.[32]

[30] See e.g. Abowd, Kramarz, Perez-Duarte, and Schmutte (2010) for an interpretation of estimated person effects in a structural matching framework.

[31] We use the Abowd, Creecy and Kramarz (2002) algorithm as implemented for STATA in a2reg by Amin Ouazad.

[32] Although the time span for estimating the person effects is short we find a very high correlation (0.96) with estimates of person effects for the full 1985-1996 period, which is reassuring.

The analysis presented in this section is an attempt to assess the importance of assortative matching in the labor market without relying on the correlation between worker and firm fixed effects, an approach popularized after Abowd, Kramarz, and Margolis (1999). Indeed, recent work has established the difficulty of assessing assortative matching relying on wage data only.[33] Instead, our approach brings in productivity data in an attempt to establish the link between individuals' earning capacity and firm level productivity. Our 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, which allows to compare the size of the selection responses with the wage responses previously studied. 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.

It should be noted that the labor productivity and TFP measures we use are at the firm level, and not at the job level. Hence, we cannot attribute productivity differences to each individual job, which is fundamental to assess the importance of sorting in most search models where the commonly held assumption is one-firm-is-one-job (see Eeckhout and Kircher (forthcoming) for a discussion). Our analysis is an attempt to provide a causal estimate of the impact of firm level productivity on the skill mix of organizations, a topic that has received recent attention in the literature.[34]

[33] See Eeckhout and Kircher (forthcoming) and the references therein for a discussion. This study also provides a procedure that allows assessing the strength of sorting, but not the sign, based on wage data only.

[34] Empirically, Haltiwanger, Lane, and Spletzer (1999) and Mendes, Van Den Berg, and Lindeboom (2010) study the association between firm level productivity and the skill mix. As we argue in the case of wages, establishing the direction of causality is not straightforward. Theoretically, Eeckhout and Pinheiro (2010) present a model with multi-worker firms and analyze the relationship

The results from the IV model including firm fixed effects are presented in Table 7. Column 1 shows the overall results, and columns 2 to 4 show separate estimates for samples of workers with less than high school education, high school-educated employees and workers who attended tertiary education, respectively. The estimates of the elasticity of the portable earnings capacities to firm idiosyncratic and sectoral productivity are very close to zero, in particular those relative to idiosyncratic productivity, and non-statistically significant. When we split the sample according to observable skills, we see a tendency toward negative assortative matching in response to sector-specific productivity in the group with the lowest skills.[35] All estimates related to idiosyncratic productivity are tiny and non-different from zero at standard levels of testing. We have also experimented with productivity lags in this specification, but the effects remain insignificant and small.

Overall, the analysis therefore suggests that the skill composition of workers within a firm is largely unaffected by changes in both sectoral and idiosyncratic productivity. This result may appear at odds with Eeckhout and Pinheiro (2010), which shows first-order stochastic dominance of the skill distribution in high productivity firms in a model of multi-worker organizations. An alternative interpretation is that firms' personnel policies are changed infrequently, and changes in TFP are transmitted into changes in the skill mix with a significant lag. In line with this interpretation, Haltiwanger, Lane, and Spletzer (1999) find in matched employer-employee data for the US that while the skill distribution within establishments

between the skill distribution within the firm and TFP.

[35] Interestingly, these estimates are in line with the conclusions in Abowd, Kramarz, Perez-Duarte, and Schmutte (2010), who estimate a structural job assignment model with coordination frictions. They find that low ability workers have a comparative advantage in highly productive firms within manufacturing, but that the empirical influence of sorting is minor because of limited heterogeneity. For results that instead point towards positive assortative matching and further references, see Mendes, Van Den Berg, and Lindeboom (2010).

is tightly linked to the average sales per worker, there is virtually no relationship between changes in productivity and changes in the worker mix.

4.8 Measurement issues

In the main text, we have stressed the importance of using the right price measures to deflate output in the main text. Following our discussion in Section 2.2.2, wage shocks will transmit into measured real output series if sectoral prices are used when deflating sales to obtain real output, generating a positive bias in the estimated impact of idiosyncratic productivity. This conjecture is confirmed by the results presented in Table 8. The first column replicates our baseline results for the sake of comparison. In column 2, we use 3-digit PPI deflators instead of firm-level prices to derive gross output. Using sectoral deflators results in an estimated elasticity of idiosyncratic productivity of almost twice the size (0.092) of the benchmark. Two-sided tests show that this difference is statistically significant at the 1% level.

As a second experiment, we show the impact of using a measure of value-added instead of gross output to derive the productivity and technology series.[36] As discussed in Section 2.2.2, the main problem with value–added based measures of TFP is that they will be negatively correlated with the intensity of the use of primary inputs, including labor, if there are decreasing returns to scale. Column 3 of Table 8 presents the results of using a value-added Solow residual to instrument for value-added labor productivity.[37] We see that the value-added estimates are con-

[36] Real value added va_{jt} is measured as gross output minus intermediary and energy costs deflated by our firm-level price index. The growth rate of the primary input index is measured as $\Delta x_{jt}^{VA} = \left(\frac{C_{jK}}{C_{jK}+C_{jN}}\right)\Delta v_{jt} + \left(\frac{C_{jN}}{C_{jK}+C_{jN}}\right)\Delta n_{jt}$. Value-added labor productivity is simply obtained as real value added per employee.

[37] The value-added calculations require different data and give rise to a slightly different sample. We have estimated the baseline model with this restricted sample, obtaining virtually identical coefficients as those shown in Column 1.

siderably smaller than those based on TFP, as expected if demand shocks have a positive impact on wages. The negative bias is somewhat larger for the sectoral elasticity, suggesting that the wage effect of labor demand is more likely to be seen when demand shocks are shared within sectors

Finally, we have experimented with alternative series of TFP, which are based on a specification derived from estimated capital from book values. This approach has limitations, but relaxes the assumption of perfect complementarity between the flow of capital utilization and electricity use we made in our preferred TFP series. Appendix C.1 provides the details of the construction of the alternative TFP measures. The estimated elasticities are slightly smaller than those reported in the main text, but deliver a very similar message.[38]

5 Conclusions

We have studied how individual wages are affected by the changes in productivity of the firms where the workers are employed. In order to derive the causal impact of firm productivity on individual wages we have relied on a carefully constructed measure of physical total factor productivity as an instrument for measured labor productivity. Importantly, we use unique data on firm-level prices and outputs, which allows us to accurately measure technical change purged of relative price adjustments in our panel of manufacturing plants. In addition, we have relied on matched employer-employee data to purge the analysis of sorting on both the supply and demand side.

[38] The resulting sample after the calculation of TFP based on a measure of the capital stock is slightly different than the sample used in the main text. Hence, for the sake of comparison we have repeated the analysis also with our preferred TFP series. We find an elasticity of 0.042 with TFP based on capital instead of 0.047 for the idiosyncratic component, and 0.12 for TFP based on capital instead of 0.15 with our preferred TFP series, in the case of sectoral productivity.

We find that firm-level productivity has an impact on workers' wages, which contrasts to simple frictionless competitive models where individual wages only depend on aggregate labor market conditions and individual skills. Changes in productivity that are shared within a sector have a 3 times larger impact on wages than purely idiosyncratic innovations. The long-run impact of both types of shocks is about twice as large as the short-run impact, but the relative importance is the same. The results therefore suggest that both workers and firms benefit from firm-level technological advancements, but that substantially less of the benefits are extracted by workers if the productivity increases are purely idiosyncratic. However, since the standard deviation of idiosyncratic (within-match) productivity is about three times larger than that of sectoral productivity, it plays a similar role in shaping workers' wage increases: a one standard deviation increase in either sector-specific or idiosyncratic productivity has a wage impact amounting to about one quarter (half) of the average yearly wage growth of incumbent workers in the short (long) run.

Our analysis reveals that systematic sorting of workers is of minor importance in this context, which suggests that firms' recruitment policies largely remain unaffected by changes in firm-level productivity. Our results do, however, show that the use of a properly defined TFP measure is crucial for the identification of the causal impact of labor productivity on individual wages. We show that OLS estimates of sectoral productivity are downwardly biased, which is consistent with wage impacts stemming from demand shocks in the presence of decreasing returns and upward sloping wage curves. In contrast, demand shocks are expected to yield an upward bias on estimates of idiosyncratic productivity if the TFP series that is used to instrument labor productivity is derived from standard measures based on sector-deflated output, rather than output deflated using firm-level prices. Our empirical exercises are also in line with this theoretical prediction. These findings, in turn, suggest an important role for other firm-level shocks such as idiosyncratic demand shocks in the determination of individual wages, a feature that deserves further research.

Overall, the paper provides three important insights for future research on the relationship between firm productivity and wages. The first is that proper measurement is crucial to understand the relationship between wages and productivity. Most notably, our estimates suggest that productivity as it is typically measured is a function of wages through the relative-price component, which remains when revenues or value-added are deflated by sectoral prices. Thus, it is perfectly possible to find links between productivity and wages even if the labor market is perfectly competitive and firms produce according to constant returns to scale technologies, as long as product markets are imperfect. The second insight is the lack of positive assortative matching between workers' earning capacity, as estimated from previous earnings, and the time-varying productivity of firms or sectors. This lack of assortative matching is perhaps surprising, considering that the shocks we analyze are of permanent nature. We speculate that this might reflect relatively rigid human resource policies at the firm level, which are reviewed infrequently. Finally, it appears to be clear that changes in productivity in other firms within narrowly defined sectors matter more for individual wages than the changes of firm-level productivity that are purely idiosyncratic. If this is due to the importance of collective bargaining above the firm level, or due to market forces related to the quality of outside opportunities in–or poaching offers from–competing firms is an interesting question that deserves further research, although estimates presented in this paper suggest a role for both.

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Appendices: not for publication

A Wage Setting Institutions in Sweden

This Appendix discusses wage-setting institutions in Sweden.[39] The Swedish model of wage determination is typically associated with centralized wage bargaining and wage compression. Wage negotiations on a nationwide level were implemented in the 1950s and remained a key feature of Swedish institutions until the early 1980s. The central agreements were initially laid out by the central blue collar (LO) and the employer confederation (SAF), but with a gradually increasing role also for white collar unions (TCO and SACO). Interestingly, one of the key motivations for centralized bargaining was the idea that wages should not vary between firms or industries depending on productivity ("*equal pay for equal work*"). The theory, which was widely accepted, was that wages should reflect differences between individuals' qualifications, but not firms' abilities to pay. On the employer side, the policies also meant that the most productive firms were allowed to make large profits without sharing them with the workers. Central from the unions' perspective was that this would lead to the closing of unproductive firms, but that active labor market policies should help workers move from low productive parts of the economy into more productive segments. In that sense the wage policy was highly growth oriented. Importantly, although central agreements laid out the central principles for wage setting, they were always followed by negotiations at the industry and firm levels. Even in the hay-days of central bargaining, about half of individual wage increases in the industrial sector was "wage drift", i.e. wage changes above central agreements (Hibbs and Locking (1996)).

Although the model in principle was in favor of wage differences between workers of different qualifications, unions had a clear ambition to reduce overall wage differences. This led to increasing complaints from the employer side during the late 1970s. Following a few years of turmoil, the metalworkers' union signed a separate industry-level agreement with the employer side in 1983. This was essentially the end of centralized wage bargaining in Sweden, leaving room to a model of industry level bargaining. Importantly, collective ac-

[39] This section draws on Nordström Skans, Edin, and Holmlund (2009).

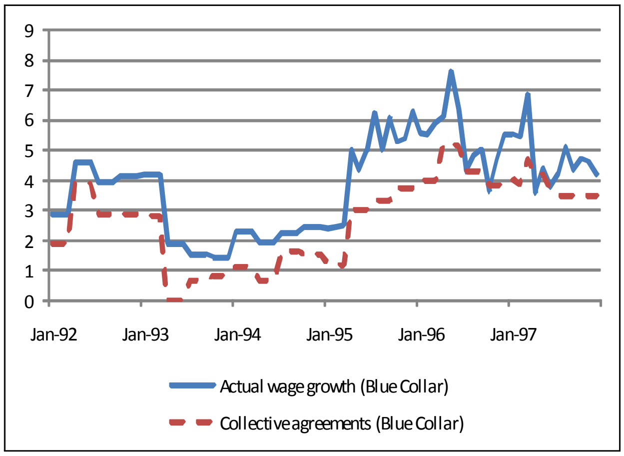

Figure 1: Annual wage growth for blue collar workers in the private sector, actual and bargained wages. Source: National Mediation Office.

tions in the form of strikes or lock-outs where allowed when negotiating at the industry level but not at the firm level. However, occasional illegal strikes and other forms of reduced effort, e.g., in the form of excessive sickness absence, during firm-level bargaining suggest that collective actions may have also played an important role in local bargaining. An exception to the decentralized bargaining was made in the early 1990s when an economic crisis was approaching and inflation was in double digits. In an effort to curb wage inflation and preserve the fixed exchange rate, a government commission coordinated the social partners into a one-time central agreement (the *"Rehnberg agreement"*) for the period $1991-92$. Following the agreement, Sweden returned to a period of uncoordinated industry-level bargaining. During the mid 1990s a very generous agreement in the paper-producing sector spurned fears that wage inflation was again becoming a serious threat to macroeconomic stability, and to the labor market recovery, which was yet to be seen. As a response, white and blue-collar unions in the industrial sector jointly suggested a new system of coordinated bargaining where the industry sector jointly bargained first, and other sectors followed. This system of coordinated industry-level bargaining has remained largely unchanged since its start in 1997.

Our period of study is 1990−96. With the exception of the Rehnberg agreement, this was a period of uncoordinated industry-level bargaining and firm-level wage drift. Empirically, the period coincides with a period of continuous increase in wage dispersion from the mid-1980s after several decades of wage compression (see e.g. Gustavsson (2006)). As shown by Nordström Skans, Edin, and Holmlund (2009), the increasing overall wage dispersion is primarily due to increased wage differences between firms (both within and between industries). The wage dispersion within firms has remained largely unchanged since the early 1990s. Figure 1 shows the evolution of negotiated (at the sectoral level) and actual wage increases during the early 1990s for blue collar workers in the private sector. As the figure shows, both components were substantial.

B Data Construction

The firm data set we use is primarily drawn from the Industry Statistics Survey (IS) and contains annual information for the years 1990-1996 on inputs and output for all Swedish manufacturing plants with 10 employees or more and a sample of smaller plants. The data is matched to RAMS, which adds individual wages and worker characteristics of each employee of the manufacturing plants included in the sample. Here we focus on continuing plants that are also a firm.

When computing labor productivity, labor input, N, is measured as the average number of employees during the year and is taken from the IS. Based on Swedish sectoral level data within manufacturing, Carlsson (2003) reports that the growth rate of hours per employee is acyclical. Thus, we are not likely to leave out any important variation in labor input by looking at only the growth rate of the extensive margin. To compute the input index, $\Delta\widetilde{x}$, used to estimate the returns to scale and change in technology, real intermediate inputs, M, 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, is measured as the plants' electricity consumption in MWh taken from the IS.

When computing the (overall) cost shares, we also need a measure of the firms' labor cost, which is defined as total labor cost including e.g. payroll taxes available in the IS. Also, to calculate the cost shares by education in expression (6) as well as the growth rate for respective category of labor input, we use the RAMS data (see discussion in section 3 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.[40] Moreover, since Sweden experienced a boom bust cycle in the late 80s and early 90s we do not use observations from firms experiencing large losses when calculating the two-digit cost shares. In the calculations we drop observations for firms where the (residual) capital share is below -10 percent of sales. This procedure gives rise to aggregate manufacturing cost shares that are similar to those obtained using the data underlying Carlsson (2003).[41]

Although we have removed obviously erroneous observations, the firm data set still contains very large observations in Δy_{jt} and $\Delta \widetilde{x}_{jt}$. 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 2, 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 e.g. $dy = 0.6$ corresponds to an annual increase of 82 percent in real output.[42] This procedure removes 160 firms from the sample leaving us with $1,138$ firms.[43] In order to decompose the technology series into a sectoral and an idiosyncratic part we need to drop

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

[41] The aggregate manufacturing shares in Carlsson (2003) (our sample) equals $C_M = 0.65$ $(0.66), C_N = 0.25$ $(0.20), C_K = 0.07$ (0.12) and $C_V = 0.03$ (0.03).

[42] The chosen intervals are slightly more limiting with respect to the distribution for dy and dx. However, making a small increase in these two intervals yields very similar results, relative to those presented in Tables 1 to 3 in the main text.

[43] We do not remove observations with large movements in labor productivity since this variable will be instrumented in the econometric procedure.

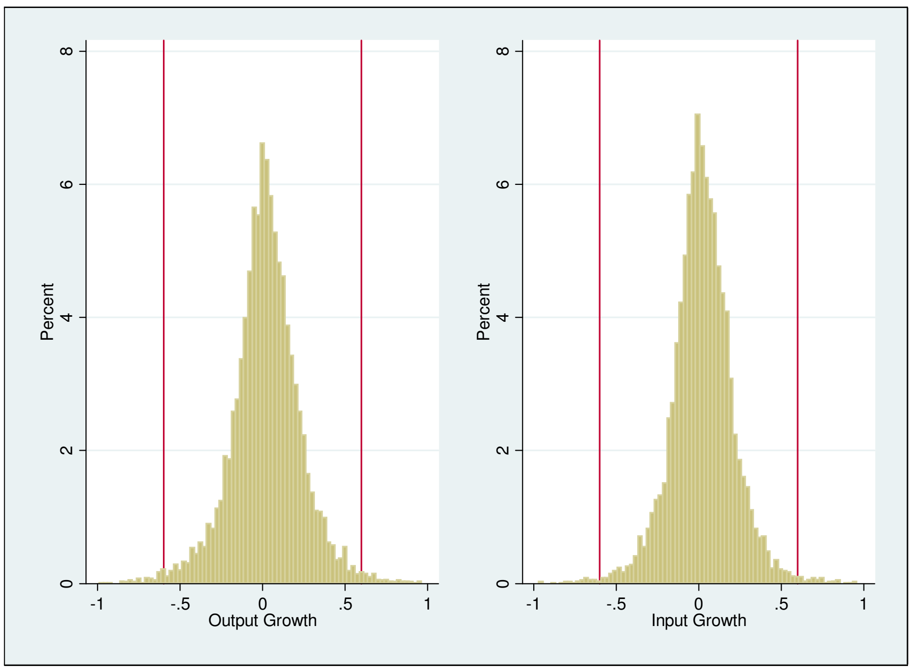

Figure 2: Distribution of output and input growth rates. Vertical lines indicate truncation limits.

two additional firms since 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 the specification (7).

After merging the final firm-level data with the employee data in RAMS we arrive at $474,528$ employee observations across $106,815$ individuals. Removing observations where education information is missing we have $472,555$ observation across $106,050$ individuals left. This data set covers about 10 percent of the total manufacturing sector employment.

Finally, unemployment and vacancy data on the local labor market level is collected from the National Labor Market Board (AMS). The data contains 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 labor markets constructed by Statistics Sweden using commuting patterns, which divides Sweden into 109 areas.

C The problems with using VA to derive TFP

A standard approach to derive TFP is to rely on value-added as a measure of production. As we illustrate in this Appendix, 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.

Using the implicit definition of the divisia index of value-added, we arrive at[44]

$$\begin{aligned} \Delta va_{jt} &= \psi_j \Delta x_{jt}^{VA} + \frac{\Delta tfp_{jt}}{1 - C_{jM} - C_{jV}} \\ &\quad + \frac{(\psi_j - 1)}{1 - C_{jM} - C_{jV}} \left(C_{jV} \Delta v_{jt} + C_{jM} \Delta m_{jt} \right), \end{aligned} \tag{C1}$$

where $\Delta x_{jt}^{VA} = \left[\left(C_{jK} / \left(C_{jK} + C_{jN} \right) \right) \Delta k_{jt} + \left(C_{jN} / \left(C_{jK} + C_{jN} \right) \right) \Delta n_{jt} \right]$ is the weighted growth rate of primary factors and Δva_{jt} is the growth rate of real value-added.[45] As can be seen

[44] See Basu and Fernald (1995) for a full derivation. Note that Basu and Fernald (1995) does not separate between intermediates and energy as is done here.

[45] For clarity, we do not substitute Δk_{jt} with Δv_{jt} here. This is, however, done in the empirical work.

in equation (C1), real value-added will not only depend on primary factors, but also on materials and energy growth, unless there are constant returns. To see why, one can think of real value-added as a partial TFP measure subtracting the productive contribution of materials and energy from real gross output under the assumption of perfect competition and constant returns. Hence, when constructing a value-added Solow Residual, Δtfp_{jt}^{VA},

$$\begin{aligned}\Delta tfp_{jt}^{VA} &= (\psi_j - 1)\Delta x_{jt}^{VA} + \frac{\Delta tfp_{jt}}{1 - C_{jM} - C_{jV}} \\ &\quad + \frac{(\psi_j - 1)}{1 - C_{jM} - C_{jV}} \left(C_{jV}\Delta v_{jt} + C_{jM}\Delta m_{jt} \right),\end{aligned} \tag{C2}$$

by subtracting Δx_{jt}^{VA} from Δva_{jt}, the resulting measure will also depend on materials and energy use, unless there is constant returns. We also see that there will be an effect on the value-added Solow residual working via primary inputs growth through the implied $(\psi_j - 1)\Delta x_{jt}^{VA}$ term in Δtfp_{jt}^{VA}. Note, though, that this particular effect (but not the effect working through intermediate materials and energy growth) would vanish if we allowed for non-constant returns when computing TFP from value-added data.

Comparing expressions (C1) and (C2), it is easy to see that unless there are constant returns to scale, or a very special covariance structure across the different production factors, there will be a component in the correlation between (C1) and (C2) that is driven by input factor growth and not technology growth. To the extent that e.g. demand shocks are correlated with factor input growth, this type of shock will affect both the instrument as well as the instrumented variable, giving rise to a bias in the coefficient of labor productivity on wages.

C.1 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 $\Delta \widetilde{x}_{jt}^{C} = C_{jK}\Delta k_{jt} + C_{jV}\Delta v_{jt} + C_{jN}\Delta n_{jt} + C_{jM}\Delta m_{jt}$. The capital stock, K_{jt}, is computed using a variation of the perpetual inventory method which utilizes all the information we have available in the data.

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

$$K_{jt} = \max\left\{(1-\delta)K_{jt-1} + I_{jt}, BookValue_{jt}\right\}, \tag{C3}$$

where δ 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) and $BookValue_{jt}$ is the real book value of fixed tangible assets (computed using the same deflator as for investment) and

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

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

$$K_{jt-1} = \frac{K_{jt} - I_{jt}}{(1-\delta)}, \tag{C4}$$

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

Tables

Table 1: Returns to Scale Regression

Industry	RTS
Durables	0.986
	(0.194)
Non-Durables	0.882
	(0.224)
AR(2)	[0.210]
AR(3)	[0.886]
Hansen	[0.296]

Note: Sample 1991-1996 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 brackets.

Table 2: Summary Statistics

	All		Men	
	Mean	S.D.	Mean	S.D.
Wages:				
w_{ijt}	9.615	0.313	9.662	0.308
w_{ijt} (Within Match)	-	0.146	-	0.146
Productivity:				
lp_{jt}	6.835	0.667	6.865	0.677
lp_{jt} (Within Match)	-	0.155	-	0.157
lp^S_{jt}	-	0.049	-	0.050
lp^S_{jt} (Within Match)	-	0.036	-	0.037
lp^I_{jt}	-	0.669	-	0.680
lp^I_{jt} (Within Match)	-	0.130	-	0.131
TFP				
Φ^S_{jt}(Within Match)	-	0.022	-	0.022
Φ^I_{jt}(Within Match)	-	0.092	-	0.092
Worker characteristics:				
Age_{ijt}	39.8	11.8	39.7	11.9
Share of Men	0.794		1	
Share of HE	0.511		0.519	
Share of TE	0.122		0.127	
Share of Non-Immigrants	0.895		0.904	
Firm-Size	212.6		213.8	
Observations	472,555		374,975	

Note: The "Within match" rows shows the dispersion within a combination of person and firm. All statistics are weighted according to the number of employees.

Table 3: The Impact of Productivity on Individual Wages. OLS and IV Results

	(1)	(2)	(3)	(4)	(5)
Estimation Method:	OLS	IV	IV	IV	IV
Sample:	All	All	All	All	Males
lp_{jt}^{S}	0.027	0.123**	0.149**	0.149**	0.149**
	(0.022)	(0.036)	(0.037)	(0.038)	(0.041)
lp_{jt}^{I}	0.033**	0.032**	0.050**	0.051**	0.054**
	(0.008)	(0.011)	(0.010)	(0.010)	(0.011)
Firm FE	Yes	Yes	Yes	–	–
Worker FE	No	No	Yes	–	–
Worker Characteristics	No	No	Yes	Yes	Yes
Match Specific Fixed Effects	No	No	No	Yes	Yes
Observations	472,555	472,555	472,555	472,555	374,975
Firms	1,136	1,136	1,136	1,136	1,136
Kleibergen-Paap rk LM statistic	-	46.83	NA	44.52	39.94
P-value	-	0	NA	0	0
Worker*Firm Matches	-	-	-	107,086	82,702

Note: * (**) denotes significance at the 5 (1) percent level. Standard errors clustered on firms reported inside parentheses. All specifications include time effects and labor market tightness. Individual controls include age, age squared and age cubed (columns 3-5). K-P denotes the Kleibergen-Paap (2006) rk LM statistic for testing the null hypothesis that the equation is underidentified. P-value denotes the associated p-value for the test.

Table 4: First-Stage Regressions

	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Dependent Variable:	lp_{jt}^S	lp_{jt}^I	lp_{jt}^S	lp_{jt}^I
tfp_{jt}^S	0.854**	0.000	0.842**	-0.020
	(0.087)	(0.148)	(0.088)	(0.141)
tfp_{jt}^I	-0.000	0.846**	0.000	0.838**
	(0.008)	(0.031)	(0.009)	(0.031)
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
F-Stat($\Phi_{jt}^S = \Phi_{jt}^I = 0$)	49.00**	382.8**	46.82**	378.8**
Worker by Firm Matches	-	-	107,086	107,086

Note: * (**) denotes significance at the 5 (1) percent level. Standard errors clustered on firms reported inside parentheses. Regressions also include time effects and labor market tightness. Individual controls (columns 3-4) include age, age squared and age cubed. F denotes the F statistic for the excluded instruments.

Table 5: OLS and IV with Decreasing and Constant Returns to Scale

	(1)	(2)	(2)	(4)
Estimation Method:	OLS	IV	OLS	IV
Returns to scale:	Decreasing returns (Non-Durables)		Constant returns (Durables)	
lp_{jt}^{S}	0.027	0.140**	0.177**	0.169**
	(0.028)	(0.042)	(0.038)	(0.055)
lp_{jt}^{I}	0.033**	0.052**	0.050**	0.049**
	(0.007)	(0.012)	(0.011)	(0.017)
Worker Characteristics	Yes	Yes	Yes	Yes
Worker by Firm FE	Yes	Yes	Yes	Yes
Observations	286,907	286,907	185,648	185,648
Worker*Firm Matches	64,084	64,084	43,002	43,002
Firms	720	720	416	416
Kleibergen-Paap rk LM statistic	NA	42.89	NA	63.50
P-value	NA	0	NA	0

Note: * (**) denotes significance at the 5 (1) percent level. Standard errors clustered on firms reported inside parentheses. All specifications include time effects and labor market tightness. Worker characteristics include age, age squared and age cubed. K-P denotes the Kleibergen-Paap (2006) rk LM statistic for testing the null hypothesis that the equation is underidentified. P-value denotes the associated p-value for the test.

Table 6: Dynamic Effects

	(1)	(2)	(3)
Estimation method:	IV	IV	IV
lp^S_{jt}	0.149**	0.052	0.226
	(0.038)	(0.095)	(0.130)
lp^S_{jt-1}		0.104	0.198
		(0.088)	(0.211)
lp^S_{jt-2}			-0.122
			(0.147)
lp^I_{jt}	0.051**	0.035**	0.033*
	(0.010)	(0.012)	(0.014)
lp^I_{jt-1}		0.034**	0.032**
		(0.013)	(0.012)
lp^I_{jt-2}			0.026*
			(0.010)
Total Sector Effect	0.149**	0.157**	0.303**
s.e.	(0.038)	(0.045)	(0.110)
Total Idiosyncratic Effect	0.051**	0.068**	0.091**
s.e.	(0.010)	(0.015)	(0.020)
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*Firm Matches	107,086	99,473	93,316
Kleibergen-Paap rk LM statistic	44.52	25.79	4.775
P-value	0	0	0.029

Note: * (**) denotes significance at the 5 (1) percent level. Standard errors clustered on firms reported inside parentheses. All specifications include time effects and labor market tightness. Worker characteristics include age, age squared and age cubed. K-P denotes the Kleibergen-Paap (2006) rk LM statistic for testing the null hypothesis that the equation is underidentified. P-value denotes the associated p-value for the test.

Table 7: The Effects of Productivity on the Selection of Workers

	(1)	(2)	(3)	(4)
Estimation method:	IV	IV	IV	IV
Sample:	All	Less than High School	High School	Tertiary Education
lp^S_{jt}	-0.024	-0.044*	-0.001	-0.026
	(0.015)	(0.020)	(0.018)	(0.059)
lp^I_{jt}	-0.003	-0.001	-0.007	-0.008
	(0.003)	(0.004)	(0.004)	(0.009)
Firm FE	Yes	Yes	Yes	Yes
Worker Characteristics	Yes	Yes	Yes	Yes
Observations	417,870	161,954	207,236	48,675
Number of Firms	1,136	1,135	1,135	1,128
Kleibergen-Paap rk LM statistic	43.83	47.58	38.93	44.33
P-value	0	0	0	0

Note: The dependent variable is the person effect of the individual as extracted from a wage regression on person and establishment fixed effects, an age polynomial, and year dummies for the entire private sector during 1985-1989. * (**) denotes significance at the 5 (1) percent level. Standard errors clustered on firms reported inside parentheses. All specifications include time effects and labor market tightness. Worker characteristics include age, age squared and age cubed, a gender dummy, a high school dummy, a university dummy, immigration dummies by seven regions of origin. K-P denotes the Kleibergen-Paap (2006) rk LM statistic for testing the null hypothesis that the equation is underidentified. P-value denotes the associated p-value for the test.

Table 8: The impact of different deflators and output measures

	(1)	(2)	(3)
	IV	IV	IV
Prices (Productivity)	Firm-level	3-digit PPI	Firm-level
Prices (TFP)	Firm-level	3-digit PPI	Firm-level
Output Measure	Gross Output	Gross Output	Value-added
lp_{jt}^{S}	0.149**	0.112**	0.040*
	(0.038)	(0.035)	(0.017)
lp_{jt}^{I}	0.051**	0.092**	0.023**
	(0.010)	(0.016)	(0.005)
Worker characteristics	Yes	Yes	Yes
Match fixed effects	Yes	Yes	Yes
Observations	472,555	472,555	469,044
Number of Firms	1,136	1,136	1,136
Kleibergen-Paap	44.5	121.6	77.56
P-value	0	0	0
P-value $\left(lp_{jt}^{S}\right)$	–	0.285	0
P-value $\left(lp_{jt}^{I}\right)$	–	0.008	0

Note: * (**) denotes significance at the 5 (1) percent level. Standard errors clustered on firms reported inside parentheses. All specifications include time effects and labor market tightness. Worker characteristics include age, age squared and age cubed. K-P denotes the Kleibergen-Paap (2006) rk LM statistic for testing the null hypothesis that the equation is underidentified. P-value denotes the associated p-value for the test. P-value (lp_{jt}^{S}) denotes the associated p-value for a two-sided test of equality of coefficients in the case of sectoral productivity with respect to the coefficients shown in Column 1. P-value (lp_{jt}^{I}) denotes the associated p-value for a two-sided test of equality of coefficients in the case of idiosyncratic productivity with respect to the coefficients shown in Column 1.