Institute for Employment Research
The Research Institute of the Federal Employment Agency

IAB-Discussion Paper 7/2016
Articles on labour market issues

The productivity effects of worker mobility between heterogeneous firms

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ISSN 2195-2663

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Abstract

Several empirical studies find that worker inflows from more productive or otherwise superior firms increase hiring firms' productivity. We conduct a similar analysis for Germany, using a unique linked employer-employee data set, and ranking sending and hiring establishments by their median wage. We find that inflows from superior (higher-paying) establishments do not increase hiring establishments' productivity, but inflows from inferior establishments seem to. Further analyses suggest this effect is due to positive selectivity of such inflows from their sending establishments. Our findings can be interpreted as evidence of a reallocation process by which the best employees of lower-paying establishments become hired by higher-paying establishments. This process reflects the increasingly assortative pattern of worker mobility in Germany, to which our findings suggest a micro-foundation at the establishment and worker levels.

Zusammenfassung

Empirische Studien zeigen, dass Zugänge von Beschäftigten aus produktiveren Betrieben die Produktivität der einstellenden Betriebe steigern. Wir untersuchen diesen Zusammenhang für Deutschland anhand eines eigens generierten Linked Employer-Employee Datensatzes. Dabei ordnen wir Herkunfts- und Zielbetriebe von Betriebswechslern anhand ihres Medianlohns. Unsere Ergebnisse zeigen, dass Beschäftigtenzugänge aus höher entlohnenden Betrieben keine Wirkung auf die Produktivität der Zielbetriebe haben. Zugänge aus geringer entlohnenden Betrieben hingegen haben unseren Ergebnissen zufolge einen positiven Produktivitätseffekt. Weitere Analysen ergeben, dass dieser Effekt in einer Positivauswahl dieser Beschäftigten aus ihren Herkunftsbetrieben begründet liegt. Ein Teil der produktivsten Beschäftigten von Betrieben mit niedrigerem Lohnniveau wechselt also zu Betrieben mit höherem Lohnniveau. Dieser Prozess spiegelt ein bereits bekanntes Muster der Beschäftigtenmobilität in Deutschland wider, wonach sich hochbezahlte Beschäftigte zunehmend in hoch entlohnende Betriebe sortieren. Unsere Ergebnisse können daher als Mikro-Fundierung für dieses gesamtwirtschaftliche Muster dienen.

JEL classification: D24, J61, J62, R23

Keywords: Knowledge Spillovers, Labor Mobility, Plant-Level Productivity

Acknowledgements: We would like to thank Uwe Blien, Linda Borrs, Anette Haas, Frank Neffke, Michel Serafinelli, Nick Zubanov, Thomas Zwick, conference participants at the Scottish Economic Society 2015 Annual Conference, the Spring Meeting of Young Economists 2015, the SOLE/EALE World Meetings 2015, ERSA Annual Conference 2015, seminar participants at the Center for International Development (Harvard), Helmut Schmidt University, University of Würzburg, and IAB for helpful comments and advice. We thank Johannes Schäffler for sharing his record linkage routine with us and Steffen Mueller for sharing his capital stock calculation routine. Thanks also to Steffen Kaimer, Pirmin Schanne, and Sven Uthmann for excellent research assistance. Stockinger acknowledges financial support from the joint graduate program of IAB and the University of Erlangen-Nuremberg (GradAB). All remaining errors are ours.

1 Introduction

Can firms get more productive by hiring particular workers? If so, who are these workers, and what makes them particularly valuable? These are the questions we address in this paper. A growing literature has come to the tacit consensus that worker inflows to a firm increase productivity if they come from – in some sense – superior firms (notably, Stoyanov/Zubanov 2012, 2014; Serafinelli 2013; and Balsvik 2011). Broadly speaking, superiority here is defined as higher productivity, but partly also by a higher wage level. The results from this literature are interpreted as evidence of spillover effects between heterogeneous firms, as workers moving from superior to inferior firms transfer their acquired knowledge from a superior origin to the hiring firm. Thus, firms can get more productive by hiring from superior firms and accessing these workers' superior experience.

However, this finding may not be obtained if workers moving from "better" to "worse" firms are not randomly selected. Indeed, as they move to a potentially less attractive employer, they could be negatively selected from their sending firms. In contrast, movers from inferior to superior firms could be positively selected. As a novelty to the literature on productivity effects of worker inflows, thus, we control for this kind of selectivity. To do so, we consider the relative wage position of moving workers within their sending establishments. Using this measure, we study whether the heterogeneity of sending and hiring establishments alone accounts for potential productivity effects of worker inflows, or whether workers' relative wage position in the sending establishment also plays a part.

In contrast to previous studies, our findings for Germany suggest that inflows from inferior firms increase hiring firms' productivity. At the same time, these inflows are positively selected, that is, they have held above-average wage positions at their sending firms. Once we control for this selectivity, the inflows' positive effect on hiring firms' productivity disappears. Descriptive findings indicate a simple rationale for the observed pattern: Upward-moving workers, who are individually highly productive, simply may not be able to receive an adequate wage with their initial (inferior) employer. Thus, their only possibility to correct the mismatch is moving to a superior firm. We cannot confirm previous' studies result that inflows from superior firms positively affect productivity. We can, however, rationalize their neutral effect as stemming from a neutral sending-firm wage position.

We thus contribute to the literature on firm-level productivity effects of worker inflows, and more broadly to the broad research area of labor mobility as a channel of spillover effects at the firm level. We tackle endogeneity and sensitivity issues by various econometric methods, as we cannot rely on quasi-experimental or otherwise randomized variation in our explanatory variables. To the best of our knowledge, our study is the first of its kind for Germany. Contrasting previous studies' findings for other countries, our results also point to the importance of labor market structures and institutions in shaping mobility processes, although it is beyond the scope of this study to address these directly. Finally, our study complements recent empirical research on the rise of (Western) German wage heterogeneity by demonstrating, at the micro level, a process of worker mobility between heterogeneous firms that may be at the root of increasing firm-level wage inequality.

This paper proceeds as follows. The next section reviews theoretical considerations and previous empirical work on spillovers through worker mobility between firms. Section 3 presents the model framework we employ to detect worker inflows' effects on firm productivity. In Sections 4 and 5, the empirical model and descriptive statistics are presented. In Section 6 we discuss the econometric implementation of our model and estimation results. In Section 7, we draw conclusions.

2 Theoretical concepts and previous evidence

A starting point in the theoretical literature about worker mobility as a channel of firm-level productivity effects is the literature on knowledge spillovers, where it is widely acknowledged that workers can act as carriers of knowledge. The fact that not all knowledge can be codified (notably, in the form of patents), but that its exchange and implementation usually require personal interaction ("tacitness of knowledge"), has spurred a rich literature on localized knowledge spillovers, see e. g. Breschi and Lissoni (2001, 2009), Rosenthal and Strange (2004), Power and Lundmark (2004), and Abel et al. (2012). Given the tacitness of knowledge, the most concrete and arguably most effective channel of knowledge spillovers is the mobility of workers, who carry knowledge from one firm to another. According to the studies of Almeida and Kogut (1999) and Song et al. (2003), it is the clustering of skilled workers, combined with a high degree of mobility, that accounts for the localization of knowledge spillovers in the semiconductor industry in Silicon Valley. Thus, knowledge spillovers are a strongly localized phenomenon exactly because labor mobility is spatially concentrated.

Following the pioneer studies on Silicon Valley, a growing number of studies have considered worker mobility as a channel of knowledge spillovers, building on the idea that any (skilled) worker is a potential carrier of knowledge. A theoretical model including worker flows as the channel of spillovers has been developed by Dasgupta (2012), who seeks to explain knowledge diffusion processes through worker flows from multinational enterprises (MNEs) to host-country domestic firms. The basic proposition of this model and recent empirical studies is that there is potential for spillovers when workers move from "superior" firms, which should possess a great stock of knowledge and technological capacities, to "inferior" firms which benefit from the additional knowledge thus received. These empirical studies include Stoyanov and Zubanov (2012, 2014), Serafinelli (2013), and Maliranta et al. (2009), who also find that firms do not fully compensate incoming workers (knowledge carriers) for their productivity effects, implying that worker inflows indeed are a channel of positive externalities to firms.

Thus, previous studies emphasize the role of firm heterogeneity, arguing that the occurrence and extent of spillovers through worker mobility depend on the characteristics of sending firms. A specific branch of literature focuses on knowledge spillovers between multinational enterprises (MNEs) and domestic firms, with the distinction between multinational and domestic firms being a classical dividing line between heterogeneous firms (Melitz 2003). The underlying assumption is that domestic firms receiving worker inflows from MNEs thus receive new knowledge on technology, workplace practices, or markets, since MNEs generally work at a higher scale and use more advanced technology than Non-MNEs (for a theoretical argument, see also Helpman et al. (2004)). One of the first studies in this area is Görg and Strobl (2005), who find that Ghanaian manufacturing firms whose executives have previously worked for MNEs achieve higher productivity levels than their domestic competitors. Balsvik (2011) finds evidence of spillovers from MNEs in the Norwegian manufacturing sector, as firms with high shares of workers with MNE experience achieve higher productivity levels. Similarly, Poole (2013) finds evidence of spillovers from worker flows between MNEs and domestic firms in Brazil, as identified by the wages of the receiving firms' incumbent workers.

The productivity gap between sending and receiving firms and its implications for knowledge spillovers have also been studied more generally (beyond the multinational-domestic context). Stoyanov and Zubanov (2012, 2014) find that labor productivity and total factor productivity in Danish manufacturing firms are positively associated with the inflow of workers from more productive manufacturing firms, and the relationship gets stronger as the productivity gap between sending and hiring firms widens. The effect is small but robust (hiring an average quantity of knowledge carriers with average quality, as compared to hiring none, corresponds to a productivity gain of 0.35 percent). Taking several means to reduce endogeneity bias, Stoyanov and Zubanov (2012, 2014) thus identify the upper bound of a potentially causal effect of hiring employees from more productive firms on hiring firms' productivity. However, the effect is statistically not significant for (otherwise equal) inflows from less productive firms. Closely related to Stoyanov and Zubanov's (2012, 2014) productivity gap approach, Serafinelli (2013) studies the impact of worker inflows from high-paying firms (a proxy for highly productive firms) on receiving (non-high-paying) firms' productivity, finding a positive effect. This result, too, survives a number of measures against reverse causality bias, e. g. using local high-wage-firm downsizings as an instrument for the number of inflows from such firms. Analogous to Stoyanov and Zubanov's (2012, 2014) results, it is found that inflows from non-high-paying firms do not have a similar effect.

A number of related studies indicate qualitatively similar patterns – having hired workers with particularly valuable experience is typically positively associated with hiring firms' productivity, probably reflecting a positive externality to hiring firms. To mention just a selection, Møen (2005) finds that Norwegian manufacturers partly internalize knowledge spillovers from separating R&D workers by setting relatively steep tenure-earnings profiles for these workers. Kaiser et al. (2008) analyze Danish firms' innovation, finding that the inflow of R&D workers is strongly related to the number of a firm's patent applications. Maliranta et al. (2009) come to similar conclusions concerning hiring firms' Non-R&D activities, i. e. firms benefit from inflows' earlier R&D experience in terms of their Non-R&D productivity. In sum, these studies substantiate the claim that firms can benefit from other, structurally superior firms' productive and innovative activities by hiring workers previously employed there.

While the evidence on the positive effects of superior-firm inflows is growing, and the interpretation of these effects as knowledge spillovers is compelling, it is neither theoretically nor empirically straightforward to expect such an effect. A theoretical reason not to expect positive effects from such 'downward' inflows is that they might be negatively selected from their sending firms. In some cases, hiring firms might actively attract such workers, precisely because they expect them to bring new, advanced knowledge to the firm. However, that might require them to offer unusually high wages (compared to the firm's average wage level) to the worker, in order to outbid the sending firm. In general, inferior firms may not be able to set such wage incentives, and so employees at superior firms (let alone their better employees) might be better off staying with their current employer. In contrast, since moving from inferior to superior firms is likely to be beneficial to the moving worker's wage, superior firms should be able to select the best employees from inferior firms, inducing a positive selection of worker flows in the upward direction.

Empirical evidence pointing in this direction has been provided, e. g., by Martins (2011), who shows that worker flows from domestic (inferior) to foreign (superior) firms in Portugal typically have been the better-paid employees in their sending establishment. Accordingly, the argument continues, productivity spillovers may arise from inferior to superior firms, rather than in the opposite direction. In the context of Germany (and other countries), furthermore, related empirical findings also suggest that "upward" worker flows may boost destination firms' productivity. As documented by Card et al. (2013), wage inequality in Western Germany has increased substantially since the 1980s, one of the main reasons being an increasingly positive sorting pattern between workers and firms, that is, high-wage workers increasingly sort into high-wage firms. Therefore, the average high-wage worker (who should be relatively productive) should be moving up, rather than down, in terms of the firm's wage level. This pattern suggests that upward worker flows (from inferior to superior firms) are positively selected, and movers in the opposite direction, possibly negatively selected. Therefore, any study on the productivity effect of inflows from superior firms (versus inflows from inferior firms) has to take into account their potential selectivity, a point given great emphasis in Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013). Against this background, it seems uncertain whether the positive productivity effects from downward-mobile workers found in these studies prevail in Germany, and the answer is likely to depend on the precise nature of worker inflows' selection.

3 A model of worker inflows' productivity effects

The above-cited studies, Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013) in particular, seek to explain firms' output and value added by the quality of worker inflows, in terms of whether their sending firm is superior or inferior to the hiring firm. In some sense, Stoyanov and Zubanov's (2012, 2014) "productivity gap" model generalizes previous approaches in the literature, by actually using firm productivity to define which sending firms are superior and which inferior, whereas earlier studies have focused on MNEs or R&D-conducting firms, both of which were regarded as superior. Serafinelli's (2013) approach is equally general, but ranks sending and hiring firms by firm-fixed wage effects instead of productivity levels.

Stoyanov and Zubanov (2012, 2014) refer to inflows from more productive firms "spillover potentials" (SPs), since it is these workers who possess superior knowledge from their firms of origin. For the sake of brevity, we will use the same term for inflows from superior establishments, while referring to all other *inflows* (those from inferior establishments) as Non-SPs. Serafinelli (2013) takes a slightly different approach, first dividing *all* firms into high-wage firms (HWFs) and Non-HFWs, according to their fixed wage effect. This effect is obtained from a regression of individual wages including person and firm fixed effects, as first proposed by Abowd, Kramarz, and Margolis (1999) (henceforth AKM) and implemented by Abowd, Creecy, and Kramarz (2002). All firms in the top third of the firm fixed effect distribution are classified as HWFs, the remaining two thirds as Non-HWFs. In a second step, Non-HWFs are analyzed with respect to worker inflow effects on productivity. The estimation approach common to all of the just-cited studies is to regress output (or value added) on separate measures of inflows from superior and inferior firms, controlling for capital, labor, and controls for firms', incumbent workers', and inflows' characteristics.

We employ an estimation framework building on Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013). As shown by Stoyanov and Zubanov (2014), a simple production function framework can be used to estimate the effect of worker inflows on hiring firms' productivity. Therein, labor is modeled as a heterogeneous input consisting of two groups: Inflows from superior firms (SPs) and all other workers (Non-SPs in our terminology plus incumbent workers), where SPs are supposed to be individually more productive due to their superior experience. Note that we may just as well hypothesize Non-SPs to be more productive than the rest; yet we follow Stoyanov and Zubanov's (2014) notation to simplify the exposition. We now briefly sketch their production function framework, starting from a hiring firm's production function in Cobb-Douglas form,

$$Y_{it} = A_{it} K_{it}^{\beta_K} L_{it}^{\beta_L},$$

where Y_{it} is the value added of firm i in year t. Labor in efficiency units is defined as

$$L_{it} = L_{it}^{rest} + \varphi H_{i,t-1}^{SP} = \left(L_{it}^{rest} + H_{i,t-1}^{SP}\right)(1 - s_{it} + s_{it}\varphi) = \tilde{L}_{it}[1 + s_{it}(\varphi - 1)],$$

with L_{it} as effective labor input, $H_{i,t-1}^{SP}$ as the number of SPs who arrived at t-1 (hires from more productive firms), $\tilde{L}_{it}$ as the total number of workers $(L_{it}^{rest} + H_{i,t-1}^{SP})$,

$s_{it} = \frac{H_{i,t-1}^{SP}}{\tilde{L}_{it}}$ as the share of SPs in total employment, and the productivity advantage of SPs over other workers as $\varphi > 1$. Inserting the expression for effective labor input into the production function yields

$$Y_{it} = A_{it} K_{it}^{\beta_K} \tilde{L}_{it}^{\beta_L} [1 + s_{it}(\varphi - 1)]^{\beta_L},$$

indicating that the labor productivity effect of hiring SPs is described by the factor

$$1 + s_{it}(\varphi - 1)$$

and their effect on total factor productivity is

$$[1 + s_{it}(\varphi - 1)]^{\beta_L}.$$

Since $s_{it}(\varphi - 1)$ is close to 0 for reasonable range of s_{it} and φ, one can use the approximation

$$\ln[1 + s_{it}(\varphi - 1)] \approx s_{it}(\varphi - 1)$$

to infer the production function in logs (indicated by lower-case letters):

$$y_{it} = a_{it} + \beta_k k_{it} + \beta_l \tilde{l}_{it} + \beta_l(\varphi - 1)s_{it}. \tag{1}$$

This equation states that firm productivity depends positively on the share of SPs within all of the firm's employees. We may simplify this expression to

$$y_{it} = a_{it} + \beta_k k_{it} + \beta_l \tilde{l}_{it} + \vartheta s_{it}, \tag{2}$$

where ϑ replaces the combined effect of labor productivity and SPs' productivity advantage over the firm's other employees. According to this reduced-form model, thus, a firm's productivity depends positively on how many SPs it has hired in the previous period, expressed as a share within all of the firm's employees.

However, as pointed out by both by Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013), inflows from superior firms might not be randomly selected from their sending firms. Considering that moving from a highly productive (or high-paying) firm to a less productive one might yield a negative outcome for the moving worker (a small or even negative wage change), workers moving in this direction could be negatively selected. The cited studies account for such a possible "lemons bias" by including accurate individual-level control variables on worker inflows. Thus, SPs and Non-SPs, or HWF and Non-HWF inflows, are supposedly made equal in all individual-level aspects independent of firm-level characteristics, and differ only with respect to the relative productivity (or wage) level of their sending firm. Generally (that is, concerning both SPs and Non-SPs), our analysis focuses on skilled workers, who possess the potential to carry substantial productive knowledge. Also, we choose to focus on job moves without long interruptions (periods of non-employment between jobs), during which the skills and knowledge acquired in the sending firm may depreciate. In the following, we present our empirical implementation of the above-sketched model approach, devoting particular attention to our distinction of superior and inferior firms, and to the problem of worker inflows' potential selectivity.

4 Empirical implementation

4.1 Data

We construct a linked employer-employee data set based on German data provided by the Institute for Employment Research (IAB). Individual-level data are obtained from the Integrated Employment Biographies (IEB), establishment-level data from the Establishment History Panel (BHP) and the IAB Establishment Panel. The two former databases are 100 percent records of employment subject to social security contribution, while the IAB Establishment Panel is the largest establishment survey in Germany. The IEB contain precise information about individuals' labor market biographies. They are based on different administrative sources and contain daily information on every individual in Germany who is either in employment subject to social security, registered unemployed, or participating in measures of active labor market policy, excluding only civil servants and the self-employed. A detailed description of the IEB's construction is given in vom Berge et al. (2013). The assignment of workers to establishments, as well as crucial variables such as begin and end dates of employment spells, are highly accurate and reliable as they are drawn from the official employment statistics of the Federal Employment Agency, which serves as the basis to compute contributions to social security. In our data, employers are not firms in any legal sense, but establishments, that is, spatially fixed production units which may be part of multi-establishment firms. While the lack of firm-level data (such as balance sheet information) does set limits to our analysis, we think that establishments are well suited for the analysis of worker inflows and productivity, as workers can be assigned unambiguously to establishments (unlike firms), allowing us to conduct a relatively fine-grained analysis of productivity.

We count an individual worker as an inflow in establishment i if he or she was employed in another plant j before and both employment spells are at least seven days long. Since we consider newly hired workers as knowledge carriers, we require them to satisfy several conditions. Most importantly, we disregard all inflows of unqualified workers, requiring inflows to have a tertiary education or at least hold a vocational degree. We exclude all inflows employed as apprentices, interns, or "marginal" employees, either in the sending or hiring establishment.[1] Moreover, only incoming workers between the ages of 15 and 65, the official retirement age, are

[1] Marginal employment is defined as employment not subject to social security contribution, with the monthly wage not exceeding (currently) 450 Euros, see Section 8, Subsec. 1, No. 1, of the German Social Code IV (SGB IV).

included.[2] Furthermore, we choose to allow a maximum gap of half a year (182 days) between two consecutive employment spells. In case of a period of unemployment between two employment spells, it must not be longer than three months. By German standards, these transition periods should be generous enough to retain most if not all of the relevant worker transitions, but rule out overly long employment gaps during which workers' recently acquired experience (interpreted as human capital) may already begin to depreciate.

The key criterion for the identification of inflows from other establishments is a change in the establishment identification number (establishment ID). In this context three issues, which have plagued previous analyses of inter-firm worker flows using German employment data, have to be discussed. First, a worker could be employed by two employers at the same time. For each point in time (i. e. each day), we assign each worker to a single employer, using the highest daily wage as the criterion of assignment. Second, as Hethey and Schmieder (2010) point out, establishment IDs appear and disappear not only in case of plant creation and closure, but also in case of spin-offs, acquisitions, restructurings, and changes of owner. In our context, this means that we must not consider flows between establishment IDs to be real labor flows if all or a substantial fraction of incoming workers come from the same establishment ID, as this might reflect a spin-off, restructuring, acquisition, or change of owner. For each establishment and year, we detect and remove clustered outflows from an establishment ID that, according to Hethey and Schmieder (2010), are probably incidents of an owner change, acquisition, or similar events. Third, we must ensure that establishments between which we observe worker flows are not part of the same firm. We make use of a Stata routine developed by Schäffler (2014) to estimate which establishments probably belong to the same firm, and disregard worker flows between such establishments. This procedure is based on establishments' names and legal form (for details, see Schäffler 2014).[3] Thus, we ensure that the worker flows entering our analysis are not spurious in the sense that they do not represent worker mobility between two economically independent (potentially competing) units of production.

Since the IEB contain no information on establishment-level variables like value added or capital, we draw these data from the IAB Establishment Panel, an unbalanced panel survey of German establishments, of which we use the waves 2003-2011 (see Fischer et al. 2009) for more information on the Establishment Panel). For details on the linking of employer and employee data, see Heining et al. (2013). In line with most of the previous literature, we only analyze productivity effects for (hiring) establishments in the manufacturing sector, which we define as the range of

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[2] In fact, the youngest inflow we observe is 19 years old, as it is hardly possible to obtain a vocational degree at a younger age.

[3] We thank Steffen Kaimer (IAB) for running this procedure, which requires the use of non-anonymized establishment data, on our behalf.

NACE[4] Rev. 1.1 (or, equivalently, ISIC[5] Rev. 3.1) divisions 15 through 41.[6] The interpretation of revenues (proxy for output) and intermediate inputs, and therefore value added, is more consistent when focusing on this sector.[7] To obtain the capital stock, we use the modified perpetual inventory method (PIM) by Müller (2008), deducing capital from net investment, which is surveyed in the Establishment Panel. The method uses investment data to infer the capital stock and industry-level depreciation rates for different categories of investment goods. We reckon that the method is adequate for the manufacturing sector, where the quality and depreciation of capital should be comparable within each of the different manufacturing industries. As emphasized by Ehrl (2013), whose procedure we also employ, the PIM must be further corrected for restructuring events such as insourcing, closure, sell-off, and spin-off of parts of the establishment.

4.2 Identifying superior and inferior establishments

The key to deriving our estimation model is to identify worker inflows to each establishment in our sample and to determine for each inflow whether s/he comes from a superior or inferior establishment. Unlike Stoyanov and Zubanov (2012, 2014), we do not have data on sending establishments' output, sales, or inputs – we only have these data (from the IAB Establishment Panel) for the sample of (potential) hiring establishments (some establishments, obviously, do not report any hires, but are still included in our analysis of productivity effects). Similar to Serafinelli (2013), thus, we consider ranking establishments using establishment fixed wage effects. We obtain these fixed effects from OLS wage regressions, separate for each of the relevant years, of all regular full-time employees (excluding apprentices and marginal employees) in any of the sending or hiring establishments at the reference date June 30. By performing the regression separately for each of the years, we identify the establishment fixed effect not from variation across time, but across workers. More explicitly, we estimate for each year:

[4] Nomenclature Générale des Activités Economiques dans I'Union Europèene.

[5] International Standard Industrial Classification.

[6] Within the period from which we draw data, the industry classification scheme has changed several times, notably, from the Classification of Industries 1993 (WZ93) to WZ03 in 2003 and from WZ03 to WZ08 in 2008. We deal with this problem by merging the industry code assigned by Eberle et al. (2011), who used intertemporal imputation of industry codes within establishments (establishments virtually never change industries) and a crosswalk between different classifications.

[7] A problem of the IAB Establishment Panel is that the entity referred to as the establishment may differ between the administrative records and the survey. To address this problem, we compare the total numbers of employees reported in the administrative register and the survey. We therefore drop establishment observations for which the reported numbers of regular employees (subject to social security, excluding marginal employees) deviate from each other by an implausibly large amount.

$$\ln w_{pi} = \beta_0 + \beta_1 male_p + \beta_2 age_p + \beta_3 age_p^2 + \sum_{l=1}^{L} \beta_{4,l} occ_{stat_{l,p,i}} + \sum_{m=1}^{M} \beta_{5,m} qual_{mp} + \sum_{n=1}^{N} \beta_{6,n} d\_occ2_{n,p,i} + \theta_i + \epsilon_{pi}, \quad (3)$$

where $\ln w_{pi}$ is the log wage of worker p working at establishment i, $occ\_stat_{l,p,i}$ is a categorical variable indicating the occupational status of worker p in that particular job at plant i (e. g., blue-collar vs. white-collar, which can be related to different wage groups defined in collective agreements), $qual_{mp}$ is a categorical variable of worker p's qualification level, and $d\_occ2_{n,p,i}$ is a two-digit occupation dummy. Wages, which are censored at the social security contribution limit (censoring concerns some 15 percent of employees), are imputed for censored observations adapting a modified version of the procedure proposed by Gartner (2005).[8] Importantly, the results suggest that some 70 percent of unexplained wage variance is due to establishment fixed effects, indicating the importance of establishments for the determination of wages (see the estimation results for the first (2000) and last year (2010) in Appendix Table A 1). This finding is perfectly in line with empirical results for Denmark (for which Stoyanov and Zubanov conduct their analyses), despite marked structural differences between both countries' labor markets (see Christensen et al. 2005).

To be used as a criterion for ranking pairs of sending and hiring establishments, the $\hat{\theta}$ are regressed on a set of industry dummies at the three-digit level, analogous to Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013), yielding a corrected establishment fixed effect $\widehat{\theta'}$. This correction accounts for systematic productivity differences, e.g. due to industry-specific technologies, that we do not want to determine the ranking between pairs of establishments.

A simpler, readily available measure to rank sending and hiring establishments is their median wage. We merge the establishment median wage (computed only for full-time workers) from the BHP. We take its logarithm and, as above, clear it of 3-digit industry fixed effect, using the obtained measure as an alternative criterion to rank sending and hiring establishments. To assess both measures, which we want to reflect establishments' productivity, we compare it to direct measures of productivity, where available. We have information on value added and capital from the IAB Establishment Panel for all sample establishments (not including sending establishments, cf. above), so we assess the quality of $\hat{\theta}$ and the log median wage as measures of firm quality for these establishments. We compare both measures to TFP and log value added per worker as direct measures of establishment productivity, by which we would prefer to rank all sending and hiring establishments if we

8 Additional to the covariates in (3), in the imputation we use region and industry fixed effects, the mean non-censored log wage in the establishment and year, and the share of censored worker observations in the establishment and year. Rather than including gender dummies, we run the imputation separately for four cells, dividing the population not only between women and men but also between Eastern and Western Germany.

could. Table 1 presents these correlations. While far from a perfect fit, $\hat{\theta}$ is fairly correlated with labor productivity (log value added per worker). The correlation with TFP, obtained as the residual from a simple OLS regression of value added on capital and labor (all in logs), is rather low at about 0.28. This may be due to the measurement of capital, which we obtain using investment data and the perpetual inventory method, implying that also TFP is measured with some error. Comparing both alternative ranking criteria, the establishment median wage reflects productivity better than the establishment fixed effect, albeit by a small margin. We thus use both measures to rank sending and hiring establishments, to assess the robustness of our approach.

Table 1
Correlations between productivity and establishment ranking criteria

Correlations	TFP	log value added per worker	establishment fixed effect	log estab. median wage
TFP	1.000			
log value added per worker	0.889	1.000		
establishment fixed effect	0.277	0.528	1.000	
log estab. median wage	0.317	0.553	0.918	1.000

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

One might also consider using an establishment fixed wage effect from an AKM-style regression to rank sending and hiring establishments. Serafinelli (2013) uses such an effect to divide sending firms into high-wage and non-high-wage firms. An equivalent effect (the “CHK establishment effect”) has already been computed for German establishments by Card, Heining, and Kline (2015), and it is available for a large fraction of our sample. However, we still prefer $\hat{\theta}$ and the log median wage, as the CHK establishment effect is necessarily time-invariant across most of our observation period (it is constructed for several eight-year intervals), since it is derived from worker movements across establishments. Therefore, its correlations with direct productivity measures are substantially lower (at .13 for TFP and .33 for log value added per worker).

4.3 Sample

Our final estimation sample contains 1,791 manufacturing establishments (4,233 observations) and ranges over the years 2002 to 2007, where we have up to six observations per establishment. Grouping establishment observations by whether they have any inflows, any SP inflows, or any Non-SP inflows, yields the total numbers displayed in Table 2: Half of all establishment observations in our estimation sample have a positive number of worker inflows who satisfy all our criteria (qualified, full-time, etc.). Among these, only 29 percent have at least one inflow from a superior (higher-paying) establishment, in line with the intuition that it may be hard for low-wage employers to attract such workers. In contrast, nearly three in four hiring establishment have at least one Non-SP inflow.

Adding up inflows and incumbent workers, the sample represents 884,595 workers; therein (due to data cleaning), 14,976 workers are counted as inflows, 43 percent of which (6,441) are classified as SPs. The sample excludes obvious outliers in terms of our central model variables, notably regarding the number of inflows. Furthermore, observations with missing values in any variable used for estimation are excluded. Establishments with less than five full-time equivalent employees are also excluded.

Table 2
Number of establishment observations by number of worker inflows

All sample establishments			Establishments with >0 inflows		
	Freq.	Percent		Freq.	Percent
>0 inflows	2,108	49.80	>0 Non-SPs	1,508	71.54
No inflows	2,125	50.20	>0 SPs	600	28.46
Total	4,233	100.00	Total	2,108	100.00

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

5 Descriptive analysis

5.1 Establishments

Table 3 summarizes establishment characteristics. It is worth noting that half our sample establishments are located in Eastern Germany, far above their share in the actual establishment population. This disproportion is due to the sampling design of the Establishment Panel, and we will account for it by running separate regressions for East and West. A potentially worrisome point in this context is that worker flows between East and West may be asymmetrically Westbound, due to the Western regions' higher productivity and wage levels. Yet this is not the case: Over 90 percent of flows change employers within the same part of the country, and East-to-West moves are no more frequent than moves in the reverse direction.

Table 3
Establishment characteristics

	log value added	log capital	log labor	Eastern dummy	median wage
Mean	15.099	14.913	4.104	0.499	81.473
SD	1.849	2.238	1.454	0.500	27.703
Min	9.483	7.346	1.609	0.000	15.165
Max	21.588	22.492	9.723	1.000	184.977
Means by subgroup:					
>0 inflows	16.147	16.075	4.968	0.406	91.542
No inflows	14.059	13.761	3.246	0.592	71.484
>0 Non-SPs	16.541	16.448	5.283	0.352	96.945
>0 SPs	15.155	15.136	4.177	0.540	77.963

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

The lower panel of Table 3 separates establishments by whether they had any hiring, zero hiring, hiring of SPs (spillover potentials, i. e. inflows from higher-paying establishments), or hiring of Non-SPs (inflows from lower-paying establishments). Clearly and unsurprisingly, establishments with a positive number of hires are larger and have higher value added and capital levels than non-hiring establishments. Among those which hire any workers, those hiring SPs are slightly smaller and have lower value added and capital levels than those hiring at least one Non-SP worker.[9] This was to be expected: By definition, hiring SPs means hiring from more productive establishments; thus, the larger and more productive an establishment, the less likely it is for a given worker inflow to be an SP.

Table 4 summarizes employment characteristics of our establishment sample, again with the focus on distinguishing hirers, non-hirers, and hirers of SPs, respectively Non-SPs.[10] Reassuringly, hiring establishments (irrespective of SP or Non-SP hiring) have substantially higher employment growth rates than non-hirers. Other characteristics follow the same ordinal pattern, notably the share of high-qualified workers (those with an academic degree) and the mean age of the employees (where better firms seem to employ younger workers).

Table 4
Employment-related establishment characteristics

	empl. growth rate	share high-qual.	share male	mean age	share inflows
Mean	0.016	0.104	0.800	41.704	0.017
SD	0.129	0.090	0.149	2.874	0.021
Min	-0.732	0.000	0.000	17.000	0.000
Max	2.269	0.899	1.000	59.000	0.323
Means by subgroup:					
>0 inflows	0.024	0.109	0.809	41.529	0.019
No inflows	0.007	0.068	0.737	43.063	0.000
>0 Non-SPs	0.024	0.110	0.814	41.410	0.020
>0 SPs	0.026	0.101	0.764	42.453	0.014

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

5.2 Worker inflows and incumbent workers

In Table 5, we take a look at incumbent workers' and inflows' individual characteristics, also separating SPs and Non-SPs.[11] We find that inflows are more highly qualified than incumbents, yet they earn substantially lower wages (at the hiring estab-

[9] Descriptive statistics are based on the SP definition using the log establishment median wage, but almost unchanged if the establishment fixed wage effect is used instead (not reported).

[10] All statistics weighted by each establishment's full-time equivalent number of employees.

[11] Incumbent workers here are restricted by the same criteria as inflows (only qualified full-time employees).

lishment, i. e. after the job move), presumably to their lower age and tenure.[12] SPs have a much better skill structure than Non-SPs: The share of high-skilled SPs is roughly double that of high-skilled Non-SPs. This finding is intuitive as, by definition, SPs have been employed at a relatively high-paying establishment early on (potentially all their previous working life). Such employers likely have higher formal qualification requirements, thus the better skill profile compared to Non-SPs. Non-SP inflows are also younger than SPs. Younger workers' job moves have been found to respond more strongly to wage incentives (cf. Hunt 2006), which suggests that Non-SPs might be following a stronger wage incentive than SPs. Last, we consider AKM-style individual fixed wage effects to examine whether SPs and Non-SPs differ in their unobserved productivity. We merge the person fixed effect from the data provided by Card, Heining, and Kline (2015), where we find well over 90 percent of all inflow workers matched. As the person fixed effect is identified from worker movements across establishments within an eight year interval (here 2002-2009), for most of our inflows its value depends on the wage in the sending establishment as well as in the receiving establishment. Unsurprisingly, given the definition of SPs and Non-SPs, the former have a higher average person fixed effect.

Table 5
Worker characteristics

	Mean age	Share male	Share high-qualified	Mean daily wage (hiring estab.)	Person fixed effect (CHK)
Incumbent workers	41.674	0.844	0.138	123.175	--
All inflows	36.524	0.852	0.190	121.581	3.908
SPs	37.706	0.835	0.274	135.289	4.036
Non-SPs	35.631	0.865	0.127	111.316	3.798

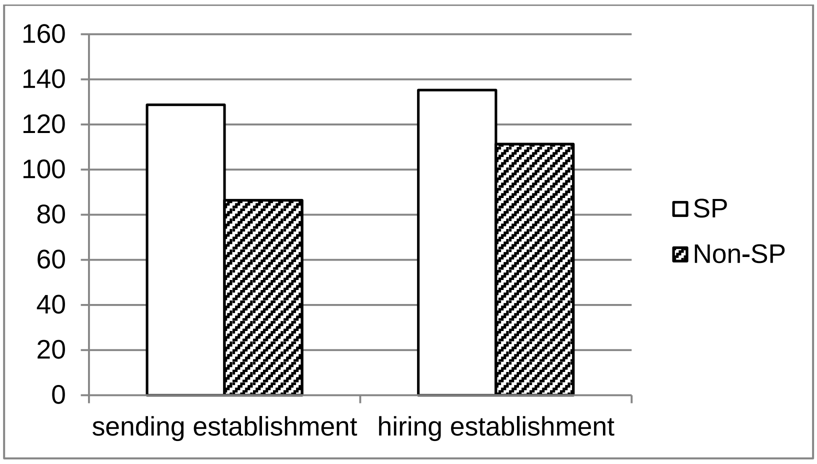
Note: Share of SPs in all inflows = 0.43

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Thus, looking at the wage profiles of SPs and Non-SPs (Figure 1), as we would expect given the person fixed effect, we find that SPs (white columns) have higher earnings levels both before and after the job move: Their mean sending-establishment daily wage (129 €) is well above Non-SPs' (shaded columns; 86 €), and is still some 20 percent higher at the hiring establishment, even though Non-SPs achieve tremendous wage gains (25 € on average) by their job move, almost four times as high as SPs' average wage change. Generally, thus, job movers appear to move out of opportunity rather than necessity, which suits our intention to focus on voluntary moves between jobs, rather than moves out of unemployment. It is not surprising that wage gains are larger for Non-SPs (movers to higher-paying establishments), but the magnitude of their gains appears striking, given that Non-SPs are less highly qualified and have lower long-run individual fixed effects than SPs.

[12] All monetary variables are deflated to 2010 levels using the consumer price index.

Figure 1
Mean daily wage, SPs and Non-SPs



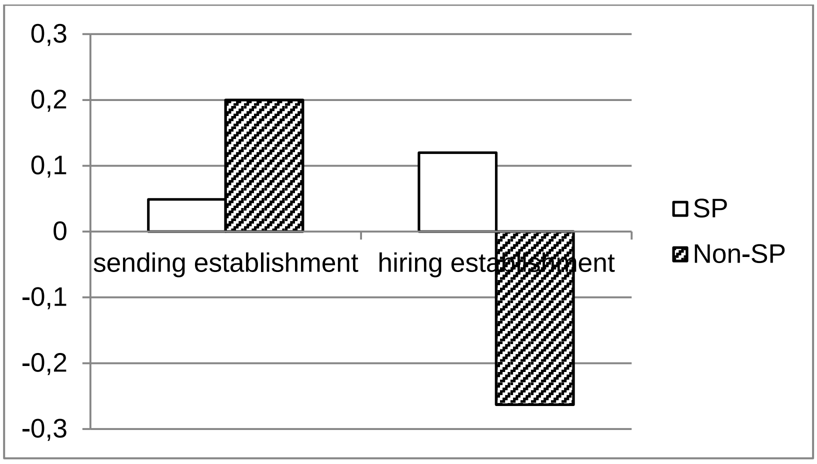
Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

It is not surprising that SPs generally earn higher wages, particularly at their sending establishments, as these are defined by paying relatively high wages. Comparing sending-establishment wages between SPs and Non-SPs is therefore trivial with respect to the between-establishment dimension. However, we have yet to consider the within-establishment dimension, to address the potential selectivity of within both groups of worker flows. We therefore compare the workers' rank (or relative quality) compared to their co-workers at the sending establishment. This metric, which we present in Figure 2, indicates whether the workers are positively or negatively selected from their sending establishment. We obtain the wage position of each moving worker, both for the sending (j) and hiring (i) establishment, from the wage regression used to obtain the establishment fixed effect (3). We normalize the residual $\hat{\epsilon}_{pi}$, to make it comparable across establishments:

$$\hat{\epsilon}'_{pi} = \frac{\hat{\epsilon}_{pi} - \overline{\hat{\epsilon}_{i}}}{SD(\hat{\epsilon}_{i})} = \frac{\hat{\epsilon}_{pi}}{SD(\hat{\epsilon}_{i})}$$

(the mean residual of establishment i's workers, $\overline{\hat{\epsilon}_{i}}$, is equal to zero because the wage regression includes a constant). The parameter $\hat{\epsilon}'_{pi}$ indicates each worker's wage position relative to co-workers with the same age, gender, qualification, occupation, and occupation status. Thus, positive values of $\hat{\epsilon}'_{pi}$ indicate above-average earnings in a thus defined cell, while negative values indicate the opposite. We can therefore determine for each worker inflow whether the worker is positively or negatively positioned within his or her establishment. According to our estimates of ϵ'_{pj}, Non-SPs are clearly positively selected among their peers in the sending establishment. This is not necessarily the case for SPs, who are only slightly positively selected from sending establishments, on average. Yet, Non-SPs do not move to a better relative wage position than SPs at their hiring establishments: Once arrived there, Non-SPs belong to the low-wage earners among their co-workers. In contrast, SPs generally move into positive wage positions. This leads to the higher CHK person effect for SPs compared to Non-SPs, as this effect is identified from wages both at the sending and the hiring establishment. Again, we checked whether there are obvious imbalances between Eastern and Western German establishments, but found very little difference.

Figure 2
Mean wage position, SPs and Non-SPs



Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Against this background, we do not have a clear expectation regarding our main research question – which worker inflows increase hiring establishments' productivity? On the one hand, SPs' generally higher wage levels and their experience at high-paying (and therefore, supposedly, highly productive) establishments suggests that SPs could be highly productive knowledge carriers, capable of increasing hiring establishments' productivity. On the other hand, Non-SPs are obviously a positive selection from their sending establishments, suggesting that Non-SPs could be even more likely than SPs to increase hiring establishments' productivity. In the following econometric analysis, thus, a central task is to control as thoroughly as possible for inflows' individual productivity, in order to identify their productivity effect solely in terms of their origin (superior for SPs, inferior for Non-SPs) and to explore the reasons underlying this effect.

6 Econometric analysis

6.1 Specification

We implement the approach of Stoyanov and Zubanov (2014), that is, we estimate the productivity effects of hiring SPs and Non-SPs within a production function framework, where SPs and Non-SPs, together with the establishment's incumbent employees, can be thought of as heterogeneous factor inputs. Practically, the employment share of both inflow groups is added in the production function as derived above. Our estimation equation can be formulated as follows:

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \vartheta_1 share\_SP_{it} + \vartheta_2 share\_Non\_SP_{it} + controls\_SP_{it} + controls\_Non\_SP_{it} + ESTAB_{it} + EMPL_{it} + \varepsilon_{it},$$

where y is log value added and k and l are log capital and log labor,[13] respectively.[14] The core explanatory variables are $share\_SP_{it}$ and $share\_Non\_SP_{it}$, the labor share of SPs and Non-SPs. Inflows are defined as all qualified full-time employees (satisfying a number of further criteria such as a plausible age range) who have arrived at some point between January 1st, t-1 and January 1st, t, and are still present at January 1st, t. Their classification into SPs and Non-SPs is based on the sending and hiring establishment's median wage (or fixed wage effect) at June 30, t-2, since this is the last year they have potentially entirely spent at their former employer.

If it does not matter to hiring firms' productivity whether their skilled worker inflows originate from more or less highly-paying (and therefore, approximately, more or less productive) establishments, we should obtain the same estimate for ϑ_1 and ϑ_2. If inflows do not matter for productivity at all we should obtain insignificant estimates for ϑ_1 as well as for ϑ_2. However, to ensure that we can interpret our estimates in this way, we have to ensure that SPs and Non-SPs do not differ in their individual productivity-relevant characteristics. We know from descriptive analysis that they do differ in terms of qualification, age, wages, and wage positions, both in their sending and hiring establishments. Thus, we include several control variables for inflows (vectors $controls\_SP_{it}$ and $controls\_Non\_SP_{it}$): the share of high-qualified workers[15] among all (Non-)SPs; their respective mean age and mean of age squared; and optionally, their mean unobserved fixed wage effect as estimated by Card et al. (2015), which comprises unobserved individual-level wage (and therefore, productivity) determinants. These controls are analogous to those used in Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013). As argued above, an important characteristic of Non-SPs is their strongly positive selection from sending establishments. Since hiring Non-SPs may increase hiring establishments' productivity for precisely this reason, we optionally include the mean wage position of SPs and Non-SPs in their sending establishments.

The control-variables vector $ESTAB_{it}$ includes categorical variables indicating whether the establishment is part of a larger enterprise, its legal form, the (self-reported) state of technical equipment, a dummy indicating young establishments (less than ten years old), and the share of exports in total revenues. Since these variables are almost entirely time-invariant, we drop them from all specifications based purely on within-establishment variance, without thereby affecting the results. $EMPL_{it}$ is the vector of employment structure controls, containing the share of high-

[13] In measuring labor, we approximate full-time equivalents by applying the standard weights of .3 and .6, respectively, to workers with less than 18 hours per week, and those with 18 or more weekly work hours but less than full-time (we do not observe work hours more precisely).

[14] In most specifications, we include two lags of the dependent variable to account for autocorrelation.

[15] Holders of a university or university of applied sciences degree.

qualified employees (holding a university or university of applied sciences degree), the mean age, and the share of males among all employees.

Concerning the estimation of establishment-level production functions, a fundamental problem is that inputs' coefficients are estimated with bias in the Pooled OLS case since there can be omitted idiosyncratic productivity shocks and reverse causality, i. e. a direct influence of expected future productivity on inputs (for a very comprehensive and detailed discussion, see Eberhardt and Helmers 2010). In our context, if we find a positive correlation between establishments' productivity and their hiring of certain workers, this might mean either that the worker inflows increase productivity due to these workers' individual characteristics, or that highly productive establishments attract these workers because they anticipate their positive productivity path. The two main approaches to minimize this bias are, first, "structural" (control function) approaches trying to model unobserved idiosyncratic productivity determinants explicitly (Olley and Pakes 1996; Levinsohn and Petrin 2003; Ackerberg et al. 2006; Wooldridge 2009), and second, dynamic panel data (DPD) approaches which use internal instruments in panel data sets (Arellano and Bond 1991; Blundell and Bond 1998, 2000). For a detailed discussion of the respective pros and cons, see Appendix A. Both approaches have their advantages and disadvantages, and in our view, there is no straightforward reason to give one approach preference over the other. We will therefore employ both classes of estimators. One limitation we face either way, as already pointed out by Stoyanov and Zubanov (2012), is that we cannot control for unobserved hiring preferences regarding the origin of newly hired workers. This is because such preferences are not necessarily part of the unobserved idiosyncratic productivity shock that the "structural" estimators model explicitly. When using either of the DPD estimators, we must not assume that such preferences are time-invariant, so we cannot be sure to get rid of their biasing influence.

6.2 Main results

As a baseline, we estimate the above empirical model using Pooled OLS, where we include two lags of the dependent variable (log value added) as this is found to remove residual autocorrelation. To begin with, we estimate a simplified model including the labor share of all inflows (SPs plus Non-SPs divided by labor) and the set of control variables defined in section 6.1. The first column of Table 6 indicates that productivity is not significantly related to hiring intensity as such (the share of inflows in total employment). In the second and third columns, we split inflows according to their classification as SPs or Non-SPs. Although we have found the log median wage to be more strongly related to establishment productivity, we also present results using the establishment fixed wage effect to define (Non-)SPs in the second (middle) column. The results indicate a positive association of Non-SP hiring with productivity, whereas the coefficient of SP hiring is near zero and insignificant.

Table 6
OLS estimates

	All inflows		(N)SPs defined by FE		(N)SPs defined by MW	
L.Log value added	0.567	***	0.566	***	0.566	***
L2.Log value added	0.164	***	0.164	***	0.164	***
Log capital stock	0.037	***	0.037	***	0.037	***
Log labour	0.237	***	0.237	***	0.238	***
Share high-qual. inflows	-0.078	**	-0.071	**	-0.071	**
Mean age inflows	0.015		0.002		0.003	
Mean age sq. inflows	-0.000		-0.000		-0.000	
Labor share inflows	0.292					
Labor share SPs			-0.050		-0.157	
Labor share Non-SPs			0.764	*	0.852	**
Observations	4233		4233		4233	
R-squared	0.954		0.954		0.954	

Dependent variable is log value added. Standard errors clustered at establishment level. Year, 2-digit industry and labor market region (LMR) dummies included. ESTAB and EMPL control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Concerning the core production factors capital and labor, our estimates imply slightly increasing, but almost constant returns to scale (the sum of long-run capital and labor coefficients is 1.04). Finding that inflows' average qualification (measured by the share of high-qualified inflows) is negatively related to productivity may be surprising at first sight, yet manufacturing establishments may profit particularly from hiring workers with a vocational degree, who are specialized in industry-specific work tasks. In Germany, such workers have usually received their highest degree in the apprenticeship system, which defines them as mid-qualified (rather than high-qualified), notwithstanding their high productivity in the production process.

Table 7
OP and LP estimates

	Olley/Pakes estimator				Levinsohn/Petrin estimator			
	SPs defined by FE		SPs defined by MW		SPs defined by FE		SPs defined by MW	
Log intermediate inputs	0.609	***	0.608	***				
Log capital stock	0.059		0.054	*	0.117	***	0.118	***
Log labour	0.324	***	0.325	***	0.724	***	0.722	***
Share high-qual. inflows	-0.040	*	-0.038	*	-0.081	*	-0.080	*
Mean age inflows	0.000		0.001		0.004		0.004	
Mean age sq. inflows	-0.000		-0.000		-0.000		-0.000	*
Labor share SPs	0.244		0.135		-0.066		-0.556	
Labor share Non-SPs	0.622	**	0.770	***	1.017	*	1.442	***
Observations	7908		7908		4233		4233	

Dependent variable is log revenues (OP) resp. log value added (LP). Standard errors obtained by bootstrap (1,000 replications). Trend (OP) resp. year dummies (LP), 2-digit industry and labor market region (LMR) dummies included. ESTAB and EMPL control variables included. LP regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

However, the OLS estimates are likely to be biased by determinants of productivity (deriving either from the amount of output or the efficiency of production in terms of factor use) observed by the establishment but not by the econometrician. Therefore, we estimate both of the latter OLS specifications using the estimators developed by Olley and Pakes (1996) and Levinsohn and Petrin (2003). The results are displayed in Table 7 and confirm the above finding: Having hired Non-SPs in the previous year is positively and significantly related to an establishment's productivity.

Another concern not yet addressed is that establishment heterogeneity, which is arguably rather persistent and to a large extent unobserved, may strongly co-determine productivity outcomes and hiring strategies. To address this additional concern of the endogeneity of our central explanatory variables, we apply the System GMM estimator. This estimator accounts for unobserved time-invariant establishment heterogeneity by using within-establishment variation, and addresses reverse causality by instrumenting current differences of endogenous variables with past levels, and current levels with past differences. The results are presented in Table 8. Let us focus first one the more parsimonious specifications in the first and second column. Hiring Non-SPs is still found positively related to productivity, and it is significant at least for our (preferred) definition of (Non-)SPs by the median wage. For the definition based on establishment fixed wage effects, the p-value of the Non-SP coefficient (3.830) is still not too far from significance, at 0.186. Thus, even when controlling for reverse causality and unobserved time-invariant establishment characteristics, the share of Non-SPs is positively associated with productivity. The long-run capital and labor coefficients still indicate near-constant returns to scale, with their sum close to one and therefore close to the results from the pooled specifications, suggesting our production function is appropriately specified.

Table 8
System-GMM estimates

	(N)SPs defined by FE		(N)SPs defined by MW		(N)SPs defined by FE		(N)SPs defined by MW	
L.Log value added	0.406	***	0.445	***	0.432	***	0.410	***
L2.Log value added	0.034		0.055		0.035		0.030	
Log capital stock	0.141	**	0.170	***	0.066		0.081	
Log labour	0.396	***	0.275	*	0.486	***	0.448	***
Share high-qual. inflows	-0.035		-0.039		-0.072		-0.085	*
Mean age inflows	0.006		0.006		0.006		0.010	**
Mean age sq. inflows	-0.000	*	-0.000	**	-0.000	*	-0.000	**
Labor share SPs	-0.831		-0.284		-1.237		-1.301	
Labor share Non-SPs	3.830		5.461	**	1.333		0.348	
Mean sending-est. wage pos. SPs					-0.082		0.016	
Mean sending-est. wage pos. Non-SPs					0.099		0.043	
Observations	4233		4233		4233		4233	
Sargan p-value	0.26		0.58		0.518		0.204	

Dependent variable is log value added. Standard errors clustered at establishment level. Year dummies included. EMPL control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Our (so far) preferred estimate of Non-SPs' productivity effect (5.461) would imply that the productivity gains of hiring Non-SPs are substantial: Hypothetically, the average sample establishment (which has a Non-SP labor share of about 0.9 percent) is roughly 4.9 percent more productive than an otherwise equal establishment that hires no Non-SPs. However, our result could be due to unobserved systematic differences between SP and Non-SP inflows, for which we have controlled so far only by including inflows' share of high-qualified, age, and age squared. Thus, we continue by addressing the insight of our descriptive analysis that Non-SPs constitute a positive selection from within their sending establishments, as assessed by their wage position relative to comparable co-workers (co-workers with the same age, qualification, occupation, etc.). We extend our specification to include inflows' mean sending-establishment wage position, separately for SPs and Non-SPs (columns three and four of Table 8). While the coefficients of both these variables are insignificant, Non-SPs' labor share coefficient drops sharply in magnitude and significance, using either the fixed-effect or the median-wage definition of (Non-)SPs. This finding suggests that the positive productivity outcome related to Non-SP hiring is due to these workers' positive selection from their sending establishments, so there is no statistically significant productivity effect of hiring workers from inferior establishments per se.

This is the main finding of our analysis: Hiring workers from inferior establishments is positively related to productivity because these workers are positively selected from their sending establishments. In contrast, hiring workers from superior establishments does not affect productivity; their superior-establishment experience is not valuable enough to affect hiring establishments' productivity through knowledge spillovers. These results can be further rationalized by our descriptive findings, which indicate that movers from inferior to superior establishments achieve tremendous wage increases. This is not least due to the fact that the bulk of unexplained wage variance between workers is due to establishment-level effects, as we have found in our auxiliary wage regression (equation (3) and Table A 1). That is, the best workers at lower-paying establishments, who are already being much better paid than their equally qualified co-workers, have little scope for further wage improvement when staying with their current employer. By moving to higher-paying establishments, thus, good workers are reallocated towards good firms, which not only increases their wages, but also hiring establishments' productivity.

6.3 Effect heterogeneity and robustness

In this section, we address some less obvious concerns regarding the generality and robustness of our findings. In particular, we account for the disproportionate sampling of Eastern German establishments and control inflows' individual characteristics even more precisely. First, we estimate the specifications derived above for the subsample of Western German establishments. The results, summarized in Table 9, corroborate the previous findings for the entire German sample: Non-SP hiring has a substantially positive and partly significant productivity coefficient, unless we control for inflows' selection from sending establishments. In the latter case, not only does the coefficient drop steeply, but we also find, in the specification where (Non-)SPs are defined by establishment fixed effects, Non-SPs' positive selectivity to be significantly related to productivity. The latter finding further substantiates our interpretation that hiring workers from inferior establishments increases productivity due to the positive selection of these workers.

Table 9
System-GMM estimates, Western German establishments only

	(N)SPs defined by FE		(N)SPs defined by MW		(N)SPs defined by FE		(N)SPs defined by MW	
L.Log value added	0.338	***	0.368	***	0.359	***	0.350	***
L2.Log value added	0.011		0.017		-0.015		0.017	
Log capital stock	0.104		0.104		0.036		0.081	
Log labour	0.506	***	0.512	***	0.611	***	0.533	***
Share high-qual. inflows	-0.072		-0.079		-0.071		-0.089	
Mean age inflows	0.003		0.003		0.002		0.006	
Mean age sq. inflows	-0.000		-0.000		-0.000		-0.000	
Labor share SPs	1.216		0.638		1.278		-0.695	
Labor share Non-SPs	2.091		5.041	*	0.890		2.528	
Mean sending-est. wage pos. SPs					-0.051		0.028	
Mean sending-est. wage pos. Non-SPs					0.139	**	0.047	
Observations	2120		2120		2120		2120	
Sargan p-value	0.523		0.8		0.865		0.438	

Dependent variable is log value added. Standard errors clustered at establishment level. Year dummies included. EMPL and inflow control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

To further enhance the robustness of our results, we perform another set of estimations with an even stricter set of control variables regarding inflows' individual characteristics, which we have found to differ systematically between SPs and Non-SPs: The CHK individual wage fixed effect. This variable (there called the AKM effect) has already been employed by Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013), to capture individuals' unobserved ability. We can merge the CHK person effect for nearly all worker inflows into the Western German establishments in our sample, as CHK conducted their analysis for Western Germany only. Thus, we complement the Western estimations by the mean CHK person effect of inflows, see Table 10.[16] Compared to the above set of Western German estimations, this leaves our results essentially unchanged. Inflows' mean CHK person effect itself is not significantly related to productivity, and it never gets anywhere close to conventional significance levels.

[16] Compared to the previous estimation for Western Germany, we lose exactly one observation due to a missing value in the mean inflow CHK effect. We have checked and verified that this leaves our results virtually unchanged.

We report the estimates for Eastern German establishments in Table A 2 in the Appendix. We do not find any significant productivity effects associated with worker inflows into these establishments. The overall estimates (for Germany in total) would thus be larger and more precisely estimated, were it not for the disproportionate share of Eastern German establishments in our sample.

Table 10
Sys-GMM estimates, Western Germany, including mean CHK person effects

	(N)SPs defined by FE		(N)SPs defined by MW		(N)SPs defined by FE		(N)SPs defined by MW	
L.Log value added	0.353	***	0.380	***	0.368	***	0.370	***
L2.Log value added	0.016		0.021		-0.011		0.019	
Log capital stock	0.106		0.088		0.029		0.078	
Log labour	0.504	***	0.541	***	0.625	***	0.536	***
Share high-qual. inflows	-0.057		-0.078		-0.058		-0.055	
Mean age inflows	0.003		0.002		0.009		0.019	
Mean age sq. inflows	-0.000		-0.000		-0.000		-0.000	
Mean CHK person effect	-0.002		0.007		-0.035		-0.064	
Labor share SPs	1.036		0.592		1.102		-0.883	
Labor share Non-SPs	3.448		5.332	*	0.892		2.903	
Mean sending-est. wage pos. SPs					-0.062		0.009	
Mean sending-est. wage pos. Non-SPs					0.137	**	0.058	
Observations	2119		2119		2119		2119	
Sargan p-value	0.672		0.868		0.902		0.422	

Dependent variable is log value added. Standard errors clustered at establishment level. Year dummies included. EMPL and inflow control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

A final check we perform concerns external validity with respect to the business cycle. While the period of our estimates so far (2002-2007) contains both a stagnant phase in its earlier years and a period of strong growth later on, to this point, we have omitted the Great Recession of 2008/09. We have also run our regressions for a panel covering the period 2002-2010 (including both Eastern and Western German establishments), to see whether the changed hiring behavior during the recession affects the way inflows affect hiring establishments' productivity. In Germany and the manufacturing sector in particular, establishments reacted to the crisis by reducing work hours and hoarding labor, rather than by laying off large numbers of workers. The crisis also had a negative effect on hiring, and the few hires taken in during the crisis were probably different from 'normal-times' hires in non-random ways. OLS and System-GMM results, respectively, are presented in Tables A 3 and A 4 in the Appendix. While the overall pattern of results remains the same across all estimations, only OLS still yields a significant productivity coefficient of Non-SP hiring. The Sys-GMM estimator still yields a rather large coefficient which drops severely as inflows' selectivity is controlled for; however, the effect becomes insignificant. Possible reasons are that hiring numbers were too low during the recession to substantially affect production processes, or that working and machine-running hours were capped such that inflows could not make a substantial difference to the value added produced. In any case, our main findings are not challenged, but only muted, by extending our model to times of economic downturn.

As a final note on the interpretation of our estimates, let us emphasize that our findings are not necessarily causal relationships. To obtain causal estimates, one would need a source of variation in SP/Non-SP inflows that is obviously independent of hiring establishments' productivity. Such a source of variance for a large sample of hiring firms (and an even larger sample of sending firms) is hard to find. Possibly the best feasible approach has been taken by Serafinelli (2013), who uses the number of downsizings (substantial reductions in staff) of high-wage firms in the same region and industry as an IV for worker inflows from high-wage firms. Such downsizing events increase the potential supply of high-wage firm workers rather unexpectedly. We have constructed the same kind of instrument, dividing all establishments within each labor market region into a high-wage and a low-wage group, separated at the median of their median wage levels. Therein, we measure the number of downsizing establishments using several threshold values to define downsizing (the simplest one being a negative employment growth rate, others defining downsizing more narrowly). Several variants were considered in each case: First, both labor market regions and districts (NUTS 3 regions) were used as the relevant regional level. Second, instead of regions, regional industries (both at the labor market region and district levels) were considered. Unfortunately, it turned out none of the proposed instruments is strong enough in explaining our explanatory variables (the labor shares of SP and Non-SP inflows), so we have to rely on the above-presented estimates as approximations of potentially causal productivity effects.

7 Conclusions

We have investigated, at the establishment level, the productivity effects of hiring workers from superior and inferior establishments, as defined by establishments' relative wage level. In all estimations, we control for worker inflows' productivity-relevant characteristics, meaning that their productivity effects should stem only from their sending establishments' superiority or inferiority. While previous studies find positive effects from hiring workers from superior firms, our estimates suggest that hiring workers from inferior (lower-paying) establishments increases hiring establishments' productivity. We also find that these workers are positively selected from their sending establishments, where they occupy relatively high wage positions. Indeed, this selectivity explains their positive productivity effect. For the subsample of Western German establishments, which are underrepresented in our total sample, we also find that these inflows' sending-establishment wage position is significantly positively related to hiring establishments' productivity. In contrast, hiring workers from higher-paying establishments does not seem to increase productivity, which is in line with the finding that they are not positively selected from their sending establishments. One reason for these contrary results might be the marked differences between national labor markets. As Jolivet et al. (2006) show, Germany and Denmark (where inflows from superior firms have been found to increase hiring firms' productivity) differ a lot with regard to the degree of job-to-job mobility as well as the reasons for mobility. In particular, in Denmark mobility levels are higher and workers more often move involuntarily.

To grasp the economic workings behind our findings, we have to consider the individual worker's perspective: Being a top earner at her initial employer, a worker at a lower-paying firm earns far less than the average equally qualified worker at a higher-paying firm. The only way to raise her wage to an adequate level, which then probably reflects her individual productivity, is moving to a higher-paying firm. Thus, our results reflect Card et al.'s (2013) finding of assortative worker mobility across heterogeneous firms: Good workers, as compared to their co-workers, move to good firms, as compared to the firm they leave. As we investigate mobility and production processes at the worker and establishment levels, our findings may be regarded as a micro-foundation for this aggregate mobility pattern and its implications.

To conclude, we would like to point out that a broader economic discussion of our results would have to address labor market frictions, not least because of the differing findings for different countries. The finding that worker mobility across firms can yield important wage gains indicates that some workers are initially badly matched with their employer. Overcoming this mismatch by moving to a "better" firm, highly productive workers reduce the amount of mismatch in the labor market. Explicitly incorporating these frictions in the empirical analysis, however, is beyond the scope of this study, as are the welfare gains associated with the identified mobility process. Further research might generate insight on these questions.

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Appendix

Table A 1
Log wage regressions

Sample: all qualified full-time workers at sending and hiring establishments, as of June 30th					
	2000			2010	
	Coef.	SE	…	Coef.	SE
male	0.1809	0.0002		0.1705	0.0003
low-skilled	-0.1764	0.0004		-0.1818	0.0004
mid-skilled	-0.1299	0.0003		-0.1419	0.0003
high-skilled	0.0000	(omitted)		0.0000	(omitted)
age	0.0324	0.0001		0.0353	0.0001
age squared	-0.0003	0.0000		-0.0003	0.0000
N (individuals)	7,378,477			6,479,016	
n (establishments)	70,873			59,955	
R-sq:	0.4433			0.4554	
rho (fraction of residual variance due to establishment fixed effects)	0.6746			0.6841	

All regressions include establishment, 2-digit occupation, and occupation status fixed effects.

Data Source: Establishment History Panel; own calculations.

Table A 2
System-GMM regressions, Eastern German establishments only

	(N)SPs defined by FE		(N)SPs defined by MW		(N)SPs defined by FE		(N)SPs defined by MW	
L.Log value added	0.411	***	0.428	***	0.367	***	0.410	***
L2.Log value added	0.075		0.080	*	0.033		0.055	
Log capital stock	0.082		0.082		0.064		0.042	
Log labour	0.472	***	0.423	***	0.557	***	0.524	***
Share high-qual. inflows	-0.057		-0.079		-0.036		-0.050	
Mean age inflows	0.009		0.010	*	0.009		0.010	*
Mean age sq. inflows	-0.000	*	-0.000	**	-0.000	*	-0.000	
Labor share SPs	-0.887		-1.085		-0.632		-1.115	
Labor share Non-SPs	-1.353		-3.261		-1.902		-2.811	
Mean sending-est. wage pos. SPs					-0.097		0.003	
Mean sending-est. wage pos. Non-SPs					-0.020		-0.008	
Observations	2113		2113		2113		2113	
Sargan p-value	0.822		0.887		0.8		0.956	

Dependent variable is log value added. Standard errors clustered at establishment level. Year dummies included. EMPL and inflow control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Table A 3
OLS estimates, years 2002-2010

	All inflows		(N)SPs defined by FE		(N)SPs defined by MW	
L.Log value added	0.556	***	0.555	***	0.555	***
L2.Log value added	0.192	***	0.191	***	0.192	***
Log capital stock	0.032	***	0.031	***	0.032	***
Log labour	0.225	***	0.228	***	0.226	***
Share high-qual. inflows	-0.051	**	-0.046	*	-0.048	**
Mean age inflows	0.009		0.002		0.001	
Mean age sq. inflows	-0.000		-0.000		-0.000	
Labor share inflows	0.318					
Labor share SPs			0.071		0.057	
Labor share Non-SPs			0.791	**	0.604	**
Observations	7278		7278		7278	
R-squared	0.953		0.953		0.953	

Dependent variable is log value added. Standard errors clustered at establishment level. Year, 2-digit industry and labor market region (LMR) dummies included. ESTAB and EMPL control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Table A 4
System-GMM estimates, years 2002-2010

	(N)SPs defined by FE		(N)SPs defined by MW		(N)SPs defined by FE		(N)SPs defined by MW	
L.Log value added	0.318	***	0.317	***	0.332	***	0.325	***
L2.Log value added	0.019		0.011		0.021		0.019	
Log capital stock	0.145	***	0.157	***	0.115	***	0.111	***
Log labour	0.530	***	0.538	***	0.566	***	0.566	***
Share high-qual. inflows	-0.013		-0.018		-0.006		-0.000	
Mean age inflows	0.001		-0.000		-0.000		-0.002	
Mean age sq. inflows	-0.000		0.000		0.000		0.000	
Labor share SPs	-0.724		0.181		0.359		0.778	
Labor share Non-SPs	2.125		1.688		1.496		1.295	
Mean sending-est. wage pos. SPs					-0.032		-0.008	
Mean sending-est. wage pos. Non-SPs					0.029		0.061	
Observations	7278		7278		7278		7278	
Sargan p-value	0.386		0.324		0.238		0.423	

Dependent variable is log value added. Standard errors clustered at establishment level. Year dummies included. EMPL control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01.

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Appendix A: Econometric issues of production function estimation

In a very comprehensive paper, Eberhardt and Helmers (2010) (hf. EH) review the most important problems encountered by econometricians using "fat" panel data (large N, short T) to estimate firm-level production functions. We refer to their paper for its comprehensiveness and emphasis on the imperfections of the data typically used (availability and quality of output and capital data, need for proxies, etc.).

EH argue that unobserved total factor productivity (TFP) is composed of firms' mean efficiency, period-specific effects, firm-specific effects, and an idiosyncratic component, and since the latter is observed by the firm but not the econometrician, there can be unobserved factors influencing firms' input choices, implying that failing to control for these factors renders OLS and fixed-effects estimates inconsistent. More explicitly, the main problem arises from the possibility of the firm to observe its idiosyncratic TFP shock *before* choosing its levels of capital and labor; the idiosyncratic effect thus is an omitted variable that needs to be controlled for. Otherwise, it is being transmitted to the observed inputs (capital and labor), i.e. the production factors' coefficients take up the idiosyncratic effect and are thus biased upward. In contrast, a downward bias can result from imprecise measurement of inputs (attenuation bias). The idiosyncratic TFP shock represents, above all, simultaneity or reverse causality, i. e. the simultaneous or reversed determination of factor inputs with respect to the realized output.

EH discuss three approaches to combat these endogeneity biases. The first approach, instrumenting factor inputs using factor prices, can be ignored in the case of our study. Instead, we focus on the problem of endogeneity (reverse causality) bias arising from establishments' anticipation of their productivity level and their according choice of inputs. The two main approaches to minimize this bias are, first, control function approaches trying to model the idiosyncratic TFP shock explicitly, and second, dynamic panel data (DPD) approaches making use of internal instruments in panel data sets. The first class of estimators has been developed by Olley and Pakes (1996) (OP), Levinsohn and Petrin (2003) (LP), Ackerberg et al. (2006) (ACF), and Wooldridge (2009) (WOP); the second class is rooted in the work of Arellano and Bond (1991) (AB) and Blundell and Bond (1998, 2000) (BB).

To construct the control function for the idiosyncratic TFP shock observed by the firm but not the researcher, OP, LP, ACF, and WOP need to assume that this shock is the only unobservable entering the investment (respectively, intermediate inputs) function. This "scalar unobservable assumption" (EH) cannot be tested. More specifically, to identify the labor coefficient, which should be more important, given our core explanatory variables, than identifying the capital coefficient, the structural estimators assume a discrete sequence of establishments' decisions about the particular factor inputs. Again, this assumption cannot be tested empirically (EH, p. 24). At best, the assumption could be plausible in some particular production processes (industries), but we do not expect it to hold across the entire manufacturing sector (let alone other sectors).

Using the longitudinal dimension of panel data, the DPD estimators control for time-invariant unobserved establishment heterogeneity. This eliminates omitted variable bias. However, the bias due to unobserved productivity shocks would be removed only if these were time-constant. The DPD estimators indicated above, by using internal IVs, take an additional step to combat this endogeneity bias. Furthermore, unlike the “structural” estimators (OP, LP, etc.), the DPD estimators allow one to test all crucial assumptions made about the data-generating process (DGP). It could thus be argued that, overall, the DPD estimators are a more conservative choice than any of the “structural” (control function) estimators. On the other hand, due to using only within-establishment variation in a fat panel, one may fail to identify effects with any precision using these estimators. Aiming to maximize the robustness of our findings, we employ both classes of estimators.

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Imprint

IAB-Discussion Paper 7/2016
29 February 2016

Editorial address
Institute for Employment Research
of the Federal Employment Agency
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ISSN 2195-2663

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