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Productivity Spillovers Across Firms through Worker Mobility†

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Using matched firm-worker data from Danish manufacturing, we observe firm-to-firm worker movements and find that firms that hired workers from more productive firms experience productivity gains one year after the hiring. The productivity gains associated with hiring from more productive firms are equivalent to 0.35 percent per year for an average firm. Surviving a variety of statistical controls, these gains increase with education, tenure, and skill level of new hires, persist for several years after the hiring was done, and remain broadly similar for different industries and measures of productivity. Competing explanations for these gains, knowledge spillovers in particular, are discussed. (*JEL* D24, J24, J62, L60, O33)

Economic theories that try to explain growth increasingly rely on knowledge spillovers, whereby firms improve their performance through learning from each other (Romer 1990; Grossman and Helpman 1991). For instance, recently proposed models of endogenous growth with heterogeneous firms (Eeckhout and Jovanovic 2002; Luttmer 2007; Atkeson and Burstein 2010) rely on spillovers to "ensure that the technologies available to potential entrants are never so far behind those of incumbent firms that entry of new firms is not feasible" (Luttmer 2007, 1106). The mechanisms behind the spillovers, however, are little known. While the patent citations literature (Griliches 1992; Jaffe, Trajtenberg, and Henderson 1993; Hall, Jaffe, and Trajtenberg 2001) makes a strong case for knowledge diffusion among firms using each other's patents, this mechanism cannot be the only one at work, since patenting is practiced by relatively few innovating firms and excludes a wealth of uncodified knowledge. An alternative mechanism, more widely applicable than patent citations, is worker mobility.

That new workers have been seen as a source of new knowledge is evident as firms try to prevent their former employees from being hired elsewhere. For

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instance, many firms add noncompete covenants (NCCs) in their labor contracts, leading to court cases when their violations are detected.[1] Even where NCCs cannot be enforced in court, as in California or North Dakota, firms still sue their former employees for disclosing trade secrets, although trade secret law violations are harder than NCCs to detect and prosecute. Despite potential lawsuits, however, one often observes firms poaching employees from competitors in an attempt to benefit from their knowledge. This poaching is sometimes so intense that a number of Silicon Valley firms, such as Adobe Systems, Apple, Google, Intel Corporation, Intuit, and Pixar, agreed in 2009 not to approach each other's employees, even at the risk of violating the US competition law.

In addition to the news features and court cases, more systematic evidence exists that is consistent with spillovers through employee turnover. Notable studies within the literature on R&D spillovers through turnover include Rao and Drazin (2002); Kaiser, Kongsted, and Rønde (2008); and Maliranta, Mohnen, and Rouvinen (2009), who showed that hiring knowledge workers from R&D-intensive firms is linked to better performance by the hiring firms, Song, Almeida, and Wu (2003), who found that worker flows can explain patterns of patent citations, and Kim and Marschke (2005) who argued that too high mobility of R&D workers between firms may actually hinder innovation by causing firms to defend their intellectual property more rigorously. Studies on spillovers from foreign to domestic firms have broadened the scope of the spillovers through turnover literature, by looking at more general knowledge than that possessed and transferred by R&D workers alone. Thus, two recent studies, Poole (2009) and Balsvik (2011), found a positive effect on wages paid in domestic firms of the share of new workers previously employed by foreign-owned firms, and no such effect when similar workers had no foreign firm experience. Other relevant studies include Gorg and Strobl (2005) who found domestic businesses managed by ex-employees of foreign-owned companies to be more productive and more likely to survive, and Malchow-Moller, Markusen, and Schjerning (2007) who showed that workers with foreign firm experience enjoyed a wage premium paid by their new domestic-owned employers.

Building on the research summarized above, we set out to investigate whether a firm's productivity can be linked to the productivities of the firms from which it hired workers. Using matched firm-worker data from the Danish manufacturing sector enables us to register worker movements between firms and thus to link the productivity of each firm that hired new workers (*receiving firms* in our terminology) to the average productivity of the firms from which the new workers came (*sending firms*). A first look at our data reveals a correlation of 0.15 between receiving firms productivity in year $t + 1$, one year after it hired new workers, and the average productivity of their respective sending firms in year $t - 1$, when all the moving workers were still employed there. It is the movement of workers from

[1] The complete list of noncompete court cases in the United States is difficult to find. One court case database, http://www.morelaw.com, lists 25 noncompete court cases heard between 2005 and 2010 in 11 US states. A search with key words "covenant not to compete" in FindLaw database (http://caselaw.findlaw.com) gives 29 cases heard between 2000 and 2010 by US Supreme Courts and Courts of Appeals. Siegel, Brill, Greupner, Duffy, and Foster (http://www.siegelbrill.com), a law firm based in Minnesota, lists 56 noncompete cases heard in Minnesota courts alone during 2000–2008.

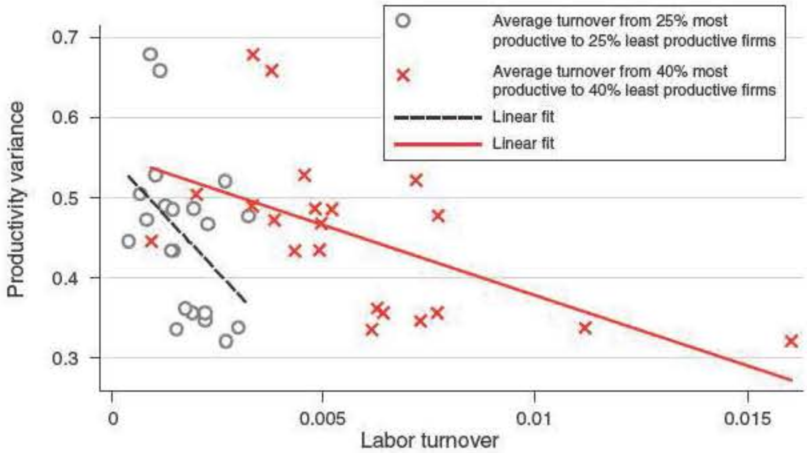

FIGURE 1. THE RELATIONSHIP BETWEEN LABOR TURNOVER AND INDUSTRY-LEVEL TFP VARIANCE BY 3-DIGIT NACE INDUSTRY

sending to receiving firms that gives rise to this correlation, since the contemporaneous correlation between the same sending and receiving firm's productivities is just 0.05. Moreover, the correlation between the sending firm's productivity in $t + 1$ and the receiving firm's productivity in $t - 1$ is high (0.214) when workers move from more to less productive firms, and low (0.097) otherwise, suggesting that less productive firms benefit from more productive ones, while the performance of the more productive firms is affected much less.

Further, if moving workers enable spillovers by spreading knowledge from one firm to another, one would expect a more concentrated productivity distribution in industries with higher rates of worker turnover from more to less productive firms. Figure 1 plots the relationship between productivity variance in 21 2-digit NACE industries and the 1995–2007 average turnover rate from top 40 percent to bottom 40 percent of firms (crosses), and from top quartile to bottom quartile (dots). As we expected, there is a strong negative correlation between worker turnover and productivity dispersion: -0.45 for the top to bottom 40 percent and -0.58 for the top to bottom quartile. That this correlation becomes stronger as we increase the productivity difference between sending and receiving firms suggests that spillovers depend on the magnitude of this difference. In fact, we find the productivity difference between the sending and receiving firms (the *productivity gap*, see subsection IA for details) to be a convenient measure of the receiving firms exposure to spillovers from hiring.

Our three most important findings are as follows. First, hiring workers from more productive firms is linked to productivity gains in receiving firms. Quantitatively, these gains amount to a 0.35 percent productivity increase one year after hiring for the average firm, or a move 0.4 centiles up in the productivity distribution for the median firm. The productivity changes associated with hiring from less productive firms, on the other hand, are negligible. That productivity gains linked to hiring are realized only when new hires come from more productive firms is consistent with the knowledge spillovers hypothesis. Second, the statistically detectable productivity gain associated with hiring from more productive firms lasts four years, after which period it fades out. The cumulative productivity gain for an average firm hiring at the same (average) gap for four consecutive years is 1.64 percent, which is equivalent to a 2.3 centile move up the productivity distribution by the median firm. Third, greater productivity gains linked to the same magnitude of the gap are observed in firms hiring workers with more education, higher skills, and longer tenure at their previous firms. This said, weaker but still significant gains are linked to hiring medium-skilled workers, which, taking the spillovers' perspective, implies that a substantial part of the knowledge transferred by job movers is not particularly sophisticated and thus is unlikely to be patented or otherwise codified. This finding is thus an important addition to the existing spillovers literature which typically deals with knowledge codified in patents or transferred by highly skilled workers such as engineers or managers.

Our work's contribution to the literature on spillovers goes beyond factual findings. Thus, our unique data enable us to present what we believe is presently most detailed and robust, though still not conclusive, empirical evidence consistent with spillovers through worker mobility. Because we operate with receiving firms' productivity rather than some indirect indicator of performance, such as the number of patents or R&D expenditure, our results can be directly applied in calibrating theoretical models of spillovers to analyze the effects of labor mobility on the distribution of firm size, productivity, growth, and welfare. We develop a measure of the receiving firm's exposure to spillovers—the productivity gap—thus extending the relevant literature which has so far operated with a $0 - 1$ variable denoting experience at a foreign firm, or an aggregate thereof. The gap performs well in our regressions, giving consistent results for various measures of it based on value added, TFP, and profit. Furthermore, our research widens the scope of the literature on spillovers through worker mobility by presenting evidence consistent with spillovers of codified, as well as uncodified, knowledge between nearly all firms in the Danish manufacturing sector. Our results are thus more widely applicable, while agreeing with those reported in earlier studies.

The rest of the paper is organized as follows. In Section I, we describe our data (subsection IA) and present the basic empirical model that we employ to estimate the relationship between receiving firm's productivity and the gap (subsection IB). Therein we also discuss relevant estimation issues and implied limitations of our approach. The regression results, along with various extensions and robustness checks, are presented in Sections II, III, and IV. Section V concludes.

I. Data and Method

A. *Data*

The key features of our data, provided by Statistics Denmark, are the total coverage of employees and firms and the match between the employee and firm records. Both these features make the data particularly suitable for our purposes, since they enable us to detect moving workers in each year and their sending and receiving firms. We use manufacturing firm-level data from 1995 to 2007 which include sales, employment, value added, materials and energy input, profit, fixed assets stock and investments, and the two-digit NACE industry identifier. A large part of firm-level data comes from annual surveys in which all firms employing 50 or more workers must participate. Small firms are surveyed less frequently so that the missing data for them are interpolated. The individual-level data are available from 1983 onwards and cover all individuals aged 15–65 and include salary (if applicable), age, gender, experience in thousands of hours, highest completed education, and occupation. In the analysis that follows, we only include manufacturing firms and employees with a positive annual salary. All individuals with multiple jobs are treated as different. Educational attainment is measured in three levels: high school, college, and university. The occupation variable consists of four categories: low-skilled, mid-skilled, high-skilled workers, and managers. The employment record is as of the end of the calendar year, so, if a worker changed jobs, we only observe the year in which it happened.

The dependent variable in much of our analysis is firm's productivity defined as the natural logarithm of value added per worker normalized by the applicable industry-year average, where industry is defined at the 4-digit level of NACE classification.[2] This normalization ensures that our productivity measure is defined for each firm *relative* to a firm with average productivity for a given industry and year. Our baseline explanatory variable is the *productivity gap* which we calculate for each firm j hiring workers in year t as follows:

$$(1) \qquad \widetilde{\widetilde{gap}}_{j,t} = \frac{\sum_{i=1}^{H_{j,t}} (A^s_{i,t-1} - A^r_{j,t-1})}{H_{j,t}} \cdot \frac{H_{j,t}}{N_{j,t}},$$

where A^r_{t-1} and A^s_{t-1} are normalized productivities of the receiving and sending firms in year $t-1$ (one year before hiring), $H_{j,t}$ is the number of new workers and $N_{j,t}$ is the total number of workers. Put simply, $\widetilde{\widetilde{gap}}_{j,t}$ is the productivity difference between the sending and receiving firm defined for each new worker i, averaged across all the new workers in firm j, and multiplied by their share in total employment $(H_{j,t}/N_{j,t})$. Intuitively, weighing the gap averaged across new workers by their share should account for the exposure of the receiving firm to the knowledge coming from the sending firms. The larger this share, the higher the exposure.

In addition to the productivity gap defined above, we calculate the *positive* and *negative productivity gaps* separately for workers hired from more and less productive firms than the receiving firm j:

$$(2) \qquad \widetilde{\widetilde{gap}}^P_{j,t} = \frac{\sum_{i=1}^{H_{j,t}} D_{i,t}(A^s_{i,t-1} - A^r_{j,t-1})}{H_{j,t}} \cdot \frac{H_{j,t}}{N_{j,t}}$$

$$(3) \qquad \widetilde{\widetilde{gap}}^N_{j,t} = \frac{\sum_{i=1}^{H_{j,t}} (1 - D_{i,t})(A^s_{i,t-1} - A^r_{j,t-1})}{H_{j,t}} \cdot \frac{H_{j,t}}{N_{j,t}},$$

[2] Subsection IVE reports estimation results for several alternative measures of productivity. These results are similar to those in our main specification.

Table 1—Summary Statistics for Workers

	Stayers	New hires	New hires from more productive firms
Wage (log)	12.23	12.21	12.26
Age (years)	39.6	36.9	37.9
Experience (years)	14.7	14.2	14.8
Male (share)	67.7	72.1	72.4
High school (share)	38	35.7	34.9
College (share)	49.8	51.8	55.2
University degree (share)	12.2	12.6	9.8
Low-skilled workers (share)	25.5	19.4	18.6
Mid-skilled workers (share)	53.5	61.2	61.6
High-skilled workers (share)	11.9	10.6	10.5
Managers (share)	9.2	8.8	9.3
Value added per worker (log)	6.36	6.36	6.21
Value added per worker in sending firm (log)		6.29	6.46
Share in labor force	0.885	0.115	0.023
Firm size (workers)	1,105	1,118	1,413
Observations	5,127,165	668,034	135,006

Notes: Summary statistics are calculated for all workers in the manufacturing sector for the time period 1995–2004. The share of new hires from less productive firms is 0.027. For the rest of new hires (0.065), the productivity of sending firm is unknown.

where $D_{i,t}$ is an indicator variable equal to one if $(A^s_{i,t-1} - A^r_{i,t-1}) > 0$, and zero otherwise. By analogy with the gap defined in equation (1), the positive and negative gaps are productivity differences averaged across workers from more and less productive sending firms separately and then weighed by the shares of these two groups in the total workforce employed at the receiving firm.

Table 1 lists descriptive statistics measured at the worker level. Between 1995 and 2007 there were about 5.8 million worker-year observations, of which 668,034 are job changers, implying an average hiring rate of 11.5 percent. The average job stayer is 39.6 years of age and has 14.7 years of experience. The majority of stayers have a (technical) college degree (49.8 percent), 12.2 percent have a university degree, and 38 percent a high school diploma. Most are classified as mid-skilled (53.5 percent) followed by low-skilled (25.5 percent), high-skilled (11.9 percent), and managers (9.2 percent). In comparison, the average job changer is 2.7 years younger and has half a year less experience. He is more likely than a job stayer to be a mid-skilled worker (61.2 percent versus 53.5 percent) and to have completed an education beyond high school (64.3 percent versus 62 percent). During the time period covered by our sample, the wage of an average job stayer was 205,000 Danish Krones ($= e^{12.23}$), or 27,000 Euros, per annum. The salary of an average job changer was 2 percent below that, but those moving from more to less productive firms, as measured by value added per worker, earned, on average, 3 percent extra. This wage premium is consistent with firms trying to attract workers from more productive firms by offering them higher salaries.

Turning to the firm-level statistics (Table 2), of the 173,929 firm-year observations in the sample, hiring took place in about half (85,123). Size is the biggest difference between hiring and nonhiring firms: the hiring firms tend to be larger. Thus, for the duration of our sample period, hiring was zero only in 3.6 percent of all observations for firms with 50 employees or more, whereas the same share on the

TABLE 2—SUMMARY STATISTICS FOR FIRMS

			Hiring share > 0		
	All firms	No hiring	Stayers	Hires from more productive firms	Hires from less productive firms
Wage (log)	11.9	11.8	12	12.1	12.1
Age (years)	40.5	42.6	37.9	35.1	35.1
Experience (years)	9.2	9.1	9.2	9.1	9.1
Male (share)	71.9	72.4	71.2	76.8	77.8
High school (share)	34.1	32.7	36.2	33.8	32.4
College (share)	55.4	57.2	52.8	54.4	56.4
University degree (share)	10.4	10	11	11.8	11.2
Low-skilled workers (share)	54	67.5	34.4	18.5	17.5
Mid-skilled workers (share)	35.4	24.2	51.8	65.8	66.8
High-skilled workers (share)	5.6	4.4	7.3	8.9	8.7
Managers (share)	5	4	6.5	6.7	6.9
Value added per worker (log)	6.03	5.93	6.19	6.16	6.32
Value added per worker in sending firm (log)				6.45	5.98
Share in labor force	10.6	0		3.5	3
Firm size (workers)	27	7	56		
Observations	173,929	88,806	85,123		

Notes: Summary statistics are calculated for all workers in manufacturing industry for the time period 1995–2004. Statistics show average values for different firms and subgroups of workers. In the last three columns, averages were first calculated for all workers belonging to a given group within each firm and then averaged across firms.

small firms ($\leq$ 49 employees) subsample is 57.7 percent. Another important difference is that hiring firms are more productive, with the average (non-normalized) log value added per worker at 6.2 versus 6 for all firms and 5.9 for nonhiring firms.[3] The productivity difference between the average hiring and nonhiring firms disguises a significant variation in the productivities of the sending firms from which new workers are hired. Thus, on the entire sample, 45 percent of new hires come from more, and 55 percent from less, productive sending firms, resulting in the productivity gap with the average of about zero and standard deviation of 0.057.

Table 3 lists detailed summary statistics for our key regression variables—the dependent variable and the positive and negative productivity gaps—on the entire sample as well as for small ($\leq$ 49 employees) and large ($\geq$ 50 employees) firms. Large firms are more productive, and, as we saw in the previous paragraph, it is the large firms that are more likely to hire employees. The sample average positive and negative gaps are about the same in magnitude, 0.0125 and -0.0128, respectively. Although large firms hire more, the positive gaps for large and small firms are close, which implies that the average positive productivity difference for large firms is lower than for small firms. This is no surprise, since large firms tend to be more productive and there are relatively few, yet more productive, firms to hire from. On the other hand, the average negative gap is greater for large firms than for small, since there is a long tail of less productive firms from which the workers come.

What types of worker flows are behind our measures of the productivity gap? In short, workers move to and from firms over the entire productivity range. The

[3] Compared with worker averages, firm-average productivity and wages are lower as a result of smaller firms, whose weight in total observations is now greater, being less productive and paying less than larger firms do.

TABLE 3—SUMMARY STATISTICS FOR VALUE ADDED PER WORKER (*normalized*) AND THE PRODUCTIVITY GAPS

	Sample	Normalized VA per worker	Positive gap	Negative gap
Mean	all firms	0.0463	0.0125	−0.0128
	N<=49	0.0208	0.0120	−0.0120
	N>=50	0.3092	0.0133	−0.0204
Standard deviation	all firms	0.6518	0.0557	0.0652
	N<=49	0.6635	0.0559	0.0657
	N>=50	0.4370	0.0528	0.0600
Standard deviation within firms	all firms	0.3462	0.0457	0.0526
	N<=49	0.3517	0.0453	0.0523
	N>=50	0.2425	0.0438	0.0488
Standard deviation between firms	all firms	0.7327	0.0516	0.0637
	N<=49	0.7426	0.0531	0.0648
	N>=50	0.4152	0.0488	0.0609
Standard deviation within industry	all firms	0.6518	0.0555	0.0652
	N<=49	0.6630	0.0558	0.0656
	N>=50	0.4135	0.0527	0.0598
Standard deviation between industries	all firms	0.0205	0.0039	0.0015
	N<=49	0.0274	0.0039	0.0015
	N>=50	0.1582	0.0040	0.0055

Notes: Summary statistics are calculated for all manufacturing firms for the time period 1995–2004. N is the total number of employees. The first column shows summary statistics for log value added per worker normalized by industry-year averages. In the last six rows, standard deviations are calculated within and between 2-digit NACE industries.

worker transition matrix in Table 4 offers an illustration of the observed worker flows. The rows and columns therein contain productivity deciles of the sending and receiving firms, respectively.[4] The upper number in cell i,j is the number of workers coming from firms in productivity decile i relative to the total intake by firms in decile j, and the lower number is the same relative to the total number of hires by all firms. For instance, 22.09 percent of the workers hired by the bottom 10 percent of firms come from the same decile. These workers make up 2.42 percent of the total number of hires with known productivities of their sending and receiving firms. The tendency for hires from own decile to exceed a tenth of the total, and for this share to decrease with interdecile productivity difference, reveals a weak prevalence of hires from similarly productive firms. However, as can be seen from the row titled "share of new workers hired from less productive firms," these shares roughly correspond to their respective deciles, implying that hiring workers from more productive firms does not make hiring from less productive ones less likely. Indeed, the average negative gap for firms hiring from more productive sources is the same as on the entire sample (-0.0128), and the average positive gap for firms hiring from less productive sources is close to average, 0.0127. Thus, although hiring may not be entirely random with respect to productivity, our measures of the gap are based on a healthy variation of sources of new hires.

[4] All deciles are defined from the same productivity distribution including all workers from all firms in the sample.

Table 4—The Worker Transition Matrix between Firms

	Productivity deciles of receiving firms									
	First	Second	Third	Fourth	Fifth	Sixth	Seventh	Eighth	Ninth	Tenth
Productivity deciles of sending firms										
First	**22.09%**	16.84%	11.68%	11.74%	9.68%	8.20%	7.13%	7.31%	5.80%	5.86%
	2.42%	1.58%	1.16%	1.24%	1.02%	0.93%	0.74%	0.62%	0.52%	0.55%
Second	14.16%	**19.87%**	13.73%	11.82%	9.95%	7.61%	6.56%	7.53%	6.28%	5.05%
	1.55%	**1.87%**	1.36%	1.25%	1.05%	0.86%	0.68%	0.64%	0.57%	0.47%
Third	10.22%	13.22%	**10.05%**	12.67%	13.03%	14.68%	8.99%	8.96%	7.55%	4.92%
	1.12%	1.24%	**1.00%**	1.34%	1.37%	1.67%	0.93%	0.76%	0.68%	0.46%
Fourth	11.65%	10.87%	14.97%	**17.34%**	13.03%	8.90%	7.93%	9.22%	7.01%	5.48%
	1.28%	1.02%	1.49%	**1.83%**	1.37%	1.01%	0.82%	0.79%	0.63%	0.51%
Fifth	9.04%	8.90%	10.93%	11.33%	**14.31%**	12.73%	14.10%	10.14%	8.13%	10.71%
	0.99%	0.84%	1.09%	1.20%	**1.51%**	1.45%	1.46%	0.86%	0.73%	1.00%
Sixth	9.93%	9.85%	13.26%	9.42%	12.79%	**19.95%**	9.28%	10.39%	7.32%	6.30%
	1.09%	0.93%	1.32%	1.00%	1.35%	**2.27%**	0.96%	0.89%	0.66%	0.59%
Seventh	7.57%	6.89%	6.87%	7.37%	8.51%	8.35%	**9.23%**	15.02%	10.66%	7.44%
	0.83%	0.65%	0.68%	0.78%	0.90%	0.95%	**0.96%**	1.28%	0.96%	0.69%
Eighth	5.24%	5.28%	6.02%	7.11%	6.58%	8.31%	8.04%	**11.71%**	9.71%	8.02%
	0.57%	0.50%	0.60%	0.75%	0.69%	0.94%	0.83%	**1.00%**	0.88%	0.75%
Ninth	5.62%	4.37%	7.55%	6.49%	6.79%	6.49%	23.36%	10.04%	**14.80%**	30.61%
	0.62%	0.41%	0.68%	0.69%	0.71%	0.74%	2.42%	0.86%	**1.33%**	2.86%
Tenth	4.48%	3.91%	6.49%	4.70%	5.32%	4.76%	5.38%	9.71%	22.74%	**15.61%**
	0.49%	0.37%	0.65%	0.50%	0.56%	0.54%	0.56%	0.83%	2.05%	**1.46%**
Share of new workers hired	9.48%	26.66%	30.12%	42.33%	51.40%	56.20%	58.64%	76.99%	70.94%	93.87%
from less productive firms	2.06%	4.97%	5.93%	8.87%	10.74%	12.66%	12.05%	12.67%	12.68%	17.37%
Share of new workers hired	90.52%	73.34%	69.88%	57.67%	48.60%	43.80%	41.36%	23.01%	29.06%	6.13%
from more productive firms	20.11%	13.98%	14.08%	12.35%	10.38%	10.08%	8.69%	3.87%	5.31%	1.16%
Mean positive gap	0.015	0.017	0.016	0.014	0.012	0.010	0.008	0.008	0.006	0.005
	(0.066)	(0.053)	(0.059)	(0.054)	(0.046)	(0.041)	(0.043)	(0.049)	(0.036)	(0.061)
Mean negative gap	−0.004	−0.007	−0.009	−0.012	−0.014	−0.017	−0.019	−0.021	−0.026	−0.036
	(0.037)	(0.043)	(0.047)	(0.058)	(0.069)	(0.074)	(0.072)	(0.074)	(0.094)	(0.124)

Notes: Columns show productivity deciles of the receiving firms and rows show productivity deciles of the sending firms. The upper number in cell $(i;j)$ shows the percentage of workers which firms in decile j recruit from firms in decile i. The lower number shows the percentage of workers moving from decile i to decile j relative to total number of hires by all firms. In the last two rows, the lower number in brackets is the standard deviation of positive and negative gap measures, respectively. Deciles are defined from the productivity distribution of all manufacturing firms which operated during 1997–2005 time period.

B. *Empirical Model and Estimation Issues*

We start by estimating the following relationship between the productivity level of the receiving firm and a measure of the productivity gap as defined above (equations (1)–(3)):

$$(4)\quad A^r_{j,t+1} = \sum_{k=0}^{L-1} \alpha_k A^r_{j,t-k} + \beta \cdot \widetilde{\overline{gap}}_{j,t} + \mathbf{X}_{j,t}\gamma_1 + \overline{\mathbf{Y}}^1_{j,t}\gamma_2 + \overline{\mathbf{Y}}^2_{j,t}\gamma_3 + \varepsilon_{j,t+1},$$

where $\mathbf{X}_{j,t}$ is the vector of the receiving firm's characteristics (including a constant term), $\mathbf{Y}^1_{j,t}$ is the vector of incumbent workers' characteristics, and $\mathbf{Y}^2_{j,t}$ is the vector of new workers' characteristics (see Table 2), both averaged at the receiving firm level.

To estimate the coefficient on the gap, β, consistently, we must ensure that $\widetilde{\overline{gap}}_{j,t}$ is uncorrelated with unobserved shocks to the receiving firm's productivity coinciding with, preceding, or following the hiring of new workers. The new workers' human capital, which is likely to be correlated with their sending firms' productivity, is one of the major sources of the shock coinciding with the hiring. Not controlling for this correlation will confuse the effect of the gap with that of the new workers' human capital. As an extra control, therefore, we introduce a comprehensive measure of workers' human capital, including a variety of its observed characteristics (age, gender, salary, experience, education, professional status), as well as its unobserved component. Our approach to inferring the unobserved human capital component from the data is based on the work of Abowd, Kramarz, and Margolis (1999) and uses workers' movement across firms to identify the person-specific component θ_i from the wage equation:

$$w_{i,j,t} = \lambda + z_{i,t}\beta + \theta_i + \psi_j + \varepsilon_{i,j,t}, \tag{5}$$

where w denotes wage, $z_{i,t}$ is the vector of worker i's personal characteristics, and ψ_j is the firm fixed effect. Specifically, our measure of workers' human capital includes both observed and unobserved components of wage in equation (5) and is calculated as the firm average of individual measures, $h_{j,t} = \overline{z_{i,t}\beta + \theta_i + \varepsilon_{i,j,t}} = (1/N_{j,t}) \sum_{i=1}^{N_{j,t}} (w_{i,j,t} - \lambda - \psi_j)$. Subtracting the firm-specific component ψ_j from the wage renders $h_{j,t}$ free from firm-level wage effects (such as compensation policies), assuming that these effects are time-invariant. Note that we include in the regression equation (5) the new workers' human capital as measured in year $t - 1$, the last full year when they were employed in their previous firms.

Even controlling for human capital, if a receiving firm j experiences a positive productivity shock in years t, $t - 1$, or earlier, it may respond by hiring workers from more productive firms who are likely to be of better quality and whom it can now better afford. Then, in addition to the effect of the productivity gap in $t - 1$ on the receiving firm's productivity in $t + 1$, $\widetilde{\widehat{gap}}_{j,t}$ will carry the receiving firm's own productivity shocks of the past. We present three approaches to isolating these productivity shocks. First, we control for productivity shocks happening before $t + 1$ by adding L productivity lags in the equation. L is determined empirically by looking at the residual autocorrelation; it turns out that adding five lags of productivity reduces residual autocorrelation to negligible levels.

Second, we apply the estimator developed in Olley and Pakes (1996) which proxies productivity shocks by capital investments (results presented in subsection IVB). This approach is potentially useful because it helps isolate contemporaneous correlation between the gap and productivity shock in $t - 1$, which may occur even if the residuals are serially uncorrelated. Third, we repeat our analysis on the subsample of "green field" firms which did not exist in $t - 1$, and thus no past productivity shock could have affected their hiring choices. The results for new firms are reported in subsection IIID. All the approaches produce similar results, implying that the correlation between the receiving firms productivity and the productivity gap cannot be explained by past productivity shocks.

Lastly, there is a possibility that the gap may be correlated with shocks in $t + 1$ if workers can anticipate them in $t - 1$ and apply for jobs in firms with better growth prospects (higher $\overline{\varepsilon}_{j,t+1}$ in equation (4)).[5] If such firms prefer workers from more productive sending firms, these workers will have a higher chance of being selected, resulting in a positive correlation between $\widetilde{\overline{gap}}_{j,t}$ and $\overline{\varepsilon}_{j,t+1}$ and, consequently, an overestimated effect of the gap. In an attempt to control for this correlation, we follow the estimation approach in Olley and Pakes (1996) and add polynomial functions of capital and investments in years t and $t + 1$, assuming that receiving firms are also able to anticipate their productivity shocks and adjust their capital input accordingly. Beyond these controls, having no experimental data or suitable instruments to convincingly identify the gap independently of productivity shocks at $t + 1$, we acknowledge unobserved hiring preferences as a major limitation to interpreting our findings as estimates of the spillover effect alone. Yet, to the extent that preferring observationally identical workers from more productive firms can be explained through the existence of spillovers from these firms, a positive correlation between the gap and the receiving firm's productivity does suggest spillovers through labor mobility, even though it will probably overestimate their magnitude.

II. Baseline Results

Table 5 reports regression results for equation (4) estimated for all manufacturing firms during 1995–2007 with the overall productivity gap as defined in equation (1). Each specification includes five lags of the receiving firm's productivity, required to remove residual autocorrelation, as well as controls: the average size of sending firms, and industry-year fixed effects. The regression results reveal a significant and positive link between the receiving firm's productivity and the gap. For instance, the coefficient 0.201 in column 1 (the specification with no additional controls) implies that a hypothetical firm hiring 10 percent of its workers from 10 percent more productive firms experiences a $0.1 \times 0.1 \times 0.201 = 0.2$ percent productivity gain in the year after hiring.

In the next three specifications, we include firm characteristics (column 2) followed by averages of incumbent (column 3) and newly hired workers (column 4) characteristics.[6] As a result of applying these controls, the coefficient on the gap goes down to 0.125, implying that the gaps effect should not be analyzed independently of the receiving firms characteristics. Interestingly, the inclusion of new workers human capital variable (column 4), measured as the firm average of their wage net of firm-specific effects (see subsection IB), is of no consequence to the estimated gaps effect. Hence, the new workers human capital as we measure it is unlikely to be an explanation behind this result.

Equation (4) implies that the gaps estimates will be equal whether a worker is hired from a more or a less productive firm than the receiving one. In other words, if one of two new workers is hired from a 10 percent less productive firm, and the other from a 10 percent more productive firm, the total effect will be zero (since the overall gap will be zero). While it is possible, in principle, that the benefits of

[5] We are grateful to an anonymous referee for bringing this possibility to our attention.

[6] The coefficient estimates for the control variables are omitted for brevity but are available in the online Appendix.

Table 5—Receiving Firm's Productivity and the Gap: Benchmark Results

	(1)	(2)	(3)	(4)
Productivity gap (β)	0.201***	0.172***	0.122***	0.125***
	(0.038)	(0.038)	(0.039)	(0.039)
Current productivity (α_0)	0.462***	0.455***	0.407***	0.407***
	(0.009)	(0.009)	(0.011)	(0.011)
Lag productivity (α_1)	0.203***	0.198***	0.192***	0.192***
	(0.009)	(0.009)	(0.011)	(0.011)
Lag2 productivity (α_2)	0.089***	0.085***	0.081***	0.081***
	(0.008)	(0.008)	(0.009)	(0.009)
Lag3 productivity (α_3)	0.083***	0.081***	0.078***	0.078***
	(0.007)	(0.007)	(0.009)	(0.008)
Lag4 productivity (α_4)	0.056***	0.052***	0.062***	0.062***
	(0.006)	(0.006)	(0.007)	(0.007)
Firm characteristics	No	Yes	Yes	Yes
Average characteristics of incumbent workers	No	No	Yes	Yes
Average characteristics of new workers	No	No	No	Yes
R^2	0.554	0.558	0.490	0.490
N	74,507	74,507	61,112	61,112

Notes: All specifications include industry-year fixed effects and average size of sending firm as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

hiring workers from more productive firms will be offset by the costs of training up workers from less productive firms, our human capital measure should account for the latter effect. Therefore, since there can be no negative learning, if the gap is to measure potential for spillovers, its estimate should be positive when the gap itself is positive, and zero when the gap is negative.

Table 6 presents results for equation (4) with the positive and negative productivity gaps. The results in column 1 confirm our expectations. The estimate for the negative productivity gap is close to zero and insignificant, implying that hiring workers from less productive firms is neutral to productivity. The estimate on the positive productivity gap is 0.28, two times higher than that on the total gap (0.125) in Table 5 and is significantly larger than the estimate on the negative gap (the p-value of the Wald test of the hypothesis $\beta_P = \beta_N$ is 0.011). Thus, the gap's estimates from Tables 5 and 6 are consistent with each other, since about half of new workers come from less productive firms. Significance of β_P and insignificance of β_N are at odds with the human capital explanation to our findings. Indeed, if positive β_P were due to human capital of new workers coming from more productive firms, β_N would also be positive and equal to β_P, since hiring workers from less productive firms would deteriorate the firm's average labor quality and thus reduce its future productivity. An implication from the results with the positive and negative gaps included separately is that hiring is associated with productivity gains as long as at least some new workers come from more productive firms, even if the average productivity gap across all new hires is negative.

As an illustration to Table 6's estimates, given the mean value of the positive gap, 0.0125 (see Table 3), and its slope coefficient $\beta_P = 0.28$, a firm hiring at the

Table 6—Receiving Firm's Productivity and the Gap Calculated Separately for More and Less Productive Sending Firms

	(1)	(2)	(3)	(4)	(5)
Positive productivity gap (β_P)	0.280***	0.238***	0.869***	0.328***	0.870***
	(0.090)	(0.093)	(0.213)	(0.088)	(0.213)
Negative productivity gap (β_N)	−0.011	−0.020	0.247	0.029	0.265
	(0.078)	(0.083)	(0.216)	(0.089)	(0.209)
Estimation sample	All firms	Small firms	Large firms	Positive hiring	
		($N <= 49$)	($N >= 50$)	($N <= 49$)	($N >= 50$)
R^2	0.491	0.449	0.639	0.455	0.637
N	61,112	51,212	8,636	18,986	8,263

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

mean positive gap gains $0.28 \times 0.0125 = 0.35$ percent in productivity the year after it hired new workers compared to an observationally identical firm that hired no one. This gain is equivalent to the median firm's moving to the 50.4th centile in the productivity distribution if this firm were the only one in the sample to hire workers. Although the gap has an economically significant association with future productivity of the receiving firm, it explains only a small portion of the observed variation in productivity levels across firms. Thus, in the purely autoregressive specification, without the gap and controls, the residual productivity (unlogged) of the ninetieth centile firm is 89 percent higher than that of the tenth centile firm.[7] Adding positive and negative gap measures reduces this difference by 0.39 percent. In comparison, including individual characteristics of new hires ($\bar{\mathbf{Y}}^2$) reduces it by 1.81 percent.

III. Further Extensions

In this section we estimate various extensions of our baseline specification (4) with positive and negative gaps entering separately. We start by re-estimating equation (4) for relatively large ($\geq$ 50 employees) and small ($\leq$ 49 employees) firms separately, in order to find out whether the estimated gap's effect differs with firm size. One reason why the gap's effect might so differ is that better managers, more likely to be found in larger firms (Lucas 1978), may better facilitate the application of the knowledge brought in by new hires. We find (columns 2 and 3 in Table 6) the coefficient on the positive gap for large firms to be larger than for small firms. If the effect of hiring at the mean is to be the same for large and small firms, this apparently spectacular difference in coefficients on the gap is less surprising than it seems, since small (and less productive) firms draw workers from

[7] In their study using the same data as ours, Fox and Smeets (2011) reported the 90 to 10 centile residual productivity difference of 221 percent. Their statistic is larger than ours because the regression specification from which we derived residual productivity also includes autoregressive terms which obviously have large explanatory power.

relatively more productive firms than do large firms. Indeed, running our equation separately for small and large *hiring* firms (that is, firms for which the gap is nonzero) (columns 4 and 5), we find that, with the mean values of the positive gap equal to 0.034 and 0.014, respectively, the implied productivity gains from hiring at the mean for the two groups of firms are similar: 1.11 percent for small and 1.21 percent for large firms.

Although the coefficient on the negative gap for large firms is also bigger than for small, it is still not even marginally significant and is considerably smaller than the coefficient on the positive gap (the *p*-value of the Wald test of the hypothesis $\beta_P = \beta_N$ is 0.03). In fact, the low magnitude of the negative gaps coefficient, relative to that on the positive gap and its low significance, persist in every further extension of our main specification. Since this coefficient is not precise enough to support or refute any particular hypothesis, we will focus on the positive gaps coefficient in further interpretations of our findings. This coefficient is of reasonable magnitude and significance, lending itself more easily to interpretations within our assumed theoretical framework and its extensions.

A. *Results for Worker Movements within and between Industry Sectors*

Knowledge can be general or specific to a particular firm or industry. Our previous results suggest, assuming the spillovers story, that knowledge coming with new hires is general enough to be applied in different firms. In this subsection, we want to see whether, and to what extent, this knowledge can overcome technological barriers between different industries. Accordingly, we differentiate between the gap calculated for workers moving within the same two-digit industry (NACE classification), and the gap for workers moving between industries, running an extension of the baseline equation (4) as follows:

$$(6)\qquad A^r_{j,t+1} = \sum_{k=0}^{L-1} \alpha_k A^r_{j,t-k} + \beta_{diff} \cdot \widetilde{\overline{gap}}{}^{diff}_{j,t-1} + \beta_{same} \cdot \widetilde{\overline{gap}}{}^{same}_{j,t-1} + \mathbf{X}_{j,t}\gamma_1 + \overline{\mathbf{Y}}^1_{j,t}\gamma_2 + \overline{\mathbf{Y}}^2_{j,t}\gamma_3 + \overline{\varepsilon}_{j,t+1},$$

where $\widetilde{\overline{gap}}{}^{same}_{j,t-1} = \sum_{i=1}^{N_{j,t}} I^{same}_{i,t}(A^s_{i,t-1} - A^r_{i,t-1})/N_{j,t}$, $\widetilde{\overline{gap}}{}^{diff}_{j,t-1} = \sum_{i=1}^{N_{j,t}} (1 - I^{same}_{i,t}) \times (A^s_{i,t-1} - A^r_{i,t-1})/N_{j,t}$, and $I^{same}_{i,t}$ is an indicator variable which takes the value of one if worker *i* moved from one firm to another in year *t* within the *same* two-digit industry, and zero otherwise. That is, $\widetilde{\overline{gap}}{}^{same}_{j,t-1}$ and $\widetilde{\overline{gap}}{}^{diff}_{j,t-1}$ are productivity gaps for workers moving within and between industries, respectively, weighed by their respective shares in the receiving firms workforce. Variables in $\overline{\mathbf{Y}}^2$ are also redefined separately for the workers coming from within and outside the industry where the receiving firm belongs. There are nine two-digit industries in the manufacturing sector, and 55 percent of all job changes took place between firms operating in the same industry.

If knowledge transfer by new hires can overcome technology borders between industries, the coefficients β^{same} and β^{diff} should be equal. In fact, as Table 7 shows,

Table 7— Receiving Firm's Productivity and the Gap Calculated for Same and Different Industries

	(1)	(2)
Productivity gap, same industry (β_{same})	0.182***	
	(0.053)	
Productivity gap, different industry (β_{diff})	0.075	
	(0.057)	
Positive productivity gap, same industry ($\beta_{P,same}$)		0.421***
		(0.117)
Positive productivity gap, different industry ($\beta_{P,diff}$)		0.184**
		(0.090)
Negative productivity gap, same industry ($\beta_{N,same}$)		0.108
		(0.092)
Negative productivity gap, different industry ($\beta_{N,diff}$)		−0.124
		(0.081)
Test $\beta_{same} = \beta_{diff}$, p-value	0.166	
Test $\beta_{P,same} = \beta_{P,diff}$, p-value		0.022
Test $\beta_{N,same} = \beta_{N,diff}$, p-value		0.018
R^2	0.491	0.492
N	59,665	59,665

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007. Firms are considered to be in the same industry if they have the same 2-digit NACE industry code.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

the gap's estimate is much larger for workers moving within the same industry. Breaking down both within- and between-industries productivity gaps onto their positive and negative parts (column 2), we find that only positive productivity gaps are significant. Assuming the spillover interpretation of our results, the fact that the effect of hiring workers from more productive firms within the same sector (0.421) is twice as high as for workers from other sectors (0.184) implies that knowledge brought in by new workers is in large part industry-specific. Hiring within the same industry thus brings more relevant new knowledge than what can be learned from workers previously employed outside.

B. *Results by Worker Education and Occupation*

Previous research has argued that there are differences in the ability of workers to transfer and apply new knowledge depending on their occupation (Song, Almeida, and Wu 2003) and education (Kaiser, Kongsted, and Rønde 2008). In this subsection, we apply insights from these studies to ascertain whether new hires education and occupation within their sending firms influence the strength of the receiving firm's productivity correlation with the gap. Starting with education, we classify the new workers' educational attainment into three categories: high school, college (or a comparable vocational degree), and university degree.

We thus respecify equation (4) to include interactions between gap and education level dummies as follows:

$$(7) \qquad A^{r}_{j,t+1} = \sum_{k=0}^{L-1} \alpha_k A^{r}_{j,t-k} + \beta_{high} \cdot \widetilde{\overline{gap}}^{high}_{j,t-1} + \beta_{coll} \cdot \widetilde{\overline{gap}}^{coll}_{j,t-1} + \beta_{univ} \cdot \widetilde{\overline{gap}}^{univ}_{j,t-1} + \mathbf{X}_{j,t}\gamma_1 + \overline{\mathbf{Y}}^{1}_{j,t}\gamma_2 + \overline{\mathbf{Y}}^{2}_{j,t}\gamma_3 + \overline{\varepsilon}_{j,t+1},$$

where $\widetilde{\overline{gap}}^{l}_{j,t-1} = \sum_{i=1}^{N_{j,t}} I^{l}_{i,t}(A^{s}_{i,t-1} - A^{r}_{i,t-1})/N_{j,t}$, l is education level, $l = \{high\ school; college; university\}$, and $I^{l}_{i,t}$ is an indicator variable which takes the value of one if worker i with education level l was hired by firm j in period t. Hence, $\widetilde{\overline{gap}}^{l}_{j,t-1}$ is the average productivity gap for education group l weighed by its share in total workforce. (We redefine new workers characteristics in the same way.) If more educated workers can convey knowledge better, then we would expect $\beta_{high} < \beta_{coll} < \beta_{univ}$, meaning that higher educated workers will transfer more knowledge and hence will cover a larger share of the productivity gap between their old and new employers.

Table 8 presents estimation results for equation (7). The estimate of the productivity gap is stronger for new workers with college or university degrees. For instance, for college graduates the coefficient on the gap is a third greater than for high school graduates, and the same coefficient for university graduates is almost 2.5 times as large. Even though the difference between these coefficients is not statistically significant, there is a clear tendency for them to increase with the level of education. Breaking the productivity gaps by education level down to positive and negative parts (column 2), we observe the same tendency for the positive gap's coefficient to grow with education level. For instance, hiring 10 percent of the workforce from a 10 percent more productive firm is associated with a 0.655 percent productivity increase in the receiving firm if all new hires have a university degree, with about 0.315 percent if they have a college degree, and by 0.214 percent if they have a high school diploma.

Turning to differences in the gap's estimates by worker occupation in the sending firm, we divide all workers into four categories: low-skilled, mid-skilled, high-skilled, and managers. This classification follows Statistics Denmark's definitions based on the International Standard Classification of Occupations. We estimate the following modification of equation (4):

$$(8) \qquad A^{r}_{j,t+1} = \sum_{k=0}^{L-1} \alpha_k A^{r}_{j,t-k} + \beta_{low} \cdot \widetilde{\overline{gap}}^{low}_{j,t-1} + \beta_{mid} \cdot \widetilde{\overline{gap}}^{mid}_{j,t-1} + \beta_{high} \cdot \widetilde{\overline{gap}}^{high}_{j,t-1} + \beta_{manager} \cdot \widetilde{\overline{gap}}^{manager}_{j,t-1} + \mathbf{X}_{j,t}\gamma_1 + \overline{\mathbf{Y}}^{1}_{j,t}\gamma_2 + \overline{\mathbf{Y}}^{2}_{j,t}\gamma_3 + \overline{\varepsilon}_{j,t+1},$$

where $\widetilde{\overline{gap}}^{l}{}_{j,t-1} = \sum_{i=1}^{N_{j,t}} I^{l}_{i,t}(A^{s}_{i,t-1} - A^{r}_{i,t-1})/N_{j,t}$, l is the occupation group, $l = \{low\text{-}skilled; mid\text{-}skilled; high\text{-}skilled; manager\}$, and $I^{k}_{i,t}$ is an indicator variable which takes the value of one if worker i's occupation at the new firm belongs to group l.

TABLE 8—RECEIVING FIRM'S PRODUCTIVITY AND THE GAP BY NEW WORKERS' EDUCATION LEVEL

	(1)	(2)
Productivity gap, high school (β_{high})	0.120**	
	(0.053)	
Productivity gap, college (β_{coll})	0.160***	
	(0.056)	
Productivity gap, university (β_{univ})	0.285**	
	(0.121)	
Positive productivity gap, high school ($\beta_{P,high}$)		0.214**
		(0.106)
Positive productivity gap, college ($\beta\beta_{coll}$)		0.315***
		(0.110)
Positive productivity gap, university ($\beta_{P,univ}$)		0.655***
		(0.153)
Negative productivity gap, high school ($\beta_{N,high}$)		0.039
		(0.121)
Negative productivity gap, college ($\beta_{N,coll}$)		0.198
		(0.130)
Negative productivity gap, university ($\beta_{N,univ}$)		−0.163
		(0.146)
Test $\beta_{high} = \beta_{coll}$, p-value	0.658	
Test $\beta_{high} = \beta_{univ}$, p-value	0.699	
Test $\beta_{P,high} = \beta\beta_{coll}$, p-value		0.524
Test $\beta_{P,high} = \beta_{P,univ}$, p-value		0.384
R^2	0.491	0.492
N	59,665	59,665

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 9 reports estimation results. At first (column 1), results seem to be mixed as the coefficient on the productivity gap of high-skilled workers is as low as for the low-skilled group. However, when we look at the results by occupation group separately for more and less productive sending firms (column 2), the estimates follow the same pattern as for the education groups (Table 8). Productivity gains in firms hiring only skilled workers from more productive firms are nearly three times higher than in firms hiring only mid-skilled workers, and firms hiring managers experience even larger productivity gains. Hiring low-skilled workers, even from more productive firms, is not linked to perceptible productivity gains.

In sum, productivity gains associated with hiring are larger when new hires are more educated and belong to higher occupation group, which makes sense from the spillovers point of view, since they had more opportunity to accumulate knowledge in their previous firms and apply it with their new employers. These workers should therefore be more attractive to potential employers. However, that even mid-skilled workers can transfer some valuable knowledge suggests that knowledge transferred through hiring does not have to be of a particularly sophisticated nature. Since this

TABLE 9—RECEIVING FIRM'S PRODUCTIVITY AND THE GAP BY NEW WORKERS' SKILL LEVEL

	(1)	(2)
Productivity gap, low-skilled (β_{low})	0.057	
	(0.065)	
Productivity gap, mid-skilled (β_{mid})	0.121***	
	(0.045)	
Productivity gap, high-skilled (β_{high})	0.080	
	(0.133)	
Productivity gap, managers ($\beta_{manager}$)	0.617	
	(0.370)	
Positive productivity gap, low-skilled ($\beta_{P,low}$)		0.022
		(0.136)
Positive productivity gap, mid-skilled ($\beta_{P,mid}$)		0.300***
		(0.095)
Positive productivity gap, high-skilled ($\beta_{P,high}$)		0.855***
		(0.224)
Positive productivity gap, managers ($\beta_{P,manager}$)		1.403***
		(0.309)
Negative productivity gap, low-skilled ($\beta_{N,low}$)		0.008
		(0.137)
Negative productivity gap, mid-skilled ($\beta_{N,mid}$)		0.081
		(0.101)
Negative productivity gap, high-skilled ($\beta_{N,high}$)		−0.289
		(0.221)
Negative productivity gap, managers ($\beta_{N,manager}$)		−0.292
		(1.324)
Test $\beta_{low} = \beta_{mid}$, p-value	0.419	
Test $\beta_{low} = \beta_{high}$, p-value	0.878	
Test $\beta_{low} = \beta_{manager}$, p-value	0.078	
Test $\beta_{P,low} = \beta_{P,mid}$, p-value		0.095
Test $\beta_{P,low} = \beta_{P,high}$, p-value		0.002
Test $\beta_{P,low} = \beta_{P,manager}$, p-value		0.000
R^2	0.491	0.492
N	59,665	59,665

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

knowledge is unlikely to be patented or otherwise codified, labor mobility seems a plausible mechanism of its transfer.

C. *The Gap and Productivity Dynamics*

As another extension to our baseline results, we study the duration of the estimated effect of the gap on the receiving firm's productivity. Our baseline specification (4) implies that hiring workers from more productive firms in the current year is linked to productivity gains in the next year. However, since productivity is an autoregressive process, the effect of a one-off hire will carry over a number of years through the autoregression terms and can be estimated from the impulse response function.

For instance, given the estimates for the AR(5) terms in equation (4) (see Table 5), the implied impulse response estimates for the effect of the positive productivity gap in year $t-1$ on the productivity in years $t+2$ to $t+5$ are 0.111, 0.098, 0.083, and 0.084, respectively. Yet, the gap's effect does not have to propagate exclusively through autoregression and can have its own dynamics. For instance, if the gap's effect is more persistent than a generic shock transmitted through the AR(5) process for productivity in (4), the impulse response function will underestimate its future effects.

Our adopted method of estimating the dynamics of the gap's effect is based on the following forecast equation:

$$(9) \qquad A^r_{j,t+p} = \sum_{k=0}^{L-1} \alpha_k A^r_{j,t-k} + \sum_{i=0}^{p-2} \delta_i A^s_{j,t+i} + \beta^p \widetilde{\overline{gap}}_{j,t-1} + \mathbf{X}_{j,t}\gamma_1^p + \overline{\mathbf{Y}}^1_{j,t}\gamma_2^p + \overline{\mathbf{Y}}^2_{j,t}\gamma_3^p + \overline{\varepsilon}_{j,t+p}, p > 1.$$

This method, also known as local projections (Jordà 2005), is easy to implement and is more robust to possible dynamic misspecifications in the original regression equation than estimating impulse responses. Because the autoregressive terms in equation (9) are recursively substituted with their respective values p years back $\left(\sum_{k=0}^{L-1} \alpha_k A^r_{j,t-k}\right)$, the coefficient β^p measures the response of the receiving firms productivity in year $t+p$ to the productivity gap in $t-1$, both through autoregression and own dynamics. Note that specification (9) is augmented with the leads of sending firms productivity $\left(\sum_{i=0}^{p-2} \delta_i A^s_{j,t+i}\right)$ to control for the effects of hiring in future years.

The estimation results for equation (9) are presented in Table 10. Moving the dependent variable forward in time in specification (9) reduces the sample size not only due to lost observations, but also because some firms exit the market. Therefore, for consistency, we estimate the dynamics of the gap's effect using only the subsample of firms with observations available for at least ten consecutive years. In the first two columns of Table 10 panel B, the positive gap's coefficient (0.371) is higher than that estimated on the entire sample (0.280, see Table 6). However, the subsample in Table 10 is also different from the entire sample in that it consists of relatively long-lived firms which are bigger and more productive than average, and for which the gap's coefficient is bigger (see Table 6).[8] Therefore, the estimates of the positive gap's dynamic effect in columns 2, 3, 4, and 5 will be compared with the appropriate benchmark coefficient in column 1.

Table 10 shows that the gap's coefficient continues to be significant for four years after the hiring is done, with the associated productivity gains being three to four times larger than those implied by the impulse response function alone. Consistent with our previous findings, this lasting, though not permanent, effect rests on hiring workers from more productive firms. To illustrate, our estimates imply that a firm which hired at the average positive gap (0.011) just once, in year *t*, is about 0.4 percent more productive in years $t+1$ to $t+4$ than a comparable firm which

[8] The average positive gap on this subsample is also smaller (0.011 versus 0.0125 for the entire sample), and the productivity gain one year after hiring linked to hiring at the mean is about the same: $0.371 \times 0.011 = 0.4$ percent compared to 0.35 percent on the entire sample.

Table 10—Dynamics in the Gap's Effect

Dependent variable:	Productivity in 1 year (1)	Productivity in 2 years (2)	Productivity in 3 years (3)	Productivity in 4 years (4)	Productivity in 5 years (5)
Panel A					
Productivity gap (β)	0.151***	0.207***	0.148	0.094	0.028
	(0.049)	(0.063)	(0.084)	(0.065)	(0.062)
R^2	0.577	0.513	0.434	0.382	0.324
N	22,160	22,160	22,160	22,160	22,160
Panel B					
Positive productivity gap (β_P)	0.371***	0.394***	0.372***	0.351**	0.168
	(0.147)	(0.123)	(0.122)	(0.147)	(0.131)
Negative productivity gap (β_N)	0.034	0.196	0.174	−0.021	−0.077
	(0.086)	(0.153)	(0.229)	(0.141)	(0.130)
R^2	0.578	0.514	0.434	0.384	0.326
N	22,160	22,160	22,160	22,160	22,160

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007. In all specifications the sample is restricted to the one used in the estimation in columns 9 and 10.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

never hired, after which time this productivity difference falls and becomes insignificant. If, however, the firm keeps hiring at the mean gap for four consecutive years, the cumulative productivity gain in the fifth year is $0.011 \times (0.371 + 0.394 + 0.372 + 0.351) = 1.64$ percent over the productivity of a similar firm which did not hire during the same period. All else equal, for the median firm this gain is equivalent to a 2.3 centile move up the productivity distribution.

D. *The Gap and the Productivity of New Firms*

In this subsection we look at the link between the gap and productivity of startups, which will extend our results in two ways. First, hiring the right workers can present a valuable opportunity for small and less productive firms to learn from their more successful competitors. Startups thus present an appropriate setting to study this effect, since new entrants are typically small and less productive and have a lot to learn from more experienced firms. Second, as discussed in the empirical model subsection, past productivity shocks can affect firms hiring strategy and, in particular, stimulate firms to seek new workers with characteristics more likely to be observed among the personnel of more productive firms. For startup firms, there is no performance history, and hence no feedback from past events to current hiring practices.

We run the following modification of model (4) on the subsample of startups:

$$A^r_{j,t+1} = \alpha_1 A^r_{j,t} + \beta \cdot \widetilde{\overline{A^s_{j,t-1}}} + \mathbf{X}_{j,t}\gamma_1 + \overline{\mathbf{Y}}^2_{j,t}\gamma_3 + \overline{\varepsilon}_{j,t+1}, \tag{10}$$

where own productivity lags and incumbent workers' characteristics $(\overline{\mathbf{Y}}^1_{j,t})$ have been removed due to their absence for startups. Similarly, because the productivity

TABLE 11—THE LINK BETWEEN SENDING AND RECEIVING FIRMS' PRODUCTIVITY FOR NEWLY ESTABLISHED FIRMS

	Future productivity		Probability of survival in the first year after start	
Dependent variable:	(1)	(2)	(3)	(4)
Productivity of sending firm	0.197***		0.154***	
	(0.017)		(0.053)	
			[0.028]	
Productivity of sending firm above industry average		0.247***		0.215**
		(0.023)		(0.098)
				[0.038]
Productivity of sending firm below industry average		0.095**		0.117
		(0.041)		(0.081)
				[0.021]
R^2	0.294	0.296	0.024	0.024
N	7,147	7,147	6,519	6,519

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. Marginal effects are in square brackets. The time period covered is 1995–2007. Columns 1–2 are estimated with OLS; columns 3–4 are estimated with Probit.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

gap cannot be constructed for startup firms, the relationship we explore is the one between own future productivity $(A^r_{j,t+1})$ and the average productivity of sending firms, $\widetilde{\widetilde{A}}^s_{j,t-1} = \sum_{i=1}^{N_{j,t}} I_{i,t} A^s_{i,t-1} / N_{j,t}$ weighed by the share of new workers. The coefficient β in equation (10) is nevertheless comparable to the β in equation (4) specified for a firm with an average productivity in year $t - 1$ $(Ar_{j,t-1} = 0)$.

Estimation results for equation (10) are presented in column 1 of Table 11. Positive and significant coefficient β corroborates our earlier findings on the entire sample of firms. At the hiring rate of 100 percent for startups, a 10 percent increase in the average productivity of the sending firms is linked to a 1.97 percent increase in the productivity of the receiving firm. Breaking the sending firms down to above- and below the manufacturing sector's average productivity (column 2), the closest we can get to positive and negative gaps included separately, we observe the same tendency as before: the productivity gain linked to hiring from more-than-average productive firms is stronger than when new hires come from less productive firms.

Finally, in columns 3 and 4 we report estimates of the correlation between sending firms' average productivity and the probability of the receiving firm's survival in the first year after its start. We find this correlation to be positive (column 3) but significant only for hires from firms with above average productivity (column 4). To gauge the importance of sending firms' productivity for a startup's survival, we calculate its implied marginal effect for an average receiving firm (shown in square brackets in Table 11). The estimated marginal effect of hiring from firms with above-average productivity (column 4 in Table 11) implies that a 10 percent increase in sending firms' productivity is linked to a 0.38 percentage points higher probability of the receiving firm's survival, a small increase compared to the first year exit frequency of 16.7 percent.

IV. Robustness Tests

A. *Contemporaneous Correlation Between Sending and Receiving Firms' Productivities*

Every regression specification so far has assumed that productivity of the sending firm in year $t - 1$ (the year before the move) is linked with productivity of the receiving firm in year $t + 1$, the year after the move. Indeed, because spillover effects take time, a correlation between sending and receiving firms' productivities instantaneous to hiring workers in year t will not be consistent with the spillovers hypothesis, implying instead that our gap measure captures other factors codetermining the sending and receiving firms' performance. To control for this possibility, we augment equation (4) with sending firms' productivity in year $t + 1$. Firm and worker characteristics in year $t + 1$ are included as well.

The estimates from the thus augmented specification, shown in column 1 of Table 12, imply that recruiting 10 percent of total workforce from 10 percent more productive firms is associated with an 0.056 percent instantaneous increase in productivity of the receiving firm, reflecting the weak positive link between sending and receiving firms' productivity reported in the introduction. This contemporaneous correlation is weak relative to the estimated gap's effects on productivity in years $t + 1$ to $t + 5$, and is not statistically significant. Adding it does not materially affect our earlier estimates. The insignificance of sending firms' productivity in $t + 1$ implies that the contemporaneous link between sending and receiving firms' productivities can be explained away by controlling for similarities in observed firm and worker characteristics, which we routinely do. Hence, common factors affecting sending and receiving firms' productivities cannot explain our main result.

B. *Productivity Shocks in Receiving Firms*

The gap's effect is identified on the assumption that it is uncorrelated with the error term $\overline{\varepsilon}_{j,t+1}$ in equation (4). Because this assumption clearly fails when the error term is serially correlated, in which case the gap will pick up the receiving firm's past productivity shocks, we have added lags of the dependent variable whenever possible to eliminate this correlation. However, even in the absence of serial correlation in the error term, one can think of plausible situations in which our identification assumption may be violated. For instance, a one-off shock to receiving firm's productivity in year $t - 1$ may affect its hiring behavior in year t, when the new hires come, by improving its ability to attract workers from more productive firms. The positive correlation between the productivity shock in $t - 1$ and the gap will induce an upward bias to the estimate of the gap's effect.

To eliminate this correlation, we apply the estimation procedure developed in Olley and Pakes (1996) which relies on the theoretical result that a profit-maximizing firm will increase capital investment at the time of experiencing a positive productivity shock. Assuming firm j's current capital stock K_{jt} is equal to the sum of capital from the previous period and capital investments, the profit-maximizing

TABLE 12—RECEIVING FIRM'S PRODUCTIVITY AND THE GAP WITH CONTROLS FOR PRODUCTIVITY SHOCKS AND FIRM FIXED EFFECTS

	(1)	(2)	(3)	(4)	(5)
Panel A					
Productivity gap (β)	0.149***	0.126***	0.122***	0.071***	0.113
	(0.043)	(0.040)	(0.038)	(0.027)	(0.103)
Mean future productivity of sending firms	0.056				
	(0.043)				
X, Y1, and Y2 for period $(t+1)$	Yes	No	No	No	No
Control for TFP shocks with investments	No	Yes	Yes	No	No
Firm-level fixed effects	No	No	No	Yes	Yes
R^2	0.497	0.490	0.507		
N	48,867	59,655	58,339	59,655	89,907
Panel B					
Positive productivity gap (β_P)	0.328***	0.278***	0.254***	0.185***	0.321**
	(0.090)	(0.090)	(0.091)	(0.064)	(0.164)
Negative productivity gap (β_N)	0.032	0.002	0.023	−0.002	0.019
	(0.072)	(0.078)	(0.075)	(0.057)	(0.094)
Mean future productivity of more productive sending firms	0.010				
	(0.073)				
Mean future productivity of less productive sending firms	0.095				
	(0.068)				
X, Y1, and Y2 for period $(t+1)$	Yes	No	No	No	No
Control for TFP shocks with investments	No	Yes	Yes	No	No
Firm-level fixed effects	No	No	No	Yes	Yes
R^2	0.498	0.490	0.507		
N	48,867	59,655	58,339	59,655	89,907

Notes: All specifications include industry-year fixed effects, average size of the sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007. In column 2, a third order polynomial of investments and capital is included to control for firm level productivity shocks in period *t*. Column 3 includes a third order polynomial of capital and investments to control for productivity shock at time periods *t* and $(t+1)$. The system-GMM estimator in column 5 includes three lags of the dependent variable, the lag length of 4 and 5 for the equation in differences and the lag length of 3 for the equation in levels. The *p*-value of the test statistic for autocorrelation in residuals is always greater than 0.1; the *p*-value for the overidentificaion test is always less than 0.05.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

choice of investment, I_{jt}, is described as a function $f(\cdot)$ of the current capital stock and the unobserved productivity shock ω_{jt} as follows:

$$I_{jt} = f(K_{jt}, \omega_{jt}).$$

Assuming $f(\cdot)$ is monotonic in both capital and productivity shock, it can be inverted to express ω_{jt} as a function of observable investments and capital (all in logs):

$$\omega_{jt} = g(k_{jt}, i_{jt}).$$

As in Olley and Pakes (1996), we approximate the unknown function g with a third-degree polynomial series of capital and investments and include this approximation in equation (4) to control for ω_{jt-1}. The estimation results presented in column 2 of Table 12 do not suggest that past productivity shocks affect the choice of firms from which the new hires come, since the estimates for the gap are similar to those obtained earlier from specifications without extra controls for unobserved productivity shocks. That said, these extra controls do help explain the variation in productivity among firms, since most of the terms in the polynomial approximation for function $g(\cdot)$ (not shown) are statistically significant.

Continuing with the issue of productivity shocks in receiving firms, even when past and contemporaneous shocks have been controlled away, the gap may still be correlated with productivity shocks happening in the future if job movers can anticipate future productivity developments and choose to apply to firms where productivity prospects are brightest. These firms will then have more applicants than vacancies, and if they prefer to hire workers from more productive firms there will be a positive correlation between the productivity shock in year $t + 1$, $\overline{\varepsilon}_{j,t+1}$, and the gap, leading to an overestimate of its effect. Once again we use the insight from Olley and Pakes (1996) to control for this future productivity shock by a polynomial function of capital stock and investments in years t and $t + 1$.

The results of this exercise, shown in column 3, are very similar to our previously reported findings, although we hasten to add that our controls for future productivity shocks may be weak. In the absence of more elaborate tools to rule out this "anticipated shocks" explanation behind our results, we do acknowledge that our findings fall just short of a straight knowledge spillovers story. There may be a number of factors escaping our control that contribute to the preference for workers from more productive firms. If spillovers are one of these factors, our results should be taken as evidence for their presence. However, the importance of spillovers in influencing hiring preferences is unknown.

C. *Unobserved Firm-Level Heterogeneity*

Continuing on the issue of identifiability of productivity spillovers separately from other unobserved influences, suppose now that firms do not hire workers randomly but target sending firms with particular characteristics. For instance, some domestic firms may prefer to hire workers from multinationals in pursuit of productivity gains from their knowledge. If the (long-term) stable preferences in hiring reflect certain management practices, their presence may result in a correlation between the sending and receiving firms productivities over time which is not due to the productivity gap as such. Although, by construction, the gap cancels common influences to the receiving and sending firms' productivities, as an extra control for their long-term productivity determinants, we add the receiving firms' fixed effects as a robustness check.

Including the receiving firm fixed effects leads to a considerable reduction in the gap's coefficients (column 4 of Table 12). The fixed effects estimate for the positive productivity gap, 0.185, implies a productivity gain of 0.23 percent for an average firm, which is a third less than the 0.35 percent implied by the baseline specification (Section II). However, we caution against relying too much on these estimates because of the fixed effects estimator's known downward bias to the coefficients on the lags of the dependent variable. Because the gap is by construction negatively correlated with the receiving firm's productivity in year $t - 1$, a downward bias to the coefficient on $A^r_{j,t-1}$ will result in a downward bias to the coefficient on the gap as well. To try to correct for this bias, we apply the system GMM estimator proposed by Blundell and Bond (1998). This estimator works by exploiting the moment conditions involving the error term and lags of the dependent variable acting as instruments.

The moment conditions we exploit are as follows:

$$(11) \qquad E[z_{j,t-s}\Delta\varepsilon_{jt}] = 0 \quad \text{and} \quad E[x_{j,t}\Delta\varepsilon_{jt}] = 0 \quad \text{for } s = 4,5$$

$$(12) \qquad E[\Delta z_{j,t-s}\varepsilon_{jt}] = 0 \quad \text{and} \quad E[\Delta x_{j,t}\varepsilon_{jt}] = 0 \quad \text{for } s = 3,$$

where $z_{j,t} = (A^r_{j,t})$, $x_{j,t} = \left(\widetilde{gap}_{j,t-1}, \mathbf{X}_{j,t}, \overline{\mathbf{Y}}^1_{j,t}, \overline{\mathbf{Y}}^2_{j,t}\right)$, and $\Delta x_{j,t} = x_{j,t} - x_{j,t-1}$. The first set of moment conditions (11), as in Arellano and Bond (1991), is obtained from first-differencing equation (4), which removes firm fixed effects and makes past lags of productivity orthogonal to the first-differenced error term $\Delta\varepsilon_{jt}$. The second set of moment conditions (12) comes from the original equation (4) where lagged *differences* of the dependent variable are used as instruments for the lagged productivity levels. Blundell and Bond (1998) demonstrate that exploiting the additional moment conditions in (4) can lead to a dramatic improvement in estimation efficiency, especially when the autoregression process for productivity shows high persistence as we demonstrate in subsection IIIC. The moment conditions were chosen so as to eliminate residual autocorrelation (which would invalidate them). The presence of residual autocorrelation under a given set of moment conditions can be tested, and in this specification, three lags of the dependent variable were enough to eliminate it.

The system GMM estimates in column 5 of Table 12 are similar to the previously reported OLS results, indicating that recruiting workers from more productive firms is associated with future productivity growth. The implied productivity gain for a firm hiring at the mean gap is 0.4 percent the year after the hiring, very close to the previously reported estimates. However, the system GMM specification is not without problems of its own. Apart from the requirement of no autocorrelation in the error term, which it satisfies, another condition for its consistency is that the instruments employed in the moment conditions must all be exogenous. We were unable to find a configuration of the moment conditions that would comply with this requirement at an acceptable level of the overidentification test statistic, implying that some of the instruments that we employ in the moment conditions may be correlated with the error term. In fact, several previous studies (Griliches and Mairesse 1995; Levinsohn and Petrin 2003) also encountered technical problems implementing system GMM. We therefore stick to OLS regression results as our preferred specification.

D. *Human Capital Revisited*

Recall the human capital interpretation of our results that we have considered before. Namely, because more productive workers are more likely to be found in above-average firms, the gap may absorb the effect of unobservable skills of the new workers as well as spillovers. So far we have two pieces of evidence against the human capital explanation of our results: the gap's effect hardly changing after controlling for human capital, and the gap's effect being different for the positive and negative gaps. In this subsection we devise yet another test for the human capital versus the spillovers story which is robust to the possibility that workers' skills may not be fully reflected in the salary. This possibility may arise, for instance, when more productive firms screen high-quality workers better.

We look at the effects of the positive gap defined for moving workers with different length of tenure at their sending firms. Suppose that tenure at the sending firm along with education and skill level (recall subsection IIIB) facilitates spillovers by improving a worker's ability to accumulate knowledge. Then the coefficient on the gap should increase with tenure. If, however, the gap's coefficient does not change with tenure, two possibilities arise: (i) tenure does not facilitate spillovers, or (ii) the gap's coefficient measures not spillovers but the effect of unobserved and uncompensated skills of new workers coming from more productive firms.

Since we do not have information on workers' employment outside our sample period, the constructed measure of tenure will be censored for early years. For this reason, we focus on the time period between 2001 and 2007, which will enable us to derive a reasonably uncensored tenure variable for the first six years. With this measure, we construct average positive productivity gaps for six groups of new workers who have tenure in the sending firm varying from 1 to 6-plus years, the last group including all observations with censored tenure.

Results presented in Table 13 show a tendency for the gap's coefficient, and its significance, to increase with new workers' tenure at their sending firms, which is consistent with the spillovers hypothesis. However, because the gap's coefficients by tenure are estimated rather imprecisely, the restriction that they are equal cannot be rejected. Therefore, on the basis of this evidence alone we cannot decisively reject the alternative, "uncompensated skills," explanation. Nor can we accept this alternative explanation, since the underlying assumption that longer-tenured workers are better able to accumulate knowledge may be wrong. In any case, the significant difference between the coefficients on the positive and negative gaps remains the only strong evidence in favor of the spillovers versus the human capital story behind our results.

E. *Alternative Measures of Productivity*

Until now we have measured productivity as value added per worker. Although this measure is widely used, it disregards differences in capital intensity. In this subsection we use several alternative measures of economic performance, defined relative to a multifactor production technology, which take into account the intensity of factors of production other than labor. With firm level output Q, production technology F, and the vector of input factors X, we can define the total factor productivity (TFP) of firm j at time t, A_{jt}, as its output net of input factor contributions:

$$a_{jt} = q_{jt} - f(x_{jt}), \tag{13}$$

where $z = \ln Z$. Using different parametrizations of the production function and estimates of its coefficients, we construct several measures of TFP. To remove common

TABLE 13—RECEIVING FIRM'S PRODUCTIVITY AND THE GAP BY LENGTH OF TENURE AT SENDING FIRM

Positive productivity gap for workers with 1 year of experience at sending firm	0.196
	(0.175)
Positive productivity gap for workers with 2 years of experience at sending firm	0.263
	(0.183)
Positive productivity gap for workers with 3 years of experience at sending firm	0.288**
	(0.134)
Positive productivity gap for workers with 4 years of experience at sending firm	0.263**
	(0.131)
Positive productivity gap for workers with 5 years of experience at sending firm	0.410***
	(0.151)
Positive productivity gap for workers with 6+ years of experience at sending firm	0.333***
	(0.106)
R^2	0.497
N	44,496

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), and firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 2001–2007.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

influences on productivity, we demean a_{jt} by industry-year averages defined at the 4-digit level of the NACE classification.

As a first parametrization of $F(\cdot)$ we take the Cobb-Douglas production function with capital (k), labor (l), and materials and energy (m) inputs as the factors of production, calculating

$$a_{it} = q_{it} - \alpha_k k_{it} - \alpha_l l_{it} - \alpha_m m_{it}. \tag{14}$$

We apply a selection of empirical methodologies to estimate the parameters of Cobb-Douglas production technology, $\alpha_k, \alpha_l, \alpha_m$. Our starting point is the ordinary least squares (OLS) estimator of the production function, which we apply separately for each three-digit industry group in our sample, to allow for differences in input elasticities within the sample. The regression results with TFP estimated with OLS are reported in column 1 of Table 14. The results with this alternative measure of productivity are consistent with our benchmark results for value added per worker. The coefficient on the productivity gap and its positive part are positive and statistically significant, while the coefficient for new workers coming from less productive firms is insignificant. In addition to the OLS estimates of TFP, we employ a two-step semiparametric estimator by Olley and Pakes (1996) to control for input factor endogeneity to unobserved past productivity shocks. The TFP measure based on Olley and Pakes-estimated production function is highly correlated with that based on OLS. Consequently, the changes in the estimates of our interest (column 2) are fairly small.[9]

[9] Alternative estimates of Cobb-Douglas production function, such as Levinsohn and Petrin (2003) and Wooldridge (2009), yield productivity measures which are very similar to OLS and Olley-Pakes estimates.

TABLE 14—RESULTS FOR ALTERNATIVE MEASURES OF FIRM PERFORMANCE

	Productivity measure					
	OLS (1)	OP (2)	Translog (3)	Profit (4)	Profit (5)	Profit (6)
Panel A						
Productivity gap (β)	0.076***	0.102***	0.035	0.018	0.034	0.038
	(0.033)	(0.036)	(0.031)	(0.014)	(0.099)	(0.040)
R^2	0.321	0.301	0.332	0.543	0.540	0.527
N	59,362	59,362	59,362	83,477	41,265	40,382
Panel B						
Positive productivity gap (β_P)	0.101***	0.166***	0.186***	0.310***	0.312**	0.207***
	(0.039)	(0.078)	(0.068)	(0.078)	(0.151)	(0.050)
Negative productivity gap (β_N)	0.024	0.042	−0.055	−0.101***	−0.130	−0.118
	(0.036)	(0.047)	(0.070)	(0.033)	(0.0830)	(0.112)
R^2	0.322	0.302	0.332	0.544	0.542	0.528
N	59,362	59,362	59,362	83,477	41,265	40,382

Notes: All specifications include industry-year fixed effects, average size of sending firm, characteristics of receiving firm (X), firm-average characteristics of new and incumbent workers (Y1 and Y2) as additional controls. Robust standard errors in parentheses are clustered at the firm level. The time period covered is 1995–2007. In column 1, productivity is constructed from Cobb-Douglas production function estimated by OLS. In column 2, productivity is constructed from Cobb-Douglas production function estimated by Olley-Pakes method. In column 3, productivity is constructed from the translog production function estimated by OLS. In column 4, productivity gap is constructed using value added per worker (benchmark measure), and in column 5, it is constructed using Solow residuals from the translog production function.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Next, we use a more general parameterization of production technology $F(\cdot)$ with a full set of input interactions and order terms, known as the translog production function. The translog specification offers a number of advantages over Cobb-Douglas function, most notable of which is the ability to better control for the effect of firm size on output by allowing for nonlinear effects of factor inputs on output. Estimation results for equation (4) with productivity measure derived from the translog production function are presented in column 3 of Table 14. Although the coefficient on the overall productivity gap becomes insignificant, the estimate on the positive productivity gap is even larger than in columns 1 and 2.

All specifications in Table 14 feature a positive and significant coefficient on the positive gap. The coefficients in specifications with TFP-based productivity measures (columns 1, 2, and 3) are about half of those in the benchmark specification. Although their magnitudes cannot be directly compared, we can gauge the effects of one standard deviation's change in the gap relative to the dependent variable's standard deviation. For instance, recalling the descriptive statistics in Table 3, a standard deviation's (0.056) increase in the positive gap is linked to $0.056 \times 0.28 / 0.65 = 0.024$ standard deviation's increase in log value added per worker. Analogously, a one standard deviation's (0.029) increase in Olley and Pakes's measure of the positive gap corresponds to $0.025 \times 0.166 / 0.23 = 0.021$ standard deviations of TFP. Hence, relative to their sample variation, the effects of these two measures of productivity gap are fairly similar.

Finally, in the last three columns of Table 14 we report regression results for the receiving firm's profitability. Column 4 shows results for the gap measured as the difference between the average profitability of sending firms and the profitability of the receiving firm in year $t - 1$. Consistent with our earlier results, this correlation is positive for all new workers and is stronger for workers coming from more profitable firms. This positive relationship remains broadly the same for other measures of the gap, such as value added per worker (column 5) or TFP estimated from the translog production function (column 6). Hence, our results are robust to a variety of ways in which the sending and receiving firms' economic performance are measured.

V. Conclusions

Our main finding is that hiring workers from more productive firms is associated with productivity gains in the receiving firms. This association, implying a rise in the productivity of a firm hiring at the mean by about 0.4 percent a year or a move by the median firm 0.4 centile up the productivity distribution, persists for several years after the hiring was done. We also find that the link between the gap and the receiving firms' productivity tends to be stronger for workers moving within the same industry sector, and for more highly skilled workers. Yet, the movement of even mid-skilled workers previously employed at more productive firms is also associated with productivity gains in the receiving firms. We demonstrate that our results cannot be explained by new workers' human capital or past shocks to receiving firms' productivity, or similarities between sending and receiving firms. That our estimates are stable across various measures of economic performance suggests that they reveal a genuine relationship rather than a quirk of a particular measurement technique.

Our findings are consistent with the spillovers through labor mobility hypothesis, that newly hired workers bring knowledge from their previous firms. Yet, we explore various extensions and controls to probe the robustness of our results to explanations alternative to spillovers. The first alternative we consider and rule out is that the gaps effect is due to new workers' human capital. To control for human capital, we use a measure developed in Abowd, Kramarz, and Margolis (1999), which is essentially the wage net of firm-specific effects estimated from the wage equation. Adding this measure has not changed our results. Moreover, the asymmetry of the estimated gaps effect (positive for positive gap, nearly zero for negative), one of the most robust of our findings, is inconsistent with the human capital explanation.

Another alternative explanation to our results is that firms may be better able to attract workers from more productive sending firms after having experienced a positive productivity shock in the past. This shock propagating through an autoregressive process in the error term up to $t + 1$ will cause a correlation between the gap and our regressions' error term, undermining the assumption required to identify the effect of the productivity gap proper. We try three approaches to rule out this explanation. First, we add lags of the receiving firm's productivity to eliminate residual autocorrelation, and doing so enables us to estimate the correlations for receiving firms that were equally productive in the past up until, and including, the year of hiring. Second, as an extension to the baseline specification, we apply the estimator developed in

Olley and Pakes (1996) which proxies receiving firms' productivity shocks by capital investments. Finally, as another extension, we repeat our analysis on the subsample of "green field" firms which did not exist in $t-1$, and thus no productivity shock could have affected their hiring choices. All approaches produce similar results, suggesting that the correlations we report are little affected by past productivity shocks.

Yet another plausible explanation for the positive estimate of the gap holds that worker flows are affected by *future* productivity shocks as anticipated by agents today. If this explanation is true, and if there is a preference for hiring workers from more productive sending firms, the gap will be positively correlated with the contemporary error term, resulting in an upward-biased estimate of its effect. While our attempts to control for this possibility (subsection IVB) produced similar results to the rest of the paper, we have cautioned the reader that our tools may not be adequate to fully eliminate the gaps correlation with the error term, as implied by this "anticipated shocks" explanation. In fact, assuming anticipated shocks, the extent to which our results can be taken as evidence for the existence of spillovers depends on the degree of their influence on hiring preference, which is unknown. Hence, in addition to a detailed account of productivity gains linked to worker flows that are consistent with spillovers through worker mobility, our study offers an illustration of how fragile the available evidence is at the moment.

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