Mismatch of Talent?

Evidence on Match Quality, Job Mobility, and Entry Wages*

Peter Fredriksson† Lena Hensvik‡ Oskar Nordström Skans§

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Abstract

We examine the direct impact of idiosyncratic match quality on entry wages and job mobility using unique data on worker talents matched to job-indicators and individual wages. We measure mismatch by how well the (types of) talents of recent hires correspond to the talents of tenured workers performing the same jobs. To corroborate the metric, we show that pre-hire talents among remaining workers become increasingly homogenous within jobs as tenure increases. A stylized model show that match quality has a smaller impact on entry wages but a larger impact on separations if matches are formed under limited information. Empirically, we find such patterns for inexperienced workers and workers who have entered from non-employment, which also are groups were mismatch is more pronounced on average. Most learning about mismatch happens within a year. Overall, our results suggest that workers with little experience and those who search from non-employment form matches with considerable remaining uncertainty whereas the matching of experienced job-to-job movers is best characterized by models where information is revealed before the match is formed. Earnings losses of mismatch remain substantial after 5 years for both groups.

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†Department of Economics, Stockholm University, SE-106 91 Stockholm, Sweden (Email: peter.fredriksson@ne.su.se), UCLS, IZA, and IFAU.

‡Institute for Evaluation of Labour Market and Education Policy (IFAU), Box 513, SE-751 20 Uppsala, Sweden (Email: lena.hensvik@ifau.uu.se), Uppsala Center for Labor Studies (UCLS), and CESifo.

§Department of Economics, Uppsala University, Box 513, SE-751 20 Uppsala, Sweden (Email: oskar.nordstrom_skans@nek.uu.se), UCLS, IFAU and IZA.

1 Introduction

A longstanding notion is that the allocation of workers across jobs is crucial for labor productivity and overall efficiency.[1] Idiosyncratic match quality is also fundamental to several recent theoretical contributions, including the work by Acemoglu and Shimer (1999) (on the implications for designing unemployment insurance), Eeckhout and Kircher (2011) (on the possibility of identifying sorting from wage data), Gautier et al. (2010) (on the interactions between comparative advantage and search frictions), and Helpman et al. (2010) (on the impact of trade for wage inequality and unemployment).

Although match quality is fundamental and conceptually well-defined, deriving direct, and credible, evidence on the importance of mismatch in the labor market has proven difficult. The purpose of this paper is to provide such evidence; we also document the role of ex ante uncertainty about the quality of the match. For this purpose, we use unique Swedish data containing specific information on a spectrum of workers' abilities and traits, the identities of their employers and occupations, and wages.

Much previous empirical work has been based on the Jovanovic (1979) model where match quality is unobserved at the time of hiring, but realized ex post. The typical approach has been to analyze realized patterns of exits and wages. A drawback of this approach is that alternative, and equally plausible, explanations (e.g., on-the-job training) exist for the observed associations between wages and tenure and separations and tenure.

We proceed differently. We use very detailed pre-hire data to assess if separations and entry wages respond to a direct measure of mismatch. The measure is based on workers' cognitive abilities and personality traits as documented at the military draft (which takes place at age 18 or 19). The draft data include a vector of eight productive "talents": four cognitive skills (inductive, verbal, spatial, and technical ability) as well as four traits evaluated by a trained psychologists (social maturity, intensity, psychological energy and emotional stability). Our basic presumption is that these particular talents are differentially productive in different jobs.

Our empirical strategy exploits the notion that workers should stay in their match if they have a comparative advantage within the jobs they are performing. Talents among tenured workers should therefore reflect the skill requirements of each particular job. By combining detailed data on entering workers' talents with equally detailed data on the talents of tenured workers who perform the same job, we are able to infer match quality from pre-hire data.

In our empirical work, we study data on recent hires in models with job (tasks by

[1] See, e.g., Sattinger (1975), and Tinbergen (1956) for the original work on so-called assignment models, i.e., the problem of assigning heterogeneous workers to heterogeneous jobs. In these (frictionless) models, market prices allocate workers to jobs. A more recent literature combines search frictions and worker/job heterogeneity. Gautier and Teulings (2012) calibrate such a model, and conclude that actual allocations imply very large efficiency losses.

establishment and year) fixed effects. This implies that we analyze the impact of variations in match quality between different entrants who start the same job, during the same year. Our models also account for the overall market valuation of entrants' talents and educational attainment. Identification comes from the within-job correspondence between the talents of individual workers and the skill requirements of the job (as measured by talents among tenured workers).

To frame the empirical analysis, we set up a simple model where mismatch is differentially observable at the time of hire. If mismatch is partially observed, it should be priced into entry wages, and separations respond only to revelations of mismatch (i.e. mismatch in excess of what was expected at the hiring stage). Entry wages should, on the other hand, be unrelated to mismatch if it is unobserved at the time of recruitment, in which case subsequent separations instead should respond more forcefully. As information is subsequently revealed when production commences, wage growth (within jobs) should be more forcefully related to actual initial match quality if it was unobserved at the time of hire. The amount of initial information available to the matching agents is thus key for both the wage and mobility responses.

Realistically, the available information varies with the characteristics of the match. We use two approaches to implement this idea. The first approach draws on the employer learning literature; see Farber and Gibbons (1996), Altonji and Pierret (2001) and, using Swedish data similar to ours, Hensvik and Skans (2013). We argue that labor market experience proxies the amount of information available on both sides of the labor market. In particular, for inexperienced workers, it is realistic to assume that both sides of the market fail to observe how well the detailed characteristics of the worker match the detailed skill requirements for each particular job. Notice that this assumption is valid even if both sides of the market are able to infer the market value of the opposing agent. The second approach compares workers who are hired from non-employment with workers who are hired from another job. We expect there to be less information available about match-specific value for those who enter from non-employment.

The results suggest that mismatch matters. The dispersion of talents within job decreases with tenure, and mismatch of talents predicts mobility during the first year after recruitment (but not thereafter). In line with the predictions of our stylized model, we find that mismatch is unrelated to entry wages among inexperienced workers and workers who have entered from non-employment. In contrast, experienced workers and job-to-job movers receive a wage penalty if they are mismatched. Consequently, we find a pronounced separation response to mismatch among inexperienced workers and entrants from non-employment, whereas the separation response among experienced workers and job-to-job movers is moderate. We also show that wage growth within job is negative for mismatched workers, and that this negative effect on wage growth is particularly pronouned among inexperienced workers.

Overall, we thus conclude that search for match-quality have a substantial impact on both job-mobility and wage determination. In an attempt to quantify the total costs associated with mismatch, we estimate the effect of initial mismatch on longer-term earnings outcomes for all new entrants. Five years after being a hired, a standard deviation increase in measured mismatch lowers earnings by around 2 percent (which corresponds to some 5 percent of the standard deviation in earnings). Note that this estimate represents all adjustment margins, since the impact is estimated for all individuals independently of whether they stayed or left the initial job.

The paper is structured as follows: Section 2 outlines the theoretical framework and the predictions. Section 3 describes the data. Section 4 documents the variance of talents and mismatch by tenure, experience and previous employment. Section 5 presents the empirical framework and the main results. Section 6 estimates the consequences of initial mismatch on future earnings. Section 7 concludes.

2 Framework

Here we outline a stylized model which will guide our empirical work. The model assumes that productivity is match-specific, and it allows for uncertainty about initial match quality. When workers and firms meet, they observe an initial signal of whether the particular worker is apt for the job. On the basis of that signal, they decide whether to match or not. As production commences, the worker/firm pair observes productivity, and they thus receive new (and more precise) information about match quality. To paraphrase Pries and Rogerson (2005), match quality is both an "inspection good" and an "experience good". Wages and employment decisions (i.e. separations) are adjusted to reflect current information on match quality. Separations depend on revelations about match quality, i.e., separations occur only to the extent that match quality was substantially worse than what the worker/firm pair thought initially.

Production We assume a constant returns to scale to technology and thus focus on one job. Each worker has a bundle of different skills $s_k(i)$, $k = 1, ..., K$. Productivity depends on how well these skills match with the technology (skill requirement) of the specific job. We measure the relationship between the skills and the technology by the location of the job and the worker in K-dimensional space. Let $d_k(i, j) = |s_k(i) - s_k(j)|$ denote the distance between the location of the worker and the job along the kth dimension and $d = d(i, j)$ the aggregate distance between the worker and the job (we make the empirical measure precise later on).

We take match productivity, $y(d)$, to be given by

$$y(d) = 1 - \gamma d + \theta s(i) + \lambda(j) \tag{1}$$

where $s(i)$ denotes a vector of worker skills, and $\lambda(j)$ the quality of the job. Match productivity is decreasing in the distance between the worker and the job, and thus maximal when $d \to 0$. Here γ reflects the substitutability between different skills for a particular job (see Teulings and Gautier 2004). We let $y^* = 1 + \theta s(i) + \lambda(j)$ denote maximal match productivity. For reasons we make clear below, all outcomes in the model depend on $y(d) - y^* = -\gamma d$. Therefore, we surpress $s(i)$ and $\lambda(j)$ from here on.[2]

Information and learning We assume that some uncertainty about match quality is revealed when the worker and the firm meet. In particular, we assume that they observe a signal, d_0, having the property that with probability α true match quality is observed and with probability $(1-\alpha)$ a random draw from the distribution of match quality is observed. The distribution of match quality is assumed to be uniform on the $[0, 1]$ interval. Using the signal, the worker/firm pair forms an expectation about match quality. The conditional expectation equals

$$E_0(d) \equiv E_0(d|\, d_0) = (1-\alpha)E(d) + \alpha d_0 \tag{2}$$

The conditional expectation is thus a weighted average of the signal and the unconditional mean $E(d)$; the relative weight attached to the signal is increasing in the probability of getting an informative signal.

Once production has commenced, agents learn about match quality by observing production. We make the extreme assumption, that actual match quality is revealed when production has commenced. This is only a convenience assumption that makes the model more tractable. The choice on whether to match or not depends on the initial signal (d_0). Conditional on matching, subsequent choices depend on revelations about match quality.

Hiring and wage bargaining Essentially, we follow Eeckhout and Kircher (2011) in modeling hiring and wage bargaining. We think of three stages: a meeting stage, a revelation stage, and a frictionless stage. At the frictionless stage, workers and firms receive the pay-offs associated with the optimal allocation. This assumption is of course extreme, but Eeckhout and Kircher (2011) show that less extreme assumptions do not alter the substance of any conclusions. The key is that the outside option depends on the optimal match (y^*).

At the meeting stage, each worker is paired randomly with one job. The worker/firm pair observes the initial signal (d_0) and decides on whether to match or to wait until the frictionless stage. Should they decide to match, they agree on an entry wage, where workers receive half of the match surplus. If they wait, they incur costs (c), which are shared equally between the parties (see Atakan 2006). The benefits of waiting are that

[2]This in keeping with our empirical work where we condition on (a polynomial in) individual talent and job fixed effects.

workers receive the wage associated with the optimal match, w^*, and firms receive profits π^*.

At the revelation stage, uncertainty about match quality is revealed. With access to this new information, the worker-firm pair decides to continue or to terminate the match. Terminating the match implies waiting until the frictionless stage. The total costs associated with separation are $(c + b)$, again shared equally, and the benefits are w^*, π^*, i.e., the pay-offs associated with the frictionless allocation. Here b denotes the additional cost of separating at a later stage.

2.1 Matching, wages, and separations

The outcomes at the meeting stage depend on the initial signal, see equation (2). The matching threshold can be written as[3]

$$\gamma E_0(d) + \frac{p_0}{1-p_0} b < c$$

The left-hand-side represents the (expected) losses associated with matching, and the right-hand-side, the loss associated with waiting. The first term of the left-hand-side is the production loss associated with expected mismatch. The second term on the left-hand-side is the expected additional cost of separating later; here p_0 denotes the probability of separating at the updating stage, given the information available at the time of the match.

Entry wages are determined by a surplus sharing rule with imperfect information about actual match productivity.

$$w_0(d) = \frac{1}{2}\left\{[(1-p_0)E_0(y(d)) + p_0(y^* - (c+b))] - [y^* - c]\right\} \tag{3}$$

The first term in brackets represents the expected gain from matching, while the second term represents the alternative, i.e., waiting. Notice that entry wages depend on actual match quality (d) only to the extent that the signal correlates with match quality.

At the revelation stage, the firm-worker pair revisits the employment relationship and re-negotiates wages. The set of continuing matches is defined by $\gamma d < (c + b)$; thus the match continues to be viable if the actual cost of mismatch (γd) is lower than the separation cost $(c + b)$. Separations thus occur if

$$d > \frac{c+b}{\gamma} \equiv d_s \tag{4}$$

Using the definition of the separation threshold (d_s), we can rewrite the matching thresh-

[3]Throughout we ignore discounting.

old somewhat. The set of acceptable matches is defined by

$$E_0(d) < d_s - \frac{b/\gamma}{1-p_0} \equiv d_m \tag{5}$$

Thus $d_m < d_s$, since matching implies a risk of incurring the additional separation cost (b) in the future.

Outcomes change only to the extent that there is new information. In our simple setting, the probability that agents receive new information in the future is $1-\alpha$. With probability $1-d_s$ this new information is such that the match is destroyed. Therefore, the ex ante probability of separating in the future (p_0) is

$$p_0 = (1-\alpha)(1-d_s) \tag{6}$$

and p_0 is thus independent of the signal.

At the revelation stage, wages are given by

$$w(d) = \frac{1}{2}\left[y(d) - (y^* - (c+b))\right] \tag{7}$$

2.2 Predictions

Here we summarize five predictions that we take to the data. We focus on how the responses to mismatch vary with the information content of the initial signal.

1. Exposure to initial mismatch The number of matches, and thus the exposure to initial mismatch, depends only on how the perceived cost of mismatch changes with d (see equations (5) and (6)). We have

$$\frac{\partial E_0(d)}{\partial d} = \alpha^2 \geq 0$$

and so

$$\frac{\partial^2 E_0(d)}{\partial d \partial \alpha} = 2\alpha \geq 0$$

It is obvious that the perceived cost of mismatch is increasing in the precision of the initial signal (α). This is the first prediction we take to the data. Whenever there is more information available at the time of the match, fewer matches will be formed for any given level of mismatch. The reason is simply that the perceived cost of mismatch increases with the precision of the information available at the time of the match. Therefore, a precise signal truncates the observed distribution of mismatch more than an imprecise one.

2. Entry wages and mismatch Consider the impact of actual mismatch on entry wages. From (3) it follows that

$$\frac{\partial w_0}{\partial d} = -\frac{(1-p_0)\gamma\alpha^2}{2} \leq 0$$

And so

$$\frac{\partial^2 w_0}{\partial d \partial \alpha} = -\frac{\gamma\alpha[2(1-p_0)+\alpha(1-d_s)]}{2} \leq 0$$

This is the second prediction we take to the data. With greater information content of the initial signal, the extent to which mismatch is priced into entry wages increases. In the extreme case, where the initial signal conveys no information, $\partial w_0/\partial d = 0$.

3. Separations and mismatch Separations (s) only occur if there is new information. The separation rate in this simple framework thus equals $s = (1-\alpha)(1-d_s)$, where $(1-\alpha)$ is the probability of receiving new information at the revelation stage. For a marginal match (i.e. a match where $d \to d_s$), the effect of a marginal increase in d equals

$$\frac{\partial s}{\partial d} = (1-\alpha) \geq 0$$

and therefore

$$\frac{\partial^2 s}{\partial d \partial \alpha} = -1 \leq 0$$

Thus a more precise initial signal lowers the impact on separations. This is the third prediction we take to the data.

4. Variance of talents by tenure From the first and the third prediction above it follows that the information content of the initial signal has implications for the variation of match quality within a given job. In particular, when the initial signal has little information value, all meetings result in matches and we observe the entire mismatch distribution. Information is subsequently revealed and the worst matches are destroyed. Therefore, the variance of talents should fall substantially over match tenure. More formally it is straightforward to verify that

$$\Delta var_{\alpha\to 0} = (d_s^2 - 1)var(d) \leq 0$$

$$\Delta var_{\alpha\to 1} = (d_m^2 - d_m^2)var(d) = 0$$

where var denotes the variance of the observed mismatch distribution and $var(d) = 1/12$ the variance of the original (untruncated) mismatch distribution. The fourth prediction we take to the data is that with greater information content of the initial signal, the variance of talent falls less with tenure.

5. Wage growth within job and mismatch Define $\Delta w = w(d) - w_0(d)$, where $w(d)$ is given by (7) and $w_0(d)$ by (3). We have

$$\frac{\partial \Delta w}{\partial d} = -\frac{\gamma}{2}\left[1 - (1 - p_0)\alpha\right] \leq 0$$

and

$$\frac{\partial^2 \Delta w}{\partial d \partial \alpha} = -\frac{\partial^2 w_0}{\partial d \partial \alpha} = \frac{\gamma[(1 - p_0) + \alpha(1 - d_s)]}{2} \geq 0$$

This is the fifth prediction we take to the data. With greater information content of the initial signal, the effect of mismatch on wage growth within job falls in absolute value.

3 Data and measurement

The data come from administrative employment registers collected by Statistics Sweden and the Swedish War Archives. The complete data contain annual employer-employee records for the universe of the Swedish workforce during 1985-2008, with unique person, firm and establishment identifiers but the basis of our analysis is all male workers who enter new jobs (entrants) between 1997 and 2008, and their tenured male coworkers (incumbents).[4] To these data we add socioeconomic background characteristics and military enlistment scores for both entrants and incumbents. Information from the draft is available for all males who did the draft between between 1969 and 1994. During these years, almost all males went through the draft procedure at age 18 or 19, which means that our sample consists of male entrants born between 1951 and 1976.

We also add information on wages (adjusted for working hours) and occupational codes to the data. This information is available for all public establishments and a sample of private establishments, covering almost 50% of private sector workers.[5]

3.1 Measuring match quality

The data from the draft procedure include four different measures of cognitive skills and four measures of non-cognitive skills. This information is confidential, except for research purposes. The cognitive measures are based on four subtests measuring: (i) inductive skill (or reasoning), (ii) verbal comprehension, (iii) spatial ability, and (iv) technical understanding. The tests are graded on a scale from 0 to 40 for some cohorts

[4] We focus on this period since 1997 is the first year that we have occupation information in our data.

[5] Wage and occupation information is collected during a measurement week (in September-November) each year, conditional on being employed for at least one hour during the sampling week. The sampling is stratified by firm size and industry; small firms in the private sector are underrepresented.

and from 0 to 25 for others. To achieve comparability across cohorts, we standardize the test scores within each cohort of draftees.

The non-cognitive measures are based on behavioral questions in a 20-minute interview with a trained psychologist. On the basis of the interview, the draftee is scored along four separate dimensions: (i) social maturity, (ii) psychological energy (e.g., focus and perseverance), (iii) intensity (e.g., activation without external pressure) and (iv) emotional stability (e.g., tolerance to stress).[6] The non-cognitive dimensions are graded from 1 to 5 and there is also an overall psychological score on a Stanine scale, which ranges form 1 to 9.

We contrast the eight cognitive and non-cognitive talents among new hires with those of tenured workers in the same jobs. The rationale for doing this is that tenured workers will be selected on having the right skills for the job if match quality matters. Thus, the skills of tenured workers identify the skill requirements for the job. It is important to note that we use information at the job-level under the assumption that certain jobs require certain skills or talents. It is still possible that a firm, establishment or organization can benefit from having a diverse (also in terms of talents) workforce across jobs.

For the purpose of the empirical analysis, we focus on tenured workers with at least 3 years of tenure in the current job. To measure the talents of incumbent workers with reasonable amount of precision, we require that the job (see below for a detailed definition of a job) employs at least 10 tenured males with non-missing draft scores (in Section 5.5 we show that results are similar even if we only require one tenured worker).

From an empirical point of view, we measure the distance between the skills of the worker and the skill requirements of the job ($D(i, j)$ in terms of Section 2) as:2

$$Mismatch_{ij} = \sum_{k=1}^{K} |s_{ik} - \bar{s}_{jk}| \tag{8}$$

where s_{ik} denotes the talents of the individual worker, and $\bar{s}_{jk}$ the average skill among incumbent (tenured) workers along the kth dimension. We aggregate the k components to an overall mismatch index, and then standardize the overall index for ease of interpretation (this corresponds to d in Section 2). As such, this mismatch index captures mismatch along the horizontal dimension ("the worker has different skills than incumbent workers") as well as the vertical dimension ("the worker is over- or under-skilled relative to the skill requirement"). However, our regression analysis holds individual skills and job fixed effects constant impliying that the estimates on mismatch only captures mismatch in the horizontal dimension. We present robustness checks with respect to the measurement of mismatch in Sections 5.5 and 5.7.

To show that each of the measured talents have some independent information content,

[6] The interpretation is based on Mood et al. (2010) who provide a detailed discussion of the scores.

Table 1: Wage returns to skill

	(1)	(2)
Inductive skill	0.0373***	0.0216***
	(0.0008)	(0.0007)
Verbal skill	0.0253***	0.0031***
	(0.0007)	(0.0007)
Spatial skill	0.0095***	0.0028***
	(0.0006)	(0.0006)
Technical skill	0.0350***	0.0209***
	(0.0007)	(0.0006)
Social maturity	0.0308***	0.0242***
	(0.0007)	(0.0007)
Intensity	0.0046***	0.0049***
	(0.0006)	(0.0006)
Psychological energy	0.0277***	0.0182***
	(0.0007)	(0.0006)
Emotional stability	0.0260***	0.0205***
	(0.0007)	(0.0006)
Observations	343,440	343,440
R-squared	0.3185	0.3862
Year FE:s	√	√
Educational attainment FE:s		√

Notes: Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The sample includes all males aged 35 during 1997-2001 who have non-missing information on wages and test-scores. Regressions are weighted by sampling weights to adjust for underrepresentation of small firms in the private sector.

we first relate them to prime-age (age 35) wages within our sample.[7] Table 1 shows the results. Column (1) does not control for education, while column (2) controls for level-of-education fixed effects. The results show that all skill measures have precisely determined returns, even conditional on educational attainment.[8] On average, a standard deviation increase in a talent is associated with an increase of wages by 1.5%, holding educational attainment constant. Most importantly, however, the results in Table 1 show that there is independent, and sufficiently precise, variation in the individual measures of talent. In addition, Table A2 in the Appendix corroborates that there is independent variation along all dimensions by showing the bivariate correlations between educational attainment and the different domains of cognitive and non-cognitive skills.

[7]The results in Böhlmark and Lindquist (2006) and Bhuller et al. (2011) suggest that earnings at roughly age 35 gives a good approximation of life-time earnings.

[8]This is fairly remarkable, in particular since Grönqvist et al. (2010) estimate the reliability ratio of overall cognitive ability to 73% and the reliability ratio of overall non-cognitive ability to 50%.

3.2 Proxies for information

Our empirical analysis aims to contrast groups where match productivity is likely more difficult to observe at the hiring stage with groups where match productivity is more likely observed. Our main approach is to classify matches on the basis of worker experience on the labor market. In particular, it is reasonable to assume that match productivity is largely unobserved among inexperienced workers. For experienced workers, on the other hand, the employer arguably has some information about about match quality (see Farber and Gibbons 1996 and Altonji and Pierret 2001); such information can come from work histories, previous wages or references for jobs that are similar to the job under consideration. Analogously, the employee has some information on where his/her bundle of talents can be put to most productive use. In sum, match productivity is likely to be, at least partially, observed for experienced workers. As an alternative approach we classify matches on the basis of whether the worker entered from non-employment or from another job. Here, we expect there to be less information available about match-specific value for those who are hired directly from non-employment.

3.3 Definitions of jobs, entrants and experience

As explained above, our analysis relies the variation in mismatch within job. We define a *job* as an occupation×plant×(entry year) combination. We use (the Swedish version of) the ISCO-88 (International Standard Classification of Occupations 1988) standard at the 3-digit level. Occupations are reported by the employer and the 3-digit level allows us to distinguish between 113 occupations (for instance accountants/lawyers or mining/construction workers). The definition of a job allows for the possibility that technologies differ across plants within an occupational category and that there is technological evolution within cells defined by occupation and plant.

We can trace the individuals back to 1985 in our data. Labor market *experience* is defined as the number of years which the individual is classified as being employed according to Statistics Sweden's classification system.[9] Since this information is available from 1985 onwards, we truncate experience at 13 years of experience for all entrant cohorts. The median entrant in our sample is 35 years old, and has (at least) 13 years of experience. As alluded to earlier, we focus particularly on the contrast between inexperienced and experienced workers. For the purpose of the analysis, inexperienced workers are those with less than 5 years of experience while experienced workers have at least 5 years of experience. We also separate between *job-to-job* movers (employed in the previous period) and workers entering employment from non-employment.

We define a *separation* as a case when an individual is not observed at the entry

[9]The classification relies on register data on monthly earnings (in November) and uses employment thresholds that mimic the employment definition of the Labor Force Surveys reasonably closely.

establishment during the following two years.[10] To avoid including lay-offs due to plant closures we only study entrants into establishments that remain in the year following the entry.

3.4 Descriptive statistics

Table 2 shows the characteristics of the entrants in our sample as well as some basic information about the occupations they enter. Since our main analysis focuses on entrants with at least 10 tenured coworkers within the same job, our main sample contains larger establishments (655 employees on average) than an overall sample of entrants (144 employees) during the same time period.[11] The separation rate, defined as the probability of leaving the establishment within the first year after being hired, equals 21% in our sample (29% in the overall sample); see the first row of Table 2. As expected, inexperienced workers have a higher separation rate and a lower share of inexperienced recent hires were employed during the previous year (job-to-job movers). Figure 1 shows how separation probabilities and wages evolve with tenure within our sample. Consistent with the earlier literature, these cross-sectional data show a robust negative relationship between tenure and separation and a robust positive relationship between wages and tenure.

For illustrative purposes, the lower half of Table 2 categorizes the occupations at the fairly crude 1-digit ISCO level. Most of the jobs in our used data fall in the categories "professionals", "technicians", and "machine operators", which taken together comprise 71% of our sample. In the robustness section we explore the extent to which our key results vary between different occupational levels.

The final row of Table 2 shows the values of the (standardized) mismatch index in in our sample. Inexperienced workers are mismatched to a greater extent than experienced workers. The difference between the two groups corresponds to 0.04 of a standard deviation, we return to this issue in the next section.

4 The variance of talents by experience and tenure

As argued in section 3.2, the observability of match quality is likely to differ across experience groups. In particular, initial match quality is more likely unobserved for inexperienced workers.

Under this assumption, and the definitions made in the previous section, we are able to test the a key prediction of Section 2. We should observe more mismatch in realized matches among inexperienced workers than among experienced workers.

Figure 2 thus examines measured mismatch among new hires as a function of the

[10] We impose the two year requirement to avoid defining recalls as separations.

[11] Table A1 contains information which is analogous to Table 2 for all male entrants during 1997-2008.

Table 2: Entrants 1997-2008

	All			Inexp 0-4 yrs	Exp 5+ yrs
	mean	SD	median	mean	mean
Separation rate	.21	.40	0	.24	.20
ln(Entry wage)	10.06	.37	10.0	9.82	10.11
Age	36.2	7.9	35	27.1	37.9
Experience at entry	12.5	5.1	13	2.2	13.0
Job-to-job mobility	.82	.39	1	.46	.88
Entry establishment size	655	1,180	243	710	645
Education:					
Primary school less than 7 years	.00	.06	0	.00	.00
Primary 7-9 years	.07	.26	0	.05	.07
High school short (less than 2 years)	.02	.14	0	.01	.02
High school short (2 years)	.24	.43	0	.10	.26
High school long (3 years)	.15	.36	0	.23	.14
College short (less than 2 years)	.12	.33	0	.17	.11
College short (2 years)	.10	.30	0	.07	.11
College long (3 years)	.13	.34	0	.18	.12
College long (4 years)	.13	.34	0	.18	.12
PhD short (Licentiate)	.00	.07	0	.00	.01
PhD long (Doctoral)	.02	.15	0	.01	.03
Entry occupation:					
Legislators, senior officials and managers	.05	.21	0	.04	.05
Professionals	.29	.46	0	.34	.29
Technicians and associate professionals	.24	.43	0	.20	.25
Clerks	.04	.20	0	.05	.04
Service workers and shop sales workers	.06	.24	0	.07	.06
Skilled agricultural and fishery workers	.00	.05	0	.00	.00
Craft and related trades workers	.09	.28	0	.06	.09
Plant machine operators and assemblers	.18	.39	0	.18	.18
Elementary occupations	.05	.21	0	.05	.05
Mismatch	.00	1	-.17	.04	.00
Observations	154,681			24,383	130,298

Notes: The table shows the characteristics of the entrants in the year of entry.

Figure 1: Separations and wages by tenure

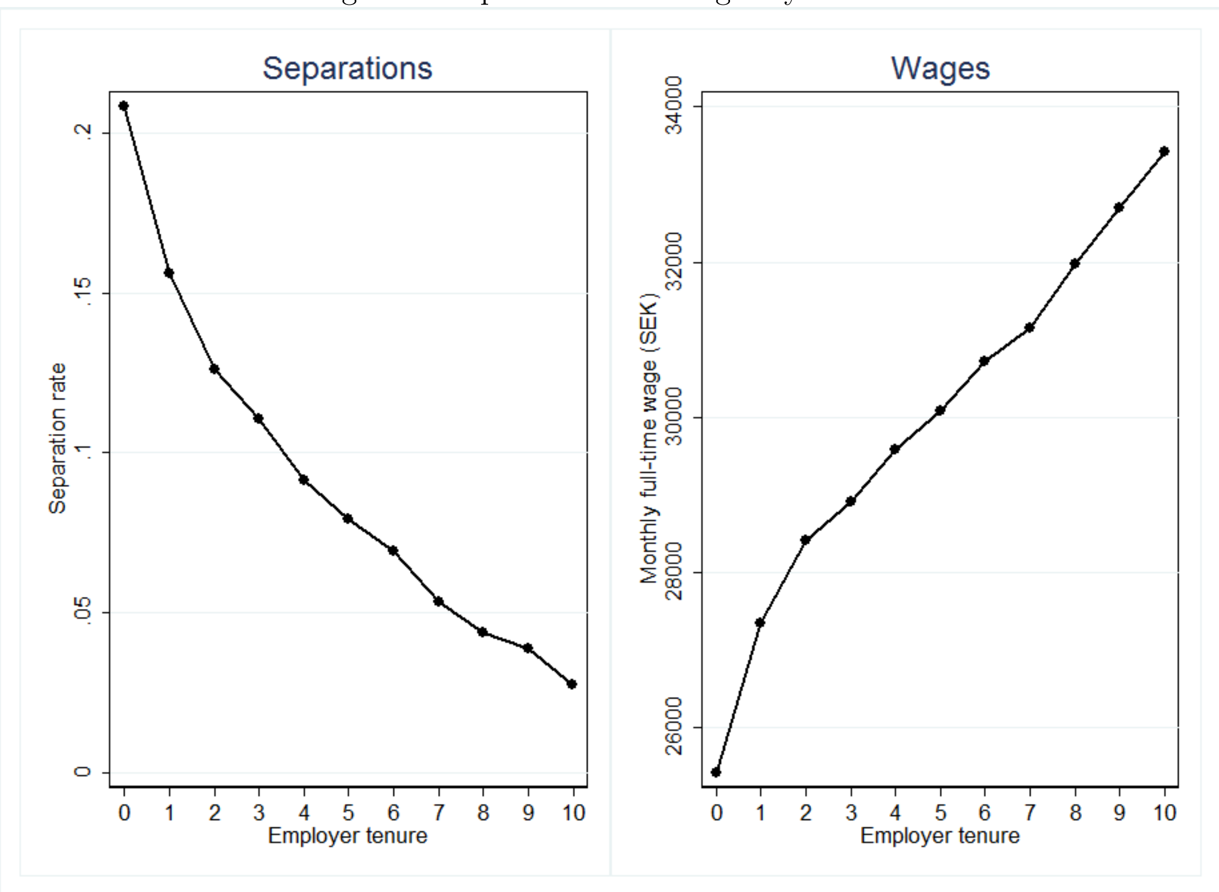
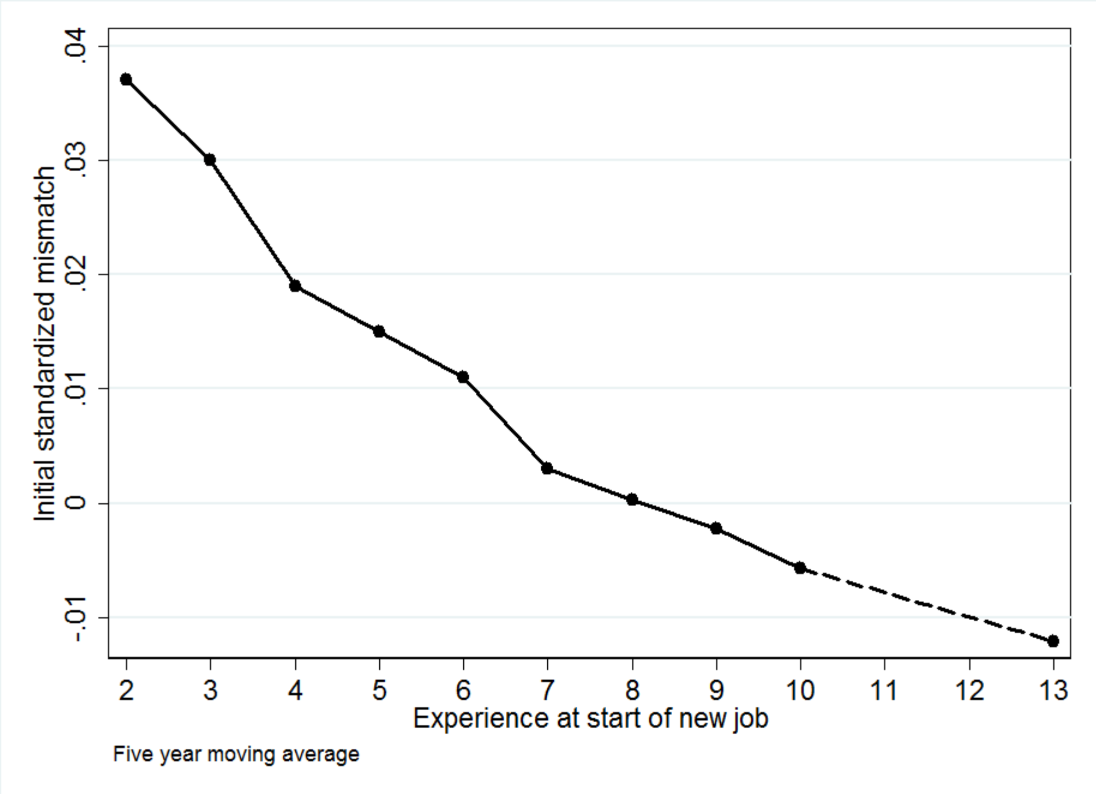


Figure 2: Initial mismatch by experience



number of years of previous labor market experience at the start of the new job. In the interest of clarity, the figure shows 5-year moving averages relative to the mid point of this interval. The first point in the graph thus represents initial mismatch among those with 0-4 years of experience at the start of the new job.

The results of the figure clearly shows that initial mismatch decreases with experience. Initial mismatch is 0.04SD higher among those with 0-4 years of experience than among the average new hire in our sample (for whom mismatch is normalized to 0).

To make this more precise, we next present detailed regression evidence on the same issue where we relate mismatch at the time of the hire to a dummy for being inexperienced (< 5 years) denoted D_i^{Inexp} and a dummy for job-to-job movers (the alternative is entering from non-employment) denoted $D_i^{Job-to-job}$. The basic regression has the following structure:

$$Mismatch_{ij} = \beta_{Inexp} D_i^{Inexp} + \beta_{Job} D_i^{Job-to-job} + g_M(s_i) + \gamma_M X_i + \lambda_j^M + \epsilon_{ij}^M \qquad (9)$$

where i refers to individuals, j to "jobs" ($j = occupation \times plant \times entry year$); X_i controls for experience and birth year (which also implies that we hold age at hiring constant, since entry year is held constant); $g_M(s_i)$ is a flexible control function (vector) in individual skills. We include 2nd order polynomials in each talent and fixed effects for educational

Table 3: Mismatch, experience, and job-to-job mobility

	(1)	(2)
Inexperienced (0-4 yrs.)	0.0455***	
	(0.0118)	
Job-to-job mobility	-0.0482***	-0.0345***
	(0.0094)	(0.0096)
Observations	156,996	156,996
R-squared	0.3229	0.3233
Education FE:s	$\surd$	$\surd$
Entrant test scores	$\surd$	$\surd$
(Entry occupation×Entry Year×Plant) FE:s	$\surd$	$\surd$
Experience FE:s		$\surd$

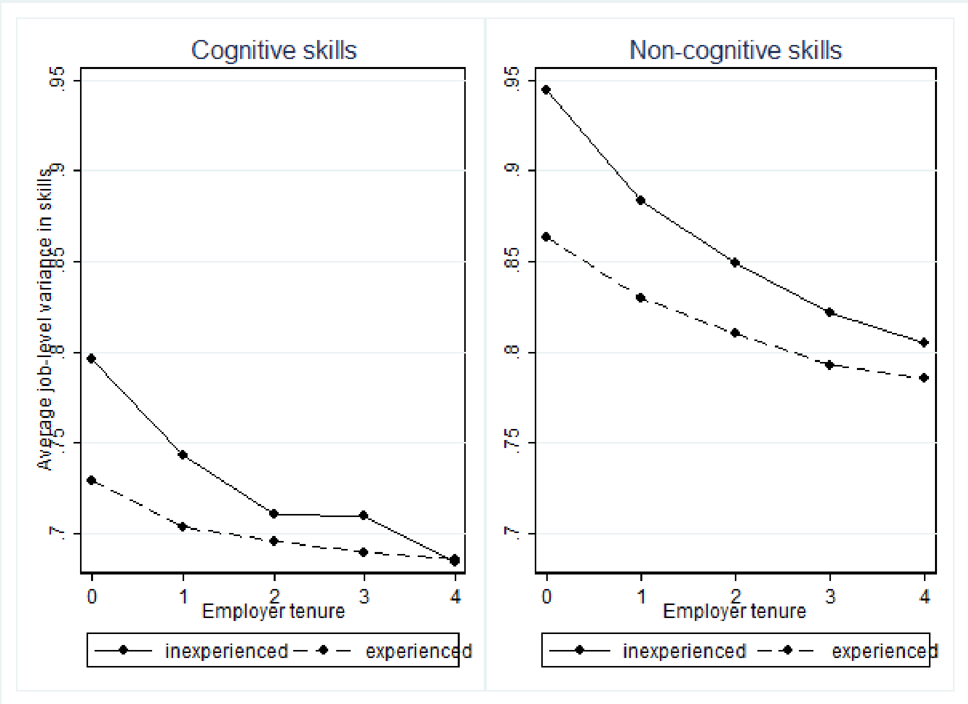
Notes: Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Mismatch is measured at the time of hiring and experience is measured at the start of the new job.

attainment.

Table 3 presents the results. The results in column (1) imply that mismatch is 0.046 standard deviations higher among the inexperienced than among experienced workers whereas job-to-job movers are exposed to 0.048 SD less mismatch than those entering from non-employment. Column (2) shows that the latter conclusion is robust to replacing the "inexperienced"-dummy with a full set of years-of-experience fixed effects (although the impact becomes marginally smaller). Job-to-job movers thus, on average, have better match quality even after conditioning on years of of previous employment, measured talents, level of education, and job-indicators. We interpret this evidence as strong support for the notion that there is less infromation about match quality among the inexperienced and for workers entering from non-employment.

The theory outlined in Section 2 also implies that pre-hire differences between inexperienced and experienced workers should be smaller among those that remain within jobs (since the worst matches are destroyed). To test this prediction, Figure 3 presents the within-job variance in skills by tenure and type of skill (cognitive/non-cognitive) separately for inexperienced and experienced workers. The pattern is rather striking. The initial within-job variance facing inexperienced workers is 0.07-0.09 SD higher than among experienced workers. As mismatched workers separate, remaining inexperienced workers become more like remaining experienced workers. Most of this adjustment takes place within the first year, and after 4 years of tenure at the firm, the within-job variance is only marginally higher among inexperienced workers than among experienced workers. One interpretation of the convergence is that any additional separation costs are marginal relative to the costs of mismatch.

Figure 3: Variance in skills by tenure and experience



Note: The figure displays the within job-level variance in skills, separately by tenure and experience. Inexperienced workers have 0-4 years of experience at the start of the new job. We weight the variance by the relative size of entry occupations (i.e. when tenure=0).

5 Match quality, entry wages and separations

Here we probe deeper into the remaining predictions of Section 2. In particular, we present regression evidence on the relationship between entry wages and mismatch as well as separations and mismatch. A key aspect of the analysis is the observability of match quality at the hiring stage. As argued above, we take the experience of the worker as the main indicator of the observability of match quality at the hiring stage, but we also explore the role of mobility from employment vs. non-employment.

5.1 Empirical Models

We run regressions, separately by experience groups, where entry wages and separations are related to mismatch at the time of the hire. These regressions have the following basic structure:

$$\ln(Entry\,Wage_{ij}) = \beta_w Mismatch_{ij} + g_w(s_i) + \gamma_w X_i + \lambda_j^w + \epsilon_{ij}^w \tag{10}$$

$$1^{st}\ year\ Separation_{ij} = \beta_s Mismatch_{ij} + g_s(s_i) + \gamma_s X_i + \lambda_j^s + \epsilon_{ij}^s \tag{11}$$

As above i refers to individuals, j to "jobs" ($j = occupation \times plant \times entry\ year$); X_i controls for experience and birth year, while $g_w(s_i)$ and $g_q(s_i)$ are flexible control functions (vectors) in individual skills. One reason to include flexible skill controls is that mismatch essentially is an interaction between worker skills and the skill requirements of the job, and interactions without main effects are hard to interpret. Moreover, the skill controls hold outside opportunities for the worker constant. As above, we include 2nd order polynomials in each talent and fixed effects for educational attainment. The job fixed effects obviously control for everything that is specific about plants and occupations and their interactions, including skill requirements of the job, job amenities and everything directly related to the skill levels among tenured workers.

The coefficients of interest are the coefficients on the mismatch index (β_w and β_s). We expect mismatch along partly observed dimensions to be priced; moreover, to the extent that mismatch is unobserved at the time of hiring, higher mismatch leads to separation (if the price of mismatch is higher than any separation cost). Initially we focus on how the coefficients on mismatch vary by experience group.

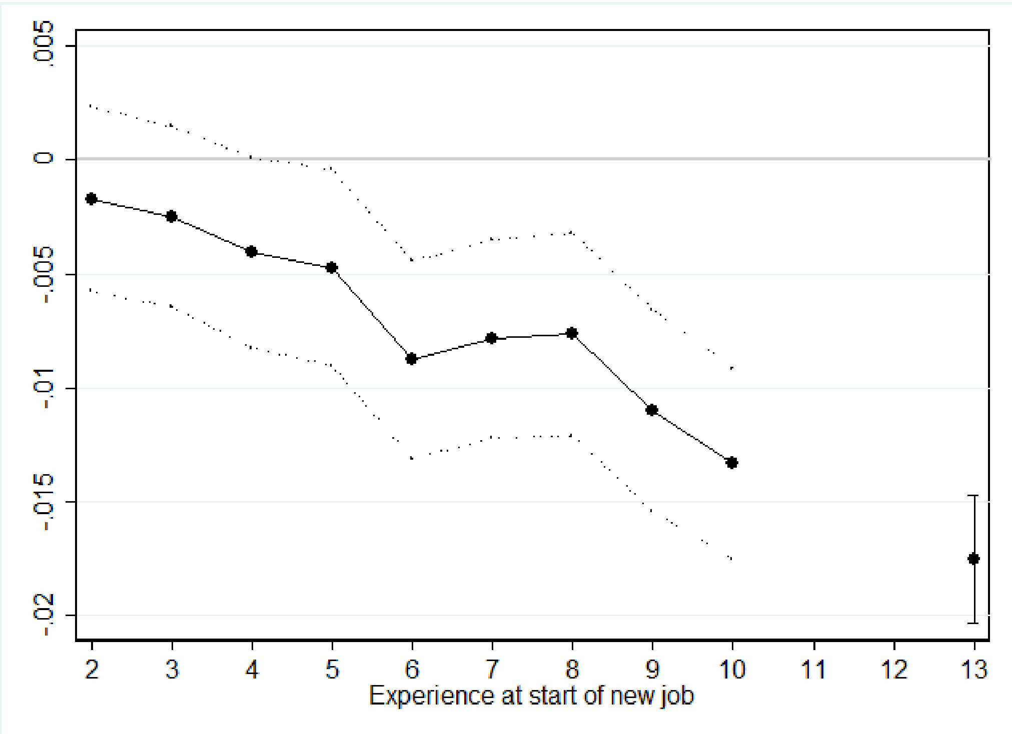
5.2 The role of experience

Figure 4 presents the first set of results for 5-year moving averages as above. The results speak to one of the predictions of Section 2: the relationship between entry wages and mismatch is negative, but only if match quality is observed ex ante. The results show that entry wages are unrelated to mismatch for workers with up to 5 years of experience. For more experienced workers, we find a negative effect of mismatch on the entry wage. For workers with at least 13 years of experience at the start of the new job, a standard deviation increases imismatch lowers entry wages by 1.7 percent.

Figure 5 speaks to another prediction of Section 2: there is a positive impact of mismatch on the probability of separations, but only if matches are formed under uncertainty. For inexperienced workers, we find that a standard deviation increase in mismatch raises separations by 2.2 percentage points. This corresponds to almost a tenth of the average separation probability for this group. The impact is considerably smaller for experienced workers; beyond 7 years of experience the relationship between separations and mismatch is not statistically significant.

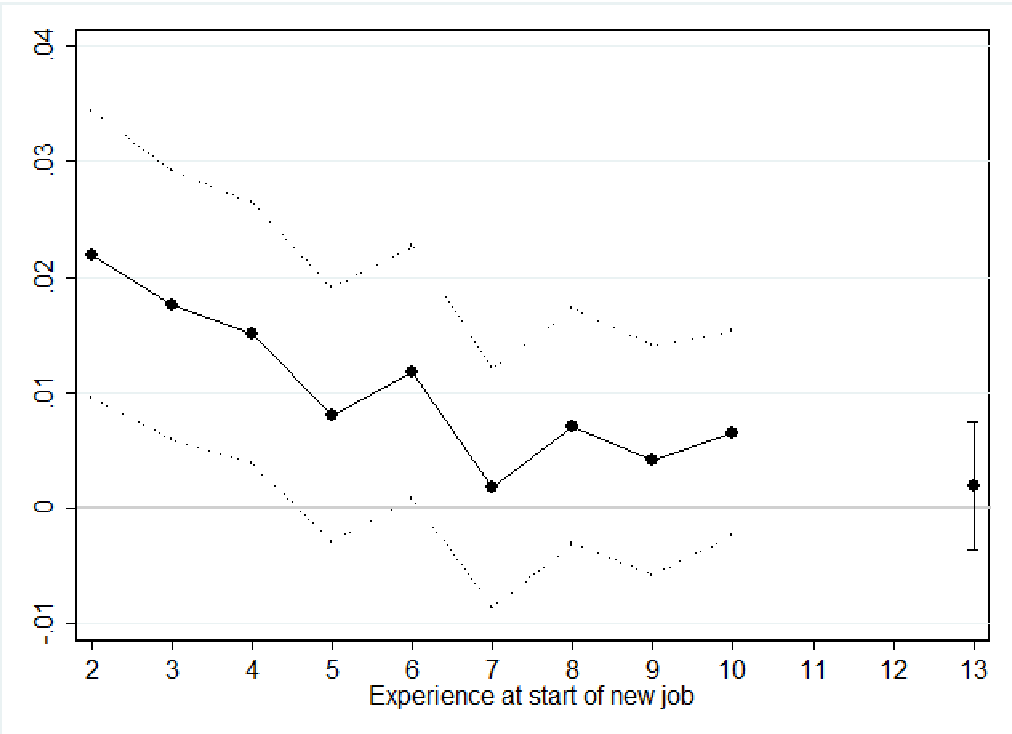
Table 4 presents more detailed regression results. The upper panel pertains to inexperienced workers and the lower panel to experienced workers using a 5 year cut-off. Column (1) shows the results of the baseline specification, which also underlies Figures 4 and 5. As is evident, the entry wage for inexperienced workers is unrelated to initial mismatch. The coefficient estimate is very small (-0.2%) and precisely determined. Matters are different for experienced workers. For this group, a standard deviation increase in mismatch reduces wages by 1.4%. Column (1) also shows that the separation response to mismatch is considerably larger among inexperienced workers than among experienced workers. A standard deviation increase in mismatch increases separations among inexpe-

Figure 4: Estimated entry wage responses to mismatch by experience



Notes: Each dot is an estimate of the wage response to initial mismatch within 5-year experience bins (+/- 2 years). The sample consists of entrants in 1997-2008. Experience can be traced back to 1985; it is truncated at 13 years for workers with 13 years experience or longer. Dotted lines are 95% confidence bands.

Figure 5: Estimated separations responses to mismatch by experience



Notes: Each dot is an estimate of the separation response to initial mismatch within 5-year experience bins (+/- 2 years). The sample consists of entrants in 1997-2008. Experience can be traced back to 1985; it is truncated at 13 years for workers with 13 years experience or longer. Dotted lines are 95% confidence bands.

Table 4: Responses to mismatch

	Inexperienced (0-4 yrs.)	
	(1)	(2)
	ENTRY WAGES	
Mismatch	-0.0023	-0.0004
	(0.0021)	(0.0032)
Observations	24,525	24,525
R-squared	0.8596	0.9215
	SEPARATIONS	
Mismatch	0.0222***	0.0219**
	(0.0063)	(0.0097)
Observations	24,525	24,525
R-squared	0.5964	0.7592
	Experienced (5+ yrs.)	
	(1)	(2)
	ENTRY WAGES	
Mismatch	-0.0142***	-0.0122***
	(0.0009)	(0.0013)
Observations	133,675	133,675
R-squared	0.8355	0.8993
	SEPARATIONS	
Mismatch	0.0047**	0.0052*
	(0.0019)	(0.0028)
Observations	133,675	133,675
R-squared	0.4777	0.6662
Education FE:s	√	
Entrant test scores	√	√
(Entry occupation×Entry Year×Plant) FE:s	√	
(Entry occ×Entry Year×Plant×Education) FE:s		√

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors robust to heteroscedasticity. Sample consists of entrants in 1997-2008. All regressions include a full set of birth cohort and experience fixed effects. The test score controls are 2nd order polynomials in each of the 8 test score domains.

rienced workers by 2.2 percentage points and by 0.5 percentage points among the pooled sample of experienced workers.

In column (2) we make the definition of a "job" even more precise by defining it as the interaction between 3-digit occupation, plant, entry year (as before) and *education level*. In practice, we are thus defining (e.g.) lawyers entering a certain establishment as having different jobs if they have a 4-year diploma than if he or she has 3-year diploma (both are possible). The results show, however, that this extension leaves the baseline results largely unaffected and we therefore use the simpler specification in the remainder of the paper. As a by-product, column (2) also shows that our results are unconfounded by potential mismatch in educational attainment.

Overall, we interpret the results in Table 4 as very much in line with the interpretative framework presented in Section 2. Because there is more information about experienced workers, and experienced workers are likely to have more information on where their skills are most apt, entry wages are negatively related to mismatch. Relative to a well-matched worker with similar skills, a mismatched worker has to accept a lower wage since match surplus is lower for this worker than for the well-matched worker. Among experienced workers, the separation response is lower than among inexperienced workers. Arguably, this is because some of the mismatch was already factored in at the time of hiring. Separations should primarily respond to mismatch that is unexpected relative to the information available at the time of hiring.

5.3 Job-to-job movers

Table 5 presents an analysis where we group workers on the basis of whether they entered the new job from non-employment or from another job. We think of this analysis as an alternative way of proxying for the amount of available information at the time of hiring. The basic presumption is that there is more information about workers entering from another job, and that this group is better informed on where their skills are more apt, than those entering from non-employment.

Column (1) presents the results for workers entering from non-employment, and column (2) shows the results for those entering from another job. The baxsic picture is very much in line with Table 4 above. For those entering from non-employment, entry wages are unrelated to mismatch but the effect on separations is instead positive and amount to an increase by 1.2 percentage points for a standard deviation increase in mismatch. For job-to-job movers on the other hand, mismatch is negatively priced in entry wages and there is a smaller separation response than among those entering from non-employment (although the difference is not statistically significant).

5.4 Wage growth among stayers

Section 2 suggested that the impact of mismatch on wage growth should be greater in absolute value for groups where there is more initial uncertainty about mismatch. This prediction comes from the fact that, over the longer run, uncertainty about initial mismatch is revealed and this should be reflected in equilibrium wages. Table 6 examine the validity of this prediction by estimating wage growth equations separately for inexperienced and experienced workers. We calculate wage growth as the 3-year difference in log wages for individuals who have stayed in the same plant, and estimate the regressions separately by experience group. Notice that the samples are reduced to around a quarter of the original size. The sample reduction has two origins. First, the wage data are collected via sampling; thus we lose a substantial fraction of observations because plants randomly exit the sampling frame. Second, wage growth within job is (of course) only observed for the selected sub-sample that stay on in the same job (note, however, that

Table 5: Responses to mismatch: entrants from non-employment vs. job

	From non-emp.	**From job**	**P-val. for diff.**
	(1)	(2)	(3)
		ENTRY WAGES	
Mismatch	-0.0014	-0.0123***	0.000
	(0.0019)	(0.0009)	
Observations	28,321	128,675	
R-squared	0.8765	0.8343	
		SEPARATIONS	
Mismatch	0.0117***	0.0058***	0.254
	(0.0057)	(0.0020)	
Observations	28,321	128,675	
R-squared	0.6191	0.4845	
Education FE:s	√	√	√
Entrant test scores	√	√	√
(Entry occupation×Entry Year×Plant) FE:s	√	√	√

Notes: Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Column (3) displays the p-value of the difference between the estimates in columns (1) and (2). All regressions include a full set of birth cohort and experience fixed effects. The test score controls are 2nd order polynomials in each of the 8 test score domains.

the prediction concerns the sample of stayers).

The results in column (1) suggest that inexperienced workers who are subjected to a standard deviation increase in mismatch see their wages grow at a 5 percent lower rate than the average worker in the same group. For experienced workers, the estimate in column (2) corresponds to a reduction in wage growth by 1.4%. The difference across the two groups is thus -3.6 percent. These estimates are thus in line with our prediction, but the difference across groups is not statistically significant at conventional levels.

The lack of statistical significance comes from the fact that we estimate the regressions very flexibly, allowing all coefficients to vary across the two groups. If we pool the two regressions, only allowing mean wage growth and the impact of mismatch to differ across the two groups, we find that mismatch is associated with 4.3 percent lower wage growth for inexperienced workers and with 1.5 percent lower wage growth for experienced workers. The difference across the two groups is -2.8 percent, which is statistically significant at conventional levels (the t-ratio is 2.2).

5.5 Robustness

This section examines the robustness of our results. We address several issues, e.g., the measurement of the mismatch index and the sample restrictions. The results of our robustness checks are displayed in Tables 7 and 8. Throughout we use the specification in column (1) in Table 4.

Table 7 presents a first set of robustness checks; for easy reference we reiterate the

Table 6: The impact of mismatch on wage growth within job

	Inexperienced (0-4 yrs.)	**Experienced (5+ yrs.)**	P-val for diff.
	(1)	(2)	(3)
Mismatch	-0.0503*	-0.0145	0.156
	(0.0265)	(0.0093)	
Observations	6,287	31,357	
R-squared	0.7881	0.6170	
Education FE:s	√	√	√
Entrant test scores	√	√	√
(Entry occupation×Entry Year×Plant) FE:s	√	√	√

Notes: Wage growth refers to the 3-year difference in log wages for individuals who have stayed on in the same plant. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Column (3) displays the p-value of the difference between the estimates in columns (1) and (2). All regressions include a full set of birth cohort and experience fixed effects. The test score controls are 2nd order polynomials in each of the 8 test score domains.

baseline estimates in Panel A.

Panel B relaxes the restriction on the number of tenured workers. Our main strategy has been to restrict the analysis to jobs with at least 10 tenured workers, in order to have a reasonably precise measure of the skill requirements of the job. Panel B drops this restriction and thus includes all jobs with at least one tenured worker. Without the restriction, sample size increases substantially. Nevertheless, our results are remarkably stable. Overall the absolute sizes of the estimates are somewhat lower which is consistent with the view that we have a somewhat less precise measure of skill requirements when we include jobs with less than 10 tenured workers.

In Panel C we explore the the extent to which mismatch in terms of the skills that matter the most in the labor market is more important than mismatch in other dimensions. To examine this issue we weight the components of the mismatch index with the estimated wage returns to the particular components; as weights we use the returns reported in Table 1. Using this weighted mismatch index does not change the results, however.

Panel D instead inquires whether it is mismatch in the cognitive or non-cognitive dimension that primarily matters. The results from models that calculate two separate indexes in these dimensions, show that the coefficients on mismatch along the cognitive and non-cognitive dimensions are not significantly different from one another.

Panel E examines whether the effects of mismatch are non-linear. We pursue this robustness check for three reasons. First, one may suspect that there are ranges of inaction, either because there is some measurement error in skills or because mobility/separation costs are substantial. If so, there should be an initial range of inaction until mismatch surpasses a certain threshold when separations should increase. Second, there is unavoidably some arbitrariness in specifying the mismatch index. The correct functional form of mismatch depends on the (unknown) production technology. Finally, the fact that the extent of mismatch vary with experience and previous employment status could potentially explain differences in responses if the impact of mismatch is highly non-linear. The results presented in Panel E shows, however, that although the impact of mismatch on entry wages are somewhat non-linear (the absolute size of the effect tends to be larger for high values of mismatch), the estimates on the second order terms are small. Further, we find that separations are literally linear in mismatch. We take this to indicate that allowing for non-linearities in mismatch is not crucial.[12]

Table 8 pursues another robustness check by examining whether mismatch has different implications depending on whether the position is high-skilled or low-skilled. One reason to pursue this extension is that the losses associated with mismatch may be larger at the higher end of the job-complexity scale. If so, firms may invest more resources in screening which may imply that initial mismatch will be priced to a greater extent; this prediction, however, relies on it being equally hard to observe what the relevant skills are in high-level and low-level positions.

Table 8 presents two ways of categorizing jobs into high- and low-skilled positions. In the upper panel we classify the job depending on whether it is a high-skill or a low-skill occupation; in the lower panel we classify the job depending on whether the individual entrant has high or low education. In both cases we interact mismatch with the indicator for a high-skill position.

The basic message of Table 8 is that the impact of mismatch is similar across the distribution of positions. In the upper panel, the only significant difference is the extent to which initial mismatch is priced among experienced workers. In the lower panel, the only significant difference is that high-skill experienced workers are more likely to separate. Neither of these results suggest that there is more information about workers and jobs at the higher end of the labor market.

5.6 Alternative explanations?

Here we raise the issue of whether there are alternative explanations for our results. We address three alternative explanations: (i) peer effects; (ii) preferences; and (iii) differential wage dispersion in different segments of the labor market.

Let us first address the quesiton of whether our results are consistent with a standard peer-effects model. Our analysis has a flavor of peer effects models, since the measurement of mismatch is based on the correspondence between the talents of entrants and tenured workers. However, the are several aspects of our analysis that differentiates it from a (standard) peer effects models. The first difference is that we compare entrants into the

[12]For experienced workers, we find that the impact of mismatch, when evaluated at a standard deviation above the mean, is -1.6%; evaluated at a standard deviation below the mean, the impact is -1.0%. We have also percentile ranked the mismatch index and allowed the effect of mismatch to vary across percentiles. Again, the effect appears to be broadly linear across the mismatch distribution.

Table 7: Robustness

	(1)	(2)	(3)	(4)
Dep var	ENTRY WAGES		SEPARATIONS	
Experience at start of new job:	0-4 yrs.	5+ yrs.	0-4 yrs.	5+ yrs.
A. Baseline				
$Mismatch$	-0.0023	-0.0142***	0.0222***	0.0047**
	(0.0021)	(0.0009)	(0.0063)	(0.0019)
Observations	24,525	133,675	24,525	133,675
R-squared	0.8596	0.8355	0.5964	0.4777
B. No restriction on # tenured workers				
$Mismatch$	-0.0020	-0.0115***	0.0174**	0.0060***
	(0.0025)	(0.0008)	(0.0080)	(0.0017)
Observations	36,194	298,619	36,194	298,619
R-squared	0.9099	0.8840	0.7583	0.6260
C. Weighted mismatch index				
$Mismatch$	-0.0022	-0.0139***	0.0215***	0.0050***
	(0.0021)	(0.0009)	(0.0063)	(0.0019)
Observations	24,525	133,675	24,525	133,675
R-squared	0.8596	0.8355	0.5964	0.4777
D. Cognitive vs. non-cognitive ability				
$Mismatch_{cognitive}$	-0.0029	-0.0095***	0.0138**	0.0036**
	(0.0018)	(0.0008)	(0.0056)	(0.0017)
$Mismatch_{non-cognitive}$	0.0003	-0.0101***	0.0173**	0.0027
	(0.0025)	(0.0010)	(0.0072)	(0.0021)
Observations	24,525	133,675	24,525	133,675
R-squared	0.8596	0.8355	0.5964	0.4777
E. Non-linearities in mismatch				
$Mismatch$	-0.0012	-0.0128***	0.0210***	0.0052***
	(0.0022)	(0.0009)	(0.0065)	(0.0020)
$Mismatch^2$	-0.0013*	-0.0016***	0.0012	-0.0006
	(0.0007)	(0.0003)	(0.0021)	(0.0007)
Observations	24,525	133,675	24,525	133,675
R-squared	0.8596	0.8355	0.5965	0.4777

Notes: Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The specification is the same as in column (1) of Table 4.

Table 8: The importance of job-type

	(1)	(2)	(3)	(4)
Dep. var:	ENTRY WAGES		SEPARATIONS	
Experience at start of new job:	0-4 yrs.	5+ yrs.	0-4 yrs.	5+ yrs.
		JOB-SKILL-TYPES		
$Mismatch$	-0.0008	-0.0186***	0.0221***	0.0045*
	(0.0025)	(0.0010)	(0.0082)	(0.0023)
$Mismatch\times$High-skilled job	-0.0034	0.0050***	-0.0010	0.0013
	(0.0027)	(0.0012)	(0.0083)	(0.0025)
Observations	24,383	130,298	24,383	130,298
R-squared	0.8554	0.8278	0.5957	0.4777
		EDUCATION		
$Mismatch$	-0.0042	-0.0165***	0.0232***	0.0028
	(0.0030)	(0.0010)	(0.0085)	(0.0022)
$Mismatch\times$High education	0.0014	0.0004	-0.0034	0.0041*
	(0.0028)	(0.0013)	(0.0083)	(0.0024)
Observations	24,525	133,675	24,525	133,675
R-squared	0.8571	0.8311	0.5959	0.4806
Education FE:s	√	√	√	√
Entrant test scores	√	√	√	√
(Entry occupation×Entry Year×Plant) FE:s	√	√	√	√

Notes: Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. High-skilled jobs are classified on the basis of the 1-digit occupational code. Managers, professionals and technicians are classified as being high-skilled jobs. High education is defined as all attainment levels requiring some college education. In addition to the control variables listed in the table, the regressions include cohort and experience fixed effects.

same job and these entrants will all be exposed to the same set of "peers", which implies that the first-order effect of peers is accounted for by the job fixed effects. The second difference is that we rely on a vector of talents, which implies that mismatch arises also for people with a similar level (but a different composition) of talents. The third difference is that we sum all absolute deviations between the talents of entering workers relative to the talents of tenured workers, regardless of whether these deviations are positive or negative. In a peers context, it would then have to be that it would be better for an under-skilled worker to work in a less-skilled environment than in a high-skilled environment. For an over-skilled worker, the pattern would have to exactly the opposite; this is not what a standard peer-effects model would predict. All in all, we do not believe that our results should be interpreted within the framework of a standard peer-effects model.

Another alternative explanation is related to preferences of the workers. Workers may have a preference for working with people who have similar traits and talents as themselves. While this could explain why mismatched workers separate to a greater extent, it cannot explain the wage patterns we observe. Indeed, if preference for similarity would be the main driving force, we expect that well-matched workers would pay a compensating wage differential for similarity, that is their wages should be lower than mismatched workers. This is clearly not the patter we observe in the data.

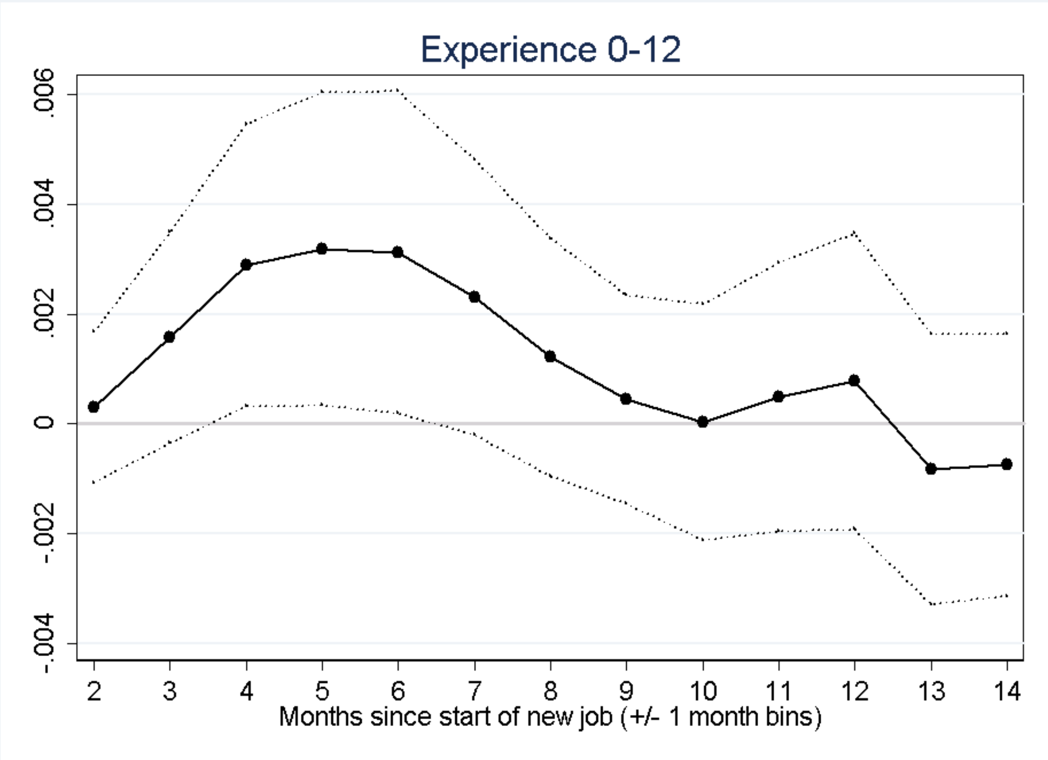
Finally, a potential concern is that experienced and inexperienced workers work in different segments of the labor market. Now, if inexperienced workers are in the lower segments of the market, and there is wage compression from below, this may be one reason we observe that entry wages are unrelated to mismatch for inexperienced workers. However, we think this explanation is unlikely for two reasons. First, within-job wage dispersion is very similar across inexperienced and experienced workers. Second, we do not find any systematic differences when we stratify the analysis by job-level or education (see Table 8).

5.7 Timing of the separation response

Since outcomes only change with the arrival of new information (see Section 2), the timing of the separation response provides information about when initial mismatch is revealed. In order to shed light on this issue, we tap monthly data. Monthly data (described in greater detail in the Appendix) are of course crucial to provide a more detailed account of the adjustment process. However, these data are of somewhat lower quality since there is some uncertainty in determining in what month a separation occured.

Figure 6 shows the separation response by months since the start of the new job. To gain precision we pool all experience groups (the separation response among those with less than 5 years of experience is larger but the time profile is almost identical). As before we use moving averages to increase precision. The first point in the figure represents

Figure 6: Timing of the separation response



Notes: The figure displays the response to initial mismatch within 3 month-bins (+/- 1 month). We calculate the monthly duration of employment using an indicator for the first and the last month of remuneration from each employer. Since entry occupations are measured in September or October (depending on sampling month), we focus on workers that entered their new job in the period August-October of each year.

separations within the first 1-3 months after the start of the new job, the second represents the response after 2-4 months, and so on. The results show that the peak of the separation response is centered on approximately 6 months since the start of the new job. In general, the speed of adjustment is thus fairly rapid and the separation response after 1 year is very limited. Here it could also be noted that Swedish employment protection allows for a 6 months probation period (during which both agents can terminate the contract at will) at the start of permanent contracts.[13] This implies that 6 months could be a focal point, and incentives, from both the employer and the employee side, are to some extent geared towards terminating the contract at 6 months if the match quality is poor.

[13] OECD characterizes Swedish Employment Protection Legislation as being around average in terms of overall strictness. The rules concerning the use of temporary contracts are however very flexible, whereas the rules pertaining to layoffs (in particular for cause) among workers on permanent contracts are rather stringent.

6 Initial mismatch and future earnings

In an attempt to quantify the costs of mismatch, we analyze how future (annual) earnings respond to initial mismatch. The future earnings response includes everyone (i.e. all entrants) who were exposed to initial mismatch, independently of whether they stayed in the initial job, moved on to another job, or left for non-employment.

Table 9 reports the results from separate regressions by experience group. In panel A, the level of earnings 5 years after the time of hiring is related to initial mismatch. To have an interpretable scale, we normalize the dependent variable with mean earnings (separately by experience group). Increases in initial mismatch appear to have rather permanent earnings effects. For the inexperienced group, the future earnings loss implied by a standard deviation increase in mismatch amounts to 1.8%; for experienced workers, the loss amounts to 2.6%.

As a way of making sense of the magnitudes, we relate the estimates to the residual standard deviation of the dependent variable, which is reported in the second row of panel A. The estimates correspond to 4.5 and 5.3 percent of the residual standard deviation of earnings for inexperienced and experienced workers, respectively. However, it should be noted that this calculation may understate the importance of mismatch since we only measure mismatch along the dimensions captured by our talents.[14]

In panels B and C of Table 9 we decompose the overall earnings effect in panel A into an "extensive" and "intensive" margin. The two components roughly correspond to employment and wages, but both are derived from information on annual earnings and the interpretation of these two components may thus be contaminated by variation in annual hours. The extensive margin is captured by an indicator for having annual earnings above a minimum threshold, defined by the full-time annual earnings of janitors. The intensive margin is the log of annual earnings conditional on passing the minimum earnings threshold. The results of the decomposition suggest that earnings losses among inexperienced workers are mainly driven by volatile employment (the extensive margin) after being exposed to mismatch. Earnings losses among experienced workers, however, are mainly driven by a poorer wage prospects after starting a job with poor match quality.

7 Conclusion

We have examined the direct impact of mismatch on wages and job mobility using unique Swedish data containing information on a multitude of talents, detailed occupational

[14]Note also that the earnings impact appears to be persistent, and that the standard deviation of earnings has permanent as well as transitory components. In both Sweden and the US (see Gustavsson 2008 and Gottschalk and Moffitt 1994), the permanent component of the total variance in earnings seems to amount to two thirds. This suggests that 80 percent ($\sqrt{(2/3)} \approx 0.8$) of the overall standard deviation is due to the permanent component. Thus, relative to the standard deviation of permanent earnings, the estimates correspond to 5.7 (6.6) percent for inexperienced (experienced) workers.

Table 9: Earnings 5 years after start of new job and initial mismatch

	Inexperienced (0-4 yrs.)	**Experienced (5+ yrs.)**
	(1)	(2)
A. Earnings/(mean earnings)		
Mismatch	-0.0185**	-0.0257***
	(0.0088)	(0.0038)
Residual SD dep. variable:	0.408	0.484
Observations	21,634	77,975
B. "Externsive margin": Pr(earnings>minimum full-time earnings)		
Mismatch	-0.0156**	-0.0058**
	(0.0072)	(0.0027)
Mean dep. variable:	0.70	0.79
Observations	21,634	77,975
C. "Internsive margin": ln(earnings) conditional on earnings>minimum full-time earnings		
Mismatch	0.0027	-0.0091***
	(0.0048)	(0.0019)
Observations	15,206	61,688
Education FE:s	√	√
Entrant test scores	√	√
(Entry occupation×Entry Year×Plant) FE:s	√	√

Notes: Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Minimum full-time earnings correspond to the monthly wage for janitors, for each year of observation, multiplied by 12. In, e.g., 2007 minimum full-time earnings (so defined) equalled 225,600 SEK (34,000 USD/25,200 EUR). Residual SD in panel A refers to the residual standard deviation in earnings after controlling for all the fixed effects and covariates that we condition on in the regressions.

information, wages, and the identity of the employer. Our empirical approach builds on the idea that any sorting model will imply that tenured workers are selected on having the right skills for the job. To measure mismatch we thus compared how well the talents of a recently hired worker correspond to the talents of incumbent workers performing the same job.

As a prelude to our empirical analysis we showed that each component of our vector of talents (inductive-, verbal-, spatial, and technical ability as well as social maturity, intensity, psychological energy, and emotional stability) is independently valued on the labor market, even conditional on educational attainment. In addition, we document seven novel facts about mismatch: First, we show that the dispersion of talents within job decreases rapidly with tenure; the decline is particularly rapid among inexperienced workers. Second, mismatch is higher among inexperienced workers and workers who have entered from non-employment, i.e., for two groups where information about match quality is likely absent when matches are formed. Third, starting wages are unrelated to mismatch for inexperienced workers and workers hired from non-employment whereas experienced workers and job-to-job movers receive a wage penalty if they are mismatched. Fourth, we find a larger separation response to mismatch among inexperienced workers and entrants from non-employment than among experienced workers and job-to-job movers. Fifth, the adjustment to mismatch is relatively fast: mismatch of talents predicts mobility during the first year after recruitment but not thereafter. Sixth, wage growth within jobs is negatively related to initial mismatch, and this effect is more pronounced among inexperienced workers. Finally, we show that the earnings losses of being exposed to mismatch are persistent. On average, across all new hires, the earnings response to a standard deviation increase in initial mismatch amounts to around 5 percent of the standard deviation in earnings.

We interpret the differential outcomes across groups as being a function of the information available at the time of hiring. For inexperienced workers, and entrants from non-employment, it is realistic to assume that both the prospective employee and the prospective employer fail to observe how well the detailed characteristics of the worker match the skill-requirements of the job. We therefore conclude that inexperienced workers, and those who search from non-employment, appear to match under uncertainty. In contrast, the matching process for experienced job-to-job movers is perhaps best described by a model where information about match quality is available already at the initial matching stage. Overall, the results support the notion that the misallocation of workers, and the inability to observe match quality for marginal applicants, is a fundamental problem in the labor market. The earnings results suggest that the efficiency costs may be substantial.

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Table A1: All male entrants 1997-2008

	mean	SD	median
Separation rate	.29	.46	0
Age	36.4	8.0	36
Experience at entry	11.3	5.5	12
Entry from employment	.73	.44	1
Entry establishment size	144	498	22
Education:			
Primary school less than 7 years	.02	.13	0
Primary 7-9 years	.11	.31	0
High school short (less than 2 years)	.03	.18	0
High school short (2 years)	.28	.45	0
High school long (3 years)	.18	.38	0
College short (less than 2 years)	.11	.32	0
College short (2 years)	.07	.26	0
College long (3 years)	.11	.31	0
College long (4 years)	.08	.27	0
PhD short (Licentiate)	.11	.04	0
PhD long (Doctoral)	.01	.08	0
Observations	2,784,253		

Appendix

Additional descriptives

Monthly data

In addition to the annual employment records used for the main analysis, we have information on the first and last month of remuneration from each employer. We use this information to measure the length (in months) of each employment spell. As previously described, our wage and occupation data are collected during a measurement week once every year (in September-November depending on the employer). Therefore, we calculate the monthly employment duration for entrants who started a new job in August-October, in order to obtain a reliable mapping between the starting month and the entry wage/occupation. The average job spell lasts for 35 months, almost three years.

One potential concern is that the first and last month of compensation are self-reported by the employers, which increases the risk of measurement error. In our sample, 35 percent of the separations occur in December (conditioning on entry in August-October), which seems high even if we consider that a disproportionate number of employment relationships are likely to terminate in December for natural reasons. For the sake of our analysis it is however important to remember that such measurement error will only be a problem if the probability of misreporting is correlated with the degree of initial mismatch, which seems highly unlikely.

Table A2: Correlation between different skills

	Schooling		*Cognitive skills:*				*Non-cognitive skills:*		
	Yrs. of schooling	Inductive skill	Verbal skill	Spatial skill	Technical skill	Social maturity	Intensity	Psychological energy	Emotional stability
Yrs. of schooling	1								
Inductive skill	.50	1							
Verbal skill	.49	.73	1						
Spatial skill	.39	.60	.54	1					
Technical skill	.42	.57	.54	.57	1				
Social maturity	.31	.34	.33	.26	.29	1			
Intensity	.17	.17	.14	.13	.16	.45	1		
Psychological energy	.30	.31	.29	.23	.26	.62	.54	1	
Emotional stability	.29	.30	.29	.24	.26	.63	.47	.56	1