Mismatch of Talent?

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

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Abstract

We provide a first direct empirical documentation of how idiosyncratic match quality affects entry wages and job mobility. The analysis uses a unique data set capturing workers' multidimensional talents matched to job-indicators and individual wages. Mismatch is measured by how well the talents of recent hires correspond to the talents of tenured workers performing the same job. To corroborate this metric, we show that talents become increasingly homogenous among remaining coworkers as tenure increases. We show that mismatch is larger for inexperienced workers and for workers entering from non-employment. We further study the impact of match quality on separations and wages after accounting for all direct (separate) impacts of the talents and the jobs. Throughout, we explore the role of ex ante uncertainty; match quality should have a smaller impact on wages, but a larger impact on separations if matches are formed under limited information. In line with these predictions, we show that mismatch is unrelated to entry wages but have a large impact on separations among inexperienced workers and workers who have entered from non-employment. In contrast, mismatch among newly hired experienced workers and job-to-job movers is priced into their wages, whereas the impact on separations is muted. The results also suggest that most adjustments to mismatch happen within a year.

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1 Introduction

A longstanding notion within labor economcis is that the allocation of workers accross 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). A key element within large parts of this literature is the notion of ex ante uncertainty about match quality.

Although match quality is fundamental and conceptually well-defined, providing direct, and credible, evidence on the importance of mismatch in the labor market has proven to be difficult. The purpose of this paper is to provide such evidence in a setting which allows us to 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 identites of their employers and occupations, and wages.

In the theoretical literature, match quality is often assumed to be unobserved at the time of hiring, but realized ex post, as in the original Jovanovic (1979) model. The typical empirical approach to study the impact of (initially) unobserved match quality has been to analyze realized patterns of exits and wages. A drawback of this approach is the existence of alternative, equally plausible, explanations (e.g., on-the-job training) for the same 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 these particular talents are differentially productive in different specific jobs.

Our empirical strategy exploits the notion that workers should stay 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. Thus, by combining detailed data on entering workers' talents with equally detailed data on the

[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 worker to jobs. A more recent literature combine search frictions and worker/job heterogeneity. Gautier and Teulings (2012) calibrate such a model, and conclude that actual allocations imply very large efficiency losses.

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 compare different entrants into the same job, during the same year, while accounting for the overall market valuation their talents as well as 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, we expect mismatch to be priced into entry wages, and that separations respond only to revelations of mismatch (i.e. mismatch in excess of what was expected at the hiring stage). If mismatch is unobserved at the time of hire, entry wages should be unrelated to mismatch and separations should fully respond to mismatch. The amount of information available to the matching agents is thus key for the wage and mobility responses to mismatch.[2]

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 more recently 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 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. In the second approach we compare 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 or stylized model (and the use of labor market experience as a proxy for information), 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 very pronounced separation response to mismaatch among inexperienced workers and entrants from non-employment, whereas the separation response among experienced workers and job-to-job movers is heavily muted in comparison.

Overall, the results confirm that mismatch between individual talents and job requirements do matter for the labor market outcomes of the involved agents. Uncertainty

[2]The scores themselves are confidential, except for research purposes.

about match quality appears to be crucial for understanding the outcomes amongst inexperienced workers and those entering from non-employment. These groups experience a sharp increase in the risk of job-loss within their first year of employment if they are mismatched.

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 explores how the separation effects vary with duration and the amount of mismatch. Section 7 concludes.

2 Framework

Our empirical analysis takes as its starting point, the general idea that there is a right man for each job. More precisely, we depart from a Jovanovic (1979)-type world whith worker and job heterogeneity where productivity is match-specific. In order to make teh concept of worker heterogeneity more precise, we assume that each worker has a bundle of differnt types of skills$s_k(i)$, $k = 1, ..., K$. The worker's productivity in a given match depends on the relationship between these skills and the technology (skill requirement) of the specific job he performs. We measure the relationship between the skills and the technology by the location of the job and the worker in a 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 let $d = d(i, j)$ denote the aggregate distance between the worker and the job (we make the empirical measure precise later on).[3]

Match productivity, $y(d) = 1-\gamma d$, is decreasing in the distance between the worker and the job, and thus maximal at $d = 0$. Here γ reflects the subsitutability between different skills for a particular job (see Teulings and Gautier 2004). Let denote $y^* = y(d = 0)$ denote maximal match productivity.

Essentially, we follow Eeckhout and Kircher (2011) in modeling the hiring process. This process has three stages: the meeting stage, the revelation stage, and the 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 making less extreme assumptions do not alter the substance of any conclusions. The key is that the outside option is related to the value of attracting the appropriate type.

At the meeting stage, each worker is paired randomly with one job. At this point, the worker and the firm observe an error ridden measure of the productivity of the match ($d^o = d+\epsilon$). They then decide to match or to wait until the frictionless stage. Should they

[3]For now, this can be thought of as the Euclidean distance between the skills of the worker and the job requirements.

decide to match, they agree on an entry wage. If they wait they incur costs (c), which are shared equally between the parties (see Atakan 2006). The benefits of waiting are that workers receive the wage associated with the optimal match, w^*, and firms receive profits π^*.

The outcomes at the meeting stage depend on expected match productivity:

$$E(d \mid d^o) = (1-\alpha)\overline{d} + \alpha d^o$$

where $\alpha = \frac{\sigma_d}{\sigma_d + \sigma_\epsilon}$ and $\overline{d} = E(d) = E(d^o)$ denotes the (common) mean of the distributions. Expected match productivity is thus a weighted average of average productivity and observed match productivity at the meeting stage. With full information at the meeting stage: $\alpha = 1$ and $E(d \mid d^o) = d^o$. With no information: $\alpha = 0$ and $E(d \mid d^o) = \bar{d}$.

At the revelation stage, uncertainty is revealed. The worker-firm pair then 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.

Our purpose now is to compare the two extreme cases: no information ($\alpha = 0$) and full information ($\alpha = 1$).

2.1 No information at meeting stage

This is a stylized version of the original Jovanovic (1979) model. All meetings result in matches (given that $\bar{d} \leq \frac{c-b}{\gamma}$). Entry wages are determined by a surplus sharing rule with no information about actual match productivity. Suppose the worker and the firm split the proceeds of the match equally. The entry wage is then given by

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

The first term in brackets represents the expected gain from matching, while the second term represents the alternative, i.e., waiting. By construction, entry wages are unrelated to actual match productivity (d).

At the revelation stage, match productivity is observed and wages are re-negotiated. The set of continuing matches are defined by

$$y(d) - (y^* - (c+b)) \geq 0 \tag{2}$$

A fraction $p(d_s) = \Pr(d \leq d_s)$ of the matches continues to be viable, while a fraction $1 - p(d_s)$ is destroyed. The separation threshold (d_s) is given by $d_s = \frac{c+b}{\gamma}$.

2.2 Full information at meeting stage

With full information, the match is formed if the productivity of the match is greater than the alternative. The alternative consists of optimal match productivity minus the total cost of delay. The set of matches is thus given by

$$y(d) - (y^* - c) \geq 0 \tag{3}$$

and the fraction of meetings resulting in matches is $p(d_m) = \Pr(d \leq d_m)$ where $d_m = \frac{c}{\gamma}$. Since $d_m < d_s$, no matches are destroyed at the revelation stage.

As above, entry wages (and profits) are determined via a surplus sharing rule. Wages are given by

$$w_0(d) = \frac{1}{2}\left[y(d) - (y^* - c)\right] \tag{4}$$

Since match surplus is falling in distance, so are wages.

2.3 Predictions

Here we outline the main predictions we take to the data:

- *Talent variance and mismatch*: The initial variance of talents is higher with unobservability than with partial observability. When match productivity is partly observed at the meeting stage, all meetings do not result in matches; therefore, we only observe a truncated distribution of skills. If the additional termination cost (b) is small relative to the waiting cost (c), the distribution of talent for tenured workers should not depend on the observability of match productivity.

- *Mismatch and wages*: When match productivity is unobserved, entry wages are unaffected by mismatch, but when match productivity is partly observed, entry wages are negatively affected by mismatch.

- *Mismatch and separations*: The difference in the acceptance decision when match productivity is unobserved and observed implies that the impact of mismatch on separation rates are higher when match productivity is unobserved.[4]

In the empirical analysis we will contrast groups where match productivity is likely to be more difficult to observe at the hiring stage with groups where match productivity is more likely to be observed. Our main approach is to classify workers on the basis of their experience on the labor market. In particular, it is plausible 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 the talents

[4]Observability of match productivity is, for instance, inferred from experience groups. Since this prediction also comes out of any kind of human capital model we do not emphasize this in our empirical work.

of the worker (see Farber and Gibbons 1996 and Altonji and Pierret 2001); such information can come from work histories, previous wages or references. 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 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 Construction of the data

The data come from administrative employment registers collected by Statistics Sweden. 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. The basis of our analysis is all male workers who enter new jobs between 1997 and 2008.[5] To these data we add socioeconomic background characteristics and military enlistment scores for all entrants, and similar information on all incumbent workers in the same plant. 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.

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. 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 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 an 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). 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; see Mood et al. (2010) for a description of the non-cognitive traits.

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

We contrast these eight talents and traits among new hires to those of tenured workers in the same jobs. The rationale for doing this is that tenured workers are selected on having the right skills for the job. Thus, the skills of tenured workers identify the skill requirements for the job. For the purpose of the empirical analysis, tenured workers have at least 3 years of tenure in the current job. To measure the traits of incumbent workers with a reasonable amount of precision, we require that the job (below we define a "job") has at least 10 tenured males with non-missing draft scores within the same 3-digit occupation as the entrant (in Section 5.1 we show that results are similar if we only require one tenured worker). Information on occupation is available for all public establishments and for a sample of private establishments, covering about a third of all private sector workers.[6]

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 the model in Section 2) as:

$$Mismatch_{ij} = \sum_{k=1}^{K} |s_{ik} - \bar{s}_{jk}| \tag{5}$$

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. 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-skilled relative to the skill requirement"). We also present robustness checks with respect to the measurement of mismatch in Sections 5.1 and 6.

It is relevant to ask whether measured talents have informational content. This comes down to questions of measurement error in observed talents and whether they vary independently. Our basic approach to address these issues is to relate wages during prime-age (age 35) to all the measured talents. The bottom line is that they are all individually relevant. On average, a standard deviation increase in talent is associated with an increase of wages by 1.6%, holding educational attainment constant.

Table 1 details these results. It shows the results of regressing male log wages at age 35 on all talents. Column (1) does not control for education, while column (2) controls for education-specific fixed effects. We focus on the prime-aged males since we want to get at the most reliable estimates of the returns to talent.[7]

Table 1 shows that all skill measures have precisely determined returns, even conditional on educational attainment. This is fairly remarkable since in particular sinceGrönqvist

[6]This information is collected in November each year conditioning on being employed for at least one hour during the sampling week. The sampling is stratified by firm size and industry and small firms in the private sector are underrepresented.

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

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: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. 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.

et al. (2010) estimate the reliability ratio of overall cognitive ability to be 73% and the reliability ratio of overall non-cognitive ability to be 50%. Thus, a rough correction for measurement error suggests that the returns are quite substantial for most of the individual talents. For instance, the returns to standard deviation increases in social maturity and emotional stability (measured at age 18-19) are in the order of 4.7-5.3%, holding educational attainment and all other talents constant. But most importantly, the results in Table 1 show that there is independent, and sufficiently precise, variation in the individual measures of talent. Table A2 in the Appendix corroborates the notion that rhere is independent variation along all dimensions by showing the bivariate correlations between educational attainment and the different domains of cognitive and non-cognitive skills.

3.2 Descriptive Statistics

Table 2 shows the characteristics of the entrants in our sample as well as some basic information about the occupations they enter. We define a *separation* as a case when an individual is not observed at the entry establishment during the following two years.[8] To avoid including lay-offs due to plant closure we also require that the plant existed in the year following the separation.

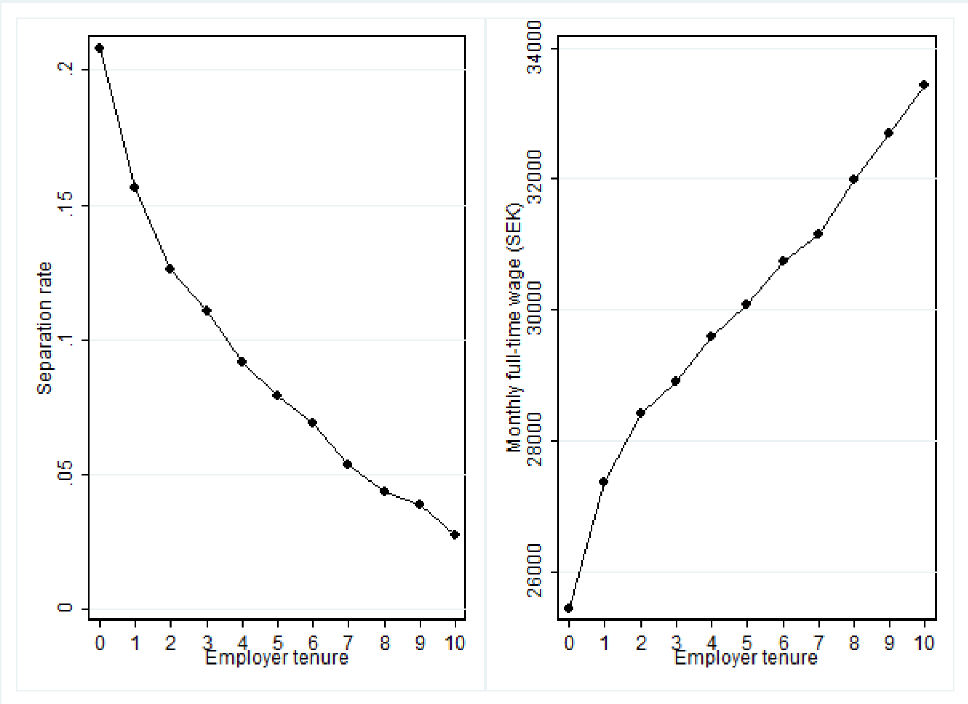
As a result of the sample restrictions, our sample contains larger establishments (and occupations) than in an overall sample of entrants during the same time period. Table A1 contain information which is analogous to Table 2 for all male entrants during 1997-2008. A comparison between Tables 2 and A1 reveals that average entry plant size is 655 which should be compared to 144 in the overall sample. The focus on larger establishments also implies that the separation rate is lower in our sample than in the overall population. The separation rate in the first row of each table pertains to the probability of leaving the establishment within the first year: it equals 21% in our sample as opposed to 29% in the overall sample.

Figure 1 shows the probability of job separation and wages by job tenure for the entrants in our sample. Consistent with the earlier literature, we find a robust negative relationship between tenure and separation and a robust positive relationship between wages and tenure.

We can trace the individuals back to 1985 in our data. Actual experience is defined as the number of years which the individual is classified as being employed according to the registers. Since this information is only available back to 1985 we truncate experience at 13 years of experience for all entrant cohorts. The median entrant in our sample is 35 years old, and has 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 expe-

[8]We impose the two year requirement to avoid defining recalls as quits.

Figure 1: Separations and wages by tenure



rienced workers have at least 5 years of experience. The separation rate is higher among inexperienced workers and they are job-to-job mover to a lesser extent than experienced workers.

Our analysis use the variation in mismatch within an jobs. We define a *job* as an occupation×plant×(entry year) cell. We use (the Swedish version of) the ISCO-88 (International Standard Classification of Occupations 1988) standard at the 3-digit level. Occupation is reported by the employer and the 3-digit level allows us to distinguish between 113 occupations (for instance accountants/lawyers or mining/construction workers). Our 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.

The lower half of Table 2 lists the distribution of entry occupations, at the 1-digit level. The most common 1-digit occupations are "professionals", "technicians", and "machine operators", which taken together comprise 71% of our sample.

The bottom row of Table 2 shows the 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.

4 The variance of talents by tenure and experience

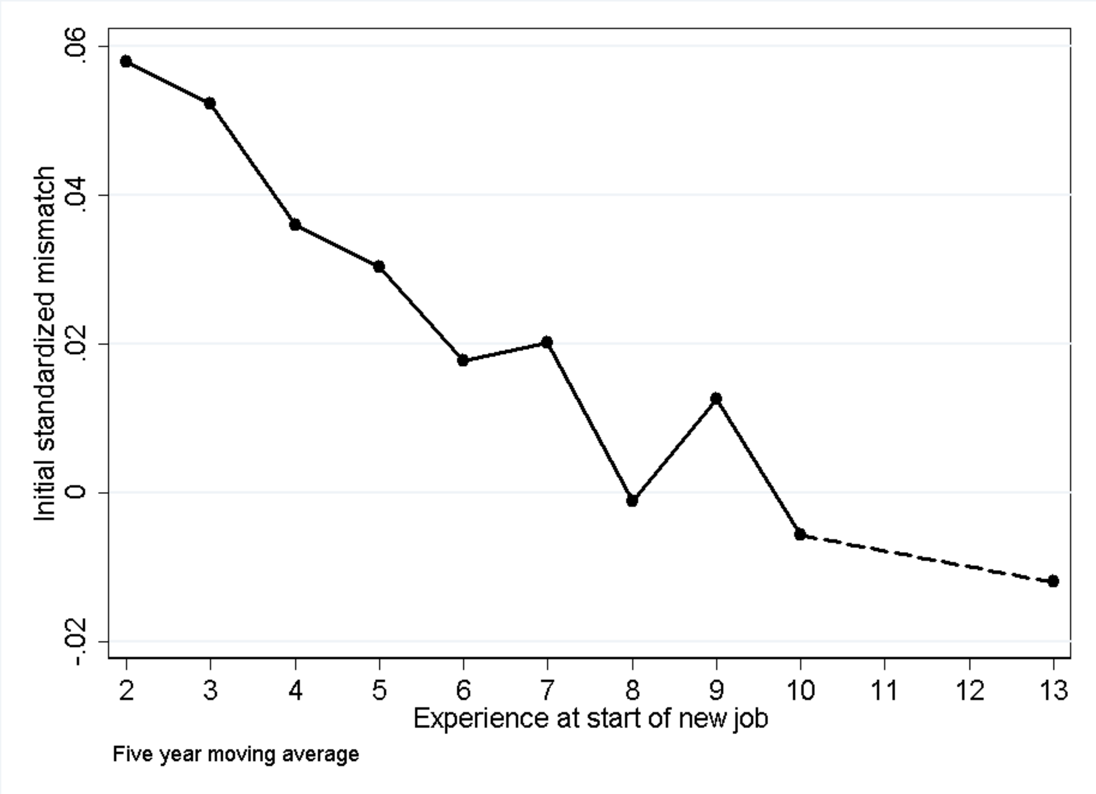
As argued in Section 2, the observability of match quality is likely to different across experience groups. In particular, initial match quality is likely unobserved for inexperienced

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
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 2: Initial mismatch by experience



workers and partly observed for experienced workers. If so, we should observe more mismatch in realized matches among inexperienced workers than among experienced workers.

Figure 2 examines whether this supposition is reasonable. It plots initial mismatch among new hires by labor market experience at the start of the new job. To get at the broader picture, we present 5-year moving averages of experience: 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 figure clearly shows that initial mismatch decreases with experience. Initial mismatch is 0.06SD 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).

Table 3 presents some descriptive regression evidence on the same issue. We regress initial mismatch on an indicator for being inexperienced and an indicator for having entered from another job (the alternative is entering from non-employment). The results in column (1) imply that mismatch is 0.046 standard deviations higher among inexperienced than among experienced workers; 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 indicator with experience fixed effects. Job-to-job movers thus, on average, have better match quality.

On a similar note, figure 3 presents the the within-job variance in skills by tenure and type of skill (cognitive/non-cognitive) separately for inexperienced and experienced work-

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	√	√
Entrant test scores	√	√
(Entry occupation×Entry Year×Plant) FE:s	√	√
Experience FE:s		√

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.

ers. The pattern is rather striking. The initial within-job variance facing inexperienced workers is 0.07-0.09 SD higher than among experienced workers. Both groups respond to mismatch. As mismatched workers leave the job, inexperienced workers become more like experienced workers. After 4 years of tenure at the firm, the within-job variance is marginally higher among inexperienced workers than among experienced workers. The fact that the variances converge for the two groups suggests that any additional separation costs are marginal relative to the overall costs of mismatch.

5 Match quality, entry wages and separations

Here we probe deeper into the predictions outlined in Section 2. In particular, we present regression evidence on the relationship between entry wages and mismatch and 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.

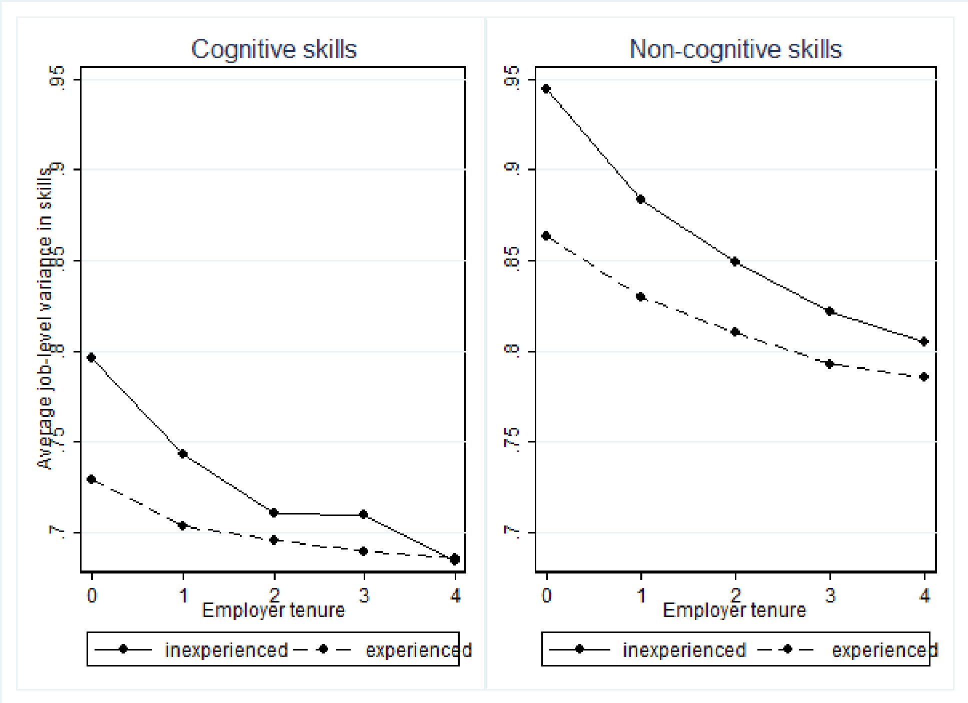
In particular, we run regressions where entry wages and separations are related to mismatch at the time of the hire. The regressions have the following basic structure:

$$\ln(Entry\,Wage_{ij}) = \beta_w Mismatch_{ij} + g_w(s_i) + \gamma_w X_i + \lambda_j^w + \epsilon_{ij}^w \tag{6}$$

$$1^{st}\,year\,Separation_{ij} = \beta_s Mismatch_{ij} + g_s(s_i) + \gamma_s X_i + \lambda_j^s + \epsilon_{ij}^s \tag{7}$$

separately by experience group. In equations (6)-(7), 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); $g_w(s_i)$ and $g_q(s_i)$ are flexible control functions (vectors) in individual skills.

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 is 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).

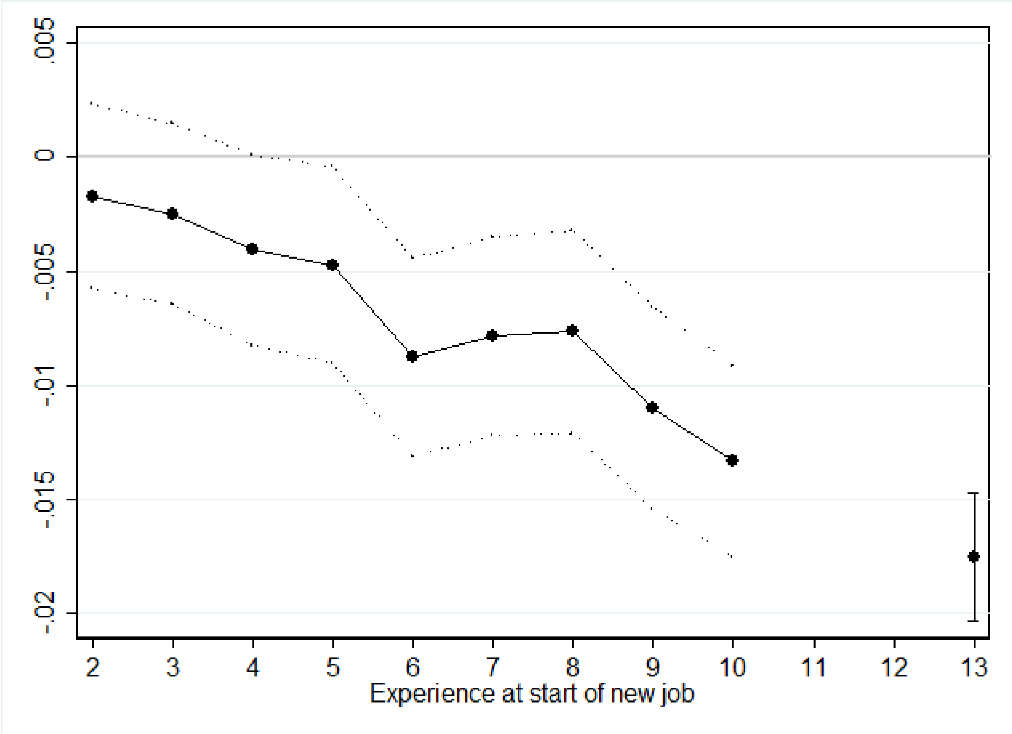
One reason to include flexible skill controls is that mismatch essentially is an interaction between worker skills and the skill requirements of the job, and interactions without main effects are hard to interpret. Moreover, the skill controls hold outside opportunities for the worker constant. We include 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 (including skill requirements of the job and job amenities) and correspond to the level we use when we calculate skills among tenured workers.

The coefficients of interest are the coefficients on the mismatch index ($\beta_w$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.

Figure 4 presents the first set of results, which pertain to the relationship between entry wages and mismatch by experience group. It shows that entry wages are unrelated to mismatch for the first two experience groups (which include workers with 0-5 years of experience). For workers who are more experienced than that, there is a negative effect of mismatch on the wage. For workers with at least 13 years of experience at the start of the new job, a standard deviation increase in mismatch lowers entry wages by 1.7 percent.

Figure 4: Entry wages and 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 is observed until 1985 and truncated at 13 years for workers with 13 years experience or longer.

Figure 5 shows the results for separations. For inexperienced workers a standard deviation increase in mismatch raises separation by 2.2 percentage points. Relative to mean separation rates among inexperienced workers, this corresponds to almost 9%.

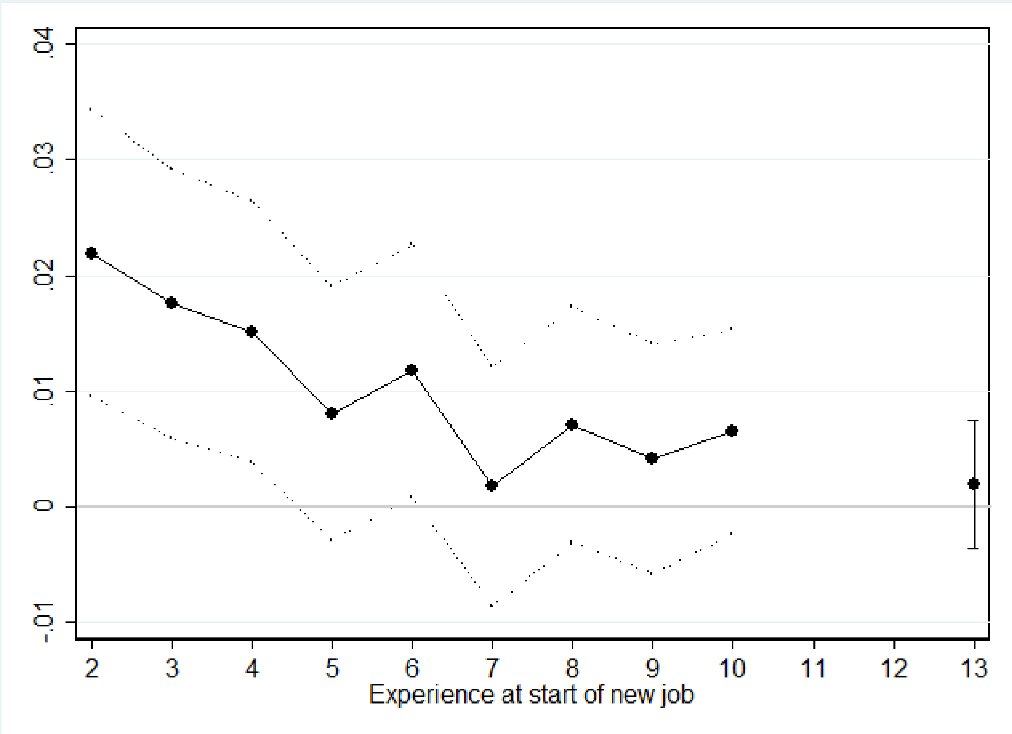
Table 4 presents the numbers underlying Figures 4 and 5. The upper panel pertains to inexperienced workers and the lower panel to experienced workers.

Column (1) shows the results of the baseline specification, which also underlies Figures 4 and 5. 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 of workers a standard deviation increase in mismatch reduces wages by 1.4%.

Column (1) also shows that the separation response to mismatch is greater among inexperienced workers than among experienced workers. A standard deviation increase in mismatch increases separations among inexperienced workers by 2.2 percentage points and by 0.5 percentage points among experienced workers.

In column (2) we make the definition of a "job" even more precise. In particular, a job is defined as a cell defined by the interaction between 3-digit occupation, plant, entry year, and education level. In practice, we are thus saying that, e.g., a lawyer in a given plant has a different job if he or she has 4 years of university education than if he or she has 3 years of university education. As a by-product of this extension we also examine

Figure 5: Separations and 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 is observed until 1985 and truncated at 13 years for workers with 13 years experience or longer.

whether the results are confounded by potential mismatch in educational attainment.

Column (2) shows that this extension has no implications for the baseline results. There are only marginal changes to the coefficients of interest. Since, the more elaborate specification produces no changes in the results, we stick to the simpler baseline specification in the remainder of the paper.

The results in Table 4 are much in line with the interpretative framework of Section 2. Because there is more information about experienced workers, and experienced workers have more information on where their skills are most apt, entry wages are negatively related to mismatch. Relative to a well-matched worker with identical 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 only respond to mismatch that was unexpected relative to the information available at the time of hiring.

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 getting at the information component. Presumably, there is more information about workers entering from another job, and this group is also better informed on where their skills are more apt, than those entering from non-employment.

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.

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.

Column (1) presents the results for workers entering from non-employment, and column (2) shows the results for those entering from another job. The flavor of the results are very similar to Table 4. For those entering from non-employment, entry wages are unrelated to mismatch; the effect on separations is positive and amount to an increase by 1.2 percentage points for a standard deviation increase in mismatch. For job-to-job movers, mismatch is negatively priced and there is a smaller separation response than among those entering from non-employment (although not significantly so).

5.1 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 analyses are displayed in Tables 6 and 7. Throughout we use the specification in column (1) in Table 4. Table 6 presents the results for entry wages and Table 7 the results for separation.

For easy reference we display the baseline results in Panel A. of Tables 6 and 7. Panel A in each table reiterates the basic pattern: there is no relation between entry wages and mismatch among inexperienced workers; as a consequence, separation rates respond more strongly to mismatch in this group than in the other group. The punchline of Tables 6 and 7 is that these baseline results are robust to alternative conceivable specification.

Panel B reports the results from a first robustness check. In order to have a reasonably precise measure of the skill requirements of the job, panel A restricts the analysis to jobs

Table 6: Robustness: Entry wages and mismatch

	(1)	(2)
Experience at start of new job:	0-4	5+
A. Baseline		
Mismatch	-0.0023	-0.0142***
	(0.0021)	(0.0009)
Observations	24,525	133,675
R-squared	0.8596	0.8355
B. No restriction on # tenured workers		
Mismatch	-0.0020	-0.0115***
	(0.0025)	(0.0008)
Observations	36,194	298,619
R-squared	0.9099	0.8840
C. Weighted mismatch index		
Mismatch	-0.0022	-0.0139***
	(0.0021)	(0.0009)
Observations	24,525	133,675
R-squared	0.8596	0.8355
D. Cognitive vs. non-cognitive ability		
$Mismatch_{cognitive}$	-0.0029	-0.0095***
	(0.0018)	(0.0008)
$Mismatch_{non-cognitive}$	0.0003	-0.0101***
	(0.0025)	(0.0010)
Observations	24,525	133,675
R-squared	0.8596	0.8355

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.

with at least 10 tenured workers. Panel B drops this restriction and thus includes all jobs with at least one tenured worker. Without the restriction, sample size increases substantially. Panel B shows that 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.

Panel C entertains the idea that mismatch in terms of certain skills are 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 inquires whether it is mismatch in the cognitive or non-cognitive dimension that primarily matters. The coefficients on mismatch along the cognitive and non-cognitive dimension are not significantly different from one another. Interpreted literally, however, the differences in the two sets of coefficients imply that there is less observability of mismatch along the non-cognitive dimension.

Table 7: Robustness: Separations and mismatch

	(1)	(2)
Experience at start of new job:	0-4	5+
A. Baseline		
Mismatch	0.0222***	0.0047**
	(0.0063)	(0.0019)
Observations	24,525	133,675
R-squared	0.5964	0.4777
B. No restriction on # tenured workers		
Mismatch	0.0174**	0.0060***
	(0.0080)	(0.0017)
Observations	36,194	298,619
R-squared	0.7583	0.6260
C. Weighted mismatch index		
Mismatch	0.0215***	0.0050***
	(0.0063)	(0.0019)
Observations	24,525	133,675
R-squared	0.5964	0.4777
D. Cognitive vs. non-cognitive ability		
$Mismatch_{cognitive}$	0.0138**	0.0036**
	(0.0056)	(0.0017)
$Mismatch_{non-cognitive}$	0.0173**	0.0027
	(0.0072)	(0.0021)
Observations	24,525	133,675
R-squared	0.5964	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) in Table 4.

Table 8 examines whether mismatch has different implications depending on whether the position 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. 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 relevant skills 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.

6 Timing and dose response

Here we raise two questions: (i) How long does it take until agents respond to mismatch? (ii) Does the degree of mismatch matter? The first question deals with the issue of how long it takes until information about initial mismatch is revealed. The second question addresses the measurement of mismatch

6.1 Timing: When do individuals respond to mismatch?

In addressing this question we tap monthly data. The monthly data are of somewhat lower quality, but have the obvious advantage of providing a more detailed account of the adjustment process. The monthly data are described in greater detail in the Appendix.

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. The first point in the figure represents separations within the first 1-3 months after the start of the new job, the second the response after 2-4 months, and so on.

Figure 6 shows that the peak of the separation response is centered on 6 months since the start of the new job. The adjustment is, in general, remarkably rapid. Essentially there is no separation response after 1 year.

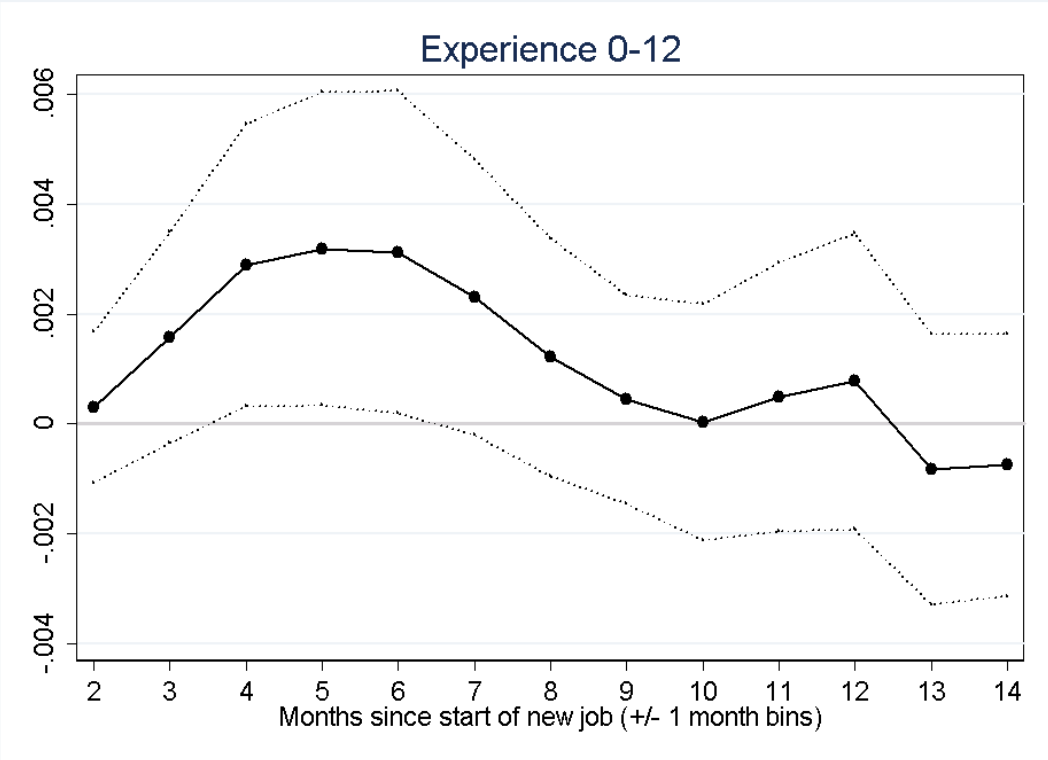
That there is a peak at 6 months is in accordance with Swedish labor market in-

Table 8: Robustness: 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×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×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.

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.

stitutions. The first 6 months is a probation period, where employers can terminate the contract at will. This implies that 6 months should be a focal point, where incentives, from both the employer and the employee side, are geared towards terminating the contract at 6 months.

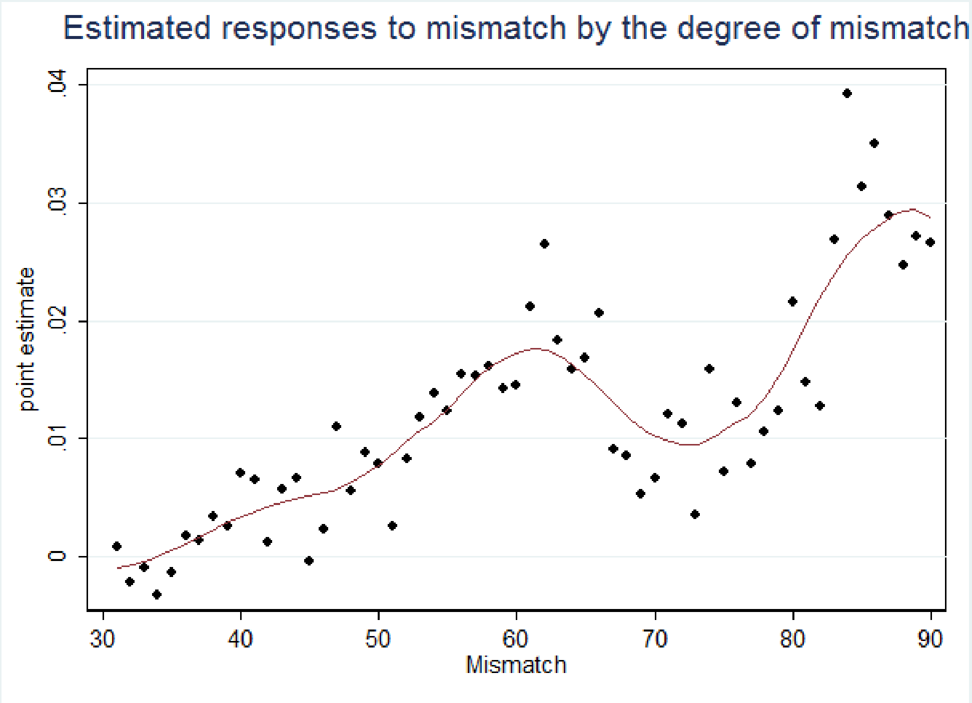
6.2 Dose: Adjustment across the distribution of mismatch

Figure 7 shows separation responses to mismatch across the distribution of mismatch. There are two reasons for pursuing this extension. First, one may suspect that there are ranges of inaction, either because there is some measurement error in skills or because there are mobility/separation costs. In this scenario, there is an initial range of inaction; but when mismatch surpasses a certain threshold there is a separation response. Such behavior would thus yield a non-linear response to mismatch.

The second reason is that there is some arbitrariness in calculating the mismatch index. The correct functional form of mismatch depends on the production technology, which we have no information about.

To avoid imposing a particular functional form we percentile rank the mismatch index.

Figure 7: Separation responses across the distribution of mismatch



Notes: Each dot shows the estimated average response to mismatch in 20 percentile bins in the mismatch distribution compared to the lowest quintile. The fitted line is from a local polynomial regression estimated using the Epanechnikov kernel with a bandwidth of 2.97.

Each point in Figure 7 reports the relation between separations and a particular segment of the mismatch distribution. The segments are defined as $p \pm 10$; the first dot in the figure thus compares the separation response for those on the 20th to the 40th percentile of the mismatch distribution to those at the bottom quintile of the distribution (which is the omitted category throughout).

Generally, the separation response to mismatch is increasing in the extent of mismatch. There is some evidence of a non-linearity around the 70th percentile (corresponding to the 4th quintile), but this is well within the standard error of the estimates (which we have suppressed in this graph).

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 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.

Using these data we have documented five interesting and novel facts. We first show that the vector of talents (inductive-, verbal-, spatial, and technical ability as well as social maturity, intensity, psychological energy, and emotional stability) are independently valued on the labor market, even conditional on educational attainment. Second, we document that the dispersion of talents within job decreases rather rapidly with tenure; the decline is particularly rapid among inexperienced workers. Third, mismatch is unrelated to entry wages among inexperienced workers and workers who have entered from non-employment, i.e., for two groups where information about match quality is likely absent; in contrast, 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.

We think the likely explanation for the third and fourth facts is the amount of 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 for the job. Notice that this information friction does not rely on the assumption that both sides of the market are unable to infer the market values for each agent. Throughout our analysis we hold the overall market valuation of talents (and educational attainment), as well as the market valuation of the job, constant.

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Appendix

Additional descriptives

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		

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

Monthly data

TBA

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