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Language-skill complementarity: returns to immigrant language acquisition

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Abstract

We examine how language acquisition affects immigrant earnings growth for Soviet immigrants to Israel. Using retrospective information on linguistic proficiency to control for heterogeneous ability, we find that language complements high-skill occupations. Improved Hebrew accounts for 2/3 to 3/4 of the differential in earnings growth between immigrant and native programmers and technicians. In contrast, immigrant construction workers and gas station attendants have no wage convergence with natives, with language acquisition having no discernible effect. These findings invite reinterpretation of previous studies on returns to language, as positive estimated returns to language acquisition in cross-sections may suffer from (positive) ability bias in low-skilled occupations.

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1. Introduction

Economists generally agree that immigrants experience faster wage growth than do native workers. One explanation is that over time immigrants learn the host-country language and thereby become more productive in the labor market. Considerable research supports the view that in a cross-section regression, fluency can account for a significant portion of that wage convergence.

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However, the cross-sectional evidence is subject to several problems. On the one hand, both the correlation between fluency and earnings and the correlation between fluency and time in the host country may be partially or totally spurious, thereby generating upwards-biased estimates of the importance of fluency for wage convergence. On the other hand, measures of fluency are very noisy probably biasing estimated coefficients downwards.

The major alternative explanation for convergence is that recent immigrants, like young natives, engage in considerable job shopping. Immigrants exhibit considerable job mobility (Lalonde and Topel, 1991; Eckstein and Weiss, 1998). They may be engaged in Burdett/Jovanovic job-matching or simply seeking jobs with greater rents. Except for recent labor market entrants, native workers will have had more time to shop for jobs than otherwise comparable immigrants and will therefore have a smaller marginal return to job search. Of course, job shopping and language may be complementary. Knowledge of the native language may facilitate job search. Similarly, the jobs with which workers are best matched may change rapidly as they acquire fluency in the language of the host country.

Accurate measures of the effect of language acquisition on wages are important. If growing fluency accounts for a significant portion of wage convergence, receiving countries can speed that convergence by supporting effective language programs. If, on the other hand, language is relatively unimportant, language programs may be economically wasteful or merely a disguised form of welfare.

In this paper, we use a unique data set collected by one of the authors to cast light on the role of language acquisition in wage convergence within jobs. The data contain measures of fluency and wages, both currently and when the individual started the job, thereby allowing us to measure the effect of changing fluency on the change in wages. Because of the way the data are measured, there is likely to be less measurement error in the change than in the level. Moreover, since we follow workers within jobs, we can distinguish wage growth within jobs from wage growth due to job shopping.

We study immigrants from the former Soviet Union (hereafter Russians) to Israel who were employed in one of four occupations in Israel (gas station attendant, construction worker, computer technician, software engineer). We find that Hebrew fluency had almost no effect on wage growth in the low-skill occupations (gas station attendant, construction worker). Moreover, these occupations show no evidence of wage convergence. In contrast, computer technicians and software engineers show evidence of considerable wage convergence, much of which can be accounted for by increasing Hebrew fluency among workers in these occupations.

We interpret our findings as strong evidence for an effect of language on earnings, as it is free of a bias due to time-invariant heterogeneity in ability. The contrast between the lack of estimated returns to language for low-skill workers and high returns for high-skill workers is interesting for two reasons. First, it provides some evidence against the argument that faster wage growth among those who learn Hebrew more rapidly is due to their ability to learn many skills more quickly. More importantly it establishes evidence for an intuitively plausible result, that language complements occupational skills.

Section 2 reviews the literature. Section 3 provides an overview of Soviet immigration to Israel. Section 4 describes the data. Section 5 describes methods. Section 6 provides results and Section 7 concludes.

2. Literature review

Since the pioneering research of Chiswick (1978) and related work by Carliner (1980), it has been widely recognized that the earnings of immigrants increase more rapidly than those of natives. Subsequent research by Borjas (1985) engendered a lively debate regarding whether immigrants tend to surpass equivalent natives and about the extent of bias in cross-sectional estimates of ‘‘catch-up’’ (see, for example, Friedberg, 1992; Duleep and Regets, 1996). Nevertheless, researchers generally agree that immigrant wages rise relative to native wages as the time spent by the immigrant in the receiving country increases (Borjas, 1994).

Borjas (1994) argues that we know relatively little about why wages of immigrants and natives converge. Although there are a number of plausible hypotheses, the only one that has received extensive study is the view that immigrants' relative wages rise as they master the language in the receiving country. There is considerable evidence that knowledge of the host-country language is correlated with higher wages and that knowledge of the host-country language is correlated with years since migration.[1]

However, Borjas argues that this evidence is not convincing because ‘‘English proficiency and earnings might be correlated simply because more able workers are more likely to speak English and to earn more’’. He goes on to recognize that some researchers (e.g., Chiswick and Miller, 1992) have tried to correct for the potential endogeneity of language knowledge by using instrumental variables techniques, but he questions the exogeneity of the identifying instruments. More recent work (Dustmann and van Soest, 2001) uses fathers' education as an instrument for language. The authors argue that immigrants do not obtain networks through their parents and thus education is exogenous to wages. However, the exogeneity assumption is questionable to the extent that parental education is correlated with unobserved investments in children's human capital other than language or is correlated with unmeasured ability.

There is also reason for concern that estimates of the effect of years since migration on linguistic proficiency are biased. Dustmann (1999) finds that individuals who intend to spend less time in the host country are less likely to know the host-country language. If immigrants who fail to master the host language return to their home country or if those, whose immigration is temporary, fail to learn the language, the estimated effect of time in country on language facility will be biased.

Finally, if the type of immigrants admitted to a country changes over time, differences in language knowledge may reflect cohort rather than time in country effects. Carliner (2000) addresses this last problem by using synthetic cohorts. He establishes that within a cohort, language fluency increases with time spent in the United States. However, synthetic cohorts cannot be used to control for the effect of return migration on the estimates.

So far, we have concentrated on reasons that estimated effects of language acquisition on the convergence of immigrant and native earnings may be biased upwards. However,

[1] See Chiswick (1998) and the references therein as well as Carliner (1996, 2000), Chiswick and Miller (1999), and Hayfon (2001).

there is a strong reason to believe that the estimates are biased downwards-measurement error. Knowledge of a language is uniformly measured as self-reported fluency, in a small number of crude categories. Individuals may vary in their assessment of what constitutes "good" or "very good" knowledge of the native language. In addition, individuals may themselves give inconsistent answers. Dustmann and van Soest (2001, 2002) analyze knowledge of German in the German Socio-Economic Panel (GSOEP). Most of the immigrants in the GSOEP survey had been in Germany for quite a while.[2] Consequently, the sample shows little or no improvement in German over time. This feature of their sample makes it particularly useful for studying the effect of measurement error on the estimated return to fluency.

Dustmann and van Soest show that for this sample, reported knowledge of German is unchanged from one survey to the next in 58% of the cases and is as likely to decline from one survey to the next as it is to improve! They show further that within individual variation accounts for 28% of the variation in reported fluency. Since some of the between individual variation in reported fluency is likely also due to measurement error, the fluency variable must be very noisy. Using reported fluency from other years to instrument for current reported fluency almost triples the estimated effect of fluency on earnings and reduces the estimated effect of years since migration on earnings to close to zero.[3]

While our discussion so far has treated the effect of language as constant across individuals, it is plausible that the return to language differs across individuals. Those studies that allow the return to vary across education or occupation groups confirm this (see for example McManus et al., 1983; Dustmann and van Soest, 2001; Carliner, 1996; Hayfron, 2001).

In particular, Eckstein and Weiss (1998) find faster wage growth among more skilled immigrants than among the less skilled. They term this "rising prices of imported skills", though they remain agnostic as to whether it is due to an increase in demand for imported skill or to an increase in its quality.[4] Both education and working in an occupation, which requires postsecondary education, are predictors of Hebrew ability for previous cohorts of immigrants (Beenstock, 1996; Chiswick and Repetto, 2001). Beenstock (1996) also reports that Hebrew ability is a predictor of employment. Thus, it is plausible that increasing fluency raises the relative productivity of skilled workers by making their human capital more usable.

The combination of faster wage growth and quicker improvement of Hebrew among more skilled workers does not necessarily imply that language complements skill. There is evidence of considerable job turnover among immigrants. Skilled workers may take more time to acquire information about appropriate matches in the labor market. Matching takes time. Learning a language also takes time. Therefore, fluency and match quality may well be correlated, but the relation need not be causal.

To summarize, an ideal study of the effect of language on the assimilation of immigrants would address at least the following four issues—correlation between

[2] Mean years in Germany varied between 14.6 and 21.3 years depending on the wave of the survey.

[3] Note that since within individual variation in fluency appears to be almost entirely measurement error, they cannot use the panel nature of the data to correct for the other biases we discuss.

[4] Eckstein and Weiss (1998), page 7.

unobserved ability and fluency, spurious correlation between fluency and time in host country, measurement error, and inter-individual variation in the return to fluency. Moreover, it would distinguish between the returns to job shopping and to returns to language.

The data and approach we use in the following sections do not allow us to fully address all of these issues. Nevertheless, we are able to largely mitigate their effects by using retrospective information on wages and linguistic proficiency within the same job. We will argue that our data are relatively, although not completely free of the sorts of bias, discussed above. Before discussing our data and approach in detail, we turn to a brief discussion of Russian immigration to Israel.

3. Russian immigration to Israel

In 1989, the Soviet Union conducted a major policy shift, removing restrictions and allowing free migration of Jews to Israel, while the US reduced access to Soviet immigrants by restricting the application of refugee status. As a result, a large wave of immigrants began arriving in Israel in the Fall of 1989. By 1995, about 600,000 immigrants had arrived, increasing the Israeli population by 12%.

It is worth stressing that, in contrast to the high cost of migration for earlier waves, who faced confiscation of property and often lost their jobs when applying for exit permits, migration to Israel since 1989 has been much easier. Recent immigrants faced virtually no exit restrictions in the CIS and have arrived in a country with a significant Russian subculture. Overall, the absorption of this wave of immigrants has been surprisingly successful. Immigrants from the former Soviet Union have improved their standards of living fairly quickly with relatively little culture shock (Beenstock and ben Menahem, 1997; Friedberg, 2001).

In relation to the literature on immigration, the low cost and high return to migration for the current wave make them an unusual group of immigrants in the sense that self-selection is probably much less important for this group than for other immigrants studied in the literature (Chiswick, 1978; Borjas, 1987).

4. Data

Our primary data source is the Workplace Occupational Survey (OS), a survey of male workers in workplaces with a high proportion of immigrants in 1994, 5 years into the large wave of migration from the former Soviet Union to Israel.[5] The survey covered 348 immigrants who had arrived since 1989 and 603 natives working in the same occupations and workplaces.

The most valuable feature of these data is retrospective questions on earnings and language ability on entry into the current job. This method is consistent with recent insights from survey design (Belli et al., 2001) which stress the importance of focusing on significant

[5] For details see Siniver (1998).

events to minimize measurement error in responses. The idea is that in a retrospective question, earnings and language ability will be much easier to recall for a memorable date such as the date of hire than for an arbitrary date, such as January 1 of last year.

The strength and weakness of the OS sample is that it focuses on four occupations in which there were high concentrations of Russian immigrants. As a result, it provides relatively large samples in these individual occupations although, at under 100 per occupation, they are still modest in absolute size. On the other hand, there is no guarantee that Russian immigrants in these occupations are representative of Russian immigrants as a whole. This problem may be exacerbated by the fact that we observe only those immigrants who are in these occupations at a particular point in time, 1–5 years after immigration. If exit from these occupations is nonrandom, our sample may not even be representative of immigrants in these occupations over time. We address the issue of representativeness by comparing the OS with a national sample of immigrants.

For comparison, we draw on the Israel Central Bureau of Statistics' Income Survey (IS), a long form applied to outgoing rotations of the Labour Force Survey. This is a household survey, which currently samples about 7000 households per year, reporting detailed information about individuals aged 15 and older.[6]

Table 1 reports descriptive statistics for the OS, with a sample of immigrants from the IS included for comparison. Male immigrants in the OS are surveyed in four occupational groups: software, technicians, construction workers and gas station attendants.[7] This grouping was designed to cover both the high and low skill ends of the occupational distribution of immigrants. For comparison, about 22% of recent male Soviet immigrants are scientists, academics, professionals and technicians, the equivalent high-skill occupational groups, and about 12% are unskilled workers in services or production workers in manufacturing which are roughly equivalent low-skill occupations. The OS tended to survey younger workers, with a mean age of 30, almost 10 years younger than the IS mean. OS workers average 0.8 years less education and 14% lower earnings. These differences seem to be mainly due to the occupations chosen. Natives in the OS averaged 31.4 years of age and 12.4 years of education (not shown). The mean Soviet immigrant in both data sets had been in Israel for 3 years.

Job tenure in the OS is short, averaging 1.3 years. This is due to both the short interval since migration and high turnover in construction and gas stations. (See Table 5 for descriptive statistics for each of the four OS occupational groups.) Proficiency in spoken Hebrew is self-assessed and measured on a scale of 1 to 5 corresponding to the classifications: "not at all", "a little bit", "not so well", "well", and "very well". The average score was 2.96 on entry into the current job and 3.32 when interviewed.

[6] The LFS population is Israel's permanent population aged 15+, including potential immigrants and permanent residents staying abroad for up to 1 year. Sampling is conducted in two phases: in phase 1, localities are sampled. In phase 2, households are sampled within localities. Probability of inclusion for each household in the population is approximately 1%. The sample is drawn once a year, and divided into four "panels". Panels are interviewed for two consecutive quarters, not interviewed for the next two and then interviewed for another two consecutive quarters. The sample in each quarter is composed of four panels spanning two or three sampling years. See Israel Central Bureau of Statistics (various years) for details.

[7] Technicians were surveyed in eight different companies, software engineers in nine. Twenty gas stations and 18 construction sites were surveyed.

5. Methods

Our goal is to estimate the effect of linguistic proficiency on wages. The now standard approach is to estimate an equation of the form:

$$\ln(w_{it}) = \alpha_i + \beta' z_i + \gamma x_{it} + \delta_1 y_{it} + \delta_2 y_{it}^2 + \rho_1 v_{it} + \rho_2 v_{it}^2 + \omega h_{it} + \epsilon_{it}, \tag{1}$$

for $i = 1 \ldots N$ persons and $t = 1 \ldots T$ periods. Here, w is monthly earnings, x is labor market experience, y is years since migration and v is current job tenure. The variable h measures Hebrew language proficiency. The individual effect α_i represents a time invariant influence on earnings, which we label “ability”.

The coefficients we seek to estimate in Eq. (1) are the causal effects of the covariates on wages, that is, those we would recover from the population regression with the values of covariates randomly assigned. Cross-sectional estimates of coefficients will be biased if unobserved ability is correlated with the covariates. The coefficient on Hebrew is especially suspect since one’s ability to learn a language will be reflected in h but may also be correlated with unobserved earning ability, α_i.

This ability-bias can be addressed by estimating

$$\Delta ln(w_{it}) = \gamma \Delta x_{it} + \delta_1 \Delta y_{it} + \delta_2 \Delta y_{it}^2 + \rho_1 \Delta v_{it} + \rho_2 \Delta v_{it}^2 + \omega \Delta h_{it} + \Delta \epsilon_{it}, \tag{2}$$

for $i = 1 \ldots N$ persons and $t = 1 \ldots T$ periods. These coefficients can be consistently estimated if, as defined in Eq. (2), is uncorrelated with the covariates. That condition implies, in particular, that there is no unobserved individual effect in earnings growth, which is correlated with improvements in Hebrew. In other words, we make the strong assumption that α_i is time invariant. (For instance, this assumption would be violated if match quality and Hebrew knowledge both improved more rapidly for skilled workers).

The unique feature of our data which makes estimation of Eq. (2) feasible is longitudinal observation of proficiency in Hebrew. We use a retrospective question regarding Hebrew ability at entry into the current job, along with information about the entry wage. These allow us to estimate the coefficients of

$$\Delta ln(w_i) = \delta_1 \Delta y_i + \delta_2 \Delta y_i^2 + (\gamma + \rho_1) \Delta v_i + \rho_2 \Delta v_i^2 + \omega \Delta h_i + \Delta \epsilon_i, \tag{3}$$

where: (a) the difference operator Δq indicates the difference between the level of q in the survey year and its level on entry into the current job; (b) $\Delta x = \Delta v$, since the change in experience and tenure are identical within the current job. Thus, the sum of experience and tenure coefficients $(\gamma + \rho_1)$ can be estimated but not the separate coefficients; and (c) $\Delta y = \Delta x$ for immigrants but $\Delta y = 0$ for natives, which provides enough variation to identify δ_1, the coefficient on years since migration (with the implicit assumption that the sum of returns to experience and tenure $(\delta + \rho_1)$ are the same for immigrants and natives).

Our aim is to estimate to what extent the faster wage growth of immigrants is due to improvement in Hebrew. Three important points should be recognized about our attempt to answer that question by estimating Eq. (3). First, all the variation in y, increased years

Table 1
Male immigrants from the (former) Soviet Union to Israel, occupational and income surveys compared

	Workplace Occupational Survey		Israel Income Survey	
	Mean	Standard deviation	Mean	Standard deviation
Age	29.8	4.8	39.4	11.8
Years of education	12.9	2.5	13.7	3.1
Labor force experience	10.9	4.9	19.8	11.5
Years since migration	3.1	1.3	3.0	1.3
Years since migration^2	11.0	7.7	11.0	8.1
Currently married	0.79	0.41	0.80	0.40
Job tenure	1.3	1.1	–	–
Job tenure^2	3.0	4.0	–	–
Current Hebrew[a]	3.32	0.87	–	–
Entry Hebrew[a]	2.96	0.87	–	–
Monthly earnings[b]	2168	587	2838	1665
Log earnings	7.649	0.250	7.793	0.605
Occupations (OS)				
Software	0.22			
Technician	0.25			
Construction	0.24			
Gasoline station	0.29			
Occupations (IS)				
Scientist/academic			0.13	
Professional/technician			0.09	
Manager			0.004	
Clerical			0.03	
Sales			0.03	
Service			0.10	
Agricultural			0.12	
Skilled in industry			0.25	
Skilled in services			0.11	
Unskilled and production			0.12	
Survey year	1994	0	1994.1	0.81
Observations	348		1430	

Sources: Workplace Occupational Survey data collected by Siniver in 1994. Israel Income Survey microdata 1993–1995 from the Israel Central Bureau of Statistics.

Recent immigrants are those who arrived since 1989. Entry level Hebrew is the Hebrew score on entry into the current job, as reported retrospectively. See text for details.

[a] Hebrew knowledge is measured on a scale of 1 to 5, corresponding to the classifications "not at all", "a little bit", "not so well", "well", "very well."

[b] 1994 New Israeli Shekel (about US$0.30).

since migration, comes from work years within the same job. Thus, δ_1 estimates a differential return to job tenure (and experience) between immigrants and natives. It does not capture the two other possible components of the faster wage growth of immigrants: increased earnings due to switching jobs, and human capital accumulated by residing in the destination country even without working. On the one hand, this precludes investigating the potentially important role of language skill in job search, so that we underestimate the return to language. On the other hand, it eliminates the possibility of spurious correlation between language skill and quality of job match due to the fact that they both may increase in search time.

A second point about Eq. (3) is that in many cases differencing data with measurement error generates considerable bias, because it increases the noise-to-signal ratio. Hebrew proficiency is especially subject to measurement error as it is graded on a scale with only five values, causing rounding error. In Dustmann and van Soest (2001, 2002) differencing could have resulted in Δh variable that was almost completely noise. In our case, information about present and previous Hebrew ability is collected simultaneously. Thus measurement error in the two variables is likely to be highly correlated so that differencing may actually reduce the noise-to-signal ratio.

Finally, in using retrospective data we must be concerned about the possibility of *recall error*, as recollections of Hebrew ability and of earnings may be less precise than current knowledge. We return to these issues of interpretation and measurement in our discussion of the results.

6. Results

Table 2 reports estimates of the standard cross-sectional human capital earnings function. The first function of the table is to check if the wage growth of the OS immigrants is comparable to that of new immigrants in the Israel Income Survey (IS), conditional on covariates. Column (1) reports the typical specification in the IS, including both linear and quadratic terms in years since migration (YSM). Column (2) reports the same specification estimated in the OS. The coefficient estimates on YSM and YSM^2 show the same concave return to years since migration, and are statistically indistinguishable across the two data sets.[8] While the IS YSM profile is steeper and has less curvature, this difference is largely due to unusually low coefficients in the IS for schooling and labor force experience (including a negative return to experience abroad in the older IS sample). The two data sets show significant differences in the coefficients on schooling, labor force experience and marriage, when compared to results from other countries and other groups in Israel. These differences are mostly due to atypical estimates in the IS sample of immigrants rather than to unusual results in the OS. The only really surprising characteristic of the OS sample is the negative return to marriage, which is statistically insignificant. For our analysis, the key finding is that both sources indicate rapid wage growth among immigrants which is concave in time since arrival in Israel, at rates higher than those reported for other immigrant cohorts to Israel (Chiswick, 1998; Friedberg, 2000) but consistent with the findings of Eckstein and Weiss (1998) for this 1990s arrival cohort.

[8] Since the OS and IS are sampled independently, the variance of the difference of coefficients is the sum of the variances. For example, the standard error of the difference between YSM coefficients is $(0.052^2 + 0.046^2)^{1/2} = 0.069$, so the t-ratio is $0.040/0.069 = 0.58$.

t2.1 Table 2

t2.2 Returns to tenure, in Israel and in current job occupational and income surveys

	Left-hand variable: logarithm of monthly earnings	Israel Income Survey[a]—recent immigrants	Workplace Occupational Survey—recent immigrants			
t2.3–t2.4		(1)	(2)	(3)	(4)	(5)
t2.5	Years since migration	0.130 (0.052)	0.090 (0.046)	0.114 (0.021)	0.086 (0.019)	0.067 (0.020)
t2.6	Years since migration^2	– 0.009 (0.009)	– 0.011 (0.008)	– 0.014 (0.004)	– 0.012 (0.003)	– 0.009 (0.003)
t2.7	Tenure				0.055 (0.006)	0.097 (0.015)
t2.8	Tenure^2					– 0.012 (0.004)
t2.9	Years of schooling	0.023 (0.006)	0.050 (0.005)	– 0.001 (0.003)	– 0.001 (0.003)	– 0.0005 (0.003)
t2.10	Labor force experience	– 0.007 (0.002)	0.007 (0.003)	0.002 (0.002)	0.002 (0.002)	0.001 (0.001)
t2.11	Married	0.446 (0.050)	– 0.033 (0.026)	– 0.005 (0.015)	– 0.006 (0.013)	– 0.008 (0.013)
t2.12	Software			0.620 (0.021)	0.593 (0.018)	0.593 (0.018)
t2.13	Technician			0.231 (0.019)	0.187 (0.017)	0.189 (0.018)
t2.14	Construction[b]			0.187 (0.015)	0.194 (0.014)	0.189 (0.014)
t2.15	Constant	6.95 (0.107)	6.802 (0.093)	7.208 (0.056)	7.227 (0.052)	7.227 (0.050)
t2.16	Root mean square error	0.56	0.22	0.12	0.10	0.10
t2.17	*R*-squared	0.15	0.25	0.79	0.83	0.84
t2.18	Observations	1430	348	348	348	348

t2.19 Sources: Workplace Occupational Survey conducted in 1994; Israel Income Survey microdata, 1993–1995.

t2.20 [a] Income survey regressions include 2-year indicators.

t2.21 [b] Omitted occupation is gas station workers.

The remaining columns of Table 2 use the OS to examine the measurement of skill and the return to job switching. Column (3) reports the result of adding three occupation indicators to the estimating equation. Since the sample is based on four occupations with very different wages, not surprisingly occupation explains much of the variation in wages in the data, and controlling for occupation greatly increases the precision of estimates. However, it has little effect on estimated wage growth due to years since migration. Based on this cross-sectional evidence, job mobility does not seem to be a major source of wage convergence in this sample. Columns (4) and (5) add linear and quadratic terms in tenure, respectively. These indicate that about half of the estimated return to YSM is due to increased tenure.

Table 3 uses the standard cross-sectional approach to investigate how much of those gains in earnings are due to increased proficiency in Hebrew. Column (1) reproduces column (5) of Table 2 for comparison, with linear and quadratic terms in both YSM and tenure. Column (2) reports the results of adding Hebrew proficiency to the equation. The Hebrew variable has a large, positive and precisely estimated coefficient of 0.065. That estimate predicts a 26% higher wage for an immigrant with the maximum score (of 5) over a comparable immigrant with the minimum score (of 1). Including Hebrew in the regression reduces the estimated return to tenure by about a third, evaluated at the mean, but has no appreciable effect on the estimated effect of years since migration.

The coefficient on Hebrew fluency in column (2) can be interpreted as the average earning gain associated with a single category change in self-reported level. Of course, the earnings gain associated with the transition from speaking "a little bit" to speaking "not so well" may differ from that associated with graduating to speaking "well". Column (3) reports the result of checking if earnings are linear in fluency categories. Linearity is not quite rejected by a formal test, with the coefficients suggesting convexity in the Hebrew-earnings association at the first "little bit" and again at speaking "very well". The coefficient on Hebrew should henceforth be understood as the average earnings gain associated with a single category change.

The estimated effect of Hebrew fluency on wages may be biased if more able workers are more likely to know Hebrew. We address this issue in column (4) by exploiting the availability of longitudinal information about language proficiency for immigrants. These data allow us to estimate Eq. (3), the differenced version of the human-capital earnings function, reported here for immigrants only.[9] We estimate a large, statistically significant return to Hebrew even after allowing for an individual "ability" effect in earnings. The coefficient is 0.057, or a predicted 5.7% increase in wages for each unit of Hebrew proficiency on the four step scale. This coefficient predicts a 23% increase in earnings

[9] In estimating the differenced equation we must assume that marital status and education are unchanged between entry into the current job and the survey period, since these retrospective questions were not asked. This assumption is probably benign, as the omission of these two variables in the cross-sectional regression (as in Table 3) has almost no effect on the other coefficients.

Table 3
Returns to Hebrew and ability, Workplace Occupational Survey—recent immigrants

Left-hand variable	Logarithm of monthly earnings			Change in logarithm of monthly earnings—current job		
	(1)	(2)	(3)	(4)	(5)	
Hebrew		0.065 (0.006)		0.057 (0.008)	0.058 (0.008)	ΔHebrew
Hebrew level[a]						
2—“a little bit”			0.086 (0.027)			
3—“not so well”			0.148 (0.027)			
4—“well”			0.194 (0.027)			
5—“very well”			0.314 (0.034)			
Years since migration (YSM)	0.067 (0.020)	0.047 (0.019)	0.045 (0.019)	0.059 (0.003)	0.058 (0.008)	Δtenure (= ΔYSM = Δexperience)
YSM^2	− 0.009 (0.003)	− 0.006 (0.003)	− 0.006 (0.003)		0.003 (0.002)	ΔYSM^2
Tenure	0.097 (0.015)	0.057 (0.013)	0.056 (0.013)			
$Tenure^2$	− 0.012 (0.004)	− 0.006 (0.004)	− 0.005 (0.004)		− 0.002 (0.001)	$\Delta Tenure^2$
Years of schooling	− 0.0005 (0.003)	0.001 (0.003)	0.001 (0.003)			
LF experience	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)			
Married	− 0.008 (0.013)	− 0.006 (0.012)	− 0.006 (0.012)			
Constant	7.227 (0.089)	7.064 (0.053)	7.123 (0.053)	− 0.004 (0.003)	− 0.0001 (0.002)	Constant
Three occupation indicators	✓	✓	✓			
Root MSE	0.10	0.09	0.09	0.56	0.55	Root MSE
R-squared	0.84	0.88	0.88	0.68	0.68	*R*-squared
Observations	348	348	348	348	348	Observations
p-Value of test for linearity			0.0501			

Source: Workplace Occupational Survey conducted in 1994.
Differenced estimates assume no change in marital status or education between entry and survey.
[a] The omitted category is level 1—“not at all”.

associated with fluency in Hebrew. The size of this coefficient on Hebrew is striking, considering that we have allowed for ability bias.[10]

Though large, the estimated coefficient on Hebrew in the differenced equation is somewhat smaller than that in the cross-section, suggesting either ability bias in the cross-sectional estimate or an exacerbation of classical measurement error in differences. The retrospective setup argues against the latter explanation, since measurement error in self-reported Hebrew proficiency is probably fairly constant over time for the same individual, which would make attenuation bias smaller in the differenced equation than in the cross-section.[11] We return to an analysis of the potential effects of measurement errors in discussing the occupation-specific estimates below.

Our next goal is to investigate the role of improved Hebrew fluency in explaining faster wage growth among immigrants than among natives. For that purpose, we estimate the differenced equation using both natives and immigrants, allowing a differential experience/tenure profile for immigrants. To emphasize that we are examining differential wage growth within a job, we label our key variable as a tenure/immigrant interaction rather than as YSM. (Recall that changes in job tenure, experience and YSM are identical for immigrants in this sample.)

For comparison, column (1) of Table 4 reports cross-sectional estimates of returns to tenure and experience for natives in the OS sample. Column (2) reports that when the same equation is estimated in differences the coefficients are statistically indistinguishable from those in column (1). (Note that Δtenure = Δexperience in our sample so that the coefficient on tenure in column (2) estimates the sum $\gamma + \rho_1$ in Eq. (3)). Heterogeneous ability and measurement error are therefore not significant sources of bias in estimating these tenure coefficients for natives.

Columns (3) through (6) report estimates of separate tenure profiles for immigrants and natives from the differenced earnings equation (Eq. (3)). The linear specification in column (3) reports a 4.3% increase in earnings for each year of job tenure for natives. (Recall that this combines both tenure and experience effects.) Immigrants have an additional 2.2% increase in earnings per year of job tenure, which reflects the rate at

[10] It is difficult to compare these results with those in the literature since each study uses different measures of language knowledge. The closest paper is Tainer (1988) which also uses a five-point scale and finds even higher returns to English knowledge in the United States. Her OLS coefficients are 0.13 for Europeans and 0.17 for Hispanics and Asians or about 2 1/2 times our coefficients. The remaining studies are less comparable. Chiswick (1998) reports an 11% return to having Hebrew as a primary language. This coefficient rises to 35% using IV. Dustmann (1994) finds about a 7% difference between immigrants to Germany who speak German well and those who speak it badly or not at all. His later work with van Soest (2001, 2002) shows that this estimate is quite sensitive to the assumptions underlying estimation, with coefficients ranging from close to 0 to roughly doubly the estimate in the original paper. Using OLS, Chiswick and Miller (1995) report returns of 5.3% and 8.3% to fluency in English in Australia and 16.9% to fluency in English. They report widely varying results using IV.

[11] There is a form of measurement error in language ability that would bias the differenced coefficient upwards and the cross-sectional coefficient downwards. Since the scale of language ability is bounded at both ends, measurement error could be asymmetric, causing differences in Hebrew to be underestimated and the differenced regression coefficient to be overestimated. This is unlikely as only 4% of immigrants in the sample report their Hebrew at the lowest level when hired and only 6% of the sample report their current level of Hebrew as fluent.

Table 4
Returns to tenure and Hebrew, Workplace Occupational Survey—recent immigrants and natives

Left-hand variable	Logarithm of monthly earnings—natives	Change in logarithm of monthly earnings on current job					
		Natives	Immigrants and natives				
	(1)	(2)	(3)	(4)	(5)	(6)	
Hebrew				0.054 (0.008)		0.058 (0.008)	ΔHebrew
			0.022 (0.003)	0.009 (0.003)	0.026 (0.008)	0.002 (0.008)	ΔTenure × Immigrant
					0.003 (0.003)	0.006 (0.002)	ΔTenure^2 × Immigrant ΔTenure
Tenure	0.045 (0.007)	0.049 (0.003)	0.043 (0.001)	0.044 (0.001)	0.050 (0.003)	0.050 (0.003)	(= Δexperience = ΔYSM)
Tenure^2	− 0.0010 (0.0005)	− 0.0006 (0.0004)			− 0.003 (0.001)	− 0.002 (0.001)	ΔTenure^2
Years of schooling	0.011 (0.003)				− 0.0019 (0.0013)	− 0.0017 (0.0012)	ΔYSM^2
LF experience	0.007 (0.001)						
Married	0.013 (0.012)						
Constant	7.50 (0.041)	0.009 (0.004)	0.012 (0.003)	0.010 (0.003)	0.007 (0.002)	0.006 (0.002)	Constant
Three occupation indicators	✓						
Root MSE	0.14	0.077	0.072	0.070	0.072	0.070	Root MSE
R-squared	0.73	0.71	0.70	0.71	0.70	0.72	*R*-squared
Observations	603	603	951	951	951	951	Observations
*Derivative evaluated at the mean**							
			0.022 (0.003)	0.009 (0.003)	0.022 (0.003)	0.009 (0.003)	Immigrant × (Tenure, experience), YSM

Source: Workplace Occupational Survey, 1994.

which immigrant wages are converging to those of natives, within occupations. This is rapid wage convergence compared to other countries.[12] Borjas (1994, Table 4), for example, reports that immigrants with less than 5 years in the United States in 1970 gained nine percentage points relative to natives by 1980 and that those with less than 5 years in the United States in 1980 gained 10 percentage points relative to natives by 1990.

How much of that catch-up can be attributed to Hebrew language acquisition? Column (4) reports that adding the linear Hebrew coefficient accounts for most of wage convergence, reducing the differential tenure profile from 2.2% to 0.9% per annum, a large and statistically significant decrease. Generalizing the functional form by adding quadratic terms does not change this conclusion, as reported in the bottom row of columns (5) and (6). Language acquisition, estimated here net of a linear ability effect, appears to account for more than half of the wage convergence of recent immigrants within occupations, in our sample.

It is worth noting that our analysis cannot address the contribution of language to wage convergence through occupational change. Weiss and Gotlibovski (1995) examine this question, finding no significant effect of Hebrew proficiency on the probability of receiving a job offer.[13] While language skills plausibly complement occupational upgrading, we can only speculate on whether they are more important within or between occupations.

6.1. Language-skill complementarity

It seems plausible that language complements some types of human capital more than others, so that the wage gains associated with learning Hebrew will be greater in some jobs. Our survey includes four occupational groups, drawn at opposite ends of the skill distribution: software programmers, computer technicians, construction workers and gasoline station attendants. Table 5 reports descriptive statistics for immigrants in each group. Note that the programmers and technicians average 15 and 14 years of schooling, respectively, while the lower skill occupations average less than 12. Job tenure is shorter for the less-skilled workers, though they average about the same amount of time since arrival in Israel, indicating greater turnover in these occupations. Technicians' self-reported Hebrew is clearly best, programmers and construction workers have almost the same level and gas station attendants have the lowest level. The averages for all groups fall between a 3 ("not so well") and a 4 ("well"). Strikingly, Hebrew fluency at entry is not noticeably higher in high-skill occupations.

Table 6 reveals sharp differences among occupations in returns to Hebrew once ability bias is treated. The table reports coefficients on Hebrew from both the cross-sectional and differenced equations (Eqs. (1) and (3)) for occupation separately.[14] The first two rows in the left column of Table 6 report those cross-sectional results for software programmers

[12] The implied within-occupation rate of wage growth in our sample is 6.5% per year, as compared to 6.4% in the sample studied by Eckstein and Weiss (1998, page 4).

[13] Weiss and Gotlibovski (1995), p. 22.

[14] Cross-sectional estimates pool entry and survey years in order to increase precision and to enhance comparability with the differenced estimates. This requires assuming that marital status and education are constant for individuals over the sample period.

Table 5
Descriptive statistics by occupation, Workplace Occupational Survey

	Software	Technician	Construction	Gasoline station attendant
Age	31.8 (4.8)	30.8 (4.6)	27.4 (3.5)	29.4 (5.1)
Years of education	15.1 (0.9)	14.0 (0.7)	11.8 (2.9)	11.3 (2.4)
Labor force experience	10.8 (4.9)	10.9 (4.8)	9.6 (4.3)	12.1 (5.1)
Years since migration	2.9 (1.3)	3.3 (1.1)	3.0 (1.3)	3.0 (1.3)
Years since migration^2	10.3 (7.7)	12.3 (7.1)	10.5 (7.9)	11.0 (7.9)
Currently married	0.81 (0.39)	0.74 (0.44)	0.82 (0.39)	0.79 (0.41)
Job tenure	1.5 (1.2)	2.0 (1.2)	0.9 (0.6)	1.1 (1.0)
Job tenure^2	3.7 (4.5)	5.3 (4.8)	1.1 (1.1)	2.0 (3.2)
Current Hebrew[a]	3.35 (1.08)	3.55 (0.79)	3.33 (0.78)	3.11 (0.77)
Entry Hebrew[a]	2.96 (0.89)	3.07 (0.84)	3.04 (0.87)	2.80 (0.87)
Monthly earnings[b]	3083 (432)	2130 (283)	1993 (230)	1671 (193)
Log earnings	8.03 (0.14)	7.66 (0.13)	7.59 (0.12)	7.41 (0.12)
Observations	75	87	84	102

Source: Workplace Occupational Survey, 1994.
Entry level Hebrew is the Hebrew score on entry into the current job, as reported retrospectively. See text for details.

[a] Hebrew knowledge is measured on a scale of 1 to 5. See Table 1 for details.

[b] 1994 New Israeli Shekel (about US$0.30).

and technicians. These cross-sectional coefficients on Hebrew are quite large, 6.8% in software and 11.2% for computer technicians. The middle column reports the coefficient on Hebrew in Eq. (3), a differenced specification of the same equation designed to eliminate ability bias. The differenced specification yields a larger return to Hebrew for programmers (8.3%) and a slightly smaller coefficient for technicians (10.4%). These estimates are quite large, implying that complete fluency (speaking "very well" as opposed to "not at all") is worth a wage premium of 33% for programmers and 42% for technicians. The right column reports the estimated "ability bias" (the difference

Table 6
Returns to Hebrew and ability bias by occupation, Workplace Occupational Survey—recent immigrants

Left-hand variable: log (earnings)	Coefficients on Hebrew		"Ability" bias of cross-section estimate
	Cross-section	First difference	
Occupations			
Software	0.068 (0.008)	0.083 (0.012)	− 0.015 (0.015)
Technicians	0.112 (0.011)	0.104 (0.013)	0.008 (0.007)
Construction	0.032 (0.010)	− 0.002 (0.014)	0.034 (0.010)
Gas Stations	0.031 (0.013)	− 0.00004 (0.018)	0.031 (0.012)

Source: Workplace Occupational Survey, 1994.
Cross-section specifications (Eq. (1)) include linear and quadratic terms in tenure and YSM, schooling, LF experience and an indicator for currently married as in column (2) of Table 3. Cross-sectional estimates pool data from the survey year and the entry year to increase precision and to enhance comparability with first difference results. First difference specifications (Eq. (3)) include linear and quadratic terms in tenure and a quadratic term in YSM as in column (6) of Table 4. Both specifications assume that marital status and education is the same in entry and survey years.

between cross-sectional and differenced coefficients) which is statistically insignificant for both programmers and technicians.

In contrast, the bottom two rows report that once ability bias is accounted for, proficiency in Hebrew has little if any effect on the wages of construction workers and gas station attendants. While the cross-sectional coefficients on Hebrew are 3.2% and 3.1%, respectively, these coefficients are statistical zeros in the differenced specification. For these lower skill occupations, the implied ability biases (reported in the rightmost column) are as large as the estimated cross-sectional coefficients, and statistically significant. The apparent return to Hebrew language proficiency in the cross-section is entirely due to heterogeneity (ability) bias for these two occupations.

The contrast between the high and low skill returns to language acquisition is illustrated in the two panels of Fig. 1, which plot changes in log wages against changes in Hebrew proficiency once the effects of changes in tenure and years since migration have been removed. (That is, these are plots of residuals from a regression of each differenced variable on the difference in tenure. The slope of a linear regression line for the residuals plotted is the partial regression coefficient of Hebrew in Eq. (3) by the Frisch–Waugh–Lovell theorem.) Our interpretation of this contrast is that language complements skills in increasing earnings but has no effect on the earnings of less-skilled workers.

The language-skill complementarity we find in our data is consistent with prior cross-sectional evidence in the literature. McManus et al. (1983) and McManus (1985) report that English knowledge has a bigger payoff for more educated Hispanic workers in the United States. Kassoudji (1988) finds, after correcting for sample selection, that professional and administrative jobs have higher returns to English knowledge at least for Hispanics in the United States. Chiswick and Miller (1995) report a higher return to education for Australian immigrants who speak English well than for those who do not.

6.2. Measurement issues

Can the estimates from the differenced equation in middle column of Table 6 really be interpreted as the effect of Hebrew on earnings and can the contrast between those and the cross-sectional estimates really be interpreted as ability bias? To answer those questions requires a more complete discussion of the issues pertaining to retrospective measurement and measurement of dichotomous variables.

We note first that it is relatively straightforward to use measurement error to explain either the results for skilled workers or the results for unskilled workers to derive an alternative explanation for the results. It is more difficult to find a single type of measurement error that explains them both. While it is possible that the results for skilled and unskilled workers are affected differently by measurement error, language-skill complementarity strikes us as a simpler and more natural explanation. It has the benefit of Occam’s razor. In the following discussion, we are therefore concerned with whether measurement error could bias the results for both sets of occupations.

The first issue concerns the bias due to nonclassical measurement error in reporting a continuous variable in a small number of discrete categories. To illustrate the problem,

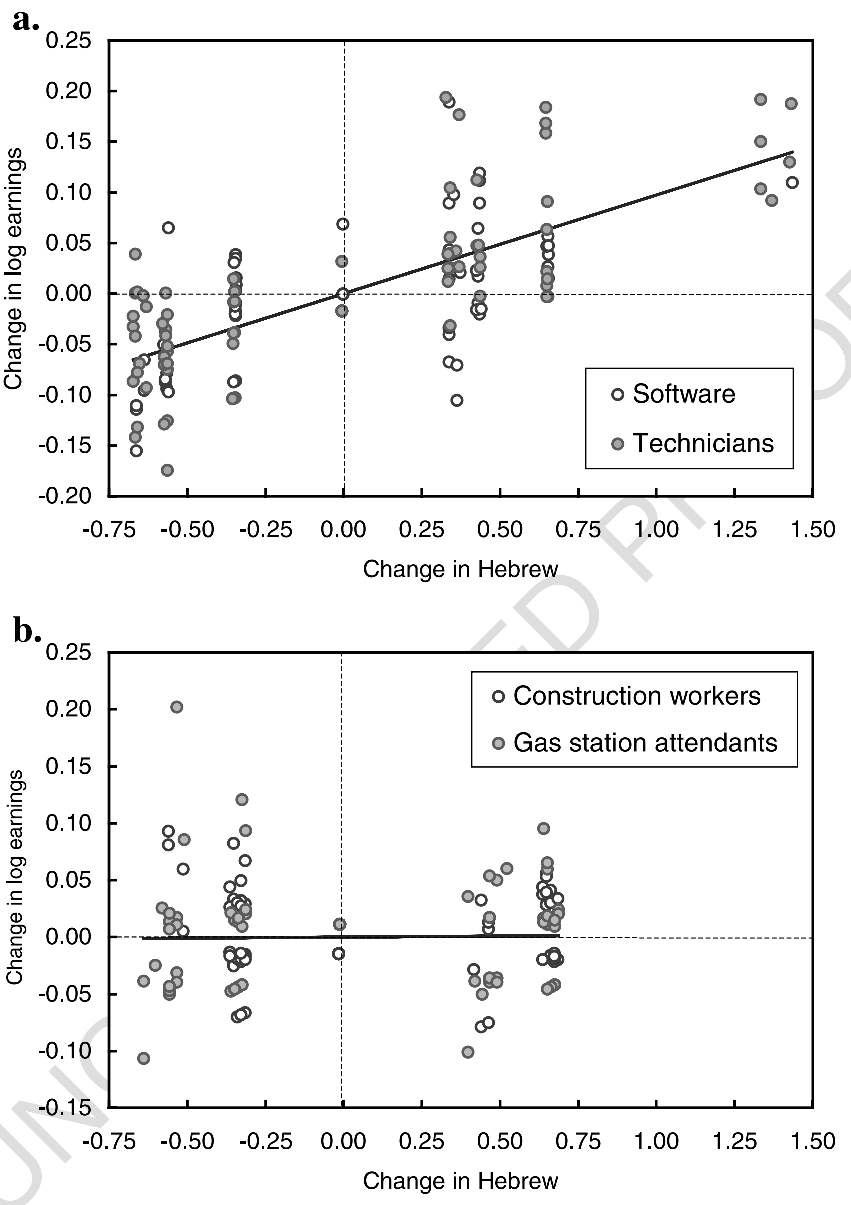

Fig. 1. (a) Changes in earnings and Hebrew: programmers and computer technicians (leverage plot). (b) Changes in earnings and Hebrew: gas station attendants and construction workers (leverage plot).

assume that Hebrew proficiency, h, takes on continuous values from 0.5 to 5.5 and that the answers on the questionnaire are simply rounded to the nearest unit so that for reported Hebrew, h^{R},

$$h^{\mathrm{R}} = \mathrm{round}(h) = h + u, \tag{4}$$

where u is measurement error. This is not classical measurement error as u and h are not independent. Ignoring the other covariates for simplicity,

$$\ln(w_{it}) = \alpha_i + \omega h_{it} + \epsilon_{it} = \alpha_i + \omega h_{it}^{\mathrm{R}} - \omega u_{it} + \epsilon_{it}. \tag{5}$$

The bias in least squares regression depends on the correlation of h^{R} and u, which depends in turn on the distribution of h. For instance, if h is uniformly distributed on the [0.5, 5.5] interval, h^{R} and u are uncorrelated and there will be no bias due to measurement error. If h has a symmetric central tendency (a lump in the middle), then h^{R} and u tend to be positively correlated since there is more "rounding up" than "rounding down" above the mean and more "rounding down" than "rounding up" below the mean. A positive correlation implies bias toward zero in the estimation of 4. The distribution of h in our data seems to have that central tendency. The distribution of current reported Hebrew by level is 2%, 13%, 41%, 38%, 6%. Fitting that distribution to normal yields a downward bias of 7.5% on the estimated coefficient in simulation.

In the differenced equation, on the other hand, the sign of the measurement error bias is ambiguous. The distribution of h could well have a mass point at zero and is skewed to the right. The distribution of reported change in Hebrew is no change—66%, increase of one level—32%, increase of two levels—2%. It is plausible that for "no change" rounding is, on average, downward, and that for improved Hebrew rounding is, on average, upward. If so, the measured change understates the true change and the bias in the differenced regression is away from zero, in our case upwards.

This form of measurement error provides a possible alternative explanation for the pattern reported in Table 6 for the high-skill occupations, since the bias in the cross-sectional estimates is likely toward zero and the bias in the differenced equation is possibly upwards. Yet, if that were the case, we would expect to see the same pattern of estimates for the low-skill occupations as well. The low-skill occupations have similar distributions of changes in Hebrew proficiency,[15] yet they yield positive estimated coefficients in the cross-section and zeros in first differences. We conclude that bias due to this type of measurement error is quite unlikely to undermine our central conclusion that true returns to Hebrew are much higher in high-skill occupations.

A second, related, measurement issue concerns how discrete answers are given in retrospective questions. Assume again that the latent variable is continuous on the [0.5, 5.5] interval and that the answer to "current Hebrew" is given according to the rounding formula above. Immediately after answering that question, the respondent is asked to evaluate his Hebrew when he entered the current job. If his current level is $h = 3.6$, which he reported as $h^{\mathrm{R}} = 4$ and the entry level was 3.4, would he report $h^{\mathrm{R}} = 3$ following the rule above or $h^{\mathrm{R}} = 4$ since the change in Hebrew proficiency was only 0.2? It's plausible that in this example he reports $h^{\mathrm{R}} = 4$, implying $\Delta h^{\mathrm{R}} = 0$ and generally follows a rule that rounds

[15] Low skill occupations have 70% no change and 30% increase by one level, as opposed to 60% no change, 35% increase by one level and 4% increase by two levels for high-skill occupations.

Δh to the nearest integer in reporting Δh^R. That practice has two implications for measurement. First, as we noted above, changes in Hebrew proficiency are probably measured more precisely than levels (as the levels involve cross-sectional variation in subjective self-reporting). More broadly, changes and levels are measured in metrics that may be monotone transforms of each other, but are probably not identical so that the coefficients of regressions in changes and in levels are not directly comparable. For that reason, we are reluctant to interpret the difference between cross-sectional and differenced estimates of the return to Hebrew as a precise measure of "ability bias". The data do suggest a positive ability bias in cross-sectional estimates for the low-skill occupations and an absence of such bias in the high-skill occupations, but the precise magnitudes of these biases are indeterminate.

A third measurement issue is recall error in retrospective questions (Belli and Stafford, 2001). Retrospective measurement of Hebrew proficiency requires recall so it is likely to be subject to more measurement error than current Hebrew. We have argued that our interview method reduces recall error by choosing a memorable event, but the extent to which it remains is an empirical issue. If recall error was particularly strong among the construction workers and gas station attendants and the measurement error was classical (uncorrelated with the residual in Eqs. (1) and (3)), then a possible result is that differenced estimates of returns to Hebrew would be biased toward zero, while cross-sectional estimates would be subject to less of the same bias. In that way, recall error provides an alternative explanation for the pattern in Table 6.

The data weigh against this explanation in two ways. First, job tenure tends to be shorter in the less-skilled occupations (see Table 5), so that entry Hebrew is a less distant memory for those workers, implying a smaller contrast between cross-sectional and differenced coefficients for the less-skilled occupations. The opposite is the case in Table 6. Second, the recall error hypothesis has the testable implication that cross-sectional estimates of Eq. (1) in the entry year should yield coefficients closer to zero than the same estimates using current data from the survey year (1994), and that this pattern should be particularly true for the less-skilled workers.[16] Table 7 reports the return to Hebrew in cross-sectional estimates of Eq. (1) in both the survey year and the entry year for each occupation group. With the exception of software engineers, the coefficients are quite similar. The column labeled "recall bias" reports the differences between the two cross-sectional coefficients. That difference is negligible in the two low-skill occupations, and only statistically significant in the case of software engineers (for whom the differenced estimates in Table 6 exceed the cross-sectional estimates). Recall bias is apparently not particularly strong among the low-skill occupations. If anything, the opposite is true. We conclude that this form of recall bias does not provide an alternative to language-skill complementarity as an explanation for the differential pattern of returns to Hebrew reported in Table 6.

Finally, we note that despite our claim that the way the data are collected minimizes measurement error, there is likely to be some measurement error in the difference. Suppose, for example, we asked, "How much has your Hebrew improved since you

[16] We thank a referee for pointing out this possible type of recall error and for suggesting this test.

Table 7
Recall bias in returns to Hebrew? Workplace occupational survey—recent immigrants

Left-hand variable: log (earnings)	Cross-sectional coefficients on Hebrew		"Recall bias" of cross-sectional estimate in entry year
	Survey year	Entry year	
Occupations			
Software	0.081 (0.009)	0.061 (0.008)	– 0.020 (0.007)
Technicians	0.112 (0.013)	0.114 (0.012)	0.001 (0.012)
Construction	0.032 (0.011)	0.030 (0.010)	– 0.002 (0.008)
Gas Stations	0.030 (0.015)	0.031 (0.013)	0.001 (0.008)

Source: Workplace Occupational Survey, 1994.
"Recall bias" is the entry year estimate less the survey year estimate. Specifications (Eq. (1)) include linear and quadratic terms in tenure and YSM, schooling, LF experience and an indicator for currently married as in column (2) of Table 3. Marital status and education recorded in the survey year is assumed to be the same in entry year and survey years.

started the job?" We would expect this measure to be subject to measurement error. If the measurement error is classical, the estimated return to Hebrew knowledge will be biased towards zero in the differenced data. Thus, the estimates of the return to Hebrew would be lower bounds. However, even in the presence of classical measurement error, the *t*-statistic is consistent. So our conclusion about the absence of a statistically significant return to Hebrew in the unskilled occupations would be unchanged.

In addition to measurement error, there is a related issue concerning selection bias. We observe only workers who are still in their jobs. In steady state, this is not an issue since we will observe high turnover workers who have recently arrived and miss high turnover workers who have recently departed. Since our data are from early in the period of Russian migration, we may miss workers with high rates of upward mobility particularly in the low-skill jobs.

6.3. How much of immigrant wage growth is due to learning Hebrew?

Given our conclusion that improved Hebrew only affected wages for high-skill workers, we return to reexamine differential returns to tenure/experience in each high-skill occupation and evaluate the effect of improved Hebrew. In Table 4, we saw that on average immigrants had higher returns to tenure than did natives and that over half of the differential was attributable to improved Hebrew, but this estimate combined the effect of Hebrew in high- and low-skill occupations. Table 8A looks at Software engineers. As in Table 4, Eq. (3) is estimated allowing a differential slope in the tenure-earnings profile for immigrants. In software earnings, growth is 2.6 percentage points higher per year for immigrants (column (1)). That coefficient is reduced to 0.6 percentage points when changes in Hebrew are included (column (2)) indicating that about 3/4 of that differential in returns to tenure/experience is attributable to improved Hebrew.

Our argument that this estimated return to Hebrew is free of ability bias requires that the rate at which individuals acquire fluency in Hebrew be uncorrelated with the rate at which other skills appreciate. If fast learners simultaneously learn programming and Hebrew

Table 8A
Tenure profiles and Hebrew—software, Workplace Occupational Survey—recent immigrants and natives

Left-hand variable: ΔEarnings	(1)	(2)	(3)	(4)	(5)
ΔHebrew		0.090 (0.011)	0.093 (0.011)		0.083 (0.012)
ΔTenure × Immigrant	0.026 (0.005)	0.006 (0.004)	0.006 (0.005)	0.074 (0.015)	0.038 (0.015)
ΔTenure^2 × Immigrant				− 0.003 (0.004)	0.001 (0.002)
ΔTenure (= Δexperience = ΔYSM)	0.032 (0.002)	0.033 (0.002)	0.031 (0.003)	0.056 (0.004)	0.056 (0.004)
ΔTenure^2				− 0.009 (0.002)	− 0.008 (0.002)
ΔYSM^2				− 0.006 (0.002)	− 0.005 (0.002)
Years of schooling			− 0.010 (0.005)		
Years of schooling × Immigrant			− 0.0003 (0.0005)		
Constant	0.022 (0.004)	0.020 (0.004)	0.176 (0.071)	− 0.002 (0.003)	− 0.001 (0.003)
Root MSE	0.06	0.06	0.05	0.06	0.05
R-squared	0.61	0.67	0.68	0.67	0.71
Observations	233	233	233	233	233
Derivatives evaluated at the mean					
Immigrant × (Tenure, experience, YSM)	0.026 (0.005)	0.006 (0.004)	0.006 (0.005)	0.032 (0.007)	0.012 (0.006)

Source: Workplace Occupational Survey, 1994. See notes to Table 4.

quickly, we may incorrectly attribute their faster wage growth to their growing Hebrew fluency. We can offer only a partial test of this alternative hypothesis. If more skilled workers learn job-related skills and Hebrew more quickly, we would expect that pattern to be

Table 8B
Tenure profiles and Hebrew—technicians, Workplace Occupational Survey—recent immigrants and natives

Left-hand' variable: ΔEarnings	(1)	(2)	(3)	(4)	(5)
ΔHebrew		0.102 (0.009)	0.104 (0.009)		0.104 (0.009)
ΔTenure × Immigrant	0.038 (0.004)	0.017 (0.004)	0.022 (0.005)	0.033 (0.016)	− 0.007 (0.011)
ΔTenure^2 × Immigrant				− 0.009 (0.007)	− 0.002 (0.004)
ΔTenure (= Δexperience = ΔYSM)	0.037 (0.002)	0.038 (0.002)	0.037 (0.002)	0.043 (0.004)	0.044 (0.004)
ΔTenure^2				0.004 (0.002)	0.003 (0.002)
ΔYSM^2				0.004 (0.003)	0.004 (0.002)
Years of schooling			− 0.001 (0.004)		
Years of schooling × Immigrant			− 0.001 (0.001)		
Constant	0.018 (0.007)	0.013 (0.006)	0.037 (0.060)	0.008 (0.007)	0.006 (0.006)
Root MSE	0.07	0.06	0.06	0.07	0.06
R-squared	0.68	0.76	0.77	0.69	0.77
Observations	252	252	252	252	252
Derivatives evaluated at the mean					
Immigrant × (Tenure, experience, YSM)	0.038 (0.004)	0.017 (0.004)	0.022 (0.005)	0.025 (0.008)	0.010 (0.006)

Source: Workplace Occupational Survey, 1994. See notes to Table 4.

Table 8C
Tenure profiles and Hebrew—construction and gas station attendants, Workplace Occupational Survey—recent immigrants and natives

Left-hand variable: ΔEarnings	Construction		Gas station attendants	
	(1)	(2)	(1)	(2)
ΔHebrew		0.001 (0.010)		– 0.004 (0.011)
ΔTenure × Immigrant	– 0.004 (0.006)	– 0.004 (0.008)	– 0.007 (0.005)	– 0.006 (0.006)
ΔTenure (= Δexperience = ΔYSM)	0.073 (0.005)	0.073 (0.005)	0.055 (0.002)	0.055 (0.002)
ΔTenure^2				
ΔConstant	0.005 (0.003)	0.005 (0.003)	0.002 (0.006)	0.002 (0.006)
Root MSE	0.04	0.04	0.07	0.07
R-squared	0.61	0.61	0.82	0.82
Observations	181	181	285	285

Source: Workplace Occupational Survey, 1994. See notes to Table 4.

reflected in faster wage growth within jobs for more educated workers. We have therefore tried including education as an explanatory regressor in column (3). The coefficient on education is insignificant, and the remaining coefficients are essentially unchanged.

The final two columns of Table 8A check robustness to adding a quadratic in tenure and years since migration. In this case, Hebrew accounts for 62% of the differential in returns to tenure/experience, evaluated at the mean.

Table 8B reports the same analysis for technicians, who have a differential return to tenure/experience of 3.8 percentage points for immigrants, of which between one-half and three-fifths is attributable to improved Hebrew, depending on the specification. Adding years of education has little effect on these results. Taken together, the results for software engineers and technicians indicate that most of the earnings convergence by recent immigrants in high-skill occupations is due to language acquisition.

Table 8C repeats this analysis for the low-skill occupations. For construction workers and gas station attendants, the stark finding is that there is no earnings convergence to explain. While returns to tenure/experience are high, they occur at the same rate for recent immigrants and natives. This is true with or without Hebrew proficiency in the equation.[17]

This contrast in wage convergence between high- and low-skill occupations has also been noted by Eckstein and Weiss (1998), who attributed it to an increased price (or quality) of imported skill. Our interpretation of that finding is that the faster wage convergence of skilled workers is due not to a secular increase in demand for imported skill, but is rather the result of improved language proficiency of skilled workers, since

[17] Note in passing that the constant term provides a very partial and weak test of the model. In the differenced specifications, the constant term measures the predicted wage growth if there were no increase in tenure/experience, years since immigration and Hebrew knowledge. Wage growth after no time on the job, and thus the constant term, should be zero. If the constant term is not zero, the equation must be misspecified. The most likely sources of that problem are a parametric specification that does not permit appropriate curvature or measurement error. For example, with classical measurement error and all observations in the positive quadrant, the constant term is biased upwards. We therefore find some support for the model in the estimated constant terms in our most general specifications (column 5 of Tables 8A and 8B and columns 2 and 4 of Table 8C). In all four cases, the constant term is precisely measured and statistically zero.

Tables 8A–8C reports that skilled workers who did not improve their Hebrew had much slower wage convergence. Language seems to complement the quality or usefulness of foreign human capital.

7. Conclusions

Previous research on the effect of host-country language fluency on the growth of immigrant earnings has been suspect because of possible biases due to: the correlation of fluency with unmeasured ability; spurious correlation of duration in the host country with fluency; and spurious correlation of measurement error with fluency. The first two biases do not arise in our work because we observe both wage growth and changes in fluency. Our data are probably subject to less measurement error in individual assessments of what constitutes a "good" knowledge of the language as it asks questions about present and past fluency simultaneously. Finally, because we examine wage growth within jobs, our findings do not confound the effects of job shopping with those of increased fluency.

These advantages in data design allow us to reach a strong conclusion, quite different from that in the literature. The estimated return to host-language fluency is statistically indistinguishable from zero for immigrants in low-skill occupations. Apparent positive returns to fluency in cross-sectional estimates are apparently entirely due to ability bias for this group. Perhaps not surprisingly then, there is no evidence of immigrant wage convergence for low-skilled workers. In contrast, high-skill occupations show evidence of considerable wage convergence, much of which is accounted for by increased Hebrew fluency, which is worth a 33–42% wage premium.

Our results are subject to caveats, which are common to this literature. First language may proxy for a range of host-country skills. As individuals master the language, they also master social rules and local customs. Second, language may play an important role in determining the type of occupation immigrants can enter. Since we have shown that language plays a more important role in some occupations than in others, we would expect individuals who are fluent in the host-country language to select into occupations that are more language intensive. Moreover, since language and skill level are complementary, increasing fluency should help individuals obtain higher paying jobs. In the cross-section, this effect appears to be small. The coefficient on Hebrew falls only from 8.0% to 6.5% when we include occupational dummies. Nevertheless, our estimates almost certainly underestimate the full value of Hebrew knowledge for immigrants.

We should be cautious about extending our results to all low-skill and high-skill occupations. It is probably more accurate to conclude that language is more complementary with some skills than with others. We would anticipate that language would be important in telemarketing, for example, even though this is a low-skill occupation by most measures.

Small sample size and the possibility of bias due to measurement error suggest further caution. The point estimate of the return to Hebrew knowledge in low-skill occupations is close to zero when there is difference in data. However, the reported standard errors are sufficiently large to permit a 15% difference in earnings between those who speak Hebrew "very well" and those who speak it "not at all".

While not focused on the debate over whether immigrants' earnings overtake those of natives, our two main results do cast light on that question (Borjas 1994). We find significantly greater wage growth for immigrants in certain occupations and can tie that differential wage growth to a plausible mechanism-language acquisition. On the other hand, this faster wage growth is confined to relatively high skill workers, in our sample. Thus, the prevalence of catch-up and surpassing of native workers may well depend on the skill level of immigrants. Skilled immigrants seem more likely to surpass otherwise comparable natives with similar skills while unskilled immigrants seem less likely to do so.

Finally, the results serve to remind us of the economic importance of language. Language may well be the most important public good/infrastructure in a society. It is nonrivalrous and provides network externalities. To the extent that language provides externalities, estimated private returns understate the social return to language training, and there may be under-investment in language skills in competitive equilibrium, especially by immigrants. In that case, supporting language classes for immigrants not only speeds their economic assimilation but may also provide a general social benefit through improved communication.

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