THE RETURN TO ENGLISH IN A NON-ENGLISH SPEAKING COUNTRY: RUSSIAN IMMIGRANTS AND NATIVE ISRAELIS IN ISRAEL

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1. Introduction

We use a unique data set to examine the return to English knowledge. Our primary focus is on Russian immigrants to Israel, but we study native Israelis as well. Understanding the role of English in this setting is important for at least three reasons.

First, globalization and the ensuing growth in the importance of foreign language knowledge has become an important theme in the popular press. The *San Francisco Chronicle* reports that families seek nannies who speak a language other than English because "They want to give their children a head start in business in 20 years." (Helen Riley-Collins, president of Aunt Ann's In-House Staffing in San Francisco, quoted in Hua, 2005). It is also commonly argued that the success of Canada, the United States and other countries with large immigrant populations reflects, in part, access to world markets fostered by the immigrant population's knowledge of foreign languages and cultures (Farooqui, 2005).

Yet, there has been surprisingly little research on the return to foreign language knowledge. The major exception is Grin (2001) who examines the role of English in Switzerland where its role as a lingua franca gives it a special status. There is also a modest literature on the value of second language knowledge in multilingual societies such as Canada and Switzerland (Grin and Sfreddo, 1998; Shapiro and Stelcner, 1997) and on the value of bilingualism among individuals in the United States, many of whom are not native English speakers (Fry and Powell, 2003).

To the extent that English has become the lingua franca for international exchange, its labor market value is likely to exceed, for the foreseeable future, that of any other language with a large population that speaks it as a second language. Thus for English-speaking countries such as the United States, information on the return to English in Israel casts light on the economic value of large-scale investment in foreign-language instruction.

Second, by examining not only Hebrew acquisition but also English acquisition among Russian immigrants to Israel, we address an important criticism of the large literature on the role of host-country language acquisition on the assimilation of immigrants. Barry Chiswick and others (Berman et al, 2003; Carliner, 1996, 2000; Chiswick, 1998; Chiswick and Miller, 1992, 1995, 1999; Dustmann, 1994, 1999; Dustmann and van Soest, 2002; McManus, 1985; McManus et al 1983; Tainer, 1988) have established, for a broad range of countries, that immigrants with good knowledge of the host-country language have higher earnings than apparently comparable immigrants with little or no ability to speak the host language. However, as recognized by Borjas (1994) and Chiswick and Miller (1992) knowledge of the host language may be indicative of other skills or characteristics of workers such as general cognitive skills. Chiswick and Miller as well as Dustmann and van Soest (2002) use an instrumental variables approach to address this problem, but IV relies on strong exclusion restrictions. Dustmann and Fabbri (2003) use propensity scores, which avoids the strong linearity assumptions required for IV but requires that there are no unobservable variables that influence both earnings and language knowledge.

Berman, Lang and Siniver (2003, hereafter BLS) attempt to address the endogeneity problem by examining the relation between wage *growth* and language knowledge *growth* and obtain results that

are similar to those found elsewhere in the literature. However, there is a parallel concern that the ability to acquire language skills may be indicative of the ability to acquire other skills.

If knowledge of host language is correlated with other skills, then we would expect that it would be correlated with knowledge of other languages. Therefore controlling for English knowledge is a partial control for unobserved worker characteristics. We find that Russian immigrants to Israel who speak good Hebrew also tend to speak good English. However, when we include knowledge of English as an explanatory variable in the wage equation, the effect on the magnitude and the interpretation of the coefficient on Hebrew is negligible because the *conditional* correlation between English and Hebrew knowledge is small.

This issue becomes even stronger in the BLS context where the speed with which individuals learn Hebrew may well capture general cognitive aptitude. If so, we would expect a strong correlation between growth of knowledge of Hebrew and of English. In fact, growth in Hebrew fluency and growth in English fluency are uncorrelated in our sample. Thus, our results reinforce the earlier literature on the role of host-language acquisition on immigrant wage growth by demonstrating that, at least in this context, second-language acquisition is unlikely to be correlated with unobserved ability.

Finally, our study contributes to the literature showing language-skill complementarity. In a manner analogous to BLS, we address the differential return to English, as well as Hebrew knowledge, across skill classes. Our results are largely supportive of that research.

We have data on a large sample of immigrants who are not native English speakers, primarily Russian immigrants. This group is particularly interesting because many of them acquired their knowledge of English after moving to Israel and almost none knew Hebrew on arrival in Israel. Moreover, we have collected data on a large sample of native Israelis working in the same occupations and workplaces as the immigrants.

As in BLS, we have data on language knowledge and wages at two points in time. Thus, we are able to examine how the wages of the immigrants and the natives change as they gain familiarity with English. As noted above, cross-section results may confound other skills with English knowledge. In particular, in most non-English speaking countries, knowledge of English is more common among better-educated and more advantaged workers, even controlling for measured characteristics.

Our key findings are as follows:

1. In cross-section estimates there is a significant return to English knowledge for both immigrants and natives with high levels of education.
2. Language acquisition is an important element in immigrant/native earnings convergence, but most of this convergence is explained by factors other than language acquisition.
3. These results are confirmed using panel data on wages and knowledge of Hebrew and English over time.
4. The benefits of English knowledge vary across occupations in ways that are largely consistent with past evidence on language-skill complementarity.
5. Natives and immigrants with high levels of education benefit similarly from knowing

English. While immigrants with low levels of education do not benefit from knowledge of English, there is some evidence that native Israelis do.

6. Conditional on occupation, the rate at which immigrants learn English and Hebrew are largely orthogonal. Therefore earlier work on the importance of knowledge of the host-country language (Hebrew) does not appear to be significantly biased by the absence of measures of English knowledge.

2. Data

We focus on native Israelis and immigrants to Israel from the former Soviet Union to whom we will refer, somewhat incorrectly, as Russians. The Russian immigration to Israel should be of broad interest to economists who study immigration for a number of reasons. First, it was an unusually large immigration. The arrival of the Russian population in the late 1980's and first half of the 1990's increased the population of Israel by about one-eighth. Second, relative to most migrations, we face fewer selection problems when studying this group. The cost of migration from the former Soviet Union to Israel was relatively low. Perhaps more importantly, there has been little return migration although there has been some onwards migration to other locations, particularly the United States.[1] This means that we have less reason to be concerned that years since migration will be correlated with unobserved characteristics because return migrants are disproportionately successful individuals who use their wealth to return home or because they are disappointed and less successful.

Our primary data source is the Workplace Occupational Survey (WOS) conducted by the Department of Economics at the College of Management in Israel under the direction of one of the authors. The survey is not intended to be representative of the Israeli or immigrant populations but instead targets the types of workplaces and occupations which previous studies have shown to have a high proportion of immigrants from the former Soviet Union. The nature of the research required the use of a convenience sample, targeting firms that were willing to allow interviewers access to their employees and which were known to have large numbers of workers in occupations in which Russian immigrants are frequently employed.

The WOS focuses on four different types of workers:

1. Physicians and nurses: This sample consists of physicians and nurses at seven hospitals, including five of the largest hospitals in Israel and two smaller hospitals. Interviewers attempted to survey all native Israeli and Russian immigrants employed as doctors or nurses in these locations. A total of 244 medical professionals of whom 123 were immigrants were surveyed.
2. Unskilled workers: This sample consists of workers at ten gas stations, hotels and supermarkets including 178 native Israelis and 170 immigrants.
3. Skilled blue-collar workers: Skilled workers were surveyed at thirty companies. The sample is comprised of 570 native Israelis and 571 immigrants.

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[1]For a fuller description, see Beenstock and ben Menahem (1997) and Friedberg (2001). The issue of selection in studies of migration is discussed in Chiswick (1978) and Borjas (1987).

4. High-tech: This sample consists of technicians, software engineers and similar workers employed in high-tech companies. Nine hundred ninety-three workers of whom 619 were immigrants were surveyed at fourteen companies.

The interviews were conducted between late 1998 and early 2000. In total, the WOS covers 1483 immigrants who arrived in Israel no earlier than 1989 and 1243 natives working in the same occupations and workplaces.

The strength and the weakness of the WOS is that it focuses on occupations in which there were high concentrations of Russians immigrants. As a result, it provides relatively large samples of immigrants in these occupations, and it is possible to analyze outcomes by occupation. In addition, since immigrants and natives are surveyed in the same firms, we are implicitly controlling for establishment in our estimates. For some purposes this is an advantage. For others it is a disadvantage. In particular, the WOS is not well-designed to study issues of occupational mobility or differences between natives and immigrants in the distribution of occupations and/or establishment of employment.

The Israelis in the sample are almost certainly not representative of the native population, and there is no guarantee that Russian immigrants in those occupations are representative of Russian immigrants as a whole. However, there are some strong similarities between WOS and a representative sample of Russian immigrants.

Table 1 compares the characteristics of the WOS sample with the sample of Russian immigrants in the Israeli Income Survey and also shows the characteristics of native Israelis in the WOS. The immigrants in the WOS are about four years younger and have correspondingly less potential labor market experience than Russian immigrants in the Income Survey. They are also slightly more educated and somewhat less likely to be married.

Table 1 also compares the natives and immigrants in the WOS sample. Relative to native Israelis, the immigrants are somewhat older with correspondingly more potential labor force experience. They also have, on average, about six-tenths of a year more education. Despite these differences, immigrants earn about thirty percent less than native Israelis.

The most valuable feature of the WOS data is the combination of current and retrospective questions on earnings and language ability. The survey asks immigrants about their ability to speak Hebrew and English. Natives are asked about their ability to speak English. Each member of the sample is questioned about both his self-assessment of his current language ability and his ability when he started the job. This approach has two advantages. First, it is consistent with recent insights from survey design (Belli 2001) which stress the importance of focusing on significant 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 April 1 of last year. Second, since there is no well-defined metric by which someone is determined to speak a language "very well," when we difference the data, we control for differences in the definition of language ability which may vary both across individuals and within individuals over time.

Respondents were asked in each case to classify their ability to speak the language (Hebrew or English) as "not so well," "well," or "very well" which we code as 1, 2 and 3. As shown in Table 1, among immigrants, the average reported Hebrew knowledge on entry into the current job was 1.6 while it was 2.4 at the time of interview. Natives are assumed to speak the language very well both at the time of entry and at the time of interview. For English, among immigrants, the average score was 1.7 on entry into the current job and 1.9 when interviewed. In contrast, for natives, it was 2.2 and 2.5. Thus natives both report better knowledge of English and more progress during their time on the job. Of course, natives have almost twice as much seniority as immigrants, so they have had more time to improve their English. Whether these differences are economically meaningful will be addressed later.

Table 2 shows the means of key variables by immigrant status and class of occupation. Within immigrant status, the average education level is increasing in the skill level of the job as would be expected. Consistent with the capital-skill complementarity hypothesis, entry-level Hebrew (among immigrants) and both entry-level and current English among natives are increasing in skill level. However, among immigrants several patterns do not immediately fit that hypothesis: entry-level English is lowest among doctors and nurses, current English is lower for doctors and nurses than for skilled and high-tech workers and current Hebrew is higher for unskilled workers than for skilled workers and high-tech workers although none of the pair-wise violations for current Hebrew is statistically significant.

We note also that within each occupational category, natives earn substantially more than immigrants even though within two of the occupations (high-tech and skilled workers), immigrants have both more education and more potential labor market experience. However, the immigrants have substantially less seniority, reflecting their relatively recent arrival in Israel.

3. Methods

We begin with the standard approach which is to estimate an equation of the form.

$$\ln(w_{it}) = \alpha_i + \beta' z_{it} + \gamma_1 x_{it} + \gamma_2 x^2_{it} + \delta_1 y_{it} + \rho_1 v_{it} + \omega H_{it} + fE_{it} + \varepsilon_{it} \qquad (1)$$

For i = 1… N persons and t = 1… T periods. Here, w is monthly eamings, z is a set of background variables including sex, education and marital status, x is potential labor market experience, y is years since migration and v is current job tenure. The variable H measures Hebrew language proficiency and the variable E measures English language proficiency. The individual effect α_i, represents a time invariant influence on earnings, which we label "ability."

Our primary goal is to estimate the effect of English and Hebrew proficiency on wages. If the covariates were randomly assigned, then the regression would recover the causal effect of these proficiencies. However, cross-sectional estimates of coefficients will be biased estimates of these causal effects if unobserved ability is correlated with the covariates. We are particularly concerned that one's ability to learn a language will be reflected in H or E but may also be correlated with unobserved earnings ability, α_i.

In the standard framework, this ability-bias can be addressed by estimating.

$$\Delta \ln(w_{it}) = \beta' \Delta z_{it} + \gamma_1 \Delta x_{it} + \gamma_2 \Delta x^2_{it} + \delta_1 \Delta y_{it} + \delta_2 \Delta y^2_{it} + \rho_1 \Delta v_{it} + \rho_2 \Delta v^2_{it} + \omega \Delta H_{it} + f \Delta E_{it} + \Delta \varepsilon_{it} \quad (2)$$

For i= 1… N persons and t = 2… T periods, and where Δ is the difference operator. This is the approach used by BLS.

There are three issues that must be addressed in our framework. The first is standard. Equation (2) can only be estimated consistently if $\Delta \varepsilon_{it}$, is uncorrelated with the covariates. This condition implies that there is no unobserved variable that influences both the rate at which individuals' wages grow and the rate at which they learn English and/or Hebrew. This condition would be violated if, for example, the abilities to learn different skills were closely related. Then we might find that people who learned Hebrew or English rapidly also increased their computer programming skills rapidly.

One useful feature of our data is that we can address this issue directly. While there are certainly significant differences between Hebrew and English, both are quite distant from Russian. In both cases Russian speakers must learn a new alphabet. It is therefore likely that the ability to learn Hebrew and the ability to learn English are similarly correlated with unobserved ability to learn other skills. If differential language acquisition is driven primarily by differences in innate learning ability, we would expect changes in fluency to be highly correlated across languages and that excluding one language from equation (2) would noticeably increase the estimated effect of the other. We can address this issue directly.

The second issue is that, in contrast with the standard setting in labor economics, while we observe each individual at two points in time because of the retrospective items in the data, the time between observations differs across individuals depending on how long they have been in their job, and there are no observations on job-switchers in the data. Nevertheless, for each immigrant $\Delta x_{it} = \Delta y_{it} = \Delta v_{it}$, and therefore only $\gamma_1 + \delta_1 + \rho_1$ and not its individual components are identified. Put differently, for immigrants, we can estimate only the joint effect of seniority, experience and assimilation (years since migration) while for natives we estimate only $\gamma_1 + \rho_1$, the joint effect of seniority and experience. We will assume that the joint effect of seniority and experience is the same for immigrants and natives so that the difference between the coefficients for natives and immigrants is the assimilation effect, δ_1.

The vector z is composed of three variables, sex, education and whether or not the individual is married. Sex and education are unlikely to change within jobs. Marital status may change. Unfortunately, we do not have data on marital status at the beginning of the job, therefore we estimate

$$\ln(w_i) = \delta_1 \Delta y_i + \gamma_2 \Delta x^2_i + (\gamma + \rho_1) \Delta v_i + \omega \Delta H_i + f \Delta E_i + \Delta \varepsilon_i \quad (3)$$

where we have dropped the redundant t subscript.[2]

The third issue concerns measurement error. Differencing data with classical measurement error can generate considerable bias because it increases the noise-to-signal ratio. It is reasonable to expect that there will be a great deal of measurement error in our measures of Hebrew and English fluency because each individual reports his fluency relative to an unknown metric. In Dustman and van Soest (2001, 2002) differencing would have resulted in a measure of fluency that was almost completely noise implying that the metric not only varies among individuals but individuals change their concept of fluency over time. Because our information about present and previous Hebrew and English ability is collected simultaneously, each individual is likely to respond to the questions about current and past fluency using a single metric for fluency. Thus differencing may actually reduce the noise-to-signal ratio by eliminating cross-individual differences in the concept without adding changes in this concept over time. Of course, we must recognize that in using retrospective data there is a risk of recall error, but by focusing on recollections of Hebrew and English ability at a time of a particular event rather than at a particular date, we have reduced the risk of recall bias (Belli et al, 2001) .[3]

Despite this optimistic assessment, there is a related difficulty. Hebrew and English knowledge are graded on a scale with only three values, causing rounding error. When differencing, this has an indeterminate effect on measurement error bias. BLS discusses this issue at length.

The essential issue can be addressed for the case where there are just two categories, say speaks and does not speak the language. In the cross-section, the estimated return to speaking the language is (everything else held equal) the return to language skill multiplied by the difference in language skill between those who report speaking the language and those who report not speaking the language. Using the longitudinal data, the estimated return is the return to language skill multiplied by the difference in the average change in the language knowledge of those who report crossing the threshold between speaking and not speaking minus the average change for those who do not report crossing the threshold. If all respondents agree on the underlying scale of language skill and the cutoff between speaking and not speaking the language (and report truthfully), then in most plausible scenarios, differencing the data would lead to lower estimates of the return to speaking the language. In essence, the people who crossed the critical boundary between speaking and not speaking the language would be likely to be drawn disproportionately from those who were initially just below the cutoff. Moreover, they would also be drawn disproportionately from those ending up just above the cutoff. The growth in skill is likely to be less than the average difference in skill of those above and below the cutoff.

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[2]The current version of the paper replaces the change in squared potential experience with potential experience.

[3]An alternate approach (Dustmann and Fabbri, 2003) is to instrument for measurement error by using two different measures of language knowledge. This approach is invalid when the instruments are both dichotomous because the measurement error in the two instruments must be correlated.

In addition, even those who do not cross the cutoff may improve their language skills. In the extreme case, suppose that everyone increased his language skills at the same rate so that there were no difference in skill gain between those who happened to cross the cutoff between not speaking and speaking the language and those who did not cross the cutoff. In that case, the estimated return using the longitudinal data would be zero regardless of how valuable language skills actually were. We will refer to these downward biases as attenuation bias.

However, it is important to note that there are circumstances in which cross-section estimates can be severely downwards biased while differenced estimates are not. In particular, if respondents have widely varying views of what constitutes speaking a language well, there may be little difference between the language skills of those reporting different levels of fluency. However, when respondents use a self-anchoring scale to report their progress, the concept of how much they have improved may be more universal. In other words, those who report no progress have genuinely made no progress while those who report a little progress have made some progress but less than those who report a lot of progress.[4]

We have three goals in this paper:

The first is to contribute to the literature on the role of host-country language acquisition in explaining the faster growth of immigrants' wages than of natives' wages. As discussed above, it is well-established that immigrant wages are positively correlated with host-country language skills, and host-country language skills increase with time spent in the host country. Thus in a statistical sense, language acquisition accounts for some of the faster wage growth among immigrants.

However, it is possible that to some degree the positive relation between host-country language skill and wages also captures the positive relation between other unmeasured skills and language skills. We address this in two ways. First, we note that other language skills are among the most plausible unmeasured skills to be correlated with host-country language skills. If including English-language skills significantly reduces the effect of Hebrew-language skills on the "years since migration" coefficient, then the case that unobserved skills badly bias the estimate of this effect becomes more plausible. On the other hand, the absence of an effect of English-language skills on the role of Hebrew-language skills would make it more difficult to develop a plausible case that the measured effect of Hebrew was badly biased. Occam's razor would suggest that the standard estimates were not badly biased.

Our second approach is similar to that of BLS. We account for permanent differences in ability by differencing and estimating equation (3). As BLS note, since all the variation in increased years since migration comes from work years within the same job, δ_1 does not capture 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.

Our second objective is to shed light on the value of foreign-language skills for both natives and

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[4]See BLS for more discussion of this and related issues.

immigrants. To the best of our knowledge, the only comparable study is Grin (2001) which examines the value of English-language knowledge in Switzerland. Unlike Switzerland, Israel has a unique national language. Even English-speaking immigrants to Israel (primarily Americans) generally achieve fluency in Hebrew either because they studied Hebrew before arriving or because they attend government-subsidized ulpan (Hebrew language schools).

However, relatively few foreigners speak Hebrew fluently so that international trade and contacts with tourists generally take place in some other language, most notably English. Medical professionals and high-skill workers in high-tech industries are likely to be able to access information more rapidly and more readily in English. But unless one of the parties to a conversation is a native English-speaker, Israelis are unlikely to communicate among themselves in English. Thus, the Israeli situation is more comparable to that of many other countries than is the relatively unique situation of Switzerland. In addition, we have an advantage over Grin in that we have longitudinal data that allow us to account for permanent differences in unmeasured ability.

Finally, we contribute to the literature on language-skill complementarity, which is the focus of BLS. As in BLS, we estimate both equations (1) and (3) separately for four different occupations. In this way we are able to assess the value of language skills for different occupations and to determine whether the language-skill complementarity hypothesis holds for English as well as knowledge of the host-country language, in this case Hebrew.

4. Results

We begin by estimating a standard log wage equation supplemented with a dummy variable for being an immigrant and with years since migration. Column (1) of Table 3 shows the results for the Israeli Income Survey while column (2) shows an identical specification for the WOS. In the WOS, we estimate that newly arrived immigrants earn 39% less than otherwise equivalent native workers while this figure is 46% in the Income Survey. The coefficient on years since migration is .033 in the WOS compared with .024 in the Income Survey. Thus we find somewhat smaller wage differentials in the WOS than in the general population, possibly because we implicitly partially control for occupation and establishment in the WOS. Other coefficients are similar except for a much larger coefficient on male in the IS which probably reflects much more part-time work among women in the IS and a lower coefficient on being married in the WOS.

Cross-Section Results

The third column simply adds seniority on the job to the specification since it is available in the WOS but not the IS, and we want to ensure that it is not responsible for changes in coefficients when we add additional controls for language knowledge. This turns out to be important because seniority and years since migration are correlated, and seniority is positively associated with earnings. Controlling for seniority lowers the annual return to years since migration from 3.3% to 2.6%.

When we add dummy variables for speaking Hebrew well and very well, the coefficients (not shown) are .073 and .127. The effect of the categorical variable is approximately linear. Since we cannot reject linearity, in column (4), we add Hebrew as a continuous variables. The results are consistent with the findings in BLS. Knowledge of Hebrew accounts for about one-quarter of the

return to years since migration, reducing the coefficient from 2.6% to 2.0%.

So far our results confirm existing results in the literature. Our first innovation in the paper is to include English language knowledge in the regression. As with Hebrew, we begin by including dummy variables for knowing English well and very well. In contrast with Hebrew, we easily reject linearity. The coefficient on knowing English well (not shown) is small, slightly negative and statistically indistinguishable from zero. In contrast the effect on wages of knowing English very well is large and positive. Column (5) augments the specification in column (4) by adding a dummy variable for knowing English very well.

There are a number of interesting findings when we add knowledge of English to the equation. First, we find a large effect of English knowledge on earnings. Knowing English well is associated with a 14% (thirteen log points) increase in earnings. Although, as noted in the previous paragraph, knowing English only well and not very well has no effect on earnings, knowing English very well increases earnings (relative to not well) as much as knowing Hebrew well.

Second, contrary to what we would have anticipated, including English knowledge has little effect on the coefficient on Hebrew. Unconditionally, knowledge of Hebrew and English are strongly correlated. Native Israelis are more likely to know English very well than are immigrants, and immigrants who know Hebrew very well are also more likely to know English very well than are other immigrants. This is consistent with our view that Hebrew knowledge (or host country knowledge more generally) may proxy for a set of other skills. However, conditional on the other factors, Hebrew knowledge and speaking English very well are uncorrelated. The major correlates of English knowledge are education and being a native Israeli. As a result, the lower frequency of English knowledge among immigrants "explains" their lower wages.

The conditional orthogonality of English and Hebrew knowledge is not only surprising but potentially important. The specifications in table 3 are traditional in the literature on immigration and language. A major concern in this literature is that knowledge of host-country language may capture other skills. If people who learn the host country language are otherwise more productive because of greater cognitive or other skills, then we may incorrectly attribute the benefit of these other skills to language knowledge. Yet the cognitive skill that seems most likely to be correlated with ability to learn the host country language is language acquisition skill more generally. And the absence of a correlation between knowledge of Hebrew and English, conditional on other measured factors, suggests that host-country language skills are not likely to be proxying for other cognitive skills.

Supplementary regressions (not shown) largely support this conclusion. Among immigrants, Hebrew knowledge is positively correlated with knowing English well. However, when we estimate the specifications in columns (4) and (5) for immigrants alone, the coefficient on Hebrew is .068 in the specification in the fourth column and .064 in the fifth column.

For completeness, in column (6) we add fixed occupation and establishment effects. There is little effect on the key coefficients except that the effect on earnings of knowing Hebrew rises notably from 6% to 10%. This result is driven primarily by the inclusion of the establishment effects and

implies, somewhat surprisingly, that immigrants who speak better Hebrew are in companies with a lower overall rate of pay.

Language-Skill Complementarity

In order to see if individuals with high levels of education (13 years and above) gain more from knowing Hebrew and English than those with low level of educations (12 years and less), we re-estimate the main specifications from table 3 separately for individuals with high and low levels of education. The results are shown in table 4. Knowledge of Hebrew shows strong evidence of complementarity with education. The estimated return to Hebrew knowledge is zero for those with twelve years of education or less. Among those with more education, knowing Hebrew very well is associated with about 24% higher earnings than for those who know Hebrew "not very well."

The evidence of language-skill complementarity is somewhat weaker for knowledge of English. The estimates suggest that the premium for knowing English well is about 14% for the more educated group but only about 7% for the less educated group. Nevertheless, the return to knowing English well is nontrivial for the less-educated group and statistically different from zero.

Further investigation (specifications not shown) reveals that the pattern of a linear return to Hebrew and only a return to speaking English well holds for the more educated group. However for the less educated group, those who speak English well earn slightly (and insignificantly) less than those who speak it not very well while those who speak it very well earn slightly (and insignificantly) more than those who speak it not very well. However, the difference between those who speak it very well and well is about 7% and is statistically significant, and this is what is picked up by the coefficient on speaking English well.

Consistent with the findings on language-skill complementarity, controlling for language knowledge has a large effect on the estimated effect of years since immigration for more educated workers, reducing the coefficient by almost half. However, it has essentially no effect on the estimated coefficient for less educated workers.

Additional estimates (not shown) including fixed occupation and establishment effects show the same pattern. Including these controls has almost no effect on the estimated return to language knowledge for more educated workers. However, it shifts the coefficient on Hebrew knowledge from negative to an insignificantly positive 3% for less educated workers. Thus the surprising results in column (6) of Table 3 can be attributed almost entirely to a negative and possibly spurious correlation between establishment-level wages and Hebrew knowledge among less-educated workers.

Table 4A repeats the exercise separately for immigrants and natives. Not surprisingly, since the return to Hebrew knowledge in the full sample is identified by its effect on immigrants, the results for Hebrew knowledge in the immigrant sample are similar to those obtained for the full sample. In the case of English, there are some differences. Overall, in the sample, immigrants and natives have similar returns to speaking English very well. There is, however, some evidence that the return to speaking English very well is higher among natives than among immigrants although the difference in the coefficients is not statistically significant.

Table 5 shows the relation between language knowledge and earnings by occupation. Consistent with the education findings, we find large and statistically significant effects of Hebrew knowledge in the two high-education areas (medical and high-tech) and small and statistically insignificant effects for skilled and unskilled workers.

The results for knowing English well are also consistent with the education results. Knowledge of English is very valuable in the medical professions and, to a lesser extent, in high-tech, but less so for skilled and unskilled workers. Again the statistically significant effect of knowing English very well for skilled workers is driven by a surprising negative effect of only knowing English well relative to knowing it "not so well."

Longitudinal Estimates

The estimated effected of Hebrew fluency and English fluency on wages may be biased if more able workers are more likely to know Hebrew and English. We address this issue in table 6 by exploiting the availability of longitudinal information about language proficiency for immigrants. Recall that respondents were asked about their earnings and their knowledge of Hebrew and English both currently and when they started their job. Along with information on their seniority, these data allow us to estimate equation (3), the differenced version of the human-capital earnings function. The results are presented in table 6.

The first column of table 6 corresponds to the third column of table 3. The results are similar. In the latter, the estimated return to years since migration is 2.6% per year. In the differenced results, the difference in the return to tenure and experience between immigrants and natives is 2.4% per year. The second column in table 6 adds the change in Hebrew knowledge. Consistent with BLS, we find no evidence of ability bias in the cross-section estimate of the return to Hebrew knowledge. The coefficient on growth of Hebrew knowledge is .07 while the coefficient on Hebrew knowledge in the cross-section (table 3, column (4)) is .06. The effect on the assimilation coefficient of including Hebrew knowledge is somewhat lower in the longitudinal estimates than in the cross-section estimate going from .24 to .20 rather than from .26 to .20.

So far our results closely resemble those of BLS, but one of the potential criticisms of that paper is that individuals who learn Hebrew quickly may also learn other skills quickly. Thus the coefficient on Hebrew acquisition would capture other dimensions of learning, and we would over-estimate the benefit from learning Hebrew. The third column of table 6 addresses this criticism by controlling for learning to speak English very well. If the ability to learn Hebrew is highly correlated with the ability to learn other skills, we would certainly expect it to be highly correlated with the ability to learn other languages. Therefore, if controlling for learning English does not affect the coefficient on learning Hebrew, this criticism of BLS becomes considerably less plausible.

In fact, controlling for English-language acquisition has little effect on the estimated benefit from learning Hebrew which goes from 7% to 6% although there is a notable benefit (12%) from learning English very well. We note that this coefficient is also almost unchanged from the cross-section estimate of the return (13%). Controlling for other measurable characteristics of workers does not noticeably affect the other results except to somewhat lower the estimated rate at which assimilation occurs.

Taken together, tables 3 and 6 provide no support for the view that there is ability bias in either the cross-section or longitudinal estimates of the benefits of language acquisition. We admit that this result is somewhat surprising. We would expect both knowledge of Hebrew and English and growth of knowledge in these languages to be correlated. We have already noted that language knowledge among immigrants is correlated but not conditional on other measured characteristics. In the case of language acquisition, one concern is that those who claim to have spoken a language very well when they began their job cannot show gains in their knowledge of that language. Perhaps, the way the data are recorded masks a correlation in acquisition of language.

There is some validity to this concern. Among those who did not speak Hebrew "very well" and did not speak English "very well" when they began their job, only 8% of those who reported no progress n Hebrew learned to speak English very well compared with 15% of those who reported some progress. However, much of this difference can be explained by the fact that workers who have been in their current job longer are more likely to have made progress in both Hebrew and English. Conditional on seniority in the job, potential experience and education (or equivalently, age and education), the correlation between Hebrew and English acquisition among immigrants who spoke neither "very well" is small and statistically insignificant. Thus acquiring Hebrew and English language skills are conditionally independent.

The last column of table 6 controls for establishment and occupation fixed effects. In contrast with the cross-section estimates where such controls noticeably increased the estimated return to Hebrew knowledge, these additional controls have essentially no effect on the results.

Longitudinal Estimates and Language-Skill Complementarity

The top panel of table 7 shows the longitudinal estimates by education and occupation sub-group. For the most part the longitudinal estimates are consistent with the results shown so far. Individuals with thirteen or more years of education have large and statistically significant returns to knowledge of Hebrew and English. Those with twelve or fewer years of education have small and statistically insignificant returns.

When we look within occupation, unskilled workers show little or no return to language skills. The longitudinal estimate of the return to Hebrew among skilled workers is actually somewhat higher than in the cross-section and is now higher than the estimated return to English. Returns to language knowledge remain high among high-tech workers.

The major difference between the cross-section and longitudinal findings concerns medical professionals. In contrast with the cross-section results where we found a large return to Hebrew knowledge, in the longitudinal estimates we find no return to Hebrew knowledge. This result is surprising and goes against the language-skill complementarity hypothesis.

One explanation for the anomalous result for medical professionals is that fluency in Hebrew in such jobs is so essential that we would not expect much wage growth until individuals become fluent and that wage growth would be fastest for those who are truly fluent. To test this conjecture, we reestimated the top panel of table 7 but dropping all those who say they spoke Hebrew very well when they started their job (not shown). For the other three occupations, this has a small and

negative effect on the estimated return to learning Hebrew. However, in the case of medical professionals the coefficient on increased Hebrew knowledge jumps to .12, still somewhat lower than in the cross-section but higher than for the lower skill occupations. What drives this result is that, among medical professional, wage increases are largest for immigrants who say that they were already fluent when they began their current job. Thus among those still learning Hebrew, wage growth is faster among those who make more progress, but fluency is central to fast wage growth. This is consistent with the language-skill complementarity hypothesis.

The bottom two panels of table 7 show the cross-section estimates of the return to language knowledge and the difference between these estimates and the longitudinal estimates. In general, we would expect the longitudinal estimates to be smaller either because the longitudinal estimates eliminate ability bias or because they are due to attenuation bias from measurement error and the use of categorical data.

In fact, consistent with BLS in the case of Hebrew, we do not see a strong pattern of different results from the longitudinal and cross-section results. With the exception of medical professions, the cross-section and longitudinal estimates do not diverge sharply and the average coefficient is the same across the two techniques. Thus, again with the exception of doctors and nurses, there is no evidence that either form of bias is strong when measuring the return to Hebrew knowledge.

In contrast, the estimated return to knowing English well is consistently higher across all six samples when we estimate the return using the cross-section than it is when we use longitudinal data although again the estimates for medical professionals stand out as being particularly divergent.

Table 8 mimics table 4A in presenting the results separately for immigrants and natives except that it provides longitudinal rather than cross-section estimates. As we would expect based on the results so far, the estimates of the return to Hebrew are similar whether we rely on the cross-section or longitudinal results, and we continue to find strong evidence of a return to Hebrew knowledge among immigrants with high levels of education but not among those with low levels of education.

The results do suggest some ability bias in the cross-section or attenuation bias in the longitudinal estimates of the return to speaking English very well among immigrants. For each of the two education groups and for immigrants as a whole, the return to English fluency is lower using the longitudinal estimates than using the cross-section estimates. However, the pattern is not maintained for native Israelis. For both education groups and natives as a whole, the estimated return to fluency is higher using the longitudinal estimates than using the cross-section estimates. There is also some evidence that the return to English knowledge is higher for natives than it is for the Russian immigrants.

5. Summary and Conclusion

Previous research on the effect of language fluency, in general, and host-country language fluency, in particular, has been suspect because of possible biases due to the correlation of fluency with unmeasured ability and correlation between duration in the host country and knowledge of the host-country language. The second bias is generally less of an issue because, as in our work, it is often

possible to control for years since migration, but there remains a concern that years since migration is endogenous to language skills, a problem that is diminished by focusing on Russian immigrants to Israel..

The more serious concern is that language skills and unmeasured ability are correlated. Since measured skill, in the form of education, and language skill are positively correlated, it seems likely that language skills are also correlated with unmeasured characteristics of workers. We addressed this issue in two ways. First, the skill that, conditional on education, is most plausibly correlated with knowledge of one second language is knowledge of another second language. If we find that, conditional on other variables, there is no correlation between knowledge of one language and knowledge of another, then it is unlikely that unmeasured skills are an important source of bias in cross-section estimates of the return to language skills.

Among Russian immigrants to Israel, knowledge of Hebrew is positively correlated with knowledge of English. However, we find no evidence that they are correlated conditional on other measured variables. As a result including English in a cross-section wage equation has little effect on the estimated return to Hebrew and vice versa.

Our second approach to addressing the question of bias is to use differenced data, that is we regress change in wage on change in language fluency. This eliminates then any bias from a permanent skill. For the entire sample, the estimated return to Hebrew knowledge is virtually identical in the cross-section and longitudinal estimates. The estimated return to knowing English very well is somewhat lower in the latter than in the former.

One concern about the longitudinal estimates is that the ability to learn a language may be correlated with the ability to acquire other scarce skills. Therefore, Russian immigrants who learn Hebrew quickly would also be people who would tend to learn other skills quickly. The high return to learning Hebrew would reflect the value of being able to learn quickly rather than the value of knowing Hebrew.

Combining our two approaches addresses this problem. We would expect the ability to learn one language to be highly correlated with the ability to learn a second language. Therefore if acquisition of Hebrew fluency is merely capturing general learning skills, we would expect adding acquisition of English fluency to greatly reduce the measured return to learning Hebrew. In fact, we find no such effect. In the longitudinal estimates, there is essentially no difference in the estimated return to learning Hebrew regardless of whether we control for increased English fluency.

Taken together, we conclude that there is little evidence of ability bias in the estimated return to Hebrew knowledge in these data. It is, of course, impossible to know whether this result generalizes more broadly to other populations so that we can have confidence in other OLS cross-section estimates of the return to language fluency, but it makes that assumption more plausible and thereby strengthens much of the existing literature.

Our results for English fluency, however, suggest the need for some caution. For the sample as a whole, we find that the estimated return to speaking English well is somewhat lower in the

longitudinal estimates (10%) than in the cross-section estimates (13%). While this difference does not change the general conclusion that there is a significant benefit for Israelis to know the world's dominant language, it does suggest that we should be careful not to draw overly strong conclusions from cross-section estimates.

The 10% return to English fluency in Israel can be juxtaposed with the 6% estimated return to education in the data. The time to fluency in a second language (at least for English-speaking adults learning a Germanic language in intensive study) is on the same order as the time spent in one to two years of regular education.[5] This suggests that, at least in Israel, investment in English is at least as valuable as investment in other forms of education.

Of course, we do not mean to suggest that the choice is either to invest in language skills or in other forms of education. In fact, one of the key findings in this paper is that knowledge of the foreign language, English, as well as the host-country language, Hebrew, is complementary with education. In the cross-section estimates, we find a large return to Hebrew knowledge only for those individuals with at least thirteen years of education and only among medical professionals and those in high-tech jobs. We find less strong evidence of English-language/skill complementarity. In the cross-section estimates there are significant returns to English fluency among those with twelve or fewer years of education but they are only half the returns received by those with higher levels of education. Similarly, we find significant returns to speaking English very well among both skilled and unskilled workers although these returns are smaller than those for medical professionals and those in high-tech jobs. The general interpretation of the results using longitudinal data is similar.

On the other hand, our results do not suggest that lack of knowledge of English is a severe disadvantage for either native Israelis or Russian immigrants. Even among medical professionals and those in high-tech jobs, our estimates of the return to speaking English very well are 14% and 11%. Our estimate for those with at least thirteen years of education is 12%. While it is probably the case that most students at the tertiary level could get a higher return from studying English than from studying many humanities fields, it is not obvious that the return to studying English is higher than the return to studying other, more technical subjects.

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[5]The Foreign Service Institute estimates that it takes 720 hours of instruction for an average language learner to reach stage 2+ proficiency in Germanic and Romance languages. If we view stage 3 as corresponding to speaking the language "very well," then the time to learn English very well would be about equivalent to a year of education. Stage 5, which corresponds to full fluency would take correspondingly longer.

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TABLE 1
Basic Data for Workplace Occupation Survey and Comparison with Income Survey

	Income Survey	WOS Survey	
	(Russian Immigrants only)	Immigrants	Israelis
Age	39.4 (11.8)	35.7 (10.3)	33.6 (10.3)
Years of education	13.7 (3.1)	14.2 (2.9)	13.6 (3.1)
Labor force Experience	19.8 (11.5)	15.5 (9.8)	13.9 (10.4)
Years since Migration	5.4 (2.1)	5.2 (2.1)	-
Currently married	0.80 (0.40)	0.72 (0.45)	0.62 (0.49)
Male	0.61 (.49)	0.56 (0.50)	0.59 (0.49)
Job Tenure	-	3.17 (2.03)	5.91 (6.27)
Current Hebrew	-	2.41 (0.68)	*
Entry Hebrew	-	1.60 (0.72)	*
Current English	-	1.94 (0.71)	2.47 (0.59)
Entry English	-	1.66 (0.66)	2.16 (0.66)
Monthly Earnings	3689 (1700)	3861 (1900)	5280 (3283)
Unskilled Worker	12%	11%	14%
Skilled Worker	36%	39%	46%
Physicians/Nurses	-	8%	10%
High-tech	-	42%	30%

*Assumed to speak Hebrew very well. Coded 0 for natives.
- Not available.

TABLE 2
Basic Data for Workplace Occupation Survey by Occupation and Immigrant Status

	Nurses & Doctors	High-Tech	Skilled	Unskilled	Nurses & Doctors	High-Tech	Skilled	Unskilled
	Immigrants				**Natives**			
Age	34.4 (7.7)	38.2 (10.3)	34.6 (10.5)	31.5 (9.6)	35.7 (8.6)	32.6 (9.6)	33.4 (10.7)	34.6 (11.2)
Years of education	16.5 (2.1)	15.3 (2.5)	13.1 (2.8)	12.6 (2.3)	17.8 (2.1)	14.7 (2.4)	12.6 (2.8)	11.6 (2.3)
Experience	11.9 (7.4)	16.9 (9.7)	15.5 (10.1)	12.9 (9.58)	11.9 (8.1)	11.9 (8.9)	14.8 (11.0)	17.0 (11.8)
Years since Migration	4.63 (1.99)	5.7 (1.9)	5.14 (2.24)	4.76 (1.81)	-	-	-	-
Currently Married	0.74 (0.44)	0.74 (0.44)	0.69 (0.46)	0.70 (0.46)	0.73 (0.45)	0.57 (0.50)	0.60 (0.49)	0.69 (0.46)
Male	0.41 (0.49)	0.52 (0.50)	0.60 (0.49)	0.69 (0.46)	0.43 (0.50)	0.57 (0.50)	0.62 (0.49)	0.66 (0.47)
Job Tenure	3.33 (2.12)	3.16 (2.00)	3.20 (2.15)	3.04 (1.73)	9.25 (7.44)	4.94 (5.95)	5.46 (5.32)	7.10 (7.82)
Current Hebrew	2.59 (0.60)	2.40 (0.67)	2.37 (0.71)	2.45 (0.62)	-	-	-	-
Entry Hebrew	1.72 (0.73)	1.66 (0.70)	1.59 (0.70)	1.35 (0.57)	-	-	-	-
Current English	1.86 (0.66)	2.01 (0.71)	1.91 (0.72)	1.79 (0.76)	2.79 (0.45)	2.61 (0.50)	2.38 (0.57)	2.21 (0.72)
Entry English	1.50 (0.55)	1.70 (0.67)	1.65 (0.67)	1.61 (0.70)	2.44 (0.58)	2.22 (0.63)	2.13 (0.62)	1.93 (0.79)
Monthly Earnings	4827 (2231)	4255 (2211)	3469 (1197)	3041 (1600)	7561 (3827)	6333 (4049)	4320 (2003)	4589 (2983)

TABLE 3
CROSS -SECTION EARNINGS ESTIMATES

	1 IS	2 WOS	3 WOS	4 WOS	5 WOS	6 WOS*
Immigrant	-0.62 (0.02)	-0.50 (0.03)	-0.39 (0.03)	-0.51 (0.04)	-0.46 (0.04)	-0.60 (0.06)
Years since Migration/10	0.24 (0.01)	0.33 (.05)	0.26 (0.05)	0.20 (0.05)	0.20 (0.05)	0.22 (0.05)
Hebrew	-	*	*	0.06 (0.02)	0.06 (0.02)	0.10 (0.02)
English (very well)	-	*	*	*	0.13 (0.02)	0.14 (0.02)
Male	0.51 (0.01)	0.16 (0.02)	0.17 (0.02)	0.17 (0.02)	0.18 (0.02)	0.17 (0.01)
Married	0.34 (0.07)	0.02 (0.02)	0.01 (0.02)	0.01 (0.02)	0.01 (0.02)	0.02 (0.02)
Education	0.08 (.002)	0.07 (.003)	0.06 (.003)	0.06 (.003)	0.06 (.003)	0.04 (.003)
Experience	0.05 (.001)	0.02 (.003)	0.02 (.003)	0.02 (.003)	0.02 (.003)	0.02 (.002)
$Exp^2/100$	-0.09 (.003)	-0.05 (0.01)	-0.05 (0.01)	-0.05 (0.01)	-0.05 (0.01)	-0.05 (0.01)
Seniority	-	*	0.02 (.002)	0.02 (.002)	0.02 (0.002)	0.02 (0.002)
Intercept	6.14 (0.05)	7.25 (0.02)	7.22 (0.04)	7.23 (0.04)	7.24 (0.04)	7.49 (0.04)
R^2	-	0.32	0.35	0.36	0.37	0.48
N	16171	2726	2726	2726	2726	2726

*Column 6 also controls for occupation and establishment fixed effects.

TABLE 4
CROSS -SECTION EARNINGS ESTIMATES
by Education

	13 or More Years Education			**12 or Fewer Years Education**		
Immigrant	-0.35 (0.04)	-0.77 (0.06)	-0.68 (0.03)	-0.25 (0.04)	-0.23 (0.06)	-0.22 (0.06)
Years since Migration/10	0.34 (0.07)	0.24 (0.07)	0.23 (0.07)	0.19 (0.08)	0.20 (0.08)	0.20 (0.08)
Hebrew	*	0.12 (0.02)	0.11 (0.02)	*	-0.01 (0.02)	-0.01 (0.02)
English (very well)	*	*	0.14 (0.02)	*	*	0.07 (0.03)
Male	0.19 (0.02)	0.19 (0.02)	0.19 (0.02)	0.13 (0.02)	0.13 (0.02)	0.13 (0.02)
Married	0.01 (0.02)	0.01 (0.02)	0.01 (0.02)	-.01 (0.03)	-.005 (0.03)	-0.01 (0.03)
Education	0.06 (.005)	0.06 (.005)	0.06 (.005)	0.04 (0.01)	0.04 (0.01)	0.03 (0.01)
Experience	0.03 (.004)	0.03 (.004)	0.03 (.004)	0.01 (.003)	0.01 (.003)	0.01 (.003)
$Exp^2/100$	-0.08 (0.01)	-0.08 (0.01)	-0.07 (0.01)	-0.03 (0.01)	-0.03 (0.01)	-0.03 (0.01)
Seniority	0.02 (.003)	0.02 (.003)	0.02 (.003)	0.03 (.003)	0.03 (.003)	0.03 (.003)
Intercept	7.20 (0.08)	7.21 (0.08)	7.19 (0.08)	7.51 (0.09)	7.51 (0.09)	7.52 (0.09)
R^2	0.36	0.37	0.39	0.23	0.23	0.24
N	1648	1648	1648	1078	1078	1078

TABLE 4A
CROSS-SECTION ESTIMATES OF THE RETURN TO LANGUAGE KNOWLEDGE
by Immigrant Status

	Hebrew	English Very Well
Immigrants		
All	0.06 (0.02)	0.11 (0.02)
High Education	0.11 (0.02)	0.13 (0.03)
Low Education	.000 (0.02)	0.03 (0.04)
Natives		
All	*	0.10 (0.03)
High Education	*	0.11 (0.04)
Low Education	*	0.08 (0.03)

TABLE 5
CROSS -SECTION EARNINGS ESTIMATES
by Occupation

	Nurses & Doctors	High-Tech	Skilled	Unskilled
Immigrant	-0.78 (0.17)	-0.71 (0.08)	-0.30 (0.06)	-0.29 (0.12)
Years since Migration/10	0.35 (0.20)	0.29 (0.09)	0.17 (0.07)	0.03 (0.16)
Hebrew	0.19 (0.06)	0.10 (0.03)	0.02 (0.02)	0.04 (0.05)
English (very well)	0.27 (0.06)	0.18 (0.03)	0.09 (0.02)	0.05 (0.04)
R^2	0.56	0.40	0.24	0.54
N	244	993	1141	348

Other controls: male, married, education, experience and experience squared, seniority.

TABLE 6
LONGITUDINAL EARNINGS ESTIMATES

	(1)	(2)	(3)	(4)	(5)
Immigrant	-0.01 (0.02)	-0.06 (0.02)	-0.05 (0.02)	-0.05 (0.02)	-0.06 (0.02)
Seniority/10	0.24 (0.01)	0.24 (0.01)	0.22 (0.01)	0.21 (0.02)	0.21 (0.02)
Seniority/10* Immigrant	0.24 (0.04)	0.20 (0.04)	0.20 (0.04)	0.16 (0.04)	0.19 (0.04)
Change in Hebrew fluency	*	0.07 (0.01)	0.06 (0.01)	0.06 (0.01)	0.05 (0.01)
Change in knowing English very well	*	*	0.12 (0.02)	0.10 (0.02)	0.09 (0.02)
Additional controls	No	No	No	Yes	Yes
Fixed occupation and establishment effects	No	No	No	No	Yes
R^2	0.13	0.14	0.15	0.18	0.29
N	2726	2726	2726	2726	2726

Dependent variable: Change in log wage
Additional controls: Experience, male, education, married

TABLE 7
LONGITUDINAL EARNINGS ESTIMATES
by Education and Occupation
and Their Relation to Cross-Section Estimates of the Return to Language Knowledge

	13 or More Years Education	12 or Fewer Years Education	Doctors and Nurses	High-Tech	Skilled	Unskilled
Immigrant	-0.04 (0.03)	-0.03 (0.03)	-0.08 (0.08)	-.003 (0.03)	-0.08 (0.03)	-0.10 (0.06)
Seniority/10	0.28 (0.02)	0.14 (0.02)	0.27 (0.05)	0.33 (0.03)	0.14 (0.03)	0.08 (0.03)
Seniority/10* Immigrant	0.16 (0.06)	0.09 (0.07)	0.57 (0.16)	0.17 (0.07)	0.09 (0.06)	0.28 (0.13)
Change in Hebrew Fluency	0.08 (0.01)	0.03 (0.02)	-0.02 (0.05)	0.09 (0.02)	0.07 ((0.02)	0.01 (0.03)
Change in Knowing English Very Well	0.12 (0.02)	0.03 (0.03)	0.14 (0.05)	0.11 (0.03)	0.06 (0.03)	0.04 (0.05)
R^2	0.21	0.08	0.37	0.26	0.09	0.07
Cross-Section Estimates (from tables 4 and 5)						
Hebrew	0.11	-0.01	0.19	0.10	0.02	0.05
English	0.14	0.07	0.27	0.18	0.09	0.05
Difference Between Cross-Section and Longitudinal Estimates						
Hebrew	0.03	-0.04	0.21	0.01	-0.05	0.04
English	0.02	0.04	0.13	0.07	0.03	0.01
N	1648	1078	244	993	1141	348

Dependent variable: Change in log wage
Additional controls: Experience, male, education, married

TABLE 8
LONGITUDINAL ESTIMATES OF THE RETURN TO LANGUAGE KNOWLEDGE
by Immigrant Status

	Hebrew	English Very Well
Immigrants		
All	0.07 (0.01)	0.08 (0.03)
High Education	0.08 (0.02)	0.11 (0.03)
Low Education	0.03 (0.02)	.001 (0.04)
Natives		
All	*	0.12 (0.02)
High Education	*	0.13 (0.03)
Low Education	*	0.06 (0.04)

Additional controls: Experience, male, education, married, seniority