LANGUAGE AND THE EARNINGS OF IMMIGRANTS

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Several studies, most of them employing straightforward regression analysis, have concluded that immigrants' proficiency in the language of their adopted country is correlated with their productivity, as measured by earnings. Two weaknesses of these studies are potential unobserved heterogeneity, which could result in *over*-estimated effects if overall ability is linked with language acquisition, and potential measurement error in the language proficiency measures, which would probably result in *under*-estimation of language effects. The present study, which uses panel data for a 10-year period in Germany, yields evidence that the latter bias tends to be much larger than the former, implying that language proficiency is far more important than suggested by the existing literature.

Immigrants' ability to communicate with members of the indigenous population is probably the most important single *alterable* factor contributing to their social and economic integration. It is therefore no surprise that language proficiency is a subject prominently featured in any political discussion that touches on immigration issues, or issues of integration of foreign-born minorities. In Europe, declining populations and excess demand for workers in various labor market segments have brought migration, once again, to the forefront of the political discussion. Proposals for a comprehensive framework of migration legislation such as has long been established in the New World have led to deep disagreements between political parties. Those who demand that immigration policies be closely linked to measures of integration cite language education as the most important of those measures. Countries that have long and extensive experience with immigration, like the United States, Canada, and Australia, view language proficiency as an important consideration in evaluating applications for citizenship.

One reason language, in this context, is interesting from a political perspective is that stimulating language learning by offering language programs and similar measures is a relatively easy and straightforward general policy. Prior to any initiative to institute such policies, however, there must be a thorough assessment of the importance of language proficiency, and one question relevant to that issue is how strongly language proficiency influences

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economic assimilation. If good knowledge of the dominant language increases productivity by a sufficient amount, governments may find it advantageous to provide an appropriate infrastructure for language acquisition to support this process, and to encourage the immigrant population to learn the dominant language. It is therefore important to obtain an accurate estimate of the effect of language on earnings.

Economists have, in fact, extensively studied the relationship between language proficiency and immigrants' economic success.[1] It is hardly surprising that most studies conclude that migrants who are fluent in the dominant language earn higher wages. However, there is strong variation among the estimates of the size of the effect. Some authors (for instance, Borjas 1994) have argued that ordinary least squares (OLS) estimates of the effect of language on earnings may be upwardly biased, since the same unobserved heterogeneity may affect both wages and language proficiency. If individuals with higher overall ability are more likely to accumulate more language capital, then the effect of language may be much smaller than OLS estimates would suggest. Chiswick and Miller (1995) addressed this criticism by instrumenting language fluency. Their instruments were family composition variables and a variable indicating minority language concentration in the locality. Their results varied across specifications and countries and were quite imprecise, probably due to the low power of their instruments. In most of their estimates, the sign of the OLS bias of the language ability variable was negative, which is opposite what the unobserved heterogeneity argument would predict.

Unobserved heterogeneity is not the only potential source of bias for the OLS estimate of the effect of language on earnings. Language proficiency measures used in this literature are almost exclusively based on self-reported evaluations, and are therefore likely to suffer from measurement errors. This would lead to downward-biased OLS estimates.

In this paper, we address the issue of endogeneity of language fluency in earnings equations. We use panel data for Germany, including seven waves with self-reported information on immigrants' language proficiency over a decade. Our data contain a broader set of variables than has typically been available for previous studies. The time dimension, which is missing from previously published studies (which have relied on cross-section data), provides a number of instruments that can be used to deal with the problem of random measurement error, like leads and lags of self-reported language proficiency. To address the unobserved heterogeneity issue, we employ two strategies. First, we use a matching type estimator by including information on the partner and household composition. Second, we use parental education variables as instruments. Although this type of instrument has been criticized in the general context of the effects of education on earnings, we argue that in the case of immigrants' language proficiency, this criticism will be less relevant.

Most studies in this field consider men only, and there are a number of reasons for this. The mechanisms that govern women's language acquisition may be more complex than those governing men's, because women's human capital investment may be determined in conjunction with fertility decisions and past and future labor supply decisions. Also, women may often have entered the host country as dependents, rather than by their own choice, and their human capital acquisition may be conditional on their husbands' decisions. In addition, in earnings equations for women, non-random participation may lead to biased coefficient estimates in OLS regressions. It is beyond the scope of this paper to address all these issues, and most of the discussion that follows refers to the male sample. Still, women are an important part of the labor force, and estimates of their

[1]See, for instance, Carliner (1981), McManus et al. (1983), Grenier (1984), Kossoudji (1988), Rivera-Batiz (1990), Chiswick (1991), Dustmann (1994), and Chiswick and Miller (1995).

language effects should yield insights, despite these problems. We present the results for women using the same model specifications as for men.

Background and Data

After the Second World War, Europe experienced four major migration waves. Movements in the years between 1945 and 1960 were predominantly caused by the war's after-effects on Europe. In these years, a total of some 20 million people were displaced, mainly from East Germany.

The second migration movement, which began in the mid-1950s and thus partly overlapped with the first movement, was predominantly economically motivated. Starting in the mid-1950s, the strong economic development in northern Europe and the resulting demand for labor led to a large inflow of migrants, mainly from the periphery countries of Europe, but also from Turkey, North Africa, South America, and Asia. The main receiving countries were Belgium, France, Germany, the Netherlands, Switzerland, and the Scandinavian countries. This second large movement came to a halt in 1973–74, the turning point of the rapid economic development in northern Europe, when countries withdrew active recruitment policies or put severe restrictions on further labor immigration (or both).

The third wave of migration, which began after 1973, was characterized by family immigration and reunification of former labor migrants and, as a result of increasing separatist movements and civil wars in many Asian, South American, and African countries and rising inequality, by asylum migration.

Finally, the last big movements were initiated in the late 1980s by a liberalization of Soviet policy, and accelerated by the fall of the Berlin Wall in 1989. These events led to a substantial East-West migration. At the same time, the end of the Soviet empire destabilized the Balkan region, which led to large refugee migrations into other countries in Europe, including Germany.

The migrant population that we analyze in this paper stems from the second and third of these movements. The West German economy experienced a strong upward swing after 1955, accompanied by a sharp fall in the unemployment rate (between 1955 and 1960, the unemployment rate fell from 5.6% to 1.3%) and an increase in labor demand. This gave rise to a large migration of workers from southern European countries and Turkey into Germany. The percentage of foreign-born workers employed in West Germany increased from 0.6% in 1957 to 5.5% in 1965 and 11.2% in 1973, and declined thereafter. The stock of the foreign population increased from 700,000 in 1961 to 3,960,000 in 1973. Bilateral recruitment agreements between Germany and Italy, Spain, Greece, Turkey, Portugal, and Yugoslavia in the 1950s and 1960s considerably reduced the migrants' cost of migration: workers entered Germany with a one-year working contract, they could not be dismissed during the first year, travel costs were reimbursed, and employers had to provide accommodation (Mehrländer 1980:82). Trade unions, fearing that cheap foreign labor would bid down wages, initiated agreements that granted migrants the same labor market rights and the same claims on social benefits as Germans (see Lohrmann and Manfrass 1974:112).

In 1973, reacting to the first oil crisis, which marked the end of the German economy's strong upswing, active recruitment of foreign labor came to a standstill. After 1973, many immigrant workers brought families and dependents into the country.

Labor migration over this period was initially considered *temporary* by both the immigration countries and the emigration countries. Individuals were not expected to settle permanently. The German recruitment policy was based on the assumption that foreign workers would, after some years, return to their home countries. Boehning (1987) estimated that more than two-thirds of the foreign workers admitted to Germany starting in the mid-1950s had returned to their home countries. Still, although return migration has been quite

Table 1. Speaking Fluency (Percentages), Various Years.

Spoken language	*1984*	*1985*	*1986*	*1987*	*1989*	*1991*	*1993*
			Men				
Very Poor	1.49	1.24	1.00	0.79	1.24	1.59	1.00
Poor	14.69	14.77	14.40	13.13	12.18	11.19	11.90
Intermediate	36.76	33.26	35.64	35.77	32.98	33.30	30.40
Good	32.86	36.40	35.80	37.66	38.31	35.91	37.00
Very Good	14.20	14.33	13.16	12.66	15.29	18.00	19.70
Sum	100.00	100.00	100.00	100.00	100.00	100.00	100.00
No. Obs.	1,613	1,368	1,299	1,272	1,125	1,072	1,000
			Women				
Very Poor	8.18	5.94	5.83	6.32	6.75	5.84	5.74
Poor	22.77	24.39	25.91	22.68	22.80	22.71	20.96
Intermediate	33.71	34.65	31.84	33.59	28.83	28.22	28.92
Good	23.44	23.22	24.09	25.93	27.71	26.68	27.63
Very Good	11.90	11.79	12.33	11.48	13.91	16.54	16.74
Sum	100.00	100.00	100.00	100.00	100.00	100.00	100.00
No. Obs.	1,344	1,111	1,046	1,045	978	907	854

considerable (see Dustmann 1996 for details), a growing number of the foreign workers settled more permanently.

The data we use for the present analysis refer to West German immigrants from the former recruitment countries Italy, Spain, Turkey, Yugoslavia, and Greece. This information is drawn from the German Socio-Economic Panel (GSOEP), which started in 1984,[2] and contains an over-sample of migrants from these origin countries. It is this subsample that we use for our analysis. The GSOEP is, to our knowledge, the only household panel that over-samples immigrants and provides a sufficient database for statistical analyses of these minorities. In the first wave, the sample includes some 1,500 households with a foreign-born head. Foreign-born individuals are asked a number of specific questions regarding their economic behavior and their economic and social integration.

The survey questions are asked in the immigrants' home country language. In the years 1984–1987, 1989, 1991, and 1993, questions were included regarding language proficiency. Language information was not reported in 1988, 1990, and 1992.

We use the questions on speaking fluency, which is reported on a five-point scale: "Spoken German very poor" (1), "Spoken German poor" (2), "Spoken German intermediate" (3), "Spoken German good" (4), and "Spoken German very good" (5). Table 1 reports the response to the language questions for the various years. The majority of the male sample reported good or intermediate knowledge; the fraction of individuals who reported very poor knowledge was around 1% in most years. Women tended to be less fluent than men. For both men and women, the frequency distributions in the sample reveal a clear improvement trend over time.

In Table 2, the cross-tabulations of self-reported speaking fluency are presented for consecutive years for the male sample.[3] Language questions were included in every survey from 1984 until 1987, and the numbers in the table refer to these years. Row entries refer to the previous year, and column entries to the current year. The num-

[2]See Wagner et al. (1993) for information on the GSOEP.

[3]Cross-tabulations for the female sample are very similar.

Table 2. Cross-Tabulations, Speaking Fluency, Men.

t – 1/t	Very Poor		Poor		Intermediate		Good		Very Good		Total
Very Poor	**12**	**(26.67)**	21	(46.67)	8	(17.78)	3	(6.67)	1	(2.22)	46
Poor	16	(3.05)	**281**	**(53.63)**	190	(36.26)	35	(6.68)	2	(0.38)	524
Intermediate	4	(0.31)	186	(14.19)	**746**	**(56.90)**	343	(26.16)	32	(2.44)	1,311
Good	2	(0.15)	37	(2.86)	321	(24.79)	**787**	**(60.77)**	148	(11.43)	1,295
Very Good	1	(0.19)	3	(0.57)	34	(6.51)	186	(35.63)	**298**	**(57.09)**	522
Total	35	(0.95)	528	(14.28)	1,299	(35.14)	1,354	(36.62)	481	(13.01)	3,697

Numbers refer to 1984–1987. Column entries: previous year. Row entries: current year. Numbers in parentheses: Transition Probabilities, year $t-1$ to year t.

bers in parentheses are the transition probabilities from year $t-1$ to year t.

The numbers in the table are rather surprising. One might expect most entries to lie on the main diagonal, indicating the same proficiency in two consecutive years, or above the main diagonal, indicating an improvement. Instead, entries seem to be symmetrically distributed around the main diagonal, with a large number of transitions from good to intermediate, or from intermediate to poor or very poor, and so on. For instance, of those who report "good" in one year, 25% report "intermediate" and 2.8% report "poor" in the next year. Although some deterioration of speaking fluency is in principle possible, the large number of below-diagonal observations strongly suggests that the classification has some random element. It is likely that this type of time-variant misclassification error is also present in cross-sectional data on self-reported language proficiency used for the analyses in the United States, Australia, and Canada mentioned before.

Table 2 only indicates misclassification that varies over time. People may also persistently over- or under-report their language ability. This individual-specific and time-constant measurement error does not show in our cross-tabulations, but has similar implications for the parameter estimates, as will be shown below.

The language variable available in our data measures self-assessed speaking proficiency on a scale from one to five. This is slightly different from the usual U.S., Canadian, or Australian (mostly census-based) language information, where the scale distinguishes between four categories only. This information is usually reduced to a dummy variable, coded 1 if language proficiency is reported as good or very good. To make our analysis comparable, we use a similar classification—the language indicator used in the estimations is 1 if spoken host country language is reported to be good or very good, and 0 otherwise. Thus the reference category is everybody whose response is "intermediate," "poor," or "very poor."[4]

The means of this binary language indicator in all waves, together with the means and standard deviations of the earnings variable and the numbers of observations, are reported in Table 3. It is again apparent that language proficiency gradually improves over time, and that men are more likely than women to be proficient in the host country language.

For wave 1 (1984), our subsample of men contains 1,613 respondents who provided information on self-assessed language ability. Of these, 1,174 had a full-time job at the time of the interview, and provided earnings information. Due to attrition, the panel is unbalanced. Between waves 1 and 2, we lose about 15% of our sample observations on language, and 17% on earnings. For the next waves, attrition reduces each

[4]We have estimated all models using the five scale categorization provided in the original data. The qualitative conclusions are very similar.

Table 3. Language and Earnings.

	Language		Log Earnings		
Year	*Mean*	*No. Obs*	*Mean*	*Std. Dev.*	*No. Obs.*
			Men		
1984	**0.47**	1,613	**7.870**	0.291	1,174
1985	**0.50**	1,368	**7.833**	0.347	976
1986	**0.48**	1,299	**7.896**	0.288	948
1987	**0.50**	1,272	**7.910**	0.300	909
1989	**0.53**	1,125	**7.967**	0.26i	823
1991	**0.53**	1,072	**7.988**	0.267	750
1993	**0.56**	1,000	**8.011**	0.264	702
All	**0.51**	8,749	**7.917**	0.298	6,282
			Women		
1984	**0.35**	1,344	**7.513**	0.309	481
1985	**0.35**	1,111	**7.528**	0.349	399
1986	**0.36**	1,046	**7.559**	0.345	362
1987	**0.37**	1,045	**7.596**	0.303	362
1989	**0.41**	978	**7.625**	0.285	338
1991	**0.43**	907	**7.651**	0.291	325
1993	**0.44**	854	**7.701**	0.281	290
All	**0.39**	8,041	**7.587**	0.318	2,557

Language: 1 if speaking fluency is good or very good, 0 otherwise.

Log earnings: real German marks per month, full-time employees only.

sample by an average of about 5%. Mean earnings for women were lower than for men. Men's and women's attrition rates were very similar.

Attrition may partly be due to return migration. This adds to the normal attrition problem in household surveys, and could be a source of bias. For example, conditional on speaking fluency and background variables, the migrants who have the highest wages might be expected to have the lowest probability of return migration. Also, the migrants who do not intend to return will have invested more in country-specific human capital and thus will have higher earnings, better speaking fluency, or both.[5]

Without aiming at a detailed analysis of return migration and attrition, we investigate the issue of selective attrition for the time period covered by our panel in a straightforward way. The data contain information about the reasons individuals leave the panel. One reason is "moved abroad." We have used this to define a dummy variable for return migration for all the respondents in the 1984 sample, with a value of 1 if the respondent left the panel to move abroad between 1984 and 1993, and a value of 0 otherwise. This "return migration" variable is 1 for 20.6% of men and 20.4% of women in the 1984 sample. Using the sample of those who worked in 1984, we have estimated a probit model explaining return migration as a function of the log wage, reported speaking fluency, and the personal characteristics age, marital status, country of origin, and years of residence (all measured in 1984).

The estimated coefficients on log wages are positive but not statistically significant (t-value = 0.0 for men and 0.9 for women). The coefficient on speaking fluency is negative but also insignificant (t-value = –0.6 for both men and women).[6] These results suggest that remaining migrants are not positively selected from the overall population of immigrants in 1984. However, as explained above, the specific sample we analyze consists of immigrants who typically came to Germany a number of years before 1984, and thus our data do not allow us to check whether selective return migration took place in the early years after migration, that is, before the start of the panel. In the final section, we will therefore briefly discuss the possible implications for our results of selective attrition due to return migration.

In our earnings regressions, the dependent variable is the natural logarithm of gross monthly earnings. We restrict our analysis to individuals who report them-

[5]See Dustmann (1999) for a detailed analysis of the effects of return intentions on human capital investments.

[6]Age is statistically significant and positive for both genders. Years since migration has a negative effect, and is statistically significant for men only. Being married is also negative and statistically significant for men, but it is positive and statistically insignificant for women.

selves to be full-time employees during the month to which the earnings information refers. For the male sample, of those who report language information, 79% are full-time employees, 3.2% are part-time employees, 7.8% are unemployed, and 9.7% are labor market non-participants. In the sample of women, 38.2% are full-time employees, 7.79% are part-time employees, 6.4% are unemployed, and 44% are labor market non-participants. The remaining individuals are either on irregular jobs or enrolled in educational programs.

As explanatory variables, we build on the standard regressors in these models. We include the years since migration, potential experience and its square, years of education, marital status, and dummy variables indicating the immigrants' country of origin (Turkey, Yugoslavia, Greece, Italy, or Spain). Exact definitions and summary statistics of all the variables for the first year and last year of our observation window can be found in Appendix Table A1.

Our sample has a structure similar to that of the PSID: new individuals enter the GSOEP only if they join households already included in the data set. We only consider individuals who were not born in Germany. Therefore, young members of immigrant households who were born in Germany are excluded from our analysis. Only individuals born outside Germany who join households already included in our data enter our sample. As a result, the sample composition changes over time. This is reflected by the numbers in Table A1. In 1984, the average time the male immigrants in our sample had spent in Germany was about 14.5 years; in 1993, this number increased to 21.3 years. Over the same period, the average age increased by about four years.

In Table 4 we report the number of periods in which individuals are observed in our sample. The percentage of individuals with repeated information is quite high; only 17% of men reported language information in only one wave, and about 36% reported language information in each of the seven waves. The respective numbers for full-time employees who reported earnings are 21.8% and 17.9%. The numbers for women are similar for the language variable, but, due to larger numbers of non-workers, the percentages on repeated information on earnings are smaller.

Table 4. Frequency of Panel Participation.

	Language		*Earnings*	
No. of Waves	*No. Obs.*	*%*	*No. Obs.*	*%*
	Men			
1	331	17.19	354	21.76
2	212	11.01	248	15.24
3	161	8.36	175	10.76
4	209	10.86	193	11.86
5	142	7.38	174	10.69
6	185	9.61	192	11.80
7	685	35.58	291	17.89
Sum	1,925	100.00	1,627	100.00
	Women			
1	324	19.48	241	30.54
2	183	11.00	138	17.49
3	161	9.68	104	13.18
4	161	9.68	92	11.66
5	115	6.92	61	7.73
6	155	9.32	65	8.24
7	564	33.91	88	11.15
Sum	1,663	100.00	789	100.00

Language Proficiency and Earnings

Most existing studies of the effect of language proficiency on earnings are based on ordinary least squares (OLS) estimation. We consider three possible sources of bias in the OLS estimate of this effect. First, language proficiency may be affected by the same unobserved individual-specific heterogeneity as is earnings, which could result in over-estimation of the effect of language on earnings if OLS is used (Borjas 1994).

Second, unsystematic measurement errors that are independent over time may lead to downwardly biased estimates of the effect of language on earnings. The numbers in Table 2 suggest that this type of measurement error can be substantial in self-reported language measures. Following the standard argument in linear models with measurement errors on the independent variables,[7] we expect measurement error on the language variable to bias the estimated effect of language toward zero.

Third, there can be measurement error that is time-persistent and is not detected by the cross-tabulations in Table 2. This type of error can typically occur in data with variables based on subjective standards. Some persons may be inherently modest or boastful, or simply unable to evaluate their own capacities. This error can also be interpreted as a special type of individual heterogeneity. Under the standard assumption that this error in the regressor is independent of the unobserved component of the wage, the OLS bias induced by this error would again be negative.[8]

We attempt to address these three sources of bias. For all specifications, we estimate earnings equations separately for each year and—to improve the efficiency of the estimates and permit a parsimonious presentation of the main results—then combine the results using minimum distance estimation (see Chamberlain 1984). This takes account of the correlation structure of the error terms over time. We use the optimal weighting matrix, obtained from the first step estimates. In all estimations we allow for time dummies, that is, for different constant terms for the various years. We consider a log earnings equation of the form

$$w_{it} = x_{it}'\beta + \gamma l_{it} + \alpha_i + v_{it}, \tag{1}$$

where w_{it} denotes log earnings, x_{it} are time-constant and possibly time-variant (exogenous) variables, l_{it} is the "true" language proficiency of the individual, and i and t are indices for individuals and time, respectively. The error term α_i is unobserved individual heterogeneity, while v_{it} is an idiosyncratic error term.

In the general model, we do not observe l_{it}, but only $\tilde{l}_{it}$ (in our case, 1 if speaking fluency is good or very good, 0 otherwise). We assume that $\tilde{l}_{it}$ relates to l_{it} as follows:[9]

$$\tilde{l}_{it} = l_{it} + \eta_{it} + \xi_i, \tag{2}$$

where η_{it} denotes (unsystematic) measurement errors that are independent over time and ξ_i denotes the time-persistent measurement error component. Substituting (2) in (1) gives

$$w_{it} = x_{it}'\beta + \gamma\tilde{l}_{it} + \alpha_i - \gamma\xi_i + v_{it} - \gamma\eta_{it}. \tag{3}$$

Assumptions on the four errors α_i, ξ_i, v_{it}, and η_{it} determine the nature of the model and the properties of its estimators. Throughout, we shall assume that the idiosyncratic errors v_{it} are uncorrelated with the x_{it}, and that the measurement errors ξ_i and η_{it} are uncorrelated with x_{it} and l_{it}. The latter assumption implies that measurement error does not change with language proficiency or other characteristics in a systematic manner. This excludes, for instance, the possibility that measurement error is reduced when language proficiency improves. Since the survey is not in German but in the immigrant's first language, this assumption does not seem too unrealistic. We also assume that the α_i are uncorrelated with x_{it}.

OLS on equation (3) leads to inconsistent estimates of γ if $E(\alpha_i + v_{it} - \gamma(\eta_{it} + \xi_i)|\tilde{l}_{it}) \neq 0$. This will generally be the case if there is time-varying or time-persistent measurement error (which leads to a correlation between η_{it} and ξ_i with $\tilde{l}_{it}$), or if l_{it} and the individual-specific heterogeneity α_i are correlated.

It is instructive to develop the expression for the asymptotic OLS bias. For convenience, assume that α_i, ξ_i, η_{it}, and v_{it} are independent of one another and of the x_{it}

[7]See, for example, Verbeek (2000:520–22) for the simple case, or Judge et al. (1980:513–16) for the more general case.

[8]If boastful people also earn higher wages, the positive correlation between the measurement error and the wage equation could also lead to a positive bias, similar to the unobserved heterogeneity bias.

[9]In a companion paper (Dustmann and van Soest 2001), we focus on modeling speaking fluency, and consider more structural models distinguishing between measurement errors that are time-persistent and measurement errors that are independent over time.

and l_{it}. Denote the covariance between α_i and l_{it} by $\sigma_{l\alpha}$, and the variances of the time-constant and time-varying components of the measurement error by σ^2_ξ and σ^2_η. Let the variance of $\tilde{l}_{it}$ be $\sigma^2_{\tilde{l}}$, and denote the multiple correlation coefficient in a regression of $\tilde{l}$ on x by $R_{\tilde{l}x}$. The asymptotic bias of the OLS estimator is given by

$$\text{(4)} \qquad \operatorname{plim}(\hat{\gamma}_{\text{OLS}} - \gamma) = \frac{\sigma_{l\alpha} - \gamma(\sigma^2_\xi + \sigma^2_\eta)}{\sigma^2_{\tilde{l}}(1 - R^2_{\tilde{l}x})}$$

The first term in the numerator is the bias due to unobserved heterogeneity. It is positive if the same unobserved component affects earnings and language proficiency in the same direction. The second term is the asymptotic (downward) bias due to time-varying and time-persistent measurement error.

Identification

Our identification strategy has to deal with all three sources of bias. An *ad hoc* way to reduce the bias due to measurement errors that are independent over time would be to substitute the noise-ridden variable by time averages (see, for instance, Solon 1992; Zimmerman 1992). In our case this procedure needs to be adjusted, since language proficiency changes systematically over time and our panel is unbalanced. To take the trend into account, we estimate fixed effects models of language proficiency on a (non-linear) time trend, and compute predictions of l_{it} based on the time trend and the individual-specific effects. This procedure should reduce the bias due to measurement errors that are independent over time. It does not eliminate it completely, due to the small number of time periods. It does not correct for time-persistent measurement error or correlation between unobserved heterogeneity α_i and l_{it}.

Another way to deal with measurement errors that are independent over time is IV estimation. We have multiple observations on the same individual, and can therefore use leads and lags of self-reported language fluency as instruments for current language fluency. This should eliminate the bias component induced by measurement error, if measurement errors are independent over time. On the other hand, these instruments are correlated with the time-persistent component of measurement error (ξ_i). Moreover, if there is correlation between unobserved heterogeneity α_i and true fluency l_{it}, then such correlation will typically also exist between α_i and leads and lags of l_{it}, so these instruments do not eliminate the potential bias due to unobserved heterogeneity.

To deal with unobserved heterogeneity, we follow two strategies. First, we add partner variables and household characteristics to the earnings equation. If mating is assortative, then these variables should capture some of the unobserved individual heterogeneity; inclusion of these variables in the wage equation will reduce or take out the correlation between α_i and l_{it}, thus reducing the bias. The resulting OLS estimator of the wage equation can be interpreted as a matching estimator in the sense described by Rosenbaum and Rubin (1983). If, conditional on partner and household characteristics and the other regressors, l_{it} is uncorrelated with α_i, this procedure eliminates the bias due to unobserved heterogeneity.

To take account of unobserved heterogeneity as well as independent-over-time measurement error, we combine this matching approach with IV estimation. We include the partner variables and household characteristics in the wage equation, and use leads and lags of language information as instruments for speaking fluency. If unobserved heterogeneity and time-independent measurement error are the only sources of bias, this approach should reduce the bias to the extent to which our background variables explain the variation in the unobserved heterogeneity component.[10]

[10]The underlying assumption is that $\xi_i = 0$ and that the η_{it} are independent over time. Together with the matching assumption made above, this implies that the errors in equation (3) are independent of x_{it} and l_{is} for $s \neq t$.

The remaining problem is the bias due to the time-persistent component of measurement error, which is not removed by the procedure described above, since conditioning on the matching variables will probably not remove the correlation between the error component ξ_i and the language measure. We therefore use an alternative set of instruments that addresses all three sources of endogeneity bias simultaneously. This set of instruments is based on the education level of the immigrant's parents. Our data set is rather distinctive in this respect, since this type of information is usually not available for immigrants.

To qualify as an instrument, the parents' education should explain some of the (conditional) variation in the offspring's language ability. There are a number of reasons why this may be the case. It is likely that attitudes valuing language for its own sake are developed at young ages inside the family, and are strongly related to the parents' intellectual background. Children from families with higher educational background may be more likely to develop an interest in learning foreign languages. Furthermore, children of better-educated parents are also more likely to be exposed to a foreign language during their childhood—for instance, if parents speak a second language. We will show below that the father's education is indeed a reasonably strong predictor of the individual's language proficiency. For women, the situation is less clear: both the father's and the mother's educational level seem only weakly related to female immigrants' language proficiency.

Moreover, the validity of these instruments requires that they are uncorrelated with $\alpha_i + v_{it}$, ξ_i, and η_{it}. The assumption that parents' education is uncorrelated with both types of measurement error seems reasonable. That parents' education is uncorrelated with $\alpha_i + v_{it}$ implies that the father's or mother's education level should not have a direct effect on earnings, conditional on the other regressors. In the general context of wage equations, people have argued that such a direct effect does exist. An argument is that better-educated parents may have well-developed networks, which can help the offspring at the start of the career. While this may be a valid criticism for estimating wage equations in general, it does not apply in our case, since migration cuts links with parental networks. This is particularly the case for the migrant population we consider in our analysis, since no networks existed for them in Germany prior to their immigration, which started in the mid-1950s.

Another potential problem with this instrument is that there may be family-specific unobservable effects, transmitted between generations, that lead to a positive correlation between α_i and the education of the parent, thus invalidating the exclusion restriction. We condition, however, on the offspring's education, and this should absorb at least part of the variation in α_i, which could possibly be correlated with the parents' education. If, however, there is any variation left in parents' education levels that is not absorbed by the other regressors and correlated with α_i, then our IV estimates will still be upwardly biased.[11]

Results

The vector of explanatory variables x_{it} includes potential labor market experience and its square, education (in years), marital status, years since migration, and country of origin dummies. As explained above, the language indicator equals 1 if the individual reports that he or she speaks the language well or very well.

[11]Treating the α_i as fixed effects and eliminating them from equation (3) by differencing would solve the problem. In our case, however, fixed effects estimates appear to be uninformative, with very large standard errors, due to the substantial time-varying measurement error in our data. Any type of difference estimator will in this case aggravate the bias by increasing the noise-to-signal ratio for the measured language indicator (see Hsiao 1986). Although, in principle, instrumental variables can be used to allow for this, the available instruments appear to be very weakly correlated with the regressors, leading to inaccurate estimates. We experimented with several specifications, but without success.

Table 5. Earnings Equation Estimates, Men.

	1 *OLS*		*2* *Average Dsp.*		*3* *Partner*	
Variable	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*
Language	**0.0503**	0.0076	**0.0926**	0.0112	**0.0473**	0.0085
YSM	**0.0047**	0.0008	**0.0039**	0.0009	**0.0041**	0.0010
Exp.	**0.0226**	0.0015	**0.0239**	0.0015	**0.0205**	0.0017
Exp.^{2}/100	**−0.0425**	0.0028	**−0.0440**	0.0028	**−0.0339**	0.0032
Education	**0.0183**	0.0018	**0.0179**	0.0018	**0.0172**	0.0021
Married	**0.1050**	0.0116	**0.1057**	0.0116	**0.2026**	0.0401
No. Obs.	**6,033**		**6,033**		**5,001**	
$\Delta\chi^2$ *(d.o.f.)*	2.09 (6)		3.58 (6)		6.75 (6)	
	4 *Leads + Lags*		*5* *Leads + Lags, Partner*		*6* *Father's Education*	
Variable	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*
Language	**0.1391**	0.0175	**0.1123**	0.0195	**0.1412**	0.0673
YSM	**0.0010**	0.0011	**0.0019**	0.0013	**0.0026**	0.0020
Exp.	**0.0233**	0.0020	**0.0199**	0.0023	**0.0267**	0.0028
Exp.^{2}/100	**−0.0405**	0.0036	**−0.0307**	0.0041	**−0.0469**	0.0040
Education	**0.0160**	0.0022	**0.0151**	0.0025	**0.0180**	0.0027
Married	**0.0965**	0.0154	**0.2222**	0.0482	**0.1071**	0.0156
No. Obs.	**4,272**		**3,647**		**4,142**	
$\Delta\chi^2$ *(d.o.f.)*	5.95 (6)		9 (6)		10.85 (4)	

All specifications allow for different constant terms in every year, and include country of origin dummies. Specifications (3) and (5) include partner variables and household characteristics. $\Delta\chi^2$ *(d.o.f.)* is the value of the test statistic for the restrictions imposed in minimum distance, where d.o.f. denotes the degrees of freedom. It follows a $\chi^2_{d.o.f.}$ distribution under the null 5% critical value for d.o.f. = 6: 12.56; d.o.f. = 4: 9.48.

Column 1: OLS. Column 2: OLS, using adjusted time averages for language variable. Column 3: OLS, matching on household and partner characteristics. Column 4: IV, using leads and lags of language as instruments. Column 5: IV, using leads and lags of language as instruments, matching on household and partner characteristics. Column 6: IV, using father's education as instruments.

We first discuss estimates for the sample of men, reported in Table 5. We present the main results for the various specifications; estimates of the time dummies and country of origin dummies are not displayed. We have tested the restriction that the language effects are the same for each year, using a χ^2 test statistic obtained from the criterion functions.[12] The test statistic is reported in the last row of Table 5. The null of equal parameters on the language variable in all years cannot be rejected at the 5% level for the first five specifications.

In Table 5, we present only the minimum distance estimates. The estimates for the coefficients of the language variable for the single years are given in Appendix Table A2. We first estimate OLS regressions, following the standard approach in this literature. Results are reported in the first column of the table. Language has a positive effect on earnings, and the coefficient estimate indicates that a good command of the host country language is associated with an earnings advantage of about 5%. This is smaller than the estimates for Australia, Canada, Israel, and the United States given by Chiswick and

[12]The difference in the minimizing criterion functions for the Minimum Distance estimators, with and without restrictions imposed, is asymptotically χ^2 distributed under the null, with number of degrees of freedom equal to the number of additional restrictions. See Chamberlain (1984) for details.

Miller (1995:276).[13] For the various years, point estimates range between 1.8 and 7.5 (Table A2). As explained above, however, our classification is somewhat different: our reference category includes individuals who reported intermediate knowledge. Standard errors are small and the effects are statistically significant for all but one year.

In the second column, we follow the *ad hoc* procedure to remove independent-over-time measurement error, generalizing the approach of Solon (1992) and Zimmerman (1992) (see above). This leads to a substantial increase in the estimated coefficient on speaking fluency. Compared to column (1), the coefficient nearly doubles, with the standard errors slightly increasing. This suggests that time-varying measurement errors lead to substantial downward bias of the language coefficient in the OLS estimates of column (1).

In column (3), we present OLS results controlling for partner and family background variables. We add partner's proficiency in the host country language and in the written home country language (on a five-point scale), partner's years of residence, partner's education, and partner's age. We further add variables measuring the household size and the number of children in the household. As pointed out above, including these variables should reduce the upward bias of the language coefficient in column (1) that is due to unobserved heterogeneity. The results point in this direction. However, the effect is modest: compared to column (1), the effect of language on earnings decreases by less than half a percentage point, with a slightly larger standard error.

In column (4) we present results of IV estimations that use leads and lags of speaking fluency as instruments.[14] This procedure should entirely remove the bias induced by the independent-over-time measurement error. In fact, the results show that coefficients are considerably larger than in the previous set of results—the effect of language on earnings has increased by a factor of almost 3. The increase is much larger still than the one obtained with the *ad hoc* procedure in column (2). The standard error of the estimate has also increased.

The estimator used for column (5) uses the same instruments as in column (4), and, in addition, includes among the regressors partner information and household background characteristics. The coefficient on speaking fluency falls in size compared to column (4). The explanation is that the positive individual heterogeneity bias is reduced. It is still far above the estimates in the OLS regression.

The estimates in column (5) will still be biased if conditioning on partner information does not entirely remove the correlation between unobserved heterogeneity and language, or if there is time-persistent measurement error in speaking fluency. The first may lead to an upward bias, the second to a downward bias of the coefficient γ. In column (6), we use alternative instruments, based on the father's educational achievement.

The father's education is measured by a vector of five dummy variables, which are explained in Appendix Table A1. It is based on survey questions in the third wave of the panel. For individuals who enter the panel after the third wave, or leave the panel before the third wave, this information can be constructed only if the father lives in the same household. We reconstructed the father's education level for the two waves before and after the third wave without losing too many individuals. After

[13]Chiswick and Miller (1995) reported that English language fluency was associated with 8.3% higher earnings in Australia (based on the 1986 census), 16.9% higher earnings in the United States, 12.2% higher earnings in Canada, and 11.0% higher earnings in Israel.

[14]In the reduced form (first stage) linear probability regression of language on the instruments and other regressors, the set of instruments is statistically significant at the 1% level for all years. Instruments are language information in all previous and future waves.

Table.6. Language and Earnings.

Specification	*Sample Using All Observations*			*Common Sample*			
	Coeff.	*Std. Error*	*No. Obs.*	*Coeff.*	*Std. Error*	*No. Obs.*	*Test*
OLS	**0.0503**	0.0076	6,033	**0.0383**	0.0110	2,882	1.51
OLS, Averages	**0.0926**	0.0112	6,033	**0.0866**	0.0166	2,882	0.49
OLS, Partner	**0.0473**	0.0085	5,001	**0.0386**	0.0113	2,882	0.61
IV, Leads + Lags	**0.1391**	0.0175	4,272	**0.1216**	0.0212	2,882	1.46
IV, Partner, Leads + Lags	**0.1123**	0.0195	3,647	**0.1239**	0.0226	2,882	1.02
IV, Father's Education	**0.1412**	0.0673	4,142	**0.1018**	0.0733	2,882	1.36

Test: Hausman test t-statistic comparing estimates of speaking fluency coefficient for the two samples.

wave 5, we lose many observations, and we therefore used the first five waves only.[15]

Column (6) presents the results. The point estimate of the speaking fluency effect is 0.141, only slightly larger than the previous IV estimate, and with a much larger standard error.[16] This suggests that the bias due to time-persistent measurement error (present in column 5 but not in column 6) is quite small. On the other hand, as explained above (in the section "Language Proficiency and Earnings"), this result should be interpreted with some care, since it relies on the validity of the father's education level dummies as instruments.[17]

Most estimates of the coefficients of the other variables are quite similar to those found in other studies. Most interesting is the coefficient on the years of residence in the host country variable (YSM, years since migration). It decreases considerably in the IV estimates (columns 4–6). Years of residence is a strong predictor of language fluency. In OLS regressions, the downward bias in the language variable coefficient thus leads to an upward bias in the years of residence effect.[18] Correcting for this considerably reduces the size of the coefficient on the years of residence variable. In all IV estimates, the effect on earnings of years of residence is small (about 0.1—0.2% per year) and not significantly different from zero. This suggests that OLS estimation of earnings regressions, conditional on language fluency, leads to an overestimate of the effect of the residence variable, due to measurement error in the language variable. A comparison with the IV estimates shows that the effect that is attributed by OLS to years of residence should instead be attributed to language proficiency. These results are consistent with findings by Charette and Meng (1994), who used subjective and objective language measures in earnings regressions; they found that the effect of years of residence decreased substantially when they used objective language proficiency measures.

One reason for the different results obtained across specifications is variation in

[15]For about 10% of the remaining sample of respondents on whose father some information is available, the father's education level is missing. These observations are retained in the sample used for the IV estimates, and a dummy indicating that the information is missing is included among the instruments (see Table A1).

[16]In the reduced form (first stage) linear probability regression, the set of instruments is statistically significant at the 5% level for three years, and at the 10% level for the remaining two years.

[17]An interesting extension of the model in equation (1) is to allow for heterogeneous returns to speaking fluency, where γ is replaced by $\gamma_i = (\bar{\gamma} + \tilde{\gamma}_i)$. If $\tilde{\gamma}_i$ is correlated with l_{it} (which is the case if individuals' language investments depend on their individual-specific returns), IV estimation does not identify the average return to language fluency, $\bar{\gamma}$ (see Heckman 1997). We tested for this, and did not find any evidence for individual-specific returns to language capital that are correlated with language proficiency.

[18]See, for example, Judge et al. (1980:515) for exact expressions for the OLS bias on β.

the sample size, which is due to missing values in variables used in the different specifications. To investigate the robustness of our results, we estimated all models on the common set of observations that is available for all estimators. The coefficients on the language variable are displayed in the right-hand panel of Table 6. The left-hand panel replicates the estimates from Table 5. Using the common sample reduces the coefficient on the language variable in all specifications but one, but the main conclusions from Table 5 remain the same: using the average of reported language proficiency over all waves as a regressor, or performing IV with leads and lags of language proficiency as instruments, leads to an increase in the estimated effect of speaking fluency. IV estimation using father's education as instrument also leads to an increase in the estimate—it is about two and a half times as large as the OLS estimate. According to Hausman t-tests, estimates based on the balanced panel do not differ significantly from those based on the unbalanced panel (final column of Table 6).

We now turn to the earnings regressions for women, where we use the same estimators as for men. As explained in the introduction, we do not aim to solve the problems that may exist for the female sample, like non-random participation. Results should be evaluated keeping this in mind. The minimum distance estimates are displayed in Table 7. The estimates of the speaking fluency coefficients for the single years are reported in Appendix Table A2.

The magnitude of the language coefficient in the OLS regression is similar to that for men. The estimated effect of the speaking fluency dummy increases from 4.2 to 7.8 percentage points when we use adjusted time averages (column 2). Adding the partner variables (column 3) leads to parameter estimates that are slightly larger than OLS estimates.

Using leads and lags of language as instruments (column 4) results in a sharp increase of the language indicator, as for the male sample. When we add partner characteristics, the coefficient decreases, and the estimated effect of good speaking fluency on the wage falls from 14% (column 4) to below 10% (column 5). All these results are in line with those for men. Using father's education as instrument leads to imprecise estimates. The reason is that the father's education level has very little correlation with women's language proficiency. The same holds for mother's education level. In the table, we present the estimates using both parents' education levels. This gives a somewhat smaller standard error on the speaking fluency coefficient than using father's or mother's education only, but the estimate is still rather imprecise and insignificantly different from zero.[19]

Our results suggest that language proficiency is important not only for men, but also for women, with similar magnitudes in coefficient estimates. The results therefore support the findings for male immigrants, indicating that language proficiency may be more important for productivity than is suggested by simple OLS regressions.

Language proficiency, particularly of women, may have implications beyond matters directly related to the individual. The ability of the mother to communicate in the host country language may assist access to, as well as understanding of, institutions that are vital for the child's development. It may also relate directly to the offspring's own acquisition of proficiency in the host country language. These intergenerational aspects of language proficiency may significantly add to the value and benefit of language education of first-generation migrants. At present, we know little about these indirect effects. Future research on these issues could yield important insights.

Summary and Discussion

Economists have extensively studied the link between language proficiency and pro-

[19]In a regression of speaking fluency on the other regressors in the earnings equation and either the father's or the mother's educational dummies, the educational dummies are jointly significant at the 5% level in only one of the waves considered.

Table 7. Earnings Equation Estimates, Women.

	1 *OLS*		*2* *Average Dsp.*		*3* *Partner*	
Variable	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*
Language	**0.0416**	0.0141	**0.0778**	0.0196	**0.0521**	0.0154
YSM	**0.0057**	0.0015	**0.0049**	0.0015	**0.0058**	0.0018
Exp.	**0.0166**	0.0024	**0.0177**	0.0025	**0.0174**	0.0028
Exp.^{2}/100	**−0.0291**	0.0047	**−0.0296**	0.0047	**−0.0249**	0.0054
Education	**0.0272**	0.0033	**0.0278**	0.0033	**0.0300**	0.0037
Married	**0.0317**	0.0163	**0.0363**	0.0163	**0.1439**	0.0781
No. Obs.	**2,403**		**2,403**		**2,125**	
$\Delta\chi^2$ (d.o.f.)	2.55 (6)		3.80 (6)		3.62 (6)	
	4 *Leads + Lags*		*5* *Leads + Lags, Partner*		*6* *Parents' Education*	
Variable	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*	*Coeff.*	*Std. Error*
Language	**0.1410**	0.0302	**0.0968**	0.0308	**0.1199**	0.0806
YSM	**0.0047**	0.0018	**0.0043**	0.0021	**0.0027**	0.0028
Exp.	**0.0164**	0.0031	**0.0154**	0.0033	**0.0184**	0.0043
Exp.^{2}/100	**−0.0261**	0.0031	**−0.0208**	0.0061	**−0.0273**	0.0067
Education	**0.0260**	0.0037	**0.0287**	0.0040	**0.0303**	0.0043
Married	**0.0292**	0.0189	**0.1063**	0.0837	**0.0509**	0.0226
No. Obs.	**1,701**		**1,574**		**1,594**	
$\Delta\chi^2$ (d.o.f.)	13.49 (6)		14.15 (6)		12.81 (4)	

Notes: Same as notes to Table 5, except that Column 6 represents the results of IV estimation using father's *and mother's* education as instruments.

ductivity. However, there is speculation that the estimators they have used lead to biased coefficients. In this paper, we have focused on the problem of the endogeneity of the language proficiency variable. We use a panel data set on immigrants that provides more information than is typically available in this type of study. The raw data show that time-varying measurement errors can indeed be a serious problem in the type of data usually used for these estimations. In our model, we distinguish among three different types of endogeneity that are likely to affect parameter estimates: correlated unobserved heterogeneity in speaking fluency and earnings, time-varying measurement errors, and time-persistent measurement errors.

Our results indicate that time-varying measurement errors lead to a large downward bias on the effect of speaking fluency on earnings, which dominates the potential positive bias due to unobserved heterogeneity. The emerging picture suggests that the effect of language on earnings is underestimated in OLS regressions. We also find that the downward bias in the language variable leads to upward-biased estimates of the years of residence variable.

The differences in estimates lead to different conclusions about the value of language acquisition. Some back-of-the-envelope calculations illustrate this. Consider, as an example, a migrant, arriving from Turkey in 1984, who is not married, has 10 years of education, and has 6 years of labor market experience (the sample averages). If he speaks German on an intermediate level at best and works without interruption, over the next 10 years he accumulates earnings of 278,000 DM (in 1984 German Marks).[20] Had he been proficient in Ger-

[20]We use the OLS estimates for education, experience, and years of residence variables. We allow for wage growth, due to time effects, of 1.4% per year, which corresponds to the average over this period.

man from the start, this number would increase to 292,100 DM, based on a 5% return estimate for language, our OLS result. If we use our most optimistic estimate (based on IV estimation, using father's education as instrument), this number increases to 319,680 DM. Accordingly, language proficiency increases the stock of capital accumulated over a 10-year period by 14,100 DM using the OLS estimate, as opposed to 41,680 DM using the IV estimate.

Can these findings be generalized to other language studies, or are our results specific to the group of migrants we analyze? As explained above (under "Background and Data"), a particular feature of the immigrants in our sample is that their migration was intended to be temporary, and return migration was considerable. Moreover, the active recruitment of migrants ceased in 1973, 11 years before the first wave of the panel was collected. Both of these facts lead to specific features of our sample that may affect the study's results and, in turn, its implications. We will discuss both below.

Consider first return migration. In the section "Background and Data," we tested the assumption of selective return migration, using information about migrants returning to their home countries within our observation window. Our results do not provide any evidence for selective return migration: neither earnings nor speaking fluency is significantly associated with the probability of leaving Germany, given the other characteristics. These results do not, however, rule out the possibility that selective return migration took place before the start of the panel.

Thus the possibility of some selectivity effect still remains. For example, if migrants who remain in Germany are positively selected on their labor market performance (conditional on speaking fluency and other characteristics), this non-random selection may lead to downward-biased estimates of the speaking fluency effect. This, however, would imply that our estimates may be considered as a lower bound, and that estimates for migrant populations with a lower frequency of return migration may be even higher.

Whether such an effect might also be present in U.S. studies is also not obvious. There is evidence of considerable return migration in the United States, although smaller in magnitude than in Germany. Jasso and Rosenzweig (1982) analyzed a random sample of immigrants admitted to permanent residence status in the United States in 1971. They estimated that, depending on the country of origin, between 20% and 50% of immigrants emigrated from the United States. Warren and Kraley (1985) estimated that around 30% of immigrants to the United States leave the country within two decades after arrival. Borjas and Bratsberg (1996) presented some evidence that return migration from the United States is selective.

Second, a feature of our sample is that the peak of immigration for the migrant groups we consider was in 1973, 11 years before the first wave of the panel was collected. As described above, migration in the years after 1973 was considerably reduced for these groups. Average duration in 1984 (the first year of the panel) was 14.6 years for men and 13 years for women (see Table A1). Language proficiency may be accumulated mainly during the first years of residence. If the noise in the language variable does not change over time, this may imply that the signal-to-noise ratio in reported speaking fluency increases with average years of residence in the sample.

It is not obvious, however, that the signal-to-noise ratio will be larger in other samples used for this type of analysis. In the cross-section data used for other countries, average years of residence is often even larger, though the dispersion in years of residence is also larger. Chiswick and Miller (1992), using the 1980 U.S. Census of Population and the Canadian 1981 Census, reported average lengths of residence of 15.75 years and 19.45 years for men and women, respectively. Chiswick and Repetto (1999), using the 1972 Census of Israel, found that the average length of residence was 21.14 years. Chiswick and Miller (1995), using the 1981 Australian Census of Population and Housing, obtained a figure of 18.57 years. The sample standard deviations of years of residence in these samples vary from 9.8 to 12.0 years, considerably higher than the 5.7 and 5.8 in our sample.

These considerations suggest that the results we have obtained may well generalize to other data sets and other countries. Other studies on the effects of language on earnings relying on subjectively measured language proficiency indicators may well suffer from measurement error bias in a similar manner. Language may play a far more important role in enhancing immigrants' productivity than the conclusions from OLS regressions indicate.

Table A1
Descriptive Statistics

	Men						Women					
	1984			1993			1984			1993		
Indep. Variable	*Mean*	*Std. Dev.*	*No. Obs.*	*Mean*	*Std. Dev.*	*No. Obs.*	*Mean*	*Std. Dev.*	*No. Obs.*	*Mean*	*Std. Dev.*	*No. Obs.*
Age	**36.69**	10.92	554	**38.80**	11.51	386	**38.95**	11.03	1,182	**40.69**	11.96	698
Potential Experience	**21.54**	11.62	554	**23.26**	11.98	386	**23.13**	11.41	1,182	**24.66**	12.15	698
Years of Residence in Germany (YSM)	**13.93**	4.94	554	**20.45**	6.17	386	**14.64**	5.29	1,182	**20.83**	6.62	698
Education (in years)	**9.15**	2.00	554	**9.53**	1.98	386	**9.82**	2.04	1,182	**10.03**	2.02	698
Log Gross Monthly Earnings (in Real D-Marks)	**7.37**	0.47	554	**7.42**	0.60	386	**7.82**	0.38	1,182	**7.97**	0.33	698
Married	**0.77**	0.41	554	**0.77**	0.41	386	**0.84**	0.36	1,182	**0.86**	0.34	698
Turkey	**0.26**	0.44	554	**0.27**	0.44	386	**0.30**	0.46	1,182	**0.38**	0.48	698
Yugoslavia	**0.24**	0.43	554	**0.30**	0.46	386	**0.18**	0.39	1,182	**0.22**	0.41	698
Greece	**0.17**	0.38	554	**0.16**	0.37	386	**0.14**	0.35	1,182	**0.11**	0.32	698
Italy	**0.17**	0.37	554	**0.16**	0.37	386	**0.21**	0.41	1,182	**0.19**	0.39	698
Spain	**0.13**	0.34	554	**0.08**	0.27	386	**0.14**	0.35	1,182	**0.08**	0.27	698
No. Children in Household	**1.12**	1.17	410	**0.74**	0.97	198	**1.19**	1.24	792	**0.90**	1.08	306
Household Size	**3.82**	1.64	410	**3.57**	1.74	198	**3.74**	1.78	792	**3.90**	1.92	306
Partner Fluent in German	**0.37**	0.48	410	**0.47**	0.50	198	**0.28**	0.45	792	**0.40**	0.49	306
Years of Schooling, Partner	**9.77**	2.18	410	**9.92**	2.09	198	**8.89**	1.95	792	**9.08**	1.94	306
Proficiency Writing Home Country Language, Partner	**0.85**	0.34	410	**0.89**	0.30	198	**0.73**	0.43	792	**0.82**	0.38	306
Age, Partner	**42.94**	9.12	410	**48.72**	7.73	198	**37.53**	9.91	792	**43.70**	8.27	306
Years of Residence, Partner	**15.63**	4.98	410	**24.17**	4.51	198	**12.78**	5.78	792	**21.01**	5.08	306
Father No Education	**0.16**	0.37	397	**0.15**[a]	0.36	303	**0.22**	0.42	900	**0.22**[a]	0.41	561
Father Primary Education	**0.38**	0.48	397	**0.36**[a]	0.48	303	**0.33**	0.47	900	**0.32**[a]	0.46	561
Father Basic Education	**0.29**	0.45	397	**0.31**[a]	0.46	303	**0.29**	0.45	900	**0.29**[a]	0.45	561
Father Intermediate Education	**0.02**	0.16	397	**0.04**[a]	0.20	303	**0.03**	0.18	900	**0.03**[a]	0.18	561
Father Secondary Education	**0.00**	0.05	397	**0.00**[a]	0.08	303	**0.00**	0.08	900	**0.01**[a]	0.11	561
Father Education Missing	**0.12**	0.33	397	**0.11**[a]	0.32	303	**0.10**	0.30	900	**0.10**[a]	0.30	561
Mother No Education	**0.33**	0.47	388	**0.32**[a]	0.47	283	**0.35**	0.48	892	**0.38**[a]	0.48	638
Mother Primary Education	**0.33**	0.47	388	**0.32**[a]	0.47	283	**0.34**	0.47	892	**0.32**[a]	0.47	638
Mother Basic Education	**0.22**	0.42	388	**0.25**[a]	0.43	283	**0.20**	0.40	892	**0.20**[a]	0.40	638
Mother Intermediate Education	**0.01**	0.07	388	**0.01**[a]	0.12	283	**0.01**	0.11	892	**0.01**[a]	0.10	638
Mother Secondary Education	**0.00**	0.00	388	**0.00**[a]	0.00	283	**0.00**	0.03	892	**0.00**[a]	0.00	638
Mother Education Missing	**0.12**	0.32	388	**0.10**[a]	0.30	283	**0.10**	0.30	892	**0.09**[a]	0.29	638

Individual characteristics correspond to the sample used for OLS regressions. Family background and partner characteristics are reported for individuals where the partner is present, and correspond to the samples used for estimations that condition on family background. Parents' education corresponds to the samples used for IV regressions, using these variables as instruments.

[a]Numbers refer to 1989.

Table A2
Effect of Language on Earnings, Various Years

	Men						*Women*					
	OLS		*Average Dsp.*		*Partner*		*OLS*		*Average Dsp.*		*Partner*	
Year	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*
1984	**0.0470**	0.0172	**0.0749**	0.0256	**0.0491**	0.0182	**0.0557**	0.0343	**0.0177**	0.0447	**0.0748**	0.0349
1985	**0.0747**	0.0219	**0.1034**	0.0339	**0.0737**	0.0236	**0.0894**	0.0428	**0.0841**	0.0614	**0.0577**	0.0436
1986	**0.0575**	0.0195	**0.1355**	0.0293	**0.0640**	0.0218	**0.0429**	0.0427	**0.1058**	0.0586	**0.0632**	0.0474
1987	**0.0660**	0.0216	**0.1264**	0.0336	**0.0811**	0.0231	**0.0574**	0.0361	**0.0623**	0.0521	**0.0420**	0.0382
1989	**0.0181**	0.0197	**0.0699**	0.0296	**0.0164**	0.0229	**0.0037**	0.0351	**0.0625**	0.0489	**0.0267**	0.0415
1991	**0.0545**	0.0214	**0.0890**	0.0310	**0.0434**	0.0240	**0.0561**	0.0365	**0.1049**	0.0524	**0.0646**	0.0428
1993	**0.0447**	0.0219	**0.0928**	0.0301	**0.0008**	0.0294	**0.0345**	0.0386	**0.1537**	0.0522	**0.0821**	0.0482
MD	**0.0503**	0.0076	**0.0926**	0.0112	**0.0473**	0.0085	**0.0416**	0.0141	**0.0778**	0.0196	**0.0521**	0.0154
	Leads + Lags		*Leads + Lags, Partner*		*Father's Education*		*Leads + Lags*		*Leads + Lags, Partner*		*Parents' Education*	
Year	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*	*Coeff.*	*Std. E.*
1984	**0.1470**	0.0485	**0.1495**	0.0549	**0.0825**	0.1674	**0.1495**	0.0549	**0.0348**	0.0914	**0.0072**	0.6014
1985	**0.0966**	0.0482	**0.0640**	0.0528	**0.2666**	0.1958	**0.0640**	0.0528	**0.0099**	0.1318	**0.0783**	0.3804
1986	**0.1987**	0.0457	**0.1904**	0.0518	**0.2258**	0.1796	**0.1904**	0.0518	**0.0506**	0.0792	**0.1329**	0.2427
1987	**0.1303**	0.0500	**0.0960**	0.0521	**0.5544**	0.2534	**0.0960**	0.0521	**0.1254**	0.0952	**0.1422**	0.2281
1989	**0.1349**	0.0423	**0.1459**	0.0488	**0.2064**	0.1677	**0.1459**	0.0488	**0.1401**	0.0833	**0.2198**	0.3491
1991	**0.1138**	0.0436	**0.1138**	0.0510	—	—	**0.1138**	0.0510	**0.2677**	0.0889	—	—
1993	**0.1856**	0.0611	**0.1181**	0.0750	—	—	**0.1181**	0.0750	**0.3459**	0.0990	—	—
MD	**0.1391**	0.0175	**0.1123**	0.0195	**0.1412**	0.0673	**0.1123**	0.0195	**0.0968**	0.0308	**0.0753**	0.0988

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