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The Wage Penalty of Regional Accents

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Abstract

Previous work has documented that speaking one's native language with an accent distinct from the mainstream is associated with lower wages. In this study, we seek to estimate the causal effect of speaking with a distinctive regional accent, disentangling the effect of the accent from that of omitted variables. We collected data on workers' speech in Germany, a country with wide variation in regional dialects. We use a variety of strategies in estimation, including an instrumental variables strategy in which the instruments are based on research findings from the linguistics of accent acquisition. All of our estimators show that speaking with a distinctive regional accent reduces wages by an amount that is comparable to the gender wage gap. We also find that workers with distinctive regional accents tend to sort away from occupations that demand high levels of face-to-face contact, consistent with various occupational sorting models.

Keywords accent, dialect, wage penalty, discrimination, SOEP

JEL Classifications J24, J7

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1 Introduction

Does speaking with a regional accent reduce a worker's wage? Millions of workers worldwide speak a dialect, a variety of a language that may depart from the standard variety in a number of ways.[1] Results from cognitive neuroscience and sociolinguistics show that one's native accent, a central component of dialect, is largely acquired during childhood and is difficult to change thereafter. At the same time, studies from social psychology show that listeners draw strong conclusions from others' speech. The combination of strong preferences regarding others' accents, plus the high cost of changing them, could result in a wage penalty for speakers with a non-standard accent.

Indeed, several studies have found a negative link between dialect and wages. Rickford et al. (2015) reported a negative correlation between earnings and the use of African American Vernacular English (AAVE), a dialect spoken by many blacks in the United States. Grogger (2011, 2019) has shown that African American workers with racially distinctive accents have lower wages than similarly skilled whites. Yao and van Ours (2018) report a negative relationship between dialect and wages in the Netherlands, where dialects vary by region rather than race.

Our goal in this paper is to better understand this relationship. Our specific aim is to estimate the causal effect of a regionally distinctive accent on workers' wages. The thought experiment we have in mind is to estimate how a worker's wage would change if she had acquired an accent during childhood that was considered standard rather than regionally distinctive. We then offer some evidence on the mechanism by which the regional accent effect arises.

We study workers in Germany, whose varied regional accents provide an ideal setting for our work. To estimate the accent-related wage penalty, we collected survey data about the strength of workers' accents. We also collected measures of skills that capture typically unobserved aspects of worker productivity, as well as measures of the worker's childhood environment that may have shaped her speech patterns. We employ these measures in different ways in order to deal with omitted variable bias.

We are concerned with two types of potential omitted variable bias. Considering the regional nature of German dialects, one type of omitted variable bias may arise if regional accents are correlated with characteristics of the regional economy that influence worker productivity. Another may arise if idiosyncratic worker characteristics that influence wages are also related to the worker's speech. Either type of selection on unobservables may lead to bias in the estimated effect of regional accents on wages.

[1] We come back to the distinction between dialects and accents in Section 2.2.

Selection on unobservables has posed a challenge for prior work. Rickford et al. (2015) and Grogger (2011) pursued a control strategy to deal with omitted variable bias, including rather small sets of controls. Grogger (2019) expanded the list, showing that the dialect wage penalty was robust to the inclusion of cognitive test scores. Yao and van Ours (2018) similarly pursued a control strategy.[2] Controlling for typically unmeasured skills is just one step of our multipart strategy to deal with unobserved heterogeneity.

Our second step focuses on unobservables that vary regionally. Since accents vary by region, it is informative to compare different estimators that make different uses of the within vs. total regional variation in the data. Region controls turn out to have little effect on the estimated regional-accent penalty, regardless of the size of the regions.

Finally, we take an instrumental variables approach to deal with worker-specific unobservables. As we discuss in more detail below, research from linguistics shows that children acquire their native accents from their childhood peers. This means, that in the absence of correlation between regional accents and regional labor market productivity, the share of one's schoolmates who speak with a regional accent provides an instrument for one's own accent. We asked respondents about the accents of their schoolmates; we construct leave-out means from the responses of respondents within the same region to construct such an instrument.

All of our estimators yield similar estimates showing that the regional-accent penalty is about 20 percent of the worker's wage. This magnitude is comparable to the gender wage gap. A natural question is, what explains that effect? We provide evidence that largely rules out employer discrimination, but is consistent with the type of occupational sorting that could arise either from consumer/coworker discrimination or from a model of task-trading along the lines of Deming (2017).

Our work is most closely linked to the earlier studies on dialect and wages cited above, but it is related to other literatures as well. Several studies have established that dialect and speech features more generally affect a variety of economic outcomes. Two studies have shown that dialect influences the housing market by affecting renters' attempts to rent apartments (Purnell et al., 1999; Massey and Lundy, 2001), and Falck et al. (2012) show that migration is higher between areas with more similar dialects, holding distance constant.

Our work also relates to two broader literatures as well. One is the burgeoning literature relating

[2] Van Ours and Yao (2016) instrumented dialect with the distance between the worker's residence and Haarlem, whose dialect is effectively the Dutch national standard. However, one might be concerned that residential location could influence wages for reasons other than the dialect the worker speaks.

non-cognitive skills to the labor market (Borghans et al., 2008). The other is the literature on what one might call unconventional dimensions of discrimination. This includes studies showing that traits such as beauty, height, and body weight influence wages (Hamermesh and Biddle, 1994; Biddle and Hamermesh, 1998; Persico et al., 2004; Cawley, 2004).

In the next section, we discuss some key findings from sociolinguistics, cognitive neuroscience, and social psychology that our work relates to. Following that, we discuss our data. In Section 4 we present our main results. Section 5 discusses different models that could generate regional-accent penalties. Section 6 concludes.

2 Background

Findings from sociolinguistics, cognitive neuroscience, and social psychology help motivate both our question and the approach we take.

2.1 Preferences toward others' speech

The social psychology literature provides abundant evidence that listeners draw strong conclusions about speakers based on their speech. In the US, both black and white listeners routinely rate AAVE speakers lower than Standard American English (SAE) speakers in terms of socioeconomic status, intelligence, and even personal attractiveness (Bleile et al., 1997; Doss and Gross, 1992, 1994; Koch et al., 2001; Rodriguez et al., 2004). Research similarly has shown that, both inside and outside the South, listeners rate Southern American English speakers lower than SAE speakers on certain subjective scales, including correctness and the degree to which the speaker sounds intelligent (Preston, 1996, 1999; Hartley, 1999; Kinzler and DeJesus, 2013; Tucker and Lambert, 1969; Bailey and Tillery, 1996; Fridland and Bartlett, 2006).

In the German setting, Gärtig et al. (2010) show that speakers of some dialects are viewed as both livelier and less educated than speakers of standard German. Heblich et al. (2015) report lab experiments in which participants are more likely to cooperate with speakers of their own dialect than with speakers of other dialects. All this evidence is consistent with the notion that people have preferences over the speech of others, which is a necessary condition for either employer or consumer/coworker discrimination on the basis of speech.

2.2 What is a dialect? What is an accent?

A dialect is a variety of a language that is related to and mutually intelligible with some standard variety of that language, usually a national standard. Like the standard to which they are related, dialects follow rules implicitly known by all speakers of the dialect. What distinguishes dialects from the standard (and each other) is that some of the rules are different.

Many different types of rules distinguish different dialects. Some involve vocabulary, others may involve grammatical features. Yet others involve pronunciation. Differences in pronunciation distinguish accents. Although we have used the term accent and dialect thus far as if they were interchangeable, accents are our focus. Indeed many speakers are able to use the grammar and vocabulary of both their native dialect and their national standard. However, even when they employ the national standard, they still speak with the accent of their regional dialect.[3]

2.3 Accent acquisition

Key to our instrumental variables strategy are findings from both sociolinguistics and cognitive neuroscience that show evidence of a ”sensitive period” for native dialect acquisition. Before the sensitive period ends, children are capable of acquiring native-sounding accents in whatever language they are exposed to. Once the sensitive period ends, it is much more difficult to acquire a native-sounding accent in a second language. Second dialect acquisition is similar to second language acquisition, in that it is difficult to acquire a native-sounding accent in a second dialect after the sensitive period ends (Siegel, 2010). The literature also indicates that one tends to acquire one's native accent from one's linguistic peers during the sensitive period, rather than from one's parents or other sources such as broadcast media (Labov, 1972).

Although there is some debate as to when the sensitive period ends, there is a fair amount of agreement that it is over before puberty concludes (Johnson and Newport, 1989; Hyltenstam and Abrahamsson, 2008; Granena and Long, 2013). Different aspects of language acquisition may have different sensitive periods. For example, the sensitive period for the acquisition of a native-sounding accent may end as early as age six or seven (Siegel, 2010; Granena and Long, 2013). These various research findings motivate the instrumental variables that we construct below.

[3]Franz Josef Strauss, the former premier of Bavaria, provides an example.

2.4 German dialects

Sensitive-period theory predicts that one will tend to acquire the accent of the region in which one grows up. German dialects exhibit widespread geographic variation, similarly to languages such as Italian, Arabic, and Chinese. Figure 1 provides a map showing the main dialect regions of the country. One observation that stems from this figure is that regional dialects are widely distributed across the country. Put differently, it is not as if one part of the country speaks standard German while another part speaks with a regional dialect. Thus speaking with a regional accent is not equivalent to coming from a particular region. We will return to this point below.

3 Data

3.1 The German Socioeconomic Panel

Our data on workers' speech and wages come from the German Socio-Economic Panel Innovation Sample, an offshoot of the long-running Socio-Economic Panel (SOEP, Wagner et al., 2007). The SOEP began in 1983 with a representative sample of German households. Like the American Panel Study of Income Dynamics after which it is modeled, it has followed those families on an annual basis, as well as families that split off from the original sample households. The base sample has been refreshed several times.

Like the SOEP, the SOEP Innovation Sample (SOEP-IS) was designed to be representative of the German population aged 17 years and older. Our SOEP-IS respondents were interviewed for the first time in 2016 as part of a special data collection effort for the Collaborative Research Center 190 (CRC-190) that was funded by the German Science Foundation. The overall sample includes 1,556 individuals residing in 1,057 households.

Of the original 1,556 respondents, 1,298 had no missing data on the covariates in our model (among those with missing data, the most frequently missing variable was place of residence at age 10). Of these, 634 were employed and provided valid wage data. We further excluded 52 workers who were self-employed. Of the 1,298 with valid covariate data, 890 were reinterviewed in 2017, yielding an additional 368 valid wage observations among non-self-employed workers. Thus our wage regressions include a total of 950 person-year observations. Below we show that the presence of wage data is not related to having a regional accent.

Interviews were carried out in person. The questionnaires included the standard SOEP questions on employment, earnings, wages, and schooling. In addition to the standard questions, the 2016 SOEP-IS included several modules designed by the research teams collaborating in the CRC. One module included a set of cognitive tests, which one might expect to be correlated with wages. Another asked respondents whether they spoke English. Another module collected information about the respondent's regional accent, while a final module asked about the accents of the respondent's grade school classmates. We now discuss these data in more detail.

3.2 Measuring respondents' regional accents

Early in the 2016 interview, we elicited the interviewer's assessment of the respondent's accent on a five-point scale. The specific question and response options were:

How would you assess the respondent's speech during the interview?

- *No regional accent (like a news anchor in the Tagesschau)*[4]
- *Weak regional accent*
- *Medium regional accent*
- *Rather strong regional accent*
- *Very strong regional accent*

Interviewers were instructed to not confuse regional accents with foreign accents of migrants.

One might be concerned that interviewers would use different standards in assessing the strength of respondents' accents. In Section 4.3, we present results from a specification that allows for this possibility. Furthermore, Purschke (2008) has shown that listeners with no special training in linguistics, like our field interviewers, assess strength of accent in a manner that correlates highly with objective measures of accent strength.

Table 1 shows the distribution of accents among those respondents with valid wages. Forty-two percent were reported to have no regional accent, whereas 45 percent had a weak regional accent. Thirteen percent were reported to have a medium or stronger regional accent. The table also shows that wages differ sharply between those with a weak accent or less and those with a medium or stronger accent. For this reason, we dichotomize this measure for the rest of our analysis. We refer to respondents with medium or stronger accents as having a regionally distinctive accent, or simply a regional accent. We refer to other respondents as mainstream speakers.

[4] A widely viewed national news program.

Figure 2 shows the geographic distribution of regionally distinctive speakers by *Regierungsbezirk*, an administrative unit that is larger than a county (*Landkreis*) but generally smaller than a state (*Bundesland*). A disproportionate number reside in the south, an area known for its distinctive dialects. At the same time, a sizeable number of such speakers comes from the center and to some extent from the north of the country. They are also divided between east and west. This illustrates an important point, which we alluded to above: workers from many regions can speak with regional accents. Having a regional accent does not mean that the speaker comes from any particular region. This fact should limit any correlation between regional unobservables and regional differences in wages, since regions with many distinctive speakers include both high-income areas (such as Bavaria, in the south) and low-income areas (such as Saxony, in the east).

An interesting question is whether workers with regionally distinctive accents speak in the manner typical of the region in which they reside, or whether they are singled out as regionally distinctive because they speak in a manner typical of a different region, from which they have moved to their current residence. Our data show that roughly 20 percent of the sample moved between states between the age of 10 and the time of our survey. However, moving is not correlated with having a distinctive regional accent, suggesting that most of our speakers with distinctive accents speak with the accent of their home region. We exclude movers from some of the regressions below.

Table 2 tabulates wages and various worker characteristics by accent. Speakers with distinctive accents have lower wages, are less likely to speak any English, have lower levels of schooling, are disproportionately male, and have more labor market experience. Their mothers generally have lower levels of education and they lived in somewhat smaller and less densely populated communities at age 10.

The final rows tabulate the skill measures that were collected specifically as part of the 2016 SOEP-IS. One measure comes from a math test; the other comes from a test of basic financial knowledge. Performance on the math test is largely independent of accent, although speakers with distinctive accents fare worse on the test of financial knowledge.

With so many differences in observable characteristics, one might be concerned that unobservable characteristics differ by accent as well. If those differences also influence wages, OLS estimates of the effect of distinctive accents on wages will be biased. To deal with this problem, we construct instruments based on speech patterns of elementary schoolmates as reported by respondents in the vicinity.

3.3 Measuring respondents' childhood dialect environment

As discussed above, the linguistics literature has found that native accent acquisition occurs during childhood and that children generally acquire their accents from their linguistic peers. Our instrument is motivated by this finding. Reckoning that respondents' schoolmates made up an important share of their linguistic peers during childhood, we asked them the following question:

Think back to when you were in elementary school. How would you rate the speech of the majority of your classmates?

1. *No regional accent (like a news anchor in the Tagesschau)*
2. *Weak regional accent*
3. *Medium regional accent*
4. *Rather strong regional accent*
5. *Very strong regional accent*

As with the variable capturing the respondent's own accent, we first dichotomize the responses to this question, coding medium and stronger accents as strong and the others as mainstream. We then form the leave-out-mean over all respondents whose municipality of residence at age 10 was in the vicinity of the municipality in which the target respondent lived at age 10. We exclude the target respondent and any of her household members in the construction of this mean. We vary with the definition of vicinity by including municipalities within a 30- to 60-kilometer radius of the target respondent's municipality. We also experiment with weighting, using various kernels (e.g., triangular, Epanechnikov) that put greater weight on nearer neighbors. Finally, we require at least two other respondents within each vicinity.

Table 3 reports a correlation matrix for the respondent's own regional accent; that of her schoolmates, based on her own report; and that of her schoolmates, based on the spatial leave-out mean over other respondents in her vicinity. Here we define vicinity as being within a 40-kilometer radius and show results for both triangular and Epanechnikov kernels.

The table makes several points. The first column shows that all of the schoolmates' accent measures are similarly correlated with the respondent's own accent. The second column shows that the spatial average measures of schoolmates' accents are highly correlated with the respondent's own report of her schoolmates' accents. Finally, the third column shows that the spatial average measures are highly correlated with each other. Below we show that they also yield similar estimates of the effect of a regional accent on wages.

To compute the instrumental variables estimates reported below, we make use of the spatial averages, rather than the respondent's own report of her classmates' regional accent. We do this for two reasons, one that involves the potential for sorting into schools and one that involves the potential for recall bias. With the choice of a particular school, parents might be able to influence the peer-group composition of their children. Thus, at the very local level, one might be concerned that parental decisions could affect the strength of the classmates' accents, in which case the instrument would not be exogenous. This is less of a concern with the spatial average, which both omits the target respondent's own report and is constructed from responses over a larger geographic area.

Recall bias could distort repondents' retrospective assessment of the accents of their classmates. Consider two identical respondents who went to the same school. After graduating, suppose that the first experiences a positive career shock. As a result, she earns more and enters jobs and social environments where people speak with less of an accent. A negative career shock hits the other one, she earns less, and people in her job and social environment speak with a stronger accent. At the time of our interview, both respondents might benchmark the accent of their elementary school peers against their current peers' accents. The first respondent might then assess the accent of her classmates as stronger than the second respondent. Such recall bias would induce a positive correlation between the respondent's own report of her classmates' accent and the outcome variable. This would violate the exclusion restriction and lead to an IV estimate that is biased toward zero (since we expect $\alpha_1 < 0$). The spatial average measure does not suffer from this problem, since it averages over the shocks of other individuals. Indeed, exploratory work showed that IV estimates based on the respondent's own report of her classmates' accents were smaller in absolute value than those based on our preferred spatial averages.

4 Estimation and Results

4.1 OLS regressions

The first step of our analysis is to estimate the regional accent penalty using regression models that include varying sets of covariates, including some typically unobserved measures of skill. We report ordinary least squares estimates of a wage regression that takes the form

$$w_i = \alpha D_i + X_i \beta + \varepsilon_i \qquad\qquad i = 1, ..., N. \tag{1}$$

where w_i is the worker's log hourly wage; D_i is the regional accent indicator, which equals one if worker i speaks with a distinctive regional accent and equals 0 otherwise; X_i includes a vector of control variables; and ε_i is a disturbance term. The parameter α_1 gives the effect of a regionally distinctive accent on wages and the vector β contains the coefficients for the control variables. We proceed by including different sets of controls in different specifications of X_i.

The first column of Table 4 reports the estimate of α_1 that results from a regression with no controls at all. It gives the mean difference in log wages between workers with and without a distinctive regional accent. The estimate (standard error) is -0.238 (0.061), a sizeable gap.

The key question is how much of that gap is due to the distinctive accent and how much is due to other traits that are correlated with the distinctive accent? One such set of traits might be language skills generally. To capture language skills, we add to the regression a dummy variable equal to one if the worker reports that she can speak English. The coefficient on the English dummy is sizeable and significant. Adding it to the regression causes the distinct accent coefficient to fall from -0.238 to -0.200.

The next column adds a fairly standard set of regressors to the model, including educational attainment dummies, a gender dummy, and experience and its square. The coefficients of these variables all behave as one might expect. Adding them to the model raises the adjusted R-squared from 0.04 to 0.24. It also causes the distinct-accent coefficient to fall to -0.176. The next column adds maternal education dummies to the model. Maternal education is jointly significant, although much of that is due to workers who report not knowing their mother's education level.

In column (5) we add scores from the math and financial knowledge tests that were administered in the 2016 SOEP-IS. These variables clearly capture important dimensions of labor market productivity, since their coefficients are both positive and significant and the adjusted R-squared increases from 0.28 to 0.32. Yet adding them to the model has little effect on the distinct-accent coefficient, if anything raising it slightly (in absolute value).

The last column of Table 4 adds geographic controls to the model. These include the population size and density of the municipality in which the respondent lived at age 10, on the grounds that regional accents may be more prevalent in rural areas. They also include a dummy for whether the respondent lived in the former East Germany at age 10, as a rough control for differences in economic and political systems. The East Germany coefficient is negative and significant, but adding these regressors has little effect on the regional-accent coefficient. At the same time, the English coefficient has fallen from 0.267 (0.081) in column (2) to 0.076 (0.078) in column (6), which includes the broadest set of controls.

In Table 5 we present estimates that provide controls for regional variation in unobservables. The first three columns present within-region estimates of equation (1), defining regions variously in terms of states, *Regierungsbezirke*, and counties of residence at age 10.[5] We estimate these models by adding the relevant set of region dummies to the model reported in column (6) of Table 4. All three estimates are quite similar, whether we control at the level of the state or at the much finer level of the county. Moreover, these estimates are similar to those reported in Table 4. This suggests that there is little correlation between regional accents and unobserved regional characteristics that influence worker productivity. This is consistent with the observation above that regional accents are widely distributed across different regions of the country.

Before proceeding, one important point from Tables 4 and 5 merits highlighting. Namely, the coefficients on the distinct-accent dummy are quite stable across different sets of controls, including those which have considerable explanatory power for wages. Excepting the raw mean difference in the log wage, reported in column (1), the estimates range from -0.176 to -0.230.

With some additional assumptions, we can use the estimates in Tables 4 and 5 to gauge not only the importance of the observables, but also of the unobservables, as potential sources of bias to our estimated regional accent coefficient. Altonji et al. (2005) and Oster (2019) provide conditions under which one can bound the omitted variable bias stemming from unobservables on the basis of how much the coefficient of interest changes when one adds additional observable regressors to the model. Oster (2019) provides a bias adjustment that yields a consistent estimator under three conditions. These are: (i) the ratios of the coefficients on the variables in X_i in equation (1) are equal to the ratios of the coeffcients on the variables in X_i in a regression of D_i on X_i; (ii) selection on observables is equal to selection on observables; and (iii) the maximum R-squared, denoted R^2_{max}, is known, where the R^2_{max} is the R-squared that would result if one could control for all observables and unobservables.

Of course, a limitation of this approach is that the first two assumptions are untestable and the third is unlikely to hold. Nonetheless, under certain conditions, the bias adjustment below may provide a useful, if rough, gauge of the extent to which the estimates in Tables 4 and 5 may be biased due to omitted variables. Although Oster (2019) notes that condition (i) is guaranteed only in the case of a single unobserved regressor, she argues that limited departures from this condition may leave her bias adjustment procedure approximately valid. Altonji et al. (2005) argue that assumption (ii) may be

[5] Our data also include current state of residence, but not current *Regierungsbezirk* or county. When we substitute state of current residence for state of residence at age 10, we obtain similar estimates.

justified if researchers seek to measure and control for the most important variables, that is, the variables that could cause the greatest bias. If so, then selection on observables may actually be more important than selection on unobservables. Finally, replacing the unknown R^2_{max} with its upper bound over-adjusts for bias.

A consistent, bias-adjusted estimate of α in equation (1), under the assumptions above, is given by

$$\alpha^* = \hat{\alpha} - [\hat{\alpha}_c - \hat{\alpha}][\frac{R^2_{max} - R^2}{R^2 - R^2_c}] \tag{2}$$

where $\hat{\alpha}$ is the estimate from a regression that includes all observable regressors; $\hat{\alpha}_c$ is the estimate from a constrained regression that includes only a subset of the observables; and R^2 and R^2_c are the R-squared statistics from the corresponding regressions.

We first limit attention to Table 4, taking $\hat{\alpha}_c$ from column (1) and $\hat{\alpha}$ from column (6). Since R^2_{max} is unknown, we set it equal to one. Assuming that there is measurement error in log wages, so that $R^2_{max} < 1$ results in an estimate that is closer to zero than the estimate based on the unknown R^2_{max}. The resulting bias-adjusted estimate is $\alpha^* = -0.108$. If instead we take $\hat{\alpha}$ from column (3) of Table 5, which includes county-level region controls, we get $\alpha^* = -0.173$. Either way, if the underlying assumptions were valid, these estimates would indicate that the true regional accent penalty was negative and sizable.

For sake of comparison, we perform a similar calculation using the English coefficient, which changes a great deal as additional regressors are added to the model. We compare the estimates in columns (2) and (6) of Table 4. This yields a bias-adjusted estimate of the effect of speaking English of -0.365. In contrast to the effect of a regional accent, the apparent effect of speaking English is so affected by the observable regressors that OLS may actually get its sign wrong.

4.2 IV estimates

The above calculations suggest that unobservable characteristics of workers may not bias the estimated regional-accent coefficient by a great deal. At the same time, those calculations are based on a set of largely untestable and somewhat unfamiliar assumptions. In this section, we present instrumental variables estimates, which are based on different and more familiar assumptions.

As discussed above, our instrument is a weighted spatial average, taken over all other respondents within the target respondent's vicinity, of the share of elementary school classmates who spoke with a distinctive regional accent. Here we present results based on a vicinity defined in terms of a 40-kilometer radius around the target respondent. We show results from two weighting schemes, one that employs a triangular and one that employs an Epanechnikov kernel. These yielded the highest first-stage F statistics among all the models we evaluated, as we discuss further below.

Table 6 presents the results of first stage regressions that take the form

$$D_i = \pi_1 Z_i + X_i \pi_2 + v_i \qquad\qquad i = 1, ..., N. \tag{3}$$

where Z_i is one of the instruments and X_i now includes all the variables included in column (6) of Table 4. Results in column (1) are based on the triangular kernel and those in column (2) are based on the Epanechnikov kernel.[6] The coefficients on the instruments are similar in the two regressions, and besides the instruments, the most significant predictors of a regional accent are education and the other skill measures.

Table 7 presents 2SLS estimates of the effect of a regional accent on log wages. Results in column (2) are based on the instrument constructed using the triangular kernel and those in column (3) are based on the instrument that uses the the Epanechnikov kernel. Column (1) presents OLS results based on the same sample for purposes of comparison. At the bottom of the Table we report effective F statistics, which provide a measure of the strength of the first stage in the presence of dependent observations (Montiel Olea and Pflueger, 2013). Both statistics exceed their 10 percent critical values by a considerable magnitude.

The IV estimates are both sizable and negative. They are also similar to each other and to the OLS estimate. At the same time, they are about the same magnitude as their standard errors.

In an attempt to increase precision, we also computed 2SLS estimates based on a logit first stage. In this approach, we estimate equation (3) via a logit model, then use the predicted values from that logit regression as the instrument for D_i in equation (1). Wooldridge (2002, ch. 16) suggests that this estimator should be more efficient than the 2SLS estimator based on the linear first stage, since the logit model provides a better approximation of $E(D_i = 1 | Z_i, X_i)$ than its linear counterpart.[7] The coefficients from the logit first stage appear in columns (3) and (4) of Table 6.

Second-stage estimates based on the logit first stage are presented in columns (4) and (5) of Table 7. Again, the regional accent coefficients are similar to the OLS coefficient. However, their standard errors

[6]The sample sizes here are a bit smaller than those above due to the restriction that there be at least two other respondents in the target respondent's vicinity in order to compute the instrument.

[7]A probit first stage yielded similar estimates.

are much smaller. The t-statistic for the estimate based on the triangular kernel is -1.8; for the estimate based on the Epanechnikov kernel, it is -1.68.

4.3 Robustness checks

Above we noted that we experimented with a number of different definitions of vicinities and different kernels in computing our instrumental variables estimates. We also experimented with different sets of regressors. Figure 3 provides a scatterplot of the first-stage effective F from each model against the resulting estimated regional-accent penalty. The figure plots results from 96 different models. These vary according to the radius used to define the vicinity over which the leave-out mean was calculated (30, 40, 50, or 60 kilometers), the kernel used for weighting (triangular, Epanechnikov, or isentropic), the functional form of the first stage (linear v. logistic), and the regressor sets corresponding to those that appear in columns (3)-(6) of Table 4. The scatters for the linear (in red) and logit (in blue) first stages are somewhat different, but both exhibit an inverted U-shape. Nearly all the estimates are negative. The estimates presented in Table 7, marked with X's, are those that yielded the highest effective F statistics within the linear and logit classes.

Table 8 presents some additional regressions to further check for sources of omitted variable bias. Since the OLS and IV estimates are so similar, and the OLS estimates are much more precise, we estimate all additional regressions by OLS using our baseline specification, the model in column (6) of Table 4.

In the first column, we add a variable that captures the worker's overconfidence with respect to mathematical abilities and another that measures attitudes toward risk. These were collected in one of the other experimental modules that were fielded by the SOEP-IS. In the second column, we add the first principal component from a set of experimental variables designed to measure the importance of factors such as personal relationships and professional as well as personal accomplishment in the worker's life. Neither of these sets of variables has much effect on the distinct-accent coefficient.

In the third column, we add to our baseline specification a set of field-interviewer dummies. The distinct-accent coefficient rises slightly, suggesting that, if anything, variation in interviewer assessments of accents causes the regional-accent penalty to be slightly understated. Column (4) reports results from a regression that contains all three sets of variables. The distinct-accent coefficient remains essentially the same.

In the fifth column we drop workers whose current state of residence is different from their state of residence at age 10 . This raises the coefficient somewhat. At the same time, it shows that movers are not driving the estimated wage penalty.

In the sixth column, we add to our baseline specification interactions between the distinct-accent and age-group dummies. This regression is designed to shed light on a reverse-causation hypothesis. Although the sociolinguistic and neuroscientific evidence suggests that it is difficult to change one's accent after childhood, some individuals may nevertheless be able to do so, particularly in response to labor market incentives. If so, then we would expect the distinct-accent penalty to be larger among older workers than among younger workers, because older workers have faced incentives to change their accent for a longer time and have had a longer time to do so. The interaction terms in column (5) are not significant, but if anything, they suggest that older workers face a lower penalty rather than a higher one.

In the seventh column, we add to our baseline specification an interaction between the distinct-accent dummy and a dummy for having lived in one of the Southern states at the age of 10. The Southern states have the highest share of speakers with distinct accent. The interaction term is positive, hinting at a lower penalty for Southern dialects. However, the interaction is far from statistical significance and the implied penalty for Southern dialects is sizable as well. This suggests that the effect is not driven by the accent of Southern speakers.

In the eighth column, we add an interaction between the distinct-accent dummy and the level of education to our baseline specification. These results suggest that the penalty is smallest for individuals with low education and larger for groups with higher levels of education. Since the number of speakers with distinct-accents within each group is rather small, standard errors are large. Overall, these results show a penalty for all education groups but indicate that the penalty may be higher for more-educated workers.

Finally, Table 10 presents results from regressions that address the question of whether the presence of valid wage data, or employment status more generally, is related to the worker's accent. If so, then the wage regressions above could be subject to the usual sort of sample selection bias stemming from the unobservability of wages among non-workers. In column (1), the dependent variable equals one if the worker met our sample inclusion criteria and equals zero otherwise. In column (2), the dependent variable equals one if the wage data is observed and equals zero otherwise. Both coefficients are small and insignificant.

5 Can we explain the distinct-accent penalty?

Above we noted several models that could potentially explain a distinct-accent penalty. They included employer discrimination; occupational sorting, stemming either from consumer/coworker discrimination or task-trading; or omitted variable bias more generally. Here we attempt to distinguish between these alternative explanations.

Most of the evidence points against omitted variable bias. The OLS estimates are quite robust to the inclusion of a number of factors that are significant predictors of wages, including both worker-specific productivity measures and and various levels of regional controls. The bias-adjustment exercise proposed by Oster (2019) suggests that the remaining selection on unobservables may not be too great. Our IV estimates are similar to the OLS estimates, further suggesting that our estimates reflect the effect of a regional dialect, rather than some omitted variable.

The two different models of discrimination predict different patterns of worker sorting and differential wage penalties across occupations (Hamermesh and Biddle (1994)). Models of consumer/co-worker discrimination generate occupational sorting. In these models, the employer has to charge consumers a lower price, or pay coworkers a higher wage, to induce them to interact with an employee against whom they are prejudiced. The result is a lower wage for workers with a stigmatized trait, such as a regional accent. To minimize their wage penalty, workers with the stigmatized trait sort into occupations involving little contact with consumers or coworkers. Deming's (2017) task-trading model also leads to occupational sorting: a worker with a trait that makes her less effective at trading tasks with others should seek occupations that do not require intensive interpersonal interactions, since she would presumably suffer a wage penalty in occupations that do. Of course, these models may not be so distinct in this setting, since consumer or coworker prejudice could be the reason why workers with regional accents are less effective in interpersonal settings.

In contrast, in Becker's (1971 [1957]) model of employer discrimination, the wage penalty is set by the marginally prejudiced employer, that is, the employer who is indifferent about hiring a distinctively accented worker. This provides an incentive for distinctively accented workers to sort away from more prejudiced employers, but none to sort into particular occupations. Likewise, there is no reason to expect that the distinct-accent penalty should vary across occupations.

To distinguish between these models, we test for sorting and differential wage penalties across occupations involving different levels of interpersonal interaction. We measure occupational interaction intensity using the index of face-to-face contact from Firpo et al. (2011), which is constructed from information on the task-intensity of occupations in the O*NET database. Grogger (2019) provides details as to how it was constructed.[8]

We assign occupations to one of three categories, depending on the tercile of the interaction-intensity index into which they fall. We use multinomial logit models to measure worker sorting across the terciles. In these logit models, the dependent variable indicates the tercile of the face-to-face contact index into which the worker's occupation falls. The variables on the right side of the model include all the regressors that appear in column (6) of Table 4. In the first column of Table 9, we report the marginal effect of a regional accent on the probability that a worker's occupation lies in each of the three terciles. These marginal effects sum to zero by construction.

To test for occupational differences in the distinct-accent penalty, we estimate wage equations that include all the variables in column (6) of Table 4 plus interactions between the distinct-accent dummy and the middle- and low-tercile dummies. In these models, the main effect gives the regional-accent penalty in occupations within the top tercile of interaction intensity as measured by the face-to-face contact index. We report that main effect in the first row of column (2) in Table 9, labeled "high-intensity." The rows labeled medium- and low-intensity report coefficients on the interactions between the regional-accent dummy and the corresponding tercile dummies. We estimate the wage equations by OLS, since the OLS estimates above were similar to the IV estimates but more precise.

The sorting coefficients in column (1) show that workers with a regional accent sort away from occupations in the top third of the interaction-intensity distribution and towards occupations in the bottom third. These coefficients are both substantial and statistically significant. The second column shows that the regional accent penalty is sizable and negative for workers in most interaction-intensive occupations. It is smaller for workers in less interaction-intensive occupations, since the corresponding coefficients are positive. The wage penalty in the most interaction-intensive occupations is significantly negative, although the differences between the wage penalties in that and the other occupations are not. This pattern is largely consistent with the predictions from the consumer/co-worker discrimination model and task-trading model discussed above. It does not tell us which of those models generated the data, but the pattern is clearly inconsistent with the model of employer discrimination.

The remaining columns of Table 9 show results from several placebo regressions. There we estimate sorting logits and sector-specific wage penalties, but we define occupational sectors in terms of tasks that

[8]We also considered Deming's (2017) social interactions measure, which yielded similar but weaker results.

are unrelated to interaction intensity. For example, we would not expect a regional accent to be particularly penalized in occupations that involve a high degree of non-routine analytical tasks. Likewise, we would not expeact that workers with a regional accent sort either into or away from such occupations. If the estimates showed otherwise, that would call our findings above into question.[9]

The estimates in column (1) of panel B indeed show that workers with regional accent do not sort systematically into occupations on the basis of their intensity in non-routine analytical tasks. Column (2) shows that occupations with both high and medium intensity in such tasks involve wage penalties, but neither the main effect nor the interaction effect is significant. Likewise, the remaining panels show that regionally accented workers do not sort systematically into occupations on the basis of their intensity involving either routine or non-routine manual tasks. Wage penalties for workers with regional accents are negative and significant in occupations within the top terciles of both routine and non-routine manual task intensity, but workers with regional accents do not systematically sort away from those occupations.

6 Conclusions

Distinctive accents are common in many countries around the world. At the same time, people make sharp judgements about the accents of others, which could result in discrimination. Yet distinguishing the labor market effect of a distinctive accent from unobserved heterogeneity has been a challenge for previous work.

We employed several strategies to estimate the wage penalty of regional accents. First, we controlled for a set of typically unobserved skill measures which are highly predictive of wages. Next, we included detailed geographic controls. Finally, we utilized an instrumental variable that was motivated by linguistic research on accent acquisition. All of these approaches yielded similar estimates. They suggest that workers with distinctive regional accents experience a wage penalty of about 20 percent, all else equal.

A lingering question one might have is, if the wage penalty for a regionally distinctive accent is so large, then why don't people acquire a standard accent? One could similarly ask why people don't complete a university education, considering the sizable wage premium earned by those who do? We suspect that there are similarities in the answers to these two questions.

Both answers involve costs. In the case of education, some of these costs are financial, in the

[9]Non-routine analytical tasks and the other tasks analyzed here are defined in Autor and Handel (2013) and constructed from the O*NET data.

form of out-of-pocket expenses and foregone earnings while in college. There are also the costs of effort involved in studying, which probably interacts with the student's underlying academic skill. The earnings-maximizing student completes college if the benefits exceed the costs, leaving many without degrees.

In the case of accent, the costs are somewhat different. The costs of acquiring an accent is lowest during childhood and rises thereafter. This means that parents make the key decisions that influence their children's accents. Considering that children acquire their native accents from their peers, this means that one's peers at school play a key role in influencing children's accents. If the use of dialect is prevalent in one's region of residence, that will be reflected in the schools. This observation motivates our instrument, but it also helps explain why parents don't just provide different environments, since the cost providing a child with a different environment could entail changing schools or even moving to a different region. It's easy to see how parents could forego such a costly choice.

Workers with regional accents sort away from occupations that involve extensive interpersonal interactions. By doing so, they avoid the large negative wage penalties that are associated with those occupations, suffering the smaller wage penalties that arise in less interactive lines of work. We cannot say whether this sorting arises from consumer/coworker discrimination or from a model of task-trading in which regional speech reduces productivity for reasons other than consumer or coworker prejudice.

We can say that the occupational sorting we see in Germany is similar to what we observe in the United States among African American workers with distinctive speech (Grogger, 2019). An abundant literature in both countries shows that people have strong views about the speech of others. Our results show that the wage penalty that may stem from these views is quite sizable.

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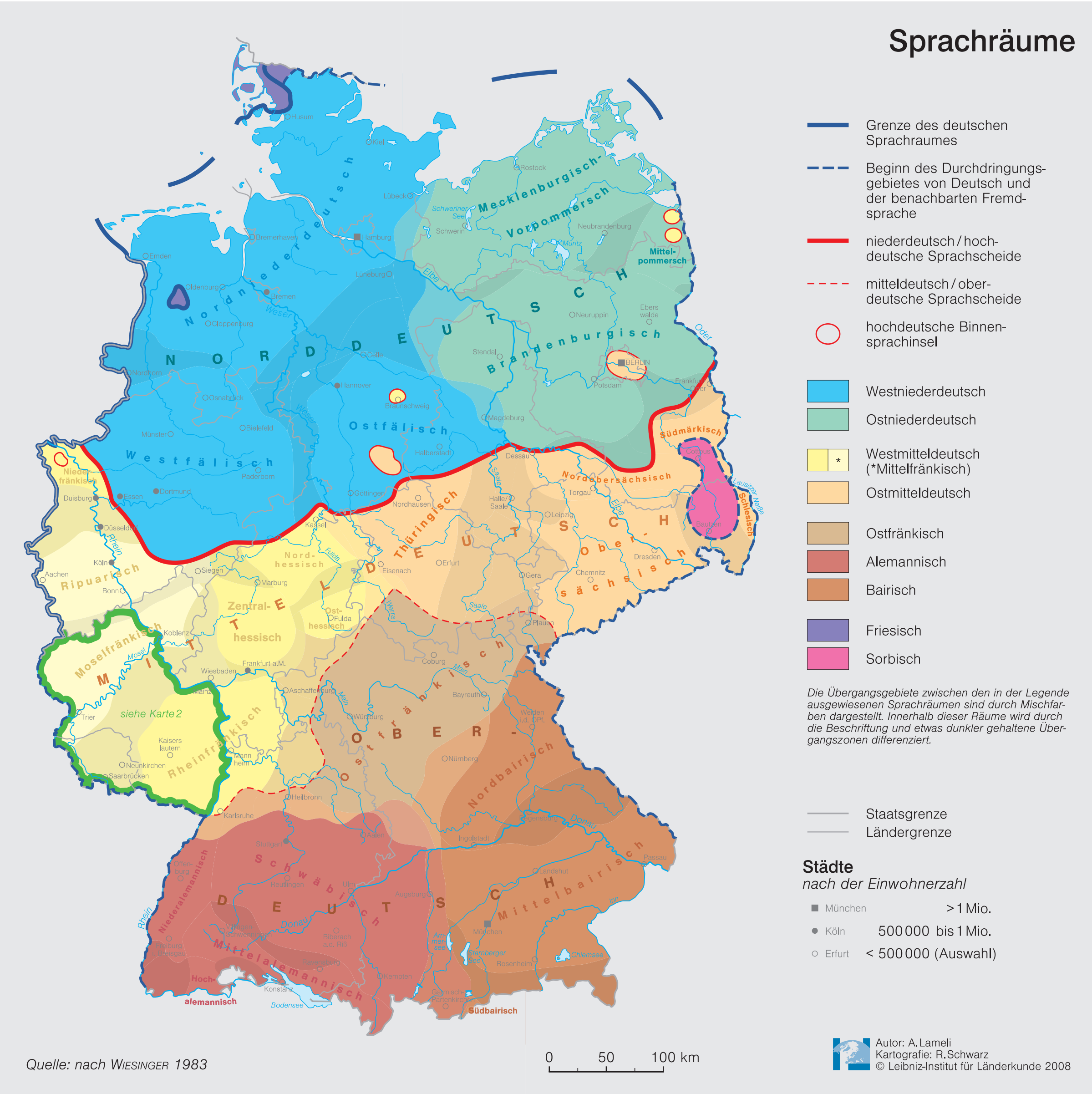
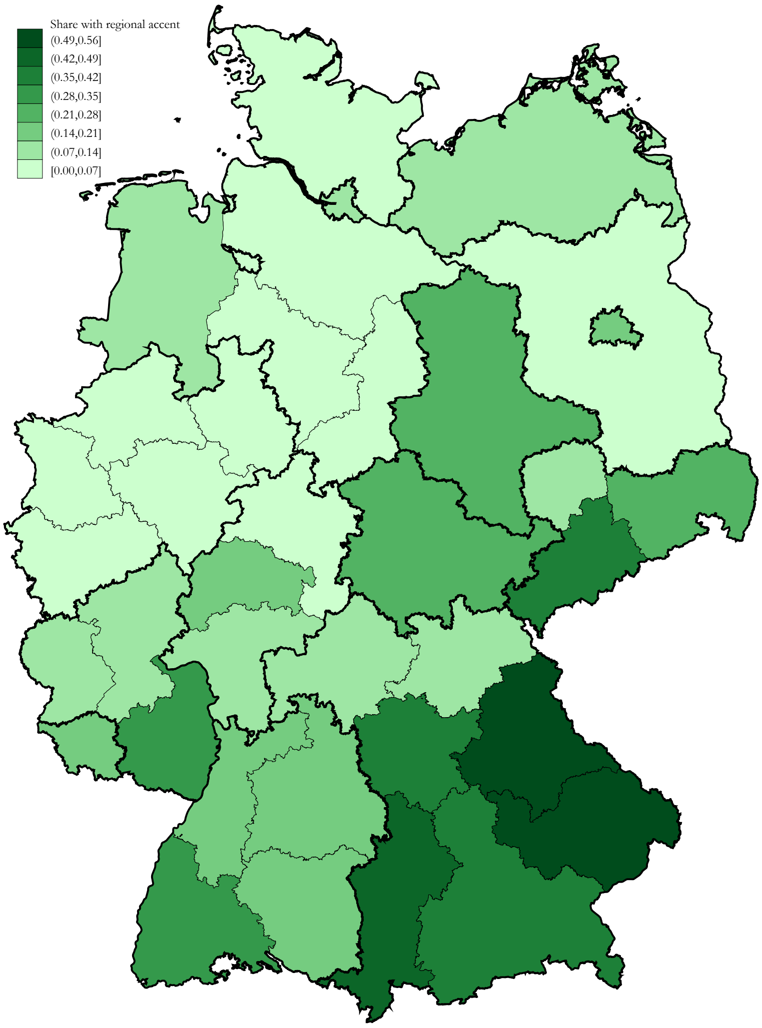
Tables and figures

Figure 1: Dialect areas in Germany



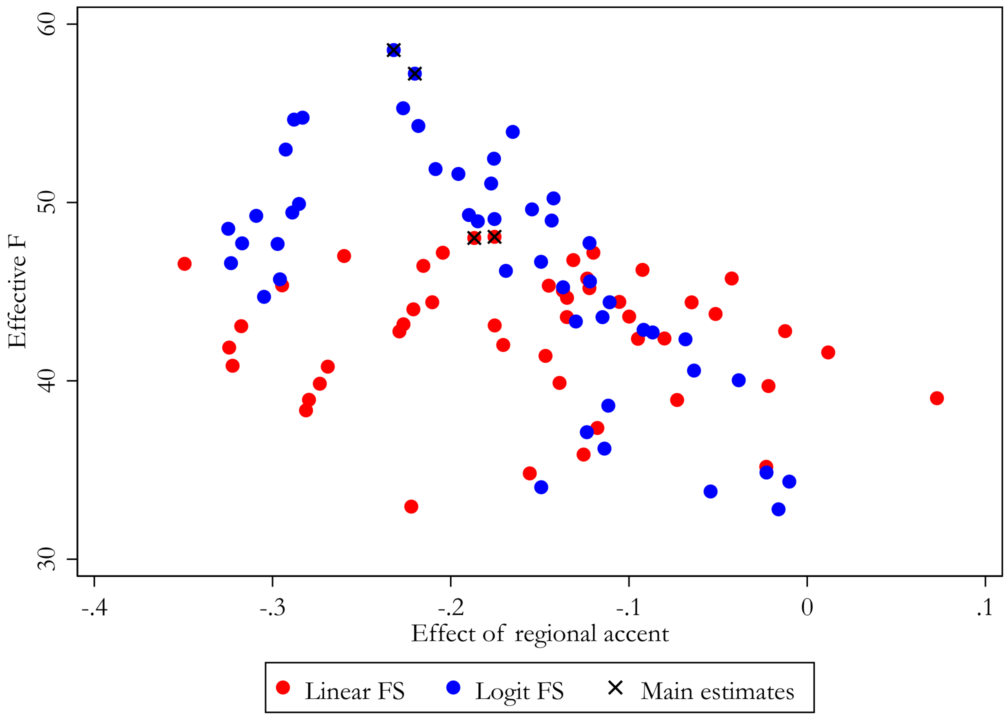
Source: Lameli (2008)

Figure 2: Geographic distribution of speakers with distinctive regional accents



Notes: The figure depicts the share of workers with distinctive regional accents.

Figure 3: IV estimates with different specifications



Notes: The figure depicts second stage coefficients (x-axis) and effective F-statistics (y-axis) from various specifications. Each dot is the result of one specification. Specifications vary by covariates chosen (covariates from column 3, 4, 5, 6 of Table 4; by radius that defines the vicinity (30, 40, 50, 60 km); by kernel for the weighting function (triangular, Epanechnikov, uniform); and linear or logit first stage. Total number of specifications is 96. The ones marked with X are the main specifications presented in Table 7.

Table 1: Distribution of interviewer assessment of regional accent and mean hourly wage

	Observations	Share (%)	Hourly wage (Euro)
No regional accent	402	42.3	17.8
Low regional accent	424	44.6	17.4
Medium regional accent	114	12	13.8
Strong regional accent	10	1.05	15.5
Total	950	100	17.1

Notes: The table shows the distribution of interviewer assessments and mean hourly wage. One individual with *very strong* regional accent has been added to the fourth category. In subsequent tables, *no* and *low* regional accent as well as *medium* and *strong* regional accent are grouped together into binary categories.

Table 2: Means and standard deviations of respondent characteristics, by distinct regional accent

	No regional accent		Regional accent	
	mean	sd	mean	sd
Hourly wage (Euro)	17.62	8.85	13.94	8.56
Speaks English at all	0.95	0.22	0.81	0.40
Lower Secondary Ed.	0.17	0.37	0.41	0.49
Secondary Ed.	0.34	0.47	0.45	0.50
Higher Ed.	0.37	0.48	0.10	0.30
University	0.12	0.33	0.04	0.20
Male	0.50	0.50	0.53	0.50
Experience (years)	23.89	13.00	32.39	11.11
Mother Lower Sec. Ed.	0.52	0.50	0.71	0.46
Mother Sec. Ed.	0.22	0.41	0.16	0.37
Mother Higher Ed.	0.06	0.23	0.00	0.00
Mother University	0.08	0.27	0.02	0.15
Mother's Ed. Unknown	0.12	0.33	0.10	0.31
Mun. pop. at age 10 (in 1000s)	296.54	738.16	280.82	784.95
Mun. pop. density at age 10	927.72	1049.61	884.68	1076.89
Lived in East Germany at age 10	0.21	0.41	0.24	0.43
Math score	-0.01	1.02	0.06	0.88
Financial knowledge	0.03	0.97	-0.23	1.14
Math score missing	0.02	0.15	0.04	0.20
Observations	826		124	

Notes: *No regional accent* refers to workers with no or weak regional accent. *Regional accent* refers to workers with medium or stronger regional accent.

Table 3: Correlation between respondent's and childhood schoolmates' regional accents

			Spatial leave-out mean	
	Regional accent	Schoolmates with distinct accent	Schoolmates w/ accent (r=40km, k=tri)	Schoolmates w/ accent (r=40km, k=epa)
Regional accent	1			
Schoolmates with distinct accent	0.323	1		
Schoolmates w/ accent (r=40km, k=tri)	0.323	0.411	1	
Schoolmates w/ accent (r=40km, k=epa)	0.324	0.409	0.996	1

Notes: First two columns are based on target respondent's report; second two are based on reports of other respondents within her vicinity. Vicinity is defined as the area within 40km of the target respondent's residence at age 10. The target respondent and other respondents from the same household are excluded from the calculation. r=radius, km=kilometers, k=kernel, tri=triangular kernel, epa=Epanechnikov kernel.

Table 4: OLS wage regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Regional accent	-0.238	-0.200	-0.176	-0.179	-0.192	-0.197
	(0.061)	(0.062)	(0.057)	(0.057)	(0.057)	(0.057)
Speaks English at all		0.267	0.236	0.200	0.130	0.076
		(0.081)	(0.081)	(0.078)	(0.074)	(0.078)
Secondary			0.082	0.071	0.027	0.051
			(0.047)	(0.047)	(0.047)	(0.048)
Post-secondary			0.297	0.258	0.179	0.186
			(0.057)	(0.059)	(0.060)	(0.060)
University			0.437	0.407	0.294	0.305
			(0.071)	(0.069)	(0.071)	(0.072)
Male			0.208	0.221	0.156	0.151
			(0.040)	(0.039)	(0.041)	(0.041)
Experience (years)			0.051	0.044	0.044	0.045
			(0.007)	(0.007)	(0.007)	(0.007)
Exp. squared			-0.001	-0.001	-0.001	-0.001
			(0.000)	(0.000)	(0.000)	(0.000)
Mother secondary				0.120	0.132	0.156
				(0.047)	(0.046)	(0.046)
Mother post-sec.				-0.065	-0.040	-0.018
				(0.081)	(0.082)	(0.081)
Mother university				0.064	0.088	0.141
				(0.079)	(0.076)	(0.078)
Mother's ed. unknown				-0.294	-0.263	-0.246
				(0.080)	(0.076)	(0.076)
Math score					0.063	0.066
					(0.020)	(0.020)
Financial knowledge					0.084	0.081
					(0.022)	(0.022)
Math score missing					-0.017	-0.009
					(0.072)	(0.070)
Mun. pop. at age 10 (in 1000s)						0.000
						(0.000)
Mun. pop. density at age 10						-0.000
						(0.000)
Lived in East Germany at age 10						-0.130
						(0.048)
Observations	950	950	950	950	950	950
R-squared	0.02	0.04	0.24	0.28	0.32	0.33

Notes: Sample restricted to employed workers with valid wage data. Figures in parentheses are standard errors, clustered by worker.

Table 5: Within-region estimates

	(1) State	(2) Regierungsbezirk	(3) County
Regional accent	-0.216	-0.230	-0.202
	(0.059)	(0.064)	(0.081)
Observations	950	950	950
R-squared	0.35	0.37	0.56

Notes: Figures in parentheses are standard errors, clustered by worker. In addition to the variables shown, all regressions include all variables from column (6) of Table 4.

Table 6: First-stage estimates

	Linear		Logit	
	(1) Triangular	(2) Epanechnikov	(3) Triangular	(4) Epanechnikov
Schoolmates w/ accent	0.363	0.369	3.919	3.934
	(0.052)	(0.053)	(0.572)	(0.568)
Secondary	-0.072	-0.074	-0.464	-0.491
	(0.048)	(0.048)	(0.365)	(0.366)
Post-secondary	-0.120	-0.121	-1.220	-1.236
	(0.045)	(0.045)	(0.489)	(0.488)
University	-0.164	-0.165	-1.533	-1.527
	(0.053)	(0.053)	(0.692)	(0.690)
Male	0.005	0.006	0.196	0.196
	(0.029)	(0.029)	(0.325)	(0.324)
Experience (years)	0.003	0.004	0.095	0.095
	(0.004)	(0.004)	(0.050)	(0.049)
Exp. squared	0.000	0.000	-0.001	-0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Mother secondary	0.007	0.007	0.078	0.093
	(0.037)	(0.037)	(0.468)	(0.468)
Mother post-sec.	-0.102	-0.098	-14.137	-14.113
	(0.031)	(0.031)	(0.422)	(0.422)
Mother university	-0.056	-0.058	-1.849	-1.841
	(0.038)	(0.038)	(1.186)	(1.184)
Mother's ed. unknown	0.021	0.022	0.263	0.275
	(0.047)	(0.047)	(0.548)	(0.549)
Math score	0.031	0.031	0.297	0.283
	(0.013)	(0.013)	(0.150)	(0.149)
Financial knowledge	-0.022	-0.022	-0.285	-0.281
	(0.015)	(0.015)	(0.148)	(0.147)
Math score missing	-0.059	-0.059	-0.791	-0.813
	(0.085)	(0.085)	(0.676)	(0.671)
Speaks English at all	-0.111	-0.110	-0.335	-0.325
	(0.077)	(0.077)	(0.479)	(0.480)
Mun. pop. at age 10 (in 1000s)	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Mun. pop. density at age 10	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Lived in East Germany at age 10	-0.038	-0.038	-0.295	-0.293
	(0.040)	(0.040)	(0.424)	(0.424)
Observations	915	915	915	915

Notes: Figures in parentheses are standard errors, clustered by worker. The regressions include all variables from column (6) of Table 4. Instruments are constructed using a radius of 40 km.

Table 7: Second-stage estimates

	OLS	Linear FS		Logit FS	
	(1)	(2) Tri	(3) Epa	(4) Tri	(5) Epa
Regional accent	-0.182 (0.058)	-0.187 (0.178)	-0.175 (0.178)	-0.232 (0.129)	-0.220 (0.131)
Effective F		48.01	48.07	58.54	57.22
10% critical value		23.11	23.11	23.11	23.11
Observations	915	915	915	915	915

Notes: Figures in parentheses are standard errors, clustered by worker. The regressions include all variables from column (6) of Table 4. Tri=triangular, Epa=Epanechnikov. Instruments are constructed using a radius of 40 km.

Table 8: OLS regressions with additional regressors

	(1) Overconfidence, risk	(2) Attitudes	(3) Interviewer FE's	(4) All three	(5) Without movers	(6)	(7)	(8)
Regional accent	-0.197	-0.191	-0.240	-0.245	-0.308	-0.301	-0.296	-0.116
	(0.057)	(0.056)	(0.062)	(0.062)	(0.070)	(0.134)	(0.078)	(0.066)
Yes × Age 35-50						-0.008		
						(0.151)		
Yes × Age>50						0.212		
						(0.159)		
Yes × Southern F.S. age 10							0.131	
							(0.106)	
Yes × Secondary								-0.153
								(0.109)
Yes × Post-sec., university								-0.033
								(0.179)
Observations	950	950	950	950	716	950	950	950
R-squared	0.34	0.33	0.54	0.55	0.53	0.33	0.33	0.32

Notes: Figures in parentheses are standard errors, clustered by worker. All regressions include all variables from column (6) of Table 4. In addition, column (1) includes measures for overconfidence and risk preferences, column (2) includes a measure for attitudes, column (3) includes interviewer FE's, and column (4) includes all of these.

Table 9: Sorting and wage penalties by occupational characteristics

	Face-to-face conctact		Non-routine analytical		Non-routine manual		Routine manual	
	(1) Sorting	(2) Wage	(3) Sorting	(4) Wage	(5) Sorting	(6) Wage	(7) Sorting	(8) Wage
High intensity	-0.151	-0.246	-0.072	-0.154	-0.054	-0.183	-0.010	-0.168
	(0.066)	(0.066)	(0.056)	(0.121)	(0.047)	(0.085)	(0.046)	(0.080)
Medium intensity	0.035	0.102	-0.019	-0.087	0.028	-0.090	-0.021	0.032
	(0.057)	(0.109)	(0.058)	(0.152)	(0.060)	(0.108)	(0.058)	(0.115)
Low intensity	0.116	0.073	0.091	0.023	0.025	0.082	0.031	-0.149
	(0.051)	(0.102)	(0.052)	(0.131)	(0.057)	(0.138)	(0.058)	(0.135)
Observations	923	923	923	923	923	923	923	923
R-squared		0.34		0.37		0.34		0.35

Notes: Coefficients in columns titled ”Sorting” are marginal effects of regional accent from a multinomial logit model that predicts taskt-intensity tercile of a worker’s occupation. Coefficients in the first row in columns titled ”Wage” is coefficient of regional accent dummy in a log wage regression. Coefficients in the second and third row are coefficients on an interaction term between the regional accent dummy and the corresponding occupational task tercile dummies. Figures in parentheses are standard errors, clustered by worker. In addition to the variables shown, all regressions include all variables from column (6) of Table 4.

Table 10: Effect of regional accent on being employment

	(1) Employed (0/1)	(2) Wage observed (0/1)
Regional accent	0.011	0.020
	(0.031)	(0.031)
Observations	2188	2188
R-squared	0.30	0.32

Notes: Coefficients from OLS regressions of indicators for observed wage data (column 1) and salaried employement (column 2) on the regional accent dummy and all variables from column (6) of Table 4. The sample is restricted to individuals aged 17 and older with no missings in the covariates. Figures in parentheses are standard errors, clustered by worker.