An Empirical Test of the Reder Hypothesis*

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Abstract

A firm that faces insufficient supply of labor can either increase the wage offer to attract more applicants, or reduce the hiring standard to enlarge the pool of potential employees, or do both. This simultaneous adjustment of wages and hiring standards in response to changes in market conditions has been emphasized in a classical contribution by Reder (1955) and leads to the effect that wage reactions to employment changes can be expected to be more pronounced for low wage workers than for high wage workers. This is the 'Reder Hypothesis'.

The present contribution sets out to test this hypothesis by applying censored panel quantile regression models to German employment register data for the period 1987-2001. Our findings support the Reder Hypothesis, suggesting that market clearing in labor markets is achieved by a combination of wage adjustments and changes in hiring standards.

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Keywords: efficiency wages, wage setting, hiring standards, overqualification, wage structure, panel quantile regression, censoring

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1 Introduction

Reder's (1955) hiring standards adjustment hypothesis can be viewed as an extension and complement of the neoclassical wage competition framework. It states that firms do not only adjust wages and take qualifications and ability as given in recruitment processes, but may change hiring standards too. This seemingly innocuous change of institutional procedures may generate an efficiency wage effect, and may therefore be a possible explanation for equilibrium unemployment, wage discrimination and overqualification.[1]

Reder (1955) applies the hiring standards mechanism to explain occupational wage differentials and the response of the wage structure to labor demand changes. The main conclusion of his theory is that the lower part of the wage distribution for a homogenous group of workers responds more to labor demand changes than the upper. For a brief exposition of the argument consider the demand for workers with identical formal qualification but differing ability and sort them with respect to ability. For sake of simplicity assume that ability takes on only three different values, low, medium and high. Assume furthermore that the production technology of the firm requires all types of workers, e.g. an instructor, a standard worker and a helper. Now, what would happen if the firm wants to extend its production and requires one additional worker of each type? In a standard neoclassical model wages of the high ability workers would respond more than wages of the other groups to labor demand shifts *in labor markets where unemployment is higher for medium and low ability workers*: If all high ability workers are employed, competition drives up their wages. On the other hand, open slots for medium and especially low ability workers can be filled from the unemployment pool. Therefore their wages are expected to respond less strongly in this framework.

Reder (1955) considers adjustment of the hiring standard as an alternative and/or complement: if additional high ability workers are not available, firms can fill open slots by promoting medium ability workers. This throttles upward pressure from wages of the high ability workers. Promotions, however, create additional gaps for medium ability workers, leaving the firm with even more open slots for medium ability workers. These slots can either be filled by promoting low ability workers or by poaching workers from other firms. Hence we expect wages to respond stronger if we move down the ability ladder since the gaps become larger at each step. This mechanism breaks down only if all open slots can be filled from unemployed workers.

[1] The argument is developed in Schlicht (2005).

This implication can be tested empirically by running quantile regressions (for different quantiles) of wages on unemployment and control variables for a homogenous group of workers. If the hypothesis is correct, the response of wages to unemployment changes should increase (in absolute value) as we move from the upper to the lower part of the wage distribution, i.e. lower quantiles of the (conditional) distribution should respond more strongly to employment changes. It should be clear, however, that this operationalization of the theory is not one-to-one and hinges on the identifying assumption that additional labor demand is distributed similarly over the ability groups. Conclusions from our empirical results stand under two further disclaimers. The first is due to incomplete information in our data set. It contains precise information an monthly (daily) wages, but working time is given only in three categories (full time, and part time with less or at least half of the standard working time. We try to avoid bias due to changes in working hours by using only full-timers for estimation. Nevertheless it is possible that overtime work is remunerated directly for workers at lower quantiles of the wage distribution whereas this is not (or less) the case for workers at higher quantiles. If this is the case, we obtain more pronounced effects of the lower wage quantiles to labor demand. The second disclaimer is that alternative theories may generate similar empirical outcomes. Higher sensitivity of lower wage quantiles with respect to labor demand changes is compatible with a combination of human capital and implicit contracts too. If high ability workers have accumulated more firm-specific human capital than their colleagues, firms will retain them in downturns and adjust labor demand by hiring and firing medium and low ability workers. Then the relation between labor demand of high ability workers and cyclical fluctuations is weaker than for the other groups, causing less pronounced wage responses. The difference between a pure hiring standards setting and the specific human capital interpretation is not of great importance, however, if the main purpose of the empirical exercise is to find evidence for the existence of efficicency wage effects since pecific human capital is very likely to generate efficiency wage effects too.

On the other hand, empirical implications generated by the hiring standards adjustment hypothesis are diametrically opposed to union bargaining models with centralized (branch level) wage setting. Büttner & Fitzenberger (2003) argue that union wage contracts set de-facto minimum wages for the low wage groups. Consequently their wages are more likely paid according to the centralized contract and should respond less to regional labor demand fluctuations than wages in the upper part of the wage distribution. These are more likely to be set in individual level bargaining and thus more prone to regional labor demand shifts. The authors use a two-step (minimum distance) estimator to test their hypothesis and find it (weakly) confirmed by the data. Their methods and the estimation period are different from ours. The minimum distance estimator applied in their study is based on aggregated data. This does not allow to control for composition bias (explained below). The same problem seems to be present in the study of Ammermueller, Lucifora, Origa, & Zwick (2007), who apply a two step procedure. In the first step they run individual level cross section wage equations including district fixed effects. The coefficients of the fixed effects are then (in the second step) regressed on district level unemployment rates. They find that response of wage on unemployment increases (in absolute size) with the (conditional) wage quantile. The problem with this approach is that the district FE coefficients from the first step are extremely prone to composition bias since they are shift parameters catching all residual wage effects not explained by the other regressors (wage, education and tenure). To show this we applied the two-step method to our data and found results similar to theirs.[2] A preliminary investigation of the composition problem is provided in the appendix. We show that the individual likelihood to be employed depends more strongly on the district level unemployment rate for the lower quantiles of the wage distribution.

The paper is organized as follows. In Section 2 we provide a short literature review. In the first part of Section 3 we derive our empirical model based on guidelines from theory. Then, we introduce our data set and discuss potential data problems or limitations in detail. In Section 4 we discuss the quantile regression methods employed in this paper. Section 5 follows with a short discussion of the results and we conclude with several qualifications and plans for future work.

2 A Short Survey of the Literature

Our hypothesis in question is related to three strands of empirical literature. The literature investigating business cycle effects on the level and structure of wages, wage curve empirics and the empirical literature on wage rigidity.

While the correlations between income or wage *levels* and the business cycle have been studied extensively (see e.g. Solon, Barsky, & Parker, 1994), only a few contributions focus on income and wage *dispersion*. The obvious reason for the selective interest seems to be that cyclicality of income and

[2]The results will be documented in the appendix of future versions of this paper.

wage *levels* plays an important role for business cycle theory, whereas no interesting theoretical contributions for the effect of economic conditions on the *distribution* of wages can be found in the literature.[3] All empirical studies on the relation between earnings *distributions* and unemployment stress the argument that low income earners face higher unemployment risks or are urged to reduce working hours more than other groups in downswings. This implies a reduction of their income shares and – by that – an increase of the income dispersion. Correlations between income inequality and (cyclical) unemployment.

Empirical work on the relation between earnings or income inequality and unemployment is based mainly on two simple specifications. The first is an income share equation proposed by Blinder & Esaki (1978)

$$S_{it} = \alpha_i + \beta_i U_t + \gamma_i \pi_t + \delta_i(t) + u_{it}$$

where S_{it} denotes income share of quintile i in total earnings, U_t and π_t denote unemployment and inflation rates, respectively, and $\delta_i(t)$ is a deterministic (nonlinear) time trend function. The five equations (one for each quintile) have to be estimated under the adding-up restriction $\sum_{i=1}^{5} S_{it} = 1$ (see Haupt & Oberhofer (2006)). In the second specification quintile S_i is replaced by an overall inequality measure as the Gini coefficients or the Theil index using aggregated (national or regional level) time series

$$G_t = \alpha + \beta U_t + \gamma \pi_t + \delta(t) + u_t$$

Parker (1999) surveys 12 studies of each type. For the income share approach most of the studies report a significant negative effect of unemployment on the lowest quintile and a significant positive on the highest. The only exceptions are Blank & Card (1993) and Björklund (1991) who find insignificant effects.[4] The results from the inequality measure approach indicate positive relations between income and unemployment, though only some of them are statistically significant. However, these studies are not relevant in our context as they analyze a composite effect of variations in wages, working hours and the number of employed workers.

A second strand of empirical literature with a focus similar to our paper is concerned with estimation of the relation between wages and regional unemployment (dubbed the 'wage curve').[5] To the best of our knowledge, all

[3]The lack of theory is documented in Buse (1982).

[4]The reason for the difference to other studies is that they exploit regional information (including year and district dummies).

[5]See Blanchflower & Oswald (1995); Card (1995); Blanchflower & Oswald (2005) and Blien (2003) for surveys.

work from this field implement conditional mean models and are therefore not useful for testing the Reder hypothesis. The only work using similar techniques to test a similar question (but delivering different results) are Büttner & Fitzenberger (2003) and Ammermueller et al. (2007).

Several papers focussing on wage rigidity appear to show the strongest relations to our work. Devereux (2000, 2002, 2004) stresses that Reder's theory implies dependence between occupational upgrading (quality adjustment) and the business cycle. He tests this implication empirically by regressing (a) shares of the high qualified in occupation cells, and (b) ocupation quality proxies for occupation cells on unemployment rates and finds it confirmed.[6]

3 Model and data

3.1 The empirical model

As stated above, the Reder hypothesis implies that higher quantiles of the conditional regional wage distribution respond less strongly to regional labor demand changes than lower ones.

We translate this into an empirical model of the form

$$w^*_{i,r,t} = \sum_{p=1}^{2} u_{r,t-p}\, b_p(\tau) + x_{i,r,t}\, g(\tau) + \alpha_i(\tau) + \delta_t(\tau) + \epsilon_{i,t} \qquad (1)$$

with indexes $i = 1, \ldots, N$ for individuals, $r = 1, \ldots, R$ for regions (districts) and $t = 1, \ldots, T$ for time. Here, w^* denotes the natural logarithm of the real wage[7], u is the natural logarithm of the unemployment rate, α is an individual specific fixed effect, δ is a time-effect (year dummy), and x contains establishment size and establishment size squared.[8]

As already mentioned in the literature survey above, the results from aggregate data include workforce composition effects. If workers at the lower part of the wage distribution face higher risks of becoming unemployed in

[6]He uses U.S. data (CPS and the PSID).

[7]More precisely, w^* is the latent uncensored wage. Further details are explained below.

[8]Note that age in perfectly collinear to the year dummies because of the inclusion of individual fixed effects and therefore redundant.

recessions, this group will shrink more than the rest of the sample during recessions.[9] This composition shift generates wage compression in an mechanical way even if wages of the workers remaining employed respond uniformly to demand shifts over the whole distribution. Composition effects are highly important especially for quantile regression analysis since its impact varies with quantiles. To understand this consider a uniform distribution of wages in the range $[0, 1]$. If all workers with wages below the median become unemployed, the minimum of the distribution increases by 0.5 whereas the median increases only by 0.25. An increase in unemployment affecting all workers in the lower half of the wage distribution would generate higher response of the lower quantiles in regression models based on aggregate data. To eliminate this composition effect we estimate fixed effects quantile regressions at the individual level. I.e. we consider the conditional effect of district level unemployment on the individual worker.

The micro data analysis has several other advantages compared to aggregate data. First, we are able to exploit differences in regional unemployment changes to obtain more precise parameter estimates. Second, the increased number of observations allows us to include a full set of year dummies. Regressions with aggregate data have to capture time effects with linear or smooth trends. This can generate artificial effects for the other regressors if the time effects in the data are not smooth enough. A potential disadvantage of our strategy can be seen in the fact that we cannot identify effects of aggregate unemployment on wages. Our regression models exploit only regional deviations from aggregate (national) unemployment.

A further possible problem of the model is caused by inter-regional wage dispersion, i.e. the fact that wage levels differ considerably between districts. This biases our results since a worker from a low wage district with wage *above* the district level median will be located *below* the median of the total sample (including high wage districts). These district level differences could in principle be catched by inclusion of district fixed effects. If the model contains individual fixed effects too, identification of both individual and district fixed effects is possible, however, only if enough individuals move between districts. A small share of district changes in our data set (less than five percent) makes inclusion of district level fixed effects practically infeasible.[10] Fortunately, the bias caused by inter-regional wage dispersion

[9] The composition effect is discussed by Solon et al. (1994) in the context of cyclicality of U.S. wages. The focus of the paper is, however, on level effects.

[10] A further crucial problem is caused by the fact that we have to draw small bootstrap subsamples to obtain the estimates. If a bootrap sample contains no movers between two

shrinks differences between quantiles in our estimated model, implying that we *underestimate* true differences between quantiles.

3.2 Data description

All data sets used here relate to the period 1987-2001 and are based on the employment register data of the German National Agency for Labor. These data contain precise and reliable information on earnings and several other demographic variables of all workers covered by the German social security system. The social security system covers nearly 80 percent of the German workforce, excluding only the self-employed, civil servants, individuals in (compulsory) military services, and individuals in so-called 'marginal jobs' (marginal jobs are jobs with at most 15 hours per week or temporary jobs that last no longer than 6 weeks).

Though earnings information is highly reliable (mis-reporting is subject to severe penalties), working time is reported only in three classes, full time, part time with at least 50 percent of full time working hours, and part time with less than 50 percent. To avoid bias due to an imprecise denominator in hourly wage computations, we restrict our sample to prime-age (20-60 years) full time working men. Furthermore we exclude East-German Workers from our sample to avoid bias due to the economic adjustment process after re-unification in 1990 (with a chaotic touch at least in the beginning). Two further restrictions of our data base are censoring of wages and a structural break in 1984. Wages are right-censored if they exceed the social security threshold. For the whole sample, censoring is moderate (about 10-15 percent). For the high qualified (college and technical college graduates), however, more than 50 percent are censored, making this group practically useless for the quantile regression analysis. Thus this group is dropped from our data sets. The second data problem, a structural break in earnings reporting, is caused by the fact that bonus payments had to be included in earnings from 1984 onwards. This could invalidate our quantile regressions since bonus payments play an important role only for earnings above the median. Our surefire (brute force) solution to the problem is to drop all years before the structural break.

As will be explained in more detail below, it would be extremely time-consuming or even infeasible, to employ the whole employment register data

district a and b, the corresponding fixed effects cannot be identified. Simply dropping such 'degenerate' bootstrap samples would bias the inference and is therefore not viable.

sample in our regressions. Therefore we obtain wages and other demographic variables from the IABS, a representative 2 percent subsample. Only district level unemployment (which would be otherwise imprecise) is computed from the complete register data set. Several other data restrictions and problems require special treatment. As mentioned above, about 10-15 percent of wages are top-coded. Censoring exceeds 50 percent for the high qualified (technical college or college) making reliable estimation of higher quantiles infeasible. Therefore we restrict our analysis to the medium (completed apprenticeship training) and low skilled where censoring is about 10 and 2 percent, respectively. Though censoring is moderate also for the medium qualified, it may have considerable impact at least on the higher conditional quantile estimates.

The empirical implications of the Reder effect are derived for a homogenous group of workers. To mimic this situation with real data, we either can select a group as homogenous as possible from our sample, or hope to construct it with help of multivariate models by using as many control variates as possible. Here we combine both approaches. First, we keep only prime age (20-60 years) full time working male since the attachment of the other groups (female, part-time) is less strong. Second, formal remuneration and recruitment regulations in public services leave less discretion to adjust wages to labor market conditions – at least in the short and medium run. To be sure that our results are not driven by the public sector, we simply exclude it from our estimation sample. Third, the effects of labor demand changes on wages may differ noteworthy between qualification groups. Therefore we estimate the model *separately* for two qualification groups: (1) workers without completed apprenticeship training and (2) workers with completed apprenticeship training.[11] Finally, we drop workers with less than 3 observations to avoid estimation problems with fixed effects.[12] After all these selections, we have 368 316 remaining obervations from 62 797 unqualified workers and 2 100 974 observations from 236 070 workers with completed apprenticeship.

[11]As mentioned above, quantile regressions for the high qualified (college or technical college graduates) would be imprecise and unreliable due to censoring rates above 50 percent.

[12]In principle, we need at least two observations per person to identify its fixed effect. We raised this limit to 3 since fixed effects estimates become extremely imprecise otherwise and persons with less than three spells appear to be a quite selective group.

4 Estimation

The response in equation (1) is subject to censoring. As a consequence we can observe $w^*_{i,r,t}$ only if it is smaller than the corresponding censoring point in time period t — say C_t, where we assume that the latter depends on t in a non-stochastic manner (and hence is observable for all, even uncensored observations in the sample). What we observe is the dependent variable

$$w_{i,r,t} = \min\left\{C_t, \sum_{p=1}^{2} u_{r,t-p}\, b_p(\tau) + x_{i,r,t}\, g(\tau) + \alpha_i(\tau) + \delta_t(\tau) + \epsilon_{i,r,t}\right\} \quad (2)$$

The τ in parentheses denotes the dependence on the corresponding quantile with $0 < \tau < 1$, though due to the data limitations mentioned above, we estimate equation (2) only for quantiles $\tau \in \{0.15, 0.35, 0.55, 0.75\}$. Besides the economic reasons stated above, there are also statistical reasons to use quantile regression on (2), which are extensively discussed in Koenker (2005).[13]

Censored quantile regression has been introduced in two seminal papers by Powell (1984, 1986). Based on the model

$$Q_\tau(Y_i|x_i) = \min\{C_i, z_i\,\beta(\tau)\}$$

Powell suggested to minimize the objective function

$$\sum_i \rho_\tau\left(y_i - \min\{C_i, z_i\,\beta(\tau)\}\right) \quad (3)$$

where $\rho_\tau(\epsilon) = (\tau - 1(\epsilon \leq 0))$. Under weak regularity conditions, Powell's estimator has desirable large sample properties, but exhibits undesirable properties in small samples. In addition numerical optimization based on (3) is extremely cumbersome, even with powerful modern computers.

In order to avoid these problems, several two-step (e.g., Buchinsky & Hahn (1998) and Khan & Powell (2001)) estimators were proposed in the literature. It is straightforward to show that the Powell estimator uses only observations with uncensored prediction. The two-step estimators exploit this property by selecting the observations with uncensored prediction using binary choice models. Here we follow an ingenious suggestion of

[13]He states that "censoring ... has proven to be one of the most compelling rationales for the use of quantile regression in applied work".

Chernouzhukov & Hong (2002), who, building among others on the work of Buchinsky & Hahn (1998) and Khan & Powell (2001), propose a three-step estimation procedure which avoids the difficulties of Powell's estimator while reaching its asymptotic efficiency.

A brief outline of our adaption of the procedure will be given in the following (further details can be found in Chernouzhukov & Hong (2002)). For expositional brevity we subsume all regressors (unemployment, control variates and fixed effect dummies) in z and drop region and time indices.

Then the first step (logit) regression explaining not-censoring has the form

$$\delta_i = \dot{z}_i \, \gamma + \zeta_i \tag{4}$$

where δ_i is the indicator of not-censoring. The logit regressions do not include fixed individual effects.[14] Instead we try to explain censoring as good as possible by inclusion of many regressors (14 time dummies, 24 sector dummies, 8 region type dummies, a cubic polynomial in age, establishment size, establishment size squared, shares of high skilled workers in establishment and a foreigner dummy).

From this we generate the quantile regression estimation sample J_0 by sorting the predicted values (propensity scores) $\dot{z}_i \, \hat{\gamma}$ from the logit model and dropping the 20 percent with lowest propensity score. This appears to be a surefire choice (since only about percent of the original sample are censored). Those observations constitute a sub-sample where the quantile hyperplane $z_i \, \beta(\tau)$ lies below the censoring value C_i. Then, the second step consists of solving the *uncensored* quantile regression minimization problem

$$\sum_{i \in J_0} \rho_\tau(y_i - z_i \, \beta(\tau)) \tag{5}$$

Model (2) may appear rather parsimonious at a glance due to the small number of control variates, but is quite flexible and general, since all time-invariant (obsvervable *and* unobservable) factors influencing individual heterogeneity are captured by the fixed effects α_i. Note that our model effectively exploits district level deviations from national employment because of the inclusion of time dummies.

[14] As is well known, the conditional logit model eliminates the individual fixed effects required for the predicted propensity scores below and is therefore not useful in this context. A consistent fixed effects probit estimator is not available.

Even our 2 percent sample of the register data (IABS) is large. After all selections 2 100 974 records from 236 070 medium qualification workers and 364 258 records from 62 290 low qualification workers. Since simple transformations applied in OLS estimation (differencing or within-transformation) are not viable for quantile regression, all individual fixed effects have to be estimated directly. Though the development of interior point algorithms for constrained linear minimization problems has extended computational possibilities of quantile regression considerably, estimation of more than several hundred fixed effect coefficients for extremely large data sets remains infeasible even with modern powerful computers. As a makeshift we apply the m out of n bootstrap surveyed by Bickel, Götze, & Zwet (1997). The basic idea is to draw in every bootstrap replication m observations with replacement from the estimation sample where m is small compared to n. Then variances obtained in this way are rescaled (by assuming $\sqrt{n}$-consistency of the estimator) to infer standard errors for the base population. The crucial advantage of the approach for our application is that we have to estimate only m coefficients for the individual fixed effects in every bootstrap replication but exploit the whole sample to compute the coefficients. A disadvantage is that we implicitly assume normality for the rescaling of variances. Fortunately the bootstrap allows us to check this assumption by comparing the bootstrapped $m-$ sample coefficients with the normal density. The results of this exercise can be found in the appendix. They show only minor deviations between the bootstrapped kernel density estimates and the normal density.

As is well known, the standard (i.i.d.) bootstrap delivers biased standard errors if the residuals of the regression model are correlated (hidden by common shocks). In our application, such correalations may be likely caused by regional demand or productivity shocks. To obtain consistent standard deviations in this environment, we use a block bootstrap procedure [15] , i.e. we draw (for each bootstrap replication) first a district (with replacement) from the base sample and then a sample of size $0.02 \cdot m \cdot n_r/N$ from the workers in this district and all its direct neighbour districts (again with replacement). Here n_r denotes the number of workers in district r and N gross sample size. This sampling step (draw first a district and then workers from this district) is repeated until 2000 and persons are obtained for the low and medium skill group, respectively.

A final word of caution. Not too much is known about censored panel quantile regression models such as (2), since until now only a limited number of papers simultaneously addressed quantile regression, censoring, and

[15] See Fitzenberger (1997) for a lucid exposition of the procedure and its properties.

panel data. Recent works of Koenker (2004) and Lamarche (2006) deal with quantile regression analysis of fixed effect panel data models. Though from quite different perspectives, Honoré (1992) and Hu (2002) are, to the best of our knowledge, the only papers dealing with LAD regression of censored panel data models based on the results of Powell (1984) discussed before. The small and large sample properties of the procedure applied in this paper remain to be investigated in detail.

5 Results

The empirical model is estimated for low and medium qualification workers separately. Since censoring (below 2 percent) appears to be negligible for the low qualified workers, censoring is handled for this group simply by dropping the censored cells.

Table 1 contains estimates and bootstrapped standard errors of regression model (2). The table shows the point estimates of the effects of unemployment on wages together with their (sample size adjusted) standard errors and the corresponding measures for differences between the conditional quantiles.[16] (Coefficients for the control variables and the fixed effects are not reported to save space but available from the authors on request.)

To start with, consider the panel showing the results for the low qualified. To be on the save side we interpret only the regressions including 22 sector dummies since they deliver more conservative results. For this group the 15 percent quantile of wages responds with an approximate 0.01 percent decrease to a one percent increase of the unemployment rate. The response shrinks to 0.007, 0.007 and 0.008 percent when we move to the 35, 55 and 75 percent quantiles. Here the differences are not significant. The corresponding results for the medium qualification group are, however, more clear and estimated with greater precision. Here the effect shrinks from 0.01 to 0.009, 0.007 and 0.006 when we ascend from the 15 to the 75 percent quantile. This means that lower quantiles respond more strongly to regional unemployment than the higher ones, i.e. agrees with Reder's hypothesis.

[16] To check whether the convergence rule for standard errors is valid also for small samples and our fixed effects design, we run a small simulation study with $n = 100$ persons (with $T_i = 10$ observations for each person) and $m = 10$. (The results are available from the authors on request. Even for this tiny sample, $\sqrt{n}$-convergence is a good approximation.

Table 1: Effects of log unemployment on log wage quantiles from **individual fixed effects regressions**. dependent variable: log real wage

quantile	15	35	55	75
regressions without sector dummies				
low qualification				
effect	-0.016	-0.0095	-0.0052	-0.0074
(sd)	(0.004)	(0.003)	(0.003)	(0.003)
difference ($e_\tau - e_{15}$)	-	0.006	0.011	0.008
(sd)	-	(0.001)	(0.001)	(0.001)
medium qualification				
effect	-0.015	-0.012	-0.009	-0.007
(sd)	(0.001)	(0.001)	(0.001)	(0.001)
difference ($e_\tau - e_{15}$)	-	0.003	0.006	0.007
(sd)	-	(0.001)	(0.001)	(0.001)
regressions including 22 sector dummies				
low qualification				
effect	-0.010	-0.007	-0.007	-0.008
(sd)	(0.004)	(0.004)	(0.004)	(0.004)
difference ($e_\tau - e_{15}$)	-	0.003	0.003	0.008
(sd)	-	(0.003)	(0.002)	(0.003)
medium qualification				
effect	-0.010	-0.009	-0.007	-0.006
(sd)	(0.001)	(0.001)	(0.001)	(0.001)
difference ($e_\tau - e_{15}$)	-	0.003	0.006	0.007
(sd)	-	(0.001)	(0.001)	(0.001)

effects are computed as sum of coefficients $b_1(\tau) + b_2(\tau)$ and based on 500 block bootstrap replications. All estimates include year dummies, establishment size, and establishment size squared.

As a final check we rerun the regressions with 328 fixed district effects (instead of the individual fixed effects)[17]. Comparison wit table 1 reveals no qualitative change. But the differences are now larger and significant for both qualification groups.

[17]These specifications include now age and age squared too.

Table 2: Effects of log unemployment on log wage quantiles from **district fixed effects regressions**. dependent variable: log real wage

quantile	15	35	55	75
	low qualification			
effect	-0.009	-0.005	-0.003	-0.003
(sd)	(0.002)	(0.002)	(0.002)	(0.002)
difference ($e_\tau - e_{15}$)	-	0.004	0.007	0.006
(sd)	-	(0.0015)	(0.002)	(0.002)
	medium qualification			
effect	-0.014	-0.004	0.003	0.006
(sd)	(0.005)	(0.006)	(0.004)	(0.005)
difference ($e_\tau - e_{15}$)	-	0.006	0.011	0.018
(sd)	-	(0.004)	(0.005)	(0.006)

effects are computed as sum of coefficients $b_1(\tau) + b_2(\tau)$ and based on 250 block bootstrap replications. All estimates include year dummies, 22 sector dummies, age, age squared, establishment size, establishment size squared and dummies for all districts in the estimation sample.

6 Conclusion

To summarize: our regressions show that lower quantiles of the wage distribution respond more strongly to labor demand changes than the upper part. All in all our results are suggestive for the Reder hypothesis or the presence of efficiency wage effects. However, as mentioned in the introduction, a strong interpretation of the results in favor of the Reder effect rests on the additional identifying assumption of approximately equal labor demand changes over the wage distribution. Stronger responses in the lower part of the wage distribution may be caused either by adjustment of hiring standards *or* by larger cyclical variation of labor demand changes for low wage workers. But both cases are suggestive for the presence of efficiency wage effects. On the other hand, centralized union bargaining models implying opposite effects on the wage structure are rejected by our results.

Regarding the econometric issues of the paper our experience shows that smart bootstrapping makes fixed effects quantile regression feasible even for extremely large data sets.

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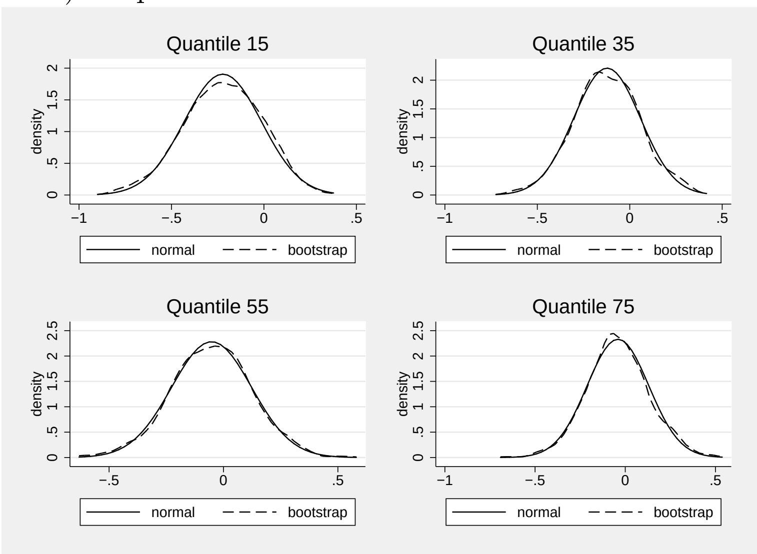
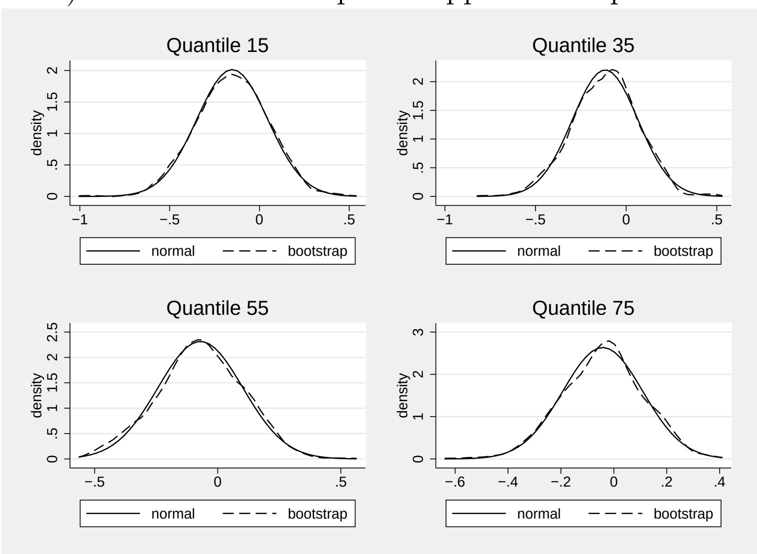
A Comparison of Bootstrap Small Sample Distributions with Normal Distribution

Here we compare the bootstrapped effects of unemployment on wage quantiles with normal densities. Kernel density plots are based on the $m-$ sample bootstrap coefficients (i.e. before sample size adjustement).

a) Unqualified workers



b) Workers with completed apprenticeship



B Preliminary Investigation of the Composition Problem

In the text above we argue that the two-step procedure in Ammermueller et al. (2007) is prone to composition bias if the relation between individual employment and district level unemployment depends on the position of the worker in the wage distribution. Here we investigate this issue in more detail. We generate a binary indicator variable y taking on value 1 if a worker drops out of our estimation sample (and 0 otherwise). This indicator is regressed (using probit and linear probability models) on the district level lagged log unemployment rate, the lagged quantile of the worker in the year before, the interaction term of unemployment and the quantile, and a set of control variables (linear and squared age, linear and squared establishment size, linear and squared share of high qualified in establishment, a foreigner dummy, sector dummies, year dummies and district dummies).

Note that the dependent variable is asymmetric, i.e. we investigate the probability of dropping out of the estimation sample but not the probability of entering. The reason is that we do not observe wages for persons before entering the sample and that the wages for the year of entrance or after are endogenous (depend on the duration of the out-of-sample period). The same problem occured if we used an indicator of the form

$$y = \begin{cases} 1 & \text{if worker is in the sample} \\ 0 & \text{otherwise} \end{cases}$$

as dependent variable of our probit estimate.

Formally we estimate the model

$$P(y_{i,t} = 1) = b_0 + b_u\, u_{i,t-1} + b_q\, q_{i,t-1} + b_{uq}\, u_{i,t-1}\ q_{i,t-1} x_{i,t}\, \beta + \epsilon_{i,t}$$

with district level log unemployment rate u and wage quantile q

The interaction effect $b_{uq} = \frac{\partial P(y=1)}{\partial u \partial q} = \frac{\partial}{\partial q}\left[\frac{\partial P(y=1)}{\partial u}\right]$ gives the desired information. If employment of workers at low wage quantiles depends more strongly on district level unemployment, it must have negative sign.

The following table shows the result of this exercise. The sign of the interaction term is negative and highly significant irrespective of the set of control variables.

Table 3: Marginal effects of district level log unemployment on individual probability of leaving our estimation sample.

regressor	low qualification			medium qualification		
age	0.00018	0.00033	0.00031	-0.00385	-0.00413	-0.00372
(sd)	0.00024	0.00024	0.00024	0.00012	0.00012	0.00012
age^2	-0.00001	-0.00001	-0.00001	0.00004	0.00004	0.00004
(sd)	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000
estab. size	0.00051	0.00056	0.00063	-0.00065	-0.00016	-0.00063
(sd)	0.00011	0.00012	0.00016	0.00005	0.00006	0.00007
estab. size^2	0.00000	-0.00000	-0.00001	0.00002	0.00001	0.00001
(sd)	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000
share hiQ	-0.10974	-0.06884	-0.11584	-0.03914	-0.02733	-0.05539
(sd)	0.01414	0.01508	0.01469	0.00428	0.00459	0.00440
share hiQ^2	0.41423	0.28566	0.40680	0.15891	0.08263	0.16732
(sd)	0.04819	0.04983	0.04912	0.01094	0.01147	0.01111
foreign	0.00676	0.00709	0.00468	0.00152	0.00031	-0.00140
(sd)	0.00074	0.00075	0.00078	0.00064	0.00064	0.00065
$\ln(u)$	0.02839	0.02224	0.01005	0.01398	0.01135	0.00459
(sd)	0.00198	0.00199	0.00295	0.00090	0.00090	0.00135
q_{-1}	-0.16068	-0.14104	-0.16811	-0.13135	-0.12413	-0.13240
(sd)	0.00786	0.00784	0.00813	0.00342	0.00342	0.00352
$ln(u)\,q_{-1}$	-0.03411	-0.02892	-0.03555	-0.02132	-0.01956	-0.02006
(sd)	0.00292	0.00292	0.00303	0.00130	0.00130	0.00134
year dummies	yes	yes	yes	yes	yes	yes
sector dummies	no	yes	yes	no	yes	yes
district dummies	no	no	yes	no	no	yes

robust standard errors below coefficients in parentheses.