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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 using German employment register data and a censored panel quantile regression approach. 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.

JEL codes: J310, J410, C240
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 is an alternative and extension 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.

Note however, that 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.

Note however, that the empirical implications generated by the hiring standards adjustment hypothesis are different from 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). Furthermore the model does not include fixed effects for individuals or districts. Nevertheless the difference in results remains puzzling.

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 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.[2] 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

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

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

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.[3] 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').[4] To the best of our knowledge, all 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) is Büttner & Fitzenberger (2003).

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.[5]

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.

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

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

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

We translate this into an empirical model of the form

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

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[6], u is the unemployment rate, α is an individual specific fixed effect, δ is a time-effect (year dummy), and x contains a set of control variates (age squared[7], establishment size and establishment size squared).

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 recessions, this group will shrink more than the rest of the sample during recessions.[8] 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

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

[7]the linear term of age is collinear to the time dummies and therefore neither feasible nor required as regressor.

[8]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.

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.[9] Fortunately, the bias caused by inter-regional wage dispersion 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 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).

[9] 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 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.

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 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. Though censoring is moderate for 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.[10] Finally, we drop workers with less than 3 observations to avoid estimation problems with fixed effects.[11] After all these selections, we have 368 316 remaining obervations from 62 797 unqualified workers and 2 100 974 observations from 23 6070 workers with completed apprenticeship.

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

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

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).[12]

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

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

[10]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.

[11]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 und persons with less than three spells appear to be a quite selective group.

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

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 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.[13] 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

[13] 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.

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.

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.[14] 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.

[14]The standard errors reported in the current version of the paper are computed drawing persons randomly from the base population. If individuals in a district are hit by common shocks, inference should account for this by drawing blocks of persons from districts (see Fitzenberger (1997) for a lucid exposition of the procedure and its properties.) This will be implemented in future versions.

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 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: Effects of unemployment on log wage quantiles. dependent variable: log real wage

quantile	15	35	55	75
	low qualification			
effect	-0.228	-0.138	-0.074	-0.073
(sd)	(0.037)	(0.031)	(0.029)	(0.028)
difference $(e_\tau - e_{15})$	-	0.090	0.153	0.154
(sd)	-	(0.019)	(0.025)	(0.032)
	medium qualification			
effect	-0.167	-0.129	-0.073	-0.046
(sd)	(0.014)	(0.012)	(0.011)	(0.011)
difference $(e_\tau - e_{15})$	-	0.042	0.096	0.122
(sd)	-	(0.008)	(0.015)	(0.023)

effects are computed as sum of coefficients $b_1(\tau)+b_2(\tau)$ and based on 200 bootstrap replications. All estimates include year dummies, age squared, establishment size, establishment size squared and individual dummies.

Table 1 contains estimates and bootstrapped standard errors of regression model (2). The table contains 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.[15] (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 upper panel showing the results for the low qualified. For this group the 15 percent quantile of wages responds with a 0.23 percent decrease to a one percentage point increase of the unemployment rate. The response shrinks to 0.14, 0.07 and 0.07 percent when we move to the 35, 55 and 75 percent quantiles. The standard errors for the differences of effect below show that the deviations are highly significant. Results are quite similar for the medium qualification group but somewhat smaller in absolute value and more precise due to the larger sample size. This means that lower quantiles respond more strongly to regional unemployment than the higher ones, i.e. agrees with Reder's hypothesis.

As a final rough check of the specification one can compute the mean over the quantile effects and compare it with the effects from wage curve estimates.[16] This mean is quite similar to the value of about -0.1 reported in most wage curve estimates.

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

[15] 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.

[16] For a survey of wage curve estimates see Blanchflower & Oswald (2005). Our estimated quantiles are not symmetric around the mean. Therefore we cannot expect to obtain the mean exactly.

other hand, centralized union bargaining models implying opposite effects on the wage structure are rejected by our results.

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Comparison of 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).