QUANTILE REGRESSION FOR DURATION DATA: A REAPPRAISAL OF THE PENNSYLVANIA REEMPLOYMENT BONUS EXPERIMENTS

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Abstract. We argue that quantile regression methods can play a constructive role in the analysis of duration (survival) data offering a more flexible, more complete analysis than is typically available with more conventional methods. We illustrate the approach with a reanalysis of the data from the Pennsylvania Reemployment Bonus Experiments. These experiments, conducted in 1988-89, were designed to test the efficacy of cash bonuses paid for early reemployment in shortening the length of insured unemployment spells

1. Introduction

Duration models play an increasingly important role in applied econometrics, and have proven to be a fertile field for the growth of semiparametric methods. Chaudhuri, Doksum and Samarov (1997) have recently stressed the usefulness of the quantile regression formulation, Koenker and Bassett (1978), for survival analysis arguing that it provides a unifying approach for transformation models more generally. Powell (1986) extended quantile regression methods to censored regression models. Horowitz and Neumann (1987) and Fitzenberger (1997) illustrate the approach with analyses of durations of employment spells.

In this paper we briefly describe the link between quantile regression and the transformation model formulation of survival analysis. We stress a general formulation of experimental treatment effects introduced by Lehmann (1974) and Doksum (1974) that is particularly well-adapted to quantile regression in survival analysis. These introductory sections draw heavily on earlier work appearing in Koenker and Geling (1999). We then describe the Pennsylvania experiment and the model employed to analyze it. The analysis and interpretation is presented in Section 6. Some concluding remarks appear in the final section.

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2. Survival Analysis and the Transformation Model

Doksum and Gasko (1990) provide a valuable survey of survival analysis emphasizing the fundamental link with binary response models and the transformation model

$$h(T_i) = x_i'\beta + u_i. \tag{1}$$

Many important parametric and semiparametric survival models may be expressed in this form: some monotone transformation of an observed survival time, T_i, represented as a linear predictor (single-index) plus *iid* error.

In the Cox proportional hazard model, undoubtedly the leading example, we have

$$\log \lambda(t|x) = \log \lambda_0(t) - x'\beta \tag{2}$$

and thus, expressing the conditional survival function $S(t|x)$ in terms of the integrated baseline hazard

$$\Lambda_0(t) = \int_0^t \lambda_0(s)ds$$

we have,

$$\log(-\log S(t|x)) = \log \Lambda_0(t) - x'\beta. \tag{3}$$

Now if we fix t, and consider the analysis of the binary response variable

$$Z_i = I(T_i > t)$$

this would lead to the complementary log-log model and the term $\log \Lambda_0(t)$ would be absorbed into the intercept component of the linear predictor $x'\beta$.

More generally, suppose we write (3) as

$$G^{-1}(S(t|x)) = h(t) - x'\beta$$

Then since,

$$\begin{aligned} P(h(T) > t|x) &= P(T > h^{-1}(t)|x) \\ &= S(h^{-1}(t)|x) \\ &= G(t - x\beta), \end{aligned}$$

this is equivalent to the transformation model

$$h(T) = x'\beta + u \tag{4}$$

where u has distribution function G. In the case of the Cox model this yields

$$\log \Lambda_0(T) = x'\beta + u$$

with u *iid* with distribution function $F(u) = 1 - \exp(-\exp(u))$. Specializing yet further, if the baseline hazard is of the Weibull form, so

$$\log \Lambda_0(t) = \gamma \log t - \alpha,$$

then, again incorporating the constants into the linear predictor, we obtain the accelerated failure time model,

$$\log T = x'\beta + u.$$

Under the strict Weibull assumption this model can be estimated by maximum likelihood. Or, relaxing the Weibull assumption, it is often estimated by conventional least squares methods.

An important extension of the Cox proportional hazard model is the class of frailty models. Such models involve some additional random component in the intensity (hazard) formulation so, for example, we might replace (2) by

$$\log \lambda(t|x,v) = \log \lambda_0(t) - x'\beta - v$$

where now v represents some source of "unobserved heterogeneity", due perhaps to omitted covariates, that shifts the baseline hazard. Assuming *iid* behavior of v, we are led back to the transformation model

$$h(T) = x'\beta + u + v.$$

See Andersen, *et al* (1994) and Horowitz (1998) for further discussion of this class of models.

The common feature of all the foregoing models is the *iid* error assumption, which asserts that for some appropriate choice of the transformation $h(\cdot)$ we can express the transformed survival times $h(T)$ as a pure location shift model in the covariates x. This formulation is perfectly natural if we are intent on representing the effect of the covariates in the p-vector β. However, it imposes rather drastic constraints on the way that covariates are permitted to influence the survival distribution.

To see this consider the implications of the *iid* error assumption on the family of conditional quantile functions. Given the transformation model (4) we may write the conditional quantile functions of $h(T)$ as

$$Q_{h(T)}(\tau|x) = x'\beta + F_u^{-1}(\tau)$$

for $\tau \in (0,1)$. The only effect of the covariates is to shift the location of the distribution. The scale and shape of the distribution is entirely determined by the distribution of u. It follows immediately from the fact that for any monotone transformation, $h(\cdot)$,

$$P(h(T) \le t) = P(T \le h^{-1}(t)),$$

that we can write the conditional quantile functions of T, itself as,

$$Q_T(\tau|x) = h^{-1}(x'\beta + F_u^{-1}(\tau)).$$

This may appear, at first consideration, a reasonably flexible specification. It is not.

As an alternative to this location shift model we propose to consider a family of linear-in-parameters quantile regression models for the transformed survival time $h(T)$,

$$Q_{h(T)}(\tau|x) = x'\beta(\tau),$$

where, potentially, all of the parameters of the p-vector $\beta(\tau)$ now depend upon the specified quantile, τ. The prior models constitute special cases in which all of the dependence on τ is concentrated in the intercept coefficient, leaving the $p-1$ slope entirely free of τ.

By allowing the slope coefficients of $\beta(\tau)$ to depend upon τ, we can introduce a wide variety of forms of heterogeneity in the conditional distribution of $h(T)$ over the "design-space" of the covariates. A particularly simple, yet important, special case is the family of linear location-scale models

$$h(T_i) = x_i'\alpha + (x_i\gamma)u_i$$

where u_i is taken to be *iid* from F. In this model we have the linear family of conditional quantile functions

$$\begin{aligned} Q_{h(T)}(\tau|x) &= x'\alpha + (x'\gamma)F_u^{-1}(\tau) \\ &= x'\beta(\tau) \end{aligned}$$

where $\beta(\tau) = \alpha + \gamma F_u^{-1}(\tau)$. In this case all of the coordinates of $\beta(\tau)$ depend upon τ in the same way up to a location *and scale* shift. This model captures a variety of models of heteroscedasticity but it is still highly restrictive, since the shape of the conditional density of $h(T)$ is the same for all values of x.

3. The Quantile Treatment Effect

To motivate a more flexible formulation of the survival model we reconsider a general formulation of the two-sample treatment response model introduced by Lehmann (1974),

> "Suppose the treatment adds the amount $\Delta(x)$ when the response of the untreated subject would be x. Then the distribution G of the treatment responses is that of the random variable $X + \Delta(X)$ where X is distributed according to F."

Doksum (1974) provides a detailed axiomatic analysis of this formulation, showing that if we define $\Delta(x)$ as the "horizontal distance" between F and G at x, so

$$F(x) = G(x + \Delta(x))$$

then $\Delta(x)$ is uniquely defined and can be expressed as

$$\Delta(x) = G^{-1}(F(x)) - x.$$

Changing variables, so $\tau = F(x)$ we obtain what we will call *the quantile treatment effect*,

$$\delta(\tau) = \Delta(F^{-1}(\tau)) = G^{-1}(\tau) - F^{-1}(\tau).$$

In the two sample setting this quantity is naturally estimable by

$$\hat{\delta}(\tau) = \hat{G}_{n_1}^{-1}(\tau) - \hat{F}_{n_0}^{-1}(\tau)$$

where $\hat{G}_{n_1}, \hat{F}_{n_0}$ denote the empirical distribution functions of the treatment and control observations respectively, and $\hat{F}_n^{-1} = \inf\{x | \hat{F}_n(x) \geq \tau\}$, as usual. Since we cannot observe subjects in *both* the treated and control states – and this platitude may be regarded as the fundamental "uncertainty principle" underlying the "causal effects" literature – it seems reasonable to regard $\delta(\tau)$ as a complete description of the treatment effect.

Of course, there is no way of really *knowing* whether the treatment operates in the way prescribed by Lehmann. In fact, the treatment may make otherwise weak subjects especially robust, and turn the strong to jello. All we can observe from the experimental evidence is the difference between the two *marginal* survival distributions, so it is natural to associate the treatment effect with this difference.[1] The quantile treatment effect provides the unexpurgated version.

Of course, it is possible that the two distributions differ only by a location shift, so $\delta(\tau) = \delta_0$, or that they differ by a scale shift so $\delta(\tau) = \delta_1 F^{-1}(\tau)$ or that they differ by a location and scale shift so $\delta(\tau) = \delta_0 + \delta_1 F^{-1}(\tau)$. But these hypotheses are both nicely nested within Lehmann's general framework.

It is also worth noting that the Lehmann quantile treatment effect (QTE) is intimately tied to the conventional two sample QQ-plot. The function $\hat{\Delta}(x) = G_n^{-1}(F_m(x)) - x$ is exactly what is plotted in the QQ-plot. This connection is further developed by Doksum and Sievers (1976).

4. The Quantile Regression Model

In the two sample treatment-control model the QTE leads naturally to the quantile regression model. Let $y_1, \ldots, y_n$ denote the full sample of treatment and control observations, and let $x_i = 1$ for treatment observations and $x_i = 0$ for controls, then

$$(\hat{\delta}_0(\tau), \hat{\delta}_1(\tau)) = \mathrm{argmin}_{\delta \in \Re^2} \sum \rho_\tau(y_i - \delta_0 - \delta_1 x_i)$$

for $\rho_\tau(u) = u(\tau - I(u < 0))$ is easily shown to yield

$$\hat{\delta}_0(\tau) = \hat{F}_{n_0}^{-1}(\tau)$$

[1]This is also the view recently espoused by Abadie, Angrist, and Imbens (1999) who note that in contrast with average treatment effects, where average differences equal differences in averages, the difference in the quantiles of the marginal distributions is not the same as the quantile of the difference, $Y_1 - Y_0$, between treatment and control response. They comment further, "Although the latter may also be of interest, we focus on the marginal distributions of potential outcomes because identification of the distribution of $Y_1 - Y_0$ requires much stronger assumptions and because economists making social welfare comparisons typically use differences in distributions and not the distribution of differences for this purpose."

and

$$\hat{\delta}_1(\tau) = \hat{G}_{n_1}^{-1}(\tau) - \hat{F}_{n_0}^{-1}(\tau).$$

In the case of p distinct treatments, we can write

$$Q_{y_i}(\tau|x_{ij}) = \delta_0(\tau) + \sum_{j=1}^{p} \delta_j(\tau) x_{ij}$$

where $x_{ij} = 1$ if observation i received treatment j and $x_{ij} = 0$ otherwise. And again the quantile regression formulation yields an optimization problem that is separable in the $p+1$ parameters and the $\delta_j(\tau)$'s may be expressed as the difference between quantile function of the control response evaluated at τ and the corresponding τth quantile of the jth treatment group.

When the treatment is continuous, as for example in dose response studies, then it is natural to consider the hypothesis that the effect is linear at each quantile and in the simplest bivariate case we may write,

$$Q_{Y_i}(\tau|x_i) = \beta_0(\tau) + \beta_1(\tau)x_i.$$

We assume thereby, of course, that the treatment effect at the τ quantile, $\beta(\tau)$ of changing x from x_0 to $x_0 + \Delta$, is the same as the effect of an alteration from x_1 to $x_1 + \Delta$. But in contrast to the classical regression model we do not assume that this effect is necessarily the same across the various quantiles.

In survival analysis it is natural to associate the robustness (or frailty) of particular subjects with their quantile in the survival distribution. By assumption this "propensity for longevity" is the same whether the subject is a treatment observation or a control. In either case, the subject is assumed to fall into the distribution at the same quantile. This may seem strange, but again we emphasize that unless subjects are observable in both the control and treatment states, there is no loss of generality involved; the implication is untestable.

5. The Pennsylvania Reemployment Bonus Experiments

The current framework of the U.S. unemployment insurance (UI) system provides short-term monetary assistance to the involuntarily unemployed. A frequent criticism of the system has been that the unemployment insurance benefit acts as a disincentive for job-seekers and prolongs the duration of unemployment spells. During the 1980's several controlled experiments tested alternative compensation schemes for UI. In these experiments, UI claimants were offered a cash bonus if they found a job within some specified period of time and if the job was retained for some specified duration. The question was: would the promise of a monetary lump-sum benefit provide a significant inducement for more intensive job-seeking?

The first two experiments were conducted in Illinois in 1984 and 1985 and are described in detail by Woodbury and Spiegelman (1987). In the first experiment, a random sample of new claimants were told that they would receive a bonus of \$500 if they found full-time employment within 11 weeks of filing their initial claim, and if they retained their new job for at least 4 months. In the second experiment, a random sample of new claimants were told that *their prospective employer* would be entitled to a bonus of \$500 provided that the claimants were able to find a job and keep it under the same conditions as the previous experiment. The two treatments were tested against a control group of claimants who followed the usual rules of the Illinois UI system.

The Illinois experiments, and especially the bonus offer made directly to claimants, provided a very encouraging initial indication of the incentive effects of such policies. They showed that bonus offers could result in a significant reduction in the duration of unemployment spells and consequently of the regular amounts paid by the state to UI beneficiaries. This finding led to further "bonus experiments" in the states of New Jersey, Pennsylvania and Washington with a variety of new treatment options. An excellent review of the experiments, some general conclusions about their efficacy and a critique of their policy relevance can be found in Meyer (1995, 1996). In this paper we will focus more narrowly on a reanalysis of data from only the Pennsylvania Reemployment Bonus Demonstration described in detail in Corson *et al.* (1992).

5.1. **Treatment Design.** The Pennsylvania experiments were conducted by the U.S. Department of Labor between July 1988 and October 1989. During the enrollment period, claimants who became unemployed and registered for unemployment benefits in one of the selected local offices throughout the state were *randomly assigned* either to a control group or one of the six experimental treatment groups. In the control group the existing rules of the unemployment insurance system applied. Individuals in the treatment groups were offered a cash bonus if they became reemployed in a full-time job, working more than 32 hours per week, within a specified period we will call the qualification period. In addition, to qualify for the bonus, claimants were required to work in the new job continuously for at least 16 weeks, or they were allowed to change jobs as long as the transition took place within a period of 5 days. The latter requirements were imposed to discourage cases of fraudulent hiring for purposes of obtaining the bonus, and to avoid the possibility of bonus payments to seasonal workers.

Two bonus levels were tested. The lower bonus was three times the weekly benefit amount, and the higher bonus was six times the weekly benefit. Bonuses were tied to the weekly benefit rather than offering a fixed amount as in the Illinois experiment, because it was felt that such a policy yielded more uniform incentives across individuals. It was also thought that such a system was politically more feasible than the fixed bonus scheme. The low bonus averaged \$500 and the high bonus averaged \$997. The two levels were chosen on the basis of both the Illinois and New Jersey experiences.[2]

[2]In Illinois study the bonus was set at \$500 while in the New Jersey case it started with 10 times the weekly benefit and declined by 10% each week.

Two qualification periods were considered: a short period of 6 weeks and a longer one of 12 weeks. The long qualification period was close to that studied in Illinois and New Jersey. The choice of the shorter period was intended to test the sensitivity of the treatment effect to alternative specifications of the qualification periods.

The bonus levels and qualification periods of the six treatment groups are described in Table 5.1. All of the treatments, except the last one, involved a voluntary option of attending a workshop designed to aid job search. However, less than three percent of eligible participants attended the workshop so we follow the practice established by prior analysts of ignoring the workshop option. In effect this enables us to pool treatments 4 and 6. Four of the treatments were created by the combination of a bonus amount and a qualification period plus the offer of the workshop. The fifth treatment included an initially high, but declining bonus over the period of 12 weeks plus the optional workshop. The sixth treatment combined the high bonus with the long qualification period without the workshop.

TABLE 5.1. Treatment Groups

Group	Bonus Amount	Qualification Period	Workshop Offer
Controls	0	0	No
Treatment 1	Low	Short	Yes
Treatment 2	Low	Long	Yes
Treatment 3	High	Short	Yes
Treatment 4	High	Long	Yes
Treatment 5	Declining	Long	Yes
Treatment 6	High	Long	No

Note: The low bonus was 3 times UI weekly benefit amount, the high benefit was 6 times this amount. The declining bonus declined from 6 times the weekly benefit to zero, over a 12 week period. The short qualification period was 6 weeks, and the long period was 12 weeks.

5.2. **Sample Design.** The Pennsylvania experiments were designed to answer two questions. Could "policy relevant," *i.e.* politically feasible, treatments yield detectable cost savings to existing UI benefit programs? And how sensitive are program costs to various elements of the treatment design? For a more detailed description of the design goals one can consult Corson *et al.* (1992). Based on these objectives, as well as prior estimates of the magnitude of the response to the bonus offers, and a budget constraint for the experiment, a formal sample allocation model was developed that fulfilled the goals. The design provided 3,000 control and 10,120 treatment plan members, allocated to the specific treatments as shown in Table 5.2 in the column headed "Target n".

Table 5.2. Target, Collected and, Analysis Sample Sizes

Groups	Target n	Collected n	Analysis n
Control	3,000	3,392	3,354
Treatment 1	1,030	1,395	1,385
Treatment 2	2,240	2,456	2,428
Treatment 3	1,740	1,910	1,885
Treatment 4	1,590	1,771	1,745
Treatment 5	1,740	1,860	1,831
Treatment 6	1,780	1,302	1,285
Total	13,120	14,086	13,913

The sample was drawn randomly from claimants at twelve Job Services (JS) offices located throughout the state of Pennsylvania. The limited selection of sites constituted a compromise between the need to obtain a fairly large sample that could accurately reflect the demographic and occupational characteristics of the state, and the need for an easy monitoring and low operational cost of the study.

Effort was made to select twelve local offices which were representative of the insured unemployed population of Pennsylvania. More specifically, the state was divided in eight UI/JS regions. One or more clusters of local offices were formed within each region according to average duration of UI receipt. This process produced twelve clusters of approximately equal size UI caseloads. Finally, one office was selected randomly from each cluster to participate in the demonstration. The twelve Job-Service offices chosen were: Coatesville, Philadelphia-North, Philadelphia-Uptown, Reading, Lancaster, Lewistown, Butler, Connellsville, McKeesport, Erie, Pittston and Scranton. Corson *et al* (1992) comment, " UI claimants were selected randomly from claimants at local offices throughout Pennsylvania. The most cost-effective way to meet this objective was first to select a random sample of local UI/JS offices, and then to select a random sample of UI claimants from each of the selected offices. This process was undertaken in a manner which ensured that each eligible claimant in the state had an equal probability of selection into the demonstration sample."

Several criteria were imposed on potential UI claimants to determine their eligibility in the experiment. To be selected and assigned to one of the six treatments or to the control group, an individual had to:

- file a claim in one of the selected offices between July, 1988 and October, 1989,
- file a non-transitional claim,
- indicate no union or employer attachment,
- apply for benefit starting no more than 2 weeks before their selection day,
- be separated from their old job for reasons other than a labor dispute.

These eligibility criteria were imposed to increase the homogeneity of the sample and thus ensure that possible differences in the response could be attributed primarily to variation in treatment. Claimants who filed for a transitional claim were excluded because of the likelihood of a previous job offer. For the same reason there was exclusion from the experiment of individuals who indicated that it was possible they could find a new job through a union channel rather than the market, or if they were waiting for some definite recall within 60 days from their former employer. This category of claimants was very unlikely to respond to a bonus offer by searching for a new job intensively. The bonus payments would simply constitute a "windfall" gain for them. The fourth eligibility criterion was established to attain the operational goal of the program to offer bonuses to claimants as soon as possible after they became eligible for UI. On the other hand, the Pennsylvania UI system permits back-dating applications as long as claimants had been eligible for benefits during previous weeks. Requiring the unemployed not to have been separated from the most recent employer due to a labor dispute was dictated from the need to conduct a test for the effectiveness of job-search services; state and federal regulations prohibit the provision of such services to such claimants.

5.3. **Data.** The final collected sample was the result of fifty-two weekly sub-samples selected in all twelve offices beginning on October 26, 1988. Prior to that date, fifteen weekly sub-samples were drawn from the Pittston site for a pilot test of all operations, which are also included in the final "collected-sample". Thus, the enrollment period for the experiment started July 1988 and ended October 1989. The design target was to identify and select 13,120 claimants with each site contributing roughly 1,100 individuals in total and a weekly target of 21 claimants per site. However, since some claimants who initially apply for benefits do not return to a local office to file further, a larger sample was selected to achieve the desired sample size for analysis. Thus, a sample ranging from 22 to 40 claimants was selected at each office per week, depending on the historical experience. Overall, 15,005 individuals were initially selected to participate in the demonstration. A total of 14,086 individuals filed for a week of UI and were included in the study. Table 5.2 presents the distribution of the final sample by treatment group under the header "Collected n". Missing values for certain variables that are needed as covariates during our data analysis stage necessitated that we restrict our attention to a total of 13,913 subjects; the last column of Table 5.2 presents the allocation of our analysis sample over the control and the six experimental groups.

Table 5.3 presents the distribution of claimants of each treatment group by quarter of entry into the experiment. An examination of Table 5.3 confirms two interventions that took place during the enrollment period. One change was dictated by the low participation rate in the job-search assistance services provided along with the group of treatments 1 to 5. As previously noted, the attendance in workshop was less than 3% which made the fourth and the sixth treatments indistinguishable. Therefore, as of July 1989–four months before the end of the experiment–individuals who would have been assigned to treatment 6 were assigned to the other treatments. A second

Table 5.3. Distribution of claimants in each Group by Selection time

Selection Quarter	Treatment Groups						
	Control	1	2	3	4	5	6
Q3.1988	1.2	1.3	1.3	1.2	1.4	1.5	1.9
Q4.1988	20.5	17.0	19.4	21.0	20.2	18.0	31.5
Q1.1989	23.7	20.1	23.4	21.9	23.3	21.2	35.7
Q2.1989	22.0	20.5	22.8	22.2	23.7	23.3	30.9
Q3.1989	25.6	22.9	27.3	25.3	26.4	25.4	0.0
Q4.1989	7.0	18.3	5.9	8.5	5.0	10.6	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0

Note: Calculations are based on the Analysis Sample of 13,913 observations. Columns may not sum exactly to 100% due to rounding.

change was made because preliminary demonstration results showed that treatment 1 had a larger than expected effect. Initially only a small proportion of the total sample was assigned to this treatment due to its perceived low policy significance. Beginning October 1989, experimenters increased its sample. This change is reflected in the relatively high percentage, 18.3%, of entries during the last quarter.

A detailed description of the characteristics of claimants under study is presented in Table 5.4 which has information on age, race, gender, number of dependents, location in the state, existence of recall expectations, and type of occupation. The table shows their distribution over the seven groups and their totals in the last column. Standard χ^2 tests for nonrandomness of the allocations to the 7 treatments for each of the covariates fall well within conventional confidence limits, confirming the success of the randomization procedure. Categorical variables related to these characteristics are used in our modeling. More specifically these are:

young: 1 if the claimant's age is less than 35 and 0 otherwise.

old: 1 if the claimant's age is more than 54 and 0 otherwise.

black: 1 if the claimant is black and 0 otherwise.

hispanic: 1 if the claimant is hispanic and 0 otherwise.

female: 1 if the claimant is female and 0 otherwise.

recall: 1 if the claimant answered "yes" when asked if he/she had any expectation to be recalled to his/her prior job.

dependents: indicates the number of dependents of the claimant. Coded 0, 1, or 2 if the number of dependents is 2 or greater.

TABLE 5.4. Characteristics of claimants by Group

	Percentage in Treatment Groups							
	Control	1	2	3	4	5	6	Total
Age								
< 35	0.238	0.097	0.176	0.141	0.130	0.129	0.090	7556
$(35-54)$	0.245	0.103	0.175	0.128	0.117	0.133	0.100	4872
> 54	0.246	0.102	0.166	0.135	0.129	0.142	0.079	1485
Race								
White	0.240	0.101	0.175	0.134	0.125	0.132	0.094	11704
Black	0.251	0.089	0.173	0.143	0.132	0.130	0.082	1623
Hispanic	0.235	0.109	0.180	0.146	0.093	0.138	0.099	506
Other	0.238	0.088	0.138	0.125	0.225	0.100	0.088	80
Recall Expectation								
Yes	0.240	0.084	0.166	0.135	0.132	0.117	0.126	1512
No	0.241	0.101	0.176	0.136	0.125	0.133	0.088	12401
Gender								
Male	0.240	0.099	0.177	0.135	0.126	0.130	0.094	8318
Female	0.243	0.101	0.171	0.136	0.125	0.134	0.090	5595
Dependents								
0	0.243	0.102	0.175	0.135	0.126	0.130	0.089	10010
1	0.232	0.093	0.182	0.146	0.119	0.130	0.098	1628
2	0.240	0.093	0.168	0.131	0.127	0.138	0.103	2275
Location								
lusd[1]	0.230	0.099	0.181	0.141	0.131	0.129	0.090	3693
husd[2]	0.241	0.099	0.175	0.127	0.120	0.141	0.098	3086
muld[3]	0.248	0.102	0.170	0.138	0.124	0.128	0.090	6094
huld[4]	0.242	0.092	0.177	0.126	0.131	0.135	0.097	1040
Occupation								
durable manuf	0.245	0.103	0.174	0.138	0.120	0.134	0.086	2068
nondurable manuf	0.235	0.097	0.179	0.132	0.130	0.134	0.092	1525
other	0.241	0.099	0.174	0.135	0.126	0.131	0.094	10320
duration								
week 1	0.233	0.094	0.184	0.140	0.128	0.134	0.087	2491
week 2	0.211	0.103	0.164	0.156	0.133	0.131	0.101	900
week 27	0.245	0.111	0.173	0.131	0.128	0.133	0.080	2510

Notes: [1]Number of claimants filed in Coatesville, Reading, or Lancaster. These sites were considered to be located in areas characterized by low unemployment rate and short duration of unemployment.

[2]Number of claimants filed in Lewistown, Pittston, or Scranton. These sites were considered to be located in areas characterized by high unemployment rate and short duration of unemployment.

[3]Number of claimants filed in Philadelphia-North, Philadelphia-Uptown, McKeesport, Erie, or Butler. These sites were considered to be located in areas characterized by moderate unemployment rate and long duration of unemployment.

[4]Number of claimants filed in Connellsville. This site was considered to be located in area characterized by high unemployment rate and long duration of unemployment.

lusd: 1 if the claimant filed in Coatesville, Reading, or Lancaster and 0 otherwise. These three sites were considered to be characterized by **l**ow **u**nemployment rate and therefore **s**horter **d**urations of unemployment.

durable: 1 if the occupation of the claimant was in the sector of durable manufacturing and 0 otherwise.

Q1-Q5: five indicator variables indicating the quarter of enrollment of each claimant.

Treatment: five indicator variables indicating the treatment group (bonus amount - qualification period) in which each claimant enrolled.

The last part of Table 5.4, under the header "**duration**" presents some limited information on the distribution of the duration of the of UI benefits, measured in weeks. This measure of duration is called "inuidur" in the final report of the experiment. It is worth noting that a large portion of spells end in the first and the twenty seventh week. It should be stressed that the definition of the first spell of UI in the Pennsylvania study includes a waiting week and that the maximum number of uninterruptedly received full weekly benefits is 26. This implies that a total 2491 subjects did not receive any weekly benefit and that most of the claimants received continuously their full, entitled unemployment benefit. A more complete description of unemployment durations in the combined sample is presented in Figure 6.1.

Tables 5.3 and 5.4 are potentially useful for checking whether the randomization of subjects to experimental groups was successful. A properly made randomization implies that any difference between the length of unemployment insurance of claimants receiving the treatment and those that do not can be attributed exclusively to the treatment effect. Despite the intermediate changes in the rate of entry in the various groups, it is generally considered that the randomization process was effective; see Corson *et al.* (1992, p 45) and Meyer (1995, p 98).

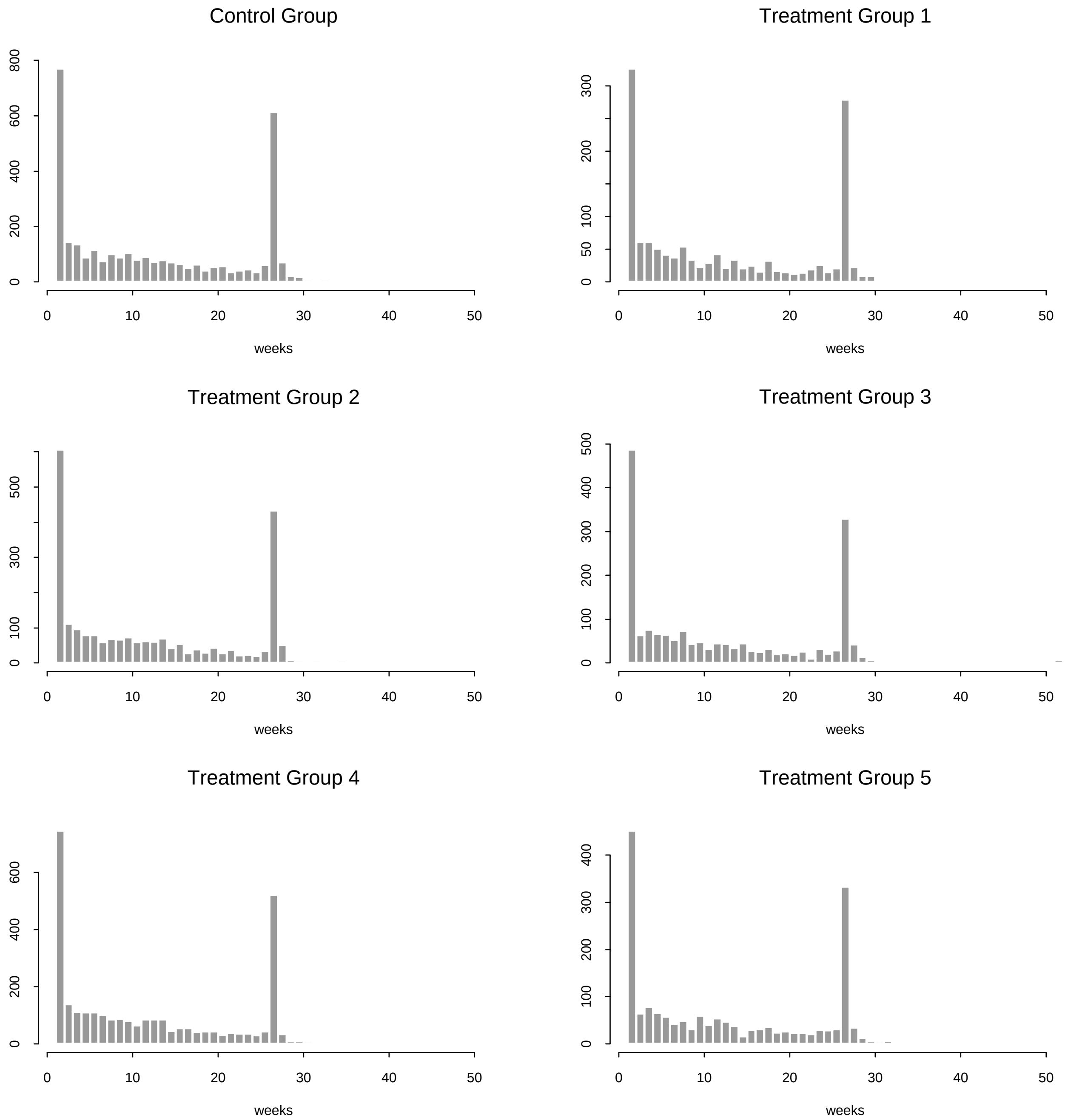
6. An Analysis of the Experiment

Our basic model for analyzing the Pennsylvania experiment presumes that the logarithm of the duration (in weeks) of subjects' spells on UI benefits have linear conditional quantile functions of the form

$$Q_{\log(T)}(\tau|x) = x'\beta(\tau).$$

The choice of the log transformation is dictated primary by the desire to achieve linearity of the parametric specification and by its ease of interpretation. Multiplicative covariate effects are widely employed throughout survival analysis, and they are certainly more plausible in the present application than the assumption of additive effects. It is perhaps worth reiterating that the role of the transformation is completely

FIGURE 5.1. Histogram of Duration of Unemployment by Group.



transparent in the quantile regression setting, where

$$Q_{h(T)}(\tau|x) \quad = \quad x'\beta(\tau)$$

implies

$$Q_T(\tau|x) \quad = \quad h^{-1}(x'\beta(\tau)).$$

In contrast, the role of transformations in models of the conditional mean function are rather complicated since the transformation affects not only location, but scale and shape of the conditional distribution of the response.

Our (provisional) model includes the following effects:

- Indicators for the 5 treatment groups, with treatments 4 and 6 pooled.
- Indicators for female, black and hispanic respondents.
- Number of dependents, with 2 indicating two or more dependents.
- Indicators for the 5 quarters of entry to the experiment.
- Indicator for whether the claimant "expected to be recalled".
- Indicator for whether the respondent was "young" – less than 35, or "old" – greater than 54.
- Indicator for whether claimant was employed in the durable goods industry.
- Indicator for whether the claimant was registered in a low employment district: Coatesville, Reading, or Lancaster.

In Figure 6.1 we present a concise visual representation of the results from the estimation of this model. Each of the panels of the Figure illustrate one coordinate of the vector-valued function, $\hat{\beta}(\tau)$, viewed as a function of $\tau \in [\alpha, 1-\alpha]$. Here we choose α to be .20 effectively neglecting the proportion of the sample that are immediately reemployed in week one and those whose unemployment spell exceeds that insured limit of 26 weeks. Confidence bands in each panel are computed by the procedures detailed in Appendix A. We note that the apparently anomalous behavior of these bands yielding narrowing the bands in the tails may be attributed to the large conditional density in these regions apparent from the histograms in Figure 5.1.

Before turning to interpretation of specific coefficients, we will try to offer some brief general remarks on how to interpret these figures. The simplest case is the pure location shift model in which we would have the classical accelerated failure time (AFT) model,

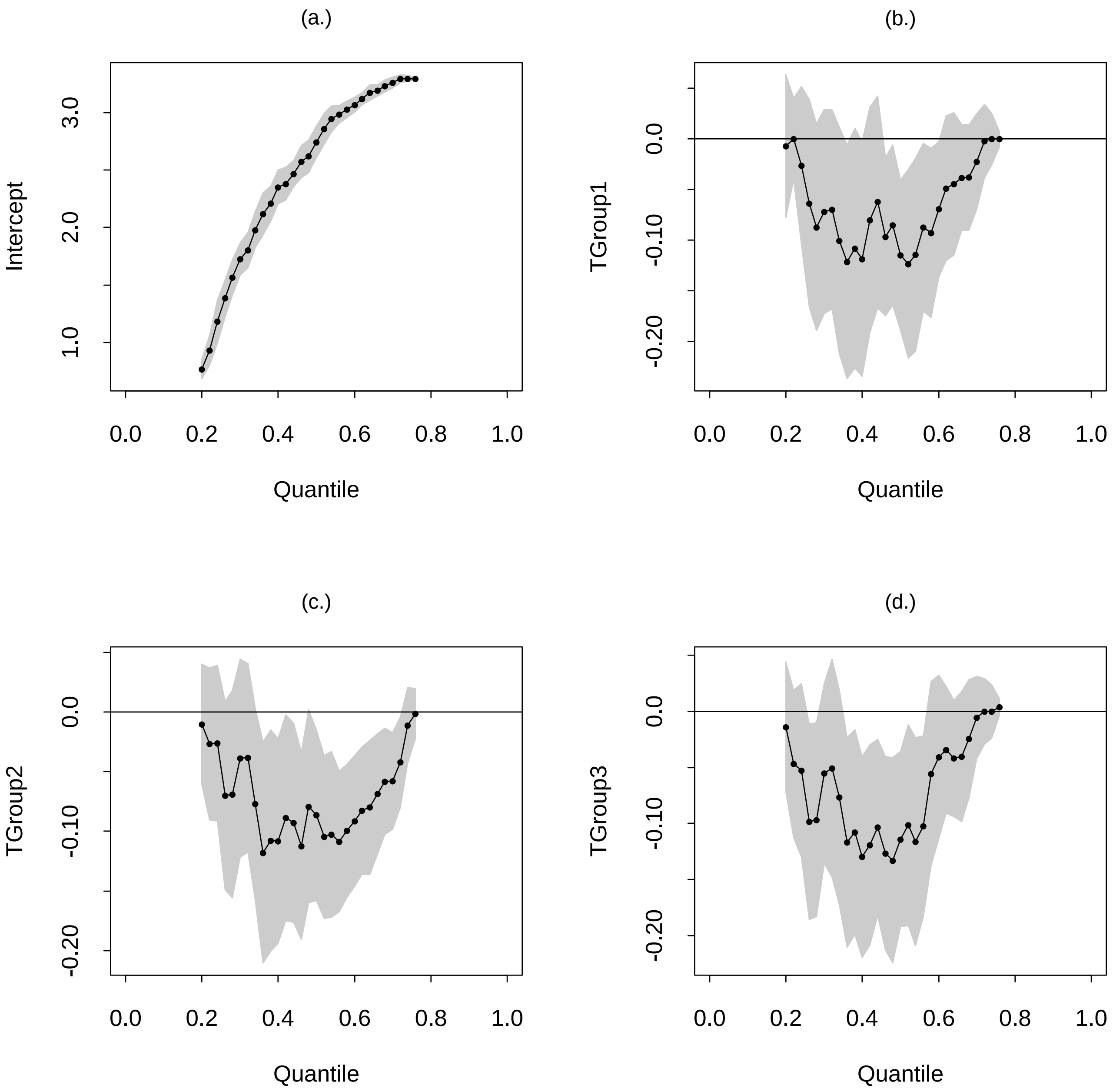
$$\log T_i = x_i'\beta + u_i$$

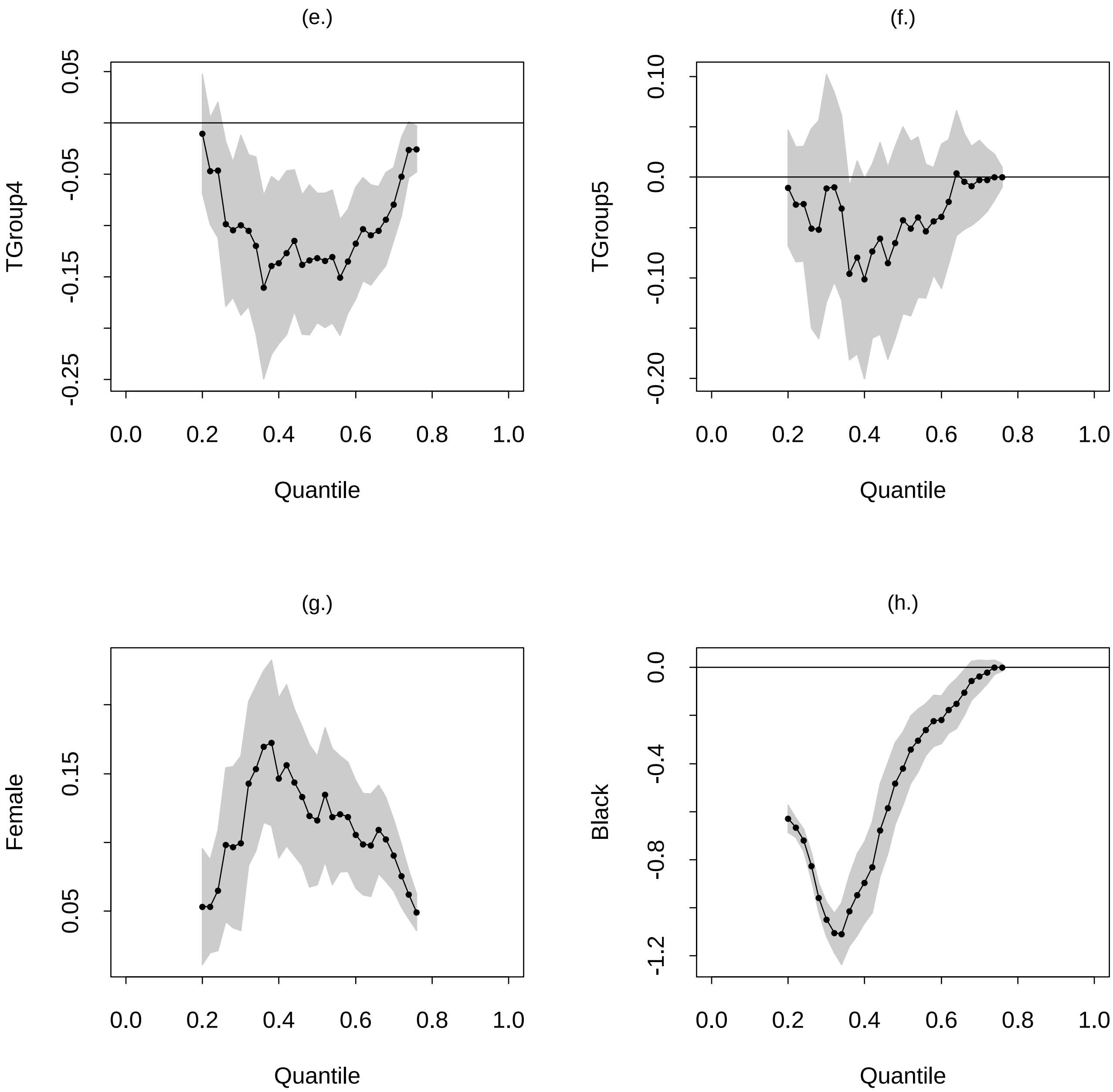
with $\{u_i\}$'s iid from some F. In this case we would expect to see coefficients $\hat{\beta}_j(\tau)$ that oscillate around a constant value indicating that the shift due to a change in the covariate is constant over the entire observed range of the distribution. Another conventional model with linear quantile functions is the linear location-scale model,

$$\log T_i = x_i'\beta + (x_i'\gamma)u_i$$

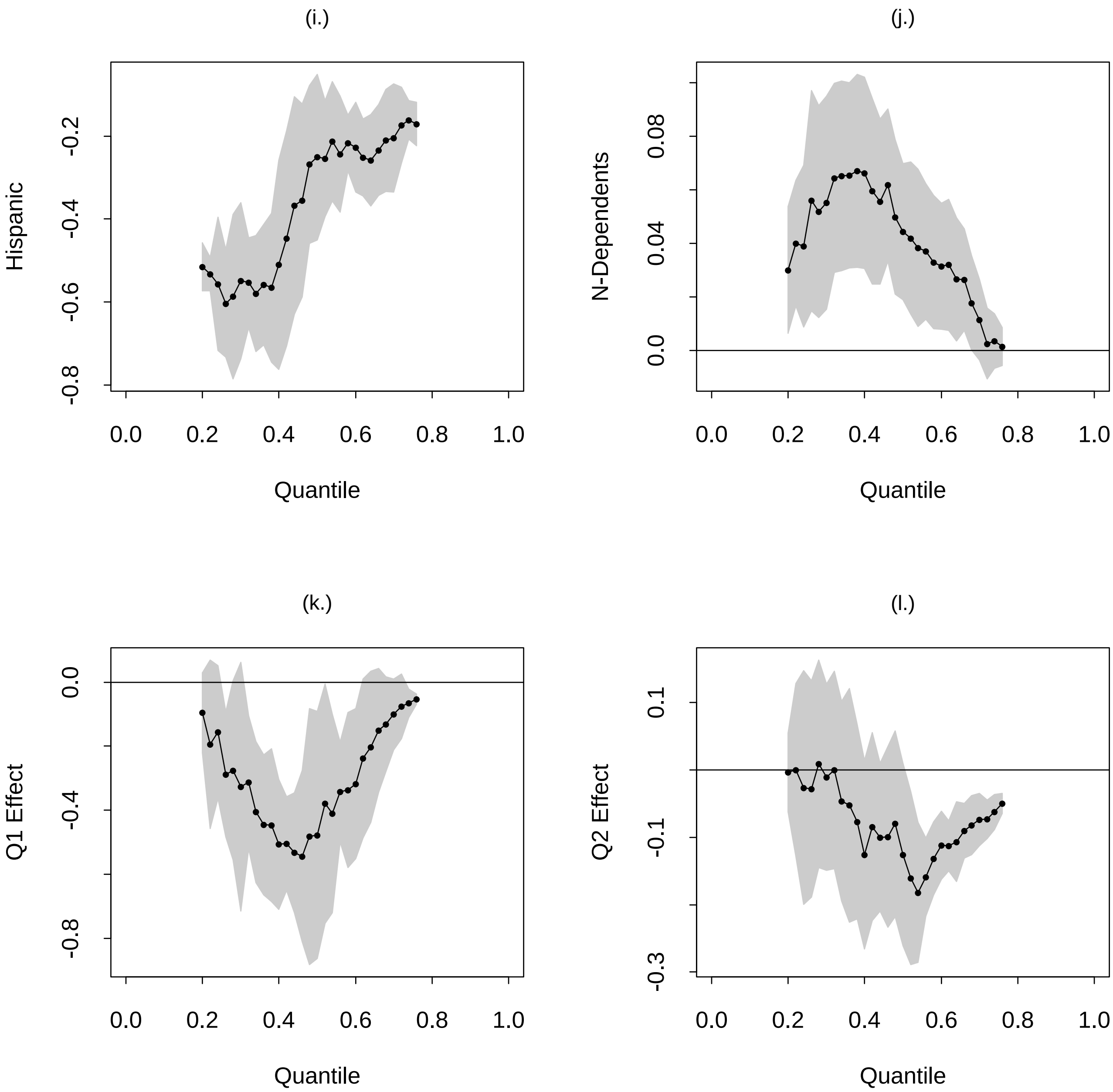
where again, u_i is taken to be iid. Now the covariates are allowed to influence the scale as well as the location of the conditional distribution of durations. In this case the "slope" coefficients $\hat{\beta}_j(\tau)$ should look just like the "intercept" coefficient up to a location and scale shift. The intercept coefficient estimates a normalized version of the quantile function of the u_i's and all the other coefficients are simply location and scale shifts of this function.

FIGURE 6.1. Estimated Quantile Regression Coefficients.

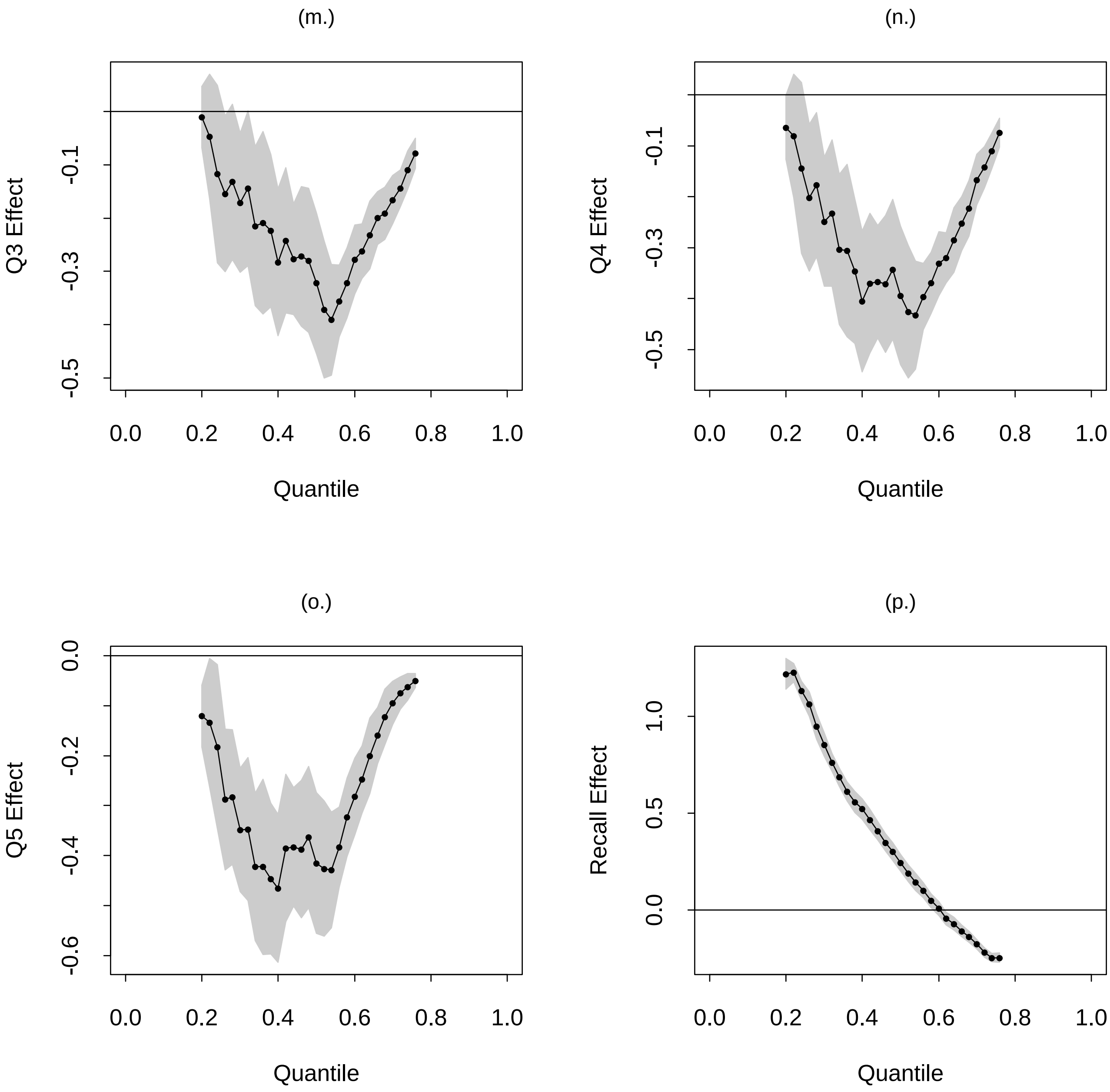




The effect of the five treatments depicted in subplots b-f are all roughly similar in shape. No treatment effect is observed in the tails implying that the treatments had no effect in either changing the probability of immediate reemployment (in week one), or in effecting the probability of durations beyond the 26 week maximum.

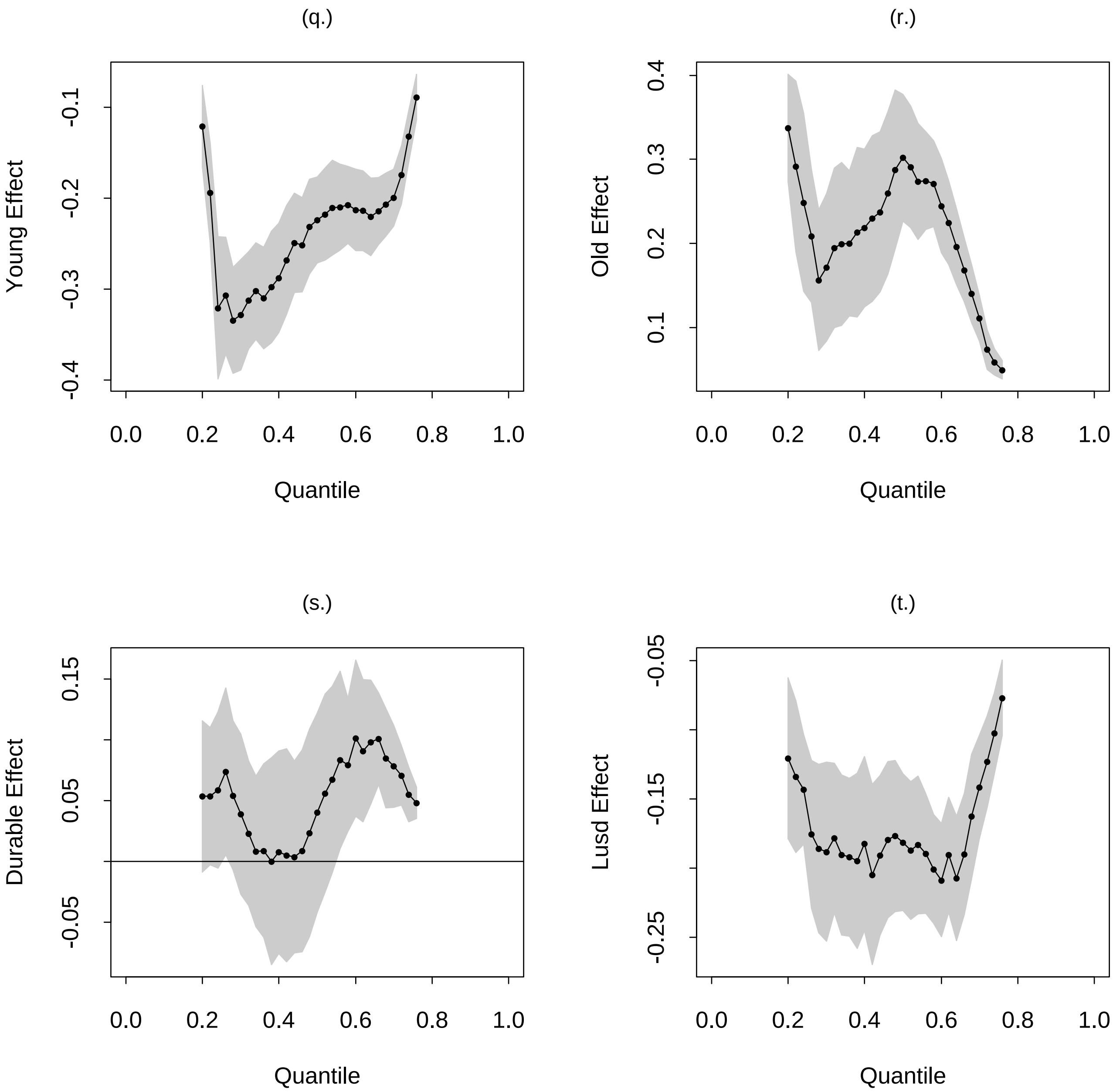


Treatments 1, 2, 3, and 5 are only marginally significant, inducing a modest 10% reduction in duration in the center of the distribution, but barely achieving nominal 5% significance for this effect. The combined treatments 4 and 6, which offered the high bonus and long qualification period, yielded a stronger effect. Roughly a 15%



reduction in median duration is observed, and this effect has a considerably stronger statistical significance than seen in the other treatments.

As we have already noted, the randomization of the experiment was quite effective in rendering the potentially confounding effects of other covariates orthogonal to the



treatment indicators. Nevertheless, it is of some interest to explore the effect of other covariates in an effort to better understand determinants of the duration of unemployment.

Women are 5 to 15% slower than men to exit unemployment. Blacks and Hispanics are much quicker than whites to become reemployed. This effect is particularly striking in the case of blacks for whom median duration is roughly half ($\approx e^{-.75}$) that of whites, and only 30% as long as controls at $\tau = .33$. The number of dependents appears to exert a rather weak positive effect on unemployment durations. The quarter-of-entry variables are inherently not very interesting, but it appears that late entry into the experiment improved one's chances for early reemployment. The recall indicator is considerably more interesting. Anticipated recall to one's prior job has a very strong and very precisely estimated detrimental effect over the entire lower tail of the distribution. However, beyond quantile $\tau = .6$, which corresponds to about 20 weeks duration for white, male controls, the anticipated recall appears to be foresaken and beyond this point recall becomes a significant force for early reemployment in the upper tail of the distribution.

Not surprisingly the young (those under 34) tend to find reemployment earlier than their middle aged counterparts, while the old (those over 54) do significantly worse. In both cases the effects are highly significant throughout the entire range of quantiles we have estimated. Prior employment in durable manufacturing has a weak disadvantageous effect on reemployment, but residing in a low unemployment district is obviously helpful in facilitating more rapid reemployment.

Some preliminary investigation of interaction effects has yielded no substantial improvement over the reported model. But we intend to continue to pursue this line of inquiry. Koenker and Xiao (2000) explore tests for joint effects, including the hypothesis of a linear location-scale model. However, informal examination of the foregoing figures makes it quite clear that the data offer little support for the location-scale hypothesis. The strongly non-monotone behavior of several of the coefficients in itself makes this highly implausible.

What have we learned from the quantile regression analysis that we could not have learned from a more conventional survival analysis? Clearly the treatment effect of the bonus offer does not conform to the location shift paradigm of the conventional models. After the log transformation of durations, a location shift would imply that the treatment exerts a constant *percentage* change in all durations. In the present instance this implication is particularly unpalatable since the entire point of the experiment was to alter the shape of the conditional duration distribution. In Figures 6.1b-f we have seen that the bonus effect gradually reduces durations from a null effect in the lower tail to a maximum reduction of 15% at the median, and then gradually again returns to a null effect in the upper tail. This finding accords perfectly with the timing imposed by the qualification period of the experiment. It might be thought that the bonus should not effect durations at all beyond the qualification period, but further consideration suggests that accelerated search in an effort to meet the qualification period deadline could easily yield "successes" that extended beyond the qualification period due to decision delay by potential employers, or other factors.

Meyer (1996), who provides a careful proportional hazard analysis of similar experiments in Illinois handles the qualification period by introducing time-varying covariates that permit a discrete jump in the treatment effect at the end of the qualification period. Whether such a jump is plausible seems debatable, however we would like to emphasize that an important challenge in extending the applicability of quantile regression methods for survival analysis involves some accommodation of time-varying covariate effects as in the Cox model. Progress in this direction may be achieved by imposing constraints across quantiles, but many details would need to be resolved.

7. CONCLUSIONS

We have argued that quantile regression offers a constructive complement to existing statistical methods of survival analysis. By enabling the researcher to focus attention on particular regions of the conditional duration distribution, quantile regression offers a more flexible approach than the more conventional transformation models in which covariates are assumed to exert a pure location-shift effect.

An analysis of the Pennsylvania Reemployment Bonus Experiments illustrates the methods. The treatments in these experiments were designed to explore the efficacy of moderate cash bonuses offered for early reemployment in reducing the duration of unemployment spells. We estimate that the Pennsylvania bonuses reduced the median duration of unemployment by about 10 to 15%, but this effect is considerably attenuated away from the median and essentially negligible in both the upper and lower tails of the distribution. These effects, if extrapolated to the full eligible population, implies a modest net savings to the unemployment insurance system. But the arguments of Meyer (1995, 1996) regarding the incentive effects on eligibility inherent in implementing the bonus system on a larger scale remain quite persuasive.

APPENDIX A.

The asymptotic behavior of the quantile regression process $\{\hat{\beta}(\tau) : \tau \in (0,1)\}$ closely parallels the theory of ordinary sample quantiles in the one sample problem. Koenker and Bassett (1978) show that in the classical linear model,

$$y_i = x_i\beta + u_i$$

with u_i iid from dfF, with density $f(u) > 0$ on its support $\{u|0 < F(u) < 1\}$, the joint distribution of $\sqrt{n}(\hat{\beta}_n(\tau_i) - \beta(\tau_i))_{i=1}^m$ is asymptotically normal with mean 0 and covariance matrix $\Omega \otimes D^{-1}$. Here $\beta(\tau) = \beta + F_u^{-1}(\tau)e_1$, $e_1 = (1, 0, \ldots, 0)'$, $x_{1i} \equiv 1$, $n^{-1}\sum x_i x_i' \to D$, a positive definite matrix, and

$$\Omega = (\omega_{ij} = (\tau_i \wedge \tau_j - \tau_i\tau_j)/(f(F^{-1}(\tau_i))f(F^{-1}(\tau_j))).$$

When the response is conditionally independent over i, but not identically distributed, the asymptotic covariance matrix of $\xi(\tau) = \sqrt{n}(\hat{\beta}(\tau) - \beta(\tau))$ is somewhat more complicated. Let

$$\xi_i(\tau) = x_i\beta(\tau)$$

denote the conditional quantile function of y given x_i, and $f_i(\cdot)$ the corresponding conditional density, and define,

$$J_n(\tau_1, \tau_2) = (\tau_1 \wedge \tau_2 - \tau_1\tau_2)n^{-1}\sum_{i=1}^{n} x_i x_i',$$

and

$$H_n(\tau) = n^{-1}\sum x_i x_i' f_i(\xi_i(\tau)).$$

Under mild regularity conditions on the $\{f_i\}$'s and $\{x_i\}$'s, we have joint asymptotic normality for vectors $(\xi(\tau_i), \ldots, \xi(\tau_m))$ with mean zero and covariance matrix

$$V_n = (H_n(\tau_i)^{-1} J_n(\tau_i, \tau_j) H_n(\tau_j)^{-1})_{i=1}^{m}.$$

This "Huber sandwich" is the quantile regression version of the Eicker-White heteroscedasticity consistent covariance matrix for the least squares estimator. In the present application we will estimate $f_i(\xi_i(\tau))$ using

$$\hat{f}_i(\hat{\xi}_i(\tau)) = \max\{0, 2h_n/(x_i'(\hat{\beta}(\tau + h_n) - \hat{\beta}(t - h_n)) - \varepsilon)$$

where $h_n = n^{-1/3}\Phi^{-1}(1-\alpha/2)^{2/3}((3/2\phi^2(0))/(2\Phi^{-1}(\tau)^2+1))^{1/3}$ is a bandwidth selected in accordance with the theory developed in Hall and Sheather (1989). This is a version of an estimator originally suggested in Hendricks and Koenker (1992). Note that the $\mathcal{O}_p(n^{-1/3})$ bandwidth is chosen to optimize performance of the sparsity estimate for purposes of Studentization; conventional theory would suggest $\mathcal{O}_p(n^{-1/5})$ if the objective were minimal mean squared error estimation of the sparsity function itself. There are several alternative schemes for conducting inference in the context of quantile regression. Rank based methods of inference for quantile regression are surveyed in Koenker (1996), and various approaches to inference based on resampling methods are discussed in Parzen, Wei and Ying (1994), Horowitz (1999), Buchinsky (1998) and Hahn (1995). Koenker and Machado (1999) discuss general goodness of fit measures and related inference methods based on the entire quantile regression process.

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