The effects of smoking and prenatal care on birth outcomes: evidence from quantile estimation on panel data*

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ABSTRACT

Unobserved heterogeneity among childbearing women makes it difficult to isolate the causal effects of smoking and prenatal care on birth outcomes (such as birthweight). Whether or not a mother smokes, for instance, is likely to be correlated with unobserved characteristics of the mother. This paper controls for such unobserved heterogeneity by using state-level panel data on maternally linked births. A quantile-estimation approach, motivated by a correlated random-effects model, is used in order to estimate the effects of smoking and other observables (number of prenatal-care visits, years of education, etc.) on the full distribution of birth outcomes.

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1 Introduction

Adverse birth outcomes have been found to result in large economic costs, in the form of both direct medical costs and long-term developmental consequences. Since better prenatal care during pregnancy is known to have a positive effect on birth outcomes, it is not surprising that the public-health community has focused efforts on improved prenatal care (e.g., through smoking cessation and better nutrition). Birthweight has served as a leading indicator of infant health, with "low birthweight" (LBW) infants classified as those that weigh less than 2500 grams at birth. Observable measures of poor prenatal care, such as smoking, have been found to have strong negative associations with birthweight. For instance, according to a report by the Surgeon General, mothers who smoke during pregnancy have babies that, on average, weigh 250 grams less (Centers for Disease Control and Prevention (2001)).

The direct medical costs of low birthweight are quite large. Based upon hospital-discharge data from New York and New Jersey, Almond et. al. (2004) report that the hospital costs for newborns peaks at around $150,000 (in 2000 dollars) for infants that weigh 800 grams; the costs remain quite high for all "low birthweight" outcomes, with an average cost of around $15,000 for infants that weigh 2000 grams. The infant-mortality rate also increases at lower birthweights.

Research by economists has also focused on the long-term effects of low birthweight on cognitive development, educational outcomes, and labor-market outcomes. LBW babies have developmental problems in cognition, attention, and neuromotor functioning that persist until adolescence (Hack et. al. (1995)). LBW babies are more likely to delay entry into kindergarten, repeat a grade in school, and attend special-education classes (Corman (1995); Corman and Chaikind (1998)). LBW babies are also more likely to have inferior labor-market outcomes, being more likely to be unemployed and earn lower wages (Behrman and Rosenzweig (2004); Case et. al. (2005); Currie and Hyson (1999)).

An enormous difficulty in evaluating initiatives aimed at improving birth outcomes is to accurately estimate the causal effects of prenatal activities on these birth outcomes. Unobserved heterogeneity among childbearing women makes it difficult to isolate the causal effects of smoking and prenatal care on birth outcomes (such as birthweight). Whether or not a mother smokes, for instance, is likely to be correlated with unobserved characteristics of the mother. To deal with this difficulty, various studies have used an instrumental-variable methodology in order to estimate the effects of smoking (Evans and Ringel (1999); Permutt and Hebel (1989)) and prenatal care (Currie and Gruber (1996); Evans and Lien (2005); Joyce (1999)) on birth outcomes. Another approach has been to utilize panel data (i.e., several births for each mother) in order to identify these effects from changes in prenatal behavior or maternal characteristics between pregnancies

(Abrevaya (2005); Currie and Moretti (2002); Rosenzweig and Wolpin (1991); Royer (2004)).

A potential limitation of these studies is that they have focused upon estimating how prenatal behaviors and maternal characteristics affect *average* birthweight. In contrast, the costs associated with birthweight have been found to exist primarily at the low end of the birthweight distribution, with the costs increasing significantly at the very low end. This paper focuses upon estimation of the prenatal effects on the entire birthweight distribution. Using state-level (maternally linked) panel data on births to control for unobserved heterogeneity, we address a major shortcoming of previous work on the association between prenatal care and the birthweight distribution. In particular, Abrevaya (2001) uses cross-sectional federal natality data and finds that various observables (such as smoking) have significantly larger effects at lower quantiles of the birthweight distribution; unfortunately, one can not interpret these "effects" as causal since the estimation has a purely reduced-form structure that does not account for unobserved heterogeneity.[1]

This paper combines the panel-data methodology of previous studies with a focus upon estimation of effects on the quantiles of birthweight. The outline of the paper is as follows. Section 2 details the quantile-estimation approach, which is motivated by the "correlated random effects model" of Chamberlain (1982, 1984). In particular, we focus upon a notion of marginal effects upon conditional quantiles that is analogous to the standard notion of marginal effects upon the conditional expectation. These effects explicitly control for unobserved heterogeneity by allowing the "mother random effect" to be related to observables. This approach is an important methodological contribution to the literature, as it provides a general framework with which empirical researchers can apply quantile regression to panel data. Section 3 describes the maternally-linked birth panel data for Washington and Arizona that are used in this study. Section 4 reports the main empirical results of the paper. There are some interesting differences between the panel-data and cross-sectional results. For example, the results from panel-data estimation, which controls for unobserved heterogeneity, indicate that the negative effects of smoking on birthweight are significantly lower (in magnitude) across all quantiles than indicated by the cross-sectional estimates. Finally, Section 5 concludes.

2 Quantile estimation for two-birth panel data

Despite the widespread use of both panel-data methodology and quantile-regression methodology, there has been surprisingly little work at the intersection of these two methodologies. As discussed in this section, the most likely explanation is the difficulty in extending differencing methods to quantiles. The outline of this section is as follows. Section 2.1 briefly reviews the fixed effects

[1]Koenker and Hallock (2001) report similar results from an application based upon Abrevaya (2001). See also Chernozhukov (2005), who has considered estimation at extremely low quantiles of the birthweight distribution.

and correlated random effects models for conditional expectations. Building upon the correlated random effects framework of Section 2.1, Section 2.2 extends the notion of marginal effects (and their estimation) to conditional quantile models. Section 2.3 discusses previous related studies.

2.1 Review of conditional expectation models with panel data

Suppose that the data source contains information on exactly two births for a large sample of mothers. A standard linear panel-data model for such a situation would be

$$y_{mb} = x'_{mb}\beta + c_m + u_{mb} \;\; (b = 1, 2;\; m = 1, \ldots, M), \tag{1}$$

where m indexes mothers, b indexes births, y denotes a birth outcome (e.g., birthweight), x denotes a vector of observables, c denotes the (unobservable) "mother effect," and u denotes a birth-specific disturbance. To simplify notation somewhat, let $x_m \equiv (x_{m1}, x_{m2})$ denote the covariate values from both births of a given mother.

From the basic model in (1), several different types of panel-data models arise from the assumptions concerning the unobservable c_m. In the "pure" random-effects version of (1), c_m is assumed to be uncorrelated with x_m. This assumption is implausible in the context of the empirical application being considered, so attention is focused upon two methods that allow for dependence between c_m and x_m: (1) the fixed-effects model and (2) the correlated random-effects model.

Fixed-effects model: The fixed-effects version of (1) allows correlation between c_m and x_m in a completely unspecified manner. For the fixed-effects model, note that the "meaning" of the parameter vector β is given by

$$\beta = \frac{\partial E(y_{mb}|x_m, c_m)}{\partial x_{mb}} \tag{2}$$

under the following assumption:

$$(A1) \;\; E(u_{m1}|x_m, c_m) = E(u_{m2}|x_m, c_m) = 0 \;\; \forall m. \tag{3}$$

It is well known that, under (A1), β can be consistently estimated by a first-difference regression (i.e., regressing $y_{m2} - y_{m1}$ on $x_{m2} - x_{m1}$). The reason that this strategy works for the conditional expectation hinges critically upon the fact that an expectation is a linear operator, so that

$$E(y_{m2} - y_{m1}|x_m) = E(y_{m2}|x_m) - E(y_{m1}|x_m) = (x_{m2} - x_{m1})'\beta. \tag{4}$$

For conditional quantiles, a simple differencing strategy is infeasible since quantiles are *not* linear operators — that is, in general, $Q_\tau(y_{m2} - y_{m1}|x_m) \neq Q_\tau(y_{m2}|x_m) - Q_\tau(y_{m1}|x_m)$, where $Q_\tau(\cdot|\cdot)$ denotes the τ-th conditional quantile function for $\tau \in (0, 1)$. This inherent difficulty has been recognized by others and is summarized nicely in a recent survey on quantile-estimation methods by Koenker and Hallock (2000):

> *Quantiles of convolutions of random variables are rather intractable objects, and preliminary differencing strategies familiar from Gaussian models have sometimes unanticipated effects.*

Without being more explicit about the relationship between c_m and x_m, it is difficult to envision an appropriate strategy for dealing with conditional quantiles, although Koenker (2004) has made some progress on this front.[2]

Correlated random-effects model: The correlated random-effects model of Chamberlain (1982, 1984) views the unobservable c_m as a linear projection onto the observables plus a disturbance:

$$c_m = \psi + x'_{m1}\lambda_1 + x'_{m2}\lambda_2 + v_m, \tag{5}$$

where ψ is a scalar and v is a disturbance that (by definition of linear projections) is uncorrelated with x_{m1} and x_{m2}. Combining equations (1) and (5) yields

$$y_{m1} = \psi + x'_{m1}(\beta + \lambda_1) + x'_{m2}\lambda_2 + v_m + u_{m1} \tag{6}$$

and

$$y_{m2} = \psi + x'_{m1}\lambda_1 + x'_{m2}(\beta + \lambda_2) + v_m + u_{m2}. \tag{7}$$

The parameters $(\psi, \beta, \lambda_1, \lambda_2)$ in (6) and (7) can be estimated by least-squares regression or other methods (see, e.g., Wooldridge (2002, Section 11.3)). These equations make it clear how the observables affect the outcomes in both periods. The vector x_{m1} affects y_{m1} through two channels, (i) a direct effect (expressed by the $x'_{m1}\beta$ term) and (ii) an indirect effect working through the unobservable effect c_m. In contrast, the vector x_{m1} affects y_{m2} only through the unobservable effect c_m. In fact, under the additional assumption[3]

$$(A2) \;\; E(v_m|x_m) = 0, \tag{8}$$

note that the "meaning" of β is given by the following equation

$$\beta = \frac{\partial E(y_{m1}|x_m)}{\partial x_{m1}} - \frac{\partial E(y_{m2}|x_m)}{\partial x_{m1}} = \frac{\partial E(y_{m2}|x_m)}{\partial x_{m2}} - \frac{\partial E(y_{m1}|x_m)}{\partial x_{m2}}. \tag{9}$$

That is, β gives the differential impact of x_{m1} upon the conditional expectations of y_{m1} and y_{m2}. In other words, β tells us how much x_{m1} affects $E(y_{m1}|x_m)$ above and beyond the effect that works through the unobservable c_m.

[2] The important assumption for the approach in Koenker (2004) is that the fixed effect appears the same way in all conditional quantiles (i.e., for all values of τ). This assumption, which implies that the effect of the unobservable is a location shift on the distribution of the dependent variable, is relaxed in what follows.

[3] The linear projection in (5) implies that c_m and x_m are uncorrelated, which is weaker than (A2).

2.2 Estimation of effects on conditional quantiles with panel data

To consider the relevant effects of the observables on the *conditional quantiles* $Q_\tau(y_{mb}|x_m)$ (rather than the conditional expectation $E(y_{mb}|x_m)$), we consider the analogous effects to those given in equation (9). In particular, the effects of the observables on a given conditional quantile are given by

$$\frac{\partial Q_\tau(y_{m1}|x_m)}{\partial x_{m1}} - \frac{\partial Q_\tau(y_{m2}|x_m)}{\partial x_{m1}} \tag{10}$$

and

$$\frac{\partial Q_\tau(y_{m2}|x_m)}{\partial x_{m2}} - \frac{\partial Q_\tau(y_{m1}|x_m)}{\partial x_{m2}}. \tag{11}$$

For example, the difference in equation (10) is the effect of x_{m1} (first-birth observables) on $Q_\tau(y_{m1}|x_m)$ above and beyond the effect on the τ-th conditional quantile that works through the unobservable.

To estimate the effects given in equations (10) and (11), a model for both $Q_\tau(y_{m1}|x_m)$ and $Q_\tau(y_{m2}|x_m)$ is needed. Unfortunately, it is non-trivial to explicitly determine the conditional quantile models. Consider, for example, the simple case in which the data-generating process is given by equations (1) and (5) (which then imply equations (6) and (7)). If all of the error disturbances (u_{m1}, u_{m2}, v_m) were *independent* of x_m, then the conditional quantile functions would take a simple form (analogous to that of the conditional expectation function under assumption (A2)):

$$Q_\tau(y_{m1}|x_m) = \psi^1_\tau + x'_{m1}(\beta + \lambda_1) + x'_{m2}\lambda_2 \tag{12}$$

and

$$Q_\tau(y_{m2}|x_m) = \psi^2_\tau + x'_{m1}\lambda_1 + x'_{m2}(\beta + \lambda_2). \tag{13}$$

Under this independence assumption, note that the effect of the disturbances is reflected by a locational shift in the conditional quantiles (ψ^1_τ and ψ^2_τ); the slopes do not vary across the conditional quantiles. Without the independence assumption, however, the simple linear form for the conditional quantile functions (like those in equations (12) and (13)) only arises in very special cases. In general, the conditional quantile functions involve more complicated non-linear expressions and, in fact, can not be explicitly written down without a complete parametric specification of the error disturbances.

Therefore, the conditional quantiles are viewed as somewhat general functions of x_m: say, $Q_\tau(y_{m1}|x_m) = f^1_\tau(x_m)$ and $Q_\tau(y_{m2}|x_m) = f^2_\tau(x_m)$. To empirically estimate the effects in (10) and (11), then, *reduced-form* models for $Q_\tau(y_{m1}|x_m)$ and $Q_\tau(y_{m2}|x_m)$ are specified. These reduced-form models should be viewed as approximating the "true" conditional quantile functions $f^1_\tau(x_m)$ and $f^2_\tau(x_m)$. In this paper, a very simple form for the reduced-form models is considered, in which the conditional quantiles are expressed as linear (and separable) functions of x_{m1} and x_{m2}:

$$Q_\tau(y_{m1}|x_m) = \phi^1_\tau + x'_{m1}\theta^1_\tau + x'_{m2}\lambda^2_\tau \tag{14}$$

and

$$Q_\tau(y_{m2}|x_m) = \phi_\tau^2 + x'_{m1}\lambda_\tau^1 + x'_{m2}\theta_\tau^2. \tag{15}$$

Based upon (14) and (15), the effects of the observables on the conditional quantiles (see (10) and (11)) are equal to $\theta_\tau^1 - \lambda_\tau^1$ (for the first-birth outcome) and $\theta_\tau^2 - \lambda_\tau^2$ (for the second-birth outcome). Without imposing further restrictions, the parameters $(\phi_\tau^1, \phi_\tau^2, \theta_\tau^1, \theta_\tau^2, \lambda_\tau^1, \lambda_\tau^2)$ can be consistently estimated with linear quantile regression, as introduced by Koenker and Bassett (1978).

Although the linear approximation may at first appear to be restrictive, we should point out that this strategy is the one usually employed in *cross-sectional* quantile regression. In the cross-sectional case, even if the data-generating process is linear in the covariates with a mean-zero error, the conditional quantiles will only be linear in the covariates in very special cases (see, for example, Koenker and Bassett (1982)). Even in cross-sectional applications, then, the conditional quantile specification chosen by an empirical researcher (linear usually) should also be viewed as a reduced-form approximation to the true conditional quantile function. In fact, empirical applications of quantile regression generally start (either explicitly or implicitly) with a reduced-form approximating model of the conditional quantile function rather than with the data-generating process (see, e.g., Buchinsky (1994) and Bassett and Chen (2001)).

The linear approximation approach is also an inherent feature of the correlated random-effects approach for the conditional expectation model given by (1) and (5). As Chamberlain (1982) originally pointed out, if assumption (A2) does not hold, the conditional expectation function is non-linear; in this case, equations (6) and (7) represent linear approximations (projections) and β represents the marginal effects of the covariates upon these linear approximations.[4]

For the application in this paper, we choose to impose the additional restriction that the effects on the conditional quantiles are the same for both birth outcomes. (This restriction is similar to the implicit restriction embodied in the linear panel-data model (1), where β does not vary with b.[5]) For the conditional quantiles, let β_τ denote the (common) effect vector, so that the restriction can be expressed as

$$\beta_\tau = \theta_\tau^1 - \lambda_\tau^1 = \theta_\tau^2 - \lambda_\tau^2. \tag{16}$$

Under this restriction, the conditional quantile functions in (14) and (15) can be re-written as

$$Q_\tau(y_{m1}|x_m) = \phi_\tau^1 + x'_{m1}(\beta_\tau + \lambda_\tau^1) + x'_{m2}\lambda_\tau^2 = \phi_\tau^1 + x'_{m1}\beta_\tau + x'_{m1}\lambda_\tau^1 + x'_{m2}\lambda_\tau^2 \tag{17}$$

and

$$Q_\tau(y_{m2}|x_m) = \phi_\tau^2 + x'_{m1}\lambda_\tau^1 + x'_{m2}(\beta_\tau + \lambda_\tau^2) = \phi_\tau^2 + x'_{m2}\beta_\tau + x'_{m1}\lambda_\tau^1 + x'_{m2}\lambda_\tau^2. \tag{18}$$

[4]To be precise, the expectation operator $E(\cdot)$ in equation (9) would be replaced by the linear projection operator (denoted $E^*(\cdot)$ by Chamberlain (1982) and others).

[5]Royer (2004) provides estimates for a conditional expectation model in which β is allowed to vary over births.

The simplest estimation strategy, based upon the second equalities in both (17) and (18), is to run a pooled linear quantile regression in which the observations corresponding to both births of a given mother are stacked together as a pair.[6] In particular, a quantile regression (using the estimator for the τ-th quantile) would be run using

$$\begin{bmatrix} y_{11} \\ y_{12} \\ \cdots \\ y_{21} \\ y_{22} \\ \cdots \\ \vdots \\ \cdots \\ y_{M1} \\ y_{M2} \end{bmatrix} \quad \text{and} \quad \begin{bmatrix} 1 & 0 & x'_{11} & x'_{11} & x'_{12} \\ 1 & 1 & x'_{12} & x'_{11} & x'_{12} \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ 1 & 0 & x'_{21} & x'_{21} & x'_{22} \\ 1 & 1 & x'_{22} & x'_{21} & x'_{22} \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ & & \vdots & & \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ 1 & 0 & x'_{M1} & x'_{M1} & x'_{M2} \\ 1 & 1 & x'_{M2} & x'_{M1} & x'_{M2} \end{bmatrix} \tag{19}$$

as the left-hand-side and right-hand-side variables, respectively. This pooled regression directly estimates $(\phi^1_\tau, \phi^2_\tau - \phi^1_\tau, \beta_\tau, \lambda^1_\tau, \lambda^2_\tau)$. Note that the difference $\phi^2_\tau - \phi^1_\tau$ represents the effect of birth parity.[7] In a traditional panel-data context, this difference would represent the "time effect." Although the application considered here does not have any birth-invariant explanatory variables ("time-invariant" variables), such variables could be easily incorporated into (19) as additional columns in the RHS matrix; like birth parity, it would not be possible to separately identify the direct effects of these variables on y from the indirect effects (working through c) on y.

The only difficulty introduced by the pooled regression approach involves computation of the estimator's standard errors. Since there is dependence within a pair of births to a given mother, the standard formula used for the asymptotic variance of a quantile estimator (Koenker and Bassett (1978)), which is based upon independent observations, can not be applied. For the same reason, the standard bootstrap approach can not be used. Instead, a modified bootstrap approach is used. In particular, a given bootstrap sample is created by repeatedly drawing (with replacement) a mother from the sample of M mothers and including *both* births for that mother, where the draws continue until the desired bootstrap sample size is reached. For a given bootstrap sample, the pooled quantile estimator is computed. After repeating this process for many different bootstrap samples, the original estimator's variance matrix can be estimated by the empirical variance matrix of the bootstrap estimates. Similarly, bootstrap percentile intervals for the parameters can be easily constructed. The only difference from the usual bootstrap method in this context is that pairs of observations are drawn in construction of the bootstrap sample.

[6] Alternatively, at some computational expense, a classical minimum-distance (CMD) approach could be used. For this approach, the parameters from (14) and (15) would be estimated in two separate quantile regressions. Then, the parameters β_τ, λ^1_τ, and λ^2_τ would be estimated by the CMD objective function subject to the restrictions in (16).

[7] Birth parity can not be included explicitly in x since the associated components of β_τ, λ^1_τ, and λ^2_τ would not be separately identified.

2.3 Review of related studies

In their recent survey of quantile regression, Koenker and Hallock (2000) cite only a single panel-data application. The cited study by Chay (1995) uses quantile regression on longitudinal earnings data to estimate the effect of the 1964 Civil Rights Act on the black-white earnings differential. Chay (1995) allows the individual effect to depend on the racial indicator variable, which amounts to a shift in the conditional quantile function and is a special case of the general approach described in Section 2.2 (where the only non-zero components of the λ parameters would correspond to the racial indicator variable). Interestingly, the application of Chay (1995) involves censored earnings data, so that quantile regression methods for censored data (Powell (1984, 1986)) are needed. Such censored-data quantile methods would also work with the general model of Section 2.2 but are not needed for the application considered in this paper.

A more recent study that uses quantile regression for panel data is Arias et. al. (2001), who estimate the returns to schooling (at various conditional quantiles) using twins data. To deal with the unobserved "family effect," the authors use proxy variables (father's education and sibling's education) in the earnings-equation model. This proxy-variable approach is somewhat related to the correlated random effects model in the sense that the latter specification can be viewed as using the observables x_{m1} and x_{m2} as proxies for the unobserved individual effect. One could also incorporate an external proxy (such as father's education in the Arias et. al. (2001) case) into the correlated random effects framework.

Another panel-data study that is directly related to our empirical application is Royer (2004), who applies a correlated random effects model to maternally linked data from Texas. Royer (2004) estimates the effects of various observables (with a focus upon maternal age) on "binary" birth outcomes (such as premature birth or LBW birth). In our application, the dependent variable (birthweight) is continuous, which allows for the estimation of conditional quantile effects. In Royer (2004), fixed-effects estimation is also possible (in the context of the linear probability model) whereas no such alternative is available in the conditional quantile case. Finally, Royer (2004) relaxes the strict exogeneity assumption (required for consistency of the fixed-effects estimator) in several interesting ways.[8] Analogous extensions to the conditional quantile models are left for future research.

[8] Unfortunately, identification of the least restrictive models requires panel data with at least three births per mother. As a practical matter, this requirement reduces the sample size to an extent that makes the estimated effects of observables rather imprecise and introduces a possible selection bias (see the discussion in Royer (2004, pp. 39ff)).

3 Data

In the United States, detailed "natality data" is recorded for nearly every live birth that occurs. Detailed information on maternal characteristics (age, education, race, etc.), birth outcomes (birthweight, gestation, etc.), and prenatal care (number of prenatal visits, smoking status, etc.) is collected by each state (with federal guidelines on specific data-item requirements). The National Center for Health Statistics compiles the data from the individual states and makes it publicly available to researchers. Due to confidentiality restrictions, it is impossible to receive comprehensive natality data with personal identifiers at the federal level, making it difficult to reliably construct maternally-linked panel data. However, individual states may release such personal identifiers to researchers, subject to confidentiality agreements in most cases. The data used in this study were obtained from two states, Washington and Arizona, and are described in detail below:

1. *Washington data:* The Washington State Longitudinal Birth Database (WSLBD) was provided by Washington's Center for Health Statistics. The WSLBD is a panel dataset consisting of all births between 1992 and 2002 that could be accurately linked together as belonging to the same mother.[9] The linking of the original data was a collaboration between the Washington State Department of Health and the Department of Epidemiology at the University of Washington. The matching algorithm used to construct the WSLBD used personal identifying information such as mother's full maiden name and mother's date of birth. For two births to be linked together, (i) an exact match on mother's name, mother's date of birth, mother's race, and mother's state of birth was required, and (ii) consistency of birth parity and the reported interval-since-last-birth was required. Only births that could be uniquely linked together were retained in the WSLBD.

2. *Arizona data:* The Arizona Department of Health Services provided the authors with data on all births occurring in the state of Arizona between 1993 and 2002. Although names were not provided, the exact dates of birth for both mother and father were provided in the data. To maternally link births together, we followed as closely as possible to the algorithm used for the Washington data. For two births to be linked together, (i) an exact match on mother's date of birth, father's date of birth, mother's race, and mother's state of birth was required, and (ii) consistency of birth parity and the reported interval-since-last-birth was required. As with the Washington data, only births that could be uniquely linked together were retained. Since births could not be linked by maternal name, we decided to also require an exact match on father's date of birth in order to minimize the chance of false matches

[9] The original WSLBD has births dating back to 1980, but mother's education is not available as a data item until 1992. The time period 1992–2002 is also comparable to the one used for Arizona.

entering the sample.[10] This decision restricts the Arizona sample to mothers whose children had the same birth father, which is not a restriction of the Washington sample.

For the purposes of this study, particular subsamples of the Washington and Arizona maternally-linked data are considered: *all pairs of first and second births to white mothers*. In particular, birth outcomes (and the effects of other variables upon birth outcomes) have been found to differ across different races and at higher birth parities. The choice of subsample circumvents this issue by focusing upon a more homogeneous sample. The resulting estimates, of course, should be interpreted as being applicable to the subpopulation represented by this sample choice.

Table 1 provides descriptive statistics for the Washington and Arizona samples, broken down by first-child and second-child births. Any mothers that had data items missing in either of her two births (for the variables summarized in Table 1) were dropped from the sample. The resulting samples used for estimation consist of 45,067 Washington mothers (90,134 births) and 56,201 Arizona mothers (112,402 births). Sample averages are reported for all variables, as well as standard deviations (in parentheses) for the non-indicator variables. The "Smoke" ("Drink") variable is equal to one if the mother reported smoking (drinking alcohol) during pregnancy. Although alcohol consumption during pregnancy is known to be severely under-reported, the "Drink" variable is included in the regressions as it may be useful a proxy for other unobservables. For the Washington data, the four prenatal-care variables ("No prenatal care," "1st-trimester care," "2nd-trimester care," and "3rd-trimester care") represent mutually exclusive categories that were constructed on the basis of the reported month of the first prenatal-care visit. Unfortunately, the month of first prenatal-care visit is not reported in the Arizona data until 1997. As a result, only the number of prenatal visits and an indicator variable for "no prenatal care" (equal to one if there are no prenatal visits) are summarized in Table 1 and used in the empirical analysis of Section 4. The other variables are self-explanatory.

The descriptive statistics in Table 1 indicate that average birthweight increases by 88 grams at the second birth for both Washington mothers and Arizona mothers. For their second birth, Table 1 also indicates that women are less likely to smoke and drink and more likely to be married, have a male child, and have their first prenatal-care visit during the first trimester. Based on the summary statistics, the two samples of mothers are quite similar. On average, Arizona mothers are slightly less educated and have babies with higher birthweight. The largest difference between the two samples appears to be the level of smoking: Washington mothers report smoking in 13.7% of pregnancies (which is right around the national average during this time period), whereas Arizona mothers report smoking in only 4.7% of pregnancies. Similarly, Arizona mothers have a lower

[10]This choice turns out to have very little impact on the estimation results reported in Section 4. Estimates for a sample matched only on mother's date of birth were extremely similar.

Table 1: Descriptive Statistics, Washington and Arizona Birth Panels

Variable	Washington		Arizona	
	1st Child	2nd Child	1st Child	2nd Child
Birthweight (in grams)	3442 (523)	3530 (536)	3339 (517)	3427 (505)
Male child	0.515	0.511	0.520	0.516
Mother's age	25.27 (5.25)	27.89 (5.35)	25.23 (5.26)	27.85 (5.36)
Mother's education	13.52 (2.32)	13.72 (2.21)	13.21 (2.68)	13.39 (2.61)
Married	0.751	0.853	0.780	0.886
No prenatal care	0.004	0.003	0.005	0.006
1st-trimester care	0.879	0.895	—	—
2nd-trimester care	0.107	0.093	—	—
3rd-trimester care	0.014	0.012	—	—
Smoke	0.143	0.132	0.049	0.044
Drink	0.017	0.014	0.009	0.007
# prenatal visits	12.06 (3.53)	11.63 (3.25)	11.83 (3.59)	11.73 (3.55)
Year of birth	1995.0 (2.2)	1997.8 (2.3)	1996.3 (2.3)	1998.9 (2.2)
# of Observations	45,067	45,067	56,201	56,201

reported rate of drinking during pregnancy.

4 Results

4.1 Regression results

This section reports the results from regression analysis (least-squares regressions and quantile regressions) using the two maternally linked datasets described in Section 3. In the interest of space, the full set of numerical results (tables) and a detailed discussion are provided only for the Washington data (Section 4.1.1). The Arizona results are reported in a graphical format comparable to the Washington results (Section 4.1.2), but the detailed tables have been omitted and the discussion is limited to comparisons with the Washington results.[11]

4.1.1 Washington data

The tables report estimates for the quantiles $\tau \in \{0.10, 0.25, 0.50, 0.75, 0.90\}$ (along with least-squares estimates for comparison), although the figures presented in this section consider marginal effects at all quantiles within the $(0, 1)$ range. Throughout this section, the dependent variable of interest is birthweight (measured in grams). In order to have a relevant comparison for the panel-data results, cross-sectional results (without incorporating the correlated random effects) are also

[11] The tables are available upon request from the authors.

reported. For the cross-sectional results, the panel structure of the data is only used for computing standard errors. Since each mother appears twice in the data, the pair-sampling bootstrap described at the end of Section 2.2 is used.

The cross-sectional results are reported in Table 2. The model specification includes the variables summarized in Table 1, along with an indicator variable for the second child and quadratic variables for both mother's age and education. For the prenatal-care variables, the omitted category corresponds to first-trimester prenatal care, so the estimates for the other three prenatal-care variables ("No prenatal care," "2nd-trimester care," and "3rd-trimester care") should be interpreted as differences from first-trimester prenatal care. Overall, the cross-sectional results are quite similar to those found in previous studies using federal natality data (Abrevaya (2001); Koenker and Hallock (2001)).

The panel-data results are reported in Table 3. These results are based upon the same model specification as used for the cross-sectional estimation, but the unobserved heterogeneity is modeled as in Section 2.2 (see equations (17) and (18)). For the pooled quantile regressions, Table 3 reports the estimates of the marginal effects β_τ. The estimates of the unobserved-heterogeneity parameters λ_τ^1 and λ_τ^2 are reported in the Appendix (see Tables 6 and 7). To provide a more complete view of the variables' effects on birthweights and to allow an easy comparison with the cross-sectional estimates, Figures 1 and 2 plot the estimated effects from both the panel and cross section. For these figures, the quantile regressions were estimated at 2% intervals, from the 6% quantile through the 94% quantile (inclusively). The panel-data estimates are represented with a solid line, and the 90% confidence intervals (bootstrap percentile intervals) for these estimates are represented with dashed lines. The cross-sectional estimates, computed at the same quantiles, are represented with a dotted line. (To avoid cluttering the figures, confidence intervals for the cross-sectional results are not reported. The size of these intervals can, however, be inferred from the standard errors in Table 2.) Since both age and education have quadratic terms in the model specification, the marginal-effect plots for age and education are based upon estimates evaluated at specific values for the two variables (25 years old for age and 12 years for education level); other choices are considered in later figures.

Table 2: Cross-Sectional Estimation Results, Washington Data

	Quantile regressions					
	10%	25%	50%	75%	90%	OLS
Second child	99.877***	94.147***	93.652***	100.620***	111.385***	99.537***
	(6.950)	(5.041)	(4.273)	(4.872)	(6.963)	(3.881)
Male child	87.939***	115.533***	128.175***	143.241***	162.446***	124.355***
	(6.156)	(4.357)	(3.853)	(4.211)	(5.597)	(3.530)
Age	20.629***	14.205***	7.788**	7.966**	6.347	12.693***
	(6.215)	(4.093)	(3.484)	(3.972)	(5.398)	(3.385)
Age^2	-0.405***	-0.268***	-0.138**	-0.123*	-0.087	-0.230***
	(0.111)	(0.071)	(0.062)	(0.070)	(0.095)	(0.059)
Education	30.223**	21.791**	28.921***	27.002***	22.981**	26.809***
	(13.397)	(8.615)	(7.615)	(6.689)	(10.427)	(7.096)
Education^2	-0.723	-0.571*	-0.878***	-0.927***	-0.756**	-0.783***
	(0.492)	(0.318)	(0.285)	(0.254)	(0.384)	(0.263)
Married	38.045***	26.731***	26.936***	22.778***	16.981*	28.295***
	(10.133)	(7.165)	(6.102)	(7.068)	(9.246)	(5.932)
No prenatal care	-339.441*	-19.984	-31.511	21.406	172.522**	-33.188
	(187.306)	(54.127)	(40.988)	(43.051)	(70.822)	(47.619)
2nd-trimester care	38.251***	28.510***	24.911***	30.894***	37.894***	38.487***
	(11.313)	(8.226)	(7.215)	(8.380)	(10.691)	(6.547)
3rd-trimester care	109.139***	64.813***	38.997**	24.150	24.621	65.764***
	(29.028)	(19.721)	(18.665)	(17.290)	(23.469)	(14.794)
Smoke	-184.857***	-181.088***	-178.764***	-177.093***	-162.289***	-177.721***
	(11.027)	(7.461)	(6.267)	(7.428)	(9.988)	(6.219)
Drink	-48.027*	-37.352*	-11.290	-20.388	4.897	-20.838
	(26.412)	(20.167)	(15.649)	(19.096)	(23.925)	(14.255)
# prenatal visits	19.458***	16.532***	15.016***	14.905***	14.072***	18.458***
	(1.367)	(0.903)	(0.767)	(0.831)	(1.084)	(0.864)
Year of birth	-4.732***	-2.820**	-3.235***	-3.769***	-3.945***	-3.914***
	(1.452)	(1.114)	(0.949)	(1.042)	(1.402)	(0.899)

Bootstrapped standard errors in parentheses, using bootstrap sample size of 20,000 (10,000 pairs) and 1,000 bootstrap replications.

'*': significant at 10 percent level, double-sided (normal dist.).
'**': significant at 5 percent level, double-sided (normal dist.).
'***': significant at 1 percent level, double-sided (normal dist.).

Table 3: Panel-Data Estimation Results (β_τ), Washington Data

	Quantile regressions					
	10%	25%	50%	75%	90%	OLS
Second child	146.280***	115.821***	113.501***	117.857***	129.274***	126.843***
	(12.593)	(8.444)	(7.232)	(8.029)	(11.514)	(6.201)
Male child	103.551***	131.336***	147.141***	159.386***	173.639***	138.680***
	(7.889)	(5.064)	(4.349)	(4.897)	(6.738)	(3.672)
Age	-29.931**	-15.960*	-29.779***	-25.504***	-43.480***	-25.358***
	(13.174)	(8.537)	(7.702)	(8.337)	(11.692)	(6.363)
Age^2	0.515***	0.280**	0.476***	0.515***	0.808***	0.462***
	(0.197)	(0.121)	(0.107)	(0.117)	(0.160)	(0.091)
Education	29.548*	18.950	25.557***	19.832**	2.592	19.073**
	(17.625)	(11.814)	(9.827)	(8.731)	(13.806)	(7.811)
Education^2	-1.099	-0.966*	-0.970**	-0.833**	-0.364	-0.844**
	(0.748)	(0.507)	(0.420)	(0.396)	(0.625)	(0.341)
Married	38.193**	17.403	30.129***	19.674*	10.078	28.628***
	(16.888)	(11.520)	(9.372)	(11.244)	(16.123)	(8.353)
No prenatal care	-318.447*	-16.165	5.172	31.742	263.254***	-18.000
	(170.299)	(59.757)	(51.786)	(53.259)	(74.783)	(48.026)
2nd-trimester care	22.497	8.119	-1.042	22.208**	31.439**	21.479***
	(14.373)	(10.445)	(8.656)	(10.169)	(14.242)	(6.924)
3rd-trimester care	62.483*	70.998***	29.725	35.269	33.807	54.505***
	(34.905)	(24.600)	(23.075)	(24.457)	(32.193)	(17.452)
Smoke	-26.199	-60.302***	-82.545***	-54.549***	-60.125***	-56.471***
	(19.221)	(14.131)	(11.260)	(12.237)	(17.510)	(9.084)
Drink	-73.406**	-38.813	-4.036	-3.625	-9.901	-24.773
	(35.327)	(24.073)	(19.747)	(22.734)	(29.454)	(15.584)
# prenatal visits	20.189***	14.966***	12.686***	12.309***	12.624***	17.464***
	(1.608)	(1.108)	(0.869)	(0.979)	(1.432)	(0.928)
Year of birth	-16.733**	-7.248	-4.432	-9.153	-6.998	-11.383***
	(7.947)	(5.259)	(4.961)	(5.626)	(7.962)	(3.990)

Bootstrapped standard errors in parentheses, using bootstrap sample size of 20,000 (10,000 pairs) and 1,000 bootstrap replications.

'*': significant at 10 percent level, double-sided (normal dist.).
'**': significant at 5 percent level, double-sided (normal dist.).
'***': significant at 1 percent level, double-sided (normal dist.).

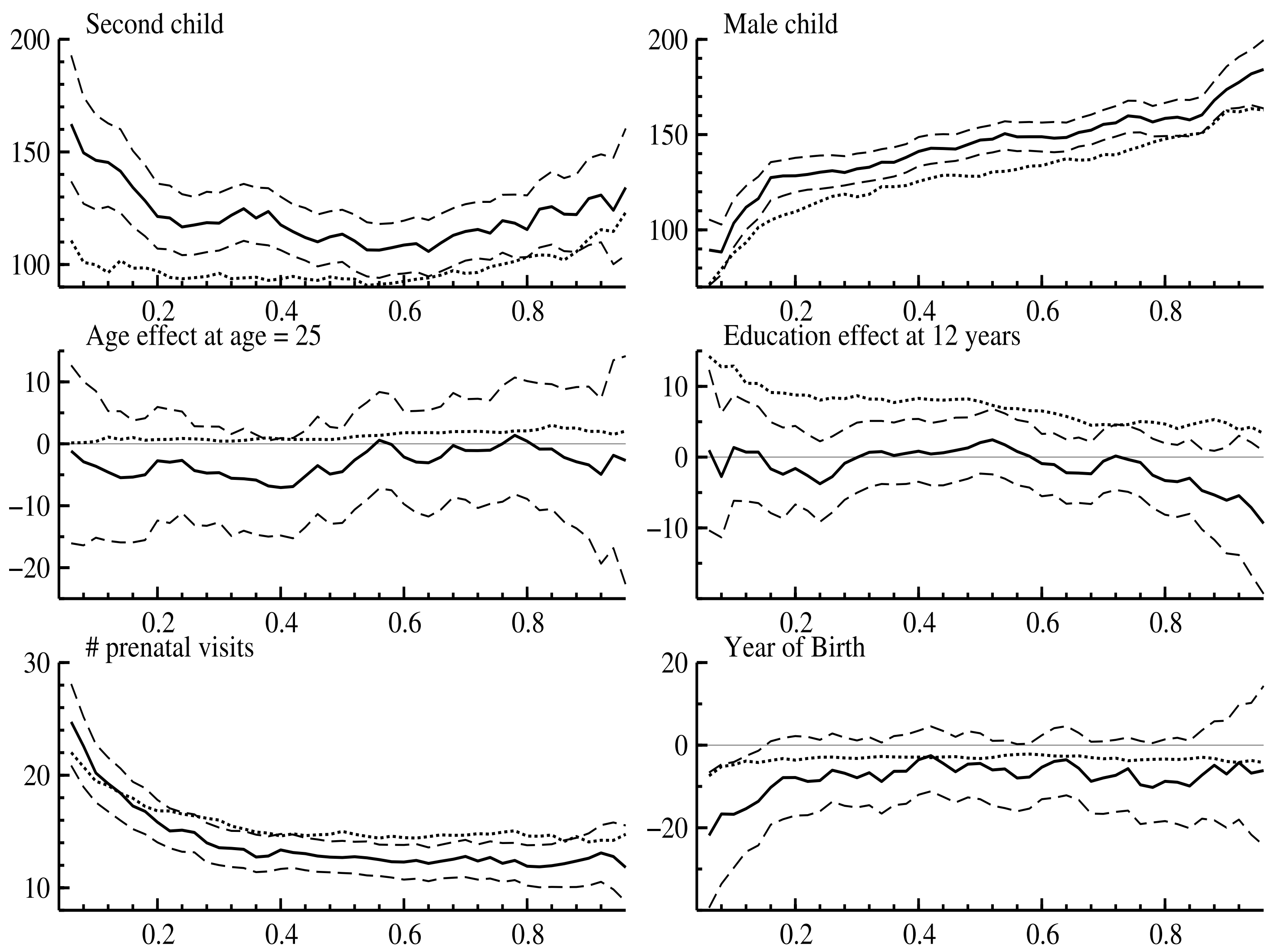

Figure 1: Washington Results, Part 1 (Solid = Panel, Dashed = Panel 90% CI, Dotted = Cross-Section)

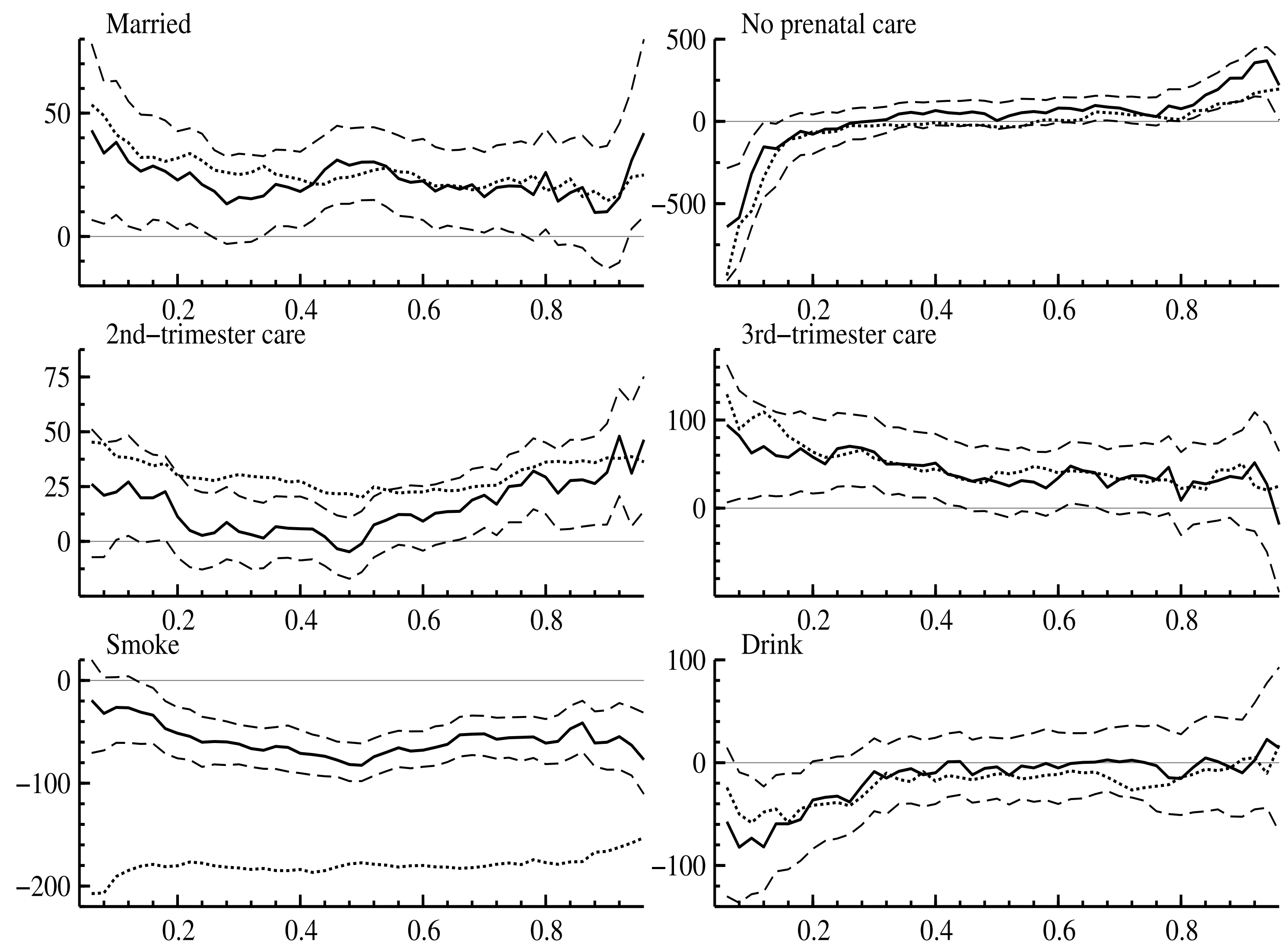

Figure 2: Washington Results, Part 2 (Solid = Panel, Dashed = Panel 90% CI, Dotted = Cross-Section)

The estimated effects of the various variables, as presented in Tables 2 and 3 and Figures 1 and 2, are discussed in more detailed below:

Second child: Birthweights are uniformly larger for second children at all quantiles, for both the cross-sectional and panel estimates. The panel estimates of the second-child effect are somewhat larger than the cross-sectional estimates, with the largest effects at the lowest quantiles (e.g., 146 grams at the 10% quantile).

Male child: It is well-known that, on average, male babies weigh more at birth than female babies. The quantile estimates indicate that the positive male-child effect on birthweight is present at all quantiles of the conditional birthweight distribution. The magnitude of the effect increases when one moves from lower quantiles to higher quantiles, with the panel estimates indicating a slightly higher effect (10–20 grams) than the cross-sectional estimates.

Age and education: Figure 1 shows the estimated (one-year) effects of age and education, evaluated at 25 years of age and 12 years of education, respectively. For age, both the cross-sectional and panel estimates are very close to zero in magnitude (and statistically insignificant at a 5% level for all quantiles). For education, the cross-sectional estimates are positive across the quantiles and statistically significant (at a 5% level) except at quantiles above 80%. In contrast, the panel estimates are statistically insignificant across all quantiles. This difference could be due to two factors: (i) the amount of within-mother variation in education is quite small, with the average change in education for the sample being about 0.2 years; and, (ii) the level of education may be correlated with the mother-specific unobservable. For the latter factor, years of schooling is likely positively correlated with c_m, which would imply that the cross-sectional estimates are biased upwards. To consider the effects of age and education elsewhere in the covariate distribution, Figure 3 shows the estimated quantile effects at an age of 35 and an education level of 16 years (i.e., college educated). In addition to the panel estimates (with 90% confidence bands) and the cross-sectional estimates, this figure also provides plots of the estimated "effects" on the unobservable c_m (as calculated from the λ_τ^1 and λ_τ^2 estimates) along with 90% confidence bands. For the age effects, the panel estimates are now above the cross-sectional estimates (whereas they were below them at an age of 25). The panel estimates of the age effect are slightly positive at all quantiles (reaching about 10 grams at the highest quantiles) and marginally significant at a 10% level for most quantiles between 55% and 85%. These estimates are somewhat different from the cross-sectional estimates, which are negative at all quantiles and significantly so at a 5% level for quantiles below 25%. Looking at the plots of the age effects on the unobservable, it appears that there is a slight positive relationship between an additional year of age (evaluated at age 35) and the unobservable c_m at the lower quantiles. These plots explain why the panel estimates of age in the original plot are so close to zero for the lower quantiles but slightly more positive at the higher quantiles. Finally, for the education effect at 16 years of education, there is not much evidence of significant effects. The cross-sectional estimates are only significant at a 10% level at the very lowest quantiles, with the point estimates reaching an 8-gram positive effect at the 4% quantile. The estimated effects from the panel specification are actually all negative, but none of the point estimates is significant at a 5% level. These results are perhaps not surprising since 16 years of schooling already represents a high level of education and an additional year would not be expected to have much of a marginal effect.

Marital status: The estimated positive effects of marriage on birthweight are quite similar for the cross-sectional and panel specifications, in the 20–50 gram range over the quantiles considered. The differential impact in the cross-sectional impacts seems to be most evident at the lowest quantiles, where the marriage effect approaches 50 grams. One should be cautious about interpreting the cross-sectional marriage estimates as causal since marital status is an explanatory variable that *a priore* would appear to serve as a proxy for mother-specific unobservables (i.e., marital status positively correlated with c_m). The panel estimates are slightly lower than the cross-sectional estimates in the lower quantiles (until around the 40% quantile), suggesting that this might be a factor in the lower quantiles. Somewhat surprisingly, however, the panel estimates of the marriage effect remain positive throughout the range of quantiles and significantly so (at the 10% level) at nearly all the quantiles below 80%. On the whole, the estimates are consistent with a situation in which marriage provides the birth mother with support (support at home, financial support, emotional support, etc.) that would lead to a more favorable birth outcome.

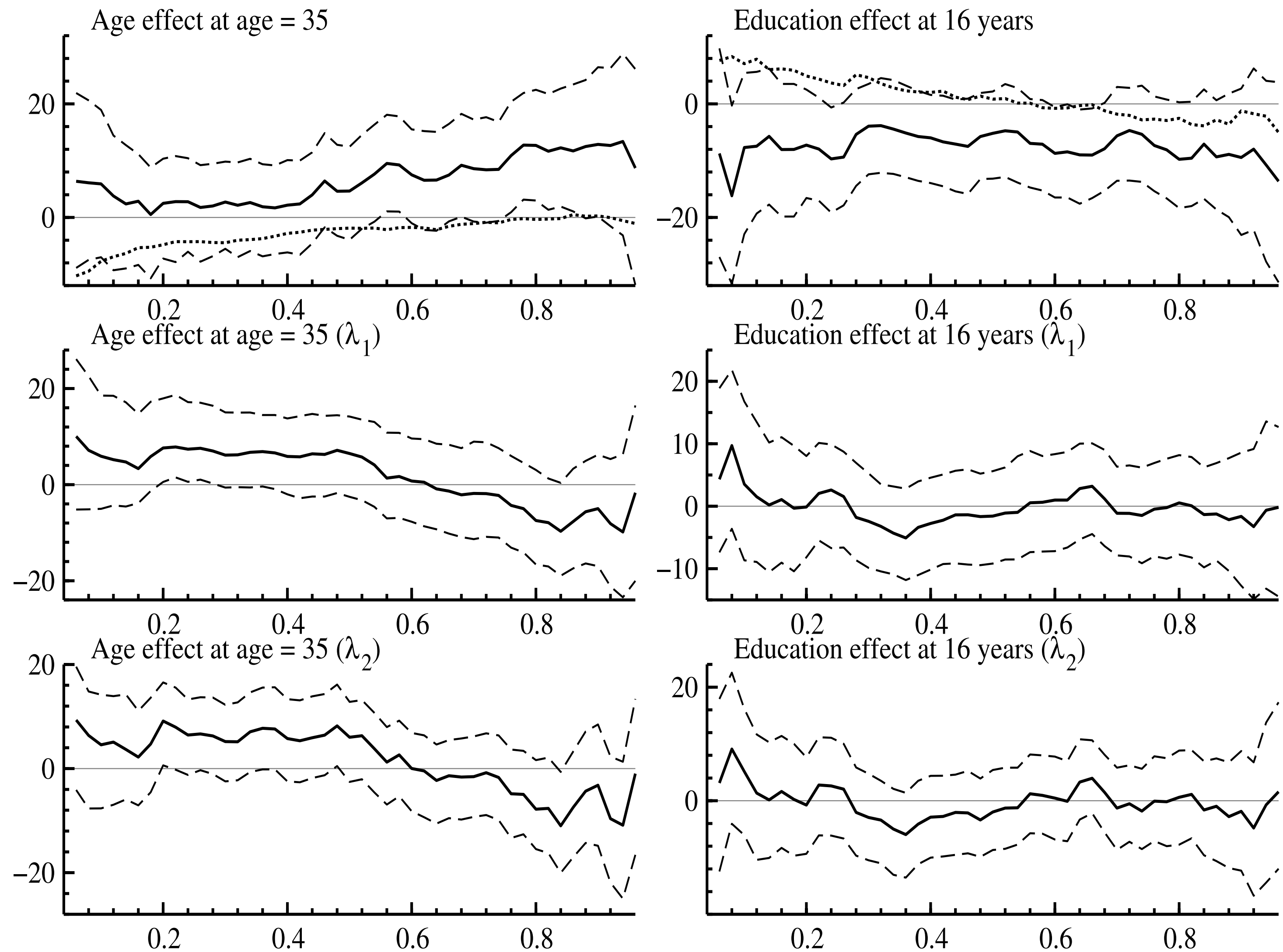

Figure 3: Age and Education Effects (Solid = Panel, Dashed = Panel 90% CI, Dotted = Cross-Section)

Prenatal-care visits: The information on prenatal-care visits in the model specification consists of (i) the trimester of the first prenatal visit (if any) and (ii) the number of prenatal visits (if any). It should be pointed out that interpreting the effect of any prenatal-care variable is a bit difficult since the *observed* prenatal care proxies for both *intended* prenatal care and pregnancy problems. For instance, if two mothers have identical intentions (at the beginning of pregnancy) with respect to prenatal-care visits, the mother that experiences problems early in her pregnancy would be more likely to have an earlier first prenatal-care visit and to have more prenatal-care visits overall. The estimated effects of the prenatal-care variables, therefore, may reflect the combined effects of intended care and pregnancy complications.[12] The estimates for the no-prenatal-care indicator variable, which are significantly negative at the 10% quantile and significantly *positive* at the 90% quantile, illustrate this point. A possible explanation for the dramatic difference at the two ends of the distribution is that lack of prenatal care is more likely to proxy for lack of intended care at the lowest quantiles and more likely to proxy for a problem-free pregnancy at the highest quantiles. At the intermediate quantiles, the effect of the no-prenatal-care indicator is found to be statistically insignificant in both the cross-sectional and panel results. For the third-trimester-care indicator variable, the cross-sectional and panel estimates are similar, indicating positive effects (as compared to first-trimester care) which become less statistically significant at higher quantiles. For the indicator variables, the largest difference between the cross-sectional and panel results shows up in the second-trimester-care variable; the cross-sectional estimates are statistically significant at all quantiles and range from 25 to 50 grams, whereas the panel estimates are somewhat lower (close to zero in intermediate quantiles) and only significantly positive at the highest quantiles. The effect of the number of prenatal visits is estimated to be significantly positive across all quantiles, with larger effects found at lower quantiles and the effects essentially "flattening out" (at around 14–15 grams per visit for the cross-sectional results and 12–13 grams per visit for the panel results). The estimated effects for the panel specification exhibit a sharper decline, leading to lower estimates (roughly a 2-gram per-visit differential) than the cross-sectional specification. This variable shows up significantly in the λ_τ^1 and λ_τ^2 estimates (see Tables 6 and 7), leading to the differences found and suggesting that the variable is correlated with the mother-specific unobservable.

Smoking: The most dramatic difference between the cross-sectional and panel results involves the estimated effects of smoking. The cross-sectional results indicate that the negative effects of smoking are in the range of 150–200 grams, with larger effects at lower quantiles. The panel

[12] This idea has been independently investigated by Conway and Deb (2005), who (i) find that bimodal residuals result from a standard 2SLS regression of birthweight and (ii) use a two-class mixture model to explicitly allow for a difference between "normal" and "complicated" pregnancies.

estimates are still significantly negative at all but the lowest quantiles, but the estimated effects are much lower in magnitude (mostly in the 50–80 gram range between the 20% and 80% quantiles). The omitted-variables explanation of this large difference would be that the smoking indicator in the cross-sectional specification is negatively correlated with the error disturbance in the birthweight regression equation. Consistent with this explanation, the smoking coefficients in both λ_τ^1 and λ_τ^2 are found to be significantly negative across the quantiles (see Tables 6 and 7). The magnitudes of the cross-sectional and panel estimates are roughly in agreement with those found by Abrevaya (2005) for the (conditional expectation) effects in federal natality data. Misclassification of smoking status could explain part of the difference found here since the effect of misclassification is more severe in the panel-data case (see, for example, Freeman (1984) and Jakubson (1986)). However, Abrevaya (2005) finds that the misclassification rate would have to be unrealistically large (with roughly 50% of smokers being misclassified as non-smokers) to explain the difference in estimates.[13]

Alcohol consumption: In contrast to the smoking results, the estimated effects of alcohol consumption (as measured by the alcohol-consumption indicator variable) are quite similar for the cross-sectional and panel specifications. Drinking is estimated to have significant negative effects at lower quantiles (below about the 20% quantile), with the magnitudes of the effects ranging between about 40 and 80 grams. Of course, very few mothers actually report that they consumed alcohol during pregnancy (only about 1.5% in our sample). The lack of strong statistical evidence regarding the effects of drinking could stem from the low variation in the indicator variable and the probable large rates of misclassification.

4.1.2 Arizona data

Figures 4 and 5 plot the estimated quantile effects (6% through 94% quantiles, inclusively) for the Arizona maternally-linked sample. The same model specification discussed above was used, except that indicator variables for second-trimester and third-trimester prenatal care were not included. The figures are comparable to Figures 1 and 2 for the Washington data, with the age effect reported at 25 years and the education effect at 12 years.

Overall, there is a remarkable similarity between the results for the two samples. The common findings for the two samples include the following:

- There is a significant positive effect of the second child across all quantiles (50–110 grams from the Arizona panel estimates).

[13] Moreover, if misclassification is correlated across births for individual mothers, the (unconditional) misclassification rate would need to be even higher to explain the observed difference in the estimates.

- The positive birthweight effect of a male child increases from lower to higher quantiles.
- Despite a positive estimated cross-sectional effect of education at lower quantiles, the panel estimates indicate no significant education effect.
- The effect of the number of prenatal visits is highest at lower quantiles, with the effect flattening out at higher quantiles. For both Washington and Arizona, the cross-sectional estimate of the effect is lower at lower quantiles and higher at higher quantiles.
- The magnitude of the negative smoking effect is significantly lower for the panel estimates (ranging between 40 and 80 grams for Arizona) than for the cross-sectional estimates.

Some differences between the results for the two samples are also worth noting:

- Although the cross-sectional estimates of the marriage effect are still significantly positive (p-values lower than 0.10 throughout the range of quantiles), the panel-data estimates indicate no statistically significant effect of marriage for Arizona mothers. The likely explanation of this finding is that the father's date of birth is required to match for both births of an Arizona mother (see Section 3), meaning that the father is the same even if marital status differs across the births. For the Washington sample, a change in marital status might also be related to a change in father.
- Drinking is not found to have a statistically significant effect at any of the quantiles (either in the cross section or the panel).
- Due to the lack of indicator variables for second-trimester and third-trimester care, the estimated effects of the no-care indicator variable and the number of prenatal visits are slightly different. The magnitude of the quantile effects for number of prenatal visits is roughly 50% lower for the Arizona sample, although the shape of the quantile-effect curve is extremely similar. The shape of the no-prenatal-care effect is also very similar to that of Washington, but the estimated panel effects are not significantly different from zero at any of the quantiles.

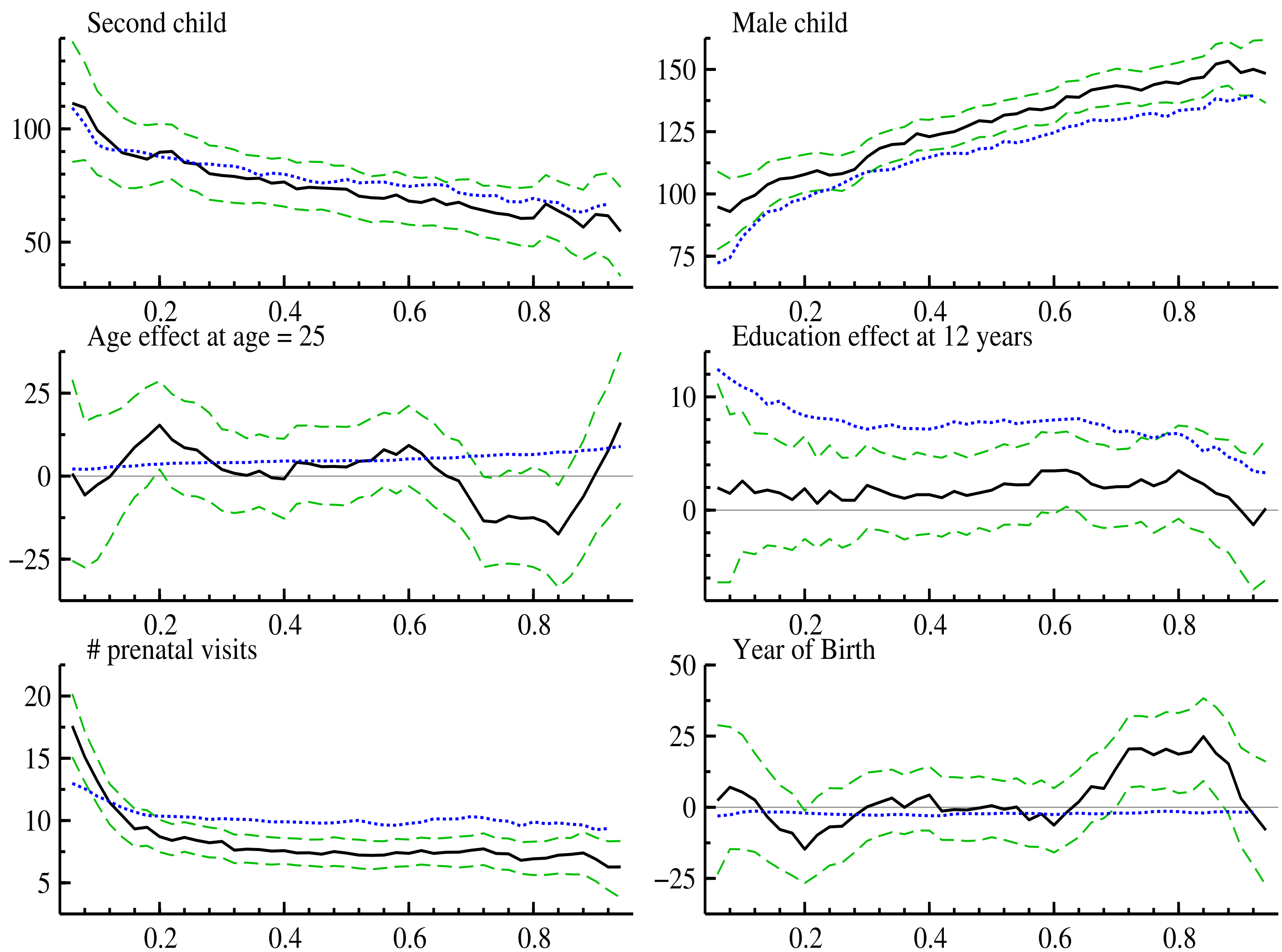

Figure 4: Arizona Results, Part 1 (Solid = Panel, Dashed = Panel 90% CI, Dotted = Cross-Section)

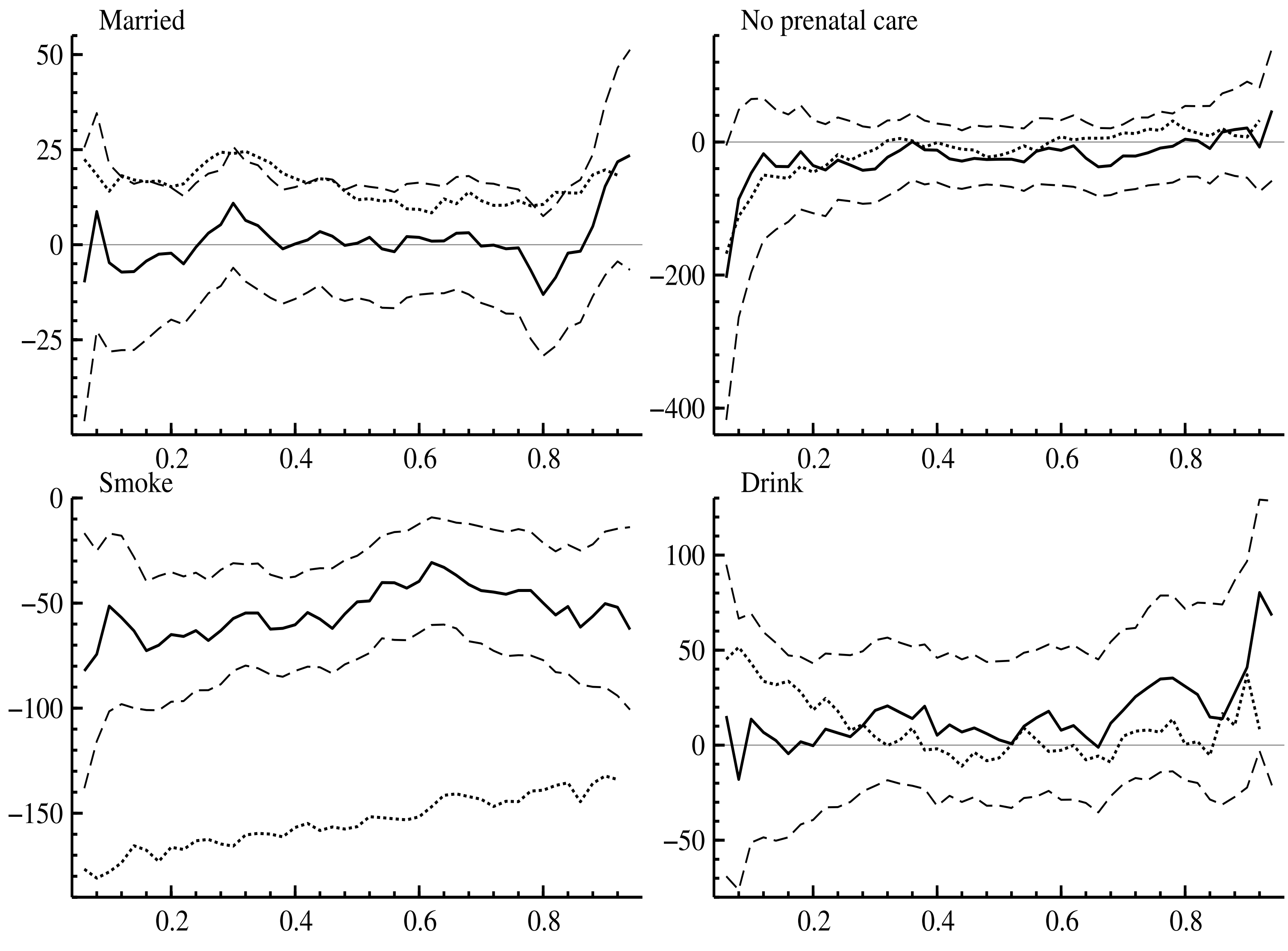

Figure 5: Arizona Results, Part 2 (Solid = Panel, Dashed = Panel 90% CI, Dotted = Cross-Section)

4.2 Hypothesis testing

In this section, we discuss the results of several hypothesis tests that were used in order to test the model specification and/or the significance of differences across the estimates at different quantiles. The minimum-distance (MD) framework of Buchinsky (1998) is used (and extended to the panel-data case) to test various (linear) restrictions placed on the parameters in the estimated models.

4.2.1 Minimum-distance testing framework

Let p denote the number of different quantiles at which the model is estimated, with $\tau_1, \ldots, \tau_p$ denoting the quantiles. For a given quantile τ, individual elements of the parameter vectors β, λ^1_τ, and λ^2_τ (recall the model in (17) and (18)) are referenced by subscripts as follows:

$$\begin{aligned}
\beta_\tau &= (\beta_{\tau 1}, ..., \beta_{\tau K})' \\
\lambda^1_\tau &= \left(\lambda^1_{\tau 1}, \ldots, \lambda^1_{\tau K}\right)' \\
\lambda^2_\tau &= \left(\lambda^2_{\tau 1}, \ldots, \lambda^2_{\tau K}\right)',
\end{aligned}$$

where K is the number of variables in x_{m1} and x_{m2}. Then, for a given quantile τ, the full parameter vector is denoted

$$\gamma_\tau \equiv \left(\phi^1_\tau, \beta_{\tau 0}, \beta'_\tau, \lambda^{1\prime}_\tau, \lambda^{2\prime}_\tau\right)', \tag{20}$$

where $\beta_{\tau 0} \equiv \phi^2_\tau - \phi^1_\tau$. The (stacked) parameter vector for all of the estimated quantiles is denoted

$$\gamma \equiv (\gamma'_{\tau_1}, \gamma'_{\tau_2}, \ldots, \gamma'_{\tau_p})' \tag{21}$$

and has dimension $p(3K+2) \times 1$. Further, let $\widehat{\gamma}$ denote the estimator of γ, and define $\widehat{A}$ to be the estimated variance-covariance matrix (obtained via the bootstrap) of $\widehat{\gamma}$.

In the MD framework, the "restricted" parameter estimator is defined as

$$\widehat{\gamma}^R = \underset{\gamma^R \in \Theta}{\arg\min} \left(\widehat{\gamma} - R\gamma^R\right)' \widehat{A}^{-1} \left(\widehat{\gamma} - R\gamma^R\right), \tag{22}$$

where R is a restriction matrix that will depend on the type of restrictions imposed. Since only linear restrictions are considered, $\widehat{\gamma}^R$ can be written explicitly as

$$\widehat{\gamma}^R = \left(R'\widehat{A}^{-1}R\right)^{-1} \left(R'\widehat{A}^{-1}\widehat{\gamma}\right). \tag{23}$$

The asymptotic variance of $\widehat{\gamma}^R$ is given by

$$var(\widehat{\gamma}^R) = \left(R'\widehat{A}^{-1}R\right)^{-1}. \tag{24}$$

For the purposes of hypothesis testing, note that under the null hypothesis that the restrictions are true (i.e., $H_0: \ \gamma = R\gamma^R$), the following MD test statistic has a limiting chi-square distribution:

$$\left(\widehat{\gamma} - R\widehat{\gamma}^R\right)' \widehat{A}^{-1} \left(\widehat{\gamma} - R\widehat{\gamma}^R\right) \xrightarrow[H_0]{d} \chi^2_M, \tag{25}$$

where M is the number of restrictions (i.e., $M = rows\,(R) - columns\,(R)$). The Appendix provides specific details on the appropriate choice of R and M for each of the tests described below.

4.2.2 Test results

Using the MD testing framework, the following hypothesis tests were conducted:

Test of correlated random effects: To determine whether a "pure" random effects specification (in which c_m is uncorrelated with x_m) would be rejected for a given quantile τ, the null hypothesis $H_0 : \lambda^1_\tau = \lambda^2_\tau = 0$ is tested. For each of the quantiles ($\tau \in \{0.10, 0.25, 0.50, 0.75, 0.90\}$) reported in Table 3, the null hypothesis is overwhelmingly rejected with a p-value extremely close to zero.

Test of the equality of the "effect vector" across quantiles: This test considers whether there are *any* statistically significant differences in the β_τ estimates across two different quantiles. Table 4 summarizes the results of this test applied to every pairwise combination of quantiles from the set $\{0.10, 0.25, 0.50, 0.75, 0.90\}$. The table reports the p-values of these pairwise tests for the panel specifications for both Washington and Arizona. For Washington, the p-values indicate very significant differences across the quantiles. The largest p-values arise for the adjacent quantiles in the panel specification, but these are all still below 2%. For Arizona, there are again very significant differences between the lowest quantiles (10% and 25%) and other quantiles, but the p-values are quite high for 50%/90% comparison (p-value of 0.231) and the 75%/90% comparison (p-value of 0.859). (The pairwise p-values for the cross-sectional specifications, which were computed but are not reported, were all lower than their panel-data counterparts.)

Test of the equality of individual variables' effects across quantiles: For a given variable (for example, marital status), this test checks whether the estimated effects at different quantiles are significantly different. For the results reported here, the set of different quantiles considered is the same as that used in Tables 2 and 3. For the marriage indicator, for instance, the null hypothesis would be $H_0 : \beta^{married}_{\tau=0.10} = \beta^{married}_{\tau=0.25} = \beta^{married}_{\tau=0.50} = \beta^{married}_{\tau=0.75} = \beta^{married}_{\tau=0.90}$. Since both age and education enter into the model specification in two terms (a linear term and a quadratic term), the appropriate tests for these two variables are joint tests of equality. The

Table 4: Pairwise Tests of β_τ Equality Across Quantiles (p-values reported)

Panel Specification (Washington)

	10%	25%	50%	75%
25%	0.000			
50%	0.000	0.015		
75%	0.000	0.002	0.020	
90%	0.000	0.000	0.000	0.012

Panel Specification (Arizona)

	10%	25%	50%	75%
25%	0.000			
50%	0.000	0.001		
75%	0.000	0.000	0.061	
90%	0.000	0.000	0.231	0.859

Based on 1,000 bootstrap replications.

test results (p-values) for all of the variables, in both the cross-sectional and panel specifications, are reported in Table 5 for Washington and Arizona. The results are very much in line with the quantile-estimate graphs in Figures 1–2 and Figures 4–5. Two variables (male-child indicator and number of prenatal visits) vary significantly across the quantiles for both the cross-sectional and panel specifications. The effect of the no-prenatal-care indicator also varies significantly (p-value of 0.013 in the cross section and 0.005 in the panel) for the Washington sample. On the other hand, there is no statistical evidence that the effects of marital status, drinking, or birth year vary over quantiles in either specification. The cross-sectional estimated effects of both age and education vary significantly across quantiles, whereas the panel estimated effects do not. Interestingly, for the smoking-indicator variable, the p-value for the Washington cross-sectional results is quite high (0.396) even though Figure 2 had suggested a slight decline in the magnitude of the smoking effect at higher quantiles. In contrast, the p-value for the smoking variable in the Washington panel specification suggests a significant difference in the estimated effects across quantiles. Finally, it should be noted that the choice of the quantile set $\{0.10, 0.25, 0.50, 0.75, 0.90\}$ is admittedly arbitrary, following what has apparently become the convention in the field of quantile regression. Other choices of the quantile set would obviously yield different numerical results (p-values), but it would be surprising if they resulted in qualitatively different results.

Table 5: Testing Equality of Marginal Effects Across Quantiles (p-values reported)

	Washington		Arizona	
	Cross Section	Panel Data	Cross Section	Panel Data
Second child	0.121	0.057	0.000	0.061
Male child	0.000	0.000	0.000	0.000
Age, Age^2 jointly	0.010	0.246	0.000	0.450
Education, Education^2 jointly	0.012	0.358	0.001	0.946
Married	0.521	0.451	0.677	0.705
No prenatal care	0.013	0.005	0.359	0.867
2nd-trimester care	0.573	0.095	—	—
3nd-trimester care	0.109	0.610	—	—
Smoke	0.396	0.045	0.160	0.976
Drink	0.318	0.429	0.327	0.834
# prenatal visits	0.004	0.000	0.010	0.000
Year of birth	0.512	0.642	0.959	0.073

Results based on 1,000 bootstrap replications.

5 Conclusion

This paper has considered estimation of the effects of various prenatal-care variables and maternal characteristics upon quantiles of the (conditional) birthweight distribution. To deal with the unobserved heterogeneity of childbearing women, a panel dataset consisting of maternally-linked births was utilized. The estimated conditional quantile effects are analogous to the conditional expectation effects that arise from the correlated random-effects model of Chamberlain (1982, 1984). Since the quantile-regression techniques (and testing methodology) are straightforward to apply and the estimated effects have a rather simple interpretation, the approach of this paper should be useful for other researchers seeking to estimate "causal" quantile effects through the use of panel data. In situations where panel data is not available, estimation of "causal" quantile effects in a cross-sectional setting has recently been considered in several studies, including Abadie et. al. (2002) and Chernozhukov and Hansen (2004, 2005).

Appendix A: Details on hypothesis testing

This section of the Appendix provides details on the hypothesis tests conducted in Section 4.2.2.

- (*Test of correlated random effects*) Test of $H_0 : \lambda^1_{\tau i} = 0 \wedge \lambda^2_{\tau i} = 0$ simultaneously $\forall i \in$

$\{1,\ldots,K\}$ and $\forall\tau \in \{\tau_1,\tau_2,\ldots,\tau_p\}$. Define

$$R' \equiv \begin{bmatrix} I_{p\times p}\otimes \begin{bmatrix} 1 & O_{1\times((K+1)+2K)} \end{bmatrix} \\ I_{p\times p}\otimes \begin{bmatrix} O_{(K+1)\times(K+2)} & I_{(K+1)\times(K+1)} & O_{(K+1)\times 2K} \end{bmatrix} \end{bmatrix},$$

and use $M = 2pK$.

- (*Test of the equality of the "effect vector"*) Test of $H_0 : \beta_{\tau_1 i} = \beta_{\tau_2 i} = \cdots = \beta_{\tau_p i}$ simultaneously for $\forall i \in \{0,1,2,\ldots,K\}$. Let i_p be a $(p,1)$ vector of ones. To perform this test, define

$$R' \equiv \begin{bmatrix} I_{p\times p}\otimes \begin{bmatrix} 1 & O_{1\times((K+1)+2K)} \end{bmatrix} \\ I_{p\times p}\otimes \begin{bmatrix} O_{2K\times(K+2)} & I_{2K\times 2K} \end{bmatrix} \\ i_p'\otimes \begin{bmatrix} O_{(K+1)\times 1} & I_{(K+1)\times(K+1)} & O_{(K+1)\times 2K} \end{bmatrix} \end{bmatrix}$$

and use $M = (p-1)(K+1)$.

- (*Test of the equality of individual variables' effects (single parameter)*) Test of $H_0 : \beta_{\tau_1 i} = \beta_{\tau_2 i} = \cdots = \beta_{\tau_p i}$ for a single $i \in \{0,1,2,\ldots,K\}$. Let

$$\begin{aligned} E_1 &\equiv I_{p\times p}\otimes \begin{bmatrix} 1 & O_{1\times((K+1)+2K)} \end{bmatrix}, \\ E_2 &\equiv I_{p\times p}\otimes \begin{bmatrix} O_{2K\times(K+2)} & I_{2K\times 2K} \end{bmatrix}, \\ E_3 &\equiv i_{p-1}'\otimes \begin{bmatrix} O_{(K+1)\times 1} & D_{ii,(K+1)\times(K+1)} & O_{(K+1)\times 2K} \end{bmatrix}, \end{aligned}$$

and

$$E_4 \equiv \begin{bmatrix} O_{(K+1)\times 1} & I_{(K+1)\times(K+1)} & O_{(K+1)\times 2K} & E_3 \\ O_{(p-1)K\times 1} & O_{(p-1)K\times(K+1)} & O_{(p-1)K\times 2K} & I_{(p-1)\times(p-1)}\otimes S_{-i\cdot} \end{bmatrix},$$

where

$$S \equiv \begin{bmatrix} O_{(K+1)\times 1} & I_{(K+1)\times(K+1)} & O_{(K+1)\times 2K} \end{bmatrix},$$

and $S_{-i\cdot}$ is equal to S without the i'th row. $D_{ii,(K+1)\times(K+1)}$ is a matrix of zeros except for the entry (i,i) which equals unity. Then, the test of H_0 can be performed by defining $R \equiv (E_1', E_2', E_4')'$, with $M = p-1$.

- (*Test of the equality of individual variables' effects (joint test of two parameters)*) Test of $H_0 : \beta_{\tau_1 i} = \beta_{\tau_2 i} = \cdots = \beta_{\tau_p i} \wedge \beta_{\tau_1 j} = \beta_{\tau_2 j} = \cdots = \beta_{\tau_p j}$ for $i,j \in \{0,1,2,\ldots,K\}$ and $i \neq j$. Let

$$\begin{aligned} E_1 &\equiv I_{p\times p}\otimes \begin{bmatrix} 1 & O_{1\times((K+1)+2K)} \end{bmatrix}, \\ E_2 &\equiv I_{p\times p}\otimes \begin{bmatrix} O_{2K\times(K+2)} & I_{2K\times 2K} \end{bmatrix}, \\ E_3 &\equiv i_{p-1}'\otimes \begin{bmatrix} O_{(K+1)\times 1} & D_{(ii,jj)(K+1)\times(K+1)} & O_{(K+1)\times 2K} \end{bmatrix}, \end{aligned}$$

and

$$E_4 \equiv \begin{bmatrix} O_{(K+1)\times 1} & I_{(K+1)\times(K+1)} & O_{(K+1)\times 2K} & E_3 \\ O_{(p-1)(K-1)\times 1} & O_{(p-1)(K-1)\times(K+1)} & O_{(p-1)(K-1)\times 2K} & I_{(p-1)\times(p-1)}\otimes S_{-ij\cdot} \end{bmatrix},$$

where

$$S \equiv \left[\begin{array}{ccc} O_{(K+1)\times 1} & I_{(K+1)\times(K+1)} & O_{(K+1)\times 2K} \end{array} \right],$$

and $S_{-ij\cdot}$ is equal to S without rows i and j. $D_{(ii,jj)(K+1)\times(K+1)}$ is a matrix of zeros except for the entries (i,i) and (j,j) which both equal unity. To test H_0, define $R \equiv (E_1', E_2', E_4')'$ and use $M = 2(p-1)$.

Appendix B: Additional results

This section of the Appendix contains the Washington results for the estimates of λ_τ^1 and λ_τ^2 (for $\tau \in \{0.10, 0.25, 0.50, 0.75, 0.90\}$) in Tables 6 and 7, respectively.

Table 6: Panel-Data Estimation Results for λ_τ^1, Washington Data

	Quantile regressions					
	10%	25%	50%	75%	90%	OLS
Male child	-30.857***	-28.675***	-25.584***	-23.708***	-16.161**	-23.806***
	(7.555)	(5.394)	(4.791)	(5.602)	(7.398)	(4.460)
Age	-9.694	-16.364	-4.497	-5.712	5.407	-12.165
	(17.078)	(10.905)	(9.701)	(10.350)	(13.419)	(9.570)
Age^2	0.209	0.330*	0.158	0.028	-0.139	0.236
	(0.289)	(0.183)	(0.170)	(0.178)	(0.229)	(0.162)
Education	-13.505	10.740	10.072	11.312	28.271**	11.474
	(15.211)	(11.101)	(11.084)	(8.528)	(11.808)	(8.409)
Education^2	0.542	-0.242	-0.366	-0.372	-0.942*	-0.350
	(0.653)	(0.457)	(0.446)	(0.376)	(0.537)	(0.356)
Married	-5.964	3.108	-8.461	-3.034	-1.918	-6.866
	(14.967)	(9.891)	(8.853)	(10.138)	(14.279)	(8.428)
No prenatal care	101.967	93.632**	38.390	51.110	-72.802	36.052
	(83.390)	(45.514)	(43.516)	(46.157)	(54.069)	(38.342)
2nd-trimester care	25.291*	24.658**	30.234***	12.346	19.901	20.927**
	(14.700)	(10.583)	(8.535)	(9.936)	(13.153)	(8.386)
3rd-trimester care	77.140**	21.917	18.521	16.725	1.121	31.732
	(30.594)	(22.977)	(22.176)	(25.450)	(33.885)	(20.161)
Smoke	-102.682***	-60.559***	-51.977***	-79.282***	-71.567***	-70.500***
	(16.452)	(12.636)	(9.859)	(11.726)	(14.822)	(9.599)
Drink	7.639	-1.262	-22.244	-14.158	-13.765	-1.669
	(30.612)	(21.502)	(21.802)	(25.047)	(28.027)	(18.455)
# prenatal visits	6.566***	7.227***	6.795***	6.901***	6.709***	5.597***
	(1.464)	(1.109)	(0.868)	(1.092)	(1.292)	(0.970)
Year of birth	8.375	2.200	-0.414	5.050	1.520	4.562
	(6.496)	(4.634)	(4.226)	(4.955)	(5.995)	(3.890)

Bootstrapped standard errors in parentheses, using bootstrap sample size of 20,000 (10,000 pairs) and 1,000 bootstrap replications.

'*': significant at 10 percent level, double-sided (normal dist.).
'**': significant at 5 percent level, double-sided (normal dist.).
'***': significant at 1 percent level, double-sided (normal dist.).

Table 7: Panel-Data Estimation Results for λ_τ^2, Washington Data

	Quantile regressions					
	10%	25%	50%	75%	90%	OLS
Male child	2.687	-2.298	-9.551*	-9.439*	-7.372	-5.574
	(7.933)	(5.427)	(5.007)	(5.500)	(7.163)	(4.557)
Age	76.044***	54.120***	53.966***	50.309***	56.741***	62.069***
	(15.685)	(11.456)	(9.896)	(10.407)	(14.098)	(9.483)
Age^2	-1.338***	-0.959***	-0.934***	-0.824***	-0.946***	-1.081***
	(0.253)	(0.180)	(0.162)	(0.168)	(0.226)	(0.152)
Education	-2.108	-4.658	-9.434	1.245	3.751	2.111
	(20.292)	(13.113)	(10.205)	(9.472)	(12.075)	(9.369)
Education^2	0.370	0.528	0.522	0.042	0.090	0.177
	(0.788)	(0.525)	(0.422)	(0.401)	(0.529)	(0.385)
Married	17.254	7.329	6.918	4.698	10.464	7.531
	(14.210)	(12.002)	(9.029)	(10.503)	(14.104)	(8.767)
No prenatal care	-99.718	-95.052*	-100.908*	-81.966	-123.891	-65.531
	(100.990)	(51.917)	(54.525)	(58.155)	(97.169)	(54.202)
2nd-trimester care	9.421	12.672	15.260	3.171	-6.068	11.843
	(14.211)	(10.296)	(9.469)	(10.085)	(13.497)	(8.158)
3rd-trimester care	6.158	-39.525	11.559	-21.768	-23.718	-8.217
	(37.490)	(26.726)	(25.653)	(23.503)	(31.090)	(20.345)
Smoke	-84.991***	-83.882***	-69.294***	-73.756***	-56.860***	-78.492***
	(16.275)	(12.908)	(11.112)	(10.591)	(15.561)	(9.230)
Drink	15.707	3.962	6.114	-19.202	24.742	7.778
	(37.664)	(28.183)	(19.269)	(21.913)	(31.972)	(19.440)
# prenatal visits	-7.520***	-4.268***	-3.311***	-3.317***	-3.327**	-4.633***
	(1.568)	(1.121)	(0.843)	(0.902)	(1.323)	(0.946)
Year of birth	4.240	2.774	2.385	1.647	2.230	3.872
	(6.531)	(4.609)	(4.556)	(4.542)	(6.494)	(3.654)

Bootstrapped standard errors in parentheses, using bootstrap sample size of 20,000 (10,000 pairs) and 1,000 bootstrap replications.

'*': significant at 10 percent level, double-sided (normal dist.).
'**': significant at 5 percent level, double-sided (normal dist.).
'***': significant at 1 percent level, double-sided (normal dist.).

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