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Rejoinder

Francesca Dominici*, Scott L. Zeger†, Giovanni Parmigiani‡,
Joanne Katz§, and Parul Christian¶,

We would like to thank Drs. Ruppert, Carroll, Cook and Stuart, for their insightful and constructive comments. Their discussion has provided new insights into our proposed approach. We are in agreement with all their points. Below is a summary of our comments in response to their suggestions.

Drs. Ruppert and Carroll correctly pointed out that our measurement error analysis, aimed at predicting the birth weight $W_i(0), W_i(1)$ for the babies that had their weights measured after the 72 hours, relies on parameter estimation outside the Gibbs Sampling. We agree that our approach might underestimate the uncertainty. However with 800 observations, we believe that the linear regression model for the pairs of points $(W_{t_i}(z), t_i)$ is estimated well. The authors proposed an elegant alternative, a heteroskedastic measurement error model, which is consistent with a full Bayesian analysis. We applaud the authors for such an ingenious idea. Visual inspection of the cross-sectional data (Figure 2) indicates that heteroskedasticity might not be a major issue. However, their approach is still challenged by the lack of longitudinal data on the birth weight, and as in our formulation, it must rely on informative prior assumptions or additional data sources.

The authors introduced a regression model for the birth weight that is consistent with our approach and that facilitates the elicitation of the prior value for ρ. Thank you! In fact, if there is no interaction between infants and treatment, then $W_i(0) = W_i(1)$ and therefore $\rho = 1$. Therefore we agree with the authors that ρ may be even higher than the correlation between successive children with the same mother or even identical twins. At the other end, we think that it is unlikely for ρ to be negative. It is plausible to assume that the between infants heterogeneity $(\sigma^2_{w,1})$ will be larger (and not smaller) than the between infants heterogeneity on how they respond to the treatment. Regardless, the authors have provided a nice alternative way of thinking about this problem. We also agree that we could have used an informative prior on the non-identified nuisance parameters such as (ρ, ψ). We have just preferred to show the sensitivity of the results to alternative choices of ρ and ψ. Again, we agree with the authors that the use of penalized splines could have been a valid alternative.

*Department of Biostatistics, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD, http://www.biostat.jhsph.edu/~fdominic/

†Department of Biostatistics, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD, http://www.biostat.jhsph.edu/~szeger/

‡The Sidney Kimmel Comprehensive Cancer Center, Johns Hopkins University, Baltimore, MD, http://astor.som.jhmi.edu/~gp/

§Department of International Health, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD, http://faculty.jhsph.edu/?F=Joanne&L=Katz

¶Department of Internationa Health, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD, http://faculty.jhsph.edu/?F=Parul&L=Christian

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