Rejoinder

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I thank *Statistical Science* for organizing a balanced discussion. I feel fortunate that nearly every part of my paper is touched upon by discussants' comments. I will subtitle my responses with one main topic attributed to each discussant; I hope the discussants will agree with my classification. The criticism of the extended combining rules seems universal, and I list the topic under my own name because I want to criticize my rules, too! This Rejoinder is intended as a dessert (or appetizer)—tasteful but light (a bit heavier for Dr. Fay, according to his order).

FAY: FRACTIONALLY WEIGHTED (MULTIPLE) IMPUTATION

"Weighted" implies multiple, so I feel safe that Dr. Fay will not accuse me of putting words (i.e., multiple) into his term; I think it is for the users' benefit to name a method as descriptively as possible. Of course, adding "multiple" after "weighted" is a bit redundant, but I guess Dr. Fay will not mind as modifying "weighted" with "fractionally" also seems redundant.

After a few years of development (documented since 1991), Dr. Fay's proposal now also adopts the framework of multiple imputation. I am particularly pleased to see such a consensus, because now we can concentrate on how to create multiple imputations and how to analyze multiply imputed data. I also hope that this will send a message to those who have been justifying single imputation by citing Dr. Fay's "criticism" of *multiple* imputation. There are nonstatistical arguments for single imputation, such as cost and public perception of authenticity, but it is ironic for any statistician to argue for single imputation. Doctor Fay understands these nonstatistical constraints far better than I do, and yet still adopts multiple imputations. I hope others will not overlook this fact when they cite Dr. Fay's work.

Given the consensus that imputation should be created multiply, Dr. Fay's proposal, at least for now, differs from Rubin's approach in creating imputations and thus accordingly in analyzing the imputed data. Rubin's strategy for creating multiple imputation is to follow the Bayesian recipe—first simulating parameters from their posterior distribution under a specified model, and then simulating missing data from their sampling distribution conditional on the observed data and the simulated parameters; the actual cooking may vary (e.g., the model can be implicit, such as with the Bayesian bootstrap; Rubin, 1987, Chapter 3), but the two steps are necessary in general if the imputations are to reflect fully their uncertainties under a posited imputation model. The advantage of having imputations with the proper variability is that consequent analyses can follow simple laws of probability, such as "total variance equals within-imputation variance plus between-imputation variance." Rubin's approach of analyzing multiply imputed data is then built upon such simplicity—analyzing each imputed data set as if it were real and then combining these "complete-data" analyses by applying the simple combining rules reviewed in Section 2.4 of my paper. The theoretical part of my paper establishes the validity of these rules under conditions that are broader than previously investigated.

In contrast, Dr. Fay's current proposal (1994a) is to skip the first step of Rubin's strategy, that is, the step that reflects uncertainty in estimating model parameters. I have not seen Dr. Fay's general approach for creating his multiple imputations, but from his discussion of the approximate Bayesian bootstrap in Fay (1994a), I surmise that Dr. Fay's strategy is to fix the model parameters at some estimates. His approach of analysis is then to combine these imputations by weighting before performing one analysis—in contrast to Rubin's approach of performing separate analyses and then combining. Since Dr. Fay's imputations do not reflect all of their uncertainties, the simple variance decomposition adopted by Rubin no longer applies, and thus more complicated techniques need to be developed. In other words, Dr. Fay's analysis approach cannot take advantage of the simplicity of computing complete-data variances, but relies on specifically constructed procedures according to his imputation scheme and possibly also the estimators being used.

I do not view the computational complexity of Dr. Fay's analysis procedure as a disadvantage, if that is what it takes to solve the real problem; as I emphasized in Section 1, the focus here is not on computation. What I cannot see is how his approach can better handle the uncongenial cases, where Dr. Fay claims that Rubin's approach does not apply. Take the simple example in Section 3.1. My understanding of Dr. Fay's approach is to fix the subclass (corresponding to $x = 1$) mean at some estimator when cre-