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Rejoinder: The Future of Indirect Evidence

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Our three discussants fit an "ideal statistican" profile, combining deep theoretical understanding with serious scientific interests. The three essays—which are more than commentaries on my article—reflect in a telling way their different applied interests: Andrew Gelman in social sciences, Sander Greenland in epidemiology, and Robert Kass in neuroscience. Readers who share my bad habit of turning to the discussions first will be well rewarded here, but of course I hope you will eventually return to the article itself. There the emphasis is less on specific applications (though they serve as examples) and more on the development of statistical inference.

Figure 1 concerns the physicist's twins example of Section 3. From the doctor's prior distribution and the fact that sexes differ randomly for fraternal twins but not for identical ones, we can calculate probabilities in the four cells of the table. The sonogram tells the physicist that she is in the left-hand column, where there are equal odds on identical or fraternal, just as Bayes rule says. In my terminology, the doctor's indirect evidence is filtered by Bayes rule to reveal that portion applying directly to the case at hand.

There is a leap of faith here, easy enough to make in this case: that the doctor's information is both relevant and accurate. We would feel differently if the doctor's evidence turned out to be just three previous sets of twins, two of which were fraternal. A standard Bayesian analysis might then start from a beta(2, 3) hyperprior distribution on the prior probability of *identical*. The calculation of posterior odds would now be more entertaining than the actual one in Figure 1, but the results less satisfying.

		Sexes		
		Same	*Different*	
Twins	*Identical*	**1/3**	**0**	1/3 (Doctor)
	Fraternal	**1/3**	**1/3**	2/3 (Doctor)
		sonogram		

FIG. 1. *Probabilities relating to the physicist's twins example of Section* 3.

How much respect is due to conclusions that begin with priors, or hyperpriors, of mathematical convenience? The discussants are divided here: Gelman, judging from the examples in Chapter 5 of his excellent book with Carlin, Stern and Rubin, is fully committed; Kass, as a follower of Jeffries, is mildly agreeable but with strong reservations; while Greenland seems dismissive (calling objective Bayes "please don't bother me with the science' Bayes").

Section 4's empirical Bayes motivation for the James–Stein rule implicitly endorses Gelman's position, except that maximum likelihood estimation of M and A in (1) finesses the use of a vague hyperprior for them. The same remark applies to the discussion of false discovery rates in Section 6. By Section 9, however, my qualms, along Greenland's lines, become evident: do the estimates $\hat{\mu}_i$ in Table 2 fully account for selection bias, as they would in a genuine Bayesian analysis? Kass and I part company here. I believe we need, and might get, a more complete theory of empirical Bayes inference while he is satisfied with the present situation, at least as far as applications go. Gelman is happy with both theory and applications.

The ground is steadier under our feet for both James–Stein and Benjamini–Hochberg thanks to their frequentist justifications, Theorems 1 and 2. We do not really need those prior distributions (1) and (7). The procedures have good consequences guaranteed for *any* possible prior, which is another way of stating the frequentist ideal. My "good work rules" comment in Section 10 had in mind the emergence of key ideas such as JS and BH from the frequentist literature.

Gelman is certainly right: Bayesian statistics has transformed itself over the past 30 years, riding a hierarchical modeling/MCMC wave toward a stronger connection with scientific data analysis. This does not make it an infallible recipe. MCMC methodology has encouraged the use of mathematically convenient distributions at the hyperprior level, perhaps a dangerous

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