Nodland and Ralston Reply: Eisenstein and Bunn's (EB) Comment [1] inaccurately reports what we did [2], is incorrect in several assertions, and does not alter our conclusions. We welcome the opportunity to clear up the matter.

Contrary to what EB say, our β is derived from observables χ and ψ and parameter $\vec{s}$. Second, EB misreport how β was chosen. It was the χ's and ψ's that came from uniform, uncorrelated distributions, which happens to be what is seen in the full data set. The Monte Carlo method assigned random variables to calculate β, using just the same rules as used for the data. This took into account the nonuniformity of the distribution on the sky. The distribution of β is whatever the Monte Carlo method decides. In procedure 1, the distribution in β in separate quadrants might be called uniform, but only in the limit of an infinite population. Each trial actually took into account fluctuations from finite statistics, a big effect for 71 galaxies. The population on the sky and the trial $\vec{s}$ determined each quadrant's population. The R correlations are very sensitive to this, and it is misleading for EB to oversimplify this as "uniform." In procedure 2, the Monte Carlo procedure varies $\vec{s}$ to maximize correlations caused by finite statistics, case by case. Trial by trial, the program adjusts parameters to select whatever distribution can maximize R from the fluctuations. It is a fact that peaked distributions have a higher R than flat ones. There is an advantage for the Monte Carlo to select these. EB neglected to take any of this into account.

As a null hypothesis, EB engage in modeling the data, their model being that the data are whatever they see on a single scatterplot for β for a particular value of $\vec{s}$ after all cuts were applied. The concept is rather circular. The approach is faulty, because the variable β is a secondary quantity, defined via $\vec{s}$ to test for anisotropy. For a null test assuming no anisotropy, it is unphysical to model unobserved "intrinsic β's." But then, modeled intrinsic distributions of the *physical variables* χ and ψ will not generally produce β's giving our correlations. These facts are obscured by EB's use of β as if it were a raw variable.

EB made a big deal of eyeballing a single scatterplot, saying that the β's are "more tightly correlated than data uniformly distributed." Yes, and this is expected. The one figure shows data processed with the good $\vec{s}$ and remaining after the cut $z \geq 0.3$. The cut depletes values of $r\cos\gamma$'s around the origin of the horizontal axis. The data is correlated like $\beta = r\cos\gamma$, and one finds a hole in the distribution of β's around the origin. Astronomical selection sets in at large $|r\cos\gamma|$. Again, because β and $r\cos\gamma$ are correlated, there is a shortage of large $|\beta|$'s.

The distribution of β's seen with our correlation is meaningful, and would be predicted in advance, given the cuts. To turn this around as a sign that something is amiss with a "null hypothesis" is very unfortunate. EB go on to suggest to "draw the angles (β) from the observed distribution," saying that "the data are not significantly more correlated than they would be if the β values were shuffled among themselves." But EB did not do the calculation, and their claim is wrong. We did the calculation, and the correlation is still significant. The $1/P$ versus $\vec{s}$ plot [our Fig. 1(b) in Ref. [2]] still showed a bump, with the bump at the same direction of $\vec{s}$ as before.

Again, this is perfectly natural, and would be predicted on the basis that the β's are correlated. Take laboratory data that exhibit a linear relation of the form $y = mx$, plotted on a diagonal line. With x cuts like ours, the data are restricted to boxes sitting on the line in two quadrants. Using data from the boxes and shuffling it, as EB prescribe, one makes a set of faux-random data. Rather than serving as a benchmark for correlations, every shuffling generates a set that is highly precorrelated. Compare, anyway, the precorrelated shufflings with the better correlated real data, and one will find a correlation. The baseline for what is declared "relatively likely" simply gets moved up. A real correlation can be artificially made to look more probable.

We find it a pretty bad procedure if a probability arbitrarily close to unity can be assigned through such a comparison, as one finds, e.g., for reshuffling of the y values in a $\delta(y - x)$ function distribution in the limit of strong x cuts. How big is the effect for us? The procedure increased P values at the peak in the $1/P$ plots by a factor of about 10. Even so, our correlation is still quite significant.

Most objectionable is the assertion that someone else's secondary observation had to be employed as the only proper null hypothesis. EB's suggestion is not capable of ruling out anisotropy: for strong cuts and a good correlation, it can inadvertently reject a signal. EB might advocate an intrinsic distribution for χ and ψ, but this does not explain the correlation seen between β and $r\cos\gamma$. Population effects do not explain the tuning of the correlation as the parameter $\vec{s}$ is varied over the dome of the sky. The EB proposal was tested by shuffling, and it does not work. In light of the facts, our conclusion that a signal of anisotropy exists in the data is unchanged.

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[1] Daniel J. Eisenstein and Emory F. Bunn, preceding Comment, Phys. Rev. Lett. **79**, 1957 (1997).
[2] B. Nodland and J. P. Ralston, Phys. Rev. Lett. **78**, 3043 (1997).

