Briere and Winstein Reply: We address below each of the criticisms in the preceding Comment [1].

Our claim to have *measured* ϕ_{+-} is termed misleading due to the need to account for the regeneration phase ϕ_f. In Ref. [2] it is clearly stated that we need ϕ_f and that we determine it from the momentum dependence of $f - \overline{f}$; we have written another paper [3] detailing our methods. We do not see how this can be misleading.

It is then noted that the value of $\phi_{+-} - \phi_f$ is not quoted. This is because ϕ_f is slightly momentum dependent; our treatment of this is detailed in Ref. [3].

Our systematic error due to ϕ_f is criticized based on Ref. [4], which concludes that an error of $\pm 3°$ is appropriate. Reference [3] contains several specific criticisms of Ref. [4], none of which has been answered. We maintain that the $\pm 3°$ error from Ref. [4] is flawed for the reasons we gave and that the treatment of Ref. [3] is far more complete. The dispersion-integral basis of the two treatments is, however, identical.

Next, the validity of derivative analyticity relations (DAR) is questioned. DARs motivate the quoted behaviors, but are not used in our actual calculations; perhaps this is not clear enough.

The failings of the DAR are worth mentioning, however. There are indeed problems in naively extending parametrizations of scattering amplitudes to low energy; effects of resonances, thresholds, and poles must be evaluated. We limit these with full dispersion relations, which give the energy dependence of distortions to the naive results. We employ the accurate low-energy *data* of Ref. [5], not any prejudice about expected behavior. None of these effects is mentioned in Ref. [4].

We are quoted as "assuming that regeneration off carbon comes only from (the) ω . . . ." While a statement *similar* to this is indeed made, it is in the context of an approximate treatment early in the paper. (The passage actually reads "assume for now. . . .")

Kleinknecht next compares the power law of $\alpha - 1 = -0.572 \pm 0.007$ obtained in Ref. [2] for regeneration from carbon and the bare ω intercept of $\alpha_\omega = 0.437 \pm 0.007$ from Ref. [3]. These two quantities differ due to nuclear screening in carbon. One *cannot* obtain an accurate ϕ_f for a complex nucleus from α_ω which applies to bare nucleons. Much of Ref. [3] is devoted to discussing screening, and the value of α_ω is given for comparisons with other determinations.

Next, it is correctly inferred that an error of ± 0.007 on the power law, α, leads to a $\sim 0.6°$ error on ϕ_f. However, in fitting the data (simultaneously for α and ϕ_{+-}) this error is correlated with the error on $\phi_{+-} - \phi_f$. The sign of the correlation is such that the error on ϕ_{+-} is reduced. The lack of proper treatment of such correlations was one of our (unanswered) criticisms to Ref. [4].

We are further criticized for assuming regeneration to be a pure power law above 160 GeV. In fact, we have not made this assumption. As discussed in Ref. [3], we use our best knowledge of effects which distort the power law, including nuclear screening and electromagnetic regeneration, neither of which is considered in Ref. [4].

Finally, it is reiterated that ours is not a "real measurement" of ϕ_{+-} because we rely on estimates of dispersion integrals over regions where there is little or no data. Uncertainties in the behavior around threshold propagate to high energies with a characteristic $1/E$ dependence; given the accurate measurements of regeneration in the few GeV region together with our own ones between 20 and 160 GeV, we can limit these effects empirically. So let us focus on the high energy regions. From measurements over the past 20 years, a picture of the behavior of regeneration and its atomic number dependence at high energies has emerged; this is summarized in Ref. [3]. This understanding allows a reliable correction for unmapped regions to be made; our total systematic uncertainty due to ϕ_f is 0.35°. Along with our statistical errors, this allows accurate comparisons with predictions of *CPT* symmetry.

This situation is not unlike other precision measurements where comparison of experimental quantities with theory requires the estimation of corrections due to unmeasured regions. We are thinking in particular of the interpretation of lepton $g - 2$ measurements or the precision determinations of fundamental parameters at the Z peak. Yes, we cannot "prove experimentally" that there will not be anomalous behavior in the regeneration amplitude just above our energy range. But similarly, one cannot prove that there are not new heavy particles that significantly alter vertex or propagator corrections in the above experiments. Best efforts at limiting such effects have been made, often using theoretical assumptions, and one should not be criticized for reporting their results.

Roy A. Briere
Department of Physics
Harvard University
Cambridge, MA 02138

Bruce Winstein
The Enrico Fermi Institute
University of Chicago
Chicago, IL 60637

Received 25 September 1995
PACS numbers: 14.40.Aq, 11.55.Fv, 12.40.Nn

[1] K. Kleinknecht, preceding Comment, Phys. Rev. Lett **75**, 4784 (1995).
[2] B. Schwingenheuer *et al.,* Phys. Rev. Lett. **74**, 4376 (1995).
[3] R.A. Briere and B. Winstein, Phys. Rev. Lett. **75**, 402 (1995).
[4] K. Kleinknecht and S. Luitz, Phys. Lett. **B336**, 581 (1994).
[5] W.C. Carithers *et al.,* Nucl. Phys. **B118**, 333 (1978).

0031-9007/95/75(26)/4785(1)$06.00