with an n^{-1} term instead of an $n^{-1/2}$ term. As a result, the bootstrap approximation of the distribution of such a modified pivot is automatically third-order correct.

We believe that this extensive theoretical study will prove to be very helpful to the users of the bootstrap technique in making a "right choice" of a bootstrap confidence interval.

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As a bootstrap practitioner and applied econometrician/statistician, my comments will focus on the implications this fine paper in bootstrap theory has for applied research. From my perspective, it seems that two branches of research into bootstrap confidence intervals have developed. What I shall call the "asymptotic" approach follows the initial motivation of Efron and emphasizes finding confidence interval estimates where no feasible analytic alternatives exist. Much of Peter Hall's paper is instead in the "finite sample" branch in that, as he notes, it largely assumes that there are suitable analytic standard error estimates and hence analytic confidence intervals are available; the bootstrap is used to improve on the large sample approximations required for most confidence interval construction. Hall's research therefore provides a simulation alternative to analytic Edgeworth expansion and inversion. I find the asymptotic and finite sample approaches complementary and, interestingly, that they each correspond to growing needs in applied econometric research.

Applied econometricians, like other kinds of statistical practitioners, often diverge sharply from standard statistics textbook approaches. One such divergence has been the estimation of very complex models in which the formulae for standard errors are too complicated to calculate analytically. For these situations the bootstrap was an important innovation and found rapid acceptance among econometricians, many of whom were already using some kind of simulation/resample technique to aid inference in this context.

To take an example along this line from my own work, I used the bootstrap to estimate confidence intervals in a complicated forecasting problem involving future electricity demand [Veall (1987a)]. As have most econometricians, I used the percentile method. Hall's "looking-up-the-tables-backwards" argument has persuaded me that what he calls the hybrid method might have been a better choice, although as the bootstrap estimate of the bias was small and there was little indication of asymmetry, it would probably have not made much difference, a view largely supported by a Monte Carlo evaluation of the bootstrap confidence intervals themselves for this specific example [Veall (1987b)]. (Incidentally, it is my impression that for examples in which bias may be a problem, most econometricians do not simply try to correct for it and then consider it no further, but instead treat empirical indications of bias as a warning of potential danger.) In any case, without analytic standard errors Hall's recommended percentile-t method is infeasible, but as Hall notes the additional improvement "from using the correct tables" in his metaphor is only third order.

Another way some econometricians diverge from more standard statistical practice is, rightly or wrongly, to try to estimate quite large numbers of parameters from relatively small data sets. This is a consequence of the complexity of the economy coupled with the inability in most cases to run controlled experiments. In most of these cases, a method such as two-stage least squares has been used, so that exact tests are not available, but there are analytic (asymptotic) standard error estimates. Finite sample expansions and approximations have therefore always been an important topic for econometric theory, although perhaps the impact on practice has not been as great. Here the Hall approach may be very useful as it allows some of the gains of Edgeworth approximations and the like, but as in the initial motivation for the bootstrap, substitutes computer calculations for tedious algebra. While Hall studies a variety of criteria, it is the avoidance of analytic correction that would steer me toward the percentile-t over the accelerated bias correction, when the former is available.

There is an intermediate case between the asymptotic and finite sample approaches to bootstrap confidence intervals and that is when there is a standard error estimate available but it is not a good one. One example of Efron's cited by Hall [see also Efron (1987), page 199] shows that the percentile-t method can perform erratically when a jackknife estimate of the standard error has been used. Another example is nonlinear estimation where a perhaps crude linear approximation has been used to calculate standard errors. How poor does the standard error estimate have to be before it is desirable to jettison the percentile-t for the accelerated bias correction? Does one need to do both and compare? Guidance for the practitioner would be welcomed. As Hall seems to have exploited what is available from studying rates of convergence, it may be that this issue can best be addressed by simulation.

Finally, to put in my rather tall order for what I would like to see from future developments in this literature, I note that these kinds of bootstrap corrections for joint confidence intervals and for joint tests of sets of restrictions would be very useful in application. Also it would be very desirable if bootstrap-type confidence intervals can be developed for the case where the bias is not finite-sample in nature but instead a consequence of the estimation process itself, such as in the example of variable selection for regression studied in the simulations of Freedman and Navidi (1986).

I thank the Editor for the opportunity to contribute to the discussion of this valuable paper.

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REJOINDER

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All the discussants make informed and penetrating comments. I am most grateful for the time they have devoted to this project. It is very interesting to see the lively debate among discussants—some of them strongly favor non-pivotal methods, others definitely like a pivotal approach. If I had to make predictions, I would say that in many years' time, when most of the dust has settled, pivotal methods (e.g., percentile-t) will tend to be favored for simple problems such as estimation of a mean, particularly when computational resources are limited, and often after appropriate transformations to stablize variance or to put the parameter space into a more useful form. Bootstrap iteration and coverage correction (e.g., the double bootstrap) may find favor as a robust, utilitarian tool, suitable for complex problems provided adequate computational resources are available. See my reply to *Beran*'s comments. The non-pivotal methods which are presently most favored by practitioners, will be largely confined to exploratory studies, highly complex problems, and certain parametric problems. I wonder how kindly time will judge these predictions!

I appreciate *Bai and Olshen*'s point that my results cannot be expected to go over automatically to random parameter models. I am fascinated by their comments following their equation (6), and look forward to seeing their forthcoming note with Bickel. Concerning their remarks about regularity conditions in their second paragraph, I must admit that things like moment assumptions did not weigh heavily on my mind while preparing my paper. I feel sure that a