DISCUSSION PAPER RESPONSE

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THIS PAPER WAS SUBMITTED for discussion in the expectation that the discussion format would allow maximum coverage of the many issues associated with the evaluation of temporal growth patterns over extensive geographic areas. To this end, the input from the discussants and numerous other reviewers has been extremely valuable, and we are sincerely grateful for their many thoughtful comments. We are also indebted to *Forest Science* for providing a forum for this controversial topic. One especially useful peripheral benefit from this exercise is the valuable insight gained regarding appropriate study designs for future investigations into forest health.

As far as past events are concerned, we are somewhat bound to using data already on hand if we are to advance beyond ignoring allegations of potentially significant changes in forest condition. Although the data used in our study were originally collected for inventory purposes, we contend they are still capable of providing valuable insight into possible causal factors behind the extensive changes in growth observed in several independent data sets. As with any information originally gathered for a different purpose, there are some imperfections and limitations in the data, but the large sample size and magnitude of the reductions make it unlikely our results have been compromised. Numerous sensitivity analyses conducted with the models and data consistently produced statistically significant reductions in the reported range. In addition, we feel that all the necessary caveats have been sufficiently stated and that our conclusions are hedged appropriately.

We reaffirm our judgement that differences involving the structural variables employed in our models are probably not responsible for the changing growth rates. Still, we acknowledge that observational data rarely define cause-effect relationships satisfactorily, so it is virtually impossible to settle the debate conclusively. Our 5-year sojourn with this study has convinced us that further deliberations on these data are not merited. Subsequent analyses would most profitably be directed toward quantifying and understanding the effects on growth of such candidate factors as old-field conditions, tree taper, hardwood competition, and climate. The remaining paragraphs expand on a few of the points raised in the discussions.

Reductions in the neighborhood of 20% to 30% may be "attention-getters," but it is illogical to gauge their credibility by comparison with Douglas-fir. We would expect loblolly pine to respond more dramatically than Douglas-fir to most changes in growing conditions because the growth potential of loblolly is so much higher. Long-term basal area growth responses exceeding 50% have been documented for loblolly stands simply upon reduction of hardwood competition (Haywood and Burton 1989).

It is not known if the geographic scope and magnitude of these reductions are unprecedented. Data capable of establishing such changes have only recently become available. We reiterate that too little is known about how growth varies under "normal" conditions to establish whether the growth differences observed in the FIA data are part of a declining trend, a long-term cycle, or a short-term aberration. While it is possible to compare the FIA data with models generated from growth and yield data sets, there are no grounds to hold any one data set or model as the accepted standard. The 1961–72 basal area growth curve for natural loblolly may be high relative to Schumacher and Coile (1960), but so is the growth curve produced by Sullivan and Clutter (1972).

High growth rates exhibited by a subset of plots from the 1961–72 period might qualify them as "atypical" when compared to current growth rates for natural stands, but they are certainly not unrealistic. The annual growth rate for 5-year-old stands averaged 6.4 ft^2/ac between 1961–72, compared to 3.5 ft^2/ac between 1972–82. Many of the stands that were 5 years old in 1961 have since been harvested, but it was possible to field-check a few of the plots that exhibited some of the fastest growth rates during that time period. All field measurements and growth computations from these samples were verified by an independent team of investigators. In any case, we have no argument with the notion that growth rates between 1961 and 1972 might be unusually high. As stated in the paper, this viewpoint is equally valid as the conclusion that growth rates during the later cycle are unusually low.

Finally, the fact that basal area growth is a completely reliable index of cubic volume only when there have been no changes in height growth or tree taper merits further discussion. Weidemann (1950–51) found that heavy thinning caused such a large concentration of growth in the lower portions of tree stems that measurements taken at breast height could lead to overestimates of volume growth by as much as 20%. To the extent that differences in initial stand densities are analagous to the effects of thinning, it is plausible that tree taper may have changed between the two cycles in question. If so, the basal area growth reductions depicted in this study do not necessarily translate into significant reductions in terms of volume growth.

LITERATURE CITED

HAYWOOD, J.D., and J.D. BURTON. 1989. Loblolly pine plantation development is influenced by site preparation and soils in the West Gulf Coastal Plain. South. J. Appl. For. 13:17–21.

SCHUMACHER, F.X., and T.S. COILE. 1960. Growth and yield of natural stands of the southern pines. T.S. Coile Inc., Durham, NC. 115 p.

SULLIVAN, A.D., and J.L. CLUTTER. 1972. A simultaneous growth and yield model for loblolly pine. For. Sci. 18:76–86.

WIEDEMANN, E. 1950–51. Ertragskundliche und waldbauliche Grundlagen der Fortswirtschaft. J.D. Sauerlander, Frankfurt-a.-M.