

Access and Outcomes of Obstetric Care

To The Editor: The article by Larimore and Davis¹ is an important attempt to link access to maternity services (as measured by physician availability) to an important health outcome—infant mortality. There are several methodological and statistical problems in the study, however, that undermine the validity of the results and conclusions.

The fundamental problem of their study is that the unique biases that can affect ecologic studies such as theirs are not even addressed. First, in contrast to control of confounding in individual-level studies, attempts to control confounding in ecologic studies rarely eliminate confounding.^{2,3} The inclusion of such variables as percentage of nonwhite study population, education, and income into the linear regression model does not mean that the association between INDEX (the indicator of physician availability) and infant mortality is not confounded by these variables. Second, even small errors in the measurement of covariates can result in a profound bias in an ecologic analysis, and this bias can produce effects vastly different from the effects introduced in individual-level studies.⁴ Income, for instance, is probably measured with some degree of error, and this measurement error might have a substantial effect on the regression coefficient for INDEX. (It is impossible to determine the magnitude and direction of this bias without analyzing individual-level data.)

Further, the statistical considerations relevant to ecologic analyses are ignored. Correlation coefficients and, therefore R^2 , are spuriously inflated in ecologic analyses relative to individual-level studies.⁵ The magnitude of this difference can be profound. Morganstern,⁵ for instance, offers an example in which data that were analyzed at the individual level resulted in an R^2 of 0.01, but when they were analyzed ecologically, the R^2 was 1.00.⁵ Despite the hazard of using correlation coefficients (and R^2), the authors use the R^2 for their linear regression model as their primary outcome measure. The reported R^2 of 0.176 is almost certainly spuriously high. If the authors had used the regression coefficient for INDEX as their primary outcome measure (because regression coefficients are not falsely elevated in ecologic analyses), their conclusions would have been vastly different. INDEX, for instance, shows the weakest association with infant mortality rate of any variable studied, approximately 500 times weaker than the association between percentage of the nonwhite study population and infant mortality.

These criticisms are not intended as a broadside against ecologic studies in general. While ecologic studies can provide valid estimates of individual-level effects under certain very limited conditions, the effect of unique biases must be evaluated and appropriate statistical methods must be used before any conclusions can be drawn. It is difficult to draw any valid conclusions from Larimore and Davis's study because they did neither. They must be congratulated for attempt-

ing to answer a difficult and important question, but the answer must await another day.

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References

1. Larimore WL, Davis A. Relation of infant mortality to the availability of maternity care in rural Florida. *J Am Board Fam Pract* 1995; 8:392-9.
2. Greenland S, Morganstern H. Ecological bias, confounding, and effect modification. *Int J Epidemiol* 1989; 18:269-74.
3. Greenland S, Robins J. Invited commentary: ecologic studies—biases, misconceptions, and counterexamples. *Am J Epidemiol* 1994; 139:747-60.
4. Brenner H, Greenland S, Savitz DA. The effects of non-differential confounder misclassification in ecologic studies. *Epidemiology* 1992; 3:456-9.
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The above letter was referred to the authors of the article in question, who offer the following reply.

To the Editor: In our article we made no inferences concerning risks to individuals based upon aggregate data; therefore, Dr. Sonis's contention that our study was ecologic and had "unique biases which can affect ecological studies" (the "Ecology Fallacy") is not, in our opinion, valid—as our basic unit of observation was the county. We had predicted that infant mortality in a county would be significantly affected by our index of physician providers of maternity care (INDEX). The statistical analysis we used revealed a necessary, but not sufficient, condition to establish cause and effect, a fact that we discussed in the paper. Nevertheless, the statistical analysis did, in our opinion, reveal a measure of truth for those with eyes to see and ears to hear.

As explained in the paper, we attempted to adjust for confounding by including only the measures of socioeconomic variables that were available to us for every county in the state. Although we feel the measures were accurate, our paper clearly stated that there could be many other covariates that were not available for our study. Of most importance, however, is that INDEX is the only real covariate the family physicians in Florida can control.

Additionally, we would point out that the regression coefficients of Table 1 were not standardized, so that each coefficient depended on the units of measurement of its corresponding independent variable; therefore, one should not use relative size of these coefficients to infer strength of association. The column for P values in Table 1 shows that the only significant variable, apart from total number of babies, in predicting total deaths was the INDEX.

Dr. Sonis suggests that we should have used "appropriate statistical methods." We hope our explanation will help him to see why we believe that the conclusions from our study are valid and that our answer to