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Empirical Analysis of the Size Distribution of Farms: Discussion

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As an outsider in the field of agricultural economics but one who has studied the size distribution of firms, I find that the most notable feature of the three papers in question is the qualitative similarity of farm size distributions to firm size distributions, particularly those of smaller firms (Evans, Hall). These similarities may be summarized as (*a*) relatively small deviations from Gibrat's law, with smaller farms growing faster than larger but not enough to have a significant effect on concentration in the periods examined. (*b*) A higher rate of exit and higher variance of growth rates among the smaller farms in the sample. (*c*) Considerable residual randomness of growth rates after controlling for exogenous or predetermined factors [such as investment in the case of firms or the human capital of the farmer in the case of farms (Sumner and Leiby)]. (*d*) Evidence of a small negative serial correlation in growth rates, which suggests either measurement error in the size variable or some kind of lagged adjustment process (Shapiro, Bollman, and Ehrensaft).

Because of lack of space, I will not discuss the econometric problems in estimating the size–growth relationship in detail here but will refer the reader to my paper for a fuller discussion of them (Hall). They can be summarized under three headings. (*a*) Selection or sample attrition: estimates based on a sample of farms which exist in two different periods are typically biased, owing to the omission of those farms which exit during the period. The sign and magnitude of the bias depend on the reasons for exit. (*b*) Serially uncorrelated measurement error in the size variable used can induce spurious negative serial correlation in the measured growth rates as well biasing the coefficient of size downward in a growth–size regression. (*c*) Size-related heteroscedasticity in growth rates is likely to result in inconsistent estimates of the standard errors of a growth regression, usually biasing them downward from the true estimates.

All of these problems are present to one degree or another in these papers, although only the first (sample attrition) is likely to bias the estimates seriously. The third can be easily dealt with by computing robust estimates of the standard errors (White).

Although all the papers in this session are concerned to some extent with the farm size and growth relationship, they each focus on a different aspect of the relationship. I will discuss them in a different order from that in which they were presented because they can naturally be viewed as increasingly more sophisticated representations of the size–growth relationship.

Shapiro, Bollman, and Ehrensaft are concerned primarily with testing a very simple hypothesis, the hypothesis that the growth rates of farms are independent of their beginning period size (Gibrat's law), using a very large data set on Canadian farms. At the outset, it should be noted that data sets of this size will almost always statistically reject point hypotheses of any kind, such as the one in question, owing to the fact that no model is a perfect description of nonexperimental real-world data. Using a fixed significance level when the sample size becomes large means that the power of the test against alternatives rises to unity, so that any deviation in the data, no matter how small, will reject the null hypothesis. A good treatment of this phenomenon is found in Leamer's 1978 book. Using his methods in this case would imply a critical value for the *F*-statistic in table 1 of around 10, so the data still reject Gibrat's law.

However, there are more compelling reasons not to accept immediately the authors' claim that Gibrat's law has been rejected: the evidence presented in table 2 indicates substantial size-related heteroscedasticity in the growth rates, thus invalidating the standard errors used to compute the *F*-test. In spite of the claims of the authors to the contrary, it is very likely that the standard errors computed under the homoscedastic assumption are underestimates of the true standard errors in this case. In addition, the selection issue alluded to above will also have an impact on the estimates of β which are presented here.

In spite of these caveats, I believe that correcting for selection and heteroscedasticity in these estimates will leave the fundamental result unchanged: a tendency for smaller farms to grow somewhat faster than larger farms, and a negative serial correlation in growth rates. The interesting result in the paper, which is that concentration has tended to increase in the 1966 to 1981 period in spite of the regression to the mean exhibited by farms, is unlikely to be overturned.

The other two papers try to provide some explanation of why growth rates might differ across firms. Although the first part of the Garcia, Offutt, and Sonka paper is devoted to a comparison of the predictions of Markov transition matrices using different size measures for farms, the second half attempts to find some explanation for the changes in size using agronomic characteristics of the farms. The authors chose to aggregate the observations on individual farms into size classes before doing the analysis in order to facilitate comparison with macrolevel size class distributions, but I would strongly recommend that they pursue at least the analysis in the second half of the paper at the microlevel, both to facilitate comparisons with other such studies and to avoid throwing away any of the information in the variables by grouping. Comparison to macroresults can be done by forming the predicted size class distributions after the analysis is performed.

Sumner and Leiby are primarily concerned with the effect of management's characteristics (human capital, etc.) on the growth of farms. In this respect their paper is a step beyond most of the existing firm growth literature, although the connection between the manager's characteristics and growth is suggested theoretically by both Lucas (1967) and Jovanovic (1982).

In the paper, they sketch out a model for the growth of dairy herd size which is based on a profit function derived from decreasing returns production and increasing marginal costs; this immediately implies that herd growth is positively serially correlated and negatively related to size, even in the absence of efficiency differences across the farmers, so testing Gibrat's law is not an issue here. I would have liked a tighter link between the model and the regressions which were actually estimated in table 3. For example, one might treat the marginal cost of cows, *L*, as a random coefficient which is a function of the human capital of the farm owner.

In closing, I would like to congratulate the authors on their very interesting forays into the size distribution literature and to thank them for introducing me to an interesting area of research in agricultural economics.

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