Gordon Marker's paper on French migration in the interval between the Franco-Prussian War and the Great War is bound to occasion some thoughts about the relevance of his work to the new economic history. I propose in this brief comment to contrast Marker's work with the new economic history, to show that his paper is largely a-historical, and on the basis of this critique to make some suggestions for further investigations into French migratory history.

Ι

There seem to me to be three principal features of the new economic history of interest to us here: (1) It is history, i.e., there is a clear and demonstrable interest in historical data and information per se; not just for that information's relevance to contemporary policy problems or as a source of empirical support for some theory or other. (2) It uses statistical and econometric techniques in the analysis of historical data, not as ends in themselves but only as an adjunct or aid to the central task of understanding historical phenomena. (3) So far the new economic history has been largely confined to studies of American economic history, and that principally in the nineteenth century. Based largely on the work of an earlier generation of quantitatively-oriented economists (many of them connected with the National Bureau of Economic Research), the present stage of U. S. studies finds one with the possibility of aggregating the primary data into macro-economic categories which have become the touchstone of economic analysis since Keynes. In no other country (Gregory King notwithstanding) has so much preliminary work been done prior to the 1950's and 60's to permit the kinds of advances made of late in U. S. economic history.

Marker's paper, in contrast to the best work of the new economic historians, is not history. His primary interest is in using French migration data to support the hypothesis that "area differences in net migration within an intercensal period reflect to a large measure differences in economic opportunity. . ." And, I might add, he is not above insinuating a closing remark that "France provides an excellent laboratory for historical-statistical investigation of the development process," which is a highly elastic substitute for: "My work, though apparently related to the dark past, is really relevant to the problems of the developing nations. . ." In this, of course, Marker is no worse than many of our contemporaries: Conrad and Meyer's classic study of

the ante-bellum slave economy mentions 'the near slavery existing today' and 'key policy questions in former colonial countries' as justification for undertaking their study.^{*} Perhaps the improving status of economic history as a fit subject for study by economists will in the end justify abandonment of these unnecessary rationalizations of historical research. The historical sterility of Marker's investigation here reported resulted directly from the incidence of some feeling that historical study of French migratory movements is not in itself interesting. In this respect his work is less effective than that issuing forth from the new economic history of the United States.

If this study is not history, what is it? --Hypothesis testing pure and simple, for which the 'real data' are of only secondary importance. In operational form the hypothesis comes down to m = $F(Y_L/L, \Delta L)$, and the outcome is that ΔL is somewhat more important as an explainor of net migration than Y_L/L , though both are significant correlates of net migration. Is anyone surprised? Does anyone know very much more about French migration, given that many of us would have accepted the proposition as axiomatic?** The level of sophistication reached in recent vears in comtemporary studies of migratory movements is probably not attainable for historical studies simply because of the limitations of data. The economic historian must perforce make his contribution in the understanding of history rather than the development of methodology. But my complaint as I read Marker's paper is not that the investigation was carried out, for surely one man's axiom is another man's hypothesis; rather it is that so much of the quality of French migration had to be drained out of the data in order to move us inexorably to the conclusion. Without asking for twinkly-eyed Parisians, stolid French peasants, or the rural-urban sins of a migratory Madame Bovary, I still would hold that something of France must be a part of French migration. Let me offer some suggestions which might breathe some of France into this discussion.

II

The observation of city size distributions within political units reaches back at least to the

* Alfred Conrad and John Meyer, <u>The Econom-</u> ics of Slavery (Chicago, 1964), p. 47.

** The paper by Mary Jean Bowman and Robert G. Myers, "Schooling, Experience, and Gains and Losses in Human Capital Through Migration," this volume, pp. 0-000, carries forward its argument with these assumptions. neoclassical economists Marshall and Pareto, the latter having developed from his Italian experience the so-called rank-size rule. Others, including the geographer Mark Jefferson, were intrigued by the development in a number of countries (England and France included) of the primate city, one which developed such scale economies that all secondary cities were much less developed than the Paretian rank-size rule would suggest. Paris is of course a prime example of primacy being several times larger than Marseilles, France's second city. Given this particular city size distribution today, one would expect some important discontinuities between Paris and all other urban places so that even the ordinal ranking employed by Marker would fail to distinguish important differences in the causes of migration to Paris itself and to all other urban places. We would like to know. moreover, whether during the period under study the agglomerative powers of the Parisian area waxed or waned: Given that some large share of total urban growth due to migration can be directly attributed to the growth of Paris (I hazard the guess of one-third), one might gain in the analysis by giving that city separate treatment. In any case it seems hard to justify the rank correlation techniques when the absolute population growth of the various departements vary so greatly. I am not satisfied that use of migratory rates per 1000 solves the problem of essential discontinuities inherent in the primate city size distribution of France.

In my own investigations of internal migration in Colombia, South America's third largest nation but with a populated area roughly the size of France, I have become intrigued with patterns of migratory movement. Perhaps surprisingly, about three-quarters of the movement to the largest cities is from smaller towns rather than from strictly rural areas. Though a pattern of step migration may seem to be the dominant theme of physical mobility, the available data are better explained by an hypothesis of fill-in migration: As small-town residents leave for the big city, their places are filled in by short-distance rural-urban migrants. I believe these two movements respond to essentially different economic variables -- the local movement to land pressure in the rural hinterland of each small town, the inter-urban movement to differential opportunities in the fraccionated, heterogeneous urban labor markets. For that reason one might suggest separate hypotheses for the two kinds of movement. In particular, a land/labor ratio in rural areas should predict rates of rural outmigration. Inter-urban movements might respond to the variables Marker lists, but I would suggest that absolute urban size may be (during certain periods of rapid urban growth) a good predictor of migration rates -- this view emanating from a theory of scale economies associated with urban size.

Finally, some questions suggested by this study:

(1) Marker notes that per capita income rankings of départements are remarkably similar at the two dates for which information is available, 1864 and 1954. How did the variance of this per capita income distribution behave over the ninety-year interval? Did interdepartmental income inequality continue in spite of (or because of) the significant internal migration which Marker has described? If workers are truly responsive to economic opportunity in their migratory plans, one would expect reductions in inequality. This subject deserves more exploration than Marker was able to devote to it in this brief paper.

(2) Did factor markets become more efficient in allocating the supply of labor? Marker's evidence which shows an increasing Tau for all migrant subsets over the interval 1872-1914 suggests that the answer is yes. His data does not, however, permit consideration of the hypothesis advanced by W. H. Nicholls* that nearness to an efficient urban labor market will make the rural labor market more efficient. During the period under study did rural-urban wage differentials vary with the absolute size of urban places or with local migration rates? These difficult problems are compounded by the lack of usable bases for comparing rural and urban incomes and levels of living. It is perhaps too much to ask Marker to solve this problem.

(3) Was land tenure an unimportant element in the determination of rates of out-migration? Certainly there are significant regional differences in tenure arrangements in France which might be correlated with migratory flows. In approaching answers to this question which would require intimate knowledge of agrarian conditions in France, the author may well arrive at a 'real' new economic history in a French setting. This paper does take us several steps in the direction of combining historical research and hypothesis testing.

^{* &}quot;Industrialization, Factor Markets and Agricultural Development," Journal of Political Economy, Vol. 69, No. 4 (August 1961), pp. 319-340.