

PDWAH384

A Critique of
"The Influence of Rural-Urban Migration on the
Fertility of Migrants in Developing Countries:
Analysis of Korean Data"

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BACKGROUND

The Urban Migrant Fertility study was conducted under the auspices of the Office of Urban Development in A.I.D.'s Bureau of Development Support, DS/UD. A Task of this office is to increase Agency understanding of the dynamics and consequences of rapid urbanization and their impact on development. DS/UD activities concern regional development; employment and productivity; urban finance and management; as well as urbanization in national development. A particular interest of DS/UD is in the area of population movement, with special reference to urban-rural migration and fertility patterns.

The study under review followed up an inquiry by Sally Findley and Ann Orr "Patterns of Urban-Rural Fertility Differentials in Developing Countries." The purpose of that work was to analyze urban and rural fertility patterns in order to identify how "place" and "person" factors influence fertility behavior. The authors wished to discover how urban and rural development projects affect fertility through their impact on place and person variables. The Findley and Orr study suggested, inter alia, the following conclusions. First, urban fertility is lower than rural fertility by some 25% for 32 developing countries circa 1970. Second, the greatest gaps between urban and rural fertility are observed among women of ages 15-19, where the differences average about 33%. Third, variations in place characteristics may account for differences in relationships between socio-economic personal variables and fertility. Fourth, although place characteristics are important in understanding urban-rural fertility differences, it is also useful to consider variations among individuals, e.g., a woman's personal attitudes and other attributes that influence her childbearing behavior.

The current investigation used the Findley-Orr study as a point of departure, with a special focus on the role of person and place characteristics as they affected fertility behavior in Korea. The main question addressed was as follows: Why do migrants have lower fertility than rural stayers? Is it because of "selection" or because of "adaptation"? Selection concerns the process where individuals predisposed toward lower fertility are self-selected from the rural population and are disproportionately represented among migrants. Adaptation, on the other hand, involves the process where migrants acquire an urban lifestyle that includes lower fertility norms than rural expectations. The selection and adaptation hypotheses are compatible, i.e., not mutually exclusive.

The original proposal was to carry out the following tasks: (1) develop a theoretical model based on hypotheses drawn from the relevant literature; (2) test implications of the model by fitting it to data from the 1974 World Fertility Survey for Korea; (3) adapt the model for use in different countries, such as Bangladesh, Ghana, and Peru; and (4) synthesize the cross country applications for use by development planners.

Funds were provided, however, only to develop the model based upon the literature and to test implications of the model by fitting it to data from the WFS for Korea. Adapting the model to different countries was to be done in a second phase. Inferring

the policy relevance for use by development planners, though, could be started in the first phase and completed during a second phase.

The Research Advisory Committee of A.I.D. called for a peer review team to be brought in to evaluate the first phase of the study. Secondly, the Committee suggested that the policy relevance of the study should be explicated at the end of the first phase in conjunction with the peer review team.

The review is in two parts. There is an internal review team consisting of T. Paul Schultz of Yale, Gary Hendershot of Maryland, Sally Findley of Minnesota, and Oleh Wolowyna of North Carolina.

The bottom line of the internal review is that the overall quality is quite high. Here are some excerpts from the internal reviews to provide a flavor for the critiques. One reviewer, for example, writes that the introduction is admirably clear. The logical presentation of ideas and the rationalization for the research is appropriate for the audience of A.I.D., and the more general, professional community. The treatment of preferences and the control thereof in the discussion may not be entirely appreciated by the general reader, but that cannot be avoided. The reviewer's inclination is to examine all migrants, not only those who migrated after marriage. The majority of the comparisons are for the post-marital rural/urban migrants, and for this group the effect of marriage is partialled out in a way that must bias the comparisons and limit their value for the inferences policymakers wish to draw. He urged that the full regressions be reported for all migrants with the socio-economic controls. Among the controls he thinks it is unwise to include, however, is the variable "women's earnings."

Based upon this critique, the investigators report full regressions for all migrants with the socio-economic controls. Some regressions are run without female earnings as a control, since this variable is a manifestation of market labor force participation and is related to labor market opportunities.

A second internal reviewer writes that:

This is an excellent report. It explicates the theoretical issues and then marshalls appropriate data to test key hypotheses. The results are clearly presented, and care is taken to address both the social scientist professional and the public policymaker.

Like any piece of social science research, of course, it is not perfect. The authors have done well in noting the shortcomings of their data and techniques, and have tried diligently to work within them. At points, however, they may not have given the reader sufficient notice of possible shortcomings, or may have gone a little farther than I think the data would take them.

As a result of the second reviewer's comments, there is a noticeable improvement in the statement of limitations of technique although not enough attention to data limitations.

A third reviewer states that:

The research is a substantial contribution to our knowledge of the interactions between migration and fertility. First, you have made a strong case that the most appropriate comparison group is the rural stayer or non-migrant. Second, your autoregressive model builds in controls for the most important childbearing variables, age, marital duration, and parity level at time of migration. Third, you show that even if there is selectivity in terms of education, and your evidence shows that there is, selectivity contributes little to the total differential due to a nonlinear relation between educational attainment and fertility. Fourth, even if selectivity does contribute to the observed differentials, these differences are considerably amplified by adaptation. Finally, ... adaptation varies with destination size. These findings have definite policy implications that eluded previous studies which suffered from poor designs which have not been able to yield the consistent results necessary to guide policy decisions.

After reading the entire document, I have the feeling that you have an unspoken bias, namely that selectivity does not operate. This is precisely the bias that ... critics will look for. If you could tone down the initial (pre-testing) statements against selectivity the report ... will be less subject to the criticism of bias.

The third internal reviewer raises a critical point about the unspoken bias in favor of adaptation at the expense of selectivity. When one begins with an idea, it is funded by an office whose representatives support the idea, and then reports evidence substantiating the idea, eyebrows are liable to be raised. My own feeling is that the researchers leaned toward adaptation over selectivity in the first place. One must understand, however, that the investigation does not demonstrate that adaptation is more important than selectivity. What it does suggest is that adaptation is important even if selectivity is taken into account. There is no definite answer to the issue of the relative effects of adaptation versus selection.

The investigation is a great improvement over the literature. Prior studies tended to accept the selectivity hypothesis without even measuring adaptation. Even if the present inquiry errs somewhat in the opposite direction, it nevertheless is a useful corrective to the literature.

In short, the internal reviewers provided excellent feedback to an on-going inquiry. The reviews acknowledge the high quality of the research and suggest ways for enhancing the effort. The investigators used the constructive feedback for the most part.

The external review used the internal critiques as a point of departure. As the Chairman of the 1979 Research Advisory Committee subcommittee that handled the original proposal, I was picked to chair the external panel. I selected a sociologist/demographer (Goldstein), and economist/demographer (Mueller), and an econometrician (Edlefsen). Given the fact that I am a political scientist, the panel has breadth and depth to do a critique of the migration/fertility study.

Instead of trying to summarize the separate critiques, I will let my colleagues make their own assessments. Prior to that, however, let me make some general observations about our review. First, I did not request that each panelist extol the virtues of the project. Instead, I asked them to identify possible weakness so that the end product could be as high quality as possible. As a result, it may seem as if my overall presentation is more positive than the individual panelists. Let there be no mistake: there is general consensus that this is an impressive study. Second, there is general agreement as to the importance of the study. As stated above, the literature's stress on selectivity needed to be corrected with an emphasis on adaptation. Since adaptation is studied so infrequently, the study is unique. Since the study is unique it could be perceived as a bit quacky: it is outside the mainstream of the literature. The autoregressive model, moreover, is also unique: that is, using lagged fertility in an autoregressive model of migration is an outstanding methodological innovation. The lagged dependent variable tries to account for excluded variables that may be related to selectivity, which itself is indirectly indexed by social/economic variables. Third, there is consensus in the group as to the need for clearer definition of key concepts such as adaptation and selectivity.

Before turning to my colleagues, let me give some of my own general reactions to the study. The work may be evaluated from the point of view of the data, model, methodology, and policy relevance. The model and methodology may be stronger than the data and policy relevance. The data, however, may be the best available to carry out such an inquiry. With respect to policy the study sticks very close to its evidence, as it fails to make wild speculations. Even though the study goes into the policy relevance of the findings, one should not expect the policy relevance to be this study's main contribution. The skills necessary to carry out high quality econometric analysis may not be the same skills necessary to draw out the policy implications. What is needed is a set of contractors whose task is to operate at the intersection of science and policy. These contractors would work with social scientists and development planners; therefore, scientific quality would not be sacrificed for policy relevance as might be the case if social scientists spent all of their days worrying about how their research will help a bureaucrat. A high quality econometric study is generally going to be difficult for government people to follow, suggesting an additional reason for there to be a middleman between the scientists and the policymakers.

Let's now return to the main finding that adaptation is an important explanation of declines in fertility. One question arises. Why? Why does adaptation work out so well in this investigation in contrast to prior studies? There are better data than that which were available to other investigations. For example, year of migration and personal history data are now available.

The model used here is a better model than has been used before. It is a correctly specified model, as demonstrated by the significance of the coefficients and absence of serious multicollinearity. Economic theory is used in a creative fashion to specify the model. For example, based upon theoretical considerations, fertility rates are regressed on previous level of fertility, age, education, etc. Duration of marriage is important to include in a model of fertility. Prior studies such as the one by T. Paul Schultz on Colombia, did not include duration of marriage, while the present inquiry does. Thus, it is possible to compare fertility for women with different scores on a duration of marriage variable. The study compares the fertility of women married for different lengths of time, concluding that fertility varies according to duration; the longer the marriage, the more children a woman has. If migrants' marriage duration differed from that of non-migrants, then it would bias the effect of either adaptation or selectivity.

Even though there are some differences in the data and model of the Korean and Colombian (Schultz) studies, there may be more similarities. That is, the data and model are more similar than they are different. What is left is the difference in culture. There may be something about the Korean culture that is so different from Colombia that adaptation would loom larger in Korea. Many comparative analysts argue for a cultural difference type of explanation. If the results from the Korean study hold up in countries such as Mexico and Thailand, however, then culture may not be as important as analysts suspect. The Colombia study is not a good test of the cultural explanation vis-a-vis Korea, though, because there are no data on duration of marriage in the Colombian inquiry (migrants may marry earlier than rural stayers, thus the results might be biased). So a third reason why adaptation operates so strongly in the present study is because of sample differences, e.g., between Korea and Colombia, there is cultural variation. In short, better data, a more adequately specified model, and differences in samples may explain why the present inquiry concludes that adaptation is so important.

The final question posed by the results is so what? My first thought was that the external review panel could make a start in addressing the "so what" query. Upon reflection, I concluded that the issue of policy relevance is too complex to be tackled in a review. As stated above, relevance also is too hard to be left to scientists to delineate, a fact which argues for the middleman role between the academy and polity.

Let's be clear and note that there are many aspects of the present report that are relevant now. It is my hope that the afternoon discussion can begin to assess relevance.

For example, I recently read a message about urbanization and fertility from an economic officer in a U.S. Embassy. The officer said that the range of possible actions for the U.S. Government was limited. There is little that can be done to help solve the host government's urban unemployment problem, which is a serious threat to political stability. Because of religious constraints, there are limits to the distribution of birth control technology. In the medium term, however, the U.S. could help slow down rural to urban migration via PL 480 fertilizer strategy and a small family credit program.

There are a number of misconceptions and incorrect popular beliefs concerning the relationship between rural-urban migration and urban growth and poverty. Many empirical analyses invalidate most of these beliefs. Instead they show that:

1. Rural-urban migration is not the primary cause of urban population growth.
2. The socioeconomic characteristics of urban migrants are quite similar to urban natives; though urban migrants join the ranks of the urban poor, they also join the ranks of urban working and middle classes in almost equal proportion to native urban population.
3. In most cases urban migrants are a minority in urban slums and squatter settlements even though these areas may have a slightly higher percentage of migrants than the total urban area.
4. If rural-urban migration could somehow be halted, urban poverty would persist because most of the urban poor were born in urban areas.
5. Rural-urban migration should not be confused with rural out-migration or with urban in-migration; many, and in some cases most, urban in-migrants come from other urban areas while a large percentage of rural out-migrants move to other rural areas.

Note that the embassy official cited above appears to be unaware of a link between rural to urban migration on the one hand and fertility reduction on the other hand. When options are so limited, perhaps knowledge from such studies as the one under review will provide a point of departure for better analysis than is evidenced by the above officer's reasoning.