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## Comment on *Internal Migration in Developing Countries: A Survey*

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## Comment on *Internal Migration in Developing Countries: A Survey*

### Abstract

[Excerpt] It is a privilege to discuss Todaro's paper. The material is presented clearly, succinctly, and fairly. Todaro claims credit where he has earned it, gives credit where it is due, and withholds credit where it is not due. On the whole, the resulting document is an accurate survey, valuable both to practitioners in the field and to newcomers.

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### Comments

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Comment on “Internal Migration in Developing Countries: A Survey”,

by Michael P. Todaro

Gary S. Fields

Cornell University

*Population and Economic Change in Developing Countries* (1980), 390-394

Gary S. Fields is associate professor at Cornell University. At the time this comment was written, he was associated with Yale University.

All of us who study labor market and population problems in less developed countries and their interaction via migration are indebted to Michael Todaro for the intellectual guidance he has provided over the last decade. Before he arrived on the academic scene, it was widely thought that urban unemployment in poor countries could be alleviated or even eliminated if governments could only channel enough resources and incentives to create more urban jobs. Todaro showed the futility of this kind of strategy, pointing out that more urban jobs would accelerate rural-to-urban migration and result in more rather than less unemployment. Todaro’s call for development strategies emphasizing rural growth has been heeded and is now widely accepted. I cannot begin to estimate the impact of this shift on the economic well-being of the poor throughout the world.

I also owe Todaro a personal debt. When I first arrived in Kenya in 1970 to begin to study economic development, Todaro had just left. The halls of the Institute for Development Studies at the University of Nairobi (and, I am told, the inner channels of the Kenyan government as well) were alive with the excitement his ideas had generated. His influence on academicians and policy-makers was evident. As a young graduate student, Todaro’s influence gave me hope that some day I too might be able to contribute to the economic betterment of the poor around me, whose plight I was just then beginning to grasp.

It is a privilege to discuss Todaro’s paper. The material is presented clearly, succinctly, and fairly. Todaro claims credit where he has earned it, gives credit where it is due, and withholds credit where it is not due. On the whole, the resulting document is an accurate survey, valuable both to practitioners in the field and to newcomers.

The survey is organized into three sections: toward a testable theory; review of empirical findings; and major unresolved questions. My remarks are organized accordingly.

## Toward a Migration Theory

Section 6.2 contains a valuable summary of the Todaro model in its several variants and a number of subsequent modifications. Particularly apt is the summary of the four essential features of this class of models and the assessment of the current state of thinking. The reader is thereby introduced to the main ideas in their original and current developments. On the whole, I am in agreement with the points made in this section.

The expected-income hypothesis (alternative versions of which are set forth in Todaro (1969) and Harris and Todaro (1970) admits of two interpretations. Some, apparently including Todaro himself, see it as a *literal* representation of the functioning of labor markets in LDCs, or at least as a tolerably close first approximation. Others prefer to regard the expected-income hypothesis as the *central characteristic* of a suitably embellished model. I myself prefer the latter interpretation.

In a recent paper (Fields 1975) to which Todaro makes several references in his survey, I addressed the apparent discrepancies between some of the Todaro model's assumptions and predictions, on the one hand, and real-world complexities and data, on the other. Specifically, I dealt with the gap between LDC unemployment rates and the rates predicted by a literal interpretation of the Harris-Todaro model (Harris and Todaro 1970). I hoped to show that the disparity could be reconciled within the Harris-Todaro framework, appropriately augmented. As I concluded from my analysis (Fields 1975, p. 184): “These extensions permit us to retain the quite plausible notion, as set forth by Harris and Todaro, that the voluntary movement of workers between geographical areas is the primary equilibrating force in the labor markets of LDCs, while at the same time having a theory which is not contradicted by the facts.” Thus, I

interpreted the Harris-Todaro model as being incomplete, not incorrect. For empirical research, it is crucial to work with as complete a model as is practicable. I will say more on this below.

Let me mention some other, less fundamental points where I differ with Todaro:

1. I am puzzled by Todaro’s claim in note 6 that he took into account the urban “traditional” sector in his 1969 paper. The footnote to which he refers (1969, p. 139, n. 3) shows his awareness of the existence of this sector, but this awareness is not carried over into the formal analytics. I suppose we have a semantic disagreement over what it means to take something into account.

2. In his 1969 paper and again in this paper, Todaro claims that the rising probability of employment is consistent with longer-term migrants having better contacts and information. I think he is a bit misleading on this point. In his model the rising probability of employment is the result of a cumulative job search process with infinite job fixity in which the transition probability  $\pi$  is constant. The rising probability of being employed refers to the *state* probability. It does *not*, as Todaro suggests, result from the *transition* probability increasing with length of time in the city due to better contacts (i.e., rising  $\pi^i(t)$  at the time  $t$  for migrant cohort  $i$ )

### Review of Quantitative Studies

Section 6.3 reviews the major empirical findings from the migration literature. He brings together the results of a large number of studies, including several that are as yet unpublished. I found his distinction between “nonrigorous descriptive” studies as opposed to “econometric studies” a bit artificial and the choice of terminology rather unfortunate, but his conclusions generally are clearly reasoned and are well documented where possible. Where studies of only a

few countries support a given conclusion, this lack of solid support is duly noted. As a survey, then, this section is a fine capsule summary of the existing literature.

In this section, I wish Todaro had adopted a more critical stance in evaluating the various studies. Strong and weak studies are given equal weight. The studies differ greatly, however, in conceptual clarity, data suitability, statistical method, and sophistication in interpreting the findings. In some respects, therefore, the evidence is weaker than Todaro implies.

I would also like to have seen a prescriptive statement of how to go about conducting empirical migration research relevant to the Todaro model. As my own past research on migration in the United States has shown (Fields 1976), even if one takes the view that economic factors are primarily responsible for migration behavior, *which* economic variables are included and *how* they are specified makes a great difference in the explanatory power of the economic model. My colleague T. Paul Schultz has recently undertaken an extensive formal analysis of this question (Schultz 1976). I raise this point because I find it very difficult to ascertain what Todaro would regard as an appropriate “test” of “the Todaro model.” Does statistical significance of an unemployment rate variable in a migration function constitute sufficient supporting evidence? Or does verification of the theory demand more, such as observing the same elasticity of migration with respect to employment probability as with respect to the wage rate? I looked in vain through Todaro’s survey and his other writings for guidance on just how literally to take the model, on which propositions are critical and which result from a specific mathematical formulation, and on what evidence would be conclusive in supporting or refuting these propositions. I await a statement from Todaro clarifying his position on these issues.

Another concern I have is with Todaro’s skeptical conclusion about the consequences of rural-urban migration. Todaro states his own position clearly: “migration substantially in excess

of new job opportunities is both a symptom of and a factor contributing to Third World underdevelopment.” Symptom, yes. But contributing factor? I am unconvinced and truly agnostic. These are contentious issues. As Todaro makes clear in section 6.3.1, the evidence is speculative and inconclusive. I would have liked to have seen more discussion of the positions on both sides.

One other point bothers me. In section 6.3.2 Todaro describes a procedure he has devised in his most recent work (1976*b*) for estimating the elasticities of migration with respect to urban employment opportunities and rural-urban wage differentials. Todaro contends that the suggested procedure, although not ideal, is useful “as a first approximation” to the magnitudes in question. Frankly, I doubt the validity of this approximation. We should note that the formulas upon which the estimated elasticities are based are derived from the literal Todaro model of 1969. Consequently, these estimates make no allowance for labor force heterogeneity, job search by currently employed persons, or the other real-world complications recognized earlier in the survey. Thus, Todaro does what he cautioned us against earlier: he takes his own theoretical model literally in empirical work. Indeed, the Harris-Todaro model, taken literally, gives an equilibrium urban unemployment rate of one minus the rural-urban wage ratio, or roughly  $2/3$ , well outside the range of tolerance as a “first approximation,” which is why we need an enriched model in the Harris-Todaro tradition. Might not Todaro’s recent calculations of the employment and wage elasticities be in error by a similar amount? This is more than an academic point, since there are evident dangers in basing policy on parameter estimates as imprecise as these seem to be.

## Priorities for Migration Research

The list of research issues posed by Todaro in section 6.4 contains the major unanswered questions, including those for which tentative answers are available for only a small number of countries. If we had answers to all these questions, we would have a much better understanding of the migration process and a much better sense of what policy direction to move in.

Of the topics mentioned on Todaro’s list, in my judgment two merit highest priority. One is the empirical validation of the expected-income model in predicting migration patterns. The conceptual propositions set forth by Todaro (1969) and Harris and Todaro (1970) some seven or eight years ago have not received sufficient empirical documentation in many areas. More diligent empirical research lies ahead.

The other high-priority research need is for studies of the consequences of migration for the migrants themselves and for the economies of the sending and receiving areas. I welcome the concern voiced by Todaro and others for the income distributional effects of migration as well as the overall efficiency effects. In bringing these concerns to bear, though, let me voice a general caveat. It is quite possible that overall income distribution may improve with migration even though both the urban and rural distributions appear to worsen; this would follow if, as seems to be the case, migrants were in relatively favorable positions in the rural areas before their move but enter the urban labor market at a relative disadvantage, at least in the short run. The caveat, then, is that income distribution concerns must reflect changes in the entire economy, using tools of analysis that are sensitive to changing numbers of persons in the urban and rural sectors of a dual economy. The usual measures of income distribution within the urban and rural sectors taken separately may not suffice.

In sum, Michael Todaro has prepared a valuable synthesis of the conceptual framework and empirical research on migration in less developed countries. The paper stands as an authoritative statement of where the migration field is and where it is going. The careful reader will observe not only how much is known about this important facet of economic development but also how little. As yet unanswered are key questions such as the role of migration in promoting or impeding economic growth and alleviating poverty and the extent of responsiveness of LDC workers to differential employment and earnings opportunities in present and alternative locations. Much remains to be done.