

REVIEW SYMPOSIUM/ TOUR DE TABLE SUR UN LIVRE

Intermetropolitan Migration in Canada: Changing Determinants Over Three Decades/Migrations intermétropolitaines au Canada. Évolution des causes au cours de trois décennies. R. Paul Shaw. Toronto: New Canada Press, 1985; (Eng.) 232 pages, \$24.95 cloth, \$16.95 paper; (Fr.) 240 pages, 24,95 \$ (relié), 16,95 \$ (broché).

INTRODUCTION/PRÉSENTATION

A milestone is defined literally as a stone or pillar set up along a road to show the distance travelled. In the study of Canadian population migration many miles have been traversed and, along the way, many milestones have been established. When Paul Shaw's book appeared in early 1986 it was apparent that it was a candidate for inclusion among works of significance in the field of population migration.

Hoping to take advantage of the opportunity for debate that the book presented, I asked several researchers to contribute to a discussion of it. In my request to them I indicated only that I was asking for "a brief commentary on, or reaction to the book, from people involved actively in the study of population migration." The responses to this request, along with the author's comments in reply, are presented below. Together they represent an interesting and divergent collection of the points of view of six of the leading researchers in the area.

Population migration is clearly an inexorable component of the analysis of urban and regional development and disparity. It is hoped that this review symposium represents a contribution to the discussion of these important issues.

La trajectoire déjà longue des études sur les migration des populations au Canada est jalonnée de travaux importants qui en ont balisé les étapes et marqué les progrès. D'emblée, le livre de Paul Shaw, publié en 1986, semble appartenir à cette famille d'ouvrages marquants.

Y voyant, à tout le moins, l'occasion d'un débat, j'ai eu l'idée de le soumettre à quelques chercheurs, en leur demandant simplement « un bref commentaire sur le

livre, une première réaction, de la part de personnes qui se consacrent activement à l'étude des migrations ». Cette chronique rassemble les réponses qu'ils ont fournies et la réplique de l'auteur : on appréciera l'intérêt et la diversité des points de vue exprimés, par six des principaux chercheurs qui oeuvrent dans ce domaine.

Les migrations sont de toute évidence une composante essentielle de l'étude du développement urbain et régional, et singulièrement des disparités urbaines et régionales. Puisse cette table ronde contribuer au débat sur ces enjeux importants.

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CHALLENGING ISSUES IN THE STUDY OF FISCALLY-INDUCED MIGRATION

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Regional disparity has persisted for such a long time in Canada that one must suspect that the current state of affairs represents an equilibrium that is partly supported by public policies, such as unemployment insurance, that subsidize residence in disadvantaged parts of the country. This is not to say that such policies are necessarily bad. We may want to consider the consequences for migration and regional disparity of place-oriented public policies as simply one of the costs worth bearing in the pursuit of other goals. It is still important, however, even in this case, to know what these costs are, and acquiring evidence about the influence of public policy on interregional migration is a necessary first step in calculating them. Paul Shaw's book [4] provides new evidence concerning the link between fiscal structure and internal migration and is, therefore, most welcome.

Shaw bases his work on a valuable new migration series he has put together from census data: intermetropolitan flows from 1956 to 1981. Individuals are classified as movers or stayers according to their census metropolitan area of residence in successive census years. The existence of census data at 5-year intervals from 1951 permits Shaw enough data to estimate migration equations separately for the periods before and after 1971. A comparison of the role of fiscal variables before and after 1971 is particularly interesting to those concerned with fiscally-induced migration because unemployment insurance, equalization payments and provincial natural resource revenues probably became more important in determining regional differences in

comprehensive incomes after 1971 than they were in the prior twenty years. We should therefore expect to see the influence of these fiscal variables increasing in some sense in a migration equation fitted to data after 1971, and such a demonstration would tend to bolster the view that fiscally-induced migration is indeed an important phenomenon in Canada.

In the theoretical approach to his estimating equations, Shaw casts his net somewhat wider than a comparison of the role of fiscal structure before and after 1971. He suspects that migration behaviour has changed fundamentally over the thirty years since 1950, becoming less sensitive to traditional market variables such as wage differentials and more sensitive to other factors, including quality of life indicators as well as fiscal structure. His central finding in this respect is based on a comparison of two equations, both of which omit fiscal variables, one fitted to data before 1971 and one to data from 1971 to 1981. He concludes that narrowly defined economic variables such as wage differentials have become less important after 1971, since coefficients on wages in origin and destination are smaller (in absolute value) and less significant after 1971 and the overall R^2 is lower for the post-1971 equation. This, according to Shaw, confirms his "core" hypothesis of the declining role of narrowly defined economic incentives in the migration decision.

While I agree that this is an interesting hypothesis to consider, Shaw's conclusion cannot comfortably rest on estimating equations that omit fiscal variables. An internal migration equation that omits fiscal variables is misspecified, as his Models 5 and 6 clearly show. And in these more inclusive equations the wage variables have larger and more significant coefficients *after* 1971.

In any case, statistical significance and "importance" are not the same thing, even if coefficients are normalized to account for the difference in the magnitude of explanatory variables (the beta coefficients). In the first place we must always worry about correlation of variables. Beta coefficients are correlated if explanatory variables are, so even after normalization we cannot simply rely on the relative size of coefficient values to judge the importance of the corresponding variables. But, most important, the overall role of wage differentials, say, in determining observed migration patterns is the product of the coefficient on wage variables and the actual evolution of wages over the estimation period.

Another problem with the methodology used to contrast pre- and post-1971 migration equations is the apparent neglect of statistical testing for shifts in coefficients on the traditional economic variables and for shifts in the coefficients on the fiscal variables. Differences between equations in the size of coefficients on the same variable may

not be statistically significant. There are standard techniques for checking this out, and they could have been used. Of course, "large" algebraic differences can turn out to be statistically insignificant.

Thus it seems to me that the evidence on the change in migration behaviour over the sample period is not conclusive. Yet there are still many useful results in the book for us to consider.

The strongest of these, in my view, are those concerning the role of unemployment insurance. I would also make this judgement about my own study of internal migration and fiscal structure with Denis Gauthier [6]. The single most difficult problem in estimating the influence of fiscal structure on internal migration is that of measuring net fiscal benefits appropriately. It is easy to get a measure of aggregate fiscal variables by province, such as natural resource revenues or equalization grants. But these aggregates are several steps removed from the theoretically correct variable we want to include in a migration equation explaining the individual migration decision. Ideally we should measure the net fiscal benefit accruing to an individual that results from the provincial taxation of natural resources or from the receipt by the province of equalization grants. The fact that we do not have such measures means that we cannot really be sure of what we are estimating when we use fiscal aggregates in migration equations, even though the results may be highly suggestive. The necessity of finding proxies for individual fiscal benefits is what attracted Denis Gauthier and myself to data that disaggregated migrants by income class. There is good reason to suspect that fiscal incidence varies systematically with income class, and if so, disaggregation by income class controls for one of the factors or steps that lie between aggregate fiscal variables and migrants' net benefits.

Unemployment insurance (UI) payments, unlike equalization or provincial resource revenues, are paid to individuals in specific locations and are recorded as such. Thus it is possible to construct a variable reflecting UI generosity based on what has actually been received by individuals in each location in the migration data. But this is not the only reason why the results concerning the role of UI are strong. Because the generosity of the UI system was to some extent tied to regional unemployment rates after 1971, we should expect to see the UI variables working better (in terms of the sign and significance of coefficients) in a migration equation fitted to data after 1971, and that is what we find in Shaw's Models 5 and 6. Furthermore, since UI is on the average a larger component of comprehensive income east of Ontario and Quebec, we should expect UI variables to show up better in an estimating equation explaining migration from the east than in one explaining migration from the west. Again, that is what we see in the disaggregated results.

The evidence that the UI system has a significant influence on internal migration is now reasonably compelling. We have evidence from a fairly wide variety of models and data sets, including Tom Courchene's work [1], my own, and the work presently under review. We are still not clear about what the policy implications of this are, however, a point to which I shall return later.

The other fiscal variables Shaw considers are unconditional grants and natural resource revenues. These do not yield as good results (in terms of the sign and significance of their coefficients) as the UI variables. Still, Shaw has some success with the grant variable using the complete migration data set after 1971 and when using data on migration from the east over the 1956-81 period. He has less success with natural resource revenues. This variable does not perform at all well. One of the problems here, as Shaw points out (p. 110), is that provincial natural resource revenues tend to be positively correlated with wages and other market variables. This makes it difficult to separate out the influence on migration decisions of wage differentials on the one hand and fiscal structure on the other. It is not correct to argue, however, as Shaw apparently does, that this sort of collinearity implies that provincial natural resource revenues have no role to play in recent migration trends. Collinearity makes this role difficult to estimate, but it may exist nonetheless. Until we look at the statistical significance of direct measures of the natural resource rents that individuals can expect to capture through provincial public sectors, the role of such rents in interregional migration decisions is going to remain an open question.

Other issues also provide opportunities for future research. Recent work on models with qualitative dependent variables (for example, by Daniel McFadden [2]) suggests somewhat different estimating equations than those used by Shaw, equations which are more directly built on individual constrained optimization, such as the multinomial logit model Denis Gauthier and I used, and which, unfortunately, have variance-covariance structures more complicated than that assumed in Shaw's work.

The self-selection problem still remains largely unexplored in Canada, with the important exception of the work by Chris Robinson and Nigel Tones [3]. People who migrate may differ systematically from those who do not. In this case, there is a danger of confusing the consequences of migration with the personal characteristics of migrants. Or we may confuse the effect on the migration decision of the attributes of alternative destinations with the effect of the peculiar characteristics of migrants. Making these distinctions requires that we explicitly acknowledge, econometrically speaking, that some types of individuals may be more likely to migrate than others, even when con-

sidering exactly the same set of alternative destinations. This involves more than simply adding to the list of variables explaining the migration decision.

Despite these opportunities for improving upon the econometrics of migration modelling, Shaw's book is a valuable addition to the empirical literature in Canada. It adds substantial weight to the view that variation in fiscal structure plays a significant role in determining internal migration patterns. There are also other interesting results in the book on the role in the migration decision of such varied factors as education, climate, crime, homeownership and immigration, which I have not reviewed.¹ All of this is set out carefully and in a manner which is generous to the reader.

For reasons outlined above, I am not convinced that market forces have become substantially less important as determinants of internal migration patterns over the last thirty years. My own view is that the proportion of moves that can be attributed to interregional variation in net fiscal benefits is not large, at least on a year-to-year basis. Nevertheless, this small flow should still be of concern if the effect of fiscal structure persists over decades, as I have suggested at the outset of this review may have been and may continue to be the case in Canada. In particular, the influence of fiscal structure on net migration from less developed regions of the country may still be an important, longer run issue. If measuring fiscal benefits is the foremost problem facing those interested in the link between internal migration and fiscal structure, the second most pressing problem is to find a way to make a quantitative statement about the magnitude of the influence of UI and other public policies on the pattern of internal migration over long periods of time.

Another particularly important problem that remains to be adequately addressed is that of developing useful normative frameworks for the design and evaluation of internal migration policy. As Bill Watson [5] has recently reminded us, a statement about the influence of equalization and other public policies on migration decisions is not the same as a statement about the consequences for economic welfare of the resulting migration flows.

¹In making the case that an internal migration equation will incorporate the influence of immigration from other countries via the impact of immigrants on domestic market conditions, Denis Gauthier and I referred to a particular example in a way that clearly, but erroneously, indicated that we believed that particular example to be true. I was happy to find in Shaw's results evidence suggesting that example was poorly chosen. It would appear that immigration does not reduce the attractiveness of a metropolitan area to internal migrants. Indeed, the opposite may be true.

Paul Shaw's contribution will be of substantial help to anyone who wants to pursue these and other challenging issues concerning internal migration in Canada.

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ON TESTING THE HUMAN CAPITAL MODEL OF MIGRATION

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In my commentary on Paul Shaw's new book [9], I am interpreting my mandate broadly to initiate some discussion about the nature of modelling in the migration literature. In particular, I want to argue that the available empirical evidence on the human capital model as applied to migration has enough problems and "black holes" to make me skeptical about its validity. This is in no way intended as a criticism of Shaw's work, which is very much in line with the existing literature, including some work I have been involved in over the years. In fact, this discussion can be seen as related to some of Shaw's concerns as expressed in his Appendix to Chapter 2 [9:169-171].

Human Capital

Paul Shaw's book contains a useful explanation of the basic cost-benefit nature of the human capital model in Chapter 3. Julie DaVanzo's [1] more elaborate and critical analysis of this model makes clear

that a wide variety of sociological models of migration behaviour can be fitted under the human capital umbrella, particularly if we think of costs and benefits in terms of utility (or real income). In her presentation, the main contribution of economic analysis is to emphasize the interrelated concepts of time, future discounts, uncertainty, and information. The emphasis on the labour market aspects of migration is, in my view, also a distinctive feature of the economist's research. The basic hypothesis can be stated as follows: the probability of someone (or a family) making the decision to migrate from A to B is greater, the larger are the net benefits (approximately discounted) to be obtained from doing so. Most economists would concentrate on such labour market variables as wages and employment prospects as determining these net benefits, although more recently public expenditure and tax variables have been included, and such "objective" indicators of utility as crime rates, climate, and language differences would be admissible, as in Shaw's work. Conceptually, people are pictured as comparing the situations in a variety of possible destinations (including the "home" location); they take account of the costs of moving and adjusting, and they choose the alternative which generates the largest net benefit. In what follows I shall concentrate on the testing of this human capital model in terms of the labour market variables reflecting income and employment opportunities. This is done partly to keep the discussion within reasonable bounds, but also because there is probably some consensus among researchers that these variables matter in the choices of most potential migrants. It does imply that we should exclude some special groups in our empirical analysis, such as retired people, students, armed forces, and intra-company transfers [1].

Macro Tests

Tests of this framework typically proceed in a macro context with the aid of tabulated variables including averages. Thus the dependent variable becomes a proportional frequency M_{ij}/P_i , the proportion of a population in region i who have made the move to region j during a particular period, or a relative frequency M_{ij}/M_{ii} , where M_{ii} is the group of people who are still in i at the end of the period. Shaw prefers the second specification of the dependent variable, and there are some good theoretical reasons for this (see Grant and Vanderkamp [4]). The first is usually conceived as a probability and the second as a ratio of probabilities, or "odds" specification. Either specification carries the implicit assumptions that the people actually moving from i to j are those in the P_i -group who expect to gain the largest net benefit from moving, and that among the possible destinations j represents the "best" alternative for this group. It may be countered that this problem

is inherent in tests in a macro context, and I do not deny that. But it is not a mere quibble, since it is at least conceivable that people move for other reasons, and it so happens that these reasons appear to be related, as specified in a macro test. I shall return to a construct with "other" reasons towards the end of this review.

The labour market determinants of income and employment opportunities are typically represented by two average wage rates in regions i and j (W_i and W_j) and two average unemployment rates (U_i and U_j). The implicit assumption is that the individual opportunities are somehow directly related to these average variables. This relationship between opportunities and averages must be quite complex to be consistent with some migration between most regions. I cannot find any reference to this issue in Shaw's book, but it is safe to presume that there are few zero elements in his migration matrices. For example, if we assume that there is a simple proportional relationship between individual wage opportunities and average wage variables, then a lot of the observed migration flows are in the wrong direction. This kind of thinking leads to the frequently heard, but simple-minded, observation that a large part of Canadian migration is "inefficient", as there are many flows in the "wrong" direction. If there is a stochastic relationship between opportunities and average variables, then we may be able to justify migration in every direction, but it is clear that such a relationship becomes part of a joint hypothesis that is being tested.

Finally there is the vexed question of simultaneity. Simply put, there are other relationships between migration and the so-called determinants than the one being tested. Shaw gives a brief discussion of this issue (p. 189-190), and he opts to minimize simultaneity bias by measuring his explanatory variables at the beginning of each migration period. While this obviously helps, it does not solve the technical problem of bias as long as the determinants change little over time. Moreover, this solution denies *a priori* that current conditions have an influence on migration decisions, a proposition that would be difficult to defend. The traditional types of simultaneity relate to the effect of migration flows on excess supply (demand) variables, and thus on relative wages. But there may also be other channels whereby the selectivity of migration may have a direct impact on wages and a further impact on regional employment growth. The testing of a particular hypothesis is more complicated if it needs to be conducted in the context of a simultaneous equations model, since the results regarding the hypothesis will generally vary with the specification of the rest of the model. How important is this issue? In some recent work [10] I have been surprised by the different results obtained in the estimation of the same migration equation by itself and as part of a simultaneous model of regional adjustment; in 2- and 3-stage least squares estimates

of the full model the R^2 -statistics are much lower, and the size and significance of the coefficients of the unemployment variable are most strongly affected. While these results are preliminary, they point to potential bias in testing the human capital model.

In short, there are a number of problems and shortcomings in testing the human capital framework applied to migration with the aid of macro data. In particular the test becomes a test of various joint hypotheses, including implicit assumptions, and the results may therefore not lend unequivocal support to a human capital interpretation.

Micro Testing

If macro models are hampered by these problems, the obvious alternative is to use micro data in testing the human capital framework. For example, one could attempt to duplicate the Shaw equations with the aid of individual observations drawn from one of the available micro data bases. But there are a number of difficult obstacles along this path, and it is perhaps not surprising that there are so few examples of empirical work regarding individual migration decisions. In fact I know of only one piece of published research in this area, by Julie DaVanzo [2]. She also reviewed microeconomic approaches to the study of migration decisions [1], and this review discusses in detail a number of the difficulties in conducting such empirical work. She points out that an individual choice model runs into technical problems when one works with small geographical areas, since most choice models can only cope with a few alternatives, as, for example, in the modes of transport studies. The solution may be to reduce the number of geographical regions to four or five, as is done by DaVanzo [2], but this leads to very few migrants being recorded as such and it increases the fuzziness associated with long boundaries between regions; moreover, many researchers have a strong interest in small regions *per se* and in the corresponding distance variance.

The second major problem with a micro model applied to migration is that one has to specify for all individuals their expected wage and employment opportunities in all possible choice regions including the home region. One can estimate earnings and employment probability functions based on characteristics of individuals and regions, but this encounters the perennial micro model problem of selectivity. The Heckman procedure [6] for coping with selectivity cannot really be applied in these circumstances, since there are too many alternatives. Robinson and Tones [8] deal with this difficulty by limiting the choice to move-not move, but this solution is at best partial if selectivity is a multifaceted phenomenon specific to regional choices or at least to the prospective distances of the moves.

Finally, there is the question of the appropriate functional form for micro models. Probably the two obvious estimating equations are probit and logit. To concentrate on the logit specification, this functional form is based on a particular utility function [7]. It is assumed that each individual has a utility function (for example, a logarithmic function as in Grant and Vanderkamp [4]) related to the attributes of the various choices that has a component common to all individuals plus a stochastic taste element that reflects the idiosyncracies of each individual's preferences regarding the various choices, including specific locational preferences. The representative part of the utility function is specified in terms of measurable variables, such as expected wage and employment opportunities, and the stochastic part is supposed to look after unmeasurable variables. These stochastic elements are assumed to be independently and identically distributed with the so-called extreme value distribution, and this then produces the logit equation in which the log of probability ratios is the dependent variable. The question is whether the distributional assumption can be maintained under a variety of specifications in terms of measurable variables. Shaw includes a number of variables, such as crime rates and climate, which in earlier work were ignored and therefore included under the stochastic taste component; in fact, Shaw maintains the same functional form as he expands the number of explanatory variables successively from 4 to 26. It is not clear to me that we can continue with the same assumption about the distribution of unmeasurable taste aspects as we are able to expand the number of taste-related variables, a distinct possibility if we start using more sociology-style surveys to study migration behaviour.

Moreover, the distribution of these stochastic taste elements across a particular population is bound to change as a result of migration itself, and in two opposite ways. First, those with the largest taste for moving are likely to move soonest, thus reducing the average probability of subsequent migration. Second, once some individuals have moved to another location they may provide information back to the "stayers" about conditions in potential destination areas, and this will tend to raise the average probability of subsequent migration; this is sometimes called the "friends and relatives" effect, and may work in the opposite direction if these friends turn up as return migrants. These effects call for a good micro data base to be tested, as the attempts to conduct macro tests of these phenomena are inadequate. The first tendency is also counteracted by the natural regeneration of a particular population, as for example, about 10 percent of the population of working age is "turned over" every five years. It may be thought that this problem of partly endogenous taste-distributions is unimportant in cross-section analysis of macro data, but that is incor-

rect, since the recent migration history of the region may then have an impact on observed migration probabilities. In short, the issue of specification presents a number of problems, and it may well be necessary to consider different micro specifications, depending on the variables available in the data base.

Earnings Impacts

If choice models present serious conceptual and technical problems in testing the human capital model, then we should perhaps concentrate on studying the consequences of migration decisions; for example, on individual earnings. Micro data can be used in the estimation of earnings functions that include some migration variables. As already discussed, this would involve coping with the selectivity problem. Even if we are prepared to ignore this problem, the results reported in the literature on migration pay-offs are not comforting as a test of the human capital model. As Shaw discusses in his appendix to Chapter 2, the results in Grant and Vanderkamp [5] and other analyses of migration pay-offs within a 5-year period suggest that the impact of migration on earnings is small if not negative. Robinson and Tomes [8], who attempt to control for selectivity, conclude that their results are in line with the implications of the human capital model, but they do not report any pay-off estimates to confirm this judgement.

Conclusion

In conclusion, testing for the validity of the human capital model as applied to migration, with particular emphasis on labour market determinants and consequences, is not an easy matter. The available evidence suggests the following stylized facts: people tend to migrate in larger numbers over shorter distances, from regions with low average wages, low rates of employment growth, and excess supply to regions with high average wages, high employment growth, and excess demand. Despite these tendencies there has been little change in the ranking of regions with regard to any of these variables. The estimated pay-offs to migration have been small.

For the sake of stimulating discussion and research, let me sketch an alternative set of behavioural relations that is consistent with these stylized facts but not with the human capital model. Suppose that a certain proportion of people in all locations at a certain point in their lives get "itchy feet", meaning that they wish to leave their home location. The proportion may not be the same in all locations, as it will depend on the sociological make-up of the community, and the age at which this itch strikes will generally be fairly early in the life-cycle for psychological reasons. Because of the costs of moving, these itchy feet

may not travel long distances, although those with some wealth may be more ready to do so. Since everyone needs income to live and enjoy it, these people will generally respond to job vacancies. These job vacancies arise in all regions but with greater frequency in large labour markets and in those with expanding employment bases. For exogenous reasons, such as changing comparative advantages and resource discoveries, some regions happen to be blessed with more rapid employment growth than others, and these regions therefore attract a larger proportion of the itchy feet migrants. Migration is a selection process, and those who select themselves not only have itchy feet; they are also more ambitious, more prepared to take risks, and more entrepreneurial in spirit. Once a regional population, due to fairly persistent in-migration, has been thus "enriched", this region will have a higher average level of incomes and wages. This enrichment may then also lead to a higher rate of employment growth, endogenously, due to the stronger entrepreneurial spirit. In short, in this scenario the pattern of migration "determines" the pattern of regional wage rates, not the other way around. Moreover, there is no reason to believe that the migrants will experience extraordinary income gains over a period such as five years, although we would expect them to do better over their lifetimes. No doubt I could elaborate on this "theory", but hopefully the basic idea is clear to the reader.

There is clearly a fairy-tale element in this scenario, and I for one do not believe all its components. Nevertheless, it is presented here in the hope of stimulating further analysis and empirical work on this important set of questions. No doubt this will require a better longitudinal data base than is currently available, with the important consideration that it should permit the analysis of the selectivity aspect of migration behaviour. Furthermore, we shall need to be more precise in specifying the implications of the human capital model, and possible competing theories. Paul Shaw's book is a novel and thorough contribution to the list of Canadian empirical studies in migration, and I can only hope that he forgives me for being pulled along such "devious" paths by reading it.

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COMMENTARY ON INTERMETROPOLITAN MIGRATION IN CANADA

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Shaw's work [7] is a significant contribution to the migration literature. With a set of clear and relevant themes (for example, the declining influence of the traditional economic variables on migration and the increasingly strong role played by the government) running through the whole text, the book is highly readable and particularly valuable for teaching purposes. It is also rich in substance.

Assuming that other commentators would summarize the main points of the book, I would like to focus my comments on a few aspects on which I have some research experience or a strong opinion. These aspects deal with (1) conceptual framework, (2) selection of explanatory variables, and (3) estimation and statistical inference.

Conceptual Framework

Shaw uses a generalized cost-benefit framework for a potential migrant, represented formally by the following formulation:

- (1) $0 < P(M_{ij}, t) \leq 1.0$, if and only if,
- (2) $IU_j(t) - IU_i(t) > 0$, with,
- (3) $IU_j = \int_0^n Q_j(t) e^{-rt} dt - C$,
- (4) $IU_i = \int_0^n Q_i(t) e^{-rt} dt$,

where $P(M_{ij}, t)$ = probability of migrating from place i to j at time t ; IU_j ; IU_i = the individual's discounted utility streams at place j and i ; Q_j , Q_i = overall quality of life that exists or is expected at j or i ; r = discount factor reflecting the degree of consumption time preference for the typical individual; C = initial fixed cost of migration relocation; and n = the length of the time horizon. This formulation is then gradually "filled out" in a non-rigorous, though quite systematic, fashion until an operational model is obtained.

This formulation is unattractive for several reasons. First, for a homogeneous group of individuals (that is, those with the same age, same education, and so forth), it does not allow the existence of the often-observed phenomenon of two-way flows. Second, it implies that the attractiveness of an alternative destination k does not help to determine whether the probability of migrating from i to j will be non-zero. Third, since it does not indicate how the magnitude of $P(M_{ij}, t)$ is related to the size of the difference between the $IU_j(t)$ and $IU_i(t)$, this formulation is quite irrelevant to most of the substantive hypotheses presented in Chapter 3. It would be more straightforward to replace (1) and (2) by something like:

$$P(M_{ij}, t) = f[IU_j(t) - IU_i(t)]$$

where f is a monotonically increasing function.

What Shaw ends up with is a polytomous logit model that was derived by McFadden from the random-utility choice theory [6]. I would like to mention that the choice theory has progressed substantially since the early 1970s. We have seen the emergence of the nested logit model (see Ben-Akiva and Lerman [1]), which is particularly suitable for modelling migration. The lack of symmetry between origin and destination variables, which is so clearly demonstrated by Shaw and many other migration researchers (notably Lowry [5]), indicates that the origin is perceived by a potential migrant differently from the potential destinations. In other words, there are two "naturally distinct" sets of alternatives: one set contains only the origin, and the other contains all potential destinations. This natural partition of the

choice set implies that the nested logit is a superior model. All the substantively useful ideas obtained from the cost-benefit framework can be incorporated into this "new" model, if one starts with the current version of random-utility choice theory.

Choice of the Explanatory Variables

In selecting the explanatory variables, Shaw deliberately excludes the destination population size (P_j), because it, being similar to migrant stock, "artificially boosts the R^2 value while confounding the significance of socio-economic influences which are in fact determining migration" (p. 191). In my opinion, the effect of P_j , albeit indirect, is by no means artificial, particularly in a system of Canadian metropolitan areas that range in size from about 100,000 to over 2,000,000 people. In general, the larger CMAs tend to have more high-threshold attractions (for example, first-class recreational and entertainment facilities, convenient transit system, and specialized services to different ethnic groups), which are as real as the traditional economic variables. In my analysis of the intermetropolitan destination choice pattern of Canadian labour force entrants during 1971-1976, P_j is shown to be highly significant and robust and to coexist quite well with destination employment growth [3].

To represent the severity of winter, Shaw uses the average snowfall of 1965 and 1980. It seems that a better variable is the number of degree days below 18°C. We know that St. Catharines has a mild winter and a lot of snow, whereas Saskatoon has a very cold winter and not much snow. This variable is shown to have a powerful effect on the destination choice probabilities of intermetropolitan migrants in Canada—strong for young adults and even stronger for the elderly [3;2].

Estimation and Statistical Inference

Beside the ordinary least squares (OLS) method used by Shaw, researchers have used the maximum likelihood (ML) and the maximum quasi-likelihood (MQL) methods to estimate the parameters of the logit model and compute indices (for example, the t-ratio) for statistical inference. OLS is easy to use, but it may be misleading because it does not give greater weights to more reliable observations as the other two methods do. ML is based on the unrealistic assumption that migrations are independent events; therefore, it yields unrealistically small standard errors for the estimated parameters, resulting in t-ratios of greatly inflated magnitude. In other words, strict adherence to the ML method forces the user to declare that practically any variable is significantly related to the dependent variable. MQL is applicable even

when the "bandwagon" effect exists. Since migrants tend to move with the family as a unit and are subject to the influence of friends and relatives, the MQL method is most consistent with the real-world migration behaviour [4].

No matter which method is used, the evaluation of the relative importance of the explanatory variables is complicated by the problem of "multicollinearity" (that is, the stickiness among the explanatory variables). Since we cannot carry out a controlled experiment with human migration, there is no real solution to this problem. Suspecting that P_j might be highly collinear with the economic variables, Shaw excludes it from his model. It is important to note that the reason for excluding P_j rather than some statistically weak variable like unemployment rate is that P_j does not exist in his chosen theory of migration.

Being once an immigrant myself, I wish to believe in Shaw's inference that "immigrants may stimulate labour market opportunities at a CMA through their consumption, investments and entrepreneurial talent" (p. 21). However, if both internal and foreign migrants are attracted by large population size, the positive coefficient of $IMMIG_j$ may simply be spurious.

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UNE ANALYSE ÉCONOMÉTRIQUE DE LA MIGRATION PARTICULIÈREMENT RÉUSSIE

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Shaw [3] présente une excellente étude des facteurs qui, dans les pays développés les plus riches, influencent la migration interne. Appliquée au cas des flux migratoires intermétropolitains au Canada, cette étude s'inscrit parmi les toutes premières du genre, en raison tant d'une analyse économétrique particulièrement réussie que d'une présentation méthodique exceptionnellement soignée.

Remarquons ici que l'auteur a rédigé son texte non pas en français mais en anglais. Cependant Statistique Canada, organisme auquel il est lié en tant qu'analyste principal, a pris l'heureuse initiative de s'associer à la diffusion de son ouvrage, rendant ainsi possible la publication d'une traduction française. Certes cette traduction est loin d'être parfaite du fait de nombreuses erreurs typographiques et d'imperfections notables au plan du style et du choix des mots : ainsi, l'emploi du mot « facteurs » ou « déterminants » eût été préférable à celui de « causes » dans le titre même de l'ouvrage. Néanmoins, dans l'univers francophone, où les études intéressantes sur les facteurs qui influencent la migration interne dans les pays développés n'abondent guère, la version française du livre de Paul Shaw doit devenir un ouvrage de référence très prisé.

La qualité remarquable du travail effectué par l'auteur découle en grande partie de sa volonté délibérée de se situer à un niveau élevé : « J'ai tenté d'accomplir plus qu'une autre recherche des déterminants de la migration », annonce-t-il dans l'introduction. D'une part, rejetant toute idée d'analyse partielle ou illustrative qui sous-tend tant d'études de type académique, Shaw s'efforce de parvenir à des résultats concrets qui soient utiles aux planificateurs et aux décideurs oeuvrant dans le domaine des disparités régionales. D'autre part, évitant d'épouser l'attitude relâchée de nombre d'auteurs qui (1) se contentent de réunir un ensemble de variables explicatives de tout bord sans se soucier outre mesure de leur diversité et de leur compatibilité et (2) établissent des équations de régression sur la base de règles ad hoc peu satisfaisantes, l'auteur s'efforce à suivre une démarche statistique cohérente dont les autres chercheurs s'intéressant aux facteurs de la migration feraient bien de s'inspirer. Selon cette démarche, les variables explicatives, regroupées en un certain nombre de catégories représentatives d'effets bien distincts, sont introduites de manière séquentielle. Après chaque nouvelle introduction, l'influence des variables ajoutées est

évaluée tant au point de vue de leur contribution à la valeur explicative de l'équation de régression qu'au point de vue de leur impact sur la stabilité des coefficients relatifs aux variables précédemment incorporées.

L'analyse débute naturellement par l'examen des déterminants économiques de la migration de type traditionnel. Dans une première étape, l'auteur définit un modèle de base reposant sur la seule comparaison des rémunérations salariales dans les régions d'origine et de destination, mais où figurent également deux variables représentatives des facteurs qui tendent à freiner les mouvements d'un lieu à un autre (la variable distance et une variable muette reflétant la barrière culturelle et linguistique qui s'élève entre le Québec et le reste du Canada). Appliqué à des données quinquennales de migration entre les régions métropolitaines canadiennes couvrant la période 1956-1981 (mais où il y a un trou correspondant à la période 1961-1966), ce modèle de base ne parvient à expliquer qu'environ un quart de la variance des migrations observées (un peu plus pour les années antérieures à 1971, un peu moins pour les années postérieures). À partir de là, l'auteur élargit son modèle de base en trois étapes successives marquées par l'introduction d'autres variables économiques comme la variable emploi (vu que les rémunérations salariales ne se corrigent pas d'elles-mêmes pour équilibrer l'offre et la demande de main-d'oeuvre), la variable chômage et enfin la variable mise en chantier de logements (supposée refléter l'évolution du cycle des affaires). Grâce à cet élargissement du modèle de base, la part de variance expliquée par les variables économiques traditionnelles dépasse 50 % pour la période antérieure à 1971 mais n'est portée à guère mieux que 25 % pour la période 1971-1981.

Dans une cinquième étape, Shaw ajoute une composante « secteur public » ou « fiscalité » (au moyen de variables reflétant les prestations d'assurance-chômage et les paiements de péréquation du gouvernement fédéral). Cet ajout n'est véritablement significatif que dans le cas de la période 1971-1981. Toutefois l'accroissement de la variance expliquée ne permet pas au coefficient de détermination de rejoindre la valeur correspondante pour la période antérieure, puisqu'il s'établit à un niveau qui reste inférieur à 50 %. Ce résultat confirme de façon éclatante le bien-fondé de l'intuition de l'auteur qui, se basant sur sa propre perception des changements profonds qui influencent la structure des préférences sociétales, s'attendait à mettre en évidence (1) le déclin de l'influence exercée sur la migration interne par les variables économiques traditionnelles et (2) l'importance accrue de l'impact exercé par les variables liées aux programmes gouvernementaux en matière fiscale et de sécurité sociale. Cependant, vu la crise économique du début des années 1980, l'on peut légitimement se demander

si ces tendances se sont effectivement prolongées jusqu'à nous sans infléchissement ou retournement.

Dans une nouvelle étape, l'auteur introduit d'autres variables, économiques ou non, afin d'en arriver à un modèle plus complet. Malheureusement, dans la mesure où il soulève un certain nombre de difficultés statistiques, le modèle obtenu donne lieu à une interprétation plus ou moins solide. Il semblerait cependant que des variables non économiques comme la criminalité ou le climat aient peu ou pas d'influence sur les migrations intermétropolitaines.

À partir de là, conscient d'avoir limité ses investigations à l'ensemble de la population, Shaw poursuit son analyse à un niveau plus désagrégé selon deux dimensions. Tout d'abord, isolant les flux migratoires intermétropolitains selon leur origine est ou ouest (excluant les flux issus des régions métropolitaines situées dans la partie centrale de l'Ontario), il met en évidence une variation fort sensible, entre les deux types de flux, du degré de signification des variables, surtout dans le cas des variables fiscales : l'influence de celles-ci se fait plus sentir à l'Est qu'à l'Ouest. Ensuite, reprenant son analyse initiale pour trois groupes de migrants distingués selon leur niveau de scolarité, Shaw ne parvient à déceler que de légères différences de comportement entre les trois groupes, ce qui l'amène finalement à conclure que les mêmes comportements migratoires peuvent être observés dans la population tout entière et dans la population d'âge actif.

De par leur netteté, les résultats obtenus par Shaw ne manqueront pas d'engendrer un débat social d'envergure (voir les autres comptes rendus du même livre publiés dans ce numéro). Plutôt que de se joindre à ce débat, l'on préfère ici entreprendre une critique de certains aspects méthodologiques du travail effectué par Shaw. En premier lieu, l'on regrette la restriction de l'analyse aux seules migrations intermétropolitaines : l'intensité des migrations à destination des régions non métropolitaines est non seulement non négligeable mais aussi étroitement liée à celle des migrations à destination des autres régions métropolitaines. Aussi, toute analyse future de la question se devrait d'adopter une méthodologie autorisant l'examen simultané des migrations à destination des régions métropolitaines et des migrations à destination des régions non métropolitaines. Cette méthodologie pourrait s'appuyer sur un ensemble séquentiel de trois modèles logistiques (three-level nested logit model) tel que l'ont récemment proposé Liaw, Kanaroglou et Moffett [1]. Selon cette méthodologie, le premier modèle (dichotomique) reflète la décision de migrer ou non; le deuxième modèle (également dichotomique) se rapporte, dans l'éventualité où l'on émigre, à la décision de choisir comme destination une région qui soit métropolitaine ou non; enfin, le troisième modèle (polytomique) décrit, dans le cas des migrants à destination d'une région

métropolitaine, la sélection d'une région métropolitaine particulière. À ce dernier modèle, l'on pourrait sans doute juxtaposer un autre modèle logistique polytomique décrivant, cette fois pour les migrants à destination d'une région non métropolitaine, la sélection d'une province particulière. Évidemment l'inconvénient majeur de ce type de modélisation - par rapport à l'utilisation de l'unique modèle logistique polytomique telle qu'elle est faite par Shaw - est de soulever un certain nombre de problèmes statistiques entraînant une sérieuse complication de la procédure d'estimation.

En deuxième lieu, l'on doit ici s'interroger sur le degré de pertinence associé à certaines variables explicatives retenues par Shaw, même si la sélection des variables qu'il a effectuée est la plus complète jamais réalisée dans le cadre d'une analyse des déterminants de la migration interne au Canada. Sur la base des résultats obtenus par une analyse récente de la migration interprovinciale (Liaw et Ledent, [2]), l'on peut raisonnablement penser que l'analyse de Shaw aurait beaucoup à gagner de l'utilisation de variables additionnelles, par exemple :

- un index de dissimilarité culturelle représentatif de la différence entre les compositions ethniques de la population des régions métropolitaines d'origine et de destination (au lieu de la variable muette reflétant la barrière culturelle Québec-reste du Canada);
- une variable de température susceptible à notre avis de mieux refléter la dureté du climat que la hauteur des chutes de neige (variable non significative selon Shaw); et
- un index représentatif du degré de mobilité de la population (pourcentage de la population née hors de la province), car la propension à migrer hors de la province de résidence est largement plus forte pour ceux qui sont nés hors de la province que pour ceux qui y sont nés.

Enfin, pour terminer, l'on regrettera certains préjugés exprimés par Shaw qui, s'ils n'influencent en rien ses résultats, n'en risquent pas moins d'indisposer certains de ses lecteurs. Ainsi donne-t-il un satisfecit au type de données migratoires qu'il utilise (données du recensement) tandis qu'il condamne sans justification adéquate d'autres types de données (par exemple en provenance du fichier des allocations familiales). On eût aimé une plus large discussion des limites présentées par les données du recensement, que d'ailleurs Shaw utilise faute d'une autre source possible de données migratoires relatives aux régions métropolitaines. De même l'auteur s'en prend-il de manière appuyée aux non-économistes, leur reprochant d'avoir peu contribué à la connaissance des mécanismes de la migration, notamment en raison d'un manque de rigueur théorique (voir deuxième paragraphe de l'introduc-

tion et à nouveau page 30). À ce préjugé selon lequel l'étude de la migration se réduit à la seule analyse des facteurs de ce phénomène, il nous est facile d'opposer la contribution des autres chercheurs en sciences sociales (démographes, sociologues et géographes) qui, au moyen de leurs théories et modèles propres, éclairent certaines facettes de la migration tout aussi dignes d'intérêt que les facteurs qui la déterminent.

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POLITICS AND ACADEMIC INNOCENCE: A CRITICAL REVIEW*

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Shaw's book [2] is at once empirically sound, policy relevant, and fundamentally compromised because of its too narrow focus upon questions of economic efficiency. This sums up my overall assessment of the book. However, there is much more to Shaw's book than is conveyed by this simple summary statement. Before tackling the fundamental conundrum posed but not answered by the book, some of the best parts of Shaw's study are highlighted.

To begin, it must be acknowledged that Shaw has completed an excellent empirical study of Canadian intermetropolitan migration. The utility of his empirical analysis has three essential ingredients. First, Shaw has completed one of the few systematic studies of intermetropolitan migration that covers much, if not all, of the post-World War II era. Using common explanatory variables and a common analytical framework, he has been able to develop a comprehensive empirical analysis of migration, which heretofore has been fragmentary at best.

*I would like to acknowledge helpful comments I received on a draft of this review from Len and Meric Gertler, Larry Bourne and Jim Simmons.

Second, Shaw situates his analysis within a consistent and well-developed theoretical context, following earlier work by Courchene and Vanderkamp. This means that there is a stable theoretical reference point from which to evaluate the significance of his results. Third, the analysis is technically or rather, empirically proficient. Given the data limitations, the theoretical context he chose to situate his study, and the advantages and disadvantages of available techniques, Shaw's work is empirically plausible. For these reasons, it will be read and referenced by a wide variety of researchers interested in the patterns and processes of interregional migration.

Nevertheless, one can imagine criticisms of his empirical work. I will leave the major points to the other commentators. However, to be sure that readers are aware of some of the basic limitations of Shaw's empirical work, a couple of problems embedded in his empirical methodology are emphasized. The data trap the author into a particular modeling strategy. Because the data are census based, and because the dates of the census are administrative rather than economic, the author cannot deal with temporal fluctuations in migration. That is, all the author can do is hope that census years are reasonable proxies for patterns of migration either side of the census years. If, on the other hand, there are strong fluctuations in intermetropolitan migration year-to-year, there may be a significant stochastic element in his analysis unacknowledged, unmodeled, and not factored into his theoretical and policy conclusions.

The data also trap the author into treating cities the same. That is, by virtue of his cross-sectional methodology, geographical units are treated as observations and as a consequence are not directly analyzed. It may be the case that migration as a process is quite different from place to place. However, the author cannot directly deal with these issues because of the data and because of his inherited analytical focus. There are, though, radically different empirical formulations in the literature that use time-series data to analyze the temporal stability of interregional migration and the relative spatial homogeneity or heterogeneity of migration. Granted, these kinds of issues are difficult to treat given the available data, and there is no doubt that the conventional literature has hardly begun to deal with them. Still, in a study which is otherwise quite sophisticated and knowledgeable about the advantages and disadvantages of different modelling techniques, the lack of recognition of these issues leaves this commentator puzzled. In other contexts, it has been shown that patterns of interregional migration are temporally and spatially heterogeneous (see Clark, Gertler, and Whiteman [1], on interstate migration in the United States).

Perhaps one reason that might explain why the author failed to bring to the centre these kinds of issues has to do with the primary

policy focus of the book. Throughout, there is continual reference made to the relative efficiency of interprovincial and intercity migration. Economic efficiency is considered from the standpoint of the national economy, and national macroeconomic planners in particular. The "problem" driving the empirical analysis is in fact how and why various variables affect, and have changed in how they affect, the relative economic efficiency of migration. Specifically, the question is: How have fiscal variables like unemployment insurance modified the effectiveness of wage and employment variables in allocating labour across the landscape? The answer, apparently, is that these fiscal variables have reduced the relative economic efficiency of migration. In this sense, Shaw is not really interested in the spatial heterogeneity of migration, nor is he really interested in the temporal instability of migration. Patterns of migration are rationalized around one overriding concern: macroeconomic efficiency.

There is no doubt that this issue is important. The question of allocative efficiency so overwhelms the study that potentially relevant economic issues like the temporal and spatial stability of migration processes are lost in the rush to prove a case. Taking Shaw's study methodology to its extreme conclusion, it implies migration to be a constant process whatever the time or place. There are hints in the book that Shaw does not necessarily subscribe to this view. For example, he recognizes that different linguistic communities have different information channels. Even so, his analytical framework seems to assume that migration as a behavioural phenomenon is constant whatever the information channels, and whatever the spatial and temporal context.

What also puzzles me is the author's reticence with regard to placing the economic efficiency issue in a broader context. We know that allocative efficiency is the staple commodity of economics as a discipline. The virtue of economics is its ability to rationalize governments' policies and individuals' actions with regard to their consequences for efficiency, and Shaw's book is a model in this regard. However, the implications of Shaw's work are much broader than the allocative efficiency criterion, something the author recognizes only very briefly at the end of the book. He laments the lack of coordinated urban-regional-national policy frameworks, suggesting that a rational policy would integrate in some manner various macroeconomic and regional objectives (efficiency and equity).

But surely the issues are more complex than this, and in a sense already rationalized in the current arrangement of Canadian federalism. Take, for example, the issue of unemployment insurance. By Shaw's reckoning, unemployment insurance has reduced the effectiveness of wages and employment in allocating labour across the

landscape. People remain in place despite substantial differences in prosperity between places, and return to their home provinces as soon as they can if they move for employment purposes. Surely this was intended. After all, the great advantage of unemployment insurance for provinces is that local workers are no longer forced to respond immediately to material imperatives. As a result, provinces retain labour, the local political process is sustained despite short-falls in local economic well-being, and communities are maintained even as their economic vitality is destroyed. To put the issue most crudely: Quebec would be destroyed as a functional political unit if there was no federally funded unemployment insurance. And, maybe, part of the deal that holds the Canadian federation together is the federal government's policy of insulating local workers from having to respond immediately to the changing geography of economic prosperity.

Perhaps the lack of formal integration of regional policies with macroeconomic policies reflects similar political pressures. Just imagine if Shaw's book became an item for debate within the Cabinet. Could we predict which ministers would support adoption of Shaw's findings as the basis of a newly rational federal economic (macro and regional) policy? Those ministers representing the federal economic policy establishment would surely argue the case for restructuring unemployment insurance so as to be consistent with macroeconomic policy objectives. Those ministers representing social and welfare policy making constituents would surely argue for recognition of the tremendous social costs of such a policy. And those ministers responsible for the political fortunes of the party would caution the Prime Minister about the electoral implications of macroeconomic planners' ideas.

While I have no way to know if my characterization of a hypothetical cabinet debate is at all accurate, I have no doubt that the political interests described in this simple scenario have fought one another over the relative importance of efficiency and equity in each government (Liberal and Conservative) since World War II.

Essentially, lack of formal cooperation and integration of macroeconomic and regional policy reflects an implicit bargain made between contending political interests. The bargain is to do nothing that would explicitly integrate these policy interests, and to maintain the welfare of those unemployed according to where they live. To assume that lack of integration is a mistake or simply poor policy practice seems to be naïve. To assume that the political constituency for unemployment insurance is not dependent upon regional alliances seems also to be naïve. Given Shaw's position in Statistics Canada, it is hard to believe that he is unaware of these kinds of political subtleties.

This is the conundrum mentioned at the beginning of the review. Either Shaw is a representative of the macroeconomic planning estab-

lishment, or the political issues involved are so sensitive that anything other than a naïve discussion would be politically dangerous. What is really needed is a book that tackles these issues for all their political complexity. This book is good as an empirical study but fundamentally compromised by its refusal to recognize the deep political issues.

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COMMENTS IN REPLY: NEW DIRECTIONS IN MIGRATION RESEARCH

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This review symposium must be judged a success for its breadth of commentary and provocative reflection by leading scholars in the field. That my book is the focus of the review is an added bonus, for it prompts me to reflect on theory and model specification, problems of estimation and statistical inference, realities of temporal and spatial heterogeneity, choice of explanatory variables, and the importance of broader political issues in migration studies.

Weaker Market Forces

Winer is reluctant to accept an important dimension of my "core hypothesis" that market forces have declined as influences on internal migration in Canada over the last 30 years. He questions my choice of estimating equations that confirm my hypothesis; my apparent neglect of statistical testing for "structural stability" of regression coefficients; and dangers of attributing "importance" to variables on grounds of "statistical significance" alone.

I would be reluctant to accept my "core hypothesis" as well were it not to rest on a plausible marriage of theoretical underpinnings and internally consistent evidence. Theoretical underpinnings include declining relevance of wage considerations in the decision to migrate on

the assumption that marginal utility of money diminishes and marginal utility of leisure increases as societies become wealthier. Add the plausible assumption that social security programs, such as unemployment insurance, reduce pressure to migrate at times of economic adversity and there is good reason to expect confirmation in relatively rich countries. These ideas were first advanced by Shaw [10] following a review of the literature. Since that time, rationale and supporting evidence have accumulated (see reviews by Murdock *et al.* [7]).

Internally consistent evidence in my book includes: (i) reduced absolute size of coefficients on the key economic variables over time in the hypothesized direction—à la Models 1-4; (ii) reduced statistical significance of "traditional" market variables in the post-1971 migration period at a time when far-reaching fiscal changes were introduced; (iii) larger and more significant coefficients on fiscal variables after 1971—as hypothesized; (iv) less relevance of traditional market variables versus greater relevance of fiscal variables in eastern Canada—where fiscal programs are most prevalent; (v) consistency of findings among migrants disaggregated into three educational subgroups. It is the consistency of the evidence over time, across space, and for different migrant subgroups that is impressive.

Do additional statistical tests strengthen my case? Winer would like to see tests for equality of coefficients over time. An appropriate test is the Chow test [1]. To perform Chow tests, separate regressions must first be run for two or more subgroups (for example, pre- versus post-1971 migration). The unexplained variance from each regression must then be compared with unexplained variance from a "pooled" regression that contains all observations. This information is used to test the hypothesis:

$$H_0: (B_0, B_i) \text{ pre-1971} = (B_0, B_i) \text{ post-1971},$$

$$H_1: (B_0, B_i) \text{ pre-1971} \neq (B_0, B_i) \text{ post-1971},$$

by constructing the following 'F' test;

$$F^c = \frac{[\epsilon e_p^2 - (\epsilon e_{\text{pre-1971}}^2 + \epsilon e_{\text{post-1971}}^2)] / K}{(\epsilon e_{\text{pre-1971}}^2 + \epsilon e_{\text{post-1971}}^2) / (N_{\text{pre-1971}} + N_{\text{post-1971}} - 2K)} \quad (1)$$

where B_0 = regression intercept; B_i = regression coefficient on explanatory variables; ϵe^2 = error or unexplained variance in the regression; p = pooled data for pre-1971 and post-1971; K = number of parameters estimated in the model, including the intercept; N = sample size. If $F^c > F_{0.01}$ (at a .01 level of significance), we can conclude that the function, or rather the economic relationship being studied over time has indeed changed.

Preliminary Chow tests were undertaken during preparation of my book and many tables in Chapter 4 contain information on the required pooled regressions. Results were not reported, because they do not affect my conclusions. Before doing so here, we must agree on the most appropriate model for before/after comparisons. On this point, however, we confront Winer's claim that conclusions concerning my "core hypothesis" cannot comfortably rest on estimating equations which omit fiscal variables. Winer is referring here to Model 4 in my book, which I use to test and confirm my "core hypothesis".

I interpret Model 4 as a relatively full "traditional" economic model of migration. It contains the usual wage, employment and unemployment variables; it contains an adjustment for physical and psychic costs or barriers to migration; it contains a human capital or selectivity variable; and it contains an indicator of business cycle and housing activity. It thus seems an ideal candidate for before/after comparisons. Furthermore, there is no indication of multicollinearity among independent variables in the model. This was apparent throughout my research strategy. When variables were added to a relatively simple Model 1 to sequentially produce and test Models 2, 3 and 4, coefficients on independent variables remained stable. Winer, however, suggests that Model 4 is misspecified because it does not include fiscal variables (as does Model 5). He makes this point because the estimated coefficient on the wage variable (in Model 4) becomes larger and more significant when it is estimated in the company of fiscal variables in Model 5.

My response is that Model 5 must be ruled out for "decisive" before/after comparisons because multicollinearity between wage and unemployment insurance variables distorts the wage coefficient (as acknowledged in Chapter 4). At the same time, however, coefficients on other variables *common* to Models 4 and 5 exhibit stability. This is conveyed in Table 1, Part A, where almost all variables carry the same sign, coefficient values are similar, and statistical significance is attached to identical variables in both models.¹ And these are key economic variables (jobs, unemployment, and dwelling starts as an indicator of building cycle and housing activity). My point then is that Model 4 is not misspecified; the all-important wage coefficient is merely distorted in Model 5, whereas other economic variables are not. Thus, I maintain that Model 4 remains the best candidate for evaluating structural stability of coefficients on traditional economic variables.

Results of Chow tests for Model 4, as well as for Models 3 and 5, are presented in Table 1, Part B. They confirm that the economic relationships have changed over time. Whether this is attributable to

¹Note: Only the circled variable is significantly different.

Table 1
ADDITIONAL TEST RESULTS

Part A: Stability of Regression Coefficients (B's) on Variable Common to Models 4 and 5 (excluding Wages)

Common Variables	Model 4		Model 5	
	Pre-1971	Post-1971	Pre-1971	Post-1971
Distance	-.596*	-.494*	-.657*	-.529*
Language	1.100*	1.071*	1.343*	1.152*
Jobs at Origin	-.019	-.027	-.009	-.015
Jobs at Destination	.138*	.073*	.164*	.042*
Unemployment (orig.)	-.341	.060	-.283	.222
Unemployment (dest.)	-.067	-.685*	-.039	-.960*
Education (orig.)	1.153*	.822*	.862*	.984*
Bus. Cycle (orig.)	-.185	.067	.035	.428
Bus. Cycle (dest.)	1.790*	.767*	1.912*	-.094*

Part B: Chow Test Results, Selected Models

Model	F ^c	F (0.01)	Conclusion
	34.69	>3.36	Reject H ₀
	47.68	>3.36	Reject H ₀
	58.59	>3.36	Reject H ₀

Part C: Evolution of Migration, Regression Coefficients and Values of Economic Variables over Estimation Periods, from Model 4

Variables	Pre-1971	Post-1971	% Change
1n Avg. M _{ij}	1.09	1.29	+18.3
1n Wage Coefficient	2.18	1.19	-45.4
1n Average Wage	4.60	4.95	+7.6
1n Jobs Coefficient	.13	.07	-44.3
1n Average Jobs	3.26	3.91	+19.9
1n Bus. Cycle Coefficient	1.79	.77	-57.2
1n Avg. Bus. Cycle	2.19	2.61	+19.2
1n Distance Coefficient	-.60	.49	+17.1
1n Avg. Distance	7.35	7.35	.0

Notes to Part A: * = statistically significant at .01 level; 1n = log transformation of regression coefficient or average value of variable to base 'e'; all variables are defined in Shaw [9]; Business cycles are proxied by residential dwelling starts; a = a printing error in Table 4.5, [9:109] which erroneously assigns the regression coefficient of -.094 at 't' value of 3.90; it should be .390.

changes in B_0 or the B_i 's has not, however, been ascertained. To do so, we require additional information. We must include dummy variables as additional regressors in our pooled function and test them for statistical significance:

$$M_{ij} = B_0 + B_1 X_t + B_2 (tX_t) + u_t \quad (2)$$

where: M_{ij} = migration from city i to j ; X_t = wage rates; tX_t = a dummy variable with $t=0$ denoting pre-1971 migration, $t=1$ denoting post-1971 migration. If B_2 were found to be statistically significant (if $H_0: B_2 = 0$ is rejected), we would infer that the slope of B_1 had changed over time, and that responsibility for the change could not be attributed to shifting B_0 .

Unfortunately, logistics of being a Visiting Professor in Vancouver and having my raw data stored in Ottawa prevent me from undertaking this additional test. Having said this, I very much doubt that changes in B_0 assume much importance. Consider Winer's point that the "importance" of a variable should not be judged in terms of its statistical significance alone, but rather in terms of the product of its coefficient times the actual evolution of its value over the estimation period (that is, the evolution of variable values affect B_0). In Table 1, Part C, we observe that when inter-CMA migration rates increased over time coefficients on economic variables dropped markedly, whereas average values of the variables experienced relatively small increases. An evaluation of products of coefficients and values of variables is therefore not likely to undermine general confirmation of my "core hypothesis".

Cost Benefit vs. Itchy Feet

Vanderkamp reviews several problems in the design and testing of the human capital model as applied to migration. To stimulate discussion, he proposes an alternative set of behavioural relationships called the "itchy feet" hypothesis. This hypothesis describes migration largely as a *selection* process whereby more ambitious, entrepreneurial risk-takers persistently enrich a destination to the extent that the pattern of migration determines the pattern of regional wage rates, rather than the other way around.

I too perceive the human capital model to have limitations. Several are pointed out in Chapter 2 of my book. I am particularly troubled by theoretical and policy implications of the "as if" clause—that all migrants behave "as if" they calculate benefits and costs with equal rigour (subject to information constraints). I make use of the human capital model in my book largely because it has many advantages over its "traditional" competitors (for example, the macro-adjustment" model), it is amenable to theorizing about several migration-related variables

(age, education), its cost-benefit underpinnings are familiar to economists, and its limitations do not hamper my application. Beyond this, I have reservations, and Vanderkamp's "itchy feet" hypothesis prompts me to sketch out a few.

An obvious limitation of the "itchy feet" hypothesis is that it neglects: (i) noxious events at place of origin, (ii) the probability that migration decision-making is likely to be a sequential process, (iii) the growing importance of joint decision-making in dual-earner families, and (iv) psychological elements that may channel cognition and thereby favour or preclude selection of particular destinations. Why are these so important? Consider a population in community i where wages, employment, real estate values, and so forth, are growing at rates comparable to the rest of the country. Assume that migration for reasons of job transfers, military assignments and retirement is excluded from consideration. In this context—one of "equilibrium"—I would expect little push-pull migratory behaviour as well as little allocation of time or effort to calculate costs-benefits to residence in alternative locations. Some migration as "consumption behaviour" might, however, be visible.

Now, imagine that some individuals in community i experience shocks of a socioeconomic nature. They might lose their jobs, encounter barriers to occupational or educational advancement, or experience marital dissolution and lose their homes as part of a divorce settlement. An entire community may experience a factory closure, thus "forcing" migration of many individuals whether they are entrepreneurial risk-takers or not. For example, a recent survey of "displaced workers" in Canada reveals that approximately 470,000 workers with three or more years of job tenure lost their jobs between 1981-84 for reasons of plant closure, an employer going out of business, or a lay-off from which workers were never recalled [11]. Following in the tradition of Wolpert [14;15], I submit that it is the existence of origin-specific *noxious events*, such as those described above, that are largely responsible for triggering migration decision-making.

Enter selectivity. Some of those affected by noxious events will be relatively more ambitious entrepreneurial risk-takers (AERs). It is these individuals who are likely to engage seriously in migration decision-making, or undertake migration itself. Among this group, different tools of choice or behavioural patterns are likely to come into play. Among AERs who are relatively well-educated and -informed (via news-media, educated friends in other locations), I would expect cost-benefit type calculations to be performed roughly in keeping with the human capital model. Call these AERs Group A. I would expect such calculations to be all the more rigorous among members of Group A were they to own assets that may have to be left behind or

sold (houses, businesses). The same applies to older AERs in Group A who ponder leaving long-established friendships. In contrast, I would expect relatively uneducated, poorly-informed AERs to make far cruder cost-benefit calculations. This applies particularly to calculations involving residence in far-away places. Call these AERs Group B. However, members of Group B might effectively mimic the more informed, accurate decisions of their more educated counterparts in Group A (that is, a demonstration effect may operate). If so, the human capital model would still apply in view of the "as if" clause.

I perceive two remaining subgroups of affected AERs. Both may be large. Some AERs may be affected relatively more by "bounded rationality" (à la Herbert Simon [12]), particularly in societies where ethnic dissimilarity prevails. Other AERs will be relatively "footloose" or have "itchy feet", particularly in relatively wealthy societies or among members of wealthy classes or castes. Call these individuals Group C and D, respectively.

Among Group C, the socio-psychology of different ethnic groups may be such that some destinations have traditionally strong preferences attached to them, whereas others may be largely ruled out. Cognition and cost-benefit calculations—rigorous as they might be—would thus be *channeled* to assess conditions in selected destinations along well-worn paths. Such behaviour may co-exist side-by-side with neglect of alternative, equally close destinations which offer far superior economic opportunities. I propose that the "barrier" which impacts on patterns of "French" versus "English" migration serves as a case in point. Migration theory is largely mute on implications of bounded rationality. This is one area where sociologists and psychologists might profitably explore migration decision-making (to respond to Ledent's point).

As for the "footloose" or "itchy feet" group (Group D), I suspect they are predominantly young, single, adventurous, and at risk of losing little should they migrate. Footloose individuals are not likely to have dependent children, assets requiring management, or established careers. They can afford to make mistakes. Time is on their side, and the penalty for making incorrect, highly incomplete cost-benefit calculations, or none at all, is small. I would hypothesize that return or repeat migration is most prevalent among members of this group, followed by members of Group B.

How does this crude scenario add up? An implication for modeling per se is that the human capital model might better be couched in a multi-stage sequential process amenable to estimation by nested logit analysis (as suggested by both Ledent and Liaw). A second implication has to do with Vanderkamp's "enriched destination areas". In my scenario, AERs who do not encounter noxious events will remain in their

origin area; so too will non-AERs who encounter noxious events. Alternatively AERs in Groups A and B would likely be attracted to and seek out the most prosperous areas. Return migration would be less prevalent among members of Group A than Group B, as the former would perceive opportunities more accurately and cost-benefit calculations would be performed more rigorously.

AERs in group C may or may not go to the most prosperous areas. Again, I refer to large numbers of migrants of French ethnic origin and traditional exclusion of destination areas, no matter how prosperous they may be. As for members of Group D, it is hard to predict why they should enrich one area versus another. They may aim for more prosperous areas, calculate wrong or not at all, and wind up in a non-prosperous area. Or, in the absence of calculations and real investment interests, they may float about, sampling both prosperous and non-prosperous areas, only to eventually return to their place of origin.

If we want to speculate that particular parts of the country become "enriched" by migration (à la Vanderkamp), I would propose that the most discernible part of that effect would be attributable (or limited) to Group A migrants—individuals whose migratory behaviour is consistent with predictions of the human capital model. The same would apply far less to Group B migrants, and perhaps not at all to Group C and D migrants. An additional enriching effect may come from recent immigrants. Immigrants are also likely to have made relatively complete cost-benefit calculations, assuming they faced noxious events in their origin country and anticipated relatively great physical and psychic costs to migration. As I argue in Chapter 3 of my book, immigrants tend to have multiplier effects on jobs and incomes, due largely to the greater wealth they bring with them—about 20 percent more than non-immigrants according to a recent Statistics Canada survey. Responding to Liaw, I continue to maintain that this variable exerts an independent effect on domestic migrants (that is, is a causal factor) rather than being merely correlated with it. Lack of a strong correlation is illustrated in my book (p. 80-82).

Geographer's Forte

Clark and Ledent raise legitimate concerns that I fail to analyze temporal stability of interregional migration as well as its spatial homogeneity or heterogeneity. Temporal instability is apparent when levels and patterns of migration among cities vary considerably year-to-year but not, on average, over longer time periods such as a decade. Temporal instability is also affected when federal immigration policies admit relatively large numbers of immigrants one year (many of whom flock to CMAs), relatively few the next year, but similar num-

bers over the duration of, say, a full five years. Spatial heterogeneity is often apparent when comparing determinants of migration among CMAs with those for larger units such as provinces, or for migration among metropolitan and non-metropolitan areas. To what extent are determinants the same? Or, alternatively, can policy implications deriving from analysis of inter-CMA migration be equally applied to migrants in other spatial contexts? Spatial heterogeneity also exists when a subgroup or cluster of CMAs consistently exhibits different patterns of gross-out, or net migration, than another cluster of CMAs. These two considerations—temporal and spatial heterogeneity—emerge as all the more important if they produce a joint, interactive influence on migration in one area more than another.

The examples above convey precisely the kinds of issues my book does not take up. As noted on page 11, and on page 25 (footnote 9), my study was to be accompanied by a companion volume on issues of a more temporal and spatial nature. These are important, and, as Clark's own work illustrates [2], they are deserving of an entire volume. Unfortunately, the proposed companion volume has been sidelined, given budget cuts at Statistics Canada. Second, as Clark himself points out, my use of census data (five year averages), precludes temporal analysis over the short run, and my focus on questions of interregional inefficiency and the "crowding out" hypothesis precludes a direct assault on problems of a more strictly temporal or spatial nature.

My analysis does not, however, entirely neglect spatial and temporal issues. I may not evaluate stability of migration from year to year but I do evaluate instability over four points in time covering three decades. I am cognizant of major "shocks" to the system over time in the form of changing fiscal policy. I seek to capture these effects by evaluating migration during specific time frames when policies were and were not in effect. And, by comparing my results with "pooled" migration data for all time periods, I am able to show that migration determinants differ considerably over time.

In addition to the above, I explicitly focus on inter-CMA migration rather than interprovincial migration because CMAs can be interpreted, spatially, as relatively homogeneous labour markets, whereas provinces cannot. As Clark acknowledges, I also introduce a language variable to capture spatial differences between largely French speaking and English speaking CMAs. This variable proxies changing spatial barriers to labour markets (for example, introduction of Bill 101) where language restrictions, "nationality" preferences, and perhaps "bounded rationality" affect spatial search behaviour and access to information and jobs. Furthermore, I disaggregate inter-CMA migration flows into two geographic regions representing eastern versus

western Canada and compare migration determinants in each "CMA cluster" to capture spatial differences. Finally, as noted in the Appendix to Chapter 4, I focus on migration among the same seventeen CMAs—all with well-established travel links—towards insuring that spatial barriers to migration did not change more in some parts of the system than others because of improved road transportation, and so forth.

Selecting Explanatory Variables

Liaw and Ledent propose use of more refined indices of: (i) cultural dissimilarity, versus my use of a dummy variable to capture French/English cultural barriers; and (ii) climatic variations, versus my use of average centimeters of snowfall. Liaw also debates my position on the weak explanatory import of destination population size (P_j). Both authors draw attention to their own research where these variables appear to perform well [5;6].

Having consulted their work, I agree with their suggestions concerning cultural dissimilarity and climate variables. The same does not apply, however, to population. Perhaps my reticence to use this variable stems from the days when geographers employed gravity-type models to analyze migration wherein explanation rested on three crude variables; distance and population of both origin and destination. In these models, it was never clear what "population size" was supposed to be capturing or representing [10]. Likewise it was virtually impossible to extract useful policy implications from such models. Was it population size per se or one of its many "components" that was accounting for variations in migration?

Today, geographers have moved far beyond the crudities of past gravity-type models, and this is fully evident in the papers by Liaw and Ledent. However, I submit that population size variables remain the geographer's nemesis. To illustrate, Liaw *et al* [5] make use of the variable as follows:

Since a larger population size usually indicates greater variety and number of relatively stable jobs, better amenities and entertainment, greater political clout, and a more prominent position in the information and communication networks, population size is used as a general index of attractiveness to avoid the necessity of incorporating too many explanatory variables, some of which are difficult to measure.

I have no doubt that population size captures "something" along with more precise variables used by the authors. The challenge is to: (i) estimate exactly what that "something" is, and (ii) establish which elements of that "something" are policy-manipulable. Is it the political

clout element; is it the stable jobs; or is it the prominent position in the network?

Further, I am uneasy when population size is included in regressions because its ambiguous nature and multicollinearity with other variables almost always boosts R^2 values to unrealistically high levels. Unfortunately, most readers continue to be dazzled by high R^2 . In the paper by Liaw and Ledent [6], which claims to account for 80-92 percent of the variation in migration, population size is typically the most statistically significant variable. Were it excluded from their regressions, I suspect R^2 values would fall by 50 percent and coefficients on remaining socioeconomic variables would change.

Multicollinearity and simultaneity problems plague most studies of migration, including my own. A sure way of minimizing such problems is to exclude population size variables and include more precise social, economic, geographic and psychological variables. The "data demanding" process of estimating simultaneous equations represents another, as suggested by Vanderkamp.

Deeper Political Issues

Clark is puzzled by my apparent reticence to place questions of allocative efficiency in a broader context. Is it sufficient to produce evidence consistent with the "crowding out" hypothesis? Or, is the onus on the social scientist to engender a wider policy debate? On the latter question, I suspect Clark and I would agree. Efforts to bridge the gap between empirical findings and the policy domain usually result in a more rounded study, more informed readers, and more explicit policy implications.

On the heels of this admission, I draw attention to two caveats. First, my book has been published in cooperation with Statistics Canada, an agency whose work is constrained to be politically neutral. This follows from the premise that Statistics Canada should serve as an objective data-reporting agency, not a socio-political commentator on existing government policy. Reflection on deeper political issues has thus been ruled out by a formality.

My second caveat has to do with *recurrent* policy debates. Might these not be served better by new empirical evidence than by a rehash of broader political issues? Consider the following scenario: An equity-efficiency debate has been raging on and off for many years in Canada. There is widespread agreement that regional disparity has persisted for a long time, that the current state of affairs probably represents an equilibrium, and that this equilibrium is partially supported by public policies that subsidize residence in disadvantaged parts of the country (as suggested by Winer). Furthermore, subsidized residence in "outposts", extreme geographical regions, and even entire

provinces cannot be divorced from implicit national security goals where population dispersal serves as an advance-warning/defence system. On top of all this, let us impose Clark's assumption that a lack of formal cooperation and integration of macro-economic policy (advocating allocative efficiency foremost) and regional policy interests (advocating preservation of lagging communities with subsidies), reflects an "implicit bargain" between contending political interests. In this kind of environment, knowledge about consequences of "implicit bargains" may have more impact than rhetorical assaults on "deaf political ears". In my study, consequences include hidden costs associated with policies that exert unintended effects on the labour market. The more we know about these costs, the more we are in a position to "shadow" opportunity costs, and query the worth of "implicit bargains".

Perhaps the best analogy here concerns the theory of comparative advantage, which has been used extensively to condemn protectionism in international trade as costly and inefficient over the long run; yet, the political economy of "food security" is such that governments of developed countries widely employ protectionism and specifically "exclude" agriculture from GATT accords because it is a "domestic issue". Put simply, agricultural production and canons of "allocative efficiency" have not fully integrated for a long time. Why? It is a political reality that rich countries do not want to be vulnerable to "food security" should international turmoil or war occur. They are prepared to subsidize inefficiencies and unviable producers to keep domestic supplies self-sufficient. The economic result: on average, producer prices in industrial market economies are about 40 percent above comparable world prices [8]. The World Bank [16] recently estimated that consumers and taxpayers in the OECD countries lose about \$104 billion per year in order to support a sector of the economy that contributes only small shares of national income and employment. In such contexts, the process of estimating costs of existing political priorities and "implicit bargains" represents a far greater contribution than a rehash of the broader debate itself.

Lest my caveats sound too much like apologetics, I now want to consider the political economy of unemployment insurance (UI) in Canada. As Winer points out, the evidence that UI has a significant influence on internal migration is now reasonably compelling. This adds support to the "crowding out" hypothesis. But, as Clark points out, part of the "deal" concerning the UI system may be that it helps hold the Canadian federation together. Can a normative framework advocating allocative efficiency in internal migration co-exist in a world with UI? My answer is yes, assuming that *aspects* of UI can be shown to be detrimental to the economy, and subsequently modified to eliminate negative affects on labour mobility.

A problem with UI in Canada is that it has assumed an important equity objective by extending benefits to economically depressed regions (since 1971). UI funds have also been put to "developmental" use to provide training and upgrade skills in regions particularly hard hit by long-duration unemployment. The cost of such programs has grown enormously, reaching approximately \$2.9 billion, or almost 30 percent of the total UI bill in 1984-5. Kesselman [4] submits that UI is now so big relative to welfare or redistributive programs that it inevitably competes with them for resources. Many writers further concur that UI was never intended to promote equity per se [3]; it is a social insurance program, designed to stabilize earnings at times of unemployment, much in keeping with private insurance principles. According to this view, geographic inequalities in unemployment-related hardship as well as poorer groups afflicted with chronic unemployment should be served by redistributive or welfare programs, separate from UI.

Several studies, including recommendations of the federal Commission of Inquiry on Unemployment Insurance (the Forget Commission), now suggest that *equity dimensions* of UI are impacting negatively on labour mobility. To illustrate, a 1985 royal commission on unemployment in Newfoundland concluded that UI entails many disincentives to work and to improve one's education. UI appears to increase reservation wages and long-duration unemployment to the extent that the work ethic is undermined; it appears to entice individuals to remain where they are, even though family incomes without UI remain consistently below the provincial average, and family incomes with UI remain below the national average; and it may not serve its intended purpose of enhancing effective job search (in view of a reduced work ethic and subsidized incomes), but may subsidize leisure. If these forces combine to prevent the mechanism of migration from "naturally" reallocating unemployed and underemployed labour to its best place of competitive advantage, then regions and nation alike experience a net economic loss.

Having said the above, results of my study barely scratch the surface of the complex issues involved. I have "shadowed", not quantified costs involved. I have implied that normative frameworks for future migration research and planning should be established, I have not charted them out. Students of migration in Canada have much to learn on this subject and will benefit by works such as Clark *et al* [2] and Winer and Gauthier [13]. Perhaps the most important step on the agenda is to secure funding for research in this direction and to establish contact points in the federal government that are, or should be, actively concerned with such problems.

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