A RAND NOTE

DOES MIGRATION INCREASE WAGE RATES?--
AN ANALYSIS OF ALTERNATIVE TECHNIQUES
FOR MEASURING WAGE GAINS TO MIGRATION

Julie DaVanzo and James R. Hosek

July 1981

N-1582-NICHD

Prepared for

The National Institute of Child Health
and Human Development, National Institutes
of Health

Rand
SANTA MONICA, CA. 90406

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PREFACE

This is a substantially revised version of a paper presented at the annual meeting of the Population Association of America in Denver, April 1980, and at the U.C.L.A. Labor Economics Workshop, May 1980.

The research reported herein has been supported by Grant No. R01-HD-10864-02 from the National Institute of Child Health and Human Development.

This paper has implications for studies of both determinants and consequences of migration, with special emphasis on methodological issues in estimating the gains to migration. A review and critique of previous studies of wage gains to migration may be found in another paper written under the same grant: John J. Antel, Returns to Migration: Literature Review and Critique, The Rand Corporation, N-1480-NICHD, October 1980.

SUMMARY

To estimate the wage gains to migration, migrants' post-migration wages should be compared with what the migrants would have earned had they not moved (their "counterfactual wages"), and not with the wages of apparently similar nonmigrants. Migrants are a selected sample and differ from nonmigrants in observable as well as unobservable ways. Since people who choose to migrate expect to improve their lot by doing so and those who stay also believe they are doing what's best for themselves, the post-migration wages of migrants may be a biased estimate of what nonmigrants would have received had they moved; likewise, the wages of nonmigrants may not be a good indication of what migrants would have received had they not moved.

This Note examines two alternative dependent variables for measuring the wage gains to migration: the wage rate at the end of the migration interval, and the difference between wage rates before and after the migration interval. We also consider two alternative methods for estimating the wage gains due to migration: a single regression on a pooled sample of migrants and nonmigrants that includes a dummy variable to distinguish between the two groups; and a regression-switching model that estimates separate regressions for migrants and nonmigrants and includes selectivity correction factors to control for certain unobservable differences between the two groups.

Our empirical analyses assess the effect of 1974-75 migration among labor market areas on 1975 wage levels and 1973-75 wage changes, using a sample of white male heads of household from the Panel Study of Income

Dynamics. The regression analysis of a pooled sample of migrants and nonmigrants, with migrants being distinguished by a migrant dummy, revealed no significant wage differences between migrants and nonmigrants when other wage determinants are controlled. This was true of both the wage-level and wage-change equations. In contrast, the regression-switching model showed no evidence of wage gain to migration in the analysis of post-migration wage levels, but an implausibly large wage gain to migration in the analysis of wage changes. Consequently, we believe that further empirical study will be required before the regression-switching model can be relied upon as an accurate means of measuring the wage gains to migration.

ACKNOWLEDGMENTS

We would like to thank John Antel and Christine Peterson for their thorough and proficient research assistance. Dennis De Tray, H. Gregg Lewis, Willard Manning, Peter Morrison, and participants in the U.C.L.A. Labor Economics Workshop made useful comments on an earlier draft. Robert Cotterman, who carefully reviewed the final draft, also contributed many helpful suggestions.

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I. INTRODUCTION

Numerous past studies have attempted to measure the monetary returns to migration. Regardless of whether the research was motivated by, or meant to be a test of, some economic, sociological, or other model, the studies generally fall into two groups at the empirical level: those based on cross-sectional data (with retrospective information on migration) and those based on panel data, which have multiple observations on an individual at different points in time. The cross-sectional studies typically analyze how migration affects post-migration levels of wage rates (or earnings or income), while panel studies often analyze changes in the wage rate (or earnings or income) over the migration interval.

In a sense, the present Note is a response to the heterogeneity of this past research. It is not our objective here to compare and critique these studies,[1] but instead to explore the extent to which different approaches, when applied to a given sample, yield similar results. In other words, we want to examine the sensitivity of estimates of the gains to migration to alternative definitions of "gains" and to alternative techniques for estimating them. To do this, we employ data on white male heads of household drawn from the Panel Study of Income Dynamics. These data offer sufficient detail to permit examination of many of the approaches embodied in existing work. We restrict our focus here to the effects on individuals' wage rates of migration that occurred between spring 1974 and spring 1975.

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[1] For a review and critique of the literature on the effects of migration on individuals' wages and incomes, see Antel (1980).

In laying the groundwork for the sensitivity analysis, another objective of our research emerges: to clarify how various empirical models purport to measure the gains to migration. This not only helps to reveal how different approaches can give seemingly contradictory results (e.g., a wage-level model showing a loss to migration and a wage-change model showing a gain), but also draws attention to some troublesome yet important methodological issues. To what extent, for instance, does a migration variable actually represent unobserved differences between migrants and nonmigrants? For example, if migrants are less "able" than nonmigrants, their wages might be lower than those of nonmigrants in all time periods; hence, they might appear to lose from migration when their post-migration wages are compared with those of nonmigrants, when in fact they improved their lot by moving. If migrants and nonmigrants differ in unobserved ways, does it make sense to specify a model in which the coefficient on a migration dummy is assumed to apply to any individual? That approach, we argue, contradicts the idea that movers move because they expect to gain by doing so, and stayers stay because they too expect to gain by doing so.

To measure the true gain to migration, the post-migration experiences of migrants should be compared with what they would have experienced had they not moved, and not with the experiences of nonmigrants.[2] The problem, of course, is that one cannot observe what would have happened had the migrants not moved. One avenue toward resolving this problem has appeared recently in the form of "regression switching" models, which are among those we discuss and apply in the

[2] This point was first made in the migration context by Lewis (1974).

sensitivity analysis. Such models help control for the fact that migrants and nonmigrants may be "selected" samples, in the sense that migrants may tend to be those who expect to gain from moving and nonmigrants those who expect to gain from staying. Neither group is randomly drawn from the population. As a result, one can argue that any attempt to estimate the wage gains to migration should take into consideration the selective nature of the migrant and nonmigrant samples; regression-switching models provide a framework for doing this.

In our empirical analyses, we examine 1975 wage levels and wage change between 1973 and 1975. The "conventional" approach, which measures wage gains to migration via a pooled sample of migrants and nonmigrants, reveals no wage gains to migration, regardless of which dependent variable is used. The regression-switching model, which separates migrants and nonmigrants and accounts for their being selected samples, also suggests no wage gains to migration in the wage-level analysis but large (too large) positive gains in the wage-change analysis.

Section II discusses these conceptual issues in more detail. Section III describes the data we use. Section IV presents our empirical results, and Sec. V our conclusions.

II. CONCEPTUAL AND ESTIMATION ISSUES

CONCEPTUAL MODEL AND ALTERNATIVE WAYS OF CHARACTERIZING GAINS TO MIGRATION

Our underlying hypothesis is that people move because they believe, ex ante, they will be better off by doing so. Stayers stay for the same reason. Both these statements are about ex ante expectations; these expectations may not be borne out ex post. We use the term "better off" here in the sense of lifetime real income or utility, i.e., overall well-being over the rest of the person's life. Conceptually, the focus of the hypothesis is not restricted to a monetary measure, such as wages, earnings, or income, or to a single post-migration time period. After discussing the hypothesis conceptually, we consider its implications for our empirical work on analyzing wage gains to migration.

To discuss alternative conceptual ways to measure gains to migration, we make five simplifying assumptions: (1) A person's lifetime can be divided into two time periods: time period 0, before migration (or nonmigration), and period 1, after migration (or nonmigration). In this simplified world people can move only once. (2) Each person's well-being can be measured in each of these time periods. (3) Migrants' (nonmigrants') expectations about what they will gain from migrating (staying), relative to the alternative not chosen, are unbiased. (4) Each individual is risk-neutral. (5) There are no costs of moving.

Let the subscripts M and N denote the alternatives of migration and nonmigration, respectively, between period 0 and 1. Let B_M and B_N measure the migrants' and nonmigrants' well-being before moving, and A_{MM}

and A_{NN} be measures of their respective well-being after moving. Let A_{MN} measure what the migrants' well-being would have been had they not moved, and A_{NM} measure what the stayers' well-being would have been had they moved.[1] A_{MN} and A_{NM} cannot actually be observed. However, from our hypothesis that both migrants and nonmigrants expect to gain by their actions and from our simplifying assumptions, we can say that $A_{MM} > A_{MN}$ (the migrant's well-being is greater if he moves than it would have been had he stayed) and $A_{NN} > A_{NM}$ (the nonmigrant is better off for staying). In fact, we contend that the comparison of each person's actual well-being in time period 1 with what his well-being would have been had he or she chosen the other alternative is the true gain to migration (nonmigration). Thus $A_{MM} - A_{MN}$, which is hypothesized to be positive, is the true gain to migration, and $A_{NN} - A_{NM}$, which is also hypothesized to be positive, is the true gain to staying. Note that the hypothesis and assumptions imply only that $A_{MM} > A_{MN}$ and $A_{NN} > A_{NM}$; there are no other necessary relationships among these variables or between any of them and B_M or B_N.

[1] The first subscript refers to what the person actually did; the second subscript refers to the alternative being considered. If the subscripts agree, the person chose the alternative being considered; if the subscripts disagree, the person chose not to pursue that alternative.

CONVENTIONAL MODELS

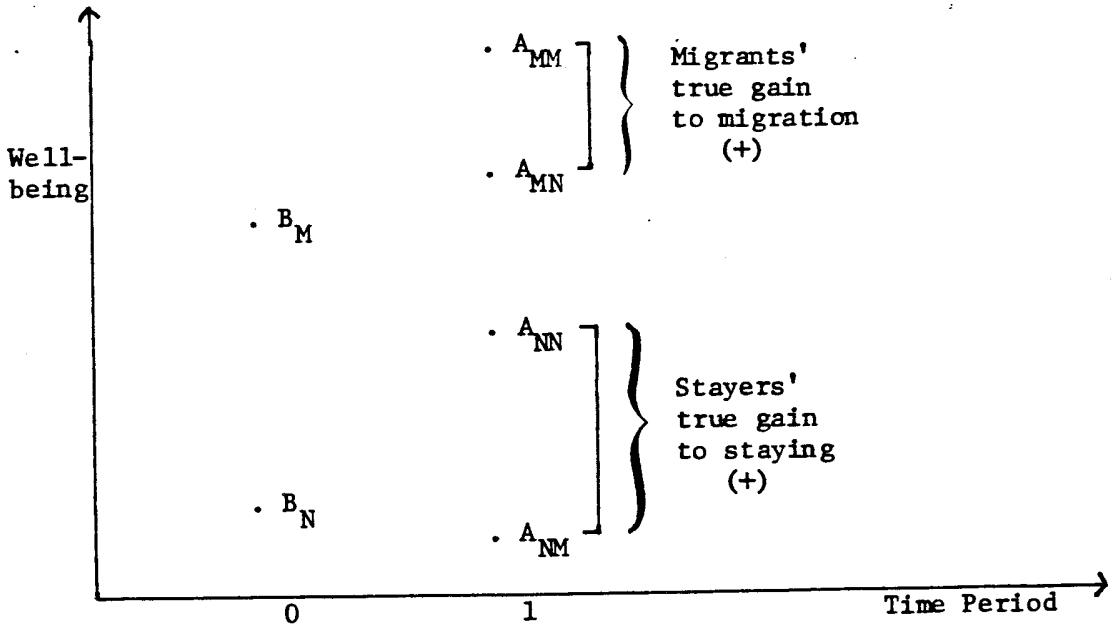
Model (1): Wage Levels

A statistical model often used to measure the gains to migration uses a pooled sample of migrants and nonmigrants and regresses an indicator of well-being (for example, income at the end of the migration interval) on a dummy variable indicating whether or not the person moved, D_M, as well as on a number of control variables, X, measuring other determinants of the dependent variable:

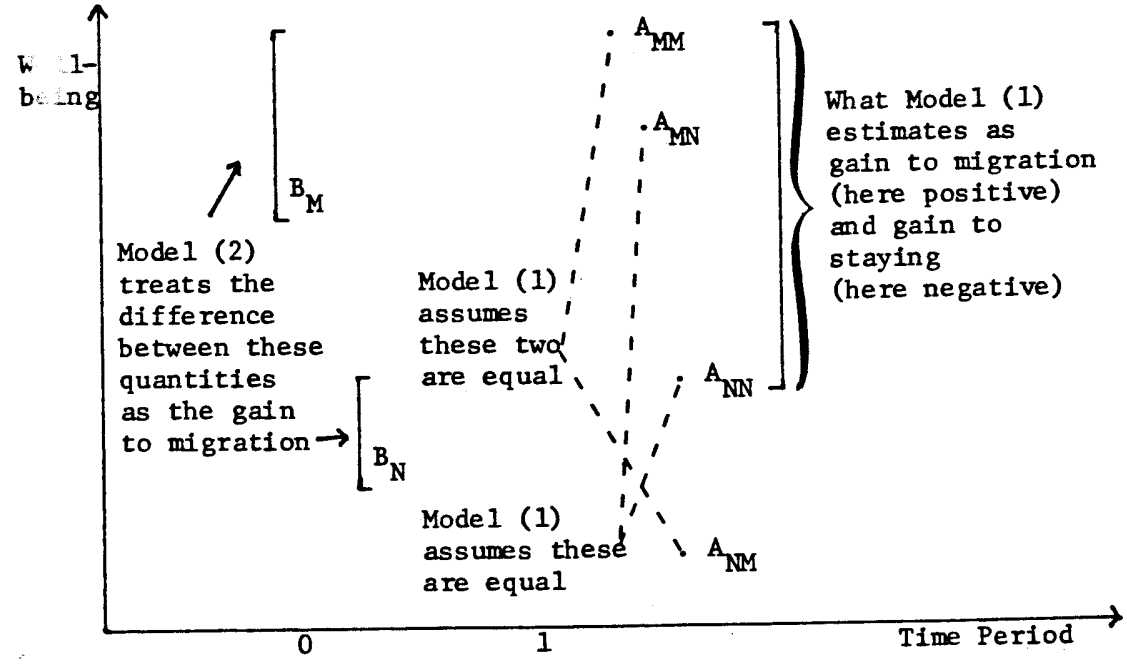
$$A = \beta X + \delta D_M \ . \qquad (1)$$

The control variables, X, are intended to make the migrants and nonmigrants similar in all respects, so that the coefficient of the migration dummy, δ, measures the gain to migration. Assume that A_{MM}, A_{MN}, A_{NN}, A_{NM}, B_M, and B_N are placed as depicted in Fig. 1. (As just noted, except for $A_{MM} > A_{MN}$ and $A_{NN} > A_{NM}$, the placement of all variables relative to one another is arbitrary.) For simplicity, assume that both groups of people in Fig. 1 have identical values of X.

Researchers who use Model (1) assume that had the migrants not moved, their well-being in period 1 would have been the same as that of stayers in period 1, i.e., $A_{MN} = A_{NN}$, and that if nonmigrants had moved, their post-migration well-being would have been the same as that of migrants, i.e., $A_{NM} = A_{MM}$. But neither of these equalities follows from the underlying conceptual model. In the cross-sectional regression Model (1), when observable differences between migrants and nonmigrants are controlled through X, $A_{MM} - A_{NN}$ is taken to be the gain to



A. Before-migration well-being and actual and counterfactual after-migration well-being. (See text for definitions of symbols.)



B. How models (1) and (2) characterize gains to migration

Fig. 2 -- ALTERNATIVE WAYS OF CHARACTERIZING GAINS TO MIGRATION

migration. This difference can be either positive (as depicted in Fig. 1) or negative (see Fig. 2). However, as shown in both Figs. 1 and 2, this estimated "gain" has no necessary relationship with a migrant's true gain to migration, $A_{MM} - A_{MN}$ (or with a nonmigrant's true gain to staying, $A_{NN} - A_{NM}$).

Furthermore, a model such as (1) assumes that if migrants gain by moving ($\delta > 0$), nonmigrants lose an identical amount by staying (or vice versa if $\delta < 0$). That is, in Model (1), $[(A_{MM} - A_{NN})|X]$ is interpreted as the return to migration, and $[(A_{NN} - A_{MM})|X]$ as the return to

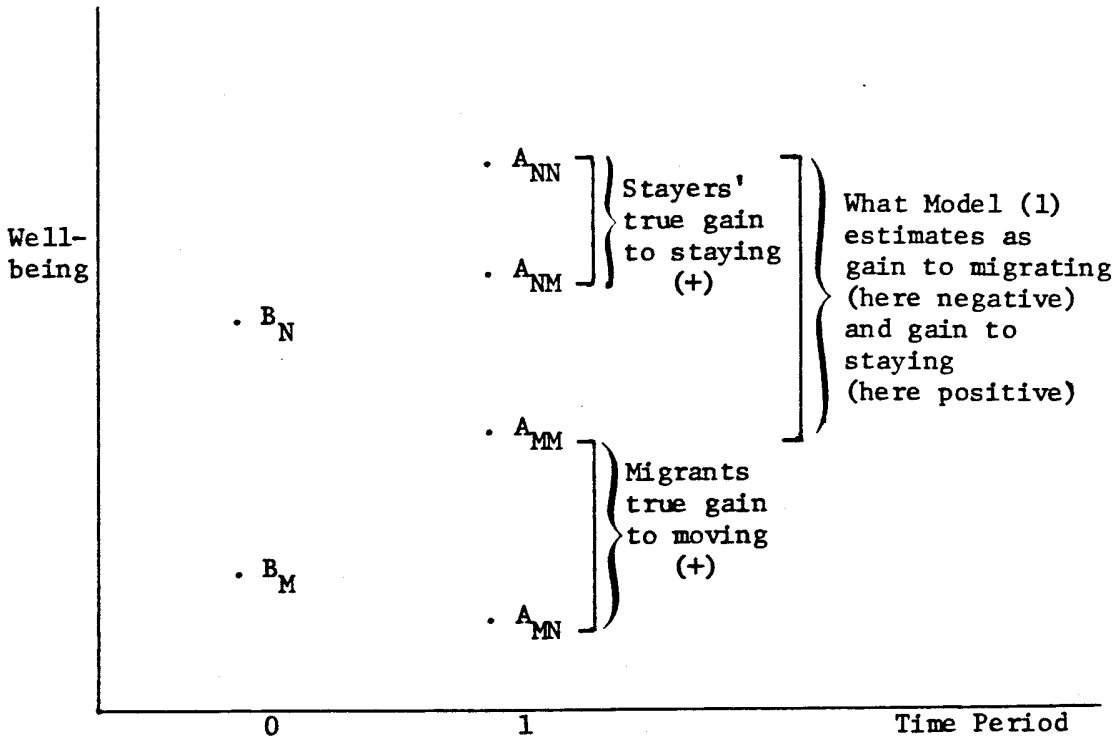


Fig. 2 -- ALTERNATIVE WAYS OF CHARACTERIZING GAINS TO MIGRATION WHERE MODEL (1) ESTIMATES A LOSS FOR MIGRANTS

staying. In Model (1), it is impossible for both migrants and nonmigrants to gain, and yet the basic assumption of our underlying hypothesis is that both groups do in fact gain. Furthermore if $\delta > 0$ and hence migrants are assumed to gain, Model (1) implies that nonmigrants could have reaped the same gain in well-being had they moved.

But, if this were the case, why didn't the nonmigrants migrate also? We contend that the unobservable differences between migrants and nonmigrants that affect the well-being they experience by migrating or staying might also affect their well-being had they made the opposite choice. Hence, it is incorrect to assume that had migrants not moved, their period 1 experiences would have been the same as those of stayers (i.e., $A_{MN} = A_{NN}$), or that had nonmigrants moved, their period 1 experiences would have been identical to those of migrants (i.e., $A_{NM} = A_{MM}$).

The problem, of course, is that one cannot observe what people would have experienced had they made the opposite choice. Recently, a technique has been devised to enable one to estimate these counterfactual values (A_{NM} and A_{MN}). We describe this technique at the end of this section and then use it in Sec. IV in an effort to come closer than is possible in Model (1) to estimating the true gains to migration for migrants and the true gains to nonmigration for stayers.

Model (2): Wage Differences

Another empirical model commonly used to estimate the "returns" to migration resembles Model (1) but uses the difference in well-being before and after migration, A - B, as the dependent variable, i.e.,

$$A - B = \beta'X + \delta'D_M + \nu' \quad . \tag{2}$$

In subtracting each person's pre-migration level of well-being from his post-migration level, Model (2) attempts to net out the influence of unobserved characteristics of each individual that do not vary over time and location. However, Model (2) still suffers from the same shortcomings as Model (1):

- Its estimate of the gain to migration, $\delta' = ([(A_{MM} - B_M) - (A_{NN} - B_N)]|X)$ bears no necessary relationship to the true gain to migration, $A_{MM} - A_{MN}$, or to the true gain to staying, $A_{NN} - A_{NM}$.
- The estimated gain (loss) to migration is assumed to be the same for everyone.[2]

[2] Some variants of Models (1) and (2) interact the migration dummy with various personal characteristics to allow estimated migration gains to vary with these characteristics. However, the gains are still assumed to be the same for each person with a given set of these characteristics. Also, some variants define the migration variable with a geographic dimension, e.g., North-North, North-South, South-North, and a base group, South-South. In this variant, "gains" to migration can differ between persons moving from South to North versus those moving from North to South. The "gains" to migration for those initially living in, say, the North, would be measured by the difference of the coefficients on the North-South and North-North dummies. As in Models (1) and (2), it would again be true that the estimated gain or loss to migration (say, from the North) would be the same for everyone initially living there.

If $\delta' > 0$, the interpretation is that migrants gain and that nonmigrants would have gained by the same amount had they migrated. But again, if this were the case, why didn't the nonmigrants move? Similarly, a negative δ' is interpreted as a loss to migration and leads one to ask why the migrants chose to move?

FROM LIFETIME WELL-BEING TO WAGES AS MEASURES OF GAINS TO MIGRATION

The discussion thus far has been about pre- or post-migration (or nonmigration) lifetime well-being, concepts that are not directly measurable. In our empirical analysis we look at individuals' wage levels and wage changes associated with migration or nonmigration.[3] These variables represent only one aspect of well-being, which of course can also be affected by such factors as climate, proximity to friends and relatives, availability of cultural facilities or recreation areas, and so forth. A person might be willing to accept a lower-wage job in a new locale that offers attractive nonpecuniary returns. As the saying goes, "Money isn't everything." In other words, people's wage rates will not necessarily be higher after moving than they would have been had they stayed (or vice-versa). Furthermore, in our empirical work we consider wages in only one pre-migration year and only one post-migration year. Conceptually, the entire lifetime following the migration decision should be considered.

[3]We view each individual as a price-taker whose gain is independent of the aggregate migration flow.

TWO-REGIME MODEL

In the preceding subsection we argued that a person's decision to migrate depends on a comparison of expected well-being given migration with expected well-being given nonmigration. Thus, a person will migrate only if migration increases his expected well-being. Put another way, the migration decision process sorts people into those who expect to gain by moving and those who expect to gain by staying. We now argue that, even though well-being cannot be measured directly, we ought to, and can, control for its effect on estimates of the individual's expected wage gain to migration (or nonmigration).

Dropping the first subscript from the earlier notation, let A_M represent a person's expected well-being should he migrate, and let A_N be his expected well-being should he not migrate.[4] Well-being depends on both wage and nonwage considerations:

$$A_M = A(W_M, Z_M) \tag{3}$$

$$A_N = A(W_N, Z_N), \tag{4}$$

where W_M and Z_M are the wage and nonwage (or "nonpecuniary") factors determining A_M, and similarly for A_N. In general, these functions will tend to differ across individuals because different persons value wage and nonpecuniary aspects of a locale differently. Also, the

[4]Recall that the first subscript indicated whether the person had already chosen to be a migrant or a nonmigrant. Thus, for example, A_{MM} represented a migrant's well-being given that he migrated and A_{MN} the migrant's well-being if he had not migrated. Now dropping the first subscript, A_M represents a person's expected well-being if he migrates and A_N represents his expected well-being if he does not migrate.

relationship for migration, $A_M = A(W_M, Z_N)$, is meant to refer to the specific location to which the person would choose to move (i.e., we are assuming that the best alternative location has been selected).

Since the migration decision process sorts people into those who expect to gain by moving and those who expect to gain by staying, migration wage rates, W_M, can be observed only for persons who chose to migrate and nonmigration wage rates, W_N, only for persons who chose not to migrate. This creates a dilemma: While nonmigrants' wage rates are a possible source of information about the wage rate a migrant might have received if he had not migrated, it is clear that nonmigrants' wage rates are generated under a condition ($A_N \geq A_M$) opposite to that for migrants ($A_M > A_N$). To control for the difference in selectivity conditions underlying observations on wage rates for migrants and for nonmigrants, we employ a two-regime regression switching model. Moreover, as described below, the model provides a means of using a regression on nonmigrant wages to infer the wage rate a migrant would have received had he chosen not to migrate.

Formally, we define a three-equation model. The first equation describes an individual's propensity to migrate; the second and third characterize the wage structures of the migration and nonmigration regimes. Beginning with the propensity-to-move equation, our preceding model implies that migration will occur if $A_M > A_N$. Although A_M and A_N are unobserved, we have assumed here that they depend, in a linear fashion, on the wage rate in each regime (W_M and W_N), variables associated with nonpecuniary factors (Z_M and Z_N), and error terms

representing the influence of unobserved factors pertinent to the individual's decision process. Thus, for person i

$$A_{Mi} = \alpha_M W_{Mi} + \gamma_M Z_{Mi} + \nu_{Mi} \tag{5}$$

$$A_{Ni} = \alpha_N W_{Ni} + \gamma_N Z_{Ni} + \nu_{Ni}. \tag{6}$$

Next, we define y, the propensity to move, as a function of the difference between A_M and A_N, which for simplicity we assume to be the following:

$$\begin{aligned} y &= A_M - A_N \\ &= \alpha_M W_{Mi} - \alpha_N W_{Ni} + \gamma_M Z_{Mi} - \gamma_N Z_{Ni} + \nu_{Mi} - \nu_{Ni} . \end{aligned} \tag{7}$$

In what follows we set $Z_M = Z_N$ [5] but allow the coefficients γ_M and γ_N to differ; their difference is being represented by the "net" effect $\gamma = \gamma_M - \gamma_N$. Also, for notational simplicity let $\varepsilon = \nu_M - \nu_N$. The propensity-to-move equation then becomes

$$y_i = \alpha_M W_{Mi} - \alpha_N W_{Ni} + \gamma Z_i + \varepsilon_i \tag{8}$$

Without loss of generality this equation can be scaled so that migration occurs when the propensity variable crosses a threshold of zero:

---

[5]We realize that differences in location amenities imply $Z_M \neq Z_N$, as we allow above in Eqs. 5 and 6. But because we do not have data on place-specific amenities, we assume $Z_M = Z_N$ as an empirical convenience.

$$A_{Mi} > A_{Ni} \quad <=> \quad y_i > 0 \quad <=> \quad \text{migration}$$

$$A_{Mi} \leq A_{Ni} \quad <=> \quad y_i \leq 0 \quad <=> \quad \text{nonmigration.}$$

The two other equations in the model, those for the wage structures of the migration and nonmigration regimes, may be written:

$$W_{Mi} = \beta_M X_{Mi} + \varepsilon_{Mi} \tag{9}$$

$$W_{Ni} = \beta_N X_{Ni} + \varepsilon_{Ni} \tag{10}$$

where X_M and X_N are vectors of exogenous variables.

Finally, the error terms ε, ε_M, and ε_N are assumed to have a trivariate normal distribution with mean vector zero and covariance matrix

$$\begin{pmatrix} 1 & \sigma_{M\varepsilon} & \sigma_{N\varepsilon} \\ & \sigma_{MM} & \sigma_{MN} \\ & & \sigma_{NN} \end{pmatrix}$$

Since the coefficients in the propensity-to-move equation can be estimated only up to a scale factor, $\sigma_{\varepsilon\varepsilon}$ is assumed to be unity; hence the "1" in the covariance matrix.

The migration wage rate, W_M, cannot be observed for the entire population and neither can W_N, the nonmigration wage rate. Instead, we observe W_M only for persons who migrate and W_N only for persons who do not migrate. Because observed wage data are conditioned on these

decisions, they will not in general lead to unbiased estimates of the wage rate structures β_M and β_N. To illustrate this, we first substitute the two wage structures into the propensity-to-move equation in order to obtain its reduced form:

$$y_i = \alpha_M\beta_M X_{Mi} - \alpha_N\beta_N X_{Ni} + \gamma Z_i + \eta_i \qquad (8')$$

where

$$\eta_i = \varepsilon_i + \alpha_M\varepsilon_M - \alpha_N\varepsilon_N.$$

The reduced-form equation relates the propensity to move, y, to explanatory variables, all of which are assumed to be exogenous, plus the combined error, denoted η. Now, following Johnson and Kotz (1972), the expected values of the wage data are

$$E(W_{Mi}|y_i > 0) = \beta_M X_{Mi} + \theta_M\lambda_{Mi} \qquad \text{for migrants (i)} \qquad (11)$$

$$E(W_{Nj}|y_j \leq 0) = \beta_N X_{Nj} + \theta_N\lambda_{Nj} \qquad \text{for nonmigrants (j)} \qquad (12)$$

where

$$\theta_M = \sigma_{\eta M}/\sigma_\eta \quad \text{and} \quad \theta_N = \sigma_{\eta N}/\sigma_\eta .$$

The λs, our selectivity control variables, are defined in Appendix A.

Like the regressors X_M and X_N, the variables λ_M and λ_N differ from person to person. In practice, a higher value of λ_M is associated with a lower probability of migrating, while a higher value of λ_N is associated with a higher probability of migrating. The coefficients θ_M

and θ_N depend on the correlations between the error term in the reduced form propensity-to-move equation (η_i) and the error term in each wage regime (ν_{Mi}, ν_{Nj}). The error $\eta = \varepsilon + \alpha_M\varepsilon_M - \alpha_N\varepsilon_N$ depends on unobserved wage and nonwage aspects that can affect a person's perceived well-being from migration versus nonmigration. θ_M and θ_N can be of either sign or zero. For example, if $\theta_M = 0$, then Eq. (11) implies (under the working assumption of trivariate normality) that data on wages of migrants can produce unbiased estimates of the migration regime structure, β_M, even though migrants are a selected sample (consisting of only those who expect to improve their well-being by migrating). A positive θ_M would result from a positive correlation between the errors η and ε_M. Controlling for observed variables, this would mean that persons more likely to migrate would tend to have a higher wage rate in the migration regime than would be predicted solely on the basis of X_M. Since λ_M is defined to be positive, if θ_M were positive then the term $\theta_M\lambda_M$ would of course be positive. It would follow that the mean wage of a migrant would exceed (by $\theta_M\lambda_M$) the mean wage in the migration regime of a person having the same X_M but drawn at random.

Following the procedure outlined by Heckman (1979), we estimate conditional wage equations for samples of actual migrants and nonmigrants. This involves using a probit propensity-to-move equation to predict λ_M and λ_N for each person[6] and then using these λs as

[6] The probit regression we have used is presented and described in App. B.

explanatory variables in separate wage equations for migrants and nonmigrants, yielding consistent estimates of the structural parameters, β_M and β_N, and the selectivity coefficients, θ_M and θ_N. Moreover, if there were a selectivity effect (i.e., θ_M and θ_N are not zero) but the conventional model (1) or (2) were used, the coefficient estimates of all coefficients would be inconsistent.

For our purposes, the final step is to use these consistent estimates to predict the wage gains to migration. For migrant i, the wage gain equals the expected wage in the migration regime less that in the nonmigration regime, both conditional on the fact that the person is migrant. Even though a migrant's expected wage in the nonmigration regime is not observed, it can be calculated by means of the consistent estimates of β_N, θ_N, and λ_M:

$$E(W_{Ni}|y_i > 0) = \beta_N X_{Ni} + \theta_N \lambda_{Mi} \tag{13}$$

where X_{Ni} represents the person's characteristics in the nonmigration regime. Using the conditional wage expressions, a migrant's expected wage gain from migration equals

$$\begin{aligned} E(W_{Mi}|y_i > 0) - E(W_{Ni}|y_i > 0) &= (\beta_M X_{Mi} + \theta_M \lambda_{Mi}) - (\beta_N X_{Ni} + \theta_N \lambda_{Mi}) \\ &= (\beta_M X_{Mi} - \beta_N X_{Ni}) + (\theta_M - \theta_N)\lambda_{Mi}. \end{aligned} \tag{14}$$

This becomes

$$(\beta_M - \beta_N)X_i + (\theta_M - \theta_N)\lambda_{Mi}) \qquad (14')$$

if the person's explanatory variables are not affected by whether he migrates or not, i.e., if $X_{Mi} = X_{Ni} \equiv X_i$. Note that Eq. (14) compares the migrant's predicted wage in the migration regime with his predicted counterfactual wage in the nonmigration regime. It does not compare a migrant with a nonmigrant.

The first term on the right-hand side of Eq. (14'), $(\beta_M - \beta_N)X_i$, is the predicted wage gain to migration for a person drawn at random with characteristics X_i. Because $\beta_M X_i$ represents the unconditional expected wage in the migration regime and $\beta_N X_i$ represents the unconditional expected wage in the nonmigration regime, the difference $(\beta_M - \beta_N)X_i$ equals the expected gain (or loss) to migration for a person drawn at random--i.e., not conditional on the person having chosen to migrate (or not migrate).

At first glance, it may appear that an empirical estimate of the difference $(\beta_M - \beta_N)X_i$ could be obtained by simply estimating separate wage regressions for migrants and nonmigrants without the λ variables, but this is not the case. In the presence of selectivity (recall $A_M > A_N$ for migrants and $A_N \leq A_M$ for nonmigrants), estimates of β_M and β_N may be biased unless the effect of selectivity is controlled. The regression switching model controls for selectivity via the λ_M and λ_N variables and yields consistent estimates of β_M and β_N. In contrast,

the conventional model of wage gains to migration, described earlier, can allow the coefficients for migrants to differ from those of nonmigrants (by interacting the migration dummy D_M with the explanatory variables, X_i), but does not control for selectivity. Hence, estimates of β_M and β_N from that model may be biased. Stated differently, our model enables the analyst to examine whether the theoretically expected presence of selectivity bias in estimates of the wage gains to migration makes any "real" empirical difference in the estimates.

The second term on the right-hand side of Eq. (14'), $(\theta_M - \theta_N)\lambda_{Mi}$, represents what we term the "sorting gains" to migration. Recall that λ_{Mi} is positive and θ_M and θ_N can be of either sign. Suppose, for example, that persons who were more likely to migrate, given X and Z, tended to have higher wage rates in the migration regime and lower wage rates in the nonmigration regime. Then we might expect θ_M to be positive and θ_N to be negative, so the difference $\theta_M - \theta_N$ would be positive. In this case, persons who migrate could expect positive sorting gains, for we would be comparing a regime (the migration regime) where their wage tends to be high, given their characteristics, to a wage regime (the nonmigration regime) where their wage tends to be low.

Moreover, if positive sorting gains exist for migrants, they would also exist for nonmigrants. This seeming paradox actually represents a potential strength of the two-regime regression switching model, for such mutual gains to migration and to nonmigration cannot exist in the conventional model. The statement follows from the fact that the expected wage gain for nonmigrant j equals

$$E(W_{Nj} | y_j \leq 0) - E(W_{Mj} | y_j \leq 0) = (\beta_N - \beta_M)X_j + (\theta_N - \theta_M)\lambda_{Ni} . \quad (15)$$

Since λ_{Nj} is negative by definition, the term $(\theta_N - \theta_M)\lambda_{Nj}$ must be positive if migrants experience sorting gains, i.e., if $\theta_M - \theta_N > 0$. Following the example above, nonmigrants would tend to be composed of persons less likely to move and more likely to have a high wage in the nonmigration regime and a low wage in the migration regime. Of course, other examples are possible, e.g., θ_M and θ_N could both be positive and θ_M might be larger than θ_N, which again would mean $\theta_M - \theta_N > 0$.

CONVENTIONAL VS. TWO-REGIME MODEL

We believe we have shown that serious conceptual problems exist in the conventional model for measuring wage gains to migration. As explained above, these problems take the form of inappropriate comparisons, namely, migrants being compared to nonmigrants. The conceptual framework we have presented is a step toward overcoming these problems.

There are two main advantages of the two-regime model for estimating wage gains to migration (or nonmigration): (1) It removes the constraint inherent in the conventional model that if migrants record wage gains to migration, then nonmigrants must record wage losses to nonmigration. (2) The two-regime model offers a natural framework for integrating the analysis of wage gains to migration with the notion that migration decisions depend on well-being in general, and not just on wage gains.[7]

[7]However, the results for the regression-switching model

A deficiency of the two-regime model is its lack of geographic specificity. In a world of numerous locations and alternative destinations, one ideally desires a multiple-regime model. A similar criticism--lack of geographic specificity--has also been lodged against the conventional model (see Antel's (1980) survey). A practical approach to the problem entails the use of location-specific control variables embedded within a regression structure specified for a broad geographic area, e.g., a region or nation.[8]

Another and perhaps more serious question concerns what causes the migration and nonmigration wage regimes to differ. We hypothesize that wage regime differences could derive from differences in migrants' and nonmigrants' information about the local labor market, and from differences in employers' information about newly arrived workers versus resident workers. Migrants will tend to have less information about local employment conditions and about the terms of work (wage, hours, fringes, overtime, vacation, health, pension, and so on) offered by different employers. Employers will have little information about migrants apart from those who are intrafirm transfers. It is also possible that persons who quit and migrate may have greater information

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presented above depend on the assumption of trivariate normally distributed errors. While the basic message would be the same, a different distributional assumption would lead to different expressions for the expected wage gains to migration and nonmigration and to a different estimation procedure. In the present paper we accept the normality assumption without test; our work is exploratory, and wage data are known to be approximately log normal. (The natural logarithm of the wage rate is the dependent variable in our empirical work.) Future work should test the appropriateness of the distributional assumption.

[8]We adopted this approach, utilizing region dummies and adjusting wages for cost of living. The region dummies were insignificant and are not reported in this paper. Also, cost of living adjustments caused only slight changes in regression results (see Appendix D).

about a particular destination than persons who are laid off (i.e., leave involuntarily) and migrate. Similarly, job leavers and job losers may possess different information about their earning prospects in their current market; job leavers, whether they migrate or not, seem more likely to have an alternative job in mind, presumably a higher-paying job. Because of these suspected differences in job information across nonchangers, job leavers, and job losers, we include in our migration and nonmigration wage regimes variables that indicate the reason for job changes (if any) that occurred during the migration decision interval.

An additional implication of the notion of differential information between migrants and nonmigrants is that wage regime structural differences should diminish as the number of years after migration increases. That is, the migration and nonmigration wage regimes may differ most sharply the first year after the migration decision interval, and then the differences may diminish as the migrants become more knowledgeable of the local market. We do not test this implication, but it deserves consideration in future work. Instead, we focus only on the first year after the migration decision interval, when the differences may be greatest, using the idea of differential information to justify separate wage structures for the migration and nonmigration regimes.

Empirically, the migration and nonmigration wage structures could turn out to be identical. In contrast, in the conventional model the wage structures for migrants and nonmigrants cannot be identical, for wage gains to migration are only possible if at least the intercepts differ between the two groups.

The two-regime model, however, permits wage gains to migration (and to nonmigration) even when the structures of the migration and nonmigration regimes are identical (i.e., even when $\beta_M = \beta_N$). These statements assume observationally equivalent persons are being compared. Thus even if migrant i and nonmigrant j have identical observed characteristics, $X_i = X_j$, and if the wage structures in the two regimes are identical, $\beta_M = \beta_N$, a migrant's wage gains to migration would be $(\theta_M - \theta_N)\lambda_{Mi}$ and a nonmigrant's wage gains to nonmigration would be $(\theta_N - \theta_M)\lambda_{Ni}$.

III. DATA, SAMPLE, WAGE MEASURES

Our estimates of the wage gains to migration derive from data from the Panel Study of Income Dynamics. In this paper we consider a sample of white male heads of household. Because we study wage rates after migration as well as wage-rate changes from before to after migration, the sample includes only those observations reporting earnings in the years preceding and following the migration decision interval. In addition we exclude from our sample men who were self-employed, students, retired, in the armed forces, or on their first job in either of these years.[1] Thus the sample contains persons who are active in the labor force in both those years, and because of this it is likely that many of the observed moves in the sample are "job related." Persons who work intermittently might not appear in the sample for want of earnings in one or both years, and of course the sample selection criteria exclude nonhousehold heads, such as young men still living with their parents, who may be actively engaged in job search within their local labor market and who ultimately may move elsewhere to take (or seek) a job.

Although other years could have equally well been selected, we have chosen the "migration decision interval" to be that between the surveys of 1974 and 1975. Our geographic units are Standard Metropolitan Statistical Areas (SMSAs) and nonmetropolitan State Economic Areas

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[1]These exclusions were made in an effort to achieve sample homogeneity with respect to the context of migration decisionmaking. Preliminary analysis was also undertaken with a broader set of sample criteria (i.e., including self-employed, students, retired, in the armed forces, or on first job). Although some differences appeared, by and large the narrow and broad samples led to the same results.

(SEAs). If the individual moves across the boundary of one of these areas between the interview in Spring 1974 and the next interview a year later, we record a move. Of the 1259 men in our sample, 1204 are nonmigrants and 55--or 4.4 percent--are migrants. To conserve on expense, we use a random sample of half of the nonmigrants in our empirical analysis; however, we always use the complete sample of migrants.[2] When we run separate regressions on migrants, the reader should keep in mind that our sample of migrants is quite small.

Defining pre-migration and post-migration wage rates is not as easy as one might think. This is because the PSID wage rate measure is the ratio of annual earnings during the calendar year before an interview divided by annual hours of work during that same year. Because our measures of wage rates refer to calendar years, they do not correspond neatly to the spring-to-spring interval over which migration is defined. We illustrate this problem in Fig. 3, which displays a time line showing calendar years and dates of the survey interviews, which took place each

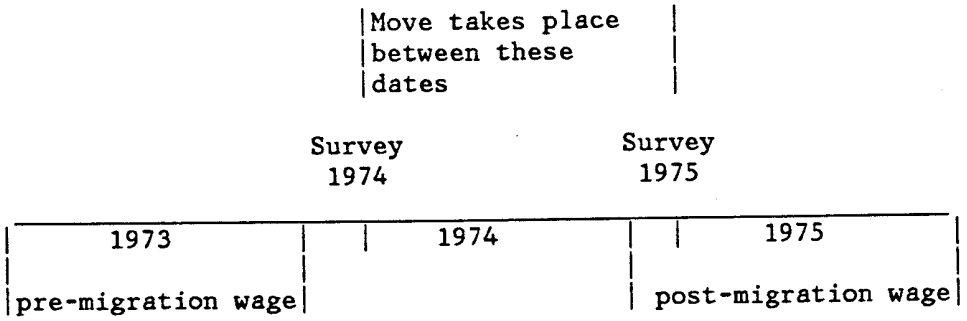

Fig. 3 -- TIME LINE OF CALENDAR YEARS AND SURVEY DATES

[2]Use of the half-sample of nonmigrants and full sample of migrants would, in general, cause inconsistent estimates of the probit equation describing propensity-to-move. However, we obtain consistent estimates by treating the sample as "choice-based" and use a suitably modified probit routine. See Hosek [1979].

spring. We measure the migration decision interval from the survey date in 1974 to the survey date in 1975, since it is for those dates that the PSID records the locations of 1974 and 1975 residence. To measure the pre-migration wage, we use the 1973 wage rate, which derives from calendar 1973 earnings data. For the post-migration wage we use the 1975 wage rate, which derives from calendar 1975 earnings data. The 1973 earnings data clearly predate the migration decision interval, but the 1975 earnings data overlap the interval somewhat. In particular, if the male head moved in the 1975 portion of the migration interval, then some of his 1975 earnings would have been received before he moved. Ideally, we would have preferred pure post-migration earnings, but we expect the effect of this possible overlap to be small because most interviews occurred within the first four months of the year and because, in order for a move to be recorded, it must have occurred before the 1975 interview. The 1973 and 1975 wage rates used in our analysis have been adjusted for areal cost-of-living differences (using the indexes appropriate for the areas where the person lived at the time of the survey in each of those years). However, as can be seen in Appendix D, our main results are the same regardless of whether we adjust wage rates for cost of living.

IV. EMPIRICAL RESULTS

Our empirical results allow us to compare the conclusions that would be reached by applying each of the several approaches described above to an identical data set. Table 1 represents regressions based on our entire sample, and includes a dummy to distinguish between migrants and nonmigrants. In the first half of Table 1, the dependent variable is the natural logarithm of the natural logarithm of the after-migration (1975) real wage. These regressions correspond to Model (1) described in Sec. II. In the second half of Table 1, the dependent variable is the difference in logarithms of real wage rates before (1973) and after (1975) the migration interval. These regressions correspond to Model (2) described in Sec. II. In Table 2 we present estimates of post-migration wage-level equations estimated separately for migrants and nonmigrants and include the selectivity bias correction factors, λ_M and λ_N, discussed in Sec. II. Table 3 presents an analogous regression-switching model that uses the after-before wage difference as the dependent variable. The remainder of this section then explores some implications of the results presented in Table 2 and 3.

CONVENTIONAL MODEL OF WAGE GAINS TO MIGRATION

Wage Level

Table 1 presents regressions on our sample of 657 white male heads of household. The dependent variable in Eqs. (L1)-(L3) is the natural logarithm of the after-migration (1975) "real" wage rate (i.e., adjusted for cost of living). Equation (L1) includes only a dummy variable

Table 1

REGRESSIONS ON AFTER-MIGRATION WAGE RATES AND AFTER-BEFORE WAGE RATE DIFFERENCES FOR ENTIRE SAMPLE
(Sample = All Migrant White Male Heads of Household with Positive Wage Rates in 1973 and 1975 and Half of Such Nonmigrants; n = 657)

	Dependent Variable = Ln (1975 Wage)			Dependent Variable = Ln (1975 Wage) - Ln (1973 Wage)		
Explanatory Variables	(L1)	(L2)	(L3)	(D1)	(D2)	(D3)
Migrated 1974-1975(D)	-.130 (-1.76)	-.0361 (-0.57)	.0228 (0.35)	.0421 (0.90)	.0280 (0.59)	.0152 (0.30)
Education		.0911 (14.4)	.0912 (14.6)		.00284 (0.60)	.00124 (0.25)
Work Experience		.0481 (8.55)	.0381 (6.52)		-.00265 (-0.62)	-.00185 (-0.41)
Experience Squared		-.000726 (-6.29)	-.000601 (-5.24)		.000019 (0.22)	-.0000007 (-0.01)
Job Tenure in 1974			.0109 (2.81)			.00313 (1.03)
Quit Job 1974-1975(D)			-.109 (-6.49)			.140 (2.47)
Quit x Tenure 1974			.0271 (2.19)			-.00694 (-0.72)
Laid Off 1974-1975(D)			-.161 (-2.08)			-.0388 (-0.64)
Laid Off x Tenure 1974			.0132 (0.98)			-.0221 (-2.12)
Union Membership 1975(D)			.187 (5.22)			-.00179 (-0.06)
Constant	1.80 (83.8)	.0737 (0.71)	.0774 (0.72)	.178 (13.1)	.188 (2.39)	.178 (2.12)
R^2	.005	.308	.362	.001	.007	.038

NOTE: t-statistics are presented in parentheses under coefficients. D=dummy variable. Variables are defined in Appendix Table C.1.

indicating whether the person migrated between the 1974 and 1975 survey interviews, and a constant term. The significantly negative coefficient on the migration dummy in Eq. (L1) indicates that, when no other personal characteristics are controlled, males who migrated between 1974 and 1975 earned 12 percent[1] less in 1975 than those who did not migrate between 1974 and 1975.

Equation (L2) adds explanatory variables measuring the person's education and work experience.[2] When these personal characteristics are controlled, the coefficient of the migrant dummy is considerably smaller in absolute value and is no longer significantly different from zero. The coefficients of education and work experience in Eq. (L2) are consistent with the literature on determinants of wage rates. (The coefficient of education is positive and highly significant. The relationship between wage rate and overall work experience is quadratic and peaks at about 33 years of work experience.) Evidently, the lower wage of migrants in Eq. (L1) is due to their smaller average amount of work experience (14.6 years for migrants vs. 21.7 years for nonmigrants).

In Eq. (L3) we add to specification (L2) dummy variables indicating whether the person was in a union in 1975, whether he changed employers between the dates of the 1974 and 1975 surveys, and if so, whether he

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[1]When the dependent variable is a logarithm, ln Y, and the explanatory variable is a dummy, D, i.e., $\ln Y = \alpha + \beta D$, the percentage change in Y due to the presence of characteristic D is $e^{\beta}-1$.

[2]In preliminary work we also entered dummy variables indicating region of residence after the migration decision interval. These never proved significant and so were omitted from subsequent regressions.

quit or was laid off.[3] We also include, alone and in interaction with the quit and layoff dummies, a variable indicating the number of years the person had been on the job he held at the time of the 1974 survey, measured as of the date of the 1974 survey. For people who did not change jobs between 1974 and 1975, this variable is one year less than their job tenure in 1975. For others, the job tenure in 1974 variable measures how much job experience they gave up when they changed jobs. For those who quit their jobs or were laid off, the total effect of tenure on wages is the sum of the coefficient of tenure alone and that of the Quit x Tenure or Laid Off x Tenure interaction. Similarly, the effect of job change is the sum of the coefficient of the dummy alone and the tenure interaction effect.

When these job-change, tenure, and union variables are included, the coefficient of the migration dummy remains insignificant.

The job-tenure coefficient, by itself, measures the effect on the wage level of an additional year of job tenure for those who did not change employer between 1974 and 1975. The coefficient is positive and significant and indicates a 1.1 percent wage increment for each year spent with that employer.

For job changers with low values of job tenure in 1974, persons who quit or were laid off their jobs between 1974 and 1975 earn less than otherwise similar individuals who did not change jobs. However, both effects are attenuated (or even offset) the more tenure the person had before leaving his 1974 job. For example, men who had had their 1974 job for 5 years and then quit earned 2 percent more in 1975 than otherwise similar individuals who did not change employer.

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[3]We recognize that job-change decisions may be made simultaneously with migration decisions, but treating the former as endogenous was outside the scope of this research. Treating both the migration and job-change variables as endogenous would require a multiple-regime model.

Men who belonged to a union in 1975 earned a substantial wage premium--21 percent. The union coefficient, which is highly significant, lies above Lewis' (1963) estimate (based on industry-level data) of 10-15 percent.[4]

Wage Change

In Eqs. (D1)-(D3) the dependent variable is the difference between the logarithm of the 1975 wage rate and the logarithm of the 1973 wage rate--a measure of the relative change in wages before and after the migration interval, or, alternatively, of the rate of growth in the wage rate between 1973 and 1975. Equation (D1), like Eq. (L1), includes only the migration dummy and the constant. The insignificant coefficient of the migration dummy indicates that the 1973-75 wage growth of migrants is not significantly different from the 1973-75 wage growth of nonmigrants. The conclusion remains the same when personal and job-related characteristics are controlled, in Eqs. (D2) and (D3).

Many of the variables that explained variation in wage levels in Eqs. (L2) and (L3) have no effect in Eqs. (D2) and (D3) on relative wage changes. Consequently R^2s are much lower in Eqs. (D1)-(D3) than in (L1)-(L3). These explanatory variables apparently affect 1973 and 1975 wage levels nearly equivalently and their effect nets out when the two wages are differenced.[5] Two exceptions to this are the Quit Job and

[4]The size of the union wage differential remains an open question. For instance, Duncan and Stafford (1980), studying union wage differentials for blue collar workers, found an effect of 20.4 percent for males. However, this falls to 6.4 percent when they adjust for differences in working conditions between union and nonunion employers. Duncan and Stafford also report that wage differential estimates in other studies (Rosen (1970), Stafford (1968), Throop (1968), and Weiss (1966)) generally exceed Lewis'.

[5]The migrant dummy coefficient in Eq. (L1) suggests that, when we

Laid Off x Tenure coefficients. Persons who quit their jobs between 1974 and 1975 experienced a 15 percent greater wage change than otherwise similar men who did not change jobs. These are the same men whose 1975 wage levels were about 10 percent lower than of otherwise similar men. These two facts together imply that these men earned very low wages in 1973, and that may indeed be the reason why they quit their jobs.[6]

Men who were laid off (and went on to take a different job) between 1974 and 1975 experienced lower rates of wage growth between 1973 and 1975 than men who did not change jobs. The difference is greater the longer they had had their 1974 jobs. For example, the wages of men who had 5 years of tenure when they were laid off grew at a rate 14 percent less than those of otherwise similar men who stayed on their jobs. Apparently men who are laid off find it difficult to transfer their job-specific capital to another employer.

As noted in Sec. II, however, none of the estimates thus far can tell us anything about the wage gains to migration or to nonmigration

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do not control for socioeconomic characteristics, 1974-75 migrants earn less in 1975 than nonmigrants. However, Eq. (D1) implies that migrants' changes in wages from before to after migration are no less than those of nonmigrants over the same time interval. These two results together imply that migrants were also earning less than nonmigrants before moving. Indeed, a regression, not shown here, of the 1973 wage rate on the 1974-75 migration dummy confirms this. That equation shows that (when no other variables are controlled) 1974-75 migrants earned even less (16 percent) than nonmigrants before moving than they did afterwards. Thus it appears that the positions on the before-and-after wage rates for migrants and nonmigrants, when other variables are not controlled, are much like points B_M, A_{MM}, B_N, and A_{NN} shown above in Fig. 2.

[6]Bartel's (1979) results suggest that persons with lower wage rates are more likely to quit and that the "strength" of the negative relationship between quit and wage rate comes from nonmigrants rather than migrants. She finds that wage rate exerts a negative effect on both the probability of migration-and-quit and on the probability of nonmigration-and-quit. The effect is significant in the latter but not the former.

because they tell us nothing about what migrants would have experienced had they not moved or what nonmigrants would have experienced had they moved, i.e., their counterfactual wage rates. We turn now to the technique that enables us to estimate these counterfactual values.

REGRESSION RESULTS FOR THE SELECTIVITY MODEL

Wage Level

In columns (M-L1) and (N-L1) in Table 2 we display the coefficients from regressions estimating the logarithm of the 1975 real wage for separate samples of 1974-75 migrants and nonmigrants, respectively, using a specification otherwise like (L3) in Table 1. This stratification of the data is, of course, nearly tantamount to interacting the migration dummy with the other explanatory variables in wage-level Eq. (L3) in Table 1.[7] Equations (M-L1) and (N-L1) allow coefficients to differ for migrants and nonmigrants and implicitly treat the migration decision as if it were exogenous to the wage determination process. In Eqs. (M-L2) and (N-L2) we include the selectivity correction variables (λ_M and λ_N) to allow for the fact that individuals are not randomly distributed between the migration and nonmigration regimes but rather are hypothesized to select the regime in which they think they will fare best. Furthermore, as noted earlier, only when the selectivity coefficients are included are the coefficient estimates consistent. Therefore we turn to Eqs. (M-L2) and (N-L2), which include the selectivity correction factors, λ_M and λ_N , to discuss the

[7]We say "nearly" because when the data we stratified by migration/nonmigration, the standard error of the estimate can be different in the migrant's regression from what it is in the nonmigrant's regression. In contrast, when a single regression, full-dummy interaction specification is used, the standard error is constrained to be the same for migrants as for nonmigrants.

Table 2

REGRESSIONS ON AFTER-MIGRATION WAGE RATES FOR MIGRANTS AND NONMIGRANTS SEPARATELY

(Dependent Variable = Ln 1975 Real Wage)

	MIGRANTS (n=55)				NONMIGRANTS (n=602)			
Explanatory Variables	(M-L1)	(M-L2)	(M-L3)	(M-L4)	(N-L1)	(N-L2)	(N-L3)	(N-L4)
λ_M, λ_N		.0218 (0.12)	.157 (1.51)	.239 (2.09)		.112 (0.39)	.368 (2.06)	.763 (3.89)
Education	.0594 (3.04)	.0595 (3.01)	.0578 (2.94)		.0945 (14.3)	.0945 (14.3)	.0941 (14.2)	
Work Experience	.0634 (3.36)	.0628 (3.19)	.0715 (3.74)		.0364 (5.87)	.0359 (5.66)	.0421 (6.89)	
Experience Squared	-.00137 (-3.09)	-.00136 (-3.02)	-.00133 (-2.99)		-.000559 (-4.64)	-.000549 (-4.50)	-.000626 (-5.13)	
Job Tenure in 1974	.0392 (1.86)	.0392 (1.73)			.0107 (2.70)	.0106 (2.66)		
Quit Job 1974-1975	-.239 (-1.35)	-.216 (-0.81)			-.0217 (-0.26)	.0126 (0.10)		
Quit X Tenure 1974	.0219 (0.45)	.0198 (0.38)			.0203 (1.54)	.0176 (1.18)		
Laid Off 1974-1975	.0551 (0.24)	.0616 (0.26)			-.179 (-2.14)	-.175 (-2.06)		
Laid Off X Tenure 1974	-.0208 (-0.71)	-.0191 (-0.57)			.00349 (0.20)	.00663 (0.35)		
Union Membership 1975	.263 (1.76)	.263 (1.83)			.182 (4.93)	.178 (4.57)		
Constant	.316 (0.95)	.273 (0.55)	.0539 (0.16)	1.273 (6.42)	.0462 (0.41)	.0599 (0.50)	.133 (1.16)	1.853 (71.7)
R^2	.505	.505	.359	.077	.361	.362	.313	.025

Note: t-statistics are presented in parentheses beneath coefficients.
Variables are defined in Appendix Table C.1.

difference between the migrant and nonmigrant coefficients. Following that we discuss the coefficients of λ_M and λ_N per se.

For the education and work experience coefficients, two main differences between the migrant and nonmigrant equations emerge in Eqs. (M-L2) and (N-L2): (1) Education has a larger and more significant effect on the wages of nonmigrants than on those of migrants;[8] and (2) the work experience profile for migrants is steeper and peaks earlier (23 years) than that for nonmigrants (which peaks at 32.7 years of work experience).

We can use the separate equations for migrants and nonmigrants to determine whether the job-change variables have a differential effect on the wages of migrants and nonmigrants. Migrants who quit their jobs earn substantially less than otherwise similar migrants who did not change employer (job transfers), though the difference becomes smaller the more job tenure the migrant had before quitting the job. However, neither the Quit effect nor the Quit x Tenure interaction effect is significant. Also, the earnings of migrants who were laid off are not significantly different from those of migrants who did not change employers.

The job-change and tenure coefficients for nonmigrants (Eq. (N-L2)) are considerably different from the corresponding ones just discussed for migrants. The coefficient on job tenure measures the increment in

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[8]The t-statistics for the selectivity-corrected models (Tables 2 and Table 3) are only approximate, being based on standard errors computed from a moment matrix using the instrument for λ_M or λ_N rather than the actual value. The actual values of λ_M and λ_N are unobserved. Correct standard errors could be computed from a maximum likelihood estimation of Eqs. (8)-(10) above.

wages associated with an additional year on the same job for those who did not change employers. For nonmigrants each additional year of job tenure is associated with a 1.1 percent increase in wages, compared with 3.9 percent for migrants (transfers). Unlike migrants, nonmigrants who quit their jobs do not earn less than those who kept the same job, but nonmigrants who were laid off earn significantly less than otherwise similar nonmigrants who did not change jobs between 1974 and 1975.

Summing up the effects of the job-change variables, we find positive and significant effects of remaining with the same employer for both migrants and nonmigrants; moreover, people who migrate but do not change jobs seem to fare better than do nonmigrants who remain with the same employer, especially when they have worked for that employer for a number of years. Among migrants, none of the Quit or Laid-off coefficients are significant; among nonmigrants only the direct effect of layoff is significant (and negative).

Both migrants and nonmigrants earn a statistically significant wage premium for belonging to a union in 1975. The premium is larger for migrants (30 percent) than for nonmigrants (19 percent).

Turning to the selectivity effects, the coefficients of λ_M and λ_N are small and insignificant in Eqs. (M-L2) and (N-L2), indicating no selectivity into the migration or nonmigration regimes. That is, persons more likely to migrate do not have higher wages in the migration regime. The same is true in the nonmigration regime.[9] In addition,

[9] This contrasts with Nakosteen and Zimmer (1980) who report weak evidence of negative selectivity of migrants, but find positive selectivity of nonmigrants. Nakosteen and Zimmer's data, explanatory variables, and time frame differ from ours. They combine men and women, blacks and whites; their dependent variable is the log of annual earnings in 1973; and their explanatory variables are only an industry change dummy and a self-employed dummy, plus the selectivity variables.

the inclusion of λ_M and λ_N in Eqs. (M-L2) and (N-L2) causes little change in the regression coefficients of (M-L2) and (N-L2) with (M-L1) and (N-L1). Thus, when education, experience, union, and reason for job change are controlled, our evidence suggests that there is virtually no empirical ground to be gained by using the two-regime approach rather than Model (1) in the analysis of post-migration wage levels.

In columns (M-L3) and (M-L4), and (N-L3) and (N-L4), we reverse the experiment of Table 1, deleting the variables we progressively added there. When we drop the job change and tenure variables, the selectivity correction coefficient becomes larger for both migrants and nonmigrants, is larger for nonmigrants than for migrants,[10] and is statistically significant for both groups. When λ_M (λ_N) is the only explanatory variable, its coefficient is largest and most significant. Again, the nonmigrant coefficient is considerably larger than the migrant coefficient. Thus, when no other characteristics are controlled, persons more likely to migrate earn more in both the migration and the nonmigration regimes but especially in the nonmigration regime.

Wage Change

Table 3 presents regression results for real wage change between 1973 and 1975. The general format is the same as in Table 2, but now the dependent variable is the difference between the logarithm of the 1975 wage rate and that of the 1973 wage rate--i.e., the relative wage change between these years. Again, the introduction of the selectivity variable has a modest impact on the education and experience

[10]This is similar to Nakosteen and Zimmer's results, which are based on a regression with very few control variables.

Table 3

REGRESSIONS ON AFTER-BEFORE WAGE RATE DIFFERENCES
FOR MIGRANTS AND NONMIGRANTS SEPARATELY

(Dependent Variable = Ln 1975 Real Wage - Ln 1973 Real Wage)

Explanatory Variables	MIGRANTS (n=55)				NONMIGRANTS (n=602)			
	(M-D1)	(M-D2)	(M-D3)	(M-D4)	(N-D1)	(N-D2)	(N-D3)	(N-D4)
λ_M, λ_N		-.285 (-1.71)	.0432 (0.46)	-.0120 (-0.13)		-.373 (-1.72)	-.306 (-2.30)	-.334 (-2.72
Education	.00216 (0.12)	.000705 (0.04)	.00236 (0.13)		.00115 (0.23)	.00109 (0.22)	.00284 (0.57)	
Work Experience	.0345 (1.98)	.0420 (2.38)	.0262 (1.54)		-.00384 (-0.82)	-.00211 (-0.43)	-.00143 (-0.31)	
Experience Squared	-.00108 (-2.66)	-.00117 (-2.90)	-.000837 (-2.12)		.0000487 (0.53)	.0000216 (0.24)	.0000211 (0.23)	
Job Tenure in 1974	.0249 (1.21)	.0256 (1.27)			.00348 (1.14)	.00386 (1.27)		
Quit Job 1974-1975	.0315 (0.19)	-.270 (-1.13)			.204 (3.21)	.0894 (0.97)		
Quit X Tenure 1974	-.0505 (-1.13)	-.0229 (-0.49)			-.0106 (-1.06)	-.00162 (-0.14)		
Laid Off 1974-1975	-.0375 (-0.18)	-.122 (-0.58)			-.0517 (-0.81)	-.0664 (-1.03)		
Laid Off X Tenure 1974	-.0483 (-1.79)	-.0713 (-2.41)			-.022 (-1.66)	-.0325 (-2.23)		
Union Membership 1975	.219 (1.67)	.225 (1.75)			-.015 (-0.55)	.00014 (0.01)		
Constant	-.0214 (-0.07)	.532 (1.21)	.00471 (.02)	.239 (1.52)	.190 (2.19)	.144 (1.59)	.137 (1.61)	.153 (9.44)
R^2	.272	.317	.129	.0003	.042	.047	.013	.012

Note: t-statistics are presented in parentheses under coefficients
Variables are defined in Appendix Table C.1.

coefficients, most of which are insignificant here, but it does alter the job change coefficients considerably. We will discuss the coefficients in the specifications that include $\lambda_M(\lambda_N)$ since only those are consistent estimates.

Education is insignificant in explaining 1975-73 wage changes for both migrants and nonmigrants. Although education significantly affects wage levels, it apparently does not affect the relative rate at which those levels change over time, either within locations (i.e., for nonmigrants) or between locations (i.e., for migrants). The experience and experience squared variables also are insignificant for nonmigrants, but they are both significant for migrants. The relative amount by which wages change when people migrate is positively related to the amount of overall work experience they have; the relationship becomes weaker as work experience increases, but does not peak until 18 years of work experience. These results suggest that, unless they are relatively old, people may "jump" to a new wage profile when they migrate.

Similarly, job tenure also has a stronger positive effect on the wage growth of migrants who do not change employers than it does for nonmigrants not changing employers, though neither coefficient is significant at the 10 percent level. The larger effect for migrants is consistent with the results in Table 2, which suggested a positive payoff to job transfers, especially for those who had held their jobs for some time.

The negative and sizable (though not statistically significant) coefficients for migrants of the Quit and Laid Off dummies and their interactions with 1974 Job Tenure indicate that migrants who changed

jobs received considerably smaller wage rate increases than did migrants who did not change employer. For both quits and layoffs, the difference between the wage growth of those who changed jobs when they migrated and those who remained with the same employer increases with the number of years worked for the 1974 employer.

By contrast, nonmigrants who quit their jobs experienced a higher rate of wage growth than those who did not change jobs, though the difference is not statistically significant.[11] Like migrants, nonmigrants who are laid off experience a significantly smaller rate of wage growth than otherwise similar men who did not change jobs. For both groups the rate of wage growth is inversely related to 1974 job tenure, again suggesting an inability to transfer much firm-specific capital. The difference between laid-off workers and those who did not change jobs is not as great for nonmigrants as for migrants.

These results are quite similar to those in a recent article by Ann Bartel (1979) in which she estimates regressions for a pooled sample of migrants and nonmigrants explaining absolute wage growth for three samples stratified by age (19-29, 26-35, and 45-59 in the base year) and includes migration-dummy/job-change interactions as explanatory variables. Both Bartel and we find a large positive payoff to those who migrate but remain with the same employer--i.e., job transfers--and both studies find that migrants who were laid off experience less wage growth than migrants who did not change employer. For all three of her age groups, Bartel finds that migrants who were laid off experience less wage growth over the migration interval than those who quit their jobs.

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[11]The difference is over twice as large (and is significantly different from zero) when the selectivity correction variable is not included.

We found this also when selectivity bias was not controlled, but the conclusion is dramatically reversed when λ_M is included in the migrant regression.[12]

For both migrants and nonmigrants, the coefficient on the selectivity correction variable in Eqs. (M-D2) and (N-D2) is negative and statistically significant. The negative coefficients imply that persons who are more likely to migrate (i.e., have a larger error term in the propensity-to-move equation) are less likely to have a large wage change (i.e., have a smaller error term in the wage-change equation) in either the migration or nonmigration regime. Thus migrants are negatively selected with respect to wage change and, relative to persons drawn at random, can expect a smaller wage increase in either regime considered separately.

When we exclude the job change/job tenure and then the education/experience variables in Eqs. (M-D3), (M-D4), (N-D3), and (N-D4), the coefficient of λ_M becomes smaller and is not statistically significant but the coefficient of λ_N remains at nearly the same level as before and becomes more significant. Thus, the extent of selectivity appears to be independent of our other explanatory variables for the nonmigration regime but not for the migration regime. Unfortunately, this might be a by-product of our relatively small sample size--55 migrants.

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[12]Bartel's comparison of migrants who quit their jobs and those who were transferred yields different results for each of her age groups. The relatively small average difference is consistent with what we find when selectivity is not controlled, but our results clearly change when λ_M and λ_N are included. In all three of Bartel's age groups, nonmigrants who quit their jobs experience smaller rates of wage growth than those who were laid off. Our result is exactly the reverse and is more consistent with theories of job separation.

The negative selectivity coefficients in the 1975-1973 wage change regressions and the effectively zero selectivity effects in the 1975 wage-level regressions together suggest that the selectivity variables λ_M and λ_N would be positively correlated with the error terms in 1973 wage-level data that have been "presorted" into observations on migrants and nonmigrants.[13] Indeed this is the case when we consider regressions of the 1973 wage level on the selectivity variable and personal characteristics (App. E). For both migrants and nonmigrants, the coefficient on the selectivity variable is positive, implying a positive correlation between the errors in the reduced-form propensity-to-move equation and the wage equations. Consequently, this set of results on wage levels and wage changes indicates the following: (1) Whether they subsequently chose to migrate or not, persons who were more likely to move between 1974 and 1975 tended to have a higher wage rate in the year before migration, 1973, even after controlling for observed characteristics. (2) Persons more likely to move tended to have a lower rate of wage increase, so much so that by 1975, the year after the migration interval, there was no longer any correlation between the errors in the propensity-to-move equation and the wage equation for either migrants or nonmigrants.

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[13] The wage change equation can be thought of as having a selectivity coefficient that equals the difference in the 1975 and 1973 coefficients, i.e.,

$$\theta_{M1975-1973} = \theta_{M1975} - \theta_{M1973} .$$

Since $\theta_{M1975-1973}$ is estimated to be negative and θ_{M1975} is estimated to be zero, θ_{M1973} should be positive.

WAGE GAINS TO MIGRANTS AND TO NONMIGRANTS

In this subsection we parlay the two-regime regression estimates of the post-migration wage rate and the after-before migration wage difference into predictions of the wage gains to migrants and to nonmigrants. These two dependent variables, post-migration wage rate and wage change over the migration interval, provide alternative measures of implied wage gains.

Tables 4 and 5 display predicted wage gains to migration and nonmigration (staying). Table 4 utilizes the wage-level equations in columns (M-L2) and (N-L2) of Table 2, while Table 5 uses the wage-change equations in columns (M-D2) and (N-D2) of Table 3. The upper panels of Tables 4 and 5 present the gains to migration; the lower panels present the gains to staying. Column 1 in each table presents the expected wage in the migration regime, col. 2 the expected wage in the nonmigration regime. In the top panel the estimate in col. 2 is the counterfactual value; in the bottom panel, the estimate in col. 1 is the counterfactual value. We will discuss Table 4 first.

In the two-regime model, wage gains to migration (or to nonmigration) can arise from three possible sources. First, the wage structures, β_M and β_N, may differ between the two regimes; even if migrants' characteristics did not differ from nonmigrants', those characteristics may be valued more highly in the migration regime than in the nonmigration regime. The magnitude of this source of gains is illustrated in col. 4 of the top row of each panel of the tables, where we show the gains to migration (nonmigration) for an individual having the mean characteristics $\overline{X}$ of the overall sample. In Table 4 the entry

Table 4

ESTIMATED WAGE GAINS TO MIGRATION AND NONMIGRATION BASED ON AFTER-MIGRATION WAGE LEVELS EQUATION
(We predict values of logarithms of 1975 wages in the migration regime ($\ln W_M$) and nonmigration regime ($\ln W_N$) for alternative values of explanatory variables (X) using coefficients of Equations (M-L2) and (N-L2) in Table 2)

Gains to Migration

Values of Explanatory Variables (X) Used	(1) $\hat{\ln W}_{MM}$	(2) $\hat{\ln W}_{MN}$	(3) $\hat{\ln W}_{MM} - \hat{\ln W}_{MN}$	(4) $(\hat{\beta}_M - \hat{\beta}_N)X$	(5) $(\hat{\theta}_M - \hat{\theta}_N)\lambda_M$
$\bar{X}$ for total sample	2.069	1.873	.196	.196	na
$\bar{X}$ for migrant sample	1.812	1.928	-.116	.032	-.148
$\bar{X}$ for average migrant who quit job 1974-75*	1.559	1.875	-.316	-.203	-.113
$\bar{X}$ for average migrant who was laid off 1974-75*	1.993	1.793	.200	.335	-.135
$\bar{X}$ for average migrant who did not change jobs 1974-1975*	2.005	2.051	-.046	.153	-.199

Gains to Nonmigration

	$\hat{\ln W}_{NN}$	$\hat{\ln W}_{NM}$	$\hat{\ln W}_{NN} - \hat{\ln W}_{NM}$	$(\hat{\beta}_N - \hat{\beta}_M)X$	$(\hat{\theta}_N - \hat{\theta}_N)\lambda$
$\bar{X}$ for total sample	1.874	2.070	-.196	-.196	na
$\bar{X}$ for nonmigrant sample	1.875	2.089	-.214	-.208	-.006
$\bar{X}$ for average nonmigrant who quit job 1975-75*	1.807	1.742	.065	.090	-.025
$\bar{X}$ for average nonmigrant who was laid off 1974-75*	1.525	1.853	-.328	-.313	-.015
$\bar{X}$ for average nonmigrant who did not change jobs 1974-75*	1.909	2.137	-.226	-.220	-.006

*In these predictions, all variables other than job change and job tenure are set equal to the actual migrant (nonmigrant)/job change category sample mean value.

na = not applicable

$(\beta_M - \beta_N)\bar{X}$ is positive, implying that structural differences alone would imply that the average person in the sample would gain (in terms of 1975 real wage levels) by migrating. Since structural differences alone can allow positive gains to either migrants or nonmigrants, but not to both, $(\beta_N - \beta_M)\bar{X}$, the wage gains to nonmigration, is negative in Table 4.

Second, compared with the average person in the population, actual migrants may possess more of the characteristics associated with higher wages in the migration regime. This is not the case in our sample. Evaluating $(\beta_M - \beta_N)X$ at $\bar{X}_M$, the average characteristics of the migrants in our sample, results in a smaller value of the wage gains to migration due to structural differences (col. 4, row 2) than did use of the overall sample average, $\bar{X}$. Similarly, this component of the wage gains to nonmigration, $(\beta_N - \beta_M)X$, is somewhat smaller when evaluated at $\bar{X}_N$, the average characteristics of nonmigrants, than at the $\bar{X}$ for the overall sample.

In the remaining rows of the top panel of Table 4, we evaluate $(\beta_M - \beta_N)\bar{X}$ for different job change categories. Structural characteristic differences together imply the largest wage gains to migration for persons who were laid off, while the wage gains to nonmigration are largest for persons who quit their jobs.

Third, even if migrants' observed wage-determining characteristics and their wage structure did not differ from nonmigrants', migrants could gain from migration because of what we call the gains to sorting, i.e., the factor $(\theta_M - \theta_N)\lambda_M$. In Cols. (M-L2) and (N-L2) of Table 2, both θ_M and θ_N are positive, indicating that persons more likely to migrate had higher wages in both the migration and the nonmigration regimes than a person of similar characteristics drawn at random, though neither θ_M nor θ_N is significantly different from zero. Because θ_N exceeds θ_M, persons more likely to migrate apparently would do even better in the nonmigration regime than in the migration regime. Hence, the sorting adjustment in Col. 5 further reduces the size of our estimate of the gain from migration for the average migrant, and in fact leads to the conclusion that the average migrant loses in terms of 1975 wage levels, compared to what he would have experienced had he not migrated. But again we must mention that the selectivity effects are statistically insignificant, so column (5) might just as fairly be read as a column of zeros. If so, Col. 3 would be equivalent to Col. 4. However, as it is reported in the table, we see that the average migrant experiences a wage loss to migration. That occurs because the negative sorting effect (Col. 5) dominates the positive structural effect (Col. 4). Therefore, on net it looks as though the migrants did worse by choosing to migrate. With respect to the theory of migration described earlier, this would imply that non-pecuniary gains or higher wages in subsequent time periods would have to accrue to migrants to compensate

for the lower wage rate immediately after moving. (Alternatively, migrants' expectations about the consequences of migration may have been biased.) If instead we had treated the sorting effect as effectively zero, the reverse conclusion would hold: Migrants do somewhat better by choosing to migrate.

The selectivity correction factor for nonmigrants, $(\theta_N - \theta_M)\lambda_N$, is negative, but small. The negative returns to sorting are considerably smaller for nonmigrants than for migrants because λ_N is typically much smaller than λ_M.

We next turn to Table 5, which describes the predicted net wage changes for migrants and nonmigrants based on the wage-difference regressions in Table 3. The following conclusions emerge from Table 5: (1) Now the sorting gains are positive; as in Table 4, they are larger in absolute value for migrants than for nonmigrants.[14] These sorting gains are statistically significant at about the 15-percent level (t=1.4). (2) For migrants the sorting gains are augmented by large structural gains, but for nonmigrants the sorting gains are more than offset by large structural losses.

The wage gains from migration due to both structural differences and sorting gains are greatest for job transfers. The wage gains from

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[14]Our results on wage changes in Table 3 showed that persons more likely to migrate tended to have a lower wage change (ln wage 1975 - ln wage 1973) whether in the migration regime or the nonmigration regime (i.e., θ_M and θ_N are both negative in the wage-change regressions). Yet in determining whether the wage change of migrants was greater in the migration regime than in the nonmigration regime, what is relevant is that $\theta_M - \theta_N$ is positive: Although migrants might expect a low wage change in either regime, they would be relatively worse off in the nonmigration regime.

Table 5

ESTIMATED WAGE GAINS TO MIGRATION AND NONMIGRATION
BASED ON AFTER-BEFORE MIGRATION WAGE DIFFERENCE EQUATIONS

(We predict values of the difference between the logarithm of the 1975 wage and the logarithm of the 1973 wage for the migration regime ($D_M = (\ln W_{75,M} - \ln W_{73,M})$) and the nonmigration regime ($D_N = (\ln W_{75,N} - \ln W_{73,N})$) for alternative values of explanatory variables (X) using coefficients of Equations (M-D2) and (N-D2) in Table 3).

Values of Explanatory Variables (X) Used	(1) $\hat{D}_{MM}$*	(2) $\hat{D}_{MN}$	(3) $\hat{D}_{MM}-\hat{D}_{MN}$	(4) $(\hat{\beta}_M-\hat{\beta}_N)X$	(5) $(\hat{\theta}_M-\hat{\theta}_N)\lambda_M$
			Gains to Migration		
$\bar{X}$ for total sample	1.085	.145	.940	.940	na
$\bar{X}$ for migrant sample	.345	-.473	.818	.674	.144
Average migrant who quit job 1974-75	.286	-.236	.522	.412	.110
Average migrant who was laid off 1974-75	.199	-.676	.875	.743	.132
Average migrant who did not change jobs j974-75	.463	-.668	1.131	.938	.193
	$\hat{D}_{NN}$	$\hat{D}_{NM}$	$\hat{D}_{NN}-\hat{D}_{NM}$	$(\hat{\beta}_N-\hat{\beta}_M)X$	$(\hat{\theta}_N \hat{\theta}_M)\lambda_N$
			Gains to Nonmigration		
$\bar{X}$ for total sample	.145	1.085	-.940	-.940	na
$\bar{X}$ for nonmigrant sample	.174	1.127	-.953	-.959	.006
Average nonmigrant who quit job 1974-75	.333	.788	-.455	-.479	.024
Average nonmigrant who was laid off 1974-75	.050	.821	-.771	-.786	.015
Average nonmigrant who did not change jobs 1974-75	.167	1.181	-1.014	-1.018	.004

*D_{IJ} = (ln 1975 wage - ln 1973 wage)$_{IJ}$ where J = regime being considered, I = actual choice made.

na = not applicable

nonmigration are negative for all categories examined; they are most reasonable (i.e., least negative) for those who quit their jobs. (This was the only subgroup with positive estimates of gains to nonmigration in Table 4.)

The magnitudes of many of the numbers in Table 5 do not look realistic, for we must keep in mind that these are approximately relative rates of wage growth. The counterfactual values, D_{MN}, appear far too small (i.e., too negative) as estimates of what migrants would have experienced had they not moved, while the D_{NM}, seem far too large as estimates of what nonmigrants would have experienced had they moved.[15]

[15]The coefficients presented in Tables 2 and 3 did not in themselves look strange or unrealistic. In fact, many of those coefficients are consistent with our expectations and with findings of other studies. Moreover, the same kind of problem we encounter appears in work by Rosen and Willis (1979).

V. CONCLUSION

In this paper we have proposed a method of estimating wage gains to migration that surmounts a constraint built into much previous research, namely, that if migrants gain from migration then nonmigrants must lose. Our method integrates the analysis of wage gains to migration with the hypothesis that the decision to migrate depends on a comparison of expected well-being given migration with that given nonmigration, and recognizes that wage gains (or losses) are only one aspect of well-being. In addition, the method offers the prospect of positive control over the fact that migrants and nonmigrants are selected samples. Without controlling for selectivity, estimates of wage gains to migrants, and to nonmigrants, may be biased.

Our empirical results form a mixed pattern. An analysis of wage rates in the year following the migration decision interval reveals an apparent absence of selectivity bias for both migrants and nonmigrants. By "absence" we mean not only that the selectivity correction variables were statistically insignificant, but also that the coefficients on other variables were essentially unchanged when the selectivity correction variables were introduced. Consequently, we find that although earlier research on wage gains to migration can be criticized on conceptual grounds, these criticisms carry no weight in our empirical application to wage levels in the year immediately after migration. Since we have only examined the first year after migration, perhaps additional wage gains would accrue in subsequent years. Studies by Sandell (1977), Wertheimer (1970), and Marr and Millerd (1980) suggest

that several years may be required before significant wage gains to migration are observed.

Our "after-before" wage-change results for the two-regime model also reveal a wage increase for migrants, although we think the estimated increase is too large to be credible. Part of the effect can be traced to a difference in the effect of years of work experience in the migration and nonmigration wage regimes. In the migration regime, experience exerts a significant, positive effect; in the nonmigration regime, the effect is virtually nil. The significance of experience in the migration wage-change equation means that experience had a substantially greater effect on the wage rate for migrants in 1975, after migration, than it did for them in 1973, before migration. In contrast, there was no change in returns to experience for persons who did not move.

The other major source of large wage changes for migrants lies in the gains to sorting, which derive from the statistically significant selectivity effects in the wage-change equations. Here we found that persons who were more likely to migrate during 1974-1975 obtained lower wage increases regardless of whether they were migrants or nonmigrants. However, this effect was stronger among nonmigrants than among migrants. As a result, our model implies positive sorting gains to migration (and to nonmigration). Thus for migrants this effect complements the experience effect, and together these effects generate large net wage increases for migrants, ranging between 50 and 100 percent, and large net wage decreases for nonmigrants of the same order of magnitude.

While in the wage-change analysis the wage gains to migration arise both from structural differences and sorting effects, most of these

gains can be attributed to structural differences between the migration and nonmigration wage-change equations, especially in the effect of work experience in the migration wage-change equations. We suspect that the large effect of experience is an artifact of the data and may well disappear in a larger sample of migrants. Migrants are less experienced than nonmigrants, and the migrant data are mainly fitting the initial, most rapidly rising portion of the experience profile.

In conclusion, we would like to note that issues raised in our analysis occur frequently in demographic analyses involving comparisons of two groups that are not randomly designated but in fact are self-selected. For instance, women who choose to work may consist of those having high expected market wage rates relative to the value of their time at home. Many other social processes yield selected samples, and unless this is considered during empirical analysis (by means of techniques such as those employed here), estimates of the consequences of these processes may be biased and therefore not useful in drawing inferences for the population at large.

Appendix A

THE SELECTIVITY CORRECTION VARIABLES λ_M AND λ_N

According to equation (8') in the text, the reduced-form propensity-to-move equation is

$$y = \alpha_M\beta_M X_M - \alpha_N\beta_N X_N + \gamma Z + \eta \ ,$$

which we now abbreviate as $y = I + \eta$ where

$$I = \alpha_M\beta_M X_M - \alpha_N\beta_N X_N + \gamma Z.$$

Migration occurs if and only if $y > 0$. The probability that a person migrates equals

$$\Pr(y > 0) = \Pr(I + \eta > 0) = \Pr(\eta > -I)$$

$$= \Pr(\eta/\sigma_\eta > -I/\sigma_\eta) = F(I/\sigma_\eta)$$

where F is the cumulative standard normal function. Probit regression yields estimates of the coefficients of the index I that have been normalized by σ_η. Using the normalized coefficients the λs are defined as follows:

$$\lambda_{Mi} = f(I_i)/F(I_i) \text{ for migrant } i$$

$$\lambda_{Nj} = -f(I_j)/(1 - F(I_j)) \text{ for nonmigrant } j,$$

where f is the standard normal density.

Appendix B

PROBIT ANALYSIS OF THE DECISION TO MIGRATE

To obtain instruments for the selectivity correction variables, we estimated a reduced-form propensity-to-migrate equation using probit analysis. The results appear in Table B.1. (The explanatory variables are defined in Table C.1. The means and standard deviations of the variables, stratified by migration status, appear in Table C.2.) The reduced-form equation contains the variables considered in Tables 1-3 to affect the household head's wages, as well as variables that affect the wages of the wife (about 90 percent of the white male heads who make up our sample are married); the probit equations also include other variables that might affect a household's likelihood of migrating: head's and wife's hours of work, the household's nonwage income, the household's homeowner status, and a dummy indicating whether the household lives in the county where the head grew up.

The coefficients in Table B.1 indicate that the propensity to migrate is significantly negatively related to indicators of location-specific capital: owning a home or living in the county where the head grew up. Wife's hours of work is negatively related to the likelihood of migrating, while, by contrast, husband's hours of work is positively related to migration. Head's education and work experience do not significantly affect the family's propensity to move, but, other things being the same, households are more likely to move the more highly educated the wife. Heads who changed jobs between 1974 and 1975 are

more likely to move between those years; this is especially true for those who quit their jobs. The effect of job change is mitigated by increased job tenure before the change for those who quit, but is accentuated by tenure for those laid off. Job tenure does not affect the migration behavior of those who did not change jobs.

Table B.1

PROBIT EQUATION EXPLAINING DECISION TO MIGRATE BETWEEN SPRING 1974 AND SPRING 1975
(See Sec. III for description of sample)

EXPLANATORY VARIABLE	COEFFICIENT	ASYMPTOTIC t-STATISTIC
Household Head's Characteristics		
Education	- 0.0219	(-0.66)
Work Experience	- 0.0241	(-0.79)
Work Experience Squared	0.000316	(0.53)
Job Tenure in 1974	- 0.158	(-0.76)
Quit Job 1974-75	1.32	(5.13)
Quit x Tenure 1974	- 0.0896	(-1.59)
Laid Off 1974-75	0.342	(1.09)
Laid Off x Tenure 1974	0.128	(2.84)
Union Membership 1975	- 0.343	(-1.97)
Hours of Work in 1973	0.368	(2.84)
Grew Up in 1974 County	- 0.449	(-2.64)
Wife's Characteristics		
Education	0.0372	(1.66)
Worked in 1973	0.0899	(0.39)
Hours of Work in 1973	- 0.277	(-1.85)
Household Characteristics		
Nonwage Income	0.0137	(0.45)
Homeowner in 1974	- 0.356	(-1.94)
Intercept	- 2.12	(-3.48)
-2 x Likelihood Ratio	57.46	

Appendix C

DEFINITIONS, MEANS, AND STANDARD DEVIATIONS OF VARIABLES

Table C.1

DEFINITIONS OF VARIABLES USED IN TABLES 1-3 AND TABLES B.1 AND C.2

Table 1

Dependent Variables

ℓn 1975 Wage	Natural logarithm of average hourly wage in 1975 (total labor income in 1975 divided by total hours worked in 1975) deflated by 1970 Consumers' Price Index for the area of residence at the time of the 1975 survey.
ℓn 1973 Wage	Same as above for 1973.

Explanatory Variables

Migrated 1974-1975	A dummy that equals one if the person lived in an SMSA or nonmetropolitan State Economic Area at the time of the 1975 survey different from that lived in at the time of the 1974 survey.
Education	Years of schooling completed at the time of the 1975 survey.
Work Experience	Years of work experience = Age(1975) - Education(1975) - 5.
Job Tenure in 1974	Midpoint of number of years on current job category at time of 1974 survey. Values: .75 (0-1.5), 2.5 (1.5-3.5), 6.5 (3.5-9.5), 15 (9.5+).
Quit Job 1974-1975	A dummy that equals one if the person left his last job prior to the 1975 survey because he quit, retired, was promoted, or was drafted.
Laid Off 1974-1975	A dummy that equals one if the person left his last job prior to the 1975 survey because he was laid off, was fired, was on strike, the company folded, or the job was temporary; people who left a job but gave no reason are also included here.

Union Membership 1975	A dummy that equals one if the person belonged to a union at time of 1975 survey.

Additional variables in Table 2

λ_M	The selectivity bias correction factor for the migration regime = $f(I_i)/F(I_i)$, where $f(I_i)$ = standard normal density of the probit index for this individual and $F(I_i)$ = his predicted probability of moving (see appendix A for further detail).
λ_N	Selectivity bias correction factor for nonmigration regime = $-f(I_i)/(I - F(I_i))$ (see definition above).

Additional Variables in Appendix Table B.1

Hours of work in 1973	Annual hours worked by head (wife) in 1973 in 1000's; wife's hours of work = 0 if head is not married.
Grew up in 1974 county	A dummy that equals one if, at the time of the 1974 survey, head lived in the same county where he grew up.
Wife Worked in 1973	A dummy that equals one if the wife worked during 1973; = 0 if head is not married.
Nonwage Income	Annual family income in 1973 less the 1973 labor incomes of the head and wife.
Homeowner in 1974	A dummy that equals one if the household owned its home at the time of the 1974 survey.

Table C.2

MEANS AND STANDARD DEVIATIONS OF VARIABLES IN TABLES 1-3 AND B-1 FOR MIGRANTS, NONMIGRANTS, AND TOTAL SAMPLE

Variable	Migrants (n=55) $\bar{X}$	Migrants (n=55) σ	Nonmigrants (n=602) $\bar{X}$	Nonmigrants (n=602) σ	Weighted Average*
Migrated 1974-1975	1.0	(0)	0	(0)	.044
Education	12.7	(3.2)	12.4	(2.9)	12.4
Work Experience	14.6	(10.5)	21.7	(12.1)	21.4
Job Tenure in 1974	3.93	(4.45)	7.12	(5.64)	6.98
Quit Job 1974-1975	.455	(.548)	.095	(.293)	.110
Laid Off 1974-1975	.182	(.386)	.074	(.262)	.079
Union Membership 1975	.218	(.417)	.352	(.478)	.346
λ_M	1.64	(0.58)	---	---	---
λ_N	---	---	-.075	(0.11)	---
Heads' hours of work in 1973	2404.	(750.)	2259.	(542.)	2265.
Head grew up in 1974 county	.218	(.417)	.429	(.494)	
Wife's Education	10.9	(4.1)	10.7	(4.0)	10.7
Wife Worked in 1973	.509	(.504)	.508	(.500)	.508
Wife's Hours of Work in 1973	535.	(822.)	641.	(815.)	637.
Nonwage Income	899.	(2043.)	1557.	(3038.)	1528.
Homeowner in 1974	.436	(.496)	.718	(.451)	.705

*Since we use the entire sample of migrants but only half of all nonmigrants, we give twice as much weight to each nonmigrant here to estimate a population-representative average.

Appendix D

REGRESSIONS ON REAL AND NOMINAL WAGE RATES

In Table D.1 we compare regressions using real (i.e., adjusted for regional cost-of-living differences) and nominal (i.e., unadjusted) wages as the dependent variables. We present a regression specification like (L-3) in Table 1 estimated for the total sample and specifications like (M-L1) and (N-L1) in Table 2 estimated for the migrant and nonmigrant subsamples.

The noteworthy feature of Table D.1 is that, for both the total sample and the two subsamples, the regression coefficients are remarkably insensitive to whether the dependent variable is measured in real or nominal terms. The largest differences appear to be in the intercepts. The migrated 1974-1975 and λ coefficients, our main interest here, are somewhat smaller when real wages are considered, but neither the real-wage nor the nominal-wage coefficient of these variables is statistically significant in any of the regressions. Most other coefficients are somewhat larger when real wages are considered than when nominal wages are, but the differences are generally very small.

Hence, whether one uses real or nominal wages as the dependent variable appears to have little consequence for the results of the analysis.

Table D.1

COMPARISON OF REAL AND NOMINAL 1975 WAGE REGRESSION COEFFICIENTS FOR TOTAL SAMPLE AND FOR MIGRANTS AND NONMIGRANTS SEPARATELY

	Total Sample (n=657)		Migrants (n=55)		Nonmigrants (n=602)	
Variable	Real	Nominal	Real	Nominal	Real	Nominal
Migrated 1974-1975	.0228 (0.35)	.0324 (0.51)	---	---	---	---
λ	---	---	.0218 (0.12)	.0442 (0.24)	.112 (0.39)	.114 (0.42)
Education	.0912 (14.5)	.0859 (14.2)	.0595 (3.01)	.0531 (2.72)	.0945 (14.3)	.0895 (14.0)
Experience	.0381 (6.52)	.0379 (6.72)	.0628 (3.19)	.0591 (3.04)	.0359 (5.66)	.0361 (5.90)
Experience Squared	-.000601 (-5.25)	-.000601 (-5.43)	-.00136 (-3.02)	-.00137 (-2.93)	-.000549 (-4.50)	-.000556 (-4.72)
Tenure 1974	.0109 (2.81)	.0110 (2.93)	.0392 (1.73)	.0438 (1.96)	.0106 (2.66)	.0105 (2.74)
Quit job 1974-1975	-.109 (-1.49)	-.113 (-1.60)	-.216 (-0.81)	-.167 (-0.63)	.0126 (0.10)	.00894 (0.07)
Quit X Tenure 1974	.0271 (2.19)	.0250 (2.09)	.0198 (0.38)	.00446 (0.09)	.0176 (1.18)	.0160 (1.12)
Laid Off 1974-1975	-.161 (-2.08)	-.151 (-2.01)	.0616 (0.26)	.0979 (0.42)	-.175 (-2.06)	-.163 (-2.01)
Laid Off X Tenure 1974	.0132 (0.98)	.00967 (0.74)	-.0191 (-0.57)	-.0245 (-0.75)	.00663 (0.35)	.00241 (0.13)
Union Membership 1975	.187 (5.22)	.165 (4.77)	.263 (1.83)	.262 (1.85)	.178 (4.57)	.155 (4.13)
Constant	.0774 (0.72)	.165 (1.59)	.273 (0.55)	.344 (0.71)	.0599 (0.50)	.142 (1.24)
R^2	.362	.357	.505	.492	.362	.359

Appendix E

REGRESSIONS ON PRE-MIGRATION WAGE RATES
FOR 1974-1975 MIGRANTS AND NONMIGRANTS SEPARATELY

(Dependent Variable = Ln(1973 Real Wage Rate))

Explanatory Variables	Migrants	Nonmigrants
λ_M, λ_N	.4503 (2.39)	.5781 (1.99)
Education	.0499 (2.95)	.0934 (14.57)
Work Experience 1975	.0280 (1.63)	.0378 (5.98)
Experience Squared	-.0005039 (-1.32)	-.0005780 (-4.84)
Job Tenure in 1974	.0328 (2.39)	.0088 (2.32)
Union Membership 1975	.1031 (.84)	.1750 (4.65)
Constant	-.4402 (-1.24)	-.0759 (-.59)
n	55	602
R^2	.53	.37

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