THE WAGE DYNAMICS OF INTERNAL MIGRATION WITHIN THE UNITED STATES

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INTRODUCTION

The internal migration of workers between labor markets has important implications for the U.S. economy.[1] Assuming that workers are attracted to markets in which their labor services earn a higher real wage, aggregate migration flows are expected to be positive in the direction of low to high-income regions. Through a more efficient spatial allocation of labor resources, internal migration would then help to decrease regional earnings and employment disparities. However, the efficacy of migration as a regional equilibration mechanism is dependent upon efficient migratory choices at the individual level. As a result, economists have a keen interest in understanding the role of geographic mobility in individual labor market outcomes.

In the standard human capital paradigm, rational economic agents are posited to evaluate the costs and benefits associated with a change of location [Sjaastad, 1962]. Agents will move when the discounted value of real income available at a potential destination exceeds that at the origin by more than the cost of moving. Migration costs may be substantial: search expenditures, forgone earnings, direct out-of-pocket expenses, and the "psychic" cost of leaving friends and family must all be weighed carefully against the potential benefits of a move.[2] In this framework, migration reflects an investment undertaken with the expectation that future earnings rise as a result of expenditures on job search and incurred mobility costs.[3] Consequently, the human capital model predicts (on average) a positive return to this investment.

Contrary to this prediction empirical researchers have failed to establish consistent evidence of a positive return to internal migration. With few exceptions, early research focused on contemporaneous returns, in effect assuming that substantial pecuniary gains are realized at the time of a move. For example, both Polachek and Horvath [1977] and Bartel [1979] measure economic returns as the difference in earnings between movers and non-movers across consecutive survey periods. Only Bartel finds evidence of a positive migrant wage differential immediately following a move. The inherent drawback of this approach is its failure to allow returns to accumulate over a longer time frame. If migration is treated as an investment in human capital, pecuniary returns need not be realized immediately upon a change of location for migration to be economically viable. Migrants may be willing to accept lower starting wages across locations in anticipation of a future stream of earnings benefits. As migrants invest in a new stock of capital specific to their new location, their wage pro-

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files will tend to rise faster than non-migrants in the post-migration period [Shaw, 1991]. Estimates based on earnings data with limited time horizons will not capture this within-location wage growth, tending to downward bias estimated returns [Greenwood, 1997].

The increased availability of detailed longitudinal data sets has allowed later studies to include the accumulation of economic returns over extended time frames. Hunt and Kau [1985] are among the first to incorporate a distinctly dynamic element into their empirical work by examining wage outcomes over a four-year period following migration. Nonetheless, they find no statistically significant return to migration in the years following a move. Other panel-based studies have examined migration in conjunction with alternative modes of job mobility. Shaw [1991] demonstrates that interregional migrants have higher predicted wage growth relative to non-migrants four years after a move, especially if they remain in the same industry. Krieg [1997] finds a positive migrant earnings differential three years after a move if migrants stay within the same occupation.

Although more recent efforts have begun to exploit panel data over longer time frames, for the most part these studies constrain the effect of migration on wages to be constant over time. If pecuniary returns accumulate in a non-linear fashion following migration, using a single migration dummy is overly restrictive and is an inadequate means of capturing the timing of economic returns. For example, Borjas, Bronars, and Trejo [1992a] report a migrant wage disadvantage in the years immediately following an interstate change of residence, but suggest that this initial wage penalty dissipates with time spent in the new location. The authors allow for a time-dependent migration effect by following an approach common in the U.S. immigrant assimilation literature. Accordingly, internal migrants are classified by the duration of their post-migration residency. Their empirical work centers on the estimation of a cross-sectional wage equation which includes a migrant dummy and a "years since migration" variable. The parameter estimate on the migrant dummy provides a measure of the immediate wage differential while the estimated coefficient on the latter variable is interpreted as an indication of migrant economic progress. In all cases, the coefficient on the migrant dummy is negative and significant, suggesting that interstate migrants earn wages below observationally similar native workers immediately following a move. However, the positive coefficient on the "years since migration" variable implies that migrants experience about a six-year period of labor market adjustment during which time their wages converge toward the native level. This highlights the need to consider not only an extended earnings horizon, but also the timing of accumulated returns.

One additional complication in this literature that has been largely overlooked by researchers has been the inability to fully account for the past migration history of the individual. If migrations are identified retrospectively and the observation window does not extend back to the date of full-time labor market entry, then the past migratory history of the individual (during the work career) becomes entirely uncertain. This "left-censoring" of migration history introduces an unknown quantity of misclassification bias into the estimates since some individuals who migrated prior to the researcher's observation window are incorrectly classified as non-migrants. Misclassification across migrant status can seriously dampen estimated effects, biasing results toward little if any positive return to migration. This type of left-censoring becomes especially troublesome if pecuniary returns accumulate over an extended time horizon. For example, suppose that individuals are observed at a given point in time and that migrant status is determined retrospectively through an observation window extending back only three years. Suppose also that the majority of the pecuniary return to migration only begins to accumulate some five years after migration. In this example, two problems become readily apparent. First, the majority of the return to migration has not yet accrued for most individuals classified as migrants (i.e., those individuals observed to migrate during the observation window). Second, and perhaps more importantly, some individuals *classified as non-migrants* may have migrated prior to the observation window, and will just now be collecting the bulk of their migratory rewards. Such misclassification will no doubt lead to severely erroneous conclusions about the magnitude (or even existence) of economic returns to migration. Estimated returns will be downward biased.

Using a sample of young men drawn from the National Longitudinal Survey of Youth 1979 (NLSY79), this study attempts to document and measure the extent of the long-term wage effects associated with the interstate migration. Specifically, this study improves on the previous literature in three aspects. First, these data follow workers from the start of their working careers which substantially minimizes the potential for misclassification across migrant status. This allows me to circumvent the left-censoring of residential history problem common in previous studies. Second, by estimating a full panel-based regression with wage observations extending well beyond the date of migration, this study more fully captures long-term wage effects. Finally, the time profile of migrant earnings is explored by estimating a wage model with more flexibility than that used in previous studies of internal migration. This "flexible-form" specification is unique to the internal migration literature because it allows for examination of each (pecuniary) phase of the migratory sojourn.

This study finds that pecuniary returns generally accumulate over a five-year period following interstate migration. Starting at levels nearly identical to non-migrants in the years just prior to migration, migrants demonstrate superior wage growth relative to non-movers over the first five years after migration. After five years, migrant wages peak nearly 5 percent higher than the non-migrant benchmark. This finding is consistent with notion that migrants invest heavily into a new stock of "location-specific" capital [Shaw, 1991]. This period of intensive investment is most likely due to the high marginal return on such investments at low (initial) levels of location-specific capital stock in the years immediately following a move. In contrast to previous empirical work of this nature [Borjas, Bronars, and Trejo, 1992a], I find no evidence of a migrant wage penalty in the years just following a move; migrant wages are consistently above non-migrant levels throughout the entire post-migration period.

EMPIRICAL SPECIFICATION

The econometric approach utilized in this study centers on the estimation of two alternative wage specifications meant to capture the pecuniary return to migration. The first is a baseline model of wage determination common throughout the migration literature. By utilizing detailed longitudinal data with wage observations extending well beyond the date of migration and accounting for the left-censoring of migration history, the estimates from this baseline specification represent a marked improvement upon past empirical work. In addition, this study employs a more flexible earnings model meant to capture the time profile of migrant earnings relative to the date of migration. This latter specification is similar to that used by Jacobson, LaLonde and Sullivan [1993] and Stevens [1997] to examine the long-term earnings effects of involuntary job loss.

Given longitudinal data on individuals, the baseline earnings specification can be written as follows:

$$\ln W_{ilt} = X_{ilt}\beta + M_{i,t>k}\gamma + \alpha_i + \phi_t + \zeta_l + \varepsilon_{ilt} \tag{1}$$

where $\ln W_{ilt}$ is the natural log of real hourly earnings for individual i in location l at time t and X_{ilt} is a vector of standardizing personal and job-related characteristics that may vary over time. Let the date of migration be at time $t = k$. Then $M_{i,t>k}$ is a binary dummy variable equalling one for all observations subsequent to the date of migration (where $t > k$). It is important to note that this formulation treats all migrant "pre-migration" observations identically with non-migrant observations; at the time, "pre-migrants" are in fact non-migrants. The error term is composed of a time-invariant person-specific component (α_i), an economy-wide time effect (ϕ_t), a location-specific component (ζ_l), and a purely random element (ε_{ilt}). Discussion of the explicit set of assumptions regarding the residual is deferred until the end of this section.

While Equation (1) conforms with an approach generally taken in the literature on the returns to migration [Greenwood, 1997], this specification is unlikely to capture the true migration effect if pecuniary returns are time-varying. The additional specification proposed in this study attempts to capture a more complete profile of migrant earnings. Added flexibility is introduced into the model by allowing the effect of migration on wages to vary over time. This "flexible-form" specification can be written in the following manner:

$$\ln W_{ilt} = X_{ilt}\beta + M_{it}\gamma_t + \alpha_i + \phi_t + \zeta_l + \varepsilon_{ilt} \tag{2}$$

where M_{it} is now a vector of dummy variables indicating migration in some future, current, or previous year. If the migration premium is time-varying, the pecuniary return to migration will be captured in the parameter vector γ_t. Specifically, the migrant wage profile is captured in a vector of dummy indicators relating both pre- and post-migration wage observations to the year of migration. Two pre-migration variables (*2-3 Years Before, One Year Before*) identify wage observations from the years preceding migration, providing some indication of the pre-migration earnings trend. A variable identifying the year in which migration occurs (*Year of Migration*) yields a measure of the contemporaneous wage change across locations when compared to the parameter estimates on the pre-migration wage indicators. The next two indicator variables identify wage observations occurring in the years following migration across two-year intervals (*1-2 Years After, 3-4 Years After*). For example, the *1-2 Years After* variable refers to wage observations reported in either the first or second year following migration. The final indicator variable (*5+ Years After*) identifies all wage observations five or more years subsequent to the date of migration.

Failure to adequately control for differences between migrants and non-migrants may bias estimates of the returns to internal migration. To control for differences across observable characteristics, each empirical specification includes a rich set of personal, job, and location-related regressors. These explanatory variables are common to most "human capital" models of wage determination. These include the highest grade of completed schooling, actual experience, and current job tenure. I include both experience and tenure up to a cubic function to avoid the misspecification biases highlighted by Murphy and Welch [1990]. In addition to job tenure, other job-specific variables include union status, part-time status, self-employed status, and whether the job is in the government sector. Borjas, Bronars, and Trejo [1992a] suggest omitting such variables from the model since the process of migrant assimilation often involves a substantial amount of "job shopping." However, if job characteristics are correlated with the migration decision, then failure to include these variables in the wage equation could result in a misspecification of the model.[4] Finally, I account for several other relevant personal and location-specific characteristics that could influence the migration decision such as marital status, health status, and residence in a central city.

Unfortunately, much of the heterogeneity between migrants and non-migrants is likely to be unobserved by the econometrician. Unobserved heterogeneity is accounted for by assuming that unobservables consist of a time-invariant person-specific component, α_i, an economy-wide year fixed-effect, ϕ_t, a location-specific fixed-effect, ζ_l, and a purely random element, ε_{ilt}. To account for the unobserved macroeconomic (ϕ_t) and location-specific factors (ζ_l) affecting mobility, I include controls for the calendar year and the individual's census division of residence, respectively. The time-invariant, person-specific component (α_i) is included in the error term to capture unobservable personal differences between migrants and non-migrants. For example, unobserved factors such as ability, motivation, or attitudes toward risk tend to be highly correlated with the propensity to migrate [Greenwood, 1997]. This is essentially the "favorable self-selection hypothesis" as put forth in the international migration literature by Chiswick [1978].[5] If high-ability or highly motivated workers are more apt to migrate, failure to control for α_i will result in estimated returns which are biased upward. Assuming the individual-specific disturbance α_i is the only remaining component of the error term correlated with the migration regressors, purging the model of this term results in unbiased parameter estimates. Accordingly, this study relies on a standard fixed-effects procedure to obtain consistent estimates of the impact of migration on wages.[6] The individual-specific component of the error term is eliminated by expressing variables as deviations from person-specific means [Hsiao, 1986]. Consequently, any estimated positive effect of migration on wages found in the fixed-effects model cannot be explained by the hypothesis that the migrant sample is selected from the upper tail of the "ability" distribution while the non-migrant sample is drawn from the lower tail. It should also be noted that several regressors are netted out of the equation during fixed-effects estimation since they do not vary over time (i.e. schooling level, race). In this case, only time varying controls are used to identify the fixed-effects model.

DATA

The National Longitudinal Survey of Youth 1979 (NLSY79) provides a comprehensive data set ideally suited for the study of the long-term wage dynamics associated with internal migration. The NLSY79 began in 1979 with a sample of 12,686 men and women born between 1957 and 1964. Present within the NLSY79 data files is information specifying the state of residence at the time of interview, as well as a detailed longitudinal record of the employment history of each respondent. The sample used in this study is limited to young men due to well-known differences in migration propensity by gender, especially for married women [Mincer, 1978; Greenwood, 1985].

The work history file in the NLSY79 allows one to determine precisely when most workers make a permanent transition into the labor force. By restricting attention to observations contributed by individuals after continuous full-time labor market entry, I am able to circumvent the potentially serious drawback of unaccounted-for migrations (left-censoring). I define the starting date of the working career as the week in which the respondent makes his final exit from full-time schooling. For example, I do not treat attendance at an out-of-state college or moves coincident with graduation as migrations. Respondents are thus excluded if the starting date of the working career cannot be accurately ascertained. This can arise for a number of reasons, namely if (a) the respondent leaves school prior to January 1, 1978, (b) the respondent is continuously enrolled throughout the observation period, or (c) schooling information is incomplete or inconsistent.

Although the NLSY79 provides a weekly timeline of the individual's employment history, no such precise measure exists for residential data. Therefore, some discussion is required as to how interstate migrants are identified within the sample. Because only one residential data point is observed per survey year, I rely on changes in reported state of residence across consecutive survey dates to identify internal migrations. An interstate change of residence is only termed a migration if the move occurs after the completion of formal schooling. In order to enter the sample, a respondent must provide a valid response for state of residence in at least two consecutive surveys in the years immediately following full-time labor market entry. Further, because the state of residence can only be determined as of the survey date, it is of paramount importance that no informational gaps exist when tracking the individual's migratory history. Therefore, only consecutive survey dates from the time of labor

market entry are retained in construction of the sample. I include only the continuous, intact portion of the panel from the time of labor market entry for individuals who are not surveyed all years subsequent to labor market entry. The observations (following the gap) are omitted from the sample. This approach limits the amount of misclassification of individuals by migrant status.[7] In order to focus exclusively on internal migration within the United States, I delete all individuals ever residing abroad or outside of the 48 contiguous states and the District of Columbia over the observation period.

Sample deletions are summarized as follows. The original male cohort of the NLSY79 consists of 6,403 young men. I initially delete the 824 individuals in the military sub-sample and another 124 men who at some point reside abroad. The date of schooling exit cannot be determined for 51 individuals, while another 122 young men either remain in school or exit after the second-to-last interview date. The starting date of the working career occurs prior to the observation window for an additional 836 young men. I also delete 141 individuals failing to report at least two valid wage observations or other pertinent labor market information. Reported hourly earnings less than \$2 or greater than \$100 are treated as outlying observations. These restrictions leave a base sample of 4,305 young men contributing 33,550 wage observations over the years 1979-93 for empirical estimation.

Table 1 demonstrates how migrants' and non-migrants' personal and job-related characteristics differ as well as the overall means and standard deviations of variables included in the wage specifications. For fixed characteristics like race and schooling, the averages are taken over persons; for time-varying traits, the averages are calculated over person-years. Whites (non-black, non-Hispanic) are more likely to have migrated, while Hispanics are clearly less likely to have migrated. Blacks are as likely to have migrated as to have not migrated . On the whole, migrants tend to have more experience but less job tenure. Non-migrants are somewhat more likely to be married, a member of a union, a government employee, self-employed, or working part-time. Average wages are virtually identical across the migrant and non-migrant samples. Overall, the pooled sample includes 850 interstate migrants (19.7 percent of the individuals) contributing 7,438 observations (22.2 percent of the observations) and 3,455 non-migrants contributing 24,193 observations.

ESTIMATION RESULTS

Table 2 presents the results from OLS and fixed-effects estimation of the baseline specification described by Equation (1). The dependent variable is the log of deflated average hourly earnings.[8] Parameter estimates from OLS regression are reported in Columns (1) and (3) of the table. Because individuals contribute multiple observations to the sample and because within-person wage observations are likely to be correlated over time, standard errors for the OLS regression are corrected for both heteroskedasticity across individuals and within-person correlation using a procedure for robust variance estimation [White, 1980; Rogers, 1993]. The parameter of interest in Equation (1) is the coefficient on the post-migration wage observation indi-

TABLE 1
Means of Selected Variables Included in the Wage Models

Variable	Migrants Mean	Migrants Standard Deviation	Non-Migrants Mean	Non-Migrants Standard Deviation	Full Sample Mean	Full Sample Standard Deviation
White[a]	0.607	0.49	0.564	0.50	0.573	0.49
Black[a]	0.266	0.44	0.263	0.44	0.264	0.44
Hispanic[a]	0.127	0.33	0.173	0.38	0.164	0.37
Grade[a]	12.4	2.6	12.5	2.5	12.5	2.5
Experience (yrs.)	4.2	3.3	4.1	3.4	4.1	3.4
Tenure (yrs.)	1.9	2.3	2.7	2.9	2.6	2.8
Married	0.354	0.48	0.387	0.49	0.379	0.49
Health Status	0.030	0.17	0.025	0.16	0.026	0.16
Union Status	0.157	0.36	0.191	0.39	0.183	0.39
Govt. Employee	0.075	0.26	0.094	0.29	0.090	0.29
Self-employed	0.042	0.20	0.060	0.24	0.056	0.23
Part-time Status	0.078	0.27	0.085	0.28	0.083	0.28
Urban Residence	0.175	0.38	0.161	0.37	0.164	0.37
Ln Wage	1.70	0.48	1.69	0.51	1.69	0.47
Observations	7,438		26,112		33,550	
Individuals	850		3,455		4,305	

a. Indicates that the variable's means and standard deviations are tabulated over individuals.

cator (*Migrant*). As human capital theory would suggest, this estimate is positive and significant at conventional levels. The OLS point estimate is measured at 0.050 with a standard error of 0.011, suggesting that on average migrant wages are 5.1 percent higher than the non-migrant benchmark.[9]

As noted, OLS parameter estimates of the return to migration are biased if the time-invariant, individual-specific component of the error term is correlated with the migration regressor. To obtain a consistent estimate of the *Migrant* coefficient I purged this error component from the model using fixed-effects estimation. The results from the fixed-effects model can then be contrasted against the OLS estimate to give some indication of the relative importance of unobserved heterogeneity bias. Fixed-effects parameter estimates and standard errors of Equation (1) are reported in Columns (2) and (4) of Table 2. The fixed-effect estimate for the *Migrant* indicator variable is 0.043 with a standard error of 0.010 indicating a 4.4 percent wage premium. Notice this estimate is somewhat smaller than the OLS result, suggesting the latter is biased upward relative to the fixed-effects model. This bias is ostensibly due to positive correlation between the time-invariant person-specific component of the error term and the migration regressor. Positive correlation between these terms is consistent with the "favorable self-selection hypothesis" discussed earlier, (i.e., those who migrate are more likely to be highly able and more motivated [Chiswick, 1978]). Because greater ability and motivation will result in above average earnings over time, estimates of

TABLE 2
Estimation Results for Baseline Specification (1)

Variable	OLS[c]	Fixed-effects	Variable	OLS[c]	Fixed-effects
Migrant	0.050[a]	0.043[a]	Married	0.109[a]	0.057[a]
	(0.011)	(0.010)		(0.008)	(0.005)
Black	-0.125[a]		Health Status	-0.071[a]	-0.039[a]
	(0.010)			(0.016)	(0.013)
Hispanic	-0.058[a]		Health Missing	-0.063[a]	-0.014
	(0.013)			(0.030)	(0.023)
Grade	0.071[a]		Union Status	0.159[a]	0.118[a]
	(0.002)			(0.009)	(0.006)
Experience	0.035[a]	0.040[a]	Union Missing	-0.012	0.002
	(0.005)	(0.005)		(0.012)	(0.008)
$Exper^2/10$	-0.019	-0.036[a]	Govt. Employee	-0.078[a]	0.001
	(0.011)	(0.008)		(0.013)	(0.011)
$Exper^3/100$	0.003	0.010[b]	Self-employed	0.189[a]	0.157[a]
	(0.006)	(0.004)		(0.023)	(0.010)
Tenure	0.069[a]	0.053[a]	Part-time Status	-0.003	0.034[a]
	(0.005)	(0.004)		(0.014)	(0.008)
$Ten^2/10$	-0.058[a]	-0.054[a]	Urban Residence	0.039[a]	0.023[a]
	(0.011)	(0.007)		(0.010)	(0.008)
$Ten^3/100$	0.017[a]	0.018[a]	Constant	0.653[a]	
	(0.006)	(0.004)		(0.030)	
R^2	0.345	0.134			

Standard errors are in parentheses. Regressions also include a complete set of 1-digit industry (manufacturing omitted), census division of residence (East North Central omitted), and year dummies.
a. Significant at the 0.01 level.
b. Significant at the 0.05 level.
c. Robust standard errors are calculated using the Huber-White estimator of variance with a correction for within-person correlation.

the return to migration failing to control for person-specific fixed-effects should be viewed with caution.[10]

Both the OLS and fixed-effects estimates from Equation (1) indicate a significant positive return to migration. However, this baseline specification has little to say about the timing of these returns. Is the majority of this return forthcoming immediately upon a change of locale? How long do pecuniary rewards accumulate? Do these returns grow or recede over time? Moreover, since Equation (1) treats all pre-migration observations identically with those contributed by non-migrants, this specification does not address the profile of migrant earnings *prior* to the move. The time profile of migrant earnings can be more readily addressed within the context of the more flexible specification embodied in Equation (2). This specification provides a much more detailed picture of the wage path around the year of migration.

Table 3 presents OLS and fixed-effects regression parameter estimates and standard errors for the specification in Equation (2). I report only the parameter esti-

TABLE 3
Estimation Results for Flexible-form Specification (2)

Migration Indicator	OLS[c]	Fixed-effects
2-3 Years Before	0.022	0.009
	(0.017)	(0.015)
1 Year Before	-0.017	-0.031
	(0.016)	(0.017)
Year of Migration	0.032[b]	0.017
	(0.014)	(0.016)
1-2 Years After	0.045[a]	0.037[a]
	(0.014)	(0.015)
3-4 Years After	0.052[a]	0.046[a]
	(0.015)	(0.016)
5+ Years After	0.060[a]	0.048[a]
	(0.016)	(0.016)
R^2	0.344	0.134
F-Test for equality of estimated migration coefficients	3.49	5.74
Prob > F	0.004	0.000

Standard errors are in parentheses. Regressions also include race, education level, experience, experience squared, experience cubed, tenure, tenure squared, tenure cubed, marital status, health, urban residence, union status, government employment, self-employment, part-time status, a complete set of 1-digit industry (manufacturing omitted), census division of residence (East North Central omitted), and year dummies.
a. Significant at the 0.01 level.
b. Significant at the 0.05 level.
c. Robust standard errors are calculated using the Huber-White estimator of variance with a correction for within-person correlation.

mates for the migration indicators, since the estimated coefficients on the control variables are virtually unchanged from the results for Equation (1). Because the fixed-effects procedure controls for unobserved differences between migrants and non-migrants that could bias estimated pecuniary returns, the fixed-effects estimation results are the preferred estimates in this study. Nonetheless, I first present the OLS results shown in Column (1). The insignificant parameter estimate on the *2-3 Years Before* variable suggests little difference in the wages of migrants and non-migrants in the years preceding a move. The negative coefficient on the *One Year Before* variable (-0.017) is indicative a slight decline in the migrant wage level in the year prior to migration. The point estimate, however, is again statistically insignificant with a standard error of 0.016. Beginning in the year of the move, migrant wages increase to a level 3.2 percent above the non-migrant wage. The estimated coefficient on the *Year of Migration* variable is significant at the five-percent level. This premium increases to over 4.5 percent the following two years (*1-2 Years After*), and then to 5.3 percent by the fourth year post-migration (*3-4 Years After*). After 5 years, migrant wages peak at a level over 6 percent higher than the non-migrant benchmark. The point

estimate on the *5+ Years After* indicator is 0.060 and is highly significant with a standard error of 0.016.

Fixed-effects estimates of Equation (2) are presented in the final column of Table 3. The fixed-effects model identifies a very similar trend in migrant earnings, though slightly lower in magnitude than the OLS estimates. Two to three years prior to migration, the wage levels of (future) migrants and non-migrants are virtually identical. The estimated coefficient on the *2-3 Years Before* variable is 0.009 with a standard error of 0.015. Interestingly, the fixed-effect estimates are more supportive of a relative decrease in migrant wages in the year preceding migration. The estimated coefficient on the *One Year Before* variable suggests a 3 percent decrease in real wages with the estimate achieving significance at the 10 percent confidence level. This drop in earnings could in part precipitate the decision to migrate to an alternative labor market. Migrant wages recover in the year of the move as evidenced by the *Year of Migration* parameter estimate of 0.017. The estimate is not significant suggesting equivalence between the migrant and non-migrant wage level. In the following two years, migrant wages increase by 3.8 percent relative to the non-migrant level. The point estimate of 0.037 on the *1-2 Years After* indicator is highly significant with a standard error of under 0.016. In the third and fourth years after migration, this wage premium increases to over 4.8 percent. The *3-4 Years After* parameter estimate of 0.046 is again highly significant with a standard error of 0.016. Migrant wages reach their relative peak after five years nearly 5 percent higher than the non-migrant benchmark level. An F-test rejects the hypothesis of equality of estimated coefficients across all of the migration indicators at the one-percent confidence level.

The fixed-effects estimation results for Equation (2) demonstrate a remarkable pattern of migrant earnings. Starting at wage levels virtually identical to non-migrants, migrants initially experience an approximate 3 percent drop in hourly earnings in the year *prior* to migration. Wages recover then to their previous level during the year of migration (across locations) and begin to rise steadily against the non-migrant standard over the next five years. After five years, the wages of interstate migrants peak at a level nearly 5 percent higher than their immobile counterparts. Borjas, Bronars, and Trejo [1992a] report a similar trend in migrant earnings in a sample taken from the NLSY79. Their results suggest about a six-year period of migrant assimilation during which time migrants demonstrate superior earnings growth relative to non-migrants. However, in contrast to the findings in this study, they document an initial migrant wage penalty following migration. Moreover, their estimates imply that migrant wages remain consistently below the native level throughout the post-migration period. This divergence in results may be explained in part by differences in empirical methodology and sample construction. Borjas, Bronars, and Trejo [1992a] only estimate a cross-sectional wage equation using labor market information from the 1986 survey, whereas the current study utilizes an extended panel of yearly wage observations. Further, their estimates are more apt to suffer from some of the potential misclassification biases highlighted in this study, given the definition of migration employed in constructing their sample.[11]

The profile of migrant earnings captured in Equation (2) also highlights the need for an extended panel of data to measure pecuniary returns. In the fixed-effects model, neither of the estimated coefficients on the *2-3 Years Before* or *Year of Migration* indicators are statistically significant, while the *One Year Before* estimate is only marginally so. This suggests that earnings differ little between migrants and non-migrants in the immediate time period surrounding migration. By measuring pecuniary returns via simple wage differences across this short observation period, results would most likely indicate little if any positive return to migration. This is consistent with the lack of significant positive findings in studies examining contemporaneous earnings changes associated with migration. The reason, of course, is that the bulk of the migratory rewards do not take effect until several years post-migration.

CONCLUSIONS

This study has demonstrated that young interstate migrants receive significant positive returns to migration. Estimating a more flexible model than those used in previous work on the returns to migration, this study shows that pecuniary rewards generally accumulate over a five-year period following migration. Over this time frame, migrants demonstrate superior wage growth relative to non-migrants, culminating in nearly a 5 percent wage advantage. This finding of superior wage growth in the early post-migratory phase is consistent with the notion that migrants invest heavily into a new stock of location-specific capital following migration [Shaw, 1991]. This is most likely due to the high marginal return on such investments at low (initial) levels of location-specific capital stock in the years immediately following a move. Borjas, Bronars, and Trejo [1992a] document a similar trend in migrant wages over this post-migration time period. However, in stark contrast to their results, I find no evidence of a migrant wage penalty in the years just following a move; migrant wages are found to be consistently above the non-migrant level throughout the post-migration observation period.

These results are generally robust across specifications and methods of estimation. Fixed-effects regression techniques were used to limit unobserved heterogeneity bias in estimation results. Of particular interest, this paper demonstrates that OLS estimates of the return to migration are upward biased relative to a fixed-effects model. This is consistent with the "favorable self-selection" of the internal migrant stock, whereby more highly able or motivated workers tend to be the most apt to migrate [Gabriel and Schmitz, 1995]. It is important to note, however, that the fixed-effects procedure accounts for correlation between migration and unobserved, time-invariant individual characteristics typically referred to as "ability." Therefore, the estimated positive effect of migration on wages found in the fixed-effects model can not be explained by the hypothesis that those who migrate are more highly able.

NOTES

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1. Internal migration within the United States is typically defined as the movement of workers across well-defined jurisdictional boundaries (counties, states, census divisions, etc.) but within national borders [Greenwood, 1997].
2. Life-cycle factors also play a prominent role in this framework. Factors such as age, education, labor market skills, and family composition affect individual perceptions of the costs and benefits of migration. Differences in amenities (for example, climates, crime rates, or pollution) and cost of living must also be factored into this figurative cost-benefit ledger [Gabriel and Schmitz, 1995].
3. Critical to the human capital model of migration is the assumption that economic factors are an important source of motivation for migration. A non-exhaustive list of studies providing empirical support for this proposition include Nakosteen and Zimmer [1980], Falaris [1988], and Borjas, Bronars, and Trejo [1992b].
4. Heckman and Hotz [1989] point out that simultaneity bias is likely to arise for one of two reasons: either due to selection on observables or selection on unobservables. Fixed-effects regression techniques can handle the latter if the unobservables are assumed to be a person-specific, time-invariant component of the error term. To control for selection on observables, Heckman and Hotz propose using a control function estimator. Since the factors related to simultaneity are in fact observable, construction of the control function is tantamount to simply entering the variables in question into the specification. The important point is that failure to control for these observables in the regression produces biased parameter estimates.
5. This hypothesis asserts that the migrant stock is primarily composed of those individuals possessing greater innate "ability" and/or "motivation" than their immobile counterparts. Assuming that earnings are highly correlated with ability and motivation, the migrant pool should then be drawn from the upper-tail of the earnings distribution. Gabriel and Schmitz [1995] test this proposition by regressing pre-migration wage levels on a set of standardizing characteristics and a dummy variable indicating a future migration. Consistent with the hypothesis, they find that future migrants earn a premium above non-migrants.
6. One could also treat the individual-specific component of the error term as a random effect and use an instrumental variables procedure to handle the correlation between this component and the migration regressors. However, because obtaining valid instruments is a contentious endeavor, I simply choose to purge this component of the error term directly by deviating observations from within-person means. The drawback of the fixed-effects procedure is that the results are less efficient than random-effect GLS estimation [Hsiao, 1986].
7. It is conceivable that (unreported) multiple interstate moves occur between survey dates. However, given the overall infrequency of multiple interstate migrations in the sample, this is likely to be a very limited occurrence.
8. Because the measurement of pecuniary returns are likely to be influenced by variation in the cost of living across locations, some care must be taken to separate true pecuniary returns from cost-of-living pay differentials. Workers searching for alternative employment opportunities may be more apt to migrate from low to high cost-of-living areas if such areas offer an increased density in available jobs. For example, job prospects may be significantly improved by migrating from a rural locale to a large metropolitan area. Accordingly, all wages are deflated using either regional-specific price indices (I use separate indices specific to each of the four major census regions) or, if available, price indices for Standard Consolidated Statistical Metropolitan Areas (CSMA). More detailed information on these price indices are available from the author upon request.
9. Percentage returns are calculated using the formula $e^{\gamma} - 1$.
10. It is also possible to test whether the fixed-effects specification is warranted by the data. An F-test firmly rejects the null hypothesis of an overall constant term in lieu of person-specific intercepts further commending the appropriateness of the fixed-effects model. This test statistic is reported as $F(4304,29198) = 8.33$.

11. Borjas, Bronars, and Trejo [1992a] classify individuals as migrants if the state of residence reported in the 1986 interview differs from the state of residence reported *at age 14*. The major concern here is that many of these migrations take place prior to entry into the labor market. Therefore, some individuals labeled as migrants will have accumulated all of their labor market experience in only one location. For example, an individual's parents may have located in a new state while that individual was still in high school, or migrations may be coincident with attending an out-of-state college. Moreover, respondents in the NLSY are as old as 22 in the initial year of the survey (1979). As a result many individuals are likely to have significant gaps (between the year in which they turned age 14 and the initial survey date) for which *no* residential data is available. In such cases, the authors impute the probable state of residence for the entire period for which data are unavailable.

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