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THE ROLE OF EQUILIBRIUM AND DISEQUILIBRIUM IN MODELING REGIONAL GROWTH AND DECLINE: A CRITICAL REASSESSMENT

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ABSTRACT. The assumption of interregional equilibrium in migration research has recently been attacked. At issue is the motivation for on-going migration if rents and wages accurately compensate for spatial amenity variations; but if rents and wages fail to accurately compensate potential migrants, then amenity valuations must be flawed. We here show that arguments supporting substantial disequilibrium in the U.S. economy are unconvincing. The substantive issues are then clarified by a model which allows for both equilibrium and disequilibrium migration. We conclude that intertemporally *systematic* migration stems predominantly from equilibrium forces.

1. INTRODUCTION

In Evans's (1990) recent study, the plausibility of the assumption of interregional equilibrium in recent research on migration and amenity valuation is called into question. Specifically, Evans argues that explanations for continuing net migration in a world characterized by equilibrium are unsatisfactory. Moreover, he asserts that recent research suggests the United States economy is in disequilibrium. He then argues that the presence of substantial disequilibrium introduces (perhaps quantitatively important) bias into existing valuations of amenities which emerge from equilibrium models. We here argue that Evans is unconvincing in his primary substantive argument; we provide an alternative reading of the literature that supports this assertion.

In Section 2 we consider the apparent inconsistency in the literature regarding the role of equilibrium and disequilibrium assumptions. The issue is how one can expect ongoing migration if property values and wage rates accurately compensate for variations in attractiveness across space; if property values and wage rates fail to accurately compensate potential migrants, then amenity valuations will be flawed. We reproduce the section headings of Evans here as subheadings to facilitate a comparative reading of our papers. Section 3 concludes the paper,

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illustrating the substantive issues in terms of a simple formal model that allows for both equilibrium and disequilibrium migration.

2. EQUILIBRIUM VERSUS DISEQUILIBRIUM

As an abbreviated overall critique, Evans builds to his conclusion that the world is largely characterized as being in disequilibrium on the basis of a series of apparently compelling—but faulty—steps. Throughout his discussion, Evans frequently focuses on empirical phenomena which have their roots in the heterogeneity of individual preferences or incomes. Yet he consistently ignores heterogeneity. For example, some of his criticisms of the equilibrium model hinge on the observation of ongoing migrant flows—yet those flows are to be expected in a world of nonidentical people. In a closely related vein, Evans also ignores the role of endogenous disamenities and downplays important life-cycle effects. Evans brings into consideration the role of rent compensation rather late (in his Section 6), yet it is central to the equilibrium arguments and it is directly relevant to much of his earlier discussion. Finally, he does not appear to be appropriately concerned about the time frame over which equilibrium is assumed. We believe that the lack of a formal model of the way that equilibrium and disequilibrium structures interact is largely responsible for Evans's errors in interpretation.

We now take up his arguments seriatim.

An Equilibrium Model. In his Section 2, Evans presents a brief description of the equilibrium notions as spelled out in the recent literature. There is nothing controversial in this section, and Evans accurately characterizes the equilibrium model. He argues (p. 518) that "people or households cannot gain by migrating, since higher amenity values in one area will be compensated by lower wages and/or higher property values." Moreover, "since households are freely mobile across areas, differences in 'the level of well-being,' for the purposes of the analysis, can be assumed not to exist at all."

Evans argues that this model cannot possibly provide a useful characterization of the observed population system since it requires that firm capital, housing capital, and workers must respond instantly to differences in relative prices. Given the implausibility of the model, he then suggests amenity valuation techniques that assume equilibrium must produce flawed estimates of amenity valuation.

The claim of implausibility, we believe, fails to recognize that the critical issue is not whether the assumption is literally true, but rather whether it is a close enough approximation to the truth to produce useful implications and methods of analysis. Simplifying assumptions are not only necessary but *desirable* if models of sufficient simplicity to generate predictions are to evolve. The fruitfulness of assumed instantaneous adjustment in supply and demand analysis more generally suggests that such a modeling approach might be a useful first start in regional modeling as well.

It is further worth noting that adjustment speed is best gauged relative to the extent of variation in the economic environment over time. In a fully stable environment, the system will be close to equilibrium even if adjustment is slow in

absolute terms. Where location opportunities vary dramatically over time, even highly responsive migration may not be sufficient to keep the system near equilibrium. Hence, whether adjustment is rapid enough to be viewed as "instantaneous" is an empirical question that depends on both the adjustment speed and the environment.

Studies that attempt empirical estimation of amenity valuations often recognize the possibility that the observed system may deviate from equilibrium. For example, in Roback (1982) population growth is included as an indicator of the extent of disequilibrium. Hence in practice these authors hold constant the extent of disequilibrium in the development of their equilibrium amenity valuations. We are somewhat surprised that Evans does not note this, since the repeated attacks on the estimates based on the equilibrium model (e.g., Evans, pp. 517, 520, 521, 529, 530) are largely deflated by this observation. Since defending the empirical equilibrium analysis is only peripheral to the concerns of the present paper, we shall say little further about this oversight. Plausibility like beauty is, of course, in the eyes of the beholder. Evans's central concern is reconciling the observed migration flows with the assumption of equilibrium, a topic he turns to in his Section 3.

Kinds of Equilibrium. Evans is quite clear when he argues (p. 519) "if the above model was correct then *either* net migration between regions would be virtually zero *or* some theoretical explanation must be found which would reconcile equilibrium and continuing net migration." We agree with Evans when he argues that the analysis of Schachter and Althaus (1989) fails to convince the reader that net flows are "close to zero." Schachter and Althaus are correct in claiming that the high observed correlation between migrant inflows and outflows is to be expected when the system is in equilibrium.¹ Yet, we would argue that such high correlations say more about heterogeneity of the population (in terms of income and tastes) than they do about equilibrium.

Consider a world in which people are heterogeneous in their preferences for amenities. As people generally move into desirable areas over time, perhaps due to the normal or superior nature of amenities as emphasized by Graves (1979), they will drive rents up and wages down. But some original occupants, not possessing such high amenity demands, will be increasingly "priced out" of the desirable areas. Not only will such individuals be paying more for the amenities than they are worth to them, but they will possess a portfolio containing too high a proportion of their wealth in the form of housing. Some of these people will move to other cities in the urban system (e.g., going from Los Angeles to Seattle), buying less expensive locations and "cashing out" of homes in the more desirable locations.

The possibility that net growth or decline at a location may cause both in- and out-migration to increase is also suggested when one incorporates endogenous amenities and disamenities into the analysis. As desirable areas receive in-migrants

¹The way that the equilibrium-seeking character of the population system induces such a positive correlation is modeled in Mueser and White (1989).

and become larger, some amenities take on higher levels (e.g., goods diversity, museums) while others take on lower levels (e.g., those affected by congestion, pollution). The latter will be expected to drive out those households that are sensitive to such disamenities. Hence, one should expect migrant flows, potentially fairly large, from—as well as to—the desirable locations.

Our point is that the existence of large, opposing flows of migration does not imply that net migration is unimportant. The processes we have described suggest that such flows may be the *result* of net growth or decline. Hence, we agree with Evans that net migration flows “are substantial and persist.” We disagree very strongly, however, about what this says, if anything, about the degree of disequilibrium in the spatial system at a point in time.

Evans pursues three possible explanations for how continuing net migration can be reconciled with continuing equilibrium. His first possibility, the life-cycle argument, is that a static equilibrium is maintained in the presence of movements first from city center, then to suburbs, then to retirement centers over the life cycle. In a steady state system, the children (labor force entrants) return to the city at the same rate that families with small children are leaving for the suburbs and the elderly are dying at the same rates that the new elderly are arriving. Hence, one could observe continuing migration with little or no change in the rent or wage compensation across locations.

Evans correctly points out that the homogeneous preferences assumed by Blomquist, Berger and Hoehn (1988) may result in wage and rent compensation which will not provide valid amenity values for all households. But how important is this criticism? It is obviously the case that a person having unusual preferences relative to the dominant majority that determine rent and wage compensation gets a utility gain—real spatial consumer surplus—for being different. But, as long as there are people of all three types (new workers, suburban families, retirees) in *all* areas in the spatial system, it must be the case that some individuals in each group receive the same level of satisfaction in all areas. Unless preferences differ drastically within subgroups, the “average compensation” is unlikely to differ greatly from that required to equalize utility for marginal individuals in any large subgroup.

In a sense, Evans’s criticism is merely a criticism of failure to disaggregate the hedonic analyses sufficiently to gain insights into the values of disparate groups in society. But, this does not seem too important—for any practical policy (say cleaning up pollution) the use of “average preferences” rather than accurately measured individual preferences may make very little difference. A related critique of the equilibrium amenity valuation studies that could have been made by Evans, but was not, is that the 16 amenities included in Blomquist, Berger and Hoehn (1988) are far from exhaustive: the more unusual the preferences of a particular person, the more the hedonic valuations of locations will be flawed for that person.

Evans’s criticism is correct if one’s interest lies in understanding the valuations of amenities by heterogeneous groups. However, Evans makes an explicit error in arguing that “workers would find wages too low and property prices too high at the retirees’ locations, while retirees would find property prices too high at

the workers' locations" (p. 520). The last half of the quoted passage is correct, but the first half is not. If appreciable numbers of workers are found at all locations, wages, amenities, and rents must compensate for one another; whereas retirees (at least those who migrate at retirement) may well be concentrated in locations which are strictly preferable for them. It is workers who are the "marginal" migrants, and it is their preferences that are equalized (see Graves and Waldman, 1991, for further detail).

Evans's second possibility is that the regional system is in a dynamic equilibrium in which different regions are growing at different rates. This is precisely the notion of "moving equilibrium" first presented by Graves (1979).² The level of well-being in the growing regions must exceed the level of well-being in the declining regions, but the utility differential must be small enough to be negligible. Evans (p. 521) feels that "this does not seem acceptable either." He argues that you cannot have it both ways: if utility differences are small enough to ignore in determining quality-of-life, how can they be large enough to stimulate costly household migration? He cites Greenwood's (1985, p. 535) argument that "households must typically overcome a high degree of inertia before they make a decision to move."

A problem with this argument is that it implicitly assumes that movement costs are high for all households when it is quite clear that movement costs for some (the marginal) households are quite small. And it is precisely those marginal households that will be determining the compensation that generates the ubiquitous utility level. The example of the freeway in rush hour is instructive: many drivers are averse to changing lanes (migrating), but the movements of the few guarantee that all lanes move at roughly the same speed (constant utility). There are many times when a movement is quite natural, especially upon leaving college or high school. As long as the preferences of the movers do not differ dramatically from those of the stayers, they may all be in equilibrium even though 80 percent of them never move at all.

The second difficulty Evans sees with the moving equilibrium argument is that although the equilibrium argument may be plausible for economic differences, "it is difficult to sustain if one is considering climatic and environmental differences, since these do not change as regions grow" (p. 521). He argues that the common observation that desirable climates stimulate in-migration (see Graves, 1979; Cushing, 1987) is inconsistent with the equilibrium model: "But, of course, if

²The Graves approach was similar in spirit to the earlier work of Borts and Stein (1962). Those authors had movement by agricultural labor driving the growth of urban areas, in a world where the urban demand curves for labor were quite flat. Hence, like that of Graves, their model implied that growth was driven by labor supply. However, we are not sure what position they would take on the issue of disequilibrium/equilibrium in the system. Our suspicion is that a persistent, and perhaps reasonably large (even adjusted for cost-of-living) differential in utility may have existed to stimulate those flows. Information about various locations was certainly much less readily available during the periods of greatest agricultural outflows (higher moving costs and inertia). Also, retraining costs associated with rural to urban movement would certainly have been large due to the low transferability of farming to urban skills. Since most movements today are among urban regions, in a much more spatially informed world, these arguments lose considerable force.

the system were in equilibrium, these climatic differences would have been compensated by differences in wage levels or housing costs. After all, it cannot be argued that climatic differences vary in an unpredictable manner, so that the migration that is observed is because the system is continually adjusting to changes in climates" (p. 521).

The preceding apparent difficulty was precisely the motive underlying the Graves (1979) paper [see also the more formal Graves and Linneman (1979), or Graves (1983) which briefly presents the argument, referring to the other papers]. As stated in Graves (1983, p. 542), "In brief, as household incomes change over time, amenities are like hamburger or caviar in that some are inferior and some superior. With average incomes rising, one would expect net movement to locations offering a normal or superior bundle of amenities." Evans ultimately acknowledges this mechanism, which we consider central, but only as the basis for a criticism. He suggests that it is a serious problem that if preferences gradually change over time, measures of the quality-of-life in different areas can only be relevant at a point in time and are systematically changing. Yet we would argue that this should not be viewed as a problem. The relative value of one location among many is not expected to be any more stable than the relative value of one grocery item among many.

Evan's primary criticism of this model makes very little sense to us. He argues (p. 522) that "There is a major objection to this argument, i.e., it extends an intra-urban model to the analysis of interregional differences. It is plausible to assume that if amenities are normal or superior goods, then, within an urban area, households with higher incomes will tend to locate in areas with higher-quality environments, and that households whose incomes increase will tend to move into neighborhoods with better environments. But it does not seem plausible to assume that this will occur interregionally." Evans's reason for thinking the amenity superiority argument is implausible stems from a mistaken belief that it implies that "one would find high-income households locating in one part of the country and lower-income households located in another part of the country" (p. 522). That average incomes are not unduly dissimilar among locations is taken by Evans as evidence that refutes the Graves argument.

In fact, there is nothing in Graves's argument that suggests that income distributions should be dissimilar among locations. We wish to make two theoretical observations which lead to some empirical implications. First, in the production of goods, high-skilled (hence high income) individuals tend to work in positions where they are complementary with low skill (and thus low income) individuals. Hence, the labor market will prevent the kind of segregation by income across regions that Evans believes will result from Graves's model. Second, in the consumption of goods, the high income individual will demand locally-produced goods (eg., pumped gas, maids) that have a low-skilled labor component.

The empirical implication of the preceding observations is that in desirable areas the wages of the low income occupants may not be lower than elsewhere—they may well be *higher*. This follows from the fact that all households compete in the same land market. The bid-rents of those with high incomes will tend to drive rents up to where they exceed the value of the amenities present to the poor. Hence,

the poor will have to be compensated with higher wages to make up the difference.³ It is likely, then, that the entire income distribution is shifted up, relative to the national average, in nice areas, while the distribution around the mean might be quite similar among areas. Those with high incomes in the nicest places will have lower incomes than they would have elsewhere, but because they are the highest bidders they may likely have incomes exceeding the typical high income person in less desirable areas. The latter will, of course, have incomes that "look" larger than they are because of the positive wage compensation needed to keep them there.

Whether one can *measure* human capital with sufficient accuracy to verify these conjectures is debatable. A principal problem is that the wage compensation that is occurring everywhere will differ among skill groups, since different skill groups compete in the same land markets in all locations. In any event, a testable implication of the preceding points from theory is that skilled wages relative to unskilled wages should be somewhat lower in the more attractive areas.

Interestingly, Evans notes (p. 523) that, "The evidence suggests, however, that, if anything, wage levels differ less for high income workers than for low-income workers." This is, ironically, presented as evidence against the hypothesis that amenities are normal or superior goods; in fact, as is clear from the preceding, this quoted passage actually is consistent with the equilibrium hypothesis. If low skill workers in desirable areas have higher wages to compensate for the fact that their amenity advantage is *more* than compensated for in a rental equilibrium that involves high rents, they will have wage levels that differ relatively greatly over space. The rich with the highest skill levels will tend to occupy the very nicest places and will be paid less than their skill levels would receive in less desirable locations. The rich in less desirable locations will generally be those with less human capital, but receive compensation in the form of higher wages for the less desirable locations. The net effect, in light of the difficulty of observing accurately measured skill levels, is that variations in wages for the rich may be smaller.

Evans notes (p. 522) that "Graves (1983) also ignores the possibility that interregional differences will be capitalized into wages as well as property values," indicating in a footnote that more recent work (Graves and Knapp, 1985; and Knapp and Graves, 1989) "has recognized this possibility." This does not correctly represent the literature. One of the principal findings in Graves (1979) was that, in failing to control for amenities, migration researchers were frequently finding "wrong" signs for the coefficient of the income variable. It was argued there that measured income was taking on the wrong sign because people were being compensated in higher wages for disamenities. Hence, if amenities are normal, one would expect movement toward *low*-income locations—what in fact was observed.

³Actually, wages must be higher by the amount rents exceed the amenity value to the poor or by the amount of commuting costs to less desirable, lower rent areas. For example, most low-skill employees in Aspen, Colorado live in Basalt or Carbondale. Moreover, while not central to our main points, the rich will be buying relatively fewer goods with a significant low-skilled labor component since these goods will be relatively more expensive in the desirable areas.

Including amenities in the equations stripped the compensation component from the variation in incomes, making income variations more likely to correspond to real utility differentials.⁴

The argument applies similarly to rent. If rent can proxy for amenities, holding rents constant will allow income variations to come closer to representing what they were intended to represent, utility variations to be acted upon. In the interests of brevity, Graves (1983) did not restate the argument.

Indeed, it was not the compensation in wages that was originally ignored in the rest of the migration literature, but rather the role of rents. Until Graves (1983), the migration literature viewed rents primarily as cost-of-living variables to be held constant. There was little understanding of the role that rents would play in establishing spatial equilibrium in the presence of amenity differences. As noted by Roback (1988) and Graves and Knapp (1985), where rents fill this role, they represent not cost-of-living variation but rather *benefit*-of-living variation!

The existence of variation in production amenities over space means that sometimes rents are high because of production agglomerations, but in these instances equilibrium guarantees that wages will be compensatingly higher. The potentially opposing effects of "correcting" for rents, in a world where both production and consumption amenities are important, may be the reason that researchers do not find that controlling for cost-of-living in the wage or income variable makes much difference in migration studies.

In sum, our view of the issues raised in Section 3 differs from that of Evans. He argues that "the assumption that interregional equilibrium coexists with consistent and continuing patterns of interregional migration is difficult to sustain and open to theoretical objections" (p. 523). We believe that ongoing migration is fully consistent with an equilibrium in which differential desirability across locations at any one point in time is compensated by wages and rents. Indeed, theory argues strongly that net migration is necessary to maintain equilibrium—to argue otherwise denies that amenities can be normal or superior like ordinary goods. If amenities are normal or superior, then migration must occur if individuals are to consume larger quantities as their incomes increase.

Evans next turns to an examination of recent research which he feels relates to the equilibrium/disequilibrium issue, claiming there is little support for the equilibrium argument.

A Review of Some Recent Evidence. In this section, Evans discusses papers by Dickie and Gerking (1987), Goldfarb and Yezer (1987), Topel (1986), and Carlino and Mills (1987). After discussing Dickie and Gerking at some length, Evans concludes (p. 524) that "it is doubtful whether their results support either the equilibrium or the disequilibrium side of the discussion." In this we agree, although Evans has, we believe, misinterpreted their statistical tests. They perform a Chow test which fails to find statistically significant differences in the

⁴Of course, insofar as income is derived from investments or transfers that are not tied to location, this will bias empirical results.

equations predicting wages based on personal characteristics for the four major regions of the United States. Contrary to Evans's view, this is a test for all parameters fitted in the equations. This analysis takes seriously heterogeneity in the population, showing that the processes determining earnings do not appear to differ across regions. However, we do question the power of this test, and are concerned about the omission of any measure of rent from the specification, and for these reasons we view their results as inconclusive.

The Goldfarb and Yezer study assumes, as quoted by Evans (p. 525), "that the more skilled occupations are more likely to be in interregional equilibrium than the unskilled, so that differences between regions in the wage rates paid to skilled workers are more likely to reflect amenity and cost-of-living differences. They, therefore, subtract the predicted wage rates for each of the groups of skilled workers from the wage rates paid to unskilled workers." They interpret this difference as a measure of disequilibrium in the unskilled occupations. It is far from clear to us that those in skilled occupations are more likely to be in interregional equilibrium than are unskilled; that markets are thinner for skilled labor can account for the greater average distance of moves of the more highly educated. Goldfarb and Yezer find that skilled wages are more equal across regions, but heterogeneity of skills, combined with common bidding in the land market, as discussed above, make it difficult to draw conclusions from the analysis conducted by Goldfarb and Yezer. As quoted by Evans (p. 525), Goldfarb and Yezer conclude that "overall the results are not consistent with the hypotheses that observed nominal wage differentials for the three higher-skill groups reflect equilibrium variations and that unskilled blue collar wage differences have been converging to eliminate real wage differentials." Since we do not believe that anything about the equilibrium model implies that the unskilled wage differences should converge, it is difficult to evaluate Goldfarb and Yezer's conclusion.

Throughout this section, Evans fails to recognize that the existence of the moving equilibrium notion of Graves does not preclude the possibility of other forces influencing migration as well. This observation has important bearing on the class of questions that a model is intended to answer, namely whether the model is intended to cast light on very short-run variations in migration (e.g., year-to-year variations) or more long-term variations. Employment opportunity varies dramatically over relatively short periods, so that deviation from equilibrium may well occur as a result of such variations. Hence, migration to an area over the very short run may be largely a function of employment opportunity.

What is quite notable in this context is that the impact on long-term population redistribution of fluctuations in employment opportunity may be quite modest if such changes are not positively correlated over time. Areas that grow due to economic opportunity in one period may not grow, or may even decline, in successive periods. Empirical analyses which focus on migration over relatively short periods may fail to capture the full impact of amenities. Indeed, the empirical studies cited by Evans as showing the importance of disequilibrium tend to focus on employment effects and often consider relatively short migration periods. Topel, for example, considers annual changes (as do Greenwood and Hunt, 1989,

discussed earlier by Evans), showing quite plausibly that net migration variation from year to year is driven by disequilibrium employment shifts. Topel also does not include rents in his analysis, which makes his results difficult to interpret in terms of the equilibrium framework.

Carlino and Mills consider county migration over a decade, and purport to show that movement toward equilibrium is slow even over that period. Evans correctly repeats their conclusion that population redistribution for a decade only amounts to 16 percent of the difference between observed population and the long-run equilibrium population. The method of analysis used by Carlino and Mills suffers from a very simple but serious problem: It assumes that measured variables fully determine the ultimate equilibrium population. If there are any unmeasured stable differences between locations that determine equilibrium population, this imparts a systematic bias that will reduce the apparent speed of movement toward equilibrium. In fact, it seems highly likely that there are many omitted factors. For example, the analysis includes no measures of natural amenities. Similarly it does not include land rent, which is a critical equilibrating variable in the equilibrium view. Nor does the analysis include the additional wide variety of location-specific factors that draw migrants and employers to a locale (e.g., presence of a deep water harbor, established cultural institutions, the presence of a hinterland in need of commercial services). The results of Carlino and Mills are likely to reflect the omission of important factors influencing equilibrium population.

In Evans's footnote 8 (p. 526), he argues that the Carlino and Mills results complement those of Wheat (1986). Evans notes that Wheat "found manufacturing growth in the United States to be oriented towards markets. Manufacturing employment growth was positively correlated with distance from the main manufacturing areas of the northeastern section of the United States and also tended to grow faster in areas where the level of disposable income was high compared to the existing level of manufacturing employment." These results are, of course, exactly what would be predicted by the moving equilibrium model—as high income people move to areas with greater amenity levels, manufacturing growth will tend to follow to minimize transportation cost to the newly-emerging high-income locations.

Interregional Differences. Section 5 of Evans largely repeats earlier assertions—and as with earlier sections ignores the role of heterogeneity of people-types and rent capitalization. For example, Evans writes (p. 527) "Suppose that a region has a favorable environment and that people are migrating to this region. The evidence suggests that wages in the local labor market would be higher than the equilibrium level." This is not necessarily true at all; wages may well be close to their equilibrium level but rents could be lower than the equilibrium level.

Migration and Rents. In his Section 6, Evans turns to the relationship between migration and property values or rents. In this section, Evans argues that the explanation for the relationship given by Graves (1983) and Knapp and Graves (1989) on the moving equilibrium model is unconvincing. He notes further that "it

is not difficult to provide another explanation for the positive correlation between in-migration and rent levels once it is recognized that the regional economics are not in long-run equilibrium." Evans motivates his argument by the experience of Aberdeen in which house prices and rents were low prior to the discovery of North Sea oil, with prices leaping up after that discovery until they were as high or higher than those in London. Hence, Evans argues that the clear relationship between migration and rents is one where high in-migration causes high rents rather than vice-versa, as suggested by the equilibrium approach of Graves, et al.

There are two serious flaws in Evans's argument, in terms of relating it to the work of Graves. First, in the Aberdeen example he fails to hold wages constant. It would be quite unlikely that the large employment influx into that area could have occurred at the pre-existing wage rates, for otherwise in-migrants would have come in the absence of the oil discovery. The Evans approach is somewhat like observing low car prices in a recession, combined with low car sales, and high car prices in a boom period, combined with high car sales—is he willing to conclude from this that demand curves for cars are upward-sloping? Graves holds constant income when he finds that people are moving toward high rent areas. Moreover, the rent levels employed by Graves (to avoid the well-known bias discussed by Greenwood and Sweetland, 1972) are *beginning of period rents*. It is very difficult to imagine an immigrant flow, holding income constant, that causes rents to rise *before* the arrival of migrants.

It is certainly the case that positive intertemporal correlations in in-migration "for whatever reason" (p. 529) could cause even beginning of period rental data to be correlated with in-migration in a way that would make it appear that high rents caused in-migration when in fact in-migration in prior periods caused rents to be high. But two questions are raised at this point: First, what is the model that generates the "whatever reason" that does not affect income (which is being held constant by Graves)? And, second, why would housing suppliers not adjust smoothly, supplying housing at roughly constant cost, to in-migrants if the positive intertemporal correlations in in-migration are long-standing? By way of example, the annual influx of 700,000 in-migrants into California has led to a thriving competitive construction industry to provide housing, housing whose cost is higher due largely to higher land costs, not construction disequilibrium.

3. CONCLUSIONS

In his concluding Section 7, Evans reiterates his position that the equilibrium assumption is untenable, and to the extent that it is so, amenity values are systematically underestimated in growing areas and overestimated in areas in decline. We find the equilibrium assumption plausible, in the sense that the spatial system is not likely to be far from a general equilibrium at a particular point and that those disequilibrium influences which exist may well be nonsystematic, largely white noise from the intertemporal perspective of relevance. That the values of amenities are to be expected to change over time as incomes rise and people move around is hardly surprising—but this merely argues for periodic updates in those values based on the latest wage-rent compensation data.

Our view concerning use of the equilibrium approach can be illustrated with a very simple model based on the treatment in Mueser and Graves (1991). This model, in contrast to most in the literature, admits the possibility of both equilibrium and disequilibrium processes. It therefore allows us to compare equilibrium and disequilibrium approaches within a unified framework. In the interest of simplification, the model does not incorporate population heterogeneity. While our discussion above makes it clear that heterogeneity is important in understanding a variety of empirical phenomena in migration, the principal issues of population equilibrium and movement toward it can be adequately addressed in a model that assumes identical agents.

As Evans notes, the equilibrium model posits that wages and rents adjust so that utility and cost of production are equalized across all locations. Following Roback (1982), specify an indirect utility function to depend on wages w and rent r in a location, as well as natural consumption amenities, indexed by a . Similarly define a cost function for a composite commodity produced at all locations, also depending on location-specific rent and wages, and production commodities, the latter indexed by b . The equilibrium model implies

$$(1a) \quad U(w_t, r_t, a_t) = U^*$$

$$(1b) \quad C(w_t, r_t, b_t) = C^*$$

where U^* and C^* are common values of utility and cost for all locations. Viewed in terms of a single location, the equilibrium number of households is determined by the land market. The population must be such that at equilibrium prices specified by Equation (1), the total demand for land by firms and households $D(\cdot)$ must equal that available at the location L

$$(2) \quad D(r_t, w_t, N_t) = L$$

where it is reasonable to take $D_1 < 0$ and $D_3 > 0$, and possibly $D_2 > 0$.

Disequilibrium would imply that Equation (1) was not satisfied at all points in time. It is natural to assume that the greater is the deviation of realized utility from the target utility, the greater will be net migration. Similarly, the deviation of costs from the equilibrium general cost level would indicate employment growth (or decline) in the area. Specifying net migration as $\alpha[U(w_t, r_t, a_t) - U^*]$ and employment growth as $\beta[C^* - C(w_t, r_t, b_t)]$, and ignoring for simplicity issues of labor force participation and unemployment (so that each household provides one worker), dynamic consistency requires that these be equal

$$(3) \quad \alpha[U(w_t, r_t, a_t) - U^*] = \beta[C^* - C(w_t, r_t, b_t)]$$

In conjunction with Equation (2), this fully specifies levels of net migration at a point in time in a particular location.⁵ It is clear that if parameters are constant, the system tends toward the stable population equilibrium specified by Equation (1).

⁵It should be noted that (1) corresponds to the concept of equilibrium used by Evans and adopted here. However, condition (3) may be interpreted as identifying a "dynamic equilibrium" derived from

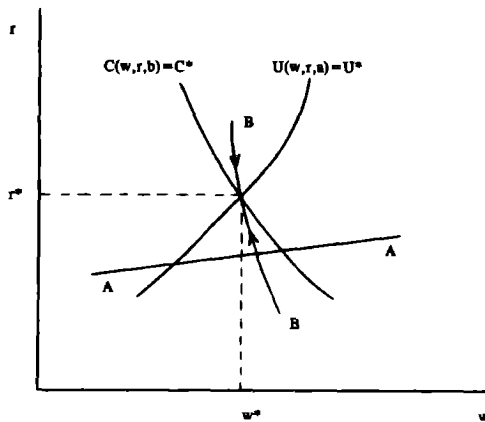


FIGURE 1. Model Dynamics.

Figure 1 illustrates the dynamics of this model. The intersection of the curves for constant utility and constant costs identifies the long-run equilibrium (w^*, r^*) . Following from Equation (2) are a family of curves identifying combinations of wages and rents consistent with equilibrium in the land market, each curve associated with a particular population level. AA identifies one such curve, in which the population level is below the long run equilibrium. BB identifies combinations of wages and rents consistent with Equation (3). For population at any given level, the intersection of AA and BB indicates the "dynamic equilibrium," indicating the level of net migration. The intersection in the figure indicates positive net migration, which will shift the AA curve upward and thus produce movement along BB toward the long-run equilibrium.

In terms of the issue of equilibrium versus disequilibrium, the question is whether the system remains "close enough" to the long-run equilibrium to make it a useful approximation, or, more generally, whether movements in the equilibrium point approximate those observed in the system. Hence, if Equation (1) is close to being satisfied at all times, the population equilibrium model is valid. It is easy to see that if α and β are sufficiently large relative to shifts in exogenous factors, a and b , migration will maintain the system near equilibrium. In this case, levels of migration merely reflect changes in the equilibrium population level. On the other hand, sudden shifts in a or b will cause displacement from the equilibrium.

In his introduction, Evans notes that those who have argued for the equilibrium model of migration have also claimed that amenities played an important role in migration, while those who have supported the disequilibrium

optimization by individuals and firms in the presence of adjustment costs, as shown in Mueser and Graves (1991). Whereas the treatment presented here assumes that workers and firms act on the basis of current values of wages and rents, the more general model allows agents to consider future values and to form rational expectations regarding them.

model have considered employment opportunity more important. Evans argues that this association is not logically necessary. However, as an empirical matter, we believe the reason for the association is clear.

Changes in the evaluation of amenities that occur with increased average incomes are likely to be gradual. At a location with abundant amenities, such changes cause a movement of the constant utility curve upward and to the left, equivalent to growth in a . The equilibrium model suggests that such a location would display gradual increases in rents and declines in wages. Observed positive migration would occur over an extended period, so that overall population gain in the long run could be large.

Of course, where changes in technology or relative prices occur gradually over time, the system may also remain close to equilibrium despite continuing net migration. However, as an empirical matter, much of the variation in employment opportunity is sudden, occurring over relatively short periods. Furthermore, as noted above, shifts in one period are often not closely tied to those in other periods. In terms of the model, this amounts to shifts in the value of b , causing the constant cost curve to move back and forth. If the period of observation is short enough, such shifts may induce observable migration which is clearly a response to disequilibrium.

It is then clear that with regard to the important question—how significant is the deviation from equilibrium at any one point in time?—we conclude that the answer will depend on the time period of interest to the researcher. If one is interested in year-to-year impacts on migration, we certainly do not rule out a major role for employment opportunities which can vary dramatically over relatively short periods (military base or plant closing or openings, large fluctuations in energy prices favoring or disfavoring regions, varying regional effects of the national business cycle). However, the role of such influences over more lengthy periods (e.g., ten-year census intervals, as is commonly used) is likely to be modest if such employment impacts are not positively correlated over time.

Even small systematic equilibrating influences (e.g., rising amenity demands with real income increases over time as emphasized in Graves, 1979, and elsewhere) can have a dominant cumulative effect. There is some evidence for our claim that amenities are becoming more important (perhaps due to the income effects emphasized here) in migrants' decisions. Those areas which began drawing particularly large numbers of elderly from other states in the fifties and sixties also began attracting large numbers of labor force participants, possibly with a bit of a lag. Presumably, those elderly who are not moving back to a previous residence would be sensitive to the availability of local amenities. The increased attractiveness of such areas to labor force participants may, to some degree, reflect employment opportunities in the local industries serving these relatively affluent elderly, but it does not seem likely that observed levels of net migration can be explained this way (the flows of workers are much larger than the flows of retirees). It seems more likely that higher incomes may be drawing employed migrants to those high amenity areas that were first "discovered" by the elderly.

Greenwood, et al. (1991) perform an empirical analysis which suggests that

deviations from equilibrium have little important impact on estimates of equilibrium wage differences across locations. They explicitly calculate wage levels for states which would produce no migration and compare these with observed wages. Notwithstanding substantial differences in migration, deviations of equilibrium wages are modest relative to differences in wages across locations.

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