"DIFFERENT STEPS ON THE LADDER: A MULTILEVEL ANALYSIS OF TRANSITIONS ACROSS THREE WAGE THRESHOLDS"

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ABSTRACT

PHILIP WEICKERT: Different Steps on the Ladder: A Multilevel Analysis of Transitions Across Three Wage Thresholds
(under the direction of Ted Mouw)

This paper uses multilevel logistic models incorporating individual random intercepts and state and panel fixed effects, and data from the 1990-2004 panels of the Survey of Income and Program Participation, to achieve three goals: more accurate estimates than other studies have provided of the effects of individual characteristics on likelihood of transition out of low wages; estimates of direct and interactive effects of the state-level unemployment rate on that likelihood; and rough estimates of how these effects vary at two higher wage transition points.

The study's major finding is that a one-point increase in unemployment is associated with a 7% decrease in likelihood of transition, and this negative effect is significantly greater for males than females and for high-school dropouts than college graduates.

Also, with caveats due to unobserved heterogeneity, the study indicates that at higher wage transition thresholds, state-level variables generally matter less and individual-level variables matter more. And although higher unemployment has the same negative effect on likelihood of high school dropouts' transition across all wage thresholds, its effect on highly-educated workers' likelihood of transition is significantly smaller or disappears altogether at higher thresholds.

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The likelihood of transition out of low-income status—whether defined as poverty, working poverty, or welfare receipt—is a longstanding, central area of concern for both sociologists and labor economists. And with increasing income inequality in the United States (e.g. Autor et al 2008) and stagnant wages since the 1970s (e.g. Mishel et al 2009), the issue has become an even more important and popular one in recent years. Because of richer longitudinal data and more sophisticated quantitative methods, however, it now is possible to study this likelihood, and how it is affected by short-term economic context, in new ways. Prior research using longitudinal data sets and focusing on probability of transition, as opposed to expected wage change, generally has used simple logistic regression or linear probability models. But multilevel logistic regression can produce more valid results than either method and, additionally, provides a way to estimate the effects of contextual variables.

Among those contextual variables is the business cycle, which generally is defined by economic output or the unemployment rate. Economists frequently have studied the effects of the business cycle on various labor market outcomes, but it remains understudied by sociologists. Including a business cycle measure in wage transition models, both by itself and in interaction with other independent variables, provides insight into the effect of economic context on individual wage trajectories and offers a way to test certain versions of sociological theories about hiring and promotion queues.

Two other shortcomings are common in probability-based models of transitioning out of low-wage status. One is that all wage thresholds are essentially arbitrary lines in the sand drawn by researchers; they are not particularly important to the respondents, themselves, and one arbitrarily drawn threshold probably is no more inherently meaningful than another. A transition upwards out of any low-wage status is good, obviously, but the salient factors might affect transition across a different threshold in quite different ways, or not at all. Therefore, comparing several different thresholds can help determine the breadth of a study's validity. Second, although many researchers have sought to address selection issues deriving from unobserved differences between low-wage and other workers, this author has not found any attempt to address potential within-low-wage bias by controlling for prior wages. Low-wage workers usually are studied as if they all are the same distance from the wage threshold, so differences between their likelihoods of transition that are due to variance in respondents' initial wages can be wrongly ascribed to race, gender, or other factors.

This study uses multilevel logistic regression of data from the Survey of Income and Program Participation to try to address some of these issues. By using the SIPP rather than a longer, narrower longitudinal dataset like the Panel Study of Income Dynamics, it provides a more limited view of long-term wage trajectories and left-censorship, but a more demographically detailed picture of short-term trajectories and a more precise measure of transitions' immediate economic contexts. It estimates the effect of state-level unemployment on the likelihood of transition and explores possible variation in the magnitude of that effect. It uses multilevel logistic regression, with a combination of random effects at the individual level and fixed effects at the state level, in order to improve validity and flexibility while limiting the assumptions necessary. And finally, it tests two additional wage thresholds to explore possible differences in key variables' effects.

Wages and income across the spectrum

Upward intragenerational mobility in the United States seems to have slowed in recent decades. Mishel et al (2009) use several synthetic cohorts to track family income growth from when the main earner was 30 to when he/she was 50, finding that, whereas income more than doubled for the cohort beginning in the mid-1940s, it increased 92% for the cohort beginning in the mid-1950s and only 60% for that beginning in the mid-1980s. Bernhardt et al (2001), looking at short-term wage trajectories for young males with at least three years' experience, find an increase in the inequality and uncertainty of wage gains. The flattening of wage trajectories has been accompanied by higher within-individual variability of family income (Hertz 2006) and wages (Dynan 2008), trends that have been more dramatic at the lower end of the income scale. Nor have flatter wage trajectories been offset by higher entry-level wages; French et al (2006) find that these have been stagnant over the last twenty-five years.

Several individual characteristics, such as gender and race, have been found to have an effect on the wages of workers across the entire income spectrum. Although human capital theory holds that differences in human capital must be associated with these individual characteristics, female workers—to begin with gender—make less than male workers, even after controlling for human capital factors (Kilbourne et al 1994). The gender wage gap almost disappears, however, when one compares men and women with the same jobs, indicating that gender wage inequality is based on between-occupation rather than within-occupation inequality; "female occupations" pay less than "male occupations" (England and Browne 1992, Kilbourne et al 1994, Petersen and Morgan 1995). Many researchers have used the concept of labor queues, developed by Lester Thurow (1969) in analyzing discrepancies in black and white unemployment levels, to help explain the gender wage gap. Fernandez and Mors (2008), studying a group of applicants for jobs at a call center, find differential sorting of male and female applicants into hiring queues but no within-queue wage differences once sorting is complete. Black workers also have been found to make substantially less than whites (Cancio et al 1996; Darity and Mason 1998). And as with women, blacks' labor market disadvantage occurs largely during the job search and hiring processes (Fix and Struyk 1993; Bertrand and Mullainathan 2004).

Education always has had a large effect on wage outcomes, of course, but as Kalleberg (2009) points out, it has become increasingly important as a result of the removal, in recent decades, of institutional protections. Despite the large increase in the supply of college-educated labor, the college wage premium continued to rise through the 1980s and 1990s (Goldin and Katz 2008). Butler et al (2008) find that educational attainment is, along with family structure and savings, one of the strongest determinants of whether Americans move up or down the economic ladder. Motherhood, especially among married women, is associated with lower wages for women (Budig and England 2001), although a marriage premium exists for men (Gray and Vanderhart 2000; Hersch and Stratton 2000). Another well-documented salient factor is union membership, which is associated with a wage premium ranging from 9.1% for white women to 22.7% for black men (Mishel et al 2009). And although even nonunion workers receive a premium if they are in highly unionized industries and occupations, the range of those who benefit has narrowed as unionization has declined; from 1978 to 2005, for example, the percentage of blue-collar workers that are unionized has dropped from 43.1% to 19.2%.

To summarize a large economics literature on the interaction of unemployment and wages, most recent economists who have identified any significant relationship find that wages are procyclical, although the relationship is complicated by such factors as wage stickiness, inventories, capacity utilization, and fluctuations in workforce composition. Both in the U.S. and abroad, entry wages and wages for job-changers are more sensitive to economic cycles (Solon et al 1994, Devereux 2001, Carneiro et al 2010, Devereux and Hart 2006).[1] There is consensus that long-term unemployment has less effect on wages than short-term

[1] Kudlyak (2010) has developed an estimate of the "price of labor," which is distinct from entry wages and much more procyclical. She finds that a 1% increase in unemployment leads to a more than 4.5% decrease in the price of labor.

unemployment, since the short-term unemployed are closer to the margin and therefore play a larger role in establishing the price of wages (Nickell 1997).

The magnitudes of certain factors' effects on wages may have changed in recent years. There are some reasons to think that gender- and race-based wage disadvantages have decreased, for example. From 1974 to 2004, inflation-adjusted median income for men in their 30s fell by 12% (Isaacs et al 2008), while women's wages have shown considerable improvement across the board, but especially for higher-income women (Mishel et al 2009). Trends in the difference by race in income have been less consistent; from 1995 to 2000, average income growth was higher for black families (2.9%) than for white families (2.1%), although white income has declined more slowly than black income in the 2000s (Mishel et al 2009). There is some disagreement about changes in wages' cyclicality: Abraham and Haltiwanger (1995) find that real wages' procyclicality has been greater in the post-1970 period, while Krueger et al (2001) find that the magnitude of wages' response to unemployment appears to have declined in the 1980s and 1990s.

Upward mobility among low-wage workers

Like this study, a very large body of literature has dealt more specifically with low-income groups, usually defined as welfare recipients, those under the poverty line, those in the bottom income quintile, or those in working poverty. I use the working poverty threshold, following common—though certainly not universal—practice by placing it at twice the federal poverty level. Acs et al (2000), who use the same threshold,[2] find that, contrary to some perceptions, those in working poverty are rarely teenagers (only 7% of them are), and they are more likely to have children than better-paid workers are. Low-wage workers are disproportionately female, black, Hispanic, less educated, unmarried, and foreign-born (Acs et al 2000, Andersson et al 2005, Loprest et al 2009).

At the lowest end of the scale, studies of the likelihood of transition out of poverty have found results varying from around 20% to 35% per year, with a slight downward trend in likelihood of exit during the early 1990s and a possible increase in the late 1990s (Cellini et al 2008). Researchers focusing on low-skill workers have found that a sizeable subgroup tends to be stuck in wages close to the minimum wage throughout many of their prime earning years (Boushey 2005, Carrington and Fallick 2001). Boushey also finds that over a third of prime-age adults in minimum-wage jobs remain in minimum-wage jobs three years later. Turning to low-wage workers in general, researchers have found that a substantial proportion of workers experience significant gains, but that wage growth is highly dispersed. Andersson et al (2005), using LEHD data, find that half of workers who spent three years in low wages acquire higher-wage employment over the course of the following five years; only 8%, however, made "complete transitions," going from consistently earning less than $12,000 to over $15,000. Newman (2006), using a subsample of the 1996 SIPP panel with age and race composition matching that of 300 Harlem fast-food workers, finds that 40% experienced a decline in real wages, while 12% achieved real wage gains of greater than $5/hour over a four-year

[2] Acs' definition includes a minimum hours-worked threshold that I do not apply.

period. Studies looking at workers in the lowest quintile have found similarly varied worker outcomes. Auten and Gee (2007) find that 47% of heads of taxpaying units in the bottom income quintile in 1987 experienced at least a two-fold increase in real wages by 1996, while 23% experienced no growth or declines. The figures (50% and 18%, respectively) are almost the same for the years 1996 to 2005. While outcomes for low-wage workers have been extremely varied, on the average, they seem to be stagnating. Researchers have found that more recent worker cohorts have been less likely to move out of low wages (Bernhardt et al 2001) and that young male workers were much less likely to earn more than twice the poverty level in the 1980s than in the 1970s (Duncan et al 1996).

Most of the factors that influence low-wage upward mobility are those that have been found to influence upward mobility for workers as a whole. Among demographic variables, being male and white, being highly educated, and having no children (for women) are associated with increased mobility rates (e.g. Rank and Hirschi 2001, McKernan and Ratcliffe 2002, Ribar and Hamrick 2003, Andersson et al 2005, Acs and Zimmerman 2008, Mouw and Kalleberg 2010). Being married is advantageous for men and disadvantageous for women (Pencavel 1998), while being a native-born American is associated with increased likelihood of transition (Boushey 2005). Turning to work-related variables, Schmitt et al (2008) find that union membership raises wages in fifteen large low-wage occupations an average of 16%. The value of work experience is debated by supporters of the stepping-stone and sorting models of wage mobility, but in the aggregate, it is associated with increased likelihood of transition, as is job turnover (Jovanovic and Mincer 1981, Eckstein and Wolpin 1995, Andersson et al 2005). As Mouw and Kalleberg (2010) show, however, experience and job turnover must be combined properly to be effective; occupational experience, they find, increases the probability of upward mobility to occupations with similar skill sets. Andersson et al (2005) find that it is beneficial for low-wage workers to be in the construction or manufacturing industries and work for firms that are larger, have lower turnover, and have a history of paying a wage premium. They also find that the "complete" transition out of low wages, as they define it (see above), is concentrated in a relatively small number of firms.

There has been less study of the effect of unemployment, or any measure of the business cycle, on transition likelihood. Andersson et al (2005) find that strong economic growth and minimum wage increases during the late 1990s did not have much effect, although earnings growth did appear to be less during 2001. Danziger and Gottschalk (1986) find that economic growth had a strong antipoverty effect through the early 1970s, but that this effect weakened through the mid-1980s, possibly implying a positive, but declining, effect of unemployment on transition likelihood. Hines et al (2001) look at differences in the average wage growth in twelve different sectors and conclude that improved cyclical conditions do lead to a small increase in wage growth, especially for workers in the bottom half of the distribution. French et al (2006), in their base model, find that the return to experience is strongly procyclical, but in a model accounting for common time effects, they find no relationship between the returns to experience and the business cycle.

Several researchers have found changes over time in the effects of some of the variables listed above on wages or transition likelihood. Bernhardt et al (2001) find that not only is the likelihood of transition lower for a more recent cohort of workers, but the returns to job change have declined. Acs and Zimmerman (2008) use PSID data to find that the salience of education and race declined from 1994 to 2004. French et al (2006) find that the returns to work experience, which determine most of the variation over time in wage growth, change from year to year, but they do not find evidence of a secular trend in that change.

The literature offers just a few indications of how the effects of either individual or contextual variables might differ at higher income thresholds. As we have seen, Hines et al (2001) find that wage growth is more (though still only slightly) procyclical in the bottom half of the income distribution. The glass ceiling theory holds that women and minorities encounter greater resistance in accessing top positions than low or medium ones; Zeng (2011) finds the opposite, however: that their disadvantage, at least in management, is concentrated in mid-level transitions. If I were to extrapolate incautiously from these findings, I would predict that the effects of unemployment and of being female and a racial minority are less negative at higher thresholds. But this probably would be pushing the external validity of researchers' findings further than they would, themselves.

Research questions

Of the several questions this study seeks to address, the most specific and important is, what is the effect of unemployment on the likelihood of transition out of low-wage status and how does that effect vary by other covariates? This question is the main reason that the SIPP has been chosen. Given that wage growth has been found to be procyclical, I expect that unemployment will have a negative effect on wages, but that this effect might be smaller for more experienced and educated workers, whose skills presumably are scarcer and who therefore might be better insulated from downturns. If unemployment's effects are more negative for women and minorities, that would lend support to a permutation of labor queue theory, since one could conclude that white men are the first to achieve—or, put another way, the last to lose—whatever wage gains are possible in a poor economic climate.[3] On the other hand, because male workers' wages have been more stagnant than female workers', and because male-heavy industries like construction and manufacturing are, though higher-paying, also more procyclical, there is some reason to believe that higher unemployment will hurt men disproportionately.

The second research question is more general and less original: what individual-level factors are associated with upward transition out of low wages, and have the effects of these factors changed between 1990 and 2007? Although I have little reason to expect to identify different factors than the researchers cited above

[3] The research cited above generally finds that labor queues operate during job searches more than during jobs, and my study does not capture respondents who cannot enter the workforce or who fall out of it. But female and/or minority workers nevertheless could experience additional disadvantage from high unemployment rates if, as labor queue theory would predict, their ability to switch jobs is more negatively affected.

have, the methodological advantages of this study should allow it to identify the effects of those factors more accurately than other logistic analyses.[4]

Finally, the third research question is whether the factors affecting the likelihood of transition out of working poverty have different effects at higher wage thresholds. Any differences found will have to be interpreted cautiously, for reasons explained in the methods section, and this aspect of the study is entirely exploratory; the models will not be refit to each data sample and the mechanisms behind any differences will not be more fully analyzed. The purpose of this question essentially is to see whether more work should be done to determine whether, and explain how and why, upward mobility operates differently at different points on the income spectrum.

Data, Measures, and Descriptive Statistics

This study draws its individual-level variables from the Survey of Income and Program Participation, which was initiated by the US Census in 1983 and is the source of official estimates of income and poverty. The SIPP consists of a continuous series of national panels of variable size, each of which is interviewed between eight and twelve times over the course of three of four years. The 2004 panel includes 46,500 households, which were interviewed eight times; panels from the 1990s include roughly half as many households.

[4] Following exploratory research, a related question has been added: Does the effect on likelihood of transition of the interaction of gender and marital status vary by educational attainment?

The SIPP covers too few years per panel to adequately estimate time in poverty for long-term spells. Therefore, left-handed censorship, which is a problem for all studies of intragenerational mobility, typically is a greater concern with studies of short-term mobility. On the other hand, the SIPP's breadth provides greater statistical power than is possible with longer-term longitudinal datasets; this breadth allows estimation of interactive effects that longer, narrower studies like the Panel Studies of Income Dynamics might not, thereby providing a way to test theories about stratification and labor queues. Even more importantly, the high frequency of interviews in the SIPP allows researchers to more precisely identify the time of wage transitions and, therefore, the transitions' economic context. More limited than long-term studies at estimating the effects of individual-level factors on long-term mobility, the SIPP is well suited for studying the effects of factors with substantial short-term variability, such as unemployment.

The threshold of low wages (the "twopov" threshold) used in this study is a wage sufficient for an unmarried, childless individual to earn an amount equal to twice the poverty threshold in a fifty-week work year: $10.83 in 2009 dollars. For comparison, I create two other thresholds at three times and four times the poverty wage level. In order to compensate somewhat for wage volatility, a transition is defined as two observations below a threshold followed by two above it. The transition is deemed to have taken place at the time of the third observation, the first of the sequence of four in which wages above the threshold are reported.

The full SIPP sample is restricted in several ways. All observations in which wages were not earned have been dropped, as have observations in which adjusted wages (in 2009 dollars) are less than $1. Because of the two-down, two-up definition of a wage transition, only respondents with at least four observations are retained.[5] Respondents younger than 18 and older than 64 have been dropped, as have those with inconsistent measures of race or sex. Only whites, blacks, and Hispanics are included.

Individual-level independent variables include age and age-squared, which proxy here for work experience.[6] Gender and marital status are included, as is a categorical measure of the number of children under 18; respondents with more than two children are pooled into a single category. The educational attainment recode includes four categories: did not complete high school, completed high school, some college, and BA or higher. Respondents with advanced degrees are combined with other college graduates in order to facilitate convergence of multilevel logistic models. The SIPP's measure of race includes the categories white, black, Hispanic, and "other"; "other" has been omitted because of presumed within-group diversity, but "Hispanic" has been retained despite concerns that it often is considered an ethnic rather than a racial classification.[7] Indicators of union membership and work-limiting disability also are included.

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[5] Some respondents lack one or more wage observations, so the exact time of some wage transitions is more imprecise than if only respondents with full observations were kept. The potential selection bias of dropping the large majority of the sample, however, seems to outweigh the risk of some imprecise transition time estimates.

[6] Although the SIPP includes a measure of total work experience, it is missing data for a much larger proportion of observations than the age variable is.

[7] It should be noted that almost half of the Hispanics in the analysis samples are foreign-born, although the large number of missing "foreign-born" observations leads me to omit that measure. Missing data concerns also lead me to omit the

Finally, for each of the three thresholds, a logged wage ratio has been created consisting of the natural log of the quotient of a respondent's average prior wage divided by the threshold in question. This ratio is presumed to be a better indicator of a respondent's proximity to the threshold than simple wages, and it facilitates comparisons across thresholds. Many studies of crossing a wage or welfare threshold do not control in any way for prior income; including such a control can reduce within-group selection problems resulting from the fact that some respondents are much closer to wage thresholds on initial observation and therefore are much more likely to cross them.[8]

The state-level variable of greatest interest for the purposes of this study is monthly unemployment as reported by the Bureau of Labor Statistics; it is used as an approximate measure of the business cycle. Because transitions could have happened at any point during the months between observations, unemployment lagged by one, two, four, and six months was compared to unemployment during the month of the transition observation using likelihood ratio tests. Lagged unemployment did not fit the model as well, however, so I have used the unlagged measure.

SIPP's measure of "student," despite concerns that working college students' wages are lower than they could find as full-time workers, perhaps leading to a small downward bias on the estimated effect of "some college" or "BA or higher."

[8] Of course, this control cannot address across-threshold selection problems; those respondents in working poverty may be atypical in unobserved ways, so it cannot be assumed that their wage trajectories, and the factors affecting them, will be identical to those of the population as a whole. Some researchers have used Heckman two-stage estimators to adjust for selection bias when studying populations under a certain wage threshold, but as Mouw and Kalleberg (2010) point out, this method requires the use of instrumental variables and, therefore, strong, untestable assumptions.

Other state-level variables include the proportion of the over-25 population with a high school degree (which, according to a likelihood ratio test, fit the full model better than the proportion with a college degree), the percent foreign-born, and the median household income. For all three variables, data come from the 1990 and 2000 U.S. Census, as well as the 2006-8 American Community Survey (for the education and foreign-born measures) and the 2007-2010 U.S. Census Annual Social and Economic Supplement (for the income measure). Figures for other years were estimated by linear interpolation. Because month-to-month variation in these measures cannot be estimated as accurately as unemployment can, the corresponding coefficients should not be treated with the same level of confidence. Their primary role here is as controls.

Descriptive statistics are included in Tables 1a, 1b, and 2.[9] The "all" sample in Table 1a includes all respondents who earned wages in at least one wave. The "2pov opps" sample includes all respondents whose adjusted wage was less than $10.83 for two consecutive observations before their last and next-to-last observations; these respondents had at least one opportunity to earn more than $10.83/hr in two consecutive observations and thereby qualify for upward transition. The "2pov exits" sample includes the subset of "2pov opps" that successfully made the transition. The remaining samples are defined in the same way, though using the two other wage thresholds.

[9] The descriptive statistics should not be considered an accurate representation of the entire working population, since they are not adjusted for the SIPP's survey weights (pweights cannot be applied to xt commands in Stata 11). But because analytic estimates control for at least and perhaps all of the factors used to determine weights, the results of the analysis should be considered more valid than the descriptive statistics.

Because the data are longitudinal, the overall variance for each variable can be decomposed into between-respondent and within-respondent variances. In most cases, as one would expect, the within-respondent variation is much lower (see Table 1a). Because some respondents either had children or earned a degree during the study period, Table 1b's "individuals" columns for those two variables always total more than 100%; within each column, the figures in each row represent the percent of the sample that fell into the corresponding category for at least one observation. Table 2, which includes state-level variables, is based on neither total respondent observations nor data for all months during the study period; instead, each state contributes one observation for every wave during which at least one of its inhabitants was in working poverty, or "twopov" status.[10] The values here might differ from their counterparts in Table 1a because different states had different numbers of potential transitioners. For example, states with high proportions of foreign-born clearly included more potential transitioners than those with smaller proportions.

As we would expect from the literature, those in twopov status are less often white, more often female, and less often highly-educated than their full-sample counterparts. They also are less likely to belong to a union and more likely to have a work-limiting disability. Again consistent with the literature, successful transitioners, when compared to all potential transitioners, are the opposite in all these regards. Successful transitioners are different in another important way: at all three wage thresholds, they have median incomes that exceed the threshold, and

[10] The corresponding "threepov" and "fourpov" tables are almost identical and therefore are not included here.

their prior-wage-ratios, as indicated by "ln(wg_rat)," are much closer to 1 than those of their unsuccessful counterparts, highlighting the need to control for prior income. It also is evident that the mean twopov transitioner had a smaller distance to go, both in absolute dollars and as a proportion of his or her income, than the mean threepov and fourpov transitioner. This difference may help explain why transition rates at the lower threshold are so much higher.

In general, higher-level transitioners have the same advantages over twopov transitioners that the latter have over the "twopov opps" group. For example, those exiting twopov status are better educated, less frequently disabled, and more likely to be in a union than those failing to exit; fourpov exiters have all these same advantages to a higher degree. Finally, transitions are considerably more likely to occur when the state foreign-born population is higher. Exploratory analysis (available on request) indicates that on an individual level, being foreign-born is associated with substantially lower likelihoods of transition, so upwardly-mobile immigrants themselves probably do not account for the positive association of the state foreign-born population. Possibly, healthy state economies both provide better opportunities for upward mobility and also attract unusually large numbers of foreign-born workers.

Figures 1 and 2 demonstrate significant variation by both year and state in the proportion of successful transition opportunities, indicating the need to control for both. Transition likelihood also varies by unemployment (Figure 3), decreasing as unemployment increases. Figure 4, which provides the distribution of unemployment measures among observations with twopov transition opportunities, shows why the line graphs in figure 3 should be considered much more valid where unemployment is above three and below eight; very few transition opportunities occurred outside that range.

Method

Observations are nested within individuals, who are nested within states. Ideally, a random intercept would be established at both the individual and state levels, allowing estimation of both between-individual and between-state variability in transition likelihood. This is not done at the state level, however, both because of the resulting computational complexity and because the state-level error term may be correlated with the independent variables, which would violate a critical random-effects assumption. Unobserved state characteristics—such as political climate, business-friendly policies, job training, and education spending, and perhaps even available natural resources and proximity to trade routes—might not be orthogonal to either state-level unemployment or individual-level demographic and educational variables. Consequently, I use dummy variables to create state fixed effects. Fixed effects cannot reliably be applied in logistic analysis when the number of cases within each group is small, an issue known as the incidental parameter problem (Rabe-Hesketh and Skrondal 2008, Allison 2005). When the number of cases within each group is very large, however—and the number of respondents per state is very high in the samples used here—bias due to the incidental parameter problem is likely to be quite small (Greene 2002).

Choosing between two imperfect options, I am using random effects rather than fixed effects at the individual level. The use of fixed effects would trigger the incidental parameter problem and allow the use of unchanging demographic variables only in interactive form. The use of random effects in a longitudinal dataset, however, essentially assumes that the difference in effect associated with a within-respondent one-unit change in an independent variable is equivalent to the difference in effect associated with a contemporaneous one-unit difference between two respondents (Gould 2001). That may or may not be a safe assumption in this case.

To control for unobserved variation over time in factors affecting exit likelihood, I have used panel fixed effects rather than year fixed effects. Panel effects fit the data better, according to a likelihood ratio test; more importantly, they control for possible distributional differences between SIPP panels, which—because of overlapping panels in some years—year fixed effects would not do.

The use of panel fixed effects ensures that estimates are derived on the basis of within-panel variation. The use of state fixed effects means that only salient variation for state-level variables is the within-state variation. But the coefficients on the individual-level variables, on the other hand, are estimated on the basis of both within-individual and between-individual variation. The coefficients on the state and individual variables therefore must be interpreted quite differently.

Although this model nests observations within individuals within states, the fact that states, like panels, are accounted for by dummy variables makes it operate as a two-level rather than a three-level model. If p_{ij} represents the probability at any observation *i* that an individual *j* crossed a wage threshold in state *k* during panel *m*, then:

Level 1 (obs)	$\ln[p_{ij}/(1-p_{ij})] = \beta_{0j} + \beta_1 X_{1ij}$ $+\beta_2(X_{2ik} - \overline{X}_{2k}) + \omega_k + \gamma_m$	β_{0j} β_1 β_2 X_{1ij} $X_{2ik} - \overline{X}_{2k}$ ω_k γ_m	= individual-level intercept = fixed coefficient on individual-level variables = fixed coefficient on state-level variables = individual-level variables = difference between state-level variables at observation *i* and their within-state means. Time-invariant state characteristics are subtracted out of the equation. = fixed effect for state *k* = fixed effect for panel *m*
Level 2 (indiv)	$\beta_{0j} = \beta_0 + \mu_{0j}$	β_0 μ_{0j}	= mean of individual-level random intercepts = individual-specific deviation from mean

Note that there are no random effects in the equation, so the assumption is that the effects of the independent variables do not vary by state or individual. Also, this model assumes that there are no salient unobserved short-term changes in the effect of living in a given state.[11]

The final models have been determined with several factors in mind: hypotheses about variables that might affect likelihood of transition, missing data, quality of models' data fit, and multicollinearity concern. The second of these factors leads me to exclude the variables "student" and "foreign-born," even though they might affect likelihood of transition. The third is why, in the model comparing married and unmarried men and women, gender and marital status have been

[11] This probably is a safe assumption. A three-level logistic model with state and individual random intercepts but without covariates (at the twopov level; results available on request) estimates the standard deviation of the state random intercept to be .261. When state-level variables are included, the standard deviation of the state random intercept drops to .019, indicating that, even without state fixed effects, the state variables I am using can account for almost all the between-state variability in transition likelihood.

interacted, as opposed to gender and number of children. Multicollinearity, the fourth factor, is only a concern for models including interaction terms, but high-level interactions of categorical variables do result in enough multicollinearity that the coefficients of interest are rendered non-significant in the fullest desired model. Therefore, three different "full" models rather than one have been developed, each in a different way.

First, to test whether the effects of the individual-level independent variables, in addition to unemployment, have varied over time, each was interacted with the panel variable, individually at first, then all together in a single model.[12] Very few of the many resulting coefficients on interaction terms were significant, fewer of these were substantial, and none formed or followed a recognizable trend (results available on request). This study, then, does not support any earlier findings that predict change over time—or, at least, from 1990 to 2007—in any of the independent variables of interest.

Second, to test whether the effects of unemployment vary according to any of the individual-level variables, a similar approach was used. A set of test models was run, each interacting a separate variable with unemployment, then the final model including all interaction terms. Having found that gender and educational status are the only variables that interact significantly with unemployment, I have included a model with both of those interactions.

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[12] The same test was conducted interacting with the year variable instead of the panel one, with similarly inconclusive results. The results for the tests are not included in this paper but are available upon request.

The third full model tests the interaction of gender, marital status, and educational attainment. Although exploratory analysis found, consistent with the literature, that having children lowers women's likelihood of transition and increases men's likelihood, adding that interaction resulted in multicollinearity that rendered the coefficients of interest non-significant. In this third model, therefore, three variables but not four are interacted.

Sociologists often present their models incrementally, with a more limited model first, then one demonstrating how the addition of more variables affects the coefficients on those in the first. This approach is not appropriate to non-linear models, however, unless the coefficients are either fully-standardized or, at least, y-standardized.[13] The problem is that coefficients on the observed variables are dependent on the amount of unobserved heterogeneity in the model, which changes when independent variables are added to or subtracted. Therefore, without y-standardization, estimates from non-linear regression cannot reliably be compared across models. Unfortunately, y-standardization is not currently feasible for this study, so rather than comparing models with different sets of coefficients, I simply will present the full model appropriate to each research question. The unobserved heterogeneity problem also affects comparisons of models at different thresholds. I will draw only guarded conclusions from comparisons of coefficients at different thresholds, therefore, but will more confidently compare within-threshold

[13] See, for example, Winship and Mare (1984) and Allison (1999). Bauer (2009), Williams (2010), and Mood (2010) all offer ways to address the problem, but only Bauer specifically addresses the problem in terms of multilevel logistic regression. Long and Freese's spost9 program for Stata includes the "listcoef" command, which provides y-standardized and fully-standardized estimates for single-level logistic regression, but as of this writing, no command does the same for multilevel models.

differences. For example, comparing specific estimated likelihoods across the twopov and threepov thresholds may be problematic, but comparing the difference in likelihoods between college-educated and high-school-educated respondents across those thresholds is less so.

Results

To start with, a multilevel logistic model with random intercepts at the individual and state level, but without covariates, was run. Despite the concerns already expressed about random effects assumptions at the state level, this model can provide at least a general sense of the variance of transition likelihood at each of the two levels. The results, found in Table 3, show that the variance at the individual level is much greater than that at the state level; not surprisingly, who one is matters much more than where one lives. It is noteworthy, however, that both random intercepts are very different at the fourpov threshold: the standard deviation at the individual level is about twice as great as in the twopov model, and the standard deviation at the state level, .00869, is a tiny fraction of its counterpart in the twopov and threepov models. Even if we take these results with several grains of salt—on account of the random effects assumptions, cross-model discrepancies in unobserved heterogeneity, and the larger, more economically diverse sample eligible for fourpov transition—it seems safe to conclude that at the highest threshold used here, individual characteristics affect likelihood more and state-level context affect it less.

Results from Model 2, which includes all covariates but no interacted terms, can be found in Table 4. If we focus on the twopov model, we see that most of the individual-level variables have significant effects in the predicted directions. Age (a proxy for work experience) is curvilinear, with its maximum positive effect on the likelihood of transition occurring, on average, at age 35.[14] Being female is associated with 43% lower odds of transition, while being married—which is treated in this model, unrealistically, as an equivalent event for men and women—is associated with 27% higher odds of transition. Having children is associated with slightly lower odds of transition, while higher educational status, particularly a college degree, increases the odds dramatically. Blacks have 18% lower odds of transition than whites, while Hispanics have 31% lower odds, although, as mentioned earlier, preliminary tests indicate that foreign-born Hispanics have lower odds than native-born ones, a distinction that is not apparent in this model. Respondents with work-limiting disabilities have 43% lower odds of transition, while the few union workers in working poverty are fully 235% more likely than non-union workers to transition out.

As described earlier, results for individual-level covariates are based on a combination of within-individual and between-individual variation, the second of which probably factors more into the estimates. State-level results, on the other hand, are based on within-state variation and therefore on change over time in the independent variables. For example, if all characteristics of all individuals in a state were to remain constant, but unemployment increased one point, each respondent's

[14] This calculation is based on the more precise unexponentiated coefficients and is not shown here.

odds of transition would be expected to be 7% lower.[15] The other state variables' effects on odds of transition all are significant and in the direction indicated by the descriptive statistics; as described above, however, these coefficients should not be considered as trustworthy as that on unemployment.

Most of the differences between the coefficients at the twopov level and those at the higher two levels are not large, and it is important to remember that, because of the unobserved heterogeneity problem, differences should be quite substantial before fully acknowledged. Age has slightly more positive an effect at higher thresholds, and its maximum benefit comes later at the threepov level (40 years old) and fourpov level (42 years old) than at the twopov level (35 years old). It is worth noting that the effects of gender and race change little at different thresholds; the penalties on each are slightly larger at the threepov level than the twopov level but then are reduced again at the fourpov level. This offers some indication that the net effect, if any, of hiring queues, promotion queues, and/or job information network effects probably is not substantially different at these different points on the wage spectrum. The benefit of higher educational achievement, relative to being a high school dropout, is substantially greater at higher thresholds, so much so that we can tentatively conclude that education affects transition likelihood more positively than work experience does. Being disabled and being in a union also have quite different effects at the three different thresholds. Always

[15] This interpretation assumes only two possible outcomes for each respondent in twopov status: receiving increased wages and transitioning out, or continuing to receive low wages. Those who actually lose their jobs as a result of higher unemployment play no role in generating this odds ratio estimate. Thus, the negative effect of unemployment on average would-be workers' wages, as opposed to workers' average wages, is greater than these estimates imply.

beneficial, union membership is much more so at lower thresholds. Having a disability has a smaller negative impact on crossing higher thresholds than lower ones, which again may be due to the different occupations at hand. One reason for the difference could be that lower-wage occupations may more frequently involve physical labor, to which disabilities can present more of an obstacle. Finally, prior wages matter much more at higher thresholds; evidently, the same percentage gain in wages is easier to achieve at a lower point on the wage spectrum, where fewer actual dollars are involved.

As we would expect from the finding that states matter less at higher thresholds, the effect of unemployment is lower for threepov (though not significantly) and fourpov transitions. In terms of likelihood of upward mobility, at least, lower-wage workers evidently are more susceptible than medium-wage workers to economic cycles.

As explained in the methods section, in order to determine whether the effect of unemployment is consistent, a set of models identical to the previous was run, with the addition of two-variable interaction terms between unemployment and both gender and educational status. Because of unobserved heterogeneity, the addition of new variables to a non-linear model can change existing coefficients whether or not the variables are orthogonal to one another. For this reason, I will focus only on the interaction terms and their root variables, assuming that Model 2's results for all other covariates are more reliable.

According to the twopov version of Model 3, respondents with college degrees are significantly less negatively affected by unemployment than high school dropouts are, as can be seen in Table 5. The same trend is apparent for high school graduates and those with some college, but the differences are not significant in those cases. Also, at this threshold, females are significantly less negatively affected by unemployment than men. If we look at higher thresholds, we see that one of these effects is stronger and the other weaker. At the fourpov level, for example, all other education categories are significantly less negatively affected by unemployment than high school dropouts are, and the difference is such that the net effect of unemployment for college graduates is very close to zero, since it is entirely offset by the effect of the interaction term. At higher thresholds, the interaction of female and unemployment is not significant, meaning that women are no less negatively affected by high unemployment than men are. They also are slightly better off when unemployment is low than in the twopov model, however, as can be seen by the coefficients on female.

Simultaneously interpreting coefficients on interaction terms and their roots is very difficult, especially when they are expressed in terms of odds ratios, so I provide marginal estimates to demonstrate the variables' combined effects. To create the estimates used in Figures 5 and 6, all covariates other than the relevant interaction term and its root variables have been held constant. The prior wage ratios have been set to .95, meaning that in order to cross the threshold in question, a respondent's wages must be 5.26% higher than the average of his or her wages in all previous observations. The scales are not identical at the different thresholds, so the slopes can be deceptive. Individual lines and likelihoods of transition should not be compared across graphs; the within-graph relationships between the lines and likelihoods, however, can be. Finally, it is worth remembering that, during this study, unemployment almost always was between 3 and 8, so predictions outside those lines are considerably less solid.

Figure 5 clearly shows that high educational attainment, especially a college degree, greatly improves the likelihood of transition at each threshold. At the lowest threshold, however, high unemployment does substantially reduce the likelihood of transition even for the most educated. But it is evident that for threepov and, especially, fourpov transitions, unemployment affects college graduates' chances far less negatively than it does those of other groups. The scale of the graph is such that it is harder to see the distinctions between the three lowest educational categories, yet here, too, the differences become starker at higher wage thresholds. Higher education helps at all thresholds, but it is interesting that it offers greater protection against high unemployment at higher thresholds. It may be that more of the occupations that pay twopov wages are highly affected by unemployment, in which case all workers within those occupations, even the more highly educated ones, would be vulnerable to economic cyclicality.

Figure 6 shows that men's likelihood of transition always is higher than women's at each threshold,[16] but it also demonstrates that at lower thresholds, the negative effect of unemployment falls more heavily on male workers. In the fourpov threshold, however, women's likelihood of transition is affected by unemployment just as negatively in dollar terms (and for what it is worth, even more in percentage

[16] Workers who are laid off drop out of this sample, however, and therefore do not contribute to the denominator for transition likelihood. If men are more likely than women to be laid off, then this model exaggerates their advantage.

terms) than men's. Here again, the explanation could be occupations. Men in working poverty may be more likely to work in industries like manufacturing or construction, which can be particularly sensitive to business cycles. Male workers higher on the wage spectrum might work in industries and occupations that are much less sensitive.

Table 6 contains estimates for Model 4, which is identical to Model 2 except for a three-way interaction between gender, marital status, and education, as well as two-way interactions between the three root variables. The odds ratios here are difficult to interpret not only because they are difficult to add, but because they can refer to a narrower group than it might seem. For example, the effect of the interaction of gender and educational attainment shows that women gain much higher benefits from high educational attainment (relative to the lowest education category) than men do. These odds ratios are specific to married females, however; the low odds ratios listed in the threeway interaction show that the education benefit is much less for married than for unmarried women.

Model 4's key findings can be seen more easily in Figure 7, which is based on marginal estimates with assumptions similar to those used for Figures 5 and 6. Beginning once again by focusing on twopov transitions, we see that married men enjoy a substantial advantage in transition likelihood over the other three groups, particularly (in percentage terms) at the "high school graduate" and "some college" education levels. Married men's advantage is consistent with the existing literature on wages, but these results do not reveal the reasons, which could include different priorities for married men, the greater likelihood that a man with higher wages will be married, or employers' decisions to pay married men more for equal work.

On the whole, women are less likely than men to transition, but that disadvantage grows smaller at each higher stage of educational attainment, with the exception of married, college-educated women. Single college-educated women, in fact, are as likely to transition out as their male equivalents. Clearly, the substantial negative effect of being female is highly dependent on marital status and educational achievement.

Comparing the twopov results to those at higher thresholds, we see that married men retain their advantage over unmarried men, but it is slightly smaller. Also, consistent with earlier models, the advantage associated with a college degree is greater for threepov and fourpov transitions. The most interesting trend across models probably is the disappearance of college-educated women's "marriage penalty" at higher thresholds, a trend that is driven by increased disadvantage for single women rather than reduced disadvantage for married women. The reasons for this curious trend are not clear and the subject merits further analysis. Marriage homophily (highly-educated women are likely to marry highly-educated men) and consequent lack of financial pressure could explain why educated married women would place as high a priority on a high-trajectory job. But this does not explain why this particular marriage penalty only seems to exist for one educational category, at one wage threshold.

Conclusion

To sum up, this study confirms several prior researchers' findings about many individual-level variables' effect on transition out of working poverty but, because of the use of multilevel logistic regression with state and panel fixed effects, its estimates might be considered more precise and valid than the linear probability analyses and simple logistic regressions that generally have been used previously. Unlike some prior research, it does not find significant, consistent change over time in the independent variables' effects. It finds a small but significant effect of state-level differences on transition likelihood. The most important of these is the estimate that a one-point increase in unemployment is associated with a 7% reduction in individual likelihood of transition; this effect is more negative for men than women and for high school dropouts than college graduates. Also, while transition out of working poverty almost always is less likely for women than for men, this study finds that less-educated women and married women experience more disadvantage than highly educated and single ones.

In addition, despite problems comparing coefficients across models, the differences between results of models run at different transition points do permit some guarded conclusions: State-level variables matter less at higher wage thresholds and individual-level variables, on the whole, matter more. The effects of education and work experience are greater at higher thresholds, while those of having children, belonging to a union, and being disabled are less. Curiously, being black or Hispanic is associated with greater disadvantage at three times the poverty level than at two times it, but the same is not true at four times the poverty level.

This analysis is designed to estimate a particular set of effects: the effects of state-level unemployment on likelihood of crossing a wage threshold, for individuals of different educational achievement and sex. But because, to my knowledge, other papers have not compared the likelihoods of crossing different wage thresholds, this also is exploratory analysis. Consequently, there is more of a focus here on identifying effects and effect trends than on fully explaining the mechanisms behind them. Some tentative explanations are offered in this paper, but more research clearly is needed. Additional contextual variables such as residential segregation, state education programs, or state economic policy certainly would be useful, assuming that they could be measured frequently enough and that the use of state fixed effects would not mask their salience. Introducing measures of occupation and industry seems a more promising approach, since it could help explain why, among other questions, the effect of unemployment varies by gender, educational status, and the wage threshold at hand. Occupational data also could be used to identify threshold-specific ladders, since the skills or occupational experience that make individuals more likely to transition out of working poverty might not be effective at higher thresholds.

In addition, the upward transitions in this analysis could be disaggregated into within-job and between-job transitions or, possibly, within-occupation and between-occupation or within-firm and between-firm transitions. Because the likelihood of a wage increase and the mechanism underlying it may be different for each of these possibilities, different factors may be more or less salient for each. Mouw and Kalleberg (2010), for example, find that low-wage workers who switch occupations to ones requiring skills acquired in previous occupations experience greater wage increases than either those who remain within their original occupations or those who switch to unrelated ones. Applying a similar approach to three different wage thresholds might shed light on the value of switching jobs and the transferability of job skills at different points on the wage spectrum. This question is particularly important given the evidence that women's and blacks' wage disadvantage occurs more at the point of hire than within jobs.

The real takeaways are that individual chances of upward transition are substantially affected by economic context and that both individual and contextual factors' influences upon those chances are different at higher points on the wage spectrum. Especially since we seem to have entered an extended economic downcycle, further research is needed to better understand how this new climate might affect the chances of upward mobility for both low and medium-wage workers.

TABLES AND FIGURES

Table 1a: Descriptive Statistics--Continuous and Binary Variables

		all		2pov opps		2pov exits		3pov opps		3pov exits		4pov opps		4pov exits	
Variable		Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
age	overall	38.01	12.01	34.47	12.45	34.35	11.67	36.25	12.03	37.57	10.76	37.18	11.77	39.53	10.19
	btw		12.39		12.51		11.68		12.17		10.80		11.95		10.25
female	overall	0.50	0.50	0.60	0.49	0.53	0.50	0.56	0.50	0.47	0.50	0.54	0.50	0.42	0.49
married	overall	0.57	0.50	0.45	0.50	0.48	0.50	0.52	0.50	0.60	0.49	0.55	0.50	0.67	0.47
	btw		0.49		0.48		0.48		0.48		0.47		0.48		0.45
	within		0.13		0.14		0.16		0.14		0.15		0.14		0.14
disabled	overall	0.06	0.25	0.07	0.25	0.04	0.20	0.06	0.23	0.03	0.18	0.05	0.22	0.03	0.17
	btw		0.21		0.21		0.16		0.20		0.14		0.19		0.13
	within		0.14		0.14		0.13		0.13		0.12		0.13		0.11
union	overall	0.13	0.33	0.06	0.23	0.08	0.28	0.09	0.29	0.15	0.36	0.12	0.33	0.20	0.40
	btw		0.30		0.18		0.21		0.24		0.30		0.28		0.35
	within		0.16		0.14		0.17		0.15		0.19		0.16		0.19
wg ('09$)	overall	18.83	28.88	9.92	8.51	12.62	11.10	12.24	14.56	18.04	32.26	14.07	14.70	23.37	38.10
	btw		34.69		4.21		5.27		6.23		12.36		7.05		14.79
	within		19.66		7.36		9.75		13.03		29.75		12.77		35.11
wg (median)		14.74		9.06		10.99		11.32		16.32		12.78		21.53	
ln(wg_rat)*	overall			-0.18	0.31	-0.01	0.30	-0.37	0.36	-0.07	0.31	-0.53	0.40	-0.09	0.32
	btw				0.29		0.25		0.34		0.27		0.40		0.28
	within				0.11		0.15		0.09		0.16		0.09		0.16
unemp	overall	5.62	1.51	5.56	1.49	5.34	1.45	5.58	1.50	5.38	1.47	5.60	1.51	5.44	1.47
	btw		1.36		1.31		1.26		1.31		1.27		1.31		1.27
	within		0.75		0.73		0.74		0.75		0.76		0.76		0.77
st_medinc	overall	49.73	6.83	48.50	6.64	49.58	6.59	48.95	6.68	50.24	6.67	49.19	6.71	50.65	6.64
	btw		6.79		6.47		6.39		6.52		6.47		6.56		6.44
	within		1.70		1.70		1.91		1.70		1.89		1.70		1.92
HSgrads	overall	79.61	4.88	78.92	4.93	79.52	4.67	79.20	4.93	79.79	4.58	79.31	4.92	79.86	4.50
	btw		4.98		4.94		4.66		4.94		4.57		4.93		4.49
	within		0.72		0.74		0.83		0.73		0.82		0.72		0.82
%foreign	overall	9.70	0.07	8.99	7.25	9.40	7.29	9.06	7.15	9.85	7.26	9.19	7.15	10.36	7.44
	btw		0.07		7.18		7.19		7.09		7.18		7.10		7.36
	within		0.01		0.91		1.14		0.88		1.10		0.87		1.11
N(obs)		1,812,094		442,059		127,556		820,146		122,783		1,055,250		99,674	
N(indivs)		297,817		58,822		14,991		107,188		13,983		136,787		11,172	

*The log of the wage ratio (ln (wg_rat)) is threshold-specific.

Table 1b: Descriptive Statistics--Categorical Variables

	all		2pov opps		2pov exits		3pov opps		3pov exits		4pov opps		4pov exits	
Variable	obs	indivs	obs	indivs	obs	indivs	obs	indivs	obs	indivs	obs	indivs	obs	indivs
kids <18:														
0	52.77	60.18	51.74	60.01	53.69	63.62	52.69	60.68	54.51	62.91	52.76	60.63	53.34	61.54
1	20.01	27.48	21.49	32.80	21.29	33.64	20.81	31.54	19.67	31.17	20.58	31.11	19.78	31.37
2	17.57	22.05	16.32	24.47	15.67	23.93	16.89	24.33	17.24	24.54	17.32	24.51	18.41	25.90
3+	9.64	11.54	10.44	14.23	9.45	12.95	9.60	13.00	8.59	11.55	9.34	12.52	8.47	11.43
total	100.00	121.50	100.00	131.51	100.00	134.15	100.00	129.54	100.00	130.18	100.00	128.76	100.00	130.25
educ attain:														
<HS	11.55	15.26	17.63	22.26	11.00	13.69	14.13	17.74	5.95	7.29	12.28	15.48	3.78	4.76
HS	31.72	35.22	38.76	43.60	34.55	38.28	37.95	41.64	28.50	30.34	36.19	39.49	22.85	24.12
some coll.	32.24	34.37	33.29	37.60	37.12	42.05	33.88	37.16	35.27	38.37	33.98	36.82	32.55	34.83
>=BA	24.48	24.14	10.32	11.76	17.33	20.18	14.04	15.09	30.27	32.48	17.56	18.35	40.82	42.36
total	100.00	109.00	100.00	115.22	100.00	114.20	100.00	111.62	100.00	108.48	100.00	110.15	100.00	106.07
race:														
white	77.66	77.00	70.03	69.78	74.52	74.42	73.65	73.19	80.37	80.23	75.82	75.26	83.66	83.39
black	11.52	11.84	14.45	14.51	12.80	12.83	13.16	13.35	10.85	10.89	12.27	12.53	9.41	9.53
Hispanic	10.82	11.26	15.51	15.71	12.68	12.74	13.18	13.46	8.77	8.88	11.91	12.21	6.93	7.08
N	1,812,094	297,817	442,059	58,822	127,556	14,991	820,146	107,188	122,783	13,983	1,055,250	136,787	99,674	11,172

Table 2: Descriptive Statistics--State-Level

Variable		Mean	Std.Dev.	Min	Max	Obs
unemp	overall	5.03	1.22	1.80	9.52	N = 2417
	between		1.01	2.93	7.59	n = 51
	within		0.71	2.47	7.04	T-bar = 47.3922
st_medinc	overall	49.65	7.29	33.67	71.85	N = 2417
	between		7.13	35.83	65.94	n = 51
	within		1.84	45.04	55.90	T-bar = 47.3922
HSgrads	overall	81.34	5.10	68.23	91.20	N = 2417
	between		5.05	70.96	90.04	n = 51
	within		1.18	78.14	85.75	T-bar = 47.3922
% foreign born	overall	6.89	5.37	0.97	26.27	N = 2417
	between		5.39	1.03	25.09	n = 51
	within		0.52	5.00	9.96	T-bar = 47.3922

based on months with potential twopov transitions

Table 3—Model 1
Results of Three-Level Logistic Regression Predicting Likelihood of Upward Transition: State and Individual Random Intercepts Only

VARIABLES	twopov_exit	threepov_exit	fourpov_exit
std. dev. of state random intercept	0.261***	0.196***	0.00869***
	(0.0395)	(0.0282)	(0.00112)
Std. dev of indiv. random intercept	1.180***	0.923***	2.305***
	(0.0286)	(0.0218)	(0.0304)
Observations	191,480	415,136	570,058
Number of groups	51	51	51

seEform in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4—Model 2
Odds Ratios of Upward Transition: No Interactions

VARIABLES	twopov_exit	threepov_exit	fourpov_exit
age	1.102***	1.137***	1.118***
	(0.00781)	(0.00945)	(0.0109)
age_sq	0.999***	0.998***	0.999***
	(9.21e-05)	(0.000104)	(0.000119)
female	0.572***	0.551***	0.566***
	(0.0143)	(0.0142)	(0.0156)
1.marital status	1.265***	1.220***	1.235***
	(0.0345)	(0.0340)	(0.0375)
# children (0 omitted)			
1	0.915***	0.899***	0.934*
	(0.0274)	(0.0291)	(0.0328)
2	0.810***	0.888***	0.910**
	(0.0287)	(0.0323)	(0.0351)
3+	0.802***	0.893**	0.895**
	(0.0346)	(0.0418)	(0.0455)
educational attainment (<HS omitted)			
HS	1.549***	1.737***	1.726***
	(0.0605)	(0.0885)	(0.113)
some college	2.170***	2.708***	2.713***
	(0.0881)	(0.140)	(0.178)
B.A. or higher	5.188***	8.681***	8.007***
	(0.259)	(0.495)	(0.550)
race (white omitted)			
black	0.820***	0.777***	0.847***
	(0.0306)	(0.0316)	(0.0384)
Hispanic	0.689***	0.633***	0.669***
	(0.0278)	(0.0290)	(0.0351)
disabled	0.572***	0.662***	0.718***
	(0.0302)	(0.0396)	(0.0490)
union	2.351***	1.865***	1.324***
	(0.103)	(0.0652)	(0.0439)
ln_wage_ratio_twopov	9.600***		
	(0.480)		
ln_wage_ratio_threepov		23.74***	
		(1.175)	
ln_wage_ratio_fourpov			31.23***
			(1.566)
unemp	0.931***	0.938***	0.965**
	(0.0122)	(0.0125)	(0.0138)
st_medinc	1.028***	1.023***	1.011*
	(0.00550)	(0.00558)	(0.00590)
HSgrads	1.081***	1.106***	1.091***
	(0.0136)	(0.0144)	(0.0150)
% foreign-born	1.060***	1.078***	1.078***
	(0.0201)	(0.0211)	(0.0225)
Observations	191,480	415,134	570,056
Number of idnew	58,822	107,188	136,787
sigma_u	1.324	1.657	1.639
rho	0.347	0.455	0.450

seEform in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5—Model 3
Odds Ratios of Upward Transition: Unemployment Interactions

VARIABLES	twopov _exit	threepov_exit	fourpov_exit
age	1.102***	1.137***	1.117***
	(0.00780)	(0.00944)	(0.0109)
age_sq	0.999***	0.998***	0.999***
	(9.21e-05)	(0.000104)	(0.000119)
female	0.473***	0.486***	0.644***
	(0.0428)	(0.0452)	(0.0646)
1.marital status	1.267***	1.222***	1.236***
	(0.0345)	(0.0340)	(0.0375)
# children (0 omitted)			
1	0.915***	0.900***	0.935*
	(0.0274)	(0.0291)	(0.0328)
2	0.810***	0.889***	0.911**
	(0.0287)	(0.0323)	(0.0351)
3+	0.803***	0.895**	0.897**
	(0.0346)	(0.0418)	(0.0455)
educational attainment (<HS omitted)			
HS	1.381**	1.384*	1.145
	(0.207)	(0.268)	(0.287)
some college	1.841***	2.002***	1.679**
	(0.277)	(0.386)	(0.416)
B.A. or higher	3.791***	5.528***	4.039***
	(0.662)	(1.105)	(1.003)
race (white omitted)			
black	0.820***	0.777***	0.847***
	(0.0306)	(0.0316)	(0.0384)
Hispanic	0.693***	0.638***	0.673***
	(0.0280)	(0.0292)	(0.0353)
disabled	0.572***	0.662***	0.719***
	(0.0302)	(0.0396)	(0.0490)
union	2.351***	1.867***	1.324***
	(0.103)	(0.0653)	(0.0439)
ln_wage_ratio_twopov	9.590***		
	(0.479)		
ln_wage_ratio_threepov		23.70***	
		(1.172)	
ln_wage_ratio_fourpov			31.22***
			(1.565)
unemp	0.890***	0.880***	0.891***
	(0.0229)	(0.0284)	(0.0368)
female#unemp	1.035**	1.023	0.977
	(0.0164)	(0.0166)	(0.0170)
educ. attain.#unemp (<HS#educ omitted)			
HS#unemp	1.020	1.040	1.074*
	(0.0264)	(0.0346)	(0.0460)
some college#unemp	1.030	1.054	1.087**
	(0.0267)	(0.0349)	(0.0459)
B.A. or higher#unemp	1.058*	1.083**	1.128***
	(0.0318)	(0.0369)	(0.0476)
st_medinc	1.028***	1.023***	1.011*
	(0.00549)	(0.00557)	(0.00589)
HSgrads	1.081***	1.105***	1.091***
	(0.0136)	(0.0144)	(0.0150)
% foreign-born	1.060***	1.077***	1.077***
	(0.0201)	(0.0211)	(0.0224)
(state and panel dummy variables omitted)			

Observations	191,480	415,134	570,056
Number of idnew	58,822	107,188	136,787
sigma_u	1.321	1.654	1.634
rho	0.347	0.454	0.448

seEform in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6—Model 4
Odds Ratios of Upward Transition: Gender, Marital Status, and Educational Attainment Interactions

	(1)	(3)	(5)
VARIABLES	low_wage_exit	threepov_exit	fourpov_exit
age	1.101***	1.137***	1.119***
	(0.00781)	(0.00948)	(0.0109)
age_sq	0.999***	0.998***	0.999***
	(9.22e-05)	(0.000104)	(0.000119)
female	0.440***	0.373***	0.457***
	(0.0418)	(0.0607)	(0.110)
1.marital status	1.255***	1.232*	1.135
	(0.107)	(0.136)	(0.166)
(female#0.marital_status omitted)			
female#1.marital_status	0.936	1.060	1.014
	(0.122)	(0.220)	(0.311)
educational attainment (<HS omitted)			
HS	1.262***	1.385***	1.367**
	(0.0899)	(0.139)	(0.188)
some college	1.513***	2.092***	2.287***
	(0.109)	(0.210)	(0.309)
B.A. or higher	3.747***	6.995***	6.858***
	(0.335)	(0.749)	(0.940)
(female#<HS omitted)			
female#HS	1.434***	1.689***	1.396
	(0.154)	(0.297)	(0.357)
female#some college	1.746***	1.845***	1.349
	(0.186)	(0.319)	(0.338)
female#B.A. or higher	2.359***	2.248***	1.520*
	(0.297)	(0.400)	(0.380)
(1.marital_status#<HS omitted)			
1.marital_status#HS	1.437***	1.308**	1.368*
	(0.145)	(0.162)	(0.221)
1.marital_status#some college	1.491***	1.336**	1.257
	(0.155)	(0.166)	(0.200)
1.marital_status#B.A. or higher	0.963	0.962	1.053
	(0.123)	(0.127)	(0.169)
(female#0.marital_status#educ omitted)			
(female#1.marital_status#<HS omitted)			
female#1.marital_status#HS	0.624***	0.658*	0.736
	(0.0941)	(0.148)	(0.240)
female#1.marital_status#some college	0.691**	0.615**	0.780
	(0.105)	(0.137)	(0.249)
female#1.marital_status#B.A. or higher	0.623***	0.664*	0.878
	(0.112)	(0.153)	(0.280)
# children (0 omitted)			
1	0.902***	0.885***	0.925**
	(0.0271)	(0.0289)	(0.0328)
2	0.799***	0.870***	0.899***
	(0.0284)	(0.0319)	(0.0349)
3+	0.789***	0.871***	0.882**
	(0.0342)	(0.0410)	(0.0451)
race (white omitted)			
black	0.802***	0.769***	0.844***
	(0.0300)	(0.0313)	(0.0383)
Hispanic	0.673***	0.625***	0.665***

	(0.0271)	(0.0286)	(0.0350)
disabled	0.565***	0.658***	0.716***
	(0.0298)	(0.0393)	(0.0489)
union	2.352***	1.855***	1.311***
	(0.103)	(0.0649)	(0.0436)
ln_wage_ratio_twopov	9.270***		
	(0.461)		
ln_wage_ratio_threepov		22.95***	
		(1.135)	
ln_wage_ratio_fourpov			30.67***
			(1.539)
unemp	0.932***	0.938***	0.965**
	(0.0121)	(0.0125)	(0.0138)
st_medinc	1.028***	1.023***	1.012**
	(0.00549)	(0.00557)	(0.00590)
HSgrads	1.079***	1.105***	1.092***
	(0.0136)	(0.0144)	(0.0150)
foreign	1.058***	1.078***	1.079***
	(0.0200)	(0.0211)	(0.0225)
(state and panel dummy variables omitted)			
Observations	191,480	415,134	570,056
Number of idnew	58,822	107,188	136,787
sigma_u	1.310	1.649	1.636
rho	0.343	0.452	0.449

seEform in parentheses

*** p<0.01, ** p<0.05, * p<0.1

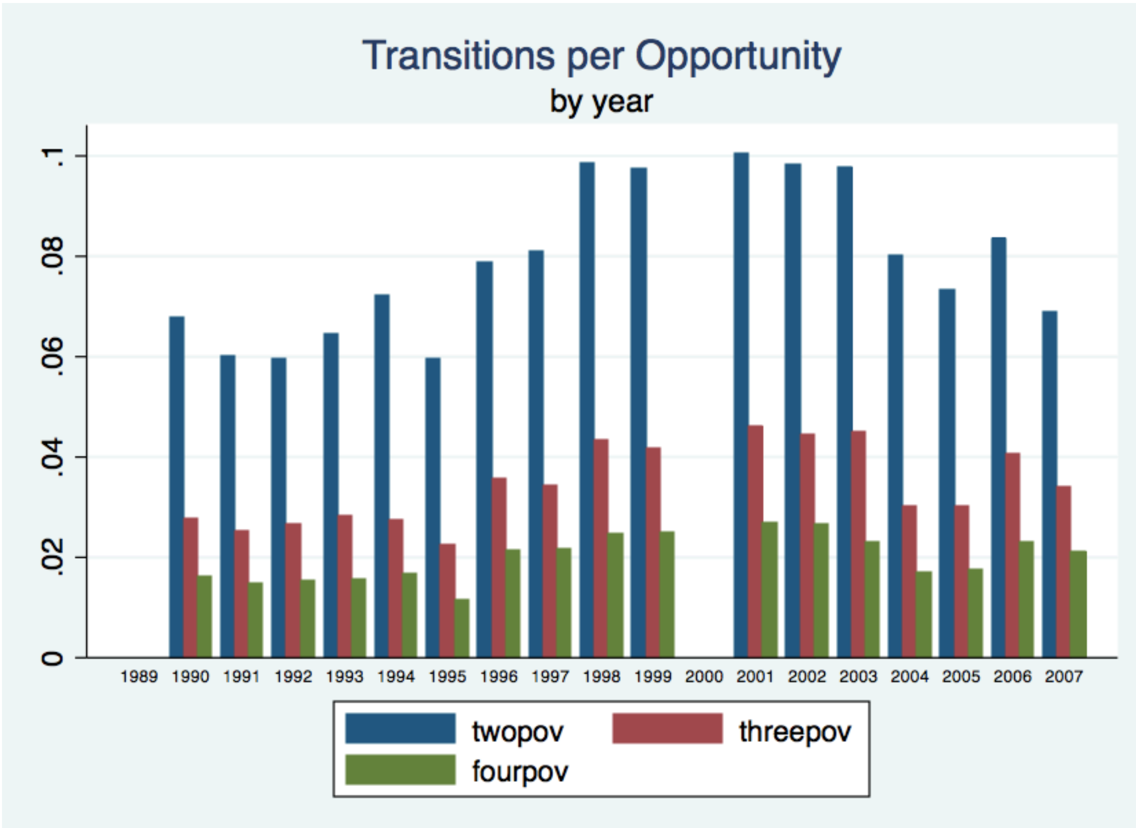

Transitions per Opportunity
by year
0
.02
.04
.06
.08
.1
1989 1990 1991 1992 1993 1994 1995 1996 1997 1998 1999 2000 2001 2002 2003 2004 2005 2006 2007
twopov
threepov
fourpov

Fig 1

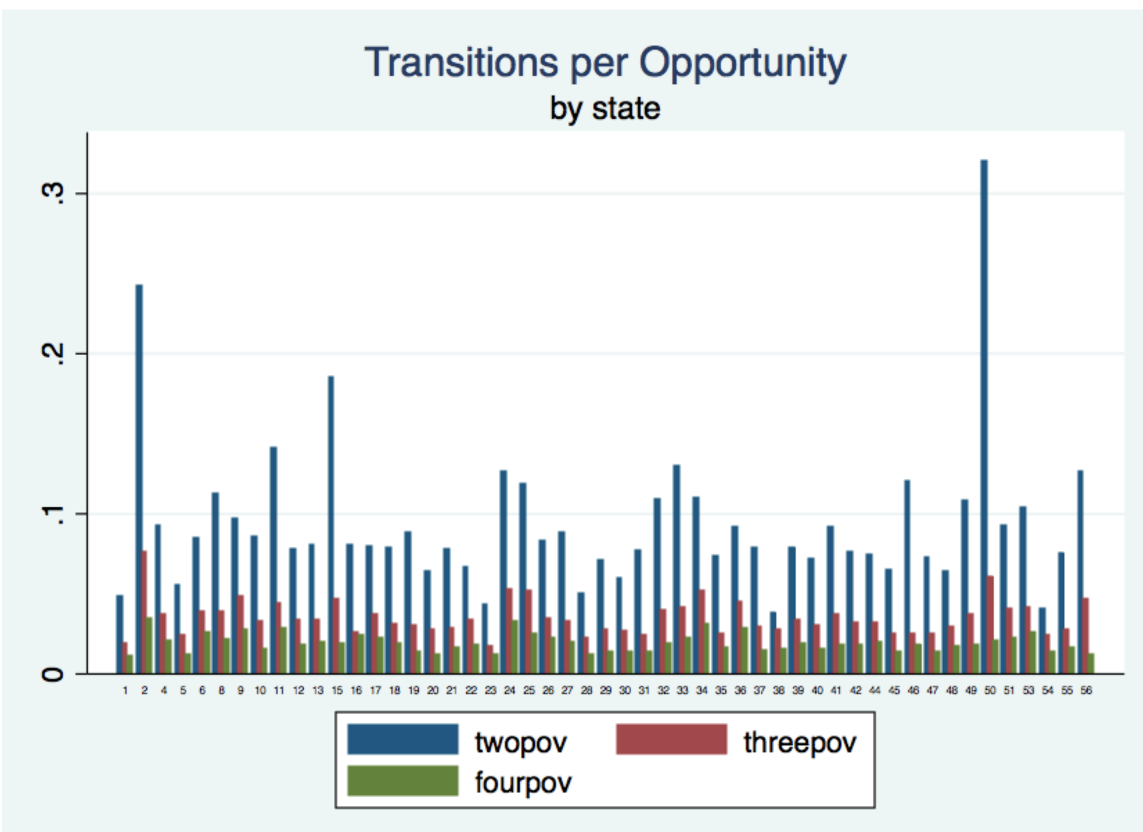

Transitions per Opportunity
by state
0
.1
.2
.3
1 2 4 5 6 8 9 10 11 12 13 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40 41 42 44 45 46 47 48 49 50 51 53 54 55 56
twopov
threepov
fourpov

Fig 2

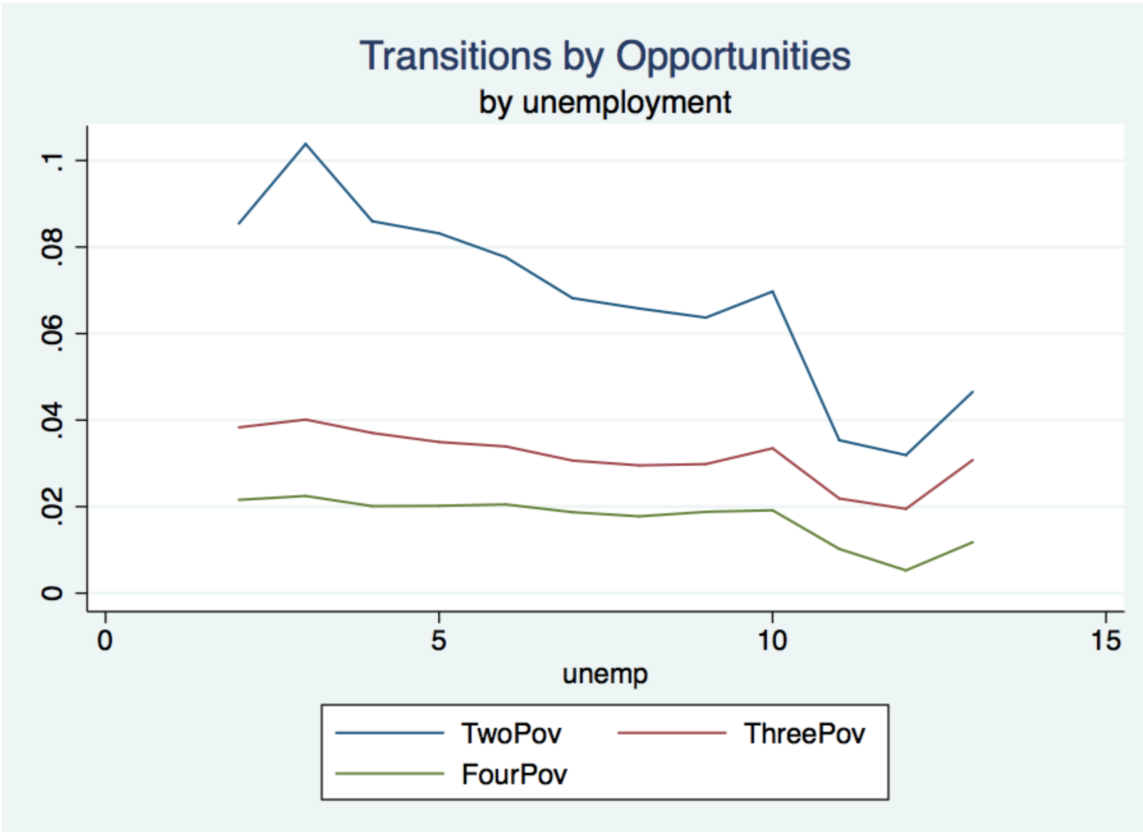

Transitions by Opportunities
by unemployment
0
.02
.04
.06
.08
.1
0
5
10
15
unemp
TwoPov
ThreePov
FourPov

Fig 3

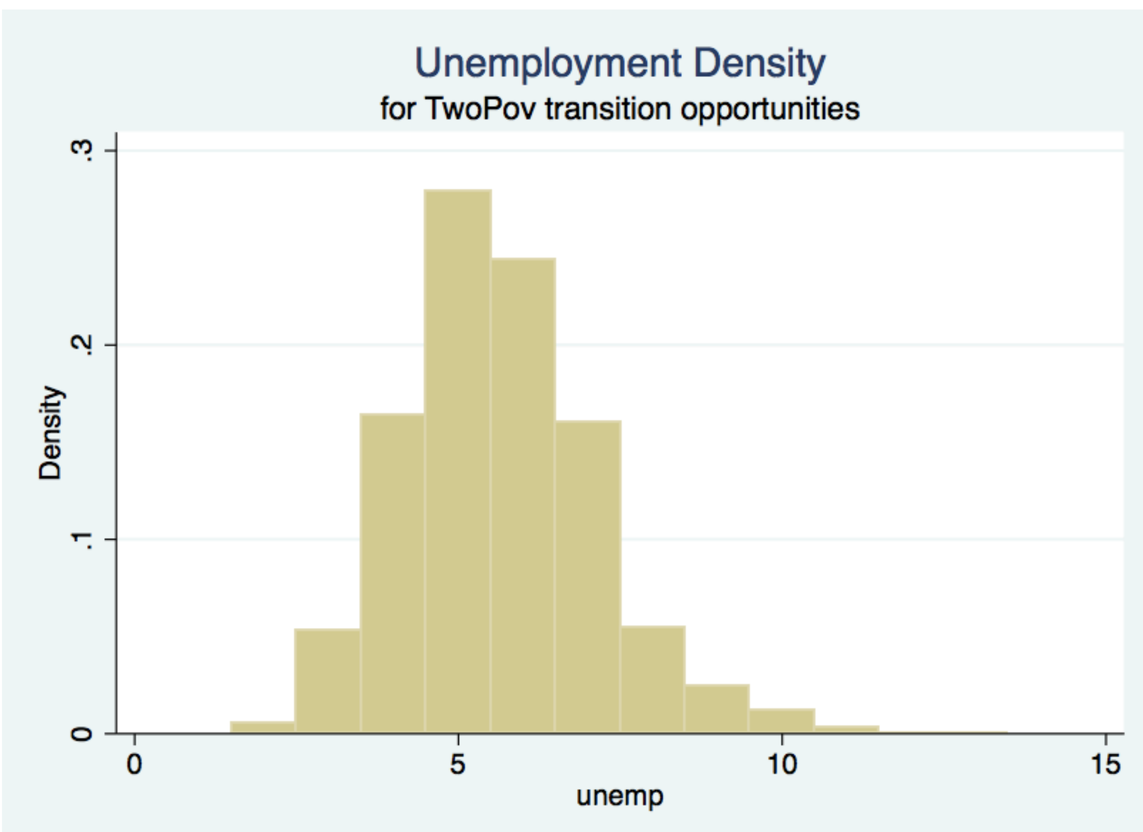
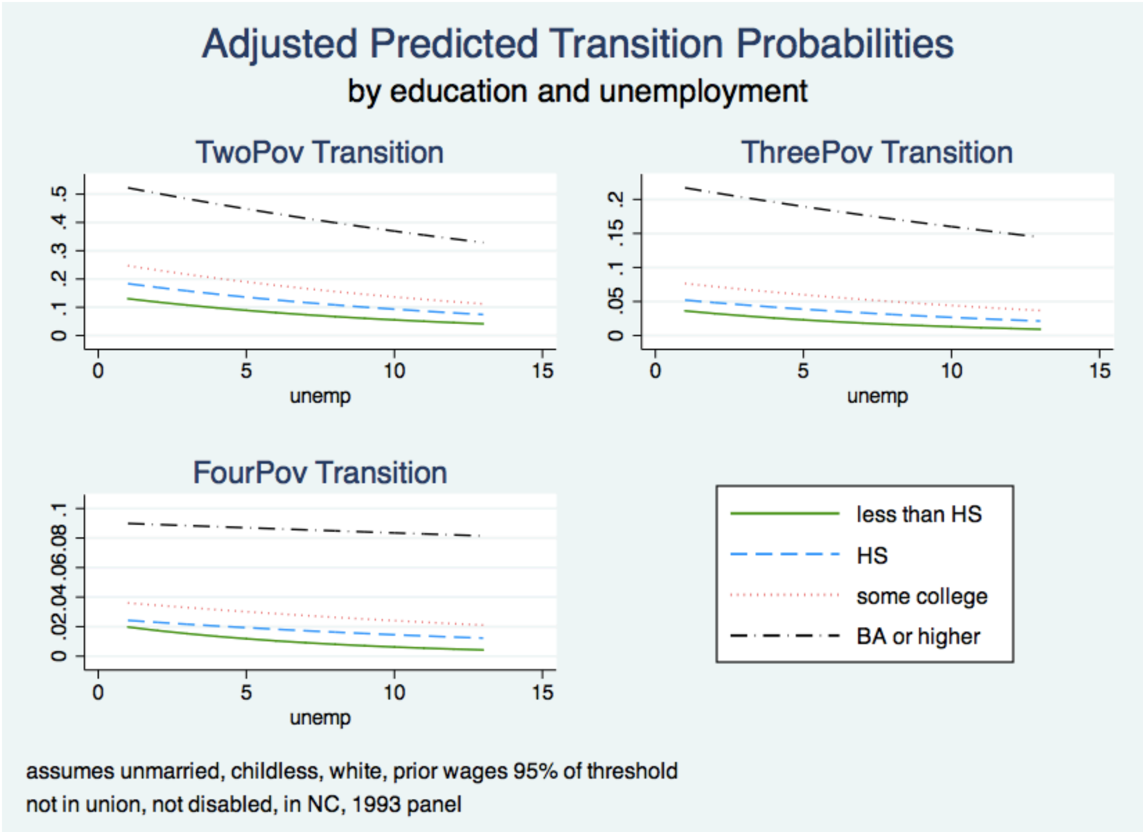
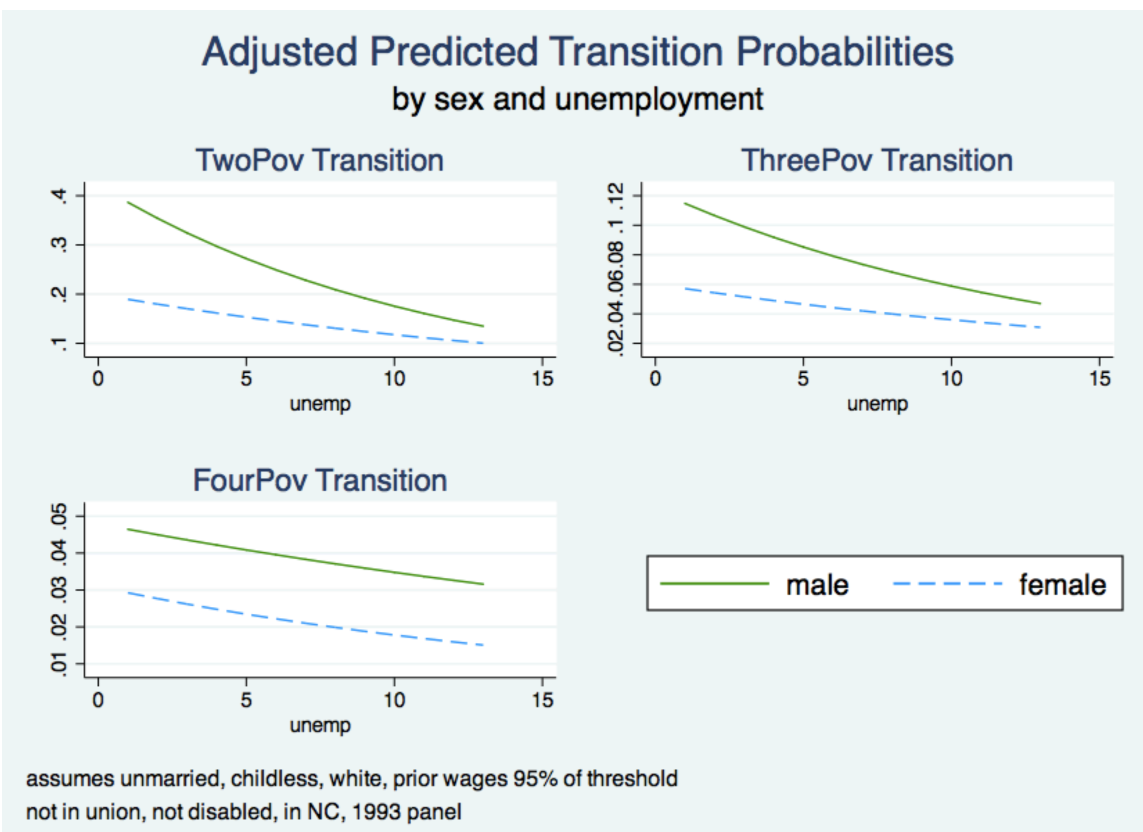

Unemployment Density
for TwoPov transition opportunities
Density
0
.1
.2
.3
0
5
10
15
unemp

Fig 4



Adjusted Predicted Transition Probabilities
by education and unemployment
TwoPov Transition
ThreePov Transition
FourPov Transition
unemp
less than HS
HS
some college
BA or higher
assumes unmarried, childless, white, prior wages 95% of threshold
not in union, not disabled, in NC, 1993 panel

Fig 5



Adjusted Predicted Transition Probabilities
by sex and unemployment
TwoPov Transition
ThreePov Transition
FourPov Transition
unemp
male
female
assumes unmarried, childless, white, prior wages 95% of threshold
not in union, not disabled, in NC, 1993 panel

Fig 6

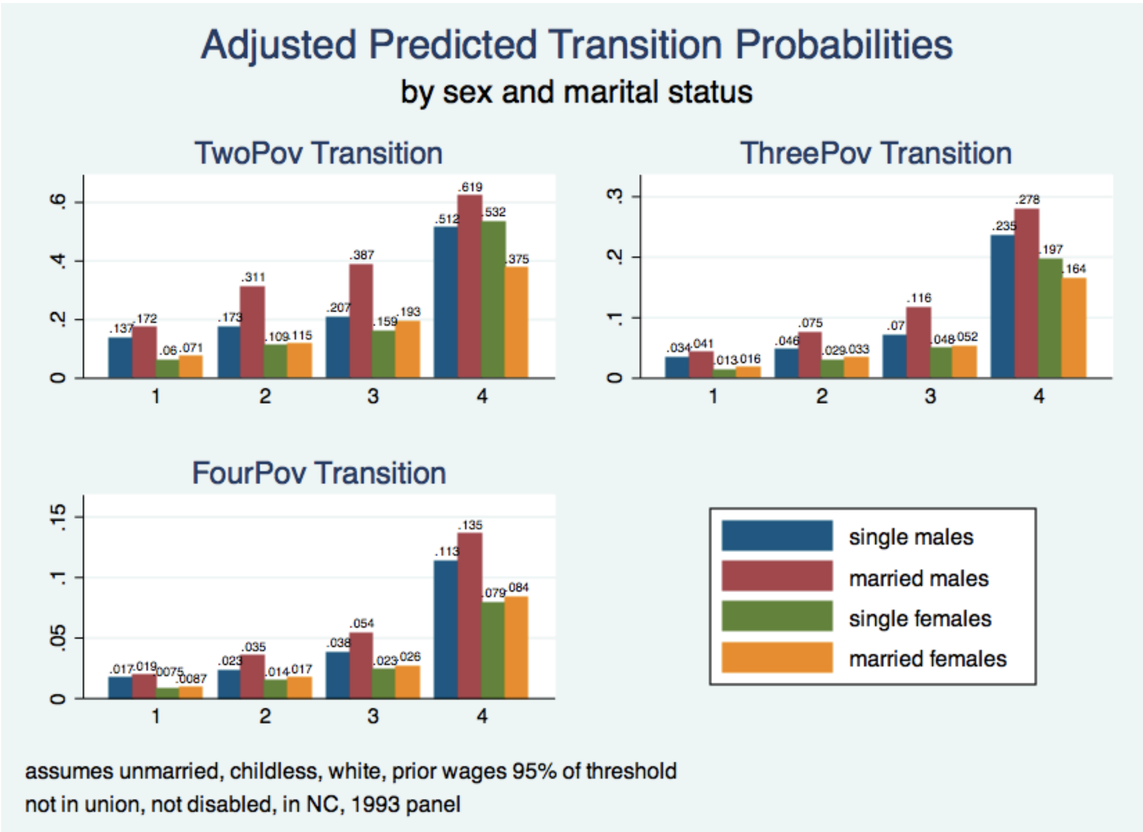

Adjusted Predicted Transition Probabilities
by sex and marital status
TwoPov Transition
ThreePov Transition
FourPov Transition
single males
married males
single females
married females
assumes unmarried, childless, white, prior wages 95% of threshold
not in union, not disabled, in NC, 1993 panel

Fig 7

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