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OCCUPATIONAL MOBILITY AND THE BUSINESS CYCLE

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ABSTRACT

Do workers sort more randomly across different job types when jobs are harder to find? To answer this question, we study the mobility of male workers among three-digit occupations in the matched files of the monthly Current Population Survey over the 1979-2004 period. We clean individual occupational transitions using the algorithm proposed by Moscarini and Thomsson (2008). We then construct a synthetic panel comprising annual birth cohorts, and we examine the respective roles of three potential determinants of career mobility: individual ex ante worker characteristics, both observable and unobservable, labor market prospects, and ex post job matching. We provide strong evidence that high unemployment somewhat offsets the role of individual worker considerations in the choice of changing career. Occupational mobility declines with age, family commitments and education, but when unemployment is high these negative effects are weaker, and reversed for college education. The cross-sectional dispersion of the monthly series of residuals is strongly countercyclical. As predicted by Moscarini (2001)'s frictional Roy model, the sorting of workers across occupations is noisier when unemployment is high. As predicted by job-matching theory, worker mobility has significant residual persistence over time. Finally, younger cohorts, among those in the sample for most of their working lives, exhibit increasingly low unexplained career mobility.

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1 Introduction

A major function of labor markets is to allocate people efficiently, namely, to ensure that each worker finds the right job. Does unemployment make this process noisier? Do workers sort more randomly when jobs are scarce? In this paper, we precisely formulate these questions in a theoretical framework, and we present new empirical evidence in favor of a positive answer.

A prominent tradition in macroeconomics, going back to Schumpeter (1939), emphasizes the continuous reallocation of resources across heterogeneous production units as the “mode” of aggregate business fluctuations and economic growth. If capital is a quasi-fixed factor, technological progress can only be implemented through the “creative destruction” of installed capital and the reallocation of labor to new production processes. Recent empirical work on establishment-level and matched employer-employee longitudinal data sets supports two central tenets of this tradition. Substantial idiosyncratic heterogeneity remains in the productivities of firms and workers after conditioning on their observable characteristics (Abowd, Kramarz and Margolis (1999), Mortensen (2003)) and persists through time at the firm level (Haltiwanger, Lane and Spletzer (2000)). The reallocation of existing inputs across establishments explains half of total productivity growth in US manufacturing (see Haltiwanger (2000) for a survey). Aggregate labor market flows have taken center stage.¹

In this paper, we study the reallocation of male workers among Census three-digit occupations, using micro-data representative of the US population, the monthly matched files of the Current Population Survey (CPS) over the 1979-2004 period. Our main goal is to study the causal effect of cyclical labor market conditions on the process of workers sorting, in this case, through occupations. We argue, and provide evidence, that high unemployment weakens the role of individual worker characteristics in the choice of occupations. Thus, while reallocation is partially determining aggregate fluctuations, there is also an effect in the opposite direction.

The theoretical foundation of our empirical work is Moscarini (2001)’s version of the Roy model with search and matching frictions. In the equilibrium of that model, when unemployment rises, and job offers are slower to materialize, workers pay less attention on locating in the specific occupations which are best suited to their skills and focus more on remaining employed. As a consequence, the predictive power of individual worker characteristics for their occupational mobility should be lower in a depressed labor market. This is, indeed, the case in our sample, which suggests that the quality of sorting and employment reallocation

¹For job turnover see Davis, Haltiwanger and Schuh (1996), Davis, Faberman and Haltiwanger (2006). For worker turnover, see Murphy and Topel (1987), Blanchard and Diamond (1990), Fallick and Fleischman (2004), Moscarini and Thomsson (2008).

is procyclical, although this further implication awaits careful scrutiny.

Our focus on three-digit occupations is motivated by two considerations. First, mobility at this level of disaggregation has not only an obvious interest for labor economists, who have investigated career mobility from their own viewpoint, but also for macroeconomists. In the Schumpeterian perspective, the reallocation of a worker is relevant insofar as it implies a change in the technology applied to his or her labor services. By definition, *a change of occupation necessarily entails a change of technology for the worker*, while the same conclusion does not necessarily hold for a change of employer or sector. For example, a secretary may perform the same tasks for different employers in different industries. Hence, research on the creative destruction process should be primarily concerned with occupational mobility. Indeed, Kambourov and Manovskii (2007) find evidence that the bulk of specific human capital accumulation occurs at the level of occupation, not of industry or job.

Second, we focus on a much finer degree of disaggregation of occupations, at the three-digit level, than most of the existing literature, because this degree appears to more closely correspond to a “career.” In contrast to the few one-digit occupational groups commonly considered—such as technical and sales, laborers etc.—there exist over 450 three-digit occupations. Important moves at the three-digit level can be easily missed at the one-digit level. For example, an examination of the Census Occupational codes reveals that the clearly distinct three-digit categories Architects, Dieticians, and History Teachers are all included in the same one-digit group “Managerial and Professional Specialty occupations.”² Since the raw data on occupational transitions appear to be extremely noisy in the monthly CPS, we adopt occupational transition data from Moscarini and Thomsson (2008), based on matched monthly CPS files and cleaned by an algorithm which crucially exploits the short longitudinal dimension of the CPS rotating panel.

Our choice of data source and sample period is due to a combination of factors. Ideally, we would like a long time series to examine the behavior of the mobility measure under different macroeconomic conditions. However, for a microeconomic study of the factors determining mobility one needs a consistent data set comprising the same variables measured in a consistent manner through time. These considerations lead us to focus on the 1979-2004 CPS monthly files. In this data set, Moscarini and Thomsson (2008) find the pattern illustrated in Figure 1. The reallocation of employed men across three-digit occupations averages about 3.5% per month, is clearly procyclical,³ increases from the late 1970s, peaks in the mid

²Finer classifications are not available in the CPS data that we employ. In the Standard Occupational Classification, the 3-digit category Architects (e.g.) is divided into such 4-digit categories as Landscape Architects, Architectural Designers, Supervising Architects, and the like. However, job switches among these finer 4-digit occupations are not particularly significant in terms of skill reallocation, while job changes among the Census 3-digit categories definitely are.

³Similarly, Jovanovic and Moffitt (1990) find in the NLS that worker reallocation across three broadly

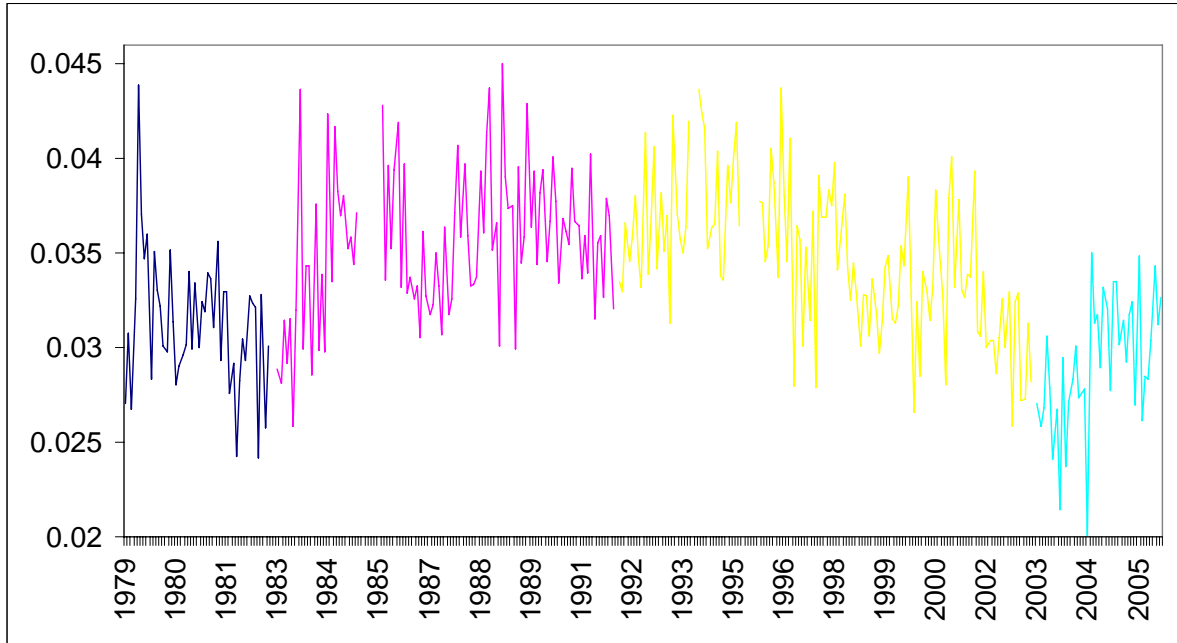


Figure 1: Monthly 3-digit Occupational Mobility, Men (from Moscarini and Thomsson (2008))

1990s, then drops, sharply after the 2001 recession.⁴ We attempt to estimate the respective contributions of various individual characteristics and aggregate business conditions to the individual mobility decisions underlying this aggregate pattern.

First, different workers have different propensities to change careers. We ask whether the changing composition of the labor force is contributing to the changes in aggregate labor mobility. A similar issue pertains to possible changes over time in worker unobservable characteristics, and we describe our strategy for dealing with this below.

Second, worker reallocation can also be impeded by a variety of frictions, such as ad-

defined sectors was procyclical in the 1970's, with instances of countercyclical churning. Murphy and Topel (1987) find in 1968-1985 CPS data that worker mobility across sectors declined over time and in recessions. Kambourov and Manovskii (2008) study the time series behavior of occupational mobility in the PSID, for the period 1968-1997. Taking into account differences in sample disposition, their findings on annual mobility are quantitatively consistent with the monthly series in Figure 1.

⁴Occupational reclassifications make the level of mobility across subperiods not directly comparable, as shown in Figure 1 with different colors by subperiod. There is, however, no evidence of major discontinuities upon reclassifications. In previous drafts of this paper, using annual March files, we also studied *Net Reallocation*, namely one half of the sum of the absolute changes in occupational employment shares, to measure the reshuffling required to accommodate changes in the distribution of employment across occupations, ignoring offsetting moves that cancel out (Murphy and Topel (1987) and Jovanovic and Moffitt (1990)). In this draft we re-focus on data at monthly frequency, ideal for studying turnover, where the cleaning algorithm of Moscarini and Thomsson (2008) detects genuine occupational transitions and rejects spurious ones, but does not improve the accuracy of the exact occupation a worker is in. Due to this limitation, we do not compute Net Reallocation, which requires precise knowledge of the employment shares of the different occupations.

justment costs to labor on the demand side, the cost of retraining displaced workers on the supply side, search-and-matching frictions on both sides. Then, the state of the labor market interferes with worker sorting. Moscarini (2001) spells out this mechanism when workers sort *ex ante* across different types of jobs, based on information that they know in advance. When few jobs are available, workers accept any job that comes along and are willing to mismatch. When jobs are easy to find, individual comparative advantages matter more, unemployed workers search more selectively and mismatch less. Moreover, employed workers search on the job more intensively to upgrade. An implication is that workers who change occupation less frequently, because they have more specialized skills, are more sensitive to business cycle conditions. We present the simplest version of the model that delivers this prediction, and test this hypothesis explicitly. We consider the effects of the unemployment rate faced by each type of worker, which is an excellent proxy for his/her job-finding rate, on the occupational mobility of different groups of workers, who have different transition rates on average across business cycles.

Finally, the canonical job-matching theory (Jovanovic (1979)) of *ex post* worker sorting, based on the success of the match that cannot be predicted without trying, implies that “separation begets separation.” A displacement may force some workers to accept jobs in new occupations, wasting some accumulated occupation-specific knowledge, and thus raise expected subsequent separations and mobility. McCall (1990) finds supporting evidence of this mechanism for occupations.⁵ Similarly, learning-by-doing on the job reduces the incentives to job-to-job mobility over time (Pissarides 1994), and leads to a similar destruction of specific capital upon a separation. Motivated by these hypotheses, we examine the presence of persistence (dynamic effects) in mobility.

To evaluate the contribution of these three factors, we specify a statistical model of occupational mobility at the individual level that is directly motivated by the structural equations of the theoretical model. Clearly, we face an endogeneity problem related to the work decision. As the ideal data set, a long panel based on a large representative sample and observed at high frequency, does not exist, we continue with the CPS. As this does not provide us with a sufficient number of repeated observations on the same individuals, our key identification assumption is that the unobserved heterogeneity underlying the endogeneity of worker characteristics, including employment status, is birth-cohort specific. This seems a reasonable assumption, since individuals born at approximately the same point in time will be subject to similar unobservable forces such as changing educational or training systems. Accordingly, we construct a pseudo panel which allows us to control for these cohort level

⁵A similar mechanism is emphasized by Pries (2004) as a source of persistence of inflows into unemployment and of the unemployment rate itself.

effects with cohort dummies. This is effectively equivalent to instrumenting all potentially endogenous determinants of occupational mobility, such as education and marital status, with a birth cohort dummy in the original, individual-level data set. Also, averaging across individuals in the same cohort eliminates their individual unobserved heterogeneity. The loss of sample size and within-cell sampling error are not major concerns in our data set, which comprises over a million of month-individual observations. The size of our synthetic panel is about the same, on either dimension, as that of standard individual-level panels, such as the PSID or the NLSY, which suffer from much lower frequency of observation and non-representative sampling.

The economic meaning of our identification assumption and strategy is transparent. While education and occupational mobility may be the result of unobservable worker preferences —e.g., for risk and time—the changes in employment rates and in educational attainments across birth cohorts are more plausibly due to other factors, related to economic growth. Note that even if cohorts differ in their average preferences for risk, time, education and mobility, this can be captured by a cohort dummy. This strategy, proposed by Deaton (1985), and extended by Verbeek and Vella (2005) to a dynamic setting, attains identification under our assumption provided that the covariates of interest are not linearly dependent over time. This data requirement appears to be satisfied by our sample.

Our econometric investigation yields several new results. Occupational mobility declines with age, family commitments (marital and head of household status) and education. All these effects are strong, both statistically and economically, and have a causal interpretation. High unemployment weakens and can even reverse the negative effect of college education and marital status on career mobility. Indeed, the cross-sectional dispersion of regression residuals, computed and plotted month by month, is strongly countercyclical, jumping suddenly by a factor of three in the severe 1981-1982 double recession, and by a factor of two in the milder recessions of 1991 and 2001. This striking fact is consistent with the hypothesis that worker sorting across occupations is noisier in recessions. As predicted by job-matching theory, worker mobility has significant residual persistence over time, unexplained by that of employment composition and of unemployment. Finally, we detect important unobserved cohort-specific effects. In particular, later birth cohorts, except for the most recent that are in the sample only early in their working lives, have increasingly low unexplained occupational mobility, which contributes considerably to the downward trend in total employment reallocation over the period under examination.

Section 2 illustrates our theoretical framework, Section 3 presents our empirical model and our estimation strategy, Section 4 describes the data and some relevant issues, Section 5 presents and discusses the results, Section 6 concludes.

2 A Frictional Roy Economy

Our empirical investigation is motivated by a view of the labor market as a search-frictional Roy model, proposed by Moscarini (2001). Workers are endowed with fixed occupation-specific skills, which determine their comparative advantages. Given skill prices, workers self-select across occupations. This sorting finds a partial impediment in search frictions, which make it costly and time-consuming to find a job both from unemployment and from employment. When unemployment is higher, frictions bite more and individual comparative advantages become less relevant in a worker's occupational choice. This is the key prediction of the model that we will test. We now present the simplest version of the model that illustrates and formally delivers this prediction.

Time is discrete. The economy is inhabited by an unit measure of infinitely-lived risk neutral workers, who are endowed with a skill $x \in [0, 1]$ distributed according to a given skill density $g(x)$, symmetric around $1/2$. Workers maximize expected wealth discounted with factor $\varrho < 1$. When jobless, workers enjoy a flow value of leisure $b(x)$.

The economy has two occupations $i = 0, 1$. There is a continuum of firms of each type i , ensuring free entry in both occupations. Firms and workers match 1:1. Output in occupation i is $pf_i(x)$, where p is aggregate TFP, f_0 is increasing, and $f_1(\cdot) = 1 - f_0(\cdot)$, to capture the idea that low x workers have a comparative advantage to work in occupation 0. Exogenous idiosyncratic productivity shocks force the closure of active matches in each occupation i with probability $\delta(x) > 0$ each period. Unemployed workers of type x find job offers with chance $\lambda(x)$, and employed workers find outside offers with chance $\psi(x)\lambda(x)$. Search is costless. Here $\delta(x)$, $\lambda(x)$ and $\psi(x)$ are taken as given, but can easily be endogenized via ex post job matching heterogeneity, a matching function, a vacancy flow cost, and free entry. Both $\delta(x)$ and $\psi(x)$ can be occupation-dependent.

When contacting another firm, an employed worker triggers an ascending auction between that firm and the current employer. In equilibrium under perfect information, no bidding takes place as the weakly more productive firm can preempt the opponent. The worker continues with the more productive of the two firms, and keeps bargaining on the wage. See Moscarini (2001) for details.

We focus on steady state equilibria. The Bellman equation of an unemployed worker reads

$$V^u(x) = b(x) + \frac{\lambda(x)}{2}\varrho [\max \langle V_0^e(x), V^u(x) \rangle + \max \langle V_1^e(x), V^u(x) \rangle] + [1 - \lambda(x)]\varrho V^u(x).$$

The unemployed worker of type x enjoys a flow value of leisure $b(x)$, finds a job next period with chance $\lambda(x)$, in which case the job can be in either occupation with, due to the symmetry

assumptions, equal chances. The value of employment in occupation i is $V_i^e(x)$. This solves the Bellman equation $V_i^e(x) = \max \langle W_i(x), V^u(x) \rangle$ where for $i = 0, 1$:

$$\begin{aligned} W_i(x) &= w_i(x) + \delta(x)V^u(x) + (1 - \delta(x))(1 - \psi(x)\lambda(x))\rho V_i^e(x) \\ &\quad + (1 - \delta(x))\psi(x)\lambda(x)\rho \max \langle V_0^e(x), V_1^e(x) \rangle. \end{aligned}$$

After receiving the wage $w_i(x)$, the worker may either be displaced, with chance $\delta(x)$, or stay where he is, or receive an outside offer, which is acceptable only if it yields more than the current job.

Let $I_i(x) = I \{V_i^e(x) < V_{1-i}^e(x)\}$ denote the indicator function of the worker's willingness to switch occupation should the opportunity arise. The Bellman equation of an operating firm in occupation i employing worker x is $\max \langle J_i(x), 0 \rangle$ where free entry yields a zero value when vacant, and

$$J_i(x) = pf_i(x) - w_i(x) + (1 - \delta(x))(1 - I_i(x))\rho J_i(x).$$

Rents are shared and wages are set by Nash bargaining, with a share γ going to the worker:

$$\gamma J_i(x) = (1 - \gamma) [V_i^e(x) - V_i^u(x)].$$

Using this condition, we can solve for the wage $w_i(x)$ in each occupation as a function of the values. Then, we place it in the Bellman equations and solve for the value functions.

Optimal search and separation behavior is easily described. Without search and matching frictions, workers with skill $x < 1/2$ would only accept jobs in occupation 0, and the others only a job in occupation 1, breaking ties in favor of the latter. With frictions, assuming that the value of leisure $b(x)$ varies slowly with x , there exists $M \in (0, 1/2)$ which solves:

$$V_0^e \left(\frac{1}{2} - M \right) = V^u \left(\frac{1}{2} - M \right)$$

such that the “specialists” who accept only jobs in one of the occupations are restricted to the skill sets $[0, 1/2 - M]$ for occupation 0 and $[1/2 + M, 1]$ for occupation 1, and symmetrically for occupation 1 (by symmetry, the value of M is the same in either case.) Workers with intermediate skills $(1/2 - M, 1/2 + M)$ do not have sufficiently strong comparative advantages to afford rejecting any offer. Of them, those with skills in $(1/2 - M, 1/2)$ and who end up in occupation 1 are mismatched, as they could produce more in occupation 0, and similarly for workers with skills in $(1/2, 1/2 + M)$ who are employed in the other occupation. So M is a measure of mismatch, which is in turn the engine of offsetting worker flows across occupations.

Flow balance equations determine the density of unemployed $u(x)$ and employed $g(x) - u(x)$ workers by skill x . Clearly, for x in the region $[0, 1/2 - M] \cup [1/2 + M, 1]$ where workers search selectively only in one occupation, there are only exogenous separations and no flows to the other occupation. In the skill set $(1/2 - M, 1/2 + M)$ of workers who accept both kinds of jobs, there are also accessions from and separations to the other occupation, but in a symmetric model in steady state they cancel out. So for all skill levels exogenous separations (separation rate times employment) equal total hires from unemployment:

$$\delta(x) [g(x) - u(x)] = \lambda(x) u(x) \quad (1)$$

which can be solved for $u(x)$.

How many of the employed in each occupation are mismatched? Inflow into the “wrong” occupation can only occur from unemployment, as employed workers do not quit from a job in the “wrong” direction, while the outflow can occur both to unemployment and through an upgrading to the other occupation. By symmetry, there is an even chance that a received offer is in the “right” occupation. Thus the density $e(x)$ of mismatched workers solves

$$\frac{\lambda(x)}{2} u(x) = \left[\delta + \psi(x) \frac{\lambda(x)}{2} \right] e(x).$$

Solving for $e(x)$, replacing for $u(x)$ from (1), and simplifying yields

$$e(x) = \frac{\delta(x) [g(x) - u(x)]}{2\delta(x) + \psi(x)\lambda(x)} = \frac{\lambda(x)\delta(x)g(x)}{[2\delta(x) + \psi(x)\lambda(x)] [\delta(x) + \lambda(x)]}.$$

So the density of mismatched of skill x are a fraction $[2 + \psi(x)\lambda(x)/\delta(x)]^{-1} < 1/2$ of the employed at skill x . This fraction decreases with the unemployment rate (as $\lambda(x)/\delta(x)$ increases). The number of mismatched workers is thus countercyclical.

To derive testable implications, we focus on workers employed in consecutive periods. The chance that a worker employed at times t and $t + 1$ is observed in different occupations equals the chance of three mutually exclusive events: (i) he is in the mismatched set and he is employed in the wrong occupation but finds a job in the other occupation without experiencing unemployment, (ii) the same without any intervening unemployment, and (iii) he is in the mismatched set, employed in the right type of job, loses his job, spends less than one period unemployed, and finds another job, but this time in the wrong occupation. With a slight abuse of notation, in order to be consistent with our monthly data set, we now allow for time aggregation and the possibility that a worker loses a job and finds another one in between periods. So:

$$\Pr(\text{employed at } t \text{ and } t+1, \text{ switches occupation}) = \begin{cases} \Phi(x) & \text{for } x \in (1/2 - M, 1/2 + M) \\ 0 & \text{otherwise} \end{cases}.$$

where

$$\begin{aligned}\Phi(x) &= : \frac{e(x)}{g(x) - u(x)} [(1 - \delta(x)) \psi(x) + \delta(x)] \frac{\lambda(x)}{2} + \frac{g(x) - u(x) - e(x)}{g(x) - u(x)} \delta(x) \frac{\lambda(x)}{2} \\ &= \frac{\lambda(x)\delta(x)}{2} \frac{1 + \psi(x) \frac{\lambda(x)}{\delta(x)} + \psi(x) \frac{1-\delta(x)}{\delta(x)}}{2 + \psi(x) \frac{\lambda(x)}{\delta(x)}}.\end{aligned}$$

Since occupational mobility rates are typically of the same order of magnitude as separation rates $\delta(x)$, few percentage points per month (see Figure 1,) while job-finding probabilities at monthly frequency are much smaller than 1, it must be the case that $\psi(x) > \delta(x) / [1 - \delta(x)] \approx \delta(x)$.

Aggregate productivity affects occupational mobility rates through two channels. First, as p rises, the job finding rate $\lambda(x)$ rises and the job losing rate $\delta(x)$ falls. This is both empirically accurate and the generic implication of a model with free entry, a matching function and endogenous separations. Thus, the unemployment rate declines. But $\psi(x) > \delta(x) / [1 - \delta(x)]$ implies that $\Phi(x)$ decreases in $\lambda(x)/\delta(x)$, or increases in the unemployment rate, through the increases in the mass of mismatched workers for given M . Second, the extent of mismatch M declines as p rises, because ideal jobs become easier to find. So, fewer workers switch occupation from job to job or through unemployment. Thus, if the average occupational mobility rate is observed to co-move inversely with the unemployment rate of that worker group, as we will find in the data, it must be the case that this effect stems entirely from the intensive margin, $\Phi(x)$, the conditional chance of switching given skills, because the set of skills that switches occupations (extensive margin) shrinks. In turn, examining the expression for $\Phi(x)$, it decreases in $\lambda(x)/\delta(x)$. Hence, any positive effect of cyclical conditions on occupational mobility must stem entirely from an increase in $\lambda(x)\delta(x)$. That is, the increase in the job-finding rate in an expansion must be proportionally larger than the decrease in the job-losing rate, so that the increased availability of jobs in an aggregate expansion more than makes up for the reduction in aggregate mismatch and raises overall occupational mobility. It has indeed been recently documented (Shimer (2005)) that job finding rates are substantially more volatile than separation rates into unemployment.

Workers with intermediate, non specialized skills are always willing to switch occupation; specialized-skill workers are selective in their job search, especially in a tight labor market, when their "ideal" jobs abound. As a consequence, selective workers switch less on average across the ups and downs of business cycles, but are also more cyclically sensitive. Of course, in reality there are all sorts of temporary reasons affecting occupational (non) mobility, such as spousal relocation or occupation-specific demand shocks. So observed mobility is never exactly zero for any type of worker, but decreases for selective workers when unemployment is lower. This is the equilibrium prediction that we will test empirically: observable individual

worker characteristics that predict occupational mobility, thus proxy for unobserved skills x , have less explanatory power for mobility at times of high unemployment, when all workers tend to become less discriminating independently of skill levels.

The challenge is to estimate consistently the causal effects of various observable and unobservable worker characteristics x on mobility $\Phi(x)$, in a way that can be interacted with measures of labor market tightness, such as the unemployment rate. Several papers have analyzed the effects of unemployment on mismatch in a symmetric environment where every worker is specialized in some occupation (Marimon and Zilibotti (1999), Barlevy (2002)). Formally, skills and jobs are placed symmetrically on a circle. Our model (Moscarini 2001) differentiates between weak and strong comparative advantages, thus allows for cross-sectional heterogeneity in mobility rates across workers, making this testable prediction meaningful.

3 An Empirical Model of Occupational Mobility

3.1 The Selection Problem

Consider a situation where we have T cross sections, comprising of N_t individuals at time $t = 1, 2..T$. For each individual $i = 1, 2..N_t$, in each cross section t we define a latent process of mobility

$$\text{MOB}_{i,t}^* = x'_{i,t}\varphi + \varepsilon_{i,t}$$

where $\text{MOB}_{i,t}^*$ is the latent variable capturing the individual i 's propensity to change job type between times t and $t + 1$; $x_{i,t}$ is a vector of individual explanatory variables, which may also be interacted with aggregate variables; φ is the unknown parameter vector of interest; and $\varepsilon_{i,t}$ denotes some zero mean error term. The latent measure of mobility is not observed and we conduct our empirical work with the observed measure

$$\text{MOB}_{i,t} = 1 \text{ if person } i \text{ in cross section } t \text{ changes occupation between } t \text{ and } t + 1$$

$$\text{MOB}_{i,t} = 0 \text{ otherwise.}$$

where

$$\text{MOB}_{i,t} = \mathbb{I} \{ \text{MOB}_{i,t}^* > \underline{\text{MOB}} \},$$

which says that the latent variable is above some minimum threshold $\underline{\text{MOB}}$, and $\text{MOB}_{i,t}$ is observed in the absence of any additional censoring mechanisms. Notice that the subindex t on $\text{MOB}_{i,t}^* = 1$ refers to the period *preceding* the decision to move. This empirical model draws directly from our theoretical analysis, which justifies specifying the mobility decision as a function of worker characteristics and aggregate labor market conditions.

A key issue is the treatment of joblessness. We are only interested in movements to a different occupation, because this implies the skills of the individual are transferred to *an observationally different technology*. When jobless, a worker has an “occupation” where he/she produces either job search, or home goods, or leisure (or combination thereof). These activities are not easily quantifiable, and do not contribute to our measures of GDP and labor productivity. In contrast, the occupations we focus on refer to formal employment. Thus, we exclude joblessness from our occupations. This choice entails treating the individual participation and mobility decisions separately. Another reason for excluding unemployment from our “occupations” is that we are interested in cyclical patterns of reallocation. Since unemployment is inherently countercyclical, its inclusion among our occupations would generate, in recessions, a burst of “reallocation” towards unemployment. Thus, we focus on the effects of business cycles on the career mobility of those who remain employed and the mobility variable is only observed for the subsample reporting employment in two consecutive months. Thus, we need to address the endogeneity of a worker’s employment status.

To this purpose, consider the following model

$$\text{BEMP}_{i,t} = \mathbb{I} \{ x'_{i,t} \alpha + \nu_{i,t} > 0 \}, \quad t = 1, 2..T; i = 1, 2..N_t$$

where

$$\begin{aligned} \text{BEMP}_{i,t} &= 1 \text{ if person } i \text{ in cross section } t \text{ is employed in both } t \text{ and } t + 1 \\ \text{BEMP}_{i,t} &= 0 \text{ otherwise.} \end{aligned}$$

and α is an unknown parameter vector. Next

$$\text{MOB1}_{i,t} = \text{BEMP}_{i,t} \cdot \text{MOB}_{i,t} \tag{2}$$

where $\text{MOB1}_{i,t}$ is the observed measure of mobility. To accommodate the possible endogeneity of employment to mobility, and the consequent sample selection, one would typically assume that the errors $\varepsilon_{i,t}$ and the $\nu_{i,t}$ are correlated for each individual.

Failing to account for the process by which individuals are employed in consecutive periods, when estimating the mobility equations, might introduce a sample selection bias. That is, the parameters that we estimate by examining only the sample for which $\text{BEMP}_{i,t} = 1$ are consistent for those individuals, but are generally inconsistent for the labor force comprising $\text{BEMP}_{i,t} = 0$. There are two solutions to this problem. The first, while not totally satisfying, is to acknowledge that the inferences that we draw from our empirical analysis is restricted to those comprising the $\text{BEMP}_{i,t} = 1$ population. The second approach is to employ some estimation procedure which accounts for the selection process into the $\text{BEMP}_{i,t} = 1$ sample.

To do this one could adopt some form of control function procedure whereby the inclusion of a constructed variable, typically relying on some distributional assumptions and/or exclusion restriction, is able to restore the orthogonality conditions violated by the operation of the selection process. Alternatively, one can make some assumptions about the process generating the errors in both the employment and mobility equations and exploit the assumed structure. We adopt the latter approach. We aggregate the data and assume that those within the same group, after the aggregation, have similar values for the common components of $\varepsilon_{i,t}$ and the $\nu_{i,t}$. We then eliminate this common component, and that generating the endogeneity, through appropriate data transformations.

3.2 The Identification Strategy: Birth-Cohort Synthetic Panel

Our main empirical strategy tackles the problem of sorting and selection into employment by means of a synthetic panel. For each year we combine individuals born in the same year, and compute the average value of each variable of interest across those individuals. We then construct a pseudo-panel comprising these averages for each cohort in each year.

More specifically, let c denote a birth cohort. Each year t we observe $c = 1, 2, \dots, C_t$ cohorts in a complete manner, as all $N_{c,t}$ individuals in cohort c are of working age in that year t . Let

$$\overline{\text{BEMP}}_{c,t} \equiv \frac{\sum_{i \in c} \text{BEMP}_{i,c,t}}{(\#i : i \in c)} = \mathbb{E}_{i \in c} [\text{BEMP}_{i,c,t}]$$

denote the employment rate in consecutive periods t and $t + 1$ of the entire working age sample of cohort c at that time t , and

$$\overline{\text{MOB}}_{c,t} \equiv \frac{\sum_{i \in c} \text{MOB}_{i,c,t}}{(\#i : i \in c, \text{BEMP}_{i,c,t} = 1)} = \mathbb{E}_{i \in c} [\text{MOB}_{i,c,t} | \text{BEMP}_{i,c,t} = 1]$$

denote the average mobility between times t and $t + 1$ of the members of the cohort c who are employed both at t and $t + 1$. Similarly, $\bar{x}'_{c,t}$, $\bar{x}_{c,t}^{\text{BEMP}}$, $\bar{\varepsilon}_{c,t}$, $\bar{\nu}_{c,t}$ denote the cohort-wide averages of (resp.) the characteristics of the sample, of the characteristics of the employed (in consecutive periods), and of the error terms. Finally, let a_t be a vector of economy- or labor market-wide aggregate factors that may affect the individuals' propensity to change career of each worker. This might include, for example, monthly dummies to capture seasonal effects, or unemployment as a proxy for the state of the economy.

With this notation, our empirical model is

$$\begin{aligned} \overline{\text{MOB}}_{c,t} &= a'_t \vartheta + \bar{x}_{c,t}^{\text{BEMP}'} \beta + \bar{\varepsilon}_{c,t}, \quad t = 1..T; c = 1..C \\ \overline{\text{BEMP}}_{c,t} &= a'_t \phi + \bar{x}'_{c,t} \theta + \bar{\nu}_{c,t}, \quad t = 1..T; c = 1..C \end{aligned}$$

The two errors $\bar{\varepsilon}_{c,t}$, $\bar{\nu}_{c,t}$ are allowed to be correlated across cohorts. Without loss in generality we can define each of the two random variables $\bar{\varepsilon}_{c,t}$, $\bar{\nu}_{c,t}$ to be the sum of a common component and an orthogonal component, both random variables

$$\begin{aligned}\bar{\varepsilon}_{c,t} &= \eta_{c,t} + e_{c,t} \\ \bar{\nu}_{c,t} &= \eta_{c,t} + n_{c,t}\end{aligned}$$

with $cov(e_{c,t}, n_{c,t}) = cov(\eta_{c,t}, n_{c,t}) = cov(e_{c,t}, \eta_{c,t}) = 0$ and $var(\eta_{c,t}) = cov(\bar{\varepsilon}_{c,t}, \bar{\nu}_{c,t})$. Our identification assumption is that the correlation embedded in $\eta_{c,t}$ is time-invariant. That is, it has to do exclusively with birth-cohort membership, while the time-varying components of cohort-specific errors in employment and mobility are uncorrelated. Formally

Assumption 1 (Cohort-Based Identification) *The birth-cohort specific unobserved characteristics that affect both employment and career mobility outcomes are time-invariant.*

$$\begin{aligned}\bar{\varepsilon}_{c,t} &= \bar{\eta}_c + \bar{e}_t \\ \bar{\nu}_{c,t} &= \bar{\eta}_c + \bar{n}_t.\end{aligned}$$

Since cohort effects are assumed to cause the endogeneity of $\overline{\text{BEMP}}_{c,t}$ and $\bar{x}_{c,t}$, we estimate the model

$$\overline{\text{MOB}}_{c,t} = a_t' \vartheta + \bar{x}_{c,t}^{\text{BEMP}'} \beta + CD_c' \omega + \bar{e}_t \quad (3)$$

by including cohort dummies CD_c as additional regressors to account for, and estimate, the fixed effects $\bar{\eta}_c$. By controlling for the fixed effects we are able to consistently estimate β under Assumption 1.

This estimation approach is a fixed effects procedure along the lines discussed by Deaton (1985) and the procedure we adopt is similar to fixed effects estimation of the sample selection model at the individual level. The conditions under which the model is consistent at the individual level are discussed in Verbeek and Nijman (1992) and the assumptions that we employ here are similar but at the cohort level. An advantage of this approach is that any regressor which is endogenous, due to the presence of the cohort effects, is made exogenous via the inclusion of the cohort dummies.

In addition to assuming that the source of the endogeneity is birth cohort specific and time invariant, we also require, for identification of the parameters, that each of the explanatory variables displays some linearly independent relationship with the birth cohort variable. This means that the explanatory variables must vary with the birth cohort in a way which is not fully predictable by the movement in the other variables. Figure 3 appears to provide empirical support to this assumption. Historically, the proportion of College graduates rises

over time, and across birth cohorts, presumably for aggregate growth reasons unrelated to the average individual characteristics of the members of each cohort. Similarly, the proportion of men who are married and/or heads of their households constantly declines across birth cohorts. These trends do not appear to be linearly synchronized.

A different endogeneity problem arises when trying to quantify the trade-off between individual and macroeconomic incentives to pursue different careers. To this purpose, we introduce among our controls the unemployment rate and its interactions with the cohort characteristics. For example, if highly educated people switch careers less than average, their relative (to other groups) mobility should be countercyclical, although overall mobility is procyclical. Ideally, we would like to employ group-specific unemployment rates, as workers of different education, marital and family status, and age typically face very different labor market conditions. So the unemployment rate would go in the $\bar{x}_{c,t}$ vector of cohort characteristics. These group-specific unemployment rates $u_{x,t}$ are likely to be endogenous to career mobility: a shock to the propensity to change career is sure to generate some unemployment from reallocation. Therefore, we instrument the group-specific unemployment rates with the civilian, economy-wide male unemployment rate from the BLS. Specifically, we use the level of the male civilian unemployment rate u_t and its interaction with cohort characteristics as instruments for the level of the cohort unemployment rates and its interactions. So u_t and $x_{c,t} \cdot u_t$ are instruments for each $u_{x,t}$ and $x_{c,t} \cdot u_{x,t}$, $x \in \bar{x}_{c,t}$. Our maintained identification assumption, that we consider fairly plausible, is that any residual reverse causality from each single group's mobility to the US unemployment rate at large is absorbed by that group's cohort dummy.

To illustrate the economic meaning of our assumptions, consider the following example. Suppose that individuals differ by their unobserved level of risk aversion. Suppose further that risk aversion determines three types of individual choices: whether to work or not (formally, through $v_{i,t}$), whether to change career or not (through $\varepsilon_{i,t}$), and whether to acquire a College degree or not (through the observed level of education in $x_{i,t}$). It is plausible, in particular, that more risk averse individuals are more likely to work, less likely to change career and to attain higher educational levels. Fuchs-Schündeln and Schündeln (2005) provide strong evidence of unobserved worker characteristics that affect both their career choices and their precautionary saving behavior. This endogeneity creates an obvious bias in, say, the estimated effect of education on career mobility. We assume that the average risk aversion of the individuals in each cohort c is invariant over time, and we absorb it into the $\bar{\eta}_c$ error, that can be dealt with by standard panel methods. In other words, the correlation between employment and mobility due to unobservable individual characteristics, such as risk or time preference, should be a much lesser concern across

birth cohorts than across individual workers. Averaging across members of the same birth cohort should eliminate most of the unobserved individual heterogeneity (see, for example, Attanasio and Davis (1996),) and any residual effect differentiating cohorts should then be captured by the fixed effect $\bar{\eta}_c$. We stress that both the sample selection into employment and the endogeneity of some individual characteristics, such as education and marital status, to mobility are problems that affect any microeconomic study of worker turnover (see, for example, Farber (1994).) In our approach, the residual difference in educational levels across cohorts must be uniquely due to the fact that they were born at different times. That is, younger cohorts go to school longer because of such growth phenomena as income effects in education, exogenous to the cohort itself, and not because they like going to school better than their parents. The same principle applies to the choice to get married and live with the spouse, and to head the household.

3.3 Dynamics

Another advantage of the pseudo-panel approach is that it allows for the estimation of dynamic effects operating through the dependent variable. The job-matching theory of worker turnover originating with Jovanovic (1979) emphasizes the accumulation of work experience and learning specific to a job, which result in mobility declining with tenure. The same mechanism applies to occupations, as corroborated by the evidence in McCall (1990). An exogenous innovation in mobility above the predicted declining tenure/experience profile dissipates matching human capital, and leads workers to shop for new jobs for several subsequent periods. Hence, we would expect innovations to mobility to persist.

The model we estimate

$$\overline{\text{MOB}}_{c,t} = \rho \overline{\text{MOB}}_{c,t-1} + a'_t \vartheta + \bar{x}_{c,t}^{\text{BEMP}'} \beta + CD'_c \omega + \bar{e}_t \quad (4)$$

is based on the approach of Verbeek and Vella (2005), who augment the static model (3) with the lagged value for the cohort. They discuss the conditions for identification and consistency and they do not differ greatly from the static model. However, it is necessary that the lagged variable displays variation with cohorts which cannot be exactly replicated by the variation in the cohort averages in the explanatory variables.

4 Data and Sample Disposition

Our data set includes 303 monthly cross-sections, from 1979:01 to 2004:03, of the US population contained in the monthly files of the Current Population Survey. We consider this type of data set to be the most appropriate for our investigations, for three reasons. First,

as our focus is macroeconomic we require a representative sample collected in a consistent manner over a long period. The CPS is designed for this purpose and is the source for the official aggregate labor market statistics. Second, we attain identification of the employment decisions through the construction of a pseudo-panel by birth cohort. This would not be feasible with other longitudinal surveys of workers, because it requires a very large sample of individuals of the same age every month. Finally, the high (monthly) frequency of the observations minimizes time aggregation (a worker may change occupation more than once between interviews).

The monthly CPS is a rotating panel and we exploit this feature to construct our measure of occupational mobility. We use cleaned measures of mobility from Moscarini and Thomsson (2008). We refer to their paper for a discussion of issues relating to occupational classification, imputation, geographical attrition in the monthly CPS. The idea behind their cleaning algorithm is to examine a worker’s career trajectory over his first four consecutive months in sample, and to judge the plausibility of any occupational transition between months 2 and 3 in light of information on the months before and after. In addition, they exploit the increased reliability of CPS occupational measurement after 1994, when Dependent Coding techniques were introduced to minimize spurious transitions, as well as additional features of the job that are likely to be correlated with a genuine occupational change, such as changes in class of worker (private, government, or self-employed) and in the three-digit industry code, and active job search between interviews in months 2 and 3.⁶

Our sample comprises male civilian non-institutionalized adults of working age (16 to 64, included) who are not in school or at home full time. We plan to consider female workers in future research, but we exclude them for now because of the sharp transitional dynamics of female participation over the 1979-2004 period under consideration.

We consider an individual i to be employed both this month t and the following month $t + 1$ (and set $\text{BEMP}_{i,t} = 1$) if he reports to be either a salaried or a self-employed worker who worked either full time full year or full time at least part of year. All of these workers have a Census three-digit occupation in both months, and Moscarini and Thomsson (2008) show how to purge transitions from imputation and measurement error. Among the employed, we consider individual i a job mover if he reports at time t a different occupation from next month: $\text{MOB}_{i,t} = \mathbb{I}\{\text{OCC}_{i,t} \neq \text{OCC}_{i,t+1}\}$. Although the persistence of half the sample across months introduces correlation in errors, due to individual fixed effects, our birth-cohort aggregation takes care of this problem.

⁶After 1994, Dependent Coding also allows to obtain an accurate measure of job-to-job transitions (see Moscarini and Thomsson (2008)). Interestingly, over 1/3 of all occupational transitions after 1994 occur without change of employer, most likely promotions or demotions.

There exist an average of 453 occupational categories at the three-digit level, each containing an average 0.22% of employment. The largest category, “Sales Supervisors,” comprises on average about 8% of employment. Some categories have empty cells in some months. “Mathematical scientists” is a typical occupation that always comprises some individuals in the sample every year, but averages less than one out of ten thousand workers.

Another important issue concerns measurement error in employment status. We do not perform an Abowd-Zellner (1985)-type correction, because we consider only workers who report being employed and a valid occupation for *two* consecutive months, who are very unlikely to be unemployed workers misclassified as employed.

Measurement of education in the CPS is also problematic. In 1979-1991 the CPS files contain the years of education of the individual, with an auxiliary dummy variable indicating whether the highest grade attended was completed. Starting in 1992, the measurement of educational levels becomes coarser. Frazis and Stewart (1999) discuss how to partially amend the transition. Based on their results, and on a background check of our own, the only reliable measure of education that we can consider consistent through the two subperiods (hence through 1979-2004) is a pair of dummies, one indicating whether the individual achieved a High School degree or got some College, the other whether he/she achieved a College degree (BA or equivalent) or even had some graduate studies. Our check consists of measuring the fractions of the active population who fall into each educational category and tracking them over time. Any finer classification than the one we adopt (for example separating High School graduates from those who also had some College) leads to a jump of these fractions between 1991 and 1992, suggesting an inconsistency between classifications. The loss of information caused by aggregating education at these three levels should not be too severe if these educational attainment effects can be captured by few dummies. Clearly this will not be true if there is large variation within categories.

We consider yearly birth cohorts of men born between 1921 (who were 58 years old and about to retire at the beginning of the sample in 1979) and 1985 (who were 19 years old in the last year of the sample 2004), who are employed both in that calendar month and the following one, so can generate an occupational transition. There are about a million of them in the data set, on average about 4,000 per month, of which 81 in each birth cohort. To obtain each observation, each month we average across all those individuals born in the same year. Given the unbalanced nature of our pseudo panel we have a total of 13,565 observations, still a very sizable panel despite the severe aggregation by birth cohort necessary to our identification strategy.

Figures 2 and 3 summarize the characteristics of our sample for each calendar month in

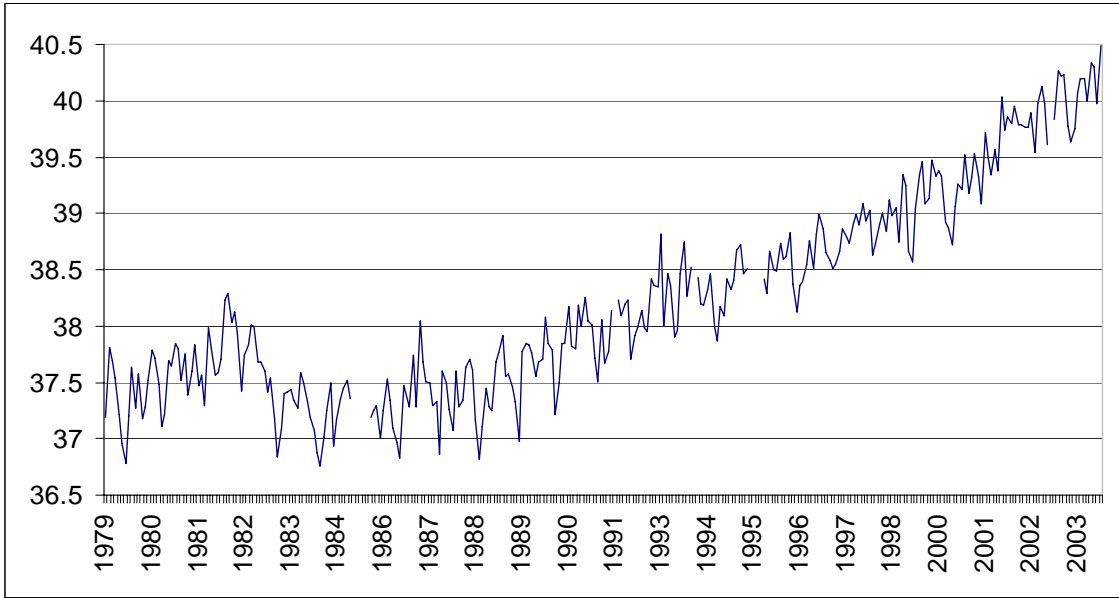


Figure 2: Average age of the sample

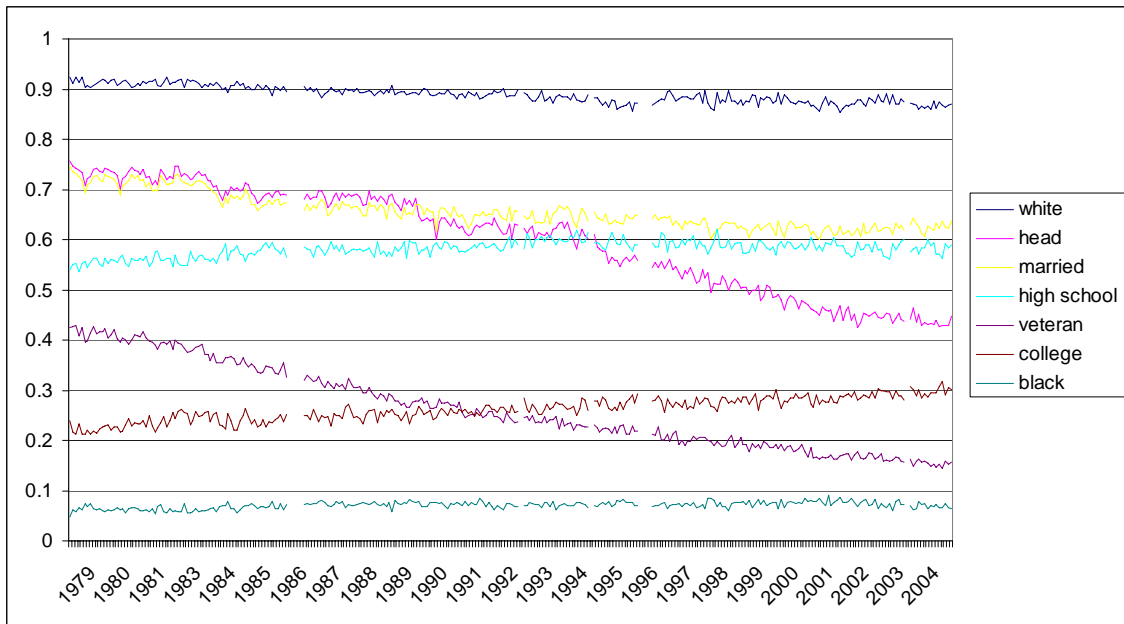


Figure 3: Other sample characteristics

the period under consideration.⁷ These plots reveal some interesting trends. Average age declined through the late 1970’s and then climbed back, as the aging baby-boomers claimed an increasing share of the labor market. The proportion of whites and African-Americans in total employment very slightly declined in favor of Hispanics and other ethnic groups, and the proportion of men in the sample who were married decreased significantly. The increasing educational levels of the US population are witnessed by the rise in the proportion of High-School graduates, which ended in the mid-1990’s, and by the ongoing increase in the proportion of College graduates and post-graduates.

5 Regression Specifications and Results

To estimate the synthetic cohort model (3) and explain the variation in career mobility, we employ a 4th-order polynomial in age, and four dummies: married with spouse present, head of household, High School graduate or some College, College (BA) graduate with or without post-graduate studies. As mentioned, we include the cohort-specific unemployment rate and its interaction with cohort characteristics, both instrumented with the corresponding terms where the group-specific unemployment rate is replaced by the civilian unemployment rate. The interactions are meant to capture the effects of the job-finding rate on ex ante sorting, as illustrated earlier and elaborated upon later in this section. We also experimented with White ethnicity, African-American ethnicity, and War Veteran status. We do not present the results because we do not have any prior about their theoretical and independent relevance for career mobility, and because race composition of employment exhibits minor variations over the period.

Three unfortunate lacunae of the CPS files for our purposes are measures of tenure on the current job, work experience, and wage/earnings. Tenure is surveyed only every few years in a Tenure Supplement. We do not proxy experience by age minus education, since age and the educational dummies are among the explanatory variables. We choose to focus on flexible age effects and interpret experience as being captured by age. This approach seems less problematic for males than it would be for females. Usable wage/earnings information is available only in the March Income Supplement.⁸

Table 1 presents the regression results, estimates and (in smaller font, beneath) t-ratios. Estimates that are statistically significant at the 1% level are in boldface. All specifications

⁷In a previous draft, based on annual (March) CPS files, we verified that these averages are identical in trends but different in levels for the labor force, whence including the unemployed. This is clearly suggestive of some strong selection by employment status, that we address with our cohort-based strategy.

⁸The monthly files only contain wage information for the outgoing rotation groups (month in sample 4 and 8,) while the approach taken by Moscarini and Thomsson (2008) to obtain clean measures of career mobility is based exclusively on individuals in months 2 and 3 of their rotation.

Dependent variable: average occupational mobility of birth cohort c in calendar month t ($mob_{c,t}$) in /100						
	OLS	OLS	IV	OLS	IV	IV
Constant	23.73182 8.42438	30.26450 9.67223	32.27710 10.71234	29.06575 9.25220	30.90179 10.19941	56.73560 15.73176
Lagged mobility ($mob_{c,t-1}$)				0.03382 4.01799	0.03347 3.97425	0.03242 3.73787
Cohort Unempl. Rate ($u_{c,t}$)		-0.05414 -4.78038	-0.07081 -4.62610	-0.05271 -4.65463	-0.06784 -4.43010	-0.16709 -2.54490
Age of the cohort ($age_{c,t}$)	-1.91353 -5.98409	-2.46553 -7.25706	-2.63560 -8.09817	-2.36060 -6.93153	-2.51578 -7.70383	-5.07949 -17.46119
Age ²	0.06810 5.87405	0.08599 7.06346	0.09150 7.80521	0.08235 6.74989	0.08739 7.43139	0.18285 18.58899
Age ³	-0.00108 -5.85726	-0.00134 -6.96723	-0.00142 -7.61973	-0.00128 -6.66383	-0.00135 -7.26344	-0.00288 -19.73583
Age ⁴	6.33E-06 5.83258	7.7E-06 6.86920	8.1E-06 7.44825	7.4E-06 6.57613	7.7E-06 7.10838	16.5E-06 20.39509
Head	0.00942 2.58580	0.01000 2.74371	0.01017 2.79179	0.00928 2.54383	0.00944 2.58933	0.01311 1.38454
Head x $u_{Head,c,t}$						-0.00347 -1.11989
Head total effect						-0.00051 -----
Married	-0.02039 -5.46656	-0.01922 -5.14539	-0.01886 -5.04873	-0.01854 -4.95947	-0.01822 -4.87436	-0.02505 -1.80881
Married x $u_{Married,c,t}$						0.00432 1.12542
Married total effect						-0.00647 -----
High School	0.02017 7.85288	0.02040 7.94575	0.02047 7.97414	0.01953 7.58415	0.01960 7.61324	0.03665 3.36430
High School x $u_{High Sch,c,t}$						-0.00140 -1.42770
High School total effect						0.02828 -----
College	0.00678 1.48440	0.00705 1.54415	0.00713 1.56323	0.00658 1.44149	0.00666 1.45990	-0.02501 -4.54349
College x $u_{College,c,t}$						0.01596 4.19277
College total effect						0.01398 -----
Number of observations	13,565	13,565	13,565	13,565	13,565	13,565
Adjusted R ²	0.236	0.237	--	0.238	--	--

In all specifications:

- numbers under estimated coefficients are correct t statistics; estimates in boldface are significant at 1% level
- birth cohort and month of the year dummies included

In specifications estimated by IV:

- unemployment rate of birth cohort c at time t ($u_{c,t}$) instrumented with US civilian unemployment rate at t
- group i total effect = i coefficient + i specific average unemployment rate (u_i) * interaction coefficient ($i \times u_i$)

Table 1. Regression Results.

include month and cohort dummies. “OLS” indicates specifications where the cohort/group unemployment rate and its interaction with cohort characteristics are either absent or introduced as covariates. “IV” refers to the specifications where they are introduced but instrumented with the civilian unemployment rate. Note that the estimates account for the different cell sizes via a weighted least squares procedure. Moreover, the standard errors in the IV specifications are appropriately corrected for these different cell sizes.

Our empirical strategy is the following. Initially we explore the relationship between worker characteristics and the probability of an occupational transition. This is reported in the first column of Table 1. Then we introduce the cohort’s unemployment rate, we instrument it with the civilian unemployment rate, then we add the lagged dependent variable, with and without unemployment instruments, and finally we interact the group-specific unemployment rates with the cohort characteristics in the IV approach. This final column represents our chosen specification and we present the other estimates so that the reader can gauge the degree of sensitivity across specifications. The discussion that follows is primarily based on the estimates from the last column. Notice that due to the interaction of the unemployment rates with group characteristics it is necessary to assess the effects of both the direct group effect and that operating through the interaction terms. The “total effect” of characteristic x is obtained by summing the estimate of the direct effect φ_x and the product between the interaction coefficient and the sample mean over time of that x group’s unemployment rate $u_{x,t}$. This, however, has no implications for the lagged mobility and age effects.

Worker Characteristics. As expected, age has a negative and diminishing impact on career mobility. This effect is quite strong and robust across all specifications. For the 1960 cohort, at age 16 the probability of a change in occupation is 28 percent although this decreases at the sample mean to approximately 8 percent in 15 years. The age effect then slowly decreases to approximately 4 percent at which point it appears to level out. We interpret this negative effect as “occupational matching” stemming from work experience. By trying different occupations, the worker learns which occupations are most suitable to his talents. The age effects may also capture the effects of accumulated occupation specific human capital acquired while on the job. This effect is consistent with the evidence presented by McCall (1990) and Neal (1998). The former shows that the retention rate of a worker on a new job is significantly higher if the occupation is the same as in the previous job. The latter documents that young workers follow a two-stage search strategy, first shopping for a career, and then for an employer within the chosen career. As a consequence of both mechanisms, career mobility should decrease with age.

Heading the household causes a 1% increase in the man’s career mobility, a quite large effect. Roughly double in absolute value is the negative effect of being married with spouse present. We interpret these findings as saying that male heads need to work to support their families, thus are more prone to change career to remain employed, while married men with spouse present are less likely to switch, because of the mobility cost associated to the spousal job. Notice, however, that both effects are significantly weakened, and lose statistical significance, when interacted with the unemployment rates of heads and married men, appropriately instrumented.

Very strong support in favor of cyclical worker sorting is the effect of education on mobility. This support emerges from the comparison between high school and college graduates, who make up most of the sample. In our preferred specification, shown in the last column, the estimates indicate college graduates change careers slightly more often than high school dropouts, but significantly less than high school graduates. The latter appear to be less committed to a specific occupation and appear more willing to switch occupations irrespective of cyclical factors. In contrast, college graduates stay where they are in tight labor markets, and move around much more when many of them are unemployed. Notice that the interaction term for college graduates not only has a strong and very significant effect, but also changes the baseline estimate of the college effect.

In terms of Moscarini (2001)’s sorting theory, college graduates are “specialized” workers, as they respond more to changes in their own jobless rate. Recall that the exogenous effect of education on career mobility is identified by comparing birth cohorts with different educational levels. This finding, which is one of our main contributions, stands in contrast to the oft-cited but hardly corroborated claim that college education provides mostly general human capital. While college education certainly does provide general skills, it appears that it induces its recipients to change career relatively less when jobs are plentiful and when overall occupational mobility is higher.

Finally, we note that these estimates only capture the effects of the quantity of education. Changes in the quality of education over the decades are cohort-specific and should then be reflected in the estimated cohort effects.

Unemployment. We envision various sources of macroeconomic shocks that affect total occupational mobility: aggregate shocks to labor demand, such as shocks to TFP, preferences for leisure, or monetary policy. While measuring macroeconomic shocks is notoriously difficult, we choose to proxy them with the unemployment rate of the group-cohort, appropriately instrumented by the civilian unemployment rate measured as the yearly average of the monthly male unemployment rates published by the Bureau of Labor Statistics. In

our view, it is labor market tightness, not GDP growth per se, that drives workers' career choices. Since vacancies and unemployment are almost perfectly inversely correlated at cyclical frequencies (see Shimer 2005), but lag the cycle, the unemployment rate appears to be an appropriate proxy for labor market tightness. We cannot identify business cycle effects through time dummies, due to the presence of the age and birth cohort effects. We assume there are no direct time effects beyond those captured by the macroeconomic variables.

A cohort's unemployment rate, instrumented by the economy-wide rate, has a negative association with mobility. The direct effect is quantitatively strong, especially when we include the interaction terms involving education, and always statistically significant at the 1 percent level. This substantive result is also consistent with previous raw correlations found in sectorial mobility by Murphy and Topel (1987) and by Jovanovic and Moffitt (1990), although these authors only condition on worker age and do not address the endogeneity of unemployment to mobility. The existing literature has focused more on employment reallocation over business cycles across industries, rather than across occupations. The stylized fact is the "Cyclical Upgrading of Labor": workers move to high-wage, cyclical industries in expansions and vice versa (see McLaughlin and Bils (2001).) However, this phenomenon predicts a correlation between unemployment and *net* flows. Even assuming that a similar phenomenon exists for occupations, this still fails to account for the negative association of unemployment with the size of the *gross* flows that we find.

Dynamics. A main advantage of the pseudo-panel is the possibility of estimating consistently a dynamic effect in the dependent variable. In our case of occupational mobility, the economic reason to expect such a residual persistence is the presence of an occupational-matching component in productivity. Suppose that an occupation-specific shock displaces many workers from their career, and forces them to search for a new one. Then as the re-learning process takes time, we would expect those of them who are less lucky to keep changing occupation even a year later, independently of any other observable event or individual characteristics.

Our estimates of the lagged effects are precise and stable across specifications. Given that the model is a linear regression the interpretation is straightforward. That is, suppose that in going from month t to $t + 1$ we observe 100 individuals change occupations. The estimate implies that in going from $t + 1$ to $t + 2$, more than three of these individuals will change occupation again in the next month. Thus, even in a state where the other explanatory variables are combining in a manner to produce no additional reallocation we can see that there remains a significant degree of mobility. Note that these are job changers and not individuals who are transiting to jobs from the unemployment pool, so workers who changed

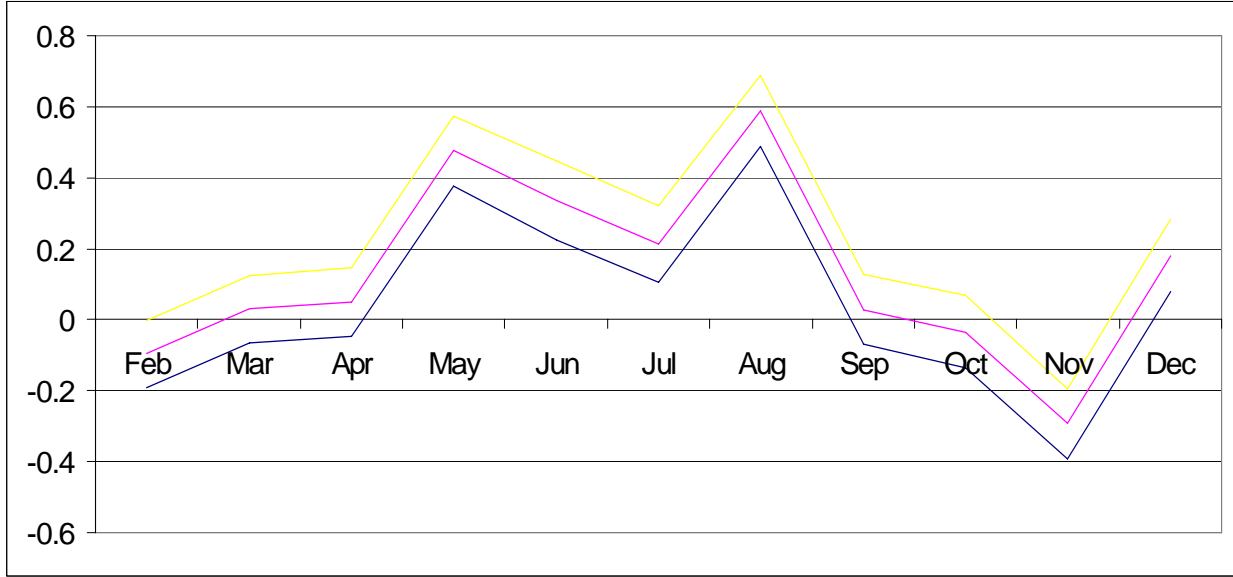


Figure 5: Monthly dummy estimates and 99% confidence intervals.

occupation in one period will continue to do so in subsequent periods. These estimates point to an important effect of propagation of any shock to the economy which affects occupational mobility, with all of the obvious implications for aggregate productivity and welfare. This appears to be the first evidence which substantiates the presence of this dynamic effect, in addition to exploring its magnitude.

Seasonal Effects. We now comment on the estimates of the monthly dummies, noting that January is the excluded month, reported with 1% confidence bands in Figure 5. As expected, there are spikes in mobility in May (between May and June), when schools close, and in August (between August and September), when they re-open. After that, the Fall is a season of below-average career mobility.

Birth Cohort Effects. We now comment on the pattern of the estimated cohort dummy coefficients, excluding 1921. Given the large number of estimates we report them by plotting them in Figure 6 for the last specification (column) in Table 1, as a time series with their 1% confidence bands.

The results are striking and several of their features merit comment. First, the range in the cohort effects is large suggesting that a lot of the variation in mobility rates across cohorts is purely due to factors which vary by cohort and which are not included in the mobility

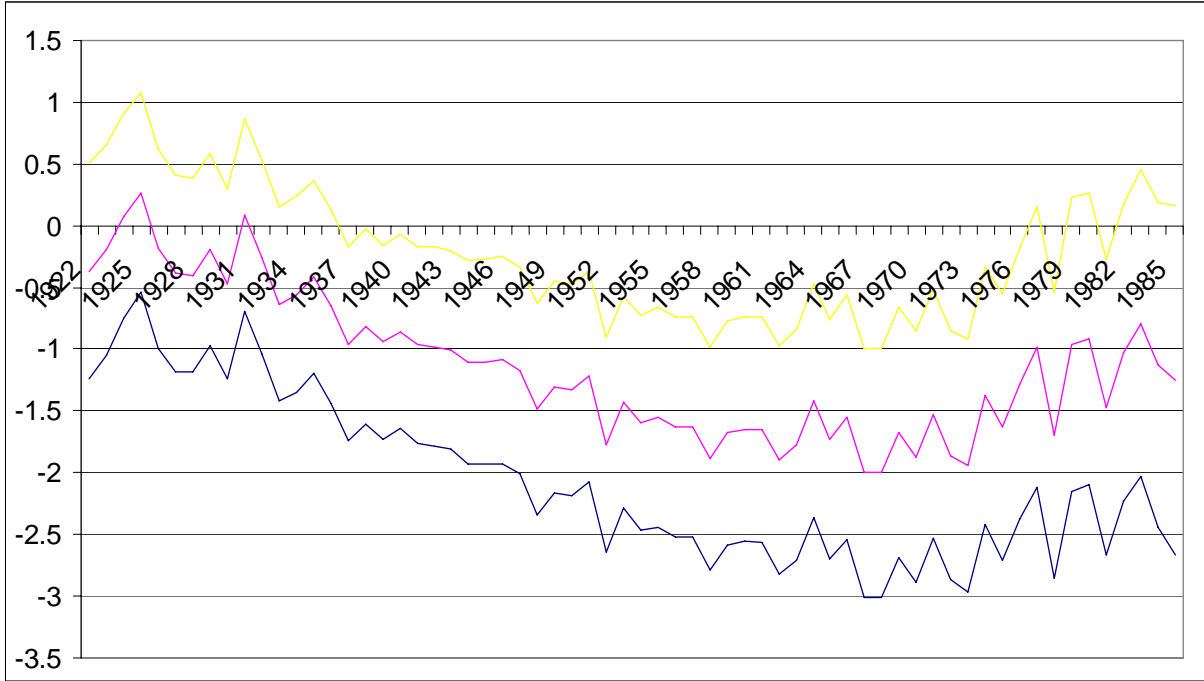


Figure 6: Birth cohort dummy estimates and 99% confidence intervals

equation. Second, in our preferred, latter specifications the estimates of the cohort effects are typically and significantly declining over time, suggesting that later cohorts (starting from those born in the mid-1930's) have an unexplained and statistically significantly lower propensity to change occupation. This effect begins to vanish and reverse course with the cohorts born in the 1960s, and is almost entirely undone by an increase in unexplained mobility by workers born in the late 1970s and 1980s, who appear to be as flexible and mobile as their grandparents born in the 1920s and 1930s. These effects cannot be due to age differences, because we do control for ageing in a quite flexible manner, and because age effects are typically monotonic. It is also unlikely that they are due to composition effects of different cohorts, because we do control for several of these characteristics that we would expect to affect career mobility.

At this stage, we can only speculate about the interpretation of this generational pattern. Younger generations seem to be more flexible than their parents, but no more than their grandparents. We can only speculate that this is due to the increasing specialization required of workers after WWII, with the latest cohorts reacting to increasing turbulence in the US economy by attaching themselves less tightly to a specific career (Kambourov and Manovskii 2007.)

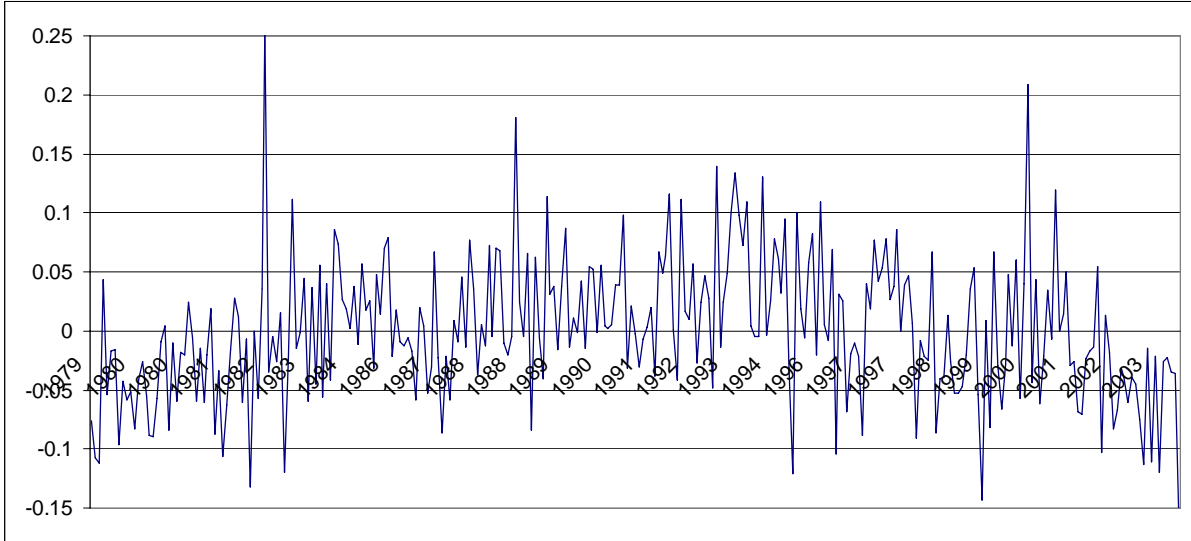


Figure 7: Regression residuals, last column of Table 1

Discussion. In the light of our findings concerning the long-run changes in the composition of employment in terms of various characteristics (Figures 2 and 3), and their causal effects on career mobility (Table 1), we now attempt to explain the trend and cycle in total employment reallocation across occupations (Figure 1).

Figure 7 illustrates the time series of the regression residuals from the last specification in Table 1. While the residuals look “white,” some of the late decline in mobility after the 2001 recession remains unexplained. Other than that, the pronounced movements at cyclical frequencies appear to be well captured by the unemployment rate, including its indirect effect on the propensity to change career of differently educated workers. Some effects stem also from the cyclical composition of employment.

The average age of the US population reached a minimum in the mid-1970’s, corresponding to the peak of the baby boom, to then steadily increase subsequently. Therefore, younger cohorts are relatively less important in size, and contribute less to raise overall mobility. This may explain the mild negative trend in occupational mobility documented in Figure 1 post 1980s.

The estimated interaction terms tend to be always of the opposite sign of the direct impact estimate. This suggests that individual characteristics that are useful to predict career mobility become less useful for doing so in times of high unemployment. To verify this conjecture, in Figure 8 we plot the 9-month rolling standard deviation of regression residuals. There are three clear spikes corresponding to the three recessions, in 1981-92,

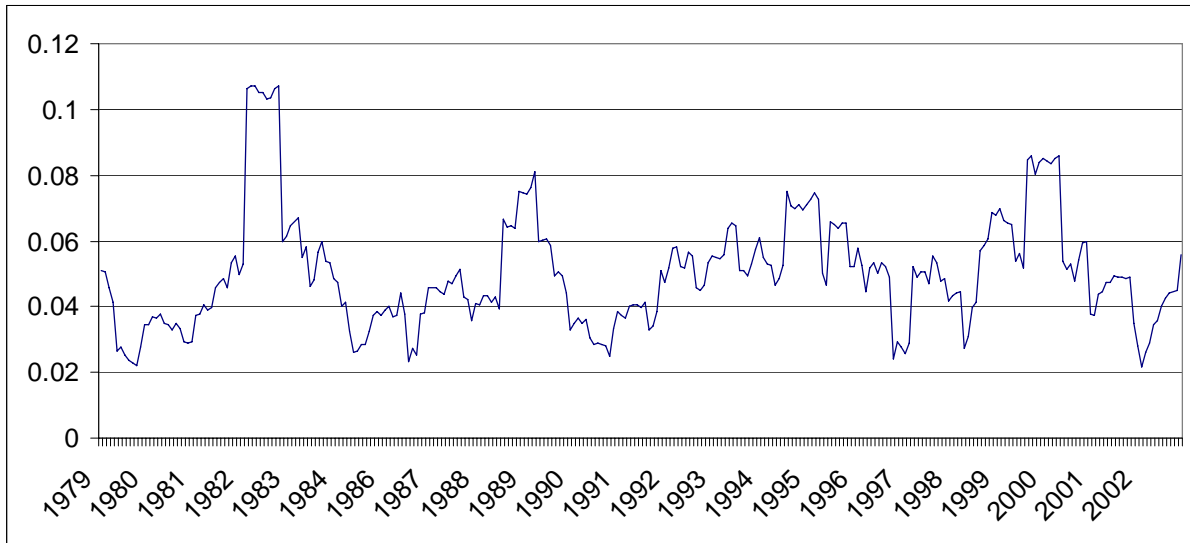


Figure 8: Rolling standard deviation of regression residuals, 9-month window

1990 and 2001. Since the series is constructed by a two-sided 9-month window, including four months forward, it tends to lead the cycle. This is the strongest evidence in favor of our prediction. At times of high unemployment, workers scramble to find any kind of job, and it becomes harder to predict how frequently they will change career just based on their characteristics. Interestingly, we also observe an increased “turbulence” in the mid-1990s, when the US economy experienced a temporary slowdown after the Mexican crisis of 1994, although not technically a recession.

Regarding our findings on the cohort effects, we propose two interpretations. One is in terms of the quality of human capital. Card and Lemieux (2001) find a break in the returns to education for cohorts born since the mid 1950’s. Their interpretation is that the slowdown in the growth of educational attainments generated a skill shortage relative to a “balanced growth” allocation and raised the College premium for these young workers. This may be explained by their rising career mobility. Gosling, Machin and Meghir (2000) also provide a cohort-based interpretation of the rise in men’s wage inequality in the United Kingdom since the late 1970’s, when the cohorts born in the mid 1950’s started to appear on the labor market. They argue that educated workers in these later cohorts received a different quality of human capital in school and College.

The second interpretation that we offer is that the “corporate culture” in the US has changed across generations, shifting emphasis away from lifetime loyalty to the same employer and towards “loyalty to an occupation,” independently of the employer. Using the

same data set and roughly the same period, Stewart (2002) documents a rise in job-to-job transitions (namely those with less than two weeks of intervening unemployment), lending some support to this second hypothesis.

6 Conclusions

In this paper, we formulate and test the hypothesis that workers are more prone to mismatch in a depressed labor market. In a large, representative, long and high-frequency data set, we study career mobility, that we let depend on worker individual characteristics, both observed and unobserved. The latter are assumed to depend only on the birth year of the worker, to capture such cohort effects as cultural background, quality of education, and parental experience. We show that occupational mobility declines with age, family commitments, and education. These effects, though, tend to be reversed when the worker's peers experience high unemployment. In addition, the predictive power of individual and aggregate effects declines when unemployment is high in the US economy. We conclude that worker sorting across occupations, likely the most important type of employment reallocation, occurs more randomly in a loose labor market.

We see this paper as the first of several steps to further inquire into the nature of cyclical mismatch, as well as into the meaning of the strong cohort effects in career mobility that we find. The next steps will exploit information about earnings and hourly wages, which is available in the same data set that we use, the monthly CPS, albeit only for a fraction of the sample, the outgoing rotation groups. Whatever the conclusions that we can draw from wage information, we have established strong evidence that different workers tend to behave more similarly in a depressed labor markets. In tight labor markets, conversely, individual heterogeneity emerges as the primary and strong driving force of career mobility decisions. Coupled with the well known and studied effects of reallocation on unemployment (Lilien (1982)), the reverse effect of unemployment on worker mobility that we uncover suggests a complex dynamic inter-relationship between aggregate economic activity and the pace of factor reallocation. Future research must take this two-way relationship into consideration when studying the determinants of productivity growth.

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