AGGREGATE WORKER REALLOCATION
AND OCCUPATIONAL MOBILITY
IN THE UNITED STATES: 1976-2000*

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Abstract
We investigate the evolution and the sources of aggregate employment reallocation in the United States in the 1976-2000 March files of the Current Population Survey. We focus on the annual flows of male workers across occupations at the Census 3-digit level, the finest disaggregation at which a moving worker changes career and relocates to an observationally different technology. The total reallocation of employment across occupations is strongly procyclical and mildly declining until the early 1990s, and then relatively flat. The negative trend is entirely due to younger workers, as it is reversed for men over age 40. To reveal the sources of these patterns, while correcting for possible worker selection into employment, we construct a synthetic panel based on birth cohorts, and estimate a model of occupational mobility. We find that the cross-occupation dispersion in labor demand, as measured by an index of net employment reallocation, has a strong association with total reallocation. The demographic composition of employment, more specifically the increasing average age and College attainment level, explains some of the trend and cycles in worker flows. High unemployment reduces both the level of mobility directly and the importance of the education effect: differently educated workers adjust differently their mobility decision to cyclical conditions. As predicted by job-matching theory, occupational mobility has residual persistence, so shocks to aggregate employment reallocation propagate through time. Finally, cohorts born after the mid-1950’s have increasingly low occupational mobility beyond what can be explained by their observable characteristics.

*The first version of this paper (September 2002) addressed the 1971-2000 period and was titled accordingly. This new draft focuses on the shorter 1976-2000 period, to exclude the effects of the 1976 change in the imputation method for missing records in the Current Population Survey. The results have not changed much. We thank seminar participants at the FED Board of Governors, Yale University and the “Malinvaud Seminar” at INSEE-CREST for very useful comments. Moscarini thanks Yale University and the Alfred P. Sloan Foundation for financial support, and the hospitality of the European University Institute.
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1. Introduction

A prominent tradition in macroeconomics, initiated by 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 plant-level and matched employer-employee longitudinal datasets 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) and persists through time at the firm level (Haltiwanger, Lane and Spletzer 2000). The reallocation of existing inputs across plants explains about half of total productivity growth in US manufacturing (see Haltiwanger 2000 for a survey).

Existing evidence on labor market-wide turnover documents the magnitude and time series patterns of job turnover (e.g. Davis, Haltiwanger and Schuh 1996), worker turnover across sectors (Murphy and Topel 1987a, Bils and McLaughlin 1992) and employment states (e.g. Blanchard and Diamond 1990). This paper adds further evidence by studying the reallocation of male workers among Census three-digit occupations, using micro-data representative of the US population, the March Files (Annual Demographics plus Income Supplement) of the Current Population Survey (CPS) over the 1976-2000 period.

Our focus on occupations, at this level of disaggregation, is motivated by the following two reasons. First, mobility at this level 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. From
this point of view, macroeconomists interested in the creative destruction process should be concerned with occupational mobility.

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”.¹

Our choice of 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 base comprising the same variables measured in a consistent manner through time. As explained in detail later, these considerations lead us to the 1976-2000 period.

Our first objective is to document the behavior in the time domain of the Gross reallocation of employment across three-digit occupations. This is the proportion of workers employed in two consecutive years who change, at least once, occupation in between. This represents a measure of average worker mobility. We also report the behavior of Net reallocation, namely one half of the sum of the absolute changes in occupational employment shares. This measures the reshuffling required to accommodate changes in the distribution of employment across occupations, ignoring offsetting moves that cancel out in the aggregate.²

¹Finer 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.

²This measure is used by Murphy and Topel (1987a) and Jovanovic and Moffitt (1990) as an alternative index to the dispersion of employment growth rates proposed by Lilien (1982). The two indices behave very similarly.
We find that Gross reallocation of employed workers across occupations has been declining mildly for men and more visibly for women. Among men, the decline is disproportionately attributed to younger workers, as men over age 40 actually witnessed a slight increase in career mobility. Gross reallocation has also been strongly procyclical, with the exception of the 1990’s, when it did not respond to the long expansion. In contrast, Net reallocation has been relatively flat.

On the basis of these remarkable time series patterns that we uncover, our second objective is to identify the determinants of Gross reallocation. We group the potential sources of these worker flows into three main categories. First, on the labor demand side, we consider the effects of Net reallocation of employment across occupations on Gross reallocation. Second, on the labor supply side, we attempt to identify the effects of various observable individual characteristics on the mobility decision. This allows us to examine if the changing composition of the labor force is contributing to the changes in 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. Finally, we examine the presence of dynamic effects. Job-matching theory (see, for example, Jovanovic 1979) implies that “separation begets separation”. For example, job separations, due to a recessionary economy, 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. 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.

To evaluate the respective roles of these factors we specify a statistical model of occupational mobility at the individual level. Clearly, such a model is contaminated with the endogeneity related to the work decision. Therefore, ideally one would estimate the model, while accounting for the complications arising from this endogeneity, using a long panel

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3A similar mechanism is emphasized by Hall (1995) and Pries (2002) as a source of persistence of inflows into unemployment and of the unemployment rate itself.
based on a large representative sample. As such a panel does not exist we continue our focus on 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 is birth-cohort specific. This seems a reasonable assumption, since individuals born at approximately the same point of time will be subject to similar unobservable forces such as changing educational systems or labor markets. Accordingly, we construct a pseudo panel which allows us to control for these cohort level effects.

Our econometric investigation yields several new results. The cross—occupation dispersion in labor demand, as measured by the index of Net employment reallocation, has a strong association with total worker mobility. Education appears to influence occupational mobility but the magnitude, and even the direction of the effect, appears to depend on the level of unemployment. High unemployment weakens the effects of education on career mobility, suggesting that the importance of worker self-selection across occupations declines in recessions. Therefore, the demographic composition of employment explains some of the variation in worker flows. We also find that, as predicted by job-matching theory, worker mobility has significant residual persistence over time. Finally, we detect important unobserved cohort-specific effects. In particular, later cohorts have increasingly low unexplained occupational mobility, which contributes considerably to the downward trend in total employment reallocation over the period under examination.

The following section documents the remarkable movements in worker mobility for our sample period. Section 3 presents our empirical model and our estimation strategy. Section 4 presents a more detailed description of the data issues and Section 5 discusses the results. Section 6 concludes.

We commence our investigation by plotting our measures of Gross and Net reallocation in 1976-2000.\(^4\) Although our focus is on men, whose patterns are reported in Figure A.1, for the sake of comparison we report in Figure A.2 the same time series for women. As our measure of Gross reallocation is computed only for workers employed in consecutive years, we compute Net reallocation on the same sample to ensure comparability.\(^5\) We also report the time series of *Churning*, measured as the difference between Gross and Net. This represents the “excess” reallocation of employment not warranted by net redistribution, which can also be interpreted as a measure of “turbulence” in labor markets, generated by idiosyncratic shocks. This preliminary illustration offers an overview of the main facts that motivate our econometric investigation.

- **LEVEL.** Gross reallocation averages over 8% per year. This, however, is likely to be an underestimate due to time aggregation. That is, since we observe occupations one year apart we miss multiple occupational switches within a year. Net reallocation averages around 2% per year. Thus, churning accounts for over three quarters of these movements for men, slightly less for women, suggesting that idiosyncratic uncertainty about occupational choice at the individual (worker, job or match) level accounts for the majority of employment reallocation. This confirms previous findings on worker and job churning.

- **CYCLES.** The Gross reallocation of both male and female workers is strongly procyclical. The only remarkable exception is the failure of male mobility to rebound after 1992. Net reallocation, in contrast, appears much less procyclical. The early 1990’s recession is preceded in 1989 by a surge in Net reallocation of men across occupations.

\(^4\)The precise definitions of these series are provided below.

\(^5\)We also computed the Net reallocation index including flows in and out of joblessness. Although the characteristics of the workers in the two sample differ substantially, as shown later, the two Net series are quite similar. However, the series based on the working sample is, as expected, less cyclical.
• Trend. There appears to be a moderate decline in male occupational mobility, especially once we consider that the initial value in 1976 was a cyclical trough, namely below trend, on the basis of subsequent cyclical behavior. There is a more pronounced decline for females. The decline in occupational turnover is strong among young men, as men aged 40 or more witness a mild but steady increase throughout the period (Figure A.3). Net reallocation is fairly flat around 2% for all three demographic groups.

It is useful to contrast the findings from these series with the existing evidence from “macroeconomic” empirical studies of labor market flows, based on representative samples, although no such study covers our entire sample period. Davis et alii (1996) stress that job reallocation in the US manufacturing sector is countercyclical. Jovanovic and Moffitt (1990) find in the NLS that employment reallocation across three broadly defined sectors was procyclical in the 1970’s, with instances of countercyclical churning. Murphy and Topel (1987a) find in 1968-1985 CPS data that worker mobility across sectors declined over time and in recessions. This is similar to what we find in the overlapping periods of our samples, 1976-1985, and for our different definition of mobility. Finally, Kambourov and Manovskii (2002) report an increase in three-digit occupational mobility of male heads of household, older than 24, in the PSID in 1968-1993. This appears broadly consistent with our findings, as in our sample mobility is mildly increasing for men over age 40, as seen in Figure A.3, and most men in that age bracket head their respective households.6 Since we are the first to document occupational reallocation of labor in the entire 1990’s, we highlight that the dramatic structural break that occurred for males in the last decade is first reported here. The only similar finding, of which we are aware, is Fallick and Fleischman (2001). In the monthly CPS files for 1994-2001 they find, contrary to the conventional wisdom of strongly procyclical quits, a surprisingly flat employer-to-employer flow.

6Compared with our sample, Kambourov and Manovskii (2002) exclude also public sector workers, whose occupational mobility interestingly appears to be particularly low on average and sharply declining over the period. Their selection of head of household, private sector workers is not innocuous for the representativeness of the sample. As we show in Figure A.4, the proportion of heads among working age men declined over the period from almost 90% to less than 65%. Also, non-heads produce most of labor force transitions. As for public sector workers, they represent over 10% of US employment.
3. An Empirical Model of Occupational Mobility

We now turn to an examination of the various contributions to these time series patterns of Gross reallocation. Ideally one would like to estimate a model of individual behavior using a representative panel of individuals over the relevant period of time. As no such dataset is available, we employ the repeated cross-sections of the March CPS. To motivate our use of pseudo panel data we first introduce an empirical model of mobility at the individual level.

3.1. Individual Mobility and the Selection Problem

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

$$\text{mob}^*_{i,t} = x'_{i,t-1} \delta + \varepsilon_{i,t}$$

where $\text{mob}^*_{i,t}$ is the latent variable capturing the individual $i$'s propensity to change job type between times $t - 1$ and $t$; the $x_{i,t-1}$ is a vector of individual explanatory variables; $\delta$ is 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

$$\text{mob}_{i,t} = \begin{cases} 1 & \text{if person } i \text{ in cross section } t \text{ changed occupation between } t - 1 \text{ and } t \\ 0 & \text{otherwise.} \end{cases}$$

where

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

which says that the latent variable is above some minimum threshold $\text{mob}$, 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 following the decision to move.$^7$

$^7$This cutoff rule is a natural specification for a rational individual. For example, it can be interpreted as the optimal mobility policy in Moscarini (2001)'s equilibrium search-frictional Roy model, which features a continuum of skills $x$ and a finite number of job types.
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. A “jobless” occupation produces either job search, or home goods, or leisure (or combination thereof), which 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, which entails treating the individual participation and mobility decisions separately. Another reason for excluding unemployment is that we are interested in cyclical patterns of reallocation. Since unemployment is inherently countercyclical, its inclusion among our occupations would generate a large inflow in recessions and a burst of “reallocation”. 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 both the interview period and the previous year.

Consider the following model

\[
\text{BEMP}_{i,t} = \mathbb{I}\left\{x_{i,t-1}' \lambda + \nu_{i,t} > 0\right\}, \quad t = 1, 2..T; \quad i = 1, 2..N_t
\]

where

\[
\text{BEMP}_{i,t} = 1 \text{ if person } i \text{ in cross section } t \text{ is employed in both } t \text{ and } t - 1
\]

\[
\text{BEMP}_{i,t} = 0 \text{ otherwise.}
\]

and \( \lambda \) is an unknown parameter vector. Next

\[
\text{MOB1}_{i,t} = \text{BEMP}_{i,t} \cdot \text{MOB}_{i,t}
\]

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. We adopt a strategy in the spirit of the second approach. The estimation of this cross-sectional model, without making somewhat restrictive assumptions about the unobservables, requires the existence of some exclusion variable which affects the propensity to work but does not directly the mobility decision. The existence of such a variable seems problematic and does not appear to be available in the CPS. Accordingly, to correct for sorting 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 \( \nu_{i,t} \).

### 3.2. Birth-Cohort Synthetic Panel

Our main empirical strategy tackles the sorting-selection problem via the use of a synthetic panel. For each year we combine individuals born in the same year, and compute the average value for each variable. 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 \( c = 1, 2...C_t \) cohorts in a complete manner, as all \( N_{c,t} \) individuals in cohort \( c \) are of working age in that year \( t \). The model is

\[
\overline{\text{MOB}}_{c,t} = V_{t-1} \tilde{\theta} + \overline{\text{BEMP}}_{c,t} \beta + \overline{\varepsilon}_{c,t}, \quad t = 1..T; \ c = 1...C
\]

\[
\overline{\text{BEMP}}_{c,t} = V_{t-1} \hat{\phi} + \overline{x}_{c,t-1} \theta + \overline{\nu}_{c,t}, \quad t = 1..T; \ c = 1...C
\]

where

\[
\overline{\text{MOB}}_{c,t} \equiv \frac{\sum_{i \in c} \text{MOB}_{i,c,t}}{\#(i : i \in c, \text{BEMP}_{i,c,t} = 1)} = \text{E}_{i \in c, \text{BEMP}_{i,c,t} = 1} [\text{MOB}_{i,c,t}]
\]
is the average mobility of members of the cohort employed both last and this period. Similarly for \( \bar{x}_{c,t-1}^{\text{BEMP}} \), \( \bar{\xi}_{c,t} \). Next

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

is the employment rate of the entire working age sample of cohort \( c \) at that time \( t \). Similarly for \( \bar{x}'_{c,t-1} \), \( \bar{\nu}_{c,t} \). Finally, \( V_{t-1} \) is a vector of economy, or labor market, wide factors that may affect the individuals’ propensity to change career of each worker. \( V_{t-1} \) might include, for example, unemployment as a proxy for the state of the economy, or Net reallocation as a measure of structural change in the economy.

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

\[
\bar{\xi}_{c,t} = \lambda_{c,t} + \epsilon_{c,t}
\]
\[
\bar{\nu}_{c,t} = \lambda_{c,t} + n_{c,t}
\]

with \( \text{cov}(\epsilon_{c,t}, n_{c,t}) = \text{cov}(\lambda_{c,t}, n_{c,t}) = \text{cov}(\epsilon_{c,t}, \lambda_{c,t}) = 0 \) and \( \text{V}(\lambda_{c,t}) = \text{cov}(\bar{\xi}_{c,t}, \bar{\nu}_{c,t}) \).

Our identification assumption is that the correlation embedded in \( \lambda_{c,t} \) 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)**

\[
\bar{\xi}_{c,t} = \bar{\lambda}_{c} + \bar{\epsilon}_{t}
\]
\[
\bar{\nu}_{c,t} = \bar{\lambda}_{c} + \bar{n}_{t}.
\]

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

\[
\text{MOB}_{c,t} = V_{t-1}' \theta + \bar{x}'_{c,t-1}^{\text{BEMP}} \beta + CD'_{c} \gamma + \bar{\epsilon}_{t} \tag{3.2}
\]
by including cohort dummies $CD_c$ as additional regressors to account for, and estimate, the fixed effects $\lambda_c$. By controlling for the fixed effects we are able to consistently estimate $\beta$.

The estimation approach is a fixed effects procedures 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 A.4 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. The proportion of veterans is strongly cohort-dependent due to the timing of the major war events in the XX century. These trends do not appear to be linearly synchronized.

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 get higher education. 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 absorb it into the \( \tilde{\lambda}_c \) 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 \( \bar{\lambda}_c \). 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 microeconometric study of worker turnover (see, for example, Farber 1994).

### 3.3. Dynamics

An 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 Gross reallocation to persist. A similar positive auto-correlation might originate from aggregate variables, such as labor market tightness, which impact on reallocation and are typically very persistent. Hence, we use in the vector \( V_{t-1} \) the aggregate unemployment rate and Net reallocation to control for, respectively, aggregate and occupation-specific macroeconomic disturbances.\(^8\)

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\(^8\)Their first-order serial correlation is 0.98 for both Gross and Net reallocation if a constant is omitted, 0.63 and 0.43 respectively if a constant is included in the regression. The unemployment rate behaves locally almost like a random walk.
The model we estimate

$$\bar{MOB}_{c,t} = \rho \bar{MOB}_{c,t-1} + V'_{t-1} \theta + \bar{x}_{c,t-1}^{\text{EMP}} \beta + CD'_c \gamma + \bar{e}_t$$  \hspace{1cm} (3.3)

is based on the approach of Verbeek and Vella (1998) in which the static model (3.2) is augmented with the lagged value for the cohort. Verbeek and Vella (1998) 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

Our dataset includes 25 yearly cross-sections, from 1976 to 2000, of the US population contained in the March Files of the Current Population Survey. We consider this type of dataset to be the most appropriate for our investigations, for two 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 year.

Although the CPS is a rotating panel, we do not exploit this aspect because each individual is observed at most eight (nonconsecutive) times. We restrict attention to annual observations to avoid dealing with the formidable seasonality in worker mobility and to exploit the wealth of information available in the March survey. Questions are asked in the third week of March and concern household’s information concerning the previous week as well as, for a subset of variables, the previous year.
4.1. Sample

The choice of the explanatory variables is constrained by their availability for the entire period in a uniform format, or, at least, by the requirement that some uniform recoding be possible. We explain our selection and recoding rules below. Some reclassifications were already present in the version of the data that we used, commercialized by Unicon, Inc. along with an extraction software. The variable names that we employ below are drawn from that version, which also takes into account the 1994 re-design of the CPS. This selection of explanatory variables led us to focus on the 1976-2000 period, and to discard much useful information, which is available only for shorter and partially overlapping subperiods.

Our sample comprises male civilian non-institutionalized adults (\(\text{POPSTAT}=1\)) of working age (16 to 64, included) who are not in school or at home full time (0 \(\leq \text{ESR} \leq 3\)). After 1988, the Bureau of the Census modified the way it processed the raw interview data, introducing a new imputation method of missing answers and matching of records for the same individuals, and flagging with the variable FL-665 (recoded as SUPREC by Unicon) those cases where the entire record was imputed to make it internally consistent. On that occasion the data were released both in the old (March 1988) and new format (March 1988b). A comparison between the two reveals that we need to discard, in 1988b, 1989 and thereafter, all individuals with \(\text{SUPREC}=1\) to maintain consistency of definitions through the 1976-2000 period.\(^9\)

We consider an individual \(i\) to be employed both this year \(t\) and last year \(t-1\) (and set \(\text{BEMP}_{i,t}=1\)) if he reports to be either a salaried or a self-employed worker (1 \(\leq \text{CLASS} \leq 3\)).

\(^9\)This exclusion reduces sample size by about 10% after 1988. This incongruence between 1988 and 1988b was a major hurdle early in the early stages of our analysis. We thank Charles Nelson of the Bureau of the Census for pointing to the flag variable FL-665 as a possible explanation. Gross occupational reallocation is virtually the same in 1988b as in 1988 when excluding individual records with \(\text{SUPREC} \neq 1\) in 1988b, while it is much higher in 1988b than in 1988 when including those individuals in 1988b. A closer look reveals that the Gross reallocation of individuals with \(\text{SUPREC} \neq 1\) after 1988 is one order of magnitude larger than that of all other individuals in all years (about 60% to 70% as opposed to less than 10%), suggesting that the imputation of occupational codes in those records is quite noisy and unreliable. After circulating the first draft of our paper, we became aware of a new working paper by Stewart (2002), where he notices the same problem for job changes, irrespective of occupational or industry changes.
who worked either full time full year (FTPT= 1) or full time at least part of year (FTPT= 3), and who reports a valid Census three-digit occupation for last week’s occupation (OCC) and the occupation on his major job last year (OCCLYR). Among the employed, we consider individual \( i \) a job mover if he reports at time \( t \) a different occupation from last year: 
\[
\text{MOB}_{i,t} = \mathbb{I}\{\text{OCC}_{i,t} \neq \text{OCCLYR}_{i,t}\}.
\]

There exist an average of 453 occupational categories with each containing an average 0.22% of employment. The largest category, “Sales Supervisors”, comprises on average 7.5% of employment, while the smallest categories are a few occupations that have empty cells in some years. “Mathematical scientists” is a typical occupation that always comprises some individuals in the sample, but averages less than one out of ten thousand workers. The average number of observations per year is approximately 32,000. Identifying the largest gainers and losers over the entire period is difficult because of the changes in coding in 1983 and 1992, illustrated shortly below. In 1992-2000 the largest gainer was “Managers and Administrators (not otherwise classified)”, which went from 5.8% to 7% of employment, the main loser was “Technicians (not otherwise classified)”, from 0.7% to 0.1% of employment.

4.2. The Coding of Occupations in the March CPS

A hurdle that any study of worker turnover faces concerns measurement and coding of the relevant unit of employment, be it occupation, industry and employer, all subject to considerable measurement error. While in principle a bias in either direction may occur, it is quite plausible that erroneous classifications would generate an excess of spurious transitions, and bias upward our mobility measure. Such misclassifications originate from many sources, which require a careful consideration and various corrections.

Occupational codes at the three-digit level are not available before 1968, and the Census coding of three-digit occupations has changed three times in 1971, 1983, 1992, along with each decennial Census, from the initial 1968 system. Every year \( t \), the occupation of the job held the week before the interview (OCC\(_{i,t}\)) and the occupation of the main job in the previous year (OCCLYR\(_{i,t}\)) are collected by the interviewer in the form of a verbal description of the tasks
performed on the job, and later are both coded in the Bureau of the Census’ offices according to a unique system valid for year $t$, so there is no issue of spurious reallocation for that reason. However, reallocation might be rising spuriously upon occupational reclassifications if they become finer. Indeed, upon each re-coding we observe exclusion of dying occupations, introduction of new ones, and finer coding of existing occupations. The coding used for 1968-1970 is significantly coarser than, and thus incomparable with, those used later. This is why we focus on post-1970 data. Within the last three decades, the 1983 and 1992 coding systems are virtually identical and somewhat finer than the 1971 system, with about 20% more occupational categories employing less than 10% of all workers. This should slightly increase in a spurious manner our measured reallocation between 1982 and 1983, when the new finer system is in place.

The next issue is the imputation of information to missing records. We already mentioned our correction based on the FL-665 variable post-1988. An even more serious problem exists in 1975-1976, when the procedures for imputation of earnings were completely revised. Missing items, including occupation of employment, are typically imputed in the CPS according to a “hot deck” method, namely by matching the demographic characteristics of the worker. This procedure, however, appears quite noisy until 1976, when a major revision took place (see Lillard et alii, 1986, for details). Until 1976, the individual characteristics excluded sex and education, and used only 10 one-digit occupations, as opposed to the 453 three-digit occupations from the 1970 Census code adopted from 1976 on. These shortcomings make the pre-1976 data both suspicious and incomparable with later records. In fact, pre-1976 occupational mobility appears greatly inflated by huge measurement error. Following our method of computation, three-digit occupational mobility in 1975 is almost twice as large as in 1976, a difference that, judging from later episodes, even a recession cannot justify. Since no correction appears feasible, we start our sample in 1976. This is also the likely (albeit not explicitly stated) reason why also Stewart (2002) starts in 1976 his investigation of job mobility in the same dataset. This is not to deny that post-1976 CPS data are immune
from mismeasurement due to imputation of missing items. Our assumption is that the improved imputation method uniformly adopted from 1976 onwards made measurement error invariant to time and business cycle conditions.

Murphy and Topel (1987b) criticize the use of the CPS as a source of information about occupations. They conclude that in the 1977-1986 matched March CPS data most occupational transitions are spurious. They reason as follows. Every worker surveyed in March of years \( t - 1 \) and \( t \) (half the year-\( t \) sample) reports how many employers he had the year before, NEMPLYR\(_{i,t}\). Therefore, if he reports in March of year \( t \) having had only one employer the year before, then he should also report at \( t \) as last year’s main occupation the same as the one he reported in March of year \( t - 1 \) as his current (last week’s) occupation. Namely, \( \{\text{NEMPLYR}_{i,t} = 1\} \Rightarrow \{\text{OCC}_{i,t-1} = \text{OCCLYR}_{i,t}\} \). The matched March CPS data reveal that this implication fails for the majority of the relevant workers with NEMPLYR\(_{i,t} = 1\), and the authors conclude that this is due to measurement/coding error. We find this reasoning unconvincing for many reasons, that we discuss in detail because they also shed light on various other measurement issues.

First, a worker can change occupation without changing employer, if he is promoted during the year after the March interview. When this occurs, it is natural that the following year the worker would report his main job the previous year to be the one after the promotion. In fact, we verified on a random sample of 1,000 workers matched in 1999 and 2000 that, of those who fail the Murphy and Topel test, namely \( \{\text{NEMPLYR}_{i,t} = 1\} \Rightarrow \{\text{OCC}_{i,t-1} \neq \text{OCCLYR}_{i,t}\} \), over 82% report OCCLYR\(_{i,t} = \text{OCC}_{i,t}\), so they appear as non-movers according to our criterion. Promotions cannot be directly observed in the CPS. However, if on average a worker is promoted and changes one-digit occupational group while staying with the same employer every 13 years, or roughly three times over his working life, then one can explain over half of the 17.8% discrepancy that Murphy and Topel report at the one-digit level, without appealing to measurement error.

Second, the CPS adopts a “dependent coding” technique. The interviewer first uses
various questions to determine whether the main job last year is the same as the job last week, and if so marks a check item. The programmer then automatically codes the three-digit occupation and industry to be the same for both jobs. Therefore, independent coding of the two jobs (which increases the potential for their mismeasurement) occurs only if the two jobs are definitely not the same, which occurs in a small minority of all cases. This interviewing technique, expressly designed to keep spurious occupational and industry transitions to a minimum, makes the March CPS data the highest quality data available for such transitions. In fact, miscoding may occur only when there is a job change. Thus, if no mistakes are made in identifying jobs last year and last week as the same—and many questions are asked to this purpose—our measures of mobility are an upper bound to the true ones at annual frequency. Of course total true mobility is higher than the annual measure due to the time aggregation that we discuss below.

Finally, Murphy and Topel’s calculate a “true” one-digit occupational mobility, namely the percentage of $\text{occ}_{i,t} \neq \text{occlyr}_{i,t}$ reports among all those who do meet their test, equal to 2% per year, and a two-digit industry mobility of 3.9% per year. This order of magnitude for both measures is inconsistent with all available evidence on other types of worker transitions in the CPS and on the same transitions in other US datasets. For example, job-to-job quits amount to over 2% per month in the CPS (Fallick and Fleischman 2001), and, as a very conservative estimate, over 20% of the employed workforce changes job from March to March. In the NLSY, two-digit industry mobility averages 36% per year (Moscarini and Vella 2003). While measurement error could be important there as well, and the young age of the NLSY sample inflates mobility, 36% is still a long way from their 3.9%. Kambourov and Manovskii (2002) find in the PSID for a comparable period an average one-digit occupational mobility in excess of 13%. Although this high number casts doubts also on the PSID as a source of valid information on occupations (after all, dependent coding should make our much lower estimates an upper bound to the true value), this casts doubt also on the Murphy and Topel’s estimates. In conclusion, while we are aware of the measurement error in occupations, we
believe that the residual error after our corrections is relatively benign and roughly time-invariant in the period we examine. At any rate, we could not find any additional corrections that would not severely select the sample.

4.3. Time Aggregation

Our definition of an occupational transition, through the comparison of the job in March $(\text{OCC}_{i,t})$ a week before the interview to the longest job in the previous year $(\text{OCCLYR}_{i,t})$, introduces time aggregation, because the longest job in the previous year has variable duration and may differ from the job in March of the previous year. Due to dependent coding, our comparison between $\text{OCC}_{i,t}$ and $\text{OCCLYR}_{i,t}$ introduces far less measurement error than the two possible alternatives that are more immune from time aggregation problems. These are, only for matched individuals in their second rotation: comparing jobs in March of consecutive years $(\text{OCC}_{i,t} \text{ vs. } \text{OCC}_{i,t-1})$, and comparing longest jobs in two consecutive years $(\text{OCCLYR}_{i,t} \text{ vs. } \text{OCCLYR}_{i,t-1})$. The main advantage of our procedure is truly dependent coding, which eliminates virtually all of measurement error of one type (attributing different occupations to the same job). We can also use all men workers in first and second rotation, doubling the sample size and halving the sampling error.

The implications of time aggregation are straightforward. If a worker changed occupation only once, in April, May or June every year, he would always appear to us as non-mover, because his main job in the year would be the one held from April-June to December, and would still be held in March of the following year when he is interviewed (again). If instead the occupational switch occurred after June each year, then the job held in March is more likely to be the main job, and the following year’s March interview would capture the correct March-to-March transition.
4.4. Other Measurement Issues

An important feature of the CPS for our purposes is its address-based nature. People who change permanent residence at any time between the first and the eighth interviews are dropped from the sample thereafter. This might bias downward our estimate of occupational reallocation, as an individual who changes occupation is also more likely to change residence. Several considerations suggest that this should not be a major issue. First, about 1/8 of the sample population in March is new and does not suffer from this problem. Second, after the first interview, the interviewer returns to the same address, and if this is under construction or vacant, he or she keep returning, until finding new members of the household living at that address, possibly an entirely new (the so-called “replacement”) household. It is plausible that these individuals, who enter the sample survey just because they changed residence, are as likely to have moved to that address because they changed occupation as those who left the household. In this case the two geographical relocations would leave total occupational reallocation in the sample correctly measured. Hence, on average the selection effects on occupational reallocation due to the inflow into and outflow out of each household-address (including complete replacement) tend to cancel out, and this is our maintained assumption. In fact, in March 2000 the Gross occupational reallocation of the individuals in their first month in the sample was 7.9%, as opposed to 7.6% of the total sample, a small difference in relative terms, possibly due to sampling error. We decided to use the full sample, rather than focus on the ideal subsample of first-time interviewed, because we believe that the advantages of the eight-fold gain in terms of sample size more than offset the disadvantages of this small bias.

Another important issue concerns measurement error in employment, which naturally tends to inflate reallocation. We do not perform an Abowd-Zellner (1985)-type correction of measurement error on employment status, because we consider only the employed who report a valid occupation for two consecutive years, which are unlikely to be unemployed workers misclassified as employed. Indeed, the overhaul of the CPS interviewing techniques
in 1994 might have reduced measurement error so as to reduce measured reallocation in 1994-2000 relative to 1976-1993. While this might explain the low Gross reallocation of the late 1990’s, relative to the previous period, it would not explain its lack of a cyclical rebound. In addition, women do exhibit a sharp increase in reallocation after 1994 (Figure A.2).

Measurement of education in the CPS is also problematic. In 1976-1991 the CPS March files contain the years of education of the individual in March, with an auxiliary dummy variable indicating whether the highest grade attended was completed. Starting in 1992, the measurement of educational levels changes and 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, one reliable measure of education that we can consider consistent through the two subperiods (hence through 1976-2000) is a pair of dummies, one indicating whether the individual achieved a High School degree or got some College (HS), the other whether he/she achieved a College degree (BA or equivalent) or even had some graduate studies (COL). 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 inconsistent change of classification. 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.

Since surveys take place at time $t$ and ask information as of time $t$, except for employment last period, we do not observe individual variables $x_{i,t-1}$ last year, which should determine the mobility decision, but rather $x_{i,t}$. Therefore we replace $x_{i,t-1}$ with their values one period forward $x_{i,t}$, at time $t$. At any rate, the explanatory variables in $x_{i,t-1}$ we choose have extremely high serial correlation at the individual level.
5. Regression Specifications and Results

5.1. Specifications of the Birth-Cohort Synthetic Panel-Based Model

To estimate the synthetic cohort model (3.2) and explain the variation in career mobility, we employ a constant term, a 4\textsuperscript{th}–order polynomial in age, and seven dummies: white ethnicity, African-American ethnicity, married with spouse present, head of household, war veteran status, High School graduate or some College, College (BA) graduate with or without post-graduate studies. We also explore directly the role of macroeconomic variables on the average level of cohort occupational mobility, although we acknowledge that we make no attempt to control for their possible simultaneity. We include interaction terms between the unemployment rate, as a gauge of the state of the labor market, and educational attainments; the theoretical motivation for the interaction terms is illustrated later.

We construct our birth cohorts at yearly frequency. For each cohort we lose the first observation because of the initial condition of lagged mobility. While in principle we may include all individuals born between 1912 (who were 64 and about to retire at the beginning of the sample in 1976) and 1984 (who were 16 and just in the labor force in the last year of the sample 2000), we restrict attention to individuals born in 1920-1970, so each cohort is observed at least ten times in the sample and always in part during adult life. Given the unbalanced nature of our pseudo panel we have 1073 observations in total. Birth cohort dummies are included from 1921 to 1970.

Figure A.4 summarizes the characteristics of our sample. It illustrates the time series of each of these explanatory variables, averaged across individuals who fulfill our selection requirements, basically the US male labor force. The time series for the men born in the selected year span 1920-1970 are very similar, and not reported. An examination of these plots reveals 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 declined in favor of Hispanics and other ethnic groups, and the proportion of the sample that was 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. The proportion of labor force participants who are employed in two consecutive years (the $\text{BEMP}_{i,t} = 1$ sample) was strongly procyclical, and declined somewhat over the period.

To cast some light on the possible selection problems from examining the employed both last and this year ($\text{BEMP}_{i,t} = 1$) we report in Figure A.4 the plots of cross-sectional averages for both the $\text{BEMP}_{i,t} = 1$ sample (dashed) and the entire sample (solid). The two series look reasonably similar in their trends but differ in their levels. This strongly suggests that the unobservables may also be different across groups and this may create a selection problem in the absence of some appropriate correction.

Before proceeding, it is useful to discuss some potentially important information which we are either unable to use or decide not to employ. Two unfortunate lacunae of the CPS March files for our purposes are measures of tenure on the current job and of work experience. Tenure is surveyed only every few years in a Tenure Supplement. The “experienced labor force status” dummy is not useful, because all workers who are employed in two consecutive years are “experienced” in this sense. 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. We choose not to exploit wage information, which might be useful to distinguish between voluntary and involuntary changes of occupation, because this distinction is not particularly relevant to our purposes. The higher mobility of (say) less educated individuals might be due to their higher risk of displacement, with consequent forced change of occupation, or by their willingness to accept any kind of job. We do not explore such an interpretation, and restrict ourselves to detecting the total effect of worker (as opposed to job) characteristics on mobility.
5.2. Results

Table 1 presents the regression results. We report the estimates from a range of specifications, labelled I through IV, to illustrate the sensitivity of the estimates to the inclusion of additional variables. Estimates that are statistically significant at the 1% level are in boldface.

Our empirical strategy is the following. Initially we explore the relationship between the individual’s characteristics and the probability of occupational mobility. This is reported in column I of Table 1. This specification represents a dynamic model with birth cohort dummies but does not include any role for the macroeconomic variables. In the remaining columns we augment this specification to include variables capturing the original occupation (column II), and then the macro variables (column III) and some interaction terms involving the macro variables and the education measures (column IV). It is this final column that 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 column IV estimates.

Worker Characteristics (Excluding Education). First consider the role of the background variables on the probability of occupational mobility. The results in column IV indicate that there is an important role played by the race of the individual with the coefficients on the race dummy variables indicating a statistically significant difference between Hispanics and both whites (11 percentage points) and African Americans (18 percentage points). In both instances the Hispanic group reveals a lower tendency to change occupation. Note that these effects are large in magnitude given the overall level of mobility.

Aspects of the individual’s family commitments are captured by the head of the household indicator and the marital status dummy variable. Men who are married with spouse present are 9 percentage points less likely to change occupation than males who are unmarried. Since spousal considerations influence geographical mobility in many ways, the expected
direction of the effect is unclear. That is, a married man cannot as easily change residence
to pursue a new career opportunity, but may also consider career changes to stay in the same
location or accommodate his spouse’s career. It appears that the first effect dominates. The
independent effect of being also the head of the household is also negative, a 3 percentage
point decrease in mobility. Again we highlight that these effects are large.

The war veteran status of a worker has a positive and precisely estimated effect on
mobility. The coefficient suggests an increase in the probability of occupational mobility of
around 5 percentage points. There are two possible interpretations. First, the skills acquired
while serving in the military make these workers more flexible in the labor market. Second,
veterans are a selected part of the labor force, in a way that their birth date cannot fully
capture. In separate regressions that we have performed and do not report here, we found
that this positive effect was absent among veterans of WWII, and quite strong for veterans
of later wars. It is tempting to conclude that the former were conscripted \textit{en mass}, thus not
selected, while the latter group reflected self-selection.

The coefficients capturing the role of age are estimated precisely and the profile from our
preferred specification is presented in Figure A.6. The profile, which is presented for the
1960 cohort, implies that career mobility falls with age at decreasing rates. More explicitly,
we see that at age 16 the probability of a change in occupation is 28 percent although this
decreases to the sample mean of 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.

**Source Occupations.** Column II exploits information on the source occupation of movers and stayers. Occupational choice is endogenous to mobility, so including it without controlling for its endogeneity will lead to inconsistency. Under our assumption that the endogeneity of occupational choice is birth cohort specific, and time invariant, the inclusion of the cohort dummies is a first step in overcoming this endogeneity. Ideally, we would regress mobility on the shares of all (but one) of the three-digit occupations used to construct our dependent variable. However, this would entail estimating over 400 extra parameters. In the second column of Table 1 we include the share of employees belonging to each cohort who worked last year (when the mobility decision was taken) in each of five major occupational groups: Managerial and Professional Specialty occupations; Technical, Sales, and Administrative Support occupations; Service occupations; Farming, Forestry and Fishing occupations; and Operators, Fabricators and Laborers, excluding the sixth group Precision Production, Craft, and Repair occupations. The results suggest that Laborers are more likely, and workers employed in Farming and Fishing-related careers are much less likely, than others to switch to a different three-digit occupation, within or outside their one-digit group. These results probably reflect a combination of factors, such as regional considerations and human capital requirements. Note, that due to the potential endogeneity of these variables, combined with the lack of sensitivity of the other coefficients to their inclusion, we decided to proceed while not including them.

**Macroeconomic Factors.** We envision two sources of macroeconomic shocks that affect total occupational mobility. The first is the variance of occupation-specific shocks, which remake continuously the pattern of labor demand, determining the birth, success and death of an occupation. This is an ongoing process that originates both from technological progress, which creates new professional categories, and from demand for different goods, whose mix
changes with income and demographic characteristics. To measure this type of shock we adopt a Lilien (1982)-type index of net employment reallocation across occupations. If this process of growth and decline of different occupations has either a trend or a cyclical component, this measure should detect them. Implicitly, we are assuming that the entire net reallocation across occupations is driven by labor demand, while changes in labor supply affect total mobility. This assumption finds support in the fact that net reallocation is similar, while gross reallocation differs widely, when comparing men, women and men over 40 (in Figures A.1, A.2, and A.3 respectively). So net reallocation appears independent of the demographic group we consider. Our assumption says precisely that net reallocation is exogenous to labor supply, and caused by other factors, that we naturally interpret as shocks to the relative demand for different occupations.

The other macroeconomic source of employment reallocation across occupations are 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 civilian unemployment rate measured as the yearly average of monthly unemployment rates published by the Bureau of Labor Statistics. The average over the entire period is 6.6%. 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 2003), but lag the cycle, the unemployment rate appears to be an appropriate proxy for labor market tightness.

The interpretation of the estimates for the two macro variables is slightly complicated by the fact that unemployment may be caused by Net reallocation, as argued by Lilien (1982)’s sectorial shift hypothesis. Although this hypothesis has not survived subsequent scrutiny, some of the effect of Net reallocation might be working through induced additional unemployment.\textsuperscript{10} However, rather than abandon the investigation, we examine if there

\textsuperscript{10}Ideally, we would like to employ also the cohort-specific unemployment rate and Net reallocation. However, the total unemployment rate seems a less noisy measure of labor market tightness; also, the relatively small number of individuals in each cohort (about 200 on average) makes the estimate of net reallocation across 450 occupations too noisy at the cohort level. We did in fact compute and used this cohort-specific
is any role, noting that the results should be treated somewhat tentatively. 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.

We do find that Net reallocation has a multiplicative and very precisely estimated effect on its total counterpart. For example, consider a period contained in the data which witnessed a large redistribution of employment. One such instance is 1989-1990, which saw Net reallocation at the three-digit level rise from 2.9% to 3.7%, the largest year-to-year change since the first oil shock. According to our estimate, this 0.8% burst of additional Net reallocation implies per se an extra .85% of Gross reallocation. This appears a large figure given that Gross reallocation was around 8% on average.\textsuperscript{11}

As expected from the trends in the unconditional series, unemployment has a negative association with mobility. The direct effect is not statistically significant at the 1 percent level when we include the interaction terms involving education. However, in the previous specification where the interaction terms are excluded the direct unemployment effect is statistically significant at this level. This substantive result is also consistent with previous raw correlations found in sectorial mobility by Murphy and Topel (1987a) and by Jovanovic and Moffitt (1990), although these authors only condition on worker age. 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 Bils and McLaughlin 1992). However, this phenomenon predicts a correlation between unemployment

\textsuperscript{11}To better understand the nature of this episode, we also considered Net reallocation at the one-digit occupational level. In 1990 it rose to 1.15% from 0.5% the year before and from an average of 0.7% in the three previous years. We observe expanding employment shares for Technical, Sales, and Administrative Support occupations and for Service occupations, at the expenses of the other four groups (Managerial and Professional, Farming and Fishing, Operators, Fabricators and Laborers, and Precision Production, Craft, and Repair). According to our estimate of the “multiplication” effect, based on three-digit occupations, this 0.65% burst of additional Net reallocation implies per se an extra 0.8% of Gross reallocation, a very large figure given that Gross reallocation at the one-digit level was below 5% in that period.
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.

**Education and its Interaction with Unemployment.** We now turn to formal education, a feature of particular interest both as a potential measure of skill specialization and because of its dramatic change in the sample period. By introducing cohort dummies, we are effectively instrumenting all variables with birth year. The idea is that later cohorts are more educated not because they are different in some unobservable trait that also affects mobility, but only because they were born later, when the higher income levels made education more affordable and pervasive. This allows us to interpret the cross-cohort differences in education as exogenous, and the estimates as unbiased.

We also interact the educational dummies with the unemployment rate to examine whether the influence of formal education varies over the business cycle. One model which provides such a prediction is Moscarini (2001)’s Roy model of worker self-selection in frictional labor markets. This model predicts that workers choose types of jobs based on a trade-off between microeconomic incentives, captured by their individual comparative advantage that we measure with formal education, and macroeconomic incentives, which we measure with unemployment. When unemployment is low and the labor market is tight, workers can afford to wait for the “perfect” job, and comparative advantages matter. When unemployment is high, finding a job is more important than the type of job, and predicting which type of job a worker will take becomes more difficult.

We find that education has a strong and precisely estimated effect on occupational mobility both by itself and in its interaction with unemployment when, and only when, both terms are present. When the interaction terms are omitted, education per se has no discernible effect, a near-zero estimate that hides ample cyclical variations. The direct effect of holding a High School degree, or even some College, is to increase career mobility by over 10%.
Conversely, the direct effect of holding a College degree reduces career mobility by over 18%. However, these estimates are misleading given that the education coefficients are interacted with unemployment and thus these estimates correspond to a zero unemployment rate. In Figure A.7 we report the total effects based on the estimates of our preferred specification and the evidence is quite remarkable. In contrast to the lowest education group we see that the High School effect is generally positive and relatively constant over the sample period. In contrast we see that a College degree generally slightly raises occupational mobility, although the effect turns negative in expansions. However, it is worth highlighting the magnitude and the variability of this College effect. In the early 1980’s we see that holding a College degree increases the probability of mobility by approximately 14 percentage points while by the end of the sample it actually reduces the probability by about 6 percentage points. This represents a substantial reversal and appears to be a feature of the 1990’s. Also, recall, that this College effect is in comparison to the lowest education group. A similar comparison with the completed High School category produces an even more dramatic change. This evidence clearly suggests that educational attainment is playing a major role in the patterns of our time series measures of mobility. Finally, we note that these estimates only capture the effects of the quantity of education, while changes in the quality of education over the decades are cohort-specific and should then be reflected in the estimated cohort effects.

**Dynamics.** A main advantage of the pseudo-panel is the possibility of estimating consistently and relatively easily 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 quite precise and reasonably stable across specifications. Given that the model is simply a linear regression the interpretation is straightforward. That is, suppose that in going from time $t$ to $t+1$ we observe 100 individuals change occupations. The estimate implies that in going from $t+1$ to $t+2$, more than eight of these individuals will change occupation again. So the chance of not having settled in a new career within a year exceeds 8%. 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 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 this dynamic effect, in addition to exploring its magnitude.

**Birth Cohort Effects.** We now comment on the pattern of the estimated cohort dummy coefficients. Given the large number of estimates we report them by plotting them in Figure A.8, for each of the four specifications, 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 equation. Second, in our preferred, latter specifications the estimates of the cohort effects are typically declining over time, suggesting that later cohorts (starting from those born in the mid-1950’s) have an unexplained and statistically significantly lower propensity to change occupation. This effect cannot be due to age differences, because we do control for aging in a quite flexible manner, and because younger cohorts should be expected to change career even more often.
The strength of the decline varies by specification. Those, for example, which include the unemployment rate as a control seem to have a more distinctive downward pattern, in contrast to the specification which controls for the initial occupational distribution. This indicates that later cohorts are more relatively frequently choosing occupations that feature below-average exit rates. Finally, additional specifications with cohort-education interaction effects (whose results we do not report) reveal no direct role for education or cohort effects, in that both effects operate through the interaction. This suggest that the education effects are operating purely through the cohort effects or, alternatively, the cohort effects are education effects themselves. One mechanism underlying such a result is that the type of education is varying by cohort and thus it is difficult to disentangle the two effects. It may also suggest that education does have a direct role, as would be expected, but it is difficult to identify it in the face of changing educational quality and quantity by cohorts. This difficulty is also compounded by the complicated relationship existing between educational attainment and unemployment which we reported in A.7.

5.3. Discussion

In the light of our findings concerning the long-run changes in the composition of labor supply (Figure A.4) in terms of various characteristics, 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 A.1 and A.3).

The pronounced movements at cyclical frequencies appear to be well captured by both measures of macroeconomic shocks, Net reallocation and mainly 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. As well-known, highly educated and experienced workers are disproportionately represented in employment during recessionary phases. While the effect of educational composition is roughly offset by the change in individual attitudes due to unemployment, the composition of employment by experience contributes to synchronize mobility with the business cycle.
In Figure A.4, 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 A.1. However, Figure A.3 illustrates that older workers have experienced a slight but steady increase in occupational mobility over the period. By simple accounting, younger cohorts must have experienced a sharp drop in the propensity to change occupation. Some of this drop can be explained by the increasing proportion of College graduates, and by the decreasing proportion of mobile veterans, in the US the male workforce. However, the increasing proportions of unmarried and Hispanic workers, both characteristics that predict higher occupational mobility, work in the opposite direction. In fact, cohort effects reveal that some of the decrease in occupational mobility among later birth cohorts remains unexplained by our statistical model.

Our main findings concerning cohort effects are, in the specification that we prefer based on our underlying theoretical view of occupational turnover, a downward trend in the cohort effects contributing to reallocation, and a subtle acceleration in the rate of decline for the cohorts born in the mid 1950’s and onwards. There is a striking parallel with the findings of Card and Lemieux (2001), who 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. 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.

At this stage we formulate two tentative conjectures for our finding, whose rigorous investigation we leave for future research. First, the quality of College education in the US has changed over the decades, and has become increasingly specialized, along the lines
of the European model. The increase in number and the fragmentation of College majors supports this hypothesis. Since later cohorts are also more educated, their unexplained lower mobility could be explained through measurement error in education. This is also the interpretation embraced by Gosling, Machin and Meghir (2000), who argue that educated workers in these later cohorts received a different quality of human capital in school and College. We remark that if this new human capital is more specialized than before in the type of skills that the market turned out to require, then we should not be surprised by the “unexplained” simultaneous rise in the College premium and decline in occupational reallocation that we observe for workers born after the mid 1950’s. We note that the evidence in Table 1 suggests that the cohort effects appear to have some educational component in them. This is supported by the evidence that despite the cohort coefficients being very precisely estimated, there appears to be some difficulty disentangling the cohort and the education effects when one allows for interaction effects.

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 dataset 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. Conclusion

We investigate the evolution and the sources of aggregate employment reallocation in the United States in the 1976-2000 March files of the Current Population Survey. We focus on the annual flows of male workers across occupations at the Census three-digit level, the finest disaggregation at which a moving worker changes career and relocates to an observationally different technology.

We find that the total reallocation of employment across occupations has been strongly
procyclical and declining, especially among younger workers, until the early 1990s, before remaining relatively constant in the last decade. To reveal the sources of these patterns, while correcting for possible worker selection into employment, we construct a synthetic panel based on birth cohorts, and estimate various models of worker occupational mobility. We obtain five main results. The cross-occupation dispersion in labor demand, as measured by an index of net employment reallocation, has a strong association with total worker mobility. The demographic composition of employment, more specifically the increasing average age and College attainment level, explains some of the vanishing size and procyclicality of worker flows. High unemployment also directly reduces worker mobility. Moreover, the unemployment level also interacts with the education to influence worker mobility although the magnitude, and even the sign, of the total effect is dependent on the level of unemployment. Worker mobility has significant residual persistence over time, as predicted by job-matching theory. Finally, we detect important unobserved cohort-specific effects. In particular, later cohorts have increasingly low levels of occupational mobility beyond what can be explained by their observable characteristics. These cohort effects have contributed considerably to the downward trend in total employment reallocation over the last three decades. Unobserved heterogeneity of labor supply across birth cohorts has been suggested by other authors to play an important role in the increasing wage inequality over our sample period. A natural direction of future research is to uncover the nature of such heterogeneity.
References


KAMBOUROV, GUEORGUI AND IOURII MANOVSKII, 2002, “Rising Occupational and


A. Appendix. Figures and Tables
Figure A.1: reallocation of male workers across 3-digit occupations.
Figure A.2: REALLOCATION OF FEMALE WORKERS ACROSS 3-DIGIT OCCUPATIONS.
Figure A.3: REALLOCATION OF MALE WORKERS AGED 40 OR MORE ACROSS 3-DIGIT OCCUPATIONS.
Figure A.4: Sample characteristics, men: labor force (solid) and employed in years $t$ and $t-1$ (dashed).
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Figure A.5: Regression results, synthetic panel.
Figure A.6: Profile of occupational mobility by age for an otherwise average male worker.
Figure A.7: Total effect of education on occupational mobility, including interaction with unemployment (specification X)
Figure A.8: Estimates of birth cohort effects, with 1% confidence bands.