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Working Paper

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IFS Working Papers, No. 02/18

Provided in Cooperation with:
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Suggested Citation: Moscarini, Giuseppe; Vella, Francis (2002) : Aggregate worker reallocation and occupational mobility in the United States: 1971 - 2000, IFS Working Papers, No. 02/18, Institute for Fiscal Studies (IFS), London, https://doi.org/10.1920/wp.ifs.2002.0218

This Version is available at:
https://hdl.handle.net/10419/71431

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AGGREGATE WORKER REALLOCATION AND OCCUPATIONAL MOBILITY IN THE UNITED STATES: 1971-2000

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THE INSTITUTE FOR FISCAL STUDIES
WP02/18

Aggregate Worker Reallocation and Occupational Mobility in the United States: 1971-2000*

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September 2002

Executive Summary

We investigate the evolution and the sources of aggregate employment reallocation in the United States in the 1971-2000 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. We find that the total reallocation of employment across occupations has been strongly procyclical and sharply declining 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 weakens the effects of individual worker characteristics on their occupational mobility. 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 unexplained occupational mobility, which contributes considerably to the downward trend in total employment reallocation over the last three decades.

*Moscarini thanks Yale University and the Alfred P. Sloan Foundation for financial support, and the hospitality of the European University Institute when working on this project.

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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. First, substantial idiosyncratic heterogeneity remains in the productivities of firms and workers after conditioning on their observable characteristics (e.g. Abowd, Kramarz and Margolis, 1999) and persists through time at the firm level (Haltiwanger, Lane and Spletzer 2000). Second, resource reallocation across plants explains about half of total productivity growth in US manufacturing (see Haltiwanger 2000 for a survey).

The applied literature has provided evidence on several measures of different definitions, varying by level of disaggregation, of labor market-wide turnover. The macroeconomic side of this literature has documented the magnitude and time series patterns of job turnover (e.g. Davis, Haltiwanger and Schuh 1996), worker turnover across sectors (Murphy and Topel 1987) and employment states (e.g. Blanchard and Diamond 1990). These findings have greatly influenced theoretical work in macroeconomics, as best exemplified by Caballero and Hammour (1996). We continue this line of empirical investigation on the worker flows side. More precisely, we study the reallocation and the mobility of male workers among Census 3-digit occupations, using micro-data representative of the US population, the March Files (Annual Demographics plus Income Supplement) of the Current Population Survey over the 1971-2000 period. This is the finest level of disaggregation of occupations at which a career change represents a reallocation of skills to an observationally different technology.[1] There exist over 450 such occupations, as opposed to the six or seven major occupational groups commonly considered in the literature.

Our first goal is to document the time series behavior of *Gross and Net Employment*

[1]In this respect, occupational mobility appears *a priori* more meaningful than the more explored labor reallocation across sectors, industries, or plants, where the difference in technologies combining worker skills and capital is both unobservable and more questionable. A secretary performs similar tasks in many different industries or firms although, of course, considerable heterogeneity exists at finer levels of disaggregation. In the Census Occupational codes, in the same 1-digit group "Managerial and Professional Specialty occupations", we find such well distinct 3-digit categories as Architects, Dieticians, and History Teachers. Finer 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. We contend that 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.

Reallocation. Gross is the proportion of workers employed in two consecutive periods who change occupation in between (at least once), and is also a measure of average worker mobility. Net is one half of the sum of the absolute changes in occupational employment shares. This is a measure of the reshuffling required to accommodate changes in the distribution of employment across occupations, adopted both by Murphy and Topel (1987) and Jovanovic and Moffitt (1990) as an alternative (but similar) index to the dispersion of employment growth rates proposed by Lilien (1982).[2] We also report the time series of *Churning* which is commonly defined to be the difference between Gross and Net. This represents the "excess" reallocation of employment not warranted by net redistribution. Fig.1 and Fig.2 report these time series, respectively, for men and women. The striking patterns they reveal motivates our econometric analysis. The remainder of the paper focuses on males although for completeness, and to allow for comparison, we report also the time series for women.

- TREND. From 1971 to 1992, the total (Gross) occupational reallocation of male workers falls by about 30%. In the long 1990's expansion the decline in the series flattens out. The annual average level is 9.5% in the 1970's, 8% in the 1980's and 7.2% in the 1990's. In the 1970's, Net Reallocation follows a similar but less volatile pattern; since the first oil shock, in 1974-1975, its trend is slightly positive while Gross Reallocation keeps declining. Thus, Churning follows a similar and even more pronounced pattern than Gross, with no significant time series variation after 1992. The Gross occupational Reallocation of female workers shows a similar trend, although there also appears to be a relatively steady increase in its Net component throughout the 1990's.

- CYCLES. Gross occupational Reallocation of male workers appears strongly procyclical until the 1990-1992 recession. Net Reallocation, in contrast, appears much less procyclical. The negative effect of the first oil shock in 1975 is particularly severe and persistent on both measures, while the early 1990's recession is preceded in 1989 by a surge in Net Reallocation. After 1992, the link with the business cycle is broken, and all measures of reallocation are almost perfectly flat, without the recovery in the 1990's which would have been expected from previous cyclical patterns. Female workers show similar, even more pronounced patterns, but in the 1990's the Gross, and especially Net, series rebound strongly as in previous expansions.

[2]Throughout the paper, "Net Reallocation" denotes our statistical measures, and "net reallocation" the general concept of redistribution of employment across occupations. Similarly for Gross Reallocation. Our Net Reallocation is computed on the same sample of workers employed in consecutive periods used for Gross, to make the two measures directly comparable. We also computed the 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; our series is obviously less cyclical.

- SIZE. Gross Reallocation averages over 8% per year, which understates the true amount of total reallocation, due to time aggregation. 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 bulk of employment reallocation. This fact confirms previous findings on worker and job churning.

Extant "macroeconomic" empirical studies of worker reallocation, based on large and representative samples to detect aggregate phenomena and business cycle effects, do not extend significantly beyond the 1980's. Davis *et alii* (1996) stress that job reallocation is countercyclical, although their finding seems unique to the US. Jovanovic and Moffitt (1990) is a rare empirical investigation of job-matching theory and sectorial employment reallocation, which also provides some evidence on the effects of business cycles, albeit limited to the NLS. They find that reallocation 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, along the lines of what we find for the same sub-period of our sample. The 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 analyzing the monthly CPS files for 1994-2001, they find a surprisingly flat employer-to-employer flow. This is contrary to the conventional wisdom of strongly procyclical quits.[3]

Our second and main goal is to identify the separate contributions of various factors to these trends and cycles. The literature mentioned earlier emphasizes idiosyncratic labor demand shocks or differential responses to aggregate shocks across firms, plants, industries as the main source of ongoing reallocation. However, the ultimate effects of such shocks also depend on the flexibility of labor supply. We take a wider perspective and contend that the mobility of workers across jobs, industries, and in our case occupations also plays a central role in shaping aggregate employment reallocation. We group the sources of aggregate reallocation and worker mobility into four main categories, corresponding to different (but compatible) macro or microeconomic theories of labor turnover.

[3]There is a more than suggestive parallel between the declining worker reallocation that we document and the secular decline in the volatility of GDP in the US and other developed countries (e.g. see Blanchard and Simon 2000). The secular expansion of the service sector reduced the volatility of absorption, but cannot explain the decline in occupational reallocation that we observe. In most service industries and related occupations total employment is relatively stable at cyclical frequencies, but worker turnover and churning are unusually high. Thus, total occupational reallocation should have increased as the industrial structure of the US economy shifted towards services.

1. Net Reallocation of employment across occupations. The Schumpeterian view emphasizes imbalances in labor demand across sectors or firms as an engine of employment reallocation and economic growth. Plausible causes are technological progress, changes in relative preferences, and similar types of reallocative shocks, holding total employment rates constant. An example is the contraction of manufacturing in favor of services requiring computer skills. Our Net Reallocation measure captures this source under the identifying assumption that the net redistribution of employment across job types is demand-driven. This assumption implies, for example, that the recent massive creation of skilled jobs originated from an exogenous increase in the demand for computer skills rather than from one in the supply of skilled workers.

2. Unemployment and its effect on job search. Unemployment may have a direct effect on occupational Churning, as it does for job flows, possibly because idiosyncratic employment risk is exacerbated by recessions. In addition, changes in the optimal job search policies of individual workers may originate from "environmental" shifts, such as macroeconomic and labor market policy, or aggregate cyclical factors which assist job search, most notably labor market tightness. In recessions, a worker may become less choosy and more willing to accept a job in a different occupation (Moscarini 2001), or less willing to spend effort to search on-the-job for, and quit to, better employment conditions (Barlevy 2002).

3. Employment composition effects. Our Gross Reallocation measure is based on the pool of employed workers who report valid occupational codes in two consecutive years. One of our objectives is to estimate the effects of compositional changes of the employed, in terms of worker characteristics, on Gross Reallocation. For example, if age and education reduce individual occupational mobility, then the observed aging and the increasing educational attainment levels of the US labor force may explain the decline in reallocation. A similar issue pertains to possible changes over time in worker unobservable characteristics, such as the quality of their education. Although these are harder to detect, they need to be accounted for in estimation.

4. Dynamic effects of job-matching. Job-matching theory (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.[4] Similarly, learning-by-doing on the job reduces the incentives to

[4] A similar mechanism is emphasized by Hall (1995) as a source of persistence of inflows into unemployment

job-to-job mobility over time (Pissarides 1994). These effects suggest that a dynamic aspect should be incorporated in any model of occupational mobility.

Theories of worker turnover typically adopt a partial equilibrium approach and are built upon state variables characterizing the *ex-post* heterogeneity of the employment relationship. Tenure is taken as a measure of accumulated knowledge of either match quality or learning-by-doing. The microeconomic empirical literature on worker turnover generally adopts a similar approach (Miller 1984, Farber 1994), and gives less emphasis to the *ex-ante* heterogeneity of worker characteristics, skills and preferences, that determine their mobility choices. That is, this literature tends to avoid the issue of sorting on unobservables, obviously because this type of individual characteristics, by definition, is unobserved. The macroeconomic empirical literature on labor market flows conditions on (if any) observable worker and firm characteristics, which are almost always endogenous and make it difficult to interpret the results.[5] Our analogous investigation in Moscarini and Vella (2000) employs the NLSY79 panel which allowed us to control directly for unobserved individual heterogeneity. The evidence there suggested that the unobservable individual effects appeared to be an important determinant of reallocation. This encourages us to control for unobservables at the higher degree of data aggregation we employ here.

We specify a statistical model of occupational mobility that encompasses the four classes of factors mentioned above. We do not attach ourselves strictly to any single model of occupational mobility, but endeavour to evaluate the relative importance of these various factors. We assume the unobserved heterogeneity underlying the endogeneity of worker characteristics is birth-cohort specific and construct a pseudo panel which allows us to control for these cohort level effects. Hence, we address the issue of unobserved heterogeneity through a cohort-based approach, and accommodate both *ex-post* and *ex-ante* sources of worker heterogeneity relevant to occupational turnover.

We also consider the effect of aggregate business cycle conditions on reallocation, noting that we treat these factors as exogenous to gross occupational reallocation. That is, while we attempt to account for the potential simultaneity of individual characteristics, such as education and marital status, we have no corresponding strategy for the environmental variables such as unemployment and Net Reallocation. These latter effects on employment reallocation have remained relatively unexplored and warrant further investigation. Note, however, that Net Reallocation is exogenous under our maintained assumption that the net redistribution of employment is caused entirely by occupation-specific labor demand

and of the unemployment rate itself.

[5]Davis *et alii* (1996) provide a sobering discussion of this issue with regards to firm size and its correlation with job creation.

processes. At any rate, it is useful to characterize the correlation between reallocation and business cycle indicators after conditioning on the other variables.

We find that the four classes of factors affect total reallocation as predicted by the respective theories. Among individual characteristics, College education, age, and some family commitments negatively influence occupational mobility. The effect of College education weakens considerably and even reverses in high unemployment periods. Unemployment has a residual large negative coherence with mobility, contradicting the presumption of higher idiosyncratic uncertainty in recessions. Net Reallocation is positively associated with total reallocation, so Churning appears an inevitable by-product of net employment redistribution. Worker mobility also has an estimated positive serial correlation, unexplained by the impact of persistent macroeconomic variables, which is consistent with the persistence of turnover innovations predicted by job-matching theory. Finally, we uncover a substantial decrease in the tendency of later birth cohorts' members to change careers. This tendency appears to accelerate for individuals born in the mid 1950's.

In Section 2 we present our empirical models of mobility and discuss our strategy for dealing with unobserved heterogeneity. In Section 3 we present the data and Section 4 contains a discussion of the results.

2. An Empirical Model of Occupational Mobility

To estimate a model of occupational mobility that explains the aggregate patterns of employment reallocation that we documented, one would ideally employ a representative panel of individuals over the relevant period of time. This would allow the investigation, and ability to control, for a range of individual characteristics, both observed and unobserved, in addition to the estimation of dynamics and time effects. As no such data is available we employ the repeated cross-sections of the CPS. To motivate our use of a pseudo panel, illustrate our goals and the potential selection problem, we first introduce an empirical model of mobility at the individual level.

2.1. 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

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

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; δ_t is

unknown parameter vectors; and $\varepsilon_{i,t}$ denotes some zero mean error term. The objective is to estimate the unknown parameters noting that they may vary over time. The latent measure of mobility is not observed and we conduct our empirical work with the observed measure

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

where

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

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 *following* the decision to move. Under the assumption of rational expectations, this decision is based on information available at time $t-1$.[6]

A key issue is the treatment of joblessness, which may be considered an "occupation" in itself. In this case we need to assess the size and membership of this "home production" or "search" occupation. We may either try to identify those who decide voluntarily not to participate in employment, or we can assume that all unemployment and non-participation are voluntary and treat them as home production. We are interested only in those changes that imply a movement to a different occupation, where presumably the skills of the individual are employed by *an observationally different technology.* Therefore we must restrict attention to formal employment only, because GDP measures only the output of this part of the economy, and we must exclude a "jobless" occupation. This 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 automatically create a large inflow in recessions and a burst of "reallocation", which would hide the cyclical changes in the labor force composition and in individual behavior that we are interested in.

The mobility variable can thus only be observed for the subsample that report that they were employed both in the interview period and in the previous year. Thus consider the following model

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

where

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

[6]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.

and λ_t is another unknown parameter vector. Next

$$\text{MOB1}_{i,t} = \text{BEMP}_{i,t} \cdot \text{MOB}_{i,t} \tag{2.1}$$

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 across individuals i. As our data will show later, there appears to be selection into employment on the basis of observable worker characteristics, which suggests that the same might be true for unobservable worker characteristics − such as time and risk preference, or quality of education − absorbed by the equation errors.

Consider the economic and econometric implications of incorporating this additional selection process. First, by failing to account for the process by which individuals are employed in consecutive periods, when estimating the mobility equations, we 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 both strategies below. However, note that 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 $\text{BEMP}_{i,t}$ variable but does not directly affect the mobility decision.[7] 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 $\nu_{i,t}$. We address this in the following sections.

2.2. Birth-Cohort Effects

Consider an extension of the above model which allows for the possibility that the effect of the conditioning variables varies not only over time but also across birth cohorts. Allowing variation by cohorts seems sensible as one would expect that individuals making human capital investments and subsequent labor market decisions at approximately the same time

[7] As we implied above, the model will also be identified if we make, and exploit, strong assumptions about the nature of the unobservables in the model, even in the absence of exclusion restrictions.

would be influenced by similar factors. We extend our statistical model of individual mobility to capture cohort effects.

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

$$\text{MOB}^*_{i,c,t} = \mathbb{I}\left\{x'_{i,c,t-1}\theta_{c,t} + \varepsilon_{i,c,t} > 0\right\},\ t = 1, 2..T; c = 1, 2..C_t; i = 1, 2...N_{c,t} \tag{2.2}$$

where the unknown parameters $\theta_{c,t}$ depend on time and the cohort c.

This yields a set of $\Sigma_{t=1}^{T} C_t$ (one for each year-cohort pair) estimates for each determinant of mobility. This formulation of the model captures generational differences in mobility behavior, but does not control for the potential selection problem discussed in the previous section. To do so would again require that we estimate an equation for $\text{BEMP}_{i,c,t}$ and control for the selection bias. Once again, this would require an exclusion restriction, as the cohort variation in the mobility equation is also assumed to appear in the employment equation. We now take a different approach to address this problem.

2.3. 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

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

where

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

is the average mobility of members of the cohort employed both last and this period. Similarly for $\bar{x}^{\text{BEMP}}_{c,t-1}$, $\bar{\varepsilon}_{c,t}$. Next

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

is the employment rate of the entire working age sample of cohort c at that time t. Similarly for $\bar{x}'_{c,t-1}$, $\overline{\nu}_{c,t}$. Finally, V_{t-1} is a vector of economy- or labor market-wide ("environmental") 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 $\bar{\varepsilon}_{c,t}$, $\overline{\nu}_{c,t}$ are allowed to be correlated across cohorts. Without loss in generality we can define each of the two random variables $\bar{\varepsilon}_{c,t}$, $\overline{\nu}_{c,t}$ to be the sum of a common component and an orthogonal component, both random variables

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

with $cov\left(e_{c,t}, n_{c,t}\right) = cov\left(\lambda_{c,t}, n_{c,t}\right) = cov\left(e_{c,t}, \lambda_{c,t}\right) = 0$ and $\mathbb{V}\left(\lambda_{c,t}\right) = cov\left(\bar{\varepsilon}_{c,t}, \overline{\nu}_{c,t}\right)$.

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)

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

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

$$\overline{\text{MOB}}_{c,t} = V_{t-1}'\vartheta + \bar{x}_{c,t-1}^{\text{BEMP}\prime}\beta + CD_c'\gamma + \bar{e}_t \tag{2.3}$$

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

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. It should be noted that 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. Fig.3 appears to provide empirical support to this assumption. Historically, the proportion of College graduates rises over time, and in fact 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. All these trends appear far from being linearly synchronized.

More generally, 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 Attanasio and Davis 1996 for an application of the same idea to consumption), and any residual effect differentiating cohorts should then be captured by the fixed effect $\bar{\lambda}_c$. For example, if a cohort is more risk-averse than average, because it lived through the Great Depression, or if it experiences a particularly poor quality of education, the cohort dummy should capture directly such heterogeneity.

2.4. 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 of 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 "environmental" variables, such as labor market tightness, which impact on reallocation are typically very persistent. Hence, we use the aggregate unemployment rate and Net Reallocation to control for those disturbances, in the vector V_{t-1}.[8]

The model we estimate

$$\overline{\mathrm{MOB}}_{c,t} = \rho \overline{\mathrm{MOB}}_{c,t-1} + V'_{t-1}\vartheta + \bar{x}^{\mathrm{BEMP}\prime}_{c,t-1}\beta + CD'_c\gamma + \bar{e}_t \tag{2.4}$$

is based on the approach of Verbeek and Vella (1998) in which the static model (2.3) 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

[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.

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. We highlight that the inclusion of the lagged dependent variable is not a trivial extension. The explanatory variables are highly correlated over time, so the estimation of a static model, when the true model is dynamic, will lead to biased estimates of the slope parameters.

3. Data

Our dataset includes 30 yearly cross-sections, from 1971 to 2000, of the US population contained in the March Files of the Current Population Survey. In spite of the increasing availability of longitudinal data of long duration on workers, with or without matching information about employers, we consider this type of dataset to be the most appropriate for our investigations, for two reasons. First, our focus is macroeconomic, hence we require a representative sample collected in a consistent manner over an extended period of time. The CPS is carefully designed to uniquely achieve just that, and it 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 same age individuals 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, while we need a long continuous time series for our microdata-based macroeconomic analysis. We restrict attention to annual observations mostly to avoid dealing with the formidable seasonality in worker mobility, but also to exploit the wealth of information on individuals 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.

Sample. Due to our emphasis on time series patterns, the choice of the explanatory variables is constrained by their availability for the entire period in a uniform format, or, at least, some uniform recoding must 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 leads us to focus on the 1971-2000 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 (_POPSTAT= 1) of working age (16 to 64, included) who are not in school or at home full time ($0 \leq$ _ESR≤ 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 those cases with the variable FL-665 (recoded as SUPREC by Unicon). On that occasion the data were released both in the old (March 1988) and new format (March1988b). A comparison between the two reveals that we need to discard, in 1988b, 1989 and thereafter, all individuals with SUPREC$\neq 1$ to maintain consistency of definitions through the 1971-2000 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 ($1 \leq$ _CLASS≤ 3) 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 3-digit occupation for last week (OCC) and 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: $\text{MOB}_{i,t} = \mathbb{I}\{\text{OCC}_{i,t} \neq \text{OCCLYR}_{i,t}\}$. Notice that $\text{OCC}_{i,t-1}$ and $\text{OCCLYR}_{i,t}$ are not the same, as they would be in a panel, because most sampled individuals change across years.

Given our focus on occupations, we believe that 3-digit is the most meaningful level of disaggregation to define reallocation. However, as there exist an average of 453 such occupational categories, each containing an average 0.22% of employment, it is imperative to have a large sample size, in our case about 32,000 per year on average. 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. 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.

[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 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 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.

Sources of Measurement Error. 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 might find 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. Finally, an interviewer might return to an address for a follow-up interview and find it under construction or vacant. In that case the address is not permanently dropped from the sample (unless it has become infeasible for residential purposes, which happens in a tiny fraction of the cases) and new attempts to find some new household there are made in subsequent months. 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. We are aware that occupational codes are also subject to considerable measurement error, but a similar correction appears infeasible. 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 1971-1993. 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 (Fig.2), and it is unlikely that without the 1994 CPS reform the

reported female reallocation would have been much higher.

Occupational codes at the 3-digit level are not available before 1968, and the Census coding of 3-digit occupations has changed three times in 1971, 1983, 1992, along with each decennial Census, from the initial 1968 system. Every year t, last week's and last year's occupations ($\text{OCC}_{i,t}$ and $\text{OCCLYR}_{i,t}$) are imputed according to the same coding 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 1971-2000. Within these 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.

Measurement of education in the CPS raises some issues. In 1971-1991 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. After several experiments, we found that the only reliable measure of education that we can consider consistent through the two subperiods (hence through 1971-2000) 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). Consistency through the periods is tested by observing the fractions of the active population who fall into each category over time. Any finer classification (for example dividing High School graduates and those who also had some College into two separate categories) leads to a jump of the times series of these fractions between 1991 and 1992, suggesting an inconsistent change of classification.

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 $x_{i,t-1}$ (say, marital status) last year as required by the model and rational expectations, but rather $x_{i,t}$. Therefore we replace $x_{i,t-1}$ with their values one period forward, 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.

4. Regression Specifications and Results

To explain the variation in Gross Reallocation we employ a number of variables capturing the characteristics of the male individuals in our sample, and estimate our models of occupational mobility by OLS. The time series for each of these explanatory variables is presented in Fig.3. An examination of 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 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 Fig.3 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.

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 or income information, which might be useful to distinguish between voluntary and involuntary changes of occupation, because this distinction is not too 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. Our regressions are exclusively meant to control for composition effects in the labor force.

4.1. The Individual Mobility Model: Repeated Cross-Sections

We first estimate the individual mobility model separately for each cross-section. This naturally prevents us from using any aggregate covariate V_{t-1}, which has no cross-sectional variation in each year. We employ a constant term, a $4^{th}-$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. The sample size is approximately between 25,000 and 41,000 for each year. The time series of the estimated coefficients with associated 1% confidence bands are presented in Fig.4. Note that the age effect is computed by increasing age by one year starting from the predicted mobility for the "average" individual, where all regressors are replaced by their sample means. We did not compute and report the confidence bands for the age effects, because the estimates on all four powers of age are extremely precise and statistically significant at virtually any conventional level.

As expected, age and marital status have a strong negative effect on occupational mobility. Ethnicity appears to have no discernible effect in that both the white and African-American dummies seem to be close to zero for the whole period. Veterans tend to change occupation more often, which is partly unexpected because veterans typically enjoy special privileges in the access to some types of jobs. In fact, when we recall that the average value of gross mobility for the last decade of our sample is approximately 7 percent, the marginal effect from being a veteran of around 1.5 percent is high.

The most interesting results relate to the education coefficients. Education, both at the High School and College level, reduces occupational mobility. Early in the sample we see that having a College degree has a particularly strong negative effect on the mobility decision. However, both effects increase towards zero throughout the sample period and we see that from about 1995 and onwards there is no statistically significant effect of education.

It is important to question why we observe such substantial variation in the partial effects of these conditioning variables over this time period. The first possibility is that the relationship between mobility and these variables has simply changed. Second, the composition of the sample may have changed over time in an unobservable manner, so as to modify the nature of the relationship between mobility and the conditioning variables. For example, it is possible that the type of human capital varies by birth cohort and this is reflected in different responses as the composition of the sample changes with the introduction of the new cohorts. Finally, it is possible that the partial effects of these variables are sensitive to the business conditions of the economy. As the economy has changed over the period of our investigation this may be reflected in time varying partial effects.

Given this particular role for the business cycle it is useful to consider the role of macro

economic variables on this time series of cross sectional estimates. As the model is estimated separately for each cross section it is not possible to include any macro variables directly. Thus it is useful to see how the time series of coefficients appears to be related to the aggregate state of the economy. The estimated effects generally tend to vanish in absolute value in the negative phases of the business cycle (1975-1976, 1980-1983, 1991-1992). This suggests that in depressed labor markets individual characteristics matter less, and observationally different workers tend to behave more similarly. This type of behavior is consistent with the model of Moscarini (2001) which predicts that during economic slumps, workers are primarily concerned with finding or keeping a job. Accordingly, they are less selective in their job search (from employment or unemployment) and their individual characteristics become less important in predicting their job changes.

4.2. The Individual Mobility Model: Birth-Cohort Effects

To investigate the possibility that the time variation is generated by differences in the behavior of different birth cohorts we now estimate the individual model with cohort effects (2.2). To estimate age effects we need age variation within each birth cohort. This leads us to define 10-year cohorts.[10] We continue to use the individual record data but we estimate the model separately for each 10-year cohort in each time period, enabling us to obtain cohort specific estimated parameters for each variable in each period. We then regress these estimated coefficients, for each of the variables, on cohort and time dummies.

Of particular interest are the birth cohort effects and accordingly we report only their estimates in Fig.5. We omit the age estimates, although the first-step regression includes a cubic in age, because we observe different 10-year cohorts at quite different stages of their life-cycle. An examination of Fig.5 reveals a number of remarkable trends. The education effects reflect an increasingly strong negative impact for each of the later cohorts until there is a slight increase for the 1970 cohort. Recalling that the average level of mobility for the sample is approximately 8 percent, both the education variables appear to provide a large proportion of the total effect. Both High School and College education have been associated with a large decrease in mobility. The College effect is particularly well estimated. The large change in the education coefficients is striking and we return to a discussion of what they may reflect below. The other notable movements over time are associated with the veteran and marital status variables. The most recent birth cohorts of veterans are far more mobile, while marital status appears to have a strong negative effect for all cohorts except the earliest and the last. Note that in the case of the veteran status the variation across

[10]The specification is similar to that in the previous section except that since age can only adopt a value in a 10 year range we employ a cubic in age rather than a quartic.

cohorts is large and an upward trend is apparent. In contrast, the marital status coefficient is relatively stable with the exception of the first and last cohorts.

The large changes in the estimated individuals coefficients reflect some differences across cohorts. One explanation is that the observable characteristics of the cohorts have changed over time. To investigate this possibility we repeated the above second-step regressions but rather than use cohort dummies we employed the average characteristics of the cohort. That is, we used as regressors the cohort average values of the explanatory variables. Although the results of this exercise are not reported here there seemed to be no relationship between the mean values of the cohort characteristics and the estimated cohort specific coefficients. This seems to suggest that the differences are due to unobservable characteristics of the cohort. Alternatively, the qualitative nature of the cohort's variables have changed. For example, perhaps the nature of education has changed across cohorts. We address this below.

4.3. Birth-Cohort Synthetic Panel

We now estimate the synthetic cohort model (2.3). This is the core of our econometric investigation of the time series pattern of aggregate occupational reallocation that we documented in Fig.1. We use the same specification as for the individual cross-sections (Section 4.1), and we augment it in several alternative directions, made possible by the richness of panel structure. We also explore directly the role of macroeconomic variables on the average level of cohort occupational mobility. Since all cohorts are pooled together and we do not need within-cohort age heterogeneity to control directly for aging, 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 1907 (who were 64 and about to retire at the beginning of the sample in 1971) 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 1909-1980, so each cohort is observed at least three times in the sample and at least once after teen-age years. Given the unbalanced nature of our pseudo panel we have 1385 observations in total. Birth cohort dummies are included from 1910 to 1980. Table 1 presents the results from the employed specifications, labelled I through X. Estimates that are statistically significant at the 1% level are in boldface.

I. The first specification, reported in column I, replicates that used for the repeated cross section data (Section 4.1). This specification includes no cohort effects and does not allow a role for dynamics and aggregate covariates. Although many estimated coefficients are similar in sign and magnitude to those reported in Fig.4, there are some puzzling outcomes. Most notable is the seemingly unreasonable large coefficients associated with the race variables.

Also unexpected is the positive and statistically significant effect of marital status on mobility. It is difficult to determine what is exactly the cause of such results, but they do suggest some form of misspecification.

II. In column II we augment this specification with a lagged dependent variable. Note that the sample was "trimmed" before the estimation of the first specification − excluding for each cohort the initial observation which is missing the first lagged mobility − to ensure that the samples are the same for both columns I and II. A number of results are worth noting. First, the lagged mobility coefficient is reasonably large in magnitude and very precisely estimated. This is a very important result as it provides empirical evidence that any shock to the economy, which affects occupational mobility, will take several periods before its full effect is realized. This does not imply that the recently unemployed will continue to search for work for multiple periods, but rather that individuals who changed occupations in one period will continue to do so in subsequent periods.[11] While the presence of such a dynamic effect might be expected, this appears to be the first evidence which substantiates such a result, in addition to exploring its magnitude. Our evidence complements that of McCall (1990), who finds that maintaining the same occupation upon a change of employer significantly raises the average tenure on the new job. Before we focus on the magnitude we attempt to identify the specification in which we have the most confidence. Note at this point, however, that the positive serial correlation is supportive of the job-matching theory. Second, there is a large reduction in the race related coefficients. While they still appear large they are far less unreasonable. Third, there is a notable reduction in the effect of College education.

III. While the estimates in column II seem reasonable, they are inconsistent in the presence of cohort fixed effects. Accordingly, Column III augments the dynamic specification of column II with the cohort dummies, as suggested by Verbeek and Vella (1998), to capture these effects. The evidence suggests that cohort dummies are capturing factors that are important determinants of mobility. As this is an important finding we delay our discussion of the magnitude, and interpretation, of these effects to a more appropriate point of the paper. The estimates for the other controls are reported in Table 1. Once again there are a number of interesting findings. First, the presence of the cohort effects has some impact on the point estimate of the lagged dependent variable. However, the evidence continues to suggest that the occupational mobility decision has a dynamic component. Second, the inclusion of the cohort dummies appears to explain away any direct role of education on

[11] It is possible that some of these movements may be originating from the unemployed pool, in that individuals may be experiencing unemployment while changing occupations.

occupational mobility. The other demographics have effects of plausible signs and are often precisely estimated, with the exception of an unexpected positive estimate for marital status. This suggests that the striking patterns revealed in Fig.5 capture the changing nature of the cohort. Note that one could not easily disentangle a pure cohort effect from a cohort effect which operated purely through educational attainment. However, we attempt to shed some light on this possibility below.

IV. It is natural to exploit information on the source occupation of movers and stayers. However, occupational choice is clearly endogenous to mobility, so including it without any attempt to control 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 the endogeneity of the occupational distribution. That is, one can consider the initial occupational distribution of each cohort as partially reflecting unobserved cohort specific heterogeneity. However, this is precisely the form of heterogeneity which is captured through the cohort dummies. As any subsequent occupational distribution of the cohort is highly dependent on the initial allocation, one might argue that our approach is a good first step towards accounting for its endogeneity.

Therefore, in column IV of Table 1 we include among our regressors 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. Ideally, we would like to regress mobility on the shares of all (but one) of the 3-digit occupations that we used to construct our dependent variable. However, this would entail estimating over 400 extra parameters, and the sample size in each occupation would be less than 100 individuals on average with many near-empty cells. Aggregating to one-digit occupational groups resolves both problems, at the cost of losing some information.

The results in column IV suggest that workers employed in Technical and Sale occupations are more likely, and workers in Farming and Fishing-related careers are much less likely, than others to switch to a different 3-digit occupation, within or outside their 1-digit group. These results probably reflect a combination of factors such as regional considerations and human capital requirements. At the same time, the effects of the other demographics do not change from the previous specification III, and the effect of education is not precisely estimated.

V-VII. We now investigate the impact of macroeconomic factors. Ideally we would enter time dummies to capture the business cycle effects. However, we cannot identify time effects through time dummies due to the presence of the age and birth cohort effects. Accordingly, we proxy for the business cycle with the aggregate civilian unemployment rate, the yearly average of monthly unemployment rates published by the Bureau of Labor Statistics. The average over the entire period is 6.4%. In columns V through VII we enter, separately, and then simultaneously, Net Reallocation and unemployment rate.[12] As discussed in the Introduction, we acknowledge that we have no effective strategy for dealing with the potential endogeneity of unemployment. However, rather than abandon the investigation altogether we examine if there is any role noting that the results should be treated somewhat tentatively.

Net Reallocation has a positive and strong effect on its Gross counterpart, as suggested by the Schumpeterian tradition. The magnitude of the estimated effect hovers consistently near 3.75. Under our maintained assumption that Net Reallocation reflects entirely exogenous occupation-specific demand shocks, a 1% increase in net redistribution of employment from shrinking to expanding occupations implies about 3.75% more total reallocation, with Churning an unavoidable and dominating by-product.

As expected from the trends in the unconditional series, unemployment has a strong negative effect on mobility. This is 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. In columns V and VII we see that the coefficient is not only large in magnitude but is also very precisely estimated. Even bearing in mind the possible endogeneity of this variable it is surprising that, with the exception of age and lagged mobility, the other variables all appear to have no relationship with mobility. This is consistent with the initial findings that many of the partial effects seemed to be cyclical. Indeed, Net Reallocation and unemployment are meant to capture two different kinds of aggregate shocks, occupation-specific and aggregate respectively. Note, when both of these macro variables are entered in the same specification (VII), marital status exhibits a sizable expected negative and precisely estimated effect.

The interpretation of the estimates for the two macro variables is slightly complicated by the fact that unemployment may be caused by net employment reallocation, as argued by Lilien (1982)'s sectorial shift hypothesis. Although this conjecture has not survived subsequent scrutiny, some of the effect of Net Reallocation might be working through induced

[12]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 Net Reallocation, but its effect is statistically insignificant, most likely due to sampling error.

additional unemployment.

The existing literature has focused more on employment reallocation across industries over business cycles, 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). 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 flows that we find.

Although both Net Reallocation and unemployment are quite persistent, only the former absorbs some of the observed serial correlation in Gross Reallocation, and yet only half of it, while unemployment has no effects on mobility dynamics. Therefore, a quite significant persistence in reallocation remains, and we continue to take this as strong evidence of occupational matching.

VIII-X. The evidence thus far suggests that education appears to have no consistent direct effect on occupational mobility. In columns VIII through X we look for possible education effects operating through interactions with unemployment. As illustrated in the Introduction, labor market tightness may alter workers' choosiness in jobsearch. We consider specifications with or without Net Reallocation − the results are similar, except that as before the persistence of mobility is reduced by Net Reallocation. The demographic variables generally have statistically insignificant effects except for the negative influence operating through the head variable. We note that head and married are highly correlated, so it is difficult to separate their respective contributions. The most striking result is that now College education has a strong effect on mobility. Both the negative direct effect and the one operating through the interaction term are large, fairly stable across specifications, and are highly statistically significant.

Given that the variables enter in this interactive manner it is important to evaluate the derivative respect to education, which will vary depending on the level of the unemployment rate. We report the total effects in Fig.7. Indeed, the average effect of College education is moderately positive, especially in recessions, and turns negative at cyclical peaks. This seems to suggest that a College degree provides relatively more specialization and comparative (as well as absolute) advantages in some careers that are exploited when jobs are abundant, while it makes it easier for the worker to switch out of troubled occupations in bad times. Overall, our findings are strongly suggestive that sorting is more pronounced in expansions.

In a specification that we do not report here we interact the cohort dummies with educational attainment. Since the proportion of College graduates is quite low for the very early cohorts (for historical reasons) and in the very late ones (for age reasons), we focus

on cohorts born from 1915 to 1975 for the cohort-College interaction. This specification absorbs all the effect of education itself, and drastically reduces the coefficient on the lagged dependent variable. However, the variation of education for each cohort can of course occur only over time, and thus originate only from sampling error. In fact this specification seems to suffer from severe multicollinearity, so we do not put much weight on its implications.

Discussion. A number of the results from Table 1 are of interest. First, there is a very strong lagged effect indicating the operation of dynamics in the mobility decision. The point estimates from our preferred specifications, adhering to the theories that inspire our work, appear to be in the order of 0.07 after controlling for reallocative shocks through Net Reallocation, and is generally quite precisely estimated. 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 $t+2$, seven of these individuals will change occupation again. 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. Again we highlight that these are job changers and not individuals who are transiting to jobs from the unemployment pool. Recall that the persistence in total reallocation is substantially stronger if Net Reallocation is excluded from the regression.

Second, Net Reallocation appears to exert an almost four fold impact on its gross 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 3-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 3% of Gross Reallocation, a very large figure given that Gross Reallocation was around 8% on average. To better understand the nature of this episode, we also considered Net Reallocation at the 1 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 3-digit occupations, this 0.65% burst of additional Net Reallocation implies per se an extra 2.5% of Gross Reallocation, a very large figure given that Gross Reallocation at the 1-digit level was below 5% in that period.

Third, while there initially appeared to be a strong educational effect, this is explained

partially away by inclusion of the cohort dummies. This may suggest that the two are closely related. In fact, rather than conclude that the education effects are eliminated via the inclusion of the cohort dummies it appears that the cohort dummies appear to be capturing some features of the educational investments of the respective cohorts. Unemployment masks the effect of College education, which is close to zero on average, after conditioning on the cohort effects, but very cyclical.

Fourth, while we accept that we have no strategy for controlling for the endogeneity of the macro effects, it appears that occupational reallocation is very strongly negatively associated to the unemployment rate, independently of its impact operating through education.

Fifth, while the evidence is not overwhelming, and varies across specification, there appears to be some role for background demographic variables such as being head of household. However, the effects are not precisely estimated. The age effect is consistently negative as expected, stable across specifications and very precisely estimated.

The final result from Table 1 worth remarking upon is the existence of the cohort effects. In Table 1 we can only see that the coefficients on some of the remaining variables are sensitive to the inclusion of the cohort dummies. We now explore the pattern of the estimated cohort dummy coefficients. Given the large number of estimates we report them by plotting them as a time series. These are reported in Fig.8, with their 1% confidence bands, for each specification III-X where the dummies were included.

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, the estimates of the cohort effects are typically negative and declining over time, suggesting that later cohorts have an unexplained and statistically significantly lower propensity to change occupation. 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, the specification which includes occupational distribution as a control displays a less drastic decline for the 1920 and beyond cohorts. This indicates that later cohorts are more relatively frequently choosing occupations that feature below-average exit rates. Third, the cohorts born in the early 1970's are more mobile than their immediate predecessors and successors. This effect cannot be due to age differences, because we do control for aging in a quite flexible manner, and because younger cohorts born after 1975 should be expected to change career even *more* often. At this stage we can offer no explanation of this result. Finally, the specifications with the cohort-education interaction effects (whose results we do not report) reveal no direct role for education or cohort effects. This again suggests that the cohort effects are in some way operating through education.

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.

Our main findings concerning cohort effects are, in most of the specifications, 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, with the exception of the early 1970's discussed earlier. 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 large increase in the 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. A growing literature claims that "job instability" has recently risen in the US (see, for example, Jaeger and Huff-Stevens 1999), lending some support to this second hypothesis.

5. Conclusion

We investigate the evolution and the sources of aggregate employment reallocation in the United States in the 1971-2000 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.

We find that the total reallocation of employment across occupations has been strongly procyclical and sharply declining 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 weakens the effects of individual worker characteristics on their occupational mobility. 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 unexplained occupational mobility, which contributes 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 the period that we focus on. A natural direction of future research is to uncover the nature of such heterogeneity.

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Figure .1: REALLOCATION OF MALE WORKERS ACROSS 3-DIGIT OCCUPATIONS.

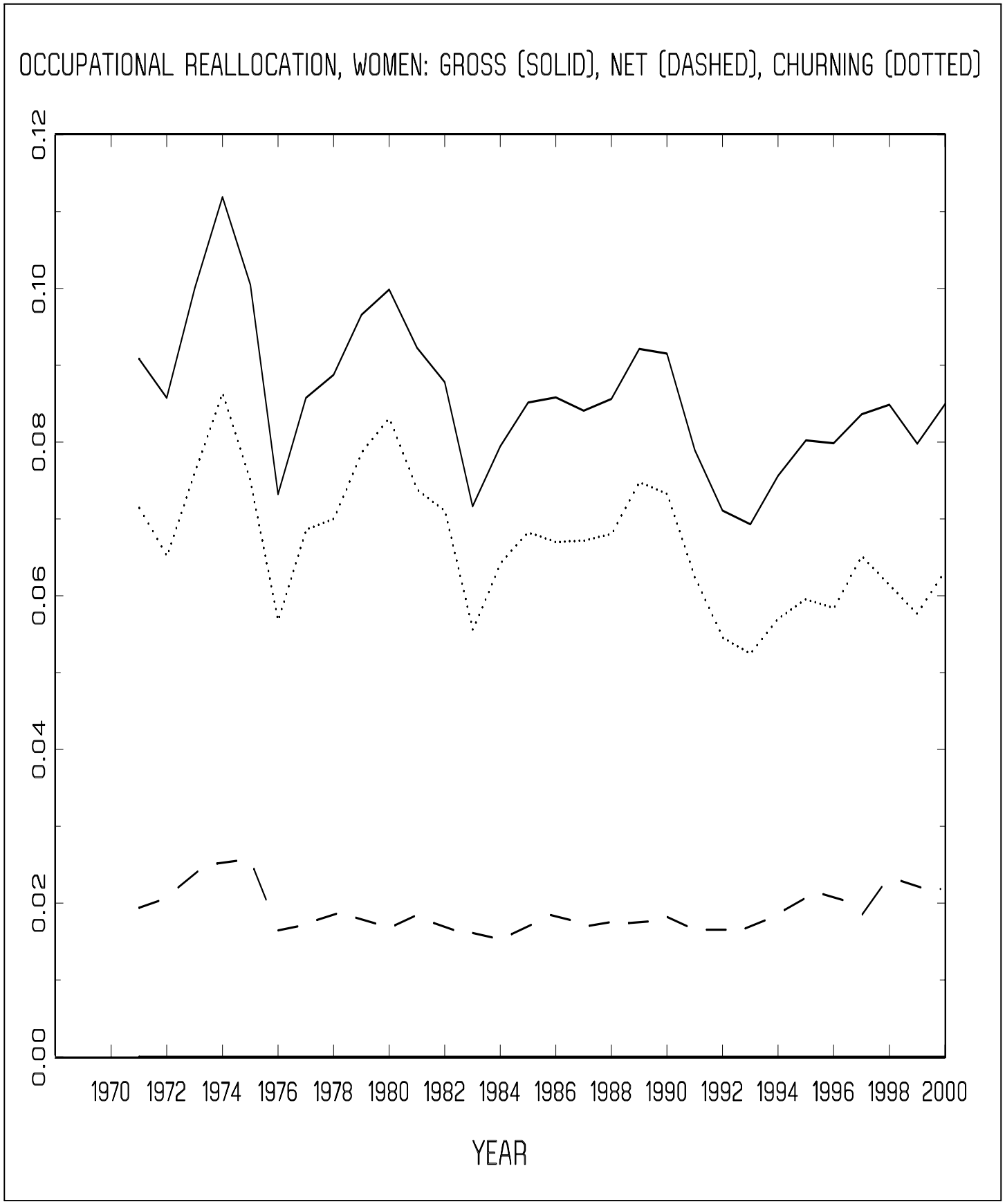

Figure .2: REALLOCATION OF FEMALE WORKERS ACROSS 3-DIGIT OCCUPATIONS.

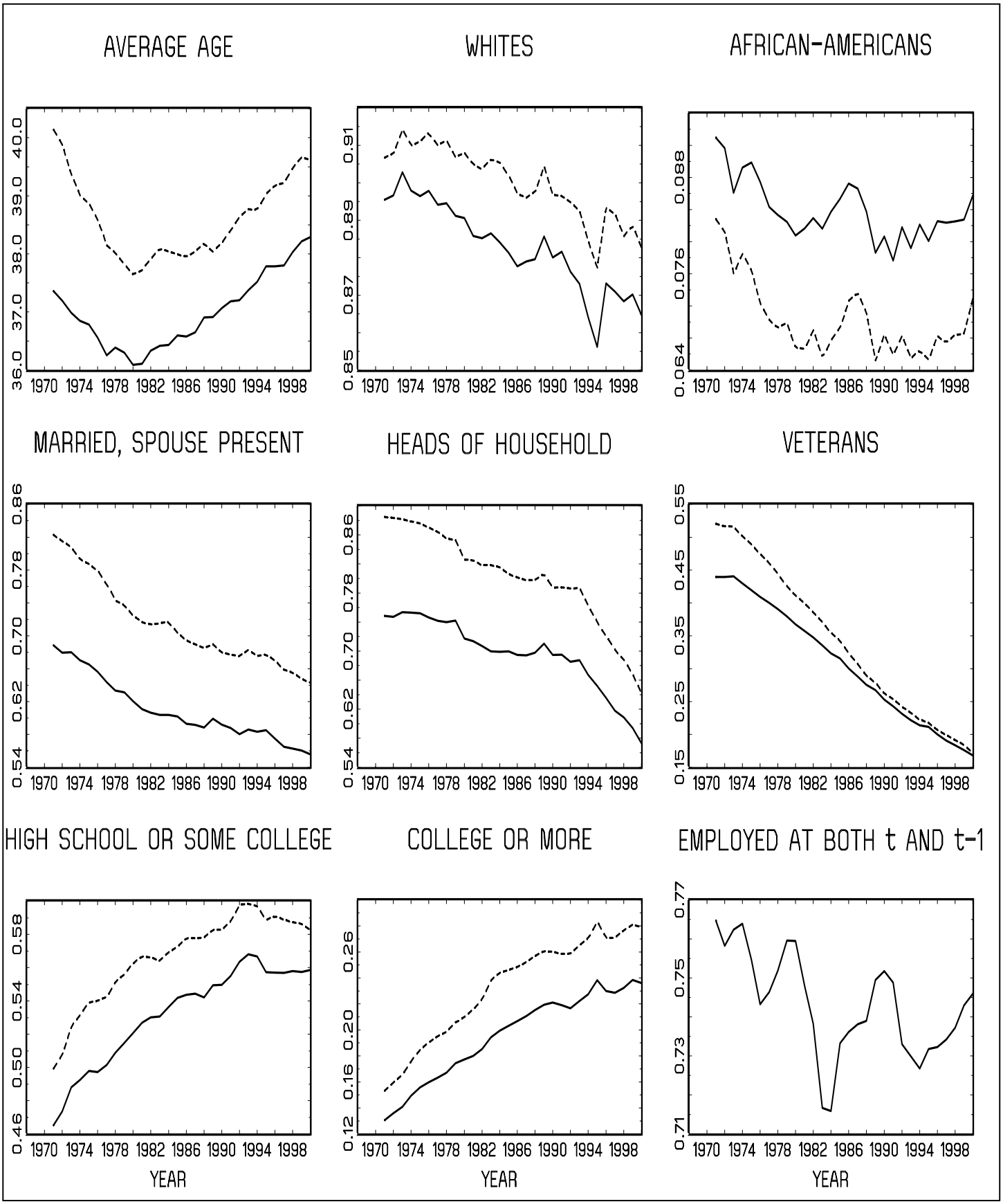

Figure .3: Sample characteristics, men: labor force (solid) and employed in years t and $t-1$ (dashed).

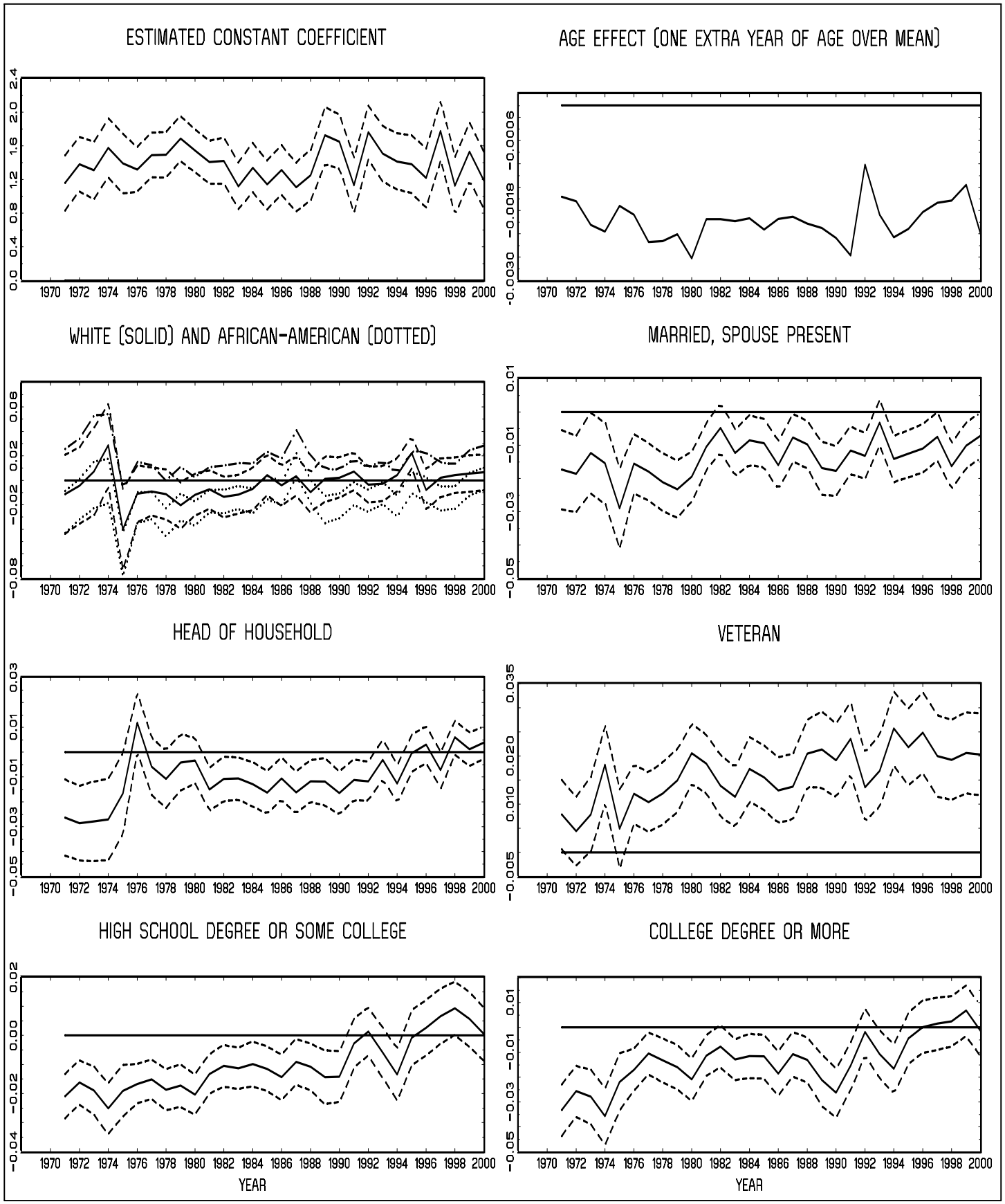

Figure .4: Estimated effects on occupational mobility and 1% confidence bands. Repeated cross-sections.

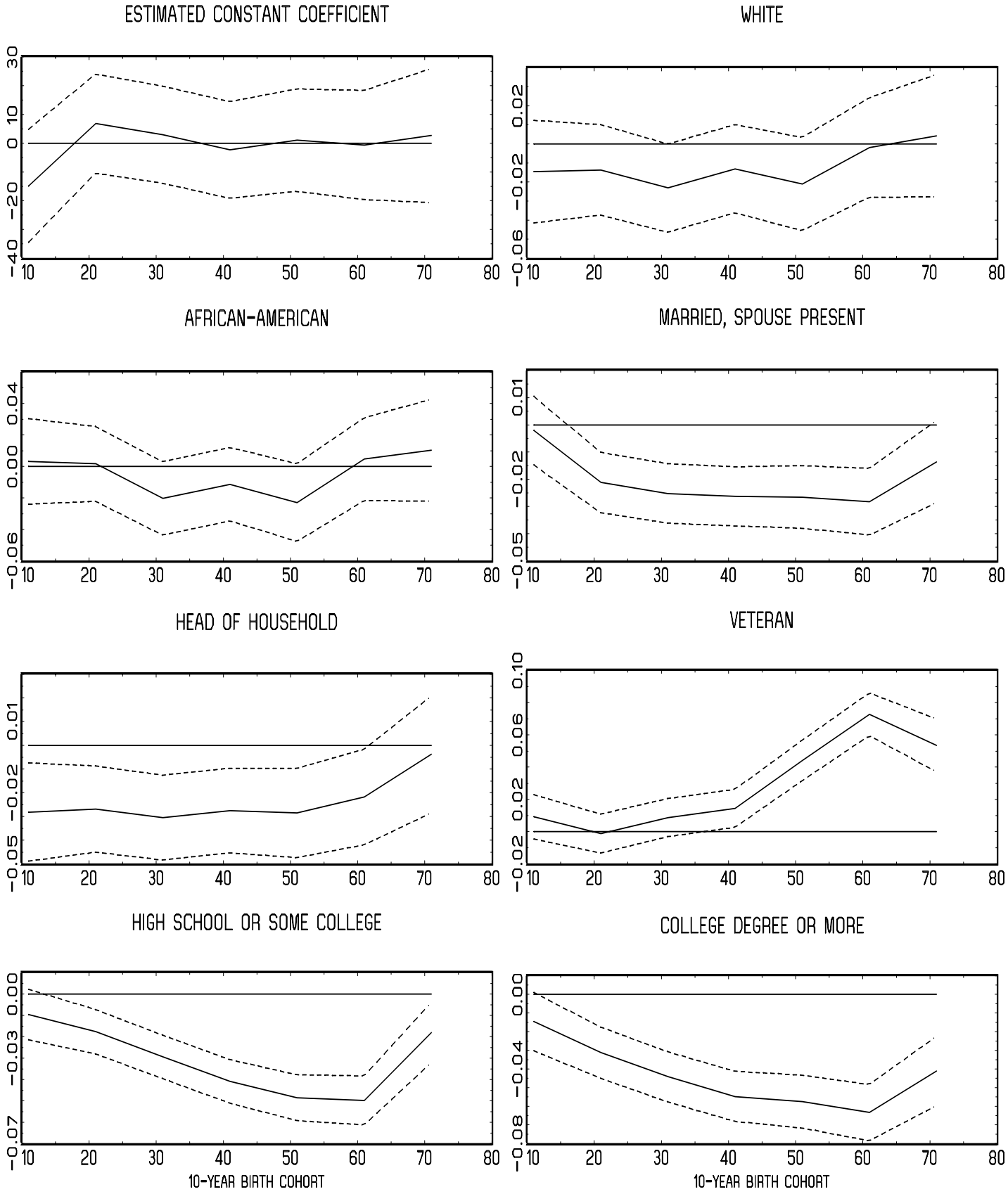

Figure .5: Estimated interaction of birth cohort with individual effects on mobility, and 1% confidence bands.

SPECIFICATION	I	II	III	IV	V	VI	VII	VIII	IX	X
CONSTANT	**1.3463**	**0.9736**	**1.0546**	**1.2266**	**1.1925**	**1.3244**	**1.3922**	**1.3292**	**1.3965**	**1.4700**
	0.1071	0.1081	0.1486	0.1603	0.1335	0.1463	0.1326	0.1441	0.1427	0.1279
AGE	**-0.1193**	**-0.0855**	**-0.0828**	**-0.1016**	**-0.0917**	**-0.1001**	**-0.1045**	**-0.1043**	**-0.1046**	**-0.1095**
	0.0086	0.0088	0.0142	0.0146	0.0128	0.0139	0.0125	0.0137	0.0135	0.0121
AGE^2*10E-1	**0.0369**	**0.0266**	**0.0255**	**0.0328**	**0.0286**	**0.0314**	**0.0330**	**0.0324**	**0.0326**	**0.0344**
	0.0031	0.0031	0.0048	0.0050	0.0043	0.0047	0.0042	0.0046	0.0046	0.0041
AGE^3*10E-3	**-0.0512**	**-0.0372**	**-0.0352**	**-0.0473**	**-0.0401**	**-0.0441**	**-0.0467**	**-0.0450**	**-0.0455**	**-0.0482**
	0.0049	0.0049	0.0071	0.0074	0.0064	0.0069	0.0063	0.0068	0.0067	0.0060
AGE^4*10E-5	**0.0265**	**0.0193**	**0.0181**	**0.0254**	**0.0209**	**0.0231**	**0.0246**	**0.0233**	**0.0236**	**0.0252**
	0.0028	0.0028	0.0039	0.0041	0.0035	0.0038	0.0034	0.0037	0.0037	0.0033
WHITE	**0.1501**	0.0980	0.0452	0.0457	-0.0456	-0.0166	-0.0876	0.0063	0.0177	-0.0528
	0.0569	0.0548	0.0636	0.0626	0.0573	0.0618	0.0562	0.0611	0.0604	0.0542
AFRICAN-AMER.	**0.3134**	**0.2060**	**0.1722**	0.1183	-0.0038	0.0587	-0.0803	0.0769	0.0815	-0.0593
	0.0663	0.0643	0.0744	0.0737	0.0675	0.0729	0.0665	0.0720	0.0710	0.0641
VETERAN	-0.0101	-0.0070	0.0298	0.0315	0.0159	0.0168	0.0067	0.0302	0.0362	0.0272
	0.0054	0.0052	0.0280	0.0276	0.0251	0.0271	0.0245	0.0268	0.0265	0.0237
HEAD OF HOUS.	-0.0257	-0.0234	**-0.0628**	**-0.0520**	-0.0292	-0.0100	0.0095	**-0.0364**	**-0.0463**	-0.0288
	0.0148	0.0142	0.0178	0.0187	0.0161	0.0181	0.0164	0.0183	0.0181	0.0163
MARRIED	**0.0632**	**0.0479**	**0.0430**	**0.0452**	-0.0142	-0.0020	**-0.0453**	0.0244	0.0312	-0.0108
	0.0168	0.0162	0.0213	0.0213	0.0194	0.0211	0.0193	0.0211	0.0209	0.0189
HIGH SCHOOL +	0.0037	-0.0046	0.0070	-0.0238	0.0054	0.0100	0.0078	**0.1269**	0.0064	0.0083
	0.0072	0.0069	0.0102	0.0124	0.0092	0.0099	0.0090	0.0154	0.0254	0.0227
COLLEGE +	**-0.0536**	**-0.0368**	-0.0040	-0.0211	-0.0177	0.0047	-0.0101	**-0.1409**	**-0.2306**	**-0.2612**
	0.0120	0.0116	0.0277	0.0342	0.0248	0.0267	0.0242	0.0353	0.0380	0.0341
MOBILITY[t-1]		**0.2691**	**0.2040**	**0.1642**	**0.0869**	**0.1843**	**0.0792**	**0.1730**	**0.1750**	**0.0667**
		0.0244	0.0257	0.0259	0.0240	0.0250	0.0234	0.0247	0.0244	0.0226
OCCUP. SHARES:										
MANAGERS-PROF.				-0.0294						
				0.0373						
TECHNICAL-SALES				**0.0894**						
				0.0359						
SERVICE				0.0263						
				0.0413						
FARMING-FISHING				**-0.2140**						
				0.0463						
LABORERS				0.0424						
				0.0326						
NET REALLOC.					**3.8801**		**3.6320**			**3.7089**
					0.1090		0.1074			0.1035
UNEMPL. RATE						**-0.4238**	**-0.3275**		**-1.3336**	**-1.2601**
						0.0439	0.0402		0.2250	0.2016
UNEMPL. x HS								**-1.6875**	0.1417	0.0847
								0.1671	0.3500	0.3135
UNEMPL. x COL								**2.5122**	**3.8965**	**4.1573**
								0.3967	0.4560	0.4087
COHORT DUMMIES			x	x	x	x	x	x	x	x

Table 1. Regression results, birth cohort synthetic panel. Standard errors are in small font.

Figure .6:

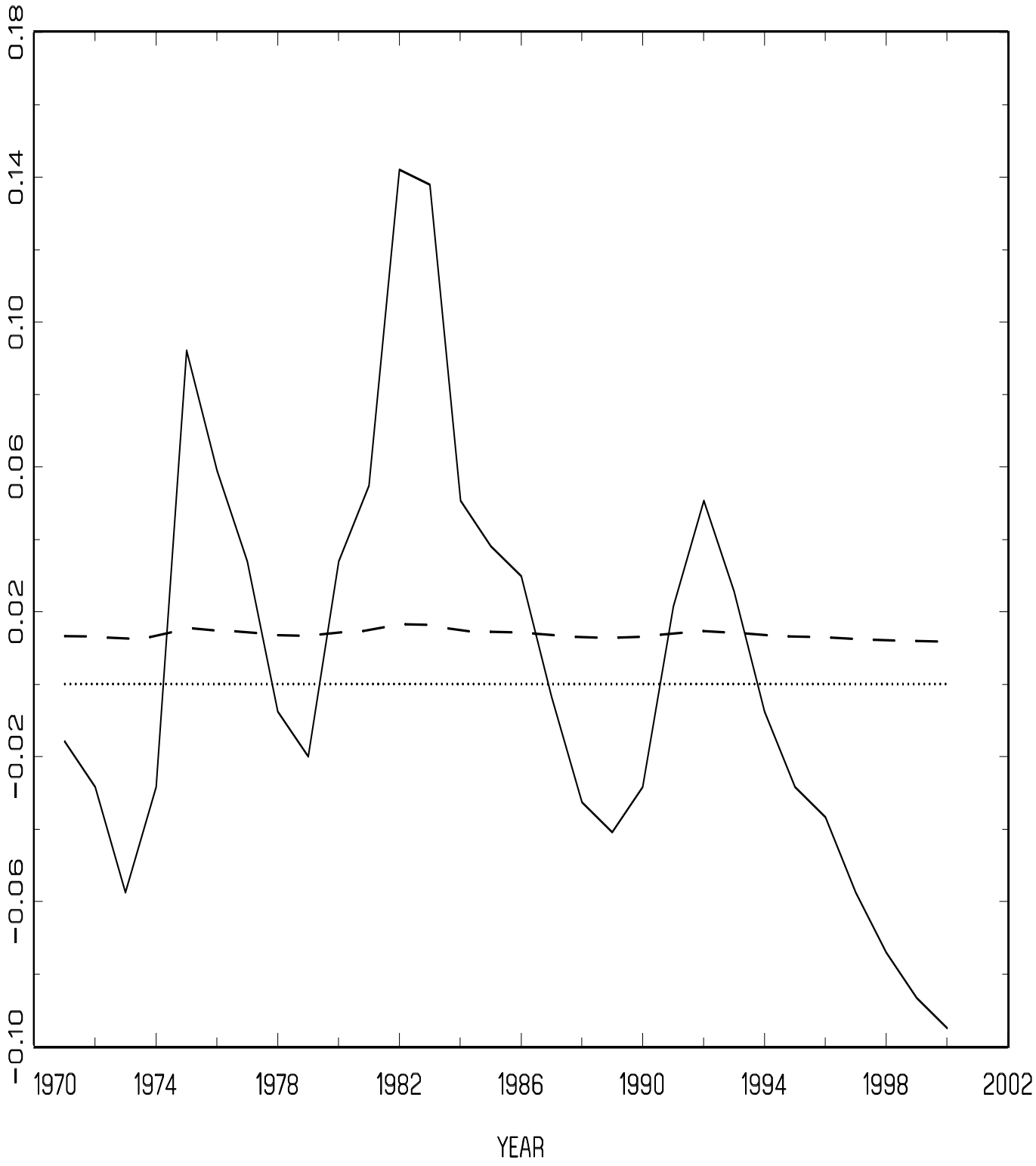

Figure .7: Total effect of education, including interaction with unemployment (specification X)

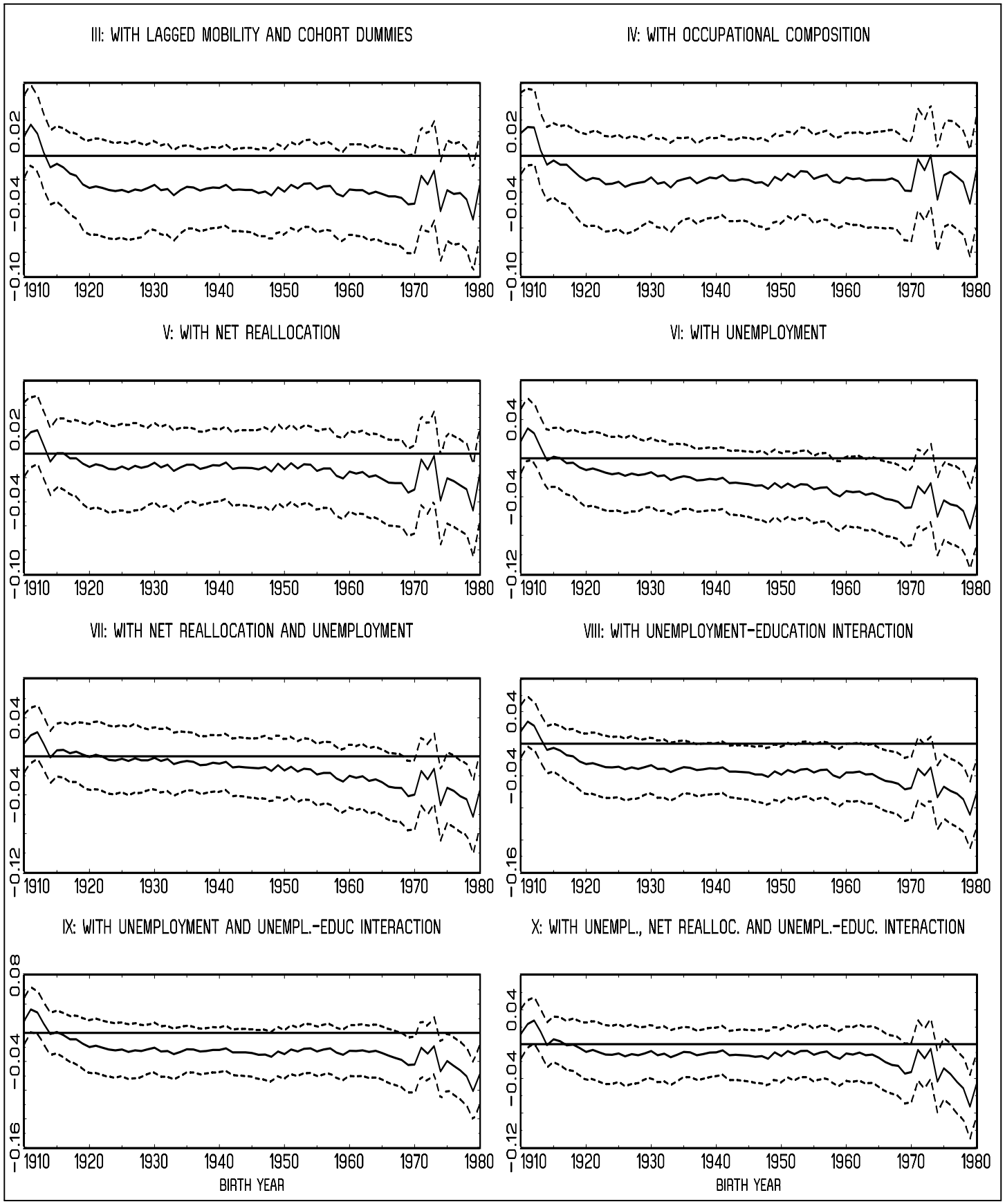

Figure .8: Cohort dummy estimates and 1% confidence bands, synthetic panel.