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EMPLOYMENT DURATION AND INDUSTRIAL LABOR MOBILITY

IN THE UNITED STATES, 1880-1980*

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Abstract

Recent studies of job tenure raise the question of the appropriate duration statistic to use in historical research. This article compares duration measures and examines their empirical and theoretical implications for historical research on employment tenure. Using a variety of data from the late 19th and early 20th centuries, the authors find that there existed a sector of stable jobs but that most industrial jobs were brief. Since World War I, however, there has been a sharp shift in the relative size and importance of the short- and long-term job sectors.

Economists and statisticians have developed sophisticated techniques for estimating the duration of unemployment, employment, and other phenomena. These methods should be useful to economic historians, especially those interested in the historical development of labor markets. The new approach to duration modelling can incorporate data from incomplete (censored) spells and permits more precise comparisons of past and present. With appropriate data, one can study changes over time in the prevalence of long-term unemployment (and other issues related to labor market efficiency) or in the prevalence of long-term jobs (and other issues related to internal labor markets). This article focuses on the latter and asks how, if at all, has the distribution of employment tenure in the United States changed since the late 19th century?

To answer that question, we discuss different measures of job duration and their relevance to historical research. Next we explore the modelling of job duration and the assumptions that sometimes must be made when using historical data. Those assumptions can be rather restrictive or implausible, a point we illustrate by a critical examination of recent historical studies of job duration. Finally, we present a variety of evidence -- some of it based on the new approach to duration modelling -- showing that most industrial jobs at the turn of the century were less stable than in more recent years.

I. Duration Statistics

The standard published measure of job duration -- taken from cross-sectional surveys -- is the mean or median duration of job spells currently in progress (so-called right-censored spells, because their termination date is not observed). In a steady state, the average duration of spells in progress is roughly one-half their completed duration. This latter statistic

-- the average completed duration of spells in progress -- is analogous to the average life span of those alive at a point in time when ages are surveyed. Because it weights spells by their contribution to total duration, one may call the statistic experience-weighted duration, or S_{EW} .¹

But there is a different statistic that weights equally the duration of all spells, including those not currently in progress. This statistic -- termination-weighted duration or S_{TW} -- measures the average length of all spells that terminate during a given period. It is analogous to the average age at death of a population dying during some period (say, a year); in a steady state it is equivalent to life expectancy at birth. S_{EW} is larger than S_{TW} because longer spells are more likely to be in progress at any point in time. Akerlof and Main, from whom our terminology is derived, point out that "long-lived persons are more apt to be seen in any given census; thus the person who dies at eighty is visited by eight decennial censuses; the child who dies at ten is seen by only one decennial census." Akerlof and Main estimate that males in manufacturing jobs in 1973 had an S_{EW} of 18 years. But they also find that S_{TW} for the same group was about 4.5 years. Others have found differences of similar magnitude.²

Which is the appropriate duration statistic? Each has its vices and virtues. S_{EW} suffers from length-biased sampling. It oversamples spells that are long and undersamples spells of short duration, many of which ended prior to the survey. While some who held these latter jobs are re-employed

¹The intuition for this is simple. Under steady state or stable conditions, if a point is picked at random to observe spells in progress, a captured spell is truncated with uniform probability over its length. Hence, on average, an observed spell is half its completed length and, for a large population (by the law of large numbers), S_{EW} is twice the mean of observed (censored) spells. See Kaitz, "Analyzing the Length of Spells."

²Akerlof and Main, "Measure of Durations," p. 1004. Also see Abraham and Farber, "Job Duration, Seniority, and Earnings," p. 284.

by the time of the survey, others remain unemployed or exit the labor force. That is, S_{EW} does not adequately incorporate information on short-duration jobholders.³

S_{TW} has the virtue of including information on the work experiences of all workers whose spells terminate during a given period (interval) of time, not only those currently employed. The problem with S_{TW} , however, is that it contains repeated observations of workers who have held more than one job during the analysis period.

Yet when it comes to comparing duration distributions in different historical periods, S_{EW} potentially is an inferior statistic. If there has been a major change in the distribution's lower-tail, S_{TW} would pick it up whereas S_{EW} would understate it. Demography provides a useful analogy. If a country has a major change in its infant mortality rate, time-series statistics on average life expectancy at birth will reflect this change, whereas statistics on average age will not fully register it. In Mexico, for example, male infant mortality declined by half between 1921 and 1960. While the average age of the male population in 1921 and 1960 stayed the same (about 22 years), male life expectancy at birth rose from 34 years in 1921 to 45 years in 1960.⁴

³This is not a trivial issue. It has been argued that even in a quasi-panel dataset like the CPS (Current Population Survey), with its 4-week point-in-time sampling scheme, there may be substantial undercounting of short spells. Kiefer, Lundberg, and Neumann, "How Long is a Spell?" p. 126. Also see Clark and Summers, "Labor Market Dynamics," pp. 24-33.

⁴Arriaga, New Life Tables for Latin American Populations, pp. 196-206. Stable life tables illustrate the same phenomenon. Take a population with a gross reproduction rate of two, in which the infant mortality rate drops from 306 per thousand births to 32 per thousand. The population's average age will decline slightly (from 34 to 28); average age at death (roughly equivalent to S_{EW}) will increase 85% (from 34 to 63), while life expectancy at birth (S_{TW}) will increase nearly 300% (from 20 to 78). Coale and Demeny, Regional Model Life Tables, pp. 656, 679, 728, 774.

S_{EW} and S_{TW} are likely to differ across different demographic, occupational, and industrial subgroups. Recent advances in econometric duration analysis (and the availability of panel or longitudinal data) allow one to model the entire duration distribution and assess the impact of different covariates on the distribution.⁵ Both in terms of statistical estimation and the interpretation of coefficients, it is generally convenient to specify models in terms of the hazard function. In the continuous case, the hazard function is the conditional density at time t , given survival until time t ; in the discrete case, it is the conditional probability of failure at time t , given survival to at least time t . The distribution function, density function, and hazard function are mathematically equivalent in the sense that specifying any one of them implies specifying the other two.⁶

An advantage of these econometric duration methods is that information contained in censored observations can easily be incorporated in the estimation procedure. However, as the proportion of duration spells that are censored increases, the parameter estimates of any model are less precisely estimated because of the loss of information due to censoring.

The absence of information from completed spells can be a particular problem for historical researchers, who often have available to them only data comprised entirely of censored spells (e.g., from a point-in-time survey). Consider a sample of employment durations in which all observations are censored. For simplicity and ease of exposition we abstract from covariates, the argument being true more generally. Let t_i , $i = 1, 2, \dots, N$ be the censored observations for the i^{th} individual. Suppose we fit a two-

⁵See Kiefer, "Duration Data and Hazard Functions," pp. 646-79.

⁶See, for example, Cox and Oakes, Survival Data, pp. 14-15.

parameter Weibull model (with parameters α, λ).⁷ The likelihood function for the data is

$$L_1 = \prod_{i=1}^N \exp[-\lambda t_i^\alpha]$$

The maximization of the above likelihood yields the corner solution $\lambda = 0$. This makes intuitive sense. When all observations are censored the maximum likelihood estimator makes the mean as large as possible. Further, the duration dependence parameter α is not identified. (The duration dependence parameter captures the effect of time on the hazard function. For the Weibull model, $0 \leq \alpha < 1$ implies negative duration dependence; $\alpha > 1$ implies positive dependence; and $\alpha = 1$ gives the exponential case with no duration dependence.)

If one has employment data consisting entirely of spells in progress at a point in time, longer spells are more likely to be sampled than shorter ones (i.e., we have length-biased sampling).⁸ To evaluate the contribution of the observed censored spells under such a sampling scheme, we need to make the steady state assumption of constant entry. The combination of length-biased sampling and the steady state assumption is what identifies α

⁷The hazard function of a distribution $F(t)$ with density $f(t)$ is $h(t) = f(t)/[1-F(t)]$. The density and distribution functions can be written in terms of the hazard function as

$$\begin{aligned} f(t) &= h(t) \exp[-\int_0^t H(u)du] \\ F(t) &= 1 - \exp[-\int_0^t h(u)du] \end{aligned}$$

When we have covariates the hazard conditional on covariates is commonly modelled using the proportional hazard framework

$$h(t, X, \alpha, \beta) = h(t, \alpha) \cdot \phi(X, \beta)$$

where X is a row-vector of regressors and β the associated column-vector of coefficients. A natural choice for $\phi(X, \beta)$ [which is widely used] is $\exp(X\beta)$, because besides being reasonably flexible, it guarantees non-negativity of the hazard without adding to the complexity of computation.

⁸Salant, "Search Theory and Duration Data," pp. 39-57; Frank, "How Long is a Spell of Unemployment?", pp. 285-302.

and gives an interior solution for λ . Keep in mind, however, that the steady state assumption is a very serious simplification. The likelihood for the data, then, is,

$$L_2 = \sum_{i=1}^N \left[\frac{\exp(-\lambda t_i^\alpha)}{\lambda \frac{1}{\alpha} \Gamma(1+\frac{1}{\alpha})} \right]$$

Note that the only difference between likelihoods L_1 and L_2 is the denominator, which represents the "correction" for length-biased sampling under the steady state assumption. The estimated parameters should be interpreted as conditional on the assumptions that the probability of getting a job is independent of calendar time and that the composition of individuals becoming employed is stable over time.⁹ It is clear that these assumptions are not tenable when a labor market is in flux.

When estimating duration models, another problem that can arise concerns the regressors. These are included in hazard specifications to control for heterogeneity among individuals who, because of different characteristics and circumstances, have different duration distributions. In historical research, it may happen that key variables affecting duration are not included among the regressors, perhaps because data are inadequate or unavailable. But this will give biased estimates of both the duration dependence parameter and the coefficients of the included regressors. The

⁹Nickell makes this point in his analysis of unemployed spells, noting that these assumptions constitute a serious simplification: "... Thus, we are assuming that throughout the relevant period, either changes in labor market conditions are of minor importance in affecting changes in an individual's conditional probability of obtaining a job at any particular time, or such changes do not corrupt the results in relation to the variables we are considering." Nickell, "Estimating the Probability of Leaving Unemployment," p. 1255. See also Frank, "How Long is a Spell of Unemployment?", pp. 285-302.

estimate of the duration dependence parameter is generally biased downwards. The effect on the coefficients of the included covariates is more complicated. The estimates are biased and inconsistent but a general result on the direction of the bias is not known. Further, so-called neglected heterogeneity induces dependence between the included covariates and the duration, so that the proportional hazard assumption, if assumed, is violated because of misspecification.¹⁰

Neglected heterogeneity can be handled by specifying a baseline hazard, a model for the effect of covariates, and a "mixing" distribution for the heterogeneity.¹¹ Recent research indicates that specification of the mixing distribution is less important than an appropriate choice for the baseline hazard, which is crucial. Parameter estimates are more sensitive to the latter.¹²

Cox's partial likelihood approach can be used to make inferences about β (the covariates' coefficient vector) under the proportional hazard assumption without specifying the form of the baseline hazard function.¹³ Although the information contained in censored observations is used, the key to this approach is utilizing the information conveyed by the ordering or ranking of the completed observed durations. Unfortunately, when data consist only of censored spells, one cannot take advantage of this semi-

¹⁰Lancaster, Econometric Analysis of Transition Data, Ch. 4; Sharma, "Specification Diagnostics for Econometric Models of Durations."

¹¹Heckman and Singer, "Minimizing the Impact of Distributional Assumptions," pp. 271-320.

¹²Manton, Stallard, and Vaupel, "Heterogeneity of Mortality Risks Among the Aged," pp. 635-44; Ridder, "Sensitivity of Duration Models to Misspecified Heterogeneity".

¹³See Cox and Oakes, Survival Data, Chs. 7 and 8, for an exposition of the partial-likelihood approach.

parametric method to estimate the impact of the regressors.

Finally, it should be pointed out that one must be clear as to what exactly the duration variable measures -- job duration or employment duration. If the variable of interest is employment duration, but the data do not distinguish between job and employment durations, then there is an error-in-variables problem. Even in large samples, this is liable to bias the estimates of the regressor coefficients.¹⁴ In 1983, the Bureau of Labor Statistics changed its duration measure from the former to the latter. Because workers can change jobs without changing employers, the new measure is a more accurate gauge of employment patterns (and gives larger duration figures). Unfortunately, historical surveys of employment duration do not usually distinguish between these two measures, as for example, in the 1892 survey discussed below.¹⁵

II. Historical Studies

In a recent article, Susan Carter and Elizabeth Savoca estimate job duration using historical data from an 1892 survey of San Francisco workers. They find average completed job tenure (S_{EW}) in 1892 was 8.5 years for non-union males in San Francisco and 13 years for the nonfarm United States. The latter is almost three-fourths as large as Akerlof and Main's S_{EW} estimate of 18 years for the 1970s, a degree of similarity, say Carter and Savocca, "one would hardly expect". They conclude that at the turn of the century as today, "most employment was concentrated in lengthy spells". While Carter and Savoca admit that there were "far fewer lifetime jobs" in the 19th century, they nevertheless think their research "calls into ques-

¹⁴Lancaster, Econometric Analysis of Transition Data, p. 61.

¹⁵Jacoby and Mitchell, "Sticky Stories," pp. 33-37.

tion the 'spot market' characterization of labor markets in that era."¹⁶

Carter and Savoca's study is an innovative attempt to apply modern duration modelling techniques to historical data. It is careful and precise. But for the very reason that the study is important and likely to influence future research, we think it necessary to point out some of its shortcomings. Based on our preceding discussion, we question both the interpretation and reliability of their findings.

As should be clear by now, use of an S_{EW} measure by Carter and Savocca masks changes in duration patterns over time (especially for short-term jobs) and undersamples spells of short duration. The first problem recalls our discussion of a decline in infant mortality, which an S_{EW} statistic may not detect. The second problem is an echo of the 1970s debate over unemployment -- whether most jobless spells are frictional and brief or whether most unemployment is spent in lengthy spells. As it turned out, each side was right. Differing policy purposes were served by emphasizing either the majority of spells found in the lower tail or the fact that most time spent in unemployment was concentrated in upper-tail spells. Yet both phenomena need to be acknowledged whether one is concerned with unemployment in the 1990s or job tenure in the 1890s.¹⁷

Another difficulty with Carter and Savocca arises from their data being comprised entirely of interrupted spells. As in the case of the Weibull model discussed above, identification in their model is achieved through a combination of length-biased sampling and the steady state assumption. Hence their results -- estimates of regressor coefficients and the finding

¹⁶Carter and Savoca, "Labor Mobility and Lengthy Jobs," pp. 1-16. Also see Carter and Savoca, "Learning and Earning in Late 19th Century America".

¹⁷Akerlof, "Case Against Conservative Macroeconomics," pp. 219-37.

that $\alpha = 1$, i.e., no duration dependence -- may simply be an artifact of this combination. Moreover their empirical analysis is crucially dependent on the steady state conditions that the probability of getting a job is independent of calendar time and that the composition of individuals becoming employed is stable over time.

These conditions are not valid in the case of the San Francisco labor market in 1892. When the San Francisco survey was conducted in 1892, the United States economy was still in an expansion phase, as Carter and Savoca note. San Francisco, however, already was depressed. As early as March 1892, the Coast Seamen's Journal was reporting that "not for over twenty-five years has San Francisco witnessed such destitution, misery, and suffering." Another source reports that "substantial unemployment plagued San Francisco in 1892." The depression no doubt boosted job duration (especially of spells in progress) in San Francisco in 1892, as short-term workers lost their jobs and remaining workers were loath to quit. Thus, the shift into depression casts serious doubt on the steady-state assumption underlying Carter and Savoca's estimation procedure.¹⁸

Third, Carter and Savoca have potential problems of neglected heterogeneity due to the omission from their study of some key variables, including occupational status and skill. Previous historical studies of mobility have focused on blue-collar workers, the group whose employment patterns are thought to have changed the most during the past century. Over time, blue-collar jobs came to have some of the stability that previously

¹⁸ Cross, History of Labor in California, p. 217; Knight, Industrial Relations in San Francisco, p. 31. Demography again provides a way of seeing the relationship between S_{EW} (a population's average life span) and employment growth (reproductive rates). At a given mortality level (job separation rate), the reproduction rate inversely determines average life span. For numerical examples, see Coale and Demeny, Regional Model of Life Tables, passim.

had been the hallmark of white-collar work (although Table 1 shows that occupational status differences still remain). Yet Carter and Savoca lump together blue- and white-collar wage earners; their sample includes manual workers as well as clerks, foremen, managers, salesmen, and pharmacists.¹⁹

Their failure to take account of skill is bothersome. It was a primary determinant of job tenure at the turn of the century, when skilled industrial workers had lower rates of geographic and labor mobility than operatives and laborers. Indeed, in earlier work on the San Francisco survey, Carter found that 93% of the long-term male jobholders (over 20 years' tenure) were skilled workers. Although Carter and Savoca include schooling in their model, education at best is an imperfect proxy for occupational skill, especially in the late 19th century. Skilled blue-collar workers are over-represented in the 1892 San Francisco sample, both union and non-union. (This explains why one-third of the male workers surveyed in 1892 were union members, which is roughly three times greater than San Francisco's unionization rate at the time.)²⁰

¹⁹ Carter and Savoca also lump together blue- and white-collar workers (and urban and rural workers) in their Table 1, which compares tenure of jobs in progress -- from which S_{EW} is derived -- in San Francisco versus other 19th century areas. On blue- versus white-collar work, see Kocka, White Collar Workers, and Jacoby, "Progressive Discipline," pp. 213-60. As with personnel practices, unemployment rates for blue- and white-collar workers sharply differed at the turn of the century; white-collar workers were much less likely to experience joblessness. But the historical evidence on geographic mobility and occupational status is ambiguous, with some studies (e.g., Thernstrom) finding higher rates of mobility for manual workers and others (e.g., Guest) finding the opposite. Keyssar, Out of Work, pp. 54-58; Thernstrom, The Other Bostonians, p. 230; Guest, "Notes From the National Panel Study," pp. 63-77. Also see Decker, White Collar Mobility in 19th Century San Francisco.

²⁰ Carter, "Lifetime Jobs," pp. 291-92. On skill and mobility, see Thernstrom, Other Bostonians, p. 230 and Slichter, Turnover, pp. 57-74. The San Francisco survey is described in California Bureau of Labor Statistics, Fifth Biennial Report. Another variable that would have been useful to include is establishment size (in employees) by industry. Both in the past and today, large firms have lower turnover rates, perhaps because of higher

These sources of neglected heterogeneity are cause for concern. It is entirely possible that the true underlying duration dependence (in their Weibull model) is positive (i.e., $\alpha > 1$) but because of uncorrected heterogeneity Carter and Savoca's downward-biased estimate of α is close to one.²¹ Further, given the strong restrictions needed for identification of relevant parameters and the omission of key covariates, we feel that the Carter-Savoca results are unreliable and at best should be taken as tentative.

Finally, it is doubtful that Carter and Savoca's results are representative of the San Francisco or nonfarm U.S. labor markets of the early 1890s. This issue always arises when research is based on fragmentary, local, data. It is particularly a problem for historians, since often these are the only kind of data available to them. The 1892 San Francisco survey is fraught with "sampling bias". During the late 19th century, San Francisco had a very large number of Asians (mostly Chinese males) in its population. In 1890, Chinese workers made up 17% of the city's male labor force (or about 21% of its nonunion workforce). Because of severe discrimination, the Chinese were often relegated to jobs of low pay and status; presumably these jobs were unlikely to offer career opportunities. Yet the researchers who collected the San Francisco data interviewed

pay (efficiency wages?) or better management. Capital-labor ratios, which Carter and Savocca include, do not fully capture the size effect. Brissenden and Frankel, Turnover in Industry, pp. 54-55; Osterman, "Turnover Rates".

²¹In a job matching model in which a worker learns about his tasks and the firm environment, we would expect that initially the hazard rate of leaving will increase and then eventually start decreasing as the worker and the employer realize that the match is a good one. The hazard function would have an inverted-U shape. Depending on the shape and curvature of the U-shaped hazard function, the impact of covariates, and the distribution of covariates in the sample, a monotonic hazard imposed on the data could yield increasing, decreasing, or constant duration dependence.

"whites" only; not a single Chinese man was surveyed! This is hardly surprising, given racist attitudes in California in the 1880s and 1890s. Yet it does not inspire confidence in Carter and Savoca's results to discover that their sample nonrandomly omits one of five male workers. To say the least, there is a sample selection problem that leads to undercounting of short employment durations.²²

Carter and Savoca do, however, recognize that San Francisco differed from other American cities and attempt to correct for this. They derive an estimate of completed spells in the United States by taking 1890 nonfarm U.S. means for three variables (homeownership, dependents, and marital status) and "plugging" them into their San Francisco model. Even if one accepts the reliability of their model and of this procedure (which we do not), the resulting all-United States duration estimate is unreliable because of failure to control for other important differences between San Francisco and the nonfarm United States at this time.

Duration-related peculiarities of San Francisco include its relatively sluggish employment growth during the late 19th century. In the 1880s the increase in the number of San Francisco's industrial wage-earners lagged slightly behind the United States (47 versus 45 percent). But during the 1890s, industrial employment rose by only 0.5% in San Francisco versus 25% nationwide. As we have seen, even the timing of the 1893 depression was different in San Francisco.²³

Other idiosyncracies include the maturity of San Francisco's male labor force, which in 1890 had proportionately more men between the ages of 25 and

²² Eleventh Census: 1890, vol. 1, pt. 2, pp. 728-29; Twelfth Census: 1900, vol. 1, pt. 1, pp. 531, 565; Saxton, Indispensable Enemy, pp. 7, 210.

²³ Twelfth Census: 1900 - Manufactures, pt. 1, p. 3 and pt. 2, pp. 1002-03.

64 than the nation as a whole.²⁴ Second, San Francisco's unionization rate in 1890 (12.4%) was more than double that of the nonfarm United States, which fits with the city's reputation as a "labor town". While Carter and Savoca properly exclude union members from their sample, they are still left with a proportionately smaller nonunion workforce than would be found in most other cities at this time.²⁵ Third, San Francisco's industrial base was comprised of relatively small artisanal shops (14 wage-earners per manufacturing establishment in 1890), very different from a city like Chicago, which had 30 workers per establishment. (The average in 1890 for 164 U.S. cities was 22 workers.)²⁶ Because it was a port city, San Francisco in 1900 had only 25% of its male labor force employed in manufacturing, less than in any major American city except Washington, D.C. and New Orleans. Labor market dynamics would be affected by the smaller number of alternative employment opportunities available to San Francisco's nonunion industrial workers.²⁷

Finally, San Francisco had an unusual ethnic structure. During the 1880s and 1890s it remained relatively untouched by the "new" immigration wave that originated in Southern and Eastern Europe. In 1900, 71% of the

²⁴Men between the ages of 25 and 64 accounted for 74% of San Francisco's labor force versus 68% of the entire U.S. labor force. Eleventh Census: 1890, vol. 1, pt. 2, pp. 728, 744.

²⁵The unionization rate is derived from Knight's estimate of 15,000 San Francisco union members in 1888. The rate for the entire U.S., using Wolman's estimate of union membership in 1890, was 5.1%. The denominator in each case is the nonagricultural male labor force. See Knight, Industrial Relations in San Francisco, p. 25; Wolman, Growth of Trade Unions, p. 32; Eleventh Census: 1890, vol. 1, pt. 2, pp. 304, 628.

²⁶Of course, these city differences also reflect industry mix. See Twelfth Census: 1900 - Manufactures, pt. 1, p. ccxlviii.

²⁷Twelfth Census: 1900, vol. 2, pt. 2, pp. 505-07, 590; Issel and Cherny, San Francisco, pp. 54-57.

city's population was made up of European immigrants and their children, nearly all of whom (94%) came from Northern Europe (Scandinavia or the British Isles). Thus, of the foreign-born workers in the 1892 sample, 82% were born in Northern Europe or Canada. Previous studies have shown that workers from the "new" immigrant wave were more mobile than earlier immigrants (e.g., they had higher rates of return migration), a factor that may account for Carter and Savoca's finding that nativity had no effect on their duration estimates. But nativity should be considered when comparing San Francisco and the United States.²⁸

In light of these various omissions and United States-San Francisco differences that Carter and Savoca did not control for, their projected estimate (13 years) of S_{EW} for the nonfarm United States is extremely difficult to interpret. There is no reason to believe that it constitutes even a rough approximation. That leaves us with their San Francisco S_{EW} figure of 8.5 years. At best, given their data and statistical methodology, this figure must be taken as highly tentative. Even if it was correctly estimated, it could be telling us more about San Francisco in 1892 than about job duration and employment stability in late 19th century American cities. In short, we have reason to doubt the claim that "jobs were not 'brief' in the nineteenth century".²⁹

²⁸Kazin, Barons of Labor, p. 35; California Labor Bureau, Fifth Biennial Report, p. 218. On the "new" immigration, see Hourwich, Immigration and Labor.

²⁹At the risk of seeming to flog a dead horse, it should be pointed out that a legitimate national projection from San Francisco data should also control for regional differences in variable interrelationships (covariance). For example, national homeownership rates were affected by ethnicity, age, and occupational status; there is also evidence of these effects in San Francisco. The question is, were the national effects the same as in San Francisco? See, for example, Monkkonen, America Becomes Urban, pp. 199-203; Tygiel, "Workingmen in San Francisco," pp. 273-86.

III. A Different Perspective

Our own view of urban, industrial labor markets is best expressed in dual labor market terms: In the late 19th and early 20th centuries, a majority of blue-collar workers and jobs were unstable but there existed a small sector where workers held quasi-permanent jobs. Since that time, the sectors reversed their relative size so that by the 1970s, most industrial jobs were found in the stable sector.³⁰ In what follows we present data from a variety of sources concerning the shape -- then and now -- of the upper and lower tails of the tenure distribution.

Upper tail: The tenure distribution's upper tail is the locus of so-called "primary" or "lifetime" jobs. At the turn of the century, stable manual jobs were the preserve of skilled workers, typically employed in medium-to-large sized firms that operated on a year-round basis producing standardized commodities. Employers in these firms offered stable employment to their skilled workers partly to retain scarce, skilled labor (skill differentials were comparatively wide in the U.S. at this time) and partly to preempt unionization. In industries such as meatpacking, printing, and rail transportation, skilled workers were union members who exerted "craft control" but combined it with employment rules adapted to the employer. Out of this grew practices such as promotional job ladders governed by seniority as well as seniority-based layoff systems. In pursuing these policies, trade unions adhered to the concept that "employment was a permanent relationship between the union (a set of workers) and the employer (a set of jobs)." As a result, unions came "very close to creating a bureaucratic

³⁰There is evidence, however, that the size of the primary sector has been shrinking since 1980. Jacoby and Mitchell, "Sticky Stories," passim.

employment system for their mostly skilled members."³¹

[TABLE 1 HERE]

Putting to one side the issue of whether Carter and Savoca's estimates are reliable, a comparison of their upper tail with similar statistics from a contemporary study shows that employment was less concentrated in lengthy spells in the 1890s than in the 1970s. Table 1 shows the rapid growth of lifetime jobs between the earlier and later periods, a change that is obscured by simply comparing estimated means from the two period. The proportion of workers in lengthy jobs (over 20 years) doubled or even tripled between the 1890s and the 1970s, going from 27% in 1892 to somewhere between 49 and 82 percent in the 1970s. This is consistent with Carter's earlier work on San Francisco, which showed the proportion of 1892 workers in jobs with current or eventual tenure of 20-plus years to be between a tenth and a half of modern levels. Today, the majority of male workers over age 40 hold jobs that have lasted or will last 20-plus years; less than a quarter held such jobs in 1892. In other words, the relative size of the primary and secondary sectors has been reversed.³²

³¹Jacoby, Employing Bureaucracy, p. 30. See also Licht, Working for the Railroad; Jackson, The Formation of Craft Labor Markets; Elbaum, "Making and Shaping of Job and Pay Structures," pp. 71-107.

³²Carter, "Lifetime Jobs," p. 291; Hall, "Importance of Lifetime Jobs." For those wondering about the paucity of observations in the "under one year" cells of Table 1, note two things: first, that these figures are derived from spells in progress, a measure that undersamples spells of short duration (recall that 29% of spells in progress in the 1970s were under one year versus 64% of completed spells); second, that in a steady state spells in progress are observed on average halfway through their completed duration. Among spells in progress for less than a year, the majority have a duration greater than 6 months (this is our first point), so most will have a predicted completed duration greater than one year. Also see Kiefer, Lundberg, and Neumann, "How Long is a Spell?", p. 126.

TABLE 1
Distribution of Completed (Predicted) Job Durations

<u>Proportion with Completed Duration</u>	<u>U.S., 1968-1981</u>		
	<u>San Francisco, 1892</u>	<u>Managerial and Professional</u>	<u>Blue-Collar</u>
Less than 1 year	0	0.17	1.1
1-3 years	6.4	0.84	8.3
3-10 years	66.7	16.7	41.9
Over 10 years	26.9	82.3	48.8

Note: All figures are for nonunion males and give the predicted completed duration of censored spells.

Sources: The data in column 1 are from Carter and Savoca, "Labor Mobility and Lengthy Jobs," p. 13. The data in columns 2 and 3 are from Abraham and Farber, "Job Duration, Seniority, and Earnings," p. 287.

Tables 2 and 3, which are derived from censored spells in progress, show similar results (even when the upper tail is cut at 5 years, as in Table 3). Moreover, when comparing upper tails from the two periods it should be kept in mind that labor force participation rates for older males (over 60) fell from 66% in 1900 to 32% in 1980.³³ All other things equal, this would reduce the relative size of the upper tail in the 1970s and cause reported historical changes to be understated.

[TABLES 2 and 3 HERE]

Lower tail: That most jobs and workers were unstable at the turn of the century is expressed repeatedly in contemporary accounts of tramping, "floating", reverse migration, and high quit and dismissal rates in industrial firms. Instability was concentrated among the mass of unskilled and immigrant workers, although some craft workers still had a tradition of itinerance. Job shopping was common at all ages, not just among young workers as today. On the employer side, work was seasonally unstable and the model firm offered few incentives for unskilled workers to sink roots. That is, the lower tail of the tenure distribution was sizeable and volatile.³⁴

Admittedly, labor turnover can be high but average duration lengthy if turnover is concentrated within an unstable minority of the labor force. In a classic study of the 1910s, Sumner H. Slichter claimed that turnover "is due to a few men changing rapidly while the great majority of the force is stable." For Slichter, stable workers were those with average durations

³³Ransom and Sutch, "The Labor of Older Americans," p. 14.

³⁴Carter, "Lifetime Jobs," pp. 295-96; Raff, "Ford Welfare Capitalism," pp. 90-105; Jacoby, Employing Bureaucracy, passim; Monkkonen (ed.) Walking to Work.

TABLE 2

Percent of (Censored) Job Spells Over 10 Years

	<u>1913-14</u>	<u>1928</u>	<u>1973</u>
Males	15	-	33
All	-	16	30

Note: 1973 figures are for manufacturing workers.

Sources: Column 1 is from Brissenden and Frankel, Labor Turnover, p. 126.
Column 2 is from Woytinsky, Three Aspects of Labor Dynamics,
p. 40. Column 3 is from U.S. Department of Labor, Job Tenure of
Workers, pp. A13-A14.

TABLE 3

Percent of (Censored) Job Spells Over Five Years

	<u>Manufacturing</u>	<u>Automobiles</u>	<u>Chemicals</u>	<u>Machinery</u>	<u>Metals</u>	<u>Textiles & Apparel</u>
1913-14	31	21	10	32	32	51
1917-18	25	21	10	32	24	29
1973	49	60	55	48	55	44

Sources: 1913-14 (n = 49,970 workers in 28 manufacturing establishments)
 1917-18 (n = 45,791 workers in 40 manufacturing establishments)
 are from Brissenden and Frankel, Labor Turnover, pp. 118-19. 1973
 is from U.S. Department of Labor, Job Tenure of Workers, p. A13.

(spells in progress) of a year or more.³⁵ But was Slichter right? Slichter's data were made up entirely of censored spells, from which it is not possible to derive the size of the unstable labor force (many of whom are between jobs at any point in time). Also, as previously noted, spells in progress are longer, on average, than completed spells. Among today's nonunion blue-collar workers, 64% of completed spells are under one year, whereas 29% of spells in progress are under a year. In other words, censored spells in progress (at given point in time), do not accurately gauge the duration pattern or size of the unstable labor force.³⁶

Next we may ask, did the lower tail change its shape since the 1910s? Recall that spells in progress surveyed at points over time will not fully register a change in lower-tail volatility, just as periodic censuses of average age can mask a large drop in infant mortality. What we need but do not have are historical data on S_{TW} . Even so, the data on spells in progress show sharp changes over time, with the proportion of workers in short-term jobs (under one year, which was Slichter's criterion) dropping by about 50% in the manufacturing sector since the 1910s (see Table 4). The changes are especially notable in the automobile and chemical industries, where the proportion of workers in short-term jobs today is one-third the level of the 1910s. Change was less dramatic in the textile and apparel industries, which either may be due to peculiarities of those industries or to measurement error. Firms that maintained seniority records in the 1910s were a

³⁵Slichter, Turnover, pp. 43, 45.

³⁶Abraham and Farber, "Job Duration, Seniority, and Earnings," p. 284. The combination of stability and mobility is reconciled by, among others, Zunz, in his study of turn-of-the-century Detroit. He found the city's blue-collar population to consist of a core of stable homeowners who served -- as landlords and Landsleute -- as way stations for a much larger population of unstable and transient immigrants. Zunz, Changing Face of Inequality, pp. 178-86.

select group, presumably with job duration levels above the "true" mean.³⁷

[TABLE 4 HERE]

Although we lack historical data on S_{TW} , there are other statistics that track changes in the unstable labor force. Each shows the same result as Table 4 -- that the unstable labor force was large at the turn of the century and has declined sharply over the years. These statistics include measures of unemployment and labor turnover (which were concentrated among short-term jobholders and had the effect of chopping up job spells into shorter segments) and of spatial mobility (a source of job mobility). We examine each of these in turn.

Mean levels of cyclical and seasonal unemployment were higher prior to the 1920s than today. Seasonality at the turn of the century was especially sharp. Industrial employment levels in 1909 fluctuated by 14% over the year, rising to 45% in the automobile industry, whereas the industrial average in 1989 was 1%.³⁸ Unemployment need not have severed the employment relation if employers had regularly rehired workers on layoff. But rehiring was less common at the turn of the century than it is today.³⁹ A

³⁷ Concerning their data, Brissenden and Frankel warned that "the establishments from which the Bureau of Labor Statistics has secured labor mobility figures have necessarily been concerns which had the figures to give, that is to say, concerns which had given more attention than most firms to their force-maintenance problems ... In such establishments the instability is not likely to be so serious as in the general run of American concerns, which as a rule pay little or no attention to the flow of labor in and out and give little attention to its control." Brissenden and Frankel, "Mobility of Labor," p. 40.

³⁸ Keyssar, Out of Work, pp. 65-67; Jacoby, Employing Bureaucracy, p. 22; Employment and Earnings, Table B-2, 1989-1990, various issues; Romer, "Spurious Volatility," p. 3.

³⁹ Statistics from a large metalworking plant reveal that only 8% of all new hires after the depression of 1907-1908 were rehires versus a recall rate in the 1970s of about 60 to 65 percent. Slichter, Turnover, p. 126; Clark and Summers, "Labor Market Dynamics," p. 49.

TABLE 4

Percent of (Censored) Job Spells Under One Year

	<u>Manufacturing</u>	<u>Automobiles</u>	<u>Chemicals</u>	<u>Machinery</u>	<u>Metals</u>	<u>Textiles & Apparel</u>
1913-14	38	42	64	37	24	15
1917-18	42	52	65	37	44	31
1973	22	15	16	22	18	25

Sources: 1913-14 (n = 49,970 workers in 28 manufacturing establishments)
1917-18 (n = 45,791 workers in 40 manufacturing establishments)
are from Brissenden and Frankel, Labor Turnover, pp. 118-19. 1973
is from U.S. Department of Labor, Job Tenure of Workers, p. A13.

useful statistic that summarizes these changes is unemployment incidence -- the proportion of workers affected by job loss each year. Incidence rates averaged about 24% in the late 19th century; for trade union members, the average between 1908 and 1922 was 26% and rose to nearly 40% in 1908 and 1921. By contrast, mean incidence for all workers between 1958 and 1987 was 15% and only once rose above 20% -- during the 1982 recession.⁴⁰

Data on labor turnover point in the same direction. Average turnover rates were very high in the 1900s and 1910s, with monthly separation rates commonly in excess of 10%, even during recessions. A survey of 14 Detroit industrial concerns found an average monthly separation rate of 15.3% in 1913-14. Turnover rates dropped sharply between the 1920s and the 1950s. Although a spike occurred during World War II, it was considerably smaller than the previous wartime peak. By the 1960s, separation rates were running about 2 to 4 percent; the highest monthly rate in manufacturing during the 1960s was 4.9% in 1963.⁴¹

Spatial mobility is another way of tracking job mobility. Although the measure is imperfect (since some separations involve changes between jobs in the same area and some moves are by persons outside the labor force), it is a useful gauge of trends in job mobility.⁴² Studies by Thernstrom and other urban historians have found that spatial mobility (and by implication job mobility) was greater in the late 19th century than today. It was not unusual for decennial persistence rates in late 19th century American cities to be as low as 35%, meaning that 65% of the population residing in a city

⁴⁰Keyssar, Out of Work, pp. 51-53; BLS, Handbook of Labor Statistics, pp. 220-23.

⁴¹Jacoby, "Industrial Labor Mobility," pp. 262, 268.

⁴²Currently 40% of separations involves a move and over 60% of moves entail a separation. Bartel, "The Migration Decision," pp. 775-86.

at a point in time were no longer living there 10 years later. Carter and Savoca argue that a low geographic persistence rate need not be inconsistent with lengthy residential spells. They estimate that, even with a persistence rate of only 35%, the average resident stayed for over 10 years. But this is an S_{EW} estimate, based on the completed tenure of the population residing in a city at the start of a decade. It ignores the ceaseless movement into and out of a city over the course of a decade, which can cause the mobile population to turn over several times. That is precisely what happened in American cities of the late 19th and early 20th centuries.⁴³

For example, Boston's population grew moderately during the 1880s, from 362,839 to 448,477 persons. The 1880s saw Boston's highest decennial persistence rate of the 19th century -- 64% -- which would imply an average residential tenure (S_{EW}) of 22 years. But the appearance of stability is misleading. While net migration during the 1880s was only 65,000 individuals, gross flows into and out of Boston during those years were enormous, possibly as high as 1,500,000 individuals (a figure that dwarfs Boston's stable population). What was residential S_{TW} during the 1880s? While we cannot give a precise figure, it was somewhere between 1.3 years (or less) and 9.1 years, much lower than the S_{EW} estimate of 22 years. This is consistent with data showing S_{EW} to be three to five times as large as S_{TW} in recent years.⁴⁴

⁴³Thernstrom, Other Bostonians, pp. 222-23.

⁴⁴During the 1880s, 138,572 households left Boston. We do not know the average size of these households; it could have been slightly greater than 1 (if mostly single males) or slightly larger than 5 (which was the average size of resident Boston households). A reasonable assumption is that departing households contained 3.5 persons, meaning that 485,000 residents left Boston during the 1880s. The persistence rate indicates that 130,000 of these outmigrants resided in Boston in 1880; the remaining 355,000 arrived after 1880. Average tenure for the first group was somewhere between 2 and 20 years; average tenure for the second group probably fell

Critics have found various biases in Thernstrom's estimates. Yet even the critics admit the biases can go in either direction. Monkkonen, for example, argues that Thernstrom's figures are downward biased, while Galenson and Levy note, "in practice measured persistence rates for a population will rarely be greater than true ones," although, they add, "the relationship between the two often might be difficult to bound with confidence."⁴⁵ One way to "bound with confidence" is to examine other data sets. Estimates of employment persistence derived from longitudinal payroll records are less fraught with potential bias than persistence data derived from city directories. Yet the findings on 19th century employment persistence -- that it was low relative to levels of recent years -- mirror the evidence on spatial mobility. Thus, data on geographic mobility are a reliable guide to job mobility and suggest that a majority of the population was residentially and occupationally unstable at the turn of the century.⁴⁶

Conclusion

Information drawn from a variety of labor market statistics paint a consistent picture. Most industrial jobs and workers were unstable during the decades spanning the turn of the century but there was also a small, stable sector whose relative size and importance increased during the

between 1 and 5 years. This gives sufficient information to calculate tenure as a weighted average of the two groups. It should be emphasized, however, that these figures are suggestive rather than definitive. Thernstrom, Other Bostonians, pp. 11-21; Akerlof and Main, "Measure of Durations," p. 1007.

⁴⁵ Monkkonen, American Becomes Urban, p. 195; Galenson and Levy, "Biases in Persistence Rates," p. 176.

⁴⁶ Ginger, "Labor in Massachusetts Cotton Mill," p. 84; Gitelman, "The Waltham System," pp. 227-53; Navin, The Whitin Machine Works, pp. 160-61. Early 20th century payroll records show similar instability; see Whatley and Wright, "Black Workers at the Ford Motor Company".

decades following World War I. Accounting for the historical shift in sectoral dominance is beyond the scope of this paper. Suffice it to say that the change resulted from a complex interaction between labor supply factors (e.g., declining immigration), demand factors (increased size and bureaucratization of firms), and institutional forces (mass unionism and public regulation of the labor market).

Future historical studies of job duration must carefully consider the statistical assumptions that allow derivations of completed spell duration when data are only available on censored spells. Attention should also be given to the properties of various duration statistics. Ideally, one would like to have panel data, akin to the modern Panel Study on Income Dynamics (PSID), for late 19th and early 20th century workers. These data conceivably could be gleaned from censuses and city directories. Alternatively, more systematic mining of corporate payroll records would yield a sharper historical picture of job duration. Yet care must be taken when analyzing samples from a particular firm or region to place the data in historical context and to control for sampling bias. Finally, it need hardly be emphasized that no single statistic can capture all facets of a distribution's shape, much less changes in its shape over time.

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