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The Extent and Consequences of Job Turnover

EMPIRICAL STUDIES OF LABOR TURNOVER play an important role in improving our understanding of the labor market. For example, theories of frictional unemployment increase in significance if total turnover is found to be large. If a dominant form of turnover is temporary separations without a permanent job change, theories of temporary layoff unemployment (based on a view of the labor market in which firms and workers form long-term attachments) gain importance. Similarly, the problems associated with structural unemployment are most likely to be of concern if permanent separations due to plant closings or cutbacks make up a large part of turnover. Additionally, separations are likely to result in larger earnings losses if a high-quality job match is destroyed, or if the worker had accumulated firm-specific human capital. Because such losses are likely to be high, and because firms also incur losses in the form of hiring and training costs when turnover occurs, both parties have an incentive to reduce turnover in these cases. Thus turnover patterns can be informative on the nature of the matching of workers to jobs and on the accumulation of firm-specific human capital.[1] Despite the importance of turnover,

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1. See Jovanovic (1979); Oi (1962); and Becker (1962).

though, our knowledge of it is surprisingly slight, and much of what is known comes only from the manufacturing sector.

By the terms *turnover* or *worker reallocation* we mean the formation and dissolution of employee-employer job matches. We classify this turnover as either permanent or temporary based on whether a separation is ultimately followed by a return to the same employer. Following Davis and Haltiwanger, we further subdivide turnover: turnover due to job-position creation and destruction as firms expand or contract, and turnover due to job-match creation and destruction as workers begin at or leave from continuing positions.[2] Overall, then, total turnover, or worker reallocation, can be decomposed into three parts: temporary turnover at continuing job matches; permanent turnover due to job-position creation and destruction, or job reallocation; and permanent turnover due to other causes, or simply job-match creation and destruction at continuing positions.

Up through 1981 the Bureau of Labor Statistics (BLS) published figures on turnover based on a voluntary survey of large manufacturing establishments. In general, these data suggested that a large fraction of layoffs were temporary; total monthly separations and accessions each hovered around 3 to 5 percent.[3] Perhaps in part because of information such as this, past work has emphasized the importance of temporary separations.[4] More recently, manufacturing establishment data from the Longitudinal Research Data (LRD) file have been used by Davis and Haltiwanger to show that job reallocation through gross job creation and destruction is much larger than would be inferred from figures on net employment changes.[5] This view of the economy in which there is a large amount of job reallocation has also motivated theoretical innovations to matching models, such as those by Blanchard and Diamond or Mortensen and Pissarides.[6]

Good information on turnover is important because such information is a key building block in formulating theories of unemployment and labor market dynamics. Thus it is useful to update the information on permanent

2. See Davis and Haltiwanger (1992).

3. See U.S. Department of Labor (1982).

4. See Hall (1972); Feldstein (1975); Baily (1977); Azariadis (1975); Topel (1983, 1990); and Katz and Meyer (1990).

5. See Davis and Haltiwanger (1990, 1992).

6. See Blanchard and Diamond (1989, 1990) and Mortensen and Pissarides (1991).

and temporary separations and accessions that was once provided by the BLS survey, and to expand the scope to industries outside of manufacturing. Similarly, the information on gross job creation and destruction needs to be expanded to cover other sectors. While the cost of implementing a new survey to provide this information would likely prove prohibitive, data that allow such turnover rates to be calculated are currently collected as part of the administrative systems of state unemployment insurance (UI) programs.

To expand our knowledge of turnover, we use data collected as part of the Continuous Wage and Benefit History (CWBH) project from eight states' UI systems. We find that turnover is greater in magnitude than was previously found, with a larger fraction being permanent than once was thought. While turnover is concentrated in a subset of individuals, it reaches more people than previous analyses have indicated. We also find large effects on turnover probabilities of the level of earnings, industry, and firm size, both when we do and do not allow for individual fixed effects. Additionally, in expanding the turnover measures of the BLS survey and the LRD to a broad range of industries, we find that manufacturing is atypical in many ways.

Not only are we able to approximate the permanent and temporary layoff measures of the BLS survey and the gross job creation and destruction measures of the LRD, but we are also able to directly link these different concepts in order to decompose turnover into its three components. Our decomposition indicates that about 28 percent of turnover is temporary; 31 percent is due to job-position creation and destruction, and the rest is due to job-match creation and destruction only. We are also able to explicitly link turnover to workers' costs in terms of lost employment and earnings. While the costs of most turnover in terms of lost earnings are not high, a small fraction of separations (particularly those that are permanent) do result in large losses. In addition, we find that total turnover is procyclical, although temporary turnover is countercyclical, and, at least at annual frequencies, job reallocation is countercyclical.

After briefly discussing the main theories of turnover, we review past empirical work on job turnover and explain the weaknesses of commonly used data sources. Data from the CWBH are explained. We then use these data to analyze the characteristics of turnover and to explore the costs of turnover in terms of workers' lost earnings and employment. Gross job creation and destruction rates across industries are compared, and the role

of this job reallocation in total worker reallocation is assessed. Our paper concludes with some general comments.

Theories of Turnover

The decision to dissolve a job match can be analyzed as a worker decision, a firm decision, or a joint decision.[7] We begin by thinking only of the worker; it is clear that a job change should occur if the net present value of the gain in utility from moving to a new job outweighs the cost of moving. Thus we can formalize the mobility decision by looking at the lifetime utility maximizing decision where a worker will choose to change jobs if the following condition holds:

$$\sum_t \frac{N_t}{(1 + r)^t} - \sum_t \frac{O_t}{(1 + r)^t} > C,$$

where t indexes time, N_t represents utility each period on the new job, O_t represents utility each period on the old job, C represents one-time costs of moving, and r represents the interest rate. Despite the simplicity of this model, several predictions can be made as to the effect of current job characteristics on mobility.[8]

All else equal, higher current wages or nonwage benefits will reduce turnover, as will higher predicted wage growth or higher future benefits such as pensions. The ability of these aspects of compensation to reduce voluntary turnover is the basis for several well-known theories where compensation functions as an incentive device. Several papers have formalized the idea that a firm may want to pay higher wages to reduce quits.[9] In these models, firms choose to pay above market wages, be-

7. These first two decisions correspond to worker quits and firm layoffs, while the third is associated with the notion of efficient turnover, where all separations are considered to be joint decisions. See McLaughlin (1991) for a recent summary of efficient turnover.

8. See Pencavel (1970) for a slightly more complicated but very similar model. The main implications of our simple model would hold with the addition of uncertainty and risk aversion.

9. See Parsons (1972); Pencavel (1972); and Salop (1973). This idea has been recently adopted in the efficiency wage literature. See Akerlof and Yellen (1986) and Weiss (1990) for useful summaries of this literaure.

cause the marginal cost of increasing the wage is outweighed by the marginal benefits of decreased turnover and increased productivity. A related class of models focuses on tilting the compensation profile: workers are "underpaid" early in their careers but "overpaid" later on, so in present value terms the job is as valuable as an alternative job in which the worker is always paid a wage exactly equal to his or her marginal product. Such a job not only will reduce turnover due to the steep wage growth but also will appeal mainly to workers who plan to have long tenures, and thus are perhaps inherently less likely to quit. An equivalent result can be obtained through the use of pensions as a reward for long tenure.[10] The wage path on the current job may be above that for an equivalent alternative job even if compensation is not used as an incentive device. A simple theory of investment in firm-specific human capital will also predict this result.[11]

Each of the above models predicts that wages will grow with tenure. This is not a necessary feature, however, of models that predict that turnover will decrease with tenure. In a simple matching model workers participate in "job shopping," since the quality of a given job match is only revealed over time. If a job match reveals itself to be bad, a worker will move to sample a new job match. Since workers who find themselves in good matches reveal this by not moving, the longer the job tenure the more likely the match is of high quality and the less likely the worker is to change jobs.[12]

A final theory of voluntary turnover focuses on the exit-voice trade-off. In these models a worker unhappy with his present situation has two choices: change jobs (exit) or change the situation in such a way that remaining on the job is optimal (use his voice). While the possibility of using one's voice is always open, the presence of a union may make this option more effective.[13] As a result, unionization may reduce

10. See Ippolito (1991) for a review of both theories and empirical results that indicate that the use of pensions is more important than wage-tilt in encouraging long-term tenure.

11. See Becker (1962).

12. See Jovanovic (1979) and the discussion of several other papers in Mortensen (1986, pp. 876–77).

13. See Hirschman (1973) and antecedents such as Shister (1950). Parnes (1954) also points out that unions could alternatively increase quits by making workers aware

turnover directly, independent of any effect that works through the wage.

While each of the above theories can be used to explain voluntary worker mobility, this is only one component of turnover. It is important to also consider the firm's decision to lay off or hire workers. In the simplest model of static labor demand, firms simply hire up to the point where the wage is equal to the marginal revenue product. A more dynamic framework, however, provides richer implications. Since firms incur adjustment costs when changing employment (for example, recruiting and training costs, severance pay, and costs of unemployment insurance), this implies that there is a wedge between the current wage and marginal product. As a result, the higher are adjustment costs, the lower is the variability of employment.[14] Note there is an overlap here with the theories of quits, since training costs may arise from the shared costs of investment in firm-specific human capital. More investment will reduce both worker-initiated and firm-initiated turnover. Also, besides reducing layoffs when these costs are high, firms have a strong incentive to reduce quits.

These models of labor demand, however, are generally based on the assumption of a continuing representative firm. Ignored, then, is the role of firm births and deaths in generating turnover.[15] This process has been explored in the industrial organization literature, with much of the recent empirical work based on the model of Jovanovic.[16] In this model a firm's costs are considered to be a random draw from a known distribution. Through operation, information is revealed that allows the firm to update its belief about its true costs. If a firm discovers it has low costs, it will survive and grow; if it has high costs, it will fail. This model predicts that younger (and hence smaller) firms are more likely to fail, but those that do not fail will grow faster than the older (and hence larger) firms. As the firms mature (grow), their growth rates will

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of alternative employment. Freeman (1980) provides empirical evidence on unions and quit rates.

14. See Anderson (1993) for a recent example of this result, and see Hamermesh (1993, chaps. 7 and 8) for a summary of past results.

15. Hamermesh (1993) discusses more fully the distinction between the assumption of a continuing representative firm and the reality of old firms dying and new firms being born.

16. See Jovanovic (1982) for the model and Dunne, Roberts, and Samuelson (1989b) for an example of an empirical test.

converge. Thus turnover is likely to be higher at smaller firms because of more firm deaths as well as higher rates of job creation.

A final class of models focuses on the joint decisionmaking of the worker and the firm through the forming of long-term attachments.[17] These models assume that some amount of firm-specific human capital is acquired on the job. Such an investment then creates an incentive (both for the workers and for the firms) to create these long-term attachments. Also, firms are typically assumed to maximize profits subject to providing a given level of utility for an average worker. Since this average worker may prefer a risk of layoff to a fluctuating wage, these models also predict that temporary layoffs may be used instead of wage reductions in the face of declining demands.[18]

Overall, then, we have several classes of theories relating to turnover of different types. Theories in which workers and firms form long-term attachments and meet demand shifts through the use of temporary layoffs can be applied to explaining that fraction of worker reallocation attributable to temporary separations and returns. Theories of firm expansion and contraction are best applied to explaining the job reallocation aspect of turnover in which jobs are created and destroyed. Finally, those theories that focus only on the workers' decisions may best explain the fraction of worker allocation that is neither temporary, nor attributable to this job reallocation, but rather to other reasons that cause workers to move among continuing job positions. In the next section we briefly review some of the past empirical work on turnover and discuss where new work may yield additional insights.

Past Empirical Work on Job Turnover

One motivation for theories of long-term attachments in which temporary layoffs are the optimal response to declines in product demand is the picture painted by the BLS manufacturing turnover data. In 1981, the last year such data were collected, monthly layoff rates per one hundred employees averaged 1.6, while recall rates averaged 1.0, and

17. The implicit contract literature provides an example of this class of models. Rosen (1985) presents a survey of this literature.

18. Feldstein (1975); Bailey (1977); and Topel (1983, 1990) apply such models to an exploration of the role of the UI system in encouraging the use of temporary layoffs.

new hire rates averaged 2.0, with quit rates averaging 1.3.[19] These figures imply that over 60 percent of layoffs ended in recall, and one-third of all accessions were from temporary layoffs. While the scenario is indeed one wherein temporary, rather than permanent, layoffs play the largest role, there are several reasons to believe that the BLS turnover data were not very representative of the economy as a whole.

The data's most obvious drawback is that their coverage was limited to manufacturing, which accounted for only 22 percent of total employment in 1980 and just 18 percent by 1990.[20] A larger problem, however, was that the survey was not even representative of manufacturing. First, large firms were overrepresented; the BLS tried to include at least 60 percent of those establishments having over one hundred employees but only 5 percent of all other establishments.[21] A second problem was the voluntary nature of the survey. The data were collected by sending a form each month to the sampled firm. The firm was required to fill in the number of quits, discharges, layoffs, and other separations, as well as the number of new hires, recalls, and other accessions during the month. Additionally, the firm was asked to report the total number of workers on the payroll during the pay period covering the twelfth of the month.[22] As has been pointed out by others, the higher the level of turnover at a firm, the more onerous was the task of filling out this form.[23] Firms that voluntarily provided turnover data were likely to have had lower turnover than firms that opted not to provide the data.

A second major source for past empirical work on turnover issues is the Current Population Survey (CPS). However, since the CPS focuses only on the individual, and not on the employer-employee match, it presents its own drawbacks when used to investigate turnover issues. Workers were typically categorized as employed, unemployed after a temporary layoff, or unemployed for other reasons.[24] Given this classification based on current labor force status, the fraction of unemployment attributable to temporary layoffs can be calculated. As was noted,

19. U.S. Department of Labor (1982, p. 80).

20. U.S. Department of Commerce (1992, p. 396).

21. U.S. Department of Labor (1962).

22. U.S. Department of Labor (1976).

23. See Hall and Lilien (1979). Parson (1977, footnote 13, p. 219) reports underestimation or overestimation of different types of turnover with these data.

24. See Feldstein (1978) and Topel (1983, 1990).

though, unemployment is not the only cost of turnover. This approach ignores all types of turnover that do not involve unemployment. Moreover, it is not clear exactly what this method, in its sole focus on unemployment, is capturing, since the definition of a temporary layoff is imprecise. Essentially, a layoff is classified as temporary if the survey respondent expects to return to work. The expectation of recall, however, may be incorrect, and the worker may never actually return.[25] Alternatively, a worker who initially expected to be recalled may have a different expectation much later at the time of interview. Thus the CPS temporary layoff concept captures neither initial recall expectations, nor whether a worker actually returns. Special CPS supplements with information on current job tenure have also been used to investigate turnover.[26] While it is possible to estimate completed tenure from these incomplete spells, panel data are required to fully investigate why turnover seems to be concentrated among a fraction of individuals.[27]

More recently, the firm side of turnover has been explored using panels created from establishment-level data, such as those collected by the LRD or the Census of Manufactures.[28] With these data it is possible to define the gross change in employment as the sum of employment gains at growing firms and of employment losses at shrinking firms, and to directly investigate firm births and deaths. This method, however, only approximates true job reallocation. When using plant-level data, one will mistakenly identify as job creation and destruction any firm-level reorganization that results in the transfer of jobs across plants. At the same time, with either plant-level or firm-level data, true job reallocation may be missed, if restructuring results in different jobs but the same employment level. While these shortcomings should be kept in mind, looking at changes in employment will provide information on true job creation and destruction.

Having information only on the firm, like having information only on the worker, means that the picture is incomplete. Although this work can address the issue of job reallocation, it cannot address the larger

25. See Katz and Meyer (1990) for more on the role of recall expectations.

26. See Akerlof and Main (1981) and Hall (1982).

27. Some work on this has been done with the smaller samples of the National Longitudinal Survey (NLS) and the Panel Study of Income Dynamics (PSID). See Hall (1972).

28. See Davis and Haltiwanger (1990, 1992) and Dunne, Roberts, and Samuelson (1989a, 1989b).

issue of the relationship between worker reallocation and job reallocation. Using the CPS, Davis and Haltiwanger do attempt to indirectly calculate that 35 to 56 percent of worker reallocation can be attributed to job reallocation. These establishment-level data sets are limited not only because they cannot fully address the issue of worker reallocation but also because they analyze the manufacturing sector only. Thus, as with the BLS survey, the results may not be representative of the economy as a whole, and comparisons across major industry groups are precluded. In the next section we describe the CWBH panel data set, which covers all industries and has both person and firm components. This allows us to overcome some of the drawbacks of past work.

Data from State Unemployment Insurance Systems

The data we use to expand our picture of turnover to all industries come from administrative records of the unemployment insurance systems of eight states that participated in the Continuous Wage and Benefit History project.[29] The data are of two types: quarterly wage records and weekly unemployment insurance records. The quarterly wage records are for a sample (typically 10 to 20 pecent) of the state's covered workers. The main categories of noncovered workers are federal employees and the self-employed. Therefore, our sample is likely to be representative of close to 90 percent of those working in the state. In addition to the dollar amount of wages received by the employee, the records contain a firm identifier—the federal employer identification number (FEIN)—and several firm characteristics, including four-digit standard industrial classification (SIC) industry, average monthly employment over the quarter, and the total quarterly wage bill. The number of quarters of data available differs by state but averages about twenty quarters between 1978 and 1984 (see appendix table A-1). Since the wage records contain a firm identifier and firm employment, the data can be used to create firm panels. A clear advantage of using the CWBH data over many past data sets is that all industries can be included, rather than just manufacturing.

The real strength of our approach, however, stems from the fact that

29. The eight states are Georgia, Idaho, Louisiana, Missouri, New Mexico, Pennsylvania, South Carolina, and Washington.

the data are essentially person based. For a sample of these individual workers, we have created quarterly job-match histories.[30] From these we can observe when separations (job-match dissolutions) and accessions (job-match formations) occur, and we can determine if the match re-forms, implying the turnover was temporary. Since the wage records are quarterly, we will be unable to observe temporary layoffs that last less than a full calendar quarter. However, since we also have weekly unemployment insurance records, we will identify the temporary layoff if unemployment insurance is received. The appendix provides a more complete description of the data processing, along with several sample job histories and their classification. Occasionally, a wage record will be missing due to a processing error, rather than a true separation. Similarly, mergers and acquisitions may result in FEIN changes that we will misclassify as turnover. To the extent that these events occur, we will overestimate actual turnover. Below we investigate the likely size of these problems.

In order to estimate the costs to the workers of the separation, we construct a measure of the number of "earnings weeks" lost. While it may be more typical to think of the costs simply in terms of weeks of unemployment, the data do not provide information on weeks worked. Arguably, though, since earnings losses are also a cost to turnover, an "earnings weeks" measure that takes this into account may be desirable. Thus we first calculate "usual" weekly earnings from the quarter prior to the separation. Comparing this measure with earnings in the quarter of separation and those in the quarter of reemployment allows us to estimate the number of weeks lost in these quarters. In addition, we assign thirteen lost weeks to each missing quarter in between. To the extent that there are earnings losses upon reemployment, our measure will overstate the actual number of weeks unemployed. The likely extent of this overstatement is also explored below.

In order to investigate job creation and destruction, we construct a firm sample from those states with sampling rates of at least 10 per-

30. Because of the difficulty of processing close to 30 million wage records, we have used a subsample of workers chosen so that each state contributes approximately 150,000 wage records for a total of over 1 million job-match quarters. We refer to this as the individual sample. We have also chosen a subsample of workers based on firm identifiers. We refer to this as the firm sample. The initial processing of the two samples is identical.

Table 1. Individual Sample and Firm Sample Compared with U.S. Averages
Percent

Variable	*United States*	*Individual sample*[a]	*Firm sample*[a]
Unemployment rate[b]			
1980	7.1	7.2	6.9
1983	9.6	10.4	9.6
Change, 1980–83	2.5	3.2	2.7
Change in employment, 1980–83[c]	−0.2	−0.3	1.3
Unionization rate, 1982[d]	21.9	18.0	14.5
Industry shares, 1981[e]			
Agriculture	1.1	1.6	0.4
Mining	1.2	2.4	3.1
Construction	4.4	7.5	4.6
Manufacturing	22.6	22.6	21.3
Transportation/communications	5.2	6.1	6.5
Wholesale trade	6.0	6.2	3.7
Retail trade	16.6	19.2	18.7
Finance, insurance, and real estate	5.8	5.5	5.9
Services	19.1	25.0	27.2
Public sector	18.1	3.9	8.5
Enterprise size, 1982[f]			
99 or fewer employees	39.4	45.8	16.9
100–499	13.8	22.0	23.2
500 or more	46.9	32.2	59.8

a. For the unemployment rate, change in employment, and unionization rate, weighted averages of state rates are used. For 1981 industry shares and 1982 enterprise size, the numbers are averages from the respective samples.
b. Civilian unemployment rate is from Bureau of Labor Statistics, *Geographic Profile of Employment and Unemployment*, annual editions. U.S. Department of Labor.
c. Employees on nonagricultural payrolls are from Bureau of Labor Statistics, *Handbook of Labor Statistics*, Bulletin 2340. U.S. Department of Labor, August 1989.
d. Percent organized from Leo Troy and Neil Sheflin, *U.S. Union Sourcebook: Membership, Finances, Structure, Directory* (West Orange, N.J.: IRDIS, 1985).
e. Private employment is from unpublished tabulations, Bureau of Labor Statistics. Government employment is from Bureau of Labor Statistics, *Handbook of Labor Statistics*, Bulletin 2340. U.S. Department of Labor, August 1989.
f. Small Business Administration, *Handbook of Small Business Data*, 1988. Calculations exclude the self-employed.

cent.[31] With such a sampling rate there is a 0.995 probability that at least one worker will be sampled from a firm with fifty or more employees. Thus, after sampling on firm identifier, and processing the data in the same manner as the individual sample, only records from those workers at these larger firms are retained. Table 1 provides summary statistics that allow a comparison of our sample states to the entire United States.

For the most part our sample states are not very different from the rest of the United States, but there are some important differences. The

31. This restriction leaves us with Georgia, Idaho, Louisiana, New Mexico, South Carolina, and Washington.

unemployment rate in the eight states used in the individual sample was nearly the same as that for the entire United States at the beginning of our sample period, and it was only 0.8 percentage point higher by the end of the period. For the six states used in the firm sample, the unemployment rate is 0.2 percentage point lower in 1980 than it was for the entire United States, but it is identical to the U.S. rate by 1983. Overall, employment fell by 0.2 percent, while for the states in the individual sample it fell by 0.3 percent. However, for the states in the firm sample, employment rose by 1.3 percent between 1980 and 1983.

There are also some appreciable differences in the unionization rate for our states and in the percentage of government workers in the samples. The unionization rate is lower in our individual sample states by 3.9 percentage points and by 7.4 percentage points in the firm sample states. In both samples we miss most government workers. The underrepresentation of government employees occurs both because the CWBH data omit federal workers and because state and local governments that self-insure under the UI system are often missing. Other industry shares are roughly comparable, though we have greater representation of agriculture and construction in the individual sample. This overrepresentation may be partly due to our unit of observation (a job-match quarter) since it would cause a greater representation of high-turnover industries. In the firm sample mining is overrepresented, and wholesale trade is most notably underrepresented. These differences are most likely due to the firm sample being limited to slightly larger firms.

This limitation of the firm sample is easily seen in the bottom panel, where the smallest firms are quite underrepresented. In the individual sample, by contrast, it is the largest firms that are somewhat underrepresented. Again, this is most likely due to our unit of analysis being the job-match quarter. Thus one should remember that some of the differences in industry share and firm size are because of differences in data sources and methods. Furthermore, in many of the analyses below, we control for industry, firm size, and the state unemployment rate.

One might also wonder how the UI systems, particularly experience rating incentives regarding layoffs, compare in our states and the United States. Three-quarters of our states, like about three-quarters of states nationally, use reserve ratio experience rating. In these six reserve ratio states we can compare aggregate measures of experience rating reported

in previous work to those for our states. Such comparisons indicate that our states are broadly representative of the entire country.[32] Complete details on the creation of the data sets can be found in the appendix.

The Extent of Job Turnover

Although the extent of temporary and permanent turnover has important implications for theories of unemployment, most of what is known applies only to manufacturing, and very little is known about the role of firm characteristics. An important first step, then, is simply to document the level of turnover (both permanent and temporary) for different groups of firms and individuals. More formally, we use the following definitions for different categories of job turnover:

—New Hires = Job Creation + New Hires at Existing Positions

—Total Accessions = Recalls + New Hires

—Permanent Separations = Job Destruction + Separations from Continuing Positions

—Total Separations = Temporary Layoffs + Permanent Separations

—Total Permanent Turnover = New Hires + Permanent Separations

—Total Turnover = Total Accessions + Total Separations.

Table 2 presents selected quarterly turnover rates for the sample overall and for major industry group, firm payroll per worker class, firm size class, and job-match tenure class.[33] The rates are calculated as the total number of separations (or accessions) over the total number of job matches. Out of more than 1 million quarterly job-match observations, 23 percent of job matches dissolved during a quarter; 17 percent permanently dissolved. A difference in our definitions from those of the BLS involves the classification of separations.[34] Although the BLS survey differentiated separations based on who was reported to

32. See Topel (1990, p. 120), where aggregate experience rating measures for all six states are reported for the 1977–81 period, and Card and Levine (1994), where industry by state measures are reported for the 1978–87 period for five of our states.

33. Missing values for these classification variables led us to drop 65,029 quarters from the analysis.

34. Another difference is that the old BLS survey obtained rates by dividing total monthly separations (or accessions) by midmonth employment.

Table 2. Quarterly Turnover Rates by Industry, Firm Size, Payroll per Worker, and Tenure

Classification	*Number of observations*[a]	*Permanent separations*	*New accessions*	*Temporary separations*	*Total separations*
Total	1,011,408	0.1723	0.1613	0.0581	0.2304
Industry					
Agriculture	16,409	0.3764	0.3569	0.1032	0.4796
Mining	24,035	0.1988	0.1746	0.0612	0.2600
Construction	75,683	0.2991	0.2769	0.0823	0.3814
Manufacturing	228,113	0.1135	0.0979	0.0892	0.2027
Transportation/ communications	61,974	0.1224	0.1113	0.0520	0.1743
Wholesale trade	63,059	0.1490	0.1378	0.0415	0.1905
Retail trade	194,044	0.2285	0.2180	0.0390	0.2675
Finance, insurance, and real estate	55,687	0.1196	0.1157	0.0292	0.1488
Services	252,977	0.1702	0.1653	0.0481	0.2183
Public sector	39,427	0.0955	0.0933	0.0464	0.1419
Quarterly payroll per worker ($1,000s)					
Less than 1	46,993	0.3034	0.2916	0.0649	0.3683
1–2.5	292,639	0.2389	0.2303	0.0601	0.2990
2.5–5	446,472	0.1383	0.1288	0.0559	0.1941
5–7.5	166,974	0.1161	0.1026	0.0562	0.1723
7.5 or more	58,330	0.1542	0.1272	0.0648	0.2190
Firm size					
Fewer than 20 employees	231,895	0.2193	0.2053	0.0576	0.2768
20–99	230,216	0.2165	0.2045	0.0555	0.2720
100–499	223,836	0.1771	0.1656	0.0621	0.2392
500–1,999	177,876	0.1252	0.1167	0.0601	0.1852
2,000 or more	147,585	0.0792	0.0722	0.0545	0.1336
Tenure at firm					
One year or more	582,268	0.0743	N/A	0.0567	0.1310
Less than 1 year	429,140	0.3053	N/A	0.0600	0.3653

Source: Authors' calculations based on individual sample.

a. Number of job-match quarters, where the total number of job-match quarters (1,011,408) consists of 228,588 unique job matches, representing 112,903 individuals and 95,355 firms.

have initiated it, as a quit or a layoff, we are unable to observe how the separation was initiated. The distinction between a quit and a layoff is not clear cut, however, and the presence of a quit may eliminate the need for a layoff.[35]

It is important to note that our turnover rates represent an upper bound on permanent separations and on temporary separations that last at least one quarter. Recall that if a firm changes its FEIN, or there is a clerical error in recording the FEIN, we will incorrectly conclude that both a permanent separation and a new accession occurred. Alternatively, if exactly one-quarter is missing due to firm oversight, we will incorrectly classify that as a temporary separation and recall. We are confident, though, that this upper bound is reasonably close to the true value, since it is possible to investigate the importance of misclassifications. In the first case we would expect to see a worker with a change in FEIN but other firm characteristics remaining similar. Of all permanent separations about 10 percent involve no change of four-digit industry. Only 2 percent, though, have both no change in industry and have firm employment levels within 20 percent of the previous level. Ruling out true industry changes implies that our quarterly separation rates are 1.7 percentage points too high, while ruling out true industry changes combined with employment changes greater than 20 percent implies that our actual overstatement is only 0.3 percentage point.[36] Similarly, 15 percent of all separations are temporary separations with no unemployment insurance received. In the unlikely case that these are all recording errors, our separation rates will be 3.5 percentage points too high.[37] Even in the worst-case scenario, however, comparisons across groups would be valid as long as these sorts of misclassifications are uncorrelated with firm characteristics.

As a final check, we can make a rough comparison with the turnover rates previously published by the BLS in 1980, a year in which each of

35. The theory of efficient turnover, for example, posits that all separations are essentially joint decisions. If a higher wage would prevent a quit, by not offering that wage, the firm implicitly caused the separation. See McLaughlin (1991) for a recent discussion.

36. Given our permanent separation rate of 0.1723, these are calculated as .10*.1723 and .02*.1723.

37. This is based on our total separation rate of 0.2304 and thus is calculated as .15*.2304.

our states had data.[38] In both the CWBH and the BLS data it is possible to separate our new hires from total accessions, so we will focus on accessions rather than separations. The biggest difference between the CWBH and the BLS survey data, however, is the sample composition. In order to approximate the oversampling of large firms by the BLS, we calculate accession rates for manufacturing firms with 1,000 or more employees. Over half of the CWBH manufacturing wage records come from firms of this size. A final difference is that the BLS rates are monthly, while the CWBH rates are quarterly. Multiplying the BLS numbers by 3 to approximate quarterly rates, we obtain a new hire rate of 6.3 and an overall accession rate of 10.5 percent. The new hire rate in this sample of large manufacturing firms from the CWBH is amazingly similar (6.3 percent as well). Overall accessions, however, are appreciably higher in the CWBH data (15.9 pecent). If we exclude all temporary layoffs that are identified without unemployment insurance (and hence may reflect recording errors), the overall accession rate from the CWBH data drops, but only to 12.0 percent. Thus it is clear that the CWBH data identify more turnover than do the BLS survey data, although the difference is mainly in the recall rate. Remember that because of the voluntary nature of the BLS survey, turnover may be understated.

The Determinants of Turnover

Consider now the turnover rates by major industry group in table 2. It is clear that manufacturing is quite different from the other industries, especially from the other large industries of retail trade and services. The 11 percent rate of permanent separation in manufacturing is lower than in all industries outside of the public sector. At the same time, the 9 percent rate of temporary separation is higher than in any other industry except for the highly seasonal agriculture sector. These numbers result in a total separation rate that is just slightly below that for the economy overall (20 percent compared with 23 percent). By contrast, the 22 percent overall separation rate in services is composed of a 17 percent permanent rate and a 5 percent temporary rate. In retail trade the difference from manufacturing is even more pronounced, with a 23 percent permanent rate and only a 4 percent temporary rate.

38. See U.S. Department of Labor (1982).

Compared with other industries, manufacturing is more likely to rely on temporary layoffs and less likely to induce permanent separations. This result is consistent with the main theories of turnover reviewed above, since many of the conditions under which permanent separations are likely to be lower are characteristic of manufacturing. For example, the use of compensation schemes designed to increase worker productivity is often associated with firms with high levels of capitalization, where monitoring of activity is more important than in other industries. Studies investigating the role of training in wage growth provide weak evidence that firm-specific capital may be more important in manufacturing than in other industries.[39] Manufacturing also is more highly unionized than are most industries. In addition to the possible role of increased voice in discouraging turnover, unions are associated with higher levels of nonwage benefits.[40] Some union contracts may explicitly specify the use of temporary layoffs as a means of dealing with fluctuating demand, or at least preclude the cutting of wages or hours. Manufacturing firms are also generally larger than firms in other industries. If the inverse relationship between firm size and job reallocation that others have found in manufacturing holds for other industries, we would expect higher turnover outside of manufacturing for this reason.[41] An increased probability of survival would reinforce the incentives to form long-term attachments and thus may provide another reason why manufacturing firms would be more disposed to structuring compensation in a manner conducive to forming such relationships.

From the firm size data in table 2, it is clear that the largest firms, which are most represented in the BLS survey, are not representative of the overall economy. While firms with fewer than 100 employees have permanent separation rates of close to 22 percent, the rate for firms with 500 to 1,999 employees is under 13 percent, and for firms with 2,000 or more employees the rate is just 8 percent. Temporary separation rates are similar for all the size classes (between 5 and 6 percent in each case). A similar pattern emerges from the payroll class data in table 2. The payroll class is defined by taking total wages (in $1,000s) paid in the quarter divided by average monthly employment over the

39. Brown (1989, p. 975) shows that manufacturing jobs require the highest levels of training.

40. See Freeman (1981).

41. See Dunne, Roberts, and Samuelson (1989b) for an example of this result.

quarter. Here the 30 percent permanent separation rate for the lowest payroll class is well above the 17 percent rate overall, and it is even quite a bit higher than the 24 percent rate for the next lowest payroll class. After bottoming out at a 12 percent rate for the $5,000 to $7,500 class, rates rise somewhat for those firms with average payrolls of $7,500 or more. While the cause of this increase is not clear, it may be due to changes in the industrial composition of the classes.

The final part of the table divides job matches into two categories: those that have lasted at least one year and those that have lasted less than one year.[42] Interestingly, over 40 percent of the observations are of job matches that have lasted less than a year. As might be expected, these job matches have very high permanent separation rates of over 30 percent. By contrast, a job match that has lasted a full year has only a 7 percent chance of permanently dissolving. Again temporary rates are similar across the classes, hovering around 6 percent.

The Cyclicality of Turnover

An additional area of interest is to examine turnover patterns over the business cycle. Since the sampling period differs across our states, a simple comparison of rates over time would be somewhat misleading as the sample composition changes. Therefore table 3 presents quarterly turnover by state and year, along with the average monthly unemployment rate over the year. Several patterns do appear in this table. First, for most states, temporary turnover tends to be higher when permanent separations are lower and vice versa. This reflects the procyclical nature of quits, which tend to fall in recessions. Additionally, new hires tend to be lowest in the high unemployment years of 1982 and 1983, which is also to be expected. Note that total separations do not tend to be highest in these years, implying that the drop in voluntary separations is larger than the increase in layoffs during a recession. Thus from table 3 total separations seem procyclical.

We can explore the cyclical properties of turnover more formally within a regression framework. For each state we have calculated a time series of quarterly turnover rates. These rates are then used as the

42. As noted in the appendix, the sample used for the analysis in this section is restricted to observations from one year or more into the data collection period. This allows us to determine if the job has lasted less than a year.

Table 3. Quarterly Turnover Rates by State and Year

State and year	Number of observations[a]	Permanent separations	New accessions	Separations		Unemployment rate (%)
				Temporary	Total	
Georgia						
1979	26,358	0.2177	0.2384	0.0586	0.2763	5.10
1980	26,150	0.2026	0.2065	0.0641	0.2668	6.45
1981	25,836	0.2003	0.1944	0.0608	0.2611	6.36
1982	24,598	0.1788	0.1700	0.0734	0.2522	7.79
Idaho						
1979	15,575	0.1843	0.1828	0.0619	0.2463	5.72
1980	29,538	0.1548	0.1455	0.0630	0.2178	7.80
Louisiana						
1980	22,265	0.2078	0.1353	0.0770	0.2847	6.68
1981	45,223	0.2027	0.2190	0.0736	0.2762	8.41
1982	42,677	0.1764	0.1638	0.0655	0.2419	10.34
1983	41,663	0.1623	0.1443	0.0544	0.2167	11.78
Missouri						
1979	30,313	0.1528	0.1562	0.0583	0.2111	4.55
1980	28,988	0.1444	0.1359	0.0730	0.2174	7.02
1981	27,715	0.1408	0.1339	0.0797	0.2204	7.68
New Mexico						
1980	41,520	0.2267	0.2303	0.0549	0.2816	7.38
1981	41,865	0.2287	0.2216	0.0537	0.2824	7.31
1982	40,687	0.2148	0.2020	0.0540	0.2688	9.12
Pennsylvania						
1980	31,617	0.1263	0.1308	0.0914	0.2178	7.78
1981	41,134	0.1300	0.1189	0.0701	0.2000	8.36
1982	40,030	0.1164	0.1067	0.0835	0.1999	10.92
South Carolina						
1979	30,264	0.1840	0.1845	0.0410	0.2250	4.96
1980	38,529	0.1643	0.1561	0.0508	0.2151	6.88
1981	37,642	0.1534	0.1416	0.0559	0.2093	8.39
1982	35,090	0.1377	0.1208	0.0863	0.2240	10.83
Washington						
1980	23,505	0.1900	0.1954	0.0593	0.2493	7.49
1981	46,221	0.1773	0.1747	0.0536	0.2308	9.53
1982	44,278	0.1651	0.1502	0.0564	0.2215	12.13

Sources: Authors' calculations based on individual sample. Civilian unemployment rate is from Bureau of Labor Statistics, *Geographic Profile of Employment and Unemployment*, annual editions. U.S. Department of Labor.

a. Number of job-match quarters. Data from the last year of collection are excluded, since temporary layoffs cannot be determined without subsequent data.

Table 4. Cyclicality of Quarterly Job Turnover

Quarterly state rate used as dependent variable[a]	*Coefficient on percent unemployed in state*[b]
Permanent separations	−0.0058
	(0.0006)
New accessions	−0.0126
	(0.0010)
Temporary layoffs	0.0017
	(0.0006)
Total separations	−0.0041
(permanent separations + temporary layoffs)	(0.0009)
Total permanent turnover	−0.0184
(permanent separations + new accessions)	(0.0014)

Source: Authors' calculations based on individual sample.

a. We do not include a row for recalls because it is difficult for us to determine the timing of some recalls. All regressions also include state dummy variables and quarterly seasonal dummy variables. N = 135.

b. Average of state monthly rates over the quarter. Standard errors are in parentheses.

dependent variable in a regression where the independent variables are the state's average monthly unemployment rate over the quarter, a set of state dummy variables, and a set of seasonal quarterly dummy variables. The results, presented in table 4, confirm the findings of table 3. Both the permanent separation rate and the new accession rate, and hence total permanent turnover, are found to be strongly procyclical. By contrast, temporary layoffs are only slightly countercyclical, so that total separations remain procyclical. Because we cannot easily determine the quarter of return from a short temporary layoff, we focus on total separations and total permanent turnover, rather than total accessions and total turnover.

The Distribution of Turnover across Workers

The high rate of turnover for jobs that have lasted less than a year is consistent with the continued movement of some workers from one short job to another, while others remain in a relatively stable job. Using data from interrupted job tenures, Akerlof and Main estimate that whereas the average job lasts only a short time, the average person is in a job of long duration.[43] In a similar exercise Hall determines that the median person is in a job that will last for about eight years, and that 28 percent of people are in a job that will last twenty years or

43. See Akerlof and Main (1981).

more.[44] The distinction between jobs and individuals can be seen clearly in the CWBH data. For each person we have calculated the total number of overall separations and permanent separations per quarter, per year, and for a three-year period.[45] Additionally, we have calculated the number of different job matches per person over the three years. This distribution is presented in the top panel of table 5. More than 59 percent of the individuals are observed at only one job over the three-year period; another 21 percent have just two different employers.

The number of jobs held, while informative, does not reveal the true extent of turnover. In the next panel of table 5 we see that about 23 percent of the people separate one or more times from a job during a quarter, with 17 percent separating permanently at least once. If the probability of separating in a quarter is 23 percent, and the probability is independent over time for a given worker, we would expect in the course of a year to see 35 percent of the people never separating.[46] Clearly, this is not the case. Rather, over 47 percent of the individuals do not permanently separate at all over a year. Additionally, while the permanent separation rate of 17 percent would imply that only 47 percent of the individuals would not leave their jobs over the year, we instead see 58 percent remaining at the same job. Over the three-year period, 21 percent of the sampled individuals do not separate at all, and almost 31 percent never separate permanently. Under the assumption of independence over time, the quarterly rates would imply that there would be more than a 95 percent chance of some separation over three years and almost a 90 percent chance of a permanent separation.

Clearly, then, independence is an untenable assumption. Instead, there are people with different degrees of job stability in the population. Some people have a very low probability of separating, while others have a high probability and experience a large share of total turnover. The final panel of table 5 confirms this assessment. Fifty-five percent of total turnover is accounted for by those individuals with three or more separations during the three years. Recall from the previous panel that this is just 21 percent of the individuals. Temporary layoffs are not

44. See Hall (1982).

45. Here we have limited the sample to twelve quarters for each state in order to have a balanced three-year panel.

46. Given that the probability of not separating in a quarter is 0.77, the probability of not separating in each of the four quarters of a year is $(0.77)^4$.

the main source of this turnover, since 43 percent of permanent turnover is accounted for by those with three or more permanent separations (just under 13 percent of the individuals).

While it is true that the average person is much more stable than the average job, we find that 69 percent of the individuals in our sample permanently leave a job at least once, although about 43 percent leave once and only once. This seems to reflect a labor market that is somewhat more unstable than that documented by Hall, who found that 28 percent of current workers are in a job that will last over twenty years.[47] An important difference between our study and his may be the treatment of those with low labor-market attachment. Hall's work is based on a special supplement to the CPS, in which only those *currently* working are asked about their tenure on the job. The CWBH data will include all those *ever* working over the sample period. This difference can be significant. While we find that just under 31 percent of the sampled individuals never permanently leave a job, over 59 percent of workers have only one job over this three-year period. This fact implies that a significant number of people enter or leave the labor force, or enter or leave our sample, by moving across state lines or becoming self-employed.

Multivariate Analyses

Given the concentration of turnover among certain people, the obvious question is whether firm characteristics are important predictors of a job match dissolving, or if personal characteristics are the only important factor. The pattern we observe might occur if unstable workers sorted themselves into jobs at smaller, lower paying firms in industries such as retail trade. To properly sort out the effects of size, wage level, tenure, and industry, we control for all these factors together through the use of a linear probability model.[48]

The dependent variable is either a 1 if the job match dissolves (permanently or temporarily) or 0 if it remains intact. An observation is

47. See Hall (1982, p. 720).

48. A logit or probit model would generally be the method of choice in this situation. However, since we would have well over 100,000 individual dummy variables to include when we do fixed effects estimation, these techniques are impractical. Since most of the separation probabilities are neither extemely high nor low, a linear approximation is not likely to lead us too far astray.

Table 5. Distribution of Job Turnover across Individuals

Number of jobs over three-year period[a]	*Number of persons*	*Percent*	*Cumulative*
1	65,720	59.41%	59.41%
2	22,976	20.77%	80.18%
3 or more	21,926	19.82%	100.00%
Total	110,622		

Number of separations per quarter	*Overall separations*			*Permanent separations*		
	Number of person quarters	*Percent*	*Cumulative*	*Number of person quarters*	*Percent*	*Cumulative*
0	547,315	76.53%	76.53%	591,184	82.66%	82.66%
1	154,169	21.56%	98.08%	113,523	15.87%	98.54%
2	11,875	1.66%	99.75%	9,186	1.28%	99.82%
3 or more	1,822	0.25%	100.00%	1,288	0.18%	100.00%
Total	715,181			715,181		

Number of separations per year	*Overall separations*			*Permanent separations*		
	Number of person years	*Percent*	*Cumulative*	*Number of person years*	*Percent*	*Cumulative*
0	116,216	47.23%	47.23%	143,399	58.27%	58.27%
1	93,618	38.04%	85.27%	79,662	32.37%	90.64%
2	24,869	10.11%	95.37%	16,310	6.63%	97.27%
3 or more	11,386	4.63%	100.00%	6,718	2.73%	100.00%
Total	246,089			246,089		

Number of separations over three-year period[a]	*Overall separations*			*Permanent separations*		
	Number of persons	*Percent*	*Cumulative*	*Number of persons*	*Percent*	*Cumulative*
0	23,446	21.19%	21.19%	33,735	30.50%	30.50%
1	45,375	41.02%	62.21%	47,662	43.09%	73.58%
2	18,643	16.85%	79.07%	15,202%	13.74%	87.32%
3 or more	23,158	20.93%	100.00%	14,023	12.68%	100.00%
Total	110,622			110,622		
Number of separations over three-year period[a]	*Number of total separations*		*Percent*	*Number of permanent separations*		*Percent*
Fewer than 3	82,661		44.90%	78,066		57.33%
3 or more	101,435		55.10%	58,110		42.67%
Total	184,096		100.00%	136,176		100.00%

Source: Authors' calculations based on individual sample.

a. Sample period is twelve quarters. (Georgia, 79:1–81:4; Idaho, 79:1–81:4; Louisiana, 81:1–83:4; Missouri, 79:1–81:4; New Mexico, 80:1–82:4; Pennsylvania, 80:1–82:4; South Carolina, 79:1–81:4; Washington, 80:1–82:4.)

Table 6. Linear Probability Models for Probability of Separating in Quarter

Independent variable[a]	*Total separations*	*Total separations*	*Permanent separations*	*Permanent separations*	*Temporary separations*	*Temporary separations*
Quarterly payroll per worker ($1,000s)						
Less than 1	0.099	0.082	0.084	0.081	0.015	0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
1–2.5	0.053	0.047	0.045	0.049	0.008	−0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
2.5–5	0.004	0.007	0.005	0.013	−0.001	−0.006
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
5–7.5	−0.020	−0.012	−0.013	−0.006	−0.007	−0.006
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Firm size						
Fewer than	0.041	0.104	0.033	0.095	0.008	0.009
20 employees	(0.002)	(0.003)	(0.001)	(0.003)	(0.001)	(0.002)
20–99	0.042	0.083	0.040	0.079	0.002	0.004
	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
100–499	0.034	0.063	0.030	0.055	0.004	0.007
	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
500–1999	0.016	0.035	0.012	0.027	0.004	0.008
	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
Tenure at firm						
Less than 1 year	−0.200	0.002	−0.198	−0.004	−0.002	0.007
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unemployment rate (%)	0.006	0.008	0.001	0.003	0.005	0.006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)
Individual effects	No	Yes	No	Yes	No	Yes
N (job-match quarters)	1,011,408	1,011,408	1,011,408	1,011,408	1,011,408	1,011,408
R^2	0.108	0.324	0.120	0.338	0.021	0.202

Source: Authors' calculations based on individual sample.

a. All models also include two-digit SIC industry, state, and calendar quarter effects. Standard errors are in parentheses.

each quarter that we observe a worker at a job and have data available for the next quarter (so that we can determine if she stays on the current job into the next quarter). A binary choice model estimated in this way with an observation for each quarter is a type of discrete time hazard model. Explanatory variables include a full set of two-digit SIC industry indicators, dummy variables for the size and payroll classes defined in table 2, an indicator for job tenure of over one year, the average monthly unemployment rate in the state over the quarter, state dummy variables, and separate indicators for each calendar quarter of the sample.[49] As implied by the simple tables presented earlier, each of the classes of variables we include is significant in predicting turnover. More important, when individual fixed effects are included, this significance remains.

Table 6 presents the coefficients and standard errors for the size and payroll class variables, the tenure indicator, and the unemployment rate.[50] Results are presented for overall separations, as well as separately for permanent and temporary separations. Looking first at the role of firm size, we see in the first regression that the largest class is significantly different from all others, with the two smallest classes having separation rates that are about 0.03 higher than the largest class, and even the second largest is almost 0.02 higher. When individual fixed effects are included in the model, the largest class remains significantly different, and the magnitude of the effect is actually greater. Additionally, the effect declines monotonically with firm size, from 0.10 to 0.08 to 0.06 to 0.04.

Turning to the role of payroll class, we see that the addition of individual effects hardly alters the coefficients. The effect of the lowest class decreases from about 0.10 to 0.08, and the others change only slightly. As might be expected, allowing for individual effects dramatically alters the role of tenure. Without these effects, jobs lasting a year or more are estimated to be 0.20 less likely to dissolve, but with them

49. An additional determinant of separations is the degree of UI experience rating a firm faces, but this issue is too complex to be properly covered in this paper. For an in-depth discussion of the estimation of experience rating effects on layoffs using these data, see Anderson and Meyer (1993b).

50. The standard errors of our estimates are likely to be understated (especially in the models without individual fixed effects) because of dependence between the observations for a given individual and firm. Recall that there are only 112,903 unique individuals, implying on average about nine observations per worker.

they are 0.002 more likely to dissolve. This positive effect of tenure actually stems from its role in generating temporary layoffs. Permanent separations remain 0.004 less likely to occur, while temporary separations are 0.007 more likely to occur. One should use caution in interpreting these fixed effects estimates, however. Since tenure is not exogenous, the fixed effects estimates when we control for tenure are likely to be biased. However, if we repeat the models with fixed effects from table 6 without including tenure, the results are essentially unchanged.[51]

While the firm characteristics remain significant in the linear probability models even with the inclusion of individual effects, it is informative to more formally analyze how two-digit SIC industry, firm, and individual characteristics affect the probability of a separation. After controlling for state and quarter, we allow random effects for industry, firm, and individual to assess the relative importance of these factors. Let p_{jt} be equal to one if a separation occurs for person j, in quarter t, and 0 if a separation does not occur. Then we take the probability that $p_{jt} = 1$ to be determined by the equation

$$\text{Prob}[p_{jt} = 1] = S_j + Q_t + \epsilon_I + \epsilon_F + \epsilon_j,$$

where S and Q represent state and calendar quarter effects, and ϵ_I, ϵ_F, and ϵ_j are error components related to the industry, firm, and individual, respectively. Thus we estimate

$$p_{jt} = S_j + Q_t + \epsilon_I + \epsilon_F + \epsilon_j + \epsilon_{jt}$$

and determine the variance of ϵ_I, ϵ_F, and ϵ_j as well as the variance of the idiosyncratic error, ϵ_{jt}. Due to the computation time required for this analysis, we restrict ourselves to a random subsample of individuals who experience just over 15,000 job-match quarters, and we use a minimum variance quadratic unbiased estimation (MIVQUE) method.[52] When analyzing total turnover, we find that industry, firm, and individual account for 5.6, 7.6, and 7.3 percent of the variance, respectively. For permanent turnover, the corresponding numbers (not reported) are 5.9, 5.3, and 5.9 percent. This result indicates that industry, firm, and individual characteristics are of roughly equal importance. The corre-

51. Of all the coefficients reported in table 6, just four are different and then only in the third digit (not reported).

52. See Hartley, Rao, and LaMotte (1978).

sponding numbers for temporary turnover are 0.8, 3.9, and 4.6 percent and thus indicate that individual and firm are much more important determinants of temporary separations than is industry. In all cases, though, more than 75 percent of the variance is attributable to the idiosyncratic error term. Overall, there is no simple story of one factor being the dominant influence on turnover.

Some Implications and Further Results

While specific characteristics of individuals are clearly a major source of variance in turnover rates, firm characteristics, such as size and payroll per worker class, have implications for the theories of turnover discussed above. Recall that a major source of decreased turnover in the models of worker mobility is the receipt of wages on the current job that are above the market alternative. It is not surprising, then, that we find a negative relationship between wages and turnover. However, the question remains why a worker may be receiving a wage above her alternative; possibilities include investment in firm-specific human capital or the use of efficiency wages or other incentive compensation schemes. The need to use compensation as an incentive system is often associated with monitoring difficulties. In fact, the tendency for large firms to pay higher wages than small firms is typically attributed to such difficulties.[53] Here wages and firm size both have a negative effect on turnover, even when we control for the other. In addition to paying higher wages, large firms are more likely to provide training.[54]

Although the effect of higher wages on turnover may occur as part of incentive schemes, the effects on turnover of both wage and firm size are consistent with the role played by the accumulation of firm-specific human capital. The negative effect of tenure is consistent with this effect of firm-specific human capital investment. Note, however, that the theory of job shopping, where workers search for a high-quality match, could also imply this result. In addition, firm size could serve as a proxy for such things as greater unionization, more internal advancement options, or the use of deferred compensation in the form of pensions, which would also imply lower turnover.

53. Examples of this idea go back at least as far as Coase (1937).
54. See Baron, Black, and Lowenstein (1987) and Holtmann and Idson (1991).

As was noted above, a negative relationship between tenure and turnover will be implied by both the accumulation of firm-specific capital and by learning about job-match quality. In order to explore the role of tenure more closely, we limit our sample to job matches that begin during the sample period. First we calculate separation hazard rates by quarters of tenure on the job. As seen in the top half of table 7, there is a striking monotonic decline in the permanent separation rate as the number of quarters on the job increases. Assuming a uniform start and end date for jobs within a quarter, jobs ending in the same quarter in which they began would be on average about three weeks long, those ending in the second quarter of employment would be on average about three months long, and those ending in the third quarter would be on average about six months long, and so on. These results suggest a decline in turnover with tenure even at very short durations, although the decline in the first two quarters, while statistically significant, is not especially large in magnitude.

Our results differ from those of Farber using the NLSY; he found that turnover was highest three months after a job started.[55] Differences in samples and methods may explain the differences in our results. Our findings do support earlier theoretical arguments that turnover will decline as more match-specific capital is accumulated on the job. However, the observed decline in the hazard rate could also be due to heterogeneity across workers in their underlying separation rates.

In the bottom half of table 7, we investigate the role of tenure while controlling for other characteristics. Here we estimate a linear probability model with firm characteristics as controls, and we use dichotomous variables for quarters on the job as explanatory variables. The results generally confirm the impressions from the top panel. The probability of separating (both permanently and overall) declines dramatically over time when compared with the first quarter. While the drop in the second quarter is relatively small, it is strongly significant in each case. Table 7 also investigates the effect of tenure on the rate of temporary separations. In both panels the temporary separation rate first rises, reaching a peak in the fourth quarter of employment, and then

55. See Farber (1993b, p. 48). Farber examines whether the paucity of short spells in his data could be due to underreporting of such spells, since respondents are asked to recall their jobs over the past year. He finds some evidence of underreporting of the shortest spells, but overall the evidence is mixed.

Table 7. Quarterly Separation Hazard Rates

Quarters of tenure	*Risk set (number of observations)*[a]	*Permanent separation rate*	*Temporary separation rate*	*Total separation rate*
1	169,579	0.3898	0.0450	0.4348
2	108,646	0.3445	0.0648	0.4093
3	75,598	0.2310	0.0711	0.3022
4	63,889	0.1845	0.0727	0.2572
5	56,280	0.1397	0.0677	0.2074
6	46,133	0.1209	0.0660	0.1869
7	38,279	0.1055	0.0602	0.1657
8	32,252	0.0942	0.0578	0.1520
9	27,310	0.0867	0.0579	0.1446
10	23,303	0.0833	0.0603	0.1436
11	19,619	0.0726	0.0544	0.1270
12	16,607	0.0699	0.0524	0.1223
13	13,953	0.0621	0.0544	0.1165
14	11,534	0.0597	0.0520	0.1118
15	9,358	0.0558	0.0493	0.1050
16	7,922	0.0573	0.0485	0.1058
		Coefficient (standard error)[b]		
2		−0.035	0.022	−0.013
		(0.002)	(0.001)	(0.002)
3		−0.139	0.029	−0.110
		(0.002)	(0.001)	(0.002)
4		−0.178	0.031	−0.147
		(0.002)	(0.001)	(0.002)
5		−0.217	0.025	−0.191
		(0.002)	(0.001)	(0.002)
6		−0.233	0.024	−0.208
		(0.002)	(0.001)	(0.002)
7		−0.242	0.021	−0.221
		(0.002)	(0.001)	(0.003)
8		−0.254	0.019	−0.235
		(0.002)	(0.001)	(0.003)

Source: Authors' calculations based on individual sample.

a. Job-match quarters. Only job matches observed to start in the sample period are included.

b. From regression controlling for state, industry, firm size, average payroll per worker, and calendar quarter. One quarter of tenure is the omitted class.

levels out, although it always remains above the initial level. That temporary layoffs do not appreciably decline with tenure is supportive of models of long-term attachment in which temporary layoffs are a valuable part of the compensation package. Workers may value these layoffs if they allow them to obtain some leisure and to receive unemployment benefits during periods of low firm labor demand.

We also briefly explore the role of seasonality by substituting separate year and quarter dummy variables for the full set of calendar quarter variables used in generating table 6.[56] Here we find that overall separations are least likely to occur in the first quarter, and they become increasingly likely through the fourth quarter.[57] Looking only at temporary separations, however, we find the opposite result. Temporary separations are instead most likely to occur in the first quarter and least likely to occur in the third and fourth quarters.[58] Previous evidence on seasonal cycles has found that unemployment is highest in the first quarter, and it declines through the fourth quarter, with employment rising through the year.[59] Recall that in tables 3 and 4 temporary turnover was generally positively related to the unemployment rate for the state, while total separations were negatively related. Thus the behavior of turnover over the seasonal cycle appears to be similar to that over the business cycle. Such a conclusion of strong similarities across seasonal and business cycle frequencies has also been found in previous work.[60]

The Costs of Job Turnover

The costs of turnover can be measured in many ways. From the point of view of the workers, it is typical to consider the unemployment and

56. The quarterly unemployment rate is not included since the quarterly dummies are meant to proxy for cyclical effects at seasonal frequencies. When the unemployment rate is included, results for permanent separations are unchanged, while coefficients for temporary separations are no longer significant.

57. With the fourth quarter as the excluded category, the coefficients are −.04, −.02, and −.01 for the first, second, and third quarters respectively. All are significantly different from zero.

58. With the fourth quarter as the excluded category, the coefficients are .004 and .001 for the first and second quarters respectively, and they are significant. The coefficient for the third quarter is essentially zero.

59. See Barsky and Miron (1989).

60. See Barsky and Miron (1989) and Beaulieu, MacKie-Mason, and Miron (1992).

earnings losses that result from a separation as capturing the cost.[61] Focusing on these sorts of losses implicitly assumes that involuntary displacement is the main source of costs from turnover. Another common approach is to focus on losses due to distortions from the unemployment insurance system, which provides a subsidy to layoffs.[62] This work is typically embedded in an implicit contract framework, thus implicitly assuming that long-term worker attachments are the norm. Less common is an explicit emphasis on the role of turnover in generating adjustment costs to firms, although this is clearly a background assumption for turnover efficiency wage models. While measuring the actual dollar costs of such things as recruiting and training is difficult, it is clear that at high rates of turnover they may be a significant part of the total costs.

We explore the costs of turnover by considering the worker's unemployment and earnings experience. Because we condition on reemployment during our sample period, we will miss some people with extremely long unemployment spells. Table 8 presents the distribution of separations by earnings weeks lost for all separations for which we observe reemployment. Overall, 48 percent of these separations result in less than two lost earnings weeks, while for permanent separations the percentage is even higher (52 percent). At the same time about 9 percent of the permanent separations result in over a year of lost earnings weeks. Also clear from table 8 is the occurrence of false temporary layoffs because a firm neglects to send in quarterly wage records. The unusual increases in temporary layoffs at quarterly intervals can be attributed to this problem, as was discussed earlier. Recall that 15 percent of all separations are temporary separations during which no unemployment insurance was received.

Unfortunately, it is somewhat difficult to assess the validity of our loss measure, since it is not strictly comparable to most estimates in the literature. A simple first step is to calculate our loss measure for those individuals who do not separate. For 72 percent of these observations, we would estimate the correct zero weeks lost, while less than

61. See Jacobson, LaLonde, and Sullivan (1993) and Farber (1993a).

62. See Feldstein (1975, 1978) and Topel (1983) for examples that focus on increases in unemployment, and see Anderson and Meyer (1993c) for an example that focuses directly on the deadweight loss.

Table 8. Distribution of Lost Earnings Weeks from Separations Ending in Reemployment

	Number and percentage of separations		
Time period	*Total*	*Permanent*	*Temporary*
Less than 2 weeks	100,311	73,065	27,246
	48.42%	52.37%	40.26%
2 to 4 weeks	17,990	10,861	7,129
	8.68%	7.78%	10.53%
1 to 2 months	15,877	10,798	5,079
	7.66%	7.74%	7.51%
2 to 3 months	10,545	7,421	3,124
	5.09%	5.32%	4.62%
3 to 4 months	18,252	7,217	11,035
	8.81%	5.17%	16.31%
4 to 5 months	4,426	2,716	1,710
	2.14%	1.95%	2.53%
5 to 6 months	4,234	2,567	1,667
	2.04%	1.84%	2.46%
6 to 7 months	7,890	4,377	3,513
	3.81%	3.14%	5.19%
7 to 8 months	2,622	1,489	1,133
	1.27%	1.07%	1.67%
8 to 9 months	2,423	1,500	923
	1.17%	1.08%	1.36%
9 to 10 months	4,875	3,112	1,763
	2.35%	2.23%	2.61%
10 to 11 months	1,230	863	367
	0.59%	0.62%	0.54%
11 to 12 months	1,195	907	288
	0.58%	0.65%	0.43%
More than 1 year	15,318	12,625	2,693
	7.39%	9.05%	3.98%
Total	201,788	139,518	67,670

Source: Authors' calculations based on individual sample.

13 percent lose more than two weeks, for an average of one lost week.[63] This result indicates that almost all of the earnings losses that we observe following separations are real and would not occur without a separation. Furthermore, it is likely that most of the losses reflect unemployment rather than a decline in weekly earnings; previous work

63. Some of these individuals may suffer short temporary unemployment that is not UI compensated, and hence they may have nonzero losses. Recall that we are unable to observe these spells.

has found much smaller declines in weekly earnings. For example, Farber estimates that displaced workers' weekly reemployment earnings are only 13 percent lower.[64] Even these losses may be an overstatement for our sample, since the majority is voluntary separations, for whom lower weekly earnings are likely to be less important than for displaced workers.

As a final check, we compare our loss measure with past work, although it is difficult to consider officially published unemployment figures, since there are major differences in the concepts being compared. First, it is necessary to restrict our sample to only those incurring nonzero losses, since we identify all separations, not just those separations resulting in unemployment. Applying this restriction results in an average loss of twenty-four weeks, with a median loss of thirteen weeks. While these numbers may seem high, it is important to recall that the CWBH data do not differentiate between being unemployed and being out of the labor force. Thus our sample will include spells such as those of discouraged workers and individuals on personal leave, which are likely to be longer.[65] Additionally, published unemployment figures refer to the average length of spells in progress, not the average length of a completed spell. Clark and Summers's estimate of completed spell length, which tries to take into account the effect of discouraged workers, is perhaps most comparable to our measure.[66] They estimate that in 1975 this average was 18.8 weeks. Recall, however, that the presence of false spells from missing quarters is likely to bias up our estimates.[67] Overall, then, our estimate of the level of losses is likely to be somewhat overstated, but the analysis of relative losses likely remains valid.

While it is significant that 52 percent of permanent separations incurred very little if any lost earnings weeks, the long right tail results in an average of fourteen weeks lost for each separation, even though the median is just one week, as seen in table 9. Table 9 also presents the mean and median earnings-weeks lost per separation for several

64. See Farber (1993a, p. 110).

65. For example, seasonal workers will appear as unemployed in the off-season, even if they are not actually looking for work, and hence would not be counted in official statistics.

66. See Clark and Summers (1979, p. 36).

67. If we exclude all spells exactly divisible by thirteen, the mean loss drops to nineteen weeks.

Table 9. Earnings Weeks Lost after Separations Followed by Reemployment by Firm Size, Payroll per Worker, and Industry

Classification	*Total separations*			*Temporary separations*			*Permanent separations*		
	Number of observations	*Mean weeks lost*	*Median weeks lost*	*Number of observations*	*Mean weeks lost*	*Median weeks lost*	*Number of observations*	*Mean weeks lost*	*Median weeks lost*
Total	207,188	13.14	2	67,670	11.50	4	139,518	13.94	1
Firm size									
Fewer than 20 employees	57,268	13.56	2	15,817	13.39	9	41,451	13.62	1
20–99	55,959	12.92	2	15,032	11.77	5	40,927	13.35	1
100–499	47,539	12.75	2	15,828	10.76	3	31,711	13.74	1
500–1999	29,222	13.38	2	11,946	10.98	3	17,276	15.05	1
2,000 or more	17,200	13.12	2	9,047	9.69	3	8,153	16.93	2
Quarterly payroll per worker ($1,000s)									
Less than 1	16,342	14.04	1	3,813	13.25	4	12,529	14.28	0
1–2.5	78,544	14.24	2	20,694	13.51	6	57,850	14.50	1
2.5–5	76,048	12.87	2	28,331	10.90	4	47,717	14.04	1
5–7.5	24,767	10.94	2	10,506	8.83	3	14,261	12.49	1
7.5 or more	11,487	10.86	2	4,326	10.67	4	7,161	10.97	0
Industry									
Agriculture	6,274	11.73	0	2,024	11.09	2	4,250	12.04	0
Mining	5,461	12.62	3	1,754	12.79	9	3,707	12.54	1
Construction	26,909	11.66	2	7,415	10.65	4	19,494	12.05	1
Manufacturing	43,236	11.59	2	22,709	7.49	2	20,527	16.12	2
Transportation/ communications	9,403	11.37	1	3,644	10.33	4	5,759	12.02	0
Wholesale trade	10,510	13.34	3	3,060	12.43	9	7,450	13.71	1
Retail trade	46,301	14.31	2	8,959	14.27	11	37,342	14.32	1
Finance, insurance, and real estate	7,088	13.71	2	1,887	13.92	13	5,201	13.63	1
Services	47,165	14.38	2	14,140	15.61	13	33,025	13.86	0
Public sector	4,841	16.49	4	2,078	16.16	13	2,763	16.73	1

Source: Authors' calculations based on individual sample.

classes of firms. Permanent separations from the largest firms result in a larger mean number of lost earnings weeks. For firms with more than 2,000 employees, almost seventeen weeks are lost. Similarly, for firms with 500 to 1,999 employees, average weeks lost are fifteen. This is in contrast to firms with fewer than 500 employees; their average is between thirteen and fourteen weeks lost. By contrast, the highest payroll classes result in a slightly lower number of lost weeks than do the lowest payroll classes. Differences across industries are also apparent, with manufacturing being especially above average at sixteen weeks. The higher number of earnings weeks lost in manufacturing is perhaps not surprising, given the earlier findings of a lower incidence of permanent turnover, since this result remains consistent with the theories discussed earlier. Since voluntary turnover is expected to be lower, the separations are more likely to be the result of displacements. Because these displaced workers are likely to have invested in firm-specific human capital or to have received above-market compensation designed to reduce turnover, finding a comparable paying new job may well be difficult.

As might be expected, temporary layoffs result in fewer mean weeks lost (just over eleven weeks on average), although the median loss is higher (four weeks). Recall that these losses are likely to be slight overestimates, due to including observations of exactly thirteen weeks that may not truly be separations. Interestingly, in contrast to the case for permanent separations, the larger size classes produce temporary separations that result in somewhat lower numbers of weeks lost. The pattern across payroll classes is fairly similar to that for permanent separations, with the lowest paying firms producing the highest number of weeks lost. Patterns across industries are again apparent. Manufacturing once more is the standout with a well-below-average 7.5 weeks lost. This lower number of earnings weeks lost, taken together with the higher incidence of temporary layoffs, suggests that drops in demand are typically met by cycling workers through temporary layoffs. While there is a large discrepancy between permanent and temporary separations in manufacturing, for some other industries there is little distinction. For example, approximately fourteen earnings weeks are lost in retail trade after a separation, regardless of whether the separation is permanent or temporary. In services, temporary layoffs actually result

Table 10. Regression Models for Lost Earnings Weeks after Separations Followed by Reemployment

Independent variable[a]	*Total separations*	*Total separations*	*Permanent separations*	*Permanent separations*	*Temporary separations*	*Temporary separations*
Firm size						
Fewer than 20 employees	0.520	−0.841	−1.563	−0.965	1.959	0.359
	(0.267)	(0.313)	(0.396)	(0.423)	(0.301)	(0.531)
20–99	−0.069	−0.696	−2.013	−0.838	1.254	−0.039
	(0.258)	(0.302)	(0.387)	(0.409)	(0.284)	(0.505)
100–499	−0.176	−0.571	−1.999	−0.674	0.888	−0.277
	(0.252)	(0.296)	(0.384)	(0.404)	(0.265)	(0.478)
500–1,999	0.218	−0.283	−1.252	−0.287	0.814	0.070
	(0.259)	(0.301)	(0.398)	(0.417)	(0.267)	(0.455)
Quarterly payroll per worker ($1,000s)						
Less than 1	0.452	−1.600	1.462	−1.601	−0.979	−0.287
	(0.339)	(0.347)	(0.455)	(0.474)	(0.436)	(0.510)
1–2.5	0.873	−1.129	1.723	−1.359	−0.006	−0.368
	(0.284)	(0.294)	(0.392)	(0.411)	(0.341)	(0.392)
2.5–5	0.515	−0.567	1.575	−0.442	−1.008	−0.223
	(0.272)	(0.280)	(0.380)	(0.397)	(0.317)	(0.352)
5–7.5	−0.078	0.221	1.140	0.750	−1.415	−0.543
	(0.297)	(0.290)	(0.420)	(0.421)	(0.338)	(0.336)
Unemployment rate (%)	0.651	0.972	0.943	1.249	0.084	0.465
	(0.078)	(0.077)	(0.106)	(0.112)	(0.098)	(0.093)
Type of separation						
Temporary	−2.105	−1.084				
	(0.127)	(0.135)	—	—	—	—
Individual effects	No	Yes	No	Yes	No	Yes
N (separations)	207,188	207,188	139,518	139,518	67,670	67,670
R^2	0.038	0.629	0.037	0.671	0.106	0.838

Source: Authors' calculations based on individual sample.

a. All models also include two-digit SIC industry, state, and calendar quarter effects. Standard errors are in parentheses.

in slightly more weeks lost than do permanent separations (sixteen weeks compared with fourteen weeks).

As was the case with separation probabilities, a regression framework allows us to look more carefully at the role of firm characteristics and individual attributes. Table 10 presents the results of regressions similar to those in table 6, but the dependent variable is earnings weeks lost, and the universe is all separations for which reemployment is observed. For the case where individual fixed effects are not included in the model, each group of explanatory variables is significant. The results tend to confirm the impression gained from the simple means table. Consider the size class variables, for example. When we control for other firm characteristics, we see that the largest firms continue to produce permanent separations that lead to more weeks lost. The estimates imply that the losses generated by the smaller firms are one to two weeks shorter. Similarly, the lower payroll classes generate spells that are one to two weeks shorter. Since permanent characteristics of individuals may make it harder or easier for them to find a job, it is important to consider including individual effects in the model. When this is done, the size class variables are no longer jointly significant at conventional levels, having a p-value of 0.098. However, the smallest size class remains associated with a significant reduction of almost one week. The inclusion of individual effects also reverses the role played by payroll per worker class. Compared with the highest class, the lowest classes are associated with a reduction in lost earnings weeks of between 1 and 1.5 weeks.

Looking at earnings weeks lost from temporary separations, when no individual effects are included in the model, we find that firm characteristics are all significant. The smaller firms generate higher losses compared with the largest firms—almost two weeks longer for the smallest firms and about one week longer for the others. The highest payroll per worker class also generates higher losses, about one week more than all but the second lowest class. However, when individual effects are included in the model, these firm characteristics are no longer important and are not statistically significant.

These results on the role of firm characteristics in generating losses from permanent separation seem generally consistent with the interpretation of their effects on turnover. While not always significant, the pattern of coefficients on size and payroll class, when including indi-

vidual fixed effects, is just the opposite in table 10 from table 6. Essentially, aspects of the current job that lead to low turnover also imply that there is a low probability of finding an equivalent or better job. Thus we would expect larger losses to be associated with the same characteristics that were negatively related to turnover. For example, we would expect a worker who has accumulated large amounts of firm-specific human capital to experience larger losses after a permanent separation, since this human capital will not be rewarded at a new firm. Assuming again that average payroll is correlated with the level of firm-specific human capital, a positive relationship between average payroll and lost earnings weeks, such as we find, is predicted. Note, however, that a theory of job-match quality is also consistent with the results. Implicit in this discussion is the assumption that when a worker's reemployment earnings are likely to fall, she may spend a long time looking for work. The patterns we observe could also be influenced by differing ratios of quits to layoffs by firm size, wage level, or industry.

As was indicated, losses from unemployment are just one of the likely costs of turnover. Unfortunately, a firm's adjustment costs are difficult to measure, with a wide range of estimates obtained from the few management studies that exist.[68] At the low end of the estimates is an average hiring cost of $910 (less than three weeks' pay). This amount is relatively small when compared with our estimate of fourteen lost earnings weeks, but it is not insignificant. However, other estimates of turnover costs, particularly for some classes of workers, are much higher. For example, a study of a large pharmaceutical company placed the present value of the cost of replacing a worker at 1.5 to 2.5 times *annual* salary. Another study estimated the full cost of replacing a truck driver to be $7,000, or about twenty weeks.[69] In addition, training costs and lost earnings may be somewhat related; if larger earnings weeks lost are attributed to greater amounts of firm-specific human capital, it is also likely that training costs are above average.

68. These are reviewed in Hamermesh (1993, p. 208).

69. The weekly measures are based on average, private, nonagricultural weekly earnings of $345.35 in 1990. Following Hamermesh, we express all costs in 1990 dollars.

The Components of Worker Reallocation

Earlier in the paper we explored the extent of turnover by focusing on total worker reallocation, broken down into permanent and temporary components. Turnover can be broken down further by splitting permanent worker reallocation into that due to job reallocation and that due to other causes. Worker reallocation due to job reallocation can be attributed to the fact that workers are displaced as firms decline or go out of business, while at the same time new jobs are created at newly opened and expanding firms. Although net employment growth or decline may be relatively small, gross job reallocation is likely to be quite large.[70] This job reallocation, though, is only one possible contributor to permanent worker reallocation. We also see job matches dissolve, while the actual position continues, only to be filled with a new worker. Thus workers continually reallocate themselves among new positions and continuing positions. More formally, we can further decompose total turnover as follows:

—Temporary Turnover (Temporary Reallocation) = Temporary Layoffs + Recalls

—Job Creation and Destruction (Permanent Job Reallocation) = New Hires at New Positions + Separations from Ending Positions

—Job Match Creation and Destruction (Other Permanent Reallocation) = New Hires at Existing Positions + Permanent Separations from Continuing Positions

—Total Turnover (Worker Reallocation) = Temporary Turnover + Job Creation and Destruction + Job Match Creation and Destruction.

The terminology in parentheses parallels more closely the existing literature. We follow Davis and Haltiwanger in calculating job creation and destruction rates at time t for each firm:

$$\text{Job Creation} = \frac{N_t - N_{t-1}}{.5*(N_{t-1} + N_t)}, \text{ for } N_t - N_{t-1} > 0, \text{ and}$$

$$\text{Job Destruction} = \frac{|N_t - N_{t-1}|}{.5*(N_{t-1} + N_t)}, \text{ for } N_t - N_{t-1} < 0,$$

70. See Davis and Haltiwanger (1990, 1992) and Dunne, Roberts, and Samuelson (1989a).

Table 11. Annual Job Reallocation Rates by Industry, State, and Year

Classification	*Number of firm years*	*Job creation rate*	*Job destruction rate*	*Net job growth*	*Gross job reallocation*
Total	5,522	0.1135	0.0992	0.0143	0.2128
Industry					
Agriculture	106	0.1494	0.1251	0.0244	0.2745
Mining	159	0.1252	0.1219	0.0033	0.2470
Construction	567	0.2173	0.2960	−0.0787	0.5133
Manufacturing	1,035	0.1015	0.1145	−0.0130	0.2161
Transportation/communications	358	0.0751	0.0922	−0.0171	0.1674
Wholesale trade	559	0.1380	0.1558	−0.0178	0.2938
Retail trade	1,026	0.1458	0.0732	0.0726	0.2190
Finance, insurance, and real estate	447	0.1080	0.0541	0.0539	0.1621
Services	1,091	0.1061	0.0867	0.0195	0.1928
Public sector	174	0.0545	0.0514	0.0031	0.1059
State					
Georgia	1,530	0.1036	0.0592	0.0445	0.1628
Idaho	416	0.0684	0.0959	−0.0276	0.1643
Louisiana	1,276	0.1421	0.1006	0.0415	0.2427
New Mexico	607	0.0565	0.1068	−0.0503	0.1633
South Carolina	760	0.1897	0.1631	0.0266	0.3527
Washington	933	0.1183	0.1124	0.0060	0.2307
Year					
1979	689	0.0949	0.0527	0.0422	0.1475
1980	1,026	0.0752	0.0774	−0.0023	0.1526
1981	1,413	0.1386	0.1126	0.0260	0.2511
1982	1,328	0.1289	0.1100	0.0189	0.2389
1983	1,066	0.1174	0.1262	−0.0088	0.2436

Source: Authors' calculations based on firm sample.

where N_t is employment in period t. Recall that such a measure will not perfectly capture job creation and destruction. Restructuring that causes job creation and destruction, but which leaves employment constant, will be missed. Similarly, if a firm transfers a job to another plant across state lines, we will misclassify this as job reallocation.

Previous work on turnover has focused on job reallocation, or on worker reallocation, but not on their relationship. Additionally, what is known about job reallocation is limited to manufacturing, and much of the analysis is carried out at annual or longer frequencies. We use the CWBH data to create a firm panel that covers all industries and that allows us to explore quarterly job reallocation rates. When these data are matched back to the individual records, we can decompose total worker reallocation into its three parts: temporary turnover, job creation and destruction, and job-match creation and destruction. These are large advantages to using the CWBH data, but there remains a drawback. Because the data were collected by sampling workers, they are not a representative firm sample. Thus in calculating levels of job reallocation, we limit ourselves to the six states with sampling rates of at least 10 percent.[71] We then retain only those firms with at least fifty employees in any quarter of the sample. In this way we can be at least 99.5 percent certain that the disappearance of a firm is not solely because none of its workers is being sampled.[72] The details of the sample and the computation of gross job reallocation rates are explained in the appendix. Since the analysis is limited to the somewhat larger firms, it is not strictly comparable to prior work. However, comparisons across industries and sampling frequencies remain informative, as does the decomposition of total worker reallocation.

Job Creation and Destruction, Annual and Quarterly

Tables 11 and 12 present job reallocation rates for the overall sample by industry, by state, and over time. In table 11 annual rates are computed by calculating job creation and job destruction across first quarters; in table 12 quarterly rates are computed by calculating job creation and destruction across adjacent quarters. The results in table 11 are

71. These states are Georgia, Idaho, Louisiana, New Mexico, South Carolina, and Washington.

72. In applying this screen we retain 83 percent of employment.

Table 12. Quarterly Job Reallocation Rates by Industry, State, and Calendar Quarter

Classification	*Number of quarterly firm observations*	*Job creation rate*	*Job destruction rate*	*Net job growth*	*Gross job reallocation*
Total	24,371	0.0707	0.0635	0.0072	0.1342
Industry					
Agriculture	457	0.2084	0.1935	0.0149	0.4019
Mining	673	0.0525	0.0489	0.0036	0.1013
Construction	2,524	0.1151	0.1466	−0.0315	0.2617
Manufacturing	4,590	0.0580	0.0623	−0.0043	0.1203
Transportation/communications	1,563	0.0410	0.0415	−0.0005	0.0825
Wholesale trade	2,506	0.0849	0.0832	0.0016	0.1681
Retail trade	4,482	0.0757	0.0555	0.0202	0.1312
Finance, insurance, and real estate	1,983	0.0589	0.0412	0.0177	0.1001
Services	4,819	0.0789	0.0668	0.0122	0.1457
Public sector	774	0.0652	0.0506	0.0146	0.1159
State					
Georgia	6,759	0.0508	0.0374	0.0135	0.0882
Idaho	1,832	0.0442	0.0459	−0.0017	0.0901
Louisiana	5,449	0.0835	0.0730	0.0106	0.1565
New Mexico	2,358	0.0472	0.0597	−0.0125	0.1069
South Carolina	3,335	0.1193	0.1100	0.0093	0.2293
Washington	4,638	0.0930	0.0775	0.0155	0.1705

Calendar quarter					
1978:3	405	0.0580	0.0372	0.0208	0.0952
1978:4	533	0.1555	0.0725	0.0830	0.2280
1979:1	544	0.0791	0.0482	0.0309	0.1272
1979:2	654	0.0589	0.0437	0.0152	0.1025
1979:3	662	0.0393	0.0809	−0.0416	0.1202
1979:4	899	0.0705	0.0261	0.0444	0.0966
1980:1	930	0.1056	0.0508	0.0548	0.1564
1980:2	984	0.0460	0.0801	−0.0341	0.1261
1980:3	977	0.0595	0.0714	−0.0119	0.1310
1980:4	1,323	0.0762	0.0897	−0.0134	0.1659
1981:1	1,350	0.0896	0.0630	0.0266	0.1525
1981:2	1,349	0.0725	0.0350	0.0375	0.1075
1981:3	993	0.0593	0.0926	−0.0333	0.1518
1981:4	994	0.0585	0.0539	0.0046	0.1124
1982:1	1,382	0.0929	0.0849	0.0079	0.1778
1982:2	1,252	0.0613	0.0542	0.0072	0.1155
1982:3	1,325	0.0962	0.0650	0.0313	0.1612
1982:4	1,292	0.0685	0.0749	−0.0064	0.1434
1983:1	1,314	0.0587	0.0747	−0.0160	0.1334
1983:2	1,350	0.0678	0.0435	0.0243	0.1113
1983:3	1,339	0.0561	0.0569	−0.0008	0.1129
1983:4	1,038	0.0622	0.0337	0.0285	0.0960
1984:1	1,034	0.0457	0.1137	−0.0680	0.1595

Source: Authors' calculations based on firm sample.

most comparable to those presented in detail in Davis and Haltiwanger, so it is informative to start with a comparison to their work. Over the years 1973 to 1986 (excluding 1974, 1979, and 1984), they find annual job creation and destruction rates of 0.092 and 0.113 respectively for manufacturing.[73] The corresponding rates in our sample are 0.102 and 0.115 for the years 1979 through 1983. Since Davis and Haltiwanger also present yearly rates and rates by firm size, we can evaluate the likely effect of our sample covering a different time period and excluding the smallest firms. Gross rates of job reallocation decline from 0.304 for firms with fewer than 100 employees, to 0.191 for firms with 250 to 499 employees, to 0.138 for firms with 1,000 or more employees. Thus we would expect that, if anything, our rates would be below those of Davis and Haltiwanger. At the same time, their gross rates for the years 1980 to 1983 range from 0.173 to 0.227, and they average 0.201. Since this is similar to their rate for 1973 to 1986, the fact that our data cover a subset of the period should not affect comparisons. Although we would not expect to exactly replicate the results of Davis and Haltiwanger, given the differences across samples, the CWBH numbers do appear to be in line with their results.

As was the case above, manufacturing differs from the other industries. While the net employment decline in manufacturing of 1.3 percent is a change in the opposite direction from the overall net growth of 1.4 percent, the gross job reallocation rate of 21.6 percent is almost identical to that for gross job reallocation overall. By contrast, the public sector stands out as having particularly low gross reallocation rates (11 percent), followed by transportation and finance, insurance, and real estate (around 16 percent each); construction is especially high (51 percent). Most of the large industries hover between 20 and 25 percent. These industry differences are explored in more detail below, in concert with the decomposition of total worker reallocation.

We have presented yearly rates for completeness, but the comparisons across years may be somewhat misleading due to the differing sample compositions across time. The sampling period is not consistent across states, implying that different states represent differing fractions of the overall sample over time. As can be seen by these state compar-

73. See Davis and Haltiwanger (1992, pp. 830–31, 841).

isons, differences in job reallocation rates across states can be fairly substantial.[74] Rates by state and year are not presented, since small cells tend to be overly influenced by large-plant births and deaths.

While comparisons of these annual rates are telling, perhaps more interesting are the quarterly reallocation rates presented in table 12. To the extent that jobs are created and destroyed at seasonal frequencies, examination of year-to-year changes will overlook a portion of total job reallocation. At these quarterly frequencies we see patterns across industries that are similar to those found at annual frequencies. However, the annual rates implied by these quarterly figures differ from the annual rates in table 11.

As can be seen in the first row of table 12, gross job reallocation averages 13.4 percent quarterly, implying that in the course of a year the number of jobs created and destroyed is equal to 53.6 percent of average employment. Note that this last number includes jobs that are created during the year and destroyed before the end of the same year. Similarly, it includes jobs that are destroyed during the year and recreated before the end of the same year. Thus the 21.3 percent annual rate calculated from year-to-year changes represents just 40 percent of this reallocation rate. In manufacturing, though, the 12 percent gross rate would imply a 48 percent annual rate, so the year-to-year change captured only 45 percent of the job reallocation. The difference between quarterly and annual patterns is even more extreme in services, where the 14.6 percent quarterly rate implies that only 33 percent of the total is captured by the annual change measure. Thus employment in services is clearly much more variable within the year than is employment in manufacturing. Again in the tables we present figures for each quarter, but one should recall that the changes in the states included in the sample over time reduce the comparability of these numbers over time.[75]

74. South Carolina's rate is highly sensitive to the handling of a firm's disappearance and reappearance. When these observations are treated as missing, the gross job reallocation rate falls to 20 percent. For all other states, the rate falls by just a few percentage points at most. Thus this number should be treated with some caution.

75. Additionally, the smaller the cells, the more likely a change in employment at a single large employer will exert undue influence. For example, the destruction rate in 1984:1 drops to 0.058 if two large firms suffering big declines are excluded.

Table 13. Components of Quarterly Worker Reallocation by Industry

Industry	*Number of quarterly observations*	*Job creation rate*	*Total accession rate*	*New hire rate*	*Job destruction rate*	*Total separation rate*	*Permanent separation rate*
Total	24,371	0.0707	0.2229	0.1616	0.0635	0.2135	0.1544
Agriculture	457	0.2084	0.8745	0.6928	0.1935	0.8627	0.6996
Mining	673	0.0525	0.1604	0.1114	0.0489	0.1547	0.1080
Construction	2,524	0.1151	0.4974	0.3661	0.1466	0.5358	0.3906
Manufacturing	4,590	0.0580	0.2473	0.1303	0.0623	0.2455	0.1325
Transportation/communications	1,563	0.0410	0.1159	0.0828	0.0415	0.1116	0.0795
Wholesale trade	2,506	0.0849	0.2388	0.1817	0.0832	0.2409	0.1842
Retail trade	4,482	0.0757	0.2907	0.2563	0.0555	0.2639	0.2287
Finance, insurance, and real estate	1,983	0.0589	0.1351	0.1108	0.0412	0.1229	0.0988
Services	4,819	0.0789	0.1797	0.1322	0.0668	0.1699	0.1279
Public sector	774	0.0652	0.1272	0.0937	0.0506	0.1038	0.0746

Industry	*Total worker reallocation*	*Permanent worker reallocation*	*Permanent job reallocation*	*Other permanent reallocation*	*Temporary worker reallocation*
Total	0.4364	0.3160	0.1342	0.1818	0.1204
Agriculture	1.7371	1.3924	0.4019	0.9904	0.3448
Mining	0.3151	0.2193	0.1013	0.1180	0.0958
Construction	1.0332	0.7567	0.2617	0.4951	0.2765
Manufacturing	0.4928	0.2628	0.1203	0.1426	0.2300
Transportation/communications	0.2276	0.1623	0.0825	0.0798	0.0653
Wholesale trade	0.4797	0.3660	0.1681	0.1979	0.1137
Retail trade	0.5546	0.4850	0.1312	0.3537	0.0696
Finance, insurance, and real estate	0.2580	0.2095	0.1001	0.1094	0.0485
Services	0.3496	0.2601	0.1457	0.1144	0.0895
Public sector	0.2310	0.1683	0.1159	0.0525	0.0627

Source: Authors' calculations based on firm sample.

The Relationship between Job Creation and Destruction and Total Turnover

A major reason for studying gross job flows is to better understand the relationship between job reallocation and total worker reallocation. In order to address this important question, Davis and Haltiwanger combine information from the Current Population Survey (CPS) with the LRD to indirectly estimate that 35 to 56 percent of total worker reallocation is due to changes in job opportunities arising from firm growth and firm decline.[76] We are able to directly associate the workers' wage records with the firm employment changes presented in table 12 to determine what fraction of worker reallocation is accounted for by job reallocation. To inflate randomly sampled wage records to equal firm employment, we weight each record by the inverse of the state sampling rates shown in the appendix. Separations and accessions are then calculated for each firm, and worker reallocation rates are computed by dividing these by the average employment, just as was done in computing job reallocation rates. The first part of table 13 presents these quarterly worker reallocation rates and compares them with the job reallocation rates for the overall sample and for each industry. We then decompose total worker reallocation into permanent and temporary components, with permanent worker reallocation further decomposed into that from job reallocation and that from other causes.

Overall, the 0.44 rate of total worker reallocation is made up of a 0.13 permanent job reallocation rate, a 0.18 other permanent reallocation rate, and a 0.12 temporary worker reallocation rate. Thus about 31 percent of quarterly gross worker reallocation can be accounted for by gross job reallocation. Differences across industries, however, are apparent. Looking first at manufacturing, one of the largest industries, we see that the total worker reallocation rate is about 0.49, but permanent job reallocation is 0.12. Therefore, only 24 percent of quarterly worker reallocation can be attributed to job reallocation. By contrast, in the finance, insurance, and real estate industry and in services, close to 40 percent is from job reallocation. As was the case in table 2, manufacturing has an above average rate of temporary worker reallocation (0.23 compared with 0.12 overall). Thus 47 percent, or almost half, of turnover in manufacturing is temporary, while just 28 percent of turnover

76. See Davis and Haltiwanger (1992, pp. 820–21).

overall is temporary. This fraction is especially low in retail trade; 13 percent of turnover is temporary, with almost 64 percent being due instead to permanent job-match creation and destruction (other permanent reallocation). In manufacturing, by comparison, such turnover at continuing positions is only 29 percent of the total, while for the sample overall it is 42 percent of the total.

In sum, then, job creation and destruction accounts for 31 percent of total turnover, temporary turnover accounts for 28 percent, and other turnover at continuing positions accounts for 42 percent. Thus job reallocation does not appear to be the major source of worker reallocation. Instead, job-match creation and destruction, attributable to other permanent sources of worker reallocation across continuing positions, is responsible for the largest fraction. It is likely, even, that 31 percent is an overestimate of the amount of total turnover actually attributable to *permanent* job creation and destruction. While we categorize all job reallocation as permanent, in fact, when looking at quarterly frequencies, some of it is likely to be temporary. This implies that the fraction of turnover attributable to permanent job creation and destruction is actually lower.

In interpreting these results, one should recall that each of the main components of total worker reallocation can be associated with a branch in the turnover literature discussed above. Temporary reallocation can be associated with theories of long-term attachments, while the literature from industrial organization on firm growth and work on labor demand are best applied to explain job reallocation. Similarly, models of firm-specific human capital and matching can be associated with other types of permanent reallocation. Table 13, then, has implications for assessing the role of each of these branches. Note first that those industries with relatively low job reallocation rates tend to also have low reallocation due to other causes. This tendency supports the idea that firms with high survival probabilities may find it more beneficial to induce long-term attachment, perhaps through the use of compensation incentives or by providing training. Outside of manufacturing, though, this evidence of higher levels of long-term attachment is not associated with higher levels of temporary reallocation. These other industries may operate under less variable demand conditions, making temporary layoffs relatively unimportant.

Table 14. Cyclicality of Components of Quarterly Worker Reallocation

Quarterly state rate used as dependent variable[a]	*Coefficient on percent unemployed in state*[b]	*Coefficient on net employment growth*[c]
Job creation	−0.0019	0.4736
	(0.0033)	(0.0466)
Job destruction	0.0004	−0.5264
	(0.0035)	(0.0466)
Net growth (job creation − job destruction)	−0.0023	N.A.
	(0.0051)	
Total worker reallocation	−0.0124	0.1565
(total separations + total accessions)	(0.0047)	(0.0965)
Permanent worker reallocation	−0.0235	0.0731
(permanent separations + new hires)	(0.0035)	(0.0830)
Permanent job reallocation	−0.0015	−0.0528
(job creation + job destruction)	(0.0047)	(0.0931)
Other permanent reallocation	−0.0220	0.1259
(permanent worker reallocation − job reallocation)	(0.0054)	(0.1161)
Temporary worker reallocation	0.0111	0.0834
(total worker reallocation − permanent worker reallocation)	(0.0032)	(0.0663)

Source: Authors' calculations based on firm sample.
N.A. not applicable.
a. All regressions also include state dummy variables and quarterly seasonal dummy variables. N = 109. Standard errors are in parentheses.
b. Average of state monthly rates over the quarter.
c. Net employment growth equals job creation minus job destruction.

The Cyclicality of the Components of Worker Reallocation

It is also possible to more formally investigate the impact of the business cycle on the components of worker reallocation. As was the case earlier, we aggregate over individuals and firms to form a quarterly time series for each state. We then regress the various components of turnover on the average monthly unemployment rate in the state over the quarter, state dummy variables, and quarterly seasonal dummy variables. The results are presented in table 14. As before, temporary turnover is countercyclical, while permanent turnover is procyclical. Splitting permanent turnover into that due to job reallocation and that due to other causes shows that job reallocation is not significantly related to the unemployment rate. This is true for both gross and net reallocation, as well as for job creation and destruction separately.

Past work, however, has tended to find gross job reallocation to be

countercyclical, with the procyclicality of job creation outweighed by the countercyclicality of job destruction.[77] This past work, though, has used somewhat different data and methods. First, the rates have been calculated at annual frequencies rather than quarterly frequencies. In fact, if we repeat the exercise using our annual rates of job creation and destruction and the average monthly unemployment rate over the year, we do find gross job creation to be mildly countercyclical, with the coefficient (standard error) on the unemployment rate being 0.034 (0.015). Neither job creation nor job destruction is significantly cyclical at conventional levels, however, with the coefficient (standard error) on the employment rate being 0.011 (0.009) and 0.023 (0.015) for job creation and job destruction, respectively.

A second difference is that Davis and Haltiwanger use net job reallocation as their measure of the business cycle, rather than an unemployment rate as we have used here. Thus in the final column of table 14 we substitute net employment growth (defined as job creation minus job destruction for the state in the quarter) for the state unemployment rate. Here we do find job reallocation to be countercyclical but not significantly so, and total worker reallocation to be countercyclical but also not significantly so. However, job creation is significantly procyclical, and job destruction is significantly countercyclical. Looking at annual frequencies, we do find that total job reallocation is significantly countercyclical, with the coefficient (standard error) on net growth for the state in the year being -0.458 (0.175).[78]

The overall results provide strong evidence for the procyclicality of total worker reallocation, and especially of permanent worker reallocation. At the same time temporary turnover, and to a much lesser extent job reallocation, are somewhat countercyclical. The types of separations and accessions and the pool of job seekers appear to change

77. See Davis and Haltiwanger (1992).

78. The difference between the quarterly and annual rates may be partly due to a measurement issue. Consider a change in average employment between 1980:1 and 1980:2, which we would label as job reallocation in 1980:2 in our quarterly analysis. The job creation or destruction, however, is actually distributed over the first six months of 1980, since we observe only average quarterly employment. Similarly, what we label as job reallocation in 1980 is the change in average employment between 1980:1 and 1981:1, so the job creation or destruction is actually taking place over fifteen months. Since twelve is a larger fraction of fifteen than three is of six, the annual analysis may be less affected by this problem.

in fairly complex ways over the business cycle. Clearly, then, macroeconomic models of the business cycle have complex patterns to replicate.

Conclusions

The picture of the labor market painted in this paper is more dynamic than is generally thought. We document a very high rate of turnover in most industries, and we confirm that turnover is concentrated in a subset of individuals. However, a larger fraction of workers is affected than previous research indicates. The probability of a separation, though, is monotonically declining with job tenure. We also find that the levels of earnings, industry, and firm size have large effects on turnover probabilities, both when we do and do not allow for individual fixed effects. Turnover is negatively related to firm size as well as to average payroll per worker at the firm. A particularly notable difference across industries is the above average reliance of manufacturing on temporary layoffs, along with a below average occurrence of permanent separations.

An advantage of our CWBH data is that they also allow us to decompose turnover, or total worker reallocation, into three main components. The first component is simply temporary turnover at continuing job matches. The second component is permanent turnover due to job-position creation and destruction (job reallocation), which occurs as firms are born and expand, or as they decline and die. Finally, the third component is permanent turnover from other causes, that is, job-match creation and destruction. For our sample, total worker reallocation is made up of 28 percent temporary turnover, 31 percent permanent job reallocation, and 42 percent permanent turnover from other causes, although the composition of turnover varies significantly across industries. We also find that those industries with relatively low job reallocation rates tend to have low permanent turnover due to other causes. This tendency supports the idea that firms with higher survival probabilities will find it most beneficial to induce long-term attachment, perhaps through the use of compensation incentives or by providing training.

We find strong evidence for the procyclicality of total worker reallocation, and especially of permanent worker reallocation. At the

same time temporary turnover appears to be countercyclical. While past work has found job creation and destruction to be countercyclical, our results are somewhat mixed. We do find job reallocation to be significantly countercyclical at annual frequencies. These differing responses of the various components of turnover imply that macroeconomic models of the business cycle must replicate complex patterns.

We can draw a loose association between the formation of long-term attachments and the use of temporary layoffs to adjust to demand changes, as well as between theories of firm growth and decline and job reallocation. Additionally, there is a relationship between job-match creation and destruction and theories such as those of job shopping and of mobility affected by the accumulation of firm-specific capital, or by the use of compensation as an incentive device. Given such relationships, the patterns that we observe across industry, firm size, and payroll per worker class reflect on each of these major theories of turnover. While the use of temporary layoffs to meet demand changes is clearly important, it is much less so outside of manufacturing. The past emphasis on explaining high rates of temporary layoff may be somewhat misplaced, since temporary turnover economywide is only about half that in manufacturing. More generally applicable appear to be theories of job shopping and of mobility affected by the accumulation of firm-specific human capital, or by the use of compensation as an incentive device. The patterns of turnover that we find are consistent with what is known about the patterns of training investments across industry and firm size and payroll classes, as well as with the use of compensation incentives across these groups. Such theories are also consistent with the decline in turnover with tenure that we observe, as is our finding of a greater decline in earnings following a separation from the types of firms with lower turnover.

One other goal of this paper has been to demonstrate the research potential of UI administrative data. These data have been used not only to analyze turnover but also to look at labor demand and adjustment costs, the costs of job displacement, and many aspects of the UI program.[79] They can be used to examine a wide range of other questions about earnings, turnover, and firm employment policies. The wage

79. See Anderson (1993); Jacobson, LaLonde, and Sullivan (1993); and Anderson and Meyer (1993a, b, c, d).

record data are currently being evaluated as a tool for determining the effectiveness of training programs, since they offer the potential to determine the long-term effects of training by tracking those who do and do not receive training over many years.[80]

An ongoing national UI database would have several advantages over the data we analyze. One could follow the earnings and employment patterns of individuals whose job changes take them across state lines, and such data would be nationally representative. Quarterly earnings data are currently collected by nearly all state UI programs, but they are not assembled in one place in a standard format. Thus the development of a national wage record database would not require a costly data collection effort. Rather, it would require only standardization and compilation of existing data.

Appendix

The Continuous Wage and Benefit History (CWBH) data are administrative records from the unemployment insurance (UI) systems of Georgia, Idaho, Louisiana, Missouri, New Mexico, Pennsylvania, South Carolina, and Washington. For each of these states, wage records were collected for a sample of the UI-covered workers; the sampling rate varies by state, although typically it is 10 to 20 percent. Table A-1 presents the exact sampling rates. The CWBH data also include records for the weekly UI received (if any) for each of the sampled individuals. Since the wage records contain both individual and firm identifiers, we can form quarterly job-match histories over the sample period, which will allow us to identify the creation and destruction of job matches. While this sample period differs by state, it is always at least three years. Appendix table A-1 also presents the sample period for each state. For much of the analysis, though, the first year of data is dropped to allow us to identify jobs that have already lasted at least one year. Similarly, the last quarter of data cannot be used, since we will not be able to identify if a separation occurs in that period.

In order to identify unique job matches, the wage records are sorted by the firm and individual identification codes. A new hire is then

80. See National Commission for Employment Policy (1992).

Table A-1. Sampling Period and Rates for State Wage Records

State	*Original sample period*	*Original sample rate*	*Individual sample rate*[a]	*Firm resample rate*[b]
Georgia	78:1–84:1	0.10	0.003	0.03
Idaho	78:3–82:1	0.20	0.026	0.12
Louisiana	80:3–84:2	0.10	0.007	0.06
Missouri	78:1–83:1	0.05	0.004	N.A.
New Mexico	79:1–84:1	0.20	0.021	0.06
Pennsylvania	79:2–84:1	0.01	0.003	N.A.
South Carolina	78:2–84:1	0.20	0.007	0.03
Washington	79:3–83:4	0.10	0.007	0.06

Source: Authors' calculations based on firm sample.
N.A. not applicable.
a. Fraction of state's covered workers.
b. Fraction of state's originally sampled firms.

identified if a job match first appears in a quarter other than the first quarter of data collection, and a permanent separation is identified if a job match last appears in a quarter other than the last quarter of data collection. Note that it is possible for an individual to hold more than one job at a time, and thus be a part of more than one job match in any given quarter. We then calculate quarterly new hire (permanent separation) rates as the number of new hires (permanent separations) divided by the number of job matches. We are able to identify some of the temporary turnover in a similar manner. If there is a gap in the quarterly job-match history, we define the quarter before the gap to be a temporary separation and the quarter after to be a return from a temporary separation. Because our unit of analysis is a job match, it is possible for an individual to be involved in one or more job matches before returning, just as an individual may hold more than one job at any given time.

Looking only at the quarterly wage records, however, one will miss any temporary layoffs that do not encompass an entire calendar quarter. If such a layoff results in a UI claim, we are able to identify it by matching the UI experience to the wage records. We summarize the weekly UI history into a quarterly record of receipt and match this back to the individual wage records by the quarter of initiation of unemployment insurance. Then, if a claim is initiated in a quarter not previously coded as a separation, that quarter is assumed to contain a short temporary layoff. Note, however, that the return from this temporary layoff may actually occur in the next quarter, so the returns from temporary

Figure A-1. Classification of Separations and Accessions

	Quarters[a]				
	1	2	3	4	5
Person 1[b]	Firm 1	Firm 1		Firm 1	Firm 1
Person 2[c]	Firm 1 Firm 2	Firm 2	Firm 2	Firm 2 unem- ployment insurance received	Firm 2
Person 3[d]	Firm 1		Firm 2	Firm 3	Firm 3

a. Data collection occurs at least one quarter before and after those quarters shown.
b. Temporary separation in quarter 2; return in quarter 4.
c. Permanent separation in quarter 1; new accession in quarter 1; temporary separation in quarter 4; return in quarter 4.
d. Permanent separation in quarter 1; new accession in quarter 3; permanent separation in quarter 3; new accession in quarter 4.

layoff cannot be properly analyzed at quarterly frequencies. Like the permanent turnover rates, the quarterly temporary separation rate is defined as the total number of temporary separations, divided by the number of job matches. Finally, we define total accessions as the sum of new hires and recalls, and total separations as the sum of permanent and temporary separations; the rates are defined in an analogous manner. In order to grasp the coding of turnover more easily, we present in figure A-1 some sample wage record configurations for three typical individuals, along with our classification of permanent and temporary separations and accessions.

We also use the wage records to calculate lost "earnings weeks" following a separation, based on normal weekly earnings in the quarter prior to the separation.[81] Here we limit our sample to those separations for which we observe reemployment during the sample period. First, consider that total lost weeks are made up of the weeks lost in the calendar quarter of the separation, the weeks lost in the calendar quarter of reemployment, and the weeks lost in the quarters in between with

81. Normal weekly earnings are total wages in the quarter divided by thirteen.

no job at all. We then calculate the "earnings weeks" analogues to each of these components. Weeks lost in the quarter of separation are defined as earnings in the quarter prior to separation minus earnings in the quarter of separation, divided by normal weekly earnings. Similarly, weeks lost in the quarter of reemployment are earnings in the quarter prior to separation minus earnings in the quarter of reemployment, divided by normal weekly earnings. Each of these measures is then truncated to be an integer between zero and thirteen, before being added together. Finally, we add in thirteen weeks for each full quarter of missing wage records.

Because processing the wage and unemployment insurance records of the CWBH data consumes a large amount of computer resources, we work with subsamples of the full 30 million wage record sample. The first subsample is chosen using the last digits of the individual identification numbers to obtain a sample of about 1 million wage records with approximately equal numbers per state. The result is sampling rates that range from 0.3 percent to 3 percent, and average close to 0.5 percent, rather than the original rates of 1 to 20 percent. Table A-1 provides these new sampling rates in addition to the original rates. The second subsample is chosen using the last digits of the firm identification numbers to obtain a sample of approximately 1 million wage records. As a result, we have 10 to 20 percent (the original sampling rate) of the workers for those firms that are included in the subsample. As shown in table A-1, 3 to 12 percent of firms are included.

In using this firm sample to calculate levels of job reallocation, we limit ourselves to the six states with sampling rates of at least 10 percent, so that the probability of a firm with at least fifty workers appearing in the original sample is at least 0.995. We then retain only those firms with at least fifty employees in any quarter of the sample. In this way it is very unlikely that the disappearance of a firm is solely because none of its workers was sampled. We then calculate job creation and destruction rates following the method of Davis and Haltiwanger.[82] For each pair of adjacent quarters, we calculate the change in employment as $N_t - N_{t-1}$, labeling positive changes *job creation* and negative changes *job destruction*. A rate is then calculated by dividing job creation (or negative job destruction) by average employment:

82. See Davis and Haltiwanger (1990, 1992).

$$\frac{N_t - N_{t-1}}{.5*(N_{t-1} + N_t)}.$$

Note that this implies that the rate is bounded between 0 and 2 inclusive. Average rates are calculated by weighting each observation by average employment. This implies that the job creation rate at time t for a given cell is

$$\frac{\Sigma(N_t - N_{t-1})}{.5*\Sigma(N_{t-1} + N_t)},$$

where sums in the numerator are taken only over those observations in the cell for which $N_t - N_{t-1} > 0$. The denominator is summed over all observations in the cell. Similarly, the job destruction rate for a given cell is

$$\frac{\Sigma|N_t - N_{t-1}|}{.5*\Sigma(N_{t-1} + N_t)},$$

where sums in the numerator are taken only over those observations in the cell for which $N_t - N_{t-1} < 0$. Again the denominator is summed over all observations in the cell.

Before calculating these rates, we make two adjustments to prevent data errors from exerting undue influence. A typical problem to be expected is a data entry error that would imply a large employment change that did not actually occur. Inspection of the data indicated that the most egregious of these types of errors could be easily identified by looking at the average quarterly wage (in \$1,000s). If this average is below 0.9 or above 20, we have recoded the observation to missing. This is a conservative approach to recoding, and undoubtedly some errors remain. However, with this method very few valid observations will be dropped, and those errors that are likely to have a large impact on the results will clearly be deleted.[83] The proper treatment of gaps in a firm's employment series is less obvious. On the one hand, it is entirely possible that a firm may close in one period, only to reopen at a later date. On the other hand, we know that there are also processing errors that result in missing wage records. We have again taken a conservative approach and chosen not to do any recoding. Rather, we

83. Without this recoding, quarterly gross job reallocation rates are approximately 4 percentage points higher than those presented here.

treat these disappearances as true job destruction and recreation.[84] For this reason, the numbers should be considered somewhat of an upper bound. When all such disappearances are deleted, quarterly job reallocation rates are about 3 percentage points lower than those presented here.

We then directly associate the workers' wage records with the firm employment changes to determine what fraction of worker reallocation is accounted for by job reallocation. To inflate randomly sampled wage records to equal firm employment, we weight total quarterly separations and accessions by the inverse of the state sampling rates shown in table A-1. Worker reallocation rates for quarter t are then computed by dividing weighted separations and accessions by the average employment, just as was done in computing job reallocation rates.

84. Note that this is consistent with our having identified missing quarters as separations in the individual sample.

Comments and Discussion

Comment by John Pencavel: Previous empirical research on labor turnover is based on information either on firms (or aggregations of firms) or on workers, but characteristics of both workers and firms are typically not available, at least not at a disaggregated level. The distinctive feature of the time-series observations in Anderson and Meyer's very informative paper is that both the firm and the worker can be identified, so both firm panel data and worker panel data can be constructed. The data are drawn from administrative records of the unemployment insurance system from eight states. These provide quarterly observations on earnings and weekly observations on unemployment insurance payments.

The authors identify three types of turnover: a temporary separation, when a worker leaves a firm and then rejoins it; a permanent separation, when a worker leaves a firm and does not return to it; and a new accession, when a worker joins a firm for the first time. Information distinguishing employee-initiated separations (quits) from employer-initiated separations (layoffs) is not available.

These data provide information on the detailed industry to which the firm belongs, the firm's average monthly employment, and the firm's quarterly wage bill (so, upon dividing the quarterly wage bill by employment, an estimate of the firm's quarterly earnings per worker can be derived). The length of an individual's employment with a firm can be constructed, although the data are both left and right censored, an issue the authors neglect. The nature of the censoring problem is not straightforward to evaluate because the panel is not balanced: for example, the observations on Georgia are from 1978-I to 1984-I, while

those for Idaho are from 1978-III to 1982-I. This implies different censoring thresholds in different states.

Although an individual worker's turnover experiences can be constructed over a period of about five years, basic information about a worker's characteristics are unavailable. We do not know a worker's age, gender, race, schooling, or marital status. Nor do we know whether the firm is unionized or located in an urban or rural area. All these are variables that previous research has suggested may well be associated with turnover, and this paper cannot add to our knowledge on these issues.

Each quarterly observation on a worker employed in a firm identifies a job-match quarter. Of all the job-match quarters observed in the sample, 23 percent were dissolved during a quarter, an extraordinary amount of turnover. This 23 percent of dissolutions decomposes into 17 percent permanent separations and 6 percent temporary separations. In view of the heavy, if not exclusive, reliance on information from manufacturing industries in previous research on turnover, a very important finding that runs throughout the paper is that manufacturing is not representative of industry more generally. For instance, although the total separation rate in manufacturing (20 percent) is only a little below the 23 percent for all industries, its decomposition into permanent and temporary separations is quite unusual: temporary separations in manufacturing are more frequent than in any other industry except agriculture, while permanent separations in manufacturing are the lowest outside of the public sector. In a table supplied at the Brookings conference but deleted from the final version of their papers, the authors showed not merely that manufacturing turnover is unusual, but that there are some sharp differences in turnover within manufacturing: the total separations rate is 30 percent in Apparel and only 11 percent in Chemicals.

Even after controlling for characteristics observed by researchers, previous research has suggested that workers differ in their propensity to separate from employment. In the simplest case, where there are two types of workers, the familiar representation is the distinction between "movers" and "stayers." Similarly, even after researchers account for differences that they observe among firms, some firms appear to display consistently higher turnover rates than other firms. The authors confirm

these findings by demonstrating that the probability of a worker separating from a job is not independent over time (their table 5) and that firm fixed effects are highly significant in linear probability equations accounting for the probability of a job match ending in any quarter (their table 6). I expected this finding of permanent differences among workers and among firms would inspire the construction of a matrix describing the sorting of workers across firms.

In the simplest of cases, workers could be sorted into movers and stayers on the basis of their behavior in the first two years of the time series (say, during the quarters from 1979-I to 1980-IV); stayers might be defined as those who never separate in the first two years. Correspondingly, firms might be grouped into those whose turnover rates in the first two years are greater or less than the average. With workers and firms thus defined, a matrix (M) can be constructed using observations on job matches during the second part of the time series (say, from 1981-I to 1982-IV). The element m_{ij} of this matrix indicates the fraction of job matches that pair worker type i with firm type j. Sorting occurs when worker-stayers are matched with low-turnover firms and workers-movers are matched with high-turnover firms. Of course, the matrix can be more detailed than the two-by-two version I have described.

Models of turnover that the authors describe in which some firms pay wage premiums to attract and select workers with low separation propensities imply just this sorting. The wage premiums tend to be found in firms whose workers embody specific human capital. As an asset owned jointly by the firm and the workers, specific human capital can be exploited only through the mutual agreement of the firm and the worker. Efficient contracts in the presence of specific human capital should match worker-stayers with low-turnover firms. Anderson and Meyer's data offer an opportunity to investigate this implication.

In table 12 Anderson and Meyer document a remarkable amount of job reallocation: in one year the number of jobs created and destroyed represents about 50 percent of average employment. Even in 1982 and 1983, the period of this country's heaviest unemployment since the 1930s, job creation was considerable. Single representative agent models of the macroeconomy will be hard pressed to accommodate the heterogeneity of experiences that are clearly manifested in these data.

Even when *net* job growth rates are strongly positive or negative, the economy displays significant gross job destruction and job creation rates.

I expected this paper to relate the labor turnover rates across firms to parameters of the payroll tax system for financing unemployment insurance. As is well known, these parameters vary across states, and firms occupy different positions on the payroll tax rate schedules. A natural question to ask of these job turnover rates is the extent to which knowledge of the payroll tax rate parameters accounts for differences in turnover across firms.

The authors are very well acquainted with these tax issues and have already written a most enlightening description of the extent of experience rating in the tax rates facing firms.[1] In their previous paper they found that industries such as construction, manufacturing, mining, and agriculture tend to pay in payroll taxes for unemployment insurance less than their workers receive in benefits. In this earlier paper they expressed an intention to investigate whether turnover rates across firms are associated with the incentives presented by the unemployment insurance payroll taxes. I wish they had carried this out in the present paper so that we learned how much of the measured differences in turnover rates across firms could be attributed to the features of the payroll tax system.

Nevertheless, this remains a most invaluable piece of research and something that will be extensively consulted. I am very glad I read it.

Comment by Mark J. Roberts: This paper uses an enormous database of unemployment insurance records to provide a large catalog of new facts and to verify several old facts concerning the magnitude of job turnover. The paper contributes to the literature in several ways, including extending the job flow literature to industries other than manufacturing and quantifying the degree to which turnover is concentrated in a subset of individuals. It also measures the total employment turnover resulting from three sources: the creation and destruction of positions as employers enter, grow, and exit; the permanent movement of workers in and out of existing positions; and the temporary movement of workers caused by layoffs and recalls. The recent literature on job

1. Anderson and Meyer (1993b).

creation and destruction has focused on the first source, and this paper is one of only two that have been able to measure both the flows of positions and the flows of workers through the positions (Hamermesh, Hassink, and van Ours is the other).[2] Rather than repeating the main findings, which are clearly spelled out in the paper, I will focus on a few suggestions for future exploration.

One interesting pattern deals with the durability of the worker-employer match. A common finding in this paper and the literature on job flows is that there is a tremendous turnover of workers and positions, yet earlier studies by Akerlof and Main and by Hall suggest that many workers are in jobs of very long duration.[3] High turnover rates can coexist with long-duration jobs if the turnover is concentrated among a subset of the individuals or firms. This paper provides some evidence on the worker side: 55 percent of the total turnover is generated by 21 percent of individuals who experience three or more separations during a three-year period. When combined with evidence that employers differ systematically in their ability to provide long-duration jobs,[4] it raises the issue of whether workers with preferences for long-duration jobs are paired with employers that can provide them.

The data set used in this paper is rich enough to provide some evidence on this issue by separating firms providing long-duration jobs from those providing short-duration jobs and then examining whether worker-initiated turnover differs between the two groups. If worker turnover is lower among firms offering long-duration positions (or positions that are only temporarily interrupted), then these job matches should be ones in which firm investments in worker training or worker investments in firm-specific human capital would be particularly valuable. This in turn should lead to differences in wages, which the authors' data set will allow them to examine.

On the data construction side of the project, it is easy to lose sight of the enormity of the task that the authors have undertaken. Nonetheless, it is important with any new data set to continue to subject the numbers to consistency checks with other sources, and there are several issues here that the authors could explore in more detail as they continue to refine their estimates. Unlike the establishment-based surveys and

2. Hamermesh, Hassink, and van Ours (1994).
3. Akerlof and Main (1981), and Hall (1992).
4. Dunne and Roberts (1991)

censuses recently used to calculate the flows of positions, this study relies on samples of workers drawn from unemployment insurance records and must estimate the flows of employment positions. This is possible because every worker's record contains information on the employer, including a firm ID number and the total number of employees in the firm. The flows of employment positions added or lost over time are estimated from the worker data records by identifying a sample of firm ID numbers and calculating the job flows using the total firm employment reported. These flows are blown up to the state level using sampling weights. This estimation procedure should work well for measuring changes in employment positions in firms that are in operation and have workers sampled in two adjoining periods, but it is problematic if the firm does not have any workers included in the unemployment insurance samples in a time period. In this case the total employment in the firm will be classified as new job creation the first time the firm has a sampled worker and as a permanent loss of positions the last time the firm has a sampled worker. The authors have attempted to minimize this problem by including only firms with at least fifty employees, thus guaranteeing that at least one worker is likely to be sampled any time the firm is in operation. The accuracy of the procedure could be checked by calculating the flows of employment positions resulting from firm entry, expansion, contraction, and exit by state for the manufacturing sector and then comparing them with the job flows constructed by Davis and Haltiwanger using establishment surveys.[5]

A second data issue involves separating worker movements into permanent versus temporary flows. For example, permanent separations are distinguished from temporary layoffs by observing if the worker returns to the same firm at a later year in the sample. It is impossible to know if separations that are still in progress at the last survey date are permanent or temporary because it is impossible to observe if the worker returns to the same firm at a later time. Similarly, when workers enter the sample, it is not possible to tell if they are new hires or recalls from a temporary layoff that was in progress at the initial survey date. The authors recognize this censoring problem and deal with it by deleting the first and last quarter of data for each state. Appearances or disappearances in the remaining quarters are classified as permanent,

5. Davis and Haltiwanger (1990).

which will tend to overestimate the importance of permanent, relative to temporary, worker flows if temporary layoffs last more than one quarter. An alternative procedure would make use of information on the distribution of the duration of temporary layoffs. Knowledge of this distribution, either drawn from other sources or estimated using these samples, could help place bounds on the proportion of censored observations that represent permanent versus temporary flows.

General Discussion: Several participants suggested areas of additional research. Henry Aaron said that it might be worthwhile to focus on implicit labor contracts by looking at workers who are steadily employed with a firm, rather than at those who experience separation or accession. He said, however, that a four-year period—the longest for any of the states studied in the paper—is probably not long enough for such an examination, because implicit labor contracts within a company do not apply to all workers but rather to a core group. He suggested that the authors look at this issue in one or two of their states, mentioning Georgia and Pennsylvania as the best candidates, using additional years of information.

Peter Reiss noted that several authors working from an industrial organization perspective have used data from the Census of Manufactures to look at both firm and job turnover. He suggested that the authors use their own data to try to identify the portion of job turnover that is attributable to firm turnover, while also relating their work to these other studies. Reiss noted that job turnover resulting from firm turnover raises the intriguing question whether such job turnover should be regarded as worker behavior or firm behavior. Bruce Meyer said it would be difficult to examine this issue with their data because they represented only a 10 or 20 percent sample, which made it possible to infer the probability of firm births and deaths, but not to determine them with certainty.

Robert Staiger was interested in knowing whether workers who were separated because of job destruction had different lost earnings from those workers who were separated for other reasons. Meyer responded that because worker reallocation across existing positions and destruction of jobs occur simultaneously, the data in the paper cannot be used to examine that question directly. Meyer suggested, however, that this issue could be approached indirectly by creating a variable to represent

the fraction of turnover at a particular firm attributable to destruction of positions. Because the data represent only a small sample, he cautioned, this variable could probably be created only for the largest firms.

Several participants commented on measurement and data issues. Sam Peltzman noted that the paper's quarterly data show that during recessions overall job destruction rises, overall job creation remains constant, and overall accessions fall. He wondered why overall job creation did not move in tandem with accessions. Anderson said that these are separate phenomena that do not move consistently; permanent turnover is procyclical, while temporary turnover is countercyclical.

According to Tom Plewes of the Bureau of Labor Statistics (BLS), Congress has requested that the BLS develop a national wage record database that covers the paper's data gathered by the Continuous Wage and Benefit History project. Because many of the problems of the authors' study stem from the fact that data from that project are available only for eight states and that they are old and difficult to work with, the construction of this new database will allow for better research into the job turnover issue. Plewes also said that the job vacancy and turnover survey, which had been discontinued in 1981, might be resurrected. This would be an important additional source of information, he said, because unlike data drawn from the proposed national database, which would be lagged by one or two years, the data from this survey could provide insight into current labor market conditions. Thus, Plewes, concluded, construction of a new database and the job vacancy survey would complement each other.

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