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The purpose of this note is to document some empirical regularities pertaining to labor turnover, and in particular to the quit-layoff distinction. The regularities established in the literature include:

(i) The separation rate declines with labor market experience (or age), employment tenure, and education (Mincer and Jovanovic 1981; Borjas and Rosen 1980; Hall 1982; Topel and Ward 1985).

(ii) The separation rate is lower for union workers (Freeman 1980).

(iii) The quit rate is lower for union workers and males (Freeman 1980; Antel 1985; Barnes and Jones 1974; Viscusi 1980).

(iv) The quit rate is procyclical and the layoff rate countercyclical (Moore 1961; Ehrenberg and Smith 1985, 317; Prescott, et al. 1983; McLaughlin 1985).

(v) Controlling for general human capital, the quit rate is decreasing in the wage prior to separation; the effect of the pre-separation wage on the layoff rate is much weaker and is not clearly negative (Parsons 1972; Antel 1985).

(vi) Quits (layoffs) exhibit higher (lower) wage growth in the employment transition than stayers (Bartel and Borjas 1981; Antel 1985; Mincer 1986).

(vii) Controlling for its direct effect on wage growth, experience interacts with turnover status: wage growth of both quits and layoffs falls with experience; turnover status interactions are also evident for education, tenure, health, marital status, and cyclical shocks (Mincer 1986).

(viii) The quit-layoff ratio (or the quit rate conditional on separation) falls with experience (or age) (Leighton and Mincer 1982, 242; Mincer 1986).

(ix) Quits (layoffs) have a lower (higher) probability of experiencing an intervening spell of unemployment (Gottschalk and Maloney 1985).

Some additional results documented in this note include:

(x) The separation rate is lower for married workers.

(xi) The ratio of quits to layoffs (or the quit rate conditional on separation) is increasing in education, and is higher for nonunion and white workers.

(xii) Quits exhibit positive wage growth in the employment transition; the average wage growth of layoffs, while positive in my sample, is not significantly different from zero.

(xiii) Controlling for direct effects on wage growth, education, employment tenure and union status interact with turnover status; interaction effects for experience and cyclical shocks are not detected.

Items (i), (ii) and (x) pertain to the separation rate; (iii)-(v), to quit and layoff rates; and (vi)-(ix) and (xi)-(xiii), to differences between quits and layoffs with the separation decision given. The observational distinction between quits and layoffs is established by properties (iii)-(ix) and (xi)-(xiii).

In documenting these turnover regularities, I focus on two themes: conditional versus unconditional turnover rates and the effect of turnover status on wage growth in the employment transition. One contribution of this note is in decomposing the quit and layoff rates into the component parts of the separation rate and the quit rate conditional on separation. A second contribution is in estimating these two components.

Estimates of the component rates provide information regarding the quit-layoff regularities. Consider an example: The effect of the pre-separation wage on quit and layoff rates has been discussed for more than two decades. (See Becker 1962; Oi 1962; Parsons 1972; Mortensen 1978; Hashimoto and Yu 1980; and Antel 1985.) If variation in the pre-separation wage is due to variation in the amount of firm-specific human capital, the theory predicts negative covariation between the pre-separation wage and both quits and layoffs. If, however, the variation in the pre-separation wage is due to variation in the worker's share of the proceeds from the employment match, then quits are predicted to be negatively related to the pre-separation wage and layoffs positively related. In the presence of both effects, the impact of the pre-separation wage is expected to be stronger on quits and possibly positive for layoffs. Previous empirical work supports this result.

I separately identify the two effects -- specific capital and sharing -- in estimating the effect of specific capital on the separation rate, and the effect of the worker's share on quits relative to layoffs (more precisely, on the conditional quit rate). In addition, the results are not limited to the pre-separation wage: I apply the methodology to the full set of determinants of turnover.

A second issue is the effect of turnover status -- quit or layoff -- on wage growth in the employment transition. The empirical evidence indicates that quits (layoffs) have higher (lower) wage growth than stayers. A more restrictive hypothesis is that quits have positive wage growth and layoffs negative. This hypothesis is tested formally herein, because distinguishing between the two forms helps calibrate theoretical models designed to account for regularities (vii), (viii), (xi) and (xiii) (McLaughlin 1986b).

In section I, I outline the empirical strategy of decomposing (unconditional) quit and layoff rates into their component parts: the separation rate, and the quit rate conditional on separation. Presented in section II are the empirical results from my sample of male household heads, a sample drawn from the Panel Study of Income Dynamics (PSID) for the years 1975-1980.[1] Properties of the estimated separation rate and conditional quit rate are discussed first; then the estimates of the unconditional quit and layoff rates are contrasted with the estimates of their component parts. I estimate the turnover rates both with and without the pre-separation wage as a regressor. The four sets of estimates are generated by the probit estimator. Wage growth by turnover status is considered in the next subsection. The differential incidence of unemployment spells is not investigated here as Gottschalk and Maloney's results are from a subset of my data. Section III contains my conclusions.

I. EMPIRICAL STRATEGY

Most empirical research on the quit-layoff distinction examines quit and/or layoff rates (e.g., Parsons 1972; Pencavel 1972; Viscusi 1980; Weiss 1984; Antel 1985). I take an alternative approach here by examining quits and layoffs conditional on separation. Although generated by the joint wealth maximizing hypothesis of McLaughlin (1986a), this procedure has a more general justification.

Define indicator variables S and Q such that if the worker separates from his current employer S=1 (otherwise S=0) and if the separation is a quit Q=1 (a layoff Q=0). Conditioning on two sets of regressors **X**, **Z**, the joint probability of observing Q and S is

$$(1)\qquad \Pr[Q, S|\ \mathbf{X}, \mathbf{Z}] = \Pr[Q|\ S, \mathbf{X}]\cdot\Pr[S|\ \mathbf{Z}]\ .$$

The (unconditional) quit and layoff rates are

$$(2.1)\qquad q(\mathbf{X}, \mathbf{Z}) \equiv \Pr[Q=1, S=1|\ \mathbf{X}, \mathbf{Z}] = \Pr[Q=1|\ S=1, \mathbf{X}]\cdot\Pr[S=1|\ \mathbf{Z}] = \bar{q}(\mathbf{X})\cdot s(\mathbf{Z})$$

$$(2.2)\qquad \ell(\mathbf{X}, \mathbf{Z}) \equiv \Pr[Q=0, S=1|\ \mathbf{X}, \mathbf{Z}] = \Pr[Q=0|\ S=1, \mathbf{X}]\cdot\Pr[S=1|\ \mathbf{Z}] = [1-\bar{q}(\mathbf{X})]\cdot s(\mathbf{Z})$$

where $s(\mathbf{Z})$ and $\bar{q}(\mathbf{X})$ denote, respectively, the separation rate and the conditional quit rate.

From these analytical representations it is apparent that the partial effect of any particular regressor y common to the sets $\mathbf{X}$ and $\mathbf{Z}$ depends on possibly conflicting influences: y might effect the separation and conditional quit rates in opposite directions.

$$(3.1) \qquad q'(\mathbf{X}, \mathbf{Z}) = \bar{q}'(\mathbf{X})\cdot s(\mathbf{Z}) + \bar{q}(\mathbf{X})\cdot s'(\mathbf{Z}) \ .$$

$$(3.2) \qquad \ell'(\mathbf{X}, \mathbf{Z}) = -\bar{q}'(\mathbf{X})\cdot s(\mathbf{Z}) + [1-\bar{q}(\mathbf{X})]\cdot s'(\mathbf{Z}) \ .$$

(The prime denotes the derivitive with respect to some particular regressor y.[2]) A variable such as the pre-separation wage decreases both the separation rate and the conditional quit rate, resulting in an ambiguous effect on the unconditional layoff rate. Similarly, education decreases the separation rate but increases the conditional quit rate; hence the net effect of education on the unconditional quit rate is ambiguous.

I adopt the following empirical methodology: On the full sample, I estimate the separation rate $s(\mathbf{Z})$. On the subsample of movers (i.e., observations with S=1), I estimate the conditional quit rate $\bar{q}(\mathbf{X})$. This procedure uncovers offsetting influences on the unconditional turnover rates. However, the estimates of the conditional quit rate might be subject to selection bias since the subsample selection criterion relies on S an endogenous variable. To overcome this problem, the model can be estimated simultaneously by maximum likelihood methods.

II. RESULTS

In this section, I report probit extimates of the separation rate $s(Z)$, the conditional quit rate $\bar{q}(X)$, and the quit and layoff rates, $q(X, Z)$ and $\ell(X, Z)$.[3] The normality assumption which underlies probit estimation is especially convenient in the current context. If the underlying randomness is distributed bivariate normal, both the marginal and the conditional densities are normal; that is, the separation rate and the conditional quit rate are generated by normal variates and are estimable as probits. The maximum likelihood estimator is a bivariate probit with selection (or one empty cell). If Q and S are uncorrelated, the maximum likelihood estimator reduces to two univariate probits: a separation rate probit and a conditional quit rate probit. Estimates of the two univariate probits are reported in Tables 2 and 3. (Maximum likelihood estimates have been computed, but do not deviate from these simpler estimates and are not reported.) In Tables 4 and 5, I report the univariate probit estimates of the unconditional quit and layoff rates. Although the model implies these are not <u>univariate</u> probits, they are included for comparison with the literature.

Also discussed in this section are results on the effect of turnover status -- quit or layoff -- on wage growth in the employment transition. These results are reported in Tables 6 and 7.

<u>Separation Rate</u>

I begin this section by briefly examining the properties of the separation rate $s(Z)$. As shown in Table 1, the separation rate as an average declines sharply with age for the young before flattening out at the older ages. The various probit estimates in Table 2 also reveal that the probability of separating is a negatively sloped convex function of age

TABLE 1

TURNOVER RATES BY AGE GROUP
(%)

Age Range	Separation Rate	Quit Rate	Layoff Rate	Conditional Quit Rate
18-58	16.11	10.08	6.03	62.56
	(0.35)	(0.29)	(0.23)	(1.15)
	[10,922]	[10,922]	[10,922]	[1,760]
18-24	30.09	18.51	11.58	61.50
	(1.03)	(0.87)	(0.72)	(1.99)
	[1,994]	[1,994]	[1,994]	[600]
25-29	20.32	13.48	6.83	66.36
	(0.78)	(0.66)	(0.49)	(2.03)
	[2,663]	[2,663]	[2,663]	[541]
30-34	12.93	8.31	4.62	64.25
	(0.81)	(0.67)	(0.51)	(3.23)
	[1,709]	[1,709]	[1,709]	[221]
35-39	10.68	6.63	4.05	62.07
	(0.94)	(0.76)	(0.60)	(4.52)
	[1,086]	[1,086]	[1,086]	[116]
40-44	10.85	6.64	4.21	61.22
	(1.04)	(0.83)	(0.67)	(4.95)
	[903]	[903]	[903]	[98]
45-49	7.04	3.81	3.24	54.05
	(0.79)	(0.59)	(0.55)	(5.83)
	[1,051]	[1,051]	[1,051]	[74]
50-58	7.26	3.89	3.36	53.64
	(0.67)	(0.50)	(0.46)	(4.78)
	[1,516]	[1,516]	[1,516]	[110]

Note: Standard deviations of the means in parentheses. Number of observations in brackets.

(column 1); this property also applies to the relationship between the separation rate and both experience and tenure (columns 2-4). Although statistically significant, the effect of experience in reducing the separation rate is dominated by the tenure effect (column 4). The separation rate declines by approximately 1.8 percentage points for each year of continued employment, and approximately 0.2 percentage points for each additional year of labor market experience. Consider the effect of an additional year with the incumbent employer: approximately seven-eighths of the reduction in the separation rate is attributable to increased experience in the firm (tenure) and one-eighth to increased experience in general. The separations probit in column (5) includes a set of controls: Education exhibits a strong negative effect on separations; four years of college reduces the separation rate 2.4 percentage points. The separation rate of union members is 5.7, of married workers 4.9, and of racial minorities 3.1 percentage points lower than their respective counterparts. The coefficient on the cyclical variable indicates that the separation rate is procyclical; a cyclical expansion of one percentage point increases the separation rate nearly one percentage point.

Conditional Quit Rate

The quit rate conditional on separation is computed for the various age groups in Table 1. With the exception of eighteen to twenty-four year olds, the conditional quit rate falls with age.[4] Table 3 contains the probit estimates of the conditional quit rate $\bar{q}(X)$ on the subsample of prime-age males. The results in column (5) indicate that the conditional quit rate is falling in age by about half a percentage point per year (column 1). Columns (2), (4) and (5) indicate that $\bar{q}$ is also negatively related to experience; the conditional quit rate falls by up to nine-tenths of a percentage point for

TABLE 2

SEPARATION PROBITS[a]

Regressors	(1)	(2)	(3)	(4)	(5)
constant	1.525 (0.191)	-0.352 (0.036)	-0.471 (0.023)	-0.316 (0.036)	0.072 (0.103)
AGE(0)	-0.120 (0.011)				
$AGE(0)^2$ /100	0.120 (0.015)				
EXPER(0)		-0.074 (0.005)		-0.032 (0.006)	-0.021 (0.006)
$EXPER(0)^2$ /100		0.121 (0.014)		0.069 (0.015)	0.038 (0.015)
TENURE(0)			-0.111 (0.005)	-0.096 (0.006)	-0.090 (0.006)
$TENURE(0)^2$ /100			0.195 (0.016)	0.168 (0.017)	0.165 (0.017)
EDUCAT(0)					-0.024 (0.007)
UNION(0)					-0.230 (0.035)
MARRIED(0)					-0.195 (0.041)
RACE					0.126 (0.035)
CYCLIC					0.031 (0.008)
Industry(0)					yes [63.3]
Occupation(0)					yes [24.0]
log-likelihood	-4,552.2	-4,560.8	-4,404.7	-4,388.9	-4,259.1
psuedo-R^2	.056	.054	.087	.090	.117

TABLE 3

CONDITIONAL QUIT PROBITS[b]

Regressors	(1)	(2)	(3)	(4)	(5)
constant	0.760 (0.146)	0.530 (0.069)	0.300 (0.051)	0.523 (0.069)	-0.207 (0.221)
AGE(0)	-0.013 (0.004)				
EXPER(0)		-0.014 (0.004)		-0.024 (0.005)	-0.016 (0.006)
TENURE(0)			0.007 (0.007)	0.030 (0.008)	0.024 (0.009)
EDUCAT(0)					0.032 (0.015)
UNION(0)					-0.174 (0.086)
MARRIED(0)					0.020 (0.102)
RACE					0.454 (0.084)
CYCLIC					0.068 (0.022)
log-likelihood	-759.2	-757.9	-763.3	-751.6	-720.2
psuedo-R^2	.006	.008	.001	.016	.057

each year of labor market experience. The results in columns (3) - (6) for employment tenure reveal a different phenomenon. Although unrelated to $\bar{q}$ in column (3), tenure exhibits a sizeable positive effect on the conditional quit rate when experience and other variables are controlled for: conditional quits increase by about one percentage point per year of employment tenure (columns 4 and 5).[5] The results in column (5) indicate that the conditional quit rate: (a) increases with education -- four years of college increases $\bar{q}$ five percentage points; (b) of nonunion members exceeds that of union members by more than six percentage points; (c) is seventeen percentage points higher for whites; and (d) is strongly procyclical -- a cyclical expansion of one percentage point results in a 2.6 percentage point increase in the conditional quit rate.

Quit and Layoff Rates

Probit estimates for the unconditional quit and layoff rates are presented in Tables 4 and 5. The results in Table 4 correspond to the estimates of $s(Z)$ and $\bar{q}(X)$ in Tables 2 and 3. In Table 5, I include the pre-separation wage as an additional regressor to facilitate comparison with the estimates in the literature and to investigate the effects of specific-capital and sharing on the unconditional turnover rates.

The results in Table 4 indicate that the quit rate falls with experience, tenure, and education; also, union, married, and nonwhite workers exhibit lower quit rates. Employment tenure, union membership, marriage, and being white reduce the layoff probability; and layoffs do not vary with experience or education. Although quits are strongly procyclical, an odd property present in these data is that layoffs are acyclical.

TABLE 4

UNCONDITIONAL QUIT AND LAYOFF PROBITS[c]

Regressors	Quits	Layoffs
constant	-0.586 (0.147)	-0.703 (0.172)
EXPER(0)	-0.018 (0.010)	0.005 (0.011)
$EXPER(0)^2$ /100	0.020 (0.024)	-0.009 (0.028)
TENURE(0)	-0.063 (0.007)	-0.082 (0.008)
$TENURE(0)^2$ /100	0.106 (0.020)	0.150 (0.022)
EDUCAT(0)	-0.026 (0.009)	-0.013 (0.011)
UNION(0)	-0.253 (0.047)	-0.105 (0.054)
MARRIED(0)	-0.155 (0.058)	-0.174 (0.068)
RACE	0.276 (0.048)	-0.109 (0.055)
CYCLIC	0.058 (0.012)	-0.003 (0.013)
Industry(0)	yes [32.2]	yes [48.4]
Occupation(0)	yes [13.6]	yes [40.4]
log-likelihood	-2,318.3	-1,539.2
psuedo-R^2	.084	.104

To understand these sign configurations, it is useful to consult the results in Tables 2 and 3. Consider several examples: Since experience reduces both the separation rate and the conditional quit rate, the two effects are reinforcing in reducing the quit rate and are counteracting in influencing the layoff rate. If the effect of experience on $\bar{q}$ were to dominate its effect on s, the layoff rate would be increasing in experience. The results in Table 4 indicate that the two effects are offsetting as experience does not effect the layoff rate. The results for employment tenure are clear: the effect of tenure on the separation rate dominates any effect on the conditional quit rate. Both quits and layoffs are strongly decreasing in tenure.[6] That union status decreases separations and conditional quits yields an ambiguous prediction for its effect on layoffs. Again the dominance of the separation rate effect is established here by the negative coefficient on the union dummy in the layoff probit. The results in Tables 2 and 3 indicate that white racial status increases both separations and conditional quits. Hence the combined effect of race on layoffs is ambiguous <u>a priori</u>. The negative coefficient associated with the dummy variable RACE in the layoff probit indicates the conditional-quit-rate component dominates for this variable. If the cyclical variation in the separation rate were weak, the procyclical conditional quit rate would account for the well-known regularities of procyclical quits and countercyclical layoffs. However, the results in column (5) reveal that the procyclical variation in the separation rate in these data is strong enough to make the layoff rate essentially acyclical.

The estimates in Table 5 include the pre-separation wage as a regressor. The results in columns (S1) and (S2) exhibit a strong negative effect of the pre-separation wage on the separation rate even with employment tenure controlled for. This suggests a strong specific-capital element which is not

TABLE 5

TURNOVER PROBITS WITH THE PRE-SEPARATION WAGE[c]

Regressors	Quits Q1	Quits Q2	Layoffs L1	Layoffs L2	Separations S1	Separations S2
constant	-0.464 (0.147)	-0.517 (0.148)	-0.576 (0.179)	-0.620 (0.174)	0.081 (0.128)	0.400 (0.023)
log W(0)	-0.352 (0.062)	-0.276 (0.063)	-0.345 (0.075)	-0.245 (0.077)	-0.403 (0.055)	-0.073 (0.010)
EXPER(0)	-0.037 (0.009)	-0.014 (0.010)	-0.021 (0.011)	0.008 (0.012)	-0.038 (0.008)	-0.002 (0.002)
EXPER(0)^2 /100	0.036 (0.024)	0.011 (0.024)	0.018 (0.027)	-0.015 (0.028)	0.038 (0.020)	0.002 (0.004)
TENURE(0)		-0.059 (0.007)		-0.079 (0.008)		-0.017 (0.001)
TENURE(0)^2 /100		0.101 (0.021)		0.148 (0.022)		0.037 (0.004)
EDUCAT(0)	-0.010 (0.010)	-0.014 (0.010)	0.009 (0.011)	-0.004 (0.011)	-0.006 (0.008)	-0.002 (0.002)
UNION(0)	-0.216 (0.049)	-0.192 (0.049)	-0.069 (0.057)	-0.407 (0.058)	-0.192 (0.042)	-0.028 (0.008)
MARRIED(0)	-0.153 (0.058)	-0.141 (0.059)	-0.174 (0.067)	-0.162 (0.067)	-0.194 (0.051)	-0.044 (0.011)
RACE	0.309 (0.048)	0.310 (0.049)	-0.071 (0.054)	-0.082 (0.055)	0.177 (0.041)	0.035 (0.008)
CYCLIC	0.038 (0.011)	0.061 (0.012)	-0.029 (0.013)	-0.001 (0.013)	0.011 (0.010)	0.008 (0.002)
Industry(0)	yes [39.6]	yes [29.6]	yes [67.0]	yes [49.0]	yes [90.2]	yes [61.6]
Occupation(0)	yes [7.6]	yes [9.6]	yes [37.0]	yes [35.0]	yes [16.6]	yes [13.6]
log-likelihood	-2,346.3	-2,308.7	-1,583.3	-1,534.2	-3,182.3	-3,092.9
psuedo-R^2	.073	.088	.078	.107	.077	.103

tenure related. In unreported results, I find that the pre-separation wage has no effect on the conditional quit rate. These two results yield the prediction that both quits and layoffs are negatively related to the pre-separation wage in these data. This is borne out in the quit and layoff rate regressions in columns (Q1), (Q2), (L1), and (L2). A ceteris paribus increase in the pre-separation wage of ten percent reduces the quit rate by approximately half a percentage point, and the layoff rate by appoximately a third of a percentage point. Notice that when tenure is included in the model (columns Q2 and L2), the effect of the pre-separation wage is diminished.

With the pre-separation wage included as a regressor, the functional form of the quit and layoff rate model is similar to that of Parsons (1972) and Antel (1985). However, the estimates differ considerably. Antel, using National Longitudinal Survey data, finds that the pre-separation wage variable is not statistically significant in the layoff rate probit.[7] This suggests that the effect of the pre-separation wage on the conditional quit rate is strong in his data. If the theory of these two off-setting influences is true, this should be documented using my decomposition on Antel's data. The results from my data indicate that only the specific-capital element applies.

Wage Growth

In Table 6, I report the average growth rate of wages by various turnover and age groups. The results in the first row indicate that job movers have only slightly more wage growth than stayers. More striking is the difference between quits and layoffs: quits exhibit the highest growth (5 percent), layoffs the lowest (2.6 percent), and the difference between the two is statistically significant. Quits clearly exhibit positive wage growth in the employment transition; while there is no evidence of negative wage growth for layoffs, a test of zero wage growth for layoffs cannot be rejected.[8]

TABLE 6

WAGE GROWTH BY TURNOVER STATUS AND AGE GROUP
(ΔlogW X 100)

Age Range	Sample: Full	Stayers	Quits	Layoffs
18–58	3.72	3.66	4.96	2.57
	(0.23)	(0.23)	(1.25)	(1.57)
	[10,043]	[8,794]	[842]	[407]
18–24	7.01	7.07	7.89	4.77
	(0.65)	(0.65)	(2.17)	(2.96)
	[1,762]	[1,325]	[287]	[150]
25–29	4.43	4.41	5.72	1.18
	(0.49)	(0.48)	(1.97)	(3.10)
	[2,425]	[2,022]	[294]	[109]
30–34	3.93	3.90	4.25	4.16
	(0.52)	(0.49)	(3.52)	(4.22)
	[1,592]	[1,433]	[107]	[52]
35–39	2.00	2.43	−4.47	0.43
	(0.63)	(0.64)	(3.40)	(2.97)
	[1,024]	[941]	[56]	[27]
40–44	1.13	2.02	−11.43	−3.53
	(0.74)	(0.64)	(7.86)	(4.34)
	[847]	[778]	[47]	[22]
45–49	2.06	1.64	19.23	0.61
	(0.63)	(0.62)	(6.16)	(6.38)
	[996]	[954]	[25]	[17]
50–58	2.11	2.11	3.24	1.31
	(0.64)	(0.65)	(4.71)	(5.39)
	[1,397]	[1,341]	[26]	[30]

Note: Standard deviations of the <u>means</u> in parentheses. Number of observations in brackets.

Consider next the life-cycle variation in wage growth. For the full sample (the first column), wage growth clearly falls with age. The result carries over for stayers as well. The evidence on wage growth over the life cycle is not clear for either quits or layoffs. Perhaps the strongest life-cycle irregularity is for quits: quits in their early forties have wage growth averaging -11 percent; for those quits in their late forties, this averages 19 percent. However, the sample means for quits and layoffs by age are not precise -- the sample sizes are small and there is large underlying cross-sectional variation in wage growth rates. (See McLaughlin (1986c) for an analysis of the diffuseness of empirical wage growth distributions in the PSID.)

A common practice in the empirical analysis of wage growth by turnover status is to control for growth-related variables (Bartel and Borjas 1981; Antel 1985; Mincer 1986). If growth-related variables such as education, experience, employment tenure, union status and business cycle shocks are correlated with turnover status, the estimated effects of turnover status on wage growth are biased.[9] By including these variables as regressors in wage growth regressions, one can compare the wage growth of quits, layoffs, and stayers holding constant observable characteristics.

In Table 7, I report OLS wage growth regressions which control for several growth-related variables. The parameter estimates associated with the experience and tenure variables correspond with a concave life-cycle wage profile: wage growth declines with both experience and employment tenure. Also evident in Table 7 is that wage growth is increasing in education, is lower for union workers, and covaries positively with business cycle movements.[10] The quit and layoff dummies indicate that quits have approximately the same wage growth as stayers with similar characteristics,

TABLE 7

WAGE GROWTH REGRESSIONS[a]
(ΔlogW X 100)

Regressors	(1)	(2) Direct	(2) Q-Interac	(2) L-Interac
constant	5.04 (1.54)	5.57 (1.60)		
EXPER(0)	-0.153 (0.070)	-0.163 (0.072)	0.113 (0.145)	0.226 (0.166)
EXPER(0)^2 /100	0.003 (0.001)	0.003 (0.002)		
TENURE(0)	-0.546 (0.096)	-0.490 (0.099)	-0.724 (0.277)	-0.350 (0.327)
TENURE(0)^2 /100	0.014 (0.003)	0.012 (0.003)		
EDUCAT(0)	0.244 (0.096)	0.168 (0.100)	0.863 (0.328)	1.347 (0.532)
UNION(0)	-1.136 (0.515)	-0.355 (0.539)	-8.619 (1.956)	-8.164 (2.633)
MARRIED(0)	-0.282 (0.688)	-0.311 (0.687)		
RACE	0.574 (0.547)	0.580 (0.546)		
GOVT(0)	-1.176 (0.723)	-1.105 (0.722)		
SOUTH(0)	0.443 (0.526)	0.413 (0.525)		
ΔCYCLIC	0.517 (0.096)	0.470 (0.010)	0.257 (0.335)	0.236 (0.478)
Q	-0.597 (0.863)	-7.38 (4.46)		
L	-2.908 (1.196)	-17.15 (6.83)		
Industry(0)	yes [1.54]	yes [1.53]		
R^2	.017	.023		

but layoffs exhibit significantly lower wage growth (-2.9 percent) in the employment transition. These results are similar to those of Mincer (1986) who also employs the PSID; however, using superior wage data, Antel (1985) estimates a similar wage growth regression and finds that the average wage growth of quits (layoffs) is 7.4 percent higher (6.7 percent lower) than that of stayers.

Mincer (1986) investigates the interaction of turnover status with growth-related variables in wage growth regressions. He finds, for example, that the wage growth of both quits and layoffs declines over the life cycle. I report results from my investigation of these interactions in regression (2) of Table 7. The column headed "Direct" lists the OLS parameter estimates of the direct effects on wage growth. The other two columns under regression (2) list the two types of interaction effects: quit interactions and layoff interactions.[11] For instance, 0.863 is the estimate of the parameter associated with the variable Q X EDUCAT(0).

Several conclusions emerge from the estimates in Table 7. First, comparing the results in regression (1), which suppresses the interactions, with the estimated direct effects in regression (2), I conclude that these terms are largely invariant to the inclusion of the interactions. Next consider the estimated interaction effects. Since the signs are the same and the magnitudes similar across turnover status, the interaction effects work the same way across quits and layoffs. The strongest interaction effects are for education and union status: Wage growth in the employment transition is increasing in education for both quits and layoffs; workers leaving union employment exhibit eight percent lower wage growth for both quits and layoffs. The interactions of turnover status with employment tenure suggests that wage growth between jobs declines with tenure for quits and (weakly) for layoffs.

The interaction effects for experience and business cycle shocks are essentially zero. (Note that these results control for the direct effect of each variable on wage growth).

III. CONCLUSIONS

In this note, I suggest an empirical strategy for analyzing the quit-layoff distinction: decompose the quit and layoff rates into the component parts -- the separation rate and the quit rate conditional on separation. This strategy is employed in investigating the empirical regularities listed in the introduction. (See the introduction for the list of results.)

I offer several conclusions. First, the determinants of turnover have much stronger effects on the separation rate than on the conditional quit rate. Although one can reject the null hypothesis of no behavioral difference between quits and layoffs, the differences at the individual level are at best weak. The set of turnover determinants account for a small fraction of the variation in quits conditional on separation. This is common in economic models with qualitative dependent variables estimated on data at the individual level. Even the strong co-movements between separations and the determinants of turnover account for only about one-tenth of the variation in the separation probability.

Second, the statistical decomposition to the separation and conditional quit rate is successful in accounting for the sign configuration and magnitudes of the the estimated coefficients of the unconditional quit and layoff rates. Regarding the pre-separation wage, I find that the "sharing effect" is absent in the Panel Study of Income Dynamics (PSID) data: the pre-separation wage captures only the effect of specific-capital.

Furthermore, I conclude that specific-capital is largely unrelated to tenure; it is match specific rather than accumulated.

Third, in terms of wage growth in the employment transition, quits clearly exhibit positive wage growth but the wage growth of layoffs is not significantly different from zero. That the wage growth of layoffs is not negative might be an artifact of the sampling frame of the PSID. (See note 8 above.) Counter to the results of Mincer (1986), neither experience nor cyclical movements interacts with turnover status in accounting for wage growth. Strong interaction effects are established for education, union status, and employment tenure.

Are theories of the quit-layoff distinction consistent with these empirical regularities? The regularities are rich enough in detail that competing models are likely to be distinguishable on at least a few margins. Various models of the quit-layoff distinction are designed to account for only (a) procyclical quits and countercyclical layoffs, or (b) wage growth of quits exceeding the wage growth of layoffs. These models are potentially refutable with respect to the other regularities.

NOTES

1. The data are described fully in McLaughlin (1986a); see especially the data appendix. The variable CYCLIC, a business cycle variable, is the deviation of (log) real GNP from its long-run trend (McLaughlin 1985). The analysis in this paper excludes those age 59 and older because preliminary results indicated that retirement behavior complicates the analysis.

2. The derivitive $\bar{q}'(X)$ includes the indirect effect of y on the probability of inclusion in the conditioned upon sample:

$$\bar{q}'(X) = \frac{\partial Pr[Q=1 \mid S=1, X]}{\partial y} + \frac{\partial Pr[Q=1 \mid S=1, X]}{\partial Pr[S=1 \mid Z]} \cdot \frac{\partial Pr[S=1 \mid Z]}{\partial y} .$$

3. Estimates of the probit coefficients are reported in Tables 2-5. In the text, for each variable and turnover rate, I indicate the variable's partial effect. The partial effects are derived by multiplying the estimated probit coefficients by a turnover-rate-specific factor of proportionality. Consider the separation rate case: $s(Z) \equiv \Phi(Z\gamma)$ where Φ is the standard normal cumulative distribution function. Hence γ is the vector of probit coefficients. The partial effect of any regressor z in Z is

$$\frac{\partial s}{\partial z} = \phi(Z\gamma) \cdot \gamma_z ,$$

where ϕ is the standard normal probability distribution function. I evaluate ϕ at the sample mean of s. For the separation rate, ϕ_s = .24913; for the conditional quit rate, $\phi_{\bar{q}}$ = .37748; for the quit rate, ϕ_q = .16264; and for the layoff rate, ϕ_ℓ = .11579.

4. The behavior of the 18-24 age group is mysterious. Leighton and Mincer's (1982, 242) evidence is counter to mine in that their estimated conditional quit rate declines monotonically in age. Also, an important feature is omitted from my results: retirees are nearly all quits.

5. The sign of the effect of tenure on the conditional quit rate is sensitive to the method of estimation. The sizeable positive effect reported in the text is replaced by a sizeable negative effect when estimated by maximum likelihood.

6. Education is predicted to have an ambiguous effect on the quit rate since it reduces separations but increases quits relative to layoffs. The negative coefficient on EDUCAT(0) in the quit probit indicates the dominance of education's effect of the separation rate. In fact, the negative

effect is stronger in the quit probit than in the layoff probit; this can occur only because the quit and layoff rates are estimated as univariate probits.

7. Antel (1985) excludes layoffs from his quit probit and quits from his layoff probit. In unreported results, I find that this does not account for the differences across the two data sets: the probit estimates in Table 5 are largely unaffected by these exclusions.

8. To account for many of the empirical regularities documented in this paper, the model of the quit-layoff distinction in McLaughlin (1986b) requires that layoffs exhibit negative wage growth in the employment transition. There are several reasons why negative wage growth for layoffs would not appear in the data even if it does hold. First mean wage growth for layoffs is likely to be biased upward since subsequent wages are not observed for the unemployed (who are predominantly layoffs). However, since the number unemployed in the subsequent period is low (roughly 3 percent), the bias is not sizeable enough to rationalize the observed positive wage growth of layoffs. Second, the sampling frame biases against the hypothesis of negative wage growth of layoffs. Survey respondents in the PSID who change employers in the year between surveys report the reason for the separation (e.g., quit or layoff). In addition, I use the wages at the two survey dates to compute wage growth. Combined, these two features are problematic for the following reason. Intervening employment spells are censored. Layoffs could go to lower paying jobs (i.e., negative wage growth), and subsequently quit to a higher paying job which is held at the second survey. The transition would be reported as a layoff but the wage data could reveal positive growth. (A similar argument implies that the average wage growth of quits is biased down.) The use of retrospective employment history surveys, such as in parts of the National Longitudinal Survey (NLS), would rectify this problem.

9. In the current work, I ignore the endogeneity of quits and layoffs which is apparent from the probit results above. In related research, I intend to control for the endogeneity of turnover status in estimating wage growth regressions. See McLaughlin (1986b) for a start on this problem.

10. The business cycle variable CYCLIC is differenced in this specification since, unlike the life-cycle variables, it is not monotonically related to wage growth: growth is zero at both the peak and the trough of the cycle.

11. I limit the interaction effects to those variables which are statistically significant in regression (1) and exclude the quadratic terms.

NOTES TO TABLES 2-5 AND 7

Asymptotic standard errors in parentheses. In brackets are the likelihood ratio test statistics associated with tests of the joint significance of either the eleven industry dummy variables or the eight occupation dummy variables.

Psuedo-R^2 is McFadden's measure: $R^2 \equiv 1 - \mathcal{L}(k)/\mathcal{L}(0)$, where $\mathcal{L}(k)$ is the log-likelihood with k (nonconstant) regressors. The variables with names ending in "(0)" take their pre-separation values.

- *a* 10,922 observations per probit
- *b* 1,160 observations per probit. Observations with S=0 or age$\leq$24 are excluded.
- *c* 8,928 observations per probit. Excluded are observations with age$\leq$24.
- *d* 10,043 observations per least squares regression. 879 observations which do not have a reported wage in the subsequent period are excluded.

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