An Analysis of Job Destruction and Downsizing

by

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Abstract

This thesis explores trends in employment growth over recent decades, providing empirical evidence to dispel widespread downsizing claims. The objective of this series of essays is to both chronicle such temporal shifts and also investigate the role that financial constraints have played in determining the employment decisions of firms. I have sought to establish an empirical body of work that details the extent of contemporary job destruction episodes while presenting possible reasons for firm-level employment behavior.

Chapter 1, entitled "An Analysis of Downsizing: 1976-1994," assesses the relationship between job creation and job destruction and pays close attention to the behavior of job destruction in the 1990s. Results from the Compustat sample spanning 1976-1994 indicate: 1.) no secular increase in the aggregate rate of job destruction, 2.) a growing dispersion of total destruction across major industrial sectors, and 3.) a widening in firm-level employment growth distributions across time. Rising rates of job destruction in the 1990s are observed in three non-manufacturing industries – wholesale and retail trade; finance, insurance and real estate; and health, legal and educational services. However, these increases become statistically negligible after controlling for each industry's gdp growth. This chapter rejects claims of aggregate downsizing in the 1990s and serves as a platform for further theoretical and empirical investigations of firm-level job creation and destruction behavior.

Chapter 2 is titled "The Labor-Liquidity Relationship: The Effects of Financial Constraints on Employment Growth." Traditional labor hoarding models predict that, in the face of significant adjustment costs, firms opt to smooth employment patterns over expected product demand cycles. Underlying these partial adjustment theories is the assumption of perfect capital markets. In this essay, I argue that firms with limited access to capital will exhibit higher employment elasticities with respect to sales growth than financially liquid, or unconstrained, firms. I designate firms as financially constrained using two measures: 1.) the median dividend-to-income ratio and 2.) the Standard & Poor's senior debt rating. Results from regression estimations support my initial conjecture of excess employment sensitivity to sales. The employment pattern of

financially constrained firms is significantly more responsive to sales fluctuations than that of their unconstrained counterparts. Smoothing, or labor hoarding, is practiced to a larger degree by firms with access to less costly external capital and greater proportions of internal funds.

"Long-term Job Stability: Still A Reality?" is the title of the final chapter. Chapter 3, extending the findings of Chapter 1, chronicles the tenure experience of American workers from 1968-1992. The popular belief is that the pool of low-tenured workers has become increasingly composed of previous lifetime job holders. The economic implications of increased displacement of long-term job holders are substantial, potentially signaling lower adjustment costs, increasingly obsolete skill sets, or the emergence of a "temporary, outsourcing" workforce. This chapter supplements recent literature on the topic of declining tenure by utilizing a new measure of job security that takes advantage of the Panel Study of Income Dynamic's longitudinal properties. I estimate shifts in aggregate tenure and the tenure distribution (as approximated by the PSID sample). Unlike past studies which have, at best, been able to trace synthetic cohorts biannually, this essay directly measures the employment survival rates – the probability of continuing a job in a subsequent year – of selected subgroups. It then tracks such probabilities through time. I find virtually no evidence to substantiate either a decline in overall tenure or a decline in long-term job stability.

Thesis Advisor: Olivier J. Blanchard, Class of 1941 Professor of Economics
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CONTENTS

CHAPTER ONE

1. INTRODUCTION

Popular accounts of the U.S. labor force cite the recent surge in downsizing episodes as an immense, if not the principal, concern of American workers today. The dark cloud that has formed over "the great American job," these reports contend, has come in the form of extensive, new layoff strategies, or downsizing, aimed at "cutting the fat" from inefficient corporate giants. Factors distinguishing contemporary turnover experiences from job destruction episodes of the past are perceived to be: 1.) the growing share of highly educated, highly tenured, and highly skilled displaced workers 2.) the lack of equally high paying post-displacement employment options, 3.) the increase in job destruction by firms and industries previously thought to be immune to downsizing activity, and 4.) the dramatic increase in the number of jobs destroyed in the most recent wave of layoffs. It is claimed that these four factors have rendered both job stability and security, for a large and growing segment of the U.S. labor force, outdated concepts. Whether such factors can be empirically substantiated is the focus of this essay.

Thus far, examinations of the well-publicized downsizing trend have originated from the "employee" perspective, applying individual survey data to tackle the first two factors mentioned above. Relying on such sources as the Current Population Survey (CPS) and the Panel Study of Income Dynamics (PSID), these studies compute annual changes in job separation and retention rates as well as the subsequent impact of displacement on wages.[1]

Conversely, there exists little, if any, corresponding research from the "industry" perspective, incorporating firm-level data to investigate the latter two factors. Few studies have analyzed the extent to which job destruction has both risen in the 1990s and been concentrated in certain sectors of the economy. Anecdotes and individual case

[1] Farber (1996) and Rose (1995), respectively, find that the rate of involuntary job loss remained steady, but high, among those who are less skilled and/or "blue collar." With a new employer, initial job losers earned less income than they had in their previous position. This finding supports research by Stevens (1994) on the future wage outcomes of job displacement.

studies of downsizing, meanwhile, have flourished. Companies most often the targets of such scrutiny include IBM, General Motors, AT&T, Exxon and Digital Equipment Corporation, whose combined layoffs between 1991 and 1994 sum to 259,000 jobs.[2]

This essay approaches the downsizing issue by offering evidence to identify shifts in both the concentration and the magnitude of aggregate job destruction. Utilizing annual Compustat data from 1975 to 1994, I provide an empirical answer with which to view the possible sources of such layoffs. I offer evidence suggesting that aggregate job destruction rates – either on average or adjusted for the effects of the business cycle – have not risen in the 1990s. Disaggregated industry analyses, however, do reveal slightly higher destruction activity in three major industry classes – wholesale and retail trade; finance, insurance and real estate; and health, legal and educational services – when controls for industry gdp growth are excluded from the analysis. These findings may help to explain why much of the attention paid to downsizing focuses on the circumstances of white-collar, or non-manufacturing, workers.

The discussion highlights the following dimensions of employment dynamics in an attempt to distinguish the popular perception of higher aggregate downsizing from the empirical facts concerning aggregate and industry-specific job destruction:

1.) Rates. I find that the aggregate job destruction *rate* has remained relatively stable in the 1990s (with a moderate spike in 1991), whereas the job creation rate has climbed steadily upward. While a larger number of employment positions were destroyed, job destruction rates of the 1990s were offset by an expansion in the pool of existing jobs, due in large part to higher creation rates in the mid- to late 1980s. The aggregate job destruction has not grown significantly. Meanwhile, at the industry-level, three major non-manufacturing sectors did, in fact, exhibit statistically significant, albeit non-secular, increases in the incidence of job destruction.

2.) Concentration. Results from three measures of concentration confirm a diminished role of manufacturing in aggregate layoffs, suggesting that job destruction

[2] This figure is computed with annual data from Compustat.

has become increasingly concentrated in other sectors of the economy.[3] The dispersion of job destruction is further validated by evidence of higher job destruction rates in three of the eight major non-manufacturing sectors.

3.) Distribution. Results suggest that employment at the level of the firm (as observed in each year of the sample) is most frequently adjusted in small increments, but that the size of such increments has increased over time. Histograms and kernel density illustrations indicate that the majority of firms are clustered within and around a single employment growth spike, the placement of which is strongly correlated with aggregate economic growth. The distribution of firm-level employment growth becomes increasingly dispersed over the nineteen-year sample period. The standard deviation of annual firm-level employment growth rates rises steadily throughout the sample. Such results support the perception that the bulk of job destruction activity, particularly in the 1990s, has been increasingly comprised of larger discrete downsizing events. This may also suggest that the fixed costs of adjusting labor, either by hiring or firing employees, have grown over the sample period.

In sum, this essay examines the properties of job destruction from a time dimension. It offers strong evidence regarding changes in the incidence, concentration and distribution of job destruction activities at the industry and aggregate levels, paying particular attention to shifts occurring in the later years, 1990-1994, of the sample.

2. LITERATURE REVIEW: A Comparison to Existing Evidence

The most comprehensive empirical works on the subjects of job creation, job destruction and aggregate employment fluctuation are by Davis and Haltiwanger (1990, 1992) who use plant-level data from the Longitudinal Research Database and Annual Survey of Manufactures in amassing a range of descriptive information on job creation

[3] In the case of firms, yearly concentration ratios, the C-10 and C-20, reveal a decrease in the concentration of job destruction among the ten and twenty biggest annual job destroyers.

and destruction dynamics in manufacturing industries. Davis and Haltiwanger (1990,1992) and Davis, Haltiwanger and Schuh (1996) estimate mean creation and destruction rates from 1972 to 1988 equal to 9.1 percent and 10.3 percent, respectively.[4] The maximum creation rate of their sample stood at 13.3 percent in 1984. Conversely, the destruction rate peaked at 15.6 percent in 1983. In addition, Foote (1997), analyzing Michigan firms from 1978-1988, presents job creation and destruction rates of 9.6 percent and 10.0 percent, respectively.

There is a clear disparity between the figures presented in this paper and those of Davis and Haltiwanger and Chris Foote. The aggregate creation and destruction rates computed in this paper fall several percentage points below the authors' numbers. Possible reasons for the discrepant results are as follows. First, the evidence presented by Davis and Haltiwanger applies only to the nondurable and durable manufacturing sectors. The findings of this paper incorporate data from all ten major industrial classes. Nevertheless, manufacturing-specific calculations of mean job creation and destruction from 1990 to 1994 fail to yield rates as high as those of Davis and Haltiwanger; the average 1990-1994 destruction rates in the nondurable and durable manufacturing sectors were 3.28% and 3.49%, respectively. The reason for this inconsistency, I suspect, stems from a plant- versus firm-level distinction. Both studies generate descriptive statistics of two separate, but related entities – the plant and the firm.

The unit of analysis in Davis and Haltiwanger's piece was the manufacturing plant while this paper's focus is firm-level employment changes. Combining this paper's findings with those of Davis and Haltiwanger may imply that firms display smoother employment patterns than do their existing plants. Firms may shift employees from one plant to another or expand one plant while reducing the employment count at another. Such practices may entail moving employees between plants without opening job vacancies or laying off employees. Firm-level data registers only the change in the total employment numbers of a firm from one year to the next. Consequently, relocation activity is eliminated from creation and destruction calculations. Plant-level analysis,

[4] These numbers are considerably larger than those here where I find the average creation rate to stand at 4.68 percent and the average destruction rate at 3.03 percent.

conversely, accounts for all shifting activity within a firm and labels it "job creation" or "job destruction." Summing these plant-level changes over the entire firm yields smaller discrete fluctuations in employment as losses recorded at some plants are offset by employment gains made in others. Therefore, the large difference between these creation and destruction figures and those of Davis and Haltiwanger may simply be a product of the chosen levels of analysis.

A final reason for the disparity may lie in the sample selection processes of each dataset. As described earlier, the plant-level data used by Davis and Haltiwanger originates from the Longitudinal Research Datafile and the Annual Survey of Manufactures – datasets containing information on plants with as few as 5 employees. Those plants with 250 or more employees appear in their samples with certainty and the probability with which smaller plants are included decreases thereafter with size.[5] Similarly, the firm-level data used by Foote was not restricted by size. Conversely, the Compustat extract incorporates data from publicly traded companies that tend to be larger in terms of employment levels and sales. The fact that these companies issue securities exchanged on either the NYSE, NASDAQ or AMEX markets suggests that they are a more established class of firms than those remaining privately-owned. The relative stability of these firms may therefore coincide with lower rates of job creation, destruction and reallocation.

Davis and Haltiwanger's disaggregated results further strengthen this hypothesis, indicating that: 1.) plants producing more diversified product lines exhibit less volatile job creation and destruction, 2.) the one-year survival rates for existing jobs is positively correlated with the size and age of the employing firm, and 3.) larger firms experience lower rates of job reallocation than do their smaller counterparts. Furthermore, Davis and Haltiwanger conclude that two-thirds of creation and destruction occur in plants altering employment levels by more than 25 percent while plant closures account for one-forth of job destruction. Since the Compustat extract contains only those presently existing and continuing firms, it eliminates a significant source of employment fluctuations used in

[5] Davis and Haltiwanger present selected time average statistics for the larger companies in their sample. This limits possible comparisons of their results to those of this paper.

Davis and Haltiwanger's calculations and therefore yields smaller rates of jobs creation and destruction. The results from this chapter should be interpreted as applying to comparatively larger, more established firms – those firms most often the target of downsizing accounts.

3. DATA

3.1 Description

Compustat incorporates firm-level information from all industries, manufacturing and non-manufacturing. Standard & Poor's compiles the information found in dataset from a variety of sources including annual and quarterly shareholder's reports, the Wall Street Journal, news releases and newswires, and firm 10-Ks, 10-Qs and 20-Fs.[6] Compustat contains data on roughly 7,000 publicly traded U.S. companies from 1975-1994, spanning all ten 1-digit SIC codes described below:

1-Digit SIC Code	Broad Industrial Category
0	Agriculture
1	Mining and Construction
2	Manufacturing, Nondurables
3	Manufacturing, Durables
4	Transportation, Communication, and Utilities
5	Wholesale and Retail Trade
6	Finance, Insurance and Real Estate
7	Services: Business, Personal, Entertainment, etc.
8	Services: Health, Legal, Education, Personal
9	Public Administration

The extract possesses approximately 49,000 observations and includes variables such as employment level, 4-digit SIC code,[7] sales, net income, debt-to-asset ratio, and most information typically found in the balance sheets of firms. The Compustat manuals

[6] According to Compustat, the data is subject to revision upon the discovery of supplemental reports and general double-checking activity.
[7] A firm is assigned the 4-digit SIC code of the industry from which the largest percentage of its sales originates. Such information is gathered from 10-Ks and annual reports.

document shareholder's reports as the primary source of most of the variables used in this study including the annual number of employees on a firm's payroll.[8]

Also captured in the sample is a variable AQS, or the Acquisition-Sales Contribution, defined in Compustat as the effect of "a purchase or pooling of interest acquisition in the current year on a company's sales for the prior year." The variable AQS was of great relevance to employment rate calculations since the objective was to find the actual job creation or destruction of a firm, not changes in employment due to a merger, sell-off, or acquisition. Since Compustat offers no information regarding the effect of an acquisition on the employment level of a firm, the employment data needed to be corrected for such business activity. The correction procedure is described below.

1.) All firms with no acquisitions for a given year were grouped by 4-digit SIC code and firm size where the size breakdown, based on the sales levels of firms,[9] consisted of three categories -- small , medium, and large. The cutoffs for the small and medium groupings were the 33rd and 66th percentiles, respectively, of firm sales within annual sic-size subsamples. For example, denote a firm's sales in a given year as Q_y and assume that S_{iy} and M_{iy} are the computed size cutoffs of non-acquiring companies in a given industry and year. If

$Q_y < S_{iy}$,	firm = small
$S_{iy} <= Q_y < M_{iy}$,	firm = medium, or
$Q_y < M_{iy}$	firm = large,

[8] The Compustat manuals describe the form in which the "Employees" variable is reported as either "an average number of employees" over the year in question or "the number of employees at year-end." The manual notes that "No attempt has been made to differentiate between these bases of reporting. If both are given, the year-end figure is used." As a rule, the employee totals given encompass full-time, part-time and seasonal workers while omitting contract workers, consultants and employees of unconsolidated subsidiaries.

[9] The precise definition of sales (net), as provided in the Compustat manual, is "Gross sales (the amount of actual billings to customers for regular sales completed during the period) reduced by cash discounts, trade discounts, and returned sales and allowances for which credit is given to customers." This figure includes installment sales, franchise sales and operating revenue and excludes interest income, non-operating income, rental income, royalty income, discontinued operations and any gains from the sale of securities.

where the subscripts represent the 4-digit industry and year, respectively. The average employee-sales ratio, $EMPSALE_{isy}$, was then computed for each sic-size-year cell.

2.) Acquisitions were identified as small, medium or large using by the same annual per-industry cutoffs computed in the previous step and then, based on these size designations, industry, and year, were assigned the appropriate employee-sales ratio computed in the previous step. For instance, if an acquisition by a company in sic 3571 in 1986 was recognized as "small" in terms of sales, it was then matched to the small firm, sic 3571, 1986 employee-sales ratio, or $EMPSALE_{3571, s, 1986}$, calculated above using the sample of non-acquiring firms. Multiplying AQS* $EMPSALE_{isy}$ yielded the final estimation of the number of employees gained or lost through the acquisitions or sell-offs of a firm; this variable was called AQEMP. Note that Compustat contains limited information about the type of acquisition performed and provides no information on the acquired company (if relevant).

3.) AQEMP was then subtracted from the yearly reported employment totals of each firm, EMP, resulting in a corrected employment level termed NET. In most cases, no acquisitions occurred, rendering NET=EMP.

In order to account for the acquisitions, the calculations of the job destruction and creation rates involved slight modifications to formula proposed by Davis, Haltiwanger and Schuh (1996). The equation used for the firm-level job creation rate was

$$(NET_t - EMP_{t-1}) \,/\, [\tfrac{1}{2} * (NET_t + EMP_{t-1})]$$

where $NET_t - EMP_{t-1} > 0$. Computations of job destruction rate applied the same formula for those instances where $NET_t - EMP_{t-1} < 0$. While firms may have conducted both job creation and destruction activity during a given year, the nature of the year-to-year firm-level employment data permitted only the determination of whether a firm had more or less employment than it possessed during the previous year. The resulting change in firm employment from one year to the next (detailed in the numerator above) represents the within-year job creation minus job destruction, or net job creation. As described above, positive and negative values reflect job creation and destruction, respectively. Size-, industry- and aggregate-level job creation and destruction rates were then calculated as averages of the appropriate firm-level rates weighted by the corresponding NET, or corrected employment, value.

3.2 Subsampling: Firm Size

As mentioned in the introduction, a common portrayal of downsizing is that of a large, established corporation reducing its employee pool due to increased pressure by smaller or foreign competitors. In response to these claims, job destruction and creation rates were computed for groups of firms falling within three broad size categories. Firms with less than 5,000, 5,000-24,999, and 25,000 or more employees were classified as small, medium or large. Subsequent analyses were then performed on each group.

4. RATES

This section chronicles aggregate job creation and destruction rates as well as corresponding rates at the firm-size and industry levels. A discussion of the distribution of employment growth rates will follow in a later section. Figure 4.1 graphs aggregate creation and destruction rates for the years 1976-1994. The most striking features, apart from the variance of each series, are the relative positions of the creation and destruction rates. The aggregate job destruction rate surpassed the creation rate in only four years of the sample – 1980, 1981, 1991, and 1992 – the first three of which were years of negative real gdp growth. When these four years are excluded from the sample, the job creation rate is higher than the aggregate job destruction rate by an average of 2.4 percentage points. The largest differences came in the expansionary years of the late 1970s and the mid- to late-1980s.

A formal one-tail test of creation and destruction rates over the entire sample yields a t-statistic of 4.62, confirming the alternative hypothesis that the average creation rate exceeded that of destruction; the respective means were 0.047 and 0.030. Note the sharp rise in the job destruction rate from 1989 to 1991, only to be countered from 1991 through 1994 with a more sustained rise in creation. The same pattern appears in the earlier 1980-1982 recession, where creation seems to move procyclically, rising in the positive growth years following a recession. A discussion of the variance estimates of job creation and destruction is included in Appendix A.[10]

Figure 4.1

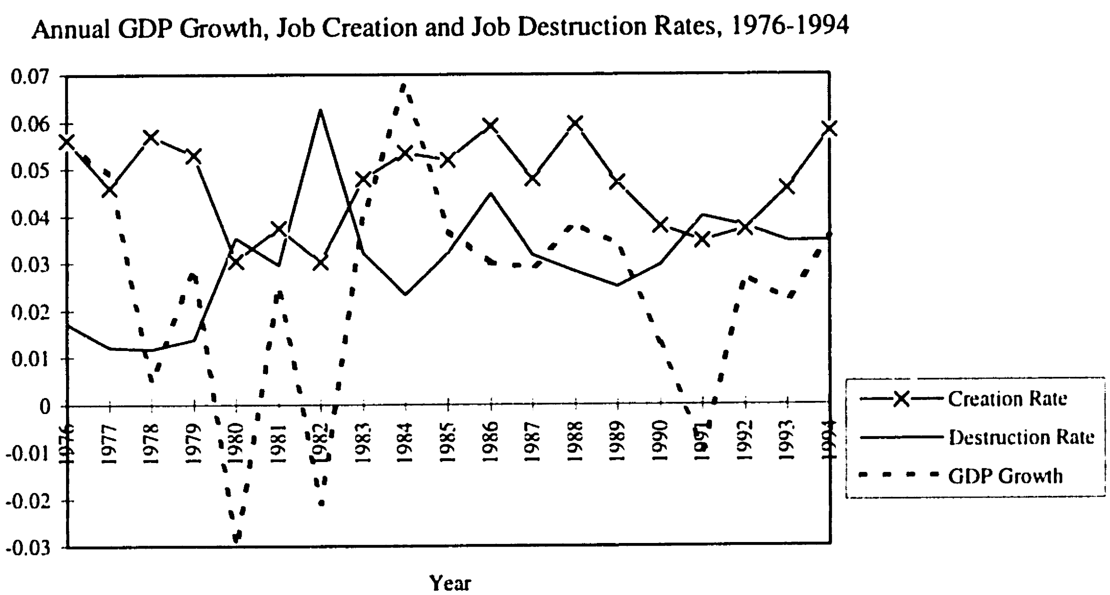


Figure 4.1 illustrates annual real gdp growth, job creation and destruction rates over the 1976-1994 sample. It may appear that the job destruction series displays a slight upward trend over the sample period. This perception is, in part, due to the comparably

[10] Davis and Haltiwanger show that destruction fluctuates dramatically over the business cycle. The countercyclical nature of destruction dominated employment dynamics; the variance of creation was comparatively slight. This paper suggests that creation and destruction have similar variances (as in Foote, 1997). Creation and destruction have procyclical and countercyclical properties of similar magnitude.

low job destruction rates of the 1970s and subsequent rise in the series in the early 1980s. Unfortunately, the Compustat extract reaches back only until 1975, so inferences of job destruction based on a wider time horizon are prevented. It may have been the case however that, prior to 1975-1976, aggregate job destruction rates more closely matched those observed in the later years of the sample. This scenario is feasible given the recessionary oil shock years of the early 1970s. Furthermore, evidence presented by Davis and Haltiwanger show distinctly higher manufacturing job destruction rates in the early 1970s than in the mid- to late-1970s. These findings support the hypothesis that the relatively low job destruction rates observed in the late-1970s were more a product of a temporary economic expansion than a long-term secular trend.

As for evidence of downsizing in the 1990s, the figure depicts destruction rates increasing from 1989 to 1991 as real gdp growth rates were falling. However, the heightened propensity for the economy to destroy jobs may be both side effect of and causal factor in most recessions. T-tests assuming unequal variances can not reject the null hypothesis of stable job destruction rates across the sample period. Table 4.1 contains the average job destruction rates and t-statistics yielded from the mean equality tests.

Table 4.1. Test of Equality of Job Destruction Rates
H_0: JD $Rate_{19xx\text{-}19xx}$ = JD $Rate_{1990\text{-}1994}$, H_1: JD $Rate_{19xx\text{-}19xx}$ < JD $Rate_{1990\text{-}1994}$

Null Hypothesis	T-Stat (P-Value of one-sided test)	JD Rate, Comparison Group	JD Rate, 1990-1994
JD $Rate_{1976\text{-}1989}$ = JD $Rate_{1990\text{-}1994}$	1.667 (0.057)	).028	0.035
JD $Rate_{1980\text{-}1989}$ = JD $Rate_{1990\text{-}1994}$	0.211 (0.418)	0.034	0.035
JD $Rate_{1985\text{-}1989}$ = JD $Rate_{1990\text{-}1994}$	0.778 (0.233)	0.032	0.035

Regardless of the comparison group, job destruction rates failed to deviate significantly from past levels when no other variables such as aggregate economic factors are taken into account.

4.1. Measuring the Effects of the Business Cycle

The more meaningful issue is not whether destruction rates have climbed in the 1990s, but, rather, if the rise in job destruction observed over the economic downturn of the 1990s was large in comparison to increases exhibited during past recessions. In order to measure changes in the response of job destruction and creation to real gdp growth, I construct peak-to-trough (and trough-to-peak) differences in both real gdp growth rates and job destruction rates, intending to capture movements in both series from the height of an economic cycle to its lowest point. I compute the following ratio for each identified business cycle:

$$(\text{JD Rate}_p - \text{JD Rate}_t) / (g_p - g_t)$$

where p and t represent the years of highest and lowest real gdp, respectively, and g is the real gdp growth rate. This ratio computes the movement in the job destruction (or creation) rate during a given aggregate economic downturn or growth period. The outcome of these calculations represents the slope between real gdp growth and the job destruction rate and can be broadly interpreted as the elasticity of job destruction with respect to gdp;[11] the results are reported in Table 4.2.

[11] The same calculations were performed for job creation. Trough-to-peak analyses were also conducted where the ratio computed was $(\text{JD Rate}_t - \text{JD Rate}_p) / (g_t - g_p)$.

Table 4.2. Peak-to-Trough and Trough-to-Peak Changes in Destruction and Creation

Dates (Peak-to-Trough)	Change in Real gdp Growth Rate	Change in JD Rate	Peak-to-Trough Ratio (Change in JD Rate/ Change in gdp Growth Rate)	Change in JC Rate	Peak-to-Trough Ratio (Change in JC Rate/ Change in gdp Growth Rate)
1976-1980	-0.059	0.018	-0.307	-0.026	0.436
1981-1982	-0.046	0.033	-0.716	-0.007	0.153
1990-1991	-0.023	0.011	-0.478	-0.003	0.130

Dates (Trough-to-Peak)	Change in Real gdp Growth Rate	Change in JD Rate	Trough-to-Peak Ratio (Change in JD Rate/ Change in gdp Growth Rate)	Change in JC Rate	Trough-to-Peak Ratio (Change in JC Rate/ Change in gdp Growth Rate)
1980-1981	0.028	-0.006	-0.202	0.007	0.251
1982-1990	0.034	-0.034	-1	0.008	0.235
1991-1994	0.036	-0.005	-0.150	0.023	0.649

For every one percentage point decline in real gdp growth from its peak in 1981 to its trough in 1982, the job destruction rate grew by 0.716 points. The slowing of the economy was clearly associated with a rise in job destruction rates. Surprisingly though, the impact of lower economic growth on job destruction rates appeared to decline with successive recessions as, in the 1990-1991 decline, aggregate destruction rose by 0.478 points for every one percentage point drop in real gdp growth. The 1990s upsurge in job destruction rates barely matched that of past recessions, contradicting accounts that downsizing both accelerated beyond historical rates.

Meanwhile, the trough-to-peak analysis indicates a disproportionate rise in job creation rates during the most recent expansionary period. The job creation rate grew 0.649 percentage points for every one point rise in real gdp growth since 1991. This greatly outpaced the response of creation to economic growth in former recovery periods where the trough-to-peak ratios from 1980-1981 and 1982-1984 stood were 0.251 and 0.235, respectively.

4.2 Adjusting for the Business Cycle

Job destruction activity, after controlling for the effects of the business cycle, has not increased. This issue was partly addressed in the peak-to-trough analysis above. Here, I conduct a more formal regression analysis to determine whether the portion of job destruction attributable to changes in the real rate of gdp growth has changed in the 1990s.

I begin the analysis with the following equation

$$\text{JD Rate}_t = a + i\, g_t + e_t$$

where g is the rate of real gdp growth, e_t is the residual at time t.[12] The constant term and the coefficients are then determined using a standard OLS framework. I caution that my objective is not to report the gdp parameter, for the equation written above clearly suffers from endogeneity problems. Rather, I am interested in documenting movements in the unexplained portion of job destruction.

The next step involves computing the predicted residual using the parameter values estimated from the equation above.[13]

$$\hat{e}_t = \text{JD Rate} - a - i\, g_t$$

The resulting predicted residuals represent the unobserved portion of destruction that is not accounted for by changes in real gdp growth. Mean comparisons of $\hat{e}_{1990\text{-}1994}$ to $\hat{e}_{1976\text{-}1989}$ then allow for the identification of a possible structural break in the relationship between job destruction and aggregate economic growth in the early 1990s. If the

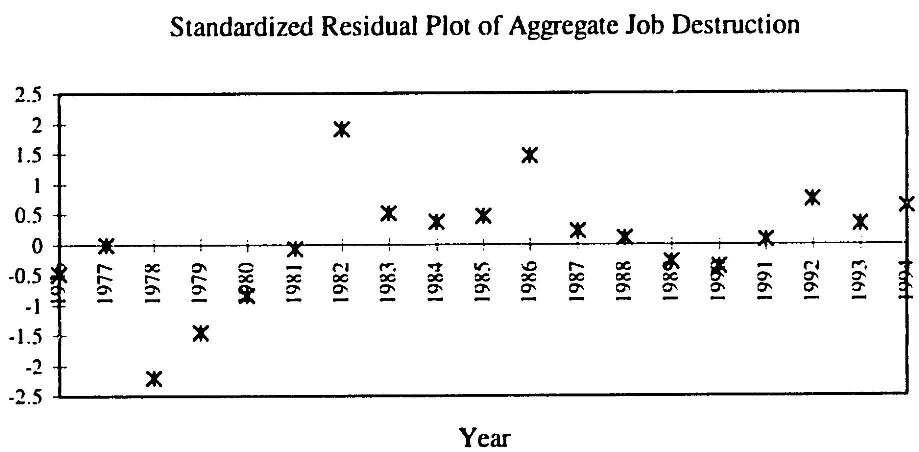
[12] The dummy variable for 1976-1979 was included in an additional regression to correct for an biases in the trend coefficient arising from the drastically lower job destruction rates in the 1970s. The results of t-tests do not change. Illustrated above are the standardized residuals from the regression without the 1970s dummy.

[13] An equivalent method is to include a dummy for the years 1990-1994. Such regressions yielded coefficient on the dummy that was not significantly different from zero. This agrees with the results of the following standardized residual test.

average predicted standardized residual for 1990-1994 is found to be significantly greater than that of the earlier years of the sample, this implies that, after adjusting for the effects of the business cycle, job destruction displays an unexplained rise over the 1990s.

Results from this analysis show that it is, in fact, not the case that downsizing has enjoyed an increased presence in the 1990s. Tests comparing the average estimated standardized residuals of 1990-1994 to the 1976-1989 mean indicate that the both sets of residuals are statistically indistinguishable. Figure 4.2 graphs these estimated residuals across the years of the sample.

Figure 4.2



The negligible difference between the residuals allows for a rejection of the increased job destruction hypothesis. The unexplained portion of job destruction has not increased, but has remained relatively stable over time. Any secular changes in the rate of job destruction after controlling for the strong effects of the business cycle are statistically insignificant.

4.3 Analysis at the Industry-Level

Having observed the lack of an increase in the aggregate job destruction rate, I now turn my focus to an examination of broad industry-level destruction rates in order to determine which industries, if any, experienced dramatic upswings in destruction rates during the 1990s. It may be the case that the stable aggregate destruction figures presented in the previous section smooth over industry-level changes. Graphs of creation and destruction rates for each industry are in Appendix B.

The most salient features of these figures are the comparative magnitudes of creation and destruction activity. The service-oriented industries, those with SIC code 6 through 8, possessed creation rates in excess of destruction for all but one year of the sample (1990 for SIC6, 1991 for SIC7 and 1989 for SIC8). This was also true in wholesale and retail trade where creation dipped below destruction only in 1980. Conversely, within the manufacturing, mining, construction, agriculture, and transportation, communication, and utilities sectors, neither series dominated the other.[14]

To isolate within-group deviations from each industry's average destruction and creation rate, averages for 1990-1994 were compared to their respective prior long-term means. The analysis involved testing the equality of each industry's 1990-1994 job destruction rates to their time series averages prior to 1990. Below is the null hypothesis and corresponding one-tail alternative:

[14] One-tail t-tests performed on each sector's creation and destruction rates verify these observations; results are included in Table 4.3.1.

Table 4.3.1. Mean Job Destruction and Creation Rates, by Industry: 1990-1994

SIC; Industry	Dest. Rate	Creation Rate	T-Stats (P-values)
0; Agriculture	.0221	.0296	.7954 (.2158)
1; Mining and Construction	.0691	.0523	-1.176 (.1253)
2; Manufacturing, Nondurables	.0328	.0402	1.498 (.0716)
3; Manufacturing, Durables	.0349	.0368	.3754 (.3549)
4; Transportation, Comm., and Utilities	.0251	.0296	1.154 (.1285)
5; Wholesale and Retail Trade	.0241	.0658	9.284 (4.87E-10)
6; Finance, Insurance and Real Estate	.0290	.0611	4.943 (.00001)
7; Services: Bus., Personal, Ent.	.0181	.0962	7.580 (5.34E-8)
8; Services: Health, Legal, Ed.	.0247	.1237	5.879 (.000005)
9; Public Administration	.1173	.1314	.1986 (.4222)

No statistically significant difference was found in the mean creation and destruction rates of the two manufacturing sectors, construction and mining, agriculture, or transportation, communication and utilities, whereas the more service-oriented, typically white-collar industries displayed average rates of job creation that were discernibly higher than their destruction rates.

$$H_0\text{: JD Rate}_i = \textit{JD Rate}_i \qquad H_1\text{: JD Rate}_i > \textit{JD Rate}_i$$

where normal and italicized notations symbolize the 1990-1994 averages and 1976-1989 time series averages, respectively.

Three non-manufacturing sectors (SIC 5, 6, 8) – wholesale and retail trade; finance, insurance and real estate; and health, legal and educational services – exhibited significantly positive deviations from their job destruction means during 1990-1994 while the remaining seven (SIC 0-4, 7, 9) showed neither significant positive nor negative differences. These results are listed in Table 4.3.

Table 4.3. Test of Equality of Job Destruction Rates, by Industry: 1976-1989 to 1990-1994

SIC; Industry	T-Statistic (P-Value)	Mean JD Rate, 1976-1989	Mean JD Rate, 1990-1994
0; Agriculture	0.833 (0.468)	0.022	0.023
1; Mining and Construction	-0.171 (0.433)	0.070	0.067
2; Manufacturing, Nondurables	0.264 (0.397)	0.033	0.034
3; Manufacturing, Durables	1.524 (0.073)	0.032	0.042
4; Trans., Comm., & Utilities	1.329 (0.108)	0.023	0.031
5; Wholesale and Retail Trade	2.860 (0.008)	0.022	0.031
6; Fin., Ins. & Real Estate	2.482 (0.012)	0.026	0.038
7; Services: Bus., Pers., Ent.	1.653 (0.061)	0.015	0.026
8; Services: Health, Legal, Ed.	2.509 (0.012)	0.021	0.036
9; Public Administration	0.679 (0.264)	0.106	0.150

The fact that no industry's 1990-1994 average destruction rate was statistically less than its 1976-1989 time series mean, coupled with the increases seen in three of the non-manufacturing industries, suggests that large contributors to the rise in job destruction *rates* since 1990 have been these typically white-collar industries.[15] Additionally, industry results hint that large differences exist in the job destruction behavior of the major industrial sectors. This observation is strengthened by the sectoral creation and destruction rates illustrated in Appendix B.

[15] As for deviations in creation, the results are far less dramatic. Despite growth in aggregate creation rates during the 1990s, none of the broad sectors recorded a significant divergence from their mean creation rates.

The results of the following regressions are aimed at detecting secular trends in job destruction unique to different industrial sectors. Here, I include sector-specific real gdp growth rates and net employment rates[16] to identify potential changes in job destruction activity stemming from factors other than aggregate or industry-level growth. The objective is to determine whether there has been a significant change in sectoral job destruction beyond that explained by movements in industry-specific growth. The following regression was estimated:

$\text{JD Rate}_{st} =$

$$a + b\,(\text{IND} \times \text{FIVE}_t) + c\,\text{IND} + d\,\text{FIVE} + f\,\text{TREND} + g\,\text{INDgdp}_t + h\,\text{NETEMP}_t + e_t$$

where IND represents a dummy variable for 1-digit SIC codes, FIVE is a dummy corresponding to four five-year intervals spanned by the sample (1975-1979, 1980-1984, 1985-1989, and 1990-1994), TREND is an annual time trend variable, INDgdp is the annual real gdp growth rate for each 1-digit industry, and NETEMP is the annual net employment rate of the same 1-digit industries. Both the INDgdp and NETEMP variables were used in the estimation to control for changes in job destruction arising from shifts in the economic "health" of the relevant industrial sectors. Their inclusion ideally permits an examination of pure secular movements in job destruction behavior – those movements attributable to neither aggregate nor industrial output cycles.

The dependent variable, JD Rate, reflects the industry job destruction rate at the 4-digit SIC level. Including the 4-digit rate as the left-hand side variable bypasses endogeneity issues that might have arisen had I opted to use the 1-digit SIC rate as the dependent variable. Since the objective is to trace changes in the job destruction rate of major industrial sectors across the years of the sample, the vector of coefficients I am

[16] Sector specific industrial production figures were taken from the National Income and Product Accounts and the Current Survey of Business. When only nominal values were reported, these numbers were deflated using industry-specific price indices. Net Employment Rates were computed from employment figures available in various issues of the Monthly Labor Review published by the Bureau of Labor Statistics.

most interested in is that corresponding to the interaction term of IND x FIVE. If a given industry displayed a substantial secular rise in its job destruction rate during 1990-1994, I would expect to observe a statistically significant coefficient on the IND x 1990-1994 interaction term. F-tests comparing the IND x 1990-1994 coefficient to those of earlier periods can then be conducted to identify whether a structural break, or secular rise, in job destruction occurred in the early 1990s.

Results of the regression analysis appear in Table 4.4 The results in Column C reflect the concurrent usage of industry gdp and net employment rate as explanatory variables, whereas, in the two remaining columns, either INDgdp or NETEMP was excluded. The analysis involved the use of the Huber standard error correction technique that adjusted the computed standard errors of the coefficients by accounting for the panel nature of the data. The weighted least squares method of estimation used the annual sums of employment by each 4-digit industry as weights.

Table 4.4. Weighted Least Squares Regression Results: Dependent Variable – 4-Digit Industry Annual Job Destruction Rate

Explanatory Variables	(A)	(B)	(C)
5-Year Interval Dummies			
1980-1984	0.0591 (4.328)	0.0372 (2.725)	0.0378 (2.781)
1985-1989	0.0355 (2.331)	0.0134 (0.839)	0.0136 (0.847)
1990-1994	0.0325 (2.365)	0.0002 (0.015)	0.0003 (0.016)
Industry Net Employment Rate		-0.3358 (-5.699)	-0.3132 (-5.257)
Industry Real gdp Growth	-0.131 (-3.328)		-0.0214 (-0.549)
Year	-0.0002 (-0.403)	0.0007 (1.127)	0.0007 (1.287)
Interaction Terms			
Nondurables x 1980-1984	-0.0405 (-2.802)	-0.0336 (-2.364)	-0.0342 (-2.397)
Nondurables x 1985-1989	-0.0126 (-0.776)	-0.0066 (-0.410)	-0.0072 (-0.443)
Nondurables x 1990-1994	-0.0172 (-1.262)	-0.0079 (-0.583)	-0.0086 (-0.627)
Durables x 1980-1984	-0.0366 (-2.585)	-0.0282 (-1.963)	-0.0292 (-2.076)
Durables x 1985-1989	-0.0215 (-1.382)	-0.0139 (-0.897)	-0.0147 (-0.940)
Durables x 1990-1994	-0.0120 (-8.770)	-0.0009 (-0.069)	-0.0019 (-0.139)
Trans x 1980-1984	-0.0361 (-2.465)	-0.0253 (-1.741)	-0.0260 (-1.779)
Trans x 1985-1989	-0.0177 (-1.122)	-0.0051 (-0.321)	-0.0061 (-0.383)
Trans x 1990-1994	-0.0088 (-0.604)	0.0063 (0.425)	0.0055 (0.366)
Trade x 1980-1984	-0.0507 (-3.582)	-0.0373 (-2.682)	-0.0382 (-2.760)
Trade x 1985-1989	-0.0280 (-0.532)	-0.0174 (-1.154)	-0.0181 (-1.198)
Trade x 1990-1994	-0.0185 (-1.380)	-0.0072 (-0.532)	-0.0079 (-0.585)
Finance x 1980-1984	-0.0396 (-2.145)	-0.0229 (-1.301)	-0.0242 (-1.376)
Finance x 1985-1989	-0.0096 (-1.744)	0.0012 (0.068)	0.0004 (0.023)
Finance x 1990-1994	-0.0058 (-0.400)	0.0046 (0.325)	0.0040 (0.281)
Services, Business x 1980-1984	-0.0455 (-3.242)	-0.0290 (-2.077)	-0.0301 (-2.177)
Services, Business x 1985-1989	-0.0272 (-1.015)	-0.0145 (-0.925)	-0.0152 (-0.965)
Services, Business x 1990-1994	-0.0152 (-1.149)	-0.0011 (-0.079)	-0.0018 (-0.135)
Services, Other x 1980-1984	-0.0690 (-4.665)	-0.0547 (-3.759)	-0.0555 (-3.840)
Services, Other x 1985-1989	-0.0186 (-1.262)	-0.0063 (-0.342)	-0.0069 (-0.374)
Services, Other x 1990-1994	-0.0180 (-1.301)	-0.0041 (-0.290)	-0.0048 (-0.340)
Constant	0.416 (0.441)	-1.287 (-1.083)	-1.410 (-1.243)
Observations	6,112	6,112	6,112
R-Squared	0.0846	0.0958	0.0959

Note*:* T-Statistics are in parentheses. Each regression also included 1-digit SIC dummies. The comparison group (that combination of dummies omitted from the regressions) were in mining/construction from 1976-1979. SIC 0 and 9 – agriculture and public administration – were excluded from the regression because of the comparably higher volatility of the within-industry 4-digit SIC job destruction rates. Job destruction rates were regressed as positive values.

Irrespective of the industry health measure used, two important and consistent findings emerge from all three regressions. First, the results in Columns B and C suggest that, during the 1990-1994 period, job destruction rates at the 4-digit industry level were not statistically different than they had been in the earliest part of the sample, 1976-1979, when average aggregate destruction rates were at their time-series minima. Furthermore, as proven in F-tests, the coefficients on the 1990-1994 five-year interval dummy are statistically indistinguishable from those on the 1980-1984 and 1985-1989 dummies.[17] These findings are in accordance with the aggregate rate results documented in the preceding section. They reinforce the assertion that the incidence of downsizing, at the economy-wide level, did not increase significantly from the 1980s to the 1990s.

Second, F-tests comparing the SIC x 1985-1989 and SIC x 1990-1994 vector of coefficients confirm that none the seven major industrial sectors experienced significant job destruction increases between the two 5-year periods that could not be accounted for by growth (or the lack there of) at the industry level. Such an increase would be represented by a less negative coefficient, or higher job destruction rate, on a 1990-1994 interaction term than that coefficient on a 1985-1989 interaction term. With the exception of nondurables manufacturing, all major industries included in the regressions analysis yielded larger coefficients for the 1990-1994 period. Since the differences between the 1985-1989 and 1990-1994 coefficients were, in all cases, insignificant, I conclude that, both at the aggregate and broad industry level, job destruction rates did not experience appreciable secular increases from the 1980s to the early-1990s. The results of the three F-tests are contained in Table 4.5.

Table 4.5. F-test Results from Job Destruction Regression

H_0: $b_{1985\text{-}1989} = b_{1990\text{-}1994}$	Column (A) Regression		Column (B) Regression		Column (C) Regression	
Industry	**$F_{(1, 6078)}$**	**Prob(F<=f)**	**$F_{(1, 6078)}$**	**Prob(F<=f)**	**$F_{(1, 6078)}$**	**Prob(F<=f)**
Manufacturing: Nondurs.	0.01	0.9174	0.01	0.9229	0.13	0.7228
Manufacturing: Durables	0.94	0.3314	0.97	0.3249	0.52	0.4697
Trans., Util., & Comm.	0.62	0.4298	0.59	0.4410	0.38	0.5372
Wholesale & Retail Trade	0.64	0.4229	0.64	0.4226	0.56	0.4551
Fin., Ins. & Real Estate	0.06	0.8094	0.05	0.8188	0.07	0.7917
Business Services	1.00	0.3171	1.01	0.3138	0.82	0.3642
Other Services	0.01	0.9085	0.01	0.9042	0.00	0.9744

[17] The fact that job destruction rates from 1980-1994 were significantly greater those in 1976-1979, even after controlling for industry health, implies that there may have been some structural break between the two time periods. Since the Compustat sample spans only the last four years of the 1970s, a test for structural break may not be conclusive and/or representative of the pre-1980s status of job destruction.

4.4. Firm Size

Mean destruction rates were significantly less than creation rates in all three small, medium and large firm-size groupings. Moreover, the only series that showed significant deviation from its sample mean was the creation rate of the largest firm category which surprisingly fell to 0.033, below its long-term average of 0.041. The remaining destruction and creation series neither increased nor decreased considerably from their means. Appendix C contains graphs of each firm-size category.

5. CONCENTRATION

Another critical dimension to the understanding of movements in aggregate job destruction is the determination of which industries, if any, account for the bulk of downsizing episodes over recent history. In the preceding section, I offered evidence rejecting secular increases in job destruction within every major industrial sector, but also noted that three of the ten sectors experienced higher non-secular rates of job destruction in the 1990s. These findings imply that traditionally white collar, non-manufacturing industries have contributed increasing amounts to job destruction totals and that job destruction has therefore become less concentrated in certain sectors – such as manufacturing – of the aggregate economy.

This section introduces three measures of concentration typically used in industrial organization settings, applied here to quantify industry shares of annual job destruction. Following these analyses, there is a brief discussion of job destruction concentration within the twenty-most employing industries.

5.1 The Herfindahl-Hirschman Index (HHI)

The first method implements an annual Herfindahl-Hirschman index to determine relative concentrations of a selected aggregate variable across a given group. The index itself represents the sum of squared shares of a variable belonging to each member of the investigated group. In other words, assuming that a group has n number of participants, each with shares of some variable x equal to s_i for i = 1 to n, the Herfindahl-Hirschman Index is:

$$HHI = (s_1)^2 + (s_2)^2 + (s_3)^2 + (s_4)^2 + \dots + (s_n)^2$$

where the sum of all shares totals to 100.

A larger Herfindahl value connotes an increased concentration of the variable in question, which, in this case, is job destruction. Each relevant share is the percent of a certain year's aggregate job destruction contributed by a given industry. As a hypothetical example, assume the U.S. economy has three industries – manufacturing, agriculture and services. Each industry is responsible for 30, 50 and 20 percent, respectively, of aggregate job destruction in the year 2000. The resulting Herfindahl index for the year 2000 would be $(30)^2 + (50)^2 + (20)^2$ or 3800. The next year, a major drought forces farming companies to scale back employment, thereby altering the percentage decomposition of job destruction to 25, 60, 15 percent.[18] The Herfindahl index then increases to $(25)^2 + (60)^2 + (15)^2$ or 4450. Larger index values indicate a higher concentration of job destruction within one, or a few, sectors of the economy.

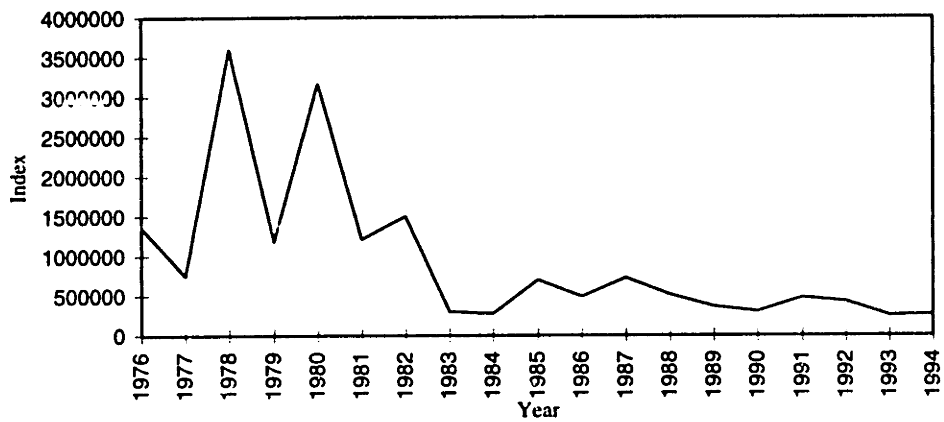
The actual index was computed at the 1-digit SIC code level. Each squared share value, i.e. - $(s_n)^2$, was normalized with the annual industry employment shares in order to correct for changes in the job destruction share stemming from the growth or reduction of a sector's overall employment level relative to the economy as a whole. This procedure is intended to correct for a reduction in the Herfindahl index occurring due to the shrinkage (in terms of employment size) of large job destroying industries relative to other major sectors. Without normalizing, it would not be clear whether the reduction in the

Herfindahl was due to a reduction in manufacturing's presence in the economy or an actual increase in the job destruction rates of other major industrial sectors.

The results are graphed in Figure 5.1 and show a distinct decline in the concentration of job destruction since large spikes in 1978 and 1980.[19]

Figure 5.1.

Annual Herfindahl Index of Job Destruction Concentration: 1 Digit SIC



Such a downward trend in job concentration is due largely to the reduced contribution of the manufacturing sector (SIC codes 2 and 3) whose job destruction share was 76 percent in 1978 and 71 percent in 1980 as compared to between 47 and 53 percent in the 1990s. With the exception of public administration, mining, construction and agriculture, the non-manufacturing industries showed slight to moderate gains in their job destruction shares over the same time period. Since the late 1970s, job destruction has become increasingly dispersed throughout economic sectors, rather than concentrated in manufacturing industries.

The shift in the concentration between the 1980s and 1990s has also been significant. Results of t-tests comparing the average 1-digit SIC Herfindahl indices over time are reported in Table 5.1.[20]

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[18] These percentages follow the same order as in the year 2000.

[19] A deficiency of the Herfindahl approach is its inability to discern between changes within the destruction shares, for a change in the ratio of an industry's job destruction to aggregate job destruction may be due to changes in either or both of the numerator or denominator. To avoid this possibility, the Herfindahl shares were normalized.

[20] Similar results are produced using the 4-digit SIC breakdown.

Table 5.1. Two t-Tests: Equality of Herfindahl Indices,
H_0: $HHI_{1976\text{-}1989}$ = $HHI_{1990\text{-}1994}$, H_0: $HHI_{1980\text{-}1989}$ = $HHI_{1990\text{-}1994}$ &
H_0: $HHI_{1985\text{-}1989}$ = $HHI_{1990\text{-}1994}$

	Herfindahl, 1976-1990	Herfindahl, 1980-1989	Herfindahl, 1985-1989
t-Stat	2.919	2.062	2.668
P(T<=t) one-tail	0.006	0.035	0.016
t Critical one-tail	1.761	1.833	1.895
P(T<=t) two-tail	0.011	0.069	0.032
t Critical two-tail	2.145	2.262	2.365

Tests for mean equality reject the hypothesis that concentration of job destruction remained the same between the two decades. Mean comparisons of the 1990-1994 period to 1976-1989, 1980-1989 and 1985-1989 instead yield t-stats of 2.919, 2.062 and 2.668. These t-values show the average job destruction Herfindahl index to have declined since the 1980s and the distribution of job destruction activities across industries to have widened (or become decreasingly concentrated). Figures 1-3 Appendix D graphically represent the distribution of job destruction during each time period. The reader should pay close attention to the diminished roles of the two manufacturing sectors (SIC 2 and 3) which comprised approximately 50 percent of total job destruction in the 1990s, down from 60 percent from 1980-1989.

5.2 The C-10 and C-20 Ratios

The next two measures of concentration are an extension of the most commonly used method of computing market power within an industry, the C-4. Employing the C-4 calculation, as it applies in a market concentration problem, involves summing the four highest market shares within a given industry with which one can then determine the degree of monopoly power. In this framework, I use the C-10 and C-20 to determine the concentration of annual job destruction within the top ten and twenty job destroying firms. For example, if an economy contains twenty firms out of which ten are responsible for nine percent each of job destruction in the year 2000 and each of the remaining ten are responsible for one percent each, the resulting C-10 would be ninety percent. The groups used in the annual computations of the C-10 and C-20 constituted the largest destroyers during each given year of the sample and therefore did not each consist of the same ten or twenty firms across years.

Figures 5.1 and 5.2 trace the C-10 and C-20 ratios over the 1976-1994 sample. Similar to the Herfindahl index, both the C-10 and the C-20 peaked prior to 1980, rose marginally in the 1980s, and experienced a decline in the 1990s.

Figure 5.2

C10: Share of Annual Job Destruction by Top Ten Job Destroying Firms

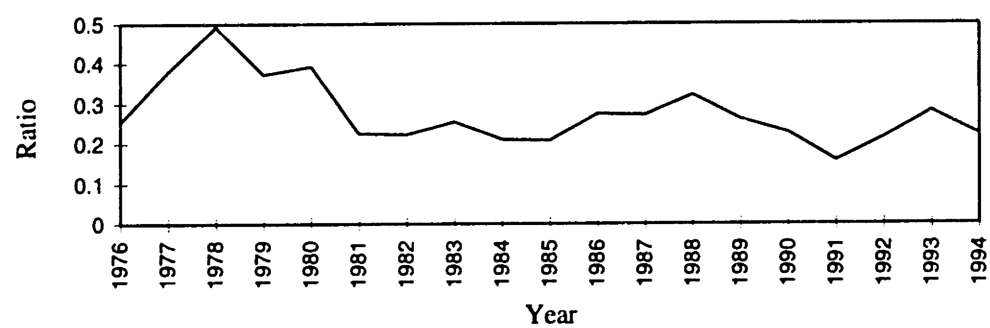
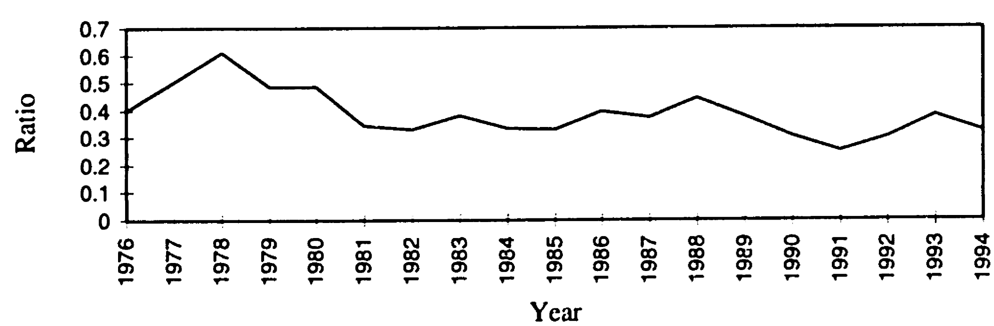


Figure 5.3

C20: Share of Annual Job Destruction by Top Twenty Job Destroying Firms



The highest degree of concentration in each case is recorded in 1978 where the ten and twenty largest destroyers accounted for 49 and 61 percent, respectively, of aggregate job destruction. Juxtapose the finding of 1978 with that of 1991 – the year with the lowest degree of concentration where the top ten and twenty destroyers were responsible for 16 and 25 percent of that year's aggregate job destruction – and the dispersion hypothesis is reinforced. Additionally, t-tests to determine between-time period equality in average C-20 shares of job destruction indicate that, at the 5 percent confidence level, there was significantly less concentration of job destruction in the 1990-1994 period than in 1980-1989 or 1985-1989.[21] This lends great support to the claim of increased dispersion of job destruction in the 1990s.

To reiterate, the methods described above offer four major findings concerning the concentration of job destruction in the U.S. economy from 1976-1994. First, the largest contributor to job destruction in every year of the sample is manufacturing, accounting for 47 to 76 percent of annual job destruction totals. Second, a growing portion of job destruction in the 1990s has been generated from services; financial, insurance and real estate operations; and transportation, communications and utilities industries. Lastly, manufacturing's share gradually decreased through the 1980s and 1990s, resulting in a significantly increased dispersion of job destruction across all industrial sectors.

5.3 Firm Size

Herfindahl indices measuring the concentration of job destruction among small, medium and large firms remained steady throughout the 1980s and 1990s, rejecting the possibility that job destruction has become more of a "large-firm" practice. The null hypothesis of unchanged concentration cannot be rejected; job destruction did not become significantly more localized in any size subsamples.

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[21] An identical test comparing the C-20s of 1990-1994 and 1980-1984 is narrowly rejected at the 5 percent level with a t-stat of 1.76.

6. DISTRIBUTION

This section examines the issue of increased job destruction dispersion by examining the concentration of job destruction at an even more disaggregated level, namely that of the firm. Thus far, this essay has focused on trends in overall job destruction rates and the coinciding concentration of job destruction activity. Little has been mentioned about the manner in which such jobs are being destroyed. Are firms eliminating jobs more gradually or are they destroying jobs in larger, less frequent bunches? Any observed changes in firm behavior with respect to job destruction has clear and relevant implications for the adjustment cost literature.

The purpose of this exercise is to trace changes in the composition of job destruction activity by narrowing the scope of analysis to the employment decisions of individual firms. I present this evidence in response to speculation that a growing proportion of firms in the 1990s eliminated job positions in larger discrete intervals. Whether immense downsizing events have altered the distribution of employment growth across all firms is the focus of this section's analysis. If the large downsizing events are more common, I expect to observe an increase in the variance of the employment growth distribution for the later years of the sample. Tests for distributional equality can also shed light on this question.

Figures 1and 2 in Appendix E depict both the annual kernel density estimates and the annual weighted histograms of firm-level employment growth.[22] The Epanechnikov kernel density illustrations represent a comparatively smoother approximation of the employment growth density, although both graphic representations suggest similar conclusions. The pictures show that, in each year, most firms depart only slightly from their previous year's employment level. Each graph exhibits one spike around zero -- the placement of this spike clearly corresponds to yearly aggregate economic conditions[23] –

[22] This analysis consolidated the destruction and creation rates in order to form the employment growth variable, the only difference being that destruction rates now assume negative values.

[23] The overall distributions tend to shift to the right (left) during years of high (low) economic growth. For example, the employment growth distribution functions of the expansionary period of the mid- to late-1980s are centered in more positive territory than their recessionary year – 1980, 1982 and 1991 – counterparts.

which then rapidly tapers off relatively symmetrically through its two tails. The overwhelming majority of firms in each year alter employment in a similar pattern, as the frequency distributions appear clustered around their spikes.

The most noticeable feature of the graphs is the extent to which the densities seem to retain their shapes across the years of the sample. Irrespective of placement within the business cycle, all curves closely resemble one another with respect to their limited amounts of skewness and kurtosis. Note, however, the declining height of the spike over time. This observation provides the basis for tests of both variance and distributional equality where I examine whether these distributions are indeed identical. For, if evidence supports the null hypotheses that there has been no systematic change in the distribution and/or variance of employment growth across time, this would clearly contradict the perception that a larger percentage of hiring and firing is being conducted en masse. Conversely, a rejection of the null would indicate an increased in bunching activity on the part of firms in terms of employment levels. This could then provide an empirical justification for an investigation of 1.) the changing role of adjustment costs in affecting the employment behavior of firms or 2.) changes in adjustment costs themselves.

6.1 Tests for Distributional Equality

I first employ a Kolmogorov-Smirnov equality of distributions test on both pairwise and pooled years to determine whether the distribution of firm-level employment growth changed across the years of the sample. The former tests compare each annual distribution in the twenty year sample to the distributions of all other years. The latter creates broader comparison groups, pooling all observations into four five-year interval samples. The pooling procedure combined observations from the years 1975-1979, 1980-1984, 1985-1989 or 1990-1994 into four distinct, yet comparable subsamples. The Kolmogorov-Smirnov test considers two distributions per analysis and calculates a statistic D to determine whether the distributions in question are indeed identical. The test identifies the largest positive and negative differences in the two distributions and tests the significance of such differences.

The hypotheses under scrutiny are the following:

$$H_0: \; F(e) = G(e)$$
$$H_1: \; F(e) <> G(e)$$

where e is the employment growth rate and $F(\cdot)$ and $G(\cdot)$ denote sample distribution functions computed from the observations of each separate year or each pooled year group, depending on the level of comparison. If the null hypothesis is rejected, the sample employment growth distributions of the two functions are found to be significantly different. The maximum difference between the two distributions is significantly greater than zero, implying that the cross-sectional employment growth distributions displayed dissimilar properties between the two years or periods in question.

The mathematical definitions of the K-S directional and combined statistics are:

$$D^+ = \max_e [F(e) - G(e)],$$
$$D^- = \max_e [G(e) - F(e)], \text{ and}$$
$$D = \max_e | F(e) - G(e) |.$$

The D^+ and D^- statistics measure the significance of the largest positive values of the differences between the two functions. The more commonly reported combined statistic, meanwhile, represents only the larger of the two previous figures. The null hypothesis is then rejected if

$$\left[\frac{mn}{m+n}\right]^{1/2} D > c$$

where m and n are the number of observations used to empirically compute $F(\cdot)$ and $G(\cdot)$, respectively. The value of c depends on both the chosen level of significance and the values of m and n.[24]

[24] See Conover (1980), Degroot (1989), or Shorack and Wellner (1986) for a more in depth discussion of the Kolmogorov-Smirnov test.

Prior to running the distribution tests, the annual and pooled employment growth observations were demeaned (the mean values were subtracted from each observation) so that any disparity in the distributions arising from aggregate rightward or leftward shifts in the distribution would be eliminated. This procedure thus focused on highlighting changes in deviations from the mean rather than changes in the mean employment growth rate itself. Constructing the relevant annual and pooled distributions then involved integrating over the density functions and setting the limits of the integration procedure equal to the maximum and minimum deviation during each year or period. Since all years possessed statistically indistinguishable maxima and minima, each annual and pooled density function was integrated using limits from -2 (the minimum mean deviation) to 2 (the maximum mean deviation).

The appropriate Kolmogorov-Smirnov statistics appearing in Table 6.1 were then computed by identifying the largest absolute differences between the distributions.

Table 6.1. Kolmogorov-Smirnov Results: Pooled Distributions

Comparison Periods (F,G)	D^+ (P-value)	D^- (P-value)	D (P-value)
1990-1994, 1976-1979	0.0748 (0.000)	0.0866 (0.000)	0.0866 (0.000)
1990-1994, 1980-1984	0.0330 (0.000)	0.0851 (0.000)	0.0851 (0.000)
1990-1994, 1985-1989	0.0649 (0.000)	0.0103 (0.087)	0.0649 (0.000)
1985-1989, 1980-1984	0.0277 (0.000)	0.0810 (0.000)	0.0810 (0.000)

The K-S statistics rejects the null hypotheses. In each case, the five-year pooled employment growth distributions were not found to be identical to the comparison group. These findings were consistent across all pairwise annual comparisons. Between-year comparisons of every possible annual pairing rejected hypotheses that any distribution was identical.

These findings suggest that employment growth distributions varied substantially across <u>all</u> years and periods included in the sample. The aim is to identify the source of the differences in the distributions and, in particular, to note those distributional properties which may have changed across time. I therefore turn to an examination of the dispersion of these employment growth distributions and ask whether the variance of employment growth changed over the sample period. Findings of wider dispersion would confirm the larger, infrequent downsizing hypothesis.

6.2 Variance Tests

The annual variances of the employment growth variable were between 0.201 to 0.337. Table 6.2 summarizes standard deviations associated with histograms included in the appendix. [25]

Table 6.2. Standard Deviation and Skewness Coefficient of Annual Firm-Level Employment Growth

Year	Standard Deviation	Skewness
1977	0.180	-0.535
1978	0.188	-1.548
1979	0.195	-2.523
1980	0.191	-0.825
1981	0.227	-2.017
1982	0.242	-0.739
1983	0.225	-1.033
1984	0.237	-1.381
1985	0.251	-0.663
1986	0.261	-0.267
1987	0.275	-0.400
1988	0.269	-1.014
1989	0.276	-0.861
1990	0.262	-1.246
1991	0.261	-0.866
1992	0.281	-1.060
1993	0.281	-0.668
1994	0.281	-0.779

The second column containing standard deviation figures suggests a slight upward trend in the variance of employment growth over time. The distributions appear to be less clustered around the mean in the later years of the sample. Tests for variance equality support this observation, rejecting the hypotheses that the standard deviation statistic for 1990-1994 was equal to the 1976-1979, 1980-1984 or 1985-1989 values. Results from

[25] Related t-tests show that the average employment growth experienced in the latest trough year of the sample, 1991, significantly outpaced employment growth observed during the preceding recession year, 1982. The relevant t-statistic for the trough-to-trough analysis was -9.85.

one-sided F-tests show the standard deviation of 1990-1994 pooled distribution to be significantly larger than any previous five-year period. The following null hypotheses:

H_o : Std. Dev. (1990-1994) = Std. Dev. (1976-1979) H_1 : Std. Dev. (1990-1994) > Std. Dev. (1976-1979)
H_o : Std. Dev. (1990-1994) = Std. Dev. (1980-1984) H_1 : Std. Dev. (1990-1994) > Std. Dev. (1980-1984)
H_o : Std. Dev. (1990-1994) = Std. Dev. (1985-1989) H_1 : Std. Dev. (1990-1994) > Std. Dev. (1985-1989)

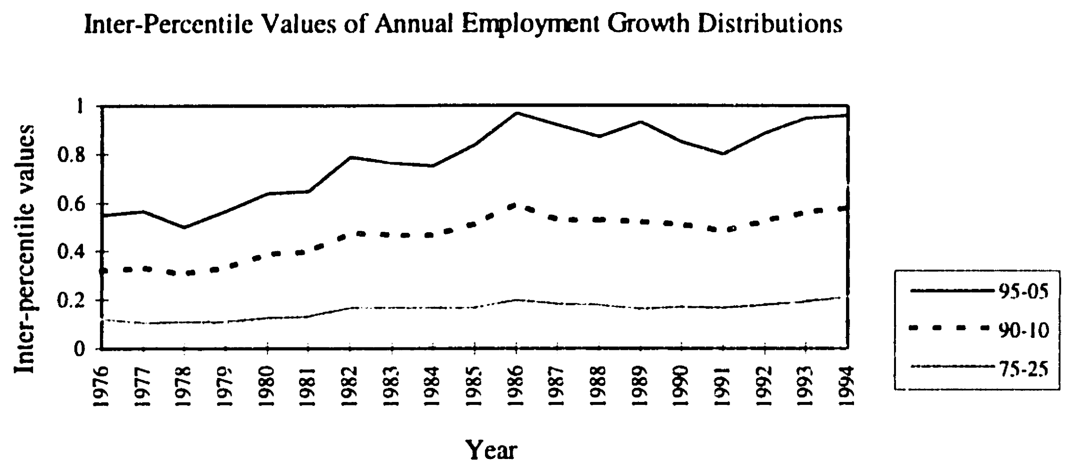
are rejected in all three tests.[26] From such evidence, I conclude that the distribution of employment growth has widened over time.

Additionally, the skewness coefficients presented in the third column do not change dramatically with time. Regressing the skewness coefficients on a time trend and/or aggregate gdp growth yields insignificant coefficients. Any tendency for firms to destroy jobs in larger amounts has been countered with an increased tendency to create jobs as well. U.S. firms have displayed a growing propensity to alter employment in either direction in infrequent, lumpy intervals, suggesting that the costs of adjusting (firing and hiring) labor have also changed over time.

Figure 6.1 illustrates the 95-05, 90-10 and 75-25 spreads across the sample years. The 95-05 line represents the value of the 5th percentile of the employment growth distribution subtracted from the 95th percentile. A rise in this inter-percentile value reflects an increase in the spread of the employment growth distribution; ninety percent of the firms fall within a wider range of employment growth values. This implies that more firms are opting to either create or destroy employment positions in greater proportions. Similar reasoning can be applied to the 90-10 and 75-25 lines.

[26] Regressing the annual standard deviation on a constant and a time trend yields a significantly positive coefficient of 0.006 with a t-stat of 10.01.

Figure 6.1



An interesting feature of this graph is the relative magnitude of the slopes of the three lines. The 75-25 line is virtually flat while the two wider ranging lines both possess noticeable upward slopes. This suggests that 1.) employment growth has become increasingly dispersed with time, but that 2.) the bulk of the dispersion is occurring within the tails of the distribution. Within the middle portions of the distributions, only a slight increase in the dispersion can be detected as measured by the 75-25 line. In contrast, both the 90-10 and the 95-05 lines – encompassing a larger portion of the distributional range – display sharp upward trends throughout the sample. Over time, eighty and ninety percent, respectively, of the firms fall within a broader portion of the distribution with respect to employment growth.

This evidence, in conjunction with the variance evidence presented above, suggests that proportionally larger creation and destruction episodes have become more frequent with time. A greater percentage of firms in the aggregate economy are engaging in larger-scale employment changes, creating and destroying job positions by more sizable intervals. This trend persisted throughout the sample.

7. CONCLUSION

The aggregate U.S. unemployment rate in 1995 stood at a noticeably low 5.6 percent of the civilian labor force. The fact that unemployment continues to hover at, if not below, the so-called "natural rate of unemployment" has prompted doubt over the validity of an extensive downsizing trend and its potentially harmful impact on labor force particpants. For, if job destruction has indeed risen dramatically, the following must also be true; higher job destruction coupled with lower unemployment rates must mean that, holding the labor participation rate stable, the economy is also rapidly absorbing such layoffs and creating jobs, or churning.

This essay finds substantial evidence in support of the doubters' contentions. The aggregate rate of U.S. job destruction neither increased nor decreased appreciably from its long-term average of approximately 3 percent. Such aggregate employment dynamics hides large dispersions in the downsizing activities between various sectors of the U.S. economy and between firms themselves. Those sectors exhibiting non-secular increases in their rates of job destruction were wholesale and retail trade; finance, insurance and real estate; and health, legal and educational services – all non-manufacturing industries. Job destruction originating in the two manufacturing sectors has comprised a decreasing proportion of total job destruction over time.

Evidence presented in this essay also describes and tests for dynamics changes in employment growth at the firm level, finding a significant increase in the variance of firm-level employment growth distributions. This result is in accordance with the perception that layoff events have increased in magnitude, comprised of more discrete, "lumpier" episodes. Such lumpiness in employment behavior may be the direct effect of structural shifts in adjustment costs to labor that, as discussed by Caballero, Engel and Haltiwanger (1995), render too costly continuous, small changes in firm-level employment. In sum, while there have been conspicuous changes in terminology surrounding job destruction – it is now referred to as "downsizing," aggregate job destruction itself has remained relatively constant through the 1990s. The underlying hiring and firing behavior of firms, however, has changed significantly. Future empirical and theoretical research on adjustment costs should clarify the roles that hiring and firing costs play in determining both the timing and magnitude of downsizing (and employment growth) episodes.

Appendix A. VARIANCE

Here, I present evidence regarding the relative movements in and volatility of aggregate job destruction and creation rates. The results confirm a degree of decoupling and asymmetry of creation and destruction while they contradict the widely held view that job creation is less volatile than destruction. A formal F-test does not reject the null hypothesis that the variances of job creation and job destruction rates are equal. This is in contrast to evidence presented by Davis and Haltiwanger who identify job destruction as significantly more volatile than job creation. They contend that job destruction displays prominent countercyclical motion while job creation is only slightly procyclical; the authors consequently distinguish job destruction activities as the principal forces behind employment fluctuations, attributing only a small contributory role to creation.

A quick review of the Figure 4.1 confirms the volatile nature of job creation beyond the levels discerned in earlier works focusing on manufacturing. The inclusion of services, trade, finance, insurance, etc. clearly adds to the variance of job creation, making it a sizable part of job reallocation. The average share of reallocation rates attributable to job creation is roughly 61%, a value higher than previously estimated.[27]

The next issue is whether destruction and creation have displayed consistent movements between themselves and with economic cycles. Suggestions of a negative correlation between destruction and creation are somewhat substantiated, as the actual correlation coefficient is found to be -0.48 over the entire sample. This appears to retract from the possibility of an efficient economy, as defined by Caballero and Hammour (1994), since creation and destruction appear not to display parallel movements. Furthermore, the magnitude of these relationships with real gdp growth are roughly the same in absolute value, as the covariances of job destruction and job creation rates with aggregate economic growth are -0.000160 and 0.000166, respectively. Destruction and creation rates yield correlations with real gdp growth equal to -0.52 and 0.69. The two series move in proportional, but opposite directions with respect to economic growth.

[27] As expected, the actual share declines with the GDP growth rate, as job destruction rates are increasing.

The discrepancy between the results attained here and those of Davis and Haltiwanger can best be explained by the fact that Davis and Haltiwanger's extensive work originated from a sample of strictly manufacturing plants. The Compustat sample, on the other hand, applied in this essay spans all industry-types -- manufacturing, trade, services, etc. Interestingly, similar results only emerge for the four major industrial classifications with 1-digit SIC codes under 5. These codes encompass broad agricultural, mining, construction and manufacturing categories. For codes 5 though 9, classifications which include business and general services, retail trade, and insurance, annual creation rates were not only significantly different from, but also *more variable* than annual destruction rates. The table below includes variance and correlation information for each major industrial sector as well as the p-values from equality of variance tests. The industries for which the variances of creation and destruction were found to significantly differ (i.e.- p-values less than 0.05) were denoted with an asterisk.

Table A.1. Variance and Correlation Results, by 1-Digit SIC Industry

SIC; Industry	Var(JD)	Var(JC)	P-value [28]	Corr(JC, JD)
0; Agriculture	0.00087	0.00080	0.428	-0.468
1; Mining and Construction	0.00324	0.00064	0.999	-0.418
2; Manufacturing, Nondurables	0.00028	0.00018	0.187	-0.300
3; Manufacturing: Durables	0.00032	0.00017	0.891	-0.780
4; Transportation, Communication, Utilities	0.00020	0.00009	0.949	-0.350
5; Wholesale and Retail Trade *	0.00008	0.00031	0.003	-0.699
6; Finance, Insurance, and Real Estate *	0.00024	0.00057	0.036	0.489
7; Services: Business, Entertainment*	0.00027	0.00175	0.0001	-0.530
8; Services: Health, Legal, Education *	0.00032	0.00506	1.64E-7	-0.249
9; Public Administration *	0.01003	0.08608	1.63E-5	0.571

For each industry listed with an asterisk, additional one-tail tests supported the alternative hypothesis that Var(JC Rate)>Var(JD Rate), countering aggregate-level evidence of variance equality. It appears that such an equality is confined to more blue-collar manufacturing-oriented sectors of the aggregate economy.

[28] P-values emerge from the following hypothesis test

$$H_0: \text{Var(JC rate)} = \text{Var(JD rate)}$$

computed for each industry separately.

In terms of co-movements with the aggregate gdp growth rate, correlation measures reveal a wide dispersion in the cyclical properties of both creation and destruction among the major industrial sectors. Of the ten primary industries, only three – manufacturing: durables, trade, and business services – possess correlation coefficients that even roughly match that of the aggregate economy. The remaining seven industries display tenuously slight relationships between economic growth and one or both of the two employment series. Particular aberrant with respect to the aggregate economy is the health, legal and educational services sector where job creation and destruction move slightly countercyclically and procyclically, respectively. Industry-level correlation coefficients are listed in Table A.2.

Table A.2. Correlations of Job Destruction and Job Creation with GDP Growth (g)

SIC; Industry	Corr(JC Rate , g)	Corr(JD Rate , g)
0; Agriculture	0.0815	-0.6067
1; Mining and Construction	-0.0640	-0.1128
2; Manufacturing, Nondurables	0.3637	-0.1604
3; Manufacturing: Durables	0.6701	-0.6981
4; Transportation, Comm., Utilities	-0.1005	-0.0374
5; Wholesale and Retail Trade *	0.6554	-0.6398
6; Finance, Insurance, and Real Estate *	-0.1430	-0.1277
7; Services: Business, Entertainment*	0.5210	-0.5579
8; Services: Health, Legal, Education *	-0.0464	0.1623
9; Public Administration *	0.0065	0.2846

Note that agriculture is the only major industry that is in accord with the findings of Davis and Haltiwanger, experiencing slightly procyclical job creation and strongly countercyclical job destruction. Otherwise, it appears that the finding of countercyclical destruction and procyclical creation at the macro-level belies significant disparities in the disaggregated industry results.

A.1. Firm Size

For each size category, the null hypothesis that Var(JC Rate)=Var(JD Rate) could not be rejected. The relevant correlation coefficients are listed in the following table.

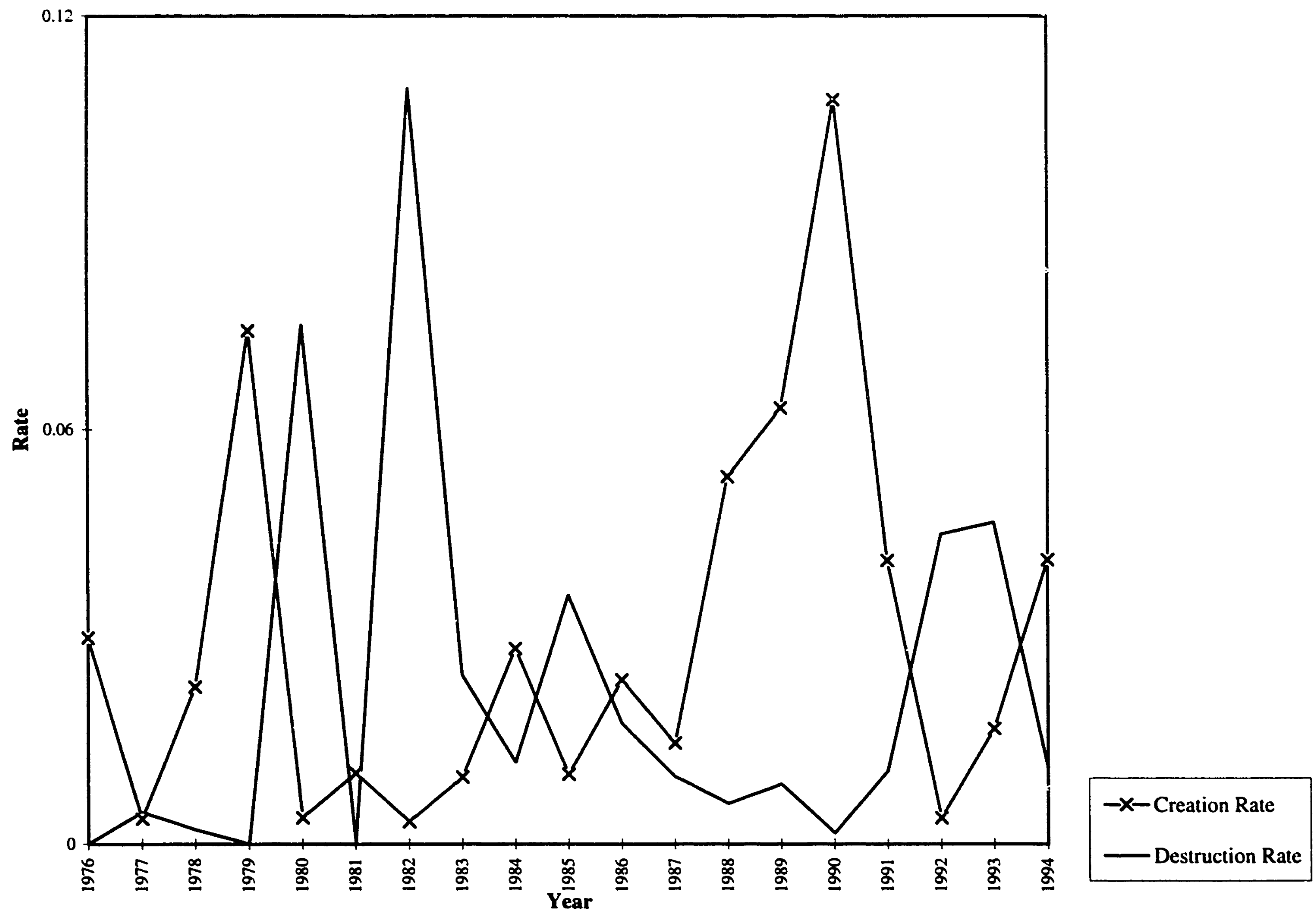
Table A.1.1. Correlations: Job Creation, Destruction and Real gdp Growth (g), by Firm Size

SIZE	Corr (JC, JD)	Corr (JC, g)	Corr (JD, g)
Firms with <5,000 Emps.	-0.299	0.491	-0.501
5,000-24,999 Emps.	-0.328	0.551	-0.484
Firm with 25,000+ Emps.	-0.599	0.636	-0.500

Unlike the industry-level analysis, the procyclical properties of job creation and the countercyclical properties of job destruction, in each size classification, closely conform to those of the aggregate economy.

In conclusion, the findings of relatively high job creation variance as well as higher than anticipated co-movement with gdp growth, in turn, have enormous implications for the search and adjustment cost literature. Until now, the assumptions of smooth job creation and countercyclical job destruction purported by the Davis and Haltiwanger findings have meant that job reallocation rates also display countercyclicality. Job reallocation has typically been thought to be highest during recessions. The evidence presented above warrants additional theoretical examinations of the relationships underlying aggregate employment dynamics; such research would attempt to explain proportional, yet opposite movements in job destruction and creation.

Appendix B. Figure 1.
Annual Creation and Destruction Rates for 1-digit SIC == 0, 1976-1994

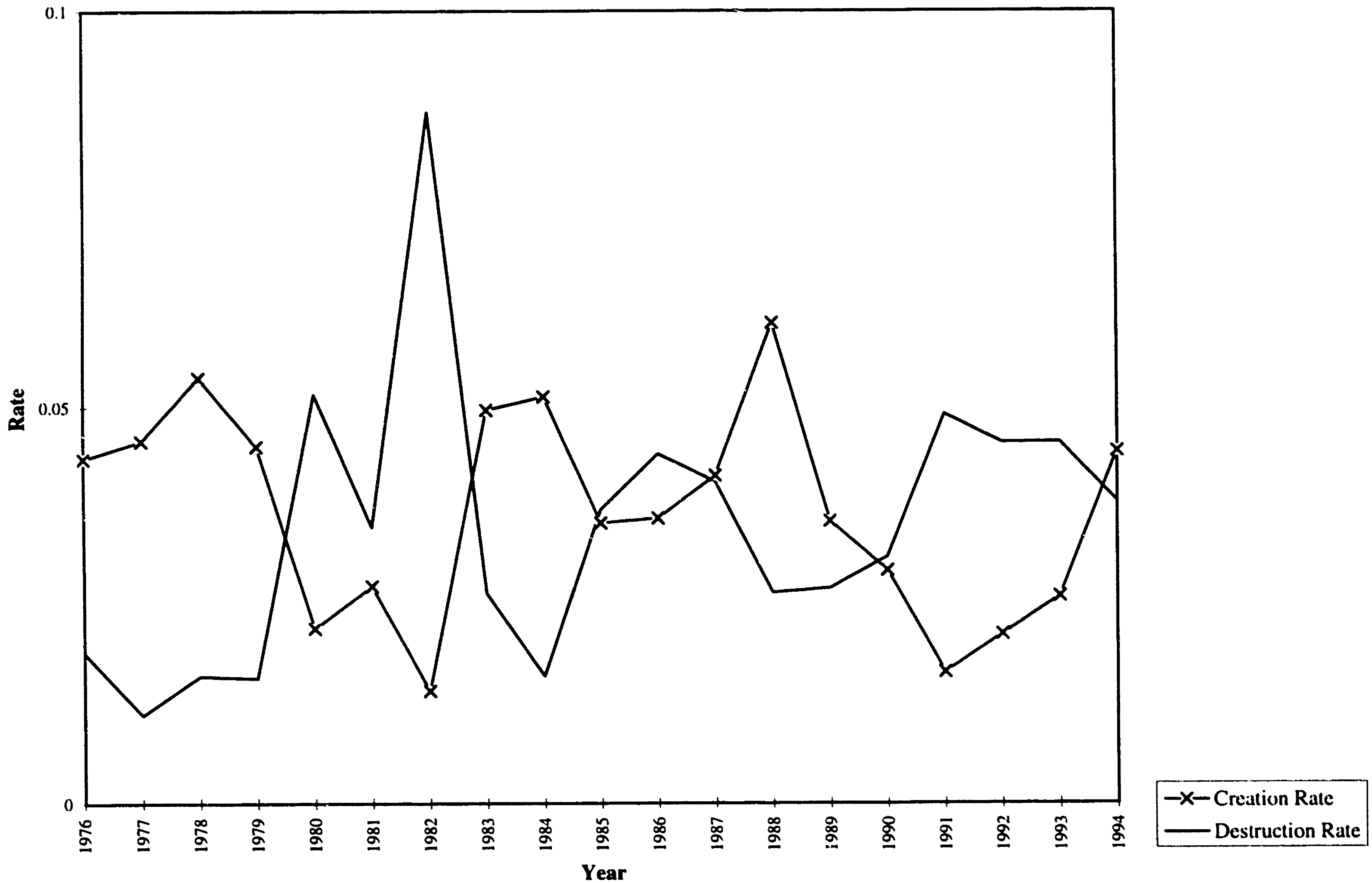


Appendix B. Figure 2.
Annual Creation and Destruction Rates for 1-Digit SIC==1, 1976-1994

Appendix B. Figure 3.
Annual Creation and Destruction Rates for 1-Digit SIC==2, 1976-1994

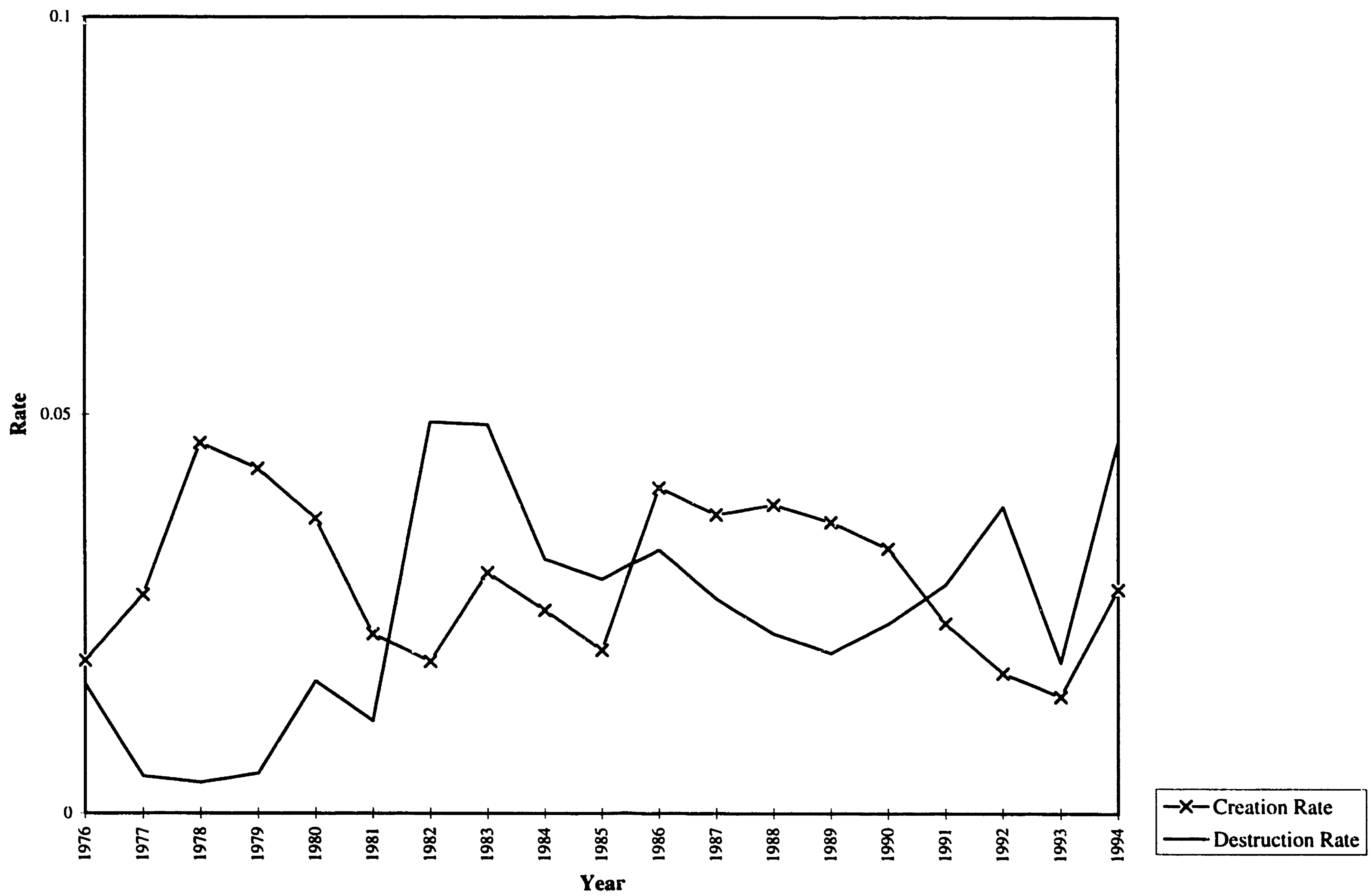
Appendix B. Figure 4.

Annual Creation and Destruction Rates for 1-Digit SIC==3, 1976-1994

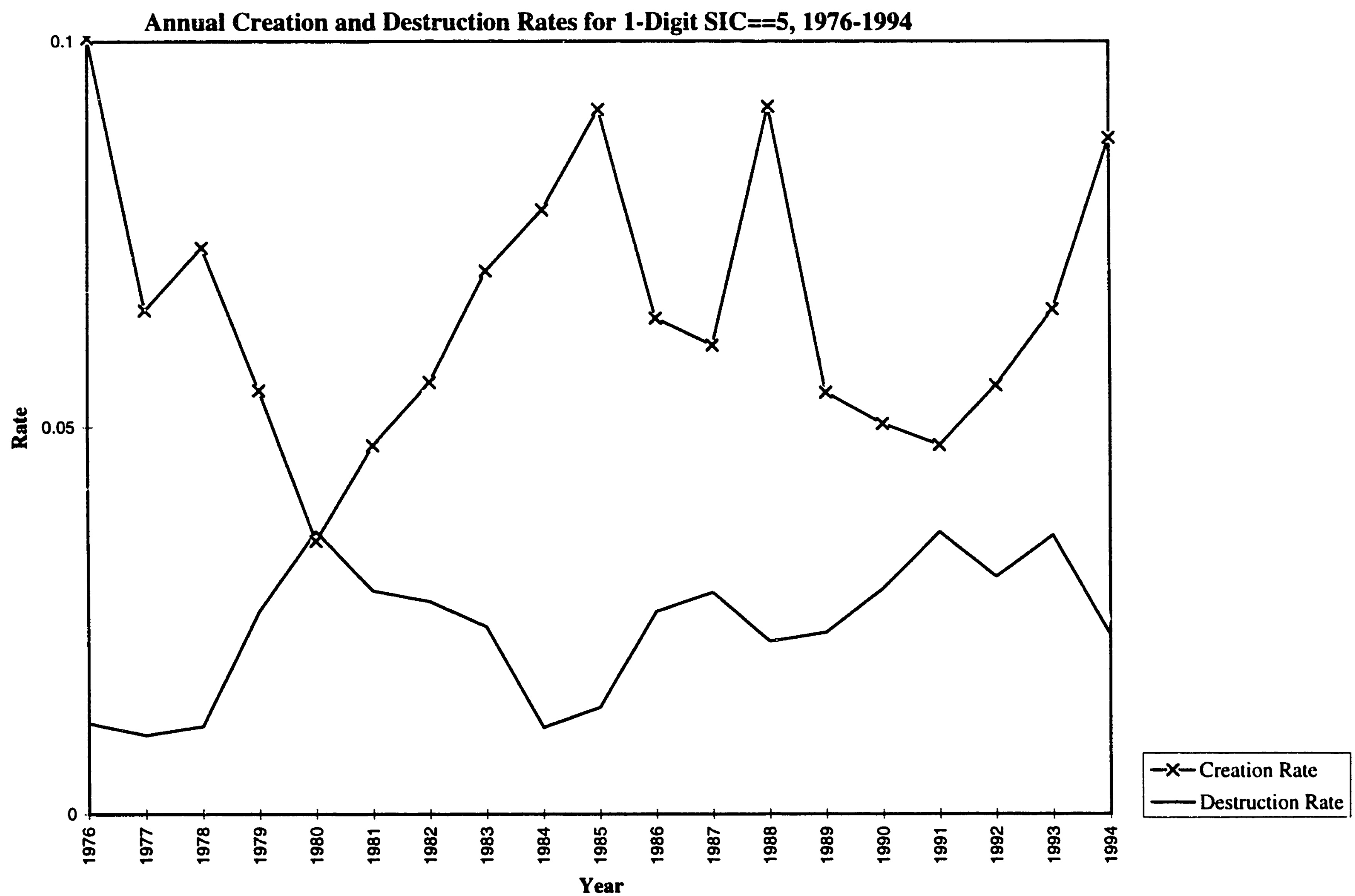


Appendix B. Figure 5.

Annual Creation and Destruction Rates for 1-Digit SIC==4, 1976-1994

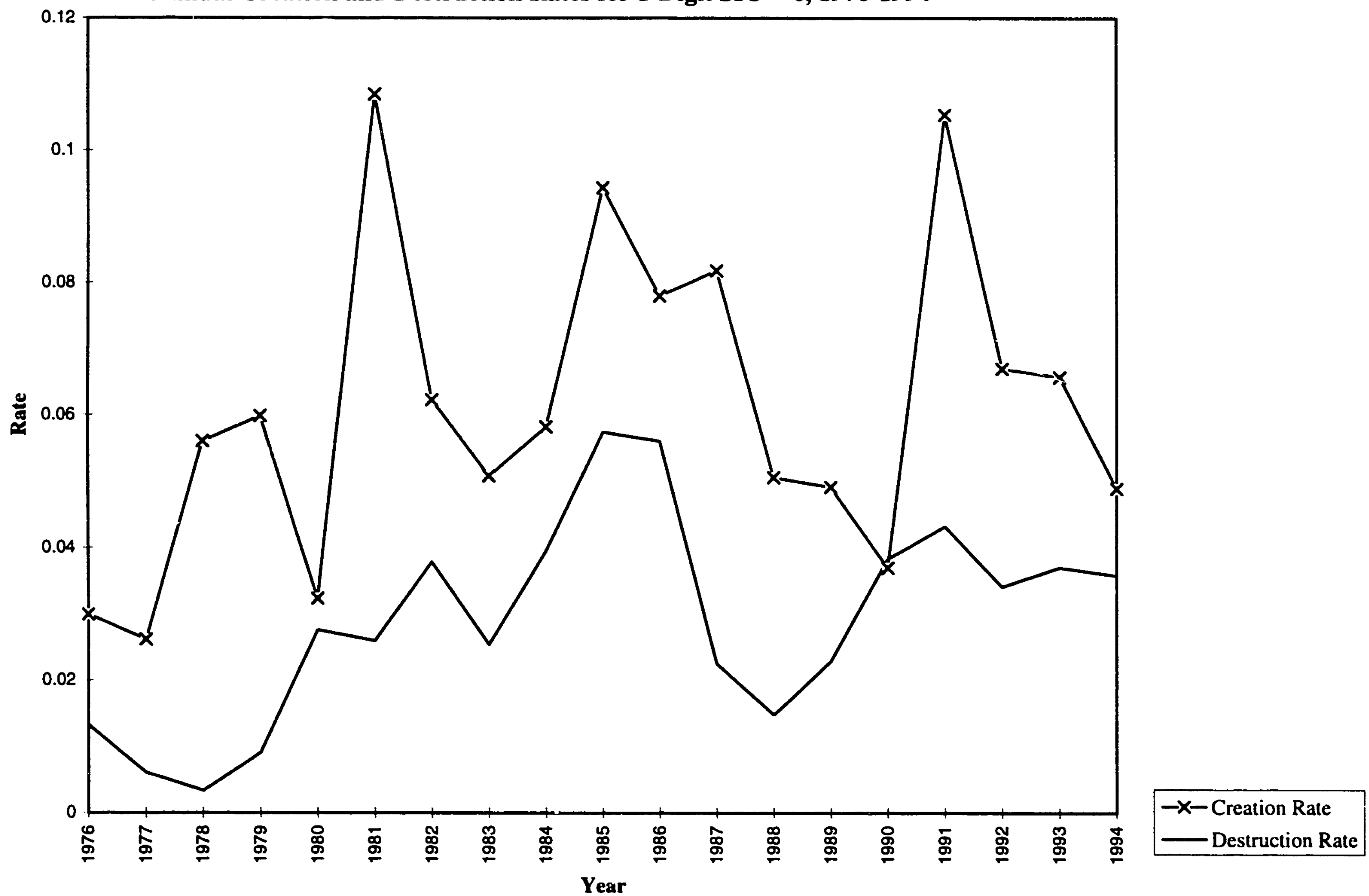


Appendix B. Figure 6.



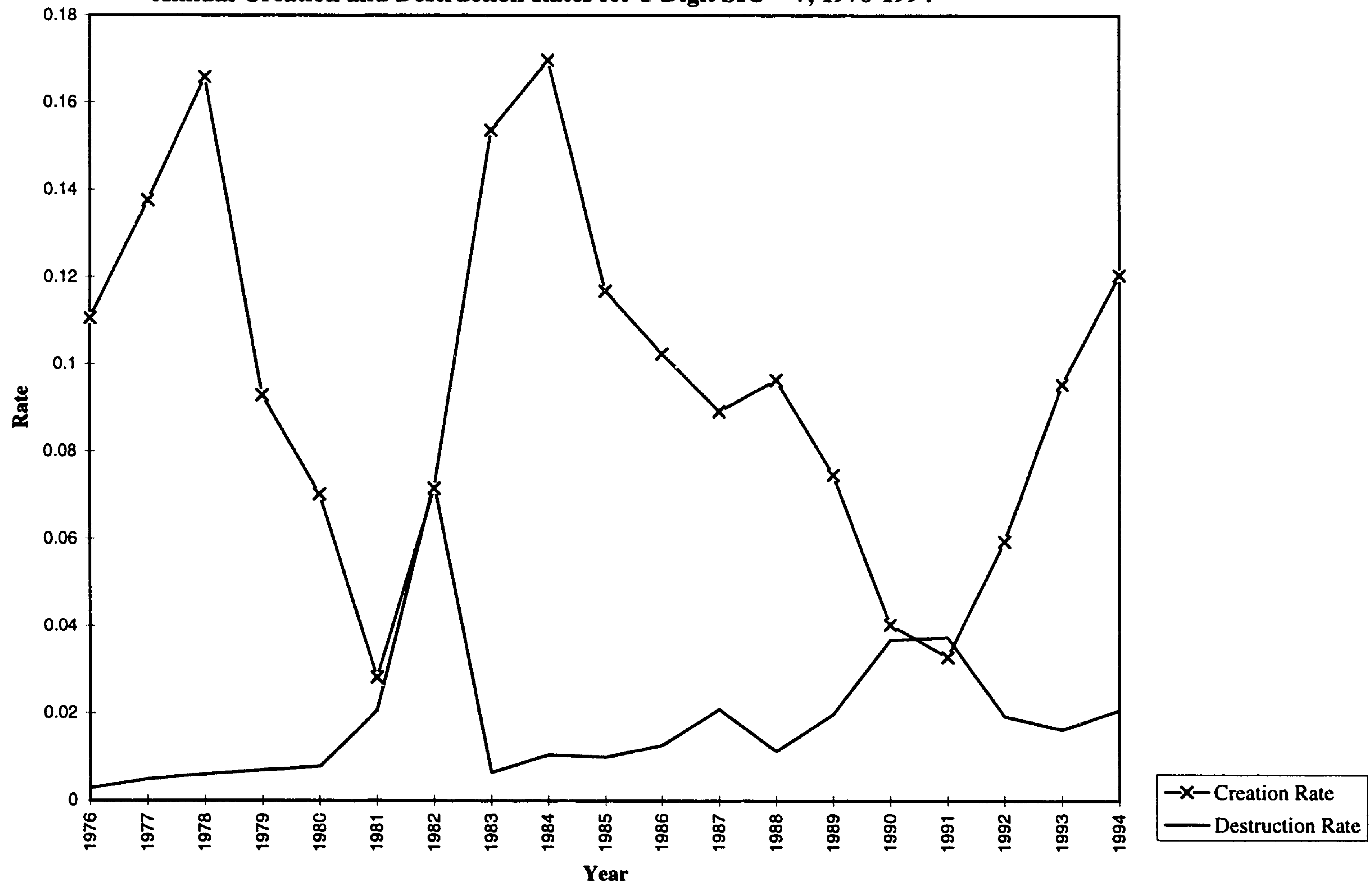
Appendix B. Figure 7.

Annual Creation and Destruction Rates for 1-Digit SIC==6, 1976-1994



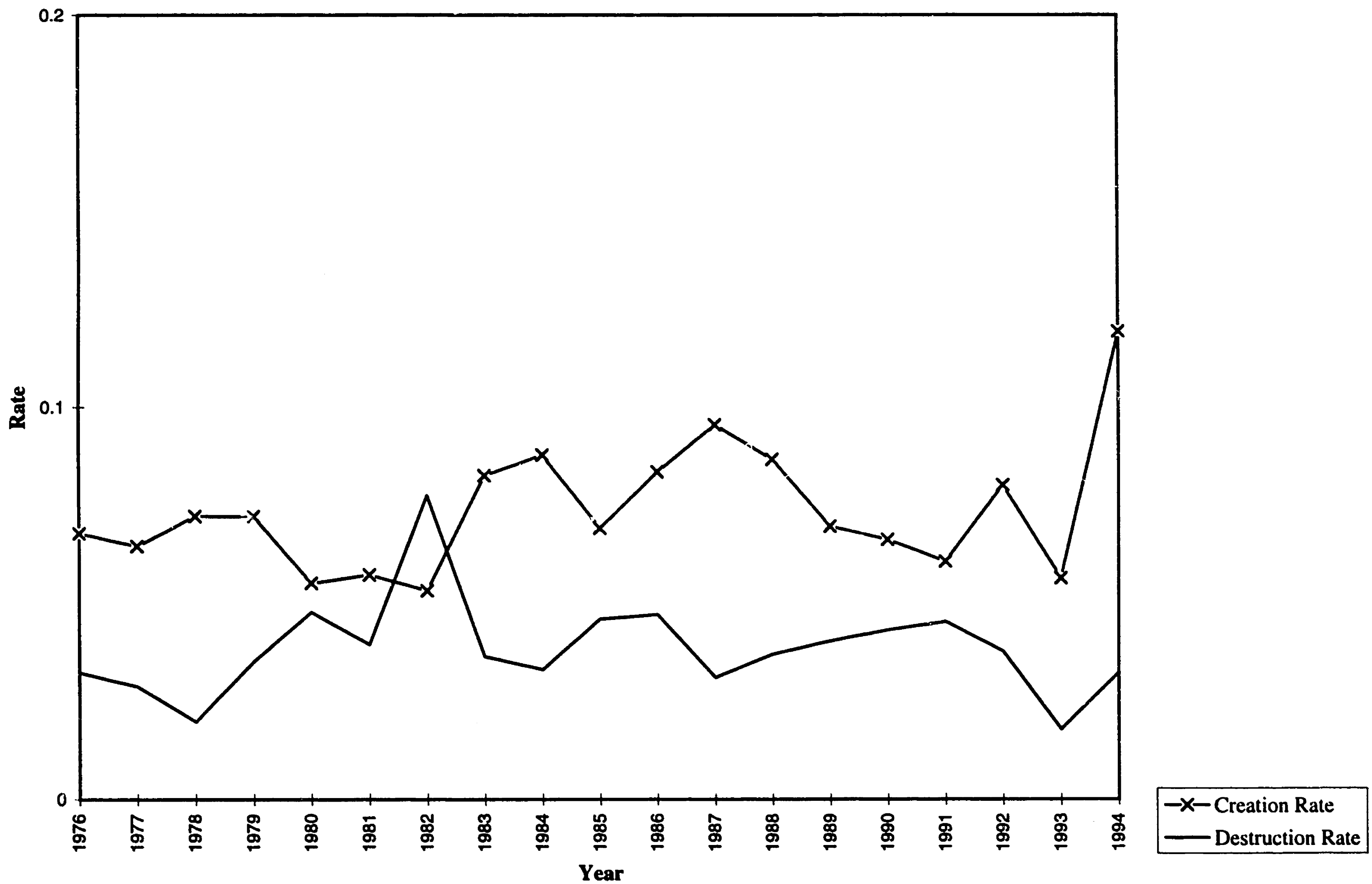
Appendix B. Figure 8.

Annual Creation and Destruction Rates for 1-Digit SIC==7, 1976-1994



Appendix C. Figure 1.

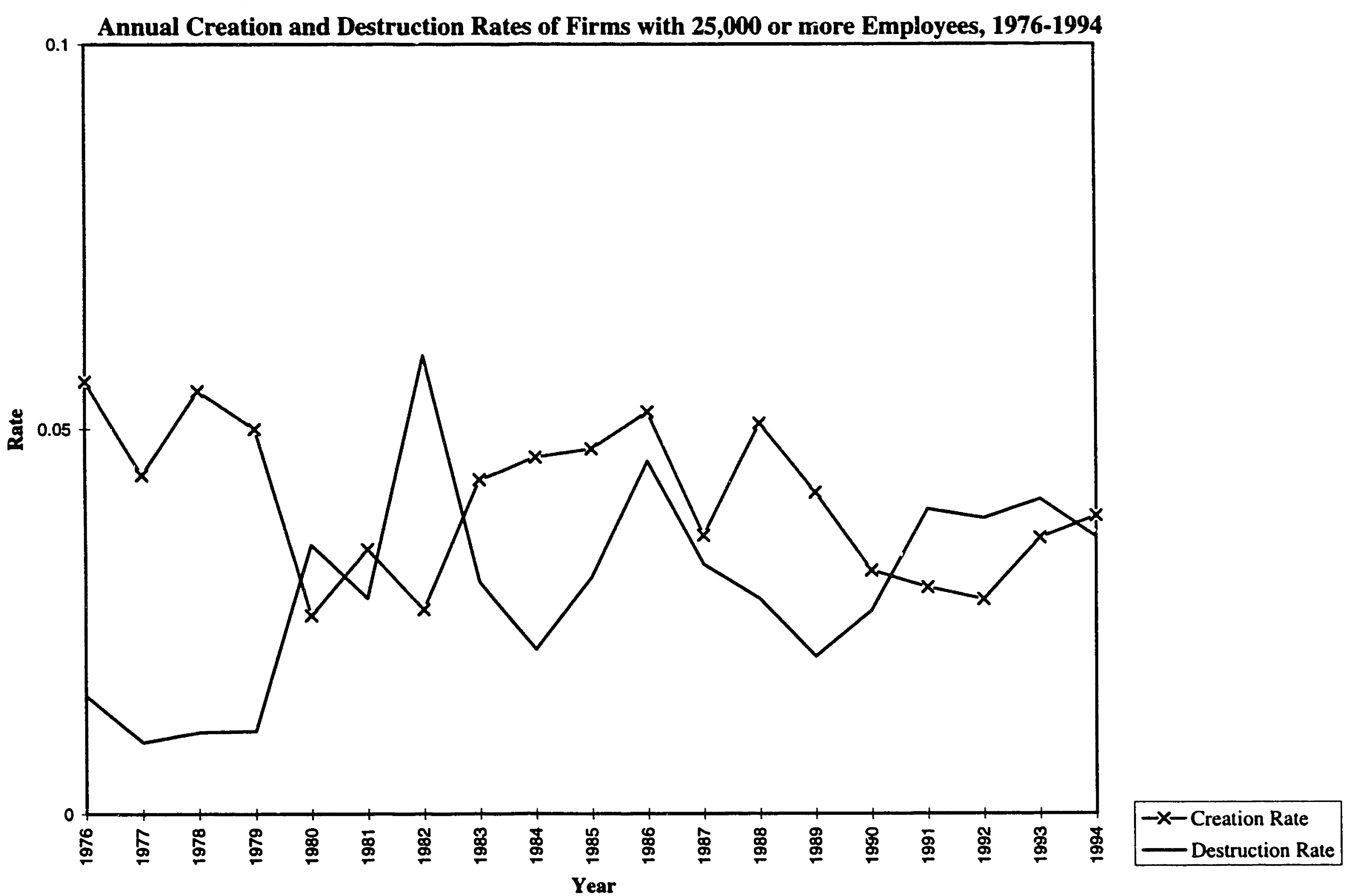
Annual Creation and Destruction Rates of Firms with less than 5,000 Employees, 1976-1994



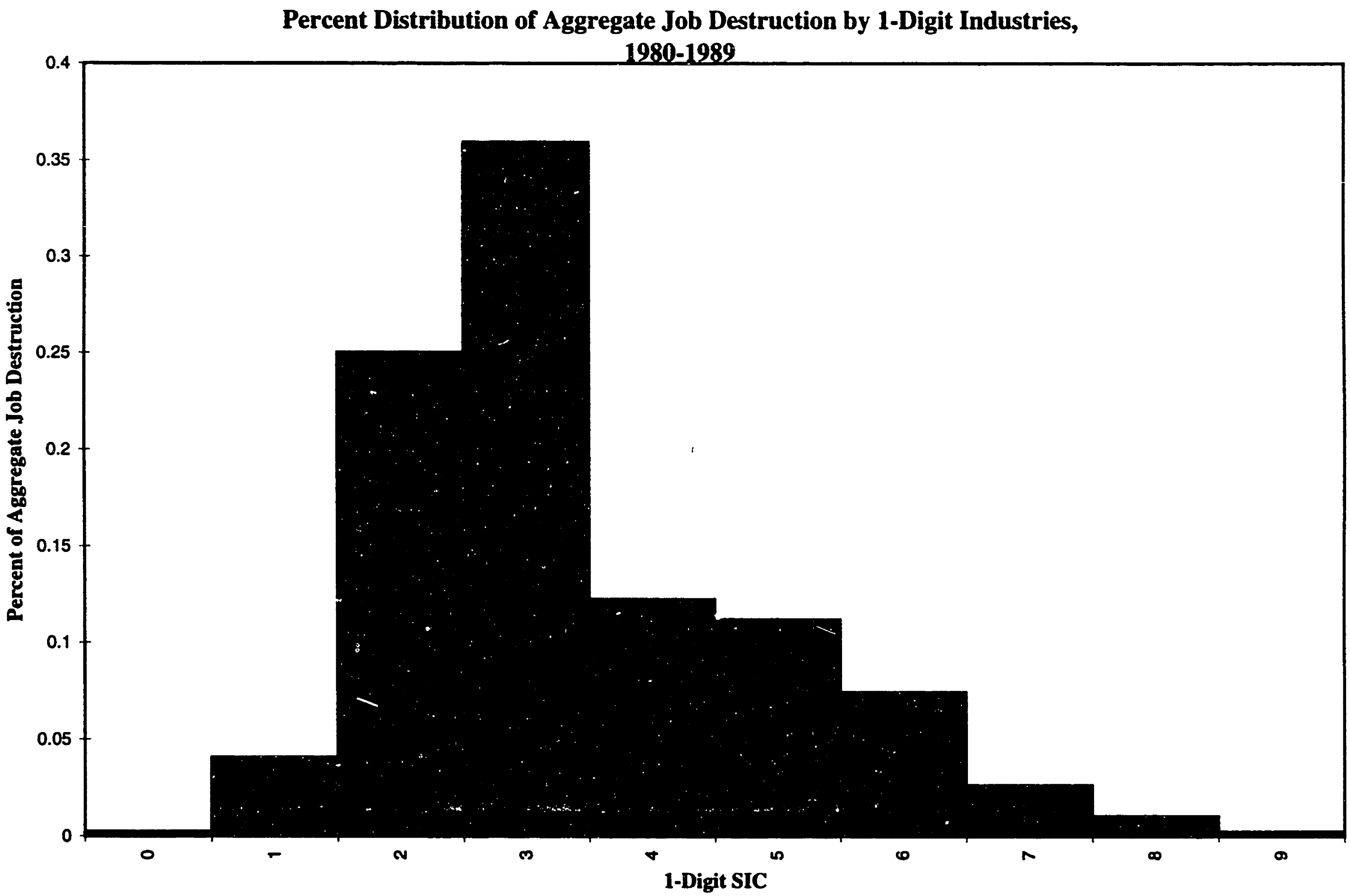
Appendix C. Figure 2.

Annual Creation and Destruction Rates of Firms with 5,000-24,999 Employees, 1976-1994

Appendix C. Figure 3.

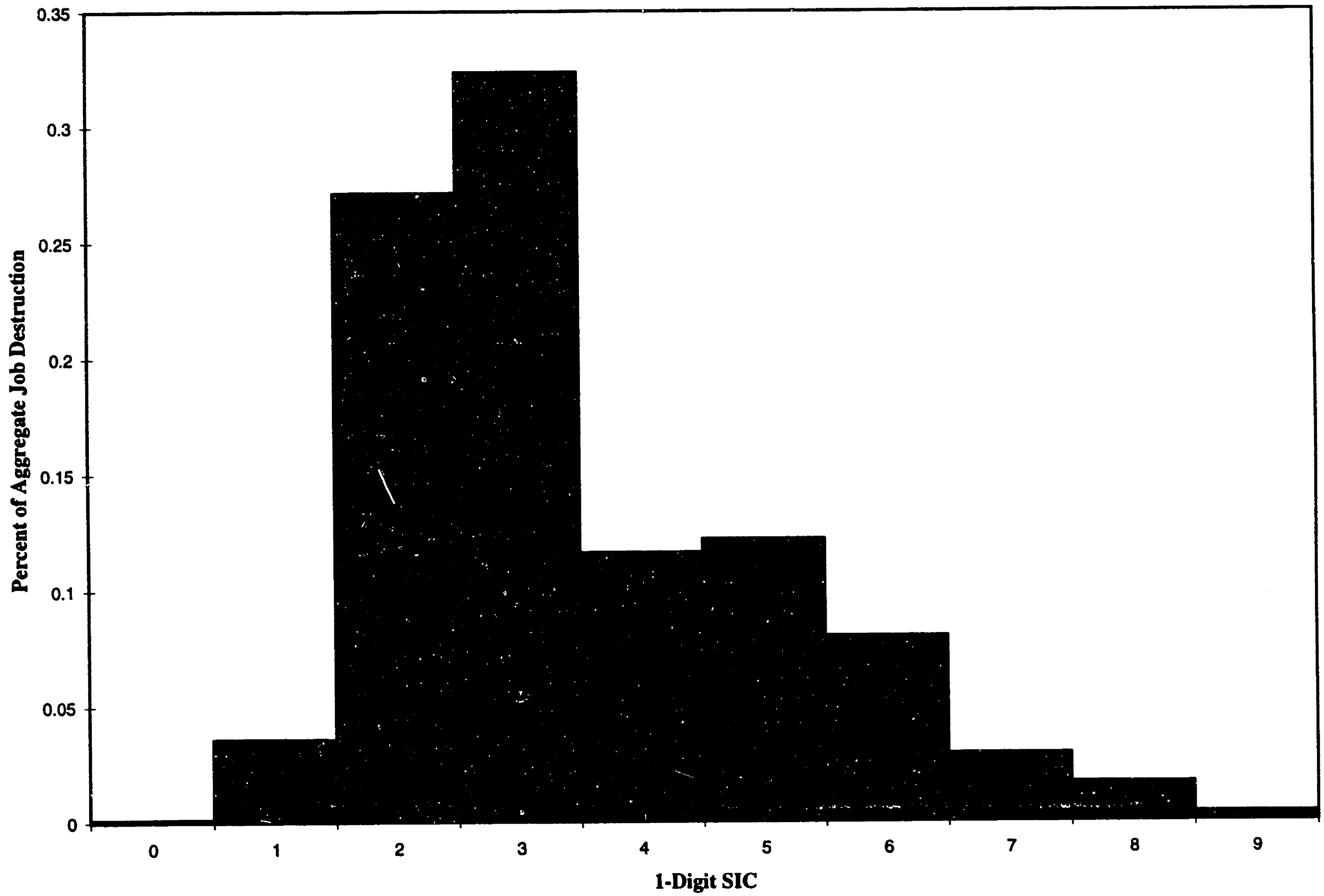


Appendix D. Figure 1.



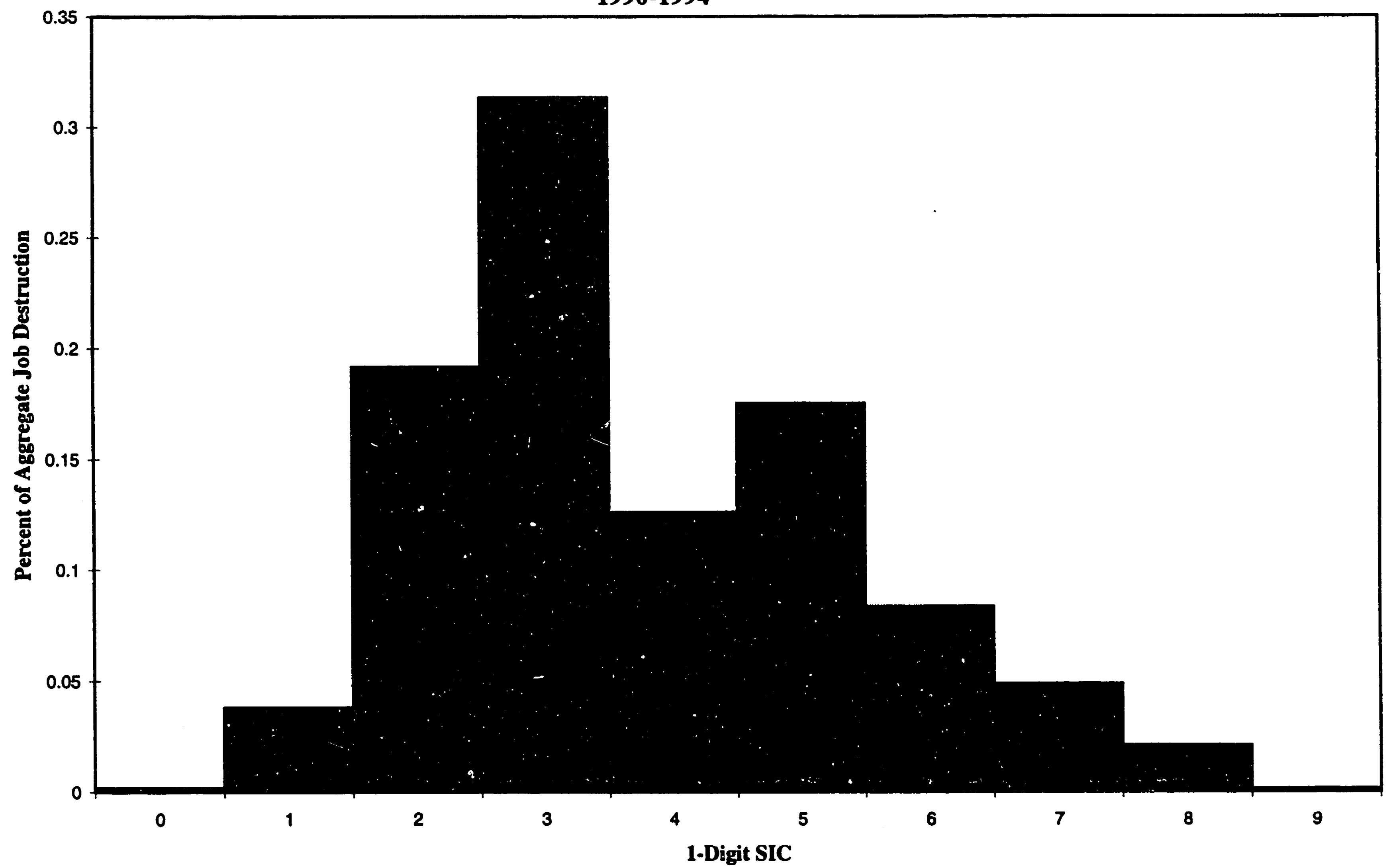
Appendix D. Figure 2.

Percent Distribution of Aggregate Job Destruction by 1-Digit Industries, 1985-1989

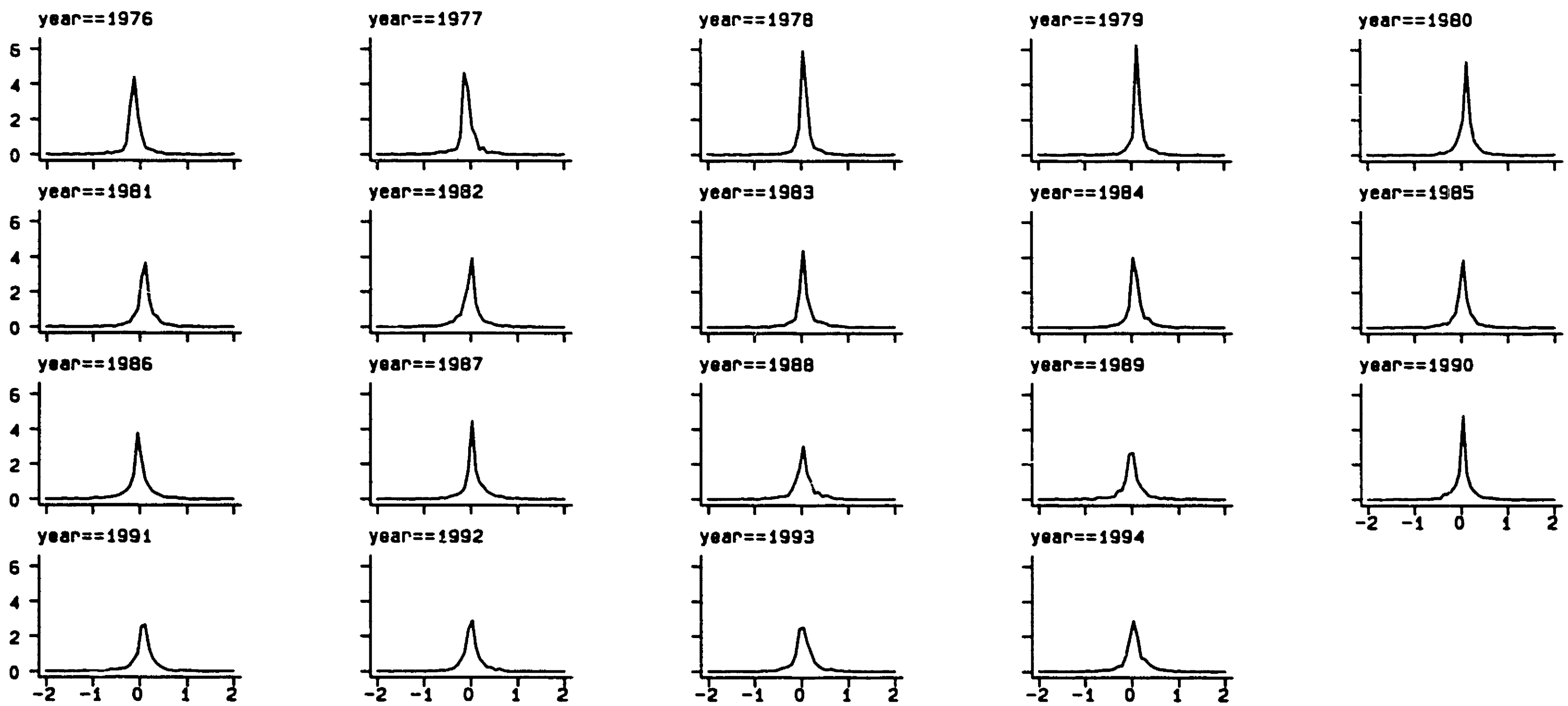


Appendix D. Figure 3.

Percent Distribution of Aggregate Job Destruction in 1-Digit Industries, 1990-1994

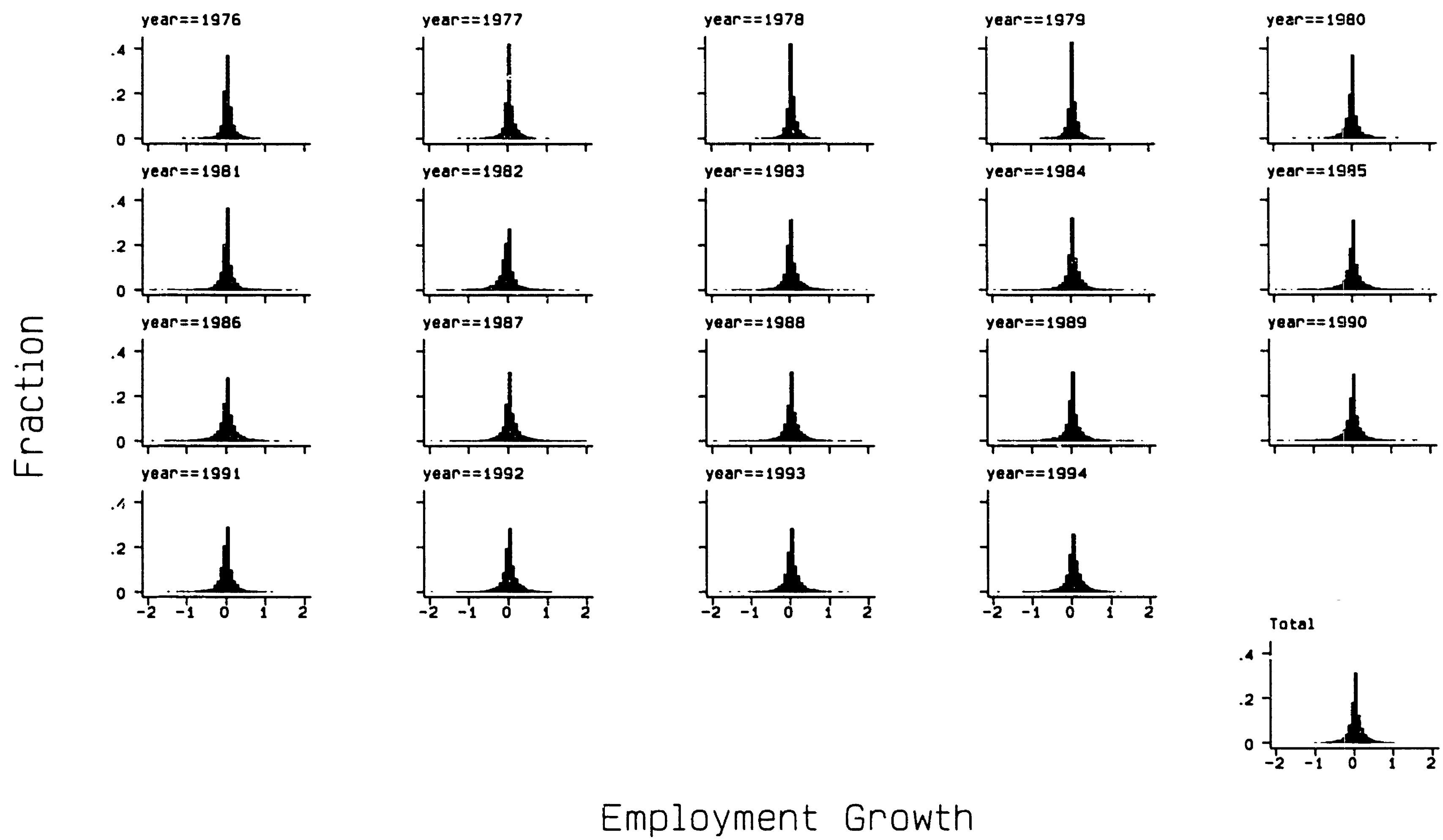


Appendix E. Figure 1.
Kernel Density Estimation of Employment Growth, by Year



Employment Growth

Appendix E. Figure 2.
Employment Growth Distributions, by Year



CHAPTER TWO

1. INTRODUCTION

The rise in leverage and corporate debt accumulation during the 1980s has attracted much attention from researchers intent on examining the responsiveness of input decisions of firms operating within imperfect capital markets. The growing debt obligations of U.S. corporations have prompted several economists to question whether firms' capital and labor patterns depend significantly on the availability of and access to credit resources.

An increasingly large body of literature has recognized the potential impact of imperfect credit and capital markets in influencing the capital investment behavior of firms. Recent findings by Gross (1995); Hubbard, Kashyap and Whited (1995); Whited (1992); Fazzari, Hubbard and Peterson (1988) and others[29] support the contention that firms' investment decisions depend on their financial positions. Each of the above studies constructs distinct specifications and, in some cases, uses different variables to gauge firms' relative liquidities. The general conclusions all bear similar tones – namely, that firms with limited internal funds and access to external financing exhibit higher sensitivities of capital investment to changes in sales or cash flow. By removing the perfect capital markets assumption, these examinations amend well-established investment theories, offering strikingly different results.

My objective in this chapter is to extend the findings of the credit constraint literature to encompass the employment decisions of firms. I ask whether the difference in investment behavior of financially constrained firms – those with an excess sensitivity of employment to sales – translates into excess sensitivity in terms of employment decisions as well. Do firms exhibit heightened employment variability as they become increasingly leveraged? Are financially constrained firms more likely to layoff workers ("downsize") during periods of slack demand? Such a correlation between credit

[29] Hoshi, Kashyap and Scharfstein (1991); Kashyap, Lamont and Stein (1994); Hubbard, Kashyap and Whited (1995).

constraints and employment elasticities could help to explain my findings presented in Chapter 1 that U.S. corporations have displayed steadily wider variances in employment growth through the 1980s and 1990s, hiring and firing workers in greater proportions. This observed increase in "lumping" activity could be in response to rising debt levels, increasingly scarce internal funds and costly external financing options. Caballero, Engel and Haltiwanger (1995) substantiate firm-level employment "lumping" behavior although their findings do not include a time dimension.

Thus far, two papers by Cantor (1990) and Sharpe (1994) have examined the employment responses of firms facing financial leverage constraints. While each author approaches the sensitivity issue using different methodologies and definitions of "financially constraints," the similar outcomes confirm the hypothesis presented in this paper. Both papers find evidence suggesting that the employment pattern of financially constrained firms is significantly distinct from the behavior of less leveraged firms.

This essay presents a study of the employment behavior of financially constrained versus unconstrained firms using panel data of roughly 5,000 publicly traded U.S. corporations. It tests whether the employment volatility or employment sensitivity to sales growth of financially illiquid and credit constrained corporations differs from that of their unconstrained counterparts. To provide a context to its findings, I present a brief discussion of labor hoarding theories. I suggest that the presence of imperfect capital markets prohibits financially constrained firms from responding to adjustment costs in an optimal manner (i.e. smoothing). Accordingly, credit constrained firms may be compelled to alter employment levels more frequently and more readily in response to changes in sales or cash flow.

The empirical methodology in this essay resembles that of Fazzari, Hubbard and Petersen (1988). It first classifies firms into relative constraint groupings, and then estimates whether these groups display significant differences in employment sensitivities to real sales growth. I use two measures of financial conditions: 1.) the median dividend-to-income ratio, and 2.) the Standard & Poor's Senior Debt Rating (the latter of these two measures is available for only 1,122 of the original 5,000 Compustat firms). Firms are then grouped into selected categories depending on their values of either of the aforementioned measures. The resulting designations are used to compare observed employment elasticities.

The results indicate that employment responses of low dividend payout firms significantly exceed those of high dividend payout firms. Results involving the latter S&P debt rating measure confirm these findings. However, as shown in Section 4.3 titled "Persistence and Expectations," the employment elasticities of firms critically depend on the time-series persistence of firm-level sales. Financial constraints significantly determine employment responsiveness to sales only in firms with persistent sales patterns. Those firms with relatively less correlated sales alter employment only slightly, if at all, in reaction to period-to-period sales changes. For these firms, employment growth is independent of financial constraints. Section 6 concludes.

2. LABOR HOARDING

This essay uses as its foundation the large amount of research on labor hoarding behavior by employers facing adjustment costs to labor and uncertainty over future product demand. Bentolila and Bertola (1990) describe the employment decisions of firms as hinging on both the revenue and the hiring and firing costs associated with labor inputs. The two authors formulate an optimization scenario under which firms maintain constant levels of employment when the marginal product of labor falls within a range determined in part by hiring and firing costs. Given the presence of adjustment costs to labor (that is, costs associated with the turnover and attrition or workers), firms opt to smooth labor over their expected product demand.

This partial adjustment behavior, originally described by Oi (1962), Solow (1968) and several others, translates into the purposeful hoarding of labor by firms in slack demand times. Absent any capital market imperfections, firms optimize employment by smoothing labor demand in the face of an expected output pattern. Viewing a volatile product demand, a firm partially adjusts labor to the changes in output. These standard labor hoarding models depict rigidities in employment adjustment as arising from uncertain future demand as well as hiring and firing costs to labor.

An important assumption in the traditional labor hoarding story is that firms have access to capital either through internal funds or readily available external financings. Under this assumption, firms hoard labor in order to avoid firing costs and retain more workers than is otherwise optimal. They consequently accept a potentially negative marginal product of labor. Wages paid to this excess labor must, by definition (since the marginal product is negative), be borrowed or taken out of internal funds.

The numerical dynamic programming results presented by Gross (1995) describe input decisions of firms under similar financial constraints. Concentrating his efforts on capital (rather than labor) inputs, Gross establishes a model of firms' long-term input behavior subject to adjustment costs, exogenous shocks to production and cash-flow constraints. The methodology he employs centers on a value function and corresponding constraints which incorporate adjustment costs, asset holdings and debt payments into a broader profit maximization formula. The control variable, that factor chosen by each firm, is a per period capital investment decision. Gross' findings strongly support the excess sensitivity results of previous constraint-type literature. Specifically, he observes a significant reduction in capital investment among financially constrained corporations with diminished sales. Less constrained firms, on the other hand, display investment patterns largely unaffected by current demand conditions.

Gross' results can be applied to the labor decision of a firm as well as that of capital if one accepts similar assumptions. First, labor, like capital, may "depreciate" through dissolutions of employee-employer relationships. That is, a constant percentage of labor exogenously separates from employment positions each period. Second, a firm faces costs of voluntarily adjusting its stock of labor in response to demand conditions and shocks. Inserting labor into Gross' framework as a replacement for capital would yield identical theoretical results. Firms that experience liquidity problems exhibit a heightened responsiveness of labor inputs to changes in cash flow and sales.

3. DATA

My analysis relies on a series of annual firm observations extracted from the Compustat database released by Standard & Poor's. The sample spans the years 1977-1994 and includes roughly 49,000 firm-year observations. Each firm listed in the Compustat extract is incorporated within the United States. In any given year, a firm may leave or join the sample. I exclude from the final sample firms for which there does not exist a continuous series of annual observations. The number of annual observations for each firm is dependent on when the firm enters and exits the sample. Thus, each firm has a maximum of eighteen observations in the final extract (one for each year), but it is not mandatory that a firm's set of observations span the entire sample. I further limit the sample to firms within major manufacturing and non-manufacturing industries and exclude any firms whose primary operations are classified in agriculture, mining or construction sectors.

Firms that experienced an acquisition, merger or asset sale comprising 10% or more of previous asset holdings were excluded from the sample. The remaining corporations that underwent some type of acquisition, merger or sell-off during a given sample year were initially isolated from the sample to allow for corrections of the sales and employment data. The data section of Chapter 1 details the methodology used to cleanse the employment level data of any difference that may be attributed to an acquisition-related event. As for the sales data, Compustat contains a variable listing the contribution of such events to the total sales revenue of a firm during an event year.

Extracting from the sample the sales figures of the acquisition contribution is simply then a matter of subtracting such contributions from the sales variable.[30]

The Compustat extract contains a wide range of information on each corporation including, among other things, asset and liability breakdowns; debt (both short- and long-term) responsibilities; detailed balance sheets values; employment; sales; profit; income; interest expenditure; and SIC (industry) code. The bulk of such information originated as fiscal year-end values in the each firm's annual 10-K and other related published corporate documentation required of all publicly traded companies. The Compustat manuals note a particularly important exception to this rule: the employment variable in all cases is reported in annual company reports, but the time frame over which the employment variable is tallied falls into two categories. The majority of the employment figures correspond to the level of labor at a firm at a given year end while the remaining minority of observations assigns an average annual employment level. Compustat does not provide coinciding comments to differentiate the two measures employed and, as a result, I treat all figures and annual changes similarly.[31]

For comparison purposes, non-ratio items such as income and sales were deflated using the 2-digit annual consumer and producer price deflators.[32] The sales and employment values were then computed as percentage changes from preceding years. The notation ΔE_t and ΔS_t will be utilized throughout the remainder of the chapter where E_t and S_t are firm-level employment and (deflated) sales figures, respectively, and Δ denotes the corresponding annual percentage change.

To investigate the impact of credit availability on employment behavior, I separate the original extract of firms into four distinct groups according to the liquidity of each firm. This exercise is used by Fazzari, Hubbard and Petersen (1988) and virtually all other "credit constraint" works cited in this chapter. This practice of grouping firms into

[30] Sharpe (1994) and Fazzari, Hubbard and Petersen (1988) choose to deal with the acquisition issue by ejecting from their samples firms that were involved in merger and acquisition activity resulting in a change of greater than ten to fifteen percent of a firm's original (preceding year's) asset level.
[31] This seems to be standard practice in works using Compustat employment data such as Sharpe (1994).
[32] This information is available in its complete form from the Survey of Current Business Office in Washington, D.C. They may be reached by calling (202) 606-5307. A hard-copy version is in Tables 10 and 14 of Yuskavage (1996). These tables record only selected years between 1959 and 1994.

relative liquidity categories based on dividend-income ratio information is not without its critics. Kaplan and Zingales (1995) undertake an "in-depth analysis" of the firms designated by Fazzari, Hubbard and Petersen as being low-dividend, and therefore, financially constrained. Using additional information from the management discussions in each firm's annual report, Kaplan and Zingales refute the findings of Fazzari, Hubbard and Petersen. They contend that some of the companies originally characterized as "constrained," in fact, had substantial cash holdings (e.g. Hewlett-Packard). Fazzari, Hubbard and Petersen (1996) respond to this critique, arguing that: 1.) the sample size used by Kaplan and Zingales was too small (49 firms), and 2.) Kaplan and Zingales base their rejection of credit constraint designations on "self-serving managerial statements that may present a distorted picture of firm's availability of finance."

Recognizing the controversy surrounding the implementation of the dividend-income ratio as a gauge of financially liquidity, I present summary statistics of relative interest coverage ratios and cash flow margins in tables below suggesting that low-dividend firms are more likely to be financially illiquid, possessing significantly less cash holdings and more debt obligations. I use a supplementary measure, the S & P debt rating, to confirm the results yielded by the dividend payout analysis.

Regarding the classification scheme, I determined the appropriate distribution after scanning decile breakdowns of firms' median constraint measure, dividend-to-income ratio.[33] Those firms with a dividend-to-income ratio greater than 0.45 – those in the highest 25 percent of the entire sample – were broadly lumped in the least leveraged, least credit constrained category. The results section contains a more detailed description of my classification scheme.

All firms within the Compustat database are relatively large and established publicly traded firms. The differentiation with respect to credit availability within the

[33] The dilemma discussed repeatedly in the credit constraint literature centers around the issues of adequately measuring a firm's financing capacity. Underlying this is the problem of designating a firm as constrained while taking into account all other possible facets of a firm's operating environment. Firms may, for example, voluntarily choose to hold a high level of debt relative to assets because that may be the norm in a certain industry. Furthermore, firms with high debt-to-asset ratios may be in such circumstances due to investment opportunities made available to them. Fazzari, Hubbard and Petersen first attempted to mitigate this problem by breaking firms into classes based on consistent dividend payout ratios.

sample will therefore be less than that of a complete cross-sectional sample of both private and public or young and old companies. Since these firms have already acquired financing through securities issuances, capital is presumably more accessible to them than to small, privately owned firms. For these latter types of firms, information concerning ownership rules, histories, management procedure and philosophies are more difficult to collect than publicly traded corporations who are required to periodically disclose accounting figures and management objectives to shareholders.

4. RESULTS (ENTIRE SAMPLE)

4.1 Summary Statistics

Firms are divided into high, medium-high, medium-low and low dividend payout firms for the purposes of employment sensitivity examinations. Those firms assigned to the "least constrained" group, Class 4, fell in the uppermost quartile of the dividend-to-income distribution and possessed dividend-to-income ratios greater than 0.45. Class 3 firms recorded median ratios between 0.45 and 0.12 while Class 2 firms recorded ratios between 0.12 and 0.[34] Class 1 corporations – those firms considered "most constrained" – did not distribute any dividends in the majority of sample years and consequently possessed median dividend-to-income ratios equal to 0.

The rationale behind such a classification scheme has been thoroughly discussed in literature relying on similar measures of financial liquidity. The primary assumption is that low-dividend, high retention firms encounter larger costs of capital, labor, production and investment than can be accommodated by their cash holdings. They retain the bulk of their net income in order to maintain their operations. These firms, relative to their "unconstrained" counterparts, should be both more sensitive to and more dependent on external sources of financing when retained income levels fall. Fazzari, Hubbard and Petersen (1988) state that, "Observed retention practices provide a useful a priori criterion for identifying firms that are likely to face relatively high costs of external finance. If the cost disadvantage of external finance is large, it should have the greatest effect on firms that retain most of their income. Is the cost disadvantage is slight, then retention practices should reveal little about financing practices, q values, or investment behavior."

Summary statistics computed for each of the dividend-income stratification are reported in Tables 4.1 and 4.2 below.

Table 4.1. Mean Statistics of Firms (Entire Sample)

Dividend - Income Ratio Grouping	Real Sales Growth Rate	Std. Dev. of Emp. Growth Rate	Std. Dev. of Real Sales Growth Rate	# of Firms
Class 1 (D-I Ratio == 0)	0.048	0.278	0.301	3,201
Class 2 (0.12 > D-I Ratio > 0)	0.044	0.182	0.175	322
Class 3 (0.45 > D-I Ratio > 0.12)	0.022	0.130	0.137	1,172
Class 4 (D-I Ratio > 0.45)	0.023	0.110	0.151	535

Table 4.2. Mean Statistics of Firms (Entire Sample)

Dividend - Income Ratio Grouping	Interest Coverage Ratio[35]	Median Cash Flow Margin	# of Firms
Class 1 (D-I Ratio == 0)	0.252	0.019	3,201
Class 2 (0.12 > D-I Ratio > 0)	0.248	0.103	322
Class 3 (0.45 > D-I Ratio > 0.12)	0.132	0.106	1,172
Class 4 (D-I Ratio > 0.45)	0.092	0.181	535

The standard deviations of both employment growth and real sales growth rise significantly as the dividend payout ratio falls. Indeed, the level of dividend payout may be directly impacted by the stability of firm sales. Corporations with more volatile sales may choose to retain more earnings as a precautionary, albeit sometimes insufficient, cushion in the event of adverse sales. When retained earnings shrink in response to a negative demand shock, cash stores, already low from previous sales cycles, may not be enough to pay labor costs. The low-dividend firm must fire workers.

[34] The Class 2 threshold dividend payout ratio was set at 0.12 to agree with Hubbard, Kashyap and Whited (1995) and other credit constraint papers that designate a firm with a dividend-income ratio below 0.12 - 0.17 as "financially constrained."

[35] Interest Coverage Ratio refers to (Interest Payments)/(Interest Payments + Income Before Extraordinary Items). The statistics include firms with positive interest coverage ratios. Furthermore, the figures represent interest coverage ratio means for the year 1994.

The average cash flow margin, defined as the ratio of total cash flow to net sales, rises significantly with the median dividend-to-income ratio classification. Those firms that extend proportionally fewer dividends to their shareholders are more restricted in terms of cash holdings, across sample years, than firms that offer larger dividend payouts. This suggests that, although low-dividend firms may retain a greater percentage of their income, they are also substantially less liquid. This is consistent with the hypothesis that high-retention firms allot more of their cash-on-hand to costs rather than saving devices. They are more likely seek external sources of financing under adverse product demand conditions.

Furthermore, these low dividend payout firms possess more extensive and expensive debt obligations relative to revenue. Not only do the lowest dividend payout firms allocate larger proportions of their sales income to interest payments (note the various interest coverage ratios), but they also exhibit, on average, higher debt-to-asset ratios than any other class of firms in the sample. These results provide an appropriate foundation on which to estimate employment growth elasticities with respect to real sales movements, particularly as such sensitivities differ across dividend payout classes.

4.2 Regressions

The series of regressions presented in Table 4.3 below rely on slightly different specifications. All regressions rectify biases in coefficients' standard errors inherent in panel estimation by employing Huber correction methods. Where it is noted, selected estimations control for 2-digit industry fixed effects and implement an instrumental variables approach to correct for the assumed endogeneity arising from the current employment growth and current real sales growth relationship. The instruments used to correct for the endogeneity of the real sales growth variable are aggregate gdp as well as 2- and 3-year lagged real sales growth figures.

For this initial set of regressions, I use as the gauge of financial liquidity the median firm-level dividend-to-income ratio of each corporation appearing in the

Compustat extract. Consistent with the groupings in the descriptive statistics section of this essay, the interaction terms originate from the classification scheme where each firm is parceled into a one of four groups depending on the value of its median dividend-to-income ratio.

For firm i in year t, the employment regressions take the following form:

$$\Delta E_{it} = a + b(CC)*\Delta S_{it} + I_i + T_t + SZ_i + u_{it}$$

where CC denotes the credit constraint dummies, I denotes the industry-specific fixed effect for firm i , T refers to annual time effects, SZ is the predetermined size grouping, and u represents the error term.[36] In selected regressions, I control for fixed effects by removing 2-digit industry means from each variable. This procedure eliminates the need to include major industry dummies in the specifications, but, as shown in the tables below, does not change the results dramatically.

The general specification estimates employment growth at time t as being dependent only on real sales growth at time t and not on expected future sales growth. This omission does not presume that each firm's employment decisions are independent of expected future product demand. Rather, I assume that the following condition holds:

$$E(\Delta S_{t+1}) = \Delta S_t + e_{t+1}$$

where e is an idiosyncratic shock and all **available** information at time t about sales in future periods is incorporated into the value of current real sales growth. Since the shock

[36] I classify firms into five size-specific groups based on their real median asset levels; each grouping contains an equal number of firms. The 20^{th}, 40^{th}, 60^{th} and 80^{th} percentage values of real median assets provided the appropriate cut-off points. The following table lists the range of median assets for each class.

Group	**Asset Range (in Millions of Dollars)**
Size 1	0 - 11.139
Size 2	11.140 - 45.379
Size 3	45.380 - 157.491
Size 4	157.492 - 865.116
Size 5	865.117 or more

cannot be observed at time t, expectations of sales at time t+1 are equal to sales growth rate at time t.[37] I remove this assumption in Section 4.3 where I examine the impact of sales persistence anticipation in similarly specified regressions.

The results reported in Table 4.3 confirm that credit constraints significantly effect firms' employment responsiveness to sales. Those coefficients most relevant to this essay are those on the real sales rate variable (instrumented or not) and the interaction terms with the financial constraint measure. The first two columns report estimations without the introduction of instrumental variables or fixed-effects corrections. Subsequent columns introduce these procedures separately and, finally by Column I and J, in tandem. The regressions generating the results in Columns F through J were purged of 2-digit industry fixed effects.

[37] Sharpe (1994) proposes that firms have perfect foresight in terms of future sales growth. Consequently, he includes actual observed real sales growth at time t+1 in each estimation of current employment growth. This method, I assert, is based on an incorrect assumption and, furthermore, introduces a significant source of endogeneity and bias to his estimations.

Table 4.3. Employment Growth Regression Results (Entire Sample). Dependent Variable: Firm-Level Employment Growth

Explanatory Variables	A: Coeffs. (S.E.)	B: Coeffs (S.E.)	C: Coeffs. (S.E.)	D: Coeffs. (S.E.)	E: Coeffs. (S.E.)
Class 2	-0.009 (0.003)	-0.014 (0.003)	-0.008 (0.003)	-0.033 (0.004)	-0.028 (0.004)
Class 3	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.007 (0.003)	-0.007 (0.003)
Class 4	0.002 (0.005)	0.004 (0.004)	-0.002 (0.005)	-0.003 (0.005)	-0.011 (0.006)
Sales Rate	0.492 (0.026)	0.497 (0.026)	0.497 (0.026)	0.597 (0.048)	0.624 (0.054)
Class 2 x Sales Rate	-0.103 (0.028)	-0.113 (0.28)	-0.108 (0.028)	-0.126 (0.065)	-0.124 (0.065)
Class 3 x Sales Rate	-0.144 (0.041)	-0.149 (0.041)	-0.142 (0.041)	-0.310 (0.069)	-0.304 (0.069)
Class 4 x Sales Rate	-0.340 (0.043)	-0.328 (0.044)	-0.345 (0.043)	-0.353 (0.108)	-0.345 (0.107)
Size 2	0.013 (0.004)		0.013 (0.004)		0.015 (0.006)
Size 3	0.020 (0.004)		0.020 (0.004)		0.023 (0.006)
Size 4	0.020 (0.004)		0.020 (0.004)		0.025 (0.006)
Size 5	0.012 (0.004)		0.011 (0.004)		0.011 (0.006)
Manufacturing: Durable	-0.015 (0.003)		-0.014 (0.003)		-0.016 (0.004)
Trans., Comm., & Utilities	0.008 (0.004)		0.009 (0.004)		0.012 (0.005)
Wholesale and Retail Trade	0.006 (0.004)		0.007 (0.004)		0.008 (0.005)
FIRE	0.022 (0.004)		0.023 (0.005)		0.017 (0.005)
Health, Ed. & Legal Services	0.025 (0.005)		0.026 (0.007)		0.020 (0.007)
Business Services	0.028 (0.007)		0.029 (0.007)		0.022 (0.009)
2-Digit gdp			-0.080 (0.013)		
Constant	-0.013 (0.008)	0.002 (0.002)	-0.011 (0.006)	0.003 (0.003)	0.001 (0.007)
R-Squared	0.2345	0.2255	0.2351		
# of Observations	49, 378	49, 883	49, 378	44, 114	44, 114

Note: Columns A, B and C use neither instrumental variables or the fixed effects corrections. Columns A, C and E exclude size-1 firms and nondurable manufacturing firms and include year dummies. Columns D and E instrument for real sales growth rate using gdp, 2-year and 3-year lagged real sales growth.

Table 4.3. Continued. Employment Growth Regression Results (Entire Sample). Dependent Variable: Firm-Level Employment Growth

Explanatory Variables	F: Coeffs. (S.E.)	G: Coeffs. (S.E.)	H: Coeffs. (S.E.)	I: IV (S.E.)	J: IV (S.E.)	K: IV (S.E.)	L: Interact. (S.E.)
Class 2	-0.014 (0.003)	-0.014 (0.003)		-0.033 (0.004)	-0.028 (0.004)		-0.015 (0.003)
Class 3	-0.003 (0.003)	-0.004 (0.003)		-0.008 (0.003)	-0.009 (0.004)		-0.003 (0.003)
Class 4	0.003 (0.004)	0.003 (0.004)		-0.005 (0.005)	-0.013 (0.006)		-0.002 (0.005)
Sales Rate	0.493 (0.026)	0.490 (0.026)	0.505 (0.011)	0.558 (0.048)	0.560 (0.048)	0.615 (0.047)	0.333 (0.036)
Class 2 x Sales Rate	-0.112 (0.028)	-0.110 (0.028)	-0.121 (0.012)	-0.116 (0.064)	-0.124 (0.064)	-0.091 (0.053)	-0.009 (0.028)
Class 3 x Sales Rate	-0.148 (0.041)	-0.145 (0.041)	-0.161 (0.019)	-0.282 (0.068)	-0.282 (0.068)	-0.338 (0.077)	-0.118 (0.037)
Class 4 x Sales Rate	-0.326 (0.044)	-0.324 (0.044)	-0.277 (0.015)	-0.323 (0.105)	-0.327 (0.104)	-0.405 (0.082)	-0.230 (0.042)
Size 2		0.013 (0.006)			0.015 (0.006)		0.009 (0.004)
Size 3		0.020 (0.006)			0.024 (0.007)		0.008 (0.004)
Size 4		0.027 (0.007)			0.026 (0.007)		0.008 (0.005)
Size 5		0.012 (0.007)			0.012 (0.006)		0.001 (0.004)
Constant	0.002 (0.002)	0.003 (0.002)	-0.010 (0.002)	0.005 (0.003)	-0.013 (0.008)	-0.015 (0.002)	-0.002 (0.005)
R-Squared							
# of Obs.	49, 883	49,883	49,883	44,114	44,114	44,114	44, 114

Note: All regressions, with the exception of Columns H and K, correct for 2-digit industry fixed effects. Columns H and K control for firm effects and include size x sales interaction terms. Columns G and J 's estimations include year and firm size dummies; size-1 firms were dropped from the reports. Column I, J and K 's regressions instrument for real sales growth rate using aggregate gdp as well as 2- and 3-year lagged real sales growth. R-Squared figures are meaningless in 2SLS analyses. As such, they are not reported.[38] Column L includes interaction terms of real sales growth with the major industry group (1-digit SIC) and real sales growth with the size group.

The coefficients from Table 4.3 indicate that an increase in the sales growth rate of 1 percentage point generates anywhere from a 0.333 to a 0.627 percentage point rise in the growth rate of firm-level employment among the lowest dividend-paying firms. These differences are accounted for by the introduction of explanatory variables, instruments

[38] I attempted to use real annual cash holdings as an additional explanatory variable under the hypothesis that sales growth could not pick up all period-to-period short-term asset conditions. I was pleased to see that the cash holdings coefficient was never significant. I thus proceeded to rely on real sales growth as a proxy for firm-level short-term asset fluctuations.

and fixed industry-level corrections. As the median dividend payout improves, the responsiveness of employment to sales declines significantly. These results are consistent across the regressions presented above. The coefficients on the interaction terms retain their approximate, increasingly negative values. The addition of size x sales and industry x sales interaction terms in Column J diminishes the responsiveness across dividend payout categories, but the between-class differences remain significant. The coefficient on the least constrained class ranges from -0.230 to -0.353, suggesting that these firms' employment decisions are much less responsive to sales fluctuations than those of the less financially liquid firms.

Additional regressions were estimated using only those observations with negative real sales growth values. My objective is to observe whether the employment sensitivity - liquidity relationship holds when the credit constraints of firms are binding. As sales decrease, constrained firms retain fewer earnings and, as I have argued throughout, respond by lowering employment in larger proportions.

The results of this exercise appear in Table 4.4 and confirm that employment growth in firms with the lowest dividend payout ratios is the most sensitive to decreases in real sales. Class 3 and 4 firms (the least leveraged) exhibit much less sensitivity of employment. All interaction coefficients on the Class 3 and 4 groups are significantly negative.

Table 4.4. Negative Sales - Employment Growth Sensitivity Regressions. Explanatory Variable: Firm-Level Employment Growth.[39]

Explanatory Variables	A: Coeffs. (S.E.)	B: Coeffs. (S.E.)	C: Coeffs. (S.E.)	D: Coeffs. (S.E.)
Class 2	-0.055 (0.008)	-0.058 (0.008)	-0.084 (0.005)	-0.075 (0.006)
Class 3	-0.006 (0.008)	-0.008 (0.008)	0.020 (0.005)	0.017 (0.005)
Class 4	-1.0E-4 (0.009)	-0.010 (0.009)	0.048 (0.007)	0.044 (0.007)
Sales Rate	0.540 (0.059)	0.547 (0.058)	0.766 (0.235)	0.949 (0.248)
Class 2 x Sales Rate	-0.191 (0.062)	-0.183 (0.062)	-0.153 (0.257)	-0.260 (0.261)
Class 3 x Sales Rate	-0.173 (0.085)	-0.162 (0.085)	-0.501 (0.255)	-0.460 (0.252)
Class 4 x Sales Rate	-0.384 (0.091)	-0.413 (0.091)	-0.575 (0.235)	-0.432 (0.223)
Size 2		-0.021 (0.009)		0.009 (0.009)
Size 3		-0.037 (0.009)		0.003 (0.009)
Size 4		-0.033 (0.009)		0.014 (0.009)
Size 5		-0.017 (0.008)		0.023 (0.009)
Constant	0.008 (0.006)	0.057 (0.014)	-0.059 (0.004)	-0.084 (0.017)
# of Observations	18, 999	18, 868	17, 307	17, 307

Note: Industry (2-digit) fixed effects were removed from each regression. Specifications recorded in Columns B and D incorporate size and time dummies into the estimations and omit size-1 firms. Regressions in Column C and D employ 2SLS techniques using aggregate gdp as well as 2- and 3-year lagged sales as instruments.

Clearly, high dividend firms exhibit much more employment smoothing over the course of sales cycles. This smoothing pattern tends to diminish as earnings retention – and, perhaps, dependence on such retention behavior – rises.

4.3 Persistence and Expectations

The specifications of the previous section assume that firms' expectations of their sales processes depend solely on real sales in the current period. The findings are based

[39] Includes only those observations with negative real sales growth figures.

on the premise that firms either ignore all sales persistence information or hold no information about the persistence and volatility of their sales and, consequently, can only react (by changing employment) to current sales figures. This section removes that assumption, allowing employment elasticities to vary not only by the extent of firms' financial constraints, but also by the degree to which firms expect changes in sales to persist. Firms observing little persistence in sales across periods, I contend, are unlikely to significantly alter employment levels in response to current sales shocks. The diminished responsiveness of employment to sales stems from uncertainty over the direction of future sales. If a firm recognizes that a negative shock to sales in time t will be reversed in time t+1, the firm will choose to maintain current employment levels rather than firing today and re-hiring at t+1 and incurring a double dose of adjustment (firing and hiring) costs. Firms will be reluctant to respond employment fully to the initial change in sales when sales changes are slightly persistent. As sales are more persistent, the sensitivity of employment grows. Firms bear adjustment costs in anticipation of lasting sales changes.

Differences in the employment sensitivities of firms arising from sales persistence experiences imply that the results from the previous section may represent an overly broad characterization of the employment behavior of financially liquid and illiquid firms. In this section, I modify the earlier estimation of employment elasticity to account for firms' expectations of future sales changes. This modification, I hypothesize, will yield significantly different elasticities which depend on each firm's sales persistence history.

In order to measure the persistence of sales, the following sales process is estimated for each firm in the sample:

$$\log(\text{Sales})_t = bt + \rho \log(\text{Sale})_{t-1} + u_t$$

where t is a trend variable and u is the residual. The logarithm of real annual sales is assumed to follow an AR(1) process with a trend. I investigated several minor variations of the above specification – including one with an additional term, Δ Log(Sale)$_{t-1}$, on the right-hand side – none of which greatly changed the final persistence classifications described below. Given the data limitations of the short time series available for each firm, I restrict the persistence estimations to those firms with 11 or more sequential annual observations. I then run 2,472 regressions – one for each firm – and retain each sales persistence parameter, ρ. The AR(1) process, while perhaps overly simplified, yields, at the least, a rough estimate of the persistence estimations. It was not possible to inspect and test each of the 2,472 regressions for correct specifications so the assumption that each firm's sales exhibit something resembling AR(1) behavior is critical.

I classify firms according to the estimated sales persistence parameters. As I am most interested in the **relative** persistence rankings of the sample firms, the downward bias in ρ resulting from the short time series lengths and the inclusion of a time trend in the specifications is of little relevance to the final classifications. I consider firms with a ρ equal or greater to 0.85 to have high sales persistence, between 0.85 and 0.25 to have medium sales persistence and less than 0.25 to have low sales persistence. These groupings correspond to approximately the 85th and 12th percentile distribution of firm-level persistence parameters.

The results in Table 4.5 are generated from separate regressions estimated on each of the sales persistence groups. The specifications, similar to that of Column J of Table 4.3, remove 2-digit industry effects and include size and industry interactions with real sales growth.

Table 4.5. Employment Sensitivity Regressions, by Observed Firm-Level Sales Persistence. Dependent Variable: Firm-Level Employment Growth

Explanatory Variable	A: High Persistence (ρ>0.85)	B: Medium Persistence (0.85>ρ>0.25)	C: Low Persistence (0.25>ρ)
Class 2	-0.023 (0.006)	-0.020 (0.003)	-0.020 (0.011)
Class 3	-0.004 (0.005)	-2.1E-4 (0.003)	-0.013 (0.011)
Class 4	-0.008 (0.012)	-0.003 (0.006)	2.4E-4 (0.020)
Real Sales Rate	0.401 (0.118)	0.308 (0.053)	0.058 (0.088)
Class 2 x Sales Rate	-0.061 (0.059)	0.057 (0.042)	0.086 (0.074)
Class 3 x Sales Rate	-0.252 (0.072)	-0.071 (0.048)	0.080 (0.082)
Class 4 x Sales Rate	-0.267 (0.106)	-0.188 (0.048)	-0.096 (0.084)
Size 2	0.005 (0.013)	-1.89E-4 (0.005)	0.018 (0.013)
Size 3	0.004 (0.012)	0.004 (0.005)	0.020 (0.014)
Size 4	0.004 (0.004)	0.001 (0.005)	0.021 (0.014)
Size 5	-0.003 (0.012)	-0.003 (0.005)	0.012 (0.013)
Constant	-0.011 (0.012)	-0.007 (0.005)	-0.007 (0.016)
# of Observations	5, 477 (384 firms)	26, 496 (1,790 firms)	3, 960 (298 firms)

Note: The 2-digit SIC fixed effects are removed from each regression. These regressions include interaction terms of real sales growth with size.

The most notable feature of these results is the discontinuity of employment sensitivities across persistence groupings. Results from both the high persistence and medium persistence classes resemble the results of Column J in Table 4.3 insofar as the employment elasticities decline as financially liquidity increases. However, these sensitivities differ according to firms' perceptions of their own sales patterns. Firms observing long-lasting sales changes react more completely, in terms of employment, to sales than firms with a history of drastically different (or even uncorrelated) year-to-year sales levels.

Note that, as sales become less persistent, the effect of credit constraints on employment responsiveness diminishes, eventually becoming insignificant across all dividend payout classes. None of the coefficients on the real sales growth x credit constraint interaction terms are significantly different from zero; this suggests that the volatility of sales among firms in this class negate the relevance of credit constraints in determining firm-level responsiveness to sales. The most constrained firms in this class may 1.) not encounter binding liquidity constraints because sales move dramatically over time or 2.) recognize, like their unconstrained counterparts, that it may be more costly to constantly adjust labor to such unpredictable and frequent sales changes. They respond to sales, in effect, by not responding at all, for the alternative would prove significantly too costly. Conversely, credit constrained firms who are able to better anticipate future sales (through higher sales persistence) react significantly more to sales through employment changes. The sensitivity coefficient of the most constrained class of firms rises from 0.058 to 0.308 to 0.401 as sales progress from the least persistent to the medium persistence to the high persistence designation. Thus, it appears that credit constraints become an important determinant of employment sensitivities only when firms possess some foreknowledge of sales behavior.

5. STANDARD & POOR'S RATING RESULTS

This section presents estimates of employment sensitivities using a dramatically different improved measure of financial liquidity, the Standard & Poor's (S&P) senior debt rating.[40] Whited (1992), in analyzing investment sensitivities, uses the fact that some firms have a corporate bond rating to distinguish them, in liquidity terms, from those firms for whom a rating was not available or did not exist. Whited, however, does not attempt to gauge the various degrees of liquidity based on the specific published debt rating. I choose to present this evidence after the prior section for two reasons: 1.) roughly one-fourth of the firms in the previous section's sample possess senior debt ratings and 2.) the results reported in this section validate the use of the dividend-to-

[40] Compustat contains other similar measures such as the short-term borrowing rate and the commercial paper rate. The senior rating was available for the largest number of firms.

income measure as an appropriate proxy for financial well-being. Those firms with the more favorable senior debt ratings display, on average, significantly higher dividend payout ratios than those firms classified as riskier borrowers. Moreover, the results yielded by the senior debt rating specifications confirm the results of the dividend payout regressions in the previous section.

The advantages of using the S&P senior debt rating over other possibilities that measure only a single aspect of a firm's financial status are apparent. Incorporating into its rankings elements such as willingness and ability to repay debts, the Standard & Poor's measure bypasses many of problems associated with using debt-to-asset or dividend-to-income ratios as proxies for financial liquidity. It minimizes the likelihood that a firm with high debt obligations relative to asset holdings (which may have otherwise been classified as credit constrained) may have accumulated such a debt so as to take advantage of lucrative investment opportunities. In sum, the S&P rating system represents a more broad-based approach toward and analysis of the financial status of firms, one that accounts for a comprehensive set of firm-level liquidity measures.

The S&P rating system spans twenty-five potential grades, from AAA to BB- to D to past default. A description of each category appears in Appendix A. According to the Compustat manual, the Standard & Poor's senior debt rating is an "assessment of creditworthiness of an obligor with respect to a senior ... debt obligation. Senior debt represents long-term debt that are not subordinate to any other long-term debt issues." S&P bases these ratings and assignments on the likelihood of default. This concept encompasses "1.) the capacity and willingness of the obligor to the timely payment of interest and repayment of principal in accordance with the terms of the obligation and 2.) the nature and provisions of the obligation."

The consideration of the unique features of each firm's debt contracts, financial situation, current and future product demand/sales positions, and history of debt servicing activity render the S&P debt rating variable an attractive candidate to measure firm-level credit worthiness and external financing accessibility. Presumably, those firms assigned AAA ratings have strong internal financing options and solid cash holdings, much more so than corporations with debt ratings below A+. The S&P rating represents an amalgamation of risk factors, each of which plays a role in financing and repayment operations of firms.

Recognizing that the numerous different potential debt ratings were overly narrow for the purposes of my analysis, I grouped all 1,112 firms into three broader categories spanning AAA through AA- (Group 3, 161 firms), A+ through BBB- (Group 2, 629 firms), and BB+ through D (Group 1, 331 firms). The first grouping, those firms ascribed with the worst debt ratings, may initially appear overly broad, but an examination of the distribution of the original ratings reveals that merely eight firms in the sample possessed a rating of CCC+ or below; the remaining corporations in the bottom third were clustered between BB+ and B-. Compustat describes these firms as having an "identifiable vulnerability of default." Conversely, the highest rated companies' capacities to pay interest and repay principal are "extremely strong."

5.1 Summary Statistics

Table 5.1 provides mean standard deviation statistics of the S&P subsample. Consistent with findings from the previous section, the standard deviations of both employment growth and real sales growth rise as the categories become composed of firms with increasingly worse debt ratings. These results imply that the greater variability of sales may, in fact, contribute to a firm's inconsistent, or substandard, debt repayment activity and exacerbate employment fluctuations. Not surprisingly, Group 1 firms, those with the lowest debt ratings, displayed significantly higher employment and sales volatility. Conversely, Group 3 firms, the more traditional, "blue chip" corporations, exhibited limited variance in sales and employment. The issue of comparative employment sensitivities is left to the following section where I present results suggesting that a significant portion of the additional employment volatility cannot be explained solely by the higher sales variability.

Additional summary statistics by major industrial sectors are reported in Tables 5.2- 5.5. My primary intention in presenting Tables 5.4 and 5.5 is to validate the S&P ratings as they apply to other, more traditional and frequently applied measures of financial liquidity -- dividend-to-income ratios and debt-to-asset ratios. In both cases, the ratings correspond consistently to the classification schemes used in past research; as debt ratings improve, the average debt-to-asset ratio falls while the dividend-to-income ratio rises. These results strengthen my contention that the S&P rating is an all-inclusive and extremely valuable gauge of firms' overall credit and financial conditions. The remaining industry tables substantiate the findings in Table 5.7.

Table 5.1. Mean Standard Deviation of Firms (S&P Rated Firms)

Debt Rating Grouping	Standard Deviation of Employment Growth Rate	Standard Deviation of Real Sales Growth	Observations
Group 1 (BB+ through C)	0.194	0.179	331
Group 2 (A+ through BBB-)	0.133	0.154	629
Group 3 (AAA through AA-)	0.080	0.115	161

Table 5.2. Mean Standard Deviation of Employment Growth Rate of Firms, by Major Industry and Debt Rating

Major Industry Group	Group 1 (BB+ through C)	Group 2 (A+ through BBB-)	Group 3 (AAA through AA-)
Manufacturing: Nondurable	0.189	0.127	0.080
Manufacturing: Durable	0.186	0.131	0.072
Trans., Comm., & Utilities	0.176	0.104	0.062
Wholesale and Retail Trade	0.204	0.127	0.168
Finance, Insurance, R. Estate	0.209	0.167	0.096

Table 5.3. Mean Standard Deviation of Real Sales Growth Rate of Firms, by Major Industry and Debt Rating

Major Industry Group	Group 1 (BB+ through C)	Group 2 (A+ through BBB-)	Group 3 (AAA through AA-)
Manufacturing: Nondurable	0.186	0.130	0.121
Manufacturing: Durable	0.188	0.151	0.111
Trans., Comm., & Utilities	0.153	0.122	0.079
Wholesale and Retail Trade	0.163	0.105	0.088
Finance, Insurance, R. Estate	0.219	0.235	0.175

Table 5.4. Mean Debt-to-Asset Ratio of Firms, by Major Industry and Debt Rating

Major Industry Group	Group 1 (BB+ through C)	Group 2 (A+ through BBB-)	Group 3 (AAA through AA-)
Manufacturing: Nondurable	0.403	0.266	0.171
Manufacturing: Durable	0.313	0.226	0.154
Trans., Comm., & Utilities	0.504	0.369	0.315
Wholesale and Retail Trade	0.406	0.268	0.225
Finance, Insurance, R. Estate	0.250	0.288	0.276

Table 5.5. Mean Dividend-to-Income Ratio of Firms, by Major Industry and Debt Rating

Major Industry Group	Class 1 (BB+ through C)	Class 2 (A+ through BBB-)	Class 3 (AAA through AA-)
Manufacturing: Nondurable	0.199	0.367	0.472
Manufacturing: Durable	0.171	0.276	0.342
Trans., Comm., & Utilities	0.176	0.611	0.716
Wholesale and Retail Trade	0.087	0.269	0.174
Finance, Insurance, R. Estate	0.343	0.392	0.293

Table 5.6 contains the descriptive statistics that are most germane to not only the regression analysis of the following section, but also the premise of the entire essay. I have postulated throughout that liquidity constrained firms are prone to deviate from standard labor hoarding practices due to an inability to meet labor costs during downturns in demand. For these constrained firms, their patterns of employment, particularly in adverse demand conditions, correspond more closely to the fluctuations of sales. During downturns, a constrained firm has neither sufficient cash-on-hand nor adequate borrowing sources to prevent the necessary firing of workers; it simply cannot pay its wages and must layoff employees as sales decline. A more liquid firm, conversely, will display an employment pattern whose correlation with sales will be considerably less – the reason being that the liquid firm is able to optimally smooth labor over its sales cycles so as to avoid incurring repeated hiring and firing costs.

If smoothing is a viable option for firms with substantial cash holdings, one expects to observe comparatively higher sales-to-employment ratio standard deviations among such firms. The real sales-to-employment ratio is defined as the amount of real sales per employee for a given firm. Imagine, for example, two extreme cases, one firm that keeps labor constant (an extreme smoothing case) and another that sets its labor pattern to mimic that of sales. With the exception of the constant sales case, the smoothing firm will display larger movements in the sales-to-employment ratio, rendering its standard deviation larger as well.

The results in Table 5.6 confirm this hypothesis. In total and along industry and size dimensions, the poor debt-rating firms in Group 1 – those considered most liquidity constrained – possessed the smallest standard deviations of sales-to-employment growth, suggesting that employment levels closely followed real sales. In support of the relative smoothing practice, the standard deviation results of the least constrained firms were consistently the highest out of the three ratings categories.

Table 5.6. Mean Standard Deviation of Firm-Level Real Sales-to-Employment Ratio, by Major Industry and Size (S&P Rated Firms)

	Group 1 (BB+ through C)	Group 2 (A+ through BBB-)	Group 3 (AAA through AA-)
ALL	26.785	35.281	42.071
BY INDUSTRY:			
Manufacturing: Nondurable	24.271	29.367	35.689
Manufacturing: Durable	30.330	33.103	39.576
Trans., Comm., & Utilities	30.860	39.245	39.778
Wholesale & Retail Trade	23.014	22.156	31.082
F. I. R. E.	47.720	56.933	64.982
Health, Legal & Education Services	17.034	19.295	N/A
Business Services	17.029	11.466	N/A
BY SIZE:			
Size 3	25.139	38.748	31.737
Size 4	23.772	29.775	46.828
Size 5	31.809	37.256	41.631

Note: Size groupings 1 and 2 do not appear in the table because only 7 firms rated by S&P possessed small enough median real assets to be classified in these groups. The determination of size groups was based on a 20-, 40-, 60-, and 80-percentile breakdown of all firms' median real assets. These results exclude those firms with sales-to-employment standard deviations in excess of 100. Annual sales figures were deflated using the 2-digit industry price deflators for each year.

5.2 Regressions

In the table below, I employ the Standard & Poor's senior debt rating as a substitute for the dividend-to-income classifications utilized in the regressions of the previous section. These regressions are aimed at determining excess sensitivities of employment to real sales growth in the least liquid, most financially constrained firms. Each specification controls for industry-level fixed effects. Standard errors are adjusted using Huber estimation techniques due to biases caused by panel data analysis. The regressions include three classes of firms. Group 1 firms were assigned the worst debt ratings by Standard & Poor's. As the classification progresses from Group 1 to Group 3 the debt ratings improve. The regressions presented in Columns E through H correct for 2-digit industry effects. To ensure that financially constrained firms exhibit excess sensitivity in adverse sales conditions, I present in Table 5.8 the results of similarly specified regressions using only those observations with negative annual real sales growth.

Table 5.7. Employment Growth Regressions Results (S&P Rated Subsample). Dependent Variable: Firm-Level Employment Growth

Explanatory Variable	A: Coeffs. (S.E.)	B: Coeffs. (S.E.)	C: IV Coeffs. (S.E.)	D: IV Coeffs. (S.E.)	E: Coeffs. (S.E.)
Group 2	0.019 (0.005)	0.021 (0.005)	0.022 (0.007)	0.032 (0.007)	0.017 (0.005)
Group 3	0.015 (0.006)	0.022 (0.005)	0.024 (0.008)	0.039 (0.008)	0.013 (0.006)
Sales Rate	0.538 (0.039)	0.531 (0.039)	0.658 (0.136)	0.667 (0.148)	0.522 (0.039)
Group 2 x Sales Rate	-0.179 (0.046)	-0.172 (0.046)	-0.286 (0.049)	-0.260 (0.049)	-0.189 (0.046)
Group 3 x Sales Rate	-0.283 (0.059)	-0.266 (0.056)	-0.445 (0.158)	-0.464 (0.158)	-0.301 (0.053)
Size 2		0.012 (0.036)		0.049 (0.039)	
Size 3		0.079 (0.012)		0.124 (0.015)	
Size 4		0.057 (0.005)		0.082 (0.006)	
Size 5		0.037 (0.004)		0.051 (0.004)	
Manufacturing: Durable		-0.006 (0.004)		0.001 (0.005)	-0.007 (0.005)
Trans., Comm., & Utilities		0.011 (0.004)		0.016 (0.005)	
Wholesale and Retail Trade		0.025 (0.007)		0.033 (0.009)	13, 539
FIRE		0.049 (0.006)		0.056 (0.007)	
Health, Ed. & Legal Services		0.036 (0.009)		0.051 (0.011)	
Business Services		0.076 (0.015)		0.094 (0.026)	
Constant	-0.011 (0.004)	-0.005 (0.004)	-0.002 (0.006)	-0.007 (0.010)	
R-Squared	0.2558	0.2788			
# of Observations	13, 539	13, 539	12, 425	12, 428	

Note: Specifications recorded in Columns B and D incorporate size, 1-digit industry and time dummies into the estimations and omit size-1 firms and nondurable manufacturing firms. R-Squared figures are meaningless in 2SLS analyses, so they are not reported in Columns C and D. Regressions employing 2SLS techniques use aggregate gdp as well as 2- and 3-year lagged sales as instruments.

Table 5.7. Continued. Employment Growth Regressions Results (S&P Rated Subsample). Dependent Variable: Firm-Level Employment Growth

Explanatory Variable	F: Coeffs. (S.E.)	G: IV Coeffs. (S.E.)	H: IV Coeffs. (S.E.)	I: Interaction (S.E.)	J: Interaction (S.E.)
Group 2	0.021 (0.005)	0.018 (0.008)	0.030 (0.007)	0.016 (0.004)	0.015 (0.004)
Group 3	0.021 (0.006)	0.017 (0.008)	0.037 (0.008)	0.017 (0.005)	0.015 (0.005)
Sales Rate	0.518 (0.040)	0.753 (0.143)	0.784 (0.154)	0.450 (0.078)	0.454 (0.079)
Group 2 x Sales Rate	-0.181 (0.046)	-0.367 (0.155)	-0.353 (0.153)	-0.087 [illegible]	-0.085 (0.043)
Group 3 x Sales Rate	-0.285 (0.053)	-0.549 (0.160)	-0.515 (0.160)	[illegible] [illegible]	-0.208 (0.051)
Size 2	0.017 (0.039)		0.051 (0.042)	0.002 (0.036)	0.002 (0.037)
Size 3	0.084 (0.013)		0.127 (0.015)	0.076 (0.015)	0.077 (0.015)
Size 4	0.062 (0.006)		0.086 (0.006)	0.038 (0.005)	0.038 (0.005)
Size 5	0.040 (0.004)		0.052 (0.005)	0.020 (0.005)	0.020 (0.003)
Constant	-0.006 (0.009)	-0.009 (0.007)	-0.008 (0.010)	-0.051 (0.006)	-0.028 (0.008)
R-Squared					
# of Observations	13, 539	12, 425	12, 428	12, 428	12, 428

Note: Industry (2-digit) fixed effects were removed from the regressions of Columns E-H. Specifications recorded in Columns F and H incorporate size and time dummies into the estimations and omit size-1 firms. R-Squared figures are meaningless in 2SLS analyses and fixed effects specifications, so they are not reported. Regressions employing 2SLS techniques use aggregate gdp as well as 2- and 3-year lagged sales as instruments. Columns I and J include interaction terms of real sales growth with the major industry group (1-digit SIC) and real sales growth with the size group. Column J's regression includes year dummies.

Table 5.8. Negative Sales - Employment Growth Sensitivity Regressions (S&P Rated Firms). Explanatory Variable: Firm-Level Employment Growth.[41]

Explanatory Variables	A: Coeffs. (S.E.)	B: Coeffs. (S.E.)	C: Coeffs. (S.E.)	D: Coeffs. (S.E.)
Group 2	0.022 (0.013)	0.010 (0.013)	-0.022 (0.060)	-0.019 (0.064)
Group 3	0.011 (0.012)	0.004 (0.013)	-0.017 (0.061)	-0.013 (0.065)
Sales Rate	0.488 (0.090)	0.489 (0.091)	0.729 (0.264)	0.651 (0.220)
Group 2 x Sales Rate	-0.181 (0.048)	-0.144 (0.047)	-0.391 (0.117)	-0.339 (0.135)
Group 3 x Sales Rate	-0.356 (0.102)	-0.306 (0.101)	-0.589 (0.117)	-0.507 (0.140)
Size 2		0.003 (0.073)		-0.014 (0.083)
Size 3		0.090 (0.031)		0.090 (0.032)
Size 4		0.060 (0.013)		0.080 (0.010)
Size 5		0.036 (0.010)		0.065 (0.007)
Constant	-0.019 (0.011)	-0.009 (0.018)	-0.010 (0.054)	-0.012 (0.064)
# of Observations	4, 826	4, 826	4,517	4,517

Note: Industry (2-digit) fixed effects were removed from each regression. Specifications recorded in Columns B and D incorporate size and time dummies into the estimations and omit size-1 firms. Regressions in Column C and D employ 2SLS techniques using aggregate gdp as well as 2- and 3-year lagged sales as instruments.

Table 5.7 shows the dramatic and significant difference in the responsiveness to sales of constrained versus unconstrained firms. Those firms with the worst debt ratings display higher employment elasticities, increasing employment growth by 0.45 to 0.78 percentage points in response to a one percentage point rise in real sales growth. The slope decreases to approximately 0.22 (a sum of the sales rate and interaction term coefficients) in firms assigned the best debt ratings. This reinforces earlier findings. Firms facing costly external financing and limited internal funds alter employment levels in greater proportions when confronted with similar changes in sales. Table 5.8 reveals that the credit constrained - employment sensitivity relationship holds during periods of negative firm-level sales growth as well.

[41] Includes only those observations with negative real sales growth figures.

When the additional interaction terms are introduced to the estimations, the credit constraint x real sales growth coefficients unilaterally decline in absolute terms. These observed decreases reflect the importance of both firm size and industry in determining employment elasticities. A firm's responsiveness of employment to sales may be a product of some size or industry related factor which dictates the extent to which each firm reacts to sales changes. These factors could include, but are not limited to, higher labor adjustment costs (due to, for example, a historically greater union presence) in larger firms or selected industries. The fact that each liquidity-related coefficient presented in Columns I and J, upon correcting for the additional interactions, remain significantly different from zero, however, strengthens the hypothesis of this essay. Regardless of the specification employed, financial constraints are major determinants of employment growth rates, particularly as they influence the response of employment to real sales growth. Smoothing, or labor hoarding, is practiced to a much larger degree by firms with cheaper access to capital and a greater proportion of internal funds.

5.3 Persistence and Expectations

In the spirit of Section 4.3, I present in the table below estimations of employment sensitivities which account for differences in firm-level sales expectations. As discussed earlier, the previous regressions in Sections 4.2 and 5.2 consider firms' employment reactions to sales changes as being dependent solely on current sales growth rates. Here, I allow firms to anticipate sales patterns using information of their sales persistence histories. To estimate firm-level sales persistence, I run separate AR(1) regressions with a constant and time trend for each firm and use the observed coefficient on the lag of log (Real Sales), ρ, as the persistence measure. I classify each firm into one of three groups depending on the observed ρ.

The threshold values of the high, medium and low persistence categories are different from the values used in Section 4.3 because the Section 4.3 values resulted in small S&P Rated sample sizes. A firm with high sales persistence possessed a value of ρ greater than 0.75 while low sales persistence firms possessed a value of ρ less 0.38. Firms with observed coefficients falling between these two values were designated as having medium sales persistence. The persistence estimates were computed only on firms with 11 or more observations in the sample. Based on the relative persistence estimates, I label firms as having high, medium or low sales persistence and run separate employment sensitivity regressions on each group. Results are reported in Table 5.9.

Table 5.9. Employment Sensitivity Regressions, by Observed Firm-Level Sales Persistence. (S & P Rated Firms) Dependent Variable: Firm-Level Employment Growth

Explanatory Variable	**A: High Persistence** (ρ>0.75)	**B: Medium Persistence** (0.75>ρ>0.38)	**C: Low Persistence** (0.38>ρ)
Group 2	0.023 (0.006)	0.020 (0.006)	-0.009 (0.016)
Group 3	0.024 (0.005)	0.012 (0.008)	0.003 (0.019)
Real Sales Rate	0.583 (0.111)	0.552 (0.064)	0.215 (0.176)
Group 2 x Sales Rate	-0.031 (0.079)	-0.153 (0.049)	0.085 (0.133)
Group 3 x Sales Rate	-0.241 (0.085)	-0.186 (0.067)	-0.036 (0.171)
Size 4	0.005 (0.006)	0.017 (0.005)	0.073 (0.096)
Constant	-0.038 (0.007)	-0.037 (0.007)	-0.014 (0.017)
# of Observations	2, 073 (135 firms)	4, 467 (287 firms)	1, 239 (83 firms)

Note: The 2-digit SIC fixed effects are removed from each regression. These regressions include interaction terms of real sales growth with size.

The results of Table 5.9 corroborate the findings of the earlier persistence section. Liquidity constraints become important in the determination of employment elasticities as firm-level sales become more persistent. A firm that has relatively uncorrelated year-to-year sales levels will be more reluctant to change employment in response to sales changes. This is presumably because a firm with low sales persistence wants to avoid the costs of constantly adjusting labor to unpredictable movements in sales. Regardless of the extent of firm-level liquidity constraints, unpredictable sales minimize the responsiveness of employment to sales growth. Only when a firm is better able to anticipate future sales (a higher ρ) do credit constraints become a significant explanatory factor in employment elasticities with respect to sales.

6. CONCLUSION

The general null hypothesis of this essay is that financially constrained firms – firms which regularly retain the bulk of their income and have been objectively assigned bad debt ratings – display the highest sensitivity of employment growth to changes in real sales growth. This is because they lack "cheap" external financing options and are unable to engage in labor hoarding activity. Firms that consistently operate with low dividend payout ratios, I argue, use their retained earnings for investment finance and production operations. Such expenditures are necessarily pared down in times of diminished sales revenue because the cost disadvantages of gaining external finances to maintain steady spending activity are high. Those firms with the most liberal dividend policy and larger available cash holdings enjoy fewer disruptions in investment and employment behavior; when needed, dividend payments can be cut back and used as a source of cheaper internal funding. Consequently, firms with lower financing costs (through internal or external means) optimize employment behavior subject to budget constraints and adjustment costs and are more apt to smooth employment levels across expected sales fluctuations.

The evidence I have presented clearly supports this hypothesis. In a series of regressions using either dividend-to-income ratios or senior debt ratings as gauges of firm-level credit accessibility, I find that the elasticity of employment with respect to real sales depends significantly on firm-level financial constraints. Firms that face financial constraints exhibit higher sensitivity of employment to sales growth.

However, the difference in the employment responsiveness of financially illiquid and liquid firms declines as firm-level sales patterns become less persistent. Specifically, credit constraints become irrelevant in the determination of employment growth among firms displaying the least persistent sales patterns. Limiting employment sensitivity, I argue, is less costly for this subset of firms than continually adjusting employment and incurring the related hiring and firing costs in response to unpredictable sales. The employment behavior of firms with significant sales persistence, conversely, resembles that of the entire sample in that credit constraints are a significant determinant of employment sensitivity. These results suggest a need to reevaluate the role that corporate debt accumulation plays in the employment fluctuations of U.S. corporations.

APPENDIX A.

Description of S & P's Senior Debt Rating Appearing in PC Compustat Database.

- Debt obligations by issuers outside of the United States and its territories are rated on the same basis as domestic corporate and municipal issues. The ratings measure the creditworthiness of the obligor but do not take into account the currency exchange or related uncertainties.
- To provide more detailed indications of credit quality, S&P may modify ratings from "AA" to "CCC" with the addition of a plus sign (+) or minus sign (-) to show relative standing within the major debt rating categories.
- Under present commercial bank regulations issued by the Comptroller of the Currency, bonds rated in the top four categories ("AAA," "AA," "A," and "BBB") are commonly known as investment grade ratings and generally are regarded as eligible for bank investment. In addition, the laws of various states governing legal investment impose certain rating or other standards for obligations eligible for investment by saving banks, trust companies, insurance companies, and fiduciaries.

Rating	Description
AAA	"AAA" indicates the highest rating assigned by Standard & Poor's. Capacity to pay interest and repay principal is extremely strong.
AA+ AA AA-	"AA" indicates a very strong capacity to pay interest and to repay principal and differs from the higher rated issuers only in small degree.
A+ A A-	"A" indicates a strong capacity to pay interest and repay principal, although it is somewhat more susceptible to adverse effects of changes in circumstances and economic conditions that debt in higher rated categories.
BBB+ BBB BBB-	"BBB" indicates an adequate capacity to pay interest payment and repay principal. Although it normally exhibits adequate protection parameters, adverse economic conditions or changing circumstances are more likely to lead to a weakened capacity to pay interest and repay principal than debt in higher rated categories.
BB+ BB BB-	"BB" indicates less near-term vulnerability to default than other speculative issues. However, it faces major ongoing uncertainties or exposure to adverse business, financial, or economic conditions which could lead to inadequate capacity to meet interest and principal payments.
B+ B B-	"B" indicates a greater vulnerability to default but currently has the capacity to meet interest payments and principal repayments. Adverse business, financial, or economic conditions will likely impair capacity or willingness to pay interest and repay principal.
CCC+ CCC CCC-	"CCC" indicates a current identifiable vulnerability to default, and is dependent upon favorable business, financial, and economic conditions to meet timely payment of interest and repayment of principal. In the event of adverse business, financial, or economic conditions, it is not likely to have the capacity to pay interest and repay principal.
CC	"CC" is typically applied to debt subordinated to senior debt that is assigned an actual or implied "CCC" rating.
C	"C" is typically applied to debt subordinated to senior debt that is assigned an actual or implied "CCC-" rating. S&P also uses the "C" rating to cover a situation where a bankruptcy petition has been filed, but debt service payments are continued.
CI	"CI" is reserved for income bonds on which no interest is being paid.
D	"D" indicates that payment is in default. S&P uses the "D" rating category when interest payments or principal payments are not made on the due date even if the applicable grace period has not expired, unless S&P believes that such payments will be made during such grace periods. S&P also uses the "D" rating upon the filing of a bankruptcy petition if debt service payments are jeopardized.

CHAPTER THREE

1. INTRODUCTION

In recent years, the popular media have puolished a series of accounts proclaiming the death of long-term job security in the United States. The overwhelming sentiment is typified in the following quote from the July 17, 1993 issue of The Economist which clearly states that the present reformation toward flexibility has endangered the "lifetime job."

> "It is true that the middle-class expectations which have encouraged and sustained a generation of aspiring workers are under attack. Many who toiled long and hard to climb a career ladder are, indeed, finding that the rungs are falling away under their feet. Working in an office, even a nice big one, is no guarantee that you will have a job next year...With extraordinary zeal, western firms have embraced the idea that the best way to cope with a fast-changing world is not only to slash jobs, but also to scrap any promise of long-term, full-time employment to surviving employees..." (The Economist, July 17, 1993 p. 13)

The implications of decreasing job security for our understanding of current and past employment relationships are many. Dramatic changes in overall employment tenure or job security may indicate underlying shifts in firm-specific human capital, employer-employee matching processes, hiring and firing costs, union presence or the increased availability of cheaper outside replacements (or "outsourcing"). A decline in worker bargaining power or adjustment costs to labor may reduce employers' incentives to maintain long-term relationships with workers in favor of more flexible worker relationships. The adverse result of diminished adjustment costs and increased outsourcing activity is a widespread reduction in long-term, high-tenured employment relationships – an issue that has drawn considerable attention in recent years.

This chapter extends Robert Hall's (1982) contentions of the importance of lifetime jobs in the U.S. economy by investigating contemporary trends in lifetime tenure.

Of particular interest is Hall's earlier finding that, in 1982, 40 percent of males over the age of 30 could expect to hold their current job for more than 20 years. This essay examines whether the stable employment relationships described in Hall (1982) can still be viewed with such unequivocal certainty in the modern labor market setting.

Specifically, I present results from panel data to investigate a significant shift in the tenure composition of jobs in the U.S. over recent years. The unique features of the PSID (Panel Study of Income Dynamics) extract – namely that it is both large and paneled – permit the inspection of a possible decline in job security by tracking individual workers across the years of the sample. By using the PSID data, I offer new, and perhaps more applicable, evidence to confirm the findings of Farber (1995) and Diebold, Neumark and Polsky (1994). I approach the tenure issue from several angles, including standard regression estimations, survival rates and distribution analyses in search of evidence of lessened job stability, particularly among highly tenured individuals. Like its predecessors, this paper cannot support the view that job stability, at the aggregate level and in any industry or occupation, diminished in the 1990s.

2. PAST RESEARCH

This project examines claims of a reduction in job tenure in light of recent evidence to the contrary by Farber (1995) and Diebold, Neumark and Polsky (1994). Before discussing these two contemporary studies, it seems appropriate to acknowledge an earlier paper by Jacoby and Sharma (1992) which approaches the topic of job duration in the United States from an historical perspective. The authors argue that current comparisons of duration reduction should account for broader contextual foundations in past tenure patterns. They incorporate data from 1880-1980 to claim that the lower tail of the tenure distribution – that is, individuals holding jobs for roughly less than one year – has decreased noticeably from the earliest decades of the twentieth century.

Jacoby and Sharma describe employee-employer relationships during the early 1900s as short-term in nature. They attribute the existence of shorter job spells to myriad factors[42] which subsided across time, to be gradually superseded by government regulations, unions and other elements contributing to a system of lengthy employment commitments. In particular, Jacoby and Sharma cite manufacturing as the industrial sector that has experienced the most substantial decline in short-term jobs; the percentage of employees possessing less than one year of tenure decreased by approximately fifty percent from 1917 to 1973. While short-term tenure probabilities in other industries during this time did not exhibit such a drastic reduction, average job duration within non-manufacturing fields, most notably in the service sector, displayed an upward trend.

In a working paper titled "Are Lifetime Jobs Disappearing?", Farber (1995) applied quantile and logit regressions using data from the Tenure Supplement of the CPS to conclude that no deterioration in the prevalence of lifetime jobs occurred between 1973-1993. Changes in long term job tenure, he found, befell two major groups: women with at least a high school education and men with less than twelve years of schooling. The former group experienced an increase in the probability of lifetime job possession[43] while Farber inferred the opposite in the latter group. Results from Farber's logit analyses indicate that, among males aged 35-64 with less than 12 years of schooling, the probability of an employment engagement spanning more than ten years has in fact exhibited a downward linear time trend. Since Farber does not include year dummies in his specifications, it is not clear whether this downward trend is hiding non-linear changes in tenure over time. For instance, it may have been the case that, within his sample, low-skilled male tenure dropped significantly from the 1970s to the 1980s and then remained relatively stable through the 1990s. The usage of year dummies rather than a time trend would have identified such temporal shifts. Nevertheless, Farber claims that

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[42] Jacoby and Sharma, "Employment Duration and Industrial Labor Mobility in the United States, 1880-1980," p.172. "That most jobs and workers were unstable at the turn of the century is expressed repeatedly in contemporary account of tramping, 'floating,' reverse migration, and high quit and dismissal rates in industrial firms...Job shopping was common at all ages, not just among young workers as today. On the employer side, work was seasonally unstable and the modal firm offered few incentives for workers to sink roots."

a decline in tenure is isolated to males with low educational attainment; the remainder of the population experienced no such decrease in the length of job duration.

Diebold, Neumark and Polsky (1994) find similar results of no secular decline in job tenure. Relying on a series of four- to ten-year retention rates, the authors disaggregate their CPS sample by age, sex, education and race and observe slight declines in the duration of blacks relative to whites as well as high school graduates relative to college graduates. Such findings of reduced job tenure experiences within large, albeit declining, sectors of the labor market support Farber's parting claim that, "Simply put, no evidence presented here supports the popular view that long-term jobs are becoming less common in the United States."

Conversely, Farber (1996) and Rose (1995) discern a sizable increase in the separation rates of high skilled workers, acknowledging the empirical finding that involuntary job separations have historically been and continue to be concentrated among low skilled workers. Farber contends that the results of his more recent paper do not contradict his 1995 piece since, in the later paper, he focuses only on separation rates by skill level rather than separation rates by both skill level and tenure. He argues that the combined findings of both papers are consistent with a "compositional effect" where higher skilled workers may be more frequently occupying mid- to low-tenure positions relative to low skilled workers. As high skilled workers spend more time earning college and graduate degrees and, therefore, out of the labor force, their tenure will (all other things equal) be lower than their less educated counterparts. The rise in separation rates among highly educated workers need not correspond to a decline in aggregate tenure.[44]

Rose, on the other hand, notes that the distribution of workers across three self-constructed categories of job duration (weak, medium, and strong) shifted away from lifetime tenure during the 1980s. Restricting his PSID sample to males, Rose finds that the percentage with strong tenure dwindled to 52% in the late 1980s, down from 67% in

[43] Most would concur that this finding is in accordance with the higher female labor participation rates witnessed over recent decades.
[44] Another possibility is that high skilled, low tenured workers may have experienced higher separation rates in the 1990s. The high skilled, high tenured workers may have exhibited no such rise in separation rates. This scenario is consistent with either an increase in outsourcing activity or a maintenance of a more flexible subgroup of a firm's employment pool.

the late 1970s. Additional examinations reveal that the wage profiles of the job losers in Rose's sample resemble those noted in the literature on displaced workers. That is, once a worker involuntarily separates from a job match and then finds a new employer, acceptance of lower wages is the norm. Rose concludes that there occurred a dramatic reduction in job stability in the 1980s – particularly within blue collar, service, and clerical occupations – resulting in masses of workers abandoning hope of preserving long-term, relatively high wage employment relationships.[45]

3. DATA

The empirical portion of this project utilizes longitudinal data from the PSID for 1968-1992. This is the only project, to date, that uses monthly tenure information available in PSID for this specific purpose. Unlike the CPS, the PSID tracks the same individuals and households across time so that the resulting panel allows researchers to trace employment histories of individual workers. The extracted sample includes observations of both males and females aged 16-65. It provides labor market information with respect to employment tenure, occupation, employment status, average hourly wage as well as the standard individual characteristics such as age, race, marital status, education, and regional residence. Agricultural workers are omitted from the sample due to the inherently sporadic and seasonal nature of occupations involving farming, fishing or forestry activities.

While the PSID sample appears to be an ideal lens through which to analyze trends in long-term employment spells, a cursory examination of the raw data reveals its

[45] Rose makes no attempt to classify people by region, age, industry or race. Nor does Rose provide logit or probit estimations calculating the effects of sex, race, industry, education and region on the probability of keeping a job from one year to the next. Furthermore, he bases his conclusions of higher separation rates on broad decade comparisons (e.g. – 1980 vs. 1990) without controlling for gdp. Since his annual data is only available through 1992, his results contrasting tenure or separation rates in the 1980s with the those of the 1990s do not account for the fact that the first three years of the 1990s displayed low, if not negative, aggregate growth rates while the majority of years in 1980s were boom years. One would expect separation rates to be higher during recessionary periods. Without controlling for gdp growth, Rose presents results that are not surprising given the differing economic climates of his comparison periods.

major flaw. The primary and quite substantial problems embedded in the PSID tenure data are the obvious inconsistencies in sequential observations recording monthly job duration. Glaring examples of this are abound, making it difficult for researchers to "correct" and transform such error-ridden data into usable, reliable and unbiased estimates of an individual's true employment tenure.[46]

Brown and Light (1992) offer a clear description of these discrepancies:

> "Researchers must rely on reported tenure to infer job changes, but measurement error in tenure responses can lead to incorrect inferences. Consider a case where reported tenure falls from 2 years to 6 months over the course of a year. In the absence of exogenous job-change information, we might infer that the respondent changed employers during the interim. However, the decline in tenure could just as well reflect coding error or reporting error – for example, the respondent might mistakenly report his time in position rather than his job tenure. Only exogenous job-change information would ensure that we correctly assign observations to jobs in such cases...Similar ambiguities exist with virtually any observed tenure sequence. If tenure jumps from 2 years to 5 years over the course of a year, or if it rises from 2 years to only 2.5 years, we might infer that no employer change has occurred. In the former case, we would assume the respondent simply forgot when his job started, or perhaps that he began rounding his responses to the nearest 5 years as his tenure increased. We might attribute the latter case, where tenure increased by too little relative to calendar time, to the respondent subtracting a period of nonwork from his tenure."

Brown and Light undertake the task of examining a series of alternative methods aimed at mitigating the measurement error. The changes they impose on the data replace "the 'raw' tenure data with imputed measures that are internally consistent." They employ several partitioning methodologies to identify errors in consecutive annual observations of employment tenure.

They also mention inconsistencies caused by interpretations of different employment tenure questions in annual PSID surveys. Two of the surveys, 1984 and 1985, ask for an estimation of employment tenure in year intervals, while the remaining

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[46] The PSID collects survey data from the heads of families. Estimates of job duration may be biased downward if those individuals who move for job-related reasons actually drop out of the survey altogether.

years request monthly responses. Out of those that require answers in monthly units, it is sometimes unclear whether the response refers to either time spent in one particular position or time spent with one particular employer. This ambiguity seems to be a direct result of the incompatible phrasing of tenure questions throughout the desired time frame. Listed below by interview year are the actual questions adopted in the PSID surveys. As one can imagine, the questions posed in the PSID are open to a multitude of potential interpretations and responses, many of which may be inconsistent with one another.

> 1968: "How long have you been working for your present employer?"
> 1969-1975: "How long have you had this job?"
> 1976-1978,
> 1981-1983: "How long have you worked for your present employer?"
> 1979-1980: "How long have you had your present position?"
> 1984-1985: "How many years altogether have you worked for your present employer?"
> 1988-1989: "How many years experience do you have altogether with your present employer?"

Any examination and subsequent corrections must first identify the nature of the response in the context of all other responses provided by the individual. The 1979-1980 version, for example, substantively differs from the "duration"-oriented questions of all other sample years. It asks the respondent to report tenure in a given job position rather than tenure with a present employer. The former type of question prompts the respondent to account for promotions, lateral movements or shifts in job responsibilities or titles; the latter, conversely, inquires about an uninterrupted employment spell with a given employer.

To identify tenure inconsistencies caused by the different questions, I verify the legitimacy of the responses by comparing each observation's alleged duration with the time elapsed between the interview date and employment start date. In instances where the durations did not agree with the reported tenure and where the respondent recorded no interim unemployment spells, the tenure figure was adjusted to the prior year's corrected tenure plus the elapsed time between interviews. I pay close attention to the (dis)congruity in consecutive responses for the years 1978-1981. In particular, I use unemployment, wage, job change, occupation and industry information to identify those responses referring to duration of a job position as opposed to duration of an employment spell. I then rely on the procedure described above to correct the reported tenure.

In other cases, sequential annual tenure observations corresponding to the same individual were obviously inconsistent, listing, for example, a 36 month employment spell in one year and 144 months of tenure in the next year. Situations such as this arose frequently and required inspection of past tenure sequences, unemployment spells, survey questions, job departure reasons, wage changes, occupation codes, industry codes, original survey and interview dates, and any other variables with which to corroborate corrected and raw tenure figures of each individual across survey years. Each misreporting instance involved a unique tenure pattern, fixable only through direct observation of the aforementioned variables. All relevant information required to complete this data inspection is available in the PSID. The extensive act of correction is then a matter of following predetermined rules, carefully considering all potentially contributory information. For a more detailed description of the correction procedure, please refer to Brown and Light (1992).

Fortunately, the PSID contains another helpful variable – I label it REASON – that permits the additional scrutiny of potential errors in the data by providing the cause for an employment change, if indeed such an event occurred. This allows me to ensure that employment stoppages, as discerned from the REASON variable, agree with changes in imputed monthly employment duration. For example, a response of an involuntary separation should correspond in the same year with an imputed tenure less than or equal to the time elapsed since the individual's last interview date.

The PSID furnishes researchers with a final way of flagging disparities in job duration data. Supplementing the query regarding monthly employment tenure is a question asking survey candidates if they have "changed employers over the past year." Ensuring that these responses are consistent with reported job duration for the same year allows for a "third strike" with which to locate and eradicate flaws in the data. The table below provides examples of the original "raw" PSID data and the corresponding corrections used in this essay's subsequent analysis.

Table 3. Example of Tenure Correction

ID	Year	"Raw" Tenure (in mos.)	Industry Code[47]	Unemployed In Year?[48]	Reason Left Job[49]	Interview Month[50]	Corrected Tenure (in mos.)
1906	1982	48	2	5	0	4	48
1906	1983	70	2	5	0	3	59
1906	1984	72	2	5	0	3	71
1906	1985	89	2	5	0	6	86
1906	1986	99	2	5	0	4	96
1906	1987	130	2	5	0	5	109
1906	1988	120	2	5	0	4	120
1906	1989	132	2	5	0	4	132
1906	1990	156	2	5	0	4	144
1906	1991	156	2	5	0	4	156
1906	1992	180	2	5	0	4	168

Note: ID represents the authors' unique coding system given to each respondent in the PSID extract. Data in all columns, with the exception of the corrected tenure figures and ID number, originate directly from the published PSID.

The above example reports an incomplete listing of the information available for a particular individual appearing in the PSID from 1982 until the last available survey year, 1992. The person numbered 1906 apparently misreported his employment duration across the survey years. This act, as I have mentioned previously, could have been due to the precise question posed to the respondent, uncertain memory, or simple rounding practices on the part of the respondent. The actual tenure recorded for the years 1986 through 1991 could not be accurate as this person 1.) was never unemployed and 2.) held one job throughout the sample period. Additionally, the elapsed time between interview dates (as reported by the survey taker) preclude the possibility that the differences in year-to-year tenure are valid. For example, the respondent retained his job between 1990 and 1991 (as verified in the fourth through seventh columns), yet he stated that his tenure had

[47] A coding of 2 refers to a broad mining classification.

[48] A response of 5 to the question of whether the subject had been unemployed at any time during the survey year means that the person was never unemployed.

[49] A response of 0 refers to the respondent having never left his job during the survey year.

[50] This number corresponds to the month during the survey year in which the subject was interviewed. An answer of 4 refers to an interview having occurred in April.

not changed from 156 months. This, I contend, was due to rounding error; the respondent most likely assumed that he had worked for his employer for roughly thirteen years.

In correcting these errors, I not only inspect the REASON variable for any changes in employment status, but I also examine changes in industry and occupation codes. The occupation codes are not always consistent across time even when a worker's tenure has been uninterrupted. The different codes may be a result of movements in job positions or titles. Once I determine that a worker stayed with the same employer, I then verify the employment duration responses by comparing the change in duration from year to year with the time elapsed between interview dates. As respondents are likely to guess and conjecture over the course of a litany of survey questions, the correction procedure favors the elapsed time between interview dates as the annual tenure change. This elapsed time is added to the corrected tenure from the previous year. In the case of person 1906, the final corrected tenure values are listed in the last column of Table 3. I believe that the entire body tenure information has been cleansed of virtually all inconsistencies. As such, I feel confident relying on this data for subsequent analysis.

The advantage of using the PSID over the CPS is the more detailed nature of job turnover and tenure information in the former dataset. Such data can be traced across the sample years for specific individuals. Any flaws in consecutive reports of tenure by individuals can be identified and subsequently corrected. The CPS supplemental data, in contrast, does not contain panel data and therefore cannot be inspected for employment tenure misreporting. The disadvantage of the PSID relative to the CPS is smaller sample size (roughly 6,000 in 1968 to 9,000 in 1992, totaling a sample of 94,000 observations).

4. DESCRIPTIVE STATISTICS

Table 4 contains the yearly means of tenure (in months) as computed using the corrected PSID extract. The averages in the first column are not restricted to certain classes of individuals, but rather span the entire sample. The last two columns contain weighted tenure means of males and females, respectively. Pairwise t-tests using the entire sample and the male-only sample reject significant movements in the annual tenure averages between 1980 and 1992. This suggests that, without controlling for compositional changes in the sample, individual traits, or aggregate economic conditions, tenure did not change appreciably through the last twelve years of the PSID sample. Not surprisingly, the mean tenure of females has actually risen (quite substantially) over the sample period – from roughly 76.3 months in 1968 to 86 months in 1992 – thereby confirming findings in Farber's 1995 paper. This increase reflects the importance of women as a growing portion of the labor force. Means were computed using the individual weights provided in the PSID.

Table 4. Mean Tenure (in Months) of Employment Spell, by Year: 1968-1992

Year	**Entire Sample**	**Males**	**Females**
1968	95.4	98.9	76.3
1969	94.5	99.0	71.8
1970	93.0	98.1	67.5
1971	92.7	98.9	65.0
1972	95.4	100.7	71.4
1973	92.9	98.3	69.9
1974	93.0	98.7	69.4
1975	93.8	99.8	70.3
1976	94.9	102.2	67.3
1977	96.2	103.4	69.1
1978	93.5	101.1	65.2
1979	90.6	100.1	58.0
1980	94.6	104.4	52.2
1981	96.1	106.6	63.0
1982	96.1	107.3	61.6
1983	99.8	109.9	69.1
1984	98.9	108.7	69.4
1985	98.9	108.8	70.8
1986	100.1	110.8	70.7
1987	100.3	108.7	76.7
1988	100.5	107.9	80.4
1989	98.7	107.5	78.7
1990	97.1	103.3	80.4
1991	100.0	105.7	84.5
1992	100.0	105.7	86.0

Tenure averages broken down by education, age, industry or occupation are graphically illustrated in Appendix A. These figures exhibit no distinct declines in the average tenures at most disaggregated levels, with the exception of : 1.) males aged 55-65, 2.) all persons with less than 12 years of schooling, and 3.) males possessing sales and clerical occupations. A possible explanation for the observed decrease in tenure among the eldest subgroup included in the sample extract is the documented rise in early retirement across the 1980s and early 1990s. As a result, the incidence of 55-65 year-olds holding "lifetime" jobs has also declined. It would be tenuous, at best, to conclude that the observed experience of the eldest set applies to all other age groups. Rather, annual tenure remained statistically constant across the sample, regardless of the subgroup analyzed. This suggests that the "decreased lifetime jobs" hypothesis should be rejected.

5. CROSS-SECTIONAL TENURE DISTRIBUTIONS

In this section, I apply Epanechnikov kernel density estimations to smooth the various sets of tenure distributions. I offer these figures as further proof of negligible changes in the incidence of lifetime employment. The tenure densities for all selected year groupings are presented without controls for education, industry or age. I include these variables in later regression analyses. The kernel estimations appear in Figures 1 and 2 of Appendix B, superimposed on top of one another to highlight the differences and similarities in the curves.

The bulk of each distribution is clustered around one or two years of tenure, as workers presumably search for "good" employer matches. Each distribution then tapers off rapidly as tenure increases. In terms of between year comparisons of the tenure distributions, the results are not so startling in light of the evidence presented in later sections of this chapter. There is a distinct lack of differentiation in the tails of each distribution. A small and statistically constant percentage of workers possessed employment spells over 10 years, regardless of whether a two or a four year window was examined. No shrinkage in the upper portions of the tenure distributions is observed in the late 1980s to early 1990s.

Given that tenure means stayed relatively stable across the year of the sample, a decline in long-term jobs would have had to coincide with an increased bunching of the tenure distribution around its mean. This hypothesis is rejected since pairwise tests indicate that the standard deviation of tenure remained statistically constant in the sample. Across the years of the sample, there has been no significant change in the proportion of individuals possessing long-term employment spells.

Figure 3 of Appendix B traces the 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentiles of the annual tenure distributions across time. Presumably, a substantial decline in the proportion of lifetime job holders would correspond to a narrowing of the right-hand tail of the tenure distribution and a drop in the 90^{th} percentile value. Figure 3 clearly contradicts the premise of heightened long-term job instability, displaying no substantial movements in the uppermost decile of the tenure distribution.

6. TENURE REGRESSIONS

In the estimations to follow, I examine trends in tenure and lifetime job holdings from several perspectives. I first present estimates from regressions which analyze the across-year and across-period changes in tenure. Tenure is used as the dependent variable on which the effects of time and individual characteristics such as gender, age, education are measured. The objective here is to identify differences in the coefficients on the time and time interaction dummies after controlling for the effects on tenure of sex, education, age, occupation and the aggregate business cycle. I consider the following specification

$$\text{TENURE} = a + b\ \text{EDUCATION} + c\ (\text{EDUCATION} \times \text{YEAR}) + d\ \text{YEAR} + f\ \text{SEX} + h\ \text{AGE} + j\ \text{REGION} + h\ \text{gdp} + k\ \text{OCCUP} + e$$

in order to identify secular changes in employment tenure. For the regressions which exclude the education interaction terms, F-tests comparing the estimated coefficients of time dummies reveal whether tenure secularly declined during specified sample periods. Similar tests are performed using the coefficients on the interaction terms (years x education) to determine if tenure changes were isolated within certain educational strata.

Table 6.1. Regression Results Using Tenure (in Years) as Dependent Variable

Explanatory Variables	A: Coeffs. (S.E.)	B: Coeffs. (S.E.)	C: Coeffs. (S.E.)	D: Coeffs. (S.E.)	E: Coeffs. (S.E.)
Education					
High School	0.177 (0.211)	0.033 (0.215)	0.073 (0.204)	-0.337 (0.309)	-0.091 (0.312)
Some College	0.001 (0.244)	-0.172 (0.247)	-0.148 (0.237)	0.022 (0.485)	0.396 (0.474)
College	-0.384 (0.254)	-0.777 (0.278)	-0.581 (0.258)	-0.608 (0.462)	-0.044 (0.423)
Male	1.747 (0.232)	1.429 (0.248)	1.724 (0.216)	1.409 (0.247)	1.736 (0.232)
Real gdp Growth Rate	1.074 (0.871)	0.967 (0.879)	0.116 (0.993)	0.663 (0.860)	0.765 (0.851)
Age					
25-34	2.114 (0.078)	2.078 (0.078)	2.065 (0.082)	2.050 (0.079)	2.085 (0.078)
35-44	5.860 (0.168)	5.833 (0.169)	5.733 (0.164)	5.781 (0.170)	5.807 (0.168)
45-54	9.178 (0.303)	9.230 (0.304)	9.108 (0.300)	9.167 (0.304)	9.115 (0.304)
55-65	11.791 (0.431)	11.963 (0.427)	11.857 (0.435)	11.911 (0.424)	11.730 (0.428)
Years					
1973-1977	0.582 (0.130)	0.580 (0.131)	0.486 (0.124)	0.732 (0.195)	0.751 (0.195)
1978-1982	0.953 (0.176)	0.956 (0.176)	0.643 (0.168)	1.238 (0.284)	1.293 (0.283)
1983-1987	1.757 (0.189)	1.756 (0.190)	1.472 (0.183)	1.749 (0.340)	1.808 (0.341)
1988-1989	1.928 (0.211)	1.925 (0.211)	1.653 (0.206)	1.247 (0.407)	1.315 (0.410)
1990-1992	1.833 (0.213)	1.846 (0.212)	1.531 (0.211)	0.509 (0.390)	0.597 (0.394)
Interaction (Educ. x Years)					
High School x 1973-1977				-0.150 (0.264)	-0.223 (0.268)
High School x 1978-1982				-0.333 (0.363)	-0.471 (0.365)
High School x 1983-1987				0.363 (0.418)	0.240 (0.421)
High School x 1988-1989				1.527 (0.504)	1.398 (0.511)
High School x 1990-1992				2.373 (0.501)	2.201 (0.508)
Some College x 1973-1977				-0.380 (0.357)	-0.474 (0.355)
Some College x 1978-1982				-0.612 (0.519)	-0.787 (0.515)
Some College x 1983-1987				-0.387 (0.567)	-0.601 (0.565)
Some College x 1988-1989				0.088 (0.649)	-0.135 (0.650)
Some College x 1990-1992				1.162 (0.648)	0.916 (0.647)
College x 1973-1977				-0.605 (0.450)	-0.710 (0.445)
College x 1978-1982				-0.543 (0.530)	-0.725 (0.524)
College x 1983-1987				-0.415 (0.564)	-0.571 (0.563)
College x 1988-1989				0.594 (0.658)	0.422 (0.662)
College x 1990-1992				1.199 (0.644)	0.955 (0.641)
Constant	-0.353 (0.329)	-0.816 (0.433)	-0.182 (0.321)	-0.777 (0.433)	-0.245 (0.357)
R-Squared	0.295	0.302	0.309	0.305	0.299
Observations	91,123	90,858	88,137	90,892	91,123

Note: Three region dummies were included in the specifications with the exception of Column D. Column B and D include six occupation dummies while the Column C includes 10 industry dummies. The observations were weighted using the PSID sampling weights.

Weighted Huber regressions using employment tenures as dependent variables do not yield statistically different coefficient estimates on the latest time period in the sample. In all regressions, the coefficient on the 1990-1992 time dummy was statistically indistinguishable from those on the preceding 1988-1989, 1983-1987 or 1978-1982 dummies. The regressions without interaction terms yield 1990-1992 coefficients that are actually statistically greater than the two earliest periods, 1968-1972 and 1973-1977. Employment duration, correcting for education, age, occupation, industry and gender, did not decline significantly from the late 1980s to the early 1990s.

Estimates in Column A of Table 6.1 additionally show that workers did not experience overall declines in their tenure from earlier to later years of the sample, regardless of education. Instead, the coefficients on successive interaction terms grew, albeit insignificantly, across the years of the sample. Neither college graduates, high school graduates nor those persons with some college exhibited significant changes in tenure between years of the sample. In sum, the estimated tenure regressions reveal no secular increase or decrease in the employment duration across the sample years.

7. COMPOSITIONAL ANALYSIS

The next step involves examining the data for changes in the composition of tenure across time. I ask if the changes, if any, in observed tenure are the result of changes in the distributions of annual tenure at the tails. Has the probability of possessing a long-term, or lifetime, job decreased over time? Along the same line of reasoning, has the incidence of short-term jobs increased within the sample? Both questions attack the issue of lifetime job security directly by examining changes in the frequency of jobs of certain lengths. For this particular estimation, I restrict the sample to those persons aged 35 years old or greater, presuming that younger persons would be unlikely to have participated in the labor force long enough to maintain 10 or more years of tenure. The results of the relevant analyses appear in Table 7.1.

Table 7.1. Logit Regression Results. Probability of 10 or More Years of Tenure

Explanatory Variables	**A: Coeffs. (S.E.)**	**B: Coeffs. (S.E.)**
Education		
High School	-0.257 (0.186)	0.010 (0.105)
Some College	0.266 (0.302)	0.037 (0.131)
College	0.078 (0.266)	-0.124 (0.124)
Male	0.646 (0.103)	0.634 (0.101)
Real gdp Growth Rate	1.485 (0.563)	1.471 (0.583)
Age		
25-34		
35-44	-1.074 (0.103)	-1.044 (0.103)
45-54	-0.429 (0.096)	-0.431 (0.096)
55-65	omitted	omitted
Years		
1973-1977	0.069 (0.089)	0.029 (0.073)
1978-1982	0.462 (0.130)	0.428 (0.104)
1983-1987	0.538 (0.140)	0.685 (0.101)
1988-1989	0.514 (0.166)	0.787 (0.109)
1990-1992	0.281 (0.168)	0.654 (0.108)
Interactions (Education x Years)		
High School x 1973-1977	-0.069 (0.184)	
High School x 1978-1982	-0.096 (0.233)	
High School x 1983-1987	0.492 (0.237)	
High School x 1988-1989	0.814 (0.266)	
High School x 1990-1992	0.872 (0.262)	
Some College x 1973-1977	-0.191 (0.270)	
Some College x 1978-1982	-0.290 (0.374)	
Some College x 1983-1987	-0.188 (0.353)	
Some College x 1988-1989	-0.100 (0.373)	
Some College x 1990-1992	0.141 (0.367)	
College x 1973-1977	-0.363 (0.318)	
College x 1978-1982	-0.411 (0.395)	
College x 1983-1987	-0.088 (0.325)	
College x 1988-1989	-0.233 (0.352)	
College x 1990-1992	0.272 (0.344)	
Constant	-0.480 (0.171)	-0.390 (0.129)
R-Squared	0.056	0.049
Observations	44,918	44,935

Note: Three region dummies were included in the Column A specification. The observations were weighted using the PSID sampling weights.

Logit regression results indicate that the incidence of 10+ years of uninterrupted employment during the initial three years of the 1990s did not change significantly from the 1988-1989 period. Nor is any change detected between the 1990-1992 period and any other time subgroup included in the analysis. These results are also consistent across education levels as comparative tests of the education x time interaction terms' coefficients do not reject equality in either of the above specifications.

8. PROBABILITY OF CONTINUED EMPLOYMENT

The second set of logit regressions presented in this chapter are in the spirit of duration models. Many duration models within the employment context are applied to continued unemployment spells and measure the probability of unemployment at time t+1, given unemployment in time t. Typically, this probability is examined not for changes across time, but rather for individual traits associated with higher or lower incidences. Conceptually, the aim and results of this section are distinct from those of the previous section insofar as the earlier logit regressions examined compositional shifts in the tenure distribution across time. They estimated temporal changes in the probability that an individual of a certain age, education level, etc. possessed an employment tenure greater than 10 years. This section, conversely, investigates shifts in the likelihood of continuing an employment spell from one year to the next, **given a certain level of tenure**. As tenure increases, how does the probability of keeping a job change? Has this probability significantly increased or decreased with time?

Here, I use the duration framework to identify changes in the probability of continued employment across time. By interacting the tenure variable with the year dummies, I am able to consider how, as tenure increases, the probability of working in year t+1 given employment in year t changes. The survival rate in this model represents the probability of keeping a job conditional on individual characteristics, aggregate and industry factors and, of greatest relevance to this analysis, tenure. I include time dummies and time interactions in the regression specifications under the supposition of a secular change in the survival rate among the individuals with the highest seniority. If the incidence of job loss among the highest-tenured individuals declines from 1988-1989 to 1990-1992, I would expect to observe a significant one-tailed difference in the relevant coefficients. In other words, the probability of losing a lifetime job would have increased from the former to latter years, supporting the hypothesis of a significant temporal evolution in security within the right-hand extreme of the tenure distribution.

The formal estimation incorporates an empirical model of the following form:

$$P(\text{Work for Employer Z at time t+1} \mid \text{Worked for Employer Z at time t}) =$$
$$a + b\,\text{TENURE} + c\,\text{YEAR} + d\,(\text{YEAR} \times \text{TENURE}) + f\,X + h\,\text{gdp} + e$$

where X is a vector of individual characteristics including education level, gender, region of residence, industry and occupation. In some cases, tenure-squared (tenure x tenure) is added as an explanatory variable to account for the possibility that the survival rate displays concave features, increasing decreasingly with tenure.

One should recognize that the ability to measure such an equation is principally due to the unique nature of the PSID. As opposed to larger datasets such as the CPS, the panel feature of the PSID permits researchers to incorporate employment histories (and futures) of individual workers into analyses. In this instance, knowledge of time t+1 events at time t allows for an examination of job security across time. I am therefore able to tackle the question: For workers with x years of tenure, has the probability of working one additional year changed over time? In other words, have long-term jobs become less secure? This, I contend, is pivotal inquiry in the lifetime job literature that cannot be approached using a successive series of cross-sections like those available in the CPS or Census. With the CPS, all data is "right-censored." Researchers have no information about the full length of an employment spell; they only know, up to the survey date, the job duration of each respondent. The PSID, conversely, possesses data on both the starting **and ending** date of each individual's job spells. The closest counterpart to this section's questions that is answerable with CPS-like data is whether the incidence of long-term employment spells has declined with time.

Table 8.1 contains the results of these logit regressions.[51] Table 8.2 records regressions fashioned in a similar spirit to those reported in Table 8.1 with a slight difference in the utilized tenure measure. Unlike the preceding regressions which include tenure and tenure-squared as continuous variables, the regressions of Table 8.2 use dummy variables for intervals of tenure (i.e. - 1 to 4 years, 5 to 9 years, 10 to 19 years, and 20 or more years). The objective is to examine the progression of or change in the stated probability of continued employment within certain tenured-groups over the sample period.

[51] The last year that each individual appeared in the sample extract was excluded from the estimation because there was no information concerning continuing employment. As such, the analysis omitted all observations from the year 1992, the last year of the sample.

Table 8.1. Logit Results. Dependent Variable: Probability of Continued Employment

Explanatory Variables	A: Coeffs. (S.E.)	B: Coeffs. (S.E.)	C: Coeffs. (S.E.)	D: Coeffs. (S.E.)	E: Coeffs. (S.E.)
Education					
High School	0.210 (0.046)		0.207 (0.044)	0.196 (0.045)	0.177 (0.044)
Some College	0.123 (0.060)		0.168 (0.057)	0.116 (0.058)	0.117 (0.060)
College	0.188 (0.070)		0.277 (0.066)	0.195 (0.068)	0.192 (0.079)
Years					
1973-1977	0.031 (0.088)	0. 049 (0.087)	0.058 (0.068)	0.037 (0.072)	0.049 (0.069)
1978-1982	-0.287 (0.122)	-0.260 (0.122)	-0.149 (0.075)	-0.227 (0.100)	-0.168 (0.072)
1983-1987	-0.128 (0.110)	-0.097 (0.110)	-0.090 (0.071)	-0.097 (0.101)	-0.073 (0.071)
1988-1989	-0.178 (0.129)	-0.150 (0.129)	-0.162 (0.092)	-0.183 (0.116)	-0.148 (0.092)
1990-1991	0.220 (0.178)	0.250 (0.178)	0.286 (0.120)	0.250 (0.140)	0.282 (0.119)
Tenure	0.147 (0.013)	0.145 (0.013)	0.319 (0.013)	0.311 (0.013)	0.317 (0.013)
Tenure-Squared			-0.010 (4E-4)	-0.009 (4E-4)	-0.009 (4E-4)
Interaction (Tenure x Years)					
Tenure x 1973-1977	0.030 (0.021)	0.031 (0.021)	0.024 (0.012)	0.019 (0.012)	0.026 (0.013)
Tenure x 1978-1982	0.098 (0.023)	0.099 (0.023)	0.062 (0.014)	0.058 (0.014)	0.061 (0.014)
Tenure x 1983-1987	0.071 (0.019)	0.074 (0.019)	0.059 (0.012)	0.051 (0.012)	0.058 (0.012)
Tenure x 1988-1989	0.048 (0.024)	0.052 (0.023)	0.050 (0.015)	0.044 (0.015)	0.049 (0.015)
Tenure x 1990-1991	0.093 (0.043)	0.097 (0.043)	0.074 (0.025)	0.067 (0.025)	0.072 (0.025)
Constant	0.036 (0.098)	0.172 (0.093)	-0.222 (0.091)	-0.187 (0.089)	-0.193 (0.250)
Log Likelihood	-32,063.652	-32,104.284	-32,856.119	-31,530.419	-32,634.187
Pseudo R-squared	0.146	0.145	0.155	0.160	0.157
Observations	76,371	76,371	79,114	76,371	78,878

Note: Real gdp as well as ten industry, one sex, four age and three region dummies were also included in the Column A, B and D specifications. Column C's specification includes only region dummies. Column E's specification includes six occupation dummies as well as three region dummies.

Table 8.2. Logit Results. Dependent Variable: Probability of Continued Employment

Explanatory Variables	A: Coefficients (S.E.)	B: Coefficients (S.E.)
Years		
1973-1977	0.103 (0.065)	0. 101 (0.065)
1978-1982	-0.124 (0.072)	-0.144 (0.068)
1983-1987	-0.027 (0.070)	-0.022 (0.070)
1988-1989	-0.116 (0.090)	-0.117 (0.090)
1990-1991	0.355 (0.110)	0.334 (0.108)
Tenure: 5 to 9 years	0.975 (0.126)	0.976 (0.125)
Tenure: 10 to 19 years	1.353 (0.162)	1.357 (0.162)
Tenure: 20 years or more	2.263 (0.323)	2.254 (0.323)
Interaction (Tenure x Years)		
Tenure: 5 to 9 x 1973-1977	0.143 (0.160)	0.138 (0.160)
Tenure: 5 to 9 x 1978-1982	0.527 (0.161)	0.522 (0.161)
Tenure: 5 to 9 x 1983-1987	0.380 (0.157)	0.376 (0.157)
Tenure: 5 to 9 x 1988-1989	0.202 (0.196)	0.202 (0.196)
Tenure: 5 to 9 x 1990-1991	0.680 (0.234)	0.674 (0.234)
Tenure: 10 to 19 x 1973-1977	0.216 (0.238)	0.217 (0.238)
Tenure: 10 to 19 x 1978-1982	0.870 (0.224)	0.864 (0.224)
Tenure: 10 to 19 x 1983-1987	0.688 (0.207)	0.707 (0.207)
Tenure: 10 to 19 x 1988-1989	0.659 (0.258)	0.655 (0.259)
Tenure: 10 to 19 x 1990-1991	0.854 (0.342)	0.850 (0.342)
Tenure: 20+ x 1973-1977	-0.310 (0.405)	-0.299 (0.405)
Tenure: 20+ x 1978-1982	-0.269 (0.429)	-0.258 (0.429)
Tenure: 20+ x 1983-1987	-0.012 (0.468)	-0.003 (0.468)
Tenure: 20+ x 1988-1989	0.585 (0.771)	0.595 (0.771)
Tenure: 20+ x 1990-1991	0.064 (0.838)	0.070 (0.837)
Constant	0.133 (0.077)	0.228 (0.090)
Log Likelihood	-34,154.095	-34,128.875
Pseudo R-squared	0.122	0.122
Observations	79,122	79,144

Note: Real gdp as well as three education, one sex and four age dummies were also included in the Column A and B specifications. Column B includes three region dummies, as well.

Not surprisingly, when tenure is measured as a continuous variable, an increase in tenure is associated with a significant rise in the probability of continued employment. This finding supports the theory of Jovanovic (1982) who constructs a model of job separations in which the quality of an employee-employer match is revealed within a relatively short window period after hiring. During this period, the likelihood of separation is highest as the worker launches a sort of comparative job shopping campaign. The passage of time permits the flow of more information regarding the current job match with which the worker can compare outside options. However, the outside options must represent better opportunities as tenure increases since, presumably, productivity and search costs also increase with tenure. Consequently, the lowest survival rates will be concentrated in the inaugural months and years of a job match. Bad worker-employer matches separate early along in the tenure timeline as workers search for better fits and employers fire those workers whose ability may not suit their production needs. Higher tenure, therefore, is associated with lower separation probabilities.[52]

Across time, however, statistically insignificant changes occur in the relationship between continuing employment and tenure. Pairwise comparisons of the tenure x time interaction terms' coefficient each reveal chi-squared statistics too low to allow for a rejection of the null hypotheses of equality. The likelihood of separation did not increase significantly with both tenure and time. Note that these coefficient values reflect the secular trends in the probability of continued employment after controlling for the aggregate business cycle as well as a range of individual characteristics.

This finding is further substantiated by the results recorded in Table 8.2. Of particular relevance to this essay is the temporal evolution in the relationship between high tenure and the likelihood of continuing employment. T-tests reveal that, for the two highest tenure classifications (10 to 19 years and 20 or more years of tenure), the probabilities do not change significantly over the time period in question. Such tests compared the coefficients of the 1990-1991 interaction term with coefficients from each of the preceding years' interaction terms. The null hypotheses could not be rejected in any of the comparison tests. I conclude from these rather convincing findings that no such secular decreases in the incidence of continued employment appear across the sample years.

Appendix C contains graphical representations of the predicted probability outcomes yielded by regressions similar to those presented in Table 8.2. These illustrations trace the predicted probability of continued employment for selected tenure

[52] Ruhm (1987) details alternative reasons for the positive correlation between the survival rate and tenure.

categories (0-4 years, 5-9 years, 10-19 years, and 20 or more years of tenure) across the sample time period. More detailed predictions by age and by education level are presented in later figures within the appendix. The most striking feature of each of these graphs is the lack of predicted downward movement in the employment probabilities through the 1980s and 1990s. The models predict, conversely, that in many of the education and age classifications, the probability of continued employment actually rose between the two decades; this pattern is most visible in the groups with 0-4 and 5-9 years of tenure. Among the highest tenured group, which was comprised of persons with 20 or more years of uninterrupted tenure, the predictions stayed seemingly constant throughout the entirety of the time period. This suggests that long-term job security has also remained stable. These results clearly refute the perception that job stability, at any level of tenure, has declined significantly over time. The actual experience among selected groups of workers has been quite the opposite.

9. CONCLUSION

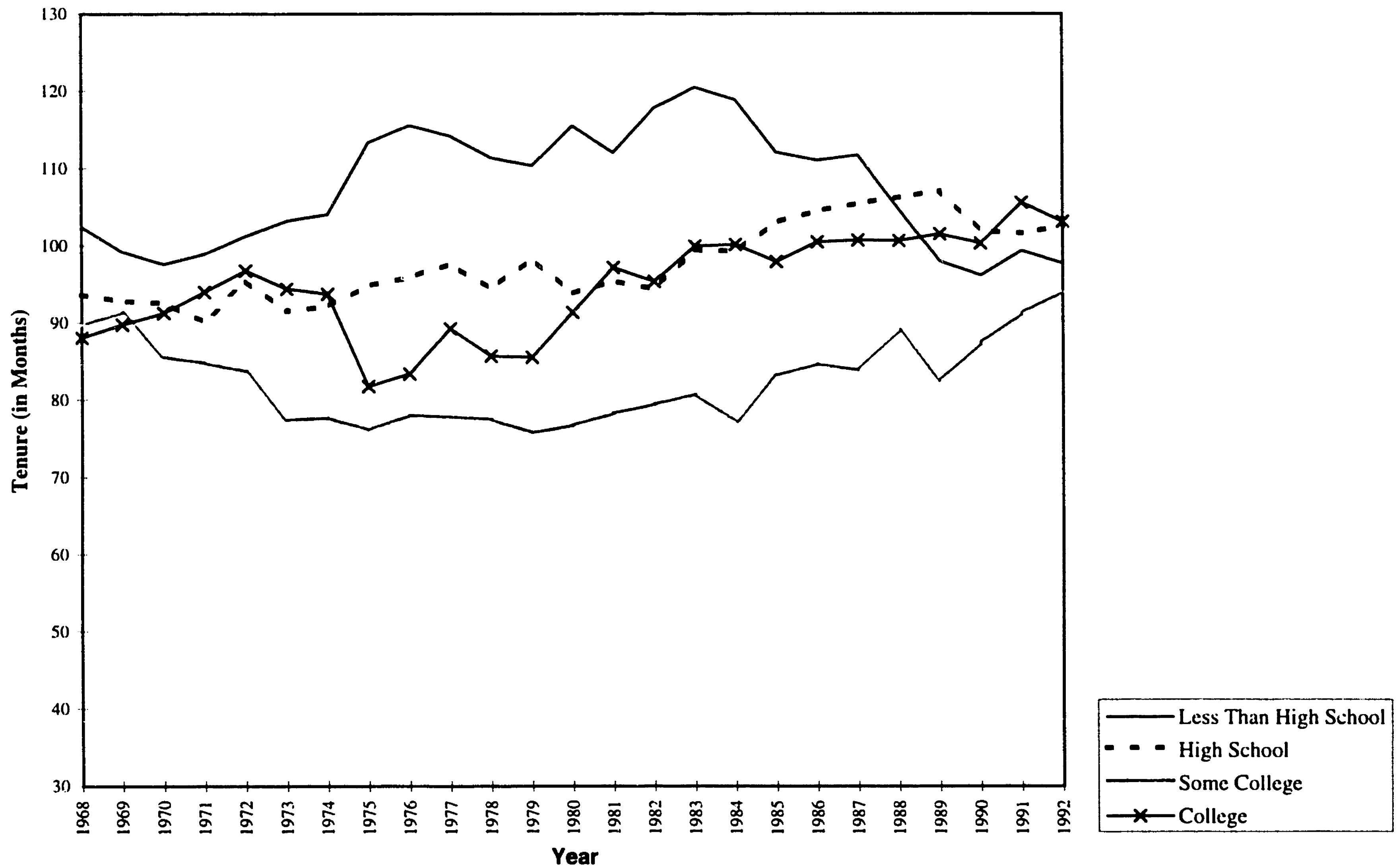
Dissolved job matches between low skilled workers and their employers continue to represent the bulk of turnover in the U.S. labor market. Nevertheless, much recent attention has focused on the share of job losers, both of blue collar and white collar status, whose high-tenured jobs were perceived as stable and long-lasting. The results of this essay reject diminished job security claims. This chapter offers several findings which challenge the emergence of a structural change away from institutional reliances on long-term, or lifetime, employment relationships. Aggregate figures of employment tenure from the PSID reveal no significant shifts, positive or negative, in seniority patterns. Fluctuations in average tenure have been minimal, if not negligible.

The most conclusive evidence is the stability of the time dummy coefficients found in the collective regressions, be they OLS or logit specifications. Equality tests of the time coefficients reject the hypothesis of a change in either aggregate tenure levels or the proportion of workers with lifetime employment. Distribution tests confirm this result, rejecting the possibility that highly-tenured workers have comprised less and less of successive annual tenure distributions.

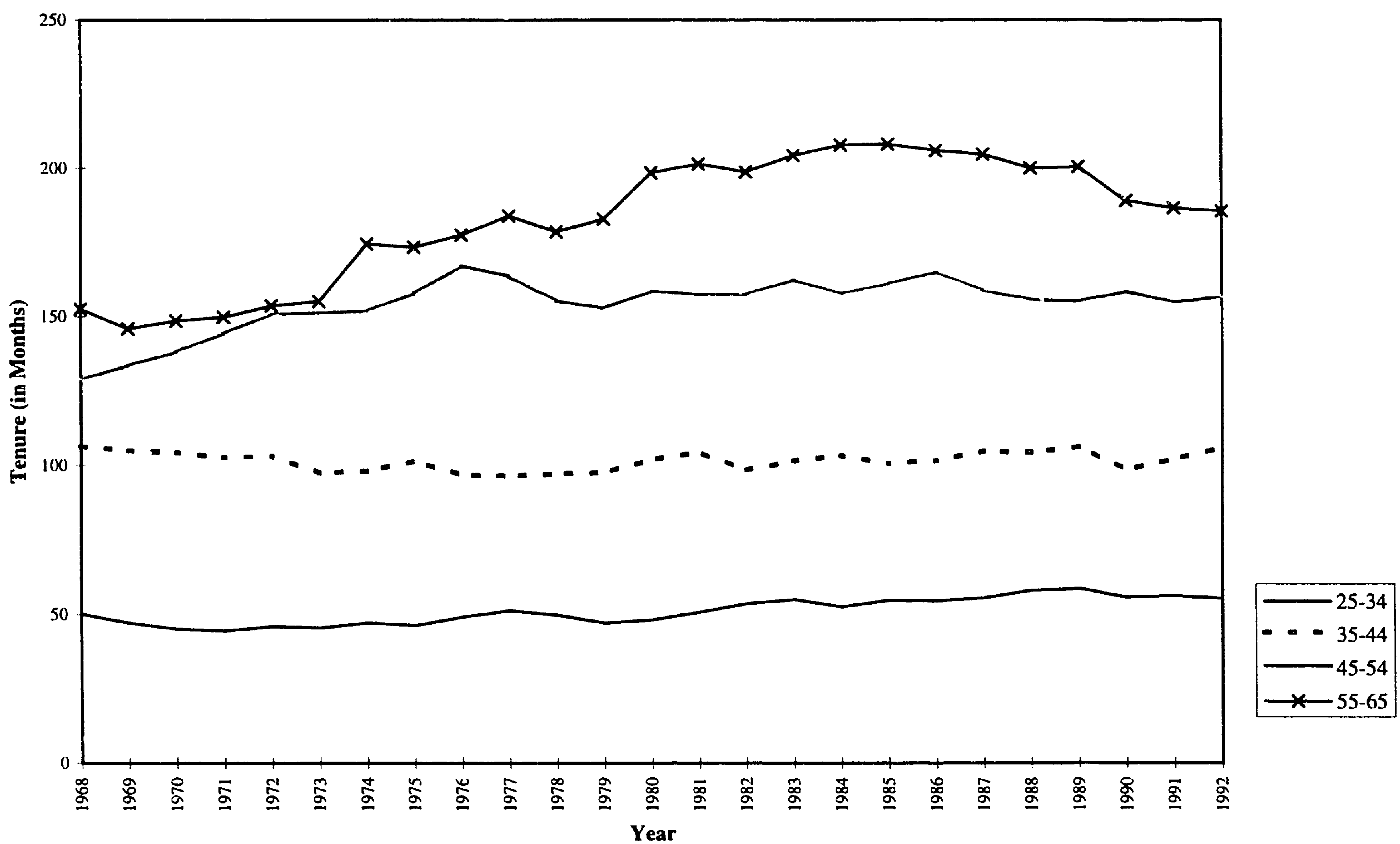
Additionally, I have exploited the panel properties of the PSID by measuring the job retention probabilities of workers over time. In doing so, this essay contributes a new dimension to the current tenure literature. It finds that the probability of continued employment has remained statistically stable throughout **most** of the sample, irrespective of reported tenure levels. Counter to popular perception, this essay reveals that the probability of keeping a job from one year to the next **rose** slightly from the 1980s to the 1990s.

As more recent PSID data becomes available, particularly for 1993 and 1994, recent conjecture of pervasive movements away from long-term employee-employer attachments may seem more or less accurate. Nevertheless, evidence presented in the essay using data through 1992 can in no way substantiate such conjecture. It suggests instead that the aggregate tenure experience of American workers stayed relatively constant across the twenty-five years examined.

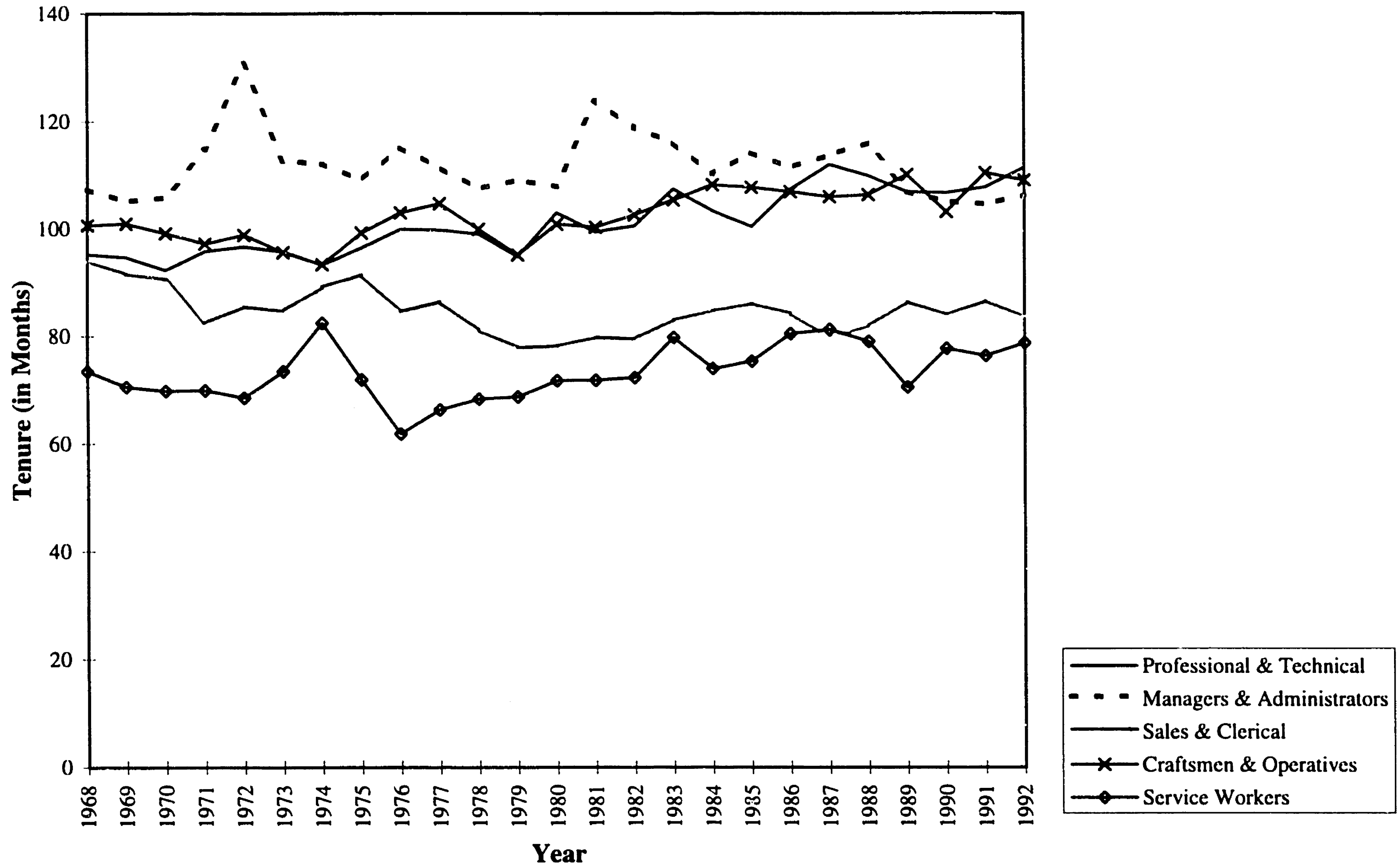
Appendix A. Figure 1.
Annual Weighted Average Tenure (in Months), by Education



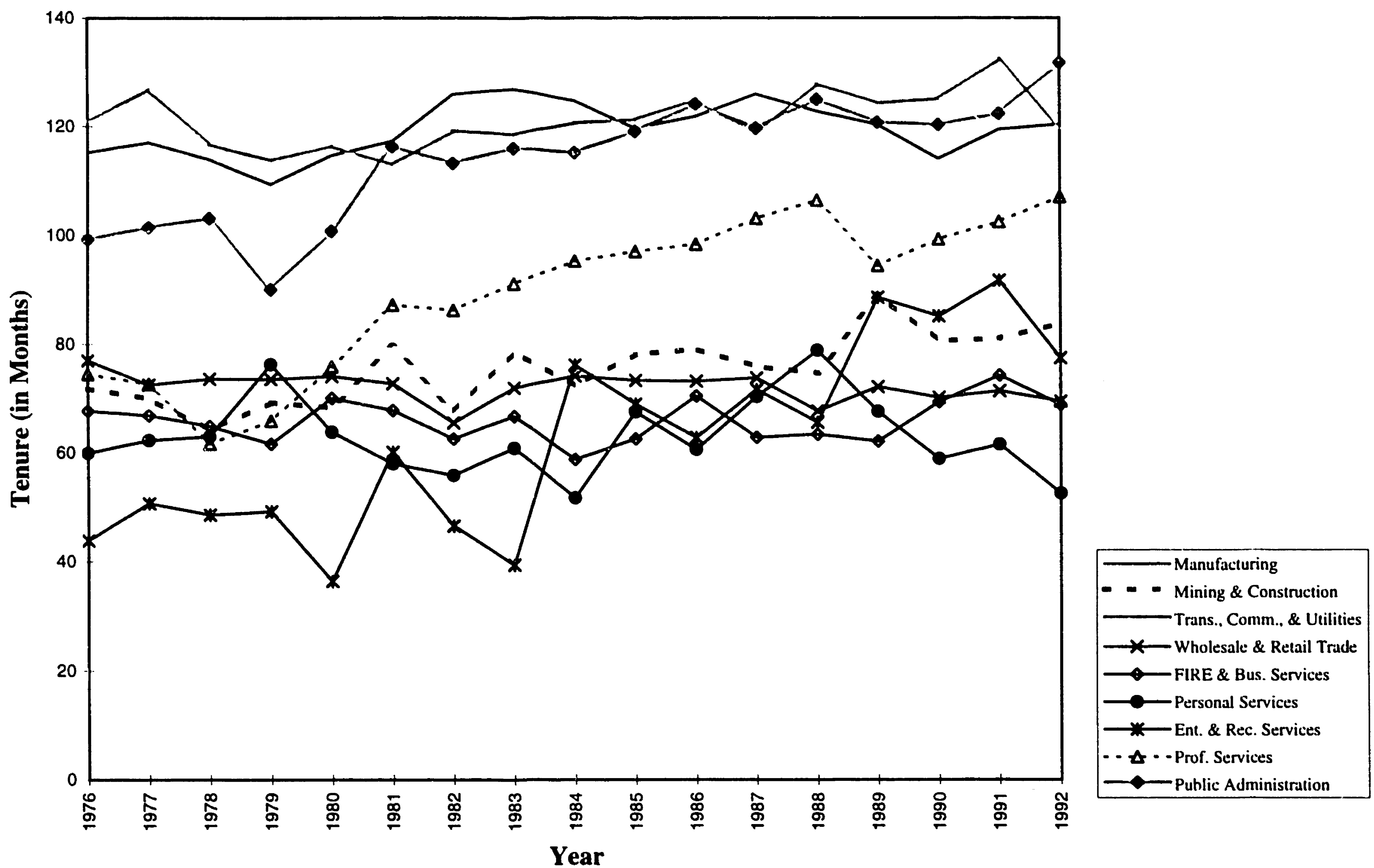
Appendix A. Figure 2.
Annual Weighted Average Tenure (in Months), by Age Group



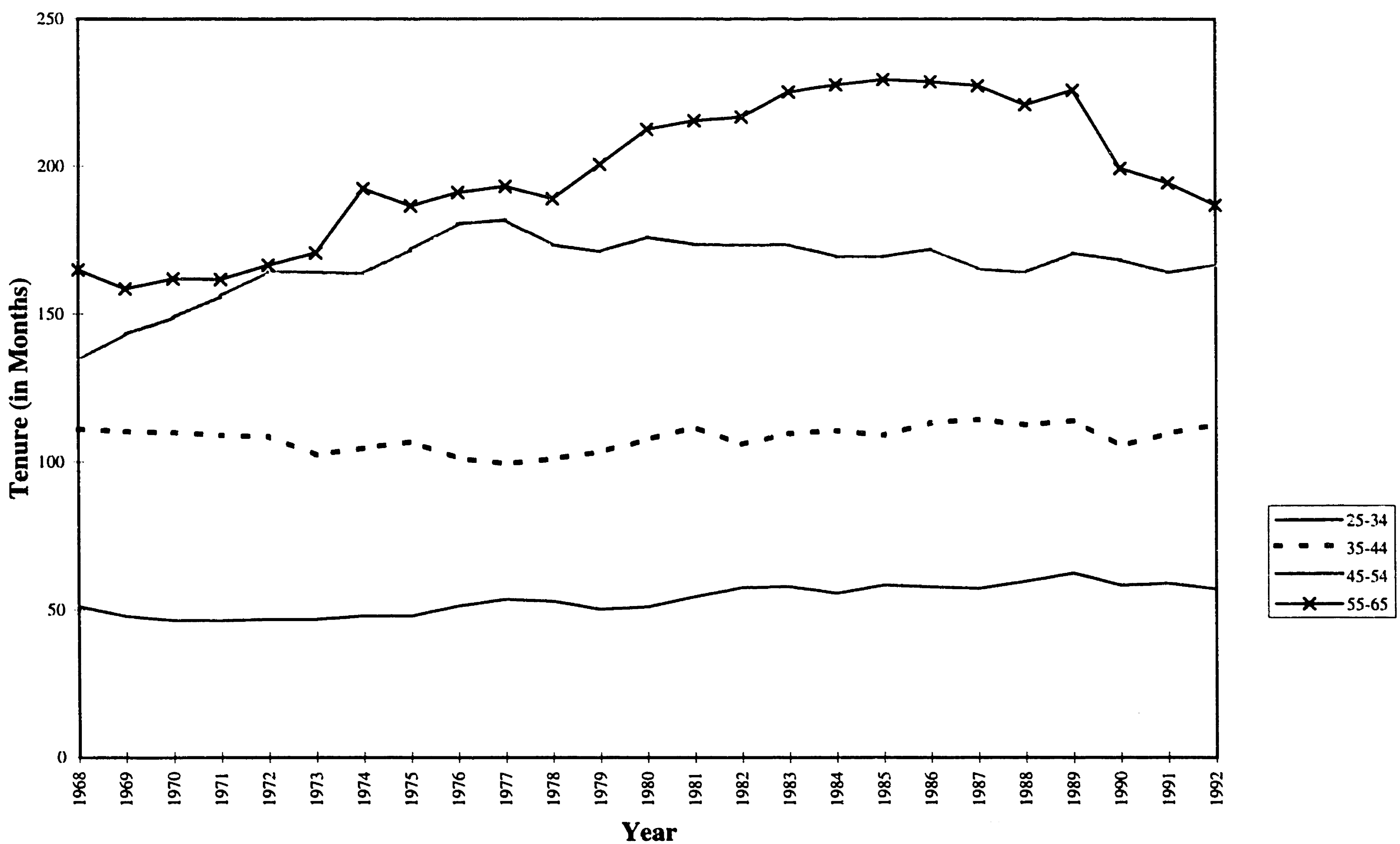
Appendix A. Figure 3.
Annual Weighted Average Tenure (in Months), by Occupation



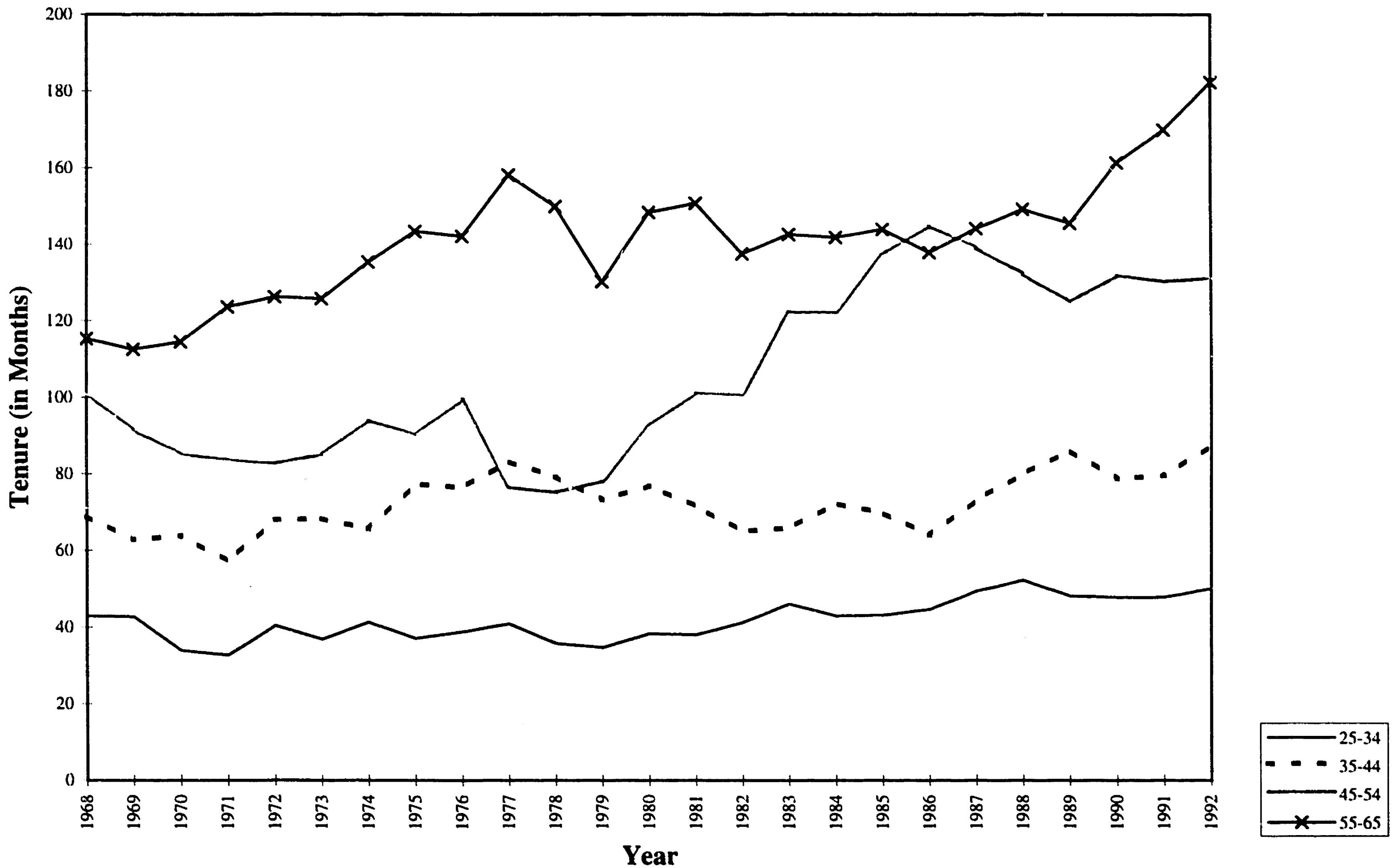
Appendix A. Figure 4.
Annual Weighted Average Tenure (in Months), by Industry



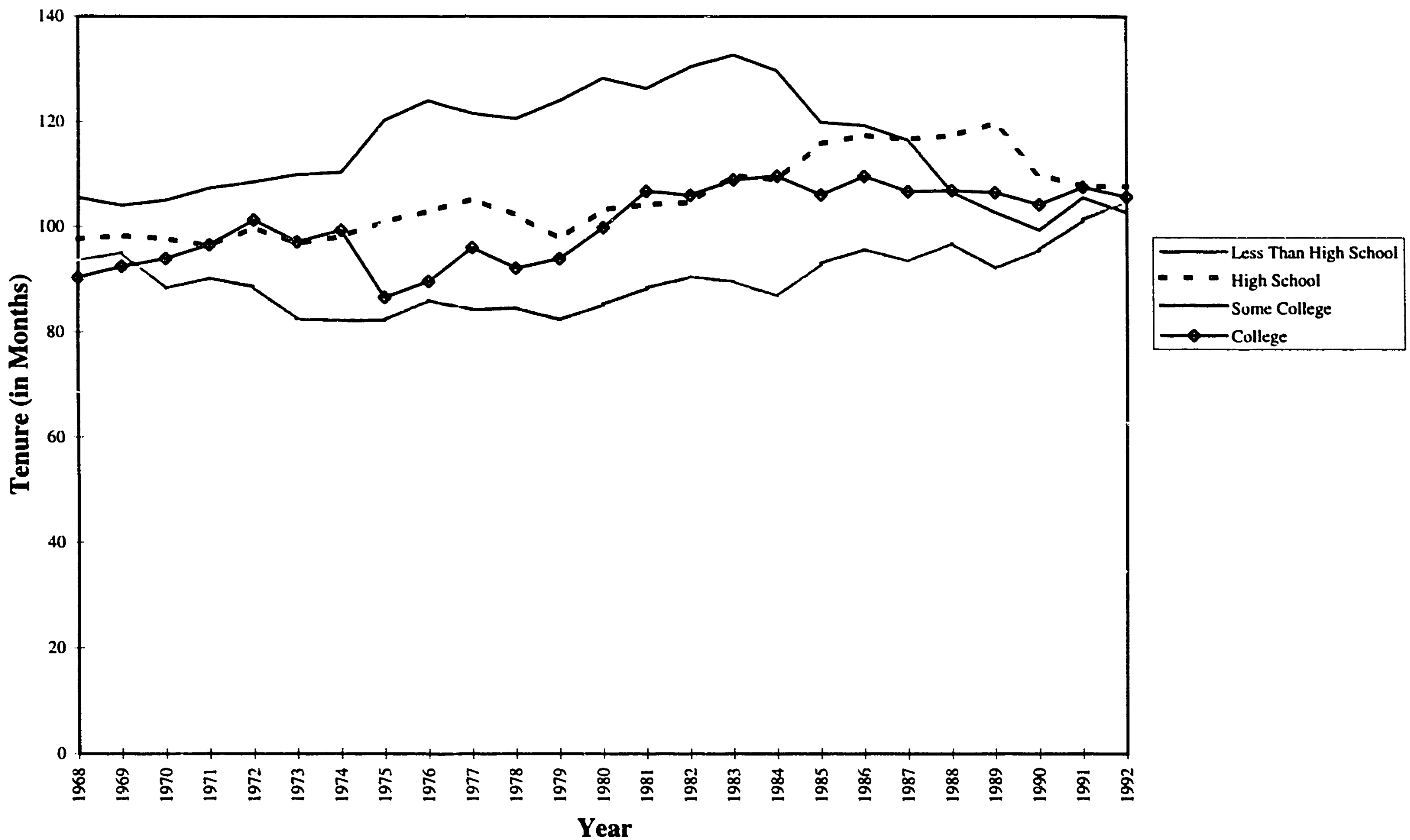
Appendix A. Figure 5.
Annual Weighted Average Tenure (in Months), by Age Group: Males



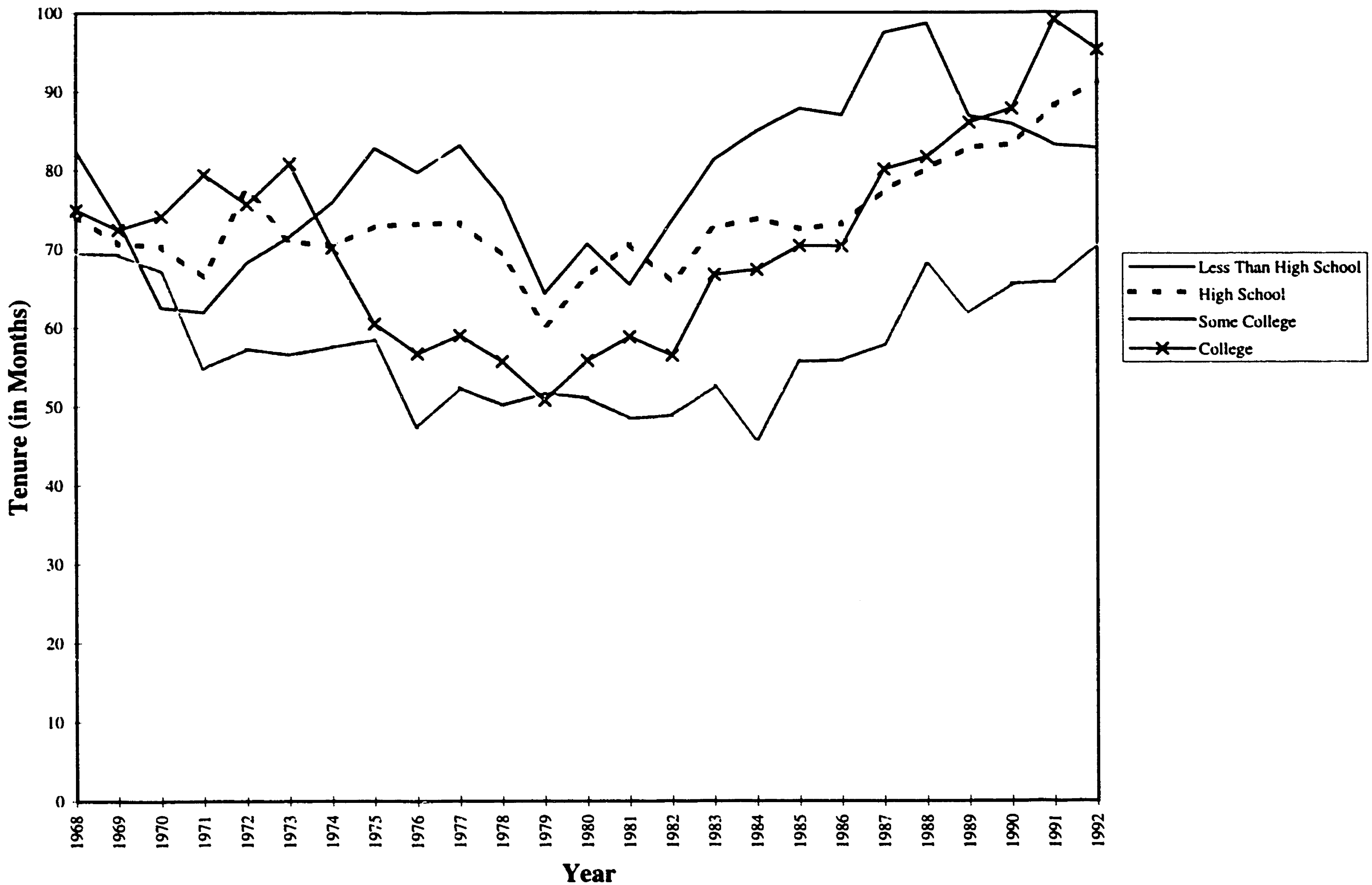
Appendix A. Figure 6.
Annual Weighted Average Tenure (in Months), by Age Group: Females



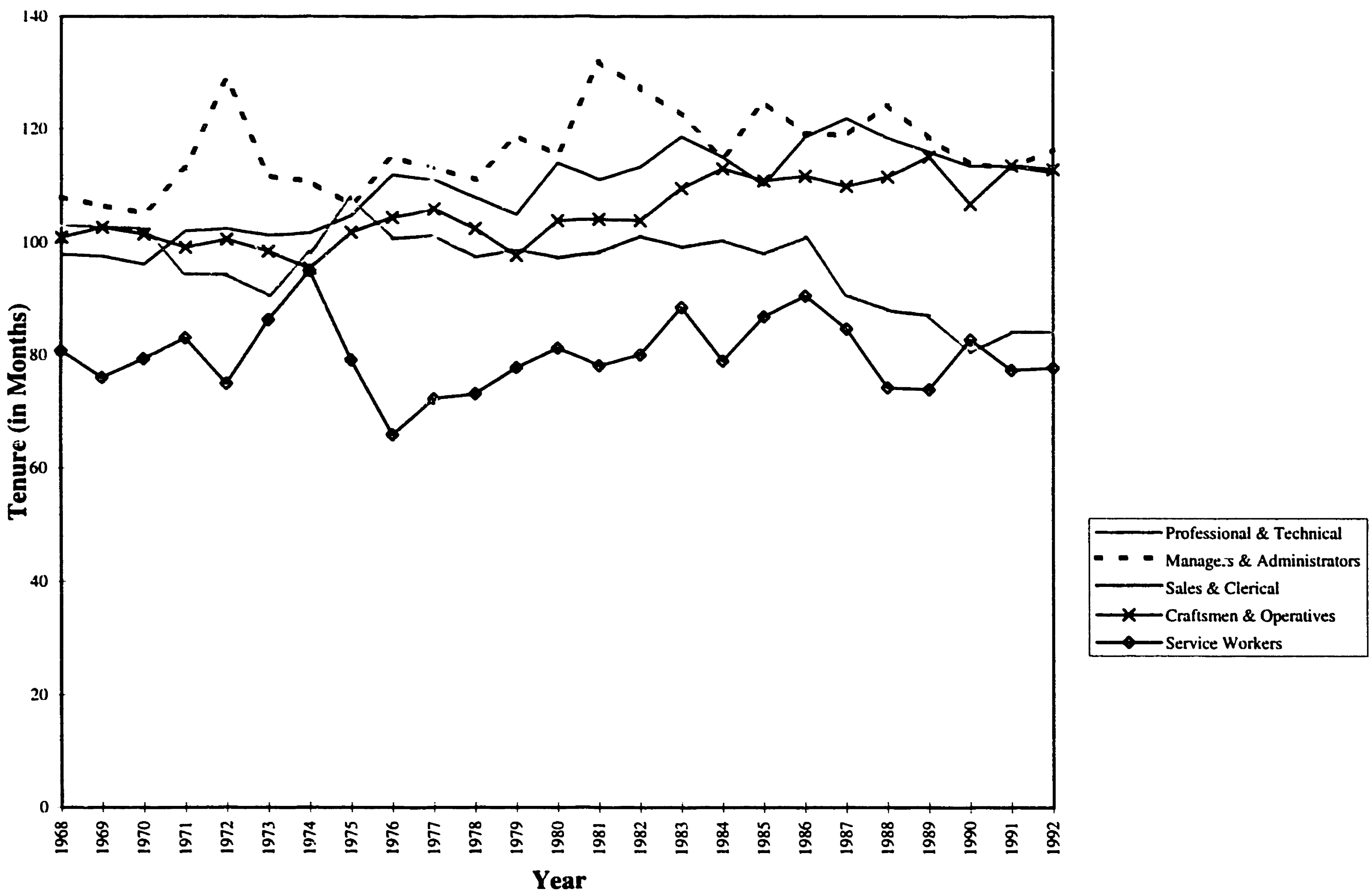
Appendix A. Figure 7.
Annual Weighted Average Tenure (in Months), by Education: Males



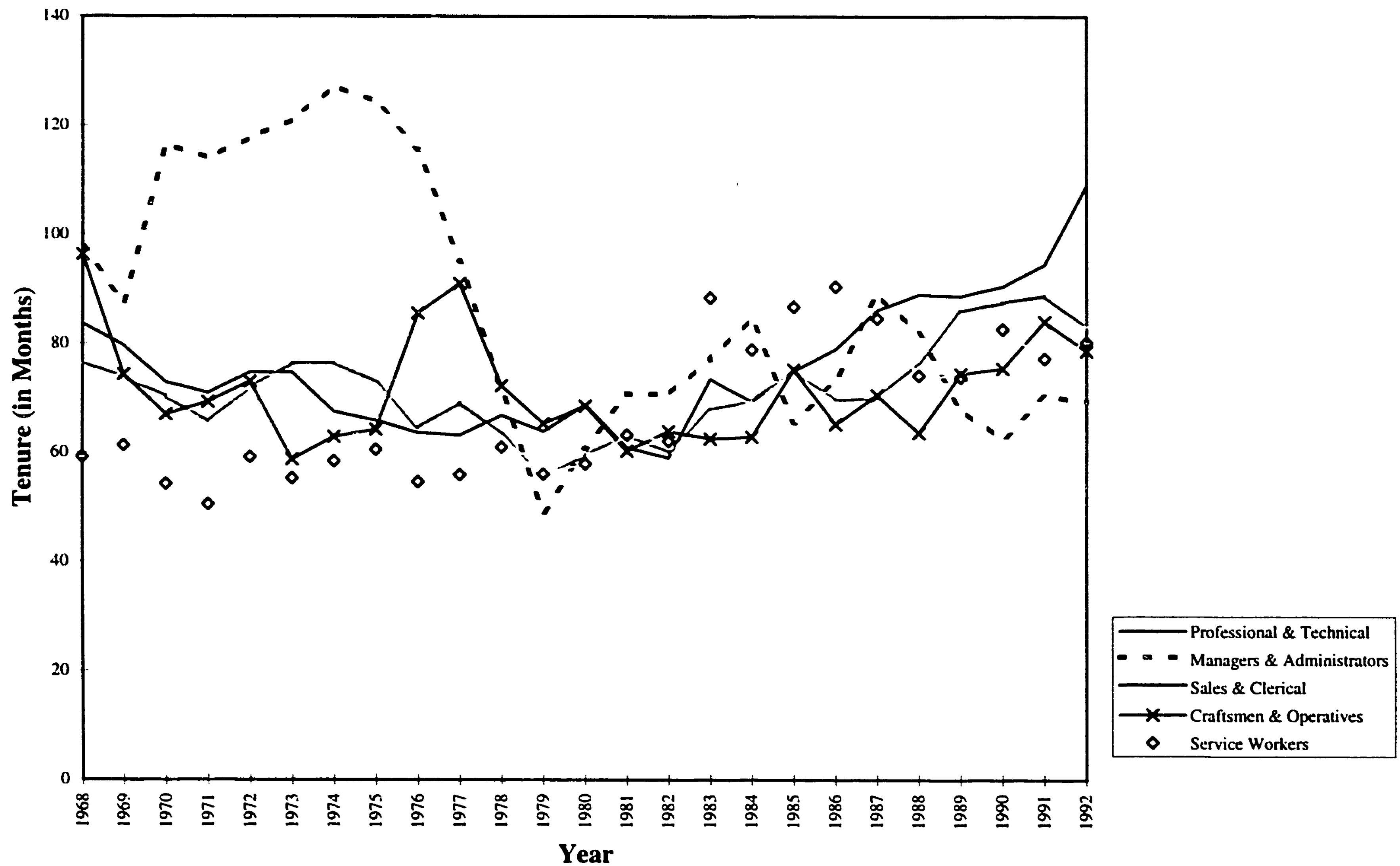
Appendix A. Figure 8.
Annual Weighted Average Tenure (in Months), by Education: Females



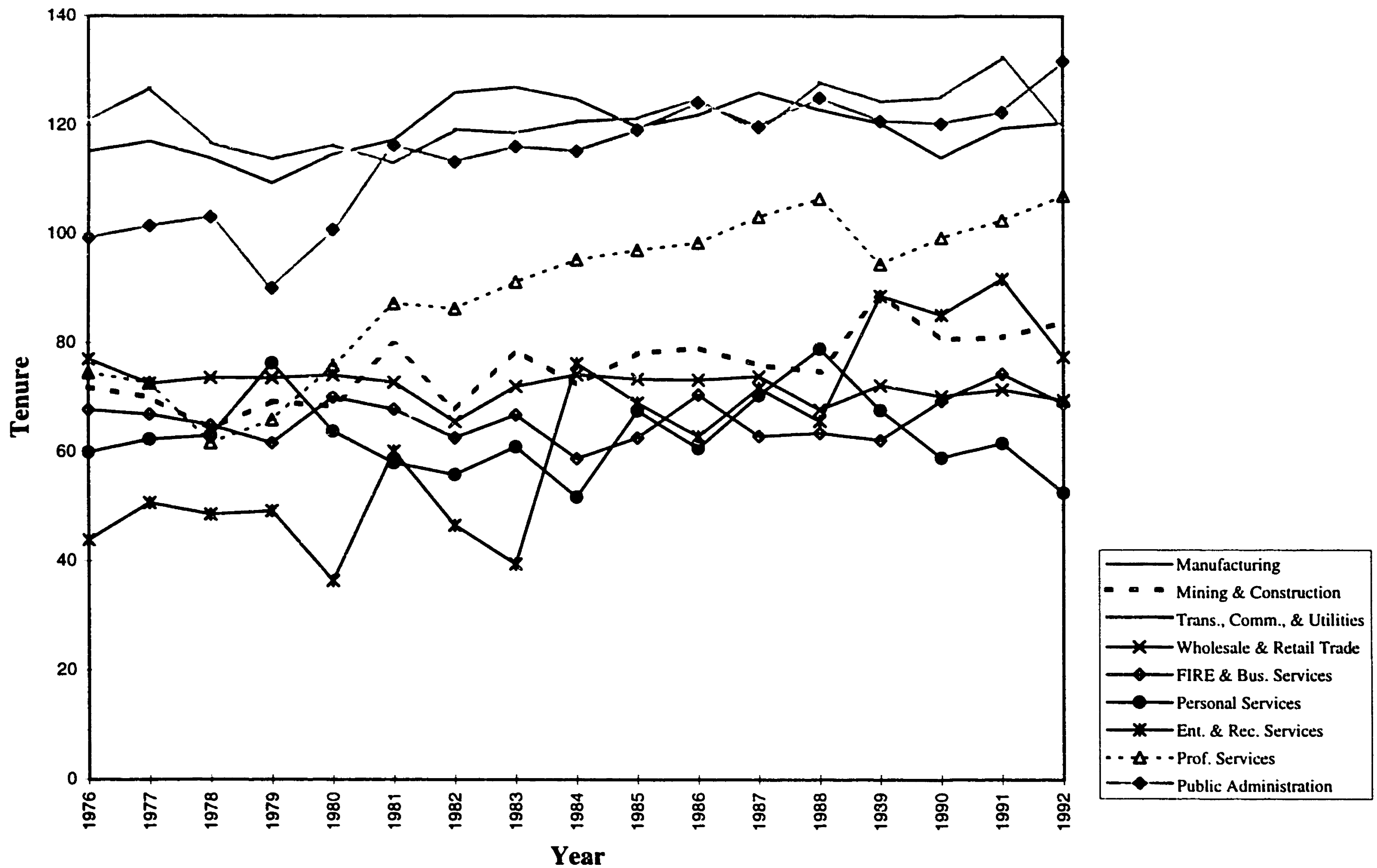
Appendix A. Figure 9.
Annual Weighted Average Tenure (in Months), by Occupation: Males



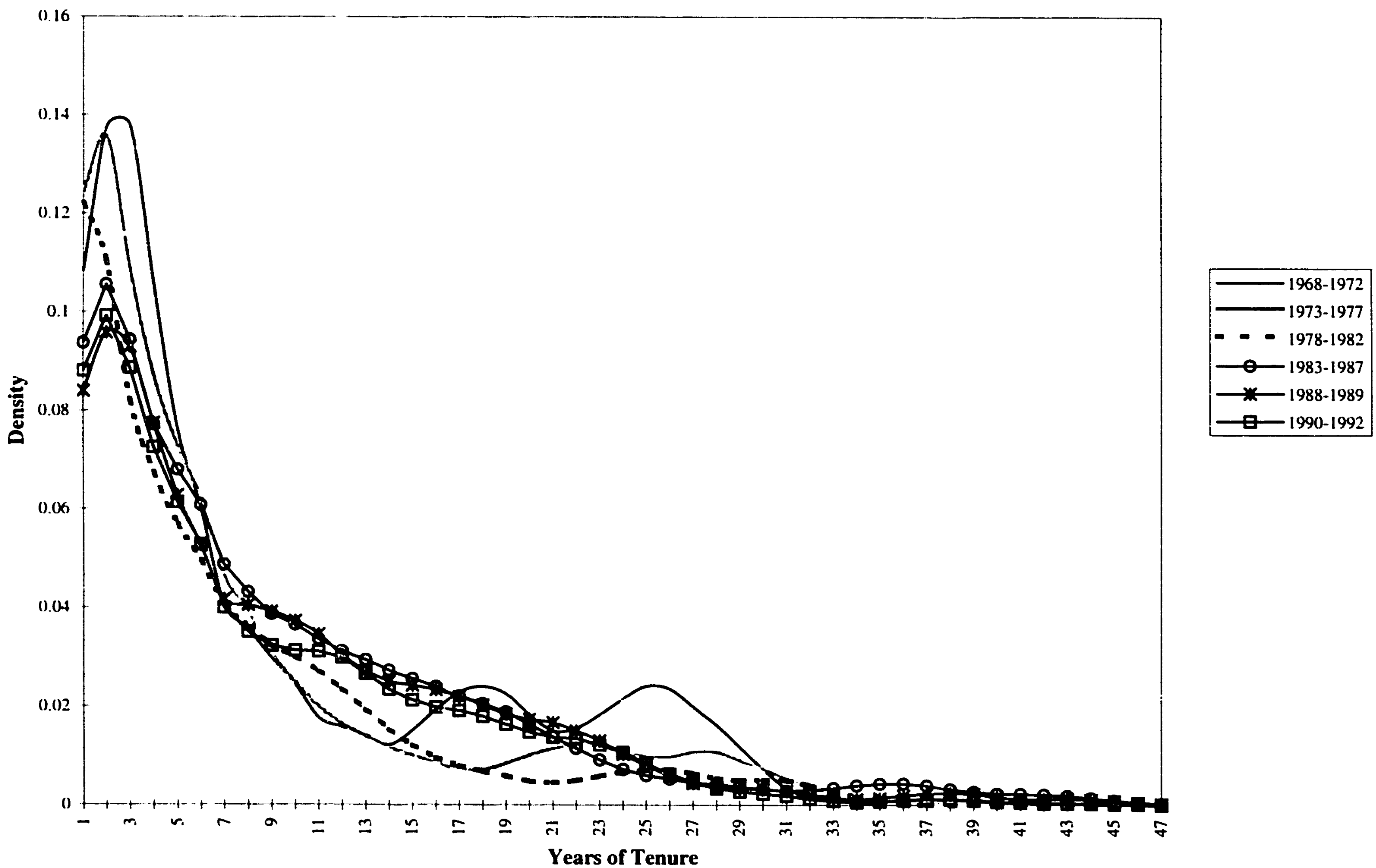
Appendix A. Figure 10.
Annual Weighted Average Tenure (in Months), by Occupation: Females



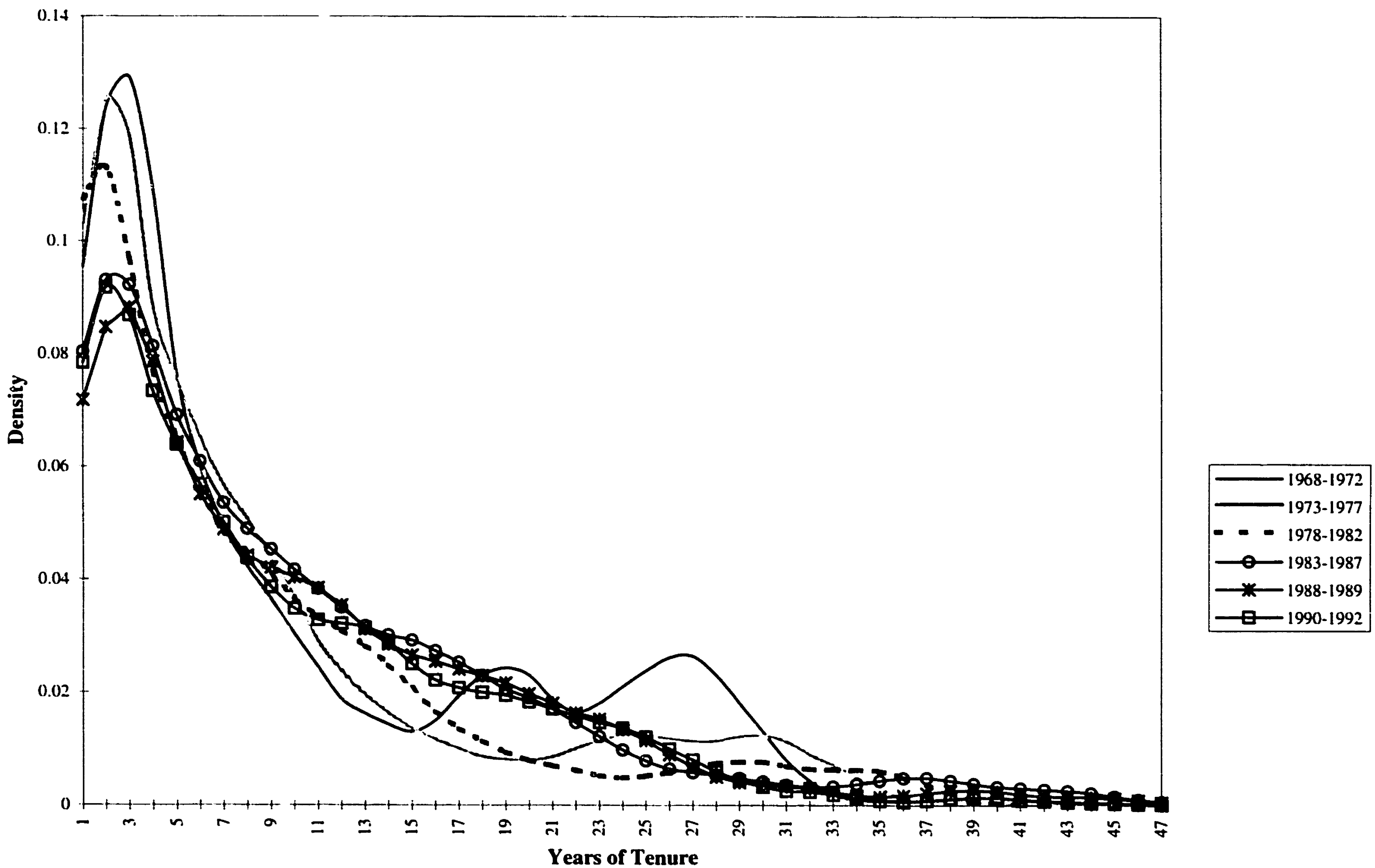
Appendix A. Figure 11.
Annual Weighted Average Tenure (in Months), by Industry: Males



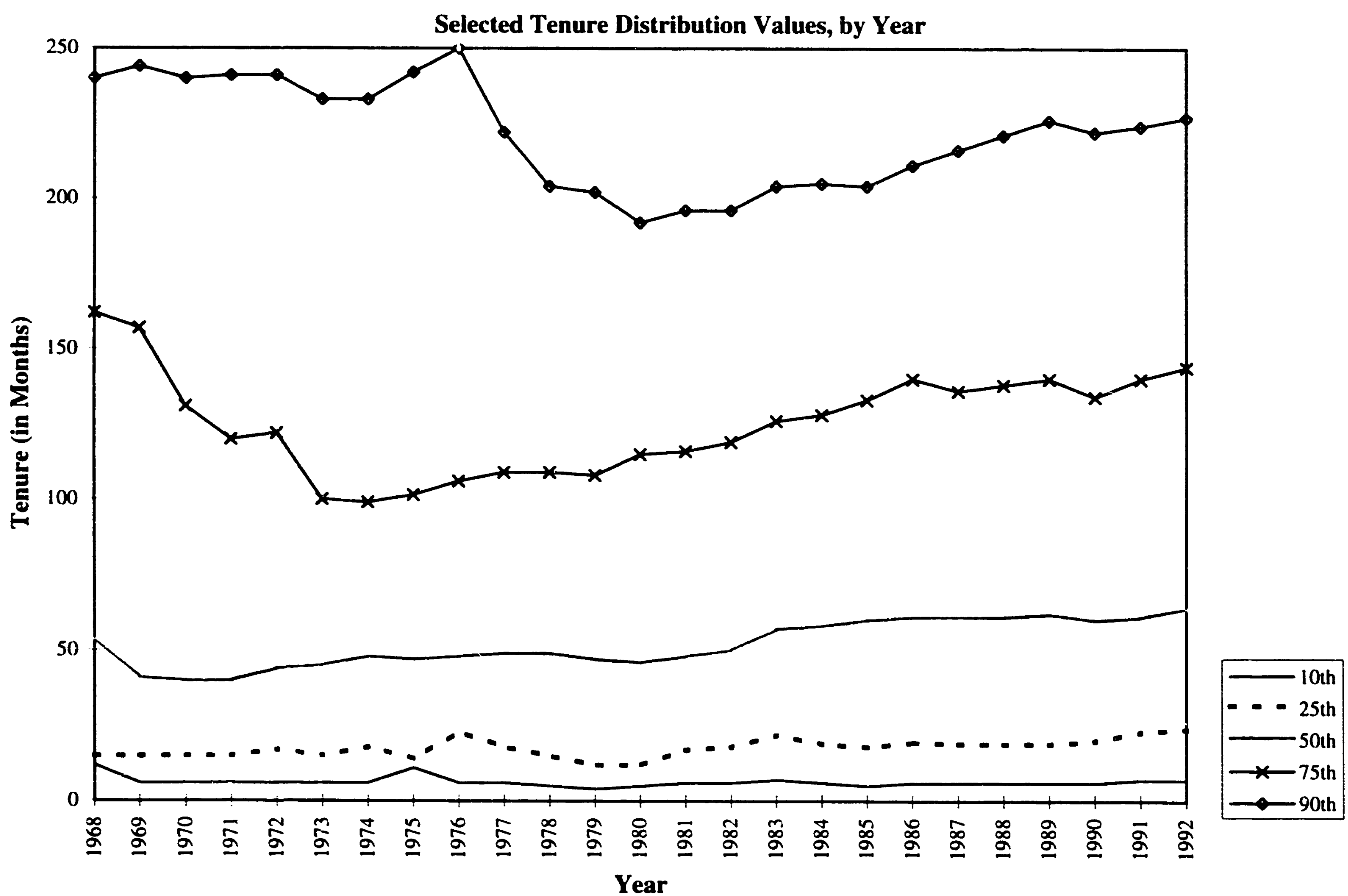
Appendix B. Figure 1.
Kernel Density of Tenure by Years: 1968-1992



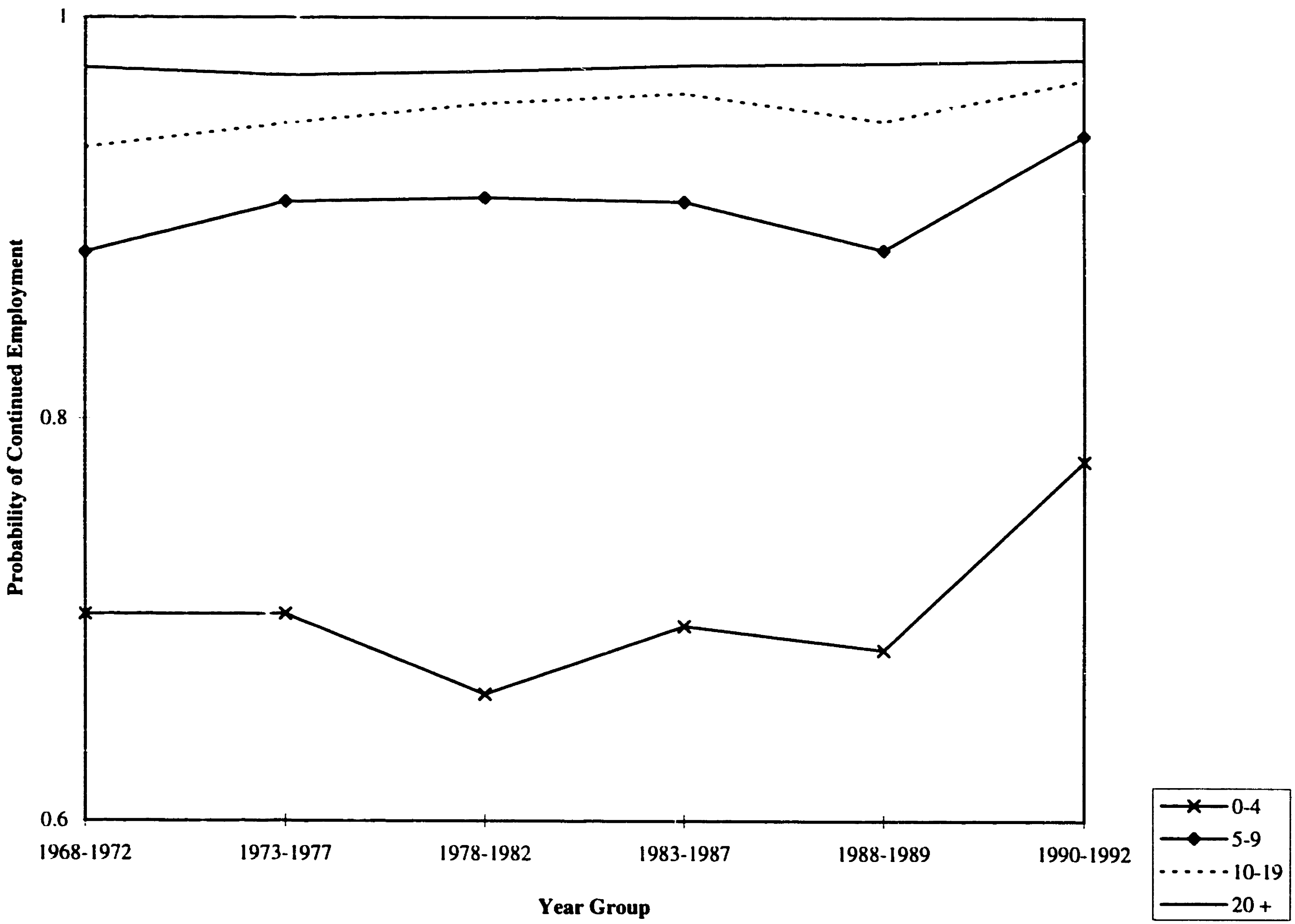
Appendix B. Figure 2.
Kernel Density of Tenure by Years, Males: 1968-1992



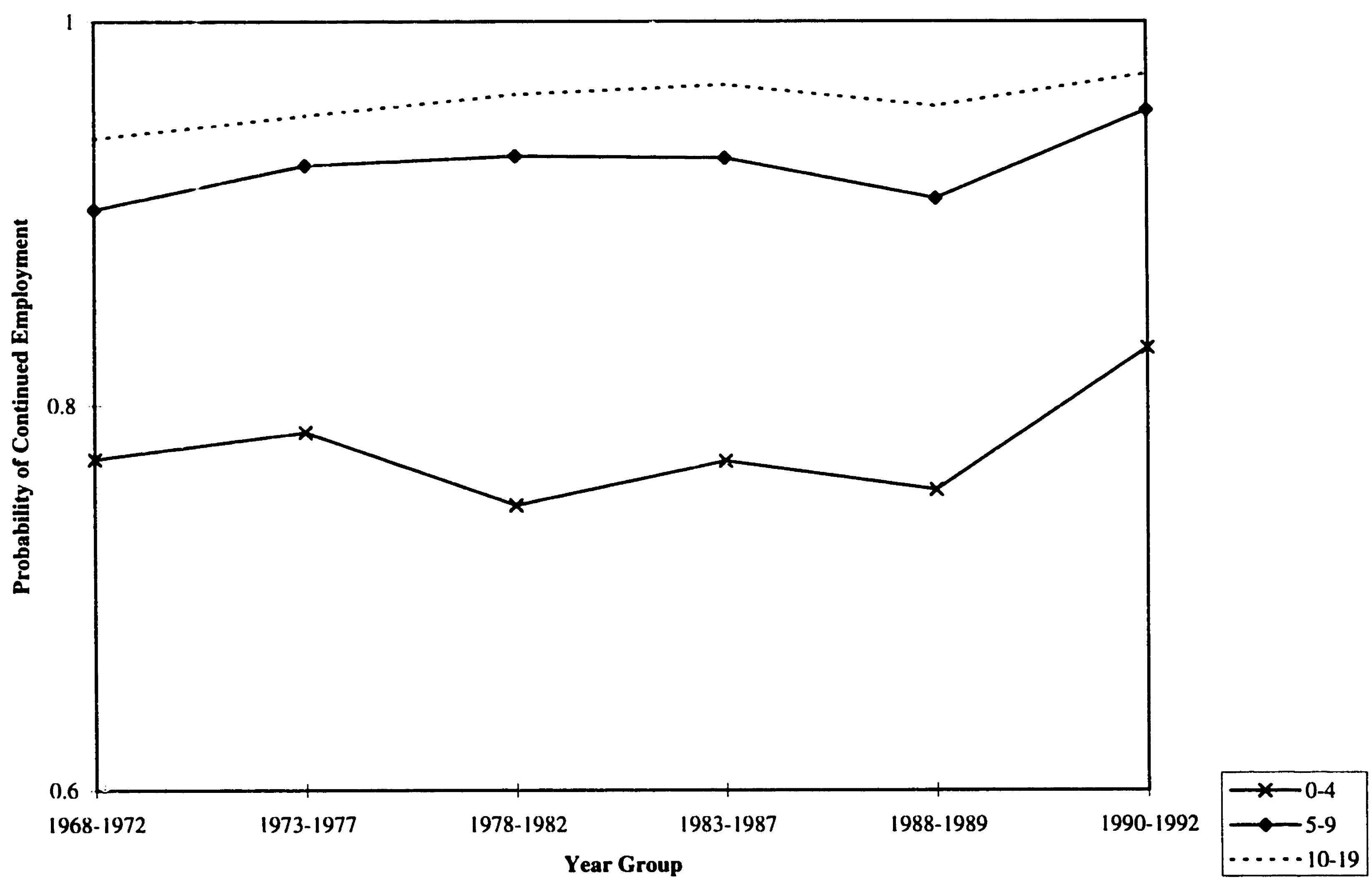
Appendix B. Figure 3.



Appendix C. Figure 1.
Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure

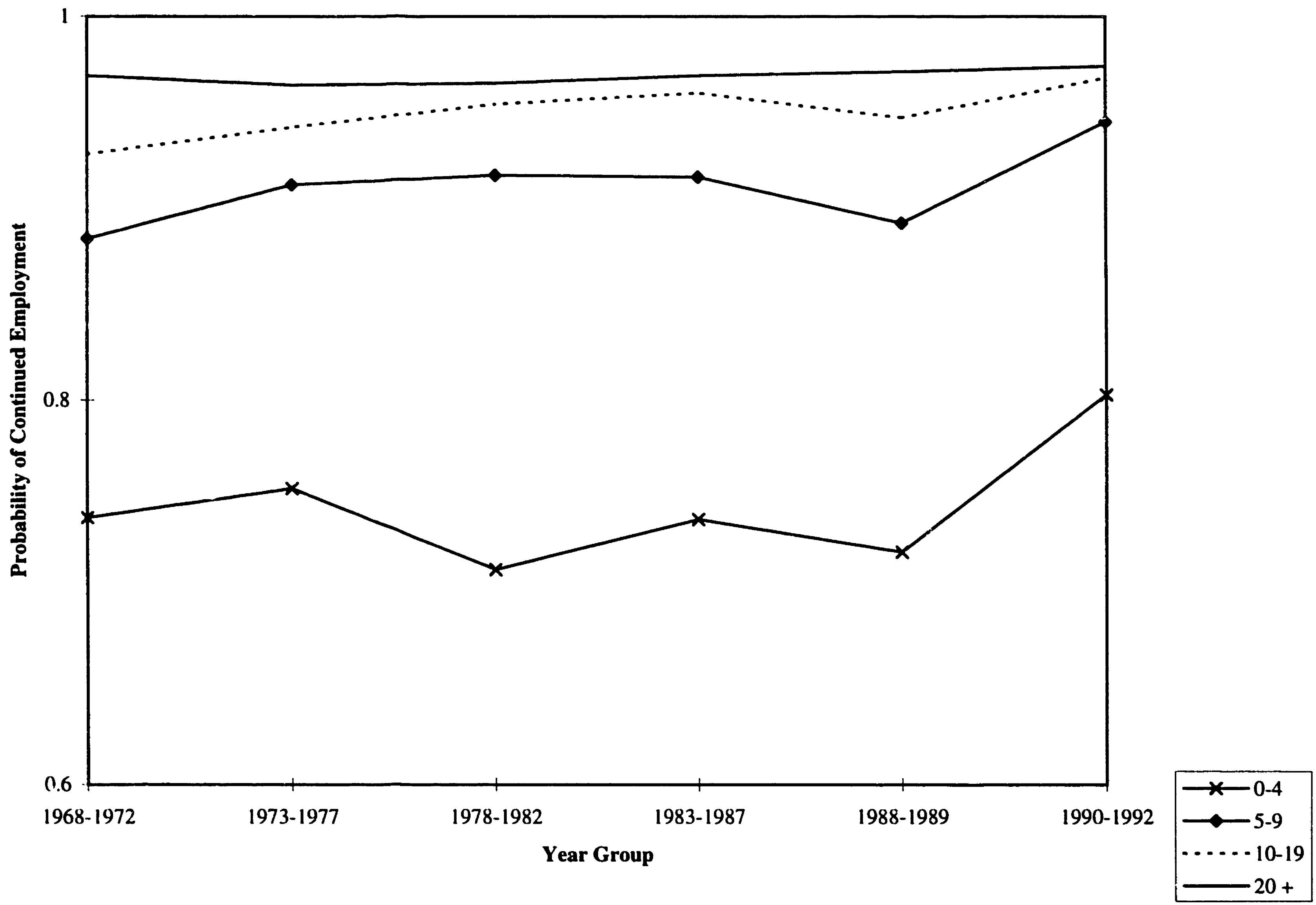


Appendix C. Figure 2.
Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: 25-34 Years Old



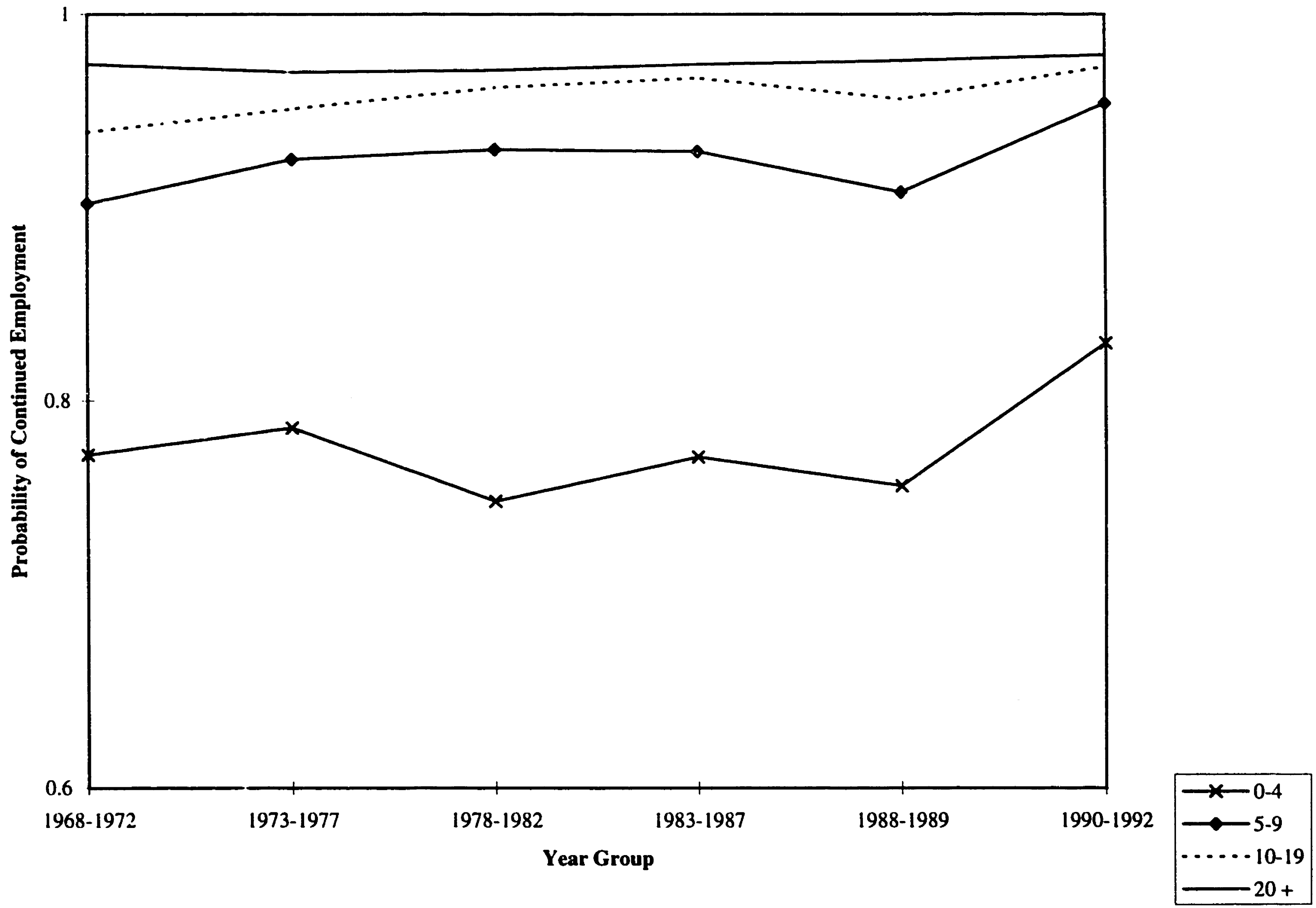
Appendix C. Figure 3.

Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: 35-44 Years Old



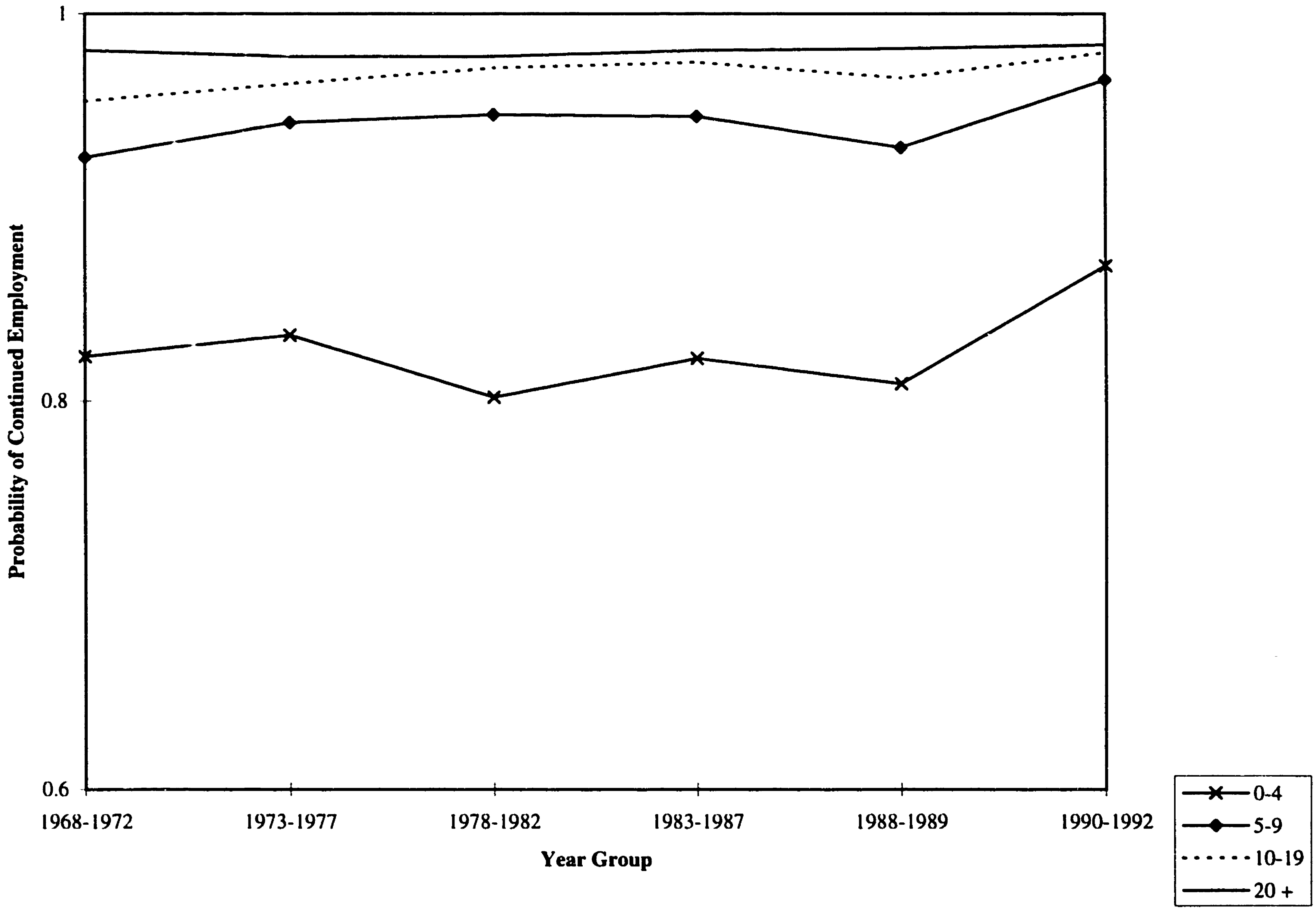
Appendix C. Figure 4.

Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: 45-54 Years Old



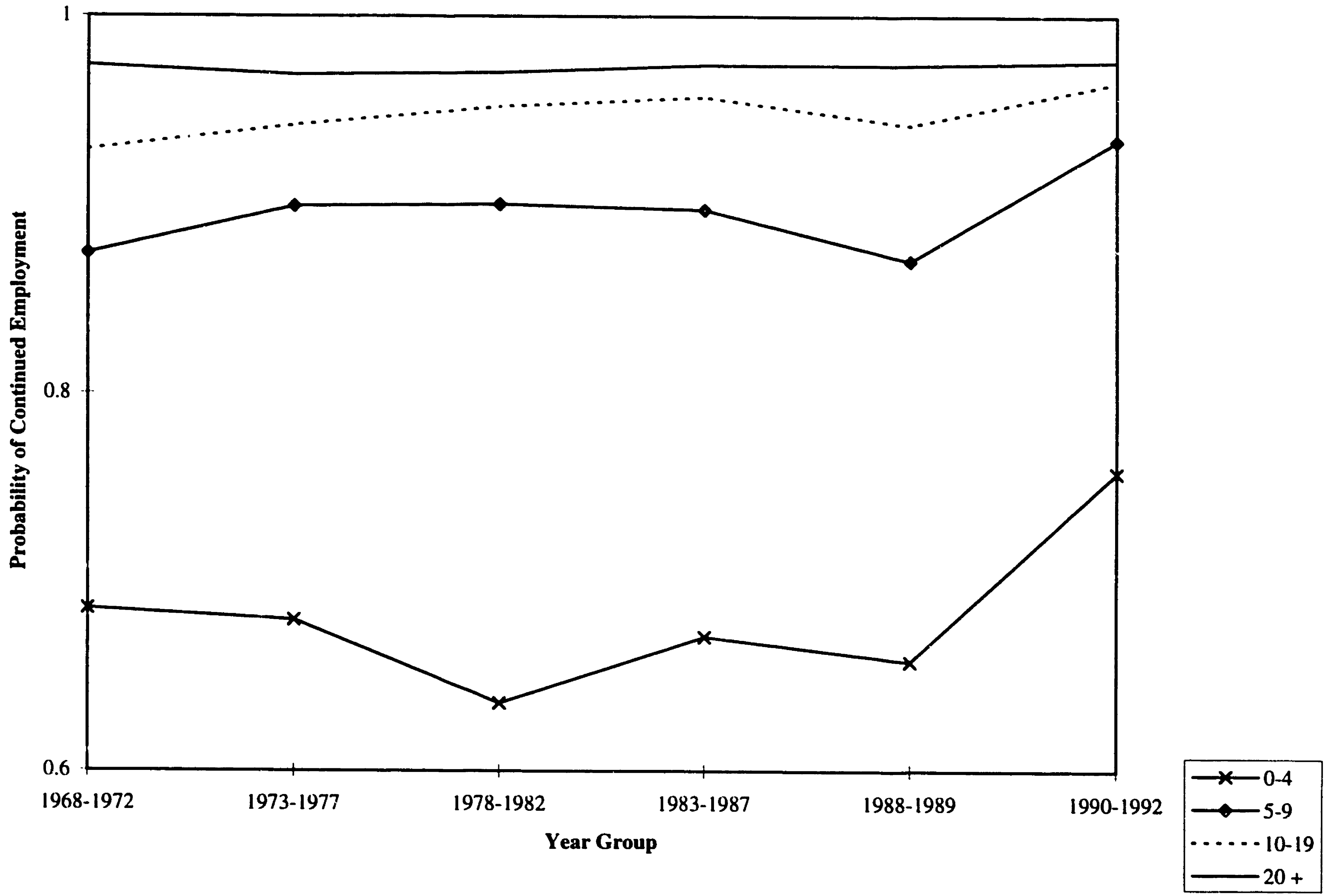
Appendix C. Figure 5.

Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: 55-65 Years Old



Appendix C. Figure 6.

Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: Less Than High School

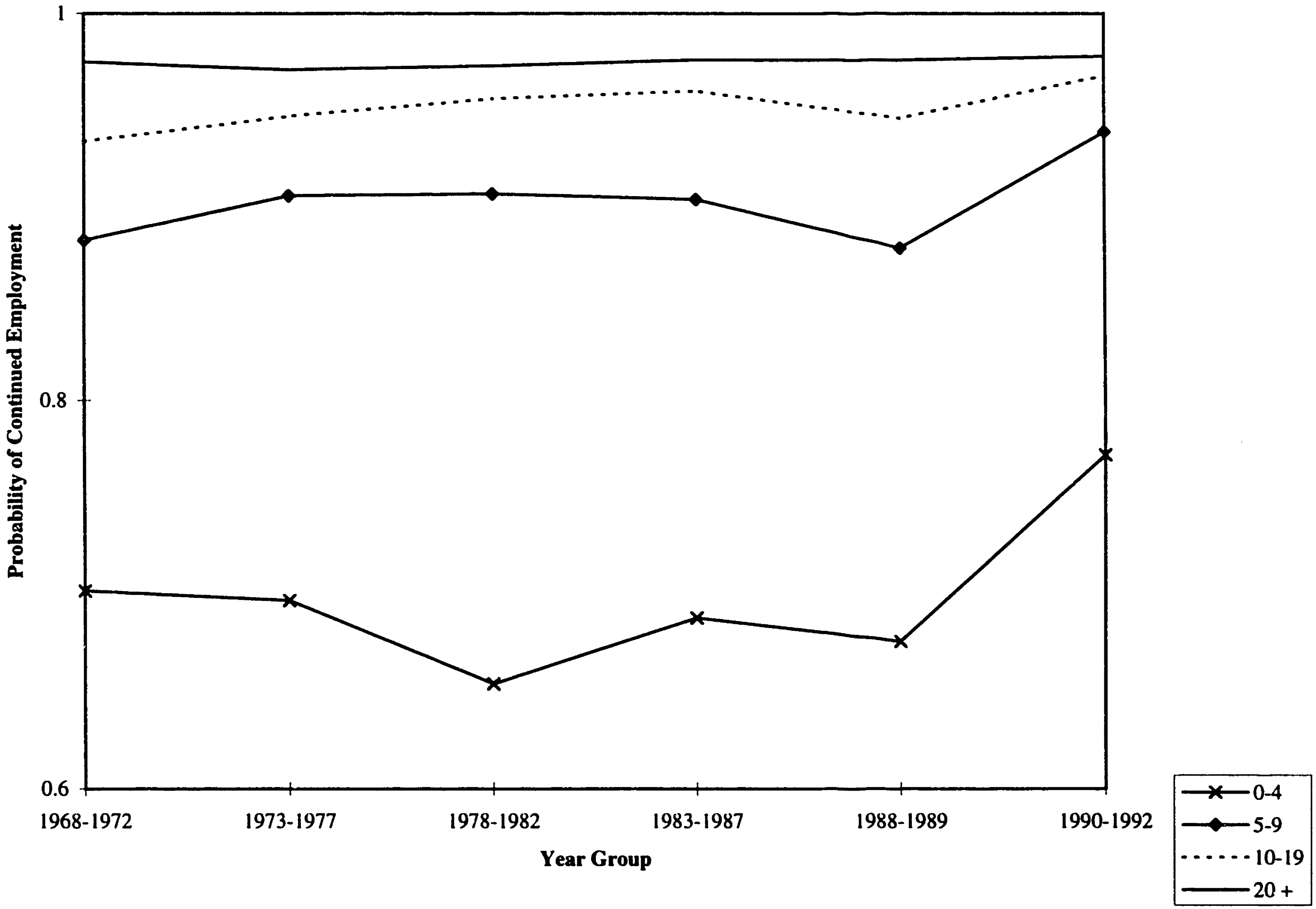


Appendix C. Figure 7.

Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: High School

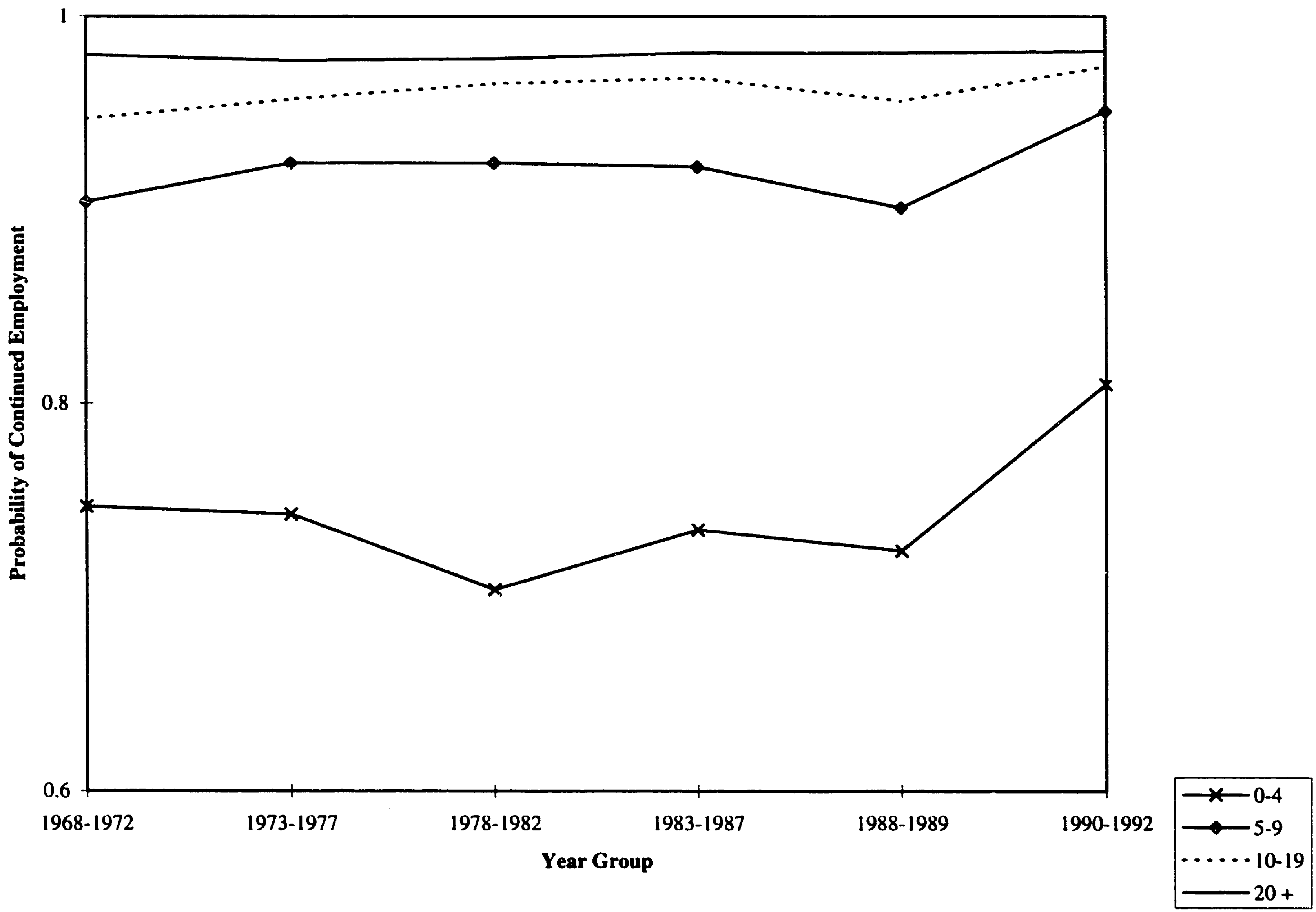
Appendix C. Figure 8.

Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: Some College



Appendix C. Figure 9.

Predicted Probabilities of Continued Employment from Year t to t+1, by Tenure: College or More



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