Declining Job Security

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Abstract

Although common belief and recent evidence point to a decline in "job security," the academic literature to date has been noticeably silent regarding the behavioral underpinnings of declining job security. In this paper, I define job security in the context of implicit contracts designed to overcome incentive problems in the employment relationship. Contracts of this nature imply the possibility of inefficient separations in response to adverse shocks, and they generate predictions concerning the relationship between job security parameters—such as worker seniority, aggregate shocks, and sectoral economic conditions—and the probability of separations. To test these predictions, I use Panel Study of Income Dynamics (PSID) data for the period 1976-92, combined with tabulations from the March Current Population Surveys (CPS) for the same (and earlier) years. I use these data to estimate binomial and multinomial models of job separation decisions. The results are consistent with a decline over time in the incentives to maintain existing employment relationships.

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I. Introduction

Popular concern about worker job security has been on the rise in recent years, and it became particularly acute early in the 1996 presidential election campaign. Rising media attention to this issue coincided with and has been reinforced by the role of job security in monetary policy formation: Federal Reserve Chairman Alan Greenspan has cited worker job security fears as a key factor holding down inflation in 1996, and rising job security as a potential inflationary factor in 1997 and 1998.

Much of the early evidence regarding job insecurity was fragmentary and anecdotal, as newspapers and other popular sources described the impact of major corporate downsizings and changes in workers' perceived job security. The first few academic papers (e.g., Farber 1995, Diebold, Neumark, and Polsky 1997, Swinnerton and Wial 1996) focused on average job tenure and found that it had been relatively constant for men since the early 1970s, which some observers interpreted as being inconsistent with the declining job security view.

However, recent evidence has been more supportive of the declining job security view. The U.S. Bureau of Labor Statistics (1997) reported that average job tenure for men declined between 1983 and 1996, after controlling for the aging of the male work force by examining average tenure within age groups. Furthermore, the 1996 Displaced Worker Survey (DWS) revealed relatively high displacement rates during 1993-95 compared to earlier periods, particularly for skilled white-collar workers (Kletzer 1996). Finally, using monthly CPS data, Valletta (1996) and Valletta and O'Toole (1997) found an upward trend in involuntary separations into unemployment during the past several decades. In this paper, I argue that these recent findings reflect a long-run underlying trend towards reduced job security, and I use panel data to identify and examine the nature of these trends during the period 1976-91.

Formal analysis of changing job security requires defining the concept in terms of standard economic models of the employment relationship and turnover decisions. Turnover costs and specific investments imply optimality of ongoing employment relationships for wide classes of workers and jobs. Furthermore, the efficient separations view (McLaughlin 1991) of job mobility implies that only inefficient (i.e., non-surplus producing) matches are dissolved, with the resulting turnover benefitting workers and firms. Models with costly or suppressed renegotiation of wages (Hashimoto 1981, Antel 1985, Hall and Lazear 1984, Hall 1995) imply inefficient separations in response to shocks but do not formally elucidate the underlying reasons for such rigidity *vis-a-vis* permanent separations.

Moreover, the efficient separations view has limited implications for changes in turnover behavior and outcomes beyond those caused by productivity shocks that alter the relative value of existing job pairings. In this paper, I define and analyze changing job security in the context of implicit employment contracts designed to overcome incentive problems within the employment relationship. Early efficiency wage variants of such models implied no incentives for employer dishonesty (for example, Shapiro and Stiglitz 1984, Bulow and Summers 1986). Other models, particularly those with rising sequences of wages, include incentives for employer malfeasance but appeal to reputational constraints to eliminate employer malfeasance in equilibrium (e.g., Lazear 1979, Bull 1987). Such behavior is not excluded under all conditions. Idson and Valletta (1996) found evidence consistent with the view that involuntary separations of high tenure workers follow a pattern consistent with employer breach of implicit employment arrangements under adverse economic conditions. However, limitations of their data set (a sample of layoff unemployment spells for the years 1982-83) prevented extending the results to separation decisions more generally.

For the present work, I use as my theoretical starting point Ramey and Watson's (1997) model of bilateral incentive problems for workers and firms. In this model, the contract is structured to overcome incentives for each party to behave opportunistically, where opportunism entails performing at levels lower than those to which the parties agreed. This situation exhibits properties akin to a prisoner's dilemma game; with specific investments, opportunistic behavior may occur in bad states. This causes inefficient separations even when wage renegotiation is unconstrained. Furthermore, incorporating costly monitoring of worker effort changes the firms' incentives regarding opportunistic behavior towards contracted workers.

In the context of such a model, declining "job security" implies that given existing economic conditions, workers who had a reasonable expectation (presumably based on past firm behavior) of not being dismissed are being dismissed. This implies a change in the relationship between contract parameters (such as tenure and economic conditions) and the probability of being dismissed. I test the empirical implications using data for household heads and wives from the Panel Study of Income Dynamics for the years 1976-92. I estimate binomial models of dismissals, and multinomial models of separation decisions more generally. The results reveal significant changes over time in the relationship between job tenure and turnover decisions by workers and firms. These results appear consistent with secular changes in incentives to maintain ongoing employment relationships.

II. Implicit Contract Models of Job Security

A. Contracts With Bilateral Incentive Problems

In a world of fully efficient job separations, job security is irrelevant: only matches that produce no joint surplus are dissolved, and their dissolution renders both parties better-off. Models with suppressed renegotiation (Hall 1995, Hall and Lazear 1984) imply the incidence of inefficient separations but essentially beg the question by imposing wage rigidity for *ad hoc* reasons.

More promising are models that analyze and provide solutions to incentive problems in the employment relationship. If monitoring is imperfect, workers must be provided with incentives to exert appropriate effort on the job. This consideration motivates the efficiency wage and deferred payment contracts literatures. With the notable exception of Bull (1987), however, the possibility of firm malfeasance is not explicitly modeled.

A recent contribution that recognizes the potential for bilateral incentive problems is Ramey and Watson (1997). In addition to the standard worker effort constraint that must be overcome, firms face an incentive to cheat on workers; both forms of noncooperation yield a short-run payoff at the expense of dissolution of the job match. In this sub-section, I describe a simplified version of their model, which I use as a baseline for analyzing changes in job security and worker/firm attachment more generally; in the next sub-section, I describe a modification that accounts for costly monitoring of worker behavior.

We proceed by modeling the employment relationship as a strategy game that conforms to a prisoner’s dilemma under possible realizations of relevant state variables. In particular, consider the following payoff matrix for a given job pairing in a single period:

Employment Payoffs		
	Worker	
Firm	cooperate	not cooperate
cooperate	$\tilde{z}$	y_f , x_w
not cooperate	x_f , y_w	0 , 0

Above, subscript "*f*" identifies employer (firm) variables, and "*w*" identifies worker variables. Let $\tilde{z}=z_f+z_w$ and $x=x_f+x_w$. In this schema, $\tilde{z}$ is a random variable representing the net return if the worker and firm cooperate, which is divided between them according to a wage agreement. The current period payoffs to unilateral noncooperation are x_f and x_w. The benefit to the worker, x_w, can be interpreted as the utility gain from reducing effort on the job. Similarly, the benefit to the firm, x_f, can be interpreted as reflecting an effort choice by the firm's owner or manager. Alternatively, x_f can be interpreted as the firm's gains from worker reassignment to a less desirable job, reduction of staff or capital support or other job related perquisites, or other employer decisions that have a short-run payoff to the firm but reduce the worker's well-being. In this setting, y_f and y_w represent the firm's and worker's return when the other party does not cooperate. Assume that unilateral selfish behavior reduces joint returns below those associated with the cooperative outcome and bilateral selfish behavior, i.e.:

$$\tilde{z} > 0 > (x_f + y_w) \ , \ (y_f + x_w)$$

We assume that noncooperative behavior (also referred to as shirking, cheating, breach, or malfeasance) is detected immediately by both parties, but cooperation can not be enforced (even through a third party, such as the courts). Assume further that noncooperation by either party in a period results in dissolution of the relationship at the end of that period.

The key implication of this model is that given the realization of $\tilde{z}$, productive job pairings can be destroyed even with fully renegotiable wages. In particular, given sufficiently low realizations of $\tilde{z}$, the benefits of non-cooperation for individual agents will outweigh job rents, and at least one party will have the incentive to separate. In a single period, incentives to dissolve the relationship exist if $\tilde{z}<x_f+x_w$. Under these circumstances, either $z_f<x_f$ or $z_w<x_w$: at least one agent's share of the job rents falls below their benefit from malfeasance.

In a multi-period setting, the relationship will continue only if it generates sufficiently high returns in the future to overcome the attractiveness of outside opportunities and the incentives for non-cooperation. In particular, assuming multiple future periods, a shared discount rate δ, and streams of returns (discounted to the end of the current period) equal to g for the current relationship and $w(=w_f+w_w)$ for outside relationships, continued cooperative behavior occurs if and only if the following holds:

Cooperative equilibrium condition —

$$\tilde{z} + \delta g \;\geq\; x + \delta w \tag{1}$$

If this condition does not hold, there is no profitable wage profile that maintains the relationship, and the existing wage agreement simply determines which agent shirks. Essentially, the inability to enforce cooperative effort levels, and the corresponding benefits to noncooperation, create a gap between the value of outside opportunities and the surplus $\tilde{z}$ that maintains the relationship.

At this point, the model is similar to an efficient turnover model in terms of the separation decision; the presence of x simply implies that some separations under adverse conditions may be inefficient. Furthermore, the model provides no insights into shocks that differentially affect current job rents and the value of workers' alternative opportunities. The implications are richer—particularly in regard to job security—when we account for firm-specific investments. Assume that employers make an investment (α) in workers when the contract begins, that this investment is fixed (i.e., it can not be altered once production has begun), and the costs are born entirely by the firm.[1] Assume further that the returns to α depend on the realization of a good (G) or bad (B) state, with $z^G(\alpha)>z^B(\alpha)>0$ (i.e., the relationship is productive even in the bad state).

Ramey and Watson (1997) show that depending on the probability of the bad state and the returns to and costs of specific investment, firms will choose either *robust* or *fragile* contracts. Under a robust contract, the level of investment preserves the job pairing in both states. Under a fragile contract, the level of investment is inadequate to preserve the relationship when the bad state arises. The partial equilibrium under which firms will choose fragile contracts is summarized by the following condition:

Fragile Contract Condition—

$$\lim_{\rho \to 0}\left[z^B(\alpha^U) + \delta g^R(\alpha^U)\right] < x + \delta w \qquad \text{where } g^R(\alpha^U) = \frac{(1-\rho)z^G(\alpha^U) + \rho z^B(\alpha^U)}{1-\delta} \tag{2}$$

In this expression, ρ denotes the probability of the bad state, α^U is the chosen level of investment

[1] The assumption that the employer pays the full investment costs is consistent with view that training jobs are good jobs from the start. Incorporating contractibility of the investment (i.e., shared costs) does not change the results.

if x (i.e., incentives for malfeasance) can be ignored, and $g^R(\alpha^U)$ represents the corresponding discounted stream of expected returns. If x, w, and the relative return to α take values such that this condition holds, the firm chooses a fragile contract when ρ is sufficiently small, and the relationship dissolves upon realization of the bad state. This result holds even though there are no restrictions on wage renegotiations. It arises due to the assumed fixity of the firm-specific investment α, which precludes the reinvestment needed to maintain positive returns in the bad state.[2]

Figure 1 illustrates the structure of the model and the distinction between robust and fragile contracts. The investment level corresponding to outcomes G and B generates a fragile contract: the employment relationship is dissolved after the bad state has occurred. In contrast, the higher investment level corresponding to outcomes G′ and B′ produces a robust contract, which is maintained in both the good and bad states.

The key point to note is that a robust contract can be interpreted as representing job conditions that entail job security. In particular, under robust contracts the relationship specific investment is sufficiently high that workers' jobs are maintained in the face of adverse productivity shocks. In contrast, jobs pairings under fragile contracts are vulnerable to separations under adverse states—i.e., such jobs are productive but not secure.[3]

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[2] The key role of fixed specific human capital investment in the model suggests that the prevalence of such contracts (both robust and fragile) may increase as more jobs are characterized by fixity in specific human capital investments.

[3] The model retains features of efficient turnover models, in that the distinction between quits and firings is somewhat arbitrary: the bad state generates separations under fragile contracts, but the determination of which party initiates the separation relies purely on the wage (which specifies sharing of the rents prior to separation).

After adding a matching market to their model, Ramey and Watson simulate its business cycle properties and find that the impact of productivity shocks (the distribution of which is random across firms) is greatly magnified by the resulting destruction of productive employment relationships. However, the model also is interpretable in terms of secular changes in the degree and nature of job security. For example, differences or changes in the size of x, the probability of the bad state, and the returns to fixed investment all affect the choice of robust and fragile contracts described by equation (2).[4] I discuss the corresponding empirical implications further below, after first discussing a key model extension.

B. Costly Monitoring

An important feature of the preceding model is the assumption that firms costlessly detect shirking by workers, and that separation occurs immediately (a similar assumption is made regarding worker detection of contract breach by firms). This assumption is implausible in a wide variety of jobs. I now incorporate the realistic assumption that monitoring of worker effort is imperfect, which has been the focus of the efficiency wage and deferred payment contract literatures.

Assume that firm profits increase with worker effort, which is observed imperfectly. One scheme to solve the worker motivation problem is to promise the worker a bonus B at the end of his stay at the firm, where B is financed by the joint value created by the pairing in all periods. This contract possesses features of a deferred payment scheme (such as Lazear 1979) without

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[4] Ramey and Watson (1997) also show that an increase in the exogenous match probability raises the probability of a fragile contract equilibrium by increasing the attractiveness of outside opportunities.

requiring up-front bonds, which Akerlof and Katz (1989) have argued are infeasible.[5] Moreover, Akerlof and Katz show that under assumptions common to deferred payment and efficiency wage models, the optimal wage profile under imperfect monitoring pays the bonus at workers' retirement date. If the discount rate is zero and workers are risk neutral, this bonus equals the ratio of the worker's benefit to shirking in a period divided by the probability of detection.[6] In the present notation:

$$\textit{Worker Motivation Bonus:}\quad B = x_w/p\ .$$

where p=the per-period probability of detecting worker shirk behavior. With a nonzero discount rate, this quantity increases by an amount proportional to the discount rate and the time remaining from the worker's shirk decision until retirement.

As with any delayed payment contract, firms face incentives to breach their agreement to pay B, perhaps through dismissing the worker prior to the time it is paid. Reputational costs are the primary constraint on such behavior: firms that promise to pay B and then refuse later will face increased labor costs, as workers shirk at higher rates or demand insurance against such

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[5] A standard efficiency wage scheme—in which an excess wage payment is made in all periods, not just at retirement—also could solve the worker motivation problem. However, the current model already implies that wages for all workers exceed their wages in alternative jobs, by an amount at least as large as x_w. Moreover, as Bulow and Summers (1986) note, rising wage profiles typically are associated with jobs in which output is not directly observed. Akerlof and Katz (1989) also refer to the observed incidence of pensions and other deferred compensation schemes as an important empirical implication of their model.

[6] The intuition for this result derives from the fixed cost nature of feasible deferred payment schemes: the expected value of lost wages, which functions as a shirking penalty, must exceed the worker's gains from shirking in all time periods.

contract breach. Bull (1987) demonstrates that employer commitment to a similar deferred payment scheme is part of a Nash equilibrium outcome if each worker decides whether or not to exert effort based on whether the firm paid B to the worker immediately preceding him in the hiring sequence.[7] More generally, in operational labor markets workers are likely to assess a firm's reliability based on the treatment of a comparison group of workers.

Because such reputational mechanisms rely on information transmission between workers hired at different times, and also information transmission to potential workers, the stability of the resulting equilibrium will be sensitive to changes in information flow. In Bull's model, if the firm breaks its agreement with a single worker, all subsequently hired workers shirk. In operational labor markets, the information may be less precise (i.e., it is not known with certainty whether the firm truly cheated) but is likely to flow to a wider group of workers than in Bull's model. Under these circumstances, workers' decision rule regarding the perception of firm breach will be less stark and the resulting equilibria will be less knife-edge, thereby offering the potential for a richer set of behaviors and outcomes. In particular, the reputational constraints that enforce employer honesty may not always bind. For example, if there is rapid turnover in a firm's labor force due to changing demand conditions, information about the firm's dismissal policy may be diluted sufficiently to make breach of the implicit agreement profitable for the firm (as in Idson and Valletta 1996). The involuntary separations caused by such opportunism raise permanent layoffs

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[7] Rather than assuming costly monitoring, Bull assumes costless monitoring but turnover (quit and firing) costs that are prohibitively high. The key implication in both of our settings is that contracts will be structured to minimize worker shirking.

above the level required for optimal adjustment to changing demand conditions.[8]

C. Empirical Implications

The baseline bilateral incentive model implies that productive employment relationships will be fragile, or insecure, under various realizations of productivity. Furthermore, extension of the model to account for imperfect monitoring of worker performance implies the use of retirement bonuses or delayed wage payments, which add additional incentives for opportunistic dissolution of the employment relationship by firms.

Translating this theoretical framework into an empirical specification requires matching model parameters with observable variables. Although firm level data would be ideal, the requisite model information is unavailable in existing firm level data sets, so I use individual panel data (as described in the next section). To apply these data to the model, I begin by making a key assumption:

Empirical Assumption 1:

In a sample of workers the proportion of workers employed under robust contracts is higher for groups with higher tenure at their current firm (conditional on workers' general productivity and job prospects).

If the model is interpreted strictly, this assumption is tautological under the presence of adverse

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[8] In more concrete terms, some of the corporate downsizing and reorganization associated with the early 90s recession may have reflected firm opportunism, in addition to cutbacks induced by the aggregate downturn.

productivity shocks: workers who remain at the firm over multiple periods receive wage payments that eliminate joint dissolution incentives under all realizations of worker productivity over those periods. More generally, if exogenous separations (for example, for family reasons) are distributed randomly across workers, those whose job pairings have lasted longer also are those whose wage profiles (and underlying specific investments) successfully bind them to the firm.

Under this assumption, tenure reduces turnover because it proxies for robust contracts. This by itself is not a test of the model.[9] However, we can make a set of predictions regarding interactions between tenure and other variables, the effects of aggregate and sectoral productivity shocks, and changes over time.

Empirical Predictions:

(1) Changes over time in turnover incidence for high tenure workers reflect secular changes in model parameters. In particular, a rise in dismissal and quit rates for higher tenure workers likely is attributable to rising returns to noncooperative behavior (x), which in turn increases the incidence of fragile contracts. Such changes may be due, for example, to rising use of on-the-job search or rising capital requirements (computers) in various jobs. It may also be due to depreciation in the value of existing job-specific investments (the quantity of which is denoted α in the model).

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[9] Antel (1985) makes a similar point in the context of a costly renegotiations model of employee turnover.

(2) Because robust employment contracts withstand sectoral productivity shocks (which alter workers' value on the current job relative to alternative employment), dismissals of high tenure workers should be less sensitive to sectoral shocks than dismissals of low tenure workers, if contract incentives are maintained.

Alternatively, rapid employment change in a worker's sector may relax the reputational constraints that commit firms to pay the retirement bonus (B). If so, an appropriate measure of adverse conditions in a worker's employment sector is likely to reduce the tenure (contracting) effect on dismissal probabilities.

These effects are likely to be most pronounced among skilled white-collar workers, for whom incentive problems and job-specific investments are likely to be more significant than for lesser-skilled and blue-collar workers. Furthermore, obstacles to direct observation and enforcement of employee output are most severe in various categories of white-collar, highly skilled jobs, in which tangible output measures are less prevalent than in blue-collar jobs. Thus, we might expect to see larger changes in the effects of job security parameters among skilled white-collar workers than among other worker groups. This is consistent with a more pronounced upward trend (post 1990) in displacement of white-collar than of blue-collar workers in the Displaced Worker Survey.[10]

III. Data

[10] However, Boisjoly et al. (1994) did not find a corresponding result using PSID data for 1968-92.

I use data from the Panel Study of Income Dynamics (PSID) for the years 1976-92, combined with March Current Population Survey data for the same and previous years. The PSID provides the requisite information concerning worker and job characteristics for household heads for all years, and for wives for all years except 1976-78; this enables the formation of pooled married/single individual samples by sex. The primary sample restriction in each survey year is to workers aged 21-64 and not self-employed. I excluded the Survey of Economic Opportunity low income oversample. The data set combines information on worker and job characteristics in survey year t (employed individuals only) with information from survey year t+1 regarding whether the worker no longer is working at the same firm as in year t. The observations used therefore end in survey year 1991 (but incorporate job change information through 1992).

Key variables for the analysis are measures of the incidence of and reason for changing firms, and years of tenure at the current firm. For individuals who no longer are at the same firm as in the previous year (which excludes those on temporary layoff), the four reasons identified in the survey are: (1) quit; (2) plant closed; (3) permanently laid-off or fired (which I term "dismissed");[11] (4) other reasons (including temporary or seasonal job ended). Tenure at the

[11] Boisjoly et al. (1994) conducted a special recoding of their PSID data set to distinguish between individuals laid-off due to a decline in demand and individuals fired for cause. They found that approximately 16% of the observations in this category involved firings for cause, which they excluded from their measure of involuntary job loss. For reasons related to unemployment insurance eligibility, workers who want to quit may misbehave in order to induce a firing. Such behavior probably is tempered, however, by the signaling costs faced by fired workers (Gibbons and Katz 1991). It is more likely that quits by workers are due to firm malfeasance, because the resulting reputational costs for firms probably are smaller than such costs arising from a direct firing of the worker. In any event, the distinction between quits and firings is not particularly important to my model.

current firm is measured in months, which I converted to years. As discussed by other researchers, this variable is subject to substantial error (see for example Topel 1991).[12] I corrected tenure by assuming that its value in a base year was correct and then forcing tenure to be consistent within and across the job spells identified by the job change variables.[13]

I tabulated measures of economic conditions in workers' industry/region sector using data from the March Current Population Surveys (CPS) for various years. The CPS files provide information on current employment status and labor market experience over the previous year. I calculated the change in sector employment levels for the 10-year periods preceding the sample frame and use it as a measure of sectoral conditions relevant to job security.[14] I also calculated and use the sector-specific unemployment rate as an alternative measure of sectoral conditions; following Murphy and Topel (1987), I measure this rate as total weeks unemployed divided by total labor force weeks in that cell. The sectors are defined by respondents' industry and geographic location of current residence. I use 43 detailed industry categories and 9 geographic

[12] During all years prior to 1976, only information on tenure and turnover in a position (rather than at the firm) was available. Given the potential for associated errors in the tenure and turnover variables, I chose to begin the male sample in 1976. A similar problem exists for the 1979-1980 tenure data, but my tenure correction overcomes it. Furthermore, my results for men are very similar when I use data beginning in 1981 (with the expected minor diminution in the time trend results).

[13] More precisely, I treat the reported value of tenure for the last year in an individual's first sampled job spell as correct. I then count backwards to the beginning of that spell, and I use the yearly job change information to identify additional spells and count forwards within them. My results change very little when I follow the procedure used by Topel (1991), which is similar to mine but based on the maximum reported tenure in job spells. The results also are similar when I use reported tenure without any corrections.

[14] Because my CPS data begin in 1968, the sector employment change figures for 1976 and 1977 are 8 and 9 year changes, respectively, both expanded to a 10-year rate.

division categories, which produces 387 sectors.[15] As a measure of aggregate economic conditions, I use the official national unemployment rate in each year.

Restriction to non-missing values of regression model variables yields pooled samples of 22,469 for men (1976-91) and 15,400 for women (1981-91);[16] Appendix Table A1 lists sample means. Table 1 shows tabulations of average job tenure and turnover rates (total and by reason) for men and women in the regression samples, for selected years that span the sample period. Average job tenure was virtually constant for men over the period, with a slightly higher value in 1982. In contrast, average tenure for women increased noticeably from 1979 to 1991. These tabulations are reasonably consistent with the findings of Diebold et al. (1997) and Farber (1995).

For both men and women, Table 1 reveals substantial year to year variability in turnover rates, which swamps any trends over time. Quits account for the largest share of turnover incidence in general, and they demonstrate a substantial cyclical pattern, with high rates during ongoing expansions (1985 and 1988) and reduced rates during recessions (1982 and 1991). Dismissals are less frequent than quits, but they appear to demonstrate a countercyclical pattern, as we might expect. The incidence of job loss due to plant closings and other reasons does not demonstrate any noticeable pattern over time or the business cycle.

IV. Empirical Analysis

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[15] I weighted all CPS tabulations by the March supplement weights, and I treat the tabulated variables as fixed in the regressions.

[16] I restricted the female regression sample to post-1980, due to the combination of missing values for wives' variables in 1976-78 and missing values for tenure at the firm in 1979-80.

A. Framework

My empirical analysis consists of binomial probit equations for the probability of dismissals (permanent layoffs and firings) and multinomial logit models of turnover probabilities (using the four reasons described in the data section as outcomes). I estimate the following basic probit equation for dismissal incidence (D):

$$Pr(D_{it}=1) \;=\; H_{it}\beta \;+\; T_{it}\gamma_1 \;+\; U_t^a\gamma_2 \;+\; \Delta E_{it}^s\gamma_3 \;+\; t\gamma_4 \;+\; (t\cdot T_{it})\lambda_1 \;+\; (\Delta E_{it}^s\cdot T_{it})\lambda_2 \qquad \textbf{(3)}$$

In this equation, *i* indexes individuals, *t* indexes time, and the Greek letters denote coefficients to be estimated. The matrix *H* represents a relatively standard set of human capital and other control variables: educational attainment (6 category dummies), years of full-time work experience since age 18 and its square, number of children, ln(real hourly wage at the current job),[17] and dummy variables for government employment, union membership, non-white, whether married, and MSA residence. These variables are intended to control for workers' general productivity and job prospects. The other variables are job tenure *T*, the aggregate unemployment rate U^a, sector employment growth ΔE^S (or the sector unemployment rate), a time trend variable *t*, and tenure interacted with the time trend and with the sectoral employment growth or unemployment figure. In addition to standard probit estimates on the pooled data, I estimate a random effects probit model, which accounts for individual specific error components (see Chamberlain 1980).[18]

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[17] I inflated the wage in each year to 1992 levels using the GDP deflator for personal consumption expenditure.

[18] It also is possible to account for individual fixed effects in the binomial dismissal models. However, this requires eliminating from the sample individuals who were never dismissed (or dismissed in every period); obtaining unbiased estimates in some models requires further restriction to a minimum number of observations per individual. Furthermore, the fixed-effects

The basic multinomial logit equation is:

$$Pr(C_{it}=j) \;=\; \frac{e^{Q_{it}\Psi_j}}{1+\sum_{k=1}^{4} e^{Q_{it}\Psi_k}} \tag{4}$$

$$where\;\; Q_{it}\Psi_j \;=\; H_{it}\beta_j \;+\; T_{it}\gamma_{1j} \;+\; U_t^a\gamma_{2j} \;+\; \Delta E_{it}^s\gamma_{3j} \;+\; t\gamma_{4j} \;+\; (t\cdot T_{it})\lambda_{1j} \;+\; (\Delta E_{it}^s\cdot T_{it})\lambda_{2j}$$

In (4), C_{it} denotes job change outcomes by individual *i* at time *t*, and *j* takes on 4 values defined by the four job change categories (with no change as the omitted category). The Greek letters again denote coefficients to be estimated. This model enables testing of separate hypotheses regarding voluntary and involuntary turnover. For example, the distinction between quits and dismissals is identified through differing effects of the aggregate unemployment rate on these outcome categories. Furthermore, comparison of the coefficients for the dismissal and plant closing categories indicates whether dismissal decisions are being made in ways that distinguish between employees at a site, or are uniform across employees at a site.

B. Results

Table 2 contains results of probit regressions for dismissal incidence, using the pooled samples of men (panel A) and women (panel B). The estimated coefficients for the general control variables (H) are unsurprising and therefore are omitted from the tables (with the exception of the wage variable). The different specifications in the table include various combinations of tenure interactions with the time trend and with sector employment growth.

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model is not well suited to my purposes due to the identity linking job tenure and time within individual job spells.

Column (5) presents results from a specification identical to that in column (4), except for the inclusion of random individual effects; this has virtually no effect on the estimated coefficients and standard errors.

Turning first to the control coefficients, the hourly wage variable has a strong and consistent negative effect across the various specifications. This presumably reflects unobserved productivity enhancing features of individual job matches. As expected, dismissals increase substantially with the aggregate unemployment rate, but they decline substantially with job tenure. The effects of these variables are large, particularly for job tenure. Using the coefficients in column (1), five additional years of tenure reduce the dismissal probability by nearly half for the typical male in the sample. By comparison, a one standard deviation increase in the log hourly wage reduces the dismissal probability by about 20%, and a one standard deviation increase in the aggregate unemployment rate increases it by about the same amount.

Several interesting results are apparent in regard to the sensitivity of employment relationships over time. The first column results reveal a significant upward time trend in the probability of dismissals. However, inclusion of the tenure*time interaction in column (2) reveals that the upward time trend is concentrated among high tenure workers: the coefficient on the interaction variable is significant, and its inclusion substantially reduces the size and precision of the estimated time trend effect alone. This result suggests that male workers with substantial job tenure—i.e., workers whose jobs are most likely to be characterized by incentive based implicit employment contracts—faced rising risk of dismissal during 1976-91. This reduction in the tenure effect over time is large. Based on the results in columns (2), (4), and (5), the interaction coefficient implies a reduction in the tenure effect of 55-70% during the 15 years ending in 1991.

This increase in the probability that high tenure workers will be fired is consistent with substantial erosion over time in the incentives to maintain ongoing employment relationships.

The other key contract model variable—the interaction between tenure and the change in sector employment—also produces interesting results. The effect of sector employment growth by itself essentially is zero (columns 1 and 2). However, this masks variation in the effect of sector growth across workers at different tenure levels. In particular, the significant negative interaction effect between tenure and sector growth implies that the negative tenure effect on dismissal probabilities is reinforced in expanding sectors. Equivalently, the negative tenure effect on dismissals is reduced in declining sectors—i.e., high tenure (contracted) workers are more likely to be dismissed in declining sectors. Assuming that sectoral decline impedes the transmission of information regarding employer default on delayed payment contracts, this result suggests that contracted workers face increased risk of employer default in declining industries; this is consistent with Idson and Valletta's (1996) results regarding the tenure pattern in recall from temporary layoff. The magnitude of this default parameter, however, is small compared to the tenure coefficient; the tenure effect is reduced noticeably only in sectors that experience excessive shrinkage.

In contrast to men, the results for women in panel B of Table 2 reveal no apparent changes or sensitivity in employment contract conditions. The wage, aggregate unemployment, and tenure variables have effects for women that are similar to those for men. However, no time trend or sector effects are apparent. This is consistent with the general perception that changing job security primarily is an issue for male workers. In the remainder of the paper, I therefore discuss results for men only.

Table 3 presents results for models identical to those in Table 2 (panel A), except with the sector unemployment rate replacing sector employment growth. The results in general are very similar to those from the previous table. The positive effect of the aggregate unemployment rate on dismissals remains but is reduced nearly in half, with a similar sized but more precisely estimated contribution coming from the sector unemployment rate.

In contrast to the Table 2A results for the tenure*sector interaction, however, the interaction effect of tenure and sector unemployment is insignificant (the t-statistic in column (3) falls just below the 10% critical value). Furthermore, the negative point estimates have the opposite interpretation of those on sector employment growth in Table 2: they indicate that the negative effect of tenure on dismissal incidence is larger in sectors that are experiencing high unemployment rates for attached workers. A significant negative coefficient on sectoral unemployment could reflect robust contracts: high tenure (contracted) workers in sectors experiencing difficulties are protected from those difficulties by specific investments and underlying contract terms. However, given the weakness of these coefficients, particularly in the full specification in columns (4) and (5), my preferred interpretation is that sector employment growth provides a better measure of long-run industry prospects, hence the incentives for employer contract breach, than does the contemporaneous unemployment rate.

Table 4 presents multinomial logit results for general turnover incidence, for a specification that otherwise conforms to that in column (4) of Tables 2-3. Turnover declines with the hourly wage, except turnover for "other reasons." Rising aggregate unemployment decreases quits, increases turnover due to dismissals and "other reasons," but has no effect on job loss due to plant closures. All forms of turnover decline with job tenure. The magnitudes of these effects in the quit and dismissal equations are quite large. For the typical male in the sample, 5 additional years of tenure reduce quit and dismissal probabilities by 40% and 65%, respectively; a standard deviation increment in log hourly wages reduces these probabilities by 20-25%, with a slightly smaller impact attributable to aggregate unemployment.

The most interesting results in Table 4 revolve around trends in the incidence of quits and dismissals. The dismissal column confirms that high tenure workers became increasingly likely to be dismissed over the sample period, with a similar result in the quit column. As with the dismissal probit results in Table 2A, these results are consistent with rising returns to noncooperative behavior (x in the theoretical model), which increases the incentive for both parties to dissolve productive employment relationships (which are indexed by tenure).

Table 4 also reveals significant interactions between tenure and the time trend in their effects on job loss due to plant closures and other reasons. High tenure workers have been increasingly likely to lose jobs due to plant closures. This suggests that the erosion of contract incentives may have affected the pattern of plant closures, perhaps through disproportionate closure of plants with a large share of high tenure workers. The positive tenure*time interaction effect is not uniform for all job change categories, however; it is negative in the "other reasons" category, which suggests that the tenure*time interaction effects are not purely an odd (but consistent) artifact of the data.

Interestingly, sector employment growth increases quits, job loss due to plant closures, and dismissals. The positive effect on quits is consistent with improving outside employment opportunities for workers currently employed in expanding sectors, although the negative effect of the tenure/sector interaction suggests that high tenure workers do not fully share in this pattern. Similar coefficients in the plant closed column suggest that plant closures represent an adjustment mechanism in expanding (rather than declining) industries, although high tenure workers are somewhat insulated from the resulting job losses. The positive effect of sector employment growth on dismissals appears somewhat surprising. However, the full effect of sector employment growth on dismissals, with the tenure interaction effect evaluated at the mean tenure level, is negative. As in the dismissal probits, the tenure*sector interaction coefficient indicates that the negative effect of tenure on dismissals is mitigated by sectoral decline; this is consistent with reduced obstacles to employer breach of delayed payment contracts in declining sectors.

As noted in Section IIC, trends in job security are likely to be most pronounced in skilled white-collar jobs, in which incentive and monitoring difficulties are likely to be most severe. Table 5 presents results from regressions that test this proposition; the sample is restricted to white-collar jobs excluding sales and service occupations. The results are very similar to those reported for the full sample, using a probit equation that otherwise corresponds to column (4) in Table 2 and a multinomial logit comparable to Table 4. However, the tenure*time interaction coefficients in the dismissal equations are approximately twice as large in the restricted white-collar sample as they are in the full sample. In contrast, the interaction of tenure and sector employment growth has a smaller effect on dismissal probabilities in this sample than it did in the full sample; the relevant interaction coefficient attains only marginal significance. Thus, Table 5 presents mixed support for the claim that changing job security parameters have been particularly important for skilled white-collar workers: they appear to face the largest erosion in basic contract incentives, but there is little evidence for greater employer contract breach despite the presumption of greater monitoring costs in such jobs.

V. Conclusions

I specified a general employment contracting framework that accounts for performance incentive problems for workers and firms, combined with imperfect monitoring of worker performance. Under these circumstances, incentives to maintain existing employment relationships may change over time and be responsive to measures of economic conditions. Using data from the Panel Study of Income Dynamics for the years 1976-92, I found evidence consistent with changing employment security (for men) in the context of such a model. In particular, the negative effect of job tenure on the probability of dismissals has weakened over time, as has the corresponding negative effect on quits. Furthermore, my results indicate erosion of the negative tenure effect on dismissal probabilities in declining sectors, which is consistent with employer default on delayed payment employment contracts (as in Idson and Valletta 1996).

My results do not support unambiguous conclusions regarding the source of declining job attachments. This partially reflects the tradeoff between model breadth and precision of empirical predictions: the model is sufficiently broad to explain a variety of changes in turnover behavior, but precise tests require better empirical analogs to the model parameters. In general, declining attachment of high tenure workers (through both rising dismissals and rising quits) is broadly consistent with rising returns to noncooperative behavior in employment relationships, and rising fixity or declining value of job-specific investments. My results regarding interaction effects between sectoral economic conditions and tenure in the determination of dismissal probabilities suggest that employers may be breaching deferred payment compensation schemes. However, the absence of a stronger result for white-collar workers, for whom such contracts are likely to be more prevalent due to high monitoring costs, weakens support for this view.

This paper largely was motivated by recent results from the Displaced Worker Survey which suggest that the rate of involuntary job loss was very high during 1993-95, and by the view of some policy makers that rising job insecurity contributed to moderate wage and price inflation in 1996. Although constraints on available data precluded extending my model to the this recent period, I identified a long-run trend toward declining job security that probably continued through 1996. Extending the analysis to later years of data (when available) should prove to be particularly interesting.

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Table 1
Average Tenure & Turnover Rates (Selected Years)
PSID Data

PANEL A: MEN (household heads)

Year	Average Tenure	Changed Jobs	Quit	Dismissed	Plant Closed	Other Reasons	Sample Size
1976	7.764	0.151	0.081	0.028	0.015	0.028	1421
1979	7.744	0.159	0.095	0.032	0.015	0.017	1520
1982	8.090	0.172	0.058	0.061	0.015	0.038	1548
1985	7.676	0.211	0.119	0.035	0.019	0.039	1580
1988	7.695	0.178	0.123	0.032	0.014	0.010	1614
1991	7.723	0.152	0.079	0.043	0.012	0.019	1562

PANEL B: WOMEN (wives and household heads)

Year	Average Tenure	Changed Jobs	Quit	Dismissed	Plant Closed	Other Reasons	Sample Size
1976	N/A	N/A	N/A	N/A	N/A	N/A	N/A
1979	4.849	0.138	0.097	0.015	0.012	0.013	1047
1982	5.043	0.156	0.075	0.036	0.018	0.027	1246
1985	5.175	0.240	0.175	0.026	0.014	0.024	1381
1988	5.471	0.211	0.161	0.023	0.015	0.013	1485
1991	6.045	0.174	0.118	0.026	0.011	0.019	1516

Note: The sample is initially restricted to employed individuals, aged 21-64 and not self-employed in the survey year. The sample is further restricted to individuals with non-missing values of the regression model variables (see Tables 2-5).

Table 2
Probit Regressions for Dismissals
(Dependent Variable = 1 if laid-off/fired, 0 otherwise)

PANEL A: MEN (1976-91)

Variable	(1)	(2)	(3)	(4)	(5) Random Effects
ln(real hourly wage)	-0.178**	-0.181**	-0.175**	-0.177**	-0.173**
	(0.037)	(0.037)	(0.037)	(0.037)	(0.039)
U.S. unemployment rate	6.449**	6.681**	6.423**	6.643**	6.691**
	(1.359)	(1.367)	(1.360)	(1.367)	(1.351)
Tenure	-0.054**	-0.083**	-0.053**	-0.085**	-0.083**
	(0.004)	(0.012)	(0.004)	(0.011)	(0.011)
Time trend	0.016**	0.006	0.016**	0.005	0.004
	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
Tenure*(time trend)	--	0.003**	--	0.003**	0.004**
		(0.001)		(0.001)	(0.001)
Δln(sector employment)[1]	-0.001	-0.002	0.063	0.086	0.068
	(0.047)	(0.047)	(0.058)	(0.059)	(0.061)
Tenure* (Δln(sector employment))	--	--	-0.018	-0.024*	-0.022*
			(0.009)	(0.010)	(0.010)
Log-likelihood	-3294.5	-3289.8	-3292.8	-3286.9	--
Pseudo-R^2	0.102	0.103	0.103	0.104	--

Number of Observations = 22469

** indicates significance at the 1% level
* indicates significance at the 5% level

[1] 387 sectors defined by 43 industry categories and 9 geographic regions.

Note: Standard errors in parentheses. Other variables controlled for include educational attainment (6 category dummies), years of full-time work experience since age 18 and its square, number of children, and dummy variables for government employment, union membership, non-white, whether married, and MSA residence.

(continued)

Table 2 (continued)

PANEL B: WOMEN (1981-91)

Variable	(1)	(2)	(3)	(4)	(5) Random Effects
ln(real hourly wage)	-0.101*	-0.101*	-0.101*	-0.101*	-0.097*
	(0.048)	(0.048)	(0.048)	(0.048)	(0.050)
U.S. unemployment rate	4.498	4.490	4.506	4.498	4.637
	(2.521)	(2.521)	(2.521)	(2.521)	(2.487)
Tenure	-0.067**	-0.075**	-0.061**	-0.070**	-0.076**
	(0.007)	(0.026)	(0.009)	(0.026)	(0.026)
Time trend	0.007	0.005	0.007	0.005	-0.001
	(0.012)	(0.013)	(0.012)	(0.013)	(0.014)
Tenure*(time trend)	--	0.001	--	0.001	0.002
		(0.002)		(0.002)	(0.002)
Δln(sector employment)[1]	0.060	-0.060	0.114	0.113	-0.098
	(0.083)	(0.083)	(0.107)	(0.107)	(0.111)
Tenure* (Δln(sector employment))	--	--	-0.021	-0.020	-0.018
			(0.025)	(0.025)	(0.025)
Log-likelihood	-1649.3	-1649.2	-1649.0	-1648.9	--
Pseudo-R^2	0.067	0.067	0.067	0.067	--

Number of Observations = 15400

** indicates significance at the 1% level
* indicates significance at the 5% level

[1] 387 sectors defined by 43 industry categories and 9 geographic regions.

Note: Standard errors in parentheses. Other variables controlled for include educational attainment (5 category dummies), years of full-time work experience since age 18 and its square, number of children, and dummy variables for government employment, union membership, non-white, whether married, and MSA residence.

Table 3
Probit Regressions for Dismissals, 1976-91, Men
(Dependent Variable = 1 if laid-off/fired, 0 otherwise)
(Using sector unemployment)

Variable	(1)	(2)	(3)	(4)	(5) Random Effects
ln(real hourly wage)	-0.191**	-0.194**	-0.193**	-0.194**	-0.188**
	(0.037)	(0.037)	(0.037)	(0.037)	(0.039)
U.S. unemployment rate	3.453*	3.683*	3.477*	3.676*	3.884**
	(1.430)	(1.438)	(1.430)	(1.438)	(1.428)
Tenure	-0.053**	-0.081**	-0.043**	-0.073**	-0.072**
	(0.004)	(0.011)	(0.007)	(0.013)	(0.013)
Time trend	0.016**	0.006	0.016**	0.007	0.005
	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
Tenure*(time trend)	--	0.003**	--	0.003**	0.003**
		(0.001)		(0.001)	(0.001)
Sector unemployment [1]	3.402**	3.411**	4.015**	3.813**	3.605**
	(0.462)	(0.463)	(0.593)	(0.596)	(0.611)
Tenure*(sector unemployment)	--	--	-0.166	-0.109	-0.098
			(0.102)	(0.103)	(0.102)
Log-likelihood	-3268.3	-3263.4	-3266.9	-3262.9	--
Pseudo-R^2	0.109	0.111	0.110	0.111	--

Number of Observations = 22469

** indicates significance at the 1% level
* indicates significance at the 5% level

[1] 387 sectors defined by 43 industry categories and 9 geographic regions.

Note: Standard errors in parentheses. Other variables controlled for are the same as in Table 2.

Table 4
Multinomial Logit Regression by Reason for Job Change, 1976-91, Men
(Omitted Category = no change)

Variable	Quit	Plant Closed	Other Reason	Dis-missed
ln(real hourly wage)	-0.586**	-0.404**	-0.128	-0.487**
	(0.053)	(0.123)	(0.092)	(0.084)
U.S. unemployment rate	-6.204**	2.864	22.409**	13.940**
	(2.026)	(4.720)	(3.329)	(2.925)
Tenure	-0.127**	-0.084**	-0.063**	-0.222**
	(0.012)	(0.020)	(0.021)	(0.027)
Time trend	-0.020**	-0.021	0.018	0.010
	(0.007)	(0.018)	(0.015)	(0.012)
Tenure*(time trend)	0.006**	0.004	-0.006*	0.008**
	(0.001)	(0.002)	(0.003)	(0.002)
Δln(sector employment)[1]	0.317**	0.417*	-0.078	0.273*
	(0.085)	(0.195)	(0.163)	(0.127)
Tenure* (Δln(sector employment))	-0.030*	-0.045*	0.002	-0.075**
	(0.012)	(0.020)	(0.026)	(0.025)
Log-likelihood		-13911.8		
Pseudo-R^2		0.085		

Number of Observations = 22469

** indicates significance at the 1% level
* indicates significance at the 5% level

[1] 387 sectors defined by 43 industry categories and 9 geographic regions.

Note: Standard errors in parentheses. Other variables controlled for are the same as in Table 2.

Table 5
Probit (Dismissals) and Multinomial Logit (Job Change), 1976-91, Men
White-Collar Workers Only (excluding sales and service occupations)

	Probit	Multinomial Logit			
Variable	Dis-missed	Quit	Plant Closed	Other Reason	Dis-missed
ln(real hourly wage)	-0.203** (0.062)	-0.568** (0.076)	-0.396* (0.185)	-0.261 (0.150)	-0.619** (0.149)
U.S. unemployment rate	6.476* (2.663)	-0.576 (3.038)	-2.233 (7.853)	28.351** (5.279)	14.969* (6.174)
Tenure	-0.119** (0.023)	-0.100** (0.016)	-0.093** (0.033)	-0.088** (0.032)	-0.319** (0.061)
Time trend	0.007 (0.010)	0.012 (0.012)	-0.020 (0.029)	-0.006 (0.024)	0.016 (0.025)
Tenure*(time trend)	0.007** (0.002)	0.003* (0.001)	0.002 (0.003)	-0.003 (0.004)	0.019** (0.004)
Δln(sector employment)[1]	0.232* (0.114)	0.386** (0.131)	0.081 (0.322)	-0.453 (0.267)	0.591* (0.264)
Tenure* (Δln(sector employment))	-0.021 (0.015)	-0.044** (0.015)	0.007 (0.037)	0.062 (0.042)	-0.053 (0.038)
Log-likelihood	-958.3	-5563.8			
Pseudo-R^2	0.099	0.080			

Number of Observations = 10268

** indicates significance at the 1% level
* indicates significance at the 5% level

[1] 387 sectors defined by 43 industry categories and 9 geographic regions.

Note: Standard errors in parentheses. Other variables controlled for are the same as in Table 2.

Appendix Table A1
Summary Statistics (Means and Standard Deviations)

Variable	Men (1976-1991)	Women (1981-1991)
Completed grade 6, 7, or 8	0.040	0.019
	(0.197)	(0.136)
Completed grade 9, 10, or 11	0.116	0.088
	(0.321)	(0.283)
Completed high school	0.214	0.307
	(0.410)	(0.459)
Completed some college	0.353	0.357
	(0.478)	(0.479)
College graduate	0.182	0.168
	(0.386)	(0.374)
Graduate school	0.087	0.066
	(0.282)	(0.247)
Years of full-time work experience since age 18	14.143	9.312
	(10.904)	(8.756)
Government employment	0.203	0.245
	(0.402)	(0.430)
Union membership	0.246	0.135
	(0.430)	(0.342)
Non-white race	0.083	0.103
	(0.276)	(0.306)
Married	0.863	0.761
	(0.344)	(0.426)
Number of children	1.116	0.961
	(1.187)	(1.102)
MSA residence	0.684	0.676
	(0.465)	(0.468)
Real hourly wage at current job (1992 dollars)	17.172	12.386
	(16.633)	(16.484)

(continued)

Table A1 (continued)

Variable	Men (1976-1991)	Women (1981-1991)
Fired	0.039	0.024
	(0.193)	(0.154)
Quit	0.102	0.141
	(0.302)	(0.348)
Changed job for	0.025	0.022
other reasons	(0.156)	(0.146)
Plant closed	0.015	0.014
	(0.122)	(0.117)
Aggregate U.S. unemployment rate	0.070	0.070
	(0.013)	(0.014)
Tenure (years at current firm)	7.729	5.359
	(8.493)	(5.663)
% change in sector employment,	0.119	0.230
previous 10 years	(0.356)	(0.263)
Sector unemployment rate	0.051	0.040
(yearly labor force measure)	(0.034)	(0.027)
Number of observations	22469	15400

Standard deviations in parentheses.