IS THE BEHAVIOR OF HOURS WORKED CONSISTENT WITH IMPLICIT CONTRACT THEORY?*

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This paper examines the determinants of hours worked when employment relationships are influenced by risk-sharing considerations. The environment considered is an extension of the standard symmetric-information risk-sharing model that allows for the possibility of enforcement problems on the part of both the employer and the employee. We show that this class of risk-sharing models unambiguously predicts hours to be influenced by wages only through an income effect. Using data from the PSID, we find evidence in favor of this extended version of the risk-sharing model.

I. INTRODUCTION

Contract theory has developed largely as an alternative to the failings of the spot market model of the labor market. One dimension in which the shortcomings of the spot market model have been widely discussed, and in which contract theory has been suggested as a potential explanation, is with respect to temporal variations in hours worked. In a spot market model, changes in hours worked should reflect only changes in wages and preferences. However, the vast empirical literature that has tried to identify the links between wages and hours predicted by the spot market model has been rather unsuccessful (see Pencavel [1986] or Card [1991] for an assessment of this literature). In fact, much of the movement in hours worked is observed at a fixed wage rate [Abowd and Card 1989], and aggregate variables have been found to predict changes in hours even after conditioning on wage changes [Ham 1986]. Although such evidence seems at odds with the spot market model, it might well be consistent with contracting models since these models predict a decoupling between observed wage payments and hours worked.

Nevertheless, before ascribing any virtues to the contractual paradigm, it is necessary first to derive a set of testable predictions about hours worked that come naturally from contract models, and second to test these predictions. In this paper we strive to do both. Specifically, we propose a framework for examining the determi-

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nants of hours worked when employment relations are influenced by risk-sharing considerations. The environment we examine extends the standard symmetric-information risk-sharing model by allowing for the possibility of enforcement problems on the part of both the employer and the employee. In terms of the partial correlation between hours and wages, we show that within this class of models, wages never play a direct allocation role beyond that prescribed by income effects. With current wages decoupled from current marginal product, movements in hours are primarily driven by variations in the value of the worker's marginal product: wage changes induce only income effects.

In order to estimate the model, we exploit different instrumental-variable strategies suggested by the decomposition of variance for wages inherent in the model. The testing of our identification restrictions allows us to examine the validity of our assumptions regarding the nature of the contractual environment. Using data from the PSID, we find evidence that the temporal variation in hours worked is consistent with the predictions of the risk-sharing model that includes the possibility of commitment constraints.

This paper is closely related to the study of wage behavior presented in Beaudry and DiNardo [1991]. In that study we examined whether the cyclical movements in real wages are more consistent with a spot market model or a contractual framework. We found that individual wages moved with market conditions in a manner particularly consistent with a contract model in which there are enforcement constraints. In fact, we found that an individual's real wage tends to increase when labor market conditions improve but remains unchanged when labor market conditions deteriorate. We interpreted this pattern as evidence in favor of a contractual structure of the labor market of the type presented in Harris and Holmstrom [1982]. The current paper can be viewed as pursuing the predictions of this class of models one step farther by examining restrictions imposed on the joint behavior of hours and wages.

The remaining sections of the paper are structured as follows. In Section II we present a multiperiod risk-sharing model under symmetric information and show how the determination of hours worked can be analyzed in this framework. We take particular care to discuss how parameters of the model can be identified using an individual-level panel data set on employment histories. Section III discusses the data we use to estimate and test the model, Section IV describes our empirical strategy, and Section V presents results. Finally, Section VI concludes.

II. Wages and Hours in a Risk-Sharing Contract Model

In this section we provide a general framework for analyzing the determinants of hours worked in the presence of implicit risk-sharing contracts.[1] We consider an environment in which firms have access to a perfect capital market but workers do not. In contrast to the standard risk-sharing models of Azariadis [1975] and Baily [1974], however, we do not impose a priori that all contracts be perfectly enforceable. Instead, we allow enforcement problems to affect the design of optimal contracts as in Harris and Holmstrom [1982] and Thomas and Worrall [1988]. In the implicit contract setting, allowing for the possibility of enforcement problems seems particularly important since contracts are assumed to be enforced only by reputation or good will.

Consider the determination of a contract for worker j who is searching for employment at time t. In general, a contract will specify a sequence of wage-hour contingencies, where a time $t + i$ contingency is a complete description of events leading up to and including time $t + i$. Assume that each period's uncertainty is summarized by the random variable θ_t, and let $\theta^{t+i} = \{\theta_0, \ldots, \theta_{t+i}\}$ denote the time $t + i$ contingency. Furthermore, let the worker's marginal productivity at time $t + i$ be denoted by $\psi(\theta^{t+i}, X_{j,t+i})$, where $X_{j,t+i}$ represents the worker's personal characteristics. Given this environment, a contract agreed upon at time t needs to specify a pair of functions $\{w(\theta^{t+i}, \theta^t, X_{j,t+i}),\ h(\theta^{t+i}, \theta^t, X_{j,t+i})\}$. For example, $w(\theta^{t+i}, \theta^t, X_{j,t+i})$ is the hourly wage paid in contingency θ^{t+i} in a job that began in contingency θ^t for a worker with characteristics $X_{j,t+i}$. It is useful to note explicitly the dependence of wages and hours on both θ^{t+i} and θ^t since both hours and wages may depend on the current contingency, as well as the contingency in which the relationship began.

If we assume that all contracts are enforceable, then competition among employers will lead to an outcome that can be represented by the worker's preferred contract among all those rendering nonnegative profits to the employer. The assumption of complete enforceability of contracts underlies most of the early implicit contract literature. However, if either the worker or the employer can renege on an agreement without having infinite penalties imposed, then such a "first-best" (perfect risk-sharing) contract may not be achievable. Instead, without full enforcement the equilibrium contract will be constrained to belong to the set of contracts that both the worker and the employer can be expected to

1. For an introduction to this class of models, see Rosen [1985].

respect. In order to account for enforcement problems, we explicitly add commitment constraints to the standard risk-sharing problem. For example, if $\hat{U}(\theta^{t+i},X_{j,t+i})$ represents the expected lifetime utility that the worker would obtain after he reneges on a contract in state θ^{t+i}, then the commitment constraint in state θ^{t+i} can be expressed as forcing the worker's payoff to be at least as great as $\hat{U}(\theta^{t+i},X_{j,t+i})$. Similar constraints can be imposed on the employer's payoff where his reservation payoff is given by $\hat{\Pi}(\theta^{t+i},X_{j,t+i}) < \infty$.[2]

The equilibrium contract offered to a worker in state θ^t, taking into account the possibility of enforcement problems, is given by the solution to the following maximization problem. For notational convenience, the dependence of hours and wages on personal characteristics is subsumed in subscript j. We also assume that a worker's preferences are time-separable and firms have access to constant returns to scale technology. These last two assumptions can be relaxed but are maintained to allow easy comparison with the bulk of the empirical literature on intertemporal labor supply. The maximization problem is

$$\max_{\{w_j(\theta^{t+i},\theta^t),h_j(\theta^{t+i},\theta^t)\}} E_{\theta^t}\left[\sum_{i=1}^{\infty}(\beta)^i U(w_j(\theta^{t+i},\theta^t)h_j(\theta^{t+i},\theta^t),h_j(\theta^{t+i},\theta^t))\right]$$

subject to

$$\text{(a)}\quad E_{\theta^t}\left[\sum_{i=0}^{\infty}\frac{\psi_j(\theta^{t+i})h_j(\theta^{t+i},\theta^t)-w_j(\theta^{t+i},\theta^t)h_j(\theta^{t+i},\theta^t)}{(1+R)^i}\right]=0$$

and for all θ^{t+k}

$$\text{(b)}\quad E_{\theta^{t+k}}\left[\sum_{i=k}^{\infty}(\beta)^i U(w_j(\theta^{t+i},\theta^t)h_j(\theta^{t+i},\theta^t),h_j(\theta^{t+i},\theta^t))\right]\geq \hat{U}_j(\theta^{t+k})$$

$$\text{(c)}\quad E_{\theta^{t+k}}\left[\sum_{i=k}^{\infty}\frac{\psi_j(\theta^{t+i})h_j(\theta^{t+i},\theta^t)-w_j(\theta^{t+i},\theta^t)h_j(\theta^{t+i},\theta^t)}{(1+R)^{i-k}}\right]\geq \hat{\pi}_j(\theta^{t+k}).$$

In this maximization, $U(\cdot,\cdot)$ is the per period utility function, β is the worker's discount factor, and R is the real interest rate. Constraint (a) in the above program is the statement that the firm

2. Note that in this formulation $\hat{U}(\theta^{t+i},X_{j,t+i})$ and $\hat{\Pi}(\theta^{t+i},X_{j,t+i})$ include any reputation effects associated with reneging on a contract. Moreover, $\hat{U}$ and $\hat{\Pi}$ are taken to represent reservation utilities and therefore are assumed to be independent of the state of the world at the time the worker entered into the current contract.

makes zero expected profits viewed from the beginning of the relationship. Although the assumption that firms have access to a risk-neutral contingent claims market is implicit in this formulation, the results of the paper are easily extended to the case where some risks are not diversifiable.

The set of constraints under (b) and (c) reflect the possibility of enforcement problems on the part of the worker and the employer. Constraint (b) states that, in every contingency, it must be in the interest of the worker to pursue the current relationship instead of reneging on the contract and obtaining his reservation utility. A similar statement applies to the restrictions imposed on the employer's payoff by constraint (c).[3]

PROPOSITION 1. In the optimal contract, wages and hours worked always satisfy the intratemporal efficiency condition given by (1):

$$\frac{-U_h(w(\theta^{t+i},\theta^t)h(\theta^{t+i},\theta^t),\, h(\theta^{t+i},\theta^t))}{U_c(w(\theta^{t+i},\theta^t)h(\theta^{t+i},\theta^t),\, h(\theta^{t+i},\theta^t))} = \psi(\theta^{t+i}). \tag{1}$$

Proof. All proofs are given in the Appendix.

Proposition 1 states that the standard efficiency condition between the intratemporal marginal rate of substitution and the marginal productivity of labor must always be satisfied at the optimum. Hence, the addition of commitment constraints does not create any trade-off between ex post efficiency and optimal risk sharing. Commitment constraints do not intervene with allocative efficiency since allocative efficiency relates only to an *intratemporal* problem. Commitment constraints, however, do relate to difficulties with carrying out *intertemporal* agreements. The intratemporal efficiency condition, therefore, is not affected by enforcement constraints. Equation (1) expresses a fundamental relationship between wages and hours implied by the implicit contract model; that is, risk-sharing contracts imply a decoupling of wages and productivity such that the only direct effect of wages on hours is through changes in the marginal rate of substitution between leisure and consumption. In other words, holding productivity constant, an increase in wages affects hours worked only because it affects consumption. Therefore, if leisure is a normal good, as we

3. It is important to note that we are assuming that the constraint set is not empty. This assumption is not very restrictive, and in particular it is satisfied if $\bar{U}$ and $\bar{\Pi}$ are explicitly modeled as in either Harris and Holmstrom [1982] or Thomas and Worrall [1988].

will assume throughout, an increase in wages should be associated with a fall in hours. The fall in hours arises because an increase in wages represents a pure income effect. By design, any substitution effect is eliminated by holding productivity constant. Intuitively, if market pressures force one's wage to increase in the absence of any increase in productivity, then part of that wage increase should be used to buy leisure time.

Given that we have stated our contractual problem in a rather general form, we believe the prediction that wages influencing hours only through an income effect is a very important implication of the symmetric-information risk-sharing models. Therefore, we adopt this prediction as the basis for examining this class of models.[4] Note that this relation between hours and wages is very different from that obtained from the intertemporal labor supply model. The canonical intertemporal labor supply model predicts that an increase in wages leads to an *increase* in hours holding the marginal utility of consumption is constant.

In order to examine this prediction empirically, it is helpful to consider a log-linear approximation to (1). It will also be useful to consider equation (2) in this first-differenced form for reasons we discuss in detail in the next section:

$$(2) \quad \Delta \log h_{j,t+i} = \Omega_1 \Delta \log w(\theta^{t+i},\theta^{t},X_{j,t+i}) + \Omega_2 \Delta \log \psi(\theta^{t+i},X_{j,t+i}),$$

where $\Omega_1 = \; < 0$.

Equation (2) is an exact implication of (1) when utility is of the form,

$$U(c,h) = (ce^{-\alpha h^{\gamma}})^{1-\sigma}/(1-\sigma).$$

Equation (2) helps clarify the implication of implicit contracts for the partial correlation between hours and wages growth. In particular, it highlights the potential difficulties associated with estimating such a relation. As discussed above, the coefficient on wage growth in this equation represents only the income elasticity of labor supply; by decoupling wages and productivity, contracts remove the standard allocation role played by wages. The obvious difficulty with estimating equation (2) and obtaining estimates of Ω_1, therefore, is in determining whether wages actually vary independently of productivity and how such variation can be

4. The tests of the implicit contract model performed by Abowd and Card [1987, 1989] are not based on equation (1). Instead these authors test whether the marginal utility of consumption is constant across time. This is a special case of the above model when there are no enforcement problems.

identified. In particular, in the absence of perfect measures of productivity, the estimation of (2) requires instruments for wage growth that are uncorrelated with productivity growth. In order to uncover an identification strategy for estimating Ω_1, it is therefore useful to examine the implications of our model for the process governing wages.[5] These implications are summarized in Proposition 2.

PROPOSITION 2. When implicit contracts are subject to enforcement constraints, wage payments are history dependent and inherit both a time-of-entry fixed-effect and a time-varying time-of-entry effect.

Proposition 2 indicates that wages governed by implicit contracts have two sources of systematic dependence on history beyond that contained in current productivity. First, wages exhibit history dependence because a worker who enters a relationship in a more favorable market condition will generally receive superior contract offers, and therefore wage payments are likely to be relatively high throughout his contract. We call this effect a time-of-entry fixed-effect since it refers to an effect that is related to the state of the world that was realized at the time the worker began the job. Second, there is a time-varying time-of-entry effect that arises because of enforcement problems. This history dependence in wages reflects the possibility that a change in productivity will have different effects on wages depending on the period in which one entered a job. The economic mechanism at play behind the time-varying time-of-entry effect is that an increase in productivity is more likely to cause the commitment constraint to bind for a worker who entered in a relationship in a relatively unfavorable time since contracts on the market are likely to become more attractive than the one he possesses. Therefore, an increase in productivity is likely to lead to a larger wage increase for a worker who entered in a unfavorable time in comparison with a worker who entered in a favorable time. In particular, such a mechanism creates cross-sectional variation in wage growth that is systematically related to the time one entered a job.

In order to help clarify the implications of Propositions 1 and 2, Figures I and II depict time paths for contractual wages and hours calculated from a two-state example where worker A was

5. Ω_1 is a function of both the intertemporal elasticity of substitution in consumption and the intertemporal elasticity of substitution in leisure. Therefore, estimates of Ω_1 could be used to derive confidence intervals for these parameters.

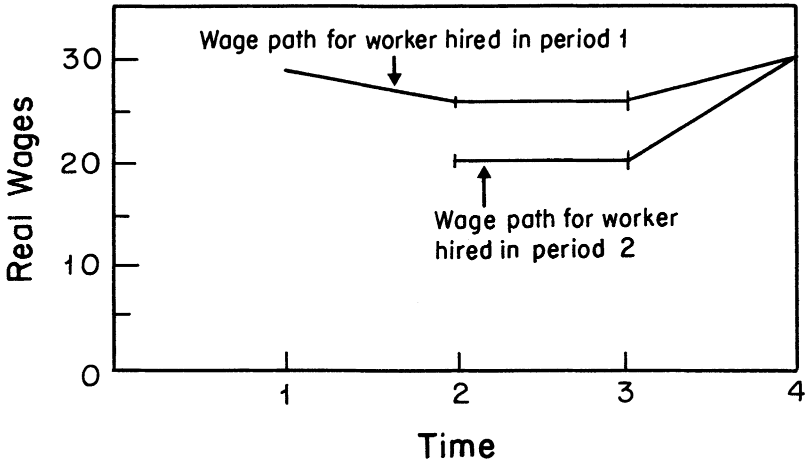

FIGURE I
Wage Path
Periods 1,4 are high productivity (30/hr). Periods 2,3 are low productivity (20/hr).

hired in a good state in period 1 while worker B was hired in the less productive state in period 2. In the example, productivity is \$20 per hour in the bad state and \$30 per hour in the good state. The workers are assumed to have no commitment technology; that is, they can always switch employers at no cost. In contrast, the employer is assumed to be able to commit to short-term losses.[6] In the figures, periods 2 and 3 are periods of low productivity, while periods 1 and 4 are periods of high productivity.

The most insightful aspects of these figures are the pattern of wages and hours in periods 3 and 4. In period 3 worker A is paid more than worker B (\$25.77 per hour instead of \$20 per hour) since he was hired in a better state and is currently receiving his "insurance" premium. Correspondingly, worker A works less than worker B because of the associated income effect (776 hours instead of 1000 hours). This illustrates how time-of-entry fixed-effects create cross-sectional variation in the levels of hours and wages and why these time-of-entry effects have effects on wages and hours that are opposite in sign. In period 4 the state of technology changes from bad to good. The contract wage for both

6. The utility function used in the example is $(ce^{-.001/h})^{.3}/.3$, the discount rate is .1, the maximum loss allowed by an employer is \$7000 per period, and the transition probabilities are .6 from the bad state to the good state and .2 from the good state to the bad state.

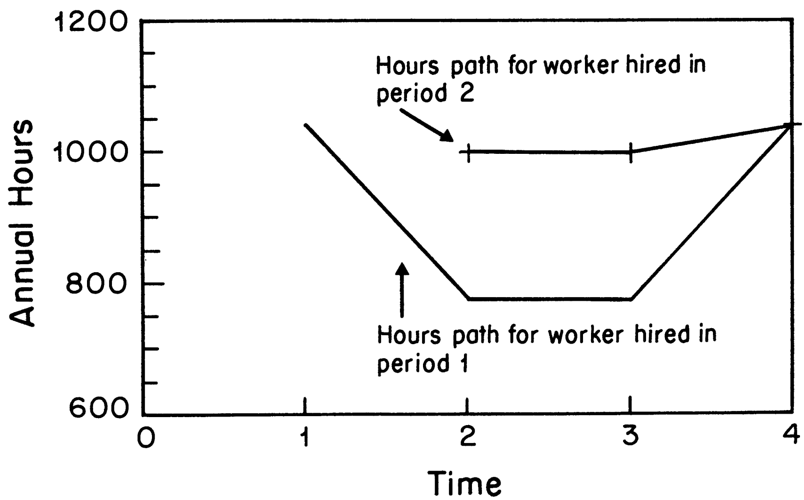

FIGURE II
Hours Path

workers increases to \$28.79 per hour, which implies that the wage and hours growth observed between periods 3 and 4 depends on when the worker entered into a job. This shows how the time-varying time-of-entry effect creates cross-sectional variation in both wages and hours growth; again the impact on wages is the opposite of the effect on hours. The wage increase for worker B is larger than for worker A because B's wage needs to be increased more to stop him from accepting an outside offer. In contrast, hours increase less for worker B than for A (an increase of 42 hours instead of an increase of 267 hours) due to the larger income effect. Note that in spite of the income effect hours are increasing for both workers between periods 3 and 4 because of the substitution effect induced by the change in productivity.[7]

The history dependence for wages given by Proposition 2, and illustrated in Figure I, suggests two instrumental-variable strategies for estimating equation (2). When enforcement problems are not relevant, the only source of variation in wages that is independent of productivity changes is the time-of-entry fixed-effect. This suggests that Ω_1 can be estimated by exploiting (in individual level

7. This example highlights the fact that negative correlation between hours and earnings examined by Abowd and Card [1987] is not a prediction of implicit contract theory that is robust to the addition of enforcement constraints.

data sets) the difference in wage changes at times of job change across workers who are leaving jobs they began at different times. In practice, such an instrumental-variable strategy for identifying Ω_1 can be implemented by using changes in time-of-entry dummy variables as instruments for wage growth. Such an identification strategy is based on exploiting observations on job switchers.

When enforcement problems are relevant, there is a second source of variation in wages that is independent of productivity. This source of variation corresponds to the time-varying time-of-entry effects, and suggests that independent variation in wages can be identified by exploiting the cross-sectional variation in on-the-job wage growth associated with different year-of-entry-into-a-job cohorts. In this case, a whole set of year-of-entry *cross*-year dummies can be used as instruments for wage growth.

Note that the evidence presented in Beaudry and DiNardo [1991] and Jacobson, LaLonde, and Sullivan [1993] can be interpreted as providing evidence to support the particular history dependence in wages which is at the core of our proposed instrumental-variable strategy for estimating (2). In effect, in Beaudry and DiNardo [1991] we found significant evidence of such history dependence when the time-of-entry effect is parameterized by the unemployment rate at the time the person got his job and the time-varying time-of-entry effect is parameterized by the lowest unemployment experienced since the worker began his job. The current suggestion of using dummy variables to capture the history dependence is simply a less restrictive approach than that of parameterizing the history dependence as a function of unemployment rates.

In summary, the risk-sharing model predicts that (1) holding productivity constant, changes in hours should be negatively related to wage changes since wage changes only induce income effects; and (2) variations in wages that are independent of variations in productivity can be identified by exploiting information on the time-of-entry cohorts, both through the variation in the time-of-entry fixed effects and through the time-varying time-of-entry effects.[8] Therefore, the implications of implicit contracts for the behavior of hours worked can be estimated within the context

8. It is worth noting that the above predictions also hold with respect to the joint behavior of consumption and hours worked; that is, the model also predicts time-of-entry effects in consumption growth to be negatively related to changes in hours worked. We have chosen to concentrate on the prediction regarding the joint behavior of hours and wages given availability and quality of data.

of equation (2) by first using time-of-entry information to instrument the wage then examining whether the resulting estimate of Ω_1 is negative. Furthermore, the validity of the proposed instruments can be examined by the use of an overidentification test.

The above empirical strategy proposes to test a class of implicit contract models without specifying any particular alternative hypothesis. It is interesting to consider, however, whether this approach has any power with respect to rejecting the model if the alternative is a spot market model. The answer to this question depends on how much structure one places on the alternative. If we allow ourselves the freedom under the alternative to interpret any time-of-entry effect in wages as reflecting productivity variations (of unknown source),[9] then the IV estimate we obtain by regressing hours growth on predicted wage growth is simply a combination of an income effect and a substitution effect. Moreover, if we are completely agnostic with respect to the relative size of income and substitution effects, then obviously our approach has no power with respect to this alternative (since, in this case, the alternative places no restrictions on the joint behavior of hours worked and wages). However, if one assumes that the long-run elasticity of labor supply is zero, then under a spot market model one should expect to find a nonnegative effect of wages on hours using our instrumental-variable strategy since the substitution effect should be at least as great as the income effect.[10] In this case, our approach has power against this alternative.

Before estimating equation (2), it is useful to consider the extent to which our proposed approach is robust to unobservable heterogeneity. The previous discussion implicitly assumes that time-of-entry information is not systematically related to firm or worker quality. This assumption would in general be valid if turnover was strictly exogenous. However, there may be reason to question this assumption. For example, it may be the case as found by Solon, Barsky, and Parker [1994] that different quality workers are hired at different stages of the business cycle, or that particular types of firms hire in booms instead of in recessions. In effect, as we discuss below, such issues may indicate the need for additional controls when estimating equation (2).

9. In the case of a spot market model, there is no obvious reason why individual productivity, and therefore wages, should exhibit any time-of-entry effects. The finding of such effects may therefore by itself be considered evidence against a spot market model. This is the line of reasoning adopted in Beaudry and DiNardo [1991].

10. This inference assumes that any time-of-entry effect in current wages is associated with at most a one-to-one increase in future wages.

Consider first the issue of unobserved worker quality. If workers of different quality tend to be hired at different stages of the businesses cycle, then time-of-entry will be an indicator of productivity. However, if worker quality is interpreted as an individual-specific fixed-effect (as is standard in the labor literature), it does not cause any problem for the estimation of equation (2) using time-of-entry information as instruments. In fact, our proposed instrumental-variable procedure remains appropriate in this case since, by considering variables in growth rates, the individual-specific fixed-effect in productivity is eliminated. This is the reason why we specified equation (2) in log-differences instead of in terms of log-levels. Hence, our proposed approach is robust to the presence of time-invariant individual-heterogeneity.[11]

In addition to unobserved worker quality, selection issues over the business cycle may lead time-of-entry information to be correlated with firm-specific characteristics. This would be the case if firms in certain industries hire disproportionately more at particular phases of the business cycle. One way to handle this is to include industry dummies when estimating (2). Yet it may also be possible that workers taking jobs in good times may be treated differently because these jobs may be in more cyclically sensitive sectors. For example, if hours increase more in high wage cyclical sectors in boom times, this will induce a negative correlation between wage and hours changes that will not reflect the contracting considerations which are our primary concern. To control for this possibility, we actually need to let mean wage and hour growth vary by sector and by year. This can be achieved by including a set of industry-cross-time dummies in all our regressions. Therefore, by adding industry-cross-time dummies to equation (2), we attempt to control for any significant time variation in industry-specific heterogeneity and thereby better isolate the effects we are interested in.

III. THE DATA

We investigate our extended implicit contract model with data drawn from the Panel Study of Income Dynamics (PSID). Specifi-

11. Unobserved individual heterogeneity may affect productivity in a manner more subtle than that captured by a standard fixed-effect representation. Although we cannot rule out such possibilities, we believe it unlikely to be a problem. We performed a number of experiments to see whether observed heterogeneity (especially education) was correlated with simple summary measures of conditions at the time of entry. We found very small (usually insignificant) and unstable relationships between the measures.

cally, we use data on real wages and total annual hours for the years 1976–1987 of male heads of households who were heads of households at the time of the 1987 interview. An unusual feature of the PSID is that for a subset of workers two sets of wage observations are available. The wage measure used in the vast majority of empirical studies is constructed by dividing a measure of annual earnings by annual hours. Because this measure is available for almost all employed workers in the data set, we naturally report results using this measure. However, one possible problem with this measure is division bias, which might result if persons had reasonably accurate reports of their previous year's earnings, but less accurate measures of their previous year's annual hours. In that case, the measurement error would introduce a spurious negative correlation between log hours and log wages (which is just log earnings less log hours). Therefore, we also examine the robustness of our results by using a "point-in-time" measure of wages, that is, a measure of hourly wages reported at the time of the interview. The alternative point-in-time measure of wages is from the employment battery of the PSID and is a direct question about hourly wages in the current period. This measure can then be appropriately aligned with the hours questions that refer to the previous year. The major drawback of this measure is that it is available for only about half the sample.

In constructing our sample, we excluded any individual who was self-employed any time during the sample period, as well as those who were less than 21 years or more than 71 years of age in 1987. Furthermore, yearly observations were also dropped if the individual reported more than 4680 hours or no hours of work, began their job before 1948, or had other missing information.

In order to determine the year a worker entered into a job, we needed to use the reported information on tenure.[12] Since the tenure information is less reliable in the early waves of the PSID, we use only the waves after 1976. Table I reports the means and standard deviations for the data sample used in this study.

12. It is widely known that there are difficulties in using the PSID question on tenure in the current job. In particular, the question is not asked in 1979 and 1980 (the alternative question asked is how long have you been in this present position) and the data are not always consistent within or across jobs. We used the same procedure as in Beaudry and DiNardo [1991] and follow a suggestion of Brown and Light [1992] and force both our tenure and age measures to be consistent across years. We ran a number of experiments with the original measures, and do not find our results sensitive to that precise treatment of tenure.

TABLE I
SAMPLE STATISTICS

Variable	Mean	Standard deviation
Years of school	12.53	3.10
Log annual hours	7.59	0.413
Log average hourly earnings in cents (1982 CPI)	7.02	0.516
Union status	0.35	0.478
Experience	21.95	11.53
Married	0.88	0.325
Tenure (months)	148.87	99.69
Number of children	1.33	1.27
Number of observations: 15,684		

IV. EMPIRICAL IMPLEMENTATION

In Section II we examined how the presence of implicit contracts restricts the joint distribution of wages and hours worked. In particular, we showed that, controlling for productivity, wage changes should be negatively correlated with hours in a risk-sharing relationship since wage changes only create income effects on labor supply. In order to examine this prediction without access to direct measures of productivity, we use the instrumental-variable strategy discussed previously. Moreover, we pay attention to testing our identifying restriction since our interpretation of the effect of wages on hours worked is only valid if the instrumental-variable strategy is appropriate. Our estimations are based on equation (3), a specification of equation (2) that explicitly accounts for the possibility that productivity be related to the length of tenure on a job and to general work experience. We performed all estimation with the data in differences to avoid biases caused by unobserved time-invariant individual heterogeneity. The equation we estimate is

$$\text{(3)}\quad \Delta \log h_{j,t+i} = \Omega_1 \Delta \log w_{j,t+i} + \sum_{k=1}^{k=K} \Omega_2^k \,\Delta \log A_{j,k,t+i} + \Omega_3 \Delta Exp_{j,t+i}^2 + \Omega_4 \Delta Ten_{j,t+i} + \Omega_5 \Delta Ten_{j,t+i}^2 + \Omega_6 \Delta Z_{j,t+i} + \epsilon_{j,t+i}.$$

To derive equation (3), the productivity term ψ from equation (2) is decomposed into a time-varying industry-specific productivity effect $A_{j,k,t+i}$, a quadratic experience profile, and a quadratic tenure profile. The terms $Ten_{j,t+i}$ and $Exp_{j,t+i}$ represent the level of tenure and job experience held by individual j at time $t + i$. The time-varying productivity term, $A_{j,k,t+i}$, is accounted for by including a fully flexible contemporaneous year-cross-industry effect in the regression, where the index k represents the industry of employment and K is the total number of industries. The data allow us to include twelve industry dummies in each yearly regression, which is meant to control for any important industry differences in wage cyclicality.

The tenure and experience terms are included in equation (3) to capture the average effect on productivity of the accumulation of general and firm-specific human capital.[13] The matrix Z contains union and marital status as additional controls. Finally, the error term $\epsilon_{j,t+i}$ captures any stochastic changes in person-specific productivity, tastes, and measurement error.

In general, there will be a nonzero correlation between wage growth and $\epsilon_{j,t+i}$ because of either person-specific productivity changes or correlation of measurement error with wages. The history dependence of wages implied by Proposition (2) suggests two sets of instruments for this situation. Our empirical strategy consists of estimating Ω_1 using these instruments and reporting Basmann-overidentification test statistics [Basmann 1960] associated with the validity of such procedure. The Basmann-overidentification test corresponds to testing whether the instruments are correlated with the error term from equation (3). This test is computed by performing an F-test on the significance of the excluded instrumental variables in the auxiliary regression of the 2SLS residuals on all the instrumental variables.[14]

Since the unit of observation is annual hours, the results might be biased by the fact that upon switching jobs a worker often goes through a period of search-unemployment which should preferably be excluded from our analysis. Therefore, all our estimates of equation (3) use data that correspond to differences over two years and where workers who switched jobs in either year

13. In general, we could allow a fully flexible tenure profile in equation (3) without losing identification since Ω_1 can be identified using only the time-varying component of the tenure profile.

14. For the pooled overidentification test, we perform the analogous F-test summing the appropriate residuals across the different years. Since the panels are not independent, however, we calculated the probability value by bootstrapping the F-statistic. We then use the bootstrapped distribution of F-statistics to calculate a percentile value.

t or $t + 2$ are excluded from the sample.[15] However, we found a very similar pattern of results when we looked at one-year differences as long as we excluded switchers in either year t and $t + 1$.

V. RESULTS

We report our results in two parts. First, we estimate equation (3) using the two measures of wages, that is, both the conventional average hourly wage measure and the point-in-time measure. Next we consider modifications and extensions to equation (3), again reporting results using both measures for wages. Finally, we discuss the economic content of our estimates and evaluate their plausibility.

1. Estimating and Testing Baseline Specification

Tables II and III report estimates of Ω_1 based on the specification presented in (3). Table II is based on the conventional measure of wages (average earnings divided by average hours), and Table III compares results for the two different measures of wages. Note that the set of excluded instruments used in Tables II and III are, for each cross section, a full set of year-of-entry dummy variables in levels and in differences.[16] We report results only for the estimates of Ω_1 since the coefficients on other (nondummy variable) regressors explain little of the variation in changes in hours.[17]

The IV estimates of Ω_1 in Table II, which are based on the conventional measure of hourly wages, range from a $-.084$ to $-.569$. The pooled estimate reported in the last row of the table is $-.291$ and is highly significant. Note that the finding of a signifi-

15. It is worth noting that our estimating strategy identifies Ω_1 from individuals who are switching jobs in the same year, who do not switch again in the following year, and who did not begin their previous job in the same year. This sampling design greatly complicates computation of statistics for the full sample since it is highly unbalanced. To compute the pooled estimate, we minimize the quadratic form $(\beta - \mu i)'V_\beta^{-1}(\beta - \mu i)$, where β is the ten estimated coefficients, μ is the pooled estimate, i is a vector of ones, and V_β^{-1} is the inverse of the empirical covariance matrix of the β's from 200 bootstrap replications of each of the ten coefficients (2000 β's in all). The procedure allows for the fact that repeated observations from the same individual are not i.i.d. Extensive experimentation at an earlier stage of the paper provided evidence that 200 replications were adequate to get reliable estimates of the covariance matrix.

16. In effect, the instruments used are the year-of-entry dummy variables corresponding to the job in the base year of the difference operator interacted with a dummy variable that indicates whether or not an individual switched employers during the two-year period under study.

17. The finding that hours changes are not systematically related to individual specific characteristics is consistent with the results reported in Abowd and Card [1989].

TABLE II
ESTIMATION OF BASELINE SPECIFICATION: CONVENTIONAL WAGE MEASURE

Year	Ω_1	Standard error	OID(P-value)	Number of observations
1978	−.084	(.114)	0.994	1226
1979	−.479	(.111)	0.117	1338
1980	−.154	(.118)	0.002	1410
1981	−.474	(.109)	0.000	1528
1982	−.249	(.086)	0.181	1609
1983	−.295	(.125)	0.097	1655
1984	−.569	(.087)	0.026	1681
1985	−.350	(.097)	0.025	1718
1986	−.015	(.111)	0.122	1746
1987	−.448	(.080)	0.000	1773
Pooled	−.291	(.058)	0.000	15684

Table II reports IV estimates of the effect of wage growth on hours growth. Excluded instruments are the level and change of year-of-entry dummies. Other variables included in the regression are tenure, tenure squared, experience, union status, gender, and time-cross-industry dummies. OID(P-value) refers to the probability value of the Basmann test for the validity of the exclusion restrictions.

cantly negative estimate for Ω_1 corresponds to the prediction of our implicit contract model.

One possibility that must be considered whenever an instrumental-variable estimator is used is the possibility of spurious results. This is especially relevant for results in Table II given that the raw correlation between changes in hours and wage changes is potentially contaminated from measurement error induced by the division bias associated with the way wages are constructed (annual earnings divided by annual hours). Therefore, it is appropriate to question whether these results may reflect small sample problems associated with instrumental-variable estimation. In order to assess this possibility, Table III presents IV and OLS results for the subsample for which both the conventional measure and the point-in-time measure of wages are available.

Several interesting results emerge from comparing the IV and OLS estimates for the two wage measures. First, the choice of sample does not seem to affect the results, and replicating Table II's results with this restricted sample does not significantly change the point estimate of Ω_1. (Compare the results in the column labeled Conventional IV with the results in Table II.) Second, the point estimates of Ω_1 using the conventional measure of wages are negative and similar for both the IV and OLS

TABLE III
COMPARING RESULTS WITH DIFFERENT WAGE MEASURES

Wage measure	Point-in-time		Conventional	
Estimation method	OLS	IV	IV	OLS
Year				
1978	.11	−.13	−.11	−.27
	(.09)	(.24)	(.17)	(.05)
1979	.10	.09	−.43	−.61
	(.08)	(.25)	(.15)	(.04)
1980	−.07	−.25	−.26	−.42
	(.07)	(.21)	(.12)	(.03)
1981	−.07	−.60	−.33	−.54
	(.08)	(.23)	(.14)	(.04)
1982	−.08	−.21	−.31	−.35
	(.08)	(.20)	(.16)	(.04)
1983	−.09	−.07	−.46	−.37
	(.10)	(.27)	(.19)	(.05)
1984	−.15	−.52	−.57	−.33
	(.07)	(.16)	(.09)	(.04)
1985	−.01	−.15	−.31	−.26
	(.06)	(.14)	(.08)	(.03)
1986	.04	−.40	−.21	−.37
	(.07)	(.14)	(.10)	(.03)
1987	−.04	.02	−.29	−.33
	(.05)	(.14)	(.07)	(.03)
Pooled	−.02	−.21	−.38	−.36
	(.03)	(.06)	(.05)	(.03)

Table III reports OLS and IV estimates of the effect of wage growth on hours growth using two measures of wages. Point-in-time refers to the direct question on hourly wage rates from the employment battery of the PSID. Conventional refers to the wage measure calculated as annual earnings divided by annual hours. Instrument set and specification is identical to that in Table II. Standard errors are in parentheses.

estimates.[18] It is this type of evidence that suggests that the results in Table II may be driven by measurement error.

However, the first two columns of Table III provide strong evidence against the hypothesis that the results reported in Table II are in fact spurious. In particular, the OLS estimates of Ω_1 using the point-in-time measure of wages are frequently positive and now hover very close to zero. In contrast, the pooled IV estimate of Ω_1 using the point-in-time wage is significantly negative and not significantly different from the pooled estimate reported in Table II. In fact, the OLS estimate for the pooled sample is −.02 (with a

18. The negative OLS estimates might appear surprising, but are really quite standard. See, for example, Abowd and Card [1987, 1989].

standard error of .03), whereas the pooled IV estimate is a significantly more negative −.21 (with a standard error of .06).

Notwithstanding the above positive results for a contractual view of the labor market, we must point out that the model's overidentifying restrictions are generally rejected by the data. For example, Table II reports that in six out of the ten cases, the Basmann statistic implies rejection of the overidentifying restrictions at the 5 percent level and decisively rejects the restrictions for the pooled sample. That is, the Basmann statistic rejects the hypothesis that the only effect of our instruments on hours is through their effect on the wage. We obtained a very similar pattern of results when using the point-in-time measure of wages. Consequently, before assessing the success or failure of our model, it seems necessary to explore different reasons to explain why we might observe these rejections of the overidentifying restrictions.

2. Exploring Extensions of the Baseline Model

In this subsection we explore three potential explanations for the negative results associated with our testing the overidentifying restrictions implied by the model. A first possibility is that the model may not be an appropriate description of behavior in both the union and nonunion sectors. One obvious difference between the two sectors is that wages in the union sector are negotiated collectively for all workers, typically at the same time. Furthermore, much research suggests that unions typically mitigate the impact of business cycle fluctuations. This could lead to rejection of the overidentification restrictions and possibly affect the magnitude of our estimates of Ω_1. If this is true, then it is possible that our framework is not appropriate for the union sector.

Table IV repeats the exercise of Table II, this time excluding observations when a worker's wage is covered by a collective bargaining agreement. One immediate observation from this table is that the estimates of Ω_1 are slightly more negative for this subsample of workers. The second observation is that the overidentification test passes much more easily in this case, suggesting that the model may indeed only be appropriate for the nonunion sector.

A second possibility that may account for the rejection of the overidentifying restrictions is that movements across jobs may induce hours and wage fluctuations not accounted for by the risk-sharing considerations developed in our model. Altonji and Paxson [1986], for example, have found that the variance in hours

TABLE IV
ESTIMATION EXCLUDING UNION WORKERS

Year	Ω_1	Standard error	OID(*P*-value)	Number of observations
1978	−.086	(.133)	.984	759
1979	−.590	(.108)	.864	849
1980	−.197	(.126)	.059	895
1981	−.292	(.113)	.225	987
1982	−.372	(.081)	.076	1043
1983	−.441	(.139)	.288	1079
1984	−.544	(.107)	.004	1098
1985	−.205	(.082)	.486	1130
1986	−.154	(.137)	.333	1165
1987	−.583	(.085)	.0003	1205
Pooled	−.329	(.053)	.240	10,210

Table IV reports IV estimates of the effect of wage growth on hours growth using conventional wage measure. Instrument set and specification is identical to that in Table II. OID(*P*-value) refers to the probability value of the Basmann test for the validity of the exclusion restrictions.

worked is much greater for job switchers than for job stayers. One approach is merely to eliminate observations of workers switching jobs. (Note that we have already dropped hours observations in period t or $t - 2$ that involve periods of job changing. Now we also drop observations that involved switching in period $t - 1$.) However, this approach, which we discuss below, means that we cannot use differences in time-of-entry effects as instruments since they are identical to zero for such workers. Before we excessively reduce our sample, we present results in Table V that augment Table II by introducing a switcher dummy that varies across time in both the hours and wage equations. While the point estimates in Table V are very similar to the point estimates in Table II, the overidentification restrictions are rejected less frequently. In particular, these restrictions are not rejected for the pooled sample. Therefore, this suggests that the previous rejections of the overidentifying restrictions may have been caused by a systematic relationship between hours changes and wage changes for switchers which, for example, may reflect time variation in unobserved individual heterogeneity that induces the observed switches.

Last, Table VI displays results from the estimation of equation (3) under the assumption that there is a time-of-entry fixed-effect in productivity. This possibility might arise due to the selection of different types of jobs over the business cycle. In this situation, because of firm-specific heterogeneity, the hours equation may

TABLE V
ESTIMATION INCLUDING TIME-VARYING SWITCHER EFFECT

Year	Ω_1	Standard error	OID(P-value)	Number of observations
1978	−.116	(.114)	.984	1226
1979	−.500	(.110)	.236	1338
1980	−.246	(.116)	.001	1410
1981	−.460	(.110)	.000	1528
1982	−.287	(.091)	.013	1609
1983	−.284	(.125)	.116	1655
1984	−.550	(.088)	.012	1681
1985	−.444	(.098)	.870	1718
1986	−.011	(.111)	.136	1746
1987	−.526	(.080)	.006	1773
Pooled	−.342	(.054)	.190	15,684

Table V reports IV estimates of the effect of wage growth on hours growth using conventional wage measure. Instrument set and specification are identical to that in Table II with the addition of time-cross-switcher dummies in the set of regressors. OID(P-value) refers to the probability value of the Basmann test for the validity of the exclusion restrictions.

inherit a time-of-entry fixed-effect beyond that accounted for by wages. Therefore, to properly estimate Ω_1, it is necessary to either directly include changes in the year-of-entry dummy variables in the hours equation or simply delete from the sample all observations that involve job switchers. In other words, changes in

TABLE VI
ESTIMATION WITH ONE SET OF INSTRUMENTS

Year	Ω_1	Standard error	OID(P-value)	Number of observations
1977	−.046	(.146)	.980	1226
1978	−.621	(.130)	.255	1338
1979	−.174	(.125)	.997	1410
1980	−.725	(.149)	.266	1528
1981	−.217	(.205)	.833	1609
1982	−.488	(.229)	.754	1655
1983	−.395	(.156)	.991	1681
1984	−.512	(.160)	.999	1718
1985	−.326	(.208)	.168	1746
1987	−.363	(.159)	.330	1773
Pooled	−.384	(.059)	.993	15,684

Table VI reports IV estimates of the effect of wage growth on hours growth using conventional wage measure. Specification is identical to Table II except that the instrument set is restricted to the change in year-of-entry dummies. OID(P-value) refers to the probability value of the Basmann test for the validity of the exclusion restrictions.

year-of-entry dummy variables are inappropriate identifying variables in this case. Table VI provides the results for the case where year-of-entry dummy variables are included in the hours equation. (The results for the case where observations involving job switchers are excluded from the sample are similar.) It is important to note that in Table VI it is only the cross-sectional variance in the year-of-entry component in wage growth for job stayers that is identifying Ω_1. Moreover, this source of variation in wages is predicted to arise only if there are problems in enforcing contracts.

First note that in Table VI the overidentification test is again more readily accepted than in Table II. At the 5 percent significance level, the Basmann test is never rejected. Moreover, the estimates of Ω_1 are all negative as predicted by the theory.

In summary, Tables IV, V, and VI indicate that only slight modifications to our baseline specification are needed for the model's overidentifying restrictions to no longer be rejected by the data, and that with these modifications our estimates of Ω_1 remain significantly negative. This obviously provides considerable evidence in favor of our contractual model. Furthermore, let us emphasize that in all these tables, the identifying variables are highly significant in the first stage regression. This should not be surprising given the results on the pattern of history dependence in wages presented in Beaudry and DiNardo [1991].[19] It is also consistent with the evidence presented in Jacobson, LaLonde, and Sullivan [1993] which was obtained with very different data. In fact, the R^2's from the first-stage regression do not indicate the presence of a problem with our instrumental-variable estimates of the sort discussed in Nelson and Startz [1990]. In particular, Nelson and Startz [1989] recommend that the quantity NR^2 be greater than 2 in the first-stage regression. The smallest such quantity in the foregoing first-stage regressions is 25. Furthermore, the F-statistic associated with the predictive power of the instruments in the first-stage regression generally have P-values smaller than .01.

Nevertheless, it remains relevant to examine whether the results presented in Tables IV, V, and VI are robust to the use of the point-in-time measure of wages as opposed to the conventional average hourly earnings measure of wages. Table VII therefore reports results using the point-in-time measure of the wage. The

19. When performing regressions similar to Beaudry and DiNardo [1991] on the larger data set we use in this paper, we obtained virtually identical results.

TABLE VII
REPLICATING RESULTS USING POINT-IN-TIME MEASURE OF WAGES

	Ω_1	OID	Ω_1	Ω_1
Estimation method	IV	(*P*-value)	IV	IV
Year				
1978	−.31	.97	−.14	−.34
	(.35)		(.24)	(.51)
1979	−.03	.56	.04	.58
	(.28)		(.27)	(.45)
1980	−.40	.32	−.29	−.08
	(.23)		(.21)	(.35)
1981	−.39	.28	−.60	−.17
	(.25)		(.23)	(.36)
1982	−.36	.96	−.15	−.31
	(.25)		(.20)	(.33)
1983	−.04	.68	−.07	−.07
	(.41)		(.29)	(.42)
1984	−.15	.67	−.50	−.64
	(.18)		(.16)	(.27)
1985	.07	.00	−.22	−.19
	(.21)		(.14)	(.34)
1986	.13	.03	−.41	−.59
	(.24)		(.14)	(.19)
1987	−.39	.01	.14	.07
	(.17)		(.15)	(.25)
Pooled	−.16	.19	−.15	−.16
	(.08)		(.06)	(.11)

Table VII reports estimates of the effect of wage growth on hours growth using the point-in-time measure of wages. Specification and instrument set for estimates in the first column are identical to those in Table IV. OID(*P*-value) refers to the probability value of the Basmann test for the validity of the exclusion restrictions associated with the estimates in column 1. Specification and instrument set for estimates in the third column are identical to those in Table V. Specification and instrument set for estimates in the fourth column are identical to those in Table VI. Standard errors are in parentheses.

first column in Table VII reports the estimates of Ω_1 for the case where only nonunion workers are included in the sample. The second column reports the associated *P*-values for the overidentification test. The last two columns of Table VII report the estimates of Ω_1 analogous to those reported in Tables V and VI, now using the point-in-time measure of wages. Since the pattern for the overidentifying test statistic in the two latter cases is almost identical to that reported in the second column, they have been omitted. In Table VII note that our model still fares rather well when using these more restrictive samples and the point-in-time measure of wages. In particular, the overidentifying restrictions are not rejected, and the pooled estimates for Ω_1 are still negative and generally significantly different from zero, although somewhat smaller in magnitude than those reported in Table III. The sole exception is the estimate in the last column of Table VII. Although the point value is in line with the previous estimates, the pooled estimate of Ω_1 is sufficiently imprecisely estimated that it is no longer significantly different from zero at conventional levels of significance. Taken as a whole, however, the overall pattern of results seems supportive of the view that the behavior of hours worked is consistent with basic predictions from contract theory.

3. The Economic Significance of the Results

In light of the estimated coefficients in previous tables, it seems warranted to ask whether the identified effects are economically significant and of reasonable magnitude. To perform this assessment, it is useful to draw in part on results reported in Beaudry and DiNardo [1991]. For example, suppose that the economy is coming out of a recession and the unemployment rate falls by 2 percent. The estimates in our previous paper suggest that this fall in unemployment will be associated with a 6 to 8 percent increase in wages for workers who were hired during the recession. Now consider a worker in this situation for whom the increase in wage is not associated with an increase in productivity. The estimates in the previous tables suggest that the consequence of this income effect is a 1 to 2 percent reduction in the worker's hours. That is, the worker and firm agree that worker should take more vacation. In this case, the extra amount of vacation time turns out to be approximately three to five days a year—a magnitude that is both economically significant and plausible.

VI. Conclusion

One of the persistent questions facing labor economics is whether the temporal variations in wages and hours worked can be explained as the outcome of intertemporal optimizing behavior. The development of contract theory has in part been a response to the difficulties encountered by the spot market model in explaining the observed facts. In this paper we explore a set of predictions regarding the behavior of hours worked under the assumption that labor is transacted through enforcement-constrained long-term risk-sharing contracts. Our evidence is generally favorable to the view that the labor market is well described as a market for implicit contracts.

APPENDIX

Proof of Proposition 1. Proposition 1 is a direct implication of the first-order conditions associated with the maximization.

The first-order conditions associated with the maximization problem are given by equations (A.1) and (A.2), where $U_c(\cdot)$ and $U_h(\cdot)$ represent, respectively, the marginal utility of consumption and the marginal disutility of hours' worked. The Lagrange multipliers for the constraints under (a), (b), and (c), are, respectively, $\lambda(\theta^t)$, $\mu(\theta^{t+i},\theta^t)$, and $\gamma(\theta^{t+i},\theta^t)$, where $\lambda(\theta^t)$ does not depend on θ^{t+i} because there is only one constraint under (1). Finally, terms of the form $\text{prob}(\theta^{t+i}/\theta^{t+j})$ represent the probability of state θ^{t+i} conditional on being in state θ^{t+j}. The first-order conditions are

$$\text{(A.1)}\quad U_c(w(\theta^{t+i},\theta^t)h(\theta^{t+i},\theta^t),\, h(\theta^{t+i},\theta^t)). \left\{\text{prob}\,\frac{\theta^{t+i}}{\theta^t}\,\beta^i + \sum_{1\le k\le i}\sum_{\theta_{t+k}}\mu(\theta^{t+k},\theta^t)\text{prob}\,\frac{\theta^{t+i}}{\theta^{t+k}}\,\beta^{i-k}\right\} - \left\{\frac{\lambda(\theta_t)}{(1+R)^i} + \sum_{1\le k\le i}\sum_{\theta_{t+k}}\frac{\gamma(\theta^{t+k},\theta^t)}{(1+R)^k}\right\} = 0$$

$$\text{(A.2)}\quad U_h(w(\theta^{t+i},\theta^t)h(\theta^{t+i},\theta^t),\, h(\theta^{t+i},\theta^t)). \left\{\text{prob}\,\frac{\theta^{t+i}}{\theta^t}\,\beta^i + \sum_{1\le k\le i}\sum_{\theta_{t+k}}\mu(\theta^{t+k},\theta^t)\text{prob}\,\frac{\theta^{t+i}}{\theta^{t+k}}\,\beta^{i-k}\right\} + \left\{\frac{\lambda(\theta_t)}{(1+R)^i} + \sum_{1\le k\le i}\sum_{\theta_{t+k}}\frac{\gamma(\theta^{t+k},\,\theta^t)}{(1+R)^k}\right\}\psi_j(\theta^{t+i}) = 0.$$

The ratio of these two conditions is equation (1).

QED

Proof of Proposition 2. Using the first-order conditions associated with the maximization, wage payments can be solved as a function of the set of Lagrange multipliers and the state of productivity. The wage therefore inherits the properties of the Lagrange multipliers. There are two classes of Lagrange multipliers that each give rise to one of the properties of wages stated in the proposition. First, there is the single Lagrange multiplier associated with constraint (a) ($\lambda(\theta^t)$). This multiplier depends only on the contingency that existed at the time of hire since the constraint is in the form of an expectation taken at time t. This multiplier therefore gives rise to a time-of-entry fixed-effect in wages. Second, there is a whole set of Lagrange multipliers associated with the constraints under (2) and (3) ($\mu(\theta^{t+i},\theta^t)$ and $\gamma(\theta^{t+i},\,\theta^t)$). The values of these multipliers depend on the contingency at the time of hire, but also vary over time as to reflect the stringency of the different constraints. Therefore, wages also inherit a time-varying time-of-entry effect.

QED

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