THE EFFECT OF IMPLICIT CONTRACTS ON THE MOVEMENT OF WAGES OVER THE BUSINESS CYCLE: EVIDENCE FROM THE NATIONAL LONGITUDINAL SURVEYS

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A 1991 study by Paul Beaudry and John DiNardo found evidence of internal labor markets that simultaneously augment incumbent workers' wages when the external labor market is tight (when unemployment is low) and shield their wages when it is slack. Current wages, they found, depend on the tightest labor market conditions observed since a worker was hired, not current labor market tightness or labor market tightness at the time of hiring. This paper replicates and extends that research using data from six cohorts of the National Longitudinal Surveys that together span more than three decades, as well as an estimation framework more robust than that in the original study. The author finds strong support for Beaudry and DiNardo's key prediction. Supplementary regressions confirm other implications of the theory, as well. Recently, at least, the effect of implicit contracting on wages has been similar for men and women.

> [Does] the business cycle affect all employees similarly, or is there evidence that incumbent employees become part of an internal labor market that shields them from external market effects? This is a central question that has received surprisingly little attention.
>
> Baker, Gibbs, and Holmstrom (1994)

One of the few studies to compare the importance of internal labor markets and external market effects in wage determination over the business cycle was Beaudry and DiNardo (1991). These authors nested three wage-setting models into a simple regression equation. The three models are as follows: a spot market, in which skill-adjusted wages co-move with the *current degree of labor market tightness;* a full-

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The data used in this paper are publicly available from the Center for Human Resource Research at Ohio State University. Detailed results for all regressions and programs used to obtain results are available from the author at the Department of Economics, University of Texas at Arlington, Box 19479, Arlington, Texas 76019–0479.

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commitment risk-sharing model, in which wages are determined by *labor market tightness at the time the worker was hired;* and a risk-sharing implicit contract model with worker mobility, in which wages increase when the labor market gets tighter but do not decrease when it gathers slack. In this last model, current wages depend on *the tightest labor market conditions observed since the worker was hired.* Estimating this equation on the 1976–84 Panel Study of Income Dynamics and the May 1979 and May 1983 Current Population Surveys, Beaudry and DiNardo found consistent empirical support for the last model, but weak and inconsistent support for the other two. They concluded not only that this risk-sharing implicit contract model pertains in the labor market, but also that it supersedes the spot market model as a predictor of the movement of wages over the business cycle.

There is not yet a general consensus in support of (or against) the risk-sharing implicit contract model developed by Beaudry and DiNardo. For example, in a survey of seven well-known labor economics textbooks, all of which discuss implicit contracting in either a macroeconomic context (unemployment, wage cyclicality) or a microeconomic context (payment systems, internal labor markets), only two (Borjas 2000:499; Hamermesh and Rees 1993:561) referenced the Beaudry and DiNardo result, both in a somewhat negative light. The lack of consensus may be attributable to a paucity of follow-up studies, which are quite rare, as discussed below. This paper presents a replication and extension of Beaudry and DiNardo's work.

This study expands on the original in three dimensions. First, I develop alternate tests of the theory. The original study focused on the behavior of wages; I also examine quit behavior. Second, I consider the sensitivity of the results to the way the tests are implemented, that is, to the empirical methodology. While this concern was not ignored in the original study, I develop a more refined, demanding test of the implicit contract model that addresses additional methodological concerns. Simpler specifications show support for a "nonsense contracting hypothesis" that has no basis in theory. The more refined specification does not. Third, the sample on which the tests are implemented is expanded temporally and to include women as well as men. The sample in the original study covered the years 1976–84, just one full business cycle according to the measure of labor market tightness used in that paper and this one. (The cyclical behavior of wages in general can be sensitive to the time period studied [Sumner and Silver 1989].) My sample, taken from six cohorts of the National Longitudinal Surveys, contains both women and men and spans the years 1966–98, which encompass four and one-half business cycles.

Model and Previous Work

Let the unemployment rate be used as the measure of labor market tightness.[1] Then wages should be related to the current unemployment rate under the spot market model, to the initial unemployment rate (at the time of hiring) under the full-commitment risk-sharing model, and to the minimum unemployment rate since being hired under the implicit contract model with worker mobility. Adding some control variables, this yields the regression equation

$$(1)\quad \begin{aligned} \ln(w_{i,t}) &= \beta_C U_t + \beta_I U_{t_0} + \beta_M \min(U_{t_0} \ldots U_t) + \gamma X_{I,t} + \delta_i + \varepsilon_{i,t} \\ &= \beta_C U_C + \beta_I U_I + \beta_M U_M + \gamma X_{i,t} + \delta_i + \varepsilon_{i,t}, \end{aligned}$$

where i indexes individuals and t time in years, t_0 is the year the worker was hired, $\ln(w)$ is the log of the real, hourly wage, X is a vector of individual characteristics or controls that can vary over time, δ_i is an individual fixed effect, and ε is the error term. (The constant is subsumed in the fixed effects.)

[1]Results presented in an earlier version of this paper support this measure over two alternates: the growth rate of real GDP, and the cyclical component of GDP growth, calculated using the Hodrick-Prescott (1997) filter, expressed as a percentage of GDP.

The regression estimates the vectors β, γ, and δ. The spot market model implies $\beta_C < 0$, $\beta_I = 0$, $\beta_M = 0$; the full commitment risk-sharing model, $\beta_C = 0$, $\beta_I < 0$, $\beta_M = 0$; and the implicit contract model, $\beta_C = 0$, $\beta_I = 0$, $\beta_M < 0$. These three models do not exhaust all possibilities, however, and they are not mutually exclusive. Each can be viewed as a special case of a more general class of risk-sharing models.[2] Thus, it is possible for two or all three coefficients to be legitimately negative, in which case we should conclude that the data support one of these more general models, or that different models apply in different sectors of the economy. Equation (1) was used in the original study, although the fixed effects were sometimes omitted, and it is my basic estimating equation too.

Applications of this equation to data have been rare. The first was by Beaudry and DiNardo, of course; their results, both with and without individual fixed effects, are found in rows 7 and 14 of Table 2. The coefficients on U_M are large, negative, and statistically significant; those on U_C are smaller and sometimes statistically significant; those on U_I are statistically insignificant or positive. On this basis Beaudry and DiNardo argued for the superiority of their implicit contract model. The only other estimations of this equation in the literature appear to be McDonald and Worswick (1999), using Canadian data for 1984–93, and Seltzer and Merrett (2000), using historical Australian data. The former study is somewhat supportive of the implicit contract model; the latter is not. (Seltzer and Merrett viewed their estimates as supportive of implicit contracting, but in their paper β_M was significantly *positive,* contrary to the theory, and β_I was statistically insignificant.) In both cases data limitations restrict the strength of the conclusions that can be drawn. McDonald and Worswick could not control for individual fixed effects; a critical variable, job tenure, was measured categorically (five categories); average weekly earnings were substituted for the hourly wage. Seltzer and Merrett examined the personnel records of a single firm.

A few other theories have tested a risk-sharing contracting model in some other way. Brown (1982), in an under-heralded paper, derived some time series properties of wages under risk-sharing contracts with worker mobility, and found they were supported by the data. Jacobsen et al. (1993), in their well-known study of the effect of permanent job displacement on wages, found that labor market tightness at the time the worker was laid off has an influence on the current wage. And Baker et al. (1994), in their study of a firm's personnel records, found that labor market tightness at the time the worker was hired influences the current wage. None of these studies asked two contracting models to "compete" with each other, as in equation (1).

Thus, while there is a body of evidence supporting risk-sharing implicit contracting theories, the evidence is not pervasive enough or strong enough to lead to general consensus. Nor is there, or could there be, consensus on the type of implicit contract model that is most consistent with the data, or its importance relative to a spot market—questions that are examined in this paper.

Data

The data come from six cohorts of the National Longitudinal Surveys (NLS). These cohorts are described in Table 1. (The NLS-Youth is broken into separate male and female cohorts for most of the analysis, to facilitate male/female comparisons in implicit contracting.) The National Longitudinal Surveys are well suited to the current study for several reasons. Each cohort is a long panel, unlike the Current

[2]The three models come from assuming either no worker mobility or perfect mobility, and either no access to capital markets (to share risk) or perfect access. Intermediate possibilities, or the assumption that the firm cannot perfectly pre-commit to honoring the implicit contract, would yield models with hybrid outcomes, in which wages respond to two or all three of the unemployment measures in the regression (Thomas and Worrall 1988).

Table 1. Description of the Data: Six Cohorts of the National Longitudinal Surveys.

NLS Cohort	*Number of Interview Years Used*	*Span of Years*	*Age Range on Jan. 1 of the First Survey Year*	*Individuals in Sample (after restrictions)*	*Total Person*Year Observations in Sample*
Youth–Female	18	1979–1998	14–21	4,846	23,768
Youth–Male	18	1979–1998	14–21	5,429	33,297
Young Women	15	1968–1988	14–24	3,283	13,953
Women	14	1967–1989	30–44	2,585	12,481
Older Men	9	1966–1983	45–59	3,094	10,357
Young Men	12	1966–1981	14–24	3,895	18,076
TOTAL	86	1966–1998	14–59	23,132	111,932

Population Survey (CPS); yet in the aggregate, the sample size is larger than in the Panel Study of Income Dynamics (PSID). The total time period spanned by the data is larger than in either of these other surveys. The tenure question is not ambiguously posed (a problem with the PSID; see Beaudry and DiNardo 1991), nor is there "heaping" of the tenure responses into five-year intervals (a problem with the CPS; see Ureta 1992). Logical inconsistencies in the NLS tenure information (such as reported tenure increasing by five years in interviews that are one year apart) amount to less than 2% of all observations. (Knowing the individual's job tenure is vital for determining that individual's U_I and U_M.) The NLS-Youth (NLSY) cohorts also contain additional variables that will be useful in supplementary regressions conducted below.

There are two main disadvantages to the NLS. One is that there are no interviews in some years for each cohort, so that β_C is estimated partly over longer time spans than is in the spirit of the spot market (business cycle) model. The other is that union status information is missing in some years, eliminating a potentially important control variable. As it turns out, however, this is not much of a problem for testing the implicit contract model, because U_M is essentially uncorrelated with union status (after conditioning on the other controls).[3] Using an adaptation of equation (1), I estimated separate β_M coefficients for each of the 43 cohort*year intersections in which union status information is available, and then re-estimated them omitting the union status variable. The two sets of coefficients were virtually identical. So years without union status information are included in the sample, and a dummy for missing union status is included as an explanatory variable.

Although each data set is nationally representative as a cohort (with weighting, which is used), the surveys as a group are not representative of the United States as a whole, because each cohort contains a limited range of ages. This is a disadvantage in that one does not estimate a single set of "national" coefficients. On the other hand, it can be valuable to compare the coefficients across cohorts, which vary by age, sex, and range of years sampled.

The standard regression specification—here called the "basic specification"—contains the unemployment measures, individual fixed effects, and the following time-varying individual control variables: potential work experience and its square, tenure and its square, region dummies, industry dummies, union status, marital status, residence in an SMSA, and a dummy for miss-

[3]It might have some effect on tests of the spot market model. Grant (2001) indicated that union employment was more sensitive to the business cycle than nonunion employment until the early 1980s; then the relationship was reversed.

ing union status information. Additional details about these controls can be found in the note to Table 2. These variables were also used in the original study, except for the missing union status dummy, which was unnecessary, and the square in tenure, for which there is adequate empirical support (Topel 1991).

The wage measure is the NLS-constructed "hourly rate of pay" variable, deflated using the Consumer Price Index, in logs. The unemployment measure is the annual, national, civilian rate for ages 16+, whereas the original study used the rate for ages 20+. The former measure is much easier to obtain for prewar years; the correlation between the two in the postwar years exceeds 0.99. This unemployment rate is depicted for the last 40 years in Figure 1a. The relationship between job tenure and U_M is presented for four specific years in the sample in Figures 1c–1f; the average over the years 1966–98 is in Figure 1b. As can be seen in the figure, the greatest intra-year variation in U_M occurs when unemployment is currently high, as in Figure 1d, and the least occurs when unemployment rate is currently low, as in Figure 1f.

The sample inclusion criteria are designed to isolate those private sector jobs most conducive to the conditions presumed by the theory. To be included in the sample, an individual had to be employed at the time of the interview; could not be attending school; had to be working full-time (usual weekly hours ≥ 35); had to be working in the private sector; in the NLSY cohorts, could not be working an "odd job" (this information is not available in the other cohorts); had to have started the job at age 16 or higher; and had to be at least 21 and no older than 64. Only the final restriction was imposed in the original study (and the sample was restricted to men).

Basic Results

Rows 1–6 of Table 2 present results for each cohort, using the basic specification. Each coefficient is multiplied by 100, so that it translates a one percentage point change in the unemployment rate of interest into a percentage change in wages.

The β_C coefficients, on the current unemployment rate, are large (in magnitude) and statistically significant for the younger cohorts, smaller but still significant for women, and smaller still and insignificant for the Older Men cohort. The coefficient in the original study falls comfortably in the middle of this (admittedly wide) range of values. These results generally correspond with expectations and with past work on wage cyclicality (Abraham and Haltiwanger 1995).[4]

The β_I coefficients, on the initial unemployment rate, are negative and statistically significant in three cohorts and insignificant in the other three. Even those coefficients that are significant, however, are not large in magnitude. The coefficient in the fixed effects specification in the original study is similar to these, but less precisely estimated. When the additional controls discussed in the next section were adopted, the estimate of β_I was negative and statistically significant in only one cohort (the Older Men). So I conclude that U_M is preferred to U_I as an explanatory variable, and drop the latter from all regressions subsequent to those in Table 2. Its omission from the specification has only a modest effect on estimates of β_M.

The β_M coefficients, on the minimum unemployment rate observed since the worker was hired, are significantly negative in five cohorts and significantly positive in the other. In the three male cohorts, the coefficients are all sizable as well as statistically significant—they are in the neighborhood of –2.5. This too is similar to the fixed effects estimate in the original study (which only used men). In the female cohorts, two coefficients are negative and one is positive. This raises some doubt as to whether

[4]Estimates of β_C were similar when both other unemployment measures were removed. In the order by which the cohorts appear in Table 1 (standard errors in parentheses), they are –3.81 (0.23), –3.45 (0.19), –1.23 (0.16), –0.68 (0.20), 0.19 (0.36), and –2.99 (0.22).

Figure 1. Unemployment Rates.

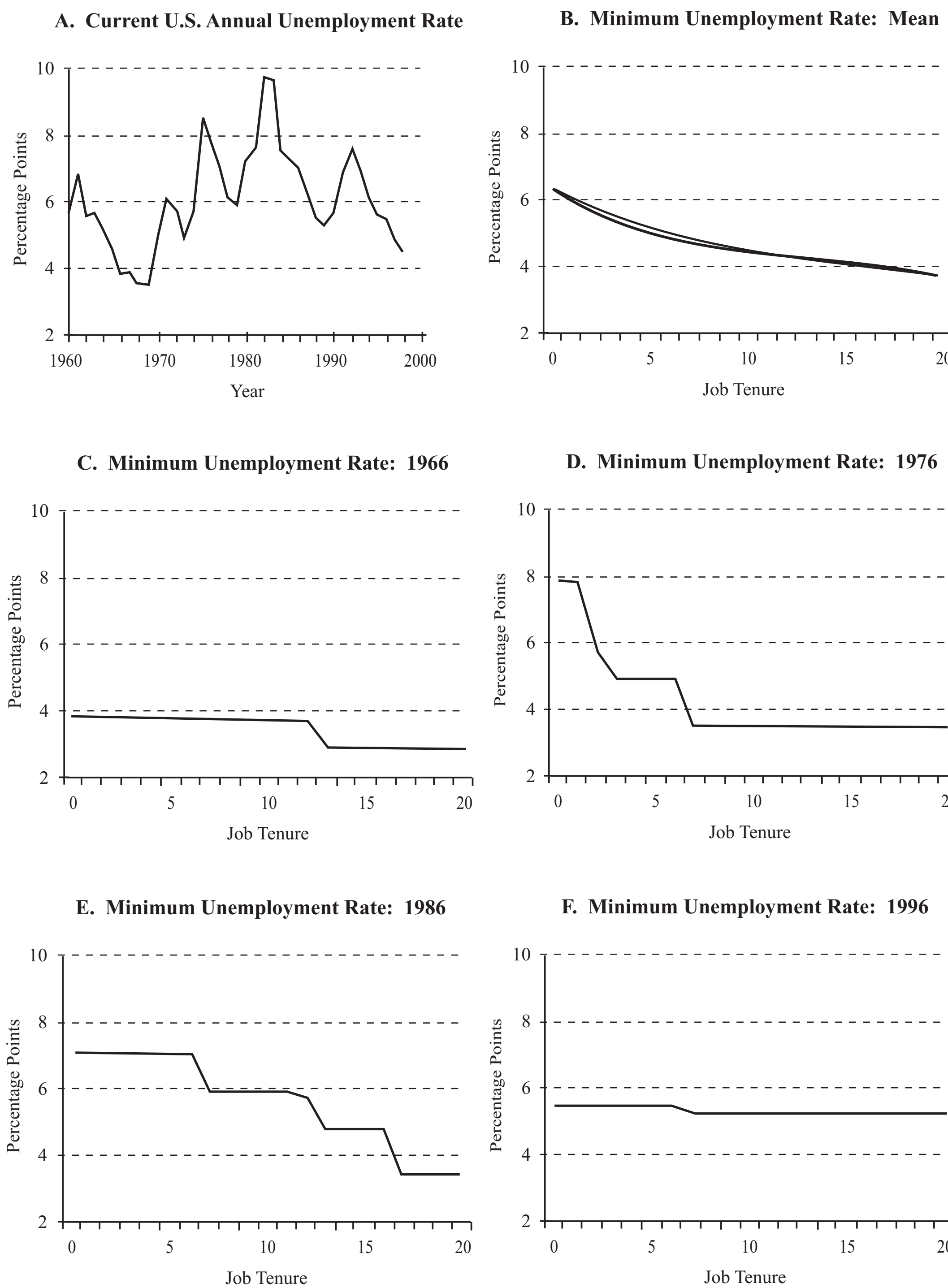

Table 2. Basic Specification Regression Results Using All NLS Cohorts.

Sample	*Regressor* Current Unemployment Rate (β_C)	Initial Unemployment Rate (β_I)	Minimum Unemployment Rate (β_M)
Individual Fixed Effects Included			
1. Youth–Female	–2.62* (0.33)	–0.95* (0.43)	–2.11* (0.67)
2. Youth–Male	–2.25* (0.28)	–0.12 (0.35)	–2.54* (0.55)
3. Young Women	–0.95* (0.20)	0.61 (0.41)	–1.76* (0.56)
4. Women	–0.74* (0.20)	–0.71* (0.34)	0.99* (0.43)
5. Older Men	0.41 (0.36)	–0.69* (0.23)	–2.92* (0.76)
6. Young Men	–2.37* (0.25)	0.60 (0.43)	–2.29* (0.56)
7. Beaudry and DiNardo PSID (1976–1984, men only)[a]	–0.7* (0.25)	–0.6 (0.7)	–2.9* (0.8)
No Individual Fixed Effects			
8. Youth–Female	–1.73* (0.37)	0.56 (0.36)	–3.38* (0.68)
9. Youth–Male	–0.88* (0.32)	0.52 (0.30)	–2.38* (0.57)
10. Young Women	–0.91* (0.26)	0.74 (0.41)	–1.45* (0.64)
11. Women	–0.40 (0.32)	–1.45* (0.37)	0.56 (0.56)
12. Older Men	0.17 (0.51)	–0.35* (0.11)	–0.75 (0.68)
13. Young Men	–2.11* (0.30)	0.49 (0.44)	–2.73* (0.65)
14. Beaudry and DiNardo PSID (1976–1984, men only)[a]	0.0 (0.2)	1.3* (0.4)	–5.9* (0.6)

Notes: OLS coefficients; standard errors in parentheses. All coefficients and standard errors are multiplied by 100. The dependent variable is the logarithm of the real, hourly rate of pay (standard deviation approximately 0.60, standard error of the estimate approximately 0.25–they vary by cohort). The independent variables, in addition to those listed in the table, are the following: individual fixed effects, potential experience (age – schooling – 6) and its square, tenure and its square, region dummies (two, four, or nine, as available in the data), industry dummies (twelve), union status (a dummy that equals one if the worker is a union member or is covered by a collective bargaining agreement), marital status (married with spouse present), residence in an SMSA, and a dummy for missing union status information. All regressions use sampling weights.

[a]The figures in these rows have fewer decimal places because of rounding in the original study from which they were taken (Beaudry and DiNardo 1991, Table 2, rows 6 and 10). Tenure squared and the missing union status dummy are not included in these regressions.

*Statistically significant at the .05 level in a two-tailed test.

Source: Six cohorts of the National Longitudinal Surveys, described in Table 1.

the implicit contract model applies to women, a matter that will be investigated further as we proceed. But there is consistent evidence that the model applies to men. In this regard my results are comparable to those of Beaudry and DiNardo.

There are many ways to interpret these coefficients. Consider a one percentage point decline in the current unemployment rate. This will raise male workers' expected wages by up to 2.5%, according to β_C. For those workers who have not experienced lower unemployment while working for this firm, wages will increase an additional 2–3%. Alternatively, consider a life-cycle perspective. Figure 1b shows that U_M falls by roughly 2.5 percentage points over the course of twenty years with a single employer. The male β_M coefficients imply that this leads to wage growth of 6.5–7.5%. This is about one-fifth of the returns to 20 years of tenure, as estimated in Topel (1991). Finally, consider an intertemporal macro perspective. Using the median male β_M estimate (in row 6), I determined the average year-to-year change in aggregate (average) wages that is generated by changes in U_M from one year to the next. (Details of the simulation, which is also referred to below, are included in the appendix.) This average was 0.4%. By all of these metrics, the wage changes implied by the male β_M coefficients are substantial.

Does the implicit contract model supersede the spot market model? I do not conclude that it does, for two reasons. First, in most cohorts, estimates of β_C are sizable and statistically significant, especially in the fixed effects specification. This is also true in Beaudry and DiNardo's fixed effects specification, in row 7 of the table. Perhaps their conclusion regarding the superiority of the implicit contract model was influenced by other regressions, such as the one reproduced in row 14, in which β_C was small or insignificant. These regressions do not contain individual fixed effects. While the omission of fixed effects does not have tremendous effects on GKbeta estimates in the NLS cohorts, in rows 8–13 of Table 2, it does in Beaudry and DiNardo's sample from the PSID. (In their study of compositional effects on estimates of wage cyclicality, Solon, Barsky, and Parker [1994] suggested that a larger effect is found in the PSID because it contains a larger range of ages, and so is subject to compositional effects across cohorts as well as those within cohorts. The NLS are only subject to the latter.) Solon, Barsky, and Parker's (1994) evidence supporting the fixed effects specification in this context, the significance of β_C in the fixed effects regressions here and in the original study, and the fairly close correspondence between these estimates and those in the general literature on wage cyclicality collectively argue persuasively for the continued relevance of the spot market model.

Second, I also find that U_C explains more variation in aggregate wages over the business cycle than U_M does. This conclusion follows from a spectral analysis of simulated aggregate wages, described in the appendix. It is a consequence of the fact that U_C exhibits far more variation than U_M does over short time intervals, especially when unemployment is trending upward. (See Figure 1a.) For these two reasons, I do not conclude that the implicit contract model supersedes the spot market model. Both models (or a more general model containing features of each) help us understand the movement of wages over the business cycle.

Methodological Issues

So far there is preliminary evidence that implicit contracts matter, at least among men. Now I seek to refine that evidence by considering some methodological issues. (Some of these were also considered in the original study.) Table 3 contains the results, for the β_M coefficients only. The third row of the table uses the basic specification used in Table 2 (except that U_I has been removed). Rows 1 and 2 contain simpler specifications, rows 4–6 more sophisticated specifications, and row 7 a "foil" regression, all to be discussed shortly.

In thinking about potential sources of bias, it helps to recognize that, in the basic specification, β_M is identified by variation in U_M *within* years and *across* years. One cause of bias could be that year-to-year variation in U_M is associated with other factors that can influence aggregate wages, such as inflation or lagged unemployment

Table 3. Regression Results for All NLS Cohorts Using Alternative Specifications.

	Cohort					
Specification	*Youth–Female*	*Youth–Male*	*Young Women*	*Women*	*Older Men*	*Young Men*
1. No Individual Fixed Effects, Linear Tenure Control	–4.36* (0.46)	–3.61* (0.39)	–1.27* (0.38)	–1.60* (0.37)	–0.80 (0.68)	–2.65* (0.33)
2. Add Quadratic in Tenure	–2.62* (0.48)	–1.69* (0.41)	–0.53* (0.39)	–0.87* (0.38)	–0.89 (0.68)	–2.10* (0.34)
3. Add Individual Fixed Effects (Basic Specification)	–3.15* (0.47)	–2.67* (0.39)	–1.13* (0.37)	0.27 (0.27)	–3.80* (0.70)	–1.64* (0.31)
4. Add Year Dummies	–2.68* (0.55)	–2.60* (0.45)	–0.79 (0.41)	0.17 (0.35)	–3.64* (0.70)	–2.06* (0.33)
5. Add Tenure Dummies	–1.88* (0.58)	–1.63* (0.48)	–0.16 (0.42)	0.62 (0.36)	–3.61* (0.71)	–1.86* (0.34)
6. Add Tenure * Current Unemployment Interaction	–1.95* (0.70)	–1.92* (0.58)	–0.36 (0.50)	1.12* (0.38)	–3.01* (0.80)	–1.09* (0.39)
7. Maximum Unemployment Rate, Year and Tenure Dummies	1.06 (0.72)	–1.02 (0.57)	0.90 (0.60)	0.03 (0.41)	0.25 (0.38)	–0.71 (0.61)

Notes: OLS coefficients; standard errors in parentheses. Coefficients are on U_M in rows 1–6 and on U_{MAX} in row 7, as defined in the text. All coefficients and standard errors are multiplied by 100. The dependent variable is the logarithm of the real, hourly rate of pay. The independent variables in row 1 are the current unemployment rate, the minimum unemployment rate observed since being hired, nonwhite race, schooling, potential experience and its square, tenure, region dummies, industry dummies, union status, marital status, residence in an SMSA, and a dummy for missing union status information. All regressions use sampling weights. In regressions 2–6, the additional variables listed are added to the specification in the previous row.

*Statistically significant at the .05 level in a two-tailed test.

Source: Six cohorts of the National Longitudinal Surveys, described in Table 1.

(which might have delayed effects on wages, as in a lagged Phillips Curve). The simplest way to address this concern is also the most complete—add a set of year fixed effects to the regression. The results are presented in the fourth row of the table. Coefficients fall notably for the NLSY–Female and Young Women cohorts; the other cohorts are essentially unaffected.

In these regressions, β_M is identified only by variation in U_M across individuals in the same year, as in Figures 1c–1f. Therefore, sources of bias must now be correlated with U_M across individuals in the same year. A likely place to look for bias is job tenure, which is correlated with U_M within-years by construction. One possibility is that the quadratic in tenure that is included in the basic specification does not completely capture the effect of tenure on wages. In this case U_M could be correlated with the regression residual, biasing the coefficient. The simplest way to address this is to add another set of dummy variables—this time, in tenure (rounded to the nearest year). The results are presented in the fifth row of the table. There is little effect for the Older Men and the Young Men, but the tenure dummies are jointly significant in the other cohorts, and there is a non-trivial dampening of the coefficient for the NLSY (men and women) and Young Women. Thus the male results appear to be quite robust, but there is increasing doubt as to whether the model applies to women.

A final, more subtle possibility is that β_M is picking up a mix of unemployment and tenure effects: a dynamic influence of unemployment on the slope of the tenure profile that is not attributable to the implicit contract model. For example, wages may be stickier (in both directions) for higher-tenure workers, so that the wage-tenure profile flattens in expansions and steepens in recessions.[5] This may occur because of contracting considerations, but it is not predicted by the Harris and Holmstrom (1982)–inspired model generated by Beaudry and DiNardo. To test for higher wage stickiness among higher-tenure workers, I added a TENURE * U_C interaction term to the model. The results are shown in the sixth row of the table. There is evidence that this effect obtains, but it does not destroy the evidence for the Beaudry-DiNardo model. In the Women, Older Men, and Young Men cohorts, the interaction term is positive and significant or marginally significant, and β_M moves modestly in the positive direction. In the other cohorts, the interaction term is negative but insignificant, and there is little effect on estimates of β_M.

When we look back (upwards) from row 6 up to row 3 of the table, cohort by cohort, we can see the cumulative effect of the steady addition of more control variables on the estimate of β_M. The cumulative effect is always in the positive direction and is often substantial in magnitude. If we continue on up to row 1, the simplest specification used by Beaudry and DiNardo, there is evidence of even greater change.[6] Beaudry and DiNardo's estimates of β_M range from –2.9 to –5.9, which is larger than I find here. The differences in specification explain much of this difference in magnitudes.

A last regression provides a final check on the validity of the more elaborate specification and a direct test of the asymmetry of the relationship between unemployment and wages inherent in the implicit contract model. I replace U_M with the *maximum* unemployment rate observed since being hired: U_{MAX}. Since there is no realistic economic model in which U_{MAX} has a causal effect on wages, β_{MAX} should be insignificant; if it is not, there may yet be specification bias. The results, in row seven, show that β_{MAX} is indeed insignificant in every cohort. Strikingly, when using the basic specification instead (this is not shown in the table), β_{MAX} is large and significant in four of the six cohorts, again suggesting the presence of bias in that simpler specification.

Implicit Contracting among Women

The results in Tables 2 and 3 consistently indicate that the implicit contract model applies to men. Concerning women, things are more uncertain. Estimates of β_M are positive for the Women, negative but not always statistically significant for the Young Women, and negative and significant for the women in the NLSY. We could feel more certain about the extent of implicit contracting among women if we better understood these differences in results.

While further analysis is not definitive, it does provide evidence that the statistically significant results in the NLSY-Female cohort, the most recent cohort, are not an isolated or ephemeral phenomenon. First, the possibility that implicit contracting has grown among women over time is tenable. Theoretically, this could result from increases in women's labor force attachment (Beaudry and DiNardo 1991, Section V) or decreases in discrimination against women. Empirically, when the Women and Young Women cohorts were split in half, chronologically, and the model was estimated separately on each half, β_M was more negative in the more recent half of each cohort. Second, implicit contracting among women is not sensitive to the industry/occupation mix of women's employment. I calculated the change in implicit contracting among women over the sample period that is implied by changes in women's union/indus-

[5]One can see in Figures 1c–1f that the U_M-tenure profile also flattens in expansions and steepens in recessions. So this should impart a negative bias to the coefficient.

[6]Much of this stems from the inclusion of the quadratic in tenure, in row 2 of the table. In the absence of the quadratic term, the β_M coefficient is mostly contrasting the concavity of the wage-tenure profile with the mirror-image convexity of the U_M-tenure profile (Figure 1b). Although the inclusion of individual fixed effects has only a modest effect in most cohorts of the NLS, it was quite important in the original study, cutting estimates of β_M by half.

try/occupation employment shares (taken from the NLS and the results in Even and Macpherson 1993), both in absolute terms and relative to men, using the results presented in the next section and more detailed breakdowns not reported in this paper. There was virtually no change in implicit contracting in any of these calculations.

Third, estimates for the older female cohorts may be contaminated by bias. As a referee points out, the regression specification in equation (1) relies on the assumption that the starting wage is a stable function of the unemployment rate. This assumption is violated if there is a secular trend in real wages over the time period used for estimation. This was certainly the case for women during the 1960s and 1970s, during which most interviews for the Women and Young Women cohorts were taken. Bias may also arise from changes in the female wage-tenure profile that are unrelated to the contracting issues considered here. For example, Lazear-style deferred payment contracts are more feasible when women have greater job attachment. In additional regressions that attempted to account for these changes in the structure of female pay, estimates of β_M were generally more negative than before and were sometimes statistically significant.[7] Implicit contracting among the Women and Young Women may have been greater than my earlier estimates indicated. One cannot rule out, however, that there also has been genuine growth in the effect of implicit contracting on women's wages. In any event, both alternatives strengthen the claim that the implicit contract model pertains to women as well as men. Estimates from the NLSY cohorts indicate that currently the effect of implicit contracting on wages is similar for men and women.

Supplementary Results

I now present some supplementary regressions that test additional implications of the theory or explore its robustness in various dimensions. The data used in all these regressions come from the NLSY, with men and women combined for simplicity. The NLSY alone contains the information necessary to estimate many of these regressions. Also, unlike the other cohorts, it contains a long, uninterrupted span of annual interviews (1979–94), which is ideal for estimating regressions in differences (as opposed to levels). The NLSY contains more individual-level observations than all the other cohorts combined. For each regression, the basic specification presented earlier is used, along with an expanded specification that also includes year dummies and a log(1 + tenure) term. (Adding this term to the quadratic in tenure was statistically indistinguishable from using the full set of tenure dummies. Also, the TENURE * U_C interaction term was statistically insignificant in the NLSY and is omitted.)

The first set of regressions explore intersectoral differences in implicit contracting. Separate regressions are estimated by industry, occupation, and union status. (Beaudry and DiNardo conducted a more detailed examination of industry differences than is attempted here.) These are included in the first eight rows of Table 4. The most important intersectoral difference is between union and nonunion workers. Implicit contracting is much stronger in the union sector, as might be anticipated. (Hogan [2001] argued that a union can act as a mechanism for enforcing implicit contracts, by monitoring the employer's adherence to the "terms" of the implicit contract.) There also seems to be weaker contracting in service industries. But overall, the similarities are more notable than the differences. Implicit contracts are fairly widespread in the economy, at least for this cohort.

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[7] In row 4 of Table 3, the Women coefficient is 0.17 (standard error 0.35), and the Young Women coefficient is –0.79 (0.41). When a full set of dummies was added for the year in which the woman began her job, the estimates were –1.43 (0.77) and –2.23 (0.69). When the quadratic in tenure was allowed to vary in each year, the estimates were 0.19 (0.55) and –1.66 (0.68).

Table 4. Supplementary Regressions Using the NLS-Youth.

Regression Descriptor	Log Wage Regressions[a] (Coeff. on U_M) Basic Specification	Log Wage Regressions[a] (Coeff. on U_M) Expanded Specification	Quit Probits[b] (Marginal Effect of U_M at the Mean of Quits) No Tenure Controls	Quit Probits[b] (Marginal Effect of U_M at the Mean of Quits) Full Tenure Controls
Sectoral Differences				
1. Union	–4.39* (0.82)	–3.91* (1.01)	—	—
2. Nonunion	–2.40* (0.35)	–1.39* (0.41)	—	—
3. Production Occupations	–2.08* (0.29)	–1.68* (0.54)	—	—
4. Nonproduction Occupations	–2.50* (0.44)	–1.57* (0.53)	—	—
5. Manufacturing Industries	–2.30* (0.65)	–1.03 (0.80)	—	—
6. Service Industries	–1.88* (0.64)	–0.59 (0.77)	—	—
7. Trade Industries	–3.32* (0.74)	–2.17* (0.87)	—	—
8. Other Industries	–2.88* (0.76)	–3.30* (0.89)	—	—
Difference Specification				
9. Levels on Subsample Used for Differencing	–2.99* (0.44)	–2.09* (0.53)	—	—
10. Cochrane-Orcutt Method with $\rho = 0.36$	–1.77* (0.55)	–1.41* (0.63)	—	—
11. Full Differencing	–1.69* (0.53)	–1.16* (0.58)	—	—
Quit Regressions				
12. Quit	—	—	–7.77* (0.32)	–1.21* (0.37)
13. Quit, Controlling for Health Plan	—	—	–7.10* (0.33)	–1.13* (0.38)

Notes: OLS coefficients/probit marginal effects; standard errors in parentheses. All coefficients, marginal effects, and standard errors are multiplied by 100.

[a]In rows 1–9, the dependent variable is the logarithm of the real, hourly rate of pay; in rows 10–11, the dependent variable is the individual difference in the logarithm of the real, hourly rate of pay. The control variables in the basic specification are individual fixed effects, potential experience and its square, tenure and its square, region dummies, industry dummies, union status, marital status, and residence in an SMSA. The expanded specification adds year dummies and log(1 + tenure). In rows 1–8, separate regressions are run for each sector. All regressions use sampling weights.

[b]The dependent variable is a dummy that equals one if the worker quit his job in that year and zero otherwise. The mean of quits is 0.22. The independent variables, in addition to those listed in the table, are potential experience and its square, schooling, nonwhite race, sex, residence in an SMSA, union status, marital status, and region, industry, and year dummies. All probits use sampling weights.

*Statistically significant at the .05 level in a two-tailed test.

Source: The National Longitudinal Survey–Youth Cohort (men and women), described in Table 1.

Although not reported in the table, in the basic specification β_C is consistently negative across sectors as well. This is not compatible with an economy in which a spot market applies in some sectors and the implicit contract model applies in others.

It is more likely that wages are typically set by a more general contracting model that implies that both β_C and β_M are negative.

The second set of regressions employ differencing. First, Cochrane-Orcutt differencing is used to account for autocorrelation of the residuals across time: $\varepsilon_{i,t} = \rho\varepsilon_{i,t-1} + \eta_{i,t}$. This autocorrelation could result from some workers taking jobs with faster wage growth than normal (Baker 1997). My best estimate of autocorrelation is 0.36;[8] results adjusting for this autocorrelation are presented in row 10. The coefficient estimates do fall, by about one-third, but remain sizable and statistically significant.

Second, full differencing (row 11) is performed to allow for the possibility that individual wages follow a random walk. (This, of course, is not supported by the evidence in the previous paragraph. Also, see Baker [1997] for more contradictory evidence.) It also serves as an additional robustness check. The coefficients fall further, but again are sizable and statistically significant. Thus, the fundamental results are robust with respect to differencing.

In the final two rows I estimate (reduced form) quit regressions. (Here the individual fixed effects are removed and replaced with sex and race dummies and years of schooling completed.) If decreases in U_M really do raise compensation relative to the alternative, then workers should be less likely to quit. The coefficient on U_M should be positive. If it is not, there must be differences in job requirements, in the quality of the job match, or in unmeasured ability (not captured by the individual fixed effects in the wage equation) that are related to U_M.

Quit probits are estimated with and without a control for a health plan, and with and without tenure controls.[9] The coefficients and marginal effects in all regressions are positive, statistically significant, and sizable in magnitude, which is consistent with the theory and with my previous results. A decrease in U_M of one percentage point decreases the quit rate by 1–7 percentage points, *ceteris paribus*. One can also combine these estimates with the wage coefficients presented earlier to calculate the implied elasticity of quits with respect to wages. These elasticities range from 1.9 to 13.5. There is no definitive estimate in the labor literature with which to compare these elasticities, but it is certainly plausible that the true wage-quit elasticity of the workers in the NLSY lies in this range.

Conclusion

I have presented a replication and extension of tests of the implicit contracting model laid out in Beaudry and DiNardo (1991), using a different data source, a much longer span of years, and a more demanding regression specification that addresses a number of methodological concerns. There is strong and consistent empirical support for the key prediction of the model: estimates for men imply that wages rise by about 2.5% for each percentage point decline in the lowest annual national unemployment rate that has been observed

[8] In the full sample, the estimate of ρ is actually not significantly different from zero. But this is partly due to measurement error, which tends to "wash out" the autocorrelation, and partly due to the shortness of the panel—there is an average of only five observations per individual. (If there are only two observations per individual, $\rho = -1$.) When the sample was restricted to individuals with ten or more observations and outliers were removed ($|\varepsilon| > 1$), ρ rose to 0.36.

[9] When tenure controls are included in the regression, the effect of tenure on quits is "overestimated" to the extent that contemporaneous tenure reflects individual heterogeneity in quit propensities. (This heterogeneity is documented in Farber 1994.) This should bias the coefficient on U_M in a positive direction. On the other hand, when the tenure controls are omitted, the coefficient on U_M should be biased in a negative direction, because genuine state dependence in quit propensities is not accounted for, and is picked up by U_M instead. Thus, the true coefficient on U_M probably lies between the two sets of estimates.

since the worker was hired. This effect is smaller than the range of estimates ascertained in the original study, but it is still economically significant and larger than most estimates of macroeconomic influences on wages in the general literature on wage cyclicality. The difference between the two magnitudes is primarily due to the inclusion of a large number of additional control variables in my regressions, some or all of which were missing in the various specifications presented in the original study. Their inclusion, which is generally supported statistically, tends to reduce the magnitude of the coefficient of interest, but also substantially reduces the likelihood that my estimates are contaminated by omitted variable bias.

Supplementary results are also consistent with the model. While implicit contracting is widespread in the economy, it is strongest in the union sector. A reduction in the lowest unemployment rate observed since being hired reduces the probability of quitting. And the effect for women may have been increasing over time, coincident with increases in women's labor force attachment and increases in the relative earnings of women. Currently, the wages of men and women are influenced roughly equally by implicit contracting, as well as can be determined.

There is not much empirical support for a full-commitment risk-sharing model in which wages are influenced by the unemployment rate at the time the worker is hired. But I do find that the contemporaneous unemployment rate also has an independent effect on wages, and is more important than the lowest unemployment rate since being hired for explaining business cycle variation in aggregate wages. The joint importance of these two unemployment measures means that wages are not universally set by a spot market; nor are they universally set strictly by the implicit contract model developed by Beaudry and DiNardo. Wages probably follow a more general contracting model that predicts partial wage insurance against negative labor demand shocks coupled with partial wage responsiveness to current labor market conditions (see footnote 3). For efforts in this direction, see Thomas and Worrall (1988) and Beaudry and DiNardo (1995).

Appendix
Variance Decomposition of Simulated Wages

Let individual wages be determined by the equation $\ln(w_{i,t}) = \beta_C U_C + \beta_M U_M$, where there is individual variation in U_M within years because of differences in job tenure. I wish to decompose the inter-year variation in the average wage. This is

$$\text{var}(\overline{\ln(w)}) = \beta_C^2 \text{var}(\bar{U}_C) + 2\beta_C\beta_M \text{cov}(\bar{U}_C, \bar{U}_M) + \beta_M^2 \text{var}(\bar{U}_M),$$

where the means have been taken within years (across individuals) and the variances and covariances across years. Variation in the average wage over time can be broken down into three components: one due to variation in U_C, one due to the covariation of U_C and U_M, and one due to variation in U_M, as expressed in equation (2). This can also be done for business cycle variation specifically, using standard spectral analysis procedures, as outlined in Hamilton (1994). Following the macro literature (for example, Cunningham and Vilasuso 1997), I defined business cycle frequencies as those less than eight years in duration.

All components in this equation were simulated for the period 1966–98. For a nationally representative frequency distribution of job tenure, I relied on Figure 1d of Neumark et al. (1999), which used the February 1995 Contingent Work Supplement to the CPS. (The distribution of job tenure in the pooled NLS data is skewed toward short tenures, because there is a disproportionate number of young workers.) This distribution of job tenure is prescribed to apply in each year and U_M calculated straightforwardly. The actual, annual, U.S. unemployment rates are used. The values of β_C and β_M were chosen from various regressions in Table 2 and are identified in Table A1.

Table A1
Decompositions of Simulated Wages into Components Attributable to the Current Unemployment Rate and the Lowest Unemployment Rate Observed Since Being Hired

	$\beta_C = -0.7$ $\beta_M = -2.9$ *(Table 2, Row 6)*	$\beta_C = -1.0$ $\beta_M = -1.8$ *(Table 2, Row 2)*	$\beta_C = -2.4$ $\beta_M = -2.4$ *(Table 2, Row 1)*
Portion of Total Variance in the Average Wage Attributable to:			
Business Cycle Frequencies	6.2%	9.4%	11.9%
Long Run Frequencies	93.8%	90.6%	88.1%
Portion of Business Cycle Variation in the Average Wage Attributable to:			
Variation in U_C	32.8%	59.8%	75.0%
Variation in U_M	29.1%	10.0%	3.9%
Covariation in U_C and U_M	38.1%	30.2%	21.1%

Note: Details of the simulations are found in the text of the Appendix.

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