Have Lazear-Style Implicit Contracts Disappeared?

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Abstract

I. Introduction:

But later theoretical work by Lazear (1983) showed that a properly designed defined-benefit pension plan can serve as a substitute for a mandatory retirement provision in an LSIC. It is therefore possible for LSICs to persist despite the ban on mandatory retirement. But the massive shift in recent years away from defined-benefit plans in favor of defined-contribution plans (which cannot support LSICs) seems to suggest that this has not happened.

Why have defined-benefit plans not been employed

technologies and/or organizational structures that rely less heavily on firm-specific human capital.

LSICs contribute to positive measured returns to firm tenure (i.e., they contribute to the positive effect of an increase in firm tenure on wages). An implication of the Hypothesis, therefore, is that firm tenure should decline. A further implication is that this decline should be greater for less experienced workers. To see this, consider two dates: one shortly before the hypothesized effect began and one shortly after. The incidence of LSICs among experienced workers will be the same on these two dates, while the incidence of LSICs for new entrants to the labor force will be smaller at the later date. This means that, all else equal, the returns to firm tenure among older workers should be the same on the two dates, whereas returns to firm tenure among younger workers should be lower in the later period.

This empirical implication can be taken to the data: the main test of the Hypothesis consists of using a 1981-1992 sample from the Panel Study of Income Dynamics (PSID) to estimate a wage equation for the main sample (prime-age, full-time, private sector, non-union male workers who are not self-employed). This is the group that, acco

implications of the Hypothesis; Section V describes the data and provides summary statistics; Section VI describes the estimation procedure and presents the main empirical results; Section VII discusses alternative possible explanations for the main empirical results and presents additional results; and Section VIII concludes.

II. Theoretical Background:

There is evidence that tenure-earnings profiles have steeper slopes than tenure-productivity profiles. (Medoff & Abraham 1980, 1981; Kotlikoff & Gokhale, 1992). This suggests that something other than human capital accumulation may be driving observed tenure-earnings profiles.[5] Lazear (1979) develops a theory in which tenure-earnings profiles[6] that are steeper than tenure-productivity profiles are a property of implicit contracts that exist between workers and firms.

These implicit contracts exist to address the following effort-elicitation problem:[7] suppose that the efficient labor contract (i.e., the contract that maximizes total social surplus) would stipulate high employee effort and high pay. Ideally, workers and firms could write a contract with these features. The problem, however, is that some types of workers are difficult to monitor so it is difficult to detect shirking by these workers. There is no full-employment equilibrium in which such workers are paid a wage equal to what their spot marginal revenue product (MRP) would be if they exerted high effort. If this were the prevailing wage rate, all workers would

[5] Other possible explanations for this phenomenon have been suggested. Social norms of fairness may dictate that workers should "move up the ladder" as they acquire greater tenure. Alternatively, workers may desire rising consumption over their lifetimes but be unable to discipline themselves to save (Neumark, 1995).

[6] Discussions of Lazear's theory have alternately referred to age-earnings profiles, experience-earnings profiles, and tenure-earnings profiles. In the context of the theory, these are all equivalent because workers are assumed to remain with the same firm for their entire careers. But the focus is rightly on firm tenure because LSICs involve the relationship between the worker and the specific firm.

[7] Actually, this mechanism can be employed to combat any kind of principal-agent problem where the agent's actions are not fully observable to the principal.

shirk since they would know that if they were caught and fired, they could find another job at the same wage. In light of this fact, firms would be unwilling to offer that wage. If workers are unable to commit to high effort, then both workers and firms will be forced into inefficient contracts which are characterized either by worker shirking (and a correspondingly low wage) or by a high level of costly monitoring.

Lazear argues that LSICs arose to combat this problem. Workers are paid below their MRP in the early years of their careers. The difference between pay and MRP in the early years of a career is held by the firm as a "bond" and is repaid in the form of compensation above MRP in the later years if the worker is still employed by the firm. If the worker shirks and is fired, he forfeits the bond.[8]

Using LSICs to induce employee effort can be superior to exclusive reliance on employee monitoring. Firms can maintain a given level of deterrent by making the tenure-earnings profile steeper (which makes the performance "bond" larger) and making an appropriate reduction in the probability of catching shirkers (i.e., by monitoring less).

There is a limit to the extent that LSICs can be used as a substitute for monitoring because steeper tenure-earnings profiles make it more difficult for firms to credibly commit to honoring their commitments to older workers. Furthermore, in order to attract workers, firms using LSICs will have to offer higher expected pay as compensation for the risk that the firm will renege.[9] For this reason, (feasible) LSICs are not always superior to costly monitoring. Since LSICs and monitoring are substitutes, the theory predicts that LSICs will be more attractive (and more common) in occupations where employee effort is more difficult to monitor.

[8] Workers have a greater incentive to cheat (or shirk) if the bond is small, and firms have a greater incentive to renege (by withholding the promised overpayment) if it is large. In equilibrium, there is still some cheating and some reneging.

Lazear's theory has been tested by a number of researchers (Lazear, 1979; Lazear & Moore, 1984; Goldin, 1986; Hutchens, 1987; Hellerstein, Neumark, & Troske, 1999; Lazear, 2000). The evidence, while mixed, provides a measure of empirical support for the theory.

III. The Decline or Demise of LSICs:

worker to credibly commit not to engage in behavior that, rationally anticipated by the firm, would make it unwilling to enter into the (mutually beneficial) LSIC.

In recent years the Age Discrimination in Employment Act (ADEA) has been amended in ways that essentially outlaw mandatory retirement in the United States. In 1978, mandatory retirement below age 70 was banned. In 1986 it was banned altogether in almost all occupations. Mandatory retirement can therefore no longer serve as a device to support LSICs. In his original (1979) paper, Lazear argues against the legal ban on mandatory retirement for exactly this reason.

The elimination of mandatory retirement does not make LSICs impossible. In principle, the LSIC could be structured to induce retirement at the efficient date by including a provision for a large wage cut at that date. This is probably not feasible, however; firms will not want to enter into such an arrangement since they know that it would be costly to actually make these wage cuts when the time came (possibly because workers cannot credibly promise not to agitate against these wage cuts when they arrive).[1010]

terms that had been (presumably) optimally chosen. Any major change in the accumulation of pension benefits would likely also necessitate (costly) changes in wages and other benefits.

There is some evidence that defined-benefit pension plans have not been employed to "rescue" LSICs following the ban on mandatory retirement. In recent years there has been a shift away from defined-benefit pension plans altogether in favor of defined-contribution plans, which cannot support LSICs (Gustman and Steinmeier, 1992; Kruse, 1995; Ippolito, 1995). This trend is even more pronounced for new plans. Of plans established in 1995, only 3.2% of the plans (containing 10.6% of the participants) were defined-benefit (Private Pension Plan Bulletin, U.S. Department of Labor, 1999). This trend suggests (but does not prove) that defined-benefit pension plans are not being modified on a wide scale in order to support LSICs.

B. Declining Benefits of LSICs

LSICs tend to make workers less likely to quit their jobs. In order to induce a worker who is party to an LSIC to quit, an outside job offer would not only need to be superior, but superior by enough to induce the worker to forfeit the "bond" that he expects to recoup in the form of overpayment when old.[13,14] LSICs also tend to make firms less likely to dismiss workers. In the absence of an LSIC the firm will dismiss any worker who is not productive enough to justify his compensation. But firms that are party to LSICs retain some workers that the firm would prefer to dismiss because dismissal might be perceived as a violation of the LSIC. Since LSICs prevent some (efficient) separations, they tend to lead to relatively long job matches.

[13] In an LSIC, the present discounted value of lifetime compensation (at the beginning of the contract) is equal to the present discounted value of MRP. But the underpayment in the early years of the contract is not fully repaid until the LSIC's last day. Therefore at any point in an LSIC, the present discounted value of the contract's *remaining* compensation exceeds the present discounted value of the remaining MRP.

[14] In his original (1979) paper Lazear shows that severance pay (i.e., returning the value of the unclaimed bond) can generate efficient separations. But severance pay is a limited phenomenon. It is certainly not the case that an

Since LSICs tend to limit efficient separations, anything that makes efficient separations more likely (i.e., anything that makes workers or firms more likely to prefer to separate), reduces the benefits of LSICs and so makes them less likely to be formed. Efficient separations are more likely when workers find moving less distasteful, because alternative job offers are more likely to be perceived as superior. They are also more likely when firms have access to technologies and/or organizational structures that are more flexible and therefore rely less heavily on firm-specific human capital. It is widely believed that both of these changes have taken place in recent decades.[15]

IV. Empirical Implications:

The purpose of this section is to develop empirically testable implications of the Hypothesis of this paper: that the formation of new LSICs between firms and young workers has declined or ceased.[16]

Let $\rho(t)$ represent the fraction of new entrants to the workforce at time t who enter into LSICs. Let t_d represent the date at which the net benefits of LSICs began to decline. Assume that $\rho(t)$

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employee can simply demand the unpaid portion of an LSIC bond whenever he finds a better opportunity outside the firm.

[15] There is substantial evidence of a decline in job stability for males over the last few decades. (See Jaeger & Stevens (1999) and Neumark, Polsky & Hansen (1999), for evidence. See Stewart (1999) for a contrary view. Also see Bernhardt, Morris, Handcock & Scott (1999) for evidence specifically related to young males). This is consistent with an increase in worker mobility and/or a decrease in the importance of firm-specific human capital. The job stability literature is not directly on point, however, because it only reflects changes in the equilibrium frequency of job separations. This may be influenced by factors other than worker tastes and firm production processes.

[16] In an earlier version of the paper, I tested the Hypothesis using an approach based on the one used by Lazear & Moore (1984) to demonstrate that LSICs did in fact exist. In that paper, the authors develop a modified version of Lazear's original theory so that it predicts a positive relationship between the slope of the age-earnings profile in an occupational category and lifetime earnings in that category. This positive relationship, if it exists, might be due to the presence of LSICs, or it might be the case that those occupational categories with steeper slopes have faster human capital accumulation, which leads to higher earnings. Lazear & Moore argue that data on self-employed workers can be used to purge the estimates (for wage and salary workers) of human capital accumulation effects since there are no incentive problems for self-employed workers. They argue that any remaining relationship between slope and lifetime earnings is due to LSICs. They used Current Population Survey (CPS) data from 1978.

begins to decline at t_d and continues to decline thereafter as workers and firms gradually adapt to the new environment. This means that, for all $t > t_d$, there is an (increasing) number of "types" of workers who do not enter into LSICs, but who would have done so had they entered the workforce before t_d. These types of workers have a different structure of compensation depending on when they entered the labor force.

This change in the structure of compensation for otherwise identical workers can generate testable empirical implications of the Hypothesis. If the rate of formation of LSICs among young workers entering the labor force is smaller after t_d (and decreasing after that), then the proportion of workers in the economy who are party to LSICs must also be smaller after t_d (and decreasing after that). Since LSICs tend to increase the returns to firm tenure, we can predict that average returns to firm tenure in the economy must decline after t_d. This prediction can be tested, with no additional assumptions beyond those listed above, as long as the sample period includes some years after t_d. The prediction can be stated as follows:

Prediction 1: *Returns to firm tenure declined after t_d.*

This is not a strong test, as there are a number of alternative reasons why returns to firm tenure might decline over time. But a stronger test can also be performed. If the Hypothesis were correct, we would not expect this decline in the returns to firm tenure to be the same for all levels of experience: specifically, we would expect the decline in the returns to firm tenure to be greater for low-experience workers. To see this, consider two dates: one shortly before t_d and one shortly after. The incidence of LSICs among high-tenure, high-experience workers will be

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I repeated their test on CPS data from every year from 1978-1999. While the results (available upon request) were consistent with the Hypothesis, a variety of empirical problems cast doubt on the validity of this approach.

the same on these two dates, while the incidence of LSICs for new entrants to the labor force (low-tenure, low-experience workers) will be smaller at the latter date. (The incidence of LSICs among low-tenure, high-experience workers is presumably low at both dates, as LSICs are not typically associated with older workers who start new jobs.) This means that, all else equal, the returns to firm tenure among high-experience workers (both high-tenure and low-tenure) should be the same on the two dates, whereas returns to firm tenure among low-experience workers should be lower in the later period.[17] This prediction can be stated as follows:

Prediction 2: *The decline in the returns to firm tenure was greater for younger workers.*

Note that testing Predictions 1 and 2 on a "full" sample of workers (including those who would not have been party to LSICs even before t_d) is conservative, as the predicted effects might be present but be attenuated by the presence of the other workers in the sample.

V. Data and Summary Statistics:

The data for this study come from the Panel Study of Income Dynamics (PSID). The earliest plausible date for t_d is 1978 (the year that mandatory retirement below age 70 was banned), so the ideal sample would begin before 1978. Unfortunately, the PSID did not ask about job tenure

in 1979 and 1980, so the sample begins in 1981 and runs through 1992. This sample restriction does not change the Predictions as long as the adjustment from the old equilibrium to the new one was not completed before the sample begins.

The main sample consists of male "heads" of households who are PSID sample members[18] and who are aged 25-54. Only these prime-age workers are included in order to avoid concerns related to the more erratic labor supply behavior of younger and older workers. Wages are deflated to 1976 dollars using the Consumer Price Index: All Urban Consumers (CPI-U). A person-year is included in the sample if the individual is a full-time (more than 1500 hours) worker in the private sector with hourly labor earnings greater than $1.50 who is not self-employed. Government and unionized workers are excluded because they are difficult to fire; since LSICs are based on the threat of firing, these workers are unlikely to be party to them. Observations from the Survey of Economic Opportunity low-income over-sample are also excluded. Summary statistics are presented in Column 1 of Table 1.

As discussed in the Appendix, the estimation approach that I use requires that the data for each individual be "partitioned" into different job matches. In other words, it is necessary to identify each job that an individual held during the sample period and to assign each observation to one of those jobs. It is not always obvious to which job match a particular observation belongs. Brown & Light (1992) show that biased coefficients on firm tenure (in wage equations using PSID data) can arise due to errors in assigneat oto t

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assumes that a job change has occurred whenever reported tenure is less than the elapsed time since the previous interview. I follow their recommendation and use partition T.[19]

VI. Estimation and Main Results:

A. Estimation

To test Predictions 1 and 2, I estimate the following wage equation:

$$(1)\quad \ln wage_{ijt} = \beta_1 TEN_{ijt} + \beta_2 TEN_{ijt}^{\,2} + \beta_3 EXP_{it} + \beta_4 EXP_{it}^{\,2} + \beta_5 TIME_t + \beta_6 TIME_t^{\,2} + \beta_7 TEN_{ijt} \cdot EXP_{it} + \beta_8 TEN_{ijt} \cdot TIME_t + \beta_9 EXP_{it} \cdot TIME_t + \beta_{10} TEN_{ijt} \cdot EXP_{it} \cdot TIME_t + \Gamma X + \alpha_i + \theta_{ij} + \varepsilon_{ijt}$$

TEN is firm tenure, *EXP* is potential work experience (age-education-6),[20] *TIME* is a linear time trend, and ***X*** is a vector of standard control variables including four education dummies, a non-white dummy, an ever-married dummy, four region dummies, and 2-digit 1970 census industry and occupation dummies.[21] α and θ are individual specific and job-match specific

$$\frac{\partial^2 \ln wage}{\partial TEN \cdot \partial TIME} = \beta_8 + \beta_{10} EXP < 0$$

Prediction 2 is that this effect is less pronounced for more experienced workers, or:

$$\frac{\partial^3 \ln wage}{\partial TEN \cdot \partial TIME \cdot \partial EXP} = \beta_{10} > 0$$

There is considerable controversy regarding the correct method for estimating returns to firm tenure (Abraham & Farber, 1987; Altonji & Shokotko, 1987; Topel, 1991). I follow Parent (2000) and use the instrumental variables (IV) approach of Altonji & Shokotko (described in the Appendix). Since the PSID follows individuals over time, there is an individual-specific component in the error term. In order to get the correct standard errors, a cluster correction is performed.

B. Main Results

Full-sample IV regression results for three different specifications of the wage equation are reported in Table 2. Two sets of results are reported for each specification: one that includes dummies for the two-digit 1970 census industries and occupations, and one that does not. For comparison purposes, OLS results (for Specification 3 only) are reported as well. In this section I will discuss the Specification 3 IV results (with industry and occupation dummies included), which are the most important since Specification 3 corresponds to Equation 1 above.

[21] Industries and occupations with fewer than 25 observations are omitted.

The IV estimate of β_8 is -.0081 and IV estimate of β_{10} is equal to .00035. Both of these coefficients are significant at the 1% level. These results confirm Prediction 1: returns to firm tenure have decreased on average.[22] They also confirm Prediction 2: the decline in returns to firm tenure is large for low-experience workers, but not for high-experience workers.

VII. Alternative Explanations and Additional Results:

As discussed above, the main empirical analysis confirms that tenure-earnings profiles have changed according to Predictions 1 and 2. But this does not necessarily constitute confirmation of the Hypothesis; it remains possible that the Hypothesis is false but that some other phenomenon has caused Predictions 1 and 2 to be confirmed. The purpose of this section is to examine the extent to which it is possible to be confident that the changes in tenure-earnings profile discussed above are due to the hypothesized cause rather than to some alternative one.

A. Alternative Explanations

The most obvious candidates for alternative explanations include changes in technology and in worker tastes for mobility. I consider each of these in turn:

1. Technology:

Suppose that technology changes at time t_{tech} so that firm-specific human capital is less valuable than it was before. For simplicity and without loss of generality, assume that only low-

[22] The functional form of Equation 1 imposes a linear relationship between the change in returns to tenure and experience, which means that, as long as $\beta_{10} > 0$, the change in returns to tenure will eventually become positive. Since this relationship is positive only at high levels of potential experience, we can be confident that, on average, the change is negative. Another way to see this is to look at Specification 2, where the IV estimate of the coefficient on the interaction term between the time trend and the tenure variable is equal to -.0047, which is significant at the 5% level.

experience workers invest in firm-specific human capital and that the level of investment depends on the state of technology. Those workers who entered the work force after time t_{tech} would have less firm-specific human capital (due to the reduced investment) and also a lower return (due to the technology change). Those workers who entered the work force before time t_{tech}, on the other hand, would have an unchanged level of firm-specific human capital, but would still have a lower return. This technology-based explanation would generate both Prediction 1 (all workers have a decline in returns to firm tenure), and Prediction 2 (the decline in returns to firm tenure is larger for low-experience workers).

If the technology change were small, then the change in the average return to firm tenure would also be small. As can be seen in Specification 2, this is not the case: the coefficient on the interaction between firm tenure and the time trend is equal to -.0047, and is significant at the 5% level. In order for the technology explanation to be correct, the technology change during the sample period would have had to be substantial.

2. Tastes:

Suppose that tastes changes at time t_{taste} so that low-experience workers object to changing jobs less than they had in the past. For simplicity and without loss of generality, assume that only low-experience workers invest in firm-specific human capital and that the level of investment depends on the worker's tastes (workers who believe that a preferred job opportunity is likely to come along invest less in firm-specific human capital). Those workers who entered the work force after time t_{taste} would have less firm-specific human capital (due to the reduced investment) and an unchanged return. Those workers who entered the work force before time t_{taste}, on the other hand, would have an unchanged level of firm-specific human capital and an

unchanged return. This taste-based explanation would generate both Prediction 1 (on average, workers have a decline in returns to firm tenure), and Prediction 2 (the dec

would not have done so (Category B). This division would permit the following test: if the Hypothesis were correct, we would expect Predictions 1 and 2 to hold for the Category A sub-sample, but not for the Category B sub-sample, because only in the Category A sub-sample is there a decline in the proportion of workers who are entering into LSICs: in the Category B sub-sample, no LSICs were being formed in the first place. As a practical matter, however, any actual division of the sample will only approximate this ideal division. This means that both sub-samples will contain some workers who would properly be assigned to Category A. For this reason, we would expect that Predictions 1 and 2 would hold to some extent for both sub-samples, but would hold more strongly for the one that is intended to approximate Category A.

To understand what it means for the Predictions to hold "more strongly" for one sub-sample than for the other, consider the following thought experiment: suppose we had a perfect mechanism for assigning workers to the correct category. In this case, we would expect the following results (if the Hypothesis is correct): (i) there would be no change in the returns to tenure for workers in Category B, regardless of the level of experience; and (ii) there would be a decline in the returns to tenure for low-experience workers in Category A, but this decline would approach zero at high levels of experience. Since the functional form of Equation 1 imposes a linear relationship between the change in returns to tenure and experience, this corresponds to a decline in returns to tenure for low-experience workers, and an increase in returns to tenure for workers with sufficiently high experience. Figure 1 plots the predicted relationship between the change in returns to tenure and experience for the two "perfect" Categories.[23] If the estimation produced results such as those in Figure 1, we would say that the Hypothesis holds for Category A, but not for Category B. In practice, the imperfect method for assigning workers to categories

means that both sub-samples will contain some workers who would properly be assigned to Category A. This means that we would expect the Category B line to be upward sloping, but to start higher and have a flatter slope than the Category A line. See Figure 2 for an illustration. If the estimation produced results such as those in Figure 2, we would say that the Hypothesis holds more strongly for Category A than for Category B

In order to split the sample in the manner described above, it is necessary to find some

Occupations."[25] I omit job matches in which the worker's white-collar/not-white-collar status changes within the match. For this reason, the sum of the sample sizes of the two sub-samples is smaller than the sample size of the main sample (4802 vs. 6395). Summary statistics for the two sub-samples are presented in Columns 2 and 3 of Table 1.

Regression results for the white-collar and not-white-collar sub-samples are reported in Tables 3a and 3b. Two sets of results are reported for each specification: one that includes dummies for the two-digit 1970 census industries, and one that does not. Occupation dummies are omitted because the white-collar and not-white-collar categories are themselves derived from the occupation variable. For the white-collar sub-sample, the IV estimate of β_8 is equal to -.0047 in the specification that controls for industry, and is equal to -.0063 in the specification that does not. Neither of these coefficients is significant at conventional levels. The IV estimate of β_{10} is equal to .00024 in the specification that controls for industry, and is equal to .00033 in the specification that does not. Neither of these coefficients is significant at conventional levels.

For the not-white-collar sub-sample, the IV estimate of β_8 is equal to -.0120 in the specification that controls for industry, and is equal to -.0133 in the specification that does not. Both of these coefficients are significant at the 1% level. The IV estimate of the coefficient β_{10} is equal to .00046 in the specification that controls for industry, and is equal to .00048 in the specification that does not. Both of these coefficients are significant at the 1% level.

[25] I performed a similar exercise by splitting the sample between high and low levels of the "substantive complexity" of an occupation. The substantive complexity variable is a weighted average of a number of occupational characteristics as measured by the Dictionary of Occupational Titles (DOT). The DOT defines about 14,000 occupations and rates each one according to a large number of such characteristics. That is, it assigns each occupation a score for each characteristic. I follow Roos & Tremain (1980) in converting these scores from the DOT occupations to the 1970 3-digit census occupations. There are many more DOT occupations than there are 3-digit census occupations. The score for a given census occupation for each characteristic is the employment-weighted average of the scores of the constituent DOT occupations. An approach along these lines was first used by Hutchens (1987) to test for the existence of LSICs. The results for this sample division are similar to those for the "white-collar" vs. "not-white-collar" division and so are not reported.

These coefficients all have the correct signs (though they are statistically insignificant in the white-collar sub-sample). Both sub-samples exhibit a decline in the returns to firm tenure for low-experience workers, and the magnitude of this decline diminishes with experience for both sub-samples. However, contrary to what we would expect if the Hypothesis were correct, these results hold more strongly for the not-white-collar sub-group. These results are illustrated in Figures 3a (based on the specification that includes industry dummies) and in Figure 3b (based on the specification that does not).

The split-sample results do not support the Hypothesis (because the results were stronger for the not-white-collar sub-sample), but do not strongly refute it either (because both sets of coefficients had the correct signs). This lack of support may mean that they Hypothesis is false, or it may be a reflection of the weaknesses of the split-sample approach. These weaknesses include: (i) the crudeness of the method used to determine who is likely to be party to an LSIC,[26] (ii) the imprecision of the regression results (particularly for the white-collar sub-sample), and (iii) the possibility that the two sub-groups had different experiences in terms of the accumulation of and/or returns to human capital during the sample period.

2. Female Sample

To perform this test, I use a sample of female workers. The female sample is defined in the same way as the male sample, except that it consists of female PSID household heads and of female "wives" who are married to male household heads. Summary statistics are presented in Column 4 of Table 1.

[26] Monitoring difficulty is only one factor that influences the likelihood that an LSIC will form. Other factors include the size and age of the firm (workers will only make implicit contracts with firms that are expected to be around decades in the future to honor them), the degree of intrinsic motivation of the employees (which might be

may be much smaller. For this reason, the specification that omits the industry and occupation dummies may be the preferred one. These results are illustrated in Fi

been for the legal ban on mandatory retirement. On the other hand, perhaps the benefits of LSICs were declining to the point that they would have eventually ceased to be formed even if mandatory retirement had remained legal. It is unclear whether the ban on mandatory retirement is the primary cause for the decline of LSICs, or if it is merely the trigger.

If in fact LSICs are no longer being formed with younger workers, a number of interesting things may come about as a result. It is widely believed that in the 1990s the U.S. entered a period of widespread corporate "downsizing" in which firms laid off workers -- often well-educated workers with high tenure -- to an extent far greater than in the past. Recent empirical studies have lent some support to this belief. This phenomenon may be due in part to the decline of LSICs for younger workers. If firms are no longer forming LSICs with young workers, then they have a diminished incentive to protect their reputations for trustworthiness and therefore have a greater incentive to renege on their obligations to older workers with LSICs by laying them off. I develop and test this argument in another paper (Balan, 2002).

The demise of LSICs will tend to cause heavier reliance on alternative mechanisms for dealing with principal-agent problems. This may include more monitoring. It may also include heavier reliance on efficiency wages. In future research, I plan to investigate whether or not efficiency wages became more heavily used as LSICs declined.

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the main sample in Figures 4a and 4b are based on the specification that omits the nonwhite variable.

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one of high ability and the other of low ability. The high-ability individual will tend to have high tenure and the low-ability individual will tend to have low tenure. But neither is more likely than the other to have a high value of the tenure instrument because for every individual, every value that the instrument takes on is balanced by another value of the same magnitude and the opposite sign. A similar argument demonstrates that tenure is uncorrelated with θ_{ij}. The tenure instrument is correlated with tenure. Within a given job, higher values of tenure always

Table 1: Descriptive Statistics

Variable	Full Sample	"White-Collar" Sub-Sample	"Not White-Collar" Sub-Sample	Female Sample
Age (years)	36.42 (7.84)	37.53 (7.82)	35.58 (8.19)	37.20 (8.41)
Percent Education < 12	.109	.016	.224	.102
Percent Education = 12	.318	.128	.477	.468
Percent Education = 13-15	.223	.200	.184	.254
Percent Education >=16	.351	.657	.116	.175
Percent Nonwhite	.061	.053	.093	N/A
Percent Ever Married	.884	.869	.893	.850
Percent in Northeast	.199	.266	.150	.218
Percent in North Central	.300	.301	.287	.247
Percent in South	.329	.263	.396	.338
Percent in West	.171	.169	.168	.197
Firm Tenure	7.10 (7.04)	7.89 (7.82)	5.56 (6.32)	5.83 (5.72)
Potential Experience	16.80 (8.30)	16.29 (8.09)	17.37 (8.80)	18.26 (9.00)
Annual Hours Worked	2322 (424)	2348 (418)	2283 (444)	2053 (353)
Hourly Labor Earnings (1976 dollars)	7.96 (5.36)	10.64 (6.78)	5.53 (3.01)	5.00 (2.81)
Number of Individuals	1193	464	663	1048
Number of Observations	6395	2248	2554	4381

Notes:

a. The full sample, the "white-collar" sub-sample, and the "not white-collar" sub-sample consist of male household "heads" who are PSID sample members between 25 and 54 years of age who have full-time, private-sector, non-union jobs and who are not self-employed. The "female" sample is similar but consists of female household heads and of "wives."

b. Standard deviations in parentheses.

c. Note that the sum of the observations in the male sub-samples is less than the number of observations in the full male sample. This is because the sub-samples do not include matches where the status changes within the match.

Table 2. Full Sample Wage Equation Estimates

	(1)		(2)		(3)			
	Without Industry and Occupation Dummies	With Industry and Occupation Dummies	Without Industry and Occupation Dummies	With Industry and Occupation Dummies	Without Industry and Occupation Dummies		With Industry and Occupation Dummies	
Variable	IV Estimates	IV Estimates	IV Estimates	IV Estimates	OLS Estimates	IV Estimates	OLS Estimates	IV Estimates
Tenure	.0331***	.0250***	.0350***	.0292***	.0287***	.1397***	.0268***	.1019***
	(.0043)	(.0042)	(.0055)	(.0055)	(.0077)	(.0306)	(.0067)	(.0272)
Tenure squared	-.0010***	-.0005**	.0027***	.0022**	-.0009***	.0016**	-.0008***	.0008
	(.0002)	(.0002)	(.0010)	(.0011)	(.0003)	(.0008)	(.0002)	(.0007)
Potential Experience	.0203***	.0210***	.0308***	.0298***	.0245***	.0293***	.0244***	.0303***
	(.0063)	(.0053)	(.0091)	(.0084)	(.0058)	(.0092)	(.0052)	(.0077)
Potential Experience squared	-.0002	-.0004**	-.0012***	-.0011***	-.0004***	.0002	-.0004***	-.0001
	(.0002)	(.0001)	(.0004)	(.0004)	(.0002)	(.0003)	(.0001)	(.0002)
Time Trend	-.0181***	-.0136**	.0420*	.0288	-.0077	.0393**	-.0058	.0269*
	(.0069)	(.0063)	(.0216)	(.0217)	(.0091)	(.0156)	(.0082)	(.0138)
Time Trend squared	.0012**	.0009**	.0005	.0005	.0011**	.0018***	.0009*	.0014***
	(.0005)	(.0005)	(.0007)	(.0006)	(.0005)	(.0006)	(.0005)	(.0005)
Tenure*Potential Experience					.0005	-.0059***	.0003	-.0040***
					(.0004)	(.0016)	(.0004)	(.0014)
Tenure*Time Trend			-.0062***	-.0047**	-2.71E-05	-.0113***	-.0004	-.0081***
			(.0020)	(.0022)	(.0011)	(.0032)	(.0009)	(.0028)
Potential Experience*Time trend					-.0005	-.0029***	-.0005	-.0024**
					(.0004)	(.0011)	(.0004)	(.0010)
Tenure*Time Trend*Potential Experience					.00001	.00046***	.00003	.00035***
					(.00004)	(.00014)	(.00004)	(.00012)
R-Squared	.3698	.5144	.0074	.2681	.3900	.3032	.5169	.4801
Number of Observations	6395	6395	6395	6395	6395	6395	6395	6395

Notes:
a. Dependent variable is log of hourly labor earnings.
b. IV results are reported for Specifications 1 - 3. OLS results are reported for Specification 3 only.
c. The full sample consists of male household "heads" who are PSID sample members between 25 and 54 years of age who have full-time, private sector, non-union jobs and who are not self-employed.
d. Unreported regressors include four education dummies, a nonwhite dummy, an ever-married dummy, and four region dummies. The "With Industry and Occupation Dummies" Specifications also include 2-digit 1970 census industry and occupation dummies.
e. Standard errors are in parentheses.
f. A clustering correction is made allowing the error terms for the observations of an individual to be correlated.
g. One, two, and three stars indicate significance at the 10, 5, and 1 percent levels, respectively.

	Without Indsutry Dummies	With Industry Dummies	Without Indsutry Dummies	With Industry Dummies				
Variable	IV Estimates	IV Estimates	IV Estimates	IV Estimates	OLS Estimates	IV Estimates	OLS Estimates	IV Estimates
Tenure	.0414***	.0404***	.0459***	.0475***	.0253**	.1617***	.0310***	.1505***
	(.0070)	(.0070)	(.0086)	(.0088)	(.0127)	(.0472)	(.0115)	(.0423)
Tenure squared	-.0016***	-.0013***	.0015	.0011	-.0014***	.0011	-.0014***	.0002
	(.0003)	(.0003)	(.0010)	(.0010)	(.0004)	(.0014)	(.0004)	(.0012)
P1"fential Experienc		.04020-4679(.00168-4679(.00177-4679(.00196-4679(.00198-4679(.00153-4679(.00119-46498.02500)]TJ20.6 -1.273 Td [(.00787))-4013((.0070))-4013((.00101))-4(						
	(.00032	(.00042	(.0004)	(.0003)	(.00142	(.0004)	(.00032	(.0004)Teime Tren
				(.00717	(.01151	(.00500	(.0118)	(.01236

	-.00358**	.00028-34516-	
	(.00507	(.01129	(.0118)

	Without Industry and Occupation Dummies	With Industry and Occupation Dummies	Without Industry and Occupation Dummies	With Industry and Occupation Dummies				
Variable	IV Estimates	IV Estimates	IV Estimates	IV Estimates	OLS Estimates	IV Estimates	OLS Estimates	IV Estimates
Tenure	.0385***	.0361***	.0428***	.0418***	.0485***	.0770*	.0503***	.0931***
	(.0052)	(.0049)	(.0065)	(.0058)	(.0105)	(.0416)	(.0096)	(.0316)

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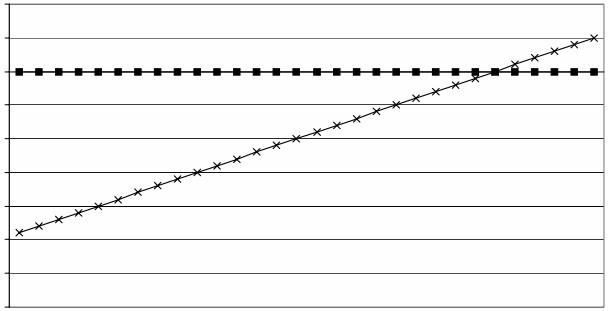

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Figure 4a--Main (male) vs. Female
(based on regressions with industry and occupation controls)