SHEEPSKIN EFFECTS BY COHORT: IMPLICATIONS OF JOB MATCHING IN A SIGNALING MODEL

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In the presence of job matching, the returns to education signals are shown to decline in value as additional work experience allows more direct observation of productivity. This is tested by estimating sheepskin effects across five age cohorts of non-minority males in 1991. The effects are large and significant in early cohorts and virtually nonexistent in later cohorts. This pattern is partially confirmed with estimations within cohorts showing sheepskin returns declining from 1979 to 1991. The pattern within cohorts suggests that the 1991 pattern is not merely the result of vintage effects but caution is expressed in drawing conclusions.

1. Introduction

More than two decades ago Spence (1973) and Arrow (1973) each published research which formalized the hypothesis that education served as a credential which signaled high innate productivity. Researchers since that time have contested whether or not returns to education signals should decline as workers gain increased labor market experience. This paper contends that the relationship between experience and returns to education signals deserves further inquiry. In particular, we suggest that within a job-matching framework, there exist strong reasons to suspect that the returns to education signals will attenuate with workforce experience. Indeed, contrary to the examinations of two decades ago, we find empirical support for just this pattern by examining sheepskin returns to education in the United States.

Hungerford and Solon (1987) confirmed the existence of sheepskin effects in the returns to education. Using both spline and step functions, they identified significantly larger returns to diploma years than to other years of education for US workers. The finding that the diploma had value apart from the accumulated years of schooling indicated that education credentials workers as more productive in addition to any role it actually plays increasing their productivity. This was a natural conclusion given that earlier work took the absence of sheepskin returns as sufficient to dismiss the signaling (or screening) hypothesis altogether (see Layard and Psachopoulos, 1974).

Additional work (Belman and Heywood 1991) supports the estimates of Hungerford and Solon presenting a nearly identical 9–10% return for the

Sheepskin effects are the returns specific to educational credentials rather than to accumulated years of education. The term follows from the tradition of presenting diplomas on parchments usually made from the skin of a sheep. Such parchments date from the second century BC in Asia Minor where they replaced papyrus.

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credential of a college degree. This research also goes beyond the white male sample of Hungerford and Solon to examine the pattern of sheepskin effects for women and racial minorities. Although the pattern is somewhat ambiguous, it roughly supports the generalization that women and minorities receive larger sheepskin effects for college and graduate school but smaller sheepskin effects for high school graduation.

Heywood (1994) found that the evidence for sheepskin effects varied by the institutional structure of the labor market. The size and strength of such effects is greatest in the private non-union sector of the economy. Sheepskin returns in the public and union sectors are much smaller and often statistically insignificant. To the extent that one suspects private non-union employers of being most concerned with (or most able to execute) cost minimization, the stronger sheepskin effects for these employers may provide added evidence of the importance and economic value of education signals.

Jaeger and Page (1996) found strong evidence of sheepskin effects using not only the traditional measure of years of schooling but also using data which identified the actual degree completed. Indeed, using this more precise measure (e.g. high school diploma vs. 12 years of school) they produced larger estimated sheepskin effects. They fail, however, to confirm a pattern of effects that differs by gender or race.

The discussion so far makes clear that the existence of sheepskin effects has been taken as evidence of signaling and that the pattern of effects by race and gender has been suggested as evidence of differential signal quality across demographic groups. None of the above studies, or others that claim to have identified education as a signal (see Lang and Kropp, 1986, and Weiss, 1988, for instance), should be taken as evidence that education cannot improve skills. They do, however, indicate that further implications about the nature and strength of education signals should similarly be examined by further isolating the pattern of sheepskin effects.

Our contention that sheepskin effects should attenuate with work experience would not surprise those unsympathetic to the signaling hypothesis. Layard and Psacharopoulos (1974) argue that the return to educational signals 'will fall with experience as employers come to have better information about their employees’ real productivity (p. 992). Their examination of the relevant studies of the time shows no such decline, thereby providing evidence against the signaling hypothesis. Riley (1979), who is sympathetic to the hypothesis, rejects the original argument and so finds the lack of evidence irrelevant. He points out that a basic condition for a signaling equilibrium is that employers find out that their predictions based on the education signals are correct on average. Thus, as the employer gains additional information about the actual productivity of a group of workers, earnings may be adjusted, but for any particular

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2 Indeed, we agree with Layard and Psacharopoulos (1974, p. 989) who claim that ‘no one ... would maintain that all earnings differentials are in fact due to screening... Equally, no sane person would deny that the idea had some truth in it.’
educational signal the wage increases should offset the wage decreases leaving the original relationship between the signal and the average wage in place. Farber and Gibbons (1996) present a sophisticated demonstration of this point in a regression context. As long as the new information acquired with work experience is orthogonal to the education signals, the observed return to those signals will not decline.

Yet, the assumption that all new information is orthogonal to educational signals acquired early in life seems unrealistic. As the worker has greater labor market experience, employers will come to know more about the actual productivity of the worker or will have stronger signals on which to base a hiring or compensation decision. Thus, after 10 or 20 years of experience, job matching will be improved as workers are sorted into more appropriate positions. Real productivity gains will result, gains likely to be associated with the original educational signal. The consequence will be that educational credentials will not be as important in determining earnings for those with greater work experience.

In what follows, the next section details our argument that the returns to education signals should attenuate with work force experience. The third section describes our methodology and data and presents results for five cohorts of nonminority males. The fourth section examines the possibility that vintage effects drive the empirical results. The final section draws several tentative conclusions and briefly discusses alternative explanations.

2. Education signals and work experience

Our contention that the observed return to education signals should decline with additional work experience rests on the assertion that as the employer (and employee) learn more about worker productivity, behavior will change in ways not sufficiently considered in earlier work. Specifically, the revelation of new information about worker productivity leads to improved matching of workers and jobs. This is a common view of the labor market and implies relaxing the assumption that productivity is fixed over time (see Waldman, 1984, and Gibbons and Katz, 1991, for asymmetric information models and Gibbons and Katz, 1992, for a model with symmetric information like the one described below). Instead, worker productivity increases with better job matches. Yet, because the opportunities for improved matching are not evenly distributed across educational signals, the returns to those education signals are lower after improved matching than initially. This argument is now given a brief axiomatic treatment.

3 As Waldman (1984) points out, the worker's employer will directly learn of productivity and other potential employers will indirectly learn of productivity by observing the first employer's response in such areas as job assignment and changes in earnings.

4 Note that this is separate from the contention that education itself will not be correlated with earnings. It still may be. Thus any methodology must control for the return to years of education in an effort to identify the value of the signal or credential per se.
Imagine \( N \) ordered worker types consisting of equal numbers of workers, \( T_i, i = 1, 2, \ldots, N \). The productivity of each worker depends on their particular job match but can never exceed that associated with their worker type, \( v_i \), where \( v_i > v_{i-1} \). Imagine \( N \) ordered job types also consisting of equal numbers of jobs, \( t_i, i = 1, 2, \ldots, N \). When a worker of type \( T_i \geq T_j \) is matched with a job of type \( t_j \), the resulting value of marginal product is \( v_j \). For all other matches, worker type \( T_i < T_j \) being matched with job type \( t_j \), the value of marginal product is \( v_j \). Thus, workers can realize their full productivity only when matched with a job corresponding to their type or higher.

Now consider the purchase of an ordered educational credential which signals productivity, \( S_i, i = 1, 2, \ldots, N \). In particular assume that of those who have purchased any given \( S_i \), the proportion \( \alpha > 1/N \) are of type \( T_i \). This requirement implies that the signal has informational content and is, indeed, an indicator of productivity. To simplify the analysis we assume \( 1/N \) of all workers purchase each of the signal types. The actual proportion that purchase the signal depends on the relative costs and benefits to each worker. The assumption of \( 1/N \) purchasing simplifies the presentation but is not crucial to the point. The signaling model breaks down only when all or none of the workers purchase a particular signal.

In the first period the type \( t_j \) firms hire workers with the signal of productivity \( S_j \). They pay a wage based on the expected productivity of the job matches. The resulting first period wage at firm type, \( t_j \), follows then from the expected productivity of their workforce

\[
 w_j^1 = \alpha v_j + (1 - \alpha)(1/(N - 1))[\sum_{i=1}^{N-j} v_i] 
\]

Here the first term captures the share of hires who are exactly of type \( j \) and the second term captures the share of workers who are not of type \( j \). Of the second group, some are of types greater than \( j \), "over matched", and have productivity \( v_j \) while some are of types less than \( j \), "under matched", and earn their respective productivities \( v_i \).

The first period return to any two adjoining educational signals can be easily computed as the difference in expected wages. This difference is determined by lagging (1) by one educational signal and subtracting the lagged value from (1)

\[
 w_j^1 - w_{j-1}^1 = \alpha(v_j - v_{j-1}) + (1 - \alpha)((N - j)/(N - 1))v_j > 0 
\]

This difference reflects the higher earnings of the share \( \alpha \) of workers exactly matched and it reflects that those workers over matched will have higher productivity in firm type \( j \) than in firm type \( j - 1 \).

In the second period the true type of each worker is revealed.\(^5\) \( T_i \) workers are

\(^5\)This is obviously an extreme version. It is possible that there is simply better information on productivity and that the share of correct matches increases but that some incorrect matches remain.
all now bid to \( t \) firms and receive wages equal to their properly matched productivity. Of interest is the second period return to the educational signal. Of those who purchased \( S_j \), \( \alpha \) now receive \( w_j = v_j \) and the remainder, \((1 - \alpha)\) are evenly distributed across the other types of firms earning \( w_i = v_i \neq v_j \). The resulting average wage for those with educational signal \( S_j \) can be computed

\[
w_j^2 = \alpha_j v_j + (1 - \alpha)(1/(N - 1)) \left( \sum_{i \neq j} v_i \right)
\]

Again, by lagging one education signal and subtracting from the earnings in (3), the second period return to adjacent signals can be determined

\[
w_j^2 - w_{j-1}^2 = \alpha(v_j - v_{j-1}) - (1 - \alpha)(1/(N - 1))(v_j - v_{j-1}) > 0
\]

with the sign unambiguous as \( \alpha > 1/N \).

As returns to any adjoining signals are positive in both the first and second period, the ultimate issue is whether the return in the later period, after worker productivity is known, is below the return in the first period. The return in the second period can be subtracted from that in the first

\[
(w_j^2 - w_{j-1}^1) - (w_j^2 - w_{j-1}^2) = (1 - \alpha)[(N - j - 1)/(N - 1)](v_j - v_{j-1}) > 0
\]

The difference is positive, indicating an unambiguous decline in the return to the educational signal in the second period.

The decline reflects two related facts. First, undermatched workers with a given signal enjoy second period wage increases greater than the wage declines suffered by those overmatched. Thus, \( w_j^2 \geq w_j^1 \) for any signal \( j \). This seems the essence of a matching model. The average productivity of those with any given signal is no lower (and generally higher) after proper matching. Second, the increase in average productivity is greater for those with the lowest education signal. For high education signals the average wage in the first period is very close to that in the second as only very few workers are undermatched. Indeed, for those with the highest signal, \( S_N \), the wage does not change: \( w_N^2 = w_N^1 \). This follows because no worker will be reassigned in a manner that allows greater productivity. The consequence of larger increases in productivity for those with lower educational signals is a decline in the return to the signal.

Note also that the first period wage associated with each firm type falls below that associated with the corresponding worker type. Thus, the value of \( w_j^1 \) (in eq. (1)) is less than \( v_j \) as the undermatched workers hold down the first period wage. This difference grows as the share of undermatched workers grows. There are obviously no undermatched workers associated with the lowest firm type. As the earnings for low firm types increase little or not at all and those for high firm types increase substantially, the earnings variance is greater in the second period. This result, familiar from earlier signaling models, persists in our matching model.

This discussion illustrates that our model differs from traditional ones in only two fundamental respects. First, aggregate productivity is increased by
more appropriate job matches. Thus, new information does not just allow a better congruence between individual fixed worker productivity and wage but it causes productivity and wages to increase. Second, the increases in productivity are not orthogonal to the existing educational signals. The mean increase in productivity is highly (and negatively) correlated with the educational signal. These differences represent a sharp departure from Farber and Gibbons (1996) who clearly assume that any new information is orthogonal to the existing signals. In a regression context, our model amounts to the assumption that first period estimates of worker productivity based on education signals fail to include as a relevant variable the quality of the job match. This quality represents a correlated omitted variable biasing up first period returns to education signals.

To illustrate the role of our assumptions, imagine that the productivity of workers does not vary with job matches. In this case firm type $t_j$ pays a first period wage identical to the expected productivity of its workers all of whom present signal $S_j$:

$$w^1_j = \alpha_j v_j + (1 - \alpha)(1/(N - 1)) \left( \sum_{i \neq j} v_i \right)$$

(6)

where regardless of the match all workers have the productivity of their worker type. While this wage is shared by all workers with the signal $S_j$ regardless of their actual productivity, it is identical to the average wage that will be paid to these workers once their productivity is known (as shown by eq. (3)). Thus, even though the new information causes each worker's wage to equal their productivity, the returns to educational signals do not change with that new information (eqs (3) and (6) are identical). This result shares the flavor of those from Riley (1979) and Farber and Gibbons (1996).

Despite the differences in our presentation, there is little outside the usual flavor of signaling models. Indeed, the basics of the equilibrium are in place as the Appendix indicates. Additional experience increases the chance that workers will be placed in good matches, increasing aggregate productivity but reducing the return to education signal. At issue is whether this predicted pattern can be confirmed in empirical estimation.

3. Data and results

The requirements for this study demand a large individual based data source. The 1991 'outgoing rotation file' derived from the Current Population Survey is used as it includes well over 100,000 observations on employed workers. These observations will be needed as divisions by race, gender, and cohort

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6. Thus our model might be considered part of a broader class in which workers are likely to suffer shocks in productivity correlated with educational signals.

7. See Altonji and Pierret (1996) for a model that predicts declining returns to educational signals but retains more of the assumptions of Farber and Gibbons (1996).
can yield subsamples that are a small proportion of the original sample. We begin by limiting our attention to non-agricultural non-black males between the ages of 24 and 65. Following Hungerford and Solon (1987), we estimate a discontinuous spline function with years of education, \( S \), divided into three regions. The discontinuities are at years of education equal to eight and 12. Thus, the return to a year of education is allowed to vary between elementary school, secondary school, and college. The dependent variables is the natural log of the ratio of usual earnings to usual hours, \( \ln W \). Following the specification in both Hungerford and Solon (1987), and Belman and Heywood (1991), other independent variables include a constant, years of experience, experience squared, and dummy variables equal to whether the worker has years of education greater than or equal to eight (\( D_8 \)), greater than or equal to 12 (\( D_{12} \)), greater than or equal to 16 (\( D_{16} \)), equal to 17 (\( D_{17} \)), and equal to 18 (\( D_{18} \)). Thus, the final specification can be written:

\[
\ln W = \beta_0 + \beta_1 ex + \beta_2 ex^2 + \beta_3 S + \beta_4 D_8 + \beta_5 D_8(S - 8) + \beta_6 D_{12} + \beta_7 D_{12}(S - 12) + \beta_8 D_{16} + \beta_9 D_{16}(S - 16) + \beta_{10} D_{17} + \beta_{11} D_{18} \tag{7}
\]

The coefficients on the dummy variables, especially those for years 12 and 16 (\( \beta_8 \) and \( \beta_6 \)), estimate the sheepskin effects.\(^8\) The discontinuous spline function in (7) is represented in Fig. 1 with the coefficients \( \beta_6 \) and \( \beta_8 \) among those highlighted. To test the hypothesis that the return to education signals

\(^8\)Hungerford and Solon (1987) use the May 1978 CPS for their examination which also included a variation which added years of education to power two and to power three as explanatory variables. Adding such variables violates tolerance requirements in some of our smaller cohorts so we forego this variation.
should fade with experience, the sample of non-black males is divided into five
cohorts based on age. These are ages 24 to 30, 31 to 40, 41 to 50, 51 to 60, and
61 to 65. Table 1 presents descriptive statistics for these five cohorts. A total
sample of 68,838 is available making it unlikely that the absence of sheepskin
effects is the result of insufficient degrees of freedom. The education level across
the cohorts is similar but not identical. Those in the early and late cohorts have
slightly lower average achievement. The pattern of earnings across the cohorts
is what might be expected, increasing through much of the profile but decreasing
toward the end. The dispersion in earnings follows that predicted with the
standard deviation of the earnings measure increasing monotonically with the
cohort age. A standardization dispersion measure, the coefficient of variation,
shows a less dramatic increase with the first three cohorts being essentially the
same and the final two substantially larger.

For each cohort the spline function described above in (7) is estimated with
special attention paid to the pattern of sheepskin effects. Table 2 presents these
estimates. They reflect a single ‘stacked’ equation in which each variable in (7)
is entered separately by cohort using cohort specific interactions. The youngest
cohort generates a statistically significant sheepskin effect for a college degree
of about 12.0% (based on the coefficient for the grade 16 dummy). This esti-
mate is a couple of percentage points above the full sample estimates presented
earlier by Hungerford and Solon (1987). There is also evidence of a smaller but
still positive and significant sheepskin effect for high school graduation. Many
of the other controls are also statistically significant. Note that the negative
coefficient on the dummy for year 17 does not mean that this year lowers
earnings, only that it brings a low return. This coefficient must be subtracted
from the others in the spline function to compute the net return.

In keeping with the hypothesis, the second cohort reveals smaller sheepskin
effects than the youngest cohort. The sheepskin effect for the college degree
year remains statistically significant but drops 6.3 percentage points to 5.7%. A
similar, but not as dramatic, drop appears also for the sheepskin effect for the
high school graduation year. In the third cohort, the sheepskin effect estimates
for college and high school have declined further. Indeed, they are no longer
Table 2
Earnings regressions for 1991 non-black males

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>0.980</td>
<td>1.304</td>
<td>1.196</td>
<td>3.200</td>
<td>1.365</td>
</tr>
<tr>
<td></td>
<td>(10.29)</td>
<td>(18.71)</td>
<td>(9.511)</td>
<td>(11.06)</td>
<td>(0.721)</td>
</tr>
<tr>
<td>Experience</td>
<td>0.0770</td>
<td>0.0287</td>
<td>0.0402</td>
<td>0.0642</td>
<td>0.0874</td>
</tr>
<tr>
<td></td>
<td>(12.53)</td>
<td>(4.934)</td>
<td>(4.114)</td>
<td>(3.913)</td>
<td>(0.992)</td>
</tr>
<tr>
<td>Experience squared</td>
<td>−0.0020</td>
<td>−0.0004</td>
<td>−0.0006</td>
<td>−0.0008</td>
<td>−0.0014</td>
</tr>
<tr>
<td></td>
<td>(5.949)</td>
<td>(2.414)</td>
<td>(3.177)</td>
<td>(3.632)</td>
<td>(1.434)</td>
</tr>
<tr>
<td>Years of education</td>
<td>0.0218</td>
<td>0.0323</td>
<td>0.0320</td>
<td>0.0271</td>
<td>0.0020</td>
</tr>
<tr>
<td></td>
<td>(1.354)</td>
<td>(2.792)</td>
<td>(2.713)</td>
<td>(2.125)</td>
<td>(0.074)</td>
</tr>
<tr>
<td>Spline for ed. ≥ 8</td>
<td>0.0571</td>
<td>0.0450</td>
<td>0.0485</td>
<td>0.0289</td>
<td>0.0119</td>
</tr>
<tr>
<td></td>
<td>(2.084)</td>
<td>(3.043)</td>
<td>(3.107)</td>
<td>(1.804)</td>
<td>(0.382)</td>
</tr>
<tr>
<td>Spline for ed. ≥ 12</td>
<td>0.0072</td>
<td>0.0135</td>
<td>0.0124</td>
<td>0.0301</td>
<td>0.0362</td>
</tr>
<tr>
<td></td>
<td>(0.621)</td>
<td>(1.303)</td>
<td>(1.063)</td>
<td>(2.461)</td>
<td>(1.450)</td>
</tr>
<tr>
<td>Dummy ed. ≥ 8</td>
<td>0.0963</td>
<td>0.0672</td>
<td>0.0145</td>
<td>0.1231</td>
<td>−0.1720</td>
</tr>
<tr>
<td></td>
<td>(1.944)</td>
<td>(1.581)</td>
<td>(0.339)</td>
<td>(2.877)</td>
<td>(2.116)</td>
</tr>
<tr>
<td>Dummy ed. ≥ 12</td>
<td>0.0755</td>
<td>0.0628</td>
<td>0.0436</td>
<td>0.0190</td>
<td>0.0241</td>
</tr>
<tr>
<td></td>
<td>(2.954)</td>
<td>(2.643)</td>
<td>(1.513)</td>
<td>(0.629)</td>
<td>(0.411)</td>
</tr>
<tr>
<td>Dummy ed. ≥ 16</td>
<td>0.1131</td>
<td>0.0553</td>
<td>0.0269</td>
<td>0.0181</td>
<td>0.0876</td>
</tr>
<tr>
<td></td>
<td>(6.364)</td>
<td>(3.753)</td>
<td>(1.564)</td>
<td>(0.723)</td>
<td>(1.510)</td>
</tr>
<tr>
<td>Dummy ed. = 17</td>
<td>−0.0640</td>
<td>−0.0413</td>
<td>−0.0519</td>
<td>0.0001</td>
<td>−0.0957</td>
</tr>
<tr>
<td></td>
<td>(2.716)</td>
<td>(2.278)</td>
<td>(2.656)</td>
<td>(0.007)</td>
<td>(1.278)</td>
</tr>
<tr>
<td>Dummy ed. = 18</td>
<td>0.0304</td>
<td>0.0012</td>
<td>−0.0225</td>
<td>−0.0546</td>
<td>0.0312</td>
</tr>
<tr>
<td></td>
<td>(1.532)</td>
<td>(0.081)</td>
<td>(1.488)</td>
<td>(2.307)</td>
<td>(0.638)</td>
</tr>
<tr>
<td>N</td>
<td>18,438</td>
<td>22,926</td>
<td>16,308</td>
<td>9,574</td>
<td>1,592</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.238</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note that t-statistics are in parentheses.

The pattern of sheepskin effects supports the prediction that the return-to-education signals decline with work experience. Despite enormous subsamples in the third and fourth cohorts and despite the traditional role played by the other education and experience variables, no sheepskin effects could be confirmed. Yet, the first two cohorts had significant sheepskin effects, larger in the first cohort than in the second cohort. Perhaps even more convincing, both the high school and college sheepskin effects decline monotonically across the four statistically different from zero (although the t-statistic for the college dummy is close). Note that two of the three variables from the spline remain positive and significantly different from zero and the experience variables also remain significant. The return to years of education remains but the return to the signal drops. This follows over to the fourth cohort in which neither the high school nor college sheepskin effects are significant and in which the general pattern of results for the spline and the experience variables continues to hold. By cohort five, by far the smallest, the equation collapses without any meaningful estimates of coefficients. The final cohort remains outside the prime working age and the estimates may be influenced by attrition into retirement.
Table 3

Pair-wise statistical tests

<table>
<thead>
<tr>
<th>Earlier cohort</th>
<th>Later cohort</th>
<th>2nd</th>
<th>3rd</th>
<th>4th</th>
</tr>
</thead>
<tbody>
<tr>
<td>1st</td>
<td>2.5, 0.4</td>
<td>3.5, 0.8</td>
<td>3.1, 1.5</td>
<td></td>
</tr>
<tr>
<td>2nd</td>
<td>1.3, 0.5</td>
<td>1.3, 1.5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3rd</td>
<td>0.7, 0.3</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

All entries are t-statistics generated for difference of coefficient tests. The first cohort in the difference is listed vertically and the second is listed horizontally. The first entry in each cell relates to college sheepskins and the second to high school sheepskins.

cohorts in which meaningful estimates emerged. The probability of observing three straight declines in each of the two sheepskin effects is 0.0156 if the direction of the changes were randomly distributed.\(^9\) Thus it seems highly unlikely that the observed pattern of decline is by chance.

The stacked estimation allows tests of statistical significance for the difference in coefficients across any two cohorts. A complete variance—covariance matrix is associated with the estimation which allows for computing the standard error associated with any difference.\(^10\) Table 3 shows a full set of pair-wise tests for the significance of coefficient differences. The first entry in each cell is the t-statistic for the difference in college sheepskins and the second is for the difference in high-school sheepskins. Reading the first entry across the first row, the size of the college sheepskin in the first cohort is significantly larger than that in any of the three subsequent cohorts at any reasonable significance level. The other pair-wise college results are somewhat less convincing. Nonetheless, if the formal test adopts a one-sided alternative, that the sheepskin effect declines, the hypothesis of no change can be rejected at the 10% level when comparing the second cohort with either the third or fourth. Using a similar standard, the high-school sheepskin in the fourth cohort is significantly smaller than that in either the first or second cohort. Thus, both the overall pattern of decline plus the pair-wise comparisons suggest support for the attenuation of sheepskin effects with work experience.

Finally, the rather low r-squared is of the same general magnitude as those reported by Hungerford and Solon (1987). The generally low values may result from the compounding of measurement error from both the numerator and the denominator of the dependent variable.

\(^9\) There are three changes in coefficients moving across the first four cohorts for each of the two sheepskin effects. The odds that any one change is a decline is 0.5. The odds that all six changes are declines is \((0.5)^6 = 0.0156\).

\(^10\) Indeed, because no worker is observed in more than one cohort, all covariances in the matrix are zero.
4. Age effects vs vintage effects

There exist problems using a single cross-section to investigate what we hypothesize is a dynamic effect. Even though the size of sheepskin effects attenuates as we examine older cohorts, this does not prove that when the younger cohorts age their sheepskin effects will attenuate. Instead, the size of the sheepskin effect could be specific to workers of a particular vintage. The older cohorts in our 1991 sample may have always had no sheepskin effects and the younger sample may have sheepskin effects that persist even when they are older. Thus, if we followed any given cohort over time we would see no attenuation of the sheepskin effect and our cross-sectional pattern resulted only from the nature of the labor market for workers in particular cohorts. While we have no particular reason to expect vintage effects, we do recognize it as a potential problem and investigate its extent.

One solution for disentangling age and vintage effects is the use of longitudinal data to follow specific workers over their work experience (see Hoffman, 1979, for an early application). Yet, our particular application makes this solution difficult to implement. First, the typical sheepskin estimate has required the precision of very large samples because the spline specification essentially requires education to be entered as eight separate variables. Second, even the 20+ years of data in typical longitudinal sources may be inadequate to estimate the full attenuation in sheepskin effects that happen over a work life that may be more than twice that long. We recognize that a longitudinal investigation may still be useful, but follow an alternative approach that provides reasonable leverage for the issue of interest.11

Our fundamental hypothesis that sheepskin effects decline with work experience requires examining data from earlier years to obtain within cohort comparisons. Our strategy is to draw a 1979 CPS sample similar to that from the 1991 CPS and create ‘synthetic’ cohorts. We place together all workers from either year which are in the same age cohort. As there are 12 years between 1979 and 1991, workers in the 41 to 50 cohort in 1991 are matched with workers between ages 29 and 38 in 1979. Thus, we compare the returns to a college degree for the cohort in 1991 to returns for the same cohort in 1979. The 12-year time span between the data years necessitated dropping the first cohort and even treating the second cohort with some caution.

The testing framework re-estimates the spline function in 1991 and compares these new estimates with those from 1979. If the results in Table 2 masked a vintage effect, the size of sheepskin effects should not change as a cohort ages. On the other hand, if the results in Table 2 truly reflect smaller sheepskin effects as workers gain work experience, the size of the effects should diminish as a cohort ages.

11 We also note that the exact specification of a longitudinal equation designed to capture the cohort effects we investigate is not immediately clear.
Table 3 summarizes the results for the four possible cohorts by presenting the coefficients on the eighth grade, high school, and college dummies for both 1991 and 1979. The sheepskin effect for a college degree displays a clear pattern of declining effects within cohorts. The 1991 results mimic those from the last four cohorts in Table 2, those 31 to 40 years old have a significant college sheepskin effect but those for other cohorts are insignificantly different from zero. Yet 12 years earlier two of the cohorts with insignificant sheepskin effects, the second and third, each had significantly positive sheepskin effects. Indeed, each of the three prime age cohorts had smaller sheepskin returns in 1991 than in 1979. Thus the declining college sheepskin effect over the 12 years confirms the results from the 1991 cross-section. As the results reflect separate stacked estimations for each year, tests of significance are readily available. They indicate statistically significant declines in the sheepskin effect for the first and third cohorts.\textsuperscript{12}

The high-school sheepskin effects present a less clear pattern with those for two cohorts increasing and two decreasing. Interestingly, the estimated high-school sheepskin effects in 1979 are generally statistically insignificant. While not supportive of a declining return to education signals, neither is it easily compatible with vintage effects. The eighth grade coefficients also show mixed results with three of the four cohorts showing a decline in the estimated sheepskin. Such mixed results for the grade eight sheepskins might have been expected as the 1991 cross section also failed to suggest a consistent pattern.\textsuperscript{13}

We recognize that these estimations may not fully confirm the hypothesis that returns to education signals decline. Indeed, estimation issues remain. For instance, the college return in the youngest 1979 cohort is estimated on a somewhat unusual sample as it obviously includes many workers who may eventually earn college degrees. Yet, having admitted that doubt remains, we are hard pressed to attribute the strong cross-sectional results to vintage effects. The declining college sheepskin effects in the synthetic cohorts suggests that vintage effects hold only modest sway (and perhaps none). At a minimum, it would seem the burden of proof remains on those who contend the 1991 results reflect vintage effects.

5. Conclusion

This paper is the first to use the pattern of sheepskin effects to test the contested relationship between education signals and work-place experience. In drawing conclusions we hasten to add that nothing in this paper contradicts

\textsuperscript{12}The \textit{t}-statistics are 5.9 for the first cohort, 0.9 for the second cohort and 1.8 for the third cohort. Note that none of the changes for the eighth or 12th grade effects are statistically significant.

\textsuperscript{13}Indeed, eighth grade sheepskin effects have not been at issue in much of the empirical literature. However, see Heckman \textit{et al.} (1996) for recent evidence of positive 8th grade sheepskin effects.
the view that education can enhance skills and earnings. The findings do suggest, however, that degrees may serve as signals of productivity. Further, we argue that as workers gain experience the returns to such signals do seem to diminish as employers can more directly observe actual productivity. As we have argued, this outcome is likely when productivity is a function of the quality of job matches.

The evidence is most clear in the case of college degree years. The largest sheepskin effect for each cross section is for the youngest cohort and there exists a routine pattern of declining effects across cohorts and as each cohort aged. The pattern for eighth grade and high school graduation years was somewhat contradictory with the results providing mixed support for the hypothesis that returns to education signals decline.

We end on several notes of caution. First, signaling hypotheses need not be the only possible explanation for the presence of sheepskin effects. We recognize Chiswick’s (1973) point that graduates are, on average, more efficient learners and that as a result they enjoy proportionately larger increases in productivity than their years of education alone would indicate. Thus, the sheepskin effect could arise from education increasing productivity and a self-selection process rather than signaling. While entirely plausible, it remains difficult to reconcile such a view with the pattern that the sheepskin effects diminish over time. Second, we emphasize that our model of job matching can only be obliquely supported. Any other theory suggesting a decline in sheepskins might not be easily distinguished from ours on the basis of the statistical estimates. Thus, we remain content to confirm the pattern of diminishing sheepskin effects and to note its consistency with signals that bring larger returns early in labor market histories.
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References


APPENDIX

This appendix presents an informal demonstration that both the firms and workers value the signal and its use represents an equilibrium.

Competition between firms guarantees that each worker is paid the value of their expected productivity. That same competition guarantees that workers with higher expected productivity
in one class of firms will be bid to those firms. Specifically, the \( j \) type firms have higher aggregate productivity using the signal in period one rather than not using it

\[
\alpha v_j + (1 - \alpha)(1/(N - 1)) \left[ (N - j)v_j + \sum_{i=1}^{j-1} y_i \right] > (N - j)(1/N)v_j + (j/N) \left[ (1/j) \left( \sum_{i=1}^{j} w_i \right) \right]
\]  

(8)

Further, as each worker is paid her productivity in period two, the return to the signal is captured in period one and is identical for all types of workers: \( w_j - w_{j-1} \). This return will be attractive to workers only if the costs of purchasing the signal, \( c_j \), are lower

\[
w_j - w_{j-1} > c_j
\]

(9)

Starting with the Spence, the assumption is that the average cost is lower for the higher productivity workers. Yet there is surely variance around the averages such that a disproportionate share of those purchasing the signal, but not all, will be of the productivity associated with the signal. Indeed, this is the essence of an imperfect signal (see Golbe, 1985) and assures that \( 1/N < \alpha < 1 \). Thus, the equilibrium persists as long as the probability of \( 5 \) workers purchasing signal \( S_j \), is greater than that of all \( T_i \) workers, \( j > i \). As the benefits are identical, these probabilities are a function of the underlying distributions of costs for each worker type, \( f_i \). Thus, the condition for the equilibrium can be summarized as

\[
F_j(w_j - w_{j-1}) > F_i(w_j - w_{j-1}) \text{ for } j > i
\]

(10)

where \( F_j \) is the cumulative probability for each worker type \( i \) of purchasing \( S_j \) for cost less than or equal to the return to the signal. This condition is very much in keeping with those traditional in signaling models.

\[^{14}\] The usual presumption is that the high productivity workers are able to spend less time and effort in school to achieve any given signal.