Leadership Skills and Wages*

Peter Kuhn and Catherine Weinberger

University of California, Santa Barbara

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American business is devoting a significant and increasing amount of resources to identifying and developing a worker characteristic called "leadership skill". Is there such a thing, and is it rewarded in labor markets? Using the Project *Talent*, NLS72 and High School and Beyond datasets, we show that men who occupied leadership positions in high school earn more as adults, even when cognitive skills are held constant. The pure leadership-wage effect varies, depending on definitions and time period, from four percent to thirty-three percent. According to our estimates, this effect is not an artifact of measurement error in cognitive skills or differences in a wide array of other physical or psychological traits. High-school leaders are more likely to occupy managerial occupations as adults, and leadership skills command a higher wage premium within managerial occupations than in other jobs. Finally, we find some evidence that, rather than being completely determined before high school entry, leadership skills may be fostered by exposure to leadership opportunities during high school.

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1. Introduction.

Today's businesses, workers and educational institutions are making large investments in identifying and developing a personal characteristic called "leadership". A recent search of books in print revealed over 5000 titles containing the word "leadership," the majority of which are training or self-help manuals aimed at a business audience. Academic articles on the development and measurement of leadership have filled the Leadership Quarterly since 1990. Elite universities are placing increased emphasis on high school leadership in their admissions decisions (Morse, 2001), top business schools send MBA students to Marine-run "boot camps" for the express purpose of fostering leadership skills (Scannell, 2001), and Fortune 1000 companies are offering leadership skills training to rapidly growing numbers of employees (Lawler, et. al. 2001).[1]

Compared to the kinds of cognitive skills measured by tests like the AFQT —on which economists have focused almost exclusively in studies of earnings determination— interpersonal skills including leadership rank much higher in the list of employee attributes employers say they seek in workers. For example, employers of new college graduates report that communications skills, motivation/initiative, teamwork skills, and leadership skills are all more highly valued than academic achievement/GPA (NACE, 2000). Similarly, employers seeking less educated entry-level workers report a growing importance of "soft skills" (Moss and Tilly 2001). Labor economists have noted a widespread change in work organization towards increased employee involvement and teamwork (Lindbeck and Snower 2000). Goleman (1997, 1998) argues persuasively that, relative to academic ability, "emotional intelligence"—a constellation of social skills including leadership—is an increasingly important factor in workplace success.

Is there such a thing as "leadership skill"? Is it rewarded in labor markets, and can it be measured and distinguished from analytical or cognitive skills as well as from other forms of

[1] Between 1990 and 1999, the proportion of Fortune 1000 companies offering leadership skills training to at least 40 percent of their employees tripled, from 11 percent to 32 percent (Lawler, et. al. 2001).

human capital such as education and experience? To examine these issues, we use three data sets that are representative of the national population of high school students in 1960, 1972 and 1982. All three surveys include information on family background, scores from standardized cognitive tests taken in high school, and labor market outcomes approximately ten years after high school. All three include a "behavioral" measure of leadership taken during high school (acting as a team captain or club president) while two also contain other measures of leadership skill.

Our basic methodology regresses adult wages on indicators of leadership skills taken before labor market entry (i.e. in high school), controlling for standard measures of cognitive ability, family background, and (in some specifications) high school fixed effects. Our use of pre-labor market measures of leadership skill allows us to avoid certain kinds of endogeneity, for example the possibility that individuals who do well in the labor market for some unrelated reason (e.g. a "lucky" promotion) might begin to develop those very leadership skills whose effects we wish to measure.[2] The control for cognitive skills ensures that we are capturing only the additional effect of a "noncognitive" skill such as leadership.[3] We conduct a variety of tests to determine whether leadership, as measured here, is in fact a distinct skill that is related to the management of people. These include examining interaction effects between leadership skills and other determinants of earnings, adding controls for other psychological and physical characteristics for which leadership may be acting as a proxy, and estimating the effect of leadership skills on occupational assignment.

Our main results are as follows. First, controlling for family background, for standard measures of human capital, for mathematics test scores, and for all factors associated with the high school attended (via high-school fixed effects), individuals who exhibited leadership propensities in high school earn significantly more about ten years later. This effect is observed

[2] Even more to the point, in wage regressions using *concurrent* self-assessed measures of leadership, accidentally-successful individuals might simply interpret their success as evidence of exceptional leadership ability.

in all three data sets and in all econometric specifications. Its size varies with the nature of the leadership measure and across years, ranging from four percent for one indicator of leadership in 1971 to 33 percent for a more exclusive indicator in 1991. Second, less than one quarter of the leadership effect on adult earnings operates through differences in educational attainment after high school. This, together with other results summarized below, suggests that our leadership measures are not acting as proxies for other personal attributes, such as persistence, a low discount rate, or measurement error in cognitive skills. Third, the skill identified by this leadership measure is distributed in a manner that is largely orthogonal to cognitive skills, as measured by mathematics test scores. Relatedly, we find that the marginal effect of leadership skills on earnings does not vary with the respondent's level of education, or with the level of math scores, working just as strongly for low-math-score and low-education individuals as for others.

In the course of our analysis, we conduct several additional exercises to ensure that our measure of leadership is not acting as a proxy for some other physical or psychological characteristic. In two of our data sets we are able to add controls for "beauty" or physical attractiveness; in one we can also control for height. Even though "beauty" affects earnings exactly as expected (Hamermesh and Biddle 1994, 1998; Averett and Korenman 1996), in neither case does accounting for the effects of beauty change the estimated leadership coefficient. Adolescent height does not have a significant effect on adult earnings in our data. We also present instrumental variables evidence that argues against interpreting our estimated leadership effect as a proxy for measurement error in cognitive ability.

Controlling for a very rich set of psychological characteristics available in the Project *Talent* data (including "vigor", "maturity", "self-confidence", and "tidiness") explains very little

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[3] The term "non-cognitive skills" seems to have been adopted by economists to distinguish personality measures or social skills from standard math and verbal skill measures, which have generally been referred to as "cognitive skills". Psychologists sometimes object to this distinction, rightly pointing out that cognition is involved in the use of these other skills as well.

of the leadership coefficient. Significantly in our opinion, the largest reduction in the leadership coefficient occurs when we add a control for "sociability" (a self-assessed measure of enjoyment of being around people). We take this as confirmation that our leadership variable is capturing some sort of social skill. The final piece of evidence that our leadership measure truly captures an ability to lead other people involves occupational outcomes: *ceteris paribus*, high-school leaders are more likely to be managers as adults. Just as important, while high school leadership is rewarded in both managerial and nonmanagerial occupations, the marginal market value of extra leadership skills is considerably greater within managerial occupations than in other jobs.

The research in this paper forms part of a growing literature on the role of non-cognitive skills in wage determination. Much of this literature is reviewed in Bowles, Gintis and Osborne (2001); it includes papers examining the effects on adult outcomes of childhood measures of aggression and withdrawal (Osborne 1999); of both child and adult measures of "locus of control" (Goldsmith, Veum and Darity 1997, Osborne 1999, and Coleman and DeLeire 2000); of measures of motivation (Goldsmith, Veum and Darity 2000), and of indicators of behavioral problems during high school (Cawley, Heckman and Vytlacil 2001). The current paper differs from this research in its focus on a skill that firms explicitly say they seek in new hires, and that firms actually pay to foster among their incumbent workers.[4] Our work also relates to a recent group of papers on the labor market effects of high school athletic participation (Anderson 2000; Barron, Ewing and Waddell (2000); Eide and Ronan 2000; Stevenson 2000).[5] All of these papers find that participation in high school athletics has small positive effects on adult wages. Our work differs from these papers in its focus on leadership within *and* beyond sports, and in the richer set of measures and additional sensitivity testing that is possible in our three data sets,

[4] Our paper also focuses much more on the top of the wage distribution, while much existing research focuses on problems employers encounter when employing low-wage workers.

especially the much larger and more comprehensive *Talent* data.

2. Data and Methods

As noted, we use three data sets: Project *Talent* (1960), The National Longitudinal Study of the Class of 1972 (NLS72), and the sophomore cohort of High School and Beyond (HSB 1982 seniors). All are surveys of a representative sample of U.S. high-school students, who were re-interviewed between 9 and 13 years after leaving high school. In all three data sets, we restrict our attention to white men. This restriction allows us to study the labor market valuation of leaders without the confounding effects of race or gender discrimination or of the changing roles and expectations of women during this time period. Aside from providing information from three different decades, each of the data sets has some unique advantages that contribute to our understanding of leadership effects.

The earliest of the data sets, the Project *Talent* study of 1960 High School Students, is also by far the largest. The students in this study were surveyed during high school in 1960, and followed longitudinally for eleven years after high school. *Talent* has a much more complete inventory of personality, behavioral, and ability measures than all the more recent nationally-representative panel surveys, including the PSID, NLSY, NLS72, and HSB. During the base year, over 400,000 students —approximately five percent of all U.S. high school students— devoted two full days to the study, during which they responded to a 400-question survey and were given comprehensive cognitive and psychological assessments. Of these 400,000 students, approximately 150,000 were men in grades 10 through 12. Our focus in this paper is on the representative sample of the latter group who were selected for re-interview eleven years after finishing high school (in 1971, 1972, or 1973) and in addition where white, employed, and had

[5] This literature, in turn, relates to (but does not always cite) an older literature, mostly in psychology, sociology and education journals, linking participation in high school extracurricular activities with positive educational, behavioral and economic outcomes (see Eccles and Barber 1999 for a thorough review of the older literature; recent contributions include Camp 1990, Gerber 1996, and Barber, Eccles and Stone 2001). In a wide-ranging study of the earliest of our

graduated from high school by that time.[6] Aside from its size and focus on multiple years of high school, a key distinctive feature of *Talent* is the framing of its leadership activity measure: in contrast to NLS72 and HSB (which refer to leadership in the senior year only), the *Talent* survey counts all leadership activity in the *past three years*. The *Talent* survey also includes an independent measure of leadership taken from the personality inventory.

The NLS72 followed 1972 high school seniors until 1986. HSB followed 1980 high school sophomores until 1992. In both of these studies, the students were asked about leadership roles and club/sports participation during the senior year of high school only. Other variables are similar to those available in Project *Talent*. Senior year math scores, parents' education and own educational attainment could be coded in exactly comparable ways.[7] Unfortunately, the 1992 HSB survey includes only annual, rather than hourly, earnings. It does, however, contain annual earnings for several years (1988-1991) and a question about physical attractiveness not available in the other surveys, as well as information on participation in three other high school leadership activities that allows us to construct a more continuous measure of leadership.

Two of the three data sets used in our analysis contain plausible measures of leadership in addition to the team and club activity measures. *Talent* has a self-assessed leadership scale (*silead*) which was constructed by the original *Talent* investigators from the respondent's indication that each of the following five statements described himself "extremely well" or "quite well":

1. I am the leader in my group.

2. I am influential.

3. I have held a lot of elected offices.

4. People naturally follow my lead.

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data sets (Talent), Jencks (1977) detected a wage effect of high school leadership but did not explore its origins or structure in much detail.

[6] Wage equations are restricted to those with hourly earnings between $1 and $50. This includes 99.5% of all respondents with hourly earnings reported.

5. I like to make decisions.

These questions were contained in a 150-question Student Activities Inventory designed to assess 13 different personality traits. An individual's score simply sums the number of positive responses.[8] About 38 percent of men in our (weighted) sample agreed with none of the above statements, while only 3 percent agreed with all five. In some of our analysis, five dummy variables distinguish the exact *silead* score. For other treatments, *silead* is transformed into a standardized score with mean zero and standard deviation one within each grade cohort.

In HSB, we were able to construct a similar leadership measure by summing the number of distinct types of leadership activities in which an individual had participated during the past year. Two of these are the team captain and club president activities already described. The other three are:

1. Spoke before a group of 50 or more.

3. Headed a group problem-solving session.

3. Chaired a meeting.

Aside from checking the robustness of our results to the measurement of leadership, these two alternative leadership measures have the advantage of being more continuous than our indicators of club presidency or captainship. This will allow us to explore the form of the leadership-earnings relationship in more detail, focusing especially on those individuals with rare and very high endowments of leadership skills.

Because of the long time interval covered by each of these studies, sample attrition could potentially be a serious problem. The treatment of attrition in the NLS-72 and HSB is already familiar to economists. Weights were constructed by the NCES based on the assumption that non-respondents are similar to the respondents with the same observable characteristics. The

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[7] Math scores were made comparable by coding them as percentile scores among all white men in the same grade who were tested in the base year.

designers of the Project *Talent* study took a more rigorous and costly approach. In this study, at each resurvey a representative subsample of non-respondents was randomly selected and aggressively pursued. For the white men in this special sample, the researchers achieved a 100% success rate, making it fully representative of (initial) non-respondents.[9] In our analysis we use weights derived from this aggressively-followed sample to adjust for attrition bias in the larger sample. These weights simply up-weight the responses of this aggressively-followed sample by the inverse of their share of the (initially) non-responding population, to generate means that are representative of the entire 1960 high school population.[10]

Descriptive statistics for all three data sets are presented in Table 1. The complete *Talent* sample includes all 24041 white men who were working at the eleven year interview date, whose measured wages fell between one dollar and fifty dollars per hour, and who had non-missing data on math scores and team/club membership/leadership.[11] Preliminary analysis consistently showed no relationship between verbal scores and later earnings, after controlling for math scores. Rather than lose observations with missing verbal scores, the verbal score was dropped from all further analysis.

Consistent with other evidence on time trends in the supply of college-educated labor (Card and Lemieux 2001), there is little change between the 1971 and 1991 surveys in the fraction of male high school seniors going on to a college degree. The fraction pursuing some postsecondary education did, however, rise substantially. Finally, the fraction of men whose parents had high school and college degrees also rose substantially between the 1971 and 1991

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[8] Unfortunately we do not have access to data on the five questions separately, so we are forced to use this "scale" as it was constructed by the original Talent investigators. See note 21 below. *Silead* is correlated with the leadership activities measure used in the previous section: those who were both captain and president were twice as likely as members of the full sample to agree with four or five of the *silead* statements.

[9] As a last resort, collection agencies were used to locate recalcitrant respondents.

[10] Special sample respondents represent 6.7 percent of individuals in the sample used for analysis, but 44.1 percent of the weighted sample. To avoid artificially inflating the precision of the estimates, Stata's *p-weight* option was used throughout the paper.

[11] Despite significant sample attrition, Table 1 shows that our usable Talent sample is more than *eight times* as large as our NLS72 sample and more than ten times the size of our HSB sample.

surveys.[12]

Turning to our leadership measures, fully 78% of the *Talent* men reported that, within the past three years, they had participated both on athletic teams and in clubs or other organizations; an additional 18% participated only in clubs, and another 3% only on teams. Almost 58% (.218+.138+.221) said they had taken some leadership role (either as a team captain or club president) during that three-year window. Because this is such a large group, much of our analysis in what follows focuses on a smaller group of individuals exhibiting more intense leadership activity. In *Talent*, this is the 22% of men who reported that they had served *both* as captain of an athletic team and as president of a club or other organization over this period.

Leadership activities in the NLS72 and HSB samples are described in the last two columns of Table 1. As in *Talent*, these samples are restricted to those working at the survey date and earning wages that are not in the extreme tails of the distribution. Recall that unlike *Talent*, these samples consist of high school seniors only (for comparison, summary statistics for only the seniors in *Talent* are reported in column 2). More importantly, the leadership activity questions refer only to the past year, rather than the past three years. Only 6.5 percent of NLS72 men were both team captains and club presidents in their senior year, and this was true of 12.1 percent of men in HSB.

The very high estimated leadership participation rates in Project *Talent*, especially in comparison with our other two data sets, do raise some concerns.[13] In our opinion, there are three likely explanations for this phenomenon. First, the Project Talent leadership questions refer to

[12] There was also a substantial decline across surveys in the number of respondents with missing educational attainment information, from about 12 percent in Talent to under one percent in HSB (and from 3 to 2 percent for parental education). Trends in educational attainment are however not substantially different when these missing values are considered as a separate category. In all our regression analyses, we include indicators for missing own or parental education.

[13] Is it possible that the high reported rates of leadership in our sample reflect nonrandom selection of leaders into the sample that was re-interviewed after 11 years? Examination of our data on the entire base year sample indicates very clearly that this was not the case. For example, among all 37,000 male seniors in the base year sample, 22.4 percent said they were both captain and president in the past 3 years, compared to 21.2 percent of seniors in our sample (Table 1, Column 2). Among juniors and sophomores in the base year sample, the proportions were also high: 0.218 (n=42,000) and 0.261 (n=48,000), respectively.

leadership over a three-year, rather than a one-year, window of observation. By definition, an individual is more likely to have done something in the past three years than in the past year. Second, it is possible that high school men were (more) likely to exaggerate their leadership activities in 1960 than in other years. Finally, perhaps men's actual participation in high school leadership activities dropped substantially between 1960 and 1972.

Evidence comparing responses to leadership questions with different windows of observation is available from the NCES National Educational Longitudinal Survey of 1988 8th graders (NELS). Unlike any of the surveys used in this article, participants in the NELS were asked about leadership more than once: in particular during the 8th, 10th and 12th grades. In this sample, white men were about twice as likely to report that they were both captain and president over the three possible years than in the senior year alone. We conclude that this type of leadership turnover (or measurement error) could explain the difference in reported leadership between 1960 and 1982, but not the larger drop in 1972. Given the widespread disaffection in American high schools during the Vietnam era, it seems likely that students reduced both actual participation and self-reports (or exaggeration) of participation during this time.

If turnover or measurement error (including exaggeration) in leadership participation does play a significant role in explaining our higher estimated leadership levels in Project *Talent*, what is the likely effect on our results? In all these cases, the likely effect is a downward bias in the leadership coefficient –in the measurement error case, because of traditional attenuation bias; in the turnover/exaggeration cases because a less-selected group of leaders is likely, on average, to have a less intensive degree of underlying leadership "skill".[14] Thus, we treat our estimates of the effects of captainship and presidency in the *Talent* data, reported in Tables 2 and 3, as a lower bound to what would obtain if we had a senior-year-only measure of participation in those

[14] These notions are analyzed formally in a previous version of the paper, available from the authors.

activities.[15]

Finally, before embarking on a detailed and separate study of leadership, it seems appropriate to ask whether leadership is just another manifestation of greater "general" intelligence. While we explore the separate effects of leadership and cognitive skills in considerable detail later in this paper, it is instructive at the start to simply look at the joint distribution of these two traits in the population under study. Note first from Table 1 that the mean percentile math score in all of our samples is close to 50, which confirms that our weighting procedure removes any obvious effects of attrition bias.[16] Next, consider that exactly half of the 1960 seniors who had been both captains and presidents in the last three years had math scores above the 50th percentile: in other words, math scores and leadership were essentially orthogonal in 1960. While the association between math scores and leadership seems to have strengthened over time (in the 1972 and 1982 cohorts of seniors, 58 and 65 percent —respectively— of the "both" group had math scores above the 50th percentile), it seems clear that even in the most recent survey, the correlation between these two skills remains weak.

3. Results: Leadership and Earnings

a. Leadership Activities: Captains and Presidents

Table 2 presents log hourly earnings regression results for the *Talent* sample, eleven years after leaving high school. Column 1 shows the simple correlations between our high school leadership measures and wages, controlling only for participation in sports or club activities (whether as a leader or not) and geographic location. On average, men who were either team captains or club presidents (but not both) between 1958 and 1960 earned 3.0 to 4.3 percent higher wages eleven years after high school graduation, relative to men who were only members of

[15] This does not apply to estimates based on our more continuous, self-assessed, leadership measures, which produce a much more similar distribution of leadership between *Talent* and later data sets.

[16] Percentile math scores were calculated using all base year observations, including those not matched in the followup survey.

teams and/or clubs. Men who were both captain and president earn 6.9 percent more than those who joined both teams and clubs but held no leadership position. While team and club membership are also associated with higher earnings, the leadership effect is distinct from these participation effects.

Column 2 of Table 2 adds controls for cognitive test scores — a variable which is determined before labor market entry. As expected, this variable has a significant effect on wages. However, controlling for cognitive scores has essentially no effect on our "captain and president" coefficient. Indeed, the stability of this coefficient (which for brevity we henceforth call the "leadership wage effect") in the face of controls for cognitive ability confirms the evidence presented earlier that these two skills were essentially uncorrelated in 1960. Column 3 of Table 2 adds controls for parents' educational attainment. Although those with better educated parents tend to earn more as adults, the leadership effect cannot be accounted for by differences in parental education. In fact, controls for parent education have almost no effect on the estimated leadership coefficients.[17]

Because educational attainment beyond high school is properly seen as endogenous in the current context, it is not clear that we should control for it as a regressor. However, to understand the extent to which the market returns to high school leadership are driven by differences in educational choices between individuals with different levels of leadership skills, column 4 adds controls for the respondent's further education. This reduces the "captain and president" coefficient by about 20 percent (from .053 to .042) suggesting that less than one quarter of the

[17] The results are robust to including controls for student-reported family income. This variable was dropped from the analysis because it was missing for about one-third of the sample, and to maintain comparability across data sets. Within this smaller sample, using the specification of Table 2, Column 7, but including controls for the log of family income and an indicator of father's employment in a professional, managerial or technical occupation has very little effect on the estimates: there is only a tiny drop in leadership coefficients (less than one-third of the standard error for all 3 leadership categories).

leadership effect works "through" further education decisions.[18] This suggests that our leadership coefficient is capturing something other than college scholarship opportunities, or personal characteristics such as "persistence" or good study habits associated with continuing on in school.

Columns 5-8 estimate models identical to columns 1-4 but with the addition of fixed effects for each of the 1005 high schools in our *Talent* data. Effectively, these regressions compare respondents only with others who attended the same high school, as a way of controlling for omitted family background, school, peer and neighborhood quality effects, as well as any other factor associated with the high school attended. The most surprising thing about these regressions is how similar each one is to the corresponding regression without fixed effects: the leadership effect never differs by more than one half of a percentage point. We take this as strong evidence that our leadership variables are not acting as proxies for unmeasured aspects of the student population that vary systematically across schools.

Table 3 presents earnings regressions from the NLS72 and HSB data, constructed to be as similar as possible to the *Talent* regressions.[19] As noted, unlike Project *Talent*, in each of these newer data sets we can only look at a single cohort of individuals (persons interviewed as seniors in 1972 and 1982 respectively). Also, in one of the two data sets (HSB) we do not have access to a true hourly wage measure; instead we only know total annual earnings. To address these potential noncomparabilities, columns 1-4 of Table 3 present (log) annual earnings regressions for seniors only in Project *Talent*. These are very similar to those for the full *Talent* sample in Table 2; thus we can be quite confident that any differences in the results between our three data sets are not driven by differences in the high school interview year, nor by the use of hourly rather than annual earnings as the dependent variable. It is not surprising that the two earnings measures give comparable results since the overwhelming majority of young adult white men

[18] However, it is worth noting that the high school leaders do have greater educational persistence at every level of attainment. Compared to other high school graduates, those who were both captains and presidents are 18% more likely to have completed at least some college, 24% more likely to have completed a college degree, and 38% more likely to have completed a graduate degree, despite having virtually identical cognitive test scores.

work full time and full year. We were, however, surprised that the leadership coefficients (and means) for seniors were so similar to those for juniors and sophomores.

As in our *Talent* results, leadership coefficients in more recent data sets reported in Table 3 are positive and statistically significant in all specifications. Perhaps surprisingly, however, they are much larger than the *Talent* coefficients reported in Table 2. As noted earlier, one possible explanation for this is downward bias in the Talent coefficients arising from the longer, three-year measurement window for leadership activity. Another is of course a true rise in the market price of leadership skills over time. While it is impossible to definitively separate these two hypotheses given the data available to us, further evidence that both of them play some role emerges from the analysis of alternative measures of high school leadership in the next section.[20]

b. Other Measures of Leadership

Table 4 reports similar regression results to those in Tables 2 and 3, but using the alternative measures of leadership described earlier in Project *Talent* and HSB.[21] Aside from assessing the robustness of the results to different leadership measures, these alternative measures –because they take six ordered values-- can give us a better idea of the effects on earnings of different intensities, or degrees of leadership skill. Of particular interest to us is the upper tail of the leadership distribution.

In the case of the *Talent* data, recall that the "self-assessed" measure of leadership available to us is essentially the sum of positive answers to five yes/no questions. Thus the indicator can take a total of six distinct values (0 to 5); we include a dummy variable for each of

[19] Table 3 presents only those specifications that include a full set of high school fixed effects. Results with state fixed effects are presented in Appendix Table A; they are nearly identical.
[20] In a previous version of this paper (available from the authors) we attempted to quantify the likely bias arising from the longer (3-year) measurement "window" in Talent compared to the other studies. Under plausible assumptions about the measurement process, we argued that changes in the length of the window alone could not explain the increase the estimated returns to high school leadership across surveys.
[21] Table 4 presents only those specifications that include a full set of high school fixed effects. Results with state fixed effects are presented in Appendix Table B; they are very similar.

these values (aside from zero, the omitted category) in the Table 4 regressions.[22] For convenience, descriptive statistics on the distribution of this leadership indicator are provided in the leftmost column of Table 4: In *Talent*, only three percent of respondents answered "yes" to all five questions; seven percent answered "yes" to four questions. Thus, the group with four positive responses constitute the 90th to the 97th percentiles of the leadership distribution, according to *Talent*'s *silead* measure. Thirty-seven percent answered "no" to all five questions.

Recalling that about 20 percent of the *Talent* sample were both a team captain and a club president in the three years preceding their high school interview, the *Talent* results in the first four columns of Table 4 are broadly consistent with those in Table 2: Looking at the most highly-parameterized specification in column 4, those in the 80th to 90th percentiles of self-assessed leadership ability (*silead* = 3) enjoyed a 5.5 percent wage advantage 11 years after high school over those in the bottom 38 percent. (Recall that, using the leadership *activity* measures in column 8 of Table 2, the top 20 percent enjoyed a 3.8 percent advantage over the bottom 58 percent). Aside from demonstrating the robustness of our results to different indicators of leadership skill, the main new information provided by Table 4 concerns those individuals with very high levels of (self-assessed) leadership skill: these persons enjoy much larger earnings advantages 11 years later than those with only moderate self-assessed leadership skills. For example, the top three percent enjoy a wage advantage of 16 percent, which is higher than the effect of completing a bachelor's degree in this sample.[23]

Results of our alternative leadership measure in HSB are shown in columns 5-8 of Table

[22] As noted earlier, we do not have access to the responses to the individual questions. Thus the specification in Table 3 contains the most detailed representation of responses to the leadership questions that is possible.

[23] Because all respondents in each of these data sets attended high school at the same time and were re-interviewed at the same time, our education coefficients need to be interpreted with care: persons with a college degree have, on average, four years less work experience than those with only high school. This collinearity between education and experience explains why we do not include an experience indicator in our earnings regression, and explains the relatively low returns to education we estimate. Of course this does not affect the quality or interpretation of our leadership coefficients because the one variable (education) suffices to hold both education and experience constant.

4, and are very similar in pattern.[24] While our "continuous" 1971 and 1991 leadership measures may not be exactly comparable, it is a happy coincidence that the distribution of responses to them allows us to compare a top 3 percent (and approximately a top 10 and 25 percent) of high school leaders across years. By construction, such comparisons are not influenced by the "window" effect that generated a much larger number of leaders in *Talent* than in later surveys using the activity-based measure. The fact that estimated returns are much larger for a smaller group with "intense" leadership skills is consistent with the notion that these "window" effects matter. At the same time, the fact that the wage coefficients for the top three percentile groups – all of comparable size-- are much larger in 1991 than in 1971 is consistent with the notion that the market price of leadership skills did increase over the two decades.[25]

4. The Structure of the Leadership Effect

Section 3 has, we believe, convincingly demonstrated that men who occupied leadership positions in high school, or who perceived themselves to be leaders while in high school, earn more as adults, even when traditional measures of human capital and cognitive skill are held constant. But is this necessarily because they possess a particular skill in dealing with groups of people? In the current section, we argue that additional clues to the source of the leadership-wage effect can be obtained by asking whether this effect differs substantially in size and significance across population subgroups. For example, if the leadership-wage effect is confined to those men who went on to college, it might be simply that high-school leadership activities provide access to

[24] Since we constructed this measure ourselves, we *do* of course now have access to the answers to each of the underlying questions. For comparability with the Talent data, however, we choose to analyse them only in this simple "sum of positive responses" form.

[25] Given the relatively small size of the HSB data set and the small fraction of men (3%) in this top leadership category, we were concerned that this very high estimated coefficient of .325 could just be a statistical fluke. While the effects of any pure statistical noise should be reflected in our standard errors, to increase our confidence in these estimates we used the fact that HSB respondents are interviewed about their earnings annually, rather than just once. When we replaced our one-year measure of 1991 earnings by the average of 1990 and 1991 earnings, the estimated coefficients on the five leadership categories in column 8 of Table 4 changed to .330 (.073), .194(.050), .146(.048), .170(.042) and .064(.035) respectively. Thus a better measure of permanent income yields nearly the same coefficient estimates with smaller standard errors, increasing our confidence that the true effect of being in the top leadership category is between 30 and 35 percent.

higher-quality colleges, both by impressing admissions committees and via the financial support of scholarships, athletic and otherwise. Similarly, if the leadership-wage effect is confined to those with high mathematics test scores, we would be less confident that this effect is distinct from cognitive ability. Although —for reasons already discussed— the *Talent* data has the smallest estimated leadership coefficients of the three data sets available to us, we conduct all our analysis of these interaction effects using the *Talent* data because of its much larger sample size.

Leadership-wage coefficients within math test score groups, within own-education groups, and within parental-education groups from the *Talent* data are reported in Table 5. Separate regressions, identical to those in column 8 of Table 2 except for the interactions, were run to estimate each of these sets of interactions. Leadership wage effects, for both types of leadership measures used in this paper, are essentially *identical* for persons above and below the median math score. In our opinion this strengthens the case that leadership ability is something quite distinct from the cognitive skills measured by mathematics test scores.[26] With the exception of the "captain only" coefficient, the same is true for the respondent's own education: leadership wage effects are the same for persons who completed a bachelor's degree as for those who did not. Somewhat surprisingly, being a team captain (but not a club president) during high school has a significant effect only on the wages of individuals who did *not* go on to finish college. Perhaps sports is the arena where leadership skills among the less academically-inclined are disproportionately exhibited.

Finally, the evidence regarding parents' education is more mixed, perhaps because the sample is now divided more finely into three groups: leadership activities seem to have a bigger wage effect among students with highly-educated parents, while self-assessed leadership matters more among students whose parents did not complete high school. In sum, none of our evidence

indicates that the leadership-wage effect is confined to one readily-identifiable population subgroup. Instead it is broadly detectable across groups with different math test scores, own education, and family background.[27] This increases our confidence that leadership really is a distinct skill, rather than a byproduct of some other skill or characteristic.[28]

5. Alternative Explanations for the Leadership Effect

In this section we consider three alternatives to interpreting our estimated leadership-wage effect as the return to a social skill. While some of these alternative processes do turn out to have important effects on adult wages, we show that they cannot account for the robust leadership-wage effect observed in the analysis so far.

a. Is it just measurement error in cognitive ability?

One alternative explanation of the leadership-wage effects documented in Tables 2 through 5 is that our math test is only a noisy measure of some underlying level of cognitive ability, and that high school leadership acts as a proxy for this unmeasured, permanent component of ability. If leadership activities are correlated with "true" ability, they can now have a positive earnings coefficient even in the absence of any causal effect on wages.[29] Fortunately, students in the HSB data were given similar math tests in both the 10th and 12th grades, allowing us to address this issue in one of our data sets. Assuming independence of measurement error

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[26] While the more recent data sets are too small to replicate Table 5 exactly, the result that leadership effects do not differ significantly between those with low versus high test scores is robust. When an interaction term between "both" and the math score was entered into the regressions of Table 3, columns 8 and 12, the estimated coefficient on the interaction term was highly insignificant (less than one-fourth of the standard error) in each of the recent cohorts: 0.02 (0.18) in NLS72 and 0.03 (0.17) in HSB.

[27] We conducted a similar exercise to those in Table 5 using family income (during high school) as our measure of family background (recall this information was available only for a considerably smaller sample, and may be subject to more measurement error than parents' education. Self-assessed leadership had a strong and significant wage effect among low, medium *and* high family income groups. The pattern for the three leadership activity measures ("captain", "leader" and "both") was more complex, but looking across all the measures, positive leadership effects were seen at both ends of the income distribution.

[28] In a previous version of the paper (available from the authors) we used the HSB data to study one other interaction effect; that between leadership and (early) labor market experience. We found, somewhat surprisingly, that the effect of high school leadership on wages becomes stronger with experience, suggesting a model in which the market takes time to learn individuals' leadership skills.

[29] The same reasoning appears in the black-white wage gap literature, e.g. Neal and Johnson (1996).

across tests, we can therefore correct for measurement error in math scores by using the 10^{th} grade score as an instrument for the 12^{th} grade score.

IV and comparable OLS coefficients for our HSB data are reported in Table 6. The regressions reported there are identical in specification to those in column 12 of Table 3, with the exception of a small decline in sample size resulting from the need to observe math scores in the 10^{th} grade. As expected, instrumenting for math scores increases the coefficient on this variable: in fact it nearly doubles, suggesting that measurement error in test scores is important. Strikingly, however, the other coefficients in the regression –in particular, the returns to being a president only, captain only, or "both"-- are essentially unchanged. We conclude that measurement error in math scores does not explain our estimated wage effects of leadership skills, including the very high leadership effect we find in the most recent year of our data, 1991.

b. Is it Beauty? Or Height?

Another alternative explanation of our result is that the leadership variable is acting as a proxy for a very different unobserved determinant of earnings: "beauty" or physical attractiveness (Hamermesh and Biddle 1994; Averett and Korenman 1996; Biddle and Hamermesh 1998). It is possible that teams and clubs select physically attractive persons into "leadership" roles, and that the labor market's propensity to reward beauty, not the managerial or other social skills these people possess, explains our results. Relatedly, personnel psychologists have presented evidence that, *ceteris paribus*, tall people are more likely to win promotions into managerial positions and to receive favorable job evaluations (see for example Melamed and Bozionelos 1992). If tall people are more likely to be team captains in high school, this could also account for our result.

While we do not have as detailed measures of "beauty" as the two Biddle/Hamermesh papers had, we can still test the beauty hypothesis in two of our three data sets. HSB contains a measure of self-assessed beauty in the question "Others think I'm unattractive". *Talent* contains the height and weight information which (a) allow us to assess the "height" hypothesis, and (b)

allow us to compute each respondent's body mass index (BMI): the "beauty" indicator used by Averett and Korenman. While neither of our beauty measures will allow us to identify individuals who are exceptionally attractive, Hamermesh and Biddle (1994) find that the most important effects of beauty on earnings occur at the lower, not the upper tail of the beauty distribution, effects which should be well captured by our measures here.

Part (a) of Table 7 reports the HSB beauty and leadership coefficients from identical regressions to those in columns 9, 11 and 12 of Table 3, with beauty coefficients added. In HSB, the beauty variable is clearly capturing something that affects earnings as expected, with individuals who report that others think they are unattractive earning 17 percent less 9 years later. Interestingly, however, this coefficient becomes much smaller when high school fixed effects are added to the regression. More importantly, the estimated leadership effect in column 6 of Table 7(a), at .202, is almost identical to that without beauty controls, .221, in Table 3, column 12.

Adding BMI and height to the *Talent* regressions has similar effects, shown in parts b and c of Table 7 for the "activity based" and self-assessed measures of leadership respectively. In these regressions, note first that BMI does affect earnings as one might expect: those who are substantially overweight in high school face significant earnings penalties as adults (again, the difference in timing avoids endogeneity issues: in this case the possibility that poor labor market outcomes might cause extreme weight gain). Persons who are underweight also earn less later but the coefficient is insignificant in most specifications. Although there is some indication that short people earn less than others, the height coefficients are generally insignificant as well. Most importantly, adding controls for both BMI and height has essentially no effect on either the activity-based or self-assessed leadership coefficients. We conclude that neither beauty nor height are likely to account for our estimated leadership-wage effect.[30]

[30] A possible objection to Table 7 is that the controls for bmi and height are not detailed enough. To address this we replicated the analysis of panels (b) and (c) replacing the four bmi and height indicators used there (very tall, short, obese, thin) by twelve, for the top and bottom 2%, 5% or 25% of the height and bmi distributions. The leadership coefficients were virtually unchanged.

c. Is it some other psychological characteristic?

When economists discuss the residual in earnings regressions, a long list of psychological traits, such as "drive", "energy", "work ethic", "persistence", and "motivation" are usually mentioned in addition to factors like IQ. Most data sets, of course, do not allow the effects of these traits on earnings to be distinguished. In the *Talent* data, however, we have a unique opportunity to ask whether our leadership coefficients are acting as proxies for such other (presumably productive) psychological characteristics, or whether they are more specifically associated with a skill in motivating and co-ordinating groups of people as we have been arguing here.

To distinguish the leadership effect from other personality characteristics that might affect earnings, Table 8 examines returns to other self-assessed psychological characteristics measured in the *Talent* data. Each of these measures is constructed in a manner similar to the *silead* measure of leadership, from different sets of questions within the same 150-item instrument. The measures include "sociability" (based on 12 questions such as "I'd rather be with a group of friends than at home by myself" and "I enjoy getting to know people"), "tidiness" (11 questions such as ""I am never sloppy in my personal appearance" and "Before I start a task, I spend some time getting it organized"), "vigor" (7 questions such as "I can work or play outdoors for hours without getting tired" and "I am full of pep and energy"), self-confidence (12 questions such as "People seem to think I usually do a good job on whatever I'm doing" and "I'd enjoy speaking to a club group on a subject that I know well"), mature personality (24 questions such as "I do my job, even when I don't like it," "I do things the best I know how, even if no one checks up on me," "I think that if something is worth starting, it's worth finishing," "I am dependable," "People consider me persistent," "I am conscientious" and "I am reliable"). For easy comparison, all of these measures are standardized to have mean zero and standard deviation 1 within each grade cohort.

The first six columns of Table 8 describe regressions in which these additional psychological measures are added to our *Talent* leadership regressions one at a time. The upper portion of the table adds the additional psychological measures to the wage regressions that use the self-assessed leadership measure (*silead*); the lower portion does the same for the wage regressions which use our activity-based measures of leadership. In the lower portion of the table we report only the leadership coefficients; the coefficients on the other psychological measures are very similar to those in the top half of the table.

Looking at the effects of these newly-introduced psychological measures in the top half of the table, it is clear that (when added on its own) each one of them has a statistically significant, positive effect on *log* wages, with coefficients ranging from .012 (.004) to .027 (.004). Importantly, however, none of these psychological traits on its own, nor all of them combined, reduces the estimated leadership effect to insignificance: controlling for all five measures, the leadership coefficient remains strongly statistically significant at .024 (.005). Of these additional personality measures, the sociability index stands out, causing the largest reduction in the *silead* coefficient, from .037 (.004) to .027 (.005). The last three rows of Table 8 show that similar results obtain when the behavioral measure of leadership is used. We conclude that leadership is not simply capturing any of these other, potentially productive psychological traits. If anything, we take the largest attenuation of our results when "sociability" is controlled for as further confirmation that our leadership variable is capturing some sort of social skill.

6. Leadership Skills and Adult Occupations

If our measures of leadership are truly picking up a skill that is useful in motivating groups of people, we should expect to find that persons with these skills will be assigned to occupations —such as managerial ones—where leadership skills are most useful. To address this question, Table 9 shows the relationships between our leadership measures and entry into

managerial occupations.[31] Column 1 of Table 9 shows that men who acted in *any* leadership capacity in high school (captain, president or both) have a higher probability of occupying a managerial job 11 years later, compared to men who attended the same high school with the same math scores, parental education, and own education. There is apparently no additional effect from more "intense" high school leadership activity. Mirroring this result, having a self-assessed leadership score above the 65th percentile raises the probability of occupying a managerial job by about 30 percent, but no additional effects are found at higher percentiles of the leadership skill distribution.

Table 10 presents the results of wage regressions that control for occupation fixed effects. These results shed some important light on the relatively weak effect of "intense" high-school leadership on selection into managerial occupations noted above, by showing that the effect of leadership on earnings operates primarily through channels other than selection into a managerial job. In fact, most of the return to very high levels of leadership skill takes the form of higher wages *within* managerial occupations. To see this, note first (comparing columns 1 and 2) that adding fixed effects for very detailed (a total of 694) occupational categories has only a very small effect on our estimated leadership wage effects.[32] For example, when subsequent educational attainment is not held constant (columns 1 and 2), the wage return to being both a captain and a president falls by about thirteen percent (from 5.5% to 4.8%) when occupation controls are introduced. When educational attainment is held constant (columns 4 and 5), adding occupation controls does not change the estimated return to leadership activity at all. Next,

[31] "Managerial" occupations include the following categories of occupation, selected from 58 pages of identified jobs: Business management, administration or supervision (e.g. foreman, sales manager, office manager, executive, bank president, engineering manager, data processing manager/supervisor, production manager, personnel administrator), public administration (e.g. government administrator, hospital administrator, school principal, college dean, park administrator, youth director or leader), artistic director (e.g. musical conductor, theatrical director or producer), and military officer. In our sample, 15.7% were employed in one of these "managerial" occupations, 87% of those (13.6% of the sample) in the business management, administration or supervision category. A detailed list of managerial occupations is available on request from the authors.

[32] To be able to introduce this battery of occupation dummies, we replaced the school fixed effects in column 8 of Table 2 by a continuous variable equal to the log of mean wages in the respondent's state. Comparing the Table 2 results with column 1 of Table 10, it is clear that this has almost no effect on the results.

comparing columns 2 and 3, or 5 and 6, note that leadership skills are rewarded both in those occupations we classified in Table 9 as managerial, and in other occupations, but that the marginal rewards are greater in managerial occupations. In all cases, the return to leadership in nonmanagerial occupations is only slightly smaller than in Table 2 (which estimates a population-weighted average effect). For example, looking within nonmanagerial occupations in column 6, the "both" group earns 4.2 percent more than other men. Within managerial occupations, however, this rises to (4.2 + 2.7=) 6.9 percent. A more dramatic (and statistically significant) difference is evident for team captains. Finally, a one-standard deviation increase in self-assessed leadership raises wages by 2.1 percent in nonmanagerial occupations, but by (2.1 + 3.8=) 5.9 percent in managerial occupations, a highly significant difference.[33]

Taken together, two of our results in this section --the greater likelihood that high-school leaders occupy a managerial position as adults, and the greater economic return to leadership skills within managerial versus other occupations—increase our confidence that our measures of leadership skill capture a social skill that is useful in managing or motivating groups of people. At the same time, we are intrigued by two other results of this section: (a) the fact that most of the wage effects of leadership skill operate *within*, not between, very detailed occupational groups, and (b) even among men who do not occupy management positions as adults, those who engaged in leaderhip activities during high school earn higher wages. These results suggest that leadership skills may have a component that is rewarded even when an individual does not explicitly occupy a leadership position.

7. Is Leadership Determined before High School Entry?

Consider for a moment two very different models of the production of leadership skills.

[33] The high return to leadership within managerial occupations can be further illustrated by a wage regression identical to column (6) of Table 10, but restricted to persons in managerial occupations, that breaks down our continuous self-assessed leadership measure (SILEAD) into a series of dummy variables. Coefficients (standard errors) for SILEAD levels of one through five are, respectively, .057 (.027), .104 (.034), .130 (.032), .208 (.047), and .230 (.049).

In one model, an individual's level of leadership skill is fixed before entry into high school. The measures of leadership activity during high school used in the preceding empirical analysis merely provide observable indicators of the presence of this skill, thus identifying pure economic rents to a scarce endowed talent. According to a second view, leadership skills are acquired during high school, perhaps as a result of occupying a leadership position during that period. Even though these two views have quite different implications for education policy –especially for policies which might affect the number of leadership opportunities available to high-school students-- nothing in our analysis so far allows us to distinguish between them.

In this section we attempt to shed some light on the distinction between selection into leadership roles and human capital generated by occupying those roles by controlling for indicators of leadership at high school entry in the wage regression. Again we turn to the *Talent* data, not just because of sample size but because of the unique multicohort structure of this data set. Even though the *Talent* students were interviewed only once during high school, *Talent*'s multi-cohort structure allows us to distinguish between the leadership skills of students early in their high school careers (as measured by their self-assessed leadership abilities or by their previous participation in leadership activities) and the *number of (per-student) leadership opportunities that will be available to those students over the next few years* (as measured by participation rates of older students in their school). We refer to the latter as school-level-opportunities (SLO's). If SLO's continue to affect adult earnings even when individual, entry-level leadership indicators are held constant, this should increase our confidence that leadership skill is, in part, produced via participation in leadership activities during high school, and not just a consequence of self-selection into high school leadership roles by boys who already possess

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Although the specifications are not directly comparable, it is still noteworthy that each of these coefficients exceeds its counterpart in column 4 of Table 4.

those skills.[34]

The results of this exercise are reported in Table 11. Because the tenth grade is the youngest age at which all students are enrolled in schools with eleventh and twelfth graders, we use the tenth grade as our "baseline", focusing only on the acquisition of leadership skills after that point. In columns 1-5 we restrict the sample to just these tenth-grade students and ask whether, controlling for their "endowed" leadership level in grade 10, their adult earnings are higher if they are exposed to more leadership opportunities during eleventh and twelfth grades.[35] Columns 1 and 2 describe the relationship between school-wide leadership opportunities (SLOs) and adult earnings—Column 1 with no individual leadership controls, and Column 2 with controls for tenth grade self-assessed leadership (*silead*). At first glance, these results are not strongly favorable to the "acquired skills" hypothesis: tenth-graders who will be exposed to more leadership opportunities over the next two years earn more as adults, but the estimated effect is small and statistically insignificant. Column 3 however suggests that this is not the whole story: among tenth graders with high *silead* scores, the effect of subsequent leadership opportunities is strong.[36] Tenth graders whose personality assessments suggest that they have an inclination to leadership earn nearly ten percent more as adults if they attend schools where 25 percent of older students are heavily involved in leadership activities, compared to otherwise similar students who attend schools where only 15 percent of older students are in leadership activities.[37]

This result suggests to us that there may be a causal effect of leadership opportunities on

[34] In a previous version of this paper we also used SLO's as an instrument for individual leadership. Large effects were found but it is unclear whether this is a valid instrument. This analysis did, however, address the possibility that the SLO measure might be picking up differences in the overall educational environment of the school or the family background of its students by including controls for schoolwide average math scores, schoolwide average gains in math scores between 10th and 12th grades, and the proportion of students in the school with high or low family income, as well as individual level measures of math scores, parents' education, and geographic variation in wage levels.

[35] Specifically, we measure future opportunities by the proportion of 11th and 12th graders in their school who were both captain and president.

[36] It remains possible that the increment in future earnings associated with high-SLO schools is produced by some other characteristic of high schools we are not able to observe. Sorting this out clearly requires a valid instrument, either for individual leadership activity, or for SLOs at the school level. However, the fact that the benefits of SLOs accrue only to students with a prior inclination to leadership suggests that the effect works through leadership development, rather than some other mechanism.

later outcomes. If our SLO measure was *only* picking up unmeasured differences in school quality, it would tend to be correlated with higher earnings for *all* students in the school. Instead, we find that the availability of more school-level opportunities predicts higher earnings only for those students who had previously demonstrated a propensity to leadership. While other explanations are of course possible, the simplest explanation of this observation is that high-SLO schools tend to provide additional leadership opportunities to students with an inclination to leadership, and the affected students benefit from their experiences.[38]

Column 4 of Table 11 performs the same exercise as column 3, substituting an activity-based measure of "entry-level" leadership --previous leadership experience of tenth-graders—for *silead*. Here, the effect of schoolwide 11th and 12th grade leadership opportunities on students who had been both captain and president by tenth grade is similar to the effect of opportunities on students with high *silead* scores. It is perhaps worth stressing that this result, like those in columns 1-3, obtains even though we do not know *which* of these tenth-graders actually participated in the leadership opportunities (SLO's) available to them over the next two years; we can only speculate that the effects would be considerably stronger if we could in fact identify those individuals.

Finally, in columns 5 and 6 we repeat the exercise of columns 2 and 3, but using all students in grades 10 through 12, and a SLO measure based on all students. Clearly, inclusion of controls for individual *silead* will now bias our estimates of the effects of SLOs, particularly among the older students. However, if —as we might expect— SLO's contribute to stronger growth in self-assessed leadership, this bias should be downward. Thus, these estimates —based on a much larger sample— should place a lower bound on the actual effects of school-level opportunities. Again we see that, among students with high self-assessed leadership ability

[37] A contrasting result is that the availability of leadership opportunities has similar effects on students with low and on students with high math scores.

(*silead)*, students at schools with more leadership opportunities earn significantly more than those at schools with fewer opportunities. Students with lower *silead* scores who attend the same schools show little, if any, benefit from the availability of leadership opportunities.

In sum, in our attempt to distinguish between "endowed" (i.e. determined before high school entry) versus acquired (during high school) models of the production of leadership skills, we have shown the following: *Holding constant the leadership skills (either self assessed or measured by previous participation) of tenth-grade students*, students attending schools where they will be exposed to more leadership opportunities in grades 11 and 12 earn more as adults. Interestingly, the effect is significant only for students who already have high indications of leadership skill by the tenth grade, suggesting a kind of complementarity between endowed and acquired components of leadership skill.

[38] Unmeasured aspects of school quality could of course still explain our results if they have a larger effect on the adult earnings of students who enter high schools with a measured inclination towards leadership than other students (holding test scores and other factors constant). It is hard, however, to think of aspects of school quality other than leadership development opportunities themselves that would have this property.

8. Discussion

This paper has taken a peek into the "black box" that labor economists call "unobserved ability". It has taken its inspiration from a quality employers repeatedly list among their most highly-valued attributes when hiring workers, yet which has been largely ignored by economists to date. It has shown that indicators of leadership skill taken before labor market entry exhibit correlations with adult wages which are both economically and statistically significant. These correlations are not artifacts of measurement error in cognitive skills or of differences in a considerable array of physical and psychological attributes. The notion that these correlations capture the market value of a "social" skill related to the management of people is buttressed by the fact that high school leaders are more likely to be managers as adults, and the fact that the wage premium associated with high school leadership is higher *within* managerial occupations than other occupations.

While less definitive than our findings concerning the pricing of leadership skills, we also present some results concerning the production process for leadership skills. These results suggest that at least some component of leadership skill is fostered by occupying leadership positions during high school. If true, this may help explain the intensive recent efforts of many parents, schools and businesses to involve students and employees in leadership-development activities. Given these findings, researchers may thus be well advised to devote more attention to the study of "soft" interpersonal skills such as leadership, relative to those components of human capital that have occupied most of the profession's attention to date.

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Table 1: Sample Means

	Talent, grades 10 - 12 (High School 1960, Earnings 1971-73)	*Talent*, grade 12 only (High School Seniors 1960, Earnings 1971)	NLS72 (High School Seniors 1972, Earnings 1986)	HSB (High School Seniors 1982, Earnings 1991)
Earnings:				
Hourly Earnings	$5.30		$12.60	
Annual Earnings		$11,000		$26,100
Leadership: [1]				
Both Captain & President	0.218	0.212	0.065	0.121
Captain only	0.138	0.137	0.102	0.133
President only	0.221	0.225	0.156	0.222
Membership: [1]				
Both on team & in club	0.776	0.778	0.427	0.490
On team only	0.027	0.019	0.163	0.111
In club only	0.179	0.189	0.237	0.267
Math Score (percentile/100)	.487	.482	.465	.538
Educational Attainment:				
HS only	0.329	0.342	0.358	0.290
Some College	0.278	0.263	0.298	0.320
College Degree or higher	0.392	0.395	0.344	0.390
Parents' Education:				
High School[2]	0.548	0.556	0.554	0.601
College Degree[3]	0.195	0.187	0.312	0.326
Number of Schools Represented	1005	761	824	699
Sample Size	24,041	8,269	3,083	2,383

Notes: In all cases the sample includes only white men.
1. *Talent* Leadership/Membership refers to any time in the past three years; NLS and HSB to the last year only.
2. At least one parent is a high school graduate, but neither is a college graduate.
3. At least one parent is a college graduate.

Table 2: Effects of High School Leadership Activities on Log (hourly earnings) eleven years later, Project *Talent* Data.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leader: [1]								
Both Captain & President	0.059 (0.014)**	0.054 (0.014)**	0.053 (0.014)**	0.042 (0.014)**	0.054 (0.012)**	0.050 (0.012)**	0.049 (0.012)**	0.038 (0.012)**
Captain only	0.030 (0.015)	0.030 (0.015)	0.030 (0.015)	0.029 (0.015)	0.035 (0.013)**	0.036 (0.013)**	0.036 (0.013)**	0.034 (0.013)**
President only	0.043 (0.013)**	0.025 (0.013)	0.024 (0.013)	0.014 (0.013)	0.036 (0.011)**	0.019 (0.011)	0.019 (0.011)	0.010 (0.011)
Member: [1]								
Both on team & in club	0.098 (0.027)**	0.057 (0.027)*	0.051 (0.027)	0.037 (0.026)	0.109 (0.025)**	0.075 (0.025)**	0.070 (0.025)**	0.056 (0.025)*
On team only	0.083 (0.039)*	0.061 (0.038)	0.058 (0.038)	0.058 (0.037)	0.087 (0.035)*	0.065 (0.034)	0.063 (0.034)	0.062 (0.034)
In club only	0.026 (0.029)	-0.007 (0.028)	-0.011 (0.028)	-0.021 (0.028)	0.038 (0.027)	0.009 (0.026)	0.005 (0.026)	-0.005 (0.026)
Math Score:		0.235 (0.018)**	0.230 (0.019)**	0.139 (0.021)**		0.218 (0.016)**	0.213 (0.017)**	0.131 (0.019)**
Parents' Education:								
High School[2]			0.026 (0.012)*	0.015 (0.012)			0.021 (0.011)	0.012 (0.011)
College Degree[3]			0.020	-0.006			0.019	-0.003
			(0.014)	(0.015)			(0.014)	(0.014)
Educational Attainment:								
Some College				0.066 (0.013)**				0.049 (0.012)**
College Degree or Higher				0.147 (0.015)**				0.134 (0.013)**
State Controls and Rural Indicator?	Yes	Yes	Yes	Yes	No	No	No	No
School Controls?	No	No	No	No	Yes	Yes	Yes	Yes
Observations	24041	24041	24041	24041	24041	24041	24041	24041
R-squared (adj.)	0.066	0.091	0.091	0.101	0.165	0.183	0.184	0.195

Standard errors in parentheses: * significant at 5% level; ** significant at 1% level. Sample Restricted to White Men, Grades 10-12 in 1960 who completed high school, hourly earnings between $1 and $50. All regressions include controls for grade in 1960 (cohort). Columns 3-4 and 7-8 include an indicator for missing parental education. Columns 4 and 8 include an indicator for missing own educational attainment.

1. *Talent* Leadership/Membership refers to any time in the past three years.
2. At least one parent is a high school graduate, but neither is a college graduate.
3. At least one parent is a college graduate.

Table 3. Effects of High School Leadership Activities on Log (earnings), Various Data Sources and Dependent Variables

	Talent: Log of annual earnings in 1971 (11years after high school).[4]				NLS 72: Log of hourly earnings in 1986 (13years after high school)[5]				HSB: Log of annual earnings in 1991 (9 years after high school)[6]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Leadership: [1]												
Both Captain & president	0.067 (0.023)**	0.061 (0.023)**	0.059 (0.023)*	0.046 (0.022)*	0.151 (0.055)**	0.123 (0.054)*	0.122 (0.054)*	0.112 (0.054)*	0.236 (0.055)**	0.224 (0.055)**	0.231 (0.056)**	0.221 (0.056)**
Captain only	0.013 (0.020)	0.012 (0.020)	0.010 (0.020)	0.005 (0.020)	0.050 (0.041)	0.042 (0.040)	0.042 (0.040)	0.032 (0.041)	0.114 (0.048)*	0.106 (0.048)*	0.111 (0.048)*	0.109 (0.048)*
President only	0.042 (0.017)*	0.024 (0.017)	0.020 (0.017)	0.012 (0.017)	0.021 (0.033)	-0.006 (0.032)	-0.005 (0.032)	-0.017 (0.032)	0.174 (0.038)**	0.163 (0.038)**	0.167 (0.038)**	0.165 (0.039)**
Membership: [1]												
Both on team & in club	0.156 (0.036)**	0.120 (0.036)**	0.109 (0.036)**	0.098 (0.036)**	0.131 (0.036)**	0.096 (0.036)**	0.094 (0.036)**	0.086 (0.037)*	0.050 (0.051)	0.014 (0.051)	0.017 (0.051)	0.017 (0.051)
On team only	0.082 (0.053)	0.071 (0.051)	0.067 (0.051)	0.074 (0.051)	0.080 (0.039)*	0.057 (0.039)	0.056 (0.039)	0.049 (0.039)	0.085 (0.064)	0.064 (0.064)	0.065 (0.064)	0.071 (0.065)
In club only	0.057 (0.039)	0.028 (0.039)	0.020 (0.039)	0.012 (0.038)	0.078 (0.037)*	0.070 (0.036)	0.069 (0.036)	0.065 (0.037)	-0.056 (0.053)	-0.073 (0.052)	-0.073 (0.053)	-0.071 (0.053)
Math Score:		0.255 (0.026)**	0.237 (0.027)**	0.160 (0.028)**		0.324 (0.040)**	0.322 (0.041)**	0.268 (0.045)**		0.235 (0.054)**	0.250 (0.055)**	0.208 (0.061)**
Parent Education:												
High School[2]			0.028 (0.016)	0.016 (0.016)			0.047 (0.033)	0.043 (0.033)			-0.071 (0.066)	-0.060 (0.069)
College Degree[3]			0.059 (0.021)**	0.038 (0.022)			0.032 (0.038)	0.015 (0.038)			-0.102 (0.070)	-0.101 (0.074)
Own Education:												
Some College				0.021 (0.021)				-0.029 (0.029)				-0.033 (0.041)
College Degree or higher				0.113 (0.020)**				0.089 (0.032)**				0.052 (0.046)
Observations	8269	8269	8269	8269	3083	3083	3083	3083	2383	2383	2383	2383
R-squared (adj)	0.217	0.242	0.244	0.253	0.148	0.176	0.176	0.183	0.189	0.201	0.201	0.203

Standard errors in parentheses * significant at 5% level; ** significant at 1% level All specifications include school fixed effects. Columns 3, 4, 7, 8, 11 and 12 include an indicator for missing parental education. Columns 4, 8, and 12 include an indicator for missing own education. All samples restricted to white men only.

1. *Talent* Leadership/Membership refers to any time in the past three years; NLS and HSB to the last year only.
2. At least one parent is a high school graduate, but neither is a college graduate.
3. At least one parent is a college graduate.
4. Grade 12 in 1960 who completed high school, Annual earnings between $735 and $29,400 (equivalent to $2500-100,000 in 1991 dollars).
5. Grade 12 in 1972, Hourly earnings between 2.70 and 135 in 1986 (equivalent to $1-$50 in 1960 dollars).
6. Grade 12 in 1982, who eventually completed high school, Annual Earnings between $2500 & $100,000 in 1991

Table 4: Effects of Alternative High-School Leadership Measures on Adult Earnings, Project *Talent* and HSB Data.

	Project *Talent*: log hourly wages, 11 years later				**HSB: log annual earnings, 9 years later**			
Leadership score (percentiles in distribution):	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
5 (top 3% in *Talent*) (top 3% in HSB)	0.221 (0.027)**	0.183 (0.028)**	0.182 (0.028)**	0.162 (0.027)**	0.355 (0.095)**	0.321 (0.094)**	0.337 (0.095)**	0.325 (0.095)**
4 (90th -97th percentile in *Talent*) (88th-97th percentile in HSB)	0.124 (0.018)**	0.099 (0.018)**	0.098 (0.018)**	0.079 (0.018)**	0.260 (0.052)**	0.226 (0.052)**	0.238 (0.054)**	0.225 (0.054)**
3 (81st-90th percentile in *Talent*) (78th-88th percentile in HSB)	0.085 (0.014)**	0.068 (0.014)**	0.067 (0.014)**	0.055 (0.014)**	0.172 (0.053)**	0.147 (0.052)**	0.154 (0.053)**	0.149 (0.053)**
2 (65th-81st percentile in *Talent*) (60th-78th percentile in HSB)	0.056 (0.012)**	0.048 (0.012)**	0.047 (0.012)**	0.038 (0.012)**	0.180 (0.045)**	0.154 (0.045)**	0.160 (0.045)**	0.153 (0.046)**
1 (38th-65th percentile in *Talent*) (36nd-60th percentile in HSB)	0.047 (0.010)**	0.041 (0.010)**	0.041 (0.010)**	0.035 (0.010)**	0.073 (0.037)	0.059 (0.037)	0.063 (0.038)	0.061 (0.038)
Math Score:		0.203 (0.015)**	0.196 (0.016)**	0.123 (0.018)**		0.244 (0.054)**	0.260 (0.055)**	0.212 (0.061)**
Parents' Education:								
High School[1]			0.025 (0.011)*	0.016 (0.010)			-0.072 (0.066)	-0.067 (0.070)
College Degree[2]			0.010 (0.013)	-0.010 (0.014)			-0.105 (0.072)	-0.110 (0.076)
Own Education:								
Some College				0.042 (0.011)**				-0.017 (0.040)
College Degree or Higher				0.121 (0.012)**				0.066 (0.046)
Observations	27413	27413	27413	27413	2373	2373	2373	2373
R-squared (adj.)	.164	.180	.180	.191	0.179	0.192	0.192	0.194

Standard errors in parentheses. * significant at 5% level; ** significant at 1% level
Talent Sample Restricted to White Men, Grades 10-12 in 1960 who completed high school, Hourly earnings between $1 and $50. HSB sample defined identically, using comparable real values for earnings, but for seniors only.
All regressions include high school fixed effects. *Talent* regressions include controls for grade in 1960 (cohort). Columns 3-4 and 7-8 include an indicator for missing parental education. Columns 4 and 8 include an indicator for missing own educational attainment.

1. At least one parent is a high school graduate, but neither is a college graduate.
2. At least one parent is a college graduate.

Table 5: Marginal Effects of Selected Leadership Measures on log (wages) within education and math test score groups: Project *Talent* Data.

	Math Score:		Own Education:		Parents' Education:		
	Below 50th percentile	50th percentile or above	Less Than Bachelor's Degree	Bachelor's Degree or More	Less Than High School	High School	College Degree
Leadership Activity							
Both Captain and President	0.037 (0.017)*	0.038 (0.015)*	0.037 (0.015)*	0.045 (0.021)*	0.017 (0.024)	0.042 (0.018)*	0.053 (0.025)*
Captain Only	0.034 (0.017)*	0.035 (0.017)*	0.054 (0.017)**	0.018 (0.020)	0.019 (0.025)	0.046 (0.017)**	0.010 (0.026)
President Only	0.019 (0.016)	0.002 (0.014)	0.018 (0.015)	0.013 (0.017)	0.005 (0.023)	0.008 (0.015)	0.029 (0.021)
	N=24041		N=21484		N=22953		
Self-Assessed Leadership							
silead (standardized)	0.029 (0.008)**	0.025 (0.005)**	0.027 (0.007)**	0.028 (0.007)**	0.032 (0.011)**	0.029 (0.006)**	0.015 (0.009)
	N=25383		N=22667		N=24235		

Note: Six separate regressions were run to estimate each set of interactions (Math scores, own education, Parents' education interacted with leadership measures). Regressions also included controls for all other determinants of log earnings included in column (8) of Table 2. Each regression drops observations with missing information on variables of interest in that regression (own educational attainment, parents' education, and the leadership measures).

Table 6: Effects of instrumenting for math test scores on returns to leadership and math scores

	(1) OLS	(2) IV
Leader: [1]		
Both Captain & President	0.237 (0.057)**	0.237 (0.058)**
Captain only	0.098 (0.050)*	0.097 (0.050)
President only	0.169 (0.041)**	0.167 (0.041)**
Member: [1]		
Both on team & in club	0.022 (0.053)	0.008 (0.053)
On team only	0.059 (0.067)	0.050 (0.066)
In club only	-0.078 (0.055)	-0.087 (0.055)
Math Score:	.211 (.064)**	.343 (.085)**
Parents' Education:		
High School[2]	-0.044 (0.072)	-0.050 (0.071)
College Degree[3]	-0.066	-0.078
	(0.078)	(0.077)
Educational Attainment:		
Some College	-0.042 (0.042)	-0.056 (0.042)
College Degree or Higher	0.041 (0.047)	0.003 (0.051)
State Controls?	No	No
School Controls?	Yes	Yes
Observations	2260	2260
R-squared	.449	.447

Table 7: Introducing Measures of Physical Appearance into Leadership Earnings Regressions.

	(1)	(2)	(3)	(4)	(5)	(6)
a. HSB data						
Beauty:						
"Others think I'm	-0.172	-0.162	-0.164	-0.108	-0.106	-0.110
unattractive"	(0.045)**	(0.044)**	(0.044)**	(0.051)*	(0.050)*	(0.050)*
Leadership:						
Both Captain & President	0.157 (0.046)**	0.148 (0.045)**	0.144 (0.045)**	0.209 (0.058)**	0.208 (0.058)**	0.202 (0.059)**
Captain only	0.076 (0.042)	0.067 (0.041)	0.061 (0.041)	0.104 (0.050)*	0.098 (0.050)*	0.094 (0.050)
President only	0.110 (0.033)**	0.101 (0.033)**	0.101 (0.033)**	0.145 (0.039)**	0.138 (0.039)**	0.136 (0.040)**
b. *Talent* data, Leadership Activity						
Body Mass Index & Height:						
BMI high (overweight)	-0.142 (0.045)**	-0.122 (0.044)**	-0.118 (0.045)**	-0.156 (0.045)**	-0.140 (0.045)**	-0.138 (0.046)**
BMI low (underweight)	-0.068 (0.036)	-0.054 (0.037)	-0.059 (0.036)	-0.066 (0.033)*	-0.059 (0.033)	-0.061 (0.032)
Tall	0.002 (0.011)	-0.002 (0.011)	-0.003 (0.011)	0.000 (0.010)	-0.002 (0.010)	-0.002 (0.010)
Short	-0.020 (0.021)	-0.006 (0.021)	-0.006 (0.021)	-0.156 (0.045)**	-0.019 (0.016)	-0.020 (0.016)
Leadership:						
Both Captain & President	0.057 (0.014)**	0.052 (0.014)**	0.041 (0.014)**	0.053 (0.012)**	0.048 (0.012)**	0.037 (0.012)**
Captain only	0.032 (0.016)*	0.032 (0.016)*	0.032 (0.015)*	0.037 (0.013)**	0.037 (0.013)**	0.037 (0.013)**
President only	0.041	0.022	0.012	0.033	0.017	0.008
	(0.013)**	(0.013)	(0.013)	(0.011)**	(0.011)	(0.011)
c. *Talent* data, self- assessed Leadership						
Body Mass Index & Height:						
BMI high (overweight)	-0.141 (0.046)**	-0.123 (0.044)**	-0.117 (0.045)**	-0.154 (0.045)**	-0.140 (0.045)**	-0.136 (0.046)**
BMI low (underweight)	-0.065 (0.035)	-0.052 (0.036)	-0.059 (0.035)	-0.062 (0.033)	-0.056 (0.033)	-0.058 (0.032)
Tall	0.001 (0.011)	-0.003 (0.011)	-0.003 (0.011)	0.001 (0.010)	-0.001 (0.010)	-0.001 (0.010)
Short	-0.023 (0.020)	-0.008 (0.021)	-0.009 (0.020)	-0.032 (0.016)*	-0.020 (0.016)	-0.021 (0.016)
Leadership:						
silead (standardized)	0.047 (0.005)**	0.038 (0.005)**	0.031 (0.005)**	0.044 (0.005)**	0.036 (0.005)**	0.030 (0.005)**
Other Controls (all regressions in column):						
Math Score, Parents' Education	No	Yes	Yes	No	Yes	Yes
Own Education	No	No	Yes	No	No	Yes
State Dummies and Rural Indicator	Yes	Yes	Yes	No	No	No
School Dummies	No	No	No	Yes	Yes	Yes

Note: Membership controls included in all leadership activity regressions.

Dependent variable: Log annual earnings in HSB; log hourly earnings in *Talent*. All *Talent* regressions included dummy variables for the grade attended in 1960. [overweight:bmi>31, underweight: bmi< =17, tall: >=72", short: <= 65"]

Table 8: Effects of other psychological characteristics on the estimated returns to leadership

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Using Self-Assessed Leadership (*silead*)								
silead (standardized)	0.037 (0.004)**	0.027 (0.005)**	0.030 (0.005)**	0.032 (0.005)**	0.033 (0.005)**	0.034 (0.005)**	0.024 (0.005)**	0.028 (0.005)**
Enjoys Socializing (Social)		0.027 (0.004)**					0.022 (0.005)**	
Physically Energetic (Vigor)			0.018 (0.004)**				0.008 (0.005)	0.015 (0.005)**
Hardworking & Reliable (Mature)				0.012 (0.005)*			-0.002 (0.006)	-0.002 (0.006)
Self Confident (Self)					0.014 (0.004)**		0.007 (0.004)	0.011 (0.004)*
Neat and Organized (Tidy)						0.012 (0.004)**	0.003 (0.005)	0.006 (0.005)
Using Leadership Activity:								
Both Captain and President	0.050 (0.012)**	0.038 (0.012)**	0.040 (0.012)**	0.041 (0.012)**	0.044 (0.012)**	0.044 (0.012)**	0.033 (0.012)**	0.036 (0.012)**
Captain Only	0.036 (0.013)**	0.030 (0.013)*	0.030 (0.013)*	0.033 (0.013)**	0.033 (0.013)**	0.034 (0.013)**	0.028 (0.013)*	0.030 (0.013)*
President Only	0.019 (0.011)	0.011 (0.011)	0.015 (0.011)	0.013 (0.011)	0.014 (0.011)	0.015 (0.011)	0.007 (0.011)	0.010 (0.011)
Additional Controls:	None	Social	Vigor	Mature	Self	Tidy	All Five	All But Soc

Notes: All regressions include controls for math test score, parents' education, grade in 1960 indicators, school fixed effects. Captain/President regressions included controls for team and club participation. Sample sizes 24030 with leadership activities, and 27413 without. All psychological measures are standardized to have mean zero, standard deviation 1 within grade cohorts.

Table 9: Means and Linear Probability Model Coefficients for Selection into "Managerial" Occupations, *Talent* Data

	Coefficient	**Proportion in Managerial Occupations**
Leadership Activity:	**(std. error)**	
Both Captain & President	.036 (.011)**	.173
Captain only	.036 (.013)**	.177
President only	.036 (.010)**	.179
Neither Captain nor President	—	.149
Self-Assessed Leadership (continuous)	0.023 (0.004)**	
Self-Assessed Leadership (categorical):		
silead = 5 (top 3%)	0.063 (0.020)**	.188
silead = 4 (90^{th} -97^{th} percentile)	0.057 (0.018)**	.194
silead = 3 (81^{st}-90^{th} percentile)	0.053 (0.014)**	.190
silead = 2 (65^{th}-81^{st} percentile)	0.057 (0.011)**	.199
silead = 1 (38^{th}-65^{th} percentile)	0.008 (0.008)	.145
silead = 0 (below 38^{th} percentile)	—	.134

Robust standard errors in parentheses
* significant at 5% level; ** significant at 1% level

Other controls included in all regressions: math test score, parents' education, own education, grade in 1960 indicators, school fixed effects. Captain/President regressions included controls for team and club participation. Sample sizes 24939 with leadership activities, and 28425 with *silead*

Table 10: Introducing Occupation Controls into Leadership Wage Regressions: *Talent* Data.

	(1)	(2)	(3)	(4)	(5)	(6)
a. Leadership Activity						
Both Captain and President	0.055 (0.014)**	0.048 (0.011)**	.046 (.012)**	0.042 (0.014)**	0.043 (0.011)**	0.042 (.012)**
Captain Only	0.029 (0.015)	0.028 (0.013)*	.013 (.014)	0.028 (0.015)	0.027 (0.013)*	0.013 (0.013)
President Only	0.023 (0.013)	0.022 (0.010)*	.013 (.011)	0.013 (0.012)	0.018 (0.010)	0.010 (0.011)
"Both" x Managerial Occupation			.030 (.033)			0.027 (0.032)
Captain Only x Managerial Occupation			.102 (.036)**			0.103 (0.035)**
President Only x Managerial Occupation			.059 (.030)*			0.055 (0.029)
b. Self-Assessed Leadership						
silead (standardized)	0.038 (0.005)**	0.030 (0.004)**	.023 (.005)**	0.031 (0.005)**	0.028 (0.004)**	0.021 (0.005)**
silead x Managerial Occupation			.040 (.012)**			0.038 (0.012)**
Own Education controls	No	No	No	Yes	Yes	Yes
Occupation controls (694 categories)	No	Yes	Yes	No	Yes	Yes

Robust standard errors in parentheses. * significant at 5% level; ** significant at 1% level. Sample size: 23980 for all regressions
Controls included in all regressions: math score, parents' education, grade in 1960, log(state average wages). Controls for team and club membership included in all leadership activity regressions. *silead* standardized to mean 0, sd 1 within grade cohort
Dependent Variable: log of hourly wage 11 years after high school.

TABLE 11—Effects of Schoolwide Leadership Opportunities (SLO) & Student Characteristics (X) on Adult Wages, controlling for tenth-grade measures of individual leadership.

	(1)	(2)	(3)	(4)	(5)	(6)
Schoolwide leadership opportunities (SLO)	0.014 (0.011)	0.013 (0.011)			.011 (0.005)*	
SLO*High X			0.048 (0.026)	0.059 (0.027)*		0.023 (0.009)*
SLO*Low X			0.005 (0.011)	-0.007 (0.012)		0.008 (0.005)
WHERE high X is:			*Silead* $\geq$ 3 in 10th grade	Both captain and president by 10th grade		*Silead* $\geq$ 3
% with high X			0.17	0.25		.19
Individual *silead* controls?	No	yes	yes	no	yes (interacted with cohort)	yes (interacted with cohort)
Individual leadership activity controls?	No	no	no	yes	no	no
Grade Cohorts	10	10	10	10	10-12	10-12
Observations	5874	5874	5874	5513	23388	23388
R-squared	0.05	0.06	0.06	0.06	0.09	0.09

"Schoolwide leadership opportunities (SLO)" is the standardized schoolwide mean of "Both captain and president" among 11th and 12th graders in the same school (columns 1-4), among 10th-12th graders columns 5 & 6

Robust standard errors in parentheses.
* significant at 5%; ** significant at 1%

Columns 1-4: Sample restricted to 10th graders in schools with at least 20 observations on 11&12 th graders.
Columns 5-6: Sample restricted to 10th-12th graders in schools with at least 20 observations on 10th- 12 th graders.

Controls included in all regressions: math scores, log(state average wages), rural, parents' education. Additional Controls: Six *silead* level controls in Columns 2&3, club and team leadership and membership controls (as in table 2) in column 4, *silead* level interacted with grade cohort in columns 5 & 6.
Dependent Variable: log of hourly wage 11 years after high school.

Appendix Table A. Effects of High School Leadership Activities on Log (earnings), Various Data Sources and Dependent Variables: specifications with state fixed effects.

	Talent: Log of annual earnings in 1971 (11years after high school).[4]				NLS 72: Log of hourly earnings in 1986 (13years after high school)[5]				HSB: Log of annual earnings in 1991 (9 years after high school)[6]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Leadership: [1]												
Both Captain & president	0.062 (0.024)*	0.056 (0.024)*	0.051 (0.024)*	0.041 (0.024)	0.120 (0.058)*	0.103 (0.060)	0.103 (0.059)	0.095 (0.061)	0.185 (0.045)**	0.168 (0.045)**	0.172 (0.044)**	0.167 (0.044)**
Captain only	0.003 (0.029)	-0.004 (0.029)	-0.006 (0.029)	-0.009 (0.029)	0.088 (0.043)*	0.080 (0.040)*	0.080 (0.039)*	0.069 (0.039)	0.092 (0.041)*	0.085 (0.040)*	0.086 (0.040)*	0.080 (0.040)*
President only	0.059 (0.020)**	0.036 (0.020)	0.032 (0.020)	0.025 (0.020)	-0.018 (0.039)	-0.044 (0.038)	-0.042 (0.037)	-0.055 (0.036)	0.134 (0.033)**	0.120 (0.032)**	0.123 (0.032)**	0.123 (0.033)**
Membership: [1]												
Both on team & in club	0.139 (0.031)**	0.098 (0.031)**	0.081 (0.032)**	0.074 (0.031)*	0.107 (0.046)*	0.074 (0.046)	0.072 (0.043)	0.064 (0.043)	0.128 (0.044)**	0.080 (0.045)	0.084 (0.045)	0.081 (0.045)
On team only	0.103 (0.052)*	0.091 (0.051)	0.082 (0.050)	0.091 (0.050)	0.061 (0.051)	0.034 (0.052)	0.034 (0.050)	0.027 (0.048)	0.136 (0.052)**	0.103 (0.052)*	0.105 (0.052)*	0.107 (0.053)*
In club only	0.042 (0.037)	0.007 (0.036)	-0.006 (0.037)	-0.010 (0.036)	0.055 (0.047)	0.051 (0.046)	0.049 (0.044)	0.044 (0.042)	-0.002 (0.047)	-0.024 (0.047)	-0.023 (0.047)	-0.023 (0.047)
Math Score:		0.275 (0.031)**	0.250 (0.032)**	0.180 (0.033)**		0.333 (0.047)**	0.334 (0.053)**	0.257 (0.051)**		0.282 (0.046)**	0.304 (0.048)**	0.257 (0.055)**
Parent Education:												
High School[2]			0.044 (0.020)*	0.033 (0.020)			0.030 (0.049)	0.030 (0.047)			-0.049 (0.054)	-0.049 (0.056)
College Degree[3]			0.072 (0.023)**	0.051 (0.024)*			0.014 (0.056)	-0.009 (0.057)			-0.085 (0.058)	-0.096 (0.061)
Own Education:												
Some College				0.030 (0.025)				-0.032 (0.028)				-0.049 (0.033)
College Degree or higher				0.103 (0.024)**				0.125 (0.034)**				0.054 (0.038)
Observations	8269	8269	8269	8269	3083	3083	3083	3083	2383	2383	2383	2383
R-squared (adj)	0.054	0.091	0.095	0.102	0.084	0.117	0.117	0.130	0.075	0.095	0.096	0.100

Standard errors in parentheses * significant at 5% level; ** significant at 1% level All specifications include state fixed effects and rural indicator. Columns 3, 4, 7, 8, 11 and 12 include an indicator for missing parental education. Columns 4, 8, and 12 include an indicator for missing own education. All samples restricted to white men only.

1. *Talent* Leadership/Membership refers to any time in the past three years; NLS and HSB to the last year only.
2. At least one parent is a high school graduate, but neither is a college graduate.
3. At least one parent is a college graduate.
4. Grade 12 in 1960 who completed high school, Annual earnings between $735 and $29,400 (equivalent to $2500-100,000 in 1991 dollars).
5. Grade 12 in 1972, Hourly earnings between 2.70 and 135 in 1986 (equivalent to $1-$50 in 1960 dollars).
6. Grade 12 in 1982, who eventually completed high school, Annual Earnings between $2500 & $100,000 in 1991

Appendix Table B: Effects of Alternative High-School Leadership Measures on Adult Earnings, Project *Talent* and HSB Data: specifications with state fixed effects.

	Project *Talent*: log hourly wages, 11 years later				**HSB: log annual earnings, 9 years later**			
Leadership score (percentiles in distribution):	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
5 (top 3% in *Talent*) (top 3% in HSB)	0.234 (0.028)**	0.191 (0.029)**	0.190 (0.029)**	0.168 (0.028)**	0.332 (0.071)**	0.280 (0.072)**	0.291 (0.073)**	0.281 (0.075)**
4 (90^{th} -97^{th} percentile in *Talent*) (88^{th}-97^{th} percentile in HSB)	0.130 (0.022)**	0.103 (0.023)**	0.102 (0.023)**	0.080 (0.023)**	0.200 (0.049)**	0.157 (0.049)**	0.166 (0.049)**	0.156 (0.049)**
3 (81^{st}-90^{th} percentile in *Talent*) (78^{th}-88^{th} percentile in HSB)	0.094 (0.016)**	0.075 (0.016)**	0.075 (0.016)**	0.061 (0.016)**	0.180 (0.045)**	0.137 (0.044)**	0.145 (0.044)**	0.142 (0.044)**
2 (65^{th}-81^{st} percentile in *Talent*) (60^{th}-78^{th} percentile in HSB)	0.064 (0.015)**	0.057 (0.014)**	0.056 (0.014)**	0.046 (0.014)**	0.190 (0.035)**	0.157 (0.035)**	0.159 (0.035)**	0.151 (0.035)**
1 (38^{th}-65^{th} percentile in *Talent*) (36^{nd}-60^{th} percentile in HSB)	0.048 (0.011)**	0.044 (0.010)**	0.044 (0.010)**	0.038 (0.010)**	0.086 (0.034)*	0.066 (0.033)*	0.069 (0.033)*	0.064 (0.033)
Math Score:		0.223 (0.017)**	0.217 (0.017)**	0.134 (0.019)**		0.319 (0.047)**	0.336 (0.048)**	0.279 (0.055)**
Parents' Education:								
High School[1]			0.028 (0.011)*	0.017 (0.011)			-0.051 (0.058)	-0.055 (0.060)
College Degree[2]			0.012 (0.014)	-0.012 (0.014)			-0.087 (0.062)	-0.105 (0.064)
Own Education:								
Some College				0.059 (0.012)**				-0.035 (0.033)
College Degree or Higher				0.134 (0.014)**				0.072 (0.038)
Observations	27413	27413	27413	27413	2373	2373	2373	2373
R-squared (adj.)	.071	.095	.095	.108	0.058	0.084	0.085	0.090

Standard errors in parentheses. * significant at 5% level; ** significant at 1% level
Talent Sample Restricted to White Men, Grades 10-12 in 1960 who completed high school, Hourly earnings between $1 and $50. HSB sample defined identically, using comparable real values for earnings, but for seniors only.
All regressions include state fixed effects and a rural indicator. *Talent* regressions include controls for grade in 1960 (cohort). Columns 3-4 and 7-8 include an indicator for missing parental education. Columns 4 and 8 include an indicator for missing own educational attainment.

1. At least one parent is a high school graduate, but neither is a college graduate.
2. At least one parent is a college graduate.