High School Leadership, Educational Attainment and Wage

Leadership skill is of substantive value to both employers and academic institutions. Research suggests that such skill may be fostered or signaled through leadership experience while in high school, yet few economic studies have examined the role of such experience in determining future labor market outcomes. Moreover, in the limited research that exists, the studies have been limited to distinct sub-populations and have focused on ordinary least squares specifications and results. My dissertation work seeks to fill the gaps in the limited literature by using three different datasets to assess the impact of high school leadership on future wage and educational attainment of both males and females. It will address the issue of causality by employing three econometric approaches, namely ordinary least squares, propensity score matching and instrumental variables. Preliminary results are robust to various econometric approaches and suggest that high school leadership has a statistically significant positive effect on log wages, years of education and the probability of holding a college degree of both genders. Interestingly, these effects differ by gender in their magnitude and suggest that high school leadership plays a more important role in terms of educational attainment for females while the majority of the wage effect persists beyond education for males. These apparent cross gender differences offer an interesting avenue of additional research which has not yet been exploited.

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I. Motivation and Overview

Many employers and academic institutions rank "soft-skills" such as communication, motivation and leadership higher on their list of desirable employee/student attributes than traditional academic skills as indicated through a high grade point average or class rank[1]. One skill which has gained particular attention recently is that of leadership. For instance, Kuhn & Weinberger (2005) report that top MBA programs are sending their students to leadership boot camps and that Fortune 500 companies are paying for leadership training of their employees. An examination of elite university web pages also reveals that many undergraduate institutions list leadership experience among their top admission criteria. Oprah Winfrey has even emphasized the importance of leadership, recently opening a boarding school for girls in South Africa which she calls the "Oprah Winfrey Leadership Academy for Girls".

Recent research suggests that these so-called "non-cognitive[2]" skills may be fostered through participation in extracurricular activities while in high school. Barron et al (2000), for example, argue that athletic participation in high school may increase traits such as self-discipline, motivation and competition which are subsequently rewarded in the labor market in the form of higher wages. Likewise, undertaking a leadership position in high school, such as being a team captain or a class officer, may increase one's leadership skill. Universities also use evidence of leadership experience in high school as a selection mechanism in the admissions

[1] For instance, in a recent survey by the National Association of Colleges and Employers, employers rank communication, motivation, work ethic, and teamwork skills above academic credentials in their list of top skills they look for in job candidates.

[2] Following Lee and Seng (2005), I define "non-cognitive" skills as those "intangible qualities not measurable by classroom learning attainment, cognitive tests, receipt of a diploma/degree, or acquisition of specific job skills through training (p. 2)."

process. It is therefore likely that students with leadership experience have a better chance of college admittance and more lucrative financial aid offers.

The growing importance of leadership skill in the labor market and college admissions process, together with the potential role of high school leadership in fostering and signaling such skill, begs the following questions: *Does holding a leadership position in high school lead to future wage premiums? Does high school leadership contribute to future educational attainment?*

The limited economic evidence on this subject suggests that the answer to both of these questions is yes. Kuhn and Weinberger, for example, use self-reported measures of leadership skill and high school leadership positions to estimate wage returns to leadership skill. Using three different datasets, the authors find that leadership skill translates into future wage premiums ranging from 4 to 33% for white males and that these returns are greatest in managerial occupations. Looking at educational attainment outcomes, Lozano (2004) finds high school leadership is associated with a higher probability of college attendance for all demographic groups and is associated with a higher college graduation rate of Hispanic students whose first language is not English.

While leadership skill is clearly of substantive value to both employers and admissions committees, with the exception of the above mentioned papers, to date there has been little economic work which has sought to explain the development of and return to such skills. Moreover, the analysis in these papers has been limited to distinct sub-groups of the population. Kuhn and Weinberger limit the analysis to white men, while the primary focus of Lozano (2004) is the Hispanic population. While both papers provide evidence of a positive association between high school leadership and later-life outcomes, it is less clear whether or not these estimates represent causal relationships.

My work seeks to fill gaps in this limited literature by using data from the National Education Longitudinal Study (NELS), the National Longitudinal Study of the Class of 1972 (NLS-72), and the High School and Beyond (HS&B) to estimate the causal effect of high school leadership experience on future wages and educational attainment. The samples will include both genders and will not be limited to a particular race.

In contrast to the previous research which relies on parametric approaches such as Ordinary Least Squares (OLS) and linear probability models to control for observable confounding variables, I will also use propensity score matching (PSM) to control for selection into high school leadership positions. I intend to estimate the effects of high school leadership in the context of program evaluation where high school leadership is viewed as the "treatment." PSM is arguably an improvement over OLS because it is not constrained by the assumption that the treatment effect is linearly related to the outcome. Further, unlike OLS, by matching each treated observation with an untreated counterpart, the researcher can explicitly test whether there is sufficient overlap between the two groups. Matching ensures that for every set of characteristics, X, there exists both a treated and non-treated case. Unlike OLS, matching explicitly avoids extrapolation into areas of the causal effect distribution which are not on the common support.

It is important to note that PSM does not control for selection based on unobservable characteristics. As such, if selection into leadership positions is due to such characteristics, PSM estimates will suffer from an endogeneity bias. A common solution to the endogeneity problem is to find an instrumental variable which is related to the leadership propensity but directly unrelated to the outcome of interest[3]. Indeed, while not the focus of their work, this approach is used by Lozano and Kuhn and Weinberger[4]. Both of these analyses use measures of school-level leadership opportunities as an instrument for individual leadership. Results from each paper suggest that there is indeed a causal relationship between high school leadership and later life outcomes. However, as noted by the authors, these results should be taken with caution as they depend on the validity of the instrumental variable.

One advantage of using PSM to control for selection is that, unlike IV, it does not require a valid instrument. As such, in analyses in which a suitable instrument is difficult to find, PSM offers an attractive alternative to IV. Indeed, "there is some evidence that estimators based on 'matching' cases with comparison observations using observed covariates produces better estimates of causal effects in quasi-experimental samples (Jones & Richmond (2006), p. 851)." In particular, PSM has been shown to perform well when outcomes are measured identically across the treated and untreated groups and when the data contain a rich set of covariates[5]. Recently, PSM has been used to re-assess prior IV estimates of some widely studied relationships. For instance, Nguyen, Taylor and Bradley (2006) use PSM to re-examine the effect of Catholic schooling on educational outcomes; while Jones and Richmond (2006) use PSM to re-examine the causal effects of alcoholism on earnings.

Each of these methods provides distinct advantages and disadvantages when compared to their econometric alternatives[6]. For instance, while PSM does not require a valid IV, it does not

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[3] It is important to note, however, that while PSM generally recovers an estimate of the average treatment effect on the treated (ATT), IV estimates only recover ATT under the restrictive assumption that the treatment effect is constant across the population. Under a different set of assumptions, IV estimates recover the local average treatment effect (LATE). This is discussed in more detail in Section V.
[4] While not reported in the final version of their paper, Kuhn and Weinberger report in a footnote that earlier versions of their paper included IV estimates where school-level leadership opportunities as measured by the number of leaders per student in the sample, was used as the IV.
[5] For example, see Diaz & Handa (2006), Heckman, Ichimura and Todd (1997, 1998), and Heckman et al. (1998).
[6] These approaches are discussed at length in Section V.

control for unobservable characteristics. However, if an instrumental variable is weak, IV may not provide better estimates those from simple OLS or PSM. Moreover, DiPrete and Gangl (2004) argue that "because the approaches rely on different information and different assumptions, they provide complementary information about the causal relationships (abstract)." Given this, I propose estimating leadership effects using each method. In doing so, I will be able to offer evidence on the extent to which high school leadership estimates vary by the econometric method employed and will hopefully be able to provide further evidence as to whether the relationship between high school leadership and later-life outcomes is, in fact, causal.

In addition to its methodical contribution, estimating the causal impact of high school leadership may have important implications from a policy perspective. For instance, if leadership positions are shown to have a large positive impact on students' future successes in the labor market, this would indicate that, in addition to the academic aspects of schooling, the non-academic aspects of education may also be important determinants of a student's future educational and labor market success. This would have serious implications for financial cutbacks on extracurricular activities. It may also provide justification for the development of high school programs whose goal is to foster leadership skills of its students.

Also of interest is the extent to which (if at all) differences in leadership propensities or returns can explain observed gender gaps in educational attainment and wages. While changes in cognitive skills and, to a lesser extent, non-cognitive skills have been used to assess wage and educational attainment differences across gender, the specific skill of leadership has not been examined in this capacity. Apart from the basic interest in estimating the causal returns to leadership, the returns to high school leadership may have other important implications. For, if leadership is found to have a positive effect on female wages, for example, then the increasing presence of women in leadership positions could explain part of the decreasing gender wage gap. Furthermore, use of three surveys based at different points in time will allow for cross-cohort comparisons of leadership differentials and effects.

Preliminary results are robust to various econometric approaches and suggest that high school leadership does have a statistically significant positive effect on log wages and educational attainment of both genders. Interestingly, these effects differ by gender in their magnitude and suggest that high school leadership plays a more important role in terms of educational attainment for females while the majority of the wage effect persists beyond education for males. These apparent gender differences offer an interesting avenue of additional research, and I believe an analysis which exploits this result and attempts to shed light on why high school leadership impacts males differently than females will provide an attractive focal point for my job-market paper.

The remainder of this proposal is organized as follows. In the following section I describe three bodies of literature to which I believe my work most closely relates. In the third section, I describe a simple conceptual model that provides an economic framework under which these questions may be assessed. In section IV, I discuss the data that will be used in the empirical analysis. The empirical methods are described in Section V. Preliminary results derived from the NELS public-use data are provided in section VI. Section VII concludes.

II. Literature Review

The proposed research is related to three areas of the existing labor economics literature. First, it will contribute to an emerging literature on the importance of non-cognitive skills on labor market outcomes. Such research has shown that non-cognitive skills play an equally important role in wage determination as cognitive skills. Differences in such skills have also been used to explain differences across occupations in racial wage discrimination and to explain the growing gap between males and females in educational attainment. Second, the work will add to a body of literature which assesses the effect of high school characteristics and activities on educational attainment and wages. Finally, the proposed work will contribute to and is most closely related to a very narrow body of literature which specifically seeks to assess the relationship between leadership skills, wages, and educational attainment. In the remainder of this section, I provide a detailed discussion of the previous research in each of these areas.

Non-cognitive skills

While the relationships between schooling, cognitive skill development and resulting labor market outcomes have been studied extensively in the labor economics literature, there have been far fewer studies exploring the role of non-cognitive skills in the labor market. The majority of these studies have examined the role of non-cognitive skills using measures of locus of control or self-esteem. Others have explored the role of non-cognitive skills more broadly. As such, I first discuss papers addressing locus of control and self-esteem and then move to a brief discussion of papers which have explored non-cognitive skills in a broader sense.

One of the earliest studies to look at the return to personality as it relates to locus of control is Andrisanni and Nestel (1976). Using the National Longitudinal Study of Mature Men (NLSm), the authors use multiple regression analysis to assess the effect of locus of control on labor market success. Results suggest individuals who are more internal, or who perceive their actions to be largely effective in controlling their outcomes, experience a significantly positive wage return as compared with their external counterparts. In a follow up study, Andrisanni (1978) finds similar results, leading to the conclusion that "attitudinal change among both whites and blacks with 'external' attitudes would result in greater initiative and more successful labor market experience (p. 325)." Duncan and Morgan (1981) use the Panel Study of Income Dynamics (PSID) to replicate Andrisanni (1978). Their estimates of the wage return are smaller than those of Andrisanni, but are larger once the time period used is extended. However, few of the estimated coefficients are statistically significant leading the authors to be skeptical of Andrisanni's claim stated above.

Further evidence of the effect of locus of control and self-esteem is seen in Goldsmith, Veum and Darity (1997). Using data from the National Longitudinal Study of Youth 1997 (NLSY), the paper examines the impact of psychological capital on wages. Using two-stage least squares and treating locus of control as an exogenous determinant of self-esteem, the authors jointly estimate wage and self-esteem equations. Results suggest that psychological capital has a significant positive effect on wages both directly through self-esteem and indirectly through locus of control. Estimates suggest changes in psychological capital have a larger impact on wages than changes in traditional human capital. For instance, whereas a 10% expansion of education is predicted to lead to a 1.1% increase in wages, the same 10% increase in self-esteem is predicted to improve wages by 4.8%. These findings lead the authors to conclude that "increased psychological capital is an important avenue to subsequent economic well-being that warrants greater consideration in future research aimed at understanding the determinants of personal productivity and other labor market outcomes (p. 828)."

Building a bridge between the empirical evidence and economic theory, Coleman and DeLeire (2003) develop an economic model to explain how locus of control affects wages in the context of the human capital model. Specifically, the authors model locus of control as affecting an adolescent's belief about the return to human capital investment. An adolescent who is more internal and therefore believes his actions will impact his outcomes will be more likely to invest in schooling. Using the NLSY, the authors test the theoretical model using probit models to estimate the relationship between locus of control and the probability of high school completion and four-year college attendance. Results are supportive of the implications of the theoretical model, namely that locus of control is an important determinant in educational attainment and that this effect operates primarily through an adolescent's belief about the link between his actions and their resulting outcomes.

Non-cognitive skills have been studied in a broader sense to explain wage and educational attainment differentials among cognitively equivalent individuals. For instance, Heckman and Rubinstein (2001) use differences in non-cognitive skills to explain the fact that while GED recipients and high school graduates who do not go to college are assumed to be cognitively equivalent, GED recipients earn less than traditional high school students. The authors conclude that "the GED is a mixed signal that characterized its recipients as smart but unreliable (p. 148)." Similarly, using the NELS, Jacob (2002) uses differences in non-cognitive skills to explain gender differentials in educational attainment. Findings suggest that differences across measures of non-cognitive skills and higher premiums among women can explain almost 90 percent of the gender gap in higher education.

More recently, research by Heckman, Stixrud and Urzua (2006) examines the effects of cognitive and non-cognitive abilities on a wide range of labor market outcomes and social behavior. The authors estimate a factor model with endogenous factor loadings in which latent cognitive and non-cognitive skills are used to generate measured cognitive and non-cognitive test scores. According to the authors, the method allows the researcher to control for potential endogeneity and measurement error without the use of instrumental variables. Results suggest that "a change in non-cognitive skills from the lowest to the highest level has an effect on behavior comparable to or greater than a corresponding change in cognitive skills (p. 412)." Using the same methodology, Flossman, Piatek and Wichert (2006) assess the role of non-cognitive skills on wages in Germany. Using the 1999 wave of the German Social Economic Survey (GSOEP), the authors use a factor structure model to examine a number of variables which are thought to proxy non-cognitive ability in the form of self-esteem and life control. Controlling for education and professional experience, the authors find that non-cognitive skills are an important determinant of labor market success.

High school curriculum, athletics and extracurricular participation

Only a few papers have examined the effect of high school curriculum on labor market outcomes. The first to explicitly examine the effect of curriculum on wages is Altonji (1995) which uses data from the NLS-72 to estimate the effect of high school curriculum on postsecondary education and wages using both OLS and IV techniques. Specifically, in the IV model, school-level variation in high school curriculum is used as an instrument for individual course choice. Results suggest that the return to additional courses is small and lead Altonji to infer that the return to high school education operates primarily through a screening mechanism rather than through human capital development.

Similarly, using data from the NLSY and HS&B senior cohort, Levine and Zimmerman (1995) examine the effect of math and science courses on wages. OLS results indicate that the number of math courses increases wages by 3% and 2% for males and females respectively, while there is not a significant wage premium for science courses. However, the math effects drop out once IV is employed. More recently, Rose and Betts (2004), estimate the returns to math classes. The work differs from previous work in two important ways. First, it analyzes the return to math classes by the level of math class. Second, it uses variation in course work to explain wage gaps by gender, socioeconomic class, and race. Results suggest that there is a significant wage premium associated with math classes and that this premium is larger for more advanced courses.

Another aspect of schooling which has been examined by a small selection of papers is that of athletic participation and other extracurricular activities. One of the first studies to examine the role of athletic participation on labor market outcomes is Long and Caudill (1991). Using a survey of over 10,000 entering college freshmen in 1971, the authors compute the effect of intercollegiate athletic participation on wages. Their findings suggest that males who participate in college athletics receive a 4% wage premium compared to their non-athlete counterparts whereas female athletes do not enjoy such a wage advantage. While the study focuses on college athletics, the results suggests athletics play a role in wage determination and provide qualitative support for parallel research at the high school level.

Turning to high school students, Ewing (1995) uses the NLSY to examine whether black high school athletes earn future wage premiums. The paper improves upon previous work by formulating a simple economic model by which athletic participation may be assessed. Ewing develops a team production model similar to that of Alchian and Demsetz. The model, which is set up in a time allocation framework, suggests athletics may foster personality traits such as perseverance or discipline that are subsequently rewarded in the labor market. As an additional explanation, Ewing discusses a signaling framework whereby athletic participation serves to signal a low value of leisure to employers. The research also differs from an empirical standpoint. Unlike previous research, Ewing uses the Heckman selection correction technique to account for sample selection bias. Results support those of Long and Caudill (1991) and suggest high school athletic participation increases the wages of black males by 8 to 11 percent.

In similar work, Barron, Ewing and Waddell (2000) use the NLSY and the NLS-72 to examine the effects of high school athletic participation on education and labor market outcomes of males. The paper uses an allocation of time model to explain high school athletic participation and its resulting later life outcomes. Empirically, the authors first employ a simple OLS specification. Then, using a handful of school and individual health characteristics as instruments for athletic participation, the authors employ instrumental variables techniques. Results provide some evidence of a positive impact of high school athletic participation on the wages and educational attainment of males; however the effects are small when instrumental variables are used.

Using the 1980 sophomore cohort of the HS&B, Eide and Ronan (2001) use height as an instrument for athletic participation. The work differs from previous work in that the analyses include females. Results indicate that high school athletic participation has a negative effect on the wages of white males, but has a positive effect on the wages of black males and females.

More recently, Betsey Stevenson (2006) uses state variation in the athletic participation of males along with Title IX legislation to instrument for female athletic participation. The identification arises from the observation that states which had higher male athletic participation prior to the enactment of Title IX would have had to increase female participation by a greater extent to comply with the legislation. Stevenson finds female high school athletic participation leads to numerous positive outcomes for females including higher educational attainment, increased labor force participation and increased participation of females in traditionally male-dominated careers.

Finally, Lipscomb (2006) uses the NELS to examine the role of extracurricular activities in a broader sense. In addition to athletic participation, Lipscomb includes club involvement in his analysis. The work also differs from previous research in that it examines the short-run effects of high school involvement and uses a fixed effects approach. Lipscomb estimates the effect of changes in high school involvement on math and science test scores and on Bachelor's degree expectations. Results suggest that involvement in high school athletic participation is associated with a two percent increase in math and science test scores while club participation leads to a one percent increase in math scores. In addition, participation in either club or sports is estimated to increase Bachelor's degree expectations by five percent.

The evidence discussed above suggests that not only does schooling matter for labor market outcomes, but what an individual does while in school (i.e. the courses she takes, the sports she plays) also contributes to her outcomes. Likewise, we might expect holding a leadership position in high school to have similar effects on later life outcomes.

Leadership

Studying the time allocation decision among college students, Siebert, Davis, Litzenberg and Broder (2002) look at leadership experience specifically. They use a survey of agribusiness firms to assess the relative importance to potential employers of a variety of college experiences of its applicants. Results suggest that while students perceive high grade point averages and interview preparation to be of highest value to potential employers, the employers actually seek students with work and leadership experience. The authors conclude that "students seeking a firm's maximum starting salary offer will need to enhance academic performance with substantial work experience and leadership experience p. 222)." The study suggests that leadership experience is valued by employers and is used to select potential applicants. While it focuses on college students, it provides evidence that non-academic activities are an important determinant of labor market success and lends credence to the hypothesis at hand, namely those non-academic aspects of education such as leadership experience are also important at the high school level.

Using data from the National Longitudinal Surveys' 1966 Young Men Cohort (NLSYM), Lee and Yip (2005) examine the returns to military leadership of Vietnam generation young men. Parametric selection corrections and propensity score matching techniques are used to control for two sources of selection bias: selection into the military itself and the eventual rank achieved. Results indicate there is a positive wage return to military leadership through rank, but this premium does not come with mere veteran status. That is, it is rank, not just veteran status that leads to wage premium for veterans.

Finally, the papers to which my proposed work most closely relates are those discussed briefly in the introduction, namely Kuhn and Weinberger (2005) and Lozano (2004). Kuhn and Weinberger examine the relationship between leadership and wages using three samples of white males. The authors use data from Project TALENT, the NLS-72, and the sophomore cohort of HS&B to assess whether leadership skill is associated with higher future wages. Results indicate a wage premium ranging from 4% to 33% depending on the sample and measure of leadership used. The leadership premium appears to be greatest in managerial occupations. In addition to their primary analyses which use high school leadership as a proxy for leadership skill, Kuhn and Weinberger attempt to shed light on the issue of causality by including a school-level measure of leadership opportunities interacted with previous leadership experience. These results suggest there is a causal relationship between high school leadership and that school-level leadership opportunities predict higher wages for those individuals with prior leadership experience.

Lozano (2004) uses data from the NELS to assess whether differences in high school leadership activities can explain observed Hispanic educational gaps. Results suggest that, after controlling for demographic and school variables, there is no significant difference in leadership propensities between Hispanics and Non-Hispanics. In addition, high school leadership is estimated to increase college attendance of all demographic groups and to increase college graduation probabilities of Hispanic students whose first language is not English. The results are robust to IV estimation in which school-level leadership opportunities are used as an instrument; however, the author notes that "these results must be taken with caution, especially since they are associated with very high standard errors (p.29)."

From the perspective of my research, these papers are limited in a number of ways. First, while the papers address the questions at hand, the analyses are limited to distinct populations. Moreover, my focus diverges slightly from theirs as I intend to better understand the treatment effect of holding a high school leadership position rather than simply estimating the return to leadership skills which may or may not be fostered through the occupation of such a position.

III. A Conceptual Model of High School Leadership Experience

I hypothesize that leadership experience may translate into higher future wages through a couple of different channels. From the perspective of non-cognitive skill formation, leadership experience in high school may serve as a way of increasing one's non-cognitive skill of leadership. The experiences provided by the occupation of a high school leadership position help to increase an individual's leadership skill. Since this skill is sought out by employers and seen as a productive asset, from this perspective, taking on a leadership position in high school may be seen as investment in psychological or non-cognitive capital. In this way, leadership experience would lead to higher wages in the form of a return to a higher level of such capital across all education levels.

On the other hand, high school leadership also serves as a signal of one's leadership ability to university admission committees. In this way, individuals with innately higher leadership ability may take on leadership positions in order to separate themselves from their non-leader peers in the college admissions process. As such, we would expect the return to leadership skill to disappear after controlling for education. We would also expect to see a positive relationship between post-secondary educational attainment and high school leadership.

Below I develop a simple economic model which incorporates these two channels and illustrates how high school leadership may lead to higher future wages and educational attainment. The model is in the spirit of Barron, Ewing and Waddell (2000) who examine the effect of high school athletic participation on wages using a model of time allocation. However, whereas Barron, Ewing and Waddell examine high school athletic participation in a two-period model, I replace athletic participation with leadership and extend the model to three periods. The cost of high school leadership comes in the form of the student's foregone study and leisure time in the first period. She potentially will trade her time for leadership experience in this period, because (a) she learns something from holding a leadership position which increases her non-cognitive human capital and (b) high school experience increases her probability of attending college, both of which subsequently lead to higher income in periods two and three.

Formally, the model is defined as follows. Utility in the first period depends on leisure and leadership such that

$$U_1 = f(L, l) \tag{1}$$

where L is leisure time and l equals one if the individual chooses to be a leader[7]. Assuming utility is linear and separable, utility in period one is given by the following equation:

$$U_1 = \alpha_1 u(l) + \alpha_2 v(1 - S - l * \bar{\tau}) \tag{2}$$

where total time is normalized to one, S is time spent on academics, and $\bar{\tau}$ is an exogenously determined amount of time required by the leader of an activity (such as additional meetings with faculty advisors). The parameters α_1 and α_2 denote the marginal utility of leadership and leisure, respectively.

In periods two and three, the individual maximizes her income (Y_2, Y_3). For simplicity, hours worked are assumed exogenous in both periods. In period two, she continues her education by going to college or she goes to work full time. If she goes to college, she does not work. Rather, she incurs costs of tuition, C. Individuals who do not attend college immediately join the workforce and receive a wage which is a function of her leadership experience and her innate cognitive ability such that $wage = w(l, \gamma)$ where $w_l > 0, w_\gamma > 0$.

The probability that an individual is accepted into and attends college , π^C, depends on time spent on academics in high school, innate cognitive ability, and time spent on leadership positions such that

$$\pi^C = \pi^c(S, \gamma, l) \tag{3}$$

where $\pi^c{}_S > 0, \pi^c{}_\gamma > 0, \pi^c{}_l > 0$. As discussed by Barron, Ewing and Waddell, the parameter γ allows for the inclusion of differences in cognitive ability. The importance of the parameter arises from the possibility that cognitively higher ability students may be able to achieve a higher

[7] Note, leadership is included in the utility function to capture the fact that, for some students, serving as a leader is a fun, social, stress-free endeavor; while for other students (shy students, for instance), serving as a leader is not necessarily an enjoyable experience, per se.

level of human capital investment with less time spent studying than their lower ability counterparts.

In the third period, all individuals work. Income in this period depends on whether the individual has a college degree. Individuals with high school education continue to receive their period two wage ($[wage \mid coll = 0] = w(l,\gamma)$), while individuals with a college education receive a college wage premium, ϕ, such that $[wage \mid coll = 1] = w(l,\gamma) * \phi$ where $\phi > 1$.

Formally, income in periods two and three is defined as follows:

$$[Y_2 \mid coll = 1] = -C \tag{4}$$

$$[Y_2 \mid coll = 0] = w(l,\gamma) * \overline{H}_2 \tag{5}$$

$$[Y_3 \mid coll = 1] = w(l,\gamma) * \phi * \overline{H}_3 \tag{6}$$

$$[Y_3 \mid coll = 0] = w(l,\gamma) * \overline{H}_3 \tag{7}$$

where $coll$ equals one if the individual attended college and zero otherwise. Combining equations (2) through (7) leads to the resulting three-period maximization problem:

$$\begin{gathered}\max_{l,S} EU = \alpha_1 u(l) + \alpha_2 v(1 - S - l) + \\ \beta[\pi^c(S,l,\gamma)(-C) + (1 - \pi^c(S,l,\gamma))(w(l,\gamma) * \overline{H}_2)] + \\ \beta^2[\pi^c(S,l,\gamma)(w(l,\gamma) * \phi * \overline{H}_3) + (1 - \pi^c(S,l,\gamma))(w(l,\gamma) * \overline{H}_3)]\end{gathered} \tag{8}$$

such that $l \geq 0$, $S \geq 0$, $1 \geq S + l$, and where β is the discount factor.

Thus, high school leadership affects future wages through two channels. First, high school leadership indirectly affects wages by signaling leadership skill to admission committees and subsequently increasing the probability that an individual attends college, π^c. Additionally, occupying a leadership position in high school contributes to the accumulation of non-cognitive skills and directly increases one's wages through non-cognitive human capital investment. This suggests that for a given level of education, higher levels of leadership skill may increase one's wage. As such, we would expect to see a leadership premium even within an education group.

IV. Data

The primary data I plan on using come from the NELS. In addition, I will use the NLS-72 and HS&B to test the sensitivity of the NELS results to data drawn from alternative time periods. All of the datasets come from the National Center for Education Statistics (NCES). The earliest survey is the NLS-72. Participants in the survey were first interviewed during their senior year of high school in 1972. Subsequent interviews were conducted in 1973, 1974, 1976, 1979, and 1986. The HS&B includes individuals who were sophomores in 1980 (the sophomore cohort) and students who were seniors in 1980 (the senior cohort). Both cohorts were re-interviewed in 1982, 1984, and 1986. The sophomore class was also re-interviewed in 1992. The NELS includes individuals who were in eighth grade in 1988. The participants were re-interviewed in 1990, 1992, 1994 and 2000.

In each survey, the students, their parents, their teachers and their school counselors were interviewed. The datasets each contain a rich collection of both individual and school level characteristics. For the purposes of this research, these studies are particularly well-suited as each asks a number of questions covering a wide range of extracurricular activities. Moreover, the responses include an indicator of whether the individual was a participant, a non-participant or if he was an officer or a captain in the particular activity, thus allowing me to construct a dummy indicator for high school leadership experience. These measures are available when the students were in their senior year in the NLS-72 and HS&B. The NELS also contains indicators of eighth and tenth grade extracurricular involvement.

The preliminary analyses have been estimated using the public-use version of the NELS data. A description of all of the variables used the analyses is provided in Appendix A, Table A1. A detailed description of key variable construction is also available in Appendix A. After dropping those individuals for whom key variables are missing and students who changed schools between their sophomore and senior years of high school, the sample contains 2,310 males and 2,461 females[8].

Since we may expect the effect of leadership experience to vary for males and females, I analyze the two groups separately. As my interest is in the effect of high school leadership experience on future outcomes, it is instructive to examine the difference in summary statistics across leadership status. As such, the summary statistics are disaggregated by gender and leadership status. For males, there are 1,109 leaders and 1,189 non-leaders, while for females there are 1,272 leaders and 1,189 non-leaders. Thus, the leadership rate is slightly higher for females than males (52%, 48%)[9]. The summary statistics are presented in Tables 1 and 2 for males and females, respectively. Below I discuss some of the important observations from the tables.

First, a simple comparison of mean log wages and educational attainment supports my main hypotheses. That is, for both males and females, log wages of leaders are higher than their non-leader counterparts. Specifically, the mean log wage difference of male leaders and non-leaders is 0.122, while for females, it is 0.123. Additionally, both male and female leaders have a

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[8] I eliminate individuals who changed high school between their sophomore and senior year, because I do not separate the effect of leadership experience by these 2 years. Thus, when I apply school fixed-effects, it is necessary to have students who did not change schools. Also note, for some analyses, the sample size is reduced due to the nature of the econometric approach. For instance, in the case of 1-to-1 matching with replacement, some of the untreated cases are eliminated from the analysis.

[9] These rates seem fairly high. However, high school leadership has been defined as having been an officer or captain of any activity in either tenth or twelfth grade. Also, many activities have more than one officer or captain which may make the rate higher than one would expect. That being said, rates of 50% seem very high and could be a function of measurement error. As such, in further analyses I have restricted the measure of leadership to be elected leaders.

higher educational attainment both in terms of years of education and in the probability of being a college graduate. In particular, 51.7% of male high school leaders are college graduates while just 30.4% of non-leaders are college graduates. A similar trend is seen for females as nearly 60% of female leaders are college graduates compared to only 35.6% of non-leaders.

In addition to the differences in mean outcomes of leaders and non-leaders, there is also evidence of heterogeneity in other characteristics between the two groups. For instance, high school leadership appears to be positively correlated to test scores. Indeed, across all test score means, both the male and female high school leader samples have a higher average test score quartile than the non-leaders. High school leaders are also more likely to report that others see them as popular, athletic and important in eighth grade. Additionally, they are more internal. That is, the mean locus of control for male and female leaders is higher than the non-leaders, suggesting students who are high school leaders perceive their actions to have an impact on their outcomes.

On average, high school leaders also have different family backgrounds than the non-leaders. High school leaders come from families with higher incomes. Their parents are also more likely to be married when the students are in eighth grade and have higher educational attainment than the parents of non-leaders. Compared to male leaders, 56% percent of which have at least one parent with a college education, only 46.3% of male non-leaders have a parent with a college education. Likewise, nearly 54% of female leaders have at least one college educated parent while only 44% of non-leader females have a parent with a college degree. On average, both male and female high school leaders also have fewer older siblings than the non-leaders.

V. Empirical Approach

Since the goal is estimating the causal effect of high school leadership, the selection problem must be addressed. This problem arises because, in contrast to a pure experimental setting where treatment assignment is random, selection into a high school leadership position is not random. Students either select themselves into a leadership position or someone else elects them into such a role based on an individual's characteristics which may or may not be directly observed. As is clear from the summary statistics, there is substantial heterogeneity between leaders and non-leaders across many of the characteristics. As such, the empirical analysis must control for these differences in order to recover an effect of high school leadership which is void of selection bias.

To do so, I will first control for selection on observables parametrically using OLS both with and without school-level fixed effects. Then, I will control for selection on observables non-parametrically using PSM. Finally, I will address the possibility of unobserved heterogeneity by using IV to control for the selection on unobservable factors[10]. I discuss these methods in turn.

Approach 1: OLS

In order to compare my results with those from earlier studies, I will first employ OLS estimations to capture the effect of high school leadership on wages and educational attainment. The equations will be of the following form:

$$y_i = \alpha_0 + \alpha_1 LEAD_i + \alpha_2 X_i + \nu_i \tag{9}$$

[10] Though not discussed here, it may also be possible to control for individual-level heterogeneity using a siblings or twin fixed-effects approach. Since the HS&B contains two cohorts, it may be likely that there are a sufficient number of sibling pairs to use a sibling fixed-effects approach. The other two datasets include data from only one grade-level. As such, analysis using siblings from these datasets will require a sufficiently large set of twins who are also both in the fourth follow-up survey which is less likely.

where y_i is the outcome of interest, $LEAD_i$ is a dummy indicator of high school leadership, and X_i is a vector containing a wide range of individual, family and school characteristics[11].

Since the primary focus of the paper is on high school leadership, "hslead" is the primary treatment effect of interest. However, it may also be of interest to see if participation leads to higher wages and educational attainment. Likewise, it may be interesting to see if being in an elected leadership role has a different impact on these outcomes than holding a leadership position in general[12]. I also estimate the models using any participation (hspart) and elected leadership (hselect) as the treatment dummies.

The primary outcome of interest is "lnwage99." The total effect of leadership on wages is given by models that do not control for education while the direct effect is given by models in which "educ" is included as a control. Since wages are in log form, the coefficient on the leadership dummy variable can be interpreted as a percent change.

I will also explicitly test the effect of leadership on education by changing the outcome variable from "lnwage99" to "educ." In these models, the coefficient on the leadership dummy represents a change in the number of years of schooling due to high school leadership. In addition, a logit model will be used to test the effect of leadership on the probability of holding a college degree, "coll_grad", and on the probability of attending a four-year institution, "four_yr".

The OLS estimate of the leadership treatment effects in equation (9) is a consistent estimate of the treatment effect if $E[v_i \mid LEAD_i, X_i] = 0$. That is, provided selection into a leadership position is random for a given set of covariates, OLS will yield consistent estimates of

[11] Specifically, current analyses include all test score, demographic, family background, geographic, self-reported 8th grade characteristics and school enrollment variables as defined in Appendix A, Table A1.
[12] As aforementioned, using elected high school leadership as the treatment effect may also be useful in light of the high percentage of high school leaders in the sample.

the leadership treatment effect. In this case, the average effect of the treatment (ATE) is equivalent to the average effect of the treatment on the treated (ATT) and is given by $\hat{\alpha}_1$.

Since the data include indicators of the high school in which each individual attended, it is possible to include school fixed effects to control for the possibility of unobserved school-level heterogeneity. Thus, each OLS model will also be estimated with the inclusion of high school fixed effects.

Approach 2: PSM

An alternative to controlling for observable characteristics by OLS is to estimate treatment effects non-parametrically by use of matching estimators. Matching techniques are particularly useful when selection is based on observables and there are a rich set of variables that can be used as controls. As previously mentioned, matching is arguably an improvement over OLS because it is not constrained by the assumption that the treatment effect is linearly related to the outcome. Further, unlike OLS, by matching each treated observation with an untreated counterpart, the researcher can explicitly test whether there is sufficient overlap between the two groups. That is, matching ensures that for every set of characteristics, X , there exists both a treated and non-treated case. Unlike OLS, matching explicitly avoids extrapolation into areas of the causal effect distribution that are not on the common support.

The treatment effect of interest is the average effect of treatment on the treated (ATT). The ATT in this context is defined as follows:

$$ATT = E[y_1 - y_0 \mid LEAD = 1] = E[y_1 \mid LEAD = 1] - E[y_0 \mid LEAD = 1] \tag{10}$$

where y_1 is the outcome of interest in the treated state and y_0 is the outcome of interest in the untreated state. The problem which naturally arises in this context is that the counterfactual is not observed. That is, we only observe the outcome for the treated in the treated state. Subsequently, the counterfactual, y_0, must be constructed. The basic idea underlying the matching methodology can then be described as follows. For each treated individual, find an untreated individual, or group of untreated individuals, who are observationally equivalent across a number of covariates, X_i. Then, the observed outcome of these individuals can be used as the counterfactual for the treated individual. The justification for using the matched outcome as the counterfactual follows from the *conditional independence assumption* (CIA) which states that conditional on the observed covariates, X_i, the outcome of the non-treated individual is independent of the treatment. Formally, the CIA states that $(y_1, y_0) \perp LEAD_i \mid X_i$.

When there are a large number of observable characteristics, it becomes increasingly difficult to find an exact match for each treated individual. This problem, commonly known as the dimensionality problem, is addressed through the use of matching on the propensity score. The propensity score is defined as the probability of treatment conditional on observed characteristics, X_i. Formally, the propensity score is defined as $p(X_i) = pr(LEAD_i = 1 \mid X_i)$. Rosenbaum and Rubin (1983) show that if CIA holds such that y_0 is independent of $LEAD_i$ given the covariates X_i, then it is also independent of the propensity score. That is, if $(y_1, y_0) \perp LEAD_i \mid X_i$, then $(y_1, y_0) \perp LEAD_i \mid p(X_i)$. As such, rather than using exact matching, matching can be done on the propensity score.

PSM is implemented by first estimating the leadership selection equation by a probit or logit model. Then, an estimator of the treatment effect is constructed by matching the treated individuals to a non-treated individual or group of non-treated individuals based on their propensity score. There are a number of ways in which PSM estimators may be constructed. In general, they are of the following form:

$$ATT = 1/N_T \sum_{i \in T} [y_i - \sum_{j \in C_i} w(i,j) y_j] \tag{11}$$

where N_T represents the number of treated individuals, C_i is the set of control individuals for each treated individual i, and $w(i,j)$ is some weighting function that depends on the choice of matching estimator.

Perhaps the simplest PSM technique is the nearest neighbor (NN) estimator. In the NN, each observation is matched to the non-treated individual (or K individuals) with the most similar propensity score. Formally, the set of control individuals is defined as $C_i = Min_k \parallel p_i(X_i) - p_k(X_k) \parallel$. NN can either be 1-to-1 or 1-to-K. In the cases of 1-to-1 matching, equation (11) becomes

$$ATT = 1/N_T \sum_{i \in T} [y_i - y_j]. \tag{12}$$

One issue that arises in the case of 1-to-1 matching is whether to match with or without replacement. Matching without replacement means that each control observation is matched to one and only one treated individual. In contrast, if matching is done with replacement, each control may be assigned to more than one treated individual. The decision between the two methods represents a tradeoff between bias and variance. That is, matching without replacement increases the number of controls used in the analysis and subsequently decreases variance; however, if these matches represent inferior matches, the method increases bias. 1-to-K NN matching is always done with replacement and the ATT is given by

$$ATT = 1/N_T \sum_{i \in T} [y_i - 1/K \sum_{k \in C_i} y_k]. \tag{13}$$

Closely related to NN, caliper matching uses all of the matches within a given distance of the propensity score of the treated unit. That is, the number of matches, K, varies by treated individual such that $ATT = 1/N_T \sum_{i \in T}[y_i - 1/K_i \sum_{k_i \in C_i} y_{k_i}]$.

An alternative matching estimator is the kernel matching estimator. Kernel matching uses all control individuals within the area of common support. In contrast to NN or caliper matching in which each control unit is assigned an equal weight, in kernel matching each control observation is given a different weight defined by the specified kernel. That is, control individuals with the closest propensity score are given relatively large weights while little weight is attributed to those furthest away. Other matching estimators include radius, local linear regression, and mahalanobis matching, among others.

In general, there is not yet a consensus as to which method results in the best PSM estimator. Thus, I propose using a few of the methods described above, namely 1-to-1 NN with replacement, caliper matching, and kernel matching. The outcomes of interest will be defined exactly as in the OLS specifications. In addition, covariates used in the analyses will parallel those included in the OLS specifications. Finally, since the estimates will rely on estimated propensity scores, I will use the bootstrap method to estimate the standard errors of the PSM estimates.

PSM estimates will result in consistent estimates of the high school leadership treatment effect provided that a) there are no unobservable factors that affect selection into a leadership position and the outcome after conditioning on X_i or b) any existing unobservable factors have the same impact on the treated and untreated individuals within a given stratum or matched pair. However, if the above assumptions do not hold, a hidden bias will still exist and the PSM estimates will not provide consistent estimates of the high school leadership treatment effect.

While PSM methods can not address endogeneity by directly controlling for unobservable factors, it is possible to estimate the extent to which an unobserved factor, u, would need to affect the leadership propensity in order to alter the qualitative or quantitative inferences about the effects of high school leadership as estimated with PSM. This method, the Rosenbaum Bounds (RB) approach, is used by DiPrete and Gangl (2004). The authors implement RB to assess the possible impact of unobservable factors on PSM estimates of the effect of unemployment insurance on the timing of reemployment, the post-unemployment wage and the probability of relocation[13].

The method, as discussed by Diprete and Gangl (and developed by Rosenbaum (2002)) essentially assumes the potential existence of a hidden confounding variable, u, which conditional on X, affects the probability of selection into treatment. Then, one can construct bounds on the significance level of a test statistic, or alternatively, may construct bounds on the estimated coefficients for a given postulated impact of u on the odds of treatment. This method is discussed at length in Rosenbaum (2002), Chapter 4. DiPrete and Gangl also provide a detailed summary in Appendix A of their paper which follows directly from Rosenbaum (2002), Chapter 4. For reference, a copy of their Appendix A is included in this proposal in Appendix B.

Approach 3: IV

As aforementioned, an important limitation of both OLS and PSM is that both techniques control for selection on observable characteristics only. As such, if it is the case that unobserved traits both lead a high school student to pursue leadership positions and to acquire additional education or to have higher future wages, OLS and PSM estimation strategies will not result in consistent estimates of the treatment effect.

[13] Specifically, the authors implement the method using matched pairs. They also indicate the availability of a STATA ado file that may be used to empirically apply the method.

One natural way to address this issue explicitly is to use an IV procedure where the leadership propensity is estimated in the first stage from a selection equation and this predicted probability of leadership replaces the leadership variable in the main outcome equation. In a general sense, such a model is defined as follows:

$$\text{Leadership Selection: } d_i^* = \lambda_1 z_i + u_i \text{ such that } d_i = 1[d_i^* > 0] \tag{14}$$

$$\text{Main Outcome Equation: } y_i = \beta_0 + \beta_1 \hat{d}_i + \beta_2 X_i + \varepsilon_i \tag{15}$$

In the context of treatment effects, the IV method provides an estimate of the ATE or ATT under the restrictive assumption that the treatment effect is constant within the population. Under this assumption, the ATE is equivalent to the ATT and can be directly compared to the OLS and PSM estimates. Under the more realistic case in which the treatment effect is not constant and under additional assumptions[14], Angrist, Imbens and Rubins (1996) show that IV estimation provides an estimate of the local average treatment effect (LATE). The LATE is the average effect of the treatment for those students who, due to a change in the value of the IV, are induced to select themselves into a high school leadership position.

As previously mentioned, IV requires at least one variable which is related to the leadership decision, but is directly unrelated to education or wages. Admittedly, finding such a variable in this instance presents an arduous task. However, like Kuhn and Weinberger and Lozano, I am able to construct various measures of school-level leadership opportunities (SLO).

[14] These assumptions are described by DiPrete and Gangl in their footnote (5) and are as follows: (1) stable unit treatment values, (2) random assignment to treatment, (3) valid exclusion restriction, (4) nonzero causal effect of the IV on treatment status and (5) monotonicity.

This methodology is similar to that used by Altonji (1995) who, rather than using individual-level course choice, uses the school average course choice[15].

For my purposes I can construct a couple of different measures of SLO. For instance, like Lozano (2006), I can use the average school-level leadership propensity within my sample. Since I separate my analyses by sex, I will use the sex-specific, school-level mean leadership propensity. Importantly, while my NELS sample includes only individuals who were included in the fourth follow-up survey, I am able to construct the SLO measure from a much larger sample available in the earlier follow-up surveys. In addition, with the NELS data, I have counselor-provided information on the number of extracurricular activities available as well as school enrollment size. As such, I can construct a measure of the number of available extracurricular activities per student.

While these measures should both be able to capture SLO, they may differ if, for instance, a small number of students undertake numerous available leadership positions. Alternatively, in many activities, there is more than one leadership position available. For instance, student council, National Honor Society, and many other clubs have a president, a vice president, a secretary, and a treasurer. Consequently, the number of extracurricular activities per student would tend to understate the true number of SLO available to the students. It is also not possible to separate the number of available extracurricular activities by sex. As such, I will not be able to create a sex-specific IV using the number of available extracurricular activities[16].

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[15] Altonji argues that use of such a measure will be a valid instrument provided that (1) "all schools provide a menu of courses of various types, quality and difficulty to students but differ in the inducements that they provide for students to take particular sets of courses" and (2) "these differences are unrelated to student characteristics and other unobserved school characteristics (p. 414)."

[16] Additional IVs may be available once the restricted data are used. For instance, Dhuey and Lipscomb (2006), use variation in school-age cutoffs to create exogenous variation in within grade age and find that relatively older students are four to eleven percent more likely to become class leaders than their younger peers. While this admittedly will not provide a valid IV with respect to education, it may provide an additional IV with respect to wages.

The consistency of the IV estimates will rely on the validity of the IV. In order for an IV to be valid, it must satisfy two criteria, namely that (1) the IV is significantly correlated to high school leadership and (2) it is related to the outcome only through its effect on leadership. In the initial analyses, I use the sex-specific, school-level average leadership propensity as the IV. To test whether the first assumption holds, I run a gender-specific reduced form regression of high school leadership on the IV. Indeed, as hypothesized, the coefficient on the IV is positive and statistically significant in both regressions[17]. Since in the current analyses, the model is exactly identified, I am unable to test the validity of the second assumption. However, in additional analyses, I will use the number of available extracurricular activities per student and, once the restricted data is available, I will search for additional variables with the hope that I will be able to test the existence of valid exclusion restrictions with an over-identifying restrictions test.

VI. Preliminary Results

As is evident from previous sections, the proposed work includes three datasets, various outcomes, different treatment measures and a variety of econometric methods. However, in the interest of brevity, for the purpose of this proposal I have estimated the effects using only the primary data, the NELS[18]. Additionally, while many of the proposed analyses have been estimated, I limit my discussion to the primary treatment effects of interest, namely the estimated effect of high school leadership ("hslead") on log wages, years of education, and the probability of holding a college degree. Reported results are those estimated by OLS with and without FE, 1-

[17] Specifically, the coefficient on the IV in the male regression is .936 with a corresponding standard error of .045 while that of females is .945 with a standard error of .045.
[18] Of the three datasets, I believe the NELS data to be the best of the three datasets for my purposes as it contains leadership information for three years of school as opposed to only one available in the HS&B and NLS-72. Furthermore, it represents the most recent data and the data was not used by Kuhn and Weinberger.

to-1 NN (with replacement) PSM, and IV. Results from supplementary analyses which have been estimated but are not discussed in detail here can be found in Appendix C[19].

Estimates of the high school leadership effect on log wages are given in Table 3. Results for males are reported in panel A while those for females are in panel B. The first row in each panel gives the results from analyses in which education is not included as a control (the "full" effect) while estimates in the second row include education (the "direct" effect). In the case of the direct effect, coefficients on both high school leadership and education are reported.

Initial estimates suggest that male high school leaders do earn more than their non-leader counterparts. With each econometric method, the estimates are statistically significant and range from a wage premium of 7.6% (OLS) to 11.9% (FE)[20]. Once education is included as a control, the estimates drop slightly in magnitude, but with the exception of the IV estimate, they remain statistically significant. Direct wage effects range from 6.1% to 10.8%, representing a drop in magnitude of roughly 1.5 percentage points. With the exception of the IV estimates, the estimated wage effects of high school leadership are generally smaller for woman and range from 6.2% to 12.2%. As with the male sample, once education is included as a control, the estimated coefficients drop in magnitude; however, in contrast to males, only the NN PSM and IV estimates remain statistically significant at conventional levels. Additionally, while the male estimates fall by only 1.5 percentage points, the coefficients on female effects drop by a larger magnitude. For instance, the OLS estimate falls from 6.2% to 3.6% and is no longer statistically significant at conventional levels. It is also interesting to note that while the estimate of the

[19] These include estimates from other matching techniques and parallel analyses using "hspart" and "hselect" as the treatment effect of interest. In addition, the education models have also been estimated using the probability of college attendance ("four_yr") as the education outcome of interest.
[20] PSM estimates using caliper and kernel matching techniques are also within this range and are reported in Appendix C, table C1.

return to an additional year of education is roughly 7% for females, the corresponding return for males ranges from just 2.8% to 4.5%.

Interestingly, of the four reported econometric techniques, in both the male and female analyses, the OLS estimates provide the lower bound of the range and the IV estimates are larger than the OLS. Following the common argument drawn from the education literature in which one typically assumes that the unobserved heterogeneity is indicative of the so-called “ability bias”, we would generally presume that the OLS estimates would be biased upwards. Following this logic, IV estimates which correctly control for such bias should be smaller than those derived from OLS. Yet, I find just the opposite. Lozano (2006) finds similar results using school-level leadership opportunities as the IV and suggests that using an average leadership propensity may be correcting for measurement error in the individual-level leadership variable. This explanation would be consistent with larger IV results since measurement error would tend to bias the OLS estimates downward.

From an economic standpoint, the magnitude of these effects is quite large. The estimated leadership effect on the wage of males, for example, is actually greater than the corresponding return to education across all econometric approaches. It is also worth noting that the estimates are in the range reported by Kuhn and Weinberger who estimate a return to leadership skill ranging from 4 to 33%. In addition, the fall in magnitude of the coefficients once education is included as a control suggests that there is indeed an indirect effect of high school leadership through increased educational attainment. However, for males, the majority of the high school leadership effect persists for a given level of education, while for females; much of the estimated effect is eliminated once education is included as control. This suggests that the education effects of high school leadership play a comparatively larger role for females than males[21].

To explicitly test the role of high school leadership on future educational attainment, the next set of results uses the number of years of education and the probability of a college degree as the outcomes of interest. Education results are reported in Table 4. As in Table 3, the top panel contains results for males while results for females are given in the lower panel. Results from varying econometric approaches are reported across the columns.

Similar to the wage results, the education estimates are consistent with my hypothesis, suggesting that high school leadership is positively related to years of education and the probability of holding a college degree. Indeed, results for males and females with respect to both outcomes are positive and statistically significant at the 1% level across all econometric approaches. In contrast to wages, with the exception of the NN PSM estimates[22], the effect of high school leadership on the probability of holding a college degree appears to be larger for females than for males. High school leadership is predicted to increase the probability of college graduation of females by 11.7% (NN PSM) to 16.9% (IV), whereas for males, the comparable range is between 7.7 % (IV) and 13.8% (NN PSM)[23]. The gender patterns are not as clearly

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[21] With the exception of IV estimates, results using elected high school leadership as the treatment effect are slightly greater in magnitude than those reported using “hslead” as the treatment. They qualitatively mirror these results, indicating wage effects are greater for men and, compared to women, fall less in magnitude once education is included as control (see Table C3). Results using participation are less clear. Few coefficients are statistically significant. In all cases, the estimated coefficients are greater than those estimated using “hslead” and “hselect”, however, in some cases the effect is greater for males while in others, it is greater for females (see Table C2)

[22] Note all PSM estimates are statistically significant at the 1% level. Interestingly, in contrast to results derived from alternative econometric approaches, they suggest the effect of high school leadership on the probability of holding a college degree is greater for males than females. See Table C4 for details.

[23] Note that participation results on the probability of holding a college degree are greater than the leadership results and, in contrast to these results, indicate the effect is larger for males (see Table C5). The coefficients on elected leadership are also generally higher than those estimated using “hslead”. Results from the logit and IV models are consistent with the leadership results and suggest that elected leadership has a greater impact on educational attainment of females. In contrast, results from the FE and NN PSM specifications suggest the effect is larger for males. In each case, the estimates only differ by roughly 1% (see Table C5).

defined when years of education is used as the outcome of interest. Coefficients from OLS and IV estimates are larger for females while FE and NN PSM estimates are greater for males[24].

While, compared to males, estimates suggest that the impact of high school leadership on the educational attainment of females is larger than that of their male counterparts; this difference is relatively small and is not large enough to fully explain the drop in the effect of leadership on wage seen in Table 3. Rather, the gender difference in the wage effect drop appears to be largely drawn from the 3.5% gender difference in the estimated return to education[25].

While the differences in the impact of high school leadership on educational attainment across gender appear to be small, the fact remains that, for females, leadership plays a much larger role in terms of educational attainment than it does for their future wage. In contrast, for males, much of the estimated leadership effect on wage remains after controlling for education. A comparison of the results across gender thus begs the question of *why,* compared to males, the wage effects would be smaller for women. One possible explanation which is worthy of further investigation comes from previous work by Kuhn and Weinberger which finds that leadership skill is rewarded more in managerial occupations. As such, the estimated larger wage returns for males may be due to the fact that males are occupying the majority of managerial positions. This

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[24] Note, participation results using "educ" at the outcome are greater than the leadership results and indicate the effect is larger for males (see Table C5). In contrast, the elected high school leadership estimates are smaller than the general leadership results. Similar to the leadership results, the gender pattern is not clearly defined (see Table C6). In equations where the probability of college attendance is the outcome of interest, logit and FE estimates are larger for females, while NN PSM and IV estimates are larger for males (see Table C7).

[25] Consider the OLS estimates of high school leadership on years of education (Table 4), for example. High school leadership is predicted to increase educational attainment of males by 0.336 years and is associated with a 0.371 year increase in the education of females. Given the predicted return to education reported in Table 3, for males, the leadership effect falls by 0.045*0.336, or 0.015, while the comparable drop for females is 0.070*0.371, or 0.026. If we were to replace the female return to education with that of the males, the corresponding drop would be 0.017 which is quite close to that of the men, 0.015. Alternatively, if we were to replace the female leadership effect on education with that of the male, the drop would be 0.024 which is still quite close to the female drop. Clearly, the differences are driven by the difference in the return to education rather than the educational return to leadership.

is interesting and deserving of further attention. In future analyses, I will explore this potential difference by examining differences across leadership effects within a given occupation.

VII. Conclusion

Leadership skill is of substantive value to both employers and academic institutions and some research suggests that such skill may be fostered or signaled through leadership experience while in high school. Interestingly, few economic studies have examined the role of such experience in determining future labor market outcomes. Moreover, in the limited research that exists, the studies have been limited to distinct sub-populations and have focused on OLS specifications and results.

My dissertation work seeks to fill the gaps in the limited literature by using three different datasets to assess the impact of high school leadership on future wages and educational attainment of both males and females. It will address the issue of causality by employing three econometric approaches, namely OLS, PSM, and IV. Clearly, each method is based on a different set of assumptions. While the only method that can explicitly control for endogeneity is IV, I believe much can be learned by comparing estimates across differing methods. As noted by DiPrete and Gangl, "because the approaches rely on different information and different assumptions, they provide complementary information about the causal relationships (abstract)." Moreover, work by Heckman and various co-authors has shown that PSM may lead to better estimates than IV provided there are a rich set of covariates and the outcomes are measured in the same way.

Preliminary results using data from the public-use NELS suggests that high school leadership has a statistically significant positive effect on log wages, years of education and the probability of holding a college degree of both genders. Results are robust to various econometric approaches suggesting that there is indeed a causal relationship between high school leadership, wages and educational attainment. Interestingly, these effects differ by gender in their magnitude and suggest that high school leadership plays a more important role in terms of educational attainment for females while the majority of the wage effect persists beyond education for males. Certainly the preliminary results are promising and provide evidence that lends credence to my main two hypotheses, namely (1) high school leadership leads to future wage premiums and (2) high school leadership increases educational attainment. Moreover, the apparent differences across gender offer an interesting avenue of additional research which has not yet been exploited.

To conclude, in addition to the work I have done thus far, which will hopefully form the basis for a first chapter of the dissertation, I specifically foresee my dissertation progressing as follows. First, given the preliminary results, I believe an analysis which exploits the apparent gender differences and attempts to shed light on why leadership experiences impacts males differently than females is certainly warranted. As such, my immediate focus will be on using the NELS data to carefully explore these differences with the hopes that the research will turn into my job-market paper and will also form a second chapter of the dissertation. Then, in the last chapter of the dissertation, I will use the NLS-72 and HS&B datasets test the sensitivity of the NELS estimates to data drawn from alternative time periods.

Table 1. Summary Statistics from NELS88 Sample: Males, Leaders vs. Non-Leaders

	High School Leaders		Non-Leaders	
	Mean	Std. Deviation	Mean	Std. Deviation
Activities:				
hslead	1.000	0.000	0.000	0.000
hselect	0.356	0.479	0.000	0.000
hspart	1.000	0.000	0.876	0.330
by_lead1	0.188	0.391	0.083	0.276
by_part	0.951	0.215	0.852	0.355
Labor Outcomes:				
f4hi99	34933.05	26894.38	30460.87	17874.68
f4blhpw	45.17	11.95	44.38	10.64
f4bwkswk	48.41	8.12	48.61	8.19
hrs99	2207.24	717.06	2174.27	648.42
wage99	16.681	13.274	15.346	34.131
lnwage99	2.667	0.502	2.545	0.494
Education:				
educ	14.974	1.466	14.270	1.521
hs_drop	0.002	0.042	0.010	0.099
hs_deg	0.082	0.275	0.175	0.380
some_pse	0.287	0.452	0.356	0.479
asscer	0.113	0.316	0.156	0.363
bach_deg	0.461	0.499	0.278	0.448
grad_sch	0.056	0.230	0.026	0.159
coll_grad	0.517	0.500	0.304	0.460
four_yr	0.720	0.449	0.495	0.500
Test Scores:				
f1_readq	3.002	1.329	2.794	1.402
f1_sciq	3.280	1.298	3.033	1.359
f1_mathq	3.236	1.247	2.943	1.343
f1_histq	3.250	1.388	2.943	1.416
f2_readq	2.861	1.082	2.639	1.087
f2_sciq	3.142	0.991	2.912	1.040
f2_mathq	3.145	0.993	2.819	1.054
f2_histq	3.075	1.046	2.848	1.058
Demographic:				
hispanic	0.085	0.279	0.113	0.317
black	0.065	0.247	0.051	0.220
white	0.783	0.413	0.760	0.427
age	26.337	0.531	26.347	0.554
Family Background:				
by_faminc	10.499	2.254	9.908	2.238
by_parmar	0.866	0.341	0.834	0.372
by_par_hs	0.410	0.492	0.475	0.500
by_par_coll	0.560	0.497	0.463	0.499
by_num_oldsib	1.149	1.360	1.237	1.467
Geographic:				
by_urban	0.227	0.419	0.237	0.426
by_suburban	0.427	0.495	0.433	0.496
by_rural	0.346	0.476	0.330	0.470
by_south	0.322	0.467	0.346	0.476
Self-Reported 8th grade Characteristics:				
by_popular	0.211	0.408	0.126	0.332
by_athletic	0.436	0.496	0.237	0.426
by_important	0.234	0.423	0.151	0.358
by_locus1	0.173	0.646	0.039	0.669
by_locus2	0.170	0.569	0.040	0.564
School Characteristics:				
f1_10enroll	259.310	196.195	322.773	201.077
Number of Observations	1109		1201	

Table 2. Summary Statistics from NELS88 Sample: Females, Leaders vs. Non-Leaders

	High School Leaders		Non-Leaders	
	Mean	Std. Deviation	Mean	Std. Deviation
Activities:				
hslead	1.000	0.000	0.000	0.000
hselect	0.423	0.494	0.000	0.000
hspart	1.000	0.000	0.899	0.301
by_lead1	0.211	0.409	0.104	0.306
by_part	0.972	0.166	0.895	0.307
Labor Outcomes:				
f4hi99	26437.51	19338.5	22393.28	11504.89
f4blhpw	41.17	9.78	39.91	10.17
f4bwkswk	47.24	9.32	47.60	9.29
hrs99	1967.53	631.57	1922.61	641.83
wage99	14.170	15.505	12.588	12.808
lnwage99	2.505	0.484	2.382	0.498
Education:				
educ	15.242	1.430	14.471	1.504
hs_drop	0.001	0.028	0.004	0.065
hs_deg	0.055	0.228	0.146	0.354
some_pse	0.222	0.416	0.294	0.456
asscer	0.124	0.330	0.198	0.399
bach_deg	0.517	0.500	0.323	0.468
grad_sch	0.080	0.272	0.034	0.180
coll_grad	0.597	0.491	0.357	0.479
four_yr	0.742	0.438	0.495	0.500
Test Scores:				
f1_readq	3.156	1.164	2.857	1.380
f1_sciq	2.992	1.274	2.640	1.427
f1_mathq	3.175	1.159	2.807	1.378
f1_histq	3.064	1.264	2.764	1.466
f2_readq	3.087	0.951	2.725	1.020
f2_sciq	2.807	1.025	2.462	1.043
f2_mathq	3.041	0.953	2.612	1.038
f2_histq	2.892	1.013	2.544	1.041
Demographic:				
hispanic	0.082	0.274	0.120	0.325
black	0.082	0.274	0.084	0.278
white	0.758	0.429	0.728	0.445
age	26.195	0.440	26.248	0.477
Family Background:				
by_faminc	10.356	2.295	9.595	2.512
by_parmar	0.861	0.346	0.804	0.397
by_par_hs	0.421	0.494	0.477	0.500
by_par_coll	0.539	0.499	0.441	0.497
by_num_oldsib	1.116	1.369	1.214	1.392
Geographic:				
by_urban	0.213	0.410	0.243	0.429
by_suburban	0.419	0.494	0.420	0.494
by_rural	0.368	0.482	0.337	0.473
by_south	0.352	0.478	0.315	0.465
Self-Reported 8th grade Characteristics:				
by_popular	0.198	0.399	0.112	0.315
by_athletic	0.267	0.442	0.146	0.353
by_important	0.227	0.419	0.176	0.381
by_locus1	0.192	0.666	0.063	0.668
by_locus2	0.156	0.574	0.010	0.577
School Characteristics:				
10th grade enrollment	265.448	195.492	317.073	193.868
Number of Observations	1272		1189	

Table 3. Estimates of the Effect of High School Leadership on Log Wages

	Econometric Method			
A. Males	OLS	School FE	NN PSM	IV
Log Wage Full Effect:				
High School Leadership	0.076 ***	0.119 ***	0.106 **	0.100 *
	(0.022)	(0.027)	(0.031)	(0.052)
Number of Observations	*2310*	*2052*	*1675*	*2256*
Log Wage Direct Effect:				
High School Leadership	0.061 **	0.108 ***	0.089 **	0.085
	(0.022)	(0.027)	(0.032)	(0.053)
Education	0.045 ***	0.028 **	N/A	0.045 ***
	(0.008)	(0.010)		(0.009)
Number of Observations	*2310*	*2052*	*1658*	*2256*
B. Females	OLS	School FE	NN PSM	IV
Log Wage Full Effect:				
High School Leadership	0.062 **	0.062 ***	0.081 **	0.122 **
	(0.020)	(0.026)	(0.031)	(0.051)
Number of Observations	*2461*	*2200*	*1866*	*2388*
Log Wage Direct Effect:				
High School Leadership	0.036	0.036	0.051 *	0.089 ***
	(0.020)	(0.026)	(0.030)	(0.051)
Education	0.070 ***	0.073 ***	N/A	0.070 ***
	(0.008)	(0.009)		(0.008)
Number of Observations	*2461*	*2200*	*1846*	*2388*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.
Full Log Wage estimations do not control for education while direct log wage regressions include years of education.

Table 4. Estimates of the Effect of High School Leadership on Educational Attainment

	Econometric Method				
A. Males	OLS	Logit	School FE	NN PSM	IV
Years of Education	0.336 ***	N/A	0.374 ***	0.433 ***	0.346 **
	(0.055)		(0.069)	(0.097)	(0.135)
Number of Observations	*2310*		*2052*	*1675*	*2256*
Probability of College Degree		0.118 ***	0.114 ***	0.138 ***	0.077 *
		(0.024)	(0.023)	(0.031)	(0.044)
Number of Observations		*2310*	*2052*	*1675*	*2256*
B. Females	OLS	Logit	School FE	NN PSM	IV
Years of Education	0.371 ***	N/A	0.360 ***	0.338 ***	0.461 ***
	(0.054)		(0.068)	(0.092)	(0.134)
Number of Observations	*2461*		*2200*	*1866*	*2388*
Probability of College Degree		0.156 ***	0.121 ***	0.117 ***	0.169 ***
		(0.024)	(0.023)	(0.031)	(0.045)
Number of Observations		*2461*	*2200*	*1866*	*2388*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.

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Appendix A. Variable Construction and Definitions

The primary treatment effect of interest is given by the dummy variable “hslead” which denotes high school leadership. It is set equal to one if the individual indicates that she is an officer or captain of any school activity in the tenth or twelfth grade or if she indicates that she is an elected class officer in either grade. The variable “by_lead1” is a parallel dummy for eighth grade leadership experience. I also construct a dummy variable “hselect” which is set equal to one if the individual was an elected class officer in tenth or twelfth grade. Participation dummy variables are given by “hspart” and “by_part”, respectively.

The key dependent variable is log wage in 1999, “lnwage99”. This wage represents a student’s wage seven years after high school graduation. It is constructed by taking the log of “wage99” which is, in turn, derived by dividing total annual income from employment in 1999, “f4hi99”, by total hours worked, “hrs99”. Total hours worked in 1999 is calculated as the product of total number of weeks worked in 1999, “f4bwkswk”, and typical hours worked per week from all jobs in 1999, “f4blhpw.” Individuals who reported having no earnings or hours worked in 1999 are eliminated from the analysis. Further, individuals with calculated wages that are less than $2.00 are also eliminated.

Education variables are constructed from various questions regarding an individual’s highest level of education as of the fourth follow-up survey (f4hsdipl, f4attpse, and f4hhdg). The dummy variables “hs_drop”, “hs_deg”, “some_pse”, “asscer”, “bach_deg”, and grad_sch correspond to the following levels of education, respectively: high school dropout, high school degree, some post-secondary education but no higher degree, associates degree, bachelor’s degree and graduate school. I also create a dummy indicator for college graduates which is set equal to one if the individual has a bachelor’s degree or higher, “coll_grad.” Educational attainment in terms of total years is then created from these dummy variables as follows. High school dropouts are allocated 10 years of education, individuals with a high school degree are assigned 12 years of education, and individuals with some post-secondary education (regardless of associates degree or not) are allocated 14 years of education. Bachelor's degree recipients are assigned 16 years of education and individuals who attended graduate school are assigned 18 years of education. Individuals with missing educational attainment values are dropped from the sample.

Control variables include a variety of family background and individual demographic indicators such as family income, parents' marital status and education, number of older siblings, age and race. I also include a number of standardized test score variables, geographic controls and eighth grade self-reported student characteristics. A full list of the variables used in the analyses is given in Table A1 below.

Table A1. Variable Definitions (NELS88)

Variable	*Description*
Activities:	
hslead	Equal to 1 if r was a leader in any school activity in 10th or 12th grade
hselect	Equal to 1 if r was an elected class officer in 10th or 12th grade
hspart	Equal to 1 if r was a participant in any school activity in 10th or 12th grade
by_lead1	Equal to 1 if r was a leader in any school activity in the 8th grade
by_part	Equal to 1 if r was a participant in any school activity in the 8th grade
Labor Outcomes:	
f4hi99	Annual income from employment in 1999
f4blhpw	Typicial hours worked per week in 1999
f4bwkswk	Total number of weeks worked in 1999
hrs99	Annual hours worked in 1999 (equal to f4blhpw*f4bwkswk)
wage99	Hourly wage in 1999 (equal to f4hi99/hrs99)
lnwage99	Log of wage99
Education:	
Educ	Number of years of education completed by 4th follow-up
hs_drop	Equal to 1 if r is a high school dropout
hs_deg	Equal to 1 if r is a high school graduate but has no post-secondary education
some_pse	Equal to 1 if r has some pse education, but no ps degree
asscer	Equal to 1 if r has an associates degree
bach_deg	Equal to 1 if r has bachelor's degree but no graduate education
grad_sch	Equal to 1 if r has some graduate education (masters or Phd)
coll_grad	Equal to 1 if r is a college graduate (may or may not have grad school)
four_yr	Equal to 1 if r attended a four year institution
Test Scores:	
f1_readq	R's 10th grade reading score quantile (from 1 to 4)
f1_sciq	R's 10th grade science score quantile (from 1 to 4)
f1_mathq	R's 10th grade math score quantile (from 1 to 4)
f1_histq	R's 10th grade history score quantile (from 1 to 4)
f2_readq	R's 12th grade reading score quantile (from 1 to 4)
f2_sciq	R's 12th grade science score quantile (from 1 to 4)
f2_mathq	R's 12th grade math score quantile (from 1 to 4)
f2_histq	R's 12th grade history score quantile (from 1 to 4)
Demographic:	
male	Equal to 1 if r is a male
hispanic	Equal to 1 if r is hispanic
black	Equal to 1 if r is black
white	Equal to 1 if r is white
age	R's age in years
Family Background:	
by_faminc	Index of family income in 1988 when r is in 8th grade (ranges from 1 to 15)
by_parmar	Equal to 1 if r's parents are married when r is in 8th grade
by_par_hs	Equal to 1 if either parent has a high school degree but neither has a college degree
by_par_coll	Equal to 1 if either of r's parents has a college degree
by_num_oldsib	Number of r's older siblings when r is in 8th grade
by_num_sibs	Number of r's siblings when r is in 8th grade
Geographic:	
by_urban	Equal to 1 if r's school is in an urban area in 8th grade
by_suburban	Equal to 1 if r's school is a suburban area in 8th grade
by_rural	Equal to 1 if r's school is in a rural area in 8th grade
by_south	Equal to 1 if r lives in the South in 8th grade
Self-Reported 8th grade Characteristics:	
by_popular	Equal to 1 if r indicated that others see him as very popular in 8th grade
by_athletic	Equal to 1 if r indicates that others see him as very athletic in 8th grade
by_important	Equal to 1 if r indicates that others see him as very important in 8th grade
by_locus1	8th grade locus of control composite designed to be comparable to HS&B and NLS72
by_locus2	8th grade locus of control composite constructed from various questions ranges from low to high loc
School Characteristics:	
f1_10enroll	10th grade enrollment
f1sch_id	High school id

Appendix B: DiPrete & Gangl (2004) Appendix A[26]

Appendix A: The Rosenbaum Bounds Method

We recapitulate the Rosenbaum bounds method (Rosenbaum 2002) of sensitivity analysis for the estimation of treatment effects using data on matched pairs. Rosenbaum developed this approach to assess the impact of hidden bias on the computation of test statistics from the family of sign-score statistics, which are nonparametric tests that include Wilcoxon's signed rank test and McNemar's test. While Rosenbaum developed the theory for a more general case, we limit the discussion here to the case of matched pairs, which corresponds to the situation when propensity score analysis is used.

Test statistics in the family of sign score statistics have the form

$$T = t(Z, r) = \sum_{s=1}^{S} d_s \sum_{i=1}^{2} c_{si} Z_{si} \qquad (11)$$

where Z is the variable that registers which of each of the s pairs was treated, and r measures the outcome for each case in the S pairs. Z_{si} equals 1 if a case is treated, and 0 otherwise. "c" is defined as follows:

$$\begin{aligned} c_{s1} &= 1, c_{s2} = 0 \quad if \quad r_{s1} > r_{s2} \\ c_{s1} &= 0, c_{s2} = 1 \quad if \quad r_{s1} < r_{s2} \\ c_{s1} &= 0, c_{s2} = 1 \quad if \quad r_{s1} = r_{s2} \end{aligned}$$

Finally, d_s is the rank of $|r_{s1} - r_{s2}|$ with average ranks used for ties. Essentially, the product of the c and Z variables cause pairs to be selected where the outcome for the treatment was greater than the outcome for the control. The ranks of these cases are summed and compared to the distribution of the test statistic under the null hypothesis that the treatment has no effect.

In the case where the assignment to treatment is not random, the above test statistic can be bounded. Rosenbaum proposes that one assume that there is an unmeasured variable U that

[26] Note, this is the appendix from a working paper version of the DiPrete & Gangl (2004) paper.

affects the probability of receiving the treatment. If we let π_i be the probability that the ith unit receives the treatment, and X are the observed covariates that determine treatment and that also determine the outcome variable, then the following treatment assignment equation applies

$$\log\left(\frac{\pi_i}{1-\pi_i}\right) = \kappa(X_i) + \gamma U_{i.} \qquad (12)$$
$$where \quad 0 \le U_i \le 1$$

Rosenbaum shows that this relationship implies the following bounds on the ratio of the odds that either of two cases which are matched on X (or alternatively on the propensity score P(X)) will receive the treatment

$$\frac{1}{\Gamma} \le \frac{\pi_{s,1}(1-\pi_{s,2})}{\pi_{s,2}(1-\pi_{s,1})} \le \Gamma \qquad (13)$$

where s indexes the matched pair, s=1,…,S, and $\Gamma = \exp(\gamma)$.

Under the assumption that a confounding variable U exists, equation (11) becomes the sum of S independent random variables where the sth variations equals d_s with probability

$$p_s = \frac{c_{s1}\exp(\gamma u_{s1}) + c_{s2}\exp(\gamma u_{s2})}{\exp(\gamma u_{s1}) + \exp(\gamma u_{s2})}$$

and equals 0 with probability $1 - p_s$. Define

$$p_s^+ = \begin{cases} 0 \quad if \quad c_{s1} = c_{s2} = 0 \\ \dfrac{\Gamma}{1+\Gamma} \quad if \quad c_{s1} \ne c_{s2} \end{cases}$$

and

$$p_s^- = \begin{cases} 0 \quad if \quad c_{s1} = c_{s2} = 0 \\ \dfrac{1}{1+\Gamma} \quad if \quad c_{s1} \ne c_{s2} \end{cases}$$

Rosenbaum shows that for any specific γ, the null distribution of t(Z,r) is bounded by two known distributions for T^+ and T^- that are attained at values of W which perfectly predict the signs of c_{si} in equation (11), where:

$$E(T^+) = \sum_{s=1}^{S} d_s p_s^+$$
$$E(T^-) = \sum_{s=1}^{S} d_s p_s^-$$
$$Var(T^+) = \sum_{s=1}^{S} d_s^2 p_s^+ (1 - p^s)$$
$$Var(T^-) = \sum_{s=1}^{S} d_s^2 p_s^- (1 - p^-)$$

One can use these formulas to compute the significance level of the null hypothesis of no effect. For any specific Γ, one computes

$$(T - E(T^+)) / \sqrt{Var(T^+)}$$

and

$$(T - E(T^-)) / \sqrt{Var(T^-)}$$

where T is the Wilcoxon signed rank statistic. These two values give bounds of the significance level of a one-sided test for no effect of the treatment.[1]

Under the more stringent assumption of a constant (or "additive") treatment effect, Rosenbaum (2002) also derives bounds on the Hodges-Lehmann point estimate of the treatment

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[1] Normally, the Wilcoxon signed rank test is used for outcomes with continuous values. However, we established via simulation that it also performs very well when the outcome is discrete. We thank Michael Lavine for his assistance in our study of the properties of the Wilcoxon signed rank test.

effect, enabling the researcher to frame the sensitivity analysis in the more common metric of an interval of point estimates rather than in terms of implied significance levels for the estimated treatment effect. To arrive at an interval of plausible point estimates given a specific bias level Γ, Rosenbaum defines the Hodges-Lehmann point estimate of the treatment effect

$$\hat{\delta} = \frac{\inf\{\delta : t' > t(Z, Y - \delta Z)\} + \sup\{\delta : t' < t(Z, Y - \delta Z)\}}{2}$$

Though not generally known, the expectation of that signed rank statistic is bounded by the expectations of T^+ and T^- calculated at

$$t_{\min} = \frac{p^- S(S+1)}{2} \qquad \text{and} \qquad t_{\max} = \frac{p^+ S(S+1)}{2}$$

where

$$p^- = \frac{1}{1+\Gamma} \qquad \text{and} \qquad p^+ = \frac{\Gamma}{1+\Gamma}$$

as before. Since the bounds on the signed rank statistic are sharp, we can calculate an *interval* of point estimates consistent with these bounds by calculating the statistic at $t = t_{\max}$ and $t = t_{\min}$. By similar reasoning, Rosenbaum also derives bounds for the confidence interval of the point estimate.

Appendix C: Supplementary Estimations

Table C1. PSM Matching Estimates of the Effect of High School Leadership on Log Wages

	Econometric Method		
A. Males	NN	Caliper	Kernel
Log Wage (Full)	0.106 **	0.108 **	0.077 ***
	(0.031)	(0.030)	(0.024)
Number of Observations	*1675*	*2295*	*2309*
Log Wage (Direct)	0.089 **	0.085 **	0.068 **
	(0.032)	(0.032)	(0.024)
Number of Observations	*1658*	*2299*	*2310*
B. Females	NN	Caliper	Kernel
Log Wage (Full)	0.081 **	0.086 **	0.060 **
	(0.031)	(0.031)	(0.023)
Number of Observations	*1866*	*2442*	*2461*
Log Wage (Direct)	0.051 *	0.054 *	0.031
	(0.030)	(0.029)	(0.024)
Number of Observations	*1846*	*2445*	*2461*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.
Full Log Wage estimations do not control for education while direct log wage regressions include years of education.

Table C2. Estimates of the Effect of High School Extracurricular Participation on Log Wages

	Econometric Method			
A. Males	OLS	School FE	NN PSM	IV
Log Wage Full Effect:				
High School Leadership	0.084 **	0.140 **	0.150	0.145
	(0.042)	(0.053)	(0.082)	(0.052)
Number of Observations	*2310*	*2052*	*2308*	*2256*
Log Wage Direct Effect:				
High School Leadership	0.054	0.118 **	0.147	0.095
	(0.042)	(0.053)	(0.094)	(0.053)
Education	0.046 ***	0.031 **	N/A	0.047 ***
	(0.008)	(0.010)		(0.009)
Number of Observations	*2310*	*2052*	*2302*	*2256*
B. Females	OLS	School FE	NN PSM	IV
Log Wage Full Effect:				
High School Leadership	0.109 **	0.112 *	0.043	0.257 **
	(0.045)	(0.060)	(0.084)	(0.051)
Number of Observations	*2461*	*2200*	*2455*	*2388*
Log Wage Direct Effect:				
High School Leadership	0.075 *	0.077	0.037	0.217
	(0.045)	(0.059)	(0.086)	(0.051)
Education	0.071 ***	0.074 ***	N/A	0.073 ***
	(0.007)	(0.009)		(0.008)
Number of Observations	*2461*	*2200*	*2451*	*2388*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.
Full Log Wage estimations do not control for education while direct log wage regressions include years of education.

Table C3. Estimates of the Effect of Elected High School Leadership on Log Wages

	Econometric Method			
A. Males	OLS	School FE	NN PSM	IV
Log Wage Full Effect:				
High School Leadership	0.089 ***	0.132 ***	0.107 **	0.032
	(0.028)	(0.035)	(0.039)	(0.052)
Number of Observations	*2310*	*2052*	*708*	*2256*
Log Wage Direct Effect:				
High School Leadership	0.076 **	0.122 ***	0.080 *	0.010
	(0.028)	(0.035)	(0.042)	(0.053)
Education	0.046 ***	0.030 **	N/A	0.050 ***
	(0.008)	(0.010)		(0.008)
Number of Observations	*2310*	*2052*	*717*	*2256*
B. Females	OLS	School FE	NN PSM	IV
Log Wage Full Effect:				
High School Leadership	0.068 **	0.069 ***	0.065 *	0.083
	(0.024)	(0.031)	(0.036)	(0.051)
Number of Observations	*2461*	*2200*	*949*	*2388*
Log Wage Direct Effect:				
High School Leadership	0.044 *	0.043	0.025	0.057
	(0.024)	(0.030)	(0.035)	(0.051)
Education	0.071 ***	0.073 ***	N/A	0.074 ***
	(0.008)	(0.009)		(0.008)
Number of Observations	*2461*	*2200*	*934*	*2388*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.
Full Log Wage estimations do not control for education while direct log wage regressions include years of education.

Table C4. PSM Estimates of the Effect of High School Leadership on Educational Attainment

	Econometric Method		
A. Males	NN	Caliper	Kernel
Years of Education	0.433 ***	0.419 ***	0.342 ***
	(0.097)	(0.095)	(0.072)
Number of Observations	*1675*	*2295*	*2310*
Probability of College Degree	0.138 ***	0.131 ***	0.102 ***
	(0.031)	(0.030)	(0.023)
Number of Observations	*1675*	*2295*	*2310*
B. Females	NN	Caliper	Kernel
Years of Education	0.338 ***	0.346 ***	0.346 ***
	(0.092)	(0.090)	(0.070)
Number of Observations	*1866*	*2442*	*2461*
Probability of College Degree	0.117 ***	0.121 ***	0.110 ***
	(0.031)	(0.030)	(0.023)
Number of Observations	*1866*	*2442*	*2461*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.

Table C5. Estimates of the Effect of High School Extracurricular Participation on Educational Attainment

	Econometric Method				
A. Males	OLS	Logit	School FE	NN PSM	IV
Years of Education	0.660 ***	N/A	0.690 ***	0.936 **	1.049 ***
	(0.108)		(0.135)	(0.246)	(0.273)
Number of Observations	*2310*		*2052*	*2308*	*2256*
Probability of College Degree	N/A	0.230 ***	0.182 ***	0.306 ***	0.362 ***
		(0.035)	(0.045)	(0.057)	(0.044)
Number of Observations		*2310*	*2052*	*2308*	*2256*
B. Females	OLS	Logit	School FE	NN PSM	IV
Years of Education	0.486 ***	N/A	0.475 **	0.425 *	0.552 *
	(0.120)		(0.157)	(0.259)	(0.134)
Number of Observations	*2461*		*2200*	*2455*	*2388*
Probability of College Degree	N/A	0.205 ***	0.092 *	0.178 **	0.304 **
		(0.054)	(0.053)	(0.071)	(0.103)
Number of Observations		*2461*	*2200*	*2455*	*2388*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.

Table C6. Estimates of the Effect of Elected High School Leadership on Educational Attainment

	Econometric Method				
A. Males	OLS	Logit	School FE	NN PSM	IV
Years of Education	0.284 ***	N/A	0.339 ***	0.390 **	0.433 **
	(0.071)		(0.091)	(0.124)	(0.158)
Number of Observations	*2310*		*2052*	*708*	*2256*
Probability of College Degree	N/A	0.144 ***	0.121 ***	0.142 ***	0.116 **
		(0.035)	(0.030)	(0.040)	(0.044)
Number of Observations		*2310*	*2052*	*708*	*2256*
B. Females	OLS	Logit	School FE	NN PSM	IV
Years of Education	0.338 ***	N/A	0.353 ***	0.379 ***	0.351 **
	(0.063)		(0.080)	(0.102)	(0.134)
Number of Observations	*2461*		*2200*	*949*	*2388*
Probability of College Degree	N/A	0.152 ***	0.115 ***	0.130 ***	0.125 **
		(0.030)	(0.027)	(0.035)	(0.049)
Number of Observations		*2461*	*2200*	*949*	*2388*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Standard errors are in parenthesis.
Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.

Table C7. Estimates of the Effect of High School Leadership on Educational Attainment

	Econometric Method			
A. Males	Logit	School FE	NN PSM	IV
Probability of College Attendance	0.155 ***	0.105 ***	0.120 **	0.164 ***
	(0.024)	(0.022)	(0.031)	(0.044)
Number of Observations	*2305*	*2047*	*1662*	*2251*
B. Females	Logit	School FE	NN PSM	IV
Probability of College Attendance	0.162 ***	0.122 ***	0.109 **	0.127 **
	(0.023)	(0.022)	(0.030)	(0.043)
Number of Observations	*2458*	*2197*	*1853*	*2385*

Note: *,**, and *** denote statistical significance at the 10%, 5% and 1% level, respectively.

Standard errors are in parenthesis.

Specifications include controls for demographic, family background, geography, 8th grade characteristics, test scores and school enrollment.