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How Large are Returns to Schooling?  
Hint: Money Isn't Everything

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HOW LARGE ARE RETURNS TO SCHOOLING?  
HINT: MONEY ISN'T EVERYTHING

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How large are returns to schooling? Hint: Money isn't everything  
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### **ABSTRACT**

This paper explores the many avenues by which schooling affects lifetime well-being. Experiences and skills acquired in school reverberate throughout life, not just through higher earnings. Schooling also affects the degree one enjoys work and the likelihood of being unemployed. It leads individuals to make better decisions about health, marriage, and parenting. It also improves patience, making individuals more goal-oriented and less likely to engage in risky behavior. Schooling improves trust and social interaction, and may offer substantial consumption value to some students. We discuss various mechanisms to explain how these relationships may occur independent of wealth effects, and present evidence that non-pecuniary returns to schooling are at least as large as pecuniary ones. Ironically, one explanation why some early school leavers miss out on these high returns is that they lack the very same decision making skills that more schooling would help improve.

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## **I. Introduction**

This paper explores the many avenues by which schooling affects lifetime well-being. Economists' traditional views about schooling have been substantially influenced by Gary Becker's (e.g. 1964) approach to modeling long term decision-making. Usually under this approach schooling is viewed as purely a financial investment: Individuals spend money and time to acquire (or signal) human capital, in hopes of greater lifetime wealth in return. With more wealth comes more opportunity for consumption, and with more consumption comes more well-being. Differences in costs and benefits, which may themselves depend on social-economic backgrounds, may help explain why some obtain more schooling than others (e.g. see Card 2001).

Treating schooling as a financial investment has been hugely successful in explaining labor market behavior. It also has helped simplify macro and micro models involving attainment choices, with parameters that can be estimated using readily available data and computing power (even from the 1950s). Better data and innovative empirical techniques have led to, we think, general consensus that in the United States and other developed countries: 1) the monetary returns to annual adult income from spending one year in high school or college are about 7 to 12 percent, on average; 2) returns are generally higher among individuals from more disadvantaged backgrounds; and 3) returns have generally increased since the 1980s.

With basic theoretical and empirical findings on the financial returns to schooling well-established, or at least well-debated, researchers are now paying more attention to what schooling actually does. In the traditional investment model, schooling itself is often treated as a black box. Increased human capital comes in the form of increased 'productivity' or one-dimensional 'skill'. Alternative views consider more realistic cases in which schooling generates many experiences and affects many dimensions of skill that, in turn, affect many central aspects of individuals' lives both in and outside the labor market. Schooling may not only affect income but also the degree one enjoys working, or the likelihood of not being able to find work. Schooling could also lead individuals to making better decisions about health, marriage, and parenting style. Some suggest schooling improves patience, making individuals more goal-oriented and less likely to engage in risky behavior. Occasionally, social scientists also recognize student-life has consumption value too.

These other views imply potentially very important non-pecuniary returns. One way to think about this is to consider how much someone's future well-being might change from additional schooling without a change in wealth (suppose, for example, after two years of college an individual graduates in an unexpected recession that leads to lower future wages that are enough to perfectly offset earnings increases from schooling). We attempt to depict this type of scenario in Figure 1 by estimating differences in self-reported adult happiness across school attainment levels, with and without holding family income constant. The black bars graph the fraction of 25 to 45 year-old Americans born in the 1972 to 2000 General

Social Surveys (GSSs) who self-report being overall happy or very happy with life, after conditioning for a large set of family background controls and using the overall fraction happy among high school graduates (89 percent) as the baseline.<sup>1</sup> While obviously an imperfect measure of the causal effect of schooling on well-being, a relationship between self-reported happiness and school attainment levels clearly exists.<sup>2</sup> High school graduates with no additional schooling report being happy 8 percentage points more often than high school dropouts. College graduates report being happy 5 percentage points more often than high school graduates. What we want to emphasize is what happens when we graph this relationship when also conditioning on a proxy for wealth. The white bars in Figure 1 show the same relationship between schooling and happiness, but after conditioning on the family income bracket an individual reports in a given survey year, again using the overall

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<sup>1</sup> The sample includes all 25-45 year-olds from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. The graph reports relative differences in average self-reported happiness by whether an individual's highest level of schooling is less than high school (displayed as 0-11 years of schooling), high school (12 years), some college but no bachelors degree (13-15 years), or at least a bachelors degree (16+ years). Before conditioning for income, the variable, whether an individual self-reports being happy or very happy about life overall, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father's education, mother's working status, father's occupational prestige score, family's relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor's degree (13-15), and at least a bachelor's degree (16+), with those reporting high school as their highest level of schooling omitted. The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year. The data appendix provides more details.

<sup>2</sup> Subjective measures of well-being cannot accurately capture true levels of well-being. But even with measurement error, the evidence is clear that these questions correlate well with more objective signals of well-being. Schwarz and Strack (1999) provide a useful survey and summary of these results. See also Castriota (2006) for a review on the literature linking schooling and happiness.

fraction happy among high school graduates as the baseline. The relationship weakens, but only by about half. That is, after reporting having roughly the same annual household income, high school graduates still report being happy about 4 percentage points more often than high school dropouts, and college graduates report being happy slightly more than 2 percentage points more often than high school graduates. The graph is at least suggestive of the possibility that schooling affects individual well-being through many additional channels other than through income.

This article explores this possibility further by presenting recent research on the theoretical and empirical links between schooling and non-pecuniary outcomes. The next section documents many relationships between schooling and measures of social-economic success, which seem to exist while holding income constant. We include a discussion on the various mechanisms researchers have proposed to explain how these relationships might occur. We also emphasize the importance of not underestimating the consumption value of schooling for some individuals when considering non-pecuniary returns. The third section addresses issues in how to interpret these relationships. We discuss earlier work, and attempt to provide more convincing evidence that these relationships are causal by using within sibling and twin variation in schooling for a large sample of Norwegian adults, and by using schooling differences induced by changes in US compulsory schooling laws over time. Section four concludes with a discussion on the implications from the possibility that the combined pecuniary and non-pecuniary benefits from additional schooling are very large.

Note that we deliberately restrict our discussion to private returns. Additional effects of higher aggregate schooling on outcomes such as economic growth, innovation, city crime, tax revenue, and other externalities are beyond the scope of this paper. Interested readers may consult review articles by Moretti (2004) and Hanuchek (2002).

## **II. What does schooling do?**

### *Non-pecuniary returns to schooling in the Labor Market*

An easy starting place to look for non-pecuniary returns to schooling is fringe benefits, paid by employers on top of earnings. We are defining pecuniary returns here as strictly monetary because returns to schooling on earnings only captures monetary gains. Fringe benefits are not included. These include medical insurance, pension contributions, paid vacations, stock options, and so on. They tend to flow disproportionately more to workers with more schooling. Haveman and Wolfe (1984) cite studies that suggest monetary equivalent returns to schooling are 10 to 40 percent higher when factoring in these indirect gains.

Possibilities for non-pecuniary returns grow quickly when realizing just how much of daily life involves work. Schooling affects not only how much we earn, but how we do it. Some jobs offer more rewarding challenges and experiences. Some

offer more opportunities for more enjoyable social interactions. The Occupational Information Network (O\*NET) measures these kinds characteristics for each occupation in the United States. In particular, the O\*NET defines a set of ‘worker value’ variables that measure aspects of work “important to a person’s satisfaction”.<sup>3</sup> Figure 2 graphs the relationships between schooling and three of these variables: Achievement (a measure of accomplishment that employees may feel while on the job), Independence (a measure of an occupation’s autonomy and opportunity for creativity), and Relationships (a measure of how much social interaction occurs on the job). Each variable is on a 7 point scale, with 7 being the highest level. We match these descriptors and their corresponding occupations to workers aged 25 to 45 year-old in the General Social Survey. The black bars graph the mean O\*NET values after conditioning on a large set of family background controls and using the mean value among high school graduates as the baseline. The white bars show the same relationship but after conditioning on family income bracket reported in the same survey year (the GSS’s only measure of respondent income), and again using the high school mean as a reference point. The patterns are clear: workers from similar observable family backgrounds but with more schooling are in jobs that offer more sense of accomplishment, more autonomy, and more opportunity for social interaction. Even among workers with roughly similar incomes, the more educated have more rewarding jobs. The pictures look the same when looking at the three other ‘work value’ variables in the O\*NET: recognition

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<sup>3</sup> The O\*NET’s web site is [www.onetcenter.org](http://www.onetcenter.org).

(inside and outside the firm), support (from managers and co-workers), and working conditions (including job security).<sup>4</sup>

Non-pecuniary returns arise from how work affects individuals on and off the job. Work provides a reference by which individuals define themselves relative to others. As Robert Solow puts it:

“We live in a society in which social status and self-esteem are strongly tied both to occupation and income. Of course occupation and income are correlated, but not perfectly correlated. It seems undeniable to me that both occupation and income are significant variables. The way others look at us, and the way we look at ourselves, are both income related, and both are job related at given income.”

(Solow 1990, p. 9)

The first two panels of Figure 3 show the relationship between schooling and overall measures of job satisfaction using the same data and definitions above (and using the same data and definitions for other outcome variables below).<sup>5</sup> Occupational Prestige Scores reported in the GSS are calculated by compiling

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<sup>4</sup> Workers in jobs with less desirable traits may implicitly be compensated with higher wages compared to similarly skilled workers in more enjoyable jobs. If compensating wage differentials explained all of the observed schooling-occupational-quality relationship, the corresponding income relationship should be negative. Clearly this is not the case. Pecuniary and non-pecuniary effects for individuals with more schooling and more skills are additive rather than offsetting.

<sup>5</sup> Our measure of occupational prestige compiles a nationally representative sample of people’s subjective rankings of occupations and matches the overall ranking back to workers’ jobs in the sample. Cecas and Seff (1989) suggest that occupational prestige and self-esteem are related.

subjective prestige rankings of occupations by a representative sample and matching overall scores to workers' jobs. The first panel shows that workers with one to three years of college with similar family background are in jobs that measure, on average, 4.5 points higher in occupational prestige than high school graduates without college (the overall standard error is 10.5). Workers with 4 or more years of college have jobs that rank almost 10 points higher. These differences remain about the same after adding additional controls for family income. The same pattern arises when looking at self-reported job satisfaction. While few workers say they are a little or very dissatisfied with their job, about 4 percent more of high school graduates without college do so compared to college graduates, and 4 percent more of high school dropouts do so compared to high school graduates. The gradient of this overall relationship falls by only 30 percent when adding family income controls.

Any effects from schooling on the probability of being unemployed or on welfare are in addition to effects on workers' earnings. Long-term unemployment and welfare receipt are linked to depression and low self-esteem (e.g. Sheeran, Abrams, and Orbell 1995,). Time series data show that unemployment shocks precede worsening mental health (Bjorklund and Eriksson 2007), and that the non-pecuniary effects appear to be much larger than the effect that stems from the associated loss of income (Winkelmann and Winkelmann 1997). Schooling strongly relates to unemployment. The third panel of Figure 3 shows this with our data from the GSS. The same holds true when looking at welfare receipt (not shown).

Schooling also relates to how quickly the unemployed find work (Riddell and Xueda 2008), suggesting from it improves adaptability.

*Non-pecuniary returns outside the labor market*

One of the key purposes of schooling is to develop skills. Skills taught in medical school, for example, improve doctors' abilities to treat the sick. John McPeck (1994) calls these "knowledge-based" because their "general range of applicability is limited by the form of thought being called upon," like performing well at "Trivial Pursuit". Critical thinking and social skills, while less tangible, are likely overall more important for getting by in life. Critical-thinking helps individuals "select pertinent information for the solution of a problem [and] formulate relevant and promising hypotheses." In other words, it helps individuals process new situations or problems and make better decisions. Social skills facilitate interaction and communication with others. They help individuals distinguish between acceptable and unacceptable behavior in different public settings.

The education literature is remarkably unclear about how critical-thinking and social skills are acquired. The two are strongly and positively correlated with schooling (Cascio and Lewis 2006, Soskice 1993, Heckman 2006, Glaeser et al. 2005). Perhaps students learn them over time while writing essays or interacting with schoolmates outside of class. Or perhaps individuals with these traits excel at

school and thus find it easier to obtain more. In either case, they come in handy in many situations, not just those on the job.

Economists have two general models in mind to describe how better skills generate non-pecuniary returns outside the labor market.<sup>6</sup> The productive efficiency model suggests improved skills act as a technology shock; individuals are able to get more done in the same amount of time for the same amount of money. Perhaps this occurs from improved multi-tasking or time management skills. The allocative efficiency model pertains to situations in which the more skilled choose a different mix of inputs in trying to maximize the household production function. In other words, individuals with better skills make better decisions when faced with similar circumstances.

Good health, shown in Panel A of Figure 4, is often singled out as a key non-pecuniary benefit from additional schooling. Many studies find a strong positive correlation between schooling and multiple measures of health outcomes, healthy habits, and healthy activities, with this correlation remaining large after conditioning on income (see Grossman 2006). Evidence is mixed, but appears to lean more toward the allocative efficiency hypothesis. Wagstaff (1986), for example, concludes that schooling improves health while simultaneously reducing the number of physician visits, supporting the productive efficiency hypothesis. However, Glied and Lleras-Muney (2008), Chen and Lang (2008), Kenkel (1991), and de Walque (2004a, 2004b) provide strong evidence that new information on

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<sup>6</sup> Grossman (2006) formulates these models more explicitly.

health induces faster and more pronounced responses for those with more schooling.<sup>7</sup>

Some economists believe that more schooling not only makes individuals more attractive to employers, but more attractive period. Men and women with more earnings potential or with more prestigious jobs become more appealing in a competitive marriage market (Becker 1973, Chiappori et al. forthcoming). Indeed, Goldin (1992) concludes that the main purpose of going to college for women in the mid-twentieth century was to attract a college-educated husband. Numerous empirical studies document a tendency for persons to choose partners of similar schooling attainment (Rockwell 1976), and this tendency appears to be increasing (Mare 1991).

Improved allocative efficiency from schooling may also translate to more stable marriages. Critical thinking and social skills that help one succeed in the labor market also probably help in the marriage market. Panel two of Figure 4 shows substantially lower ever-divorced rates among those with more completed years of schooling of similar age and family background.

Overwhelming empirical evidence shows that women with more schooling have fewer children (e.g. Jones and Tertilt 2008). The dominant explanation for this is a trade-off between number of children and parental investment per child.<sup>8</sup> The idea is that, since more educated tend to work more, they also try to avoid spreading their time too thin by parenting fewer children. Recent evidence is mixed.<sup>9</sup> The flip

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<sup>7</sup> Kenkel (2000) and Grossman (2000) provide good reviews.

<sup>8</sup> The theory originates to Becker and Lewis (1973) and Becker and Tomes (1976).

<sup>9</sup> See, for example, Angrist, Lavy, and Schlosser (2006) and Qian (2009).

side of the coin is that individuals who prefer fewer children may enjoy more schooling and career opportunities (Jones et al. 2008). Others suggest more educated simply use contraceptives more effectively, in line with the allocative efficiency hypothesis.

For couples with children, parental schooling strongly relates to children's development and social-economic success throughout life. Health, social integration, test scores, and labor market outcomes all correlate positively with both mother and father's attainment. Differences in income may explain some of these relationships. For example, limited resources and an aversion to or lack of knowledge about financial aid may limit a child's access to college.<sup>10</sup> Differences in birth weight could arise from poor mothers not being able to afford good nutrition or time to exercise.<sup>11</sup> On the other hand, conditioning on income does not eliminate these kinds of intergenerational relationships (see the example below for grade repetition). Just as schooling may improve skills to help with marriage, it may do the same for parenting. Recent research on the determinants of human development underscores parenting as the most important determinant for children's cognitive and non-cognitive development, even among families with similar incomes (Cunha and Heckman 2009). Panel three of Figure 4 provides at least some evidence that parenting styles differ by school attainment. The fraction of parents in our GSS sample who strongly agree that "it is sometimes necessary to discipline a child with

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<sup>10</sup> See Belley and Lochner (2007) for evidence that family income affects college access.

<sup>11</sup> See Currie and Moretti (2007) for evidence that maternal schooling affects infant mortality and birth weight.

a good hard spanking” is substantially lower for respondents with college experience, with and without additional controls for family income.<sup>12</sup>

### *Effects on Preferences*

Schooling may also change people’s preferences by providing information about new opportunities for consumption or by developing patience. As Becker and Mulligan (1997) put it:

“Schooling focuses students’ attention on the future. Schooling can communicate images of the situations and difficulties of adult life, which are the future of childhood and adolescence. In addition, through repeated practice at problem solving, schooling helps children learn the art of scenario simulation. Thus educated people should be more productive at reducing the remoteness of future pleasures.” (pp. 735-736).

The GSS variable, “Live only for today” may serve as a proxy for time preference. Respondents were asked whether they agree to the statement, “Nowadays, a person has to live pretty much for today and let tomorrow take care of itself”. Panel 1 of Figure 5 shows a distinct declining relationship with schooling,

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<sup>12</sup> We use this variable to demonstrate differences in parenting styles by school attainment. The effectiveness of corporal punishment on children, and under what conditions, remains unclear. Many countries legally prohibit it. The American Academy of Pediatrics states that “Corporal punishment is of limited effectiveness and has potentially deleterious side effects” (Stein and Perrin 1998).

holding various observable family background variables constant. More than half of high school dropouts agree with this statement while less than 30 percent of college graduates do. Similar to the previous patterns above, conditioning on reporting the same family income bracket in the same survey year reduces the gradient of this relationship, but not by much.

The second and third panels of Figure 5 show outcomes that may arise as a consequence from “living only for today”.<sup>13</sup> Teen fertility and criminal activity (as well as unhealthy lifestyles) are risky behaviors often considered driven by “affective” thinking (a focus on immediate feelings) rather than “cognitive” thinking (a focus on long-term benefits and costs). Efforts to reduce them aim to improve conditions later on in life. Like several other studies (e.g. Black, Devereau and Salvanes (2008), Lochner and Moretti (2004)), Figure 5 shows both outcomes negatively correlate with years of completed schooling.<sup>14 15</sup>

Several studies suggest schooling fosters trust. Social scientists place great emphasis on the importance of trust in improving social interaction and fostering community involvement. A more trusting and engaged society is often used to justify public subsidies to schooling (e.g. Hanushek 2002), but these traits offer private returns too. Arrow (1974) notes that in the face of transaction costs, trust

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<sup>13</sup> Black, Devereau and Salvanes (2008) suggest two additional non-pecuniary reasons how schooling could affect teen fertility and crime. First, staying in school could reduce the amount of time and opportunity for engaging in risky behavior. Second, schooling may increase both current and expected future earnings and thus increase the opportunity cost of engaging in risky behavior. Also, as mentioned above, schooling could lead to more efficient contraceptive use.

<sup>14</sup> The view that young offenders are myopic lines up with evidence from Lee and McCrary (2005).

<sup>15</sup> See also Ross and Mirowsky (1999) for a discussion on how schooling, by developing patience and control, may help encourage healthier lifestyles.

underlies almost every economic transaction. Its individual importance arises in situations when trust promotes reciprocity. Lab experiments and ethnographic studies suggest that a willingness to engage and work or help others often leads to others being nicer and more cooperative in return (Fehr and Gächter 2000, Uslaner 2000). Schooling is one of the most important predictors of trust. Helliwell and Putnam (1999) point out that a causal relationship could occur for relative reasons (it raises social status for some individuals while propping down status of others), additive reasons (it teaches people how to interact successfully with others), or super additive reasons (raising overall attainment levels makes everyone more trusting). Figure 6 shows outcomes of trust and social participation and their relationship with schooling. Individuals with similar family backgrounds but more schooling are more likely to agree that, generally speaking most people can be trusted. They also are much more likely to vote, and to volunteer for a community or organization. Conditioning on reported family income bracket, as we have seen earlier, does not alter these relationships substantially.

### *Negative non-pecuniary returns*

We do not rule out the possibility that with schooling comes negative returns, especially in regards to added stress and constraints on time.<sup>16</sup> The costs of losing one's job when making more are obviously higher. Jobs that pay more may also come with more responsibility, more travel, and more effort, all of which may

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<sup>16</sup> We are well aware of these possibilities after writing this paper.

add stress and pressures to work more. Surprisingly, Cohen et al. (2006) finds that stress hormones are negatively associated with income and schooling. The authors suggest that any additional pressures from working in higher paid occupations are offset by better health and social support. Figure 7 shows other time use outcomes from our GSS sample. These questions were asked only in a subset of years, so sample sizes are smaller and the patterns are less precise than the ones presented above. Fewer individuals with more schooling report always feeling rushed for time than those with less. College graduates are almost 6 percentage points less likely to feel rushed than high school graduates with no college. Conditioning on family income generally strengthens this relationship. Perhaps lower income households feel more rushed because they are not able to afford commodities that would help save time. We do at least find a tendency for college graduates to report wanting to spend more time with friends and in leisure activities.

### *Schooling as consumption*

The satirical newspaper, *The Onion* (2000), published a story about an accountant manager at a meeting who became distracted during his presentation looking out the window. The paper quoted the manager remarking to his co-workers that the weather reminded him of “this great day when me and a bunch of my buddies climbed up onto the roof and spent the whole day just drinking beer and cranking U2 and soaking up the sun. Man, that was awesome”. The human capital

model usually treats time spent in school as an opportunity cost in terms of forgone earnings. Some researchers add 'psychic costs' to account for the mental effort required to perform on exams and complete the necessary requirements to graduate. Except for the more intellectually curious, most people would rather be doing something else than studying or sitting in class. Fortunately, schooling provides many additional opportunities for more pleasurable experiences, especially for those attending full time. These include viewing and participating in sports, socializing with others the same age, dating, attending nearby entertainment events, living among other youth away from parents, and enjoying campus scenery.<sup>17</sup>

Quantitative evidence for the consumption value of schooling comes from showing that some students make enrollment decisions based in part on factors not likely help after completion. Several researchers estimate low or even negative pecuniary returns from majoring in some college majors or enrolling in graduate school (even after conditioning on academic potential) and attribute this behavior to school consumption (Alstadsaeter 2004, Arcidiacono 2004, Lazear (1977)). Another excellent example is by Pope and Pope (2008), who show an increase in the quantity and quality of students applying to colleges that performed well in basketball and football the previous year.<sup>18</sup>

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<sup>17</sup> On the other hand, students with children or working significant hours are much less likely to partake in these activities.

<sup>18</sup> Along these lines, we collected data from Princeton Review's college rankings and estimated whether top "party" or "sports" colleges were harder to get into compared to other colleges with similar academic ranking in the same region. Specifically, for the sample of 324 colleges with ACT composite data in the 2010 edition of the Princeton Review rankings, we regressed the mean Freshman ACT

### III. Measurement Issues

A host of measurement issues crop up when trying to understand true impacts from schooling. The fact that individuals usually choose how long they wish to stay in school and that researchers usually collect data only on the length of time people spent in school makes estimation and interpretation challenging. Below we touch on these concerns and present more convincing approaches to generate evidence of large non-pecuniary returns.<sup>19</sup>

#### *Heterogeneity*

The schooling relationships mentioned last section are averaged over some individuals who benefit more and some less. This makes assessing potential returns to schooling for sub-groups complicated. Clearly, individuals differ by their tolerance for taking tests and their degree of parental support. Access to job networks and a little luck also leads to different outcomes. So not all schooling

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score (used for admissions) on a linear or quadratic variable for academic ranking, fixed effects for state or first three digits in the college's ZIP code, and dummy variables for whether the college was included among the top 20 "Party Schools", "Most Beautiful Campuses", or sports schools ("Students Pack the Stadiums"). Admissions requirements were significantly higher for party and sports schools. The estimated effects weakened and become only marginally significant after further conditioning on for the log of college size. The results generally support the conclusion that which college students attend depends, at least in part, on sports and social opportunities. Results are available on request from the authors.

<sup>19</sup> Card (2001) discusses empirical estimation issues with respect to pecuniary returns in more detail.

investments pay off. With sufficient sample size, results can be separated by group (e.g. by gender), which partly offsets these problems. Econometric techniques sometimes also help in identifying individuals we are often most interested in, like those on the verge of leaving school without college or a high school degree (see below). Still, it must be recognized that the effects are averaged and do not necessarily reflect real impacts for every individual.

### *Schooling versus education*

Years of schooling and degree attainment are not particularly good measures of education. They provide limited information on what it is about schooling that produces pecuniary and non-pecuniary returns. A better understanding about which particular skills generate returns and how skills are actually acquired would lead to better measures of school quality. Despite much interest, we know very little about the impact of different curricula or about different pedagogical methods and ways of organizing and running schools. An over-reliance on quantitative- and qualification based measures has neglected qualitative evidence and theoretical perspectives. It may well be argued that the effects of schooling depend just as much on the nature and quality of learning as on the number of years spent in school. The reasons of course for why most of the research so far has focused on years of schooling or grades completed is that these data are readily available while collecting information on curricula and teaching methods are much more expensive. To extend our knowledge, we need to look more carefully at what happens during

learning experiences and expand the range of measures to include the more qualitative dimensions of learning environments.

### *Signaling skill through schooling*

Schooling may help develop skills or it may help signal skills that individuals already have. If those with more schooling also have more inherent abilities (perhaps because schooling for them is easier or more enjoyable), employers can use schooling to predict better candidates. This is especially helpful when desirable worker attributes, like perseverance, discipline, and time management are, not easily observed. Schooling may affect pecuniary and non-pecuniary outcomes regardless of whether it develops or signals skills; the returns appear the same. However, individuals are more likely to view schooling under a signaling story as a necessary waste of time for indicating to employers they already possess a desired set of skills. It is very difficult to disentangle the extent to which returns to schooling are driven more by signaling or skill development mechanisms because both theories generate very similar empirical predictions. Our view of the literature is that there is evidence of both.<sup>20</sup>

Causal evidence of non-pecuniary returns to schooling supports the skill development theory more than it does signaling. Exogenous increases to schooling affect a person's relative ranking in terms of overall attainment, but this matters

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<sup>20</sup> For example, see Arcidiacono et al. (2008).

only to employers or possibly potential spouses that statistically discriminate. Relative school rank should not affect individual decisions such as whether to smoke, vote, spank, or “live for today”. If schooling affects these decisions, it likely plays more than just a signaling role.

### *Causality*

Schooling is often used as the prototypical example for demonstrating challenges in trying to estimate causal effects. Usually people choose how much schooling to take. Skills that individuals already possess may therefore be correlated with it. Estimated returns are upward biased if those who would have attained more social-economic success regardless tend to take more schooling anyway (e.g. perhaps for consumption reasons).

One approach involves using siblings or twins with different levels of schooling. By comparing outcomes between brothers or sisters with different attainment we hold constant all common family factors. Similar genetic and family environment influences are completely eliminated. The question, then, is why would siblings (especially twins) end up with different levels of schooling? Perhaps one sibling became more inspired by friends or teachers to go to continue. On the other hand, even small genetic differences between siblings can lead to differences in mental development and academic achievement (Fletcher and Lehrer 2009). If the reasons why some siblings obtain different schooling amounts are mostly

unrelated to later social-economic success, then the approach provides a useful estimation strategy.<sup>21</sup>

Table 1 presents sibling and twin returns to schooling estimates for several outcomes with and without conditioning on income. We take advantage of Norwegian administrative data, which contain extremely large samples of siblings and twins.<sup>22</sup> Our sample includes individuals 28 to 60 years old in 2005. A more detailed data description is given in the appendix. Column 1 displays the mean for each outcome. Column 2 shows the estimated returns for siblings. For comparison, the first row in Column 2 shows that working siblings with one more year of schooling have, on average 5.2 percent more annual income than their less educated siblings. As has been found previously for Norway and indeed the other Nordic countries the return is on the lower spectrum than for the most other OECD countries. Turning to non-pecuniary outcomes, siblings with more schooling are less likely to be unemployed and on welfare. As a measure for health, we also estimate effects on the likelihood of receiving health disability payments.<sup>23</sup> Siblings near retirement with more schooling are significantly less likely to be receiving these benefits, and therefore healthier. Consistent with the earlier patterns presented last section, siblings are also less likely to be divorced with more schooling. They are less likely to give birth as teenagers and more likely to be

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<sup>21</sup> See Neumark (1999) for more discussion about this approach.

<sup>22</sup> Behrman & Rosenzweig (2002) use a small sample of identical twins to compute within-twin estimates of the effect of mother's schooling on children's schooling.

<sup>23</sup> The Norwegian government provides disability benefits to ensure sufficient income for subsistence for people whose earning ability is permanently impaired by at least 50 per cent due to illness, injury or defect. Disability pensions are granted if it is quite clear that there are no prospects of an improvement in earning ability.

married to spouses who have higher earnings themselves. The results in Column 4 just for twins are basically the same as those for the full sibling sample.

The added challenge in trying to estimate non-pecuniary returns is trying to separate them from pecuniary ones, since more money may be used to improve the very same outcomes we are interested in. Most researchers estimate non-pecuniary returns without worrying whether they occur through wealth effects or not. Some outcomes, like unemployment, voting, and teen fertility are mechanically or unlikely related to income. For others, however, we would ideally like to use two separate sources of exogenous variation – one that affects schooling and another that affects income. For example, comparing siblings with different levels of schooling and in different firms where some unexpectedly get let go because of downsizing. Conditioning on observable income may still help if income variation uncorrelated with schooling is also uncorrelated with the outcomes of interest.<sup>24</sup> Columns 3 and 5 show non-pecuniary returns to schooling estimates after conditioning on sibling and twin's incomes.<sup>25</sup> The estimated schooling effect on divorce rates falls by less than half. The estimated effect on teen fertility falls by very little, similar to the patterns presented in the previous section.

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<sup>24</sup> Schooling effects after conditioning on income are probably downward biased because individuals with above average schooling but below average income likely have inherently poor skills relative to their attainment level that affect both why they earn less and why they fare poorly on other outcomes. On the other hand, income only approximates lifetime wealth. Results could be upward biased if the schooling effect still comes from increases in wealth that are not adequately controlled for by conditioning on income.

<sup>25</sup> The results in column 3 are presented after including quartic polynomial controls for annual income.

Another approach to estimating causal returns involves taking advantage of policies that affect schooling costs without affecting benefits. For example, whether individuals can commute to school impacts their likelihood of going. If a new college opens up in a remote neighborhood, it allows a follow-up analysis with local youth who become, on average, more educated than youth from other remote neighborhoods without nearby colleges.<sup>26</sup> Sometimes policies differ across region and over time. Researchers can compare relative schooling differences and corresponding outcomes between groups of individuals from different regions before and after policy changes that affect schooling attainment for younger birth cohorts from one region but not the other. The caveat with this approach is that the reasons behind the policy changes or other policies introduced at the same time need to be unrelated to the later outcomes of interest.<sup>27</sup>

By far the most common policy instrument used to examine pecuniary and non-pecuniary returns is compulsory schooling. Minimum schooling legislation changed over time in many countries. The changes made some youth stay in school that would have otherwise left earlier in absence of the more restrictive laws. Angrist and Krueger (1991) were the first to use compulsory schooling to estimate

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<sup>26</sup> David Card (1995) uses this policy change to look estimate pecuniary returns to schooling.

<sup>27</sup> Kane and Rouse (1993) use tuition changes over time to estimate large pecuniary returns to schooling. Currie and Moretti (2007) use college openings in the mid-twentieth century and find significant impacts of maternal schooling on children's health, and Kenkel, Lillard, and Mathios (2006) use differences in high school graduation requirements and local spending on education to find schooling effects on smoking and obesity.

pecuniary returns to schooling, and many have done so since.<sup>28</sup> More recently, other researchers have used these laws to estimate non-pecuniary effects on just about everything from trust (Milligan, Moretti, and Oreopoulos 2004) to eyesight problems (Soloveichik 2007).

Table 2 shows several estimates of pecuniary and non-pecuniary returns to compulsory schooling.<sup>29</sup> Most of the results come from a large sample of native-born Americans aged 25 to 64 from the 1950 to 2000 U.S. Censuses and 2001 to 2007 American Community Surveys. The first row of column 2 shows the estimated pecuniary returns. One year of compulsory schooling increases weekly earnings by 13.1 percent, on average. It also impacts other labor market outcomes including occupational prestige, the likelihood of being unemployed, and the likelihood of being on welfare.<sup>30</sup> Outside the labor market, compulsory schooling decreases the chances of ending up in jail (among black youth), being divorced, pregnant before age 20, and even being in a mental institute.<sup>31</sup> It also affects mortality and voting behavior.<sup>32</sup> We find additional intergenerational effects on the likelihood that a

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<sup>28</sup> For example, Meghir and Palme (2005), Oreopoulos (2006), and Aakvik, Salvanes, and Vaage (2009).

<sup>29</sup> The estimates use the minimum school leaving age faced when aged 16 as an instrumental variable. The appendix provides more details.

<sup>30</sup> Machin, Pelkonen and Salvanes (2009), Li (2006), and Oreopoulos (2007) also find effects on unemployment.

<sup>31</sup> These crime results are also consistent with Lochner and Moretti (2004) and Anderson (2009), and the teen fertility results are consistent with Black, Devereux, and Salvanes (2008) and Fort (2007).

<sup>32</sup> The effects on mortality are taken from Lleras-Muney (2005). De Walque (2005) also estimates causal effects of schooling on mortality. Data for the estimates on voting are from the November Current Population Surveys. See appendix for more details. Milligan, Moretti, and Oreopoulos (2004), and Dee (2004) present results for additional civic participation outcomes.

child repeated a grade.<sup>33</sup> And finally, using changes to compulsory schooling laws in the United Kingdom and data on self-reported well-being, we also estimate that compulsory schooling affects overall life satisfaction.<sup>34</sup> All of these estimates fall by less than half when conditioning on individual income.

#### **IV. Conclusion**

Time distinguishes schooling from other financial investments. Time in school helps shape students' identity and perspectives on life. Students interact with others, learn alternative points of view, and discover new ideas. Along the way, they acquire skills, some which help earn them more money in the labor market. Increasing wealth clearly provides a central motivator for why students forgo earnings and suffer through exams and writing assignments. But, as we argue in this paper, the experience and skills acquired generate many other non-pecuniary returns. Gains from school occur from being in a job that not only pays more but

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<sup>33</sup> The results are similar to those presented in Oreopoulos, Page, and Stevens (2006). Black, Devereux, and Salvanes (2008) use a different set of compulsory schooling laws in Norway to find intergenerational effects on education. Caneiro, Meghir, and Parey (2007) also estimate intergenerational effects using differences in economic conditions at the time individuals make schooling attainment decisions to estimate parental schooling effects on children's academic achievement around age 10. Plug (2004) uses a different approach to also estimate intergenerational effects by comparing children with the same biological parents but who have been adopted.

<sup>34</sup> Also see Oreopoulos (2007) for additional estimates on returns to schooling on life satisfaction.

also offers more opportunities for self-accomplishment, social interaction, and independence. Schooling generates occupational prestige. It reduces the chances of ending up on welfare or unemployed. It improves success in the labor market and the marriage market. Better decision-making skills learned in school also leads to better health, happier marriages, and more successful children. Schooling also encourages patience and long-term thinking. Teen fertility, criminal activity, and other risky behaviors decrease with it. Schooling promotes trust and civic participation. It teaches students how to enjoy a good book and manage money. And for many, schooling has consumption value too.

All of these gains hold after conditioning on income, suggesting that pecuniary returns cannot explain non-pecuniary returns. To get a rough back-of-the-envelope measure of the relative importance between the two, we can compare our estimated schooling effects on happiness in Table 2 before and after controlling for income. The effect before conditioning on income falls by only one-quarter after conditioning, suggesting that about three quarters of the schooling effect on self-reported life satisfaction is due to non-pecuniary factors. A 12 percent increase in annual earnings would then imply that the total non-pecuniary gains are equivalently worth another 16 percent increase in earnings (for a total of 28 percent).

If returns to schooling are so high, why do some students not stay on longer? One reason is that these returns are averaged over some individuals who benefit more and some less. Since children begin school with different capabilities and face

different obstacles, not everyone faces the same costs and benefits.<sup>35</sup> The results still imply at least some students should expect large returns. Under the basic investment model of schooling, the upfront costs for these students would have to be extremely large or the benefits extremely uncertain to rationalize early exit decisions (e.g. see Oreopoulos (2007). In our opinion, the estimated returns are too large to support these theories. If so, it means that some people are missing out on significant welfare-increasing opportunities. We suggest two alternative explanations worth further research, each carrying quite different implications about education policy. First, low-income families may face financial obstacles in trying to afford school. They may be averse to accepting thousands of dollars in debt for an indefinite amount of time, or they may be unaware about how to obtain financial aid. Recent work by Bettinger, et al. (2009), for example, show that helping individuals from disadvantaged families complete college financial aid application forms dramatically increases enrollment.<sup>36</sup> Second, other students may simply be shortsighted. Parents with teenagers can attest that youth are particularly predisposed to downplaying or ignoring future consequences from current behavior (see also Laibson 1997, O'Donoghue and Rabin 1999, Spear 2000 for more academic evaluation). Exploring these issues more thoroughly would shed further light on the overall education attainment decision-making process and help identify ways to

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<sup>35</sup> We also note that years of schooling and degree attainment are not particularly good measures of education. A better understanding about which particular skills generate these returns and how skills are actually acquired would lead to better measures of school quality. Despite much interest, surprisingly little progress has been made.

<sup>36</sup> See also Belley and Lochner (2009) and Field (2009) for more evidence and further reading on the importance of financial constraints.

make individuals realize large returns from schooling. Large amounts of money appear to be lying on the sidewalk, as well as opportunities for more satisfying and secure jobs, better health, happier marriages, etc...

## **Data appendix**

### **For Figures 1 to 7**

The figures were created by first constructing a subsample of the combined 1972-2000 General Social Surveys. We restricted our data to non-immigrants who were 14 years old since 1970 and at least 25 years old in the survey year. Dropping those born before 1956 cuts the sample substantially (from 34,173 to 6,811) and ends up limiting the upper age range to 45, but this allows us to focus the analysis on more recent cohorts. We did not find this to affect the overall results (if anything the results were schooling relationships were stronger for older cohorts). For some graphs, where the outcome variable was collected only over a subset of years, the sample is smaller.

The education variable is grouped by highest years of completed schooling: 0-11, 12, 13-15, and 16 or more, which we refer generally to as high school dropouts, high school graduates without college, some college, and college graduates respectively. To graph the black bars in the figures, the outcome variable of interest is regressed on fixed effects for state of birth, age, year of birth, and survey year fixed effects (multiple survey years allow us to estimate these relative fixed effects simultaneously), plus dummy variables for gender and race, as well as father's occupational prestige score (interacted with a missing value indicator), father and mother's education attainment (and indicators if these values were

missing), indicators for household composition at age 16 (living with both parents, mother only, etc.), whether mother worked (or if this value was missing), and whether family's income when 16 years old was far below average, below average, average, above average, far above average, or missing, as well as the education categories. The indicator for having 12 years of completed schooling was omitted. The remaining relative education coefficient estimates (the estimated outcome differences by school attainment relative to high school graduates) were added to the overall high school graduate sample mean and presented in the graphs. The white bars show the same coefficients, but after adding fixed effects for family income bracket for each survey year. In earlier years the number of bracket options were 13, but expanded to 24, with options ranging from "under \$1,000" to "\$110,000 or more" after 1997.

### **For Table 1**

The data used in Table 1 is derived from administrative registers and prepared for research by Statistics Norway. The cohorts include all 28 - 60 year-olds in 2005. The data set is a longitudinal population data set consisting of all people living in Norway born from the 1920s. Different registers for own family, parental family, educational outcomes, marriage, divorce, and other outcomes are merged using the same personal identifier. Persons are also matched to plants/firms as well as other labor market outcomes including on unemployment, on welfare,

retirement, or disability pension. The month and year of birth is given in the data set and hence twins can be identified. Schooling is measured as the normalized number of completed years. An individual is classified as employed if he has a plant identification number at that time, and as unemployed if he does not and is registered with some months of unemployment during the year. Earnings or annual income is measured as annual income that provides pension points in the national security system. The included components are regular labor income, income as self-employed, and benefits received while on sick leave, being unemployed and on parental leave. Married is registered as married in 2005. Divorced is registered as divorced in 2005. Teen fertility is whether women had a child as a teenager. Welfare is if received welfare in 2005. Assortative mating is measured in years of education for spouse in 2005. Disability pension is measured as disabled at the age of 58 or older from 1993-2005.

## **For Table 2**

Table 2 uses an extract of native-born individuals aged 25 to 64 from the combined six decennial census microdata samples between 1950 and 2000 and the seven American Community Surveys between 2001 and 2007.<sup>37</sup> Hawaiian- and Alaskan-born respondents

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<sup>37</sup> The specific Census files used were the 1950 General 1/330 sample (limited to those with long-form responses), the 1960 General 1 percent sample, the 1970 Form 2 State 1 percent sample, the 1980 Metro 1 percent sample, the 1990 1 percent unweighted sample, and the 2000 1 percent unweighted random sample. The data was downloaded from the IPUMS web site <http://usa.ipums.org/usa/index.shtml>.

were excluded. We coded the schooling variable for individuals in the 1950 to 1980 data as highest grade completed. Average years of schooling were assigned to categorical values in the 1990 and 2000 Censuses and the American Community Surveys using an imputation method discussed in Oreopoulos (2006b). The earnings variable, log weekly wage, was calculated by dividing annual wage and salary income by weeks worked, then taking logs. We used an indicator for welfare receipt from 1970 data and onwards. The 1950 to 1980 Censuses provided information on whether a respondent resided in a correctional or mental institution, which we used to create our outcome variables “in jail” and “in mental institution” respectively. Whether a mother gave birth as a teenager is calculated by determining whether the difference between a mother’s age and the age of her eldest own child in household is 19 years or less, for children 30 years old or less. We also used the Occupational prestige score measured using the methodology by Nakao and Treas (the variable “PRENT” from the IPUMS web site). Among children in the household aged 8 to 16, an indicator for ever repeated a grade was calculated as whether a child was below the median grade level of other children the same age and quarter of birth, state, and census year. The indicator was matched to the child’s mother and/or father. The voting indicator variable comes from the 1978 to 2000 November Current Population Surveys, the same data used by Milligan, Moretti, and Oreopoulos (2004). We restrict the sample to those of voting age (18 years old and older).

We restricted our data to individuals aged 16 between 1915 and 2000, aged 25 to 64 in the survey year. We also removed individuals with more than 12 years of completed schooling since these college educated were not substantially influenced by the minimum school leaving age (see Oreopoulos 2007). The results are generally the same, but less precise if we keep the larger sample. Similar to Oreopoulos (2006b), we measured each school-leaving age as the minimum between a state’s legislated dropout age and the minimum age required to obtain a working permit. We also record the dropout age under exceptions – for

example if students could leave with permission from parents or if working full time. We grouped the small percent of the sample that faced school-leaving ages lower than 14 into one category (school-leaving age < 14). All others faced dropout ages of 14, 15, 16, 17, or 18. The dropout age is matched to individuals according to the year in which they were 16 years old and their state of birth (which is likely their state of residence when 16 as well).

The results reported in Table 2 are from regressing the outcome variables on fixed effects for birth cohort, state of birth, data year, gender and race (for the full sample), a quartic polynomial for age, and years of completed schooling, with schooling instrumented by the dropout age under no exceptions and the dropout age under exceptions. The sample is weighted by the person weight variable from the IPUMS. Standard errors are clustered by state and year of birth.

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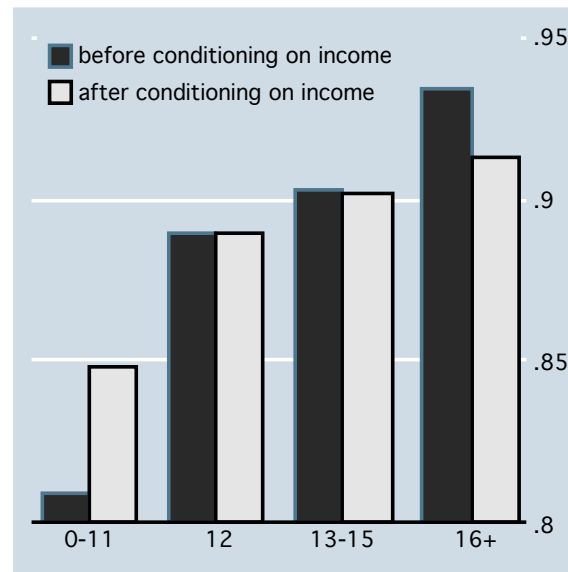
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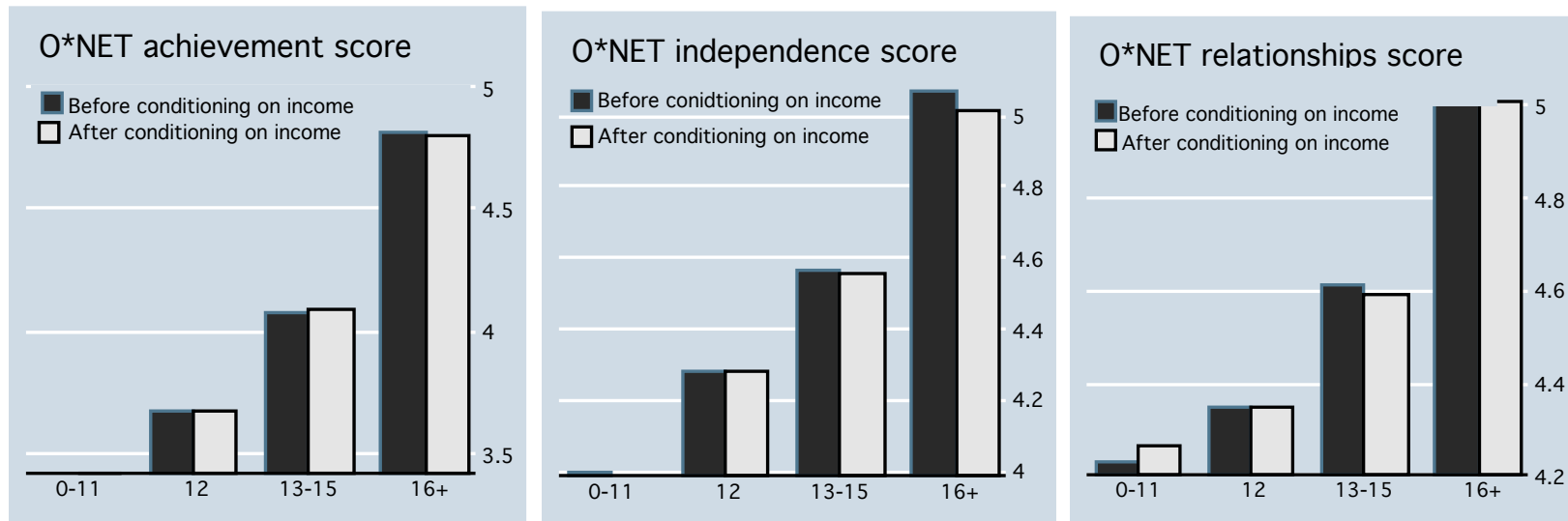
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**Figure 1**  
**Happiness and Years of Completed Schooling**  
**Before and After Conditioning on Income**



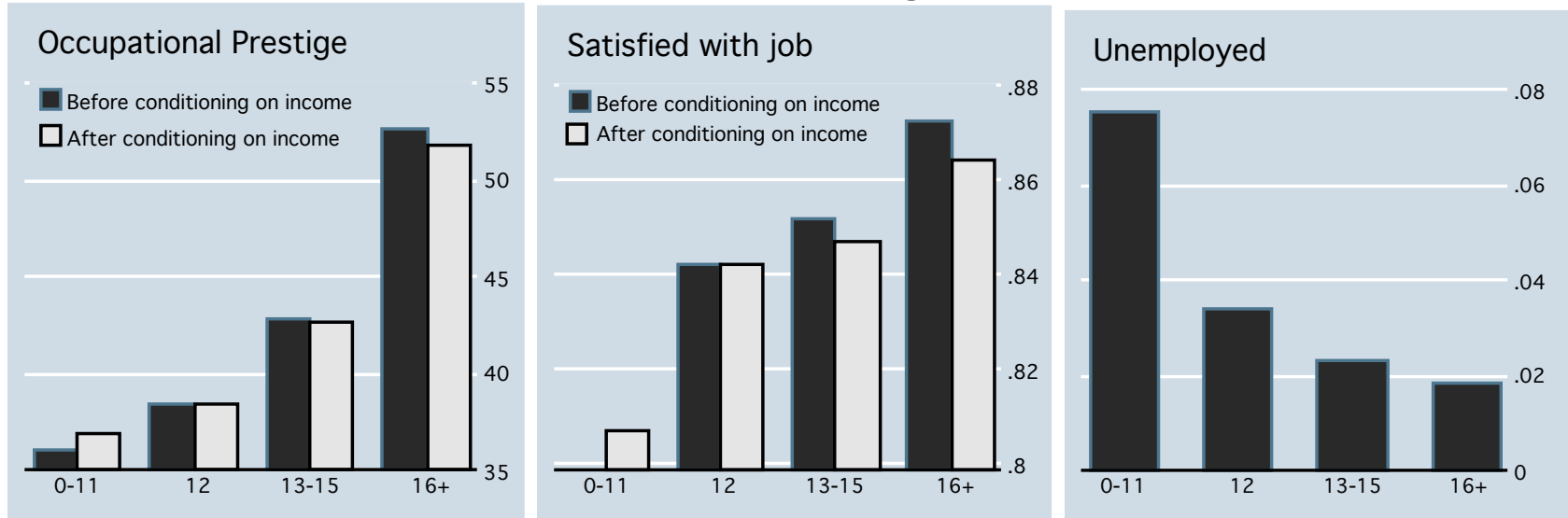
**Notes:** The sample includes all 25-45 year-olds from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. The graph reports relative differences in average self-reported happiness by whether an individual's highest level of schooling is less than high school (displayed as 0-11 years of schooling), high school (12 years), some college but no bachelors degree (13-15 years), or at least a bachelors degree (16+ years). Before conditioning for income, the variable, whether an individual self-reports being happy or very happy about life overall, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father's education, mother's working status, family's relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor's degree (13-15), and at least a bachelor's degree (16+), with those reporting high school as their highest level of schooling omitted). The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year.

**Figure 2**  
**On the Job Opportunities and Years of Completed Schooling**  
**Before and After Conditioning on Income**



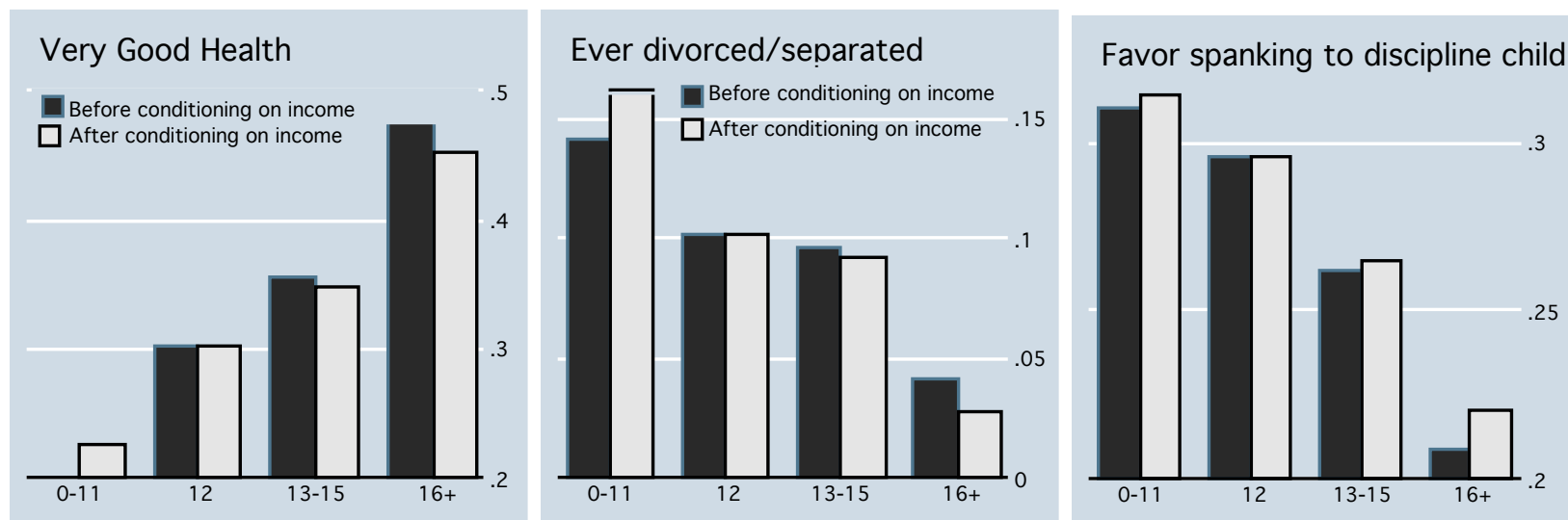
**Notes:** The sample includes 25-45 year-old workers from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. Outcomes are “Work-Value Descriptors” from the Occupational Network (O\*NET) Database. Achievement, independence, and relationship scores are designed to measure, on a seven point scale, the degree to which a worker’s occupation provides opportunities to feel a sense of a accomplishment, to feel independent, and to socialize with others respectively. Before conditioning for income, the outcome variable, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father’s education, mother’s working status, family’s relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor’s degree (13-15), and at least a bachelor’s degree (16+), with those reporting high school as their highest level of schooling omitted). The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year.

**Figure 3**  
**Measures of Labor-Market Well-being and Years of Completed Schooling**  
**Before and After Conditioning on Income**



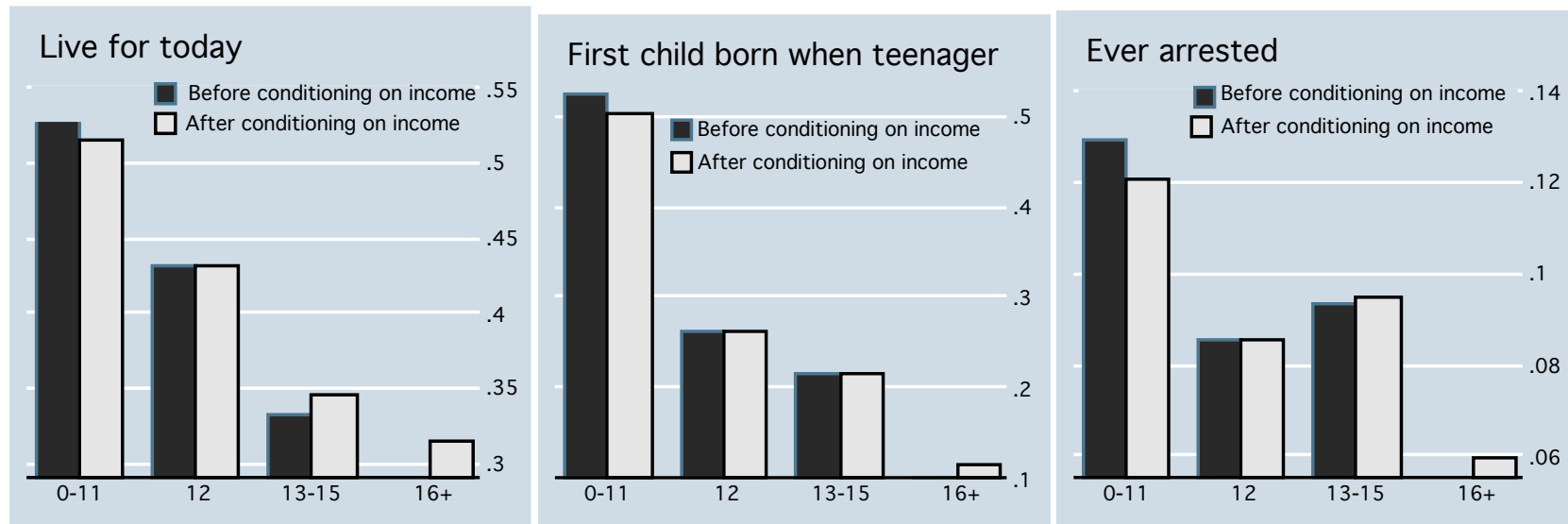
**Notes:** The sample includes 25-45 year-olds from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. Occupational prestige (respondent's occupational prestige score, 1980) ranges from 17 (lowest prestige) to 86 (highest prestige), with a standard deviation of 13.6. Satisfied with job is an indicator variable for whether an individual responds to be fairly, very, or completely satisfied about the job they're in. The sample for these two variables includes only those working. The unemployed sample includes only those either working or looking for work. Before conditioning for income, the outcome variable, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father's education, mother's working status, family's relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor's degree (13-15), and at least a bachelor's degree (16+), with those reporting high school as their highest level of schooling omitted). The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year.

**Figure 4**  
**Measures of Nonmarket Well-being and Years of Completed Schooling**  
**Before and After Conditioning on Income**



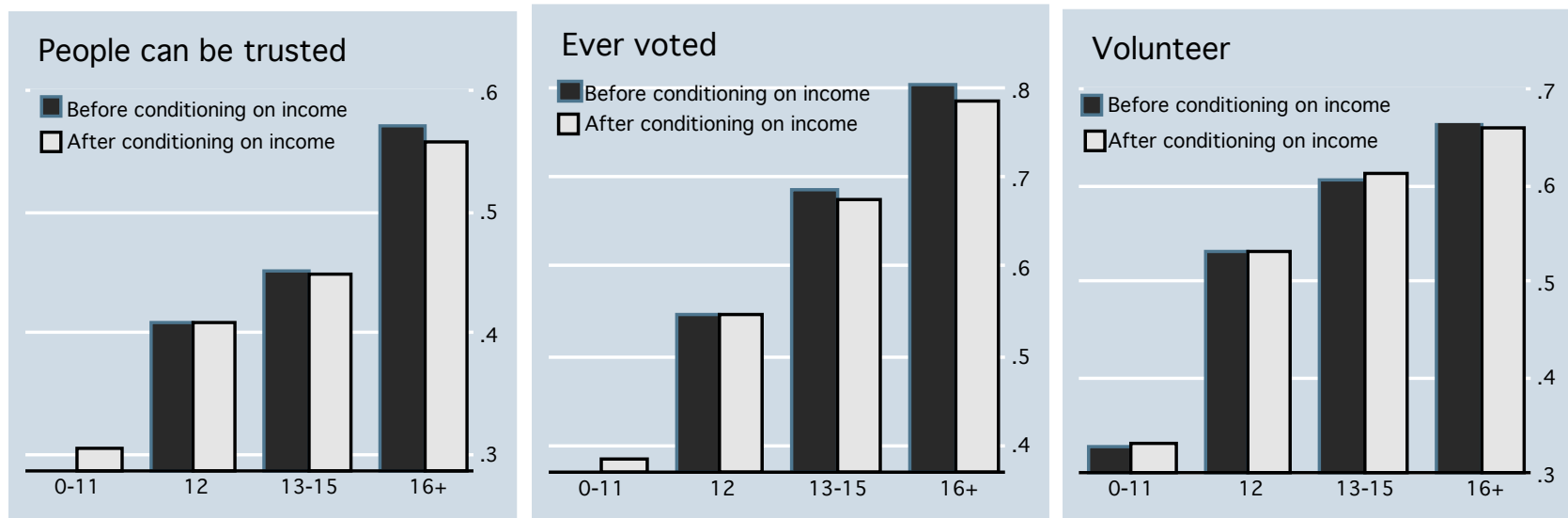
**Notes:** The sample includes all 25-45 year-olds from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. Very good health is an indicator variable for whether an individual responds to be in very good health overall at the time of the interview. Before conditioning for income, the outcome variable, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father’s education, mother’s working status, family’s relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor’s degree (13-15), and at least a bachelor’s degree (16+), with those reporting high school as their highest level of schooling omitted). The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year.

**Figure 5**  
**Measures of Preferences and Years of Completed Schooling**  
**Before and After Conditioning on Income**



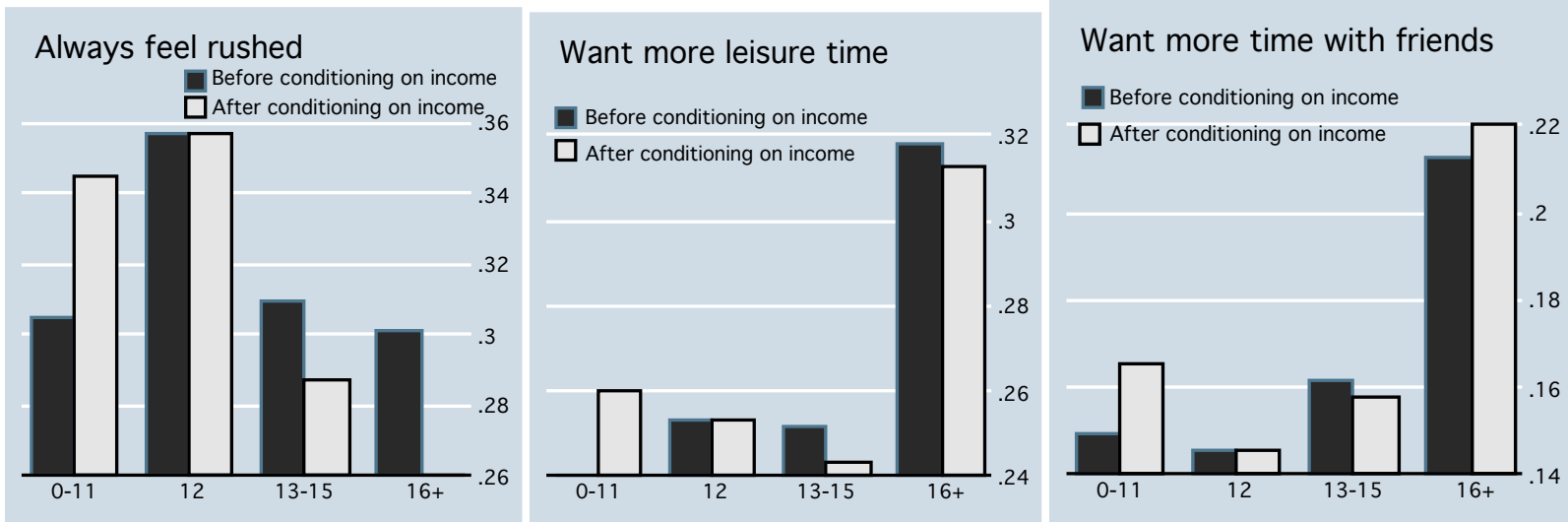
**Notes:** The sample includes all 25-45 year-olds from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. Very good health is an indicator variable for whether an individual responds to be in very good health overall at the time of the interview. Before conditioning for income, the outcome variable, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father’s education, mother’s working status, family’s relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor’s degree (13-15), and at least a bachelor’s degree (16+), with those reporting high school as their highest level of schooling omitted). The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year.

**Figure 6**  
**Measures of Social Participation and Years of Schooling**  
**Before and After Conditioning on Income**



**Notes:** The sample includes all 25-45 year-olds from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. Very good health is an indicator variable for whether an individual responds to be in very good health overall at the time of the interview. Before conditioning for income, the outcome variable, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father's education, mother's working status, family's relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor's degree (13-15), and at least a bachelor's degree (16+), with those reporting high school as their highest level of schooling omitted). The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year.

**Figure 7**  
**Measures of Time Use and Years of Schooling**  
**Before and After Conditioning on Income**



**Notes:** The sample includes all 25-45 year-olds from the 1972-2000 General Social Surveys, born in the United States in 1970 or later. Very good health is an indicator variable for whether an individual responds to be in very good health overall at the time of the interview. Before conditioning for income, the outcome variable, is regressed on age, year, gender, race, state of birth, and year of birth fixed effects, as well as family composition at age 16, mother and father’s education, mother’s working status, family’s relative income at age 16, and the schooling attainment categories (less than high school (0-11), high school (12), some college but no bachelor’s degree (13-15), and at least a bachelor’s degree (16+), with those reporting high school as their highest level of schooling omitted). The coefficients are presented relative to the overall high school graduate mean. The results after conditioning for income include fixed effects for self-reported income categories in each dataset year.

**Table 1**  
**Estimated Effects from Schooling Between Siblings and Twins**  
**Before and After Conditioning on Income**

	[1]	[2]	[3]	[4]	[5]	[6]
		<b>Sibling Sample</b>		<b>Twin Sample</b>		
<b>Outcome</b>	<b>Mean</b>	<b>Change in mean from 1 Year of Schooling Before Inc. Controls</b>	<b>Change in mean from 1 Year of Schooling After Inc. Controls</b>	<b>Change in mean from 1 Year of Comp. Schooling Before Inc. Controls</b>	<b>Change in mean from 1 Year of Comp. Schooling After Inc. Controls</b>	<b>Sample Specification</b>
<b>Log annual income</b>	12.0407	0.0517*** (0.0003)	NA	0.0476*** (0.0026)	NA	Working
<b>Unemployed</b>	.07356	-0.0047*** (0.0001)	NA	-0.0047*** (0.0001)	NA	Full
<b>On welfare</b>	.02332	-0.0034*** (0.0001)	NA	-0.0028*** (0.0005)	NA	Full
<b>Disability pension</b>	.1672	-0.01*** (0.0001)	NA	-0.01*** (0.0007)	-0.004** (0.0007)	55-67 yrs old
<b>Divorced</b>	.11413	-0.0023*** (0.0001)	-0.0017*** (0.0001)	-0.0027* (0.0011)	-0.0027* (0.0011)	Full
<b>Spouse's Years of Schooling</b>	11.8347	0.228*** (0.0019)	0.221*** (0.0019)	0.229*** (0.0173)	0.227*** (0.0175)	Married
<b>First child born when teenager</b>	.11167	-0.0083*** (0.0002)	-0.0071*** (0.0002)	-0.0041*** (0.0011)	-0.0372** (0.0012)	Women

Notes: Unless specified in column 6, all estimates are generated with Norwegian administrative data and include all 28 - 60 year-olds in 2005. The table reports the coefficients corresponding to total years of schooling (mean = 12.0, standard deviation = 3.2), after regressing each outcome on county of current residence and a quartic in age. Results in Columns 3 and 5 are from regressions that also include a fourth order polynomial in log weekly income, and drop those without income. For log annual income obviously only age and county of residence is controlled for. Some results are not applicable (NA) because they involve conditioning on income, when the outcome itself depends on whether someone works or not. Standard errors are clustered by state and year of birth. Single, double, and triple asterisks indicate significance at the 10, 5, and 1 percent level respectively. The full siblings sample size is 1,433,006, and the full twins sample size is 26,056. See text for more details.

**Table 2**  
**Estimated Effects from an Extra Year of Compulsory Schooling**  
**Before and After Conditioning on Income**

<b>Outcome</b>	[1] <b>Mean</b>	[2] <b>Change in mean from 1 Year of Comp. Schooling Before Inc. Controls</b>	[3] <b>Change in mean from 1 Year of Comp. Schooling After Inc. Controls</b>	[4] <b>Sample Specification</b>
<b>Log weekly income</b>	3.06	0.131 [.006]***	NA	Working
<b>Log Occupational Prestige Score</b>	3.27	0.063 [.003]***	0.046 [.003]***	Working
<b>Unemployed</b>	0.045	-0.005 [.002]***	NA	Full
<b>On welfare</b>	0.019	-0.015 [.002]***	NA	Full
<b>In Jail</b>	0.027	-0.006 [.003]*	NA	Black men 21-65 yrs old
<b>In mental institute</b>	0.003	-0.001 [.000]*	NA	Full
<b>Probability of dying within 10 years (^)</b>	0.11	-0.037 [.006]***	NA	14 yrs old in 1914-1939 in 1960-1980 Censuses
<b>First child born when teenager</b>	0.078	-0.008 [.002]***	-0.008 [.002]***	Women
<b>Divorced</b>	0.137	-0.01 [.002]***	-0.005 [.003]***	Full
<b>Oldest child behind grade level</b>	0.199	-0.032 [.004]***	-0.026 [.006]***	Parent matched to oldest 8-16 yr old in household
<b>Voted last national election (validated)</b>	0.49	0.09 [.025]***	0.089 [.027]***	Voting age 1978-2000 November CPS
<b>Satisfied with life overall</b>	0.86	0.048 [.010]***	0.035 [.012]***	25-65 yrs olds born in UK from 1973-98 Eurobarometers

Notes: Unless specified in column 4, all estimates are generated with a combined sample from the 1950 - 2000 U.S. Censuses and 2001 - 2007 American Community Surveys and include all 25 - 65 year-old native-born Americans aged 16 in 1915 or later, with no more than a high school degree. Predicted minimum school leaving ages and exceptions are calculated according to individuals' year aged 16 and state of birth. The table reports the coefficients corresponding to total years of schooling, after regressing each outcome on year of birth, data year, gender, and race fixed effects, a quartic in age, and on total years of schooling, instrumented using the predicted minimum school leaving ages. Results in Column 3 are from regressions that also include a fourth order polynomial in log weekly income, and drop those without income. Some results are not applicable (NA) because they involve conditioning on income, when the outcome itself depends on whether someone works or not. The estimated effects from compulsory schooling on mortality (^) are from Lleras-Muney (2004). Standard errors are clustered by state and year of birth. Single, double, and triple asterisks indicate significance at the 10, 5, and 1 percent level respectively. See text for more details.