Learning to do well or learning to do good? Estimating the effects of schooling on civic engagement, social cohesion, and labor market outcomes in the presence of endowments [☆]

Jason Schnittker [a,c,*], Jere R. Behrman [b,c]

[a] Department of Sociology, University of Pennsylvania, United States
[b] Department of Economics and Sociology, University of Pennsylvania, United States
[c] Population Studies Center, University of Pennsylvania, United States

A R T I C L E   I N F O

A B S T R A C T

Although some point to the large effects of schooling on civic engagement (usually measured in terms of volunteering and participation in civic organizations) and social cohesion (usually measured in terms of social networks and relationship quality), the effects of schooling on social outcomes have not been estimated with the same rigor as the effects of schooling on labor-market outcomes, such as earnings. In particular, previous research has failed to consider (i) the many potential and often unobserved confounding factors ("endowments") influencing both schooling and social outcomes, including family upbringing, innate characteristics, and personality, and (ii) the ways in which schooling pushes individuals in multiple directions simultaneously, including toward greater social engagement, but also toward more independent and market-driven pursuits. Using samples of unrelated persons, ordinary siblings, and identical twins, this study explores the effects of schooling on measures of civic engagement and social relationships, as well as labor-force earnings and labor-force participation. The siblings models reveal a more complex picture than typically suggested by standard individual estimates. On one hand, the results reveal a robust positive effect of schooling on earnings: well-schooled persons work more and earn more, albeit not as much as associations without control for endowments suggest. On the other hand, the results reveal more tenuous and occasionally negative effects of schooling on social outcomes. The effects of schooling on volunteering and membership in civic organizations, for example, disappear almost entirely with control for endowments. Also, within-identical-twins models reverse the positive effects of schooling on reports of support from friends, family, and coworkers. These results may reflect the tension schooling creates between market and non-market commitments, as well as between independence and interpersonal reliability. Schooling may, indeed, induce some pro-social behaviors, but schooling allows individuals choices of whether to pursue more personal interests as well.

1. Introduction

Social scientists are generally very optimistic about the role of education in human and social development. Education is thought to promote a cluster of capabilities necessary for success, including knowledge, self-confidence, and the ability to utilize information effectively (Bowen, 1977; Pallas, 2000) and, for this reason, more education for those who otherwise would be at the low end of the distribution lies at the center of many proposed efforts to reduce poverty (Hauser, 2002). At the same time, education has been linked to a variety of social benefits, which are increasingly emphasized by those interested in promoting civic society. Although the best empirical examples are recent, the roots of the general idea are old. Following Plato's idea of the common good, Durkheim (1956) saw education as the mechanism whereby an essentially asocial person becomes capable of leading a "moral and social" life (p. 72). Likewise, James Mill (cited in Durkheim, 1956) saw education as the mechanism whereby the individual becomes "an instrument of happiness for himself and for his fellows." Contemporary empirical research has verified associations of the schooling component of education with outcomes related to at least some of these claims (see Pallas (2000) for a review).[1]

Yet the claim that education is good for both the individual and society is more complicated than it might seem *prima facie*. First, precisely because the putative benefits of education are broad—cutting across market and non-market domains—they are also potentially in conflict. Education may lead to success in some domains society values, but it is unlikely to do so across all domains in equal measure if, for example, time devoted to activities in one role takes away from time devoted to activities in another (see Jacobs and Gerson (2004) on time and work-family conflict).

Second, even if education can bring happiness to an individual, there is little to guarantee that it will bring happiness to others. Indeed, much of what is thought to make education desirable—perhaps particularly in the United States—is how it allows the individual to form and pursue personal goals and ambitions quite apart from the needs and interests of others (Labaree, 1997). Yet it is rarely acknowledged that what individuals might do in pursuit of this life—whether searching for a better career, a new relationship, or more wealth—could very well have negative implications for others.

Third, it is unclear whether the standard empirical representation of education, schooling, is merely *associated* with these assorted social outcomes or whether it *causes* changes in these outcomes. Although a considerable body of research has interrogated the effects of schooling on market outcomes, especially wages, in light of the putative determinants of schooling (Angrist and Krueger, 1991; Behrman et al., 1980; Card, 1999), the effects of schooling on social outcomes have not been subject to the same level of empirical scrutiny even though many of the same issues apply. For example, there is growing evidence that adult social responsibility has developmental roots, suggesting that much of what constitutes a supportive and extroverted character is determined well before schooling is complete (Graves et al., 1998).

We seek to make the debate over the social and economic effects of schooling more broadly empirical. In particular, this study explores the relationship between schooling and both market outcomes and non-market social outcomes in light of unobserved confounding factors, or, as they are known in some literatures, endowments. Our empirical strategy has three key elements: (i) measuring social outcomes broadly, including civic engagement, varied measures of social relationships, and schooling assortation in marriage; (ii) measuring assorted labor-market outcomes, including earnings, work hours, and basic indicators of work-family conflict; and (iii) dealing more effectively with unobserved confounding variables by using siblings and identical twin pair fixed-effects. The identical twins data allow us to control better for social and genetic endowments than siblings data, which, as we show, are consequential for understanding at least some schooling effects.

2. Background

Although scholars seem generally to agree on the value of schooling, there remain conflicting visions of what schooling should provide, which inform empirical research on what schooling actually does (see Labaree (1997) for a broad discussion). Some scholars understand schooling as a mechanism to prepare students for responsibilities of citizenship and, thus, see liberal arts as a core part of the curriculum; some understand schooling as a mechanism to prepare students to be have more productive roles in society and, thus, emphasize early and deep training in highly specialized vocations; some see schooling as a mechanism for developing general capacities necessary for greater productivity, such that the student and others are better off as a result; and some understand schooling as a commodity, designed to provide individual students with a competitive advantage when seeking the most desirable market opportunities and, thus, emphasize the positional nature of schooling. Although schooling-as-commodity perhaps dominates discussions of the current educational system, all four goals have been part of the educational experience for many years and continue to be part of an ongoing debate (Labaree, 1997).

The coexistence of multiple goals of schooling is reflected in the empirical literature, especially in claims regarding the many consequences of schooling (Wolfe and Haveman, 2003). In particular, it is difficult to separate what scholars think schooling ought to do and what schooling actually does given that not all of the empirical research is equally rigorous, allowing philosophical expectations to favor a causal interpretation when such an interpretation may not be warranted given the evidence. Schooling's relationship with socioeconomic attainment has received the most sustained empirical attention and

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[1] We view education as learning, which can occur through many life experiences, one of which is schooling. Though our primary interest is in education, most empirical work, including the estimates we present below, focuses on schooling. Therefore we consistently refer to schooling, not some broader concept of education, when we are discussing such work.

perhaps also the most rigorous evidence. The basic effect of schooling on socioeconomic attainment is now clear: schooling increases labor-force participation, as well as the likelihood of being employed full-time for long durations. Blau and Duncan (1967) famously link schooling to the status of one's first occupation, while others link schooling to the quality of one's workplace, including supervising others and engaging in non-routine tasks (Ross and Reskin, 1992). Schooling also increases earnings (Sewell and Hauser, 1975) and wealth (Land and Russell, 1996), and schooling's effect on wages may be growing over time (Murnane et al., 1995). In addition to the market-based skills schooling might instill, schooling also puts one in contact with other well-schooled people, leading to the familiar pattern of marital homogamy on the basis of schooling, which itself increases household earnings and opportunities (Mare, 1991).

Schooling has been at least correlated with a host of non-market social outcomes as well, but such outcomes have generally not been a central focus. For example, schooling is associated with more time devoted toward volunteering and civic organizations (Helliwell and Putnam, 1999). Schooling is also associated with having larger and more extensive social networks (Helliwell and Putnam, 1999; House et al., 1988), as well as higher quality relationships, in part reflecting, as before, homogamy on the basis of schooling. More-schooled persons tend to experience less conflict in their relationships or at least smoother resolutions of any conflicts that develop (Bowen, 1977; Schieman, 2000; Thoits, 1984), which is reflected in lower divorce rates (Martin, 2004; Tzeng, 1992). Providing a potential mechanism for many of these findings, some studies report a positive relationship between schooling and pro-social values. Schooling is, for instance, negatively associated with misanthropy and positively associated with valuing autonomy, equity, and liberalism (Davis, 1979; Smith, 1997).

Although these results provide the foundation for those who see schooling as embodying social progress, closer examination reveals some findings that might give pause. Parallel to research on the effects of schooling has been research finding that schooling has an opposing relationship between productivity in the labor market and productivity in other domains. In an examination of the intergenerational effects of schooling using twins, for example, Behrman and Rosenzweig (2002) demonstrate that schooling one generation of women significantly reduces the schooling of their children, which they link to increased labor force participation and less time for parenting among more-schooled mothers. Likewise, other studies indicate that more-schooled persons tend to experience more negative emotional spillovers from work to family, suggesting that work leads to stress at home (Grywacz and Marks, 2000). Even the apparent accentuated advantage resulting from schooling homogamy in marriage may be vulnerable to trade-offs of this sort. While better-schooled persons might, in certain respects, make "better" choices regarding spouses, the most documented intra-family effects of schooling pertain to roles of well-schooled spouses in providing information, advice, and assistance regarding labor-market opportunities, rather than assistance in other domains (see Wolfe and Haveman (2003) for a broad review). Depending on other features of their marriages, then, well-schooled partners might provide an environment especially—and perhaps only—conducive to labor-market attainments, making marital assortment a key element to any study concerned with understanding schooling's effects.

Still other evidence points to the role of schooling in shaping notions of responsibility that would seem to stand in contrast to those discussed above (Markus et al., 2001). This research suggests that schooling may promote social responsibility in general, but the type of responsibility it encourages is hardly selfless in the manner schooling's advocates seem to believe. Markus and colleagues use open-ended interviews to elicit lay conceptions of social responsibility, a method that reveals sharp differences by schooling levels. For those with a college degree, responsibility is associated with avoiding dependence and protecting one's own rights as much as the rights of others. For those with a high school degree, meanwhile, responsibility is associated, first and foremost, with meeting needs and expectations of others. These different conceptions of responsibility, in turn, entail different behaviors. When discussing responsibility, the college-educated invoke notions of "juggling," "balancing," and "taking initiative," whereas those with high school diplomas invoke "adjusting," "coping," and "being dependable." These orientations may have consequences for some social outcomes linked with schooling—they suggest, for example, that well-schooled persons prioritize market activities over other activities—but to date research has not identified well these consequences.

2.1. Methodological concerns with previous research on schooling

Isolating schooling effects is difficult given that schooling lies at the center of a nexus of other influences. There is evidence from the schooling and income literature that, in usual cross-sectional associations, schooling is proxying for aspects of family background influences, at least in part (Behrman et al., 2011; Behrman and Rosenzweig, 1999, 2002; Black et al., 2005; Card, 1999; Plug, 2004) and, for this reason, the magnitude and significance of the association between schooling and wages can change substantially between specifications. The same may apply to the associations between schooling and social outcomes and, indeed, the problem of confounding may be even stronger for social outcomes given possibly greater relevance of background factors.

Along these lines, it is clear that orientations that promote social engagement in adulthood have deep developmental roots. Research has linked adult social relationships to childhood socioeconomic resources (Rossi, 2001), parental affection (Graves et al., 1998), and interpersonal adversity (Mickelson et al., 1997). Some more specific family processes have been proffered for these associations, reflecting the idea that parents provide role models for pro-social behavior. For example, families tend to be either "emotion-coaching" or "emotion-dismissing," with the former leading to more supportive relationships in adulthood than the latter (Gottman, 2001). Likewise, many dispositional factors are related to reports of support and reflect, at least in part, genetic influences. Social support is positively related to social competence (Heller and Lakey, 1985) and negatively related to neuroticism (Watson and Pennebaker, 1989), and both of these factors tend to be relatively stable

traits by early adulthood. If these family and dispositional factors were unrelated to schooling, there would not be an estimation problem, but many of these influences are, in fact, related to schooling attainment. Progress in schooling is associated with family resources and upbringing (Sewell and Hauser, 1975), as well as assorted trait-life factors, including drive (Bowles et al., 2001; Jencks et al., 1979). Schooling is itself at least moderately heritable (Behrman et al., 1980).

At a minimum, this research reveals the possibility of spuriousness, but the problem is more complex in the sense that none of the above influences is related to the putative outcomes of schooling in precisely the same fashion. After accounting for spuriousness, schooling may still have significant effects, but the direction of these effects remains uncertain. Fig. 1 presents a summary of the empirical difficulties. It builds on a simple spurious association scenario in presenting both effects, represented by solid arrows, and associations, represented by dashed lines. The figure also presents assorted other relationships and possibilities related to the net effect of schooling. It is possible, for example, that schooling has negative influences on some social outcomes, especially if we imagine that schooling is related primarily to market productivity and/or if the confounding influences outlined above work in opposing directions.

One method for partially controlling for many confounding influences is to use siblings data and, thus, to exploit differences in schooling attainment among siblings from the same family. Siblings fixed-effects estimates eliminate the influence of characteristics shared by siblings, many of which are unobserved in conventional surveys. This permits considerable control, but ordinary siblings entail other limitations. In particular, fixed-effects among ordinary siblings do not control for factors that differ among siblings, which can be considerable, as ordinary siblings are born at different times and, thus, are exposed to different family circumstances (e.g., regarding parental employment, health, or neighborhood school options). Furthermore, the use of fixed-effects among ordinary siblings does not permit control for differences in individual-specific genetic endowments. The use of fraternal twins overcomes the first problem, as fraternal twins share the same birthday, but it does not overcome the latter, as fraternal twins remain ordinary siblings in every other sense. For both problems, more complete control is possible using identical twins. Identical twins share not only similar upbringings, as do ordinary siblings, and birthdays, as do fraternal twins; they share 100% of their genes at conception, rather than on
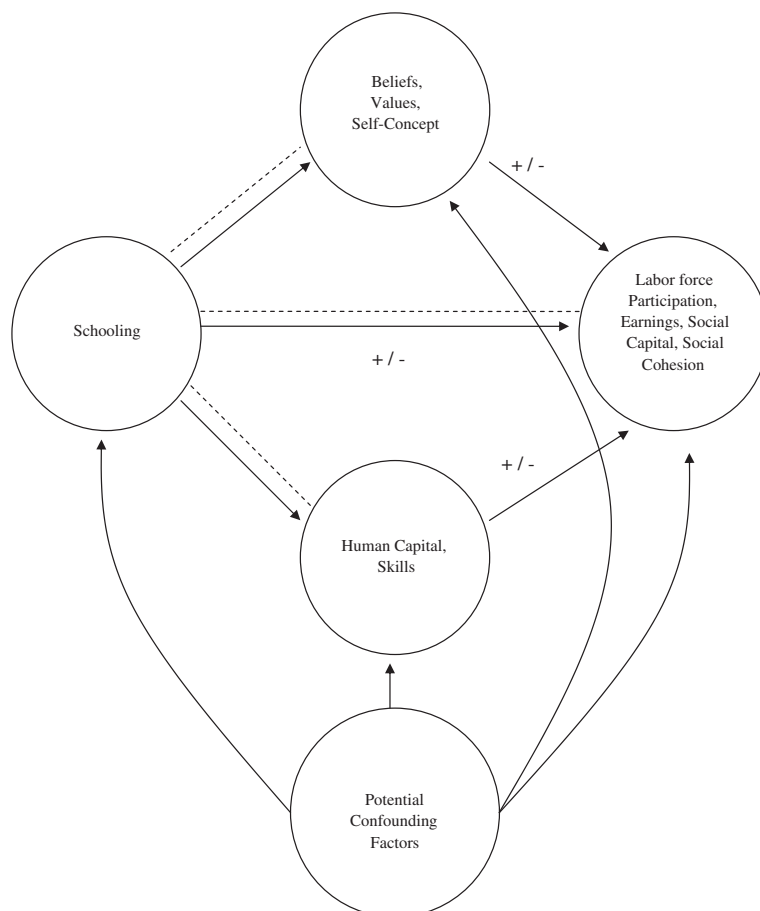

Fig. 1. Conceptual model for understanding the relationships between education and social capital, social cohesion, and earnings.

Table 1
Within and between family variation in schooling: 1995–1996 MIDUS.

	Ordinary sibling sample (%)	Fraternal twin sample (%)	Identical twin sample (%)
Variation within families	31	24	15
Variation between families	69	75	85

average 50%.[2] Below we estimate a series of reduced-form equations in which schooling effects are estimated in samples that provide us with progressively more purchase on background influences: a sample of unrelated persons with no control for unobserved factors,[3] a sample of ordinary siblings with control for common unobserved factors shared by siblings, and a sample of identical twins with control in addition for individual-specific genetics.

3. Data

We use *The National Survey of Midlife Development in the United States* (MIDUS) (Brim et al., 2007). The survey was conducted during 1995 and 1996 under the auspices of the MacArthur Foundation Network on Successful Midlife Development. It contains a main sample of unrelated persons, a siblings sample, and a twins sample. The main sample is a nationally-representative sample of non-institutionalized, English-speaking adults, age 25–74. Phone and mail instruments were administered separately, but this study focuses on respondents who completed the mail instrument, as virtually all our outcome variables appeared on the mail instrument only.

Siblings were identified from members of the main sample. A random subset of siblings was selected from those reporting one or more siblings from the same biological parents. Among the families selected, an attempt was made to interview all biological siblings. Twins were identified in a similar fashion, either within the national sample directly (i.e., they were respondents in the main sample) or within the main-sample respondents' families. About 2% of main-sample respondents were themselves twins, and 11% of the singletons in the sample had a twin in their family. Twins were asked a series of questions designed to evaluate their zygosity, including questions regarding shared physical characteristics. Although physical similarity is by no means a perfect indicator of zygosity, studies reveal a high level of concordance between indicators based on reports of appearance (e.g., whether they look as similar as "two peas in a pod") and indicators based on definitive molecular tests (Chen et al., 1999). After eliminating observations with incomplete information, the final maximum sample consists of 3023 unrelated persons, 1366 siblings (including some in the national sample), and 1588 twins, of whom 954 were fraternal (dyzygotic, DZ) and 634 identical (monozygotic, MZ). In our models, DZ twins are included as ordinary siblings (see below), resulting in a total maximum siblings sample of 2330. The actual siblings sample varies (slightly) among models because some outcomes (e.g., support from coworkers) are limited to those who meet certain criteria (e.g., are employed). Item non-response was very low, accounting for generally less than 2% of cases. Controls for age are included to account for life-cycle differences among ordinary siblings. The Appendix Table A1 presents summary statistics.

3.1. Variables

The key determinant of market and non-market outcomes in this study is schooling, measured in years of completed attainment, ranging from 2 to 20. The siblings design depends on between-siblings differences in schooling. Table 1 presents variation in schooling within and between families for ordinary siblings, fraternal twins, and identical twins. As expected, there is more within-family variation in the sample of ordinary siblings than in the sample of fraternal or identical twins, providing evidence for both family life-cycle differences (otherwise ordinary siblings and fraternal twins would be more similar) and genetic influences (otherwise fraternal and identical twins would be more similar). Among identical twins only 15% of the variation in schooling is within families, resulting in greater imprecision in the within-MZ-twins models than in the other models. In many models, the standard errors for schooling coefficients are quite large.[4] Nevertheless, even in the within-MZ-twins models, many of the coefficients are statistically significant by conventional two-tailed standards ($p < .05$). Moreover, in several cases, the coefficients for schooling are significant *only* in the within-MZ-twins model, as might be possible if, in

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[2] A familiar criticism of twins research is that identical twins share more similar environments than fraternal twins, thereby violating the assumption of equal environments. This issue is important when estimating heritability, but its implications for our study are not negative. We are concerned with eliminating influences of social and genetic endowments from the estimated schooling effects. More similar environments among identical twins than among fraternal twins (or other siblings) increase our ability to control for endowments.

[3] For purposes of comparison, we also report on estimates in which we control for some commonly-used components of family background such as parental schooling attainment.

[4] Within-sibling models also increase the leverage of cases where siblings differ a great deal, such as 10 grades or more of schooling attainment. Although there are only a few such cases in our data, they remain a concern given that we document some reversals in the schooling coefficients between samples. In supplementary models, we re-estimated the within identical-twins models after dropping twins with relatively large schooling differences. Overall, this had little effect on our estimates, although in a few cases it made our results stronger. For example, in Table 3 we report negative effects of schooling on support from family, friends, and coworkers in the within-twins models. After dropping cases with large differences, these coefficients remained negative and significant. The schooling coefficient increased in absolute size from −.095 to −.113 for support from coworkers, whereas coefficients for the other two outcomes decreased by no more than .005.

the other models, the effects of schooling and individual-specific endowments cancel out. Alternative versions of the models also include controls for current household earnings (measured in $10,000), which allow us to evaluate how much of the apparent effect of schooling operate through earnings.

Our social outcome measures are broad, spanning social support, supportive behavior, and civic engagement. Our social support measures were chosen to reflect two key dimensions. *First*, our measures reflect the *sources* from which individuals derive support. In particular, our measures reflect the concentric circles outlined by Antonucci (1994), from close relationships, such as spouse, family, and friends, to role-based relationships, such as coworkers. *Second*, our measures reflect the *content* of supportive exchange, as measured through perceived social support, as well as the frequency of supportive exchange. By examining both dimensions it is possible to evaluate the degree to which the schooling-support association is driven by perceptions of support or behaviors related to support.

3.1.1. Perceived social support

Perceptions of support were measured for four sources: spouses, other family members, friends, and coworkers. For support from families and friends, support was measured using a summary scale, consisting of the mean of sub-indicators for both positive support and strain (after reverse coding). Results from disaggregated measures are reported where appropriate, although not presented in the tables. The items used in the MIDUS instrument were adapted from validated items used in earlier studies (Schuster et al., 1990). The positive support items were: "how much do (members of your family/your friends/partner/spouse) really care about you"; "how much do they understand the way you feel about things"; "how much can you rely on them for help if you have a serious problem"; and "how much can you open up to them if you need to talk about your worries." The strain items were: "how often do (members of your family/your friends/partner/spouse) make too many demands on you"; "how often do they criticize you"; "how often do they let you down when you are counting on them"; and "how often do they get on your nerves." Support from a spouse included these items, as well as four additional items: "how much does he or she appreciate you"; "how much can you relax and be yourself around him or her"; "how often does he or she argue with you"; and "how often does he or she make you feel tense." For each of these items, response categories ranged from "never" to "all the time" on a five-point scale. The summary scales, in general, showed high coefficient reliability. In addition, exploratory factor analysis revealed strong evidence for a single dimension. The inter-item correlations cut across both support and strain items, although the reliability increased consecutively for more definite questions: the Cronbach's alphas were .78 for friend support, .82 for family support, and .92 for spousal support. Only two questions were asked regarding support from coworkers: "how often do you get help and support from your coworkers" and "how often are your coworkers willing to listen to your work-related problems," both of which had five-point response categories ranging from "never" to "all of the time."

3.1.2. Supportive behavior and exchange

Five items were used to evaluate the frequency of supportive behavior and exchange. Respondents were asked, "how often are you in contact with any members of your family/friends, including visits, phone calls, letters, or electronic messages?" Responses on the eight-point scale ranged from "never or hardly ever" to "several times a day." Similarly, respondents were asked two questions about the frequency of reciprocal emotional exchange: "how often do any friends, relatives, or coworkers turn to you for advice or help with a personal or practical problem they have" and "how often do you turn to a friend, relative, or coworker for advice or help with a personal or practical problem you have." In this case, response categories ranged from "never" to "more often than a couple of times a week" on a six-point scale. Finally, respondents were asked, "how often do you and your spouse or partner have a really good talk about something important to you," with responses on the five-point scale ranging from "less than a few times a month" to "at least once a day." These items are especially important if schooling restricts time committed to friendships, even if it does not change evaluations of those friendships.

3.1.3. Civic engagement

A number of questions were asked regarding civic engagement, similar to those used in the social capital literature (see, for example, Putnam, 2000). Respondents were asked how many hours per month they spend doing formal volunteer work in four types of organizations: health-care oriented organizations; school or youth-related groups; political organizations or causes; and any other organization, cause, or charity. In addition, they were asked how many times a month they attend meetings of the following types: religious services; meetings of religious groups, unions or other professional groups; sport or social groups; and any other group, other than those required by work. From responses to these items, four variables were created: whether the respondent volunteered at all; whether they attended any civic group meetings; the total number of hours volunteered per month; and the total number of meetings attended per month. The last two variables were highly skewed and, thus, were recoded as the natural log (plus one to the original count, to preserve observations with zeros).

3.1.4. Earnings, work hours, and negative work-home spillovers

Given the potential importance of earnings as a mediating variable, as well as the possibility of conflict between market and non-market activities, it is important to explore the relationship between schooling and labor market outcomes. To this end, we first explore respondents' personal annual earnings and average work hours per week (among those working for pay). Although wages are ordinarily modeled using a natural log, supplementary analysis revealed a better fit for models that used the level of earnings. Average work hours per week were adjusted to reduce the influence of outliers: the bottom and

top of the distribution were trimmed to 6 and 70 h per week respectively (corresponding to approximately the top and bottom 3%). Models estimated with the adjusted values fit better than those estimated with the original data, which included a few implausibly high hours.

In order to explore the kinds of tradeoffs more schooling might lead to, we also explore negative emotional spillovers between work and home, which provide a potential bridge between our non-market and market outcomes. Negative *home-to-work spillover* was measured using four items: "responsibilities at home reduce the effort you can devote to your job"; "personal or family worries and problems distract you when you are at work"; "activities and chores at home prevent you from getting the amount of sleep you need to do your job well"; and "stress at home makes you irritable at work." Response categories ranged from "all the time" to "never" on a five-point scale and were coded such that higher values reflect more negative spillover. Negative *work-to-home spillover* was measured using a parallel series of items: "your job reduces the effort you can give to activities at home"; "stress at work makes you irritable at home"; "your job makes you feel too tired to do the things that need attention at home"; and "job worries or problems distract you when you are at home."

4. Results

We first test the validity of two features of our siblings models and explore the functional form of the schooling effect.

4.1. Testing two assumptions of siblings models

First, siblings models, of necessity, rely on data from families with more than one child. If there are systematic differences in schooling effects (or their antecedents) between singletons and non-singletons, our estimates will not to be informative about singletons.[5] This might be expected if, for example, families with larger sibships make more constrained economic decisions in light of diminished resources or if dynamics between siblings are important for learning. In supplementary analysis, we estimated multiplicative interactions between schooling and a singleton (non-singleton) dichotomous variable. None of these interactions was statistically significant, confirming that the multivariate associations found in the data are not different for singletons versus others (we are aware of no other study that tests this assumption). *Second*, our models combine DZ twins with ordinary (non-twin) siblings and adjust for age to account for family circumstance differences among siblings. It is possible, however, that DZ twins are different from ordinary siblings. In supplementary models, we estimated sample-specific effects of schooling in a siblings fixed-effects context. We then tested for equality between the ordinary-siblings schooling coefficient and the DZ-twins schooling coefficient. The vast majority of these *t*-tests were insignificant: of the 17 outcomes, only three were significant, and all three differences were positive, suggesting that DZ twins provide additional control on endowments positively correlated with schooling.[6]

4.2. Schooling specification tests

In the models presented below, we estimate a linear term for completed grades of schooling, but other relationships are possible. If we imagine that schooling does little more than certify skill, we would expect "sheepskin" effects, corresponding to bumps in the outcomes for credential-relevant grades, such as 12th. Alternatively we might expect a non-linear relationship between completed grades of schooling and our outcomes, either indicating that only basic literacy matters or that schooling is more important at higher levels (Collins, 1979). Table 2 presents some tests for alternative function forms for schooling for each of the outcomes. The first explores whether the effects of schooling are non-linear in a continuous fashion by adding a schooling-squared term to the baseline equation. The second explores sheepskin effects by including dummy variables for 12 and 16 completed grades of schooling, corresponding to graduation from high school and from a 4-year college or university. The joint significance of these coefficients was evaluated using an *F*-test. As Table 2 makes clear, the effects of schooling rarely depart from a linear specification, and, when they do, these departures never remain significant in the within-MZ-twins model.

4.3. Main results

Tables 3–7 present reduced-form estimates of the impact of schooling on our outcomes first using individual data, then siblings data excluding identical twins, and finally identical twins data, comparing across equations.

Table 3 explores perceived support from family, friends, and coworkers and reveals a theme that continues throughout the remaining tables. The first model reveals coefficients for schooling that are similar to those found in previous research.[7]

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[5] Indeed, many previous twins studies have used data consisting *only* of twins, such that it was impossible to test whether relations differed between singletons and others (Ashenfelter and Krueger, 1994; Behrman and Rosenzweig, 1999, 2002).

[6] The specific outcomes where the DZ-twins coefficient differed from the ordinary-sibling coefficient were support from friends, frequency of contact with family, and frequency of contact with friends. For support from family and contact with friends, the DZ coefficient was between the ordinary-sibling and MZ coefficients. For contact with family, the DZ coefficient was significant and negative, whereas for the other two groups the coefficient was insignificant.

[7] The coefficient for support from family is marginally insignificant ($p = .068$). In the table, it appears statistically significant—the standard error is half the size of the coefficient—only because of rounding.

Table 2
Specification tests for the effects of schooling: 1995–1996 MIDUS.

Outcome	Non-linear effects of schooling		Sheepskin effects	
	Unrelated persons βSchooling^2 = 0	Within identical twins βSchooling^2 = 0	Unrelated persons βHigh School = βFour-Year College = 0	Within identical twins βHigh School = βFour-Year College = 0
Social support				
From family	ns	ns	$p < .01$	ns
From friends	ns	ns	$p < .05$	ns
From coworkers	ns	ns	ns	ns
From spouse	ns	ns	ns	ns
Spouse schooling	$p < .001$	ns	$p < .001$	ns
Frequency of discussion/ contact				
With spouse	ns	ns	ns	ns
With family	$p < .05$	ns	ns	ns
With friends	$p < .05$	ns	ns	ns
Frequency with which				
Others turn to you	$p < .05$	ns	ns	ns
You turn to others	$p < .05$	ns	ns	ns
Any volunteering	ns	ns	ns	ns
ln(total hours volunteering)	ns	ns	ns	ns
Any civic groups	ns	ns	ns	ns
ln(total number of meetings)	ns	ns	ns	ns
Personal earnings	$p < .001$	ns	ns	ns
Average work hours	ns	ns	ns	$p < .05$
Work to home spillover	ns	ns	$p < .05$	ns
Home to work spillover	ns	ns	ns	ns

Note: All tests based on equations also containing a coefficient for completed grades of schooling. Tests for the non-linear effects of schooling based on a coefficient t-test. Tests for sheepskin effects based on an F-test of joint significance.

Table 3
Coefficients from regressions of social support from three sources: 1995–1996 MIDUS.

	Among unrelated persons		Within siblings	Within Identical Twins
From family				
Schooling	0.006 (0.003)	0.005 (0.004)	−0.005 (0.006)	−0.045** (0.017)
Household earnings		0.004 (0.003)		
Individuals	2978	2978	2307	624
Families			977	312
From friends				
Schooling	0.009** (0.003)	0.005 (0.003)	0.010 (0.006)	−0.041* (0.017)
Household earnings		0.010** (0.003)		
Individuals	2983	2983	2311	622
Families			980	311
From coworkers				
Schooling	0.015* (0.007)	0.006 (0.007)	0.009 (0.015)	−0.095* (0.042)
Household earnings		0.023** (0.006)		
Individuals	1908	1908	1204	354
Families			533	177

Note: Except for the within identical twin models, all models also include controls for age and gender (coefficients not shown). Standard errors in parentheses.
* $p < .05$.
** $p < .01$.

Schooling is either positively associated with support, as in the case of support from friends and from coworkers, or has a weak and insignificant relationship with support, as in the case of support from family members (a result echoed in the spousal support models, to which we turn shortly). The second model introduces controls for family earnings. Both of the coefficients that were significant in the first model are no longer significant in the second, suggesting that much of the positive association between schooling and support reflects the role of schooling in increasing earnings (as well as spouse selection, given that we are exploring household earnings).

But the next two models reveal very different patterns. Controls for siblings fixed-effects do little to change the magnitude of schooling coefficients in two cases, though the estimates are insignificant; in the third case the estimate is negative,

Table 4
Coefficients from regression of spouse schooling and spouse support variables: 1995–1996 MIDUS.

	Spouse schooling			Support from spouse			
	Among unrelated persons	Within siblings	Within identical twins	Among unrelated persons	Among unrelated persons	Within siblings	Within identical twins
Schooling	0.504** (0.017)	0.356** (0.037)	0.344** (0.108)	−0.007 (0.021)	−0.006 (0.021)	−0.036 (0.037)	0.186* (0.084)
Spouse schooling				0.022 (0.022)	0.023 (0.022)	−0.009 (0.036)	0.173* (0.078)
Schooling × Spouse schooling				0.000 (0.001)	−0.000 (0.001)	0.001 (0.002)	−0.012* (0.005)
Household earnings					0.008* (0.004)		
Individuals	2170	1604	432	2114	2114	1576	428
Families		698	216			685	214

Note: Except for the within identical twin models, all models also include controls for age and gender (coefficients not shown). Standard errors in parentheses.
* p < .05.
** p < .01.

Table 5
Schooling coefficients from regressions of supportive behavior and exchange variables: 1995–1996 MIDUS.

	Among unrelated persons	Within siblings	Within identical twins
Frequency of discussion/contact with			
Spouse			
Schooling	0.011 (0.011)	−0.027 (0.020)	0.046 (0.062)
Individuals	2108	1568	426
Families		681	213
Family			
Schooling	−0.042** (0.011)	−0.026 (0.018)	−0.058 (0.048)
Individuals	2949	2271	612
Families		964	306
Friends			
Schooling	0.046** (0.012)	0.043 (0.023)	0.050 (0.066)
Individuals	2969	2292	612
Families		972	306
Frequency with which			
Others turn to you			
Schooling	−0.064** (0.009)	−0.079** (0.018)	−0.107* (0.048)
Individuals	2964	2276	616
Families		965	308
You turn to others			
Schooling	−0.057** (0.008)	−0.047** (0.015)	−0.089* (0.044)
Individuals	2964	2290	622
Families		969	311

Note: Except for the within identical twin models, all models also include controls for age and gender (coefficients not shown). Standard errors in parentheses.
* p < .05.
** p < .01.

though also insignificant. Controls for identical-twins fixed-effects, however, reverse the coefficients' signs and retain statistical significance. In each case, this negative coefficient exceeds in magnitude the positive coefficient found in the first model. For purposes of evaluating magnitudes, a useful comparison is provided by the female–male difference (estimated as a female coefficient in the first model, but results not shown), which is .05 for family support, .12 for friend support, and .78 for coworker support. Previous research demonstrates that sex differences in social support are among the largest social differentials (see Turner and Marino (1994), especially Table 6 for general evidence). The negative effects of schooling are comparable: the difference entailed by an additional completed grade of schooling exceeds the male–female difference for coworker support and essentially matches the difference for family support. In the case of friend support, three additional completed grades of schooling are sufficient to exceed the male–female difference. That the findings in Table 3 are largely consistent for each of the three sources of support suggests a general process that cuts across roles and relationships.

Table 4 explores both the determinants and consequences of spousal schooling and of schooling homogamy. The analysis of spousal support presents unique opportunities. Unlike for friends, family, and coworkers, we have some information on spouses. Most notably, we have their schooling, which allows us to estimate the influence of spousal schooling and schooling

Table 6
Schooling coefficients from regression of civic engagement variables: 1995–1996 MIDUS.

	Among unrelated persons	Within siblings	Within identical twins
Any volunteering			
Schooling	0.193** (0.015)	0.161** (0.032)	0.084 (0.100)
ln(total hours volunteering)			
Schooling	0.097** (0.008)	0.092** (0.017)	0.015 (0.042)
Any civic groups			
Schooling	0.172** (0.017)	0.208** (0.042)	−0.032 (0.113)
ln(total number of meetings)			
Schooling	0.073** (0.007)	0.065** (0.012)	0.009 (0.036)
Individuals	3030	2334	632
Families		990	316

Note: Except for the within identical twin models, all models also include controls for age and gender (coefficients not shown). Standard errors in parentheses.
* $p < .05$.
** $p < .01$.

Table 7
Schooling coefficients from regression of work, earnings, and negative work-home spillovers: 1995–1996 MIDUS.

	Among unrelated persons	Within siblings	Within identical twins
Personal earnings			
Schooling	0.271** (0.013)	0.188** (0.026)	0.141** (0.052)
	3030	2334	632
		990	316
Average work hours			
Schooling	0.234* (0.101)	0.361 (0.212)	0.597 (0.469)
	2270	1554	436
		680	218
Work to home spillover			
Schooling	0.036** (0.006)	0.038** (0.012)	0.072* (0.028)
Home to work spillover			
Schooling	0.011* (0.005)	0.005 (0.010)	0.039 (0.026)
Individuals	2145	1440	422
Families		629	211

Note: Except for the within identical twin models, all models also include controls for age and gender (coefficients not shown). Standard errors in parentheses.
* $p < .05$.
** $p < .01$.

homogamy on reports of support. The models predicting spousal support include own schooling, spousal schooling, and multiplicative interactions between the two. If the interactions have coefficients of the opposite sign from that for spousal schooling, then the direct linear effect of spousal schooling is offset with greater schooling homogamy. The table begins with spousal schooling as the outcome. The relationship between one's own schooling and the schooling of one's spouse is positively significant within the sample of unrelated persons, suggesting that for every additional single grade of own schooling, one's spouse has an additional half grade of schooling. If the true causal effect of own schooling on one's spouse's schooling is given by the within-MZ-twins estimates, then one's spouse has about a third more grade of schooling for every additional grade of own schooling. The cross-sectional estimate overstates this effect by about 50%.

The remaining models explore spousal support as a function of own schooling and spouse schooling. The first equation suggests no significant relationship, either between own schooling and spousal support or between spousal schooling and spousal support. The within-siblings model also reveals no significant relationship. However, the within-identical-twins model reveals a positive relationship between support and both own schooling and spousal schooling, and a negative interaction between the two. As revealed in the next two tables, part of this interaction may reflect how schooling promotes greater independence and stronger commitments to work.

Table 5 explores supportive behavior and exchange, and reveals two key findings. *First*, it does not seem to be the case that schooling decreases perceived support simply because it reduces the frequency of contact with others. The coefficients in the frequency of discussion/contact equations are estimated with imprecision, but even so they reveal no clear pattern. If anything, more schooling reduces contact with family members, while increasing contact with friends and one's spouse, but none of these coefficients is significant in within-identical-twins models. The bottom section of Table 5 explores reciprocal help-seeking. Recall that the questions ask about the frequency with which "you turn to others" and "others turn to you" regarding a "personal or practical problem." Here, the within-identical-twins models reveal a stronger negative coefficient

than that found in the sample of unrelated persons. Schooling decreases the frequency with which individuals turn to others, as well as the frequency with which others turn to them.

Table 6 explores civic engagement. The first column of coefficients suggests that, under the usual interpretation, schooling increases the likelihood and extent of civic engagement. These coefficients are robust to controls for the kinds of social and genetic endowments captured by the within-ordinary-siblings models; however, they are not robust to controls for genetic-specific endowments. The third column reveals coefficients that are both statistically insignificant and a good deal smaller than the coefficients in the first column.

Table 7 explores labor-market effects and negative spillovers between work and home. Assuming the within-MZ estimate is correct, the estimate found among unrelated persons is inflated by as much as 92%.[8] Nevertheless, the coefficient remains significant in the within-identical-twins estimates, suggesting real returns of schooling to earnings. With respect to work hours, the schooling coefficient for the sample of unrelated persons is significantly positive, but, in this case, the schooling coefficient becomes insignificant in the within-siblings and within-MZ-twins models. The absolute magnitude of the coefficient, however, increases in size with the addition of more rigorous controls, suggesting that were it not for measurement error (see Section 5), the coefficients might be statistically significant and numerically large relative to the coefficients presented in the first column.

The items related to negative spillovers are consistent with the idea that schooling increases labor-force commitment. In general, more schooling leads to more negative spillover between work and home. Furthermore, as with supportive exchange, the coefficient found when using within-MZ-twins fixed-effects is larger than that found using unrelated persons. Nevertheless, only the coefficient for work-to-home spillover remains significant, suggesting that among the well-schooled, the resolution of strains in work-family balance may tilt toward compromising family more than work. In combination with the preceding tables, Table 7 suggests that schooling has a stronger relationship with market engagement relative to non-market engagement.

A clear implication across the tables is that siblings fixed-effects, and particularly identical-twins fixed-effects, capture a great deal of unobserved heterogeneity, so that estimates from these models often differ substantially from estimates from unrelated individuals. Our estimates for unrelated individuals, however, control only for age and gender, which are minimal controls. This raises the question of how much of the gap between these estimates and our within-siblings (identical twins) estimates is eliminated with the types of parental family background controls that are usually available and routinely used in cross-sectional studies. Some recent estimates suggest that the parental variables often used to represent the impacts of parents in childhood (e.g., parental schooling attainment, parental occupation) account for a small portion of the family fixed-effects that are controlled for in siblings and MZ estimates (Behrman and Rosenzweig, 2004). In supplementary models, we introduced controls in the unrelated sample for the highest schooling of the respondents' parents and whether they reported ever receiving Aid for Dependent Children during their childhood or adolescence. In most cases one or both of these coefficients was significant, but these models provided only partial control. The schooling coefficient was generally intermediate between the unrelated sample estimates and the within-ordinary-siblings models, but closer to the ordinary-siblings estimates than the MZ-twins estimates, revealing only very partial control for unobserved heterogeneities.

5. Discussion

Although scholars, policy-makers and others regularly promote schooling as a key determinant of civic engagement and social cohesion, the nature of this relationship is in doubt. Schooling decisions are endogenously determined, and many of the factors that influence schooling may influence social outcomes as well. In an attempt to provide better estimates, this study utilized data from unrelated persons, ordinary siblings, and identical twins. The results reveal that estimates of schooling impacts on social outcomes depend a great deal on the degree to which analysts can control for confounding factors. The relationship between schooling and civic engagement disappears entirely with controls for social and genetic endowments, while the relationship between schooling and income is not as sensitive.

The relationship between schooling and social support is more complicated, but, like the relationship between schooling and civic engagement, is quite different from the interpretations provided in previous research. Whereas the schooling-social support cross-sectional association is positive and significant, it is generally reduced a great deal with simple controls for family earnings, which suggests that schooling promotes social support first and foremost by improving socioeconomic resources. Yet herein are the beginnings of a more complex story—higher income does not come without a cost, and the causal effects of schooling are not the same thing as its correlates. The within-identical-twins models reveal negative relationships between schooling and many indicators of social support, and other models shed additional light on these findings. For one, they reveal that more schooling decreases the amount of emotional support one seeks, as well as the amount of support others seek from you. Furthermore, more schooling increases negative spillovers from work to home but not from home to work, suggesting that schooling leads to a greater emphasis on labor-market investments than non-market opportunities. If higher income allows individuals to sustain a more supportive interpersonal environment, it also may increase possibilities of conflict, as some of the same traits that increase labor market engagement (e.g., ambition, drive, and independence) likely also

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[8] Substantial declines in schooling coefficients, such as those reported here, are reported in most twins studies concerned with earnings, though with variation in extent across studies, as would be expected with variations in labor and schooling markets across time and space (see Behrman and Rosenzweig (1999) for a survey).

decrease the likelihood of harmonious interpersonal relationships (see Riehl (2001) for a review). Although there may be more particular mechanisms for certain associations—among family members, for example, schooling might increase conflict by inflating generational differences—the negative effects of schooling are found across relationship types.

There is one exception in this regard: spousal support. In this case, the results reveal the potential for a positive effect, but also the conditions for its undermining. Having a more-schooled spouse tends to increase support, but more schooling also increases the likelihood of marrying someone who is also more-schooled, and the within-identical-twins models reveal that schooling homogamy undercuts the benefits of personal and spousal schooling somewhat. That is, the main effects of spouse and own schooling are both positive, but the interaction between the two is negative. Since most persons with high levels of schooling marry other persons with high levels of schooling, the average sample-wide effect of schooling on spousal support is small (see also Glenn and Weaver (1978) and Campbell et al. (1976) on a small or negative average effect). This result, too, is consistent with the idea that schooling promotes labor-market productivity at the expense of social relationships, even, in this case, close ones.

Beyond these specific findings, the results have a number of general implications. For one, they reveal that current reports of civic engagement and social support reflect processes long in the making. Our study treats these influences as a nuisance and does not identify what they are.[9] Nevertheless, future research should be more sensitive to the influence of confounding variables, whether they are observed or not. Here, too, it is not clear what these unique genetic-specific endowments are, and, in general, these may be more difficult to identify than other family endowments. Previous research does, however, suggest several possibilities. It may be the case that once the coefficients for schooling are shorn of endowments related to, for example, extraversion, the need to please, or emotional intelligence, schooling promotes traits that cut against civic engagement and social support, such as ambition, self-interest, or high opportunity costs of time.

5.1. Limitations and external validity

Although siblings models are useful in the context of the schooling literature, this method is not without limitations, especially in the case of identical twins (Bound and Solon, 1999; Griliches, 1979). It is popular to think of the siblings design as a means of controlling for unobserved components of family background and, thus, to exploit between-siblings (twins) differences, but siblings are not, of course, randomly assigned to different schooling attainment. The key question is whether the forces determining this differentiation are also related to our outcomes, as might be the case, for example, with an especially influential mentor who both provides support that enhances labor-market skills and encourages further schooling attainment, but only for one twin. In this case, within-MZ-twins estimates still overstate the impact of schooling on earnings, but the opposite also may occur as well when, for example, an especially influential friend discourages schooling but encourages the development of social skills. In this case, within-MZ-twins estimates understate the impact of schooling on social interactions. We have no means of evaluating these alternate possibilities, although we believe they are less consequential than the many things that within-MZ-models do effectively control for, as demonstrated in our results.

It is also worth emphasizing that within-siblings estimates compound problems with measurement error, as they filter out much of the true signal of the independent variables without also filtering out measurement error (Bishop, 1977; Griliches, 1979). Although schooling would seem rather straightforward in its reporting, there remains considerable measurement error (Bound and Solon, 1999). Random measurement error of the magnitude reported in previous studies might account for some of the between-sample differences, but it could not account for all of them. It could not account for differences, for example, between the within-identical-twins models versus the within-siblings models or the reversed signs that occur in some cases. Nor could random measurement error account for the increases in the absolute magnitude of some coefficients between the unrelated-persons samples and the within-identical twins samples, as is the case for the estimated schooling coefficients for the frequency with which others turn to you in Table 5. Nor could random measurement error account for the drops in magnitudes of some coefficients between the unrelated persons samples and the within-identical twins samples, such as those of over 80% for total hours volunteering and total number of meetings in Table 6.[10]

Even if we could estimate the impact of schooling perfectly, there would still be some limitations to our design. The average effect of schooling on wages and civic engagement, for example, reflects a good deal of complexity and context, including the lessons schooling instills. If, on one hand, the prevailing curriculum is geared toward labor-market activities, schooling's influence may tilt toward conventional wage-labor productivity. If, on the other hand, a substantial fraction of the curriculum is devoted toward social skills, schooling might encourage relatively more non-market productivity. So, too, are there

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[9] Identifying what these factors are may be challenging. As discussed at the end of our results section, some recent estimates and our estimates suggest that parental variables routinely included in analyses to represent the influence of parents in childhood account for a small portion of the family fixed-effects that are controlled for in within-siblings and within-MZ-twins estimates. This, then, presents a challenge for theory as much as for empiricism.

[10] The asymptotic bias for individual estimates due to random measurement error in schooling with a noise-to-signal variance ratio of "s" is $1/(1 + s)$ and the asymptotic bias due to the same measurement error "s" for within-sibling estimates for whom the true schooling correlation is "r" is $s/((1 + s)(1 - r))$ (e.g. Behrman et al., 1980, pp. 82–83). If the noise-to-signal ratio "s" is 0.10 (approximately the value in most of the studies referred to in Bound and Solon, 1999) and the true sibling schooling correlation "r" is 0.50 (the within-DZtwins schooling correlation in the data that we use), the downward bias in the individual estimates due to random measurement error in schooling is 9.1% and the downward bias in the within-sibling or within-MZ-twins estimates due to random measurement error in schooling is 18%. So, under these assumptions, a decline in the absolute value of estimated schooling coefficients of about 9% would be due to exacerbation of biases due to random measurement error in the within-sibling estimates, but in many cases we find much larger changes, suggesting that unobserved factors are also playing a role, and not just random measurement error.

differences between countries in the average effects of schooling, especially insofar as schooling is designed to integrate an individual into a particular economy, culture, and ethos, such that social participation is more or less important. Likewise, the effects of schooling depend on prevailing labor market conditions and policies, which in turn vary over time and space. In a highly competitive market, for example, trade-offs between social engagement and personal income might be greater than they would be in a more protected market, but these trade-offs might be eased when employees are offered a flexible work schedule or more casual supervision.

The results are also indeterminate with respect to the mechanisms linking schooling to our outcomes, especially in the case of social support. Some of our support-related outcomes reflect both the content of support and the evaluation of support and, thus, reflect some combination of influences we do not observe. This elision leaves the results open to multiple interpretations. It is possible, for example, that more-schooled persons have higher expectations regarding support and, thus, are more likely to be frustrated with the support they receive, but it is also possible that more-schooled persons are more assertive and less willing to compromise. Certain features of the analysis speak to at least some of these issues. Recall that more schooling decreases the likelihood of turning to others, as well as the likelihood of others turning to you. If more schooling increases independence both from the perspective of providers and recipients of support, it seems likely that the strain more-schooled respondents report is induced more by providing too much support than by not receiving enough. A related limitation stems from uncertainty surrounding the specific relationships being considered. Reports of spousal support reflect a specific person about whom we have some information, but the remaining reports reflect, in most cases, a plurality of persons culled from a variety of sources. It is unclear, then, whether schooling exerts more influence on the selection of partners, the negotiation of an ongoing relationship, or the interpretation of supportive behavior.

5.2. Conclusion

Despite these limitations, we think our evidence pushes research in several new directions, especially with respect to appreciating the entire field of schooling's influence. We also think our evidence encourages greater use of siblings data, particularly identical twins. Siblings data are used occasionally in sociology, and often to great effect (Conley et al., 2006a,b), but samples of unrelated persons are far more common. For a number of reasons, we believe the value of twins data is particularly high. For one, the analyst's ability to control for all the relevant confounding variables is limited, especially perhaps in the schooling literature but elsewhere as well. Furthermore, the theoretical literature is developing at a pace that perhaps surpasses the capacity of researchers to collect all the relevant information in new surveys. The value of twins data is also high given the schooling literature's penchant for bold claims. Although the schooling-as-panacea view coheres with the progressive leanings of social science, the specific claims they entail need to be evaluated with empirical rigor, as a more rigorous evaluation could reveal precisely what schooling does. To date, most of the discussion on this topic, although voluminous, has been speculative (see, for example, Wilson, 2002).

Appendix A

Table A1
Descriptive Statistics: 1995-1996 MIDUS.

	Range	Mean	Standard Deviation
Years of Schooling	2 - 20	13.65	2.70
Support			
From Family	1 - 4	3.15	.51
From Friends	1 - 4	3.14	.45
From Coworkers	1 - 5	3.65	.78
From Spouse	1 - 4	3.16	.55
Spouse Schooling	2 - 20	13.55	2.59
Frequency of discussion/contact			
With Spouse	1 – 5	3.24	1.33
With Family	1 – 8	5.84	1.56
With Friends	1 – 8	5.65	1.71
Frequency with which			
Others Turn to You	1 – 6	3.47	1.40
You Turn to Others	1 – 6	4.30	1.17
Any Volunteering	0 - 1	.40	
ln(Total Hours Volunteering)	0 - 6.03	.90	1.25
Any Civic Groups	0 - 1	.72	.45
ln(Total Number of Meetings)	0 - 3.43	1.38	1.04
Personal Earnings (Annual earnings/10,000)	0 - 9.1	2.41	2.24
Average Work Hours	6 - 70	40.99	13.21
Work to Home Spillover	1 - 5	2.62	.74
Home to Work Spillover	1 - 5	2.08	.66

Note: All descriptive statistics taken from sample of unrelated persons.

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