THE EFFECTS OF CATHOLIC SCHOOLING ON CIVIC PARTICIPATION

Thomas S. Dee
Swarthmore College
dee@swarthmore.edu

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INTRODUCTION

The nature of government involvement in elementary and secondary education differs markedly across nations. For example, in several developed countries (e.g., Australia, France, Germany and Spain), the government provides a considerable amount of financial support to private schools (Kober 1999). However, the United States has an extensive network of publicly financed and managed schools and provides almost no financial support to private schools. The relatively unique institutional arrangement in the United States has its origins in the dramatic 19th century expansion of universal, public schooling. In particular, one of the fundamental motivations for public schooling during that period was the concern that publicly managed schools were necessary to assimilate largely Catholic waves of immigrants into the civic culture of the United States. The contemporary debate over educational choice (in particular, publicly financed vouchers for private schools) has turned in part on similar claims. One of the most fundamental justifications for the status quo (i.e., the almost exclusively public production of education) is the hypothesis that the regulation of private schools cannot adequately ensure that the desired social benefits of schooling will be produced (Poterba 1996).1 In particular, there is concern that private schools may focus on producing skills and knowledge with clear individual benefits and place less value on the external social benefits that are derived from instilling a variety of civic values and beliefs (e.g., Levin 1991, Rosen 2002, page 70). Similarly, the sectarian and possibly segregated nature of private schooling could also harm future civic engagement (e.g., Levin 1991, 1998). However, since private schools may be more efficient at producing human capital and may simultaneously increase student exposure to social capital (e.g., shared norms, social cohesion and trust), it is also possible that they are more effective than public schools at promoting civic engagement (Bryk, Lee and Holland 1993, Campbell 2001).

The relative effects of private schooling on individual student achievement (i.e., test scores, educational attainment) have received considerable attention. Yet, surprisingly, there is relatively little evidence on how private and public schools compare with regard to the promotion of civic engagement, another central goal of schooling. And the limited evidence that does exist generally suggests that private schools (Catholic schools, in particular) are often better at promoting civic participation and political tolerance than public schools (e.g., Greeley and Rossi 1966, Greene 1998, Campbell 2001, Wolf et al. 2001). However, the contemporary empirical evidence is based on youth, not adult, behaviors. Furthermore, a widely appreciated problem from empirical studies of student achievement is that conventional inferences about the effectiveness of private school may be contaminated by selection biases. In particular, since students whose families choose to send them to Catholic schools may have an unobserved propensity for civic participation, these results may overstate the true civic returns to Catholic schooling.

In this study, I present new empirical evidence on these issues by evaluating the relative effects of Catholic schooling on civic participation as an adult. This evidence is based on two nationally representative longitudinal surveys conducted by the U.S. Department of Education: High School and Beyond (HS&B) and the National Education Longitudinal Study of 1988 (NELS88). These surveys provide richly detailed information on the backgrounds of students and also included follow-up interviews with questions about adult civic participation. I find that students who attended Catholic school in 10th grade were substantially more likely to vote as adults and that this relationship is generally robust to conditioning on a wide variety of observed demographic, socioeconomic and attitudinal measures. I then attempt to assess whether these results reflect uncorrected selection biases by exploiting three of the instrumental variables that have been employed in recent studies of other youth outcomes. These candidate instruments for attendance at a Catholic school include Catholic religious affiliation (Evans and Schwab 1995, Neal 1997), the local density of Catholic high schools (Neal 1997, Grogger and Neal

2000) and the availability of mass transit (Figlio and Ludwig 2000). I find that bivariate-probit and 2SLS estimates based on these instruments suggest that Catholic schooling has even larger effects on voter turnout and possibly negative effects on volunteering. However, I then assess whether these instruments provide a valid basis for identifying the effects of Catholic schooling using an approach introduced by Altonji, Elder and Taber (2002). The results suggest that system estimates based on these instruments may often be plagued by large, confounding biases. I conclude that, though there is a robust partial correlation between Catholic schooling and adult civic engagement, we cannot completely dismiss the possibility that this reflects selection biases. Nonetheless, the lack of clear evidence that Catholic schools are inferior at promoting adult civic participation does raise questions about the effectiveness of public schools at achieving one of their most fundamental goals.

THE CONSEQUENCES OF CATHOLIC SCHOOLING

In a widely discussed and controversial analysis of data from High School and Beyond (HS&B), Coleman, Hoffer and Kilgore (1982) presented evidence that, conditional on background measures, students who attended Catholic schools had higher test scores than students attending public schools. The authors attributed the relative success of Catholic schools to school-specific traits like higher academic expectations and stricter discipline. However, critics suggested that apparent effectiveness of Catholic schools merely reflected the fact that students with higher but unobserved propensities for academic achievement were more likely to attend Catholic schools (e.g., Cain and Goldberger 1982, Murnane et al. 1985). More recent studies have examined the effects of Catholic schooling on educational attainment instead of test scores and have also attempted to correct for possible selection biases. For example, Evans and Schwab (1995) find that the HS&B respondents attending Catholic schools were substantially more likely to graduate from high school and to enter college (approximately 13 percentage points). They present evidence that these results are not contaminated by selection bias by relying on two variables that may generate exogenous variation in Catholic-school attendance: whether the student is Catholic and the county-level percent of Catholics. Neal (1997) also concludes that Catholic schooling appears to have causal effects on educational attainment and wages, particularly for urban minorities.2 His assessment of possible selection biases turns on similar instrumental variables: Catholic religious affiliation, the county-level percent Catholic and the county-level density of Catholic high schools. In another recent contribution, Figlio and Ludwig (2000), relying on measures of the size of the local mass-transit system as an instrument for the probability of attending Catholic schools, conclude that Catholic schooling may reduce some risky youth behaviors (e.g., sexual activity, drug use).

The most fundamental concern with these results is that they may overstate the returns to Catholic schooling because the instrumental variables (i.e., Catholic religious affiliation, measures of the local availability of Catholic schools) influence student outcomes as well as school choice. Altonji, Elder and Taber (2002) address this concern in the context of student achievement and conclude that Catholic religious affiliation and measures of the proximity of Catholic schools do not provide a valid basis for identification. Their conclusions are based on two types of evidence. First, they assess the amount of bias that might be due to contaminated instruments by using the relationship between the instruments and the observed determinants of student achievement ("selection on observables") as a guide to how the instrument varies with the unobserved determinants of student achievement ("selection on unobservables"). Second, they estimate the amount of bias in 2SLS estimates by examining the reduced-form effects of the instruments on a sub-sample of respondents for whom the availability of Catholic schools is largely irrelevant: those who attended 8th grade in a public school.3 They find that instruments for Catholic schooling often appear to have large effects on academic outcomes among these students, which clearly suggests that the exclusion restrictions

are invalid. However, in an earlier, related study, Altonji, Taber and Elder (2000) also show that, to eliminate the apparent effect of Catholic schooling on high school graduation, the degree of selection into Catholic schools on unobservables would have to be several times larger than the degree of selection on observables. Since that seems highly unlikely, they conclude that the apparent effects of Catholic schooling on high school graduation (and, to a lesser degree, college entrance) are largely real.4

Though the promotion of civic engagement is widely viewed as one of the fundamental goals of schooling, there is surprisingly little evidence on the comparative effects of public and Catholic schools. The limited empirical evidence that is available actually suggests that Catholic schools may be relatively effective at promoting civic engagement. For example, in an early empirical study, Greeley and Rossi (1966) found that Catholics who attended Catholic schools, particularly those from more recent birth cohorts, were more tolerant of civil liberties than Catholics who did not. They also found that a Catholic education was unrelated to community involvement and attitudes towards other groups. In a more recent study, Greene (1998) finds that 12th graders in private schools (which are predominantly Catholic) have higher levels of racial tolerance, volunteering and commitment to community than those in public schools.5 Campbell (2001) also analyses recent survey data and finds that, relative to public school students, those in Catholic high schools have higher levels of civic participation and exhibit better civic attitudes and knowledge. His analysis is based on the 4,213 high school students who participated in the 1996 National Household Education Survey and conditions on a fairly extensive set of individual, family and school-level controls. In another multivariate analysis, Wolf et al. (2001) find that college students in Texas who had attended private schools exhibited significantly more political tolerance than their peers who had attended public schools.

The literature on student achievement clearly suggests why many researchers are likely to view any positive, partial correlations between Catholic schooling and civic outcomes with considerable skepticism. There are a variety of inherently unobservable variables that are likely to positively influence a family's choice to pay for Catholic schooling (e.g., shared norms and beliefs, community attachment). Since those same variables are also likely to promote the development of civic engagement, the apparent effects of Catholic schooling could actually be quite misleading. An informal assessment of the data sets I use in this study (i.e., HS&B and NELS88) suggests that this concern is a valid one. Specifically, with respect to adult voter and volunteer participation, I find consistent evidence of strong, positive selection into Catholic schools.6 In the next section, I present a conventional, multivariate analysis of the effects of Catholic schooling on adult civic participation. This analysis exploits the relatively rich set of background controls that are available in the High School and Beyond (HS&B) longitudinal study as well as several merged controls reflecting county and state-level traits. This study also improves upon the prior empirical evidence by analyzing adult, not teen, outcomes.7 I find that, conditional on these controls, Catholic schooling appears to have a positive effect on adult voter turnout, but not volunteering. In the subsequent section, I assess whether these results reflect undiagnosed selection biases by exploiting three of the conventional instrumental variables for Catholic schooling (i.e., Catholic religious affiliation, the local density of Catholic schools and the local availability of mass transit). I also examine whether these instruments provide a valid basis for identifying the effects of Catholic schooling by evaluating their effects among the HS&B and NELS88 respondents who attended public schools for 8th grade (Altonji, Elder and Taber 2002). The final section summarizes and discusses several important caveats.

A MULTIVARIATE ANALYSIS

The data for this section are drawn from High School and Beyond (HS&B), a major longitudinal study conducted by the U.S. Department of Education. This detailed study began with a cohort of high school sophomores in

1980. A final follow-up interview of roughly 12,000 members of the sophomore cohort occurred in 1992 when most respondents were 26 years old.8 In this interview, respondents were asked four civic-related questions: whether they were currently registered to vote (mean=.66), whether they had voted in a local, state or national election within the past year (mean=.35), whether they had voted in the 1988 Presidential election (mean=.54) and whether they had volunteered in the last month (mean=.37). Nineteen percent of these HS&B participants attended Catholic schools as sophomores.9

The basic linear specification I use to assess the effects of attending a Catholic high school on the 1992 outcomes takes the following form:

(1)

$$Y_i = \beta CS_i + X_i'\gamma + \varepsilon_i$$

where CS is a binary indicator for sophomore-year attendance at a Catholic school and X' is vector of control variables. I present OLS and 2SLS estimates of equation (1) as well as estimates based on probit and bivariate probit specifications. Because of the clustered nature of the sampling design, I also allow the error term, ε, to have a school-specific heteroscedasticity. In the sparsest versions of equation (1), X' only includes five demographic controls for gender, age and race/ethnicity. However, other models introduce 17 other variables reflecting the family income, parental education and family structure of each respondent and two dummy variables reflecting the urbanicity of the base-year schools (see appendix). Subsequent models also introduce five controls that vary at the county or state level. One of the county-level variables is a well-measured proxy for the civic attitudes of the community in which the respondents grew up: the county-level voter turnout in the 1980 Presidential election. Two other county-level variables reflect other possibly relevant community traits: the percent of adults aged 25 or older with high school degree and the percent of the population that is Catholic. And two state-level variables reflect influential voter regulations defined as of 1992 (Knack 1995). One is a binary indicator for whether the state had an active policy of allowing voter registration by mail. The second is the number of years the state had active "motor-voter" regulations in place.10 The available evidence suggests that a years-based measure is the appropriate variable for identifying the early effects of "motor-voter" policies because state drivers licenses are renewed in cycles as long as six years (Knack 1995). Finally, some models also introduce 8 fixed effects for the Census division in which the base-year school was located.

The key results from single-equation probits based on these controls are presented in Table 1. The results suggest that attending a Catholic school has large, positive and statistically significant effects on voter participation. More specifically, these results suggest that attending a Catholic school increased adult voter turnout by 7.6 to 11.8 percentage points and increased voter registration by 9.6 percentage points (Model (5)). Given that the mean levels of these outcomes vary from 35 to 66 percent, these marginal effects are quite large. However, the estimated effects of Catholic schooling on volunteer participation are much smaller, often negative and statistically insignificant.11 Interestingly, the estimated effects of Catholic schooling on voter participation are quite sensitive to the introduction of the family and parental controls, suggesting the existence of some positive selection. However, the marginal effects were largely invariant to the introduction of the remaining school, county and state-level controls.

In Table 2, I present further information on the robustness of these estimates by replicating them in specifications that introduce a variety of other individual-level control variables. These controls include measures of religiosity (i.e., 3 fixed effects for attendance at religious services), a measure of peer quality, base-year predictors of future civic engagement (i.e., a scale measure on attitudes towards inequality and a standardized score on a civics exam) and controls for highest subsequent educational attainment (i.e., 3 fixed effects for high school graduate, associate's degree, bachelor's degree).12 The results based on these controls should be interpreted with some caution because any of these variables could be reasonably

viewed as endogenously determined outcomes of Catholic schooling. However, these variables may also provide valid controls for many of the difficult-to-observe factors that influence a possibly confounding pattern of selection into Catholic schools. The results in Table 2 indicate that Table 1's results are generally quite robust to conditioning on these variables. Even the final specification, which includes all of the control variables, suggests that the effects of Catholic schooling on voter participation, though smaller, are still quite large and statistically significant.

Another way to frame the problem of selection bias in this context is to recognize that Catholic schools have more latitude than public schools with regard to accepting and expelling students. To the extent that Catholic schools are more likely to admit students with a propensity towards future engagement (and expel those who don't), the results in Tables 1 and 2 could be highly misleading. One ad-hoc way to evaluate this concern is to consider how the effects associated with Catholic schooling vary with school selectivity (Evans and Schwab 1995). For example, the school survey in HS&B solicited information on whether the school had a waiting list. The Catholic-school students in HS&B were split roughly evenly between schools with and without waiting lists (see appendix). If the civic effects of Catholic schooling were similar across these schools, it would suggest that selection bias is not important. However, if more selective Catholic schools (i.e., those with waiting lists) appear to be significantly better at promoting civic participation, it would imply the possibility of selection bias or simply that more selective schools are better in this regard. The results in the top panel of Table 3 indicate that the civic effects associated with Catholic schooling are statistically indistinguishable across this measure of school selectivity. However, in the bottom panel of Table 3, I report the results from models that use a different measure of school selectivity: whether the Catholic school had an entrance exam. Unfortunately, these results are not entirely dispositive. More specifically, these estimates suggest that Catholic schools with entrance exams are significantly better at promoting two of the three measures of voter participation. This could be construed to reflect the existence of selection bias or the relative inferiority of unselective Catholic schools.13 However, it should also be noted that this measure of selectivity appears to be a distinctive one. Less than 3 percent of the full sample (i.e., 16 percent of those attending Catholic schools or about 330 observations) attended a Catholic school without an entrance exam. And 59 percent of these observations were concentrated in just 5 states.14

Apart from the issue of selection bias, another possible concern with the results in Tables 1, 2 and 3 involves the quality of the self-reported data on civic participation. Comparisons of self-reported and validated measures of voting indicate that respondents often overstate their voter turnout (e.g., Silver, Anderson and Abramson 1986). The basic understanding of this phenomenon is that factors associated with pressure and guilt about civic responsibilities increase the probability of over-reporting. Not surprisingly, this reporting bias is particularly acute in surveys that focus on political values and behavior. But particularly relevant in this context is the evidence that religiosity is positively associated with the probability of over-reporting (Bernstein et al. 2000). The existence of that sort of reporting bias suggests that the effects of Catholic schooling on voter turnout may be biased upwards. In other words, Catholic schooling may simply create civics-related guilt that leads respondents to say they have voted even when they have not.

This issue cannot be addressed definitively in this context since HS&B did not validate self-reported voting. However, the available evidence suggests that this is not particularly problematic. First of all, the HS&B respondents had comparatively little incentive to over-report since the survey instrument focused almost exclusively on labor-market and educational experiences, not political values and participation. The November voter supplements to the Current Population Surveys shared this feature and the aggregate voter-participation rates implied by those self reports are relatively close to the actual rates (Teixeira 1992, Appendix A). Furthermore, the voter-registration rate implied by the HS&B responses (67 percent) is similar to the contemporaneous CPS-reported

rate for 25-34 year olds (61 percent, U.S. Census Bureau 1996). And the percent of HS&B respondents who reported voting in the past year (36 percent) is actually lower than the CPS-reported turnout rate for 25-34 year olds in the 1992 Presidential election (53 percent).15 However, further comparisons with the CPS data suggest that the HS&B respondents' 1992 recall of having voted in 1988 may be more biased. In the 1988 CPS survey, approximately 38 percent of 21-24 year olds reported voting in the Presidential election while the 1992 HS&B survey suggests that 55 percent of respondents did. So, a caveat about this particular variable is appropriate.

Second, the relationship between Catholic religious affiliation and voter turnout also suggests the absence of confounding reporting biases. Specifically, if Catholic schooling created substantive reporting biases, it would seem reasonable to expect there to be similar reporting biases associated with Catholic religious affiliation. However, estimates based on self-reported voter turnout from HS&B and on actual county-level turnout in the 1980 election both suggest that Catholic religious affiliation has small and statistically insignificant effects. Finally, it should be noted that, even if attending Catholic schools did increase over-reporting, that would necessarily imply that Catholic schooling has a type of structural effect (i.e., instilling a sense of civic obligation) that should also generate true increases in civic engagement. In other words, though these evaluations would not identify the true effect of Catholic schooling on civic participation, the very existence of such reporting biases would suggest the existence of true civic benefits to Catholic schooling.

EVIDENCE FROM INSTRUMENTAL VARIABLES

High School and Beyond (HS&B)

The results from the multivariate analysis suggest that Catholic schooling may have surprisingly large, positive effects on voter participation. However, it is still reasonable to be concerned that these results may reflect undiagnosed selection biases. In an attempt to assess this fundamental concern, I constructed 2SLS and bivariate-probit estimates that rely on three different instrumental variables used in recent studies of Catholic schooling. One is a binary indicator for self-reported Catholic religious affiliation (Evans and Schwab 1995, Neal 1997). The second is a county-level density index: the number Catholic high schools in 1979 per square mile (Neal 1997). The third is a county-level measure of the availability of mass transit: the percent of workers using public transportation in 1980 (Figlio and Ludwig 2000). The key maintained assumptions of studies that use these instruments are that they influence the decision to choose Catholic schools (e.g., through tastes or effective costs) but are otherwise unrelated to student outcomes.

In Table 4, I present the key results from linear-probability version of the first-stage models. These results uniformly suggest that each of the instruments has plausibly signed and statistically significant effects on the probability of attending a Catholic high school. For example, Catholic students are 29.4 percentage points more likely than non-Catholics to attend Catholic high schools. Similarly, doubling the mean value of the density index (i.e., an increase of 0.04) would increase the probability of attending a Catholic high school by 3.4 percentage points. And, doubling the mean value of the mass-transit index (i.e., an increase of 0.08) would increase the probability of attending a Catholic high school by approximately 8.3 percentage points. The results in Table 4 also suggest that these first-stage estimates are generally robust to the introduction of the individual, family, school, county and state-level controls.

The results in Table 4 clearly demonstrate that each of these candidate instruments appears to generate variation in the choice to attend Catholic schools. However, a second and more controversial maintained assumption is that these variables do not also reflect other determinants of student outcomes (i.e., that they can be legitimately excluded from the outcome equations). I turn to this question in detail below. But, assuming for now that these exclusion restrictions are valid, I present the key bivariate-probit and 2SLS results based on each of these instruments in Table 5. The results

from models that use Catholic religious affiliation as an instrument are quite similar to the results from single-equation probits. More specifically, they suggest that attending Catholic high schools increases voter participation by roughly 7 to 11 percentage points. The results based on the density and mass-transit indices suggest that Catholic schools have even larger and statistically significant effects on voter participation. Interestingly, some of these results suggest that Catholic schooling may have actually reduced adult volunteer participation.

Assessing the Exclusion Restrictions

The main criticism likely to be raised about the results in Table 5 is that the instruments may be flawed because their variation reflects unobserved determinants associated with these civic outcomes. For example, it is reasonable to be concerned that Catholic religious affiliation may influence adult civic participation independently of its effects on Catholic school attendance. Similarly, the variation in the density and mass-transit indices may reflect community-wide determinants of civic participation as well as of school choice. One straightforward approach to assessing these important concerns is to examine how the variation in the candidate instruments relates to the variation in other observed determinants of civic participation. The nature of this "selection on observables" may suggest how the instruments vary with the unobserved determinants of civic participation (Altonji, Elder and Taber 2002). Perhaps the most obvious choice of an observable is the 1980 voter turnout in the county of each respondent's high school. I estimated three auxiliary regressions where this turnout rate was the dependent variable and which included the full set of controls (e.g., as in model (5) in Table 1). The results indicated that each of the three candidate instruments had a negative and statistically significant relationship with the turnout rate. The sign and significance of these estimates suggest that violations of the exclusion restrictions may actually impart a negative bias to the estimates in Table 5 (which would not confound the voting results). However, I also constructed measures of observables based on the probit indices (i.e., $X'\hat{\gamma}$) from models of civic participation. I generally found that the candidate instruments had positive and statistically significant partial correlations with these instruments, which does suggest the possibility that the instruments are flawed.16

Altonji, Elder and Taber (2002) suggest another, straightforward way to assess the size and significance of the 2SLS bias generated by possibly contaminated instruments. This approach is motivated by the observation that, for students who attended a public school for 8th grade, the availability of Catholic high schools is largely irrelevant. Very few students who attended public schools for 8th grade went on to attend Catholic high schools.17 Therefore, among the public 8th graders, the reduced-form effect of a candidate instrument, Z, should be zero if the exclusion restriction is valid. However, if an instrument appears to have a large reduced-form effect, it clearly implies that the instrument influences adult civic participation independently of its effects on Catholic-school attendance (i.e., cov(Z, ε) ≠ 0). A straightforward way to assess the direction and magnitude of the bias in a 2SLS estimate is to recognize that the IV estimate in a just-identified model equals the ratio of the reduced-form and first-stage effects of Z (i.e., $\beta_{IV} = \beta_{RF}/\beta_{FS}$). The 2SLS bias can, therefore, be calculated by dividing the reduced-form effect based on the public 8th grade sample by the first-stage estimate. Alternatively, the amount of bias can be identified more directly by the estimating the parameter, δ, in the following, stylized, reduced-form model:

(2)

$$Y_i = X_i'\alpha + (Z_i\hat{\beta}_{FS})\delta + \nu_i$$

One practical problem with this type of bias calculation is that the reduced-form estimates may be biased by the non-random selection of students out of public schools. However, Altonji, Elder and Taber (2002) argue that, in the context of student achievement, this pattern of selection should make the case against the conventional

instruments even stronger. A similar argument is likely to apply here. More specifically, the positive selection of students into Catholic schools implies that those who remain in the public 8th grade sample despite a large Z have an unobserved propensity for lower civic participation. This non-random selection implies that the estimates of δ from equation (2) will be negatively biased and, therefore, imply a lower bound on the 2SLS bias. A more problematic complication is that about 5 percent (n=429) of those who said they attended a public 8th grade also attended a Catholic high school as sophomores. Furthermore, a relative large number of students (roughly 12 percent) did not respond to the HS&B question about their 8th grade school. I assess the empirical relevance of these concerns by replicating the bias calculation in models that exclude the public 8th graders who went on to attend Catholic schools and in models that include those who did not respond to the question about their 8th grade school.18

I report the key results of these bias calculations in Table 6. As points of reference, the left column in Table 6 repeats the 2SLS estimates of the Catholic-school effect based on the full sample for different outcomes and instruments (Table 5). The bias estimates in the top panel of Table 6 are based on estimates of δ from equation (2) where Z is a binary indicator for Catholic religious affiliation. The "Bias 1" estimates are based on those who reported that they attended a public 8th grade. The results indicate that being Catholic had statistically insignificant effects on voter registration, having voted in the 1988 election and volunteering. Though two of these three point estimates are relatively large, their imprecision suggests that Catholic religious affiliation may provide a valid source of identification in this context. However, these results also indicate that Catholics who attended public schools for 8th grade were significantly more likely to have voted in the last 12 months even though they were substantially less likely to attend Catholic high schools. The 2SLS bias implied by this estimate (.087) is almost equal to the 2SLS estimate based on this instrument (.089). However, excluding those who actually attended Catholic high schools makes this estimate smaller and statistically insignificant (Bias 2). And including those who did not provide information about their 8th grade school (Bias 3) also reduces the size and significance of the bias.

The overall impression from the top panel of Table 6 is that Catholic religious affiliation might provide a valid basis for identification.19 In the remaining panels of Table 6, I present estimates of the 2SLS bias implied by using the county-level density and mass-transit indices as instruments. The results suggest that both instruments appear to violate the exclusion restrictions. More specifically, the effects of each index on adult civic participation are often quite large and statistically significant even though relatively few of the public 8th graders attended Catholic high schools. These results suggest that these indices reflect determinants of adult civic participation that operate independently of Catholic school attendance (i.e., $cov(Z, \varepsilon) \neq 0$).

National Education Longitudinal Study of 1988 (NELS88)

The results from the previous section indicate that, though the county-level indices might not provide good instruments in this context, Catholic religious affiliation might. However, there are a number of caveats to this evidence. For example, the amount of non-response in HS&B to the question about the school attended for 8th grade is relatively high (12 percent). Furthermore, Catholic religious affiliation was based on student, not parent, reports. Additionally, the HS&B data are based on respondents who attended high school over twenty years ago, not a more recent cohort. The recently released data from the 2000 follow-up interviews conducted as part of NELS88 provide an opportunity to reconsider an identification strategy based on Catholic religious affiliation using data that do not have these possible shortcomings.

NELS88 is a nationally representative, longitudinal study conducted by the U.S. Department of Education (Curtin et al. 2002). It began in 1988 with cohort of students in 8th grade. My extract consists of 1990 high-school sophomores attending public and Catholic schools who also participated in the 2000 interview.20

The 2000 interview, which occurred when most respondents were approximately 29 years old, elicited information on several types of civic participation. These consisted of whether the respondent was currently registered to vote (mean=.79), had voted in the last 2 years (mean=.43), had voted in the 1996 Presidential election (.58) and whether they had volunteered in the last year for a youth organization (mean=.20), a civic organization (mean=.22) and a political campaign (mean=.04). The results presented here condition on a rich set of background controls available in NELS88 including information on gender, age, race/ethnicity, family income, parental education, family structure, school urbanicity and regional fixed effects (see the appendix for details).21

In the first column of Table 7, I present OLS estimates of the effect of attending a Catholic school in 10th grade on the adult measures of civic participation. The results suggest that Catholic schooling increased voter registration by a statistically significant 4.4 percentage points and voter turnout in the 1996 Presidential election by a statistically significant 6.1 percentage points. However, the estimated effect on voting in the last two years is smaller and statistically insignificant. And, as in the HS&B results, the estimated effects of Catholic schooling on volunteering are quite small and statistically indistinguishable from zero. In the next column of Table 7, I present 2SLS estimates that rely on Catholic religious affiliation as an instrument for Catholic school attendance. These results suggest that Catholic schooling has much larger effects on voter participation, increasing these measures by a statistically significant 16.7 to 27.4 percentage points.

The 2SLS results suggest that a conventional, multivariate analysis that does not acknowledge non-random selection into Catholic schools may understate the civic effects of Catholic schooling. However, that interpretation hinges critically on the assumption that Catholic religious affiliation does not influence adult civic participation independently of its effects on school choice. In the final column of Table 7, I provide empirical evidence on that assumption by using the sample of public-school 8th graders to estimate the 2SLS biases. The results suggest that the 2SLS estimates for voter participation do have large and positive biases. In particular, Catholic religious affiliation has large and statistically significant effects on adult voter registration and 1996 voting even though almost none of the public 8th graders attended Catholic high schools. Furthermore, the amounts of bias implied by these effects are roughly equivalent to the size of the 2SLS estimates (12 to 24 percentage points). However, in the case of voting in the past 2 years, this bias estimate is imprecise. Nonetheless, these results clearly suggest that Catholic religious affiliation has large, positive effects on adult voter participation that are independent of having attended a Catholic high school and, therefore, may not provide a valid basis for identification.

CONCLUSIONS

The promotion of adult civic engagement is widely viewed as one of the fundamental goals of public schooling in the United States. Yet we have had surprisingly little evidence on the relative effectiveness of public schools in fostering important civic outcomes. In this study, I presented new, empirical evidence on this issue by assessing the effects of Catholic schooling on measures of adult civic participation. I found that students who attended Catholic high schools were substantially more likely to vote, though not volunteer, as adults and that these relationships were robust to conditioning on an unusually rich set of control variables. I also attempted to correct for possibly undiagnosed selection biases by leveraging three instrumental variables for Catholic schooling. The systems estimates based on these identification strategies also suggested that Catholic schooling had large effects on adult voter participation. However, I also presented evidence that these instruments may violate the exclusion restrictions. More specifically, I found that these instruments often appear to have large, statistically significant effects among respondents for whom the availability of Catholic schools was largely irrelevant (i.e., those who attended public schools for 8th grade). The magnitudes of the biases implied by these effects generally suggest that these instruments do not

provide a valid basis for identification.

The lack of a credible source of exogenous variation in Catholic school attendance implies that this study's results are necessarily qualified. Nonetheless, the estimates presented here do provide suggestive evidence that public schools may suffer from a "productivity crisis" with regard to some of their fundamental civic goals. These inferences are consistent with the explicit motivation for recent policy initiatives designed to improve civic education. For example, in introducing "The American History and Civics Education Act," Senator Lamar Alexander suggested that public schools have abandoned their traditional function of promoting shared civic norms and noted the evidence from national exams suggesting that that a third of students are "civic illiterates."

However, a less qualified interpretation of this study's results is that Catholic schools are not inferior at promoting civic participation. This more cautious inference is also policy-relevant since one of the criticisms of voucher proposals is that private schools would be less effective at promoting civic outcomes. However, two additional caveats should also be noted. First, this study examined the effects of Catholic schooling on adult civic participation but not on civics-related attitudes. A reasonable concern is that this study's inferences may be misleading since Catholic schooling could promote civic participation but simultaneously reduce political tolerance and respect for democratic pluralism. The lack of a large, nationally representative survey that simultaneously provided data on Catholic school attendance and such adult outcomes meant that this issue could not be addressed directly. However, the limited evidence available from recent studies of student-level data suggests that this concern is not empirically relevant. In particular, Campbell (2001) presents weakly significant evidence that, conditional on an extensive set of individual, family and school-level controls, students at Catholic schools actually exhibit more political tolerance than students in public schools (i.e., more support for free speech against religion, less support for banning books). A second and possibly more relevant caveat is that the civic consequences of Catholic schooling would not necessarily generalize to other types of private schools. In particular, there is suggestive evidence that private, religiously affiliated, non-Catholic schools simultaneously promote civic participation but reduce political tolerance (Campbell 2001). The low enrollment shares of such schools imply that their possibly pejorative civic consequences are attenuated. Nonetheless, this evidence suggests comparisons of public and private schools should be careful to acknowledge the heterogeneity among private schools.

(FOOTNOTES)

1 Furthermore, the public production of schooling may reduce possibly wasteful private-sector competition (Frank 1996) as well as limit normatively undesirable private-sector responses to the negative externalities created by unruly or low-achieving children (Poterba 1996).

2 Grogger and Neal (2000) find similar results in an analysis based on more recent data from the National Education Longitudinal Study of 1988 (NELS88).

3 Very few of the students who attended public 8th grade went on to attend a Catholic high school. Furthermore, Altonji, Elder and Taber (2002) argue that the possible selection biases created by looking at this non-random sub-sample make the case against the conventional instruments even stronger.

4 In that analysis, the authors limit the variation in possibly confounding unobservables by focusing on the sub-sample of respondents who attended a Catholic school for 8th grade.

5 This analysis was based on data from the National Education Longitudinal Study of 1988 (NELS88) and controlled only for socioeconomic status and class racial composition. I present results based on recent adult interviews from NELS88 and, using a more exhaustive set of controls, find small and statistically insignificant relationships between Catholic schooling and types of volunteering.

6 Specifically, I examined indices of the observed determinants of civic participation (i.e., $X_i\hat{\gamma}$ from

equation (1)) and found that they were persistently higher among those who chose to enter Catholic schools. To the extent that this selection on observables has the same sign as the selection on unobservables, positive selection biases are indicated.

7 This distinction may be an important one if Catholic schools exert more coercive effects on student activity and attitudes that do not necessarily persist into adulthood.

8 See the data appendix for further information on the study and the extract used here.

9 This percentage is high because Catholic schools with large minority enrollments were oversampled (Zahs et al. 1995).

10 "Motor-voter" regulations bundle an application for voter registration with those for driver licenses. All states were required to institute "motor-voter" policies by 1995 as part of the National Voter Registration Act. It should also be noted that North Dakota does not have voter registration. The results reported here are robust to excluding observations from respondents who attended high school in that state.

11 One concern with this result is that it reflects a downward bias due to lower levels of church-related volunteering among those who attended Catholic schools. However, I find similar results (Table 7) in models based on NELS88 data, which discriminated between types of volunteering (e.g., youth organizations, civic organizations and political volunteering).

12 Because data are missing for some of these variables, the sample sizes vary across these models. See the appendix for details on each variable.

13 Interestingly, these estimates also suggest that Catholic schools that are more selective in this regard are less effective at promoting volunteering.

14 And, in models that include state fixed effects, the effects associated with Catholic schools with and without entrance exams are not significantly different. However, the waiting-list results are similar to those reported in Table 3.

15 That difference is reasonable since nearly all of the HS&B responses occurred before the November 1992 general election.

16 Interestingly, my initial analysis found that county-level instruments often had negative correlation with these indices. However, upon closer examination, I found that these results were driven by a small number of observations from a major city with extremely high values for the density and mass-transit indices but poor observables for adult civic participation.

17 Though HS&B began with sophomores, the first follow-up interview included a retrospective question about the type of school attended for 8th grade. Altonji, Elder and Taber (2002) report that only 0.3% of NELS88 respondents who attended a public 8th grade went on to Catholic high schools. In HS&B, which over-sampled Catholic schools with large minority enrollments, this percentage is higher (i.e., 5%).

18 I also replicate this analysis in the next section with NELS88 data. Since that survey began with 8th graders, complete information on the 8th grade school is available.

19 I present additional evidence on this in the next section using better and more recent data from NELS88.

20 See the appendix for details. The base-year sample was "freshened" so that a nationally representative sample of 1990 10th graders could be constructed.

21 Because NELS88 included an extensive survey of parents, the amount of missing background information is relatively low.

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Table 1 - Estimated Marginal Effects of Catholic Schooling on Adult Civic Behaviors, Single-Equation Probits

Dependent Variable	(1)	(2)	(3)	(4)	(5)	Sample Size
Registered to vote	.122‡ (.013)	.090‡ (.013)	.093‡ (.013)	.091‡ (.013)	.096‡ (.013)	12,159
Voted in last 12 months	.103‡ (.015)	.075‡ (.014)	.081‡ (.015)	.069‡ (.014)	.076‡ (.014)	12,225
Voted in 1988 Presidential election	.161‡ (.014)	.115‡ (.015)	.116‡ (.015)	.114‡ (.015)	.118‡ (.014)	12,159
Volunteered in last 12 months	.015 (.014)	-.014 (.013)	-.012 (.013)	-.005 (.014)	-.005 (.013)	12,284
Family/parental controls (17)	no	yes	yes	yes	yes	
Urbanicity dummies (2)	no	no	yes	yes	yes	
State/county-level controls (5)	no	no	no	yes	yes	
Census division dummies (8)	no	no	no	no	yes	

All models include binary indicators for gender (1), age (1) and race/ethnicity (3). Standard errors, adjusted for clustering, are reported in parentheses.

* Statistically significant at the 10-percent level

† Statistically significant at the 5-percent level

‡ Statistically significant at the 1-percent level

Table 2 - Estimated Marginal Effects of Catholic Schooling on Adult Civic Behaviors, Single-Equation Probits with Alternative Controls

Additional controls	Registered to vote	Voted in last 12 months	Voted in 1988 Presidential election	Volunteered in last 12 months
Base specification: Model (5), Table 1	.096‡ (.013)	.076‡ (.014)	.118‡ (.014)	-.005 (.013)
Fixed effects for attendance at religious services	.086‡ (.013)	.063‡ (.014)	.104‡ (.015)	-.022* (.012)
Mean socioeconomic status of base-year school peers	.094‡ (.013)	.073‡ (.014)	.104‡ (.015)	-.017 (.013)
Attitude towards social/economic inequality	.097‡ (.013)	.075‡ (.014)	.117‡ (.015)	-.005 (.013)
Score on civics test	.094‡ (.014)	.072‡ (.015)	.110‡ (.015)	-.012 (.014)
Educational attainment as of 1992	.077‡ (.014)	.057‡ (.014)	.088‡ (.015)	-.017 (.013)
All of the above	.076‡ (.015)	.049‡ (.016)	.070‡ (.017)	-.045‡ (.014)

Standard errors, adjusted for clustering, are reported in parentheses.
* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 3 – Estimated Marginal Effects of Catholic Schooling on Adult Civic Behaviors by School Selectivity

Independent variables	Dependent Variable			
	Registered to vote	Voted in last 12 months	Voted in 1988 Presidential election	Volunteered in last 12 months
Catholic School with Waiting List	.090‡	.084‡	.117‡	-.017
	(.018)	(.018)	(.019)	(.018)
Catholic School without Waiting List	.092‡	.057‡	.096‡	.001
	(.019)	(.019)	(.020)	(.018)
p-value	.9540	.2711	.4369	.4799
Catholic School with Entrance Exam	.110‡	.082‡	.120‡	-.022
	(.014)	(.016)	(.016)	(.014)
Catholic School without Entrance Exam	.041	.039	.060†	.033
	(.033)	(.028)	(.026)	(.031)
p-value	.0477	.1563	.0423	.0883

All specifications include the same controls as Model (5) in Table 1. Standard errors, adjusted for clustering, are reported in parentheses. The p-value refers to a log-likelihood test of the null hypothesis that the effects associated with each type of Catholic school are equal.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 4 – The First-Stage Marginal Effects of the Instrumental Variables on Catholic School Attendance, Linear Probability Models

Instrumental Variable	(1)	(2)	(3)	(4)	(4)
Catholic Religion	.343‡ (.025)	.329‡ (.024)	.312‡ (.023)	.295‡ (.023)	.294‡ (.022)
1979 county-level Catholic high schools per square mile	.780‡ (.115)	.897‡ (.110)	.809‡ (.109)	.869‡ (.122)	.855‡ (.141)
1980 county-level percent workers using public transportation	.845‡ (.146)	855‡ (.141)	1.04‡ (.152)	.918‡ (.164)	1.04‡ (.184)
Family/parental controls (17)	no	yes	yes	yes	yes
Urbanicity dummies (2)	no	no	yes	yes	yes
State/county-level controls (5)	no	no	no	yes	yes
Census division dummies (8)	no	no	no	no	yes

The sample size is 12,289. All models include binary indicators for gender (1), age (1) and race/ethnicity (3). Standard errors, adjusted for clustering, are reported in parentheses.
* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 5 – The Effects of Catholic Schooling on Civic Participation, Bivariate Probit and 2SLS Estimates

	Catholic as IV			Density Index as IV			Mass Transit Index as IV		
	Bivariate Probit		2SLS Estimate	Bivariate Probit		2SLS Estimate	Bivariate Probit		2SLS Estimate
Dependent variable	$\hat{\beta}$	ATE		$\hat{\beta}$	ATE		$\hat{\beta}$	ATE	
Registered to vote	.283‡ (.095)	.095	.111‡ (.036)	.346‡ (.118)	.115	.172‡ (.045)	.419‡ (.125)	.138	.231‡ (.058)
Voted in last 12 months	.175† (.078)	.065	.089‡ (.035)	.292‡ (.106)	.109	.115† (.051)	.269† (.114)	.100	.117† (.052)
Voted in 1988 Presidential election	.278‡ (.083)	.103	.101‡ (.035)	.436‡ (.109)	.160	.192‡ (.060)	.549‡ (.108)	.200	.281‡ (.068)
Volunteered in last 12 months	-.132* (.078)	-.048	-.044 (.035)	-.203* (.119)	-.073	-.051 (.054)	-.343‡ (.112)	-.120	-.142‡ (.057)

The sample sizes are as in Table 1. All models include binary indicators for gender (1), age (1), race/ethnicity (3), family/parental controls (17), urbanicity dummies (2), state/county-level controls (5) and Census division dummies (8). Standard errors, adjusted for clustering, are reported in parentheses.

* Statistically significant at the 10-percent level

† Statistically significant at the 5-percent level

‡ Statistically significant at the 1-percent level

Table 6 – The Implied Biases in 2SLS Estimates of the Effects of Catholic Schooling on Civic Participation, HS&B Public 8th Grade Samples

Dependent variable	2SLS Estimate	Bias 1	Bias 2	Bias 3
		Catholic as IV		
Registered to vote	.111‡	.045	.018	.047
	(.036)	(.047)	(.050)	(.043)
Voted in last 12 months	.089‡	.087†	.068	.075*
	(.035)	(.044)	(.046)	(.040)
Voted in 1988 Presidential election	.101‡	.008	-.019	.024
	(.035)	(.044)	(.046)	(.041)
Volunteered in last 12 months	-.044	-.057	-.056	-.029
	(.035)	(.043)	(.045)	(.041)
		Density Index as IV		
Registered to vote	.172‡	.120	.208†	.112*
	(.045)	(.073)	(.096)	(.064)
Voted in last 12 months	.115†	.098*	.117*	.144†
	(.051)	(.055)	(.062)	(.061)
Voted in 1988 Presidential election	.192‡	.219‡	.278‡	.222‡
	(.060)	(.072)	(.091)	(.071)
Volunteered in last 12 months	-.051	-.067	-.078	-.039
	(.054)	(.065)	(.083)	(.061)
		Mass Transit Index as IV		
Registered to vote	.231‡	.169‡	.171‡	.190‡
	(.058)	(.063)	(.067)	(.060)
Voted in last 12 months	.117†	.021	.001	.090
	(.052)	(.062)	(.066)	(.060)
Voted in 1988 Presidential election	.281‡	.188‡	.173†	.234‡
	(.068)	(.066)	(.070)	(.063)
Volunteered in last 12 months	-.142‡	-.112*	-.115*	-.130†
	(.057)	(.061)	(.065)	(.058)

The sample sizes are as in Table 1. Bias 1 is based on those who reported attending a public 8th grade (n=8,298). Bias 2 is based on those who reported attending a public 8th grade and who did not attend a Catholic high school (n=7,869). Bias 3 is based on those who reported attending a public 8th grade and those who did not identify the type of school they attended in 8th grade (n=9,681). All models include binary indicators for gender (1), age (1), race/ethnicity (3), family/parental controls (17), urbanicity dummies (2), state/county-level controls (5) and Census division dummies (8). Standard errors, adjusted for clustering, are reported in parentheses.

* Statistically significant at the 10-percent level

† Statistically significant at the 5-percent level

‡ Statistically significant at the 1-percent level

Table 7 – OLS, 2SLS and Bias Estimates for the Effect of Catholic Schooling on Civic Participation, NELS88

	Full-Sample Estimates		Public 8th Grade Sample
Dependent variable	OLS	2SLS	2SLS Bias
Currently registered to vote	.044‡	.202‡	.237‡
	(.016)	(.068)	(.075)
Voted in last 2 years	.025	.167†	.121
	(.024)	(.079)	(.090)
Voted in 1996 Presidential election	.061‡	.274‡	.238‡
	(.020)	(.077)	(.083)
Volunteered for youth organization in last 12 months	-.008	-.009	.029
	(.017)	(.062)	(.071)
Volunteered for civic organization in last 12 months	.003	.061	.079
	(.018)	(.063)	(.075)
Volunteered for political campaign in last 12 months	.007	.001	-.012
	(.009)	(.031)	(.036)

All models include binary indicators for gender (1), age (1), race/ethnicity (3), family/parental controls (25), urbanicity dummies (3) and regional fixed effects. The excluded instrument for Catholic schooling is being a Catholic. Standard errors, adjusted for clustering, are reported in parentheses.

* Statistically significant at the 10-percent level

† Statistically significant at the 5-percent level

‡ Statistically significant at the 1-percent level

DATA APPENDIX

A. High School & Beyond (HS&B)

HS&B, one of the U.S. Department of Education's major longitudinal studies (Ingels and Baldridge 1995), began with 1980 high school sophomores and seniors. The base-year samples were based on a two-stage, stratified, probability design. In the first stage, high schools were chosen. Certain types of schools (e.g., those with large Hispanic enrollments, Catholic schools with large minority enrollments) were oversampled (Zahs et al. 1995). In the second stage, as many as 36 sophomores were randomly chosen from participating schools. The initial HS&B sample included over 30,000 high school sophomores from 1,105 schools. Follow-up interviews of a stratified sample of the original sophomore cohort occurred in 1982, 1984, 1986 and 1992. This study is based on the 12,640 respondents from the sophomore cohort who participated in the fourth (1992) follow-up interview. The interviews for the fourth follow-up occurred from February 1992 through January 1993. Some observations were deleted because they attended non-Catholic private schools (n=351). Of the remaining 12,289 respondents, almost all answered the 4th follow-up questions on voting and volunteering (see Table A1). The extract includes basic information on the gender, race/ethnicity, age, and religious affiliation of the respondents. This extract also includes base-year information for each respondent on family income (9 categories), highest parental education (5 categories), family structure (6 categories) and the urbanicity of the high school area (3 categories). The other base-year variables are a standardized score on a civics test, a question about inequality, the mean SES of sampled peers in each base-year school and four binary indicators for frequency of attendance at religious services. The 1980 attitudinal question on the importance of correcting social/economic inequality has three possible responses: not important (1), somewhat important (2) and very important (3). These HS&B respondents were also matched by the location of their base-year school to 1980 county-level data on voter turnout, adult educational attainment and the percent of county workers using public transportation, which were drawn from ICPSR study number 8314. The 1980 data on Catholic adherents by county are described in Quinn et al. (1982) and were combined with 1980 Census data to calculate percent Catholic. Data on Catholic high schools by county were drawn from a 1979 directory published by the National Catholic Educational Association (see Neal 1997 and Grogger and Neal 2000) and matched to county-level data on land area (ICPSR study number 8314). Data on state-level voter regulations in 1992 were taken from Knack (1995, Appendix B).

Table A1 - HS&B Variables and Means

Variables (Survey Year)	Mean	Sample Size
Currently registered to vote (1992)	.66	12,159
Voted in past 12 months (1992)	.35	12,225
Vote in 1988 Presidential election (1992)	.54	12,159
Any volunteer work in last 12 months (1992)	.37	12,284
Attended Catholic school (1980)	.19	12,289
Attended Catholic school with waiting list (1980)	.09	11,436
Attended Catholic school without waiting list (1980)	.09	11,436
Attended Catholic school with entrance exam (1980)	.15	11,283
Attended Catholic school without entrance exam (1980)	.03	11,283
Attended public school in 8th grade	.79	10,469
Female	.52	12,289
Black	.13	12,289
Hispanic	.21	12,289
Other Race	.05	12,289
Older (Born Before 1964)	.29	12,289
Catholic	.39	12,289
Family income missing	.23	12,289
Family income <$8,000	.06	12,289
Family income $8,000 to $14,999	.12	12,289
Family income $15,000 to $19,999	.10	12,289
Family income $20,000 to $24,999	.11	12,289
Family income $25,000 to $29,999	.13	12,289
Family income $30,000 to $39,999	.13	12,289
Family income $40,000 to $49,999	.07	12,289
Family income $50,000 or higher	.08	12,289
Parent education missing	.17	12,289
Parent high school dropout	.29	12,289
Parent high school graduate	.20	12,289
Parent some college	.21	12,289
Parent college graduate	.14	12,289
Single mother	.14	12,289
Single father	.03	12,289
Natural mother/stepfather	.06	12,289
Natural father/stepmother	.02	12,289
Other family structure	.11	12,289
Both parents	.66	12,289
Urban school	.23	12,289
Suburban school	.51	12,289
Rural school	.26	12,289
Attends religious services at least once a week	.41	12,289
Attends religious services at least a few times a year	.31	12,289
Never attends religious services	.14	12,289
Attendance at religious services missing	.15	12,289
Mean SES of base-year school peers	-.09	12,288
Community orientation score	.02	11,121
Civics standardized score	50.6	10,366
Highest attainment – High school graduate	.49	11,775
Highest attainment – Associate's degree	.09	11,775
Highest attainment – Bachelor's degree	.28	11,775
1980 County-level percent Catholic	.24	12,289
1980 County-level votes for President ÷ 18+ population	.53	12,289
1980 County-level percent high school graduates among 25+ population	.66	12,289
1979 County-level Catholic high schools per square mile	.04	12,289
1980 County-level percent workers using public transportation	.08	12,289

B. National Education Longitudinal Study of 1988 (NELS88)

The basic extract used in most of this analysis consists of the 1990 sophomores in public and Catholic high schools who were also included in the 2000 (fourth follow-up) interviews (n=9,962). Most of these respondents were first interviewed as 8th graders in 1988. However, a small number of respondents (approximately 300) were "freshened" into the sample as part of the first follow-up so that a nationally representative cross-section of 1990 high school sophomores could be established (Curtin et al. 2002). Some of the results presented here are also based on the subset of these respondents who were in public 8th grade in 1988 (n=8,516). Only 35 of these respondents attended a Catholic high school. This sub-sample includes some "freshened" respondents whose initial 1990 interviews included a question about the type of school they attended in 8th grade (g8ctrl2). Information on family income and composition is not available for the "freshened" respondents who entered the study in the first follow-up. In the 2000 interview, respondents were asked questions about voter registration, voter participation in last 2 years and in the 1996 Presidential election and questions about volunteering for youth, civic and political organizations. The response rates to

these questions were high (see Table A2). Information on Catholic religion, family income, parental education and family structure are based on parent surveys.

Table A2 — NELS88 Variables and Means

Variables (Survey Year)	Mean	Sample Size
Currently registered to vote (2000)	.79	9,719
Voted in last 2 years (2000)	.43	9,775
Voted in 1996 Presidential election (2000)	.58	9,708
Volunteered for youth organization in last 12 months (2000)	.20	9,787
Volunteered for civic organization in last 12 months (2000)	.22	9,785
Volunteered for political campaign in last 12 months (2000)	.04	9,786
Attended public school in 8th grade	.85	9,962
Attended Catholic school in 10th grade	.07	9,962
Female	.53	9,962
Black	.09	9,962
Hispanic	.13	9,962
Other Race	.08	9,962
Older (Born Before 1974)	.31	9,962
Catholic	.28	9,962
Family income missing	.12	9,962
Family income = $0	.003	9,962
Family income < $1,000	.01	9,962
Family income $1,000 to $2,999	.01	9,962
Family income $3,000 to $4,999	.01	9,962
Family income $5,000 to $7,499	.02	9,962
Family income $7,500 to $9,999	.03	9,962
Family income $10,000 to $14,999	.06	9,962
Family income $15,000 to $19,999	.07	9,962
Family income $20,000 to $24,999	.10	9,962
Family income $25,000 to $34,999	.18	9,962
Family income $35,000 to $49,999	.20	9,962
Family income $50,000 to $74,999	.13	9,962
Family income $75,000 to $99,999	.03	9,962
Family income $100,000 to $199,999	.02	9,962
Family income $200,000 or higher	.01	9,962
Parent education missing	.03	9,962
Parent high school dropout	.09	9,962
Parent high school graduate	.21	9,962
Parent some college	.41	9,962
Parent college graduate	.27	9,962
Family composition missing	.10	9,962
Mother and father	.65	9,962
Natural mother/stepfather	.09	9,962
Natural father/stepmother	.02	9,962
Other 2-adult family	.01	9,962
Adult female	.13	9,962
Adult male	.02	9,962
Urban school	.27	9,962
Suburban school	.40	9,962
Rural school	.33	9,962
Urbanicity missing	.002	9,962

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