Online Appendix to
"Education and Attitudes toward Redistribution in the United States"

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November 15, 2019

Introduction

This appendix to "Education and Attitudes toward Redistribution in the United States" has three sections.

The first section, "Background Information," contains descriptive information. It begins with summary statistics and with complete information about sample sizes for each of the regressions described in the article. The remainder (and bulk) of the section is about individual variables: where the data came from, how questions were worded, and how data were combined into summary measures. Some of this information is in the article itself, but the appendix adds detail that could not be included in the article.

The second section, "Supplements to the Main Analyses," contains information that space constraints prevented me from including in the article. These include additional robustness checks for the main regressions. In the same spirit, the section surveys other attendance-law instruments that have been used in the study of education, including those used in a previous version of this article. It shows how the results would have differed had I instead used these other instruments. It also shows the results from a minimal specification that includes almost no covariates at all. And it takes up questions about missing data and the particular effects that southern U.S. states may have had on the results.

The final section, "Other Considerations," answers questions that readers of previous drafts have had but that I could not include in the article itself. These include questions about graduate equivalence degrees, social desirability bias in response to some survey questions, and whether education affects respondents' interpretations of certain outcome questions.

Background Information

Summary Statistics

	N	Mean	SD	Min	Max
Redistribution to poor (1) (GSS)	25,895	.46	.33	0	1
Years of education	25,895	11.86	1.80	0	13
Compulsory attendance $\in \{8, 9, 10\}$	25,895	.66	.47	0	1
Compulsory attendance ≥ 11	25,895	.30	.46	0	1
Male	25,895	.44	.50	0	1
Female	25,895	.56	.50	0	1
White	25,895	.83	.38	0	1
Black	25,895	.14	.35	0	1
Other race	25,895	.03	.18	0	1
Born in the United States	25,895	.97	.17	0	1
Age	25,895	45.73	17.49	18	89
Year turned 14	25,895	1962.97	19.31	1910	2008
Year of interview	25,895	1994.71	9.49	1978	2012

Table A1: Summary statistics for baseline-model analysis of "redistribution to poor (1)" (GSS).

	N	Mean	SD	Min	Max
Redistribution to poor (2) (GSS)	11,331	.55	.30	0	1
Years of education	11,331	11.95	1.69	0	13
Compulsory attendance $\in \{8, 9, 10\}$	11,331	.66	.47	0	1
Compulsory attendance ≥ 11	11,331	.30	.46	0	1
Male	11,331	.44	.50	0	1
Female	11,331	.56	.50	0	1
White	11,331	.81	.39	0	1
Black	11,331	.15	.36	0	1
Other race	11,331	.03	.18	0	1
Born in the United States	11,331	.97	.17	0	1
Age	11,331	45.79	17.39	18	89
Year turned 14	11,331	1964.67	18.59	1910	2006
Year of interview	11,331	1996.47	7.61	1985	2010

Table A2: Summary statistics for baseline-model analysis of "redistribution to poor (2)" (GSS).

	N	Mean	SD	Min	Max
Guaranteed standard of living (ANES)	17,955	.56	.30	0	1
Years of education	17,955	11.77	1.89	0	13
Compulsory attendance $\in \{8, 9, 10\}$	17,955	.69	.46	0	1
Compulsory attendance ≥ 11	17,955	.25	.44	0	1
Male	17,955	.46	.50	0	1
Female	17,955	.54	.50	0	1
White	17,955	.81	.39	0	1
Black	17,955	.13	.33	0	1
Other race	17,955	.07	.25	0	1
Born in the United States	17,955	.78	.41	0	1
Age	17,955	44.04	17.21	17	96
Year turned 14	17,955	1956.35	18.91	1910	2004
Year of interview	17,955	1986.39	9.73	1972	2008

Table A3: Summary statistics for baseline-model analysis of "guaranteed standard of living" (ANES).

	N	Mean	SD	Min	Max
Health care (ANES)	13,420	.47	.35	0	1
Years of education	13,420	11.72	1.94	0	13
Compulsory attendance $\in \{8, 9, 10\}$	13,420	.69	.46	0	1
Compulsory attendance ≥ 11	13,420	.26	.44	0	1
Male	13,420	.45	.50	0	1
Female	13,420	.55	.50	0	1
White	13,420	.80	.40	0	1
Black	13,420	.13	.34	0	1
Other race	13,420	.07	.25	0	1
Born in the United States	13,420	.71	.45	0	1
Age	13,420	44.72	17.02	17	96
Year turned 14	13,420	1955.99	19.60	1910	2004
Year of interview	13,420	1986.71	11.61	1970	2008

Table A4: Summary statistics for baseline-model analysis of "health care" (ANES).

	N	Mean	SD	Min	Max
Help the poor (GSS)	23,695	.48	.29	0	1
Years of education	23,695	11.91	1.76	0	13
Compulsory attendance $\in \{8, 9, 10\}$	23,695	.66	.47	0	1
Compulsory attendance ≥ 11	23,695	.30	.46	0	1
Male	23,695	.44	.50	0	1
Female	23,695	.56	.50	0	1
White	23,695	.82	.38	0	1
Black	23,695	.14	.35	0	1
Other race	23,695	.04	.19	0	1
Born in the United States	23,695	.97	.17	0	1
Age	23,695	45.85	17.53	18	89
Year turned 14	23,695	1964.06	19.05	1910	2008
Year of interview	23,695	1995.92	8.76	1983	2012

Table A5: Summary statistics for baseline-model analysis of "help the poor" (GSS).

	N	Mean	SD	Min	Max
Welfare (GSS)	22,173	.63	.39	0	1
Years of education	22,173	11.83	1.81	0	13
Compulsory attendance $\in \{8, 9, 10\}$	22,173	.66	.47	0	1
Compulsory attendance ≥ 11	22,173	.30	.46	0	1
Male	22,173	.45	.50	0	1
Female	22,173	.55	.50	0	1
White	22,173	.83	.38	0	1
Black	22,173	.14	.35	0	1
Other race	22,173	.03	.17	0	1
Born in the United States	22,173	.97	.16	0	1
Age	22,173	45.45	17.37	18	89
Year turned 14	22,173	1962.15	19.48	1910	2008
Year of interview	22,173	1993.61	10.17	1978	2012

Table A6: Summary statistics for baseline-model analysis of "welfare" (GSS).

Sample Sizes

Figure 2 reports results from 24 different models: there are six outcomes, and four models are estimated for each of the six outcomes. The number of observations used varies from model to model, mainly because different outcome questions were asked in different years. (See Figure A11.)

Table A7 reports the number of observations used in each of the 24 analyses. There are four rows, corresponding to the four rows of Figure 2. The first row presents the numbers of observations for the baseline model, Equation 2. The second row presents the numbers of observations after cohort-year fixed effects are added to the model. The third row reports the numbers of observations after I add state-when-young × year-when-young trend variables, and the fourth row does the same for the model that includes political and demographic control variables.

	redistrib. to poor (1) (GSS)	redistrib. to poor (2) (GSS)	guarantee SOL (ANES)	health care (ANES)	help poor (GSS)	welfare (GSS)
(1)	25,895	11,331	17,955	13,420	23,695	22,173
(2)	25,895	11,331	17,955	13,420	23,695	22,173
(3)	25,895	11,331	17,955	13,420	23,695	22,173
(4)	24,912	10,941	17,133	12,765	22,823	21,294

Table A7: Each cell indicates the number of observations of the corresponding model in Figure 2. The first row reports the numbers of observations used to estimate the baseline (Equation 2) models. The subsequent rows show how sample size declines (or doesn't) as I add cohort-year fixed effects, state-when-young × year-when-young trend variables, or political and demographic controls.

Outcomes

The outcome variables are the six ANES and GSS attitude items that are directly related to redistribution and have been asked of at least 10,000 people for whom compulsory schooling data are also available:

- *Redistribution 1* (GSS EQWLTH). "Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor." Respondents were asked to place themselves on a seven-point scale, where the endpoints meant "government ought to reduce the income differences between rich and poor" and "government should not concern itself with reducing income differences."
- *Redistribution 2* (GSS GOVEQINC and GSS EQINCOME). "It is the responsibility of the government to reduce the differences in income between people with high incomes and those with low incomes." Respondents were asked to place themselves on a five-point scale, where the endpoints meant "agree strongly" and "disagree strongly."
- *Guaranteed standard of living* (ANES VCF0809). "Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Others think the government should just let each person get ahead on his own. Where would you place yourself on this scale, or haven't you thought much about this?" Respondents were asked to place themselves on a seven-point scale, where the endpoints meant "government should see to job and good standard of living" and "government should let each person get ahead on his own."
- *Government health care* (ANES VCF0806). "There is much concern about the rapid rise in medical and hospital costs. Some feel there should be a government insurance

plan which would cover all medical and hospital expenses for everyone. Others feel that medical expenses should be paid by individuals, and through private insurance like Blue Cross. Where would you place yourself on this scale, or haven't you thought much about this?" Respondents were asked to place themselves on a seven-point scale that ranged from "government insurance plan" to "private insurance plan."

- *Helping the poor* (GSS HELPPOOR). "Some people think that the government in Washington should do everything possible to improve the standard of living of all poor Americans; they are at Point 1 on this card. Other people think it is not the government's responsibility, and that each person should take care of himself; they are at Point 5." Respondents were asked to place themselves on the five-point scale.
- *Welfare* (GSS NATFARE). "Are we spending too much, too little, or about the right amount on welfare?"

All items are coded so that responses range from 0 to 1, and so that more conservative responses have higher values.

Attendance Laws

The first American compulsory attendance laws were enacted by Massachusetts in 1852 (Lleras-Muney 2002, 403n11).[1] By 1918, all states had such laws. The laws specified ages during which attendance was required.[2]

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[1] Legislation requiring education of children has a much older history: the first such laws were enacted by Massachusetts in 1642. But compulsory attendance laws require parents to send their children to school, not just to provide them with an education (Lleras-Muney 2002, 403n11).

[2] Other authors have noted that attendance laws make children born in particular months eligible to start school earlier than other children. They have therefore used quarter of birth as an

The attendance-law variables used in this analysis indicate the number of years for which a child must attend school. To compute the number of years of schooling required by a law, I subtract the minimum age at which attendance is required from the maximum age at which it is required.

Figure A1 plots the evolution of these laws over time. Three patterns are evident. First, the strictness of the laws has increased over time in each state. Second, the changes in these laws are nonmonotonic in more than a few states. Third, there were more changes in the first half of the twentieth century than in the second, but there were also an ample number of changes in the second part of the century. This third point is particularly important: it indicates that the estimates of education's effects in this article are identified not just by schooling-law changes that happened early in the twentieth century, but by late-twentieth-century changes as well.

In this study, data on schooling laws that were in effect from 1910 through 1913 come from Goldin and Katz (2011). Data on laws from 1914 through 1978 come from Acemoglu and Angrist (2001), who were the first to use laws as instruments for education. Matthew Bettinger, Mary McGrath, Celia Paris, Lauren Sexton, and I collected the data on laws from 1979 through 2010. In all cases, year-by-year data on laws are provided for the contiguous 48 states and Washington, D.C. Because data on laws in other places are unavailable in the pre-1979 datasets, I exclude from my analysis subjects who lived in Hawaii, Alaska, or outside the US when they were 14 (for ANES subjects) or 16 (for GSS subjects).

Acemoglu and Angrist (2001) gathered data exclusively from a handful of compilations that were produced by federal government agencies. Goldin and Katz (2011) relied primarily on similar

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instrument for education (e.g., Angrist and Krueger 1991). In this article, compulsory schooling laws themselves are instruments, and quarter of birth is not at issue.

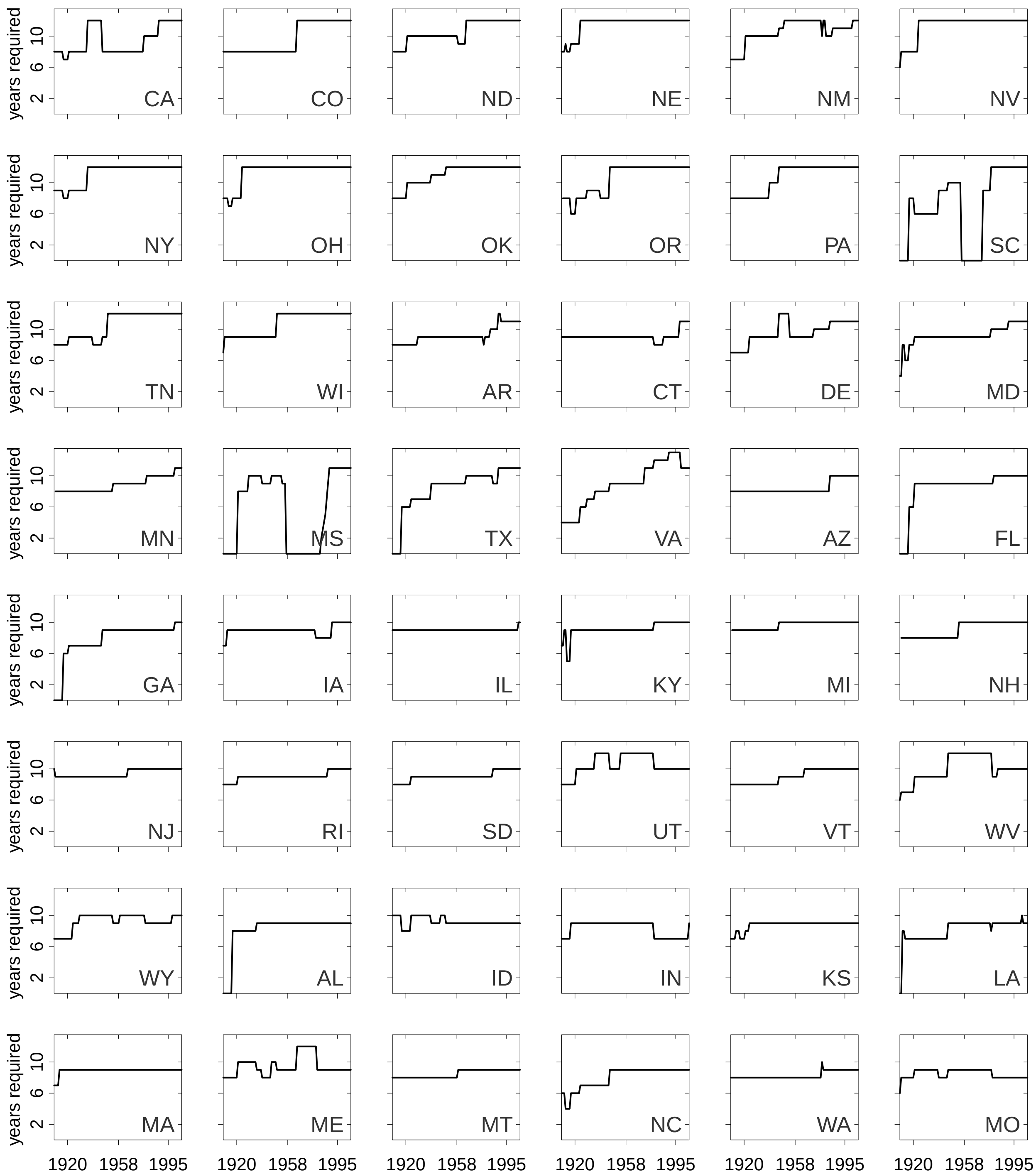

Figure A1: Variation in Compulsory Schooling Laws across States and Years. Each panel plots the number of years in school required by a state's compulsory attendance laws. States are ordered according to the strictness of their attendance laws in 2005. The "dips" for South Carolina and Mississippi in the 1950s and 1960s reflect those states' reactions to *Brown v. Board of Education.* Some states, including Massachusetts and Missouri, permit local boards of education to establish their own stricter policies.

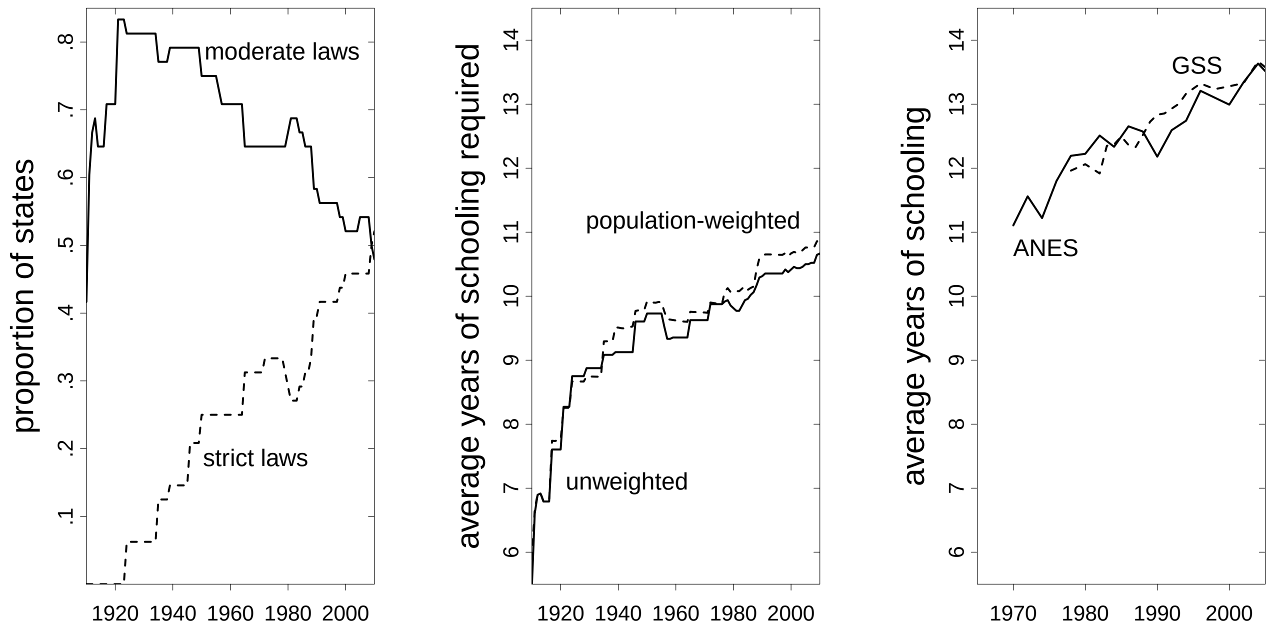

Figure A2: Average Strictness of Compulsory Attendance Laws and Average Years of Schooling. The first panel plots the proportions of states that had moderate attendance laws (8 to 10 years required) or strict attendance laws (more than 10 years required) in any given year. The second panel plots the average number of years required by the states; the dashed line in this panel reflects an average in which states are weighted according to their populations. The third panel plots the actual number of years of schooling for respondents in different ANES or GSS interview years.

compilations. I gathered data on the laws that were in effect from 1979 through 2010 directly from the relevant state statutes.

The coding of the compulsory schooling laws used in the analyses generally follows the procedure described by Acemoglu and Angrist (2001, 53–55). Values for three variables are assigned to each state-year. To aid comparison, I use the same variable names that Acemoglu and Angrist do:

1. *enroll_age* is the age by which a child must enroll in school,
2. *drop_age* is the earliest age at which a child is allowed to drop out of school, and

3. *req_sch* is the number of years of schooling that a child must obtain before dropping out.

These variables are summaries of complex laws. Inevitably, I had to make further coding decisions for the original (i.e., 1979-2010) data collection:

- *Consent:* a small number of statutes stipulate that a child may drop out of school or start work during school hours at a relatively old age—or at a younger age (typically one year younger) if the child gets the consent of a parent, guardian, principal, or superintendent. In these cases, the younger age was recorded as the value of *drop_age*.

- *Programs that lead to an equivalency diploma:* a small number of statutes permit unconditional dropping-out of high school at a relatively advanced age—or dropping out at a lower age if the student will be in a program that leads to an "equivalency diploma." In these cases, the lower age was recorded as the value of *drop_age*.

- *Programs that lead to graduation from high school:* a small number of statutes permit unconditional dropping-out of high school at a relatively advanced age—or dropping out at a lower age if the student will be in a program that leads to "high school graduation." In these cases, the higher age was recorded as the value of *drop_age*.

- *Timing:* values of the variables were assigned to each state for each calendar year. If a law did not specify when it was to take effect, it was coded as taking effect in the year that it was passed. If a law specified that it was to take effect on or before September 1st of a particular year, it was coded as taking effect in that year. If a law specified that it was to take effect on or after September 2nd, it was coded as taking effect in the following year.

- *Vocational and technical schools:* a small number of statutes require children to attend school until a relatively advanced age if they live near vocational, technical, or adult-education schools. Otherwise, they are permitted to leave school at a lower age (typically one year earlier). In these cases, the younger age was recorded as the value of *drop_age*.

- *Within-state geographic variation in requirements:* five states required, for at least one year between 1910 and 2010, greater schooling for particular cities or counties than for the rest of the state. In these cases, the values of *enroll_age* and *drop_age* assigned to the states were the values that applied to the majorities of the states' populations.

From the three variables described above, I create a further variable:

4. $CA = \max(req\_sch, drop\_age - enroll\_age)$.

CA indicates the number of years of schooling that were required by compulsory attendance laws. It was used to create the instruments: one indicator for people who were required to attend school for between eight and ten years, and a second indicator for people who were required to attend school for more than ten years.

Inspection of the state statutes revealed several errors in the Acemoglu-Angrist data. I corrected these errors in the data that I use:

- Acemoglu and Angrist code Alabama as having a *req_sch* value of 12 from 1950 through 1978. I have found no statutory support for this coding, and even today, Alabama's laws do not require 12 years of schooling. Session laws from 1956 and 1971 indicate an *enroll_age* of 7 and a *drop_age* of 16, but no law or regulation corresponding to a *req_sch* variable. I have therefore corrected the dataset so that *req_sch* assumes its 1949 value, 8, for 1950 through 1978. (See Laws 1956, 2nd special session, Chapter 117, page 446 and Laws 1971, Chapter 2484, page 3965.)

- California first required twelve years of schooling in 1987. Acemoglu and Angrist coded California as starting to require twelve years of schooling in 1978.

- Acemoglu and Angrist code Georgia as having a *req_sch* value of 12 from 1946 on. I cannot find any statutory support for this coding, and the current Georgia statute on compulsory attendance makes no mention of any *req_sch* value. (This is not to say that there was no compulsory attendance in Georgia from 1946 on. Like most states in the post-World War II era, Georgia had minimum and maximum required ages of attendance. See above for the definitions of those variables, *enroll_age* and *drop_age*, and for the definition of the *req_sch* variable.)

- Acemoglu and Angrist code Illinois as having a *req_sch* value of 12 in 1978. There is no statutory support for this coding; the correct value of *req_sch* for Illinois in 1978, and in every other year, seems to be 0. (This is not to say that there was no compulsory attendance in Illinois in 1978. Like every other state in that year, Illinois had minimum and maximum required ages of attendance. See above for the definitions of those variables, *enroll_age* and *drop_age*, and for the definition of the *req_sch* variable.)

- Acemoglu and Angrist code Kentucky as having a *req_sch* value of 12 from 1935 through 1978. I have located no statutory support for this coding; as best I can tell, the appropriate value of *req_sch* in that span is 0. (This is not to say that there was no compulsory attendance in Kentucky during that span: Kentucky did have minimum and maximum required ages of attendance. See above for the definitions of those variables, *enroll_age* and *drop_age*, and for the definition of the *req_sch* variable.)

- Acemoglu and Angrist code Massachusetts as having an *enroll_age* value of 6 in 1978. There is no statutory support for this coding; the correct value of *enroll_age* for Massachusetts in 1978 seems to be 7. (See Massachusetts session laws for 1965, chapter 741. Laws passed in that session specify an *enroll_age* value of 7, and the legislature did not revise this law between 1965 and 1978.)

- South Carolina repealed its compulsory schooling laws in 1955 (Laws 1955, no. 57), Mississippi in 1956 (Laws 1956, ch. 288). Acemoglu and Angrist dated the repeals to 1959.

Years of Education

Unlike the cumulative GSS dataset, the cumulative ANES dataset does not include a measure of respondents' years of education. But every individual ANES time-series dataset released from 1970 through 2008 included such a measure. I drew on the individual time-series datasets to construct a years-of-education variable for ANES respondents.

State of Residence When Young

Since 1978, GSS respondents have been asked "in what state or foreign country were you living when you were 16 years old?" The variable, REG16, is not available for download from a public website, but it is available to the public upon application to NORC.

Before this study, REG16 had been collected for decades but never released to the public. Indeed, it had never been coded in electronic form or mentioned in GSS codebooks. I thank Jibum Kim, formerly of NORC, for coding the variable and making it available.

Between 1974 and 1984, ANES respondents were asked "where did you live when you were about 14 years old?" (This variable is not in the cumulative ANES file.) And since 1952, ANES respondents have been asked "what part of the United States did you grow up in?" or "where was it that you grew up?" (variables VCF0132 and VCF0133 in the ANES cumulative file). Respondents who grew up in more than one state were coded by the ANES as growing up in the state during which they lived for the longest time between the ages of 6 and 18.

When no other data are available for ANES respondents, I impute state of residence at age 14 from state of birth. State of residence at age 14 is imputed in this way for 1,426 ANES respondents.

Matching attendance laws to GSS respondents on the basis of the year in which they turned 14 and their state of residence at age 16 implies an assumption that they did not move into a different attendance-law regime between the ages of 14 and 16. A similar assumption applies to ANES respondents who are matched to attendance laws on the basis of the year in which they turned 14 and the state in which they lived when they "grew up." The assumptions are sure to fail in at least a few cases: there is measurement error in the instruments. Measurement error in valid instruments does not affect IV estimates, but it does increase their standard errors. In this sense, the estimates reported throughout the article are conservative: if data on state of residence at age 14 were available for every subject, the standard errors would be smaller, and the statistical significance of the estimates would increase.

Data on the states in which adults lived as adolescents are rare. As a result, scholars almost always match attendance laws to respondents based on the year in which they turned 14, but based on their state of residence at birth or at time of interview (e.g., Acemoglu and Angrist 2001, 20–21; Lochner and Moretti 2004, 163; Milligan, Moretti, and Oreopoulos 2004). The instruments used here, which match respondents to laws mainly on the basis of actual data on state of residence at age 14, are thus somewhat more accurate than the instruments that have been used in related research.

Political and Demographic Control Variables

The analyses in this article rely on the cumulative datasets of the American National Election Studies and the General Social Survey. But they also rely on a host of variables that are not in those datasets. Most of these other variables are measured at the state-year level. The data on these characteristics of U.S. states in the 20th century were collected for this study, partly from primary sources.

STATE-YEAR POLITICAL CONTROLS

Data on turnout are from Gans (2010); the variable is measured for even-numbered years, i.e., normal congressional election years. Data on two-party vote are from Leip (2012); the variable is measured for presidential election years. Missing values are linearly interpolated.

Turnout is defined as the percentage of the voting-age population that cast votes for the highest office on the ballot in the year in which a subject turned 14. It proxies for the extent to which one grows up in a politically participatory culture: if growing up in such a culture affects the enactment of attendance laws and (independently) affects adult attitudes toward redistribution, the analyses reported throughout the article must account for the extent to which one grows up in such a culture. Admittedly, it may seem farfetched to suggest that the extent to which political cultures are participatory affects both the enactment of attendance laws and the economic attitudes that children in those cultures hold decades later, when they are adults. But in some models reported in the article, I err on the side of including turnout as a control variable. As with the other political controls, it makes little difference to the results.

For subjects who turned 14 in a presidential election year, two-party vote is the percentage of votes won by the Democratic nominee (out of votes cast for either the Democratic or the Republican candidate). For subjects born in other years, two-party vote is a weighted average of the percentages of votes won by the Democratic candidate in the presidential elections that preceded and followed the year in which the subject turned 14.

The six other political control variables are self-explanatory. See the "Data" section of the article.

STATE-YEAR DEMOGRAPHIC CONTROLS

Teacher-pupil ratios were constructed from the numbers of students and teachers in public primary and secondary schools in each state-year. These data are from various editions of the *Biennial Survey of Education,* the *Biennial Digest of Education*, and the *Digest of Education Statistics*, which were produced by the Federal Security Agency, the Department of the Interior, the Department of Education, and the Department of Health, Education, and Welfare. Data were unavailable for years before 1917 and for several years between 1917 and 1965.

The same sources provided data on teacher salaries. In some years, the data were described as the salaries of "teachers." In other years, the only salary data included were for "instructional staff." Instructional staff consists overwhelmingly of teachers, but it also includes "supervisors [and] principals" (Snyder and Hoffman 1995, 87). Where data on teacher salaries were unavailable, data on instructional staff salaries were used. Data on neither variable were available for years before 1917 or for several state-years between 1917 and 1977.

Data on the number of residents enrolled in college are from various editions of the *United States Statistical Abstract* and from Snyder (1993). Enrollment numbers include resident or extension students who were attending post-secondary degree-granting institutions. They exclude students who were taking courses by mail, radio, or television, and they exclude students who were enrolled in branches of U.S. institutions that were operated in foreign countries. Data were unavailable for some state-years.

Data on the percentages of the population in each state-year that were black, foreign-born, working in manufacturing, working as doctors, or living in urban areas are from 1% samples of the Public Use Microdata Series of U.S. Census data (Ruggles et al. 2019).

To maximize consistency of the definition of "urban" across Census years, Census respondents were generally coded as living in urban areas if the Census "metro" variable indicated that they were living in a "metro area." The "metro" variable is unavailable in 1970 and 1990 Census data; in these cases, I used the closely related "metarea" variable. As with most coding of urban residence in the United States, this coding is generous: for example, counties that contain more than 10,000 people are considered "urban" by this coding, and the entire state of New Jersey is coded as "urban" from 2000 to the present. See http://usa.ipums.org/usa-action/variables/alphabetical?id=M for more information about the definitions of the "metro" and "metarea" variables.

Following Acemoglu and Angrist (2001, 26), missing values were linearly interpolated for the state-years in which data on demographic control variables were missing.

Mechanisms

All mechanism variables come from the ANES or the GSS. All are scaled so that they range from 0 to 1.

- *Married* equals 1 if MARRIED (GSS) is coded as "married," 0 otherwise.

- *Income (logged)* is a logged version of REALRINC, a GSS variable that indicates respondents' income in real dollars. The cumulative ANES does not include a respondent income variable. It does include a variable that indicates the percentile of one's family income, and education's effects on that variable seem stronger still.

- *Currently employed* equals 1 if ANES variable VCF0116 is coded as "working now," 0 otherwise. Retirees and students were excluded from the analysis that includes the "currently employed" variable.

- *Not in poverty* equals 1 if POVLINE (GSS) equals "not poor," 0 otherwise.

- *Verbal ability* (WORDSUM, GSS) is a ten-item vocabulary test. The items are a subset of the Wechsler Adult Intelligence Scale (WAIS); they are derived from a test that was originally developed by Thorndike and Gallup (1944). They have often been used as a measure of "verbal ability" or "verbal cognitive proficiency" (e.g., Alwin 1991; Nie, Junn, and Stehlik-Barry 1996, esp. 42) and as a more general measure of "cognitive ability" (e.g., Luskin 1990; Gross and Kinder 1998, 464–65; see Caplan and Miller 2010, 639–40 for an overview).

- *Standard of living is getting better* is the GSS GOODLIFE variable: "The way things are in America, people like me and my family have a good chance of improving our standard of living' – do you agree or disagree?" Responses are coded on a five-category scale.

- *Occupational prestige higher than father's* equals 1 if GSS PRESTIGE is greater than GSS PAPRES16, 0 otherwise. The variable was first used by Alesina and La Ferrara (2005, esp. 902).

- *Society should ensure equal opportunity* measures agreement with the claim that "Our society should do whatever is necessary to make sure that everyone has an equal opportunity to succeed." There are five response options, ranging from "disagree strongly" to "agree strongly." (ANES VCF9013)
- *Equal opportunity already exists* is built from six items in the 1972 ANES:
  - The poor are poor because the wealthy and powerful keep them poor (V720686).
  - People are poor because there just aren't enough good jobs for everybody (V720687).
  - The poor are poor because the American way of life doesn't give all people an equal chance (V720689).
  - Poor people didn't have a chance to get a good education—schools in poor neighborhoods are much worse than other schools (V720691).
  - The seniority system in most companies works against poor people—they're the last to be hired and the first to be fired (V720692).
  - Good skilled jobs are controlled by unions and most poor people can't get into the skilled unions (V720694).

  Respondents were asked to express disagreement or agreement on a four-category scale that ranged from "agree a great deal" (coded 1) to "disagree a great deal" (coded 4). Cronbach's α for the six items is .74. The items were summed, and the resulting index was rescaled to range from 0 to 1.

- *Equal opportunity already exists (1 item)* is ANES variable VCF9015, "one of the big problems in this country is that we don't give everyone an equal chance." Agreement or disagreement with the item is registered on a five-category scale.

- *Hard work matters more than luck* is the GSS GETAHEAD variable. Respondents were asked "Some people say that people get ahead by their own hard work; others say that lucky breaks or help from other people are more important. Which do you think is most important?" There were three response options. In this analysis, "hard work" is coded as 0, "both equally" is coded as 0.5, and "luck or help" is coded as 1.

- *Poor people are lazy.* The index is composed of three items from the 1972 ANES:

  - With all the training programs and efforts to help the poor, anyone who wants to work can get a job these days (V720688).
  - Many poor people simply don't want to work hard (V720693).
  - Maybe it is not their fault, but most poor people were brought up without drive or ambition (V720695).

Respondents were asked to express disagreement or agreement on a four-category scale that ranged from "disagree a great deal" (coded 1) to "agree a great deal" coded 4. Cronbach's α for the three items is .50; removal of any item would lower it. Items were summed, and the resulting index was rescaled to range from 0 to 1.

Feldman (1982) also uses a fourth item from the 1972 ANES to measure perceptions of the work ethic of the poor: V720690, "Most poor people don't have the ability to get ahead." But in Feldman's analysis, the item loads more weakly onto the "work ethic" dimension than any of the other three items; and in my analysis, it is the only item in the battery that reduces reliability (Cronbach's α). These results are not surprising, as the item lacks face validity: unlike the other three items, it is not a direct measure of beliefs about work ethic. I therefore exclude it from my analysis.

- *Welfare causes laziness* is the GSS WELFARE1 variable: "Welfare makes people work less than they would if there wasn't a welfare system." Four response options, ranging from "strongly disagree" to "strongly agree." Seventeen subjects responded "don't know," and "don't know" is thus counted as a fifth category (and as the middle category). But the results are unchanged if responses from these 17 subjects are omitted from the analysis. The variable was included in only the 1986 GSS.

- *Perceived extent of inequality* and *ideal extent of inequality* are built from a series of questions that the GSS asked in 1987 and 2000. In both years, subjects were asked

> We would like to know what you think people in these jobs actually earn. Please write in how much you think they usually earn, each year, before taxes.

and

> [H]ow much do you think they should earn each year before taxes, regardless of what they actually get?

of people in 15 different occupations: bank clerks, bus drivers, doctors in general practice, Supreme Court justices, and so on. Following Trump (2018), I use these data to compute two ratios for each respondent. The ratio of highest estimated income to lowest estimated income is the measure of the perceived extent of inequality. And the ratio of highest ideal income (the income that people in a given job "should earn") to lowest ideal income is the measure of the ideal extent of inequality.

Bivariate Associations between Education and Attitudes toward Redistribution

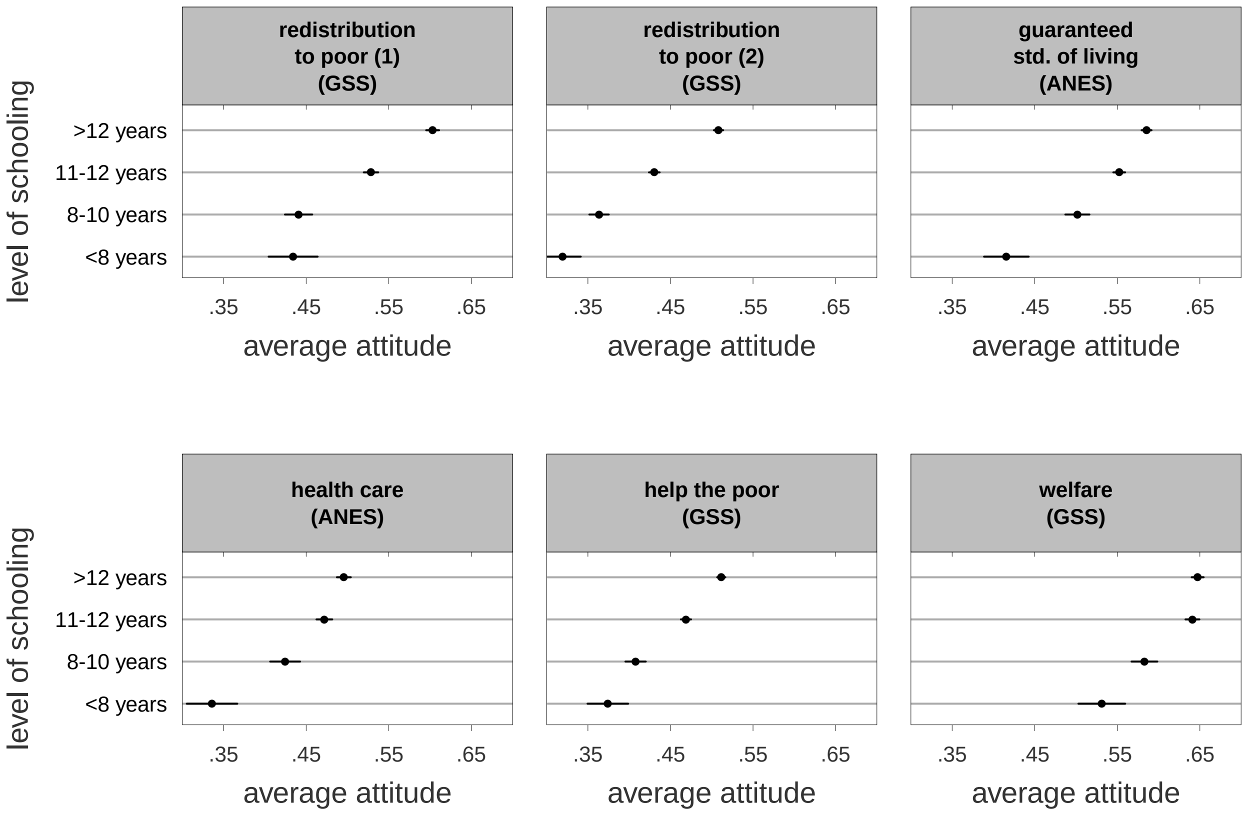

Figure A3: Education is associated with increased opposition to redistribution. Each panel shows mean responses, at different levels of education, to a redistribution-related item in the ANES or the GSS. Each item has been scaled to range from 0 to 1, with higher values indicating more conservative responses. And for each item, higher levels of education are associated with greater opposition to redistribution. Black lines are 95% confidence intervals. Sample sizes range from 26,014 for the first GSS "redistribution to poor" item to 11,397 for the second GSS "redistribution to poor" item. See page A10 for further details about the items.

Supplements to the Main Analyses

Main Estimates in Tabular Form

Figure 2 presents 2SLS estimates of education's effects on six different outcomes. Four models are estimated for each outcome, and the figure therefore presents $6 \times 4 = 24$ estimates.

Table A8 presents the same results in tabular format. Model 1 is the baseline model (Equation 2). Model 2 adds cohort-year fixed effects. Model 3 adds state-when-young × year-when-young trend variables, and Model 4 adds both political and demographic controls.

	redistrib. to poor (1) (GSS)		redistrib. to poor (2) (GSS)		guarantee SOL (ANES)		health care (ANES)		help poor (GSS)		welfare (GSS)	
(1)	.061	.019	.068	.032	.053	.017	.061	.023	.038	.020	−.006	.018
(2)	.055	.024	.056	.040	.053	.021	.073	.039	.038	.027	−.002	.024
(3)	.110	.040	.091	.046	.113	.059	.130	.059	.032	.042	−.013	.032
(4)	.111	.045	.105	.065	.102	.038	.177	.075	.145	.069	−.062	.041
Number of observations	24912 to 25895		10941 to 11331		17133 to 17955		12765 to 13420		22823 to 23695		21294 to 22173	

Table A8: Each entry is an estimate or a standard error from a separate two-stage least squares regression. This table corresponds exactly to Figure 2; the difference is that it presents the estimates and standard errors in tabular form rather than graphically.

First-Stage and Reduced-Form Estimates

Figure 2 reports the estimated effects of an extra year of secondary education on six different outcomes. For each outcome, four different regression models are estimated. The figure thus reports estimates from $6 \times 4 = 24$ different regressions.

Tables A9-A14 offer additional information: for each of the 24 regressions in Figure 2, the tables report the corresponding first-stage and reduced-form estimates. These estimates differ from regression to regression partly because the set of control variables differs across regressions. But the estimates also differ because the regressions are estimated with different subsets of data. For example, the first GSS redistribution question was asked in 20 different years, and data from all of these years are used to estimate the first-stage regressions for that question. But the second GSS redistribution question was asked in only nine different years, and data from only those nine years are used to estimate the regressions for that question. (See Figure A11 on page A63 for more information about the years in which questions were asked.)

Tables A9-A14 also report the F statistics for the exclusion of the instruments in each of the first-stage regressions. These F statistics are relevant because two-stage least squares (2SLS) estimates, like those in Figure 2, are consistent but not unbiased. The bias may be large if the instruments have only weak effects on the endogenous treatment (Bound, Jaeger, and Baker 1995). Fortunately, the first-stage F statistics can help us to characterize the extent of the bias that is due to weak instruments. Stock, Wright, and Yogo (2002, 522) propose, as a rule of thumb, that the F statistic for the exclusion of the instruments in the first stage should exceed 10 to ensure (at $p = .05$) that the bias in 2SLS estimates is no more than one-tenth the bias in the corresponding OLS estimates.

As Tables A9-A14 show, the first-stage F statistics exceed 10 for the baseline model of all six outcomes. They also exceed 10 in almost every other case. The exceptions are the regression for the second redistribution-to-the-poor item that includes political and demographic controls (Table A10a, $F = 6.44$), the guaranteed-standard-of-living regression that includes state-year trends (Table A11a, $F = 7.46$), the health-care regressions that include state-year trends and political and demographic controls (Table A12a, $F = 9.30$ and $F = 7.88$), and the help-the-poor regression that includes political and demographic controls (Table A13a, $F = 7.71$).

The reduced-form estimates in Tables A9-A14 suggest that increasing the strictness of schooling laws has a conservative effect on redistribution-related attitudes. In general, strict schooling laws seem to move responses in a conservative direction by an average of 2 to 6 percent of the range of the outcome, regardless of the particular model that is estimated. The effects of moderate schooling laws are slightly smaller but still noteworthy and statistically significant in almost every case. The important exception is the welfare item: for this item alone, the estimated effects of schooling laws are null or liberal, and they never approach statistical significance. (See Table A14b.)

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ {8, 9, 10}	.615 .098	.465 .095	.411 .110	.319 .100
CA ≥ 11	.774 .108	.572 .105	.472 .119	.421 .111
R^2	.19	.21	.21	.20
Standard error of regression	1.62	1.61	1.61	1.58
F for exclusion of instruments	62.46	33.79	16.23	13.42
Number of observations	25895	25895	25895	24912

Table A9a: First stage: effects of compulsory schooling laws on educational attainment (redistribution to poor 1, GSS). Cell entries are OLS estimates and standard errors. The response variable is years of education. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ { 8, 9, 10 }	.040 .012	.028 .012	.049 .015	.043 .014
CA ≥ 11	.045 .014	.029 .014	.044 .017	.045 .017
R^2	.06	.07	.06	.06
Standard error of regression	0.32	0.32	0.32	0.32
Number of observations	25895	25895	25895	24912

Table A9b: Reduced form: effects of compulsory schooling laws on "redistribution to the poor (1)." Cell entries are OLS estimates and standard errors. The four columns correspond to the four models used in Figure 2. "CA" is "compulsory attendance." All models include controls for age, gender, race, year of interview, year in which the subject turned 14, state of residence at age 14, state of residence at time of interview, and whether the subject was born in the United States. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ {8, 9, 10}	.521 .139	.399 .137	.485 .156	.331 .153
CA ≥ 11	.635 .151	.503 .151	.513 .170	.420 .167
R^2	.18	.20	.20	.19
Standard error of regression	1.54	1.53	1.53	1.52
F for exclusion of instruments	20.19	11.85	10.51	6.44
Number of observations	11331	11331	11331	10941

Table A10a: First stage: effects of compulsory schooling laws on educational attainment (redistribution to poor 2, GSS). Cell entries are OLS estimates and standard errors. The response variable is years of education. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ { 8, 9, 10 }	.037 .017	.026 .016	.045 .020	.039 .019
CA ≥ 11	.040 .020	.025 .019	.037 .024	.041 .023
R^2	.09	.10	.09	.09
Standard error of regression	0.29	0.29	0.29	0.29
Number of observations	11331	11331	11331	10941

Table A10b: Reduced form: effects of compulsory schooling laws on "redistribution to the poor (2)." Cell entries are OLS estimates and standard errors. The four columns correspond to the four models used in Figure 2. "CA" is "compulsory attendance." All models include controls for age, gender, race, year of interview, year in which the subject turned 14, state of residence at age 14, state of residence at time of interview, and whether the subject was born in the United States. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ {8, 9, 10}	.723 .120	.581 .119	.311 .133	.434 .140
CA ≥ 11	.894 .134	.711 .134	.406 .147	.577 .150
R^2	.23	.25	.25	.23
Standard error of regression	1.67	1.65	1.64	1.60
F for exclusion of instruments	60.98	37.75	7.46	17.22
Number of observations	17955	17955	17955	17133

Table A11a: First stage: effects of compulsory schooling laws on educational attainment (guaranteed standard of living, ANES). Cell entries are OLS estimates and standard errors. The response variable is years of education. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ { 8, 9, 10 }	.036 .013	.029 .013	.034 .016	.042 .016
CA ≥ 11	.054 .016	.042 .016	.047 .020	.059 .018
R^2	.12	.13	.13	.13
Standard error of regression	0.29	0.29	0.29	0.28
Number of observations	17955	17955	17955	17133

Table A11b: Reduced form: effects of compulsory schooling laws on "guaranteed standard of living." Cell entries are OLS estimates and standard errors. The four columns correspond to the four models used in Figure 2. "CA" is "compulsory attendance." All models include controls for age, gender, race, year of interview, year in which the subject turned 14, state of residence at age 14, state of residence at time of interview, and whether the subject was born in the United States. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ {8, 9, 10}	.669 .122	.427 .122	.394 .142	.282 .142
CA ≥ 11	.847 .135	.535 .137	.511 .157	.441 .154
R^2	.24	.26	.26	.23
Standard error of regression	1.70	1.67	1.68	1.61
F for exclusion of instruments	41.28	15.99	9.30	7.88
Number of observations	13420	13420	13420	12765

Table A12a: First stage: effects of compulsory schooling laws on educational attainment (health care, ANES). Cell entries are OLS estimates and standard errors. The response variable is years of education. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ { 8, 9, 10 }	.034 .016	.024 .017	.049 .019	.045 .020
CA ≥ 11	.061 .021	.053 .021	.069 .025	.077 .024
R^2	.07	.08	.07	.07
Standard error of regression	0.34	0.34	0.34	0.34
Number of observations	13420	13420	13420	12765

Table A12b: Reduced form: effects of compulsory schooling laws on "health care." Cell entries are OLS estimates and standard errors. The four columns correspond to the four models used in Figure 2. "CA" is "compulsory attendance." All models include controls for age, gender, race, year of interview, year in which the subject turned 14, state of residence at age 14, state of residence at time of interview, and whether the subject was born in the United States. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ {8, 9, 10}	.557 .103	.417 .101	.362 .115	.275 .107
CA ≥ 11	.664 .113	.483 .111	.382 .124	.309 .117
R^2	.18	.20	.20	.19
Standard error of regression	1.59	1.58	1.57	1.55
F for exclusion of instruments	44.21	23.71	11.01	7.71
Number of observations	23695	23695	23695	22823

Table A13a: First stage: effects of compulsory schooling laws on educational attainment (help the poor, GSS). Cell entries are OLS estimates and standard errors. The response variable is years of education. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ { 8, 9, 10 }	.023 .012	.017 .012	.013 .015	.040 .014
CA ≥ 11	.022 .014	.015 .014	.000 .018	.044 .017
R^2	.08	.08	.08	.08
Standard error of regression	0.28	0.28	0.28	0.28
Number of observations	23695	23695	23695	22823

Table A13b: Reduced form: effects of compulsory schooling laws on "help the poor." Cell entries are OLS estimates and standard errors. The four columns correspond to the four models used in Figure 2. "CA" is "compulsory attendance." All models include controls for age, gender, race, year of interview, year in which the subject turned 14, state of residence at age 14, state of residence at time of interview, and whether the subject was born in the United States. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ {8, 9, 10}	.811 .106	.628 .103	.568 .120	.502 .119
CA ≥ 11	.926 .116	.695 .113	.616 .130	.602 .131
R^2	.20	.22	.21	.20
Standard error of regression	1.63	1.61	1.61	1.58
F for exclusion of instruments	89.21	51.04	26.41	25.28
Number of observations	22173	22173	22173	21294

Table A14a: First stage: effects of compulsory schooling laws on educational attainment (welfare, GSS). Cell entries are OLS estimates and standard errors. The response variable is years of education. Standard errors are clustered at the state-year level.

	baseline	with cohort-year fixed effects	with state-year trend vars.	with political and demographic controls
CA ∈ { 8, 9, 10 }	−.003 .015	.000 .015	−.007 .018	−.032 .019
CA ≥ 11	−.010 .018	−.010 .018	−.012 .022	−.037 .022
R^2	.08	.09	.09	.09
Standard error of regression	0.37	0.37	0.37	0.37
Number of observations	22173	22173	22173	21294

Table A14b: Reduced form: effects of compulsory schooling laws on "welfare." Cell entries are OLS estimates and standard errors. The four columns correspond to the four models used in Figure 2. "CA" is "compulsory attendance." All models include controls for age, gender, race, year of interview, year in which the subject turned 14, state of residence at age 14, state of residence at time of interview, and whether the subject was born in the United States. Standard errors are clustered at the state-year level.

OLS Estimates

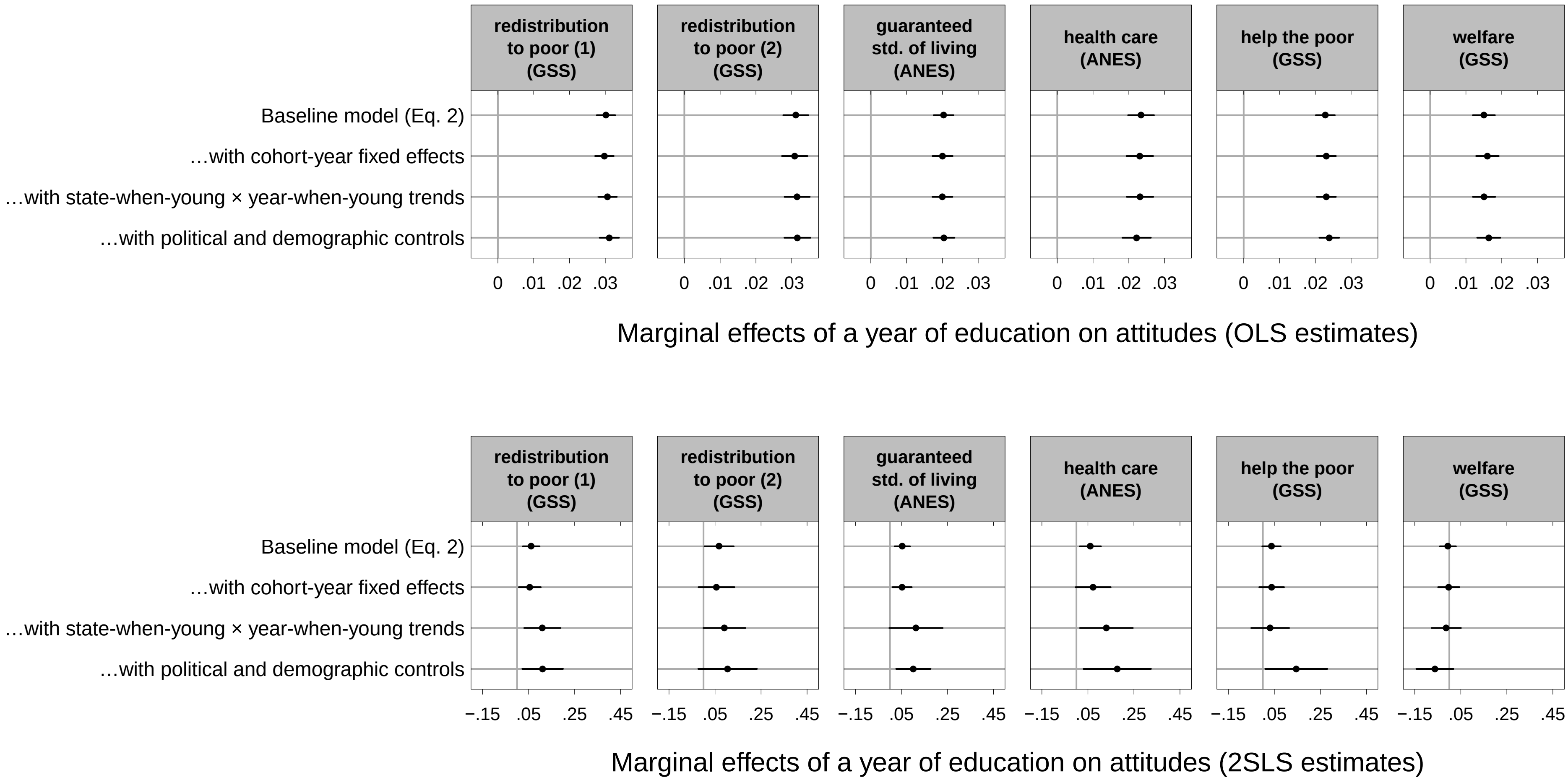

Figure A4: Effects of education on redistribution-related attitudes (OLS and 2SLS estimates). Each plotted point in the top panels is an OLS estimate of the marginal effect of education. Black lines are 95% confidence intervals. (In some cases, the confidence intervals are so narrow that they are obscured by the plotted points.) Standard errors are clustered at the state-year level. To aid comparison, the 2SLS estimates from Figure 2 are depicted in the bottom panels. Sample sizes range from 25,895 for the first "redistribution to poor" item (baseline model) to 10,530 for the second "redistribution to poor" item (model with political and demographic controls).

Standardized Estimates

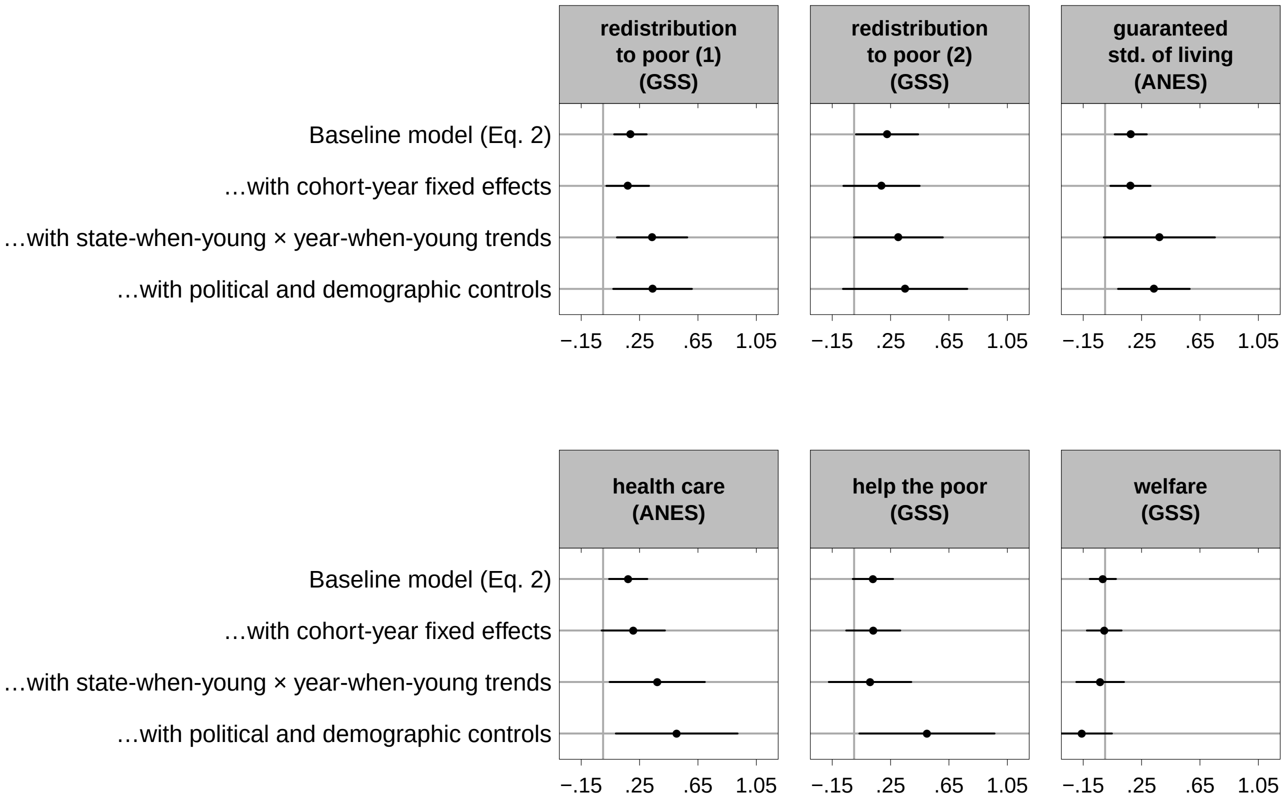

Figure A5: Effects of education on redistribution-related attitudes (standardized outcomes). Each plotted point is a two-stage least squares estimate from a separate regression. Black lines are 95% confidence intervals. Standard errors are clustered at the state-year level. This figure is similar to Figure 2; the difference is that this figure plots the effects of a year of education on standardized attitude variables, whereas Figure 2 plots the effects of education on unstandardized variables.

Further Discussion of Robustness Checks

The “Alternative Explanations and Robustness Checks” section of the article reports four analyses which suggest that, in the data used here, attendance laws satisfy the exclusion restriction and the ignorability requirement. In this section of the appendix, I report two further analyses that also speak to these assumptions. I also include further details about some of the analyses that are reported in the article.

ATTENDANCE LAWS HAVE LITTLE EFFECT ON COLLEGE ENROLLMENT

The approach used in this article is based partly on the assumption that increasing the strictness of attendance laws causes average educational attainment to increase. But one might suspect that attendance laws merely proxy for parental or cultural factors, which are the true influence on educational attainment. If this charge is correct, the ignorability assumption is violated, and the estimates of education’s effects that I report in the article are invalid. But the charge has an observable implication: if it is correct, attendance laws should be correlated with enrollment in college. After all, it is hard to imagine that other influences on educational attainment—for example, parental or cultural influences—would lead students to stay in high school without also leading some of them to enroll in college as well.

The first three columns of Table 1 show that the correlations between attendance laws and college enrollment are paltry. Moreover, the F statistics for the exclusion of attendance laws in these regressions are small, further suggesting that the laws have little to do with college enrollment. Other scholars have also found this pattern in other datasets, albeit when using a more limited range of schooling-law data: see Acemoglu and Angrist (2001, 32–35) and Milligan, Moretti, and Oreopoulos (2004, 1684). These findings bolster the claim that attendance laws satisfy the ignorability requirement, because it is hard to imagine that unobserved confounds—for example, unobserved parental or cultural influences—would lead students to stay in high school without prompting at least some of them to also enroll in college.[3]

PARENTAL EDUCATION STRONGLY MODERATES THE EFFECTS OF ATTENDANCE LAWS

We can also probe the charge that schooling-law strictness merely proxies for an unobserved variable by considering an observable implication of the charge. More-educated parents have higher aspirations for their children's education (Sewell and Shah 1968, esp. 196; Rumberger 2011, 190–93), and because they do, schooling laws should matter less to their children: their high expectations make their children likely to stay in school whether the laws are strict or weak. If schooling laws *cause* any children to receive more schooling, their effects should be concentrated among children of less-educated parents, as those are the children whose parents are less likely to insist that they stay in school. By contrast, if schooling-law strictness only proxies for another variable, we have no reason to expect that parental education is a moderator.

Figure A6 reports the relevant results, which are from a regression of years of education on schooling-law strictness and its interactions with parental education. The figure shows that the effects of attendance laws are trivial among parents with a parent who graduated from high school. But we see large effects among those whose parents never graduated from high school (31% of the sample): for these students alone, both strict and moderate laws increase educational attainment by more than a year. These are the results that we should expect if attendance laws truly affect educational attainment. They are hard to reconcile with the claim that the laws proxy for an

[3] As in previous research (Acemoglu and Angrist 2001, 32–35; Lochner and Moretti 2004, 164–65), the estimated effects of attendance laws on college enrollment are slightly negative. The conventional explanation for negative effects is that increases in attendance-law strictness may cause states to divert funds from community colleges to secondary schools.

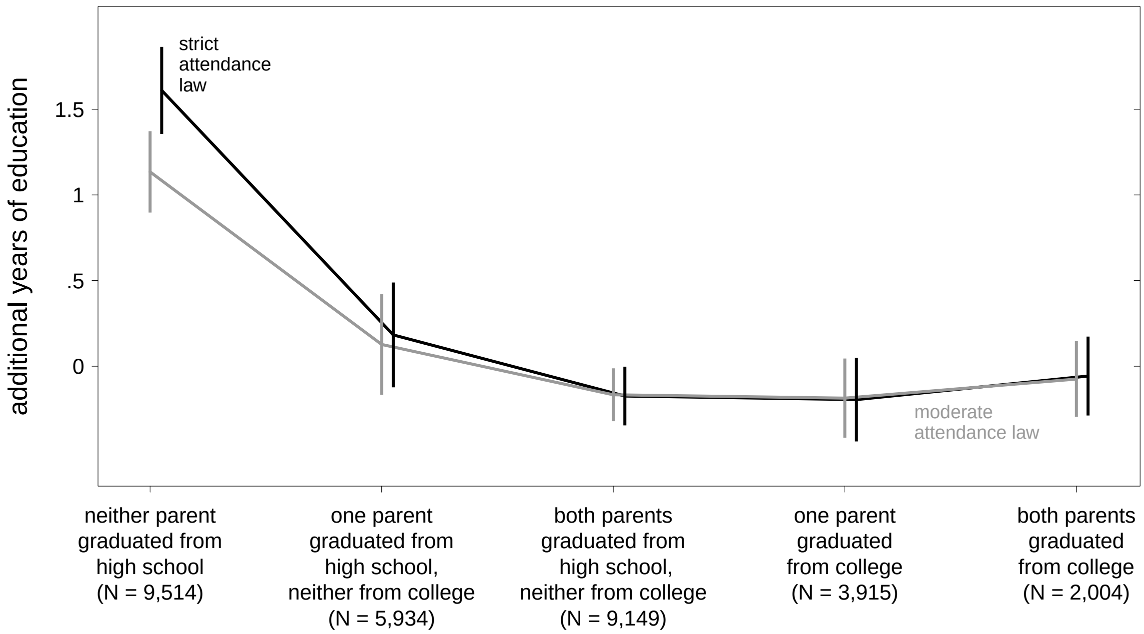

Figure A6: Moderation of schooling-law effects by parental education. The figure reports estimated effects of attendance laws on educational attainment among children whose parents have different levels of education. Vertical lines are 95% confidence intervals. The regression includes the controls and fixed effects that are in the baseline model (Equation 2). Standard errors are clustered at the state-year level. Data are from the GSS; the ANES does not regularly collect data on parental education. N = 30,516.

unobserved confound, because it is hard to imagine an unobserved confound that would create this very particular pattern.

AGE-RESTRICTED ESTIMATES

The exclusion restriction requires that the schooling laws in force when people are 14 do not affect their adult economic attitudes, save through educational attainment. Another way to describe the restriction is to say that it is violated if schooling laws have effects on adult economic attitudes that are both indirect (operating through some variable other than educational attainment) and long-lasting. Effects that violate the exclusion restriction must be long-lasting in this sense: the laws in place when people are 14 years old must affect them when they are at least 18 years old (because the ANES and the GSS generally do not interview people who are younger than 18).

Of course, it is possible for indirect effects of this sort to be "long-lasting" in the sense that they persist for four years. But the older that respondents are when they are asked about their attitudes, the less plausible violations become: it is one thing for schooling laws to indirectly affect people only four years after the age of 14, and quite another for those laws to indirectly affect people 20, 30, or 40 years later. With this idea in mind, I re-estimated the effects of education after removing all subjects under age 34 from my samples. Figure A7 reports the age-restricted estimates. These estimates are based on smaller samples, and the standard errors are larger—but the estimates themselves are quite similar to the normal, unrestricted-sample estimates. The mean of the absolute differences between the unrestricted estimates and the corresponding age-restricted estimates is .0178. The mean of the differences (rather than the absolute differences) is .0008. This similarity of results suggests that, for a violation of the restriction to be at work, schooling laws would need to be affecting attitudes through some path other than education *and* at least 20 years after the age of 14. Such a violation is unlikely.

MODELS THAT INCLUDE POLITICAL CONTROLS, DEMOGRAPHIC CONTROLS, AND STATE × YEAR TRENDS

When attitudes toward health care and a government-guaranteed standard of living are the outcomes, perfect collinearity among some of the state-level control variables precludes estimation of models that include political controls, demographic controls, and state × year trends. But such models can be estimated for the other outcomes, and the estimates in these cases are consistent with (and sometimes larger than) those reported in the article. In this case, the estimated effect of a year of education on responses to the first redistribution item is .115 (SE .060). For the second redistribution item, the estimate is .080 (SE .046). The estimates for the "help the poor" and "welfare" items are .092 (.060) and −.039 (.055), respectively.

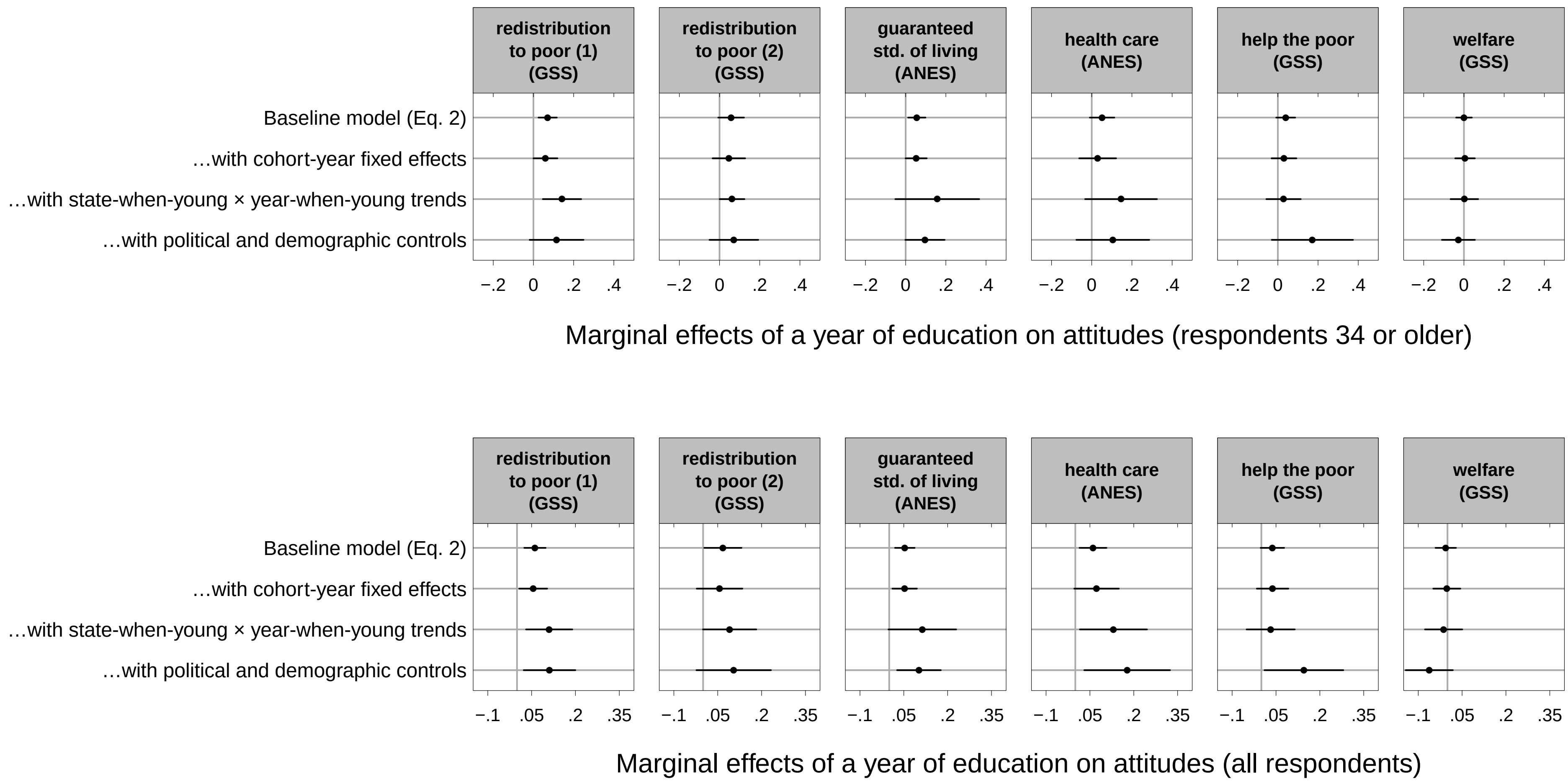

Figure A7: Effects of education on redistribution-related attitudes (age-restricted estimates). Each plotted point in the top panels is an estimate from a separate instrumental-variables regression. Black lines are 95% confidence intervals. Standard errors are clustered at the state-year level. The estimates are akin to the estimates in Figure 2; the difference is that they are based only on data from respondents who were at least 34 years old at the time of interview. Sample sizes in these panels range from 17,309 for the first "redistribution to poor" item (baseline model) to 7,710 for the second "redistribution to poor" item (model with political and demographic controls). To aid comparison, the 2SLS estimates from Figure 2, which are based on age-unrestricted samples, are depicted in the bottom panels.

Minimal Specification

A common concern about IV analyses is that the estimates, and the claims about the ignorability of the instruments, depend on a very specific combination of covariates. Tweak the model, and the results will vanish. The analyses presented in the "Alternative Explanations and Robustness Checks" section of the article should allay this concern, as should the further robustness checks presented in this appendix.

We can go further still by considering a minimal model that is stripped of almost all covariates: a model in which attitudes are regressed on only years of education, state-when-young fixed effects, and a linear trend in the year of birth. As before, attendance laws instrument for schooling. Results from this model are shown in Table A15. In every case, the estimates are still conservative, and with the exception of the "help the poor" estimate (which declines from .038 to .035) they are larger than the baseline estimates reported in Figure 2. The F statistics for the exclusion of the instruments are larger, too; and they are all far above 10, suggesting that weak instruments are not a concern in this analysis (Stock, Wright, and Yogo 2002, 522). As with the baseline model, the reduced-form estimates for the moderate attendance-law instrument in this model are weak for welfare (.019, SE .015), stronger for the help-the-poor item (.026, SE .013), and stronger still for every other item. And as with the baseline model, the reduced-form estimates are stronger when we consider the strict rather than the moderate attendance-law instrument.

	2SLS		F	N
redistribution to the poor (1)	.069	.015	95.176	26,014
redistribution to the poor (2)	.090	.025	38.593	11,397
guaranteed standard of living	.067	.016	50.553	18,090
health care	.064	.021	79.836	13,549
help the poor	.035	.017	72.347	23,810
welfare	.018	.015	127.507	22,271

Table A15: Results from a minimal specification. This table presents estimates from a minimal model in which attitudes are regressed only on years of education, state fixed effects, and a linear trend for the year in which respondents were born. As in the main analyses, compulsory attendance instruments for years of education.

Results from Different Instruments

The instrumental variables that I use in this article indicate the strictness of state attendance laws that were in effect when respondents were 14 years old. Many other articles have also used the strictness of attendance laws or related child-labor laws as instruments for educational attainment.[4] But almost every article uses a slightly different set of instruments. That is, almost every article that employs this general identification strategy has used a slightly different set of variables, or a familiar set of variables coded in slightly different ways. In this section of the appendix, I describe the different instruments sets that have been used and show that the results that I report are very similar to those that one obtains with these different instrument sets.

It may seem strange that some authors have used child-labor laws as instruments for educational attainment. But the rationale becomes clear when one reviews the history of these laws, which parallels the history of compulsory attendance laws. Compulsory attendance laws stipulate the oldest age at which one may start school, the youngest age at which one may leave school, or the amount of schooling that one must obtain before one brings one's schooling to a halt. (See pages A11-A17.) Child-labor laws, too, have often specified the earliest age at which children may leave school—that is, the age at which they may leave school to start a job. And the leaving ages that are specified by compulsory attendance laws and child-labor laws are often not the same. In other words, it is common for a state to have an attendance law that specifies a leaving age of (say) 16 but a labor-law exception which stipulates that children may leave school at 14 to begin gainful employment. Child-labor laws seem to have an independent effect on educational attain-

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[4] The well-known article by Angrist and Krueger (1991) is not a member of this category. Angrist and Krueger note that attendance laws make children born in particular months eligible to start school earlier than other children, and they therefore use quarter of birth as an instrument for education. But compulsory schooling laws themselves play no direct part in their article.

Articles	Attendance Instruments	Child-Labor Instruments	Interaction Instruments
Acemoglu and Angrist (2001, 34, 37–38); Lochner and Moretti (2004, 163)	four dummy variables indicating whether children were required to attend school for fewer than 9, 9, 10, or 11 or more years of school	none	none
Acemoglu and Angrist (2001, 34, 37–38); Oreopoulos, Page, and Stevens (2006, 737–38)	none	four dummy variables indicating whether children were required to attend school for fewer than 7, 7, 8, or 9 or more years of school before leaving to work	none
Acemoglu and Angrist (2001, 37–38); Milligan, Moretti, and Oreopoulos (2004, 1680)	four dummy variables indicating whether children were required to attend school for fewer than 9, 9, 10, or 11 or more years of school	four dummy variables indicating whether children were required to attend school for fewer than 7, 7, 8, or 9 or more years of school before leaving to work	none
Acemoglu and Angrist (2001, 37–38)	four dummy variables indicating whether children were required to attend school for fewer than 9, 9, 10, or 11 or more years of school	four dummy variables indicating whether children were required to attend school for fewer than 7, 7, 8, or 9 or more years of school before leaving to work	interactions between the attendance-law and labor-law dummy variables
Dee (2004, 1714)	none	dummy variable: required to attend school for at least 9 years before leaving to work	none

Articles	Attendance Instruments	Child-Labor Instruments	Interaction Instruments
Lleras-Muney (2005, 196–97); Glied and Lleras-Muney (2008, 744, 750–51)	none	integer-valued variable for years of required schooling before leaving to work (possible values: 0, 4, 5, 6, 7, 8, 9, 10)	none
Oreopoulos (2009, 89–90, 99–104)	dummy variable: school-leaving age ≥ 16	none	none
Oreopoulos (2009, 89–90, 99–104); Oreopoulos and Salvanes (2011, 176–77)	integer-valued variable for school-leaving age (possible values: 16, 17, 18)	none	none
previous version of this article	dummy variable: required to attend school for at least 10 years	dummy variable: required to attend school for at least 8 years before leaving to work	interaction of each dummy variable with year respondent turned 14
current version of this article	two dummy variables indicating whether children were required to attend school for 8-10 years or for more than 10 years	none	none

Table A16: Schooling-law instruments for U.S. educational attainment at the secondary-school level. This table describes the schooling-law instruments that have been used in other articles about the effects of secondary education in the United States. Some articles have multiple entries because they report results from multiple instrument sets.

ment (Lleras-Muney 2002), which is why they have sometimes been used in combination with attendance laws as instruments for educational attainment.

Table A16 describes eight different sets of attendance-law and labor-law instruments that have been used in prominent articles about the effects of U.S. secondary education. There is a great deal of variation in these instrument sets, but three patterns emerge. First, neither attendance nor labor laws are dominant: three of the sets use attendance-law instruments alone, three use labor-law instruments alone, and two use instruments of both types.[5] Second, interaction terms have been used as instruments, but they are not common. (They have been used mainly on the logic that the effects of attendance laws may be enhanced by restrictive child-labor laws. See Acemoglu and Angrist 2001, 37, and Margo and Finegan 1996, 107.)

Third, most attendance- and labor-law instruments are specified as dummy variables. For example, an attendance-law instrument will indicate whether a respondent grew up in a state that generally required at least 11 or more years of schooling; a labor-law instrument will indicate whether he could be granted an exception if he had completed at least nine years of school, so that he might end his schooling early to work. There are two advantages to "dummying out" the instruments in this way. First, it increases statistical efficiency. Second, it provides conceptual simplicity: treating continuous instruments as dummies provides a simple nonparametric model for the first-stage relationship, and it lets IV estimates be seen as mixtures of Wald estimators. See Angrist and Pischke (2009, 133–38) for more information.

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[5] In Table A16, the two instrument sets in Oreopoulos (2009) are coded as using "attendance instruments" rather than "child-labor instruments." These instrument sets are defined primarily by attendance laws, but labor laws in a minority of states also play a part in their definition. See Oreopoulos (2009, 89–90).

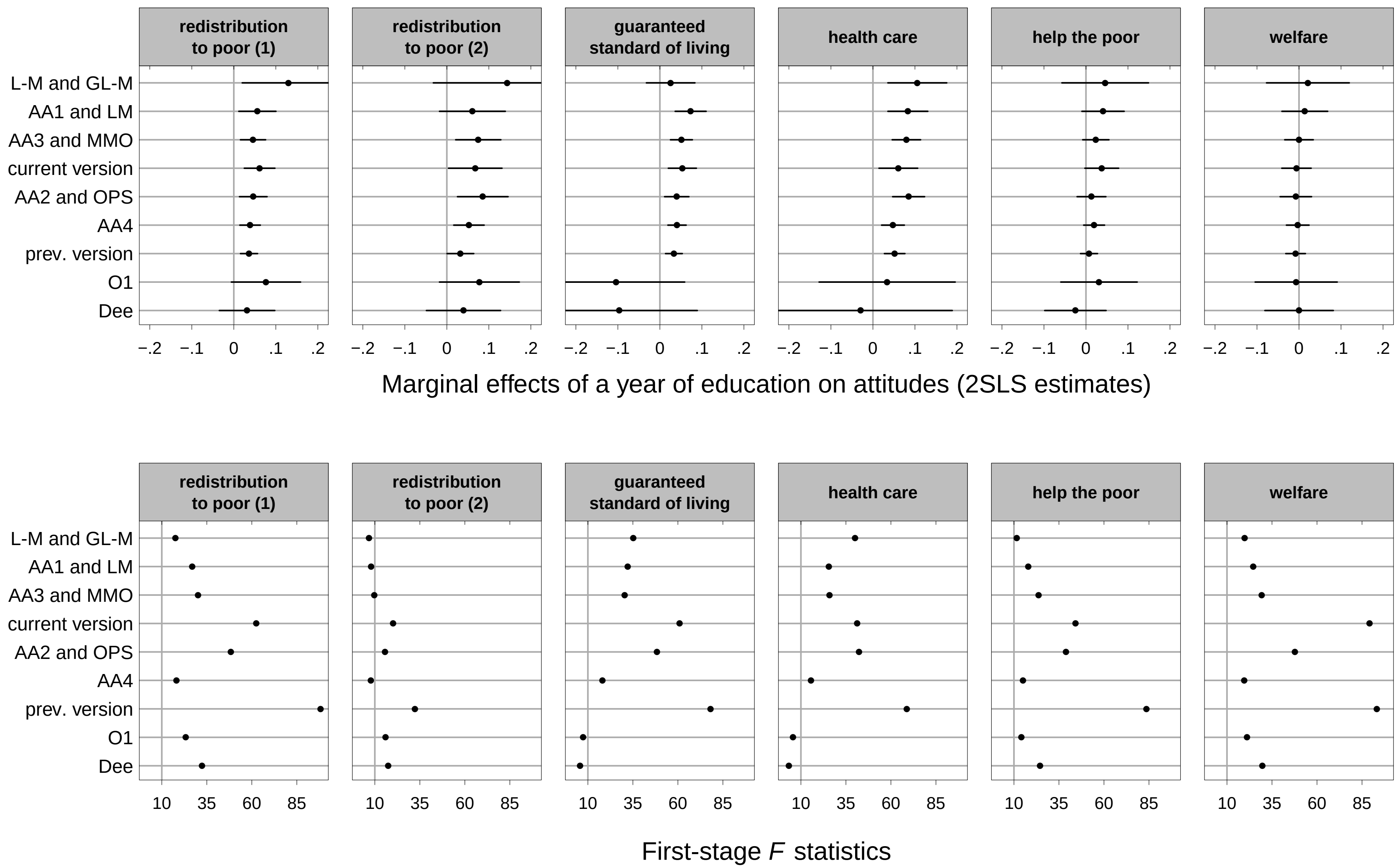

Figure A8: Estimated effects of education with different sets of instruments. Estimates and F statistics are from the baseline model (Equation 2). Rows are ordered by average estimate size. Sample sizes range from 25,895 for the first "redistribution to poor" item to 11,331 for the second "redistribution to poor" item. Abbreviations in the row names are of authors' names, which are given in Table A16. For example, "L-M" refers to the instrument set used in Lleras-Muney (2005), and "AA1" refers to the first instrument set in Acemoglu and Angrist (2001). "Current version" and "prev. version" refer to the instrument sets used in the current and previous versions of this article.

Figure A8 reports the estimated effects of an extra year of education that I obtain with each of the instrument sets that is described in Table A16.[6] It also reports the associated first-stage F statistics. And to aid comparisons, it also reports the estimates and first-stage F statistics that I obtain from the instrument set that I use in this article and that I used in a previous version of the article.

The main story of Figure A8 is the stability of the estimates across different instrument sets. To see this stability, begin by considering the leftmost panel in the top row, which reports the estimated effects of a year of education on responses to the first "redistribution to the poor" item. Of the nine estimates in the panel, six range from .04 to .06. Of the three exceptions, two are larger (.08 and .13), and all three have standard errors that are larger than the median standard error.

Similar patterns prevail for every other outcome. Variation is greatest for the guaranteed-standard-of-living item, but even here, all estimates range from .03 to .07 save for two outliers (both −.10). The patterns for the remaining outcomes are similar but still more constrained.

The rows of the Figure A8 panels are ordered by the average estimate size produced by each instrument. For example, the instrument used by Lleras-Muney (2005) and Glied and Lleras-Muney (2008) produces, on average, the highest estimates, while the instrument used by Dee

[6] There is one exception: Figure A8 does not report estimates that are based on one of the instruments used by Oreopoulos (2009, 89–90, 99–104), which was also later used by Oreopoulos and Salvanes (2011, 176–77). The instrument is an integer-valued variable which indicates the minimum school-leaving age that children faced in different states and years. It takes on only three values: 16, 17, or 18. Estimates based on this instrument are not reported because the ANES and GSS contain only a small number of respondents who faced minimum school-leaving ages of 17 or 18. Oreopoulos (2009) and Oreopoulos and Salvanes (2011) do not face the same problem because they apply the instrument to data from samples that are far larger.

(2004) produces the lowest estimates. This ordering makes clear that there is nothing exceptional about the estimates produced by the instrument set used in this article. On average, this article's estimates are near the middle of the pack: they are larger than those produced by five instrument sets, smaller than those produced by three other instrument sets.

The similarity of estimates produced by different instruments is encouraging, but it does not imply that the various instruments are all of similar quality. Among other things, the credibility of instruments depends on the first stage—that is, on the effects of the instruments on the amount of education that people receive. And as the bottom panels in Figure A8 show, the first stages differ substantially across different instrument sets. In almost all cases, the first-stage F statistics exceed 10, which is the rule-of-thumb threshold for "weak instrument" concerns (Stock, Wright, and Yogo 2002, 522). But there is a lot of variation above this threshold, and when political and demographic control variables are added to the baseline model, the F statistics for some of these alternative instrument sets dip well below 10. Of the nine instrument sets reported in Figure A8, the first-stage F statistics are strongest for the instrument set that I used in a previous version of this article, second-strongest for the instrument set that I use in this version. The latter result shows that the current instrument set is better than most others at predicting educational attainment among ANES and GSS subjects.

The previous version of this article used a complex set of instruments: a dummy variable for the strictness of attendance laws, another for the strictness of labor laws, and interactions between these variables and the years in which subjects turned 14. As Figure A8 shows, the estimates produced by that instrument set were similar to the estimates produced by the current instrument set, and the F statistics were strong. But those instruments were problematic in other ways: the identification assumptions that one must make are more complex when schooling laws are interacted with time, and the first-stage estimates implied that, in the present day, strict schooling laws substantially *reduce* the amount of schooling that children receive.

Such a result is not plausible. Further inspection revealed that these implausible negative first-stage estimates were due to the sparseness of the data. With fewer than 25,000 respondents spread across 49 states and 101 years (1910-2010, the years for which I have attendance-law data), the ANES and the GSS samples used here have, on average, only five respondents per state-year—far too few to permit robust estimation of over-time variation in the effects of schooling laws.[7] The switch to the current instrument set was driven by the realization that, because it does not interact schooling-law variables with time, it invokes simpler identification assumptions and generates more credible first-stage estimates. (See page A28.) That said, the point of Figure A8 is that the results reported in this article hold up handsomely under many different instrument sets.[8]

Age, Period, and Cohort: The Treatment of Time in the Analyses

Those who analyze repeated cross-sectional data must often confront the notorious age-period-cohort problem: the difficulty of distinguishing age effects, period effects, and cohort effects from each other (e.g., Mason and Fienberg 1985; Neundorf and Niemi 2014). This article takes up a

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[7] Recall that, in all of the regressions reported in this article, each respondent is matched to the attendance law that was in effect when he was 14. I have attendance-law data that run from 1910 through 2010, and in each of those years, some respondents turned 14. This is the sense in which respondents are "spread across" 101 years.

[8] The current instrument set also has the advantage of cleaving attendance laws neatly at levels of education that have seemed important to state legislatures. That is, the instruments distinguish between systems that require fewer than 8 years of school, systems that require 8-10 years of school, and systems that require more than 10 years of school—and in the period that I study, fully 45% of all changes made to U.S. schooling laws involve the crossing of one of these "cutpoints," e.g., a raising of the attendance-law requirement from seven years to eight.

related problem: the problem of distinguishing education's effects from other effects associated with age, period (i.e., year of interview), and cohort (e.g., year turned 14). The problem of distinguishing education's effects from other cohort-related effects may be especially acute. More recent cohorts tend to have spent more time in school; how, then, can we be confident that the conservative effects attributed to schooling are not instead due to other variables that also take on higher values in more recent cohorts?

Problems like these are notorious because age, period, and cohort are perfectly collinear when they are measured on the same scale—for example, in years. This perfect collinearity implies that we cannot control for all three variables in a linear model (Glenn 2005, 6–11). A common approach is to surmount the technical problem by constraining some coefficients to be equal. For example, one might assume that year-of-interview (period) effects are equal across some set of years. A minimal version of that approach is taken here: when including year-of-interview fixed effects in the baseline model, I include a single fixed effect for the interview years 1994 and 1996, thereby treating the two interview years as equivalent. I choose these two years because they are the only consecutive interview years in which all six outcome questions were asked. In other analyses, I find that using different pairs of years makes no substantive difference. Still, this approach is a solution to only the technical problem of controlling for age, period, and cohort in the same model. It is not a solution to the larger problem of ensuring that effects attributed to education are really due to education, rather than to other age-, period-, or cohort-related forces. In the article, I tackle this problem with a series of regression analyses and placebo tests. (See the section entitled "Alternative Explanations and Robustness Checks.") But to explore possibilities like these, it may also help to turn to descriptive graphical analyses (e.g., Yang 2011, 19-20; Glenn 2005).

Begin with the top row of panels in Figure A9. Each panel displays associations between the outcomes of interest and one variable in the APC trio. Panel 1 shows that economic attitudes become increasingly conservative until about age 45, after which there is no clear trend. Panel 2

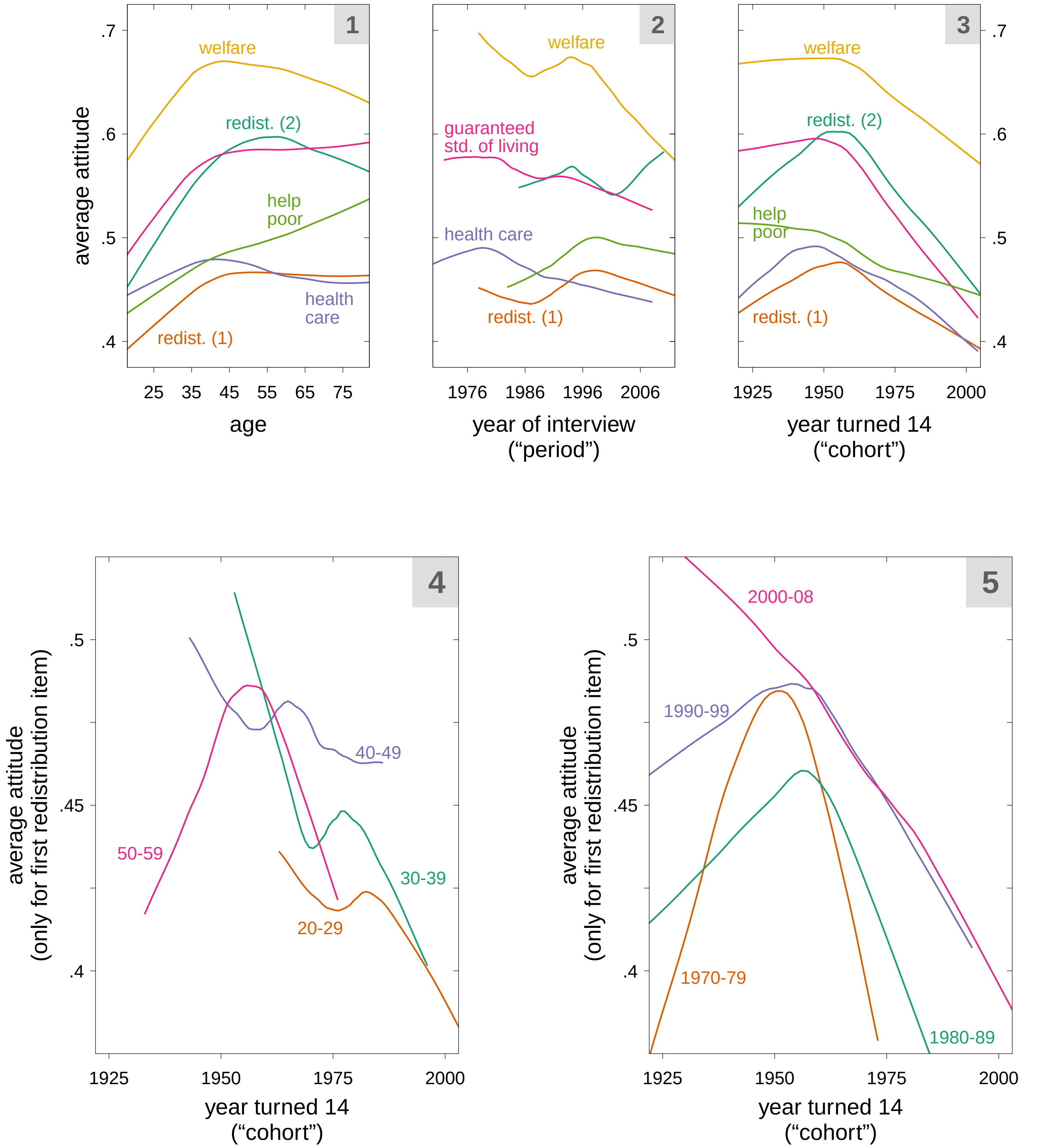

Figure A9: Associations of outcomes with age, period, and cohort. Panels 1-3 plot loess-smoothed associations of each outcome with age, year of interview ("period"), or the year that respondents turned 14 ("cohort"). There are six lines in each of these panels: one line for each outcome studied in the article. Panels 4-5 take up a single outcome, the first "redistribution to the poor" item, in more detail. In Panel 4, each line represents a separate age group (age 20-29, 30-39, and so on). And in Panel 5, each line represents a separate period group (interviewed in 1970-79, 1980-89, and so on). Each outcome has been scaled to range from 0 to 1, with higher values indicating more conservative responses.

shows that attitudes became more liberal from the late 1970s until the mid-1980s, took a conservative turn for a decade, and then resumed a liberal trend.[9] These patterns are consistent with a lagged "thermostatic" reading of U.S. public opinion whereby it trends opposite the party that holds national power (Stimson 2015, ch. 2; Wlezien 1995). In particular, the liberal turn that begins in the late 1990s is consistent with the rise of the Republican Party, which captured Congress in 1994 and the White House in 2000. Notably, the sharpest change appears in welfare attitudes: it starts to become more liberal in 1996, which is the year in which major restrictions on the provision of welfare were passed into law.

Panel 3 shows how cohort varies with the outcomes of interest. The pattern is clear: a quadratic relationship between cohort and redistributive attitudes for five of the six outcomes. The cohorts of peak conservatism seem to be those of 1946-1956: people who turned 14 years old in those years seem to be the ones who, in adult life, adopted the most conservative attitudes.[10] But in cohorts that came of age after 1956, we observe a steady turn toward liberalism.

These are promising results. More recent cohorts tend to have spent more years in school, and as Panel 3 shows, they are also more liberal. If education merely proxies for cohort effects in

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[9] The exception is the GSS item that asks about redistribution from "people with high incomes" to "people with low incomes." For this item, the smoothed association trends in a conservative direction after the year 2000.

[10] These are the same people whose "impressionable years" (Alwin, Cohen, and Newcomb 1991) were marked by the ascendance of the Eisenhower-led Republican Party and the decline of the Democratic Party, which was led, in the early 1950s, by a historically unpopular president. The pattern is thus consistent with the finding that people's political attitudes are marked for life by the relative popularity of Democrats and Republicans in their impressionable years (e.g., Bartels and Jackman 2014).

our analyses—with higher levels of education proxying for more recent cohorts—we would expect education to be associated with increasing liberalism. But our actual finding, from Figure 1, is that education is associated with increasing conservatism. The finding is thus hard to square with the claim that the estimates reported in Figure 1 represent general cohort effects rather than the specific effects of education.

Of course, Panels 1-3 depict only bivariate relationships. They control for nothing, and they are thus consistent with many different interpretations. For example, the growing liberalism that we observe in post-1950s cohorts may be due to age effects rather than to cohort effects. Nothing in Panel 3 permits us to distinguish these possibilities.

We can sharpen this descriptive analysis by considering panels that account simultaneously for cohort and either age or period. This is what Panels 4 and 5 do. Both panels examine a single item, the first GSS redistribution item. (The results are similar for all other outcomes.) In each panel, cohort is presented along the *x* axis. In Panel 4, the lines show how answers to the redistribution item vary across cohorts in four different age groups.[11] In general, this panel follows the patterns of Panel 3, and the similarity of the two panels implies that accounting for age may make little difference to our understanding of cohort effects. In particular, we see, in all four age groups of Panel 4, the same liberal trend among post-1950 cohorts that we saw in Panel 3.[12]

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[11] The decadal age groups depicted in Panel 4 are those for which the GSS has the most data. Other age groups follow the same patterns, but for ease of interpretation, Panels 4 and 5 have been limited to four lines: four age groups in Panel 4, four period groups in Panel 5.

[12] The regression estimates of education's effects reported in the last panel of Figure 1 are quite similar to those that we obtain from a minimal model that includes no controls for age. This similarity of estimates further suggests that accounting for age may make little difference to our understanding of cohort effects. See page A44.

Panel 5 follows the pattern of Panel 3 even more closely. And because it does, it suggests that the greater liberalism of later cohorts that we observed in Panel 3 is not an artifact of the year in which people were interviewed. The implication is that real and general forces, distinct from those associated with year of interview, may be pushing later cohorts in a liberal direction. If education merely proxies for these general cohort effects, we would expect it to be associated with more liberal attitudes, as it is greater among the more recent, more liberal cohorts. But as we saw in the previous section, education is instead associated with greater *conservatism.* This contrast—education associated with greater conservatism, even as more recent cohorts are associated with greater liberalism—suggests that education is unlikely to be proxying for general cohort effects in the article's main analyses.

Of course, these graphical analyses are speculative. They include few controls or none, and they speak only indirectly to education's effects. To take up the role of time in a more controlled way, we should return to the estimation of models like Equation 2.

To begin, one might fear that the effects attributed to education are instead due to other age-associated processes. For example, aging is correlated with the assumption of new responsibilities and work experience that may affect redistributive attitudes. But age is unlikely to be a confound in the analyses presented here, as the baseline model (Equation 2) includes a polynomial term that allows for flexible effects of age—increasing over some periods, decreasing over others. The association of education with conservative attitudes also holds when we restrict the analysis to relatively old subjects or simply omit age from the analysis (pages A44 and A40). Taken together, these results imply that age is unlikely to be a confound.

By the same token, one might worry that the effects attributed to education are instead due to other period-related processes. But the year-of-interview fixed effects in the baseline model control for period effects in a very flexible fashion, allowing each interview year to have a distinct effect on respondents' answers to the outcome questions.[13] This flexibility of control for different periods makes it less likely that unaccounted-for period effects are biasing the estimates.

Concerns about cohort effects require a more elaborate strategy. For example, schooling laws are enacted by politicians who may be responding to political incentives. Changes in a state's political culture, then, may lead to changes in its schooling laws. Patterns of this sort may not be problematic, in the sense that they may not violate the conditions for IV estimation. But if the culture of one's childhood state of residence affects educational attainment or adult attitudes both through schooling laws and through another path, and if the model does not account for these cohort effects, then the ignorability condition is violated. And if the enactment of a strict schooling law changes something other than educational attainment that in turn affects adult attitudes, the exclusion restriction is violated. In either case, the estimates would be biased because of the failure to account for cohort effects.

The baseline model accounts for cohort effects by using a linear trend for the year in which subjects turned 14. But this control variable cannot account for nonlinear cohort effects. In particular, it cannot speak to the possibility that the apparent effects of education are really due to the temporal clustering of attendance-law changes. For example, if many changes to attendance laws occur in 1968, and if coming of age in the distinct culture of 1968 causes you to hold views that you wouldn't hold if you had come of age at another time, some of the effects attributed to education may really be due to a year-specific cultural change. This year-specific cultural change would be a cohort effect for which the baseline model does not account. A glance at Figure A2 (page A14) should assuage concerns of this sort, as it shows that the history of attendance laws does not feature this sort of temporal clustering. But we can more systematically account for the possibility by adding fixed effects for each cohort-year to the model.

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[13] The exception is that 1994 and 1996 are constrained to have the same effect. See page A53.

Some authors refrain from using cohort-year fixed effects with small datasets like the ANES and the GSS. Such fixed effects account for the nonlinear impact of national events over time, but given that most schooling decisions are made by states and municipalities, these authors argue that national events are unlikely to have an important additional influence on the effects of schooling (Clay, Lingwall, and Stephens 2012, 16). They further note that the use of cohort fixed effects discards potentially relevant information and thereby weakens the instruments (Milligan, Moretti, and Oreopoulos 2004, 1678n21; see also Lochner and Moretti 2004). But Figure 2 shows that, in this study, cohort fixed effects do not much change the estimated effects of education. Averaging across the first five outcomes, the estimates that include these controls are 1% smaller than the baseline estimates. None of the differences for individual items approach statistical significance. At the same time, the addition of cohort fixed effects increases the standard errors by an average of 36%, and estimates for two items are thereby pushed below the conventional threshold of statistical significance. (With cohort fixed effects, the estimate for the second redistribution-to-the-poor item is .056, SE .040; for the health care item, it is .073, SE .039.) Even so, the stability of the estimates under this specification suggests that the baseline results reflect a real effect of education rather than an effect of some other cohort-related variable.

Impact of the South

Inspection of Figure A1 (page A13) shows that within-state variation in the strictness of attendance laws has been especially great in southern states. For example, there are seven states that required no education at all in at least one year from 1910 through 2010, and all of these states are southern states. Moreover, the median range of years of schooling required by southern states in this period was seven years: while some southern states required a substantial amount of schooling throughout this period, a greater number of southern states changed substantially, requiring little schooling at some times and much more at others. By contrast, there is much less attendance-law variation in non-Southern states during this period. For example, the median range of years of schooling required by non-Southern states was only three years.

Because the effects of education in the article are identified via within-state changes in the strictness of attendance laws, this pattern of regional variation gives rise to the possibility that the results are heavily driven by Southern states. To investigate this possibility, I divide the ANES and GSS samples into two groups—people who lived in the South when they turned 14, and people who did not—and I estimate education's effects separately for each group.[14]

Figure A10 reports the estimates. Dividing the sample in this way makes it impossible to detect statistically significant differences between groups. Certainly, there is no strong suggestion here that the overall results reported in Figure 2 are driven to a great extent by those who grew up in the South.[15]

Imputation of Missing Data

When observations are missing on particular variables for some respondents, analyses involving those variables may be biased in unclear ways. Missingness is of particular interest in this article

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[14] In this section, "Southern states" refers to the sixteen states that the U.S. Census Bureau designates as "the South": Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. Washington, D.C. is also counted as part of the South in this analysis, as it is in the Census definition.

[15] A similar figure, not shown, depicts the estimated effects of education not on all southerners but on white southerners alone. Nothing is to be learned from this analysis: with sample sizes ranging from 6,256 to as low as 2,670, the uncertainty in the estimates makes them nearly meaningless.

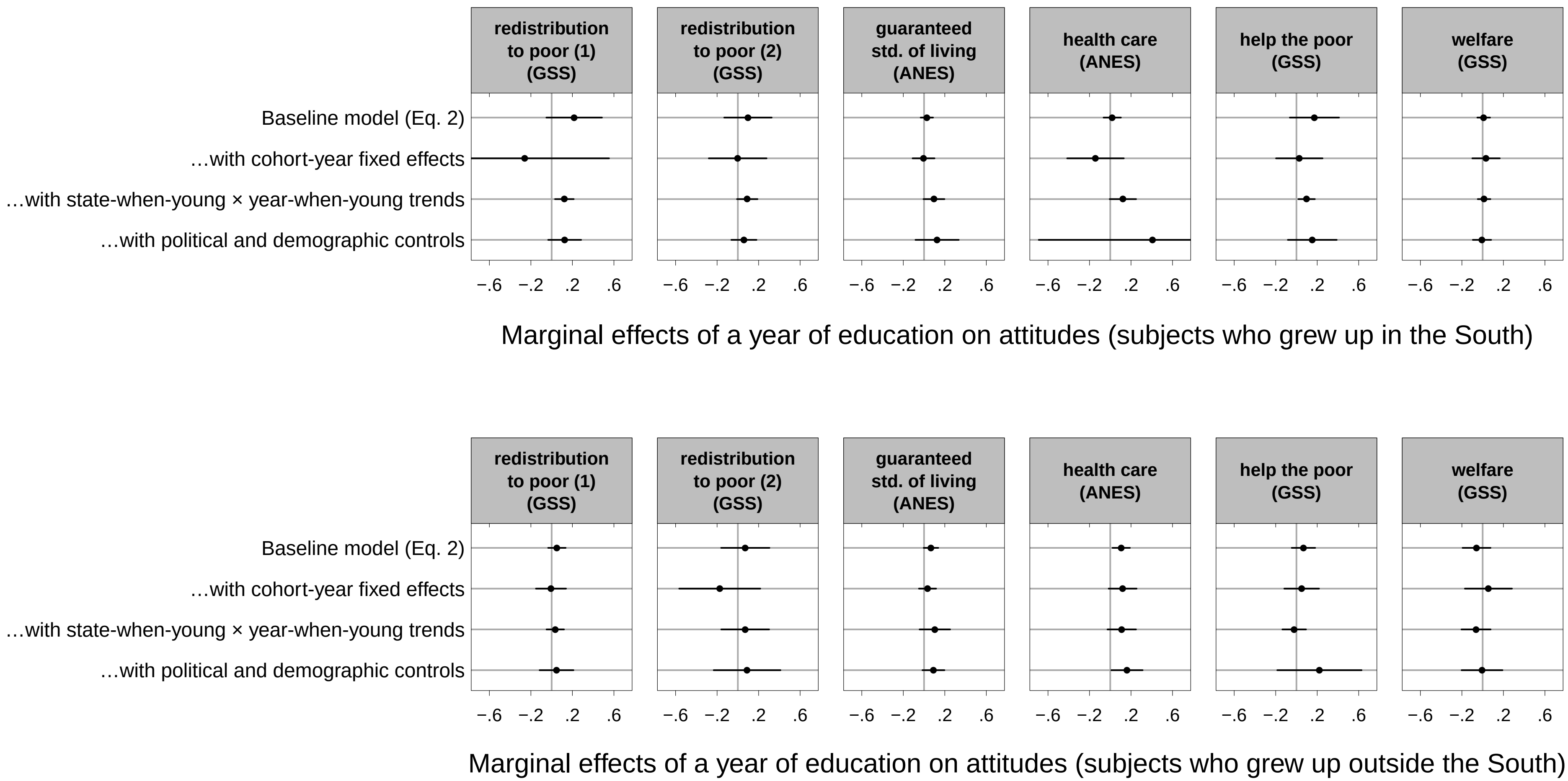

Figure A10: Effects of education on redistribution-related attitudes (South and non-South). Each plotted point is an estimate from a separate instrumental-variables regression. Black lines are 95% confidence intervals. Standard errors are clustered at the state-year level. The estimates are akin to those in Figure 2; the difference is that these estimates are based on data from only those who turned 14 when living in the South (top panels) or outside the South (bottom panels). Sample sizes in these regressions range from 3,783 for the second "redistribution to poor" item (bottom row in the top-tier panel) to 17,154 for the first "redistribution to poor" item (top row in the bottom-tier panel).

because it is typically correlated, in survey data, with levels of education (Rapoport 1979; Narayan and Krosnick 1996, esp. 69–70).

In the analyses reported here, almost no data are missing for the treatment variable, years of education. It is available for 99.7% of GSS respondents and 99.6% of ANES respondents. Missingness rates for the six outcomes analyzed in the ANES and the GSS are also under 1%, in the sense that almost no one who began an interview refused to answer a relevant question or ended an interview before a question could be asked. On the other hand, 10% and 14% of ANES subjects answered “don’t know” in response to the health-care and standard-of-living questions, respectively. And the propensity to answer “don’t know” is clearly correlated with education among both ANES respondents and (to a much lesser extent) GSS respondents.

Figure A11 gives a more detailed view of missingness in the treatment and outcome variables that are used in the article. It depicts the degree of missingness for each year in which education or an outcome of interest was measured. (Data for years before 1970 are not shown because no outcome of interest was measured before 1970.) In this figure, missingness is defined to include “don’t know” responses to questions as well as refusals to answer or cases in which a respondent ended an interview before she could be asked a particular question.

Two patterns are apparent. First, almost no data are missing on education in either the GSS or the ANES. Second, missingness is more common in the ANES than in the GSS. The main reason is question formatting: for both ANES outcome questions (one about a government-guaranteed standard of living, another about health care), the ANES included an explicit “don’t know” option. But the GSS typically did not offer a “don’t know” option for the questions that are of interest here.

A related concern is about the correspondence between education and missingness. Figure A12 shows that, as in most surveys, data are less likely to be missing for more educated respondents in the ANES and the GSS. Each cell of the figure reports the difference, between those who did not graduate from high school and those who graduated from college, in the percentage of miss-

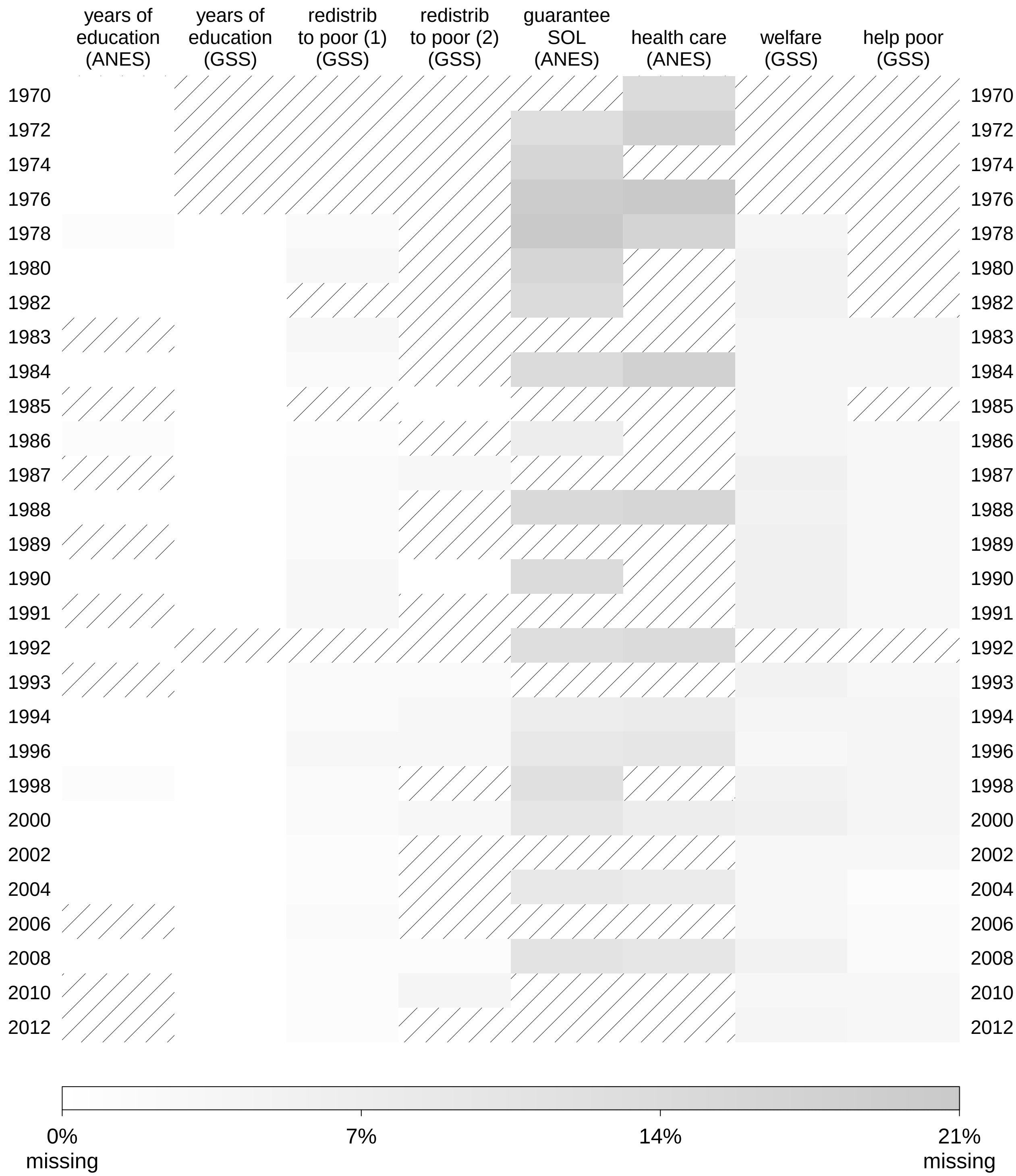

Figure A11: Missing Data in the ANES and GSS. Each cell indicates the extent of missingness in ANES and GSS variables. Data are coded as missing if a respondent skipped a question, otherwise refused to answer it, or answered "don't know" or "haven't thought much." Within each column, diagonal lines indicate that a question was not asked in a particular year. (Diagonal lines also appear for pre-1978 GSS cells and for the 2012 ANES cells. Missing state-of-residence-when-young data prevent the use of the ANES and GSS in those years.)

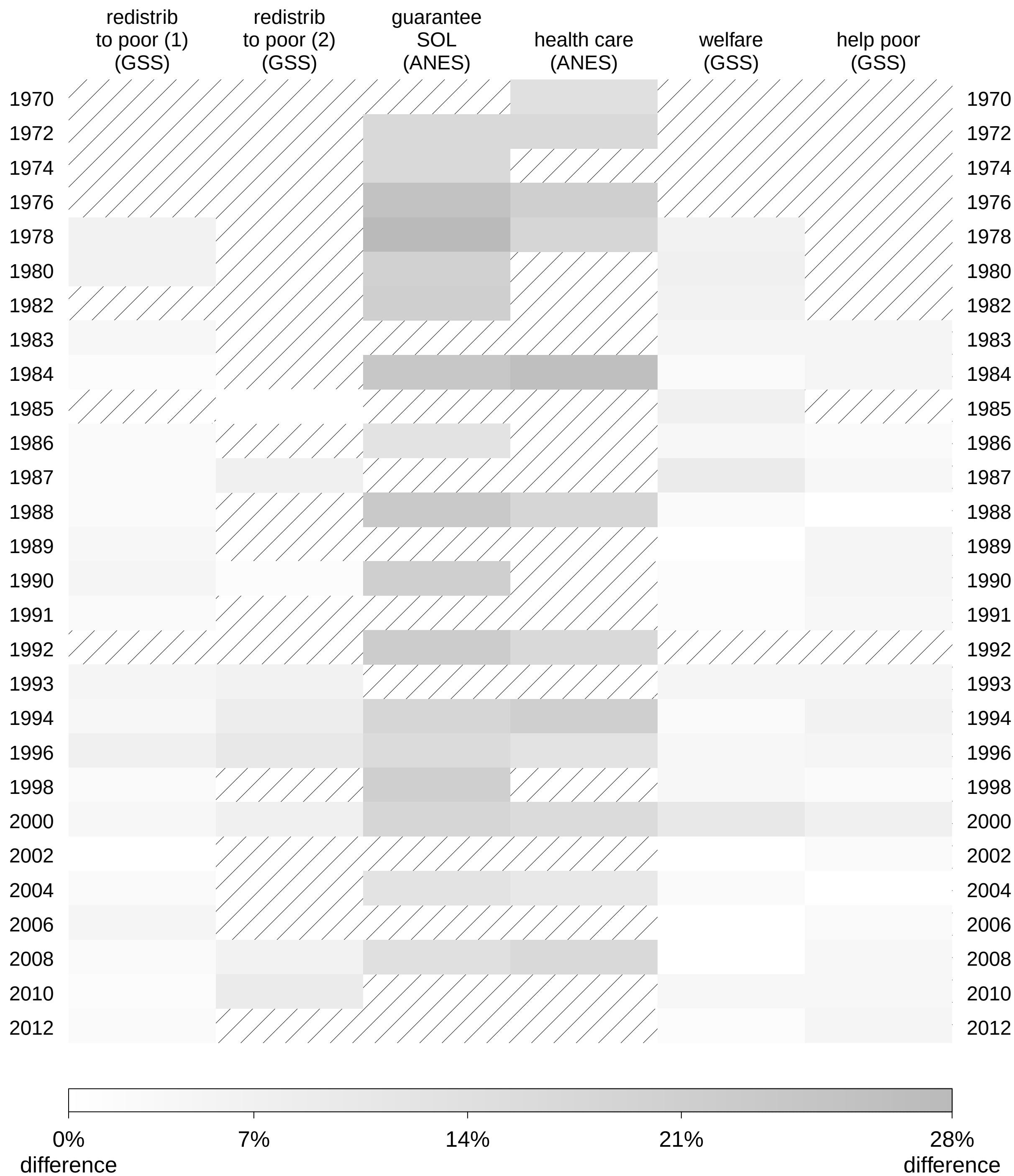

Figure A12: Differences in Missingness by Education Level. Each cell indicates (% missingness among college graduates) – (% missingness among people who did not graduate from high school) for a given question-year. For example, the cell for the welfare question in the 2000 GSS indicates that missingness was nine percentage points higher among those who did not graduate from high school. Within each column, diagonal lines appear for years in which a question was not asked of any subjects. (Diagonal lines also appear for pre-1978 GSS cells and for the 2012 ANES cells. Missing state-of-residence-when-young data prevent the use of the ANES and GSS in those years.)

ing data. “Missing data” is again defined to include “don’t know” responses. The main pattern in Figure A12 is similar to the one in Figure A11: education-based differences in missingness rates are higher for ANES items than for GSS items. The most plausible explanation, too, is the same: the ANES included an explicit “don’t know” option for the questions examined here, while the GSS typically did not.

Both overall missingness and education-based differences in missingness are due overwhelmingly to cases in which respondents replied to a question by saying that they “didn’t know” or “hadn’t thought much” about the issue at hand. Whether responses such as these are a problem for inference is a matter of interpretation. If one suspects, following Converse ([1964] 2006, esp. 245), that many people simply do not have positions on many issues, and if one further suspects that “don’t know” responses indicate the absence of such positions, then it is sensible to omit such responses from one’s analysis. This is the approach used in the current article, in all of the most closely related articles (e.g., Dee 2004; Milligan, Moretti, and Oreopoulos 2004; Alesina and La Ferrara 2005), and in much other political science research (e.g., Scheve and Slaughter 2001a,b; Bartels 2006, 221). But if one assumes that such responses reflect reticence rather than a true lack of opinion, a different approach is in order.

To speak to readers who may worry that “don’t know” responses mask real attitudes, I use `Amelia II` (Honaker, King, and Blackwell 2012) to impute substantive values for such responses under the assumptions that the missing and nonmissing data have a multivariate normal distribution and that the missing data are “missing at random,” i.e., that the patterns of missingness are a function of observed values. As above, “missing data” is defined here to include “don’t know” responses as well as the rarer cases in which an interviewee refused to answer or ended an interview before she could be asked a relevant question.[16]

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[16] The assumption that all data are drawn from a multivariate normal distribution can be only approximately true. But it is a common assumption because “experience has repeatedly shown

I do not impute values of outcome variables for years in which the outcome questions were not asked. Because different questions were asked in different years, a different set of imputations is created for each outcome.

All variables used in the instrumental-variables analyses are included in the imputation model for each dependent variable. Fifty-five additional variables that have often been measured in the ANES and the GSS were considered for inclusion: if $R^2 > .005$ for a regression of the dependent variable on one of these 55 variables, it was included in the imputation model. A complete list of the additional variables considered for inclusion appears at the end of this section.

Figure A13 reports estimates of education's effects that are based on datasets in which missing and "don't know" entries are replaced with imputed values. On average, the estimates are 22% smaller, but they remain conservative and sizable—and given that the standard errors are also smaller, the statistical significance of the estimates scarcely changes at all. In particular, imputing data causes only five of the estimates to rise above the conventional thresholds of $p = .05$ or $p = .10$. Where the second redistribution-to-the-poor question is concerned, p changes from .037 to .125 in the baseline model. For the guaranteed-standard-of-living question, it rises from .047 to .058 in the model that includes state-when-young × year-when-young trend variables. And for the health-care question, it changes from .080 to .130 in the model that includes cohort-year fixed effects, from .030 to .091 in the model that includes state-when-young × year-when-young trend variables, and from .036 to .058 in the model that includes political and demographic controls.

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that MI [multiple imputation] tends to be quite forgiving of departures" from the assumption (Schafer and Olsen 1998, 550). Moreover, the assumption seems to work as well as more complicated assumptions even for categorical data (Schafer 1997, esp. ch. 6).

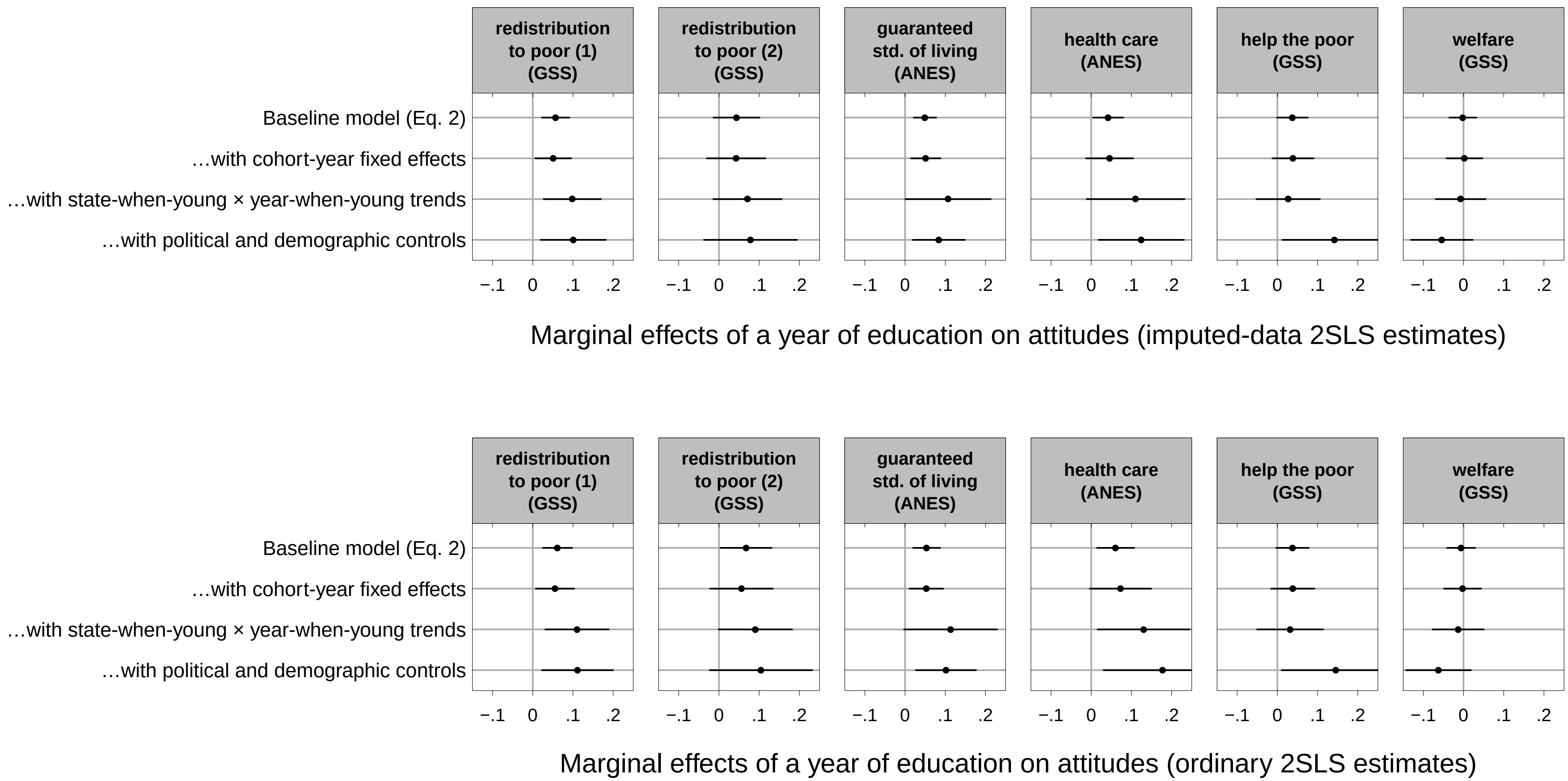

Figure A13: Effects of education on redistribution-related attitudes (missing-data-imputed estimates). Each plotted point in the top panels is a 2SLS estimate from a separate regression. Some of the data for these regressions have been imputed. Black lines are 95% confidence intervals. Standard errors are clustered at the state-year level. To aid comparison, the estimates from Figure 2, which are not based on imputed data, are depicted in the bottom panels.

VARIABLES THAT WERE CANDIDATES FOR INCLUSION IN THE IMPUTATION MODELS

All variables used in the IV analyses were used in the imputation models. In addition, the following variables were considered for the imputation models that used GSS data:

- *Demographic:* wrkstat, wrkslf, prestg80, hompop, income, rincome.
- *Gender attitudes:* fehome, fework, fepres, fepol, fechld, fepresch, fefam.
- *Racial attitudes:* racmar, raclive, rachome, busing, racpres, racdif1, racdif2, racdif3, racdif4, helpblk.
- *Spending attitudes:* natspac, natenvir, natheal, natcity, natcrime, natdrug, nateduc, natrace, nataid, natroad, natsoc, natmass, natpark.
- *Tolerance:* spkath, colath, libath, spkrac, colrac, librac, spkcom, colcom, libcom, spkmil, colmil, libmil, spkhomo, colhomo, libhomo.
- *Other variables:* partyid, polviews, relig, reliten.

And these variables were considered for the imputation models that used ANES data:

- *Demographic:* VCF0111, VCF0114, VCF0118, VCF0119, VCF0127, VCF0128, VCF0130, VCF0137, VCF0143, VCF0146, VCF0147, VCF0156, VCF0157, VCF9002.
- *Group thermometers:* VCF0206, VCF0207, VCF0208, VCF0209, VCF0210, VCF0211, VCF0212, VCF0220, VCF0223, VCF9004, VCF9006.
- *Ideology, issue positions, and related questions:* VCF0803, VCF0830, VCF0834, VCF0838, VCF0839, VCF0846, VCF0847, VCF0851, VCF0852, VCF0853, VCF0854, VCF0867A, VCF0871, VCF0872, VCF0880, VCF0881.
- *Partisanship:* VCF0301, VCF0201, VCF0202, VCF0218, VCF0224, VCF0324, VCF0411.

- *Trust and support of the political system:* VCF0604, VCF0605, VCF0606, VCF0619, VCF0620, VCF0621, VCF0704A.

Income and Employment by Level of Education

In the article, I argue that the conservative effects of high school on attitudes toward redistribution may be due partly to the ways in which education changes self-interest considerations. At the margin, education's effects on income, employment, and related human-capital factors may make educated people less likely to benefit from redistribution, which may in turn reduce their support for redistribution. This chain of hypothesized effects is most plausible when redistributive programs are targeted to those whose incomes are below a certain threshold, as they often are in the United States.

But this chain of hypothesized effects begins with the claim that education does, in fact, increase income, employment, and other human-capital factors. Is the claim correct? In Figure 3, I show that it does seem to be correct for the people in my sample. And much research suggests that claim holds generally in the United States (e.g., Goldin and Katz 2008, esp. 71–84). But we can turn to another sample—that of the United States Census Bureau—for additional evidence.

Figure A14 shows that, among respondents to the 2010 American Community Survey, education is powerfully associated with higher income and lower unemployment. For example, the figure shows that those whose education ended with 10th grade had an average income of $9,474, while those whose education ended sometime in 12th grade—but who did not receive a GED or a diploma—had an average income of $17,228. The corresponding decline in unemployment was from 27% to 20%. These data are purely descriptive, but they do suggest that education's effects on economic attitudes may be transmitted through self-interest mechanisms.

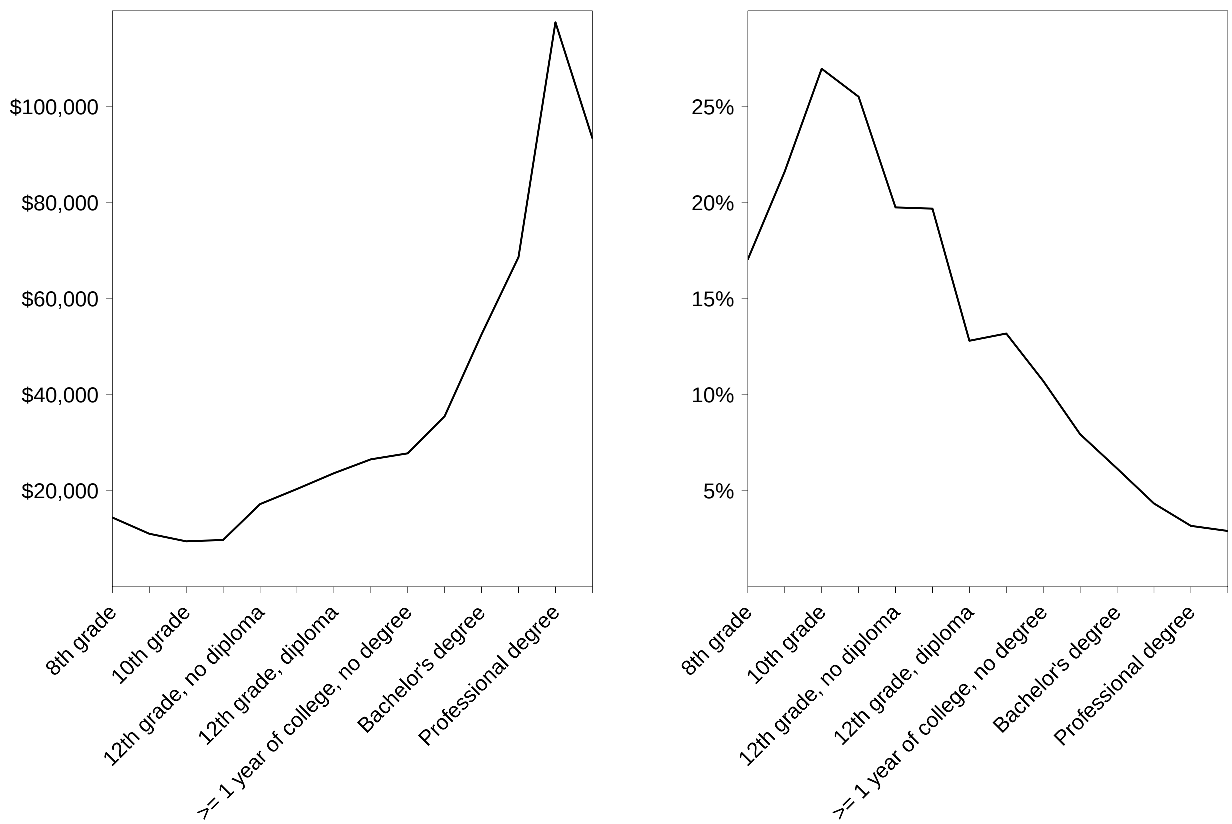

Figure A14: U.S. Income and Unemployment by Level of Education. The panels present income and unemployment rates by level of education. Data are from the 2010 American Community Survey. Income is reported in 2010 dollars.

Other Considerations

Limitations of Experiments in the Study of Education

The scholar who wants to estimate education's effects confronts methodological difficulties that may be even greater than those that arise in most other areas of social and political inquiry. This is so because the array of forces that affect educational attainment seems vast, and because the difficulty of observing many of those forces, let alone all of them, seems especially great. (See the "Empirical Difficulties in the Study of Education's Effects" section of this article. See also Kam and Palmer 2008, esp. 614–17, and Henderson and Chatfield 2011.)

A common move in such cases is to conduct experiments. But true experiments in education are rare. And even when they are conducted, other obstacles usually preclude their use in political science analyses. Some of the most ambitious experiments, like Tennessee's Project STAR study of class-size effects, are beset by implementation problems (Howell and Peterson 2004; Hanushek 1999). Others involve too few students. Large and well-implemented experiments have been conducted, but they rarely involve the collection of data on politically relevant attitudes. They are also likely to affect many variables in addition to the amount of education that children receive, and they are therefore unlikely to isolate the effects of receiving more education rather than less.[17] Finally, some studies involve the use of discontinuities or random lotteries in school admissions. They can be used to examine the effects of attending one school instead of another (e.g., Dobbie

[17] Consider the Perry Preschool Experiment. Treated students received a year or two of preschool. They and their mothers also received weekly 90-minute visits by teachers to their homes. The treatment increased graduation rates, but it also independently raised student IQ and altered student and parent behaviors (Schweinhart, Barnes, and Weikart 1993, 72–74, 137; Schweinhart and Weikart 1980, 47, 70–71). It is therefore unlikely that the effects of the experiment on adult outcomes are due exclusively to its effects on educational attainment.

and Fryer 2013). But these studies, too, are not well suited to telling us about the effects of receiving more education rather than less. It is for these reasons that scholars who focus on accurately estimating the effects of educational attainment turn mainly to quasi-experimental designs like the one used in this article, rather than to actual experiments.

School Quality

One may worry that the estimates of education's effects in Figure 2 are confounded by school quality. School quality is an unobserved factor (inasmuch as it is distinct from teacher salaries, teacher-pupil ratios, college attendance rates, and the other variables that *are* observed), and states with stricter attendance laws may have higher-quality schools. Even so, school quality is unlikely to confound the estimates.

For school quality to confound the estimates, it would need to vary across states: because the instruments are measured at the state level, within-state variation in school quality cannot affect the estimates. But the state-level fixed effects that are included in all models will account for state-level variation in school quality that is unchanging over time. So the confound must be subtler: it must be due to simultaneous variation in school quality across states and years, rather than to stable school-quality differences across states. But the state-when-young × year-when-young trend variables that are added to the model account for this more complex sort of variation. And the demographic controls—in particular, the state-year data on teacher-pupil ratios and teacher salaries—account for this more complex sort of variation in an even more flexible fashion. So the objection must be subtler still: for a school-quality confound to exist, it must be due to variation in school quality that changes across both states and years, such that school-quality differences between states themselves vary across years; and these differences must be of a sort for which neither the demographic controls nor the state-year trend variables can account.

Figure A6 and Table 1 suggest that this subtler objection is implausible. If the connection between strict attendance laws and years of education received is driven by the presence of higher-quality schools in strict-attendance-law states, parental education should not moderate the link between attendance laws and years of education, and the laws should have more than a meager ability to account for variation in college enrollment. These are not the patterns that we observe in Figure A6 or Table 1. It is therefore hard to conclude that school quality is a confound that biases the estimates reported in Figure 2.

Enforcement of Attendance Laws

Throughout the nineteenth century, compulsory attendance laws were generally not well enforced (Clay, Lingwall, and Stephens 2012, 10–11; Eisenberg 1988; Lleras-Muney 2005, 197). But by 1910, compliance with the laws had become a strong norm. In part, this was true because laws were enacted after majorities of youth in each state had become "always-takers" who would have been attending even in the absence of attendance laws (Goldin and Katz 2011, esp. 290; see also Clay, Lingwall, and Stephens 2012, 10–11). But it was also true because many attendance laws were enacted as much to deter truancy and vagrancy as to promote education (Goldin and Katz 2011, 303). Growing compliance with attendance laws was also aided by the passage of complementary child labor laws, in the early part of the 20th century, in most states. These laws were well enforced, and by foreclosing the option of employment to children, they helped to ensure that children remained in school (Margo and Finegan 1996, 107).

Compliance had also increased by the beginning of the 20th century because most schooling laws in force at that time specified—as earlier laws had not—mechanisms for enforcement (Clay, Lingwall, and Stephens 2012, 8). The main tool for enforcement was the establishment of a "school census": a roster of every child who was supposed to be enrolled in school (Kotin and Aikman 1980, 32–33; see also Deffenbaugh and Keesecker 1935, 23–25). These rosters guided the efforts of municipal officials to enforce attendance laws, and by 1928, every state law required each school district to have one (Goldin and Katz 2011, 302). A complementary early-20th-century development was the hiring of "truant officers" whose jobs were created for the express purpose of attendance-law enforcement (Schmidt 1996, 121).

There is thus little reason to think that noncompliance was a large problem in the period under consideration, 1910-2010 (although see Deffenbaugh and Keesecker 1935 for an occasionally contrary view about enforcement in the earliest part of this period). Moreover, to the extent that enforcement varied across states, the validity of the estimates is not threatened, for the reason elaborated in this article: identification of the effects of schooling in this article comes from within-state changes in schooling laws, not from cross-state comparisons. Any threat to the validity of the estimates must therefore come from within-state variation in enforcement, not from across-state variation. And the literature on attendance-law enforcement does suggest two ways in which enforcement may have varied within states: it may have varied by race and between urban and rural areas.

In the United States, schooling laws have had weaker effects on blacks and Native Americans than on whites (Lleras-Muney 2002, 416; Goldin and Katz 2008, ch. 6; Clay, Lingwall, and Stephens 2012, 15). This pattern may have arisen because of differences in enforcement that were only *de facto*, but *de jure* explanations seem more likely. Compulsory attendance laws do not apply to Native Americans who live on reservations. And many state laws include exceptions for students who do not live in the vicinity of a public high school. These exceptions were more likely to apply to black children than to others—especially because, in the pre-*Brown vs. Board of Education* South, schools were segregated and many black children were exempted from attendance laws because they did not live in the vicinity of public high schools that served blacks (Margo 1990, 20; Lleras-Muney 2002, 416).

Many states also made exceptions, especially before 1940, for children who lived in rural areas (Schmidt 1996, 121) or who left school to work in agricultural occupations (Kotin and Aikman 1980, 67–68). Moreover, the sparser population of rural areas may have made enforcement more difficult in those areas (e.g., Jernegan [1931] 1960, 115). On the other hand, fewer resources may have been required to ensure attendance of children in rural areas, simply because there were fewer children in those areas. And in the last three decades, dropout rates have been approximately equal in rural and urban areas after controlling for other factors (Jordan, Kostandini, and Mykerezi 2012), which suggests that enforcement may not have varied in those decades across the rural-urban continuum.

Considering all of the evidence, it seems likely that there was some systematic within-state variation in enforcement of attendance laws, such that black children, Native American children, and rural children were less exposed to the laws than others. As noted above, within-state variation in enforcement was greater in earlier parts of the century. It is thus a small minority of respondents in the dataset who would have been systematically subject to weaker enforcement (or no enforcement) of the laws. Nevertheless, this variation in enforcement does affect interpretation of the estimates reported in the article.

There are several reasonable ways to think about how the estimates should be interpreted in light of the variation in enforcement, but the best way is suggested by the discussion, in the article, of the estimates as *local average treatment effects*. That is, the estimates do not indicate the average effects of schooling on all people in the sample. They instead indicate the average effects of schooling among “compliers”: those who were induced by strict schooling laws to stay in school longer than they otherwise would have, and those who would have stayed in school longer had they grown up under stricter laws. This is the population for which the effects of schooling are knowable, given the use of schooling laws as instruments. By contrast, we cannot know the effects of schooling among “always-takers” who would have stayed in school for a long time regardless of the type of attendance law to which they were subject. Nor can we know the effects of schooling among "never-takers" who would have dropped out at relatively early ages regardless of the type of attendance law to which they were subject. When we use attendance laws as instruments, we learn about the effects of schooling among those whose level of schooling is affected by attendance laws. (For further discussion of local average treatment effects, compliers, always-takers, and never-takers, see Angrist, Imbens, and Rubin 1996, 448–49.)

The LATE interpretation of the estimates is directly relevant to within-state variation of enforcement. It is probable that some rural, black, and Native American respondents in the ANES and GSS grew up in states that had moderate or strict schooling laws, but were not personally subject to those laws. And if the laws were not enforced among these subjects, these subjects are unlikely to have been compliers: they are unlikely to have been induced by the laws to stay in school. Because these respondents were not compliers, the estimates in the article do not apply to them. Moreover, the effects of schooling on these subjects are unknowable, at least given the identification strategy used in this article. (But see page A89, which shows that the main estimates of the article do not differ when I restrict the dataset to data from whites alone.)

High School Graduation Rates

There are many arguably legitimate ways to estimate the high school graduation rate (Rumberger 2011, ch. 3). Most of these methods show that pre-2010 graduation rates peaked in the early 1970s at slightly below 80%. Reports of higher rates before 2010 tend to rely on the classification of recipients of graduate equivalence degrees as graduates (Rumberger 2011, 83; Heckman and LaFontaine 2010) or on grossly misleading practices. For example, New Jersey counted some out-of-state GED holders as in-state graduates to inflate its graduation rate (Heckman and LaFontaine 2010, 247–48). And until recently, North Carolina's Department of Education defined the gradua-

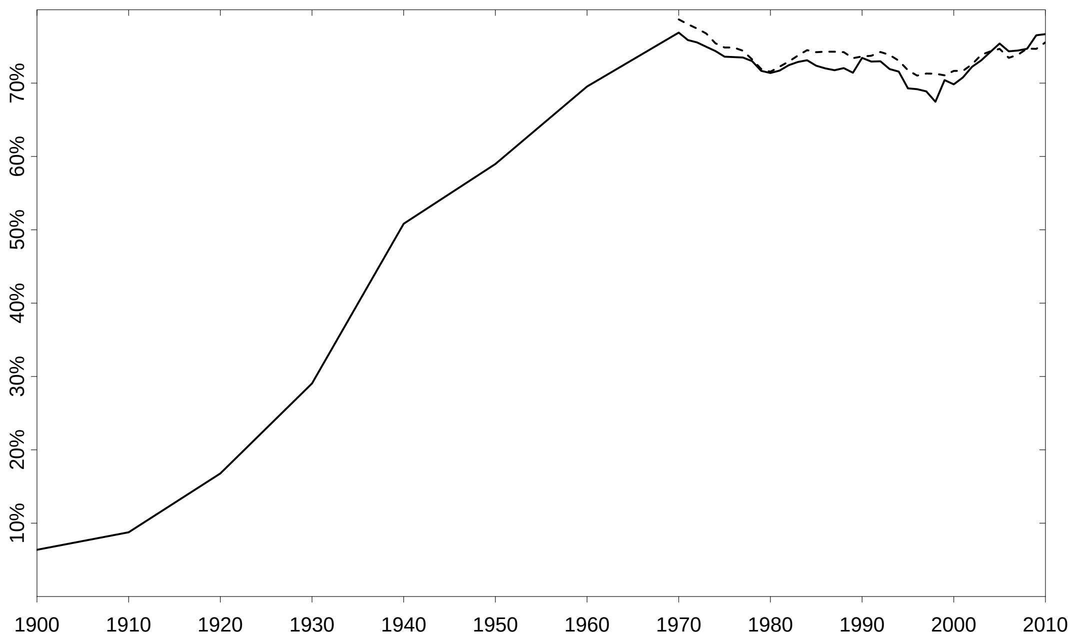

Figure A15: U.S. High School Graduation Rates, 1900-2010. The solid line indicates the percentage of the 17-year-old population that has graduated from high school. The dashed line indicates the "averaged freshman graduation rate": the percentage of public school students entering ninth grade who graduate within four years. Data are from Snyder and Dillow (2010, Table 110).

tion rate as the proportion of *graduates* who graduate within four years—thereby ignoring dropouts entirely and massively overstating the true graduation rate (Swanson 2004, 93).

Graduate Equivalence Degrees

Until 2012, neither the ANES nor the GSS distinguished between respondents who hold a high school diploma and those who hold a graduate equivalence degree (GED). The conflation of these groups is apparent in a question that ANES interviewers used from 1974 through 2008 to measure levels of education: "Did you get a high school diploma or pass a high school equivalency test?" Answers were coded as "yes" or "no." (See, for example, American National Election Studies 1974, 642.) Until 2012, this was the only question in the ANES time-series studies that mentioned equivalency degrees, and before 1974, equivalency degrees were not mentioned at all in ANES questionnaires. The situation for GSS measurement of education is almost identical in this respect (e.g., National Opinion Research Center 2013, 3100).[18]

With respect to intelligence and academic ability (as indicated by standardized tests), GED-holders seem similar to high school graduates who don't pursue further education (Heckman and LaFontaine 2010, 245; Heckman, Humphries, and Mader 2011). But with respect to other characteristics—for example, perseverance, personal efficacy, and self-esteem—GED-holders seem more similar to high school dropouts (Heckman, Humphries, and Mader 2011, 446–52). The adult earnings of GED-holders are also closer to those of dropouts than to those of high school graduates who don't pursue further education (Heckman, Humphries, and Mader 2011, 432–46). Extended discussion of these points can be found in Heckman, Humphries, and Kautz (2014).

The conflation of GED-holders and graduates is unlikely to affect the estimates reported in the article, which are estimates of the marginal effects of a year of secondary schooling. It is more likely to affect the estimates in a later section of this appendix, which are of the effects of graduation from high school. Specifically, the conflation of GED-holders and graduates is likely to cause the estimates in that section to *understate* the true effects of high school. For example, if high school graduation makes economic attitudes more conservative because it increases one's future earnings, conflating GED-holders (who earn relatively little) with graduates (who earn more) may cause the estimates to understate the true effect of graduation from high school. Similarly, if graduation makes attitudes more conservative because it proxies for learning or socialization that occurs

[18] The measurement issue is further complicated by state policies on the awarding of diplomas to holders of GEDs. As of 2007, ten states issued high school diplomas to people on the basis of their GED scores, even though they had never graduated from high school (Heckman and LaFontaine 2010, 247).

in high school, the conflation of GED-holders (who typically complete fewer than four years of high school) with graduates may also cause the estimates of the effects of high-school graduation to be understated.

All of that said, the conflation of graduates and GED-holders probably makes little difference to the estimated effects of high school graduation. The main reason is that, although the GED is far more popular than it was 40 years ago, it remains rare in an absolute sense: see the solid line in Figure A16. Following Heckman, Humphries, and Mader (2011, 425), the figure shows that the annual fraction of completers (i.e., people who receive a high school diploma or a GED) who are GED-holders has never reached 20% and is now approximately 12.5%. Indeed, the figure shows that GEDs have become markedly less common in the past fiteen years. Moreover, many ANES and GSS respondents reached age 24 in years during which the GED was still quite rare. (The large majority of GEDs are awarded by age 24: see Heckman, Humphries, and Mader 2011, 469.)

Although neither the ANES nor the GSS have typically asked respondents whether they received a GED, it is possible to render a rough estimate of the proportion of respondents in each cumulative file who hold GEDs. To do so, we multiply the percentage of completers in any year who hold GEDs by the number of completers among all respondents who turned (say) 18 in that year. For example, 473 completers in the 1948-2008 ANES cumulative file turned 18 in 1956, and 2.8% of all people who completed high school in that year received a GED. We may estimate that $473 \times .028 = 13.2$ ANES respondents who turned 18 in 1956 are GED recipients. Extending the exercise from 1956 through 2007, the latest year for which data are available, suggests that 3.4% of ANES respondents and 5.2% of GSS respondents in the cumulative files are GED recipients.

Of course, not everyone who receives a GED receives it at 18. Many receive it at slightly later ages. But these differences do not change the estimates much. For example, if we match the percentage of GED recipients among all completers in a given year to the cumulative-file

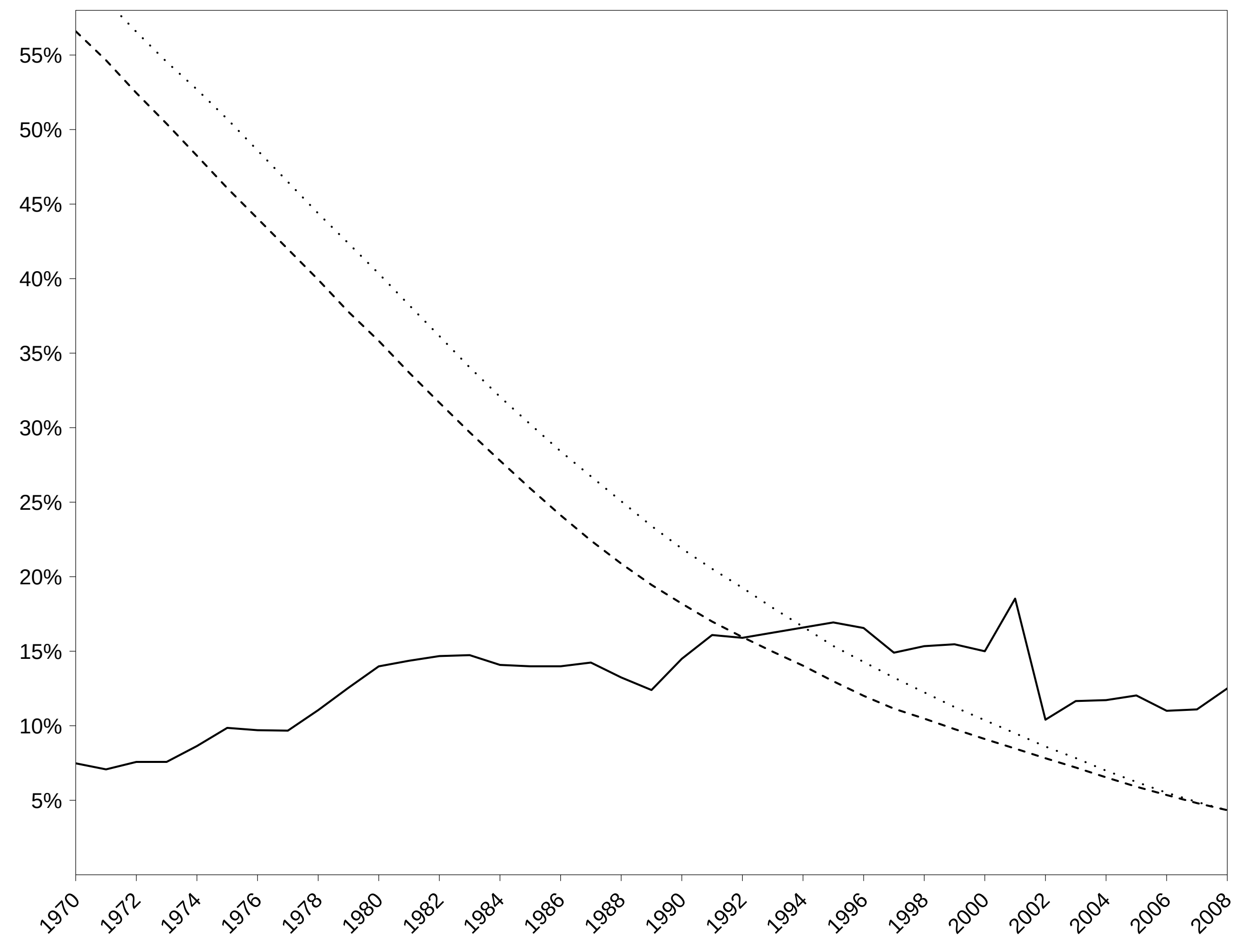

Figure A16: GED Recipients as Proportions of All High School Completers. The solid line shows the percentage of high school completers in each year who received a GED. ("Completers" are those who receive a diploma or a GED.) Data are from Heckman, Humphries, and Mader (2011, 425).

The dashed line shows the percentages of respondents in the ANES cumulative file who (a) were interviewed in or after 1970 and (b) turned 24 in or after the year indicated by the *x* axis. The dotted line shows the corresponding percentages for respondents in the GSS cumulative file. For example, the dotted line shows that, of all GSS respondents interviewed in or after 1970, about 11% turned 24 in 1994 or in a later year.

The main lesson of the figure is simple: estimates that are based on the ANES or GSS cumulative files are unlikely to be much affected by the conflation of GED recipients with other high school completers. This is so for two reasons. First, the absolute percentage of GED recipients remains low. Second, most respondents in the cumulative files reached adulthood when GEDs were relatively uncommon. Respondents in the cumulative files are therefore unlikely to be GED recipients. (Most noninstitutionalized GED recipients receive the GED by age 24: see Heckman, Humphries, and Mader 2011, 469.)

ANES and GSS respondents who were interviewed before 1970 are eliminated from the analysis because no data from those interviews are used in the article. (None of the outcomes analyzed in this article were measured before 1970.)

respondents who turned 24 (not 18) in that year, we estimate that 4.4% of ANES respondents and 6.4% of GSS respondents are GED recipients.

Even these low percentages are likely to be overestimates. The reason is that much of the growth in GED-holding over the last two decades is due to the use of the GED in prisons (Heckman, Humphries, and Mader 2011, 466). The ANES and the GSS sample household members—not prisoners or other institutionalized people—and the proportions of GED holders among ANES and GSS respondents is therefore likely to be smaller still.

Respondents' Interpretations of Outcome Questions

One may suspect that the main finding—that secondary education causes people to be less supportive of redistribution—comes about because well and poorly educated respondents interpreted the outcome questions in different ways. In particular, well and poorly educated respondents may be equally opposed to taxation, but well-educated respondents may think that the policies described in the outcome questions entail greater taxation, while poorly educated respondents may not draw the connection between taxation and redistribution.

This possibility certainly would not upend the main finding of this project. It would remain true that secondary education causes people to be less supportive of redistribution. Rather, this possibility would speak to the mechanisms that account for the finding. To the two mechanisms discussed in the body of the article, we would add a third: the effects of education on support for redistribution would be due partly to the way that education causes people to make different inferences about what redistribution entails.

The ANES and GSS themselves cannot speak to this possibility. To examine the matter, I therefore conducted a new survey in which I probed people's understandings of the outcome questions. I conducted the study through Amazon.com's Mechanical Turk service in August 2015. Mechanical Turk is sometimes subject to criticisms on the ground that it is not representative of the American population, especially with respect to education—and indeed, it typically is not. Among Mechanical Turk "workers," the well educated are much better represented than the poorly educated (e.g. Krupnikov and Levine 2014, 66). But in this case, I was able to speak to the concern by oversampling those with low levels of education—no more than a high-school degree—from the pool of Mechanical Turk workers. (I did this by drawing from a set of Mechanical Turk workers who had previously offered demographic information about themselves, including their highest level of education, in an unrelated study. I measured education levels again during my own study.)

Three hundred and eighty-two respondents were recruited to take a "study of cognitive styles" and subsequently passed a screening test designed to ensure that they were attending to the questions in the survey. Of these respondents, 136 had earned a four-year bachelor's degree or a more advanced degree, while 118 reported that 12th grade or a lower grade was their highest level of schooling. The analyses below involve comparisons of these two groups.

All respondents were asked the six main outcome questions used in this study (in random order). But immediately after each question, subjects were also asked two additional, related questions: "If the government [undertakes the policy in question], how likely will it be to raise taxes?" and "how likely will it be to raise *your* taxes?" Seven response options were offered in each case, ranging from "very unlikely" (coded 1) to "very likely" (coded 7).

Inspection of the answers suggests that well and poorly educated respondents interpret the questions in very similar ways so far as taxes are concerned. Across all twelve questions about taxes, there is only one case in which the difference of means between these two groups approaches statistical significance. It lies with a question related to the second redistribution question in the GSS: both groups think that taxes are likely to increase if the government "tries to reduce differences in income," but the well educated see this outcome as slightly more likely than the poorly educated: 5.79 vs. 5.43, $p = .03$. However, the corresponding difference between the two groups with respect to "*your* taxes" is even more slight and statistically insignificant: 4.11 vs. 4.06,

$p = .83$. And for the other ten questions about taxes, the difference of means never exceeds .19 on the 1-7 scale, and $p \geq .29$ in every case. This descriptive exercise thus offers no support for the idea that weaker support for redistribution among the well educated can be attributed to differences in question interpretation across education levels.

Socially Desirable Responses to Questions about Mechanisms

Socially desirable responding (SDR) is the tendency to give socially desirable answers to questions when those answers do not reflect one's true feelings. One may suspect that SDR plays a role in the mechanism results that are reported in the article. It is unlikely to affect some of the items that speak to mechanisms—for example, the items in the verbal ability battery—but a role for it in responses to other items may be more plausible. Might responses to these other items—for example, questions about the work ethic of poor people—be affected by socially desirable responding?

To begin, note that the mere presence of SDR in responses to these questions may not affect the findings even if it is very widespread. To affect the findings, SDR must be correlated with education, such that educated people overstate, to a greater degree than others, their belief that (say) the poor are hard-working.

But the evidence for education-driven SDR is generally sparse.[19] There is noteworthy evidence of education-driven SDR for some self-report measures of behaviors—in particular, of voter turnout (Ansolabehere and Hersh 2012)—but those are not the measures at issue here.

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[19] It is especially sparse in studies of responses to questions about redistribution (or equality of any sort). Moreover, the bulk of evidence that does exist may point in the direction of no link between education and socially desirable responding to such questions (Heerwig and McCabe 2009; Knudsen 1995).

The lack of evidence aside, the critical point is that even if education does induce socially desirable responding in response to questions about the mechanisms studied here, it is unlikely to affect the conclusions about mechanisms. To see why, begin by noting that the items for which one might most expect education-driven SDR are probably those about the work ethic of the poor. Education is estimated to have approximately no effect on responses to these items, but if social desirability is at work, the true relationship between education and beliefs about the poor may be conservative rather than null. That is, if SDR is at work, education may make people more likely to believe that the poor are lazy. This result would be consistent with the suggestion that such beliefs mediate education's conservative effects on social attitudes. But it would leave the bulk of the results on perceptions of fairness unchanged: Figure 3 reports results for eight different measures of those perceptions, and SDR is unlikely to affect most of them. Thus, even if SDR does affect responses to questions about the work ethic of the poor, it leaves intact the overall pattern of mixed and weak results where education's effects on perceptions of fairness are concerned.

Progressivity of Redistribution

One argument in the article is that the conservative effects of secondary education on redistributive attitudes work partly through self-interest. That is, by increasing income and other human-capital-related variables, secondary education makes people less dependent on redistributive policies, and more likely to be taxed to support those policies.

But one may wonder whether secondary education in the United States really makes people less likely to benefit from redistributive policies. After all, tax expenditures in the United States are less progressive than in most advanced industrial democracies (Kenworthy 2014, 121–22). Much redistribution in the United States involves transfers not from the rich to the poor but from those in the highest quintiles of income to those in the middle quintiles (e.g., Kenworthy 2014, 80). By increasing people's income, then, secondary education may move them into the middle class and make them more likely to benefit from at least some redistributive policies. Moreover, acquiring the benefits of redistribution often requires some skill—for example, skill at interacting with large bureaucracies—and better-educated people may be more likely to have that skill.

In response to these concerns, two points seem important. The first is that the relevant redistributive programs are those mentioned in the six outcome questions. They do not include the programs whose benefits accrue most disproportionately to the middle and upper classes, e.g., home-mortgage subsidies. They instead include, for example, redistribution from "the rich" to "the poor" and government-guaranteed health care. Programs like these are especially likely to benefit those who have low incomes. And those adults who have not completed high school have the lowest incomes in the United States. (See page A69.)

The second point is that we may be able to learn about the assumption—that secondary education makes people less likely to benefit from the social safety nets that redistributive policies provide—by studying government data on tax incidences and effective tax rates for various groups. No government agency tabulates tax incidence or tax rates by educational groupings, but the Congressional Budget Office does tabulate tax incidence and tax rates by income quintile. It shows that households in the lowest pre-tax income quintile had, as of 2013, an average income tax rate of −7.2% (Congressional Budget Office 2016, 11, 36). (An income quintile has a negative tax rate if tax credits in that quintile, like the earned-income tax credit, exceed income tax liabilities.) Tax rates for all other quintiles are substantially higher. By contrast, government transfers as a proportion of total income are greatest for those in the lowest income quintile (Congressional Budget Office 2016, 31, Table 1).

The connection to secondary education follows directly. Secondary education has a substantial effect on income (see Figure 3 and page A69), and the CBO data suggest that even at low levels of income, increases in income lead both to higher taxes and to lower benefits from government transfers. The CBO data thus imply that, by increasing income, secondary education makes people less likely to benefit from redistributive programs.[20]

A related concern about the self-interest argument in the article stems from the model of Meltzer and Richard (1981). Under this model, uniform increases in income—that is, increases that leave everyone's position in the distribution of income unchanged—may not change anyone's demand for redistribution. Such increases do not change the extent of inequality; those who "have less" remain as poor as before, relative to those who "have more." Under this model, it is changes in some people's pretax income relative to the mean pretax income in the population, rather than absolute changes to incomes *per se,* that should be expected to affect demands for redistribution (Meltzer and Richard 1981, 920–22; Drazen 2000, 314–15).

The most critical point to make about this application of the Meltzer-Richard model is that the result described above depends on the assumption of lump-sum redistribution, such that all people, rich or poor, receive the same amount of money via redistribution. But in reality, redistribution is largely progressive and many redistributive programs target people who are below a certain income threshold. And it is clear that redistribution can raise some people above the threshold—and thus reduce their benefit from further redistribution—even if their position in the income distribution remains unchanged or nearly unchanged. The applicability of the Meltzer-Richard model is thus far from clear.

[20] I refer to a CBO report for 2013 (published in 2016) because subsequent CBO reports seem narrower in scope. In particular, while the CBO report for 2013 discusses the effects of all federal government transfers, subsequent reports in the same series are limited to a discussion of only means-tested transfers.

Liberalism of Professors and High School Teachers

The "Effects of College" section of the article takes up reasons why high school seems to have conservative effects on economic attitudes even as college seems to have liberal effects. One answer that has been offered before—without much evidence to support it—is that professors are more liberal than high school teachers (e.g., Alesina and Glaeser 2004, 205).

It is difficult to gauge the average liberalism or conservatism of people in any particular profession. The chief reason is the lack of surveys that have samples of adequate size. A further reason is the lack of adequate data on respondents' occupations: many surveys only classify respondents as belonging to one of several general occupational categories, making it impossible to identify relatively specific groups like professors or high school teachers.

The General Social Survey is better in this regard. All of its respondents' occupations are classified according to the U.S. Census Bureau's extensive occupation-classification scheme, making it possible to determine which respondents are professors and which are high school teachers. (By contrast, the American National Election Study has used an extensive occupation-classification scheme for only a small subset of its respondents.)

In Table A17, the four GSS outcomes are regressed (via OLS) on a dummy variable that equals 1 if a respondent is a high school teacher, 0 if she is a professor. Respondents who are in neither category are excluded from the analysis. The models include year-of-interview fixed effects, but they include no other control variables: the purpose of these regressions is purely descriptive. The positive estimates on each variable indicate that, as Alesina and Glaeser (2004, 205) and many others have suspected, American professors really are more liberal than American high school teachers. Specifically, high school teachers seems to be between 4% and 14% more opposed to redistribution, depending on which of the four outcomes one is examining. All of the estimates are statistically significant at $p \leq .01$ (two-tailed) save the estimate for the help-the-poor regression, in which $p = .23$.

	redistrib. to poor (1) (GSS)	redistrib. to poor (2) (GSS)	help poor (GSS)	welfare (GSS)
High school teacher	.09 .03	.14 .05	.04 .03	.14 .03
R^2	.08	.12	.07	.12
Standard error of regression	.29	.31	.26	.36
Number of observations	470	188	450	554

Table A17: Professors are more liberal than high school teachers. Each column reports OLS estimates and standard errors from a different regression. In each regression, the outcome ranges from 0 to 1. It has been regressed on year-of-interview fixed effects and a dummy variable that equals 0 if the respondent is a professor, 1 if she is a high school teacher. Respondents who are neither professors nor high school teachers are excluded from the data. Data are from the GSS; the ANES does not permit one to identify which respondents are high school teachers or professors.

One qualification is in order. Donations to candidates and political committees can be used to estimate the average ideology of donors in different occupational groups (Bonica 2014, esp. 381–83), and this method suggests that, among those who have made multiple political donations, professors are approximately as liberal as primary or secondary school teachers, rather than more liberal. But note that professors are nearly three times more likely than primary- or secondary-school teachers to have made multiple donations. The finding is thus compatible with a model in which donors in the two occupational groups are equally liberal, in which non-donors in the two occupational groups are equally moderate, and in which donors are far better represented among professors than among primary- and secondary-school teachers. This model is consistent with the finding that Democratic donors are substantially more liberal than Democratic non-donors (Hill and Huber 2017). And it is consistent with the GSS data described above, which suggest that professors are (on average) substantially more liberal than secondary-school teachers.

Estimated Effects When the Sample Is Restricted to Whites

A thread of literature, closely related to research on "linked fate" (e.g., Dawson 1994, esp. 81-82), offers reason to expect that education may cause black Americans to hold more *liberal* attitudes toward redistribution than they otherwise would. The suggestion is that education creates group consciousness among black Americans. Increased group consciousness, in turn, may make black Americans more supportive of redistribution. If the suggestion is correct, the results that are presented in Figure 2 are weaker than they would otherwise be because they average over two groups of respondents: black Americans, whom education may cause to hold more liberal attitudes, and other Americans, whom education may cause to hold more conservative attitudes because of the processes that are described on pages 3-4.

Figure A17 reproduces the analyses of Figure 2 when the samples are restricted so that they contain only white respondents. Comparison of the two figures reveals few differences. The main patterns of Figure 2 are all clearly intact in Figure A17. The estimates are not consistently larger or smaller in the whites-only analyses. (The standard errors are consistently larger, as one might expect given the smaller sample sizes in the whites-only analyses.)

The similarity of estimates in Figures 2 and A17 should not be taken as strong evidence that race does not moderate the effects of education on economic attitudes. Whites are the large majority in the samples used for all of the Figure 2 analyses; even if education affects members of other races in systematically different ways, the average results that are reported in Figure 2 will chiefly reflect the effects of education on whites. What comparison of Figure 2 to Figure A17 reveals is that inclusion of non-whites in the analyses does little to affect the results apart from shrinking the standard errors.

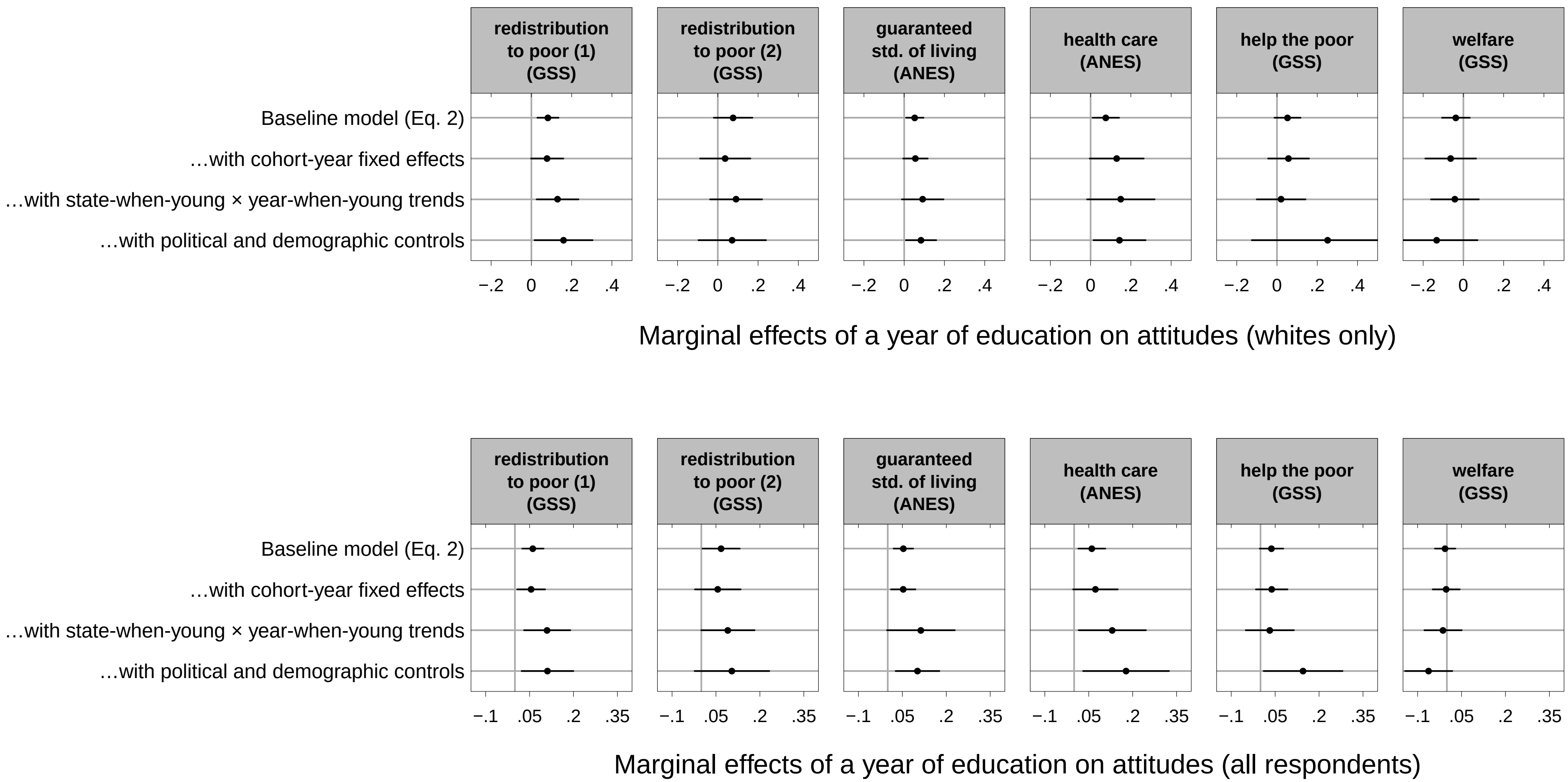

Figure A17: Effects of education on redistribution-related attitudes (sample restricted to white respondents). Each plotted point in the top panels is an estimate from a separate instrumental-variables regression. Black lines are 95% confidence intervals. Standard errors are clustered at the state-year level. The estimates are akin to the estimates in Figure 2; the difference is that these panels report results from whites alone. Sample sizes in these panels range from 21,423 for the first "redistribution to poor" item (baseline model) to 9,229 for the second "redistribution to poor" item (model with political and demographic controls). To aid comparison, the 2SLS estimates from Figure 2 are reproduced in the bottom panels.

Estimated Effects of High School Graduation

In the article, I report estimates that are are based on a measure of years of completed schooling. One might instead use a dummy variable that equals 1 for respondents who graduated from high school, 0 for others. But the interpretation of dummy-variable measures of educational attainment requires some spelling-out, and in IV applications, such measures are likely to be biased (Marshall 2016; Angrist and Imbens 1995, 436).

Begin with the interpretive issues. Dummy-variable measures divide samples into two groups: those who have a given level of education and those who don't. In both the ANES and the GSS, the median person who did not finish high school completed nine years of schooling, and the median person who did finish high school completed thirteen years of schooling. In a regression of adult attitudes on a high-school-graduation dummy variable, the coefficient on graduation thus bundles together two effects. The first is the effect of spending more time in school—which is in turn composed of both in-school effects (e.g., effects of classroom socialization) and after-graduation effects (e.g., effects of earning more money because one has more classroom training). The second is the signaling effect of holding a diploma, distinct from time spent in school.

More worrisome than the interpretive issues is the probability that IV estimates which are based on dummy-variable measures of educational attainment are biased. In the case of a high-school-graduation dummy variable, estimates are biased if instruments induce educational attainment among some people without causing them to graduate from high school, and if the extra education that these people receive in turn affects the outcome of interest. Where compulsory attendance laws are concerned, this set of conditions is likely to hold. These laws cause some people to graduate who otherwise would not, but they also cause others to conclude their education with 10 years of education instead of 9, or 11 instead of 10, and so on. The problem is an exclusion-restriction violation: when attendance laws instrument for high school graduation, the exclusion restriction stipulates that they must not affect the outcomes of interest except by causing people to graduate from high school. If attendance induces changes in educational attainment that do not involve high school graduation, and if those changes affect the outcome, the exclusion restriction is violated.

These qualifications aside, it remains true that dummy-variable measures of educational attainment are popular in studies of education. To aid comparison to results published elsewhere, I report, in Figure A18, estimates that are based on a high-school-graduation dummy variable.

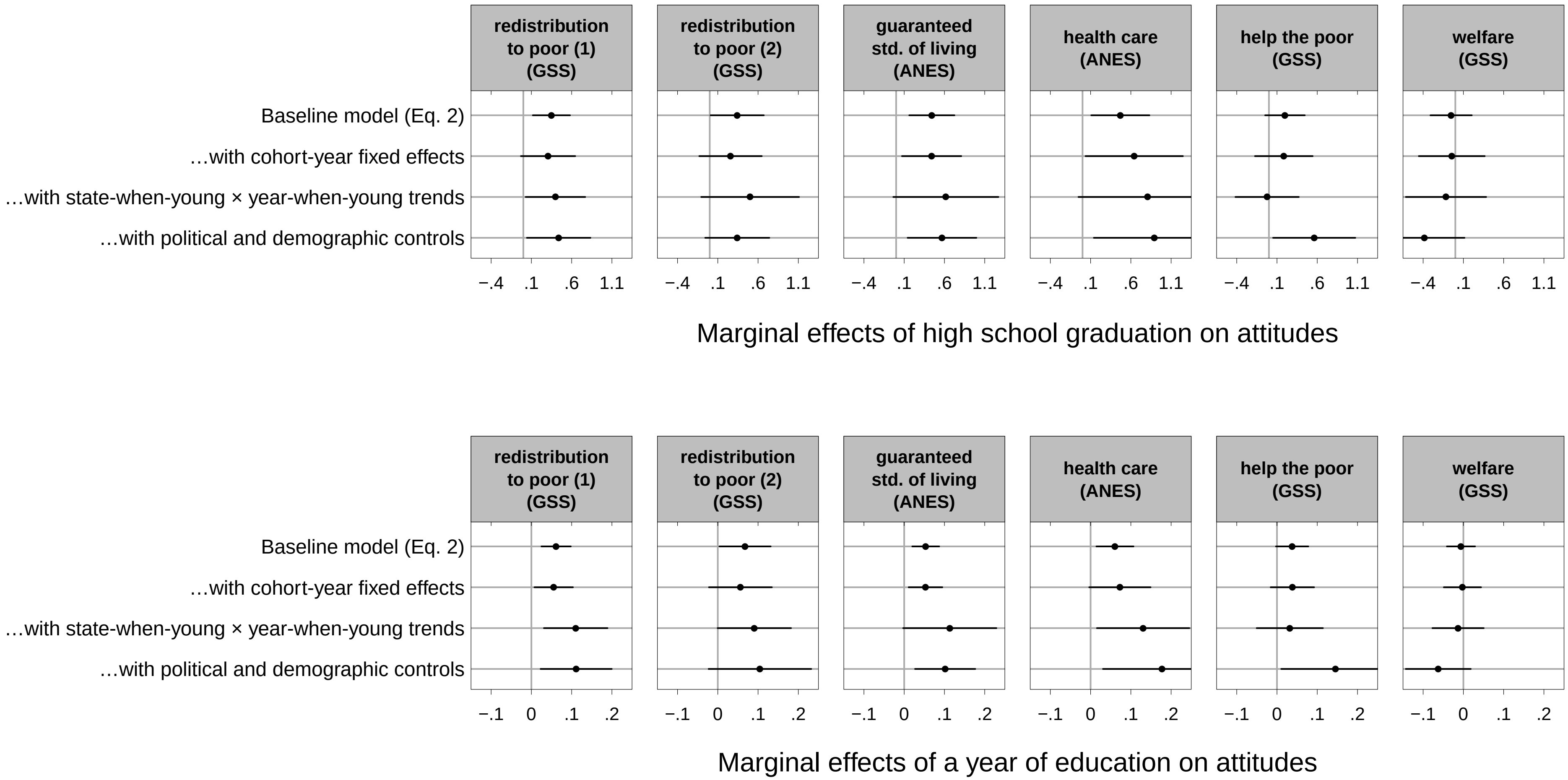

Figure A18: Effects of high school graduation on redistribution-related attitudes. Each plotted point in the top panels is an estimate from a separate instrumental-variables regression. Black lines are 95% confidence intervals. Standard errors are clustered at the state-year level. The estimates are akin to the estimates in Figure 2; the difference is that the treatment variable in these panels is high-school graduation, a dummy variable, rather than years of schooling. Sample sizes in these panels range from 25,895 for the first "redistribution to poor" item (baseline model) to 10,941 for the second "redistribution to poor" item (model with political and demographic controls). To aid comparison, the 2SLS estimates from Figure 2 are reproduced in the bottom panels.

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