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ARTICLE

# Education and Attitudes toward Redistribution in the United States

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**Abstract**
Although scholars have studied education's effects on many different outcomes, little attention has been paid to its effects on adults' economic views. This article examines those effects. It presents results based on longitudinal data which suggest that secondary education has a little-appreciated consequence: it makes Americans more opposed to redistribution. Placebo tests and other analyses confirm this finding. Further investigation suggests that these conservative effects of education operate partly by changing the way that self-interest shapes people's ideas about redistribution.

**Keywords:** education; redistribution; self-interest; inequality; welfare

Primary and secondary schooling is the greatest expense of America's state and municipal governments (Barnett et al. 2014). It is also one of the most widely shared long-term experiences in modern society. In the United States, for example, well-enforced attendance laws ensure that people are far more likely to spend years in school than to ever become married, have children, or hold a job. The links between schooling and other aspects of society have thus received much attention from scholars. Converse (1972, 324) goes so far as to call education the 'universal solvent', promoting good outcomes throughout the spectrum of politically relevant variables.

Still, little attention has been paid to the link between schooling and a topic of burgeoning interest: attitudes toward inequality and redistribution. The inattention is striking in light of the effects on public opinion that many scholars attribute to education, and in light of the effects on redistributive policy that many scholars attribute to public opinion (eg, Alesina and Glaeser 2004, chs. 6–7; Brooks and Manza 2007; Page and Shapiro 1982, esp. 182).

The dearth of evidence about education's effects on economic attitudes may be due partly to growing recognition of empirical difficulties. Receipt of education is nothing like a randomly assigned treatment; instead, those who receive a lot of it are profoundly different from those who receive a little. And conditioning on the variables that are available in widely used datasets may not suffice to make the well educated comparable to the less educated. As a result, efforts to study education's effects via regression or matching now confront mounting skepticism (Henderson and Chatfield 2011; Sondheimer and Green 2010).

The point of this article is to provide evidence about the effects of secondary education on redistribution-related attitudes, and to do so in a way that speaks to concerns about prior research. The argument that follows is based partly on a new dataset, created for this study, about the characteristics of US states and schools throughout the twentieth century. Within-state, over-time variation in the strictness of schooling laws serves as an instrument for educational attainment. No work along these lines can be dispositive, but the results of the analyses seem to be robust to alternative explanations. And these results suggest that secondary education in America has a little-appreciated consequence: it makes Americans more opposed to redistribution.

I begin by examining theories which imply that education may affect support for redistribution, focusing on those that involve considerations of self-interest or fairness. The next section shows that secondary education limits support for redistribution. I then revisit the theoretical explanations, furnishing evidence that strengthens the case for self-interest explanations but offers little support for fairness-based explanations.

## How Education May Affect Economic Attitudes

*Education* refers to time spent in classrooms or on related academic activities – for example, homework. Defined so broadly, its effects may arise through many different mechanisms: exposure to different social milieux, inculcation of beliefs by teachers and textbooks, change in the material conditions of one's life, and much more (eg, Kam and Palmer 2008). Most research does not distinguish between these potential mechanisms. In this article, I examine the mechanisms to which the data speak relatively well: those rooted in self-interest and in perceptions of fairness.

*Self-interest* refers to the material conditions of one's life and to circumstances that directly affect those conditions. This definition follows most political research on the topic (eg, Chong, Citrin and Conley 2001, 542; Feldman 1982, 446). Like that research, it excludes conceptions of 'enlightened self-interest,' which are rooted in costs and benefits that accrue to one only indirectly – for example, through strangers who live in one's community.

The standard rationale for self-interest as a mechanism through which education affects economic attitudes can seem simple, even axiomatic. People are hedonists, motivated by pleasures and pains. And they are egotists, caring more about their own pleasures and pains than about the pleasures and pains of others (Sears and Funk 1990, 147). They prefer to have more, and having more therefore causes them to oppose redistribution to those who have less. Consistent with this explanation of education's effects on redistributive attitudes, there is now much support for the claims that education boosts income (eg, Goldin and Katz 2008, esp. 71–84; Heckman, Humphries and Veramendi 2018) and that people with higher incomes hold more conservative economic views (Bartels and Cramer 2018; Erikson and Tedin 2007, 191–6; Peterson 2016). This explanation is also consistent with the argument that attending high school causes people to vote more conservatively because it increases their income (Marshall 2016b; Marshall 2019).

Of course, self-interest is often held to be a weak influence on political attitudes (Sears and Funk 1990). But the evidence for its effects on views of redistributive policies is strong (eg, Alesina and La Ferrara 2005; Brunner, Ross and Washington 2011; Doherty, Gerber and Green 2006), perhaps because these policies' effects are often highly salient (Chong, Citrin and Conley 2001; Green and Gerken 1989, 8–11). Recent research also supports the idea that personal economic experiences have large effects on attitudes toward redistribution (Lerman and McCabe 2017; Margalit 2013; Neundorf and Soroka 2018). One may therefore expect that education increases opposition to redistributive policies because – by increasing income, employment, and related factors – it reduces the chance that one will benefit personally from those policies.[1]

Education may also affect attitudes toward redistribution through channels other than self-interest. And one potential channel of growing interest is perceptions of fairness (eg, Bénabou and Tirole 2006; Lipset 1977; Osberg and Smeeding 2006; Trump 2018). At its most basic, the intuition is that if people deem economic inequality unfair, they will be more likely to support

[1]See Weeden and Kurzban (2017) for a comprehensive argument that the effects of self-interest on political attitudes are substantial and have been underestimated. And see Appendix p. A84 for a discussion of the links between the self-interest argument presented here, progressivity of redistribution, and the Meltzer-Richard model.

redistribution.[2] By contrast, if people take economic inequality to reflect unequal effort, the intuition is that they will be less likely to support redistribution. Empirically, just this correspondence between perceptions of fairness and support for redistribution has been observed (Alesina and Glaeser 2004, esp. ch. 7), and the result holds even after controlling for many individual characteristics that may seem relevant (eg, Alesina and La Ferrara 2005).

But even if we grant that perceptions of fairness shape redistributive attitudes, we cannot immediately conclude that education's effects work through those perceptions. Some research does suggest that education affects these perceptions, but the suggestion must be regarded as tentative, because most of this research does little to distinguish education's effects from those of its correlates (eg, Alesina and Glaeser 2004; Bowles and Gintis, [1976] 2011). Determining the mechanisms through which education operates is a thorny empirical question, and one to which we shall return.

The question of mechanisms becomes thornier still when we acknowledge that there are multiple kinds of attitudes toward redistribution, and that some of these attitudes may be rooted in self-interest while others are rooted in perceptions of fairness. Cavaillé and Trump (2015, esp. 154) make just this argument when they distinguish between questions about 'redistribution from' and questions about 'redistribution to'. Questions in the 'redistribution from' category usually allude to inequality, and while they often mention the intended beneficiaries of redistribution, they tend to do so in brief and general ways. (For example, 'Government should redistribute income from the better-off to those who are least well-off.') By contrast, questions in the 'redistribution to' category are distinguished by an emphasis or exclusive focus on the intended beneficiaries. (For example, 'Many people who get welfare don't really deserve any help.') Cavaillé and Trump argue that attitudes toward 'redistribution from' are influenced more by self-interest than by perceptions of fairness, while the reverse is true of attitudes toward 'redistribution to'. The relative effects of self-interest and perceptions of fairness on redistributive attitudes may thus depend on which kind of redistributive attitude we are talking about.

Critically, the research presented below is about the effects of pre-college schooling. Those who have studied the link between education and economic attitudes do not deny that college also affects those attitudes (eg, Alesina and Glaeser 2004; Bowles and Gintis, [1976] 2011). But their focus on political dispositions and self-interest warrants attention to the level of schooling at which dispositions and human capital are first developed: pre-college schooling. Attention to high school is especially warranted because political ideas of any complexity first develop, for many, during adolescence (eg, Adelson and O'Neil 1966; Campbell 2006, esp. chs. 6–7; Jennings and Niemi 1974). Moreover, political ideas developed at this age seem likely to have a lasting effect on one's views in adulthood (Campbell 2006, 96–97; Sears 1981, 183–93). I therefore attend to the effects of high school.

Whether our theories invoke self-interest or perceptions of fairness, we have reason to expect that secondary education affects people's attitudes toward redistributive programs. Still, the lack of evidence on this point is nearly absolute. Correlations have been computed, but they speak only weakly to the question of whether education *causes* people to be more or less supportive of redistribution. Why the dearth of evidence?

### *Empirical Difficulties in the Study of Education's Effects*

Credible findings about education's effects remain scarce because of methodological problems. Two deserve mention. The first is 'post-treatment bias', which arises whenever one controls for variables that are themselves functions of the 'treatment' – in this case, education. For example, it is common to see outcomes of interest regressed on both education and adult income, or on both education and adult attitudes. It is difficult to believe the estimates of education's

[2]A large body of work now complicates this intuition by noting that inequality seems to reduce support for redistribution. See Cavaillé and Trump (2015, 146–9) for a review.

effects in these regressions, because controlling for post-treatment variables biases the estimates – often severely, and in unknown directions – even if all other aspects of the analysis are pristine. Although the problem is now well recognized (eg, Angrist and Pischke 2009, 64–68; Montgomery, Nyhan, and Torres 2018; Rosenbaum 1984), it continues to affect a great deal of research on education.

But the more fundamental problem is simply that people with higher and lower levels of education differ in many ways. Most analyses of education rest on the assumption that we can account for these differences with control variables, but a tide of recent evidence suggests that the assumption is false, partly because the breadth of relevant variables is too great for a control-variable strategy to be suitable. For example, educational attainment seems to be affected by parental education (Oreopoulos, Page and Stevens 2006), parental income (Belley and Lochner 2007), family size and structure (Astone and McLanahan 1991), personality and other 'noncognitive' dispositions (Heckman, Stixrud and Urzua 2006), and the characteristics of one's classmates (Ammermueller and Pischke 2009), neighborhood (Crane 1991), and school (Koedel 2008). And this is hardly a comprehensive list.

The control-variable strategy is suspect because no dataset includes all of these variables, let alone measures of political outcomes as well. This problem is increasingly well recognized (eg, Sondheimer and Green 2010, 174–6). Indeed, one group of authors goes so far as to argue that it may be 'practically impossible to recover unbiased causal estimates' of education's effects with methods that rest mainly on the use of control variables (Henderson and Chatfield 2011, 647). The implication of such a critique is that most studies of education's effects are biased.

The treatment of interest in this article is years spent in school (ie, 'schooling' or 'educational attainment'). To estimate treatment effects while overcoming the problem of unobserved confounders, I use compulsory attendance laws as instrumental variables for educational attainment. In principle, IV research designs permit estimation of education's effects even when important variables are unobserved (Angrist, Imbens and Rubin 1996). But the assumptions that these designs invoke are considerable, and after describing the data used in the current analysis, I will consider whether those assumptions hold here.

## Data

By 1918, every US state had enacted compulsory attendance laws (Lleras-Muney 2002, 403). These laws were enacted mainly to promote assimilation to prevailing social norms and to provide industry with skilled workers (eg, Goldin and Katz 2008, 147, 164; Kotin and Aikman 1980, 10–18, 46–47). Their strictness generally increased over the course of the twentieth century, but it also decreased at times – for example, in response to federal pressure to racially integrate schools, or to increase the labor supply (eg, Goldin and Katz 2008; Kotin and Aikman 1980, esp. 36).

Throughout their history, the main effect of attendance laws has been to increase educational attainment at the level of high school. By contrast, the laws have had approximately no effect on college attendance (eg, Acemoglu and Angrist 2001, 32–35). This result is consistent with the finding that the laws' effects are concentrated among children of low socioeconomic status, who are especially prone to dropping out of school (Oreopoulos 2007). The result also implies that IV analyses that are based on attendance laws cannot teach us about the effects of primary education or college. By the same token, though, they are well suited to isolating the effects of secondary education from the effects of education at other levels.

I use annual data on attendance laws in each of the forty-eight contiguous US states and Washington, DC. Data on laws from 1910 through 1978 come from Acemoglu and Angrist (2001) and Goldin and Katz (2011). I collected the data on laws in effect from 1979 through 2010 for this study. I use these data to create two indicators: one for people who were required to attend school for between eight and ten years, and another for people who were required to attend school for more than ten years. These dummy variables are instruments for education, and for convenience, I refer to them as 'moderate' and 'strict', respectively. In 1910, no state had strict laws by this definition. By 2010, thirty-one states did.[3]

To be of use, attendance-law data must be merged with measures of economic attitudes, educational attainment, and the states in which subjects lived when they were young – that is, when schooling laws might have affected their education. I use two datasets that contain all of these variables: the cumulative cross-sectional datasets for the American National Election Studies (ANES) and the General Social Survey (GSS).

I match attendance laws to each ANES subject based on the year in which he turned fourteen and his state of residence at this age, which is the lowest common dropout age across states (Acemoglu and Angrist 2001, 26; Lleras-Muney 2005, 196). I match laws to each GSS subject based on the year in which he turned fourteen and his state of residence at age sixteen. (GSS subjects report their state of residence at age sixteen, not age fourteen.)[4]

### *Education*

I use a years-of-completed-schooling measure of educational attainment, top-coded at 13. In IV regressions of attitudes on years of schooling, the coefficient on the latter variable is a weighted average of the effects of completing different years of schooling (Angrist and Imbens 1995, esp. 433–6). The weights are greatest for the years of schooling that the instruments affect most heavily: in this case, years nine through twelve (eg, Acemoglu and Angrist 2001, 33; see also Table 1).[5] It is because attendance laws affect educational attainment at the secondary level – but not at other levels – that this study can speak to the specific effects of secondary education.[6]

### *Outcomes*

The IV estimator is less efficient than the OLS estimator, and standard errors of estimates that are based on attendance-law instruments are typically 10–150 times larger than the corresponding OLS standard errors (eg, Acemoglu and Angrist 2001, 26, 34; Dee 2004, 1713). One implication is that IV estimation will be underpowered when done with sample sizes that suffice for OLS estimation. Finding questions that have been asked of many people is thus a prerequisite if IV estimation is not to be underpowered. I use 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 attendance-law data are also available. They are about:

1. redistribution from 'the rich' to 'the poor' (GSS),
2. redistribution from 'people with high incomes' to 'those with low incomes' (GSS),
3. whether government should ensure that 'every person has a job and a good standard of living' (ANES),
4. government-provided health insurance (ANES),
5. whether government should do 'everything possible to improve the standard of living of all poor Americans' (GSS), and
6. 'welfare' (GSS).

[3] Appendix p. A45 shows that the results are robust to many different codings of the attendance-law variables.

[4] Although the GSS state-of-residence-when-young data had been collected by interviewers for decades, they had never been coded or made available, even on a restricted basis. I thank Jibum Kim, formerly of the National Opinion Research Center, for coding the data and making them available for this study. They are now available to the public upon request.

[5] The weights are small for years of education beyond high school, because attendance laws have little power to predict education beyond high school (Table 1). For this reason, top coding makes little difference to the main results. For the same reason, it increases the first-stage $F$ statistics. That said, the $F$ statistics reported in Figure 1 would all exceed 10 – the common threshold for 'weak instrument' concerns – even without top-coding.

[6] Instead of a years-of-schooling measure, one might use a binary measure of high school graduation. The results are stronger with such a measure. But estimates that are based on such measures are likely to be inflated (Marshall 2016a). See Appendix p. A91 for a discussion of this point.

Each item has five or seven response options, save the welfare item, which has only three. Each is coded so that responses range from 0 to 1, and so that more conservative responses have higher values. And each was asked in at least nine different years between 1970 and 2012. (Appendix Figure A11 reports the exact years in which each question was asked.)

### *State-level controls*

In the analyses that follow, it will prove useful to control for factors that may influence both the adoption of attendance laws and adult attitudes toward redistribution. All of the models in this article therefore include fixed effects for each state. In addition, some models account for political and demographic characteristics of the states in which subjects lived when they were young. These variables are measured for each state in each year between 1910 and 2010.

The set of political controls includes turnout, the percentage of the two-party vote won by the top-of-the-ticket Democratic candidate, and an interaction between two-party vote and the year in which each subject turned fourteen. Turnout proxies for the extent to which one grew up in a politically participatory culture. The interaction accounts for over-time change in the meaning of the two-party vote: a 60 per cent vote for McGovern in 1972 may not imply the same degree of liberalism in one's state as a 60 per cent vote for Clinton in 1992.

The set of political controls also includes three segregation-related measures. The politics of segregation had their greatest effects on attendance laws in Mississippi and South Carolina, both of which repealed those laws in the wake of *Brown v. Board of Education*. The laws were reinstated only after more than a decade had passed. I use dummy variables to indicate whether one was white and turned fourteen years old in one of those states during the years in which attendance laws were not in effect. Following Lochner and Moretti (2004), I also add a variable to indicate whether one was black and turned fourteen in a southern state in 1956 or later.

Finally, the set of political controls includes the party of the president and the DW-NOMINATE score (McCarty, Poole and Rosenthal 1997, 45–54) of the median member of the US House when respondents turned fourteen. These variables help to account for changes in the political culture of one's youth that are not captured by the other variables.

The demographic controls are teacher-pupil ratios, teacher salaries (in real dollars), and the percentages of the population that were black, foreign-born, urban, working in manufacturing, working as doctors, or enrolled in college.[7] These variables account for sources of variation in the enactment of attendance laws. For example, the laws were adopted largely to encourage assimilation of immigrants to cultural norms, and the 'percentage foreign-born' variable proxies for this motivation. The laws were also strengthened or weakened to control the supply of labor for manufacturing jobs (Goldin and Katz 2008, 205–10), and the 'percentage working in manufacturing' variable proxies for this motivation. The extent to which states were urban also affected the enactment of these laws: compliance with the laws became more feasible as populations became denser and thus more capable of supporting neighborhood schools (Goldin and Katz 2008, 207–08).

## Compulsory Attendance Laws as Instruments for Education

Some IV analyses are hard to believe because the assumptions that underpin them are hard to justify (Sovey and Green 2011, esp. 193–5). It is therefore critical to consider whether those assumptions are satisfied in the current analysis, which uses attendance laws as instruments for educational attainment. Valid instruments must satisfy five assumptions: the first-stage, noninterference, monotonicity, exclusion-restriction, and ignorability assumptions (Angrist and Pischke 2009, 177).[8]

[7]Some of these variables were obtained from the Integrated Public Use Microdata Series (IPUMS USA, Ruggles et al. 2019). The appendix reports further information about each variable.

[8]These are the five assumptions of the heterogeneous-treatment-effects IV framework, which permits treatment effects to vary across subjects. They replace the two assumptions of the classical IV framework, which does not permit treatment effects to vary across subjects. See Angrist, Imbens and Rubin (1996, esp. 449–50) for a comparison of the frameworks.

The first three requirements are easily satisfied. Attendance laws affected educational attainment throughout the twentieth century (first stage; see Figure 1 and Lleras-Muney 2002). The sampling designs of the studies used here make it unlikely that any respondent's attitudes were affected by the education of other respondents (noninterference). And it is not plausible that raising the age at which students may leave school would have decreased the amount of time that they spent in school (monotonicity).[9]

The exclusion restriction and the ignorability requirement demand more scrutiny. The exclusion restriction stipulates that attendance laws must affect the outcomes of interest exclusively through their effects on educational attainment. If passing a strict attendance law causes children to hold different economic attitudes decades later, when they are adults – not because it leads them to spend more time in school, but through some other channel – the restriction is violated.[10]

The ignorability requirement or 'independence assumption' stipulates that, conditional on other observed variables, the instrument must be independent of the potential treatments and the potential outcomes. For example, if the culture of one's childhood state of residence affects the strictness of attendance laws in that state, and if it independently affects adult attitudes, and if its independent effect on adult attitudes is not captured by covariates, then the ignorability requirement is violated.

Previous research suggests that attendance laws satisfy these requirements. For example, Lleras-Muney (2002, esp. 424–6) presents a variety of analyses of changes in attendance laws and concludes that they were not endogenously determined. And Goldin and Katz (2011) show that these laws played only a small role in the US 'high school movement' that spanned from 1910 to 1940, which implies that changes in the laws were not a large part of social trends that might have influenced economic attitudes. (See also Dee 2004, 1715–6; Lochner and Moretti 2004, 163–8; and Milligan, Moretti, and Oreopoulos 2004, 1683–4.) As a result of findings like these, the use of attendance laws as instruments for education has become common since Acemoglu and Angrist (2001) introduced the practice. Even so, caution is in order: the exclusion restriction and the ignorability requirement may hold in some analyses but be violated in others. Below, I consider whether they hold in the present study.

## Findings

To a first approximation, IV estimates can be thought of as the ratios of the corresponding reduced-form and first-stage estimates. In this analysis, reduced-form estimates indicate the effects of schooling laws on attitudes, and first-stage estimates indicate the effects of those laws on educational attainment. All else equal, non-null IV estimates are more plausible if they are part of a seamless set of results: results which show that schooling laws change attitudes (reduced form), that schooling laws increase educational attainment (first stage), and that schooling laws and education itself push attitudes in the same direction.

In this study, the first-stage and reduced-form estimates are OLS estimates of $\beta_1$ and $\beta_2$ in models that have the following form:

$$\begin{aligned} \text{outcome}_{it} &= \beta_0 + \beta_1 M_{it} + \beta_2 S_{it} + \beta_3 (\text{year turned 14})_{it} \\ &= + \sum_{a=1}^{4} \kappa_a (\text{age}^a)_{it} + \lambda_t + \rho_{s_1} + \rho_{s_2} + \gamma \mathbf{X_{it}} + \epsilon_{it}. \end{aligned} \tag{1}$$

In this equation, *outcome* is an attitude (for the reduced form) or years of education (for the first stage). $M$ and $S$ are the instruments: they are dummy variables that indicate whether one grew up

[9]See Jin and Rubin (2009, 32–33) for a similar argument about noninterference.

[10]'Exclusion restriction' has a broader meaning in the classical IV framework. It is decomposed, in the heterogeneous-treatment-effects IV framework used here, into two parts: a narrower requirement, also called the 'exclusion restriction'; and the ignorability requirement. See Angrist, Imbens and Rubin (1996, 449–50).

under a moderate or a strict schooling law. $\sum_{a=1}^{4} \kappa_a(\text{age}^a)_{it}$ is a quartic polynomial in age, which permits effects associated with age to vary over time. And $\lambda_t$, $\rho_{s_1}$, and $\rho_{s_2}$ are fixed effects that indicate year of interview, state of residence at time of interview, and state of residence at age fourteen (ANES) or sixteen (GSS).[11] $\mathbf{X}$ is a set of control variables: race, gender, and whether one was born in the United States. In later analyses, I augment $\mathbf{X}$ so that it also includes political and demographic controls.[12]

Under this model, identification of $\beta_1$ and $\beta_2$ comes from over-time changes in the strictness of schooling laws *within each state*. The main identifying assumption is not that schooling laws are randomly assigned (or 'as if' randomly assigned) across states. The use of state fixed effects ensures that identification does not depend on such an assumption. Instead, the main identifying assumption is that, conditional on the variables in the model, schooling-law changes within each state are unrelated to unobserved variables that affect attitudes toward redistribution.

The first panel of Figure 1 reports the first-stage estimates. As one might expect, schooling laws seem to increase the amount of education that one receives. Relative to weak schooling laws, moderate laws cause people to attain (on average) between one-half and three-quarters of a year more schooling. And strict laws cause people to receive an additional one-fifth to one-third of a year of school. The second panel of the figure shows that the first-stage $F$ statistics for the exclusion of the

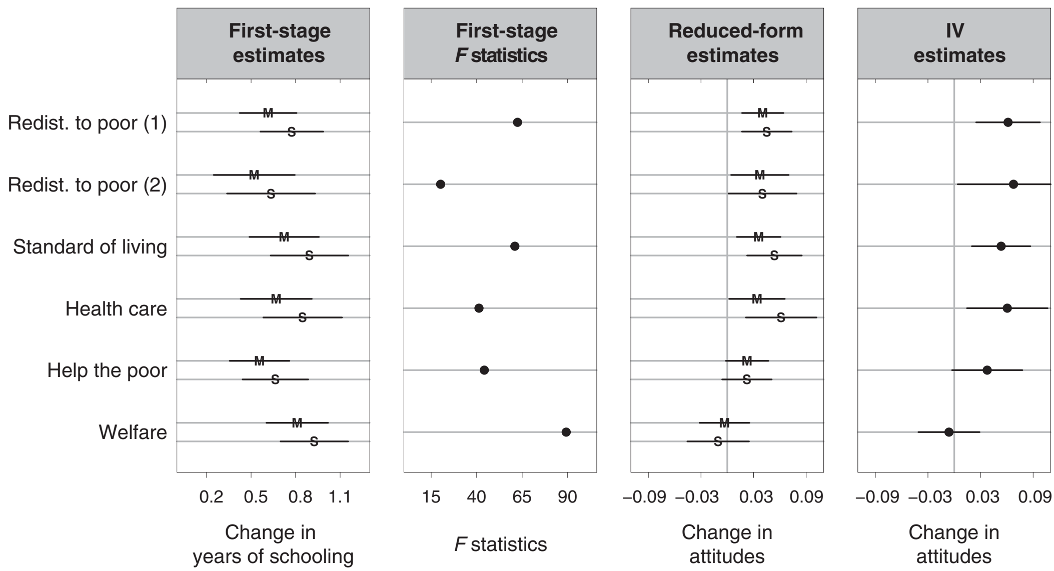


**Figure 1.** Effects of schooling laws and years of schooling

*Note*: in the first and third panels, 'M' and 'S' are OLS estimates of the effects of the moderate and strict schooling-law instruments. Each pair of 'M' and 'S' estimates is from a separate regression. In the fourth panel, each plotted point is an estimate of the effect of a marginal year of school. Black lines are 95 per cent confidence intervals. Standard errors are clustered at the state-year level. Sample sizes range from 25,895 for the first 'redistribution to the poor' question to 11,331 for the second 'redistribution to the poor' question. They vary because different questions were asked in different years. The first-stage estimates and $F$ statistics vary across outcome questions for the same reason.

[11]The triple {age, year of interview, year when subject turned fourteen} is linearly dependent, making simultaneous control of all three variables impossible. To overcome the problem, I use a single year-of-interview fixed effect to indicate whether one was interviewed in 1994 or 1996. (These are the only consecutive survey years in which all of the outcome questions were asked.) Using a single fixed effect for other pairs of interview years does not affect the results.

[12]Because identification of education's effects in this study comes from variation at the state level – that is, from attendance laws – individual-level controls make only a slight difference to the results. Adult income, adult attitudes, and other post-treatment variables are excluded from the model because their inclusion would bias the estimates. See the section entitled 'Empirical Difficulties in the Study of Education's Effects.'

instruments are all above 20, confirming that the laws account for a significant portion of the variation in educational attainment.

The third panel of Figure 1 reports the reduced-form results. These estimates suggest the long-term influence of policy on public opinion. More precisely, they show that increasing the strictness of schooling laws when one is a child seems to have a conservative effect on the economic attitudes that one holds as an adult. For the questions about redistribution to the poor, a government-guaranteed standard of living, and health care, adopting a moderate schooling law moves responses in a conservative direction by an average of 3–4 per cent of the range of the outcome. The effects are slightly stronger for strict schooling laws. They are slightly weaker when the item is 'help the poor,' and they vanish when subjects are asked about welfare.

Schooling laws thus seem to affect both the amount of education that people receive and their adult attitudes toward redistribution (save their attitudes toward welfare). These are promising conditions for IV analysis of the effects of education. But the first three panels of Figure 1 do not report the effects of education itself, which are the main effects of interest in this article. To learn about those effects, we must estimate a different model:

$$\begin{aligned}\text{attitude}_{it} &= \beta_0 + \beta_1(\text{education})_{it} + \beta_2(\text{year turned 14})_{it} \\ &= + \sum_{a=1}^{4} \kappa_a(\text{age}^a)_{it} + \lambda_t + \rho_{s_1} + \rho_{s_2} + \gamma\mathbf{X_{it}} + \epsilon_{it},\end{aligned} \tag{2}$$

where education, for which I instrument with the attendance-law variables described above, is a years-of-schooling variable. The main results of this article are the estimates of $\beta_1$, the coefficient on education in this equation. And as with Equation 1, the main identifying assumption is that, conditional on the variables in the model, schooling-law changes within each state are unrelated to unobserved characteristics that affect attitudes toward redistribution.

The final panel of Figure 1 reports the estimated effects of a marginal year of educational attainment ($\beta_1$), and it indicates that those effects are conservative. To see this pattern, begin by examining the top row of the panel. It shows that an extra year of schooling makes people less supportive of redistribution from 'the rich' to 'the poor' by 6 per cent of the range of the scale (on average), or 19 per cent of a standard deviation. Similar effects are found on responses to the second redistribution question (7 per cent and 23 per cent), the guaranteed-standard-of-living question (5 per cent, 18 per cent), and the health care question (6 per cent, 17 per cent). Critically, all of these estimates are positive, indicating that education's effects are conservative. And none of the effects are likely to seem conservative by chance alone: $p < 0.05$ for each item. The estimate for the fifth item, about helping the poor, is slightly smaller (4 per cent, 13 per cent, $p = 0.07$), but it too is conservative.

On the final item – a question about welfare – the estimate is almost exactly zero. This result is not surprising. Much research shows that welfare attitudes are unlike attitudes toward redistribution in general, largely because 'welfare' stirs competing considerations, ranging from altruism to racial stereotypes (Gilens 1999; Peffley, Hurwitz and Sniderman 1997). A related argument is that media coverage of welfare is often negative but that the educated are less affected by this coverage – hence the absence of a conservative relationship between education and welfare attitudes (Alesina and La Ferrara 2005).

The estimates for the first five items suggest that, on average, an extra year of high school makes people's redistributive views 3–6 per cent more conservative. How can we gain an intuitive sense of the size of these effects? One way is to think about the cumulative effects of high school. If education's effects are constant, we can multiply by four to estimate these cumulative effects. If they instead increase throughout high school, the effect of an extra year of high school is greater than the effect of (say) one's first year of high school. In this case, four is too large a multiplier, because the cumulative effect is less than four times the marginal effect. On the other hand, some research suggests that the political effects of schooling are largest during early adolescence (eg, Hess and Torney [1967] 2005), in which case four is too small a multiplier. But even if four is too large a multiplier, the cumulative effects of secondary school on economic attitudes are likely to be larger than those that are often attributed to age, gender, or religiosity, albeit smaller than those attributed to race or income (Fisher 2014).

Like all IV estimates, the estimates presented here are estimates of local average treatment effects. They represent education's effects not among all respondents, but only among 'compliers': students whose state's attendance laws caused them to stay in school, and dropouts who would have been induced to stay in school by stricter laws (Angrist, Imbens and Rubin 1996, 448–9). Recent research suggests that education has similar effects on others (Amin et al. 2016; Lochner and Moretti 2004, 169–70; Oreopoulos 2006), but this research is still in its infancy. What is clear is that US dropout rates have been a major policy concern for more than a century, and they have made this group of compliers prominent in policy-oriented discussions of education (Rumberger 2011, esp. 78–81).[13] The analyses presented here have a direct link to those discussions: the instruments are the very policies that governments use to promote educational attainment, and they identify education's effects on the very group that governments have long targeted. Education increased this group's opposition to redistribution – even though many redistributive policies were designed to help this group, whose members have incomes that are often well below the US median.[14]

The estimates in the final panel of Figure 1 imply that education promotes opposition to redistribution. Just as importantly, they offer no support for the claim that education makes people more favorable to redistribution. Still, one might worry that the apparent effects of education are really due to other factors. So far as attendance-law-based estimates are concerned, prominent alternative explanations are rooted in ideas about political culture, demographics, and parental characteristics.

### *Alternative Explanations and Robustness Checks*

If attendance laws are related to adult attitudes through factors other than education, and if these other factors are not represented in Equation 2, the estimates discussed above are not accurate. 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 are not necessarily 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 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.

In this section, evidence for the ignorability requirement and the exclusion restriction takes four forms. First, the results are robust to the addition of cohort fixed effects and state-year trend variables. These additions reduce the possibility of bias due to differences between cohorts or states. Second, the results are robust to controls for political and demographic factors that may have affected attendance laws and economic attitudes. Third, attendance laws are shown to affect

[13] Dropout rates remain near 20 per cent nationally and above 30 per cent in many major American cities. See Rumberger (2011) and Appendix p. A76.

[14] Within-state variation in enforcement of the laws does not seem to have been great, especially in recent decades. To the extent that it exists, it bears on the local-average-treatment-effect interpretation of the estimates. See the appendix section on enforcement of the laws.

educational attainment in secondary school but not in college – a pattern that is hard to reconcile with alternative explanations of the results. And fourth, placebo tests bolster the case for a causal interpretation by showing that the attendance laws of one's youth do not predict the education levels of one's parents.[15]

### *Cohort fixed effects and state-year trends*

To begin assessing the validity of the estimates, note that they are based on models that control for the states in which subjects grew up and the years in which they turned fourteen. These control variables capture enduring cultural differences between states.[16] They also capture national differences between different eras – for example, a national trend toward racial tolerance and integration. Such differences are thus unlikely to bias the estimates. That said, these control variables cannot speak to the possibility that the apparent effects of education are simply due to temporal clustering of attendance-law changes. For example, if many changes to attendance laws occurred in 1968, and if coming of age in the distinct culture of 1968 caused you to hold views that you wouldn't have held 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 for which the baseline model does not account. A glance at Figure A1 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.

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 estimates. Averaging across the first five outcomes, the estimates that include these controls are 1 per cent 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 per cent, 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 0.056, SE 0.040; for the health care item, it is 0.073, SE 0.039.) Even so, the stability of the estimates under this specification suggests that the baseline results reflect a real effect of education rather than a statistical artifact.

A concern distinct from temporal clustering is that the baseline model cannot account for variation across states that itself varies across years. For example, a state may become more conservative over time, even as most of the nation becomes more liberal. And if the state's growing, out-of-step conservatism is correlated with variation in its schooling laws, the ignorability requirement may be violated. Failure to account for state-specific trends of this sort may have led to inflated estimates in previous research (Stephens and Yang 2014).

We can assess this possibility by considering how the estimates change when state-specific trend variables are added to the model. There are forty-nine such 'state × year' variables: forty-nine interactions of state (and DC) dummies with the year-at-age-fourteen variable. While the state fixed effects control for the distinct cultures of particular states, these trend variables

[15]The case for the exclusion restriction and the ignorability requirement is also bolstered by analyses of older respondents and of the moderating role of parental education. See Appendix p. A39.

[16]For example, to the extent that 'the essential patterns of the three political cultures [of the American states] were set when the continent was first populated' (Elazar 1984, 131), their influence is accounted for by the state fixed effects in the baseline model.

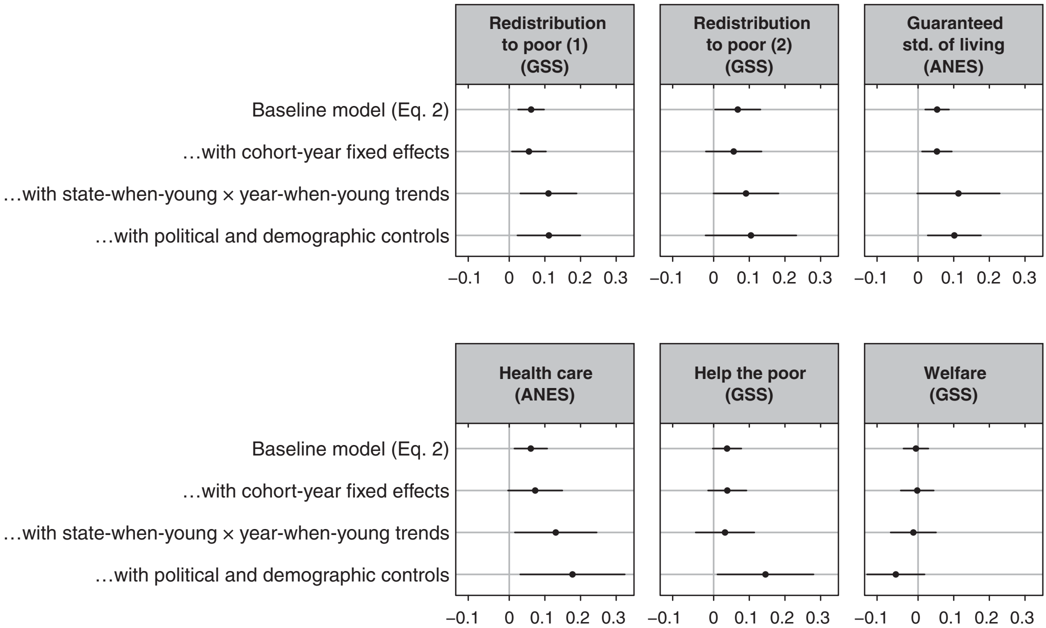


**Figure 2.** Marginal effects of a year of education on redistribution-related attitudes
*Note*: each plotted point is a two-stage least-squares estimate from a separate regression. Black lines are 95 per cent confidence intervals. The baseline estimates in the top row are the same as those that appear in the last panel of Figure 1. Standard errors are clustered at the state-year level. 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). The sample sizes vary mainly because different outcome questions were asked in different years.

allow the state-specific effects to vary from year to year. The disadvantage of incorporating these trend variables is that they discard potentially relevant information and reduce the power of the instruments, because most states experienced upward trends in both attendance-law strictness and educational attainment. The loss of power is reflected in larger standard errors.

Notably, most of the estimates from models that include these trend variables – reported in the third row of each Figure-2 panel – are slightly larger than the baseline estimates. For the first four outcomes, the estimates are 0.110 (SE 0.040), 0.091 (0.047), 0.113 (0.059), and 0.124 (0.057). All of these estimates are larger than the corresponding baseline estimates, and they bolster the claim that education moves redistributive attitudes in a conservative direction. The exception lies with the helping-the-poor question: after adding state-year trends to the model, the estimated effect of education on answers to this question declines from 0.038 to 0.032. At the same time, the standard error more than doubles, and as a result, the new estimate is indistinguishable from zero ($p = 0.045$).

### *Political and demographic controls*

The limitation of the state × year trend variables is that they are linear. They therefore do not fully account for changes in state culture that vary over time in a nonlinear way. For example, the trend variables will not fully account for a state whose political culture is conservative in the beginning and end of the twentieth century, but moderate in the middle of the century. Nor will they fully account for the waxing and waning of the supply of manufacturing jobs in some states over the decades of the twentieth century. This nonlinear change in the supply of manufacturing jobs is relevant, as attendance laws were sometimes modified in response to the supply of those jobs

(Goldin and Katz 2008, 205–10). Unobserved variation of this sort may lead to violations of the ignorability requirement or the exclusion restriction.

To explore this concern, we can control for the sixteen political and demographic variables described above. These variables were measured separately for each state and for each year during which respondents turned fourteen years old. They further account for changes in state political culture and for other factors that have been associated with the passage of strict attendance laws. Estimates that incorporate these variables are reported in the bottom rows of the Figure-2 panels – and in every case, they are larger than the baseline estimates. Among the first five items, they range from 0.102 (SE 0.038) for the guaranteed-standard-of-living item to 0.177 (SE 0.075) for the help-the-poor item. (The welfare estimate remains negative: –0.062, SE 0.041.)

### *No effects on enrollment in college*

A further analysis also speaks to the charge that attendance laws merely proxy for parental or cultural factors, which are the true influence on educational attainment. If this charge is correct, attendance laws should be correlated with enrollment in college. After all, it is hard to imagine that parental or cultural influences would lead students to stay in high school without prompting at least some of them to also enroll in college. But 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. It is difficult to imagine an unobserved confound that could create this particular pattern in the data.

**Table 1.** Effects of attendance laws on college enrollment and parental education

| | Enrolled in college (pooled) | Enrolled in college (ANES) | Enrolled in college (GSS) | Parent graduated from HS (GSS) | Parent graduated from college (GSS) |
|---|---|---|---|---|---|
| Moderate law | −0.017 0.008 | −0.022 0.012 | −0.013 0.012 | 0.004 0.014 | −0.031 0.012 |
| Strict law | −0.010 0.012 | −0.016 0.017 | −0.007 0.016 | 0.018 0.017 | −0.041 0.015 |
| $R^2$ | 0.12 | 0.13 | 0.11 | 0.25 | 0.09 |
| Std. error of regression | 0.46 | 0.45 | 0.47 | 0.38 | 0.39 |
| *F* for instrument exclusion | 2.51 | 2.10 | 0.69 | 1.36 | 3.39 |
| Number of observations | 73,707 | 31,854 | 41,853 | 35,443 | 31,314 |

*Note*: cell entries are OLS estimates and standard errors. ‘Moderate’ and ‘strict’ laws are as defined in the ‘Data’ section. The outcomes are binary variables indicating whether one ever enrolled in college, or whether either of one’s parents graduated from high school or college. All variables whose estimates are reported are coded 0 or 1. All regressions include the control variables and fixed effects that are in the baseline model (Equation 2). Standard errors are clustered at the state-year level. Parental education data are from the GSS alone; the ANES does not collect information on parental education.

### *Parental-education placebo tests*

The first-stage results in Figure 1 show that attendance-law strictness is correlated with children’s education. But if the analyses presented here truly capture the effects of children’s education, then attendance-law strictness should be a poor predictor of *parents*’ education. By contrast, if attendance laws merely proxy for parents’ educational levels or for related variables, the laws in effect when one is fourteen should ‘predict’ the education levels of one’s parents.

The final columns of Table 1 display the relevant placebo estimates, in which parental education is regressed on the strictness of the attendance laws that were in effect when one was fourteen. All of these estimates are minuscule. (Recall that the estimated effects of attendance laws on respondents’ own educations, shown in the left-hand column of Figure 1, are massive by comparison.) Like the other results in this section, these placebo results bolster the assumption that changes made to attendance laws when one is a child are unassociated with parental characteristics or other unobserved variables that affect one's adult economic attitudes.

The four analyses in this section suggest that attendance laws, as used in the models presented here, satisfy the ignorability requirement and the exclusion restriction. In turn, they suggest that the estimates in Figure 2 merit a causal interpretation: secondary education causes people to hold more conservative economic attitudes. Still, none of the preceding analyses indicate *how* education exerts this effect.

## Mechanisms

Secondary education may promote conservative economic attitudes in many ways. For example, it may expose children to different social groups and the norms that prevail in those groups, which may in turn affect the attitudes that they hold as adults. Teachers and curricula may also lead children to internalize certain norms that in turn affect their economic attitudes. But the ANES and GSS data used here are especially suited to shedding light on the roles played by a third class of potential mechanisms: self-interest mechanisms. They also speak, albeit somewhat less well, to the role that perceptions of fairness may play as mechanisms.[17]

A role for self-interest may seem especially likely given the outcomes studied here: of the six outcome questions about redistribution, the first four are 'redistribution from' items in the sense of Cavaillé and Trump (2015). That is, while these four outcomes mention the intended recipients of redistribution, they do so only briefly, and in the context of a larger question about redistribution. It is just such questions that Cavaillé and Trump (2015) argue are likely to be affected by perceptions of self-interest.[18]

To evaluate the explanatory power of self-interest mechanisms, I turn to the conservative approach advocated by Bullock, Green, and Ha (2010). The approach entails estimating education's effects on a variety of variables that may be mediators.[19] While this method does not furnish estimates of the extent to which an effect is transmitted through a mediator, it can make some variables more plausible as mediators while making others less plausible. In particular, when we learn that education affects a variable, it becomes more plausible that the variable mediates education's effects on attitudes. But when we learn that education does not influence a variable, it becomes less plausible that the variable is a mediator.

The study of mechanisms is always speculative (Bullock and Ha 2011; Bullock, Green and Ha 2010), but the speculative nature of the enterprise is acute when samples are small, as they often are here. While questions that speak to some of the mechanisms studied here have been asked in many years, most have been asked in only a few years – in some cases, only a single year. The IV strategy used in this article cannot be applied in such cases.[20] I use OLS to estimate education's effects on these mechanism variables, and I also describe the corresponding IV estimates (not shown) when they can be computed. The proviso given above remains in place: as the OLS estimates rest exclusively on the use of control variables to account for potential confounds, those

[17]Another potential mechanism is rooted in interpretation of the outcome questions that were examined in the previous section. Perhaps high- and low-education respondents give different answers to questions about redistribution because they make different inferences about the tax implications of redistributive policies. But a new survey conducted for this project indicates that this mechanism is unlikely to be at work. See Appendix p. A81.

[18]The fifth outcome, 'help the poor', may also be a 'redistribution from' item. See Table 2 of Cavaillé and Trump (2015, 154) for a categorization of items into the 'redistribution from' and 'redistribution to' categories.

[19]I use 'mediator' and 'mechanism' interchangeably.

[20]Most critically, within-state variation in attendance laws is a prerequisite for the IV strategy used here, but attendance laws do not vary within a state over the course of a single year. The IV strategy therefore cannot be used with questions that were asked in only one year. Some other questions examined below were asked in several years but of so few respondents that IV estimates are very imprecise.

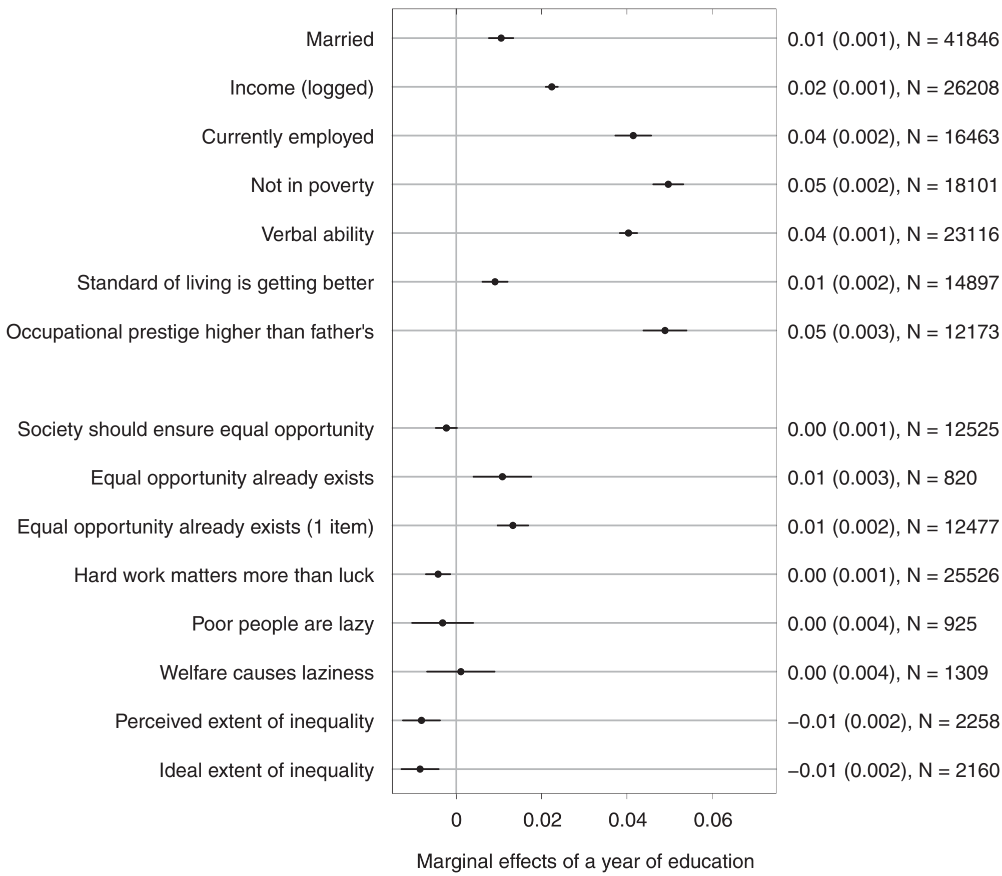


**Figure 3.** Effects of education on potential mechanisms

*Note*: each plotted point is an OLS estimate from a separate regression. The regressions are based on Equation 2; the main difference is that attendance laws have not been used to instrument for education's effects in the analyses reported here. Three outcomes (those with $N \leq 1{,}309$) were measured in only one year, and for these outcomes, the model does not control for year of interview. Each outcome has been scaled to range from 0 to 1. Black lines are 95 per cent confidence intervals. Standard errors are clustered at the state-year level.

estimates must be treated as speculative. That said, we can repose some confidence in them by noting that they have the same signs as the corresponding IV estimates in all but two cases. And of these two cases, even the larger difference between the estimates is only 0.021.

The first five rows of Figure 3 report OLS estimates of education's effects on potential self-interest mediators. And they are consistent with a role for self-interest. They indicate that education increases employment and income. It reduces the chance that one lives in poverty. It increases verbal ability, a prized attribute in many job settings.[21] And it increases the chance that one is married, which tends to reduce dependence on welfare and related policies (eg, Smock, Manning and Gupta 1999). All of these results also hold when the effects of education are estimated via IV. Indeed, the IV estimates are larger in every case, ranging from 0.03 to 0.08 ($p < 0.02$ in every case). And in expectation, all of these results suggest that education reduces the chance that one will benefit from the safety net that some redistributive policies provide.

[21]The measure of verbal ability is a vocabulary test based on the measures developed by Thorndike and Gallup (1944). The first-stage $F$ statistics exceed 30 for every IV estimate mentioned in this section.

These first five results speak to straightforward measures of material well-being. But the GSS also includes two measures that speak to prospects for social and economic mobility, and these measures, too, may be part of a self-interest explanation. The first of these measures is straightforward: subjects are asked how much they agree that 'people like me and my family have a good chance of improving our standard of living,' and their answers are recorded on a five-category scale.[22] The second of these measures is a dummy variable that indicates whether one has a higher occupational prestige score than one's father. Of course, this latter variable speaks most directly to past experience rather than to future expectations. But as Alesina and La Ferrara (2005, esp. 902) argue when they introduce the variable, people draw heavily on their past experience when making guesses about their future socioeconomic mobility. Both measures may thus speak to self-interest: if people feel as though their own standards of living are on an upward trajectory, they may be wary of redistributive programs because they expect to benefit less from those programs in the future.

And the data are consistent with this expectation. At the margin, a single year of secondary education seems to move answers to the 'improving our standard of living' question in a positive direction by about one percentage point. It also seems to increase, by five percentage points, the probability that one's occupational prestige score is higher than the score of one's father. In both cases, $p < 0.001$.

Self-interest is surely not the only channel through which education changes our views of redistribution. In particular, perceptions of fairness also seem to affect those attitudes. Perhaps education itself affects those attitudes by first changing perceptions of fairness. We can begin to probe this possibility with several ANES and GSS items.

Consider the ideal of economic individualism that lies at the heart of lay American economic thought. Feldman (1982) argues that beliefs about equality of economic opportunity are the core of this ideal. And these beliefs are of two types: one may believe that equality of economic opportunity is desirable, and one may believe that it already exists. The ANES furnishes measures of both beliefs, and in both cases, there is evidence for no more than a weak effect of education. The desirability of equal opportunity is captured by a question about whether 'society should do whatever is necessary to make sure that everyone has an equal opportunity to succeed', and the estimated effect of a marginal year of education on answers to this question is 0.00 (SE 0.001). In 1972, the ANES fielded six questions about whether equal opportunity already exists ($\alpha = 0.074$), and the effects here are slightly larger: an extra year of schooling seems to increase belief in the existence of equal opportunity by 1 per cent of the range of the scale (SE 0.003). The finding for a single item that has been asked in many years – 'one of the big problems in this country is that we don't give everyone an equal chance' – is again 1 per cent of the range of the scale (SE 0.002).

Of course, perceptions of fairness are not just about equality of opportunity. They also include beliefs about the relationship between effort and success. Several GSS and ANES items speak to these beliefs – and in this area, the evidence for an effect of education is weaker still. For example, GSS respondents are often asked whether 'hard work' or 'lucky breaks' matter more to whether people 'get ahead', and education seems to make no difference to responses to this variable. (The estimate is 0.00, with a standard error of 0.001.) Similarly, the 1972 ANES included three items which speak to the belief that poor people are poor because they don't work hard. Here, too, education seems to have no effect (0.00, SE 0.004). Finally, the GSS asks whether 'welfare makes people work less', and the estimated effect of education on answers to this question is the same (0.00, SE 0.004).

We can probe perceptions of economic fairness in one further way. Much attention has been devoted, in recent years, to the discrepancy between actual and perceived measures of inequality.

[22] A similar GSS question about respondents' standard of living relative to their parents' standard of living produces nearly identical results.

Building on this work, Trump (2018) uses GSS data to measure both perceived and ideal levels of income inequality. The GSS has asked respondents to guess the average incomes of people in fifteen different occupations (for example, bus driver, doctor in general practice) and also to state the incomes that they should receive. Trump takes the ratio of the highest and lowest perceived incomes as a measure of perceived inequality. Similarly, she takes the ratio of the highest and lowest 'how much do you think they *should* earn' answers as a measure of 'ideal inequality'. I use the same ratio variables here – after rescaling them to range from 0 to 1, as with all of the other potential mechanisms in this section.

One might imagine that education promotes conservative attitudes toward redistribution because it reduces the extent of perceived inequality, or because it increases acceptance of inequality (that is, increases one's 'ideal extent of inequality'). The findings on these points are mixed. As the bottom rows of Figure 3 show, an extra year of education seems to reduce the 'perceived extent of inequality' measure by 1 per cent of the range of the scale, but it also reduces the 'ideal extent of inequality' by the same amount.

To summarize, an extra year of secondary schooling increases income and otherwise reduces the chance that one will rely on social assistance programs. But it has only meager effects on perceptions of fairness and inequality. Taken together, these findings are consistent with a self-interest explanation of education's effects on redistributive attitudes. But they offer little support for explanations whereby education's effects work through perceptions of fairness. Those perceptions may be important (eg, Alesina and La Ferrara 2005; Trump 2018), but they do not seem to account for education's effects on people's views of redistribution.

### *Effects of college*

There is an obvious tension between the findings reported here and a common belief about the effects of college. Specifically, the suggestion that self-interest is a mechanism through which education makes people economically conservative may seem to be at odds with the belief that college makes people more liberal. After all, both secondary schooling and college increase income and other self-interest-related variables. How can it be, then, that secondary school makes people more conservative (economically speaking) while college makes them more liberal?

There are several answers. One is that students encounter liberalizing forces in college that they do not encounter in lower levels of schooling (Hastie 2007; Pascarella and Terenzini 1991). For example, Alesina and Glaeser (2004, 205) speculate that college faculty are more liberal than secondary-school faculty, and in ways that affect people's views of redistribution.[23] But a more powerful answer is simply that, like high school, college may not have liberalizing effects on economic attitudes. Its effects on other variables have been studied often, but studies of its effects on economic attitudes are rare. And the best studies in this area indicate that college in the United States reduces both support for redistribution across class lines and concerns about fairness in the distribution of income (eg, Mendelberg, McCabe and Thal 2017; Fisman, Jakiela and Kariv 2014, 10, 16–17; see also Fisman et al. 2015, esp. pg. aab0096-6). That said, good studies of college's effects on these outcomes are rare, and much remains to be learned on this point.

## Conclusion

Education is the chief expense of America's state and municipal governments, and Americans' support for spending on education is both broad and deep (Barnett et al. 2014, 4; Stimson 2015, 33–34). Even so, little research speaks to education's effects on people's views of programs or policies. This article does speak to those effects. And on average, the data suggest that secondary education in the United States has a little-appreciated consequence: it reduces support for redistribution.

[23]The GSS data indicate that Alesina and Glaeser are correct. See Appendix p. A87.

Studies of education increasingly fall afoul of the charge that they are biased because they fail to account for the huge variety of influences on educational attainment (eg, Henderson and Chatfield 2011; Kam and Palmer 2008; Sondheimer and Green 2010). The instrumental-variables strategy used here is designed to surmount this problem. When the strategy works, it renders accurate estimates of education's effects even if important variables are unobserved. Of course, one might object that the strategy does not work in this case. For example, the use of schooling laws as instruments for education might violate the exclusion restriction or the ignorability assumption. But the accumulated evidence on schooling laws suggests that they are robust to challenges along these lines, and the analyses presented in this article also suggest the robustness of this approach.

Why does secondary education in America have a conservative effect on economic attitudes? There are doubtless many answers. The data used here speak well to one class of potential mechanisms: self-interest mechanisms, which suggest that education changes attitudes toward redistribution because it improves the material conditions of people's lives. And the results are highly consistent with a role for self-interest. Specifically, they suggest that high school reduces poverty, increases income, and increases employment. It increases verbal ability, which is prized by many employers. And it increases marriage rates. All of these findings are consistent with the claim that education causes people to hold more conservative attitudes by making them less likely to benefit from the safety net that redistributive programs often provide. Many have argued that self-interest does little to shape policy views, but scholars have long carved out an important exception, finding that self-interest plays a larger role where redistributive matters are concerned (Brunner, Ross and Washington 2011; Doherty, Gerber and Green 2006; Peterson 2016; see Weeden and Kurzban 2017) for a review). The findings in this article bolster the latter body of research.

The data also speak to the possibility that education affects people's attitudes by changing their perceptions of fairness. But on this point, the evidence is weaker. Previous work suggests rather strongly that these perceptions affect people's redistributive views: for example, believing that economic inequality is the result of unequal opportunity, or that success is largely a matter of luck, does seem to make people more favorable toward redistribution (eg, Alesina and Glaeser 2004). But the data analyzed here do little to indicate that secondary education's effects work through these perceptions.

To make progress in the study of education's effects, we need new research designs. As one set of authors has argued, it may be 'practically impossible' to derive unbiased estimates of education's effects from conventional regression or matching strategies: given the great variety of influences on educational attainment, conventional research designs do not seem capable of isolating education's effects from the effects of its many correlates (Henderson and Chatfield 2011, 647). Moreover, different designs will be needed to identify the effects of different levels of schooling, or of schooling in different places. This is so because a design that isolates schooling's effects at one level or in one place is unlikely to also isolate its effects at another level or in another place.

The introduction of new research designs must await future work in this area. What the current study shows is that, on average, the effects of US secondary education on economic attitudes seem to be conservative. Schooling at this level increases opposition to redistribution. And because economic attitudes figure prominently in many political phenomena – including ideological cleavages, party polarization, and party competition – this link between education and economic attitudes merits more attention than it has received.

**Supplementary material.** Replication datasets can be found in Harvard Dataverse at https://doi.org/10.7910/DVN/PDPQFB, and online appendices can be found at https://doi.org/10.1017/S0007123419000504.

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