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ORIGINAL PAPER

Education and Political Participation: Exploring the Causal Link

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Abstract One of the most consistently documented relationships in the field of political behavior is the close association between educational attainment and political participation. Although most research assumes that this association arises because education causes participation, it could also arise because education proxies for the factors that lead to political engagement: the kinds of people who participate in politics may be the kinds of people who tend to stay in school. To test for a causal effect of education, we exploit the rise in education levels among males induced by the Vietnam draft. We find little reliable evidence that education induced by the draft significantly increases participation rates.

Keywords Turnout · Voting · Education

One of the most consistently documented relationships in the field of political behavior is the close association between educational attainment and political participation. For more than half a century, researchers have found that more educated citizens are more likely to vote in elections and participate in campaigns (for reviews, see Campbell et al. 1960; Hillygus 2005; Nie et al. 1996; Schlozman 2002; Wolfinger and Rosenstone 1980).

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For some scholars, the reason for this relationship is clear: education gives citizens the skills and resources needed to participate in politics. For instance, Verba et al. (1995) argue that education not only directly increases levels of participation, but also allows citizens to acquire the civic skills necessary to effectively communicate their concerns to politicians. Similarly, Rosenstone and Hansen argue that education "imparts the knowledge and skills most essential to a citizen's task….Because of their schooling, the well educated have the skills people need to understand the abstract subject of politics, to follow the political campaign, and to research and evaluate the issues and candidates" (1993, p. 136). From this point of view, it is the process of formal education that causes increased political participation.

One mystery, however, is why political participation has failed to increase with rising levels of education in the United States. Brody (1978) identified this puzzle in the realm of voter turnout, but it applies to other areas of political participation as well. If education truly imparts the civic skills that drive political participation, then increased levels of college attendance in America should be a rising tide that lifts all boats. The fact that participation levels have not kept pace with education gains calls into question the true causal effect of education.

In this paper, we consider a solution to Brody's puzzle. Perhaps education is less a measure of one's civic skills than it is an index of status in society, cognitive skills, and personality traits that leads to civic engagement. Education, in this view, is a proxy for the types of characteristics that lead to a taste for politics, one determined by pre-adult experiences and dispositions (Kam and Palmer 2008). In this paper, we employ a natural experiment caused by temporal variation in draft procedures and the rates at which men were called to service during the Vietnam War to assess the causal impact of higher education on political participation. Contrary to the conventional view, we find that education itself has little reliable causal effect on voter turnout.

The Traditional Versus the Revisionist View

How can we adjudicate between the traditional view of education as causal agent and education as indicator of the motivation to participate—what Kam and Palmer (2008) call "education as cause" and "education as proxy"? The types of regression analysis used in the study of political participation for much of the last 50 years can only get us so far because the two perspectives are observationally equivalent in cross-sectional data. Instead, we need to turn to new sources of data and new analytic techniques.

Two recent papers have employed panel data to address this question. Kam and Palmer (2008) use a matching-based approach with data from the Political Socialization Panel Study. This study collected data on high school seniors and their parents in 1965 and re-interviewed these same subjects in 1973. Kam and Palmer treat college attendance as the treatment variable. By matching the treated and untreated 1965 high school seniors on relevant characteristics, such as the wealth, education, and civic participation levels of their parents, Kam and Palmer construct

two groups that are comparable except that seniors in the first group went onto college and seniors in the second group did not. Kam and Palmer find that while college education seems to increase participation before the seniors were matched on background correlates, this apparent effect is due entirely to preexisting characteristics. After matching the 1965 high school seniors using propensity score techniques, education itself has no effect on levels of participation (though see Henderson and Chatfield (forthcoming) for a critique of these results).

In a similar vein, Tenn (2007) uses Current Population Survey panel data to compare two groups of respondents who are alike except for their levels of education. Specifically, Tenn compares those respondents who will acquire a particular level of education in the following year to those respondents who are one year older and have already acquired the comparable level of education. In effect, Tenn compares respondents who are about the get the treatment (a year of education) to those who already have received the treatment. This comparison enables him to gauge the marginal impact of a year of education. Tenn's design is different from Kam and Palmer's in that he estimates only the short-run impact of education for those currently in the process of schooling. However, like Kam and Palmer, Tenn's results do not support the traditional view of the effect of education. Instead, he finds that an additional year of schooling has little impact on the propensity to turn out. Both studies therefore indicate that other factors, such as family background, not education itself, lead to increased participation.

These studies are interesting and important, but they are not the final word on the matter. Other scholars have used instrumental variables (IV) analysis to control for the non-random component of educational attainment and find evidence that comports with the traditional view of education as a cause of participation. Dee (2004) explores two instruments—state-wide variation in compulsory education laws (Acemoglu and Angrist 2000) and proximity from an individual's high school to a 2-year community college (Card 1995). Dee finds that instrumenting education, if anything, *increases* the predictive power of education, indicating that educational attainment itself has a large and significant impact on levels of political participation. Using different datasets, but a similar analytic strategy, Milligan et al. (2004) find that instrumenting educational attainment with state-wide variation in compulsory education laws does not diminish the large positive impact of education on voting participation.[1] Finally, Sondheimer and Green (2010) track individuals who participated in three studies designed to increase high school educational attainment.[2] In all three studies, they find that exogenously induced changes in high school graduation rates lead to higher voter turnout.

In sum, the current findings are ambiguous. While Sondheimer and Green (2010), in particular, present strong experimental evidence of the effects of high school graduation, additional research is necessary to adjudicate between the "education as

[1] Though Dee (2004) and Milligan et al. (2004) come to common conclusions concerning the effect of education on turnout, the independent variables do differ across the studies. Milligan et al. (2004) use an indicator variable measuring completion of high school, while Dee uses college attendance (for the proximity instrument) and the highest grade of schooling completed (for the compulsory education instrument).

[2] Two of these studies were randomized studies and one was quasi-experimental.

cause" and "education as proxy" perspectives in the context of higher education. In the rest of this paper, we follow the path of Dee and use an instrumental variables approach to explore the effect of college education on participation. Instead of variation in compulsory education laws, we use cross-time variation in exposure to risk of the draft by the baby-boom generation in the late 1960s as an instrument for educational attainment. We can therefore assess whether the findings of Dee regarding the effect of college education differ from those of Tenn and Kam and Palmer because of the instrumental variables approach they adopt or because of the particular instruments they use.

A Natural Experiment: The Vietnam-Era Draft

The Vietnam-era draft provides several possible strategies for employing instrumental variables. From 1948 until 1973 the Selective Service drafted eligible men to fill vacancies in the armed forces that could not be filled through voluntary means.[3] Men between the ages of 19 (18.5 after 1951) and 26 were eligible for the draft. Until the late 1960s, local draft boards could grant deferments to individuals for a variety of reasons relating to school attainment and family obligations. However, in the middle and late 1960s, the U.S. government changed the selective service procedures on several occasions. The Military Selective Services Act of 1967 standardized the education deferment process, dictating that college students could defer military service until the receipt of an undergraduate degree or until reaching the age of 24, whichever came first. This policy stayed in effect through September 1971, when all new college deferments were eliminated (though men who were in college at that time were allowed to maintain their deferment until age 24). In part, this new policy was a reaction to the establishment of a draft lottery in 1969. On Nov. 26, 1969, President Richard M. Nixon signed into law an amendment to the Military Selective Service Act of 1967, which permitted establishment of a random selection sequence for induction into the armed forces. The first lottery was held on December 1, 1969 and generated the draft order for recruits in 1970 among men born between 1944 and 1950. The lottery assigned an order-of-call number to each birthday. A key feature of the lottery was that each cohort was only at risk of induction for a single year rather than for the entire period between the ages of 19 and 25. If men were not called to service before the next lottery, they were not required to enter the military. Additional lotteries were held in 1970, for the 1951 birth cohort, in 1971 for the 1952 birth cohort, and in 1972 for the 1953 cohort.[4]

At the same time that the education deferment laws were changing, the risk of induction into the armed services changed as well. With the ramp-up of the war effort in late 1965, the total number of monthly inductions increased almost threefold compared to the first half of 1965. The induction rate remained high for the next 4 years, before beginning to drop in late 1969. By the end of 1971, the

[3] This information is drawn from the Selective Services website (http://www.sss.gov/, accessed August 11, 2008) and Card and Lemieux (2001).

[4] This last cohort was never called to service as the draft ended in February 1973.

induction rate fell to a trickle (Card and Lemieux 2000). Thus, in the mid-to-late 1960s, draft eligible youth faced greater incentives to avoid the draft than did men eligible before and after that time.

All told, the Vietnam-era draft provides several possible avenues for instrumental variables analysis. Those males eligible for the first draft lottery were also eligible for educational deferments. Because the draft order was generated through a random process, we could use lottery number as an instrument for college attendance. Presumably those individuals with the lowest draft numbers—those most likely to be called to service—would be most likely to seek education deferments. Indeed Angrist and Krueger (1992) find that individuals with low lottery numbers were more likely to attend college than those with high numbers.

The temporal variation in draft procedures and induction rates over the course of the 1960s and 1970s provides another possible avenue for instrumental variables analysis. Between 1965 and 1975, college enrollments in the U.S. increased, then declined. This pattern was not random; strong sex differences were at work. Relative to females, males' college education rose for the cohort born between 1944 and 1950, peaked for those born between 1946 and 1948, then fell abruptly for males born between 1951 and 1953. Card and Lemieux (2001) demonstrate that this pattern of enrollment reflected draft-avoidance behavior. Men born in the peak years were eligible for the draft and could obtain education deferments. Males born in the early 1940s could technically receive deferments, but rarely did so, probably because they were older and already in the workforce when the risk of induction surged. Males born after 1950 were part of later draft lotteries and, assuming they drew a low number, might not be eligible for educational deferments by the time they were to begin college because of the changes to the deferment policies discussed above. But even before the draft lottery went into effect, as noted above, the rates of induction slowed greatly after 1969 and, by June 1971, fell to a trickle. As Card and Lemieux note, "The limited period of exposure [to the draft], coupled with the relatively low rate of inductions after 1969, substantially reduced the incentives for enrolling or staying in college to avoid the draft" (2001, p. 98).

Using census data from the October release of the Current Population Survey (CPS), Card and Lemieux (2001) estimate that draft avoidance raised the fraction of men in the 1947 birth cohort with some college education by about four percentage points, and raised the proportion with a college degree by 2.2%.[5] Card and Lemieux find that the rise and fall in educational obtainment was not paralleled by females, further suggesting that the increases in education were driven by responses to changing draft regulations and induction rates.

[5] Card and Lemieux (2001) explore the possibility that the increased educational attainment of this cohort is attributable to post-service GI Bill benefits. They reject this alternative explanation. First, they find that most of the excess college enrollment was attributable to men who had not yet served in the military. Second, they find that, if anything, the rate of post-service degree attainment was lower for Vietnam-era veterans than for earlier cohorts. Using a similar model, with data from the National Health Interview Survey, MacInnis (2006) finds that the pre-lottery Vietnam draft caused approximately a 3.7% increase in college completion.

The Choice of Instruments

The draft lottery is the more potentially promising of the two instruments, because the lottery was a true random process. The randomly assigned risk of military induction has been used to explore the effect of veteran status on lifetime earnings (Angirst 1990), mortality (Angrist et al. 1996), and the effect of education on earnings (Angrist and Krueger 1992). Moreover, Erikson and Stoker (2009) used draft numbers to study attitudes toward the Vietnam War.

There are, however, some practical concerns that make this strategy difficult. First, obtaining the necessary data to examine political questions is extraordinarily difficult. Respondents' birthdays are often redacted to preserve confidentiality in those datasets that contain information on turnout. But even with information on date of birth, using the draft lottery as an instrument requires national sample datasets of several hundred thousand respondents in order to isolate a large enough sample of the relevant cohorts.[6] Thus, using the draft lottery as an instrument is not practical for our purposes. In the next section, we instead exploit the temporal variation to explore the impact of education on turnout.

Methodology

The discontinuity in college education levels uncovered by Card and Lemieux (2001) provides another exogenous source of variation in education, which several researchers have exploited. MacInnis (2006), for example, uses it to examine the causal effect of education on her dependent variable of interest: health outcomes. Grimard and Parent (2007) and de Walque (2007) study the effect of education on smoking. We adopt a similar strategy to assess the causal effect of college education on voter turnout.

Following Card and Lemieux (2001), we compare men born in the late 1940s (our treatment group) to women also born during this period, and to men and women born before or after this cohort. We focus on the three peak years in the discontinuity: 1946, 1947, and 1948. We also show, in the robustness analysis to follow, that our results are similar when we focus on all the affected cohorts (1944–1950). Given the inefficiency of IV estimators, we maximize our sample size by combining individuals born between 1935 and 1959 from the November release of the CPS from 1972 to 2000, which yields about 250,000 individuals.[7] Our results are similar when we limit the control group comparison to those born just before and

[6] Moreover, educational deferments were eliminated just over a year after the lottery was introduced, resulting in relatively few individuals who were eligible for both the lottery and educational deferments. An additional problem is that the draft lottery must be used as an instrument for both education and veteran status. Identification therefore must proceed via assumptions concerning the functional form of these relationships (Angrist and Krueger 1992).

[7] We did not include the 1976 CPS because the survey did not record state of residence. It should be noted that the results presented below hold if each individual study is excluded and the analysis rerun. Also, we exclude respondents who fail to finish high school because they would not be eligible to receive the treatment of a college education.

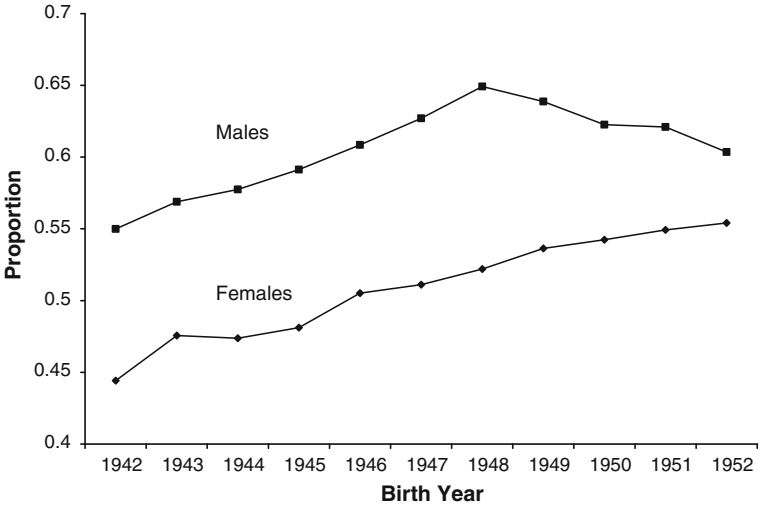

Fig. 1 Proportion completing some college by sex. *Source*: 1972–2000 CPS, November Supplement

just after the peak years rather than the full period from 1935 to 1959 (see robustness analysis to follow).

Using the CPS data, Figs. 1 and 2 show that males completed some college and served in the military at higher rates.[8] For presentational purposes, we present data only for the 1942–1953 birth cohorts. Card and Lemieux (2001) present data for the full 1935–1959 cohorts using October CPS Census data and find very similar results. As these figures also make clear, these rates vary across birth years within the treatment cohorts. The tendency of males to complete some college peaks toward the end of the treatment period, while their tendency to serve in the military peaks toward the beginning. In contrast, the female completion rate increases across birth years as it converges toward the male completion rate. College attendance and military service therefore vary independently by birth year—variation we exploit to identify the effects of college attendance on the decision to vote.

As the dependent variable, we use self-reported turnout in previous elections. Our key explanatory variable, *Some College*, is an indicator for having attended at least some college. We use this variable because we expect that the greatest effect of draft avoidance would be seen in college attendance, rather than degree completion. Empirically, Card and Lemieux (2001) find that, in fact, the draft had a large effect on the number of males enrolling in college.[9] We instrument Some College with interactions between *Birth Year* indicator variables for the 1946–1948 cohorts and *Male*.

[8] The CPS sampling frame excludes active members of military. Thus, no respondents in our sample are active members of the military.

[9] The basic results are similar when we use either college degree or the approximate total number of years in college as our measure of educational attainment (see Table 5).

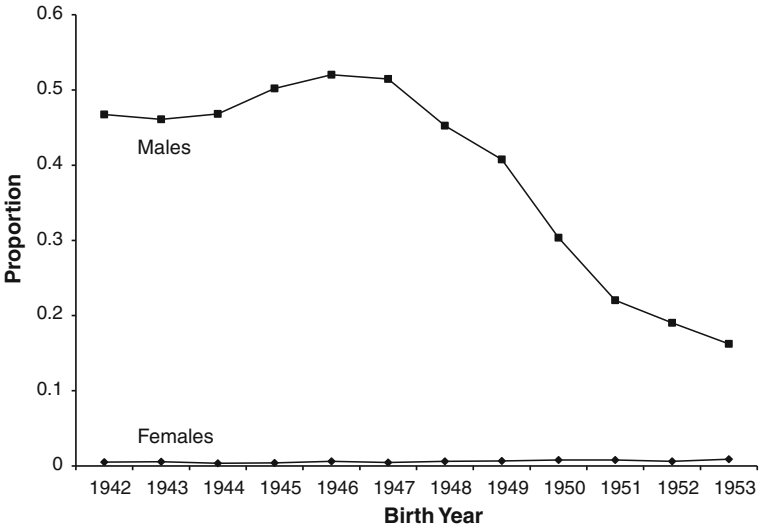

Fig. 2 Proportion of veterans by sex. *Source*: 1972–2000 CPS, November Supplement

We also include several control variables in our analyses. Between 1935 and 1959, college enrollment rates rose, irrespective of the draft, and females' college enrollment continued to converge on (and later surpass) male enrollment. To account for these trends, we include a Birth Year variable, which we also interact with sex (Birth Year * Male).[10] We also estimate models with more flexible functional forms, but these leave the main results unchanged (see robustness analysis to follow). We also include indicator variables for *White*, *Married*, and *South*.[11]

A potential limitation of this research design is that the treatment could influence turnout through a causal path unrelated to education, violating what researchers call the exclusion restriction. For example, induction risk not only increases education in our cohort, but also increases the share of veterans, which could itself influence turnout. Although studies generally find that veteran status increases participation (e.g., Teigen 2006), the Vietnam War appears to be unusual. Research on the panel of 1965 high school seniors, many of whom served in Vietnam, finds no effect of veteran status on participation (Jennings and Markus 1976; Teigen 2006). Using the much larger cross-sectional CPS, Teigen (2006) finds a small negative effect of

[10] By including this interaction, we also control for the downward trends in male veteran rates, which fall from a high of 59% in the 1935 cohort to a low of 11% in the 1959 cohort, the last one we examine. To render coefficients more interpretable, we rescale Birth Year to vary between zero and one.

[11] We include a variable for married because it strongly influences turnout. However, the results with the "married" variable should be interpreted with caution because this variable may introduce post-treatment bias. We did not include income because it could be an intervening variable (i.e., the draft leads to more education, which leads to higher incomes, which fosters political participation). Previous research has shown that veteran status negatively affects future earnings among Vietnam-era males (Angirst 1990). However, as a robustness check, we included income quintiles as a series of dummy variables in our regressions and the results did not change.

serving in the Vietnam War. Vietnam-era veterans, all else equal, are about one-percentage point less likely to vote compared to their peers. To address bias from this alternative causal path, we follow Grimard and Parent (2007) and also instrument veteran status with interactions between Birth Year in our treatment group and Male.[12] For this approach to succeed, the patterns of military service and college enrollment must vary across the treatment group. As we showed above, they do. Males in the treatment cohorts are generally much more likely to serve than to attend some college (compared to the control groups), and these tendencies vary across birth years within the treatment cohorts. We exploit this variation to identify the effects of college and veteran status on voter turnout.

Results

Table 1 presents the results of the regression analysis where we ignore the potential endogeneity of educational attainment.[13] Here we appear to find confirmation for the long-standing positive education and turnout. Among the cohort of individuals born between 1935 and 1959, attending some college increases the probability of voting by about 19%. This result, however, cannot tell us if education causes the increased likelihood of voting, or is instead a proxy for personality and family characteristics that lead to high levels of participation. To adjudicate between these explanations, we turn to our instrumental variables analysis.

Table 2 presents the first stage estimates of the IV regression for college; the effect of the treatment—being a male born between 1946 and 1948—on education. We estimate draft-avoidance effects by regressing the Some College variable on the control variables and the treatment cohorts interacted with Male.[14] Consistent with Fig. 1 and previous studies, we find that males born between 1946 and 1948 were significantly more likely to complete some college. As expected, the difference in college enrollment varies by birth year. Males born in 1946, for instance, are about five percentage points more likely to attend college than we would expect based on the long-term trends in male college attendance. In 1947 this effect rises to seven percentage points, and by 1948 it rises to 10 percentage points. The positive sign on the Birth Year coefficient reflects the upward trend in female college enrollment between 1935 and 1959, while the negative coefficient on the interaction between Male and Year of Birth reflects the fact that the rate of college attendance among females began catching up with males during this period. Although these simple linear trends capture the broad patterns in the data, they of course do not fit it

[12] We use an indicator of veteran status. Theoretically, respondents could have served in the armed forces, but not in Vietnam. For our treatment group, however, veteran status indicates service during the Vietnam era, as defined by the CPS codebook.

[13] Fixed effects for CPS study year are included but not shown in all models. Following the convention of applications of this instrumental variable approach in economics (Angrist and Pischke 2009; Grimard and Parent 2007; MacInnis 2006), we use linear probability models in both stages of the regression.

[14] This model therefore assumes that the relative schooling choices of men and women would follow the same path in the absence of sex-specific factors, notably the draft (Card and Lemieux 2001).

Table 1 Effect of some college on voter turnout for the Vietnam era cohort in the 1972–2000 CPS (OLS)

	(1)	(2)	(3)	(4)
Some college indicator	0.19***	0.19***	0.19***	0.19***
	(0.0018)	(0.0018)	(0.0018)	(0.0018)
Veteran indicator	−0.028***	−0.028***	−0.027***	−0.028***
	(0.0028)	(0.0028)	(0.0028)	(0.0028)
Male	−0.023***	−0.024***	−0.024***	−0.032***
	(0.0043)	(0.0042)	(0.0042)	(0.0042)
Birth year	−0.23***	−0.23***	−0.23***	−0.21***
	(0.0043)	(0.0042)	(0.0042)	(0.0042)
Male * birth year	−0.013**	−0.014**	−0.014**	−0.0014
	(0.0064)	(0.0063)	(0.0063)	(0.0063)
White		0.060***	0.056***	0.038***
		(0.0028)	(0.0028)	(0.0028)
South			−0.042***	−0.044***
			(0.0023)	(0.0022)
Married				0.095***
				(0.0019)
Constant	0.71***	0.66***	0.67***	0.62***
	(0.0035)	(0.0043)	(0.0044)	(0.0045)
N	247876	247876	247876	247876
R^2	0.074	0.076	0.077	0.086
SER	0.43	0.43	0.43	0.43

Dependent variable is turnout in presidential elections. Fixed effects for CPS study year included but not shown. Standard errors in *parentheses*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

perfectly.[15] But even though the R^2 statistics for the first stage regressions are small, the *F*-statistics for the excluded instruments in those regressions are around 115, well above the rule of thumb value of 10 suggested by Staiger and Stock (1997). Thus, while the effect of the induction risk (as captured by these indicator variables) on attending college is not huge, the instruments work in the predicted manner.

Table 3 presents the first stage estimates for Veteran status. Consistent with Fig. 2 and previous studies, males born between 1946 and 1948 faced a considerably greater risk for serving in the military compared to the control groups. The effect is particularly large for males born in 1947, who are 17% more likely to have served in the military. Unlike the effect of birth cohort on education, which peaks in 1948, these effects peak earlier, in the 1946 and 1947 cohorts. Thus, compared to what we would otherwise expect, males born in 1946 and 1947 were the most likely to serve, while males born in 1948 were most likely to complete some college. The *F*-statistics for the excluded instruments in these first stage models are larger than 1,000, well above the rule of thumb value.

[15] For instrumental variables estimates to be consistent, however, the first stage need not be correctly specified (Angrist and Pischke 2009, p. 191). It must only predict exogenous variation in the variable being instrumented.

Table 2 Effect of draft on some college for the Vietnam era cohort in the 1972–2000 CPS (first stage)

	(1)	(2)	(3)	(4)
Male	0.14***	0.14***	0.14***	0.14***
	(0.0045)	(0.0045)	(0.0045)	(0.0045)
Birth year	0.14***	0.14***	0.14***	0.14***
	(0.0048)	(0.0048)	(0.0048)	(0.0049)
Male * birth year	−0.15***	−0.15***	−0.15***	−0.15***
	(0.0070)	(0.0070)	(0.0070)	(0.0070)
White		0.049***	0.047***	0.051***
		(0.0032)	(0.0032)	(0.0032)
South			−0.019***	−0.018***
			(0.0026)	(0.0026)
Married				−0.023***
				(0.0022)
Male * birth year = 1946	0.054***	0.054***	0.054***	0.055***
	(0.0072)	(0.0072)	(0.0072)	(0.0072)
Male * birth year = 1947	0.072***	0.072***	0.072***	0.073***
	(0.0066)	(0.0065)	(0.0065)	(0.0065)
Male * birth year = 1948	0.095***	0.095***	0.096***	0.096***
	(0.0068)	(0.0068)	(0.0068)	(0.0068)
Constant	0.33***	0.29***	0.29***	0.30***
	(0.0039)	(0.0049)	(0.0049)	(0.0051)
N	247876	247876	247876	247876
R^2	0.027	0.028	0.028	0.028
SER	0.49	0.49	0.49	0.49

Dependent variable is an indicator variable for attending some college. Fixed effects for CPS study year included but not shown. Standard errors in *parentheses*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4 presents the instrumental variables analysis. The estimate of education's influence on the propensity to vote is considerably lower than the standard positive effect. In the simplest model, which controls only for sex and year of birth, completing some college increases the probability of voting by just 6%—less than a third of the 19% increase from the standard OLS approach (see Table 1). Adding controls for race, living in the South, and being married slightly increases this effect to about 8%, still less than half the OLS estimates.[16] These estimates are admittedly imprecise, which is often the case with instrumental variables. Because of this imprecision, both the 19% OLS estimates in Table 1 and a null effect of zero fall within the 95% confidence interval of the IV estimate. Nevertheless, the point estimates results suggest that much of the OLS effect arises, not because education is causal, but because it proxies for other factors, that is, the kinds of people who get educated are the kinds of people who tend to participate in politics anyway.

To check the robustness of these findings, we ran numerous additional specifications and found similar results. Table 5 presents a wide range of these

[16] Including the married indicator variable slightly changes the coefficient, but this result should be interpreted with caution because this variable may introduce post-treatment bias.

Table 3 Effect of draft on veteran status for the Vietnam era cohort in the 1972–2000 CPS (first stage)

	(1)	(2)	(3)	(4)
Male	0.61***	0.61***	0.61***	0.61***
	(0.0028)	(0.0028)	(0.0028)	(0.0028)
Birth year	0.0035	0.0034	0.0035	0.0042
	(0.0031)	(0.0031)	(0.0031)	(0.0031)
Male * birth year	−0.54***	−0.54***	−0.54***	−0.54***
	(0.0044)	(0.0044)	(0.0044)	(0.0045)
White		−0.00077	0.00032	−0.00047
		(0.0020)	(0.0020)	(0.0020)
South			0.011***	0.011***
			(0.0016)	(0.0016)
Married				0.0042***
				(0.0014)
Male * birth year = 1946	0.15***	0.15***	0.15***	0.15***
	(0.0046)	(0.0046)	(0.0046)	(0.0046)
Male * birth year = 1947	0.17***	0.17***	0.17***	0.17***
	(0.0041)	(0.0041)	(0.0041)	(0.0041)
Male * birth year = 1948	0.13***	0.13***	0.13***	0.13***
	(0.0043)	(0.0043)	(0.0043)	(0.0043)
Constant	−0.013***	−0.013***	−0.016***	−0.018***
	(0.0025)	(0.0031)	(0.0031)	(0.0032)
N	247876	247876	247876	247876
R^2	0.291	0.291	0.291	0.291
SER	0.31	0.31	0.31	0.31

Dependent variable is an indicator for veteran status. Fixed effects for CPS study year included but not shown. Standard errors in *parentheses*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

checks. As we noted above, trends in education and veteran status are broadly linear by cohort year in our sample. To ensure that deviations from this linear pattern do not drive our results, the first two rows of Table 5 control for Birth Year squared and cubed (and their interactions with Male). Reassuringly, these flexible functional forms leave the results unchanged. Next, we check the sensitivity to changes in the control cohorts, which are males born 1935–1945 and 1949–1959 and females born 1935–1959. We first show that the results remain the same when we drop females from the analysis. Then, we show that the findings also remain unchanged when we limit the control cohorts to males and females born just before or just after our treatment cohorts, using 4 year windows (i.e., 1942–1945 and 1949–1952). Finally, they also remain unchanged when we exclude affected cohorts who are not in the peak years (i.e., 1944, 1945, 1949 and 1950). As the next rows of Table 5 show, we also ran our models using other measures of education—namely the attainment of a college degree and the number of years in college and graduate school.[17] These

[17] The total number of years in college was not available for the full CPS sample. To approximate the number of years in college variable, we use estimates from the 2000 National Annenberg Election Study.

Table 4 Effect of college on voter turnout for the Vietnam era cohort in the CPS 1972–2000 (IV)

	(1)	(2)	(3)	(4)
Some college indicator	0.057 (0.15)	0.065 (0.15)	0.072 (0.15)	0.083 (0.15)
Veteran indicator	0.056 (0.077)	0.052 (0.076)	0.048 (0.076)	0.027 (0.076)
Male	−0.056** (0.029)	−0.056* (0.028)	−0.054* (0.028)	−0.051* (0.028)
Birth year	−0.22*** (0.022)	−0.21*** (0.022)	−0.22*** (0.022)	−0.20*** (0.021)
Male * birth year	0.013 (0.023)	0.010 (0.022)	0.0093 (0.022)	0.012 (0.022)
White		0.066*** (0.0079)	0.061*** (0.0075)	0.044*** (0.0081)
South			−0.045*** (0.0043)	−0.047*** (0.0042)
Married				0.092*** (0.0042)
Constant	0.76*** (0.051)	0.69*** (0.044)	0.71*** (0.045)	0.65*** (0.047)
N	247876	247876	247876	247876
R^2	0.048	0.053	0.057	0.070
SER	0.44	0.44	0.44	0.43

Dependent variable is turnout in presidential elections. Some college is instrumented with interactions between birth year and male for years of birth between 1946 and 1948. Fixed effects for CPS study year included but not shown. Standard errors in parentheses. The *F*-statistics for the excluded instruments in the first stages are around 115 for the Some College models and well above 1,000 for the Veteran indicator models

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

models yield similar results, .13 for College degree and .14 for College years, thus also suggesting an education effect below the OLS estimates for these variables (which are .19 and .30, respectively). In the last robustness check shown in Table 5, we check the sensitivity to which birth years we classify as treated. While the draft-induced shock to education was largest in the 1946–1948 cohorts, it had a smaller effect on those born 2 years earlier and 2 years later. When we expand the treated to include all the affected cohorts identified by Card and Lemieux (1944–1950), the effect becomes slightly negative, though still in a similar range.[18] Besides these

Footnote 17 continued
Based on these estimates, we code some college in the CPS to 13.8 years, college degree to 16 years, and more than a college degree to 18.3 years. We then re-scale this variable to vary between 0 and 1.

[18] We also checked to see if the results differed according to their proximity to the Vietnam War. One reviewer argued that if some other factor influenced the educational attainment and voting propensity of men born between 1946 and 1948, a violation of the exclusion restriction, the effect of the treatment might vary by time. In particular it might be largest in the elections most proximate to the Vietnam War. To explore this possibility, the reviewer suggested that we exclude the 1972–1980 period and see if the results hold up. We performed this analysis and our results did not change.

Table 5 Robustness tests for effect of college on voter turnout for the Vietnam era cohort in the CPS 1972–2000 (IV)

	College effect on turnout	
	OLS	IV
Including nonlinear trends by cohort		
Birth year^2 and male * birth year^2	.192 (.001)	.046 (.184)
Birth year^2, male * birth year^2, birth year^3, and male * birth year^3	.192 (.002)	.059 (.218)
Alternative control groups		
Males only	.200 (.002)	.070 (.152)
Four year windows: 1942–1945 and 1949–1952	.190 (.003)	.033 (.250)
Excluding affected, non-peak years (1944, 1945, 1949 and 1950)	.191 (.002)	.068 (.146)
Switching the instrumented variable from some college to		
College degree as instrumented variable	.196 (.002)	.127 (.360)
College years as instrumented variable	.304 (.003)	.138 (.320)
1944–1950 Treatment group (instead of the peak years 1946–1948)		
Some college as dependent variable	.190 (.002)	−.073 (.074)
College degree as dependent variable	.194 (.002)	−.040 (.162)
College years as dependent variable	.303 (.003)	−.070 (.170)

Dependent variable is turnout in presidential elections. These estimates include the following controls: male, birth year, male * birth year, White, South, Married, and fixed effects for CPS study year. Standard errors in *parentheses*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

robustness checks, we also conducted additional analyses where we combined data from several large-scale surveys of political behavior and found a similar effect of education on turnout.[19]

All told, these results raise doubts about the true causal impact of education implied by the strong OLS results (and, by extension, the empirical literature on political participation).[20] Admittedly, the instrumental variable estimates are quite noisy.[21] Because the standard errors for our IV estimates are large, we cannot reject

[19] Specifically, we combined data from: the Cumulative National Election Study (NES), the General Social Survey (GSS), the 2000 Annenberg Election Study, and the 2004 Annenberg Election Study. These data are less ideal because each study asks about different elections and different numbers of elections. In addition, we have no consistent indicator of veteran status. However, even given these differences we find a surprisingly consistent set of results, also suggesting that education may fail to increase turnout. These results are available from the authors upon request.

[20] These findings also may shed some light on the effect of Vietnam-era service on the propensity to vote. As noted above, research on this war finds a reduced propensity to vote among Vietnam era veterans, a finding we replicate in Table 1. This negative finding could arise, however, not because serving suppresses voting, but because of a selection effect. The kinds of people who served may be predisposed not to vote for other reasons. The IV estimates provide may some support for this alternative, as instrumenting veteran status flips the sign on the veteran coefficient (see Table 4). However, this effect is not statistically significant.

[21] The imprecision of the IV estimate here is similar to that found in Grimard and Parent (2007).

the hypothesis that the standard OLS estimate is correct. However, while, like Henderson and Chatfield (forthcoming), our estimates do not allow us to say anything definitive in a statistical sense about the causal relationship between education and voter turnout, the point estimates of our IV analysis are consistently smaller than the OLS estimates. Moreover, our finding does not appear to be an artifact of our method. Other researchers have used similar instruments for education and found strong statistically significant effects of education on other outcomes, such as smoking avoidance (Grimard and Parent 2007; de Walque 2007).[22] In other words, our approach is not bound to produce null findings. Thus, the instrumental variables analysis casts doubt on the strong and precise positive effect of education found in the OLS analysis.[23]

Conclusion

Our findings call into question the large direct effect of education on political participation. Using an instrumental variables approach similar to Dee (2004) and Milligan et al. (2004), we find suggestive evidence that college education may not reliably increase the likelihood that an individual will vote in an election. Although they are admittedly imprecise, the point estimates from the IV analysis indicate that the true effect of education on the propensity to vote is positive but smaller than the substantively meaningful OLS estimate. Consistent with Kam and Palmer (2008) and Tenn (2007), our results suggest that factors such as family background or cognitive skills may lead individuals to both attend college and participate in politics. This conclusion reinforces concerns about voice and inequality in American politics (Verba, Schlozman, and Brady 1995), especially the tendency of upper-class and middle-class citizens to be heard because of their higher levels of participation. Previous research indicating that education increased participation suggested a policy prescription for leveling the playing field: more education. But, education levels have risen over the past generation, yet participation levels have failed to increase. Our findings indicate that education may not be entirely "the great leveler" and may partly be just "the great proxy" of preexisting characteristics.

[22] In addition, when we use the same IV estimation replacing turnout with income quartiles as the dependent variable, we find that the instrumented education measure has a positive and significant relationship with the respondents' income, as expected.

[23] It is, of course, important to note two caveats that generally apply to instrumental variable analysis. Our estimates only describe the effect of education among those induced to attend college because of the draft. This effect may be different from the effect of college among individuals who attend college voluntarily. Researchers refer to this aspect of instrumental variables estimates as a Local Average Treatment Effect (LATE). In our case, it is hard to say a priori whether the treatment group would be more or less predisposed to vote. They might be less likely to vote than other educated people because of mistrust of government. Alternatively, they might be more "activist" and so more likely to vote. The second caveat is that, with instrumental variables analysis, bias from an exclusion restriction violation always remains a possibility.

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