Does Postsecondary Education Result in Civic Benefits?

William R. Doyle
Vanderbilt University

Benjamin T. Skinner
Vanderbilt University

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Abstract

Public support for higher education depends in part on the idea that additional postsecondary education results in civic benefits. Among these civic benefits are voting, volunteering and donating to non-profit causes. We expand on the literature on the civic benefits for higher education by utilizing a rich set of location-based instruments to identify the relationship between additional postsecondary education and civic behaviors, including voting, volunteering and donating money to non-profit organizations. Using data from the National Longitudinal Survey of Youth, 1997, we estimate the impact of postsecondary education on civic behaviors for a group of young people who were age 29-33 by 2013. These new estimates indicate that an additional year of higher education increased the probability of voting by 7.7 percent in the 2010 election. We also find statistically significant though substantively small impacts of postsecondary education on both voluntarism and donations to non-profits, with effect sizes of .1 for voluntarism and .13 for donations.

Public and private support for higher education at the undergraduate level hinges on two key impacts of higher education on individuals (McMahon, 2010). First, it is expected that higher education results in an increase in both market and non-market benefits that accrue directly to the individual. The market benefits are greater earnings (Card, 2001). The non-market benefits are various other aspects of the individual's well being, including health and happiness (Paulsen & Smart, 2007; Rowley & Hurtado, 2002). Second, it is expected that higher education results in substantial public benefits. These public benefits accrue to the society at large, improving life even for those individuals who did not go to college (Colby, Ehrlich, Beaumont, & Stephens, 2003; McMahon, 2010).

A large body of research has been established linking increased education—including increased postsecondary education—to better civic outcomes (Bowman, 2011; Colby et al., 2003; Hurtado, 2007). This finding has been established through correlational studies, through experimental studies and through studies that make use of instrumental variables (Dee, 2004; Perna, 2005; Smets & van Ham, 2013). There may be reasons to doubt, however, whether the previously observed link between education and civic outcomes still holds. The cohort of young people who entered the labor market in the period between 2007 and 2010 faced some of the worst economic conditions in half a century (Bell & Blanchflower, 2011; Elsby, Hobijn, & Sahin, 2010). Changes in postsecondary education, the labor market and in societal values may have led to changes in the link between postsecondary education and civic outcomes. In this study, we ask: to what extent does postsecondary education continue to cause an increase in civic behaviors, including voting, volunteering and donation to non-profits?

This study makes three contributions to the literature in this area. First, it updates the literature on the civic returns to higher education by reporting results from the National Longitudinal Survey of Youth 1997 (NLSY97) cohort, which was of voting age by the 2004 election. We provide results for every national election year from 2004 to 2010, producing estimates for Congressional election years as well as presidential election years. We similarly provide multiple estimates of the impact of postsecondary education on volunteering and charitable donations across multiple years. Second, we use a much richer set of location-based instruments in order to test the sensitivity of the results to the choice of instruments. Last, due to refinements in the literature regarding how estimates from instrumental variables approaches should be interpreted, we extend the literature by discussing what each estimate means when properly understood as a weighted averages of causal effects for subpopulations that were induced into treatment by the instruments (Angrist & Pischke, 2008).

Previous Findings

A large literature connects additional postsecondary education to higher earnings (Card, 1995, 2001). A similarly large literature exists that describes or extols the possible civic benefits of higher education (Colby et al., 2003; Ehrlich, 1999, 2000; Kezar, Chambers, & Burkhardt, 2015; Nie, Junn, & Stehlik-Barry, 1996; Pascarella, Ethington, & Smart, 1988; Pascarella & Terenzini, 2005). A much smaller—but still substantial—number of studies have been undertaken to establish the causal link between higher education and civic benefits such as volunteering and voting (Dee, 2004; Verba & Nie, 1972; Wolfinger & Rosenstone, 1980). Instead of comprehensively reviewing this literature, we focus on a smaller subset of recent studies that feature compelling identification strategies.

McMahon (2010) provides a broad overview of the literature on the externalities from higher education, including the effects on measures of democratization, human rights, political stability, life expectancy, inequality, crime, environmental impacts, overall levels of happiness and utilization of new technology. Other studies have identified the correlational link between education and civic outcomes. For example, Perna (2005) estimates the impact of postsecondary degree attainment on various outcomes, including economic, non-market and civic outcomes. Perna finds that higher levels of degree attainment are associated with a higher probability of voting. Perna further finds that the association between increased education and voting differs by sex and race.

Perna's findings are echoed by a broader meta-analysis of the impact of various personal characteristics on voting rates. Smets and van Ham (2013) find that among 90 eligible studies that had been conducted between 2000 and 2010, 67 used education as an independent variable. In those studies, approximately 70 percent found a relationship between education and earnings, with an estimated effect size education on turnout of .72 (Smets & van Ham, 2013). As is the case with non-experimental studies, however, even those studies with statistically significant findings are unlikely to account for possible endogeneity between the outcome and key independent variable, in this case, voting and education.

Sondheimer and Green (2010) provide experimental evidence of the impact of education on civic outcomes. They make use of existing experiments that resulted in increased educational attainment in the treatment group. They use three different experiments as the basis of their research. The authors used either project data or their own follow ups to measure voting rates among both treatment and control students in each of these experiments. They find that inducing a student who might otherwise drop out from high school to graduate from high school would change that student's probability of voting from 15.6% to 62.5% (Sondheimer & Green, 2010). Though these experimental findings clearly can be interpreted as causal, what is not clear is the extent to which these results may be extended to other populations (Sondheimer & Green, 2010).

A key quasi-experimental study using large-scale data comes from Dee (2004). In his paper, Dee uses two separate datasets and two different identification strategies to estimate the impact of additional years of education on civic outcomes. Dee's research is important because of the strength of the identification strategy utilized and the high degree of external validity of the study.

Dee first takes up measures of voter registration, voting in the previous year, voting in the 1988 Presidential election and volunteering in the previous 12 months. Using data from the High School and Beyond survey of 1980, he estimates the impact of postsecondary entry on these civic behaviors for a group of 1980 high school sophomores in 1992, when the cohort's average age was 28. Dee uses two instruments to identify the impact of postsecondary entry on voting: the number of two-year colleges in the county and the distance from the students' home to the nearest two-year college. Dee finds that postsecondary entry increases the predicted probability of voting by 21.5 percentage points but does not find an observable impact of postsecondary entry on volunteering (Dee, 2004).

In his second estimation strategy, Dee uses the General Social Survey to provide broader estimates of the impact of education on civic outcomes for the entire adult population. In particular, he uses the highest grade completed as the independent variable to predict voting, newspaper readership, group membership, and attitudes regarding freedom of speech. In his instrumental variable estimates, Dee makes use of variation of child-labor laws across states. Dee finds that for each additional year of education, the predicted probability of voting in the previous Presidential election increases by 6.8 percent (Dee, 2004).

We provide additional evidence beyond Dee's contribution along several dimensions. First, using data from the NLSY97 we are able to update Dee's estimates, which are from 1992, by up to 20 years. Second, we propose a broader array of location-based instruments and demonstrate how estimates vary depending on the choice of instrument (Angrist, Imbens, & Rubin, 1996; Card, 2001). Last, we are able to provide multiple estimates across time periods for our cohort.

Theoretical Background

Why would higher education increase an individual's probability of voting or volunteering? First, an increase in education might increase the intrinsic value of civic behavior, as an individual learns to value the contributions he or she can make to society (Colby et al., 2003). An increase in education might also increase the extrinsic value of civic behavior, as an individual enjoys the increase in social status associated with higher levels of civic participation (Ariely, Bracha, & Meier, 2009; Gerber, Green, & Larimer, 2008). Last, as we discuss below, voting is likely a special case in which two additional terms—the policy benefit for a certain candidate winning and the probability of a vote being pivotal—play a role (Gerber et al., 2008).

A general model of civic participation

The basic model of civic participation (either voting, volunteering, or donating funds) is based on a calculation of utility on the part of the individual. We base this model and our discussion on the model in Gerber et al. (2008). We assume that every individual receives a benefit from civic behavior, which we term D. In general, a person will vote, volunteer or donate if D is greater than the costs of civic participation, C:

$$Civic = \begin{cases} 1 & \text{if } D > C \\ 0 & \text{if } D \leq C \end{cases} \tag{1}$$

Costs include foregone income and travel costs for voting and volunteering, and the direct costs of donating. The utility of civic behavior can be thought to have two components, intrinsic and extrinsic:

$$D = U(D_I, D_E) \tag{2}$$

Higher education could affect equation (1) in several ways. Higher education could change either D_I or D_E, as individuals gain increased satisfaction (D_I) or feel that it is important that they be known as the type of person who engages in civic behaviors (D_E). Higher education could also change the cost of participation, C, likely by raising the cost of foregone income for an individual.

Higher education could increase D_I by providing an increased understanding of how valuable civic participation can be. Students in higher education may take classes that let them know how society depends on voluntary behavior such as voting or donating. This knowledge may change a student's perceived intrinsic rewards. This view conforms most closely with the self-perception of most colleges and university leaders, judging by their public statements (Colby et al., 2003; Ehrlich, 1999, 2000).

Another possible route by which higher education may affect the utility associated with voting is via an extrinsic reward, denoted D_E above. Extrinsic rewards accrue when other people learn about the individual's voting, volunteering or donations. Individuals vary in the extent to which they seek public recognition of their civic behavior (Olson, 2009). The extrinsic rewards for civic behavior might be higher for a more educated individuals as they are expected by their peers to be the kinds of persons who participates in civic activities (Ariely et al., 2009).

In summary, while higher education may actually increase the costs of various civic activities, it may also at the same time increase both the intrinsic and extrinsic rewards associated with greater civic participation. If we observe higher rates of voting among those who attend higher education, particularly in a well-identified model, we can safely assume that higher education has increased D above C, although we cannot parse out whether extrinsic or intrinsic benefits played the larger role.

The model for voting has some additional terms, based on two additional factors. First, candidates' policy positions may promise to directly benefit individuals, which would go beyond the intrinsic and extrinsic benefits of the act of voting itself. Again following Gerber et al. (2008), we label this policy benefit, based on the difference between the two candidates on various policies, as B. The model for voting therefore becomes

$$pB + D > C, \tag{3}$$

where p is the probability that an individual's vote is pivotal, B is the difference in the utility to the individual of candidate's policy positions, D is the direct benefit of voting and C is the cost of voting. For most individuals in most elections, p is (rightly) understood to be very small, leaving the decision about voting to the size of the direct benefit, D, and the cost of voting, C.

There are two ways in which higher education might affect the above benefit and cost calculation for any individual. First, higher education could lower an individual's calculation of p. On the other hand, higher education could change the relative weight of B as individuals understand more about the importance of various policy positions. While our data will not allow us to parse out the contribution of the various terms, our assumption is that additional postsecondary education increases both B and D, thereby increasing the likelihood of voting among more educated individuals.

Data

Our data come from the NLSY97, a Bureau of Labor Statistics (BLS) survey which follows a nationally representative cohort of approximately 9,000 people born between 1980 and 1984 beginning in their teenage years (Moore, Pedlow, Krishnamurty, & Wolter, 2000). Subjects were annually interviewed from 1997 to 2012, and every other year since. Because the components of the survey vary across years, we do not have data on civic behaviors for every individual in every year. For voting, we include data from four national elections—the years 2004, 2006, 2008 and 2010. For volunteering and donations to non-profits, we have data from four years: 2005, 2007, 2011 and 2013. BLS reports information on county of residence for respondents in each year. Our data are limited to those individuals for whom we have county of residence when they were 17 years old, the year in which we construct measures of college choice. These restrictions give us slightly variable sample sizes across the years (Moore et al., 2000).[1]

Voting

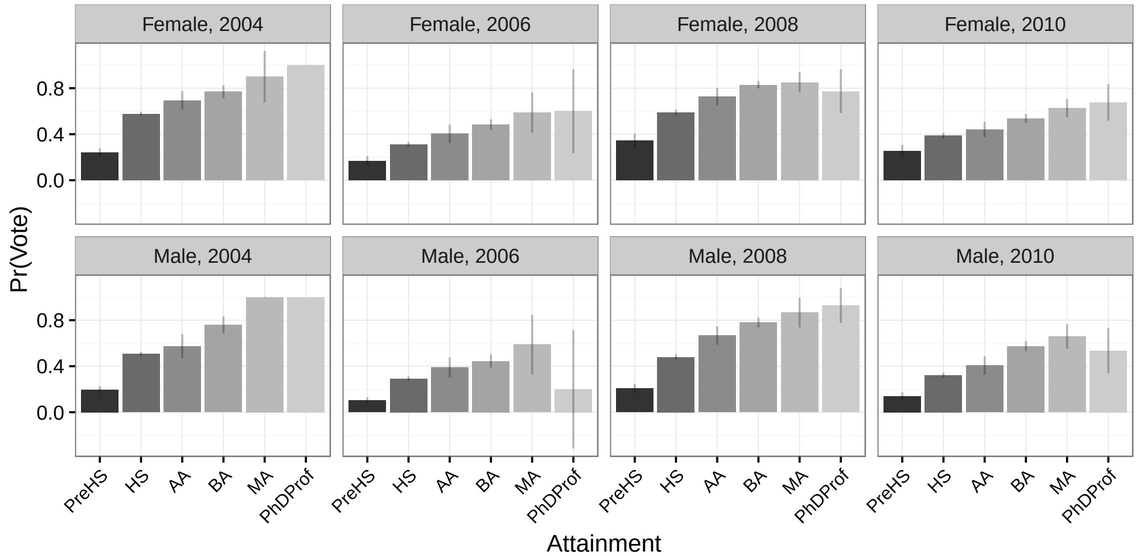
Our dependent variable for voting is based on the respondent's answer to the following question:

> Let's talk about [the recent election/the election last November]. In talking to people about elections, we often find that a lot of people were not able to vote because they weren't registered, they were sick, or they just didn't have time. Which of the following statements best describes you:
>
> One, I did not vote (in the election [this/last] November);
> Two, I thought about voting this time, but didn't;
> Three, I usually vote, but didn't this time; or
> Four, I am sure I voted?[2]

[1] NLSY97 uses a complex multistage sampling procedure. For simple descriptive statistics that are intended to provide estimates of population parameters, the Bureau of Labor Statistics provides sampling weights. For regression and other more complex techniques such as instrumental variables estimation, the BLS explicitly states that sampling weights should not be used (Bureau of Labor Statistics, 2016). In addition, our instrumental variable results can only be said to apply to a group of compliers whose characteristics we cannot directly observe. For these reasons, we characterize our sample as a large sample of young people from across the nation, but not as nationally representative.

[2] See question YPOL-110 [S49211.00] at `https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121128/nlsy97r8pol.html`

Figure 1. Voting by educational attainment and sex, 2004 to 2010



We set our dependent variable to one for those who answered that they were sure they voted (statement four) and zero for all other responses. We acknowledge from the start that this is self-reported data, and that it is likely that some individuals reported voting who did not vote. There is a large literature on likely voter models (Bernstein, Chadha, & Montjoy, 2001; Murray, Riley, & Scime, 2009). Most of the literature emphasizes the importance of asking questions in the way that the BLS does so that the individual does not feel pushed to provide a certain answer. Studies of likely voter models have also found that questions regarding previous voting behavior are highly predictive of future voting behavior. (Bernstein et al., 2001; Murray et al., 2009). Figure 1 shows voting rates by levels of educational attainment for men and women in our sample.

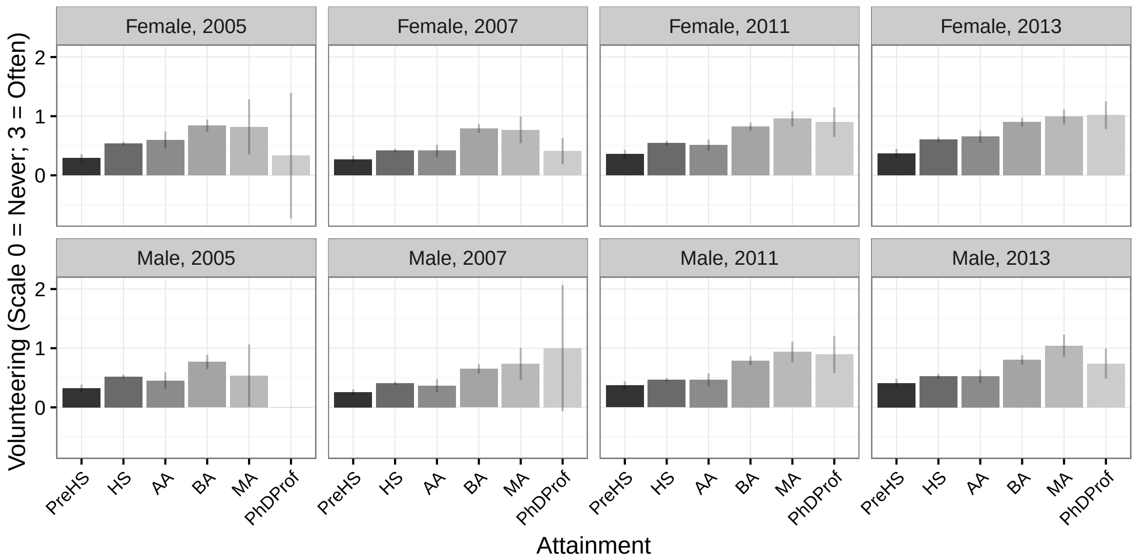
Volunteering

We base our dependent variable for volunteering on two questions:

> In the last 12 months, how often did you do any unpaid volunteer work, including activities aimed at changing social conditions, such as work with educational groups, environmental groups, landlord/tenant groups, or other consumer groups, women's groups or minority groups?
>
> 1 Never
> 2 1 - 4 times
> 3 5 - 11 times
> 4 12 times or more[3]
>
> Which of the following is the main reason you do volunteer work?

[3]See question YSAQ-300V1 [S63174.00] at https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121128/nlsy97r9saq1.html

Figure 2. Volunteering by educational attainment and sex, 2005 to 2013



1 Court ordered
2 Required for a school or religious group
3 Strictly voluntary[4]

So that we only include those respondents who volunteered of their own volition, we recode the first question to redefine as "Never" those individuals who indicated in the second question that they were either ordered by a court or required by a school or religious group to volunteer. We also shifted each of the response values for the first question down by one, which leaves us with an ordinal response that ranges from 0, *never volunteered*, to 3, *volunteered 12 times or more*. Our operational definition of volunteering only includes strictly voluntary work. Patterns of volunteering by educational attainment and sex are displayed in Figure 2.

Donating

Our dependent variable for donating is based on the following two questions:

In the last 12 months, have you donated money to a political, environmental, or community cause?

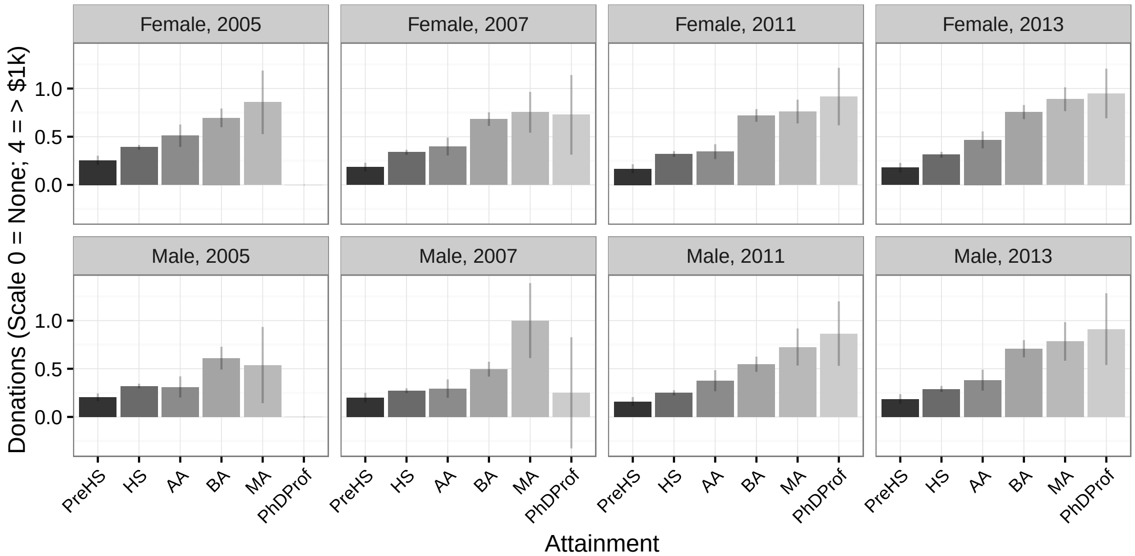
1 Yes
0 No[5]

What is the total amount of money that you have donated to these causes in the last 12 months?

[4]See question YSAQ-300V2 [S63175.00] at `https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121128/nlsy97r9saq1.html`

[5]See question YSAQ-300V4 [S63177.00] at `https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121128/nlsy97r9saq1.html`

Figure 3. Donations to causes by educational attainment and sex, 2005 to 2013



1 $1 - $100
2 $101 -$500
3 $501 - $1,000
4 More than $1,000[6]

For this outcome, we interact the dummy variable from the first question with the results of the second question in order to add a new base group of non-donations to the second question scale. This gives an ordinal response ranging from 0, *no donations*, to 4, *more than $1,000*. We display donations to charitable organizations by levels of educational attainment in Figure 3.

Responses to all three of our dependent variables are most likely subject to social desirability bias, which if random would impact the overall reporting but would not affect our estimates except to make them less precise (Greene, 2011). However, if social desirability is linked to education–if more educated people are socialized to think that civic behavior is socially desirable–then our estimates would be biased by social desirability bias.

Independent Variable

Our key independent variable is an individual's completed years of postsecondary education. Ideally, we would have measures of what individuals specifically knew and were able to do. Instead, we have measures of the amount of time that individuals spend in postsecondary education. NLSY97 provides month-by-month data on educational attainment, allowing us to report this information on a fractional basis (Moore et al., 2000).

[6]See question YSAQ-300V5 [S63178.00] at `https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121128/nlsy97r9saq1.html`

Control Variables

We use a set of control variables in every specification of the model. We include year and quarter of birth as individuals in this cohort faced very different labor markets when entering into the working world due to the Great Recession, which took place when respondents were between 24 and 28 (Bell & Blanchflower, 2011; Elsby et al., 2010). We include race because there may be unobserved yet critical aspects of individuals' experiences as members of different racial or ethnic groups that may lead them to higher or lower levels of civic behavior (Hurtado, 2007). We include sex because differences in civic behavior have been previously observed between men and women (Wilson, 2000). The indicator for residence in the South is included as Southern states typically have lower levels of civic participation (McDonald & Popkin, 2001). The indicator for residence in an Metropolitan Statistical Area is included to control for the size of the local labor market, which may have important impacts on foregone earnings associated with civic behaviors[7] (Card, 1995).

We include the Armed Services Vocational Aptitude Battery (ASVAB) score as a measure of academic ability, which may impact an individuals civic behaviors (Carneiro & Heckman, 2002). Finally, we include a state-level measure of voter turnout. We include this measure as state-level civic engagement may be driving a substantial amount of the civic behaviors of interest[8] (Costa & Kahn, 2003). We include state-level results because research shows that states are the primary locus of civic culture (Mondak & Canache, 2014).

Geographic Measures

Our identification strategy detailed below depends on geographic variation in postsecondary opportunity. Following the original work by Card (1995) on economic returns to higher education and the work by Dee (2004) on civic returns, we include two measures of the presence or absence of either a public two-year or a public four-year institution in the young person's county at age 17. The National Center for Education Statistics' Integrated Postsecondary Education Data System (IPEDS) provides the geolocation of individual campuses. We overlay these positions with county information in order to determine whether a respondent has at least one public four-year or one public two-year institution in the county (Bivand, Pebesma, & Gómez-Rubio, 2013).

Our remaining measures are not based on single institutions, but instead are measures of the density of postsecondary opportunity for a given individual at age 17. The first of these measures is the inverse log distance to all public two-year colleges in the state. Inverse log distance is given by:

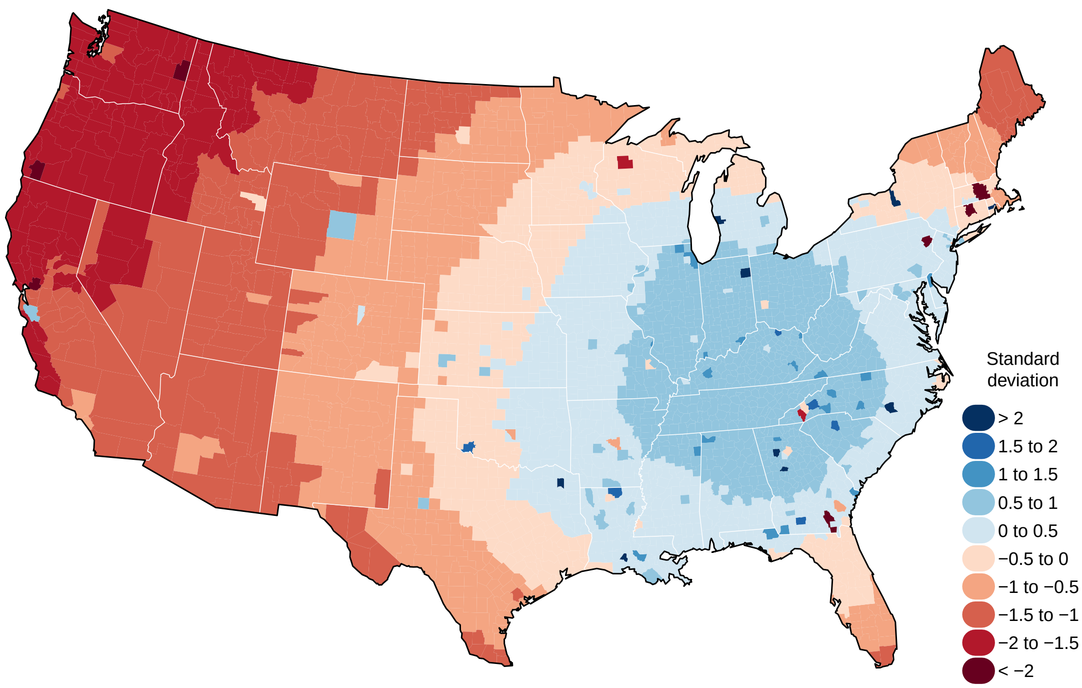
$$w_i = \sum_{k=1}^{K} log(d_{ik})^{-1}, \tag{4}$$

where w_i is the measure of inverse log distance for individual i and d_{ik} is the distance from the population-weighted geographic center of individual i's county to institution k. This measure has the property of showing very high values for individuals who live close to a large number of institutions and low values for individuals who live far from a few institutions. Distance d is measured using the Vincenty computational formula. Figure 4 shows the inverse log distance to public two-year

[7] We use MSA at age 17 as a measure of potential labor market. Unemployment during this time was subject to wide swings from year to year. The unemployment rate in a given area during the time of the Great Recession would actually be a fairly poor measure of long-term labor market potential, which is why we used an indicator for living within an MSA.

[8] We are grateful to an anonymous reviewer for suggesting the inclusion of this control.

Figure 4. Map of inverse log distance from county center to all two-year public colleges in the lower 48 states.



colleges for all counties in the lower 48 states.[9] We construct a similar measure that for each county limits the sample of public two-year colleges to those in the same state. Figure 5 shows this version of the measure. In addition to these measures of inverse log distance to public two-year colleges, we also include a measure of the inverse log distance to all institutions, public or private, two-year or four-year.

Our next two measures of geographic variation go one step beyond the measures of inverse log distance and use inverse log distance to weight the characteristics of colleges and universities for a given individual (Arbia, 2014; Bivand et al., 2013).

Our calculation for weighted average price WAP is as follows:

$$WAP_{iy} = \sum_{k=1}^{K} \frac{g_{ik} \cdot price_{ky}}{\sum_{k=1}^{K} g_{ik}}, \tag{5}$$

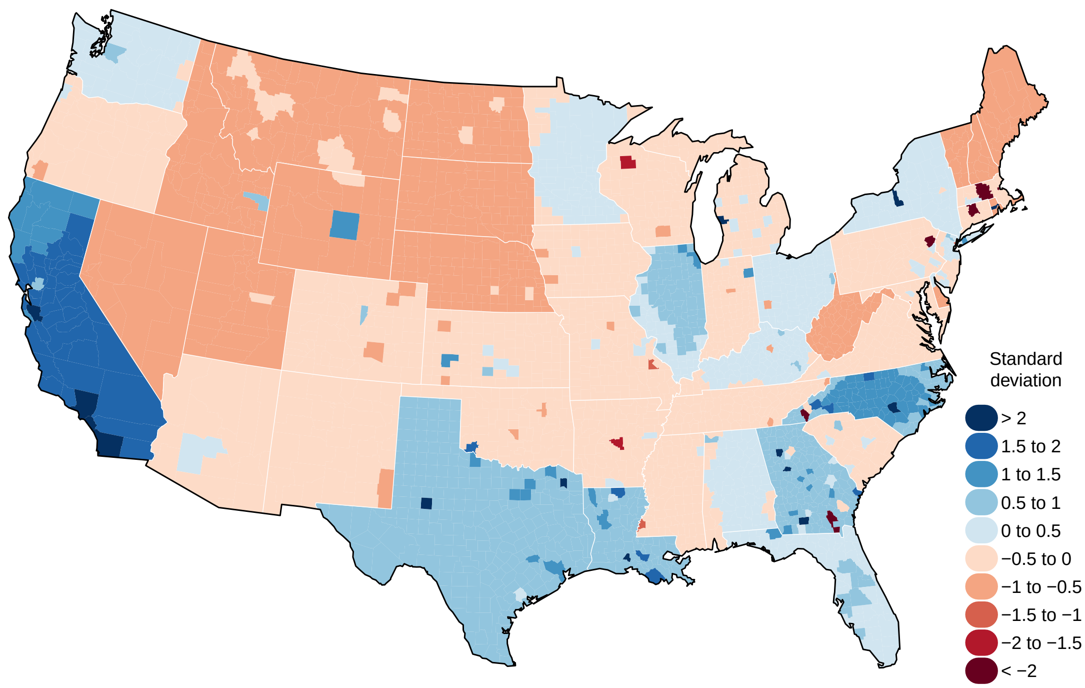
where $price_{ky}$ is price of institution k in year y and g is a weight for each cell. The weight g is defined as

$$g_{ik} = \left(\frac{d_{ik}}{\sum_{k=1}^{K} d_{ik}} \right)^{-r}. \tag{6}$$

In effect, this equation assigns an average price to every county in the country based on the prices of nearby colleges. These prices are weighted using g, with a decay function set by r. The decay

[9] We limit the analysis to the 48 states as Alaska and Hawaii create severe outlier problems in many of the measures.

Figure 5. Map of inverse log distance from county center to in-state two-year public colleges in the lower 48 states.



function sets the degree to which institutions that are further away contribute to the net price. We set the value of r at 2.[10] This measure calculates average tuition of nearby colleges for every individual in our sample, similar to how other studies have included average in-state tuition as either a control or an instrumental variable. Using average in-state tuition is equivalent to positing that the prices of all colleges in the state have the same impact on an individual, and so a simple average is appropriate. Our measure is different in that the price of nearby colleges impacts the average for an individual more than the price of colleges that are far away. For example, for a student living in San Diego, the prices of colleges in southern California will impact our measure more than the prices of colleges in northern California. Individuals who live close to a large number of low-priced colleges will have the lowest geographic weighted average on our measure, while individuals who live close to expensive colleges will have the highest values on our measure. This measure in effect takes into account the local market for higher education that students face (Gibbons & Vignoles, 2012; Long, 2004).

We use a similar procedure to calculate weighted average enrollment. The intuition behind this measure is that a young person who lives closer to institutions with higher levels of enrollment may perceive that there are more opportunities to attend higher education than a young person who lives further away from institutions with lower enrollment. Similar to the weighted price measure,

[10]This is a standard decay function in the spatial literature (Bivand et al., 2013). We tested several powers of r, and any power of r between 1.5 and 3 gives very similar results. Results are available upon request from the authors.

our enrollment measure will be highest for students who live close to a large number of high-enrollment institutions. Where our distance-weighted tuition measure posits that the price of nearby institutions is a factor in decision making, our measure of distance-weighted enrollment posits that living close to a large number of institutions that clearly have space for students can affect attainment for an individual. This effect would come about as students either observe directly that nearby colleges can enroll many people, or because they come into contact with more people who are already enrolled in higher education (Perna, 2006; Rowan-Kenyon, Bell, & Perna, 2008).

Our final measure of geographic opportunity utilizes the distance to the nearest in state public college, with a quadratic term.[11] This measure has been used quite often in studies that employ an instrumental variables approach in higher education—for example, similar measures were utilized by Bettinger and Long (2009) and Rouse (1995). The central difference when using this measure is that students need only live close to one college, rather than including the full set of institutions as we do in previous measures. In contrast to our other measures, this proposed instrument suggests that it is only the distance to the nearest college that matters for eventual enrollment, and having additional nearby colleges should have little or no effect.

Model Specification and Identification

In this section, we describe the model to be estimated and the particular estimator we use. We also describe and provide a rationale for our identification strategy. Last, we explain our understanding of how the results should be interpreted as "a weighted average of causal effects for instrument-specific compliant subpopulations" (Angrist et al., 1996; Angrist & Pischke, 2008).

The basic estimation model is described by

$$y_i = \alpha + \beta x_i + \mathbf{c}_i \gamma + \varepsilon_i, \tag{7}$$

where

y_i is the civic behavior in question: voting, volunteering or donating for individual i
α is an intercept
x_i is years of postsecondary education completed for individual i
β is the coefficient on years of education
$\mathbf{c}_i$ is a vector of control variables as described above
γ is a vector of coefficients for control variables
ε_i is an error term.

We expect that our estimate of interest, β, will be biased when estimating the above model using Ordinary Least Squares (OLS) regression. This will occur because there are many factors that might simultaneously drive a student to have more education and increase his or her civic behavior. To account for this possible endogeneity, we pursue an instrumental variables approach under which we first use measures of geographic variation to predict years of education, then use predicted years of education in place of observed years of education in equation (7). In using this two-stage approach, we assume that the only way in which geographic variation in college opportunity affects civic participation is through the mechanism of increased years of education. We test this assumption in all of our analyses.

[11] We are grateful to an anonymous reviewer for suggesting the inclusion of this instrument.

The first stage in our two-stage estimates takes the form:

$$x_i = \delta + \mathbf{z}_i\psi + \mathbf{c}_i\eta + \mu_i, \tag{8}$$

where δ is an intercept term, and $\mathbf{z}_i$ is a measure of geographic variation interacted with mother's education, and ψ is a vector of coefficients for the excluded instruments. Following Card (1995), we interact the instrument with mother's education in order to differentiate the impact of this variable on the individuals who are most likely on the margin of attendance. As in equation (7), $\mathbf{c}_i$ is a vector of control variables that now takes η as its vector of coefficients. μ_i is an error term for the first stage. The second stage is given by:

$$y_i = \alpha + \beta\hat{x}_i + \mathbf{c}_i\gamma + \varepsilon_i, \tag{9}$$

where $\hat{x}_i$ replaces x_i in equation (7).

All of our dependent variables are limited or ordinal variables. Voting is a binary variable, while volunteering is measured on a scale from 0 to 3 and donating is measured on a scale from 0 to 4. Because we are interested in estimating the conditional expectation function and not making predictions for any individual with regards to voting, volunteering or donating, we utilize the standard 2SLS estimator (Angrist & Pischke, 2008). We use robust standard errors to estimate variance in all results to account for possible heteroskedasticity in the residuals, particularly as several of our variables are limited or ordinal in nature (Angrist & Pischke, 2008).

For estimates of β in equation (9) to be unbiased,we must make two assumptions, which need to be tested. These two assumptions are exclusion and ignorability. The exclusion assumption maintains that the proposed instrument only impacts the outcome through the instruments impact on the endogenous variable. The ignorability assumption posits that assignment to treatment is as good as random conditional on the instruments.

Before outlining the tests to be used, we discuss whether our approach has face validity. We maintain that the only mechanism by which the density of postsecondary opportunity affects the civic outcomes of interest is through the mechanism of college attendance. How else might colleges and universities impact an individuals civic participation? We offer two possible alternative routes.

An individual who lives close to more colleges may gain civic awareness because of the activities of the colleges and their students (Dee, 2004). For such a scenario to affect voting in our case, effects would have to be long-lasting as many of our estimates come from individuals 10 years after we measure the geographic variation in higher education opportunity.

It may also be the case that individuals who choose to live in an area with a high density of colleges are more civically engaged and active from the start. Persons who move to "college" towns like Madison, WI, or Berkeley, CA—even if they have no intention of attending postsecondary institutions in the area—may be more likely to volunteer, vote or donate. To guard against this potential source of bias, several of our instruments are based on the location of two-year colleges. While community colleges can be important to their respective communities, there is no evidence of which we are aware that people move in order to live closer to more community colleges in the way that they move to be closer to good K-12 schools (Kane, Riegg, & Staiger, 2006). It could be, too that people move to be closer to certain types of colleges. In particular, if people move to be close to low-priced or large institutions, that would be problematic for our two distance-weighted measures of tuition and enrollment. Again, were not aware of any literature that supports this hypothesis.

To test these assumptions, we provide the results of three different tests. First, we provide the results of an endogeneity test conducted on the first-stage equation. This test examines the assumption that the presumed endogenous variable of schooling is in fact endogenous (Greene, 2008). We do not view this as conclusive evidence of endogeneity, as there are strong theoretical reasons to assume that this relationship is indeed endogenous. Second, we provide both the F statistic from the excluded instruments and the minimum eigenvalue for the excluded instruments, as described in Stock and Yogo (2002) and Stock, Wright, and Yogo (2002). This is a measure of the strength of the relationship between the excluded instruments and the endogenous regressor. If this relationship is weak, then estimates are likely to be biased. We compare our obtained minimum eigenvalue with the tables of minimum eigenvalues provided by Stock et al. (2002). Last, we included the results of the Sargan test, which examines the strength of the relationship between the excluded instruments and the error term in the second stage (Sargan, 1958). If this relationship is significant, then the exclusion restriction—the idea that geographic variation in college opportunity affects civic outcomes only through the mechanism of additional attainment—cannot be said to hold (Imbens, 2014).[12]

All of our results may be interpreted as a weighted average of the impact of postsecondary education for those subpopulations that were induced into treatment by virtue of the instrument (Kling, 2001). They cannot be generalized to include the entire population, but only the subset of the population induced into treatment by virtue of exposure to the instrumental variable, in this case the level of geographic opportunity for the individual (Angrist et al., 1996; Angrist & Pischke, 2008).

Results

In this section we report results from 2SLS estimation for each of the three dependent variables of voting, volunteering and donating to causes. For each dependent variable we first report the first-stage results and the results of specification tests. We then report second-stage estimates, provided that the specification tests have met expectations.[13]

First Stage Results

Table 1 contains estimates from the first stage for voting. The dependent variable for the first stage is number of years of postsecondary education completed. We report estimates for the years 2004, 2006, 2008 and 2010, each of which are tied to the Congressional or Presidential election for that year.

[12] We also test the as if random assumption on each of the covariates. The results of this test do not reveal any substantive patterns in the levels of the covariates as a function of the instruments. These results are available in the online appendix for this study.

[13] We used the R statistical programming language data analysis and graphics (R Core Team, 2014). We used the packages `dplyr`, `ggplot2`, `ggthemes`, `sandwich`, `lmtest` and `xtable` (Arnold, 2014; Dahl, 2014; Wickham, 2009; Wickham & Francois, 2015; Zeileis, 2004; Zeileis & Hothorn, 2002). Instrumental variables estimates and specification checks were performed in Stata 13 (StataCorp, 2013).

Table 1
2SLS first stage estimates for voting

	2004	2006	2008	2010
Public 4-Year in County	0.3835	0.4535	0.7733	0.4594
	(0.2045)	(0.2809)	(0.3649)	(0.3646)
. . . × Mother's education	-0.0247	-0.0261	-0.0398	-0.0256
	(0.0152)	(0.0209)	(0.0271)	(0.0273)
Endogeneity: F p-value	0.0008	0	0.0018	0.1298
Overidentification: χ^2 p-value	0.1004	0.9502	0.0307	0.6882
First Stage F	26.1672	25.5153	32.3051	24.1508
First Stage Min. Eigenvalue	30.3332	30.0611	35.8298	28.4471
N	4613	3785	3049	3605
Public 2-Year in County	0.6092	0.8213	0.4924	0.7886
	(0.2352)	(0.3248)	(0.407)	(0.4294)
. . . × Mother's education	-0.0415	-0.0546	-0.0168	-0.0485
	(0.0175)	(0.0243)	(0.0306)	(0.0322)
Endogeneity: F p-value	0.0006	0	0.0012	0.1437
Overidentification: χ^2 p-value	0.3704	0.1525	0.1058	0.3281
First Stage F	28.734	29.1168	31.4858	27.7937
First Stage Min. Eigenvalue	31.3808	31.1104	35.7112	29.2243
N	4613	3785	3049	3605
Inverse Log Distance to In-State Pub. 2yr	0.0869	0.1234	0.1245	0.1201
	(0.0122)	(0.0168)	(0.0216)	(0.0221)
. . . × Mother's education	-0.0056	-0.0083	-0.008	-0.0079
	(0.0009)	(0.0012)	(0.0016)	(0.0016)
Endogeneity: F p-value	0.006	0.0002	0.0006	0.0675
Overidentification: χ^2 p-value	0.0026	0.0003	0.2296	0.5256
First Stage F	43.7759	47.8667	42.9692	36.5497
First Stage Min. Eigenvalue	47.2247	47.1817	44.9335	38.1031
N	4613	3785	3049	3605
Distance-Weighted Price: In-State Pub. 2yr	-0.1399	-0.2028	-0.2924	-0.1841
	(0.0827)	(0.1151)	(0.1405)	(0.1461)
. . . × Mother's education	0.007	0.0106	0.0151	0.0041
	(0.0062)	(0.0086)	(0.0105)	(0.0109)
Endogeneity: F p-value	0.0002	0	0.001	0.0475
Overidentification: χ^2 p-value	0.6674	0.5525	0.904	0.7299
First Stage F	26.8723	26.4238	30.2343	27.7266
First Stage Min. Eigenvalue	31.2441	30.8498	35.2589	31.8235
N	4613	3785	3049	3605
Distance-Weighted Enrollment: In-State Pub. 2yr	0.0066	0.0283	0.0325	0.0188
	(0.016)	(0.0225)	(0.0277)	(0.0276)
. . . × Mother's education	0.0008	-0.0008	-0.0011	-0.0003
	(0.0012)	(0.0017)	(0.0021)	(0.0021)
Endogeneity: F p-value	0.0117	0.0003	0.0054	0.2022
Overidentification: χ^2 p-value	0.002	0.0854	0.0251	0.129
First Stage F	27.9825	26.6095	27.8945	24.235
First Stage Min. Eigenvalue	34.0866	31.8028	34.0342	28.5496
N	4613	3785	3049	3605

Continued on next page...

... table 1 continued

	2004	2006	2008	2010
Inverse Log Distance to All Colleges in Country	-0.0047	-0.0033	-0.0043	-0.0036
	(0.0021)	(0.003)	(0.0038)	(0.0039)
… × Mother's education	0.0005	0.0003	0.0004	0.0002
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Endogeneity: F p-value	0.0018	0	0.0029	0.1069
Overidentification: χ^2 p-value	0.0591	0.4305	0.0996	0.7624
First Stage F	27.5118	24.594	27.1049	24.1354
First Stage Min. Eigenvalue	32.8883	29.049	32.5833	27.9468
N	4613	3785	3049	3605
Distance to Nearest In-State Public	-0.0077	-0.001	-0.0139	0.0426
	(0.0208)	(0.0319)	(0.0372)	(0.0422)
… × Mother's education	(0.0001)	(-0.0011)	(0)	(-0.004)
	0.0017	0.0026	0.003	0.0034
Endogeneity: F p-value	0.0007	0.0023	0.0034	0.1838
Overidentification: χ^2 p-value	0.3001	0.003	0.2741	0.539
First Stage F	18.4236	16.4233	18.7592	15.9133
First Stage Min. Eigenvalue	18.6971	19.2656	20.7766	17.4124
N	4613	3785	3049	3605

Note. The critical minimum eigenevalue for one endogenous regressor and three excluded instruments at 5% bias is 13.91. All estimates are from results that include the following controls: binary variables for year and quarter of birth, race (four categories-black, Hispanic, multiracial and non-black, non-Hispanic), sex, an indicator for living in South at age 17, an indicator for living in an SMSA at age 17, a measure of state-level voter turnout at age 17 and ASVAB score.

In the first rows of panel 1 of Table 1 we show estimates for the impact of having at least one four-year institution in the county on the number of years of education completed. In 2004 the presence of at least one four-year college in the county was associated with an additional .38 years of attendance, although this result is not significant. For all of the years, neither the main effect nor the interaction effect for the presence of a four-year college in the county is significant. We display similar estimates for the other excluded instruments in each panel of Table 1. Our preferred estimates are based on the excluded variable of inverse log distance to in-state community colleges. The coefficient estimate for this variable for all years is significant, as is the interaction. Our results indicate that in 2010 the difference between being in the first quartile of this measure and the third quartile of this measure is associated with an additional 8 months of postsecondary education.

The interaction effect for mother's education and inverse log distance to nearest community college in 2010 is negative, with an estimate of -0.0079. Mother's education is measured in years of education completed, with an average for the sample of 12.5. The negative coefficient for the interaction indicates that respondents with higher levels of maternal education are less responsive to the number of nearby community colleges than are those in our sample who have lower levels of maternal education. More generally, this indicates that first generation students or others whose families have less education are more likely to be induced to attend higher education through nearby community colleges than their peers whose parents went to college. Our estimate above of 8 additional months of postsecondary education when comparing the 25th percentile to the 75th percentile on inverse log distance to all colleges declines to 5 months when estimated using only students whose parents have at least 4 years of college. Given that our instrumental variables estimates will be based on those who were induced into attendance by virtue of compliance with the instrument, it is very important to note that this group will likely be students with lower levels of maternal education.

Looking at at the specification tests for the excluded instrument of inverse log distance to in-state community colleges, we find that this variable has the desired properties. Endogeneity tests are highly statistically significant in 2004, 2006 and 2008. In Table 1, panel 3 we report results for the Sargan test. While these results are significant in 2004 and 2006, indicating that there remains a relationship between the inverse log distance to in-state community colleges and the error term in the second stage equation, they are not significant in later years. We take this as evidence that the primary means by which nearby colleges influence voting is through the mechanism of more years of school, though our results are robust only for the 2008 and 2010 elections.

We next turn to the F-test on the excluded instruments. In 2010, the F statistic for the excluded instruments is 36.5, easily exceeding the informal standard of 10. We also include the minimum eigenvalue as a measure of the strength of the relationship of the excluded instruments to the endogenous regressor. The minimum eigenvalue for the excluded instrument of inverse log distance to public two-year colleges in 2010 was 38.1. The value provided by Stock et al. (2002) as the minimum value for 5% bias is 13.91, indicating again that our instruments have a strong relationship with the endogenous regressors and that the second-stage results are unlikely to be biased by weak instruments.

None of the other proposed instruments have statistically significant main effects, leading us to conclude that the best-identified system of equations uses the inverse log distance to community colleges as the excluded instrument. Our proposed instruments show a much stronger relationship with years of postsecondary education than instruments that have been used previously. Included among the instruments with non-significant main effects is distance to the nearest in-state public college, which has been used in many other studies. The F statistic for this measure in 2010 was 15.9, in contrast to 36.5 for our preferred measure of inverse log distance to all community colleges. For this reason, we focus our comments in the remainder of the discussion on the results using the inverse log distance to in-state public two-year colleges as the excluded instrument.

Voting

Results from OLS and from the second-stage for the impact of years of college on voting are reported in Table 2. OLS estimates from this table range from . 059 in 2004 to . 04 in 2010. Our 2SLS estimates are typically higher, which follows a general pattern in the literature Dee (2004). In 2004, the impact of one year more postsecondary education on the probability of voting for those induced to attend by nearby community colleges was .136, though we cannot be certain of this estimate due the failure of the over-identification test in this year. This value is very close to that seen in 2008, however, which showed an impact of .122 with a t-value of 6.1. The estimate is also statistically significant in 2010, but the estimated impact is smaller at .077. Using the 2010 results, our estimates indicate that one additional year of college increases the probability of voting by 7.7 percentage points. This estimate is bounded by a 95% confidence interval that ranges from 3.8 to 11.6 percentage points. The lowest turnout election in the last twenty years was in 1996, when 49 percent of the population voted. The highest was in 2008, when 56.7 percent of the population voted (File, 2015; File & Crissey, 2012; United States Bureau of the Census, 2006). The percentage point difference between these two elections is 7.7 percentage points, similar to our estimate of the impact of an additional year of postsecondary education.

Looking at the results in Table 2 over the years in our analysis, we see a general pattern that exists regardless of the instruments used. In 2004, 2006, and 2008, the impact of an additional year of education on voting centers around 0.13 in the specifications for these year, with some estimates as high as 0.178 and others as low as 0.121. In 2010, the estimates are substantially lower, and are centered on 0.07. We do not have an explanation for this trend, although it is worth noting that 2010 saw a very low turnout for the population overall and for young people, while 2004 and 2008 were among the highest turnout elections for young people in recent years (File, 2015; File & Crissey, 2012; United States Bureau of the Census, 2006).

Table 2

2SLS second stage estimates for voting

	2004		2006		2008		2010	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Years of college	0.059		0.043		0.056		0.04	
	(0.005)		(0.004)		(0.004)		(0.004)	
Public 4yr in County		0.178		0.16		0.124		0.076
		(0.037)		(0.029)		(0.022)		(0.023)
Public 2yr in County		0.177		0.154		0.127		0.074
		(0.036)		(0.028)		(0.023)		(0.023)
ILD: In-State Pub. 2yr		0.136		0.124		0.122		0.077
		(0.028)		(0.022)		(0.02)		(0.02)
ILD: All Colleges		0.19		0.154		0.128		0.084
		(0.037)		(0.029)		(0.022)		(0.022)
DW Price: In-State Pub. 2yr		0.143		0.141		0.118		0.07
		(0.034)		(0.028)		(0.023)		(0.023)
DW Enrollment: In-State Pub. 2yr		0.164		0.161		0.124		0.078
		(0.035)		(0.03)		(0.024)		(0.023)
Distance to Nearest Public		0.178		0.125		0.121		0.071
		(0.037)		(0.027)		(0.023)		(0.023)

Note. ILD = Inverse Log Distance; *DW* = Distance-weighted. All estimates are from results that include the following controls: binary variables for year and quarter of birth, race (four categories–black, Hispanic, multiracial and non-black, non-Hispanic), sex, an indicator for living in South at age 17, an indicator for living in an SMSA at age 17, a measure of state-level voter turnout at age 17 and ASVAB score.

Table 2 also shows the variation in estimated impacts when using different excluded instruments. In effect, the table helps us to understand the different estimated impacts that can be observed among each group induced to attend higher education by different geographic characteristics. The table does not show a substantial amount of variation in estimates due to different choices of instruments. The lowest estimate for 2010 is 0.07, while the highest is 0.084.

From these results, we conclude first that there is an impact of additional postsecondary education on voting and that the impact is substantively important. An additional two years of postsecondary education results in an increase in the probability of voting of about 15 percent. Second, we conclude that while all of the instrumental variable estimates are larger than standard OLS estimates, the choice of instruments among the ones we have identified does not appear to affect the results to a large degree.

Volunteering

We next turn to volunteering. Results from the first-stage equation are available in Table 3. We do not report on the first stage for this dependent variable in detail as the results are similar across all three dependent variables.

Table 3

2SLS first stage estimates for volunteering

	2005	2007	2011	2013
Public 4-Year in County	0.4528	0.4024	0.306	0.2737
	(0.2384)	(0.2748)	(0.336)	(0.3655)
... × Mother's education	-0.0271	-0.0217	-0.0114	-0.0129
	(0.0177)	(0.0204)	(0.0251)	(0.0273)
Endogeneity: F p-value	0.1838	0.0287	0.0418	0.7183
Overidentification: χ^2 p-value	0.4437	0.5563	0.7614	0.0779
First Stage F	34.6783	39.3168	33.8771	22.5073
First Stage Min. Eigenvalue	41.4842	47.2617	37.7382	26.1923
N	4442	4605	4551	4416
Public 2-Year in County	0.7098	0.5238	0.3335	0.6978
	(0.2751)	(0.3161)	(0.3855)	(0.4209)
... × Mother's education	-0.0454	-0.0294	-0.0085	-0.0326
	(0.0205)	(0.0236)	(0.0289)	(0.0315)
Endogeneity: F p-value	0.146	0.0099	0.0662	0.8523
Overidentification: χ^2 p-value	0.564	0.242	0.0843	0.1499
First Stage F	37.5294	41.96	34.8321	26.6093
First Stage Min. Eigenvalue	42.6261	47.6785	38.8732	29.1665
N	4442	4605	4551	4416
Inverse Log Distance to In-State Pub. 2yr	0.1007	0.1047	0.113	0.1232
	(0.0143)	(0.0161)	(0.0203)	(0.0218)
... × Mother's education	-0.0064	-0.0067	-0.0071	-0.0081
	(0.001)	(0.0012)	(0.0015)	(0.0016)
Endogeneity: F p-value	0.1026	0.0121	0.0204	0.2015
Overidentification: χ^2 p-value	0.0598	0.8618	0.697	0.7288
First Stage F	53.0123	57.8674	46.6549	35.5994
First Stage Min. Eigenvalue	58.3358	61.6953	48.8788	37.3263
N	4442	4605	4551	4416
Distance-Weighted Price: In-State Pub. 2yr	-0.1169	-0.1749	-0.1559	-0.3047
	(0.0951)	(0.1081)	(0.1322)	(0.1439)
... × Mother's education	0.0051	0.0077	0.0039	0.0142
	(0.0071)	(0.0081)	(0.0099)	(0.0109)
Endogeneity: F p-value	0.0472	0.0117	0.023	0.1531
Overidentification: χ^2 p-value	0.1141	0.7242	0.6878	0.1651
First Stage F	34.7101	41.6317	35.476	27.2057
First Stage Min. Eigenvalue	41.3572	48.6115	39.7443	30.0537
N	4442	4605	4551	4416

Continued on next page...

... table 3 continued

	2005	2007	2011	2013
Distance-Weighted Enrollment: In-State Pub. 2yr	0.0121	0.0222	0.0264	0.0442
	(0.0188)	(0.0212)	(0.0256)	(0.0286)
... × Mother's education	0.0006	0	-0.0007	-0.0021
	(0.0014)	(0.0016)	(0.002)	(0.0022)
Endogeneity: F p-value	0.4934	0.0837	0.0415	0.7849
Overidentification: χ^2 p-value	0.0058	0.1155	0.3985	0.2162
First Stage F	36.103	42.0007	34.1486	24.8178
First Stage Min. Eigenvalue	44.6033	50.4869	38.3759	27.6523
N	4442	4605	4551	4416
Inverse Log Distance to All Colleges in Country	-0.0036	-0.0039	-0.0029	-0.0054
	(0.0025)	(0.0028)	(0.0035)	(0.0038)
... × Mother's education	0.0004	0.0004	0.0002	0.0004
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Endogeneity: F p-value	0.1501	0.0135	0.0197	0.3582
Overidentification: χ^2 p-value	0.1696	0.1529	0.0484	0.0214
First Stage F	34.6335	39.5829	32.0726	22.8231
First Stage Min. Eigenvalue	41.7565	46.9646	36.518	26.3556
N	4442	4605	4551	4416
Distance to Nearest In-State Public	-0.0129	0.0046	-0.0114	-0.015
	(0.0242)	(0.0278)	(0.0347)	(0.0373)
... × Mother's education	(0.0002)	(-0.0008)	(0.0003)	(0.0008)
	0.0019	0.0022	0.0027	0.003
Endogeneity: F p-value	0.407	0.037	0.0275	0.5791
Overidentification: χ^2 p-value	0.0883	0.5078	0.7594	0.7009
First Stage F	24.3682	28.0383	21.9337	16.3161
First Stage Min. Eigenvalue	26.129	29.3605	22.5307	16.2114
N	4442	4605	4551	4416

Note. The critical minimum eigenevalue for one endogenous regressor and three excluded instruments at 5% bias is 13.91. All estimates are from results that include the following controls: binary variables for year and quarter of birth, race (four categories-black, Hispanic, multiracial and non-black, non-Hispanic), sex, an indicator for living in South at age 17, an indicator for living in an SMSA at age 17 and ASVAB score.

Table 4 has the full results from the second stage for volunteering. Using our preferred instrument of inverse log distance to public in-state two-year colleges, the impact of one additional year of college on the volunteering scale is estimated as 0.132 in 2007, with a confidence interval bounded by [0.07, 0.19]. In 2011, the estimate for the same coefficient was 0.099, with a confidence interval bounded by [0.4, 0.16]. For the latest year, the value is the smallest, at 0.082 with a confidence interval bounded by [0.01, 0.15]. In 2013 the effect size for two additional years of education on volunteering is 0.13, while in 2011 the effect size is 0.22.

What does this mean about actual volunteering behavior? Our scale for volunteering behavior goes from 0 (did not volunteer) to 3 (volunteered 12 or more times). The modal response is volunteering between 1 and 4 times, translating to an average of 1.5 on our scale. This means that it would take an additional 6 years of postsecondary education to induce a change in volunteering from 1-4 times per year to volunteering between 5 and 11 times per year. In substantive terms, this effect is fairly small, enough to induce only a slight shift in volunteering behavior among students induced into postsecondary education by the instruments.

As with voting, we do not find that our estimates are sensitive to the choice of instruments.

Table 4
2SLS second stage estimates for volunteering

	2005		2007		2011		2013	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Years of college	0.052		0.052		0.028		0.035	
	(0.008)		(0.006)		(0.006)		(0.006)	
Public 4yr in County		0.115		0.131		0.102		0.051
		(0.048)		(0.036)		(0.036)		(0.044)
Public 2yr in County		0.12		0.144		0.094		0.043
		(0.047)		(0.036)		(0.036)		(0.041)
ILD: In-State Pub. 2yr		0.118		0.132		0.099		0.082
		(0.041)		(0.032)		(0.031)		(0.037)
ILD: All Colleges		0.146		0.142		0.108		0.091
		(0.049)		(0.036)		(0.036)		(0.04)
DW Price: In-State Pub. 2yr		0.084		0.114		0.101		0.047
		(0.046)		(0.035)		(0.036)		(0.043)
DW Enrollment: In-State Pub. 2yr		0.121		0.142		0.114		0.074
		(0.049)		(0.037)		(0.037)		(0.043)
Distance to Nearest Public		0.091		0.126		0.108		0.059
		(0.047)		(0.035)		(0.036)		(0.043)

Note. ILD = Inverse Log Distance; *DW* = Distance-weighted. All estimates are from results that include the following controls: binary variables for year and quarter of birth, race (four categories–black, Hispanic, multiracial and non-black, non-Hispanic), sex, an indicator for living in South at age 17, an indicator for living in an SMSA at age 17 and ASVAB score.

While we prefer the inverse log distance to community colleges, it does not appear that the estimates in any year are different depending on which set of instrumental variables are applied. This indicates that the estimates are similar across different groups of individuals induced into additional years of college.

Also, similar to the patterns we observed with voting, the impact of an additional year of postsecondary education on volunteering was smallest in the latest year of our sample. In the years 2005-2011, our estimates vary from 0.09 to 0.146. In 2013, no estimate exceeds 0.08. While the impact is still statistically significant in 2013, the amount by which postsecondary education increases volunteering is the smallest in all our estimates.

Donations

We last examine results for donations. As with voting and volunteering, we find that the inverse log distance to in-state community colleges is the only statistically significant predictor of years of college completed among any of the other proposed instruments, as reported in Table 5. Again, the measure of inverse log distance to two-year colleges generally passes all of the specification tests, though non-significance of the F-test for endogeneity signifies that our estimates may not be necessary in 2005 or 2007.

Table 5
2SLS first stage estimates for donations to non-profits

	2005	2007	2011	2013
Public 4-Year in County	0.427	0.4841	0.3549	0.2839
	(0.2389)	(0.2749)	(0.3319)	(0.3616)
. . . × Mother's education	-0.0252	-0.0273	-0.0151	-0.014
	(0.0177)	(0.0205)	(0.0248)	(0.027)
Endogeneity: F p-value	0.9607	0.5026	0.0017	0.0208
Overidentification: χ^2 p-value	0.4449	0.3005	0.0345	0.6847
First Stage F	35.4969	42.6035	30.8459	24.1192
First Stage Min. Eigenvalue	42.5967	47.8508	36.4316	28.0638
N	4439	4586	4584	4468
Public 2-Year in County	0.6541	0.4655	0.5498	0.7537
	(0.2754)	(0.3147)	(0.3813)	(0.4163)
. . . × Mother's education	-0.0413	-0.0245	-0.0251	-0.0367
	(0.0205)	(0.0235)	(0.0286)	(0.0312)
Endogeneity: F p-value	0.7099	0.5707	0.0011	0.0302
Overidentification: χ^2 p-value	0.7012	0.2956	0.1772	0.1745
First Stage F	38.1722	42.7794	33.3478	29.1997
First Stage Min. Eigenvalue	43.5389	47.8651	37.7506	31.3214
N	4439	4586	4584	4468
Inverse Log Distance to In-State Pub. 2yr	0.0947	0.1004	0.1269	0.1196
	(0.0144)	(0.0162)	(0.0198)	(0.0214)
. . . × Mother's education	-0.006	-0.0065	-0.0082	-0.0077
	(0.001)	(0.0012)	(0.0015)	(0.0016)
Endogeneity: F p-value	0.9183	0.2751	0.0004	0.0225
Overidentification: χ^2 p-value	0.7166	0.4993	0.4987	0.4407
First Stage F	52.3726	56.2578	48.0688	37.9839
First Stage Min. Eigenvalue	57.9066	60.4492	50.8597	39.2991
N	4439	4586	4584	4468
Distance-Weighted Price: In-State Pub. 2yr	-0.0875	-0.1715	-0.2143	-0.2778
	(0.0957)	(0.1083)	(0.1307)	(0.1428)
. . . × Mother's education	0.0026	0.0074	0.0087	0.0121
	(0.0071)	(0.0081)	(0.0098)	(0.0108)
Endogeneity: F p-value	0.9064	0.6586	0.0003	0.0515
Overidentification: χ^2 p-value	0.6583	0.8007	0.9553	0.2536
First Stage F	35.7876	42.3596	33.4511	29.0476
First Stage Min. Eigenvalue	42.6845	48.7885	38.4103	31.8398
N	4439	4586	4584	4468
Distance-Weighted Enrollment: In-State Pub. 2yr	0.0104	0.0225	0.0403	0.0333
	(0.0188)	(0.0211)	(0.0253)	(0.0282)
. . . × Mother's education	0.0007	0	-0.0017	-0.0013
	(0.0014)	(0.0016)	(0.0019)	(0.0021)
Endogeneity: F p-value	0.7016	0.4351	0.0003	0.0435
Overidentification: χ^2 p-value	0.5662	0.3661	0.7672	0.049
First Stage F	37.2375	43.261	32.6781	25.8939
First Stage Min. Eigenvalue	46.0848	50.8076	37.6416	29.1558
N	4439	4586	4584	4468

Continued on next page...

... table 5 continued

	2005	2007	2011	2013
Inverse Log Distance to All Colleges in Country	-0.0038	-0.0045	-0.0038	-0.0043
	(0.0025)	(0.0028)	(0.0035)	(0.0037)
... × Mother's education	0.0004	0.0004	0.0003	0.0003
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Endogeneity: F p-value	0.9537	0.7485	0.0003	0.0136
Overidentification: χ^2 p-value	0.6209	0.4404	0.5686	0.6941
First Stage F	35.5719	40.849	30.1183	24.3325
First Stage Min. Eigenvalue	42.8811	47.3229	35.2969	27.9872
N	4439	4586	4584	4468
Distance to Nearest In-State Public	-0.0056	-0.0114	-0.0017	-0.0234
	(0.0242)	(0.0283)	(0.034)	(0.0371)
... × Mother's education	(-0.0002)	(0.0004)	(-0.0004)	(0.0015)
	0.0019	0.0022	0.0027	0.003
Endogeneity: F p-value	0.8591	0.7876	0.0009	0.0766
Overidentification: χ^2 p-value	0.0753	0.0314	0.136	0.1027
First Stage F	25.2303	27.9128	21.2373	19.0708
First Stage Min. Eigenvalue	26.7619	29.4625	21.8027	17.654
N	4439	4586	4584	4468

Note. The critical minimum eigenevalue for one endogenous regressor and three excluded instruments at 5% bias is 13.91. All estimates are from results that include the following controls: binary variables for year and quarter of birth, race (four categories-black, Hispanic, multiracial and non-black, non-Hispanic), sex, an indicator for living in South at age 17, an indicator for living in an SMSA at age 17 and ASVAB score.

Table 6

2SLS second stage estimates for donating to non-profits

	2005		2007		2011		2013	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Years of college	0.03		0.034		0.028		0.037	
	(0.006)		(0.006)		(0.005)		(0.005)	
Public 4yr in County		0.032		0.009		0.14		0.129
		(0.037)		(0.036)		(0.037)		(0.041)
Public 2yr in County		0.016		0.013		0.141		0.118
		(0.037)		(0.036)		(0.036)		(0.038)
ILD: In-State Pub. 2yr		0.027		-0.001		0.137		0.113
		(0.032)		(0.032)		(0.032)		(0.034)
ILD: All Colleges		0.026		0.017		0.153		0.112
		(0.037)		(0.036)		(0.037)		(0.039)
DW Price: In-State Pub. 2yr		0.016		0.006		0.152		0.116
		(0.036)		(0.035)		(0.036)		(0.04)
DW Enrollment: In-State Pub. 2yr		0.028		0.021		0.158		0.137
		(0.038)		(0.037)		(0.038)		(0.042)
Distance to Nearest Public		0.036		0.024		0.143		0.106
		(0.036)		(0.035)		(0.036)		(0.039)

Note. ILD = Inverse Log Distance; *DW* = Distance-weighted. All estimates are from results that include the following controls: binary variables for year and quarter of birth, race (four categories–black, Hispanic, multiracial and non-black, non-Hispanic), sex, an indicator for living in South at age 17, an indicator for living in an SMSA at age 17 and ASVAB score.

Table 6 contains estimates from each of the different proposed instrumental variables approaches. Different from voting or volunteering, there are no observable effects of additional years of education on donations until 2011. The explanation for this is fairly simpleit is likely that the respondents lacked sufficient income to make such donations until 2011, at which time respondents were 27 to 31 years old. In 2011, our preferred estimator returns a coefficient of 0.137, with a 95% confidence interval bounded by [0.07, 0.2]. The impact is equally strong in 2013 at 0.113 with a confidence interval bounded by [0.05, 0.18]. Similar to voting and volunteering, these estimates do not appear to be sensitive to the choice of excluded instrument. In 2011, the effect size using our preferred estimate is 0.13, slightly larger than the effect sizes for volunteering.

Most people in our sample did not donate any money to charity in any year. In 2013, only 17 percent of our sample gave any amount at all to charity. On a scale of giving from 0 (never gave any money to charity) to 4 (gave more than $1,000), the average response is 0.42 in 2013. Using our preferred estimates it would take an additional 5 years of postsecondary education to induce an individual to go from the average response of no giving to donating at between one and one hundred dollars to charity.

Different from voting and volunteering, our estimates for charitable giving tended to be larger in 2011 and 2013. Some of this is likely due to our respondents' incomes increasing, raising their ability to donate some amount and increasing our ability to detect an effect of postsecondary education on giving. The estimates in 2011 are consistently higher than the estimates in 2013.

Our results overall indicate that the excluded instruments of inverse log distance to in-state community colleges had the best properties in terms of identifying the impact of postsecondary education on civic behaviors. Individuals with more years of postsecondary completed are more likely to vote, volunteer and donate money.

Conclusion

Our findings demonstrate that additional years of college resulted in increased civic behaviors among young people aged 29 to 33 by 2013. For every year of postsecondary education, young people were about 7.7 percent more likely to vote in the 2010 election, and were additionally more active in volunteering and giving charitable contributions to various causes. The impacts for voting are substantively important when considering those individuals who had completed multiple years of postsecondary education, while the impacts for volunteering and charitable giving are more modest.

One of the primary contributions of this study is to assess the utility of alternative measure of geographic opportunity as instruments for postsecondary attainment. Previous studies have used relatively simple measures such as the presence or absence of a college, or the distance to the nearest college. In contrast, we proposed a set of instruments that make use of the distance to all colleges in the country, or some relevant subset. Among the instruments we propose, we find that the inverse log distance to in-state public two year colleges has the strongest and most consistent relationship with years of postsecondary education completed. This suggests both that this could be a useful instrument for identifying other outcomes of interests related to postsecondary attendance and that two-year colleges play a large role in providing local opportunity.

Our results are similar in many ways to previous results from the literature. Our preferred estimates suggest that an additional year of postsecondary education increases the probability of voting by 7.7 percentage points. Dee (2004) finds that for each additional year of education, the probability of voting increases by 6.8 percentage points, very similar to our estimate. Milligan, Moretti, and Oreopoulos (2004) find that completing high school increased the probability of voting by about 28 percent in comparison with dropping out of high school. Their treatment group is quite broadly defined and would include individuals with any number of years of postsecondary education. In general, our results are consistent with the literature that shows increased civic engagement as a function of increased postsecondary attainment (McMahon, 2010).

One of the primary contributions that we provide is to show that the relationship between more college education and increased civic outcomes continued to hold even during the tumultuous period of the Great Recession. The results for voting are substantively important, showing that a four-year college graduate should have a nearly 30 percent higher probability of voting than a high school graduate. The results for giving and volunteering are not as impressive, indicating that these behaviors increase with more postsecondary education, but not by much.

Looking at the results over time, we find for both voting and volunteering that the estimated impact of an additional year of postsecondary education was lowest for both of these behaviors in the last year of our data. This is intriguing, and may suggest that the civic impacts of postsecondary education may fade over time (Sax, 2004). It could be that as the young person's time in college recedes, so does behavior. More evidence would be needed to support this conjecture. For charitable giving, the picture is more complicated. The impact of education on giving is strongest in the latter years in our sample. Again, we would need more in-depth analysis to account for these trends.

Our results help to place some limits on the discussion of externalities from higher education. Some observers of higher education have contended that the externalities from higher education are essentially zero—that colleges and universities do not create behaviors in individuals that are of much value to the rest of the society (Friedman, 1955). Other observers have contended that the externalities are extremely large, exceeding even the private market returns to individuals (McMahon, 2010).

Our results speak to a middle ground. We do not investigate externalities as a whole. Rather, we look at the impact of postsecondary education on three behaviors that are widely viewed as externalities from higher education: voting, volunteering and charitable giving. Using this small subset of civic behaviors, there are indeed civic benefits to higher education, which appear to accrue largely to individuals on the margins of attendance. These benefits are modest but real. They should be placed in the context of the economic returns to higher education, which are substantial. Our results suggest that there are only limited arguments for funding higher education for those who would otherwise go to college on the basis of externalities to be gained. Research has shown that low-income young people are much more likely to be on the margins of attendance, and that additional years of higher education have a larger effect on their eventual earnings (Card, 2001). Similarly, our work shows that students on the margins of attendance show the biggest impacts of college attendance in terms of civic outcomes. These impacts existed even at the height of the Great Recession, when many were questioning the public value of higher education. We take this as evidence that even in the non-market realm, the focus of policy and funding should be on those low-income and underrepresented young people who would not otherwise attend postsecondary education.

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