Higher Education and Cultural Liberalism: Regression Discontinuity Evidence from Romania

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Some studies suggest that university attendance exerts a liberalizing effect on attitudes toward cultural issues such as sexuality and sexual identity, prostitution, drug addiction, abortion, capital punishment, divorce, parenting, gender, race, religion, science, and technology. Other studies find only a weak effect or no effect. Divergent findings may stem from research designs that rest largely on observational data when assignment to treatment is nonrandom and there are many threats to inference. In this study, we enlist a unique research setting in Romania that allows for a fuzzy regression discontinuity design separating those qualified to matriculate to university from those unqualified to do so. We find that university attendance contributes to a more liberal outlook as measured by our composite index, corroborating the main (preregistered) hypothesis. Evidence for subsidiary hypotheses is mixed.

Opinion research suggests that mass publics have adopted more liberal attitudes toward many cultural issues including sexuality and sexual identity, prostitution, drug addiction, abortion, capital punishment, divorce, parenting, gender, race, religion, science, and technology. This monumental shift in attitudes may be observed over a long period of time for countries with an extended history of survey research such as the United States (Page and Shapiro 2010). A noneconomic issue dimension—variously labeled as liberal versus authoritarian (Kitschelt 1994), new politics versus old politics (Franklin 1992), integration versus demarcation (Kriesi et al. 2006), green/alternative/libertarian versus traditional/authoritarian/nationalist (Hooghe, Marks, and Wilson 2002), social left–right (Coman 2017), or postmaterialist (Inglehart 2018)—also seems to be increasingly important in defining political cleavages in advanced industrial countries (Stubager 2013).

Although each of the foregoing topics has its own history and its own specific causes, it seems likely that there are also some common causes. Note that the foregoing topics are interconnected: one's views of religion, family, and sex are probably not independent, for example.

To explain this liberalizing trend, one might point to improvements in income and health (Inglehart 2018), urbanization (Fischer 1975), the demographic transition (Dyson 2001), the rise of mass media (Shah 2011), and other factors too numerous to review. In this study, we focus on the possible role of higher education.

Over time, more and more people around the world are receiving postsecondary education (Schofer and Meyer 2005). This experience is widely believed to exert a liberalizing effect on social-cultural attitudes (for wide-ranging reviews, see Emler and Frazer 1999; Hastie 2007). Those on the left are inclined to view the transformation as enlightenment (Orrill 1997), while some on the right view it as indoctrination (Maranto, Hess, and Redding 2009). But on both sides of the aisle there seems to be general agreement on the liberalizing influence of a "liberal arts" education (Roth 2014).

However, evidence for the proposition is not well established, resting largely on observational data when assignment

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to treatment is nonrandom. As it happens, there are many threats to inference, which we review at some length below. It should also be noted that most of the literature on this question is focused on one country whose relevant features are fairly exceptional by international standards. The tertiary education sector in the United States is highly fragmented and ideologically diverse and includes a large number of small private universities, many of them denominational (Bok 2013). Additionally, politics in the United States has become highly polarized over the past several decades (Sides and Hopkins 2015). These distinctive characteristics raise questions about generalizability.

In this study, we enlist a unique research setting in Romania that allows for a fuzzy regression discontinuity (RD) design separating those qualified to matriculate to university from those unqualified to do so. The advantages of this design may be briefly noted.

First, we are able to compare those who receive a university education with those who (in most cases) receive no university education at all. This is because private and public universities in Romania apply the same criteria—passage of a nationwide baccalaureate exam—and there are few alternate paths to higher education. Our study therefore features a strong treatment. We are not measuring degrees or types of higher education but rather whether university education per se affects social attitudes.

Second, because the sample is large and the measurement of exam scores fine grained, it is plausible to regard the outcome as continuous at the cutoff. And, because the number of exam-takers is enormous (roughly 150,000 each year), we are able to focus on a very narrow bandwidth. The assumption of as-if random assignment is therefore plausible, and we are able to ex ante concentrate our statistical power on estimating the relationship between the score variable and the dependent variable on either side of the threshold.

The current study follows a preregistered design (see app. H), with a few minor deviations noted below.[1] We find that university attendance in Romania contributes to a culturally liberal outlook as measured by a composite index, corroborating our main hypothesis. Evidence for subsidiary hypotheses is mixed.

The first section of the article lays out key concepts and arguments. The next section reviews extant work and introduces our methodology. We next report our main findings and then report results from tests of several subsidiary hypotheses. The final section summarizes our findings and addresses questions of generalizability.

CONCEPTS AND ARGUMENTS

For current purposes, a "university" refers to any postsecondary institution that offers instruction in the professions and the liberal arts. It does not include vocational schools, art schools, or theological seminaries. (The terms "college," "university," and "tertiary" or "higher" education will be used synonymously.)

So defined, universities are the principal institutions governing young adulthood among the world's middle classes. Coming of age as an affluent citizen of the twenty-first century usually involves attendance at a university, and those who attend often cite formative experiences associated with particular courses, friendship circles, or college organizations. For many, it is a transformative experience (Mayhew et al. 2016).

In this light, one might expect university attendance to engender a change in attitudes toward society—that is, topics such as sexuality and sexual identity, prostitution, drug addiction, abortion, crime, capital punishment, divorce, parenting, gender, race, immigration, religion, science, and technology. A "liberal" position on these topics, we stipulate, is secular, rational, open to new ideas, nontraditional, tolerant of differences and disagreements, ameliorative (rather than punitive), egalitarian, and inclusive. We refer to this as *cultural* liberalism, which may be contrasted with *economic* liberalism (attitudes toward the economy, market regulation, redistribution, and the welfare state).

Let us now consider some of the possible pathways between university attendance and cultural attitudes. Our discussion will be brief since there is a sizable literature on the subject, to which interested readers may refer.

Insofar as professors are more liberal than the general public (van de Werfhorst 2020), they may construct lectures and course materials that reflect their social views—an *indoctrination* effect (Gross 2013 presents arguments for and against). Insofar as activist students are considerably more liberal than the general public, this may pressure students to adopt more liberal views—a *peer* effect (Klofstad 2007). Insofar as universities offer free space for reflection, plenty of fodder for gathering information on new subjects, and opportunities for cognitive development, this intellectual dynamic may lead to more liberal conclusions—an *enlightenment* effect (Nie, Junn, and Stehlik-Barry 1996). Insofar as universities are located in urban areas, and insofar as these areas are more liberal than rural areas, students from rural areas may be exposed to liberal views—a *geographic* effect (Sennett 2002). Insofar as university education enhances an

1. The preregistered proposal and preanalysis plan can be found at the Evidence in Governance and Politics Registry, now housed at the Open Society Foundation (https://osf.io/bx96g). Note that the study was originally called "Education and Traditional Values."

individual's capabilities, earning power, and status in society, this may induce a feeling of competence and security, mitigating feelings of anxiety and threat—a *psychological* effect (McClosky and Brill 1983).

Naturally, the foregoing mechanisms may work in tandem and may reinforce each other. It is also conceivable that there might be a *backlash* effect. Contact with liberal views may alienate some students, pushing them in the opposite direction. However, we view this as an occasional occurrence that could attenuate the general effect but is unlikely to overwhelm it. Thus, we hypothesize, along with most observers, that universities have a liberalizing effect on those who attend. Along with this main hypothesis we offer several ancillary hypotheses, reported in appendix I.

Before concluding, it should be pointed out that the treatment—university attendance—is better defined than the control condition. We have a good idea what it means to attend university. Not attending university, by contrast, is a vast residual category that is difficult to grasp. Any attempt to interpret university effects must wrestle with this fundamental ambiguity.

RESEARCH DESIGN

Prior research on the liberalizing effect of higher education rests primarily on observational data, which is prone to a number of problems of causal identification. By way of entrée, we review this literature in some detail.[2]

The most common methodology enlists surveys of the general public, which allows for a comparison of attitudes among those with and without a college education, relying on regression adjustments to reach causal inference (e.g., Hainmueller and Hiscox 2007; Stubager 2008; van de Werfhorst and de Graaf 2004; Weakliem 2002). The resulting model must be correctly specified, including all pretreatment background conditions that are correlated with the treatment and excluding all posttreatment background factors. Obtaining a valid causal estimate with survey samples thus involves a great many assumptions that are virtually impossible to test. As usual, threats to inference arise primarily from selection effects. These may be based on family, peer groups, social class, urbanization, intelligence, or core personality attributes—all of which may affect social attitudes and thus constitute prima facie confounders. Some of these factors are fairly easy to measure and condition in a regression framework; others are ineffable.

Selection effects are especially invidious in this instance since the decision to attend university may be influenced by the outcome of theoretical interest, introducing circularity between cause and effect. If universities are bastions of cultural liberalism, those who share this worldview may be more likely to obtain a university education than those holding more traditional views. Knowledge of the destination may affect behavior all along the educational journey. Social conservatives may be less motivated to achieve good grades in secondary school and to prepare for national exams that regulate admittance to university. When it comes time to apply they may be loath to postpone gainful employment and take on debt in order to attend an institution that challenges their deeply held views of the world, where they may be subject to social stigma.

To control these confounders, some studies enlist longitudinal comparisons in which a cohort of students is surveyed iteratively over time as they pass through the educational system. Some of these studies find a liberalizing effect (e.g., Surridge 2016) and others report null findings (e.g., Lancee and Sarrasin 2015). Since the causal counterfactual (a hypothetical reassignment of those in the treatment group to the control group) cannot be estimated, causal effects estimated for the treated group cannot be generalized to the untreated group except under very strong assumptions.

Another research design focuses on twins, some of whom attend college and others do not, thus neutralizing a whole range of potential confounders. These studies find that college attendance has only a weak association with liberal social attitudes (e.g., Campbell and Horowitz 2016). One may surmise that this association would disappear entirely if one were able to measure and condition core personality traits. After all, if one twin attends college while another does not, there is presumably an explanation for this divergence, and this factor could also lead the twins to adopt different social outlooks. Again, the confounder is obdurate. Of course, the bias might also run in the other direction. Since twins are likely to be in close contact with one another, they are not truly independent research subjects and are likely to influence each other's social views. As such, treatment and control conditions are contaminated. From this perspective, the true causal effect may be underestimated.

After surveying this field of work, one may be skeptical about whether universities change political attitudes or merely reflect them. In the following sections, we introduce our approach to the question, which features an RD design situated in Romania.

2. The following literature review focuses on studies in which tertiary education is the causal factor and cultural liberalism the outcome. Outside this rubric, i.e., with respect to primary or secondary education (Sondheimer and Green 2010) or with respect to other outcomes (e.g., Hangartner et al. 2020), some attempts have been made to enlist experimental or quasi-experimental designs.

The setting

In most countries, tertiary education is a decentralized good, allocated in a variety of unstandardized ways. There are many ways to get into college and thus many characteristics that might distinguish university students (or former students) from those who do not matriculate. This makes it difficult to estimate the causal effect of a college education, as noted. Even where random or as-if random treatments are discovered, the subpopulations exposed to these treatments are often small and idiosyncratic and therefore difficult to generalize.

Romania is a middle-income country in Central Europe with a legacy of communist rule and a fairly educated populace (see app. F). One of the legacies of the Soviet era is an education system run largely by the state, access to which rests on a nationwide high school exit exam. The baccalaureate exam (hereafter, the bac) is the final assessment that high school students in Romania take at graduation. The results of this exam determine eligibility for college education as well as chances of admission to a student's university and major of choice.

Two exams are administered each year, in June–July and August–September, respectively. The second session is only for students who did not pass or did not qualify to sit for the bac in the first session. High school graduates are entitled to take the bac free of charge twice. If the student does not pass either of the two attempts she can continue to sit for the exam but must pay a fee. All high school students in good standing are automatically registered for the first session of the bac, and students have nothing to lose by taking the bac, even if they fail. Accordingly, attendance at this annual test-taking ritual is nearly universal. (Naturally, this does not include students who attrit before completing high school; however, dropout rates in Romania are low.)

Most bac takers sit for three different subjects, which determine the final average and the student's qualifications to progress to university. Each subject is graded from 1 to 10. A grade of at least 5.0 on each of the three subjects and an overall average of at least 6.0 is required to pass. Graduates of high schools where the language of instruction is in an ethnic minority language (Hungarian or German) must pass an additional exam in their mother language and literature. We exclude these students (of whom 1,133 fulfill our other criteria), restricting our sample to those whose schooling is conducted in Romanian.

To determine students' scores, we focus on their average score across three subjects, limiting our sample to those who achieve 5.0 (the minimum score) on all three and focusing exclusively on their first attempt. Note that if, among those who fail, only the more motivated students retake the exam, this would be problematic for the RD design. For similar reasons, we also ignore any score changes after appeals, using the preappeal average, since those who appeal their grades may differ from those who do not.

Ex ante evaluations of threats to as-if random assignment

Further details about the administration of the exam are contained in appendix B. There, we explain why widespread cheating is unlikely after 2014, when reforms in the organization of the bac and a nationwide anticorruption body were undertaken. Here, we enlist available data to assess the possible manipulation of exam scores around the cutoff in the two periods before and after the reforms.

Using the Romanian government's website, we obtained publicly available bac scores for all students who took the exam between 2004 and 2019. If sorting is occurring around the cutoff, it seems likely that it is primarily in the direction of passing. In this scenario, we ought to observe a break in the density of observed exam scores at 6.0. By contrast, if the distribution of exam scores is smooth around the threshold for passage, we have less reason to worry about sorting.

Figure 1 presents histograms of students' overall bac scores, separated into the periods before and after anticheating measures were fully in place (2004–14 and 2015–19, respectively). Informally, we should focus on whether the difference between the histogram bins immediately above and below the threshold is notably larger in magnitude than that between other adjacent bins across the distribution. In the earlier time period there is a noticeable jump at the threshold. Although not definitive, this suggests problematic sorting around the cutoff and is consistent with descriptions of widespread cheating before 2015. By contrast, the histogram in figure 1*B* shows a difference between the height of the two bins around the threshold that is fairly typical of those throughout the rest of the histogram, suggesting that sorting across the threshold was minimal between 2015 and 2019.

The histograms presented in figure 1 offer an informal diagnostic with respect to possible sorting. As a complement, we conduct manipulation tests following Cattaneo, Jansson, and Ma (2018). These tests estimate the density of the score variable in a neighborhood below and, separately, above the threshold, providing a formal test of the hypothesis that the densities immediately to the right and left of the threshold are different. Figure 2 shows the results of this analysis. Figure 2*A* includes tests for 2004–14, which we discard given the concerns about cheating as well as a mix of different exam policies and grading rules. Figure 2*B* shows tests for 2015–19, which we use for our analyses. As is evident from these results, the data from the earlier period show evidence that is

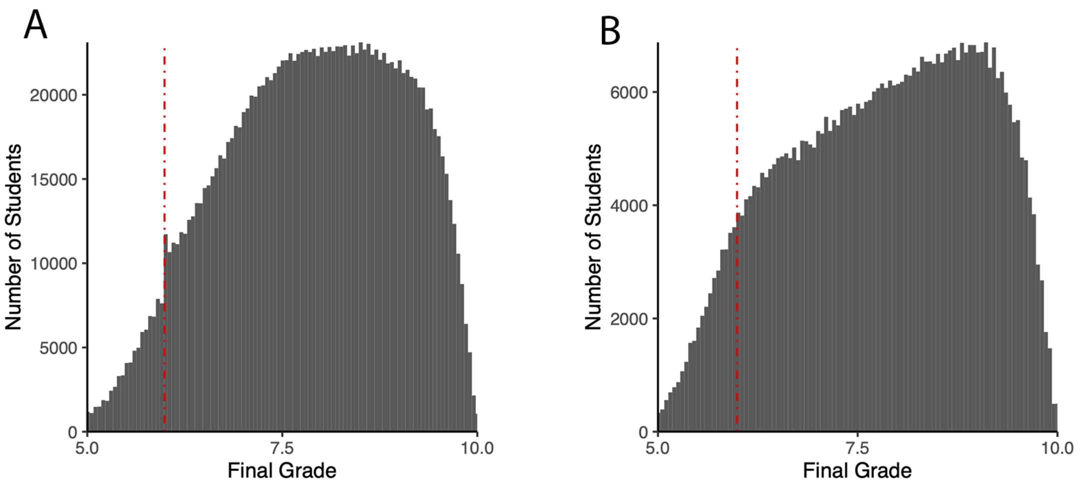

Figure 1. Histograms of overall bac scores among students scoring at least 5.0 on each component for those taking the exam from 2004 to 2014 (*A*) and from 2015 to 2019 (*B*). Vertical dot-dashed line indicates 6.0 threshold for passage.

strongly consistent with manipulation around the threshold of 6.00. The *t*-statistic for the null hypothesis of no jump in the density is 17.2 ($p < .0001$) in the earlier period. By contrast, the data from 2015 to 2019 show only a small jump in density at this threshold, one that does not reach standard levels of significance ($p < .05$).

This does not prove that there was no sorting around this cutoff. It should be noted that the *p*-value for our test is .15, which provides very weak evidence of a small jump. Thus, although we cannot dismiss the possibility of cheating entirely, it seems unlikely that there are very many rule-breakers in the more recent time period (2015–19) from which we draw our sample.

Admission to university

The process of admission to university occurs in two rounds, in July and September respectively. As such, bac-takers from both the June–July and August–September sessions may be eligible for university admission. However, the September round of university admissions is meant to fill the allotted spots unoccupied after the July round, thus making it more difficult for the bac takers from the August–September session

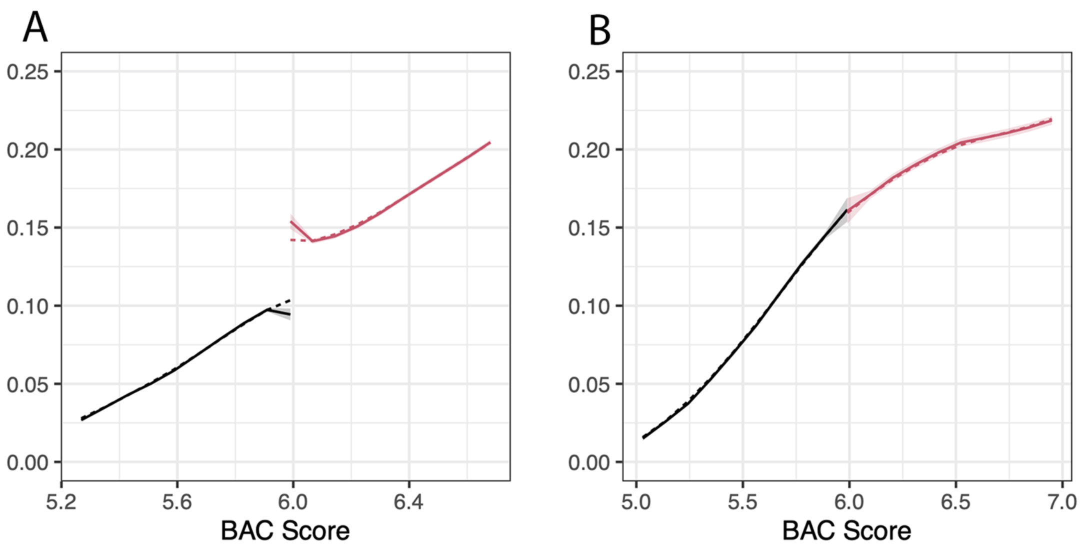

Figure 2. Nonparametric density estimates on either side of exam score threshold. Estimated density with 95% confidence intervals for individual student bac scores from 2004 to 2014 (*A*) and from 2015 to 2019 (*B*), estimated separately above and below passage threshold using approach introduced by Cattaneo, Jansson, and Ma (2018). Note that their approach uses different methods for point estimates and confidence intervals, which can result in estimates (*lines*) falling outside of confidence bounds (*shaded regions*). This is due to the differing optimality criteria for point estimation and inference. Accordingly, in addition to this mean squared error optimal point estimate (*dashed line*), we plot an estimate constructed simply by taking the average of the upper and lower bounds for the confidence interval at each point (*solid line*). Our ultimate inferences about possible sorting, which are based on the confidence interval rather than point estimates, however, are unaffected by this decision.

to be admitted to university. The admissions process is strict and explicit, as each course of study in each university has a precise formula for admission. Most majors in most universities use the final grade from the bac as the sole entrance criterion; some adopt additional criteria.

While the exam strongly affects students' college attendance, obtaining an average of at least 6.0 on the bac is neither a strictly necessary nor sufficient condition for a candidate to be admitted to her university of choice. Students who pass the exam could decide against attending university, perhaps because they did not get into their most preferred school or because of events in their personal lives. Conversely, a student who failed the bac on her first attempt could retake it and ultimately pass, subsequently matriculating to university. Alternatively, because our score variable is the initial exam average, students could improve their initial score by challenging the results of one or more of their subject exams, which may result in an average above the 6.0 threshold.

The formulas for admission (i.e., how much weight the bac and the special exam have in the final decision) are known in advance, and admission results are public. Tuition is waived for candidates with the best test scores; the rest must pay. However, tuition fees are rather low and not prohibitive for most families. For instance, yearly tuition at the University of Bucharest ranged between 2,500 ($614) and 4,000 lei ($980) per annum for 2017–18 (Dumitru 2017), less than the average monthly salary (Calculator salarii 2019).

Students who do not pass the bac can still enroll in vocational schools (*scoala postliceala*) where they learn skills that prepare them for blue-collar jobs. These vocational programs are shorter (one to three years) than university programs and are usually organized within high schools. The curricula include narrow subjects related to specific skills that require less intellectual ability than university courses. Subjects are taught by high school teachers. In the nomenclature of the Ministry of Education of Romania, this form of education is considered preuniversity (i.e., part of secondary education; Ministerul Educației Național 2022). A few Romanians are privileged to attend school outside the country; however, they are unlikely to secure a place in foreign universities—where standards are generally stricter—unless they also pass the bac.

Noncompliance, cutoff, bandwidth

In summary, two problems of compliance arise in this RD design. Recall that the score variable is a student's score on her first attempt taking the exam. Some students retake the exam, eventually manage to pass it, and matriculate to university, thus receiving the treatment of theoretical interest. Additionally, not everyone who passes the exam chooses to continue education at the university level, even though there are typically enough spaces for all passers who want to attend and tuition costs are minimal.

Evidently, the treatment is not assigned perfectly based on a student's bac score as it would be in a sharp RD. It is difficult to say, ex ante, how large these compliance problems might be (i.e., how much slippage there is on either side of the cutoff). However, it is clear that it is much easier for students to attend university if they pass the bac than if they do not. Therefore, we expect a large jump (discontinuity) in the probability of attending university between those who barely fail and those who barely pass, which creates an occasion for a "fuzzy" RD design. Our causal estimand is therefore the effect of treatment (college attendance) among compliers, where compliers are understood as people who would have gone to college if they passed the bac and would not have gone to college if they failed.

Passing the bac requires obtaining at least a 5.0 on each part of the exam and at least a 6.0 for the average of all parts. Within this group, there is a single cutoff (at 6.0) for exam passage, facilitating a straightforward RD setup in which the overall bac average is the score (or running) variable and university attendance is the treatment variable. The bandwidth is defined narrowly as scores falling within 0.2 of the cutoff. Between 2015 and 2019, 462,943 students took the bac for the first time and graduated from Romanian (nonminority) high schools. Of these, 19,402 obtained scores that fell between 5.8 and 6.2 and scored at least 5.0 on each part of the exam. This is the population of immediate interest.

Recruitment

Until 2020, the Romanian Ministry of Education posted all bac exam results complete with each exam-taker's name, score, and high school. In this fashion, we identify the students who fall within our population, as explained above ($N = 19{,}402$). Recruitment into the survey involved several steps.

First, we identify those high schools with at least one student in our target population ($N = 1{,}321$) and randomly assign them a number (from 1 to 1,321). This determines the order in which high schools are contacted. Second, we search for students in our sampling frame through Facebook (FB). FB usage is high in Romania, especially among our target population. An analysis conducted in January 2017 found that 93.2% of Romanians between age 15 and 24 use FB (https://web.archive.org/web/20190111083203/http://www.facebrands.ro/demografice.html#adoptie). Third, we invite these individuals to be FB friends with one of our online accounts (labeled "Social Attitudes in Romania"). The invitation mentions that they are invited as graduates of their high school, which was randomly selected for our study. Fourth, we send messages to each of the graduates from their high school FB account inviting them to participate in the survey.

This procedure raises several potential problems. First, there is a problem of identifying the correct individuals from each high school given that some names (even within the same high school) are likely to be identical. To alleviate this problem, we ask respondents to name the high school from which they graduated and their year of graduation. If these responses do not match the administrative records—or if a survey is begun but not completed—the survey is removed. Slightly over 100 surveys ($N = 102$) are eliminated on this basis, roughly 7% of the total.

A second anticipated problem is that women might be harder to identify, as they are likely to change their last name after marriage. As it turns out, FB has mechanisms for identifying women who may have changed their last names, which meant that we were able to contact women at roughly the same rate as men. Response rates were also similar—10.2% for women and 12.2% for men.

A third issue is that participation in FB may be posttreatment, a product of entering university. To address this potential bias, we calculated the percentage of those sampled whom we were able to locate on FB, above and below the cutoff. The two statistics are very close: 86.75% above the threshold and 86.8% below the threshold. Accordingly, there is no indication that attending a university affects one's propensity to engage on FB.

Our final data set includes 1,515 correctly identified respondents from 893 high schools. Features of this sample are explored in appendix B. Summary statistics are displayed in table B1. Comparisons between the sample and all bac takers, as well as those falling into our chosen bandwidth, are displayed in table B2. Finally, the results of a regression predicting survey response are shown in table B3. The only variable from this analysis whose coefficient is statistically significant is Female, whose impact is miniscule (−.023).

Sample size is somewhat lower than the target that we envisioned in our preregistration, which we attribute to two unexpected factors. First, the process of recruitment was slower than anticipated. Second, the arrival of COVID-19, and subsequent shuttering of universities across Romania in March 2020, meant that the treatment of theoretical interest was altered (from in-person to online instruction and from on-campus to at-home residence). Although we continued recruitment for several months (through October 2020), we ultimately decided that it would be injudicious to continue as there was no sign of university life returning to normal. Given that some of our data were collected during a period when the typical university experience was disrupted by the pandemic, estimated effects may be attenuated.

Balance

A key assumption of an RD design is that observations narrowly below and above the treatment threshold are similar on relevant pretreatment characteristics.[3] Here we consider four pretreatment characteristics: father's education, childhood socioeconomic status (SES), disciplinary track (humanities/social science vs. hard science), and location of high school (urban vs. nonurban).[4]

First, we examine the difference in means for each pretreatment covariate for values below and above the cutoff. Table 1 presents the results of these simple local randomization RD analyses with each of these pretreatment covariates as the dependent variable using a variety of bandwidths. All estimates in table 1 are from intent-to-treat (ITT) analyses based on the difference in means between the narrow failers and narrow passers of the bac exam in our sample's bandwidth.

There is some evidence that father's education is related to treatment status. However, the effect sizes, while statistically significant in many cases, are not substantively large. The estimated effect of father's education using all observations (bandwidth of .2) is −.22, a relationship that is extremely small on this variable's scale, which stretches from 1 to 11. Moreover, the relationship runs contrary to intuition: narrow passers report less parental education than narrow failers. We are not sure how to interpret this difference, which could be stochastic. In any case, any biases introduced into the main analysis would seem to bias results against what we find. Reassuringly, analyses that control for this background feature show that the main results are robust (see tables D1 and D2.)

For reported childhood SES there is less evidence of a difference between those narrowly above and below the treatment threshold. None of the results achieve conventional significance levels (although some might be termed marginally significant), and all estimates are substantively small (the childhood SES variable ranges from 1 to 5). It also appears that narrow passers are more likely to have taken the humanities/social science exam than narrow failers. There are no notable

3. These assumptions are somewhat different for the local randomization and continuity-based RD designs. See Cattaneo, Idrobo, and Titiunik (2018, 2020) for helpful discussions of these differences.

4. It should be noted that the first two of these were both measured after treatment, but it seems reasonable to assume that both were largely fixed at the time that the treatment was assigned. Of course, we cannot rule out that the treatment might be affected by the misreporting of these items. Disciplinary track and urban high school, by contrast, were both coded using administrative data.

Table 1. Regression Discontinuity Estimates of Effect of Baccalaureate Passage on Pretreatment Covariates

	Local Randomization* Bandwidth					
Dependent Variable	.2	.15	.1	.05	Minimum	Continuity-Based Estimate†
Father's education	−.22	−.22	−.17	.04	−.54	−.06
	[−.39, −.05]	[−.42, −.01]	[.41, .07]	[−.29, .37]	[−1.15, .07]	[−.75, .45]
	(.01)	(.04)	(.16)	(.82)	(.08)	(.62)
Childhood SES	−.10	−.12	−.09	.07	−.18	−.02
	[−.22, .02]	[−.26, .03]	[−.25, .08]	[−.16, .29]	[−.60, .23]	[−.31, .28]
	(.10)	(.11)	(.30)	(.57)	(.38)	(.92)
Humanities/social science track	.08	.09	.11	.09	.14	.13
	[.02, .13]	[.02, .16]	[.03, .18]	[−.02, .20]	[−.05, .34]	[.03, .20]
	(.01)	(.01)	(.01)	(.10)	(.15)	(.01)
Urban high school	−.02	−.03	−.04	−.06	−.02	−.09
	[−.07, .03]	[−.09, .02]	[−.11, .02]	[−.16, .03]	[−.19, .15]	[−.17, .05]
	(.48)	(.26)	(.20)	(.17)	(.78)	(.30)
N	1,230	815	623	339	106	1,230
N_{below}	553	373	291	146	53	553
N_{above}	677	442	332	193	52	677

Note. Estimates with 95% confidence intervals (in brackets) and *p*-values (in parentheses). Sample sizes listed are the number of bac scores in each bandwidth (including those below and above the cutoff). Number of responses to father's education and childhood socioeconomic status (SES) questions are slightly different because of a small number of missing values (less than 5% for each variable).

* Estimates from linear regressions predicting specified covariate with exam passage (i.e., having average bac score of at least 6), using data within specified bandwidth. The "minimum" bandwidth uses only observations with either the highest possible failing score (.5983333) or the lowest possible passing score (6).

† Estimates from continuity-based regression discontinuity analysis. Results are based on intent-to-treat analyses using linear regressions predicting a specified covariate with exam passage (i.e., having an average bac score of at least 6), estimating first-order (linear) polynomials for score separately on either side of the cutoff, with estimated and standard errors clustered by exam score, using the rdrobust function in the rdrobust R package.

differences in the high school locations (urban vs. nonurban) of bac passers and failers in our sample.

We also perform these pretreatment covariate analyses using our preregistered continuity-based RD specification (see the "Analysis" section for details), again plugging in each covariate separately as the dependent variable. Continuity-based results show no evidence of a jump at the treatment threshold for father's education, childhood SES, or urban high school. However, the continuity-based results do suggest a jump in the likelihood of being on the humanities/social science track (which is by definition pretreatment since test takers must select their exam track before taking the exam) at the passage threshold.

After presenting our main results below, we discuss several analyses that either include all four of these pretreatment covariates or subset respondents based on disciplinary track. Results from these analyses are similar to the main results (which do not adjust for covariates), offering some assurance that small differences in pretreatment covariates do not alter the overall results of the RD analysis. Sensitivity analyses (presented in the appendix) suggest that unobserved confounders would have to be implausibly large in order to change our main finding.

Measuring cultural liberalism

To measure cultural liberalism, we pose 29 questions to our survey respondents, as shown on the questionnaire (app. A). For the most part, these indicators are correlated with each other in the expected direction. However, the pairwise correlations are not very high, suggesting that there may be multiple dimensions to this far-flung concept.

For the main analysis, we follow the preregistered analysis plan (PAP), which proposes to combine the 29 indicators into a single index of cultural liberalism by taking the first component of a principal components analysis. This first component, which explains just over 15% of the overall variability, is then standardized, subtracting the mean and dividing by the standard deviation, to aid interpretation. This results in a measure of cultural liberalism that roughly follows a standard normal distribution within our sample

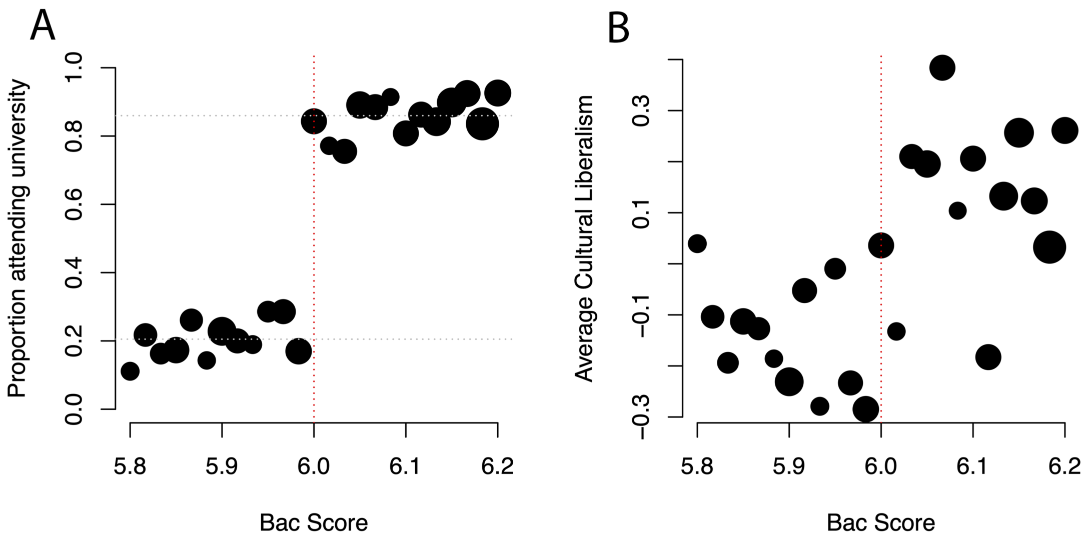

Figure 3. Relationship of bac score with treatment and with cultural liberalism. *A*, Proportion attending university among respondents having each unique value of bac score. *B*, Average level of cultural liberalism among respondents having each unique value of bac score. Vertical line denotes (fuzzy) treatment threshold of 6. Horizontal lines show averages for all respondents above/below the threshold. The size of each point is proportional to the number of observations at that Bac score value.

(see fig. C2). This index represents our main dependent variable.

As it happens, the principal component index is highly correlated with a simple average of the 29 indicator variables ($r = 0.93$).[5] Likewise, the main results are similar when using this simple average measure of cultural liberalism as the dependent variable. In an exploratory analysis, we consider various subcategories, which are also robust.

ANALYSIS

In this section we present the results of our analyses, which estimate the effect of university attendance on cultural liberalism. We then discuss a potential compound-treatment effect and introduce various robustness tests. In all analyses we begin with the simpler local randomization RD analysis (see Cattaneo, Frandsen, and Titiunik 2015) and then proceed to our preregistered continuity-based robust RD analysis (Calonico, Cattaneo, and Titiunik 2014). The final subsection disaggregates the outcome into four components of cultural liberalism.

Our main hypothesis is that university education enhances cultural liberalism. To make the comparison between treated and untreated groups as clear as possible, we exclude respondents who graduated high school in 2019 (following our PAP), some of whom received little or no university education by the time they took the survey. The remaining respondents are coded in a binary fashion (0 = no university education, 1 = at least some university education) generating the treatment of theoretical interest.

Figure 3*A* plots the proportion of students in our sample attending university among respondents at each possible bac score in our bandwidth. (Because the score variable is discrete, there are between 35 and 67 observations at each value, with an average of nearly 50.) We see a sharp increase in the probability of treatment at the threshold (6.0), indicating that those who narrowly pass the bac are significantly more likely to attend university than those who narrowly fail, consistent with the requirements of an RD design. Figure 3*B* shows evidence of a jump in cultural liberalism, which suggests that attending university increases cultural liberalism on average among people at the cutoff on our score variable.

Our formal analysis begins by presenting a local randomization analysis of the main effect, shown in table 2. Panel A shows the difference in the proportion attending university between those narrowly failing and narrowly passing the bac exam using various bandwidths. Using the .2 bandwidth, for example, the likelihood of attending university (being treated in our study) is estimated to be 65% higher for passers than for failers. Recall that our survey only samples from the population of students scoring between 5.8 and 6.2, meaning the .2 bandwidth includes all of our data. Analyses using narrower bandwidths (e.g., only respondents with bac scores between 5.9 and 6.1) show similar estimates. Even

5. For comparability, each indicator was first standardized and oriented so that higher values represent more culturally liberal responses before taking the simple average.

Table 2. Local Randomization Regression Discontinuity Estimates of University Attendance on Cultural Liberalism

	Bandwidth				
	.2	.15	.1	.05	Minimum
A: First stage	.65	.62	.61	.58	.67
	[.61, .70]	[.57, .67]	[.55, .68]	[.49, .67]	[.53, .82]
	(<.001)	(<.001)	(<.001)	(<.001)	(<.001)
B: ITT	.29	.30	.36	.31	.33
	[.18, .40]	[.17, .44]	[.20, .52]	[.09, .53]	[−.07, .73]
	(<.001)	(<.001)	(<.001)	(.01)	(.11)
C: Effect of university attendance	.44	.49	.59	.54	.49
	[.27, .62]	[.27, .71]	[.33, .85]	[.16, .92]	[−.10, 1.07]
	(<.001)	(<.001)	(<.001)	(.005)	(.11)
N	1,216	806	617	334	104
N_{below}	546	369	288	144	53
N_{above}	670	437	329	190	51

Note. Point estimates with 95% confidence intervals (in brackets) and p-values (in parentheses). First-stage estimates are based on linear regressions predicting university attendance with bac passage. Intent-to-treat (ITT) estimates are based on linear regressions predicting cultural liberalism with bac passage. Main effect estimates are from two-stage least squares predicting cultural liberalism with university attendance, instrumented by bac passage. The "minimum" bandwidth uses only observations with either the highest passing score below the cutoff (.5983333) or the lowest passing score (6).

using only the observations with the highest possible failing and the lowest possible passing bac score produces a nearly identical and highly significant estimate.

Panel B of table 2 shows the results of ITT analyses, predicting our dependent variable of cultural liberalism with bac passage. These analyses estimate that passers score roughly .3 higher on our index of cultural liberalism than failers. These estimates remain relatively stable with varying bandwidths and are highly significant except when using the narrowest possible bandwidth (which is weakly significant). Given that the dependent variable is constructed with a sample standard deviation of 1, this implies that bac passage increases cultural liberalism by somewhere around one-third of a sample standard deviation.

Panel C of table 2 presents two-stage least squares estimates for our main effect of interest: the effect of attending university on cultural liberalism among those with scores near the passage threshold. These fuzzy local randomization RD estimates use passage of the bac as an instrument for university attendance. Using the .2 bandwidth (including all survey respondents), we estimate that attending university increases cultural liberalism by .44—equivalent to nearly half of the sample standard deviation. Estimates based on narrower bandwidths are similar, although larger. All but the smallest bandwidth are highly statistically significant. Overall, these results suggest that attending university increases cultural liberalism in a nontrivial fashion.

In addition to the simpler local randomization analyses, we present the results of a continuity-based analysis that follows our PAP, as shown in table 3. This analysis estimates local linear regressions separately on either side of the cutoff, allowing for some relationship between the score variable and cultural liberalism. Because the sample is already selected to

Table 3. Continuity-Based Regression Discontinuity Estimates of Effect of University Attendance on Cultural Liberalism

	Estimate
A: First stage	.58
	[.47, .74]
	(<.001)
B: Intent to treat	.35
	[.17, 51]
	(<.001)
C: Effect of university attendance	.61
	[.27, .84]
	(<.001)
N	1,216
N_{below}	546
N_{above}	670

Note. Point estimates, 95% confidence intervals (in brackets), and p-values (in parentheses) from continuity-based regression discontinuity analysis. Results are based on first-order (linear) polynomials for score estimated separately on either side of the cutoff, with standard errors clustered by exam score, using the rdrobust function in the rdrobust R package.

be close to the threshold on the score variable, we eschew automatic bandwidth selection procedures and instead use the entire sample (bandwidth of .2) for this analysis. This approach relies on a different set of assumptions than the local randomization framework employed above.

To estimate the causal effect, we use the rdrobust function from the rdrobust R package (Calonico, Cattaneo, and Titiunik 2015) with standard options for a fuzzy RD setup except as noted below. Because our score variable is not fully continuous but instead slightly lumpy, we follow Lee and Card (2008) in clustering standard errors by exam score.[6] This fuzzy RD setup produces estimates of the average effect of university attendance on cultural liberalism for compliers (as previously defined) at the threshold.

Panel A of table 3 shows that passing the bac is estimated to increase the probability of attending university by .58, a substantively large and highly significant result. The ITT effect (effect of passing the bac on cultural liberalism) is estimated to be more than one-third of a sample standard deviation. Finally, our main result, the continuity-based RD estimate of the effect of university attendance on cultural liberalism, is .61, with a 95% confidence interval from .27 to .84, which is quite large, representing well over half a sample standard deviation.

The continuity results in table 3 are broadly consistent with the local randomization estimates in table 2, although the former suggest a slightly larger effect for university attendance on cultural liberalism. Recall that our cultural liberalism scale is standardized. Our estimates therefore imply a substantively large impact of university attendance on cultural liberalism—an effect of between one-half and two-thirds of a sample standard deviation on this dependent variable.

A potential compound treatment confounder

Insofar as there may be a causal relationship between passing the bac and becoming more culturally liberal, we are inclined to interpret this relationship as a product of university attendance/nonattendance, in line with our theoretical argument above. However, we must also consider the possibility that performance on the bac might affect young people's attitudes, independent of their subsequent matriculation (or not) to college. Students who fail the bac might feel discouraged, while those who pass might feel emboldened. This, in turn, might lead to different perspectives on society and politics.

In this version of reality, valid estimates arise from ITT estimators (shown in panel B of tables 2 and 3), not the local average treatment effect estimators (shown in panel C of tables 2 and 3), which do not satisfy the exclusion restriction. Since both estimators show similar effects (although ITT estimates are slightly weaker than average treatment effect estimates), varying assumptions about the data-generating process do not threaten our overall conclusion.

However, it is important to wrestle with the issue as it affects not only the reported point estimates but also their theoretical interpretation. For a variety of reasons, we do not think it very likely that passage or failure of the bac, by itself, affects cultural liberalism in Romania.

First and foremost, our data do not support that interpretation. Let us assume that the thrill of victory and the agony of defeat will be greatest for those who recently took the bac, attenuating over time. The more time has passed, the less likely it is that this event will loom large on one's horizons. In figure E1 we test for effects across different cohorts. There, we show that among the 2019 cohort (who recently took the bac) there is no evidence of a culturally liberal effect. By contrast, among other cohorts the effect is marked.

In addition, a number of a priori considerations incline us to discount the likelihood of a bac-only effect. First, the Romanian educational system is exam driven, so students are accustomed to the experience of passing and failing. Accordingly, the shock value of passing or failing the bac at age 18 is probably attenuated.

Second, passing the bac is not very useful in and of itself. It is not a high school diploma and is of little consequence in applying for jobs. The purpose of the bac is to gain entrance to university. Thus, although students who pass the bac may experience relief, and those who fail may be deflated, these emotions are in all probability connected to the possibility of university attendance rather than with passage of the bac per se. (If the prospect of becoming a college student changes one's attitudes, this is consistent with our theoretical framework, which identifies a variety of sociological factors by which university education could affect social and political attitudes. These may have as much to do with social roles as with formal education.)

Additional analyses

Appendix D presents the results of several additional analyses. First, to address concerns about imbalance in relevant pretreatment covariates, we perform a covariate-adjusted continuity-based RD analysis. To interpret these results as estimating the treatment effect of interest, one must impose additional parametric assumptions beyond those of the RD design. Rather than weakening our findings, table D3 shows that adjusting for covariates actually increases the magnitude of our point estimates slightly relative to the main results in tables 2 and 3.

6. See Cattaneo, Idrobo, and Titiunik (2018), esp. chap. 3, for a good discussion of these issues.

Second, we separately estimate the main effect among those taking the humanities/social science and science versions of the exam in tests shown in tables D1 (local randomization RD) and D2 (continuity-based RD). This pretreatment variable is binary, which makes subsetted analyses more straightforward to interpret and less dependent on assumptions about functional form. We find that the impact of university attendance is somewhat higher among those in the humanities/social science track than among those in the science track. The confidence intervals for these estimates overlap, so while there is suggestive evidence that humanities/social science students experience larger effects of university attendance than science students, this finding is only suggestive.

We also conduct several analyses varying the bandwidth for the continuity-based RD. Both when narrowing the bandwidth around the threshold and also when conducting so-called donut hole analyses (Bajari et al. 2011), dropping observations closer to the threshold in successive tests, our estimates remain relatively stable, albeit with a loss in precision as more data are discarded (see figs. D1 and D2).

Finally, we conduct sensitivity analyses following the approach of Cinelli and Hazlett (2021). These results, shown in figures D4 and D5, suggest that unobserved confounders would have to have an implausibly high explanatory power (well over 10 times as much as any of the four pretreatment covariates we consider) in order for our overall conclusion to change.

Components of cultural liberalism

The cultural liberalism index employed in our main analyses is derived from 29 survey items accessing diverse subjects associated with the concept of cultural liberalism. Since they are only modestly intercorrelated (as discussed in the "Research Design" section), one must consider the possibility that university education exerts different effects on different outcomes.

To test this possibility, we divide up the 29 questions into four components according to the substantive focus of each question (as per our PAP): gender/family, traditional morality, race/nationalism/immigration, and religion. (See table C1, for a full listing of items used for each component.) For each component, we first standardize each question, subtracting its mean and dividing by its standard deviation and orienting it such that higher values indicate more cultural liberalism. Then we average together these transformed items and standardize the resulting component index. Each component index is thus easily interpretable since it has mean 0 and standard deviation 1.

Table 4 shows pairwise intercorrelations across these four components. In general, they are moderately positive, although none are extremely strong and one (gender/family and religion) is close to zero.

Table 5 shows the results of RD analyses following the same strategy as our main analysis, this time substituting the four components as the dependent variable in sequential analyses. All of these estimated effects are positive, and all but one is statistically significant. The largest estimated effect is for gender/family, which is based on questions concerning a woman's role in a household, business, and other areas. It is estimated that attending university leads to an increase of more than half a sample standard deviation on this dimension. University attendance is estimated to increase the traditional morality and race/nationalism/immigration components by roughly one-third of a sample standard deviation, although the estimated effect for the former is not statistically significant. The effect of university attendance on religion is the smallest in magnitude but is highly significant.

We also conducted separate RD analyses for each of the 29 survey items. For the vast majority, university attendance is estimated to increase the likelihood of a culturally liberal response (see fig. D3). In summary, there is some evidence that the liberalizing effect of higher education is stronger for gender-related attitudes than for religion. As this set of hypotheses was not preregistered, we must regard the resulting

Table 4. Correlations among Components of Cultural Liberalism

	Gender/Family	Traditional Morality	Race/Nationalism/Immigration	Religion
Gender/family	1.00	.24***	.23***	−.03
Traditional morality		1.00	.33***	.30***
Race/nationalism/immigration			1.00	.19***
Religion				1.00

Note. Sample correlations (Pearson's r) between cultural liberalism components. Asterisks denote significance level of tests of association (significance of correlation).
* $p < .05$.
** $p < .01$.
*** $p < .001$.

Table 5. Regression Discontinuity Estimates of Effect of University Attendance on Components of Cultural Liberalism

Outcome (Cultural Liberalism Component)	Local Randomization Estimate*	Continuity-Based Estimate†
Gender/family	.54 [.37, .72] (<.001)	.60 [.36, .79] (<.001)
Traditional morality	.20 [.03, .37] (.02)	.33 [−.17, .67] (.24)
Race/nationalism/immigration	.36 [.19, .53] (<.001)	.28 [.05, .43] (.01)
Religion	−.06 [−.23, .11] (.50)	.14 [.07, .53] (.01)
N	1,216	
N_{below}	546	
N_{above}	670	

Note. Point estimates with 95% confidence intervals (in brackets) and p-values (in parentheses).
* Estimates from two-stage least squares predicting cultural liberalism with university attendance, instrumented by bac passage.
† Estimates from continuity-based regression discontinuity analysis. Results are based on first-order (linear) polynomials for score estimated separately on either side of the cutoff, with standard errors clustered by exam score, using rdrobust function in rdrobust R package.

analyses as exploratory. Even so, we have shown that the liberalizing effect of a university education holds across a diverse range of outcomes.

DISCUSSION

Does university attendance lead to more liberal social attitudes? To answer this question, we implemented an RD design in Romania, a country where admittance to university is largely determined by a nationally regulated exam (the bac) with publicly available results. We found strong evidence for our main hypothesis: Romanian students who scored just above the passing threshold of the bac registered higher scores on our composite index of cultural liberalism. This pattern also held for the components of the cultural liberalism index, which we disaggregated into measures focused on gender/family, traditional morality, race/nationalism/ immigration, and religion.

Ancillary hypotheses explored in appendix I render more ambiguous results. We find some evidence, albeit not conclusive, that the effect of a university education is stronger among students who have pursued a humanities/social science track in high school compared to those in science tracks. Two additional tests, focused on whether the effect of university attendance is stronger for students with more years of exposure and for students from rural backgrounds, yielded inconclusive or null results depending on the framework used for analysis.

Before concluding, we want to address the generalizability of the main findings. One issue concerns the potential impact of a university education for those outside the RD bandwidth. For purposes of causal inference, we chose a very narrow range including students whose bac scores fell 0.2 points above or below the threshold for passing the exam (6.0 on a 10-point scale). These students are apt to be quite different from those with much lower, or higher, scores, so we cannot infer that university education would have the same impact across the entire population of Romanian students who take the bac.

In any case, students at the threshold are probably the most policy-relevant subgroup. Consider that when enrollment in higher education expands or contracts (due to family decisions, changes in the tertiary sector, government-initiated policy changes, or economic fluctuations) those on the threshold of viability are probably those most likely to enter, or exit, the university system. These are marginal students, by definition, whose capacity for higher education is in doubt. When supply expands, they are likely to enter; when supply contracts, they are likely to exit. Those with very low scores are unlikely to attend university under any circumstances, and those with very high scores are likely to attend university under all circumstances. Never-takers and always-takers are not very relevant when one is considering policy questions. To be sure, knowing the impact of university education on the always-takers would be helpful in understanding the impact of university on society at large. But this question is not easily answered, precisely because—absent extreme and unethical constraints—there can be no control group.

A second question concerns whether the main result—pertaining to students who fall at the exam threshold—might be generalizable to other countries. To assess this issue, we must compare Romania to other countries around the world on relevant dimensions such as (*a*) the nature of our sample (proxied by educational attainment), (*b*) the nature of the treatment (i.e., university curricula), and (*c*) the measured association between university education and cultural liberalism (as revealed by observational data).

These issues are addressed in appendix F. There, we conclude that there are grounds for optimism with respect to generalizability. As is common in these situations, we have the greatest confidence with respect to countries whose social, economic, and political contexts are most similar to Romania, such as the post-Soviet states in Eastern Europe. Since Romania has been part of the European Union since 2007 and shares a Western European language and culture, and since its university system is now harmonized with the European template, the European subcontinent offers another likely terrain across which our findings are probably generalizable. Beyond that, we show that Romania displays levels of educational attainment that fall near the global mean. Finally, a naive regression using data drawn from the World Value Survey shows an association between university education and cultural liberalism that is also close to the sample mean (controlling for several background factors). In these respects, Romania appears to offer a well-chosen case for generalizing about the impact of higher education on cultural liberalism.

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