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ORIGINAL ARTICLE

The impact of university attendance on partisanship

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Abstract

Survey research shows that those with university degrees are more left-liberal along a number of dimensions than their peers without higher education. There is even some evidence to suggest a growing social and political cleavage centered on educational attainment. Yet, claims about the liberalizing effect of universities on political ideology and partisan identification rest on observational evidence where many assumptions are required to reach causal inference. This may account for conflicting findings in published research. Here, we employ a fuzzy regression discontinuity design situated in Romania, where students who pass a national baccalaureate exam are uniquely qualified to enter university. We find that university attendance causes more liberal party preferences along the cultural dimension of party politics—though not along the economic or left-right dimensions of party conflict.

Key words: Public policy; voting behavior

The influence of political ideology and partisan identification on contemporary politics is widely recognized. Much less is known about its sources. Family background is undoubtedly important (Campbell *et al*., 1960); but not everyone follows in their parents' tracks. Why do some people evolve into liberals (or leftists) and others into conservatives (supporters of the Right)? What factors condition ideology and party affiliation?

This study centers on the potential role of higher education. Survey research shows that those with university degrees are more left-liberal along a number of dimensions than their peers without higher education. Indeed, there is evidence to suggest a growing social and political cleavage centered on educational attainment, with university as the watershed (Ford and Jennings, 2020). The fact that university faculty are generally more liberal than the societies in which they are situated offers grist for the mill (Ladd and Lipset, 1975; Klein *et al*., 2005; Gross and Fosse, 2012; Langbert, 2018; van de Werfhorst, 2020). Conservative commentators suspect that college campuses have become indoctrination camps for the Left (Horowitz, 2009; Maranto *et al*., 2009). There is even some evidence to suggest that increasing levels of educational attainment may have contributed to growing polarization (Baldassarri and Gelman, 2008) and a new dimension of partisan identity (Stubager, 2013; Gethin *et al*., 2022). There is no doubt that universities are at the center of a culture war in countries such as the United States (Bérubé and Nelson, 1994).

Yet, claims about the liberalizing effect of universities on political ideology and partisan identification rest on weak evidence. Since this point bears centrally on the contribution of the present study we consider the methodological angles at some length.[1]

[1]Our review of the literature is limited to studies focused on tertiary (not primary or secondary) education and on political ideology or party identification. We do not include studies focused on participation or social attitudes.

The most common approach to this issue enlists surveys of the general public that include questions about educational attainment along with measures of party affiliation and ideology, the outcomes of interest. To sort out causal effects these studies condition on confounders in a regression framework (e.g., Weakliem, 2002). To reach causal inference the resulting model must be correctly specified such that potential outcomes are independent of treatment conditional on observed covariates, which is typically quite a strong assumption in these settings. The same caveats apply to matching estimators (e.g., Gelepithis and Giani, 2020), except that here the guesswork is in the covariates chosen for matching, as well as choices among matching estimators.

Both regression and matching frameworks involve untestable assumptions. Potential selection effects—based on family, peer groups, social class, urbanization, intelligence, or core personality attributes—are especially worrisome. If smart people are more likely to attend university *and* by virtue of their intelligence to adopt liberal views, as is sometimes claimed (Onraet *et al.*, 2015), the confounder is virtually impossible to condition. Note also that the decision to attend university may be influenced by the perceived ideological environment of the university, generating endogeneity between the right and left sides of a causal model.

Strikingly, studies that do a more credible job of controlling for background factors often report weak or null results. One such approach enlists over-time comparisons, surveying a group of students at various junctures of their educational experience. A few studies report a liberal shift in attitudes (Newcomb, 1964; Feldman and Newcomb, 1969) while others do not (Mariani and Hewitt, 2008; Elchardus and Spruyt, 2010; Strother *et al.*, 2020), or report mixed findings (Surridge, 2016). Woessner and Kelly-Woessner (2009) conclude that students exposed to political science drift to the left during the course of a semester but not in a way that is associated with their professors' political affiliation. Hanson *et al.* (2012) discovers that liberal arts colleges have a stronger liberalizing effect than other sorts of four-year colleges. Mendelberg *et al.* (2017) find that affluent students who attend schools with a high share of other affluent students experience a conservative shift in attitudes with respect to taxing the rich, arguing that social norms can activate latent class identity among this group. (Here, it is possible that the concentration of rich students is proxying for some unmeasured factor that affects student attitudes toward redistribution.) At best, these studies measure the impact of higher education—or particular types of higher education—on the treated; one cannot extrapolate these effects for the un-treated since the latter are apt to be quite different from the treated group. Others have used instrumental variables such as compulsory schooling laws with the goal of avoiding selection biases (e.g. Cavaillé and Marshall, 2019).

Another approach focuses on twins, comparing those who attend college with those who do not (Sieben and de Graaf, 2004; Campbell and Horowitz, 2016). These studies generally do not find evidence of a college effect on political ideology. Granted, null effects may be generated by contamination across treatment and control groups, as siblings are apt to influence each other's political views.

For all these reasons, one may wonder whether universities affect political ideologies, or simply reflect them. A further point of caution is that most research focuses on a single country—the United States—a country often regarded as exceptional in politics and in higher education.

In this study, we capitalize on an opportunity in Romania, where graduating high school students take a nationwide exam that qualifies some of them to matriculate to university. This allows for a comparison between young Romanians that are likely to be quite similar in most respects, but quite different in their likelihood of receiving the treatment of interest, university attendance.[2] Loosely speaking, we ask whether those who scored narrowly below the threshold, and thus were unlikely to attend university, have similar political ideologies to those who score narrowly above the threshold, and thus are very likely to attend university. Under fairly weak assumptions about the relationship between exam score and ideology, we are able to identify the effect of university

[2]The RD design is increasingly common in political science and in the social sciences more generally (Cattaneo *et al.*, 2019, 2020). Our approach shares features with Hangartner *et al.* (2020), who look at former students who narrowly pass or fail entry exams for upper-level secondary schools in Switzerland.

attendance among those scoring at this cutoff value on the exam, as discussed below. Because there are roughly 150,000 exam-takers each year, we are able to focus on a very narrow bandwidth. This plausibly minimizes the degree of confounding related to background factors that might distinguish those who attend university from those who do not.

Following the literature, our main hypothesis concerns the liberalizing effect of a university education on the cultural (noneconomic) dimension of party conflict. This hypothesis is pre-registered along with a detailed pre-analysis plan (EGAP study number 20190912AB [https://osf.io/45tu2], reproduced as Appendix F), which is closely adhered to, albeit with a few minor deviations (noted below).[3] Our main finding is that university attendance in Romania causes more liberal party preferences along the cultural dimension of party politics—though not along the economic or left-right dimensions of party conflict.

We begin by laying out a theoretical framework. The second section introduces the methodology. Next, we report the main findings. The fourth section briefly lays out the results of tests focused on various subsidiary hypotheses. The final section concludes.

1. Framework

A "university" will be defined here as any post-secondary institution that offers instruction in the professions and the liberal arts—excluding vocational schools, arts schools, or theological seminaries.

Our main hypothesis concerns the influence of university on the *cultural* dimension of party politics. This dimension encompasses a great variety of issues that do not directly affect the pocketbooks of most voters, e.g., sexuality and sexual identity, prostitution, drug addiction, abortion, crime, capital punishment, marriage and divorce, parenting, gender, race, immigration, the nation, religion, science, technology, the environment, civil liberties, and democracy.[4]

There are reasons for believing that the experience of attending a university might engender a more liberal (i.e. left-leaning) position on each of the foregoing issues, i.e., more tolerant of deviance, more protective of individual rights, more concerned with public goods, more skeptical of traditional norms and practices, and more cosmopolitan in orientation.

Several possible reasons for this liberalizing effect may be gleaned from the literature. Liberal-leaning professors may inflect course material with their political views—an *indoctrination* effect (Phelan *et al.*, 1995). Among students, liberal activists may influence fellow students—a *peer* effect (Sacerdote, 2001; Strother *et al.*, 2020). The opportunity to study and reflect, along with cognitive developments occurring in early adulthood, may engender an intellectual dynamic with liberal conclusions—an *enlightenment* effect (Hyman and Wright, 1979). Finally, the experience of higher education may enhance feelings of competence and security—a *psychological* effect (McClosky and Brill, 1983). By contrast, those who do not attend university are likely to have very different life-experiences on average, which may result in different likelihoods of supporting more liberal parties.

In addition to our main hypothesis, about the role of university education in fostering cultural liberalism, we proposed six subsidiary hypotheses in our pre-registered report. Specifically:

> H_2 (intensity): The liberalizing effect of university attendance on the cultural dimension of party ideology increases with additional years of tertiary education.

[3]Four studies were pre-registered at the same time, each utilizing data from the same RD design but focused on a different outcome: social capital (Apfeld *et al.*, 2022*a*), cultural liberalism (Apfeld *et al.*, 2022*b*), corruption (Apfeld *et al.*, 2022*c*), and partisanship (the current study).

[4]Confusingly, this noneconomic dimension of politics calls forth a number of overlapping labels, e.g., liberal-authoritarian (Kitschelt, 1994), new politics versus old politics (Franklin, 1992), integration versus demarcation (Kriesi *et al.*, 2006), green/alternative/libertarian versus traditional/authoritarian/nationalist [GALTAN] (Hooghe *et al.*, 2002), social left-right (Coman, 2017), and post-materialist (Inglehart, 2018). Our preference for "cultural" over these alternative labels derives from its simplicity and breadth.

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H_3 (rural/urban): The liberalizing effect of university attendance on the cultural dimension of party ideology is stronger for students who grow up in rural areas than for those who grow up in urban areas.
H_4 (disciplines): This liberalizing effect of university attendance on the cultural dimension of party ideology is stronger for students in the humanities and social sciences than for students in the natural sciences.
H_5 (parental deviation): Students who attend university are more likely to deviate from the party preferred by their parents.
H_6 (economic liberalism): University education enhances support for parties with a more left-liberal view of economic issues.
H_7 (left-right): University education enhances support for parties with a more left-liberal view overall (including both economic and social issues).

These subsidiary hypotheses are explored briefly in Section 4 below, and more fully in Appendix E.

2. Research design

Our research site—the country of Romania—may be described as middling in economic development and higher education. Like other countries in Central Europe it bears the legacy of Soviet rule, while moving toward Western Europe in recent decades as a member of the European Union. Educational attainment among college-age citizens is nearly at the global mean (Barro and Lee, 2013; see also Appendix D). The higher education curriculum is similar to other countries in Europe, a product of Romania's membership in the European Higher Education Area, which ensures comparability across the EU.[5]

With respect to its political system, Romania may be considered a fairly typical case within Central and Eastern Europe (see Economist Intelligence Unit, 2021). Within the context of the OECD, its electoral system, party system size, methods of internal party governance, party finance, media, campaign finance, turnout, and level of partisanship are by no means unusual (Dalton *et al.*, 2011). As in any newly democratized country, parties are fairly new and volatility is moderately high (though not extreme). Party identification is certainly not as entrenched as in the United States or Britain, and less likely to be perceived along a single left-right spectrum than in most other OECD countries (Dalton *et al.*, 2011: 87).[6] Nonetheless, parties occupy identifiable ideological niches across several dimensions, as suggested by coding provided by the Chapel Hill expert survey (see Table E4). On this account, one might expect the causal effects registered in this study to be stronger than in two party systems with entrenched political parties but perhaps not as strong as in multi-party systems with weaker partisan attachments.

Thus, along various parameters that might be expected to influence the relationship between higher education and party ideology, Romania appears to be a fairly typical case. However, other features of this case are fairly unique, setting the stage for a natural experiment in which higher education is as-if randomly assigned.

2.1 The baccalaureate exam

The baccalaureate exam (the *bac*) is taken by the vast majority of students in Romania upon graduation from high school. Performance on the exam determines eligibility for college and for chosen fields of study.

[5]See http://www.ehea.info/.

[6]Insofar as left-right placement is consequential, surveys suggest that Romanian citizens situate themselves further to the right than citizens in most other OECD countries, on par with the United States (Dalton *et al.*, 2011: 90).

Scoring on the *bac* is based on three parts, each of which is scored from 1.0 to 10.0. In order to pass, students must score at least a 5.0 on each part and average at least 6.0 across these three parts. To ensure a single passage threshold and to avoid potential sorting that could occur from failing students retaking the exam, we calculate our score variable as the overall average on a student's first attempt taking the exam (ignoring any score changes that may occur after possible challenges to the initial grade) and we only include in our sampling students who scored at least a 5.0 on each part of the exam. Among all test takers who took the exam during the years analyzed, 75 percent received a passing score on their first attempt.

We expect that students who narrowly pass the exam will be much more likely to attend university than those who narrowly fail, providing an opportunity for an RD design. Specifically, differences in the likelihood of attending university between narrow passers and narrow failers allow one to estimate the effect of university attendance among those who would have attended university if they had passed and not attended university if they had failed—so-called "compliers" in the terminology of Imbens and Angrist (1994).

One important concern arises if students are able to sort themselves around the cutoff by means other than their (unassisted) performance on the exam. If so, any comparison between passers and failers would be subject to bias, as background factors (social class, parents' education, personality) might be different.

In fact, analyses presented in Appendix B show that prior to 2015 there are significantly more narrow passers than narrow failers of the *bac*, suggesting that cheating may have been prevalent. Starting in 2015, however, with the introduction of strong anti-cheating reforms, there is no evidence of this discrepancy at the cutoff. The high concentration of final grades just above the threshold is likely a function of the work of school principals in coordination with the evaluation/grading committees. Before the reforms, the exams were held and graded in each high school by a committee made up of teachers from other high schools in the region. Throughout the examination period these graders were in close contact with the principal of the examined high school. It was common for the principal to treat these committee members to nice dinners and to offer them presents and sometimes money in exchange for overall higher passage rates for the high school. This was the goal of principals, who were evaluated based on these passage rates. Given this goal, the cheating involved giving slightly higher grades to those students who barely failed; this process was not too risky as it was unlikely to be flagged as inappropriate by any supervisory committee (the raised grades were not outrageously different), but at the same time satisfied the needs of the principal. To provide these special compensations to the examiners, parents were asked to contribute a small fee. After the reforms, this practice has ended because: (1) exams are not held in individual high schools, but in large centers with students from various high schools mixed together; (2) the grading is centralized in big centers and individual graders receive individual exams from students from many high schools of unknown identity. Further discussion of the *bac*, its role in our analysis, and potential threats to inference are contained in Appendix B.

2.2 Recruitment

The identification and recruitment of research subjects involved several steps.[7] First, we scraped a public web site maintained by the Romanian Ministry of Education, which (until 2020) listed all *bac* takers for the past decade along with their name, score, and high school. Students scoring just above and below the cutoff (from 5.8 to 6.2) in *bac* exams from 2015 to 2019 formed our sampling frame. Second, students within the sampling frame were contacted through Facebook accounts (labeled "Social Attitudes in Romania") linked to their high schools. Importantly, Facebook usage is high in Romania, especially among our target population.[8]

[7]For further details see Apfeld *et al.* (2022*a*).

[8]See www.facebrands.ro/demografice.html

The result of this lengthy recruitment protocol, carried out in 2019 and 2020, is a sample of 1515 respondents. Descriptive statistics for this sample are displayed in Table B1. Comparisons between those who fall above and below the threshold are contained in Table B2.

2.3 Ideology

As in many countries, the party system in Romania does not align neatly along a unidimensional spectrum. Consequently, there is little to be gained by asking respondents to fit themselves into a left-right scale, which might be differently interpreted or not understood at all.

Instead, we ask about party affiliation. Parties are fairly well-established in Romania and we have reason to believe that most voters identify with a particular party. Our survey asks, "If there were a national election tomorrow, for which party on this list would you vote?" This is followed by a list of six major parties (along with an "Other" option).

To ascertain the ideology of these parties we enlist the Chapel Hill expert survey (Bakker *et al.*, 1999) conducted in Fall 2019, coincident with our own survey. As an indicator of the cultural dimension of party conflict we adopt GALTAN, which measures each party according to its views on democratic freedoms and rights.

> "Libertarian" or "postmaterialist" parties favor expanded personal freedoms, for example, access to abortion, active euthanasia, same-sex marriage, or greater democratic participation. "Traditional" or "authoritarian" parties often reject these ideas; they value order, tradition, and stability, and believe that the government should be a firm moral authority on social and cultural issues.

Scoring from experts places Romanian parties on an interval scale from 0 (traditional or authoritarian) to 10 (libertarian or postmaterialist). We then reverse the original scale so that higher values correspond to a more left-liberal position. This measure serves as our primary dependent variable.

3. Analysis

We use a fuzzy RD design in our main analysis. Since our sample consists solely of students whose *bac* scores are in the narrow range of 5.8–6.2, variation in this score variable is relatively small. This means it is unnecessary to include multiple polynomial terms in our estimation of the relationship between score and the dependent variable.[9] Furthermore, we do not employ automatic bandwidth selection procedures such as those proposed by Calonico *et al.* (2020) since these are unlikely to be appropriate when applied to an already narrowly selected bandwidth of data. Instead, we use all observations within our *bac* score sampling bandwidth of 5.8–6.2. We employ the continuity-based framework in which it is assumed that the relationship between *bac* score and both treatment probability and potential outcomes for the dependent variable are continuous away from the cutoff. This allows for the estimation of each of these relationships separately for those scoring below and above 6.0. Assuming that all respondents scoring below 6.0 who attended university would also have done so if they had scored above 6.0 and that all who scored above 6.0 and did not attend university also would not have done so if they scored below this value (the so-called monotonicity assumption), we can estimate the average treatment effect among compliers who score at the *bac* passage threshold (see Imbens and Lemieux, 2008; Cattaneo *et al.*, 2016*a*, 2016*b* for further discussions of this). An alternative assumption that would justify the same approach is based on assuming independence of potential outcomes and treatment decisions (see Hahn *et al.*, 2001). In our application, we regard this setup as

[9]Gelman and Imbens (2019) also provide another, more general, perspective arguing against the use of these polynomial estimation strategies.

less plausible since people might have a sense for how much university would benefit them, which could influence their decision to attend university.

Another approach would be to rely on the local randomization framework (Cattaneo *et al.*, 2015). However, before collecting our data, it was not obvious how wide a bandwidth would be reasonable to use in this framework. Given that there were a limited number of students with *bac* scores close to the threshold, and given the challenges of locating and contacting these people for our survey, our PAP proposed an approach that would use a continuity-based RD framework allowing for the use of a somewhat wider range of data around the score variable cutoff, but also with some adjustment for potential relationships between the score variable and the dependent variable on either side of the cutoff. In any event, Appendix C presents local randomization analyses as well as multiple robustness checks. The results of these analyses are similar overall to the ones shown in this article.

Our analysis uses the methods developed by Calonico *et al.* (2014), in particular the *rdrobust* function from the *rdrobust R* package (Calonico *et al.*, 2015). We use the default options except as noted below. This approach estimates a linear regression separately on each side of the threshold with weights based on a triangular kernel. Since *bac* scores are slightly lumpy rather than continuous, we use the approach proposed by follow Lee and Card (2008) and cluster our standard errors by exam score.[10]

University attendance is our treatment variable and the score variable is respondents' *bac* scores, with the (fuzzy) treatment threshold occurring at 6.0. In order to obtain cleaner comparisons between treated and untreated respondents, we drop those who graduated high school in 2019 from our analyses since these people would have little or no university experience before our survey was fielded in the fall of that same year. Treatment was then coded as 1 for those with at least some university education and 0 for those with no university education.

A key requirement of a fuzzy RD is that there is a discontinuous increase in the probability of treatment that occurs as the score variable crosses the threshold and that the relationship between the score variable and the probability of treatment is smooth away from the threshold (at least within the bandwidth used). The left panel of Figure 1 shows the proportion of respondents attending university among those having each unique value of the *bac* score. The jump in treatment probability at the cutoff is clearly dramatic. Respondents with *bac* scores of 6.0 or higher are much more likely to attend university than are those with scores below this threshold. In fact, 86 percent of those who pass the *bac* attended university, while only 21 percent of those who did not received this treatment.

The second panel of Figure 1 shows the average value of the dependent variable (cultural ideology score) among respondents with each *bac* score in the sample, revealing evidence of a jump at the threshold. This visual presentation, although intuitive, is informal. Note, however, that because this is a fuzzy RD, the jump in the dependent variable at the cutoff understates the effect of treatment.

Table 1 displays the estimated effect of university attendance on party affiliation based on our formal RD analysis. The estimated effect is 0.54, which is highly significant with a p-value of 0.01. The GALTAN index, in theory, can range from 0 to 10, but among the political parties in our sample it ranges from 1.75 to 7.38 (see Table 2) with a sample standard deviation of 1.88. This estimate represents an increase in support for culturally liberal parties by more than one-quarter of a sample standard deviation when one attends university. The 95 percent confidence interval for this estimate ranges from 0.19 to 1.09 which translates to 11 and 62 percent of a sample standard deviation. This estimate is of the effect of university attendance for compliers—people who would not have attended university if they scored below 6.0 on the *bac* but who would have attended university if they scored at least a 6.0.[11]

[10]Cattaneo *et al.* (2018), Chapter 3 discusses these issues in more depth.

[11]This approach involves some extrapolation since *bac* scores can only take a finite number of values, with 6.0 being the lowest passing score and 5.83333 being the highest failing score. Local randomization analyses, which are presented below, are another way of estimating the treatment effect under slightly different assumptions.

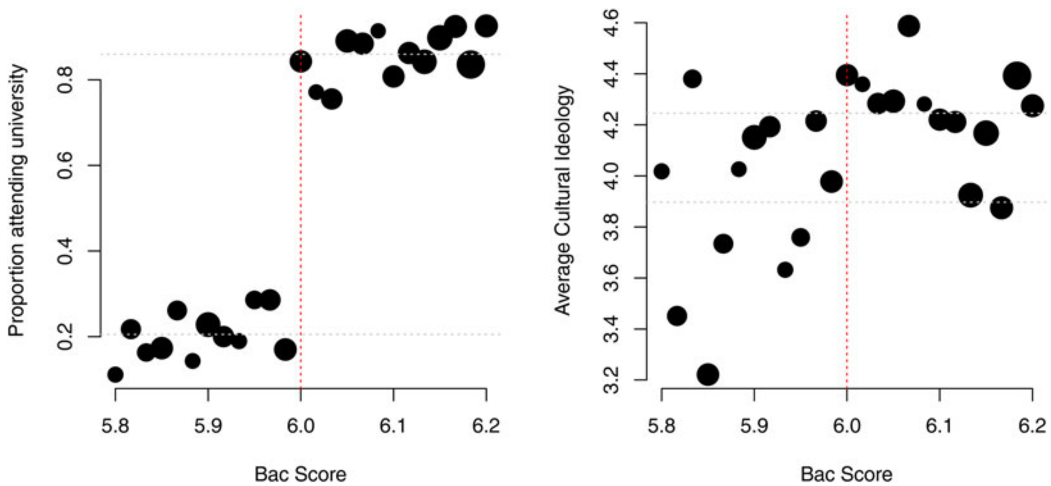

Figure 1. Relationship of *bac* score with treatment and with the cultural dimension of party conflict. Left panel shows proportion attending university among respondents having each unique value of bac score. Right panel shows average score along the cultural dimension of party conflict among respondents having each unique value of bac score. Vertical line denotes (fuzzy) treatment threshold of 6. Horizontal lines on each panel show averages for all respondents above/below threshold. The size of each point is proportional to the number of observations at that bac score.

Table 1. RD estimate of university attendance on the cultural dimension of party ideology

	Estimated impact	95% CI	p-value
University attendance	0.54	(0.19, 1.09)	0.01

Analysis using rdrobust function from rdrobust R package (version 0.99.9) clustering standard errors by bac score, using all of the data in our sampling bandwidth of 5.8–6.2, and otherwise using the function's default arguments. Confidence interval and p-value are based on robust bias-corrected results. $N = 1121$ (503 obs. below threshold, 618 obs. above threshold).

Table 2. Political parties and their scores on the cultural dimension of party ideology

Party	*N*	GALTAN (reversed scale)
Alianţa 2020 USR-PLUS [Save Romania Union-Freedom, Unity and Solidarity Party]	236	7.38
Partidul Naţional Liberal [National Liberal Party] (PNL)	362	4.00
Uniunea Democrată a Maghiarilor din România [Democratic Alliance of Hungarians in Romania] (UDMR)	11	3.94
Partidul Alianţa Liberalilor si Democratilor [Alliance of Liberals and Democrats] (ALDE)	117	3.75
PRO România [Pro Romania] (PROR)	94	3.46
Partidul Mişcarea Populară [People's Movement Party] (PMP)	88	2.79
Partidul Social Democrat [Social Democratic Party] (PSD)	228	1.75
Other/NA	99	–

Party names, number of respondents (N), and score on the GALTAN index (reversed scale: higher numbers refer to a more liberal position) from the Chapel Hill Expert Survey. Note that the UDMR is under-represented in our sample (relative to the country at-large) because our contacting procedure eliminates students who pursue a non-Romanian curriculum; this describes many of the supporters of this ethnically based party, which appeals primarily to Romanians of Hungarian descent.

This result demonstrates that attending university has a powerful effect on ideology in Romania, encouraging those who attend university to support more culturally liberal political parties than non-attenders. Importantly, because the effect estimated from RD designs applies to observations at the threshold (here, people scoring 6 on the *bac*), we cannot make direct statements about the effect of university on those with different scores. For example, our design does not allow us to estimate the effect of university for those scoring very low or very high on the *bac*. But the local effect at this passage threshold is likely to be of particular interest in our case. If university education were made either more or less widely available, this would be unlikely to affect those who did very well on the *bac* or those who did very poorly, because these two groups would likely remain very likely and very unlikely, respectively, to attend university after the change, just as they were before it. Instead, the narrow passers, as well as those with scores close to the passage threshold, are precisely those who are most likely to be affected by an expansion or contraction of the opportunity to attend university, making our estimated effect both theoretically and practically important. (For further discussion, see Section 4.)

A related question that may affect interpretation of our results is how the compliers in our sample, based on whom we estimate our treatment effect, compare to other groups on relevant covariates. Because our fuzzy RD estimates the average effect of treatment for compliers, understanding the characteristics of these compliers helps put our findings into perspective. In Figure B3 (Appendix B), we present the results of analyses following Marbach and Hangartner (2020), comparing compliers (for which our effects are estimated) to always-takers (those who would have attended university whether they passed or failed the *bac*) and never-takers (those who would not have attended university whether they passed or failed the *bac*). We estimate that roughly two-thirds of our sample consists of compliers, meaning that our estimated effect applies to the majority of narrow *bac* passers. Furthermore, compliers are estimated to have similar average values of several pre-treatment variables including father's education, childhood socioeconomic status (SES), and gender. The only covariate showing statistically significant differences between compliers and other types is attending an urban (versus small town or rural) high school. In this case, the estimated proportion urban among always-takers is 72 percent urban, among never-takers is 36 percent, and among compliers is 44 percent. Overall, then, compliers appear to resemble the general sample in most respects with the exception of urban residence.

Table B1 in Appendix B compares the characteristics of our sample to the general population of Romania and the population of those under 24 years old (93 percent of our sample is in this age group). Although our sample is obviously not a representative sample of young Romanian adults (instead it is closer to a random sample of those scoring between 5.8 and 6.2 on the *bac* in the years we consider), it is still informative to consider how they compare on important characteristics. Our sample is similar to these young adults in terms of the being unmarried, childless, and ethnically Romanian. But our sample is less male, more likely to be employed, and more likely to identify as being in one of the lower social classes. It should be noted, though, that we cannot make these comparisons to the population of those scoring between 5.8 and 6.2 on the *bac*, but instead look at those in a similar age range to our respondents.

3.1 Robustness checks and RD design validation

We conduct several analyses in Appendix C to assess the robustness of our main finding that university attendance causes an increase in support for culturally liberal parties. First, we repeat the same type of analysis in Table 1, but change the bandwidth around the treatment threshold. This type of analysis can be useful insofar as it provides information about whether RD estimates are sensitive to this arbitrary choice. In our case, we can only reduce the bandwidth used since our data collection strategy focused on contacting people whose scores fall between 5.8 and 6.2. Analyzing several narrower bandwidth choices produces similar estimates to our main results, albeit with the estimates becoming less precise as narrower bandwidths (containing fewer observations) are analyzed.

We also performed "donut hole" robustness checks (Bajari *et al.*, 2011), changing the bandwidth by removing observations lying within a certain distance of the cutoff. If students could somehow sort themselves very narrowly around the passage threshold these analyses may reduce the bias of RD estimates. In general, these results produced similar estimates to those in Table 1,

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Table 3. RD estimates of effect of bac passage on pre-treatment covariates

	Difference in means		Continuity-based RD
Father's education	−0.22		−0.06
	(−0.39, −0.05)		(−0.75, 0.45)
	(0.01)		(0.62)
Childhood SES	−0.10		−0.02
	(−0.22, 0.02)		(−0.31, 0.28)
	(0.10)		(0.92)
Humanities/social science track	0.08		0.13
	(0.02, 0.13)		(0.03, 0.20)
	(0.01)		(0.01)
Urban high school	−0.02		−0.09
	(−0.07, 0.03)		(−0.17, 0.05)
	(0.48)		(0.30)
N		1230	
(N_{below}, N_{above})		(553, 677)	

Difference in means: Local randomization estimates (with 95 percent confidence intervals and p-values underneath) from linear regressions predicting specified covariate with exam passage (i.e. having average bac score of at least 6).
Continuity-based RD: Estimates (with 95 percent confidence intervals and p-values underneath) from continuity-based RD analysis. Results are based on intent-to-treat analyses using linear regressions predicting specified covariate with exam passage (i.e. having average bac score of at least 6), estimating first order (linear) polynomial for score separately on either side of cutoff, estimated and standard errors clustered by exam score, using rdrobust function in rdrobust R package.
Sample sizes listed are the number of valid bac scores (including those below and above the cutoff). Number of responses to father's education and childhood SES questions are slightly different due to a small number of missing values (less than 5 percent for each variable).

the exception being when the radius of the "donut hole" is widened to 0.08 or 0.1, discarding a large portion of our data. In these cases the degree of extrapolation is very high the confidence intervals for the treatment effects are so wide as to make point estimates uninformative.

Another common method for evaluating the plausibility of RD assumptions is to choose one or more relevant pre-treatment covariates and substitute them as the dependent variable in the same sort of RD analysis used for the main RD analysis. In Table 3 we estimate the effect of university attendance on several variables: father's education, reported childhood SES, the disciplinary track that students selected for the *bac* exam, and attending an urban high school. It should be noted that while father's education level and reported childhood SES were measured as part of our survey, both disciplinary track and high school location are drawn from the administrative data about *bac* results and thus were measured pre-treatment. We conduct RD analyses separately, treating each of these covariates as if it were the dependent variable in a simple local randomization framework (essentially a simple difference in means between those below and above the threshold) and also the continuity-based RD analysis used for our main results in Table 1. All analyses are "intent to treat" (ITT). Using the continuity-based framework paralleling our main analysis above, we find no significant differences in three of the pre-treatment characteristics, but we do find some evidence that the humanities/social science track is more common above rather than below the threshold. Using local randomization analyses, both disciplinary track and father's education show significant differences, although it should be noted that narrow passers report having *less* educated fathers than narrow failers, which might be regarded as a bias *against* our findings. Appendix Figure B4 shows RD plots for these pre-treatment covariates, which does indicate that very narrow passing scores include some of the highest proportions of humanities or social science students, but the sample sizes are relatively small in these specific score bins.

Overall, these imbalances could be the result of sorting around the threshold or of differential nonresponse to our survey that is related to the covariates. In order to assess whether our results are influenced by covariate imbalance, we re-estimate the main treatment effect from Table 1 above, this time with covariates for each of the four pre-treatment variables in Table 3 (Calonico *et al*., 2019). Although this analysis relies more strongly on specific modeling assumptions, it adjusts estimates for possible imbalances in these predictors. These estimates are virtually

Table 4. RD estimate of university attendance on the cultural dimension of party ideology including covariates

	Estimated impact	95% CI	p-value
University attendance	0.51	(0.37, 1.22)	<0.001

Analysis using rdrobust function from rdrobust R package (version 0.99.9) clustering standard errors by bac score, using all of the data in our sampling bandwidth of 5.8–6.2, and otherwise using the function's default arguments. Confidence interval and p-value are based on robust bias-corrected results. $N = 1051$ (474 obs. below threshold, 577 obs. above threshold).

identical to our main estimates, providing further reassurance that covariate imbalances are not driving the results (Table 4).

Finally, we also conduct RD analyses in the local randomization framework as a compliment to the continuity-based analyses used here. The assumptions in the local randomization framework are somewhat different than in the continuity-based framework, meaning that if both sets of results provide similar estimates, we might be more confident in our findings. Formally, in the local randomization approach it is assumed that there exists some bandwidth on either side of the treatment threshold within which we can assume that treatment is as if randomly assigned. This allows the data to be analyzed as in a randomized experiment.[12] The local randomization analyses generally produce positive and highly significant estimates for university attendance's impact on political liberalism. These estimates are quite similar to our main result above, although when using very small bandwidths these estimates become insignificant, albeit with similar size to the other estimates (see Appendix Table C3). Estimates ranged from 0.45 to 0.61 depending on the bandwidth used, with an estimate of 0.51 when using all of our data (bandwidth of 5.8 to 6.2 as in the main analyses above). This is quite close to the effect of 0.54 estimated in our pre-registered main continuity-based analysis in Table 1 above. The fact that the overall substantive findings are similar under these two different approaches bolsters our confidence in the results. Appendix C provides more information about these local randomization estimates.

Overall, the main finding—that university attendance causes people to be more politically liberal—appears quite robust.

4. Subsidiary hypotheses

In addition to our main hypothesis, we set forth a number of subsidiary hypotheses, listed briefly at the end of Section 1. In this section, we review our findings. Details about the designs, as well as tables and figures showing the statistical tests, are contained in Appendix E. Importantly, most of these hypotheses require subsetting our main sample, which entails a significant loss of power. These results are therefore regarded as highly speculative.

First, there is no evidence that longer attendance (measured in years since matriculation) at university leads to stronger causal effects (Figure E1). At the same time, we note that estimating the effect separately among each of five graduation year cohorts produces imprecise results, so this should not be viewed as a strong null finding.

Second, we do not find that university students from rural backgrounds are more susceptible to the liberalizing effect of college than students from urban backgrounds. In fact, it may be the reverse—though the estimate is not terribly precise and may be stochastic (Table E1).

Third, the liberalizing effect of university education does not appear to differ by intended university concentration. No differences are found when we compare the impact of university on students majoring in (a) humanities and social sciences or (b) natural sciences (Table E2).

Fourth, attending university sharply increases the probability that a student will support a different political party that than of his or her father. The estimated effect of 0.22 (for a binary dependent variable with a sample mean of 0.70) is large and highly significant (Table E3).

[12]See Cattaneo *et al.* (2018: Chapter 2) for a useful description of the local randomization framework.

Finally, there is no evidence that attendance at university exerts a liberalizing effect on party ideology along economic or left-right dimensions. Indeed, there may be a modest conservative effect along these (highly correlated) dimensions of party ideology (Tables E5–E6), which would run counter to our expectations, although these results are not statistically significant in our main specifications.

We surmise that on *economic* issues (e.g., taxes, spending, redistribution, regulation) students are likely to encounter a mixture of left- and right-wing views in the scholarly literature, the predispositions of college faculty, and the content of course curricula. Consequently, students presumably receive mixed signals during their tenure at university. Additionally, attendance at a university is likely to enhance students' long-term earning power and class status. Consequently, they may view government intervention—and especially policies with a redistributive goal—with greater skepticism.

5. Conclusion

The impact of higher education on political behavior has attracted a good deal of scholarly attention, reviewed briefly at the outset. Most studies rely on observational data, which means that inferences are subject to strong assumptions.

Here, we identify a setting where restrictions on college matriculation are imposed in an as-if random fashion among those scoring close to the passage threshold on the national baccalaureate exam. Our fuzzy regression discontinuity design shows that attendance at university affects party affiliation along a cultural dimension. Specifically, those who attend university are more likely to choose parties with culturally liberal profiles, as measured by the GALTAN index from the Chapel Hill survey.

For purposes of causal inference, our study focuses on a narrow range of subjects: narrow passers and failers of the national baccalaureate exam. Accordingly, we cannot confidently generalize across other Romanian students who scored higher, or lower, on the *bac*. Strictly speaking, our RD analysis estimates apply only to compliers at the *bac* passage threshold of 6.0. Indeed, the estimated effect could be quite different from the hypothetical effect of university education for those with very high or very low scores. For example, those who excelled academically in high school may be exposed to experiences that are similar to university attendance even if they never matriculate; if so, we can anticipate a smaller treatment effect.

Yet, from a *policy* perspective, students at or near the threshold may be the most relevant subgroup. For it is this group, composed of narrow passers and failers, that is most affected by decisions to expand, or contract, access to higher education. Accordingly, our results speak directly to those who are considering policies that will affect the size of the tertiary sector—making a college education easier, or harder, to obtain.

The question arises as to whether the impact of university education is similar in other settings, i.e., outside Romania. Regrettably, there are few occasions for natural experiments, and (researcher-controlled) experiments are scarce to nonexistent. Observational data are our only recourse if we wish to gauge the generalizability of this study's findings. To that end, we conduct a regression analysis using recent survey data from the European Social Survey, as described in Appendix G. As it happens, estimates from this analysis are quite similar to estimates from our RD analysis centered on narrow passers and failers of the *bac* in Romania. This may be regarded as evidence of generalizability. The caveat is that, despite our inclusion of control variables measuring each respondent's family background, these results could be biased by the kinds of selection effects that all observational analyses are subject to, as discussed at the outset. It is difficult to say.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/psrm.2022.33. To obtain replication material for this article, please visit https://doi.org/10.7910/DVN/ZDHDIH

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