

Kent Academic Repository

Full text document (pdf)

Citation for published version

Campos, Camila and Hargreaves Heap, Shaun and Leon, Fernanda L.L. de (2017) The political influence of peer groups: experimental evidence in the classroom. *Oxford Economic Papers*, 69 (4). pp. 963-985. ISSN 0030-7653.

DOI

<https://doi.org/10.1093/oen%2Fgpw065>

Link to record in KAR

<https://kar.kent.ac.uk/58161/>

Document Version

Publisher pdf

Copyright & reuse

Content in the Kent Academic Repository is made available for research purposes. Unless otherwise stated all content is protected by copyright and in the absence of an open licence (eg Creative Commons), permissions for further reuse of content should be sought from the publisher, author or other copyright holder.

Versions of research

The version in the Kent Academic Repository may differ from the final published version.

Users are advised to check <http://kar.kent.ac.uk> for the status of the paper. **Users should always cite the published version of record.**

Enquiries

For any further enquiries regarding the licence status of this document, please contact:

researchsupport@kent.ac.uk

If you believe this document infringes copyright then please contact the KAR admin team with the take-down information provided at <http://kar.kent.ac.uk/contact.html>

The political influence of peer groups: experimental evidence in the classroom

Camila F. S. Campos,^a Shaun Hargreaves Heap,^b and Fernanda Leite Lopez de Leon^c

^aDepartment of Economics, Insper Institute of Education and Research;
e-mail: camilafsc@gmail.com.

^bDepartment of Political Economy, King's College London; e-mail: s.hargreavesheap@kcl.ac.uk.

^cSchool of Economics, University of Kent, Canterbury, CT2 7NZ;
e-mail: fernandalleon@gmail.com.

Abstract

People who belong to the same group often behave alike. Is this because people with similar preferences naturally associate with each other or because group dynamics cause individual preferences and/or the information that they have to converge? We address this question with a natural experiment. We find no evidence that peer political identification affects individual identification. But we do find that peer engagement affects political identification: a more politically engaged peer group encourages individual political affiliation to move from the extremes to the centre.

JEL classifications: D71, I23, Z19

1. Introduction

People often behave alike when they know each other well. Friends, for example, frequently vote for the same party, send their children to similar schools, choose the same types of vacations or enjoy eating at certain restaurants and not at others. Groups are formed by such commonalities and they pose a fundamental question for social science. Do such commonalities arise because people with prior preferences for 'x' naturally associate with fellow 'x' seekers and share information, or does membership of the group encourage conformity because the psychological dynamics within a group are such that individual preferences become more alike? This is the question that we address in this paper with a natural experiment, focusing on political behaviour.

The question matters because much in economics and some versions of liberal political theory turns on taking individual preferences as given. The appeal, for instance, of the Pareto criterion in welfare economics and the 'will of the people' as a justification for democratic decision-making depends on being able to identify individuals with their preference

and this becomes problematic if an individual's preferences change with those of their peers.¹ The question, however, is difficult to answer. To control adequately for possible prior commonalities, common shocks and the role of information transmission within a group, and so identify whether there is a distinct peer effect on individual preferences, is not easy. This is why experiments, where the scope for such control is often greater, are attractive. The laboratory experimental evidence, however, is mixed on this general question. For example, [Hung and Plott \(2001\)](#) interpret the evidence from their information cascade experiment as telling in favour of information transmission and against preference change in the explanation of behavioural conformity. But, the evidence on the unpredictability of music bandwagons in [Salganik *et al.*'s \(2006\)](#) experiment is difficult to reconcile with information transmission alone. In this paper, we report on a natural experiment where we attempt to disentangle the contribution of prior commonalities and the possible information transmission effect within a group from the possible influence that peers have on other individuals' specific political preferences.

We consider whether there is evidence of peer effects on two types of individual political behaviours. One is an individual's substantive political identification on a left-right spectrum and the other is on an individual's engagement with the process of politics that is revealed by their acquisition of information on candidates in an election and their willingness to vote in an election.² Where there is evidence that a peer's political identification and/or engagement affects individual political identification and/or engagement, we exploit aspects of the data to consider whether it arises from a peer influence on the political information that individuals have or over their preferences.

There is a large literature on peer effects in politics.³ The specific evidence on peer effects on political identification is mixed. Some studies find evidence consistent with the claim that people follow their peer's political affiliations ([Kenny, 1994](#); [Beck, 2002](#); [Sinclair, 2009](#)), others find no association ([MacKuen and Brown, 1987](#)). But much of this is based on correlations that are subject to selection biases: that is, the correlations could arise from people with shared prior commonalities naturally being drawn together. We address this difficulty in the natural experiment by exploiting the fact that our data consists of freshman students who have been randomly divided between different class groups for the introductory courses in their chosen major subject. This means that the characteristics of

¹ Of course, the normative appeal of democracy need not depend on this property of aggregating pre-existing individual views. The deliberative virtues of democracy depend, in principle, instead on being able to persuade others to a different point of view.

² Given the Public Choice insights with respect to 'rational ignorance' and the 'paradox of voting', an individual willingness to acquire information and/or vote is often regarded as indicating that individual has some kind of 'social preference' that is revealed by this kind of engagement with politics. Thus, we examine political behaviours where there are both personal and social preferences that are plausibly in play.

³ Many studies investigate how individuals' behaviour is associated with the behaviour or characteristics of their household members ([Nickerson, 2008](#)), people who live in the same geographical and residential area ([Huckfeldt and Sprague, 1987](#); [Huckfeldt *et al.*, 1995](#); [Cho, 2003](#); [Cho *et al.*, 2006](#); [Huckfeldt and Mendez, 2008](#)), housemates ([Klofstad, 2009, 2010](#)), discussion partners ([Mutz, 2002a, 2002b](#); [Huckfeldt, 2007](#); [Gerber *et al.*, 2012](#)), co-workers ([Mutz and Mondak, 2006](#)) or Facebook friends ([Bond *et al.*, 2012](#)). Others look at indirect measures of peer effects, for example [Gentzkow and Shapiro \(2011\)](#) compare the degree of ideological segregation in the consumption of media among friends and family.

the peers in a person's class group should be independent of his or her own characteristics. We interview students twice in an election year (before the presidential campaign and after the election). To test for peer effects, we examine how and whether their identification and engagement in the second survey correlates with their classmates' initial political orientations and engagement.

There are other studies that use an experimental or quasi-experimental framework for the same reason. For example, [Sacerdote \(2001\)](#), [Lyle \(2009\)](#), and [Carrell *et al.* \(2011\)](#) use data on randomly assigned networks to identify peer effects on student performance, physical fitness and workers' productivity, respectively. The closest to our study are the natural and field experiments that have examined peer effects on voting turnout ([Gerber *et al.*, 2008](#); [Nickerson, 2008](#); [Klofstad, 2009, 2010](#); [Funk, 2010](#); [Panagopoulos, 2010](#).) Their findings are consistent with the fact that voting is contagious in social circles. But little is known about the mechanism producing conformity in this instance. Does it arise because individuals become better informed about political choices through interaction with peers and so become more inclined to vote? Or do peer preferences for political engagement strengthen what would otherwise be weak individual preferences for political engagement?⁴ The difference matters for the reason discussed above and our natural experiment is useful in distinguishing between these possible explanations of peer effects on the likelihood of voting, as well as other aspects of political behaviour.

We find no evidence that peer political identification influences individual political identification. Interestingly, when we relax the controls for prior commonalities among the members of a group, we find an apparent peer political identification effect on individual political identification. This suggests that the failure to control fully for prior commonalities can, in practice, be a serious problem: it can lead to misleading inferences over the sensitivity of individual behaviour to peers.

We do find evidence, however, of a peer engagement effect on individual political identification and possibly on the willingness to vote. This might seem troubling for those who take preferences as given, especially as there is no evidence that this effect arises because individuals acquire more information through the media and only weak evidence that their political knowledge of the candidates improves. But, the fact that peer engagement appears to encourage individual political identification to move towards the Centre suggests a different and less troubling interpretation, especially as there is no evidence that this effect comes from the mere existence of differences in view within the peer group (as in [Mutz 2002a, 2002b](#)). It is an effect that is associated with an engaged peer group and if initial political affiliations are held with some uncertainty then discussion within an engaged group can help clarify an individual's own affiliation on the left-right-wing scale with the result that there is regression to the mean in the form of a movement to the Centre.

In the next section, we explain the data and describe the natural experiment on freshman students at Brazil's largest university. We set out the model that we use for identifying

⁴ These are open questions. [Claudine Gay \(2009\)](#) discusses the lack of knowledge about the subject in putting forward her perspective about the Future of Political Science: 'We know relatively little about how contexts in which individuals are situated shape politically relevant beliefs and opinions, and subsequently, behavior: What features of context matter? What are the mechanisms of contextual influence? What is the range of behaviors and attitudes affected? A full and compelling account of the political life of the mass public is impossible without greater attention to these questions.'

peer effects in Section 3. Section 4 presents the estimates of peer effects. Section 5 discusses these results and we conclude in Section 6.

2. Data and identification of peer effects

2.1 Overview

The data is based on freshman students at the Universidade de São Paulo (USP). The move from high school to university marks a natural transition to adulthood where new networks are formed. USP is the largest university in Brazil and the freshman students are randomly allocated to classrooms. As a result, these classes plausibly represent new randomly created peer groups for the incoming students. Our strategy was to sample these freshman students early in the academic year, and before the commencement of a presidential campaign, to establish prior values of the individual variables relating to preferences for political affiliation and engagement. For each individual we calculate peer effect variables for two key measures, relating to the political engagement and political affiliation. We then re-survey the sample at the conclusion of the presidential election and test whether the individual political affiliation, engagement and knowledge at this later date correlates with the peer variables.

The choice of surveying freshman students who are entering during a presidential election year is important for the identification of peer effects. The fact of the election makes the transition to adulthood particularly salient because voting is compulsory for everyone aged 18 or above in Brazil. The campaign, that occurs between the first and final sample of individual variables, is also a natural political event which might cause individuals to think about politics and so become exposed to peer effects, if there are any.

There are strong grounds for supposing that the social life in classrooms is an appropriate environment to measure peer effects. USP freshmen have all their introductory lectures with the same group of classmates during their first term in university (when we first interview them). They have at least two lectures together per day⁵ and they interact outside the class with each other through academic activities such as study groups and joint course projects. In addition, there are fewer alternative university peer groups than is typically the case at UK and US universities because most students are local and live at home (74%). Classmates are the first group of students they meet in college and it is a relatively large pool of possible friends (the average size of a classroom is 33 students). In short, between our surveys, students became friends, interacted in classes, and were exposed to a presidential campaign that made politics salient for discussions within social circles.

2.2 The sample and method of data collection

USP has approximately 86,187 students enrolled and offers 229 undergraduate and graduate courses. To be enrolled, undergraduate students must complete secondary education and pass an entrance exam ('Vestibular'), which is USP-major specific and runs once a year. USP is a public university, that is tuition-free, and it is one of the most prestigious universities in Latin America. For these reasons, the USP entrance exam is highly competitive: for

⁵ Students in morning courses have two lectures per day, from 7:30 to 11a.m., while students in evening have lectures from 7:30 to 11p.m.

instance, in 2011, the number of applicants was 138,888 and the year's enrolment was only 10,202.⁶

Our data come from the 2010 cohort of freshman students enrolled in specific subject majors: architecture, business administration, economics, history, law, literature, mathematics, physics, and sociology. For these majors, USP admits more than 180 students per year and divides the freshmen into at least two classes for the introductory courses. While students obviously choose their subject major, they cannot choose their class assignment: it is based either on alphabet order or a university algorithm. Since the initial process of allocating students to classes is random, our classes and the peer variables should be free from the more obvious sources of selection bias.⁷

The same survey procedure was used in all classes. An interviewer entered the classroom about 15 minutes before the end of a lecture, read an introductory script aloud, and distributed the questionnaires to all students. Lecturers also contributed by asking that attention and consideration be given to the survey. Students, then, had 10–12 minutes to complete, individually, the questionnaires. The survey was titled 'Young Adults' Political Behaviour' and the contact details of the authors were given for further information. The instructions made it clear that students should answer the survey individually. In every class, four types of questionnaires—containing the same questions but in a different order—were randomly distributed to students (to encourage individual answering). Practically all students agreed to answer the survey (in a few classrooms, one or two students failed to return the completed-out survey), and 95.54% of the respondents declared that they had answered questions in a serious manner. The questions are on individual demographics, political knowledge, political identification, media consumption and their parents' political commitments.

The first wave, pre-election, was administered during April 2010 (henceforth, referred to as t-1). The questionnaires were collected before the formal entry of all candidates in the race or of their running mates (in June) and before the beginning of the TV presidential campaign (in July) or any of the three debates on TV (in August and September). There is also evidence that media interest in the election notably picked up after the first wave.⁸ So it is likely that people tended to form opinions and discuss politics more enthusiastically from July on and after the first wave.

Nevertheless, it is possible that some peer effects had already occurred by the time of the first wave because it was conducted roughly one month after the beginning of classes. To test for this possibility, we conduct several tests for random assignment at this stage. First,

⁶ Only those students with top scores on the admission entrance exam are accepted. The level of competition varies by major of choice. For example, in the 2011 USP admission exam, 13,545 individuals applied to study Medicine and were competing for one of the 120 vacancies available. On the other hand, 260 individuals applied to study Mathematics, competing for one of the 112 places available (<http://www.fuvest.br/estat/insreg.html?anofuv=2011> [last accessed 30 September 2016]). More information about USP follows here: <http://www5.usp.br/> (last accessed 30 September 2016).

⁷ Table A1 in the online Appendix describes the number of classrooms and allocation rule per major class.

⁸ The television candidate advertisement broadcasts started (Silveira and De Mello, 2011) and the frequency of the mention of 'election' in one of the largest Brazil newspapers notably increased after the first wave, as did variation in election polls (see Figs A1 and A2 in the online Appendix).

following [Sacerdote \(2001\)](#), we use a standard test for random selection. This is discussed in Section 4.1 and does not cause any concern. Second, we test whether the variance of peer variables across classrooms per major class (the randomization level) is consistent with students' random assignment to classrooms. When peer variables differ too much or too little (with respect to the ones generated by a lottery), one explanation is that peer effects have occurred. The data largely reject the hypothesis that the variances are unusual.⁹ Based on this evidence and the early stage of the election year, we take the first wave of the survey as supplying information on pre-determined characteristics.

The second wave of the survey was administrated just after the first round of presidential elections, during October 2010 (henceforth, *t*). Students were asked the same questions as in the first wave, and they took a political quiz (that was piloted beforehand to ensure all questions were clear). The data in the first survey consisted of 1,593 student responses from 48 classes, the data in the second wave had 1,103 student responses from 39 classes. Our panel sample consisted of the students that had responded to both surveys, a total of 635 students.^{10,11} This is the main sample used in the analysis. It represents 39.8% of the initial sample. Two things should be noted about this. First, the peer variables for these individuals are calculated based on the larger initial survey of relevant individuals. Second, the panel sample has many similarities with USP students' population.¹² We test for whether the attrition is in any sense unbalanced or not random so as to bias results. We do this in three ways.

⁹ We conducted 1,000 simulations for each major class, allocating students randomly to classrooms. Then we computed the variance across classrooms for each simulated classroom allocation, and we constructed an empirical confidence interval. The actual variance for the proportion of classmates with a partisan parent was within the 10% confidence interval for all majors class. On the other hand, the actual variance for the proportion of classmates self-declared right-wing was out of the 10% confidence interval for two (out of 15) major class. Our results are not sensitive to these classrooms. We replicated the main regressions for the peer effect on ideology (in [Table 3](#)) excluding these two major classes and we find the same results. These findings are not reported, but are available on request.

¹⁰ The panel was identified based on responses about names, date of birth, and enrolled major. For a few cases, we also conducted checks on students' handwriting across surveys.

¹¹ The change in numbers between the two surveys partly occurred because the second wave of the survey was conducted in fewer classrooms. Although all contacted teachers agreed to allow us to survey their students during the first wave, Law and Architecture lecturers were conducting reviews or midterm exams during the second wave of survey. For this reason, many refused to let us conduct the survey. Another reason for the lower number of observations in the panel is that some students did not provide their names in the second wave and hence, we could not link their answers to the ones in the first survey—this occurred in 17.3% of cases (192/1103). Finally, some students missed the lecture on the day the survey was administered.

¹² We compared the characteristics of our sample with publicly available administrative records for freshmen classes. The results are presented in [Table A2](#) in the online Appendix. In general, students in our sample are less likely to come from lower socio-economic background than the universe of freshmen students, reflecting that more affluent students are more likely to attend classes (recall that USP is tuition-free). This is the population more exposed to classmates and to peer effects. It is important to note that such socio-economic selection of students is observed both in the panel and among all students observed in the first survey.

First, we investigate if there is any correlation between abstention in the second survey and our peer variables. We investigate this association across students within a major class (e.g. comparing the behaviour of students enrolled in Economics-evening, but that are assigned to different classrooms). We find no association.¹³

In other words, variations in the proportions of classmates that self-declare right-wing or those have a partisan parent in $t-1$, are unlikely to cause abstention in the survey in t .

Second, following Good (2006), we simulate random groups to calculate an empirical confidence interval for panels generated randomly, and test the null hypothesis that observed classroom panels was formed ‘like randomly’. For each classroom, we randomly drew, from the group observed in the first survey, a sample without replacement, with the same size as the observed panel. We calculated the average characteristic for those selected to be in simulated panel. We repeated this process 10,000 times to obtain an empirical 90% confidence interval of the panel characteristics, for each classroom. To summarize individuals’ many characteristics (demographics and political preferences) into a single number, we considered the conditional probability of an individual (observed in $t-1$) belonging to his/her own classroom.¹⁴ The results are presented in Table A3 in the online Appendix. They show values for observed classrooms, as compared to the confidence interval generated by the simulated groups. Out of 47 classrooms, the null hypothesis of a ‘random panel’ is rejected, at the 10% level, for only six of them. In an alternative check for whether attrition introduces selection in the data, we replicated tests of selection (explained in Section 4.1), for all students observed in the first survey and restricting the data to students in the panel, and we find null selection effects.¹⁵

Finally, we estimated the main regressions weighting each observation by the inverse probability of being observed in the panel.¹⁶ The magnitudes and level of significant of peer effect coefficients, reported in the online Appendix, largely remain the same.

We conclude that the attrition does not alter the sample in any visibly worrying respect. Nevertheless, to be sure that there is no biasing effect, we also control for students’ observable characteristics in the main regressions.

2.3 Variables

Table 1 gives descriptive statistics on the individual variables at $t-1$ and t and the peer variables. The pre-determined individual characteristics at $t-1$ are set out in Panel A. They relate to the usual demographics (gender, race, income, mother’s education and age), their declared political affiliation (left-wing, centre, right-wing), whether they have a partisan

¹³ We estimate regressions of the following for:

$$\Pr(\text{Be on the panel}) = \gamma \text{ Peer Variable} + \text{major fixed effect}$$

The coefficient γ is not statistically significant for any peer variable, with p-value of at least 30%

¹⁴ To construct this measure, we run OLS regressions using as dependent variable an indicator for whether a student is in classroom c in $t-1$. We used the same controls in the baseline specification in our main analysis, and consider the predicted value of a student belonging to his own classroom. We also experimented by calculating the propensity score measure using different controls, and find the same results.

¹⁵ The results for the restricted dataset are not shown, but are available upon request.

¹⁶ We considered the predicted probabilities from a logit model, as weights. We regressed a dummy for whether the individual is observed in the panel on classrooms fixed effects.

Table 1. Summary statistics individual and classroom level

	Mean	Stand Dev	Min	Max	Obs
<i>Panel A: Pre-determined characteristics and preferences</i>					
Female	0.490	0.490	0	1	622
White	0.781	0.414	0	1	622
Mother has a college degree	0.620	0.486	0	1	625
Have a partisan parent	0.448	0.498	0	1	620
Age	20.880	5.008	17	60	626
Right-wing	0.229	0.421	0	1	623
Centre	0.401	0.490	0	1	623
Intends to cast and invalid vote	0.168	0.374	0	1	622
Intends to watch political campaign on TV	0.451	0.498	0	1	627
<i>Panel B: Outcomes</i>					
Cast an invalid vote	0.058	0.233	0	1	625
% correct answers in the quiz	0.711	0.188	0	1	635
Asymmetric mistakes	1.704	1.367	0	6	635
Mistakes on own intended candidate	0.267	0.105	0.143	0.571	486
Mistakes on remaining candidates	0.285	0.096	0.143	0.714	622
Right-wing	0.240	0.427	0	1	622
Centre	0.363	0.481	0	1	622
No of days follows politics on TV	3.492	2.126	0	7	632
No of days follows politics on newspapers	2.590	2.168	0	7	630
No of days follows politics on internet	4.623	2.152	0	7	631
<i>Panel C: Classroom Composition (Peer variables)</i>					
Have a partisan parent	0.432	0.080	0.25	0.63	48
Intend to watch political campaign	0.426	0.119	0.20	0.75	48
Right-wing oriented	0.189	0.126	0.03	0.50	48
Number of Respondents (in t-1)	33.188	11.164	12	65	48

Note: The sample refers to students in the panel.

parent, whether they intend to cast an invalid vote and whether they intend to watch the political campaign on TV.

The individual outcomes in the second survey at time *t* are given in Panel B. Our measure of individual political identification is positioning on the left-right scale. A potential concern is that this is a stable concept and so may be less susceptible to peer influence (forcing a null result). However, in our sample of young adults, this turns out not to be the case: 29.3% of individuals changed their identification on this scale between the two surveys, suggesting that, as young voters, their political preferences were in some degree still in formation (Sears and Funk, 1999; Franklin, 2004; Prior, 2010). Nevertheless, to check the robustness of our findings, we replicate the analysis for party preference. We also asked students to cite the three most relevant socioeconomic problems among 13 alternatives and we use these answers to identify student political identification. We report these robustness results in the online Appendix.

Our measures of individual political engagement are casting an invalid vote and the number of days following politics in the media. ‘Following politics’ needs no explanation, but ‘casting an invalid vote’ may. The natural measure of a lack of engagement (not voting) is not available in Brazil because voting is compulsory. However, there is the option on the

ballot paper of voting for no one and this is what counts as an invalid vote. Of course, this could still be construed as a protest vote, but it does not, as it often does when voting is voluntary, suggest that person was at least sufficiently engaged with politics to make the effort to go to the polling booth (e.g. see [Maringoni, 2010](#)). There is no choice over making such an effort when voting is compulsory. Instead we find that those who cast an invalid vote are less informed and consume less political information (see [Table A4](#) in the online Appendix), suggesting that they are less engaged.

The final set of individual outcomes in time t are knowledge outcomes that come from a quiz containing the same number of analogous questions about each of the main presidential candidates, Dilma Rousseff, Jose Serra, and Marina Silva.¹⁷ We calculated the percentage of correct answers in the quiz, and we construct two knowledge variables that take account of the voting intentions at the time of the first survey. The variable, ‘Mistakes on Own Intended Candidate’, computes the proportion of mistakes in t made about the presidential candidate the student intended to vote for in $t-1$. Similarly, we create the variable ‘Mistakes on Remaining Candidates’ which computes the proportion of mistakes made about the other presidential candidates. As a more general measure of (dissimilarity of) knowledge about the candidates, we consider the sum of the pairwise differences in mistakes made about candidates ‘Asymmetric Mistakes’.¹⁸ A higher value of this variable reflects more asymmetric knowledge and less knowledge about candidates.

The summary statistics for the peer variables are given in Panel C. The peer political identification variable for individuals is based on their classmates’ direct responses to the political identification question at $t-1$ (i.e. the proportion who identify as right-wing). We have two peer political engagement variables formed in an analogous way as the percentage of classmates who answer in $t-1$ that they intend to watch the campaign on television and the percentage of classmates answering that a parent prefers a particular party. We call the latter the partisan parent peer variable. We use this variable for several reasons. First, most students live with their parents and it would not be surprising if politically-committed parents encouraged political engagement in their children through discussion at home, television viewing, etc. Indeed, there is evidence that having a partisan parent is associated in the first wave with a greater willingness to cast a vote and watch the campaign on television than those who do not have a partisan parent (see [Table A5](#) in the online Appendix). Second, it is possible that students misreport their political engagement when responding to the direct question as to whether they intend to watch the campaign on television because, in the context of a system of compulsory voting, this may seem like what good citizens should do or say. In contrast, there is no obvious reason for students to misreport whether they have a partisan parent. As a result of these considerations, partisan parents may be a more reliable indicator of student engagement in what becomes, in effect, a reduced form estimation of the influence of peers in this respect.¹⁹

¹⁷ The quiz was piloted beforehand to ensure all questions were clear. The quiz is in the online Appendix.

¹⁸ This is defined as:

$$\text{Asymmetric Mistakes} = |M_{\text{Rousseff}} - M_{\text{Serra}}| + |M_{\text{Rousseff}} - M_{\text{Silva}}| + |M_{\text{Serra}} - M_{\text{Silva}}|$$

where M_c stands for the number of mistakes made about each one of the three main candidates.

¹⁹ We also checked for peer effects based on students’ self-reported interest in politics. The results are qualitatively similar, but we do not present them here because this peer variable appears to be

It is important to note that although one might expect that students in the same major class are largely homogenous, there is sizable variation in the peer variables within the major class (see Table A1 in the online Appendix) and this is an important ingredient for the identification of peer effects.

3. Identification of peer effects

Following the literature on the identification of peer effects through experimental techniques (Sacerdote, 2001; Lyle, 2007), we assume students' outcomes are a function of individual and peer characteristics, as in (1).

$$Y_{mci}^t = \alpha + \beta_1 X_{mci}^{t-1} + \beta_2 \bar{X}_{c-i}^{t-1} + \beta_3 \bar{Y}_{c-i}^t + \epsilon_{mci}^t \quad (1)$$

The variable Y_{mci}^t , is the outcome at time t of individual i , enrolled in major class m , allocated to classroom c ; X_{mci}^{t-1} corresponds to own individual's pre-determined characteristics. The variable \bar{Y}_{c-i}^t represents the average behaviour of students in classroom c (excluding i) by t and \bar{X}_{c-i}^{t-1} are average characteristics of students in classroom c (excluding i), at time $t-1$.

Using Manski's (1993) outline, β_3 and β_2 correspond respectively to endogenous—that represents contemporaneous and simultaneous influence of peers—and exogenous—a sole influence of classmates on individuals—peer effects. As explained by Lyle (2007), the error term ϵ_{mci}^t can be decomposed into three terms ($\epsilon_{mci}^t = \epsilon_{1ci}^{t-1} + \epsilon_{2c}^t + \epsilon_{3mci}^t$), where ϵ_{1ci}^{t-1} represents an unobserved selection term, ϵ_{2c}^t represents common shocks and ϵ_{3mci}^t represents, a standard error term. In a non-random assignment setting, we could expect a correlation between ϵ_{1ci}^{t-1} and the peer variables (\bar{Y}_{c-i}^t and \bar{X}_{c-i}^{t-1}), as students' choice of whom to socialize with are based to some extent on individuals' tastes, which are unobservable to the researcher. This could lead to a possible bias in the estimates for β_2 and β_3 .

A related issue is that members of the same social group could be exposed to common external shocks/influences over the year (e.g. reading the same newspapers and participating in the same political events), thus leading to a positive bias for the estimates of β_2 and β_3 . This possibility is less likely when the initial allocation of individuals to classes is random (and we will show in Section 4.1, that there is no evidence of intentional selection). Further, since all students in the same major class take the same classes and are exposed to the same college environment, it seems plausible to assume they are exposed to similar sets of external influences over the election year. One important qualification, however, is that some shocks might be particular to students in some classrooms: for instance, the exposure to an instructor with extreme political views. An underlying assumption is that the influence of punctual shocks vanishes on aggregate when considering all external shocks at a level as fine as the classroom. This hypothesis is particularly important when estimating contemporaneous peer effects (β_3), as common shocks might lead to some correlation between ϵ_{2c}^t and \bar{Y}_{c-i}^t . For example, Lyle (2007) demonstrates that common shocks represent a confounder for the estimate of contemporaneous peer effects (β_3) even in the presence of a setting with random assignment, for the reason discussed above. Differently, common shocks over the election year are unlikely to be correlated with the distribution of students' pre-determined

endogenous on the basis of the analysis reported in Table 2, probably for the reasons suggested above.

characteristics across classrooms (\bar{X}_{c-i}^{t-1}) at the major class level;²⁰ therefore, $E[\bar{X}_{c-i}^{t-1}, \epsilon_{2c}^t] = 0$. For this reason, we take the average of Y_{mci}^t across classmates and obtain (3), with \bar{Y}_{c-i}^t as a function of Y_{mci}^t . Substituting (2) in (1) and rearranging, we obtain (3), which is the reduced form to be estimated and depends only on predetermined characteristics.

$$\bar{Y}_{c-i}^t = \delta + \gamma_1 X_{mci}^{t-1} + \gamma_2 \bar{X}_{c-i}^{t-1} + \gamma_3 Y_{mci}^t + \omega_{mci}^t \quad (2)$$

$$Y_{mci}^t = \phi_0 + \phi_1 X_{mci}^{t-1} + \phi_2 \bar{X}_{c-i}^{t-1} + \nu_{mci}^t \quad (3)$$

As a result, under random assignment, the coefficient ($\phi_2 = \frac{\beta_3 + \beta_3 \gamma_2}{1 - \beta_3 \gamma_3}$) captures peers' influence since it is free from a correlation with the error term. This peer effect is a function of both endogenous and exogenous peer structural parameters and these effects are not disentangled in this paper.

4. Results

4.1 Random assignment

The key identifying assumption of our study is that, conditional upon the major class of study, students were randomly assigned to classrooms. USP uses a randomizing procedure and we now check whether it had this effect. We perform the test proposed by Sacerdote (2001), regressing the peer characteristics of interest on the corresponding average of their peers. Since classrooms are small, even under random assignment, a negative correlation might be expected.²¹ To control for this, we applied the correction proposed by Guryan *et al.* (2009). They conclude that it suffices to include in the typical test the average value of the characteristic being inspected among all students in the same major class, excluding individual i (\bar{z}_{m-i}^{t-1}). The modified test corresponds to:

$$z_{mci}^t = \alpha + \gamma \bar{z}_{c-i}^{t-1} + \delta \bar{z}_{m-i}^{t-1} + \theta_m^{t-1} + \epsilon_{mci}^t \quad (4)$$

where z_{mci}^t is the outcome of individual i , enrolled in major class m , who takes classes in the first college term in classroom c . The variable \bar{z}_{c-i}^{t-1} is a peer measure based on the average characteristics of classmates while attending college in the first term, excluding himself and θ_m^{t-1} are major class fixed effects.

If peers are assigned randomly, the coefficient γ should not be statistically significant. Results are reported in Table 2. Each row represents a dependent variable and each entry reports coefficient estimates from a different regression. The specific results for the modified Sacerdote test (from eq. 4) are presented in the entries in bold (row V, column V, for $V = 1$ to 3). The coefficients for the peer variables are not statistically different from zero, thus supporting the assumption that selection is not affecting our main results.²² We also

²⁰ For example, since these are determined by the realization of the past classroom assignment lottery.

²¹ As explained by Guryan *et al.* (2009), this stems from the fact that individuals cannot be their own peers. '[T]he sampling of peers [in classrooms] is done without replacement—the individual himself is removed from the “urn” from which his peers are chosen.'

²² It is important to note that these results hold on average for students assigned by different rules—namely, random algorithm or alphabetical order. There is evidence in the US that first names

Table 2. Tests for random assignment of peers among classrooms

		Coefficient [Stand Error] on Peer Variable:		
		Right-wing	Has a partisan parent	Watch political campaign
	<i>Dependent Variable</i>	(1)	(2)	(3)
(1)	Right-wing	0.090 (0.082)	-0.2309* (0.119)	-0.047 (0.162)
(2)	Has a partisan parent	-0.276** (0.110)	-0.0500 (0.1146)	0.236 (0.150)
(3)	Intends to watch political campaign on TV	-0.086 (0.224)	0.2056 (0.1464)	-0.128 (0.115)
(4)	Number of days follows politics on TV	-0.010 (1.083)	-0.2296 (0.718)	-1.001 (0.832)
(5)	Intends to cast an invalid vote	0.025 (0.138)	0.0107 (0.125)	0.253* (0.143)
(6)	Evaluation of Lula Government (0-10)	1.028 (0.740)	0.799 (1.165)	-0.326 (0.904)
(7)	Centre-oriented	0.007 (0.208)	-0.3510* (0.184)	-0.189 (0.184)
	<i>Demographics</i>			
(8)	Female	-0.182 (0.179)	-0.1183 (0.239)	0.249 (0.162)
(9)	Mother has a college degree	-0.102 (0.193)	0.0343 (0.130)	0.160 (0.136)
(10)	Age	-2.733 (2.721)	1.7415 (4.355)	-2.098 (3.007)
(11)	White	0.147 (0.143)	0.0203 (0.136)	-0.106 (0.134)

Notes: (i) The Table reports OLS estimates from separate regressions of the relevant pre-determined individual characteristics on respective peer variables. Each entry represents an estimate from a different regression. All regressions include major-class fixed effects and average value of the peer characteristics among students in the same major-class (excluding himself); (ii) Standard errors clustered at the classroom level are in brackets; (iii) ** Statistically significant at 5%, * Statistically significant at 10%.

checked whether changes in the proportion of politically engaged peers (according to the two measures) were systematically correlated with other predetermined characteristics (to

convey individuals' demographic characteristics (Bertrand and Mullainathan, 2004). To understand whether a similar pattern was affecting our exercise, we looked for differences in classmates' characteristics according to the first letter of their first name (i.e. classroom assignment). In results not shown in the paper, we find that classmates' characteristics do not differ by name allocation for any of the observed characteristics.

rule out other possible confounding peer effects to the ones we investigate). We estimate (4), but use, as dependent variables, demographic characteristics and political behaviour variables. The results are reported in [Table 2](#), not in bold, in rows 1–11.

The right-wing and partisan parent peer variables are not related to students' media consumption of politics or intention to invalidate their votes. There is a negative association between the proportion of classmates with a partisan parent and students' propensity to identify themselves as right-wing. This association might arise for two reasons: (i) luck; or (ii) because some students felt afraid of declaring themselves to be right-wing-oriented. The latter would be worrying but the same correlation, which would be expected under (ii), is not observed among students' propensity to declare themselves to be right-wing-oriented and the proportion of classmates that are right-wing. Nevertheless, we also include classmates' average characteristics as additional controls in the main regressions.

4.2 Peer political affiliation effects on individual political affiliation and engagement

[Table 3](#) provides the estimates of a version of [eq. \(3\)](#)²³ using as dependent variable, an indicator for whether the individual self declares as right-wing in t and focusing on the influence of peers with right-wing identification. Each column reports a separate regression that differs according to the controls.

Our preferred and most complete specification is in column 4 where we control for possible sources of selection bias by including in the regressions indicators for gender, race, age, income, mother education, political identification at $t-1$, the proportion of classmates that declared to have a partisan parent in $t-1$, and major class fixed effects. We do not detect any peer effect. The coefficient on the proportion of right-wing classmates is practically equal to zero and it is not statistically significant. The results for regressions using individual left-wing identification as dependent variable mirror the ones reported here and, for reasons of space, are not presented. As additional robustness test, we conducted regressions replicating the specification in column 4, and using as dependent variables, indicators for whether the student is concerned about right-wing issues in t . These are taxes, corruption in the government, violence. We also examine the likelihood of concern in t with left-wing issues, which are poverty, quality of public schools and public transportation. As shown in [Table A6](#) in the online Appendix, the peer coefficient is not statistically significant for any issue.

We also considered more complex possible peer political identification effects. First, we test for an interaction term between the peer right-wing identification and an indicator for whether the student self-declared right-wing in $t-1$ ([Table 3](#), column 5). The purpose was to check whether peer effects worked specifically by reinforcing students' pre-determined preferences. Second, in [Table 3](#), column 6, we add to the regression a peer variable that is the proportion of classmates that both self-declare right-wing and have a partisan parent in $t-1$. This is to test for the possibility that individuals are affected by the preference of their engaged peers.²⁴ Again, we find no statistically significant effects. Third, in [Table 3](#), column 7, we extend this test to see whether this possible version of the peer effect operates on

²³ We add major class fixed effects (θ_m^{t-1}) to this equation.

²⁴ In so far as engaged peers provide a richer network for political discussion, one might also expect from the evidence in Satyanath *et al.* (2017, forthcoming) on the early dissemination of Nazi views in Germany that there would be this interaction.

Table 3. Impact of classmates' ideologies on students political orientations

Selected Controls	Dependent Variable: Self-declaring Right-wing by Election Time						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Peer Variables</i>							
% Right-wing classmates	0.372** (0.099)	0.332** (0.113)	-0.065 (0.237)	-0.204 (0.263)	-0.265 (0.263)	-0.251 (0.277)	
% Right-wing classmates X Right-wing					0.254 (0.371)		
% Right-wing classmates X						0.079 (0.176)	
Does not have a partisan parent							-0.254 (0.337)
% Right-wing classmates with a partisan parent							0.112 (0.339)
% Right-wing classmates with a partisan parent X							
Does not have a partisan parent							
<i>Pre-determined ideologies</i>							
Right-wing	0.528** (0.043)	0.526** (0.046)	0.508** (0.049)	0.513** (0.048)	0.442** (0.123)	0.513** (0.049)	0.514** (0.049)
Left-wing	-0.096** (0.025)	-0.106** (0.025)	-0.106** (0.027)	-0.099** (0.027)	-0.100** (0.027)	-0.099** (0.027)	-0.099** (0.027)
Centre (omitted)							
<i>Additional controls</i>							
Individual characteristics	no	yes	yes	yes	yes	yes	yes
% Classmates with a partisan parent	no	no	no	yes	yes	yes	yes
Major-class fixed effects	no	no	yes	yes	yes	yes	yes
Observations	612	563	563	563	563	563	563

Notes: (i) Each column represents the result from a separate OLS regression; (ii) Standard errors in brackets are clustered at the classroom level; (iii) Individual characteristics include gender, race, age, income, mother education and indicators for students' pre-determined inclination: right- and left-wing; (iv) ** Statistically significant at 5%, * Statistically significant at 10%.

the less engaged by interacting the proportion of right-wing with partisan parent with an indicator for when an individual does not have a partisan parent. The coefficient for this variable is not significant.

We report further robustness checks in the online Appendix. We decompose the peer variable into intervals over the full range and re-run the regressions to check for peer effects in any of these intervals of the full range (see Table A7 in the online Appendix). None are significant. Finally, we checked the robustness of this result by switching the dependent variable to a preference for a particular party (the PSDB-*Partido da Social Democracia Brasileira*) and the associated peer effect variable being the proportion of classmates who expressed preference for this party.²⁵ The conclusions are broadly the same (see Table A8 in the online Appendix). Reassuringly, in all these regressions we find that individuals' own pre-determined political identification are important in explaining political identification in *t*.

Result 1 There is no evidence that the political identification of an individual is affected by the political identification of his or her peers.

In comparing these findings with those in Table 3, columns 1 and 2, we check whether this result is sensitive to the control for selection biases. We find it is. If there is no control for choice of subject major class (recall, that at this level, students are randomly assigned to classrooms), and there is only control for individual demographic characteristics and pre-determined political preferences, the peer political identification variable becomes significant and positive. In other words, in the absence of random assignment, it appears that peers do affect an individual's political identification.

Result 2 The failure to control for selection biases creates the (false) impression that an individual's political identification is influenced by the political identification of his or her peers.

Table 4 gives the analogous regressions that test for a possible influence of peer political affiliation on individual knowledge, participation and the consumption of media. Each row represents a separate regression on that individual outcome and reports on the coefficient of the peer variable. Column 1 gives the results for the proportion of right-wing classmates peer variable, with the full set of controls. We find no statistically significant impacts, except for an increase in consumption of politics on the Internet (row 8).

There is a literature concerned with whether individuals are influenced by heterogeneity of political views in their group. Mutz (2002a), for example, suggests that people become confused in the presence of disagreements and so tend to participate less politically when in a heterogeneous group. We examine these possibilities for our group of students by replicating the regression in column 1, Table 4, but using Mutz's peer variable for the heterogeneity within the network (the proportion of classmates that have an opposite ideology²⁶).

The results are in Table 4, column 2. Contrary to Mutz (2002a), we find that ideological heterogeneity among classmates discourages casting an invalid vote and increases media consumption. These results are also important for countering worries that heterogeneity

²⁵ We only present results for preference for the PSDB party because, in our data, there was no change in the preference for another party between the two surveys.

²⁶ For instance, if the individual is right-wing we compute the proportion of students in the classroom that are left-wing or center-oriented. We then aggregate this individual measure to a peer variable as described before.

Table 4. Effects of ideology on knowledge and behaviour

Dependent Variables:		Peer Variables	
		% Right-wing Classmates (1)	% Opposite Ideology Classmates (2)
<i>Voting Participation and Political Knowledge</i>			
(1)	Cast an invalid vote	-0.108 (0.137) 563	-0.104* (0.055) 563
(2)	% Correct answer in the quiz	0.078 (0.117) 573	0.028 (0.047) 573
(3)	Asymmetric mistakes	0.834 (0.664) 573	0.536 (0.328) 573
(4)	Mistakes on own intended candidate	-0.028 (0.099) 444	-0.033 (0.028) 444
(5)	Mistakes on remaining candidates	0.057 (0.060) 564	0.002 (0.022) 564
<i>Consumption of Media</i>			
(6)	Number of days follows politics on TV	-1.592 (1.147) 570	0.164 (0.695) 570
(7)	Number of days follows politics on newspaper	1.366 (1.343) 568	-0.225 (0.588) 568
(8)	Number of days follows politics on internet	3.494** (1.296) 570	1.227** (0.595) 570
Individual characteristics		yes	yes
% Classmates with a partisan parent		yes	yes
Major-class fixed effects		yes	yes

Notes: (i) Each entry represents the result from a separate OLS regression; (ii) Standard errors in brackets are clustered at the classroom level; (iii) Individual characteristics include gender, race, age, income, mother education, indicators for students' pre-determined political inclination—right- and left-wing and dummies indicating whether the student has a partisan parent; (iv) ** Statistically significant at 5%, * Statistically significant at 10%.

can weaken peer effects because they occur most strongly between those who are already alike in some respects (as in Carrell *et al.*, 2013).

Result 3 There is evidence that heterogeneity of political affiliation among peers encourages individual consumption of the media and discourages casting invalid votes.

4.3 Peer engagement effects on individual engagement and political affiliation

Table 5 presents the results of the regressions that test for whether either of our two peer engagement variables (proportions, excluding self, of classmates with partisan parents and of classmates intending to watch the campaign on TV) influence individual engagement, political affiliation, knowledge and consumption of the media. Each entry again reports a separate regression result for the peer coefficient (distinguished in the column), on the dependent variable identified in the row. All regressions include controls for gender, race, age, income and mother's education indicators, political identification at t-1, a dummy for whether the student declared to have a partisan parent in t-1, major class fixed effects and for the other peer variable (i.e. the proportions of classmates that declared to be right-wing and left-wing oriented in t-1).

The one peer effect that is consistently statistically significant over the peer engagement specifications is the movement of individual political affiliation to the Centre. With the partisan parent peer variable, there is also evidence of an effect on political participation. There is also some evidence from the watch campaign peer variable that peer engagement improves individual knowledge (by reducing asymmetric mistakes over candidates). But there is no evidence that peer engagement encourages media consumption because the only significant coefficient is negative (follow 'politics on the internet' with the 'watch political campaign' peer variable).

Result 4 There is evidence that peer political engagement encourages a movement of political affiliation to the Centre (away from the extremes). There is weaker evidence that peer political engagement encourages valid voting and political knowledge.

Result 5 There is no evidence that peer engagement encourages consumption of the media.

5. Discussion

Our results are important in three respects.

First, we contribute to the debate in the literature over whether peer political identification affects individual political identification. There is mixed evidence on this, for example MacKuen and Brown (1987) on one side, and Kenny (1994), Beck (2002), and Sinclair (2009) on the other, suggesting that there is an influence. We find no evidence of such an effect (Result 1). Crucially, the earlier studies rely on correlations that do not control systematically for prior commonalities. In contrast, through the use of the experimental method we are able to control for these selection biases, by comparing behaviours among individuals randomly assigned to different peer groups. This is important not only because, once we control for these sources of similarity, we find no peer political affiliation effect, but also because our study suggests that the failure to control fully for these selection biases is, in practice, material (Result 2). It is material in the sense that the correlations without controls appear statistically significant and of considerable magnitude: an increase of 10% in the right-wing class mates would appear to increase by 3%–4% the chance of an individual declaring a right-wing political affiliation. Once we control for selection biases, this predictive power of the peer political identification disappears, yielding a very different conclusion. The absence of a peer effect is all the more notable in the context of Angrist's (2014) critique of the Sacerdote (2001) identification strategy: that is, that it can still produce a positive peer coefficient when none exists.

Table 5. Effects of political engagement on knowledge and behaviour

		Coefficient on the Peer Variable	
Dependent Variables:		% Classmates intend to watch political campaign	% Classmates with a Partisan Parent
		[1]	[2]
<i>Political Knowledge</i>			
[1]	% Correct answer in the quiz	-0.1099 (0.087) <i>571</i>	0.150 (0.138) <i>573</i>
[2]	Asymmetric mistakes	-1.635** (0.629) <i>571</i>	0.635 (0.930) <i>573</i>
[3]	Mistakes on own intended candidate	-0.0133 (0.070) <i>442</i>	-0.095 (0.102) <i>444</i>
[4]	Mistakes on remaining candidates	0.0560 (0.042) <i>562</i>	-0.093 (0.067) <i>564</i>
<i>Voting Participation and Ideology</i>			
[5]	Cast an invalid vote	0.029 (0.108) <i>561</i>	-0.294** (0.139) <i>563</i>
[6]	Centre-oriented	0.372** (0.178) <i>561</i>	0.994** (0.298) <i>563</i>
<i>Consumption of Media</i>			
[7]	Number of days follows politics on TV	0.374 (0.981) <i>568</i>	-1.043 (1.710) <i>570</i>
[8]	Number of days follows politics on newspaper	-0.586 (1.320) <i>566</i>	-1.149 (1.348) <i>568</i>
[9]	Number of days follows politics on internet	-3.422** (1.154) <i>568</i>	1.916 (1.710) <i>570</i>
Individual Characteristics		yes	yes
% Right-wing and % Left-wing classmates		yes	yes
Major-class fixed effects		yes	yes

Notes: (i) Each entry represents the result from a separate OLS regression; (ii) Standard errors in parentheses are clustered at the classroom level; (iii) Individual characteristics include gender, race, age, income, mother education, indicators for students' pre-determined political inclination—right- and left-wing and dummies indicating whether the student intended to watch political advertisement (in column 1), and has a partisan parent (in column 2); (iv) The number of observations in each regression are reported in italics; (v) ** Statistically significant at 5%, * Statistically significant at 10%.

Second, we find some weak evidence across the specifications for the peer engagement variable that students are less likely to cast invalid votes when their peers are more engaged (Result 4). In this respect, our findings echo a result in the literature with respect to social contagion in voting (Gerber *et al.*, 2008, Nickerson, 2008).²⁷

Third, we find a robust peer effect across the specifications for the peer engagement variable on individual political identification. Students with more engaged classmates tend to move to the Centre of the political spectrum and become less likely to invalidate their votes. The result is particularly interesting because it goes against the suggestion that these types of peer effects tend to reinforce initial beliefs and preferences (see Gerber *et al.*, 2012). One possible interpretation of our finding is that students acquire information about the candidates through peer contacts and when our students become better informed, they happen to identify more strongly with the Centre of the political spectrum. However, there is no evidence that the students consumed more media and only weak evidence that students became better informed about the candidates because of having engaged peers. So this seems unlikely. Since we find no evidence for peer political identification effects and anyway this is a peer engagement effect on movement to the Centre of the political spectrum, neither can this result be readily assimilated to some form of contagion of political preferences.

One possible explanation, however, is that an engaged group of students discusses politics more and this helps clarify what it means to be located on the left-right political spectrum. Such clarification will naturally produce a regression to the mean: that is the Centre in this context. If the degree of clarification depends on the intensity of the discussion and

²⁷ We replicated the test proposed by Angrist (2014) and find support for this part of Result 4. The idea behind the test, although not without controversy (see Feld and Zölitz, 2017, forthcoming), is that when pre-determined peer characteristics are included in a model that explains individual characteristics with peer effects, the presence of peer effects in the econometric sense is identical to that of a 2SLS estimator using group dummies as instrument for individual characteristics. This, in turn, is different from OLS estimates of the effect of these individual characteristics. The condition for peer effects to be present is that the 2SLS estimates should exceed the OLS estimates.

Consider the following OLS regression:

$$y_{ig} = \mu + \pi_0 x_i + \pi_1 \bar{x}_g + \varepsilon_i$$

Acemoglu and Angrist (2001) have shown that the parameter π_1 is equal to $\pi_1 = \phi(\psi_{IV} - \psi_{OLS}) \approx (\psi_{IV} - \psi_{OLS})$, where ψ_{OLS} corresponds to the coefficient of a regression of y_{ig} on x_i and ψ_{IV} is equal to the coefficient of x_i in a 2SLS IV regression of y_{ig} on x_i but using group dummies as instruments for x_i . Note that $\phi = \frac{1}{1-R^2}$ and it is in general close to one, since R^2 tends to be close to zero. So, the peer effects estimator is approximately equal to the difference between the IV and OLS estimator

This is the test that we conduct and show in Table A9 in the online Appendix. In our case, we analyse the peer effect of political engagement and the group dummies correspond to classroom dummies. We find that $\psi_{IV} > \psi_{OLS}$, indicating the presence of peer effects. (However, not only peer effects, but all factors that lead to a difference between ψ_{OLS} e ψ_{IV} also affect π_1 such as measurement error, as Feld and Zölitz [2017, forthcoming] discuss. They also argue that in the case of random assignment the impact of measurement errors are small and peer effects tend to be better estimated.)

this in turns depends on how the level of engagement in the class, then this would explain the result.

There is an interesting sense in which this possibility also blurs the distinction between peer effects that arise through information and those that occur through some kind of preference contagion or osmosis. This is an informational channel but it works on individual preferences. When people's preferences are supported by beliefs that are not held with certainty, then peer discussion has epistemic effects that are revealed in preference changes.

In short, our results are important because they suggest that there are significant peer influences on individuals but they are not the ones that encourage worries for democracy on grounds of 'group-think'. Indeed, the reverse is the case. An individual's political identification is not affected by that of their peers in our natural experiment. Peer political engagement does affect individual political identification. But this most plausibly occurs by attenuating the uncertainty over what being left- or right-wing means on this spectrum and, as a result, we observe that peer engagement encourages political identification away from the extremes towards the Centre. In so far as this is right, then these peer effects, far from damaging democracies, are likely to be beneficial because the range of dispute that democracy must bridge narrows.

6. Conclusion

Using a natural experiment on young adults, we examine whether peers influence individual preferences over political affiliation and political engagement. The question is important because one appeal of democracy in terms of its responsiveness to the 'will of the people' depends on being able to identify individuals with their preferences. If an individual's preferences depend on his or her peers, then individual 'will' in this sense has slipped its anchor in the individual. But it is also an instance of a general question that has wider importance for social science. For instance, much of economics takes individual preferences as given, the starting point for analysis, so to speak, and it would be equally damaging here to discover that an individual's preferences floated with those of their peers.

The choice of survey participants is important for our test. Our participants were young adults, embarking on a new, important and unfamiliar phase in their lives. These are precisely the uncertain and portentous circumstances where, psychologically, one might expect individual sensitivity to the cues of other. In this sense, our natural experiment was on a pool of participants where, if there are peer effects, one might expect to find them.

And we do. An individual's political identification is associated with the identification of his or her peer group in our data; and so too is an individual's political identification with the engagement of his or her peers. However, for different reasons, we argue that neither association should trouble liberal democracies; nor sound a more general warning bell for those who take individual preferences as given.

In the case of the correlation between peer political identification and individual political identification on a left-right scale, this is because we find that it disappears once we control for selection biases in the way that groups are constituted. This is an important result. Methodologically it is important because it suggests that the issues identified by Sacerdote (2001), for example, can be material in creating the false impression of peer effects when there are none. Substantively, the result is important because a direct influence on individual political identification from peer political identification could be particularly damaging for the political process.

The influence of the level of peer political engagement on individual political identification both survives these controls and is robust to different measures of peer engagement. It is real, in this sense. It also interestingly encourages movement to the Centre of the political spectrum. We argue, for a variety of reasons, that this is best explained through the likely occurrence of discussion among more engaged peers that helps clarify for individuals what being left- and right-wing mean. This should not worry supporters of democracy. Indeed, in so far as democracy functions with less difficulty when views are less polarized, then this peer effect is likely to be good for democracies.

Supplementary material

Supplementary material (the Appendix and the data files) is available online at the OUP website.

Funding

This work was supported by Insper Institute of Education and Research and the Economic and Social Science Research Council through the Network for Integrated Behavioural Science (ES/K002201/1).

Acknowledgements

We are very grateful to teachers from the Universidade de São Paulo for their support and help in the application of the survey. We thank Renata Rizzi for helping with the data collection, and Lori Beaman, Annemie Maertens, Jeff Prince, Francesco Trebbi and participants from the RES Women Committee Meeting and the 2014 EWEBE for comments.

References

- Acemoglu, D. and Angrist, J. (2001) How large are human-capital externalities? Evidence from compulsory-schooling laws, in B.S. Bernanke and K. Rogoff (eds) *NBER Macroeconomics Annual*, Vol. 15, MIT Press, Cambridge, MA.
- Angrist, J. (2014) The perils of peer effects, *Labour Economics*, 30, 98–108.
- Beck, P.A. (2002) Encouraging political defection: the role of personal discussion networks in partisan desertions to the opposition party and Perot votes in 1992, *Political Behavior*, 24, 309–37.
- Bertrand, M. and Mullainathan, S. (2004) Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination, *American Economic Review*, 94, 991–1013.
- Bond, R.M., Fariss, C.J., Jones, J.J., Kramer, A.D.I., Marlow, C., Settle, J.E., and Fowler, J.H. (2012) 61-million-person experiment in social influence and political mobilization, *Nature*, 489, 295–98.
- Carrell, S.E., Hoekstra, M., and West, J.E. (2011) Is poor fitness contagious? Evidence from randomly assigned friends, *Journal of Public Economics*, 95, 657–63.
- Carrell, S., Sacerdote, B., and West, J. (2013) From natural variation to optimal policy? The importance of endogenous peer group formation, *Econometrica*, 81, 855–82.
- Cho, W. (2003) Contagion effects and ethnic contribution networks, *American Journal of Political Science*, 47, 368–87.
- Cho, W., Gimpel, J., and Dyck, J. (2006) Residential concentration, political socialization, and voter turnout, *The Journal of Politics*, 68, 156–67.

- Feld, J. and Zölitz, U. (2017) Understanding peer effects – on the nature, estimation and channels of peer effects, *Journal of Labor Economics*, forthcoming.
- Franklin, M. (2004) *Voter Turnout and the Dynamics of Electoral Competition in Established Democracies since 1945*, Cambridge University Press, Cambridge.
- Funk, P. (2010) Social incentives and voter turnout: evidence from the Swiss mail ballot system, *Journal of European Economic Association*, 8, 1077–103.
- Gay, C. (2009) Homo politicus is not an island, in G. King, K. Scholman, and N. Nie (eds) *The Future of Political Science: 100 Perspectives*, Routledge, New York.
- Gentzkow, M. and Shapiro, J.M. (2011) Ideological segregation online and offline, *The Quarterly Journal of Economics*, 126, 1799–839.
- Gerber, A., Green, D., and Larimer, C. (2008) Social pressure and voter turnout: the results of a large scale field experiment, *American Political Science Review*, 102, 33–48.
- Gerber, A., Huber, G., Doherty, D., and Dowling, C. (2012) Disagreement and the avoidance of political discussion: aggregate relationships and differences across personality traits, *American Journal of Political Science*, 56, 849–74.
- Good, P.I. (2006) *Resampling Methods: A Practical Guide to Data Analysis*, Birkhauser, Boston, MA.
- Guryan, J., Kroft, K., and Notowidigdo, M. (2009) Peer effects in the workplace: evidence from random groupings in professional golf tournaments, *American Economic Journal: Applied Economics*, 1, 34–68.
- Hung, A. and Plott, C. (2001) Information cascades: replication and an extension to majority rule and conformity-rewarding institutions, *American Economic Review*, 91, 1508–20.
- Huckfeldt, R. (2007) Unanimity, discord, and the communication of public opinion, *American Journal of Political Science*, 51, 978–95.
- Huckfeldt, R., Beck, P.A., Dalton, R.J., and Levine, J. (1995) Political environments, cohesive social groups, and the communication of public opinion, *American Journal of Political Science*, 39, 1025–54.
- Huckfeldt, R. and Mendez, J. (2008) Moths, flames, and political engagement: managing disagreement within communication networks, *The Journal of Politics*, 70, 83–96.
- Huckfeldt, R. and Sprague, J. (1987) Networks in context: the social flow of political information, *American Political Science Review*, 81, 1197–216.
- Kenny, C.B. (1994) The microenvironment of attitude change, *The Journal of Politics*, 56, 715–28.
- Klofstad, C.A. (2009) Civic talk and civic participation: the moderating effect of individual predispositions, *American Politics Research*, 37, 856–78.
- Klofstad, C.A. (2010) The lasting effect of civic talk on civic participation: evidence from a panel study, *Social Forces*, 88, 2353–75.
- Lyle, D. (2007) Estimating and interpreting peer and role model effects from randomly assigned social groups at West Point, *Review of Economics and Statistics*, 89, 289–99.
- Lyle, D. (2009) The effects of peer group heterogeneity on the production of human capital at West Point, *American Economic Journal: Applied Economics*, 1, 69–84.
- MacKuen, M. and Brown, C. (1987) Political context and attitude change, *American Political Science Review*, 81, 471–90.
- Manski, C.F. (1993) Identification of endogenous social effects: the reflection problem, *Review of Economic Studies*, 60, 531–42.
- Marinoni, G. (2010) Voto nulo, passividade e conservadorismo, Carta Maior, August 31.
- Mutz, D.C. (2002a) The consequences of cross-cutting networks for political participation, *American Journal of Political Science*, 46, 838–55.
- Mutz, D.C. (2002b) Cross-cutting social networks: testing democratic theory in practice, *American Political Science Review*, 96, 111–26.

- Mutz, D.C. and Mondak, J. (2006) The workplace as a context for cross-cutting political discourse, *The Journal of Politics*, 68, 140–55.
- Nickerson, D.W. (2008) Is voting contagious? Evidence from two field experiments, *American Political Science Review*, 102, 49–57.
- Panagopoulos, C. (2010) Affect, social pressure and prosocial motivation: field experimental evidence of the mobilizing effects of pride, shame and publicizing voting behavior, *Political Behavior*, 32, 369–86.
- Prior, M. (2010) You’ve either got it or you don’t? The stability of political interest over the life cycle, *The Journal of Politics*, 72, 747–66.
- Sacerdote, B. (2001) Peer effects with random assignment: result for Darmouth roommates, *The Quarterly Journal of Economics*, 116, 681–704.
- Salganik, M.J., Dodds, P.S., and Watts, D.J. (2006) Experimental study of inequality and unpredictability in an artificial cultural market, *Science*, 311, 854–56.
- Satyanath, S., Voigtländer, N., and Voth, H. (2017) Bowling for fascism: social capital and the rise of the Nazi Party, *Journal of Political Economy*, forthcoming.
- Sears, D. and Funk, C. (1999) Evidence of the long-term persistence of adults’ political predispositions, *The Journal of Politics*, 61, 1–28.
- Silveira, B. and De Mello, J.M.P. (2011) Campaign advertising and election outcomes: quasi-natural experiment evidence from gubernatorial elections in Brazil, *Review of Economic Studies*, 78, 590–612.
- Sinclair, B. (2009) The multi-valued treatment effects of political networks and context: when does a Democrat vote like a Republican?, Working Paper, University of Chicago, Chicago, IL.