

The Political Influence of Peer Groups: Experimental Evidence in the Classroom *

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November 23, 2013

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? This is the question that we address with a natural experiment. We focus on the possible influence of peers on two types of individual political behaviour: political identification on a left-right spectrum and political engagement. We find no evidence that peer political identification affects individual identification. But we do find that peer engagement affects individual engagement, individual political knowledge and political identification among those who are initially least engaged. We argue this (and other evidence) is most likely to arise from peer effects on the information that individuals have and not their preferences.

*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 for comments.

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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 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 behavior.

The question matters because much in economics and liberal political theory turns on taking individual preferences as given. The appeal, for instance, of the Pareto criterion in welfare economics and of the ‘will of the people’ in politics depends on being able to identify individuals with their preferences and this becomes problematic if an individual’s preferences change with those of their peers. 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 band-wagons in the Salganik, Dodds and Watts (2006) experiment is difficult to reconcile with information transmission alone. In this paper, we report on a natural experiment where we attempt 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.

In particular, 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.¹ Where there is evidence

¹Given 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

that peer’s political identification and/or engagement affects individual political identification and/or engagement, we exploit aspects of the experimental design to consider whether it arises from a peer influence on the information that individuals have or over their preferences.

There is a large literature on peer effects in politics.² The specific evidence on peer effects on political identification is also mixed. Some studies find evidence consistent with the claim that people follow their peer’s political affiliations (Sinclair, 2009; Beck, 2002; Kenny, 1994), 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 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 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), Carrell, Hoekstra and West (2011) and Lyle (2009) use data on randomly assigned networks to identify peer effects on, respectively, student performance, physical fitness and workers’ productivity.³ The closest

has some kind of a ‘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.

²Many studies investigate how individuals’ behavior is associated with the behaviour or characteristics of their household members (Nickerson, 2008), people who live in the same geographical and residential area (Cho, 2003; Cho et al., 2006, Huckfeldt and Mendez, 2008, Huckfeldt et al., 1995, Huckfeldt and Sprague, 1987), housemates (Klofstad 2009; 2010), discussion partners (Huckfeldt, 2007, Mutz, 2002a, 2002b; Gerber et al., 2012), co-workers (Mutz and Mondak, 2006) or facebook friends (Bond et al 2012).

³In political studies, this strategy has also been used by Klofstad (2009; 2010). By examining differences among randomly assigned college dormitory roommates, he finds a positive relationship between the level of discussion of politics among freshmen and participation in civic events. Our study differs in important respects. As will be discussed in Section 2.3, we use student characteristics and preferences rather than behaviour in our peer variables. This avoids the criticism that something like the level of discussion about politics among roommates is not random, but determined by the combination of individuals’ unobservable characteristics such as personality or preferences that are correlated with civic participation. In this sense, we have a stronger case for causality. Secondly, we look at different peer characteristics to understand the relevant mechanisms of peer influence. Lastly, our study investigates effect of groups, namely classmates, rather than the effect of one roommate.

to our study are those natural and field experiments that have been used to quantify peer effects on voting turnout (Funk, 2010; Panagopoulos, 2010; Gerber, Green and Larimer, 2008, Nickerson, 2008.) Their findings are consistent with the fact that voting is contagious in social circles as people respond to social pressure by becoming more likely to vote. 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 sketched above: the ‘will of the people’ becomes a less appealing justification for democracy when individual ‘will’ floats with those of others. Our experiment enables us to distinguish between these possible explanations of peer effects on likelihood of voting, as well as other aspects of political behaviour.

We find no evidence that peer political identification influences either individual political identification or political engagement. In this respect, we find no reason to doubt the presumption that individual preferences can be taken as given. 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, willingness to vote and the political knowledge among those individuals who are initially least engaged. In particular, those individuals who are initially least engaged become more likely to vote and their political identification is more likely to move to the centre (away from the extremes) as the engagement of their peers increases. This might seem troubling for those who take preferences as given. But, it is also consistent with information transmission within the group because there is evidence that the least engaged become better informed, particularly about the candidates they are initially not inclined to vote for, and this is despite the fact that they are not more inclined to acquire information. The fact that the more informed are typically not

⁴These 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."

affected by their peers in this way also suggests an information rather than a preference link between individuals in the group underlies peer effects.

In the next section, we describe the natural experiment on freshman students at Brazil’s largest university, explain the data and we set out the model that we use for identifying 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 Experimental Design and Identification of Peer Effects

2.1 Overview

Our subjects are 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 also the largest university in Brazil and the freshman students are randomly allocated to classrooms, and hence classmates. As result, these classes plausibly represent the creation of new peer groups for the incoming students. Our strategy was to sample these freshman students early in the academic year and prior to 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 freshman students who are entering during a Presidential election year as subjects is a key aspect of the experimental design. The fact of the election makes the transition to adulthood particularly salient because voting is compulsory for everyone aged 18 or above in Brazil and the campaign that occurs between the first and final sample of individual variables is a natural political event that might cause our subjects to think about politics and so become exposed to peer effects, if there are any.

There are also 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

⁵Students in morning courses have two lectures per day, from 7:30 to 11AM, while students in

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 they are a relatively large pool of possible friends (the average size of a class is 54 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 Subjects and Method of Data Collection

USP has approximately 86,187 students enrolled and offers 229 undergraduate and graduate courses. To be enrolled, undergraduate students have to 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 instance, in 2011, the number of applicants was 138,888 and the year’s enrolment was only 10,202.⁶

Our data come from 2010 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.⁷ Since the initial process of allocating students to classes is largely random, our classes and the peer variables should be free from the more obvious sources of selection bias. Nevertheless, we check for random assignment in section 4.1.

The same survey procedure was used in all classes. An interviewer entered the evening have lectures from 7:30 to 11PM.

⁶Only 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 (Fuvest, 2011). More information about USP follows here: <http://www4.usp.br/>.

⁷The specific number of classrooms depended on students’ major class, which is a combination of major and the time of the day that students take classes – namely, 2 (for students enrolled in economics-day, economics-night, history-day, mathematics-night, physics-day, physics-night, business administration-day, law-day and sociology-night), 3 (for history-night), 4 (for architecture-day, , law-night, sociology-day), 7 (literature-day), and 8 (literature-night).

classroom about 15 minutes before the end of a class, 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 fill out, 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 clear that students should answer the survey individually. In every class, four types of questionnaires – containing exactly the same questions but in different order – were randomly distributed to students (to encourage individual answering). The large majority of students agreed to answer the survey (in a few classrooms, one or two students failed to return the filled-out survey), and 95.54% of the respondents declared that they had answered questions in a serious manner. The questions related to individual demographics, political knowledge, political identification, media consumption and their parents’ political commitments.

The first wave, pre-election, was administrated 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); the beginning of the TV presidential campaign (in July) or any of the three debates on TV (in August and September). To assess the level of discussion regarding the 2010 Presidential Election in society as a whole at these times, we conducted an engine search in the one of the largest newspapers of São Paulo, O Estado de S. Paulo, seeking stories containing the word “elections”.⁸

Figure1

During the weeks in which this wave of the survey was conducted, the word “elections” appeared in 65 stories. This number substantially increased in July, an increase which coincides with the start of the compulsory television electoral advertising,⁹ and they were 134% higher in September (month preceding the election, with a peak of 150 stories). The newspaper articles in April were mostly about possible and likely candidates entering the presidential race rather than their policy issues or candidates’ government agenda. Consistent with this, opinion polls registered with the Supreme Electoral Court show that the volatility of intended votes for the two main candidates

⁸However, we only considered stories referring to the 2010 Brazilian presidential elections, checking one by one.

⁹In Brazil, television channels are required to broadcast candidates’ advertising during weekdays, including at least a half-hour on prime time, starting around three months before election day (Da Silveira and De Mello, 2011).

– Dilma Rousseff and José Serra – increased abruptly by July (see Figure A1 in the Appendix). This suggests that people tended to form opinions and discuss politics more enthusiastically closer to election: that is, from July on. Based on this evidence, 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. 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.¹⁰¹¹ 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 on the basis of the larger initial survey of relevant individuals. Second, the panel sample has many similarities with USP students.¹² We test for whether the attrition is in any sense unbalanced or not random so as to bias results. We do this in two ways.

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).¹³ This is the level of randomization,

¹⁰The 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.

¹¹The 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 and among those that claimed to have answered a political survey before, 89.1% were found to be in the panel.

¹²We compared the characteristics of our sample with publicly available administrative records for freshmen classes. The results are presented in Table A1 in the 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.

¹³We estimate regressions of the following for:

$$Prob(Be\ on\ the\ panel) = \gamma * Peer\ Variable + major\ fixed\ effect$$

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

in which we conduct our main analysis. We find no association. According to our results, for example and importantly, 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, we simulate random panel groups. For each classroom, we randomly drew a sample without replacement, with the same size as the one observed in the panel. We repeated this process 1,000 times to obtain an empirical 90% confidence interval for each major-class. We then compared the characteristics in the actual panel with the simulated ones. Figures A2 in the Appendix shows the average values of students' characteristics for those observed in the actual panel as compared to the confidence interval that was based on the simulated panels. For most of the demographic characteristics, the panel data falls inside the confidence interval. Only two student political identification average values – centre-oriented and left-wing-oriented – fall outside the confidence interval, and for only two cases (sociology-day and law-day which account for only 6% of the observations); and only one engagement variable, the probability of casting an invalid vote, falls outside the confidence interval for two major-classes. The attrition, therefore, does not alter the sample much in these respects, but, to be sure that there is no biasing effect, we additionally 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 (observed in the first survey) are set out in Panel A. They relate to the usual demographics, whether individuals have a partisan parent and whether they intend to vote. Although voting is compulsory, there is an option of voting for no one: that is, in effect, abstaining from voting by casting an invalid vote. We use this as our measure of willingness to vote.¹⁴ In addition, to their declared political identification (left-wing, center and right-wing), we asked students to cite the three most relevant socioeconomic problems among fifteen alternatives (e.g. unemployment, high interest rates, taxes, public health system, poverty in the city, traffic jams, violence and corruption in the government). Answers to this are possibly less likely to be misreported than the self-reported political identification and so we use these responses to construct a revealed political identification index, as set out in (1) below.

¹⁴This is typically interpreted (e.g. Maringoni, 2006) as a form of protest voting.

$$RightWingIndex_{t-1i} = \left[\sum_{p=1}^3 Choice_{ipt-1} \cdot RightWing_p \right] / 3 \quad (1)$$

where $Choice_{ipt-1}$ is an indicator of individual's i choice of problem p in survey $t-1$ and $RightWing_p$ is the proportion of right-wing-oriented individuals (other than in i) who chose problem p in $t-1$.¹⁵

The summary statistics for the peer variables are given in Panel B. The peer political identification variable for each individual is based on classmates' direct responses to the political identification question (whether they identify as right-wing oriented). The peer political engagement variable is the proportion of classmates that declared that their parent prefer a particular party. We call this the partisan parent peer variable. Those with partisan parents reveal in the first survey a greater willingness to vote and watch the campaign on television than those who do not (see Table A2 in the Appendix). This is perhaps not surprising since politically committed parents seem likely to encourage political engagement among their children through discussion and the like. We use this peer variable to capture political engagement in case individuals have any tendency to misreport their level of political engagement (since it seems likely that if there is such tendency it will be much weaker when reporting on one's parent's than on one's self).¹⁶

It is important to note that although one might expect that students in a same major-class are largely homogenous, there is sizable variation in both peer variables within major-class (see Table A3 in the electronic Appendix) and this is an important ingredient for the identification of peer effects.

The outcomes (observed in the second survey) are summarized in Panel C. We use information on individual consumption of the media at this time and individual political knowledge. The latter comes from a quiz containing the same number of

¹⁵These indexes can vary between zero (i.e., no right-wing student made any of the same choices of i) and one (i.e., all right-wing students made the same choices of i). $LeftWingIndex_{t-1i}$ is analogous. To illustrate, suppose that all right-wing individuals (as self-reported in $t-1$) but i were concerned only about violence, corruption and taxes. If the individual i chose violence, public transportation and environment as his main concerns in t , then the value of his $RightWingIndex_{t-1i}$ is $1/3$ ($= [1 \times 1] + [0 \times 1] + [0 \times 1] / 3$). A higher value of the right-(left-)wing index indicates more similarity with the concerns of right-(left-)wing students. Individuals' ideologies predict their ideology indexes. We conducted regressions to determine that indicators for right- and left-wing orientation are predictors of the ideological indexes. Right-wing-oriented individuals have a higher right-wing index (28% standard deviation) and lower left-wing index (29% standard deviation), whereas left-wing individuals have a lower right-wing index (19% of standard deviation) than others.

¹⁶When we formally instrument the pre-determined interest in politics using this partisan parent variable, we obtain, effectively, the same results. These results are not shown in the paper, but are available upon request.

analogous questions about each of the main Presidential candidates, Dilma Rousseff, Jose Serra and Marina Silva.¹⁷ 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 *Assymetric Mistakes*.¹⁸ A higher value of this variable reflects more asymmetric knowledge and less knowledge about candidates. Lastly, we repeated questions on political identification and socio-economic problems in t , and construct measures of preferences, like in $t - 1$.¹⁹

Table1

3 Identification of Peer Effects

Following the literature on the identification peer effects through experimental techniques (Lyle 2007; Sacerdote 2001), we assume students’ outcomes are a function of individual and peer characteristics, as in (2).

$$Y_{mci}^t = \alpha + \beta_1 X_{mci}^{t-1} + \beta_2 \bar{X}_{c-i}^{t-1} + \beta_3 \bar{Y}_{c-i}^t + \epsilon_{mci}^t \quad (2)$$

The variable Y_{mci}^t , is the outcome at time t of individual i , enrolled in major-class

¹⁷More specifically, there were three open-ended questions about their previous political position, their party experience and their running mates, as well as four multiple-choice questions about policies previously implemented or supported by the candidates.

¹⁸This is defined as:

$$AsymmetricMistakes = |M_{Rouseff} - M_{Serra}| + |M_{Rousseff} - M_{Silva}| + |M_{Serra} - M_{Silva}|$$

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

¹⁹The political index in t is defined as:

$$RightWingIndex_{t_i} = \left[\sum_{p=1}^3 Choice_{ipt} \cdot RightWing_p \right] / 3$$

where $Choice_{ipt}$ is an indicator of individual’s i choice of problem p in survey t and $RightWing_p$ is the proportion of right-wing-oriented individuals (other than in i) who chose problem p in $t - 1$.

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 behavior 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 classroom: 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 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

²⁰i.e., since these are determined by the realization of the past classroom assignment lottery.

as a function of Y_{mci}^t . Substituting (3) in (2) and rearranging, we obtain (4), which is the reduced form to be estimated and depends only on predetermined characteristics.

$$\bar{Y}_{c-i}^t = \delta + \gamma_1 X_{mci}^{t-1} + \gamma_2 \bar{X}_{c-i}^{t-1} + \gamma_3 Y_{mci}^t + \omega_{mci}^t \quad (3)$$

$$y_{mci}^t = \phi_0 + \phi_1 X_{mci}^{t-1} + \phi_2 \bar{X}_{c-i}^{t-1} + \nu_{mci}^t \quad (4)$$

As a result, under random assignment, the coefficient ($\phi_2 = \frac{\beta_2 + \beta_3 \gamma_2}{1 - \beta_3 \gamma_3}$) 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, Kroft, and Notowidigdo (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:

$$z_{mci}^t = \alpha + \gamma \bar{z}_{c-i}^{t-1} + \delta \bar{z}_{m-i}^{t-1} + \theta_m^{t-1} + \varepsilon_{mci}^t \quad (5)$$

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.

²¹As explained by Guryan, Kroft and Notowidigdo (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."

If peers are assigned randomly, the coefficient γ should be zero. The results for the modified Sacerdote test are presented in column 1, row 1 and in column 2, row 2 of Table 2 in bold. The coefficients for the peer variables are not statistically different from zero, thus supporting the assumption that selection is not underlying our main results.²² We also checked whether changes in the proportion of peers with partisan parents and in the proportion of right-wing peers were systematically correlated with other predetermined characteristics, estimating (5), but using as dependent variables demographic characteristics and political behavior variables. The results are reported in Table 2, not in bold (rows 3-12, row 1 column 2; and column 1 row 2) .

Table2

The peer variables are not related to students' media consumption of politics or intention to invalidate their votes, which suggests that no difference exists in these predetermined political involvement across students assigned to different classes. 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

Table 3 provides the estimates of a version of equation (4)²³ using as dependent variable, individual political identification and focusing on the influence of peers right wing identification. The dependent variable is in all cases except column 7, an indicator

²²It 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 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.

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

for whether the individual self declares as right-wing in t ; and in column 7, we use the implied index of right-wing sympathy that emerges from the individual's ranking of issues (defined in (1)). Each column reports a separate regression that differs according to the controls.

Table 3

Our preferred specification is in column 4 and 7 where we control for possible sources of selection bias (including in the regressions indicators for gender, race, age, income, mother education, political identification at $t - 1$, major-class fixed effects and the proportion of classmates that declared to have a partisan parent in $t - 1$). 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.

We considered possible more complex 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 $t - 1$ (column 5). The purpose was to check whether peer effects worked specifically by reinforcing students' pre-determined preferences. Second, in 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. Again, we find no statistically significant effects. We do find, however, that individuals' own pre-determined party political identification are important in explaining political identification in t (Table 3, not in bold).

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 individual demographic characteristics and/or if there is no control for choice of subject major, the peer political identification variable becomes significant and positive. In other words, in the absence of control, 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.

4.3 Peer engagement effects on individual engagement and political affiliation

Table 4 presents the results of the regressions that test for a peer partisan parent effect on individual engagement, political affiliation, knowledge and consumption of the media. Each entry reports a separate regression result for the peer coefficient, using as dependent variable, measures presented in the rows. All regressions include controls for gender, race, age, income and mother 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 (to take into account the classroom random assignment) and the proportion of classmates that declared to be right-wing oriented in $t - 1$.

Table4

In column 1, we report results for the whole sample. There is evidence of a peer engagement effect on willingness to vote, on political identification (where it appears to encourage movement to the centre, notably from the left) and on political knowledge of candidates other than the one's preferred one, but not on consumption of the media.

Next, we supply additional insight into this partisan parent peer effect by looking at differential peer effects based on pre-determined individual engagement (self reported parent affiliation and media consumption in $t-1$). In columns 2 and 3, we repeat the analysis for distinct sub-samples of students: those that in $t - 1$, declare to have a partisan parent and those who do not have any partisan parent. The partisan parent peer effects are only statistically significant in regressions reported in column 3. Recall that individuals with partisan parents tend to consume more information initially and are less likely to cast invalid votes. They are the ones who are more engaged with politics in $t - 1$ and it seems that they are not influenced by their peers in these respects (column 2); whereas those who were less engaged with politics at $t - 1$ are influenced by their peers in these respects (column 3).

In Columns 4 and 5 split the sample into those who are low media consumers in $t - 1$ and those who are high media consumers.²⁴ Consistent with results in columns 3 and

²⁴The variable consumption of media indicates the number of days the individual follows the news on TV, newspapers and Internet. We summed the number of days across all media (TV, newspapers and Internet) and define High media consumers as those that consume above the median (10 days of

4, the partisan parent peer effects appear to be strongest for the low media consumers (e.g. effects on political identification and willingness to vote are significant at 95% level only for this sub-group) but in general they are less apparent (e.g. there is no significant peer effect for either sub-group’s political knowledge).

Result 3: *Individuals who were initially less engaged with politics are the most affected by the level of engagement of their peers. This peer effect encourages voting, a movement of political affiliation to the centre (away from the extremes) and political knowledge (specifically of candidates that they were not planning to vote for at the outset).*

Result 4: *There is no evidence among the least engaged that an individual’s consumption of the media is affected by the level of political engagement of his or her peers.*²⁵

5 Discussion

Our results are important in two 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: e.g. 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, we are able through the use of the experimental method 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). Looking only at simple correlations, we find that the political affiliation of a person’s peer group is a good predictor of that person’s political affiliation. For example, an increase of 10% in the right-wing class mates would appear to increase by 9.1% the chance of an individual

media or more) and as Low Media consumers those that consume less than the median (less than 10 days).

²⁵There is some evidence, however, that the initially more engaged reduce their consumption of the media (column 2, row 8 and column 5, row 9)

declaring a right wing political affiliation. However, once we control for selection biases, the predictive power of the peer political identification disappears, yielding a very different conclusion.

Second, we find evidence of a different kind of peer effect that is largely new to the literature: individuals who are initially less politically engaged are affected by the level of political engagement among their peers. In one respect this echoes a result in the literature with respect to social contagion in voting (Gerber, Green and Larimer 2008, Nickerson, 2008), but our result is more complicated in two respects. One is that the peer effect we identify is more specific in the sense that those who are initially less engaged are affected by their peers but others are generally not. The other is that the peer effect is more pervasive in the sense that not only is voting decision among the initially less engaged influenced by peers for this sub-group, so is their political identification and political knowledge (Result 3).

One possible interpretation of these findings is that the preferences among the initially less engaged change through interaction with the more engaged. The group dynamics are such that people within the group become more alike in their preferences. This interpretation, however, is difficult to reconcile with other aspects of our findings. For instance, if individual preferences bend with those of their peers, why is there no evidence of this with respect to the influence of peer political identification (Result 1)? Surely, one might expect such preference osmosis to occur more readily between peer and individual political identification than between peer engagement and individual political identification. Further, if there is preference osmosis at work, why does it only operate for those who are initially less engaged? Finally, while a change in individual preference for political knowledge might explain why some individuals become better informed, this is difficult to reconcile with the absence of any such peer effect on media consumption (Result 4).

The alternative interpretation that avoids these difficulties is that the least engaged learn something about the candidates in the election from those who are more engaged through social interaction with their peers. This explains why the effect only works one way since interacting with someone who knows less does not corrode the knowledge of those who know more. It also explains why the less engaged become better informed without actually becoming more inclined to engage explicitly in information acquisition through more media consumption (i.e. the improvement is a by-product of social interaction). Being better informed can also explain why they might be more inclined to vote since they are now in a better position to decide between candidates (see Lassen, 2005, and Gentzkow, 2006, for evidence that information causes political

participation). Finally, the fact that they specifically learn about candidates who they were not initially inclined to vote for makes it more likely that their affiliation will move to the centre of the political spectrum (because, in effect, of regression to the mean). Again this is consistent with other findings in the literature (e.g. see Banerjee et al., 2010).

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 experiment. Peer political engagement does affect individual engagement and political identification, but only among those who are initially least engaged. While these peer effects could arise because individual preferences exhibit ‘group-think’ malleability, they, more plausibly, reflect information transmission between individuals about the political process. In so far as this is right, then these peer effects, far from damaging democracies, are likely to be beneficial because democracies seem likely to function better when people are both better informed about candidates and more inclined to vote on the basis of this better knowledge.²⁶

6 Conclusion

Using a natural experiment on young adults, we examine whether peers influence individual preferences over political affiliation and political engagement. The particular question is important because the 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

²⁶There is a literature concerned with how individuals respond to heterogeneity of substantive belief in their group that we are also able to address in this experiment. 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. In a different vein, Mutz and Mondak, (2006) and Mutz (2002b) find that heterogeneous groups encourage tolerance. We examined whether heterogeneity in political affiliation within a peer group had any effect on individual political engagement and political affiliation. It did not as far as political affiliation is concerned, but we did find, contrary to Mutz (2002a), that heterogeneity appeared to discourage casting an invalid vote (the results are reported in Table A5 in the electronic Appendix).

with those of their peers.

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 engagement with the engagement of his or her peers. But for different reasons we argue that neither association should trouble liberal democracies; or 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, we find that this disappears once we control for selection biases in the way that groups are constituted. The more wide ranging association between peer political engagement and both individual engagement and individual political identification survives these controls for selection biases. It is real, in this sense. But we have argued it is best explained as a consequence of the transmission of information about candidates between those in a group who initially know more about the political process and those who know less. As such these peer influences seem a natural by-product of social interaction rather than an example of preference conformism within a group and so should not worry supporters of democracy. Indeed, the reverse is the case because this type of peer influence is likely to be good for the functioning of democracies. Indeed, the reverse is the case because this type of peer influence is likely to be good for the functioning of democracies. This is an especially striking conclusion. Our subjects were young adults, embarking on a new, important and unfamiliar phase in their lives and these seem to be precisely the uncertain and portentous circumstances where one might expect individual sensitivity to the cues of others.

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Figure 1- Number of stories containing the word “Elections” in the Newspaper *O Estado de São Paulo*

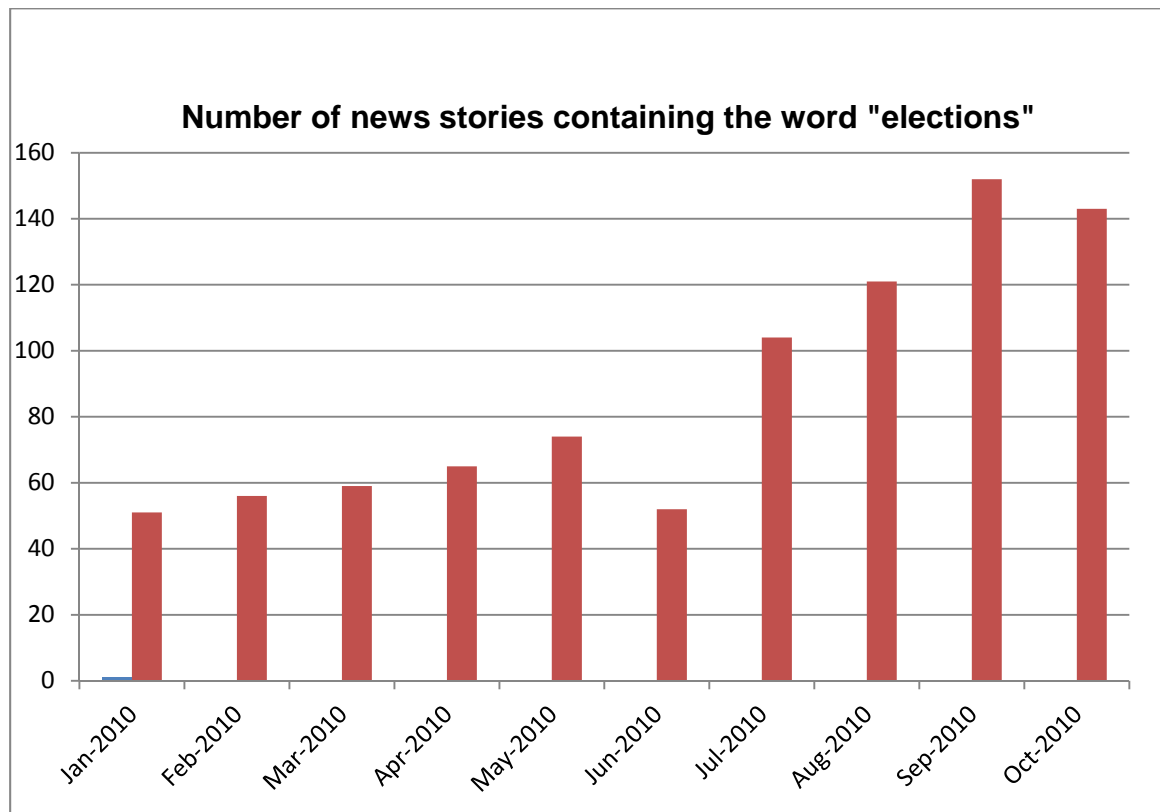


Table 1 – Summary Statistics Individual and Classroom Level

	Mean	Stand Dev	Min	Max	Obs
Panel A: Pre-determined characteristics and preferences					
Individual Characteristics					
Female	0.491	0.500	0	1	622
White	0.781	0.414	0	1	622
Mother has a college degree	0.620	0.486	0	1	622
Have a partisan parent	0.510	0.500	0	1	547
Age	20.880	5.000	17	60	626
Right-wing	0.228	0.420	0	1	623
Center	0.402	0.491	0	1	623
Left-wing	0.370	0.483	0	1	623
Right-wing Index	0.376	0.107	0.032	0.614	633
Left-wing Index	0.362	0.101	0.056	0.658	633
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
Panel B: Classroom Composition (Peer variables)					
Have a partisan parent	0.489	0.090	0.29	0.71	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
Number of Respondents (in t)	27.282	11.863	9	52	39
Panel C: Outcomes					
Cast an Invalid Vote	0.058	0.233	0	1	625
Asymmetric Mistakes	3.460	1.852	0	10	634
Mistakes on Own Intended Candidate	0.308	0.161	0	0.71	486
Mistakes on Remaining Candidates	0.301	0.132	0	0.67	622
Right-wing	0.240	0.427	0	1	622
Center	0.364	0.481	0	1	622
Left-wing	0.396	0.489	0	1	622
Right-wing Index	0.36	0.11	0.03	0.57	472
Left-wing Index	0.36	0.100	0.06	0.66	472
Number of days follows politics on TV	3.490	2.125	0	7	632
Newspaper	2.590	2.167	0	7	630
Internet	4.620	2.152	0	7	631
College Majors					
Architecture	0.047	0.212	0	1	635
Business Administration	0.063	0.243	0	1	635
Economics	0.083	0.277	0	1	635
History	0.047	0.212	0	1	635
Law	0.203	0.403	0	1	635
Literature	0.066	0.249	0	1	635
Mathematics	0.031	0.175	0	1	635
Physics	0.115	0.319	0	1	635
Sociology	0.342	0.475	0	1	635

Notes: 1) The sample refers to students in the panel. 2) The explanation of political knowledge and the right- and left-wing indexes are in the text.

Table 2 - Tests for Random Assignment of Peers Among Classrooms

		<u>Coefficient [Stand Error] on Peer Variable:</u>	
		Has a partisan parent	Right wing
<u>Dependent Variable: Pre-determined individual characteristic a</u>		[1]	[2]
[1]	Has a partisan parent	-0.0537 [0.1120]	-0.3068 [0.1449]**
[2]	Right-wing	-0.2507 [0.0964]**	0.0896 [0.0822]
[3]	Center-oriented	-0.2147 [0.1640]	0.0065 [0.2080]
[4]	Right-wing index	-0.0266 [0.0394]	0.0148 [0.0491]
[5]	Left-wing index	-0.0092 [0.0320]	0.0207 [0.0505]
[6]	Number of days follows politics on TV	0.1407 [0.8045]	-0.01 [1.083]
[7]	Intends to watch Political Campaign on TV	0.155 [0.1242]	-0.0863 [0.2240]
[8]	Intends to cast an invalid vote	0.0239 [0.1090]	0.0246 [0.1383]
<u>Demographics</u>			
[9]	Female	-0.0219 [0.2023]	-0.1816 [0.1785]
[10]	Mother has a college degree	-0.1065 [0.1295]	-0.1023 [0.1933]
[11]	Age	0.9305 [3.0249]	-2.732 [2.7207]
[12]	White	-0.0981 [0.1166]	0.1471 [0.1430]

Notes: 1) The Table reports OLS estimates from separate regressions of the relevant pre-determined individual characteristic on the proportion of classmates that have a partisan parent (Column 1) and the proportion of right-wing classmates (Column 2). All regressions include major-class fixed effects and average value of the peer characteristics among students in the same major-class (excluding himself). 2) Standard errors clustered at the classroom level are in brackets; ** 5% significance, * 10% significance.

Table 3: Impact of Classmates' Ideologies on Students Political Orientations

Selected Controls	Dependent Variable: Self-declaring Right-wing by Election Time						Right-W Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Peer Variables</i>							
% Right-Wing Classmates	0.9166	0.3322	0.065	0.0007	-0.0459	-0.0163	0.0964
	[0.0337]**	[0.1131]**	[0.2365]	[0.2637]	[0.2639]	[0.3907]	[0.0885]
% Right-Wing Classmates X Right-wing					0.1945		
					[0.3839]		
% Right-Wing Classmates with a Partisan Parent						-0.2427	
						[0.1834]	
<i>Pre-determined ideologies</i>							
Right-wing		0.5260	0.5080	0.5005	0.4472	0.4973	
		[0.0460]**	[0.0494]**	[0.0520]**	[0.1300]**	[0.0535]**	
Left-wing		-0.1060	-0.1072	-0.0851	-0.0863	-0.0927	
		[0.0253]**	[0.0270]**	[0.0294]**	[0.0292]**	[0.0305]**	
Center (omitted)							
Right-wing Index							0.4195
							[0.0604]**
Left-wing Index							0.0222
							[0.0826]
<u>Additional Controls</u>							
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	497	497	474	375

Notes: 1) Each column represents the result from a separate OLS regression. 2) Standard errors in brackets are clustered at the classroom level.

3) Right-W Index and Left-W Index are constructed ideological indexes explained in Section 2.3.

4) Individual characteristics include gender, race, age, income, mother education and indicators for students' pre-determined inclination - right- and left-wing.

5) ** Statistically significant at 5%; * Statistically significant at 10%

Table 4 - Partisan Parent Peer Effects

Coefficient on the Variable Proportion of Classmates with a Partisan Parent						
		Sample		Consumption of media		
		All	Has a Partisan Parent		Number of days per week in any media outlet (in t-1)	
			Yes	No	Less than 10 days	More than 10 days
Dependent Variables:		[1]	[2]	[3]	[4]	[5]
<u>Political Knowledge</u>						
[1]	Asymmetric Mistakes	-0.5128	1.000	-2.590	-2.175	-0.0322
		[0.5055]	[1.2966]	[1.3050]**	[1.595]	[1.424]
		506	254	252	223	283
[2]	Mistakes on Own Intended Candidate	0.3919	0.0762	0.0083	0.0705	0.0819
		[0.7010]	[0.1316]	[0.1570]	[0.2419]	[0.1333]
		402	210	192	161	241
[3]	Mistakes on Remaining Candidates	-1.016	-0.2379	-1.0933	-0.9955	-0.7068
		[0.4835]**	[0.5880]	[0.4379]**	[0.6552]	[0.4315]
		500	253	247	220	280
<u>Voting Participation and Ideology</u>						
[4]	Cast an Invalid Vote	-0.2322	0.0442	-0.466	-0.4980	0.0306
		[0.1049]**	[0.0871]	[0.1710]**	[0.1734]**	[0.1379]
		496	252	244	216	280
[5]	Center-Oriented	0.4389	0.2444	0.7689	0.4636	0.4157
		[0.1621]**	[0.2796]	[0.3293]**	[0.2958]	[0.2311]*
		497	250	247	218	279
[6]	Left-wing Index	-0.1369	-0.1056	-0.1678	-0.2722	-0.1612
		[0.0603]**	[0.1102]	[0.0616]**	[0.0498]**	[0.0890]*
		376	186	190	169	207
[7]	Right-wing Index	-0.0784	-0.1391	-0.0662	0.0345	-0.1786
		[0.0583]	[0.1119]	[0.0719]	[0.0781]	[0.0979]*
		376	186	190	169	207
<u>Consumption of Media</u>						
[8]	Number of days follows politics on TV	-1.3711	-3.7228	0.9683	-2.959	-2.2801
		[1.003]	[1.479]**	[1.468]	[2.1147]	[1.6696]
		503	252	251	283	220
[9]	Number of days follows politics on Newspaper	-0.2970	-0.6117	0.4862	2.4767	-3.789
		[1.0532]	[2.082]	[1.3789]	[1.341]*	[1.4472]**
		501	252	249	281	220
[10]	Number of days follows politics on Internet	0.5260	0.2559	0.7812	-0.3608	-0.9321
		[1.537]	[2.3965]	[1.573]	[1.2097]	[2.1695]
		503	252	250	283	220
	Individual Characteristics	yes	yes	yes	yes	yes
	% Right-Wing Classmates	yes	yes	yes	yes	yes
	Major-Class Fixed effects	yes	yes	yes	yes	yes

Notes: 1) Each entry represents the result from a separate OLS regression. 2) Standard errors in brackets are clustered at the classroom level.

3) Individual characteristics include gender, race, age, income, mother education, indicators for students'

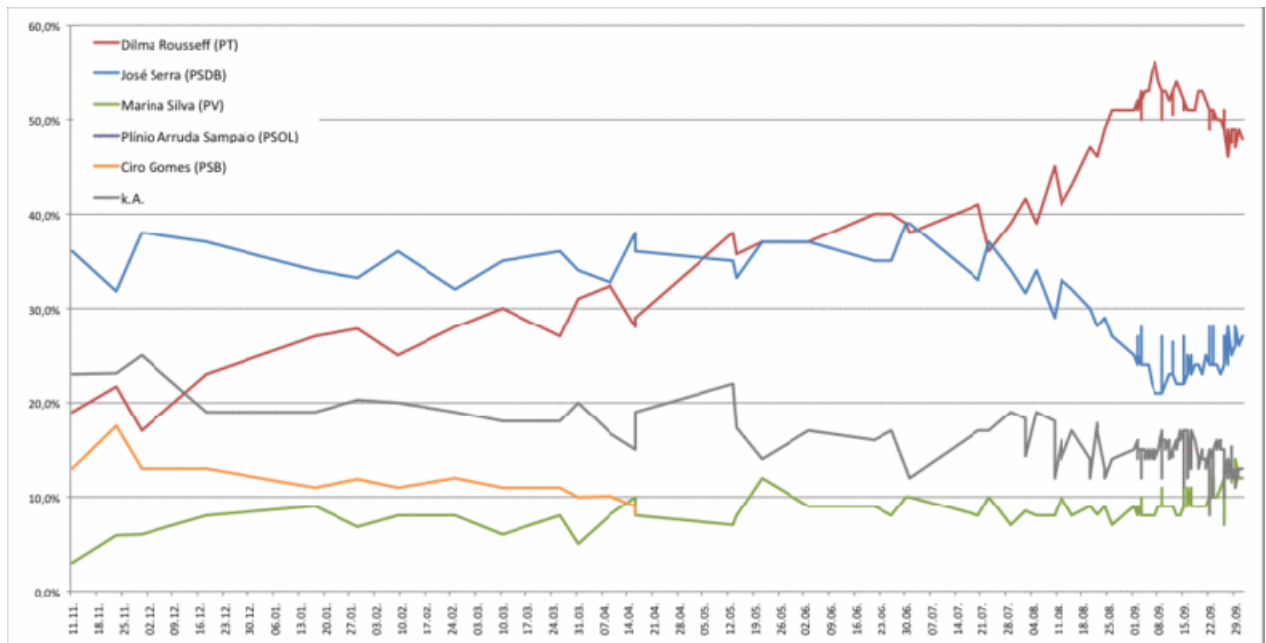
pre-determined political inclination - right- and left-wing and dummies indicating whether the student has a partisan parent.

4) The number of observations in each regression are reported in italics.

5) ** Statistically significant at 5%; * Statistically significant at 10%

Appendix

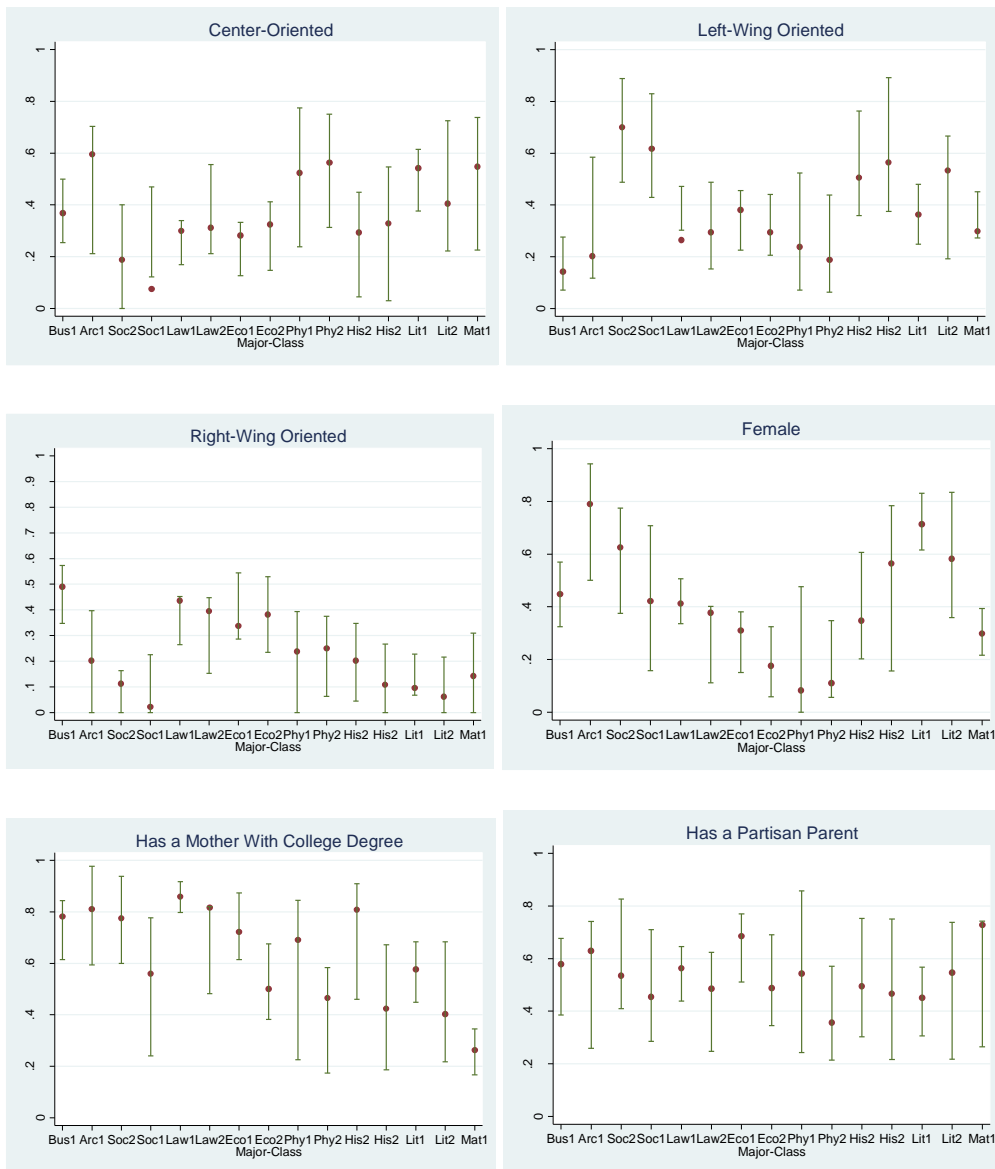
Figure A1: Presidential Race in Brazil 2010: Opinion Polls – Vote Intention – Nov/2009 to Sept/2010



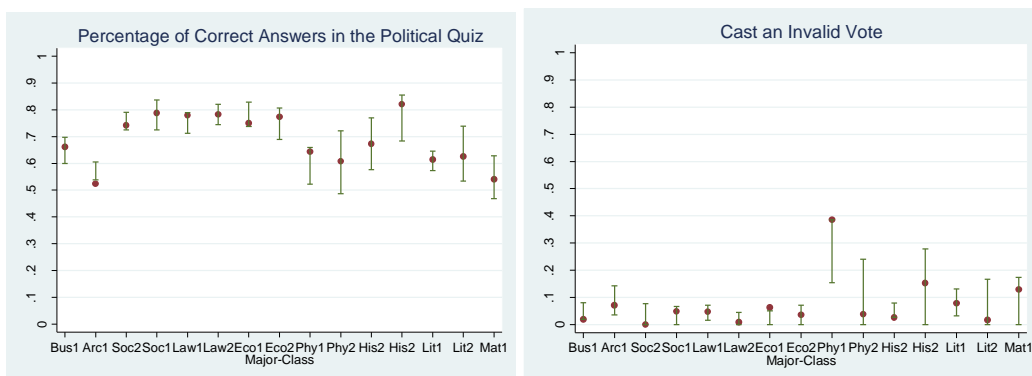
Source: Supreme Electoral Court

Figures A2 - Actual and Simulated Data – Actual Means and Simulated Confidence Intervals – Outcome and Predetermined Variables

Pre-determined characteristics



Outcomes



Note: 1) Bus, Arc, Soc, Law, Eco, Phy, His, Lit and Mat refers respectively to majors Business Administration, Architecture, Sociology, Law, Economics, Physics, History, Literature and Math. 1 and 2 refers to day and night. 2) Figures above show the average characteristics of the respondents in the actual panel by major-class. Means are positioned with respect to the confidence interval constructed based on the simulated random panel groups as explained in the text.

Table A1 - Means reported in the Sample and in USP Adminisitrative Reports

Characteristics	Major - Class														
	Business morning	Architecture morning	Sociology		Law		Economics		Physics		History		Literature		Mathematics
			morning	evening	morning	evening	morning	evening	morning	evening	morning	evening	morning	evening	morning
Female															
(A) First Survey	48.2	73.0	51.9	39.3	50.0	26.7	26.3	20.0	29.7	24.3	39.1	45.3	71.7	58.1	29.7
(B) Panel	46.3	76.7	44.1	61.1	41.5	37.5	30.8	17.6	7.7	11.8	35.0	52.4	72.0	58.2	35.0
(C) Administrative records	41.0	70.0	54.0	38.2	46.2	36.6	26.7	15.6	23.3	17.0	46.9	35.7	68.5	52.2	38.8
p-value ([B]=[C])	0.50	0.40	0.56	0.48	0.51	0.88	0.59	0.76	0.07	0.52	0.29	0.15	0.35	0.33	0.73
p-value ([A]=[C])	0.10	0.50	0.77	0.83	0.31	0.00	0.94	0.32	0.27	0.31	0.21	0.07	0.27	0.07	0.06
Mother has college education															
(A) First Survey	71.4	78.4	74.5	49.5	82.9	66.3	73.7	51.8	50.0	40.5	69.2	44.2	55.1	41.6	26.4
(B) Panel	80.0	80.0	55.9	77.8	86.3	72.2	71.8	50.0	69.2	47.1	80.0	42.9	56.6	38.0	30.0
(C) Administrative records	64.0	61.3	54.0	47.3	67.6	55.3	55.6	50.0	41.7	35.0	53.1	39.2	42.9	31.1	34.7
p-value ([B]=[C])	0.02	0.02	0.03	0.33	0.00	0.00	0.03	1.00	0.06	0.35	0.01	0.74	0.00	0.24	0.66
p-value ([A]=[C])	0.05	0.00	0.00	0.68	0.00	0.00	0.00	0.75	0.19	0.50	0.01	0.33	0.00	0.00	0.08
Mother has high school education															
(A) First Survey	22.9	18.6	17.6	29.7	13.7	24.7	21.1	34.1	35.9	32.4	20.0	34.7	29.3	34.2	40.7
(B) Panel	15.0	16.7	26.5	16.7	13.7	25.0	25.6	38.2	15.4	29.4	5.0	19.0	27.6	42.3	35.0
(C) Administrative records	24.0	20.7	28.0	26.3	17.8	22.5	17.8	33.3	35.0	30.0	21.6	32.1	27.2	34.4	40.7
p-value ([B]=[C])	0.12	0.56	0.23	0.98	0.41	0.63	0.28	0.56	0.08	0.96	0.00	0.15	0.92	0.19	0.61
p-value ([A]=[C])	0.75	0.59	0.06	0.49	0.12	0.51	0.49	0.87	0.88	0.76	0.75	0.59	0.46	0.95	0.99
Income (up to 5 min. wage)															
(A) First Survey	5.9	6.1	8.2	20.5	6.5	12.3	12.5	13.1	30.0	32.4	19.0	37.9	27.8	27.4	41.8
(B) Panel	2.4	6.9	12.1	13.3	4.1	9.0	8.1	18.2	33.3	23.5	21.1	42.9	28.3	28.4	55.0
(C) Administrative records	8.0	16.7	20.0	19.0	8.8	12.0	16.6	19.9	31.8	46.0	24.8	39.3	41.4	44.8	57.1
p-value ([B]=[C])	0.03	0.05	0.48	0.24	0.11	0.39	0.07	0.80	0.92	0.05	0.70	0.75	0.00	0.00	0.86
p-value ([A]=[C])	0.30	0.00	0.00	0.74	0.22	0.92	0.30	0.07	0.76	0.09	0.25	0.78	0.00	0.00	0.00
Income (5 up to 10 min. wage)															
(A) First Survey	23.5	31.3	28.6	22.7	18.8	21.3	20.8	27.4	28.3	48.6	34.9	28.4	37.8	38.1	35.2
(B) Panel	26.8	24.1	27.3	40.0	20.4	17.9	24.3	27.3	41.7	64.7	36.8	28.6	42.0	41.8	30.0
(C) Administrative records	31.0	28.7	28.0	30.9	26.3	25.5	26.6	23.3	30.0	30.0	38.4	30.0	26.6	30.9	24.5
p-value ([B]=[C])	0.55	0.58	0.37	0.65	0.32	0.11	0.75	0.62	0.45	0.01	0.89	0.89	0.00	0.08	0.61
p-value ([A]=[C])	0.04	0.58	0.93	0.07	0.01	0.20	0.24	0.41	0.78	0.03	0.57	0.74	0.00	0.03	0.04
Income (10 up to 20 min. wage)															
(A) First Survey	29.4	35.4	34.7	28.4	26.5	31.6	31.9	31.0	35.0	18.9	25.4	20.0	24.5	24.3	18.7
(B) Panel	29.3	41.4	24.2	40.0	28.6	34.3	37.8	27.3	25.0	11.8	26.3	14.3	20.3	19.4	10.0
(C) Administrative records	32.0	31.3	27.0	23.6	28.9	30.6	25.5	35.6	30.0	16.0	24.7	18.6	19.7	19.2	12.3
p-value ([B]=[C])	0.71	0.29	0.34	0.93	0.96	0.53	0.14	0.30	0.71	0.61	0.88	0.59	0.86	0.97	0.74
p-value ([A]=[C])	0.51	0.40	0.27	0.32	0.48	0.79	0.25	0.36	0.42	0.66	0.90	0.74	0.09	0.07	0.12
Income (above 20 min. wage)															
(A) First Survey	41.2	27.3	28.6	28.4	48.2	34.8	34.7	28.6	6.7	0.0	20.6	13.7	10.0	10.2	4.4
(B) Panel	41.5	27.6	36.4	6.7	46.9	38.8	29.7	27.3	0.0	0.0	15.8	14.3	9.4	10.4	5.0
(C) Administrative records	29.0	23.3	25.0	26.5	36.0	31.9	31.3	21.2	0.1	0.1	12.1	12.1	12.3	5.1	6.1
p-value ([B]=[C])	0.12	0.62	0.02	0.25	0.14	0.25	0.84	0.45	---	---	0.67	0.78	0.25	0.16	0.83
p-value ([A]=[C])	0.00	0.38	0.59	0.69	0.00	0.45	0.55	0.14	0.64	---	0.10	0.66	0.23	0.01	0.43

Table A2 – Students’ Pre-determined Characteristics According to Parents’ Preferences

	All	Number of	Has a	Does Not Have a	
		Observations	Partisan Parent	Partisan Parent	
	[1]	[2]	[3]	[4]	[3]- [4]
<u>Demographics</u>					
Female	0.464 [0.498]	1,556	0.456 [0.498]	0.458 [0.498]	-0.002
Mother has a college degree	0.570 [0.495]	1,565	0.629 [0.483]	0.521 [0.499]	0.108 **
Age	21.840 [6.40]	1,548	21.330 [5.56]	22.320 [7.02]	-0.990 **
White	0.763 [0.425]	1,545	0.771 [0.420]	0.769 [0.421]	0.002
<u>Political Preferences and Habits</u>					
Has a partisan parent	0.491 [0.500]	1,374			
Right-wing oriented	0.206 [0.404]	1,557	0.229 [0.420]	0.187 [0.390]	0.042 *
Center-oriented	0.386 [0.487]	1,557	0.283 [0.451]	0.469 [0.499]	-0.186 **
Left-wing oriented	0.407 [0.491]	1,557	0.486 [0.500]	0.343 [0.475]	0.143 **
Intends to watch Political Campaign on TV	0.426 [0.494]	1,570	0.495 [0.500]	0.387 [0.487]	0.108 **
Intends to Cast and Invalid Vote	0.183 [0.387]	1,549	0.144 [0.351]	0.187 [0.390]	-0.043 **

Notes: 1) Standard errors in brackets are clustered at the classroom level. 2) The statistics refer to information provided in the first survey.

3) ** Statistically significant at 5%; * Statistically significant at 10%

Appendix A3- Descriptives

Major-period	Number of Classrooms	Average Number of Respondents per Classroom	Variance by Peer Variable		Allocation Rule
			% Right-wing	% has a partisan parent	
Architecture - day	4	26.5 [3.52]	0.1413	0.2436	Random algorithm
Business - day	4	35.85 [6.57]	0.2436	0.2501	Alphabetical order
Economics - day	2	37.5 [0.50]	0.2447	0.2382	Alphabetical order
Economics - night	2	42.5 [0.50]	0.2413	0.2533	Alphabetical order
History - day	2	32.5 [0.50]	0.1528	0.2533	Random algorithm
History - night	3	35.47 [10.36]	0.0794	0.254	Random algorithm
Law - day	3	45.52 [7.23]	0.2311	0.2483	Alphabetical order
Law - night	4	49.15 [15.26]	0.21	0.247	Alphabetical order
Literature - day	6	38.88 [8.18]	0.1296	0.2459	Random algorithm
Literature - night	6	31.23 [7.57]	0.0803	0.2509	Random algorithm
Mathematics - nigh	2	46.97 [4.49]	0.1391	0.2529	Alphabetical order
Physics - day	2	37.4 [11.91]	0.1615	0.2543	Random algorithm
Physics - night	2	21.57 [6.59]	0.1576	0.254	Random algorithm
Sociology - day	2	26.29 [4.87]	0.0765	0.2414	Alphabetical order
Sociology - night	4	23.53 [4.23]	0.0919	0.2532	Alphabetical order
All Classrooms	48	37.02 [11.13]	0.1716	0.2501	

Appendix A4 -Effects of Heterogeneous Groups

	(1)	(2)	(3)	(4)
<u>Political Knowledge</u>				
Asymmetric Mistakes	-0,089 (0,551) <i>506</i>	-0,031 (0,552) <i>506</i>	-0,160 (0,545) <i>506</i>	-0,160 (0,542) <i>506</i>
Mistakes on Own Intended Candidate	-0,027 (0,039) <i>402</i>	-0,028 (0,039) <i>402</i>	-0,029 (0,039) <i>402</i>	-0,028 0,039 <i>402</i>
Mistakes on Remaining Candidates	0,013 (0,029) <i>500</i>	0,018 (0,030) <i>500</i>	0,011 (0,029) <i>500</i>	0,011 (0,029) <i>500</i>
<u>Voting Participation and Ideology</u>				
Cast an Invalid Vote	-0,169 (0,052)** <i>496</i>	-0,164 (0,052)** <i>496</i>	-0,168 (0,051)** <i>496</i>	-0,168 (0,052)** <i>496</i>
Center-Oriented	-0,043 (0,142) <i>497</i>	-0,057 (0,142) <i>497</i>	-0,046 (0,143) <i>497</i>	-0,046 (0,142) <i>497</i>
Left-wing Index	0,015 (0,036) <i>376</i>	0,021 (0,037) <i>376</i>	0,014 (0,036) <i>376</i>	0,014 (0,036) <i>376</i>
Right-wing Index	0,062 (0,043) <i>376</i>	0,067 (0,043) <i>376</i>	0,057 (0,043) <i>376</i>	0,057 (0,043) <i>376</i>
Individual Characteristics	yes	yes	yes	yes
% Right-Wing Classmates	no	no	yes	yes
% Partisan parent Peers	no	yes	no	yes
Major-Class Fixed effects	yes	yes	yes	yes

Notes: 1) Each entry represents the result from a separate OLS regression. 2) Standard errors in brackets are clustered at the classroom level.

3) 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.

4) The number of observations in each regression are reported in italics.

5) The peer variable of cross-cutting information is constructed as the percentage of individuals in the classroom, minus the own subject, that have a different political view from the subject.