I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14005

Friendship Networks and Political Opinions: A Natural Experiment among Future French Politicians

Yann Algan
Nicolò Dalvit
Quoc-Anh Do
Alexis Le Chapelain
Yves Zenou

DECEMBER 2020

I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14005

Friendship Networks and Political Opinions: A Natural Experiment among Future French Politicians

Yann Algan
Sciences Po, CEPR and IZA

Nicolò Dalvit
Sciences Po

Quoc-Anh Do
Northwestern University, Sciences Po and CEPR

Alexis Le Chapelain

Yves Zenou
Monash University, CEPR and IZA

DECEMBER 2020

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Friendship Networks and Political Opinions: A Natural Experiment among Future French Politicians*

We study how social interaction and friendship shape students' political opinions in a natural experiment at Sciences Po, the cradle of top French politicians. We exploit arbitrary assignments of students into short-term integration groups before their scholar cursus, and use the pairwise indicator of same-group membership as instrumental variable for friendship. After six months, friendship causes a reduction of differences in opinions by one third of the standard deviation of opinion gap. The evidence is consistent with a homophily-enforced mechanism, by which friendship causes initially politically-similar students to join political associations together, which reinforces their political similarity, without exercising an effect on initially politically-dissimilar pairs. Friendship affects opinion gaps by reducing divergence, therefore polarization and extremism, without forcing individuals' views to converge. Network characteristics also matter to the friendship effect.

JEL Classification: C93, D72, Z13

Keywords: political opinion, polarization, friendship effect, social networks, homophily, extremism, learning, natural experiment

Corresponding author:
Yves Zenou
Department of Economics
Monash University
Caulfield East VIC 3145
Australia
E-mail: yves.zenou@monash.edu

* We thank valuable comments from Alberto Alesina, Ghazala Azmat, Roland Bénabou, Yann Bramoullé, Emily Breza, Jing Cai, Arun Chandrasekhar, Aureo De Paula, Ruben Durante, Georgy Egorov, Ruben Enikolopov, Marcel Fafchamps, Gigi Foster, Jeff Frieden, Matt Gentzkow, Ben Golub, Sanjeev Goyal, Nicolaj Harmon, Emeric Henry, Yannis Ioannides, Emir Kamenica, Ethan Kaplan, Rachel Kranton, Eliana La Ferrara, Horacio Larreguy, Alexandre Mas, Friederike Mengel, Markus M. Möbius, Nancy Qian, Gautam Rao, Imran Rasul, Andrei Shleifer, Erik Snowberg, Enrico Spolaore, Andreas Steinmayr, Uwe Sunde, Adam Szeidl, Fabian Waldinger, Noam Yuchtman, and from seminar and conference participants at Kellogg SoM, UIUC, Adansonia conference at U Bocconi, HSE Moscow, NES, UBC, CREST Political Economy workshop, UNSW, Monash U, NUS, Queen Mary U, U of Copenhagen, Royal Holloway, Brazilian Econometric Society meeting, LMU Munich, Harvard U, U of Chicago, U Autònoma de Barcelona, Monash U Symposium on Social and Economic Networks, Barcelona Summer Forum, the Social Impacts of Economic Policies and Change conference in Heidelberg, the AEA Meeting, the Information Transmission in Networks conference at Harvard, and Aix-Marseille School of Economics. We are thankful for Sri Srikandan's assistance with the survey website. Algan acknowledges support from the European Research Council Grant 647870. Do is grateful for the generous hospitality of Harvard University's Department of Economics and Weatherhead Center For International Affairs, and financial support from the French National Research Agency's (ANR) "Investissements d'Avenir" grants ANR-11-LABX-0091

1 Introduction

The recent rise of populism and political polarization has attracted a burgeoning research area on the related role of social interactions in social networks. Several authors attribute political polarization to the rise of social media (e.g., Sunstein, 2009, 2018; Pariser, 2011), which fosters echo chambers that facilitate more interactions between like-minded individuals, thus strengthen polarization of views. Others debate the quantitative importance of such mechanism (e.g., Boxell et al., 2018; Allcott and Gentzkow, 2017; Guess et al., 2018) A key understudied input in this heated debate remains the causal impact of social interactions in social networks on political opinions, since echo chambers cannot imply the polarization of opinions in absence of such impact.

This paper provides estimates of such impact that address the endogeneity bias due to individuals' choices to interact and form networks, by exploiting a natural experiment at the elite French Institute of Political Studies, Sciences Po, that quasi-randomly allocates first-year students into groups at the beginning of their studies. In the spirit of Lazarsfeld et al.'s (1944) seminal description of friends' influence on US voters, we extend beyond average group effects to focus on the role of friendship between individuals in the same group. We investigate how the friendship between two individuals affects the chance that their political opinions converge or diverge. We explore how friendship influences individual choices of shared activities, and how it may reinforce friendship's effect on political opinions.

Sciences Po, the chosen setting of this study, is renowned for its central role in the formation of top French politicians since World War II, many of whom have reportedly shaped their political views during their time at Sciences Po.[1] Compared with other French higher education institutions, Sciences Po students are much more enthusiastic and proactive in political movements and political associations, and have more exposure to politically-oriented activities.

A major concern regarding the empirical relationship between students' interactions and opinions is the homophily bias due to their endogenous choices. In presence of homophily (namely the proclivity to befriend similar individuals),[2] this bias due to unobservable determinants of interactions

(LIEPP) and ANR-11-IDEX-0005-02 (USPC), and the Banque de France - Sciences Po partnership. The remaining errors are ours.

[1] Sciences Po's alumni include notably six of the seven French presidents after Charles de Gaulle, namely Emmanuel Macron, François Hollande, Nicolas Sarkozy, Jacques Chirac, François Mitterrand, and Georges Pompidou as well as the majority of Prime Ministers, and a large share of politicians and high-ranked civil servants (Rouban, 2011, 2014a,b). For examples, the time and experience at Sciences Po were transformational and pivotal to the political careers of both Jacques Chirac and François Mitterrand, according to their biographies.

[2] The concept of homophily was first highlighted by seminal studies in sociology since Lazarsfeld and Merton (1954), as surveyed by McPherson et al. (2001). Soon highlighted as a barrier to empirical identification by Manski (1993), it

and connections likely confounds any causal interpretation of such relationship.

We address this concern by exploiting the quasi-random assignment of students to Integration Groups (IGs) during the integration week just before their first year starts. During this week, students are assigned arbitrarily by alphabetical order to separate groups of around 16, to conduct social activities to facilitate students' socialization and integration into the new environment. In the sample of all possible student pairs, we specify the dyadic regressions of pairwise opinion differences on the same-IG indicator. We also use non-parametric randomized permutation tests of the effect of same-IG interactions.

We obtain data on IG participation, as well as other administrative data, for the entry cohort of 2013. In March 2014, we further survey their political opinions and association activities, and use Leider et al.'s (2009; 2010) incentive-compatible method to elicit their social networks of friendship.

The integration week is organized to serve as a booster of friendship formation. We estimate that the same-IG indicator increases the chance of a pair of students' lasting friendship by 17 percentage points, way larger than the effect of any observable similarity. As the IGs are dissolved before school starts, we further argue for same-IG membership's excludability that it could only affect surveyed political opinions through the friendship of pairs that remain friends. We thus use the same-IG indicator as instrument for pairwise friendship, in order to estimate its Local Average Treatment Effect (LATE) of friendship among complier pairs (those who become friends only due to being in the same IG).

Our method yields precise and powerful effects of social interactions and friendship. First, being in the same IG significantly reduces pairwise opinion difference, which is robust to various concerns about the arbitrariness of the alphabetical order of last names. Second, a friendship link between two students reduces their differences in political opinions by more than half a point (on a scale from 1 to 10) after 6 months.

We further find that the friendship effect is strongest among students with similar pre-Sciences Po political views. The evidence is consistent with what we call the "homophily-enforced mechanism," by which homophily along a dimension, such as political views, is complementary to the friendship effect on that dimension. Accordingly, between a pair of individuals with strong similarity on a dimension, friendship could make them interact much more on that dimension, consequently strengthen such similarity. In contrast, friendship might not matter much to that dimension between initially dissimilar pairs. Empirically, among politically-similar pairs, friendship strongly induces

has been further studied in economics by, e.g., Currarini et al. (2009) and Golub and Jackson (2012).

them to join the same politically-related associations, thereby likely pushes them to interact more on politics. Those pairs end up with a friendship effect on political opinions that is 50% larger than the benchmark effect. Yet, among pairs with far-apart pre-Sciences Po opinions, friendship does not push them towards the same political associations, and consequently does not produce a significant friendship effect on the subsequent political opinion gap. In short, similarity breeds friendship, which breeds similarity on the same dimension.[3]

We also discover a markedly asymmetric pattern of the effect of social interactions and friendship on polarization and extremism. Social interactions and friendship lower opinion gaps mostly by reducing the incidence of divergence (when two opinions drift apart), especially among politically similar students. In contrast, they do not encourage two opinions to converge towards each other. Consequently, social interactions and friendship lessen polarization and curtail extremist political views, while maintaining sufficient diversity of opinions.

By network characteristics, we find that the friendship effect is strongest among direct social distance, and extends to second-degree friends (friends of friends) but not further. Taking into account the effect on second-degree friends, the friendship effect on network can explain over 20% of the reduction in overall opinion gaps in the cohort. Those findings connect directly to the recent literature on non-Bayesian learning in social networks.[4]

Our methodology's use of an exogenous source of variation in network formation is distinctively novel in the recent empirical and econometric literature on social networks. Traditionally, the endogeneity of network formation received rather limited attention and treatment in studies that rely mostly on restrictions on the structure of interactions and uses of control variables, including fixed effects, with an identification underlined by Bramoullé et al.'s (2009) results, such as Bifulco et al. (2011); Calvó-Armengol et al. (2009); Patacchini and Zenou (2016); DeGiorgi et al. (2010). A different strand of the literature takes a structural approach that explicitly models the formation of network based on assumptions on individuals' interactions and expectations, and derives identification

[3]This mechanism can consolidate this paper's strong friendship effect with certain small, insignificant peer effects on academic performance occasionally found in the literature (e.g., Angrist and Lang, 2004). When friendship is built and consolidated voluntarily on a dimension, it matters to the gap in that dimension. In contrast, in a peer effect study, peer groups may or may not be formed to reinforce interactions on the same dimension as what is measured as outcome, so peer effects are not guaranteed.

[4]In the typical non-Bayesian model of learning in networks à la DeGroot (1974), effects of connected nodes are usually modeled as homogenous and linear. The literature on social learning, as reviewed by Goyal (2011), Möbius and Rosenblat (2014), and Golub and Sadler (2016), includes both Bayesian learning (e.g., Bala and Goyal, 1998, 2001; Acemoglu et al., 2011) and non-Bayesian learning (e.g., Campbell et al., 2019; DeGroot, 1974; DeMarzo et al., 2003; Golub and Jackson, 2010, 2012; Molavi et al., 2018). Recent designed experiments on the sources and mechanisms of information diffusion (e.g., Chandrasekhar et al., 2020; Möbius et al., 2015; Grimm and Mengel, 2020) have shown an important role of non-Bayesian learning.

conditions from the model, including recent developments such as Mele (2017); Badev (forthcoming); Goldsmith-Pinkham and Imbens (2013), as reviewed by De Paula (2017, 2020), Graham (2015), and Graham and De Paula (2020).[5] Different from those approaches, ours relies on a source of variation that draws its exogeneity and validity from design, not modeling assumptions, and then uses a relatively simple and transparent IV strategy to identify the LATE.

While our use of an exogenous group assignment echoes the vast literature on peer effects under randomized assignment (as surveyed by Sacerdote, 2011, 2014; Epple and Romano, 2011), our focus on friendship links, instead of peer-group relationships, is fundamentally different. We consider that friendship is chosen by individuals, not assigned by design, thus it is influenced by, and interacts with, individual characteristics and behaviors, as discovered in the case of Carrell et al. (2013). It is therefore important to understand the effect of friendship beyond that of peer group assignment.[6]

In the same vein, our results are also related to the contact hypothesis literature. Since Allport's (1954) seminal argument that intergroup contact is an effective way to eliminate prejudice between different ethnic groups under certain conditions (contact theory), it has generated a very large empirical literature with tests in various contexts (Pettigrew and Tropp, 2006; Paluck et al., 2019). Contact theory maintains that intergroup contact improves relationships between groups under certain circumstances, i.e., when both groups are of equal status or when they have common goals (Boisjoly et al., 2006; Cai and Szeidl, 2018; Merlino et al., 2019; Corno et al., 2019; Rao, 2019; List et al., 2020; Lowe, forthcoming), without the consideration of friendship and network connections.[7] Our findings are in line with the contact hypothesis in that students' exposure to each other strengthen their interactions and reduce their divergence. In addition, we extend it beyond simple within-group contact by showing the intricate role of friendship in shaping opinions.[8]

Overall, the paper highlights the importance of social interactions and friendship among the influential determinants of political opinions and behaviors, such as leaders and groups (e.g., DellaVigna

[5] For a review of the literature on empirical methods in social networks, also see Advani and Malde (2018); Blume et al. (2011); Bramoullé et al. (2016); Jackson (2011); Jackson et al. (2017); Ioannides (2013); Topa and Zenou (2015).

[6] There is also an econometric advantage in considering generic social networks, rather than the special case of peer groups. That is, the generic nature of networks (e.g., based on friendship links) introduces identifying restrictions that avoid Manski's (1993) reflection problem in linear-in-means models with peer groups, as mentioned in Jackson (2008), and formally proven in Bramoullé et al. (2009) (also see Lee and Liu, 2010; Lin, 2010; Liu et al., 2014).

[7] In this literature, peer-effect studies using instrumental variables, as surveyed in Epple and Romano (2011), are mostly concerned with endogenous group formation and measurement errors. Notably, Foster (2006) uses a related monadic specification, constructing a monadic instrument for same-dorm sophomore peers based on freshman dorm assignments, and finds no peer effect.

[8] A recent paper (Boucher et al., 2020) also examines the impact of contact on friendship formation. However, the focus is very different since they study the impact of mixing 5-years old Syrian refugees and Turkish natives on their friendship links.

and Gentzkow, 2010; Gabel and Scheve, 2007; Carlsson et al., 2015) and the media (e.g., DellaVigna and Kaplan, 2007; Gentzkow, 2006; Gentzkow et al., 2011; Kendall et al., 2015; Gerber et al., 2009). It also underlines that mixing policies, such as the assignment into IGs in this context, have strong potential to weave together the social fabric of a diverse community.

The rest of the paper unfolds as follows. Section 2 describes the study's context. Section 3 details our empirical strategy, the timing and design of our surveys, and discusses the collected data. Section 4 presents the main friendship effect on opinions and behaviors. Section 5 investigates the main drivers and mechanisms at work, section 6 shows how the friendship effect varies with network characteristics, and section 7 concludes.

2 Sciences Po background and organization

This section provides a description of the context of the natural experiment at Sciences Po, including its role in French politics and the organization of the integration week that we exploit as an exogenous source of variation in the formation of social networks. Sciences Po, or the Institute of Political Studies, has always had a major role in the training of French politicians and high level civil servants, as it was explicitly conceived to provide a modern training for the French elite since its foundation in 1872 following France's defeat in the Franco-Prussia War of 1871. From 12% to 15% of deputies of the French National Assembly elected in the last decades graduated from Sciences Po (Rouban, 2011, 2014a), as well as more than 15% of the mayors of cities above 30,000 inhabitants (Rouban, 2014b). Sciences Po alumni are also highly present in the government, as well as at the top of the French bureaucracy.

While not all Sciences Po students aspire a career in politics, it is much more important for them than for students from other universities or business schools. One tenth of the students are member of a political party, a very large proportion compared to their age group. Sciences Po students also have more exposure to politically-oriented events and participate more in political activities, organized by either student associations or the Institute. In comparison with students in French public universities, on average they are academically stronger, and come from a considerably wealthier background.

Friendship before Sciences Po. Sciences Po students generally do not known each other before their first year. About 75% of them come from high schools from all over France and have to pass two rounds of highly selective written and oral exams in the standard admission channel. About only

5% come from a variety of schools from abroad. Another 20% are admitted through an affirmative-action process called "Convention Education Prioritaire" (CEP). Those are the very best students from designated high schools in disadvantaged areas all over France, who face a different selection process by dossier and oral evaluation. Across all admission channels, the probability of students coming from the same school is very low, thus the incidence of friendship before Sciences Po is very rare. In our sample, we only find two pairs of pre-Sciences Po friends among the friendship pairs who were in the same integration group (0.2%, or 0.02% of all friendship pairs).

Integration Groups (IGs). In the integration week just before the scholar year, the incoming cohort of undergraduates are formally introduced to Sciences Po, and assigned to IGs of around 16 each based on alphabetical order. Our sample's integration week takes place in the last week of August 2013. Students experience a variety of extra-curricular activities, such as games and guided visits of Paris, separately in those groups, in purpose of creating and solidifying links among students. No activity during this week is related to academic or political matters, or students' political opinions. Individual conversations with students reveal that they remember the integration week primarily for the social bonding between new friends, and not for any other content.[9] After the week, IGs are dissolved, and never used in any other activities at Sciences Po.

After the integration week, students officially start their first year at Sciences Po, and immerse in a large number of academic and extra-curricular activities. They are no longer limited to their IGs, and are generally exposed to the whole cohort. In the first year, they all take the same core courses, although different language courses. For all extracurricular activities, they may choose to join multiple associations among about a hundred available, including notably those with close links to political parties and political movements, but also those serving other purposes such as sports and arts activities or those based on geographical and ethnic identities. Many meet regularly for practices, events, and social gatherings. Students' choices of association are our main focus in terms of students' behaviors during the first year.

Friendship continues to form during the scholar year, likely according to their exposure to each other and their similarity. At this stage, the different admission procedures mark important cleavages that may hinder friendship formation between students of different backgrounds and strengthen

[9] While the integration week's organization has faced critique as unrealistic in fostering friendship after just one week, our results in Table 5 lend credit to its designers as a surprisingly effective factor in friendship formation. Anecdotes recounted by students also corroborate such evidence.

homophily. Notably, compared with the rest, CEP students come from markedly poorer, more disadvantaged socio-economic families, and many struggle academically, at least in their first year (Tiberj, 2011). Our survey takes place in March 2014, during a vacation week in the middle of the second semester.

3 Empirical design, methodology, and measurement

3.1 Integration Group assignment and political opinions

We first focus on the main intervention of the quasi-random allocation of individuals into integration groups (IGs). We take two approaches to both test the validity of the near randomness claim, and estimate the intervention's causal effect on individuals' subsequent political opinions, namely (i) randomized permutation tests that require no parametric or functional assumption regarding the relationship between the group assignment and observable characteristics and outcomes,[10] and (ii) dyadic specifications linking the pairwise same-IG indicator with other pairwise characteristics and outcomes. Variable definitions will be detailed in subsection 3.4.

Permutation tests. This approach tests the null hypothesis of randomized group assignment against the alternative hypothesis of selection of similar individuals into groups by (i) computing a test statistic that measures within-group similarity in the original sample as well as in a large number of its randomized permutations, and (ii) comparing the original sample's statistic with its distribution from the permutations to obtain the test's p-value. In our context, we choose the test statistic as the ratio between the within-group and the between-group standard deviations of a predetermined variable, such as gender or initial opinion. Selection into groups by this variable implies a small value of the test statistic (zero in case of perfect selection into group), hence the test's p-value is calculated as the left-tail quantile of the test statistic's distribution from 300 randomly drawn permutations. This test statistic also has the advantage of being invariant to any affine transformation of the variable, such as a change of unit or a multiplication of the scale.[11]

Once we have established IG assignment's randomness, we will use the same approach to test the null hypothesis that there is no effect of group assignment on political opinion against the alternative

[10] See, e.g., Kennedy's (1995) summary of permutation tests' virtues, first studied since Fisher (1935) and Pitman (1937a,b, 1938).

[11] The statistic is directly computed for continuous and binary variables. For category variables, e.g., each student's high school major, we first break it down to binary variables (indicators) representing each category (e.g., an indicator whether a student's high school major is "scientific" or not), then compute the within-group/between-group ratio statistic, and average it over all categories.

hypothesis that group assignment makes same-IG members' political opinions more similar. The implementation simply replaces predetermined variables in the previous tests by individuals' present political opinion, so the test statistic is now the ratio between the within-group and the between-group standard deviations of political opinion.

Dyadic specifications. The second approach considers the sample of unordered pairs of students (i, j) and dyadic variables over those observations, including the same-IG indicator IG_{ij}, the absolute difference in political opinions DY_{ij}, and pairwise control variables X_{ij}.[12] First, in order to test the exogeneity of IG_{ij}, we regress it on the dyadic covariates X_{ij}'s, either each one separately or altogether. They include initial (pre-Sciences Po) difference in political opinions, common gender, common nationality, common academic program (e.g., dual-degree programs joint with another institution), common admission type (essentially regular admission versus priority admission through the affirmative action channel), common département (the French administrative district) and common region of high school, common high school major, common profession of parents, common permanent address' ZIP code, dummies for being both female, for both having to pay no tuition (i.e., both family incomes are sufficiently low), and the difference in tuition fees that proxies for the difference in household income.[13] Apart from the surveyed political opinions, the other predetermined variables come from administrative data. Exogeneity is rejected when the estimated coefficient is statistically significantly different from zero.[14]

Once IG assignment's randomness is established, we will use this dyadic approach to produce the causal effect of being in the same IG on pairwise differences in political opinions, DY_{ij}, and other dyadic outcomes, e.g., joining the same student association. Given the dyadic relationship $DY_{ij} = f(IG_{ij}, \mathbf{X_{ij}}, \eta_{ij})$, the regression of DY_{ij} on IG_{ij} estimates IG's Average Treatment Effect $\beta_{IG} = \mathbb{E}[DY_{ij}|IG_{ij} = 1, \mathbf{X_{ij}}] - \mathbb{E}[DY_{ij}|IG_{ij} = 0, \mathbf{X_{ij}}]$.

Statistical inference in dyadic specifications. As each individual is repeated in her pairs with all other students, the error terms across pairs sharing an individual can be naturally correlated. In

[12] We do not consider (i, j) and (j, i) as two different pairs (i.e., no directed friendship), as the main intervention variable IG_{ij} is symmetric by nature.

[13] At Sciences Po, a student's tuition fee is determined solely by parents' tax bracket from their tax declaration(s). In our sample, its distribution peaks at 0 and 10,200 euros (the maximum), so the difference between two students' tuition fees cannot fully capture differences between those coming from very poor or very rich families. Unfortunately, there is no better information on the precise household income, as the administrative data are mostly missing when it comes to declared parents' incomes from very rich families.

[14] In a multiple-regression test, exogeneity is rejected if there are more such statistically significant coefficients than in the randomized case.

addition, there can naturally occur common shocks within the same group, such as teacher's biases, that could drive all group members' opinions. Those shocks also create clustered standard errors, and must be taken care of in order to obtain correct standard errors and confidence intervals.[15]

To deal with this issue, we choose to correct for potential clustered standard errors by a two-way group clustering strategy. That is, we allow for arbitrary correlations in the idiosyncratic component η_{ij} between any pair of observations that overlap in an IG.[16] We make sure results are robust to different types of clustering correction.[17]

Both approaches are useful. Permutation tests are particularly helpful to examine the exogeneity of group assignment, because they are nonparametric by nature, and needs no distributional assumption. This advantage addresses parametric specifications' problem of incorrect inference due to misspecification. In our context, parametric specifications usually over-simplify the covariance structure across individuals and pairs, and it is not clear whether standard error clustering can best address the issue (see the discussion on inferences in dyadic regression below). This complexity arises because the data generating process in group assignment is equivalent to each individual sequentially drawing a group variable without replacement, so an individual's drawn group can be dependent on earlier drawn groups. On the other hand, permutation tests do not allow for control variables, hence are heavily reliant on the assumption of the random data generating process of IG assignment that will need to be tested. Furthermore, the IV design to estimate the effect of friendship on political opinion can only be adapted to the dyadic specification. The two methods are thus complementary, and both help strengthen the results' persuasiveness.

[15]Those shocks are uncorrelated to the intervention variable IG_{ij}, so they cannot bias our OLS or IV estimates.

[16]Cameron and Miller (2014) discusses Fafchamps and Gubert's (2007) method to fully account for all possible correlations between all dyads that overlap with a group or share an individual. Unfortunately in this case, Cameron et al. (2011) decomposition of the the sandwich formula for standard errors (used for a fast, economical calculation of the two-way clustering correction) becomes intractable. The only possible implementation is to undertake the full calculation of Fafchamps and Gubert's (2007) formula, which requires an excessive amount of computing memory and time, given our large sample size. Therefore, throughout our paper, we choose to implement a simplified version of this method, in which we allow for non-zero correlations between any residual terms η_{ij} and $\eta_{i'j'}$ such that either i and i' belong to the same group, or j and j' belong to the same group, or both (thus ignoring the possible same-group memberships of i and j', or of i' and j. We also have fully implemented Fafchamps and Gubert's (2007) formula in a few benchmark regressions, and found similar and stronger levels of standard errors and p-value.

[17]In the balance tests using dyadic specifications, as we aim to detect significant correlations between any covariate and the same-IG indicator, we impose a stronger restriction on the clustering structure of the error terms: We would cluster by the interaction of i's group and j's group. Compared to the benchmark clustering strategy, this restriction tends to produce more precise coefficient estimates (smaller standard errors), hence increases the test's power to detect selection into IGs by any covariate.

3.2 Friendship, Integration Group, and political opinions

We now shift our focus to the role of friendship in shaping political opinions. We assume the following dyadic relationship between the absolute difference in political opinions DY_{ij} and friendship $Link_{ij}$, a vector of dyadic covariates $\mathbf{X_{ij}}$, and the unobservable idiosyncratic residual η_{ij} (see variable definitions in subsection 3.4):

$$DY_{ij} = g(Link_{ij}, \mathbf{X_{ij}}, \eta_{ij}).$$

We are interested in the average causal effect of friendship on opinion difference, defined as $\beta_L = \mathbb{E}[DY_{ij}|Link_{ij} = 1, \mathbf{X_{ij}}] - \mathbb{E}[DY_{ij}|Link_{ij} = 0, \mathbf{X_{ij}}]$. A negative (positive) β_L means that friendship makes people's opinions closer (further apart) over the observed period of 6 months from August 2013 to March 2014, and vice versa. Under the assumption that conditional on covariates, friendship is assigned exogenously, a simple OLS regression produces an unbiased estimate of β_L. However, this conditional independence assumption is likely violated in presence of homophily in friendship formation.

The homophily bias occurs when there is a certain unobserved dimension U such that (i) individuals' similarity U_{ij} correlates with the formation of friendship links $Link_{ij}$ (homophily), and (ii) it also influences the outcome $DY_{ij} = g(Link_{ij}, \mathbf{X_{ij}}, U_{ij}, \eta_{ij})$ (outcome-relevance). When friendship formation is empirically related to U_{ij} as $U_{ij} = f(Link_{ij}, \mathbf{X_{ij}}, \varepsilon_{ij})$, then the homophily bias due to the omission of U is $\frac{\partial g}{\partial U} \times \frac{\partial f}{\partial Link}$.[18] It is larger when U is more important to the outcome, and when it is more associated with link formation. In our context, the bias likely pushes the OLS estimate away from zero. This remains a thorny issue in existing estimations of effects of network links, and one that, to our best knowledge, has not been addressed in the empirical literature using an exogenous source of variation.

To address the homophily bias, we propose a novel strategy to instrument $Link_{ij}$ by the same-IG indicator IG_{ij}. We argue below that this strategy estimates the Local Average Treatment Effect (LATE) of friendship on difference in opinions among compliers, as it satisfies all LATE conditions (Imbens and Angrist, 1994).

Instrument validity. First, in subsection 4.2, we will test the instrument's relevance, namely $\beta_{IG} = \mathbb{E}[Link_{ij}|IG_{ij} = 1, \mathbf{X_{ij}}] - \mathbb{E}[Link_{ij}|IGij = 0, \mathbf{X_{ij}}] \neq 0$. This first stage condition means that

[18] More precisely, the partial derivatives denote corresponding regression coefficients, controlling for covariates $\mathbf{X_{ij}}$. The direction of causality in those regressions does not matter to the homophily bias.

the integration week is a strong enough catalyst to form lasting friendships among students.

Second, this instrument's exogeneity is based on the mechanism of assignment into IGs by *alphabetical order* of students' family name, arguably independent from individual characteristics that matter to the formation of links. We will further test the claim of exogeneity in a balance test in subsection 3.6.

Third, we argue that the instrument IG_{ij} satisfies the exclusion restriction. The integration week was exclusively meant to facilitate students' familiarization and socialization with their new peers and new environment in Paris, without any academic- or political-related activities. The IGs are dissolved after that week, and does not relate to any other academic or extra-curricular activities afterwards.[19] Hence, it should have no meaningful channel to affect the formation and adjustment of individual opinions six months later, which guarantees the exclusion restriction of the instrument.[20]

Fourth, it is natural to make the monotonicity assumption that being in the same IG always weakly increases the incidence of friendship formation for any pair of potential friends, such that $f(IG_{ij} = 1, \mathbf{X_{ij}}, \varepsilon_{ij}) \geq f(IG_{ij} = 0, \mathbf{X_{ij}}, \varepsilon_{ij}) \ \forall(i, j)$.[21]

Taken together, those four assumptions guarantee a *causal* interpretation (Imbens and Angrist, 1994) of the result as the average causal effect of friendship on the "compliers" pairs of students, namely the pairs who would have become friends thanks to being in the same IG in the integration week. Since this is a condition that characterizes a rather strongly-complying group of student pairs (for instance, pairs that only become friends after weeks or months of acquaintance are not included), we remain cautious in generalizing our estimates to all possible pairs of Sciences Po students. However, in Imbens's (2010) spirit of "better LATE than nothing", we argue that the correct estimation of the LATE in our context already lays strong ground for further research on transmission of beliefs among students.

[19] No subsequent large-scale academic or extra-curricular activities among Sciences Po students are organized based on alphabetical order.

[20] Kitagawa (2015) shows that the exclusion restriction can be jointly tested with the other LATE assumptions. In that spirit, we examine the inferred distributions of outcomes in the two subsamples of treated and untreated compliers, by drawing respectively the distribution of unassigned never-takers with and without unassigned compliers in Appendix Figure A1, and the distribution of assigned always-takers with and without assigned compliers in Appendix Figure A2 (in each case, the difference between the two plotted distributions is the distribution of corresponding compliers). We verify that the density of compliers in each case is nonnegative, thus the LATE assumptions cannot be rejected.

[21] Its violation would mean the rather improbable event of a "defier" pair that would have become friends had they not met in the same IG, but would not have become friends because they met early in the same IG. Even without the monotonicity assumption, de Chaisemartin (2017) shows that, under a much weaker condition, one could still interpret the IV estimator as the Average Treatment Effect among a subgroup of compliers.

Analysis of compliers. Since the LATE is defined over the compliers, it is useful to characterize those pairs in order to better understand the potential difference between the IV and OLS estimates. Appendix B describes the calculation of the sample share of each group, as well as any distributional statistics within each group. We can then compare the difference between treated and untreated compliers, from which the LATE estimate obtains, and the OLS estimate that likely draws more from never-takers and always-takers.

3.3 Robustness tests with alternative strategies.

Two types of identification concerns merit additional tests. First, compliance to the alphabetical assignment in IGs is imperfect, as students could refuse to follow their assigned group. We address this issue in a test using an instrument based on the alphabetical distance between names that approximates the designed IG structure. We first rank all last names in alphabetical order, assign the rank $AlphRank_i$ to each student i, and then compute the alphabetical rank distance $AlphDist_{ij} = |AlphRank_i - AlphaRank_j|$. We then use $\min(AlphDist_{ij}, 16)$ as instrument for friendship L_{ij}. This instrument's logic is that by initial assignment, and independent from students' choice to comply with this assignment, two names with a shorter alphabetical distance between them are more likely to fall into the same IG, and then are more likely to become friends. This first stage effect should mostly fade beyond 16, the standard size of the IG – thus the truncation of $AlphDist_{ij}$ at 16.

Second, there is a potential concern that the alphabetical rank of certain family names may be correlated with confounding characteristics such as ethnic origin.[22] To address this concern, we first run "jackknife" tests, in each of which we drop all names starting with a specific letter, or all students with a specific non-French nationality. We also check the sample that drops French family names starting with "de" and similar prefixes that can correlate with an aristocratic background.

Next, we can further strengthen our IV approach by restricting the sample to only pairs of students whose alphabetical distance is sufficiently close. Intuitively, we consider same-group and different-group pairs of students within a bandwidth of the cutoff between two consecutive groups. Analogous to the logic of a Regression Discontinuity Design, around the threshold between two groups, same-group and different-group pairs are almost identical in both observable and unobservable characteristics (Lee and Lemieux, 2010), which reinforces the identification assumption of exogeneity of IG assignment.[23]

[22]For example, certain last names may constitute a large share of an ethnicity, such as Zhang among Chinese, Nguyen among Vietnamese, and Kim among Koreans. Such names may over-populate certain IGs with same-ethnic students. In reality, we do not find such ethnicities or clustering by any ethnicity in our sample.

[23]While similar, this is not a proper Regression Discontinuity Design, since the exact cutoff is unknown due to

3.4 Measurement, survey design, and data sources

The main variables in our analysis include the common IG indicator IG_{ij}, undirected friendship link $Link_{ij}$, political opinion Y_i and opinion gap DY_{ij}, and a vector of dyadic covariates $\mathbf{X}_{ij}$ (see list in subsection 3.1). First, data on the IG organization and student characteristics used to construct covariates are obtained from official administrative data.

Second, we measure the undirected friendship link $Link_{ij}$ as the indicator whether either of the two students names the other as a friend, based on our major internet-based survey in March 2014 on the cohort of Sciences Po first-year students who start in September 2013.[24]

We offer strong material incentives in the first survey in the form of a lottery for 50 mini iPads at approximately 300 Euros each (each student has an average probability of about 9% to win one). We seek a high rate of participation to avoid the problem of complex biases in network measures due to missing information on network structure (Chandrasekhar and Lewis, 2011). Eventually, 68.4% (547 out of 800) of the students answer to at least some question in the survey, and 65.6% (526 out of 800) complete the whole survey. This participation rate is similar to some of the most participated studies of social networks of students, such as Leider et al. (2009) or Goeree et al. (2010). It is well above the standard participation rate of around 20% found in studies using online surveys (Cantoni et al., 2017).[25]

In order to incentivize truthful answers, we design the elicitation of friendships as a coordination game, similarly to Leider et al. (2009). We ask students to name a list of up to 10 friends in the same cohort, and also details on how they meet each of them, how much time they spend together, and in which activities, as well as how strong they evaluate their relationships. We announce in the survey that their answers would be cross-validated with those of their named friends, and that if both answers match, each would gain points, later converted into an additional probability of winning the iPad. When a respondent starts typing some characters of a friend's name, the survey displays a dropdown list of names in the same cohort that match those characters, so as to facilitate the input of long, unfamiliar, and hard-spelling names. Those details are designed to (i) encourage respondents to list all their friends, including those whose names can be long, uncommon, unfamiliar,

partial compliance. It is thus not possible to implement standard RDD methods, or choose an optimal bandwidth. We pick the bandwidth $AlphDist_{ij} \leq 30$, noting that the results remain similar for a broad range of bandwidths..

[24] We thus focus on the OR network of undirected friendship, similar to Leider et al. (2009) and other papers that consider surveyed friendship. The results remain robust to using the AND network, which counts a link between i and j if both list the other as friend.

[25] A second survey conducted in June 2015 on the same cohort is unfortunately much less well-funded, and only attracts 300 participants. Overall, there are 235 students who have completed in both surveys. The paper makes most use of the first survey, while the second only serves in robustness checks.

and hard to spell, such as students from immigrant origin,[26] (ii) encourage them to list their strongest friendships first, as those friends are most likely to list them back,and (iii) discourage respondents from overlooking the friendship questions just because they are more time-consuming.

In this design, the cross-validated incentive may raise the concern of collusion among respondents to maximize their gains. From our interaction with students, we believe this issue is relatively limited. First, student pairs who try to coordinate are likely already friends in some way, in which case their coordination cements the validity of the friendship measure. Second, the survey is carried out during a vacation week, which limits the possibility that two students interact in person and complete the survey together. Third, since students only know the content of the questions once they open the survey website, and those who coordinate must spend much time to call each other and agree on a strategy, we further avoid potential colluders by censoring the top 5% of the sample by the amount of time spent on friendship questions.[27]

Subsection 3.5 will provide additional statistics showing the survey's high quality. We are confident that the remaining measurement errors are likely unrelated to the intervention variable, hence irrelevant to the main empirical results.

Third, the second part of the survey is devoted to questions about political opinion and values. We ask students' current political opinion in March 2014, and that before their arrival at Sciences Po in August 2013. These questions use a common scale from 1 to 10, 1 being extreme left and 10 extreme right. We define the current political opinion gap between two students i and j as $DY_{ij} = |Y_i - Y_j|$, and similarly DY^0_{ij} as the opinion gap from before Sciences Po. Given Sciences Po's emphasis on politics, students are perfectly familiar with those concepts, and there is little ambiguity in what the extremes and the moderates mean. The survey also provides information on their political participation, and any participation in associations at Sciences Po.

Beyond the political opinion gap, we also explore how pairs of students' opinions evolve at Sciences Po. Denote $\Delta Y_i = Y_i - Y^0_i$ as i's signed change in opinion from August 2013 (before Sciences Po) to March 2014 (survey time). We define that the pair (i, j) experience a strong convergence iff $\Delta Y_i(Y^0_j - Y^0_i) > 0$ and $\Delta Y_j(Y^0_i - Y^0_j) > 0$ (i.e., each opinion moves towards the other), and a weak

[26]There may be a concern of respondents listing even non-friends in order to use up the 10 slots. First, it is highly unlikely that non-friends reciprocate, so there is no incentive to list them. Second, we further use the stated intensity of friendship to address this concern in subsection 4.5.

[27]This means dropping individuals who spend more than 81.625 seconds per friend on that question. The results remain practically the same over a broad range of possibilities of right tail censorship. Right tail censorship looks necessary, given that at the top of the distribution certain students spend up to half an hour per friend. Our results are also robust to left tail censorship, although the case for censorship is much less clear, as the fastest answers still took an acceptable amount of time (more than 10 seconds on average).

convergence iff $\Delta Y_i(Y_j^0 - Y_i^0) \geq 0$ and $\Delta Y_j(Y_i^0 - Y_j^0) \geq 0$ (each opinion moves towards the other or stay the same) . Similarly, strong divergence means $\Delta Y_i(Y_j^0 - Y_i^0) < 0$ and $\Delta Y_j(Y_i^0 - Y_j^0) < 0$ (each opinion moves away from the other), and weak divergence $\Delta Y_i(Y_j^0 - Y_i^0) \leq 0$ and $\Delta Y_j(Y_i^0 - Y_j^0) \leq 0$ (each opinion moves away from the other or stay the same). Co-movement in the same direction is defined as $\Delta Y_i \Delta Y_j \geq 0$.

3.5 Data description

We consider the (symmetric) OR network in which two students are linked if at least one nominates the other. Table 1 Panel A describes the quality of the network survey. About half of the nominated friends reciprocate, a considerably larger rate than in the literature since Leider et al. (2009). The probabilities of a well-matched answer in terms of the context of the first meeting between the two friends, of the amount of time spent every week, of the type of activities mostly spent together, and of the self-evaluated strength of friendship are respectively 76%, 52%, 46%, and 52%, quite larger than in Leider et al. (2009). If answers are completely made up and randomized, the probability of matching on any of those dimensions would be rather low, given that respondents have many choices for each answer (especially in the question on the context of their first meeting). Taken together, those statistics imply that the survey answers are indeed very reliable, especially for the purpose of picking up friendships.

Panel B reports the major statistics on the number of friends and the social network structure. The average and maximum number of nominated friends per student is 8.8 and 21, respectively, with a very high variance.[28] Moreover, there seems to be some small world properties with a very small average path length (3.7) and a relatively small diameter (9). The clustering coefficient is also relatively high, which means that roughly 25 percent of students have friends of friends who are friends. In terms of network position, the mean eigenvector centrality is relatively low (0.0361).

Panel C shows the descriptive statistics of the friendship dyadic measures. We distinguish between the full sample (column 1) of all students who have participated and the benchmark sample (column 2) that corresponds to the benchmark regression (the difference is due to certain missing values). By nature, the share of measured friendship links is relatively small at 1.6%, and that of second and third order indirect links are larger at 9.3% and 38%, respectively. The dyadic same group variables are of similar magnitudes, at an average of 1.6% for same IG, and 2.3% for same tutorial groups. The friendships are partitioned rather evenly across different levels of friendship

[28] Even if the maximum number of friends that someone can nominate is 10, a student can have 21 friends since we use an undirected network approach so that a friend is assigned to a person if either her or her friend has nominated the other.

Table 1: Descriptive Statistics

Panel A: Quality of the Survey

	(1) Full Sample	(2) Benchmark Sample
Number of reported friends	8.234	8.613
	(2.522)	(1.984)
Probability of reciprocal friend	0.461	0.479
	(0.499)	(0.500)
Correct answer: meeting	0.800	0.815
	(0.400)	(0.389)
Correct answer : time spent	0.483	0.497
	(0.500)	(0.501)
Correct answer : activity	0.568	0.587
	(0.496)	(0.493)
Correct answer : strength of the relationship	0.532	0.532
	(0.499)	(0.500)

Panel B: "OR" Network Statistics

Mean of degree per individual	8.8625
Variance of degree per individual	18.4842
Median of degree per individual	10
Maximum of degree per individual	21
Minimum of degree per individual	0
Diameter of the network	9
Average path length	3.7008
Overall clustering coefficient	0.241
Average clustering coefficient	0.271
Mean eigenvector centrality	0.0361
Standard deviation of eigenvector centrality	0.0200

Notes: Summary statistics are computed on the full sample.

Panel C: Dyadic Links and Groups

Variable	(1) Full Sample Mean	Standard deviation	Obs.	(2) Benchmark Sample Mean	Standard deviation	Obs.
Friendship	0.0160	(0.1240)	147,153	0.0178	(0.1324)	52,326
2nd Order Links	0.0930	(0.2900)	147,153	0.1014	(0.3019)	52,326
3rd Order Links	0.3800	(0.4850)	147,153	0.4081	(0.4914)	52,326
Mere relationship (strength 1)	0.0014	(0.0382)	147,153	0.0018	(0.0428)	52,326
Friendship link (strength 2)	0.0063	(0.0791)	147,153	0.0070	(0.0832)	52,326
Close friendship (strength 3)	0.0041	(0.0642)	147,153	0.0047	(0.0681)	52,326
Very close friendship (strength 4)	0.0035	(0.0593)	147,153	0.0041	(0.0641)	52,326
Same Integration Group	0.0160	(0.1280)	147,153	0.0188	(0.1359)	52,326

Panel D: Monadic Dependent Variables

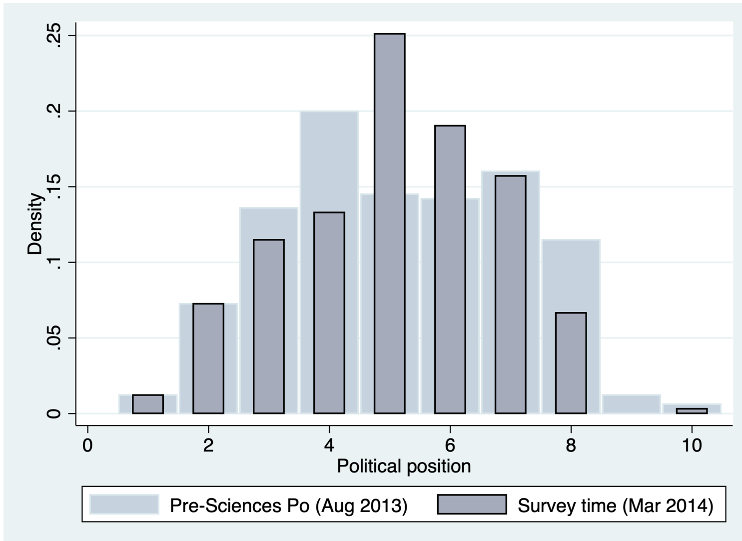
Variable	(1) Full Sample Mean	Standard deviation	Obs.	(2) Benchmark Sample Mean	Standard deviation	Obs.
Political Opinion in March 2014 (1-10)	5.044	(1.755)	472	5.091	(1.712)	331
Initial (Pre-Sciences Po) Political Opinion (August 2013) (1-10)	5.108	(1.958)	463	5.148	(1.934)	331
Political Opinion in 2014 as recalled in 2015	4.913	(1.650)	287	4.941	(1.642)	331
Political Opinion in 2015	4.853	(1.807)	285	4.818	(1.746)	331
Membership in an Association in 2014	0.597	(0.491)	499	0.642	(0.480)	330

Panel E: Dyadic Dependent Variables

Variable	(1) Full Sample Mean	Standard deviation	Obs.	(2) Benchmark Sample Mean	Standard deviation	Obs.
Difference in Political Opinions in March 2014	1.932	(1.467)	105,111	1.927	(1.469)	52,326
Initial (Pre-Sciences Po) difference in Political Opinions (August 2013)	2.211	(1.631)	101,025	2.194	(1.621)	52,326
Difference in Political Opinions in 2015	2.014	(1.538)	27,027	1.940	(1.496)	15,920
Difference in Political Opinions in 2014 as recalled in 2015	1.835	(1.424)	126,756	1.798	(1.412)	15,920
Participants in the Same Sports Activities	0.600	(0.490)	52,003	0.586	(0.493)	23,436
Membership in the Same non-Sports Association	0.085	(0.279)	52,003	0.097	(0.296)	23,436
Membership in the Same Political Association	0.018	(0.131)	52,003	0.023	(0.149)	23,436
Membership in the Same Activism Association	0.007	(0.084)	52,003	0.008	(0.088)	23,436
Membership in the Same Identity-related Association	0.005	(0.072)	52,003	0.004	(0.061)	23,436

Notes: Statistics in (1) are computed on the full sample of data available for each variable, while statistics in (2) are computed on the benchmark sample, which is detailed in Table A1.

Figure 1: Distributions of Political Opinions



Notes: Distributions of Individual Political Opinions just before joining Sciences Po (August 2013) and at the time of survey (March 2014).

strength, especially from 2 (ordinary friends) to 4 (very close friends). We also observe that there is little difference between the full sample and the benchmark sample.

Panel D lists the descriptive statistics of students' political opinion and behavior. While political opinion slightly shifts to center-left over time (i.e., to lower value, as 5.5 represents the center), participation in political parties has increased substantially. Meanwhile, the variance of political opinion decreases by 24 percent, as the measured standard deviation of opinions in March 2014 is only 1.76 on a scale of 1 to 10.[29]

Figure 1 shows the distributions of political opinions in March 2014 and in August 2013. The bimodal distribution in 2013, with two modes at 4 and 7 corresponding to rather mainstream left-right politics, becomes unimodal in 2014 with strongly dominant center in 5-6. That fact, and a strong reduction in right to extreme right positions (8-9-10), altogether explains the net decrease in variance of opinion.

3.6 Exogeneity tests of assignment into Integration Groups

This subsection evaluates the claim that the assignment into Integration Groups (IGs) by alphabetical order of the students' family name is exogenous, by checking that alphabetically close

[29] Appendix Table A1 describes in detail all variable definitions, and Appendix Table A2 completes the descriptive statistics of other variables used in the empirical analysis.

family names do not carry other information that could stack up students with similar backgrounds in the same group. We first start with a permutation test, as described in subsection 3.1. Table 2 shows the p-values of the left-sided permutation tests corresponding to all predetermined covariates, calculated based on the empirical distribution of the test statistics (the within-/between-standard deviation ratio) drawn from 300 random permutations of the original sample. None of the tests can reject the null hypothesis of randomized assignment into IGs at 5%.

Second, in Table 3 we report dyadic linear regressions of IG_{ij} on observable pairwise covariates, either altogether (Panel A) or one by one (Panel B), as explained in subsection 3.1. All coefficients are not statistically different from zero at 95% confidence, except one. Even this statistically significant coefficient shows the opposite of homophily, namely that individuals with similar high school major are more likely in different, not the same IGs. It is quite natural that among 14 estimates, one coincidentally has a p-value below 0.05. Furthermore, given their very small magnitude, their corresponding 95% confidence intervals remain small,[30] suggesting that their inclusion in the main analysis does not matter much to the coefficient of IG_{ij}, a fact that we can later verify. To remain cautious, we do control for all of those covariates throughout the empirical exercises.

4 Empirical results and interpretations

4.1 Integration Group assignment and political opinions

Based on the claim of random assignment of students into IGs, we move on to establish its causal effects on political opinions. We first implement permutation tests (as described in subsection 3.1 and also performed in Table 2) confronting the alternative hypothesis of lowered within-group

[30]The largest 95% confidence interval, corresponding to the variable whether the pair are both admitted via Sciences Po's affirmative action program, is still contained within [-0.02,0.04].

Table 2: Permutation Tests of Integration Group Assignment's Randomness

Variable	Within-Group Statistics	Actual value	p-value
Initial Political Opinion	Within-/Between- Standard Deviation Ratio	2.282	0.780
Tuition Fees	Within-/Between- Standard Deviation Ratio	1.842	0.183
Gender	Within-/Between- Standard Deviation Ratio	1.954	0.300
Affirmative-Action Admission	Within-/Between- Standard Deviation Ratio	1.753	0.157
Second Nationality	Within-/Between- Standard Deviation Ratio per Category	1.261	0.880
Admission Type	Within-/Between- Standard Deviation Ratio per Category	2.496	0.400
Program	Within-/Between- Standard Deviation Ratio per Category	2.244	0.493
Parents' Profession	Within-/Between- Standard Deviation Ratio per Category	2.356	0.300
High School Major	Within-/Between- Standard Deviation Ratio per Category	2.310	0.317
Département of High School	Within-/Between- Standard Deviation Ratio per Category	2.744	0.980
Region of High School	Within-/Between- Standard Deviation Ratio per Category	2.614	0.950

Notes: Permutation tests of IG assignment's exogeneity by 300 Monte Carlo permutations of the full sample. For continuous and binary variables, the test is performed on the distribution of the ratio of within-group and between-group standard deviations. For category variables, the test is performed on the distribution of the average of this ratio across all indicators representing each category. p-values are computed with respect to the left tail (rejection of low within-group variation with respect to between-group variation). See Appendix A and Appendix Table A1 for description of variables and sample.

Table 3: Balance Test of Same Integration Group Indicator

Panel A: Balance Test by Pooled Dyadic Regression

Dependent Variable	Same IG	Dependent Variable	Same IG
Initial Political Opinion Gap	0.000938	Same Region of High School	0.00212
(August 2013)	(0.000683)		(0.00325)
Same Gender	0.000605	Same High School Major	-0.00238**
	(0.00208)		(0.00109)
Both Female	-0.00185	Diff. in Tuition Fees	-3.78e-07
	(0.00390)		(4.56e-07)
Same Nationality	0.00741	Both Free Tuition	-0.00213
	(0.00601)		(0.00234)
Same Admission Type	-0.000123	Same Parents Profession	0.00111
	(0.00307)		(0.00212)
Both Affirmative Action	0.00699	Same ZIP Code	-0.000112
	(0.0149)		(0.00408)
Same Département of High School	0.00269	Same Program	0.00259
	(0.00697)		(0.00261)
Observations		52,326	
R-squared		0.001	
F-stat		0.71	

Notes: Balance test by OLS regression of *Same IG* on all covariates altogether. F-stats are for the joint significance of the covariates. Standard errors in brackets are clustered by individual 1's group $\times$ individual 2's group. See Appendix A and Appendix Table A1 for description of variables and sample.

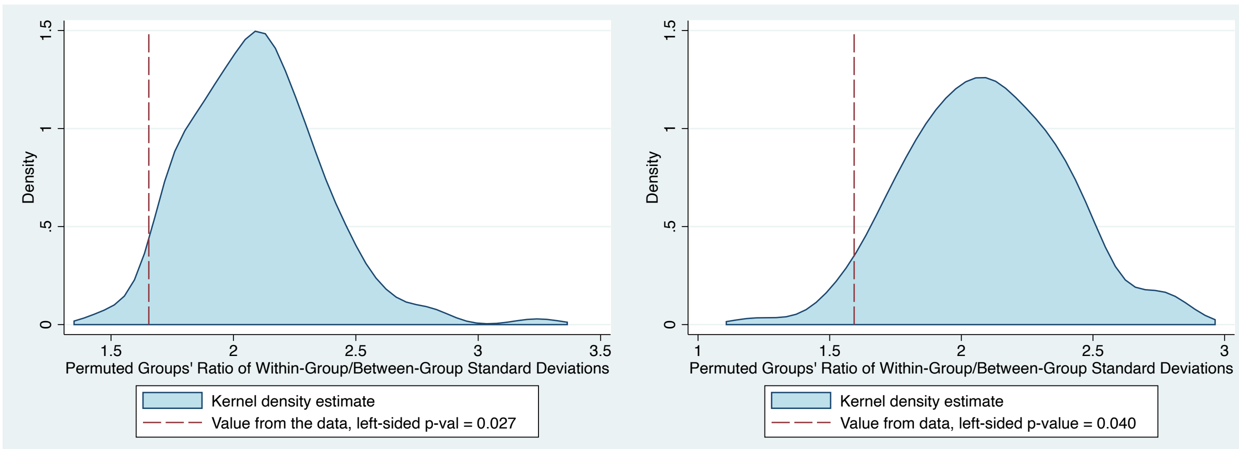
Panel B: Balance Test by Separate Regressions

Dependent Variable	Same IG	Dependent Variable	Same IG
Initial Political Opinion Gap	0.000915	Same Region of High School	0.00305
(August 2013)	(0.000681)		(0.00361)
Same Gender	0.000665	Same High School Major	-0.00221**
	(0.00113)		(0.00112)
Both Female	-0.00145	Diff. in Tuition Fees	-2.43e-07
	(0.00262)		(3.91e-07)
Same Nationality	0.00843	Both Free Tuition	-0.00110
	(0.00592)		(0.00208)
Same Admission Type	-0.000300	Same Parents Profession	0.00138
	(0.00300)		(0.00217)
Both Affirmative Action	0.00983	Same ZIP Code	-0.000645
	(0.0150)		(0.00407)
Same Département of High School	0.00452	Same Program	0.00259
	(0.00722)		(0.00261)
Observations		52,326	

Notes: Balance test by OLS regression of *Same IG* on each covariate separately. Standard errors in brackets are clustered by individual 1's group $\times$ individual 2's group. See Appendix A and Appendix Table A1 for description of variables and sample.

variation due to IG assignment against the null hypothesis of no IG assignment effect. We apply this procedure to (i) individual political opinions surveyed in March 2014 and (ii) changes in political opinions from before Sciences Po until March 2014, and plot the distributions of simulated test statistics in subfigures 2A and 2B, respectively. In both cases, we can reject at 5% the null hypothesis that there is no effect from group assignment on individual opinions.

Figure 2: Permutation Tests of Integration Group Effects on Political Opinions



(A) On Present Political Opinions

(B) On Changes in Political Opinions

Notes: Permutation tests of the effects of IG assignment. In each case, we permute individuals' group assignment across the sample 300 times, and compute the distribution of the test statistic of the ratio between within-group and between-group standard deviations of the outcome. We then calculate the p-value of a left-sided test as the quantile of the observed sample's test statistic with respect to this distribution over permutations. The outcome in subfigure 2A is individual political opinion surveyed in March 2014, and in subfigure 2B it is the change of political opinions from before Sciences Po (August 2013) to March 2014.

To better quantify those nonparametric results, in Table 4 we proceed with the parametric dyadic specification (subsection 3.1) that regresses the pairwise political opinion gap on the same-IG indicator, either without (column (1)) or with (column (2)) dyadic covariates. The estimates are negative, statistically significantly different from zero, and amount to about 5% of the average (1.93) and 6% of the standard deviation (1.47) of political opinion gap.

Further robustness checks (detailed in subsection 3.3) confirm that those results are unlikely driven by confounding factors. Column (3) reports stronger results when the alphabetical distance between names is used as instrument for the same-IG indicator to address potentially selective non-compliance to the IG allocation. To address potential confounding factors that may correlate with last names, columns (4) to (6) consider more restrictive samples, either of pairs with small alphabetical distance (in the spirit of an RDD of last names), or pairs of the same first letter of their last names, or the intersection of the two samples. Even in those subsamples of much smaller sizes, the estimates remain relatively stable and still statistically significant at 5%.

Table 4: Same Integration Group Membership and Political Opinion Gaps

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Difference in Political Opinion (March 2014)					
Sample of Pairs:	Full Sample			Alphabetical Distance ≤ 30	Same First Letter	Both Conditions
Same IG	-0.0864***	-0.0937***	-0.0913***	-0.105**	-0.138**	-0.125**
	(0.0333)	(0.0334)	(0.0232)	(0.0536)	(0.0588)	(0.0567)
Initial Political Opinion Gap	0.530***	0.529***	0.529***	0.556***	0.556***	0.576***
(August 2013)	(0.0321)	(0.0317)	(0.0317)	(0.0349)	(0.0316)	(0.0331)
Specification	OLS	OLS	IV	OLS	OLS	OLS
Instrumental Variable:	No		Alphabetical Distance	No		
Controls	No	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	52,326	52,326	52,326	3,993	3,697	2,668
R-squared	0.341	0.346	0.346	0.385	0.391	0.420
Mean Dependent Variable	1.927	1.927	1.927	1.964	1.995	1.984
St. Dev. Dependent Variable	(1.469)	(1.469)	(1.469)	(1.491)	(1.485)	(1.501)

Notes: Dyadic specifications of IG's effect on Political Opinion Gaps. Column (3) uses alphabetical distance as instrument for the same IG indicator. Column (4) restricts the sample to pairs within a short alphabetical distance of 30. Column (5) restricts the sample to pairs with the same first letter of the last names. Column (6) applies both conditions. Standard errors are two-way clustered by individual 1's group and by individual 2's group. See Appendix A and Appendix Table A1 for variable and sample definitions, and the standard set of controls.

4.2 Same-IG exposure and friendship formation

While the previous section unambiguously shows that being in the same IG reduces students' political opinion gap, the estimate is averaged among all pairs of students. We now proceed to estimate the effect of friendship on political opinion gap using the same-IG indicator as instrument, as described in subsection 3.2.

We first establish the relevance of the instrumental variable IG_{ij}, i.e., the causal effect of participating in the same IG in August 2013 on forming and maintaining a lasting friendship 6 months later. Columns (1) and (2) of Table 5 present the regression of $Link_{ij}$ on IG_{ij}, with and without observable covariates $\mathbf{X_{ij}}$, yielding an estimate of $\beta_{IG} = \mathbb{E}[Link_{ij}|IG_{ij} = 1, \mathbf{X_{ij}}] - \mathbb{E}[Link_{ij}|IGij = 0, \mathbf{X_{ij}}]$ of around 17%.[31]

Columns (3) to (6) report robustness checks, as described in subsection 3.3 and already performed in subsection 4.1. The estimate remains similar in column (3) where the same-IG indicator is instrumented by alphabetical distance to address potentially selective non-compliance to the IG allocation. So does it in columns (4) to (6), where we further address potential omitted variables that may confound last name orders by considering three restricted samples, namely that of pairs with small alphabetical distance (in the spirit of an RDD of last names), that of pairs of the same first letter of their last names, and the intersection of those two samples.

It is remarkable that this coefficient is more than 10 times larger than any coefficient on observable predetermined characteristics (the next largest coefficients are on students' ZIP code and high school

[31] This table's F-statistics are taken from OLS regressions without the required adjustment for clustered standard errors. Table 6 shows the corrected Kleibergen-Paap cluster-robust F-statistics that account for this issue.

Table 5: Same Group Membership and Friendship Formation (First Stage)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Friendship					
Sample of Pairs:	Full Sample			Alphabetical Distance ≤ 30	Same First Letter	Both Conditions
Same IG	0.166***	0.165***	0.172***	0.168***	0.170***	0.169***
	(0.0185)	(0.0185)	(0.0198)	(0.0180)	(0.0211)	(0.0211)
Initial Political Opinion Gap		-0.000912**	-0.000919**	-0.00332	-0.00386*	-0.00311
(August 2013)		(0.000428)	(0.000429)	(0.00216)	(0.00213)	(0.00267)
Same Gender		0.0136***	0.0136***	0.0335***	0.0286***	0.0363***
		(0.00202)	(0.00202)	(0.00998)	(0.0105)	(0.0132)
Both Female		-0.0111***	-0.0110***	-0.0206*	-0.0151	-0.0191
		(0.00236)	(0.00236)	(0.0119)	(0.0137)	(0.0176)
Same Nationality		0.00432	0.00426	0.0109	0.0108	0.00438
		(0.00381)	(0.00380)	(0.0233)	(0.0364)	(0.0399)
Same Admission Type		0.00530***	0.00530***	0.00632	0.00209	0.00396
		(0.00143)	(0.00143)	(0.00898)	(0.00797)	(0.0108)
Both Affirmative Action		-0.00387	-0.00392	-0.00162	-0.00916	0.00607
		(0.00695)	(0.00696)	(0.0274)	(0.0302)	(0.0459)
Same Département of High School		0.0113***	0.0112***	0.0126	0.000179	0.00579
		(0.00416)	(0.00415)	(0.0143)	(0.0144)	(0.0192)
Same Region of High School		0.00165	0.00163	0.00860	0.00650	0.00689
		(0.00184)	(0.00184)	(0.0108)	(0.0115)	(0.0157)
Same High School Major		0.00583***	0.00585***	0.0304***	0.0276***	0.0346***
		(0.00148)	(0.00147)	(0.00964)	(0.00881)	(0.0112)
Diff. in Tuition Fees		-7.10e-07***	-7.07e-07***	-4.01e-06***	-3.99e-06***	-6.39e-06***
		(2.73e-07)	(2.73e-07)	(1.44e-06)	(1.26e-06)	(1.67e-06)
Both Free Tuition		0.000611	0.000627	-0.00429	-0.00324	-0.00630
		(0.00160)	(0.00160)	(0.00800)	(0.00806)	(0.0113)
Same Parents Profession		0.000935	0.000927	0.00119	0.00214	0.00155
		(0.00123)	(0.00122)	(0.00795)	(0.00739)	(0.00896)
Same ZIP Code		0.0165***	0.0165***	0.0245	0.0237	0.0318
		(0.00418)	(0.00418)	(0.0207)	(0.0226)	(0.0281)
Same Program		0.0250***	0.0250***	0.0363***	0.0403***	0.0454***
		(0.00201)	(0.00202)	(0.00733)	(0.00681)	(0.00904)
Specification	OLS	OLS	IV	OLS	OLS	OLS
Instrumental Variable:	No		Alphabetical Distance	No		
Controls	No	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	78.87	90.10	82.45	51.23	59.20	21.02
Observations	52,326	52,326	52,326	3,993	3,697	2,668
R-squared	0.029	0.042	0.042	0.119	0.120	0.121
Mean Dependent Variable	0.0178	0.0178	0.0178	0.0546	0.0517	0.0663
St. Dev. Dependent Variable	0.132	0.132	0.132	0.227	0.221	0.249

Notes: Dyadic specifications of IG's effect on friendship formation. F-stats are for the joint significance of the variables included in the model. Column (3) uses alphabetical distance as instrument for the same IG indicator. Column (4) restricts the sample to pairs within a short alphabetical distance of 30. Column (5) restricts the sample to pairs with the same first letter of the last names. Column (6) applies both conditions. Columns (2)-(6) specify the standard set of controls that will be used throughout the paper. Standard errors are two-way clustered by individual 1's group and by individual 2's group. See Appendix A and Appendix Table A1 for variable and sample definitions.

département.).[32] It shows that "exposure by chance" to other students during the first week of a student's college life has an effect on friendship formation several orders of magnitude larger than that of most typical predetermined characteristics obtained from administrative records.

While we also find statistically significant evidence of homophily based on some of those characteristics, including political opinions, gender, background and origin (département of high school, admission category), interest (high school major category), and family income, its role is rather limited in comparison with the effect of IG exposure. The inclusion of those covariates hardly alters the coefficient of IG_{ij}.

The result can be further interpreted as evidence of the first week's special role as a "window of opportunity" for friendship formation. It hints that friendships tend to form at the beginning of college, in activities meant to facilitate socialization with same-cohort peers, and familiarization with a completely new environment, when almost all students still have no friends there yet. It is all the more striking that those friendships can last much longer beyond the window of opportunity, even when the special exposure ends right after this window, and all students become fully exposed to the whole cohort.

4.3 Friendship effect on opinion differences

We now proceed to estimate the effect of friendship on political opinion gap. First, columns (1) and (2) of Table 6 provide the comparison results from OLS regressions of pairwise opinion differences on friendship, respectively without and with covariates. On average friends have lower opinion differences than non-friend pairs, with estimates of -0.10 to -0.13, or 5-6% of the mean difference (1.93), and 7-8% of the standard deviation (1.47) (Table 1). In presence of the covariates (column (2)), the coefficient size shrinks from 0.13 to 0.10. This notable difference indicates the homophily bias due to the omission of observable covariates, and cautions that the bias can be larger if we could account for unobservable determinants of friendship. Hence it is important to address the potential homophily bias due to unobservables by our proposed methodology.

Next, columns (3) and (4) present the causal effect of friendship on opinion gap among compliers over the course of the first 6 months at Sciences Po, with friendship instrumented by the same-IG indicator, respectively without and with covariates. The respective estimates of -0.52 and -0.57, i.e., about half a point on a 1-to-10 scale, are both relatively precise (at least statistically significant at 5%) and substantial, at the level of 27%-30% of the mean difference and 35%-39% of the standard

[32] To interpret the coefficient on differences in tuition fees (proxy for family income brackets), note that tuition fee ranges from zero (i.e., a full scholarship that a fifth of each cohort receives) to full tuition of 10,000 euros.

Table 6: Friendship, Integration Group, and Political Opinion Gaps

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Difference in Political Opinion (March 2014)							
Specification:	OLS	OLS	IV	IV	IV	IV	IV	IV
Instrumental Variable:	No		Same Integration Group		Alphabetical Distance	Same Integration Group		
Sample of Pairs:	Full Sample					Alphabetical Distance ≤ 30	Same First Letter	Both Conditions
Friendship	-0.133***	-0.105**	-0.520**	-0.568***	-0.530***	-0.626**	-0.814**	-0.739**
	(0.0474)	(0.0542)	(0.202)	(0.202)	(0.0929)	(0.299)	(0.334)	(0.310)
Initial Political Opinion	0.529***	0.529***	0.529***	0.528***	0.528***	0.553***	0.553***	0.574***
Gap (August 2013)	(0.0321)	(0.0317)	(0.0322)	(0.0317)	(0.0317)	(0.0351)	(0.0325)	(0.0338)
Controls	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic			78.96	77.75	110.6	84.45	63.27	62.62
Observations	52,326	52,326	52,326	52,326	52,326	3,993	3,697	2,668
R-squared	0.341	0.341	0.340	0.340	0.344	0.380	0.382	0.405
Mean Dependent Variable	1.927	1.927	1.927	1.927	1.927	1.964	1.995	1.984
Std. Dev. Dependent Variable	(1.469)	(1.469)	(1.469)	(1.469)	(1.469)	(1.491)	(1.485)	(1.501)

Notes: Dyadic specifications relating difference in political opinions to friendship link, instrumented by the same-IG indicator in columns (3)-(4) and (6)-(8), and by alphabetical distance in column (5). Column (6) restricts the sample to pairs within a short alphabetical distance of 30. Column (7) restricts the sample to pairs with the same first letter of the last names. Column (8) applies both conditions. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the standard set of controls.

deviation of opinion differences. A linear extrapolation of this effect to 24 months spent at Sciences Po, given the serious caveats of extrapolation, would imply an effect equivalent to more than the average pairwise difference.[33]

Columns (5) to (8) further confirm the robustness of the friendship effect, using various methods described in subsection 3.3. In column (5), to address potentially selective non-compliance to the IG allocation, we use a more primitive instrument, namely the alphabetical distance between last names, which predicts the same-IG indicator and consequently friendship formation. The effect remains stable at -0.53 (significant at 1%). In addition, we take the IV strategy to more restrictive samples, (i) of pairs with small alphabetical distance in column (6), (ii) of pairs of the same first letter of their last names in column (7), and (iii) of the the intersection of those two samples. In the spirit of an RDD of last names, in the more restricted samples the unobservable characteristics that may correlate with both last names and political opinions become more balanced between assigned and unassigned observations (at the limit they become perfectly balanced, Lee and Lemieux 2010). The IV estimates are larger in magnitude but less precise (partly because the sample size shrinks by 13 to 20 times), and not statistically different from the benchmark estimates.[34]

How much could friendships have contributed to the reduction of the average pairwise opinion difference in the sample, from 2.211 before Sciences Po to 1.932 at the survey? Per dyad, there is

[33]The undergraduate program at Sciences Po includes two years at its campuses and one exchange year abroad.

[34]Appendix Tables A12 and A13 further show that the results are strongly robust to checks by dropping all last names starting with each letter, dropping all students with a specific non-French nationality, or dropping all French family names starting with "de", which might correspond to an aristocratic family background.

on average 0.0178 friendships, so an effect of -0.568 can explain $\frac{0.568 \times 0.0178}{2.194 - 1.927} \sim 3.8\%$ of the change in total pairwise differences (Table 1 Panels C and E). This modest proportion is due to the very low frequency of direct friendships in the dyadic sample. In section 6 we will introduce the effect of second-degree links to re-examine this accounting exercise.

4.4 Analysis of compliers

The IV estimates in Table 6 are much stronger in magnitude than the OLS estimates. This difference is unlikely due to a correction of the homophily bias, which alone would make the IV estimate weaker than the OLS estimate. It is rather related to the LATE interpretation of the average treatment effect among compliers. Appendix Table A3 analyzes and compares some statistics of compliers with those of never-takers and always-takers. The mean residual opinion gap (after partialing out covariates) among untreated compliers is 0.35, while it is -0.21 among treated compliers (the difference between the two constitutes the LATE estimate). In the OLS regression of opinion gap on friendship link, the estimate will be dominated by the comparison between pairs with $L = 0, IG = 0$ (unassigned to integration group, no friendship link) and pairs with $L = 1, IG = 0$ (unassigned always-takers), given the sizes of those subpopulations. Consequently, the OLS estimate should be much closer to zero, because it essentially compares between incomparable groups, namely never-takers plus compliers against always-takers. The strong effect among compliers thus dominates the homophily bias.[35]

Why are average opinions among compliers (treated and untreated) very different from those of never-takers and always-takers? In our context, compliers are the pairs who befriend easily only because of the exposure during the integration week: They are likely pairs that have strong similarities along some dimension. One possibility is that compliers are more likely pairs with more similar political opinions from before Sciences Po.

Indeed, similar calculations (Appendix Table A3) yield that 62% of complier pairs have an opinion gap before Sciences Po of less than 2 (the mean difference), while this figure is only 36% of never-taker pairs and 45% of always-taker pairs. This large difference hints that pairs with similar initial opinions may experience a markedly different friendship, compared with pairs with distinct initial opinions. This suggestion will guide our analysis of the mechanisms in section 5.

[35] Appendix Figure A3 further compares the distributions of the opinion gap (after controlling for covariates) of untreated and treated compliers. The difference clearly shows that treated compliers have smaller opinion gaps.

4.5 Robustness to survey measurement errors

Two concerns arise regarding our measures of friendship and political opinion. First, students who have less than 10 friends may still want to list non-friends to fill up the 10 slots, so the measure of friendship may be inflated beyond what students consider as friends.[36] Second, students may have imperfect, erroneous recalls of their political view from before Sciences Po.

Friendship intensity. To allay the first concern, we utilize students' answers on the intensity of friendship, surveyed on a scale of 1 (mere friendship) to 4 (very close friends), which allows us to run robustness tests using different intensity thresholds for friendship. For those tests, we create a new friendship binary variable D^x, $x \in \{1, \ldots, 4\}$, equal to 1 if the friendship intensity is x or stronger, and 0 otherwise (i.e., any variation of intensity above x or below $x-1$ is collapsed to a single level), and use our IV strategy with D^x_{ij} as the treatment variable instead of the surveyed friendship $Link_{ij}$.[37] Appendix Table A11 reports those test results in columns (2) to (5), confirming that more stringent definitions of friendship would only strengthen the discovered effect. Column (1) further confirms that if we take into full account the reported intensity of friendship, always instrumented by the same-IG membership IG_{ij}, we also find a significant effect of friendship intensity.

Retrospective questions on opinions. Second, we use a retrospective question in the survey in March 2014 on students' political opinions just before they join Sciences Po (the survey is described in subsection 3.4), which raises a potential concern that retrospective answers may incorporate a bias in the direction of the respondent's opinion today. While such a measurement error regarding retrospective survey questions on events and answers may be rather small after only 6 months,[38] the bias on opinions may also relate to the rationalization of new information that results in a hindsight bias, according to which individuals reconstruct their past opinion in light of their newly updated opinion (Fischhoff and Beyth, 1975). It is thus useful to investigate our method's robustness to this issue.

[36] We argue that the incentive for doing this is minimal, since cross-validation means that the non-friends they mention in hope of getting a higher chance of winning the prize must also reciprocate in naming them as friends as well.

[37] That is, we use the same-IG membership IG_{ij} as instrument for the treatment variable D^x_{ij} in a separate specification, to estimate the LATE of improving one degree of friendship intensity at each level. Similarly to the arguments in section 3.2, in this context the instrument still satisfies all conditions for a causal LATE interpretation. The only potential concern is whether the variable IG_{ij} is also a strong instrument for D^x_{ij} for any level of friendship intensity x, a condition that Table A11 duly tests and confirms.

[38] Wagenaar (1986) finds that 20% of subjects forget key personal events after one year. See review by Bradburn et al. (1987).

To evaluate the magnitude of the retrospective answer measurement error, we use the second survey in June 2015 to compare the answers to its retrospective question on recalled opinion back in March 2014 with the actual answers in 2014. First, Appendix Table A5 shows the joint distribution of both surveyed and recalled opinions for 2014. The mass is clearly concentrated on the diagonal, with 90% of the observations not differing more than 1 point between the two measures, implying a very strong correspondence between recalled and actual answers. This lends confidence to the accuracy of the recalled opinion expressed in March 2014 over the political opinion in August 2013.[39]

Appendix Table A6 presents further results on students' recall error, measured as recalled opinion for 2014 minus actual opinion surveyed in 2014. The absolute magnitude of the recall error has practically zero partial correlations with past and present actual political opinions, as shown in column (1). However, in column (2) we do find evidence that the signed recall error is strongly correlated with the change in opinions from 2014 to 2015, signifying that recalled opinions are biased towards present opinions (as surveyed in 2015) by the same magnitude as estimated, e.g., by Fischhoff and Beyth (1975); Biais and Weber (2009); Camerer et al. (1989).

Can the recall error strongly affect our results? First, as less than 10% of answers of the recalled opinion suffer a serious recall error, the resulting bias on our results would probably be small. Second, when we control for the recalled opinion gap in the OLS specification, if this variable is biased towards actual opinion gap of March 2014, it would create an attenuation bias of our coefficient of interest towards zero. It is because the biased variable tends to absorb more variation in the outcome variable than does the latent true opinion, thus attenuates the effect of friendship. Third, the control variables are not needed for the IV strategy's validity, and are only included to improve estimates' precision. Indeed, the results remain very similar, albeit less precise, if we do not control for pre-Sciences Po political opinions.

4.6 Friendship effect on association activities

We proceed to study whether the uncovered effects on students' political opinion also manifest in their behaviors, most naturally in terms of their participation in students' political associations and political parties. Table 7 shows results on the effects of the same-IG indicator and of friendship on the indicator whether a pair of students enroll in the same organization, using both OLS and IV strategies as described in section 3.[40]

[39] Unfortunately, due to reduced budget in 2015, the participation rate in 2015 is much lower than in 2014, resulting in a small sample that overlaps between the two waves that we cannot use as a panel to study friendship effect.

[40] We do not observe the intensity of participation, and could only consider the extensive margin. Another shortcoming is that formal political party enrollment is still very rare among first-year students. Also, most of them have not reached voting age in previous political elections.

Table 7: Friendship, Integration Group, and Association Activities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	Both are members of the same association in						
	Any area		Politics		Activism	Identity	Sports
Specification:	OLS	IV	OLS	IV	OLS	OLS	OLS
Same IG	0.0314**		0.0224*		-0.00193	0.000870	0.0110
	(0.0146)		(0.0125)		(0.00401)	(0.00314)	(0.0107)
Friendship		0.181**		0.129*			
		(0.0766)		(0.0729)			
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		38.21		38.21			
Observations	23,436	23,436	23,436	23,436	23,436	23,436	23,436
R-squared	0.000	0.011	0.000	0.011	0.000	0.000	0.000
Mean Dependent Variable	0.0969	0.0969	0.0229	0.0229	0.00772	0.00375	0.586
St. Dev. Dependent Variable	(0.296)	(0.296)	(0.149)	(0.149)	(0.0875)	(0.0612)	(0.493)

Notes: Dyadic specifications relating the indicator of being members of the same (non-sports) association with the same IG indicator (columns (1), (3), (5)-(7)) and friendship link instrumented by the same-IG indicator (columns (2) and (4)). Columns (3) and (4) focus on associations related to politics, column (5) on activism associations, column (6) on identity-related associations, and column (5) on sports activities,. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, including association categorization, and the set of controls (including initial political opinion gap).

First, being in the same IG (column (1)) and being friends (column (2)) increase the chance of a pair of students joining at least one common (non-sports) association by respectively 3% and 18%. Those effects are sizable in comparison with the average of the dyadic outcome of 8.5% (Table 1), and different from zero at 5% statistical significance.

The effect is primarily driven by common membership in political associations, with the corresponding estimates of 2.2% (columns (3)) and 12.9% (column (4)).[41] In either the OLS or the IV specifications, the effect on political association common membership amount to 71% of the respective effect on common membership in any association. Those effects are especially strong in comparison with the outcome average of 1.8% (Table 1).

For other categories of associations, including that of associations with an activist agenda (column 5), e.g., those dedicated to environmentalist causes, and that of associations defined based on students' origins and identity, e.g., those centered around a certain religion or an ethnic origin (column 6), the OLS estimate of the same-IG indicator is indistinguishable from zero. The estimate for common participation in a sports activity is slightly higher (yet a much smaller proportion of the mean outcome variable of 60%), but still statistically insignificant.[42]

[41] We consider as political associations all those that clearly state their political leaning (e.g., the Movement of Young Socialists) as well as student unions, which hold strong political aspiration albeit an officially apolitical nature. A famous example is the UNEF, declared as apolitical but well-known for its rather radical agenda and its organization of most of the students' loudest manifestations on campus. (As a student, the former French president François Hollande was also president of UNEF.)

[42] Since the reduced form estimates in columns (5) to (8) are close to zero and statistically insignificant, the unreported corresponding IV estimates are even less precise. Similarly, we do not report small and insignificant estimates on students' enrollment in political parties – partly due to the rarity of those formal enrollments.

Table 8: EFFECTS OF INTEGRATION GROUP AND FRIENDSHIP ON MOVEMENT OF OPINION PAIRS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Weak Convergence	Strong Convergence	Weak Divergence		Strong Divergence		Co-movement	
Specification:	OLS	OLS	OLS	IV	OLS	IV	OLS	IV
Same IG	0.00714	-0.00674	-0.0284**		-0.0190***		0.0168*	
	(0.0134)	(0.00881)	(0.0123)		(0.00432)		(0.00964)	
Friendship				-0.172**		-0.115***		0.102*
				(0.0751)		(0.0248)		(0.0588)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic				77.75		77.75		77.75
Observations	44,904	44,904	52,326	52,326	52,326	52,326	52,326	52,326
R-squared	0.103	0.059	0.108	0.106	0.032	0.026	0.017	0.016
Mean Dependent Variable	0.455	0.148	0.228	0.228	0.0380	0.0380	0.182	0.182
St. Dev. Dependent Variable	(0.498)	(0.355)	(0.419)	(0.419)	(0.191)	(0.191)	(0.386)	(0.386)

Notes: Dyadic specifications relating indicators of convergence, divergence, and co-movements of a pair's political opinions to the same IG indicator (columns (1)-(3), (5), (7)) and friendship link instrumented by the same-IG indicator (columns (4), (6), (8)). Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

Taken together, those results show considerable effects of exposure in IGs and of subsequent friendship on students' actual choices beyond their self-reported beliefs. Interestingly, the effect is concentrated among politically-motivated associations.

4.7 Direction of opinion changes

In this section, we investigate the mechanisms that drive our main result, first by considering the types of changes in pairwise opinions from before Sciences Po (August 2013) to March 2014, including cases that a pair's opinions converge strongly or weakly, diverge strongly or weakly, or move in the same direction.[43] Table 8 reports estimates of the effects of the same-IG indicator and of friendship on the corresponding indicators, using both OLS and IV strategies as described in section 3.

The estimates of the effect of being in the same IG on the incidences of weak convergence and strong convergence (columns (1) and (2)) are not statistically significant, and of rather modest magnitude. (Unreported IV estimates of friendship effects are similarly small and statistically insignificant.) On the other hand, columns (3) to (6) show strong evidence that being in the same IG and being friends both reduce the probabilities of weak and strong divergence. The same-IG indicator reduces the probabilities that a pair diverge weakly and strongly by 2.8% and 1.9%, respectively. The analogous effects from having a friendship link are 17.2% and 11.5%, respectively. Those effects are quite large in comparison with the empirical probabilities of weak and strong divergence of 22.8% and 3.8%, respectively.[44] Finally, there is weak evidence that being in the same

[43] As defined in subsection 3.4, strong divergence means the case when both opinions strictly move away from each other. Weak divergence is different in that the opinions may move apart or stay the same. The similar distinction applies to weak and strong convergence.

[44] Table 8's approach focuses on the extensive margin of each category of movement, thus alleviates the concern

IG and forming friendship also increase the possibility of moving in the same direction.

We draw two major conclusions from this subsection. First, the lack of result on convergence shows that IG exposure and friendship do not directly create echo chambers in terms of compressing the diversity of opinions. Those treatments may still preserve existing echo chambers of small groups of similar views by discouraging friends' opinions from drifting apart.

Second, it is important to consider the nonlinearity of the effect of friendship on each other's opinion, as the effect can be dependent on the direction of opinion change. This finding questions the typical assumption of homogenous, linear effects of direct links on one's beliefs, as modeled and estimated in the theoretical and empirical literatures on non-Bayesian learning in networks (Möbius and Rosenblat, 2014). Examples include theories using average-based belief updating processes, namely Golub and Jackson's (2012) generalized definition of DeGroot's (1974) belief updating, and other types of updating (Campbell et al. 2019, Molavi et al. 2018).

5 The homophily-enforced mechanism among similar students

Subsection 4.4 has highlighted a distinctive attribute of complier pairs, in that they are much more likely to have a below-average pre-Sciences Po opinion gap (62% versus 36% among never-takers and 45% among always-takers). This observation leads us to distinguish two types of pairs based on their pre-Sciences Po opinion gap, among whom the effect of friendship may differ substantially.

From this insight, we conjecture and test the "homophily-enforced mechanism" of the friendship effect. To illustrate this mechanism, consider two pairs of students: the first pair, François (F) and Ségolène (S), start Sciences Po with similar political opinions, whereas the second pair, Michel (M) and Dominique (D), have very different pre-Sciences Po political views.[45] Both pair become friends, thanks to F and S's common political interests and M and D's other, non-political common characteristics, e.g., their shared love of fine arts. (Conditional on their becoming friends, homophily implies that M and D are more likely than F and S to share another non-political interest.) Thence, throughout their time at Sciences Po, each pair's friendship conduces to more interaction on their bonding dimension, thus reinforces the corresponding similarity, namely, political views between F and S, and fine arts between M and D.

of the hindsight bias in students' answers to the retrospective question on pre-Sciences Po political opinion. Indeed, even if those answers can be biased towards students' present opinions, this bias does not affect weak divergence or weak convergence indicators. This issue is further discussed in subsection 4.5.

[45] More generally, this type of mechanism can be based on any dimension(s) that may facilitate friendship, not just of political opinions. Also, a priori there is no mechanical relationship between the initial opinion gap and the estimated effects.

Table 9: Initial Political Opinion Gaps and Effects of Friendship and Integration Group

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Difference in Political Opinion (March 2014)					
Sample of Pairs:	Full Sample		Initial Political Opinion Gap			
			< 2		≥ 2	
Specification:	OLS	IV	OLS	IV	OLS	IV
Same IG	-0.177***		-0.147***		-0.0569*	
	(0.0434)		(0.0444)		(0.0338)	
Same IG $\times$ Initial Gap	0.0359***					
	(0.0114)					
Friendship		-0.965***		-0.760***		-0.383
		(0.249)		(0.224)		(0.237)
Friendship $\times$ Initial Gap		0.185**				
		(0.0790)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		18.76		60.14		52.83
Observations	52,326	52,326	21,054	21,054	31,272	31,272
R-squared	0.346	0.343	0.019	0.010	0.307	0.307
Mean Dependent Variable	1.927	1.927	1.196	1.196	2.419	2.419
St. Dev. Dependent Variable	(1.469)	(1.469)	(1.020)	(1.020)	(1.520)	(1.520)

Notes: Dyadic specifications relating difference in political opinions to the same IG indicator (columns (1), (3), (5)) and friendship link instrumented by the same-IG indicator (columns (2), (4), (6)). Column (2) further includes its interaction with the pre-Sciences Po political opinion gap as instrument. Columns (3)-(4) restrict the sample to pairs with below-average gap in pre-Sciences political opinion gap. Columns (5)-(6) restrict the sample to pairs with above-average gap in pre-Sciences Po political opinions. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

Consequently, friendship matters in binding the political views of pairs of friends whose initial views are already similar, while it has no effect on the political gap between those whose initial views are dissimilar.

5.1 Test of mechanism on political opinions

Table 9 tests the homophily-enforced mechanism's implication on the effect of same-IG exposure and friendship on political opinion gap. Columns (1) and (2) reconsider the benchmark regressions in Table 6's columns (2) (same-IG effect) and (4) (friendship effect) respectively, with the addition of an interaction term between the respective treatment variable and the pair's initial political opinion gap.[46] In both columns, the estimated coefficient of the interaction term is distinctively positive, signifying that the estimated negative effects of being in the same IG and being friends come from pairs of low initial gap in political opinions, and fade to zero as the initial gap reaches 5.

The mechanism is further illustrated when we partition the sample into (i) pairs with a below-average (less than 1.9) initial opinion gap (columns (3) and (4)) and (ii) pairs with an above-average initial opinion gap (columns (5) and (6)). Comparing the former to the latter sample, the same-IG effect is 2.5 times larger and the friendship effect is 2 times larger, and statistical significance is also much stronger, albeit a subsample only two third as large.

As subsection 4.7 has shown the effects' asymmetric strength in reducing divergence of opinions

[46]In column (2), as there are now two instrumented variables, in addition to using the same-IG indicator as IV, we also use its interaction with the pair's initial political opinion gap as an additional IV.

Table 10: Initial Political Opinion Gaps and Effects on Strong Divergence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Strong Divergence in Political Opinion						Extremism	
Sample of Pairs:	Full		Initial Political Opinion Gap					
	Sample		< 2		≥ 2		< 2	
Specification:	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Same IG	-0.0453***		-0.0382***		-0.00618**		-0.0267**	
	(0.0106)		(0.0111)		(0.00308)		(0.0129)	
Same IG × Initial Gap	0.0113***							
	(0.00273)							
Friendship		-0.249***		-0.198***		-0.0416*		-0.138***
		(0.0571)		(0.0523)		(0.0229)		(0.0325)
Friendship × Initial Gap		0.0627***						
		(0.0151)						
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		18.76		60.14		52.83		62.37
Observations	52,326	52,326	21,054	21,054	31,272	31,272	21,054	21,054
R-squared	0.032	0.022	0.034	0.026	0.009	0.007	0.041	0.037
Mean Dependent Variable	0.038	0.038	0.0715	0.0715	0.0155	0.0155	0.119	0.119
St. Dev. Dependent Variable	(0.191)	(0.191)	(0.258)	(0.258)	(0.124)	(0.124)	(0.324)	(0.324)

Notes: Dyadic specifications relating the incidence of strong divergence (columns (1) to (6)) and extremism (columns (7) and (8)) to the same IG indicator (columns (1), (3), (5), (7)) and friendship link instrumented by the same-IG indicator (columns (2), (4), (6), (8)). Column (2) further includes its interaction with the pre-Sciences Po political opinion gap as instrument. Columns (3)-(4) restrict the sample to pairs with below-average gap in pre-Sciences political opinion gap. Columns (5)-(6) restrict the sample to pairs with above-average gap in pre-Sciences Po political opinions. The outcome variable in columns (7)-(8) is the indicator whether at least one among the pair holds an extreme view, namely 1, 2, or 9, 10. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

(while not inducing convergence), Table 10 elaborates that pattern based on the initial political opinion gap. First, columns (1) and (2) respectively report the same-IG and friendship effects on the probability of strong divergence (both opinions moving apart), now including an interaction term between the respective treatment variable and the pair's initial political opinion gap.[47] The estimated coefficient of the interaction term is distinctively positive, implying that the same-IG indicator and friendship only matter negatively to the probability of divergence among pairs of low initial gap, while their effects fade away as the initial gap reaches 4.

Similar to Table 9, columns (3) and (4) consider the subsample of pairs with a below-average (less than 1.9) initial opinion gap, while columns (5) and (6) focus on pairs with an above-average gap. Again, we find that the same-IG effect (friendship effect) among initially similar pairs is more than 6 times (close to 5 times) larger than among initially dissimilar pairs. While the effects remain somewhat statistically significant in the latter subsample, their statistical significance is also much stronger in the former subsample. Hence, the evidence in Tables 9 and 10 strongly corroborates the homophily-based mechanism, in that the discovered effects work mostly through initially politically similar pairs.

[47] In column (2), to instrument for both friendship and its interaction with the initial opinion gap, we use both the same-IG indicator and its interaction with the initial opinion gap.

Effects on extremism. An important corollary of the discouraging effects of same-IG exposure and friendship on opinion divergence is that same-IG exposure and friendship must also reduce the incidence of extremism, which can be measured by the indicator whether at least one individual in a pair holds an extreme view, i.e., in the set $\{1, 2, 9, 10\}$. Columns (7) and (8) test this corollary by estimating the effects of the same-IG indicator and friendship on this indicator of extremism, again among the subsample of initially politically similar pairs. The estimates are strongly negative and statistically significant, as being in the same IG reduces the chance of extremism by 2.7%, while friendship does it by 13.8% (given the mean dependent variable of 16.6% in the full sample).[48]

Appendix Table A9 also shows corroborating evidence that IG exposure reduces extremist views in monadic specifications that regress the incidence of extremism on IG-based variables and controls. A student is less likely to hold an extremist view if (s)he is exposed to more same-IG students holding an initially moderate (i.e., non-extremist) view.

5.2 Test of mechanism on association activities

The homophily-enforced mechanism also implies that initially politically similar friends tend to interact more on political topics. In Table 11, we test this prediction by estimating the same-IG and friendship effects on a pair's probability of joining the same association, focusing on the sample of below-average (less than 2) initial political opinion gap. Columns (1), (3), (5), and (7) report different OLS estimates of the same-IG effect, and columns (2), (4), (6), and (8) the IV estimates of the friendship effect, in which friendship is instrumented by the same-IG indicator. In comparison, Appendix Table A10 shows results of the same specifications on the sample of above-average initial political opinion gap.

The estimates in columns (1) and (2) of 4.9% and 22.3% are statistically significant at 5%, and respectively 2.2 times and 1.5 times larger than their statistically insignificant counterparts in Appendix Table A10. The former are also 56% and 23% stronger than their counterparts from the full sample as shown in Table 7. The evidence thus suggests that a large part of the effect on association activities comes from initially politically similar pairs.

This observation is further supported by the results that focus on participation in the same political association. Among initially politically similar pairs, the effects of being in the same IG

[48] However, as the probability of being in the same IG or being friends is rather low (1.9% and 1.8%, respectively), those effects only play a weak role in the reduction of the mean dependent variable from 19.4% to 16.6% in the observed sample, at 1.5% and 9.0% respectively. To calculate those proportions, we also use the shares of initially similar and dissimilar pairs, and the corresponding estimates in the sample of initially politically dissimilar pairs of 0.0210 and 0.141, respectively.

Table 11: Effects on Association Activities among Politically Similar Pairs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Both are members of the same association				Both are members of			
	Of any type		In Politics		Some association in Politics		Different associations in Politics	
Specification:	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Sample of Pairs:	Initial Political Opinion Gap < 2							
Same IG	0.0490**		0.0531***		0.0584**		0.00478	
	(0.0210)		(0.0186)		(0.0285)		(0.0289)	
Friendship		0.223**		0.242**		0.266*		0.0217
		(0.0968)		(0.0988)		(0.149)		(0.132)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		34.53		34.53		34.53		34.53
Observations	9,393	9,393	9,393	9,393	9,393	9,393	9,393	9,393
R-squared	0.011	0.026	0.021	0.036	0.039	0.039	0.027	0.026
Mean Dependent Variable	0.110	0.110	0.033	0.033	0.176	0.176	0.144	0.144
St. Dev. Dependent Variable	(0.313)	(0.313)	(0.178)	(0.178)	(0.381)	(0.381)	(0.351)	(0.351)

Notes: Dyadic specifications relating association membership to the same IG indicator (columns (1), (3), (5), (7)) and to friendship link instrumented by the same-IG indicator (columns (2), (4), (6), (8)). The sample only includes pairs of students with below-average (less than 2) pre-Sciences Po political opinion gap. The dependent variable is an indicator whether each pair are members in the same association (columns (1)-(2)), whether they are members in the same association in politics (columns (3)-(4)), whether both are members of some (not necessarily the same) association in politics (columns (5)-(6)), and whether they are both members of some association in politics, but not members of the same one (columns (7)-(8)). Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

(column (3)) and being friends (column (4)) are respectively 5.3% and 24.2% (both statistically significant at 5%). They are respectively 7.7 times and 5.2 times larger than the counterparts from Appendix Table A10, and 2.4 times and 1.9 times larger than those from the full sample in Table 7.

The evidence of the same-IG and friendship effects on same-association participation may still highlight a slightly different channel, in that friends influence each other's interest but need not interact more. For example, a pair of friends may both get interested in politics, but choose to follow different political currents. To test this possibility, we investigate whether friends are more likely to join some association

Interactions and common interest in politics. The effects of same-IG exposure and friendship on association participation may signify that (i) friends choose to join the same associations to interact more, and that (ii) they influence each other's interest, which results in the choice of the same association. In the latter case, it is interesting to check if the influence on interests spills over to different associations of the same type: for example, two friends who reinforce each other's interest in politics eventually choose to join some political associations, but not necessarily the same. We test this possibility in columns (5) to (8). While there is clear evidence that same-IG exposure and friendship increase the probability that a pair both join some political associations, there is no such effect (with estimates close to zero) on the possibility of joining different associations of this category. The results imply that the mechanism works mostly through increased interactions, rather than influenced interests.

Table 12: Social Distance and Political Opinion Gap

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Difference in Political Opinion (March 2014)							
Sample of Pairs by Social Distance:	Any value		Distance $\in \{1, 2\}$		Distance $\in \{2, 3\}$		Distance ≥ 3	
Specification:	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Same IG	-0.0937***		-0.135**		-0.0973**		-0.0541	
	(0.0334)		(0.0574)		(0.0450)		(0.0559)	
Social Distance		0.162***		0.382**		0.448**		-40.11
		(0.0544)		(0.169)		(0.193)		(789.0)
Initial Political Opinion Gap (August 2013)	0.529***	0.528***	0.502***	0.500***	0.516***	0.514***	0.532***	0.621
	(0.0317)	(0.0317)	(0.0351)	(0.0349)	(0.0324)	(0.0326)	(0.0316)	(1.701)
Observations	52,326	52,326	6,243	6,243	26,664	26,664	46,083	46,083
R-squared	0.346	0.341	0.327	0.321	0.333	0.318	0.348	-250.613
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		78.06		227		36.82		0.00252

Notes: Dyadic specifications relating difference in political opinions to the same IG indicator (columns (1), (3), (5), (7)) and social distance instrumented by the same-IG indicator (columns (2), (4), (6), (8)). A pair's social distance is the length of the shortest path connecting them on the (connected) social network of all students. Columns (3)-(4) restrict the sample to pairs with social distances of 1 and 2 (direct friends and their friends). The similar restriction is $\{2, 3\}$ for columns (5)-(6), and $\{3, \ldots\}$ for columns (7)-(8). Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

In sum, the findings from Tables 9, 10, and 11 support our proposed homophily-enforced mechanism. By homophily, students form new friendships because of their chance encounter (in an IG in this paper's context) according to one or a few dimensions in which they share common interest. An important area can be politics: certain pairs of students form friendships because of their similarity in political opinions. Others, however, may form friendships along other dimensions, and still become friends despite large political differences. Subsequently, pairwise interactions are shaped along the lines of common interests, so friends with similar political opinions tend to often discuss politics and join the same political associations, while friends with dissimilar opinions likely strengthen their relationship through other dimensions of homophily. In consequence, friends who begin with similar political opinions continue to influence each other's political opinions, while friends who start with large political differences do not exert much influence on each other's opinions.

This mechanism echoes Golub and Jackson's (2012) analysis on homophily and the speed of convergence in beliefs, but with the introduction of an endogenous selection of the dimension of interaction based on homophilous preferences. Since our newly discovered empirical facts imply a rather nonlinear mechanism of diffusion of beliefs, notably in the asymmetry between converging and diverging, it would be interesting to reconsider their results in light of those facts.

6 Network effects beyond direct friendship

Effects of social distance. The collected information on friendship links allows us to reconstruct the network of students to better understand the friendship effect beyond separately considering pairs of students. We further investigate dyadic opinions on this network beyond direct friends. Table 12 reports results by social distance, defined as the shortest path length between any pair in the network. While column (1) replicates the same-IG effect shown in Table 6, column (2) estimates the effect of increasing social distance on dyadic political opinion gap, using the same-IG indicator as instrument for social distance. The first stage is again very strong, and the estimated effect of increasing social distance is statistical significant at 1%. The estimated effect is interpretable as an Average Causal Response, namely a weighted average over causal effects among all pairs that comply to the IV to move closer in social distance (Angrist and Imbens, 1995; Angrist and Pischke, 2008).

In this direction, we restrict the sample into subsamples to compare between pairs of consecutive social distances: social distance 1 versus 2 in columns (3) and (4), 2 versus 3 in column (5) and (6), and 3 versus farther distances in columns (7) and (8). In each column, we still use the same-IG indicator as instrument for social distance, subject to the strength of the first stage. We find strong effects on political opinion gap when social distance shrinks from 2 to 1 (indirect friends becoming direct friends) and from 3 to 2, at respectively 0.38 and 0.45 (the effect of moving from distance 3 to 1 is then their sum 0.83). Beyond two degrees of network distance, the same-IG indicator no longer matters to social distance or to opinion gap.[49]

Similar to subsection 4.3, we perform a simple calculation of how much of the consolidation of political opinions among students, seen in the reduction of the average pairwise opinion difference from 2.194 to 1.927 ((Table 1 Panel E), can be attributed to newly formed direct friendships and second-degree links (friends of friends). We consider the pre-Sciences Po network as empty,[50] and note from Table 12 that a new friendship between two students causally reduces their political opinion difference by $0.382 + 0.448$, while the effect of a new second-degree link is 0.448. Using the frequencies of direct and second-degree friendships at 0.0178 and 0.1014 (Table 1 Panel C), the explained proportion is $\frac{(0.382+0.448)\times 0.0178+0.448\times 0.1014}{2.194-1.927} = 22.5\%$ (with a standard error of 9.7%). This high figure should be taken with caution, however, as its 95% confidence interval ranges from 4% to 42%.

Effects by network position. Next, we study whether students' positions in the social network, measured in terms of network centrality, may matter to the effects of being in the same IG and

[49]This evidence resonates Leider et al.'s (2009) description that from a social preference perspective, indirect friends of distance 3 are not distinguishable from strangers.

[50]Among pairs of friends in the sample, only 0.2%, i.e., 2 pairs, had known each other before Sciences Po.

Table 13: Friendship Effect and Network Centrality

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Difference in Political Opinion (March 2014)					
Sample of Pairs by Eigenvector Centrality:	Both Central		One Central and One Non-Central		Both Non-Central	
Specification:	OLS	IV	OLS	IV	OLS	IV
Same IG	0.0308		-0.126*		-0.101**	
	(0.115)		(0.0711)		(0.0499)	
Friendship		0.0733		-0.905*		-0.757**
		(0.273)		(0.536)		(0.363)
Initial Political Opinion Gap (August 2013)	0.519***	0.519***	0.530***	0.530***	0.530***	0.529***
	(0.0679)	(0.0680)	(0.0365)	(0.0365)	(0.0325)	(0.0327)
Observations	3,160	3,160	19,520	19,520	29,646	29,646
R-squared	0.295	0.295	0.334	0.331	0.359	0.356
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		28.78		43.72		87.77

Notes: Dyadic specifications relating difference in political opinions to the same IG indicator (columns (1), (3), (5)) and friendship link instrumented by the same-IG indicator (columns (2), (4), (6)). A central individual is one whose eigenvector centrality is in the top quartile. Across the table, the sample is restricted to pairs of two central individuals (columns (3)-(4)), pairs of one central and one non-central individuals (columns (5)-(6)), and pairs of two non-central individuals (columns (7)-(8)). Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

being friends on political opinion gap. This question is related to the argument that centrality in social networks also conveys information about a person's influence and charisma. If that is the case, the friendship effect should be much larger for pairs of a star, i.e., a highly-central student, and a non-star, than pairs of two non-stars or pairs of two stars. Table 13 explores this idea using the measure of *eigenvector centrality*, according to which a student's centrality measure is a linear combination of his friends' centrality. The three columns consider respectively the subsamples of pairs of two stars (column (1)), of one star and one non-star (column (2)), and of two non-stars (column (3)), for which a star is defined as a student whose eigenvector centrality is in the top quartile of its distribution. The same-IG and friendship effects are not present among pairs of stars, with noisily positive, statistically insignificant estimates. Among the other two subsamples, the effect is rather strong and statistically significantly negative. Overall, the evidence is consistent with the view that network stars are hardly influenceable, whereas non-stars tend to be more influenceable. However, network stars do not necessarily wield stronger influence than non-stars.

7 Concluding remarks

In this paper, we investigate empirically how incoming Sciences Po students' exposure to each other in the integration group (IG) and their newly-formed friendship may shape their political views 6 months later. We find that common exposure and friendship cause a substantial reduction in the gap between students' political views, at the respective levels of 5% (same-IG effect) and 30% (friendship effect) of the average opinion gap. Treated pairs are also more likely to participate in the same political association. Those treatments do not cause opinions to converge to each other; instead, they prevent opinions from diverging. Consequently, friendship tends to reduce polarization and extremist views among students, without actively creating echo chambers.

The results are consistent with what we term the "homophily-enforced" mechanism. When students whose initial political views are similar become friends, they continue to interact on related topics, as shown by their participation in the same political associations (and not other types of associations). Such continual interactions, as enforced by homophily, are the factor that produces the strong effect of friendship among students with high similarity in political opinions before joining Sciences Po. In consequence, those pairs of students are strongly discouraged from diverging, and thus less likely to hold extreme political views. In contrast, friends who were politically dissimilar before Sciences Po would not follow this path, hence friendship has little effect on their political opinions.

Our results also entail noteworthy nonlinearity and heterogeneity in the transmission of opinions in the network of friends. The asymmetries between friendship effects on convergence versus divergence, and between stars and non-stars (as defined by eigenvector centrality), suggest that models of non-Bayesian learning in networks based on homogenous, linear effects of direct friends (e.g., Campbell et al. 2019, Molavi et al. 2018, and Golub and Jackson 2012's generalized definition of DeGroot 1974's belief updating) may be systematically over-simplifying how much individuals learn from friends (Möbius et al., 2015).

The scope of those results' external validity is limited by the particularity of Sciences Po as the cradle of French politics and high-level bureaucracy. However, going beyond the paper's context, we believe that the paper's method can be useful in studying a broad range of questions regarding the effect of friendship and connection on individual outcomes, especially when it is possible to design groups of interaction to optimize their effect in forming links. For example, on the topic of the integration of refugees and immigrants, our results suggest that encouraging immigrants and natives to reside in the same neighborhoods and interact regularly would be useful to induce friendships between them and foster immigrants' integration. Furthermore, we can estimate the impacts of such a policy through friendship effects. This is an exciting area that we leave for future research.

References

Acemoglu, Daron, Munther Dahleh, Ilan Lobel, and Asuman Ozdaglar, "Bayesian learning in social networks," *The Review of Economic Studies*, 2011, *78* (4), 1201–1236. 4

Advani, Arun and Bansi Malde, "Credibly identifying social effects: Accounting for network formation and measurement error," *Journal of Economic Surveys*, 2018, *32* (4), 1016–1044. 5

Allcott, Hunt and Matthew Gentzkow, "Social Media and Fake News in the 2016 Election," *The Journal of Economic Perspectives*, 2017, *31* (2), 211–235. 2

Allport, Gordon Willard, *The Nature of Prejudice*, Cambridge, MA: Addison-Wesley, 1954. 5

Angrist, Joshua and Guido Imbens, "Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity," *Journal of the American Statistical Association*, 1995, *90*, 430–442. 37

_ **and Jörn-Steffen Pischke**, *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton, NJ: Princeton University Press, December 2008. 37

Angrist, Joshua D. and Kevin Lang, "Does school integration generate peer effects? Evidence from Boston's Metco program," *The American Economic Review*, 2004, *94* (5), 1613–1634. 4

Badev, Anton, "Nash Equilibria on (Un)Stable Networks," *Econometrica*, forthcoming. 4

Bala, Venkatesh and Sanjeev Goyal, "Learning from Neighbours," *The Review of Economic Studies*, 1998, *65* (3), 595–621. 4

_ **and** _ , "Conformism and Diversity under Social Learning," *Economic Theory*, 2001, *17* (1), 101–120. 4

Biais, Bruno and Martin Weber, "Hindsight Bias, Risk Perception, and Investment Performance," *Management Science*, 2009, *55* (6), 1018–1029. 28

Bifulco, Robert, Jason M. Fletcher, and Stephen L. Ross, "The Effect of Classmate Characteristics on Post-secondary Outcomes: Evidence from the Add Health," *American Economic Journal: Economic Policy*, February 2011, *3* (1), 25–53. 4

Blume, Lawrence E., William A. Brock, Steven N. Durlauf, and Yannis M. Ioannides, "Identification of Social Interactions," in Jesse Benhabib, Alberto Bisin, and Matthew O. Jackson, eds., *Handbook of Social Economics*, Vol. 1, Amsterdam: North Holland, 2011, pp. 853 – 964. 5

Boisjoly, Johanne, Greg J. Duncan, Michael Kremer, Dan M. Levy, and Jacque Eccles, "Empathy or Antipathy? The Impact of Diversity," *American Economic Review*, December 2006, *96* (5), 1890–1905. 5

Boucher, V., S. Tumen, M. Vlassopoulos, J. Wahba, and Y. Zenou, "Ethnic mixing in early childhood," CEPR Discussion Paper No. 15528, 2020. 5

Boxell, Levi, Matthew Gentzkow, and Jesse Shapiro, "Greater Internet Use is Not Associated with Faster Growth in Political Polarization among US Demographic Groups," *Proceedings of the National Academy of Sciences*, 2018, *115* (3), 10612–7. 2

Bradburn, Norman M., Lance J. Rips, and Steven K. Shevell, "Answering Autobiographical Questions: The Impact of Memory and Inference on Surveys," *Science*, 1987, *236* (4798), 157–161. 27

Bramoullé, Yann, Andrea Galeotti, and Brian Rogers, *The Oxford handbook of the economics of networks*, Oxford, UK: Oxford University Press, 2016. 5

_ , Habiba Djebbari, and Bernard Fortin, "Identification of peer effects through social networks," *Journal of Econometrics*, 2009, *150* (1), 41 – 55. 4, 5

Cai, Jing and Adam Szeidl, "Interfirm Relationships and Business Performance," *The Quarterly Journal of Economics*, 2018, *133* (3), 1229–1282. 5

Calvó-Armengol, Antoni, Eleonora Patacchini, and Yves Zenou, "Peer Effects and Social Networks in Education," *The Review of Economic Studies*, 2009, *76* (4), 1239–1267. 4

Camerer, Colin, George Loewenstein, and Martin Weber, "The Curse of Knowledge in Economic Settings: An Experimental Analysis," *Journal of Political Economy*, 1989, *97* (5), 1232–1254. 28

Cameron, Colin and Douglas Miller, "Robust inference for dyadic data," *Unpublished, University of California-Davis*, 2014. 10

_ , Jonah Gelbach, and Douglas Miller, "Robust inference with multiway clustering," *Journal of Business & Economic Statistics*, 2011, *29* (2), 238–249. 10

Campbell, Arthur, C. Matthew Leister, and Yves Zenou, "Social Media and Polarization," *CEPR Discussion Paper*, 2019. 4, 31, 39

Cantoni, Davide, Yuyu Chen, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang, "Curriculum and Ideology," *Journal of Political Economy*, 2017, *125* (2), 338–392. 14

Carlsson, Magnus, Gordon B. Dahl, and Dan-Olof Rooth, "Backlash in Attitudes After the Election of Extreme Political Parties," Working Paper 21062, National Bureau of Economic Research 2015. 5

Carrell, Scott E., Bruce I. Sacerdote, and James E. West, "From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation," *Econometrica*, 2013, *81* (3), 855–882. 5

Chandrasekhar, Arun and Randall Lewis, "Econometrics of sampled networks," *Unpublished, MIT*, 2011. 14

— **, Horacio Larreguy, and Juan Pablo Xandri**, "Testing Models Of Social Learning On Networks: Evidence From Two Experiments," *Econometrica*, 2020, *88* (1), 1–32. 4

Corno, L., E. La Ferrara, and J. Burns, "Interaction, Stereotypes and Performance: Evidence from South Africa," 2019. Unpublished manuscript, Bocconi University. 5

Currarini, Sergio, Matthew O. Jackson, and Paolo Pin, "An Economic Model of Friendship: Homophily, Minorities, and Segregation," *Econometrica*, 2009, *77* (4), 1003–1045. 2

de Chaisemartin, Clement, "Tolerating defiance? Local average treatment effects without monotonicity," *Quantitative Economics*, 2017, *8* (2), 367–396. 12

De Paula, Aureo, "Econometrics of network models," in Bo Honoré, Ariel Pakes, Monika Piazzesi, and Larry Samuelson, eds., *Advances in Economics and Econometrics: Theory and Applications: Eleventh World Congress*, Cambridge: Cambridge University Press, 2017, pp. 268 – 323. 4

— , "Econometric Models of Network formation," *Annual Review of Economics*, 2020, *12*, 775–799. 4

DeGiorgi, Giacomo, Michele Pellizzari, and Silvia Redaelli, "Identification of Social Interactions through Partially Overlapping Peer Groups," *American Economic Journal: Applied Economics*, 2010, *2* (2), 241–275. 4

DeGroot, Morris H., "Reaching a consensus," *Journal of the American Statistical Association*, 1974, *69* (345), 118–121. 4, 31, 39

DellaVigna, Stefano and Ethan Kaplan, "The Fox News effect: Media bias and voting," *The Quarterly Journal of Economics*, 2007, *122* (3), 1187–1234. 5

— **and Matthew Gentzkow**, "Persuasion: Empirical Evidence," *Annual Review of Economics*, 2010, *2* (1), 643–669. 5

DeMarzo, Peter, Dimitri Vayanos, and Jeffrey Zwiebel, "Persuasion Bias, Social Influence, and Unidimensional Opinions," *The Quarterly journal of economics*, 2003, *118* (3), 909–968. 4

Epple, Dennis and Richard E. Romano, "Peer Effects in Education: A Survey of the Theory and Evidence," in Jesse Benhabib, Alberto Bisin, and Matthew O. Jackson, eds., *Handbook of Social Economics*, Vol. 1, Amsterdam: North Holland, 2011, pp. 1053–1163. 5

Fafchamps, Marcel and Flore Gubert, "Risk Sharing and Network Formation," *American Economic Review*, 2007, *97* (2), 75–79. 10

Fischhoff, Baruch and Ruth Beyth, "I knew it would happen ? Remembered probabilities of once-future things," *Organizational Behavior and Human Performance*, 1975, *13*, 1–16. 27, 28

Fisher, Ronald A., *The Design of Experiments, Edinburgh: Oliver and Boyd.*, Edinburgh: Oliver and Boyd, 1935. 8

Foster, Gigi, "It's not your peers, and it's not your friends: Some progress toward understanding the educational peer effect mechanism," *Journal of Public Economics*, 2006, *90*, 1455–1475. 5

Gabel, Matthew and Kenneth Scheve, "Estimating the Effect of Elite Communications on Public Opinion Using Instrumental Variables," *American Journal of Political Science*, 2007, *51* (4), 1013–1028. 5

Gentzkow, Matthew, "Television and Voter Turnout," *The Quarterly Journal of Economics*, 2006, *121* (3), 931–972. 5

_ , Jesse Shapiro, and Michael Sinkinson, "The Effect of Newspaper Entry and Exit on Electoral Politics," *American Economic Review*, December 2011, *101* (7), 2980–3018. 5

Gerber, Alan, Dean Karlan, and Daniel Bergan, "Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions," *American Economic Journal: Applied Economics*, April 2009, *1* (2), 35–52. 5

Goeree, Jacob, Margaret McConnell, Tiffany Mitchell, Tracey Tromp, and Leeat Yariv, "The 1/d law of giving," *American Economic Journal: Microeconomics*, 2010, *2* (1), 183–203. 14

Goldsmith-Pinkham, Paul and Guido Imbens, "Social networks and the identification of peer effects," *Journal of Business & Economic Statistics*, 2013, *31* (3), 253–264. 4

Golub, Ben and Evan Sadler, "Learning in Social Networks," in Yann Bramoullé, Andrea Galeotti, and Brian Rogers, eds., *The Oxford Handbook of the Economics of Networks*, Oxford, UK: Oxford University Press, 06 2016, chapter 19. 4

Golub, Benjamin and Matthew O. Jackson, "Naïve Learning in Social Networks and the Wisdom of Crowds," *American Economic Journal: Microeconomics*, 2010, *2* (1), 112–49. 4

_ and _ , "How homophily affects the speed of learning and best-response dynamics," *The Quarterly Journal of Economics*, 2012, *127* (3), 1287–1338. 2, 4, 31, 36, 39

Goyal, Sanjeev, "Learning in networks," in Jesse Benhabib, Alberto Bisin, and Matthew O. Jackson, eds., *Handbook of Social Economics*, Vol. 1, Amsterdam: North Holland, 2011, pp. 679–727. 4

Graham, Bryan S., "Methods of Identification in Social Networks," *Annual Review of Economics*, 2015, *7* (1), 465–485. 4

_ **and Aureo De Paula**, *The Econometric Analysis of Network Data*, New York, NY: Academic Press, 2020. 5

Grimm, Veronika and Friederike Mengel, "Experiments on Belief Formation in Networks," *Journal of the European Economic Association*, 2020, *18* (1), 49–82. 4

Guess, Andrew, Brendan Nyhan, Benjamin Lyons, and Jason Reifler, *Avoiding the Echo Chamber about Echo Chambers*, Knight Foundation, 2018. 2

Imbens, Guido, "Better LATE than nothing: Some comments on Deaton (2009) and Heckman and Urzua (2009)," *Journal of Economic Literature*, 2010, *48* (2), 399–423. 12

_ **and Joshua Angrist**, "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 1994, *62* (2), 467–475. 11, 12

Ioannides, Yannis M., *From Neighborhoods to Nations: The Economics of Social Interactions*, Princeton, NJ: Princeton University Press, 2013. 5

Jackson, Matthew O., *Social and Economic Networks*, Princeton, NJ: Princeton University Press, 2008. 5

_ , "An overview of social networks and economic applications," in Jesse Benhabib, Alberto Bisin, and Matthew O. Jackson, eds., *Handbook of Social Economics*, Vol. 1, Amsterdam: North Holland, 2011, pp. 511–585. 5

_ **, Brian Rogers, and Yves Zenou**, "The Economic Consequences of Social-Network Structure," *Journal of Economic Literature*, 2017, *55* (1), 49–95. 5

Kendall, Chad, Tommaso Nannicini, and Francesco Trebbi, "How Do Voters Respond to Information? Evidence from a Randomized Campaign," *American Economic Review*, 2015, *105* (1), 322–53. 5

Kennedy, Peter E., "Randomization Tests in Econometrics," *Journal of Business & Economic Statistics*, 1995, *13* (1), 85–94. 8

Kitagawa, Toru, "A Test for Instrument Validity," *Econometrica*, 2015, *83* (5), 2043–2063. 12, 53, 54

Lazarsfeld, Paul and Robert Merton, "Friendship as a social process: A substantive and methodological analysis," in Morroe Berger, Theodore Abel, and Charles H. Page, eds., *Freedom and control in modern society*, New York: Van Nostrand, 1954, pp. 18–66. 2

_ **, Hazel Gaudet, and Bernard Berelson**, *The People's Choice: How the Voter Makes Up His Mind in a Presidential Campaign.*, New York, NY: Columbia University Press, 1944. 2

Lee, David S. and Thomas Lemieux, "Regression Discontinuity Designs in Economics," *Journal* *of Economic Literature*, 2010, *48* (2), 281–355. 13, 25

Lee, Lung-Fei and Xiaodong Liu, "Efficient GMM estimation of high order spatial autoregressive models with autoregressive disturbances," *Econometric Theory*, 2010, *26* (1), 187–230. 5

Leider, Stephen, Markus Möbius, Tanya Rosenblat, and Quoc-Anh Do, "Directed altruism and enforced reciprocity in social networks," *The Quarterly Journal of Economics*, 2009, *124* (4), 1815–1851. 3, 14, 16, 37

_ , _ , _ , and _ , "What do we expect from our friends?," *Journal of the European Economic Association*, 2010, *8* (1), 120–138. 3

Lin, Xu, "Identifying Peer Effects in Student Academic Achievement by Spatial Autoregressive Models with Group Unobservables," *Journal of Labor Economics*, 2010, *28* (4), 825–860. 5

List, John, Fatemeh Momeni, and Yves Zenou, "The social side of early human capital formation: Using a field experiment to estimate the causal impact of neighborhoods," *NBER Working Paper No. 28283*, 2020. 5

Liu, Xiaodong, Eleonora Patacchini, and Yves Zenou, "Endogenous peer effects: local aggregate or local average?," *Journal of Economic Behavior & Organization*, 2014, *103*, 39–59. 5

Lowe, Matt, "Types of contact: A field experiment on collaborative and adversarial caste integration," *American Economic Review*, forthcoming. 5

Manski, Charles F., "Identification of Endogenous Social Effects: The Reflection Problem," *The Review of Economic Studies*, 1993, *60* (3), 531–542. 2, 5

McPherson, Miller, Lynn Smith-Lovin, and James M Cook, "Birds of a Feather: Homophily in Social Networks," *Annual Review of Sociology*, 2001, *27* (1), 415–444. 2

Mele, Angelo, "A Structural Model of Dense Network Formation," *Econometrica*, 2017, *85* (3), 825–850. 4

Merlino, L. P., M. F. Steinhardt, and L. Wren-Lewis, "More than just friends? School peers and adult interracial relationships," *Journal of Labor Economics*, 2019, *37*, 663–713. 5

Möbius, Markus and Tanya Rosenblat, "Social Learning in Economics," *Annual Review of Economics*, 2014, *6* (1), 827–847. 4, 31

_ , Tuan Phan, and Adam Szeidl, "Treasure Hunt: Social Learning in the Field," Technical Report, National Bureau of Economic Research 2015. 4, 39

Molavi, Pooya, Alireza Tahbaz-Salehi, and Ali Jadbabaie, "A Theory of Non-Bayesian Social Learning," *Econometrica*, 2018, *86* (2), 445–490. 4, 31, 39

Paluck, Elizabeth Levy, Seth A Green, and Donald P Green, "The contact hypothesis re-evaluated," *Behavioural Public Policy*, 2019, *3* (2), 129–158. 5

Pariser, Eli, *The Filter Bubble: What the Internet is Hiding from You*, Penguin UK, 2011. 2

Patacchini, Eleonora and Yves Zenou, "Social Networks and Parental Behavior in the Intergenerational Transmission of Religion," *Quantitative Economics*, 2016, *7* (3), 969–995. 4

Pettigrew, Thomas F and Linda R Tropp, "A meta-analytic test of intergroup contact theory.," *Journal of Personality and Social Psychology*, 2006, *90* (5), 751. 5

Pitman, Edwin, "Significance Tests Which May Be Applied to Samples From any Populations: I," *Journal of the Royal Statistical Society, Series: B*, 1937, *4*, 119–130. 8

_ , "Significance Tests Which May Be Applied to Samples From any Populations: II," *Journal of the Royal Statistical Society, Series: B*, 1937, *4*, 225–232. 8

_ , "Significance Tests Which May Be Applied to Samples From any Populations: III," *Biometrika*, 1938, *29*, 322–335. 8

Rao, Gautam, "Familiarity Does Not Breed Contempt: Generosity, Discrimination and Diversity in Delhi Schools," *American Economic Review*, 2019, *109* (3), 774–809. 5

Rouban, Luc, "Sociologie Politique des Députés de la Ve République, 1958–2007," *Cahiers du CEVIPOF*, 2011, *55*, 1–99. 2, 6

_ , *La Fonction Publique en Débat* number 5396-97. In 'Les Études.', La Documentation française, 2014. 2, 6

_ , "L'Élite Urbaine: les Maires des Villes de Plus de 30 000 Habitants de 1983 à 2008," *Note No. 3, CEVIPOF, Paris*, 2014. 2, 6

Sacerdote, Bruce, "Peer Effects in Education: How Might They Work, How Big Are They and How Much Do We Know Thus Far?," in Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, eds., *Handbook of the Economics of Education*, Vol. 3, Amsterdam: North Holland, 2011, pp. 249 – 277. 5

_ , "Experimental and Quasi-Experimental Analysis of Peer Effects: Two Steps Forward?," *Annual Review of Economics*, 2014, *6* (1), 253–272. 5

Sunstein, Cass, *Republic.com 2.0*, Princeton, NJ: Princeton University Press, 2009. 2

_ , *# Republic: Divided Democracy in the Age of Social Media*, Princeton, NJ: Princeton University Press, 2018. 2

Tiberj, Vincent, "Sciences Po, Dix Ans après les Conventions Education Prioritaire," Working Paper, Sciences Po 2011. 8

Topa, Giorgio and Yves Zenou, "Neighborhood versus Network Effects," in Gilles Duranton, J. Vernon Henderson, and William C. Strange, eds., *Handbook of Regional and Urban Economics*, Vol. 5, Amsterdam: North Holland, 2015. 5

Wagenaar, Willem, "My Memory: A Study of Autobiographical Memory over Six Years," *Cognitive Psychology*, 1986, *18* (2), 225–252. 27

A Appendix: Description of data

Sample construction: The sample excludes observations (pairs of students) in which any of the above-mentioned variables is missing, when at least one of the two individuals in the couple did not answer to the related question in the survey. We also drop pairs that contain at least one individual in the top 5 percent of the distribution of time taken to name each friend (about 82 seconds per friend or 13.5 minutes for individuals with 10 friends).

Controls: The standard set of controls in dyadic specifications throughout the paper include the following variables: Initial (Pre-Sciences Po) Difference in Political Opinions (August 2013), Same Gender, Both Female, Same Nationality, Same Admission Type, Both Affirmative Action, Same Département of High School, Same Region of High School, Same High School Major, Difference in Tuition Fees, Both Free Tuition, Same Parents Profession, Same ZIP Code, Same Program.

Table A1: Description of Variables in Dyadic Data

Variable	Description
Friendship	1 if at least one of the two individual has named the other as one of her friends (the 'OR' network of undirected friendship), zero otherwise.
Same Integration Group (IG)	1 if the two individuals have attended the same Integration Group before starting the first school year at Sciences Po, 0 otherwise.
Difference in political opinion (March 2014)	Absolute difference in political opinions of the two individuals, as declared on a 1-10 scale in the main survey (March. 2014).
Difference in initial (pre-Sciences Po) political opinion (August 2013)	Absolute difference in political opinions of the two individuals from before entering Sciences Po (August 2013), as declared on a 1-10 scale in the main survey (March. 2014).
Difference in political opinion in 2015	Absolute difference in political opinions of the two individuals, as declared on a 1-10 scale in the 2015 survey.
Difference in political opinion in 2014 (Recalled)	Absolute difference in political opinions of the two individuals in 2014, as declared on a 1-10 scale in the 2015 survey.
Both members of some association	1 if the two individuals are members of some student association. Missing if at least one of them did not answer this question. 0 otherwise.
Both members of some association of type T	1 if the two individuals are members of some student association of type T (see classification of association types below). Missing if at least one of them did not answer this question. 0 otherwise.
Both members of different association of type T	1 if each of the two individuals is member of some student association of type T, without both being members of the same association of type T (see classification of association types below). Missing if at least one of them did not answer this question. 0 otherwise.
Both members of the same association	1 if the two individuals are members of the same student association. Missing if at least one of them did not answer this question. 0 otherwise.

Both members of the same association of type T	1 if the two individuals are members of the same student association of type T (see classification of association types below). Missing if at least one of them did not answer this question. 0 otherwise.
Association Types	Survey participants are members of 107 student associations at Sciences Po. We classify them into four types: • Political associations, including those directly affiliated to political parties, those that focus their actions and debates on political issues, and student unions (usually committed to political struggles), • Sports associations, • "Activism" associations with a clear activist agenda that is not politically controversial, such as human right issues or environmental protection, • "Identity" associations that gather individuals based on common personal characteristics, such as LGBT groups, religious groups, and associations based on geographical origins (such as province or country of origin).
Movement in Same Direction	1 if both individuals have changed their political opinion between August 2013 and March 2014 and their new political opinion have moved in the same direction relative to their initial one ($\Delta Y_i \Delta Y_j \geq 0$), 0 otherwise.
Strong Convergence	1 if the two individuals have different initial political positions, none of them have moved away from and at least one of them has moved towards the other initial political opinion relative to her own initial position ($\Delta Y_i(Y_{j0} - Y_{i0}) > 0$ & $\Delta Y_j(Y_{i0} - Y_{j0}) > 0$). Missing if the two individuals have the same initial political opinion. 0 otherwise.
Weak Convergence	1 if the two individuals have different initial political positions, none of them have moved away from the other initial political opinion relative to her own initial position. Missing if the two individuals have the same initial political opinion ($\Delta Y_i(Y_{j0} - Y_{i0}) \geq 0$ & $\Delta Y_j(Y_{i0} - Y_{j0}) \geq 0$). 0 otherwise.
Strong Divergence	1 if the two individuals have both moved away from each others initial political position relative to their own initial position ($\Delta Y_i(Y_{j0} - Y_{i0}) < 0$ & $\Delta Y_j(Y_{i0} - Y_{j0}) < 0$), 0 otherwise.
Weak Divergence	1 if the two individuals have not moved towards each others political position relative to their own initial position ($\Delta Y_i(Y_{j0} - Y_{i0}) \leq 0$ & $\Delta Y_j(Y_{i0} - Y_{j0}) \leq 0$), 0 otherwise.
Friendship Strength 1	1 if at least one of the two individual has named the other as one of her friends and has stated that their friendship is at least as intense as a "mere relationship", 0 otherwise.
Friendship Strength 2	1 if at least one of the two individual has named the other as one of her friends and has stated that their friendship is at least as intense as a "friendship link", 0 otherwise.
Friendship Strength 3	1 if at least one of the two individual has named the other as one of her friends and has stated that their friendship is at least as intense as a "close friendship", 0 otherwise.
Friendship Strength 4	1 if at least one of the two individual has named the other as one of her friends and has stated that their friendship is at least as intense as a "very close friendship", 0 otherwise.
Shortest Path	Shortest path between the two individuals in the 'OR' network of surveyed undirected friendship.
1st vs 2nd order only	Equal to the shortest path if it is either 1 or 2, missing otherwise.

2nd vs 3rd order only	Equal to the shortest path if it is either 2 or 3, missing otherwise.
3rd vs more order only	Equal to the shortest path if it is either 3 or more, missing otherwise.
Alphabetical distance between last names	The entire cohort's last names are ordered alphabetically, and placed on a circle (so that after last names starting with 'Z' we return to last names starting with 'A'). Any pair of last names on this circle are connected through two different arcs. Their alphabetical distance refers to the number of last names between them in the shorter arc, plus one. Put differently, denoting their ranks on the alphabetically ordered list of the cohort's last names as $r_1, r_2 \in [1, N]$, $r_1 < r_2$, the alphabetical distance is $\min(r_2 - r_1, N + r_1 - r_2)$, N being the total number of last names (exactly 800 for the cohort in consideration).
Difference in Differences in Political Opinion	Difference in Political Opinion in March 2014 minus Difference in Political Opinion from before entering Sciences Po.
Same Gender	1 if the two individuals are of the same gender, 0 otherwise.
Both Female	1 if the two individuals are both female, 0 otherwise.
Same Nationality	1 if the two individuals share a common nationality, 0 otherwise.
Same Admission Type	1 if the two individuals have been admitted through the same admission procedure, 0 otherwise. The three main procedures include the standard admission procedure (consideration of dossier, written tests, and oral tests), the international procedure (consideration of dossier and oral tests), and the priority admission (consideration of dossier and oral interview among students from schools in disadvantaged areas).
Both Affirmative Action	1 if the two individuals have both been admitted through the priority admission procedure, 0 otherwise. This is Sciences Po's affirmative action channel that targets high schools in disadvantaged areas of France (the ZEP, prioritized educational zones) under its Prioritized Education Convention (CEP). This admission procedure includes examination of dossier and of an oral interview, but not the standard written test.
Same Département of High School	1 if the two individuals have completed their high school diploma in the same French département, 0 otherwise. Metropolitan France is composed of 96 départments.
Same Region of High School	1 if the two individuals have completed their high school diploma in the same French region, 0 otherwise. Metropolitan France is composed of 22 regions.
Same High School Major	1 if the two individuals have a high school diploma with the same major classification, 0 otherwise. The categories include ES (Economic and Social), L (Literary/Language-Mathematics), S (Sciences), and Foreign Diplomas (grouped into one category).
Difference in Tuition Fees	Absolute difference in tuition fees among the couple (proxy for family income). At Sciences Po, the amount of tuition is a function of the parents' official income tax quotient, which is calculated based on total household income and household size.
Both Free Tuition	1 if both individuals do not pay tuition fees, 0 otherwise. Students pay no tuition when their parents' income tax quotient is below a threshold.
Same Parents' Profession	1 if at least one of an individual's parents has a common profession with at least one of the other individual's parents, 0 otherwise. The information on parents' profession is based on the French government's official socio-professional categories.
Same ZIP code	1 if the two individuals live in the same ZIP code area, 0 otherwise. The Greater Paris region of 'Ile de France' contains more than 528 areas with separate ZIP codes, mostly corresponding to arrondissements (districts) inside Paris and cantons outside Paris.

Table A2: Additional Descriptive Statistics of Covariates

Panel A: Monadic Independent Variables

Variable	(1) Full Sample			(2) Benchmark Sample		
	Mean	Standard deviation	Mean	Obs.	Standard deviation	Obs.
Gender (1= Female)	0.592	(0.492)	796	0.583	(0.494)	331
Honors Graduation	0.754	(0.431)	796	0.831	(0.375)	331
Tuition Fees	3602	(3495)	713	3826	(3328)	331

Panel B: Dyadic Independent Variables

Variable	(1) Full Sample			(2) Benchmark Sample		
	Mean	Standard deviation	Observations	Mean	Standard deviation	Observations
Same Gender	0.522	(0.500)	147153	0.511	(0.500)	52,326
Both Female	0.369	(0.483)	147153	0.336	(0.472)	52,326
Same Nationality	0.928	(0.259)	145530	0.969	(0.172)	52,326
Same Admission Type	0.565	(0.496)	147153	0.697	(0.459)	52,326
Both Affirmative Action	0.0291	(0.168)	147153	0.0127	(0.112)	52,326
Same Département of High School	0.0517	(0.221)	132870	0.0613	(0.240)	52,326
Same Region of High School	0.253	(0.435)	132355	0.250	(0.433)	52,326
Same High School Major	0.363	(0.481)	147153	0.382	(0.486)	52,326
Difference in Tuition Fees	3878.769	(3004.541)	122760	3743.716	(2810.757)	52,326
Both Free Tuition	0.476	(0.499)	147153	0.624	(0.484)	52,326
Same Parents' Profession	0.422	(0.494)	119316	0.445	(0.497)	52,326
Same ZIP code	0.0264	(0.160)	146611	0.0252	(0.157)	52,326
Same Program	0.520	(0.500)	147153	0.513	(0.500)	52,326

Notes: Statistics in (1) are computed on the full sample of data available for each variable, while statistics in (2) are computed on the benchmark sample, which is detailed in Table A1

Same Program	1 if the two individuals are enrolled in the same study program, 0 otherwise. In our sample, apart from the common undergraduate program that all students undertake, some students are enrolled in double-degree programs joint between Sciences Po and other, French or non-French educational institutions. In some cases they are subject to additional constraints in terms of course timing.

B Appendix: Analysis of compliers

In this appendix, we detail the technical derivations of formulae that describe the subsample of compliers in a LATE setting. Denote the three sets of compliers, never-takers, and always-takers respectively as $\mathcal{C}$, $\mathcal{N}$, and $\mathcal{A}$.

Appendix Table A3 illustrates the four cases corresponding to the values of friendship L (the treatment variable) and same integration-group IG (the instrument), in a standard LATE setting where both treatment and instrument are binary variables. Among the four cases, it is clear that $\{(i,j)|L=0, IG=1\} \subseteq \mathcal{N}$ (pairs who would not befriend even if assigned into the same IG must be never-takers), and $\{(i,j)|L=1, IG=0\} \subseteq \mathcal{A}$ (pairs who would befriend even without being in the same IG must be always-takers). The other two cases are composite, and include both compliers and never-takers/always-takers: $\{(i,j)|L=0, IG=0\} \subseteq \mathcal{N} \cup \mathcal{C}$ and $\{(i,j)|L=1, IG=1\} \subseteq \mathcal{A} \cup \mathcal{C}$ (there are no defiers, given the monotonicity assumption). For simplicity, we denote $Pr((i,j) \in \mathcal{N}|L, IG)$ as $Pr(\mathcal{N}|L, IG)$.

The unconditional shares of never-takers and always-takers can be simply calculated thanks to the exogeneity of the instrument IG as: $Pr(\mathcal{N}) = Pr(\mathcal{N}|IG=1) = \frac{Pr(L=0,IG=1)}{Pr(IG=1)}$, and similarly $Pr(\mathcal{A}) = Pr(\mathcal{A}|IG=0) = \frac{Pr(L=1,IG=0)}{Pr(IG=0)}$. We thus deduce the share of compliers as $Pr(\mathcal{C}) = 1 - \frac{Pr(L=0,IG=1)}{Pr(IG=1)} - \frac{Pr(L=1,IG=0)}{Pr(IG=0)}$.

Remark 1 $Pr(\mathcal{N}) = \frac{Pr(L=0,IG=1)}{Pr(IG=1)}$, $Pr(\mathcal{A}) = \frac{Pr(L=1,IG=0)}{Pr(IG=0)}$, $Pr(\mathcal{C}) = 1 - \frac{Pr(L=0,IG=1)}{Pr(IG=1)} - \frac{Pr(L=1,IG=0)}{Pr(IG=0)}$.

Also using the exogeneity of the instrument IG, we can estimate the conditional share of never-takers in the case $\{(i,j)|L=$

$0, IG = 0\}$ by observing that $Pr(\mathcal{N}|IG = 0) = Pr(\mathcal{N}|IG = 1)$, therefore:

$$Pr(\mathcal{N}|L = 0, IG = 0) = \frac{Pr(\mathcal{N}|IG = 0)}{Pr(L = 0|IG = 0)} = \frac{Pr(\mathcal{N}|IG = 1)}{Pr(L = 0|IG = 0)} = \frac{Pr(L = 0|IG = 1)}{Pr(L = 0|IG = 0)},$$

which is readily available from the data. Similarly, $Pr(\mathcal{A}|L = 1, IG = 1) = \frac{Pr(L=1|IG=0)}{Pr(L=1|IG=1)}$. The conditional share of compliers in the two cases $\{(i, j)|L = 0, IG = 0\}$ and $\{(i, j)|L = 1, IG = 1\}$ are thus $1 - \frac{Pr(L=0|IG=1)}{Pr(L=0|IG=0)}$ and $1 - \frac{Pr(L=1|IG=0)}{Pr(L=1|IG=1)}$, respectively.

Remark 2 $Pr(\mathcal{C}|L = 0, IG = 0) = 1 - \frac{Pr(L=0|IG=1)}{Pr(L=0|IG=0)}$, $Pr(\mathcal{C}|L = 1, IG = 1) = 1 - \frac{Pr(L=1|IG=0)}{Pr(L=1|IG=1)}$.

An analogous argument applies to any statistics defined on the population, such as the mean of a random variable S, $\mathbb{E}[S]$. The exogeneity and monotonicity assumptions imply that $\mathbb{E}[S|\mathcal{N}] = \mathbb{E}[S|\mathcal{N}, IG = 1] = \mathbb{E}[S|L = 0, IG = 1]$, and similarly $\mathbb{E}[S|\mathcal{A}] = \mathbb{E}[S|\mathcal{A}, IG = 0] = \mathbb{E}[S|L = 1, IG = 0]$, both quantities readily estimable as subsample means in the data. It follows further from

$$\mathbb{E}[S] = Pr(\mathcal{N})\mathbb{E}[S|\mathcal{N}] + Pr(\mathcal{A})\mathbb{E}[S|\mathcal{A}] + Pr(\mathcal{C})\mathbb{E}[S|\mathcal{C}],$$

that the unconditional mean of S among compliers is:

$$\mathbb{E}[S|\mathcal{C}] = \frac{\mathbb{E}[S] - Pr(\mathcal{N})\mathbb{E}[S|\mathcal{N}] - Pr(\mathcal{A})\mathbb{E}[S|\mathcal{A}]}{1 - Pr(\mathcal{N}) - Pr(\mathcal{A})}. \tag{1}$$

Regarding conditional means, observe that $\mathbb{E}[S|IG = 1, \mathcal{N}] = \mathbb{E}[S|IG = 0, \mathcal{N}]$, therefore $\mathbb{E}[S|L = 0, IG = 0, \mathcal{N}] = \mathbb{E}[S|L = 0, IG = 1, \mathcal{N}] = \mathbb{E}[S|L = 0, IG = 1]$. It follows from

$$\begin{aligned}\mathbb{E}[S|L = 0, IG = 0] = Pr(\mathcal{N}|L = 0, IG = 0)\mathbb{E}[S|L = 0, IG = 0, \mathcal{N}] +\\ +Pr(\mathcal{C}|L = 0, IG = 0)\mathbb{E}[S|L = 0, IG = 0, \mathcal{C}],\end{aligned}$$

$$\begin{aligned}\Rightarrow \mathbb{E}[S|L = 0, IG = 0, \mathcal{C}] &= \frac{\mathbb{E}[S|L = 0, IG = 0] - Pr(\mathcal{N}|L = 0, IG = 0)\mathbb{E}[S|L = 0, IG = 0, \mathcal{N}]}{Pr(\mathcal{C}|L = 0, IG = 0)} = \\ &= \frac{Pr(L = 0|IG = 0)\mathbb{E}[S|L = 0, IG = 0] - Pr(L = 0|IG = 1)\mathbb{E}[S|L = 0, IG = 1]}{Pr(L = 0|IG = 0) - Pr(L = 0|IG = 1)}. \qquad (2)\end{aligned}$$

We thus obtain an estimate of the mean of S among non-treated compliers $\mathbb{E}[S|L = 0, IG = 0, \mathcal{C}]$ as a "weighted average" of its averages over two cases $\{(i, j)|L = 0, IG = 0\}$ and $\{(i, j)|L = 0, IG = 1\}$ (but with some negative weights). A similar estimate of the mean of S among treated compliers $\mathbb{E}[S|L = 1, IG = 1, \mathcal{C}]$ is obtained analogously by:

$$\frac{Pr(L = 1|IG = 1)\mathbb{E}[S|L = 1, IG = 1] - Pr(L = 1|IG = 0)\mathbb{E}[S|L = 1, IG = 0]}{Pr(L = 1|IG = 1) - Pr(L = 1|IG = 0)}. \tag{3}$$

Notably, the difference between the formulae in equations (2) and (3) yields the LATE estimand on the variable S, namely $\frac{\mathbb{E}[S|IG=1]-\mathbb{E}[S|IG=0]}{Pr(L=1|IG=1)-Pr(L=1|IG=0)}$.

Remark 3 *Unconditional and conditional means* $\mathbb{E}[S|\mathcal{C}]$, $\mathbb{E}[S|L = 0, IG = 0, \mathcal{C}]$, *and* $\mathbb{E}[S|L = 1, IG = 1, \mathcal{C}]$ *are given in equations (1), (2), and (3).*

The statistics shown in Table A3 are computed based on remarks 1, 2, 3. Conditional on each case of (L, IG) and $\mathcal{C}$, $\mathcal{N}$, $\mathcal{A}$, the table reports the case's sample size and proportion, and the mean of the outcome of interest, $\widetilde{DY}$. It is the residual of regressing the outcome DY, namely the pairwise absolute differences in opinions after 6 months, on the set of controls (it

Table A3: Analysis of compliers, never-takers, and always-takers

			Instrument: Same Integration Group	
			IG = 0	IG = 1
Treatment: Friendship	L=0	Composition	Compliers, Never-takers	Never-takers
		Nº of observations	8,522 Cs, 42,063 Ns	807 Ns
		Share of full sample	16.3% Cs, 80.4% Ns	1.5% Ns
		$\mathbb{E}[\widetilde{DY}]$	Cs: 0.352, Ns: -0.0678	Ns: -0.0678
		Pr[Initial Opinion Gap < 2]	Cs: 0.626, Ns: 0.357	Ns: 0.357
	L=1	Composition	Always-takers	Compliers, Always-takers
		Nº of observations	756 As	163 Cs, 15 As
		Share of full sample	1.4% As	0.3% Cs, 0.03% As
		$\mathbb{E}[\widetilde{DY}]$	As: -0.078	Cs: -0.212, As: -0.078
		Pr[Initial Opinion Gap < 2]	As: 0.451	Cs: 0.431, As: 0.451

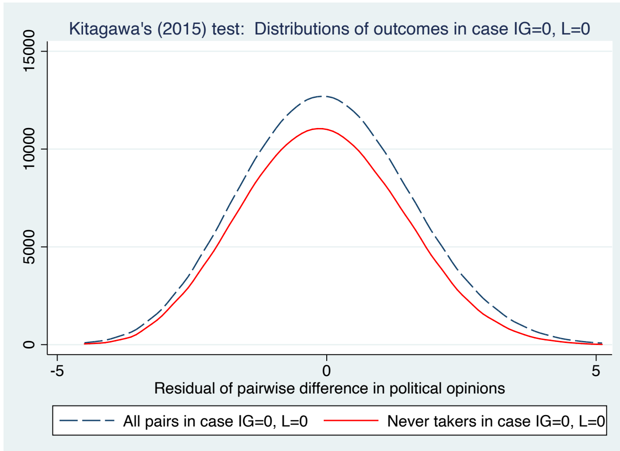
Notes: Statistics from the analysis of compliers, including subsample sizes and statistics among compliers (Cs), never-takers (Ns), and always-takers (As) in the four cases based on the values of the treatment L_{ij} and the instrument IG_{ij}. $\widetilde{DY}$ stands for the residual of the regression of pairwise absolute differences in political opinions DY on covariates – unlike DY, $\widetilde{DY}$ can take negative values.

thus can take negative values). Using equations (2) and (3), we obtain its means among treated and untreated compliers, the difference between which yields the LATE estimate. Similarly, we can use those equations to estimate its quantiles, hence its distributions, among those two groups of compliers, which are illustrated in Figure A3.

Kitagawa (2015) proposes a test of the validity of LATE assumptions based on the testable implication that in the cases of $(L = 0, IG = 0)$ and $(L = 1, IG = 1)$, the distribution of compliers is dominated by the distribution of all individuals (i.e., the remaining distributions of never-takers in $(L = 0, IG = 0)$, and of always-takers in $(L = 1, IG = 1)$, must be nonnegative). In Figures A1 and A2, we show graphically that it is clearly the case in our data. Therefore, we cannot reject the LATE assumptions in Kitagawa's (2015) test.

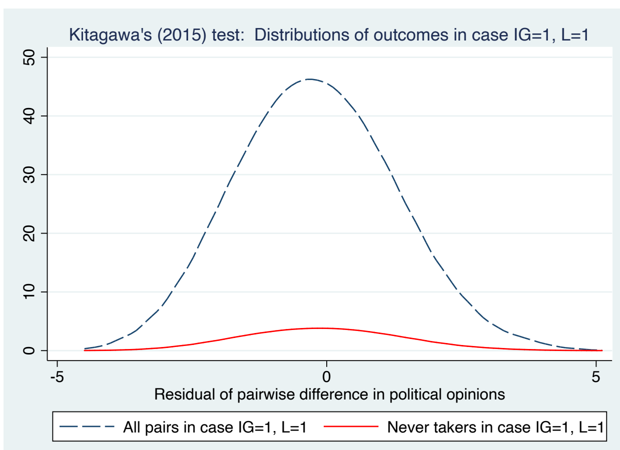
The analysis of compliers is further useful in exploring the difference between the LATE estimator and others, such as the OLS estimator. Table A3 also highlights that among compliers there is a much higher proportion of pairs with similar initial political opinions (with opinion gaps less than the median of 2): 62.6% among untreated compliers versus 35.7% among never-takers and 45.1% among always-takers. It suggests that the strong LATE effect may be concentrated on those similar pairs, a hypothesis that we further explore at length in the paper.

Figure A1: Distributions of residual outcomes in case $IG = 0$ and $L = 0$



Notes: The two curves are kernel estimates of the distributions of outcomes (the residual of the regression of pairwise absolute differences in political opinions on covariates), drawn for all observations in case ($L = 0, IG = 0$), and for never-takers among them (summarized in Table A3). Kitagawa (2015) proposes a test that rejects the validity of the LATE assumptions if those two distributions intersect.

Figure A2: Distributions of residual outcomes in case $IG = 1$ and $L = 1$



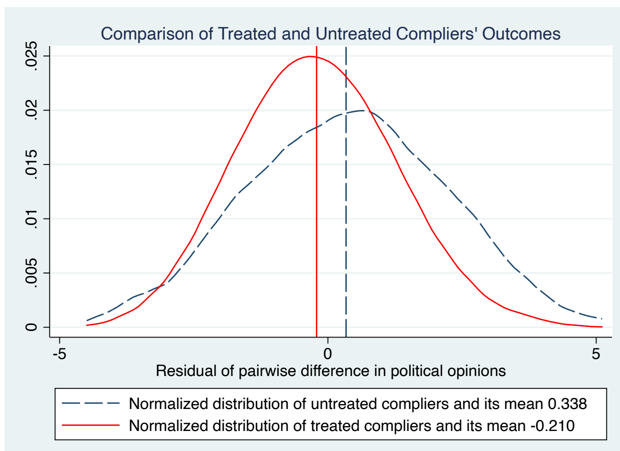
Notes: The two curves are kernel estimates of the distributions of outcomes (the residual of the regression of pairwise absolute differences in political opinions on covariates), drawn for all observations in case ($L = 1, IG = 1$), and for always-takers among them (summarized in Table A3). Kitagawa (2015) proposes a test that rejects the validity of the LATE assumptions if those two distributions intersect.

Table A4: Permutation tests of randomness of last name's first letter

Variable	Within-Group Statistics	Actual value	p-value
Initial Political Opinion (August 2013)	Within-/Between- Standard Deviation Ratio	1.806	0.410
Tuition Fees	Within-/Between- Standard Deviation Ratio	1.576	0.147
Gender	Within-/Between- Standard Deviation Ratio	1.891	0.670
Affirmative-Action Admission	Within-/Between- Standard Deviation Ratio	1.434	0.253
Second Nationality	Within-/Between- Standard Deviation Ratio per Category	2.299	0.667
Admission Type	Within-/Between- Standard Deviation Ratio per Category	4.335	0.197
Program	Within-/Between- Standard Deviation Ratio per Category	3.081	0.670
Parents' Profession	Within-/Between- Standard Deviation Ratio per Category	4.431	0.543
High School Major	Within-/Between- Standard Deviation Ratio per Category	2.797	0.110
Département of High School	Within-/Between- Standard Deviation Ratio per Category	5.872	0.810
Region of High School	Within-/Between- Standard Deviation Ratio per Category	4.622	0.810

Notes: Permutation tests over the full sample are performed over 300 Monte Carlo draws. For continuous and binary variables, the test is performed on the distribution of the ratio of within-group and between-group standard deviations. For category variables, the test is performed on the distribution of the average of this ratio across all binary (dummy) variables representing each category. p-values are computed with respect to the left tail (rejection of low within-group variation with respect to between-group variation).

Figure A3: Comparison of treated and untreated compliers' outcomes



Notes: The two curves are kernel estimates of the distributions of outcomes (the residual of the regression of pairwise absolute differences in political opinions on covariates), normalized to total weight equal 1, drawn separately for untreated and treated compliers. The vertical lines represent their respective means (0.338 among untreated compliers, -0.210 among treated compliers).

C Appendix: Additional tables

Table A5: Descriptive Statistics on Recall Bias

		Actual (Individual) Political Opinion in 2014										
		1	2	3	4	5	6	7	8	9	10	Total
Recalled Political Opinion in 2014	1	0	1	0	0	0	0	0	0	0	0	1
	2	0	5	1	2	0	0	0	0	0	0	8
	3	1	6	19	7	3	1	0	0	0	0	37
	4	0	0	7	16	21	4	1	0	0	0	49
	5	0	0	2	7	25	6	1	0	0	0	41
	6	1	0	0	1	6	21	8	3	0	0	40
	7	0	0	0	1	0	6	12	5	0	0	24
	8	0	0	0	0	0	1	6	6	1	0	14
	9	0	0	0	0	0	0	2	1	0	1	4
	10	0	0	0	0	0	0	0	0	0	0	0
	Total	2	12	29	34	55	39	30	15	1	1	218

Notes: The joint empirical distribution of actual (horizontal axes) and recalled (vertical axes) individual political opinion in 2014, based on the main survey in 2014 and the additional survey in 2015. Individuals with a missing observation in either year are excluded.

Table A6: Recall Bias Regression on Individual Data

Dependent Variable:	Absolute Recall Bias	Recall Bias
	(1)	(2)
Actual Political Opinion in 2015	0.00426	-
	(0.116)	
Actual Political Opinion in 2014	0.00609	-
	(0.137)	
Diff. in Actual Political Opinion Between 2015 and 2014	-	0.574***
		(0.0437)
Observations	216	216
Double Group Clust.	Yes	Yes

Notes: OLS predictions of recall bias based on actual opinions, on the individual linked 2014-2015 sample, including individuals present in both surveys for which the variables "political opinion in 2015", "actual political opinion in 2014" and "recalled political opinion in 2014" are not missing. The outcome variable "Recall Bias" is calculated as recalled political opinion of 2014, as answered in the 2015 survey, minus actual political opinion in 2014, as answered in the 2014 survey. "Absolute Recall Bias" is the absolute value of Recall Bias. Standard errors are clustered at the group level.

Table A7: Initial Political Opinion Gaps and Convergence

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Weak Convergence in Political Opinion					
Sample of Pairs:	Full Sample		Initial Political Opinion Gap			
			< 2		≥ 2	
Specification:	OLS	IV	OLS	IV	OLS	IV
Same IG	0.00529		0.00273		0.00973	
	(0.0226)		(0.0224)		(0.0162)	
Same IG × Initial Gap	0.000698					
	(0.00743)					
Friendship		0.0273		0.0145		0.0655
		(0.136)		(0.120)		(0.109)
Friendship × Initial Gap		0.00691				
		(0.0491)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes
WeakIV test stat.		13.44		43.28		52.83
Observations	44,904	44,904	13,632	13,632	31,272	31,272
R-squared	0.103	0.103	0.008	0.008	0.049	0.049

Notes: Dyadic specifications relating the incidence of weak convergence to the same IG indicator (columns (1), (3), (5)) and friendship link instrumented by the same-IG indicator (columns (2), (4), (6)). The dependent variable is the indicator whether the pair's opinions (weakly) move towards each other's, and not beyond the each other's initial opinions. Columns (3)-(4) restrict the sample to pairs with pre-Sciences Po difference in political opinions less than 2 (the median). Columns (5)-(6) restrict the sample to pairs with pre-Sciences Po difference in political opinions at least 2. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

Table A8: Initial Political Opinion Gaps and Co-movement

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Non-diverging Co-Movement in Political Opinion					
Sample of Pairs:	Full Sample		Initial Political Opinion Gap			
			< 2		≥ 2	
Specification:	OLS	IV	OLS	IV	OLS	IV
Same IG	0.0291*		0.0156		0.0235	
	(0.0171)		(0.0139)		(0.0146)	
Same IG × Initial Gap	-0.00354					
	(0.00515)					
Friendship		0.156		0.0810		0.158
		(0.0995)		(0.0712)		(0.103)
Friendship × Initial Gap		-0.0136				
		(0.0327)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		18.76		60.14		52.83
Observations	52,326	52,326	21,054	21,054	31,272	31,272
R-squared	0.007	0.004	0.006	0.005	0.013	0.009

Notes: Dyadic specifications relating the incidence of non-diverging co-movement to the same IG indicator (columns (1), (3), (5)) and friendship link instrumented by the same-IG indicator (columns (2), (4), (6)). The dependent variable is the indicator whether the pair's opinions (weakly) move in the same direction, and their gap does not increase. Columns (3)-(4) restrict the sample to pairs with pre-Sciences Po difference in political opinions less than 2 (the median). Columns (5)-(6) restrict the sample to pairs with pre-Sciences Po difference in political opinions at least 2. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

Table A9: Friendship Effect on Extremism

	(1)	(2)	(3)	(4)
Dependent Variable:	Extreme Political Opinion			
Share of Moderates in IG	-0.354**	-0.396**		
	(0.141)	(0.150)		
Share of Moderates × Being Extreme before ScPo	-0.427	-0.297		
	(1.111)	(1.154)		
Share of Close Moderates			-0.247*	-0.247*
			(0.126)	(0.144)
Share of Close Moderates × Being Extreme before ScPo			-0.595	-0.501
			(1.423)	(1.382)
Controls	No	Yes	No	Yes
Group Clustering	Yes	Yes	Yes	Yes
Observations	323	323	323	323
R-squared	0.235	0.250	0.230	0.244

Notes: Monadic specifications relating an individual's extremist political opinion indicator to the share of moderates in his or her IG. Extremist opinions are those in the set $\{1, 2, 9, 10\}$, and moderate ones are those in the complement set $\{3, \ldots, 8\}$. Close moderates are moderates whose gap with the individual's initial political opinion is below 2. Control variables include the within-IG share of each gender, nationality, affirmative action students, program, high school major (i.e., all category variables that are coarse enough to produce sensible, non-collinear per-group share variable), and the within-IG average of tuition fees. Standard errors are clustered by individual's IG. See Appendix A and Appendix Table A1 for variable and sample definitions.

Table A10: Effects on Association Activities among Politically Dissimilar Pairs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Both are members of the same association				Both are members of			
	Of any type		In Politics		Some association in Politics		Different associations in Politics	
Specification:	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Sample of Pairs:	Initial Political Opinion Gap ≥ 2							
Same IG	0.0219		0.00692		-0.0143		-0.0213	
	(0.0177)		(0.0131)		(0.00971)			
Friendship		0.146		0.0461		-0.0952		-0.142
		(0.108)		(0.0861)		(0.0689)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		27.25		27.25		27.25		27.25
Observations	14,043	14,043	14,043	14,043	14,043	14,043	14,043	14,043
R-squared	0.008	0.016	0.008	0.011	0.040	0.038	0.034	0.033
Mean Dependent Variable	0.0882	0.0882	0.0163	0.0163	0.174	0.174	0.158	0.158
St. Dev. Dependent Variable	(0.283)	(0.283)	(0.127)	(0.127)	(0.379)	(0.379)	(0.364)	(0.364)

Notes: Dyadic specifications relating association membership to the same IG indicator (columns (1), (3), (5), (7)) and to friendship link instrumented by the same-IG indicator (columns (2), (4), (6), (8)). The sample only includes pairs of students with above-average (at least 2) pre-Sciences Po political opinion gap. The dependent variable is an indicator whether each pair are members in the same association (columns (1)-(2)), whether they are members in the same association in politics (columns (3)-(4)), whether both are members of some (not necessarily the same) association in politics (columns (5)-(6)), and whether they are both members of some association in politics, but not members of the same one (columns (7)-(8)). Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the set of controls (including initial political opinion gap).

Table A11: Friendship Effect by Friendship Intensity

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Difference in Political Opinion (March 2014)				
Specification:	IV (by Same Integration Group Indicator)				
Friendship Intensity	-0.247***				
	(0.0850)				
Very Close Friendship		-3.275***			
		(1.248)			
Close Friendship or Stronger			-1.397***		
			(0.490)		
Friendship Link or Stronger				-0.757***	
				(0.258)	
Mere Acquaintance or Stronger					-0.589***
					(0.207)
Initial Political Opinion Gap (August 2013)	0.528***	0.528***	0.528***	0.528***	0.528***
	(0.0317)	(0.0318)	(0.0317)	(0.0317)	(0.0317)
Observations	52,326	52,326	52,326	52,326	52,326
R-squared	0.343	0.327	0.340	0.343	0.344
Controls	Yes	Yes	Yes	Yes	Yes
Double Group Clustering	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic	56.89	15.18	26.87	59.18	73.08

Notes: Dyadic specifications relating difference in political opinions to different measures of friendship intensity, instrumented by the same IG indicator. Column (1) considers the measure that assigns zero to pairs of non-friends, and the surveyed level of friendship intensity (from 1 to 4) to pairs of friends. Columns (2) to (5) consider friendship measures corresponding to intensity cutoffs of 4 to 1 respectively, each taking value zero for friendship intensity below the threshold, and one for friendship intensity equal or above the threshold. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the standard set of controls.

Table A12: Friendship Effect after Excluding Each Nationality

Dependent Variable:	Difference in Political Opinion						
Excluding:	Algeria	Germany	Armenia	Belgium	Cameroon	Spain	Guinea
Friendship	-0.456**	-0.589***	-0.568***	-0.565***	-0.568***	-0.542***	-0.568***
	(0.215)	(0.207)	(0.202)	(0.204)	(0.202)	(0.199)	(0.202)
Observations	51,040	51,040	52,326	51,681	52,326	52,003	52,326
R-squared	0.355	0.346	0.344	0.347	0.344	0.348	0.344
Weak IV test statistic	79.68	76.71	77.75	77.06	77.75	78.13	77.75
Excluding:	Italy	Kenya	Madagascar	Morocco	Senegal	Yougoslavia	
Friendship	-0.571***	-0.568***	-0.577***	-0.594***	-0.568***	-0.568***	
	(0.203)	(0.202)	(0.203)	(0.205)	(0.202)	(0.202)	
Observations	51,360	52,326	52,003	51,360	52,326	52,326	
R-squared	0.343	0.344	0.347	0.349	0.344	0.344	
Weak IV test statistic	79.09	77.75	76.70	75.81	77.75	77.75	

Notes: Dyadic specifications relating difference in political opinions to friendship link, instrumented by the same-IG indicator. Each column excludes all individuals of a nationality present in the sample. Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the standard set of controls.

Table A13: Friendship Effect after Excluding Names Starting with a Given Alphabet Letter

Dependent Variable:	Political Opinion Gap								
Excluded First Letter	A	B	C	D	E	F	G	H	I
Friendship	-0.616***	-0.460*	-0.413*	-0.892***	-0.599***	-0.583***	-0.512**	-0.542***	-0.629***
	(0.210)	(0.277)	(0.234)	(0.182)	(0.198)	(0.188)	(0.217)	(0.207)	(0.209)
Observations	48,205	38,781	43,956	43,071	51,360	48,516	46,360	47,895	51,040
R-squared	0.346	0.335	0.335	0.354	0.344	0.332	0.326	0.333	0.343
Weak IV statistic	75.21	58.95	66.05	85.12	69.46	76.63	79.20	72.21	79.14

Excluded First Letter	J	K	L	M	N	O	P	Q	R
Friendship	-0.575***	-0.529**	-0.374*	-0.451**	-0.561***	-0.655***	-0.629***	-0.587***	-0.601***
	(0.206)	(0.213)	(0.195)	(0.197)	(0.196)	(0.204)	(0.203)	(0.199)	(0.218)
Observations	49,455	51,360	41,328	44,253	51,681	51,040	47,895	52,003	46,971
R-squared	0.344	0.355	0.338	0.361	0.353	0.358	0.343	0.346	0.354
Weak IV statistic	79.48	76.01	67.25	71.28	76.22	75.19	74.51	78	71.33

Excluded First Letter	S	T	U	V	W	X	Y	Z	De/Du/D'
Friendship	-0.580***	-0.553***	-0.576***	-0.502**	-0.565***	-0.568***	-0.575***	-0.560***	-0.689***
	(0.210)	(0.208)	(0.203)	(0.201)	(0.204)	(0.202)	(0.206)	(0.202)	(0.193)
Observations	46,360	48,205	52,003	49,770	52,003	52,326	51,681	52,003	49,455
R-squared	0.344	0.343	0.345	0.342	0.342	0.344	0.345	0.344	0.335
Weak IV statistic	75.47	81.47	76.89	74	75.33	77.75	75.82	77.68	88.36

Notes: Dyadic specifications relating difference in political opinions to friendship link, instrumented by the same-IG indicator. Each column excludes all individuals whose family name starts with the corresponding letter, or with "De", "Du", or "D' ". Standard errors are two-way clustered by individual 1's group and by individual 2's group. Weak IV statistic reports the Kleibergen-Paap cluster-robust statistic, distributed as a Chi-squared under the null hypothesis of weak identification. See Appendix A and Appendix Table A1 for variable and sample definitions, and the standard set of controls.