Changing Minds: How Academic Fields Shape Political Attitudes

Yoav Goldstein, Matan Kolerman Shemer*

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Abstract

This paper studies how academic fields shape students' political attitudes. Using panel surveys of roughly 300,000 U.S. college students who enrolled between 1990 and 2015, we compare students across majors, conditioning on pre-college political attitudes and major-related preferences. The estimated effects are substantial: relative to the natural sciences, social sciences and humanities shift students left, while economics and business shift them right, with the largest effects on economic policy positions. Several findings support a causal interpretation. The estimates are robust to conservative sensitivity analyses, and the controls absorb nearly all the bias attributable to observed demographic and behavioral characteristics. Two complementary analyses exploiting supply-side variation in field availability yield consistent results. The effects also extend to related behaviors and priorities: social sciences and humanities increase political activism, whereas economics and business increase the importance students place on financial success. The evidence suggests that academic content, rather than peers or expected earnings, is the mechanism. Finally, field choice contributes to political fragmentation because students select majors that align with their preexisting attitudes.

*Yoav: The Eitan Berglas School of Economics, Tel Aviv University, yoavg2@mail.tau.ac.il. Matan: Department of Economics, MIT, kolerman@mit.edu. We are indebted to our advisors, Analia Schlosser and Moses Shayo. We also thank Michael Amior, Gal Bitton, Zach Bleemer, Josh Goodman, Jacob Light, Ro'ee Levy, Lauren Russell, and Vincent Pons, as well as participants at various conferences and seminars, for their valuable comments and suggestions. We are grateful to the HERI staff, particularly Ellen Stolzenberg, for providing access to the data used in this study. Yoav acknowledges the generous support of the Azrieli Graduate Fellowship from the Azrieli Foundation and a research grant from the Center for the Study of the U.S. at Tel Aviv University.

"Socrates does nothing that is just; he is a curious person, who searches into things under the earth and in heaven, and he makes the worse appear the better cause; and he teaches the aforesaid doctrines to others."
— Plato, Apology

1 Introduction

There is a growing political divide between college graduates and non-graduates in the U.S. and other advanced democracies (Gethin et al., 2022; Grossmann and Hopkins, 2024). Yet "college graduates" are not a homogeneous group. A defining characteristic of college education is its intellectual diversity: students sort into distinct academic fields that offer different ideas, methodological approaches, and normative frameworks. These differences raise a central question for understanding the politics of the college-educated: do academic fields shape students' attitudes? If so, through which mechanisms, and with what implications?

We study this question using rich panel survey data from the Higher Education Research Institute (HERI). The analysis covers roughly 300,000 students who enrolled in 450 American colleges between 1990 and 2015 and were surveyed both before and after college. The surveys measure students' political ideology, policy positions, goals, behaviors, and other outcomes.

Our first empirical strategy compares students' graduation ideology across college majors, conditional on a rich set of pre-college covariates. These covariates include strong predictors of graduation ideology, such as baseline ideology and policy positions, as well as strong predictors of field choice, such as intended majors and life goals. We show that, given the richness and relevance of these controls, these estimates are plausibly causal in our setting.

The estimates imply that college majors substantially affect students' political ideology. Relative to the natural sciences, studying the social sciences or humanities shifts students about 0.09 standard deviations (SD) to the left by graduation, while studying economics or business shifts them about 0.14 SD to the right.[1] Professional majors—engineering, health, and education—produce smaller rightward shifts relative to the natural sciences, about 0.06 SD. These effects are similar across student and institutional subgroups. Taken together, the estimates imply that studying the social sciences or humanities rather than economics or business shifts ideology leftward by about 0.23 SD. This is a sizable effect: for comparison, the pre-college gender (New York–Texas) gap in ideology is 0.14 SD (0.32 SD).

We next assess threats to a causal interpretation of these estimates. Sensitivity analyses following Oster (2019) and Cinelli and Hazlett (2020) suggest that selection on remaining unobservables is unlikely to overturn our results. Even under conservative benchmarking

[1]Throughout the paper, we use "liberal"/"left" or "conservative"/"right" to refer to self-identification and correlated policy positions.

scenarios, which allow all possible residual variation in graduation ideology to be as strongly correlated with major choice as observed pre-college ideology, the Oster-style lower bound for the effect of studying social sciences or humanities relative to business or economics is about 0.19 SD. This remains economically meaningful and close to our baseline estimate of 0.23 SD. Consistent with this interpretation, we show that our controls absorb almost all of the bias implied by observed demographic and behavioral variables.

We also provide complementary evidence from supply-side variation. This analysis includes two distinct approaches. The first approach uses program-specific persistence rates as an instrument for realized major choice. Specifically, we focus on students who initially intended to major in business or economics and instrument actual majoring in these fields with the leave-one-out persistence rate in the intended program. The instrument is relevant: a higher persistence rate increases the probability of ultimately majoring in business or economics by about 10 percentage points (pp). The corresponding counterfactual majors are mostly in the social sciences and humanities. The instrument is not correlated with baseline political ideology, supporting the independence assumption. Instrumental variable (IV) estimates imply that majoring in business or economics shifts ideology to the right by 0.34 SD.

The second approach exploits within-college expansions in the availability of business and economics over time. These expansions increase students' exposure to those fields. Under a parallel-trends assumption, we estimate their effects on student outcomes using a difference-in-differences approach. We provide two pieces of supportive evidence for this assumption. First, anecdotal evidence suggests that expansions are often triggered by external events, such as donations. Second, expansion timing is not correlated with students' baseline ideology. We find that these expansions increase enrollment in business and economics by about 4 pp, primarily by drawing students away from other social sciences. They also shift graduation ideology to the right by roughly 0.04 SD.[2]

We also document that academic fields affect policy positions, with especially pronounced effects for economic policy. Relative to natural sciences, majoring in business or economics substantially reduces support for redistribution-oriented policies, such as taxation and healthcare, by 0.17–0.18 SD. In contrast, social sciences and humanities increase support for these policies by 0.04–0.07 SD. For views on LGBTQ rights and abortion legislation, the effects are smaller, and students in all fields move in a more liberal direction during college.

Effects also extend to behaviors and life goals. Humanities and social sciences increase the importance students place on social values and make them more likely to participate in demonstrations. In contrast, economics and business lead students to place much greater

[2]These magnitudes are not directly comparable to our main estimates, since the expansions affect students' exposure to these fields not only through major choice but also through coursework outside their major.

emphasis on financial success and less emphasis on social values. These patterns suggest a potential channel for the economic returns to these majors through changes in life goals, consistent with evidence that studying economics increases both actual employment in, and preferences for, higher-paying industries (Bleemer and Mehta, 2022).

Then, we explore mechanisms. We first provide evidence that differences in peer ideology compositions and earnings expectations are unlikely to drive the effects. Although students' graduation ideology is strongly correlated with that of their peers, we show that within-programs, over-time changes in peer composition do not affect individual outcomes. Our estimates rule out peer composition accounting for more than about 7 percent of the total impact of majors on ideology. This does not imply that peer effects are absent universally. Rather, changes in the average peer composition within a program do not have sizable effects in our setting. Prior studies find peer effects in contexts with more intensive exposure (Strother et al., 2021; Coriale et al., 2023; Firoozi, 2023; Algan et al., 2025).

Additionally, exploiting variation in expected earnings across colleges within the same major, as well as over time in response to macro shocks that differentially affect fields, we find no heterogeneity in the effects.

We therefore argue that the most likely mechanism is the defining feature of field specialization: exposure to different intellectual environments. Because we do not directly observe course content, we use department ideological leaning as a proxy for how the same major is taught across colleges. Specifically, we exploit variation in the ideological orientation of departments teaching the same major, measured using HERI faculty surveys. Students in more left-leaning departments are not more left-leaning at baseline. Yet they graduate with significantly more left-leaning ideology, suggesting meaningful heterogeneity in field effects across departments. Among humanities and social-science majors, a one-standard-deviation increase in departmental liberalism is associated with additional leftward shifts in student ideology of about 0.14 and 0.13 SD, respectively.

Finally, we examine the broader consequences of our findings. First, we translate the estimated effects into expected voting behavior using external mappings from ideology to vote choice based on students in other surveys. The results imply that studying humanities or social sciences makes students about six pp more likely to vote Democratic than studying economics or business. For context, choosing these fields rather than business or economics is expected to have an effect comparable to growing up in a county with a 20-percentage-point higher Democratic vote share during adolescence (Brown et al., 2023).

Second, we show that the 'college divide' partly reflects the subjects students study. If students were to shift en masse into economics or business from any other majors, the

ideological shift during college would nearly vanish, and the gap between college graduates and non-graduates would shrink by about 30 percent.

Third, self-selection into majors amplifies ideological polarization among college graduates. Students with more left-leaning ideology tend to choose more left-leaning fields, causing ideological dispersion to rise during college. We show that under a uniform-major counterfactual, the increase in ideological variance during college would be reduced by about 46 percent, and the overall variance would be reduced by 4 percent. Likewise, women's greater tendency to major in left-leaning fields accounts for about 79 percent of the increase in the ideological gender gap during college and about 16 percent of the gap at graduation.

Together, the results show that the intellectual environments in which American college students study have meaningful effects on their political attitudes and behaviors. These findings bring new empirical evidence to a heated public debate and advance several strands of academic research. First, the results contribute to the literature on the growing political divide between those with and without college degrees in the U.S. and other advanced democracies (Gethin et al., 2022; Grossmann and Hopkins, 2024). There is evidence from various countries that students become more liberal during their studies, particularly on cultural issues (Broćić and Miles, 2021; Fryer, 2022; Scott, 2022; Apfeld et al., 2023). A recent study further shows associations between academic fields and voting in Europe (Hooghe et al., 2024). Other studies have examined economics students specifically, showing that their views differ systematically from other students (Frank et al., 1993; Rubinstein, 2006; Bauman and Rose, 2011; Hammock et al., 2016; Fischer et al., 2017; Girardi et al., 2024).

This paper makes four key contributions to this literature. First, it establishes the causal role of academic fields in shaping attitudes using a rigorous selection-on-observables approach, together with complementary quasi-experimental designs. Second, it documents that effects extend to behaviors and life preferences. Third, it examines the mechanisms underlying these effects, ruling out peer effects and earnings expectations, and supporting academic channels as the mechanism. Fourth, it highlights the broader implications of field specialization for the "college divide", political polarization, and gender gaps in ideology.

More broadly, this paper relates to research on how educational content shapes students' political attitudes. Existing studies have often identified causal effects using explicitly political curricula: under authoritarian regimes in Nazi Germany, China, and Taiwan (Voigtländer and Voth, 2015; Cantoni et al., 2017; Chen et al., 2023); in democratic contexts exposed to authoritarian influence, such as Marxist–Leninist instruction in Finland (Meriläinen and Mitrunen, 2025); and where students study the history of authoritarian regimes, such as Nazi

Germany or the communist GDR (Braghieri and Eichmeyer, 2024).[3] Our results extend this literature by showing that educational content can shape political attitudes even without explicit political messaging, and in a setting where students choose their fields of study.

Additionally, this paper connects to a literature in economics on the effects of education on civic engagement and cohesion—mechanisms often invoked to explain its link to economic growth (Gradstein and Justman, 2002; Milligan et al., 2004; Dee, 2004; Glaeser et al., 2007; Oreopoulos and Salvanes, 2011; Kaplan et al., 2025; Ben-Porath, 2025). The evidence presented here shows that certain fields—particularly the humanities and social sciences—enhance political and social engagement more than others. Moreover, it challenges the traditional view that education promotes civic cohesion. Instead, we show that specialization, a defining characteristic of college education, can foster ideological fragmentation. This pattern appears to reflect students' tendency to select fields aligned with their prior beliefs, possibly due to preferences for like-minded peers (Acton et al., 2025) or other mechanisms.

Finally, this paper complements existing evidence on the labor-market returns to fields of study (Hastings et al., 2013; Altonji et al., 2016; Kirkeboen et al., 2016; Andrews et al., 2017; Heinesen et al., 2022; Bleemer and Mehta, 2022; Andrews et al., 2024; Goldstein, 2025) by revealing the political effects of academic fields. Our results also suggest that changes in preferences may be an important mechanism underlying field-specific economic returns.

The rest of the paper proceeds as follows. Section 2 describes the data and setting. Section 3 presents estimates of the effects of academic fields on political attitudes. Section 4 examines mechanisms. Section 5 discusses broader implications. Section 6 concludes.

2 The Data: HERI Student Surveys

This study relies on survey data from the Cooperative Institutional Research Program (CIRP), conducted by the Higher Education Research Institute (HERI) of UCLA.[4] The Freshman Survey (TFS or baseline survey) has gathered information on incoming college students since the 1960s. It covers various topics, including family backgrounds, high school behaviors and achievements, political attitudes, and college and life plans. Since 1990, a subset of these students has also participated in an exit survey upon graduation called the College Senior Survey (CSS or graduation survey). Figure G.1 shows a complete questionnaire from 2002.

[3]Relatedly, Ash et al. (2025) show that exposure to law-and-economics training for U.S. federal judges shifted judicial decisions in a more economically conservative direction, illustrating that ideas transmitted through professional education can shape political and policy outcomes even outside traditional classroom settings.
[4]Details on the data and access are available here: https://heri.ucla.edu/cirp-freshman-survey.

Survey participation requires a fee. Still, many U.S. colleges participate each year: a few hundred in the baseline survey and several dozen to a few hundred in the graduation survey, with substantial overlap across surveys. Both surveys, though officially voluntary, are purposefully integrated into mandatory student processes, such as orientation or registration, and graduation packets or rehearsal sessions, yielding relatively high response rates.[5]

2.1 Sample

Our main linked sample includes only students who participated in both the baseline and graduation surveys, yielding 318,371 students from 452 four-year colleges. Restricting to students with non-missing graduation ideology leaves 308,636 observations. Requiring non-missing ideology in both the baseline and graduation surveys, as needed for some analyses, leaves 294,571 students. Appendix A summarizes the sample-construction pipeline.

Table 1 describes the study sample and compares it to all baseline survey respondents from four-year colleges, as well as to a weighted version representative of the college student body. Compared with all baseline respondents, the study sample is more female (0.62 vs. 0.56), more white (0.79 vs. 0.69), less southern (0.12 vs. 0.25), higher-income, and more Catholic (0.41 vs. 0.28). It is also slightly more conservative at baseline: 28 percent identify as conservative or far right, versus 23 percent in the baseline sample. Differences in intended-major shares are small. Overall, the study sample provides wide coverage of the American college population.

2.2 Key Variables

Fields of study. The data include information on students' intended majors when they start college and their actual majors at graduation. The list of majors in the survey changes slightly over time, so we aggregate them into broader groups at two levels, as detailed in Appendix Table G.1. Appendix Figure G.2 illustrates the transitions between intended and actual majors. The figure shows movement between nearly all pairs of majors. Still, a significant share of students eventually pursue their intended majors: 47-77 percent in the broad groups and 22-59 percent in the detailed classification.

Note that until 2004, the survey recorded only one major per individual, thereby omitting information on double majors. Since 2005, the survey has contained information on double majors. We use this information for a subsample analysis that examines the effects of double

[5]Relative to IPEDS enrollment data, TFS covers an average of 64 percent of incoming undergraduate cohorts, while CSS covers about 34 percent of the corresponding cohorts four years later. These rates are broadly comparable, given that roughly 40 percent of U.S. college entrants do not complete a degree.

Table 1: Summary Statistics: Baseline Survey and Study Sample

	Baseline w/ wts			Baseline			Study Sample		
Variable	Mean	SD	N	Mean	SD	N	Mean	SD	N
Female	0.54	0.50	9,527,540	0.56	0.50	9,527,540	0.62	0.48	317,885
Age	18.39	1.93	9,424,814	18.60	2.32	9,424,814	18.32	0.97	316,586
Black	0.09	0.29	9,591,655	0.08	0.27	9,591,655	0.03	0.18	318,371
Hispanic	0.06	0.24	9,591,655	0.05	0.22	9,591,655	0.04	0.19	318,371
White	0.66	0.47	9,591,655	0.69	0.46	9,591,655	0.79	0.41	318,371
South	0.26	0.44	9,591,655	0.25	0.43	9,591,655	0.12	0.33	318,371
Income below 50k	0.35	0.48	8,429,651	0.35	0.48	8,429,651	0.27	0.45	285,373
Income 50-100k	0.33	0.47	8,429,651	0.34	0.47	8,429,651	0.36	0.48	285,373
Income above 100k	0.32	0.46	8,429,651	0.31	0.46	8,429,651	0.37	0.48	285,373
Catholic	0.26	0.44	9,591,655	0.28	0.45	9,591,655	0.41	0.49	318,371
B. Baseline Ideology									
Liberal or Far Left	0.30	0.46	8,839,179	0.31	0.46	8,839,179	0.28	0.45	303,191
Middle-of-the-road	0.47	0.50	8,839,179	0.46	0.50	8,839,179	0.44	0.50	303,191
Conservative or Far Right	0.23	0.42	8,839,179	0.23	0.42	8,839,179	0.28	0.45	303,191
C. Intended Majors									
Natural Sciences	0.14	0.34	7,946,774	0.13	0.33	7,946,774	0.14	0.35	273,869
Engineering & Math	0.16	0.37	7,946,774	0.15	0.36	7,946,774	0.11	0.32	273,869
Social Sciences	0.18	0.38	7,946,774	0.18	0.39	7,946,774	0.19	0.39	273,869
Business & Economics	0.19	0.39	7,946,774	0.19	0.39	7,946,774	0.20	0.40	273,869
Humanities & Arts	0.11	0.31	7,946,774	0.12	0.33	7,946,774	0.13	0.34	273,869
Health & Education	0.23	0.42	7,946,774	0.22	0.42	7,946,774	0.23	0.42	273,869

Note: This table reports summary statistics for our study sample. "Baseline" include all students in the baseline survey. "Baseline (w/ weights)" include all students in the baseline survey with non-missing weight, and uses the survey weights for mean and SD. "Study sample" restricts to students observed at graduation. N report the number of observation with non-missing values.

majors. Table G.2 summarizes the prevalence of double majors, showing that about 30 percent of students in the relevant cohorts pursue a double major, with variation across fields.

Political ideology. The primary outcome of interest is students' responses to the question: "How would you characterize your political views?" with a response scale from 5 (far left)

to 1 (far right).[6] For clarity, we rescaled the variable so that lower values indicate more left-leaning orientations. This rescaling is used in all subsequent analyses. This ideological measure is strongly correlated with voting behavior. Because the HERI surveys do not report party-specific voting, we rely on external survey data to measure this association. Figure G.3 demonstrates that responses to a similar ideology question are highly correlated with voting patterns among students in two national surveys (CCES, 2006–2024; ANES, 1948–2024).

The trends in ideology among incoming and graduating students in our sample appear in Appendix Figure G.4. This figure highlights several trends. First, incoming college students have become slightly more left-leaning over time, especially in the social sciences, humanities, and arts. Second, students consistently shift further left from entry to graduation. Third, this shift varies by major: students in the social sciences, humanities, and arts become substantially more liberal during college—a pattern observed across all cohorts. Fourth, the magnitude of this ideological shift has increased over time across all majors.

Figure G.5 presents distributional changes in students' political ideology over the course of college, separately by major group. On average, students become more liberal during college. The magnitude of this shift varies by major group. Figure G.6 provides detailed information on the transitions between different ideological positions.

Panel A of Figure 1 compares students' baseline and graduation ideology across majors in the detailed classification. For majors in business and economics, the average ideology remains similar to the baseline, as their corresponding circles are very close to the 45-degree line. Students in health, education, engineering, and natural sciences become slightly more left-leaning, as their corresponding circles are below the 45-degree line. Finally, humanities and social sciences majors exhibit a significant leftward shift during college, as their circles are much below the 45-degree line.

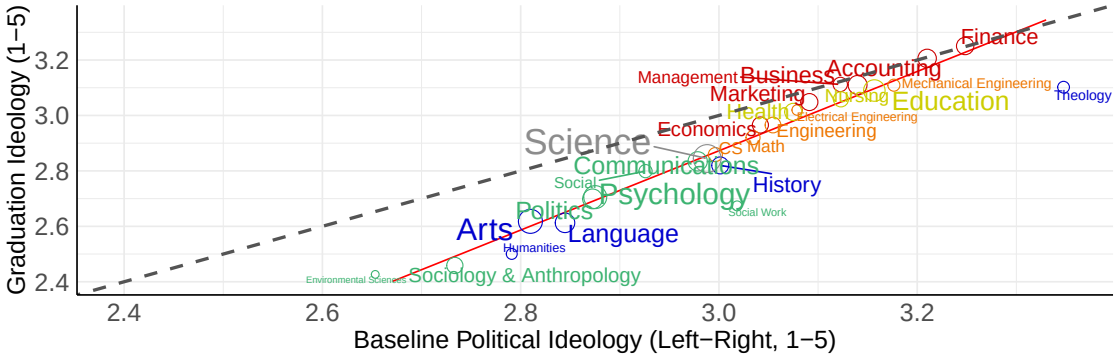
Interestingly, the fields in which students become more left-leaning are typically those that also attract more left-leaning students at baseline. There are exceptions: some majors attract less left-leaning students yet induce a more leftward shift. A notable example is theology, which, despite attracting relatively conservative students, tends to lead to a marked shift toward more left-leaning views on average.

Policy positions. The survey data include questions on specific policy issues, with responses ranging from 1 (disagree strongly) to 4 (agree strongly). We focus on twelve questions that appear in both the entry and exit surveys for a sufficient number of years in our sample period. We also include a question on climate priorities, although it is available for fewer

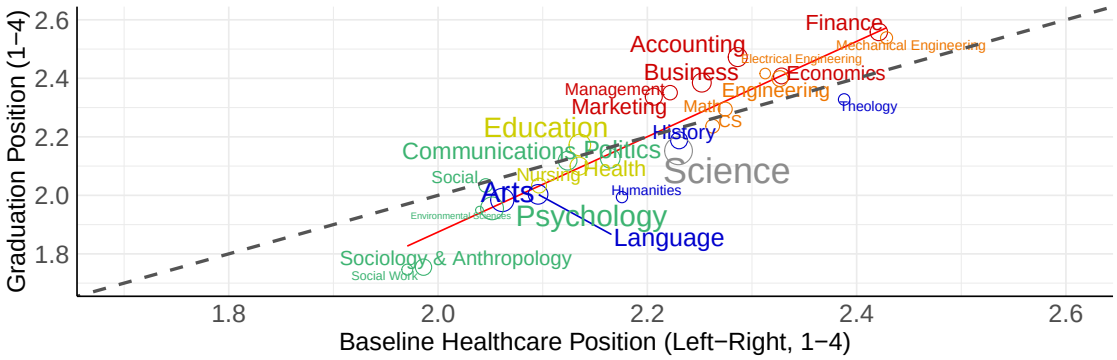
[6]While concerns may arise about comparing ordinal scales across groups (Bond and Lang, 2019), this issue is less severe in our setting because we observe the same scale for each individual before and after college.

Figure 1: Baseline and Graduation Attitudes

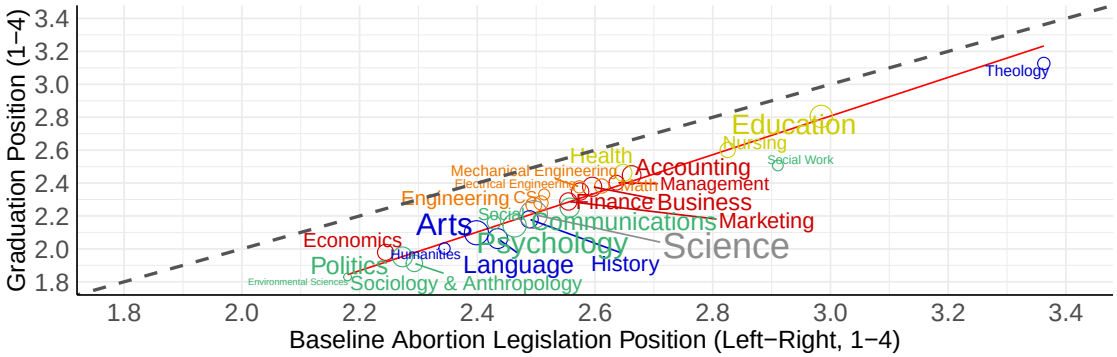
(A) Ideology



(B) National Healthcare



(C) Abortion Legislation



Note: This figure plots average baseline attitudes (x-axis) against average graduation attitudes (y-axis) by major. The sample includes students with non-missing values for both major and the relevant attitude measure: political ideology in panel (a) (294,571 students), attitudes toward national healthcare in panel (b) (149,603 students), and attitudes toward abortion legislation in panel (c) (251,945 students). Political ideology is measured on a 1–5 scale, while the healthcare and abortion measures are on 1–4 scales; in all panels, higher values correspond to more right-leaning positions. Circle size is proportional to the number of students in each major. Colors indicate the major group: blue for humanities and arts, green for social sciences, gray for natural sciences, yellow for health and education, orange for engineering and math, and red for business and economics. The black dashed line is the 45-degree line, where average graduation attitudes equal average baseline attitudes. The red solid lines show the fitted regression line across major-level means.

years. Table G.3 lists these questions and reports the share of students who agree, either strongly or somewhat, with each statement, overall and by college major, before and after college. The table reveals two patterns. For some issues, such as climate change and abortion, students across majors shift in broadly similar directions. For others, especially economic policy issues such as healthcare and taxation, changes differ more sharply across majors.

This suggests that field-of-study differences are larger for economic policy positions than for other policy domains. Panels B and C of Figure 1 illustrate this contrast. On national healthcare, students in different majors move in different directions. On abortion, by contrast, students across majors generally move toward more liberal positions during college, with much less cross-major variation in these changes.

3 Impacts of Academic Fields on Political Attitudes

This section examines the effect of academic fields on students' political ideology. In subsection 3.1 employ a selection-on-observables approach that leverages the richness of our data. This approach delivers plausibly causal estimates of the relative effect of each major, and in subsection 3.2 we provide evidence that unobserved selection is unlikely to generate meaningful bias in our setting. Then, in subsection 3.3, we report results using two complementary strategies that exploit supply-side variation in the availability of specific fields. Finally, in subsection 3.4, we show that the effects extend to policy positions, behaviors, and life goals

3.1 Baseline Estimates

Let $p_i(m)$ denote the potential political ideology at graduation for student i under major m. Let $m_i \in \{m_1, \ldots, m_k\}$ denote the realized major of student i, where k is the number of possible majors. The observed ideology is $p_i \equiv p_i(m_i)$.

Let $X_i \equiv \{X_i^p, X_i^m\}$ denote the vector of main controls, which consists of two sets of variables. The first, X_i^p, includes predictors of the outcome, most importantly, ideology measured before college. Although this is a lagged outcome, we do not view it as sufficient to account for all selection on ideology levels, given potential measurement error from discretization or survey response error. Therefore, X_i^p also includes a rich set of baseline policy positions that help absorb remaining selection on ideology levels.

A potential caveat is that major choices may be correlated not only with ideology levels but also with latent propensities for ideological change. To address this concern, we include X_i^m, a set of predictors of the treatment, realized college major. The key idea is that unobserved

predictors of ideological change threaten our estimates only to the extent that they are also correlated with major choice, for example because they shape students' preferences over fields. Controls that strongly predict major choice should therefore help absorb this remaining source of selection. X_i^m includes intended major as well as intended careers and life goals, which predict realized major.

We estimate the following equation:

$$\underbrace{p_i}_{\substack{\text{Graduation political} \\ \text{ideology}}} = \underbrace{\sum_m \delta_m D_{im}}_{\substack{\text{Major} \\ \text{effects}}} + \underbrace{X_i'\alpha}_{\substack{\text{Lagged ideology and proxies,} \\ \text{Intended major and proxies}}} + \varepsilon_i \tag{1}$$

Where $D_{im} = \mathbf{1}\{m_i = m\}$ is an indicator equal to one if student i pursues major m. Under the required assumptions, the coefficients of interest, δ_m, capture the effect of each major m on political ideology relative to the omitted reference category. The key identification assumption is that, conditional on the controls, realized majors are not correlated with unobserved factors that affect political ideology at graduation.[7] This assumption could be violated through two main channels. First, pre-existing predictors of political ideology that are not fully captured in the baseline survey may remain correlated with major choice even after conditioning on our controls. Second, post-baseline shocks that affect major choice may also affect political ideology. Below, we conduct sensitivity analyses and report a battery of robustness tests suggesting that any such bias is likely very small in our setting.

Given that this is a regression model with multiple treatments, interpreting δ_m as the average treatment effect of major m also requires restrictions on treatment-effect heterogeneity. We discuss this formally in Appendix B and argue that these restrictions are plausible in our setting. We also implement a robustness test that does not impose such restrictions, following Goldsmith-Pinkham et al. (2024), and show that it has little effect on the estimates.

Panel A of Figure 2 presents the main estimation results. The bold lines represent coefficients from a model that groups majors into six broad categories, while the thinner lines represent a more detailed specification with 27 categories. In both cases, the omitted group is the natural sciences (biology, physics, chemistry). The estimates reveal substantial effects. Panel A shows

[7] All controls are included flexibly, using indicators for each response category observed in the data. The survey also includes rich demographic information, but we do not control for such variables in the main analyses, since they are not central to our identification argument and, as we show in the robustness section, their inclusion does not meaningfully affect the results. In the robustness specifications, we also include college and year fixed effects. Table G.4 lists the controls used in the main and robustness analyses. In unreported analyses, we also verify that the results are robust to allowing interactions among key controls.

that majors in the humanities, arts, and social sciences shift students' ideology leftward by about 0.09 SD relative to the natural sciences. By contrast, economics and business majors shift students rightward by about 0.14 SD, while professional fields—health, education, and engineering—shift rightward by about 0.06 SD relative to the natural sciences.

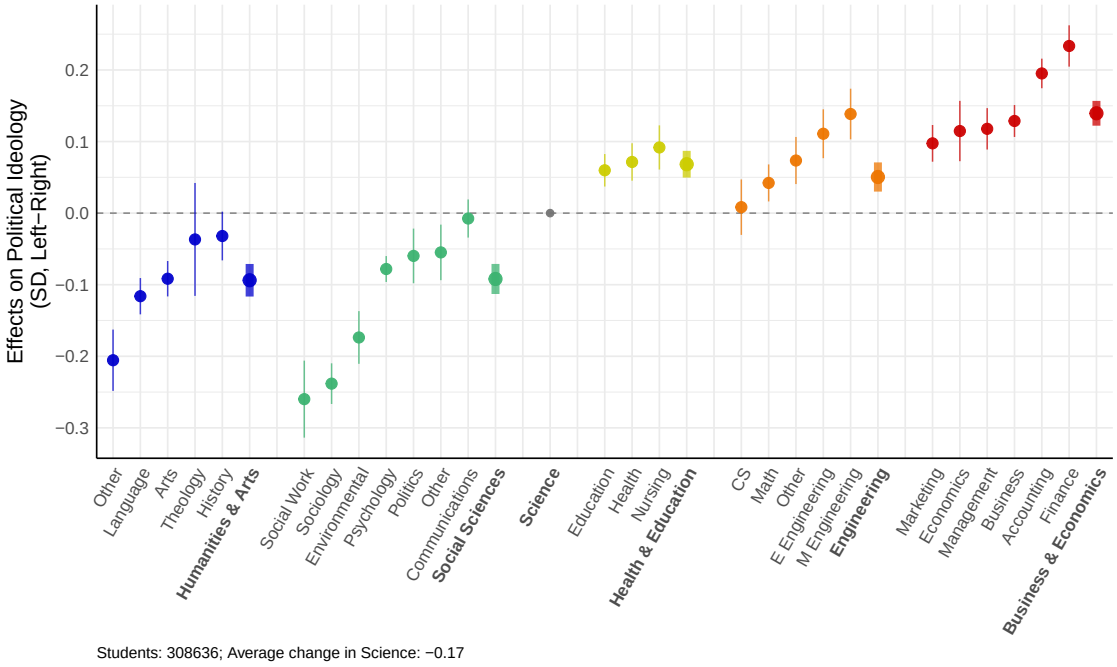
The figure also reveals heterogeneity within major groups. Among the humanities and social sciences, fields such as other humanities, sociology, social work, and environmental studies show the strongest leftward effects (0.17 to 0.26 SD). Within business-related fields, accounting and finance cause the most substantial rightward shifts (0.19 and 0.24 SD).

Robustness. Appendix C reports a battery of robustness tests for specification and estimation choices. First, the conclusions are similar when we use alternative outcome definitions, including binary measures and first differences. Second, we examine differential attrition from the graduation survey and show that it is small and unlikely to generate meaningful sample-selection bias. Third, we show that accounting for double majors does not change the estimated effects of first majors, and we also report interacted results. Finally, we estimate a specification that exploits only variation in realized major driven by intended major. This variation cannot be biased by shocks that occur after the baseline survey, and the results are very similar. Together, these tests support a causal interpretation of our estimates. In the next subsection, we further assess the scope for omitted-variable bias from any source and provide bounds for such cases.

Heterogeneity. Appendix C shows that the ideological effects of college majors are not concentrated among particular students, institutions, or cohorts. It report estimates of Equation 1 separately for several subsamples. Panel A of Table C.3 reports results by students' personal characteristics. The effects of majors on ideology are broadly similar across gender, socioeconomic background, and high school GPA. If anything, the contrast between economics or business and social sciences or humanities is slightly larger among high-GPA students, though the difference is modest: 0.27 SD for high-GPA students compared with 0.22 SD for low-GPA students. Table C.4 reports results by college characteristics. The estimates are again broadly similar across institution types, levels of selectivity, and geographic regions. Finally, Table C.5 examines changes over time. The estimates are somewhat larger after 2010, around 0.27–0.28 SD, compared with 0.22–0.23 SD in earlier cohorts.

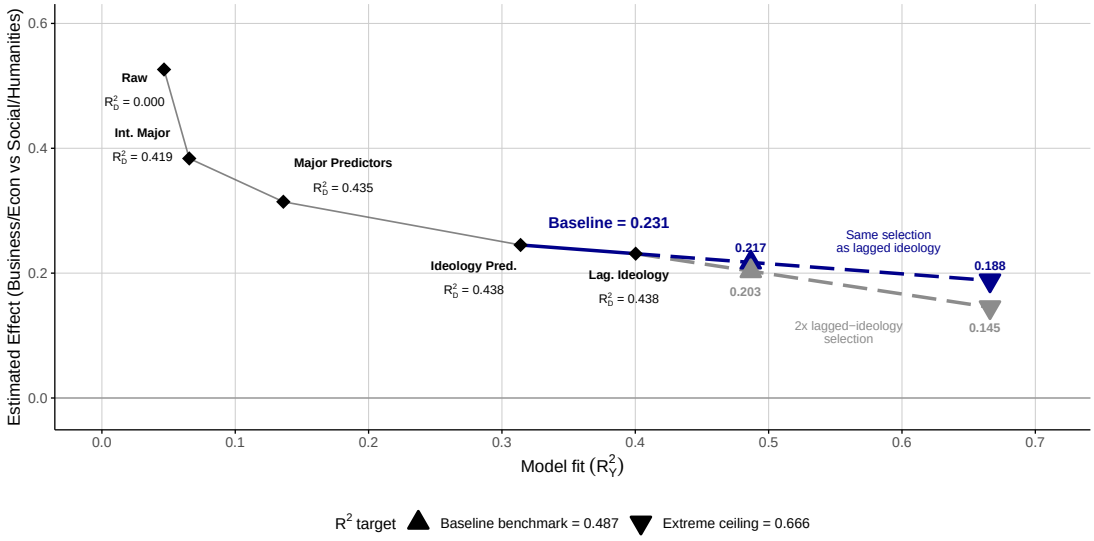
Figure 2: Effects of College Majors on Political Ideology

(A) Main Estimates



Students: 308636; Average change in Science: −0.17

(B) Sensitivity: Effects of Business/Economics versus Social/Humanities



Note: The figure in panel A plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals. The bold lines represent coefficients from a model that groups majors into six broad categories. The thinner lines show coefficients from a more detailed model with 26 specific major categories. In both models, the natural sciences serve as the omitted reference group. The outcome is based on responses to: "How would you characterize your political views?", measured in standard-deviation units. The estimates are based on the main specification, which controls flexibly for students' baseline political ideology and policy positions (predictors of the outcome), and intended majors and life goals (predictors of the treatment). The sample includes all students with non-missing major and outcome. Panel B presents an Oster-style sensitivity analysis for the contrast between business/economics and social sciences/humanities under different conservative scenarios (see main text for details).

3.2 Assessing Omitted Variable Bias

Because our baseline estimates rely on a selection-on-observables approach, we examine the extent to which omitted-variable bias could change the results. We focus on the contrast between business or economics and social sciences or humanities, where the estimated effect is largest. Specifically, we restrict the sample to students in these fields and estimate models with a single treatment indicator equal to one for students majoring in business or economics.

Panel B of Figure 2 presents a sensitivity analysis based on the logic of Oster (2019). The figure plots the estimated effect against the explanatory power of the outcome model, measured by R^2, as we progressively add controls. The raw difference is large, 0.53 SD. Adding intended major and other treatment predictors reduces the estimate to 0.36 SD, while increasing the R^2 from 0.04 to 0.13. Adding pre-college ideology and other outcome predictors further reduces the estimate to 0.23 SD, but also sharply increases the R^2 to 0.40.[8]

We then use an Oster-style extrapolation to assess how much additional selection on unobservables would change the estimate. Appendix D.1 provides the formal details. The exercise asks how far the coefficient would move if residual graduation ideology remained correlated with treatment, conditional on the observed controls, in the same way as observed pre-college ideology. This relaxes the identifying assumption in the baseline regression, which requires residual graduation ideology to be uncorrelated with treatment. To construct bounds, the remaining question is the appropriate value of $R^2_{\max}$: the explanatory power that would be obtained if all relevant determinants of the outcome were observed. This value is plausibly below one because of discretization of the underlying ideology measure, heterogeneous effects of majors across colleges, survey response error, and idiosyncratic noise.

We report bounds under two values of $R^2_{\max}$. First, we allow residual variation to explain as much of the outcome as observed pre-college ideology. This gives $R^2_{\max} = 0.487$ and a lower bound of about 0.22 SD. Second, we allow for substantially more residual explanatory power, restricted only by the four sources of measurement and idiosyncratic variation described above. This gives $R^2_{\max} = 0.649$ and a lower bound of about 0.19 SD.[9] Thus, even when residual outcome variation is allowed to be as correlated with treatment as observed pre-college ideology, the implied effect remains large and close to the baseline controlled estimate.

[8]The figure labels report the corresponding R^2 from the treatment model, R^2_D, which measures how well each specification predicts selection into business or economics rather than social sciences or humanities. These values rise sharply once intended major and major-choice predictors are added, indicating that the controls explain much of the observable sorting across fields. By contrast, the treatment coefficient becomes relatively stable once outcome predictors are included.

[9]This value is based on multiple analyses and simulations.

The gray line reports an even more extreme scenario, in which selection on residual graduation ideology is twice as strong as selection on observed pre-college ideology. Even under this scenario, the lower bound remains positive in the most conservative specification, at about 0.15 SD. Overall, these results suggest that omitted-variable bias is very unlikely to fully explain the estimated ideological difference between students in business or economics and students in social sciences or humanities. Appendix D.2 reaches a similar conclusion using the approach of Cinelli and Hazlett (2020).

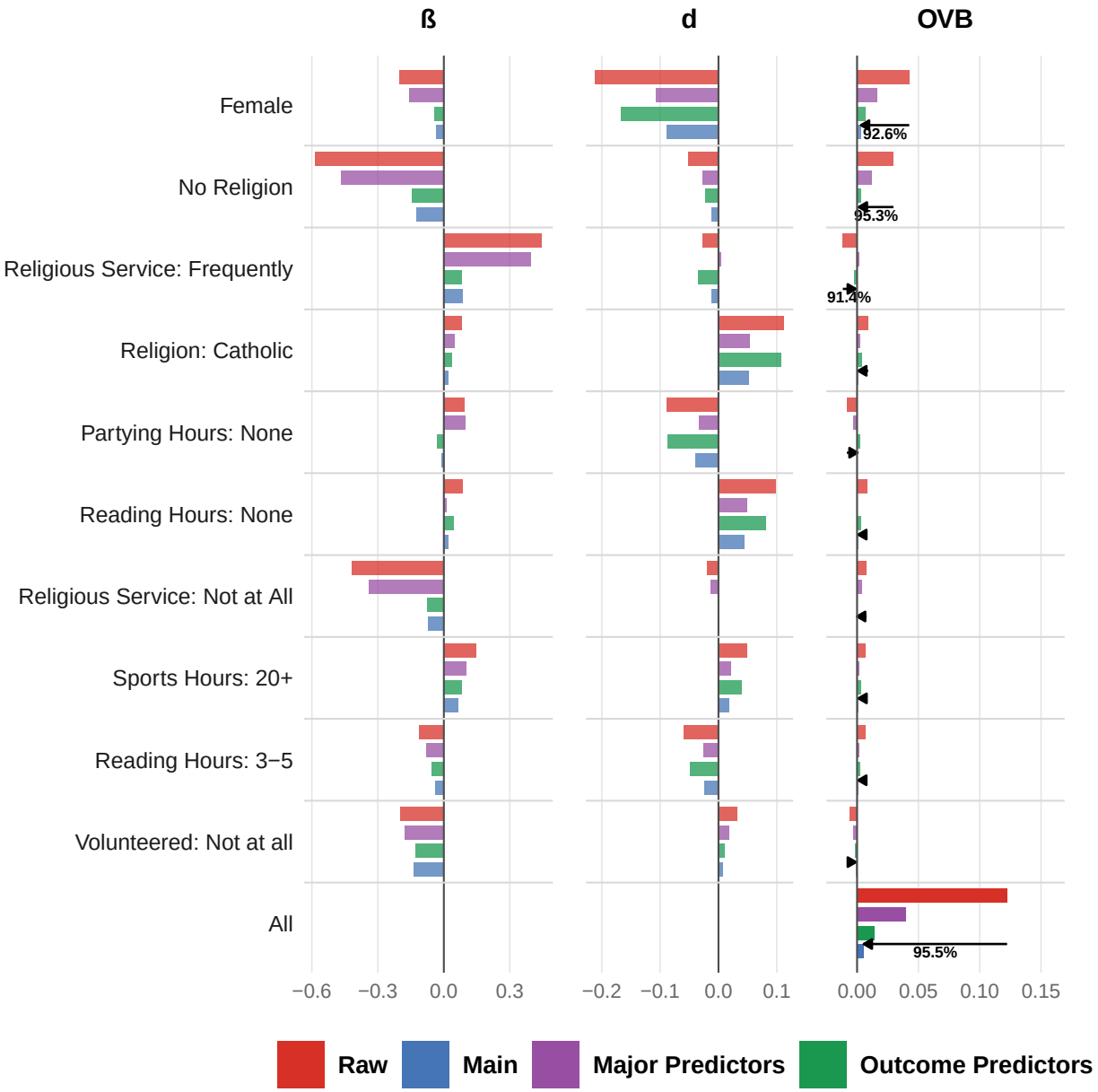
We also exploit additional demographic and behavioral variables available in the survey to assess the plausibility of the selection-on-observables assumption. First, we show that adding these variables directly to the main specification has essentially no effect on the estimated coefficient (Figure C.2). This stability is not because these variables are irrelevant. Rather, it reflects that our baseline controls already capture most of the selection generated by them.

Panel B of Figure 3 makes this point directly. For each omitted covariate, the figure decomposes its potential omitted-variable bias into two components: its association with the outcome and its association with treatment status, conditional on different sets of controls.

The product of these two terms gives the implied omitted-variable bias from excluding that covariate. We report this decomposition for four specifications: the raw specification, a specification controlling only for outcome predictors, a specification controlling only for major-choice predictors, and our main specification, which includes both sets of controls.

The bottom row reports the aggregate implied bias from excluding all additional demographic and behavioral variables. In the raw specification, these variables would generate substantial omitted-variable bias, about 0.12 SD. Our main specification reduces this implied bias by more than 95 percent. This reduction is substantially larger than in specifications that include only outcome predictors or only major-choice predictors. The reason is visible in the decomposition. Outcome predictors are especially effective at absorbing the relationship between omitted covariates and political ideology, while major-choice predictors are especially effective at absorbing the relationship between omitted covariates and selection into fields. Combining the two therefore reduces both components of omitted-variable bias.

Figure 3: Omitted Variable Bias Reduction, Observable Covariates



Notes: This figure assesses how much omitted-variable bias would be generated by additional demographic and behavioral covariates that are not included in the baseline specification. The sample is restricted to students in business/economics and social sciences/humanities. The treatment is an indicator for majoring in business/economics. For each covariate, the left panel reports its conditional association with the outcome, β; the middle panel reports its conditional association with treatment status, δ; and the right panel reports the implied omitted-variable bias, given by $\beta \times \delta$. Estimates are reported under four specifications: no controls (Raw), controls for predictors of realized major only (Major Predictors), controls for predictors of political ideology only (Outcome Predictors), and the main specification, which includes both sets of controls. The row labeled "All" reports the joint implied bias from adding all additional demographic and behavioral controls. Black arrows report the percent reduction in implied bias relative to the raw specification. The figure shows that these covariates would generate substantial bias if omitted from an uncontrolled specification, but that the main specification reduces their implied bias by more than 95 percent.

3.3 Additional Evidence Exploiting Supply-Side Variation

To complement the selection-on-observables analysis, we provide results from complementary approaches that exploit supply-side variation in treatment status.

Variation in intended-major persistence. We implement an IV approach that exploits variation across programs, defined by college-by-major cells, in the probability that students remain in their initially intended field. We focus on students who entered college intending to major in business or economics for two reasons. First, these fields are the most right-leaning major groups in our baseline estimates, providing a clear test of whether field choice causally affects ideology. By contrast, persistence in natural sciences or engineering may shift students away from both left-leaning fields, such as social sciences or humanities, and right-leaning fields, such as business or economics. Second, business and economics programs are often competitive and subject to capacity constraints, unlike many fields in the social sciences and humanities. This generates meaningful variation in whether students ultimately pursue their initially intended major.

For each college c, intended major m, and entry cohort t, we construct a leave-one-out persistence rate,

$$Z_{icmt} = \frac{1}{N_{cmt} - 1} \sum_{j \in (c,m,t),\, j \neq i} \mathbf{1}\{m_j = m\},$$

where m_j denotes student j's realized major at the time of the graduation survey and N_{cmt} is the number of surveyed students in cell (c, m, t). Thus, Z_{icmt} measures the share of other surveyed students in the same college, intended-major, and entry-year cell who ultimately remain in their initially intended program.

The endogenous treatment, $D_{i,BE} = \mathbf{1}\{m_i \in \text{Business/Economics}\}$, is an indicator equal to one if student i majors in business or economics. The outcome p_i is graduation political ideology. The first-stage and second-stage equations are

$$\begin{aligned} D_{i,BE} &= \pi Z_{icmt} + X_i'\gamma + u_i, \\ p_i &= \beta D_{i,BE} + X_i'\theta + v_i \end{aligned} \tag{2}$$

where X_i includes the main set of controls: predictors of ideology and predictors of major choice. These controls are not required for identification, but they can improve precision and reduce concerns about violations of the identifying assumption. We therefore report results that add them sequentially. In an additional specification, we also control for enrollment year, college type, and state.

Under the usual IV assumptions, the 2SLS estimate of β identifies the local average treatment effect of pursuing a business or economics major, relative to the counterfactual major choice of compliers, on political ideology at graduation. Formally, this interpretation requires four assumptions. First, independence: conditional on controls, Z_{icmt} should be uncorrelated with unobserved determinants of political ideology. Panel A of Table 2 reports falsification tests using baseline ideology as the outcome. We report results for both the full set of students observed in the baseline survey and the subset who are also observed in the graduation survey. In both samples, once intended-major fixed effects are included, the instrument is not correlated with baseline ideology, supporting for the independence assumption.

Second, relevance: Z_{icmt} should shift the probability of pursuing business or economics. The first row of Panel B in Table 2 reports the corresponding first-stage estimates. With the full set of controls, a one-percentage-point increase in the IV increases the probability of pursuing business or economics by 0.09–0.12 pp. The estimates are statistically significant, with a first-stage F-statistic of over 20 in all specifications, supporting the relevance condition.

Third, exclusion: Z_{icmt} should affect ideology only through its effect on realized major. Fourth, monotonicity: a higher-persistence environment should not make some students less likely to remain in their intended major. These assumptions could be violated if programs with higher persistence differ systematically in other ways that directly shape students' trajectories, such as institutional culture, peer composition, or broader academic environment. To reduce this concern, we also report specifications that measure persistence rates using adjacent cohorts rather than the student's own cohort.

Panel B of Table 2 reports the reduced-form and IV estimates using political ideology at graduation as the outcome. In the preferred specification, which includes the main controls, the reduced-form estimate is 0.04 SD and the IV estimate is 0.34 SD. Both are statistically significant at the 95 percent confidence level. When we add controls for enrollment year, college state, and college type, the estimates remain very similar, although the IV estimate becomes only marginally significant.

To interpret the magnitude of the IV estimate, it is useful to consider the counterfactual major choices of compliers. Panel A of Table G.5 shows that the instrument significantly reduces the probability of pursuing social sciences and humanities, by 5 and 3 pp, respectively. This implies that almost 90 percent of compliers would have pursued one of these two fields in the absence of treatment, while the remaining compliers would have pursued natural sciences, engineering, or mathematics.

Table 2: IV Estimates Based on Cohort-Specific Persistence in Intended Majors

Panel A. Falsification Tests: Baseline Ideology

	(1)	(2)	(3)
Full Baseline Survey	0.003	-0.010	-0.007
	(0.017)	(0.019)	(0.017)
N	226,208	226,208	226,208
Students Included in the Graduation Survey	0.062**	0.013	0.009
	(0.024)	(0.028)	(0.025)
N	47,579	47,579	47,579
Intended Major FE	No	Yes	Yes
Major Predictors	No	No	Yes
Outcome Predictors	No	No	No

Panel B. IV Results

	(1)	(2)	(3)	(4)	(5)
First Stage: Business/Economics Major	0.173***	0.125***	0.121***	0.115***	0.094***
	(0.019)	(0.025)	(0.025)	(0.025)	(0.017)
Reduced Form: Graduation Ideology	0.106***	0.056**	0.053**	0.039**	0.032**
	(0.027)	(0.026)	(0.024)	(0.016)	(0.014)
2SLS: Graduation Ideology	0.613***	0.451**	0.441**	0.340**	0.338*
	(0.164)	(0.227)	(0.222)	(0.163)	(0.174)
N	49,299	49,299	49,299	49,299	49,299
First-stage F	84.7	24.5	23.5	21.1	29.2
Intended Major FE	No	Yes	Yes	Yes	Yes
Major Predictors	No	No	Yes	Yes	Yes
Outcome Predictors	No	No	No	Yes	Yes
Additional	No	No	No	No	Yes

Notes: Panel A reports falsification tests using baseline political ideology as the outcome. The first row block uses the TFS baseline-survey sample, and the second uses the matched CSS sample. Panel B reports the first stage, reduced form, and 2SLS estimates for graduation political ideology in the CSS sample of students who initially intended to major in Business or Economics. The instrument is the leave-one-out cohort-specific persistence rate in the initially intended major. Entries report coefficient estimates with college-clustered standard errors shown below each estimate. "Intended Major FE" indicates fixed effects for intended majors. "Major Predictors" indicates baseline goals and career preferences. "Outcome Predictors" indicates baseline ideology and policy positions. "Additional" indicates controls for state, entry year, and institution-control-by-institution-type-by-selectivity-group fixed effects, and applies only to Panel B. "First-stage F" reports the single-instrument Wald statistic, computed as the squared t-statistic on the instrument in the first stage. Each row block uses students with non-missing values for the relevant outcome. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

This comparison also helps benchmark the IV estimate. The baseline estimate for the effect of business or economics relative to social sciences or humanities is about 0.23 SD. The IV estimate is somewhat larger, at 0.34 SD, although the difference is not statistically significant. Several forces could explain the larger point estimate: measurement error and attenuation bias in the baseline estimates, treatment-effect heterogeneity with larger effects among compliers, or sampling noise. The heterogeneity results above make the second explanation less likely, suggesting that the first or third is more likely.

Panel B of Table G.5 reports results for additional outcomes. Column (1) reports the effect of the instrument on predicted graduation ideology, constructed from students' realized majors and the baseline estimates of major effects. The predicted reduced-form effect is 0.019 SD, which is smaller than the actual reduced-form estimate of 0.032. This is consistent with the discussion above: the IV estimate is larger than what would be implied by applying the OLS estimates to the instrument-induced changes in majors. Columns (2)–(4) report reduced-form estimates for binary ideology outcomes. The pattern suggests that the effect operates mainly through a shift away from left-wing identification and toward both middle-of-the-road and conservative ideology.

Table G.6 reports a robustness analysis that measures persistence using students from adjacent cohorts. The sample is slightly smaller because not all adjacent cohorts are included in the survey. Nevertheless, the estimates are very similar. In the specification with the main set of controls, the IV estimate is 0.33 SD and is marginally statistically significant.

Field expansions. While our IV approach exploits between-program variation, we also provide evidence from within-college expansions of programs over time. Specifically, we implement a quasi-experimental design that exploits sharp increases in enrollment in business and economics programs at specific colleges.[10] As shown below, these expansions affect not only students' major choices, but also their coursework outside their majors. For this reason, the design does not isolate an IV estimate of the effect of major choice alone, but instead identifies the reduced-form effect of expanded exposure to these fields.

We use administrative data from the Integrated Postsecondary Education Data System (IPEDS) to identify program-expansion shocks. IPEDS reports the number of graduates by institution, year, and Classification of Instructional Programs (CIP) code. We classify CIP codes into the same major groups used in the survey analysis. For each college c and year t, let N_{ct} denote the total number of graduates and let N_{ct}^{BE} denote the number of graduates in business or economics. We define the business/economics share as $B_{ct} = \frac{N_{ct}^{BE}}{N_{ct}}$.

[10] We focus on shocks to business and economics for reasons similar to those discussed above, and because these fields experienced substantial enrollment expansions in recent decades, providing multiple quasi experiments.

We define a business/economics expansion shock as a sharp increase in this share. Formally, let $\Delta B_{ct} = \frac{B_{ct} - B_{c,t-1}}{B_{c,t-1}}$ denote the proportional change in the business/economics share from year $t-1$ to year t. In the main specification, college c experiences a shock in year t if $\Delta B_{ct} \geq 0.30 \quad \text{and} \quad |\Delta B_{c,t-1}| < 0.15$. Thus, a shock is defined as an increase of at least 30 percent in the business/economics share, following a year with no large change in either direction. The second condition rules out cases in which the measured increase is part of a longer gradual trend or a reversal of a large previous decline. These cutoffs are necessarily somewhat discretionary. We therefore choose the main definition using a transparent data-driven procedure and show that the results are robust to alternative definitions.[11]

Let s_c denote the first year in which college c experiences such a shock. We identify shocks at 239 colleges included in the baseline survey, 133 of which are also included in the graduation survey. We use all colleges for falsification tests and restrict the main analysis to colleges observed in the graduation survey. We estimate the following two-way fixed-effects event-study specification to trace the dynamic effects of these college-level shocks:

$$y_{ict} = e_c + s_t + \sum_{k \neq -3} \rho_k \mathbf{1}[t = S_c + k] + f(y^0_{ict}) + v_{ict} \tag{3}$$

Here, $\mathbf{1}[t = S_c + k]$ is an indicator equal to one if student i enrolled in college c exactly k years relative to the shock year S_c. The coefficients of interest, ρ_k, measure the effect of the shock relative to the omitted category ($k = -3$). We use this cohort as the reference to avoid contamination from earlier cohorts that may have already been exposed to course changes. The terms e_c and s_t denote college and cohort fixed effects. y^0_{ict} denotes the lagged dependent variable.[12] We also estimate a specification with a single post-shock indicator, which captures the average effect across the four post-shock cohorts. In this specification, we exclude the two cohorts immediately preceding the shock because these students may have been partially exposed to changes in course supply.

The key identifying assumption is parallel trends: absent the shock, colleges that experience an expansion and colleges that do not would have followed similar trends in the outcomes.

[11] Specifically, we evaluate 27 candidate shock definitions that vary the required increase in the business/economics share (20, 25, or 30 percent), the maximum allowable change in the previous year (one-half, two-fifths, or one-third of the required increase), and the minimum cohort size (0, 25, or 50 students). For each definition, we estimate the pooled post-shock first-stage effect on majoring in business or economics in the survey data and select the definition with the largest first-stage t-statistic. Figure G.9 plots, for each candidate definition, the pooled post-shock estimate for majoring in business/economics against the pooled post-shock estimate for ideology.

[12] This is baseline political ideology, policy positions, or life goals when the outcome is a survey response on these topics. When estimating effects on major choice, we exclude such controls, as intended majors may themselves be affected when colleges expand certain fields. We control for this flexibly by including fixed effects for each discrete baseline response.

This assumption is plausible if the shocks primarily reflect supply-side changes, driven by institutional decisions rather than changes in student preferences, and are not directly related to students' potential political ideology. The design is therefore similar in spirit to well-established labor-economics designs that use mass layoffs as quasi-experimental variation in job loss (Jacobson et al., 1993; Couch and Placzek, 2010). Anecdotal evidence supports this interpretation. For example, web reports indicate that the two largest affected colleges since 2010 received substantial donations specifically earmarked for expanding business and economics programs, with timing closely aligned with our identified shocks.

Panels A and B of Figure 4 present falsification tests that further assess this identifying assumption. We estimate Equation 3 using intended major and baseline ideology as outcomes.[13] Using all observations from the baseline survey, we test whether the shocks are associated with changes in students' pre-existing characteristics. These tests have high statistical power because they use the full baseline-survey sample, which is much larger than the main analysis sample of students observed in both surveys.

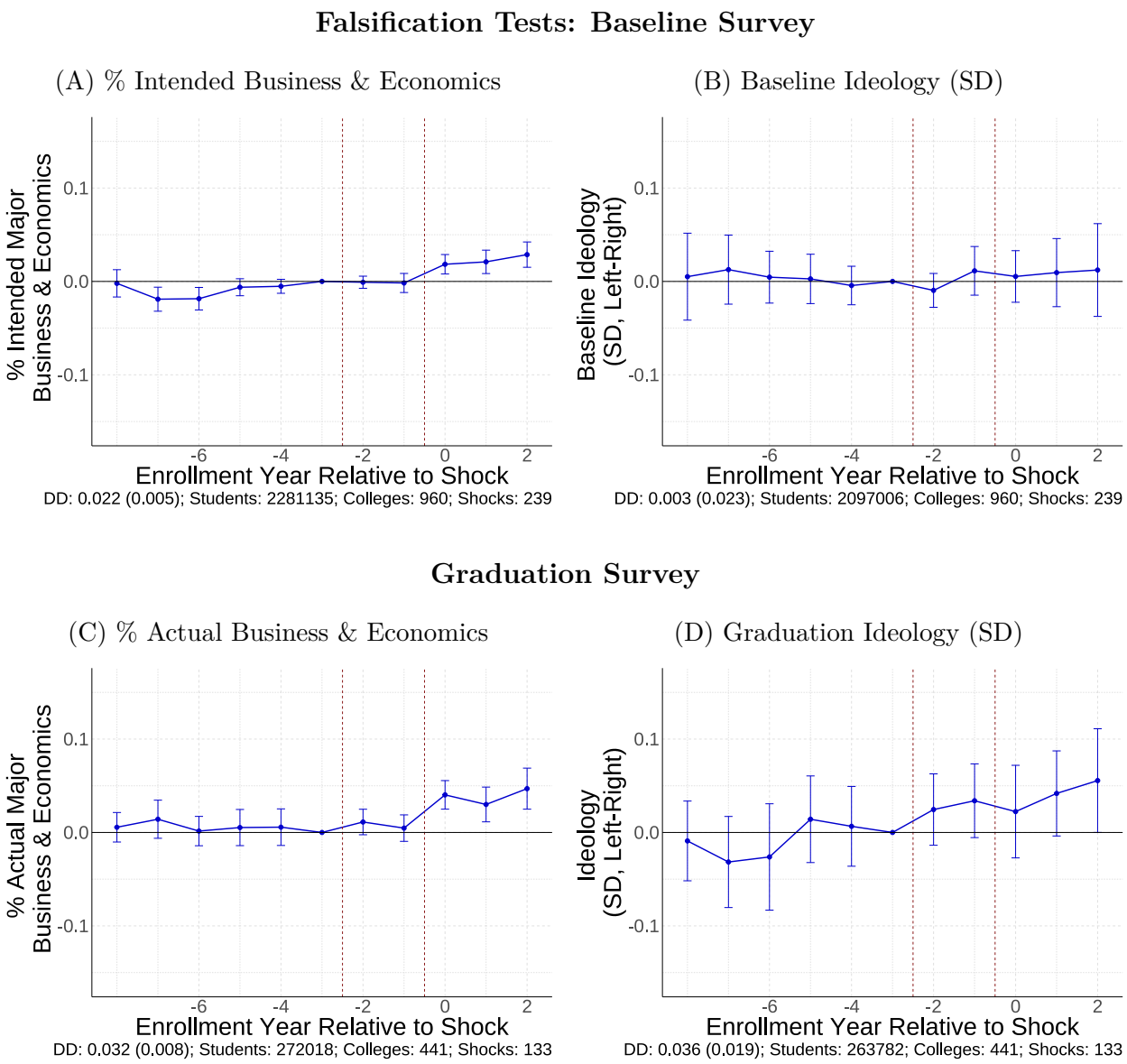
We focus on two variables that proxy for potential outcomes and treatment: baseline ideology and intended major. For baseline ideology, the event-study estimates show no pre-trend and no post-shock change in the ideology of incoming students. For intended major, we again find no pre-trend, ruling out significant increases in preferences for business or economics immediately before the shock. This supports the interpretation that the shocks are driven by program supply rather than by prior changes in student demand. After the shock, we observe an increase of about 2 pp in intended business and economics majors, consistent with students responding to expanded program offerings.

However, the increase in actually majoring in business or economics is even larger, suggesting that part of the increase appear in the first week of college and part only during college. Panel C of Figure 4 shows that following a shock, the probability of majoring in business/economics increases by about three pp. This effect is statistically significant and indicates a meaningful increase in exposure to these fields. Columns (1)–(5) of Panel A in Table G.7 report the corresponding effects on other major groups. The estimates are negative for several groups, but statistically significant only for engineering and mathematics. Column (6) reports the effect on the share of students who ever take a course in humanities or arts. Following a shock, students are 2.4 pp less likely to take such a course, suggesting that the expansion affects not only realized majors but also coursework outside the major.

Finally, Panel D of Figure 4 shows that graduation ideology shifts to the right by about 0.04 SD, with no visible pre-trend before the shock. The estimate is only marginally significant,

[13] In these falsification tests, we omit the control for the lagged outcome because the lagged outcome itself is used as the dependent variable.

Figure 4: Effects of College-Level Expansions in Economics & Business



Note: These figures plot event-study coefficients estimated from Equation 3, with standard errors clustered at the college level. Each panel also reports the pooled post-shock difference-in-differences estimate (DD), along with the number of students, colleges, and treated colleges in the estimation sample. The omitted time category is three periods before the event. We exclude the two following cohorts from the DD estimation because they may already be partially exposed to the shock. Panels A and B use baseline-survey outcomes and serve as falsification tests: intended business/economics major and baseline ideology. Panels C and D use graduation-survey outcomes: realized business/economics major and graduation ideology. Panels A—C are estimated without additional controls, while Panel D flexibly controls for baseline ideology.

and suggests that shocks to field availability affect students' political ideology.[14] Because the shocks also change coursework outside the major, and because we observe only limited information on such coursework, we could not estimate an IV estimate of the effect of major choice alone.[15] Nevertheless, the reduced-form evidence reinforces the main causal argument: exposure to fields of study shapes political outcomes. The estimated reduced-form effects are non-negligible, indicating that changes in field availability can have political consequences.

Because shock timing varies across colleges, the design is a staggered difference-in-differences design. Recent work shows that TWFE regressions can be biased in this setting when treatment effects are heterogeneous (surveyed in Roth et al., 2023). To address this concern, we also report results using the imputation estimator (Gardner, 2022; Borusyak et al., 2024). This estimator first uses untreated observations to estimate the fixed effects that would have prevailed absent treatment, and then estimates treatment effects using outcomes residualized by these predicted fixed effects. Figure G.8 reports the main event-study estimates using the imputation estimator. The estimated effects are similar to the TWFE estimates, suggesting that treatment-effect heterogeneity in the staggered design is unlikely to drive the results. The estimates are also slightly more precise.

3.4 Broader Impacts of Academic Fields

Policy positions. We examine how college majors shape students' worldviews, as reflected in their positions on specific policy issues. We estimate equation 1 using various policy positions—standardized to have mean zero and unit variance—as outcome variables.

Table 3 reports the results. Compared with the natural sciences, the social sciences and humanities consistently move students toward more liberal positions across nearly all policy domains. Students become more supportive of abortion rights, abolition of the death penalty, higher taxes, national healthcare, gun control, and marijuana legalization. They become less supportive of anti-LGBTQ statements, claims denying racial discrimination, and tough-on-crime policies. They are also less likely to agree that "an individual can do little to bring about change in our society," suggesting a stronger sense of individual agency in social change. A notable exception is climate change: students in the natural sciences are more likely than those in the social sciences and humanities to view climate change as a federal priority.

[14] Columns (1)–(3) of Panel B in Table G.7 report results for binary outcomes, which suggest a shift away from liberal and far-left and toward conservative and far-right, although these estimates are noisier.

[15] To further support this interpretation, we report results separately for students with high/low predicted probabilities of majoring in business/economics. Table G.8 shows that the effect on major choice is much larger for the high group, while changes in coursework occur in both. Ideology also shifts in both groups, consistent with the interpretation that the ideology is affected by coursework rather than only realized majors.

Table 3: Effects of College Majors on Policy Positions

	(1) Abortion	(2) LGBTQ	(3) Death	(4) Speech	(5) Race	(6) Change	(7) Taxes	(8) Healthcare	(9) Guns	(10) Criminal	(11) Marijuana	(12) Climate	(13) Index
Business &	-0.04***	0.07***	-0.04**	-0.01	0.07***	0.02	-0.17***	-0.18***	-0.02	0.03*	0.01	-0.20***	0.14***
Economics	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Engineering &	-0.05***	0.02	-0.01	-0.03**	0.08***	0.05***	-0.08***	-0.09***	-0.05**	0.04*	0.01	-0.10***	0.07***
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Health &	-0.11***	0.05***	-0.04***	0.02	-0.04***	-0.05***	-0.02	0.02	0.06***	0.03*	-0.10***	-0.20***	0.09***
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Social	0.05***	-0.06***	0.09***	0.03***	-0.12***	-0.09***	0.05***	0.07***	0.04***	-0.13***	0.09***	-0.08***	-0.12***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Humanities &	0.03**	-0.08***	0.12***	0.01	-0.10***	-0.04***	0.04***	0.05***	0.05**	-0.10***	0.09***	-0.10***	-0.11***
Arts	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
N	267431	201563	218858	243749	279207	216958	205274	158709	161592	151713	236802	63137	303463
Base Change	0.25	-0.3	0.17	-0.2	-0.16	0.12	0.04	0.08	-0.04	-0.26	0.4	0.47	-0.21

Notes: This table shows the estimates for δ_m from equation 1 and their standard errors, clustered at the college level. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. Responses are originally coded in 1-4 scale (1=Disagree Strongly, 2=Disagree Somewhat, 3=Agree Somewhat, 4=Agree Strongly) and are transformed into measure in standard deviations. The specific policy issues are: Abortion should be legal; It is important to have laws prohibiting homosexual relationships; The death penalty should be abolished; Colleges have the right to ban extreme speakers from campus; Racial discrimination is no longer a major problem in America; Realistically, an individual can do little to bring about changes in our society; Wealthy people should pay a larger share of taxes than they do now; A national health care plan is needed to cover everybody's medical costs; The federal government should do more to control the sale of handguns; There is too much concern in the courts for the rights of criminals; Marijuana should be legalized; Addressing global climate change should be a federal priority; Index is a normalized average of the above 12 items. Base Change represent the average change in the outcome during college for students in the omitted group of majors (natural sciences). *** p<0.01, ** p<0.05, * p<0.1

In contrast, business and economics majors move students toward more conservative positions relative to the natural sciences. The effects of college major are much larger for economic policy items than for most cultural issues. For example, the gap between business/economics and humanities/social sciences reaches 0.22–0.25 SD for positions on healthcare and taxation, but only about 0.07 SD for positions on abortion rights or gun control.

We next examine how the effects on political ideology align with changes in specific policy positions. To do so, we predict each student's ideology based on their responses to all questions in Table 3. These predicted values reflect how closely each student's policy positions correspond to a liberal or left-leaning outlook. The index estimates in Table 3 imply that moving from business or economics to other social sciences or humanities shifts this policy-position index by about 0.26 and 0.25 SD, respectively, suggesting that changes in policy positions closely align with the overall ideological shift.[16] [17]

Behaviors and life goals. Ideology encompasses a broad set of beliefs and values that shape how individuals interpret the world and make decisions throughout their lives. We therefore examine whether the influence extends to behaviors and life goals. We estimate equation 1 using survey responses that capture behavioral and goal-related outcomes.

We first examine students' prioritization of financial success. Table 4 presents the results: economics and business lead students to place a much higher priority on financial success relative to all fields. The estimated effect is 0.32 SD relative to natural sciences, and the effects are also much larger than those of other high-return majors such as engineering.

We also turn to other life goals. In terms of prosocial orientations, social sciences' students place a much higher priority on social goals, with smaller but still significant effects for students in the humanities and in health and education. Similarly, students in the social sciences and in health and education are more likely to prioritize helping others, whereas students in economics and business, as well as in engineering, assign lower importance to this goal. Humanities and social sciences students also place greater emphasis on staying informed about political events. By contrast, students in health and education, and in business and economics, place greater emphasis on family formation, while humanities students place less emphasis on this domain. Instead, humanities students are more likely to prioritize meaning.

[16] Column (4) of Table G.5 and Table G.7 report estimates for the policy-position index using complementary approaches. The estimates are positive, though noisy in the difference-in-differences specification.

[17] Appendix E presents a decomposition exercise that assesses how much changes in policy positions can account for the overall ideological effects of majors. The results suggest that changes in economic positions explain a substantial share of the ideological effect of business and economics relative to the natural sciences, while changes in cultural positions explain a substantial share of the relative effect of the humanities and social sciences.

Table 4: Effects of College Majors on Life Goals and Behaviors

	(1) Well Off	(2) Social	(3) Helping	(4) Politics	(5) Family	(6) Meaning	(7) Demonstrated	(8) Discussed Course	(9) Discussed Politics	(10) Vote Reg.
Business &	0.32***	0.08***	-0.09***	0.14***	0.07***	-0.05***	-0.04*	-0.16***	0.03**	-0.01
Economics	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Engineering &	0.16***	-0.06***	-0.14***	-0.03	-0.02	-0.07***	-0.06***	-0.01	-0.06***	-0.02***
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Health &	0.02	0.17***	0.08***	0.02	0.10***	0.01	0.01	-0.03*	-0.05***	-0.00
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Social	0.08***	0.34***	0.09***	0.25***	0.02**	0.03**	0.06***	-0.14***	0.24***	0.01**
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Humanities &	-0.03*	0.22***	-0.02*	0.17***	-0.06***	0.15***	0.08***	-0.07***	0.19***	0.01*
Arts	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	310054	309786	309844	292282	310027	309649	164559	317099	281720	91046
Base Change	-0.29	0.11	0.1	0.16	0.05	0.22	-0.49	NA	-0.29	NA

Notes: This table reports estimates for δ_m from equation 1 and their standard errors, clustered at the college level. Coefficients are effects of each major relative to natural sciences. The sample includes students with non-missing major and outcome. Outcomes in columns (1)-(6) are based on students' stated life goals: being well off financially (Well Off), influencing social values (Social), helping others in difficulty (Helping), keeping up-to-date with political affairs (Politics), raising a family (Family), and developing a meaningful philosophy of life (Meaning),. All goals are originally coded in 1-4 scale (1=Not important, 2=Somewhat important, 3=Very important, 4=Essential) and are transformed into measure in standard deviations. Outcomes in columns (7)-(10) are based on students' behaviors: Participated in organized demonstrations, discussed course content with students outside of class, discussed politics, and vote registration. Responses are originally coded in 1-3 scale (1=Not at all, 2=Occasionally, 3=Frequently) and are transformed into measure in standard deviations. The specific behaviors are are: Base Change represent the average change in the outcome during college for students in the omitted group of majors (natural sciences). *** p<0.01, ** p<0.05, * p<0.1

We then analyze behaviors. Compared to natural sciences majors, students in the social sciences and the humanities and arts are more likely to participate in demonstrations, consistent with higher political activism, whereas students in business and economics are less likely to do so. Moreover, humanities and social sciences students report discussing their coursework less frequently, and discussing politics more often—again reflecting greater civic engagement. Effects on voter registration are small in magnitude: social sciences and humanities students are about one pp more likely to be registered to vote (only a marginally significant estimate for the latter), while engineering students are about two pp less likely. However, this variation is limited, as roughly 90 percent of students in the sample reported being registered to vote.

4 Mechanisms of the Impacts

The results in the previous section show that academic fields substantially shape students' political attitudes. This section explores the mechanisms underlying these effects. We test three channels: peer socialization, earnings expectations, and the intellectual environments.

4.1 Peer Ideology Composition

Students in different majors are exposed to peers with varying political ideologies. Thus, we investigate whether peer effects could explain the effects of majors. To avoid reflection issues (Manski, 1993), the analysis focuses on the effects of peers' pre-college ideology.

Throughout the analysis, peers are defined as students who enrolled in the same college in the same year and intended to pursue the same major. We define peers based on intended majors rather than actual majors for two reasons. First, since students typically finalize their majors in their second or third year, using intended majors avoids potential endogeneity from early college friendships influencing final major choices. Second, the intended major data from the baseline survey covers a much larger sample than the actual major data from graduation, reducing measurement error and increasing precision. Moreover, many students with the same intended major eventually choose the same actual major (see Figure G.2A), and even those who do not are likely to share similar early course experiences.

Causal identification relies on exploiting idiosyncratic variations in the composition of peers within the same program (college by major) over time. To achieve this, we examine the relationship between a student's graduation ideology and the average baseline ideology of their peers, controlling for fixed effects at the college-by-major, college-by-year, and major-by-year

levels. In some specifications, we also include a control for the average ideology of potential peers—students in the same college and major from adjacent cohorts (two years ahead and two years behind). This control allows us to interpret the estimates as comparing the effects of actual peers versus similar potential peers.[18] Formally, we estimate:

$$p_{icmt} = \sigma P^{-i}_{cmt} + \beta \Phi^{-i}_{cmt} + \nu_{cm} + \iota_{ct} + \omega_{mt} + \varepsilon_{icmt} \tag{4}$$

Where p_{icmt} is political graduation ideology of student i studying at college c, with intended major m, starting their studies at year t; P^{-i}_{cmt} represents the average baseline political ideology of the student's peers (in SD); Φ^{-i}_{cmt} captures the average ideology of students in the same college and intended major across the five-year window ($t' \in \{t-2, t-1, t, t+1, t+2\}$; ν_{cm}, ι_{ct}, and ω_{mt} denote college-major, college-cohort, and major-cohort fixed effects, respectively. Identification of this specification relies on the assumption that potential graduation ideology is orthogonal to peers' average baseline ideology, conditional on the controls. This assumption yields a testable implication: a student's baseline ideology should be uncorrelated with their peers' ideology after conditioning on our controls. We empirically validate this below.

Measuring peer baseline ideology based on a subsample. A key challenge in our setting is that only a subset of peers is observed. Although this subsample is representative, using the observed peers' average ideology may introduce attenuation bias by incorrectly assuming that the observed and unobserved peers share similar idiosyncratic ideology components. To address this, we construct an adjusted treatment measure that predicts the unobserved peers' ideology based on Equation 4. Specifically:

$$P^{-i}_{cmt} = \alpha_{ct} P^{-i,O}_{cmt} + (1 - \alpha_{ct}) \widehat{P^{-i}_{cmt}} \tag{5}$$

Where $P^{-i,O}_{cmt}$ is the average baseline ideology of observed peers, α_{ct} is the share of students in college c and enrollment year t who are covered in the baseline survey,[19] and $\widehat{P^{-i}_{cmt}}$ is the predicted baseline ideology of students in college c and enrollment year t, with intended major m, based on all elements included in equation 4, except for P itself. Our control for potential peers' baseline ideology (Φ^{-i}_{cmt}) is also adjusted using the predicted value for the

[18] This control for potential peers follows Guryan et al. (2009) and can be viewed as implementing a recentering strategy (Borusyak and Hull, 2023).

[19] We calculate the coverage as the number of surveyed students in college c in year t divided by the number of total first-year BA enrollment based on the IPEDS administrative data.

unobserved potential peers. Under the assumption that peers' ideologies are uncorrelated conditional on controls, this constructed measure allows identification of peer effects and avoids attenuation bias. Appendix F formally derives this argument. In the main analysis, the sample is restricted to college cohorts, where the baseline survey includes at least five peers who account for at least 10 percent of enrolled students. The results are not sensitive to choosing other thresholds for sample inclusion.

Avoiding exclusion bias. Estimating peer effects in specifications that include fixed effects presents a unique challenge of exclusion bias (Guryan et al., 2009; Angrist, 2014; Caeyers and Fafchamps, 2024). The exclusion bias arises because individuals are mechanically excluded from their own peer group, creating a negative correlation between individual outcomes and peer averages in specifications that include fixed effects. For individuals in the same college-year-major cell, when an individual is above (below) the pool average, their exclusion from their peer group necessarily lowers (raises) the peer average, inducing a negative correlation. A split-sample method addresses this bias by dividing the data into separate groups for measuring outcomes and defining peer ideology (Stevenson, 2017). Our analysis builds on this approach by implementing a four-way split.[20]

Falsification tests. Table G.9 presents falsification tests, which regress students' baseline ideology on their peers' average baseline ideology using equation 4. Panel A uses the full baseline survey sample, and Panel B focuses on students who are also included in the graduation survey. We report both unadjusted and adjusted estimates, but prefer the latter, which are less likely to be attenuated. In relevant specifications, the estimated coefficients are tiny and statistically insignificant in both samples. The large sample sizes allow rejection of a positive coefficient exceeding 0.013 in the fully controlled specification using the full baseline sample. This precise null result confirms that the approach successfully isolates quasi-random variation in peers' ideology that is uncorrelated with students' own ideology.

Estimated peer effects. Table 5 presents the main estimates. The table shows that while peers' baseline ideology strongly predicts each other's graduation ideology (column 1), this relationship disappears when accounting for selection into similar programs. The estimates are small and insignificant across many different specifications. Moreover, the large sample size enables ruling out effects of about 0.017 SD or larger in the preferred specification.

[20]Specifically, it randomly divides the graduation sample into quarters, using each quarter in turn as the outcome group while constructing peer measures from the remaining three quarters combined with baseline survey observations not included in the graduation sample. The reported coefficients average the estimates from these four iterations, by including quarter dummies interacted with all variables except the treatment.

Table 5: Estimated Effects of Peer Ideology Composition

Panel A. All Majors, Various Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.747***	0.002	0.006	-0.007	-0.010	-0.010
	(0.023)	(0.011)	(0.012)	(0.012)	(0.010)	(0.010)
Estimate (Adjusted)	1.207***	0.007	0.013	-0.007	-0.017	-0.015
	(0.040)	(0.018)	(0.023)	(0.019)	(0.016)	(0.016)
N	229,660	229,660	229,660	229,660	229,660	229,660
Fixed Effects	No	Yes	No	Yes	Yes	Yes
Potential Peers' Average	No	No	Yes	Yes	Yes	Yes
Major Predictors	No	No	No	No	Yes	Yes
Outcome Predictors	No	No	No	No	No	Yes

Panel B. By Major Group

	(1) Economics & Business	(2) Engineering & Math	(3) Health & Education	(4) Humanities & Arts	(5) Natural Sciences	(6) Social Sciences
Estimate	-0.013	-0.021	-0.004	-0.010	-0.033	-0.014
	(0.024)	(0.048)	(0.028)	(0.034)	(0.048)	(0.032)
Estimate (Adjusted)	-0.032	-0.018	-0.011	-0.033	-0.049	-0.019
	(0.041)	(0.071)	(0.045)	(0.053)	(0.071)	(0.054)
N	47,124	23,154	56,083	29,600	27,637	46,062
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Potential Peers' Average	Yes	Yes	Yes	Yes	Yes	Yes
Major Predictors	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Predictors	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents estimates of peer effects on students' graduation political ideology based on equation 4. The treatment is the average baseline-survey political ideology of peers in the same college-by-entry-cohort-by-intended-major group. Entries report coefficient estimates with college-clustered standard errors shown below each estimate. Adjusted estimates correct attenuation from incomplete peer coverage using the split-sample correction in the analysis. Panel A reports various specifications. Panel B reports estimates from the fully controlled specification, for subsamples defined by students' intended majors. "Potential Peers' Average" indicates controls for the average ideology of potential peers from adjacent cohorts. "Fixed Effects" indicates college-major, college-cohort, and major-cohort fixed effects. "Major Predictors" indicates controls related to major choice, including intended majors, careers, and life goals. "Outcome Predictors" indicates baseline ideology and policy positions. The sample is restricted to students with non-missing intended major and graduation political ideology, at least 5 observed peers, and peer coverage of at least 10% of the cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A back-of-the-envelope calculation indicates that peer effects could not account for more than a tiny fraction of the estimated major effects. The baseline difference in ideology between business and economics and those in social sciences and humanities majors is 0.351 SD. With an upper-bound peer effect estimate of 0.017 SD, the maximum peer effect from switching between these majors equals 0.006 SD (0.017×0.351). Since only 37 percent of same-intended-major pairs ultimately choose the same realized major, a more conservative estimate is about 0.016 SD (0.006 / 0.37). This is roughly 7 percent of the estimated 0.23-standard-deviation effect of choosing business or economics over social sciences or humanities.

Panel B of Table 5 examines heterogeneity across major groups and finds no significant effects for any group. Appendix Table G.10 shows that this result is robust to alternative specifications, including measuring peers' ideology using an indicator for liberal identification or the policy-position index. Across all specifications, we find no evidence of peer effects.

4.2 Earnings Expectations and Self-Interest

Another potential mechanism is students' earnings expectations. A large literature documents that the economic returns to college differ substantially across fields (e.g., Altonji et al., 2016; Bleemer and Mehta, 2022; Andrews et al., 2024), and students are likely aware—at least partly—of these differences. Upon graduation, students may hold part-time jobs or job offers that shape their expectations. Consequently, future earnings may influence attitudes through self-interest: students in higher-earning fields such as engineering and economics may be more likely to favor policies that benefit them financially, such as lower income taxes.

We first examine how the estimated ideological effects of college majors vary with expected wages, proxied by the average wages of graduates from each college–major group. Using data from the College Scoreboard, we compute average post-graduation earnings by major and institution, measured 4 or 5 years after graduation.[21] Figure G.10 presents correlations between expected wages and changes in ideology during college. Panel A shows that majors associated with higher expected wages are also those with a rightward political shift. However, Panel B shows that this disappears when focusing on within-major variation.

To examine this more rigorously, we re-estimate equation 1 separately for students in colleges with above- and below-median expected earnings within each major group. Results

[21]We use program-level earnings data from the U.S. Department of Education's College Scorecard, which reports median wages of federally aided students by major and institution. In the 2018-19 Scorecard release, earnings were measured four years after graduation, while the 2019- 20 release introduced a five-year measure; no college-major cell is observed with both four- and five-year outcomes. To place all programs on a comparable five-year scale, we adjust log four-year earnings by the typical difference between log wages at four and five years, calculated within the same major cohort. The measures are based solely on students graduating in 2018 and 2019.

are presented in Figure 5. The estimated ideological effects of each major show little variation across these two groups. Panel A focuses on students' ideology at graduation. The only significant difference appears in health and education, where students in the higher–expected-wage group shift less to the right—opposite to what a self-interest channel would predict. Panel B examines support for higher taxes on the wealthy, which also yields small and statistically insignificant differences.

As a complementary analysis, we further examine how the estimated effects of college majors evolve over time. During the sample period, two major financial crises, the early-2000s dot-com bust and the 2008 financial crisis, disproportionately affected specific fields. If earnings expectations influence attitudes, we would expect, for instance, computer science students to become more supportive of higher taxes after the dot-com crash, and finance students to do so following the 2008 crisis. To test this, we estimate Equation 1 separately for cohorts entering college in the same year, using support for higher taxation as the outcome.

Figure G.11 presents the results, showing no evidence of differential changes in tax positions corresponding to these shocks. For example, the effects of engineering are similar for students enrolled between 1996 and 1998 and for earlier cohorts (1993–1995). Likewise, business and economics students enrolled before the 2008 crisis show effects comparable to those of students graduating during the recession, despite being most directly exposed to its consequences.

4.3 Departmental Ideology

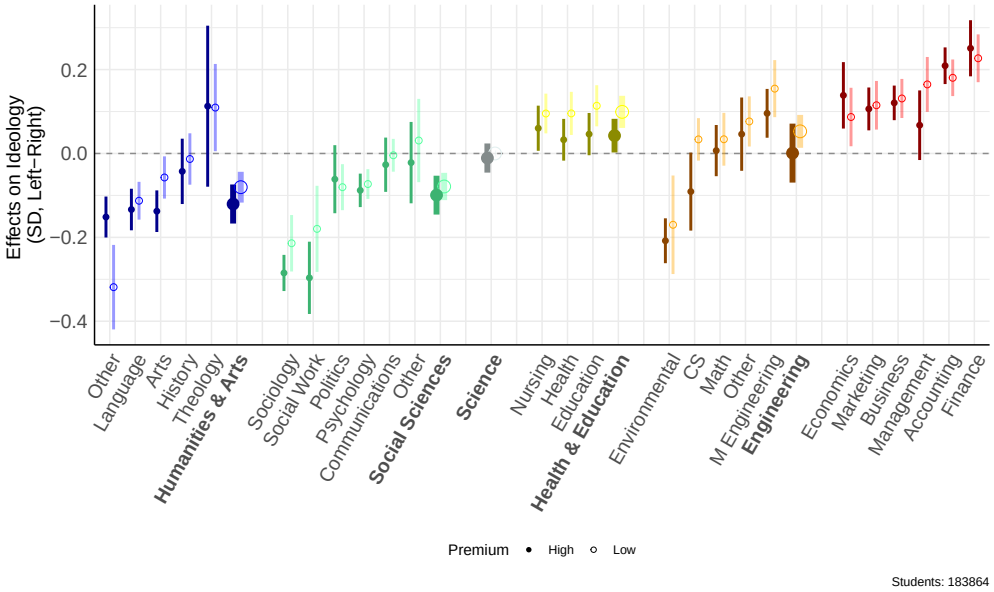
This subsection uses departmental ideological leaning as a proxy for variation in how the same major is taught across colleges. We use faculty political ideology data from the HERI surveys, conducted every three years since 1989. Figure G.12 shows time trends in faculty ideology by major group. Faculty across all fields have become increasingly liberal over time, while faculty in the humanities and social sciences are consistently more liberal than others.

We summarize the data at the department level, measuring the average ideology of faculty within each department (college by major) across all available survey years. We adopt a time-invariant measure for three reasons: (1) the faculty survey covers only a subset of years and institutions, making time-varying estimates infeasible; (2) faculty typically remain in the same institution for long periods (average tenure of 10.8 years in the survey), limiting within-institution variation; and (3) departing faculty may continue to shape curricula, making their ideology relevant beyond their tenure.

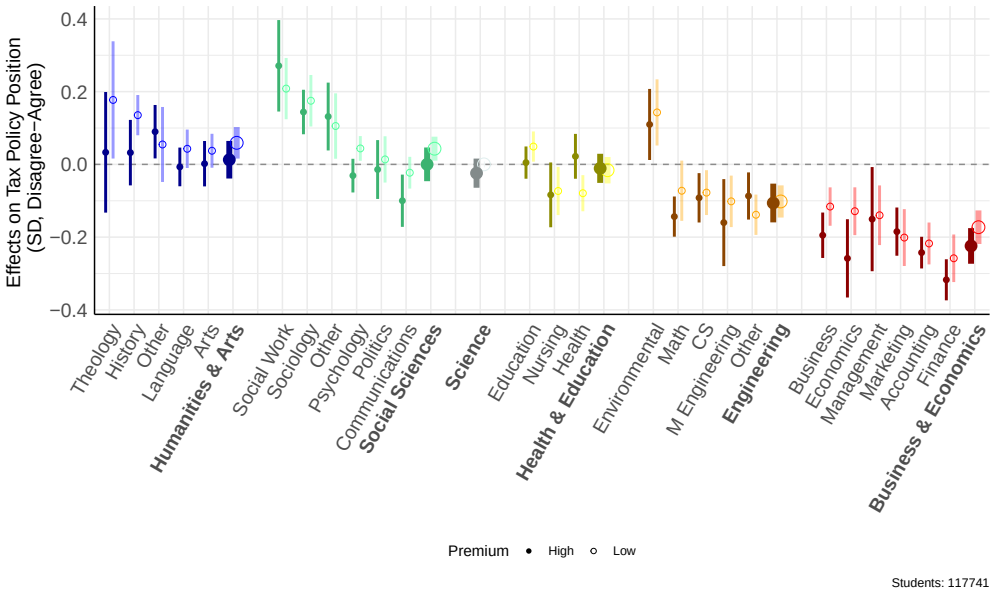
Next, we match this faculty ideology measure to students based on their college and intended majors. We match using intended rather than actual major, to avoid endogeneity

Figure 5: Heterogeneity of the Effects of College Majors, by Earnings Expectations

(A) Political Ideology



(B) Tax Policy Position



Note: This figure plots the estimates for δ_m from equation 1 and their 95 percent confidence intervals. The bold lines represent coefficients from a model that groups majors into six broad categories. The thinner lines show coefficients from a more detailed model with specific major categories. Low (high) earnings expectations are defined for each college-major combination as earnings below (above) the median across all colleges offering the same major. Effects are estimated for each major interacted with earnings-expectation indicators. In both the broad and detailed models, natural sciences with low earnings expectations is the omitted reference group and is therefore plotted at zero by construction. The detailed plot excludes electronic engineering because one earnings-expectation cell is too sparse to estimate reliably. The outcome variables are graduation political ideology and support for increasing taxes on the wealthy. Estimates are presented in standard deviation units. Both models control flexibly for students' baseline political ideology, policy positions, major preferences, life goals, and the full set of main controls (see Table G.4).

from students self-selecting into majors due to faculty ideology, consistent with our approach for peers. We then estimate the following model:

$$p_{icmt} = \tau F_{cm} + \eta_{tm} + \psi_{ctM} + \zeta_{icmt} \tag{6}$$

where p_{icm} is the graduation political ideology of student i at college c with intended major m and starting their studies at year t; F_{cm} is the average faculty ideology (in SD) in major m at college c; η_{tm} and ψ_{ctM} are major by cohort and college by cohort by aggregated-major-group fixed effects, respectively. Faculty ideology is only observed for a subsample within each department, so we adjust our measure using methods analogous to those used for peers, as elaborated in Appendix F, which reduces concerns about attenuation bias.

We assume that students' potential ideology is orthogonal to faculty ideology, conditional on controls. Under this assumption, the coefficient on τ captures the effect of studying the same major under more right-leaning faculty. This effect may operate through several channels, including exposure to academic content, teaching methods, or faculty interactions. A key threat arises if students sort into specific majors differently by ideology in colleges where faculty themselves differ ideologically. Since we include college-by-cohort-by-major-group fixed effects, such sorting would have to occur within the same college and major group.

Falsification tests. To test this, we examine whether students' baseline ideology is correlated with faculty ideology, conditional on the fixed effects included in Equation 6. Table G.11 reports both unadjusted and adjusted estimates. We focus on the adjusted estimates, which are less sensitive to attenuation bias. We use three samples: the full baseline survey, cohorts observed in the graduation survey, and students included in the graduation survey. The raw correlations between faculty ideology and students' ideology are large in all three samples, but these correlations largely reflect selection into similar programs. Once we include the fixed effects and predictors of major choice, the correlations become much smaller.

In the full baseline survey sample, we still find a small, statistically significant positive coefficient, even after conditioning on the fixed effects. This suggests some degree of differential selection. However, this relationship becomes insignificant when we restrict the sample to colleges and cohorts observed in the graduation survey, and remains so when we restrict the sample to students included in the graduation survey. This pattern may reflect either heterogeneous selection across colleges or reduced statistical power in the smaller samples. Importantly, we directly control for students' baseline ideology and other pre-college characteristics, further mitigating concerns about residual selection bias.

Estimated effects. Table 6 shows that the effects of faculty ideology are meaningful. The adjusted estimates are about 0.065 SD, implying that students studying the same major in departments with faculty who are one standard deviation more left-leaning become about 0.065 SD more left-leaning by graduation. As we sequentially add controls, the estimated magnitudes remain stable while precision slightly improves. Moreover, when restricting the sample to departments with larger coverage in the faculty survey, the results remain significant and increase to 0.10, confirming their robustness.

Panel B of Table 6 reports results by major group. The estimates show no clear effects in most fields, with the exception of the humanities and social sciences. In these fields, a one-standard-deviation increase in faculty liberalism shifts students' ideology leftward by about 0.14 and 0.13 SD, respectively. These magnitudes suggest that variation in faculty ideology across departments could explain much of the estimated difference between fields. The average gap in our faculty-ideology measure between students who intended to major in business or economics and those who intended to major in the social sciences or humanities is about 1.2 SD. The estimates imply that studying the same major with faculty who are 1.2 SD less left-wing is associated with a 0.16–0.17 SD rightward shift in graduation ideology. This accounts for most of the ideological gap between social sciences or humanities and business or economics. Because faculty ideology is only a proxy for broader features of the intellectual environment, including course content and teaching, this evidence suggests that these channels play a central role in shaping students' political ideology.[22]

Together, the evidence in this section indicates that the effects of fields of study on students' ideology are not driven by socialization or economic returns. Instead, they appear to stem from differences in the intellectual environments across majors. The effects are stronger in departments with more pronounced ideological orientations, suggesting that course content, teaching practices, and faculty interactions are key channels. Future work using direct measures of these dimensions could further clarify their respective roles.

[22]Moreover, as shown in Figure G.12, the ideological gap between faculty in economics or business and faculty in the humanities or social sciences has slightly widened over time. This is consistent with the modestly growing effects of college majors on ideology documented earlier in Table C.5.

Table 6: Effects of Faculty Ideology on Students' Ideology

Panel A. Various Specifications, All Majors

	(1)	(2)	(3)	(4)	(5)
Estimate	0.022*	0.023**	0.024**	0.032***	0.065***
	(0.012)	(0.012)	(0.011)	(0.012)	(0.021)
Estimate (Adjusted)	0.064**	0.065***	0.066***	0.072***	0.100***
	(0.026)	(0.025)	(0.024)	(0.025)	(0.032)
N	161,710	161,710	161,710	120,676	37,341
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Major Predictors	No	Yes	Yes	Yes	Yes
Outcome Predictors	No	No	Yes	Yes	Yes
Min Share Faculty	0.1	0.1	0.1	0.3	0.5

Panel B. By Major-Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Economics & Business	Engineering & Math	Health & Education	Humanities & Arts	Natural Sciences	Social Sciences
Estimate	0.021	0.001	0.009	0.067***	-0.061*	0.051***
	(0.022)	(0.021)	(0.025)	(0.026)	(0.037)	(0.019)
Estimate (Adjusted)	0.045	0.005	0.032	0.142***	-0.094	0.133***
	(0.054)	(0.056)	(0.058)	(0.052)	(0.081)	(0.040)
N	26,794	21,671	27,055	28,293	19,848	37,857
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Major Predictors	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Predictors	Yes	Yes	Yes	Yes	Yes	Yes
Min Share Faculty	0.1	0.1	0.1	0.1	0.1	0.1

Notes: This table presents estimates of the effects of faculty ideology on students' political ideology based on equation 6. The outcome is graduation political ideology and the treatment is department-level faculty ideology matched to each student's college and intended major. Panel A reports the results in various specifications. Panel B reports estimates from the fully controlled specification, for subsamples defined by students' intended majors. Adjusted estimates correct for attenuation from incomplete faculty coverage. "Fixed Effects" indicates specifications that include major-by-cohort and college-by-major-group-by-cohort fixed effects. "Major Predictors" indicates additional predictors related to major choice, including intended majors, careers, and life goals. "Outcome Predictors" indicates baseline ideology and policy positions. All specifications are estimated on students with non-missing intended major and political ideology, at least 3 observed faculty, and minimum faculty coverage of at least 10/30/50% of the department. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5 Implications

Academic specialization is a central characteristic of college education, enabling students to tailor their college experience by choosing their field of study. The results in previous sections suggest that such specialization carries important implications for politics. In this section, we quantify several key consequences—focusing on voting and political fragmentation.

5.1 Voting

Although our analysis focuses on ideological self-identification, the effects are likely to have electoral implications. To approximate their magnitude, we use voting patterns among students in the CCES and ANES, which include similarly worded ideology questions. Our estimates imply that majoring in the humanities or social sciences increases the probability of identifying as liberal by more than 10 percentage points relative to majoring in business or economics. This shift comes from a 6.7-percentage-point decline in conservatism and a 3.8-percentage-point decline in moderate identification. Based on the voting patterns in Appendix Figure G.3, this implies an approximately 6.3-percentage-point increase in the probability of voting Democratic ($0.8 \times 6.7 + 0.25 \times 3.8 \approx 6.3$).[23]

These magnitudes suggest that academic specialization is one of the most powerful experiences shaping political attitudes. For context, Brown et al. (2023) find that each additional year spent in a county with a one pp higher Democratic vote share during ages 13–19 increases the likelihood of voting Democratic by 0.05 pp. Thus, the estimated effect of majoring in social sciences or humanities (versus business or economics) is equivalent to the impact of living for six years in a county with about 20 pp higher Democratic support.

5.2 Fragmentation

Our findings have implications for polarization and political differences between groups. First, they suggest that the widely discussed "education divide" is partly driven by the specific fields students study in college. We illustrate this through a counterfactual simulation shown in Panels A and B of Table 7. Panel A focuses on ideological change during college. Students in our sample enter college with an average ideology score of 2.999 and graduate with an average of 2.872, indicating a leftward shift of 0.128 points (1–5 scale). However, if all students majored in economics or business, this change would shrink to 0.011.

[23] Because we also document small positive effects on voter registration, students in the humanities and social sciences are slightly more likely to be registered; the effect on actual voting may therefore be somewhat larger.

Table 7: Field Specialization and Politics: Counterfactuals

A. Liberalization	
Freshman mean	2.999
Senior mean	2.872
Change: Senior − Freshman	-0.128
Counterfactual Change (All Business & Economics)	-0.011
B. College Divide	
Gap (CCES)	-0.406
Counterfactual Gap (All Business & Economics)	-0.289
C. Gender Divide	
Freshman Gap	0.160
Senior Gap	0.202
Counterfactual Gap (Uniform Major)	0.169
D. Polarization	
Freshman Variance	0.661
Senior variance	0.722
Counterfactual Variance (Uniform Major)	0.694
N	294,571

Note: This table reports counterfactual quantifications based on our main estimates of the effects of college majors on students' political ideology. Panel A shows freshman and senior means in ideology (1–5 scale), the senior–freshman gap, and the counterfactual gap if all students had majored in business or economics. Panel B shows the gap between college graduates and non-college individuals in the CCES at ages 24–27, and the counterfactual gap if all students had majored in business or economics. Panel C shows gender gaps at freshman and senior year, as well as the counterfactual gender gap if all students had majored in natural sciences. Panel D shows the variance of ideology at freshman and senior year, and the counterfactual variance if all students had majored in natural sciences.

Consequently, the ideological gap between college graduates and non-graduates would narrow substantially. To quantify this, we use the CCES.[24] At ages 24–27, the ideological gap between college graduates and non-graduates is 0.406. Under the counterfactual where all students

[24]Using the CCES survey, Figure G.7 shows how ideology evolves among young adults differently by their college status. Among non-college young adults is roughly stable between ages 18–30, with a slight shift toward conservatism. In contrast, college students become more liberal during their college years.

majored in economics or business, this gap would decline to 0.289. This counterfactual implies that field specialization accounts for roughly 29 percent of the "education divide".

Furthermore, since students self-select into majors, our findings shed light on political differences within the educated population. As shown in Panel C of Table 7, differential major selection explains about 79 percent of the increase in the gender gap during college (0.033 of 0.042 points) and about 16 percent of the senior gap (0.033 of 0.202 points).

Major selection also amplifies ideological polarization: students who enter college with more liberal ideology are more likely to choose left-leaning majors, so by graduation, ideological differences among students are even larger than at entry. If all students pursued the same field, the variance of graduation ideology would decrease from 0.722 to 0.694. This accounts for about 46 percent of the variance increase during college (from 0.661 to 0.722).

Our results therefore suggest that academic specialization in modern higher education may contribute to civic fragmentation rather than cohesion, in contrast to the role often attributed to earlier stages of education (Gradstein and Justman, 2002; Glaeser et al., 2007).

6 Conclusion

This study shows that academic fields shape the political preferences of American college graduates. Using survey data, and combining selection-on-observables estimates with quasi-experimental analyses, we find that fields of study have substantial effects on ideology. The humanities and social sciences move students to the left relative to the natural sciences, while economics and business move them to the right. These effects are mirrored in specific policy positions, especially on economic issues, and extend to behaviors and life goals.

We also examine the mechanisms behind these effects. The evidence does not support peer socialization, or material self-interest as the main explanations for ideological divergence across fields. Instead, the results point to academic content and teaching—the defining features of each field's intellectual environment—as the primary channels.

These findings have broader political implications. Field choice appears to have meaningful electoral consequences, contributes to the political divide between college graduates and non-graduates, explains a sizable share of the gender gap in political ideology, and increases ideological dispersion among college graduates.

The results speak directly to salient debates about the political role of higher education. Critics often accuse colleges, and certain fields in particular, of ideological bias or indoctrination. In the U.S., such concerns have motivated legislative proposals in several states to

restrict funding for specific academic disciplines.[25] They have also contributed to declining public confidence in higher education among Republicans.[26] Yet these concerns are not new. They echo long-standing anxieties about intellectuals "corrupting the youth," most famously in the trial of Socrates. At the same time, the idea that education shapes political perspectives is central to another tradition, which views education as cultivating moral autonomy, critical reasoning, and citizenship (Plato, 1992; Kant, 2002; Dewey, 1916).

This paper does not aim to resolve that normative debate. Instead, it provides empirical evidence on a central premise underlying it: academic fields can meaningfully shape young adults' political and social perspectives. The results are relevant to both critics and advocates of higher education, and suggest that policies expanding or restricting access to particular fields may have political consequences.

The findings also raise questions for future research. Do colleges expose students to diverse viewpoints within fields? Do they foster critical inquiry and intellectual openness? These questions are especially salient in light of recent evidence by Braghieri (2024), who shows that social-image concerns and norms of political correctness lead some students to conceal their views.

[25] Associated Press (2023), "Mississippi lawmakers consider cutting funding for certain college majors," and Time (2023), "A Florida education bill would ban gender studies and diversity programs at universities."
[26] According to the *Pew Research Center* (2019), the share of Republicans who believe that colleges have a positive impact fell from about 53 percent in 2012 to 33 percent in 2019.

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Appendix A Data Appendix

This appendix describes the construction of the analysis data. The main source is the HERI Cooperative Institutional Research Program, which combines the TFS and CSS. We supplement these data with institutional enrollment records, post-college earnings data, faculty survey responses, and external survey data used for descriptive comparisons.

A.1 Study Sample Construction

The baseline survey sample (TFS) includes 9,744,537 students from 1,385 colleges. Of these, 1,689,587 students from 679 colleges belong to college-cohorts with coverage in the graduation survey (CSS). The full CSS sample includes 769,963 students from 784 colleges. 412,098 of these students are from cohorts covered in the TFS. After restricting to four-year institutions and colleges with sufficient linked observations across cohorts, the eligible CSS sample includes 404,218 students from 498 colleges. Our main study sample further requires students to complete both surveys, leaving 318,371 students from 452 colleges.

Baseline and graduation records are linked using HERI subject identifiers, survey years, and institution identifiers. Some graduation records contain baseline information but are missing the baseline subject identifier. For these cases, we recover the baseline identifier by matching on the available baseline characteristics embedded in the graduation records. This procedure recovers 62,058 graduation records. We also resolve duplicate senior-survey records before constructing the final sample.

A.2 Survey Coverage

Coverage statistics compare HERI survey counts to IPEDS first-year enrollment for college-cohorts observed in both survey systems. The coverage calculation includes 2,727 college-cohorts. In these cohorts, TFS respondents account for 63.7 percent of IPEDS first-year enrollment, while CSS respondents account for 34.5 percent of the corresponding entering cohorts. Among TFS respondents, 29.3 percent are matched to a CSS record. In levels, the coverage cohorts include 1,530,758 TFS respondents, 647,113 CSS respondents, and 367,172 matched CSS-TFS students.

Appendix B Correcting for Contamination Bias

We develop our identification argument within a potential-outcomes framework adapted from Mountjoy and Hickman (2021), who estimate wage returns to colleges by conditioning on admission portfolios (extending the seminal work of Dale and Krueger, 2002). Although our strategy differs in important respects, this framework is useful for clarifying the key assumptions in our setting. With p_{im} denoting the potential ideology at graduation for student i if she were to major in field m, we decompose each potential outcome into:

$$p_{im} = \theta_i + \delta_{im},$$

where θ_i is a student-specific component (major-invariant ideology for student i) and δ_{im} is the (possibly heterogeneous) effect of major m on i's ideology. Let m_i denote the student's realized major, and define indicator $D_{im} = \mathbf{1}\{m_i = m\}$ with $\sum_m D_{im} = 1$. The observed outcome can be written as a mixture of potential outcomes:

$$p_i = \sum_{m \in \mathcal{M}} D_{im} p_{im} = \theta_i + \sum_{m \in \mathcal{M}} D_{im} \delta_{im}.$$

We next project the student-specific component θ_i onto observed pre-college covariates X_i. Let $\mu(x) \equiv \mathbb{E}[\theta_i \mid X_i = x]$ denote the expected major-invariant ideology for students with covariates x. We write: $\theta_i = \mu(X_i) + u_i$, with $\mathbb{E}[u_i \mid X_i] = 0$. Here, u_i represents any residual (unobserved) component of student i's major-invariant ideology that is not explained by the observed covariates. In practice, X is high-dimensional, so we approximate $\mu(.)$ in flexible estimation with indicators for every value of our control variables.

The key identification assumption is that after conditioning on our rich set of controls X_i, the residual student component u_i is orthogonal to the student's major choice. In other words, our observed covariates capture all systematic selection in ideology relevant to major choice. Formally, we assume (A1): $\mathbb{E}[u_i \mid m_i, X_i] = \mathbb{E}[u_i \mid X_i]$. Under this assumption, for any given covariate cell x and for any two majors $m, k \in \mathcal{M}$ that both occur among students with $X_i = x$, the difference in average outcomes between those who chose m and those who chose k identifies the difference in their treatment effects. Formally:

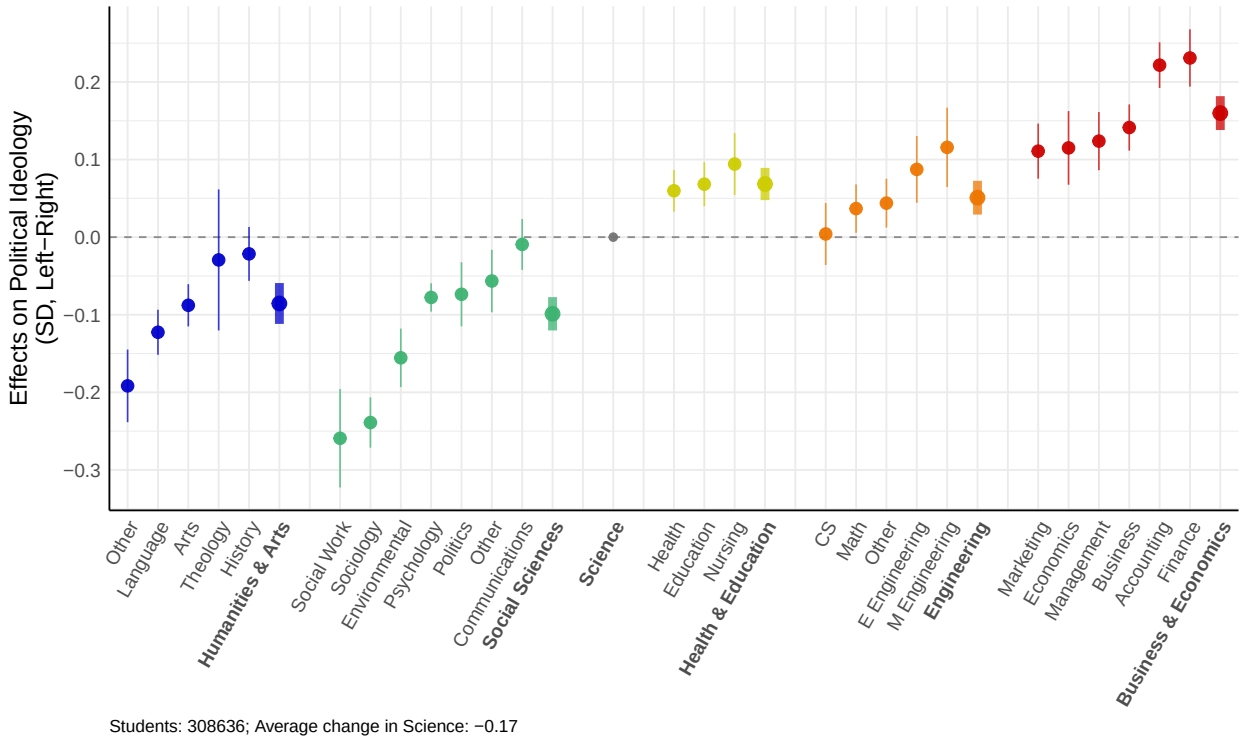
$$\begin{aligned} &\mathbb{E}[p_i \mid m_i = m, x] - \mathbb{E}[p_i \mid m_i = k, x] = \\ &\underbrace{\mathbb{E}[\delta_{im} \mid m_i = m, x] - \mathbb{E}[\delta_{ik} \mid m_i = k, x]}_{\mathrm{ATT}_m(x) - \mathrm{ATT}_k(x)} + \underbrace{\mathbb{E}[u_i \mid m_i = m, x] - \mathbb{E}[u_i \mid m_i = k, x]}_{=0 \text{ by A1}} \end{aligned} \tag{A1}$$

Therefore, within each covariate cell x, the conditional difference in outcomes between major m and major k identifies the difference in treatment-on-the-treated effects for those who self-selected into m or k in covariate cell x.

To obtain a more interpretable estimand, we impose additional symmetry conditions on how students sort on potential ideology effects. Specifically, we assume that any selection on ideology effects into majors or into the overlapping support is symmetric between m and k (see discussion in Mountjoy and Hickman, 2021). Under these conditions, OLS estimation captures the average treatment effect of each major relative to the omitted category.

Moreover, we also report results that do not require such assumptions, following a practical remedy suggested by Goldsmith-Pinkham et al. (2024): to estimate models that compare each treated major k to a single control group (the omitted major) $\tilde{k}$, using only students in either k or $\tilde{k}$. Figure B.1 reports these results, which remain very similar to our baseline estimates. This robustness not only indicates that our main results are free from contamination bias, but also suggests that our setting likely satisfies additional restrictions on treatment heterogeneity.

Figure B.1: Effects of College Majors on Ideology, Contamination Bias Correction



Note: This figure plots the estimates for δ_m from equation 1 and their 95 percent confidence intervals when using graduation ideology as the outcome variable. The estimation follows a correction for contamination bias, as offered by Goldsmith-Pinkham et al. (2024) and discussed in the main text. The estimated coefficients represent the effects of studying each major relative to the natural sciences, which are the omitted group. Standard errors are clustered at the college level.

Appendix C Robustness of the Baseline Estimates

Alternative ideological outcomes. Appendix Figure C.1 presents the baseline estimates using alternative ideological outcomes: identification as middle-of-the-road (Panel A), conservative or far right (Panel B), and liberal or far left (Panel C). Relative to the natural sciences, economics and business majors are 1 pp more likely to identify as middle-of-the-road, while humanities and social sciences majors are 2 pp less likely to do so. Humanities and social sciences majors are also 2 pp less likely to identify as conservative or far right, whereas economics and business majors are 5 pp more likely to do so. Finally, humanities and social sciences majors are 4 pp more likely to identify as liberal or far left, whereas economics and business majors are 6 pp less likely to do so.

Additional controls. As an additional robustness test, we add controls that may correlate with unobservables, including time use, demographics, and college and year fixed effects.[27] Figure C.2 reports the estimated coefficients from specifications that sequentially add the controls listed in Table G.4. The baseline specification includes only major dummies and is therefore most vulnerable to selection bias. Adding our main controls substantially shifts the coefficients, underscoring their importance. However, the estimates remain stable once additional controls are added, supporting the robustness of our results.

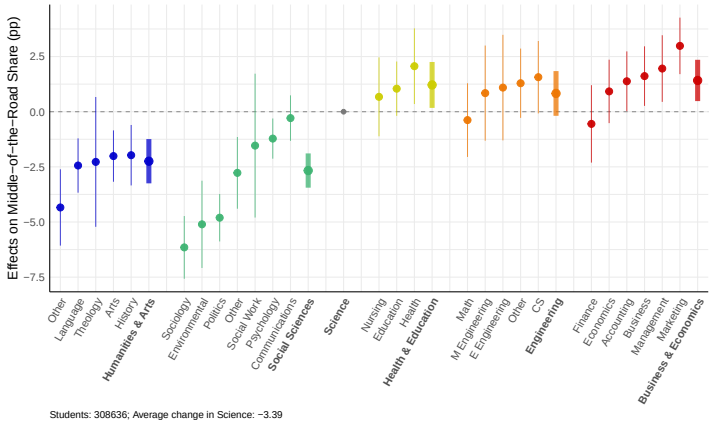
First differences. We also estimate equation 1 using a modified outcome: the change in students' political ideology from baseline to graduation (a first-differences specification). This approach relies on a conditional parallel-trends assumption, namely that, conditional on controls, students across majors would have followed similar ideological trends absent treatment. Figure C.3 reports the results, which closely mirror our baseline estimates, reinforcing their validity within a slightly different identification framework.

Attrition. The study sample consists of students who participated in both the baseline and graduation surveys. If participation in the graduation survey varies systematically across realized majors, the estimates of δ_m could be biased. Directly examining differential attrition across majors and implementing formal bounds such as those in Lee (2009) is infeasible because we lack information on the majors pursued by students who did not complete the graduation survey. However, we can test for differential attrition by intended major.

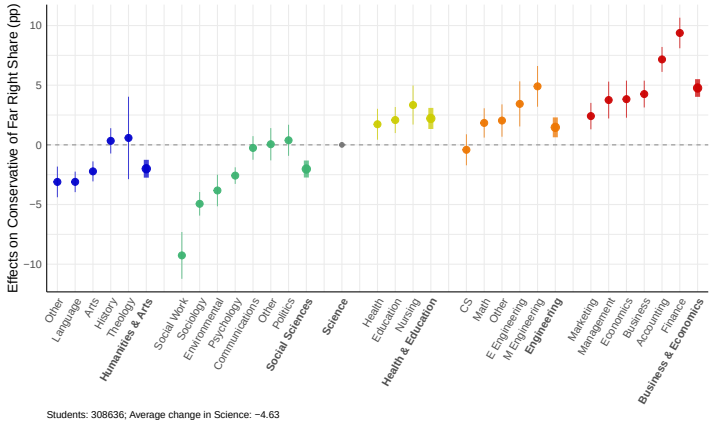
[27] We also estimate models with college-by-year fixed effects, as well as their interactions with lagged ideology and intended major; the results are similar.

Figure C.1: Effects of College Majors on Political Ideology, Indicator Outcomes

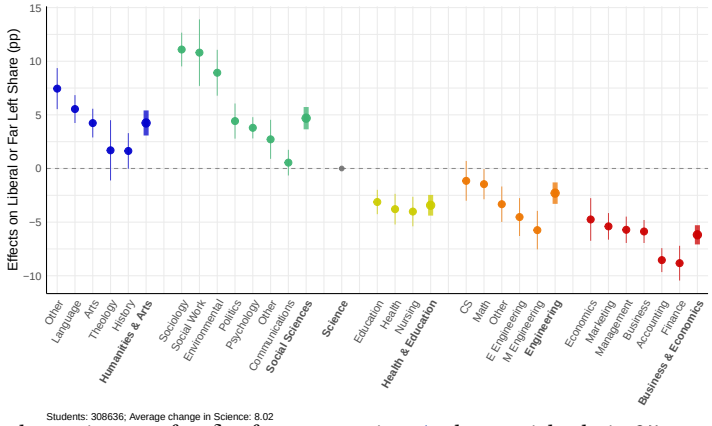
(A) Share Middle of the Road



(B) Share Far Right or Conservative

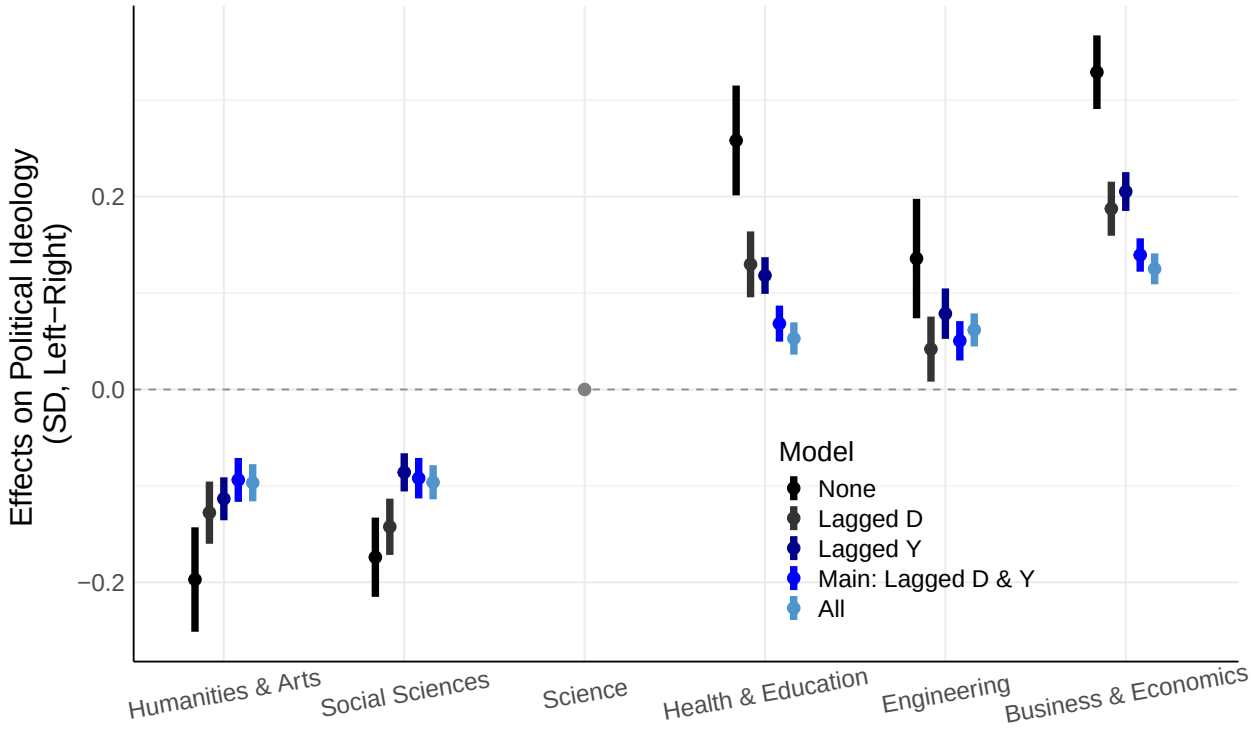


(C) Share Far Left or Liberal



Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals. The bold lines represent coefficients from a model that groups majors into five broad categories, with natural sciences as the omitted reference group. The thinner lines show coefficients from a more detailed model with 28 specific major categories, with natural sciences as the omitted reference group. The outcome variables are dummies for students' political-ideology self-identification by labels mentioned in the titles of each panel. Estimates are shown in pp. Both models control flexibly for students' baseline political ideology, policy position, intended majors, life goals, and all our main controls (see Table G.4). The sample includes only students with non-missing values in the major and outcome. Standard errors are clustered at the college level.

Figure C.2: Robustness to Including Additional Control Variables

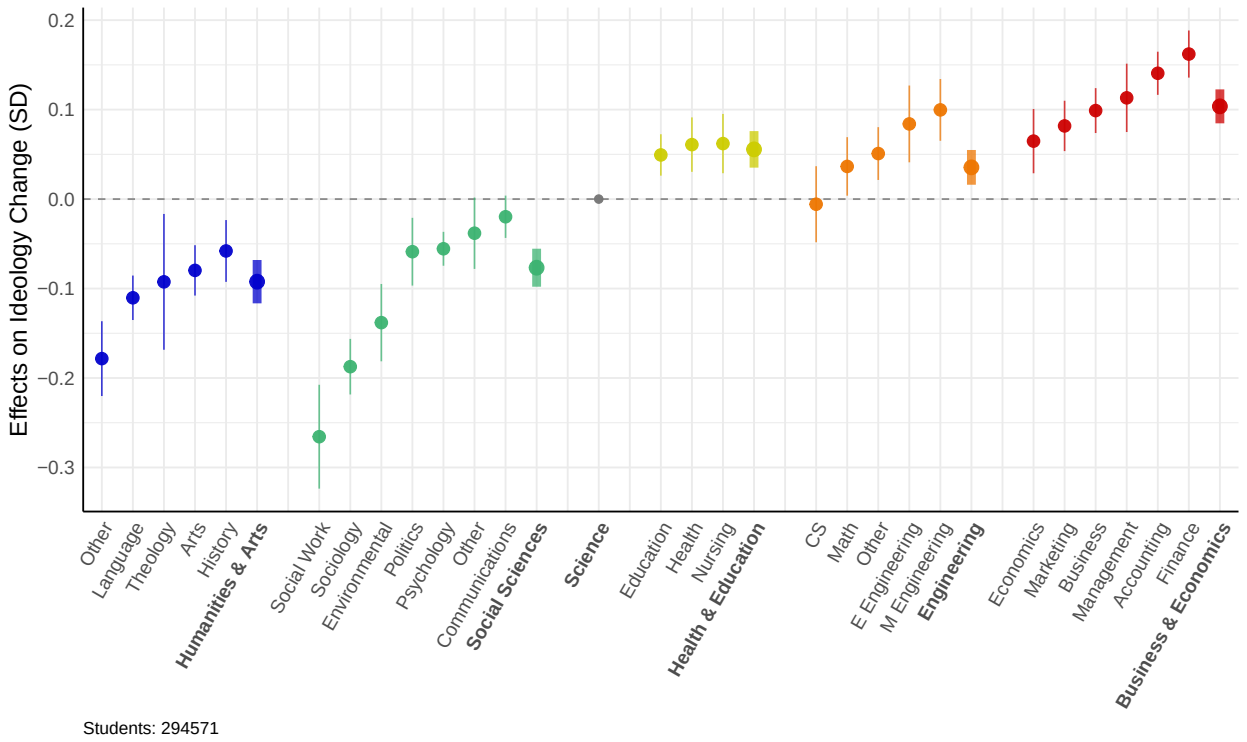


Note: This figure plots estimates of δ_m from equation 1 as control sets are added sequentially. Coefficients represent the effects of each major group relative to the natural sciences. The outcome is graduation political ideology, measured in SD. The specifications correspond to: no controls (None); treatment predictors, including intended major, career preferences, and life goals (Lagged D); outcome predictors, including lagged ideology and policy positions (Lagged Y); both treatment and outcome predictors, corresponding to the main specification (Main: Lagged D & Y); and a full set of controls, which adds college fixed effects, enrollment year, high-school behaviors and time use, and demographic characteristics (All). Table G.4 lists all control sets. The sample includes students with non-missing major and graduation ideology. Standard errors are clustered at the college level.

Table C.1 shows that the conditional response gaps by intended major are small. Columns (1)–(4) indicate that raw participation rates differ somewhat across intended majors, but these differences shrink substantially once we control for high-school GPA and other covariates. For example, the raw response gap between students intending engineering and math and those intending natural sciences is 5.3 pp (column 1), but in the controlled specification (column 4) it is only 1.5 pp. The remaining controlled gaps are also negative and not larger than one pp in absolute value. Thus, the intended natural sciences are the higher-response group in every pairwise contrast, but the response advantage is small. Such small differences imply that under a monotonicity assumption, the sample selection bias is likely very small in our setting.

A remaining concern is that the Lee monotonicity assumption could fail. In this context, non-monotonic attrition would mean that, relative to natural sciences, a given major increases

Figure C.3: Effects of College Majors on Political Ideology Change



Note: This figure plots the estimates for δ_m from equation 1 and their 95 percent confidence intervals when using the change in attitudes as the outcome variable. The estimated coefficients represent the effects of studying each major relative to natural sciences which are the omitted group. The outcome variable is changes from college entry to graduation in students' responses to the following question: "How would you characterize your political views?". Estimates are shown in SD. The sample includes only students with non-missing values in the major and outcome. Standard errors are clustered at the college level.

the probability of graduation-survey response for some students but decreases it for others. A particularly relevant violation would occur if this heterogeneity were related to potential ideology–for example, if right-leaning students were more likely to attrit from humanities while left-leaning students were more likely to attrit from business and economics. Column (5) shows that baseline ideology is only weakly related to response within intended-major groups, and that this relationship does not differ sharply across groups. Specifically, in all intended-major groups, baseline ideology is positively correlated with appearing in the graduation survey. This makes it unlikely that the small average attrition gaps in column (4) are masking substantial differential selection by political type.

Table C.1: Assessing Differential Attrition

	Appear in Graduation Survey				
	(1)	(2)	(3)	(4)	(5)
Business & Economics	-0.024**	-0.024***	-0.009***	-0.006***	-0.005**
	(0.010)	(0.003)	(0.002)	(0.002)	(0.002)
Engineering & Math	-0.054***	-0.023***	-0.018***	-0.015***	-0.015***
	(0.017)	(0.003)	(0.003)	(0.003)	(0.003)
Health & Education	-0.037***	-0.009***	-0.005*	-0.010***	-0.009***
	(0.011)	(0.003)	(0.003)	(0.003)	(0.003)
Humanities & Arts	-0.013	-0.021***	-0.010***	-0.009***	-0.009***
	(0.011)	(0.002)	(0.002)	(0.002)	(0.002)
Social Sciences	-0.023**	-0.016***	-0.003*	-0.002	-0.001
	(0.009)	(0.002)	(0.002)	(0.002)	(0.002)
Ideology					0.009***
					(0.001)
Ideology × Business & Economics					-0.003*
					(0.001)
Ideology × Engineering & Math					-0.001
					(0.002)
Ideology × Health & Education					-0.004***
					(0.001)
Ideology × Humanities & Arts					-0.001
					(0.002)
Ideology × Social Sciences					-0.003**
					(0.001)
N	1,454,817	1,454,817	1,454,817	1,454,817	1,454,817
College-Year FE		✓	✓	✓	✓
High-School GPA			✓	✓	✓
All Controls				✓	✓

Note: This table reports regressions of an indicator for appearing in the graduation survey. Columns (1)–(4) report differences in attrition by intended major relative to intended natural sciences. Column (5) adds baseline ideology and its interaction with intended major. "All Controls" includes baseline career preferences, detailed intended-major controls, life goals, and baseline policy views other than ideology. The sample includes baseline-survey students from the colleges and cohorts in the main analysis sample. Standard errors are clustered at the college level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Double majors. The main analysis is focused on students' primary major, since second majors are not observed for cohorts enrolled before 2005. To assess whether missing informa-

tion on second majors biases our results, we re-estimate Equation 1 with and without flexible controls for students' second majors. Table C.2 shows that the estimated effects on political ideology are nearly identical with and without these controls, indicating that omitting second majors does not meaningfully affect our conclusions.[28]

We also explore how the effects of first majors interact with students' second majors by estimating a version of Equation 1 that includes indicators for each major combination, with single-major in natural sciences as the reference group. Figure C.4 presents these estimates. The results reveal that the ideological effects of double majors generally depend on the combination of fields. For example, students who major in social sciences and take a second major in business or economics do not become significantly more liberal relative to students who majored only in natural sciences, suggesting that the conservative effect of business/economics offsets the liberal influence of the social sciences and humanities. However, combining social sciences or humanities with social sciences or humanities leads to a significant leftward shift, with estimated effects reaching 0.11–0.15 SD relative to natural sciences. In contrast, combining two majors in business and economics leads to a 0.2 shift toward conservatism.

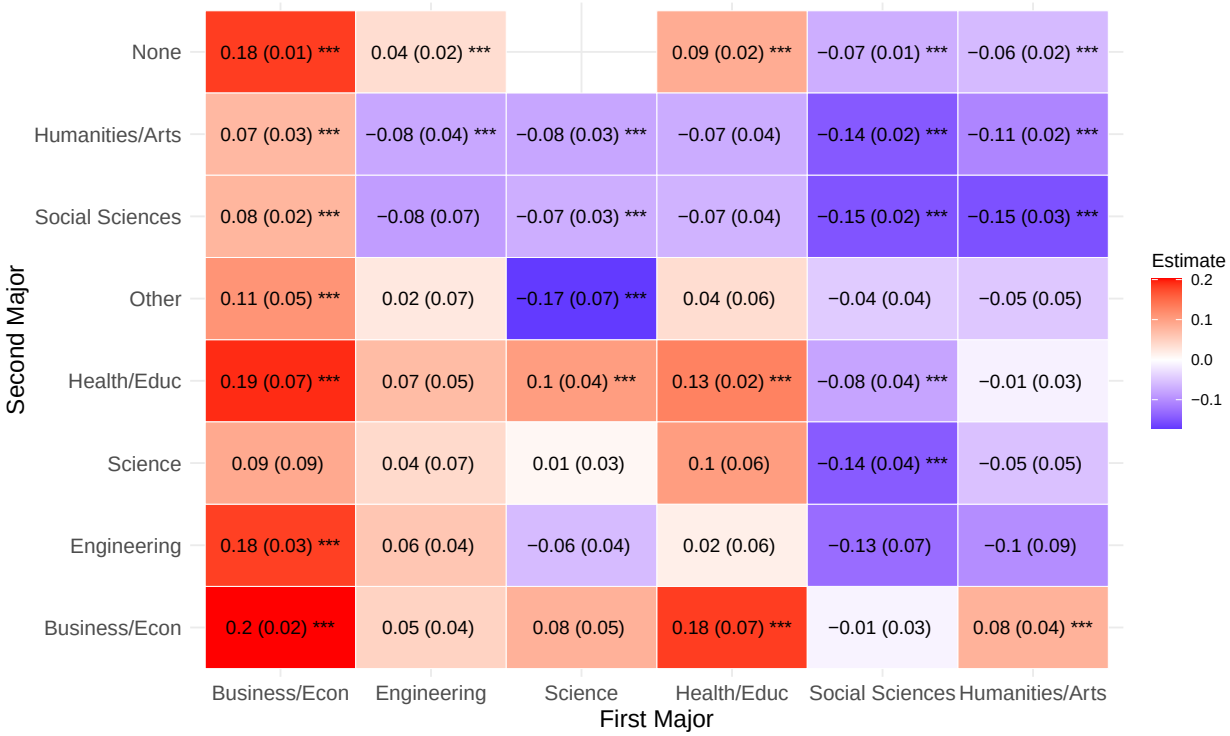
Table C.2: Robustness to Controlling for Double-Major Status

	Ideology		Index	
Control		+		+
Business/	0.18***	0.17***	-0.08***	-0.08***
Economics	(0.01)	(0.01)	(0.01)	(0.01)
Engineering/	0.04**	0.03*	-0.02	-0.01
CS/Math	(0.01)	(0.01)	(0.01)	(0.01)
Health/	0.08***	0.08***	-0.04***	-0.03***
Education	(0.02)	(0.02)	(0.01)	(0.01)
Humanities/	-0.06***	-0.05***	0.02**	0.02**
Arts	(0.02)	(0.02)	(0.01)	(0.01)
Social	-0.08***	-0.07***	0.04***	0.04***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)
N	118127	118127	118127	118127

This table shows the estimates for δ_m from equation 1 and their standard errors, clustered at the college level, with and without controlling for the double major category. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome from the enrolling cohorts of 2005 or later. The outcomes represent self-reported political ideology and are measured in standard deviations on a Left-Right scale, and a dummy for identification as Far Left or Liberal. *** p<0.01, ** p<0.05, * p<0.1

[28]The table also shows that this holds for the policy-position index that is defined in Section 3.4.

Figure C.4: Effects of College Majors and Double Majors on Students' Ideology



Note: This figure plots the estimates for δ_m from equation 1 and their clustered standard errors from a model with interaction between first and second majors. The estimated coefficients represent the effects of studying each combination of majors relative to the natural sciences with no second major (the omitted group). The outcome variable is students' responses to the following question: "How would you characterize your political views?". Estimates are shown in SD. The sample includes only students with non-missing majors and political ideology in the graduation survey from enrolling cohorts since 2005. Standard errors are clustered at the college level.

Early college experience. One concern for our main results is that some experiences during the freshman and sophomore years may influence both major choice and graduation ideology.[29] For example, consider a student who forms a romantic relationship with a politically engaged partner majoring in environmental science. The partner's influence might simultaneously shift the student's political ideology leftward and encourage them to switch into environmental studies. If such experiences are common, our estimates could conflate the causal effect of majors with the effect of these concurrent experiences.

Thus, we report results based on an alternative identification strategy designed to address concerns about any post-baseline college experiences. Specifically, we use students' intended majors at entry as instruments for their realized majors. This approach exploits variation driven by initial preferences, thereby mitigating bias from post-enrollment shocks that jointly

[29]Note that our bounding exercises also address this concern, as they assess potential bias from all unobserved covariates, including early college experiences.

affect major choice and political attitudes. The trade-off of this approach is that we cannot include intended major as a control, requiring us to rely on alternative inferior proxies for major preferences. The following equations implement this IV strategy:

$$D_{im} = \sum_m \pi_m D^0_{im} + Q_i'\gamma^0 + \theta^0_t + \lambda^0_c + u^0_i, \tag{A2}$$

$$p_i = \sum_m \tau_m D_{im} + Q_i'\gamma^1 + \theta^1_t + \lambda^1_c + u^1_i, \tag{A3}$$

Where D^0_{im} are the intended major dummies, serving as IV for actual major choices D_{iim}, and Q_i includes our main set of controls X_i excluding D^0_{im}. This approach relies on the following assumptions. First, intended majors are relevant instruments for realized majors. Figure G.2 shows that most students ultimately pursue a major within the same group as their intended major, indicating a strong first stage. Second, the independence assumption requires that intended majors are uncorrelated with factors affecting graduation ideology, conditional on our controls ($u^1_i \perp D^0_{im}$). This assumption is stronger than in our baseline analysis. However, our controls still include other major preference predictors, such as intended careers and life goals.

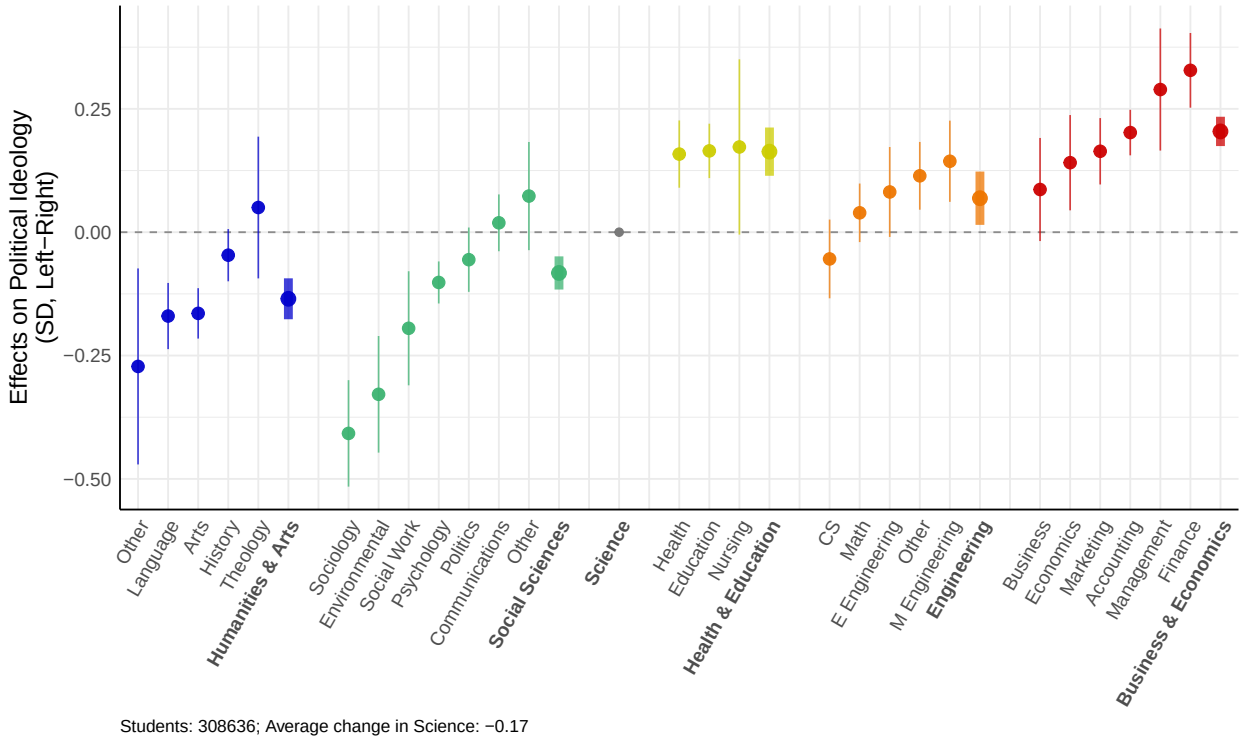
Third, this specification assumes monotonicity: a preference for a specific major can only weakly increase the likelihood of pursuing it. Formally, for each major m, $D_{im}(D^0_i = m) \geq D_{im}(D^0_i \neq m)$. This assumption is plausible in our setting, as it is unlikely that intending a major would make a student less likely to pursue it.[30]

Finally, this analysis also relies on an exclusion restriction: intended major should affect graduation ideology only through realized major. A potential violation could arise if students who initially preferred a major but ultimately did not choose it still took related courses that shaped their ideology. Such a channel would operate through the same educational mechanisms we aim to capture.

Figure C.5 shows that the IV estimates are broadly similar to the baseline OLS estimates, with somewhat larger magnitudes in some specifications. This pattern is consistent with attenuation in the OLS estimates due to measurement error in realized major, though it could also reflect violations of the exclusion restriction discussed above.

[30] As in the main analysis, this specification includes multiple treatments and may therefore suffer from contamination bias, as demonstrated in a similar setting by Kirkeboen et al. (2016). In unreported results, we find that correcting for contamination bias does not meaningfully change the coefficients.

Figure C.5: Effects of College Majors on Political Ideology, Using Intended Majors as IV



Note: This figure plots the IV estimates for τ_m from equation A2 as control sets are added sequentially. Intended majors instrument realized majors, and the coefficients represent the effects of studying each major relative to natural sciences. The outcome is graduation political ideology, measured in SD. The sample includes students with non-missing intended major, realized major, and graduation ideology. Standard errors are clustered at the college level.

Heterogeneity of the effects The following tables report heterogeneity estimates by estimating Equation 1 separately for several subsamples. Panel A of Table C.3 reports results by students' personal characteristics. Table C.4 reports results by college characteristics. Table C.5 examines changes over time. Overall, the results suggest that the ideological effects of college majors are not concentrated in a particular group of students or type of institution.

Table C.3: Effects of College Majors on Ideology, By Student's Characteristics

	Females	Males	Low SES	High SES	Low HSGPA	High HSGPA
Outcome: Ideology Score						
Business&	0.13***	0.15***	0.12***	0.16***	0.13***	0.16***
Economics&	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Engineering&	0.04**	0.06***	0.05***	0.06***	0.05***	0.06***
Math	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Health&	0.07***	0.06***	0.06***	0.10***	0.06***	0.08***
Education	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Social	-0.10***	-0.07***	-0.10***	-0.07***	-0.09***	-0.12***
Sciences	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Humanities&	-0.10***	-0.08***	-0.10***	-0.08***	-0.09***	-0.11***
Arts	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
N	192514	115665	206448	102188	200420	108216
Outcome: Left Share						
Business&	-5.93***	-6.55***	-5.64***	-7.04***	-5.64***	-7.13***
Economics&	(0.49)	(0.62)	(0.56)	(0.54)	(0.52)	(0.66)
Engineering&	-1.42*	-2.80***	-2.02***	-3.01***	-2.80***	-1.96**
Math	(0.69)	(0.70)	(0.53)	(0.84)	(0.66)	(0.69)
Health&	-3.42***	-3.08***	-2.91***	-4.48***	-3.13***	-3.82***
Education	(0.51)	(0.79)	(0.58)	(0.70)	(0.59)	(0.66)
Social	5.03***	3.05***	5.20***	3.61***	4.58***	5.83***
Sciences	(0.50)	(0.80)	(0.55)	(0.72)	(0.58)	(0.76)
Humanities&	4.68***	3.09***	4.86***	3.05***	4.16***	4.97***
Arts	(0.55)	(0.90)	(0.60)	(0.79)	(0.70)	(0.61)
N	192514	115665	206448	102188	200420	108216

This table shows the estimates for δ_m from equation 1 and their standard errors, clustered at the college level. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. The outcomes represent self-reported political ideology (measured in standard deviations, on a Left-Right scale) and an indicator for identifying as liberal or far left (multiplied by 100). *** p<0.01, ** p<0.05, * p<0.1

Table C.4: Effects of College Majors on Ideology, By College Characteristics

	Private College	Private University	Public	More Selective	Less Selective	North	South
Outcome: Ideology Score							
Business&	0.14***	0.12***	0.14***	0.17***	0.13***	0.14***	0.15***
Economics&	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Engineering&	0.04**	0.05**	0.08**	0.06***	0.06***	0.05***	0.08**
Math	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)
Health&	0.08***	0.05*	0.04	0.08**	0.06***	0.07***	0.10***
Education	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Social	-0.08***	-0.13***	-0.10***	-0.08*	-0.10***	-0.09***	-0.08***
Sciences	(0.01)	(0.03)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
Humanities&	-0.08***	-0.15***	-0.12***	-0.09**	-0.09***	-0.10***	-0.08***
Arts	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
N	212787	59878	35971	67089	241547	270505	38131
Outcome: Left Share							
Business&	-6.31***	-5.67***	-6.07***	-7.88***	-5.69***	-6.22***	-6.11***
Economics&	(0.54)	(0.94)	(1.20)	(1.14)	(0.47)	(0.51)	(0.89)
Engineering&	-1.82*	-2.39*	-3.28**	-3.20***	-2.42***	-2.30***	-3.33**
Math	(0.75)	(0.92)	(1.01)	(0.74)	(0.56)	(0.55)	(1.06)
Health&	-4.19***	-2.34*	-1.64	-3.63**	-3.17***	-3.43***	-3.83***
Education	(0.56)	(0.93)	(1.00)	(1.34)	(0.48)	(0.55)	(0.80)
Social	4.07***	6.05***	6.20***	3.93*	4.93***	4.80***	3.96***
Sciences	(0.57)	(1.39)	(0.94)	(1.63)	(0.48)	(0.60)	(0.75)
Humanities&	3.49***	6.26***	5.90***	3.80**	4.31***	4.28***	4.19***
Arts	(0.68)	(0.98)	(1.12)	(1.22)	(0.65)	(0.68)	(0.76)
N	212787	59878	35971	67089	241547	270505	38131

This table shows the estimates for δ_m from equation 1 and their standard errors, clustered at the college level. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. The outcomes represent self-reported political ideology (measured in standard deviations, on a Left-Right scale) and an indicator for identifying as liberal or far left (multiplied by 100). *** p<0.01, ** p<0.05, * p<0.1

Table C.5: Effects of College Majors on Ideology, By Enrollment Year

	2000 and Before	2000-2009	2010 and Later
Outcome: Ideology Score			
Business&	0.10***	0.14***	0.21***
Economics&	(0.01)	(0.01)	(0.02)
Engineering&	0.06***	0.06***	0.03
Math	(0.01)	(0.01)	(0.02)
Health&	0.04**	0.08***	0.11***
Education	(0.01)	(0.01)	(0.03)
Social	-0.11***	-0.08***	-0.07***
Sciences	(0.01)	(0.01)	(0.02)
Humanities&	-0.13***	-0.08***	-0.07***
Arts	(0.01)	(0.01)	(0.02)
N	122967	134631	51038
Outcome: Left Share			
Business&	-4.23***	-6.04***	-10.30***
Economics&	(0.63)	(0.62)	(0.74)
Engineering&	-2.91***	-2.29***	-1.77
Math	(0.61)	(0.67)	(1.35)
Health&	-2.29***	-3.37***	-5.06***
Education	(0.58)	(0.66)	(1.29)
Social	5.78***	4.13***	3.48***
Sciences	(0.58)	(0.73)	(0.90)
Humanities&	5.77***	3.56***	2.42**
Arts	(0.69)	(0.80)	(0.92)
N	122967	134631	51038

This table shows the estimates for δ_m from equation 1 and their standard errors, clustered at the college level. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. The outcomes represent self-reported political ideology (measured in standard deviations, on a Left-Right scale) and an indicator for identifying as liberal or far left (multiplied by 100). *** p<0.01, ** p<0.05, * p<0.1

Appendix D Accounting for Selection on Unobservables

D.1 Following Oster (2019)

This subsection provides a complementary formal explanation of how we use the coefficient-stability approach of Oster (2019) to construct bounds on bias from selection on unobservables. As described in Section 3, we apply this method to the estimated effect of choosing business or economics relative to the humanities or social sciences.

Following Oster (2019), the bias-adjusted effect for major group m can be written as:

$$\delta_m^* = \delta_{m,F} - \lambda \left(\delta_{m,P} - \delta_{m,F}\right) \frac{R^2_{\max} - R^2_F}{R^2_F - R^2_P},$$

where $\delta_{m,P}$ and R^2_P come from the parsimonious specification, which excludes the variable or variables used for benchmarking; $\delta_{m,F}$ and R^2_F come from the full specification. In our main analysis, we use lagged ideology as the benchmarking variable, since it is likely the most relevant observed proxy for unobserved ideology. It is based on the same question as the outcome, measured a few years earlier. Thus, $\delta_{m,F}$ is estimated using the main set of controls, while $\delta_{m,P}$ is estimated using the same set of controls excluding lagged ideology.

λ governs the strength of selection on unobservables relative to selection on observables. We use $\lambda = 1$ as the main specification, which assumes that unobserved determinants of graduation ideology are no more conditionally correlated with major choice than pre-college ideology is. This seems relatively conservative, given that lagged ideology is measured using the same question as the outcome and is likely the strongest observed proxy for ideological selection. We also report more conservative estimates using $\lambda = 2$.

$R^2_{\max}$ denotes the maximum attainable explanatory power. While it is mechanically bounded by 1, in our setting it is likely substantially lower due to several sources of measurement and specification limitations. We construct $R^2_{\max}$ by estimating how much of the variation in graduation ideology cannot plausibly be explained, even by a very rich model. We start from the mechanical upper bound of 1 and subtract four sources of unavoidable attenuation.

First, we estimate the contribution of survey-response, or filling, error using repeated reports of variables that should be stable across survey waves. Specifically, we compare responses by the same respondent in the freshman and follow-up surveys for characteristics such as race, sex, native-English status, and compute the share of inconsistent responses. We then use the implied inconsistency rate to simulate random misclassification in the ideology measure. Finally, we regress the simulated mismeasured ideology on fixed effects for the original ideology category and define the resulting loss in R^2 as the filling-error deduction.

Second, we estimate the contribution of discretization. Because ideology is observed on a five-point scale, we simulate an underlying continuous ideology variable with the same mean and variance as the observed measure, round it to the five-point scale, and regress the continuous variable on fixed effects for the rounded category. The unexplained share from this regression is the R^2 loss due to rounding.

Third, we estimate the contribution of treatment-effect heterogeneity. We compare a model with homogeneous major effects to a richer model with major-by-college effects. The additional variance explained by the major-by-college fixed effects, after applying the leave-one-out/Honest-Observation correction, is interpreted as variation that cannot be captured by a single homogeneous major effect. This is the heterogeneity deduction.

Fourth, we estimate the contribution of random noise. We use short-run changes in repeated ideology reports, net of measurement error, to approximate idiosyncratic noise in the outcome. The implied unexplained variance, scaled by total outcome variance, gives the random-noise deduction.

We then sum these four deductions:

$$D = D_{\text{filling}} + D_{\text{rounding}} + D_{\text{heterogeneity}} + D_{\text{noise}}.$$

The noise-adjusted ceiling is

$$R^2_{\max} = 1 - D.$$

This gives a noise-adjusted ceiling of about 0.649. We also report an alternative $R^2_{\max}$ constructed by taking the R^2 from the full specification and adding the partial R^2 of lagged ideology. This yields a value of 0.487 and corresponds to the assumption that unobservables can explain as much variation in the outcome as pre-college ideology.

Finally, we use the estimated coefficients and R^2 values to calculate the bias-adjusted estimate. In the relevant comparison, $\delta_{m,F} = 0.230$ and $\delta_{m,P} = 0.243$, while $R^2_F = 0.40$ and $R^2_P = 0.32$. Substituting these values into the Oster formula gives:

$$\delta^*_m = 0.230 - \lambda\,(0.013)\,\frac{R^2_{\max} - 0.40}{0.08}.$$

We then evaluate this expression under alternative values of λ and $R^2_{\max}$ to obtain the bounds shown in Panel B of Figure 2.

D.2 Following Cinelli and Hazlett (2020)

As an alternative robustness exercise, we follow Cinelli and Hazlett (2020) to assess sensitivity to selection on unobservables. The key difference relative to the previous section is that this approach benchmarks confounding using partial R^2.[31] As in the previous subsection, we focus on the contrast between social sciences or humanities and economics or business.

Using baseline ideology as the benchmark variable, and setting the relative strength of selection into both treatment and outcome to one, the implied bias is 0.03, and the adjusted estimate is 0.20. This is very similar to the result obtained using the approach of Oster (2019), further supporting the robustness of our estimates. Even when allowing the unobserved confounder to explain twice as much residual outcome and treatment variation as baseline ideology, the implied bias is 0.05, and the adjusted estimate remains about 0.18.

Table D.1: Robustness to Unobserved Selection

Benchmark	k_D	k_Y	Bias	Adjusted Estimate
Panel A: Key Controls				
Intended Econ/Business	1	1	0.02	0.22
Baseline Ideology	1	1	0.03	0.20
Intended Econ/Business	2	2	0.04	0.20
Baseline Ideology	2	2	0.05	0.18
Panel B: Additional Controls				
Sex	1	1	0.00	0.22
HS GPA	1	1	0.00	0.23
Sex	2	2	0.01	0.22
HS GPA	2	2	0.00	0.23
Sex	10	10	0.04	0.18
HS GPA	10	10	0.02	0.21

Note: The table shows the bias of our estimates under different assumptions about the partial R^2 of unobserved confounders (Cinelli and Hazlett, 2020). We focus on the estimated effect of majoring in humanities or social sciences relative to economics or business, which is about 0.23 SD in our main model. k_D and k_Y measure the proportion of selection on unobservables relative to observables for the treatment and outcome, respectively, both in terms of partial R^2. Benchmarking focuses on variables most correlated with the outcome (baseline ideology) and treatment (intended major) in Panel A, as well as high school GPA and sex in Panel B.

[31] We report these sensitivity analyses using non-clustered standard errors because the framework proposed by Cinelli and Hazlett (2020) is derived for the simple case without clustering. This choice is likely conservative in our context: incorporating clustered standard errors would substantially reduce the effective degrees of freedom, which enters directly into the bound formula, and would yield tighter bounds and smaller bias.

Appendix E Decomposing Effects on Political Ideology Based on Policy Positions

Given that college majors influence students' self-identified ideology as well as their specific policy positions, it is natural to ask to what extent changes in different policy positions mediate the observed ideological effects. To address this, we implement a decomposition approach following Gelbach (2016), which quantifies the contribution of each covariate in a regression to changes in the baseline coefficients. Specifically, we estimate the following model:

$$\underbrace{p_i}_{\substack{\text{Grad. political} \\ \text{ideology}}} = \underbrace{\sum_m \delta_m^* D_{icym}}_{\substack{\text{Major} \\ \text{effects}}} + \underbrace{X_i'\alpha^*}_{\substack{\text{Lag. ideology and proxies,} \\ \text{Int. major and proxies}}} + \underbrace{A_i'\beta^*}_{\text{Grad. positions}} + \theta_t^* + \lambda_c^* + \varepsilon_i^* \tag{A4}$$

This equation is identical to equation 1, but adds students' graduation policy positions as additional covariates. Importantly, these are mediators rather than controls, as they are determined post-treatment and, as discussed above, are themselves affected by the treatment. Comparing the estimated major effects (δ_m^*) from this model to the baseline estimates (δ_m) reveals the extent to which policy position effects explain the overall ideological effects. This analysis requires an additional identification assumption: that potential political ideology is independent of potential policy positions, conditional on observed covariates. While this assumption is not trivial, we believe that the strong explanatory power of our controls and mediators makes substantial omitted variable bias less likely. We also discuss supporting evidence below.

We begin by using the full sample from our main analysis and incorporate all policy position questions included in the survey.[32] We classify positions into three groups: economic, cultural, and other. For each group of questions, we construct a principal component that summarizes the responses within the group. This allows us to quantify the importance of each component as a mediator using the approach of Gelbach (2016). Results are shown in Table E.1. Policy positions explain a substantial share of the effect of college major: together, they account for roughly 48 percent of the effect of studying business and economics relative to natural sciences, and about 66-68 percent of the effects of studying social sciences or humanities.

[32] Since policy questions vary between years, this approach requires adding a dummy for missing responses or missing questions

The composition of these channels, however, differs sharply across fields. For business and economics, economic policy positions explain 34 percent of the overall effect, while cultural policy positions account for only 8 percent. In contrast, for social sciences and humanities, cultural policy issues explain 59 and 49 percent of the effects, respectively, whereas economic policy positions explain 1 and 4 percent.

Table E.1: Decomposition of the Effects of College Majors

Major 1	Major 2	Base Coef	Full Coef	Economic Views	Culture Views	Other Views	Not Explained
Business & Economics	Natural Sciences	0.132	0.069	33.86%	8.36%	5.33%	52.44%
Engineering & Math	Natural Sciences	0.077	0.032	34.42%	22.12%	2.17%	41.29%
Health & Education	Natural Sciences	0.065	0.034	13.85%	15.83%	19.03%	51.28%
Natural Sciences	Social Sciences	0.107	0.040	1.25%	58.83%	2.01%	37.91%
Natural Sciences	Humanities & Arts	0.100	0.034	4.05%	49.45%	12.87%	33.63%

Notes: This table shows the Gelbach decomposition of the estimated effects of college major on students' political ideology at graduation. Economic refers to the principal component constructed from questions about any economic issue included in the survey. Culture refers to the principal component of questions on any cultural issue. Other refers to the principal component for other issues. Base Coef is the coefficient on the major group (relative to the paired group) from the baseline model, and Full Coef is the coefficient after controlling for all policy positions. The remaining columns show the share of the base effect explained by each policy position group (expressed as a percentage of the baseline effect), with Not Explained representing the share of the major effect not accounted for by the included mediators. All models control for baseline ideology, intended major, and all our main controls.

We next test the robustness of our results by restricting the sample to students who answered the same set of policy questions in both the baseline and graduation surveys. We focus on questions that appear in multiple survey years: two economic issues (taxation and healthcare), four cultural issues (abortion, race, LGBTQ rights, and free speech), and two additional questions (marijuana legalization and the ability to effect change). The resulting sample includes 86,717 students from surveys conducted between 1996–2001 and in 2009.

Table E.2 presents the results. The overall share of the major effect explained by these questions decreases but remains significant: from 68 to 39 percent for humanities, from 62 to 50 percent for social sciences, and from 48 to 43 percent for business and economics. This suggests that missing information on other policy items is unlikely to generate substantial bias. The main conclusions remain unchanged: economic policy positions continue to account for most of the effects among economics and business majors, while cultural policy positions explain most of the effects among social sciences and humanities majors. Table E.3 provides further detail on the relative contribution of each policy item.[33]

[33]Table E.4 allows the weights on different attitudes to vary by field, capturing the possibility that majors affect not only students' positions but also the priorities they assign to different policy domains. However, differences in priorities appear to play only a minor role in explaining the overall effects.

Table E.2: Decomposition of the Effects of College Majors, Using Only Persistent Survey Questions

Major 1	Major 2	Base Coef	Full Coef	Economic Views	Culture Views	Other Views	Not Explained
Business & Economics	Natural Sciences	0.132	0.076	31.96%	10.75%	-0.04%	57.33%
Engineering & Math	Natural Sciences	0.077	0.043	27.94%	15.88%	-1.5%	56.1%
Health & Education	Natural Sciences	0.065	0.049	-5.74%	18.31%	10.76%	75.05%
Natural Sciences	Social Sciences	0.107	0.053	19.82%	30.6%	-0.51%	50.06%
Natural Sciences	Humanities & Arts	0.100	0.061	12.2%	24.73%	1.85%	61.3%

Notes: This table shows the Gelbach decomposition of the estimated effects of college major on students' political ideology at graduation. Economic refers to the principal component constructed from questions about support for higher taxes on the wealthy and for universal healthcare. Culture refers to the principal component of questions on abortion, race, LGBTQ rights, and free speech. Other refers to the principal component for questions about marijuana legalization and belief in the ability to bring about social change. Base Coef is the coefficient on the major group (relative to the paired group) from the baseline model, and Full Coef is the coefficient after controlling for all policy positions. The remaining columns show the share of the base effect explained by each policy position group (expressed as a percentage of the baseline effect), with Not Explained representing the share of the major effect not accounted for by the included mediators. All models control for baseline ideology, intended major, and all our main controls.

Table E.3: Decomposition of the Effects of College Majors, Detailed Results

Major 1	Major 2	Base Coef	Full Coef	Tax	Healthcare	Abortion	Race	Gay Prohibit	Speech	Marijuana	Change	Not Explained
Business & Economics	Natural Sciences	0.132	0.078	15.94%	14.16%	2.43%	1.65%	6.25%	-0.07%	0.28%	0.23%	59.04%
Engineering & Math	Natural Sciences	0.077	0.040	12.19%	14.45%	12.92%	2.85%	3.82%	-0.37%	-0.23%	1.48%	51.31%
Health & Education	Natural Sciences	0.065	0.038	1.24%	-6.44%	30.13%	-2.65%	0.6%	0.34%	19.32%	-1.87%	57.58%
Natural Sciences	Social Sciences	0.107	0.045	8.14%	10.55%	9.32%	7.39%	12.26%	-0.09%	7.4%	2.97%	42.09%
Natural Sciences	Humanities & Arts	0.100	0.053	6.88%	4.09%	0.63%	7.49%	14.8%	-0.14%	10.92%	2.2%	52.99%

Notes: This table shows the Gelbach decomposition of the estimated effects of college major on students' political ideology at graduation. Base Coef is the coefficient on the major group (relative to the paired group) from the baseline model, and Full Coef is the coefficient after controlling for all policy positions. The remaining columns show the share of the base effect explained by each policy position (expressed as a percentage of the baseline effect), with Not Explained representing the share of the major effect not accounted for by the included mediators. All models control for baseline ideology, intended major, and all our main controls.

Table E.4: Decomposition of the Effects of College Majors, Allowing for Different Weights

Major 1	Major 2	Base Coef	Full Coef	Economic Views	Culture Views	Other Views	Weights	Not Explained
Business & Economics	Natural Sciences	0.134	0.085	32.48%	6.25%	5.72%	-7.96%	63.51%
Engineering & Math	Natural Sciences	0.073	0.035	39.36%	12.33%	2.64%	-2.97%	48.63%
Health & Education	Natural Sciences	0.055	0.036	17.57%	14.24%	20.46%	-16.36%	64.09%
Natural Sciences	Social Sciences	0.098	0.032	-0.09%	60.42%	1.75%	5.23%	32.69%
Natural Sciences	Humanities & Arts	0.099	0.034	1.98%	47.04%	11.42%	4.88%	34.69%

Notes: This table shows the Gelbach decomposition of the estimated effects of college major on students' political ideology at graduation. Economic refers to the principal component constructed from questions about support for higher taxes on the wealthy and for universal healthcare. Culture refers to the principal component of questions on abortion, race, LGBTQ rights, and free speech. Other refers to the principal component for questions about marijuana legalization and belief in the ability to bring about social change. Weights refer to the relative importance of each component. Base Coef is the coefficient on the major group (relative to the paired group) from the baseline model, and Full Coef is the coefficient after controlling for all policy positions. The remaining columns show the share of the base effect explained by each policy position group (expressed as a percentage of the baseline effect), with Not Explained representing the share of the major effect not accounted for by the included mediators. All models control for baseline ideology, intended major, and all our main controls.

Appendix F Adjusting Peers and Faculty Ideology Measure

Estimating the effects of peer and faculty ideological composition on students' ideology poses a specific challenge in this setting: only a subset of each student's peers and faculty is observed, rather than the full group. Ignoring this limited observation share leads to attenuation bias. In this appendix, we present a correction. For clarity, we focus on the peer analysis, noting that the same logic and correction apply to the faculty setting.

Consider a hypothetical sample in which the full set of peers is observed. The following version of Equation 4 would then apply:

$$p_{icmt} = \sigma^F P_{cmt}^{F,-i} + \beta^F \Phi_{cmt}^{F,-i} + \nu_{cm} + \theta_{ct} + \lambda_{mt} + \varepsilon_{icmt} \tag{A5}$$

Definitions follow the main text. The superscript F indicates the full-peer-sample version of each variable: $P_{cmt}^{F,-i}$ is the average baseline ideology of all peers, and $\Phi_{cmt}^{F,-i}$ is the average ideology of students in the same college and intended major across the five-year window. The outcome variable p_{icmt} denotes the political ideology at graduation of student i enrolled in college c, intending major m, and starting in year t. The terms ν_{cm}, θ_{ct}, and λ_{mt} represent college–major, college–cohort, and major–cohort fixed effects, respectively.

The treatment variable can be decomposed into:

$$P_{cmt}^{F,-i} = \widehat{P_{cmt}^{-i}} + \eta_{cmt}^F \tag{A6}$$

Here, $\widehat{P_{cmt}^{-i}}$ represents the component predictable from fixed effects and controls, while η_{cmt}^F captures the residual, unpredictable component. Under our identification assumption, η_{cmt}^F has mean zero and is uncorrelated with ε_{icmt} conditional on the same fixed effects and controls. Applying the Frisch–Waugh–Lovell theorem (Frisch and Waugh, 1933), we can rewrite Equation A5 in residualized form:

$$p_{icmt}^{\perp} = \sigma^F \eta_{cmt}^F + \varepsilon_{icmt} \tag{A7}$$

Here, the superscript $\perp$ indicates residualization with respect to $\Phi^{F,-i}_{cmt}$ and the fixed effects in Equation A5. The term η^{F}_{cmt} can be further decomposed into observed and unobserved components:

$$\eta^{F}_{cmt} = \alpha_{ct}\eta^{O}_{cmt} + (1 - \alpha_{ct})\eta^{U}_{cmt} \tag{A8}$$

where η^{O}_{cmt} represents the observed share of the peer component and η^{U}_{cmt} the unobserved share. α_{ct} denotes the share of students in college c and entry year t who are covered in the baseline survey.[34] Substituting equation A8 into equation A7 and replacing the unobserved component with a mean-zero composite error term υ_{cmt} gives:

$$p^{\perp}_{icmt} = \sigma^{F}(\alpha_{ct}\eta^{O}_{cmt} + \upsilon_{cmt}) + \varepsilon_{icmt} = \sigma^{F}\alpha_{ct}\eta^{O}_{cmt} + \zeta_{icmt} \tag{A9}$$

Where $\zeta_{icmt} = \sigma^{F}\upsilon_{cmt} + \varepsilon_{icmt}$ is mean zero and uncorrelated with η^{O}_{cmt}. This Equation motivates adjusting for incomplete peer coverage by imputing predicted ideology for unobserved peers:

$$P^{-i}_{cmt} = (1 - \alpha_{ct})\widehat{P^{-i}_{cmt}} + \alpha_{ct}\eta^{O}_{cmt}. \tag{A10}$$

The corresponding residualized specification aligns with equation A9 and thus identifies σ^{F}:

$$p^{\perp}_{icmt} = \sigma\alpha_{ct}\eta^{O}_{cmt} + \varepsilon_{icmt} \tag{A11}$$

In contrast, a naive approach that treats the observed peers' ideology as representative of the full peer group yields:

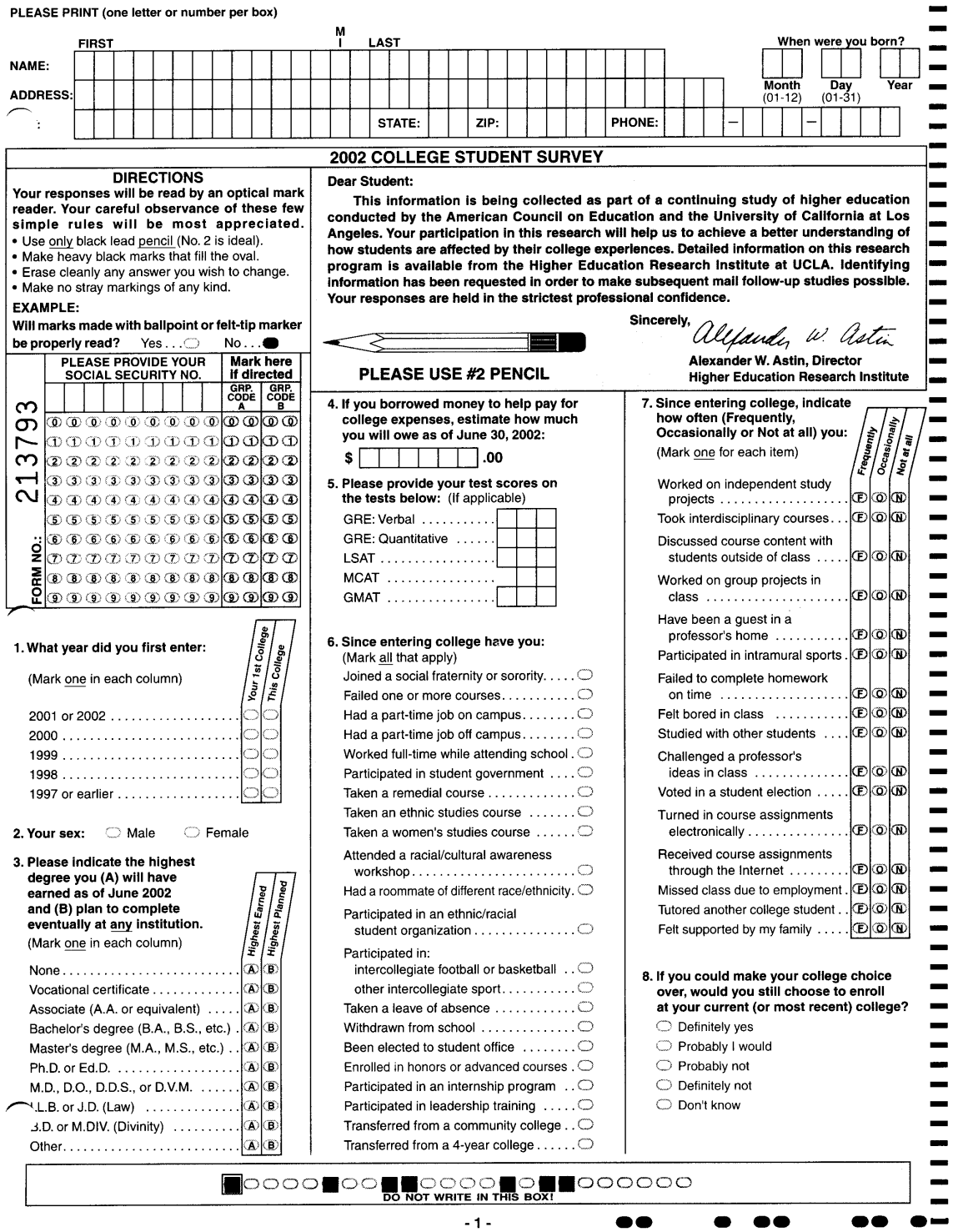
$$p^{\perp}_{icmt} = \sigma^{O}\eta^{O}_{cmt} + \varepsilon_{icmt} \tag{A12}$$

which inflates the treatment by $1/\alpha_{ct}$, ignores unobserved peers' idiosyncratic errors, and introduces standard attenuation bias.

[34] Coverage is calculated as the number of surveyed students in college c and year t divided by the total number of first-year BA enrollments based on IPEDS administrative data.

Appendix G Additional Figures and Tables

Figure G.1: 2002 College Student Survey Questionnaire



PLEASE PRINT (one letter or number per box)

NAME: FIRST | M I | LAST

When were you born? Month (01-12) Day (01-31) Year

ADDRESS:

STATE: ZIP: PHONE: – –

2002 COLLEGE STUDENT SURVEY

DIRECTIONS

Your responses will be read by an optical mark reader. Your careful observance of these few simple rules will be most appreciated.

- Use only black lead pencil (No. 2 is ideal).
- Make heavy black marks that fill the oval.
- Erase cleanly any answer you wish to change.
- Make no stray markings of any kind.

EXAMPLE:

Will marks made with ballpoint or felt-tip marker be properly read? Yes . . . ○ No . . . ●

PLEASE USE #2 PENCIL

Dear Student:

This information is being collected as part of a continuing study of higher education conducted by the American Council on Education and the University of California at Los Angeles. Your participation in this research will help us to achieve a better understanding of how students are affected by their college experiences. Detailed information on this research program is available from the Higher Education Research Institute at UCLA. Identifying information has been requested in order to make subsequent mail follow-up studies possible. Your responses are held in the strictest professional confidence.

Sincerely,

Alexander W. Astin

Alexander W. Astin, Director
Higher Education Research Institute

FORM NO: 213793

PLEASE PROVIDE YOUR SOCIAL SECURITY NO.

Mark here if directed

GRP. CODE A | GRP. CODE B

1. What year did you first enter:

(Mark one in each column)

	Your 1st College	This College
2001 or 2002	○	○
2000	○	○
1999	○	○
1998	○	○
1997 or earlier	○	○

2. Your sex: ○ Male ○ Female

3. Please indicate the highest degree you (A) will have earned as of June 2002 and (B) plan to complete eventually at any institution.

(Mark one in each column)

	Highest Earned	Highest Planned
None	(A)	(B)
Vocational certificate	(A)	(B)
Associate (A.A. or equivalent)	(A)	(B)
Bachelor's degree (B.A., B.S., etc.)	(A)	(B)
Master's degree (M.A., M.S., etc.)	(A)	(B)
Ph.D. or Ed.D.	(A)	(B)
M.D., D.O., D.D.S., or D.V.M.	(A)	(B)
L.L.B. or J.D. (Law)	(A)	(B)
D.D. or M.DIV. (Divinity)	(A)	(B)
Other	(A)	(B)

4. If you borrowed money to help pay for college expenses, estimate how much you will owe as of June 30, 2002:

$ .00

5. Please provide your test scores on the tests below: (If applicable)

GRE: Verbal

GRE: Quantitative

LSAT

MCAT

GMAT

6. Since entering college have you:

(Mark all that apply)

- Joined a social fraternity or sorority. ○
- Failed one or more courses. ○
- Had a part-time job on campus. ○
- Had a part-time job off campus. ○
- Worked full-time while attending school. ○
- Participated in student government ○
- Taken a remedial course ○
- Taken an ethnic studies course ○
- Taken a women's studies course ○
- Attended a racial/cultural awareness workshop ○
- Had a roommate of different race/ethnicity. ○
- Participated in an ethnic/racial student organization ○
- Participated in: intercollegiate football or basketball ○
- other intercollegiate sport ○
- Taken a leave of absence ○
- Withdrawn from school ○
- Been elected to student office ○
- Enrolled in honors or advanced courses ○
- Participated in an internship program ○
- Participated in leadership training ○
- Transferred from a community college ○
- Transferred from a 4-year college ○

7. Since entering college, indicate how often (Frequently, Occasionally or Not at all) you:

(Mark one for each item)

	Frequently	Occasionally	Not at all
Worked on independent study projects	(F)	(O)	(N)
Took interdisciplinary courses	(F)	(O)	(N)
Discussed course content with students outside of class	(F)	(O)	(N)
Worked on group projects in class	(F)	(O)	(N)
Have been a guest in a professor's home	(F)	(O)	(N)
Participated in intramural sports	(F)	(O)	(N)
Failed to complete homework on time	(F)	(O)	(N)
Felt bored in class	(F)	(O)	(N)
Studied with other students	(F)	(O)	(N)
Challenged a professor's ideas in class	(F)	(O)	(N)
Voted in a student election	(F)	(O)	(N)
Turned in course assignments electronically	(F)	(O)	(N)
Received course assignments through the Internet	(F)	(O)	(N)
Missed class due to employment	(F)	(O)	(N)
Tutored another college student	(F)	(O)	(N)
Felt supported by my family	(F)	(O)	(N)

8. If you could make your college choice over, would you still choose to enroll at your current (or most recent) college?

- ○ Definitely yes
- ○ Probably I would
- ○ Probably not
- ○ Definitely not
- ○ Don't know

DO NOT WRITE IN THIS BOX!

- 1 -

9. Please rate your satisfaction with your current (or most recent) college on each of the aspects of campus life listed below:
(Mark one in each row)

	Very Satisfied	Satisfied	Neutral	Dissatisfied	Can't Rate/ No Experience
General education or core curriculum courses	V	S	N	D	?
Science and mathematics courses	V	S	N	D	?
Humanities courses	V	S	N	D	?
Social science courses	V	S	N	D	?
Courses in your major field	V	S	N	D	?
Relevance of coursework to everyday life	V	S	N	D	?
Overall quality of instruction	V	S	N	D	?
Laboratory facilities and equipment	V	S	N	D	?
Library facilities	V	S	N	D	?
Computer facilities	V	S	N	D	?
Quality of computer training/assistance	V	S	N	D	?
Availability of Internet access	V	S	N	D	?
Sense of community on campus	V	S	N	D	?
Tutoring or other academic assistance	V	S	N	D	?
Academic advising	V	S	N	D	?
Career counseling and advising	V	S	N	D	?
Student housing	V	S	N	D	?
Financial aid services	V	S	N	D	?
Amount of contact with faculty	V	S	N	D	?
Opportunities for community service	V	S	N	D	?
Job placement services for students	V	S	N	D	?
Campus health services	V	S	N	D	?
Class size	V	S	N	D	?
Interaction with other students	V	S	N	D	?
Ability to find a faculty or staff mentor	V	S	N	D	?
Leadership opportunities	V	S	N	D	?
Recreational facilities	V	S	N	D	?
Overall college experience	V	S	N	D	?

10. Please indicate your enrollment status below:
(Mark one)

- ○ Full-time undergraduate
- ○ Part-time undergraduate
- ○ Graduate student
- ○ Not enrolled

11. Mark the one oval that best describes your undergraduate grade average.

- ○ A (3.75 - 4.0)
- ○ A-, B+ (3.25 - 3.74)
- ○ B (2.75 - 3.24)
- ○ B-, C+ (2.25 - 2.74)
- ○ C (1.75 - 2.24)
- ○ C- or less (below 1.75)

12. How would you characterize your political views?
(Mark one)

- ○ Far left
- ○ Liberal
- ○ Middle-of-the-road
- ○ Conservative
- ○ Far right

13. Please indicate the ethnic background of yourself, your father, and your mother.
(Mark all that apply in each column)

	You	Father	Mother
White/Caucasian	○	○	○
African American/Black	○	○	○
American Indian/Alaska Native	○	○	○
Asian American/Asian	○	○	○
Native Hawaiian/Pacific Islander	○	○	○
Mexican American/Chicano	○	○	○
Puerto Rican	○	○	○
Other Latino	○	○	○
Other	○	○	○

14. Please mark your probable career/occupation below:

- Accountant or actuary ○
- Actor or entertainer ○
- Architect or urban planner ○
- Artist ○
- Business (clerical) ○
- Business executive (management, administrator) ○
- Business owner or proprietor ○
- Business salesperson or buyer ○
- Clergy (minister, priest) ○
- Clergy (other religious) ○
- Clinical psychologist ○
- College administrator/staff ○
- College teacher ○
- Computer programmer or analyst ○
- Conservationist or forester ○
- Dentist (including orthodontist) ○
- *Dietitian or home economist* ○
- Engineer ○
- Farmer or rancher ○
- Foreign service worker (including diplomat) ○
- Homemaker (full-time) ○
- *Interior decorator* (including designer) ○
- Lab technician or hygienist ○
- Law enforcement officer ○
- Lawyer (attorney) or judge ○
- Military service (career) ○
- Musician (performer, composer) ○
- Nurse ○
- Optometrist ○
- Pharmacist ○
- Physician ○
- Policymaker/government ○
- School counselor ○
- School principal or superintendent ○
- Scientific researcher ○
- Social, welfare or recreation worker ○
- Therapist (physical, occupational, speech) ○
- Teacher or administrator (elementary) ○
- Teacher or administrator (secondary) ○
- Veterinarian ○
- Writer or journalist ○
- Skilled trades ○
- Other ○
- Undecided ○

15. For the activities listed below, please indicate how often (Frequently, Occasionally, or Not at all) you engaged in each during the past year.
(Mark one in each row)

	Frequently	Occasionally	Not at all
Smoked cigarettes	F	O	N
Felt lonely or homesick	F	O	N
Socialized with someone of another racial/ethnic group	F	O	N
Felt depressed	F	O	N
Felt overwhelmed by all I had to do	F	O	N
Attended a religious service	F	O	N
Drank beer	F	O	N
Drank wine or liquor	F	O	N
Performed volunteer work	F	O	N
Participated in organized demonstrations	F	O	N
Discussed politics	F	O	N
Overslept and missed class or appointment	F	O	N
Sought personal counseling	F	O	N
Visited an art gallery or museum	F	O	N
Discussed religion	F	O	N
Read the editorial page in the daily newspaper	F	O	N

16. During the past year, how much time did you spend during a typical week doing the following activities?
(Mark one in each row)

	Hours Per Week							
	None	Less than 1 hour	1-2	3-5	6-10	11-15	16-20	Over 20
Studying/homework	○	○	○	○	○	○	○	○
Socializing with friends	○	○	○	○	○	○	○	○
Talking with faculty outside of class	○	○	○	○	○	○	○	○
Exercising/sports	○	○	○	○	○	○	○	○
Partying	○	○	○	○	○	○	○	○
Working (for pay)	○	○	○	○	○	○	○	○
Volunteer work	○	○	○	○	○	○	○	○
Student clubs/groups	○	○	○	○	○	○	○	○
Watching TV	○	○	○	○	○	○	○	○
Housework/childcare	○	○	○	○	○	○	○	○
Reading for pleasure	○	○	○	○	○	○	○	○
Using a personal computer	○	○	○	○	○	○	○	○
Commuting	○	○	○	○	○	○	○	○
Playing video games	○	○	○	○	○	○	○	○
Prayer/meditation	○	○	○	○	○	○	○	○
Classes/labs	○	○	○	○	○	○	○	○

17. What do you plan to be doing six months from now? (Mark all that apply)

- ○ Attending undergraduate college full-time
- ○ Attending undergraduate college part-time
- ○ Attending graduate/professional school
- ○ Working full-time
- ○ Working part-time
- ○ Participating in a community service organization
- ○ Serving in the Armed Forces
- ○ Attending a vocational training program
- ○ Traveling, hosteling, or backpacking
- ○ Doing volunteer work
- ○ Staying at home to be with or start a family
- ○ No current plans

18. Compared with when you first started college, how would you now describe your:

(Mark one for each item)

	Much Stronger	Stronger	No Change	Weaker	Much Weaker
General knowledge	5	4	3	2	1
Analytical and problem-solving skills	5	4	3	2	1
Knowledge of a particular field or discipline	5	4	3	2	1
Ability to think critically	5	4	3	2	1
Foreign language ability	5	4	3	2	1
Knowledge of people from different races/cultures	5	4	3	2	1
Religious beliefs and convictions	5	4	3	2	1
Leadership abilities	5	4	3	2	1
Interpersonal skills	5	4	3	2	1
Ability to get along with people of different races/cultures	5	4	3	2	1
Understanding of the problems facing your community	5	4	3	2	1
Understanding of social problems facing our nation	5	4	3	2	1
Writing skills	5	4	3	2	1
Public speaking ability	5	4	3	2	1
Ability to work cooperatively	5	4	3	2	1
Mathematical skills	5	4	3	2	1
Reading speed and comprehension	5	4	3	2	1
Computer skills	5	4	3	2	1

19. Indicate the importance to you personally of each of the following:

(Mark one for each item)

	Essential	Very Important	Somewhat Important	Not Important
Becoming accomplished in one of the performing arts (acting, dancing, etc.)	E	V	S	N
Becoming an authority in my field	E	V	S	N
Obtaining recognition from my colleagues for contributions to my special field	E	V	S	N
Influencing the political structure	E	V	S	N
Influencing social values	E	V	S	N
Raising a family	E	V	S	N
Having administrative responsibility for the work of others	E	V	S	N
Being very well off financially	E	V	S	N
Helping others who are in difficulty	E	V	S	N
Making a theoretical contribution to science	E	V	S	N
Writing original works (poems, novels, short stories, etc.)	E	V	S	N
Creating artistic work (painting, sculpture, decorating, etc.)	E	V	S	N
Becoming successful in a business of my own	E	V	S	N
Becoming involved in programs to clean up the environment	E	V	S	N
Developing a meaningful philosophy of life	E	V	S	N
Participating in a community action program	E	V	S	N
Helping to promote racial understanding	E	V	S	N
Keeping up to date with political affairs	E	V	S	N
Becoming a community leader	E	V	S	N

20. Your current religious preference: (Mark one)

Baptist	○	Presbyterian	○
Buddhist	○	Quaker	○
Eastern Orthodox	○	Roman Catholic	○
Episcopal	○	Seventh Day Adventist	○
Islamic	○	United Church of Christ	○
Jewish	○	Other Christian	○
LDS (Mormon)	○	Other Far Eastern Religion	○
Lutheran	○	Other Religion	○
Methodist	○	None	○

21. During the past year, how often did you:

(Mark one for each item)

	Daily	2 or 3 times/week	Once a week	1 or 2 times/month	Never
Communicate via e-mail:					
with faculty	5	4	3	2	1
with students at this college	5	4	3	2	1
with students at other colleges	5	4	3	2	1
with other friends or acquaintances	5	4	3	2	1
with your family	5	4	3	2	1
Participate in class discussions via e-mail/Internet	5	4	3	2	1
Use the Internet for research or homework	5	4	3	2	1
Use the Internet for nonacademic reasons	5	4	3	2	1

22. Do you own a personal computer? ○ Yes ○ No

23. Rate yourself on each of the following traits as compared with the average person your age. We want the most accurate estimate of how you see yourself.

(Mark one in each row)

	Highest 10%	Above Average	Average	Below Average	Lowest 10%
Academic ability	○	○	○	○	○
Artistic ability	○	○	○	○	○
Athletic ability	○	○	○	○	○
Competitiveness	○	○	○	○	○
Cooperativeness	○	○	○	○	○
Creativity	○	○	○	○	○
Drive to achieve	○	○	○	○	○
Emotional health	○	○	○	○	○
Leadership ability	○	○	○	○	○
Mathematical ability	○	○	○	○	○
Physical health	○	○	○	○	○
Popularity	○	○	○	○	○
Public speaking ability	○	○	○	○	○
Self-confidence (intellectual)	○	○	○	○	○
Self-confidence (social)	○	○	○	○	○
Self-understanding	○	○	○	○	○
Spirituality	○	○	○	○	○
Understanding of others	○	○	○	○	○
Writing ability	○	○	○	○	○
Religiousness/Religiosity	○	○	○	○	○

24. How often have professors at your current (or most recent) college provided you with:

(Mark one for each item)

	Frequently	Occasionally	Not at all
Encouragement to pursue graduate/professional study	○	○	○
An opportunity to work on a research project	○	○	○
Advice and guidance about your educational program	○	○	○
Respect (treated you like a colleague/peer)	○	○	○
An opportunity to publish	○	○	○
Emotional support and encouragement	○	○	○
A letter of recommendation	○	○	○
Assistance to improve your study skills	○	○	○
Negative feedback about your academic work	○	○	○
Intellectual challenge and stimulation	○	○	○
An opportunity to discuss coursework outside of class	○	○	○
Help in achieving your professional goals	○	○	○



25. Below is a list of different major fields.
(Mark only one in each column)

Ⓤ Undergraduate major (final or most recent)
Ⓖ Graduate major (omit if you do not plan to go to graduate school)

ARTS AND HUMANITIES
- Art, fine and applied ..... Ⓤ Ⓖ
- English (language and literature) ..... Ⓤ Ⓖ
- History ..... Ⓤ Ⓖ
- Journalism ..... Ⓤ Ⓖ
- Language and Literature (except English) ..... Ⓤ Ⓖ
- Music ..... Ⓤ Ⓖ
- Philosophy ..... Ⓤ Ⓖ
- Speech ..... Ⓤ Ⓖ
- Theater or Drama ..... Ⓤ Ⓖ
- Theology or Religion .... Ⓤ Ⓖ
- Other Arts and Humanities ..... Ⓤ Ⓖ

BIOLOGICAL SCIENCE
- Biology (general) ..... Ⓤ Ⓖ
- Biochemistry or Biophysics ..... Ⓤ Ⓖ
- Botany ..... Ⓤ Ⓖ
- Environmental Science... Ⓤ Ⓖ
- Marine (Life) Science .... Ⓤ Ⓖ
- Microbiology or Bacteriology ..... Ⓤ Ⓖ
- Zoology ..... Ⓤ Ⓖ
- Other Biological Science ..... Ⓤ Ⓖ

BUSINESS
- Accounting ..... Ⓤ Ⓖ
- Business Administration (general) ..... Ⓤ Ⓖ
- Finance ..... Ⓤ Ⓖ
- International Business ... Ⓤ Ⓖ
- Marketing ..... Ⓤ Ⓖ
- Management ..... Ⓤ Ⓖ
- Secretarial Studies ..... Ⓤ Ⓖ
- Other Business ..... Ⓤ Ⓖ

EDUCATION
- Business Education ..... Ⓤ Ⓖ
- Elementary Education ... Ⓤ Ⓖ
- Music or Art Education ... Ⓤ Ⓖ
- Physical Education or Recreation ..... Ⓤ Ⓖ
- Secondary Education .... Ⓤ Ⓖ
- Special Education ..... Ⓤ Ⓖ
- Other Education ..... Ⓤ Ⓖ

ENGINEERING
- Aero-/Astronautical Engineering ..... Ⓤ Ⓖ
- Civil Engineering..... Ⓤ Ⓖ
- Chemical Engineering ... Ⓤ Ⓖ
- Electrical or Electronic Engineering ..... Ⓤ Ⓖ
- Industrial Engineering.... Ⓤ Ⓖ
- Mechanical Engineering.. Ⓤ Ⓖ
- Other Engineering..... Ⓤ Ⓖ

PHYSICAL SCIENCE
- Astronomy ..... Ⓤ Ⓖ
- Atmospheric Science (incl. Meteorology) .... Ⓤ Ⓖ
- Chemistry ..... Ⓤ Ⓖ
- Earth Science ..... Ⓤ Ⓖ
- Marine Science (incl. Oceanography) ..... Ⓤ Ⓖ
- Mathematics ..... Ⓤ Ⓖ
- Physics ..... Ⓤ Ⓖ
- Statistics ..... Ⓤ Ⓖ
- Other Physical Science .. Ⓤ Ⓖ

PROFESSIONAL
- Architecture or Urban Planning ..... Ⓤ Ⓖ
- Home Economics ..... Ⓤ Ⓖ
- Health Technology (medical, dental, laboratory).. Ⓤ Ⓖ
- Law ..... Ⓖ
- Library/Archival Science . Ⓤ Ⓖ
- Medicine, Dentistry, Veterinarian ..... Ⓤ Ⓖ
- Nursing ..... Ⓤ Ⓖ
- Pharmacy ..... Ⓤ Ⓖ
- Therapy (occupational, physical, speech) ..... Ⓤ Ⓖ
- Other Professional ..... Ⓤ Ⓖ

SOCIAL SCIENCE
- Anthropology..... Ⓤ Ⓖ
- Economics ..... Ⓤ Ⓖ
- Ethnic Studies ..... Ⓤ Ⓖ
- Geography..... Ⓤ Ⓖ
- Political Science (gov't., international relations).. Ⓤ Ⓖ
- Psychology ..... Ⓤ Ⓖ
- Social Work ..... Ⓤ Ⓖ
- Sociology ..... Ⓤ Ⓖ
- Women's Studies ..... Ⓤ Ⓖ
- Other Social Science .... Ⓤ Ⓖ

TECHNICAL
- Building Trades ..... Ⓤ Ⓖ
- Data Processing or Computer Programming Ⓤ Ⓖ
- Drafting or Design ..... Ⓤ Ⓖ
- Electronics ..... Ⓤ Ⓖ
- Mechanics ..... Ⓤ Ⓖ
- Other Technical..... Ⓤ Ⓖ

OTHER FIELDS
- Agriculture ..... Ⓤ Ⓖ
- Communications (radio, TV, etc.) ..... Ⓤ Ⓖ
- Computer Science ..... Ⓤ Ⓖ
- Forestry ..... Ⓤ Ⓖ
- Law Enforcement ..... Ⓤ Ⓖ
- Military Science ..... Ⓤ Ⓖ
- Other Field ..... Ⓤ Ⓖ
- Undecided ..... Ⓤ Ⓖ

26. Please indicate your agreement with each of the following statements.
(Mark one for each item)

	Agree Strongly	Agree Somewhat	Disagree Somewhat	Disagree Strongly
Abortion should be legal	4	3	2	1
The death penalty should be abolished	4	3	2	1
If two people really like each other, it's all right for them to have sex even if they've known each other for only a very short time	4	3	2	1
The activities of married women are best confined to the home and family	4	3	2	1
Marijuana should be legalized	4	3	2	1
It is important to have laws prohibiting homosexual relationships	4	3	2	1
Racial discrimination is no longer a major problem in America	4	3	2	1
Realistically, an individual can do little to bring about changes in our society	4	3	2	1
Wealthy people should pay a larger share of taxes than they do now	4	3	2	1
Colleges should prohibit racist/sexist speech on campus	4	3	2	1
Affirmative action in college admissions should be abolished	4	3	2	1
There is too much concern in the courts for the rights of criminals	4	3	2	1
The federal government should do more to control the sale of handguns	4	3	2	1
Same sex couples should have the right to legal marital status	4	3	2	1
Federal military spending should be increased	4	3	2	1
All federal and state documents should be printed in English only	4	3	2	1
Material on the Internet should be regulated by the government	4	3	2	1

27. Is English your native language? ○ Yes ○ No

28. Since entering college, how many of your courses have included community service/service learning?
○ None (skip to question 31) ○ One ○ Two or more

29. In your most recent course that included service, how often did the professor:
(Mark one for each item)

	Frequently	Occasionally	Not at all
Encourage class discussions	F	O	N
Deliver lectures	F	O	N
Connect the service experience to the course material	F	O	N
Require written reflections of your service experience	F	O	N
How often did you:			
Apply the course material to your service work	F	O	N
Feel that the service experience increased your understanding of the academic course material	F	O	N
Feel that your service made a difference	F	O	N

30. In this most recent course, community service was:
○ Required ○ Optional

31. Do you give the Higher Education Research Institute at UCLA permission to include your ID number should your college request the data for additional research analyses? ○ Yes ○ No

ADDITIONAL QUESTIONS: If you received an additional page of questions, please mark your answers below:

32. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	39. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	46. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
33. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	40. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	47. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
34. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	41. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	48. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
35. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	42. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	49. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
36. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	43. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	50. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
37. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	44. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	51. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
38. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	45. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	**THANK YOU!**

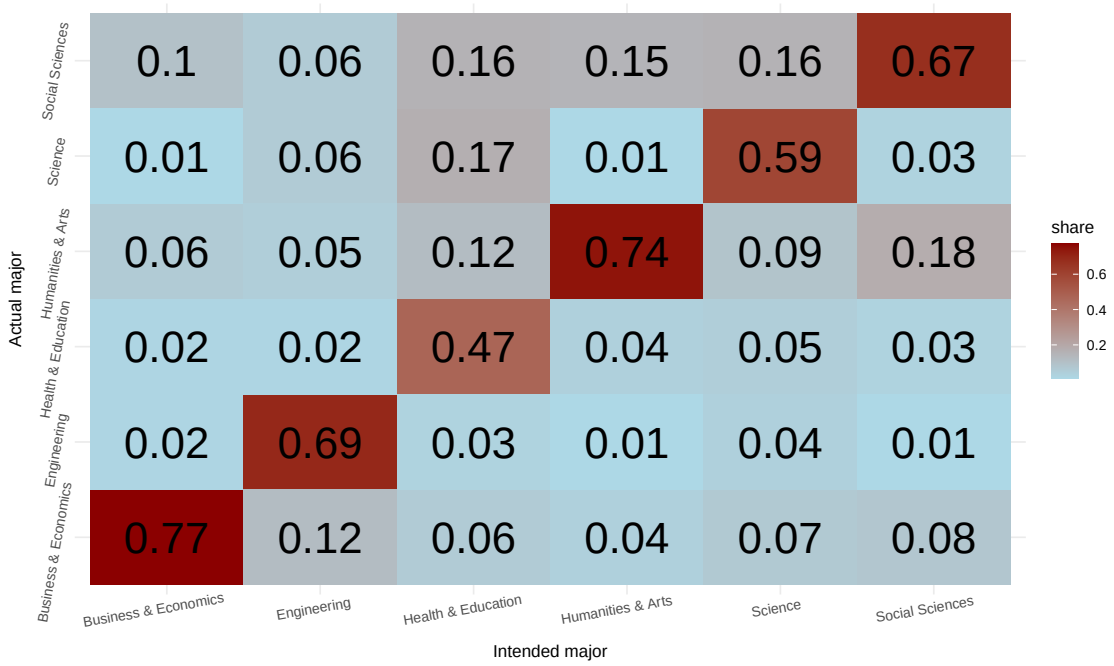
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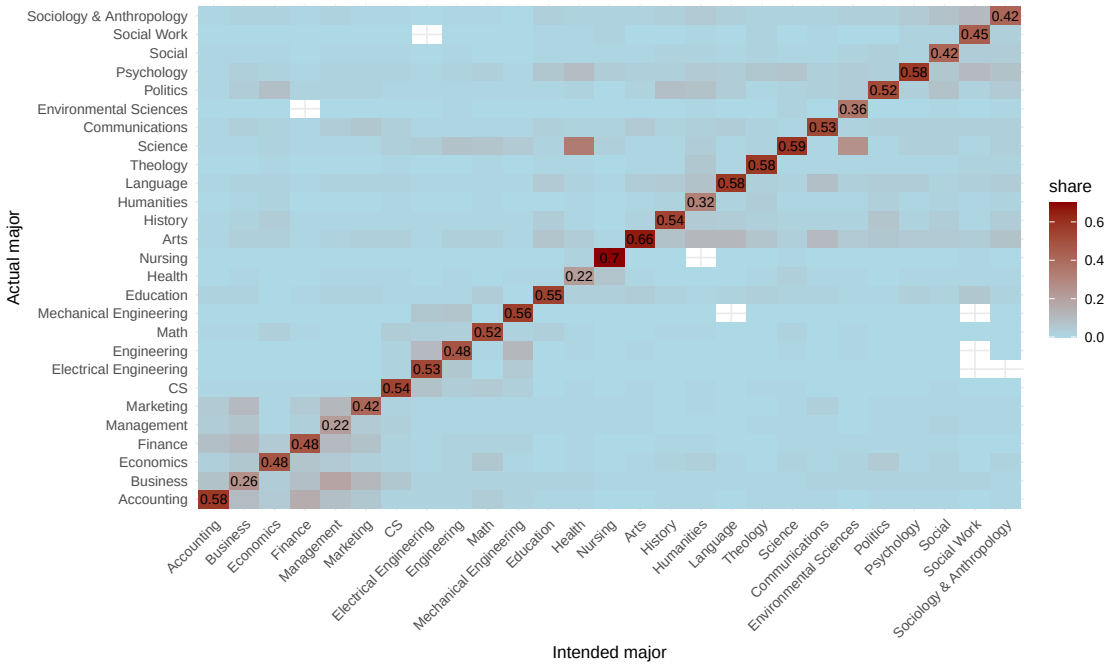
Note: This figure shows the complete 2002 College Student Survey questionnaire from the Cooperative Institutional Research Program (CIRP), conducted by the Higher Education Research Institute (HERI) of UCLA. This survey represents a typical College Senior survey used throughout our analysis period (1990-2015). The questionnaire captures students' demographic information, academic background, political attitudes, policy preferences, life goals, and college experiences. Key variables used in our analysis include political ideology (Question 12), intended major (Question 25), policy positions (Question 26), and various behavioral and preference measures throughout the survey.

Figure G.2: Intended and Actual Majors

(A) Broad Groups

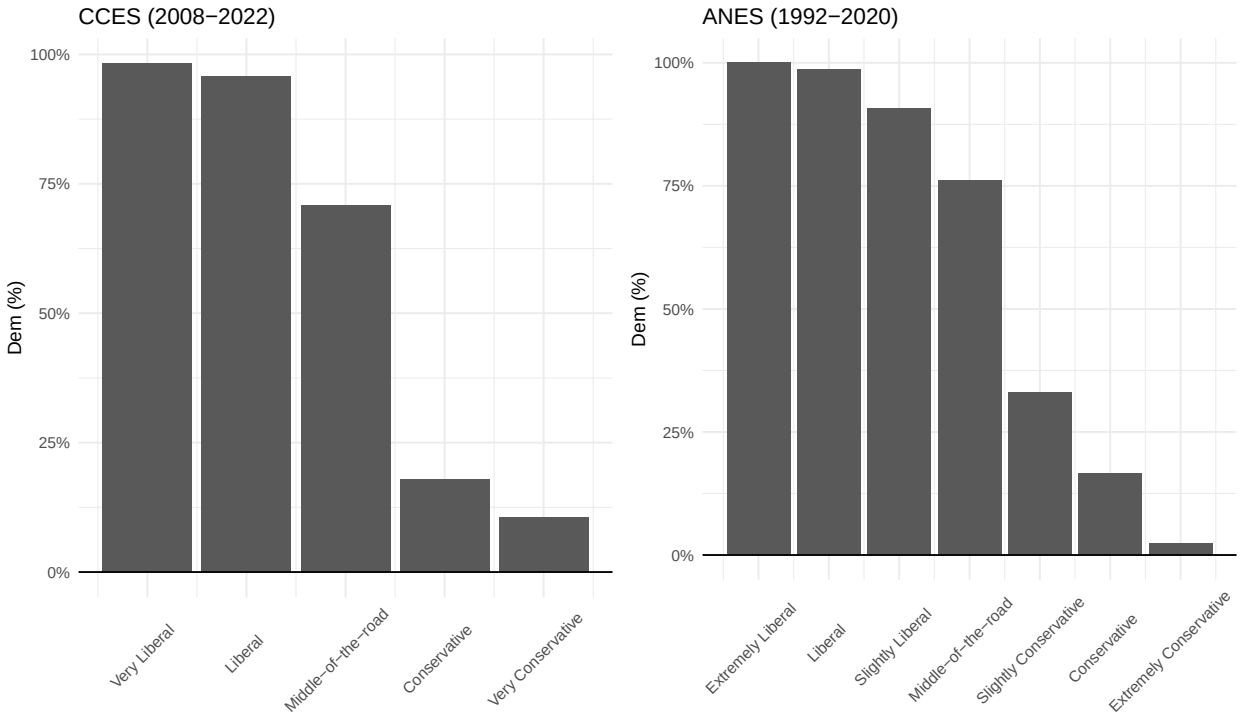


(B) Detailed Groups



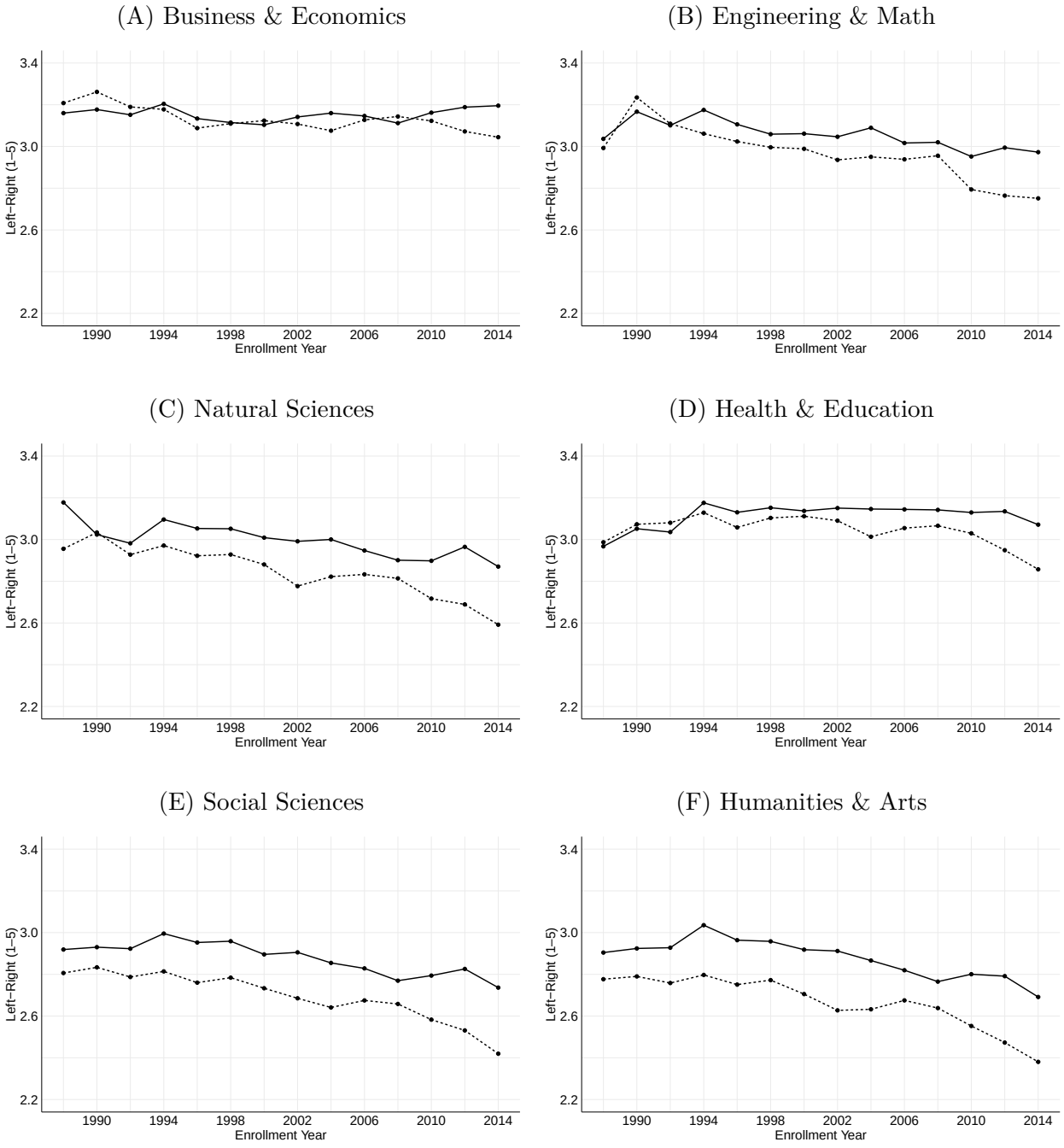
Note: This figure plots students' intended and actual majors. The sample includes all students in our sample with non-missing values for this variable, both in the entry and exit surveys (273,869 students). The first (second) panel shows the results in broad (detailed) major groups.

Figure G.3: Students' Ideology and Voting in National Surveys



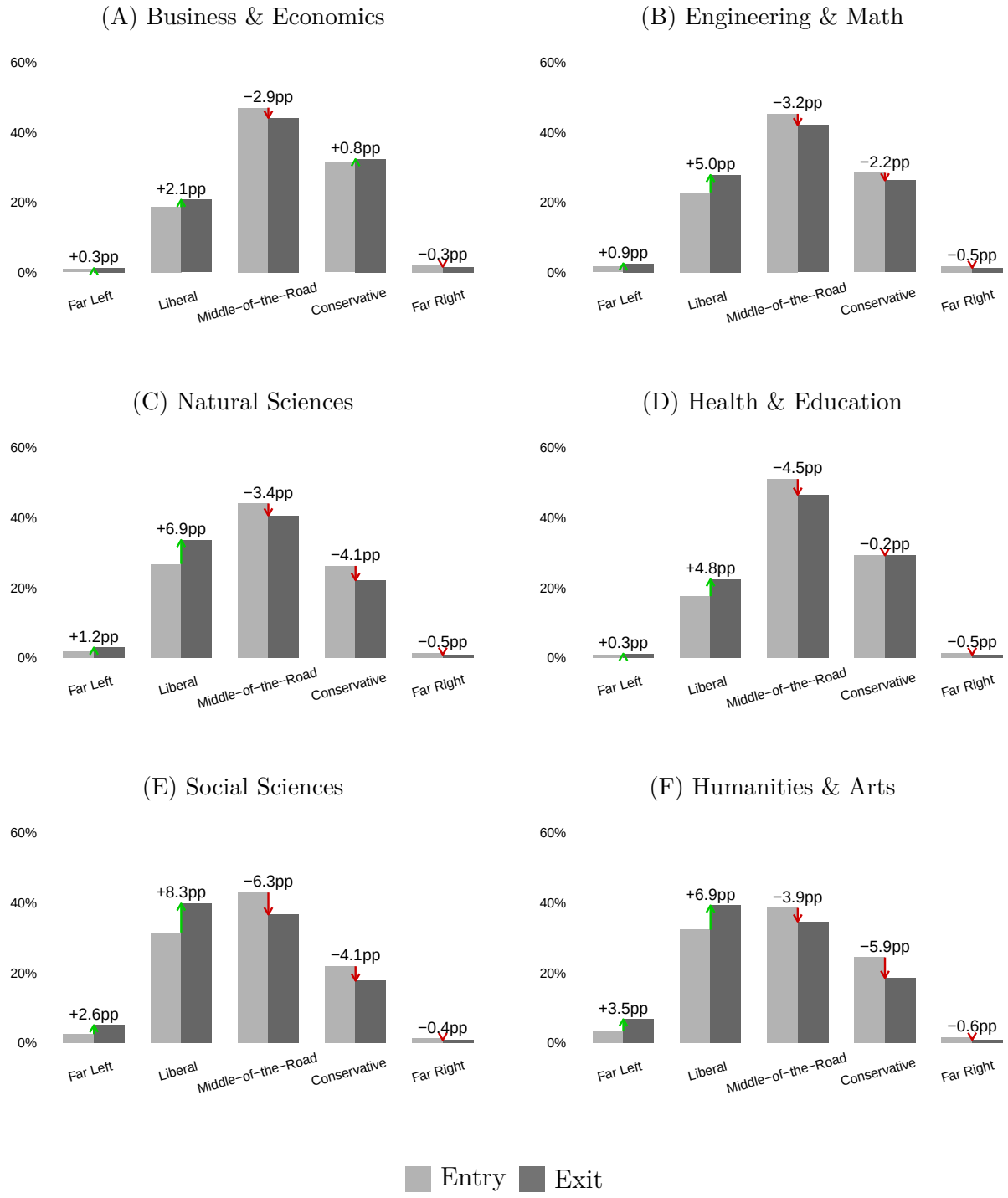
Note: This figure plots students' attitudes on a liberal-conservative scale and their voting in presidential elections. The sample includes all students in the CCES and ANES surveys with non-missing values for the scale ('How would you characterize your political views?') and voting in presidential elections variables. We focus on student responses in the ANES (American National Election Studies) and the CCES (Cooperative Election Study), both of which include an ideology scale and a presidential voting question. The larger CCES (n=11,570) aligns more closely with our surveys in query structure ('In general, how would you describe your own political viewpoint') but only dates back to 2006, limiting historical overlap. The ANES, older and with fewer students (n=345), employs a different ideology question ('We hear a lot of talk these days about liberals and conservatives. Here is a 7-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place yourself on this scale, or haven't you thought much about this.') and scale. The correlation between ideology and voting behavior is very high in both surveys and higher in the CCES (0.65) than in the ANES (0.59). The figure presents the distribution of voting to the democrat candidate in national elections in both surveys based on the ideology scale.

Figure G.4: Students' Political Ideology, HERI Survey, 1990–2015



Note: This figure plots students' political ideology at college entry (solid lines) and at graduation (dashed lines). The sample includes all students from these colleges who entered before 2016 and have non-missing ideology measures in both the entry and exit surveys (294,238 students). The 333 students who entered in 2016 are excluded. Political ideology is measured on a five-point scale: far right, conservative, middle-of-the-road, liberal, or far left. Panels A–F restrict the sample to students in specific major groups.

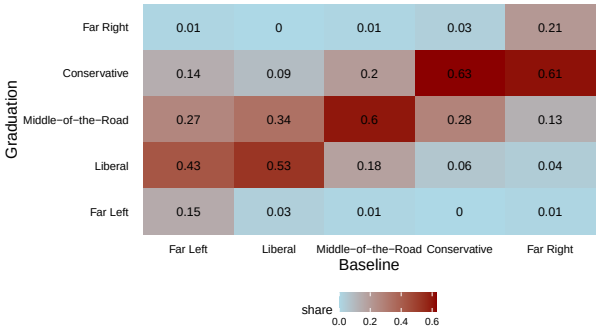
Figure G.5: Political Ideology Distributions Before and After College



Note: This figure plots the distribution of students' baseline (light grey) and graduation (dark grey) political ideology. The sample includes all students with non-missing values for this outcome in the baseline and graduation surveys, totaling 294,571 students. The samples in Panels A–F are restricted to students in specific groups of majors.

Figure G.6: Transitions in Political Ideology During College

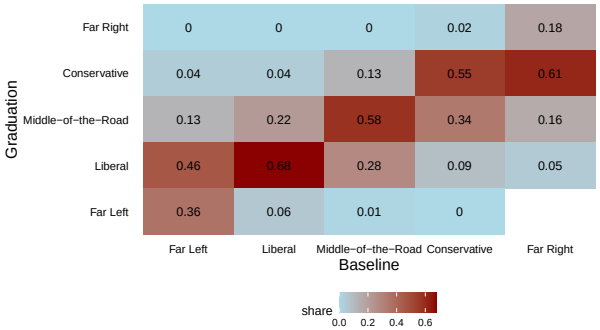
(A) Business & Economics



(B) Engineering & Math



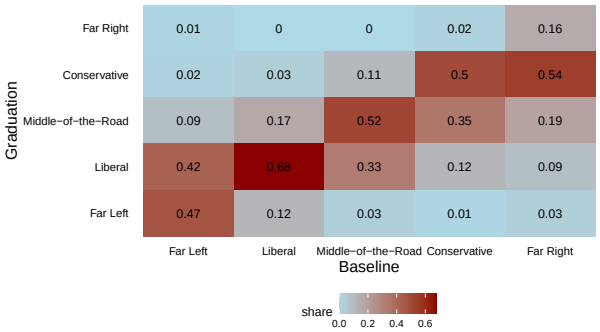
(C) Natural Sciences



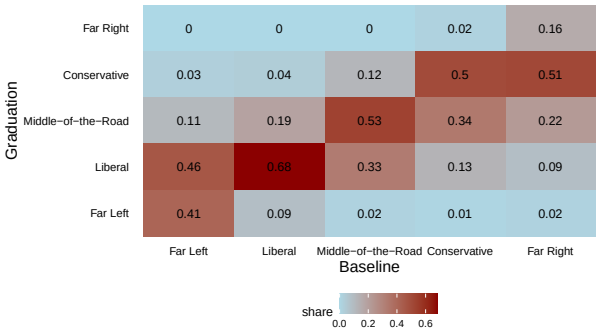
(D) Health & Education



(E) Humanities & Arts

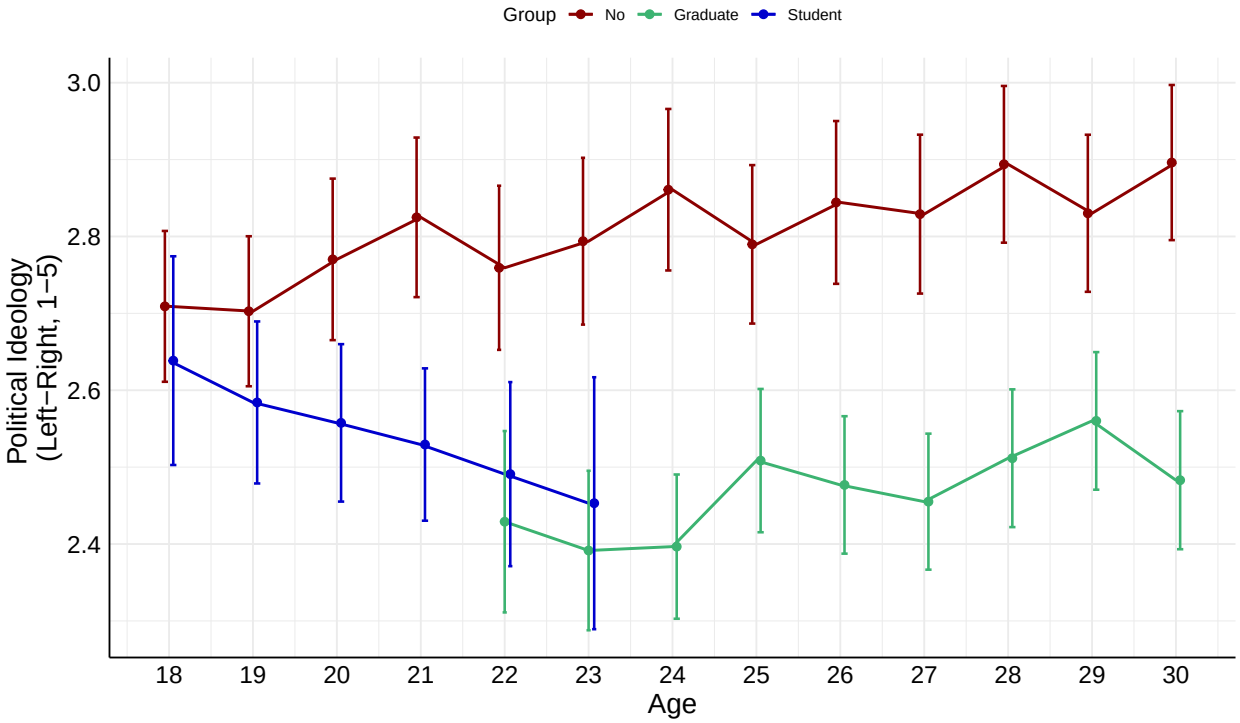


(F) Social Sciences



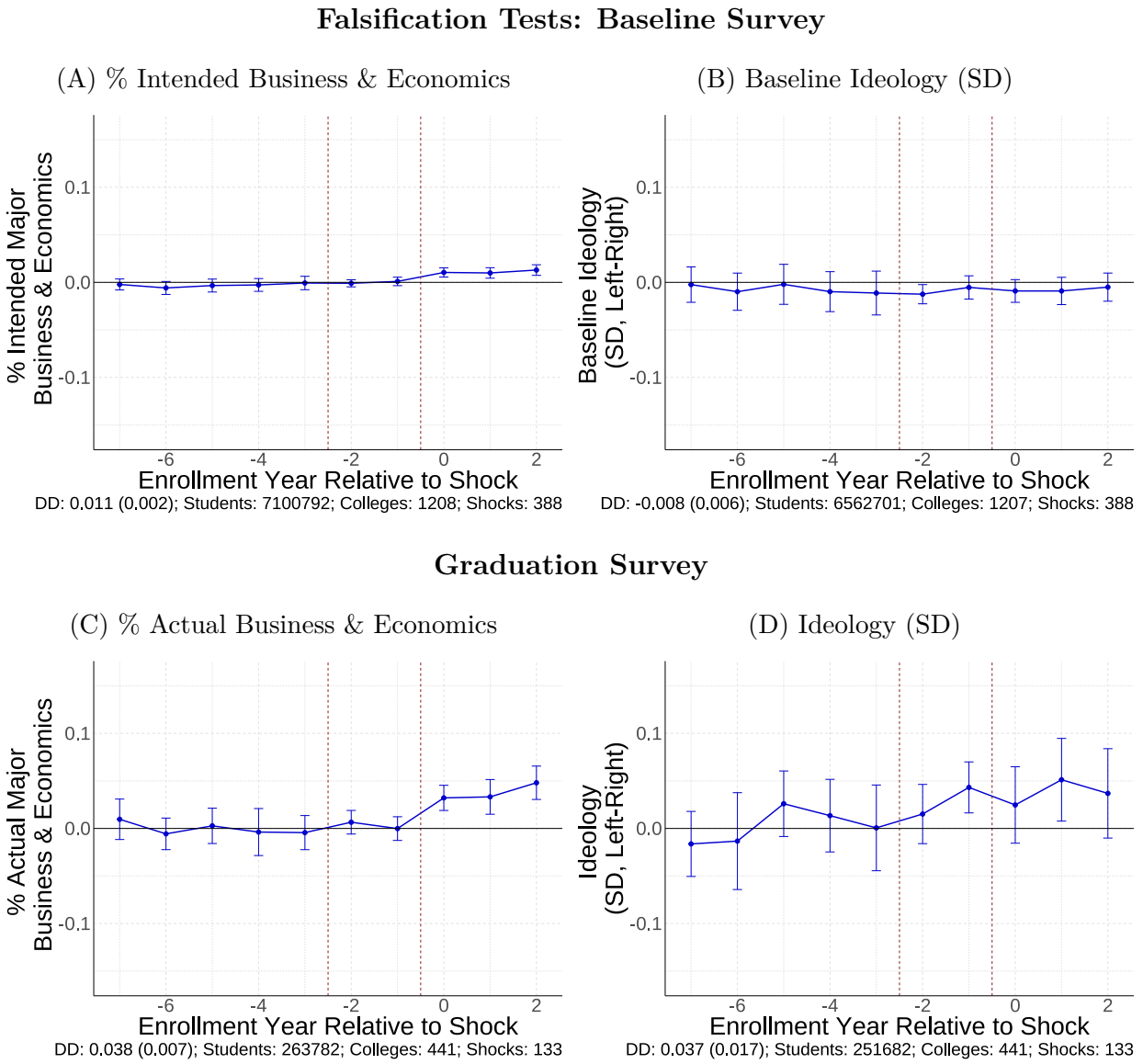
Note: This figure plots the changes in students' baseline and graduation political ideology. The sample includes all students with non-missing values for this outcome in the entry and exit surveys, totaling 294,571 students. The samples in panels A–F are restricted to those in specific groups of majors.

Figure G.7: Political Ideology by Age, Students and non-Students, CCES Survey



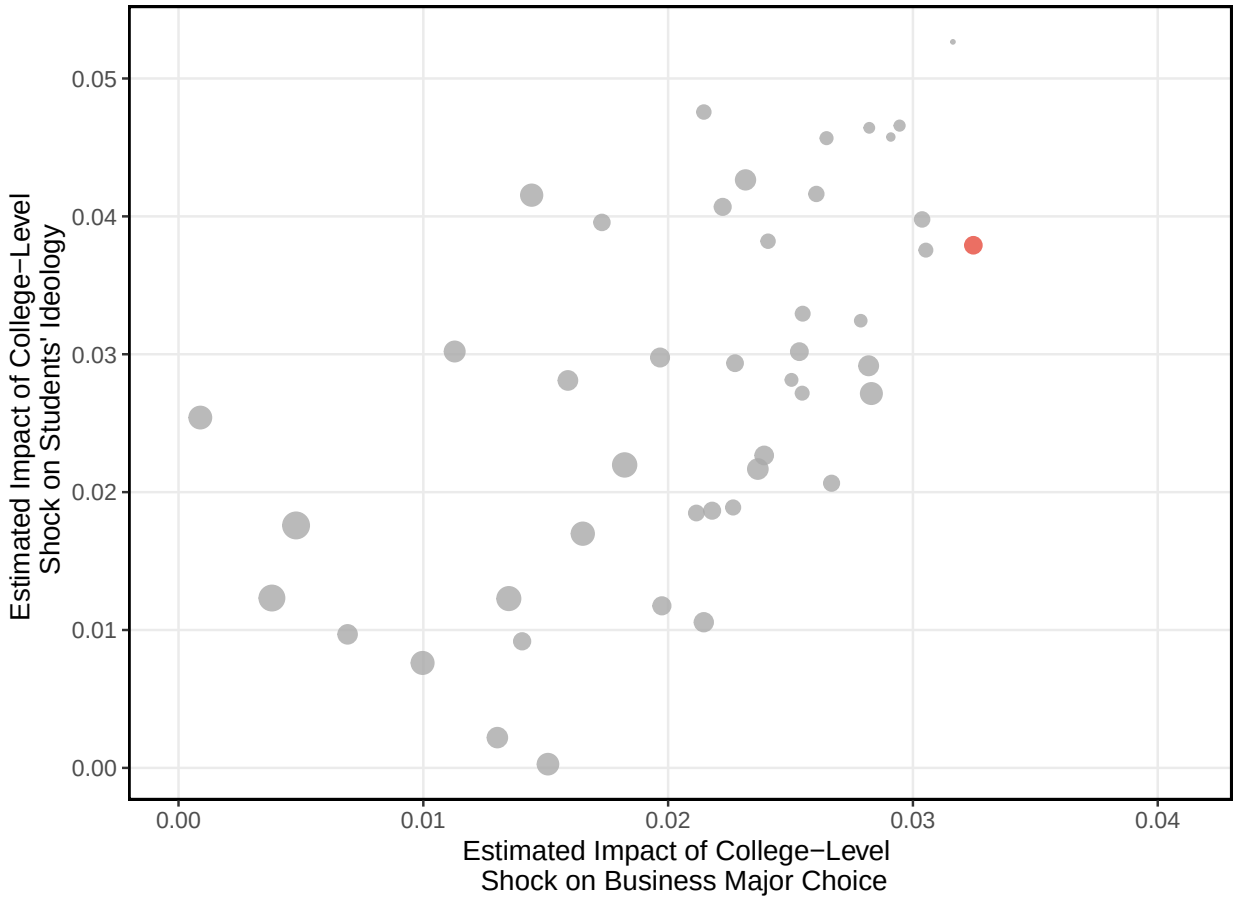
Note: This figure shows political ideology by age for college students (blue), college graduates (green) and individuals with no college education (high-school education or less, in red) in the CCES. Ideology is based on responses to "How would you characterize your political views?" on a 1-5 scale (from Very Liberal to Very Conservative). The trends are based on a linear regression with dummies for age interacted with a group dummy, controlling for state and year fixed effects. Error bars represent 95 percent confidence intervals based on robust standard errors. The sample includes 55,132 observations.

Figure G.8: Effects of College-Level Expansions in Economics & Business, Imputation Estimator



Note: These figures plot event-study coefficients estimated from Equation 3 using the imputation estimator of Borusyak et al. (2024), with standard errors clustered at the college level. Each panel also reports the pooled post-shock difference-in-differences estimate (DD), along with the number of students, colleges, and treated colleges in the estimation sample. The omitted time category is three periods before the event. We exclude the two following cohorts from the DD estimation because they may already be partially exposed to the shock. Panels A and B use baseline-survey outcomes and serve as falsification tests: intended business/economics major and baseline ideology. Panels C and D use graduation-survey outcomes: realized business/economics major and graduation ideology. Panels A—C are estimated without additional controls, while Panel D flexibly controls for baseline ideology.

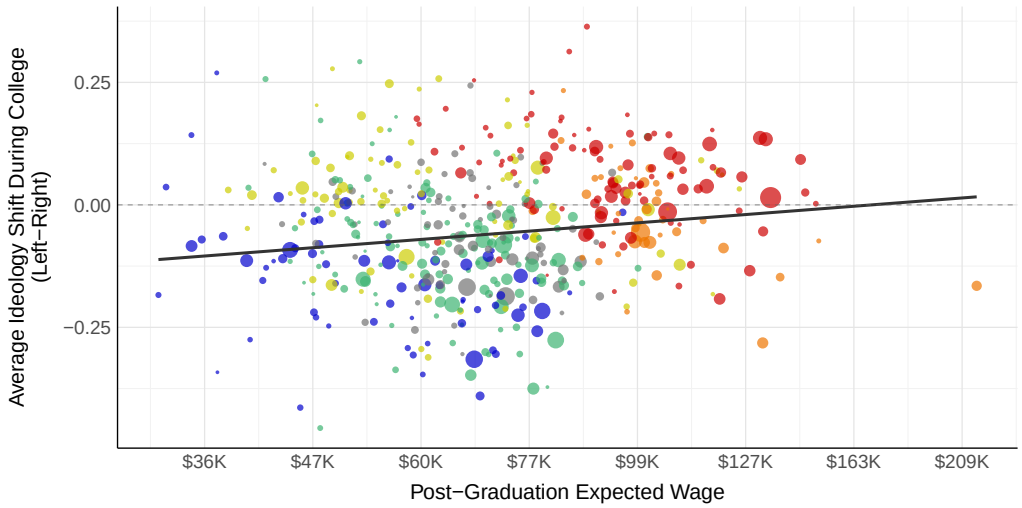
Figure G.9: Comparing Main Quasi-Experimental Results for Different Shocks Definitions



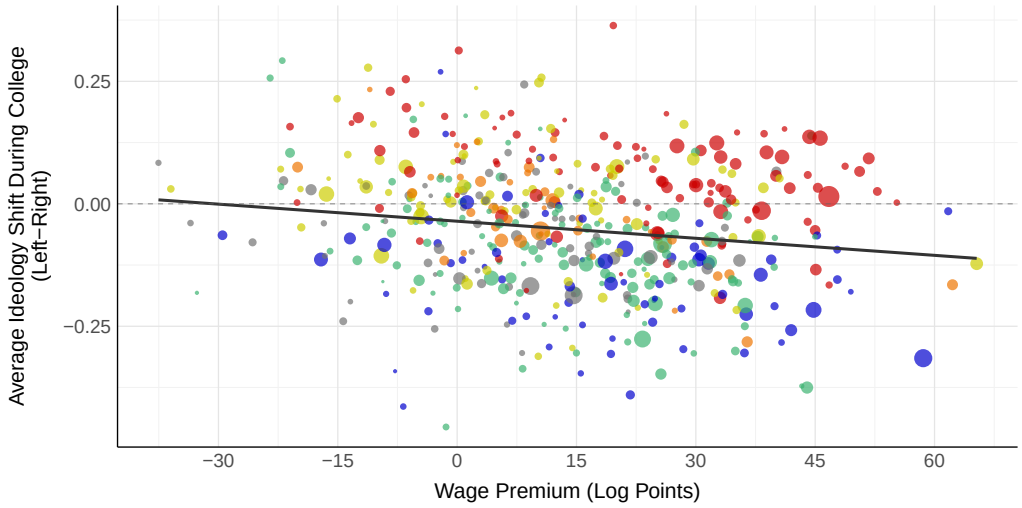
Note: Each point in the plot represents a distinct set of college-level shocks based on different definitions. The coordinates of each point correspond to estimates from the DID specification of equation 3. The x-axis displays the estimated effect on students selecting business and economics majors, while the y-axis shows the estimated effect on students' political ideology at graduation. All estimates are obtained using the two-stage imputation estimator developed by Gardner (2022). The larger red point highlights the primary shock specification used in the main analysis, the smaller grey points represent alternative shock definitions, and point size reflects the number of identified shocks.

Figure G.10: Political Ideology Shift and Expected Wages

(A) Levels

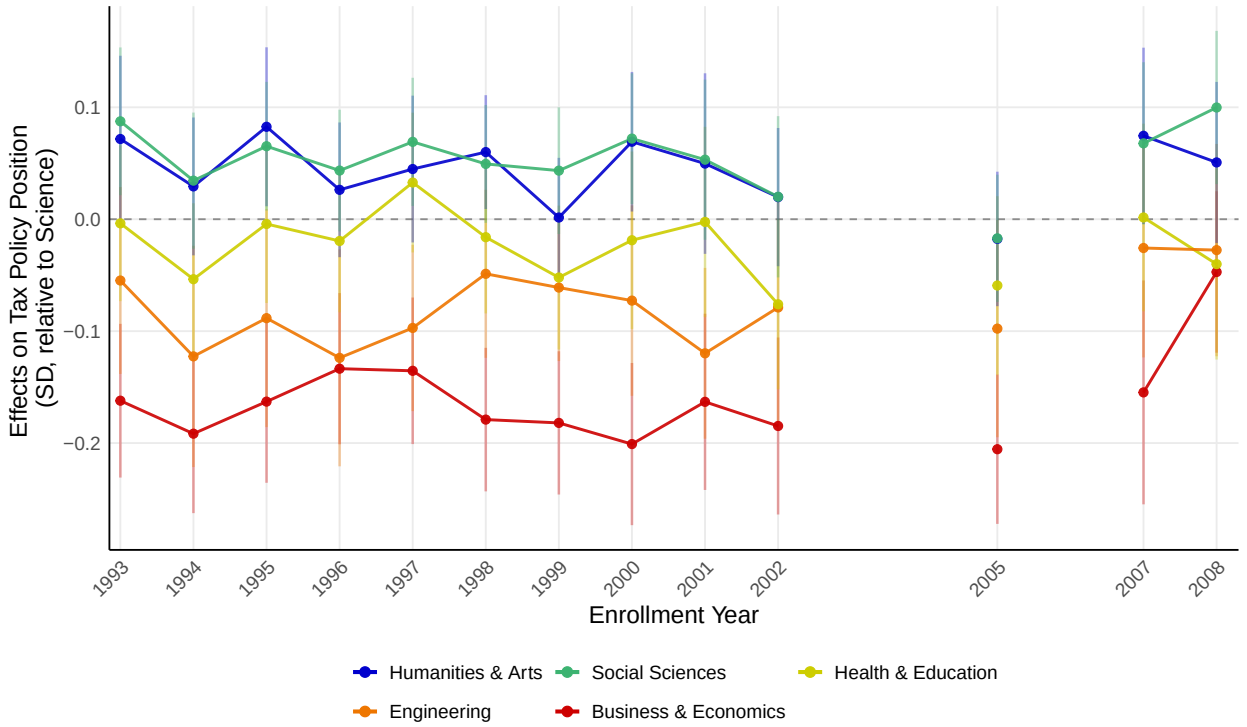


(B) Log Difference Relative to Median



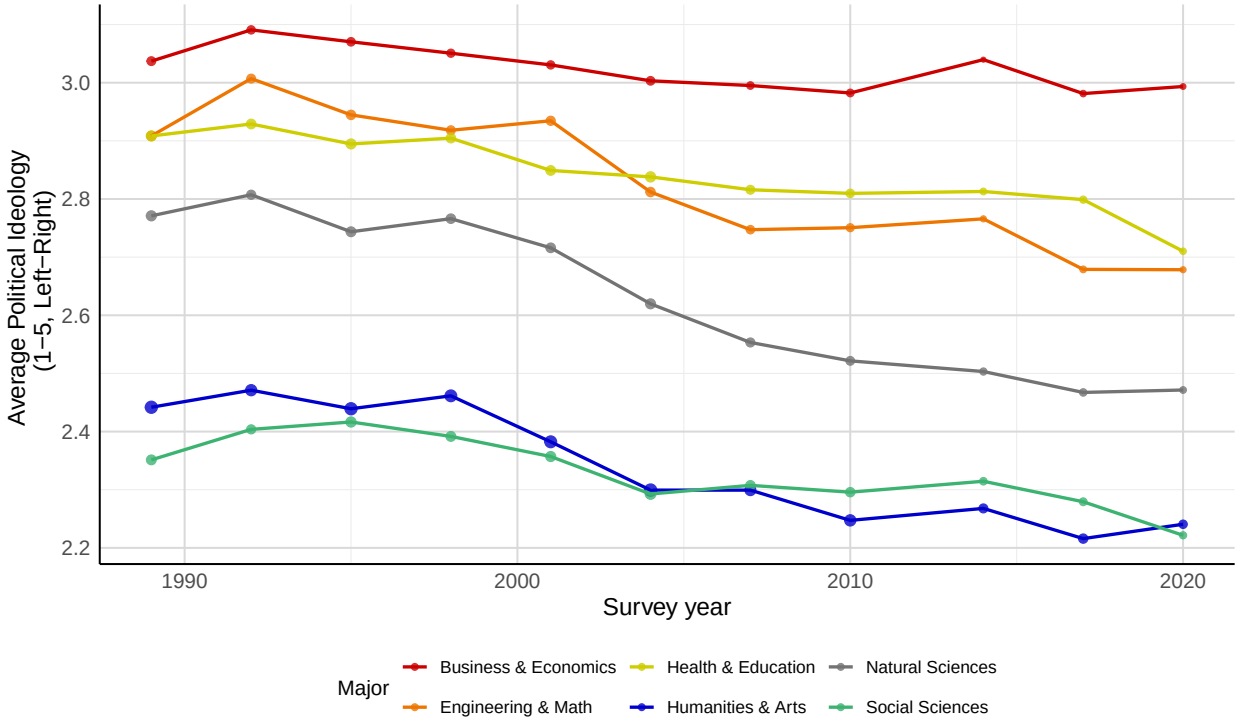
Note: Each panel plots the relationship between students' ideological change during college (y-axis, in SD) and their expected post-graduation wages (x-axis). Panel A uses expected wages in USD. Panel B uses log wage differences relative to the median wage in each major group. Each point represents the mean for a college-major pair. Colors indicate the classification group for each major: blue for humanities and arts; green for social sciences; gray for natural sciences; yellow for health and education; orange for engineering and math; red for business and economics. The sample includes 485 departments with at least 100 students who responded to the ideology question both as freshmen and as graduates. The total number of students in those departments is 131,710. The number of students in each department weights linear trends. The slope in Panel A is 0.061 (0.018). The slope in Panel B is -0.146 (0.030).

Figure G.11: Year-by-Year Effects of College Majors on Tax Support



Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals, from a model that groups majors into five broad categories, with the natural sciences as the omitted reference group. The outcome variable is students' support for higher taxes on the wealthy. Estimates are presented in standard deviation units. Models control flexibly for students' baseline political ideology, policy positions, and major preferences.

Figure G.12: Faculty Ideology Over Time



Notes: This figure plots the average faculty ideology across major-groups over time (y-axis, measured on a 1-5 scale).

Table G.1: College Majors Classification and Aggregation

Broad Group	Detailed Group	List of Majors
Business	Accounting	Accounting
Business	Business	Business, International Business, Other Business
Business	Economics	Economics
Business	Finance	Finance
Business	Management	Management
Business	Marketing	Marketing
Engineering CS Math	CS	Computer Science, Data
Engineering CS Math	Electronic Engineering	Computer Engineering, Electronic Engineering
Engineering CS Math	Engineering	Aero Engineering, Architecture, Chemical Engineering, Civil Engineering, Industrial Engineering, Other Engineering
Engineering CS Math	Math	Math
Engineering CS Math	Mechanical Engineering	Mechanical Engineering
Health Education	Education	Art Education, Business Education, Elementary Education, Other Education, Physical Education, Secondary Education, Special Education
Health Education	Health	Health Technology, Kinesiology, Med Dent Vet, Other Health, Pharmacy, Therapy
Health Education	Nursing	Nursing
Humanities Arts	Arts	Art, Design, Music, Other Art Humanities, Theater
Humanities Arts	History	History
Humanities Arts	Language	Language
Humanities Arts	Other Humanities	Ethnic Studies, Gender Studies, Philosophy
Humanities Arts	Theology	Theology
Missing Undecided	Missing Undecided	Missing, NA, Undecided
Natural Sciences	Biological	Agriculture, Biology, Microbiology, Other Biological, Zoology
Natural Sciences	Chemistry	Biochemistry, Chemistry
Natural Sciences	Physical	Earth Science, Marine Sciences, Other Physical, Physics
Other	Other	Building Trades, Home Economics, Other, Other Technical
Social	Communications	Communications, Speech
Social	Environmental	Environmental Science
Social	Politics	Politics
Social	Psychology	Psychology
Social	Social	Geography, Law Enforcement, Other Social
Social	Social Work	Social Work
Social	Sociology Anthropology	Anthropology, Sociology

Note: The table shows the names of each major as they appear in the baseline and graduation surveys, their detailed classification for the analysis, and their aggregation into broad groups.

Table G.2: Distribution of Double Majors for Cohorts Enrolled Since 2005

First Major	Second Major	Students
Business & Economics	None	16693
Business & Economics	Business & Economics	5566
Business & Economics	Humanities & Arts	1523
Business & Economics	Social Sciences	1310
Business & Economics	Engineering	671
Business & Economics	Other	383
Business & Economics	None	304
Business & Economics	Health & Education	112
Business & Economics	Science	105
Engineering	None	9179
Engineering	Engineering	751
Engineering	Business & Economics	624
Engineering	Humanities & Arts	470
Engineering	Health & Education	238
Engineering	Science	211
Engineering	Social Sciences	191
Engineering	None	189
Engineering	Other	108
Health & Education	None	10379
Health & Education	Health & Education	1270
Health & Education	Humanities & Arts	704
Health & Education	Social Sciences	369
Health & Education	None	299
Health & Education	Other	193
Health & Education	Science	168
Health & Education	Business & Economics	119
Health & Education	Engineering	119
Humanities & Arts	None	14593
Humanities & Arts	Humanities & Arts	4275
Humanities & Arts	Social Sciences	2159
Humanities & Arts	Health & Education	949
Humanities & Arts	Business & Economics	511
Humanities & Arts	None	305
Humanities & Arts	Other	290
Humanities & Arts	Science	230
Humanities & Arts	Engineering	155
Science	None	12694
Science	Humanities & Arts	1069
Science	Science	1006
Science	Social Sciences	872
Science	Engineering	431
Science	Health & Education	309
Science	Business & Economics	250
Science	None	237
Science	Other	175
Social Sciences	None	20418
Social Sciences	Humanities & Arts	3929
Social Sciences	Social Sciences	3649
Social Sciences	Business & Economics	999
Social Sciences	Other	664
Social Sciences	Health & Education	550
Social Sciences	None	477
Social Sciences	Science	370
Social Sciences	Engineering	117

Note: This table presents the distribution of double majors among students enrolled in college since 2005, when double major data was first included in the survey. The first major reflects the student's initial declaration, and the second major is declared subsequently.

Table G.3: Baseline and Graduation Policy Positions

Statement	N	Survey	All	Business & Economics	Engineering & Math	Health & Education	Humanities & Arts	Natural Sciences	Social Sciences
Abortion should be legal	267431	Base	38.7%	43.2%	42.2%	36.7%	34%	39.7%	37.7%
		Grad	62.3%	61.2%	61.5%	47.3%	65%	64.7%	68.2%
Addressing global climate change should be a federal priority	63137	Base	26.8%	27.9%	28.1%	29.5%	24%	27%	26.3%
		Grad	76.7%	70.4%	75.6%	69.1%	79.2%	82.5%	80.8%
A national health care plan is needed to cover everybody's medical costs	158709	Base	61.3%	63.6%	62.5%	62.7%	58.6%	61.9%	59.9%
		Grad	68.2%	57.3%	60.4%	70.6%	73.1%	69.3%	75.1%
It is important to have laws prohibiting homosexual relationships	201563	Base	42.7%	50.1%	48.1%	46.6%	34.7%	43.9%	38.3%
		Grad	19%	23.7%	21.1%	25%	15.1%	17.3%	14.9%
Wealthy people should pay a larger share of taxes than they do now	205274	Base	63.8%	62.7%	62.6%	67.5%	62.6%	64.1%	63.9%
		Grad	60.7%	47.7%	58.4%	61.3%	66.9%	63.1%	66.5%
Marijuana should be legalized	236802	Base	46.6%	48%	46.9%	37.7%	48%	46.7%	48.9%
		Grad	44.1%	43.1%	44.3%	28.3%	49.7%	43.6%	49.1%
The death penalty should be abolished	218858	Base	53.7%	53.3%	53.5%	57.2%	52.5%	53.5%	53.4%
		Grad	39.8%	30.6%	35.7%	31.6%	50.7%	38.6%	45.5%
The federal government should do more to control the sale of handguns	161592	Base	45.1%	47.4%	50.9%	45.9%	42.1%	46.8%	41.9%
		Grad	82.1%	79.9%	74.8%	84.6%	83.5%	81.2%	84.8%
There is too much concern in the courts for the rights of criminals	151713	Base	72.4%	71%	72.4%	73.5%	72.8%	72.1%	72.5%
		Grad	55.2%	63.3%	59.4%	62.6%	46.6%	57.1%	48.4%
Racial discrimination is no longer a major problem in America	279207	Base	58.7%	62.8%	63.2%	60.9%	54.8%	59.7%	55.1%
		Grad	13.5%	18.6%	18.4%	11.9%	10.8%	13.4%	10.1%
Realistically, an individual can do little to bring about changes in our society	216958	Base	58.9%	63.8%	64.5%	60.4%	53.7%	59.8%	55.6%
		Grad	24.2%	28.6%	31.1%	21%	22.3%	24.8%	20.9%
Colleges should prohibit racist/sexist speech on campus	243749	Base	47.7%	48.1%	48.1%	47.4%	47.8%	48.9%	46.6%
		Grad	56%	53.8%	50.6%	60.9%	55.6%	56.1%	57.8%

Note: This table presents twelve policy-related questions that appear in both the baseline and graduation surveys during the analysis period. It reports the share of students who "strongly" or "somewhat" agree with each statement, separately for the baseline and graduation surveys. The sample for each question includes only students who responded in both waves. Column "All reports" results for the full sample, with N indicating the total number of observations. Subsequent columns display results for subsamples defined by students' college major.

Table G.5: Reduced-Form Estimates Based on Persistence in Intended Majors, Additional Results

Panel A. Major Outcomes

	(1)	(2)	(3)	(4)	(5)
	Social Sciences	Humanities& Arts	Science	Engineering& Math	Health& Education
Estimate	-0.047***	-0.033***	-0.004**	-0.007**	-0.003
	(0.009)	(0.009)	(0.002)	(0.003)	(0.004)
N	49,299	49,299	49,299	49,299	49,299
Intended Major FE	Yes	Yes	Yes	Yes	Yes
Major Predictors	Yes	Yes	Yes	Yes	Yes
Outcome Predictors	Yes	Yes	Yes	Yes	Yes
Additional	Yes	Yes	Yes	Yes	Yes

Panel B. Additional Ideology Outcomes

	(1)	(2)	(3)	(4)	(5)
	Predicted	Left	Middle	Right	Index
Estimate	0.019***	-0.015**	0.007	0.008	0.039**
	(0.005)	(0.007)	(0.009)	(0.007)	(0.017)
N	49,299	49,299	49,299	49,299	48,460
Intended Major FE	Yes	Yes	Yes	Yes	Yes
Major Predictors	Yes	Yes	Yes	Yes	Yes
Outcome Predictors	Yes	Yes	Yes	Yes	Yes
Additional	Yes	Yes	Yes	Yes	Yes

Notes: This table reports reduced-form estimates of the effect of the instrument on additional outcomes, with robust standard errors clustered at the college level. The instrument is the cohort-specific persistence rate in the initially intended major. Panel A reports effects on indicators for major groups. Panel B reports additional outcomes, including the predicted effect on ideology implied by major-choice changes (based on our baseline estimates), indicators for identifying as liberal or far left, middle of the road, or conservative or far right, and an ideology index constructed from policy positions. All columns include intended-major fixed effects, major predictors, outcome predictors, and additional controls for state, entry year, and institution-control-by-institution-type-by-selectivity-group fixed effects. The sample is restricted to students who initially intended to major in Business or Economics and have non-missing values for the relevant outcome. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.6: IV Estimates Based on Persistence in Intended Majors, Adjacent Cohorts

Panel A. Falsification Tests: Baseline Ideology

	(1)	(2)	(3)
Full Baseline Survey	-0.023	-0.023	-0.018
	(0.035)	(0.035)	(0.027)
N	82,677	82,677	82,677
Students Included in the Graduation Survey	0.012	-0.022	-0.008
	(0.048)	(0.047)	(0.038)
N	37,350	37,350	37,350
Intended Major FE	No	Yes	Yes
Major Predictors	No	No	Yes
Outcome Predictors	No	No	No

Panel B. IV Results

	(1)	(2)	(3)	(4)	(5)
First Stage: Business/Economics Major	0.158***	0.117***	0.111***	0.108***	0.083***
	(0.028)	(0.031)	(0.030)	(0.030)	(0.018)
Reduced Form: Graduation Ideology	0.066*	0.035	0.039	0.035**	0.028
	(0.038)	(0.039)	(0.034)	(0.017)	(0.017)
2SLS: Graduation Ideology	0.419*	0.296	0.349	0.326*	0.335
	(0.220)	(0.322)	(0.295)	(0.190)	(0.216)
N	38,728	38,728	38,728	38,728	38,728
First-stage F	31.4	14.2	13.5	13.0	20.7
Intended Major FE	No	Yes	Yes	Yes	Yes
Major Predictors	No	No	Yes	Yes	Yes
Outcome Predictors	No	No	No	Yes	Yes
Additional	No	No	No	No	Yes

Notes: Panel A reports falsification tests using baseline political ideology as the outcome. The first row block uses the TFS adjacent-cohort baseline-survey sample, and the second uses the matched CSS adjacent-cohort sample. Panel B reports the first stage, reduced form, and 2SLS estimates for graduation political ideology in the adjacent-cohort CSS sample of students who initially intended to major in Business or Economics. The instrument is the intended-major persistence rate measured using the lagged and lead cohorts. Entries report coefficient estimates with college-clustered standard errors shown below each estimate. "Intended Major FE" indicates fixed effects for intended majors. "Major Predictors" indicates baseline goals and career preferences. "Outcome Predictors" indicates baseline ideology and policy positions. "Additional" indicates controls for state, entry year, and institution-control-by-institution-type-by-selectivity-group fixed effects, and applies only to Panel B. "First-stage F" reports the single-instrument Wald statistic, computed as the squared t-statistic on the instrument in the first stage. Each row block uses students with non-missing values for the relevant outcome. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.7: Effects of College-Level Shocks on Additional Outcomes

A. Major and Course Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	% Engineering/Math Major	% Natural Science Major	% Health/Education Major	% Social Sciences Major	% Humanities/Arts Major	% Humanities/Arts Course
Estimate	-1.052***	-0.541	0.034	-0.624	-1.050	-2.391***
	(0.358)	(0.866)	(1.262)	(1.049)	(0.806)	(0.699)
Control Mean	9.60	12.33	13.12	23.83	18.61	94.24
Shocks	133	133	133	133	133	133
Colleges	441	441	441	441	441	380
N (students)	263,063	263,063	263,063	263,063	263,063	165,203

B. Political Outcomes

	(1)	(2)	(3)	(4)
	% Liberal or Far Left	% Middle-of-the-Road	% Conservative or Far Right	Policy Position Index (SD)
Estimate	-1.722**	0.324	1.344	0.034
	(0.815)	(0.865)	(0.856)	(0.031)
Control Mean	34.18	41.16	24.66	-0.06
Shocks	133	133	133	133
Colleges	441	441	441	441
N (students)	243,502	243,502	243,502	250,896

Notes: This table reports difference-in-differences estimates of the effects of college-level shocks to the availability of business and economics fields on students' outcomes. The specification is an adaptation of equation 3 that replaces the event-study indicators with a single post-treatment indicator. The sample includes all students observed in both the baseline and graduation surveys with non-missing values for the relevant outcome. Panel A reports effects on indicators for all major groups other than Business and Economics, which is reported in the main paper, as well as an indicator for taking any humanities or arts course. Panel B reports effects on three indicators for political ideology identification, the policy-position ideology index, views on national healthcare, and self-reported participation in demonstrations. Indicators are multiplied by 100 and other outcomes are measured in standard deviations. The table reports coefficient estimates, standard errors clustered at the college level, the baseline mean of the outcome in the comparison group, and the number of shocks, colleges, and student observations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.8: Effects of College-Level Shocks, by Predicted Likelihood of Majoring in Business/Economics

A. Low			
	(1)	(2)	(3)
	% Business/Economics Major	% Humanities/Arts Course	Ideology (SD)
Estimate	0.859**	-2.023***	0.030
	(0.361)	(0.698)	(0.020)
Control Mean	4.52	95.19	-0.17
Shocks	128	128	128
Colleges	427	371	427
N (students)	129,284	85,226	125,495
B. High			
	(1)	(2)	(3)
	% Business/Economics Major	% Humanities/Arts Course	Ideology (SD)
Estimate	4.202***	-2.362**	0.040*
	(1.424)	(0.953)	(0.022)
Control Mean	40.12	93.29	0.11
Shocks	107	107	107
Colleges	388	335	388
N (students)	130,637	77,703	126,635

Notes: This table reports difference-in-differences estimates of the effects of college-level shocks to the availability of business and economics fields on students' outcomes. The specification is an adaptation of equation 3 that replaces the event-study indicators with a single post-treatment indicator. The baseline sample includes all students observed in both the baseline and graduation surveys with non-missing values for the relevant outcome. Results in Panel A are further restricted to students with lower predicted probability of taking business or economics as a major, and results in Panel B are restricted to students with higher predicted probability of taking business or economics as a major, based on a model of students' pre-college characteristics. Outcomes include an indicator for majoring in Business and Economics, an indicator for taking any humanities or arts course, and students' political ideology. Indicators are multiplied by 100 and ideology is measured in standard deviations. The table reports coefficient estimates, standard errors clustered at the college level, the baseline mean of the outcome in the comparison group, and the number of shocks, colleges, and student observations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.9: Falsification Tests: "Effects" of Peer Ideology Composition on Baseline Ideology

Panel A. Full Baseline Survey					
	(1)	(2)	(3)	(4)	(5)
Estimate	0.711***	0.019***	0.030***	0.004	0.004
	(0.014)	(0.003)	(0.004)	(0.004)	(0.004)
Estimate (Adjusted)	0.286	0.020***	0.008	0.005	0.005
	(0.191)	(0.008)	(0.005)	(0.004)	(0.004)
N	4,488,715	4,488,715	4,488,715	4,488,715	4,488,715
Fixed Effects	No	Yes	No	Yes	Yes
Potential Peers' Average	No	No	Yes	Yes	Yes
Major Predictors	No	No	No	No	Yes
Panel B. Students Included in the Graduation Survey					
	(1)	(2)	(3)	(4)	(5)
Estimate	0.746***	0.018	0.025*	0.007	0.005
	(0.022)	(0.012)	(0.014)	(0.014)	(0.013)
Estimate (Adjusted)	1.205***	0.039*	0.044**	0.022	0.020
	(0.038)	(0.020)	(0.022)	(0.022)	(0.020)
N	230,733	230,733	230,733	230,733	230,733
Fixed Effects	No	Yes	No	Yes	Yes
Potential Peers' Average	No	No	Yes	Yes	Yes
Major Predictors	No	No	No	No	Yes

Notes: This table reports falsification estimates for the peer effects analysis using equation 4 and baseline political ideology as the outcome. The treatment is the average baseline political ideology of peers in the same college-by-entry-cohort-by-intended-major group. Entries report coefficient estimates with college-clustered standard errors shown below each estimate. Adjusted estimates correct attenuation from incomplete peer coverage using the split-sample correction in the analysis. Panel A uses the full baseline-survey sample. Panel B uses only students observed in the graduation survey. "Potential Peers' Average" indicates controls for the average ideology of potential peers from adjacent cohorts. "Fixed Effects" indicates college-major, college-cohort, and major-cohort fixed effects. "Major Predictors" indicates controls related to major choice, including intended majors, careers, and life goals. Samples are restricted to students with non-missing intended major and baseline political ideology, at least 5 observed peers, and peer coverage of at least 10% of the cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table G.10: Estimated Effects of Peer Ideology Composition, Robustness

	Ideology	Liberal	Index	Ideology	Ideology	Ideology
	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	-0.010	0.010	0.013	-0.007	-0.016	-0.015
	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.033)
Estimate	-0.017	0.007	0.016	-0.013	-0.029*	-0.027
(Adjusted)	(0.016)	(0.018)	(0.019)	(0.016)	(0.017)	(0.041)
N	229,660	229,660	224,954	216,287	158,614	46,314
Share	0.1	0.1	0.1	0.3	0.5	0.7
Potential Peers' Average	V	V	V	V	V	V
Baseline	V	V	V	V	V	V
Program + Year FEs	V	V	V	V	V	V

Notes: This table presents estimates of peer effects on students' political ideology based on equation 4. The outcome variable is either self-reported ideology, or dummy for liberal, or the opinion index constructed from the graduation survey The treatment variable captures peers' outcome from the baseline survey, where peers are defined as students from the same college-cohort-intended major group. The estimates account for exclusion bias using a four-way split-sample approach. Specifications include controls for the average index of potential peers from four adjacent cohorts (Expectaions), pre-college political ideology and opinion index (Baseline), and college-major, college-cohort, and major-cohort fixed-effects (FEs). All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both major and political views, and with information available for at least 5 of their peers which represent at least x% of the cohort, with x reported in the table (Share). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table G.11: Falsification Test: "Effects" of Faculty Ideology on Students' Baseline Ideology

Panel A. Full Baseline Survey

	(1)	(2)	(3)
Estimate	0.343***	0.012***	0.010***
	(0.014)	(0.002)	(0.002)
Estimate (Adjusted)	0.754***	0.030***	0.024***
	(0.030)	(0.006)	(0.006)
N	1,635,383	4,268,106	4,268,106
Fixed Effects	No	Yes	Yes
Major Predictors	No	No	Yes

Panel B. Cohorts Included in the Graduation Survey

	(1)	(2)	(3)
Estimate	0.358***	0.005	-0.005
	(0.023)	(0.005)	(0.008)
Estimate (Adjusted)	0.792***	0.017	-0.008
	(0.046)	(0.013)	(0.019)
N	222,461	700,408	222,461
Fixed Effects	No	Yes	Yes
Major Predictors	No	No	Yes

Panel C. Students Included in the Graduation Survey

	(1)	(2)	(3)
Estimate	0.286***	-0.006	-0.003
	(0.018)	(0.012)	(0.006)
Estimate (Adjusted)	0.584***	0.001	0.001
	(0.033)	(0.026)	(0.014)
N	163,703	160,392	163,703
Fixed Effects	No	Yes	Yes
Major Predictors	No	No	Yes

Notes: This table presents falsification tests for equation 6, estimating the relationship between faculty ideology and students' baseline ideology. The outcome is the baseline political ideology and the treatment is department-level faculty ideology matched to each student's college and intended major. Panel A uses the full baseline-survey sample. Panel B restricts the baseline sample to the cohorts observed in the graduation survey. Panel C uses only students observed in the graduation survey. College-clustered standard errors below each estimate. Adjusted estimates correct for attenuation from incomplete faculty coverage. "Fixed Effects" indicates specifications that include major-by-cohort and college-by-major-group-by-cohort fixed effects. "Major Predictors" indicates additional predictors related to major choice, including careers and life goals. The sample is restricted to students with non-missing intended major and political ideology, at least 3 observed faculty, and faculty coverage of at least 10% of the department. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table G.4: Control Variables

Panel A. Specification Ladder

	(1)	(2)	(3)	(4)	(5)
	None	Lagged D	Lagged Y	Main	All
Outcome Predictors	No	No	Yes	Yes	Yes
Major Predictors	No	Yes	No	Yes	Yes
Behaviors	No	No	No	No	Yes
Time Use	No	No	No	No	Yes
Demographics	No	No	No	No	Yes
College FE	No	No	No	No	Yes
Enrollment Year	No	No	No	No	Yes

Panel B. Variable Groups

Outcome Predictors	View: How would you characterize your political views?; View: Abortion should be legal; View: The federal government should do more to control the sale of handguns; View: The death penalty should be abolished; View: Too much concern in the courts for rights of criminals; View: Important to have laws prohibiting homosexual relationships; View: Wealthy people should pay a larger share of taxes; View: Colleges should prohibit racist/sexist speech on campus; View: Realistically, an individual can do little to bring about changes in our society; View: Colleges have the right to ban extreme speakers / prohibit racist/sexist speech; View: Activities of married women are best confined to home and family; View: The federal government is not doing enough to control global warming/climate change; View: Racial discrimination is no longer a major problem in America; View: Marijuana should be legalized
Major Predictors	Intended Major; Student's probable career; Goal: Involved in programs to clean up the environment; Goal: Participating in a community action program; Goal: Helping to promote racial understanding; Goal: Helping others who are in difficulty; Goal: Being very well off financially; Goal: Keeping up to date with political affairs; Goal: Influencing the political structure; Goal: Influencing social values; Goal: Raising a family; Goal: Developing a meaningful philosophy of life
Behaviors	Past year: Discussed politics; Past year: Voted in student election; Past year: Performed volunteer work; Past year: Participated in organized demonstrations; Past year: Attended a religious service
Time Use	Hours/wk: Talking with teacher outside class; Hours/wk: Reading for pleasure; Hours/wk: Household/childcare duties; Hours/wk: Volunteer work; Hours/wk: Watching TV; Hours/wk: Student clubs/groups; Hours/wk: Partying; Hours/wk: Working (for pay); Hours/wk: Socializing with friends; Hours/wk: Exercise or sports; Hours/wk: Studying/homework
Demographics	Sex; Race/Ethnicity group; Parents' total income last year; Parents alive / divorced status; Citizenship status; First-generation college status; Student's religion; Mother's probable career; Father's probable career; High-school GPA
College FE	College fixed effects
Enrollment Year	Enrollment year fixed effects

Note: This table summarizes the control sets used in the baseline and robustness analyses. Panel A maps the displayed specifications in Figure C.2. "Lagged D" includes intended major, the student's probable career, and baseline life goals. "Lagged Y" includes baseline political ideology and policy positions. "Main" includes both sets, and "All" additionally includes college fixed effects, enrollment year, baseline behaviors, time use, and demographic characteristics. Panel B lists the exact variables in each group. All categorical controls are included flexibly using indicators for observed response categories.