Changing Minds: How Academic Fields Shape Political Attitudes

Yoav Goldstein, Matan Kolerman Shemer*

20 August 2026

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. Estimated major 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 for views on economic redistribution. 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 pre-college behavioral characteristics. Various alternative identification strategies, including a complementary quasi-experimental design exploiting supply-side variation in field availability, yield consistent results. Major 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. Academic environment, rather than peer effects or expected earnings, appears to explain major effects. Finally, field choice increases political fragmentation because students select majors that align with their preexisting political attitudes.

*Goldstein: The Eitan Berglas School of Economics, Tel Aviv University, yoavg2@mail.tau.ac.il. Kolerman Shemer: Department of Economics, MIT, kolerman@mit.edu. We are indebted to our advisors, Analia Schlosser and Moses Shayo. We also thank Michael Amior, Josh Angrist, Gal Bitton, Zach Bleemer, Daniel Firoozi, 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 the sky, 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 is not a uniform experience. One defining characteristic of higher education is its intellectual diversity: students sort into academic fields that emphasize different ideas, methods, and normative frameworks. These differences raise the question of whether academic fields shape students' political and social preferences, or whether causality runs the other way, with majors only reflecting, but not influencing, student preferences.

We study this question using rich panel survey data from the Higher Education Research Institute (HERI).[1] Our analysis covers roughly 300,000 students who enrolled at 450 American colleges between 1990 and 2015 and were surveyed at college entry and graduation. HERI surveys measure students' intended major at entry and their completed major at graduation, along with political ideology, policy positions, life 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. Covariates include predictors of graduation ideology, such as baseline ideology and policy positions, as well as predictors of major choice, such as intended majors and life goals. We provide evidence that conditioning on these variables substantially reduces selection bias in our setting, yielding plausibly causal estimates.

These estimates imply that college majors substantially affect students' political ideology. Relative to the natural sciences, studying the social sciences or humanities shifts students 0.09 standard deviations (SD) to the left by graduation, while studying economics or business shifts them 0.14 SD to the right.[2] Professional majors—engineering, health, and education—produce smaller rightward shifts relative to the natural sciences, about 0.06 SD. Among specific majors, finance (social work) produces the largest rightward (leftward) shift, 0.23 SD (0.26 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:

[1]The CIRP surveys, which are among the longest-running studies of American higher education, were founded in 1966 by Alexander W. Astin, who established HERI at UCLA in 1973.
[2]Throughout the paper, we use "liberal"/"left" or "conservative"/"right" to refer to self-identification and correlated policy positions. We group economics with business-related majors because economics aligns more closely with these fields than with the social sciences along the dimensions central to our analysis.

for comparison, among entering college cohorts, the pre-college gender gap in ideology is 0.14 SD, while the gap between students from New York and Texas is 0.32 SD.

We next assess threats to a causal interpretation of these estimates. We first show that our main controls absorb almost all of the bias implied by observed demographic and behavioral variables, which are not part of our main specification. We then apply the rigorous sensitivity framework developed by Oster (2019), which provides a transparent and disciplined way to assess the potential influence of selection on unobservables. Under a conservative benchmark that assumes that all remaining explainable variation in graduation ideology is as strongly related to major choice as baseline ideology, the Oster-adjusted leftward effect of majoring in the social sciences or humanities rather than business or economics is approximately 0.19 SD. This estimate remains close to our baseline estimate of 0.23 SD.

We also show that the results are robust to alternative identification strategies. Notably, we obtain similar estimates when using only variation in major choice predicted by students' pre-college preferences. This reduces concerns that the results are driven by post-entry shocks that jointly affect major choice and political ideology or by the weight placed on students who switch majors. The results are also robust to an instrumental variables (IV) strategy that exploits variation in the probability of remaining in one's intended major based on program-specific leave-one-out persistence rates.[3]

The causal interpretation is further supported by complementary quasi-experimental evidence. We exploit within-college expansions in the availability of business and economics fields and estimate their effects using a difference-in-differences approach. The identifying assumption is that, absent an expansion, outcomes at expanding and non-expanding colleges would have followed parallel trends. Three findings support this assumption. First, anecdotal evidence indicates that expansions are often prompted by external events, such as major donations. Second, expanding and non-expanding colleges exhibit similar outcome trends before the expansions. Third, the baseline ideology of incoming students does not change differentially following an expansion, making changes in student selection unlikely.

Expansions increase enrollment in business and economics by about 3 percentage points and shift students' graduation ideology to the right by roughly 0.04 SD. These magnitudes are not directly comparable to our main estimates because expansions affect students through both major choice and coursework outside the major, a margin we document directly: following an expansion, students become less likely to ever take a humanities or arts course, including

[3]The two IV estimates are broadly similar in magnitude to our preferred OLS estimate, although both are somewhat larger. They remain smaller than the unadjusted OLS estimate, consistent with substantial positive selection bias in naive comparisons that is largely removed by our preferred specification. We discuss possible explanations for the remaining differences, which are not statistically significant.

students unlikely to major in business or economics. Nevertheless, the results provide complementary evidence that changes in exposure to fields shape students' attitudes.

The effects of academic fields extend beyond political ideology to specific policy positions. Although the overall education gradient in redistributive preferences is close to zero (Kuziemko et al., 2026), we uncover substantial differences within the college-educated population. Relative to the natural sciences, majoring in business or economics reduces support for redistribution-oriented policies, including taxation and healthcare, by 0.17–0.18 SD. By contrast, majoring in the social sciences or humanities increases support for these policies by 0.04–0.07 SD. Effects on cultural issues, including abortion policy and LGBTQ rights, are smaller, and students in all fields become more liberal over the course of college.

The effects also extend to students' life goals and priorities. Humanities and social sciences increase political activism, including participation in demonstrations, and strengthen social, political, and meaning-oriented goals. Business and economics, by contrast, increase the importance students place on financial success. These shifts may affect subsequent career choices, providing one channel through which fields of study generate differences in earnings (Hastings et al., 2013; Altonji et al., 2016; Kirkeboen et al., 2016; Andrews et al., 2017; Heinesen et al., 2026; Bleemer and Mehta, 2022; Andrews et al., 2024; Goldstein, 2026). This interpretation is consistent with evidence that studying economics increases both employment in and preferences for higher-paying industries (Bleemer and Mehta, 2022), and that high-school curricula and fields of study can have persistent effects on beliefs, preferences, and choices (Arold, 2024; Ainsworth et al., 2025; Meriläinen and Mitrunen, 2025).

We next examine mechanisms. First, we study peer effects using the baseline ideology of students in the same college, major, and cohort. Second, we use post-college earnings data from the Department of Education's College Scorecard, which reports median earnings by college-major combination among federally aided students, to proxy for variation in expected earnings across programs. Third, we examine differences in academic environments. We use departmental ideology to proxy for variation across colleges in the environment in which the same major is taught. Departmental ideology is measured at the college-major level using HERI Faculty Surveys, which record ideology on a scale comparable to the student measure.

Each proxy is positively correlated with the estimated major effects. When we include all three simultaneously, however, departmental ideology remains strongly predictive, whereas peer ideology and expected earnings do not. Moreover, departmental ideology alone closely tracks the cross-major pattern of estimated effects, with an R^2 of about 0.86.

We then examine each mechanism more directly. Although students' ideology is strongly correlated with each other, changes in average ideology across adjacent cohorts within the

same college and major have little effect on individual outcomes. This result does not rule out peer effects more broadly. Rather, it suggests that changes in peer composition within college programs are unlikely to account for the major effects in our setting. As Sacerdote (2014) emphasizes, peer effects are highly context-dependent and vary with the structure and intensity of peer interactions. Consistent with this view, several studies find sizable effects in settings involving more intensive or sustained peer exposure (Billings et al., 2021; Strother et al., 2021; Algan et al., 2026). Finally, exploiting variation across colleges and over time, we find little evidence that the effects of majors vary with expected earnings.

We then examine departmental ideology more directly by comparing students within the same specific major across colleges with departments that differ in ideology, controlling also for college-by-cohort-by-major-group fixed effects. Departmental ideology is unrelated to students' baseline ideology in the main sample, but students in more left-leaning departments graduate significantly more left-leaning. Among humanities and social-science majors, studying the same major in a one-SD more left-leaning department is associated with additional leftward shifts in student ideology of approximately 0.08 and 0.07 SD, respectively.

This analysis does not distinguish among the effects of individual faculty members, curricular content, and instructional practices, all of which may contribute to the broader departmental environment. Evidence from other settings may therefore help interpret our findings. Baum et al. (2026) exploit within-course variation in instructor partisanship and find no effect on students' subsequent partisan affiliation. They also find no evidence that Democratic instructors cover more liberal topics when teaching the same course. To the extent that these findings generalize, they suggest that the effects of departmental environments documented here are unlikely to operate primarily through exposure to individual instructors.

Curricular content nevertheless remains a plausible channel. This interpretation connects to a growing literature showing that explicitly political curricula can shape political attitudes (Voigtländer and Voth, 2015; Cantoni et al., 2017; Chen et al., 2023; Meriläinen and Mitrunen, 2025; Braghieri and Eichmeyer, 2024).[4] Our findings suggest that similar mechanisms may operate even when curricula are not explicitly political and students select their academic environments. Complementary experimental evidence supports the underlying channel that exposure to new information and explanatory frameworks can change beliefs and preferences. Survey experiments show that informational and explanatory interventions affect beliefs and policy views (Alesina et al., 2021; Stantcheva, 2021; Dechezleprêtre et al., 2025), while

[4]Related evidence shows that educational content can shape politically and socially consequential outcomes in other settings. Ash et al. (2026) find that exposure to law-and-economics training shifted the decisions of U.S. federal judges in a more economically conservative direction, illustrating how ideas transmitted through professional education can affect subsequent policy decisions.

field experiments demonstrate that deliberately designed school curricula can strengthen interethnic social ties and prosocial behavior (Alan et al., 2021).

Finally, we examine the broader consequences of our findings. First, we translate the estimated effects into expected voting behavior using the relationship between ideology and vote choice among students in other surveys. The results imply that studying the humanities or social sciences makes students about 6 pp more likely to vote Democratic than studying economics or business. This is comparable in magnitude to the effect of growing up in a county with a 20-pp 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 average ideological shift during college would nearly vanish, and the ideological gap between college graduates and non-graduates would shrink by about a third.

Third, self-selection into majors amplifies ideological dispersion among college graduates. Students who enter college with more left-leaning ideology tend to choose fields with more leftward estimated effects, causing ideological dispersion to rise during college. We show that under a counterfactual in which all students study the same field, 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 76 percent of the increase in the ideological gender gap during college and about 16 percent of the gap at graduation.

These results highlight the broader implications of field specialization for the civic consequences of education. A large literature in economics emphasizes that education can promote civic engagement and social cohesion (Gradstein and Justman, 2002; Milligan et al., 2004; Dee, 2004; Glaeser et al., 2007; Oreopoulos and Salvanes, 2011; Ben-Porath, 2025). Our findings complicate the view of education as a cohesive force. Specialization—a defining feature of higher education—can instead increase political fragmentation by shifting students' attitudes in different directions.

Together, the results show that what students study in college meaningfully shapes their political and social preferences. The paper's main contribution is to demonstrate that, even among students with similar levels of educational attainment, exposure to different fields of study—and to distinct departmental environments within the same broad field—produces substantially different changes in political attitudes, behaviors, and priorities.

Therefore, the paper contributes to a growing empirical literature examining how education shapes political outcomes. One strand studies the effects of educational attainment and access (Marshall, 2016, 2019; Cavaillé and Marshall, 2019; Apfeld et al., 2023; Firoozi, 2026;

Kaplan et al., 2025). A second strand examines how educational institutions and resources affect political engagement, even conditional on educational attainment (Asker et al., 2024; Cohodes and Feigenbaum, 2025; Mumma, 2026).

Evidence on how fields of study shape political outcomes, however, remains limited. One recent study documents associations between fields of study and voting behavior in Europe (Hooghe et al., 2025). A related literature focuses specifically on economics education and shows that economics students hold systematically different views from students in other fields (Frank et al., 1993; Rubinstein, 2006; Bauman and Rose, 2011; Hammock et al., 2016; Fischer et al., 2017; Girardi et al., 2024).

We advance this literature in three ways. First, we provide the strongest causal evidence to date on the effects of fields of study on students' political attitudes, behaviors, and priorities. Using panel data on students across U.S. colleges over three decades, we combine a rigorous selection-on-observables approach with complementary quasi-experimental evidence. To our knowledge, this is the first to provide evidence that supports a credible causal interpretation. Second, we examine the mechanisms underlying these effects, distinguishing among peer composition, expected earnings, and departmental academic environments. Third, we quantify their broader implications for voting behavior, the college political divide, ideological dispersion, and gender differences in ideology.

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. 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). The questionnaires are available on the HERI website.[5]

Survey participation requires a fee. Still, many U.S. colleges participate each year: several hundred in the baseline survey and one to two hundred in the graduation survey, with substantial overlap across surveys. Both surveys, though officially voluntary, are purpose-

[5]Details on the data and access are available here: https://heri.ucla.edu/cirp-freshman-survey.

fully integrated into mandatory student processes, such as orientation or registration, and graduation packets or rehearsal sessions, yielding relatively high response rates.[6]

2.1 Sample

Our main linked sample includes only students who participated in both the baseline and graduation surveys, yielding 317,161 students from 451 four-year colleges. Restricting to students with non-missing graduation ideology leaves 307,524 observations. Appendix A summarizes the sample-construction pipeline. Appendix Figure A.1 maps the colleges in the sample, with each point sized by its number of students, illustrating the broad geographic coverage of the survey.

Table 1 describes the study sample and compares it to all baseline survey respondents from four-year colleges. 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. This slightly more conservative profile of the study sample reflects its concentration in private colleges and its larger share of Catholic and higher-income students relative to the baseline sample, characteristics associated with more conservative views that outweigh the more liberal tilt implied by its higher female and lower southern shares. Differences in intended majors 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 H.1. Appendix Figure H.1 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 22-70 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

[6]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			Study Sample		
Variable	Mean	SD	N	Mean	SD	N
A. Demographics						
Female	0.56	0.50	9,527,540	0.62	0.48	316,676
Age	18.60	2.32	9,424,814	18.32	0.95	315,410
Black	0.08	0.27	9,591,655	0.03	0.18	317,161
Hispanic	0.05	0.22	9,591,655	0.04	0.19	317,161
White	0.69	0.46	9,591,655	0.79	0.41	317,161
South	0.25	0.43	9,591,655	0.12	0.33	317,161
Income below 50k	0.35	0.48	8,429,651	0.27	0.45	284,268
Income 50-100k	0.34	0.47	8,429,651	0.36	0.48	284,268
Income above 100k	0.31	0.46	8,429,651	0.37	0.48	284,268
Catholic	0.28	0.45	9,591,655	0.41	0.49	317,161
B. Baseline Ideology						
Liberal or Far Left	0.31	0.46	8,839,179	0.28	0.45	302,117
Middle-of-the-road	0.46	0.50	8,839,179	0.44	0.50	302,117
Conservative or Far Right	0.23	0.42	8,839,179	0.28	0.45	302,117
C. Intended Majors						
Natural Sciences	0.13	0.33	7,946,774	0.14	0.35	272,800
Engineering & Math	0.15	0.36	7,946,774	0.11	0.32	272,800
Social Sciences	0.18	0.39	7,946,774	0.19	0.39	272,800
Business & Economics	0.19	0.39	7,946,774	0.20	0.40	272,800
Humanities & Arts	0.12	0.33	7,946,774	0.13	0.34	272,800
Health & Education	0.22	0.42	7,946,774	0.23	0.42	272,800

Note: This table reports summary statistics for our study sample. "Baseline" covers baseline-survey respondents at four-year institutions whose college appears in more than one survey year with more than 20 respondents. "Study sample" further restricts to students observed at graduation. N reports the number of observations with non-missing values.

majors. Table H.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).[7] 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 H.2 demonstrates that responses to a similar ideology question are highly correlated with voting patterns among students in two national surveys.

The trends in ideology among incoming and graduating students in our sample appear in Appendix Figure H.3, where students are grouped in both series by their actual major at graduation. 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.

Panel A of Figure 1 compares students' baseline and graduation ideology across majors in the detailed classification.[8] For majors in business and economics, the average ideology remains similar to the baseline, as their circles are close to the 45-degree line. Students in health, education, engineering, and natural sciences become slightly more left-leaning, as their circles are below the 45-degree line. Finally, humanities and social sciences majors exhibit a significant leftward shift, with their circles well below the 45-degree line.

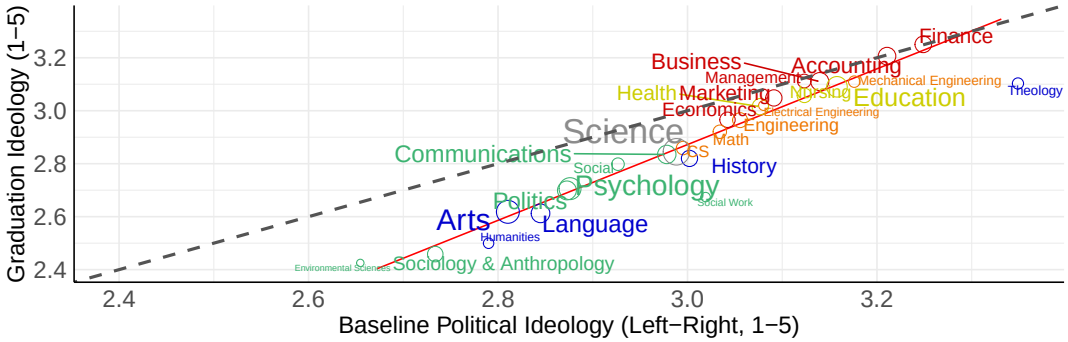
Interestingly, the fields in which students become more left-leaning are typically those that also attract more left-leaning students at baseline. There are exceptions: some majors attract less left-leaning students yet induce a more leftward shift. Notable examples are theology and social work, which, despite attracting relatively conservative students, tend to lead to marked shifts 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 eleven questions that appear in both surveys for a large number of years during our sample period, and on climate policy, which is available for fewer years. Table H.3 reports the share of students who agree with each statement by college major, before and after college. For economic policy issues, such as healthcare and taxation, students across majors shift in different directions, while for other issues, such as climate change and abortion, they shift in similar directions.

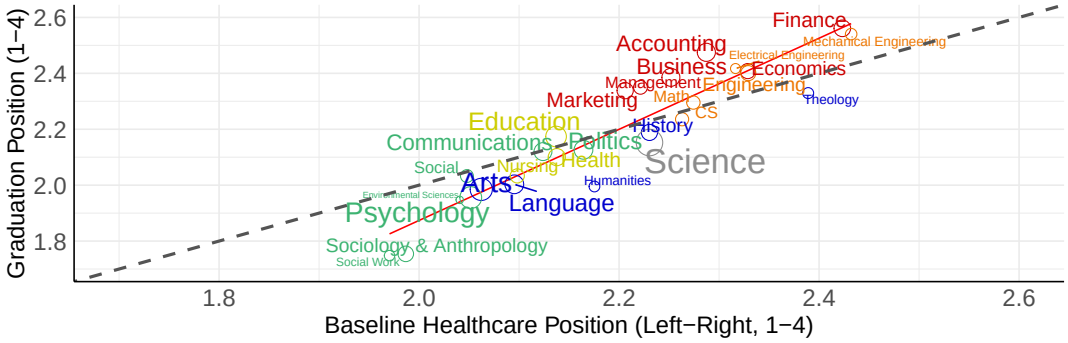
[7] While concerns may arise about comparing ordinal scales across groups (Bond and Lang, 2019), this issue is less severe in our setting because we observe the same scale for each individual before and after college.
[8] Figure H.4 presents distributional changes in ideology over the course of college, separately by major group.

Figure 1: Baseline and Graduation Attitudes

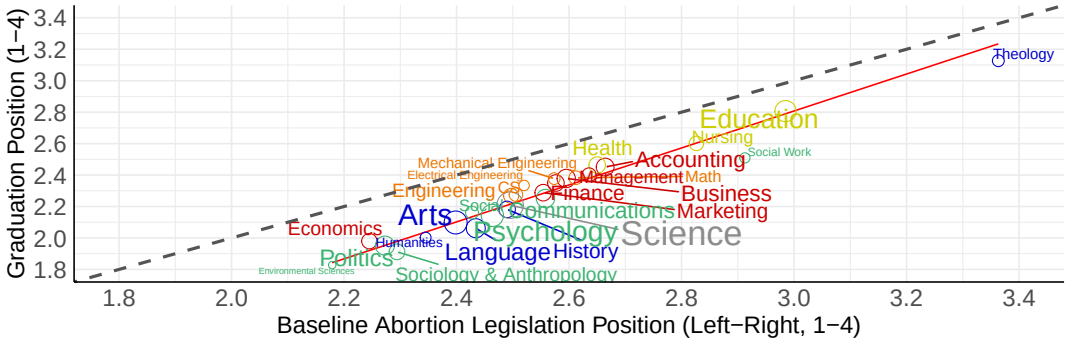
(A) Ideology



(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) (293,523 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.

This may suggest 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 Fields of Study

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 strong predictors of the outcome, most importantly ideology measured before college. Because this lagged measure is discretized and subject to survey response error, X_i^p also includes a rich set of baseline policy positions, which together capture selection on students' pre-college ideology comprehensively.

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. Table H.4 lists the full set of control variables.

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 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 ideology at graduation.[9] This assumption could be violated through two main channels. First, pre-existing predictors of 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 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.

Because the regression includes multiple treatments, the coefficients δ_m could in principle be affected by contamination bias arising from treatment-effect heterogeneity (Goldsmith-Pinkham et al., 2024). Our results are robust to a remedy they propose that imposes no restrictions on heterogeneity: estimating the effect of each major from a separate regression that includes only students in that major or in the omitted reference group (Appendix B, Figure B.1). This suggests that the heterogeneity restrictions required to interpret δ_m as average treatment effects are plausible in our setting, as we discuss in Appendix B.

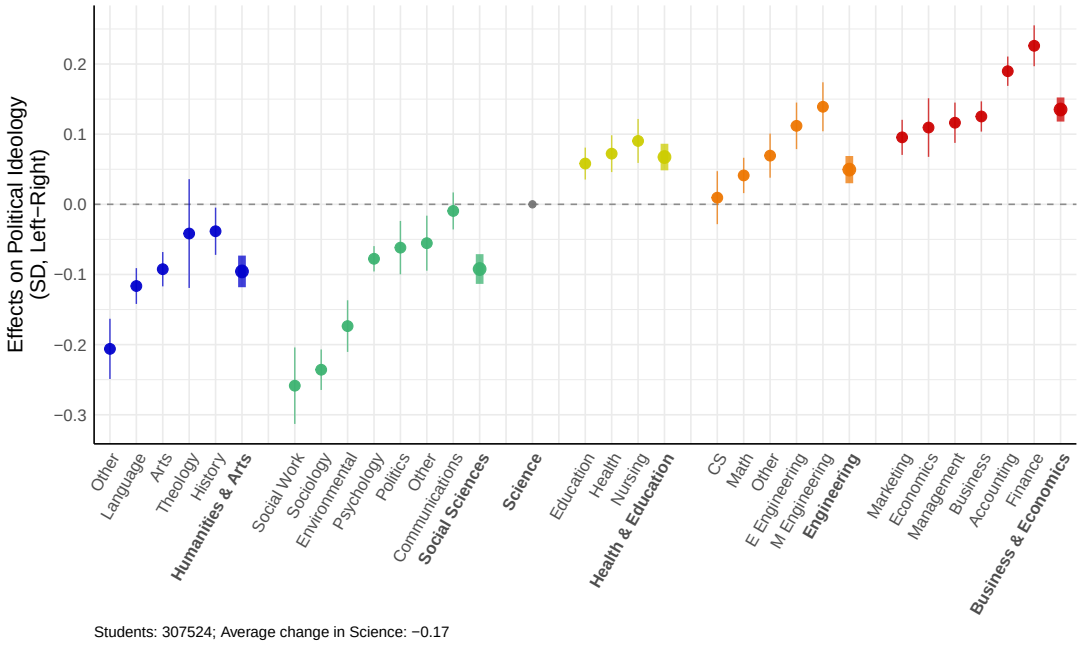
Panel A of Figure 2 presents the main estimation results. Bold lines mark coefficients from a specification that groups majors into six broad categories, while the thinner lines show results from a more detailed specification with 27 categories. In both cases, the omitted group is the natural sciences (biology, physics, chemistry). The estimates reveal substantial effects. Panel A shows 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 0.23 SD).

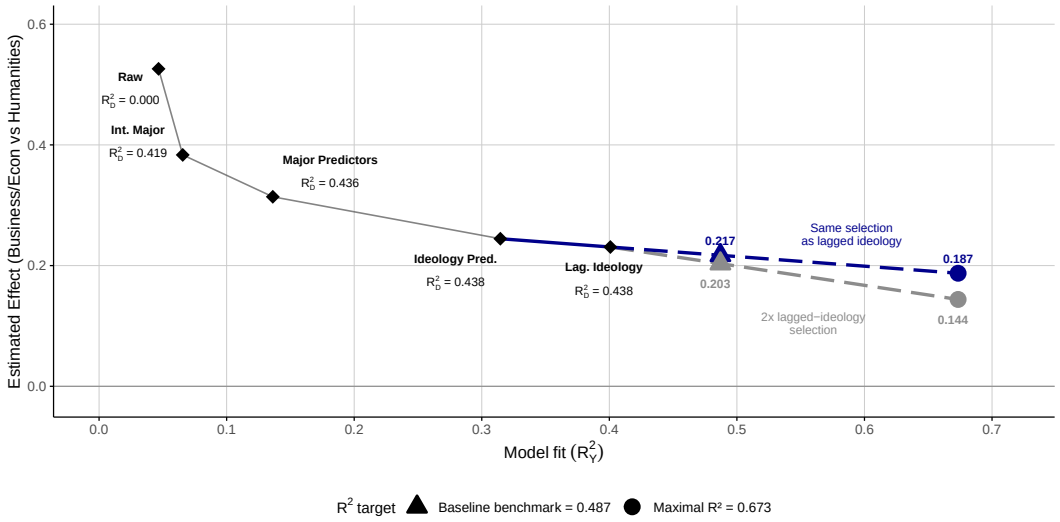
[9]Following Spiess (2025), who shows that discretion over covariate adjustment trades precision against specification-search bias, we fix ex ante the shortest control set that our identification argument justifies: predictors of the outcome and predictors of the treatment. For this reason, we deliberately leave other informative controls, such as demographics or college and year fixed effects, out of the main specification; they enter only in the robustness analyses discussed below. All controls are included flexibly, using indicators for each observed value; Table H.4 lists them. In unreported analyses, we also verify that the results are robust to allowing interactions among key controls.

Figure 2: Effects of College Majors on Political Ideology

(A) Estimates



(B) Sensitivity: Effects of Business/Economics versus 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 specification that groups majors into six broad categories. The thinner lines show coefficients from a more detailed specification with 27 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 business/economics coefficient, estimated relative to the humanities with the other fields controlled, under different conservative scenarios (see main text for details).

Robustness. Appendix C reports a broad set of robustness analyses. Appendix C.1 shows that the results are insensitive to alternative specifications. Conclusions remain similar when we use alternative outcome definitions, including binary measures and changes in ideology, when we add demographic and behavioral controls, and when we account for double majors.

Appendix C.2 examines whether survey attrition could affect the results. Our sample consists of students observed in both surveys, so differential attrition from the graduation survey across majors could in principle bias the estimates.[10] We address this in two ways. First, because realized major is unobserved for students who leave before graduation, we test for differential attrition by intended major; the conditional response gaps across intended majors are small, and within each intended major, baseline ideology is only weakly and similarly related to response.[11] Second, we re-estimate the main specification weighting observations by the inverse probability of appearing in the graduation survey, estimated from rich baseline characteristics. This correction assumes that attrition depends on these observed characteristics rather than on unobserved determinants of graduation ideology. Reweighting leaves the pattern across fields unchanged, though it raises the economics or business and engineering estimates somewhat (Appendix Table C.3). Together, these analyses indicate that sample selection is unlikely to meaningfully bias our results.

Appendix C.3 shows that the effects of college majors are not concentrated among particular students, institutions, or cohorts. It reports estimates of Equation 1 separately for several subsamples. Table C.4 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.5 reports results by college characteristics. The estimates are again broadly similar across institution types, levels of selectivity, and geographic regions. Finally, Table C.6 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.

Appendix C.4 reports results from two IV strategies that exploit distinct sources of variation in major choice. The first uses students' intended majors, reported at college entry, as instruments for their realized majors. It thus retains only the variation in realized major predicted by pre-college preferences, excluding variation from shocks occurring after the

[10] Attrition reflects two distinct sources: students who leave college before graduating, and enrolled students who are less likely to complete the graduation survey.

[11] This diagnostic is similar in spirit to the bounds of Lee (2009), where small response gaps imply little trimming and therefore tight bounds.

baseline survey that may affect both major choice and ideology.[12] The second focuses on students who entered college intending to major in business or economics, fields that are often competitive and subject to capacity constraints. It instruments their realized major with the persistence rate of their program: the leave-one-out share of students entering the same college and cohort with the same intended major who remain in that field. Students entering higher-persistence programs are more likely to complete business or economics, for reasons plausibly unrelated to their own characteristics.[13] Both approaches support the conclusion that college majors significantly affect political ideology.

The IV estimates are of the same order of magnitude as our baseline estimates, though somewhat larger. For the effect of business or economics relative to the social sciences and humanities,[14] the baseline estimate of 0.23 SD compares with roughly 0.3 SD under the intended-major IV and 0.34 SD under the persistence IV. Several forces could generate these gaps; we highlight three that we view as most plausible, none of which conflicts with our main argument or with the causal interpretation of the three exercises.

First, the approaches weight different students. The main specification conditions on intended major and is thus assigns more weight to major switchers, students who graduate from a field other than the one they intended, while both IVs are identified mainly by stayers. Stayers plausibly experience larger effects, since committing to a field from the outset implies more of its coursework and deeper engagement. Second, reported majors likely contain measurement error, from form-filling or machine-reading mistakes, which attenuates the OLS estimates; the IVs correct for this. Third, both IVs may violate the exclusion restriction through coursework: students who intended a field but ultimately left it may still have taken some of its courses, while high-persistence programs may increase business and economics course-taking even among students who graduate in other fields. Such violations would inflate the IV estimates of the effects of college majors, but only insofar as exposure to coursework in a field itself affects political ideology—a channel that is consistent with the causal relationship we seek to capture. More broadly, this possibility underscores that completed major captures

[12]Because intended majors serve as the instruments, they can no longer be included as controls in this specification. We nevertheless expect any resulting omitted-variable bias to be limited, as the specification retains our other rich proxies for students' preferences over fields: intended careers and life goals. Consistent with this, in unreported results we find that omitting the intended-major controls from our main specification leaves the coefficients essentially unchanged, indicating that intended majors carry little selection-relevant information beyond these other preference controls.

[13]Supporting the independence assumption, the instrument passes a demanding falsification test: conditional on intended major, it is uncorrelated with students' baseline ideology. Since the outcome is measured at entry, the test is not limited to students matched to the graduation survey; it draws on the full baseline-survey sample of over 226,000 intenders, compared with roughly 49,000 students in the IV estimation sample, giving it the power to detect even small violations (Panel A of Table C.7).

[14]This is the relevant contrast for the persistence IV: almost 90 percent of its compliers would have pursued one of these two fields in the absence of treatment (Panel A of Table H.6).

only the extensive margin of field exposure, whereas the ideal measure—each student's actual coursework—cannot be constructed in our data.

3.2 Assessing Omitted Variable Bias

Because our baseline estimates rely on a selection-on-observables approach, we assess whether omitted-variable bias could overturn them. We proceed in two steps. First, we use a rich set of variables that we observe but exclude from the main specification: demographic and behavioral survey items, characteristics of each student's home ZIP code, and the composition of freshman-survey respondents from the student's home ZIP code.[15] We show that our controls absorb almost all of the bias these variables would otherwise generate. Thus, any remaining bias would have to come from genuinely unobserved factors, and these factors would need to generate much larger bias than the observed covariates to meaningfully change our estimates.

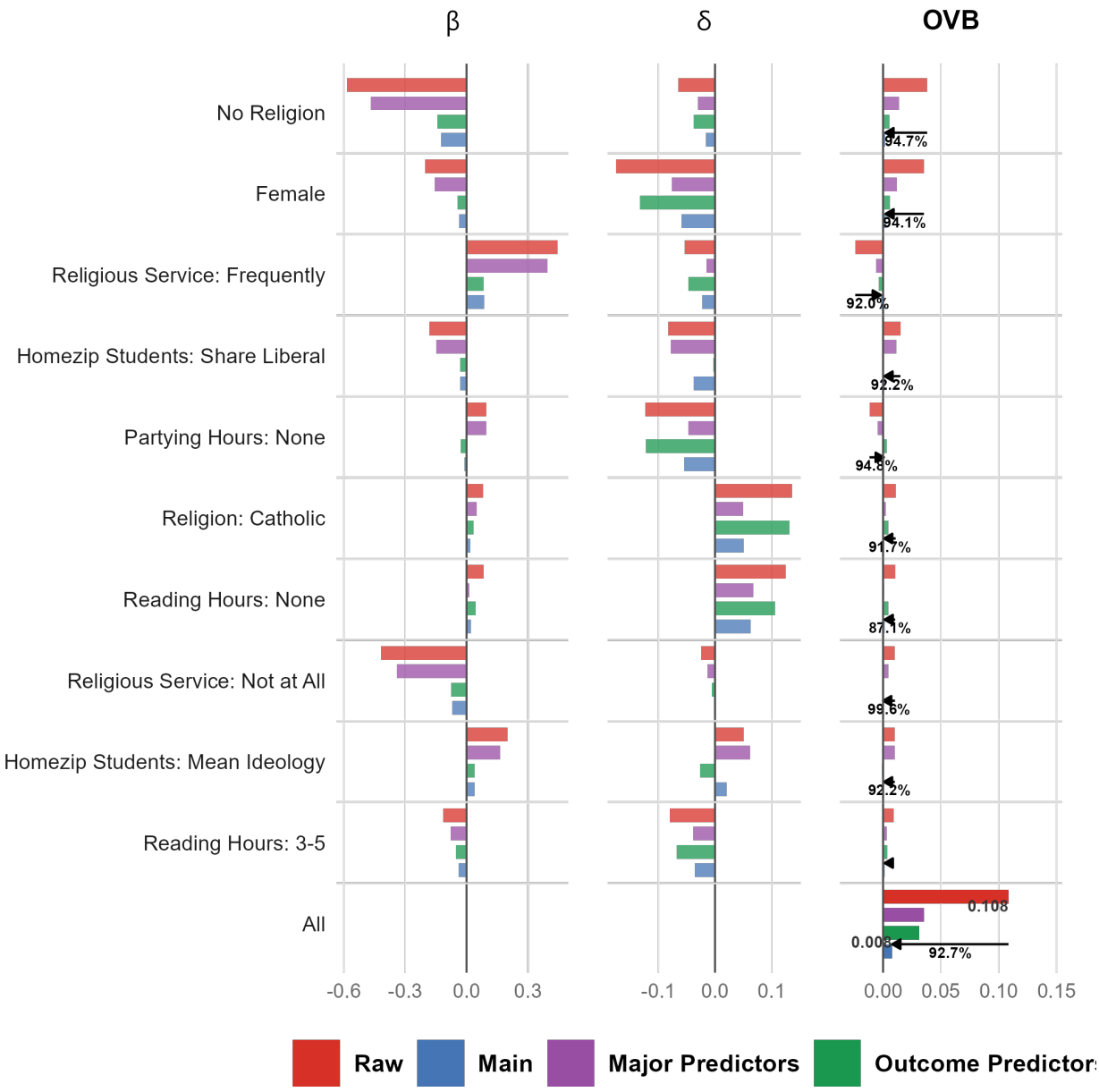
Figure C.3 shows that adding these controls has essentially no effect on the estimated coefficient. This stability does not reflect that the variables are irrelevant; rather, our main controls already capture most of the selection they would otherwise carry. Figure 3 shows this directly. For each excluded covariate, it decomposes the implied omitted-variable bias into the product of its association with graduation ideology (β) and its association with majoring in business/economics (δ), under four specifications: no controls, outcome predictors only, major-choice predictors only, and our main specification.[16]

Consider one example: students who report no reading for pleasure are both more right-leaning and far more likely to major in business or economics. In a model with no controls, omitting reading habits would bias the business/economics coefficient by about 0.010 SD; our main specification removes 87 percent of that bias. More generally, for each covariate that generates meaningful implied bias, our controls remove 87–100 percent of that bias. The bottom row aggregates across all of these covariates. With no controls, they would jointly generate substantial bias, about 0.11 SD. This is the amount by which the business/economics coefficient changes when these covariates are added to the uncontrolled regression. Our main specification removes about 93 percent of this implied bias, far more than specifications that include only outcome predictors or only major-choice predictors.

[15] Home-area variables are available for 78 to 89 percent of the sample depending on the source; students without them are retained through mean-imputation with missingness indicators.

[16] For a single omitted covariate, the implied bias equals $\beta \times \delta$, the product of its partial association with the outcome (β) and its partial association with the treatment (δ), which is the standard omitted-variable-bias formula (Angrist and Pischke, 2009).

Figure 3: Omitted Variable Bias Reduction, Observable Covariates



Notes: This figure assesses how much omitted-variable bias would be generated by additional covariates excluded from the baseline specification: demographic and behavioral survey items, and characteristics of the general population and of students in the student's home ZIP code. The sample includes all students; the treatment is an indicator for majoring in business/economics, with the humanities as the omitted reference group. The panels report each covariate's conditional association with the outcome (β), its conditional association with treatment status (δ), and the implied omitted-variable bias, $\beta \times \delta$, under four specifications: no controls (Raw), predictors of realized major only (Major Predictors), predictors of political ideology only (Outcome Predictors), and the main specification, which includes both. Home-area characteristics come from the 2000 Census and 2009–2013 ACS (ZIP-code level), presidential election returns, BLS unemployment data, the FHFA house-price index, and the Religious Congregations and Membership Study; Homezip Students variables are leave-one-out averages over all 1990–2015 freshman-survey respondents from the same ZIP code. Covariates are standardized; missing values are mean-imputed, with missingness indicators in the "All" bundle, which reports the joint implied bias from all additional controls. Rows show the ten covariates with the largest raw implied bias; arrows report the percent reduction relative to Raw.

The decomposition shows why: outcome predictors shrink the covariates' association with ideology (β), major-choice predictors shrink their association with major choice (δ), and the main specification shrinks both. Each set is well suited to its margin: lagged ideology and baseline policy positions act as value-added controls (Kane and Staiger, 2008; Chetty et al., 2014), while intended major, careers, and life goals absorb selection into fields (similar in spirit to Dale and Krueger, 2002).

Second, we bound the influence of remaining unobservables, following the omitted-variable logic of Altonji et al. (2005), Oster (2019), and Cinelli and Hazlett (2020). Throughout, we focus on the business/economics coefficient estimated relative to the humanities. Specifically, we re-estimate the main equation in the full sample, using the humanities as the omitted reference group and including the other major groups as controls.

Panel B of Figure 2 traces the business/economics coefficient as we progressively add controls, plotting it against the explanatory power of the outcome regression (R^2). The raw difference, estimated with no controls, is large, 0.53 SD ($R^2 = 0.05$). Adding intended major and the other treatment predictors reduces it to about 0.31 SD ($R^2 = 0.14$), and adding pre-college ideology and the other outcome predictors reduces it further to 0.23 SD ($R^2 = 0.40$). The figure labels also report R^2_D, the explanatory power of the treatment regression, which captures how well each specification predicts selection into business/economics: it rises from zero to about 0.42 once intended major is added and reaches 0.44 in the full specification, so the controls explain much of the observable sorting across fields even as the coefficient stabilizes.

We then ask, following Oster (2019), how far the coefficient would move under selection on unobservables; Appendix D.1 gives the formal details. The size of this adjustment depends on two inputs: $R^2_{\max}$, the R^2 the regression would attain under the ideal control, and how strongly the unobserved determinants of graduation ideology are correlated with major choice. A simple benchmark sets both at the level of our single most important control, lagged ideology (after conditioning on the other controls): we assume that the unobservables explain as much of graduation ideology as lagged ideology does and are as correlated with major choice as it is. This gives $R^2_{\max} = 0.487$ and a lower bound of about 0.22 SD.

A more conservative approach instead derives the ceiling directly: it asks how large $R^2_{\max}$ could possibly become if we kept adding covariates, where the ideal control can be thought of as each student's potential graduation ideology in the omitted group. Even this control leaves three sources of irreducible noise (heterogeneous major effects, discretization of the ideology measure, and instability in political-view responses), and a direct accounting of these sources puts the ceiling at about 2/3; we develop this calculation in Appendix D.1. Even at this ceiling, again taking the unobservables to be as correlated with major choice as

lagged ideology, the strongest predictor of the outcome, the lower bound is about 0.19 SD; doubling that assumed correlation leaves it at about 0.14 SD, which does not meaningfully change our conclusion.

Appendix D.2 reaches the same conclusion using the approach of Cinelli and Hazlett (2020), which expresses sensitivity to omitted-variable bias in terms of partial R^2 values.

Together, these analyses indicate that omitted-variable bias is very unlikely to explain the estimated effects of college majors on students' political ideology.

3.3 Quasi-Experimental Evidence

To complement the selection-on-observables analysis, we implement a quasi-experimental design that exploits sharp increases in enrollment in business and economics programs at specific colleges.[17] As shown below, these expansions affect not only students' major choices, but also the coursework of students who major in other fields. 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.

Throughout this analysis, a program refers to the aggregate business and economics group: we measure expansions in the combined share of graduates in these fields, rather than in specific majors. Detailed-major shares are small and more volatile, so we aggregate to business and economics to obtain a stable measure of expansions.

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 $B_{ct} = \frac{N_{ct}^{BE}}{N_{ct}}$.

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 $\Delta B_{ct} \geq 0.30 \quad \text{and} \quad |\Delta B_{c,t-1}| \leq 0.15$. 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

[17] We focus on shocks to business and 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, 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, these fields experienced substantial enrollment expansions in recent decades, providing a quasi-experimental setting.

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.

We interpret such sharp increases as supply-side expansions, reflecting colleges' decisions to expand their business and economics offerings (or to shrink or close other programs, particularly in the social sciences and humanities) rather than shifts in student demand. 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.[18] Let S_c denote the entry cohort corresponding to the first qualifying shock at college c: an expansion observed in the graduating class of year g is assigned to the cohort entering in $g-4$. We identify shocks at 390 colleges included in the baseline survey, 141 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.[19]

We estimate the following two-way fixed-effects event-study specification to trace the dynamic effects of these college-level shocks:

$$y_{ict} = e_c + s_t + \sum_{k \neq -3} \rho_k \mathbf{1}[t = S_c + k] + f(y^0_{ict}) + v_{ict} \tag{2}$$

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 because students typically declare their majors during the third year: cohorts entering one or two years later are still enrolled when course offerings expand, so their majors and coursework may already respond, while the $k = -3$ cohort has essentially completed college by then. The terms e_c and s_t denote college and cohort fixed effects. y^0_{ict} denotes the lagged dependent variable, which enters only when the outcome is a graduation-survey attitude or behavior.[20]

[18] 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 absolute change in the previous year (one-half, two-fifths, or one-third of the required increase), and the minimum number of business or economics graduates in the expansion year (0, 25, or 50). For each definition, we estimate a pooled two-way fixed-effects specification with college and entry-cohort fixed effects. We select the definition under which the shock predicts majoring in business or economics most strongly, that is, with the largest t-statistic for the effect of the shock on majoring in these fields (the "first stage"). Figure H.6 plots this effect against the estimated effect on graduation ideology for all 27 definitions. This criterion follows the logic of optimal instruments (Chamberlain, 1987).

[19] We retain only the first qualifying shock at each college, so the number of shock events equals the number of treated colleges. Because sample restrictions differ across surveys and estimators, we also report the corresponding counts in each figure.

[20] This is baseline ideology, policy positions, or life goals when the outcome is a survey response on these topics; we include a separate fixed effect for each discrete response rather than controlling linearly. We omit the term when the outcome is major choice, as intended majors may themselves be affected when colleges

We also estimate a specification with a single post-shock indicator, which captures the average effect across the three 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. 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; Davis and Von Wachter, 2011). Anecdotal evidence supports this interpretation. For example, two of the largest colleges experiencing such expansions since 2010 received substantial donations earmarked for their business schools, 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 a modified version of Equation 2 (excluding y^0) using intended major and baseline ideology as outcomes. 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 1 pp in intended business and economics majors, consistent with students responding to expanded program offerings.

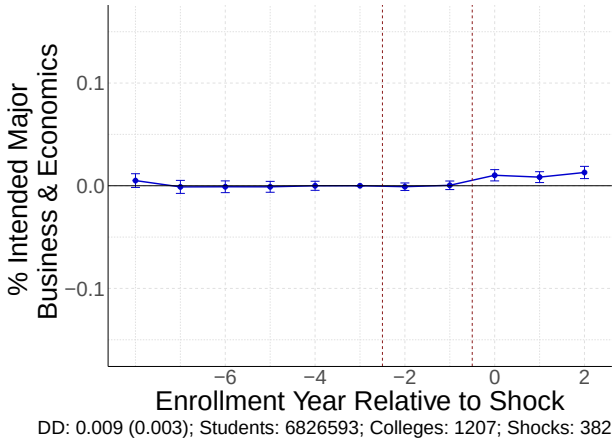
However, the increase in actually majoring in business or economics is even larger, suggesting that part of the increase occurs in the first week of college and part only during college. Panel C of Figure 4 shows that following a shock, the probability of majoring in business/economics increases by about 3.2 pp. This effect is statistically significant and indicates a meaningful increase in exposure to these fields.

expand certain fields—Panel A of Figure 4 shows that they are, while Panel B shows that baseline ideology is not—and when the outcome is course-taking, for which the baseline survey has no analog.

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

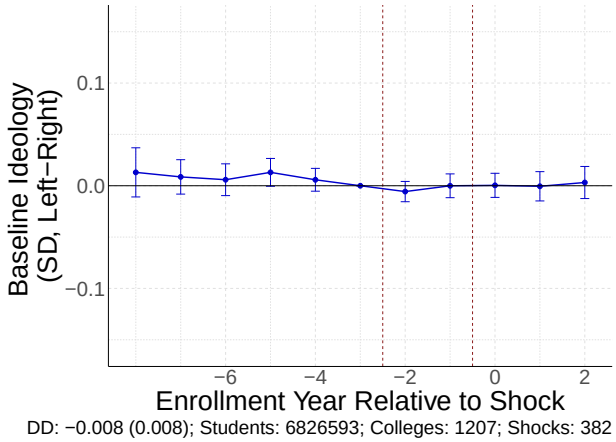
Falsification Tests: Baseline Survey

(A) % Intended Business & Economics



DD: 0.009 (0.003); Students: 6826593; Colleges: 1207; Shocks: 382

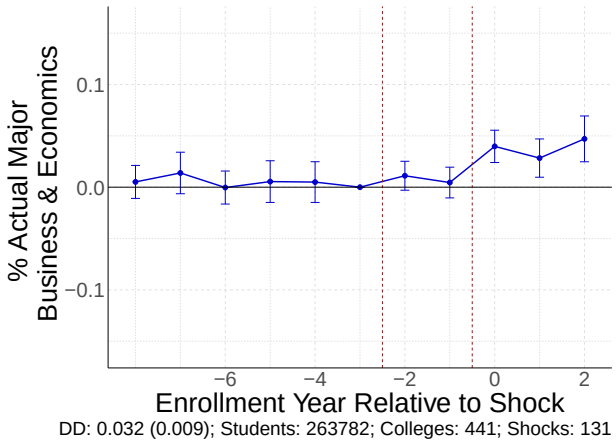
(B) Baseline Ideology (SD)



DD: −0.008 (0.008); Students: 6826593; Colleges: 1207; Shocks: 382

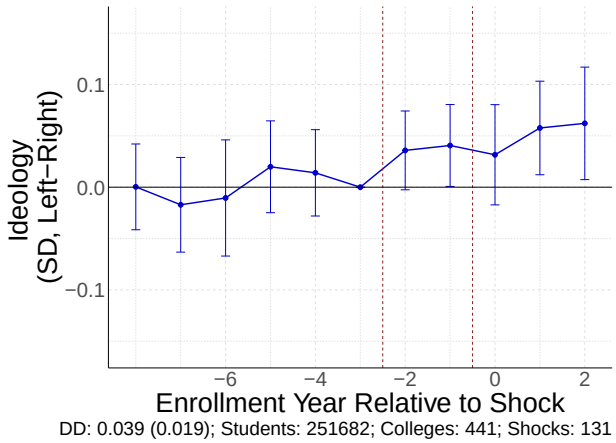
Graduation Survey

(C) % Actual Business & Economics



DD: 0.032 (0.009); Students: 263782; Colleges: 441; Shocks: 131

(D) Graduation Ideology (SD)



DD: 0.039 (0.019); Students: 251682; Colleges: 441; Shocks: 131

Note: These figures plot event-study coefficients estimated from Equation 2, 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 from the DD estimation the two cohorts immediately preceding the shock because they may already be partially exposed to it. 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. "Colleges" denotes the total number of colleges in the corresponding analysis dataset, while "Shocks" denotes the total number of treated colleges.

Columns (1)–(5) of Panel A in Table H.8 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.[21]

Finally, Panel D of Figure 4 shows that graduation ideology shifts to the right by about 0.04 SD, with no pre-trend. The estimate is significant at the 95 percent level.[22]

As shock timing varies across colleges, our design is a staggered difference-in-differences design. Recent work shows that TWFE regressions can be biased in such settings when treatment effects are heterogeneous (surveyed in Roth et al., 2023). To address this concern, we also report results using the imputation estimator (Gardner et al., 2026; 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 H.7 reports the main event-study estimates using the imputation estimator, which are similar to the TWFE estimates and more precise.

A natural question is whether the expansions can serve as an instrument for majoring in business or economics. Under that interpretation, the shock's effect on ideology is attributed entirely to the students it moves into these majors, so the implied effect per induced major switch is the ideology effect divided by the increase in majoring, roughly 1.2 SD (0.038/0.032). This is several times larger than both our baseline contrast of 0.23 SD and the persistence IV estimate of 0.34 SD, though the ratio is very noisy. We do not interpret the expansions this way because the required exclusion restriction is unlikely to hold. An expansion changes what students study beyond their major.

Two findings support this interpretation. First, following a shock, students are 2.4 pp less likely to ever take a humanities or arts course (Column 6 of Panel A in Table H.8). While this is the only measure of course-taking that we observe, and thus cannot fully assess changes in course taking, this decline suggests the existence of changes in course-taking beyond changes in major choice. Second, among students with a low predicted probability of majoring in business or economics,[23] the shock raises majoring in these fields by only 0.8 pp, and its effects on every other major group are also small and statistically insignificant, yet it reduces

[21] This is the only measure we observe for non-major coursework.

[22] Columns (1)–(3) of Panel B in Table H.8 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.

[23] We predict each student's probability of majoring in business or economics with a probit using flexible predictors for pre-college characteristics: demographics, parental education and careers, life goals, intended major and career. Low (high) refers to students below (at or above) the median predicted probability.

humanities course-taking by 1.9 pp and shifts ideology by 0.034 SD, nearly as much as in the full sample (Table H.9). For these students, the ideological shift can hardly run through major choice; a possible explanation is coursework.

3.4 Broader Impacts of Academic Fields

Policy positions. We next examine whether the effects on ideology extend to other outcomes. First, we examine how college majors shape students' worldviews, as reflected in their policy positions. We estimate equation 1 using various positions as outcome variables.

Table 2 reports the estimates for the policy positions we highlight below and for a policy-position index; estimates for the remaining policy positions appear in Appendix Table H.5. Compared with the natural sciences, the social sciences and humanities consistently move students toward more liberal positions across nearly all policy domains. Specifically, students become more supportive of abortion rights, higher taxes, national healthcare, and become less supportive of claims denying racial discrimination. 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.

The appendix reports similar patterns for other policy issues such as the abolition of the death penalty, gun control, marijuana legalization, LGBTQ rights, tough-on-crime policies, belief in the ability to change, and climate policy. Climate policy positions are a notable exception: students in the natural sciences are more likely than those in the social sciences and humanities to view climate change as a federal priority, potentially due to the greater attention to related issues in such fields.

We next examine how the effects on ideology align with effects on specific policy positions. To do so, we predict each student's ideology based on their responses to all policy position questions. These predicted values reflect how closely each student's policy positions correspond to a liberal or left-leaning outlook. The index estimates in Column (5) of Table 2 imply that moving from business or economics to other social sciences or humanities shifts this policy-position index by about 0.25 and 0.24 SD, respectively, suggesting that effects on policy positions closely align with the overall ideological effects.[24] [25]

[24]Column (4) of Table H.6 and Table H.8 report estimates for the policy-position index using complementary approaches. The estimates are positive, though noisy in the difference-in-differences specification.

[25]Appendix E presents a decomposition exercise that assesses how much changes in policy positions can account for the ideological effects of majors. The results suggest that economic positions explain a substantial

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 ideological effects extend 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. Column (6) of Table 2 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. The remaining life goals, reported in Appendix Table H.5, shift along the same ideological lines. Social sciences make students place substantially greater weight on social goals, with smaller increases among humanities and health and education students. Students in the social sciences and in health and education are also more likely to prioritize helping others, while students in economics and business, and in engineering, are less likely to do so. Humanities and social science students 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 it, prioritizing meaning instead.

We then analyze behaviors, reported in columns (7)-(9) of Table 2. 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 politics more often—again reflecting greater civic engagement. They also report discussing their coursework less frequently (Appendix Table H.5). Effects on voter registration are small in magnitude: social sciences and humanities students are about one pp more likely to be registered to vote, 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.

share of the ideological effect of business and economics relative to the natural sciences, while cultural positions explain a substantial share of the relative effect of the humanities and social sciences.

Table 2: Effects of College Majors on Policy Positions, Life Goals, and Behaviors

	Policy Positions					Life Goals & Behaviors			
	(1) Abortion	(2) Taxes	(3) Healthcare	(4) Race	(5) Index	(6) Well Off	(7) Demonstrated	(8) Discussed Politics	(9) Vote Reg.
Business &	-0.03***	-0.17***	-0.17***	0.07***	0.13***	0.32***	-0.04**	0.03***	-0.01
Economics	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Engineering &	-0.04***	-0.08***	-0.09***	0.07***	0.07***	0.15***	-0.06***	-0.06***	-0.02***
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Health &	-0.11***	-0.03***	0.02	-0.04***	0.09***	0.02*	0.01	-0.05***	-0.00
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Social	0.05***	0.05***	0.07***	-0.12***	-0.12***	0.07***	0.06***	0.24***	0.01***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Humanities &	0.03***	0.05***	0.05***	-0.10***	-0.11***	-0.03**	0.08***	0.19***	0.01**
Arts	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
N	266413	205167	157964	278065	302403	308891	163726	281267	90750
Summary Statistics (raw scale)									
Graduation Mean	2.70	2.70	2.87	1.71	-0.08	2.77	1.22	1.95	0.89
Graduation SD	1.17	0.96	0.96	0.77	1.01	0.92	0.47	0.66	0.32
Change in Omitted Group	0.29	0.04	0.08	-0.12	-0.21	-0.25	-0.22	-0.18	

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; sample sizes vary across columns because not all questions appear in every survey year. Columns (1)-(4) are policy positions, originally coded on a 1–4 scale (1=Disagree Strongly, 2=Disagree Somewhat, 3=Agree Somewhat, 4=Agree Strongly): Abortion should be legal (Abortion); Wealthy people should pay a larger share of taxes than they do now (Taxes); A national health care plan is needed to cover everybody's medical costs (Healthcare); and Racial discrimination is no longer a major problem in America (Race). The Index in column (5) is the standardized prediction of self-reported ideology from students' responses to all policy questions available in their survey year. Column (6) is a stated life goal: being very well off financially (Well Off), originally coded on a 1–4 scale (1=Not important, 2=Somewhat important, 3=Very important, 4=Essential). Columns (7)-(9) are behaviors: participated in organized demonstrations (Demonstrated) and discussed politics (Discussed Politics), both originally coded on a 1–3 scale (1=Not at all, 2=Occasionally, 3=Frequently), and an indicator for being registered to vote (Vote Reg.). Each outcome except vote registration is standardized by subtracting the mean and dividing by the standard deviation of the graduation response among all graduation-survey respondents; vote registration is not standardized, so its coefficients are in probability units. "Graduation Mean" and "Graduation SD" report the raw-scale values used for the standardization, so multiplying a coefficient by the SD row converts it into raw scale points. For vote registration, these rows report the raw share of students registered to vote and its standard deviation in the sample; the index column is reported in its own standardized units. "Change in Omitted Group" reports the average raw-scale change from baseline to graduation among students in the omitted group (natural sciences) with valid responses in both surveys; it is blank for vote registration, which is measured only in the graduation survey. Estimates for the remaining policy positions, life goals, and behaviors appear in Appendix Table H.5. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4 Mechanisms of the Impacts

4.1 Motivating Evidence on Potential Mechanisms

The results in the previous section show that academic fields substantially shape students' political attitudes. This section explores the mechanisms, focusing on three main candidates: academic environments, peer socialization, and earnings expectations. As a starting point, Figure 5 and Table H.10 report descriptive correlations between the estimated major effects ($\hat{\delta}_m$ from equation 1) and proxies for these three mechanisms measured at the major level.

The first proxies the major's academic environment using the political composition of faculty in its associated departments, drawn from the HERI Faculty Surveys conducted every three years since 1989. Faculty members teach the courses, choose the syllabi, write the textbooks, develop the main theories, and produce the research that defines each field. Their political composition therefore serves as a proxy for the ideological orientation of the academic content to which students are exposed. For each major, we calculate the simple average political ideology of all faculty members in departments teaching that major across colleges.

The second proxy captures the major's social environment. We measure the baseline political composition of each major using the average political ideology at college entry of students who ultimately graduated in that major, pooling students across colleges. Since students share classes and spend time with others who chose the same major, this measure can be interpreted as the ideological leaning of peers in the student's major.[26]

The third proxy captures earnings expectations. 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 to be at least partly aware of these differences. By graduation, they may also have part-time jobs or job offers that further shape their expectations. Expected earnings may therefore influence political attitudes through self-interest: students in higher-earning fields, such as engineering and economics, may be more likely to support policies that benefit them financially, such as lower income taxes.

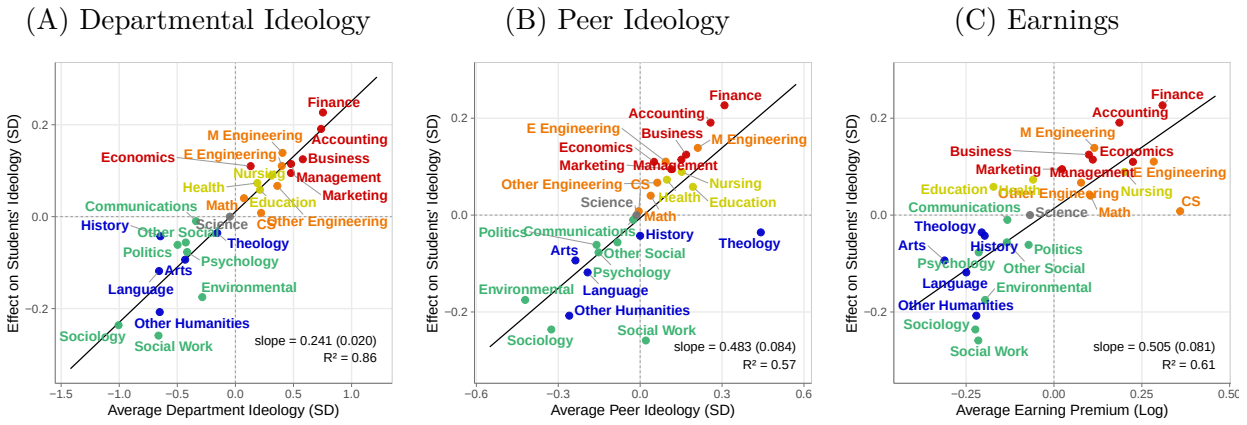
We proxy for these expectations using College Scorecard data on median post-graduation earnings by college and major, measured four or five years after graduation, or one year after completion where no later measure exists.[27] We then average this measure across

[26]The peer measure and the estimated major effects $\hat{\delta}_m$ are constructed from overlapping samples of HERI respondents. This does not introduce a mechanical correlation, because $\hat{\delta}_m$ captures the shift in students' ideology between college entry and graduation, and explicitly controls for individual baseline ideology, while the peer measure summarizes only baseline ideology.

[27]Three releases carry bachelor's-level earnings: the 2018-19 and 2021-22 releases measure wages four years after graduation and the 2019-20 release five years after, so each program-by-release record is observed at a single horizon. Programs that recur across releases contribute records at both horizons to the same

colleges, weighting each college-by-major cell by the number of students in our analysis sample. Throughout this descriptive analysis, we restrict attention to colleges with non-missing earnings data in the College Scoreboard, yielding 303,363 students across 441 colleges, aggregated into 27 detailed major groups.[28]

Figure 5: Estimated Major Effects on Political Ideology vs. Candidate Mechanisms



Note: Each panel plots the estimated effect of major on ideology ($\hat{\delta}_m$ from equation 1, in SD; y-axis) against one of three candidate mechanism measures at the major level (x-axis). Major labels are colored by broad field: 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. Each panel includes the fitted line and reports the slope (with standard error in parentheses) and the R^2. The natural sciences (the omitted reference category in the underlying major regression) appear as a 27th observation with $\hat{\delta}_m = 0$. *Panel A:* Departmental ideology is the simple average ideology of all faculty respondents in the major, pooled across the colleges in the main estimation sample (157,152 responses across 400 colleges). *Panel B:* Peers' baseline ideology is the average baseline ideology of CSS students who graduated with the major. *Panel C:* Earnings of the major from the College Scorecard, averaged across colleges weighted by the CSS student count in each college-by-major cell. Sample: 303,363 students from 441 colleges with non-missing graduation ideology and Scorecard earnings data, aggregated to 27 major groups.

All three candidate mechanisms are positively associated with the estimated major effects, but their explanatory power differs substantially. Departmental ideology is the strongest

college-major cell, which we average weighting each record by its number of graduates with measured earnings. Where a cell has no four- or five-year wage in any release, we use earnings measured one year after completion, which only the 2018-19 and 2019-20 releases report. To place all programs on a comparable five-year scale, we add to each log wage the median gap between log five-year wages and log wages at its own horizon, within the same CIP code and computed separately for each release. These adjustments are 0.14 log points for four-year earnings in the 2018-19 release and −0.02 in the 2021-22 release, and 0.41 and 0.35 log points for one-year earnings in the 2018-19 and 2019-20 releases.

[28]The Scorecard reports earnings only for students who received federal aid, about 70 percent of graduating students. Our results are robust to instead using the Census Bureau's Post-Secondary Employment Outcomes (PSEO), which cover all graduates regardless of aid but only a subset of institutions.

predictor, accounting for about 86 percent of the variation in $\hat{\delta}_m$. Peers' baseline ideology and expected earnings account for about 57 and 61 percent, respectively. In the peer panel, most majors sit close to the regression line, with two clear exceptions: social work and theology each lie about 0.25 SD from the line, attracting relatively right-leaning students but shifting them leftward. In the earnings panel, computer science and social work shift students about 0.19 and 0.16 SD further left than predicted by their expected earnings, while education shifts them about 0.13 SD further right. The departmental ideology panel shows no comparable deviations. No major deviates from the line by more than 0.12 SD: the largest residuals are history to the right and environmental science and social work to the left, suggesting that departmental ideology comes closest to capturing the pattern of estimated major effects.

The contrast becomes even sharper in the joint specification (column 4 of Table H.10). Once departmental ideology is included alongside peers' ideology and the earnings premium, the explained variation rises only marginally above the departmental-only value ($R^2 = 0.88$ versus 0.86), while the partial associations of peers and earnings with $\hat{\delta}_m$ fall by roughly three-quarters or more, with the peer measure retaining only marginal significance and the earnings premium statistically insignificant. The departmental ideology coefficient, by contrast, remains large and highly significant. In other words, conditional on the political composition of the major's departments, neither peers' baseline ideology nor expected post-graduation earnings adds meaningful explanatory power for the cross-major pattern of estimated effects.

4.2 Assessing Potential Mechanisms

These descriptive patterns motivate the more rigorous analyses that follow. In this subsection, we exploit variation across students taking the same major to identify the role of each factor separately. When examining peers and departmental ideology, we use students' intended rather than realized major to assign the mechanism proxy. The main reason is that realized major is a choice made during college and may itself be influenced by the peers and faculty a student encounters. Measures based on realized major would therefore condition on an endogenous outcome. Intended major, by contrast, is reported at college entry, before such influences are likely to occur, allowing cleaner identification of each mechanism's effect. A second advantage, specific to peers, is precision: intended major is observed for every respondent in the baseline survey, so defining peer composition this way allows us to construct the measure using a much larger sample.

Both the peer and departmental ideology measures are also subject to measurement error: the surveys cover only part of each entering cohort or faculty, so the in-sample averages are noisy, which attenuates the estimates toward zero. Following the procedure detailed in

Appendix G, we adjust each cell's measure for its survey coverage. The adjustment combines the observed average with the cell's predicted composition, with the weight on the observed average increasing in the share of the cohort observed. When coverage is complete, the adjusted measure equals the observed average; as coverage falls, the observed deviation from the predicted composition is increasingly shrunk toward zero, correcting for the attenuation induced by incomplete coverage.

Peer effects. We first test for peer effects and assess whether differences in peer ideological composition can explain the effects of majors. Appendix Section F describes this analysis in detail. We measure peer ideology as the average baseline ideology of students entering the same college in the same year and intending to major in the same field, and estimate its effect on students' ideology at graduation. We control for expected peer composition, measured as the corresponding average across the five entry cohorts centered on the student's own cohort. The effect is therefore identified from idiosyncratic, cohort-specific deviations in realized peer ideology from the level typical of that college and major. We also report additional specifications that control for a rich set of college, major, and cohort fixed effects, as well as student-level controls. Falsification tests reported in the appendix find at most a slight association between this variation and students' own baseline ideology, which the student-level controls should almost fully absorb.

Although peers' ideology is strongly correlated with students' graduation ideology in the raw data, the relationship becomes small and statistically insignificant once selection into colleges and majors is accounted for. The estimates are sufficiently precise to imply that differences in the ideological composition of peers could explain at most about 4 percent of the estimated difference between business and economics and social sciences and humanities. These results do not rule out effects arising from close friendships or other intensive peer interactions, but they suggest that the broader peer-composition changes induced by choosing a different major are unlikely to explain our findings.

Earnings expectations and self-interest. We examine how the major effects vary with expected wages, proxied by the median wages of graduates from each college–major group. We re-estimate equation 1 separately for students in college-major programs with above- and below-median expected earnings, where the median is taken within each detailed major group over the students in our sample. Results are presented in Figure H.10. 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.[29]

Departmental environments. The evidence so far suggests that neither peer ideology composition nor earnings expectations explains the observed effects of academic fields on students' ideology. The rest of 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. Figure H.11 shows time trends in this measure 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. The leftward shift is smallest for business and economics faculty, who remain the least liberal group throughout.

We summarize the data at the department level, measuring the average ideology of faculty within each department (college-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 departmental ideology measure to students based on their college and intended major. Using intended rather than realized major is conservative and avoids potential endogeneity from selection into departments, as discussed earlier. In unreported results, we find similar conclusions when matching students based on their realized majors. We then estimate the following specification:

$$p_{icmt} = \tau F_{cm} + \eta_{tm} + \psi_{ctM} + \zeta_{icmt} \tag{3}$$

[29] 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 Great Recession, 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 Great Recession. 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 H.8 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 crisis show effects comparable to those of students graduating during the recession, despite being most directly exposed to its consequences.

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 departmental 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; and ζ_{icmt} is an idiosyncratic error term. We adjust the measure for partial survey coverage exactly as for peers (Appendix G).

We assume that students' potential ideology is orthogonal to departmental ideology, conditional on controls. Under this assumption, the coefficient on τ captures the effect of studying the same major in a more right-leaning department. This effect may operate through several channels, including exposure to academic content, teaching methods, or faculty interactions. A key threat is that students may sort into majors by ideology, particularly at colleges where faculty lean differently across fields. Our main control is a set of college-by-cohort-by-broad-field fixed effects, which compare only students who entered the same college in the same year and intended a major in the same broad field. The effect of departmental ideology is then identified from differences across closely related departments within that field-group, such as political science and sociology. We additionally include major-by-cohort fixed effects, which absorb differences in average ideology across majors. Finally, in the full specification, we also control for our main individual-level controls, lagged ideology and proxies, and intended major and proxies. For sorting to bias our estimates, it would therefore have to occur across these similar departments and operate through characteristics not captured by these baseline measures.

We examine whether students' baseline ideology is correlated with departmental ideology, conditional on the fixed effects included in Equation 3. Table H.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 departmental 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.

Table 3 shows that the effects of departmental ideology are meaningful. The adjusted estimates are about 0.035 SD, implying that students studying the same major in one SD more left-leaning departments become about 0.035 SD more left-leaning by graduation. As we sequentially add controls, the estimated magnitudes remain stable while precision slightly improves. Column 4 turns the specification into a horse race, adding the two competing mechanisms, peer ideology and expected earnings, measured as in the analyses above. The departmental effect remains of similar magnitude, while the peer and earnings coefficients are small and statistically insignificant. This reinforces the earlier evidence that neither peer composition nor earnings expectations drives the ideological effects of fields of study.[30]

Moreover, when restricting the sample to departments with more extensive coverage in the faculty survey, the results remain significant and increase to 0.051, confirming their robustness.[31]

Panel B of Table 3 reports results by major group. In the humanities and social sciences, studying the same major in a 1 SD more left-leaning department shifts students' ideology leftward by about 0.076 and 0.072 SD, respectively, while the estimates show no significant effects in other fields; the economics and business estimate is also positive, at about 40 percent of the humanities and social science magnitudes, but imprecise. Column 7 pools the three field groups driving the main effects, the humanities, social sciences, and business and economics, and adds the peer and earnings controls of column 4: the effect is 0.053 SD and highly significant, while peers and earnings remain small and insignificant. These magnitudes suggest that variation across departments could explain much of the major effects.

[30]The peer measure is the coverage-adjusted, leave-own-quarter-out average baseline ideology of the student's college-cohort-intended-major cell, and the column controls for the cell's potential composition, the same average over the five cohorts centered on the student's own; expected earnings is the standardized within-college log median earnings of the intended major from the College Scorecard. Because the peer measure excludes the student's own quarter of the sample, every fixed effect and control in this column is interacted with the quarter indicator, so the estimate averages the four split-sample estimates. Peer ideology is unavailable for 10 percent of the sample and expected earnings for 14 percent; each is set to its mean with an indicator for missingness, keeping the sample unchanged while the coefficients are identified from students with observed measures. See Appendix G for a detailed description of these measures and the adjustment.

[31]The stability of the adjusted estimates supports the adjustment itself: raising the minimum coverage share from 0.1 to 0.5 moves the unadjusted estimate from 0.013 to 0.033, but the adjusted estimate only from 0.035 to 0.051. The remaining movement is consistent with noise in measured coverage, whose denominator is the IPEDS count of full-time instructional staff: it is measured at the college rather than the department level, excludes part-time instructors, and is interpolated for 2000 and 2001. Since the adjustment has less to do as coverage approaches one, noise in measured coverage attenuates the low-coverage columns most.

Table 3: Effects of Faculty Ideology on Students' Ideology

Panel A. Various Specifications, All Majors

	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.012*	0.013*	0.013**	0.011*	0.018***	0.033***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.012)
Estimate (Adjusted)	0.035**	0.035**	0.035***	0.027*	0.039***	0.051***
	(0.014)	(0.014)	(0.013)	(0.015)	(0.013)	(0.018)
Peers (Adjusted)	–	–	–	0.014	–	–
				(0.020)		
Expected earnings	–	–	–	-0.003	–	–
				(0.006)		
N	164,291	164,291	164,291	164,291	123,482	37,992
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Major Predictors	No	Yes	Yes	Yes	Yes	Yes
Outcome Predictors	No	No	Yes	Yes	Yes	Yes
Min College Coverage	0.1	0.1	0.1	0.1	0.3	0.5

Panel B. By Major-Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Economics & Business	Engineering & Math	Health & Education	Humanities & Arts	Natural Sciences	Social Sciences	Hum., Social & Business
Estimate	0.014	-0.004	0.004	0.036***	-0.028	0.028***	0.023***
	(0.012)	(0.011)	(0.014)	(0.014)	(0.020)	(0.010)	(0.008)
Estimate (Adjusted)	0.029	-0.014	0.015	0.076***	-0.044	0.072***	0.053***
	(0.029)	(0.032)	(0.031)	(0.028)	(0.044)	(0.022)	(0.018)
Peers (Adjusted)	–	–	–	–	–	–	0.027
							(0.026)
Expected earnings	–	–	–	–	–	–	0.004
							(0.008)
N	27,508	21,814	27,211	28,724	20,227	38,620	94,852
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Major Predictors	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Predictors	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Min College Coverage	0.1	0.1	0.1	0.1	0.1	0.1	0.1

Notes: Estimates of the effect of faculty ideology on students' political ideology, from equation 3: the outcome is graduation ideology and the treatment is department-level faculty ideology matched to each student's college and intended major. Panel A reports various specifications; Panel B the fully controlled specification by intended-major subsample, with a final column pooling the humanities, social sciences and business and using the column (4) specification. Adjusted estimates correct for attenuation from incomplete faculty coverage. Column (4) of Panel A and column (7) of Panel B add the competing mechanisms to the specification in column (3), on the same students. "Peers (Adjusted)" is the mean baseline ideology of students entering the same college and year intending the same major, computed leaving out the focal student's quarter of the sample and corrected for partial coverage of the cell; "Expected earnings" is the standardized College Scorecard within-college log-wage premium of the intended major. Where a measure is unavailable it is set to its mean among the students who have it and an indicator flags those students, so the columns keep the full sample; because an imputed value is constant within its indicator, the reported coefficients are still identified only off students for whom the measure is observed. The indicators are included but not reported. Those two columns also control for potential peers, the same cell averaged over the five entry cohorts around the student's own and corrected the same way; that coefficient is not reported. Because the peer mean is split by quarter, every fixed effect and every control is quarter-interacted in them, so each reported estimate is the average of the four split-sample estimates. Faculty ideology is standardized across the 22,127 college-by-department cells with at least 3 surveyed faculty, at colleges where the faculty survey covers at least 10 percent of instructional staff; estimates are per standard deviation. The faculty survey records each respondent's department but reports no department-level staff counts, so coverage is measured at the college and applied uniformly to its departments. Because a department's own coverage may differ from its college's, the correction is imperfect and the adjusted estimates may remain somewhat attenuated. "Fixed Effects" are major-by-cohort and college-by-major-group-by-cohort. "Major Predictors" are intended majors, careers and life goals; "Outcome Predictors" are baseline ideology and policy positions. "Min College Coverage" is the minimum share of the college's instructional staff responding to the faculty survey. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The average gap in our departmental-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 1.26 SD. Extrapolating the within-major departmental ideology effect, estimated on a pooled sample of these three field groups, to this between-major gap implies that studying the same major in a 1.26 SD more left-wing department is associated with about a 0.077 SD leftward shift in graduation ideology, accounting for about 34% of the relative effect of these majors.[32]

While the coverage adjustment should in principle absorb the attenuation from observing only part of each department's faculty, this calculation is still likely attenuated. Measured coverage is itself noisy, leaving some residual attenuation. In addition, the treatment assigns each student the faculty of the intended department, while some students graduate from a different field and some coursework is taught by nearby departments, so actual faculty exposure is measured with error. All of these bias the estimate downward, so the true contribution is likely larger, potentially explaining the gap in full. This is in line with the raw correlation presented in Table H.10.[33] By contrast, the corresponding upper bound for peer composition, reported above, is about 4 percent of the same gap. Together, the evidence in this section suggests 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 academic environments across majors.

5 Implications

Field specialization is a central characteristic of higher 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. Specifically, our findings have implications for political differences between groups. In this section, we quantify several key consequences.

The college divide. First, our results suggest that the widely discussed "college divide" is partly driven by the specific fields students study in college. We illustrate this through a counterfactual simulation shown in Panels A and D of Table 4. Panel A focuses on ideological

[32]We estimate Equation 3 on students who intended to major in the social sciences, humanities, or business and economics, obtaining a within-major departmental effect of about 0.061 SD per SD of departmental ideology. Multiplying by the 1.26 SD gap in average departmental ideology gives $1.26 \times 0.061 \approx 0.077$ SD.
[33]Moreover, as shown in Figure H.11, 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.6.

change during college. Students in our sample enter college with an average ideology score of 3.000 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.

Consequently, the ideological gap between college graduates and non-graduates would narrow substantially. To quantify this, we use the CCES (Cooperative Election Study Survey).[34] At ages 18–19, the ideological gap between college students and individuals with no college education is 0.075. By ages 24–27, the gap between college graduates and individuals with no college education grows to 0.315. Under the counterfactual where all students major in economics or business, this later gap declines to 0.200. Field specialization therefore accounts for about 36 percent of the observed college–noncollege gap and 48 percent of its increase from the age-18–19 baseline.

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 H.2, this implies an approximately 6.3-percentage-point increase in the probability of voting Democratic ($0.8 \times 6.7 + 0.25 \times 3.8 \approx 6.3$).[35]

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.

Ideological divisions among college graduates. 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 4, differential major selection explains about 76 percent of the

[34] Using the CCES, Figure H.5 shows how ideology evolves among young adults differently by their college status. Ideology 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.

[35] 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 4: Field Specialization and Politics: Counterfactuals

Panel (Counterfactual)	Baseline	Graduation Observed	Graduation Counterfactual	Reduction (% observed)	Reduction (% change)
A. Average Ideology					
(All Business & Economics)	3.000	2.872	2.987	4.0%	89.7%
B. Ideology Dispersion					
(Uniform major)	0.661	0.722	0.694	3.9%	45.8%
C. Gender Gap					
(Uniform major)	0.161	0.203	0.171	15.7%	76.2%
D. College–Noncollege Gap					
(All Business & Economics)	0.075	0.315	0.200	36.4%	47.8%

Note: Panels A–C report the mean, variance, and male–female gap in political ideology among college students in the HERI sample, separately at baseline and graduation. Panel D reports the ideological gap between students and non-students in the CCES. At baseline, it compares college students with individuals without any college education at ages 18–19; at graduation, it compares college graduates with individuals without any college education at ages 24–27. The All Business & Economics counterfactual in Panel A assigns every HERI student to this group of fields, and Panel D applies the implied ideological shift to the observed CCES gap. The uniform-major counterfactual in Panels B and C assigns all students to the same major. The result is identical regardless of the common major because a uniform level shift does not affect either the variance or the gap between groups. Reduction (% observed) is the absolute difference between the observed and counterfactual values divided by the absolute observed value. Reduction (% change) divides the same difference by the absolute change from baseline to graduation. Ideology ranges from 1 to 5. The HERI sample contains 293,523 students; Panel C contains 293,267 after excluding 256 students whose sex is not reported. The baseline and observed CCES samples contain 8,092 and 21,679 respondents.

increase in the gender gap during college (0.032 of 0.042 points) and about 16 percent of the graduation gap (0.032 of 0.203 points).

As shown in Panel D of Table 4, 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 academic 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.[36] They have also contributed to declining public confidence in higher education among Republicans.[37] 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, 2003; 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.

[36] Associated Press (2023), "Mississippi auditor says several college majors indoctrinate students and should be defunded," and Time (2023), "A Florida education bill would ban gender studies and diversity programs at universities."

[37] 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.

References

Ainsworth, Robert, Rajeev H Dehejia, Andrei Munteanu, Cristian Pop-Eleches, and Miguel Urquiola, "Malleable Minds: The Effects of STEM- vs. Humanities-Focused Curricula," Working Paper 34502, National Bureau of Economic Research 2025.

Alan, Sule, Ceren Baysan, Mert Gumren, and Elif Kubilay, "Building Social Cohesion in Ethnically Mixed Schools: An Intervention on Perspective Taking," *The Quarterly Journal of Economics*, 2021, *136* (4), 2147–2194.

Alesina, Alberto, Matteo F. Ferroni, and Stefanie Stantcheva, "Perceptions of Racial Gaps, their Causes, and Ways to Reduce Them," NBER Working Paper 29245, National Bureau of Economic Research September 2021. Revised July 2024.

Algan, Yann, Nicolò Dalvit, Quoc-Anh Do, Alexis Le Chapelain, and Yves Zenou, "Friendship Networks and Political Opinions," *American Economic Review*, 2026, *116* (6), 2202–2241.

Altonji, Joseph G, Peter Arcidiacono, and Arnaud Maurel, "The analysis of field choice in college and graduate school: Determinants and wage effects," in "Handbook of the Economics of Education," Vol. 5, Elsevier, 2016, pp. 305–396.

Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber, "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools," *Journal of Political Economy*, 2005, *113* (1), 151–184.

American National Election Studies, "ANES Time Series Cumulative Data File," Dataset and documentation 2026. February 5, 2026 version; cumulative file covering 1948–2024.

Ammermueller, Andreas and Jörn-Steffen Pischke, "Peer Effects in European Primary Schools: Evidence from the Progress in International Reading Literacy Study," *Journal of Labor Economics*, 2009, *27* (3), 315–348.

Andrews, Martyn J, Leonard Gill, Thorsten Schank, and Richard Upward, "High wage workers and low wage firms: Negative assortative matching or limited mobility bias?," *Journal of the Royal Statistical Society: Series A*, 2008, *171* (3), 673–697.

Andrews, Rodney J, Scott A Imberman, and Michael F Lovenheim, "Risky Business? The Effect of Majoring in Business on Earnings and Educational Attainment," Working Paper 23575, National Bureau of Economic Research 2017.

_ , _ , _ , and Kevin Stange, "The Returns to College Major Choice: Average and Distributional Effects, Career Trajectories, and Earnings Variability," *Review of Economics and Statistics*, 2024, pp. 1–45.

Angrist, Joshua D, "The perils of peer effects," *Labour Economics*, 2014, *30*, 98–108.

Angrist, Joshua D. and Jörn-Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press, 2009.

Apfeld, Brendan, Emanuel Coman, John Gerring, and Stephen Jessee, "Higher education and cultural liberalism: Regression discontinuity evidence from romania," *The Journal of Politics*, 2023, *85* (1), 34–48.

Arold, Benjamin W., "Evolution vs. Creationism in the Classroom: The Lasting Effects of Science Education," *The Quarterly Journal of Economics*, 2024, *139* (4), 2331–2375.

Ash, Elliott, Daniel L Chen, and Suresh Naidu, "Ideas Have Consequences: The Impact of Law and Economics on American Justice," *The Quarterly Journal of Economics*, 2026, *141* (1), 845–887.

Asker, Erdal, Eric J. Brunner, and Stephen L. Ross, "JUE Insight: The Impact of School Spending on Civic Engagement: Evidence from School Finance Reforms," *Journal of Urban Economics*, 2024, *143*, 103688.

Backström, Kim, Alexandru Cernat, Rasmus Sirén, and Peter Söderlund, "Measurement error when surveying issue positions: A MultiTrait MultiError approach," *Political Science Research and Methods*, 2026, *14* (2), 472–489.

Baum, Micah Y., Joaquín Endara, and Annaliese Paulson, "Do Faculty Affect Student Partisanship?," EdWorkingPaper 26-1538, Annenberg Institute at Brown University 2026.

Bauman, Yoram and Elaina Rose, "Selection or indoctrination: Why do economics students donate less than the rest?," *Journal of Economic Behavior & Organization*, 2011, *79* (3), 318–327.

Ben-Porath, Netanel, "Democratizing Education? Schooling as an Elite Strategy in Times of Reform," 2025. SSRN working paper 5471571.

Berkson, Joseph, "Are There Two Regressions?," *Journal of the American Statistical Association*, 1950, *45* (250), 164–180.

Billings, Stephen B., Eric Chyn, and Kareem Haggag, "The Long-Run Effects of School Racial Diversity on Political Identity," *American Economic Review: Insights*, 2021, *3* (3), 267–284.

Bleemer, Zachary and Aashish Mehta, "Will studying economics make you rich? A regression discontinuity analysis of the returns to college major," *American Economic Journal: Applied Economics*, 2022, *14* (2), 1–22.

Bond, Timothy N and Kevin Lang, "The sad truth about happiness scales," *Journal of Political Economy*, 2019, *127* (4), 1629–1640.

Borusyak, Kirill and Peter Hull, "Nonrandom exposure to exogenous shocks," *Econometrica*, 2023, *91* (6), 2155–2185.

_ , Xavier Jaravel, and Jann Spiess, "Revisiting event-study designs: robust and efficient estimation," *Review of Economic Studies*, 2024, *91* (6), 3253–3285.

Braghieri, Luca, "Political correctness, social image, and information transmission," *American Economic Review*, 2024, *114* (12), 3877–3904.

_ **and Sarah Eichmeyer**, "Learning from the Past: How History Education Shapes Support for Extreme Ideology," CEPR Discussion Paper 19575, Centre for Economic Policy Research 2024.

Brown, Jacob R, Enrico Cantoni, Sahil Chinoy, Martin Koenen, and Vincent Pons, "The Effect of Childhood Environment on Political Behavior: Evidence from Young U.S. Movers, 1992–2021," Working Paper 31759, National Bureau of Economic Research 2023.

Caeyers, Bet and Marcel Fafchamps, "Exclusion Bias and the Estimation of Peer Effects," *Journal of Human Resources*, 2026, *61* (6).

Cantoni, Davide, Yuyu Chen, David Y Yang, Noam Yuchtman, and Y Jane Zhang, "Curriculum and ideology," *Journal of political economy*, 2017, *125* (2), 338–392.

Carroll, Raymond J., David Ruppert, Leonard A. Stefanski, and Ciprian M. Crainiceanu, *Measurement Error in Nonlinear Models: A Modern Perspective*, 2nd ed., Chapman and Hall/CRC, 2006.

Cavaillé, Charlotte and John Marshall, "Education and Anti-Immigration Attitudes: Evidence from Compulsory Schooling Reforms across Western Europe," *American Political Science Review*, 2019, *113* (1), 254–263.

Chamberlain, Gary, "Asymptotic efficiency in estimation with conditional moment restrictions," *Journal of Econometrics*, 1987, *34* (3), 305–334.

Chen, Wei-Lin, Ming-Jen Lin, and Tzu-Ting Yang, "Curriculum and national identity: Evidence from the 1997 curriculum reform in Taiwan," *Journal of Development Economics*, 2023, *163*, 103078.

Chetty, Raj, John N. Friedman, and Jonah E. Rockoff, "Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates," *American Economic Review*, 2014, *104* (9), 2593–2632.

Cinelli, Carlos and Chad Hazlett, "Making sense of sensitivity: Extending omitted variable bias," *Journal of the Royal Statistical Society Series B: Statistical Methodology*, 2020, *82* (1), 39–67.

Cohodes, Sarah R. and James J. Feigenbaum, "Why Does Education Increase Voting? Evidence from Boston's Charter Schools," *The Review of Economics and Statistics*, 2025, pp. 1–46. Advance publication.

Converse, Philip E, "The nature of belief systems in mass publics," in David E Apter, ed., *Ideology and Discontent*, New York: Free Press, 1964, pp. 206–261.

Cooperative Election Study, "Cooperative Election Study Common Content Data, 2006–2024," Harvard Dataverse, wave-specific releases 2006–2024. Formerly the Cooperative Congressional Election Study.

Couch, Kenneth A and Dana W Placzek, "Earnings losses of displaced workers revisited," *American Economic Review*, 2010, *100* (1), 572–589.

Dale, Stacy Berg and Alan B Krueger, "Estimating the payoff to attending a more selective college: An application of selection on observables and unobservables," *The Quarterly Journal of Economics*, 2002, *117* (4), 1491–1527.

Davis, Steven J and Till Von Wachter, "Recessions and the costs of job loss," *Brookings Papers on Economic Activity*, 2011, *2011* (2), 1–72.

Dechezleprêtre, Antoine, Adrien Fabre, Tobias Kruse, Bluebery Planterose, Ana Sanchez Chico, and Stefanie Stantcheva, "Fighting Climate Change: International Attitudes toward Climate Policies," *American Economic Review*, April 2025, *115* (4), 1258–1300.

Dee, Thomas S., "Are there civic returns to education?," *Journal of Public Economics*, 2004, *88* (9-10), 1697–1720.

Dewey, John, *Democracy and Education: An Introduction to the Philosophy of Education*, New York: Macmillan, 1916.

Feldman, Stanley, "Measuring issue preferences: The problem of response instability," *Political Analysis*, 1989, *1* (1), 25–60.

Firoozi, Daniel, "Education and Partisanship," 2026. Working paper, version January 28, 2026.

Fischer, Mira, Björn Kauder, Niklas Potrafke, and Heinrich W Ursprung, "Support for free-market policies and reforms: Does the field of study influence students' political attitudes?," *European Journal of Political Economy*, 2017, *48*, 180–197.

Frank, Robert H, Thomas Gilovich, and Dennis T Regan, "Does studying economics inhibit cooperation?," *Journal of economic perspectives*, 1993, *7* (2), 159–171.

Gardner, John, Neil Thakral, Linh T. Tô, and Luther Yap, "Two-Stage Differences in Differences," 2026. Working paper, version June 2026; supersedes earlier versions by Gardner.

Gelbach, Jonah B, "When do covariates matter? And which ones, and how much?," *Journal of Labor Economics*, 2016, *34* (2), 509–543.

Gethin, Amory, Clara Martínez-Toledano, and Thomas Piketty, "Brahmin left versus merchant right: Changing political cleavages in 21 western democracies, 1948–2020," *The Quarterly Journal of Economics*, 2022, *137* (1), 1–48.

Girardi, Daniele, Sai Madhurika Mamunuru, Simon D Halliday, and Samuel Bowles, "Does studying economics make you selfish?," *Southern Economic Journal*, 2024, *90* (3), 792–814.

Glaeser, Edward L, Giacomo AM Ponzetto, and Andrei Shleifer, "Why does democracy need education?," *Journal of economic growth*, 2007, *12* (2), 77–99.

Goldsmith-Pinkham, Paul, Peter Hull, and Michal Kolesár, "Contamination Bias in Linear Regressions," *American Economic Review*, 2024, *114* (12), 4015–4051.

Goldstein, Yoav, "Heuristic Self-Evaluation in High-Stakes Decisions: Fields of Study and Long-Term Earnings," SSRN Working Paper 4584223, Tel Aviv University 2026. Originally posted in 2023; version revised April 19, 2026.

Gradstein, Mark and Moshe Justman, "Education, social cohesion, and economic growth," *American Economic Review*, 2002, *92* (4), 1192–1204.

Grossmann, Matt and David A. Hopkins, *Polarized by Degrees: How the Diploma Divide and the Culture War Transformed American Politics*, Cambridge, UK: Cambridge University Press, 2024.

Guryan, Jonathan, Kory Kroft, and Matthew J Notowidigdo, "Peer effects in the workplace: Evidence from random groupings in professional golf tournaments," *American Economic Journal: Applied Economics*, 2009, *1* (4), 34–68.

Hammock, Michael R, P Wesley Routon, and Jay K Walker, "The opinions of economics majors before and after learning economics," *The Journal of Economic Education*, 2016, *47* (1), 76–83.

Hastings, Justine S, Christopher A Neilson, and Seth D Zimmerman, "Are Some Degrees Worth More than Others? Evidence from College Admission Cutoffs in Chile," Working Paper 19241, National Bureau of Economic Research 2013.

Heinesen, Eskil, Christian Hvid, Lars Johannessen Kirkebøen, Edwin Leuven, and Magne Mogstad, "Instrumental Variables with Unordered Treatments: Theory and Evidence from Returns to Fields of Study," *Journal of Labor Economics*, 2026, *44* (3), 823–854.

Heise, David R, "Separating reliability and stability in test-retest correlation," *American Sociological Review*, 1969, *34* (1), 93–101.

Hooghe, Liesbet, Gary Marks, and Jonne Kamphorst, "Field of Education and Political Behavior: Predicting GAL/TAN Voting," *American Political Science Review*, 2025, *119* (2), 794–811.

Hyslop, Dean R. and Guido W. Imbens, "Bias from Classical and Other Forms of Measurement Error," *Journal of Business & Economic Statistics*, 2001, *19* (4), 475–481.

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan, "Earnings losses of displaced workers," *The American Economic Review*, 1993, *83* (4), 685–709.

Kane, Thomas J. and Douglas O. Staiger, "Estimating Teacher Impacts on Student Achievement: An Experimental Evaluation," Technical Report 14607, National Bureau of Economic Research 2008.

Kant, Immanuel, *On Education*, Dover Publications, 2003. Translated by Annette Churton.

Kaplan, Ethan, Jörg L Spenkuch, and Cody Tuttle, "From the Classroom to the Ballot Box: Turnout and Partisan Consequences of Education," Working Paper 34355, National Bureau of Economic Research 2025.

Kirkeboen, Lars J, Edwin Leuven, and Magne Mogstad, "Field of study, earnings, and self-selection," *The Quarterly Journal of Economics*, 2016, *131* (3), 1057–1111.

Kline, Patrick, Raffaele Saggio, and Mikkel Sølvsten, "Leave-out estimation of variance components," *Econometrica*, 2020, *88* (5), 1859–1898.

Knight, Kathleen, "Measurement of Liberal/Conservative Identification," ANES Pilot Study Report nes002267, American National Election Studies 1987.

Kuziemko, Ilyana, Nicolas Longuet-Marx, and Suresh Naidu, ""Compensate the Losers?" Economic Policy and the Origins of U.S. Partisan Realignment," *The Quarterly Journal of Economics*, 2026, *141* (3), 2087–2145.

Lee, David S, "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *The Review of Economic Studies*, 2009, *76* (3), 1071–1102.

Manski, Charles F, "Identification of endogenous social effects: The reflection problem," *The review of economic studies*, 1993, *60* (3), 531–542.

Marshall, John, "Education and Voting Conservative: Evidence from a Major Schooling Reform in Great Britain," *The Journal of Politics*, 2016, *78* (2), 382–395.

_ , "The Anti-Democrat Diploma: How High School Education Decreases Support for the Democratic Party," *American Journal of Political Science*, 2019, *63* (1), 67–83.

Meriläinen, Jaakko and Matti Mitrunen, "Communism in the Classroom: Long-Run Effects of an Experiment," 2025. Working paper.

Milligan, Kevin, Enrico Moretti, and Philip Oreopoulos, "Does education improve citizenship? Evidence from the United States and the United Kingdom," *Journal of Public Economics*, 2004, *88* (9-10), 1667–1695.

Mountjoy, Jack and Brent R Hickman, "The Returns to College(s): Relative Value-Added and Match Effects in Higher Education," Working Paper 29276, National Bureau of Economic Research 2021.

Mumma, Kirsten Slungaard, "High School Effects on Civic Engagement," EdWorkingPaper 25-1260, Annenberg Institute at Brown University 2026. Forthcoming, American Economic Journal: Economic Policy.

Oreopoulos, Philip and Kjell G Salvanes, "Priceless: The nonpecuniary benefits of schooling," *Journal of Economic perspectives*, 2011, *25* (1), 159–184.

Oster, Emily, "Unobservable selection and coefficient stability: Theory and evidence," *Journal of Business & Economic Statistics*, 2019, *37* (2), 187–204.

Plato, *Republic*, 2nd ed., Hackett Publishing Company, 1992. Translation revised by C. D. C. Reeve.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe, "What's trending in difference-in-differences? A synthesis of the recent econometrics literature," *Journal of Econometrics*, 2023, *235* (2), 2218–2244.

Rubinstein, Ariel, "A Sceptic's Comment on the Study of Economics," *The Economic Journal*, 2006, *116* (510), C1–C9.

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

Sojourner, Aaron, "Identification of Peer Effects with Missing Peer Data: Evidence from Project STAR," *Economic Journal*, 2013, *123* (569), 574–605.

Spiess, Jann, "Optimal Estimation when Researcher and Social Preferences are Misaligned," *Econometrica*, 2025, *93* (5), 1779–1810.

Stantcheva, Stefanie, "Understanding Tax Policy: How Do People Reason?," *The Quarterly Journal of Economics*, November 2021, *136* (4), 2309–2369.

Stevenson, Megan, "Breaking bad: Mechanisms of social influence and the path to criminality in juvenile jails," *Review of Economics and Statistics*, 2017, *99* (5), 824–838.

Strother, Logan, Spencer Piston, Ezra Golberstein, Sarah E Gollust, and Daniel Eisenberg, "College roommates have a modest but significant influence on each other's political ideology," *Proceedings of the National Academy of Sciences*, 2021, *118* (2), e2015514117.

Tourangeau, Roger, "Survey reliability: Models, methods, and findings," *Journal of Survey Statistics and Methodology*, 2021, *9* (5), 961–991.

Voigtländer, Nico and Hans-Joachim Voth, "Nazi indoctrination and anti-Semitic beliefs in Germany," *Proceedings of the National Academy of Sciences*, 2015, *112* (26), 7931–7936.

Wiley, David E and James A Wiley, "The estimation of measurement error in panel data," *American Sociological Review*, 1970, *35* (1), 112–117.

Wooldridge, Jeffrey M., "Inverse probability weighted estimation for general missing data problems," *Journal of Econometrics*, 2007, *141* (2), 1281–1301.

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 317,161 students from 451 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 Home-area Covariates

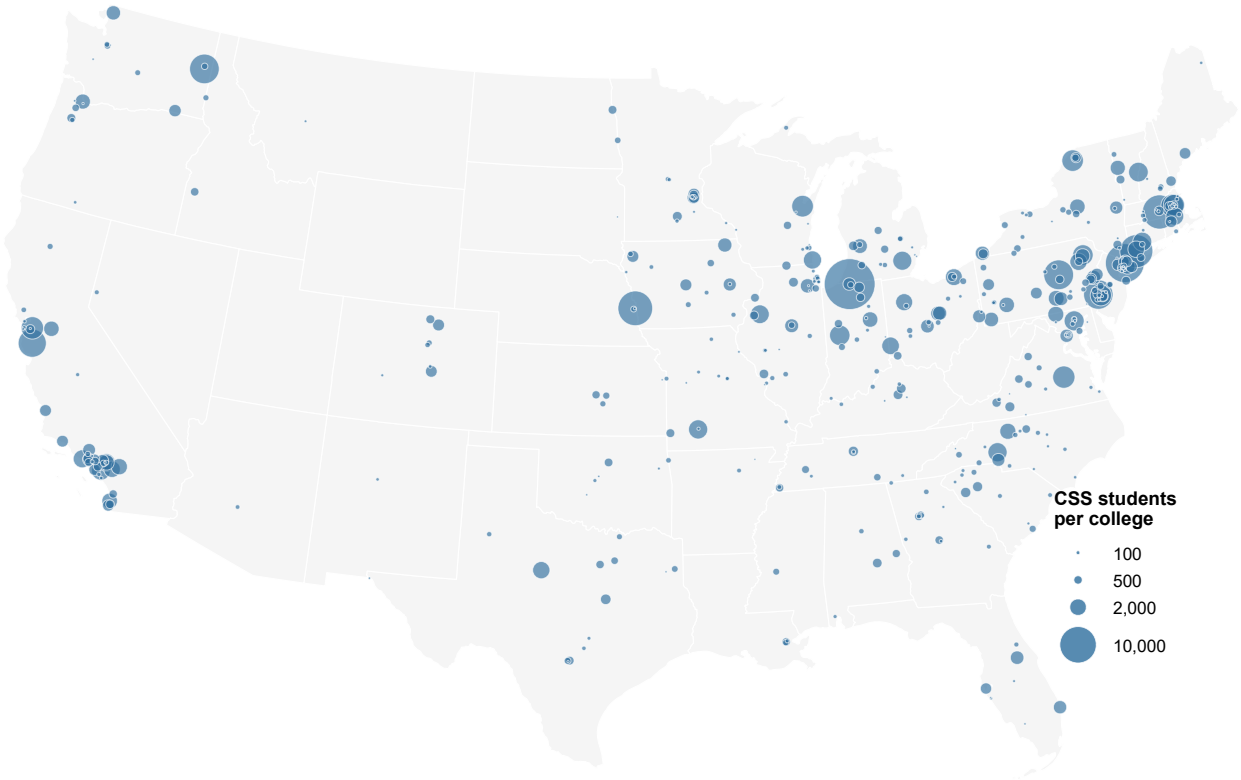
We match students to their home areas using the ZIP code reported in the freshman survey, available for about 90 percent of the analysis sample (the 1994 and 2014 entry cohorts did not retain ZIP codes). ZIP-level income, education, poverty, home values, racial composition, and homeownership come from the 2000 Census (SF3) and the 2009–2013 ACS at the ZIP Code Tabulation Area level; cohorts entering through 2005 receive the 2000 measurement and later cohorts the ACS measurement. Counties are assigned by each ZIP code's primary county. The Democratic two-party presidential share uses the most recent election preceding entry; the county unemployment rate (BLS) and five-year house-price growth (FHFA ZIP-level index) are measured at the entry year; religious adherence uses the nearest Religious Congregations and Membership Study decade. Homezip Students variables average each baseline trait over

all freshman-survey respondents from the same ZIP code in 1990–2015, excluding the student, and require at least five other respondents.

A.3 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.

Figure A.1: Geographic Coverage of the CSS Survey



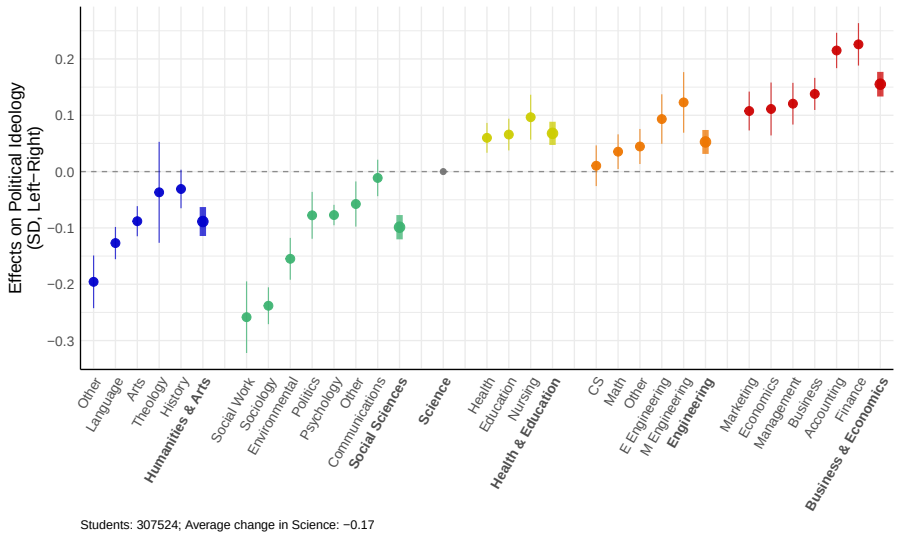
Note: Each point is a four-year college in the linked CSS analysis sample, and the area of each point is proportional to the number of CSS students from that college. The sample spans 451 colleges across 45 states and the District of Columbia, covering 317,161 students. The map shows the contiguous U.S., and the sample includes no colleges in Alaska or Hawaii.

Appendix B Correcting for Contamination Bias

As shown by Goldsmith-Pinkham et al. (2024), when a regression includes multiple treatments, the coefficients may be affected by contamination bias arising from treatment-effect heterogeneity. Formally, as discussed in detail by Mountjoy and Hickman (2021), under conditional independence alone and without restrictions on treatment-effect heterogeneity, comparisons between two majors generally identify differences in average treatment effects on the treated *for different populations*. Interpreting these comparisons as average treatment effects requires additional restrictions on heterogeneity. In particular, selection on ideological effects must be symmetric across majors, both in sorting into majors within covariate cells and in sorting into the covariate cells that provide identifying support. Under these additional symmetry restrictions, OLS coefficients can be interpreted as the average treatment effect of each major relative to the omitted category for the relevant population.

We also report results that do not require these additional assumptions, following Goldsmith-Pinkham et al. (2024): estimating separate models that compare each treated major k with 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 indicates that contamination from the inclusion of multiple majors is unlikely to be quantitatively important for our main conclusions and is also consistent with our setting satisfying the additional restrictions on treatment-effect 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. Estimation follows a correction for contamination bias (Goldsmith-Pinkham et al., 2024). 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

C.1 Specification Choices

Alternative ideological outcomes. Appendix Figure C.2 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.[38] Figure C.3 reports the estimated coefficients from specifications that sequentially add the controls listed in Table H.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). Figure C.4 reports the results, which closely mirror our baseline estimates, reinforcing their validity within a slightly different identification framework.

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 information on second majors biases our results, we re-estimate Equation 1 with and without flexible controls for students' second majors. Table C.1 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.[39]

[38] 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.

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

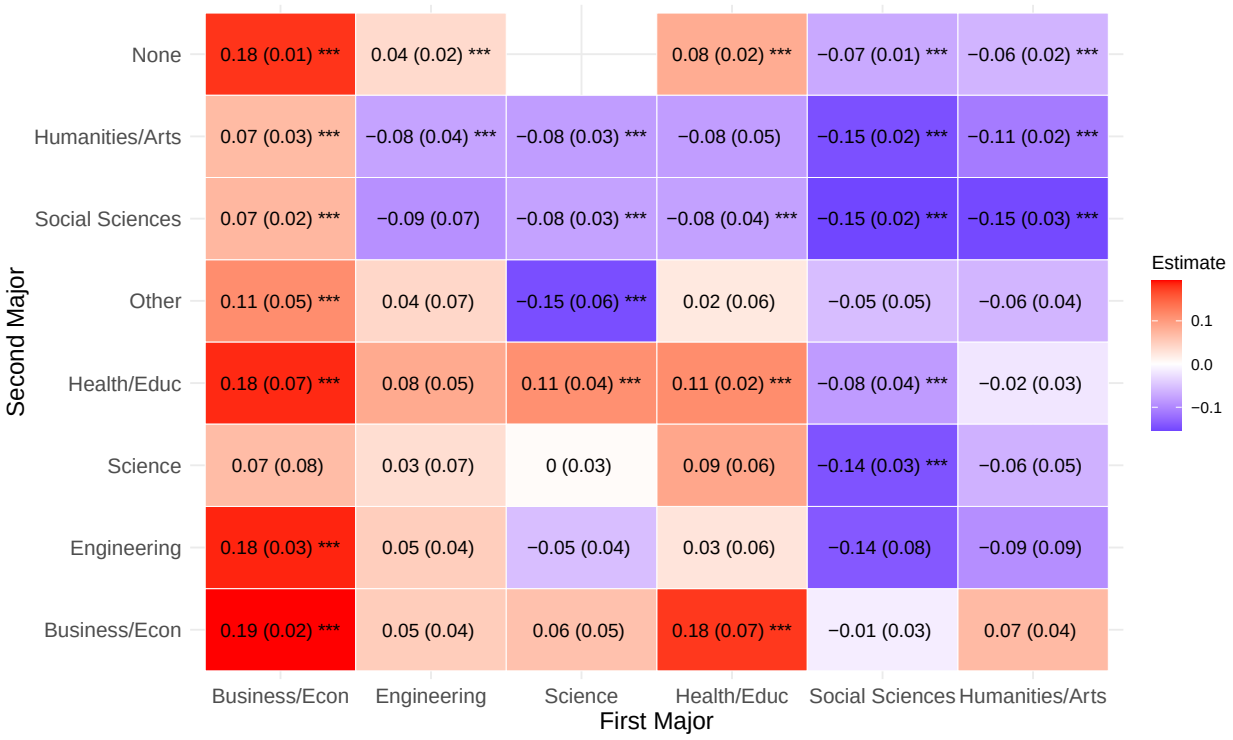
Table C.1: Robustness to Controlling for Double-Major Status

	Ideology		Index	
Control		+		+
Business/	0.17***	0.17***	0.15***	0.15***
Economics	(0.01)	(0.01)	(0.01)	(0.01)
Engineering/	0.04***	0.03**	0.04**	0.04**
CS/Math	(0.01)	(0.01)	(0.02)	(0.02)
Health/	0.07***	0.07***	0.08***	0.08***
Education	(0.02)	(0.02)	(0.02)	(0.02)
Humanities/	-0.06***	-0.06***	-0.08***	-0.07***
Arts	(0.02)	(0.02)	(0.01)	(0.01)
Social	-0.08***	-0.07***	-0.10***	-0.09***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)
N	117764	117764	114674	114674

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 are self-reported political ideology, measured in standard deviations on a Left-Right scale, and the policy-position index, the standardized prediction of self-reported ideology from students' responses to all policy questions available in their survey year, reported in its own standardized units. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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.1 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.

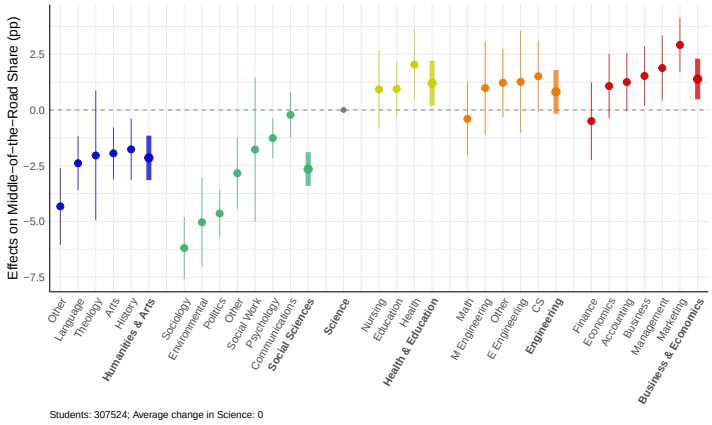
Figure C.1: Effects of College Majors and Double Majors on Students' Ideology



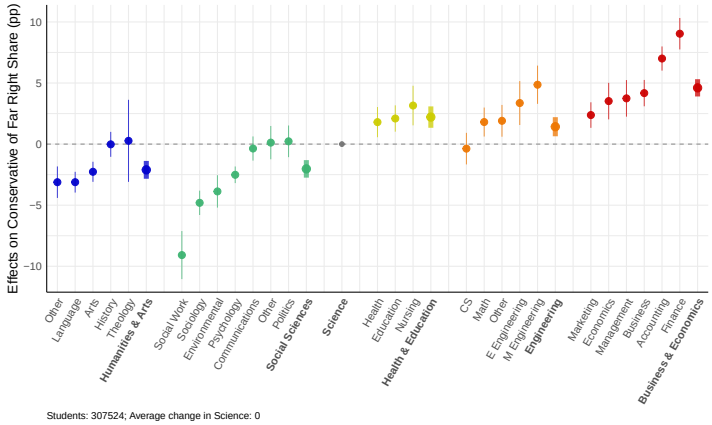
Note: This figure plots the estimates for δ_m from equation 1 and their clustered standard errors from a specification with interaction between first and second majors. The estimated coefficients represent the effects of studying each combination of majors relative to the natural sciences with no second 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.

Figure C.2: Effects of College Majors on Political Ideology, Indicator Outcomes

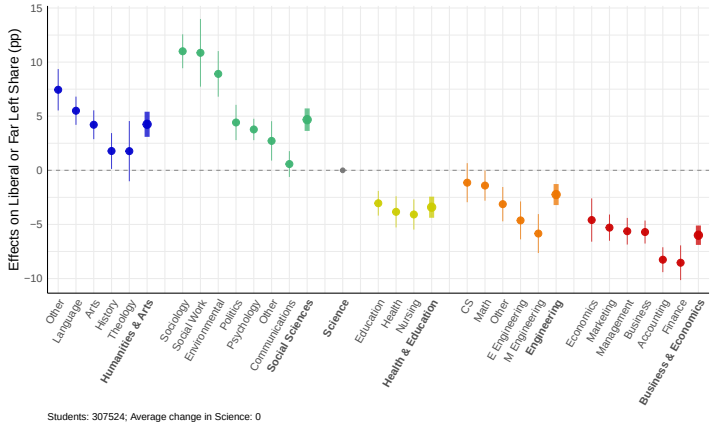
(A) Share Middle of the Road



(B) Share Far Right or Conservative

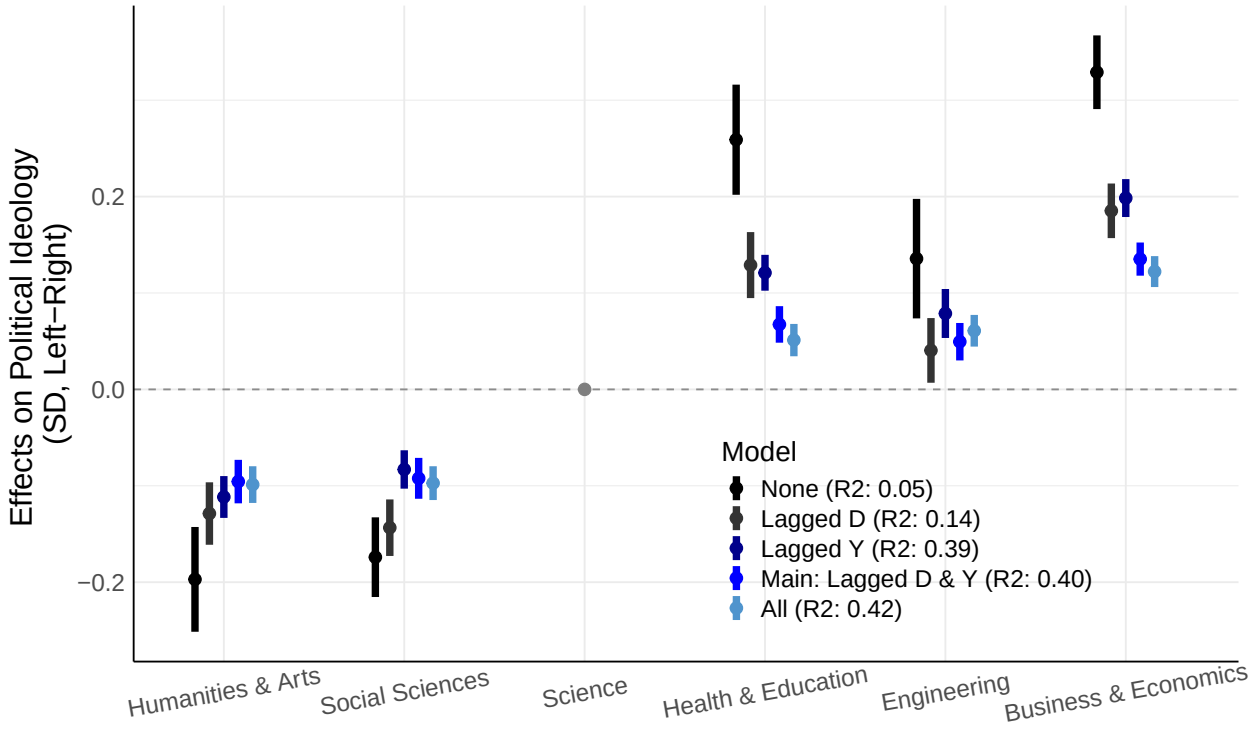


(C) Share Far Left or Liberal



Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals. Bold lines mark coefficients from a specification that groups majors into five broad categories, with natural sciences as the omitted reference group. Thinner lines show results from a more detailed specification with 27 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 H.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.3: Robustness to Including Additional Control Variables



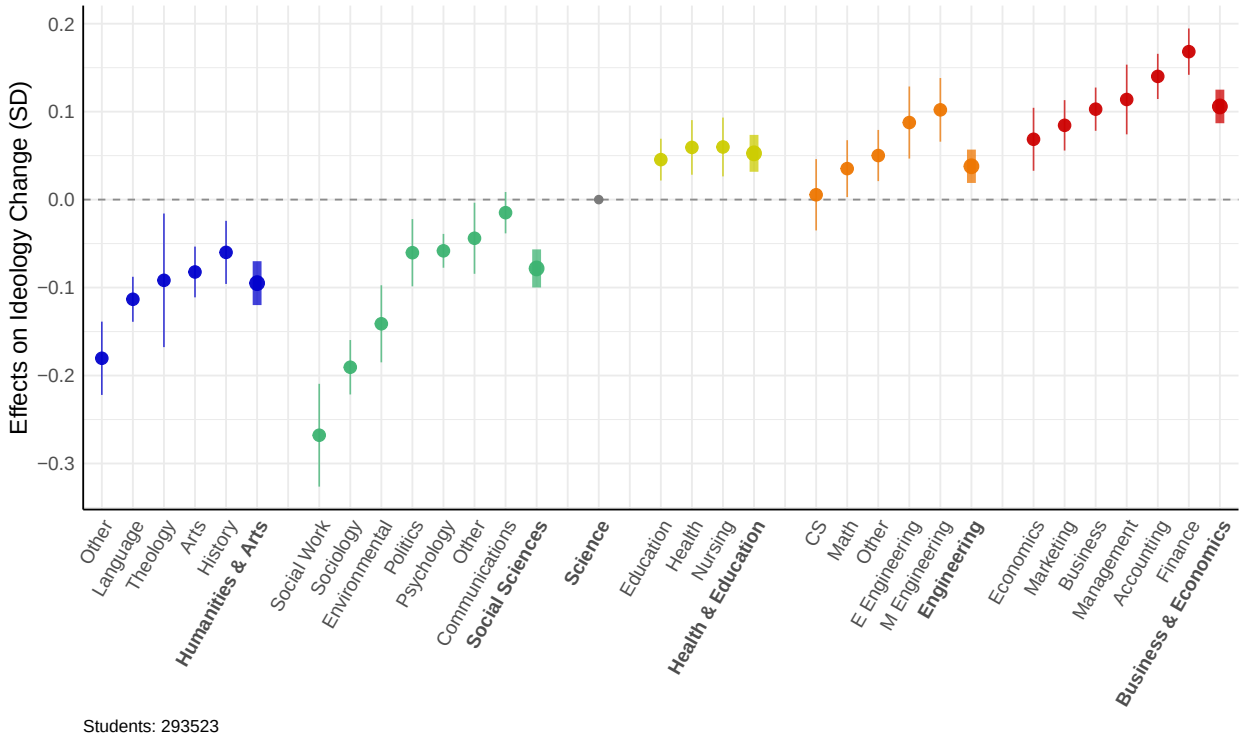
Note: This figure plots estimates of δ_m from equation 1 as control sets are added sequentially. Coefficients represent the effects of each major group relative to the natural sciences. The outcome is graduation political ideology, measured in SD. The specifications correspond to: no controls (None); 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 H.4 lists all control sets. The sample includes students with non-missing major and graduation ideology. Standard errors are clustered at the college level.

C.2 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.

Table C.2 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

Figure C.4: 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.

example, the raw response gap between students intending engineering and math and those intending natural sciences is 4.2 pp (column 1), but in the controlled specification (column 4) it is only 0.8 pp. The remaining controlled gaps are also negative and not larger than 1 pp in absolute value. Such small differences imply that under a monotonicity assumption, the sample selection bias is likely very small in our setting.[40]

[40]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 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.

Inverse-probability-of-attrition weighting. As a further check on differential attrition, we reweight the analysis sample by the inverse probability of responding to the graduation survey. We estimate this probability with a probit regression of graduation-survey participation on a rich set of baseline characteristics (life goals, intended detailed major, career, policy views, and demographics), together with college-by-enrollment-year and high-school-GPA fixed effects, and form stabilized weights trimmed at the 1st and 99th percentiles. The weights make the sample representative, on these characteristics, of the baseline-survey population of the college-by-cohort cells covered by the graduation survey.[41] Table C.3 shows that reweighting leaves the ranking and significance of the field effects unchanged, with somewhat larger estimates for economics or business and for engineering and, as expected, less precision.

[41]This correction is valid under a missing-at-random assumption (Wooldridge, 2007). The assumption is supported by the richness of the baseline controls, which include the strongest observed predictors of both response and ideology, and by the evidence above that response is only weakly related to baseline ideology within intended majors.

Table C.2: Assessing Differential Attrition

	Appear in Graduation Survey				
	(1)	(2)	(3)	(4)	(5)
Business & Economics	-0.017**	-0.018***	-0.008***	-0.005***	-0.005***
	(0.008)	(0.002)	(0.002)	(0.002)	(0.002)
Engineering & Math	-0.042***	-0.014***	-0.011***	-0.008***	-0.008***
	(0.013)	(0.002)	(0.002)	(0.002)	(0.002)
Health & Education	-0.021**	-0.006***	-0.003	-0.007***	-0.006***
	(0.009)	(0.002)	(0.002)	(0.002)	(0.002)
Humanities & Arts	-0.006	-0.016***	-0.008***	-0.007***	-0.007***
	(0.009)	(0.002)	(0.002)	(0.002)	(0.002)
Social Sciences	-0.014*	-0.012***	-0.003**	-0.002*	-0.002
	(0.008)	(0.001)	(0.001)	(0.001)	(0.001)
Ideology					0.006***
					(0.001)
Ideology × Business & Economics					-0.002**
					(0.001)
Ideology × Engineering & Math					-0.001
					(0.001)
Ideology × Health & Education					-0.002**
					(0.001)
Ideology × Humanities & Arts					-0.000
					(0.001)
Ideology × Social Sciences					-0.002**
					(0.001)
N	2,113,783	2,113,783	2,113,783	2,113,783	2,113,783
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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C.3: Main Effects under Inverse-Probability-of-Attrition Weighting

	Political Ideology (SD)		Share Liberal/Far Left (pp)	
	(1) Unweighted	(2) IPW	(3) Unweighted	(4) IPW
Business & Economics	0.135***	0.149***	-6.00***	-6.30***
	(0.009)	(0.016)	(0.46)	(0.74)
Engineering	0.049***	0.070***	-2.24***	-2.84***
	(0.010)	(0.017)	(0.50)	(0.86)
Health & Education	0.067***	0.072***	-3.41***	-3.42***
	(0.010)	(0.018)	(0.50)	(0.77)
Humanities & Arts	-0.096***	-0.088***	4.25***	4.12***
	(0.012)	(0.016)	(0.59)	(0.71)
Social Sciences	-0.092***	-0.089***	4.68***	4.96***
	(0.011)	(0.015)	(0.53)	(0.72)
Attrition weights	No	Yes	No	Yes
Observations	307,524	307,524	307,524	307,524

Notes: This table re-estimates Equation 1 with inverse-probability-of-attrition weights. Columns (1) and (3) are unweighted; columns (2) and (4) weight each observation by the inverse probability of responding to the graduation survey. All four columns use the same rows, the estimation sample of the main specification in Figure 2, so the paired columns differ only by the weights. The weights are stabilized inverse response probabilities trimmed at the 1st and 99th percentiles, from a probit model of graduation-survey participation on baseline life goals, intended detailed major, career, policy positions and demographics, with college-by-entry-cohort and high-school-GPA fixed effects. That model is estimated on the 2,122,531 baseline-survey respondents in the 3,568 college-by-entry-cohort cells from which at least one student appears in the matched sample, 317,161 of whom respond (14.9%). The weights therefore represent the baseline-survey population of those cohorts rather than the full baseline survey. Estimates are relative to natural sciences, the omitted major group. Share Liberal/Far Left is an indicator for identifying as liberal or far left. Standard errors clustered at the college level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C.3 Heterogeneity of the Effects

The following tables report heterogeneity estimates by estimating Equation 1 separately for several subsamples. Table C.4 reports results by students' characteristics. Table C.5 reports results by college characteristics. Table C.6 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.4: 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.5: 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.6: Effects of College Majors on Ideology, By Enrollment Year

	Before 2000	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). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

C.4 Instrumental Variable Estimates

Intended majors. One concern for our main results is that some experiences during the freshman and sophomore years may influence both major choice and graduation ideology.[42] 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 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:

$$D_{im} = \sum_{m'} \pi_{m'} D^0_{im'} + Q_i'\gamma^0 + \theta^0_t + \lambda^0_c + u^0_i, \tag{A1}$$

$$p_i = \sum_{m} \tau_m D_{im} + Q_i'\gamma^1 + \theta^1_t + \lambda^1_c + u^1_i, \tag{A2}$$

where $D^0_{im'}$ denotes indicators for intended majors reported at college entry. These indicators jointly serve as instruments for the realized-major indicators D_{im}, and Q_i includes our main set of controls X_i, excluding intended major.[43] Figure C.5 shows that the IV estimates are broadly similar to the baseline estimates, with somewhat larger magnitudes. This pattern is consistent with attenuation in the OLS estimates due to measurement error in the reported major. The IV estimates rely on four assumptions. First, intended majors are relevant instruments for realized majors. Figure H.1 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

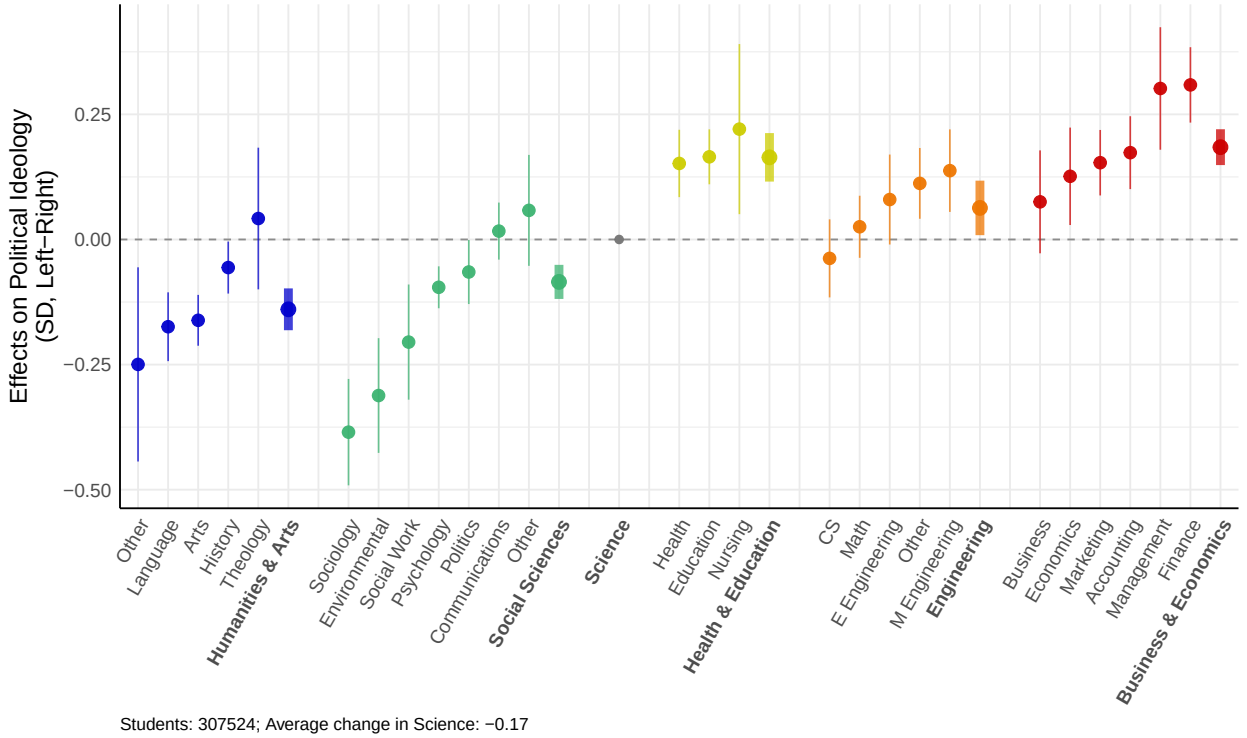
[42] Note that our bounding exercises also address this concern, as they assess potential bias from all unobserved covariates, including early college experiences.

[43] We construct the instruments using unaggregated intended-major categories, including separate categories for students who reported being undecided and for those with no intended major recorded, to exploit all available variation in intended major.

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, $D_{im}(D_i^0 = m) \geq D_{im}(D_i^0 \neq m)$. This assumption is plausible in our setting, as it is unlikely that intending a major would make a student less likely to pursue it.[44] 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: Effects of College Majors on Political Ideology, Using Intended Majors as IV



Note: This figure plots the IV estimates for τ_m from equation A1 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 major and graduation ideology. Standard errors are clustered at the college level.

[44] 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.

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. Second, business and economics programs are often competitive and subject to capacity constraints (Bleemer and Mehta, 2022), unlike many fields in the social sciences and humanities. This generates meaningful variation in whether students ultimately pursue their initially intended major.[45]

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

$$\begin{aligned} D_{i,BE} &= \pi Z_{icmt} + X_i'\gamma + u_i, \\ p_i &= \beta D_{i,BE} + X_i'\theta + v_i \end{aligned} \tag{A3}$$

where X_i includes the main set of controls: predictors of ideology and predictors of major choice. These controls improve precision and reduce concerns about violations of the identifying assumption, and we 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 C.7 reports falsification tests using baseline ideology as the outcome. We report results for both the full set of students

[45] Throughout, the intended major that defines a program is measured at the detailed level of our classification, for example, finance, accounting, or economics, rather than the broad business and economics group.

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 the independence assumption. The larger-sample test draws on over 226,000 students, giving it the power to detect even small violations.

A related concern is that the instrument may affect not only realized major but also which students appear in the graduation sample. If this selection were related to potential ideology, it would surface as an imbalance in baseline characteristics. The same falsification tests speak to this: the instrument is uncorrelated with baseline ideology in both the full baseline sample and the graduation subsample, making ideology-related selection unlikely.

Second, relevance: Z_{icmt} should shift the probability of pursuing business or economics. The first row of Panel B in Table C.7 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. This condition could fail if higher-persistence programs also differ in institutional culture, peer composition, or other features that directly shape ideology. Fourth, monotonicity: higher persistence should not make any student less likely to remain in their intended major. This condition could fail under capacity constraints, where a fixed number of seats implies that a peer who remains in business or economics may take a slot that would otherwise have gone to the student. To address these concerns, we also report specifications that measure persistence using adjacent cohorts rather than the student's own cohort. This reduces the link between the instrument and the student's contemporaneous program environment, including direct competition for seats within the same cohort.

Panel B of Table C.7 reports the reduced-form and IV estimates on graduation ideology. 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.

Table C.7: IV Estimates Based on Cohort-Specific Persistence in Intended Majors

Panel A. Falsification Tests: Baseline Ideology

	(1)	(2)	(3)
Full Baseline Survey	0.005	-0.009	-0.005
	(0.017)	(0.020)	(0.017)
N	220,538	220,538	220,538
Students Included in the Graduation Survey	0.063***	0.013	0.009
	(0.024)	(0.027)	(0.024)
N	47,461	47,461	47,461
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***	0.124***	0.121***	0.114***	0.093***
	(0.019)	(0.025)	(0.025)	(0.025)	(0.017)
Reduced Form: Graduation Ideology	0.106***	0.055**	0.052**	0.038**	0.030**
	(0.027)	(0.026)	(0.024)	(0.016)	(0.014)
2SLS: Graduation Ideology	0.611***	0.444*	0.433*	0.330**	0.317*
	(0.165)	(0.227)	(0.222)	(0.164)	(0.174)
N	49,176	49,176	49,176	49,176	49,176
First-stage F	84.4	24.2	23.2	20.7	28.7
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 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 at the finest survey level, finer than the detailed grouping used to construct the instrument. "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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To interpret the magnitude of the IV estimate, it is useful to consider the counterfactual major choices of compliers. Panel A of Table H.6 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.

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 limited heterogeneity of the main effect documented above makes the second explanation less likely, suggesting that the first or third is more likely. Consistent with this, Column (1) of Panel B in Table H.6 reports the instrument's effect on predicted graduation ideology, constructed from students' realized majors and the baseline major-effect estimates; the predicted reduced-form effect is 0.022 SD, smaller than the actual reduced-form estimate of 0.032, confirming that the IV estimate exceeds what applying the OLS estimates to the instrument-induced major changes would imply.[46]

A further possibility is a violation of the exclusion restriction. For example, a higher-persistence environment may coincide with an expanded supply of business and economics offerings, which shifts students' coursework beyond their realized major and could affect ideology through that channel.

Table H.7 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 significant ($p = 0.088$).

[46] Columns (2)–(4) of Panel B in Table H.6 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.

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. It is mechanically bounded by 1, but in our setting it is substantially lower. To see why, consider the ideal control: each student's potential graduation ideology in the omitted group. A regression of observed graduation ideology on this control and the major indicators attains the highest R^2 achievable by any control strategy, because the only variation it cannot explain is variation that no pre-college characteristic could explain. Three sources of such irreducible variation remain: heterogeneous major effects, the discreteness of the five-point response scale, and instability in the response itself.

First, treatment effects may be heterogeneous: a field may shift students who enter with different baseline views by different amounts, and the same major may have different effects across colleges that teach it with different content and faculty. The major indicators in our main specification capture only the average effect of each field, so heterogeneous effects leave

variance that no set of controls can explain. To obtain a lower bound on this variance, we allow effects to vary along one dimension, across programs: in our main specification, we replace the major indicators with an indicator for every college-by-major cell with at least 10 students (about 4,100 programs). Because each program effect is estimated from a limited number of students, the estimated effects vary across programs more than the true effects do, and part of the additional variance they explain is this sampling noise rather than true heterogeneity. We subtract the implied noise component, computed under the simplifying assumption that the residual variance is the same across programs (as in Andrews et al., 2008); the correction is about a tenth of the estimate, so allowing the variance to differ across programs (Kline et al., 2020) would matter little. The resulting contribution of program heterogeneity is about 0.07 of the outcome variance. This is a lower bound on the variance from heterogeneous effects, since effects may also vary within programs and across cohorts; a larger deduction would only lower $R^2_{\max}$.

Second, discretization. The survey records ideology in five categories, so the observed outcome is a rounded report of an underlying continuous attitude, and the within-category variation that rounding discards cannot be explained by any control. To quantify it, we simulate a continuous attitude with the same mean and variance as the observed measure (a scaled beta distribution on the one-to-five interval), round each draw to the nearest category, and ask how much of the continuous attitude's variance the five categories can explain. The unexplained share is about 0.10. As we discuss next, this rounding noise is highly correlated with instability in repeated responses rather than additional to it, so we do not deduct it separately; it serves instead as a lower bound for the joint response-noise term measured below.

Third, response instability. People give strikingly unstable answers to political survey questions: the same respondent, asked the same item again after a short interval, frequently chooses a different category, a pattern documented since Converse (1964) and studied as response instability ever since (Feldman, 1989; Tourangeau, 2021). Re-interview designs with gaps short enough to rule out genuine attitude change attribute roughly 20 to 30 percent of the variance of a single political response to such noise: about 0.22 for five-point positions on concrete policy proposals re-asked ten days apart (Backström et al., 2026); 0.19 to 0.28 for five-point agreement items on abortion and welfare re-asked a few weeks apart, among respondents with crystallized attitudes (Tourangeau, 2021); and 0.23 for the item closest to ours, a seven-point liberal-conservative self-placement re-asked about a month apart in an ANES pilot study (Knight, 1987).

The flip side of this correlation is that the two sources can be measured jointly: a student whose latent view falls between two categories flips between adjacent answers across repeated

responses, while a student well inside a category answers stably, so the test-retest behavior of a single question captures rounding and instability together. We measure it in our own data, using small samples of students who completed the senior survey before graduating, one to three years after entry. For them we observe the same student answering the ideology question twice, at gaps of one, two, and three years, with correlations of 0.662, 0.620, and 0.567 (3,796, 4,979, and 8,089 students). If responses were exact reports of a stable attitude, these correlations would equal one. They fall short for two reasons: genuine attitude change, which grows with the gap, and response noise including rounding, which is present at any gap. The gentle decline across gaps indicates that most of the shortfall is noise rather than change. Formally, under a quasi-simplex structure (Heise, 1969; Wiley and Wiley, 1970) the correlation at gap k equals ρb^k; extrapolating to a zero gap gives a single-response reliability of $\rho = 0.718$ (annual stability $b = 0.926$, with ratio-based estimates of 0.706 and 0.724) and a noise share of $1 - \rho = 0.282$. This is an upper bound on pure response noise, since attitudes change faster in the early college years and a stationary model attributes part of that early change to noise; it sits slightly above the short-gap benchmarks, as expected for young respondents with less crystallized attitudes. Consistent with the overlap, the discretization floor of 0.10 lies well below all of these totals.

Combining the sources requires a value for the response-noise share. Our own estimate of 0.282 is an upper bound, while the ANES self-placement benchmark of 0.23, the most comparable external estimate, is measured over a gap of only about a month, short enough that little genuine attitude change can enter. We adopt the midpoint, 0.256. Together with the heterogeneity deduction of 0.07, this gives a total deduction of about one-third and a ceiling of $R^2_{\max} \approx 2/3$, the value used in Panel B of Figure 2. Taking a ceiling above our own estimate implies a larger adjustment in the Oster formula, so this choice works against us.

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.[47] As in the previous subsection, we focus on the business or economics coefficient estimated on the full sample relative to the humanities.

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 0.21. 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 0.06, and the adjusted estimate remains about 0.18.

Table D.1: Robustness to Unobserved Selection

Benchmark	k_D	k_Y	Bias	Adjusted Estimate
Intended Econ/Business	1	1	0.02	0.23
Baseline Ideology	1	1	0.03	0.21
Intended Econ/Business	2	2	0.04	0.21
Baseline Ideology	2	2	0.06	0.18

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 economics or business relative to the humanities, estimated on the full sample with the humanities as the omitted group and the other major groups as controls; this effect 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).

[47] 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 Ideological Effects

Given that college majors influence students' 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:

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

This equation is identical to equation 1, but adds students' graduation policy positions as additional covariates. Comparing the estimated major effects (δ_m^*) from this specification to the baseline estimates (δ_m) reveals the extent to which policy position effects explain the overall ideological effects. Because these positions are measured at graduation, they are mediators rather than controls, and we do not interpret the decomposition as a causal mediation analysis, which would require that potential ideology be independent of potential policy positions conditional on covariates. Instead, following the use of such decompositions in survey work on policy positions (e.g., Stantcheva, 2021), we read it as a descriptive account of how much of each major's ideological effect is attributable to changes in policy positions.

We use the full sample from our main analysis and incorporate all policy position questions included in the survey.[48] 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, about 62 percent of the effect of studying social sciences, and about 66 percent of the effect of studying humanities. 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,

[48] Since questions vary between years, we add a dummy for missing responses or missing questions.

respectively, whereas economic policy positions explain 1 and 4 percent. Figure E.1 presents these results graphically.

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	34.01%	8.37%	5.32%	52.3%
Engineering & Math	Natural Sciences	0.077	0.032	34.42%	22.1%	2.15%	41.33%
Health & Education	Natural Sciences	0.066	0.034	13.85%	15.76%	19.09%	51.3%
Natural Sciences	Social Sciences	0.107	0.040	1.28%	58.81%	1.97%	37.94%
Natural Sciences	Humanities & Arts	0.100	0.033	4.03%	49.47%	12.89%	33.62%

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.

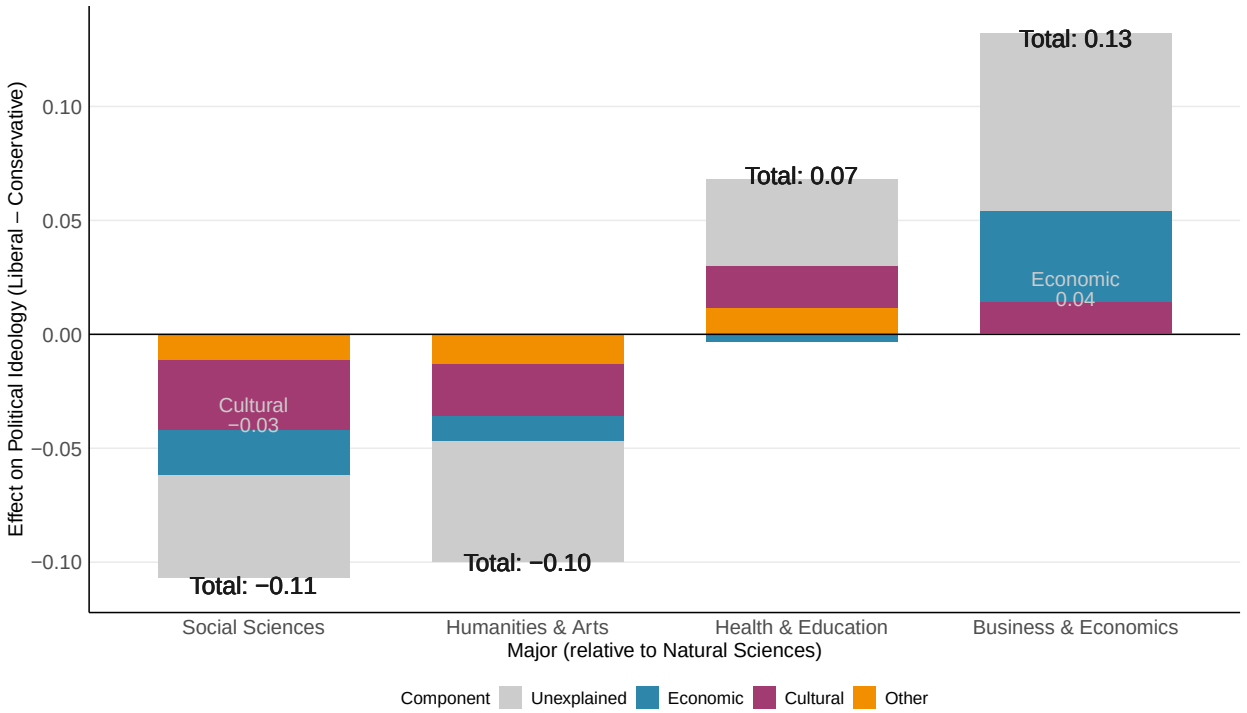
Appendix F Peer effects

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.[49] Here we use the most detailed major classification available, which corresponds to the full set of specific intended-major options in each year's survey rather than a broader grouping. We adopt this fine-grained definition to capture each student's closest peers, those they are most likely to encounter in their classes.

To avoid reflection issues (Manski, 1993), the analysis focuses on the effects of peers' pre-college ideology. Causal identification relies on exploiting idiosyncratic variations in the composition of peers within the same program (college-major) over time. To achieve this, we examine the relationship between a student's graduation ideology and the average baseline ideology of their peers. The main control variable is the average ideology of potential peers: students in the same college and major from adjacent cohorts (two years ahead and two

[49]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 helps avoid potential endogeneity arising 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 H.1A), and even those who do not are likely to share similar early course experiences.

Figure E.1: Gelbach Decomposition of the Effects of College Majors



Note: This figure plots the decomposition of the effects of each college major (relative to the natural sciences) on students' political ideology at graduation. Each bar represents the total effect for a given major, divided into components explained by economic, cultural, and other positions, and the portion not explained by the included mediators ("Unexplained"). Economic, cultural, and other components correspond to principal components constructed from relevant survey questions as described in the text. Values indicate the contribution of each component to the total effect, measured in SD of political ideology (liberal–conservative).

years behind). This control lets us interpret the estimates as comparing the effects of actual peers versus similar potential peers, so that identification comes from idiosyncratic deviations of actual peers from this benchmark.[50] We also add fixed effects at the college-by-major, college-by-year, and major-by-year levels and, in the richer specifications, individual-level baseline controls. Formally, we estimate:

$$p_{icmt} = \sigma P_{cmt}^{-i} + \beta \Phi_{cmt}^{-i} + \gamma' X_i + \nu_{cm} + \iota_{ct} + \omega_{mt} + \varepsilon_{icmt} \tag{A5}$$

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_{cmt}^{-i} represents the average baseline political ideology of the student's peers (in SD); Φ_{cmt}^{-i} 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},

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

ι_{ct}, and ω_{mt} denote college-major, college-cohort, and major-cohort fixed effects, respectively; and X_i is a vector of individual-level baseline controls: the student's own baseline ideology, life goals, intended career, and baseline policy positions. Table F.2 reports specifications with and without these controls. Identification of this specification relies on the assumption that potential graduation ideology is orthogonal to peers' average baseline ideology, conditional on the controls. One way to check this assumption is to regress students' own baseline ideology on the peer measure; because baseline ideology is the outcome of this check, the own-ideology and policy-position components of X_i are omitted from it. We report these checks below.[51]

Measuring peer baseline ideology based on a subsample. A key challenge in our setting is that only a subset of peers is observed. Using the observed peers' average ideology may introduce attenuation bias by incorrectly assuming that observed and unobserved peers share similar idiosyncratic components of ideology. To address this, we construct an adjusted treatment measure that predicts the unobserved peers' ideology based on Equation A5:

$$P_{cmt}^{-i} = \alpha_{ct} P_{cmt}^{-i,O} + (1 - \alpha_{ct}) \widehat{P_{cmt}^{-i}} \tag{A6}$$

Where $P_{cmt}^{-i,O}$ is the average baseline ideology of observed peers, α_{ct} is the share of students in college c and enrollment year t covered by the measured peer group,[52] and $\widehat{P_{cmt}^{-i}}$ 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 A5, except for P itself. The control for potential peers' baseline ideology (Φ_{cmt}^{-i}) is likewise computed from the split sample that excludes the student's quarter, and enters the specification without a coverage adjustment. Under the assumption that peers' ideologies are uncorrelated conditional on controls, this constructed measure allows identification of peer effects and avoids attenuation bias. Appendix G 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.

[51] Figure H.9 shows that substantial variation in peer ideology remains after residualization. The residual standard deviation is 0.15 in the preferred specification, compared with 0.42 in the raw specification.

[52] We calculate the coverage as the number of baseline-survey respondents with a valid ideology response and a non-missing, non-undecided intended major, excluding the quarter of the graduation sample that contains student i, divided by total first-year BA enrollment from the IPEDS administrative data. In the falsification tests that use the full baseline sample, all baseline respondents are split into quarters, and the coverage is defined identically, with the numerator counting baseline respondents outside the quarter that contains student i.

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, 2026).[53] 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.[54]

Falsification tests. Table F.1 presents these checks, which regress students' baseline ideology on their peers' average baseline ideology using equation A5, omitting the own-ideology and policy-position controls. 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. The adjusted estimates are preferred, given that the unadjusted estimates might be attenuated toward zero. In the graduation sample, the coefficients are small and statistically insignificant (0.022, s.e. 0.026, in the fullest specification). In the full baseline sample, the corresponding coefficient is statistically significant though economically very small (0.018, s.e. 0.008, roughly one percent of the unconditional association in column (1)). Peer composition thus retains a slight association with students' predetermined views after the fixed effects and the potential-peers control. This is why the preferred outcome specifications also include the individual-level controls: conditioning directly on students' own baseline ideology and policy positions should absorb most of this residual sorting.

[53] The 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.

[54] 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 F.1: Falsification Tests: "Effects" of Peer Ideology Composition on Baseline Ideology

Panel A. Full Baseline Survey					
	(1)	(2)	(3)	(4)	(5)
Estimate	0.702***	0.024***	0.031***	0.010***	0.009***
	(0.012)	(0.003)	(0.003)	(0.003)	(0.003)
Estimate (Adjusted)	1.603***	0.053***	0.070***	0.019**	0.018**
	(0.026)	(0.007)	(0.009)	(0.008)	(0.008)
N	5,786,769	5,786,769	5,786,769	5,786,769	5,786,769
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	0.751***	0.021*	0.019	0.011	0.010
	(0.021)	(0.013)	(0.014)	(0.014)	(0.014)
Estimate (Adjusted)	1.340***	0.045*	0.040	0.022	0.022
	(0.040)	(0.025)	(0.026)	(0.025)	(0.026)
N	223,104	223,104	223,104	223,104	223,104
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 A5 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 adjustment in equation A6; coverage is measured as described in Appendix G.5. In the columns without fixed effects, the unobserved share of peers is imputed from the full set of fixed effects together with the potential-peers' average, rather than from the sample mean. Panel A uses the full baseline-survey sample. Panel B uses only students observed in the graduation survey. In both panels the outcome is standardized against one reference distribution, the 8,969,044 baseline responses with a valid ideology report, so the panels' coefficients are comparable. Each reported coefficient averages the four split-sample iterations described in Appendix F. "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 college-cohorts in which baseline-survey respondents with a valid ideology response are at least 10% of first-year enrollment from the IPEDS administrative data. In the few college-cohorts in which surveyed respondents exceed recorded IPEDS enrollment, reflecting single-year IPEDS reporting errors, the coverage share is capped at one in the adjustment (1,709 observations in Panel A and 95 in Panel B). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Estimated peer effects. Table F.2 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 positive effects larger than about 0.009 SD in the preferred specification, the upper limit of the two-sided 95 percent confidence interval.

Table F.2: Estimated Effects of Peer Ideology Composition

Panel A. All Majors, Various Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.760***	0.001	-0.011	-0.006	-0.013	-0.014
	(0.024)	(0.012)	(0.013)	(0.013)	(0.011)	(0.010)
Estimate (Adjusted)	1.357***	0.003	-0.020	-0.013	-0.026	-0.027
	(0.047)	(0.023)	(0.024)	(0.024)	(0.020)	(0.018)
N	224,810	224,810	224,810	224,810	224,810	224,810
Fixed Effects	No	Yes	No	Yes	Yes	Yes
Potential Peers' Average	No	No	Yes	Yes	Yes	Yes
Baseline Ideology	No	No	No	No	Yes	Yes
Major Predictors	No	No	No	No	Yes	Yes
Policy Positions	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	-0.034	-0.012	-0.004	0.020	0.036	-0.004
	(0.027)	(0.059)	(0.028)	(0.035)	(0.039)	(0.028)
Estimate (Adjusted)	-0.052	-0.024	-0.016	0.033	0.044	-0.011
	(0.053)	(0.097)	(0.054)	(0.063)	(0.067)	(0.054)
N	46,938	24,348	52,262	28,284	29,176	43,671
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Potential Peers' Average	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Ideology	Yes	Yes	Yes	Yes	Yes	Yes
Major Predictors	Yes	Yes	Yes	Yes	Yes	Yes
Policy Positions	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents estimates of peer effects on students' graduation political ideology based on equation A5. 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 adjustment in equation A6; coverage is measured as described in Appendix G.5. In the columns without fixed effects, the unobserved share of peers is imputed from the full set of fixed effects together with the potential-peers' average, rather than from the sample mean. Panel A reports various specifications. Panel B reports estimates from the fully controlled specification, for subsamples defined by students' intended majors. Each reported coefficient averages the four split-sample iterations described in Appendix F. "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. "Baseline Ideology" indicates a control for the student's own baseline political ideology, and "Policy Positions" for their baseline positions on specific policy questions. The sample is restricted to students with non-missing intended major and graduation political ideology, at least 5 observed peers, and college-cohorts in which baseline-survey respondents with a valid ideology response are at least 10% of first-year enrollment from the IPEDS administrative data. In the few college-cohorts in which surveyed respondents exceed recorded IPEDS enrollment, reflecting single-year IPEDS reporting errors, the coverage share is capped at one in the adjustment (95 observations in the fully controlled specification). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 0.009 SD, the maximum peer effect from switching between these majors equals 0.003 SD (0.009×0.351). Since only 37 percent of same-intended-major pairs ultimately choose the same realized major, a more conservative estimate is about 0.008 SD (0.003 / 0.37). This is roughly 4 percent of the estimated 0.23-standard-deviation effect of choosing business or economics over social sciences or humanities.[55]

Panel B of Table F.2 examines heterogeneity across major groups and finds no significant effects for any group. Table F.3 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.

[55] Strother et al. (2021) and Algan et al. (2026) provide strong evidence of peer effects on students' political views during college, each identifying the effect of *actual* social ties. In line with these studies, we do not claim that peers have no effect. Our measure instead captures the ideology of all program peers, only some of whom become actual friends. It is nonetheless the relevant object for our question: choosing a different major shifts exactly this set of peers, so our estimates indicate that peer composition cannot account for more than a small share of the major effects.

Table F.3: Estimated Effects of Peer Ideology Composition, Robustness

	Ideology	Liberal	Index	Ideology	Ideology	Ideology
	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	-0.013	0.009	0.016	-0.012	-0.007	-0.026
	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.017)
Estimate	-0.026	0.010	0.023	-0.024	-0.018	-0.043
(Adjusted)	(0.020)	(0.023)	(0.024)	(0.020)	(0.021)	(0.027)
N	224,810	224,810	220,818	218,445	191,081	119,432
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 A5. 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. The four iterations are stacked in a single regression in which the quarter indicators are interacted with every term except the treatment, namely the fixed effects, the potential-peers' average, and the individual controls, each of the latter entering saturated within quarter, so the reported coefficient is the average of the four iterations. Adjusted estimates correct attenuation from incomplete peer coverage using the adjustment in equation A6, where the coverage ratio measures the share of the cohort covered by the measured peer group, which excludes the student's own quarter of the sample. Specifications include controls for the average index of potential peers from adjacent cohorts (Potential Peers' Average), pre-college political ideology (Baseline), and college-major, college-cohort, and major-cohort fixed-effects (FEs). Estimates for Ideology and Index are in standard deviation units; for Liberal, the outcome is a 0/1 indicator and the treatment is the peers' liberal share, so estimates are in probability units. The sample is restricted to students with non-missing values for both major and political views, with information available for at least 5 of their peers, and to college-cohorts in which baseline-survey respondents are at least a share x of first-year enrollment from the IPEDS administrative data, with x reported in the table (Share). In the few college-cohorts in which surveyed respondents exceed recorded IPEDS enrollment, reflecting single-year IPEDS reporting errors, the coverage share is capped at one in the adjustment (95 observations in column (1)). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix G Adjusting the Peer and Departmental Ideology Measures

Our peer measures are built from surveyed students, but the object of interest is the ideology of the whole peer group, surveyed or not. This appendix explains how we bridge that gap: why the raw peer average understates peer effects, how the adjusted measure is built, why the adjusted estimator is not attenuated at all, which assumptions deliver identification, and how coverage is measured and what could go wrong with it. Throughout, all peer averages exclude the student herself and, by the split-sample design, her entire quarter of the graduation sample; we suppress the corresponding superscripts of the main text to keep notation light.

G.1 The problem: observed peer averages understate peer effects

For student i in college c, intended major m, and entry cohort t, the effect we want is σ in

$$p_{icmt} = \sigma P^F_{cmt} + \beta \Phi_{cmt} + \nu_{cm} + \theta_{ct} + \lambda_{mt} + \varepsilon_{icmt}, \tag{A7}$$

where P^F_{cmt} is the average baseline ideology of the *full* peer group, Φ_{cmt} is the adjacent-cohort (potential peers) control, and the remaining terms are college–major, college–cohort, and major–cohort fixed effects and an error. We do not observe P^F_{cmt}. We observe the average over the measured peers only, P^O_{cmt}, which covers a share α_{cmt} of the group; the average of the unmeasured remainder, P^U_{cmt}, is missing. By simple accounting,

$$P^F_{cmt} = \alpha_{cmt} P^O_{cmt} + (1 - \alpha_{cmt}) P^U_{cmt}. \tag{A8}$$

Regressing the outcome directly on P^O_{cmt} understates σ, for a reason Equation A8 makes visible: when the measured peers happen to lean one unit further left than expected, the full group leans only α_{cmt} units further left, because the measured peers are only an α_{cmt} share of it. The regression therefore recovers approximately $\alpha_{cmt}\sigma$, not σ. Equivalently, P^O_{cmt} is a noisy version of P^F_{cmt}, and noise that sits inside a regressor pulls its coefficient toward zero: the attenuation bias associated with classical measurement error.

G.2 The adjusted measure

The correction builds the treatment from two parts: what we see, and our best prediction for what we do not see. For the unmeasured share we use $\widehat{P}_{cmt}$, the peer ideology already predicted by the fixed effects and the adjacent-cohort control, in other words, what we would have predicted for that college–major–cohort cell before seeing any of its surveyed peers. The adjusted measure of Equation A6 is the coverage-weighted combination

$$\widetilde{P}_{cmt} = \alpha_{cmt} P^{O}_{cmt} + (1 - \alpha_{cmt})\, \widehat{P}_{cmt}. \tag{A9}$$

The adjusted measure departs from the prediction $\widehat{P}_{cmt}$ only insofar as the measured peers do, and that departure is scaled by α_{cmt}, the share of the peer group for which the measured peers are informative. In the terminology of the measurement-error literature, the construction is regression calibration: an unobserved regressor is replaced by its conditional expectation given the observed data (Carroll et al., 2006). Until Section G.5 we treat the coverage of the measured group, α_{cmt}, as known; that section describes how we measure it. Because $\widehat{P}_{cmt}$ is built only from variables already in the regression and no peer measure contains the student's own response, the construction cannot generate a mechanical correlation with the outcome.

G.3 Why the adjusted estimator is not attenuated

Substituting Equation A8 into Equation A7 and adding and subtracting $(1 - \alpha_{cmt})\widehat{P}_{cmt}$ rewrites the outcome equation, with no approximation, in terms of the adjusted measure:

$$p_{icmt} = \sigma \widetilde{P}_{cmt} + \beta \Phi_{cmt} + \nu_{cm} + \theta_{ct} + \lambda_{mt} + u_{icmt}, \tag{A10}$$

where $u_{icmt} \equiv \sigma(1 - \alpha_{cmt})\big(P^{U}_{cmt} - \widehat{P}_{cmt}\big) + \varepsilon_{icmt}$. Equation A10 is the regression we estimate. Relative to Equation A7, the deviation of the unmeasured peers' average from its prediction no longer contaminates the regressor; it has moved into the error term. Under the assumptions formalized in the next subsection, this component is a mean-zero random term, uncorrelated with the adjusted measure and with every control. An error component with these properties raises the residual variance but does not enter the expectation of the least-squares estimator, so it is not expected to affect any coefficient: the regression of the outcome on $\widetilde{P}_{cmt}$ and the controls estimates the same σ as the infeasible regression on the full-group average.

In the language of measurement error, the adjusted measure carries Berkson rather than classical error. With classical error the observed regressor equals the truth plus noise, as when reading a noisy thermometer, and the noise transmits attenuation. Here the relation is reversed: the truth equals the constructed regressor plus a deviation that is unrelated to the constructed regressor. Errors with this structure are Berkson errors (Berkson, 1950; Hyslop and Imbens, 2001), and in linear models they affect precision, not consistency.

G.4 Identifying assumptions and identification

Let $\mathcal{C}_{cmt}$ collect the conditioning variables of Equation A7: the adjacent-cohort control Φ_{cmt} and the three sets of fixed effects. Identification rests on two assumptions.

(A1) *Exogenous composition.* $E[\,\varepsilon_{icmt} \mid P^{O}_{cmt}, P^{U}_{cmt}, \alpha_{cmt}, \mathcal{C}_{cmt}\,] = 0$: conditional on the fixed effects and the adjacent-cohort control, neither the ideology composition of a college–major–cohort cell nor its survey coverage predicts the unexplained part of the outcome. The composition part is the paper's design assumption, maintained throughout the peer analysis.

(A2) *Representative measurement.* $E[\,P^{U}_{cmt} - \widehat{P}_{cmt} \mid P^{O}_{cmt}, \alpha_{cmt}, \mathcal{C}_{cmt}\,] = 0$: the unmeasured peers' deviation from its prediction is unpredictable from the measured peers' average and from the level of coverage. This holds exactly when the surveyed peers are a random subsample within the cell. Conditioning on α_{cmt} makes the coverage requirement explicit even when coverage is known: response rates may differ freely across colleges, cohorts, and cells, but they must carry no information about the cells' idiosyncratic ideology deviations.

In words, (A1) requires that students who end up among more liberal or more conservative peers are not different in any other way that matters for their graduation ideology, once the fixed effects and the adjacent-cohort control are accounted for. (A2) requires the survey to take an honest snapshot of each cell: the peers we measure lean, on average, the same way as the peers we miss, so neither the measured average nor the response rate reveals anything about the unmeasured remainder. The falsification tests in Table F.1, which regress students' baseline ideology on the adjusted peer measure, test (A1) directly: a nonzero coefficient would mean that cells' composition or coverage predicts students' predetermined views. (A2) is not fully testable, but under standard conditions its failure does not bias the estimator toward spurious findings; it only attenuates, so there is no risk of a false positive.

These assumptions imply that the error term of Equation A10 is conditionally mean zero:

$$E[\,u_{icmt} \mid \widetilde{P}_{cmt}, \mathcal{C}_{cmt}\,] = \sigma\, E[\,(1-\alpha_{cmt})\big(P^{U}_{cmt} - \widehat{P}_{cmt}\big) \mid \widetilde{P}_{cmt}, \mathcal{C}_{cmt}\,] + E[\,\varepsilon_{icmt} \mid \widetilde{P}_{cmt}, \mathcal{C}_{cmt}\,] = 0,$$

where the first term vanishes by (A2) and the second by (A1), both by iterated expectations, since the adjusted measure is a function of the measured average, the coverage, and the controls. A conditionally mean-zero error is exactly the requirement for least squares to be consistent, so the adjusted regression identifies σ, the effect of the full peer group's average ideology, at any level of coverage and with no attenuation factor left to correct.

G.5 Measuring coverage

In practice we do not observe the coverage of each college–major–cohort cell; we measure coverage at the college–cohort level, which is why Equation A6 carries a ct subscript. α_{ct} is the number of baseline-survey respondents with a valid ideology response and a non-missing, non-undecided intended major in college c and cohort t, excluding the quarter of the graduation sample that contains the student, divided by total first-year enrollment from the IPEDS administrative data; the counts thus refer to the measured peer group, with the quarter exclusion already embedded.

Using α_{ct} in place of the true cell coverage α_{cmt} introduces a gap, $\kappa_{cmt} = \alpha_{cmt} - \alpha_{ct}$. A mean-zero gap unrelated to the cells' ideology composition is harmless: it multiplies the cell's mean-zero ideology deviation and averages out, contributing second-order noise rather than bias; the same holds for response rates related to outcome *levels*.[56] The damaging case is response rates related to the cells' idiosyncratic composition itself, for example if unusually conservative business cohorts were less likely to answer: the coverage that the identification argument conditions on is then no longer known. As the simulations confirm, such a violation would also surface in the falsification tests in Table F.1; those tests find at most a very small residual association, and the outcome specifications additionally condition on students' own baseline views. Finally, a systematic error in the scale of the coverage ratio passes through one for one: overstating coverage by some factor shrinks the estimate by the same factor.

[56] In unreported simulations calibrated to our setting, cell-level coverage noise of twenty percent and response rates strongly correlated with cell-level outcome shocks leave the adjusted estimate at the truth and the falsification at zero, and replacing the college–cohort coverage with the true cell-level coverage moves the estimate by less than three percent of the effect. Response rates correlated with the cells' ideology tilt itself attenuate the estimate and push the falsification coefficient away from zero, with the induced shift comparable to or larger than the attenuation it accompanies.

G.6 The departmental-ideology measure

The departmental-ideology measure is built identically, with one simplification: faculty are never outcome units, so there is no split-sample exclusion, and α is simply the surveyed share of the relevant faculty group. Assumptions (A1) and (A2) carry over with "measured faculty" in place of "measured peers", and the corresponding falsification, regressing students' baseline ideology on the adjusted departmental-ideology measure, is reported in Table H.11.

G.7 Relation to existing corrections

Two corrections for partially observed peer groups are established in the literature. Ammermueller and Pischke (2009) study classmates sampled within classrooms and treat the observed peer average as a classical error-ridden measure of the true class average: under random sampling within the class, the regression attenuates by a reliability ratio that depends on the sampled share and the variance components, which they estimate and use to rescale the coefficient after estimation. The diagnosis is the same as ours, but the remedy corrects the slope rather than the regressor. The reliability ratio is specific to each specification, because conditioning on fixed effects and controls changes the share of true peer variance that survives, and the rescaling becomes unwieldy when the observed share varies widely across groups, as it does across our college–cohorts.

Sojourner (2013) comes closer to our approach. Using random assignment in Project STAR, where prior achievement is missing for part of each class, he regresses the outcome on the observed peers' average while controlling for the numbers of observed and unobserved peers, so that, when missingness is ignorable conditional on those counts, the unobserved peers contribute only through terms the controls absorb. This is regression calibration with a coarse imputation: the missing peers are in effect assigned a stratum-level expected value. Our construction is the same principle with two refinements suited to our setting. The unmeasured peers are assigned the cell-specific prediction from the fixed effects and the adjacent-cohort control rather than a common mean, and the coverage share enters the measure explicitly, which is what produces the Berkson structure derived above and consistency at any coverage. His ignorability condition is the counterpart of our Assumption (A2).

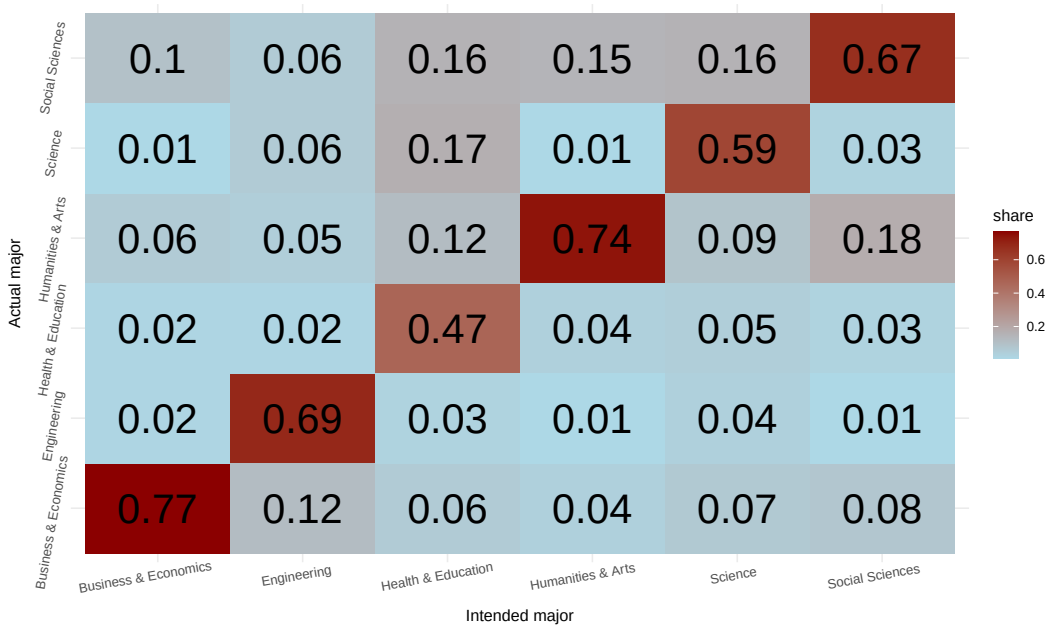
The approaches coincide in simple settings and separate in ours. With constant coverage and a conditioning set that predicts little of the peer composition, rescaling the coefficient by the reliability ratio, controlling for the counts of missing peers, and our adjustment deliver essentially the same answer. They separate when coverage varies widely across groups, when the conditioning set has real predictive power for the cells' composition, and when the true

group size is itself unobserved: the first two are exactly the features of our application, and the third is why coverage must be measured at the college–cohort level (Section G.5). A further difference is diagnostic: because the adjusted measure feeds the same specification as the main analysis, the falsification tests of Table F.1 discipline the correction itself, a check without a counterpart in the rescaling approach.

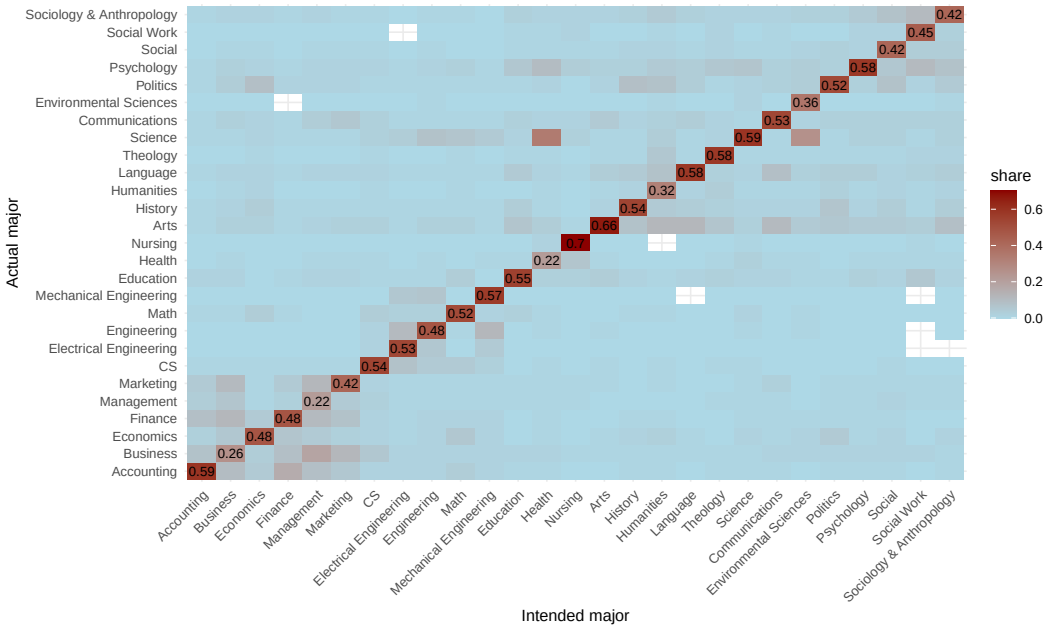
Appendix H Additional Figures and Tables

Figure H.1: Intended and Actual Majors

(A) Broad Groups

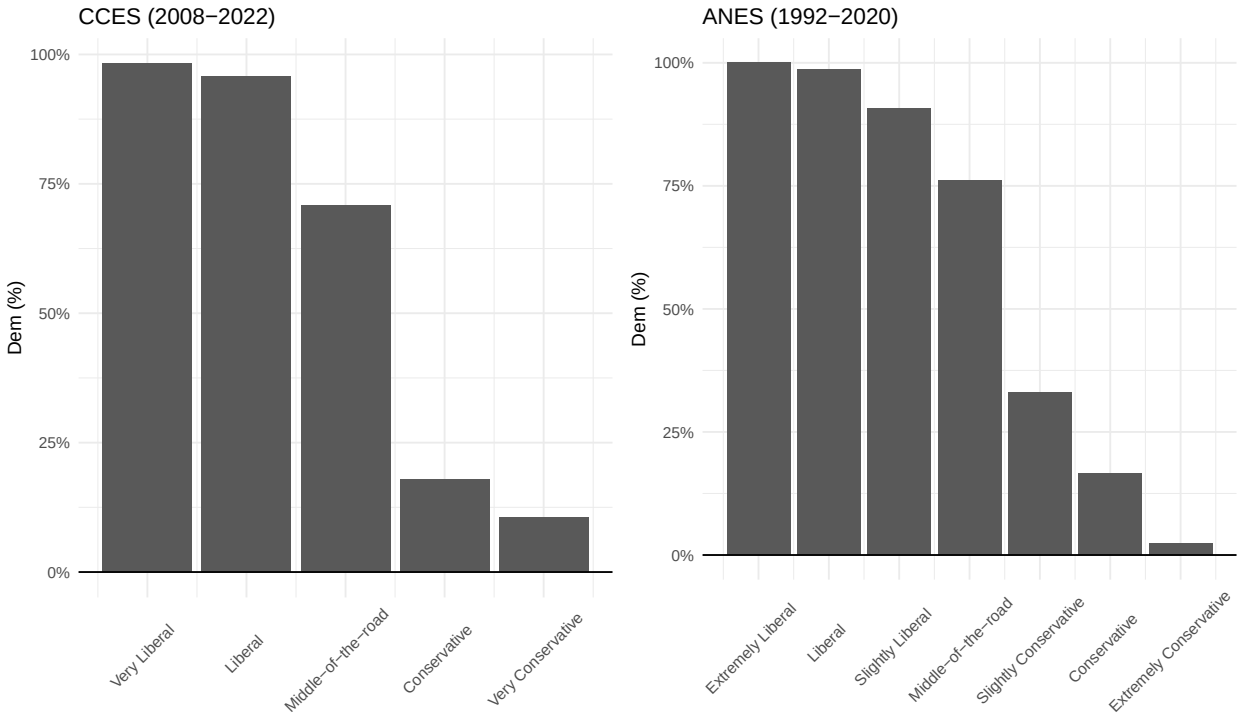


(B) Detailed Groups



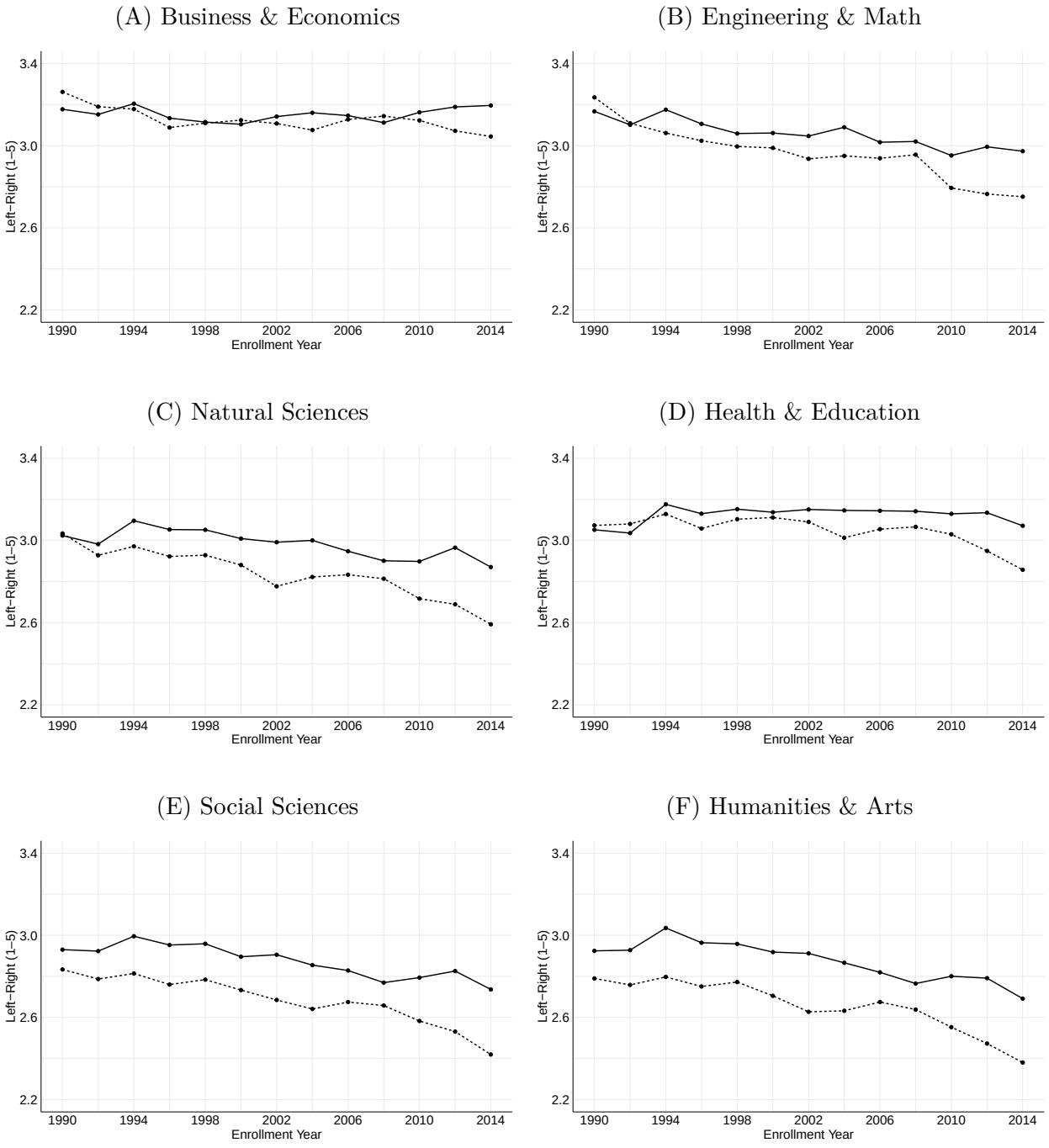
Note: This figure plots students' intended and actual majors. The sample includes all students in our sample with non-missing values for this variable, both in the entry and exit surveys (273,869 students). The first (second) panel shows the results in broad (detailed) major groups.

Figure H.2: Students' Ideology and Voting in National Surveys



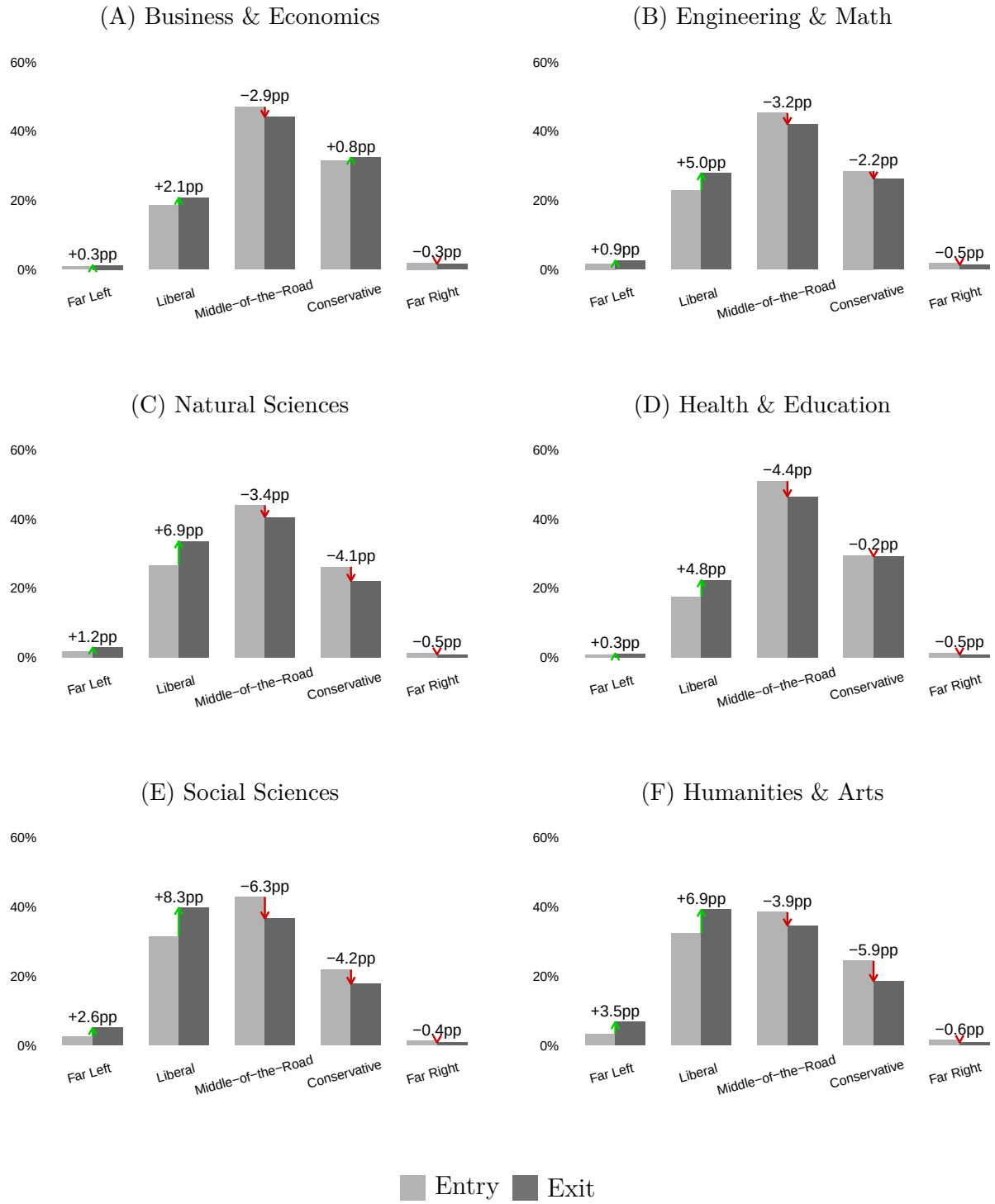
Note: This figure plots the share of students voting for the Democratic candidate in presidential elections at each ideological self-placement, using student respondents in the CCES (n=11,570) and the ANES (n=345). Both surveys include an ideology scale and a presidential-vote question; The CCES asks, "In general, how would you describe your own political viewpoint?", with five substantive response options: very liberal, liberal, moderate, conservative, and very conservative; "Not Sure" responses are excluded. The ANES asks "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?", with seven responses ranging from extremely liberal to extremely conservative. The CCES question aligns more closely with the HERI wording but dates back only to 2006, while the ANES is older but includes fewer students and uses a seven-point scale. The sample includes all student respondents with non-missing ideology and presidential vote. The correlation between ideology and voting is high in both surveys: 0.65 in the CCES and 0.59 in the ANES. Sources: Cooperative Election Study (2006–2024); American National Election Studies (2026).

Figure H.3: Students' Political Ideology, HERI Survey, 1990–2015



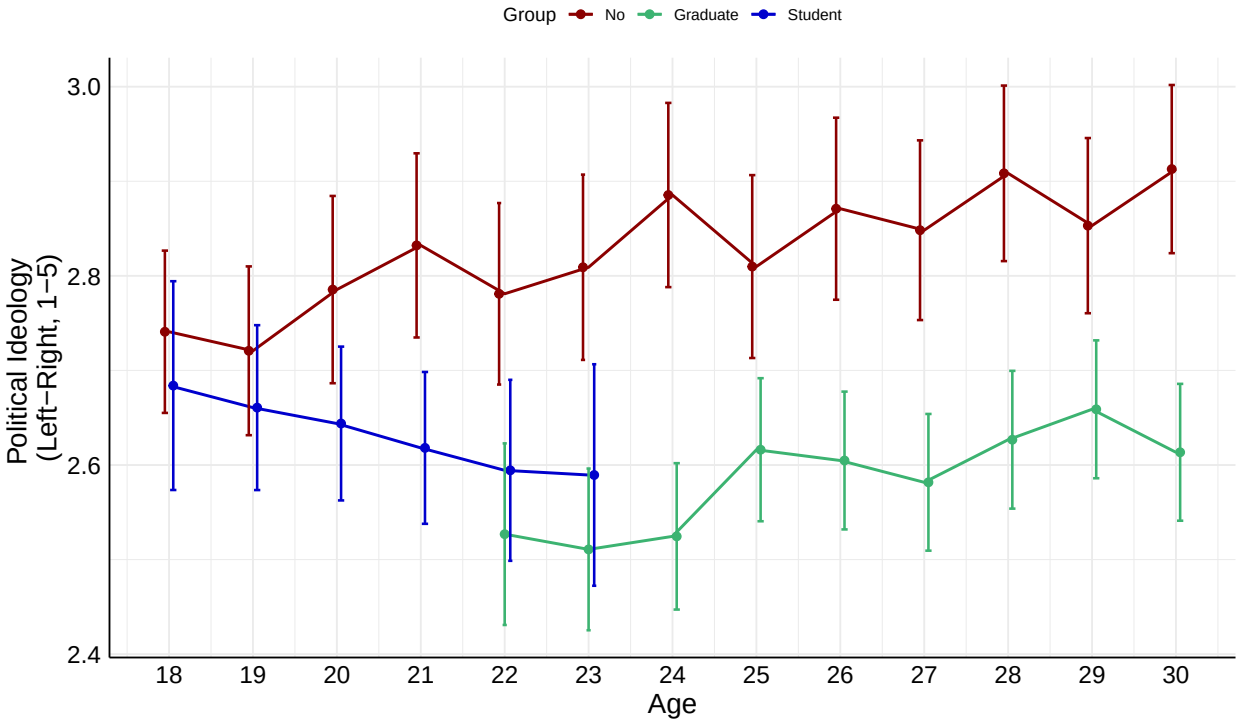
Note: This figure plots students' political ideology at college entry (solid lines) and at graduation (dashed lines). The sample includes all students from these colleges who entered before 2016 and have non-missing ideology measures in both the entry and exit surveys (294,238 students). Political ideology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

Figure H.4: Political Ideology Distributions Before and After College



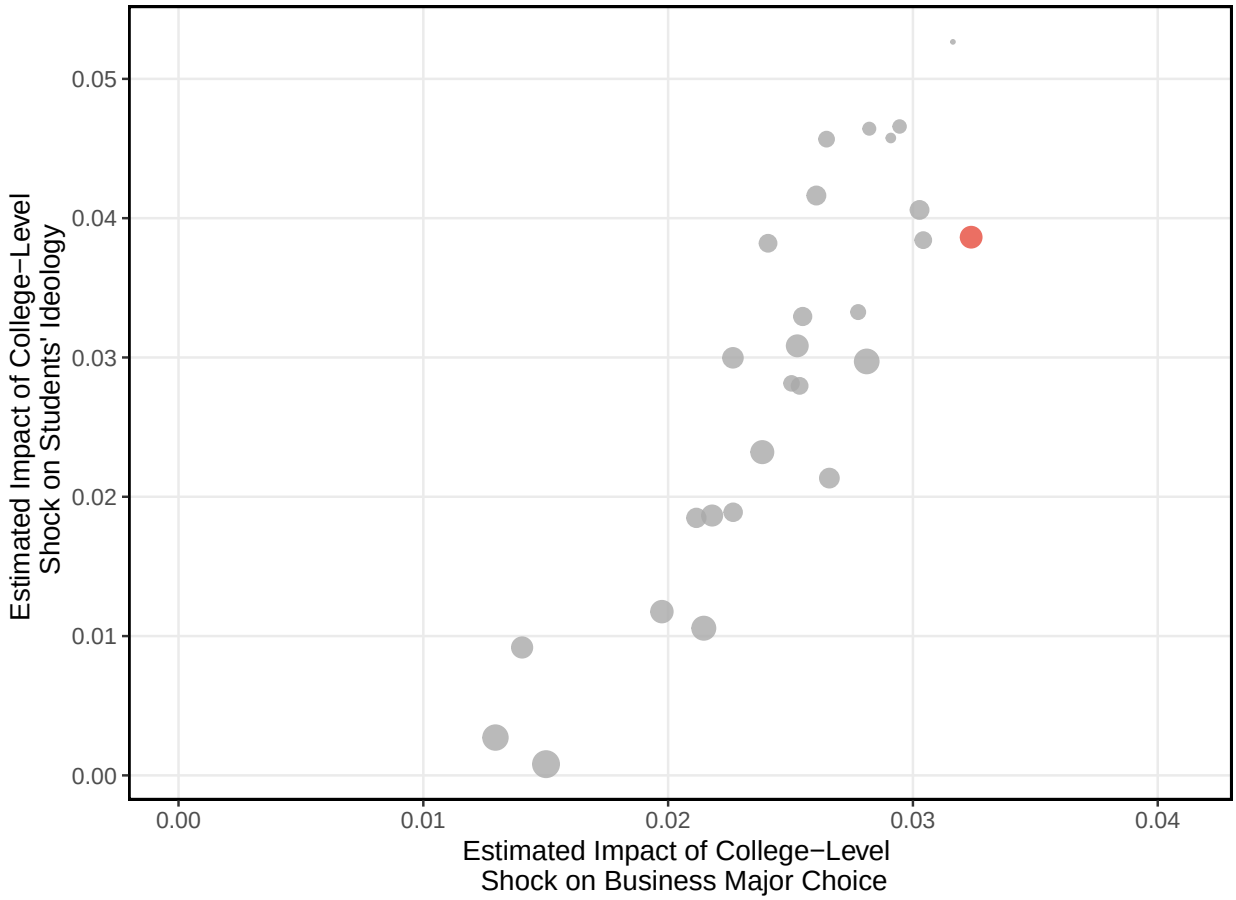
Note: This figure plots the distribution of students' baseline (light grey) and graduation (dark grey) political ideology. The sample includes all students with non-missing values for this outcome in the baseline and graduation surveys, totaling 293,523 students. The samples in Panels A–F are restricted to students in specific groups of majors.

Figure H.5: Political Ideology by Age, Students and non-Students, CCES Survey



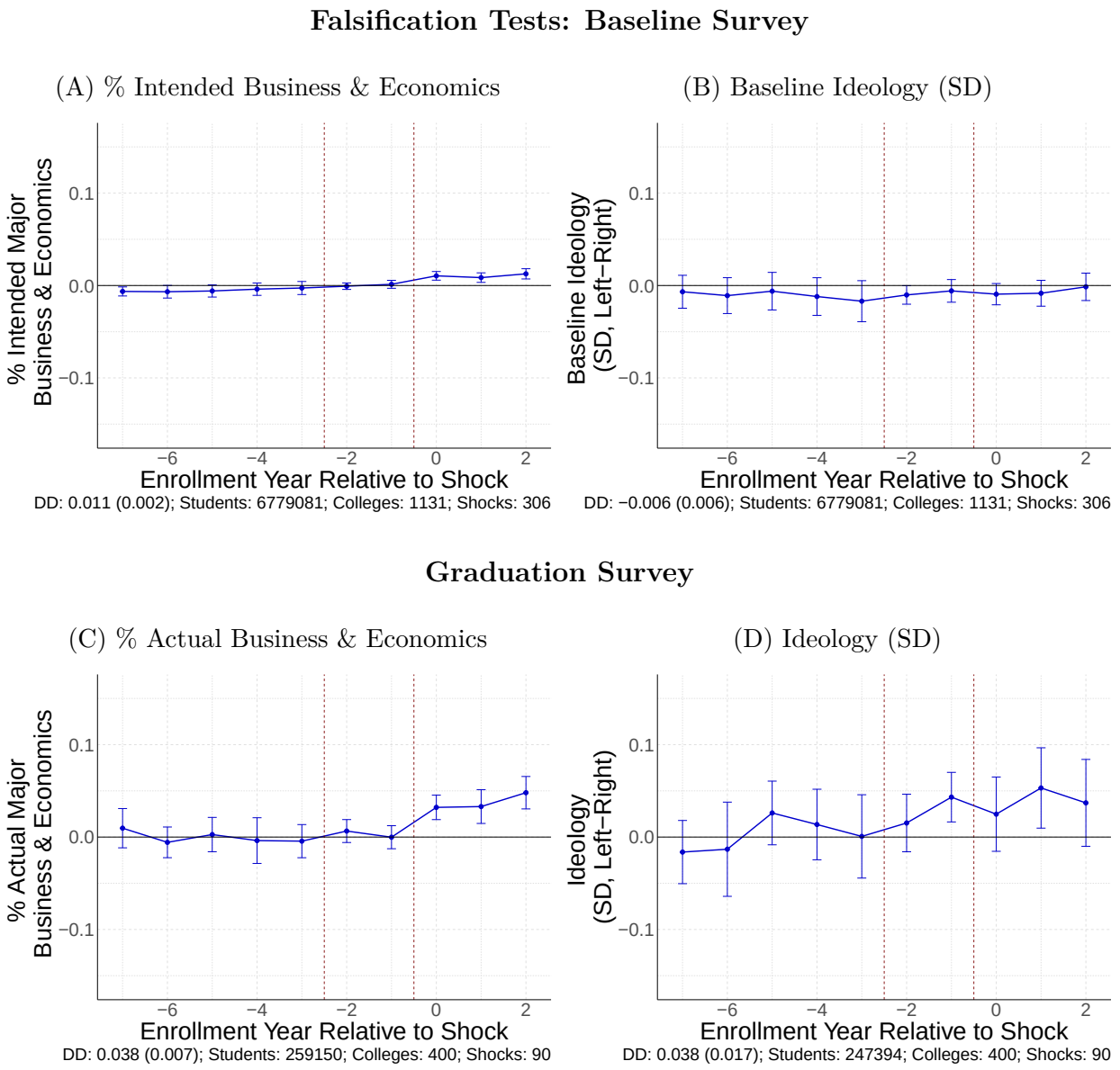
Note: This figure shows political ideology by age for college students (blue), college graduates (green) and individuals with no college education (high-school education or less, in red) in the CCES. Ideology is based on responses to "How would you characterize your political views?" on a 1-5 scale (from 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 H.6: Comparing Main Quasi-Experimental Results for Different Shock Definitions



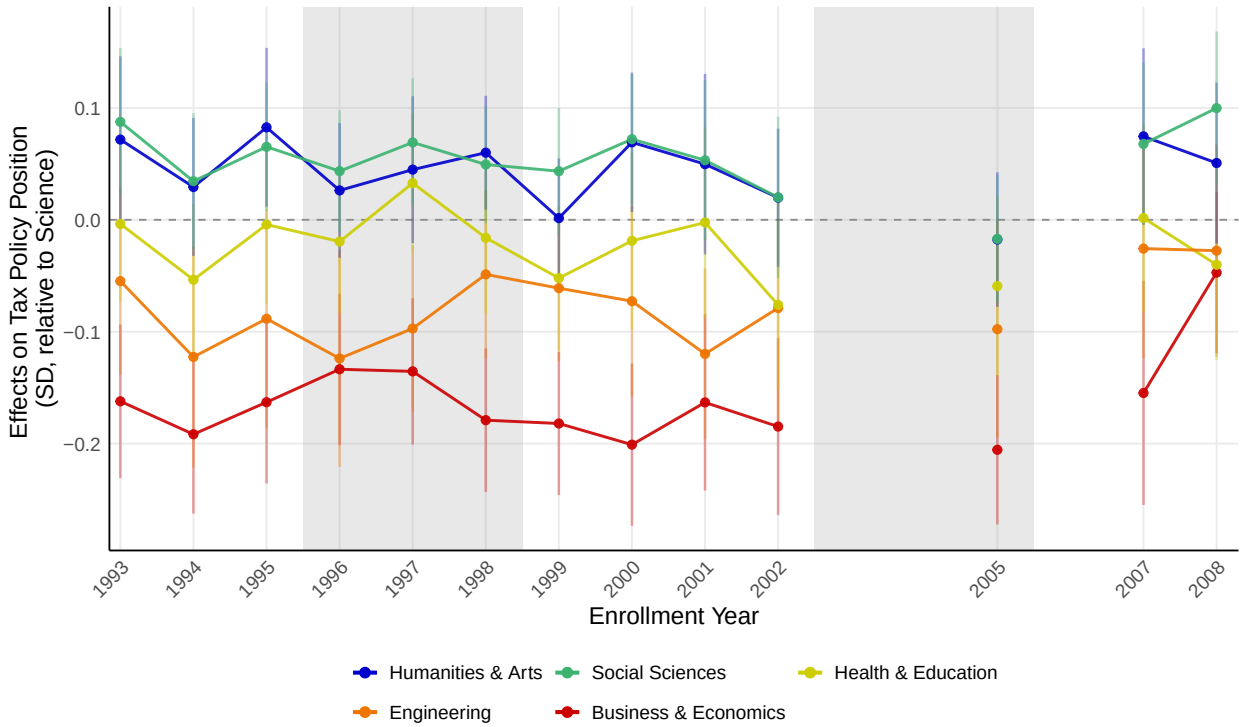
Note: Each point represents one of 27 candidate definitions of a college-level expansion in business and economics. The horizontal axis reports the coefficient on the pooled post-shock indicator from a two-way fixed-effects regression of business/economics major choice on shock exposure, with college and entry-cohort fixed effects. The vertical axis reports the corresponding coefficient for graduation ideology; this specification additionally controls flexibly for baseline ideology. Standard errors are clustered by college. The larger red point denotes the definition with the largest t-statistic for the effect on majoring in business or economics (the horizontal axis); this definition is used in the main analysis. Point size indicates the number of identified shock events.

Figure H.7: Effects of College-Level Expansions in Economics & Business, Imputation Estimator



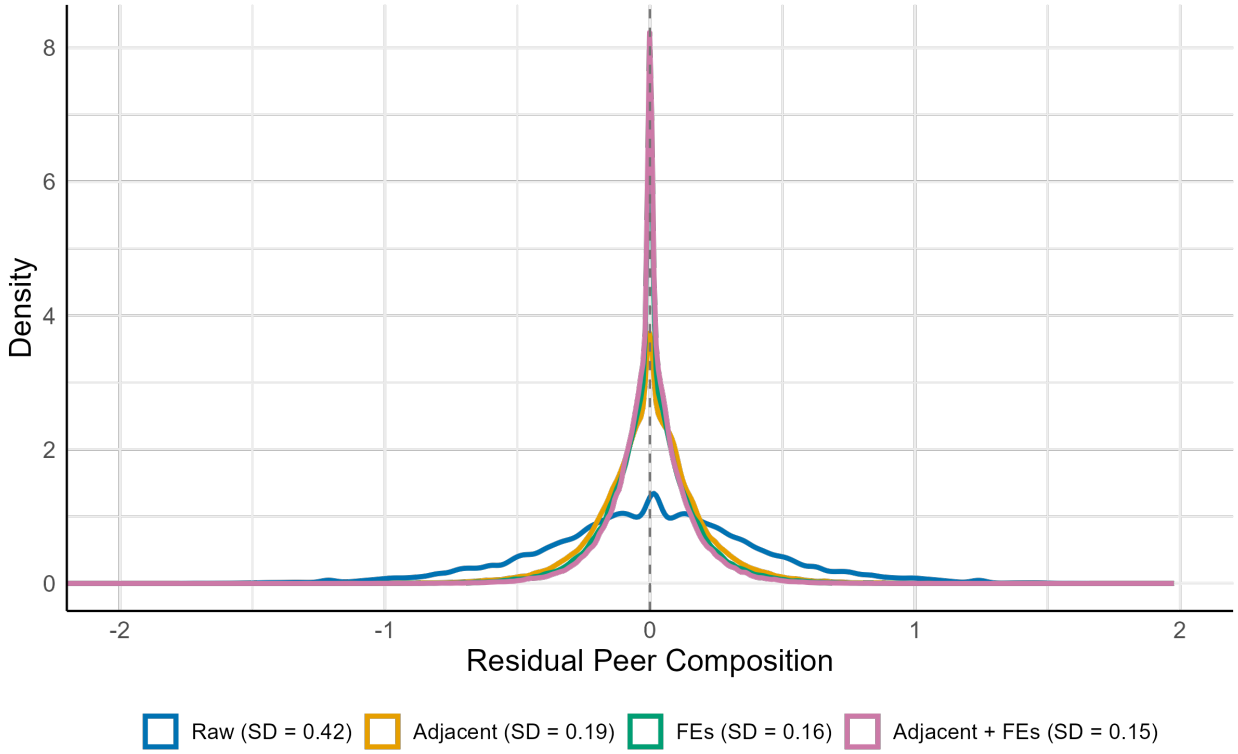
Note: These figures plot event-study coefficients estimated from Equation 2 using the imputation estimator of Borusyak et al. (2024), with standard errors clustered at the college level. Each panel also reports the pooled post-shock difference-in-differences estimate (DD), along with the number of students, colleges, and treated colleges in the estimation sample. The omitted time category is three periods before the event. We exclude from the DD estimation the two cohorts immediately preceding the shock because they may already be partially exposed to it. 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 H.8: 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 specification 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. Because the horizontal axis is the enrollment year while attitudes are measured at graduation, the shaded regions mark the enrollment cohorts whose graduation year (about four years later) falls in a financial crisis: the dot-com bust (enrollment 1996–1998, graduation 2000–2002) and the 2008 financial crisis (enrollment 2003–2005, graduation 2007–2009).

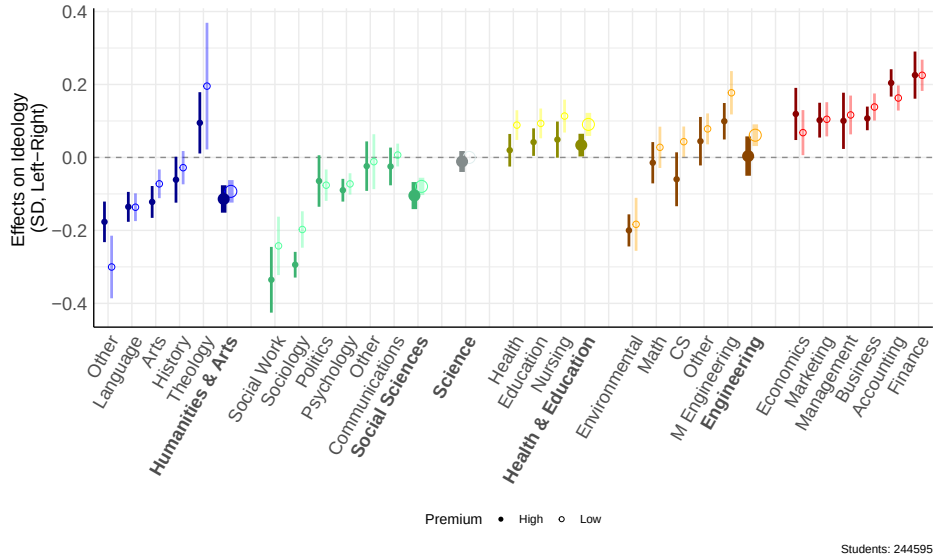
Figure H.9: Residual Variation in Same-Cohort Peer Ideology Across Specifications



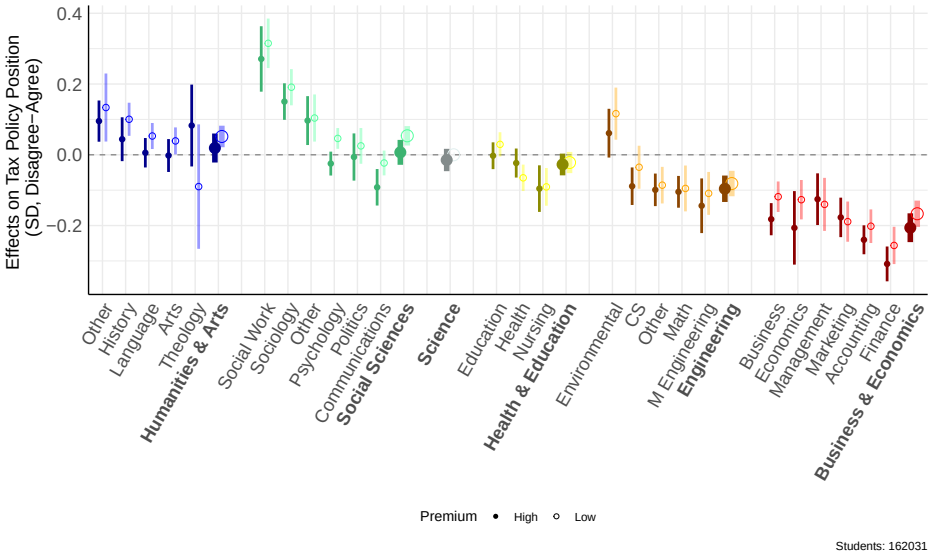
Note: This figure plots kernel-density estimates of residual same-cohort peer ideology under four residualization specifications. "Raw" removes sample-splitting fold indicators. "Adjacent" additionally controls for mean peer ideology in the five-year window centered on the entry cohort, allowing its association with same-cohort peer ideology to vary by fold. "FEs" absorbs college-by-entry-cohort-by-fold, entry-cohort-by-intended-major-by-fold, and college-by-intended-major-by-fold fixed effects. "Adjacent + FEs" combines the adjacent-cohort controls and the three sets of fixed effects. All specifications use the same sample. Parentheses report residual SD. The dashed vertical line marks zero.

Figure H.10: Heterogeneity of the Effects of College Majors, by Earnings Expectations

(A) Political Ideology

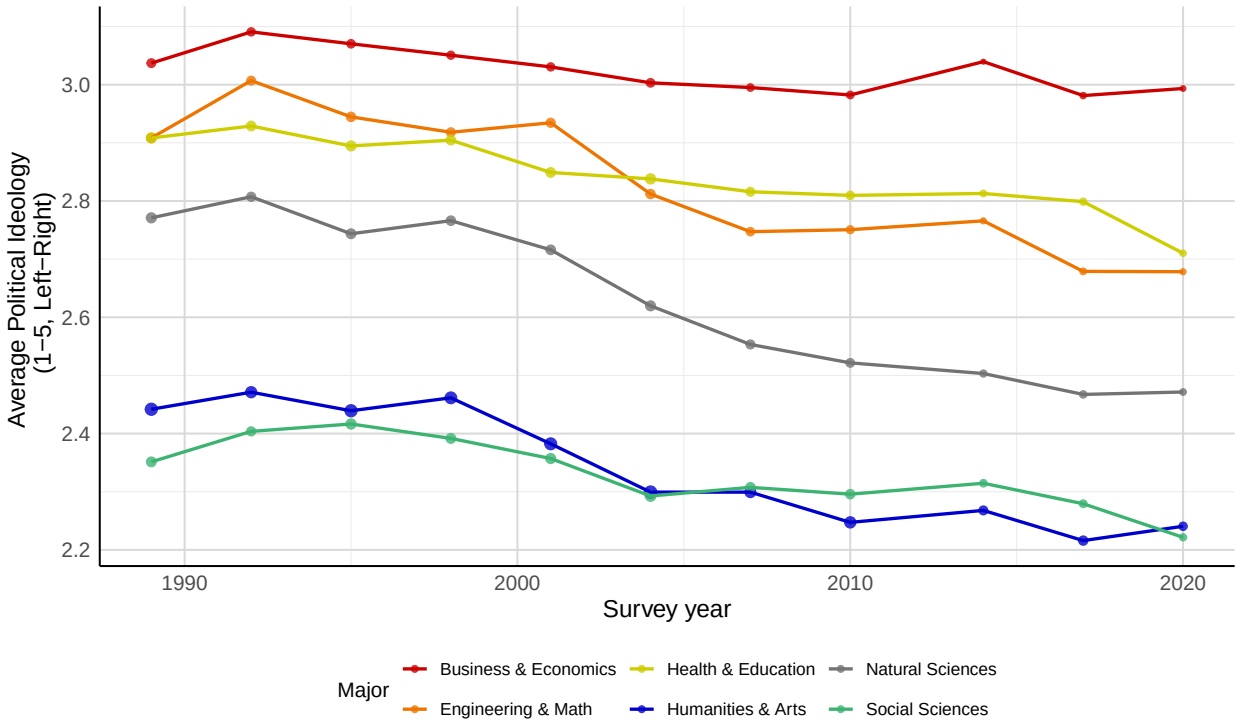


(B) Tax Policy Position



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 specification that groups majors into six broad categories. The thinner lines show coefficients from a more detailed specification with specific major categories. Low (high) earnings expectations are defined at the college-by-major level as an earnings premium below (at or above) the student-weighted median within the same detailed major, which splits each major into roughly equal-sized groups and keeps the comparison within the same specific field. 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 H.4).

Figure H.11: Departmental Ideology Over Time



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

Table H.1: College Majors Classification and Aggregation

Broad Group	Detailed Group	List of Majors
Natural Sciences	Natural Sciences	Agriculture, Biochemistry, Biology, Chemistry, Earth Science, Marine Sciences, Microbiology, Other Biological, Other Physical, Physics, Zoology
Engineering & Math	CS	Computer Science, Data
Engineering & Math	Electronic Engineering	Computer Engineering, Electronic Engineering
Engineering & Math	Engineering	Aero Engineering, Architecture, Chemical Engineering, Civil Engineering, Industrial Engineering, Other Engineering
Engineering & Math	Math	Math
Engineering & Math	Mechanical Engineering	Mechanical Engineering
Social Sciences	Communications	Communications, Speech
Social Sciences	Environmental	Environmental Science
Social Sciences	Politics	Politics
Social Sciences	Psychology	Psychology
Social Sciences	Social	Geography, Law Enforcement, Other Social
Social Sciences	Social Work	Social Work
Social Sciences	Sociology Anthropology	Anthropology, Sociology
Business & Economics	Accounting	Accounting
Business & Economics	Business	Business, International Business, Other Business
Business & Economics	Economics	Economics
Business & Economics	Finance	Finance
Business & Economics	Management	Management
Business & Economics	Marketing	Marketing
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
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
Excluded from the analysis sample		
Other	Other	Building Trades, Home Economics, Other, Other Technical
Missing / Undecided	Missing Undecided	Missing, NA, Undecided

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. The classification is the one the estimates use: the biological, chemical and physical science groups are pooled into a single natural sciences group, giving 27 detailed groups. Students whose broad group is one of the 2 categories below the line are excluded from the analysis sample.

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

First Major	Second Major	Students	Share (%)
Business & Economics	None	16944	63.7
Business & Economics	Business & Economics	5554	20.9
Business & Economics	Humanities & Arts	1523	5.7
Business & Economics	Social Sciences	1309	4.9
Business & Economics	Engineering	670	2.5
Business & Economics	Other	381	1.4
Business & Economics	Health & Education	112	0.4
Business & Economics	Science	104	0.4
Engineering	None	9330	78.3
Engineering	Engineering	747	6.3
Engineering	Business & Economics	623	5.2
Engineering	Humanities & Arts	470	3.9
Engineering	Health & Education	238	2.0
Engineering	Science	211	1.8
Engineering	Social Sciences	191	1.6
Engineering	Other	107	0.9
Health & Education	None	10630	78.3
Health & Education	Health & Education	1269	9.4
Health & Education	Humanities & Arts	704	5.2
Health & Education	Social Sciences	368	2.7
Health & Education	Other	192	1.4
Health & Education	Science	167	1.2
Health & Education	Business & Economics	119	0.9
Health & Education	Engineering	119	0.9
Humanities & Arts	None	14862	63.5
Humanities & Arts	Humanities & Arts	4264	18.2
Humanities & Arts	Social Sciences	2149	9.2
Humanities & Arts	Health & Education	946	4.0
Humanities & Arts	Business & Economics	509	2.2
Humanities & Arts	Other	287	1.2
Humanities & Arts	Science	230	1.0
Humanities & Arts	Engineering	155	0.7
Science	None	12883	75.8
Science	Humanities & Arts	1068	6.3
Science	Science	1002	5.9
Science	Social Sciences	870	5.1
Science	Engineering	431	2.5
Science	Health & Education	308	1.8
Science	Business & Economics	250	1.5
Science	Other	173	1.0
Social Sciences	None	20828	67.0
Social Sciences	Humanities & Arts	3925	12.6
Social Sciences	Social Sciences	3644	11.7
Social Sciences	Business & Economics	998	3.2
Social Sciences	Other	660	2.1
Social Sciences	Health & Education	550	1.8
Social Sciences	Science	369	1.2
Social Sciences	Engineering	117	0.4

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. "First major" is the primary major the student reports on the graduation survey, and "second major" is the additional major reported alongside it; the survey records both at graduation and does not date either declaration. "None" denotes students with no second major. "Share" gives each combination's percentage of students within the first major.

Table H.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	266413	Base	48.8%	48.4%	48.7%	37.8%	50.6%	50.4%	52.8%
		Grad	62.3%	61.2%	61.5%	47.3%	65%	64.7%	68.1%
Addressing global climate change should be a federal priority	63137	Base	30.1%	26.2%	29.1%	27%	31.4%	32.8%	32.7%
		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	157964	Base	64.3%	59.9%	58.1%	66.4%	66.5%	62.9%	68.5%
		Grad	68.2%	57.2%	60.3%	70.6%	73%	69.3%	75%
It is important to have laws prohibiting homosexual relationships	200751	Base	26.1%	30.4%	28.1%	32.3%	22.3%	25.1%	21.9%
		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	205167	Base	57%	49.4%	58.3%	59.4%	59.1%	59.5%	58.9%
		Grad	60.7%	47.7%	58.4%	61.3%	66.9%	63.1%	66.5%
Marijuana should be legalized	235668	Base	27.5%	27.8%	27%	17.3%	31.6%	25.6%	30.5%
		Grad	44.1%	43.1%	44.4%	28.3%	49.7%	43.5%	49.1%
The death penalty should be abolished	218041	Base	31.4%	25%	28.4%	28%	38.8%	30.7%	34.1%
		Grad	39.9%	30.6%	35.7%	31.6%	50.7%	38.7%	45.5%
The federal government should do more to control the sale of handguns	160770	Base	81.1%	79.8%	75.2%	83.3%	81.6%	80.2%	83.3%
		Grad	82.1%	79.8%	74.8%	84.6%	83.6%	81.2%	84.8%
There is too much concern in the courts for the rights of criminals	150932	Base	65.5%	69.7%	67.6%	69.1%	59.8%	67.5%	62.9%
		Grad	55.1%	63.2%	59.3%	62.5%	46.6%	57.1%	48.3%
Racial discrimination is no longer a major problem in America	278065	Base	16.6%	19.9%	20%	16.4%	14.2%	16.7%	14.3%
		Grad	13.5%	18.7%	18.4%	11.9%	10.8%	13.4%	10.2%
Realistically, an individual can do little to bring about changes in our society	216454	Base	21.7%	25.8%	25.4%	20.9%	18.9%	20.8%	20.1%
		Grad	24.2%	28.6%	31%	20.9%	22.3%	24.7%	20.9%
Colleges should prohibit racist/sexist speech on campus	242609	Base	50.1%	48.7%	44.9%	56.1%	50%	49.7%	50.4%
		Grad	56%	53.7%	50.6%	60.8%	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 H.4: Control Variables

Panel A. Variables

Control set	Variables (each entered as indicators for its observed response categories)
Main-specification control sets	
Outcome predictors *baseline ideology and policy positions*	Political self-placement (left–right; five response options, far left to far right); **agree/disagree (four response options) that:** abortion should be legal · the government should do more to control handguns · the death penalty should be abolished · the courts care too much about criminals' rights · laws should prohibit homosexual relationships · the wealthy should pay a larger share of taxes · there should be a national health-care plan · individuals can do little to change society · colleges should prohibit racist/sexist speech · married women's place is the home · the government should do more on climate change · racial discrimination is no longer a major problem · marijuana should be legalized
Major predictors *field choice and motivations*	Intended major; probable career; **importance of:** cleaning up the environment · a community action program · promoting racial understanding · helping others in difficulty · being very well off financially · keeping up with political affairs · influencing the political structure · influencing social values · raising a family · developing a meaningful philosophy of life
Additional control sets (robustness specifications only)	
Behaviors *past year*	discussed politics · voted in a student election · performed volunteer work · participated in organized demonstrations · attended a religious service
Time use *hours per week*	talking with teachers outside class · reading for pleasure · household and childcare duties · volunteering · watching TV · student clubs and groups · partying · working for pay · socializing with friends · exercise or sports · studying
Demographics	sex · race/ethnicity · parents' total income · parents' marital status · citizenship · first-generation status · religion · parents' probable careers · high-school GPA

Table H.4: *(continued)*

Panel B. Specification Ladder

	(1) None	(2) Lagged D	(3) Lagged Y	(4) Main	(5) 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

Note: Panel A lists the control variables in each set; all categorical controls enter flexibly as indicators for their observed response categories. Panel B maps the control sets to the specifications displayed in Figure C.3: "Lagged D" adds intended major, the student's probable career, and baseline life goals; "Lagged Y" adds baseline political ideology and policy positions; "Main" includes both sets; and "All" additionally includes college and enrollment-year fixed effects, baseline behaviors, time use, and demographic characteristics.

Table H.5: Effects of College Majors on Additional Policy Positions, Life Goals, and Behaviors

	Policy Positions								Life Goals & Behaviors					
	(1) Death	(2) Guns	(3) Marijuana	(4) LGBTQ	(5) Criminal	(6) Change	(7) Climate	(8) Speech	(9) Social	(10) Helping	(11) Politics	(12) Family	(13) Meaning	(14) Discussed Course
Business &	-0.04***	-0.02	0.01	0.07***	0.03**	0.02*	-0.21***	-0.01	0.08***	-0.09***	0.14***	0.08***	-0.05***	-0.16***
Economics	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Engineering &	-0.01	-0.05***	0.00	0.01	0.03**	0.05***	-0.10***	-0.04***	-0.06***	-0.14***	-0.03**	-0.01	-0.07***	-0.01
Math	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Health &	-0.04***	0.05***	-0.10***	0.05***	0.02	-0.05***	-0.20***	0.02*	0.17***	0.09***	0.02**	0.10***	0.02	-0.02*
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Social	0.09***	0.04***	0.09***	-0.06***	-0.13***	-0.09***	-0.08***	0.03***	0.34***	0.10***	0.25***	0.02***	0.03***	-0.14***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Humanities &	0.12***	0.06***	0.09***	-0.08***	-0.10***	-0.04***	-0.10***	0.01	0.22***	-0.02	0.17***	-0.07***	0.15***	-0.07***
Arts	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	218041	160770	235668	200751	150932	216454	63137	242609	308628	308679	291844	308866	308487	315905
Summary Statistics (raw scale)														
Graduation Mean	2.25	3.19	2.23	1.78	2.66	1.98	3.03	2.65	2.53	3.06	2.43	3.15	2.71	2.65
Graduation SD	1.02	0.90	1.06	1.00	0.84	0.83	0.91	1.03	0.90	0.78	0.92	0.93	1.01	0.52
Change in Omitted Group	0.18	-0.04	0.41	-0.28	-0.21	0.10	0.40	-0.20	0.10	0.08	0.13	0.05	0.22	

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; sample sizes vary across columns because not all questions appear in every survey year. Columns (1)-(8) are policy positions, originally coded on a 1–4 scale (1=Disagree Strongly, 2=Disagree Somewhat, 3=Agree Somewhat, 4=Agree Strongly): The death penalty should be abolished (Death); The federal government should do more to control the sale of handguns (Guns); Marijuana should be legalized (Marijuana); It is important to have laws prohibiting homosexual relationships (LGBTQ); There is too much concern in the courts for the rights of criminals (Criminal); Realistically, an individual can do little to bring about changes in our society (Change); Addressing global climate change should be a federal priority (Climate); and Colleges should prohibit racist/sexist speech on campus (Speech). Columns (9)-(13) are stated life goals, originally coded on a 1–4 scale (1=Not important, 2=Somewhat important, 3=Very important, 4=Essential): 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). Column (14) is a behavior: discussed course content with students outside of class (Discussed Course), originally coded on a 1–3 scale (1=Not at all, 2=Occasionally, 3=Frequently). Each outcome is standardized by subtracting the mean and dividing by the standard deviation of the graduation response among all graduation-survey respondents. "Graduation Mean" and "Graduation SD" report the raw-scale values used for the standardization, so multiplying a coefficient by the SD row converts it into raw scale points. "Change in Omitted Group" reports the average raw-scale change from baseline to graduation among students in the omitted group (natural sciences) with valid responses in both surveys; it is blank for Discussed Course because the freshman survey does not include an analogous question. Estimates for abortion, taxes, healthcare, racial discrimination, the policy-position index, the financial-success goal, demonstrations, discussing politics, and voter registration appear in Table 2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table H.6: 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	-0.046***	-0.034***	-0.004**	-0.006*	-0.003
	(0.010)	(0.009)	(0.002)	(0.003)	(0.004)
N	49,176	49,176	49,176	49,176	49,176
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	0.021***	-0.014**	0.007	0.007	0.037**
	(0.005)	(0.007)	(0.009)	(0.007)	(0.017)
N	49,176	49,176	49,176	49,176	48,346
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 H.7: IV Estimates Based on Persistence in Intended Majors, Adjacent Cohorts

Panel A. Falsification Tests: Baseline Ideology

	(1)	(2)	(3)
Full Baseline Survey	-0.020	-0.029	-0.024
	(0.021)	(0.023)	(0.020)
N	278,619	278,619	278,619
Students Included in the Graduation Survey	0.012	-0.023	-0.010
	(0.048)	(0.047)	(0.038)
N	37,083	37,083	37,083
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	0.156***	0.114***	0.108***	0.105***	0.079***
	(0.028)	(0.031)	(0.030)	(0.030)	(0.018)
Reduced Form: Graduation Ideology	0.066*	0.032	0.036	0.032*	0.023
	(0.038)	(0.039)	(0.034)	(0.017)	(0.017)
2SLS: Graduation Ideology	0.421*	0.279	0.335	0.304	0.291
	(0.223)	(0.329)	(0.302)	(0.194)	(0.223)
N	38,452	38,452	38,452	38,452	38,452
First-stage F	30.4	13.4	12.7	12.3	18.8
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 at the finest survey level, finer than the detailed grouping used to construct the instrument. "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 H.8: 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	-1.082***	-0.573	0.068	-0.645	-1.007	-2.391***
	(0.368)	(0.912)	(1.286)	(1.070)	(0.831)	(0.699)
Control Mean	9.60	12.36	13.12	23.86	18.53	94.24
Shocks	123	123	123	123	123	115
Colleges	433	433	433	433	433	372
N (students)	255,107	255,107	255,107	255,107	255,107	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	-1.579*	0.209	1.370	0.035
	(0.811)	(0.841)	(0.846)	(0.031)
Control Mean	34.18	41.16	24.66	-0.06
Shocks	123	123	123	123
Colleges	433	433	433	433
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 2 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 an indicator for majoring in engineering and mathematics, an indicator for majoring in natural sciences, an indicator for majoring in health and education fields, an indicator for majoring in social sciences, an indicator for majoring in humanities and arts, and an indicator for taking any humanities or arts course; the effect on majoring in business and economics is reported in the main paper. Panel B reports effects on an indicator for identifying as liberal or far left, an indicator for identifying as middle-of-the-road, an indicator for identifying as conservative or far right, and the policy-position ideology index. 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 in the pooled estimation sample. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table H.9: 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	0.767**	-1.898***	0.034*
	(0.364)	(0.728)	(0.019)
Control Mean	4.46	95.25	-0.17
Shocks	118	111	118
Colleges	417	362	417
N (students)	125,233	82,223	120,815
B. High			
	(1)	(2)	(3)
	% Business/Economics Major	% Humanities/Arts Course	Ideology (SD)
Estimate	4.165***	-2.296***	0.041*
	(1.472)	(0.865)	(0.024)
Control Mean	40.24	93.26	0.11
Shocks	103	96	103
Colleges	386	333	386
N (students)	126,843	80,714	119,904

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 2 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 are 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 in the pooled estimation sample. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table H.10: Mechanism Correlations with Estimated Major Effects on Political Ideology

	(1)	(2)	(3)	(4)
Faculty	0.241***			0.178***
	(0.020)			(0.041)
Peers		0.483***		0.120*
		(0.084)		(0.069)
Earnings premium			0.505***	0.083
			(0.081)	(0.081)
R^2	0.856	0.571	0.610	0.876
Adjusted R^2	0.850	0.554	0.594	0.859
Majors	27	27	27	27
Underlying students	303,363	303,363	303,363	303,363

Notes: The dependent variable in each regression is the estimated effect of a detailed major group on graduation political ideology ($\hat{\delta}_m$ from equation 1, in standard-deviation units), re-estimated on the subsample of colleges that have College Scorecard earnings data. The natural sciences are included as the 27th observation, anchored at $\beta = 0$ by construction (omitted category in the underlying regression). Each independent variable summarises one candidate mechanism at the major level: *Faculty* is the simple average ideology of all faculty respondents in the major, pooled across the colleges with students in the main estimation sample (157,152 faculty responses across 400 colleges); *Peers (chosen)* is the average baseline ideology of CSS students who graduated with the major, computed within college-by-major cells and averaged across colleges weighted by the number of analysis students in the cell. Each ideology proxy is standardized on the distribution of its own respondents: faculty ideology in standard deviations across college-by-department cells, and the peer measure in standard deviations of individual students' baseline ideology. Coefficients are therefore student standard deviations per one standard deviation of the corresponding measure. *Earnings premium* is the within-college log-wage premium of the major from the College Scorecard, averaged across colleges with the same student-count weight. Sample: 303,363 students from 441 colleges with non-missing graduation ideology and College Scorecard earnings data, aggregated to 27 major groups. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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

Panel A. Full Baseline Survey			
	(1)	(2)	(3)
Estimate	0.198***	0.007***	0.005***
	(0.008)	(0.001)	(0.001)
Estimate (Adjusted)	0.434***	0.016***	0.013***
	(0.016)	(0.003)	(0.003)
N	4,354,251	4,354,251	4,354,251
Fixed Effects	No	Yes	Yes
Major Predictors	No	No	Yes
Panel B. Cohorts Included in the Graduation Survey			
	(1)	(2)	(3)
Estimate	0.214***	0.003	0.002
	(0.011)	(0.003)	(0.003)
Estimate (Adjusted)	0.452***	0.010	0.006
	(0.022)	(0.007)	(0.006)
N	702,917	702,917	702,917
Fixed Effects	No	Yes	Yes
Major Predictors	No	No	Yes
Panel C. Students Included in the Graduation Survey			
	(1)	(2)	(3)
Estimate	0.282***	-0.003	-0.004
	(0.018)	(0.006)	(0.006)
Estimate (Adjusted)	0.576***	-0.000	-0.002
	(0.032)	(0.014)	(0.014)
N	162,977	162,977	162,977
Fixed Effects	No	Yes	Yes
Major Predictors	No	No	Yes

Notes: This table presents falsification tests for equation 3, 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. Within each panel, columns report the raw correlation, the fixed-effects specification, and the specification that adds major predictors. Entries report coefficient estimates with college-clustered standard errors shown below each estimate. Adjusted estimates correct for attenuation from incomplete faculty coverage. Faculty ideology is standardized across the 22,127 college-by-department cells with at least 3 surveyed faculty, at colleges where the faculty survey covers at least 10 percent of instructional staff; estimates are per standard deviation. The faculty survey records each respondent's department but reports no department-level staff counts, so coverage is measured at the college and applied uniformly to its departments. Because a department's own coverage may differ from its college's, the correction is imperfect and the adjusted estimates may remain somewhat attenuated. "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. No outcome-predictor controls are used in these falsification specifications because the falsification outcome is baseline political ideology. The sample is restricted to students with non-missing intended major and political ideology, at least 3 observed faculty in the department, and college-level faculty-survey coverage of at least 10%. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.