Changing Minds: How Academic Fields Shape Political Attitudes

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28 September 2026

Abstract

How much can what people study shape their preferences? Using panel surveys of roughly 300,000 U.S. college students over three decades, we estimate the effects of academic fields. Our approach combines a value-added model with rich pre-college predictors of field choice. Relative to the natural sciences, majoring in the humanities or social sciences shifts students about 0.09 standard deviations left, while majoring in business or economics shifts them 0.14 standard deviations right. Sensitivity analyses and several alternative strategies, including exploiting supply-side variation in field availability, support a causal interpretation. These effects have implications for ideological polarization among graduates. Because students tend to choose fields that align with their prior ideology, major effects widen differences among graduates and account for about 4 percent of the overall variance in ideology at graduation and almost half of its increase during college. The effects extend beyond politics to life goals and behavior, with fields shaping how students weigh their own financial success against social goals. The evidence suggests that academic instruction, rather than peer socialization or earnings expectations, drives these effects. Together, the results highlight preference formation as an important dimension of education's effects.

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"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*

Education can shape people's opportunities and skills, affecting what they can do, but it can also shape their preferences, affecting what they want to do. While much economic research has focused on how education affects opportunities and outcomes, public debate has long emphasized whether education also shapes preferences. This question is particularly salient in higher education, where students specialize in different fields of study and are exposed to different bodies of knowledge and ways of thinking. Critics across the political spectrum have targeted different academic disciplines and proposed changes to academic programs out of concern that these ideas shape students' beliefs and preferences.[1]

This paper provides empirical evidence relevant to this debate by studying how fields of study affect students' political and social preferences. We use panel surveys from American colleges conducted by the Higher Education Research Institute (HERI). Our main matched sample follows roughly 300,000 students who enrolled in college between 1990 and 2015 and participated in surveys both just before college and upon graduation. The surveys include rich measures of students' preferences and behaviors at these two critical points in time.

Our main empirical strategy takes advantage of the unusually rich information in these surveys. We first focus on political ideology as our main outcome. We compare students' ideology at graduation across college majors, controlling for pre-college characteristics that play two distinct roles. Baseline ideology and policy positions make the specification similar to a value-added model (e.g., Chetty et al., 2014). Intended majors, career plans, and life goals predict field choice, helping address the concern that students select majors based on other traits that also predict subsequent ideological change.

Fields substantially affect students' political ideology. Relative to the natural sciences, social sciences and humanities shift students 0.09 standard deviations (SD) to the left, while business and economics shift 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 produces the largest rightward shift, 0.23 SD, and social work produces the largest leftward shift, 0.26 SD. These effects are similar across subsamples, including by gender, socioeconomic background, and college selectivity. They have grown slightly over time but remained large throughout our sample period.

[1]For proposals to limit the social sciences and humanities, see AP (2023) and *Time* (2023). Critics have also challenged economics and business education; see *The Harvard Crimson* (2011) and *The Guardian* (2013).
[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.

Several analyses support the causal interpretation. First, sensitivity analysis following Oster (2019) leaves a substantial effect. Under a conservative benchmark that assumes that all remaining explainable variation in graduation ideology, conditional on our main controls, is as strongly related to major choice as baseline ideology, the lower-bound estimate for the effect of majoring in business and economics relative to humanities and social sciences is approximately 0.17 SD, about three-quarters of our main estimate of 0.23 SD. To further support this result, we show that our controls eliminate nearly all omitted-variable bias from additional demographic, behavioral, and home-area covariates.

Second, two instrumental variable (IV) strategies provide further support. The first uses intended majors at college entry to isolate variation in completed majors predicted by pre-college preferences. This reduces concerns that post-entry shocks jointly affect major choice and political ideology, or that the results depend on the weight placed on students who switch majors. The second focuses on students who intended to major in business or economics. It exploits variation in the persistence rate of their intended programs: the probability that students who intend to pursue a given program ultimately complete it. These persistence rates are unrelated to students' baseline ideology, yet strongly predict both their completed majors and their ideology at graduation.[3]

Third, we provide complementary quasi-experimental evidence. We exploit within-college expansions of business and economics fields and estimate their effects using a difference-in-differences framework. The identifying assumption is that, absent an expansion, outcomes at expanding and non-expanding colleges would have followed parallel trends. Anecdotal evidence suggests that such expansions are prompted by external events, such as major donations, and two findings further support the design. First, expanding and non-expanding colleges exhibit similar pre-trends. Second, baseline students' ideology does not change differentially after an expansion, reducing concerns about student selection. Expansions increase enrollment in business and economics and shift students' graduation ideology right.[4]

After establishing causality, we examine the broader implications of our results. We first show that the "diploma divide," which has become central to politics (e.g., Grossmann and Hopkins, 2024), partly reflects what students study. Under a counterfactual in which all students majored in economics or business, the average ideological shift during college would almost disappear, and the gap between graduates and individuals with no college education

[3]Both IV estimates for the effects of business and economics are about 0.30–0.34 SD, smaller than the unadjusted OLS estimate, consistent with positive selection bias in naive comparisons that our preferred specification largely removes. They are, however, slightly larger than the adjusted OLS estimate. Although these differences are insignificant, we discuss possible explanations in Section 2, including attenuation bias.
[4]These effects do not operate only through changes in major choice, because expansions could affect students through both major choice and coursework outside the major, a margin we document empirically.

would shrink by about one-third.[5] The estimated 0.23 SD effect of moving students from business and economics to the social sciences or humanities slightly exceeds the pre-college gender gap of 0.19 SD and is about two-thirds of the 0.35 SD gap between students from New York and Texas. Translating this effect into expected voting behavior implies a shift of about 6 percentage points, comparable to estimated place effects (Brown et al., 2023).

Major choice also increases ideological polarization among college graduates, as students tend to select fields that align with their baseline ideology. We estimate that if all students studied the same major, the increase in ideological variance during college would be about 46 percent smaller, and overall ideological variance at graduation about 4 percent smaller. Major choice also contributes substantially to the ideological gender gap: women's greater tendency to choose left-leaning fields accounts for about 76 percent of its increase during college and about 16 percent of the gap at graduation.

In addition, the effects extend beyond political self-identification to policy support, especially on economic issues. Relative to the natural sciences, business and economics reduce support for redistributive policies, including taxation and health care, by 0.17 SD, while the social sciences and humanities increase support by 0.05–0.07 SD. Effects on cultural issues, including abortion and LGBTQ rights, are smaller. These results are notable for two reasons. First, although the overall education gradient in redistributive preferences is close to zero (Kuziemko et al., 2026), we find substantial differences within the college-educated population. Second, despite prominent debate over the teaching of progressive ideas, the largest differences across fields concern economic rather than cultural issues. One possibility is that fields differ more sharply in how they approach economic questions than cultural ones. In contrast, students in all fields become more liberal on cultural issues during college, suggesting a common influence, although this pattern may also reflect age or time trends.

Furthermore, the effects also extend to life goals and behavior, capturing broader changes in preferences. The humanities and social sciences strengthen social goals, and these shifts are reflected in behavior as students become more politically engaged. By contrast, business and economics substantially increase the importance students place on their own financial success. This also shows up in their career preferences, as they place greater weight on earnings relative to other job attributes, such as meaning or social impact. These shifts may contribute to earnings differences across fields. Consistent with this, Bleemer and Mehta (2022) show that studying economics increases preferences for and employment in higher-paying industries.

Finally, we also examine mechanisms. We consider three main candidates: peers, expected earnings, and instruction. To study peer effects, we measure peers' baseline ideology within

[5]This counterfactual varies majors only. If courses outside the major have similar effects, a counterfactual that also changes course-taking could imply a larger reduction in the college gap.

the same college, major, and cohort. To study expected earnings, 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 study instruction, we use faculty ideology measured at the college-major level in the HERI Faculty Surveys.

Faculty ideology predicts the ideological effects of majors remarkably well: by itself, it explains about 86 percent of the variation in estimated effects across majors. Peer ideology and expected earnings are also positively correlated with these effects, but add little predictive power once faculty ideology is included. We then examine the peer and earnings channels more directly. Changes in peer ideology across adjacent cohorts within the same college and major have little effect on individual outcomes.[6] Similarly, exploiting variation across colleges and over time, we find little evidence that the effects of majors vary with expected earnings.

We then examine the instructional channel more directly by comparing students in the same major across colleges whose faculty differ in ideology. To reduce concerns that students select majors based on faculty ideology, we assign faculty ideology using students' intended majors at college entry. Our specification also includes college-by-cohort-by-major-group fixed effects, so identification comes from differences between closely related fields, such as finance and economics, within the same institution and cohort. Faculty ideology is unrelated to students' baseline ideology in our main sample, yet students exposed to more left-leaning departments graduate further to the left. This relationship is strongest in the humanities and social sciences and accounts for a non-negligible share of their estimated effects.

Evidence from other settings may help interpret this result. 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 to our setting, they suggest that our results are unlikely to be driven primarily by exposure to individual instructors. Curricular content nevertheless remains a plausible channel. Consistent with this interpretation, Light et al. (2026) document large and persistent differences across academic fields in the political content of courses. They also find that instructor-specific factors account for roughly 60–65 percent of differences in course political characteristics across colleges within the same field. This interpretation connects to papers showing that political curricula can shape attitudes (Voigtländer and Voth, 2015; Cantoni et al., 2017;

[6]This does not rule out peer effects more broadly, but suggests that they do not explain 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 peer exposure (Billings et al., 2021; Strother et al., 2021; Algan et al., 2026).

Chen et al., 2023; Meriläinen and Mitrunen, 2025; Braghieri and Eichmeyer, 2024).[7] Our findings suggest that content-based mechanisms may operate even when curricula are not explicitly political and students select into their academic environments.

Together, the results show that what students study in college affects not only their opportunities but also their preferences and objectives. These findings contribute to two strands of literature. First, a growing literature studies the consequences of field specialization in higher education (Kirkebøen et al., 2016, 2021; Heinesen et al., 2026; Bleemer and Mehta, 2022; Andrews et al., 2024; Goldstein, 2026). While this work focuses primarily on career and family outcomes, we show that fields also affect what students value and want to do, which may in turn influence later outcomes. Our findings also connect to recent evidence from earlier stages of education that curricula and fields of study can affect beliefs and preferences (Arold, 2024; Ainsworth et al., 2025; Meriläinen and Mitrunen, 2025).

Second, we contribute to the literature on education and civic outcomes. Most existing work studies the effects of educational attainment and access (Dee, 2004; Milligan et al., 2004; Oreopoulos and Salvanes, 2011; Marshall, 2016, 2019; Cavaillé and Marshall, 2019; Apfeld et al., 2023; Firoozi, 2026; Ben-Porath, 2025; Kaplan et al., 2025). Evidence on the role of fields is much more limited. One recent study documents associations between fields and voting behavior in Europe (Hooghe et al., 2025), while a related literature compares economics students with 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 systematic evidence that fields of study affect political attitudes, life goals, and behavior among students with the same level of educational attainment. Second, we examine the mechanisms behind these effects, distinguishing among peers, expected earnings, and the instructional environment. Third, we show that field specialization can widen political differences. This points to a feature of higher education that may work against the civic-cohesion effects often attributed to earlier stages of schooling (Gradstein and Justman, 2002; Glaeser et al., 2007). It also connects to the literature on political realignment, which documents an increasingly important relationship between education and politics (Gethin et al., 2022; Kuziemko et al., 2026; Longuet-Marx, 2026). Our results suggest that what people study may be an important part of that relationship.

The rest of the paper proceeds as follows. Section 1 describes the data and setting. Section 2 presents estimates of the effects of academic fields on political ideology. Section 3 discusses broader effects and implications. Section 4 examines mechanisms. Section 5 concludes.

[7]Educational content can shape socially consequential outcomes in professional settings: Ash et al. (2026) find that exposure to law-and-economics training shifted the decisions of U.S. federal judges.

1 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.[8] 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.

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 purposefully integrated into mandatory student processes, such as orientation or registration, and graduation packets or rehearsal sessions, yielding relatively high response rates.[9]

The main sample includes 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 students. Appendix A summarizes the sample-construction pipeline. Figure A.1 maps the sample colleges, with each point sized by its number of students, illustrating the broad geographic coverage.

Fields of study. The data include information on students' intended majors when they start college and their actual majors at graduation. We aggregate major into groups at two levels, as detailed in Appendix Table I.1. Appendix Figure I.1 illustrates the transitions between intended and actual majors. The figure shows movement between nearly all major pairs. Still, 47–77 percent of students graduate with a major in the same group they intended.[10]

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).[11] For clarity, we rescaled the variable so that lower values indicate more

[8] 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.

[9] Relative to IPEDS enrollment data, TFS covers an average of 64 percent of incoming cohorts, while CSS covers about 34 percent. These rates are broadly comparable, given college dropout rates.

[10] Note that the survey has contained information on double majors only since 2005. We use this information to examine the effects of double majors. Table I.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.

[11] 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.

left-leaning orientations. This rescaling is used in all subsequent analyses. We standardize ideology using the mean and standard deviation of all graduation-survey responses and apply the same transformation to baseline, peer, and faculty ideology. Unless stated otherwise, all ideology measures, including the change during college, are therefore in SD units of graduation ideology.[12] 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 I.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 I.3, where students are grouped in both series by their actual major at graduation. Figure I.4 further presents distributional changes in ideology over the course of college, separately by major group. Panel A of Figure 1 compares baseline and graduation ideology across majors. For majors in business and economics, the average ideology remains similar to 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 line. Finally, humanities and social sciences majors show the largest leftward shift.

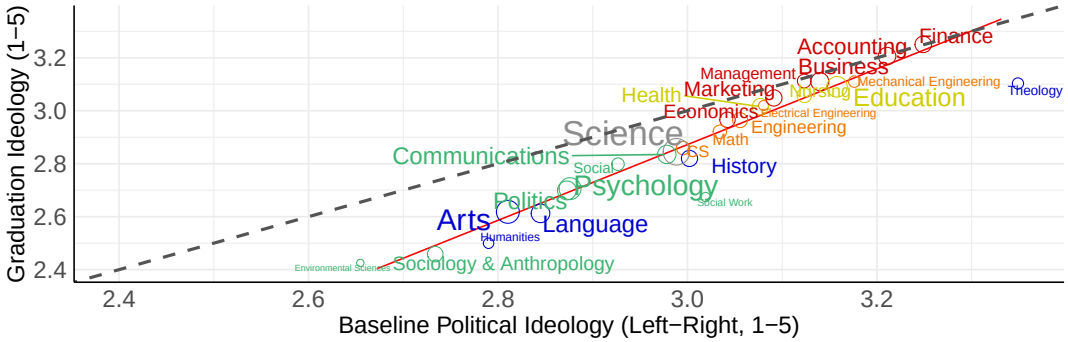
Interestingly, the fields in which students become more left-leaning are typically those that also attract more left-leaning students at baseline. There are exceptions: some majors attract less left-leaning students yet induce a more leftward shift. Notable examples are theology, which attracts relatively conservative students, and social work, whose entrants are close to the sample average, but both lead to marked leftward shifts 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 I.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. This may suggest that field differences are larger for economic policy positions than for other 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 move toward more liberal positions, with much less cross-major variation in these changes.

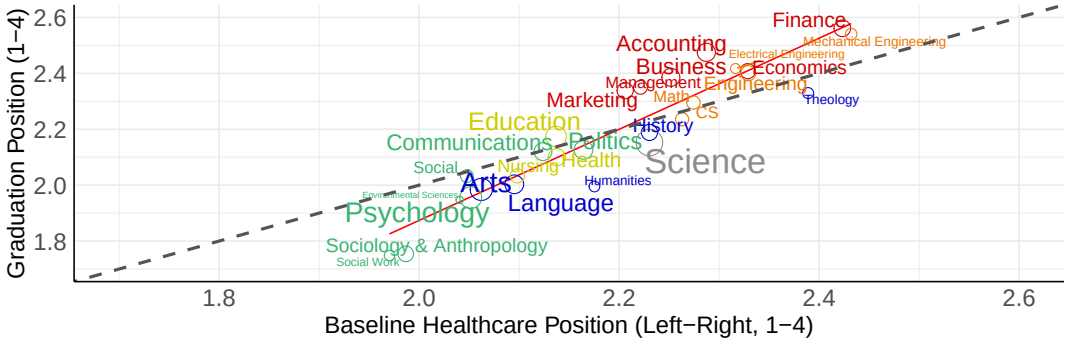
[12]The graduation-survey mean and standard deviation are 2.91 and 0.84 on the rescaled five-point scale, computed over the 720,800 graduation-survey responses with a valid answer. Faculty responses are first recoded so that, as for students, lower values indicate more left-leaning views.

Figure 1: Baseline and Graduation Attitudes

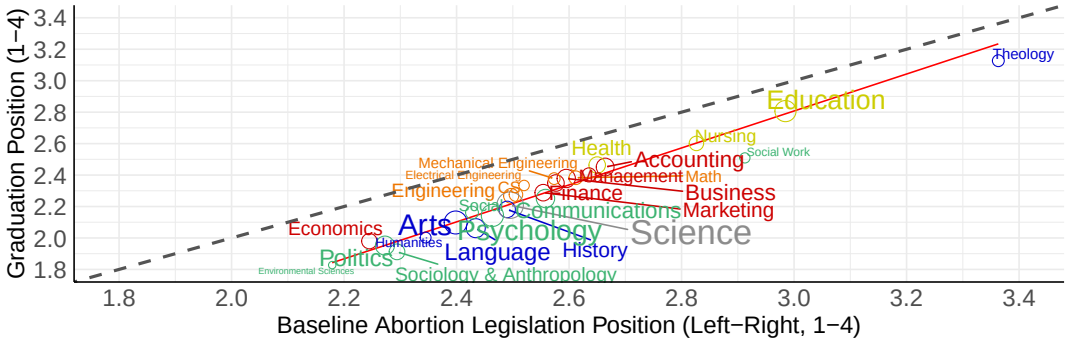
(A) Ideology



(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.

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

Notes: This table reports summary statistics for our study sample. "Baseline" covers baseline-survey respondents who entered college by 2019, 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. South indicates a college in Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia or West Virginia. Income is parents' total income as reported by the student, in nominal dollars. N reports the number of observations with non-missing values.

Coverage and representativeness. 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 reflects the study sample's larger share of Catholic and higher-income students relative to the baseline sample. Differences in intended majors are small. Overall, the study sample provides wide coverage of the American college population.

2 Ideological Effects of Fields of Study

This section estimates the effects of fields of study on students' political ideology. Section 2.1 presents estimates of the effects of college major, and shows that these estimates are robust even to extreme assumptions about remaining selection on unobservables. Sections 2.2 and 2.3 provide additional evidence using alternative identification strategies.

2.1 Baseline Estimates: Effects of College Majors

Let $p_i(m)$ denote student i's potential political ideology at graduation under major m. Let $m_i \in \mathcal{M}$ denote the student's realized major, where $\mathcal{M}$ is the set of k possible majors. 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, comprising two sets of variables.[13] The first set, X_i^p, includes predictors of the outcome, most importantly ideology measured before college. Because this measure is discrete and subject to survey response error, we supplement it with a rich set of baseline policy positions to capture selection on pre-college ideology more fully.

Identification in a value-added model is not straightforward (Rothstein, 2010). In particular, major choice may be correlated not only with ideology but also with latent propensities for ideological change. To help address this concern, we include X_i^m, a set of predictors of major choice. Unobserved predictors of ideological change threaten identification only to the extent that they are also associated with major choice conditional on the included controls—for example, because they shape students' preferences over fields. Controls that capture these preferences may therefore help account for this remaining source of selection (similar in spirit

[13]Following Spiess (2025), who show that discretion over covariate adjustment trades off precision against specification-search bias, we fix ex ante a parsimonious control set guided by our identification argument: predictors of the outcome and predictors of the treatment. We therefore exclude other informative controls, such as demographics and college and year fixed effects, from the main specification and include them only in the robustness analyses discussed below. We enter all controls flexibly, using indicators for each observed value. In unreported analyses, we also verify that the results are robust to interactions among key controls.

to Dale and Krueger, 2002). Accordingly, X_i^m includes intended major, intended careers, and life goals, all of which predict realized major. Table I.4 lists the full set of controls.

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.

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.

Identification. The key identification assumption is that, conditional on our controls, realized major is independent of students' potential ideology at graduation ($\{p_i(m) : m \in \mathcal{M}\} \perp\!\!\!\perp m_i \mid X_i$). Our controls capture the main predictors of both ideology and major choice, reducing the scope for violations of this assumption. Still, two sources of remaining confounding are possible. We discuss them briefly here and summarize the evidence reported below suggesting that neither generates large bias in our setting.

First, pre-existing determinants of ideological change may remain correlated with major choice because the baseline survey omits them or measures them imperfectly. Several exercises suggest that this source of bias is limited. Our controls have substantial explanatory power for both graduation ideology and major choice, and they absorb nearly all of the omitted-variable bias generated by a large set of additional observed characteristics.

Second, shocks occurring after the baseline survey may affect both major choice and political ideology. To address this concern, we use students' intended majors at college entry as instruments for completed majors. This approach isolates variation in completed majors predicted by pre-college choices, rather than variation generated by post-entry shocks.

Alongside our main estimates, we report sensitivity analyses that allow for remaining selection on unobservables from any source, including both forms of confounding. The resulting adjustments yield conservative lower bounds that still imply substantial effects.

Finally, we complement these estimates with two strategies that isolate supply-side variation in exposure to fields. The first uses program-specific persistence rates as instruments for completed major. The second exploits within-college expansions of business and economics in a difference-in-differences design. In both cases, pre-treatment outcomes allow us to assess implications of the key identifying assumption. The first strategy provides an IV estimate of the effect of completed major. The second identifies the reduced-form effect of expanded exposure to business and economics through both major and non-major coursework.

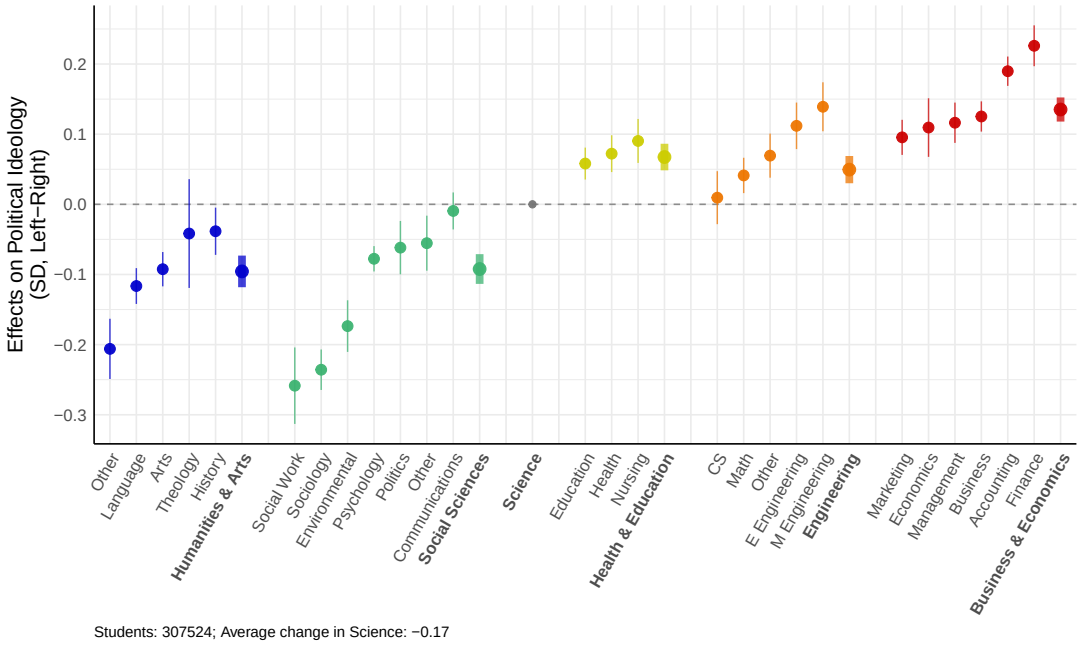
Baseline estimates. Panel A of Figure 2 presents the estimates. Bold lines mark coefficients from a specification that groups majors into six broad categories, while the thinner lines show results from a detailed specification with 27 categories. In both cases, the omitted group is the natural sciences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

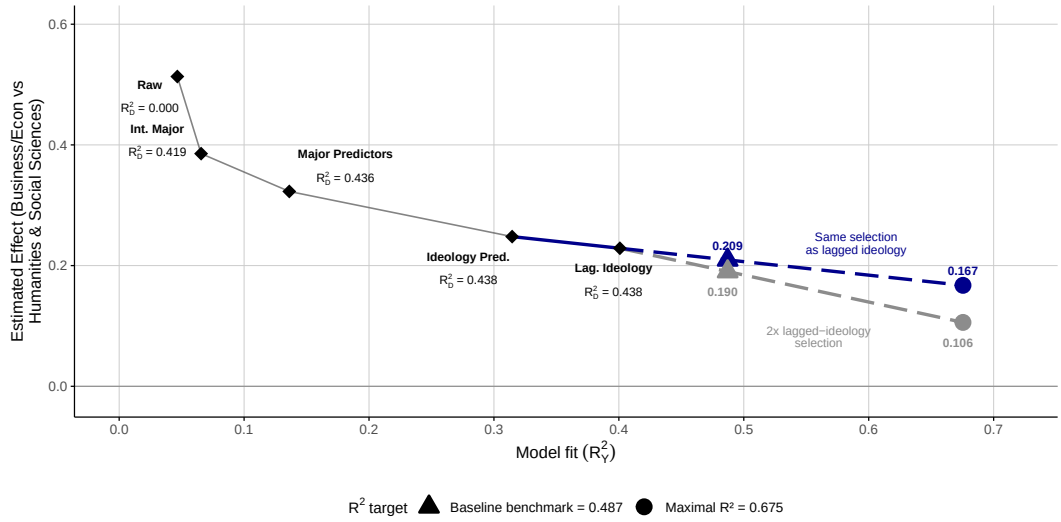
Appendix C reports a broad set of robustness and heterogeneity analyses. The results are insensitive to alternative specifications (Appendix C.1). Conclusions remain similar when we use alternative outcome definitions (binary measures and changes in ideology), add controls, and account for double majors. Sample selection is unlikely to bias our results (Appendix C.2). Additionally, the effects of college majors are not concentrated among particular students, institutions, or cohorts (Appendix C.3). The effects of majors on ideology are broadly similar across gender, socioeconomic background, and high school GPA. The estimates are also similar across institution types, selectivity levels, and geographic regions. Over time, the estimates grow somewhat but remain substantial throughout: after 2010 they are around 0.27 SD, compared with 0.21–0.23 SD earlier.

Figure 2: Effects of College Majors on Political Ideology

(A) Estimates



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



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 pooled humanities and social sciences, with the other fields controlled, under different conservative scenarios (see main text for details).

Assessing selection on unobservables. Because our baseline estimates rely on a selection-on-observables approach, we assess the potential implications of remaining selection on unobservables. Specifically, 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 largest contrast between field groups. Specifically, we re-estimate the main equation using the humanities and social sciences as the omitted reference group.

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, 0.51 SD ($R^2 = 0.05$). Adding intended major and the other treatment predictors reduces it to about 0.32 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 0.21 SD.

A more conservative approach instead derives the ceiling directly. It asks how large $R^2_{\max}$ could possibly become as additional covariates are added, where the ideal control can be thought of as each student's potential outcome in the omitted group. Even this control would leave three sources of irreducible noise: heterogeneous major effects, discretization of the ideology measure, and instability in survey responses. 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 assuming that the unobservables are as strongly correlated with major choice as lagged ideology, the strongest predictor of the outcome, the lower-bound estimate remains about 0.17 SD. This strongly supports the robustness of our main estimates: even under conservative selection on unobservables, the estimated effects remain substantial.

The figure also considers an extreme scenario in which R^2 reaches its maximal ceiling and the remaining unobservables are twice as strongly related to major choice as lagged ideology.

This runs against the pattern we observe: as observed controls explain more of the outcome, their additional explanatory power for major choice shrinks rather than grows. Even under this extreme scenario, the lower-bound estimate remains meaningful at about 0.11 SD.

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.

Additional controls and omitted-variable bias. We further assess the scope of omitted variable bias using the rich additional information in our survey. We use variables excluded from the main specification: demographics and behaviors, and characteristics of students' home ZIP codes.[14] Adding these controls does not change the estimates (Figure C.3). Figure 3 shows that this stability is because our main controls already capture most of the selection they would otherwise carry. For each covariate, it decomposes the implied bias into the product of associations with the outcome (β) and treatment (δ), under four specifications: no controls, outcome/treatment predictors only, and our main specification.

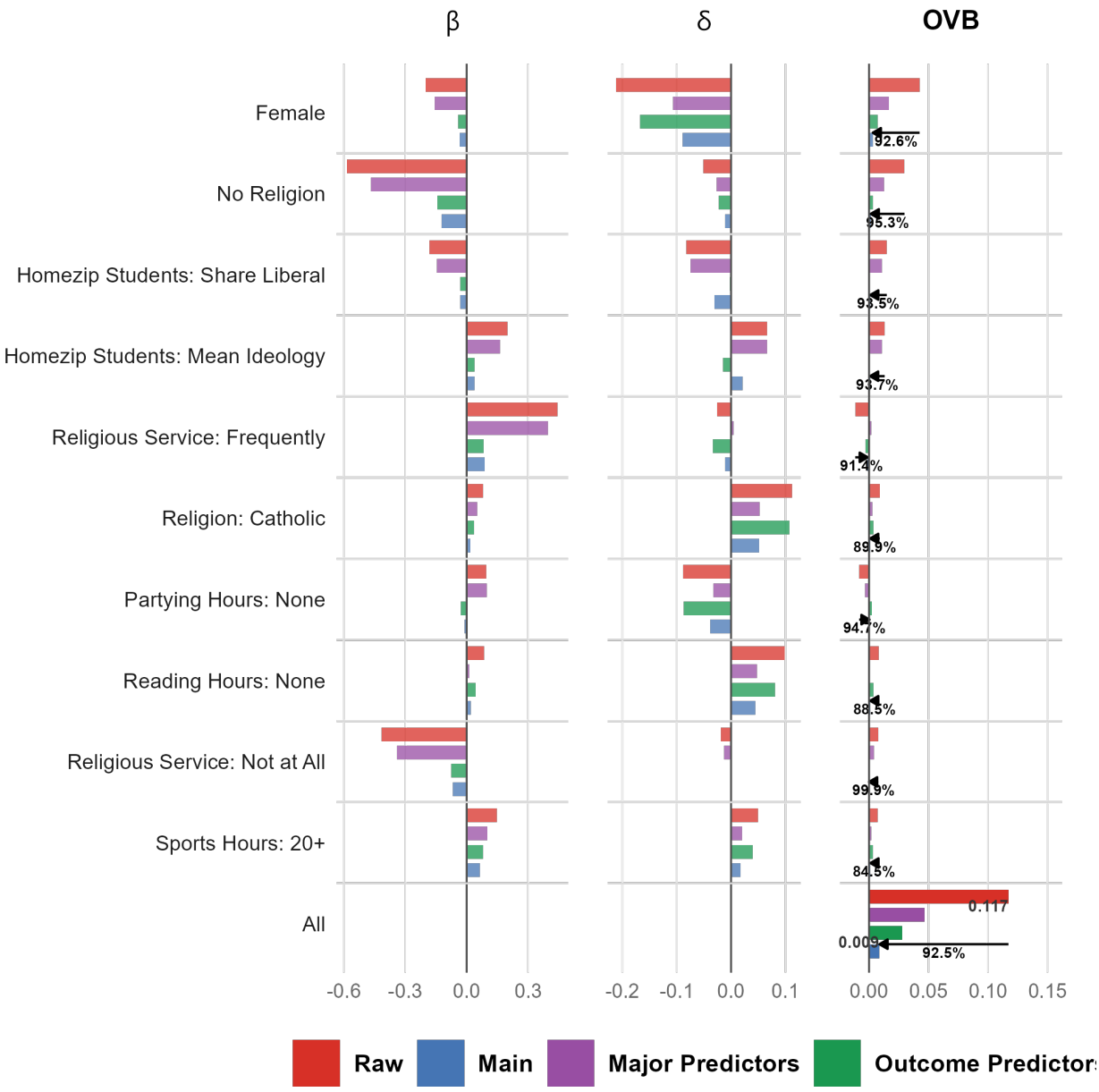
Consider one example: students who report no reading for pleasure are both more right-leaning and far more likely to major in business or economics. In a model with no controls, omitting reading habits would bias the business/economics coefficient by about 0.008 SD; our main specification removes 89 percent of that bias.

More generally, for each covariate that generates meaningful implied bias, our controls remove 84–100 percent of that bias. The bottom row aggregates across all of these covariates. With no controls, they would jointly generate substantial bias, about 0.12 SD. This is how much the business/economics coefficient changes when we add these covariates. 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.

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).

[14] 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.

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 and social sciences as the pooled 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.

2.2 Instrumental Variable Estimates

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 baseline survey that may affect both major choice and ideology.[15] 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.[16] 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,[17] 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 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

[15] 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.

[16] 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 228,000 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).

[17] This is the relevant contrast for the persistence IV: about 85 percent of its compliers would have pursued one of these two fields in the absence of treatment (Panel A of Table C.8).

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

2.3 Quasi-Experimental Evidence

To complement the selection-on-observables analysis, we further implement a quasi-experimental design that exploits sharp increases in enrollment in business and economics programs at specific colleges.[18] 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 $B_{ct} = \frac{N_{ct}^{BE}}{N_{ct}}$.

Our approach is 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). Analogously, we define a business/economics expansion shock as a sharp increase in the business/economics 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

[18] 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.

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 direction.[19]

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) rather than shifts in student demand. 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 in the graduation survey.[20]

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. The terms e_c and s_t denote college and cohort fixed effects. y^0_{ict} denotes the lagged outcome.[21] 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 expand and colleges that do not would have followed similar outcome trends. This assumption is

[19]The 30 percent cutoff is the one used in these studies, which define a mass layoff as a decline of at least 30 percent in a firm's employment, and it has become the standard threshold in this literature (Flaaen et al., 2019; Bertheau et al., 2023). Similar to the pre-layoff stability requirement in Davis and Von Wachter (2011), 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. The results are similar when we modestly vary these cutoffs.

[20]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.

[21]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 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.

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. 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.

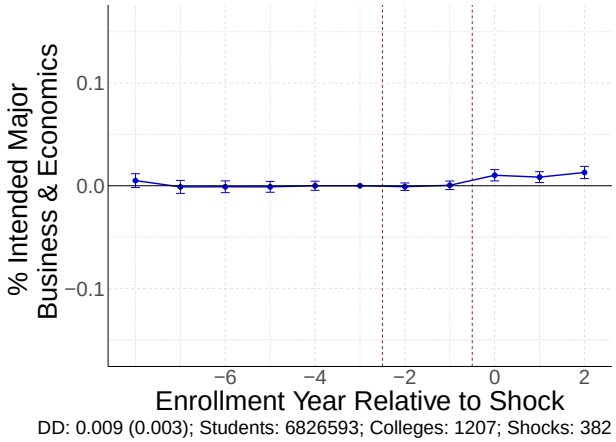
However, the increase in actually majoring in business or economics is even larger. Panel C of Figure 4 shows that following a shock, the probability of majoring in business/economics increases by about 3.2 pp. This effect is statistically significant and indicates a meaningful increase in exposure to these fields. Columns (1)–(5) of Panel A in Table I.5 report the corresponding effects on other major groups. The estimates are negative for several groups, but statistically significant only for engineering. Column (6) reports the effect on the share of students who ever take a course in humanities or arts, the only measure we observe for non-major coursework. Following a shock, students are 2.4 pp less likely to take such a course, suggesting that the expansion affects not only realized majors but also coursework outside the major. Finally, Panel D of Figure 4 shows that graduation ideology shifts to the right by about 0.04 SD, with no pre-trend. The estimate is significant at the 5 percent level.[22]

[22]Columns (1)–(3) of Panel B in Table I.5 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.

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

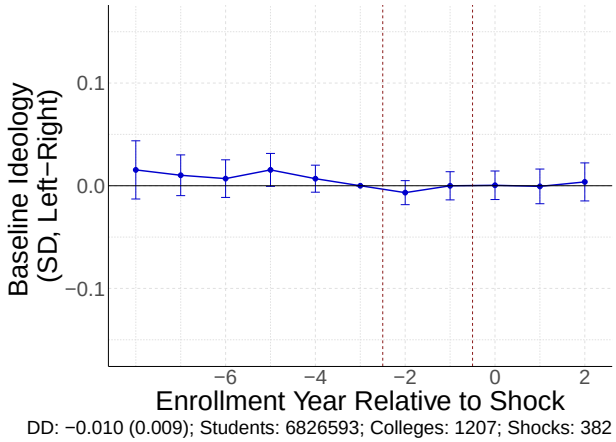
Falsification Tests: Baseline Survey

(A) % Intended Business & Economics



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

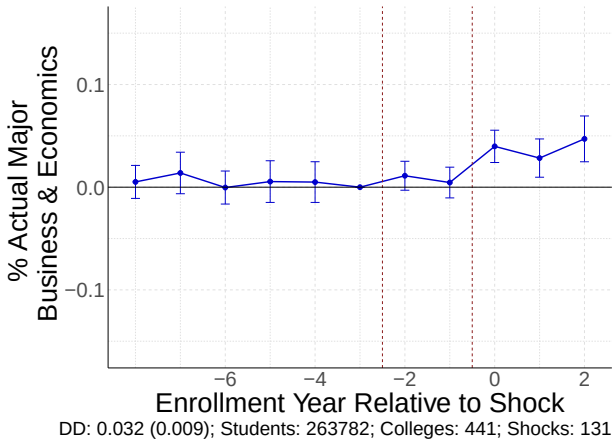
(B) Baseline Ideology (SD)



DD: −0.010 (0.009); Students: 6826593; Colleges: 1207; Shocks: 382

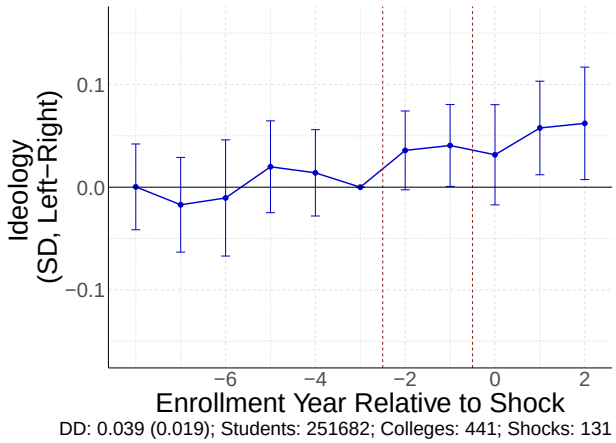
Graduation Survey

(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. Business/economics coefficients are in probability units, so 0.01 corresponds to one percentage point.

As shock timing varies across colleges, our design is a staggered difference-in-differences design. Recent work shows that TWFE estimators 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 of Borusyak et al. (2024). The estimator first uses untreated observations to estimate the fixed effects that would prevail absent treatment, and then estimates treatment effects using outcomes residualized by these predicted fixed effects. Figure I.6 reports the corresponding event-study estimates, which are similar to the TWFE estimates and more precise. The average effect is about 4 percentage points on majoring in business and economics and about 0.04 SD on ideology at graduation.

A natural question is whether the expansions can serve as an instrument for majoring in business or economics. This would imply an effect of roughly 1.2 SD (0.039/0.032), several times larger than the largest contrast in our baseline estimates (0.23 SD) or the IV estimates (0.3 SD), though the ratio is very noisy. We do not interpret the expansions this way because the exclusion restriction is unlikely to hold: expansions also change coursework outside the major. Consistent with this, students are 2.4 pp less likely to take any humanities or arts course after an expansion (Column 6 of Panel A in Table I.5). Although this is our only measure of course-taking, it suggests that exposure changes beyond major choice. Appendix F shows how modest changes in non-major coursework could reconcile the reduced-form effects.

3 Broader Effects and Implications

3.1 Implications for Political Differences

The college divide. 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 2. Panel A focuses on ideological change during college. Students in our sample enter college with an average ideology of 3.00 and graduate with an average of 2.87, indicating a leftward shift of 0.13 points (1–5 scale). However, if all students majored in economics or business, this change would shrink to 0.01.

Consequently, the ideological gap between college graduates and individuals with no college education would narrow substantially. To quantify this, we use the CCES (Cooperative Congressional Election Study).[23] 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

[23]Using the CCES, Figure I.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.

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 0.201. Field specialization therefore accounts for about 36 percent of the observed college–noncollege gap and 47 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 I.2, this implies an approximately 6.3-percentage-point increase in the probability of voting Democratic ($0.8 \times 6.7 + 0.25 \times 3.8 \approx 6.3$).[24]

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 2, differential major selection explains about 76 percent of the 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 B of Table 2, 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).

[24]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 2: 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	2.986	4.0%	89.2%
B. Ideology Dispersion					
(Uniform major)	0.661	0.722	0.694	3.8%	45.6%
C. Gender Gap					
(Uniform major)	0.161	0.203	0.171	15.6%	75.8%
D. College–Noncollege Gap					
(All Business & Economics)	0.075	0.315	0.201	36.2%	47.5%

Notes: Panels A–C report the mean, variance, and male-minus-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 counterfactuals convert the estimated effects of majors, which are in standard deviations of graduation-survey ideology, into scale points using that standard deviation (0.845). The HERI sample contains 293,523 students with ideology observed in both surveys; 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.

3.2 Policy Preferences

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.

We first 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 (1) of Table 3 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.[25]

[25]Column (5) of Table C.8 and Column (4) of Panel B in Table I.5 report estimates for the policy-position index using complementary approaches. The estimates are positive, though noisy in the difference-in-differences specification.

Columns (2) and (3) of Table 3 further report the estimates for two main policy positions, with estimates for the remaining policy positions appearing in Appendix Table I.7. Compared with the natural sciences, the social sciences and humanities shift students toward more left-oriented positions on redistribution, both on government provision of healthcare and (as is shown in Appendix Table I.7) on higher taxation, with effect sizes of about 0.05–0.07 SD. In contrast, business and economics shift students substantially to the right, with an effect of about 0.17 SD relative to the natural sciences.

Effects on cultural issues are generally smaller. For abortion (Column (2) of Table 3), the contrasts between business and economics and the social sciences and humanities are only 0.08 and 0.06 SD, respectively. Although statistically significant, these differences are about one-third as large as those for health care and taxation. Across the cultural issues in Appendix Table I.7, the contrasts are on average about half as large as for economic issues, although they are larger for views on racial discrimination and criminal justice. One possibility is that fields differ more in their treatment of economic questions than of cultural ones. Consistent with the importance of field-specific content, health and education reduce support for legal abortion by 0.11 SD, more than any other field group, possibly because some of these programs engage more directly with the issue. As we show below, faculty in these fields may also be more conservative than in other fields.[26]

3.3 Life Goals and Behaviors

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 (4) of Table 3 presents the results. Economics and business lead students to place substantially greater priority on financial success than other fields, with an estimated effect of 0.32 SD relative to the natural sciences. The effect is also considerably larger than that of other high-return fields, such as engineering. This is also consistent with the large effects shown on career priorities, although those are available only to a small number of cohorts. Appendix Table I.10 shows these results: economics and business lead students to place greater weight on earnings relative to other job attributes, such as meaning or social impact.

[26]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 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 3: Effects of College Majors on Policy Positions, Goals, Behaviors, and Well-being

	Policy Positions			Goals				Behaviors		Well-being	
	(1) Index	(2) Abortion	(3) Healthcare	(4) Well Off	(5) Social Index	(6) Family	(7) Meaning	(8) Demonstrated	(9) Volunteered	(10) Emotional Health	(11) Felt Depressed
Business &	0.13***	-0.03***	-0.17***	0.32***	0.02	0.08***	-0.05***	-0.05***	-0.11***	0.05***	-0.10***
Economics	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Engineering &	0.07***	-0.04***	-0.09***	0.15***	-0.12***	-0.01	-0.07***	-0.05***	-0.22***	-0.03**	-0.03**
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
Health &	0.09***	-0.11***	0.02	0.02*	0.12***	0.10***	0.02	-0.01	0.03*	-0.03***	-0.04***
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Social	-0.12***	0.05***	0.07***	0.07***	0.30***	0.02***	0.03***	0.06***	0.02	-0.04***	0.01
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Humanities &	-0.11***	0.03***	0.05***	-0.03**	0.18***	-0.07***	0.15***	0.08***	-0.11***	-0.06***	0.09***
Arts	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
N	302,403	266,413	157,964	308,891	306,634	308,866	308,487	163,726	108,877	309,147	311,534
Summary Statistics (raw scale)											
Graduation Mean		2.70	2.87	2.77	2.41	3.15	2.71	1.22	1.93	3.65	1.71
Graduation SD		1.17	0.96	0.92	0.66	0.93	1.01	0.47	0.68	0.90	0.62
Change in Omitted Group		0.29	0.08	-0.25	0.07	0.05	0.22	-0.22	-0.30	-0.08	0.09

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. All columns include the main controls (Table I.4); columns (8)-(9) also control for the baseline behaviors listed there, and columns (10)-(11) for baseline emotional health and depression. The Index in column (1) is the standardized prediction of self-reported ideology from students' responses to all policy questions available in their survey year. Columns (2)-(3) 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); and A national health care plan is needed to cover everybody's medical costs (Healthcare). Columns (4)-(7) are stated life goals, originally coded on a 1–4 scale (1=Not important, 2=Somewhat important, 3=Very important, 4=Essential): being very well off financially (Well Off); the social index (Social Index), the equally weighted average of five goals – helping others in difficulty, influencing social values, promoting racial understanding, participating in a community action program, and influencing the political structure; raising a family (Family); and developing a meaningful philosophy of life (Meaning). Columns (8)-(9) are behaviors, both originally coded on a 1–3 scale (1=Not at all, 2=Occasionally, 3=Frequently): participated in organized demonstrations (Demonstrated); and performed volunteer or community service work (Volunteered). Columns (10)-(11) are well-being measures: self-rated emotional health relative to peers (Emotional Health), originally coded on a 1–5 scale (1=Lowest 10%, 5=Highest 10%), and felt depressed during the past year (Felt Depressed), 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; they are blank for the policy-position index, which has no raw scale. "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 the policy-position index, whose baseline and graduation versions are each standardized within their own survey population. Estimates for the remaining policy positions, life goals, and behaviors appear in Appendix Tables I.7, I.8 and I.9. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

We next construct a social-goals index capturing goals such as helping others and contributing to social change. Economics and business do not significantly affect this index relative to the natural sciences. In contrast, the social sciences and humanities substantially increase the importance students place on these goals, with estimated effects ranging from 0.18 to 0.30 SD. Effects on the components of this index are reported in Appendix Table I.8.

We also find meaningful effects on other life goals, including raising a family and developing a meaningful philosophy of life. For example, economics and business increase the importance placed on raising a family by 0.08 SD but reduce the importance placed on developing a meaningful philosophy of life by 0.05 SD. Humanities show the opposite pattern, reducing the former by 0.07 SD while increasing the latter by 0.15 SD.

We then analyze behaviors, reported in columns (8)–(9) of Table 3. Compared to natural sciences majors, students in the social sciences and the humanities and arts are more likely to participate in demonstrations, consistent with greater 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 I.9). 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, variation is limited because roughly 90 percent of students in the sample report being registered to vote.

Volunteering follows a different pattern. Business and economics, engineering and mathematics, and the humanities and arts reduce volunteering relative to the natural sciences by 0.11, 0.22, and 0.11 SD, respectively, while the estimate for the social sciences is positive but small and insignificant. Appendix Table I.11 also reports effects on time use. There, the social sciences have a large and significant positive effect on time spent volunteering.

3.4 Well-Being

Finally, we extend our analysis to students' well-being, in columns (10) and (11) of Table 3. Relative to natural sciences majors, business and economics students report better emotional health (0.05 SD) and less frequent depression (−0.10 SD), whereas humanities and arts students report worse emotional health (−0.06 SD) and more frequent depression (0.09 SD).

These patterns echo a well-documented association between political ideology and self-reported well-being: conservatives report greater happiness than liberals, a gap often attributed to beliefs that the social system is fair and that effort is rewarded (Napier and Jost, 2008). At the same time, the correspondence with our ideology results is only partial: the

social sciences shift students to the left as much as the humanities do but have no detectable effect on depression, and health and education, which shift students slightly to the right, reduce self-rated emotional health. We therefore view these results as suggestive evidence that fields shape how students evaluate their own lives.

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: instructional environment, peer socialization, and earnings expectations. As a starting point, Figure 5 shows 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 instructional 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 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 each major's peer environment. Students share classes and spend time with others who choose the same major, creating scope for peer influence. We measure each major's baseline political composition as the average ideology at college entry among students who ultimately complete that major, pooling across colleges.[27]

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, part-time work and job offers may further inform their expectations. Expected earnings may therefore shape political attitudes through self-interest: students in higher-earning fields, such as engineering and economics, may be more likely to support parties and policies that benefit them financially, such as lower income taxes.

[27]The peer measure and the estimated major effects $\hat{\delta}_m$ are constructed from overlapping samples of HERI respondents. This does not generate a mechanical correlation: $\hat{\delta}_m$ captures changes in ideology from college entry to graduation, controlling for individual baseline ideology, whereas the peer measure is based only on baseline ideology.

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.[28]

We then average this measure across 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 Scorecard, yielding 303,363 students across 441 colleges, aggregated into 27 detailed major groups.[29]

All three candidate mechanisms are positively associated with the estimated major effects, but faculty ideology fits them far better. Figure 5 shows this measure accounts for about 86 percent of the variation in $\hat{\delta}_m$, against 57 percent for peers' baseline ideology and 61 percent for expected earnings.

Theology illustrates the gap: it attracts the most right-leaning entrants of any major, so the peer measure predicts the largest rightward shift of all, yet its estimated effect is, if anything, leftward. Its faculty also lean left, about as much as the average faculty respondent, and it sits almost exactly on the faculty ideology line. Social work is similar, though less exact: entrants near the sample average lead the peer measure to predict no shift, while faculty ideology captures much of its strong leftward effect. The earnings measure fails at its extremes, predicting a clear rightward shift for computer science, the highest-earning major, whose effect is essentially zero, and a leftward shift for low-earning education, which instead shifts students rightward. Both again sit far closer to the faculty ideology line, education almost exactly on it.

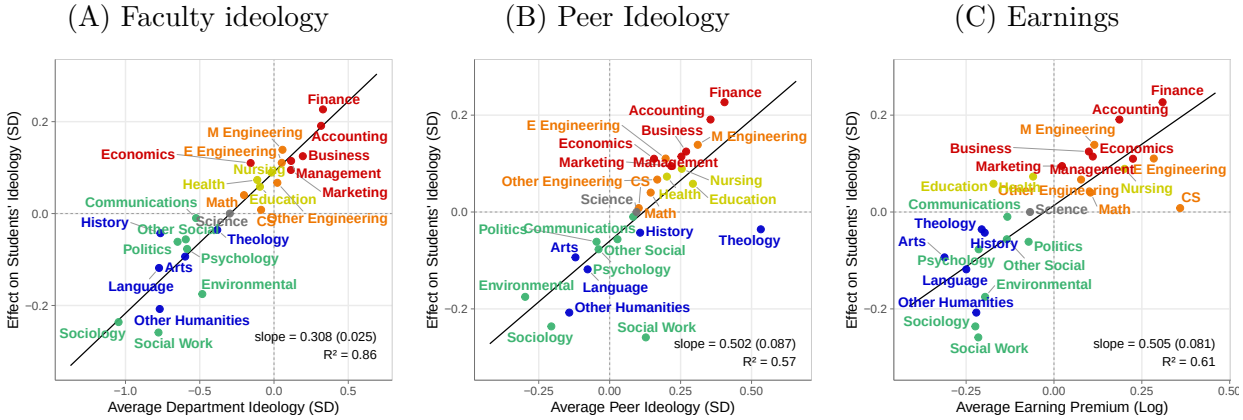
The contrast becomes even sharper in a joint specification. Adding peers' ideology and the earnings premium alongside faculty ideology raises the explained variation only marginally relative to the faculty-only specification ($R^2 = 0.88$ versus 0.86). At the same time, the partial associations of peers and earnings with $\hat{\delta}_m$ fall by roughly three-quarters or more, to 0.12 and 0.08, respectively. The peer coefficient is only marginally significant, while

[28]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 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.

[29]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.

the earnings coefficient is statistically insignificant. By contrast, the coefficient on faculty ideology remains large (0.23) and highly significant at the 1 percent level. Thus, conditional on the political composition of a major's faculty, neither peers' baseline ideology nor expected post-graduation earnings adds meaningful explanatory power for the cross-major pattern of estimated effects.

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:* Faculty 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. Faculty and peer ideology are measured, like $\hat{\delta}_m$, in standard deviations of graduating students' ideology. Zero is the average graduating student. *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.

4.2 Assessing Potential Mechanisms

These descriptive patterns motivate the more rigorous analyses that follow. In this subsection, we estimate the effect of each mechanism separately, exploiting variation within majors in each characteristic, which allows for more credible identification. Consistent with the descriptive evidence in the previous subsection, we find that faculty ideology meaningfully affects students' ideology, whereas peer ideology and earnings expectations do not.

Two measurement issues arise for the peer and faculty ideology proxies. First, we assign both by students' intended rather than realized major. Realized major is chosen during college and may itself be shaped by the peers and faculty a student encounters, so measures based on it would condition on an endogenous outcome. Intended major is reported at college entry, before these influences operate. It is also observed for every respondent in the baseline survey, which allows us to construct the peer measure from a much larger sample.

Second, both proxies are measured with 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 H, we adjust each cell's measure using two inputs: the coverage of each cohort or faculty, derived by combining our data with administrative data,[30] and the reliability, which measures how much the observed members of a cell reveal about its unobserved members. For faculty, we estimate the reliability with a split-half design; for peers, our identifying assumption implies that it is zero. The adjusted treatment combines the observed average with the cell's predicted composition, placing more weight on the observed average the higher the coverage. When coverage is complete, the adjusted treatment equals the observed average; as coverage falls, the weight declines toward the reliability, increasingly shrinking the observed deviation from the predicted composition and 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 G 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.

[30] Coverage is observed at the aggregate level rather than for each cell: IPEDS reports first-year enrollment by institution and year, not by intended major, so peer coverage is the surveyed share of the entering cohort, assumed uniform across the cohort's majors. Faculty coverage is analogous, with the college's instructional staff as the denominator. A cell's own coverage may differ from the aggregate; Appendix H.5.2 formalizes this gap and shows that it is innocuous unless it is related to the cell's views.

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.[31] 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 I.8. 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.[32]

Instructional 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 faculty ideology 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 I.9 shows time trends in this measure by major group. Faculty

[31]The 95 percent confidence interval in the full specification (Table G.2) caps the peer effect at 0.010 SD per SD of peer ideology. Switching from social sciences or humanities to business or economics changes peers' average baseline ideology by 0.359 SD, implying an effect of at most $0.010 \times 0.359 \approx 0.00359$ SD. Since only 37 percent of same-intended-major pairs share a realized major, we conservatively scale this to $0.00359/0.37 \approx 0.01$ SD, about 4 percent of the 0.23 SD gap between the fields (Appendix G).

[32]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 I.7 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.

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 faculty 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.[33] We then estimate the following specification:

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

where p_{icmt} is the graduation political ideology of student i at college c with intended major m and starting their studies at year t; F_{cm} is the average faculty ideology (in SD of graduating students' ideology) 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 and for the reliability of the surveyed faculty mean (Appendix H).

We assume that students' potential ideology is orthogonal to faculty ideology, conditional on controls. Under this assumption, the coefficient 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 faculty 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

[33] In unreported results, the estimates are larger when matching students based on their realized majors.

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 faculty ideology, conditional on the fixed effects included in Equation 3. Table I.6 reports both unadjusted and adjusted estimates. We focus on the adjusted estimates, which are less sensitive to attenuation bias. We use three samples: the full baseline survey, cohorts observed in the graduation survey, and students included in the graduation survey. The raw correlations between faculty ideology and students' ideology are large in all three samples, but these correlations largely reflect selection into similar programs. Once we include the fixed effects and predictors of major choice, the correlations become much smaller.

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

Table 4 shows that faculty ideology has a meaningful effect. With the fixed effects alone, the adjusted estimate is 0.031 SD (column 1): students who study the same major in a department whose faculty are one SD (of graduating students' ideology) further to the left become about 0.03 SD more left-leaning by graduation. Adding the predictors of major choice and of the outcome leaves the estimate essentially unchanged, at 0.032 and 0.033 SD (columns 2–3). Column 4 turns the specification into a horse race by adding the two competing mechanisms, peer ideology and expected earnings, measured as in the analyses above. The faculty effect remains positive, at 0.025 SD, though significant only at the 10 percent level, while the peer and earnings coefficients are statistically insignificant. This reinforces the earlier evidence that neither peer composition nor earnings expectations drives the ideological effects of fields of study.[34]

[34]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 (Appendix H); 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.

Table 4: Effects of Faculty Ideology on Students' Ideology

Panel A. Various Specifications, All Majors

	(1)	(2)	(3)	(4)	(5)	(6)
Estimate (Unadjusted)	0.018*	0.020*	0.020**	0.016*	0.023**	0.027***
	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)
Estimate (Adjusted)	0.031**	0.032**	0.033**	0.025*	0.035**	0.038***
	(0.015)	(0.015)	(0.013)	(0.014)	(0.014)	(0.014)
Peers (Adjusted)	–	–	–	0.022	–	–
				(0.027)		
Expected earnings	–	–	–	-0.004	–	–
				(0.006)		
Reliability ($\hat{\rho}$)	0.43	0.43	0.43	0.43	0.43	0.50
Mean coverage ($\bar{\alpha}$)	0.41	0.41	0.41	0.41	0.43	0.47
Mean weight ($\bar{k}$)	0.67	0.67	0.67	0.67	0.68	0.74
N	164,291	164,291	164,291	164,291	154,481	123,482
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	0.2	0.3

Panel B. By Major-Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Business & Economics	Engineering & Math	Health & Education	Humanities & Arts	Natural Sciences	Social Sciences	Hum., Social & Business
Estimate (Unadjusted)	0.024	-0.008	0.005	0.055***	-0.049	0.043***	0.034***
	(0.018)	(0.017)	(0.021)	(0.021)	(0.032)	(0.016)	(0.012)
Estimate (Adjusted)	0.044	-0.013	0.010	0.073***	-0.074	0.059***	0.050***
	(0.035)	(0.028)	(0.031)	(0.027)	(0.058)	(0.020)	(0.017)
Peers (Adjusted)	–	–	–	–	–	–	0.034
							(0.034)
Expected earnings	–	–	–	–	–	–	0.002
							(0.008)
Reliability ($\hat{\rho}$)	0.19	0.39	0.45	0.58	0.18	0.59	0.46
Mean coverage ($\bar{\alpha}$)	0.38	0.35	0.45	0.45	0.42	0.41	0.42
Mean weight ($\bar{k}$)	0.50	0.60	0.70	0.77	0.53	0.76	0.68
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: The table shows estimates of τ from equation 3, representing the effect of faculty ideology on students' graduation ideology. The treatment is the mean ideology of surveyed faculty in the department matching the student's college and intended major. Both are in SD units of graduation-survey ideology (mean 2.91, SD 0.84 on the 1–5 scale; higher values are more conservative). Adjusted estimates are preferred: they correct for the measurement error that arises when only part of a department's faculty is surveyed. The correction uses the coverage- and reliability-adjusted measure of Appendix H; the table reports its split-half reliability $\hat{\rho}$, mean coverage $\bar{\alpha}$, and mean weight $\bar{k}$. Fixed effects are major-by-cohort and college-by-major-group-by-cohort; major predictors are intended major, 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 covered by the faculty survey. Column (4) of Panel A and column (7) of Panel B add peer ideology and expected earnings, defined in Section 4, to the column (3) specification. Panel B estimates the column (3) specification by intended-major group; column (7) pools the humanities, social sciences, and business and economics. The sample is restricted to students with non-missing intended major and graduation ideology whose department has at least 3 surveyed faculty and whose college meets the Min College Coverage threshold. Standard errors clustered by college in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

The estimates are also robust to restricting the sample to colleges where the faculty survey covers a larger share of instructional staff: with minimum coverage of 20 and 30 percent, the adjusted estimates are 0.035 and 0.038 SD (columns 5–6), both statistically significant.[35]

Panel B of Table 4 reports results by intended-major group. In the humanities and social sciences, studying the same major in a department whose faculty are one SD further to the left shifts students' graduation ideology leftward by about 0.073 and 0.059 SD, respectively, both significant at the 1 percent level. The business and economics estimate is also positive, at 0.044 SD, but imprecise, and the estimates for the other field groups are statistically insignificant. Column 7 pools the three field groups that drive the main effects (the humanities, social sciences, and business and economics) and adds the peer and earnings controls of column 4. The effect is 0.050 SD and significant at the 1 percent level, while the peer and earnings coefficients remain insignificant, again favoring faculty over the two competing mechanisms.

To gauge magnitudes, we extrapolate this within-major effect to the between-major gap in faculty ideology. On average, students who intended to major in business or economics are assigned departments whose faculty are about 0.83 SD more conservative than those assigned to students who intended to major in the social sciences or humanities. Combined with the pooled estimate in column 7 of Panel B, this gap implies a rightward shift of about 0.042 SD (0.83×0.050) in graduation ideology. This corresponds to about a fifth of the 0.23 SD relative effects of moving between these majors.[36]

This is also substantially larger than the upper bound on peers' potential contribution, which is about 4 percent, as described above. Moreover, this calculation likely understates the role of the instructional environment. The adjusted measure accounts for incomplete coverage and for the reliability of surveyed faculty means, but coverage is measured only at the college level, and its denominator excludes part-time instructors, which overstates coverage and shrinks the estimate. In addition, the treatment assigns each student the faculty of the intended department, although some students graduate in a different field and some of their coursework is taught by other departments, which further attenuates the estimate.

Together, the evidence does not support peer socialization or material self-interest as the main explanations for ideological effects of fields. Instead, the results point to the instructional

[35]The unadjusted estimates rise proportionally more with coverage, from 0.020 to 0.023 and 0.027 SD, as expected if incomplete coverage attenuates them; the adjusted estimates are more stable. The remaining sensitivity is consistent with error in the coverage measure: its denominator, the IPEDS count of full-time instructional staff, is observed at the college rather than the department level, excludes part-time instructors, and is interpolated for 2000 and 2001. Because less adjustment is required when coverage is high, this error leaves the most residual attenuation in the low-coverage samples.

[36]Moreover, as shown in Figure I.9, 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.

environment (what is taught, and by whom) as the most likely channel: students become more left-leaning when they study the same major in departments whose faculty lean further left, and faculty ideology closely tracks the estimated effects across majors.

5 Conclusion

This paper shows that what students study in college shapes their political and social preferences. Using panel surveys that follow roughly 300,000 American students from college entry to graduation, we find that, relative to the natural sciences, the humanities and social sciences shift students about 0.09 SD to the left, while business and economics shift them about 0.14 SD to the right. Extensive evidence supports the causal interpretation.

These effects have implications for broader politics. In electoral terms, the contrast between business and economics and the humanities and social sciences corresponds to a shift of about 6 percentage points in the probability of voting Democratic, comparable to growing up for several years in a substantially more Democratic place. They also explain a substantial part of the gap between college graduates and individuals with no college education. Interestingly, because students tend to choose fields that align with their prior ideology, major effects widen ideological differences among graduates, accounting for almost half of the increase in ideological variance during college. Women's greater tendency to choose left-leaning fields accounts for most of the growth in the ideological gender gap during college.

The effects are not confined to political labels. They extend to policy positions, most strongly on economic issues such as health care and taxation, rather than on the cultural issues that dominate public debate over higher education. They also extend to life goals and behavior. Business and economics raise the priority students place on their own financial success, while the humanities and social sciences strengthen social goals and political engagement. Fields thus shape what students want, with potential consequences for later outcomes, including occupational choice and earnings on one hand, and political engagement on the other.

The evidence on mechanisms points to the instructional environment. Across majors, faculty ideology closely tracks the estimated effects, whereas peer composition and expected earnings add little explanatory power. Students who study the same major in departments whose faculty lean further left graduate further to the left. Our data cannot separate individual instructors from curricular content and teaching practices. However, together with evidence that instructor partisanship within a given course has little effect (Baum et al., 2026) and that fields differ markedly in the political content of their courses (Light et al., 2026), the results suggest that what fields teach is a central channel.

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Appendix A Data Appendix

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

A.1 Study Sample Construction

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

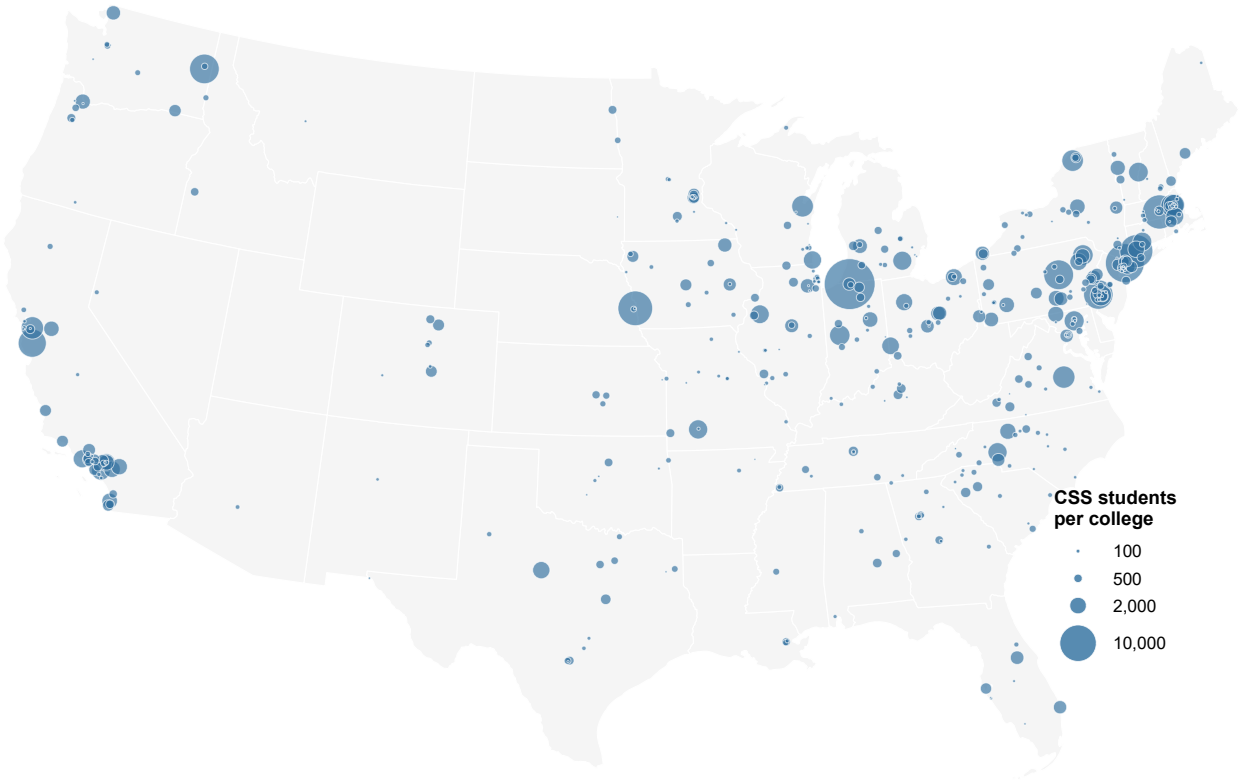
We match students to their home areas using the ZIP code reported in the freshman survey, available for about 90 percent of the analysis sample (the 1994 and 2014 entry cohorts did not retain ZIP codes). ZIP-level income, education, poverty, home values, racial composition, and homeownership come from 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



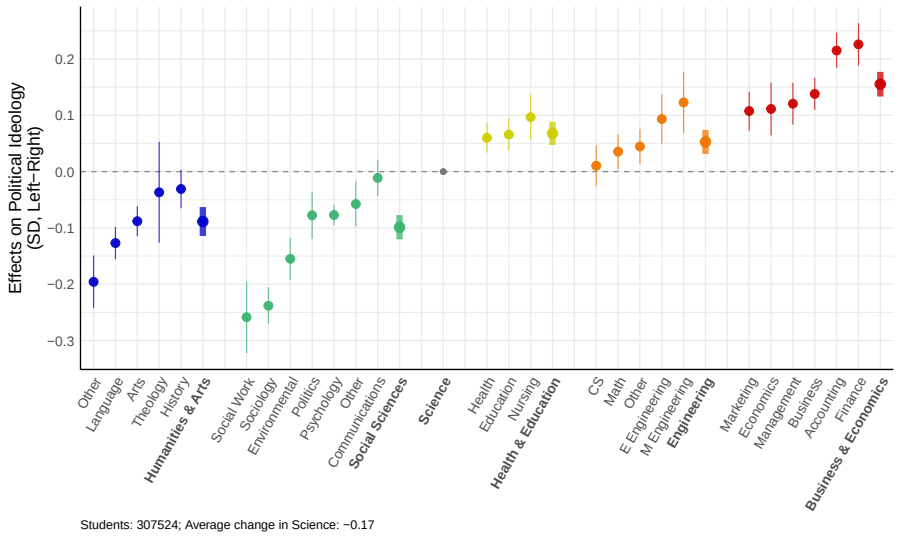
Note: Each point is a four-year college in the linked CSS analysis sample, and the area of each point is proportional to the number of CSS students from that college. The sample spans 451 colleges across 45 states and the District of Columbia, covering 317,161 students. The map shows the contiguous U.S., 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.1 presents the baseline estimates using alternative ideological outcomes: identification as middle-of-the-road (Panel A), conservative or far right (Panel B), and liberal or far left (Panel C). Relative to the natural sciences, economics and business majors are 1 pp more likely to identify as middle-of-the-road, while humanities and social sciences majors are 2 pp less likely to do so. Humanities and social sciences majors are also 2 pp less likely to identify as conservative or far right, whereas economics and business majors are 5 pp more likely to do so. Finally, humanities and social sciences majors are 4 pp more likely to identify as liberal or far left, whereas economics and business majors are 6 pp less likely to do so.

Additional controls. As an additional robustness test, we add controls that may correlate with unobservables, including time use, demographics, and college and year fixed effects.[37] Figure C.3 reports the estimated coefficients from specifications that sequentially add the controls listed in Table I.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.[38]

[37] 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.

[38] The table also shows that this holds for the policy-position index that is defined in Section 3.

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

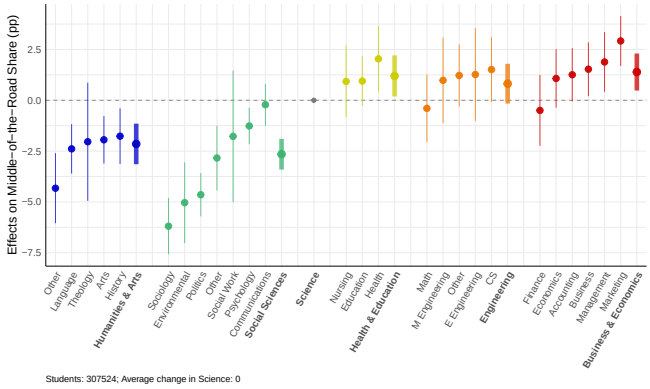
	Ideology		Index	
Second-Major Controls	No	Yes	No	Yes
Business &	0.17***	0.17***	0.15***	0.15***
Economics	(0.01)	(0.01)	(0.01)	(0.01)
Engineering &	0.04***	0.03**	0.04**	0.04**
Math	(0.01)	(0.01)	(0.02)	(0.02)
Health &	0.07***	0.07***	0.08***	0.08***
Education	(0.02)	(0.02)	(0.02)	(0.02)
Humanities &	-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	117,764	117,764	114,674	114,674

Notes: Estimates of δ_m from equation 1 with the main controls (Table I.4); the omitted group is the natural sciences. The sample is cohorts entering in 2005 or later, when second majors are recorded. Columns with Second-Major Controls add indicators for the student's second-major group. Ideology is in SD units of graduation-survey ideology (mean 2.91, SD 0.84 on the 1–5 scale; higher values are more conservative); the policy-position index (Table 3, column (1)) is in its own SD units. Standard errors clustered by college in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

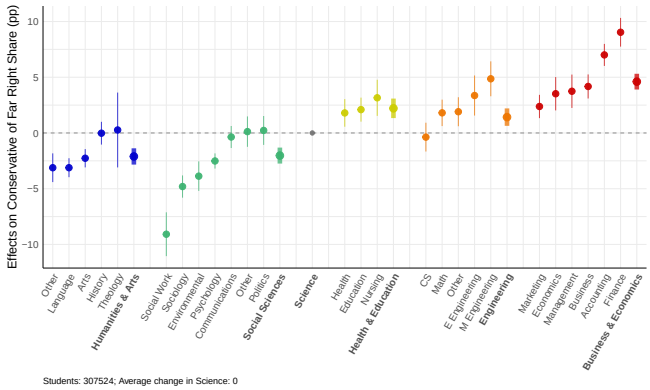
We also explore how the effects of first majors interact with students' second majors by estimating a version of Equation 1 that includes indicators for each major combination, with single-major in natural sciences as the reference group. Figure C.2 presents these estimates. The results reveal that the 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 SD shift toward conservatism.

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

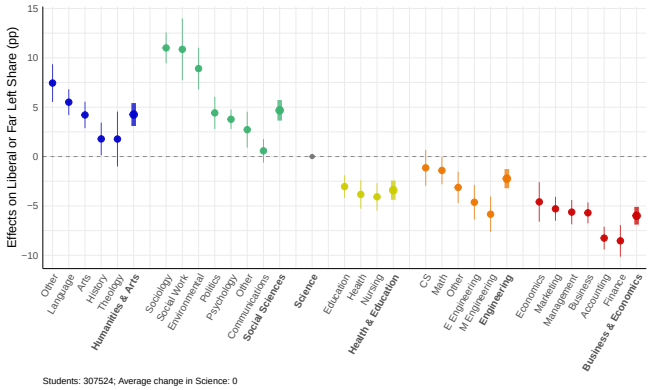
(A) Share Middle of the Road



(B) Share Far Right or Conservative

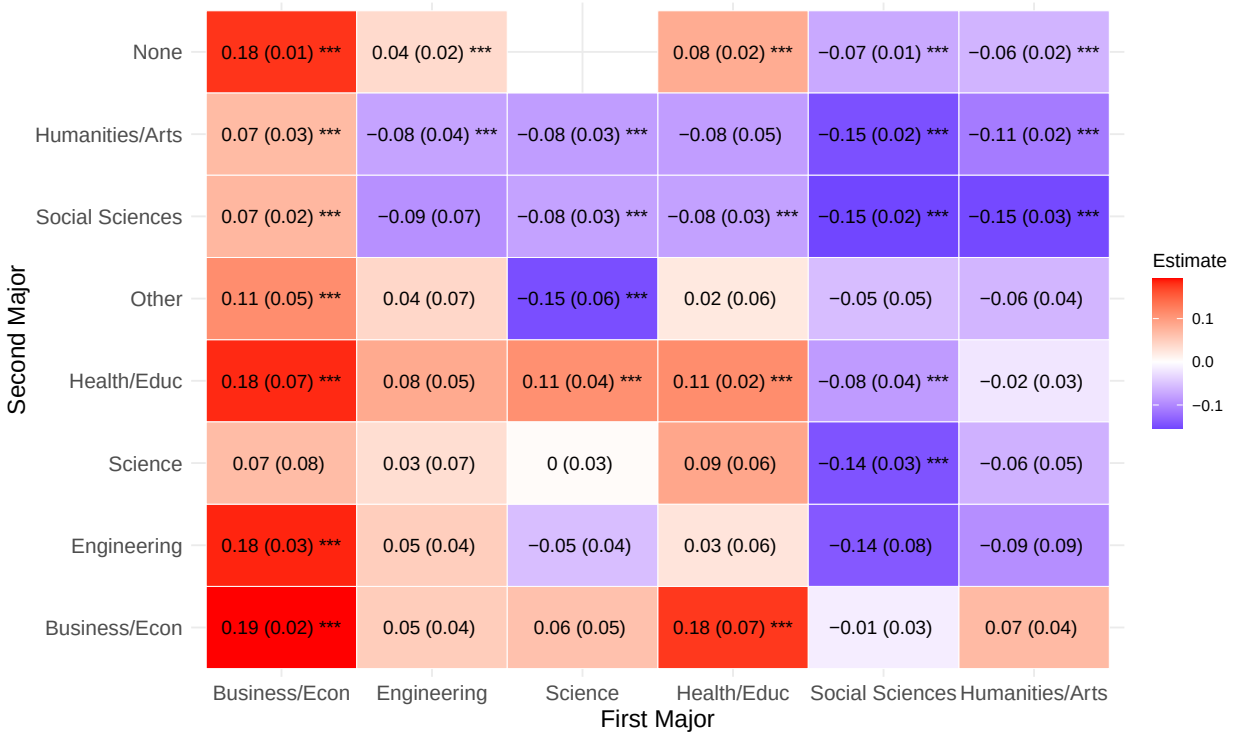


(C) Share Far Left or Liberal



Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals. Bold lines mark coefficients from a specification that groups majors into six 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 I.4). The sample includes only students with non-missing values in the major and outcome. Standard errors are clustered at the college level.

Figure C.2: Effects of College Majors and Double Majors on Students' Ideology



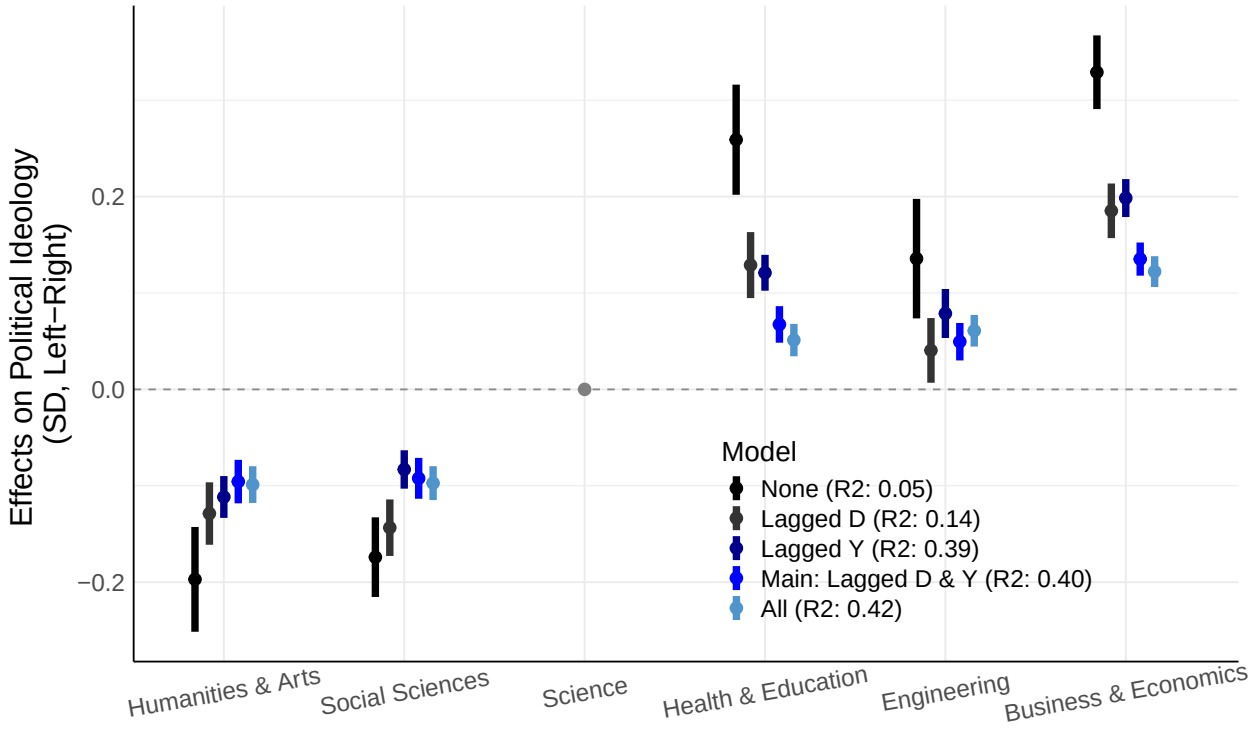
Note: This figure plots the estimates for δ_m from equation 1 and their clustered standard errors from a specification with interaction between first and second majors. The estimated coefficients represent the effects of studying each combination of majors relative to the natural sciences with no second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

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 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)

Figure C.3: Robustness to Including Additional Control Variables

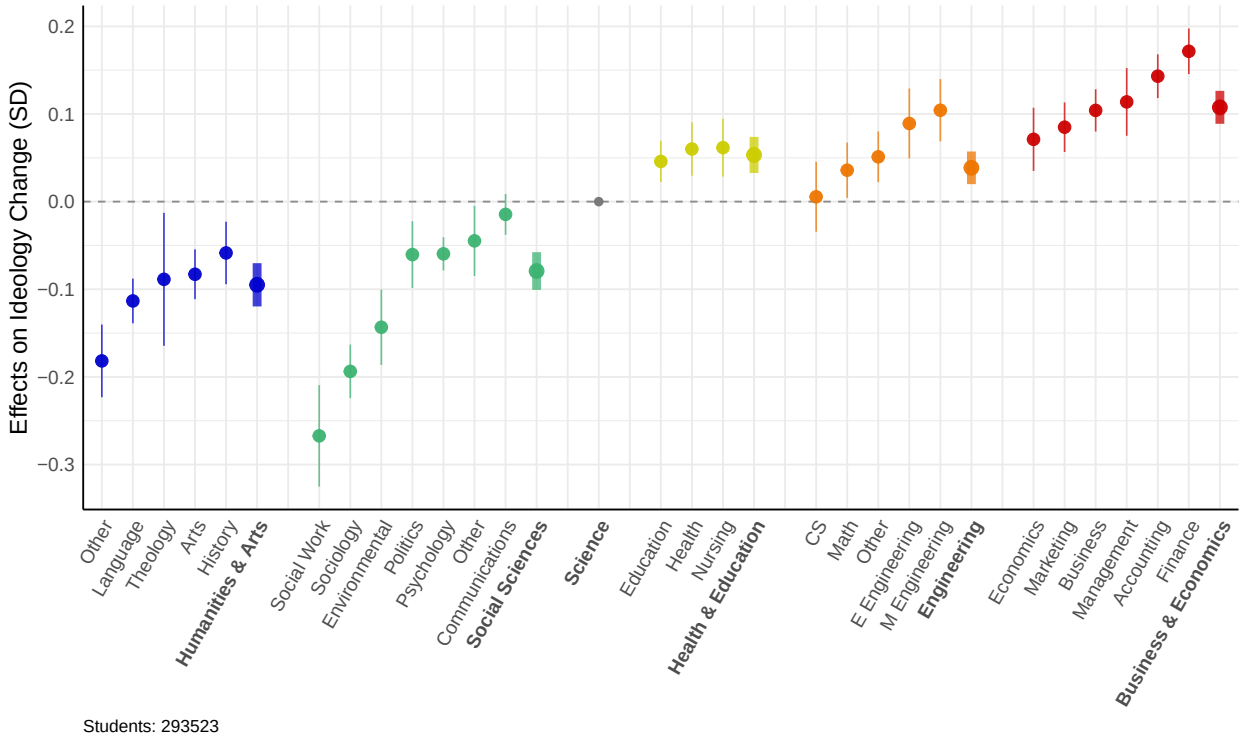


Note: This figure plots estimates of δ_m from equation 1 as control sets are added sequentially. Coefficients represent the effects of each major group relative to the natural sciences. The outcome is graduation political ideology, measured in SD. The specifications correspond to: no controls (None);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

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.[39]

[39]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 it is small in every group. Specifically, moving one SD to the right changes the probability of appearing in the graduation survey by at most about 0.3 pp in any intended-major group, against a response rate of about 15 percent. The relationship is slightly positive in most groups and close to zero among intended humanities and social science majors. This makes it unlikely that the small average attrition gaps in column (4) are masking substantial differential selection by political type.

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.

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.[40] 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.

[40]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***	-0.008***
	(0.009)	(0.002)	(0.002)	(0.002)	(0.002)
Humanities & Arts	-0.006	-0.016***	-0.008***	-0.008***	-0.008***
	(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					0.001*
					(0.001)
Ideology × Business & Economics					0.002*
					(0.001)
Ideology × Engineering & Math					0.002
					(0.001)
Ideology × Health & Education					0.002*
					(0.001)
Ideology × Humanities & Arts					-0.001
					(0.001)
Ideology × Social Sciences					-0.002**
					(0.001)
N	2,113,783	2,113,783	2,113,783	2,113,783	1,972,375
College-Year FE		✓	✓	✓	✓
High-School GPA			✓	✓	✓
All Controls				✓	✓

Notes: This table reports regressions of an indicator for appearing in the graduation survey with a valid major and graduation ideology; coefficients are in probability units. 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. In column (5), baseline ideology is the five-point self-placement (1 = far left, 5 = far right) minus the mean (2.91) and divided by the standard deviation (0.84) of the 720,800 graduation-survey responses, so its coefficients are per standard deviation of graduating students' ideology. Column (5) excludes the 141,408 students without a baseline ideology answer. "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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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

	Political Ideology (SD)		Liberal or 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 & Math	0.049***	0.070***	-2.24***	-2.85***
	(0.010)	(0.017)	(0.50)	(0.85)
Health & Education	0.067***	0.074***	-3.41***	-3.46***
	(0.010)	(0.018)	(0.50)	(0.77)
Humanities & Arts	-0.096***	-0.088***	4.25***	4.14***
	(0.012)	(0.016)	(0.59)	(0.71)
Social Sciences	-0.092***	-0.090***	4.68***	5.00***
	(0.011)	(0.016)	(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. Liberal or Far Left is an indicator for identifying as liberal or far left. Standard errors clustered at the college level are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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***	0.14***	0.11***	0.16***	0.12***	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***	0.05***
Math	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Health &	0.07***	0.06***	0.05***	0.09***	0.06***	0.08***
Education	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
Social	-0.10***	-0.07***	-0.11***	-0.07***	-0.09***	-0.12***
Sciences	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Humanities &	-0.10***	-0.08***	-0.11***	-0.08***	-0.09***	-0.11***
Arts	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
N	191,879	115,189	174,177	101,970	196,449	108,016
Outcome: Liberal or Far Left						
Business &	-5.69***	-6.39***	-5.27***	-6.90***	-5.42***	-6.94***
Economics	(0.49)	(0.62)	(0.57)	(0.53)	(0.52)	(0.68)
Engineering &	-1.42**	-2.76***	-2.23***	-3.03***	-2.78***	-1.85***
Math	(0.67)	(0.69)	(0.58)	(0.81)	(0.67)	(0.66)
Health &	-3.33***	-3.02***	-2.75***	-4.39***	-3.01***	-3.78***
Education	(0.51)	(0.81)	(0.56)	(0.70)	(0.59)	(0.68)
Social	5.05***	3.05***	5.35***	3.59***	4.63***	5.77***
Sciences	(0.50)	(0.80)	(0.55)	(0.71)	(0.58)	(0.77)
Humanities &	4.75***	3.05***	4.99***	3.03***	4.24***	4.95***
Arts	(0.54)	(0.88)	(0.62)	(0.77)	(0.68)	(0.62)
N	191,879	115,189	174,177	101,970	196,449	108,016

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. The outcomes are self-reported political ideology, in standard deviations of graduation-survey ideology (higher values are more conservative), and an indicator for identifying as liberal or far left (multiplied by 100). Low and High SES are students whose parents' reported income is below 100,000 dollars and 100,000 dollars or more, respectively; High HSGPA is a high-school GPA of A or A+, and Low HSGPA is any lower GPA. Students missing the relevant characteristic are excluded. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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***	0.13***	0.16***	0.13***	0.13***	0.15***
Economics	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Engineering &	0.04***	0.05***	0.07***	0.06***	0.06***	0.05***	0.08***
Math	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)
Health &	0.08***	0.04**	0.04	0.07***	0.06***	0.07***	0.10***
Education	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
Social	-0.08***	-0.12***	-0.11***	-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***	-0.14***	-0.13***	-0.09***	-0.10***	-0.10***	-0.08***
Arts	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
N	212,438	59,436	35,204	66,836	239,205	269,521	38,003
Outcome: Liberal or Far Left							
Business &	-6.12***	-5.60***	-5.57***	-7.63***	-5.49***	-6.06***	-5.78***
Economics	(0.54)	(0.97)	(1.18)	(1.10)	(0.47)	(0.51)	(0.90)
Engineering &	-1.76**	-2.60***	-2.88***	-3.20***	-2.38***	-2.26***	-3.20***
Math	(0.73)	(0.87)	(1.05)	(0.62)	(0.56)	(0.53)	(1.06)
Health &	-4.12***	-2.24**	-1.52	-3.50**	-3.12***	-3.40***	-3.88***
Education	(0.57)	(0.96)	(1.01)	(1.35)	(0.49)	(0.56)	(0.81)
Social	4.07***	5.96***	6.31***	3.96**	4.93***	4.76***	4.09***
Sciences	(0.57)	(1.39)	(0.90)	(1.58)	(0.48)	(0.60)	(0.78)
Humanities &	3.51***	6.08***	6.29***	3.79***	4.34***	4.29***	4.18***
Arts	(0.68)	(1.01)	(1.13)	(1.16)	(0.65)	(0.68)	(0.76)
N	212,438	59,436	35,204	66,836	239,205	269,521	38,003

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. The outcomes are self-reported political ideology, in standard deviations of graduation-survey ideology (higher values are more conservative), and an indicator for identifying as liberal or far left (multiplied by 100). Private College and Private University are private four-year colleges and private universities, and Public covers public institutions. More and Less Selective colleges are those with an entering-class SAT score, as reported by HERI, above and at or below 1,345, respectively. South covers colleges in Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia and West Virginia, and North covers all other colleges. Students missing the relevant characteristic are excluded. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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***	0.20***
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***	0.10***
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	122,222	134,627	50,675
Outcome: Liberal or Far Left			
Business &	-4.01***	-6.00***	-9.81***
Economics	(0.63)	(0.63)	(0.75)
Engineering &	-2.81***	-2.29***	-1.51
Math	(0.60)	(0.66)	(1.31)
Health &	-2.30***	-3.34***	-5.01***
Education	(0.59)	(0.66)	(1.29)
Social	5.81***	4.06***	3.54***
Sciences	(0.58)	(0.73)	(0.89)
Humanities &	5.78***	3.53***	2.52***
Arts	(0.68)	(0.79)	(0.94)
N	122,222	134,627	50,675

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. The outcomes are self-reported political ideology, in standard deviations of graduation-survey ideology (higher values are more conservative), and an indicator for identifying as liberal or far left (multiplied by 100). $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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.[41] 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:

$$D_{im} = \sum_{m'} \pi_{m'} D^0_{im'} + Q_i' \gamma^0 + u_i^0, \tag{A1}$$

$$p_i = \sum_{m} \tau_m D_{im} + Q_i' \gamma^1 + u_i^1, \tag{A2}$$

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.[42] 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 I.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 ($u_i^1 \perp D^0_{im}$). This assumption

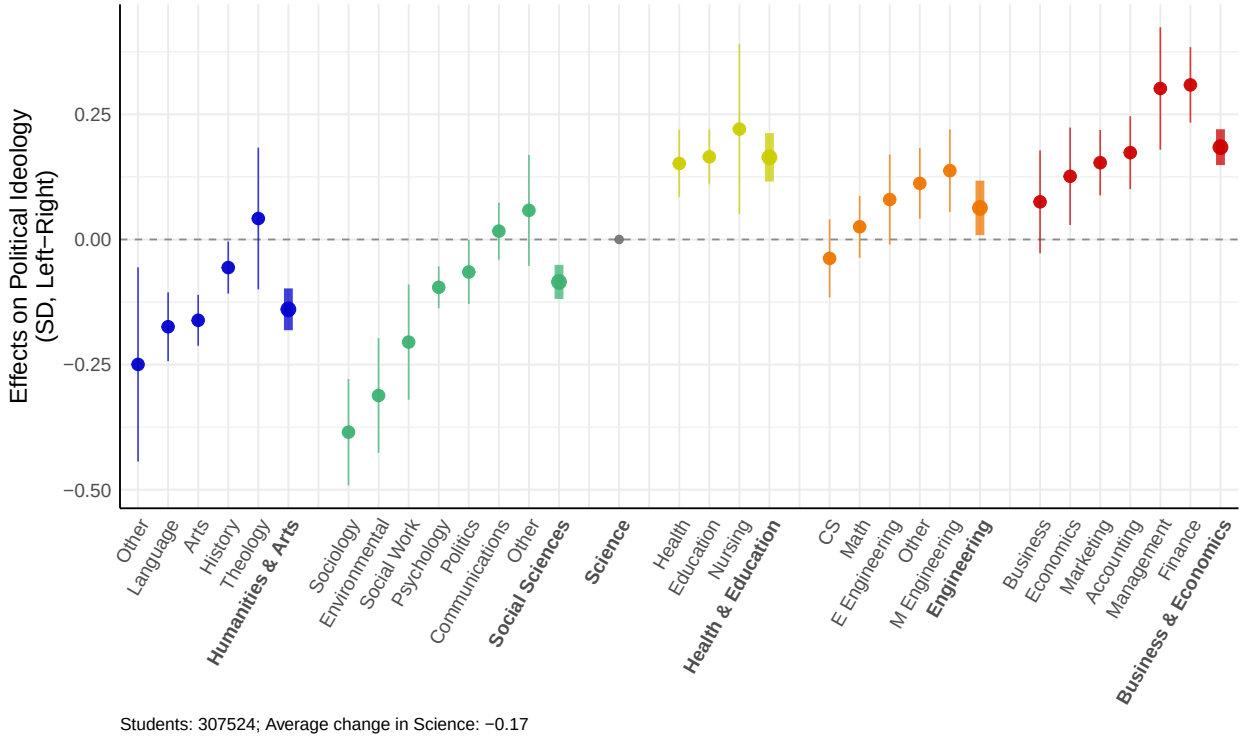
[41] Note that our bounding exercises also address this concern, as they assess potential bias from all unobserved covariates, including early college experiences.

[42] 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.[43] 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. 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.

[43] As in the main analysis, this specification includes multiple treatments and may therefore suffer from contamination bias, as demonstrated in a similar setting by Kirkebøen 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.[44]

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

[44]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 228,000 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 0.10–0.12 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. When including 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 5 percent 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	0.007	-0.009	-0.005
	(0.020)	(0.023)	(0.020)
N	228,622	228,622	228,622
Students Included in the Graduation Survey	0.061***	0.013	0.009
	(0.023)	(0.026)	(0.023)
N	47,570	47,570	47,570
Intended Major FE	No	Yes	Yes
Major Predictors	No	No	Yes

Panel B. IV Results

	(1)	(2)	(3)	(4)	(5)
First Stage: Business & Economics Major	0.176***	0.127***	0.124***	0.117***	0.096***
	(0.019)	(0.025)	(0.025)	(0.025)	(0.017)
Reduced Form: Graduation Ideology	0.109***	0.059**	0.057**	0.040**	0.031**
	(0.027)	(0.026)	(0.024)	(0.016)	(0.014)
2SLS: Graduation Ideology	0.622***	0.466**	0.458**	0.341**	0.327*
	(0.163)	(0.221)	(0.216)	(0.161)	(0.169)
N	49,288	49,288	49,288	49,288	49,288
First-stage F	87.2	25.0	24.0	21.4	30.0
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: The sample is students who intended to major in business or economics. The instrument is the leave-one-out share of students entering the same college in the same cohort with the same detailed intended major who complete that major, computed from all matched students with valid intended and realized majors (equation A3). Panel A regresses baseline ideology on the instrument in the full baseline survey and among students observed in the graduation survey. Panel B reports the first stage (outcome: business/economics major), reduced form, and 2SLS estimates for graduation ideology. Ideology is in SD units of graduation-survey ideology (mean 2.91, SD 0.84 on the 1–5 scale; higher values are more conservative). Intended Major FE are at the finest survey level; major predictors are careers and life goals; outcome predictors are baseline ideology and policy positions; "Additional" adds state, entry-year, and institution-control-by-type-by-selectivity fixed effects. First-stage F is the squared t-statistic on the instrument. Standard errors clustered by college in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

To interpret the magnitude of the IV estimate, it is useful to consider the counterfactual major choices of compliers. Panel A of Table C.8 shows that the instrument significantly

reduces the probability of pursuing social sciences and humanities, by 5 and 3 pp, respectively. This implies that about 85 percent of compliers would have pursued one of these two fields in the absence of treatment, while the remaining compliers would have pursued mostly 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 the social sciences or humanities is about 0.23 SD, and the IV estimate is somewhat larger, at 0.34 SD, although the difference is not statistically significant. The reduced form shows the same gap. Column (1) of Panel B in Table C.8 reports the instrument's effect on predicted graduation ideology, constructed from students' realized majors and the baseline major-effect estimates. This predicted effect is 0.022 SD, smaller than the actual reduced-form effect of 0.031 SD in the same specification, which adds controls for enrollment year, college state, and college type (column (5) of Panel B in Table C.7). Applying the baseline estimates to the major changes induced by the instrument therefore understates its effect on ideology. Three explanations are most plausible. First, the IV is identified by students who remain in their intended field, who likely take more of its coursework than the major switchers who receive more weight in the baseline specification. Second, measurement error in reported majors attenuates the baseline estimates. Third, the exclusion restriction may fail through coursework: a higher-persistence environment may coincide with a larger supply of business and economics courses, shifting the coursework of students who graduate in other fields. This channel operates through exposure to the field, which is consistent with the causal relationship we seek to capture. Sampling noise may also contribute.[45]

Table C.9 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 0.31 SD, though no longer statistically significant ($p = 0.11$).

[45]Columns (2)–(4) of Panel B in Table C.8 report reduced-form estimates for binary ideology outcomes. The pattern suggests that the effect operates mainly through a shift away from left-wing identification and toward both middle-of-the-road and conservative ideology.

Table C.8: Reduced-Form Estimates Based on Persistence in Intended Majors, Additional Results

Panel A. Major Outcomes

	(1)	(2)	(3)	(4)	(5)
	Social Sciences	Humanities & Arts	Natural Sciences	Engineering & Math	Health & Education
Estimate	-0.048***	-0.034***	-0.004**	-0.006*	-0.003
	(0.009)	(0.009)	(0.002)	(0.003)	(0.004)
N	49,288	49,288	49,288	49,288	49,288
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	0.022***	-0.015**	0.008	0.007	0.038**
	(0.005)	(0.007)	(0.009)	(0.007)	(0.017)
N	49,288	49,288	49,288	49,288	48,455
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: Reduced-form regressions on the persistence instrument of Table C.7, with the controls of its column (5), for students who intended to major in business or economics. Panel A outcomes are indicators for completing each major group. In Panel B, column (1) is predicted graduation ideology implied by the student's realized major and the estimates of δ_m from equation 1, in SD units of graduation-survey ideology (mean 2.91, SD 0.84 on the 1–5 scale); columns (2)–(4) are indicators for identifying as liberal or far left, middle of the road, and conservative or far right; column (5) is the policy-position index, in its own SD units. Indicators are in probability units (0–1). Standard errors clustered by college in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.9: IV Estimates Based on Persistence in Intended Majors, Adjacent Cohorts

Panel A. Falsification Tests: Baseline Ideology

	(1)	(2)	(3)
Full Baseline Survey	-0.021	-0.032	-0.026
	(0.025)	(0.027)	(0.023)
N	278,619	278,619	278,619
Students Included in the Graduation Survey	0.014	-0.019	-0.007
	(0.046)	(0.046)	(0.036)
N	37,083	37,083	37,083
Intended Major FE	No	Yes	Yes
Major Predictors	No	No	Yes

Panel B. IV Results

	(1)	(2)	(3)	(4)	(5)
First Stage: Business & Economics Major	0.158***	0.116***	0.110***	0.106***	0.080***
	(0.029)	(0.032)	(0.031)	(0.030)	(0.018)
Reduced Form: Graduation Ideology	0.068*	0.035	0.038	0.033*	0.024
	(0.039)	(0.040)	(0.034)	(0.018)	(0.017)
2SLS: Graduation Ideology	0.431*	0.299	0.346	0.310	0.294
	(0.223)	(0.329)	(0.302)	(0.196)	(0.227)
N	38,452	38,452	38,452	38,452	38,452
First-stage F	30.3	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: As Table C.7, except that the persistence instrument is computed from the adjacent (preceding and following) entry cohorts rather than the student's own cohort, and samples are restricted to cohorts with observed adjacent cohorts. The sample is students who intended to major in business or economics. The instrument is the share of students entering the same college in the previous or next cohort with the same detailed intended major who complete that major, computed from all matched students with valid intended and realized majors (equation A3). Panel A regresses baseline ideology on the instrument in the full baseline survey and among students observed in the graduation survey. Panel B reports the first stage (outcome: business/economics major), reduced form, and 2SLS estimates for graduation ideology. Ideology is in SD units of graduation-survey ideology (mean 2.91, SD 0.84 on the 1–5 scale; higher values are more conservative). Intended Major FE are at the finest survey level; major predictors are careers and life goals; outcome predictors are baseline ideology and policy positions; "Additional" adds state, entry-year, and institution-control-by-type-by-selectivity fixed effects. First-stage F is the squared t-statistic on the instrument. Standard errors clustered by college in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 2, we apply this method to the estimated effect of choosing business or economics relative to the humanities and social sciences, pooled as the omitted group.

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 increase in R^2 from including 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, $\delta_{m,F} = 0.229$ and $\delta_{m,P} = 0.248$, while $R^2_F = 0.401$ and $R^2_P = 0.315$. Substituting these values into the Oster formula gives:

$$\delta^*_m = 0.229 - \lambda\,(0.019)\,\frac{R^2_{\max} - 0.401}{0.086}.$$

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.[46] As in the previous subsection, we focus on the business or economics coefficient estimated on the full sample relative to the pooled humanities and social sciences.

Table D.1 reports these benchmarks. 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 0.025, and the adjusted estimate is 0.204. 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 0.050, and the adjusted estimate remains 0.179.

Table D.1: Robustness to Unobserved Selection

Benchmark	k_D	k_Y	Estimate	Bias	Adjusted Estimate
Intended Business & Economics	1	1	0.235	0.007	0.228
Baseline Ideology	1	1	0.229	0.025	0.204
Intended Business & Economics	2	2	0.235	0.015	0.220
Baseline Ideology	2	2	0.229	0.050	0.179

Notes: 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 and social sciences, estimated on the full sample with these two fields as the pooled 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).

[46] 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

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 students in our main analysis sample from survey years that include eight core policy questions and who answer all of them in both surveys, and incorporate all policy position questions included in the survey.[47] 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 47 percent of the effect of studying business and economics relative to natural sciences, about 61 percent of the effect of studying social sciences, and about 64 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 7 percent. In contrast, for social sciences and humanities, cultural policy issues explain 58 and 47 percent of the effects, respectively, whereas economic policy positions explain 1 and 4 percent. Figure E.1 presents these results graphically.

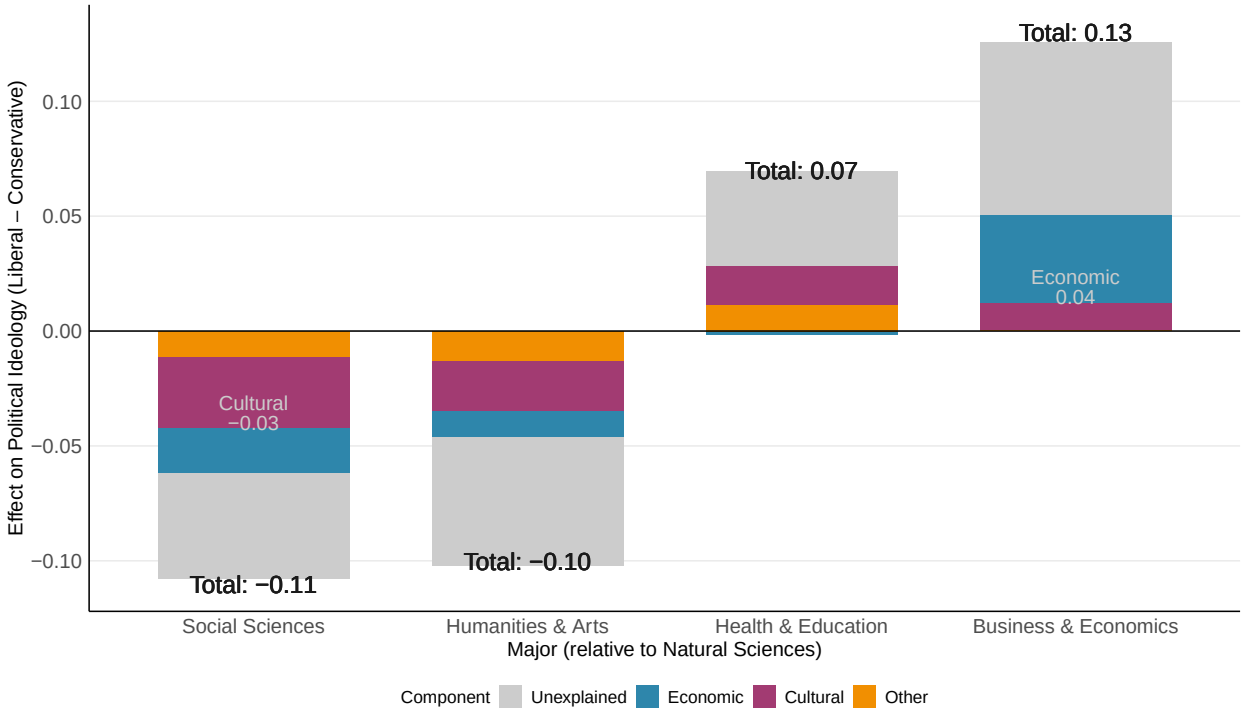
[47] Since questions vary between years, we add a dummy for missing responses or missing questions.

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	0.126	0.067	34.3%	7.1%	5.4%	53.2%
Engineering & Math	Natural Sciences	0.077	0.035	33.8%	18.2%	1.9%	46.1%
Health & Education	Natural Sciences	0.069	0.037	14.1%	13.6%	18.3%	54.0%
Social Sciences	Natural Sciences	-0.108	-0.042	1.5%	57.8%	2.0%	38.6%
Humanities & Arts	Natural Sciences	-0.102	-0.036	4.2%	47.4%	12.8%	35.6%

Notes: This table shows the Gelbach decomposition of the estimated effects of college major on students' political ideology at graduation. Economic, Culture and Other Views are the first principal components of all graduation-survey questions on economic issues (such as taxes on the wealthy, national health care and climate change), cultural issues (such as abortion, homosexual relationships, race and campus speech) and other issues (such as marijuana legalization and the ability to change society), respectively. Base Coef is the coefficient on Major 1 relative to Major 2 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. The sample is restricted to graduation-survey years that field eight core policy questions and to students who answer all eight in both surveys and report baseline ideology, so the base coefficients differ slightly from the main estimates.

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.

Appendix F Reconciling the Quasi-Experimental Evidence: The Role of Coursework

A simple calibration illustrates how changes in coursework beyond those induced by major switching could reconcile the estimates. Suppose switching into business or economics reallocates 30 percentage points of a student's coursework toward these fields, and suppose this reallocation accounts for the entire 0.3 SD ideological effect suggested by our IV estimates. Under a linear model with the same effects for the coursework changed by program expansions, each percentage point of reallocated coursework would shift ideology by 0.01 SD. An average reallocation of about 4 percentage points would therefore generate the roughly 0.04 SD reduced-form effect. Under these assumptions, the 3.2-percentage-point increase in business or economics majoring would account for about one percentage point of the average coursework adjustment, leaving approximately three percentage points to arise through additional course choices. This calculation is illustrative: it assumes both the coursework difference associated with major switching and the extent to which coursework accounts for the IV estimates.

The heterogeneity analysis in Table F.1 provides suggestive evidence consistent with this interpretation. We divide students at the median predicted probability of majoring in business or economics, estimated using their pre-college characteristics. Expansions increase business or economics majoring by 0.8 percentage points in the low-probability group, compared with 4.2 percentage points in the high-probability group. Yet the estimated declines in the probability of taking any humanities or arts course are similar, at 1.9 and 2.3 percentage points, respectively, as are the ideological shifts, at 0.034 and 0.041 SD.

Applying the 0.3 SD major-effect benchmark, the increase in business or economics majoring in the low-probability group would predict an ideological shift of only 0.0023 SD, about 7 percent of its estimated reduced-form effect. Thus, at the point estimates, additional business or economics majoring accounts for little of this group's ideological response. Instead, changes in coursework can explain it.

Table F.1: Effects of College-Level Shocks, by Predicted Likelihood of Majoring in Business/Economics

A. Predicted Probability Below the Median			
	(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)	125,235	82,223	120,817
B. Predicted Probability at or Above the Median			
	(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 majoring and course-taking outcomes are estimated without a lagged baseline control; the ideology outcome controls for the student's baseline ideology response, with a separate fixed effect for each discrete value. The 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 whose predicted probability of taking business or economics as a major is below the median, and results in Panel B to students at or above the median, 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 of graduation-survey ideology. The table reports coefficient estimates, standard errors clustered at the college level, the mean of the outcome among untreated students (at colleges without a shock or in cohorts entering before it), and the number of shocks, colleges, and student observations in the pooled estimation sample. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Appendix G 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.[48] 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.[49] 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 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:

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

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^{-i}_{cmt} represents the average baseline political ideology of the student's peers (in SD of graduation-survey ideology); Φ^{-i}_{cmt} captures the average ideology of students in the same college and intended major across the five-year window ($t' \in \{t-2, t-1, t, t+1, t+2\}$); ν_{cm}, ι_{ct}, and ω_{mt} denote college-major, college-cohort, and major-cohort fixed effects, respectively; and X_i is a vector of individual-level baseline controls: the student's own baseline ideology, life goals, intended career, and baseline policy

[48] 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 I.1A), and even those who do not are likely to share similar early course experiences.

[49] In the analysis sample this classification distinguishes 70 specific intended-major categories, compared with the 27 aggregated categories of the main analysis: biology, chemistry, and physics, for example, are separate peer groups but a single aggregated category, the natural sciences.

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

positions. Table G.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 H formally derives this argument. In the main analysis, the sample is restricted to college cohorts, where the baseline survey includes at least five peers who account for at least 10 percent of enrolled students. The results are not sensitive to choosing other thresholds for sample inclusion.

Avoiding exclusion bias. Estimating peer effects in specifications that include fixed effects presents a unique challenge of exclusion bias (Guryan et al., 2009; Angrist, 2014; Caeyers

[51] Figure G.1 shows that substantial variation in peer ideology remains after residualization. The residual standard deviation is 0.14 in the preferred specification, compared with 0.40 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.

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 G.1 presents our main 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; Panel B restricts it to the college-by-intended-major-by-cohort cells observed in the graduation survey; Panel C further restricts it to students included in the graduation survey. We report unadjusted and adjusted estimates, quoting the adjusted ones from the fullest specification; these are preferred, given that unadjusted estimates might be attenuated toward zero. In the full baseline sample, the coefficient is statistically significant though economically very small (Panel A: 0.018, s.e. 0.008, under three percent of the unconditional association in column (1)). Its significance does not survive restriction toward the estimation sample: the coefficients are similar in size but insignificant in the graduation-survey cohorts (Panel B: 0.028, s.e. 0.020) and among students in the graduation survey (Panel C: 0.022, s.e. 0.026). Residual sorting on baseline ideology is thus at most slight, and detectable only in the full sample. The preferred outcome specifications nevertheless condition directly on students' own baseline ideology and policy positions, limiting the scope for any remaining sorting to bias the estimates.

Estimated peer effects. Table G.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 0.010 SD in the preferred specification, the upper limit of the two-sided 95 percent confidence interval.

[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 G.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)		0.053***	0.070***	0.019**	0.018**
		(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. Cohorts Included in the Graduation Survey					
	(1)	(2)	(3)	(4)	(5)
Estimate	0.730***	0.021***	0.037***	0.012	0.011
	(0.016)	(0.008)	(0.008)	(0.009)	(0.009)
Estimate (Adjusted)		0.052***	0.082***	0.029	0.028
		(0.019)	(0.019)	(0.020)	(0.020)
N	1,004,934	1,004,934	1,004,934	1,004,934	1,004,934
Fixed Effects	No	Yes	No	Yes	Yes
Potential Peers' Average	No	No	Yes	Yes	Yes
Major Predictors	No	No	No	No	Yes
Panel C. 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)		0.045*	0.040	0.022	0.022
		(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 H.5.2. No adjusted estimate is reported in column (1), which includes neither fixed effects nor controls from which to predict the unobserved peers; in the remaining columns the unobserved share of peers is imputed from the specification's own controls and fixed effects. Panel A uses the full baseline-survey sample. Panel B restricts it to college-by-intended-major-by-cohort cells observed in the graduation survey. Panel C uses only students observed in the graduation survey. In all panels baseline ideology and the peer average are measured in standard deviations of graduation-survey ideology (mean 2.91, SD 0.84 on the five-point scale). Each reported coefficient averages the four split-sample iterations described in Appendix G. "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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table G.2: Estimated Effects of Peer Ideology Composition

Panel A. All Majors, Various Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.788***	0.001	-0.011	-0.006	-0.013	-0.014
	(0.025)	(0.013)	(0.014)	(0.014)	(0.011)	(0.010)
Estimate (Adjusted)		0.003	-0.021	-0.014	-0.027	-0.028
		(0.024)	(0.025)	(0.025)	(0.021)	(0.019)
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

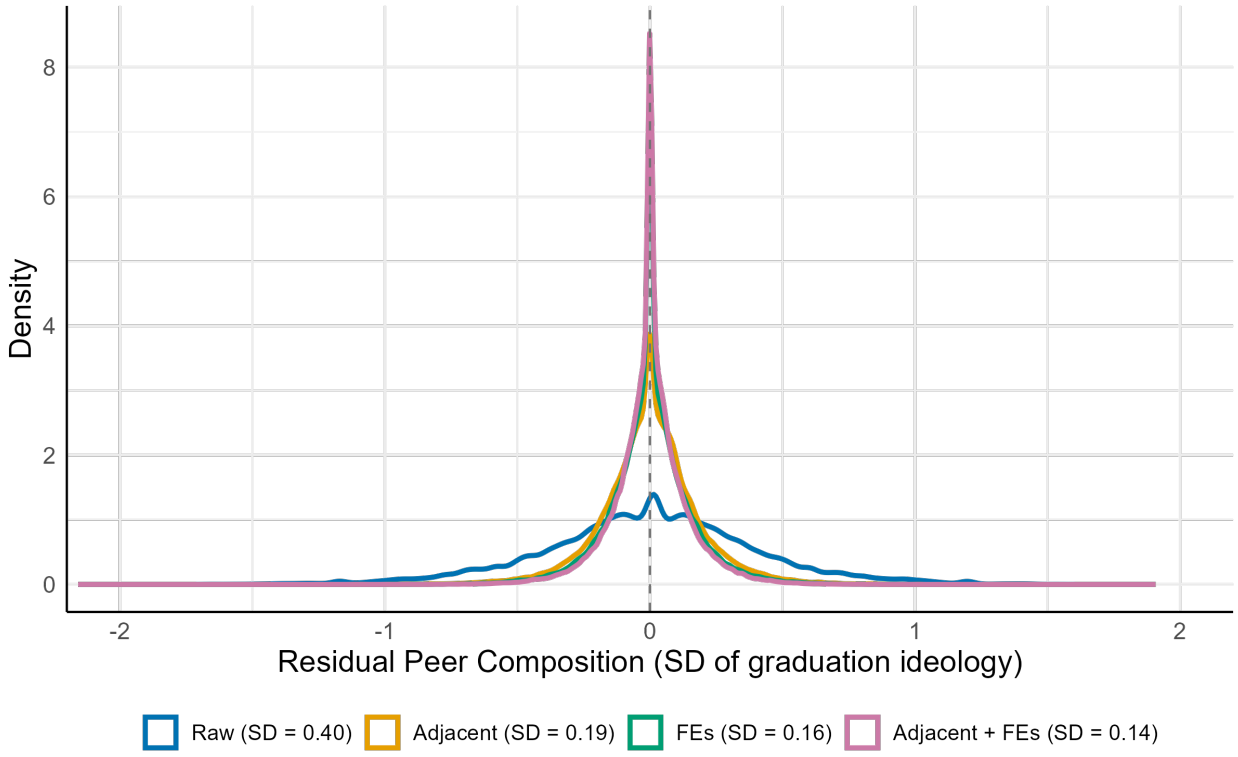
	(1) Business & Economics	(2) Engineering & Math	(3) Health & Education	(4) Humanities & Arts	(5) Natural Sciences	(6) Social Sciences
Estimate	-0.035	-0.012	-0.004	0.021	0.037	-0.005
	(0.028)	(0.061)	(0.029)	(0.037)	(0.041)	(0.029)
Estimate (Adjusted)	-0.053	-0.025	-0.017	0.034	0.046	-0.011
	(0.055)	(0.101)	(0.056)	(0.065)	(0.070)	(0.056)
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, measured, like the outcome, in standard deviations of graduation-survey ideology. 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 H.5.2. No adjusted estimate is reported in column (1), which includes neither fixed effects nor controls from which to predict the unobserved peers; in the remaining columns the unobserved share of peers is imputed from the specification's own controls and fixed effects. 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 G. "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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

A back-of-the-envelope calculation indicates that peer effects could not account for more than a tiny fraction of the major effects. The baseline difference in ideology between business and economics and social sciences and humanities majors is 0.359 SD. With an upper-bound estimate of 0.010 SD, the maximum peer effect from switching between these majors equals 0.00359 SD (0.010×0.359). Since only 37 percent of students ultimately pursue the major they intended, a more conservative estimate is about 0.01 SD (0.00359 / 0.37). This is roughly 4 percent of the 0.23-SD effect of business and economics versus social sciences or humanities.

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

Figure G.1: 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.

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

	Ideology (1)	Liberal (2)	Index (3)	Ideology (4)	Ideology (5)	Ideology (6)
Estimate	-0.013	0.009	0.016	-0.012	-0.008	-0.026
	(0.011)	(0.012)	(0.012)	(0.011)	(0.013)	(0.018)
Estimate (Adjusted)	-0.027	0.010	0.023	-0.025	-0.019	-0.045
	(0.021)	(0.023)	(0.024)	(0.021)	(0.022)	(0.028)
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
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

Notes: This table presents estimates of peer effects on students' political ideology based on equation A5. The outcome is self-reported ideology, an indicator for identifying as liberal or far left, or the policy-position index, all from the graduation survey. The treatment is the baseline-survey average of the same measure among 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 H.5.2. Each reported coefficient averages the four split-sample iterations described in Appendix G. "Potential Peers' Average" indicates controls for the average of the same measure among potential peers from adjacent cohorts. "Fixed Effects" indicates college-major, college-cohort, and major-cohort fixed effects. "Baseline Ideology" indicates a control for the student's own baseline political ideology. "Major Predictors" indicates controls related to major choice, including intended majors, careers, and life goals. Ideology estimates are in standard deviations of graduation-survey ideology, with the peer average on the same scale; Index estimates are in standard deviations of the policy-position index; 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 intended major and outcome, at least 5 observed peers, and 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 Share row. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Appendix H Adjusting the Faculty and Peer Ideology Measures

The faculty-ideology measure of Equation 3 averages the faculty of a department who answered the faculty survey, but the treatment of interest is the ideology of the whole department, surveyed or not. Regressing the outcome on the surveyed mean attenuates the coefficient, because the surveyed mean is a noisy measure of the department (Angrist and Pischke, 2009). The peer measure of Equation A5 raises the same problem. This appendix shows how the coefficient can be corrected with two quantities we can measure: the survey's coverage of each college, from administrative staff counts, and the reliability of the surveyed mean, which we estimate from the faculty survey itself. Section H.1 states the two assumptions we maintain. Section H.2 derives the adjusted treatment, whose weight combines coverage with reliability, and Section H.3 establishes the consistency of our estimator. Section H.4 shows that the coverage adjustment used in the peer analysis is the special case in which the surveyed mean carries no information about the peers we do not observe. Section H.5 estimates the reliability by a split-half design, comparing the mean ideology of two random halves of each department's surveyed faculty, and measures the coverage from the survey's respondent counts and administrative staff counts. Section H.6 relates the approach to existing corrections. We write throughout in terms of the faculty measure; the same derivation applies to the peer measure, with its cell in place of the department.

H.1 Setup and assumptions

For student i who entered college c in cohort t intending major m, the effect we want is τ in

$$p_{icmt} = \tau F^{F}_{cm} + \gamma' X_i + \zeta_{icmt}, \tag{A7}$$

where F^{F}_{cm} is the average ideology of the *full* faculty of department m at college c, ζ_{icmt} is an error, and X_i collects everything the specification conditions on: the major-by-cohort and college-by-cohort-by-broad-field fixed effects of Equation 3, η_{tm} and ψ_{ctM}, and, in the richer specifications, the individual-level controls (the student's baseline ideology, policy positions, life goals, and intended career). We do not observe F^{F}_{cm}. We observe the mean over the surveyed faculty, F^{O}_{cm}, who are a share α_{cm} of the department's faculty; we call this share the department's coverage. It is measured at the college level (Section H.5.2), so the fixed effects in X_i absorb it. The mean over the unsurveyed faculty, F^{U}_{cm}, is missing. By simple

accounting,

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

We maintain two assumptions.

(A1) *Conditional mean independence.* Given the fixed effects and the individual-level controls, students who intend a major in a department whose surveyed faculty are more left-leaning are not different in any other way that matters for their graduation ideology. Formally, $E[\,\zeta_{icmt} \mid F^O_{cm}, X_i\,] = 0$: the ideology of the department's surveyed faculty does not predict the unexplained part of the outcome. This is the design assumption of Section 4, maintained throughout. It is supported by the falsification tests of Table I.6, which regress students' baseline ideology on the faculty measure and find at most a small association once the fixed effects and controls are included, and only in the full baseline sample.[55]

(A2) *Random draw of respondents.* The surveyed faculty are a random draw of the department's faculty. Response rates may differ freely across colleges, but within a department, who responds carries no information about their views, so the surveyed mean is an unbiased measure of the department's own mean. Formally, given X_i for every student i of the department, the ideologies of the respondents and of the non-respondents are jointly distributed as they would be if the same number of respondents had been drawn at random from the department's members. Response may depend on who a member is, such as their rank, as long as this carries no information about views. Consistent with this, within a college and survey wave, departments' response rates are not significantly related to their faculty's ideology.[56]

[55] These tests estimate the "effect" of the faculty measure on students' ideology at college entry. With the fixed effects and major predictors, the coefficient on the adjusted measure is 0.016 (0.004) in the full baseline sample and −0.006 (0.013) among the students in the graduation survey. Note that the main faculty specification also controls for students' baseline ideology, further mitigating concerns about omitted variable bias.

[56] We pool departments into our 27 majors and proxy each major's response rate at a college by its faculty respondents per bachelor's degree awarded in the major around the survey wave (IPEDS). Regressing the major's mean faculty ideology on the log response rate, with college-by-major-group-by-wave and major-by-wave fixed effects (mirroring those of the main faculty specification, with survey waves in place of entry cohorts), gives −0.010 (0.007) across 19,787 cells.

Let $\widehat{F}_{cm}$ denote the department's ideology as predicted by X_i, what we would have predicted before seeing any of its surveyed faculty.[57] Let

$$\rho = \frac{\mathrm{Cov}\big(F^U_{cm} - \widehat{F}_{cm},\, F^O_{cm} - \widehat{F}_{cm}\big)}{\mathrm{Var}\big(F^O_{cm} - \widehat{F}_{cm}\big)} \tag{A9}$$

be the slope of the unsurveyed faculty's deviation from the prediction on the surveyed faculty's deviation: the share of what we see in the surveyed faculty that carries over, on average, to their unsurveyed colleagues. We take it to be the same at every level of coverage.[58] ρ is the reliability of the surveyed mean as a measure of the department: it is zero when the surveyed deviation is entirely the sampling error of the particular faculty who responded, and one when it is entirely shared by the department.

H.2 The adjusted treatment

Equation A8 splits the full department into the faculty we observe and those we do not. For the share α_{cm} who answered the survey, we know their views, so their mean F^O_{cm} enters as it is. For the remaining share $1 - \alpha_{cm}$ we have two pieces of information: the prediction $\widehat{F}_{cm}$ from X_i, and how far the surveyed faculty sit from it, $F^O_{cm} - \widehat{F}_{cm}$. The unsurveyed faculty share what the department has in common, but not the idiosyncratic views of the particular faculty who responded. Only part of the surveyed deviation therefore carries over to them, and on average that part is the share ρ of it (Equation A9). The unsurveyed faculty's mean is thus

$$F^U_{cm} = \widehat{F}_{cm} + \rho\big(F^O_{cm} - \widehat{F}_{cm}\big) + \xi_{cm}, \tag{A10}$$

where ξ_{cm} is the part of their views that neither X_i nor the surveyed faculty predict: it has mean zero given X_i and is uncorrelated with the surveyed deviation at every level of coverage.[59]

[57] The prediction is the fitted value from regressing the measure on the specification's own fixed effects and controls. Since the fixed effects absorb coverage, the prediction also conditions on it, as the derivation requires. It is formed at the student level, so it varies across a department's students with their cohort and controls; we suppress these indices.

[58] In unreported results, we allowed the reliability to vary across departments in several ways, including with their number of respondents and their field of study, with almost no effect on the estimates.

[59] Since ρ is the same at every level of coverage, ξ_{cm}, the residual of the regression in Equation A10, is uncorrelated with the surveyed deviation at every level of coverage. It has mean zero given X_i because both deviations do: the surveyed one because $\widehat{F}_{cm}$ is its prediction from X_i, and the unsurveyed one because, by (A2), the unsurveyed faculty's mean has the same expectation as the surveyed faculty's.

Substituting Equation A10 into Equation A8 and collecting the terms in the surveyed deviation,

$$\begin{aligned} F^F_{cm} &= \alpha_{cm} F^O_{cm} + (1-\alpha_{cm})\Big[\widehat{F}_{cm} + \rho\big(F^O_{cm} - \widehat{F}_{cm}\big) + \xi_{cm}\Big] \\ &= \widehat{F}_{cm} + \big[\alpha_{cm} + (1-\alpha_{cm})\rho\big]\big(F^O_{cm} - \widehat{F}_{cm}\big) + (1-\alpha_{cm})\,\xi_{cm}. \end{aligned}$$

Every term on the right but the last can be computed: the prediction and the surveyed mean come from the data, and ρ is estimated in Section H.5.1. The last term is the unpredictable part of the unsurveyed faculty's views. Dropping it gives the adjusted treatment, our measure of the full department's ideology,

$$\widetilde{F}_{cm} = \widehat{F}_{cm} + k_{cm}\big(F^O_{cm} - \widehat{F}_{cm}\big), \tag{A11}$$

with weight $k_{cm} = \alpha_{cm} + (1-\alpha_{cm})\rho = \rho + \alpha_{cm}(1-\rho)$, so that $F^F_{cm} = \widetilde{F}_{cm} + (1-\alpha_{cm})\,\xi_{cm}$. The weight has two parts: the surveyed faculty carry the observed deviation one for one, and the unsurveyed faculty carry the share ρ of it. It therefore rises from the coverage share, when the observed deviation is pure sampling error ($\rho = 0$), toward one as the deviation becomes something the whole department shares ($\rho = 1$). 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).[60]

H.3 Consistency of the adjusted estimator

Substituting $F^F_{cm} = \widetilde{F}_{cm} + (F^F_{cm} - \widetilde{F}_{cm})$ into Equation A7 rewrites the outcome equation as

$$p_{icmt} = \tau \widetilde{F}_{cm} + \gamma' X_i + u_{icmt}, \tag{A12}$$

where

$$u_{icmt} \equiv \tau\big(F^F_{cm} - \widetilde{F}_{cm}\big) + \zeta_{icmt}. \tag{A13}$$

This is the regression we estimate. The first component of the error is $\tau(1-\alpha_{cm})\xi_{cm}$, the unpredictable part of the unsurveyed faculty's views (Section H.2). It has mean zero given X_i and is uncorrelated with the surveyed deviation at every level of coverage, hence uncorrelated with $\widetilde{F}_{cm}$ and X_i; the second is uncorrelated with the regressor and X_i by (A1), since $\widetilde{F}_{cm}$

[60] Because $\widehat{F}_{cm}$ is built only from variables already in the regression, the construction cannot generate a mechanical correlation with the outcome. The same holds for the peer measure, which in addition excludes the student's own quarter of the sample, so that no peer measure contains the student's own response.

depends only on F^O_{cm} and X_i. An error with these two properties is what least squares requires, so the regression on $\widetilde{F}_{cm}$ identifies τ, the effect of the full department's ideology, at any level of coverage. In the language of measurement error the adjusted treatment carries Berkson rather than classical error, which costs precision, not consistency (Berkson, 1950; Hyslop and Imbens, 2001). Standard errors are clustered by college and treat $\widehat{F}_{cm}$ and $\hat{\rho}$ as known; Section H.5.1 shows that the second matters little.

Attenuation without the correction. Regressing the outcome on the surveyed mean F^O_{cm} itself yields a coefficient that converges to $\tau\bar{k}$, where $\bar{k}$ is the average of the weights k_{cm} across students: the surveyed mean is a noisy measure of the department, and the regression is attenuated by its reliability as a measure of the full department's deviation, a classical errors-in-variables result. To see why, consider a department whose surveyed faculty are more left-leaning than the fixed effects and controls predict. If the deviation reflects only who happened to answer the survey, the unsurveyed faculty are expected to sit at the prediction, and the department as a whole is further left than predicted by only the surveyed share of the observed deviation; the raw regression treats it as having moved by the whole deviation and understates the effect accordingly. If instead the deviation is something the department shares, the unsurveyed faculty share it, and the department has moved by the whole of it. The reliability measures where a department lies between these two cases.

H.4 The peer measure

The peer measure of Equation A5, described in Appendix G, is built identically, with the college-by-intended-major-by-cohort cell in the role of the department, the prediction formed from the fixed effects and controls of that equation, including the adjacent-cohort control, and coverage measured at the college-cohort level (Section H.5.2). One feature of the peer setting pins down the reliability of the measure. Each student is herself a member of her cell. A common component in the cell's baseline composition would therefore be a component of her own predetermined views: students in cells that lean left beyond what the controls predict would themselves lean left at baseline. This is exactly what (A1) rules out in the peer setting, and it is what the falsification tests of Table G.1 check, by regressing a student's own baseline ideology on the measure built from other students in her cell. Under (A1) the peer measure has $\rho = 0$, so $k = \alpha$ and the adjusted treatment reduces to the coverage adjustment of Equation A6, $\alpha_{ct}P^O_{cmt} + (1 - \alpha_{ct})\widehat{P}_{cmt}$: the observed deviation is scaled down to the share of the cell it speaks for. The falsification tests confirm this almost exactly. Because a student shares her cell's common component while the measure contains it plus the sampling error

of the other students, the falsification coefficient on the raw measure converges to ρ;[61] with the specification of the main peer estimates, it is 0.009 (0.003) in the full baseline sample, 0.011 (0.009) in the cohorts observed in the graduation survey, and 0.010 (0.014) among the students in the graduation survey. Reading the whole coefficient as a common component gives $\rho \leq 0.01$, so that adjusting for coverage alone, which sets $\rho = 0$, overstates the peer effect by at most one percent.

H.5 Measuring the reliability and the coverage

The adjusted treatment requires two inputs, the reliability ρ and the coverage α_{cm}. We estimate the reliability from the faculty survey by a split-half design, and we measure coverage from the survey's respondent counts and administrative staff counts.

H.5.1 Estimating the reliability

The reliability ρ is the slope of the unsurveyed faculty's deviation on the surveyed faculty's (Equation A9). We cannot compute it directly, because the unsurveyed faculty are not observed, so we estimate it by a split-half design. Within each eligible college-by-department cell we assign the surveyed faculty at random to two halves and compute each half's mean ideology, F^A_{cm} and F^B_{cm}. We attach the observed mean and both half means to every student i of the department in the estimation sample, and residualize them on the fixed effects and controls of Equation 3, the same prediction that enters the correction, which gives deviations D_{icmt}, D^A_{icmt} and D^B_{icmt}. They vary across a department's students because the fitted values depend on each student's cohort and controls. By (A2), the surveyed and unsurveyed faculty are, in their views, like two parts of a random split of the department, and the two halves are a random split of the surveyed faculty. Two non-overlapping random groups of a department have mean deviations with the same covariance whatever the groups' sizes, so the covariance of the halves equals the numerator of Equation A9, and

$$\hat{\rho} = \frac{\mathrm{Cov}\big(D^A_{icmt}, D^B_{icmt}\big)}{\mathrm{Var}\big(D_{icmt}\big)}, \tag{A14}$$

[61] This holds exactly when the members of a cell share only a common component δ_{cmt}, beyond which their own deviations are uncorrelated. Relative to their predictions, the student's baseline ideology is then $\delta_{cmt} + e_{icmt}$ and the raw measure $\delta_{cmt} + \bar{e}^O_{cmt}$, with $\bar{e}^O_{cmt}$ the other students' average deviation, so the slope of the first on the second is $\mathrm{Var}(\delta_{cmt})/[\mathrm{Var}(\delta_{cmt}) + \mathrm{Var}(\bar{e}^O_{cmt})]$, which is also the slope of Equation A9.

with both moments taken across the students in the estimation sample, so that departments are weighted as in the regression.[62] Figure H.1 shows the test: if departments carried no common component, the two halves would be independent draws and the mean of one half would not predict the mean of the other. It does, with a slope of

$$\frac{\mathrm{Cov}\left(D^A_{icmt}, D^B_{icmt}\right)}{\mathrm{Var}\left(D^A_{icmt}\right)} = \frac{0.026}{0.097} = 0.27.$$

The reliability is larger because it divides the same covariance by the variance of the mean of all the department's surveyed faculty rather than of half of them:

$$\hat{\rho} = \frac{\mathrm{Cov}\left(D^A_{icmt}, D^B_{icmt}\right)}{\mathrm{Var}\left(D_{icmt}\right)} = \frac{0.026}{0.060} = 0.43.$$

This reliability means that departments differ from their prediction in large part as departments, not only through the particular faculty who responded, so the surveyed mean is informative about the faculty we do not observe. With an average coverage of $\bar{\alpha} = 0.41$, the average weight is $\bar{k} = \hat{\rho} + \bar{\alpha}(1 - \hat{\rho}) = 0.67$, so the raw estimate of Table 4 is attenuated by about a third. The adjusted estimates of Section 4 are the estimates of Equation A12, with the adjusted treatment formed from the reliability estimated in each specification's own sample and residual space, and from each department's coverage share.[63]

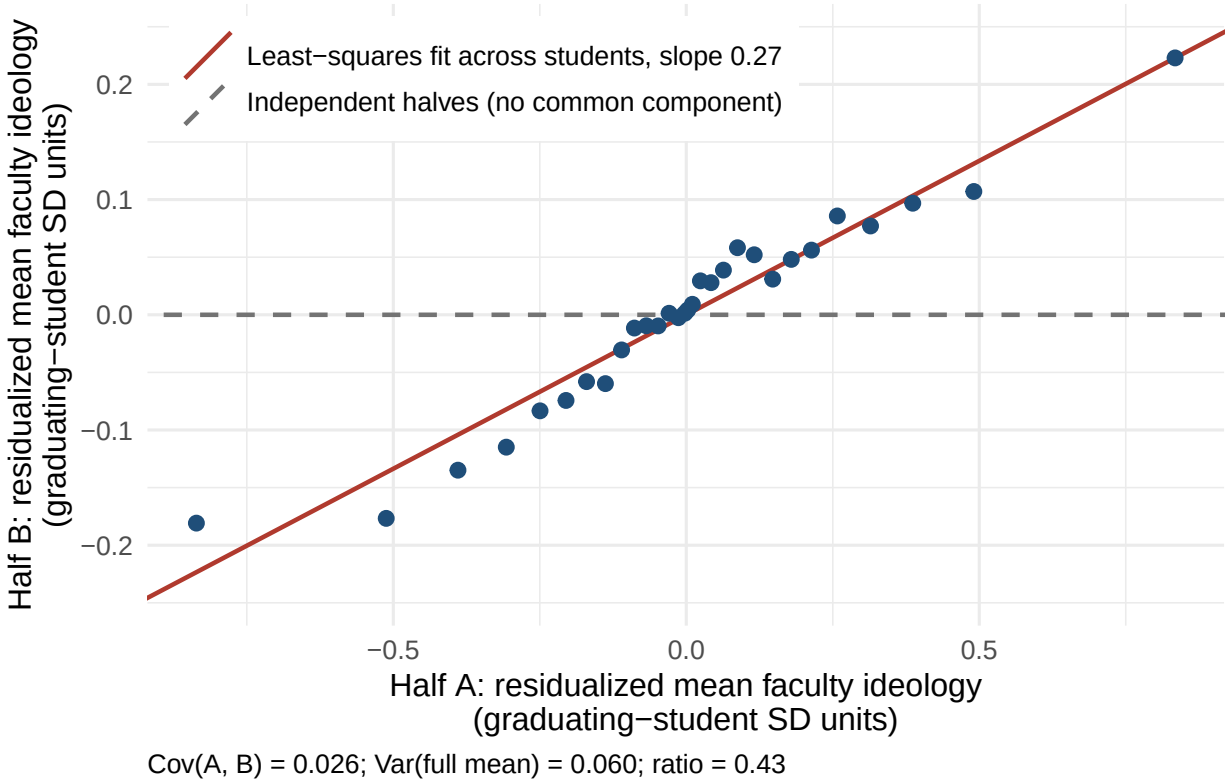
H.5.2 Measuring coverage

Data sources. The numerator of the coverage share is a count of survey respondents, and the denominator an administrative count of the population they are drawn from, taken from the Integrated Postsecondary Education Data System (IPEDS) of the National Center for Education Statistics. For the faculty measure, the numerator is the number of faculty at a college who answered the faculty survey, and the denominator is the college's count of full-time instructional staff in the salaries component of the IPEDS Human Resources survey. For the peer measure, the numerator is the number of baseline-survey respondents with a valid ideology response and a non-missing, non-undecided intended major in a college and entry cohort, excluding the quarter of the graduation sample that contains the student, and

[62] A similar approach, using the covariance between two independent noisy measures, is used in the teacher value-added literature (Kane and Staiger, 2008; Chetty et al., 2014).

[63] This estimator treats the estimated reliability as known when computing standard errors; because $\hat{\rho}$ is estimated from a very large sample, allowing for its sampling variation changes the standard errors only minimally.

Figure H.1: Split-Half Test of the Faculty-Ideology Measure



Notes: Each department's surveyed faculty are assigned at random to two halves, A and B. For each student in the estimation sample of column (3) of Table 4, we take the mean ideology of each half of her intended department and residualize it on the fixed effects and student controls of the main faculty specification, which include the student's baseline ideology, policy positions, life goals and intended career. Students are grouped into 30 equal-sized bins by their half-A residual, and each point plots a bin's average half-B residual against its average half-A residual. The solid line is the least-squares fit across all students; the dashed line at zero is what two halves with no common component would show.

the denominator is the college's total first-year enrollment in the IPEDS Fall Enrollment survey.

Measurement. Neither survey reports counts at the level of the cell that defines the measure. The faculty survey reports no department-level staff counts, so we measure the share at the college level and infer the department's coverage from it: α_{cm} is set equal to the college's share, α_c, which assumes that the survey covers each department of a college in the same proportion. For the peer measure the same inference runs from the college-cohort level, α_{ct}, to the college-by-intended-major-by-cohort cell, which is why Equation A6 carries a ct subscript; the counts refer to the measured peer group, with the quarter exclusion already embedded.

Potential bias. Inferring a department's coverage from its college's introduces a gap, $\kappa_{cm} = \alpha_{cm} - \alpha_c$, and likewise for cells. The gap is harmless as long as (A2) holds at the college level as well: if the surveyed faculty are a random draw of the college's faculty, and not only of each department's, the gap is mean-zero noise unrelated to the departments' views, which averages out. It is problematic if the departments' response shares are correlated with their views: assuming (A2) holds within each department, the college's share then overstates the coverage of some departments and understates that of others in a way related to their deviation, so the weight on the surveyed deviation is systematically wrong.[64] Finally, an error in the overall level of coverage, for example in the staff counts that form its denominator, passes into the adjusted faculty estimate only in part: since $k_{cm} = \rho + \alpha_{cm}(1 - \rho)$, a given percentage error in coverage moves the estimate by about a third as much, in the opposite direction.

H.6 Relation to existing corrections

Two papers address the estimation of group effects when only part of each group is observed. Ammermueller and Pischke (2009) estimate the effect of classmates' family background on reading scores, comparing classes within schools, with the class mean computed from the students who were sampled. Their identifying assumption is that classes within a school are formed at random, so that once school fixed effects are removed a class differs from its school only through the luck of the draw: in our notation, $\rho = 0$. Under this assumption, sampling only part of the class attenuates the within-school coefficient by the sampled share, and they correct the estimate after estimation by dividing it by that share, one factor per country computed from the average class size and the average number sampled.

Sojourner (2013) studies the same problem under random assignment of students to classes, with the peer variable observed for only part of each class. He decomposes each student's true peer mean into the mean of the observed peers, weighted by the share of peers observed, and the mean of the unobserved peers, weighted by the complement. Random assignment implies that the expected mean of the unobserved peers is the same in every class of a school, whatever the observed peers look like, so the unobserved peers contribute to the outcome only through a school-specific constant multiplied by the unobserved share. He therefore regresses the outcome on the observed peers' mean multiplied by the share observed, controlling for that share interacted with school indicators, which absorb the unobserved peers' contribution

[64] For example, if departments whose views are unusual for their college were less likely to answer, their coverage would fall short of the college's, their surveyed deviations would receive too much weight, and the estimate would be biased toward zero. In the peer setting such a pattern would surface in the falsification tests of Table G.1.

as one nuisance parameter per school. The estimator is unbiased but imprecise, because the interactions are collinear with the regressor of interest, and he trades some bias for precision by restricting the school constants to a few groups. He also shows that the naive regression on the observed peers' mean is attenuated by the share observed under random assignment, and that rescaling it, as Ammermueller and Pischke (2009) do, removes most but not all of the bias when the observed share differs across schools.

Our adjusted treatment contains both corrections as special cases and departs from them in two respects. The first is the reason Section H.5.1 exists. Both papers study groups formed by random assignment, by design in one case and by institutional practice in the other, where the residual composition of a group is the idiosyncratic draw of the individuals assigned to it, and both accordingly impose $\rho = 0$: the attenuation is then the coverage share, and the correction requires nothing beyond that share. Our peer measure is in the same situation, since (A1) implies $\rho = 0$ for it (Section H.4). Departments are not formed by random draw. Faculty are hired into them and share their orientation, so a department's deviation from its prediction is in large part something the department shares, $\hat{\rho} = 0.43$, and a correction by the coverage share alone would inflate the estimate by a factor of about 1.6. The weight on the observed deviation must therefore combine coverage with a reliability, and the reliability has to be estimated; the split-half design of Section H.5.1 does so from the survey itself.

The second departure is where the prediction of the unobserved part comes from. In Sojourner (2013) it is a constant for each school, estimated as a nuisance parameter; in our construction it is the fitted value of the specification's own fixed effects and controls. The difference matters because of the size of our conditioning set. His approach transported to our specification would require one parameter for every college-by-cohort-by-broad-field cell, every major-by-cohort cell and every individual control, each interacted with coverage: thousands of collinear nuisance parameters in place of the 75 in his data. Building the prediction into the regressor uses the same information at no cost in parameters, lets the weight vary across departments with their coverage, which runs from 0.19 to 0.71 across the middle 80 percent of departments, rather than applying one factor to all as Ammermueller and Pischke (2009) do, and allows the same specification and conditioning set to serve the outcome regression and the falsification tests. The role that random assignment plays in both papers is played here by (A2): the faculty we observe are a random draw of their department, and the fixed effects absorb any difference between surveyed and unsurveyed faculty that is common to a college and broad field, the counterpart of the school-level differences both papers allow.

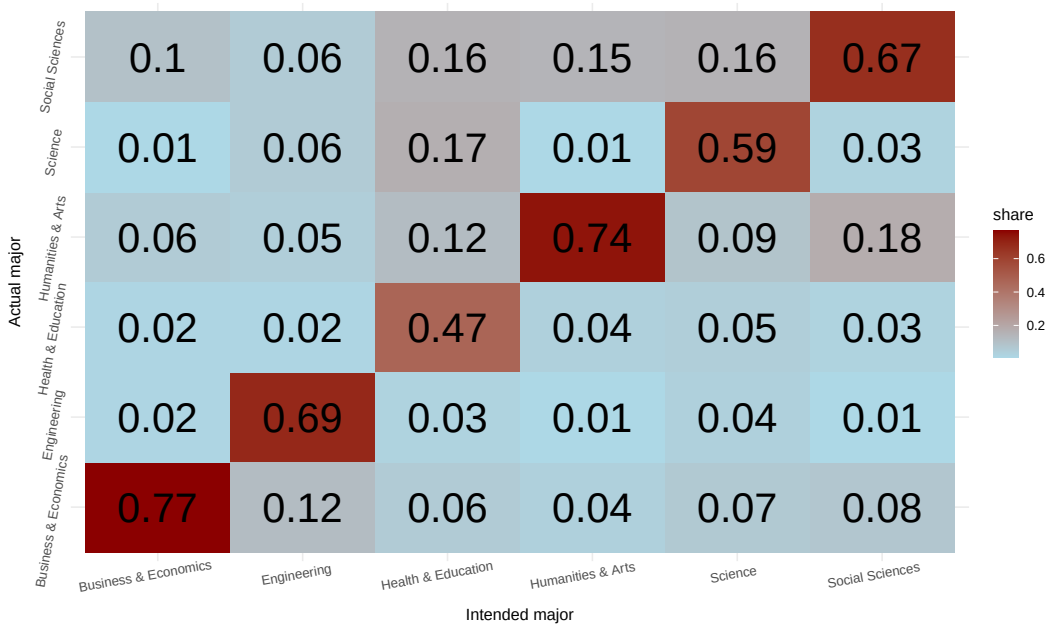
The three approaches coincide when coverage is similar across groups, the conditioning set is small, and groups carry no common component: rescaling the coefficient, controlling for the share observed, and our adjusted treatment then deliver essentially the same answer.

They separate on all three counts in our faculty setting. The first two are differences of degree. The third is unique to our setting: in both papers the groups are formed by random assignment, so the common component is zero by design, whereas departments carry one, so the reliability has to be estimated, the new input of Section H.5.1.

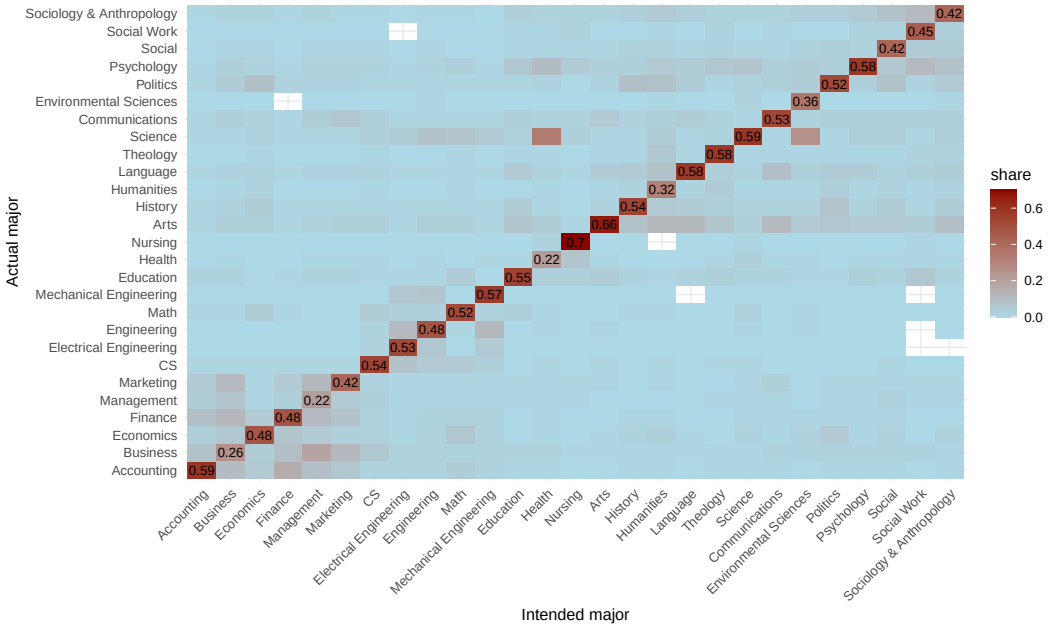
Appendix I Additional Figures and Tables

Figure I.1: Intended and Actual Majors

(A) Broad Groups

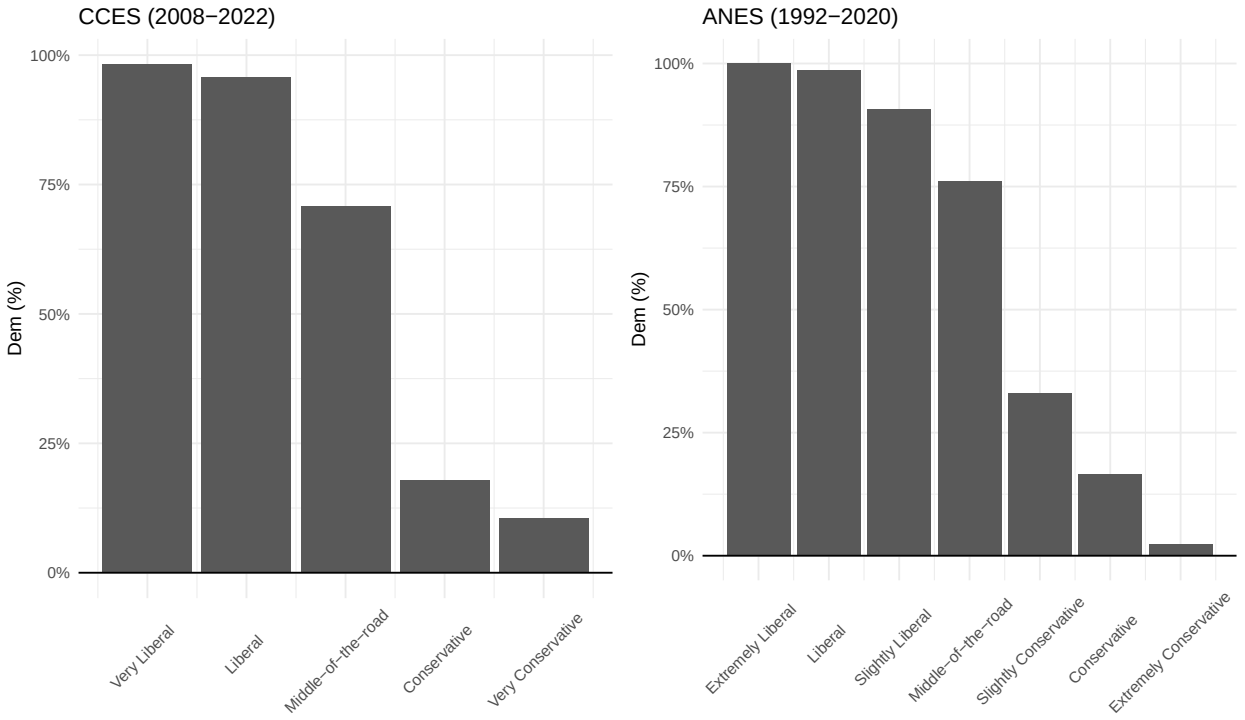


(B) Detailed Groups



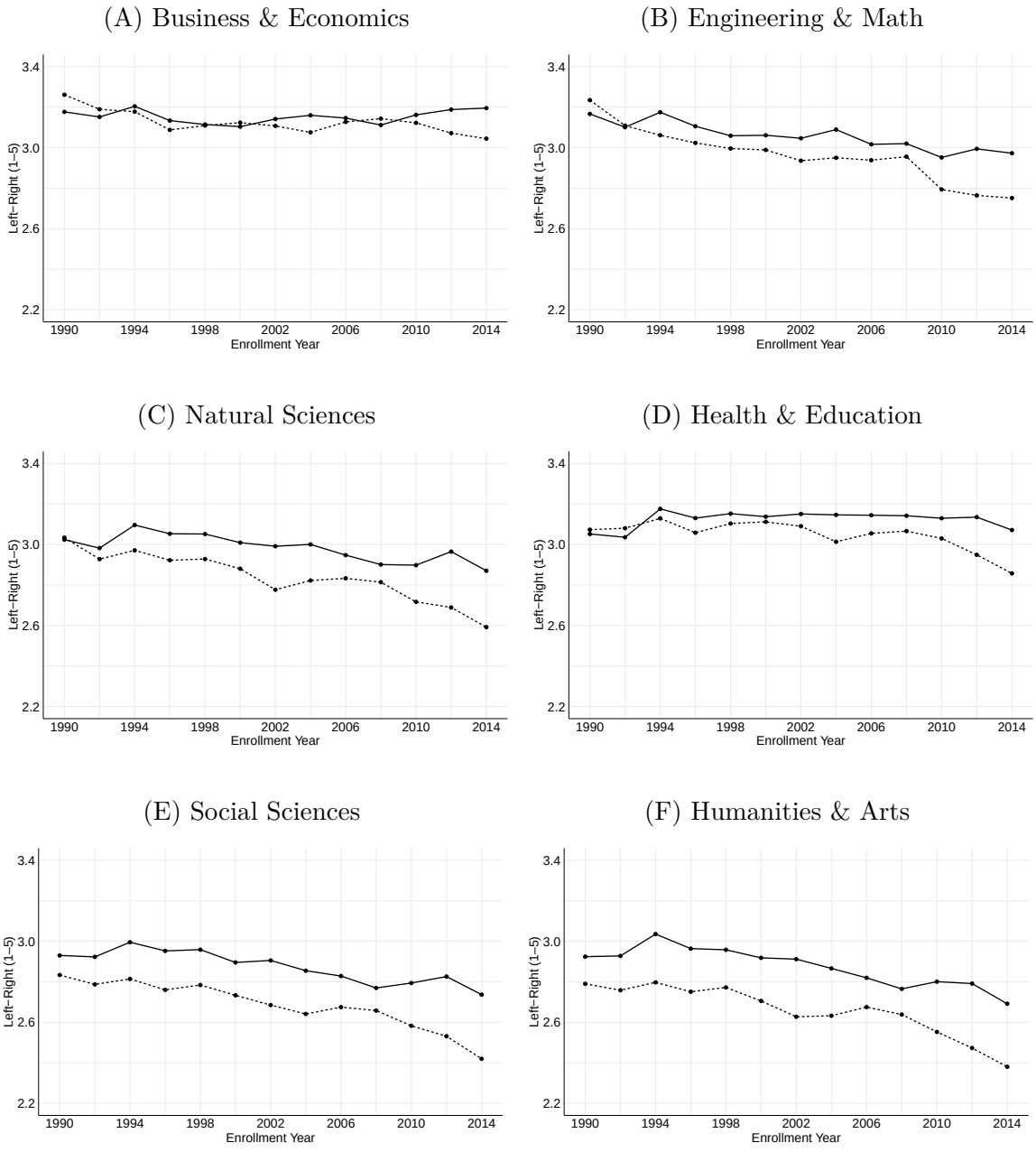
Note: This figure plots students' intended and actual majors. The sample includes all students in our sample with non-missing values for this variable, both in the entry and exit surveys (272,800 students). The first (second) panel shows the results in broad (detailed) major groups.

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



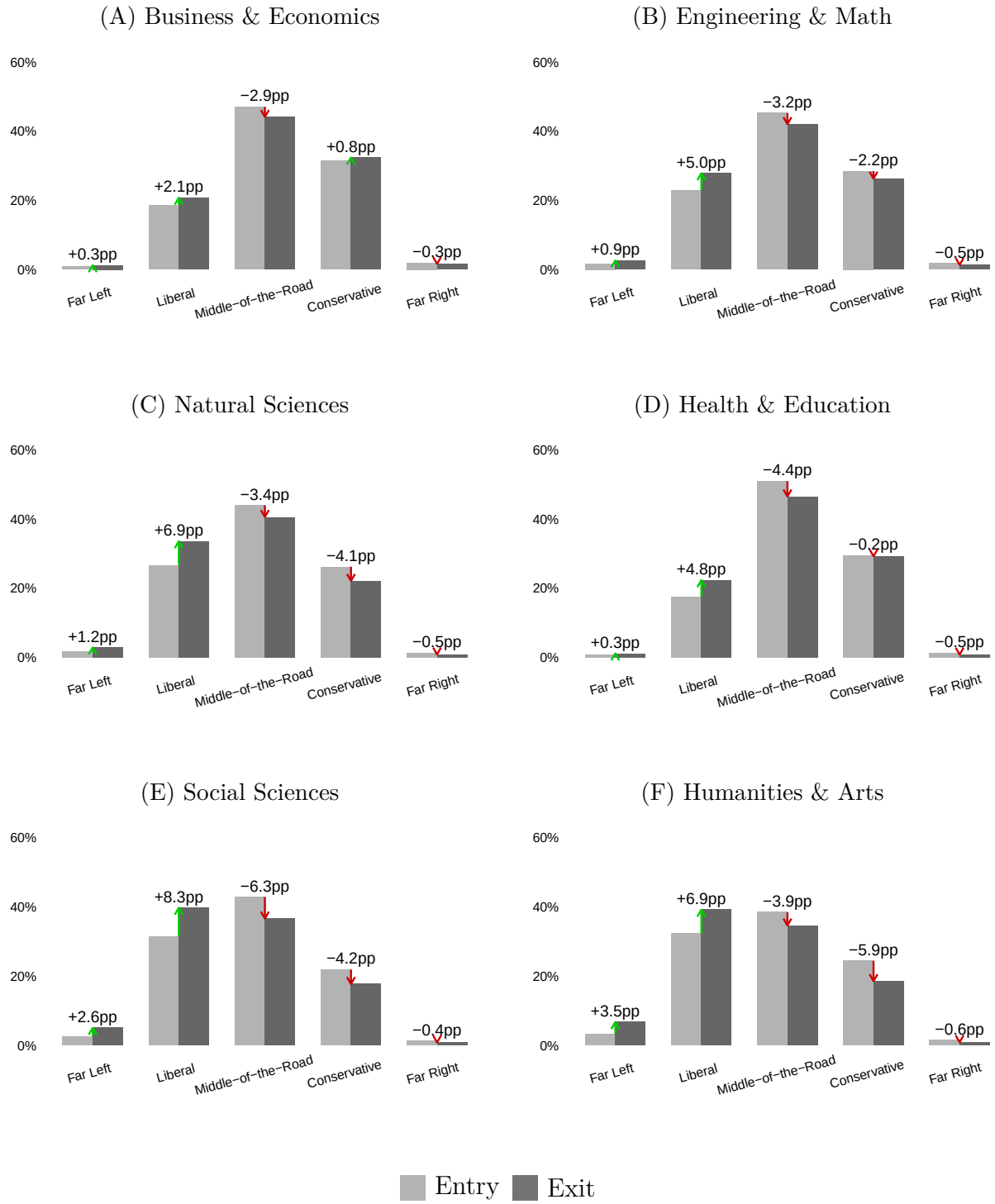
Note: This figure plots the share of students voting for the Democratic candidate in presidential elections at each ideological self-placement, using student respondents in the CCES (n=11,570) and the ANES (n=345). Both surveys include an ideology scale and a presidential-vote question; The CCES asks, "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 CCES panel pools the 2008–2022 survey waves, in which respondents report their vote in the most recent presidential election: 2008, 2012, 2016 or 2020. The 2006 and 2007 waves are excluded because the cumulative file records no presidential vote for them, and the data end with the 2022 wave. The ANES panel covers the 1992–2020 presidential elections. 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 (2006–2022); American National Election Studies (2026).

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



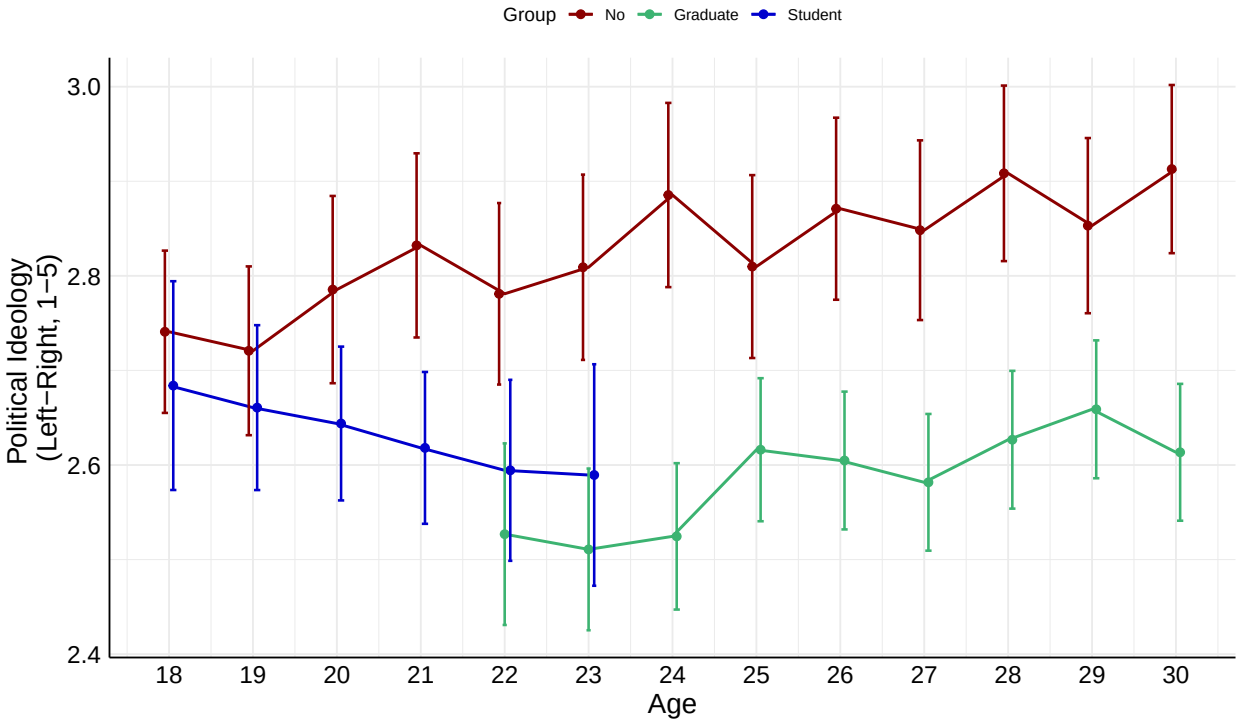
Note: This figure plots students' political ideology at college entry (solid lines) and at graduation (dashed lines). The sample includes all students who entered between 1990 and 2015 and have non-missing ideology measures in both the entry and exit surveys (293,523 students). Political ideology is 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 I.4: Political Ideology Distributions Before and After College



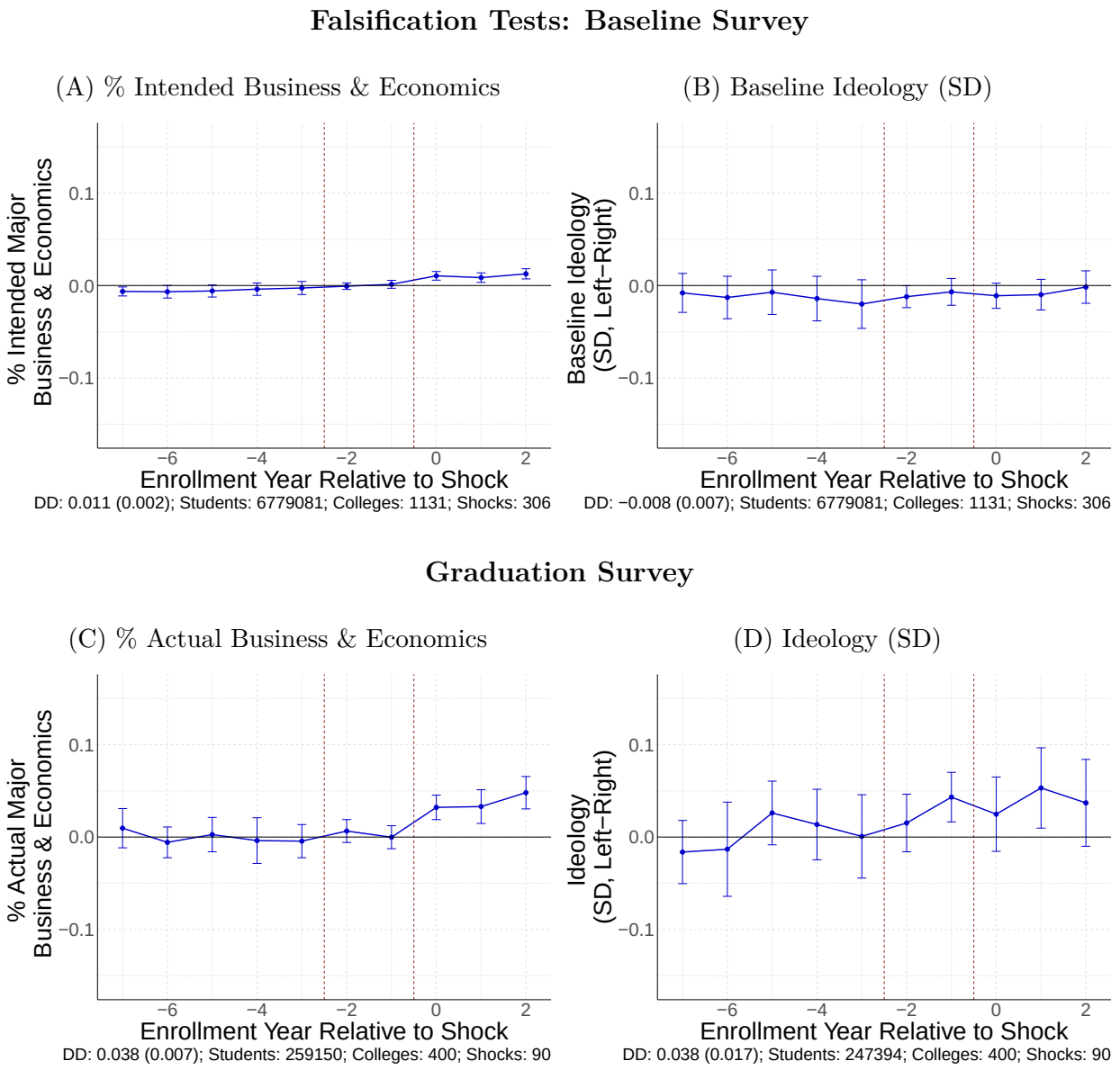
Note: This figure plots the distribution of students' baseline (light grey) and graduation (dark grey) political ideology. The sample includes all students with non-missing values for this outcome in the baseline and graduation surveys, totaling 293,523 students. The samples in Panels A–F are restricted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

Figure I.5: Political Ideology by Age, Students and Non-Students, CCES Survey



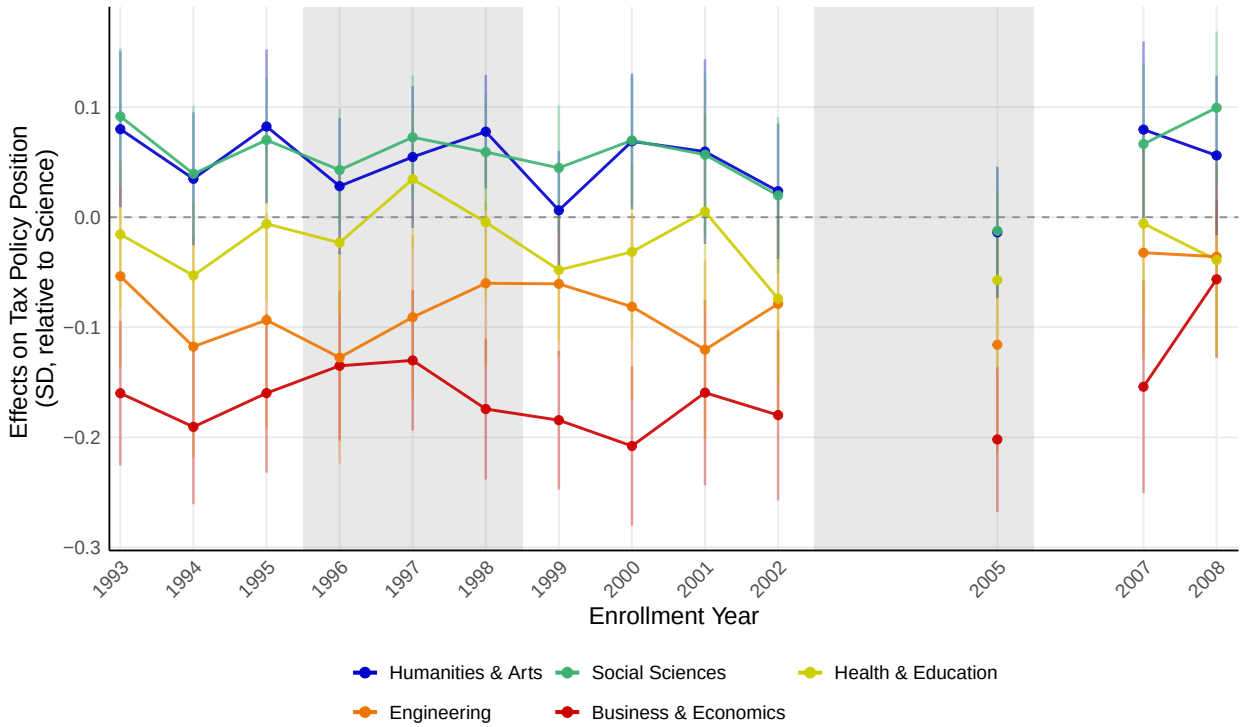
Note: This figure shows political ideology by age for college students (blue), college graduates (green) and individuals with no college education (high-school education or less, in red) in the CCES. Ideology is based on responses to "In general, how would you describe your own political viewpoint?" 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 I.6: 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. 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. Business/economics coefficients are in probability units, so 0.01 corresponds to one percentage point.

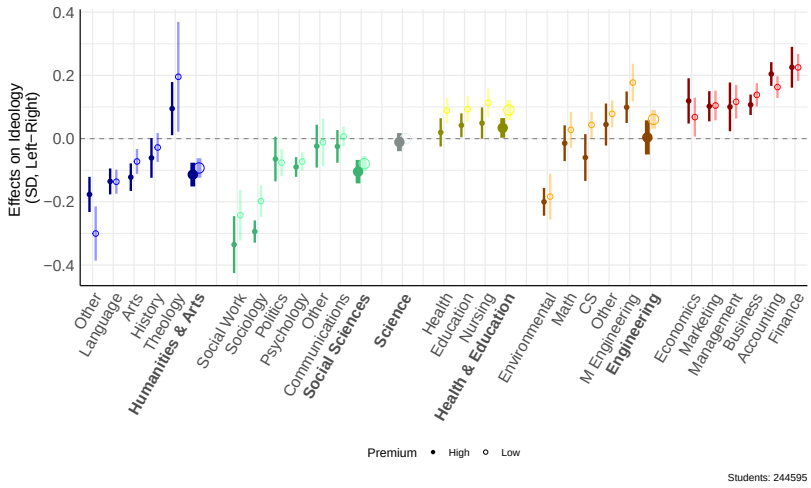
Figure I.7: Year-by-Year Effects of College Majors on Tax Support



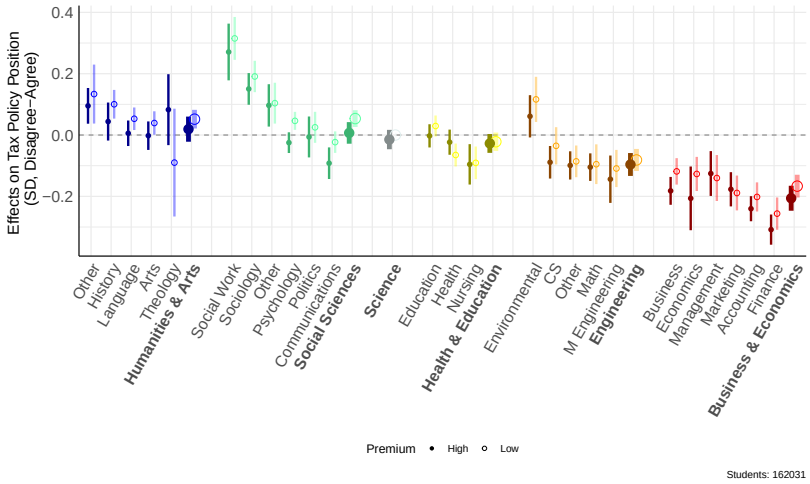
Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals, from a specification that groups majors into six broad categories, with the natural sciences as the omitted reference group. The outcome variable is students' support for higher taxes on the 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 I.8: Heterogeneity of the Effects of College Majors, by Earnings Expectations

(A) Political Ideology

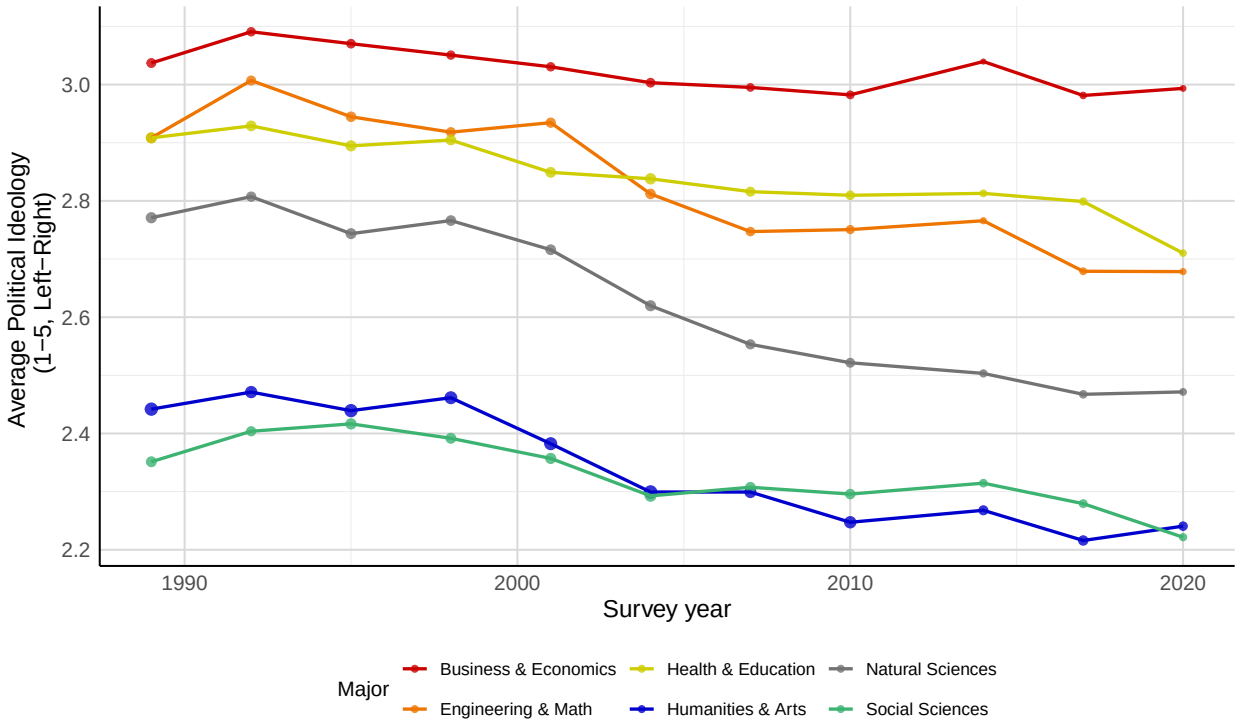


(B) Tax Policy Position



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 I.4).

Figure I.9: Faculty Ideology Over Time



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

Table I.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	Computer Science	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

Notes: 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 I.2: Distribution of Double Majors for Cohorts Enrolled Since 2005

First Major	Second Major	Students	Share (%)
Business & Economics	None	16,944	63.7
Business & Economics	Business & Economics	5,554	20.9
Business & Economics	Humanities & Arts	1,523	5.7
Business & Economics	Social Sciences	1,309	4.9
Business & Economics	Engineering & Math	670	2.5
Business & Economics	Other	381	1.4
Business & Economics	Health & Education	112	0.4
Business & Economics	Natural Sciences	104	0.4
Engineering & Math	None	9,330	78.3
Engineering & Math	Engineering & Math	747	6.3
Engineering & Math	Business & Economics	623	5.2
Engineering & Math	Humanities & Arts	470	3.9
Engineering & Math	Health & Education	238	2.0
Engineering & Math	Natural Sciences	211	1.8
Engineering & Math	Social Sciences	191	1.6
Engineering & Math	Other	107	0.9
Health & Education	None	10,630	78.3
Health & Education	Health & Education	1,269	9.4
Health & Education	Humanities & Arts	704	5.2
Health & Education	Social Sciences	368	2.7
Health & Education	Other	192	1.4
Health & Education	Natural Sciences	167	1.2
Health & Education	Business & Economics	119	0.9
Health & Education	Engineering & Math	119	0.9
Humanities & Arts	None	14,862	63.5
Humanities & Arts	Humanities & Arts	4,264	18.2
Humanities & Arts	Social Sciences	2,149	9.2
Humanities & Arts	Health & Education	946	4.0
Humanities & Arts	Business & Economics	509	2.2
Humanities & Arts	Other	287	1.2
Humanities & Arts	Natural Sciences	230	1.0
Humanities & Arts	Engineering & Math	155	0.7
Natural Sciences	None	12,883	75.8
Natural Sciences	Humanities & Arts	1,068	6.3
Natural Sciences	Natural Sciences	1,002	5.9
Natural Sciences	Social Sciences	870	5.1
Natural Sciences	Engineering & Math	431	2.5
Natural Sciences	Health & Education	308	1.8
Natural Sciences	Business & Economics	250	1.5
Natural Sciences	Other	173	1.0
Social Sciences	None	20,828	67.0
Social Sciences	Humanities & Arts	3,925	12.6
Social Sciences	Social Sciences	3,644	11.7
Social Sciences	Business & Economics	998	3.2
Social Sciences	Other	660	2.1
Social Sciences	Health & Education	550	1.8
Social Sciences	Natural Sciences	369	1.2
Social Sciences	Engineering & Math	117	0.4

Notes: 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, and "Other" a second major in one of the fields excluded from the analysis sample (Table I.1). "Share" gives each combination's percentage of students within the first major.

Table I.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	251,154	Base	51.8%	51.7%	52.6%	39.8%	53.2%	53.6%	55.9%
		Grad	61.9%	60.9%	60.8%	46.6%	64.6%	64.2%	67.7%
Addressing global climate change should be a federal priority	28,074	Base	67.6%	60.4%	64.0%	62.2%	71.8%	70.9%	72.4%
		Grad	78.0%	71.6%	78.8%	67.5%	81.2%	84.5%	81.7%
A national health care plan is needed to cover everybody's medical costs	148,983	Base	68.2%	63.4%	62.6%	71.2%	70.4%	66.0%	72.5%
		Grad	68.1%	57.2%	60.2%	70.7%	72.9%	69.2%	75.0%
It is important to have laws prohibiting homosexual relationships	195,189	Base	26.8%	31.3%	29.0%	33.2%	22.9%	25.7%	22.5%
		Grad	18.9%	23.6%	21.0%	25.1%	15.1%	17.2%	14.8%
Wealthy people should pay a larger share of taxes than they do now	196,413	Base	59.6%	51.6%	61.2%	62.6%	61.6%	61.7%	61.6%
		Grad	60.7%	47.6%	58.4%	61.3%	66.8%	63.0%	66.5%
Marijuana should be legalized	228,202	Base	28.4%	28.7%	28.1%	17.8%	32.6%	26.4%	31.5%
		Grad	43.9%	42.9%	44.0%	27.9%	49.5%	43.2%	48.8%
The death penalty should be abolished	210,921	Base	32.5%	25.9%	29.4%	28.9%	40.0%	31.7%	35.3%
		Grad	39.9%	30.6%	35.8%	31.6%	50.7%	38.7%	45.5%
The federal government should do more to control the sale of handguns	155,369	Base	83.9%	82.7%	78.1%	86.6%	84.2%	82.5%	86.2%
		Grad	82.2%	80.0%	74.8%	84.8%	83.6%	81.3%	84.8%
There is too much concern in the courts for the rights of criminals	146,507	Base	67.5%	71.9%	69.6%	71.1%	61.5%	69.3%	64.9%
		Grad	55.2%	63.4%	59.5%	62.6%	46.7%	57.2%	48.4%
Racial discrimination is no longer a major problem in America	269,899	Base	17.1%	20.6%	20.7%	16.9%	14.6%	17.2%	14.7%
		Grad	13.4%	18.5%	18.4%	11.8%	10.7%	13.4%	10.1%
Realistically, an individual can do little to bring about changes in our society	209,424	Base	22.5%	26.7%	26.4%	21.7%	19.4%	21.4%	20.8%
		Grad	24.1%	28.4%	30.9%	20.7%	22.2%	24.6%	20.8%
Colleges should prohibit racist/sexist speech on campus	191,753	Base	63.4%	62.5%	59.5%	70.1%	61.3%	63.5%	63.7%
		Grad	53.9%	51.9%	47.1%	59.5%	53.9%	53.5%	55.5%

Notes: 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 I.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 I.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

Notes: 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 I.5: Effects of College-Level Shocks on Additional Outcomes

A. Major and Course Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	% Engineering & Math Major	% Natural Sciences 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 majoring and course-taking outcomes are estimated without a lagged baseline control; the liberal, middle-of-the-road, and conservative indicators control for the student's baseline ideology response, with a separate fixed effect for each discrete value; the policy-position index controls for the baseline index in half-standard-deviation bins. 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 Figure 4. 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 mean of the outcome among untreated students (at colleges without a shock or in cohorts entering before it), and the number of shocks, colleges, and student observations in the pooled estimation sample. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table I.6: Falsification Test: "Effects" of Faculty Ideology on Students' Baseline Ideology

Panel A. Full Baseline Survey			
	(1)	(2)	(3)
Estimate (Unadjusted)	0.357***	0.012***	0.010***
	(0.014)	(0.002)	(0.002)
Estimate (Adjusted)		0.019***	0.016***
		(0.004)	(0.004)
Reliability ($\hat{\rho}$)		0.41	0.41
Mean coverage ($\bar{\alpha}$)	0.34	0.34	0.34
Mean weight ($\bar{k}$)		0.61	0.61
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 (Unadjusted)	0.388***	0.006	0.003
	(0.020)	(0.006)	(0.005)
Estimate (Adjusted)		0.010	0.006
		(0.009)	(0.008)
Reliability ($\hat{\rho}$)		0.42	0.41
Mean coverage ($\bar{\alpha}$)	0.37	0.37	0.37
Mean weight ($\bar{k}$)		0.63	0.63
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 (Unadjusted)	0.413***	-0.005	-0.006
	(0.026)	(0.009)	(0.009)
Estimate (Adjusted)		-0.005	-0.006
		(0.014)	(0.013)
Reliability ($\hat{\rho}$)		0.44	0.44
Mean coverage ($\bar{\alpha}$)	0.41	0.41	0.41
Mean weight ($\bar{k}$)		0.67	0.67
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 baseline ideology, on the common scale described below in all three panels, 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 are preferred as they correct for attenuation bias as described in Appendix H. Column (1) of each panel reports no adjusted estimate, as it omits the fixed effects on which the adjusted treatment's assumptions are conditioned. All ideology measures (students' baseline and graduation ideology, peer averages, and faculty ideology) are on one scale: the five-point self-placement (1 = far left, 5 = far right) minus the mean (2.91) and divided by the standard deviation (0.84) of the graduation-survey responses, so one unit is one standard deviation of graduating students' ideology. Faculty answer the same five-point question, coded in reverse by the faculty survey and recoded here so that 1 = far left; departmental ideology is the mean answer of the department's surveyed faculty. Departments enter with at least 3 surveyed faculty, at colleges where the faculty survey covers at least 10 percent of instructional staff; across the 22,127 such college-by-department cells, one standard deviation of departmental ideology is 0.66 units. "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%. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table I.7: Effects of College Majors on Additional Policy Positions

	Policy Positions									
	(1) Taxes	(2) Race	(3) Death	(4) Guns	(5) Marijuana	(6) LGBTQ	(7) Criminal	(8) Change	(9) Climate	(10) Speech
Business &	-0.17***	0.07***	-0.04***	-0.02	0.01	0.07***	0.03**	0.02*	-0.21***	-0.01
Economics	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Engineering &	-0.08***	0.07***	-0.01	-0.05***	0.00	0.01	0.03**	0.05***	-0.10***	-0.04***
Math	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Health &	-0.03***	-0.04***	-0.04***	0.05***	-0.10***	0.05***	0.02	-0.05***	-0.20***	0.02*
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Social	0.05***	-0.12***	0.09***	0.04***	0.09***	-0.06***	-0.13***	-0.09***	-0.08***	0.03***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Humanities &	0.05***	-0.10***	0.12***	0.06***	0.09***	-0.08***	-0.10***	-0.04***	-0.10***	0.01
Arts	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
N	205,167	278,065	218,041	160,770	235,668	200,751	150,932	216,454	63,137	242,609
Summary Statistics (raw scale)										
Graduation Mean	2.70	1.71	2.25	3.19	2.23	1.78	2.66	1.98	3.03	2.65
Graduation SD	0.96	0.77	1.02	0.90	1.06	1.00	0.84	0.83	0.91	1.03
Change in Omitted Group	0.04	-0.12	0.18	-0.04	0.41	-0.28	-0.21	0.10	0.40	-0.20

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. All columns are policy positions, originally coded on a 1–4 scale (1=Disagree Strongly, 2=Disagree Somewhat, 3=Agree Somewhat, 4=Agree Strongly): Wealthy people should pay a larger share of taxes than they do now (Taxes); Racial discrimination is no longer a major problem in America (Race); 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). Higher values indicate stronger agreement with the statement. 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. Estimates for the policy-position index, abortion and healthcare appear in Table 3. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table I.8: Effects of College Majors on the Components of the Social Index and Additional Life Goals

	Social Index Components					Additional Goals		
	(1) Helping	(2) Social Values	(3) Racial Underst.	(4) Community	(5) Political Struct.	(6) Environment	(7) Politics	(8) Community Leader
Business &	-0.09***	0.08***	-0.03**	0.00	0.09***	-0.18***	0.14***	0.07***
Economics	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Engineering &	-0.14***	-0.06***	-0.10***	-0.12***	-0.03***	-0.20***	-0.03**	-0.09***
Math	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
Health &	0.09***	0.17***	0.09***	0.03**	0.07***	-0.24***	0.02**	0.05***
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Social	0.10***	0.34***	0.21***	0.15***	0.32***	-0.18***	0.25***	0.15***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Humanities &	-0.02	0.22***	0.19***	0.07***	0.17***	-0.20***	0.17***	0.06***
Arts	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	308,679	308,628	308,384	308,262	308,782	262,942	291,844	291,673
Summary Statistics (raw scale)								
Graduation Mean	3.06	2.53	2.35	2.23	1.91	2.08	2.43	2.29
Graduation SD	0.78	0.90	0.94	0.90	0.90	0.85	0.92	0.94
Change in Omitted Group	0.08	0.10	0.08	0.07	0.04	0.14	0.13	0.07

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. All columns are stated life goals, originally coded on a 1–4 scale (1=Not important, 2=Somewhat important, 3=Very important, 4=Essential). Columns (1)-(5) are the five goals that make up the social index in column (5) of Table 3: helping others in difficulty (Helping); influencing social values (Social Values); promoting racial understanding (Racial Underst.); participating in a community action program (Community); and influencing the political structure (Political Struct.). Columns (6)-(8) are three further life goals that are not components of the index: becoming involved in programs to clean up the environment (Environment), excluded because it is fielded only through the 2010 entry cohort; keeping up-to-date with political affairs (Politics); and becoming a community leader (Community Leader), whose specification additionally controls flexibly for the corresponding baseline goal, which unlike the other goals here is not part of the main control set. 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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table I.9: Effects of College Majors on Additional Behaviors

	Behaviors						
	(1) Vote Reg.	(2) Discussed Politics	(3) Discussed Course	(4) Pol. Demonstr.	(5) Voted (Student)	(6) Voted (Nat./State)	(7) Religious Att.
Business &	-0.01	0.04***	-0.16***	-0.01	-0.03	-0.04	-0.01
Economics	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)
Engineering &	-0.02***	-0.06***	-0.00	-0.07**	-0.03	-0.04*	-0.02
Math	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
Health &	-0.00	-0.03***	-0.02*	0.06**	-0.06***	0.06**	0.08***
Education	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)	(0.03)	(0.01)
Social	0.01***	0.24***	-0.14***	0.22***	0.03**	0.07***	-0.03***
Sciences	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Humanities &	0.01**	0.18***	-0.07***	0.14***	-0.01	0.04	-0.01
Arts	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
N	90,750	281,267	315,905	27,102	174,138	39,745	311,720
Summary Statistics (raw scale)							
Graduation Mean	0.89	1.95	2.65	1.17	1.88	2.15	1.97
Graduation SD	0.32	0.66	0.52	0.42	0.74	0.76	0.79
Change in Omitted Group		-0.18			-0.04		-0.42

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. Column (1) is an indicator for being registered to vote (Vote Reg.). Columns (2)-(7) are behavioral frequencies, originally coded on a 1–3 scale (1=Not at all, 2=Occasionally, 3=Frequently): discussed politics (Discussed Politics); discussed course content with students outside of class (Discussed Course); participated in a political demonstration (Pol. Demonstr.); voted in a student election (Voted (Student)); voted in a national or state election (Voted (Nat./State)); and attended a religious service (Religious Att.). All columns also control for the baseline behaviors listed in Table I.4, and the religious-attendance specification additionally controls flexibly for the student's religious denomination reported at entry. 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 they report the raw share of students registered to vote and its standard deviation in the sample. "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, and for Discussed Course, Pol. Demonstr. and Voted (Nat./State), for which the baseline survey has no analogous question. Participation in organized demonstrations (Demonstrated) and volunteering appear in Table 3; Pol. Demonstr. is a different question, asked only in the 2007 and 2008 graduation surveys, whereas Demonstrated is asked in 1994–2006. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table I.10: Effects of College Majors on Career Priorities

	Index	Extrinsic Rewards						Intrinsic Rewards			
	(1) Index	(2) Income	(3) Status	(4) Security	(5) Jobs	(6) Leadership	(7) Hours	(8) Social Change	(9) Values	(10) Creativity	(11) Knowledge
Business &	0.17***	0.37***	0.19***	0.06**	-0.01	0.27***	0.04	-0.12***	0.05*	0.06**	-0.13***
Economics	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Engineering &	0.12***	0.16***	-0.03	0.02	-0.09**	0.04	-0.01	-0.19***	-0.10***	0.02	-0.20***
Math	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.06)
Health &	-0.14***	-0.04*	-0.02	-0.01	0.07**	0.08***	-0.01	0.11***	0.15***	0.13***	-0.06
Education	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
Social	-0.21***	0.04*	0.07***	-0.02	-0.04*	0.13***	-0.02	0.33***	0.19***	0.10***	-0.18***
Sciences	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Humanities &	-0.35***	-0.05**	0.01	-0.12***	-0.11***	0.04	-0.03	0.17***	0.30***	0.36***	-0.14***
Arts	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
N	26,562	26,715	26,677	26,683	26,657	26,656	26,611	26,690	26,676	26,679	12,288
Summary Statistics (raw scale)											
Graduation Mean		2.68	2.27	3.25	2.99	2.81	2.09	2.51	2.93	2.84	3.04
Graduation SD		0.92	0.89	0.75	0.78	0.88	0.85	0.96	0.82	0.86	0.84

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. All columns rate how important a consideration is in choosing a career, originally coded on a 1–4 scale (1=Not important, 2=Somewhat important, 3=Very important, 4=Essential): high income potential (Income); social recognition or status (Status); a stable and secure future (Security); availability of jobs (Jobs); leadership potential (Leadership); limited working hours (Hours); working for social change (Social Change); expression of personal values (Values); creativity and initiative (Creativity); and discovery or enhancement of knowledge (Knowledge). The index in column (1) is the average of the four extrinsic items (Income, Status, Security and Jobs) minus the average of the three intrinsic items (Social Change, Values and Creativity), so that higher values indicate a more extrinsic orientation; Leadership, Hours and Knowledge are reported but are not part of it. These items are fielded only in the 2007 and 2008 graduation surveys, so the sample is restricted to the corresponding entry cohorts and is much smaller than in the other tables; Knowledge is asked in 2008 only. 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; they are blank for the index, which has no raw scale. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table I.11: Effects of College Majors on Time Use

	Hours per Week						
	(1) Studying	(2) Socializing	(3) Leisure	(4) Commute & Home	(5) Paid Work	(6) Volunteering	(7) Career Planning
Business &	-2.52***	2.02***	1.01***	0.17**	1.21***	-0.33***	0.67***
Economics	(0.13)	(0.23)	(0.10)	(0.08)	(0.30)	(0.05)	(0.11)
Engineering &	0.45*	0.59***	0.03	-0.05	0.02	-0.43***	0.07
Math	(0.26)	(0.13)	(0.08)	(0.08)	(0.22)	(0.05)	(0.13)
Health &	-1.12***	-0.06	0.46***	0.75***	0.46**	-0.01	0.20
Education	(0.15)	(0.22)	(0.10)	(0.11)	(0.20)	(0.06)	(0.13)
Social	-2.33***	1.26***	0.46***	0.31***	1.14***	0.35***	0.63***
Sciences	(0.12)	(0.12)	(0.07)	(0.07)	(0.17)	(0.05)	(0.10)
Humanities &	-1.23***	1.35***	-0.01	-0.04	0.28	-0.09**	0.17*
Arts	(0.15)	(0.17)	(0.09)	(0.08)	(0.19)	(0.04)	(0.09)
N	314,065	311,719	282,039	275,195	313,815	192,962	27,128
Summary Statistics (raw scale)							
Graduation Mean	11.32	16.79	10.29	3.05	9.96	1.65	2.48
Graduation SD	7.08	11.18	8.15	5.23	8.99	3.28	3.70

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. All columns are hours per week, so coefficients, Graduation Mean and Graduation SD are all in hours. Students choose one of eight bands (None, less than 1, 1–2, 3–5, 6–10, 11–15, 16–20, over 20), converted to their midpoints (0, 0.5, 1.5, 4, 8, 13, 18, 25). Studying is homework and studying; Socializing is time with friends plus partying; Leisure is exercise or sports plus watching television; Commute & Home is commuting plus household and childcare duties; Paid Work combines the single paid-work item asked through the 2000 entry cohort with the sum of the on-campus and off-campus items asked from 2001; Volunteering is volunteer work; and Career Planning is career planning, job searches and networking. Each aggregate requires all of its components to be observed, so the cohorts covered differ across columns while the composition of a column never changes within its own sample. The specification additionally controls for the student's time use in the last year of high school. Column (6) measures hours spent volunteering, and is distinct from the volunteering frequency reported in Table 3: the two come from different survey questions asked of largely different cohorts (hours in 1990-2006, frequency in 2003-2015) and correlate 0.42 among the students who answer both. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.