Changing Minds: How Academic Fields Shape Political Attitudes

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2 December 2025

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Abstract

College education is a key determinant of political attitudes in the United States and other countries. This paper highlights an important source of variation among college graduates: studying different academic fields has sizable effects on their political attitudes. Using surveys of about 300,000 students across 477 U.S. colleges, we find several results. First, relative to the natural sciences, studying the social sciences and humanities tends to make students more left-leaning, whereas studying economics and business makes them more right-leaning. Second, the rightward effects of economics and business come from shifts on economic policy issues (taxation, healthcare), whereas the leftward effects of the humanities and social sciences come from shifts on cultural issues (LGBTQ, race). Third, these effects extend to behavior: the social sciences and humanities increase activism, while economics and business increase the emphasis on financial success. Fourth, the effects operate through teaching rather than socialization or earnings expectations. Finally, the implications are substantial. If all students majored in economics or business, the college–noncollege ideological gap would shrink by about 30 percent. A uniform-major scenario, in which everyone studies the same field, would reduce ideological variance and the gender gap among graduates. Together, the results show that academic fields shape students' attitudes and that field specialization contributes to political fragmentation.

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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."
The Apology of Socrates

1 Introduction

There is a growing political divide between those with and without college degrees in the United States and other advanced democracies (Gethin et al., 2022; Grossmann and Hopkins, 2024). Yet "college graduates" are not a homogeneous group. A defining characteristic of college education is its intellectual diversity: students sort into distinct academic fields that offer different ideas, methodological approaches, and normative frameworks. These differences raise a central question for understanding the politics of the college-educated: Do academic fields shape students' political attitudes? If so, through which mechanisms, and with what broader implications for society?

This paper provides the first comprehensive causal evidence on the political effects of fields of study. Using rich panel data from the Higher Education Research Institute (HERI), we examine how academic fields influence students' political ideology, policy positions, behaviors, and life goals as they move from college entry to graduation. We estimate plausibly causal effects of college majors by comparing students' outcomes across majors while controlling for observables (lagged outcomes) and proxies for unobservables (initial major preferences). We further assess the robustness of our findings with sensitivity analyses that quantify the strength of unobserved selection needed to overturn our results, and we complement our main estimates with quasi-experimental evidence from supply-driven expansions in specific fields. We also investigate the mechanisms underlying these effects and assess their broader implications for the college divide and polarization within the educated elite.

We begin by documenting shifts across college majors. As detailed in Section 2, the analysis focuses on roughly 300,000 students from 477 colleges for whom we observe both baseline and graduation ideology. On average, students become about seven percentage points more likely to identify as liberal or far left between entry and graduation.[1] Economics and business majors show little change, whereas social sciences and humanities majors shift more than

[1]Political ideology is measured on a five-point scale (far right, conservative, middle-of-the-road, liberal, far left), and policy positions on a four-point scale (strongly disagree to strongly agree). While concerns may arise about comparing bounded 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 the treatment. Throughout the paper, we use "liberal" and "left" or "conservative" and "right" to refer to two measurable outcomes: self-identification and correlated policy positions.

ten percentage points to the left.[2] Policy positions exhibit similar patterns. All students become more liberal on cultural issues (e.g., abortion), though the magnitude varies by field. In contrast, economic issues show divergent trends: social sciences and humanities majors become more supportive of policies such as national healthcare, while economics and business majors become less supportive.

In Section 3, we establish the significant causal role that academic fields play in shaping students' ideology. We first report plausibly causal estimates that control for an extensive set of pre-college covariates, including strong predictors of ideology (lagged ideology and policy positions), as well as variables that proxy for unobserved factors that may be correlated with both ideology and major choice (intended majors and life goals). Estimated effects are substantial. Relative to natural sciences, studying the social sciences or humanities leads students to graduate about 0.09 standard deviations further left, while studying economics or business leads them about 0.14 standard deviations further right.[3] Professional majors—engineering, health, and education—produce smaller rightward shifts of about 0.06 standard deviations.

Using a binary indicator for liberal or far-left identification yields similar patterns. Studying the social sciences or humanities increases this identification by about 4 percentage points relative to natural sciences. Studying economics or business decreases it by about 6 percentage points, and studying engineering, health, or education increases it by 2–3 percentage points. These effects are comparable across student subgroups (e.g., by gender or race) and across institutions with different selectivity levels or geographic regions.

We further support the causal interpretation of these estimates by quantifying the residual selection needed to overturn our findings (Cinelli and Hazlett, 2020). An unobserved factor as predictive of the residual outcome (conditional on all controls) as baseline ideology would need to be orders of magnitude more predictive of the residual treatment to reverse the sign of our estimates. Likewise, an unobserved factor as predictive of the residual treatment as the intended major would need to be much more predictive of the residual outcome to do so.

We also provide complementary quasi-experimental evidence from supply-driven expansions in specific fields. Using administrative data from the Integrated Postsecondary Education Data System (IPEDS), we identify years in which particular colleges experienced sudden increases in the number of slots in business and economics programs. These expansions affect students' exposure to those fields. If their timing is as good as random, we can estimate their

[2]We group economics with business rather than other social sciences, as they are more similar in their teaching and in other relevant characteristics, and—as we show—their effects are closely aligned.

[3]As a benchmark, the average ideological gap between students from New York and Texas at college entry is 0.32 standard deviations and the average baseline gender gap in ideology is 0.14 standard deviations.

effects on student outcomes using staggered difference-in-differences methods (Gardner, 2022; Borusyak et al., 2024). We support the quasi-random timing with two pieces of evidence. First, anecdotal evidence indicates that such expansions are often triggered by external events, such as donations. Second, a falsification test using students' baseline ideology shows no relationship between expansion timing and this key placebo outcome.

Estimating the effects of these expansions, we find that they increase enrollment in business and economics by about four percentage points, mainly drawing students from other social sciences. They also shift graduation ideology rightward by roughly 0.05 standard deviations—that is, about 3 percentage points fewer students identifying as left, 2 percentage points more identifying as right, and 1 percentage point more identifying as middle-of-the-road. These magnitudes are not directly comparable to the main estimates, as the shocks may also affect experiences beyond major choice (e.g., non-major coursework). Nonetheless, the results reinforce that academic fields influence students' political ideology and provide policy-relevant evidence on the political consequences of changes in field availability.

Then, in Section 4, we extend our analysis to policy positions, political behaviors, and life goals. We find that academic fields drive a sharp divergence in economic policy views. Specifically, majoring in business or economics induces a rightward shift on issues such as taxation, whereas the social sciences and humanities lead students to the left. While all college students become more liberal on cultural issues (e.g., abortion), field choice differentially affects the magnitude of this shift, though to a lesser degree than for economic issues. A decomposition (following Gelbach, 2016) reveals distinct pathways: the ideological effects of business and economics stem primarily from changes in economic positions, while the effects of social sciences and humanities are driven largely by shifts in cultural views.

Humanities and social sciences also increase participation in demonstrations and the likelihood of being registered to vote. In contrast, relative to natural sciences, studying economics and business leads students to place significantly greater emphasis on financial success—by 0.32 standard deviations—and less emphasis on helping others—by 0.10 standard deviations. These patterns suggest a potential channel for the economic returns to these majors through shifts in career preferences, consistent with evidence that studying economics increases not only students' actual employment in, but also their preferences for, higher-paying industries (Bleemer and Mehta, 2022).

Section 5 explores the mechanisms underlying the effects of academic fields on attitudes. We first provide evidence that peer ideology and earnings expectations are unlikely to drive the effects. Although students' graduation ideology is strongly correlated with that of their peers, we show that within-programs, over-time changes in peer composition do not affect

individual outcomes. Our estimates rule out peer composition accounting for more than 10 percent of the total impact of majors on ideology.[4] Additionally, exploiting variation in earnings expectations across colleges within the same major, as well as over time in response to macro shocks that differentially affect fields, we find no heterogeneity in the effects.

Therefore, the natural channel underlying these results is teaching. We use faculty ideology—measured from faculty surveys conducted by HERI—as a proxy for this channel. We exploit variation in faculty ideology both across colleges and within colleges across similar majors. Students in departments with more left-leaning faculty do not differ in their baseline ideology. Still, they graduate with significantly more left-leaning ideology, indicating meaningful heterogeneity in effects across departments. Among majors in humanities and social sciences, a one-standard-deviation increase in departmental faculty liberalism causes 0.08 and 0.07 standard deviations larger leftward shifts in student ideology at graduation, respectively. These patterns hold conditional on baseline ideology, and because faculty ideology is uncorrelated with students' baseline ideology, they reduce concerns about selection and support the interpretation that teaching plays a key role in driving these effects.

Section 6 examines the broader consequences of our findings. First, we translate the estimated effects into expected voting behavior using external mappings from ideology to vote choice based on students in other surveys. The results imply that studying humanities or social sciences makes students about six percentage points more likely to vote Democratic than studying economics or business. For context, choosing these fields rather than business or economics is expected to have an effect comparable to growing up in a county with a 20-percentage-point higher Democratic vote share during adolescence (Brown et al., 2023).

Second, we show that the "college divide" partly reflects the subjects students study. Humanities and social sciences account for a disproportionate share of the leftward shift among college graduates. If students were to shift en masse into economics or business, the ideological shift during college would nearly vanish, and the gap between college graduates and non-graduates would shrink by about 30 percent.

Third, self-selection into majors amplifies polarization. Students with more liberal ideology tend to choose more left-leaning fields, causing graduates to be more ideologically divided than when they entered. Ideological dispersion rises during college, and we show that this widening is partly caused by field specialization. Under a uniform-major counterfactual, the increase in ideological variance would be nearly halved. Likewise, women's greater tendency

[4]This does not imply that peer effects are absent universally. Rather, changes in the average peer composition within a program do not have sizable effects in our setting. Prior studies find peer effects in contexts with more intensive exposure, such as residential settings (Strother et al., 2021; Coriale et al., 2023; Firoozi, 2023).

to major in left-leaning fields accounts for about three-quarters of the growth in the ideological gender gap during college and roughly one-sixth of the overall gap at graduation.

Together, the results in this paper highlight the central role of academic fields in shaping students' attitudes and behaviors. Our results speak to salient debates in advanced democracies about the societal implications of education. Critics often accuse colleges, and certain fields in particular, of ideological bias and indoctrination. In the United States, such claims have motivated legislative proposals in several states to restrict funding for specific academic disciplines.[5] These concerns have contributed to declining public confidence in education, especially among Republicans.[6] Such criticism is not new: it echoes ancient accusations against intellectuals for "corrupting the youth", most famously in the trial of Socrates. Conversely, the idea that education shapes moral perspectives has long been central to a tradition of intellectual thought that views education as cultivating moral autonomy and critical reasoning essential for democratic citizenship (Plato, 1992; Kant, 2002; Dewey, 1916).

The empirical evidence presented in this paper supports arguments made by both critics and advocates of education, showing that the content of academic fields plays a central role in shaping young adults' political and social perspectives. While it does not resolve the inherently normative debate, it highlights the importance of this issue. Changes in access to, or the composition of, specific fields are likely to have meaningful political consequences.

This paper also contributes to several branches of the literature. First, it complements existing evidence on the labor-market returns to fields of study (Hastings et al., 2013; Altonji et al., 2016; Kirkeboen et al., 2016; Andrews et al., 2017; Heinesen et al., 2022; Bleemer and Mehta, 2022; Andrews et al., 2024; Goldstein, 2025) by revealing the political and social effects of academic fields. Moreover, the analysis suggests that changes in preferences may be an important mechanism underlying field-specific economic returns.

Second, this paper contributes to the literature on the growing political divide between those with and without college degrees in the United States and other advanced democracies (Gethin et al., 2022; Grossmann and Hopkins, 2024). There is evidence from various countries that students become more liberal during their studies, particularly on cultural issues (Broćić and Miles, 2021; Fryer, 2022; Scott, 2022; Apfeld et al., 2023). A recent study further shows associations between students' chosen fields and their voting behavior in Europe, attributing these patterns to both self-selection into fields and educational experiences (Hooghe et al., 2024). Numerous studies have also examined economics students specifically, showing that

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

their views differ systematically from those of peers in other fields and evolve distinctly over the course of their studies (Frank et al., 1993; Rubinstein, 2006; Bauman and Rose, 2011; Hammock et al., 2016; Fischer et al., 2017; Girardi et al., 2024).

This paper makes five key contributions to this literature. First, it establishes the causal role of academic fields in shaping attitudes using a rigorous selection-on-observables approach augmented with controls that proxy for unobserved factors, together with a complementary quasi-experimental design. Second, it shows that different policy positions drive the effects of various fields. Third, it documents that effects extend to behaviors and life preferences. Fourth, it examines the mechanisms underlying these effects and finds that teaching is the main channel. Fifth, it highlights the broader implications of field specialization for expected voting, the "college divide", political polarization, and gender gaps in ideology.

More broadly, this paper is related to research on how educational content shapes students' political attitudes. While extensive social science research explores this relationship, rigorous causal evidence remains limited. Existing studies have often focused on explicitly political curricula—such as those implemented under authoritarian regimes in Nazi Germany, China, and Taiwan—to identify causal effects (Voigtländer and Voth, 2015; Cantoni et al., 2017; Chen et al., 2023). In democratic contexts, Braghieri and Eichmeyer (2024) show that variation in German history education—emphasizing either Nazi or communist repression—affects susceptibility to right- versus left-wing extremism. Similarly, Meriläinen and Mitrunen (2025) find that Marxist–Leninist instruction in Finland led students to pursue more left-leaning, civic-minded occupations with lower earnings. Our results extend this literature by showing that educational content shapes students' political attitudes even in a setting perceived as neutral and in the absence of explicit political messaging.[7]

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

[7]Relatedly, Ash et al. (2025) show that exposure to law-and-economics training for U.S. federal judges shifted judicial decisions in a more economically conservative direction, illustrating that ideas transmitted through professional education can shape political and policy outcomes even outside traditional classroom settings.

2 The Data: HERI Student Surveys

This study relies on survey data from the Cooperative Institutional Research Program (CIRP), conducted by the Higher Education Research Institute (HERI) of UCLA.[8] The survey has gathered information on incoming college students across the U.S. since the 1960s. The Freshman Survey (TFS or baseline survey) covers various topics, including family backgrounds, high school behaviors and achievements, political attitudes, college and life plans, and more. 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).[9]

The sample of colleges in the survey is not random, and participation requires colleges to pay a fee. Despite this, a substantial number of colleges across the US–more than a thousand annually–choose to pay and participate in the baseline survey, and approximately 140 participate in the senior survey, with a large overlap. Both surveys, though officially voluntary, are purposefully integrated into mandatory student processes, orientation or registration for freshmen, and graduation packets or rehearsal sessions for seniors, yielding relatively high response rates.[10]

2.1 Sample

The baseline sample includes students from the baseline survey cohorts who began their undergraduate studies between 1990 and 2015, totaling roughly 10 million students from 1,500 colleges. Of these, about 8 million attended 1,200 four-year institutions. The senior survey covers about 580,000 students from 592 four-year colleges who started their studies in the corresponding years. The main analysis focuses on approximately 310,000 students from 477 four-year colleges from specific cohorts that participated in both surveys, with non-missing data on political ideology and declared major.

Table 1 reports summary statistics for the study sample (students covered in both the freshmen and graduate surveys) and compares them to all freshmen survey respondents from four-year colleges, as well as to a weighted version representative of the college student body. The study sample appears broadly representative, with minor deviations. There is a slight overrepresentation of women, white students, students from northern states, and those from

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

[9] For illustration, Figure A1 shows a complete CSS questionnaire from 2002, representative of the surveys used throughout our study period.

[10] Comparing our main dataset to IPEDS administrative enrollment data reveals that TFS covers an average of 64 percent of incoming undergraduate cohorts, while CSS covers approximately 34 percent of students from the corresponding four-year-prior incoming cohorts. These represent similar rates conditional on college enrollment, given that roughly 40 percent of U.S. college entrants do not complete their degree.

higher socioeconomic backgrounds and with no religious affiliation. Politically, the study sample includes somewhat more conservative students—28 percent compared to 23 percent in the full sample. Differences across intended majors are small. Overall, the study sample provides wide coverage of the American college population. To assess robustness, we also examine results separately by subsamples to test whether representativeness differences affect our conclusions.

Table 1: Summary Statistics: Baseline Survey and Study Sample

	Baseline w/ wts			Baseline			Study Sample		
Variable	Mean	SD	N	Mean	SD	N	Mean	SD	N
A. Demographics									
Female	0.55	0.50	5,537,832	0.56	0.50	7,719,414	0.63	0.48	367,560
Age	18.29	0.78	5,502,138	18.54	1.93	7,680,249	18.27	0.50	365,782
Black	0.07	0.25	5,426,343	0.08	0.26	7,564,235	0.04	0.19	362,580
Hispanic	0.04	0.20	5,426,343	0.04	0.20	7,564,235	0.04	0.20	362,580
White	0.76	0.43	5,426,343	0.75	0.44	7,564,235	0.80	0.40	362,580
South	0.24	0.43	5,537,832	0.25	0.43	7,752,281	0.13	0.33	368,123
Income below 50k	0.34	0.47	4,935,258	0.36	0.48	6,848,092	0.28	0.45	329,660
Income 50-100k	0.36	0.48	4,935,258	0.36	0.48	6,848,092	0.36	0.48	329,660
Income above 100k	0.30	0.46	4,935,258	0.28	0.45	6,848,092	0.36	0.48	329,660
No Religion	0.16	0.37	5,537,832	0.16	0.37	7,752,281	0.13	0.34	368,123
B. Baseline Ideology									
Liberal or Far Left	0.30	0.46	5,199,867	0.30	0.46	7,220,160	0.28	0.45	349,908
Middle-of-the-road	0.46	0.50	5,199,867	0.47	0.50	7,220,160	0.45	0.50	349,908
Conservative or Far Right	0.23	0.42	5,199,867	0.23	0.42	7,220,160	0.28	0.45	349,908
C. Intended Majors									
Natural Sciences	0.12	0.33	4,638,744	0.12	0.32	6,447,634	0.14	0.34	314,380
Engineering & Math	0.15	0.36	4,638,744	0.15	0.36	6,447,634	0.11	0.31	314,380
Social Sciences	0.19	0.39	4,638,744	0.18	0.39	6,447,634	0.19	0.39	314,380
Business & Economics	0.19	0.39	4,638,744	0.19	0.39	6,447,634	0.20	0.40	314,380
Humanities & Arts	0.13	0.33	4,638,744	0.13	0.34	6,447,634	0.13	0.34	314,380
Health & Education	0.22	0.41	4,638,744	0.23	0.42	6,447,634	0.23	0.42	314,380

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

2.2 Key Variables

Fields of study. The data includes information on students' intended majors when they start college and their actual majors at graduation. The list of majors in the survey slightly changes over time, so we aggregate them into broader groups at two levels, as detailed in Appendix Table A1. Appendix Figure A2 illustrates the transitions between intended and actual majors, indicating that most students in our sample ultimately pursued the majors within the major category they initially preferred. The figure also shows movement between nearly all pairs of majors. Still, a significant share of the students eventually pursue the majors they intended.

Note that until 2004, the survey indicated only one major for each individual, thus missing information about double majors. Since 2005, the survey has contained information on double majors. We use this information for a subsample analysis that examines the effects of double majors. Table A2 summarizes the prevalence of double majors, showing that about 30 percent of students in the relevant cohorts pursue a double major, with variation across fields.

Political ideology. The primary outcome of interest is students' responses to the question: "How would you characterize your political views?" with a response scale from 5 (far left) to 1 (far right). For clarity, we rescaled the variable so that lower values indicate more left-leaning orientations. This rescaling is used in all subsequent analyses. This ideological measure is strongly correlated with voting behavior. Because the HERI surveys do not report party-specific voting, we rely on external survey data to measure this association. Figure A3 demonstrates that responses to a similar ideology question are highly correlated with actual voting patterns among students in two national surveys: the Cooperative Congressional Election Study (CCES, 2006–2024) and the American National Election Studies (ANES, 1948–2024). We use the mapping implied by this figure to interpret our results in terms of voting behavior in Section 6.

The trends in ideology among incoming and graduating students in our sample appear in Appendix Figure A4. This figure highlights several trends. First, incoming college students have become slightly more left-leaning over time, especially in the social sciences and humanities and arts, with the average ideology score rising from about 3.0 in 1990 to 3.2 in 2015. Second, students consistently shift further left from entry to graduation. Third, this shift varies by major: students in the social sciences, humanities, and arts become substantially more liberal during college—a pattern observed across all cohorts. Fourth, the magnitude of this ideological shift has increased over time across all majors.

Figure 1 presents distributional changes in students' political ideology over the course of college, separately by major group. On average, students become 5.8 percentage points more liberal, 1.7 percentage points more far-left, 2.8 percentage points less conservative, and 0.5 percentage points less far-right during college. The magnitude of this shift varies by major group. Business and economics majors (Panel A) move slightly leftward—about 2.4 percentage points—mainly due to a decline in middle-of-the-road responses. In contrast, humanities and social sciences majors (Panels E and F) become 10.5 and 10.7 percentage points more left-wing and 6.5 and 4.5 percentage points less right-wing, respectively. Students in health, education, engineering, and natural sciences fields exhibit smaller leftward shifts, broadly consistent with the overall average. These patterns suggest that academic field choice may causally influence political ideology, a hypothesis tested in Section 3.

Figure A5 provides detailed information on the transitions between different ideological positions. In general, students who start as middle-of-the-road are more likely to become liberal (26 percent) than conservative (15 percent). However, this pattern varies substantially across majors, with 33 percent and 34 percent of the humanities and social sciences becoming liberal, compared to 18 percent in business and economics.

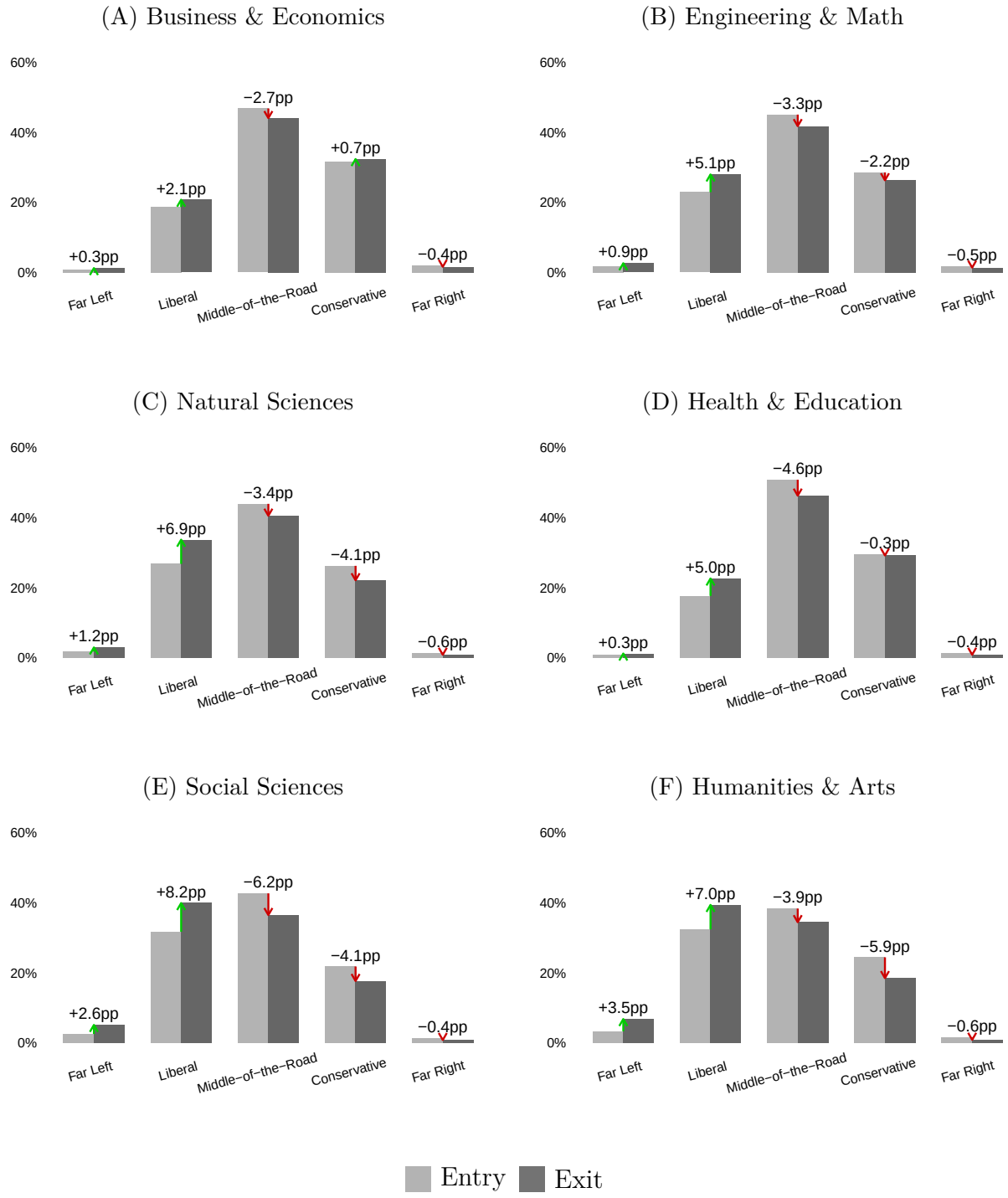
Additionally, Figure 2 compares students' baseline and graduation ideology across majors. For majors in business and economics, the average ideology remains similar to the baseline, as their corresponding circles are very close to the 45-degree line. Students in health, education, engineering, and natural sciences become slightly more left-leaning, as their corresponding circles are below the 45-degree line. Finally, humanities and social sciences majors exhibit a significant leftward shift during college, as their circles are much below the 45-degree line.

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

Policy positions. The survey data include questions on specific policy issues, with responses ranging from 1 (Disagree Strongly) to 4 (Agree Strongly). We focus on twelve such questions, chosen for their coverage across both entry and exit surveys for a sufficient number of years in our sample period. We also include a question on climate priorities, although it is available for fewer years.

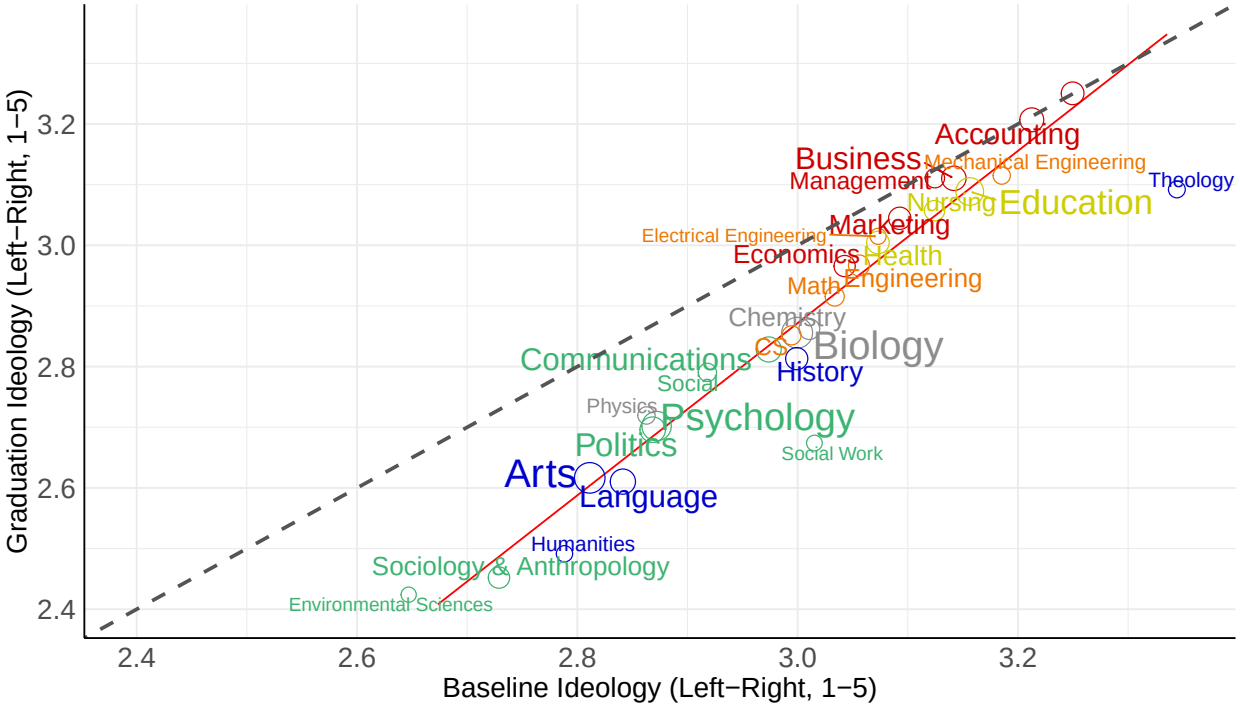
Table 2 lists these questions and reports the share of students who agree (strongly or somewhat) with each statement, overall and by college major, before and after college. The

Figure 1: 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 299,630 students. The samples in Panels A–F are restricted to students in specific groups of majors.

Figure 2: Baseline and Graduation Ideology



Note: This figure plots the average baseline (x-axis) and graduation (y-axis) political ideology of students by major, on a scale of 1–5 from far left to far right. The sample includes only students with non-missing values in the major and political ideology (299,630 students). Circle sizes are proportional to the number of students in each major. Colors indicate the classification group for each major: blue for humanities and arts; green for social sciences; gray for natural sciences; yellow for health and education; orange for engineering and math; red for business and economics. The black dashed line indicates where students' average graduation ideology equals their average baseline ideology (45-degree line), while the red solid line represents the regression line fitted to the observed means.

table reveals significant shifts in some positions that are consistent across majors (e.g., on climate change and abortion) and divergent trends across majors for others (e.g., on healthcare and taxation).

Table A3 presents the full response distribution for each question and their correlations with political ideology. Notably, the correlations between all policy responses and students' ideology strengthen in absolute value during college. This pattern indicates that as students' positions and ideological self-placement evolve, they become more internally consistent—showing a tighter alignment between their policy positions and overall political ideology.

Table 2: Baseline and Graduation Policy Positions

Statement	N	Survey	All	Business & Economics	Engineering & Math	Health & Education	Humanities & Arts	Natural Sciences	Social Sciences
Addressing global climate change should be a federal priority	28654	Base	67.5%	60.4%	64%	62.3%	71.8%	71%	72.4%
		Grad	78%	71.7%	78.9%	67.6%	81.3%	84.5%	81.7%
Abortion should be legal	246539	Base	51.9%	51.8%	52.7%	39.9%	53.4%	53.8%	56.2%
		Grad	61.9%	61%	60.9%	46.8%	64.7%	64.3%	67.9%
A national health care plan is needed to cover everybody's medical costs	152419	Base	68.2%	63.3%	62.3%	71.2%	70.3%	66%	72.5%
		Grad	68.2%	57.1%	60.1%	70.7%	72.9%	69.2%	75%
It is important to have laws prohibiting homosexual relationships	189542	Base	27.2%	31.5%	29.2%	33.4%	23.1%	25.9%	22.6%
		Grad	19.2%	23.8%	21.2%	25.2%	15.1%	17.4%	14.9%
Wealthy people should pay a larger share of taxes than they do now	192215	Base	59.9%	51.9%	61.2%	62.8%	62%	62.1%	61.9%
		Grad	60.9%	47.8%	58.3%	61.4%	67.1%	63.3%	66.7%
Marijuana should be legalized	223116	Base	28.4%	28.8%	28%	17.8%	32.5%	26.4%	31.6%
		Grad	43.8%	42.9%	43.9%	28.1%	49.5%	43.3%	48.9%
The death penalty should be abolished	205628	Base	32.3%	25.7%	29.3%	29%	40%	31.5%	35.3%
		Grad	39.4%	30.3%	35.4%	31.5%	50.4%	38.3%	45.3%
The federal government should do more to control the sale of handguns	149194	Base	83.6%	82.4%	77.6%	86.5%	84%	82.2%	86%
		Grad	82.2%	80.1%	74.6%	84.9%	83.7%	81.1%	85%
There is too much concern in the courts for the rights of criminals	140205	Base	67.4%	71.9%	69.5%	70.8%	61.4%	69.2%	64.7%
		Grad	56.2%	64.4%	60.4%	63.2%	47.3%	58%	49.2%
Racial discrimination is no longer a major problem in America	265675	Base	17%	20.5%	20.7%	16.8%	14.6%	17.1%	14.7%
		Grad	13.5%	18.6%	18.6%	11.9%	10.8%	13.5%	10.1%
Realistically, an individual can do little to bring about changes in our society	203464	Base	22.5%	26.8%	26.4%	21.6%	19.4%	21.4%	20.7%
		Grad	24.2%	28.5%	30.9%	20.9%	22.2%	24.8%	21%
Colleges should prohibit racist/sexist speech on campus	185476	Base	63.4%	62.4%	59.3%	70%	61.4%	63.3%	63.7%
		Grad	54.2%	52.1%	47.2%	59.5%	54.1%	53.8%	55.7%

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

2.3 Attrition

The sample for this study consists of students who participated in both the baseline and graduation surveys. About 29 percent of baseline respondents from cohorts covered by both

surveys are also observed at graduation. If participation in the graduation survey varies systematically across majors (differential attrition), this study's findings could be biased.

Directly examining differential attrition across actual 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 A4 shows that this is not a major concern. Columns (1)–(4) indicate that while raw participation rates differ somewhat across intended majors, these differences become negligible once we control for high school GPA and other covariates. For example, the raw participation gap between students intending to major in engineering and those in natural sciences is about 5 percentage points, but it falls to 0.1 percentage points and becomes statistically insignificant once controls are added. Other remaining gaps are statistically significant but very small—below one percentage point.

A remaining concern would be non-monotonic attrition—for instance, if right-leaning students were more likely to attrite from humanities while left-leaning students were more likely to attrite from business and economics. Column (5) shows that within intended majors, baseline political ideology is uncorrelated with attrition, alleviating this concern. These results suggest that differential attrition is unlikely to bias our estimates.

3 Impacts of Academic Fields on Political Ideology

This section examines the causal effect of academic fields of study on students' political ideology. We first employ a selection-on-observables-and-unobservables approach that leverages the richness of our data, including baseline ideology, policy positions, intended major, life goals, demographics, and many other measures collected immediately before college entry. This approach delivers plausibly causal estimates of the relative effect of each major. We provide a battery of robustness checks indicating that residual unobserved selection is unlikely to generate significant bias in our setting. As a complementary strategy, we exploit quasi-experimental variation generated by the timing of field-specific college expansions. These expansions shifted students' field-of-study choices in ways unrelated to their pre-college ideology, providing additional evidence on the causal effect of academic fields.

3.1 Baseline Estimates

Let $p_{ict}(m)$ denote the potential political ideology at graduation under major m for student i, who entered college c in year t. Let $m_{ict} \in \{m_1, \ldots, m_k\}$ be the realized major of student i,

where k is the number of possible majors, and let $D_{ictm} = \mathbf{1}\{m_{ict} = m\}$ indicate this realized major. The observed ideology is $p_{ict} \equiv p_{ict}(m_{ict})$. Let X_{ict} be the vector of main controls.

Our primary control is baseline (pre-college) ideology. While this lagged outcome should absorb most omitted-variable bias, we do not consider it sufficient for two reasons. First, the survey requires students to report their ideology on a discrete five-point scale. Measurement error from discretization may correlate with major choice. Second, survey response errors may also correlate with treatment. To absorb residual correlation, we include a rich set of additional pre-college cultural and economic policy positions that proxy for underlying determinants of ideology and help absorb remaining selection bias.

A potential caveat is that major choices may correlate not only with ideology levels but also with ideology trends or latent factors that predict such trends.[11] Therefore, we also control for the intended major reported before enrollment. Because unobserved tastes that correlate with both ideology and realized major typically operate through major preferences, the intended major is a highly informative proxy for remaining selection into majors. Lastly, we control for life goals, which are predictive of major choice (even conditional on intended major, given that students may not secure admission to their preferred field) and may be associated with political ideology, potentially absorbing additional bias, should it exist.

We also include college and year fixed effects. While these have minimal impact on our estimates, we consider them important because the set of colleges in our sample varies across years, and there are minor changes in survey question wording and response options.[12] The survey includes rich demographic variables, but we do not control for them in the main analyses, as they are not essential to our identification strategy and, as we show in the robustness section, their inclusion does not meaningfully affect the results. Table A5 lists the controls used in our main and other analyses.

We estimate the following equation:

$$\underbrace{p_{ict}}_{\substack{\text{Graduation political} \\ \text{ideology}}} = \underbrace{\sum_{m} \delta_m D_{ictm}}_{\substack{\text{Major} \\ \text{effects}}} + \underbrace{X'_{ict}\alpha}_{\substack{\text{Lagged ideology and proxies,} \\ \text{Intended major and proxies}}} + \theta_t + \lambda_c + \varepsilon_{ict} \tag{1}$$

[11] Figure A6 shows that ideology among non-college young adults is roughly stable between ages 18–30, with a slight shift toward conservatism.

[12] We also explored controlling for each college-by-year interaction. While this specification inflates the R^2 of the model (as well as the running time), it changes coefficient magnitudes by at most 1%. We therefore exclude these interactions from the main specification and report robustness to this approach in the appendix.

The coefficients of interest, δ_m, capture the effects of each major m on political ideology relative to the omitted reference category. The key identification assumption underlying this analysis is that, conditional on the controls, realized majors are not correlated with unobserved factors affecting graduation ideology ($\varepsilon_i \perp\!\!\!\perp D_{ictm} \mid X_{ict}, \theta_t, \lambda_c$).

Figure A7 illustrates the identification framework using a directed acyclic graph (DAG). The figure highlights two potential sources of bias: (1) unobserved preferences that existed before the baseline survey but were not captured in it, and (2) college experiences that affect both final major choice and political ideology. The controls in X_{ict} address bias from the first source. The second source—later college experiences—could still bias estimates if uncorrelated with these controls (for example, random shocks influencing both major choice and ideology change). To mitigate this concern, we later employ an instrumental variable strategy that instruments realized majors with intended majors and show that our main estimates are not biased by early college experiences.

Figure 3 presents the main estimation results. The bold lines represent coefficients from a model that groups majors into six broad categories, while the thinner lines show coefficients from a more detailed specification with 27 major categories. In both cases, the omitted group is the natural sciences (biology, physics, chemistry). The estimates reveal substantial effects. Panel A shows that majors in the humanities, arts, and social sciences shift students' ideology leftward by about 0.09 standard deviations relative to the natural sciences. By contrast, economics and business majors shift students rightward by about 0.14 standard deviations, while professional fields—health, education, and engineering—shift rightward by about 0.06 standard deviations relative to the natural sciences.

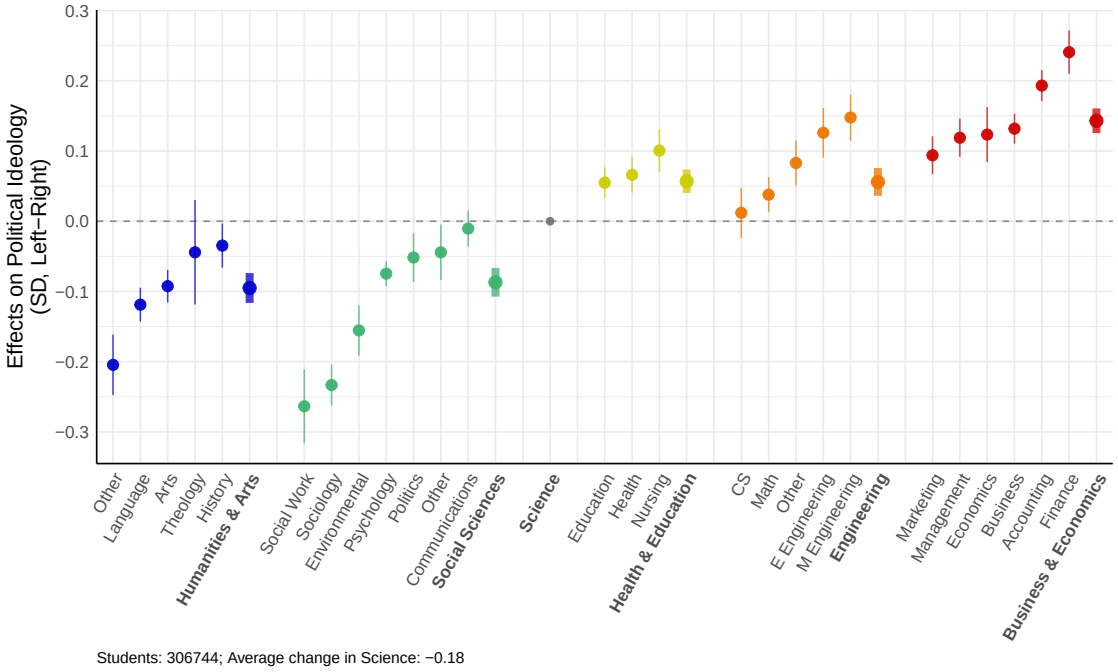
Panel B uses an indicator for whether students identify as liberal or far left. Students in the humanities, arts, and social sciences are 4 percentage points more likely to identify as such, whereas those in economics and business are 6 percentage points less likely, relative to the natural sciences. Students in professional fields are slightly less likely to do so, by about 2–3 percentage points.

The figures also reveal 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 (0.15 to 0.26 standard deviations). Within business-related fields, finance and accounting cause the most substantial rightward shifts (0.19 and 0.24 standard deviations).

The magnitudes of these effects are substantial. To benchmark them, we use differences in students' baseline ideology across familiar contexts. The average ideological gap between students from New York and Texas at college entry is 0.32 standard deviations, implying

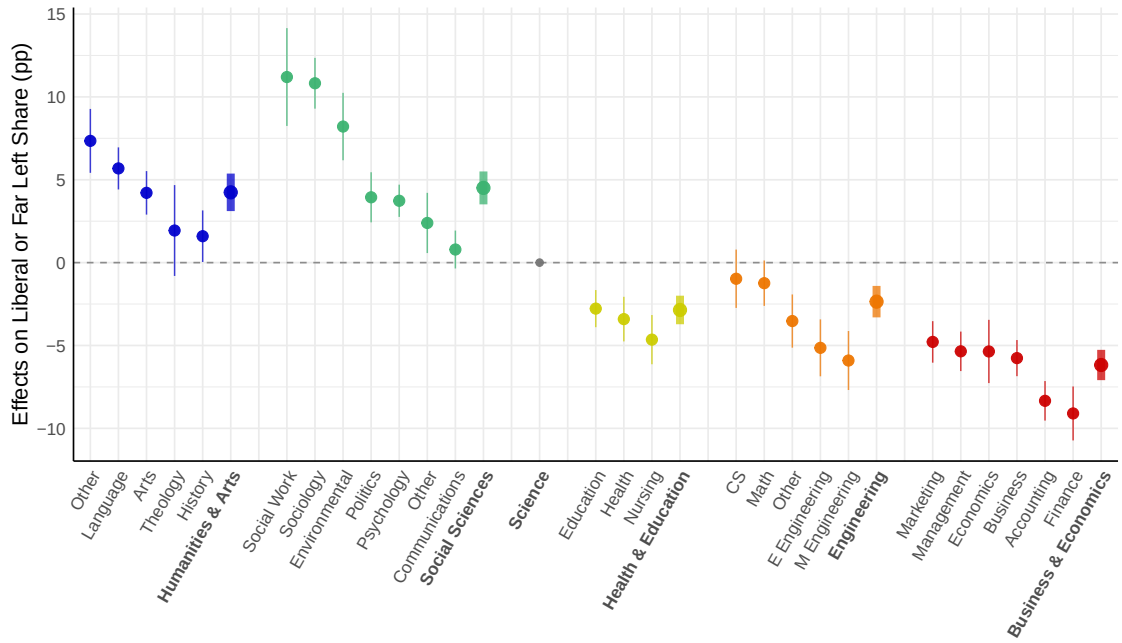
Figure 3: Effects of College Majors on Political Ideology

(A) Ideology Score



Students: 306744; Average change in Science: −0.18

(B) Share Far Left or Liberal



Students: 306744; Average change in Science: 8.07

Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals. The bold lines represent coefficients from a model that groups majors into six broad categories. The thinner lines show coefficients from a more detailed model with 26 specific major categories. In both models, the natural sciences serve as the omitted reference group. The outcome is based on responses to: "How would you characterize your political views?". Panel A is in standard-deviation units. Panel B is in percentage points for an indicator of liberal or far-left identification. Both models control flexibly for students' baseline political ideology, policy positions, intended majors, life goals, political behaviors, college type, and year (main specification). The sample includes all students with non-missing major and outcome.

that majoring in business or economics rather than in social sciences or humanities produces roughly two-thirds of that gap. Likewise, the baseline gender gap in ideology is 0.14 standard deviations, indicating that the estimated effects are about 1.6 times larger than the gender gap at entry. As elaborated in Section 6, these effects also contribute to broader political patterns—such as ideological divides—and translate into meaningful differences in voting behavior comparable to other major life experiences. Given the large sample size, most estimated effects are highly statistically significant.

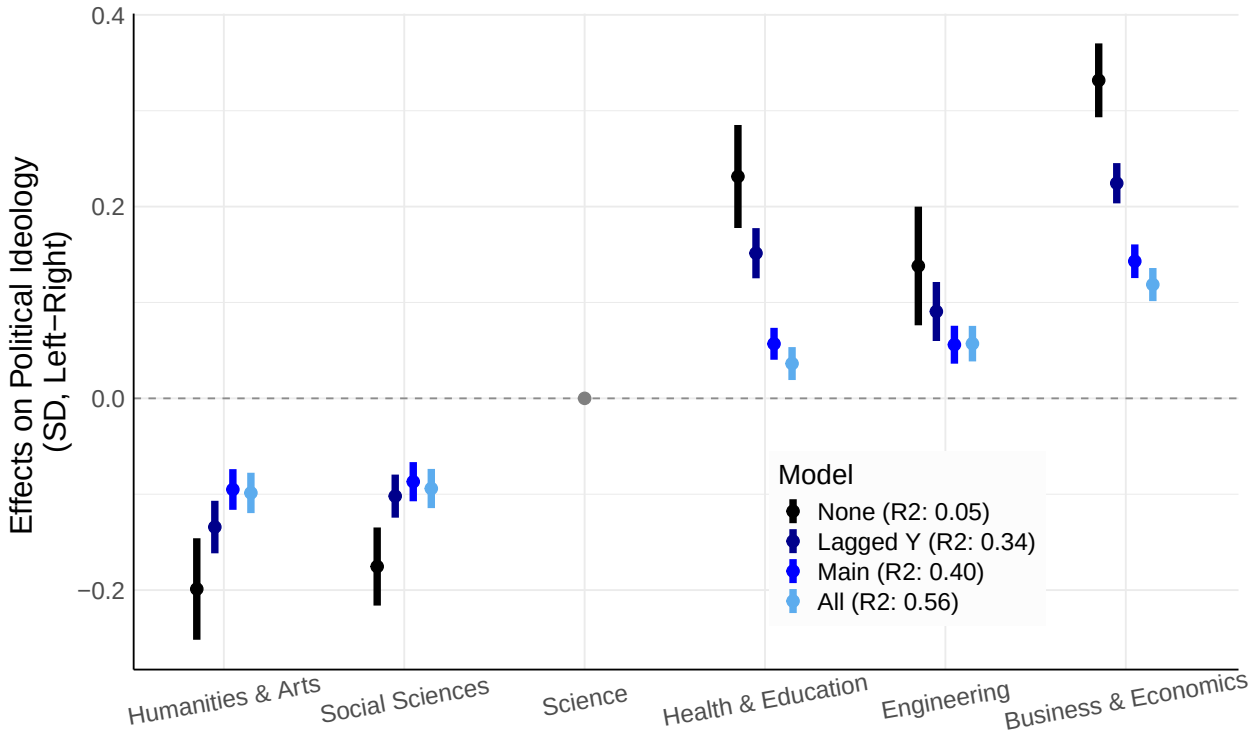
Appendix Figure A8 presents complementary results using alternative outcomes: identification as middle-of-the-road (Panel A) and as conservative or far right (Panel B). Humanities and social sciences majors are 2 percentage points less likely to identify as conservative or far right, whereas economics and business majors are 5 percentage points more likely to do so. For middle-of-the-road identification, economics and business majors are 1 percentage point more likely to report this stance, while humanities and social sciences majors are 2 percentage points less likely.

Robustness. Our specification includes multiple treatments, considering each major choice as a separate treatment. Goldsmith-Pinkham et al. (2024) show that such specifications may suffer from contamination bias without additional assumptions. To address this concern, we follow their suggested correction, estimating models that compare each treated major k to a single control group (the omitted major) $\tilde{k}$, using only students in either k or $\tilde{k}$. Figure A9 reports these results, which remain very similar to our baseline estimates. This robustness not only indicates that our main results are free from contamination bias, but also suggests that our setting likely satisfies additional restrictions on treatment heterogeneity—such as the absence of selection on treatment effects—that are sufficient for OLS to recover average treatment effects in a setting with multiple treatments. We elaborate on these assumptions in relation to our setting in Appendix C.

As an additional robustness test, we add controls that may correlate with unobservables—such as career preferences, time use, demographics, and four-way interactions between college, year, intended major, and baseline ideology. Figure 4 reports the estimated coefficients from specifications that sequentially add the controls listed in Table A5. The baseline specification includes only major dummies and is therefore most prone to selection bias. Incorporating our main controls substantially shifts the coefficients, underscoring their importance for identification. However, the estimates remain stable when additional controls are added, a strong indication of the robustness of our results.

Lastly, we estimate equation 1 using a modified outcome: the change in students' political ideology from baseline to graduation (a first-differences specification). This approach relies

Figure 4: Robustness to Including Additional Control Variables



Note: This figure plots the estimates for δ_m from equation 1 and their 95 percent confidence intervals. The estimated coefficients represent the effects of studying in each major group relative to STEM, health, and education (the omitted group of majors). The outcome variable is students' responses to the following question: "How would you characterize your political views?". Estimates are shown in standard deviations. The sample includes only students with non-missing majors and political ideology in the graduation survey (306,744 students). Table A5 provides the full list of control variables. Baseline refers to baseline political ideology and preferred refers to intended majors. Our model incorporates all variables flexibly (with fixed effects for each value available in the survey).

on a conditional parallel-trends assumption, namely that, conditional on controls, students across majors would have followed similar ideological trends absent treatment. Figure A10 reports the results, which closely mirror our baseline estimates, reinforcing their validity within a slightly different identification framework.

Heterogeneity. We also test whether the effects of college majors differ across groups by estimating Equation 1 for several subsamples. Panel A of Table A6 reports results by students' personal characteristics. The effects of majors on ideology are similar across gender, socioeconomic background, and high school GPA. If anything, the effects of studying economics and business instead of humanities and social sciences are slightly larger among high-GPA students, though the overall difference is small: 0.27 for high- versus 0.22 standard deviations for low-high-school-GPA students. Table A7 presents results by college characteristics, again

showing broadly similar effects across institution types, selectivity, and geographic regions. Finally, Table A8 examines temporal changes, showing a modest increase in effect sizes after 2010 (0.27–0.28 standard deviations) compared to earlier periods (0.22–0.23). Overall, these results indicate that the effects of college majors are largely similar across student groups and institutions.

3.2 Testing and Accounting for Selection-on-Unobservables

Negative control falsification tests. We next implement falsification tests using placebo treatments and placebo outcomes—an approach known in the statistics literature as negative control exposure and outcome (Lipsitch et al., 2010; Shi et al., 2020). Two variables are central to this analysis: baseline ideology and intended major. Falsification for the full model using these variables is infeasible, since they are already included as controls. Because they are part of the model, they cannot serve as negative controls for the full specification. The data also include other variables not used in the main model, such as demographics, but these are unlikely to proxy well for the unobserved factors that could bias the estimates.

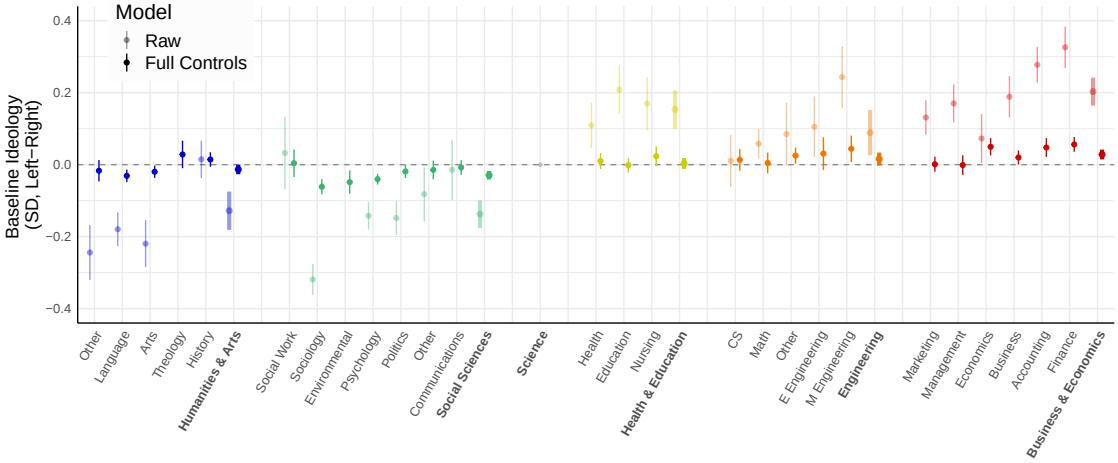
We therefore proceed as follows. First, we use lagged ideology and intended major as negative controls, testing adjusted models that exclude each in turn. This provides a demanding falsification test: correlations that persist in these reduced models would likely diminish in the full specification. Still, these tests are informative for assessing how much bias the controls remove. Second, we use these variables as benchmarks for potential unobservables, allowing a transparent quantification of how different forms of unobserved selection could bias the estimates.

Results are shown in Figure 5. Panel A presents a negative-control outcome test using baseline ideology as a placebo outcome. We report estimates with and without controls to assess how much of the correlation is reduced. The figure shows that majors are strongly correlated with baseline ideology when no controls are included, and that this correlation is substantially reduced once controls are added. For example, the coefficient for social sciences is 0.14 standard deviations without controls, and falls to 0.03 when controls are included—a reduction of about 80 percent. Similarly, the coefficient for humanities and arts decreases from 0.13 to 0.01 (about 90 percent), and for economics and business from 0.20 to 0.03 (about 85 percent). The estimates with controls remain statistically significant, indicating that models omitting baseline ideology would still be biased. However, they also suggest that most of the selection bias is absorbed by the included controls.

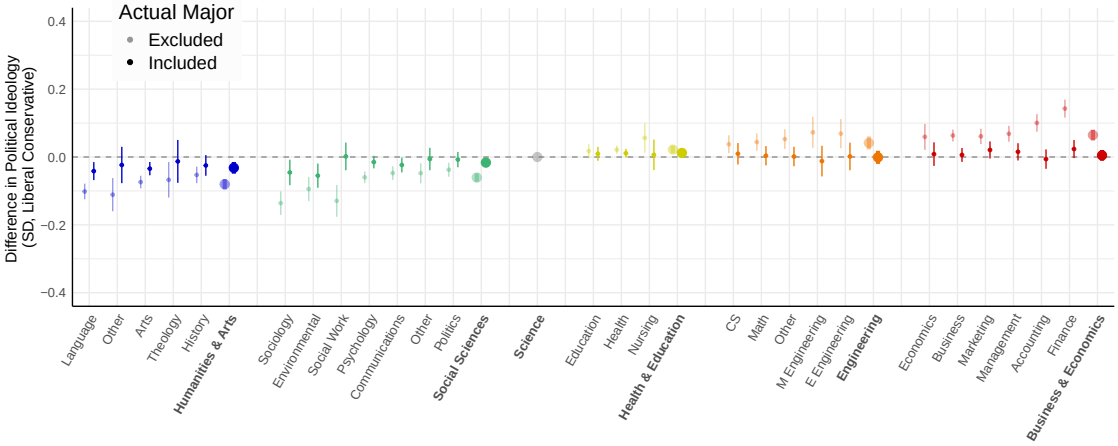
Panel B of Figure 5 reports results from the negative-control exposure test, which treats intended major as a placebo treatment. The logic of this test differs slightly: it examines

Figure 5: Negative Control Tests and Robustness to Selection on Unobservables

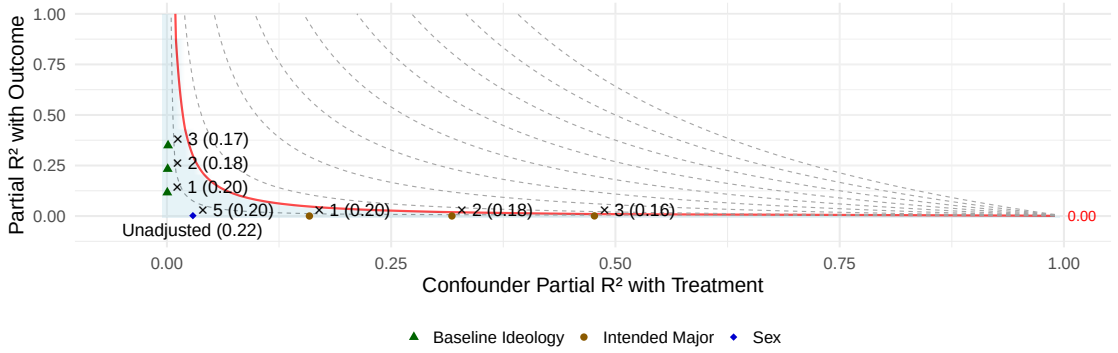
(A) Negative Control Outcome



(B) Negative Control Exposure



(C) Robustness of the Effects of Business/Economics versus Social/Humanities



Note: This figure tests our identification assumption and its robustness to unobserved selection. Panel A shows a negative-control outcome test: differences in baseline ideology across majors, reported for a raw comparison and for a model with the full control set excluding baseline ideology. Panel B shows a negative-control exposure test: coefficients for intended majors, serving as proxies for unobserved preferences, from a models with and without actual majors. Panel C presents sensitivity contours (Cinelli and Hazlett, 2020), quantifying how much selection on unobservables would be required to change the estimates, using observed covariates for benchmarking. The analysis in Panel C focuses on the effects of studying economics or business instead of humanities or social sciences.

whether the placebo treatment is correlated with the outcome conditional on the controls and the actual treatment.[13] We therefore estimate Equation 1 including all controls and report the coefficients on intended majors. If the controls (excluding intended major) fully capture unobserved preferences relevant for graduation ideology, these coefficients should be zero.

The estimated coefficients on all intended-major categories are small: approximately −0.0319 and −0.0163 standard deviations for humanities and social sciences, and about 0.0049 for economics and business. Some coefficients remain statistically significant, suggesting that specifications without controls for intended majors may be biased. However, the small magnitudes indicate that such bias is likely minor. We formally support this conclusion by using these variables as benchmarks for selection on unobservables.

The figure also reports estimated coefficients from the same model excluding realized majors. In this specification, if intended majors are correlated with realized majors, they will capture their effects on graduation ideology. Indeed, all estimated coefficients in this model are substantial—about 60–90 percent larger than in the model that includes realized majors. For example, the estimates are approximately −0.0802 and −0.0605 standard deviations for humanities and social sciences, and 0.0647 for economics and business. This pattern indicates that using intended majors as a placebo treatment is indeed a demanding test, given their strong relationship with realized majors. The analysis also motivates their use as instrumental variables in a complementary strategy, elaborated below.

Sensitivity to selection on unobservables. We first follow Cinelli and Hazlett (2020) to assess sensitivity to selection on unobservables. The exercise benchmarks the potential strength of an omitted confounder Z using observed covariates, in terms of partial R^2. We use two primary covariates for this benchmarking: intended major and baseline ideology. These are natural choices, as they are likely the variables most correlated with the treatment and outcome, respectively. We also repeat the exercise for other covariates, such as high school GPA and sex, and, as expected, the bias implied by such benchmarks is much smaller.[14]

[13] A recent paper formalizes these two types of falsification tests in the instrumental-variable setting Danieli et al., 2023. The logic is similar here and is illustrated for the less common negative-control exposure test in Figure A11.

[14] 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 bias-bound formula, and would therefore yield tighter bounds and smaller implied bias.

We restrict the sample to students in the social sciences, humanities, economics, and business, and estimate the model with the full set of controls,[15] focusing on a single treatment indicator equal to one for students in business or economics. We then compute the partial R^2 for two key covariates: baseline ideology and intended major. As expected, baseline ideology remains the strongest predictor of the outcome after conditioning on all other controls (partial $R^2 = 0.10$), while intended major is most strongly associated with the treatment (partial $R^2 = 0.14$). However, baseline ideology is nearly uncorrelated with the treatment (partial $R^2 = 0.0004$), and intended major is nearly uncorrelated with the outcome (partial $R^2 = 0.0002$).

This structure allows a straightforward benchmarking exercise: we assess how large the bias would be if an unobserved confounder were related to both the treatment and the outcome with the same partial R^2 as either baseline ideology or intended major, conditional on all other controls. This represents a conservative scenario, as it assumes an omitted variable that is as predictive of the outcome as the lagged outcome itself (conditional on it) and equally predictive of the treatment. Panel C of Figure 5 illustrates this and other conservative benchmarking scenarios. The figure shows that such omitted variables could attenuate the estimated coefficient only slightly—from 0.22 to 0.20 (a bias of 0.02). Even under more extreme assumptions, where unobservables are two or three times stronger than these benchmarks, the estimated effect remains sizable.

One might argue that using baseline ideology and intended major as benchmarks is not the most conservative approach, since each is primarily correlated with only one side of the confounding channel—the outcome or the treatment. To address this concern, the figure also presents a conservative scenario based on another covariate that may be correlated with both: sex. Even if an unobserved confounder were ten times more correlated with both the treatment and the outcome than sex, the implied bias would still be only 0.04. Panel A of Table A9 reports additional scenarios, showing that similar conclusions arise when using high school GPA as the benchmark. Moreover, the table shows that even a confounder as correlated with the treatment as intended major, and twenty times more correlated with the outcome, would only induce a bias of 0.06. The same holds when using baseline ideology as the benchmark.

We also evaluate scenarios in which unobserved factors are as predictive of the treatment or outcome as our strongest observed controls and assess how strongly they would need to predict the other variable to overturn the results. An unobserved factor as predictive of the residual outcome (conditional on all controls) as baseline ideology would need to be roughly

[15] We make one small adjustment: since the full model includes interactions between college, year, baseline ideology, and intended majors, we include here only college-by-year fixed effects, as baseline ideology and intended majors are used for benchmarking.

165 times more predictive of the residual treatment to reverse the sign of our estimates. Similarly, an unobserved factor as predictive of the residual treatment as intended major would need to be about 270 times more predictive of the residual outcome to do so.

Panel B of Table A9 reports results from an adjusted specification that excludes career preferences from the set of controls, given their strong correlation with intended majors. When excluding them, the partial R^2 of intended majors with the treatment increases markedly—from 0.14 to 0.32—reflecting their substantial predictive power. The partial R^2 with the outcome also rises from 0.0002 to 0.0005. These values imply that an unobserved confounder that is similarly correlated with both the treatment and the outcome could generate a bias of 0.07. However, this scenario is already extreme: such a confounder would explain nearly half (45%) of the remaining variation in the treatment. Moreover, a confounder as correlated with the treatment as intended major would need to be twenty times more correlated with the outcome to overturn our findings in this case.[16] Importantly, this exercise excludes career preferences, which are included in our main specification, implying that the bounds in the full model are even tighter.

Finally, we also adopt an alternative approach to assess sensitivity to selection on unobservables, following Oster (2019). While the method of Cinelli and Hazlett (2020) provides a transparent way to benchmark unobservables to observables, it is best suited for one or a small set of controls. In contrast, the Oster (2019) framework allows transparent analysis of robustness with many controls, based on changes in estimated coefficients and R^2 when new groups of controls are added (both shown in Figure 4).

As detailed in Appendix D, this methodology implies that selection on unobservables would have to be more than five times stronger than selection on the controls added in the full model but not included in the main specification. This set includes several key controls, such as career preferences. It is therefore highly unlikely that unobserved factors are stronger than these controls—yet they would need to be over five times stronger to overturn our conclusions.

To summarize, even under conservative assumptions about selection on unobservables, the estimated effects remain similar. Thus, the results support a causal interpretation and indicate that unobserved selection is unlikely to significantly bias our findings.

[16] If it were twice as predictive of both the treatment and the outcome, it could overturn our findings, but this is highly implausible—such a confounder would explain almost all (90%) of the residual variation in the treatment and would have to be a much stronger predictor of the outcome than intended majors themselves. Indeed, career preferences—arguably the most relevant omitted variable in this context—explain a large share of the residual treatment variation but little of the outcome, yielding almost no bias.

3.3 Isolating Major Effects from Other College Experiences

Double majors. The analysis so far has 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 A10 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.[17] 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 A12 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 0.09–0.12 standard deviations relative to natural sciences. In contrast, combining two majors in business and economics leads to a 0.19 shift toward conservatism.

Early college experience. As noted earlier and illustrated in Figure A7, a concern for our main results is that some experiences during the freshman and sophomore years may influence both major choice and graduation ideology.[18] 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 pursue 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

[17] The table also shows that this holds for the policy-position index that is defined in the following section.
[18] Note that our bounding exercises also address this concern, as they assess potential bias from all unobserved covariates, including early college experiences

proxies for major preferences. The following system of equations implements this instrumental variable strategy:

$$D_{ictm} = \sum_m \pi_m D^0_{ictm} + Q'_{ict}\gamma^0 + \theta^0_t + \lambda^0_c + u^0_{ict}, \tag{2}$$

$$p_{ict} = \sum_m \tau_m D_{ictm} + Q'_{ict}\gamma^1 + \theta^1_t + \lambda^1_c + u^1_{ict}, \tag{3}$$

Where D^0_{ictm} are the intended major dummies, serving as instrumental variables for actual major choices D_{iictm}, and Q_{ict} includes our main set of controls X_{ict} excluding D^0_{ictm}. This approach relies on the following assumptions. First, intended majors are relevant instruments for realized majors. Figure A2 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 ($v_{ict} \perp\!\!\!\perp D^0_{ictm} \mid Q_{ict}, \theta_t + \lambda_c$). This assumption is stronger than in our baseline analysis. However, as shown earlier in the negative-control exposure tests, our controls (excluding intended majors) capture almost all unobserved preferences correlated with treatment. Thus, it is very likely that the same set of controls captures the association with the intended major.

Third, this specification assumes monotonicity: a preference for a specific major can only weakly increase the likelihood of pursuing it. Formally, for each major m, $D_{im}(D^0_i = m) \geq D_{im}(D^0_i \neq m)$. This assumption is plausible in our setting, as it is unlikely that intending a major would make a student less likely to pursue it.[19]

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. However, the negative-control exposure test reported earlier provides evidence against this concern: conditional on realized majors, intended major is only weakly correlated with graduation ideology.

Figure A13 shows that the instrumental variable estimates are very similar to the baseline OLS estimates, though modestly larger in magnitude. For example, the coefficient on economics and business increases from 0.13 in the main analysis to about 0.23, and the

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

coefficient on humanities and arts shifts from -0.09 to about -0.14. These larger coefficients are consistent with attenuation bias in the OLS estimates due to measurement error in the realized-major variable, which may arise from student misreporting or machine-reading errors. Alternatively, they could also reflect violations of the exclusion restriction, which we discussed above.

3.4 Quasi-Experimental Evidence from Changes in Field Supply

We also provide evidence from a quasi-experimental design that exploits sharp increases in the enrollment of business and economics programs at specific colleges. The identifying assumption is that these are supply-side (i.e., product of institute decisions rather than students' preferences) shocks and do not directly correlate with their potential political ideology. This variation provides an alternative source of identification, strengthening and supporting our causal claims. We identify shocks using a data-driven approach and apply multiple tests to ensure they represent genuine supply-driven capacity expansions that affect major enrollment without correlating with students' potential outcomes. This strategy parallels well-established designs in labor economics that identify the effects of job loss by exploiting mass layoffs as quasi-experimental variation (Jacobson et al., 1993; Couch and Placzek, 2010).

We use administrative data from the Integrated Postsecondary Education Data System (IPEDS) to identify such shocks. This dataset reports the total number of graduates from each program (at the Classification of Instructional Programs, or CIP, level) by college and year. We classify the CIP codes into the same groups of majors used in our primary analysis. For each college c and year t, we calculate the share of students in business and economics majors, denoted by B_{ct}.

We focus on shocks to business and economics for two reasons. First, these fields have experienced substantial expansions in recent decades, providing multiple quasi-experimental events. Second, business and economics are the most right-leaning majors. As such, they offer a clear test of whether field choices causally affect ideology. In contrast, natural sciences and engineering may draw students from both left-leaning (e.g., politics) and right-leaning (e.g., economics) fields, making interpreting causal inference less straightforward.

In the main analysis, we define shocks as college–year cells in which B_{ct} increases by 30 percent or more, provided that the previous year shows no large change (an increase or decrease of 15 percent or more). While these cutoffs are arbitrary, we select them in a data-driven way and validate that the results are robust to changes in the definition, as we

elaborate below. Let s_c be the shock year for college c, the first enrollment year in which students are affected by the shock.

Under parallel trends and the additional assumption of homogeneous treatment effects—which is relaxed below—the following two-way fixed-effects (TWFE) event-study specification estimates the dynamic impact of these college-level shocks:

$$y_{ict} = e_c + s_t + \sum_{k \neq -3} \rho_k \mathbf{1}[t = S_c + k] + f(y^0_{ict}) + v_{ict} \tag{4}$$

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 identified shock year S_{ic}, where $k < 0$ denotes years before the shock and $k > 0$ denotes years after. The coefficients of interest, ρ_k, measure the effect of the shock relative to the omitted category ($k = -3$), three years prior to the shock. We use this cohort as the reference to avoid contamination from earlier cohorts that may have already been exposed to preparatory course changes. The terms e_c and s_t denote college and cohort fixed effects. y^0_{ict} denotes the lagged dependent variable: baseline political ideology, policy positions, or life goals when the outcome is a survey response on these topics, and intended major when the outcome is major choice. We control for this flexibly by including fixed effects for each discrete baseline response. The analysis covers a ten-year window centered on the shock (five years before and five years after). We also estimate a specification with a single post-shock indicator, which captures the average effect across the four post-shock cohorts. In this specification, we exclude the two cohorts immediately preceding the shock, as these students may have been partially exposed to curriculum changes.

Because the timing of shocks varies across colleges, this corresponds to a staggered difference-in-differences design. Recent work shows that TWFE regressions can be biased in such settings (surveyed in Roth et al., 2023) when treatment effects are heterogeneous. To address this concern and relax the homogeneity assumption, our main specification uses the imputation estimator of Gardner (2022),[20] which corrects this bias through a two-stage procedure: first estimating fixed effects using only untreated observations, then adjusting outcomes by removing the predicted fixed effects and estimating treatment effects on these adjusted outcomes. We also verify that the results are robust to alternative estimators and report results from the canonical TWFE specification in the appendix.

We apply a data-driven methodology to identify shocks. The goal is to select the shock definition that generates the most variation in business and economics enrollment—the

[20]This estimator is numerically equivalent to that of Borusyak et al. (2024) but offers improved finite-sample inference.

variation whose impact we aim to measure in this exercise. Specifically, using the IPDES administrative data, we run the same algorithm 27 times, varying three parameters: (i) the threshold for the increase in business students, B_{cy} (20%, 25%, or 30%), (ii) the restriction on changes in the previous year (one-half, two-fifths, or one-third of the given threshold), and (iii) the minimum cohort size (0, 25, or 50 students). We then select the definition that yields the largest t-statistic for the increase in business and economics enrollment in our survey data, estimated using Equation 4 in the single-post-dummy specification with business or economics majors as the outcome. This yields our preferred definition: a shock occurs in college–year cells where business graduation increases by at least 30%, provided the previous year shows no large change (defined as an increase or decrease of 15% or more). Figure A14 reports results under all the alternative definitions, plotting the effect of shocks on both the share of business and economics majors and on political ideology (conditional on baseline ideology). The figure demonstrates that every definition producing a meaningful shift in major choice also generates a meaningful effect on students' ideology.

Under the main definition, we identify large shocks in field availability at 317 colleges included in the baseline survey, 104 of which are also included in the graduation survey. We use all colleges for falsification tests and restrict to the graduation-survey colleges for the main analysis.

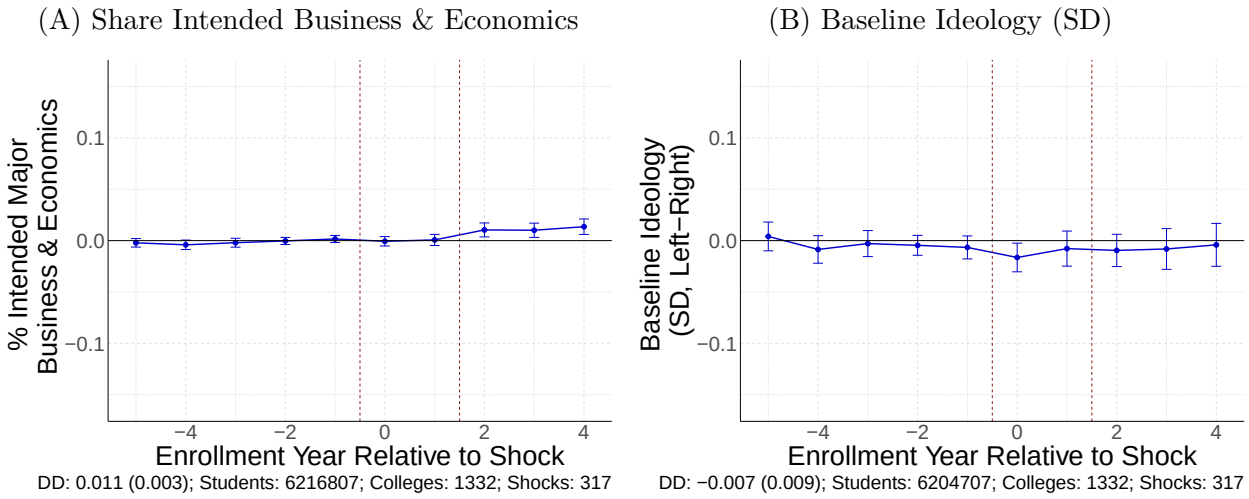
Our analysis relies on the interpretation that sharp changes in major availability are driven by supply-side (i.e, college administration side) rather than demand-side considerations. We provide empirical evidence supporting this interpretation below by examining student characteristics in colleges experiencing shocks. Anecdotal evidence is also consistent: web reports indicate that the two largest affected colleges since 2010 received substantial donations specifically earmarked for expanding business and economics programs, with timing closely aligned to our identified shocks.

Yet, these shocks cannot serve as instrumental variables for major choice because they likely violate the exclusion restriction: they may influence course-taking beyond students' chosen majors. This implies that the quasi-experimental exercise estimates a different parameter than our main analysis and is not directly comparable in magnitude. However, because these expansions reflect supply-side rather than demand-side changes, they should be orthogonal to students' potential outcomes. This enables us to estimate a policy-relevant causal parameter: the effect of expanding access to specific fields within a college.

Figure 6 presents falsification tests supporting the validity of the design. These tests are based on Equation 4, using intended major and baseline ideology as the outcome variables.[21]

[21] In the falsification tests, the control for the lagged outcome is omitted because it serves as the outcome variable.

Figure 6: Falsification Tests: College-Level Shocks to Economics & Business

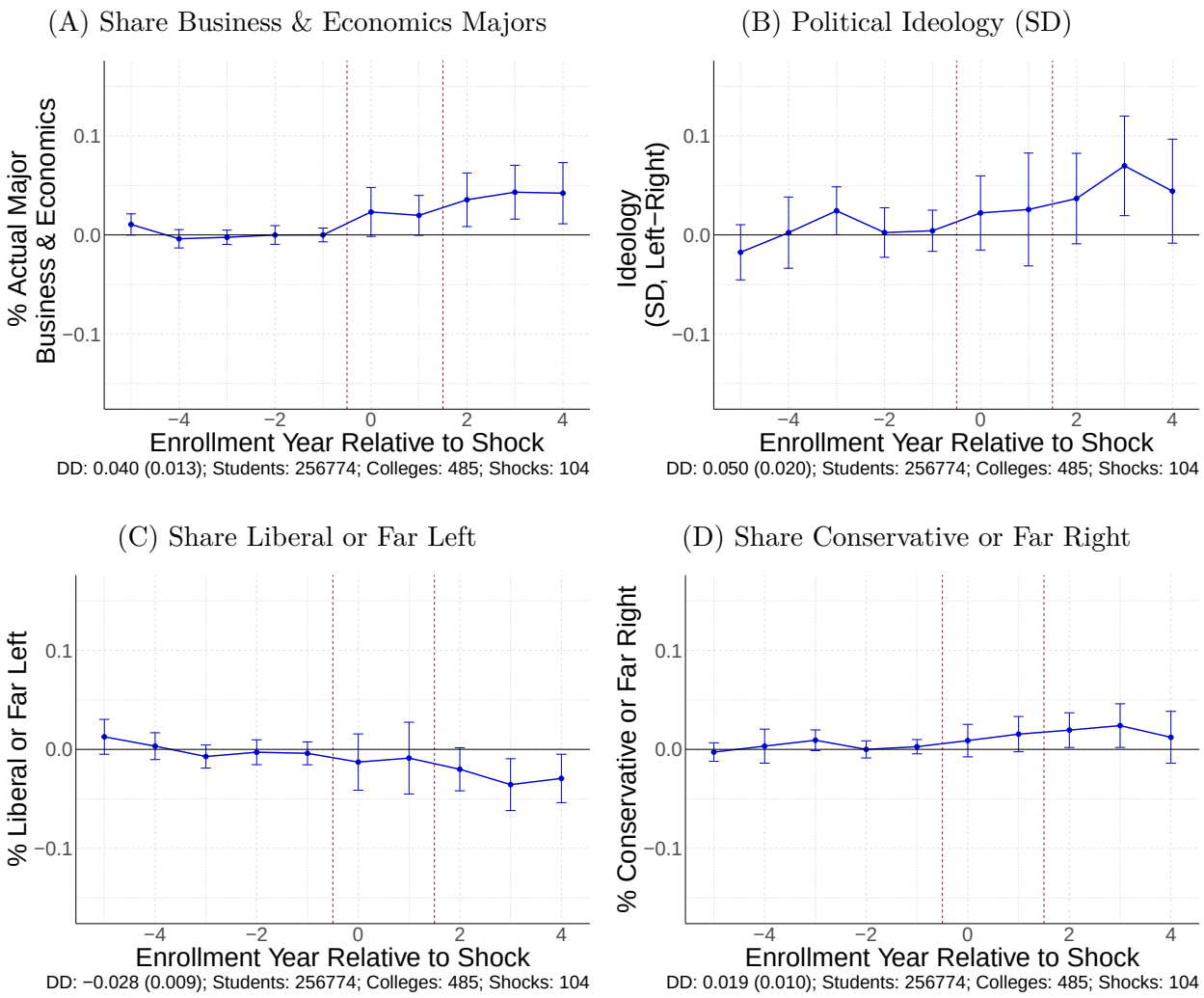


Note: These figures present falsification tests for the quasi-experimental design. Points show the estimates of ρ_k from equation 4, representing the effects of college-level shocks to the availability of business and economics fields on students' outcomes, with standard errors clustered at the college level. Each panel also reports the corresponding difference-in-differences estimate of the average effect. Estimation follows the imputation estimator of Gardner (2022). The sample includes all students in the baseline survey with non-missing outcome values. The outcome in Panel A is an indicator for intending to major in business or economics, and in Panel B is baseline ideology.

Using all observations from the baseline survey, we examine whether the shocks correlate with students' pre-existing characteristics. These tests are highly powered because they use the complete baseline survey sample, which is 25 times larger than our main analytical sample (students appearing in both surveys). We focus on two key variables that proxy for potential outcomes and treatment: baseline ideology and intended major. For baseline ideology, the plots show no pre-trend and no change in incoming students' baseline ideology following the shock. The large sample allows us to rule out positive effects as small as 0.011 standard deviations, providing strong support for the positive graduation ideology effects reported below. For intended major, we observe no pre-trend—ruling out increases larger than 0.51 percentage points in the preference for economics and business majors immediately before the shock—which strongly supports the supply-driven nature of these shocks. Following the shock, we observe a modest increase of approximately 1 percentage point in intended major preferences, likely reflecting students' responses to expanded program offerings.

Figure 7 presents the estimated effects of these shocks on students' actual major choices and graduation ideology. Panel A examines the likelihood of majoring in business or economics. Before the shock, both outcomes follow parallel trends. After the shock, the probability of majoring in business or economics rises by about four percentage points. This effect

Figure 7: Estimated Impacts of College-Level Shocks to Economics & Business



Note: These figures present results from the quasi-experimental design. Points show the estimates of ρ_k from equation 4, capturing the effects of college-level shocks to the availability of business and economics fields on students' outcomes, with standard errors clustered at the college level. Each panel also reports the corresponding difference-in-differences estimate of the average effect. Estimation follows the imputation estimator of Gardner (2022). The sample includes all students observed in both the baseline and graduation surveys with non-missing outcome values. The outcome in Panel A is an indicator for majoring in business or economics; in Panel B, graduation ideology; and in Panels C and D, indicators for identifying as liberal or far left, and as conservative or far right, respectively. All models control for the lagged outcome from the baseline survey.

is statistically significant and indicates a meaningful shift in field exposure. Yet it likely understates the full magnitude of such expansions, since non-major coursework taken by other students is also likely affected. Panels A–E of Figure A15 further explore substitution across fields. The increase in business and economics majors appears to come mainly at the expense of other social sciences (–2.5 percentage points) and natural sciences (–1.4 percentage points), though these estimates are not statistically significant.

Panels B–D of Figure 7 examine students' political ideology. Graduation ideology shifts by about 0.05 standard deviations to the right. The likelihood of identifying as far left or liberal decreases by roughly 3 percentage points, while the share identifying as far right or conservative increases by about 2 percentage points. Panel F of Figure A15 reports results for identification as middle-of-the-road, showing a small and statistically insignificant increase of about 1 percentage point. Figure A16 shows that these results are qualitatively similar when estimated using a two-way fixed effects approach.

These results suggest that shocks to field availability influence students' political ideology, reinforcing the main causal interpretation of this section. In terms of magnitude, these estimates are not directly comparable to those from the conditional-on-observables analysis, as they capture the average effect of college-level expansions rather than the individual effect of majoring in a given field. As discussed earlier, students who do not switch into business or economics may still be exposed to the shocks, for example, by taking more courses in these fields. Nevertheless, the effects are both statistically and economically significant, indicating that changes in field availability have meaningful ideological consequences.

4 Broader Impacts of Academic Fields

This section examines whether the ideological effects of college majors reflect substantive shifts in students' preferences and behaviors or merely changes in self-identification. To address this, we analyze policy positions, political behaviors, and stated life goals. All analyses rely on our baseline selection-on-observables strategy, which—as shown earlier—produces plausibly causal estimates that would require extreme unobserved selection to overturn. We also report results for key outcomes using the quasi-experimental design based on field expansions.

4.1 Policy Positions

We begin by examining how college majors shape students' worldviews, as reflected in their positions on specific policy issues, and assess how much these changes may account

for the overall ideological effects of majors. We estimate equation 1 using various policy positions—standardized to have mean zero and unit variance—as outcome variables. Table 3 reports the results, showing that college majors significantly influence students' policy positions.

Compared to natural sciences' majors, students in the social sciences and in the humanities and arts consistently shift in a more liberal direction across nearly all policy domains. They become more supportive of abortion rights, abolition of the death penalty, higher taxes on the wealthy, national healthcare, gun control, and marijuana legalization. They are less supportive of anti-LGBTQ statements, claims denying racial discrimination, and tough-on-crime policies. Interestingly, they are also less likely to agree that "an individual can do little to bring about change in our society", which may suggest stronger civic engagement. A notable exception is climate change: natural sciences' majors become more likely than those in the social sciences and humanities to view it as a federal priority.

In contrast, business and economics students become more conservative than natural sciences' students, particularly on economic issues. Notably, the effects of college major are much larger for economic policy items than for most cultural issues. For instance, the gap between business/economics and humanities/social sciences' majors reaches 0.22–0.25 standard deviations for positions on healthcare and taxation, but only about 0.07 standard deviations for positions on abortion rights or gun control.

We next examine how the effects on political ideology align with changes in specific policy positions. To do so, we predict each student's ideology based on their responses to all questions in Table 3. These predicted values reflect how closely each student's policy positions correspond to a liberal or left-leaning outlook. We then estimate equation 1 using these predicted values as the outcome. Figure 8 shows the estimated effects. Moving from business or economics to other social sciences or humanities is about 0.25 and 0.24 standard deviations, suggesting that changes in policy positions closely align with the overall ideological shift.

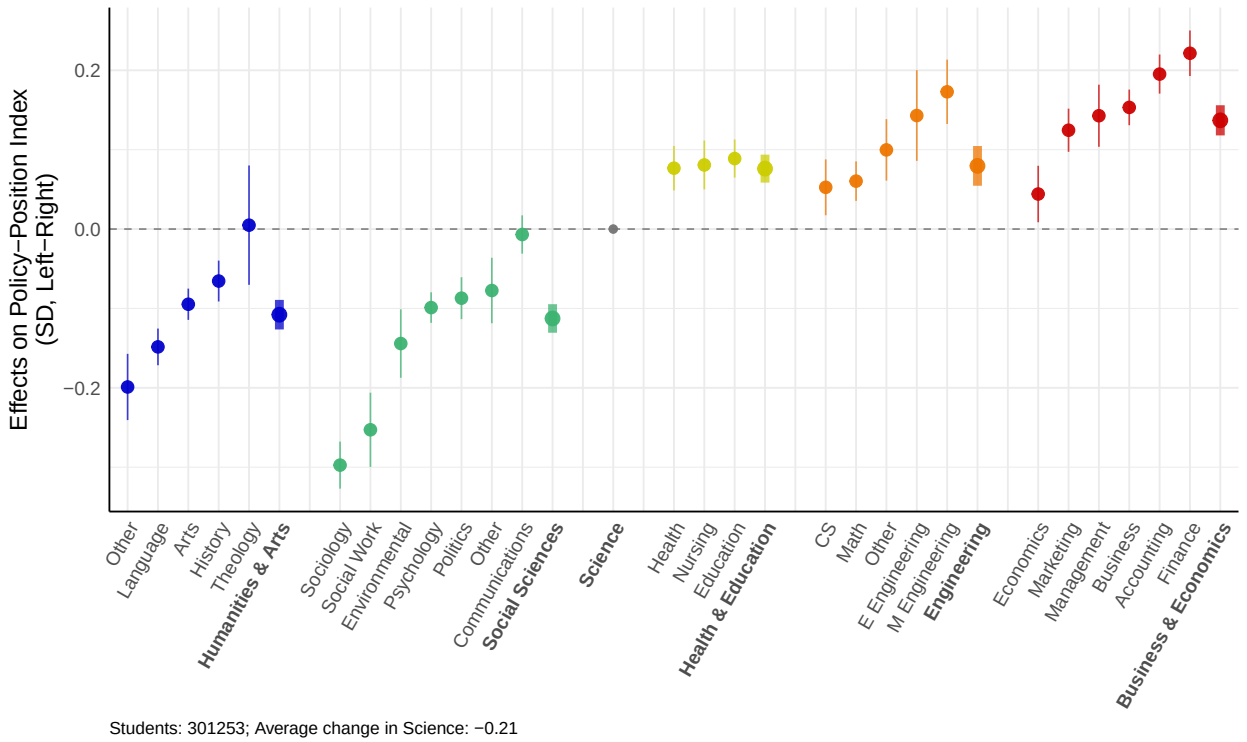
We further validate these results using the quasi-experimental design to ensure that the observed patterns reflect causal effects. Figure A17A presents event-study estimates (ρ_k from equation 4) using the policy-position index as the outcome. The results show parallel pre-trends before the expansion of business and economics programs, followed by a rightward shift in students' policy index after the shock, indicating that these expansions made students' policy positions more conservative. Figure A17B shows similar results for attitudes toward healthcare policy: students exposed to business and economics expansions become more likely to hold right-leaning views. Together, these findings reinforce that fields of study causally shape students' policy positions.

Table 3: Effects of College Majors on Policy Positions

	(1) Abortion	(2) LGBTQ	(3) Death	(4) Speech	(5) Race	(6) Change	(7) Taxes	(8) Healthcare	(9) Guns	(10) Criminal	(11) Marijuana	(12) Climate
Business &	-0.03***	0.07***	-0.05***	-0.02	0.07***	0.01	-0.18***	-0.18***	-0.03*	0.04***	0.01	-0.21***
Economics	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Engineering &	-0.04***	0.02	-0.02	-0.05***	0.08***	0.04**	-0.08***	-0.10***	-0.07***	0.03*	-0.00	-0.11***
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Health &	-0.09***	0.05***	-0.04***	0.02*	-0.03***	-0.05***	-0.02**	0.00	0.03**	0.01	-0.07***	-0.19***
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Social	0.05***	-0.06***	0.08***	0.03***	-0.12***	-0.09***	0.05***	0.07***	0.03***	-0.12***	0.09***	-0.09***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Humanities &	0.03***	-0.07***	0.11***	0.01	-0.10***	-0.04***	0.05***	0.05***	0.05***	-0.09***	0.09***	-0.09***
Arts	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	262066	194798	212513	238210	276313	210727	201434	161675	154490	144344	230710	66744
Baseline Change	0.25	-0.3	0.17	-0.19	-0.16	0.13	0.04	0.08	-0.03	-0.25	0.4	0.47

This table shows the estimates for δ_m from equation 1 and their 95% confidence intervals. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. Responses are originally coded in 1-4 scale (1=Disagree Strongly, 2=Disagree Somewhat, 3=Agree Somewhat, 4=Agree Strongly) and are transformed into measure in standard deviations. The specific policy issues are: Abortion should be legal; It is important to have laws prohibiting homosexual relationships; The death penalty should be abolished; Colleges have the right to ban extreme speakers from campus; Racial discrimination is no longer a major problem in America; Realistically, an individual can do little to bring about changes in our society; Wealthy people should pay a larger share of taxes than they do now; A national health care plan is needed to cover everybody's medical costs; The federal government should do more to control the sale of handguns; There is too much concern in the courts for the rights of criminals; Marijuana should be legalized; Addressing global climate change should be a federal priority; Baseline represent the average change in the outcome during college for students in the omitted group of majors (natural sciences). *** p<0.01, ** p<0.05, * p<0.1

Figure 8: Effects of College Majors on Policy-Position Index



This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals. The bold lines represent coefficients from a model that groups majors into six broad categories. The thinner lines show coefficients from a more detailed model with 26 specific major categories. In both models, the natural sciences serve as the omitted reference group. The outcome variable is an index variable, measuring how closely students' policy positions align with liberal perspectives (the construction procedure is described in the main text). Estimates are presented in standard deviation units. Both models control flexibly for students' baseline political ideology, policy position, intended majors, life goals, and all our main controls (see Table A5). The sample is restricted to students with non-missing values for both major and outcome.

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

$$\underbrace{p_{ict}}_{\substack{\text{Grad. political} \\ \text{ideology}}} = \underbrace{\sum_m \delta^*_m D_{icym}}_{\substack{\text{Major} \\ \text{effects}}} + \underbrace{X'_{ict}\alpha^*}_{\substack{\text{Lag. ideology and proxies,} \\ \text{Int. major and proxies}}} + \underbrace{A'_i\beta^*}_{\text{Grad. positions}} + \theta^*_t + \lambda^*_c + \varepsilon^*_{ict} \tag{5}$$

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

We begin by using the full sample from our main analysis and incorporate all policy position questions included in the survey.[22] 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 4. Policy positions explain a substantial share of the effect of college major: together, they account for roughly 45 percent of the effect of studying business and economics relative to natural sciences, and about 60–65 percent of the effects of studying social sciences or humanities. The composition of these channels, however, differs sharply across fields. For business and economics, economic policy positions explain 33 percent of the overall effect, while cultural policy positions account for only 6 percent. In contrast, for social sciences and humanities, cultural policy issues explain 62 and 48 percent of the effects, respectively, whereas economic policy positions explain 2 and 12 percent.

We next test the robustness of our results by restricting the sample to students who answered the same set of policy questions in both the baseline and graduation surveys. We focus on questions that appear in multiple survey years: two economic issues (taxation and healthcare), four cultural issues (abortion, race, LGBTQ rights, and free speech), and two additional

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

Table 4: 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	0.134	0.076	32.59%	5.53%	5.48%	56.4%
Engineering & Math	Natural Sciences	0.073	0.033	38.43%	12.92%	2.75%	45.89%
Health & Education	Natural Sciences	0.055	0.028	15.52%	13.6%	19.95%	50.94%
Natural Sciences	Social Sciences	0.098	0.035	-0.1%	62.45%	1.85%	35.8%
Natural Sciences	Humanities & Arts	0.099	0.037	2.12%	48.26%	12.07%	37.54%

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

questions (marijuana legalization and the ability to effect change). The resulting sample includes 86,717 students from surveys conducted between 1996–2001 and in 2009.

Table A11 presents the results. The overall share of the major effect explained by these questions decreases but remains significant: from 62 to 38 percent for humanities, from 65 to 53 percent for social sciences, and from 45 to 38 percent for business and economics. This suggests that missing information on other policy items is unlikely to generate substantial bias. The main conclusions remain unchanged: economic policy positions continue to account for most of the effects among economics and business majors, while cultural policy positions explain most of the effects among social sciences and humanities majors. Table A12 provides further detail on the relative contribution of each policy item.[23]

4.2 Behaviors and Life Goals

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

We first examine students' prioritization of financial success. Table 5 presents the results: economics and business lead students to place a much higher priority on financial success

[23]Table A13 allows the weights on different attitudes to vary by field, capturing the possibility that majors affect not only students' positions but also the priorities they assign to different policy domains. However, differences in priorities appear to play only a minor role in explaining the overall effects.

relative to all fields. The estimated effect is 0.32 standard deviations relative to natural sciences, and the effects are also much larger than those of other high-return majors such as engineering.[24] This pattern aligns with evidence from Bleemer and Mehta (2022), which shows that majoring in economics causes students to self-select into higher-earning industries.

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

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

We further validate these findings using the quasi-experimental design. Figure A17C presents event-study estimates (ρ_k from equation 4) using the prioritization of financial success as the outcome. The results show parallel pre-trends before the expansion of business and economics programs, followed by an increase in the importance students place on being financially well-off afterward. The estimated effect, however, is insignificant at conventional levels (p $\approx$0.11). Figure A17D presents event-study estimates (ρ_k from equation 4) using participation in demonstrations as the outcome. The results show parallel pre-trends prior to the expansion, followed by a significant decline afterward.

[24]Each response is originally measured on a 1–3 or 1–4 scale, and we use standardized versions (mean zero, unit variance) as outcome variables. Results are similar when using alternative scaling, but standardization allows interpretation in standard-deviation units.

Table 5: Effects of College Majors on Life Goals and Behaviors

	(1) Well Off	(2) Social	(3) Helping	(4) Politics	(5) Family	(6) Meaning	(7) Demonstrated	(8) Discussed Course	(9) Discussed Politics	(10) Vote Reg.
Business &	0.32***	0.08***	-0.09***	0.13***	0.08***	-0.05***	-0.05***	-0.16***	0.03***	-0.01
Economics	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Engineering &	0.14***	-0.06***	-0.14***	-0.03**	-0.01	-0.07***	-0.03**	0.01	-0.06***	-0.02***
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Health &	0.03**	0.17***	0.07***	0.03**	0.09***	0.01	0.01	-0.04***	-0.04***	-0.00
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Social	0.08***	0.34***	0.10***	0.25***	0.02*	0.03***	0.06***	-0.14***	0.24***	0.01**
Sciences	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Humanities &	-0.03*	0.23***	-0.02	0.16***	-0.07***	0.15***	0.08***	-0.08***	0.17***	0.01*
Arts	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
N	307782	307516	307575	289898	307754	307381	157101	315333	279278	95433
Baseline Change	-0.28	0.11	0.11	0.15	0.05	0.22	-0.5	NA	-0.31	NA

This table reports estimates for δ_m from equation 1 with 95% confidence intervals. Coefficients are effects of each major relative to natural sciences. The sample includes students with non-missing major and outcome. Outcomes in columns (1)-(6) are based on students' stated life goals: being well off financially (Well Off), influencing social values (Social), helping others in difficulty (Helping), keeping up-to-date with political affairs (Politics), raising a family (Family), and developing a meaningful philosophy of life (Meaning),. All goals are originally coded in 1-4 scale (1=Not important, 2=Somewhat important, 3=Very important, 4=Essential) and are transformed into a measure in standard deviations. Outcomes in columns (7)-(10) are based on students' behaviors: Participated in organized demonstrations, discussed course content with students outside of class, discussed politics, and vote registration. Responses are originally coded in 1-3 scale (1=Not at all, 2=Occasionally, 3=Frequently) and are transformed into a measure in standard deviations. Baseline represents the average change in the outcome during college for students in the omitted group of majors (natural sciences). *** p<0.01, ** p<0.05, * p<0.1

5 Mechanisms of the Impacts

The results in the previous sections show that academic fields substantially shape students' political ideology, reflecting broad shifts in their policy positions, life goals, and behaviors. This section explores the mechanisms underlying these effects. Several channels could explain these patterns: peer socialization within majors, differences in earnings expectations across fields, and the variation in intellectual environments.

We begin by examining peer socialization and earnings expectations, finding little evidence that either channel drives the results. We then analyze the role of faculty ideology, which likely correlates with other characteristics of the intellectual environment, such as teaching and academic content. Variation in faculty ideology within majors—especially in the humanities and social sciences—is strongly associated with students' ideology at graduation but uncorrelated with their baseline ideology, suggesting a causal effect. This evidence indicates that academic content, instructional approaches, and faculty interactions are the main drivers of the leftward shifts among humanities and social sciences' students.

5.1 Peer Ideology Composition

Students in different majors are exposed to peers with varying political ideology (see Figure 2). Thus, we investigate whether the political ideology of their peers influences students' political ideology. To avoid reflection issues (Manski, 1993), the analysis focuses on how one's graduation ideology is affected by their peers' baseline ideology. Throughout the analysis, peers are defined as students who enrolled in the same college in the same year and intended to pursue the same major. We define peers based on intended majors rather than actual majors for two reasons. First, since students typically finalize their majors in their second or third year, using intended majors avoids potential endogeneity from early college friendships influencing final major choices. Second, the intended major data from the baseline survey covers a much larger sample than the actual major data from graduation, reducing measurement error and increasing precision. Moreover, many students with the same intended major eventually choose the same actual major (see Figure A2A), and even those who do not are likely to share similar early course experiences.

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

same college and major from adjacent cohorts (two years ahead and two years behind). This control allows us to interpret the estimates as comparing the effects of actual peers versus similar potential peers.[25] Formally, we estimate:

$$p_{icmt} = \sigma P_{cmt}^{-i} + \beta \Phi_{cmt}^{-i} + \nu_{cm} + \iota_{ct} + \omega_{mt} + \varepsilon_{icmt} \tag{6}$$

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

Measuring peer baseline ideology based on a subsample. A key challenge in our setting is that only a subset of peers is observed. Although this subsample is representative, using the observed peers' average ideology may introduce attenuation bias by incorrectly assuming that the observed and unobserved peers share similar idiosyncratic ideology components. To address this, we construct an adjusted treatment measure that predicts the unobserved peers' ideology based on Equation 6. Specifically:

$$P_{cmt}^{-i} = \alpha_{ct} P_{cmt}^{-i,O} + (1 - \alpha_{ct}) \widehat{P_{cmt}^{-i}} \tag{7}$$

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 who are covered in the baseline survey,[26] and $\widehat{P_{cmt}^{-i}}$ is the

[25] This control for potential peers follows Guryan et al. (2009) and can be viewed as implementing a recentering strategy (Borusyak and Hull, 2023). However, absent explicit randomization, student sorting into colleges and majors may be correlated with their peers- even conditional on the average ideology of potential peers, students' unobserved characteristics may correlate with their actual peers' characteristics. The college-by-major, college-by-year, and major-by-year fixed effects help absorb these remaining differences in peer composition within the pool of potential peers.

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

predicted baseline ideology of students in college c and enrollment year t, with intended major m, based on all elements included in equation 6, except for P itself. Our control for potential peers' baseline ideology, Φ_{cmt}^{-i} from equation 6, is also adjusted using the predicted value for the unobserved potential peers. Under the assumption that peers' ideologies are uncorrelated conditional on controls, this constructed measure allows identification of peer effects and avoids attenuation bias. Appendix E formally derives this argument. In the main analysis, the sample is restricted to college cohorts, where the baseline survey includes at least five peers who account for at least 10 percent of enrolled students. The results are not sensitive to choosing other thresholds for sample inclusion.

Avoiding exclusion bias. Estimating peer effects in specifications that include fixed effects presents a unique challenge of exclusion bias (Guryan et al., 2009; Angrist, 2014; Caeyers and Fafchamps, 2024). The exclusion bias arises because individuals are mechanically excluded from their own peer group, creating a negative correlation between individual outcomes and peer averages in specifications that include fixed effects. For individuals in the same college-year-major cell, when an individual is above (below) the pool average, their exclusion from their peer group necessarily lowers (raises) the peer average, inducing a negative correlation. A split-sample method addresses this bias by dividing the data into separate groups for measuring outcomes and defining peer ideology (Stevenson, 2017). Our analysis builds on this approach by implementing a four-way split. 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.[27]

Falsification tests. Table A14 presents a balance test, which regresses students' baseline political ideology on their peers' average ideology using equation 6. Columns (1), (3), (5), (7) use the main sample, including students in both graduation and baseline survey (though the peer ideology treatment remains defined using the full baseline sample), while columns (2), (4), (6), (8) include the full sample of all students in the baseline survey. We report both unadjusted and adjusted estimates, but prefer the adjusted estimates because the unadjusted estimates suffer from greater attenuation bias. The results support the identification assumption, showing that when all controls are included, the estimated coefficients become tiny and statistically insignificant in both samples. The very large sample sizes allow rejection of

[27]We do so by estimating Equation 6 with quarter dummies interacting with all variables and fixed effects except the treatment.

a positive coefficient exceeding 0.02 in the full baseline sample. This precise null result provides compelling evidence that the identification strategy successfully isolates quasi-random variation in peers' ideology, uncorrelated with students' own ideology.

Estimated peer effects. Table 6 presents the main estimates for the impacts of peer political ideology, based on Equation 6. The table shows that while peers' ideology strongly predicts each other's graduation ideology (column 1), this relationship disappears when accounting for selection into similar programs. The estimated coefficients are small and insignificant across many different specifications. Moreover, the large sample size enables ruling out peer effects as small as 0.015 in the preferred specification (column (7), adjusted estimate).

Table 6: Estimated Effects of Peer Ideology Composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimate	0.747***	0.001	-0.006	0.004	-0.017*	-0.008	-0.011
	(0.023)	(0.011)	(0.009)	(0.012)	(0.009)	(0.012)	(0.009)
Estimate	1.206***	0.005	-0.009	0.005	-0.030*	-0.013	-0.017
(Adjusted)	(0.040)	(0.018)	(0.014)	(0.020)	(0.016)	(0.019)	(0.016)
Potential Peers' Average				V	V	V	V
Baseline			V		V		V
Program + Year FEs		V	V			V	V

Notes: This table presents estimates of peer effects on students' political ideology based on equation 6. The outcome variable measures political views using responses to the question "How would you characterize your political views?" from the graduation survey The treatment variable captures peers' responses to the same question in the baseline survey, where peers are defined as students from the same college-cohort-intended major group. The estimates account for exclusion bias using a four-way split-sample approach. Specifications include controls for the average political views of potential peers from four adjacent cohorts (Expectaions), pre-college political ideology and opinion index (Baseline), and college-major, college-cohort, and major-cohort fixed-effects (FEs). All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both major and political views, and with information available for at least 5 of their peers which represent at least 10% of the cohort. The sample includes 226947. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix Table A15 demonstrates robustness across various specifications, consistently showing small insignificant coefficients. Appendix Table A16 examines heterogeneity across major groups, finding no significant effects in any of them.

A back-of-the-envelope calculation assesses whether peer effects may explain a large fraction of the estimated college major effects. The baseline difference in political ideology between business and economics and those in social sciences and humanities majors is 0.351 standard deviations. With an upper-bound peer effect estimate of 0.015 standard deviations, the maximum peer effect from switching between these majors equals 0.005 standard deviations (0.015×0.351). Since only 37 percent of the pairs of students with the same intended major ultimately choose the same major, a more conservative estimate yields 0.014 standard deviations (0.005/0.37). This accounts for approximately 6 percent of the estimated effect of choosing business or economics over social sciences or humanities (0.22 standard deviations), suggesting that peer effects could only explain a small fraction of major-specific changes in political ideology.

5.2 Earnings Expectations and Self-Interest

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

We first examine how the estimated ideological effects of college majors vary with expected wages, proxied by the average wages of graduates from each college–major group. Using data from the College Scoreboard, we compute average post-graduation earnings by major and institution, measured 4 or 5 years after graduation.[28] Figure A18 presents descriptive correlations between expected wages and changes in political ideology during college. Panel A shows that majors associated with higher expected wages are also those with a rightward

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

political shift. However, Panel B demonstrates that once we use within-major variation in expected earnings, this relationship disappears.

To examine this more rigorously, we re-estimate equation 1 separately for students in colleges with above- and below-median expected earnings within each major group. Results are presented in Figure 9. The estimated ideological effects of each major show little variation across these two groups. Panel A focuses on students' ideology at graduation. The only significant difference appears in health and education, where students in the higher–expected-wage group shift less to the right—opposite to what a self-interest channel would predict. Panel B examines support for higher taxes on the wealthy, which also yields small and statistically insignificant differences.

As a complementary analysis, we further examine how the estimated effects of college majors evolve over time. During the sample period, two major financial crises—the early-2000s dot-com bust and the 2008 financial crisis—disproportionately affected specific fields. If earnings expectations influence political attitudes, we would expect, for instance, computer sciences' students to become more supportive of higher taxes after the dot-com crash, and finance students to do so following the 2008 crisis. To test this, we estimate Equation 1 separately for cohorts entering college in the same year, using support for higher taxation as the outcome. Figure A19 presents the results. We find no clear evidence of changes in tax attitudes corresponding to these shocks. For example, the estimated effects of engineering on tax attitudes are similar for students who enrolled between 1996 and 1998—graduating during the tech-sector downturn—and for earlier cohorts (1993–1995). Likewise, business and economics students enrolled before the 2008 crisis show comparable effects to those graduating during the recession, despite being most directly exposed to its consequences.[29]

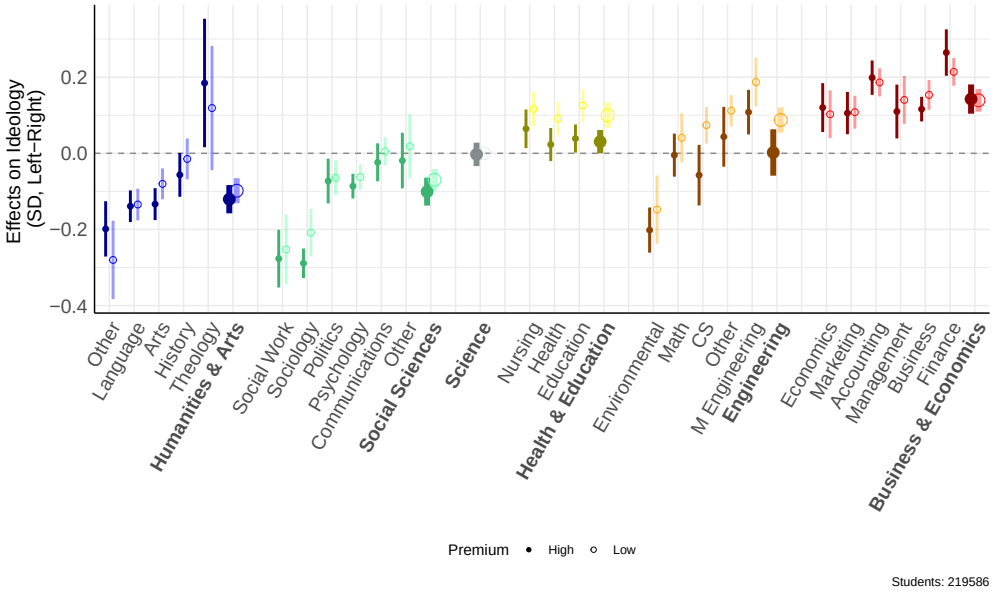
5.3 Departmental Ideology

The evidence so far suggests that neither peer ideology composition nor earnings expectations explains the observed effects of academic fields on students' ideology. Thus, a natural channel to consider is exposure to academic content, teaching practices, and faculty influence. In this section, we examine departmental ideology as a proxy for these mechanisms. We use faculty political ideology data from the HERI surveys, conducted every three years since 1989, which allow us to calculate the average ideology of faculty in each department within each college. We aggregate departments to match our major-group classification. Figure A21 displays time trends in faculty ideology by major group. Faculty across all fields have become increasingly

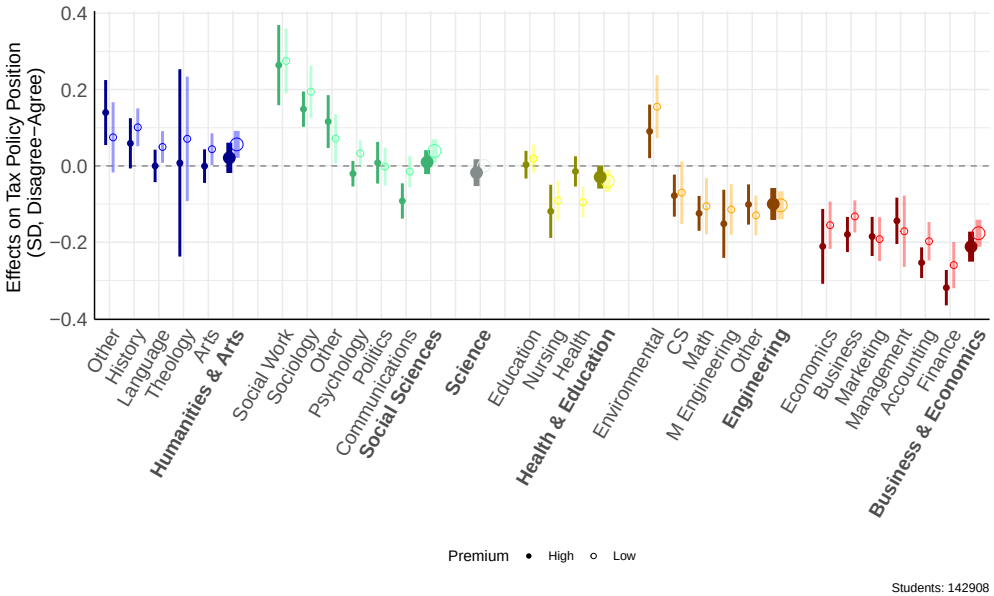
[29]Figure A20 shows these estimates for more narrow groups of majors, again with no clear pattern that supports this channel.

Figure 9: 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 along with their 95 percent confidence intervals. The bold lines represent coefficients from a model that groups majors into six broad categories. The thinner lines show coefficients from a more detailed model with 26 specific major categories. The sample is restricted to students with non-missing values for both major and outcome, as well as information on expected earnings. Low (high) earnings expectations are defined for each major-college combination as earnings below (above) the median across all colleges offering the same major. Effects are estimated for each major interacted with earnings expectations dummies. In both broad and specific majors models, the natural sciences interacted with low earning expectations serve as the omitted reference group. The outcome variables are self-placement political ideology and position toward increasing taxation for the wealthy. Estimates are presented in standard deviation units. Both models control flexibly for students' baseline political ideology, policy position, major preferences, life goals, and all our main controls (see Table A5).

liberal over time, with those in the humanities and social sciences consistently more liberal than those in other disciplines.

We further summarize the data at the college–major level, measuring the average political ideology of faculty within each program (college by major) across all available survey years. This serves as our primary measure of faculty ideology. 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 and course materials, making their ideology relevant beyond their tenure.

Next, we match this faculty ideology measure to students based on their college and intended majors. We match using intended rather than actual major, to avoid endogeneity from students self-selecting into majors due to faculty ideology, consistent with our approach for peers. We then estimate the following model:

$$p_{icmt} = \tau F_{cm} + \eta_{tm} + \psi_{ctM} + \zeta_{icmt} \tag{8}$$

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

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

To test this, we examine whether students' baseline ideology correlates with faculty ideology, conditional on the fixed effects included in 8. Table A17 reports both unadjusted and

attenuation-adjusted estimates. We focus on the adjusted ones as they are less sensitive to attenuation bias. In the full baseline survey sample, we find a significant positive relationship between students' baseline ideology and faculty ideology, even after conditioning on the fixed effects. This suggests some degree of differential selection. However, when restricting the sample to colleges and cohorts observed in the graduation survey, this relationship becomes insignificant. Moreover, when restricting it to students specifically in the graduation survey, the estimate becomes negative and insignificant. This pattern may reflect either heterogeneous selection across colleges or reduced statistical power in the smaller sample. We therefore proceed with the main analysis using our sample while also examining the robustness of our findings to potential violations within it. Importantly, we also control directly for students' baseline ideology and other pre-college characteristics, further mitigating concerns about residual selection bias.

Table 7 presents the main estimation results. The effects of faculty ideology are positive and statistically significant. As we sequentially add controls, the estimated magnitudes remain stable while precision improves. The adjusted estimate with controls is 0.034 standard deviations, implying that students studying the same major in departments with faculty who are one standard deviation more left-leaning become about 0.034 standard deviations more left-leaning by graduation. This result holds even though students' baseline ideology is uncorrelated with faculty ideology in this sample, as shown earlier. Moreover, when restricting the sample to departments where a larger share of faculty are observed, the results remain significant and increase to 0.05, confirming their robustness.

Furthermore, we estimate equation 8 separately by major group. Table 8 reports the results. The adjusted estimates show no significant effects in any field except the social sciences and the humanities and arts, where faculty ideology has a clear impact. In these majors, a one-standard-deviation increase in faculty liberalism induces larger shifts by 0.069 and 0.08 standard deviations, respectively, in students' ideology—both highly statistically significant.

The average gap in our faculty ideology measure between students who intended business or economics and those who intended social or humanities is 1.2 standard deviations. Estimates for the social sciences and humanities imply that students exposed to faculty who are 1.2 points more left-leaning become 0.083 and 0.098 standard deviations more left-leaning, respectively. This corresponds to roughly 40 percent of the total estimated effects of studying these majors instead of economics or business. Because faculty ideology is only a proxy for broader mechanisms—such as content and teaching—this evidence suggests that these channels play a central role in shaping students' political ideology.

Moreover, as shown in Figure A21 the ideological gap between faculty in economics and business and those in the social sciences has widened over time. On a raw scale, it rises from 0.6–0.7 in 1989 to nearly 0.8 in 2020. This is consistent with the growing effects of college majors on ideology documented earlier (Table A8).

Table 7: Effects of Faculty Ideology on Students' Ideology

	(1)	(2)	(3)	(4)	(5)
Estimate	0.011	0.015***	0.013**	0.017**	0.033***
	(0.007)	(0.006)	(0.006)	(0.007)	(0.012)
Estimate (Adjusted)	0.033**	0.037***	0.034***	0.036***	0.051***
	(0.015)	(0.013)	(0.013)	(0.013)	(0.018)
N	155,177	155,177	155,177	116,256	34,930
Min Share Faculty	0.1	0.1	0.1	0.3	0.5
Major FEs	V	V	V	V	V
College-Major-Group-Year FEs	V	V	V	V	V
Baseline		V	V	V	V
All Main Controls			V	V	V

Notes: This table presents estimates of the effects of faculty ideology on students' political ideology based on equation 8. The outcome variable measures political ideology using responses to the question "How would you characterize your political views?" from the graduation survey The treatment variable captures faculty's responses to the same question, based on all faculty from the department that correspond to the student's college and intended major. Specifications include controls for major-cohort, college-cohort, and major-group-college-cohort fixed-effects, pre-college political ideology (Baseline), and all our main controls. All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both intended major and political ideology, and with information available for at least 3 of their faculty included in the survey which represent at least 10% of the faculty in the department. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Together, the evidence in this section indicates that the effects of fields of study on students' ideology are not driven by socialization or economic returns. Instead, they appear to stem from differences in the academic environments across majors. The effects are stronger in

departments with more pronounced ideological orientations, suggesting that course content, teaching practices, and faculty interactions are key channels. Future work using direct measures of these dimensions could further clarify their respective roles.

Table 8: Effects of Faculty Ideology, by Major-Group

	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.012	-0.002	0.003	0.041***	-0.036*	0.025**
	(0.011)	(0.010)	(0.013)	(0.014)	(0.020)	(0.010)
Estimate	0.022	-0.007	0.012	0.082***	-0.068	0.069***
(Adjusted)	(0.027)	(0.029)	(0.032)	(0.029)	(0.044)	(0.021)
Sample	Economics& Business	Engineering& Math	Health& Education	Humanities& Arts	Natural Sciences	Social Sciences
N	25,277	20,407	25,905	27,200	18,963	37,138

Notes: This table presents estimates of the effects of faculty ideology on students' political ideology based on equation 8. The outcome variable measures political ideology using responses to the question "How would you characterize your political views?" from the graduation survey The treatment variable captures faculty's responses to the same question, based on all faculty from the department that correspond to the student's college and intended major. The estimation samples are defined by students' intended major-groups. Specifications include controls for major-cohort, college-cohort, and major-group-college-cohort fixed-effects, pre-college political ideology (Baseline), and all our main controls. All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both intended major and political views, and with information available for at least 3 of their faculty included in the survey which represent at least 10% of the faculty in the department. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6 Implications

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

6.1 Voting

While our analysis focuses on ideological self-identification, these effects likely extend to voting behavior. Using voting patterns from student populations in the CCES and ANES—which include similarly worded ideological questions—we can approximate the electoral implications. Our estimates indicate that majoring in the humanities or social sciences increases the likelihood of identifying as liberal by 10 percentage points relative to business or economics majors—shifting seven percentage points away from conservatism and three points from the middle. Based on the voting patterns in Appendix Figure A3, this implies roughly a 6.35–percentage–point higher probability of voting Democratic ($0.8 \times 7 + 0.25 \times 3 \approx 6.35$).[30]

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 percentage point higher Democratic vote share during ages 13–19 increases the likelihood of voting Democratic by 0.05 percentage points. 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 percentage points higher Democratic support.

6.2 Polarization and Divides

Our findings have important implications for understanding polarization and political differences between groups. First, they suggest that the widely discussed "education divide" is partly driven by the specific fields students study in college. We illustrate this through a counterfactual simulation shown in Panels A and B of Table 9. Panel A focuses on ideological change during college. Students in our sample enter college with an average ideology score of 3.000 and graduate with an average of 2.870, indicating a leftward shift of 0.13 points on the 1–5 scale. However, if all students majored in economics or business, this change would shrink to 0.010.

Consequently, the ideological gap between college graduates and non-graduates would narrow substantially. To quantify this, we calculate the ideology gap between college graduates and individuals with no college education in the CCES at ages 22–30. In the 1–5 scale, the observed ideological gap between college graduates and non-graduates is 0.406. Under the counterfactual where all students majored in economics or business, this gap would decline

[30] Given that we also document small positive effects on voter registration—students in the humanities and social sciences are slightly more likely to be registered—the impact on actual voting may be somewhat larger.

Table 9: Field Specialization and Politics: Counterfactuals

A. Liberalization	
Freshman mean	2.999
Senior mean	2.870
Change: Senior − Freshman	-0.129
Counterfactual Change (All Business & Economics)	-0.010
B. College Divide	
Gap (CCES)	-0.406
Counterfactual Gap (All Business & Economics)	-0.287
C. Gender Divide	
Freshman Gap	0.160
Senior Gap	0.201
Counterfactual Gap (Uniform Major)	0.169
D. Polarization	
Freshman Variance	0.663
Senior variance	0.723
Counterfactual Variance (Uniform Major)	0.696

Note: This table reports counterfactual quantifications based on our main estimates of the effects of college majors on students' political ideology. Panel A shows freshman and senior means in ideology (1–5 scale), the senior–freshman gap, and the counterfactual gap if all students had majored in business or economics. Panel B shows the gap between four-year college graduates and non-college individuals (high-school education or less) in the CCES at ages 22–30 (in 1-5 scale), and the counterfactual gap if all students had majored in business or economics. Panel C shows gender gaps at freshman and senior year, as well as the counterfactual gender gap if all students had majored in natural sciences. Panel D showns the variance of ideology at freshman and senior year, and the counterfactual variance if all students had majored in natural sciences.

to 0.287. This counterfactual implies that field specialization accounts for roughly 30 percent of the education-based ideological divide.

Furthermore, since students self-select into majors, our findings shed light on political differences within the educated population. As shown in Panel B of Table 9, differential major selection explains a significant portion of the gender gap in political ideology: it accounts for

almost 80 percent of the increase in gender gaps during college (0.032 of 0.041 change), and about 15 percent of the overall gap in graduation (0.20).

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. Therefore, if all students pursued the same field, the variance of graduation ideology would decrease from 0.723 to 0.696. This accounts for almost half of the increase in the variance in ideology during college (from 0.663 to 0.723).

Therefore, these results have implications for the relationship between education and social cohesion. Historically, education at earlier stages has been seen as fostering civic cohesion and trust, which are often cited as channels linking education to economic growth (Gradstein and Justman, 2002; Glaeser et al., 2007). However, our results suggest that the increasing academic specialization characteristic of modern college education may instead contribute to greater ideological fragmentation rather than cohesion.

7 Conclusion

This study provides new evidence on how academic fields shape the political preferences of college graduates. Using a large-scale survey and combining conditional-on-observables and quasi-experimental analyses, we show that fields have a substantial influence on students' ideology, policy positions, behaviors, and life goals. Humanities and social sciences make students significantly more left-leaning relative to natural sciences, while economics and business make them more right-leaning. These ideological shifts mirror effects on specific policy domains—economic issues drive the rightward shift in economics and business, whereas cultural issues drive the leftward shift in humanities and social sciences.

We also show that the influence of academic fields extends beyond ideology to behaviors. Humanities and social sciences increase students' political engagement—they discuss politics more often, participate in demonstrations, and place greater emphasis on social and civic values. In contrast, economics and business lead students to prioritize financial success more strongly, suggesting that preference formation is an important mechanism behind the economic returns to these fields.

We further examine the mechanisms behind these effects. The evidence shows that neither peer socialization nor earnings expectations and self-interest explain the ideological shifts across fields. Instead, the results point to academic content and teaching—the defining

characteristics of each field's intellectual environment—as the primary channels driving these effects.

The magnitudes of our estimates are substantial, suggesting that college fields of study represent a pivotal life experience in the formation of political ideology. In terms of voting, our estimates imply that studying the social sciences or humanities increases the likelihood of voting Democratic by about six percentage points—an effect comparable in size to other major formative experiences, such as exposure to different political environments during adolescence. Beyond individual ideology, these findings have broader political implications: field of study choice appears to play an important role in the college divide, helps explain a sizable share of the gender gap in political ideology, and contributes to the growing ideological polarization among college graduates.

In summary, this study highlights the central role of academic fields in shaping students' attitudes. Policies that expand or restrict access to certain fields may therefore have political consequences. The normative interpretation is open: on one hand, fostering new perspectives on social issues aligns with the long intellectual tradition viewing education as a means of enlightenment (Plato, 1992; Kant, 2002; Dewey, 1916). On the other hand, rising concern—especially from the political right—focuses on potential ideological bias within academia.

In light of current debates over ideological bias in academia, our findings raise important questions for future research. Do colleges effectively promote exposure to diverse viewpoints? Do they foster critical inquiry and intellectual openness? These questions are especially salient in light of recent evidence by Braghieri (2024), who shows that social-image concerns and norms of political correctness lead some students to conceal their views. Understanding these dynamics is essential for assessing how college education shapes intellectual diversity and for informing discussions about public trust in educational institutions.

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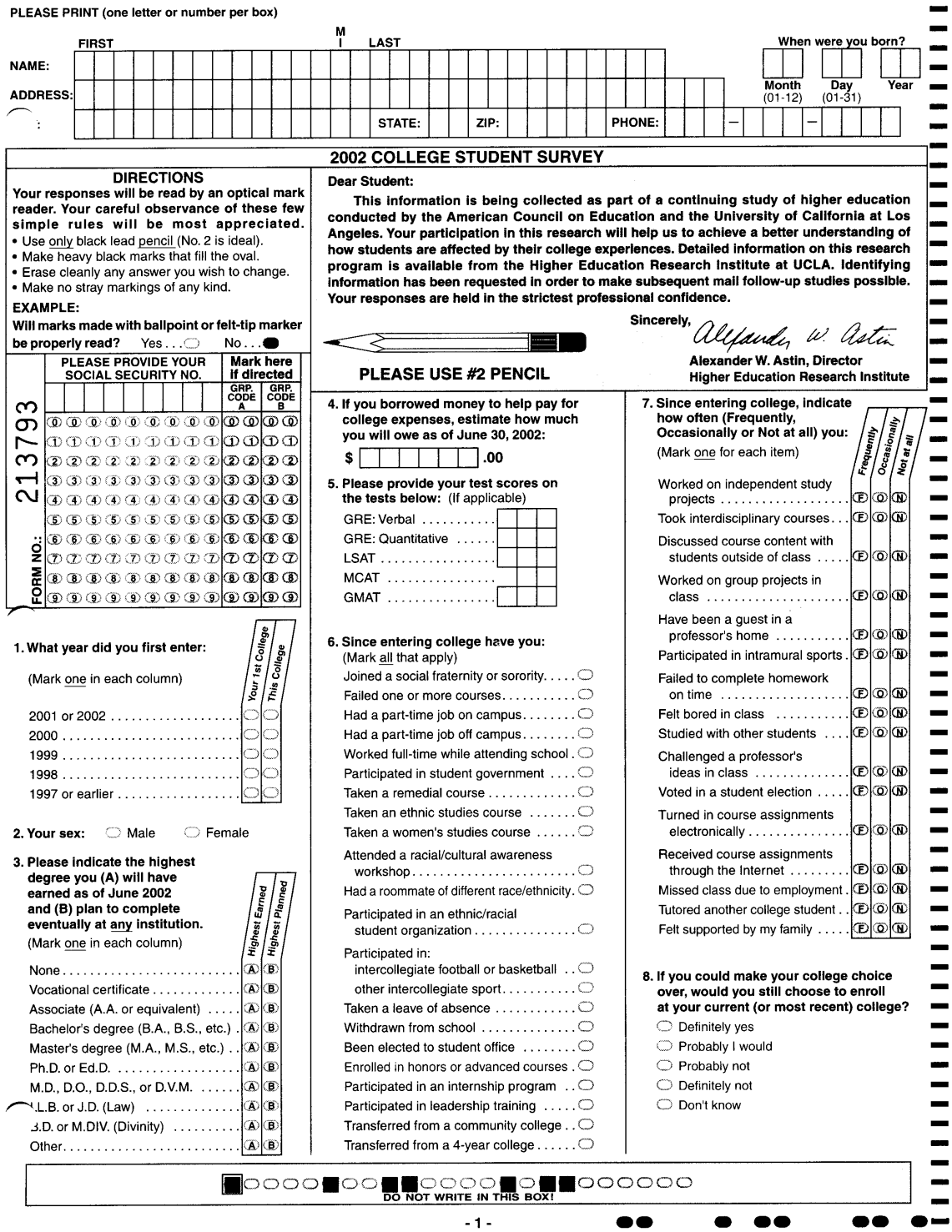
Appendix A Appendix Figures

Figure A1: 2002 College Student Survey Questionnaire



PLEASE PRINT (one letter or number per box)

NAME: FIRST | M I | LAST

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

ADDRESS:

STATE: ZIP: PHONE: – –

2002 COLLEGE STUDENT SURVEY

DIRECTIONS

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

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

EXAMPLE:

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

PLEASE PROVIDE YOUR SOCIAL SECURITY NO. | Mark here if directed | GRP. CODE A | GRP. CODE B

FORM NO: 213793

Dear Student:

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

Sincerely,

Alexander W. Astin, Director
Higher Education Research Institute

PLEASE USE #2 PENCIL

1. What year did you first enter: (Mark one in each column) | Your 1st College | This College

2001 or 2002 | 2000 | 1999 | 1998 | 1997 or earlier

2. Your sex: ○ Male ○ Female

3. Please indicate the highest degree you (A) will have earned as of June 2002 and (B) plan to complete eventually at any institution. (Mark one in each column) | Highest Earned | Highest Planned

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

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

$ .00

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

GRE: Verbal | GRE: Quantitative | LSAT | MCAT | GMAT

6. Since entering college have you: (Mark all that apply)

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

7. Since entering college, indicate how often (Frequently, Occasionally or Not at all) you: (Mark one for each item) | Frequently | Occasionally | Not at all

Worked on independent study projects | Took interdisciplinary courses | Discussed course content with students outside of class | Worked on group projects in class | Have been a guest in a professor's home | Participated in intramural sports | Failed to complete homework on time | Felt bored in class | Studied with other students | Challenged a professor's ideas in class | Voted in a student election | Turned in course assignments electronically | Received course assignments through the Internet | Missed class due to employment | Tutored another college student | Felt supported by my family

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

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

DO NOT WRITE IN THIS BOX!

- 1 -

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

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

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

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

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

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

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

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

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

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

14. Please mark your probable career/occupation below:

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

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

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

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

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

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

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

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

(Mark one for each item)

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

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

(Mark one for each item)

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

20. Your current religious preference: (Mark one)

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

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

(Mark one for each item)

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

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

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

(Mark one in each row)

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

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

(Mark one for each item)

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

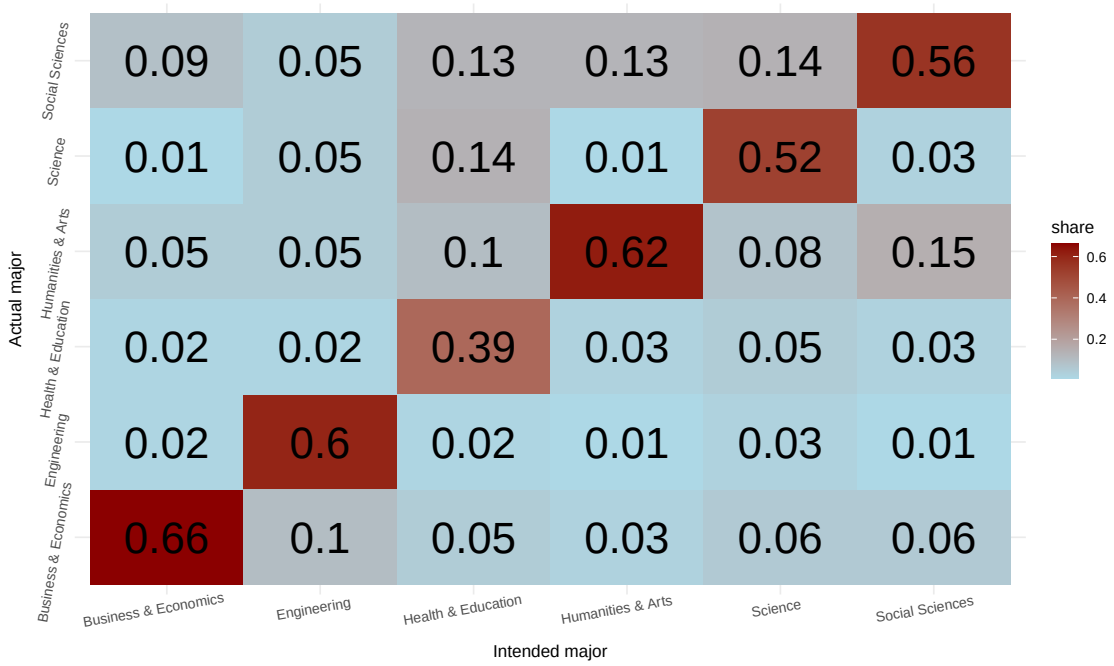
32. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	39. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	46. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
33. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	40. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	47. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
34. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	41. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	48. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
35. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	42. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	49. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
36. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	43. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	50. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
37. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	44. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	51. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ
38. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	45. Ⓐ Ⓑ Ⓒ Ⓓ Ⓔ	**THANK YOU!**

2002-R17805-Questar/10744C-03-54321

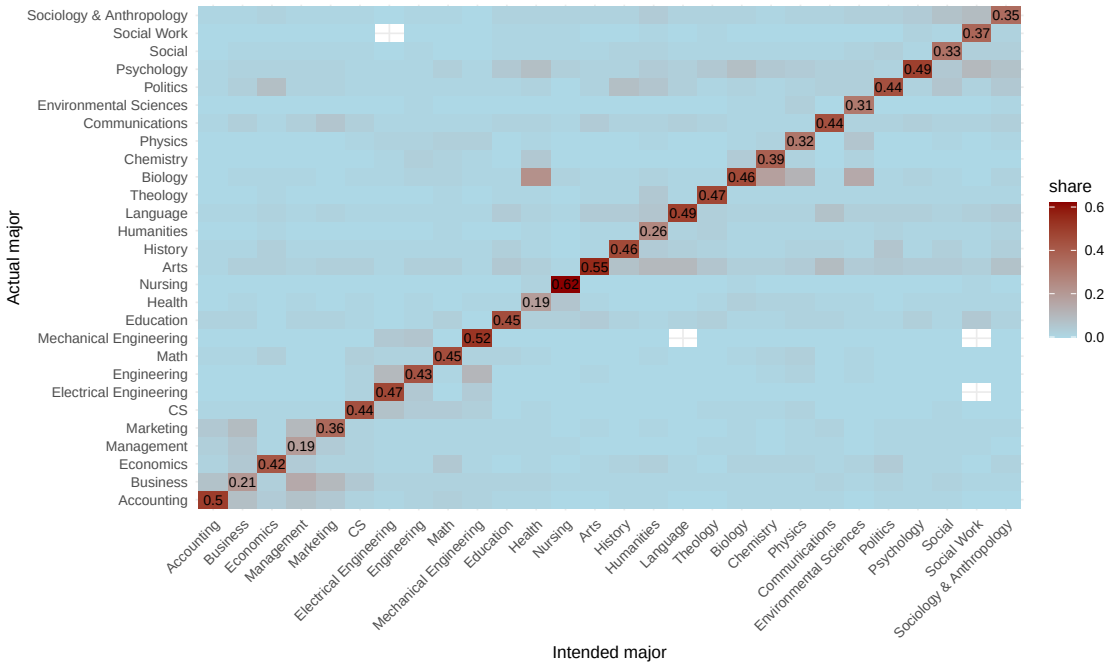
Note: This figure shows the complete 2002 College Student Survey questionnaire from the Cooperative Institutional Research Program (CIRP), conducted by the Higher Education Research Institute (HERI) of UCLA. This survey represents a typical College Senior survey used throughout our analysis period (1990-2015).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

Figure A2: Intended and Actual Majors

(A) Broad Groups

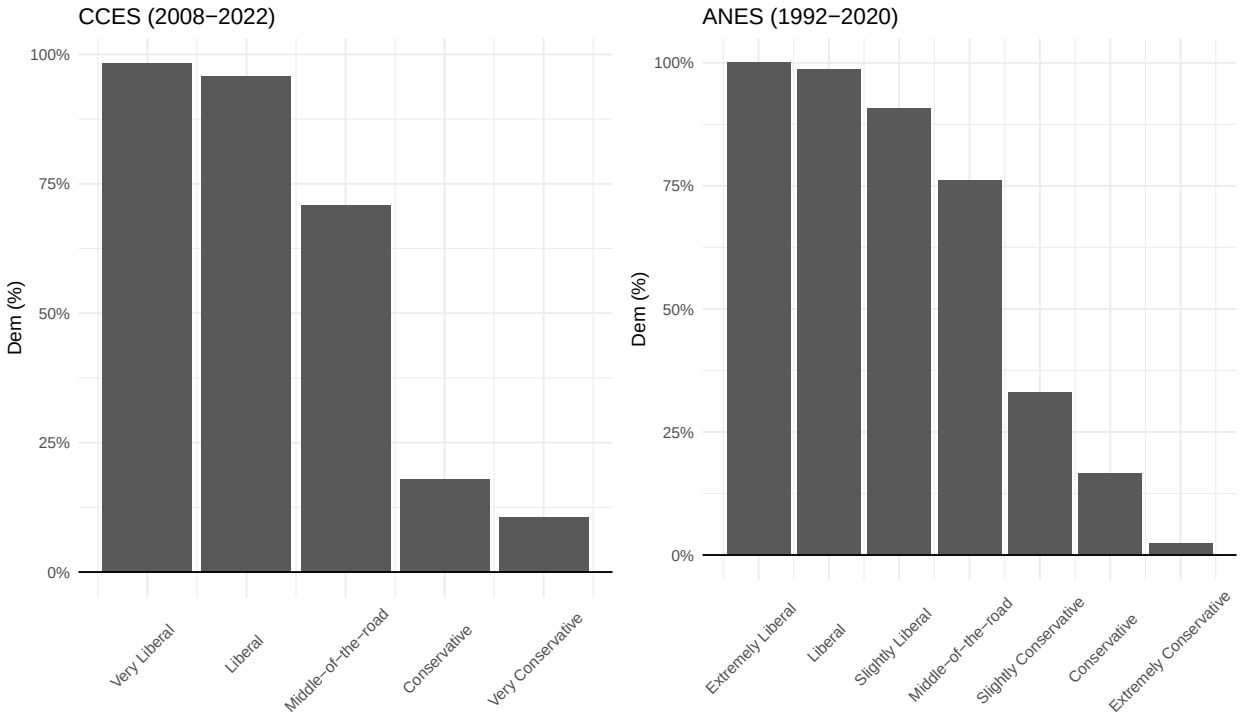


(B) Detailed Groups



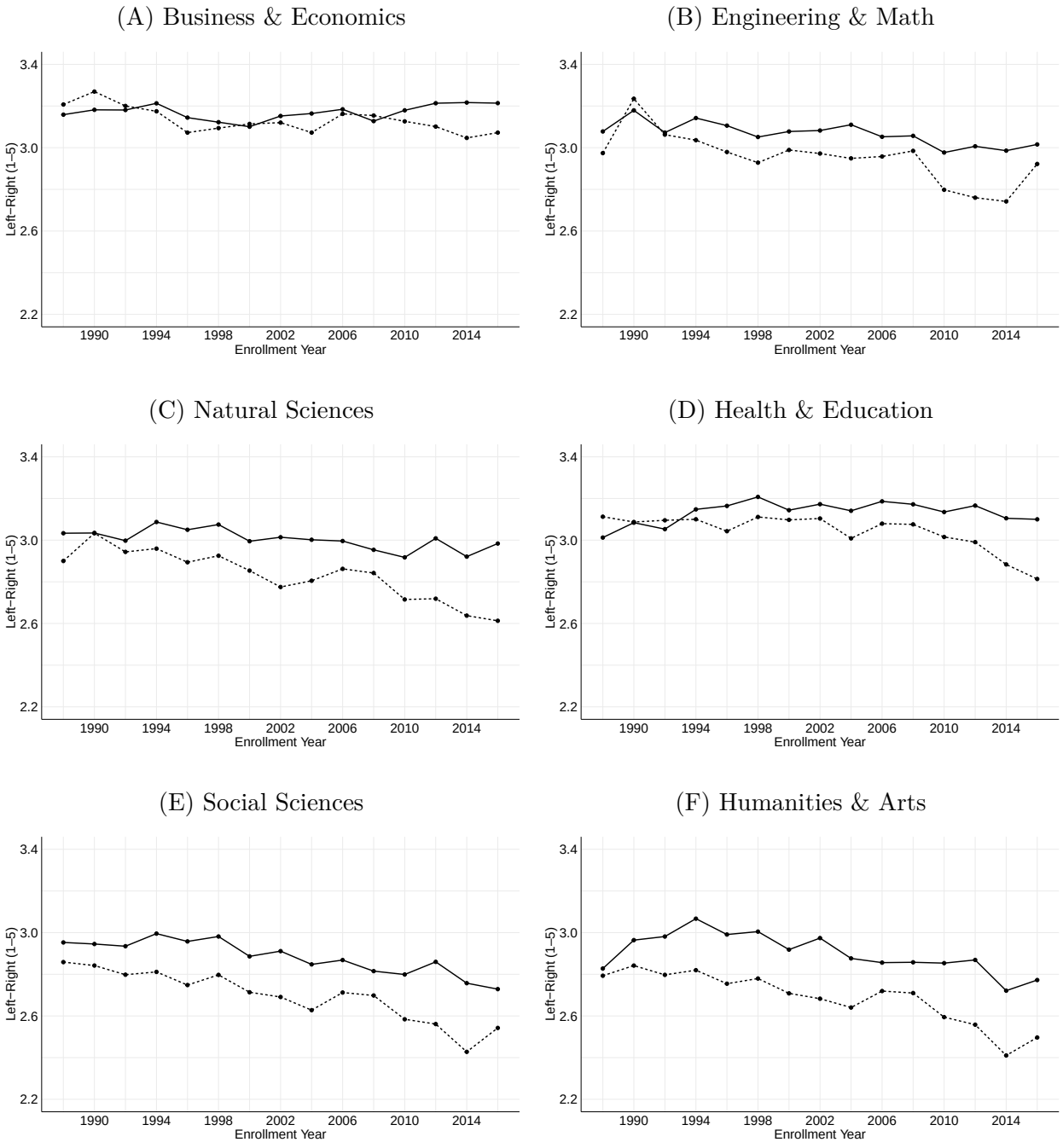
Note: This figure plots students' intended and actual majors. The sample includes all students in our sample with non-missing values for this variable, both in the entry and exit surveys (324,090 students). The first (second) panel shows the results in broad (detailed) major groups.

Figure A3: Students' Attitudes and Voting in National Surveys



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

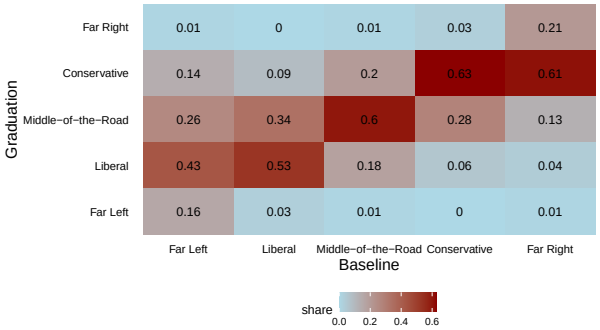
Figure A4: Students' Political Ideology, HERI Survey, 1990–2015



Note: This figure plots students' political ideology at entry (solid lines) and at graduation (dashed lines). The sample includes 121 colleges that appear consistently in the data—those observed before 1995, after 2005, and for at least 11 years. The figure uses all students from these colleges with non-missing ideology measures in both entry and exit surveys (286,823 students). Political ideology is measured on a five-point scale, with students self-identifying as far right, conservative, middle-of-the-road, liberal, or far left. Panels A–F restrict the sample to students within specific groups of majors.

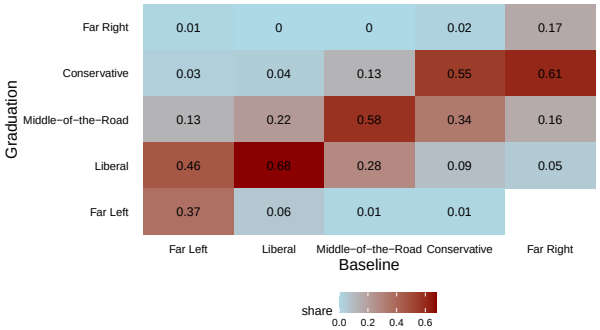
Figure A5: Transitions in Political Ideology During College

(A) Business & Economics



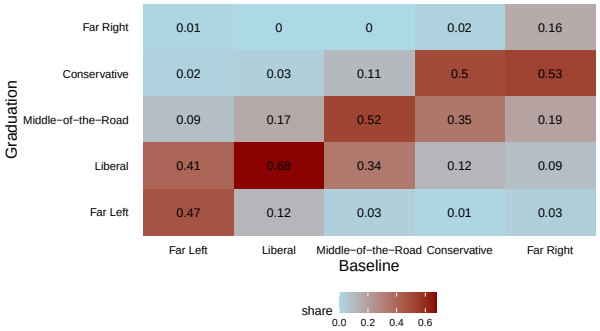
(B) Engineering & Math

(C) Natural Sciences



(D) Health & Education

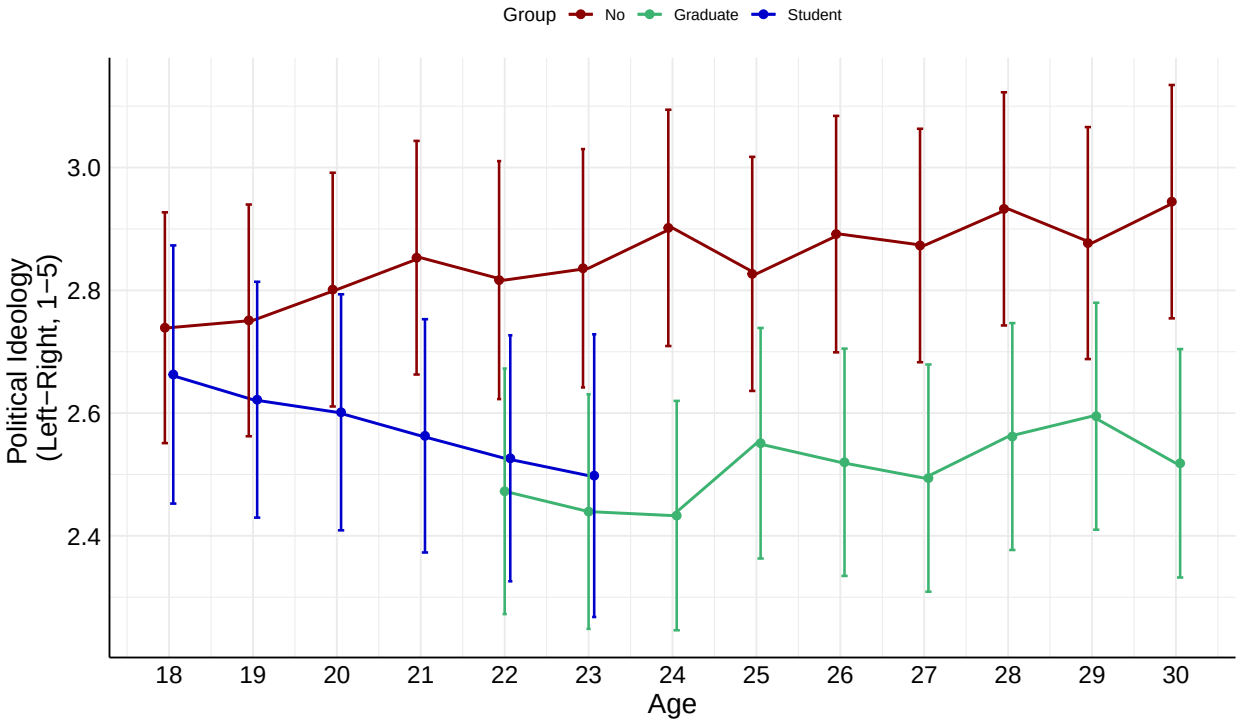
(E) Social Sciences



(F) Humanities & Arts

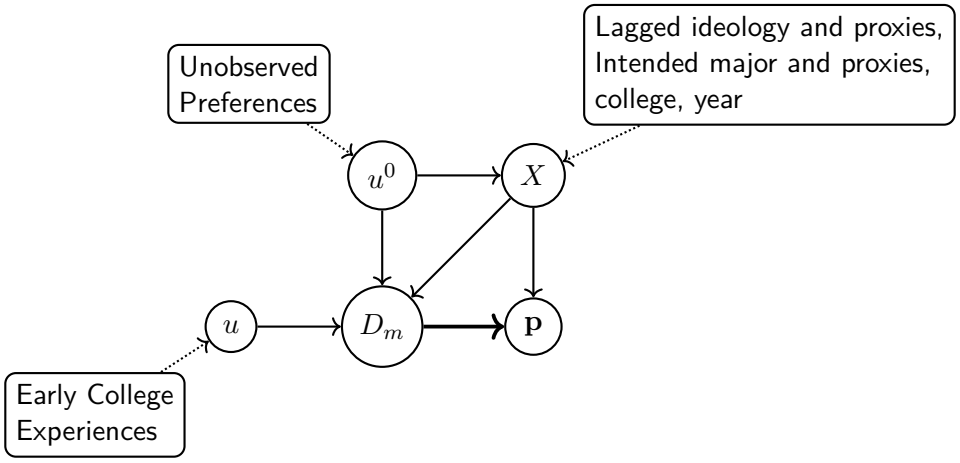
Note: This figure plots the changes in students' baseline and graduation political ideology. The sample includes all students with non-missing values for this outcome in the entry and exit surveys, totaling 299,630 students. The samples in panels A–F are restricted to those in specific groups of majors.

Figure A6: Political Ideology by Age, Students and non-Students, CCES Survey



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

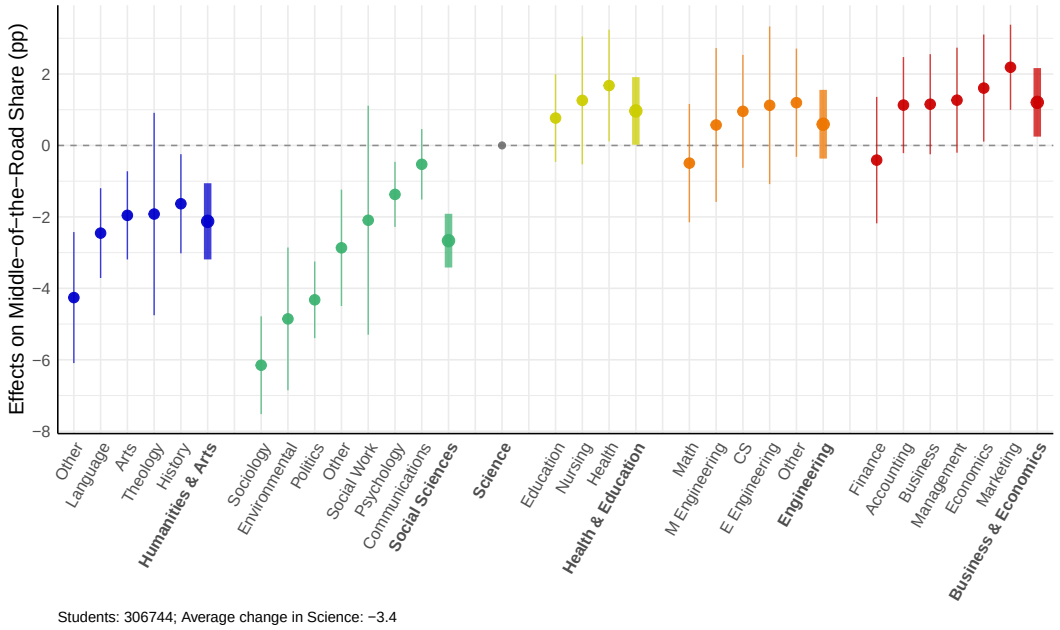
Figure A7: Directed Acyclic Graph Illustrating Our Baseline Identification Approach and Caveats



Note: This figure illustrates our baseline identification approach (equation 1) and its potential caveats using a directed acyclic graph. D_m denotes actual major choices, p represents graduation political ideology, and X includes the main control variables. u^0 captures unobserved preferences influencing major choices that are not controlled for, while u represents early college experiences occurring after the baseline survey that may also affect major choices. The graph demonstrates our identification assumption: neither u nor u^0 directly affects graduation political ideology. Figure A11 demonstrates the negative control exposure test for the validity of this assumption.

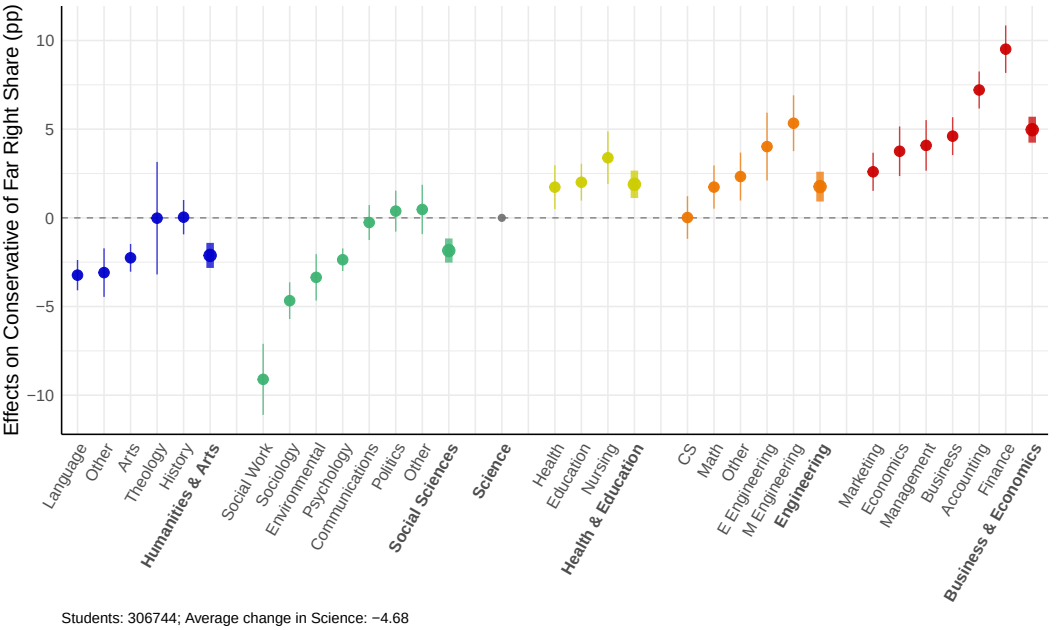
Figure A8: Effects of College Majors on Political Ideology, Indicator Outcomes

(A) Middle of the Road Share



Students: 306744; Average change in Science: −3.4

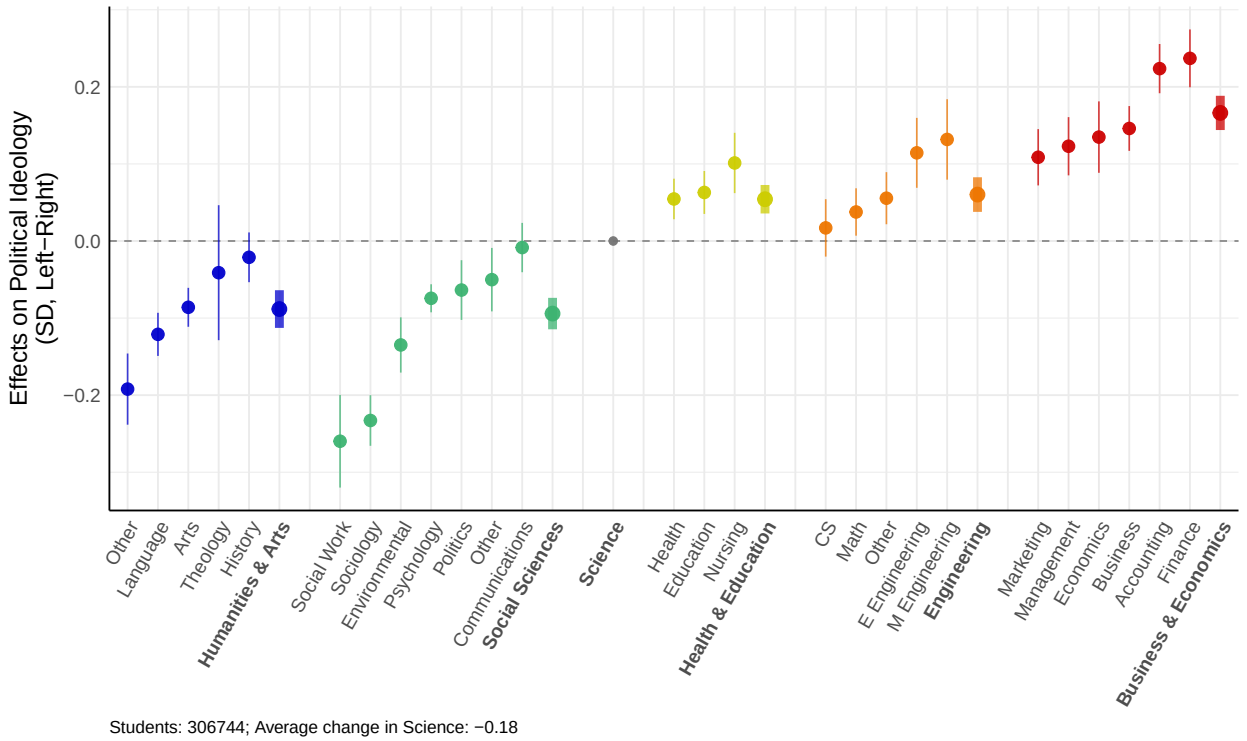
(B) Conservative Share



Students: 306744; Average change in Science: −4.68

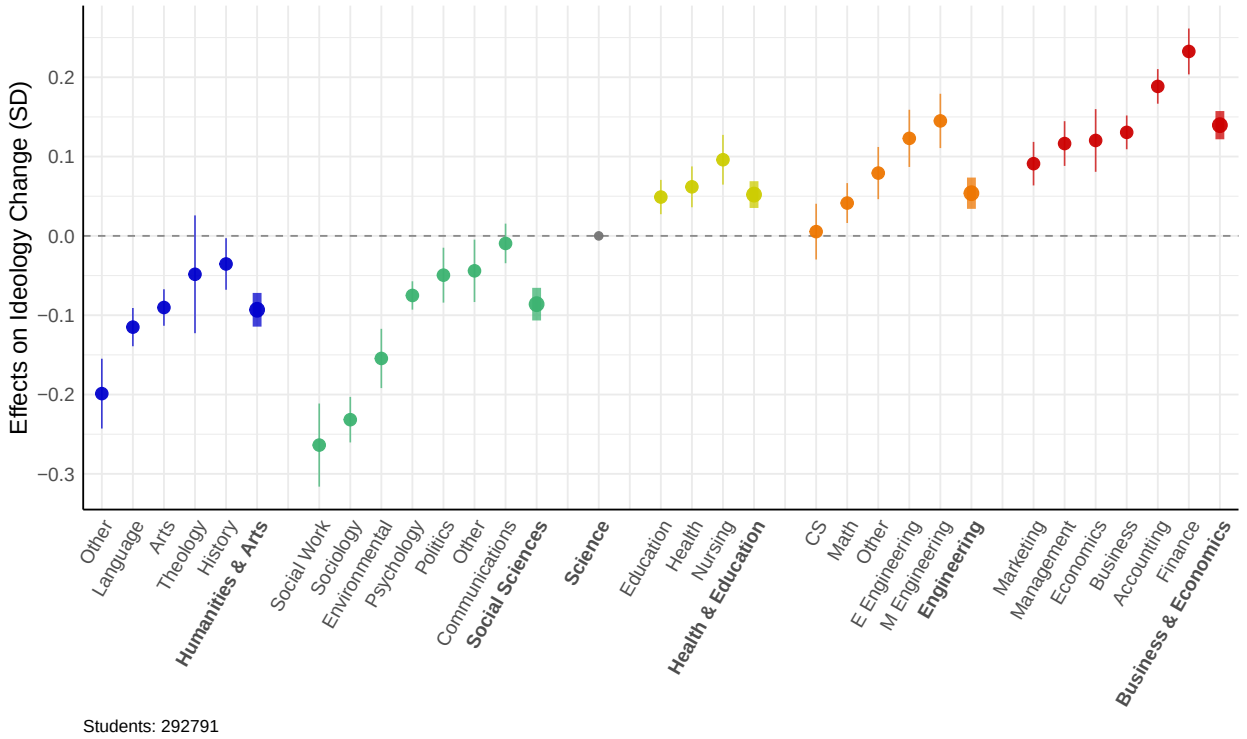
Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals. The bold lines represent coefficients from a model that groups majors into five broad categories, with STEM as the omitted reference group. The thinner lines show coefficients from a more detailed model with 28 specific major categories, where natural sciences (physics, chemistry, biology) is 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 percentage points. Both models control flexibly for students' baseline political ideology, policy position, intended majors, life goals, and all our main controls (see Table A5).. The sample includes only students with non-missing values in the major and outcome. Standard errors are clustered at the college level.

Figure A9: Effects of College Majors on Political Ideology, Robustness to Contamination Bias Correction



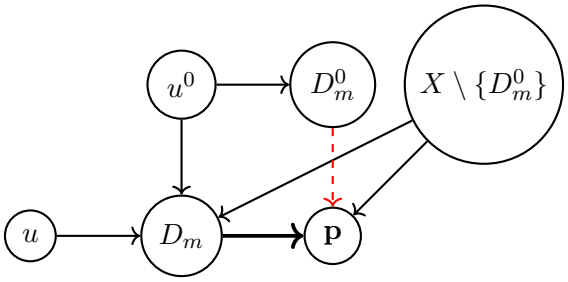
Note: This figure plots the estimates for δ_m from equation 1 and their 95 percent confidence intervals when using graduation ideology as the outcome variable. The estimation follow a correction for contamination bias, as offered by Goldsmith-Pinkham et al. (2024) and discussed in the main text. The estimated coefficients represent the effects of studying each major relative to natural sciences which are the omitted group. The outcome variable is students' responses to the following question: "How would you characterize your political views?". Estimates are shown in standard deviations. The sample includes only students with non-missing values in the major and outcome.

Figure A10: Effects of College Majors on Political Ideology Change



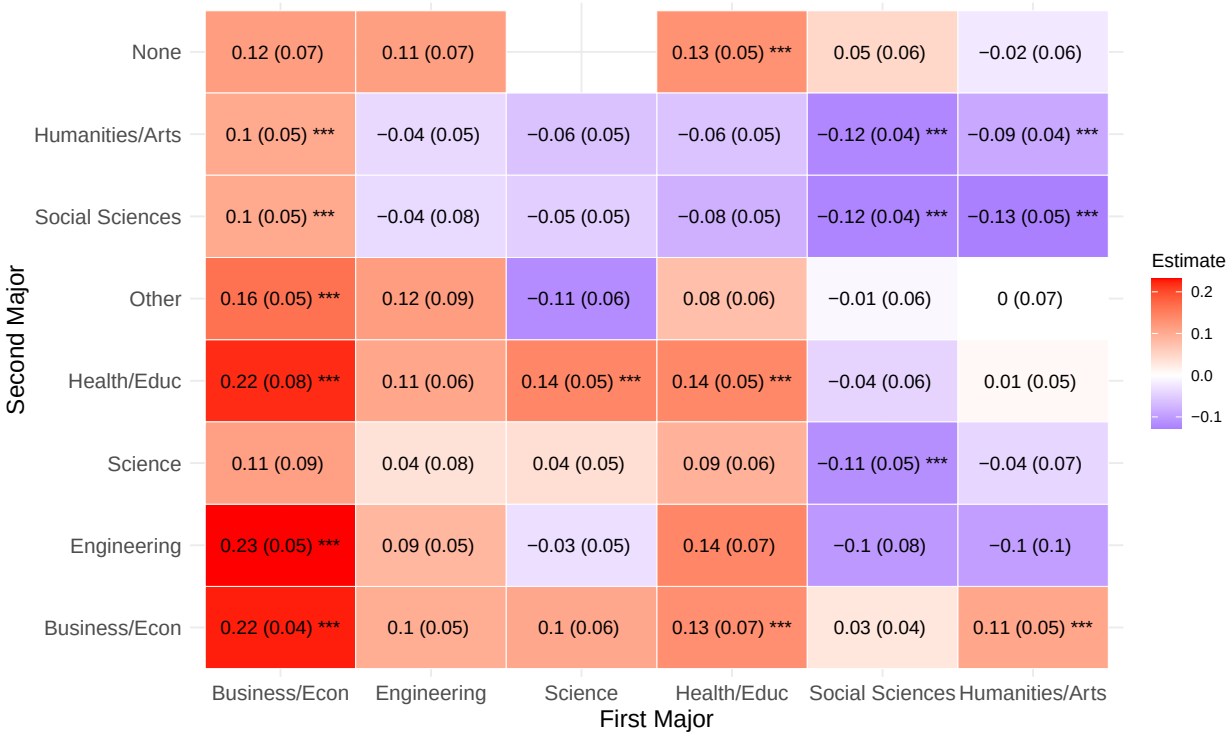
Note: This figure plots the estimates for δ_m from equation 1 and their 95 percent confidence intervals when using the change in attitudes as the outcome variable. The estimated coefficients represent the effects of studying each major relative to natural sciences which are the omitted group. The outcome 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 standard deviations. The sample includes only students with non-missing values in the major and outcome.

Figure A11: DAG Illustrating Negative Control Exposure Test Using Intended Majors



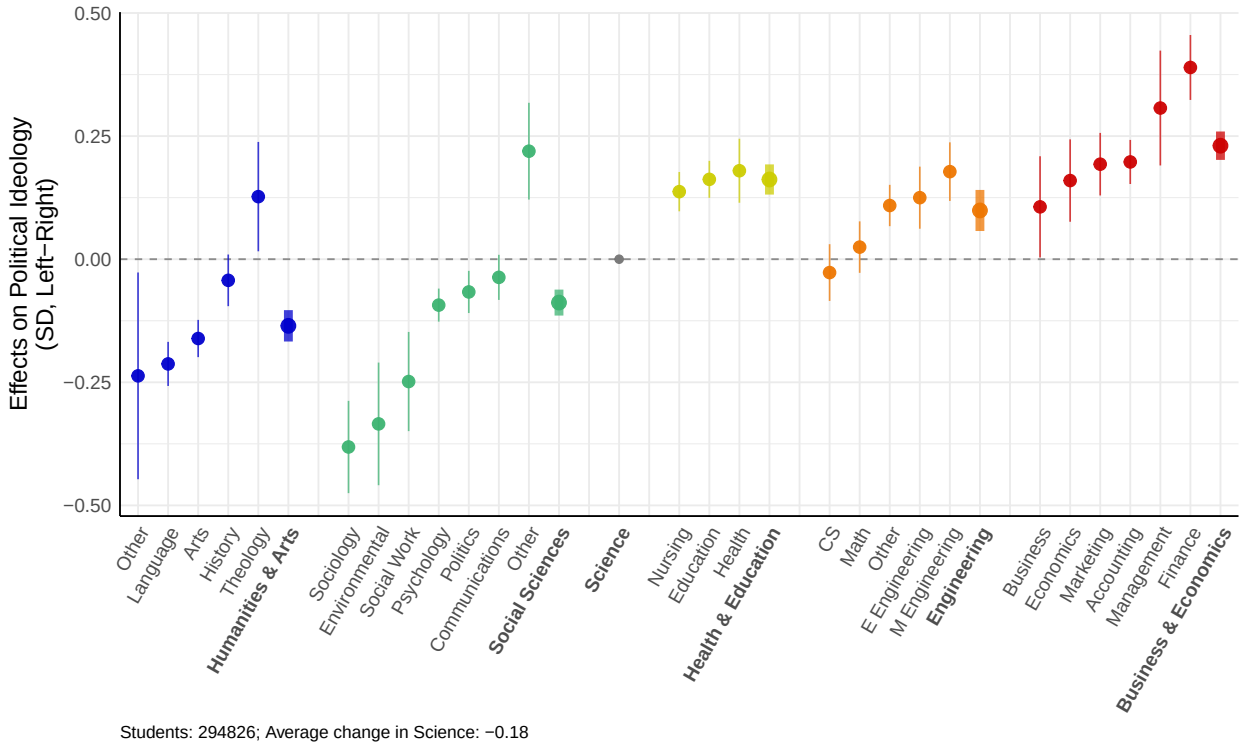
Note: This figure illustrates the negative control exposure test for the validity of our baseline identification approach (estimated using equation 1) using a directed acyclic graph. D_m denotes actual major choices, p represents graduation political ideology, and X includes the main control variables. The graph demonstrates our test supporting the assumption that unobserved preferences (u^0) do not directly influence graduation ideology, using intended majors (D_m^0) as a proxy (negative control). u represents early college experiences after the baseline survey that may also affect major choices, which we assume have no direct impact on students' graduation ideology.

Figure A12: Effects of College Majors and Double Majors on Students' Ideology



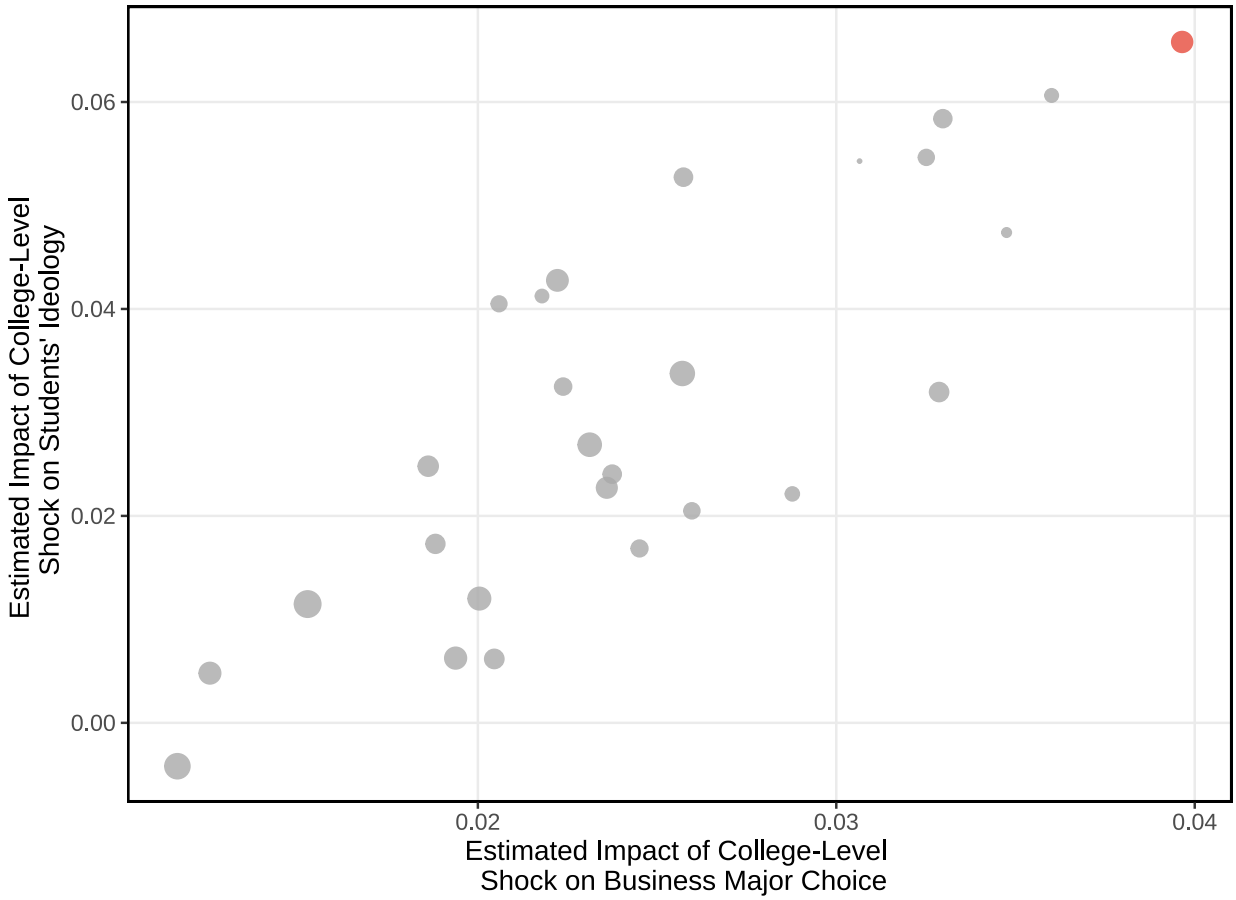
Note: This figure plots the estimates for δ_m from equation 1 and their 95 percent confidence intervals from a model with interaction between first and second majors. The estimated coefficients represent the effects of studying each combination of majors relative to STEM with no second major (the omitted group). The outcome variable is students' responses to the following question: "How would you characterize your political views?". Estimates are shown in standard deviations. The sample includes only students with non-missing majors and political ideology in the graduation survey from enrolling cohorts since 2005. Standard errors are clustered at the college level.

Figure A13: Effects of College Majors on Political Ideology, Using Intended Majors as Instrumental Variables



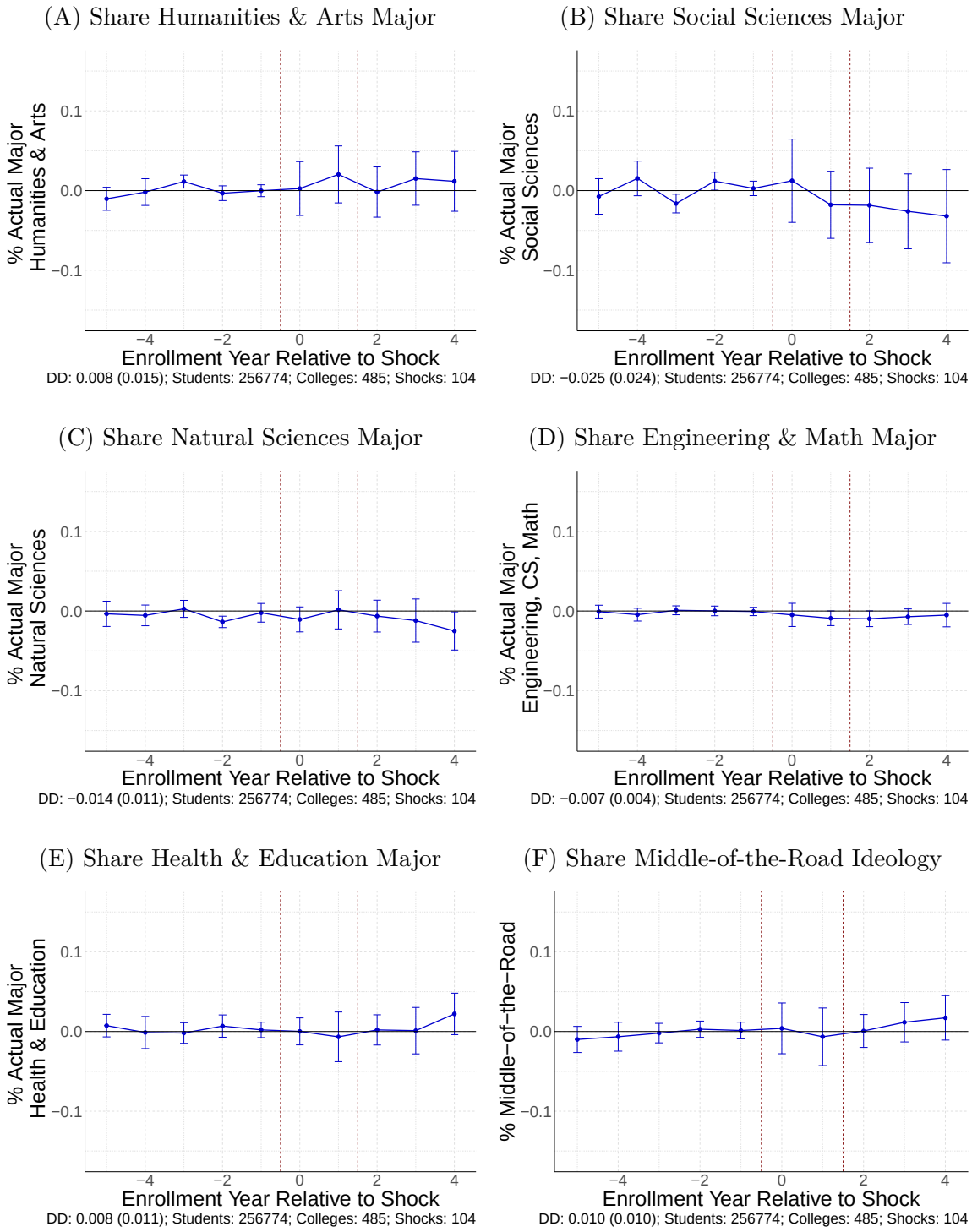
Note: This figure plots the instrumental variable estimates for τ_m from equation 2 and their 95 percent confidence intervals when using graduation ideology as the outcome variable. The estimation uses intended majors as instrumental variables for actual majors as discussed in the main text. The estimated coefficients represent the effects of studying each major relative to natural sciences which are the omitted group. The outcome variable is students' responses to the following question: "How would you characterize your political views?". Estimates are shown in standard deviations. The sample includes only students with non-missing values in the major and outcome.

Figure A14: Comparing Main Quasi-Experimental Results for Different Shocks Definitions



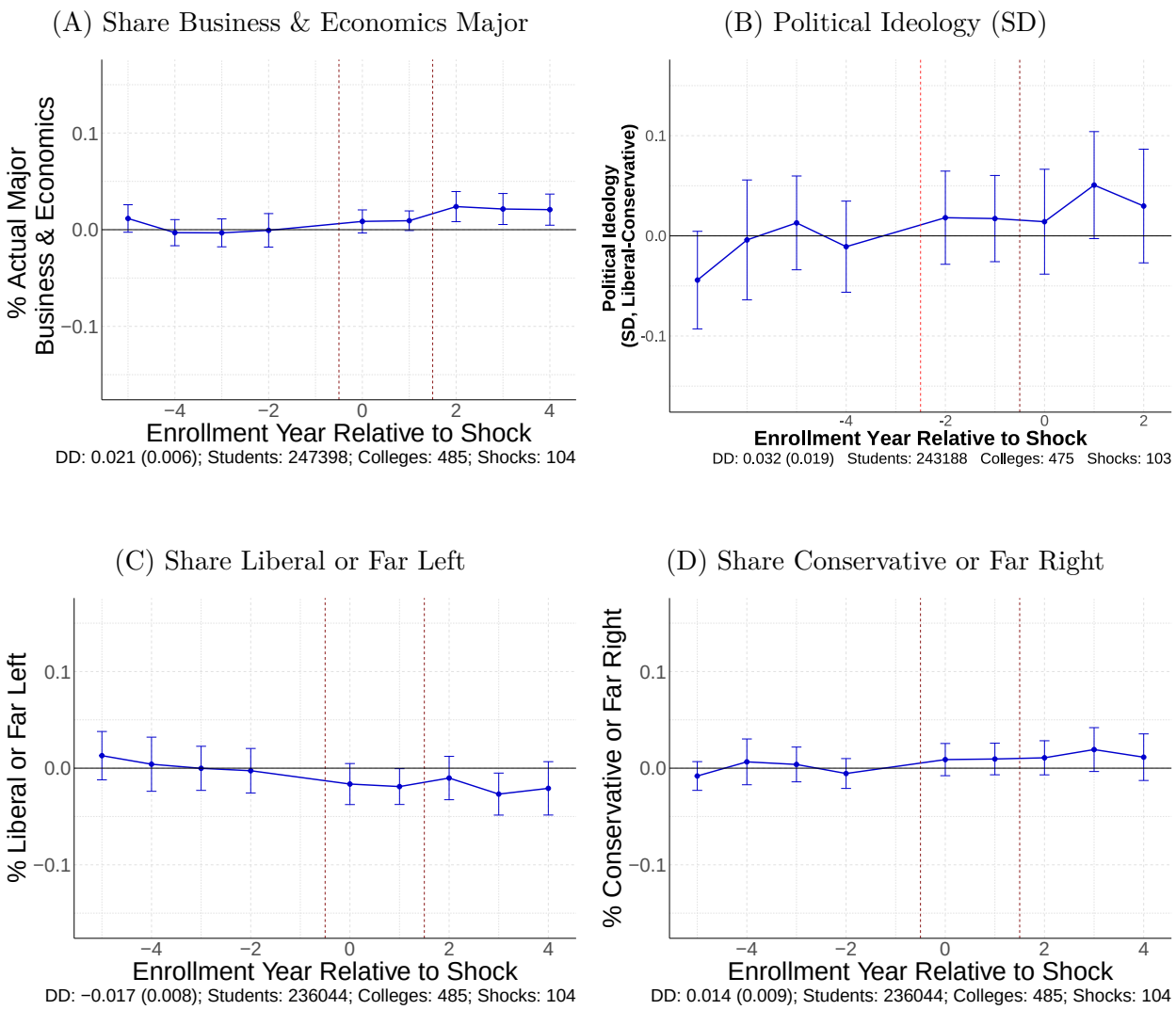
Note: Each point in the plot represents a distinct set of college-level shocks based on different definitions. The coordinates of each point correspond to estimates from the DID specification of equation 4. The x-axis displays the estimated effect on students selecting business and economics majors, while the y-axis shows the estimated effect on students' political ideology at graduation. All estimates are obtained using the two-stage imputation estimator developed by Gardner (2022). The larger red point highlights the primary shock specification used in the main analysis, while the smaller grey points represent alternative shock definitions.

Figure A15: Estimated Impacts of College-Level Shocks to Economics and Business



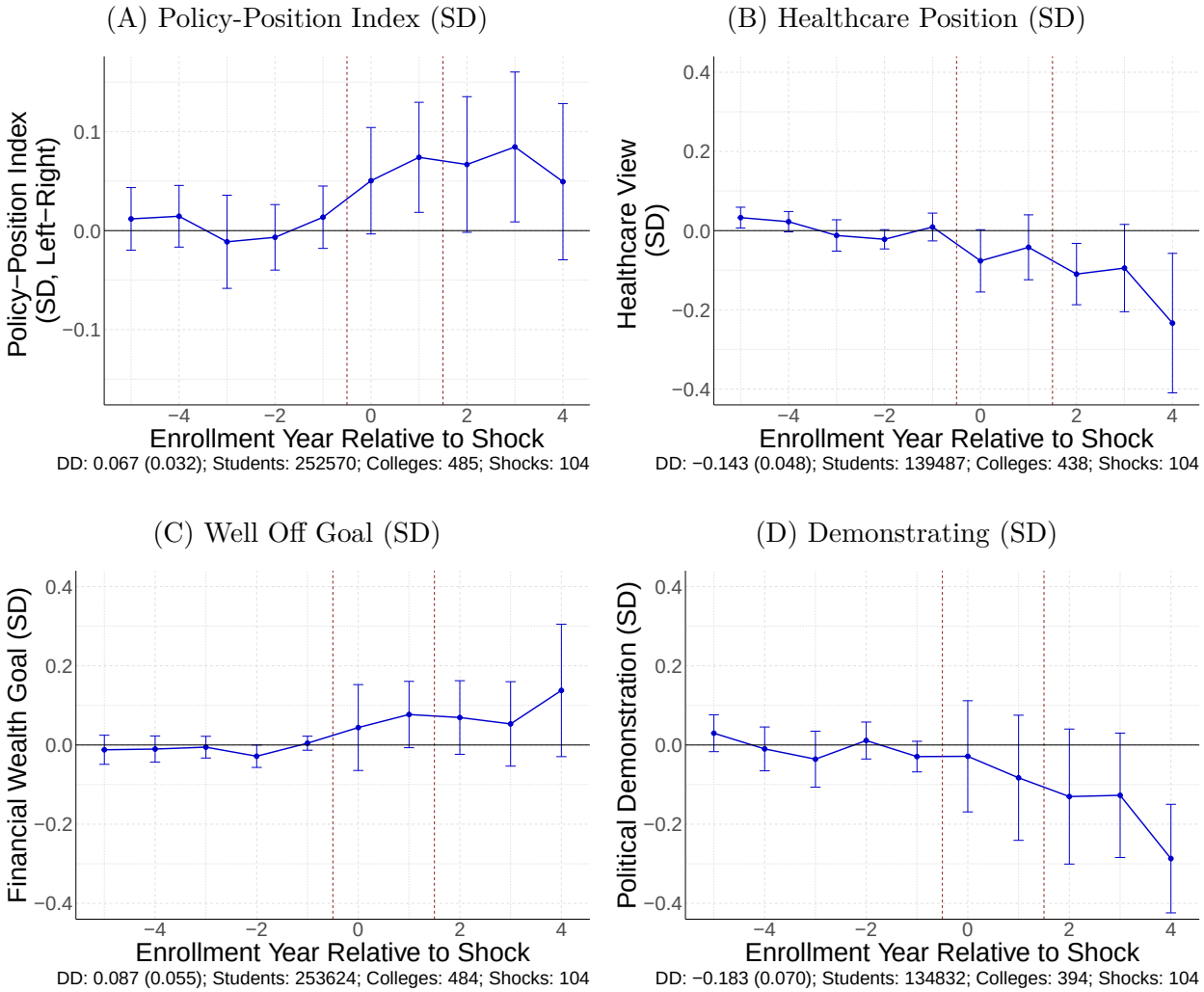
Note: These figures present results from the quasi-experimental design. Points show the estimates of ρ_k from equation 4, capturing the effects of college-level shocks to the availability of business and economics fields on students' outcomes, with standard errors clustered at the college level. Each panel also reports the corresponding difference-in-differences estimate of the average effect. Estimation follows the imputation estimator of Gardner (2022). The sample includes all students observed in both the baseline and graduation surveys with non-missing outcome values. The outcomes in Panels A–D are indicators for majoring in different major groups, and in Panel E an indicator for middle-of-the-road graduation ideology. All models control for the lagged outcome from the baseline survey.

Figure A16: Estimated Impacts of College-Level Shocks to Economics and Business, TWFE



Note: These figures present results from the quasi-experimental design. Points show the estimates of ρ_k from equation 4, capturing the effects of college-level shocks to the availability of business and economics fields on students' outcomes, with standard errors clustered at the college level. Each panel also reports the corresponding difference-in-differences estimate of the average effect. Estimation is based on a two-way fixed-effects regression. The sample includes all students observed in both the baseline and graduation surveys with non-missing outcome values. The outcome in Panel A is an indicator for majoring in business or economics; in Panel B, graduation ideology; and in Panels C and D, indicators for identifying as liberal or far left, and as conservative or far right, respectively. All models control for the lagged outcome from the baseline survey.

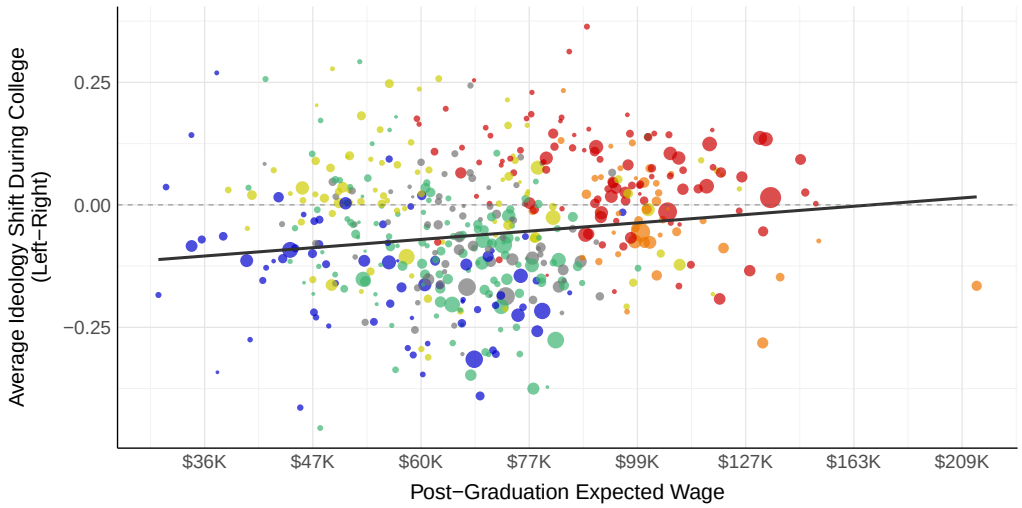
Figure A17: Estimated Impacts of College-Level Shocks on Policy Positions



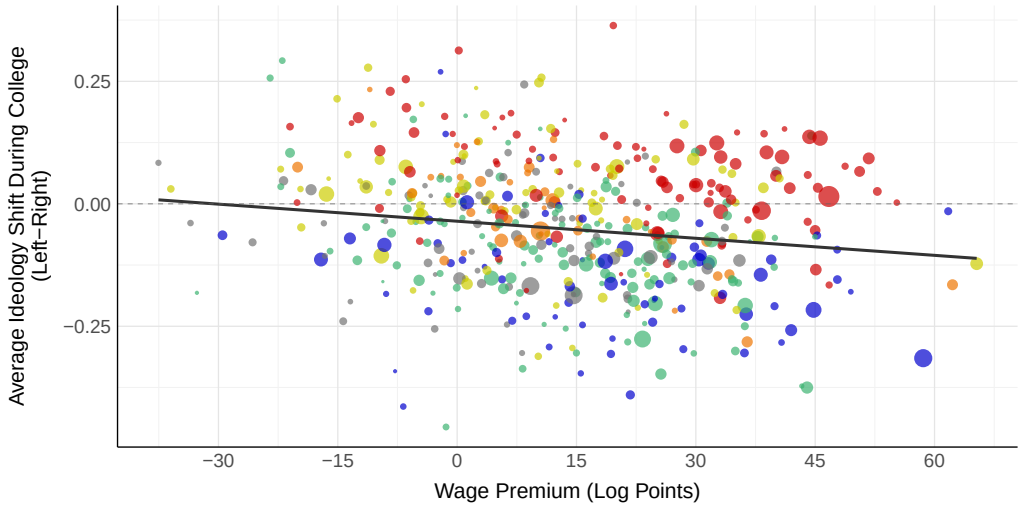
Note: This figure plots the estimates of ρ_k from equation 4, capturing the effects of college-level shocks to the availability of business and economics fields on students' outcomes at graduation, with standard errors clustered at the college level. The sample includes all students with non-missing values in the outcome. Each panel also reports the corresponding difference-in-differences estimate of the average effect. Estimation follows the imputation estimator of Gardner (2022). The outcome variables are the policy-position index (Panel A), support for national healthcare (Panel B), the importance placed on "being very well off financially" (Panel C), and participation in demonstrations (Panel D). All models control for the same outcome in the baseline survey.

Figure A18: Political Ideology Shift and Expected Wages

(A) Levels

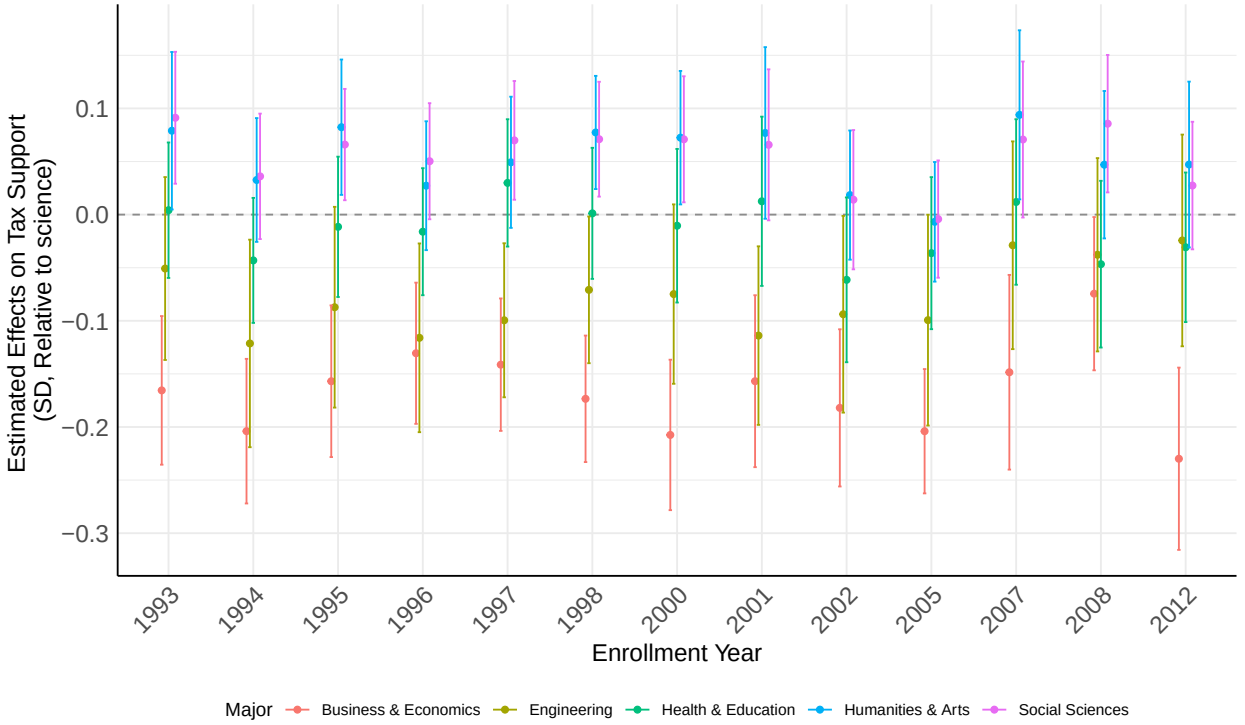


(B) Log Difference Relative to Median



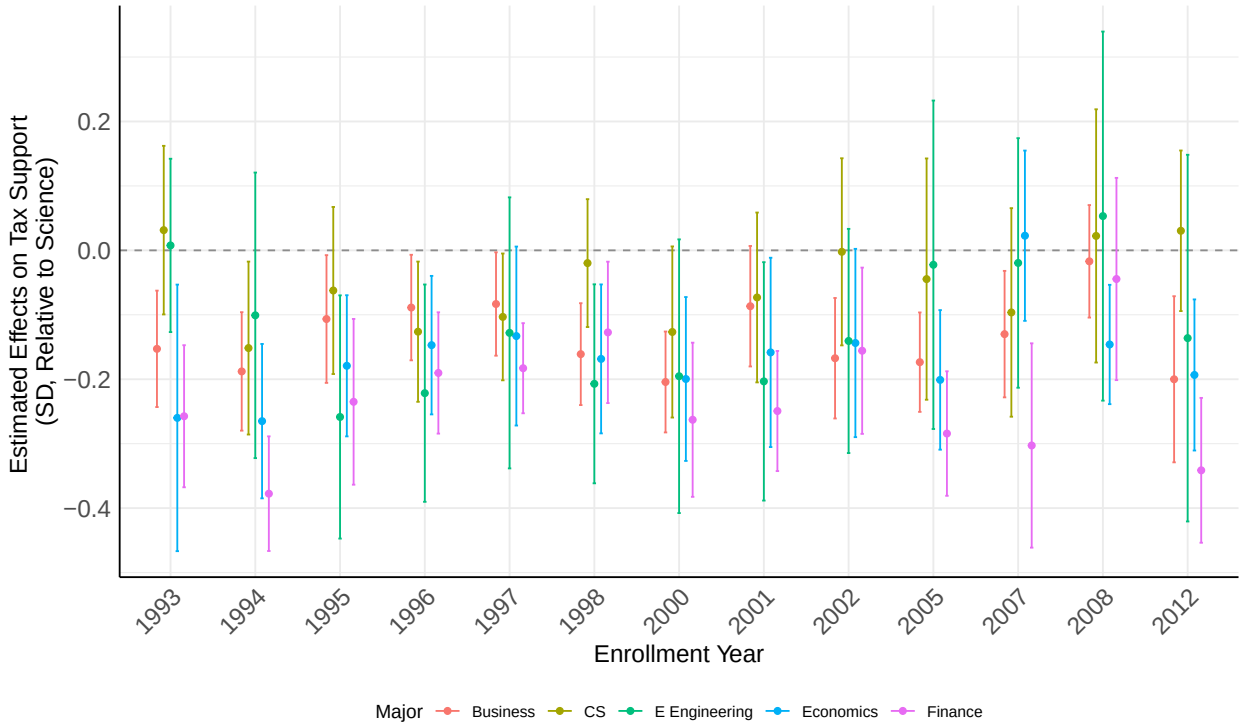
Note: Each panel plots the relationship between students' ideological change during college (y-axis, in standard deviations) and their expected post-graduation wages (x-axis). Panel A uses expected wages in USD. Panel B uses log wage differences relative to the median wage in each major group. Each point represents the mean for a college-major pair. Colors indicate the classification group for each major: blue for humanities and arts; green for social sciences; gray for natural sciences; yellow for health and education; orange for engineering and math; red for business and economics. The sample includes 498 departments with at least 100 students who responded to the ideology question both as freshmen and as graduates. The total number of students in those departments is 132,856. The number of students in each department weights linear trends. The slope in Panel A is 0.068 (0.018). The slope in Panel B is -0.116 (0.030).

Figure A19: Year-by-Year Effects of College Majors on Tax Support



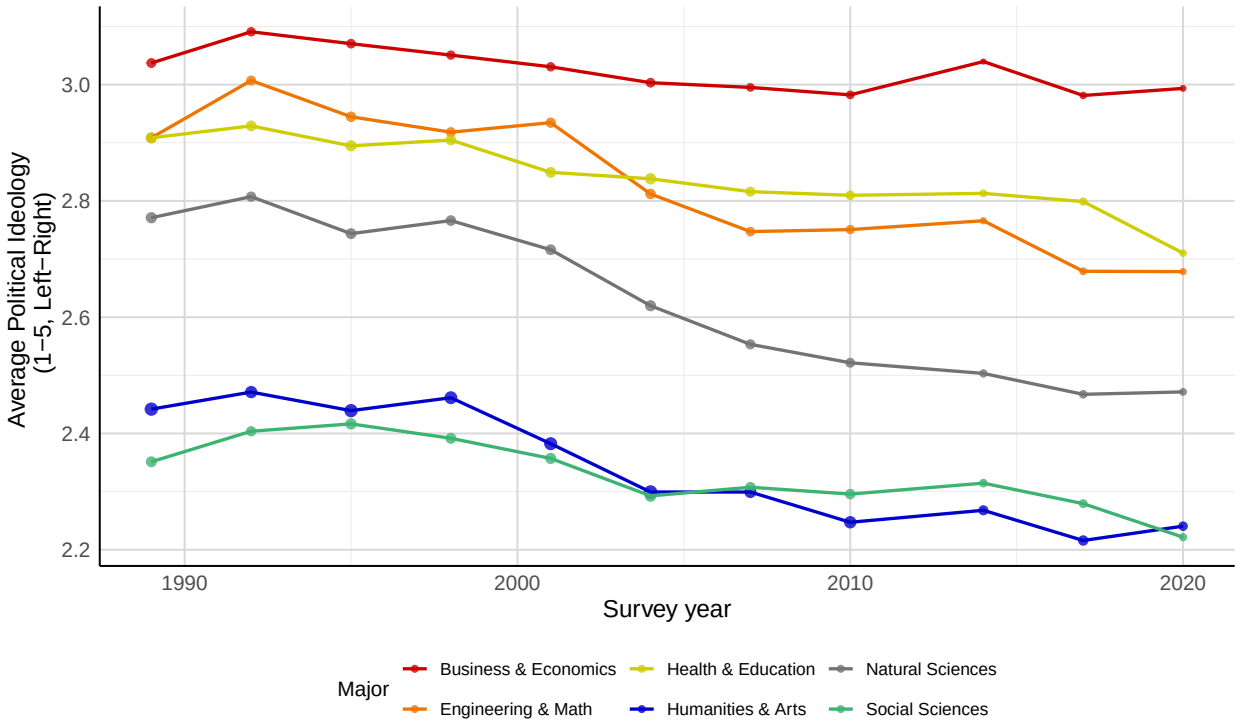
Note: This figure plots the estimates for δ_m from equation 1 along with their 95 percent confidence intervals, from a model that groups majors into five broad categories, with STEM as the omitted reference group. The outcome variable is students' support for higher taxes on the wealthy. Estimates are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

Figure A20: Year-by-Year Effects of College Majors on Tax Support, Selected Majors



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

Figure A21: 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).

Appendix B Appendix Tables

Table A1: College Majors Classification and Aggregation

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

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

Table A2: Distribution of Double Majors for Cohorts Enrolled Since 2005

First Major	Second Major	Students
Business & Econ	None	17004
Business & Econ	Business & Econ	5681
Business & Econ	Humanities & Arts	1538
Business & Econ	Social Sciences	1312
Business & Econ	Engineering	685
Business & Econ	Other	393
Business & Econ	None	304
Business & Econ	Health & Education	113
Business & Econ	Science	106
Engineering	None	9359
Engineering	Engineering	759
Engineering	Business & Econ	635
Engineering	Humanities & Arts	476
Engineering	Health & Education	241
Engineering	Science	214
Engineering	Social Sciences	195
Engineering	None	191
Engineering	Other	112
Science	None	13008
Science	Humanities & Arts	1085
Science	Science	1025
Science	Social Sciences	883
Science	Engineering	437
Science	Health & Education	316
Science	Business & Econ	250
Science	None	238
Science	Other	183

First Major	Second Major	Students
Health & Education	None	12964
Health & Education	Health & Education	1334
Health & Education	Humanities & Arts	856
Health & Education	Social Sciences	527
Health & Education	Other	471
Health & Education	None	366
Health & Education	Business & Econ	228
Health & Education	Science	196
Health & Education	Engineering	145
Humanities & Arts	None	14771
Humanities & Arts	Humanities & Arts	4319
Humanities & Arts	Social Sciences	2182
Humanities & Arts	Health & Education	960
Humanities & Arts	Business & Econ	524
Humanities & Arts	None	302
Humanities & Arts	Other	299
Humanities & Arts	Science	231
Humanities & Arts	Engineering	156
Social Sciences	None	20821
Social Sciences	Humanities & Arts	3975
Social Sciences	Social Sciences	3705
Social Sciences	Business & Econ	1010
Social Sciences	Other	684
Social Sciences	Health & Education	559
Social Sciences	None	480
Social Sciences	Science	379
Social Sciences	Engineering	118

Note: This table presents the distribution of double majors among students enrolled in college since 2005, when double major data was first included in the survey.

Table A3: Students' Policy Postions

Statement	Observations	Survey	Disagree Strongly	Disagree Somewhat	Agree Somewhat	Agree Strongly	$Cor(., P)$
Addressing global climate change should be a federal priority	28980	Baseline	11.02%	21.58%	39.02%	28.37%	-0.46
		Graduation	6%	16.07%	37.26%	40.68%	-0.48
Abortion should be legal	286338	Baseline	32.42%	15.73%	25.25%	26.59%	-0.43
		Graduation	23.2%	15.08%	27.45%	34.27%	-0.47
A national health care plan is needed to cover everybody's medical costs	170130	Baseline	10.44%	21%	43.99%	24.57%	-0.39
		Graduation	11.27%	20.09%	41.72%	26.93%	-0.45
It is important to have laws prohibiting homosexual relationships	228529	Baseline	44.12%	28.95%	15.06%	11.87%	0.39
		Graduation	57.71%	23.1%	11.49%	7.7%	0.41
Wealthy people should pay a larger share of taxes than they do now	227696	Baseline	13.54%	26.87%	39.65%	19.94%	-0.3
		Graduation	12.72%	26.66%	38.96%	21.66%	-0.39
Marijuana should be legalized	262748	Baseline	44.8%	27.03%	20.77%	7.41%	-0.28
		Graduation	30.11%	26.5%	28.83%	14.56%	-0.35
The death penalty should be abolished	244658	Baseline	31.19%	36.71%	18.54%	13.56%	-0.21
		Graduation	23.85%	36.63%	22%	17.52%	-0.3
The federal government should do more to control the sale of handguns	185516	Baseline	5.28%	10.53%	35.92%	48.27%	-0.24
		Graduation	5.63%	12%	36.74%	45.63%	-0.28
There is too much concern in the courts for the rights of criminals	174775	Baseline	5.99%	26.58%	47.7%	19.73%	0.22
		Graduation	8.33%	36.25%	42.02%	13.4%	0.27
Racial discrimination is no longer a major problem in America	305580	Baseline	37.6%	45.48%	14.72%	2.2%	0.18
		Graduation	46.72%	39.88%	11.24%	2.16%	0.24
Realistically, an individual can do little to bring about changes in our society	241029	Baseline	35.46%	41.9%	18.99%	3.65%	0.01
		Graduation	30.87%	44.91%	20.94%	3.27%	0.04
Colleges should prohibit racist/sexist speech on campus	221094	Baseline	11.51%	25.23%	35.28%	27.99%	0.02
		Graduation	16.24%	29.97%	31.93%	21.85%	-0.04

Note: This table displays twelve questions related to policy issues, as appeared in both the baseline and graduation surveys throughout our analysis period. The table shows the distribution of responses to these questions and the correlation between responses to each question and answers to political ideology (P): "How would you characterize your political views?" with a response scale from 1 (very conservative) to 5 (very liberal) in both surveys (entry and exit). The sample for each question is restricted to students who provided responses in both their baseline and graduation surveys.

Table A4: Assessing Differential Attrition

	Appear in Graduation Survey				
	(1)	(2)	(3)	(4)	(5)
Business	-0.02***	-0.01***	-0.004***	0.002	0.002
	(0.007)	(0.002)	(0.002)	(0.002)	(0.002)
Engineering	-0.05***	-0.01***	-0.01***	0.001	0.001
	(0.01)	(0.002)	(0.002)	(0.002)	(0.002)
Health/Education	-0.01	-0.002	0.0008	-0.008***	-0.008***
	(0.008)	(0.002)	(0.002)	(0.001)	(0.001)
Humanities/Arts	-0.0007	-0.01***	-0.004***	-0.003**	-0.003**
	(0.008)	(0.002)	(0.001)	(0.001)	(0.001)
Undecided/Other	-0.04***	-0.02***	-0.008***	-0.003**	-0.003**
	(0.007)	(0.001)	(0.001)	(0.001)	(0.001)
Social Sciences	-0.009	-0.008***	0.002	0.002	0.001
	(0.007)	(0.001)	(0.001)	(0.001)	(0.001)
Ideology × Business					-0.0003
					(0.0009)
Ideology × Engineering					-0.0004
					(0.001)
Ideology × Health/Education					-0.0002
					(0.001)
Ideology × Humanities/Arts					-0.002
					(0.0010)
Ideology × Undecided/Other					-0.001
					(0.0010)
Ideology × Social Sciences					-0.002***
					(0.0009)
College-Year FE		✓	✓	✓	✓
High-School GPA			✓	✓	✓
All Controls				✓	✓
Observations	1,996,987	1,996,987	1,996,987	1,749,152	1,749,152

This table presents the estimation results from regressing an indicator for participation in the graduation survey on students' intended majors, political ideology, and their interactions. The sample includes all students from the colleges and cohorts in our main sample described in the text, who participated in the baseline survey. Standard errors are clustered at the college level.

Table A5: Control Variables

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

Note: This table shows the control variables included in our analysis.

Table A6: 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***	0.16***	0.13***	0.16***	0.13***	0.16***
Economics&	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Engineering&	0.04**	0.07***	0.05***	0.07***	0.05***	0.06***
Math	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Health&	0.05***	0.06***	0.05***	0.08***	0.05***	0.07***
Education	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Social	-0.10***	-0.06**	-0.10***	-0.07***	-0.08***	-0.11***
Sciences	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Humanities&	-0.10***	-0.08***	-0.10***	-0.08***	-0.09***	-0.11***
Arts	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
N	192273	114004	204921	101823	199525	107219
Outcome: Left Share						
Business&	0.04***	0.05***	0.04***	0.06***	0.05***	0.05***
Economics&	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Engineering&	0.01**	0.02***	0.02***	0.02**	0.01*	0.02***
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Health&	0.02***	0.02**	0.02***	0.03***	0.02**	0.03***
Education	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Social	-0.02***	-0.01	-0.02***	-0.01*	-0.02***	-0.02***
Sciences	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Humanities&	-0.02***	-0.02**	-0.02***	-0.02***	-0.02***	-0.02***
Arts	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
N	192273	114004	204921	101823	199525	107219

This table shows the estimates for δ_m from equation 1 and their 95% confidence intervals. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. The outcomes represent self-reported political ideology (measured in standard deviations, on a Left-Right scale) and an indicator for identifying as liberal or far left (multiplied by 100). *** p<0.01, ** p<0.05, * p<0.1

Table A7: Effects of College Majors on Ideology, By College Characteristics

	Private College	Private University	Public	More Selective	Less Selective	North	South
Outcome: Ideology Score							
Business&	0.15***	0.13***	0.13***	0.17***	0.13***	0.14***	0.15***
Economics&	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Engineering&	0.04**	0.05***	0.08***	0.06***	0.06***	0.06***	0.07**
Math	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)
Health&	0.07***	0.03	0.03	0.05**	0.05***	0.05***	0.08***
Education	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Social	-0.08***	-0.12***	-0.10***	-0.07*	-0.09***	-0.09***	-0.08***
Sciences	(0.01)	(0.03)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
Humanities&	-0.08***	-0.14***	-0.12***	-0.09***	-0.10***	-0.10***	-0.09***
Arts	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
N	209014	58599	39131	64365	241468	268186	38302
Outcome: Left Share							
Business&	0.05***	0.04***	0.05***	0.06***	0.05***	0.05***	0.06***
Economics&	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
Engineering&	0.01*	0.02*	0.02*	0.02*	0.02***	0.02***	0.03*
Math	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Health&	0.02***	0.01	0.01	0.01	0.02***	0.02***	0.03***
Education	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
Social	-0.02***	-0.02**	-0.02*	-0.01	-0.02***	-0.02***	-0.02**
Sciences	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
Humanities&	-0.02***	-0.03***	-0.02**	-0.02**	-0.02***	-0.02***	-0.02**
Arts	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
N	209014	58599	39131	64365	241468	268186	38302

This table shows the estimates for δ_m from equation 1 and their 95% confidence intervals. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. The outcomes represent self-reported political ideology (measured in standard deviations, on a Left-Right scale) and an indicator for identifying as liberal or far left (multiplied by 100). *** p<0.01, ** p<0.05, * p<0.1

Table A8: Effects of College Majors on Ideology, By Enrollment Year

	2000 and Before	2000-2009	2010 and Later
Outcome: Ideology Score			
Business&	0.10***	0.14***	0.21***
Economics&	(0.01)	(0.01)	(0.02)
Engineering&	0.06***	0.06***	0.04
Math	(0.01)	(0.01)	(0.02)
Health&	0.02	0.07***	0.08***
Education	(0.01)	(0.01)	(0.02)
Social	-0.11***	-0.08***	-0.06**
Sciences	(0.01)	(0.01)	(0.02)
Humanities&	-0.13***	-0.08***	-0.07***
Arts	(0.01)	(0.01)	(0.02)
N	114833	136843	55068
Outcome: Left Share			
Business&	0.04***	0.05***	0.06***
Economics&	(0.01)	(0.01)	(0.01)
Engineering&	0.03**	0.02**	0.01
Math	(0.01)	(0.01)	(0.01)
Health&	0.00	0.03***	0.02*
Education	(0.01)	(0.01)	(0.01)
Social	-0.03***	-0.02***	-0.01
Sciences	(0.01)	(0.00)	(0.01)
Humanities&	-0.03***	-0.02***	-0.01
Arts	(0.01)	(0.00)	(0.01)
N	114833	136843	55068

This table shows the estimates for δ_m from equation 1 and their 95% confidence intervals. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome. The outcomes represent self-reported political ideology (measured in standard deviations, on a Left-Right scale) and an indicator for identifying as liberal or far left (multiplied by 100). *** p<0.01, ** p<0.05, * p<0.1

Table A9: Robustness to Unobserved Selection

Benchmark	k_D	k_Y	Bias	Adjusted Estimate
Panel A: All Controls				
Intended Major	1	1	0.02	0.20
Baseline Ideology	1	1	0.02	0.20
Intended Major	2	2	0.03	0.18
Baseline Ideology	2	2	0.03	0.18
Intended Major	3	3	0.06	0.16
Baseline Ideology	3	3	0.05	0.17
Intended Major	1	10	0.04	0.17
Baseline Ideology	10	1	0.05	0.17
Intended Major	1	20	0.06	0.16
Baseline Ideology	20	1	0.07	0.14
Sex	10	10	0.04	0.18
HS GPA	10	10	0.02	0.20
Panel B: Excluding Career Preferences				
Intended Major	1	1	0.07	0.15
Baseline Ideology	1	1	0.02	0.20
Intended Major	2	2	0.48	-0.26
Baseline Ideology	2	2	0.03	0.19
Intended Major	3	3		
Baseline Ideology	3	3	0.05	0.17
Intended Major	1	10	0.16	0.06
Baseline Ideology	10	1	0.06	0.17
Intended Major	1	20	0.22	-0.00
Baseline Ideology	20	1	0.08	0.14
Sex	10	10	0.04	0.18
HS GPA	10	10	0.02	0.20

Note: The table shows the bias of our estimates under different assumptions about the partial R^2 of unobserved confounders (Cinelli and Hazlett, 2020). We focus on the estimated effect of majoring in humanities or social sciences relative to economics or business, which is about 0.23 SD in our full 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), as well as high school GPA. Panel A includes all controls in Table A5, with one change: college-by-year fixed effects are not interacted with baseline ideology and intended majors. Panel B further excludes career preferences from the set of controls.

Table A10: Robustness of the Results, Controlling for Double Majors

	Ideology		Index	
Control		+		+
Business/	0.18***	0.18***	0.16***	0.16***
Economics	(0.01)	(0.01)	(0.01)	(0.01)
Engineering/	0.05***	0.04**	0.05**	0.05*
CS/Math	(0.01)	(0.01)	(0.02)	(0.02)
Health/	0.07***	0.06***	0.08***	0.07***
Education	(0.01)	(0.01)	(0.01)	(0.01)
Humanities/	-0.06***	-0.06***	-0.07***	-0.06***
Arts	(0.02)	(0.01)	(0.01)	(0.01)
Social	-0.07***	-0.06***	-0.10***	-0.08***
Sciences	(0.01)	(0.01)	(0.01)	(0.01)
N	123255	123255	119691	119691

This table shows the estimates for δ_m from equation 1 and their 95% confidence intervals, with and without controlling for the double major category. The estimated coefficients represent the effects of studying each major relative to natural sciences (the omitted group of majors). The sample includes only students with non-missing values in the major and outcome from the enrolling cohorts of 2005 or later. The outcomes represent self-reported political ideology and an index for policy attitudes (both measured in standard deviations, on a Left-Right scale. *** p<0.01, ** p<0.05, * p<0.1

Table A11: Decomposition of the Effects of College Majors, Using Only Persistent Survey Questions

Major 1	Major 2	Base Coef	Full Coef	Economic Views	Culture Views	Other Views	Not Explained
Business & Economics	Natural Sciences	0.134	0.084	30.4%	6.84%	0.32%	62.43%
Engineering & Math	Natural Sciences	0.073	0.044	31.38%	7.83%	0.64%	60.15%
Health & Education	Natural Sciences	0.055	0.038	-0.27%	19.81%	12.09%	68.37%
Natural Sciences	Social Sciences	0.098	0.046	20.24%	33.4%	-0.98%	47.33%
Natural Sciences	Humanities & Arts	0.099	0.062	10.24%	25.92%	1.31%	62.53%

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

Table A12: Decomposition of the Effects of College Majors, Detailed Results

Major 1	Major 2	Base Coef	Full Coef	Tax	Healthcare	Abortion	Race	Gay Prohibit	Speech	Marijuana	Change	Not Explained
Business & Economics	Natural Sciences	0.134	0.087	15.39%	13.06%	-0.27%	1.29%	6.13%	-0.02%	-0.1%	-0.07%	64.58%
Engineering & Math	Natural Sciences	0.073	0.042	12.09%	17.35%	9.81%	1.57%	1.39%	-0.48%	-0.08%	0.18%	58.16%
Health & Education	Natural Sciences	0.055	0.031	3.23%	-2.75%	30.57%	-3.04%	-0.58%	0.42%	19.74%	-2.69%	55.1%
Natural Sciences	Social Sciences	0.098	0.038	9.53%	9.8%	9.86%	8.96%	12.92%	-0.19%	7.11%	2.83%	39.18%
Natural Sciences	Humanities & Arts	0.099	0.054	6.11%	3.35%	3.37%	8.09%	13.1%	-0.35%	9.76%	1.83%	54.74%

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

Table A13: Decomposition of the Effects of College Majors, Allowing for Different Weights

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

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

Table A14: Falsification Tests: "Effects" of Peer Ideology Composition on Baseline Ideology

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimate	0.746***	0.711***	0.019	0.019***	0.025*	0.030***	0.007	0.004
	(0.023)	(0.014)	(0.012)	(0.003)	(0.014)	(0.004)	(0.014)	(0.004)
Estimate	1.203***	1.057***	0.039*	0.032***	0.046*	0.048***	0.017	0.008
(Adjusted)	(0.038)	(0.020)	(0.020)	(0.005)	(0.024)	(0.006)	(0.022)	(0.006)
Full		V		V		V		V
Expectation					V	V	V	V
Baseline								
FEs			V	V			V	V

Notes: This table presents falsification for equation 6. The outcome variable measures political views using responses to the question "How would you characterize your political views?" from the baseline survey The treatment variable captures peers' responses to the same question in the baseline survey, where peers are defined as students from the same college-cohort-intended major group. The estimates account for exclusion bias using a four-way split-sample approach. Specifications include controls for the average political views of potential peers from four adjacent cohorts (Expectaions), pre-college political ideology and opinion index (Baseline), and college-major, college-cohort, and major-cohort fixed-effects (FEs). All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both major and political views, and with information available for at least 5 of their peers which represent at least 10% of the cohort. Full sample include all students in the baseline survey, and the non-full sample include only those appearing in the graduation sample too. Full sample includes 4488424 students, and the non-full sample includes only those appearing in the graduation sample too, 228093 students *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A15: Estimated Effects of Peer Ideology Composition, Robustness

	Ideology	Liberal	Index	Ideology	Ideology	Ideology
	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	-0.011	0.016	0.010	-0.008	-0.014	-0.041
	(0.009)	(0.011)	(0.011)	(0.010)	(0.011)	(0.032)
Estimate	-0.017	0.022	0.014	-0.015	-0.026	-0.057
(Adjusted)	(0.016)	(0.019)	(0.019)	(0.016)	(0.017)	(0.039)
N	226,947	226,947	222,226	213,832	154,037	40,757
Share	0.1	0.1	0.1	0.3	0.5	0.7
Potential Peers' Average	V	V	V	V	V	V
Baseline	V	V	V	V	V	V
Program + Year FEs	V	V	V	V	V	V

Notes: This table presents estimates of peer effects on students' political ideology based on equation 6. The outcome variable is either self-reported ideology, or dummy for liberal, or the opinion index constructed from the graduation survey The treatment variable captures peers' outcome from the baseline survey, where peers are defined as students from the same college-cohort-intended major group. The estimates account for exclusion bias using a four-way split-sample approach. Specifications include controls for the average index of potential peers from four adjacent cohorts (Expectaions), pre-college political ideology and opinion index (Baseline), and college-major, college-cohort, and major-cohort fixed-effects (FEs). All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both major and political views, and with information available for at least 5 of their peers which represent at least x% of the cohort, with x reported in the table (Share). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A16: Estimated Effects of Peer Ideology Composition, Subsamples

	Economics& Business	Engineering& Math	Health& Education	Humanities& Arts	Natural Sciences	Social Sciences
	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.005	-0.025	-0.013	-0.008	-0.038	-0.020
	(0.026)	(0.046)	(0.028)	(0.034)	(0.047)	(0.033)
Estimate	0.000	-0.010	-0.018	-0.024	-0.061	-0.024
(Adjusted)	(0.044)	(0.073)	(0.049)	(0.054)	(0.072)	(0.056)
N	43,354	20,795	52,241	26,277	24,075	42,255
Potential Peers' Average	V	V	V	V	V	V
Baseline	V	V	V	V	V	V
Program + Year FEs	V	V	V	V	V	V

Notes: This table presents estimates of peer effects on students' political ideology based on equation 6. The outcome variable is either self-reported ideology, or dummy for liberal, or the opinion index constructed from the graduation survey The treatment variable captures peers' outcome from the baseline survey, where peers are defined as students from the same college-cohort-intended major group. The estimates account for exclusion bias using a four-way split-sample approach. Specifications include controls for the average index of potential peers from four adjacent cohorts (Expectaions), pre-college political ideology and opinion index (Baseline), and college-major, college-cohort, and major-cohort fixed-effects (FEs). All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both major and political views, and with information available for at least 5 of their peers which represent at least 10% of the cohort. Samples are defined based on intended majors or cohort size (below 50). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A17: Falsification Test: "Effects" of Faculty Ideology on Students' Baseline Ideology

	(1)	(2)	(3)
Estimate	0.009***	0.005	-0.008
	(0.002)	(0.004)	(0.007)
Estimate (Adjusted)	0.021***	0.014	-0.008
	(0.004)	(0.009)	(0.015)
N	3,482,826	647,687	154,003
Sample	Baseline	Baseline (subsample)	Graduation
FEs	V	V	V

Notes: This table presents falsification tests for equation 8, estimating the effects of faculty ideology on students' political ideology. The outcome variable measures political ideology using responses to the question "How would you characterize your political views?" from the baseline survey The treatment variable captures faculty's responses to the same question, based on all faculty from the department that correspond to the student's college and intended major. Columns (1)-(3) use different samples: all baseline survey, baseline survey restricted to colleges and cohorts included in the graduation survey, and students included in both the baseline and gradution surveys. All models controls for major-cohort, college-cohort, and major-group-college-cohort fixed-effects. All estimates are in standard deviation units. The sample is restricted to students with non-missing values for both intended major and political ideology, and with information available for at least 3 of their faculty included in the survey which represent at least 10% of the faculty in the department. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix C Potential Outcomes Framework: Identification of College Major Effects

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

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

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

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

We next project the student-specific component θ_i onto observed pre-college covariates X_i. Let $\mu(x) \equiv \mathbb{E}[\theta_i \mid X_i = x]$ denote the expected major-invariant ideology for students with covariates x. We write:

$$\theta_i = \mu(X_i) + u_i$$

With $\mathbb{E}[u_i \mid X_i] = 0$. Here u_i represents any residual (unobserved) component of student i's major-invariant ideology that is not explained by the observed covariates. In practice, X is high-dimensional, so we approximate $\mu(.)$ in flexible estimation with indicators for every values of our control variables, and also include interactions between key variables in some specifications.

The key identification assumption is that after conditioning on our rich set of controls X_i, the residual student component u_i is orthogonal to the student's major choice. In other words, our observed covariates capture all systematic selection in ideology relevant to major choice. Formally, we assume (A1):

$$\mathbb{E}[u_i \mid m_i, X_i] = \mathbb{E}[u_i \mid X_i].$$

Assumption (A1) posits that, conditional on X_i, the unobserved part of a student's ideology θ_i is independent of the chosen major. Under this assumption, for any given covariate cell x and for any two majors $m, k \in \mathcal{M}$ that both occur among students with $X_i = x$, the difference in average outcomes between those who chose m and those who chose k identifies the difference in their treatment effects. Formally, under A1 we have for any x in the support of both m and k:

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

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

To obtain a more interpretable estimand, we impose additional symmetry conditions on how students sort on potential ideology effects. Specifically, we assume that any selection on ideology effects into majors or into the overlapping support is symmetric between m and k (see discussion in Mountjoy and Hickman, 2021). Under these conditions, OLS estimation captures the average treatment effect of each major relative to the omitted category. Moreover, we also report results that do not require such assumptions, following a practical remedy suggested by Goldsmith-Pinkham et al. (2024): to estimate *pairwise* models restricted to m and k with the same X_i controls. We also report such estimates as robustness.

Appendix D Accounting for Selection on Unobservables Following (Oster, 2019)

We assess the robustness of our estimates to selection on unobservables using the coefficient-stability approach of Oster (2019). Under the assumption that selection on observables is informative about selection on unobservables, this method uses coefficient movements following the inclusion of additional controls to construct bounds on the potential bias from selection on unobservables. In the main section we report results using a more recent approach offered by Cinelli and Hazlett (2020), but we also report results following Oster (2019) as a complementary strategy.

We apply this method to the estimated effect of choosing business, economics over humanities, arts or social sciences. We rely on the estimated effects as well as R^2 for the main and most detailed regression specifications presented in Figure 4.

Following Oster (2019), a bias-adjusted effect can be defined as:

$$\beta^* = \beta_{\text{detailed}} - \delta\,(\beta_{\text{main}} - \beta_{\text{detailed}})\frac{R^2_{\text{max}} - R^2_{\text{detailed}}}{R^2_{\text{detailed}} - R^2_{\text{main}}},$$

Where β_{main} and β_{detailed} (and also R^2_{main}, and R^2_{detailed}) come from the main and the most detailed specifications. They are defined as the difference between the business or economics and humanities or arts coefficients: $\beta_x = \delta^x_{Business} - \delta^x_{Humanities}$ with $x \in \{main, detailed\}$. R^2_{max} is the maximum attainable explanatory power, and δ is the relative strength of selection on unobservables versus observables.

Our outcome variable is a discrete measure of political ideology, whereas the underlying construct is likely continuous. This discretization creates measurement error that limits the maximum explanatory power achievable through any linear regression. Based on simulation, we thus set $R^2_{\text{max}} = 0.85$ and solve for the value of δ that drives the adjusted coefficient to zero ($\beta^* = 0$).[31] Using the estimated coefficients and R^2 reported in Figure 4, we get that selection on unobservables should be 5.5 times larger relative to selection on (not essential)

[31]We conducted a Monte Carlo simulation to determine an appropriate R^2_{max} for our discrete outcome. This simulation recognizes that our most informative control is a pre-college measure of the discrete outcome—student political ideology. We generated a continuous pre-college variable $y_0 \sim N(0, 1)$ and a student-specific change variable $W \sim N(-0.05, 0.93)$ calibrated to observed changes in political ideology during college, then created a continuous outcome $y = y_0 + W$ and its discrete version Y by dividing the range of y into 5 equal-width intervals. Regressing the discrete outcome on the continuous predictors (y_0, W) achieves $R^2 = 0.85$, while using the discrete predictor Y_0 (created by dividing the range of y_0 into 5 equal-width intervals) reduces explanatory power to $R^2 = 0.78$. We set $R^2_{\text{max}} = 0.85$, which corresponds to the upper bound from these simulation benchmarks.

observables to change the sign of our estimate for the effects of studying humanities or social sciences instead of business and economics. To see this, note that the detailed coefficient is 0.21731 while the main one is 0.23805, and R_2 is 0.55828 relative to 0.40444. Thus: ($\beta_{\text{main}} = 0.25,\ \beta_{\text{detailed}} = 0.22,\ R^2_{\text{main}} = 0.40,\ R^2_{\text{detailed}} = 0.54$):

$$\delta \ = \ \frac{\beta_{\text{detailed}}}{\beta_{\text{main}} - \beta_{\text{detailed}}} \frac{R^2_{\text{detailed}} - R^2_{\text{main}}}{R^2_{\text{max}} - R^2_{\text{detailed}}} \ = \ \frac{0.21731}{0.02074} \times \frac{0.15384}{0.29172} \ \approx \ \mathbf{5.52553}.$$

This result strongly supports the validity of our main findings. From the main to the full model, we add several key controls—some of the variables most likely to bias the estimates if omitted. For instance, career preferences are likely correlated with major preferences not captured in the baseline model. Likewise, the inclusion of college-by-year-by-intended-major-by-ideology interactions absorbs much of any remaining bias. It is therefore highly unlikely that unobserved factors would be stronger than these controls; they would need to be over five times stronger to overturn our conclusions.

Similarly, we can quantify selection on unobservables relative to the model with no controls. Given that the estimated raw coefficient is 0.53055 and R^2 is 0.04544, we get that: ($\beta_{\text{raw}} = 0.52,\ \beta_{\text{detailed}} = 0.22,\ R^2_{\text{raw}} = 0.04,\ R^2_{\text{detailed}} = 0.54$):

$$\delta \ = \ \frac{0.21731}{0.31324} \times \frac{0.51284}{0.29172} \ \approx \ \mathbf{1.2196}.$$

Thus, unobservables would have to be about 22 percent **stronger** than all observed controls combined (including, among others, baseline ideology and intended majors) to eliminate the effect. This provides another strong argument in favor of the validity of our main findings.

Appendix E Adjusting Peers and Faculty Ideology Measure

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

Consider a hypothetical sample in which the full set of peers is observed. The following version of Equation 6 would then apply:

$$p_{icmt} = \sigma^F P_{cmt}^{F,-i} + \beta^F \Phi_{cmt}^{F,-i} + \nu_{cm} + \theta_{ct} + \lambda_{mt} + \varepsilon_{icmt} \tag{A2}$$

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

The treatment variable can be decomposed into:

$$P_{cmt}^{F,-i} = \widehat{P_{cmt}^{-i}} + \eta_{cmt}^F \tag{A3}$$

Here, $\widehat{P_{cmt}^{-i}}$ represents the component predictable from fixed effects and controls, while η_{cmt}^F captures the residual, unpredictable component. Under our identification assumption, η_{cmt}^F has mean zero and is uncorrelated with ε_{icmt} conditional on the same fixed effects and controls. Applying the Frisch–Waugh–Lovell theorem (Frisch and Waugh, 1933), we can rewrite Equation A2 in residualized form:

$$p_{icmt}^{\perp} = \sigma^F \eta_{cmt}^F + \varepsilon_{icmt} \tag{A4}$$

Here, the superscript $\perp$ indicates residualization with respect to $\Phi_{cmt}^{F,-i}$ and the fixed effects in Equation A2. The term η_{cmt}^{F} can be further decomposed into observed and unobserved components:

$$\eta_{cmt}^{F} = \alpha_{ct}\eta_{cmt}^{O} + (1 - \alpha_{ct})\eta_{cmt}^{U} \tag{A5}$$

where η_{cmt}^{O} represents the observed share of the peer component and η_{cmt}^{U} the unobserved share. α_{ct} denotes the share of students in college c and entry year t who are covered in the baseline survey.[32] Substituting equation A5 into equation A4 and replacing the unobserved component with a mean-zero composite error term υ_{cmt} gives:

$$p_{icmt}^{\perp} = \sigma^{F}(\alpha_{ct}\eta_{cmt}^{O} + \upsilon_{cmt}) + \varepsilon_{icmt} = \sigma^{F}\alpha_{ct}\eta_{cmt}^{O} + \zeta_{icmt} \tag{A6}$$

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

$$P_{cmt}^{-i} = (1 - \alpha_{ct})\widehat{P_{cmt}^{-i}} + \alpha_{ct}\eta_{cmt}^{O}. \tag{A7}$$

The corresponding residualized specification aligns with equation A6 and thus identifies σ^{F}:

$$p_{icmt}^{\perp} = \sigma\alpha_{ct}\eta_{cmt}^{O} + \varepsilon_{icmt} \tag{A8}$$

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

$$p_{icmt}^{\perp} = \sigma^{O}\eta_{cmt}^{O} + \varepsilon_{icmt} \tag{A9}$$

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

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