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Attribution bias in major decisions: Evidence from the United States Military Academy ☆

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ABSTRACT

Using administrative data, we study the role of attribution bias in a high-stakes, consequential decision: the choice of a college major. Specifically, we examine the influence of fatigue experienced during exposure to a general education course on whether students choose the major corresponding to that course. To do so, we exploit the conditional random assignment of student course schedules at the United States Military Academy. We find that students who are assigned to an early morning (7:30 AM) section of a general education course are roughly 10% less likely to major in that subject, relative to students assigned to a later time slot for the course. We find similar effects for fatigue generated by having one or more back-to-back courses immediately prior to a general education course that starts later in the day. Finally, we demonstrate that the pattern of results is consistent with attribution bias and difficult to reconcile with competing explanations.

1. Introduction

When making consequential decisions, such as choosing a partner or career, people are likely to reflect on their prior experiences to inform which choice will provide them with the highest well-being. The standard economic model predicts that people will recognize the role that temporary physical and emotional states, such as inclement weather or fatigue, played in their prior experiences and will not be overly influenced by those states when making their choices.

However, theory and evidence from psychology and behavioral economics suggest that individuals may make systematic mistakes when responding to changes in physical and emotional states. In particular, a growing body of literature has found evidence of prospective mistakes in the form of projection bias (Loewenstein et al., 2003): instances in which individuals act *as if* their current temporary state will persist into the future.[1] More recently, Haggag et al. (2019) proposed a model and demonstrated evidence of retrospective mistakes in the form of attribution bias: instances in which individuals misattribute the influence of a prior temporary state to a fixed property of their utility over a good or activity. For example, they find that random variation in thirst experienced while sampling a new drink has a significant influence on people's later stated willingness to drink it again in the future. In this paper, we test for attribution bias in an important decision with long-run consequences: the choice of a college major. If students are subject to

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[1] For examples of projection bias, see Conlin et al. (2007) and Busse et al. (2015).

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such attribution bias, prior incidental emotional and physical states during initial exposure to academic subjects may have undue influence on their college major choice.

Specifically, we test whether student fatigue generated by plausibly random variation in course schedules (i.e. "incidental fatigue") influences students' graduating major at the United States Military Academy (USMA). Our study design is motivated by evidence that students exhibit diminished performance in early morning courses (Carrell, 2011; Edwards, 2012; Dills and Hernandez-Julian, 2008) and in courses that are scheduled immediately after one or more back-to-back course (Pope, 2016; Williams and Shapiro, 2018), presumably due to heightened levels of fatigue. We hypothesize that the level of incidental fatigue students experience in a course may extend beyond their coincident performance; influencing their judgment of the overall corresponding subject and thus their resultant college major.

Our study employs two unique approaches to identify the effects of incidental fatigue on major choice. For both of these approaches, we take advantage of the fact that students' schedules are randomly assigned conditional on their registered courses and whether they are a Division I athlete. First, we test if being assigned to a first period (7:30 AM) section of a required course (e.g. Economics 101) influences the choice to major in a corresponding subject (e.g. Economics).[2] Second, we test whether variation in the number of back-to-back courses students are assigned immediately before (on the same day of) a required course influences major choice.[3] This second approach allows us to compare students with different levels of fatigue from the exact same classroom, thus enabling us to rule out potential classroom-level effects of fatigue on major choices such as instructor quality or peer effects. To employ these tests, we use administrative records from USMA containing the course schedules and college major choices of 18,753 students from 2001 to 2017.

We find that students are significantly less likely to major (using graduating major) in a course's subject area when conditionally randomly assigned to an early morning course. Our estimates suggest that students assigned to an early morning section of a course are approximately 10% less likely to major in a subject related to that morning course, relative to students assigned to a later time slot for the course.[4] We also find that each additional course immediately preceding a class reduces the probability that students major in a related subject by approximately 12%. Our main specifications isolate variation between students with the same set of registered courses in a semester (i.e. the same "course roster"), but who are conditionally randomly assigned to different timings for those courses (i.e. a different "course schedule"). We show that these results are robust to a number of modifications including the addition of faculty fixed effects and using a broader mapping of courses to majors. Moreover, we provide a simple falsification test against the concern that students may select (on unobservables) into preferable schedules for courses in their intended majors. We show that the same identification strategy does not predict college major when looking at the semester after students have made their college major choice (the fourth semester).

We provide a suggestive set of results pointing against salient neoclassical alternatives to attribution bias. First, we examine the hypothesis that students avoid majors sampled under fatigue because they rationally anticipate being less prepared for success in those majors. To do so, we focus on a sub-sample of subjects that have more than one required course in the core curriculum (chemistry, English, math, and physics). This sample allows us to examine whether students perform worse in follow-on courses, while avoiding the selection bias of examining upper-level courses within a major. We find that while fatigue does reduce students' grades in the initial course, those students do no worse in the subsequent required course of the sequence (in the following semesters). Insofar as the second or third course in the sequence builds on knowledge acquired in the first, this result is suggestive that students did not learn sufficiently less in the first course for them to *rationally* anticipate doing worse in the major. However, there are important caveats to this result, namely: (a) it could be that students who did worse in the first course due to fatigue rationally adjust their effort in the second course to make up their learning deficit – this extra effort cost could be sufficiently high to discourage students from proceeding in majors that didn't have a subsequent required course, (b) grades are noisy signals of learning, so it could be the case that these students indeed learned less despite performing similarly in the subsequent courses, (c) it may be that the slight reduction in performance in the first course of the sequence is enough to lower the returns the enrolling in the major, even if the student makes up for it in subsequent courses (e.g. if there are implicit grade standards in some majors) – we find that effects are not driven by "selective" majors, however, there are stories which this does not rule out (e.g. if recommendation letter writers put an out-sized weight on first courses in the major).[5] The second piece of evidence against the standard performance channel explanation is that we find quite similar effects of fatigue on major choice across our the two sources of fatigue (i.e. roughly a 10 percent decrease), despite finding quite different (though both small) effects on performance (i.e. 0.053 SD vs. 0.015 SD effects on performance for early morning vs. back-to-back courses). Finally, we find that effects on major choice survive controlling for coincident performance (though, controlling for this post-treatment outcome warrants some caution, see Angrist and Pischke (2008)).

Our results have several implications. First, we provide field evidence of attribution bias in a highly consequential decision environment. To give a sense for the economic significance of our estimates, we generate several within-sample benchmarks in Section 4.4. For example, similar to Carrell et al. (2010), we estimate a large gender role model effect in STEM courses – the early morning course effect is roughly a fourth of that magnitude (of opposite sign). We also find that an early morning course has a similar effect on major choice as being assigned to an instructor who received a one standard deviation lower evaluation in past semesters of the course. Second, college major choice significantly impacts earnings (Arcidiacono, 2004) and well-being (Wiswall and Zafar, 2014). In fact, recent studies suggest major and field choices have a greater

2 Students at USMA declare a major online or in person with a major representative during a 4–5 week window during the first semester of their sophomore year. This window opens between August 27 and September 11 and closes between September 29 and October 11.

3 Courses at USMA are all 55 min long and only start at one of six times: 7:30 AM, 8:40 AM, 9:50 AM, 11:00 AM, 1:55 PM and 3:05 PM. A course is defined as having a "back-to-back course" immediately before it if there is no break between courses in the schedule prior to it. For example, if a student has one class at 8:40 AM and another at 11:00AM, the 11:00 AM class will be treated as having 0 immediately preceding courses. If the student instead has an additional class at 9:50 AM, then the 11:00 AM class will be treated as having 2 immediately preceding courses and the 9:50 AM class will be treated as having 1 immediately preceding course. Finally, the counts are reset at 1:55 PM, as all classes prior to this period will have had a lunch break in the schedule.

4 This estimate corresponds to a 0.20 percentage point reduction relative to the mean of 1.9 percent. This baseline mean is relatively small due to the nature of the decision problem. Our approach relies on matching each of the 18 general education courses to a corresponding major (producing 14 course-to-major mappings – see Table A.1 for a breakdown). Since roughly 71.31% of students enroll in majors that do not have a clear corresponding general education course (31.18% engineering, 8.06% foreign language, and 32.07% other noncorresponding majors), the dependent variable is capturing the remaining 28.69% of majors split over the 14 course-to-major mappings.

5 While USMA does not require students to report the number of hours they spent on the course, such self-reported information would be helpful in other contexts to examine the dynamic effort response explanation of caveat (a).

influence on earnings than institution choice (Kirkeboen et al., 2016; Ost et al., 2019). Understanding the factors that influence this choice can help inform students, professors, and college administrators. Finally, our results yield simple, low-cost policy prescriptions to institutions that want to increase or decrease the probability that students select a certain major. By modifying what early morning courses are offered or manipulating the breaks before certain courses, universities can nudge students toward or away from certain majors. We discuss more far-reaching implications of attribution bias across other decisions and contexts in the conclusion.

This paper expands the behavioral literature on state-dependent preferences. Recent work has found evidence of projection bias (Loewenstein et al., 2003) – behavior resulting from individuals underestimating the degree to which their current tastes will match their future tastes in other states – in a variety of settings. For example, evidence of projection bias has been documented in catalog orders (Conlin et al., 2007), automobile purchases (Busse et al., 2015), gym attendance (Acland and Levy, 2015), and – most closely related to our study – college enrollment decisions (Simonsohn, 2009). We provide field evidence of the type of attribution bias documented in (Haggag et al., 2019) – behavior resulting from misattributing the influence of a temporary state to a fixed attribute of a good or activity. Our study thus also relates to a number of papers that have documented related types of misattribution (e.g. Bertrand and Mullainathan, 2001; Brownback and Kuhn, 2019; Bushong and Gagnon-Bartsch, 2020; Cole et al., 2012; Erkal et al., 2019; Gagnon-Bartsch and Bushong, 2019; Schwarz and Clore, 1983; Weber et al., 2001; Wolfers et al., 2002).

It is worth noting that this form of attribution bias and projection bias are closely linked. While the two biases share some similarities, we believe there is value from a policy perspective in distinguishing them. Though transient variation in an underlying state produces biased beliefs in both models, a misattribution may persist well beyond that state passing, as one's evaluation becomes "stuck" to the good or experience that was consumed in that state. By contrast, once one's state matches that of the period of future consumption, a projection bias no longer has scope to operate. The relative "stickiness" of attribution bias implies both different long-run consequences and remedial measures, a point we return to in the conclusion. Moreover, from an empirical research perspective, a narrow focus on projection bias may restrict researchers' attention to variation in underlying states at the time of consumption choices. The model presented in this paper and in Haggag et al. (2019) shifts focus to variation in states at the time of a previous consumption episode, and thus expands the potential set of areas to look for systematic errors related to state-dependent decision making. In this specific context, for example, students may still be influenced by their fatigue in the introductory course even though most formal college major choices are made at later date when students may no longer be fatigued.[6]

Additionally, this paper builds on literature examining the effects of fatigue on individual judgment and performance. Recent research suggests that both a school's start time and prior exertion can have a significant effect on outcomes and decisions in both work and academic environments. Workers are less productive, prone to make mistakes, and more likely to be injured during night shifts (Folkard and Tucker, 2003; Smith, 1994) and students perform worse when they take early morning courses (Carrell, 2011; Edwards, 2012; Dills and Hernandez-Julian, 2008). Prior exertion also appears to have a significant influence on judgment and performance: judges become less likely to make the difficult decision to parole inmates as the number of cases they have seen in a row increases (Danziger et al., 2011), medical residents make significantly more mistakes the longer they have been on their shifts (Landrigan et al., 2004; Barger et al., 2006), and students perform worse in classes if they have attended multiple prior courses (Pope, 2016; Williams and Shapiro, 2018). While each of these studies shows that fatigue affects contemporaneous judgment and performance, we are unaware of any prior studies that examine how fatigue affects future decision making.

Lastly, this paper contributes to the broad literature on college major choice. Prior research has identified a number of standard factors that influence college major choice including differential tuition and student aid (Denning and Turley, 2017; Sjoquist and Winters, 2015; Stange, 2015), expected earnings (Berger, 1988; Beffy et al., 2012; Arcidiacono et al., 2012), instructor characteristics (Bettinger and Long, 2005; Carrell et al., 2010), ability (Arcidiacono et al., 2012; Stinebrickner and Stinebrickner, 2013), and tastes and preferences (Arcidiacono, 2004; Zafar, 2013). Our paper also fits within a smaller literature on the role of behavioral influences in college major, job, and career choice, including the roles of overconfidence (Reuben et al., 2017), psychological debt aversion (Field, 2009), and social information (Coffman et al., 2017).

The remainder of the paper is structured as follows. Section 2 describes a conceptual framework of college major choice that accounts for attribution bias. Section 3 describes our study environment and data. Section 4 reports our empirical strategy and primary results. Section 5 presents robustness tests of our results and explores the rational and psychological explanations for our findings. Section 6 concludes.

2. Conceptual framework

In this section, we discuss a simple framework for how attribution bias may affect a student's selection of a college major. The framework applies the model of attribution bias outlined by Haggag et al. (2019) to college choice. In the Haggag et al. (2019) model of consumer choice, an agent attempts to predict her instantaneous utility of consuming c while in state s_t, having previously consumed the item in an alternative state s_{t-1}. The agent is said to demonstrate attribution bias if her predicted utility falls between her true utility in her current (or future) state and her realized utility in the prior different state, i.e. $\tilde{u}(c, s_t) = (1-\gamma)u(c, s_t) + \gamma u(c, s_{t-1})$ for some $\gamma \in [0, 1]$. In this context, s is understood as the fatigue generated by either time of day or prior courses taken in the day.[7]

The college major choice studied in this paper departs from the stylized consumer choice model and the experiments discussed in the earlier work in a few ways. First, the "consumption" episode (i.e. each class meeting) is repeated numerous times before the retrospective decision period (i.e. the college major choice); however,

[6] This distinction from the standard conceptualization of projection bias is a point also confronted by Simonsohn (2009) in his study estimating the effect of weather during prospective college visit days on student enrollment decisions. Though he ultimately categorized it as a study of projection bias, he noted the distinction and drew a parallel to other work on misattribution (Dutton and Aron, 1974), writing that, "rather than projecting current utility, people appear to be projecting their remembered utility." In our study, if students are effectively making their decisions while still in the fatigued state, then it's difficult to fully rule out a version of projection bias explaining the results in this paper, a point returned to in the Conceptual Framework.

[7] While we model the prediction of utility as occurring in the state of time t (and thus characterized as attribution bias), it is possible that students are making their mental *decision* in the state of time *t-1* (i.e. while sitting in the classroom). We believe this is justified since students are typically making the *choice* (i.e. declaring their major) at a time period outside the class (see footNote 2). However, the alternative case could be characterized as an initial projection bias made at time *t-1* that remains sticky through to the point of the choice at time t.

the setting is almost ideal because the state is kept relatively constant across these several repeated consumption episodes.[8] Second, we narrowly define the consumption episodes as the class meeting times; however, students are likely to engage with the subject in other time periods during the semester (e.g. doing homework in the evening), a fact that works against us finding evidence of attribution bias. Third, in making a college major choice, the student is not only forecasting utility across similar consumption episodes (e.g. class meetings which may be spread across the day in future semesters), but also across episodes that may substantially differ (e.g. working in careers associated with that major).

As with prior work, we do not identify the specific mechanisms underlying the complementarity between the state and the consumption item. For example, one may experience higher utility of consuming a food when hungry due to hunger affecting the sensitivity of taste/smell receptors or through heightened attention to the taste itself (e.g. at the expense of attending to other attributes such as nutrition or texture). Likewise, in the context of this paper, incidental fatigue could lower one's utility from taking a class by diminishing goal-oriented attention broadly (e.g. see Boksem et al. (2005)) or through how any particular attribute is evaluated (e.g. by increasing irritability with an instructor's speaking style or tone that might otherwise be ignored). Beyond those attentional or mood channels, students may make misattributions over how difficult the subject is, their own subject-specific ability, or how "interesting" the subject is. Although a variety of factors such as future job characteristics, future pay, faculty, and program reputation are likely to influence a student's choice of major, a number of studies suggest that the most important factors are a student's subject-specific abilities and whether she enjoys or is interested in the subject (e.g. Beggs et al., 2008; Malgwi et al., 2005; Zafar, 2013) – thus misattributions over these factors may be particularly important. In Section 5.2, we begin the process of disentangling these channels using revealed preference and student course evaluation data. Ultimately, future work with more detailed and mixed data sources may be able to better parse the underlying channels of misattribution.

3. Data and institutional background

Data for this study come from administrative records at the United States Military Academy (USMA) at West Point, NY and includes 233,452 student-course observations from 18,753 freshman and sophomore USMA students between the years of 2001 and 2017. The main unit of analysis in the data is a student-course observation. USMA is a 4-year undergraduate institution with an approximate enrollment of 4,400 students. In total, USMA offers 38 majors within basic science, engineering, humanities, and social science. USMA also provides all students with the equivalent of a "full-ride" scholarship, but also requires students to attend all assigned classes, graduate within four years, and complete a 5-year service commitment in the United States Army. In spite of these unique attributes, the admissions rate, student-to-faculty ratio, class size, racial composition, and standardized test performance are similar to selective liberal arts colleges such as Williams College, Davidson College, and Washington and Lee University (Carter et al., 2017). USMA admits 10% of all applicants, has a student to faculty ratio of 7:1,[9] and limits class sizes to 18 students. The racial composition of our sample, shown in Table 1, is 72.1% white, 8.7% Hispanic, 8.5% black, and 7.2% Asian. Also, the standardized test performance in our sample reflects the selectivity of USMA, with average SAT math and verbal scores of 652/800 and 639/800 respectively.[10] While in many ways the student population is similar to other selective liberal arts colleges, some characteristics are unique. Only 16.5% of students in our sample are female, 16.3% have prior military service, 22.1% have prior college experience, 16.3% previously attended a military preparatory academy, 33.2% are Division I athletes, and students come from across the country with students from every state.[11]

In comparison to other colleges, students' time and schedules are highly structured at USMA. In the first three semesters, student schedules consist of 15 required courses in basic science, humanities, and social science.[12] These 15 courses are only taught to students in particular semesters and can be seen in Appendix Table A.1. Students do not have control over the order of these courses. Students are required to take each of these courses unless they test out. When courses are offered is also highly structured: courses at USMA are all 55 min long and only start at one of six times: 7:30 AM, 8:40 AM, 9:50 AM, 11:00 AM, 1:55 PM and 3:05 PM.[13] All classes are assigned a fixed time, but not a fixed day of the week: courses rotate weekly between a Monday/Wednesday/Friday schedule and a Tuesday/Thursday schedule so all courses are taken on each day of the week throughout the semester.[14] Particularly important to our approach is that instead of students choosing their own schedules, USMA's registrar's office assigns the time, day, and instructor for each course. This plausibly random assignment to course scheduling is a key component of our identification strategy. In addition, on a typical day students are required to participate in breakfast formation at 6:55 AM and breakfast from 7:05 to 7:20 AM. Depending on students' schedules, some students start their first classes as soon as 7:30 AM and classes, studying, and rest occur from 7:30 AM to 4:00 PM with a break for lunch.[15] The evening consists of intramural and varsity athletics, dinner, and an evening

[8] More precisely, the treatment variable, class time, is held constant. The manipulated state, fatigue, may acclimate to class time as the semester progresses, but this should weaken our effects.

[9] Source: https://nces.ed.gov/collegenavigator/?q=united+states+military+academy&s=all&id=197036. Accessed 9/14/2017.

[10] National averages during this time were approximately 516/800 and 501/800 for math and verbal scores, respectively. Source: https://nces.ed.gov/fastfacts/display.asp?id=171 accessed 4/17/2018.

[11] This diversity is driven by a rule that places a limit on the number of students that can come from each congressional district.

[12] During the first three semesters at USMA, student schedules consist nearly entirely of the required courses listed in Table A.1. Specifically, students typically take 14/18 of the courses listed in Appendix Table A.1. during their first three semesters at USMA. All students who do not test out of the following courses will take Chemistry 1, Chemistry 2, English Composition, Literature, Calculus 1, Calculus 2, Psychology, Computer Science, and Physics (9 total courses). Students also select 2/3 history courses available (US History, Western Civilization, World History), and additionally, students will be assigned two of Geography, American Politics, Economics, and Philosophy in the first semester of their sophomore year. Finally, depending on math preparation, students either take a math sequence of Math Modeling, Calculus 1, and Calculus 2, or a sequence of Calculus 1, Calculus 2, and Probability and Statistics. Once a student's course roster for the semester is set, the registrar's office uses a computer algorithm to assign a student to a specific time and instructor for each course. This algorithm does account for scheduling constraints, such as Division I sport practices in scheduling, but does not consider student demographic characteristics when determining the assigned time of day for a course.

[13] These listed start times are for years 2007–2017. From 2001 to 2006, classes at USMA started at 7:35 AM, 8:40 AM, 9:45 AM, 10:50 AM, 1:50 PM, and 2:55 PM. Given the similarity of these start times to those from 2007–2017, we treat these courses as if they started at 7:30 AM, 8:40 AM, 9:50 AM, 11:00 AM, 1:55 PM and 3:05 PM, respectively.

[14] Classes that begin this rotation on the first day of the semester are called Day 1 courses and classes that begin on the second day of the semester are called Day 2 courses. In total there are six Day 1 slots and six Day 2 slots.

[15] All students are required to attend breakfast formation and breakfast. However, many students without a 7:30 AM class take a nap after breakfast.

Table 1
Summary Statistics for Cadets.

	Mean	Std. Dev.	Min.	Max.	N
Female	0.165	0.371	0	1	18,753
Asian	0.072	0.258	0	1	18,753
Black	0.085	0.279	0	1	18,753
Hispanic	0.087	0.282	0	1	18,753
White	0.721	0.449	0	1	18,753
Prior Military Service	0.163	0.370	0	1	18,753
Prior College Attendance	0.220	0.414	0	1	18,753
USMA Preparatory Academy	0.149	0.356	0	1	18,753
Division I Athlete	0.332	0.471	0	1	18,753
Age	19.8	1.005	18	27	18,753
Average Number of Courses	5.14	0.269	2	6.5	18,753
SAT Verbal	639	73.4	300	800	18,753
SAT Math	652	68.4	390	800	18,753

Observations from students attending USMA between 2001 and 2017.

study period. The scheduled study period ends at 11:30 PM and students are instructed to turn their overhead lights off at 12:00 AM.[16] Also important to our analysis is the structured process of selecting a major. Within the years of our sample, students declare a major during the first semester of their sophomore year during a 4–5 week window.[17] Students then begin coursework in their selected major in the first semester of their junior year. It should be noted that 29% of students choose a major that directly corresponds to a general education course taught in the first three semesters, and that 83% of students graduate with the major they choose in the first semester of their sophomore year.

The structured nature of the first two years for students at USMA has several characteristics that make this setting ideal for testing the relationship between course scheduling and major choice. Because all students are either required to take or test out of a set list of courses, students have nearly identical schedules. This allows us to compare outcomes among students who take the exact same roster of courses in a semester. Also, because students all select their majors at the same time during their third semester, we are able to cleanly identify which courses could plausibly influence a student's major choice. Additionally, because students do not have control of when they take classes during the day, it is plausible that students are conditionally randomly assigned to class hours.[18] Finally, because students face strict consequences for missing courses, we do not have to be concerned with differential attendance driving our results.[19]

The key assumption in our identification strategy is that students are conditionally randomly assigned to instructors and course schedules. The unique environment at USMA, where students have little control over which courses they take and do not choose their instructors or course hours, makes self-selection into courses at certain times with certain instructors unlikely. However, we examine the randomization to course schedules in Tables 2 and 3. In Table 2 we compare observable characteristics between students assigned to first period courses and courses at other periods during the day, where each observation is at the student-course level. In column 1 of Table 2 we simply regress the individual characteristics onto assignment to a first period course, controlling for year fixed effects. With 3/11 characteristics differing at the 1% level and a joint F-test p-value of 0.00, the observable characteristics are not unconditionally balanced across first and other period courses. This, however, is unsurprising. Because not all courses are offered in every period, assignment to a first hour course is partially a function of a student's course roster. Students with different course rosters are also likely to be different on other dimensions, so controlling for students' course composition is an important precursor for conditionally random assignment to courses. Additionally, Division I athletes have practice schedules that often keep them from taking afternoon courses. Therefore, in column 2 of Table 2 we control for both course roster fixed effects and an indicator for Division I athlete status. The student characteristics appear to be balanced once these controls are added, with no characteristics varying between first and other hour courses at the 5% level, a joint F-test p-value of 0.19, and only age of students varying at the 10% level.[20] Adding course fixed effects in column 3 does little to change the coefficient or joint test values. Column 4 adds a more flexible control for athlete schedules, including a sport-by-semester fixed effect instead of a general indicator. This control does make age differ across course times at the 5% level, but improves the overall balance: increasing the f-stat p-value from 0.22 to 0.63. Furthermore, an age difference of -0.001 years is likely to be economically insignificant. Adding faculty fixed effects in column 5 makes little difference for the individual or joint balance across characteristics. Altogether, columns 2–5 of Table 2 suggest that assignment to first hour courses is random after controlling for course composition and Division I athlete status.

In Table 3, we also examine whether the variation in the number of preceding courses a student has before a course appears to be conditionally random. In column 1 we do not control for student course roster fixed effects and find some differences in characteristics among students with different numbers of preceding courses. Specifically, 1/12 variables differ at the 1% level in column 1 and the Joint F-statistic p-value is 0.03. However, including course roster fixed effects and controls for Division I athlete status eliminates the imbalance across characteristics. In columns 2–4 – which add course roster fixed effects, course fixed effects, and sport-by-semester fixed effects, respectively – no variables significantly differ and all generate F-statistic P-values greater than 0.95. Adding unique course fixed effects (i.e. the interaction between instructor,

[16] Students may continue studying with a desk lamp after 12:00 AM.

[17] USMA tradition is to refer to freshman, sophomores, juniors, and seniors as Plebes, Yearlings, Cows, and Firsties, respectively. We use the more common terminology for clarity. This window to choose majors opens between August 27 and September 11 and closes between September 29 and October 11. Beginning with the graduating class of 2019, students declared a major in between their first and second year, but could easily change majors prior to their junior year. We include these students in our sample, but our results are robust to excluding the class of 2019 and 2020 and only including observations from the first year.

[18] While there are certain factors that influence when a student takes a class (e.g. certain courses are not offered at all hours and student-athlete practices that conflict with certain periods), they are observable to the researcher and assignment to classes at a certain hour are plausibly randomly assigned conditional on these factors.

[19] The typical penalty for students who have an unexcused absence is to spend 10 h during the weekend walking along a line in a formal uniform.

[20] Conditional on the other covariates, a Division I athlete is 1.99% more likely to be enrolled in an early morning class than a non-athlete (p-value = 0.000).

Table 2
Assignment to First Period Classes: Conditional Randomization Checks.

	(1)	(2)	(3)	(4)	(5)
Female	0.0023*	0.0017	0.0017	−0.0003	−0.0003
	(0.0013)	(0.0012)	(0.0012)	(0.0013)	(0.0011)
Age	−0.0005	−0.0013*	−0.0013*	−0.0014**	−0.0012**
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0006)
Asian	−0.0014	−0.0006	−0.0006	−0.0010	0.0001
	(0.0020)	(0.0019)	(0.0020)	(0.0020)	(0.0018)
Black	−0.0053***	−0.0026	−0.0026	−0.0019	−0.0019
	(0.0020)	(0.0018)	(0.0018)	(0.0018)	(0.0016)
Hispanic	−0.0004	0.0018	0.0018	0.0015	0.0013
	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0014)
SAT Verbal	0.0032***	−0.0000	−0.0000	−0.0002	−0.0006
	(0.0012)	(0.0011)	(0.0011)	(0.0011)	(0.0010)
SAT Math	−0.0048***	−0.0003	−0.0003	−0.0003	−0.0003
	(0.0016)	(0.0014)	(0.0014)	(0.0014)	(0.0013)
Academic Composite	−0.0032	0.0000	0.0000	0.0001	0.0004
	(0.0022)	(0.0018)	(0.0018)	(0.0018)	(0.0016)
Prior Service	−0.0014	0.0015	0.0015	0.0015	0.0020
	(0.0030)	(0.0028)	(0.0028)	(0.0028)	(0.0025)
Prior College	0.0010	0.0017	0.0017	0.0015	0.0013
	(0.0020)	(0.0019)	(0.0019)	(0.0019)	(0.0017)
USMA Preparatory School	−0.0038	−0.0040	−0.0040	−0.0027	−0.0026
	(0.0031)	(0.0029)	(0.0028)	(0.0028)	(0.0026)
N	233,452	233,452	233,452	233,452	233,452
R^2	0.0031	0.0323	0.2059	0.2061	0.2592
F-Stat *P*-Value	0.0000	0.1948	0.2209	0.6257	0.6018
Division I Athlete	N	Y	Y	N	N
Course Roster FE	N	Y	Y	Y	Y
Course FE	N	N	Y	Y	Y
Sport x Semester FE	N	N	N	Y	Y
Faculty FE	N	N	N	N	Y

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Each specification represents results for a regression where the dependent variable is an indicator for first period. The SAT verbal, SAT math, and academic composite variables were divided by 100 prior to estimation. All specifications include a year fixed effects and columns 2 and 3 include an indicator for being a recruited Division I athlete. In both columns 2 and 3, Division I athlete status is associated with an 0.58 percentage point increase in assignment to a 7:30 AM course (*p*-value = 0.00). The course roster fixed effect is a fixed effect for the particular combination of courses a student takes in a given semester (e.g. Calculus I, Economics, US History, and Physics). Robust standard errors are two-way clustered at the individual and section-by-year levels (15,370 total clusters).

semester, section, and course) in column 5 does lead the sex of students to vary at the 5% level. However, no other variables differ at any significance level and the joint F-statistic has a *p*-value of 0.39. Similar to our findings in Table 2, Table 3 suggests that variation in the number of preceding courses is conditionally random after controlling for course roster fixed effects.

4. Empirical strategy and main results

4.1. Empirical strategy

We start by examining the relationship between assignment to a 7:30 AM course and whether a student selects a corresponding major. Our approach is similar to that taken by Carrell (2011) to study the effects of school start time on performance at the United States Air Force Academy (USAFA).[21] Using student-course level data, the basic approach is to regress a binary variable for whether a student to major in a corresponding subject area to course c on an indicator for if the course is an early morning course (7:30 AM). Since having an early morning course is *conditionally* random (as shown in Table 2), we also include the set of fixed effects that are needed to provide this conditional random assignment (i.e. roster fixed effects, course fixed effects, and year fixed effect). In addition to these fixed effects, we will show that the results are robust to the addition of student and peer demographic controls and instructor fixed effects. To identify the causal effects of being assigned an early morning class on college major choice, we estimate the following equation:

$$Y_{icjts} = \beta F_{icts} + \delta_1 X_i + \delta_2 \frac{\sum_{k \neq i} X_{kcts}}{n_{cts} - 1} + \delta_3 R_{it} + \gamma_c + \phi_j + \lambda_t + \epsilon_{cjts} \qquad (1)$$

where Y_{icjts} is an indicator for whether individual i in course c with professor j during time-slot s in year t chooses to major in a corresponding subject area.[22]. F_{icts} is an indicator of whether the course is an early morning course (7:30 AM). X_i is a vector of student characteristics including: age, sex, race/ethnicity, SAT math and SAT verbal test scores, and leadership scores. $\frac{\sum_{k \neq i} X_{kcts}}{n_{cts}-1}$ is a vector of the average characteristics of a student's peers within a course. Particularly

[21] A more exact extension of their approach would be to estimate the effects of being assigned an early morning course on whether students select the corresponding major(s) of any of their classes throughout that day (rather than just for the first course of the day). However, Carrell (2011) only find performance effects throughout the day when the first course started at 7:00 AM. When courses started at USAFA at 7:30 AM (as at USMA) Carrell et al. find students perform poorly in the 7:30 AM course but there are no residual effects of an early morning course on the performance of students later in the day. We reproduce this finding in our context: assignment to a 7:30 AM course reduces performance in the course, but does not affect performance in subsequent courses. For this reason, we focus our analysis only on the first hour courses.

[22] We observe both the major students initially select and the major students finally choose (i.e. graduating major). Our analyses use the graduating major as the outcome of interest. However, our results do not change in magnitude or significance if we use initial major choice as our outcome instead as can be seen in Table A.15

Table 3
Number of Preceding Classes: Conditional Randomization Checks.

	(1)	(2)	(3)	(4)	(5)
Female	0.0003	0.0004	0.0004	0.0012	0.0063**
	(0.0030)	(0.0030)	(0.0030)	(0.0032)	(0.0028)
Age	0.0002	−0.0004	−0.0004	−0.0003	−0.0009
	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0013)
Asian	0.0043	0.0047	0.0046	0.0050	0.0053
	(0.0045)	(0.0046)	(0.0046)	(0.0046)	(0.0041)
Black	−0.0009	−0.0028	−0.0028	−0.0006	−0.0031
	(0.0043)	(0.0042)	(0.0042)	(0.0042)	(0.0036)
Hispanic	−0.0033	−0.0008	−0.0008	−0.0003	−0.0007
	(0.0039)	(0.0038)	(0.0038)	(0.0038)	(0.0034)
SAT Verbal	0.0012	−0.0028	−0.0028	−0.0024	−0.0016
	(0.0022)	(0.0024)	(0.0024)	(0.0024)	(0.0021)
SAT Math	0.0085***	0.0006	0.0006	0.0005	−0.0000
	(0.0029)	(0.0028)	(0.0028)	(0.0028)	(0.0025)
Academic Composite	−0.0012	−0.0002	−0.0002	−0.0000	0.0031
	(0.0037)	(0.0037)	(0.0036)	(0.0037)	(0.0032)
Prior Service	−0.0066	0.0011	0.0011	0.0006	−0.0012
	(0.0054)	(0.0054)	(0.0053)	(0.0054)	(0.0045)
Prior College	0.0070**	−0.0007	−0.0007	−0.0007	0.0027
	(0.0035)	(0.0036)	(0.0036)	(0.0036)	(0.0030)
USMA Preparatory School	0.0032	0.0022	0.0022	0.0019	0.0027
	(0.0057)	(0.0058)	(0.0057)	(0.0058)	(0.0048)
N	233,452	233,452	233,452	233,452	233,452
R^2	0.0094	0.0863	0.1227	0.0867	0.5043
F-Stat *P*-Value	0.0275	0.9635	0.9621	0.9852	0.3901
Division I Athlete	N	Y	Y	N	N
Course Roster FE	N	Y	Y	Y	Y
Course FE	N	N	Y	Y	Y
Sport x Semester FE	N	N	N	Y	Y
Unique Course FE	N	N	N	N	Y

$^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Each specification represents results for a regression where the dependent variable is the number of immediately preceding courses. The SAT verbal, SAT math, and academic composite variables were divided by 100 prior to estimation. All specifications include a year fixed effects and columns 2 and 3 include an indicator for being a recruited Division I athlete. In both columns 2 and 3, Division I athlete status is associated with 0.0087 additional preceding courses (*p*-value = 0.00). The course roster fixed effect is a fixed effect for the particular combination of courses a student takes in a given semester (e.g. Calculus I, Economics, US History, and Physics). Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Robust standard errors are two-way clustered at the individual and section-by-year levels (15,370 total clusters).

important to our analysis is our course roster fixed effect (R_{it}), which is a fixed effect for the particular combination of courses a student takes in a given semester.[23] Due to the rigid structure of when students take courses, the course roster fixed effect additionally subsumes a semester fixed effect. By comparing outcomes only among students who share the exact same combination of courses, we are able to isolate the effect of scheduling differences on course outcomes. Additionally, γ_c is a course fixed effect (e.g. Calculus I or World History),[24] ϕ_j is an instructor fixed effect, and λ_t is a year fixed effect. Our controls also include fixed effects for the number of courses a student has assigned on that same day and for the number of courses immediately preceding each course. These variables are intended to isolate the fatigue generated by early morning assignment from other sources of fatigue such as exertion.[25]In the above equation, our parameter of interest is β which measures the effect of being assigned an early morning course on the probability of a student selecting a major in that early morning course's subject area. We estimate this equation with ordinary least squares, two-way clustering our standard errors by both individual and course section.

In our second empirical strategy, we estimate whether the number of courses of immediately preceding a required course affects major choice. This design draws motivation from studies such as Pope (2016) and Williams and Shapiro (2018), which find that, conditional on a school start time, students perform worse as they spend more time in school on a given day. Additionally, this approach relates to a broader literature that links sustained effort to a drop in performance across a number of domains (e.g. Folkard and Tucker, 2003; Landrigan et al., 2004; Danziger et al., 2011). To identify the effects of the number of preceding courses on college major choice, we estimate:

$$Y_{icjts} = \beta_1 Preceding_{icts} + \delta_1 X_i + \delta_2 \frac{\sum_{k\neq i} X_{kcts}}{n_{cts}-1} + \delta_3 R_{it} + \gamma_{ctjs} + \lambda_t + \mu_i + \epsilon_{cjts} \quad (2)$$

where $Preceding_{icts}$ is a count of immediately preceding courses and γ_{ctjs} is an instructor-semester-section-course (i.e. "unique class") fixed effect. Our controls also include fixed effects for the period of the course to isolate the fatigue generated by preceding courses. All other variables are identical to those specified in Eq. 1. More specifically our regressor of interest, $Preceding_{icts}$, is defined as the streak of courses (i.e. with no break in between) before the course of interest. For example, if a student has one class at 8:40 AM and

[23] All students are required to complete or test out of all assigned first and second year courses. Additionally, several courses have honors sections that students are admitted into by either (a) having high academic qualifications or (b) expressing strong interest in majoring in the subject area. To avoid selection into testing out of certain courses or taking honors sections biasing our results, we include a fixed effect for each combination of courses students take. One caveat is that we treat all language courses as being the same course, as language courses are the courses over which students can exert the greatest amount of choice, and including all combinations of languages and core courses would begin to closely approximate an individual fixed effect.

[24] Honors and non-honors sections are treated as different courses.

[25] By controlling for the number of courses and immediately preceding courses, our equation estimates the difference in major choice from those with similar workloads (e.g. both have a break before class and have the same number of courses they have prepared for), but one is assigned 7:30 AM section and another is assigned a later section. Our estimates are robust to excluding these controls, as can be seen in Appendix Table A.2.

another at 11:00 AM, the 11:00 AM class will be treated as having 0 immediately preceding courses. If the student instead has an additional class at 9:50 AM, then the 11:00 AM class will be treated as having 2 immediately preceding courses and the 9:50 AM class will be treated as having 1 immediately preceding courses. Finally, the counts are reset at 1:55 PM, as all classes prior to this period will have had a lunch break in the schedule. The variation in the preceding course measure is shown in Appendix Table A.5.

In addition to providing another source of variation in fatigue, this second approach allows us to exploit variation in fatigue within specific classrooms. Using this within-course variation in our estimates is attractive because it allows us to rule out potential classroom level effects of fatigue on major choice, such as the fatigue of the instructor or peer effects.

Finally, as noted above, our outcome variable is the mapping of the course to its most closely linked major, as outlined in Appendix Table A.1. While other studies often pool subjects more broadly, we have chosen to apply a relatively narrow definition of the course-to-major mapping for two reasons.[26] First, our definition creates an outcome that corresponds as closely to the level of treatment as possible (e.g., we evaluate the effects of a 7:30 AM assignment in Chemistry I on whether a student majors in chemistry). For example, consider a student who took Calculus 1 at 7:30 AM and Chemistry 1 at 3:00 PM. To map the effects of fatigue in those classes into a propensity to major in STEM, we would need to decide (a) how a student weights Chemistry versus Calculus in their perception of STEM more generally, and (b) how to model interactions between the different intensities of treatment across the two classes. Second, we believe our definition of major best matches student choice: students at USMA do not explicitly choose a field of study, but do explicitly choose a major. This narrow definition allows for a more direct test for attribution bias, though it also narrows the scope for measuring how much attribution bias may move a student.

Table 4
Fatigue and Selection of Major in Subject Area.

	(1)	(2)	(3)
Panel A: Time of Day			
7:30 AM Course	-0.0019^{**}	-0.0020^{**}	-0.0020^{**}
	(0.0009)	(0.0009)	(0.0009)
N	233,452	233,452	233,452
R^2	0.0652	0.0770	0.0772
Dependent Variable Mean	0.0190	0.0190	0.0190
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
Panel B: Number of Immediately Preceding Courses			
	(1)	(2)	(3)
Number of Preceding Courses	-0.0015^{**}	-0.0023^{***}	-0.0023^{***}
	(0.0007)	(0.0008)	(0.0008)
N	233,452	233,452	233,452
R^2	0.0654	0.1361	0.1363
Dependent Variable Mean	0.0190	0.0190	0.0190
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

$^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,370 total clusters). All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 231,458, 231,457, and 231,457 in columns 1–3 of Panel A and 231,413, 231,398, and 231,398 in columns 1–3 of Panel B.

4.2. Main results

We present our estimates of Eqs. (1) and (2) in Panel A and B of Table 4 respectively. Columns 1–3 of Panel A report the estimates of the effects of early morning classes outlined in Eq. (1).[27] In each specification in Panel A our estimate of β is negative and statistically significant, indicating that assignment to a 7:30 AM course decreases the probability that a student will select a corresponding major. Column 1 of Panel A, which does not control for faculty, demographic, or peer demographic controls, suggests that assignment to an early morning course decreases the probability that a student majors in a corresponding major by 9.99%, or 0.19 percentage points (significant at the 5% level). Controlling for instructor fixed effects, demographic controls, and classmate demographics in columns 2 and 3 has no effect on our estimates: each of these specifications indicate that assignment to an early morning course reduces the probability that a student chooses a corresponding major by 10.34% and 10.53% respectively, or 0.20 percentage points (all significant at the 5% level).[28]

In Panel B of Table 4 we estimate whether prior exertion also influences what major a student selects. Specifically, we examine whether the number of immediately preceding courses a student is assigned—a different source of incidental fatigue—affects whether students choose a corresponding major. Our results corroborate our findings in Panel A and further suggest that fatigue reduces the probability that a student selects the corresponding major. In column 1 of Panel B we estimate Eq. 2 with course subject fixed effects instead of unique course fixed effects.[29] In this estimate we find that each immediately preceding course decreases the probability that a student selects a corresponding major by 7.94% or 0.15 percentage points (significant at the 5% level). In each column 2–3 of Panel B we control for unique course fixed effects, additionally including demographic controls in column 3. These specifications control for any classroom-specific variation such as the level of preparation and fatigue of the instructor, the light, smell, and temperature in the room, and the behavior of the students within the class. Both of these specifications provide consistent evidence that increasing the number of back-to-back courses before a class reduces the probability of majoring in a related subject. In column 2 we find that each immediately preceding course decreases the probability that a student chooses a narrowly defined corresponding major by 12.11% or 0.23 percentage points (significant at the 1% level). Adding demographic controls in column 3 does not change our estimates or precision.

[26] For example Ost et al. (2019) use categories of STEM, Arts/Humanities, Business, Social Sciences, Education, and Health, and Kirkeboen et al. (2016) use categories of Humanities, Social Science, Teaching, Health, Engineering, Technology, Business, and law.

[27] Estimates that replace the dependent variable with a broader mapping of courses to majors and estimates that exclude athletes can be found in Appendix Tables A.7 and A.9. Our results are robust to these alternate specifications. Mappings of courses to broad and narrow majors can be found in Appendix Table A.1.

[28] While we use a series of controls and fixed effects in our main specifications to ensure conditional randomization, we see similar results in simplified versions of our primary models. Tables A.2 and A.3 show estimates of simplified versions of our models alongside estimates of our primary models. Additionally, we show that our results are robust to a non-linear logit specification in Table A.4.

[29] This specification is nearly identical to the specifications outlined in Eq. 1 and estimated in Panel A of Table 4, except with a treatment of the number of preceding courses in place of a treatment of 7:30 AM course.

4.3. Heterogeneity

As noted in Section 3, USMA is a unique institution that draws students who are likely different on both unobservable and observable dimensions from students at other institutions. For example, only 16.5% of students are female and 22.1% have previous college experience. While the federal service academies provide key features that increase internal validity – as leveraged in many other prominent studies (e.g. Carrel et al. 2010a, 2010b, 2011) – these student body characteristic differences are important to keep in mind when considering external validity. For this reason, we estimate heterogeneous treatment effects in Table 5 along several easily-measured student and course characteristics (gender, race, prior college experience, entering SAT score, and STEM course). We do not see consistent heterogeneous effects of fatigue across gender, race, or academic aptitude. For example, while early morning effects are less pronounced for female students, immediately preceding courses have a slightly larger effect on this group (though insignificantly different). It's important to keep in mind that each of these characteristics may be correlated with unobservables in ways that may not generalize to other institutions. Thus, while these patterns could inform future tests in other contexts, they remain somewhat speculative.

4.4. Effect magnitudes

To provide context for our results, in Table 6 we estimate four benchmark comparisons for our results. The first two estimates exploit similarly (conditionally) randomly assigned influences (instructor characteristics), while the latter two use potentially endogenous regressors. The first benchmark is motivated by Carrell et al. (2010), who find that female students are more likely to select STEM majors if the instructors in their general education STEM courses are female as well. In column 1 of Table 6, we examine the effect of having a female instructor in a general education STEM course on the likelihood that a female student enrolls in the corresponding major. We find that women are about 0.64 percentage points more likely to choose a corresponding major when their instructor is female, a point estimate that is about four times larger in absolute magnitude than our estimated effects of early morning course assignment and each immediately preceding course.[30]

The second benchmark is the effect of being randomly assigned to an instructor with better prior course evaluations for that same course. In column 2 of Table 6, we find that assignment to an instructor with a 1 standard deviation higher prior aggregate course-evaluation average increases the probability that a student majors in a corresponding course by 0.19 percentage points.[31] This estimate is nearly identical in absolute magnitude to our effects of early morning courses and immediately preceding courses on major choice.

In column 3 of Table 6, we estimate the correlation between a student's course evaluation and major choice. While this relationship is not causal, we find that a student who gives a course a 1 standard deviation higher rating is 0.70 percentage points more likely to choose a corresponding major. This suggests that our effects of fatigue are comparable with approximately a 0.27 standard deviation drop in instructor evaluations. In column 4 we similarly examine the correlation between student performance and major choice and find that a 1 standard deviation improvement in grades corresponds to a 1.38 percentage point increase in the probability that a student selects a corresponding major. Therefore, our estimates are comparable with approximately a 0.14 standard deviation drop in performance.

The benchmarks we use raise interesting questions around their own mechanisms. It's possible that each may partially reflect distinct types of attribution bias themselves. For example, a well-evaluated professor may not meaningfully improve student learning of the material (Carrell and West, 2010), and a student may not face that instructor again in the future. Nonetheless, we find that these professors do have an influence on whether their students enroll in the major. On the one hand, such a student may be failing to adjust for what could be characterized as a transient influence. This shares some similarities to our conceptualization of attribution bias, albeit one in which the "state" may not be transparent to the student. On the other hand, it's plausible that such instructors could rationally influence students' assessment of the major by providing a signal of the type of individual that they will face in future courses or in that career. Similarly, it's also possible that popular instructors do a better job of actively recruiting students to enroll in the major. Ultimately, we do not attempt to disentangle the mechanisms underlying these benchmarks in this paper.

5. Mechanisms

In this section, we examine potential mechanisms driving the observed relationship between class times and college major choice. First, we establish some support for a "first stage" for attribution bias to operate, i.e. that class experiences are state-dependent with respect to fatigue. To that end, Table 7 shows that course performance is slightly diminished across both sources of fatigue (by between 0.015 SD to 0.053 SD), while Table 8 shows that instructor evaluations for early-morning courses are significantly lower (though we do not find this for back-to-back courses). While this diminished performance provides scope for attribution bias, it also raises the possibility that students may avoid majors in which they rationally anticipate diminished performance due to poorer learning outcomes. In Section 5.2, we examine this hypothesis and find that students assigned to early morning and back-to-back courses do no worse in subsequent required courses in the sequence (Tables 9 and 10) and that effects on college major source are relatively robust to controlling for performance in the initial course (Table 11), especially so for back-to-back courses. Finally, in Section 5.3, we examine a number of other threats to inference (e.g. Table 12 shows a falsification test for a particular selection on unobservables threat and Appendix Table A.4 shows the analysis is robust to using a conditional logit). The tests described in this section have several caveats which we elaborate upon in the sub-sections. While the data do not allow us to fully pin down mechanism, the results in this section provide suggestive support for attribution bias as a primary explanation for the findings.

5.1. Fatigue and attribution bias

Our analysis is motivated by the assumptions that early or back-to-back courses increase student fatigue, and that the resultant fatigue in a class reduces a student's overall enjoyment of it (i.e. that the class experience is state-dependent with respect to fatigue). An ideal dataset would allow us to measure the effect of

[30] We find that assignment of female students to either male or female instructors in STEM courses is uncorrelated with student characteristics, which allows us to estimate a causal relationship between instructor assignment and major choice.

[31] We construct our measure of prior course evaluations by averaging all of an instructor's prior evaluations within a course and then creating a z-score measure within a course. Similar to our findings in column 1, we find that student observable characteristics are uncorrelated with prior course characteristics.

Table 5
Heterogeneous Effects of Fatigue on Student Major.

	(1)	(2)	(3)	(4)	(5)
Panel A: Time of Day					
7:30 AM Course	−0.0023***	−0.0011	−0.0020**	−0.0019*	−0.0035
	(0.0008)	(0.0009)	(0.0009)	(0.0010)	(0.0022)
7:30 AM*Female	0.0037*				
	(0.0022)				
7:30 AM*Minority		−0.0021			
		(0.0016)			
7:30 AM*Prior College			0.0015		
			(0.0018)		
7:30 AM*High SAT				0.0005	
				(0.0015)	
7:30 AM*STEM					0.0021
					(0.0024)
R^2	0.039	0.039	0.039	0.039	0.039
N	233,452	233,452	233,452	233,452	233,452
Panel B: Number of Immediately Preceding Courses					
	(1)	(2)	(3)	(4)	(5)
Preceding Courses	−0.0021***	−0.0022**	−0.0026***	−0.0014	−0.0014
	(0.0008)	(0.0009)	(0.0008)	(0.0009)	(0.0011)
Preceding*Female	−0.0012				
	(0.0015)				
Preceding*Minority		−0.0003			
		(0.0012)			
Preceding*Prior College			0.0014		
			(0.0013)		
Preceding*High SAT				−0.0017	
				(0.0012)	
Preceding*STEM					−0.0014
					(0.0015)
R^2	0.041	0.041	0.041	0.041	0.041
observations	233,452	233,452	233,452	233,452	233,452

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,505 total clusters). All columns in Panel A and B include schedule fixed effects, Division I athlete controls, and sport-by-semester fixed effects, and controls for number of courses in a day. All columns in Panel A additionally include year fixed effects and indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B includes course time fixed effects and year fixed effects. Unique courses are individual classroom observations (i.e. the interaction between: instructor x year x semester x section x course). Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 231,450 in columns 1–5 of Panel A and 231,394 in columns 1–5 of Panel B.

Table 6
Benchmark Comparisons.

	(1)	(2)	(3)	(4)
Female STEM Instructor for Female Student	0.0064*			
	(0.0033)			
Prior Instructor Evaluation		0.0019***		
		(0.0007)		
Own Instructor Evaluation			0.0070***	
			(0.0005)	
Normalized Grade				0.0138***
				(0.0004)
N	17,608	60,481	88,251	233,452
R^2	0.1558	0.1103	0.1058	0.0722
Dependent Variable Mean	0.0212	0.0200	0.0205	0.0190

$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors clustered by unique course and student in parentheses. All columns include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. The dependent variable for each column is an indicator for whether the student chose the major corresponding to the relevant course. Column 1 only includes female observations in required STEM courses and reports the coefficient on having a female STEM instructor. STEM course subjects in our sample include: chemistry, computer science, math, physical geography, and physics. Column 2 includes all observations from students assigned to an instructor that has prior instructor evaluations and the reports the coefficient on the instructor's prior average evaluations. Column 3 includes all observations from students that fill out a course-specific evaluation and reports the coefficients on the student's own instructor evaluation. Column 4 includes all observations and reports the coefficients on the student's own normalized grade in the course.

the course schedule assignment on fatigue (i.e. a "manipulation check") and that a student's experience in a course is worse when fatigued (i.e. a "first stage" test of state-dependence). In the absence of these measures, we provide some suggestive evidence using the available large-scale administrative data. First, as a partial attempt at a manipulation check, we test whether students' grades in a class are affected in the expected direction by course schedules. Second, to shed light on state-dependence, we combine the class schedule data with course evaluations (over the subset of years for which there is coverage: 2008 to 2017). While there is no overall assessment of the class, and many of the questions ask students to evaluate the instructor, the pattern may shed light on

Table 7
Fatigue and Normalized Academic Performance.

Panel A: Time of Day			
	(1)	(2)	(3)
7:30 AM Course	−0.0472***	−0.0511***	−0.0525***
	(0.0087)	(0.0078)	(0.0069)
N	233,443	233,443	233,443
R^2	0.2292	0.2691	0.3435
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
Panel B: Number of Immediately Preceding Courses			
	(1)	(2)	(3)
Number of Preceding Courses	−0.0165***	−0.0132***	−0.0147***
	(0.0045)	(0.0043)	(0.0041)
N	233,443	233,443	233,443
R^2	0.2294	0.3624	0.4239
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,365 total clusters). All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A also include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Normalized academic performance measures an individual's distance in standard deviations from the course-by-semester mean performance. Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 231,449, 231,449, and 231,449 in columns 1–3 of Panel A and 231,404, 231,390, and 231,390 in columns 1–3 of Panel B.

students' enjoyment of the course. For both sets of analyses, we repeat our primary specifications but change the outcome variables to student performance or course evaluation rather than college major choice.

In column 1 of Panel A of Table 7 we find that assignment to an early morning course reduces performance by 0.047 standard deviations (significant at the 1% level).[32,33] Including instructor fixed effects, student demographics, and peer demographics in columns 2 and 3 slightly increases the magnitude of these estimates to 0.051 and 0.053 standard deviations, respectively. Panel B of Table 7 similarly shows that each additional immediately preceding course reduces performance by between 0.014 and 0.017 standard deviations. This magnitude is similar to Pope (2016), which finds that, conditional on a school start time, having a preceding course decreases math and English GPA by 0.009 and 0.015 standard deviations, respectively. Altogether our results in Table 7 are consistent with early morning courses and preceding courses increasing student fatigue and reducing student performance.

The data underlying the student evaluation exercise come from anonymous, voluntary, end-of-the-semester, online evaluations by USMA students starting in 2008. Despite these evaluations being optional, we maintain a 62% response rate among the courses in our sample. Each course evaluation includes 10 questions that were determined at the institutional level.[34] Students who start an evaluation are required to answer all questions to submit the evaluation, so we have even coverage of all 10 questions. We verify that evaluation response rates are not significantly predicted by assignment to a 7:30 AM course nor by the number of immediately preceding courses. Table 8 reports the analysis of the student evaluations. Columns 1–3 report the effect of having an early morning course on evaluations for different specifications and columns 4–6 report the effect of having immediately preceding courses. The first row shows the effect on an aggregate evaluation that puts equal weight on each of the 10 standardized questions regarding the instructor (5), course (2), schedule (2), and peers (1). Regardless of the specification, having an early morning course decreases a student's aggregate evaluation of the course by between 0.13 and 0.15 standard deviations. Although the effect size varies some for each specific question, the effect of an early morning course is highly statistically significant for each student evaluation question and ranges from 0.07 to 0.15 standard deviations. By contrast, we do not find significant effects of the number of immediately preceding courses.

Several issues complicate the interpretation of the student evaluation data. First, it could be the case that students treat course evaluations as a recommendation to future students, and thus attempt to adjust for the influence of fatigue on their enjoyment before answering. Under that interpretation, the analysis of course evaluations would itself be a test of attribution bias rather than the "first-stage" verification of state-dependence. Second, we do not have an overall assessment of the course or course content, but rather a set of questions heavily representing instructor performance. It is perhaps then unsurprising that we find negative effects for the 7:30 AM empirical strategy which may reflect instructor fatigue, but do not find it in immediately preceding courses results, where instructor and peer fatigue are held constant.[35] Ultimately, while columns 1–3 are suggestive of some scope for state-dependence, the evaluation data are limited in what they can say.

5.2. Rational response

While our results in Table 7 strongly suggest that students in early morning courses and with immediately preceding courses are fatigued, they also open the possibility that the decrease in the probability that students select a corresponding major is not driven by attribution bias, but a rational response to reduced performance. Specifically, students who were fatigued in certain courses because of random variation in their course schedules may be aware that they would have performed better in a later course or after a break *and* are aware of the influence of course schedule variation on other aspects of the college major choice,

[32] Normalized grades are calculated by taking the distance in standard deviations from the course code-by-semester mean grade. This is very similar to the approach taken by Carrell (2011). The effect size in our context is between 38% and 62% of the estimates found for the effects of a 7:30 AM class time in that paper (columns 4–6 of Table 6 in Carrell (2011)). Our specification most closely follows column 5, which is the estimate that is most similar to our own effect size.

[33] The degree to which grades reflect true learning differences hinges on the grading structure of courses. At one extreme, if every professor aimed to assign the exact same grade distribution, then average grades would be identical across all courses. While we cannot fully rule out some within-class curving, the USMA strongly discourages it. The official grading policy is as follows: "To the extent consistent with subject matter, instructors will provide cadets with a statement of the objectives for each course. Cadets will be evaluated against these objectives. Departments will avoid evaluation and grading practices that encourage reliance on curving." [Source: USMA Academic Program (Redbook), AY 2013–2013]

[34] Departments and course directors can (and do) add additional questions. However, we focus our analysis on the 10 questions asked to all students in our sample.

[35] Under this interpretation of the evaluation data, part of the treatment effect observed for the 7:30 AM identification strategy may reflect the worse performance of the instructors. Such a channel is a bit different than the model described in our conceptual framework. Rather than making an *intra*personal misattribution (i.e. failing to adjust for their own states), students may be making an *inter*personal misattribution (i.e. failing to adjust for instructors' states). Ultimately, our data do not allow us to disentangle these two subtly different mechanisms for the early morning results.

Table 8
Fatigue and Student Evaluations.

	7:30 AM Courses			Immediately Preceding Courses		
	(1)	(2)	(3)	(4)	(5)	(6)
Aggregate Evaluations	−0.1392***	−0.1482***	−0.1474***	−0.0046	−0.0035	−0.0033
	(0.0151)	(0.0107)	(0.0107)	(0.0078)	(0.0077)	(0.0077)
Instructor Encouraged Responsibility	−0.0809***	−0.0854***	−0.0842***	−0.0070	0.0015	0.0016
	(0.0119)	(0.0096)	(0.0096)	(0.0068)	(0.0074)	(0.0074)
Instructor Effective	−0.1343***	−0.1501***	−0.1493***	−0.0071	−0.0032	−0.0027
	(0.0161)	(0.0110)	(0.0110)	(0.0076)	(0.0072)	(0.0072)
Instructor Cares	−0.1030***	−0.1125***	−0.1116***	−0.0034	−0.0060	−0.0057
	(0.0149)	(0.0105)	(0.0105)	(0.0073)	(0.0073)	(0.0073)
Instructor Respectful	−0.0740***	−0.0826***	−0.0822***	−0.0009	−0.0078	−0.0076
	(0.0143)	(0.0106)	(0.0106)	(0.0073)	(0.0074)	(0.0074)
Instructor Stimulating	−0.1372***	−0.1453***	−0.1445***	−0.0034	−0.0008	−0.0004
	(0.0148)	(0.0108)	(0.0107)	(0.0074)	(0.0074)	(0.0074)
Course Motivated Learning	−0.1206***	−0.1335***	−0.1328***	−0.0003	−0.0013	−0.0009
	(0.0130)	(0.0100)	(0.0100)	(0.0072)	(0.0075)	(0.0075)
Course Increased Critical Thinking	−0.0937***	−0.1046***	−0.1043***	0.0031	−0.0021	−0.0017
	(0.0125)	(0.0102)	(0.0102)	(0.0072)	(0.0076)	(0.0075)
Schedule Allows Reflection	−0.0742***	−0.0788***	−0.0791***	−0.0081	−0.0053	−0.0052
	(0.0112)	(0.0099)	(0.0099)	(0.0072)	(0.0080)	(0.0079)
Schedule Enables Max Performance	−0.0765***	−0.0804***	−0.0806***	−0.0093	−0.0066	−0.0065
	(0.0109)	(0.0099)	(0.0099)	(0.0072)	(0.0080)	(0.0080)
Peers Contribute to Learning	−0.0964***	−0.1029***	−0.1021***	−0.0050	−0.0039	−0.0037
	(0.0103)	(0.0097)	(0.0097)	(0.0068)	(0.0075)	(0.0075)
N	88,254	88,254	88,254	88,254	88,254	88,254
Faculty FE	N	Y	Y	–	–	–
Demographic Controls	N	N	Y	N	N	Y
Peer Demographic Controls	N	N	Y	–	–	–
Unique Course FE	–	–	–	N	Y	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (8,403 total clusters). Each row/column entry reports the coefficient from a unique regression. Aggregate evaluations are a normalized sum of all the 5 point scores among 10 variables listed. Ratings from each individual question are converted from 5 point agreement scale questions to z-scores at the subject-year level. Observations come from student responses collected between 2008 and 2017. Aggregate evaluations, Schedule allows reflection, and Schedule enables max performance variables are not reported in 2008. Sample sizes for the other variables range between 100,076–100,830.

Table 9
Assignment to an Early Morning Class and Future Academic Performance.

	Panel A: Performance in Subsequent Subject Area Course		
	(1)	(2)	(3)
7:30 AM Course	0.0210*	0.0230**	0.0097
	(0.0114)	(0.0106)	(0.0081)
N	84,387	84,387	84,387
R^2	0.2000	0.2234	0.3212
	Panel B: Performance in Current Subject Area Course		
	(1)	(2)	(3)
7:30 AM Course	−0.0532***	−0.0599***	−0.0667***
	(0.0131)	(0.0116)	(0.0099)
N	84,387	84,387	84,387
R^2	0.2102	0.2633	0.3659
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (5,754 total clusters). For chemistry, English, math, and physics multiple courses are required. Panel B reports the effect of having an early morning course in the current semester on current performance in the subject (i.e. same semester normalized GPA in the subject). Panel A reports the effect of having an early morning course in the current semester on performance in the next course (i.e. next semester normalized GPA in the subject). All columns include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, fixed effects for the number of courses in a day, and indicators for whether courses were immediately preceded by other courses. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Normalized academic performance measures an individual's distance in standard deviations from the course-by-semester mean performance. Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 82,632, 82,631, and 82,631 in columns 1–3 of Panel A and 82,629, 82,628 and 82,628 in columns 1–3 of Panel B.

Table 10
Number of Preceding Courses and Future Academic Performance.

	Panel A: Performance in Subsequent Subject Area Course		
	(1)	(2)	(3)
Number of Preceding Courses	−0.0030	0.0029	−0.0001
	(0.0073)	(0.0075)	(0.0072)
N	84,387	84,387	84,387
R^2	0.2000	0.3121	0.3789
	Panel B: Performance in Current Subject Area Course		
	(1)	(2)	(3)
Number of Preceding Courses	−0.0057	−0.0016	−0.0033
	(0.0059)	(0.0050)	(0.0047)
N	84,387	84,387	84,387
R^2	0.2878	0.4531	0.5228
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (5,752 total clusters). For chemistry, English, math, and physics multiple courses are required. Panel B reports the effect of having a preceding course in the current semester on current performance in the subject (i.e. same semester normalized GPA in the subject). Panel A reports the effect of having a preceding course in the current semester on performance in the next course (i.e. next semester normalized GPA in the subject). All columns include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. Column 1 additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Normalized academic performance measures an individual's distance in standard deviations from the course-by-semester mean performance. Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 82,603, 82,564, and 82,564 in columns 1–3 of Panel A and 82,603, 82,564 and 82,564 in columns 1–3 of Panel B.

Table 11
Fatigue and Selection of Major in Subject Area, Controlling for Performance.

Panel A: Time of Day			
	(1)	(2)	(3)
7:30 AM Course	−0.0013	−0.0013	−0.0012
	(0.0009)	(0.0009)	(0.0009)
Normalized Grade	0.0126***	0.0127***	0.0140***
	(0.0004)	(0.0004)	(0.0004)
N	233,443	233,443	233,443
R^2	0.0713	0.0829	0.0836
Dependent Variable Mean	0.0190	0.0190	0.0190
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
Panel B: Number of Immediately Preceding Courses			
	(1)	(2)	(3)
Number of Preceding Courses	−0.0013**	−0.0021***	−0.0020***
	(0.0007)	(0.0008)	(0.0008)
Normalized Grade	0.0126***	0.0135***	0.0148***
	(0.0004)	(0.0004)	(0.0005)
N	233,443	233,443	233,443
R^2	0.0714	0.1419	0.1426
Dependent Variable Mean	0.0190	0.0190	0.0190
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,365 total clusters). All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A also include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Normalized academic performance measures an individual's distance in standard deviations from the course-by-semester mean performance. Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 231,449, 231,449, and 231,449 in columns 1–3 of Panel A and 231,404, 231,390, and 231,390 in columns 1–3 of Panel B.

Table 12
Falsification Test: Fatigue and Prior Major Choice.

Panel A: Time of Day			
	(1)	(2)	(3)
7:30 AM Course	0.0010	0.0018	0.0019
	(0.0017)	(0.0017)	(0.0017)
N	56,327	56,327	56,327
R^2	0.2021	0.2179	0.2186
Dependent Variable Mean	0.0167	0.0167	0.0167
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
Panel B: Number of Immediately Preceding Courses			
	(1)	(2)	(3)
Number of Preceding Courses	−0.0005	−0.0008	−0.0008
	(0.0011)	(0.0011)	(0.0011)
N	56,327	56,327	56,327
R^2	0.2021	0.2949	0.2952
Dependent Variable Mean	0.0167	0.0167	0.0167
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (3,708 total clusters). This table estimates the relationship between fourth semester variation in course schedules and third semester major choices. All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A also include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course).

such as enjoyment of the subject materials. In spite of this understanding, students may still decide not to pursue a corresponding major because their schedule caused them to believe that they are less prepared to succeed in that subject.

One reason to doubt this rational channel is the difference in magnitude of the estimates between Panel A and B in Table 7. In Table 4, our estimates of the effects of early morning courses and preceding courses on major selection are nearly identical. If both of these effects were being driven through differential preparation, we would also expect the effects of early morning courses and preceding courses on performance to be similar. However, our estimates of the effects of early morning courses on performance in columns 1–3 of Panel A in Table 7 are approximately four times the magnitude of the effects of preceding courses on performance reported in columns 1–3 of Panel B.

The size of the performance difference puts further doubt on it being the primary mechanism through which major choice is affected. While each the two sources of fatigue (each preceding back-to-back courses or an early morning course) reduce the likelihood of enrolling in a major by roughly 10%, they only reduce grades by between 0.015 to 0.053 standard deviations. While it's possible that the *causal* effect of grades on major enrollment is quite different than the *observational* relationship, the estimates in Section 4.3 suggest a much smaller correlation between grades in a introductory course and enrollment in that major (i.e. a 0.14 SD decrease in grades is correlated with a 10% decrease in majoring in that subject). Turning to causal estimates from outside USMA, two studies use regression discontinuity designs – Main and Ost (2014) find no effect of letter grades in economics on subsequent enrollment in the major, and Owen (2010) finds similar results for male (but not female) students at a different institution. However, as discussed in the Introduction, there are stories through which learning differences could still influence major choice even if they are not reflected in grades. Thus while the evidence presented in this section suggests that the rational performance channel is unlikely, it cannot be fully ruled out.

To further investigate whether it is plausible that students avoid majors due to persistent disadvantages caused by assignment to a 7:30 AM course or multiple courses in a row, we examine performance in required classes that are part of a sequence within the core curriculum (chemistry, English, math, and physics). In Tables 9 and 10, we test whether students assigned to those conditions in the first course of a sequence (e.g. Chemistry 1) perform worse in the next required class in the same subject area (e.g. Chemistry 2). Our findings in Table 9 are somewhat surprising: our estimates in columns 1 and 2 of Panel A indicate that assignment to a 7:30 section has a positive effect on performance in the next required subject area course of roughly 0.02 standard deviations. This could reflect students in the early morning course over-correcting for their diminished performance in the first course, e.g. if a student misattributes their diminished performance to their own subject-specific ability, they may supply additional effort in the subsequent course, thereby raising their grade relative to a student who was assigned to a later class time. However, this result should be taken with some caution, as adding own and peer demographic controls in Column 3 diminishes the magnitude of the point estimate to 0.01 SD, rendering it statistically insignificant. One potential concern is that students in courses identified in Panel A of Table 9 do not experience the same effects of 7:30 AM courses as identified in Table 7. Specifically, if there were little to no immediate effect of 7:30 AM courses on performance in the courses identified in Table 9, then we would be uncomfortable generalizing these results to the classes that are not followed by a required course. However, our results in columns 1–3 of Panel B of Table 9 indicate that the immediate effects of assignment to a 7:30 AM course on performance in these initial subject area courses (-0.053 to −0.067 standard deviations) is similar to the overall effects of 7:30 AM course assignment on performance reported in Table 7 (-0.047 to −0.053 standard deviations). Together the patterns in Panel A and B of Table 9 suggest that persistent negative effects of early morning course assignments on performance are unlikely to be large.

In Table 10 we explore whether being assigned preceding courses before a class affects performance in future classes in the same subject area. Our results in Table 10 are consistent with our findings in Table 9. In column 1 in Panel A of Table 10 we find a small, but statistically insignificant, negative effect of preceding courses on future performance. However, including unique course fixed effects and demographic controls in columns 2–3 generate positive, but statistically insignificant, estimates of the effects of preceding courses on future performance. One caveat to our results in Table 10 is that our estimates of preceding courses on current performance are imprecise and smaller in magnitude than our estimates in Table 7, so these estimates should be interpreted more cautiously than those presented in Table 9. Nevertheless, neither our results in Tables 9 nor 10 indicate that fatigue has persistent negative effects on performance. These results suggest that our primary results are not generated by a rational response to reduced performance.

Given that our findings are unlikely to be driven by a rational response to increased fatigue, we finally turn to exploring our primary posited mechanism of attribution bias. In our conceptual framework, we identified that student's states in early morning courses could generate attribution bias through two primary channels. First, students may misattribute the negative effects of fatigue on performance to their subject-specific ability. Second, students may misattribute the unpleasantness of taking a course when fatigued to a lack of enjoyment of the subject. One way to explore whether our effects are coming through performance or some other channel, such as tastes, is by including course performance as a covariate in our estimates. In Table 11 we include controls for course performance in the specifications outlined in Eqs. (1) and (2) to examine whether the effects of fatigue operate through the channel of performance or some other channel.[36] There are two important caveats to this approach. First, because course performance is endogenous, the estimates in these are no longer unbiased and should be interpreted cautiously. Second, even if the response to early morning courses is completely loading on performance this does not rule out other channels. For example, poor performance may be a result of a reduction of interest in the subject area. Nevertheless, we find that the results in Table 11 are suggestive that channels that rely on a response to performance are unlikely to fully explain our results. In columns 1–3 of Panel A we find that controlling for course performance reduces the absolute magnitude of our estimates of the effect of 7:30 AM courses on major choice by between 32% and 37% and makes our estimates statistically insignificant. In Panel B of Table 11 we find more compelling evidence that the effects of fatigue are not acting through a channel of reduced performance. After controlling for performance, we find in columns 1–3 of Panel B that each additional preceding course reduces the probability that students major in a related course by between 6.84% and 11.05% (or between 0.13 and 0.21 percentage points), which is nearly identical to the range of effects found in our primary estimates shown in Table 4.

5.3. Alternative mechanisms and robustness checks

An alternative explanation of our results is that poor performance in a required course could mechanically reduce the probability that students enroll in a corresponding major. While there are no formal performance standards to be admitted into a major at USMA, departments do have to approve an application to a major and have the discretion to reject an application. If departments have implicit grade standards in subject-area courses, then poor performance in a corresponding course might mechanically reduce the probability that students select a major. To address this potential concern we separate majors by the fraction of students who receive worse than a 3.0 grade point (i.e. B average) in corresponding required courses and enroll in the major. Majors with low fractions of below 3.0 grade point students we designate as "selective" majors. If our effects are driven by "selective" majors, we may be concerned about a mechanical relationship between variation in course schedules and college major choice. However, in Appendix Table A.6 we show that there is no evidence that our results are driven by selective majors but find suggestive evidence that both the effects of early morning courses and number of preceding courses on major choice are primarily driven by courses with less selective admission patterns. These results suggest that the effects are not driven by grade cut-offs.

Another potential concern is that, in spite of balance across observable characteristics observed in Tables 2 and 3, unobserved selection into course schedules could be contributing to our results. This potential unobserved selection could bias the results in either direction. Specifically, it is possible that students who have unobservable preferences for certain majors are able to arrange their schedules to have courses related to those majors at preferred times (i.e. times other than 7:30 AM and after breaks) causing an upward bias. Alternatively, if students could completely avoid early morning classes such that only "morning people" take early morning classes then our estimates may be biased downward. To test for this type of selection, we take advantage of the fact that students take required courses in both semesters during their Sophomore year, but are required to declare a major during the first semester of their Sophomore year. Instead of using graduation major as the outcome variable as done with all of the previous results, we instead use the initial major choice students make during the first semester of their Sophomore year.[37] We perform a falsification test that tests whether fatigue experienced during a course taken in the second semester of the sophomore year predicts the initial major choice made in the prior semester. If there is selection on unobservables, then the results of this falsification test should be similar to our main results. The results of this test are reported in Table 12. We find no evidence that having either an early morning class (Panel A) or preceding courses (Panel B) in the semester after a student's college major decision affects a student's major choice. The effects for early morning are in fact in the opposite direction of the main effects, although statistically insignificant. The effects for preceding courses are in the same direction of the main effects, however much smaller and statistically insignificant, suggesting a limited scope for selection on unobservables to explain the results.[38]

[36] Angrist et al. (2016) use the same approach to disentangle the crowd-in and crowd-out effects of a large grant program on other forms of student aid.

[37] Note that 83% of students graduate with the major they choose in the first semester of their sophomore year.

[38] Because of a much smaller sample size (56,327 vs. 233,452), our estimates in Table 12 are significantly less precise than our primary estimates. Nevertheless, we believe that this falsification test is informative because the results in Panel A of Table 12 are opposite signed of the primary estimates in Panel A of Table 4 and the results in Panel B of Table 12 are less than half the magnitude of the primary estimates in Panel B of Table 4.

We additionally examine the extent to which our results change as we expand the course-to-major mapping. We start in Appendix Table A.7 with a broader definition of the corresponding major, primarily as a robustness check. For example, while our main results examine how fatigue in a course (e.g. Chemistry 101) affects the likelihood of pursuing the most directly comparable major (e.g. Chemistry), in column 2 we allow for a slightly expanded set of majors (e.g. Chemistry or Chemical Engineering).[39] The results across Panels A and B of column 2 suggest that fatigue has a similar effect on the selection of narrowly- and broadly-defined corresponding majors. As we move across the table to columns 3 and 4, we follow the same approach of expanding the outcome variable, but with the goal of examining substitution more broadly (rather than as a robustness test). In particular, in column 3 we examine whether experiencing fatigue in a course (e.g. Chemistry 101) affects the likelihood that a student chooses a major within the corresponding *field* (e.g. Science, which includes 10 majors in total).[40] Finding a null effect in column 3, for example, would suggest that the effects found in columns 1 and 2 were entirely produced by substitutions within a field (e.g. switching from a counterfactual major of Chemistry to Biology). The estimates in the third column of Table A.7 are imprecisely estimated, but similar in magnitude to those in the first and second column, suggesting that the effects of fatigue are not entirely driven by substitutions within a field. Finally, in column 4 we expand the outcome to a binary classification of STEM vs. Non-STEM majors.[41] While column 3 provides evidence consistent with switching between the four broad fields, the results of column 4, although imprecise, suggest that it is confined within the binary STEM classification. While the STEM vs. non-STEM classification may be important for student earnings trajectories, there is still a large amount of variation between earnings across majors within these two classifications (Webber, 2018). Thus, while we don't observe evidence for students crossing that binary category, it is still quite possible that the major shifts that are observed would have important long-run earnings consequences.

Our findings that fatigue in a course has imprecisely estimated negative effects on majoring within a corresponding field and has no effects on majoring in the corresponding STEM classification may be a function of our institutional setting. During their first three semesters, USMA students have little control over their course schedules and must complete multiple courses from each field of study and STEM classification. As a result, students may be unlikely to generalize their experience in one course to the broader field or STEM classification. In contrast, students at most institutions are not required to take multiple courses within a field prior to selecting a major and can easily change their course schedules. Because of this flexibility, students at other institutions may be more likely to change their field or STEM classification in response to fatigue experienced in a single course.[42]

Across Appendix Tables A.9, A.10, and A.11, we run additional robustness checks to test the sensitivity of our results to the composition of our sample and the way we define the treatments. In Appendix Table A.9 we exclude athletes from our primary specifications, and find this has little effect on our estimates. In Appendix Table A.10, we simultaneously estimate the effects of being assigned to 7:30 AM, 8:40 AM, 11:00 AM, 1:55 PM and 3:05 PM courses (omitting the most commonly assigned course period of 9:40AM) on selecting a corresponding major. We find that while assignment to 7:30 AM courses reduces the probability that students major in a related subject, assignment to 11:00 AM courses *increases* the probability that students major in a course (as does assignment to a 3:05 PM course, though this coefficient loses significance in column 4). Finally, in Appendix Table A.11 we estimate Eq. (2), but replace our linear definition of preceding courses with indicators for one, two, and three preceding courses. We find similar point estimates for one and two preceding courses on major selection (with the one period estimates being measured with more precision), but a much larger negative effect of three preceding courses. Our results, however, are not precise enough to rule out linear effects.

In Appendix Table A.12 we examine the effects of fatigue separately on initial and graduating major choices, in subjects with one or multiple required courses, and in freshman and sophomore courses. In columns 1 and 2 we find that fatigue has indistinguishable effects on initial and graduating majors. These effects may be indistinguishable because students who initially avoid a major because of fatigue are unlikely to take another course in the subject, making it difficult to recognize and correct a mistake. Also, these effects may be indistinguishable because the rigid schedule at USMA makes switching difficult once students begin coursework in their major.[43] In columns 3 and 4 of Appendix Table A.12 we examine whether the effects of fatigue differ by whether a subject has only one required course or multiple required courses. While we cannot statistically distinguish between these effects, the point estimates are three to four times larger in subjects with only one required course relative to subjects with multiple required courses. The differences in these effects could be due to noise in the data, stronger biases due to fatigue when students only experience one course, or differences in the baseline popularity of majors.[44] A comparison of the effects of fatigue in columns 5 and 6 of Appendix Table A.12 shows that the absolute magnitude of point estimates are twice as large in sophomore courses than in freshman courses, but are statistically indistinguishable. Again, these differences could be due to noise, the effects of fatigue being larger in more recent courses, or due to the relative popularity of majors corresponding

[39] Broadly defined major definitions are outlined in Appendix Table A.1

[40] We define four fields: Engineering/Technology/Math (ETM), Science, Social Science, and Humanities. ETM majors (11) include all majors in the Civil & Mechanical Engineering, Electrical Engineering & Computer Science, Math, Systems Engineering, and Nuclear Engineering departments. Science majors (10) include all majors within the Chemistry & Life Science, Geography & Environmental Engineering, and Physics Departments. Social Science majors (8) include all majors within the Social Sciences and Behavioral Science & Leadership Department. Humanities majors (8) include all majors within Law, History, Foreign Language, English, Philosophy & Language, and Military Instruction departments. To construct the mapping, we additionally need to classify the introductory courses into their respective field classification, as follows. ETM courses include: Calculus I, Calculus II, Information Technology, Math Modeling, and Probability and Statistics. Humanities Courses include: Philosophy, all English courses, and all History courses. Science courses include: Chemistry I, Chemistry II, Physical Geography, and Physics. Social Science Courses include American Politics, Economics, and Psychology. We do not map majors to the Kinesiology Major in the department of education but do classify this as a non-stem major.

[41] The STEM classification simply groups the Science and ETM fields together and the non-stem classification groups Humanities and Social Science fields together along with the Kineseology major.

[42] An alternate approach to gauging the effects of fatigue on students' major choices is to examine whether fatigue leads students to move from high to low salary majors or vice versa. To examine this possibility, we match each major to an average salary from the Bureau of Labor Statistics (25–29 year-old's salaries by major) and split required courses into groups with above- and below-median corresponding major salaries. We then estimate whether fatigue affects the average earnings of a student's chosen major in required courses within each split in Appendix Table A.8. While our findings are statistically insignificant, assignment to a course that corresponds with a high-salary major at 7:30 AM (or that is preceded by an additional course) decreases the predicted earnings of the student by approximately $500. On the other hand, assignment to a course with a low salary major at 7:30 AM (or that is preceded by an additional course) increases the expected earnings of that student by $50. These results suggest that fatigue could generate costly mismatch to majors.

[43] USMA have requires students to graduate in four years and students are typically unable to begin coursework in their major until their junior year. Thus students have limited flexibility in changing majors after their major coursework begins.

[44] Majors with one corresponding required course are approximately four times more popular than majors with multiple corresponding courses. Thus the effects, in percentage terms, are indistinguishable among majors with one or multiple corresponding required courses.

to courses in sophomore year versus freshman year.[45] Overall, the estimates in Appendix Table A.12 do not suggest any significant or systematic differences in the effects of fatigue by initial vs. graduating major, single vs. multiple required courses, or freshman vs. sophomore courses.

6. Conclusion

This paper documents attribution bias in the consequential domain of college major choice. We find that USMA students are less likely to enroll in a major if they are assigned to an early morning section of the corresponding introductory (general education) course. This result is consistent with students misattributing the negative effects of their temporary fatigue in the class to some fixed attribute of the overall subject. While the early morning course assignment does generate slightly diminished performance in the class itself, we show the effects on major choice are difficult to explain by a rational expectation of diminished performance in the corresponding major. Moreover, we show that this type of attribution bias also holds for a second, but related type of fatigue generated by course timings (back-to-back courses).

While a growing literature in behavioral economics has documented the influence of projection bias in consequential policy domains (e.g. health insurance purchase in Chang et al. (2018)), there remains a paucity of similar work on the type of attribution bias discussed in Haggag et al. (2019). As noted in the Introduction, the results shown in this paper have immediate policy implications for school administrators. However, by taking a broader perspective on the model (e.g. relaxing a strict interpretation of state-dependence and consumption), we see a number of potential policy applications, including some discussed in related literatures. For example, several studies have documented that politicians are rewarded and punished for luck (e.g. natural disasters), possibly reflecting a misattribution about their own skill (Wolfers et al., 2002; Cole et al., 2012). Misattributions may similarly be made about policies themselves. For example, a policy aimed at reducing tobacco consumption may be deemed ineffective if it happens to be implemented at the same time as a tobacco price shock, as the public may misattribute the effects of the transient shock to the policy itself. While both of these examples reflect complex processes in which the misattribution may be more akin to omitted variable bias, there are others where the error is closer to under appreciating state-dependence. For example, recipients of government programs (e.g. disability benefits) are often required to schedule an initial application appointment. An individual assigned to a difficult appointment time (e.g. when the office is crowded or when the workers and/or the recipient are tired) may misattribute that initial unpleasant experience to the program itself. Even if the recipient recognizes that they won't have to repeatedly come back to that office, their initial unpleasant experience may be difficult to disentangle, and the misattribution may discourage them from participating in that program or others like it in the future.

Attribution bias may have important aggregate consequences if states are correlated across individuals. Consider a policy that is only valued when pollution levels are high. If that policy is introduced in a period with abnormally low pollution levels, then it may be undervalued and that mistaken inference may persistently undermine political will for the policy in the future.

How might one remediate or avoid attribution bias? Returning to the most immediate applications of this paper, an institution that would like to increase the number of majors in a particular subject may be able to do so at little or no cost by ensuring that the corresponding introductory course is offered after a break and outside of the early morning time-slot. While it may seem that such policies are zero-sum in the sense that one department's gain in majors will be another department's loss, this may not be the case. Given that many students in the US are on the margin of dropping out of college, it is possible that institutions could increase retention by minimizing fatigue across all introductory classes while students form their first impressions of fields and college as a whole. Beyond scheduling, institutions may wish to shift other resources to their introductory courses (e.g. higher quality instructors), as this initial experience may have a large influence on major choice. Stepping back outside of education, the results in this paper suggest that policy makers should pay close attention to the relevant underlying states in which a policy is introduced. Seemingly inconsequential details at the outset of a program (e.g. the ability of a benefits website to handle a web traffic shock) could undermine future support for it if sticky misattributions are made about the program itself. If it's not possible to control the underlying states of individuals' initial exposure to a new program, it may be worth incentivizing those individuals to re-sample the program under an alternative state. Due to the simultaneity of states of the world and experiences, the influence of attribution bias on decision making could be widespread.

[45] Majors corresponding to sophomore courses are approximately twice as popular as majors corresponding to freshman courses, making the effect, in percentage terms, indistinguishable between freshman and sophomore year.

Appendix A

See Fig. 1 and Tables A.1–A.15.

TYPICAL ACADEMIC PROGRAM

FRESHMAN YEAR		SOPHOMORE YEAR	
Term 1	**Term 2**	**Term 1**	**Term 2**
MA 103 - 4.0 Math Modeling/Intro to Calculus	**MA104 - 4.5** Calculus I	**MA205 - 4.5** Calculus II	**MA206 - 3.0** Prob & Stats
CH101 - 3.5 Chemistry I	**CH102 - 3.5** Chemistry II	**PH201 - 3.5** Physics I	**PH202 - 3.5** Physics II
EN101 - 3.0 English Composition	**EN102 - 3.0** Literature	**Lx203 - 3.5** Foreign Language	**Lx204 - 3.5** Foreign Language
HI10_ - 3.0 History	**HI10_ - 3.0** History	**SS201 - 3.5** Economics	**SS202 - 3.5** Political Science
PL100 - 3.0 General Psychology	**IT105 - 3.0** Intro to Computing and Information Technology	**PY201 - 3.0** Philosophy	**EV203 - 3.0** Physical Geography

Fig. 1. Typical USMA Schedule in First Two Years. Courses highlighted in yellow may be assigned to students in either first or second semester of the respective year. Students must complete or test out of all courses listed. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Table A.1
Mapping Between Required Courses and Majors.

Course	Year	Majors (Narrow)	Fraction	Majors (Broad)	Fraction
Chemistry I	1	Chemistry	0.008	Chemistry; Chemical Engineering	0.019
Chemistry II	1	Chemistry	0.009	Chemistry; Chemical Engineering	0.020
English Composition	1	English	0.003	English; Art, Philosophy, and Literature	0.018
English Literature	1	English	0.003	English; Art, Philosophy, and Literature	0.020
US History	1	US History	0.017	US History; International History; Military History; European History	0.044
World History	1	International History	0.023	US History; International History; Military History; European History	0.052
Western Civilization	1	European History History	0.004	US History; International History; Military History; European History	0.053
Math Modeling	1	Mathematical Sciences	0.008	Mathematical Sciences	0.008
Calculus I	1	Mathematical Sciences	0.008	Mathematical Sciences	0.008
General Psychology	1	Psychology	0.024	Psychology; Engineering Psychology	0.042
Computing and Information Technology	1	Computer Science; Information Technology	0.041	Computer Science; Information Technology	0.041
Calculus II	2	Mathematical Sciences	0.008	Mathematical Sciences	0.008
Probability and Statistics	2	Mathematical Sciences	0.031	Mathematical Sciences	0.031
Physics	2	Physics	0.014	Physics; Physics Engineering; Interdisciplinary Physics	0.015
Economics	2	Economics	0.076	Economics	0.076
American Politics	2	Political Science	0.030	International Relations; Political Science	0.057
Philosophy and Ethics	2	Philosophy	0.002	Philosophy; Art, Philosophy, and Literature	0.021
Physical Geography	2	Geography	0.075	Geography	0.075

Because majors are selected during the third semester, only a subsample of students take the listed second year courses. History courses are not offered at 7:30 AM. As a result, history courses only contribute to the analysis variation in the preceding courses specifications. The Fraction column refers to the fraction of students in the course who eventually major in the corresponding major(s).

Table A.2
Early Morning Courses and Selection of Major in Subject Area.

	(1)	(2)	(3)	(4)	(5)	(6)
7:30 AM Course	−0.0018**	−0.0022**	−0.0019**	−0.0020**	−0.0020**	−0.0020**
	(0.0008)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
N	233,452	233,452	233,452	233,452	233,452	233,452
R^2	0.0269	0.0269	0.0649	0.0766	0.0772	0.0772
Dependent Variable Mean	0.0190	0.0190	0.0190	0.0190	0.0190	0.0190
General Schedule FE	N	Y	Y	Y	Y	Y
Course Roster FE	N	N	Y	Y	Y	Y
Faculty FE	N	N	N	Y	Y	Y
Demographic Controls	N	N	N	N	Y	Y
Peer Demographic Controls	N	N	N	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,372 total clusters). All specifications include year and course fixed effects. General schedule fixed effects include an indicator for the number of courses a student has in a day and an indicator for the number of immediately preceding courses. The course roster fixed effect is a fixed effect for the particular combination of courses a student takes in a given semester (e.g. Calculus I, Economics, US History, and Physics). Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, athlete status, and overall pre-attendance student ranking. Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 233,452, 233,452, 231,458, 231,457, 231,457, and 231,457 in columns 1–5, respectively.

Table A.3
Immediately Preceding Courses and Selection of Major in Subject Area.

	(1)	(2)	(3)	(4)	(5)
Number of Preceding Courses	−0.0006	−0.0019***	−0.0015**	−0.0023***	−0.0023***
	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0008)
N	233,452	233,452	233,452	233,452	233,452
R^2	0.0269	0.0270	0.0650	0.1357	0.1363
Dependent Variable Mean	0.0190	0.0190	0.0190	0.0190	0.0190
General Schedule FE	N	Y	Y	Y	Y
Course Roster FE	N	N	Y	Y	Y
Unique Course FE	N	N	N	Y	Y
Demographic Controls	N	N	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,372 total clusters). All specifications include year and course fixed effects. General schedule fixed effects include an indicator for the scheduled course time (e.g. 7:30 AM, 8:40 AM,…,3:05 PM) and the number of courses a student has that day. The course roster fixed effect is a fixed effect for the particular combination of courses a student takes in a given semester (e.g. Calculus I, Economics, US History, and Physics). Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, athlete status, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 233,452, 233,402, 231,413, 231,398, and 231,398 in columns 1–5, respectively.

Table A.4
Fatigue and Selection of Major in Subject Area, Logit Regressions.

		Panel A: Time of Day	
	(1)	(2)	(3)
7:30 AM Course	−0.1122**	−0.1115**	−0.1148**
	(0.0495)	(0.0521)	(0.0522)
N	233,452	233,452	233,452
Dependent Variable Mean	0.0190	0.0190	0.0190
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
		Panel B: Number of Immediately Preceding Courses	
	(1)	(2)	(3)
Number of Preceding Courses	−0.1116***	−0.1200***	−0.1203***
	(0.0338)	(0.0368)	(0.0369)
	(0.0495)	(0.0521)	(0.0522)
N	233,452	233,452	233,452
Dependent Variable Mean	0.0190	0.0190	0.0190
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors clustered by student in parentheses. All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Our reported sample size includes fixed effects that do not contain variation in our outcome and thus do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 187,571, 136,921, and 136,921 in columns 1–3 of Panel A and 187,571, 40,462, and 40,462 in columns 1–3 of Panel B.

Table A.5
Patterns in Course Schedules, Core Courses.

		Number of Preceding Courses				
Period	Time	0	1	2	3	Total
1	7:30–8:25 AM	43,653	–	–	–	43,653
2	8:40–9:35 AM	18,861	11,510	–	–	30,371
3	9:50–10:45 AM	36,423	9,306	6,352	–	52,081
4	11:00–11:15 AM	20,197	15,268	3,173	1,177	35,630
5	1:55–2:50 PM	35,630	–	–	–	35,630
6	3:05–4:00 PM	24,863	6,989	–	–	31,852
Totals		179,677	43,073	9,525	1,177	233,452

Observations at the student-course level. Observations are from students in first and second year core courses. First year core courses include: mathematical modeling and introduction to calculus, calculus 1, introduction to computing, psychology, history of the United States, western civilization, world history, composition, literature, and a student success course. Second year core courses include: calculus 2, probability and statistics, economics, physics 1 and 2, philosophy, geography, and American politics.

Table A.6
Fatigue and Selection of Major in Subject Area, Selective vs. Non-Selective Majors.

	Panel A: Time of Day		
	(1)	(2)	(3)
7:30 AM Course	−0.0059**	−0.0045*	−0.0045*
	(0.0025)	(0.0024)	(0.0024)
7:30 AM*Selective Major	0.0049*	0.0032	0.0032
	(0.0026)	(0.0025)	(0.0025)
N	233,452	233,452	233,452
R^2	0.0653	0.0770	0.0773
Dependent Variable Mean	0.0190	0.0190	0.0190
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
	Panel B: Number of Immediately Preceding Courses		
	(1)	(2)	(3)
Number of Preceding Courses	−0.0023**	−0.0038***	−0.0037***
	(0.0011)	(0.0014)	(0.0014)
Preceding Courses*Selective Major	0.0013	0.0024	0.0024
	(0.0013)	(0.0016)	(0.0016)
N	233,452	233,452	233,452
R^2	0.0654	0.1361	0.1363
Dependent Variable Mean	0.0190	0.0190	0.0190
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,370 total clusters). All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A also include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 231,458, 231,457, and 231,457 in columns 1–3 of Panel A and 231,413, 231,398, and 231,398 in columns 1–3 of Panel B.

Table A.7
Fatigue and Selection of Major in Narrow Subject, Broad Subject, Field, and STEM Match.

	Panel A: Time of Day			
	Narrow	Broad	Field	STEM
7:30 AM Course	−0.0020**	−0.0024**	−0.0026	0.0004
	(0.0009)	(0.0010)	(0.0031)	(0.0029)
N	233,452	233,452	233,452	233,452
R^2	0.0772	0.0665	0.0709	0.0541
Dependent Variable Mean	0.0190	0.0306	0.2586	0.5025
	Panel B: Number of Immediately Preceding Courses			
	Narrow	Broad	Field	STEM
Number of Preceding Courses	−0.0023***	−0.0020**	−0.0034	−0.0017
	(0.0008)	(0.0009)	(0.0023)	(0.0027)

(*continued on next page*)

Table A.7 (*continued*)

Panel B: Number of Immediately Preceding Courses				
	Narrow	Broad	Field	STEM
N	233,452	233,452	233,452	233,452
R^2	0.1363	0.1288	0.1376	0.1221
Dependent Variable Mean	0.0190	0.0306	0.2586	0.5025

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,370 total clusters). Narrow and broad majors definitions are outlined in Appendix Table A.1. Fields include Engineering/Technology/Math (ETM), Humanities, Science, and Social Science. ETM majors include all majors within Civil & Mechanical Engineering, Electrical Engineering & Computer Science, Math, Systems engineering, and Nuclear Engineering departments. Humanities majors include all majors within Law, History, Foreign Language, English, Philosophy & Languge, and Military Instruction departments. Science majors include all majors within the Chemistry & Life Science, Geography & Environmental Engineering, and Physics Departments. Social Science Majors include all majors within the Social Sciences and Behavioral Science & Leadership Departments. ETM courses include: Calculus I, Calculus II, Information Technology, Math Modeling, and Probability and Statistics. Humanities Courses include: Philosophy, all English courses, and all History courses. Science courses include: Chemistry I, Chemistry II, Physical Geography, and Physics. Social Science Courses include American Politics, Economics, and Psychology. STEM classification groups Science and ETM Fields and non-stem classification groups Humanities and Social Science Fields along with a Kinesiology major. All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, fixed effects for the number of courses in a day, and demographic controls. All columns in Panel A also include indicators for whether courses were immediately preceded by other courses, faculty fixed effects, and peer demographic controls. All columns in Panel B include unique course fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample size is 231,457 in columns 1–4 of Panel A and and 231,398 in columns 1–4 of Panel B.

Table A.8
Fatigue and The Earnings of Major.

Panel A: Time of Day, Above Median Salary Majors			
	(1)	(2)	(3)
7:30 AM Course	−488.8	−500.0	−564.8
	(369.4)	(369.5)	(364.3)
N	50,604	50,604	50,604
R^2	0.1059	0.1146	0.1378
Panel B: Time of Day, Below Median Salary Majors			
7:30 AM Course	4.8	53.1	65.4
	(123.0)	(119.6)	(116.6)
N	161,979	161,979	161,979
R^2	0.1465	0.1521	0.1748
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
Panel C: Preceding Courses, Above Median Salary Majors			
Number of Preceding Courses	−396.6	−575.0*	−548.1
	(297.8)	(344.0)	(340.8)
N	50,604	50,604	50,604
R^2	0.1058	0.1750	0.1969
Panel D: Preceding Courses, Below Median Salary Majors			
Number of Preceding Courses	37.8	42.4	70.4
	(121.4)	(140.6)	(138.4)
N	161,979	161,979	161,979
R^2	0.1462	0.2134	0.2343
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses. Earnings correspond to BLS reports for earnings by major for individuals 25–29. Courses with above median-salary majors include American Politics, Economics, Information Technology, and Physics. Courses with below median-salary majors include courses in Chemistry, English, Geography, History, Mathematics, Philosophy, and Psychology. One standard deviation of Major earnings in sample is $22,854. All columns in Panel A, B, C and D include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A and C include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B and D additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course).

Table A.9
Fatigue and Selection of Major in Subject Area, Excluding Athletes.

	(1)	(2)	(3)
Panel A: Time of Day			
7:30 AM Course	−0.0017	−0.0020**	−0.0020*
	(0.0011)	(0.0010)	(0.0011)
N	155,035	155,035	155,035
R^2	0.0719	0.0873	0.0875
Dependent Variable Mean	0.0201	0.0201	0.0201
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
Panel B: Number of Immediately Preceding Courses			
Number of Preceding Courses	−0.0008	−0.0017*	−0.0017*
	(0.0008)	(0.0010)	(0.0010)
N	155,035	155,035	155,035
R^2	0.0721	0.1711	0.1713
Dependent Variable Mean	0.0201	0.0201	0.0201
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (12,529 total clusters). All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A also include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 153,533, 153,528, and 153,528 in columns 1–3 of Panel A and 153,501, 153,410, and 153,410 in columns 1–3 of Panel B.

Table A.10
Assignment to Early Morning Classes and Selection of Major in Subject Area.

	(1)	(2)	(3)	(4)
7:30 AM Course	−0.0029**	−0.0023**	−0.0023**	−0.0022**
	(0.0012)	(0.0011)	(0.0011)	(0.0011)
8:40 AM Course	−0.0011	−0.0004	−0.0004	−0.0005
	(0.0014)	(0.0014)	(0.0014)	(0.0014)
11:00 AM Course	0.0023	0.0024*	0.0024*	0.0024*
	(0.0015)	(0.0014)	(0.0014)	(0.0014)
1:55 PM Course	−0.0001	0.0003	0.0002	0.0004
	(0.0020)	(0.0020)	(0.0020)	(0.0020)
3:05 PM Course	0.0040*	0.0046**	0.0045**	0.0031
	(0.0021)	(0.0021)	(0.0021)	(0.0022)
N	233,452	233,452	233,452	233,452
R^2	0.0535	0.0657	0.0665	0.0665
Dependent Variable Mean	0.0190	0.0190	0.0190	0.0190
Faculty FE	N	Y	Y	Y
Demographic Controls	N	N	Y	Y
Peer Demographic Controls	N	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.Robust standard errors are two-way clustered by unique course and student in parentheses (15,370 total clusters). Omitted hour is 9:50 AM. All columns include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, fixed effects for the number of courses in a day, and indicators for whether courses were immediately preceded by other courses. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking.

Table A.11
Number of Preceding Courses and Selection of Major in Subject Area.

	(1)	(2)	(3)
One Preceding Course	−0.0014	−0.0026**	−0.0026**
	(0.0009)	(0.0011)	(0.0011)
Two Preceding Courses	−0.0025	−0.0031	−0.0031
	(0.0017)	(0.0019)	(0.0019)
Three Preceding Courses	−0.0083*	−0.0116**	−0.0115**
	(0.0049)	(0.0058)	(0.0058)
N	233,452	233,452	233,452
R^2	0.0654	0.1361	0.1363

(*continued on next page*)

Table A.11 (*continued*)

	(1)	(2)	(3)
Dependent Variable Mean	0.0190	0.0190	0.0190
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,364 total clusters). Each column includes year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. Additionally, column 1 includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course).

Table A.12
Dynamics of Fatigue and Major Choice.

	Panel A: Time of Day					
	Decision Type		Courses in Subject		Class Year	
	Initial (1)	Graduating (2)	One (3)	Multiple (4)	Freshman (5)	Sophomore (6)
7:30 AM Course	−0.0021**	−0.0020**	−0.0031*	−0.0009	−0.0014	−0.0035*
	(0.0009)	(0.0009)	(0.0017)	(0.0008)	(0.0010)	(0.0018)
N	233,452	233,452	116,450	117,002	166,514	64,788
R^2	0.0798	0.0772	0.0953	0.0574	0.0627	0.1000
Dependent Variable Mean	0.0194	0.0190	0.0307	0.0074	0.0149	0.0300
	Panel B: Number of Immediately Preceding Courses					
	Decision Type		Courses in Subject		Class Year	
	Initial (1)	Graduating (2)	One (3)	Multiple (4)	Freshman (5)	Sophomore (6)
Number of Preceding Courses	−0.0024***	−0.0023***	−0.0032**	−0.0008	−0.0015*	−0.0036**
	(0.0008)	(0.0008)	(0.0014)	(0.0007)	(0.0009)	(0.0014)
N	233,452	233,452	116,450	117,002	166,514	64,788
R^2	0.1379	0.1363	0.1525	0.1202	0.1279	0.1626
Dependent Variable Mean	0.0194	0.0190	0.0307	0.0074	0.0149	0.0300

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses. All columns in Panel A and B include demographic characteristics, year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A include peer demographic controls and indicators for whether courses were immediately preceded by other courses. All estimates in panel B include unique course fixed effects.. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course).

Table A.13
Fatigue and Selection of Major in Subject Area, controlling for Grade Fixed Effects.

	Panel A: 7:30 AM Courses		
	(1)	(2)	(3)
7:30 AM Course	−0.0011	−0.0011	−0.0010
	(0.0009)	(0.0009)	(0.0009)
A+	0.0626***	0.0635***	0.0657***
	(0.0038)	(0.0038)	(0.0038)
A	0.0307***	0.0309***	0.0328***
	(0.0019)	(0.0019)	(0.0019)
A-	0.0155***	0.0158***	0.0171***
	(0.0013)	(0.0013)	(0.0013)
B+	0.0054***	0.0055***	0.0062***
	(0.0011)	(0.0011)	(0.0011)
B-	−0.0059***	−0.0058***	−0.0065***
	(0.0009)	(0.0009)	(0.0009)
C+	−0.0089***	−0.0087***	−0.0102***
	(0.0009)	(0.0009)	(0.0009)
C	−0.0109***	−0.0108***	−0.0129***
	(0.0009)	(0.0009)	(0.0010)
C-	−0.0138***	−0.0136***	−0.0163***
	(0.0011)	(0.0011)	(0.0011)
D	−0.0165***	−0.0157***	−0.0188***
	(0.0014)	(0.0014)	(0.0014)
F	−0.0127	−0.0094	−0.0128
	(0.0101)	(0.0102)	(0.0102)

Table A.13 (*continued*)

	Panel A: 7:30 AM Courses		
	(1)	(2)	(3)
N	233,452	233,452	233,452
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
	Panel B: Number of Preceding Courses		
	(1)	(2)	(3)
Number of Preceding Courses	−0.0014**	−0.0021***	−0.0021***
	(0.0007)	(0.0008)	(0.0008)
A+	0.0627***	0.0668***	0.0693***
	(0.0038)	(0.0039)	(0.0039)
A	0.0308***	0.0335***	0.0354***
	(0.0019)	(0.0019)	(0.0019)
A-	0.0155***	0.0173***	0.0185***
	(0.0013)	(0.0014)	(0.0014)
B+	0.0053***	0.0058***	0.0065***
	(0.0011)	(0.0011)	(0.0011)
B-	−0.0059***	−0.0057***	−0.0064***
	(0.0009)	(0.0009)	(0.0009)
C+	−0.0089***	−0.0092***	−0.0107***
	(0.0009)	(0.0009)	(0.0009)
C	−0.0108***	−0.0115***	−0.0137***
	(0.0009)	(0.0010)	(0.0010)
C-	−0.0137***	−0.0142***	−0.0169***
	(0.0011)	(0.0012)	(0.0012)
D	−0.0165***	−0.0171***	−0.0201***
	(0.0014)	(0.0015)	(0.0015)
F	−0.0127	−0.0152	−0.0183*
	(0.0101)	(0.0100)	(0.0099)
N	233,452	233,452	233,452
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

$p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.14
Fatigue and Selection of Major in Subject Area, split by magnitude of grade effect.

	Panel A: 7:30 AM Courses		
	(1)	(2)	(3)
	b/se	b/se	b/se
7:30 AM Course	−0.0023**	−0.0022**	−0.0022**
	(0.0011)	(0.0011)	(0.0011)
7:30 AM*Negative Grade Effect	0.0014	0.0010	0.0009
	(0.0017)	(0.0016)	(0.0016)
N	231,592	231,571	231,450
R^2	0.0649	0.0766	0.0768
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y
	Panel B: Number of Preceding Courses		
	(1)	(2)	(3)
	b/se	b/se	b/se
Number of Preceding Courses	−0.0006	−0.0007	−0.0008
	(0.0007)	(0.0008)	(0.0008)
Preceding Courses*Negative Grade Effect	−0.0020*	−0.0034**	−0.0034**
	(0.0012)	(0.0016)	(0.0016)
N	231,548	231,394	231,394
R^2	0.0651	0.1357	0.1359
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

$p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.15
Fatigue and Selection of Major in Subject Area, Initial Choice.

	Panel A: Time of Day		
	(1)	(2)	(3)
7:30 AM Course	−0.0020**	−0.0019**	−0.0019**
	(0.0009)	(0.0008)	(0.0008)
N	233,452	233,452	233,452
R^2	0.0658	0.0790	0.0792
Dependent Variable Mean	0.0190	0.0190	0.0190
Faculty FE	N	Y	Y
Demographic Controls	N	N	Y
Peer Demographic Controls	N	N	Y

	Panel B: Number of Immediately Preceding Courses		
	(1)	(2)	(3)
Number of Preceding Courses	−0.0020**	−0.0019**	−0.0019**
	(0.0009)	(0.0008)	(0.0008)
N	233,452	233,452	233,452
R^2	0.0659	0.1374	0.1375
Dependent Variable Mean	0.0190	0.0190	0.0190
Unique Course FE	N	Y	Y
Demographic Controls	N	N	Y

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are two-way clustered by unique course and student in parentheses (15,370 total clusters). All columns in Panel A and B include year fixed effects, course roster fixed effects, sport-by-semester fixed effects, and fixed effects for the number of courses in a day. All columns in Panel A include indicators for whether courses were immediately preceded by other courses. Column 1 of Panel B additionally includes course time fixed effects. Demographic variables include: indicators for sex, race/ethnicity, prior military service, prior college attendance, attendance at preparatory academy, and SAT verbal scores, SAT math scores, and overall pre-attendance student ranking. Unique courses are individual classroom observations (i.e. the interaction between instructor, semester, section, and course). Our reported sample size includes singleton observations that do not contribute to our identifying variation. If we omit these observations, our effective sample sizes are 231,458, 231,457, and 231,457 in columns 1–3 of Panel A and 231,413, 231,398, and 231,398 in columns 1–3 of Panel B.

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