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The Association of Participating in a Summer Prelaw Training Program and First-Year Law School Students' Grades

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Abstract

This study estimates the association of participation in a nine-week online educational program to prepare students for post-graduate (juris doctorate) education and law school grades. We collected registrar data from 17 U.S. law schools for participants and non-participants from the same year and a prior year. We compared first-semester law school grades between participating and non-participating students weighted by propensity scores. Course participation was associated with improved first-semester grades in a keyed course (Contracts Law) and overall grade point average. According to pre- and post-survey responses, a substantial portion of those who completed the program reported feeling more prepared for law school.

Keywords: *post-graduate education, bridge programs, readiness, law school preparation*
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We have no known conflict of interest to disclose. Buzick and Klieger were independent of the team that developed and implemented the course in 2019 and 2020. The project was funded by the Access Lex Institute in the 2020 cohort (reported here), and the prior 2019 cohort as well, supporting the work of the other authors. ETS, the employer of Buzick and Klieger, has provided financial support for the course in the 2021 and 2022 cohorts (not reported on here).

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Introduction

In the United States, the three-year juris doctor (JD) degree is the professional education required for lawyers. The United States has 199 law schools accredited by the American Bar Association (ABA) and dozens more law schools not accredited by the ABA. Survey research suggests that fewer first-generation students even consider law school as an option (Association of American Law Schools & Gallup, 2018). Accordingly, some have argued that the "educational pipeline" to law school is "broken" (Redfield, 2010). This failure may be one cause of persistent disparities in the population of American attorneys and a significant crisis in access to justice (ABA, 2022; Guinier, 2015; Taylor, 2019; Young, 2020).

Once enrolled in a law school, some students struggle academically. Most graduate programs and many professional degree programs, such as medicine, typically presume that students have had foundational education at the undergraduate level to prepare them for the specific demands of their graduate or professional studies. In American law schools, however, no particular undergraduate curriculum is universally recommended, much less required. Accordingly, students come to JD programs with widely varying preparation. In qualitative studies, first-year law students have reported that they "come into law school with no expectations or understanding about how law works, [and] these responses came disproportionately from students who were the first in their families to attend law school" (Young, 2020, p. 2582). Law schools increasingly offer "academic support" programs to help raise the capability of students to succeed in law school, but as yet there is no gold-standard approach for doing so (Espinoza, 1989; McClain, 2018; Wangerin, 1988). Moreover, it may be too late to try to support students who are already matriculants since they are already struggling in their classes and competing in a zero-sum effort with their peers on high-stakes curved course assessments, which can drive early relationships with professors and shape career outcomes (Calsamiglia & Loviglio, 2019; Christensen, 2009a).

Some law schools, non-profit organizations, and private companies have developed "bridge" programs to help prepare aspiring students for law school, broaden the applicant pool, strengthen the qualifications of law school students, and expand the legal profession population (AccessLex, 2020; Manzi, 2020). Prior work identified 33 recent programs, which vary widely in design, duration, goals, and price (Cheng et al., 2023). As part of broader efforts to increase diversity in the law profession, some programs have been designed to improve knowledge and skills of students from underrepresented backgrounds. Examples include the Council on Legal Educational Opportunity (CLEO) summer program (Hampden, 1989; Schwartz, 2014) and the American Indian Law Center's Pre-Law Summer Institute (Estes & Laurence, 1992), as well as school-specific programs (Lundwall, 1994). More recent examples include the Prelaw Undergraduate Scholars (PLUS) programs offered by the Law School Admissions Council (2021). Many pipeline programs have come and gone over the past decades; however, few rigorous research studies have been developed and published to provide evidence as to whether the programs have met the goals of increased enrollment, improved law school outcomes, and diversity in the legal profession.

Despite limited evidence on outcomes from prelaw school training opportunities, there is evidence to suggest that preprofessional education training has helped students succeed in other disciplines. In particular, prelaw bridge programs can be seen as analogous to the more than 280 postbaccalaureate programs that prepare students for medical school (American Association of Medical Colleges, 2022). Researchers have found evidence that these latter programs have been associated with increased enrollment and medical school success and may be especially helpful for students from underrepresented backgrounds. (Giordani et al., 2001; Grumbach & Chen, 2006; Limpscomb et al., 2009; McDougle et al., 2015).

JD-Next Program

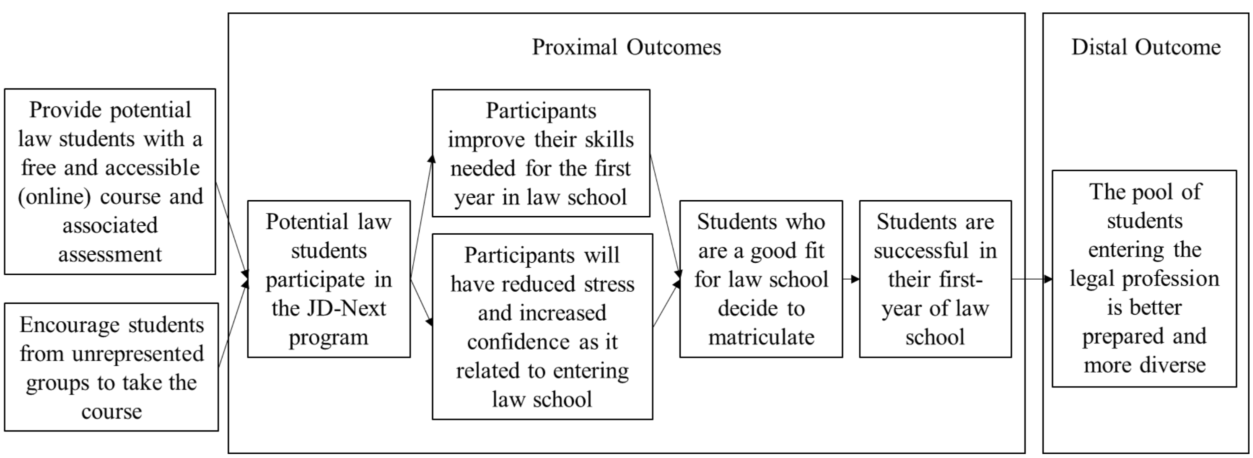
In 2019 a team of law professors and educational psychologists at the University of Arizona developed the precursor to JD-Next, offering prospective JD students prelaw education training and seeking to measure their knowledge and skills on topics covered in the course material with a standardized assessment. There is evidence that JD-Next assessment scores predict aspects of students' success in law school (Cheng et al., 2023; Findley et al., 2023). The content of the course was developed by adapting part of an existing undergraduate law course, which introduces some foundational principles of contract law and, more importantly, the general legal skills of case briefing and legal reasoning, which would translate to all of law school (Christensen, 2009b; Graham, 2014). The course was designed to be delivered in a fully online, asynchronous format, with a recommended weekly pace. Nonetheless, developers also attempted to maintain instructor presence, with video announcements, office hours, and written feedback on exemplar quizzes, to engage students and promote learning (Lawton et al., 2012; Richardson & Lowenthal, 2017). The video lectures were designed to be short, typically less than ten minutes, to maintain attention (Guo et al., 2014). The course includes frequent formative assessments to provide feedback to promote learning and test preparation skills to improve future test performance (Sargent & Curcio, 2012). See Findley et al. (2021) for more details about the course.

Purpose of the Study and Research Questions

The current study is part of a more extensive research and development effort to create, evaluate, and refine a summer bridge program and associated assessment for supporting and preparing aspiring law school students. In designing the current study, the aim was to inform the iterative development process while planning within the constraints of budget and participant burden. We built off work by Cheng et al. (2023), who estimated the effects of a precursor to JD-Next on grades in a smaller initial study.

This research is guided by the following theory of change, shown in Figure 1. The target population is students who have been accepted to or waitlisted for law school, and the intervention is the JD-Next program. The theory of change illustrates that providing these aspiring law students with a free and accessible (online) course and associated assessment, as well as encouraging students from unrepresented groups to participate in the program, will increase the pool of successful law school students and eventually strengthen and diversify the population entering the law profession.

Figure 1. *Theory of Change for JD-Next*



We designed the study to collect evidence of the promise of the JD-Next program in improving grades as a measure of success for students enrolled in their first year of law school. The secondary purposes were to collect and analyze data to explore whether the program is helpful, in particular to students from underrepresented groups, and to explore the relationship between the amount of participation in the program and first-year law school grades. Key assumptions in the theory of change that were not a part of the research design are that exposure to the methods and topics of legal education helps students decide if they are a good match to matriculate into law school and that midterm and final exam scores are useful signals to students of their potential first-year law school grades. The former could be explored as the program matures by estimating the effects of the JD-Next program participation on participants' decision to enroll and enrollment levels, especially if the course is offered to students earlier, rather than in the summer immediately before starting classes as JD students. We could not explore the latter assumption because the final exam scores of participants in the research setting had not been shared with them, given that studies on validity and reliability are ongoing (Findley et al., 2021).

We designed the study to quantify the effect of participating in the JD-Next program on law school grades for enrolled first-year students, using data from an expanded pilot of the program conducted in 2020, after an initial cohort in 2019. Our research questions were:

1. Is the JD-Next program effective at improving students' contracts class grades, writing class grades, and fall grade point average (1L GPA)? (confirmatory)
2. Does program effectiveness differ in relation to students' race/ethnicity and gender? (exploratory)
3. How does completing some versus all JD-Next modules relate to law school grades (is there a "dose response")? (exploratory)
4. Is the JD-next program also effective at improving students' self-reported sense of preparation for law school? (exploratory)

We also collected information from participants to contextualize their experiences with the program. We report on program participation and law school matriculation among those who participated in the program and also participants' perceptions about how prepared they were, both before participating in the program and after, in various knowledge, skills, and personal development topics related to the JD-Next program.

Method

The study design is quasi-experimental. In the summer of 2020, before enrolling in law school, individuals were given an opportunity to self-select into the treatment group by signing up for and engaging with the JD-Next program. In designing the study, we planned to collect data from the control and treatment groups on as many factors as possible that were likely to affect both participation and outcomes, so that our analysis approach could isolate the treatment effect.

This study evaluates the second year (2020) in which the JD-Next program was offered, but the first year in which all law schools were invited to participate. The invitations were sent by the dean of one law school to all other law school deans, via an association Listserv. The program was free of charge to both schools and their students, who were given incentives to participate and perform well on a final exam. We offered weekly tangible incentives throughout the course to encourage students to stay on track, which included law school t-shirts, an iPad, Yeti mugs, a Kindle e-reader, and gift cards (Starbucks, Amazon, Target) among others. Students who were on track at the time of the drawing received an entry into the drawing. For taking the final exam, students also were entered into a drawing to win $300 per school cohort. The Institutional Review Board at the University of Arizona determined the study to be exempt.

Each participating law school signed a memorandum of understanding and then sent several emails to their admitted students in the summer before enrolling at the law school. Some schools also invited their waitlisted students to participate. Prospective students were then directed to a webpage, where they would give informed consent as the first step towards enrollment in the course, which was then delivered on a learning management system ("D2L," aka Brightspace).

Seventeen schools agreed to participate in the study. Refer to the appendix for statistics from the American Bar Association accreditor on the first-year classes from each school's 2018 and 2020 cohorts. At the recruitment stage, the participating schools agreed to provide us with administrative data on enrolled first-year students and, after students progressed through law school, additional student data. We requested data for the entire 2018 and 2020 cohorts of first-year students, including both participants and non-participants in JD-Next. For JD-Next participants, we also collected information from the learning management system on participation in the program, including the percentage of assignments completed and total time spent with the content, midterm and final exam scores, and pre- and postsurvey questions.

Among the 17 schools, 1,191 students registered for the JD-Next program in summer 2020. For students who enrolled in fall 2020, the share of JD-Next registrants was 43%. The registration rates within race/ethnicity, for groups large enough to report, were 55% for Asians or Pacific Islanders, 34% for Blacks or African Americans, 42% for Hispanics or Latinos, and 43% for Whites. Among students who registered for JD-Next in the summer, 371 (31%) did not complete any assignments in the online program (cf., Reich & Ruiperez-Valiente, 2019). Among those who engaged with the JD-Next program content, 76 (9%) did not enroll in the fall.

Variables

We have three primary outcome variables: contracts course grades, writing course grades, and first-year law school GPA (LGPA). For the course grades, we converted letter grades to numeric scales using conversions published on each school's website. Two schools awarded points higher than 4.0 for A+ grades. We recoded these to be equal to 4.0. One school used a nontraditional scale for LGPA that ranged from 0–9. We used linear transformations to convert to the 4.0 scale by converting points to z-scores within year, then multiplying by the standard deviation of LGPA from the other schools and adding the mean LGPA from the other schools. We checked the sensitivity of this decision by conducting our analyses with and without the school with the nontraditional scale, and the results were similar.

We asked law schools to provide us with student background variables that could influence both the outcome variables and the probability that a student would have participated in JD-Next. The schools provided us with individual student data for five independent variables from the data they report to the American Bar Association (ABA, n.d.-a). The variables are gender, race/ethnicity, age, admissions test scores (LSAT or GRE), and undergraduate grade point average (UGPA). Gender was a multicategory variable that we recoded to equal 1 if female, 0 if male, and NA otherwise. There were 11 observations that we coded as NA for gender; among them, one student identified as non-binary, seven students preferred not to answer, and two were missing data. The categories for race/ethnicity were Asian or Pacific Islander, Black or African American, Hispanic or Latino, multiple, Native American or Alaskan, Native Hawaiian or other Pacific Islander, White, and race/ethnicity unknown. Participants had the option to identify themselves using an additional ABA reporting category, nonresident alien. For our exploratory analyses by race/ethnicity, we created an indicator variable representing students from underrepresented groups that was equal to 1 if students identified as Black or African American, Hispanic or Latino, multiple, Native American or Alaskan, or Native Hawaiian or other Pacific Islander, 0 otherwise. We combined the groups because we did not have a priori reason to believe that program effects would vary differentially over each group. LSAT/*c*GRE is a variable that describes either LSAT scores or GRE scores converted to the LSAT scale using the GRE Law School Comparison Tool (ETS, n.d.). Some schools reported the converted scores; we used the tool when only GRE scores were reported. To account for the fact that law schools differ in their admissions criteria for applicants' LSAT scores and UGPA, we computed deviations from the university medians of the entering class for the given year.

For our exploratory analysis on course engagement and completion, we found from preliminary analyses that our data did not meet the assumptions for modeling a continuous treatment effect with percent of assignments completed as the treatment variable. As such, we chunked the variable into three groups to compare total and partial completion of the program. A natural point for partial completion is the eight skills workshops that were available within the program. In each of the eight weekly workshops, participants were introduced to and provided an opportunity to practice a skill that would be learned in law school, such as identifying the rule in a case (Findley et al., 2023). We defined the three groups as students who completed the whole JD-Next course (course completers), students who completed all eight skills workshops but did not complete the whole course (complete skills workshops participants), and students who only partially completed the skills workshops (partial skills workshops participants).

Sample

For the treatment group, the analytic samples include students who participated in at least one assignment in the JD-Next program in the summer of 2020 and who subsequently enrolled and completed the fall semester at one of the participating schools that provided the data we requested. Four schools did not provide sufficient data to be included in any analyses. Of the remaining schools, two did not provide first-year fall LGPA, five schools did not provide contracts course grades, and one school did not provide writing course grades. For the schools that provided sufficient data, missing data on variables besides the outcome variables for particular students was permitted in our models (see Analysis). However, students with missing outcome data were excluded. Plausible explanations that students would be missing outcome data include leaving law school during the first year, variation in course-taking patterns, and university policies that vary, such as the timing of courses or not issuing grades for specific courses. Given the available data, we created separate analytic samples for the three outcomes, first-year fall LGPA (n = 2,066), contracts course grades (n = 1,657), and writing course grades (n = 2,226). The numbers of schools and students in each group for each dataset are shown in Table 1. Note that our need to create three analytic samples did not change our planned analyses, but we cannot rule out relevant unobserved influences on our results due to school-wide missing outcome data. Future studies could collect outcome measures for a longer time period to explore the robustness of our results and also to include longer-term measures of law school success.

Table 1. *Analytic Sample Sizes*

Outcome(s)	Schools	Treatment	Control
First-year LGPA (fall)	11	432	1,634
Contracts course grades	9	382	1,275
Writing course grades	12	479	1,747

Researchers obtained data from students who were in their first year of law school in 2018 for comparison because the JD-Next program was not offered then, and the 2018 cohort offers a larger and independent sample relative to students in the 2020 cohort who chose not to participate. This selection of a non-invited control group reduces the risk of selection bias because it does not condition on the non-participants' self-selection out of participation. We used 2018 data rather than 2019 data because the initial pilot of the program (Cheng, 2023) exposed some students nationwide to the intervention in 2019, which would have risked a confound.

Law schools typically use mandatory grading distribution policies to minimize grade inflation within cohorts, but it is possible that those policies could change between cohorts. We checked for evidence of grade inflation using administrative data from the 2018 and 2020 cohorts, notably because students in the later year received grades during the COVID-19 pandemic, which could have changed the grading scale. We compared the distribution of grades for all enrolled students in both years and also between a matched sample of 2018 cohort students and 2020 non-participants, finding no significant differences in grades, on average. Given that we found no evidence of grade inflation, we used the 2018 cohort as the control group.

Data Analysis

Given that our research questions are causal and our study design yielded observational data, an appropriate analysis approach is one that can estimate the effect of the JD-Next program on law school grades separately from observed factors that influenced both students' choices to participate in the JD-Next program and also their law school grades. Propensity score estimation and weighting is one option for attempting to reduce or eliminate confounding effects in estimating treatment effects (McCaffrey et al., 2004; Rosenbaum & Rubin, 1983). We estimated propensity scores with the TWANG package in R 4.1.2 (Cefalu et al., 2021; R Core Team, 2021) using our observed student covariates, LSAT/*c*GRE, UGPA, age, gender, and a five-category variable for race/ethnicity (Asian, Black, Hispanic, multiple, and White, for short). Given that the non-random assignment to treatment and control conditions occurred at the student level and not the school level, we were able to include schools as fixed effects. Including school in the weighting balances the proportional representation of schools in the sample sizes for the treatment and control groups so that treatment is not confounded with differences across schools in the choice of who enrolled in the program (e.g., Fuentes et al., 2021). The TWANG package balances the treatment and control groups on the available covariates and also attempts to balance on missing values in the covariates. The program takes a machine learning approach to modeling, automatically estimating models, including ones with nonlinear and interactive effects, to find the optimal covariate balance (Ridgeway et al., 2021, p. 2). The set of covariates in our propensity score model is an exhaustive list of available information from administrative records that explains both the choice to participate in JD-Next and students' grades, but is nonetheless limited in size. As part of the iterative process of refinement and continual evaluation of the JD-Next program, a study could be designed to uncover unobserved data related to participation and grades for future cohorts and be used to create measures to supplement administrative data and broaden the set of relevant covariates and potentially improve the precision of the estimated effects.

The propensity score estimation approach and weighted outcome model we chose estimate the average treatment effect on the treated (ATT). We chose the ATT because we believe that students' characteristics would likely determine whether they would choose to participate in JD-Next outside of a research setting and we would not expect the entire population of prospective law students to participate in the program. In the TWANG package, we used the default gradient boosting package to estimate the propensity score model, as well as default values for the maximum number of gradient boosting iterations (10,000), the interaction depth (3), and the learning rate (.01) (Ridgeway et al., 2021). For the outcome model, we used the *survey* package in R (Lumley, 2020) to fit a generalized linear model with inverse-probability weighting and design-based standard errors. Observations from the treatment group received a weight of 1, while observations from the control group received a weight based on the propensity score model.

The general form of the propensity score model is $\log \frac{P(t=1|\mathbf{x})}{1-P(t=1|\mathbf{x})} = \beta'\mathbf{x}$ where x is a function our observed confounders (Ridgeway et al., 2021, pp. 30–31), namely school fixed effects, LSAT/*c*GRE, UGPA, age, gender, and race/ethnicity. The general form of the outcome model is

$$\text{grade} = school\ fixed\ effects + \beta_1 \text{treated} + error.$$

We included school fixed effects in both the propensity score model and the weighted outcome model to account for the clustering (e.g., Leite, 2017).

For our first research question, which focused on the overall effects of JD-Next, we estimated propensity scores separately for the three analytic samples and fit separate models for the three outcome variables. Our second research question explored whether the effect of JD-Next on LGPA and contracts course grades differed, on average, for underrepresented racial and ethnic and gender groups. For this exploratory analysis, we reestimated the propensity score weights in each analytic sample by manually computing interactions of indicators for underrepresented groups and gender with each other and with the other covariates and including them in the TWANG program to ensure that our interactions of interest were included in the final propensity score model. The outcome model included an interaction term for treatment, underrepresented groups, and gender, as well as main effects.

The analyses for our third research question focused on exploring the relationship between levels of engagement and the three outcomes. For each analytical sample, we capitalized on the independent control group from 2018 to compute three sets of propensity scores and estimate three separate binary treatment models, comparing course completers, complete skills workshop participants, and partial skills workshop participants to the 2018 control group. For all exploratory analyses, we interpret results as suggestive of relationships that warrant further rigorous evaluation and, as such, did not adjust for multiple tests, consistent with guidelines in Schochet (2008).

To contextualize the results associated with our three primary research questions, we explored characteristics of participants that were associated with attrition. Specifically, we used the administrative data to describe participants at each of the three levels of participation. We also used data on participants' perceptions of their preparation to describe whether participants viewed the program as helpful. Participants were surveyed prior to taking the course, after taking the course, and after their first semester in law school. They were asked to rate how prepared they were for law school in terms of knowledge, skills, and personal development in ten areas widely acknowledged as essential for law school (ABA, n.d.-b). There were four response options: not at all prepared, somewhat prepared, very prepared, and completely prepared. We created indicators that were equal to 1 if participants' perceived levels of preparation improved over time and 0 otherwise. We assigned a value of 1 when participants reported an increase in their level of preparation over all periods or increased over one period without reporting a decrease in the other period.

Results

We start with models for LGPA as the outcome. Following Ridgeway et al. (2021), we evaluated diagnostic plots and determined that the machine learning approach for estimating the propensity score model converged. The TWANG package gives users a choice on the criterion for stopping the machine learning algorithm and completing the estimation. We chose the maximum effect size for the stopping rule, because it achieved the best balance on the covariates between the treatment and control groups, and the goal in estimating treatment effects is to have treatment and control groups that are the same on variables that relate to both the probability of being in the group and the outcomes of interest. Using this stopping rule, the effective sample size for the propensity score weighted control group sample was 873. The covariate balance before and after weighting is shown in Table 2. We achieved good balance for all variables except age and two of the schools, evidenced by standardized mean differences less than the recommended value of .10 and *p*-values associated with the test statistic comparing treatment and control groups that were greater than the recommended value of .05 (Griffin et al., 2020, p. 12). The balance statistics for age and school were close to the recommended values, but we decided to include them in the outcome model since they did not meet the recommended values for evidence of good weighting (Bang & Robbins, 2005). The model-based estimate of the effect of engaging with the JD-Next program on first-semester law school GPA was .12 (.027), $p < .001$. That is, we estimate that individuals who participate in the JD-Next program would score .12 higher on the 0–4 LGPA scale, on average, than those who do not participate. To further interpret this estimate, we used the entire 2020 student cohort to compute the class rank within university for the average within-university LGPA and also for the average LGPA plus the estimated effect. The hypothetical improvement in class rank ranged from 3 to 25, with 11 as the median hypothetical improvement in class rank. In other words, we roughly estimate that a .12 improvement in LGPA would be equivalent to moving ahead of 10 students in terms of class rank.

Table 2. *Covariate Balance in the Unweighted and Weighted Samples*

Unweighted								Weighted						
	Treat mean	Treat SD	Control Mean	Control SD	Std effect size	*t* stat/ chi-square	*p*	Treat mean	Treat SD	Control Mean	Control SD	Std effect size	*t* stat/ chi-square	*p*
Age	24.42	4.05	25.72	4.58	-0.32	-5.73	0.00	24.42	4.05	24.87	4.15	-0.11	-1.90	0.06
UGPA*d*	-0.05	0.36	-0.07	0.37	0.05	0.82	0.41	-0.05	0.36	-0.06	0.36	0.01	0.21	0.83
UGPA*d* missing	0.00	0.05	0.02	0.15	-0.40	-4.48	0.00	0.00	0.05	0.00	0.05	-0.01	-0.11	0.91
LSAT*d*	-0.18	4.15	-0.54	4.63	0.09	1.51	0.13	-0.18	4.15	-0.12	4.31	-0.02	-0.27	0.79
LSAT*d* missing	0.03	0.18	0.03	0.16	0.03	0.52	0.61	0.03	0.18	0.03	0.16	0.03	0.53	0.60
Asian	0.09	0.28	0.06	0.23	0.11	2.05	0.05	0.09	0.28	0.08	0.27	0.03	0.27	0.96
Black	0.04	0.20	0.06	0.23	-0.07	-	-	0.04	0.20	0.04	0.20	0.00	-	-
Hispanic	0.15	0.36	0.16	0.37	-0.05	-	-	0.15	0.36	0.16	0.36	-0.02	-	-
Multiple	0.03	0.16	0.02	0.15	0.04	-	-	0.03	0.16	0.03	0.16	0.01	-	-
Native American	0.01	0.10	0.01	0.09	0.02	-	-	0.01	0.10	0.01	0.07	0.04	-	-
Not a U.S. Citizen	0.01	0.12	0.02	0.12	-0.01	-	-	0.01	0.12	0.01	0.10	0.03	-	-
White	0.67	0.47	0.66	0.48	0.02	-	-	0.67	0.47	0.67	0.47	-0.01	-	-
Race/ethnicity other or missing	0.01	0.07	0.02	0.15	-0.28	-	-	0.01	0.07	0.01	0.09	-0.06	-	-
Gender	0.63	0.48	0.55	0.50	0.17	3.11	0.00	0.63	0.48	0.60	0.49	0.06	1.06	0.29
Gender missing	0.01	0.08	0.01	0.07	0.03	0.47	0.64	0.01	0.08	0.01	0.10	-0.03	-0.37	0.71
School A	0.12	0.32	0.08	0.27	0.11	3.92	0.00	0.12	0.32	0.12	0.32	0.00	1.02	0.42
School B	0.08	0.27	0.07	0.26	0.03	-	-	0.08	0.27	0.09	0.29	-0.06	-	-
School C	0.10	0.30	0.09	0.29	0.02	-	-	0.10	0.30	0.10	0.30	0.00	-	-
School D	0.04	0.20	0.12	0.33	-0.40	-	-	0.04	0.20	0.05	0.23	-0.06	-	-
School E	0.07	0.25	0.08	0.27	-0.03	-	-	0.07	0.25	0.08	0.28	-0.06	-	-
School F	0.06	0.23	0.07	0.26	-0.05	-	-	0.06	0.23	0.07	0.25	-0.04	-	-
School G	0.09	0.29	0.08	0.27	0.05	-	-	0.09	0.29	0.10	0.30	-0.02	-	-
School H	0.09	0.29	0.12	0.32	-0.09	-	-	0.09	0.29	0.06	0.24	0.10	-	-
School I	0.13	0.34	0.11	0.31	0.07	-	-	0.13	0.34	0.10	0.30	0.11	-	-
School J	0.18	0.38	0.13	0.33	0.13	-	-	0.18	0.38	0.19	0.39	-0.04	-	-
School K	0.04	0.21	0.06	0.23	-0.05	-	-	0.04	0.21	0.04	0.20	0.00	-	-

Note. d = deviation and refers to the variable minus its school-by-year median value.

Next, we turn to models for contracts course grades as the outcome. After estimating the propensity scores with the analytic sample for contracts course grades, all four stopping rules achieved good balance on all covariates. The stopping rule ES max had the highest effective control group sample size at 707, so we chose it as the stopping rule. Because we achieved good balance, the outcome model did not include covariates. We estimated the effect of JD-Next on contracts course class grades to be .10 (.04), p = .01. That is, students who participated in JD-Next scored .10 of a point higher, on the 4.0-grade point scale, relative to students who did not participate, on average.

For writing course grades, we again used ES max as the stopping rule, which had an effective sample size of 900 and achieved good balance except for one school. We added school as a fixed effect to the outcome model. The estimated effect of JD-Next on writing course grades on the 4.0-grade point scale was .08 (.03), p = .008.

For our exploratory analysis of differences by underrepresented groups and gender, we reestimated the propensity score models with additional covariates that represented the interactions of indicators for unrepresented groups and gender with the other covariates. We achieved good balance on all variables for each analytic sample. We did not find any significant gender group differences in any model. We did not find any significant differences in the relationship between JD-Next participation and both LGPA and writing course grades for students in unrepresented groups. The model with contracts course grades as the outcome suggested a statistically significantly weaker relationship between JD-Next participation and contracts course grades for students from underrepresented groups relative to all other students, on average.

Our final set of exploratory analyses focused on course engagement and completion. Model estimates are shown in Table 3. Recall that we weighted the control group to each of the three groups defined by the amount of the program they completed. As such, the values are preliminary estimates of the effects of the JD-Next program for students who share similar characteristics to the students in that particular group. For example, we estimate that students who share similar characteristics to course completers would score .17 higher on the LGPA scale (median improvement in class rank of roughly 16) if they completed the program as opposed to not participating in the program. These estimates suggest that different groups may have benefitted at different levels of participation, as opposed to not participating. Note that the estimates do not suggest what the outcomes might have been if a student who partially completed the course had completed it.

Table 3. *Relationship Between Levels of Engagement in the Program and the Three Outcomes*

	LGPA	Contracts course grade	Writing course grade
Course completers	.17 (.04), p < .001; $n_{\text{treatment}}$ = 131 Median improvement in class rank = 16	.09 (.05), p = .097; $n_{\text{treatment}}$ = 123	.12 (.04), p =.003 $n_{\text{treatment}}$ = 149
Complete skills workshops participants	.10 (.04), p = .018; $n_{\text{treatment}}$ = 131 Median improvement in class rank = 9	.18 (.05), p < .001; $n_{\text{treatment}}$ = 110	.13 (.05), p = .006; $n_{\text{treatment}}$ = 138
Partial skills workshops participants	.11 (.04), p =.007; $n_{\text{treatment}}$ = 170 Median improvement in class rank = 10	.07 (.07), p = .30; $n_{\text{treatment}}$ = 149	.02 (.05), p =.65; $n_{\text{treatment}}$ = 192
Overall Estimate	.12 (.027), p <.001	.10 (.04), p = .01.	.08 (.03), p = .008.

Note. Standard errors in parentheses.

Contextual Characteristics

In this section, we describe the characteristics of participants by their amount of participation in the JD-Next program and summarize improvements in participants' perceived preparation for law school associated with that participation. Table 4 shows the means and standard deviations of the available covariates for course participants by level of course participation for the LSAT analytic sample. While LSAT scores and UGPA tended to be highest for course completers, the differences in means across groups were not significantly higher statistically. Race/ethnicity, gender, and school proportions were also statistically indistinguishable across the groups in this sample. For the contracts course grade and writing course grade samples, the trends in the direction of differences were the same, except the differences were statistically significant in at least one of the samples for UGPA, age, and schools.

Table 4. *Characteristics of Participants by Level of Course Completion, LGPA Analytic Sample*

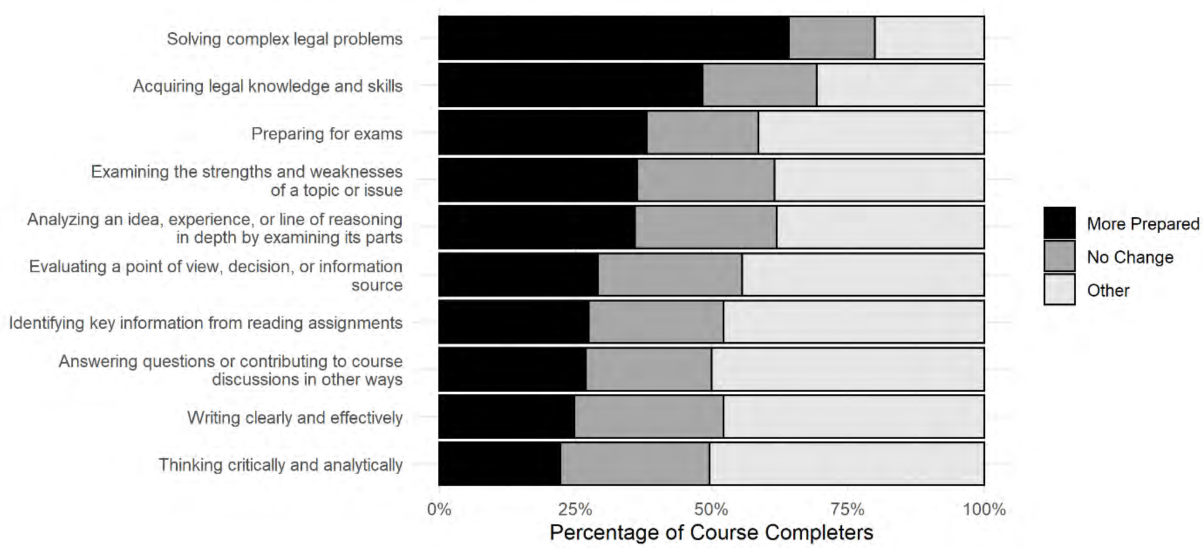
	Course completers	Complete skills workshops participants	Partial skills workshops participants	Difference among groups
Age	25.05 (4.63)	24.24 (3.39)	24.09 (4.02)	$F(2,429)=2.277$, $p = .104$
UGPA*d*	0 (0.35)	-0.05 (0.34)	-0.10 (0.38)	$F(2,427) = 2.899$, $p = .06$
UGPA*d* missing	0	0	0.01 (0.08)	$X^2(2) = 3.10$, $p = .21$
LSAT*d*	0.23 (4.76)	0.01 (4.06)	-0.64 (3.68)	$F(2,415) = 1.786$, $p = .169$
LSAT*d* missing	0.05 (0.23)	0.03 (0.17)	0.02 (0.13)	$X^2(2) = 3.04$, $p = .22$
Asian	0.12 (0.33)	0.08 (0.28)	0.07 (0.25)	X^2 (12) =11.09, $p = .52$
Black	0.05 (0.21)	0.05 (0.21)	0.04 (0.19)	
Hispanic	0.18 (0.38)	0.12 (0.33)	0.15 (0.35)	
Multiple	0.04 (0.19)	0.02 (0.12)	0.03 (0.17)	
Native American	0.02 (0.12)	0.02 (0.12)	0	
Not a U.S. Citizen	0.01 (0.09)	0.02 (0.15)	0.01 (0.11)	
White	0.60 (0.49)	0.70 (0.46)	0.70 (0.46)	
Race/ethnicity other or missing	0	0	0.01 (0.11)	$X^2(2) = .89$, $p = .64$
Gender	0.64 (0.48)	0.62 (0.49)	0.63 (0.49)	$F(2,426) = .095$, $p = .909$
Gender missing	0	0.01 (0.09)	0.01 (0.11)	$X^2(2) = 1.50$, $p = .47$
School A	0.22 (0.42)	0.06 (0.24)	0.08 (0.27)	$X^2(20) = 30.54$, $p = .06$
School B	0.06 (0.24)	0.07 (0.25)	0.09 (0.29)	
School C	0.08 (0.28)	0.12 (0.32)	0.10 (0.30)	
School D	0.02 (0.15)	0.04 (0.19)	0.06 (0.24)	
School E	0.05 (0.21)	0.07 (0.25)	0.09 (0.28)	
School F	0.05 (0.23)	0.05 (0.23)	0.07 (0.25)	
School G	0.08 (0.27)	0.12 (0.33)	0.08 (0.28)	
School H	0.07 (0.25)	0.12 (0.33)	0.09 (0.28)	
School I	0.12 (0.32)	0.13 (0.34)	0.15 (0.36)	
School J	0.20 (0.40)	0.18 (0.38)	0.16 (0.37)	
School K	0.05 (0.23)	0.05 (0.21)	0.04 (0.19)	

Note. Cells are mean values within course completion group, standard deviations in parentheses.

Recall that participants were surveyed before the course and twice after the course. There were 749 participants who engaged with the course and enrolled in law school; 90% responded to the pre-course survey. The response rate was 40% for the post-course survey and 37% for the post-first-semester survey. The response rate increased as the amount of course participation increased. Specifically, the nonresponse in the first survey was attributed to partial skills workshop participants. For both course completers and skills workshop completers, responses to how prepared they felt in the ten topic areas before taking the course were generally similar, with most feeling somewhat or very prepared in eight of the topic areas. In both groups, almost half of the participants felt unprepared for solving complex legal problems before taking the course. Approximately 20% of participants in each group felt not at all prepared in acquiring legal knowledge and skills before taking the course.

Turning to changes in perceptions over time, all but two course completers responded to either the second or third survey or both, so we summarize the responses for this group. Half (50%) of the complete skills workshop participants completed the first survey and at least one of the other surveys. Consequently, we do not report a summary of their changes in responses. Figure 2 shows the percentages of course completers who reported feeling more prepared and similarly prepared over time in the ten topic areas. The "other" category comprises course completers who reported feeling less prepared over time and those whose responses changed direction over time. The highest percentages of course completers felt more prepared in solving complex legal problems and acquiring legal knowledge and skills. These are the two topic areas in which participants tended to feel unprepared before the course. We note that in interpreting these survey responses over time, we cannot rule out the possibility that individuals used the scale differently at each time point, particularly as they learned more about these topics during their first semester in law school. Nonetheless, it is reasonable to assume that they used the scale similarly across the ten topic areas and that general comparisons across topics are supported.

Figure 2. *Course Completers' Perceptions Over Time About the Level of Preparation for Law School in Terms of Knowledge, Skills, and Personal Development in Topics Related to the Program*



Discussion

Using registrar data for both participants and others not invited to participate in an outcome model weighted by propensity scores, we report statistically significant improvements in law school performance associated with participation in the program among students who decided to enroll. These findings are consistent with the results of the study performed in 2019 (Cheng, 2023), where the prior study had an enriched sample for racial diversity and a randomized placebo-controlled design but had a small sample size and depended on self-reported data.

With our larger sample size, we were also able to explore and report greater benefits for those who completed the program compared to those who began but did not finish. This finding is useful for future program design and also suggests a positive dose-response relationship.

Comparing the effect estimates for overall law GPA (.12 of a grade point) with grades in the keyed course of Contracts Law (.10 of a grade point), or the even stronger difference for course completers, suggests that the effect was not merely driven by participants acquiring that specific course-content knowledge. Rather, it is tenable that the effect arose through a broader development of skills necessary for law school, as intended by the course developers and as self-reported by participants.

As in Cheng (2023), no significant differences were found across racial and ethnic groups for the course effect on overall LPGA. Were this finding to hold as more evidence is accumulated, it would raise questions about how to best target the program to promote equity.

Methodological strengths of the current study include a large sample size in terms of both the numbers of schools and participants, a diverse population of participating schools and students, incentives to encourage completion of the program, the use of registrar data rather than merely self-reported outcomes, and the availability of data on participants, even if they attrited from the program, along with data for non-participants and a prior cohort of matriculants who were not even invited to the program. Pre- and post-surveys were conducted to allow measurement of changes over time, rather than merely asking about student self-satisfaction after the course. We also used a control group that was not invited to participate, thus improving the range of candidates for weighting and reducing the effect of self-selection. Finally, sophisticated statistical analysis yielded model diagnostics that support the inferences being made.

Limitations

Limitations include the lack of random assignment to begin the course and students' self-selection into analysis groups by choosing whether to complete or attrit the course. Substantial numbers did, in fact, attrit. While our statistical methods attempt to remove the selection effects, we cannot be certain that we collected and accounted for every variable that would predict both participation and the outcomes. Nonetheless, our weighting strategy ensures that participant race, as well as UGPA and LSAT scores, were nearly identical across the weighted treatment and control groups, so any omitted variable that explained the difference in outcomes for participants would have to be one that is not measured by these other primary predictors of law school performance.

Still, as with all observational studies, we cannot control for differences that we do not observe in the dataset. For example, we cannot observe whether there may have been differences in other behaviors (such as participation in other preparatory courses) between participants and non-participants, which could create a confound and bias our estimates. To the extent that these other influences were at the school level (e.g., an academic support program) and constant between 2018 and 2020, our efforts to match participants and non-participants at the same schools may partially mitigate the concern.

In addition, the sample size was limited to explore effects in specific racial groups, partly due to missing data from institutions. The surveys suffered from a poor response rate.

Implications for Theory and Practice

While the JD-Next program appears beneficial, it also suggests several directions for future research and development followed by additional rigorous evaluation. First, how can such programs identify diverse populations of potential students who can benefit from the program and then recruit them to start and ideally complete the course? One possibility is for law schools to require such courses, whether for all of their matriculating students or for those at greatest risk of failure in law school.

Second, how can such courses be improved to enhance course completion and thereby expand the impact of the course? Options include alternative course schedules. For example, rather than spreading the course content over nine weeks (as implemented for JD-Next in 2020), an intensive (e.g., one-week or two-week) course may increase engagement and create a cohort experience, even if it creates higher opportunity costs for learners (who could not also work or vacation during that same week or two).

Third, what is the optimal educational content and pedagogical approach for a law school preparatory course, and can the JD-Next program be improved to enhance its benefits? Other programs, such as Harvard Law School's Zero-L course, focus more on teaching "foundational legal concepts" (Harvard Law School, n.d.), and the findings of this study cannot be extrapolated to that setting. For JD-Next, the focus is on teaching the skills of case reading and analysis with formative assessment. Although we document a substantial effect on law school grades nearly four months after completion of the JD-Next course, it is possible that providing students with other legal concepts, additional formative assessments, or more tailored feedback, could reap greater benefits. In addition, it may be possible to add other modules, such as growth mindset or professional identity formation, which could support persistence and achievement in law school.

Fourth, will the benefits of the JD-Next program translate into more distal outcomes such as final law GPA, graduation, bar passage, and success as a lawyer? These outcomes will require longer follow-up.

Finally, although the course already reaches a substantial number of schools and students, can it be further scaled up to reach all who could benefit from the program? Although the fully-online, asynchronous approach would seem to facilitate an economically efficient model, other goals, including providing students a sense of engagement to support retention and tailored feedback to support learning, may require greater investments.

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Appendix

Table A1. *Median LSAT and UGPA and Gender Composition of All Enrolled First-Year Students in Participating Schools*

	2020				2018			
School	Median LSAT	Median UGPA	Female	Male	Median LSAT	Median UGPA	Female	Male
A	166	3.61	49%	49%	163	3.57	44%	53%
B	163	3.72	44%	56%	162	3.63	53%	47%
C	163	3.56	46%	52%	161	3.7	56%	44%
D	161	3.72	55%	45%	160	3.63	55%	45%
E	160	3.76	58%	42%	157	3.51	56%	44%
F	159	3.51	51%	49%	157	3.39	44%	56%
G	158	3.65	59%	41%	156	3.63	56%	44%
H	156	3.63	53%	47%	156	3.66	57%	43%
I	155	3.63	47%	53%	153	3.37	49%	51%
J	155	3.53	57%	43%	154	3.38	42%	58%
K	155	3.39	60%	40%	153	3.35	55%	45%
L	154	3.44	65%	35%	155	3.36	51%	49%
M	154	3.42	51%	48%	154	3.32	56%	43%
N	154	3.41	54%	46%	152	3.38	62%	38%
O	153	3.33	71%	28%	152	3.41	58%	42%
P	152	3.25	63%	35%	153	3.29	54%	46%
Q	144	2.98	50%	50%	144	2.83	48%	52%

Note. Source: https://www.americanbar.org/groups/legal_education/resources/statistics/.

Table A2. *Race/Ethnicity Composition of All Enrolled First-Year Students in Participating Schools*

School	Year	Asian	Black or African American	Hispanic	Native American	Native Hawaiian or Pacific Islander	White	Two or more	Unknown	Not a U.S. Citizen
A	2020	5%	2%	13%	7%	0%	66%	2%	3%	3%
	2018	1%	2%	17%	7%	2%	62%	2%	3%	5%
B	2020	0%	2%	16%	0%	0%	73%	2%	7%	0%
	2018	4%	0%	9%	0%	0%	76%	5%	5%	0%
C	2020	2%	4%	5%	0%	0%	79%	5%	4%	1%
	2018	4%	6%	4%	1%	0%	77%	6%	2%	1%
D	2020	2%	9%	52%	0%	0%	27%	4%	3%	3%
	2018	1%	9%	47%	0%	0%	37%	0%	3%	3%
E	2020	1%	7%	17%	1%	0%	67%	6%	0%	1%
	2018	2%	8%	15%	0%	0%	69%	5%	0%	0%
F	2020	29%	2%	11%	0%	2%	21%	31%	2%	2%
	2018	29%	0%	12%	0%	2%	29%	26%	0%	2%
G	2020	3%	5%	6%	0%	0%	81%	4%	3%	0%
	2018	2%	2%	6%	0%	0%	84%	3%	2%	1%
H	2020	3%	16%	7%	0%	0%	73%	0%	1%	0%
	2018	3%	16%	3%	1%	0%	77%	0%	0%	0%
I	2020	4%	1%	4%	1%	0%	89%	0%	1%	1%
	2018	1%	1%	4%	0%	0%	88%	0%	3%	1%
J	2020	3%	7%	6%	0%	0%	61%	2%	20%	1%
	2018	5%	3%	6%	0%	0%	65%	2%	14%	4%
K	2020	1%	62%	7%	0%	0%	21%	4%	2%	1%
	2018	1%	56%	5%	1%	0%	31%	3%	2%	0%
L	2020	5%	3%	8%	0%	0%	75%	2%	4%	3%
	2018	4%	4%	9%	0%	0%	73%	1%	5%	3%
M	2020	1%	7%	12%	0%	0%	66%	3%	6%	4%
	2018	7%	10%	15%	0%	0%	61%	1%	3%	3%

N	2020	2%	6%	19%	1%	0%	65%	6%	1%	1%
	2018	4%	3%	22%	0%	0%	63%	5%	3%	1%
O	2020	22%	2%	15%	0%	0%	48%	5%	2%	6%
	2018	19%	1%	20%	0%	0%	48%	7%	3%	1%
P	2020	20%	7%	15%	0%	0%	43%	9%	2%	5%
	2018	22%	4%	11%	0%	0%	45%	7%	4%	6%
Q	2020	2%	7%	5%	0%	0%	76%	6%	3%	0%
	2018	0%	4%	6%	1%	0%	81%	4%	2%	2%

Note. Source: https://www.americanbar.org/groups/legal_education/resources/statistics/.