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Summer Credit Recovery Impact on Newcomer English Learners

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Prior research shows that English learners (ELs) lag behind their peers in academic achievement and education attainment. The persisting gap is partly attributed to ELs' limited exposure to academic content. This article investigates the efficacy of a summer credit recovery program aimed at expanding high school newcomer ELs' access to academic subjects. Leveraging student-level data from a large urban district in California, I use a difference-in-differences-in-differences approach to estimate the program's impact on high school course taking, English proficiency, and graduation. Credit recovery increased the number of math, English Language Arts, science, and social science classes taken by newcomer EL students. Effects on 4- and 5-year graduation rates are imprecisely estimated. I also find suggestive evidence for positive effects on English proficiency.

KEYWORDS: course taking, English learner, graduation, summer

About 4.9 million students in U.S. public K–12 education are English learners (ELs; National Center for Education Statistics, 2019a). Although federal education policy has focused on improving ELs' academic success, substantial gaps persist in both education achievement and attainment between ELs and their peers. Compared with 20 years ago, ELs today are lagging even further behind their peers in fourth- and eighth-grade National Assessment of Educational Progress (NAEP) math and reading (Carnoy & Garcia, 2017). High school ELs are less likely to take college-preparatory classes (Callahan, Wilkinson, & Muller, 2010) and are 18 percentage points less likely to graduate in 4 years (ED Data Express, 2017). Only 18% of ELs advance directly to 4-year colleges, compared with 43% of monolingual English speakers (Kanno & Cromley, 2013). Instead, ELs are more likely to enroll in 2-year colleges or not participate in higher

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education at all (Callahan & Humphries, 2016). As the EL population continues to grow, so does concern over their underachievement.

The performance and attainment gaps between ELs and their peers translate into inequities in career and social opportunities, which carry economic, moral, and legal implications. In the next few years, as many as 1 in 10 workers entering the labor force will have been an EL. The quantity and quality of education ELs receive in school have a direct impact on the strength of the U.S. economy and the well-being of American communities. Compared with high school graduates, dropouts earn $9,200 less a year and face a 1.5-percentage-point higher unemployment rate (U.S. Bureau of Labor Statistics, 2019), as well as higher rates of incarceration and worse health conditions (Freudenberg & Ruglis, 2007; Kearney, Harris, Jácome, & Parker, 2014). Improving the graduation rates of ELs strengthens the public sector by increasing tax revenue and reducing health care and other public assistance costs. At the same time, rigorous academic curricula are crucial to ensuring that students leave school with employable skills.

The law requires that districts and schools that receive federal funding provide EL students with equitable access to education programs. In *Lau v. Nichols* (1974), the Supreme Court ruled that denying students of a particular race, color, or national origin the education opportunities available to other students violates the Civil Rights Act of 1964. This unanimous decision affirmed the U.S. Department of Health, Education, and Welfare (1970) guidelines, which state,

> Any ability grouping or tracking system employed by the school system to deal with the special language skill needs of national origin-minority group children must be designed to meet such language skill needs as soon as possible and must not operate as an educational deadend or permanent track.

As the U.S. economy evolves to demand more high-skilled labor, policy in recent decades has also elevated standards for academic programs. The Every Student Succeeds Act (2015–2016) requires that all students in the United States be provided with high-standards academic preparation for success in college and careers. This provision protects ELs' rights to teaching and curricula that enable them to access the same postsecondary opportunities as their peers.

In response to federal regulations, districts across the nation have implemented a wide array of language support programs for ELs. Commonly offered are English Language Development (ELD) courses, which focus on vocabulary, grammar, and other aspects of English proficiency, in addition to or as replacements for monolingual English Language Arts (ELA). In addition, many districts have expanded language development options to include home language maintenance and bilingual immersion (Valentino & Reardon, 2015). But the gaps in NAEP scores and graduation rates suggest that inequities remain. To better support ELs, districts and schools need interventions that target the distinct linguistic and academic needs of students based on their age and prior education experience.

Research emphasizes that ELs literate in their first language differ considerably from ELs learning to read in any language for the first time; among older students, ELs who recently immigrated to the United States need services distinct from those suitable for ELs who have spent years in U.S. schools (e.g., Callahan, 2005; Freeman, Freeman, & Mercuri, 2002; Olsen, 2010). However, schools and districts have only recently begun to distinguish these EL subgroups in data reporting. Education attainment and achievement data disaggregated by length of residency and prior education are rarely reported, and policy research addressing EL subgroups is scarce. Recent studies also highlight the need to better identify and understand the academic development of subgroups within an ever-EL framework, such as recent immigrant ELs, former ELs who have exited language service, and long-term ELs who still receive service after 5 or more years (e.g., Estrada & Wang, 2018; Jaquet & Fong, 2017; Johnson, 2019c; Kieffer & Thompson, 2018).

Contributions of the Current Study

This article examines the efficacy of an intervention that aims to expand newcomer ELs' access to academic content courses and improve their high school graduation rates. My research question is "*How does summer credit recovery affect high school newcomer ELs' course taking, English proficiency, and graduation?*" Leveraging 11 years of student-level administrative data, I report the causal impact of a program implemented by a large urban school district.[1]

To the best of my knowledge, this study is the first to focus on interventions targeting newcomer ELs. The term *newcomer* refers to students who have lived in the United States for less than 3 years and are eligible to participate in the Title III Immigrant Student Subgrant Program (Every Student Succeeds Act, 2015–2016). Newcomer ELs in high school face a unique set of challenges that combines English language acquisition, academic content mastery, and the sociocultural adjustment of living in a new country (Short & Fitzsimmons, 2007; Umansky et al., 2018). In my sample, newcomer ELs' eighth-grade ELA test scores are approximately 1 standard deviation (*SD*) lower than their "oldcomer" peers' scores. Responding to the needs of these older, recently arrived students, many districts now offer newcomer programs. However, due to the lack of empirical evidence, little is known about their efficacy.[2] As a result, our knowledge of interventions for newcomer ELs remains scant.

My study contributes to the intersecting strands of literature on EL academic access and high school completion. It extends the research on EL course access by examining the number and level of ELA, math, science, and social science classes ELs take in high school (Callahan & Shifrer, 2016; Callahan et al., 2010; Umansky, 2016a). High school graduation rates have garnered more attention from researchers and policymakers following the No Child Left Behind Act of 2001 and its emphasis on high school accountability for student success in the labor force. Yet little discussion focuses on ELs. Causal evidence on EL graduation is even more limited. This article addresses this gap by applying new evidence to identify the extent to which targeted intervention affects ELs' 4- and 5-year graduation rates.

This is also the first article to investigate the impact of an intervention above and beyond the language support ELs receive during the academic year. Prior literature has compared the outcomes of students on the cusp of EL classification and reclassification (e.g., Robinson, 2011; Umansky, 2016a) and students receiving different types of language services—such as bilingual immersion versus transitional maintenance—during the school year (e.g., Slavin, Madden, Calderon, Chamberlain, & Hennessy, 2011; Valentino & Reardon, 2015). Yet no study, to my knowledge, has looked at additional interventions for classified ELs after school or during the summer (Goldenberg, 2008). This study is the first to identify the treatment effect of a program that aimed to accelerate the academic progress of ELs in addition to their academic year in-school language service.

Research on EL Academic Access

Even after adjusting for socioeconomic status, an achievement gap remains between current ELs and their peers, the magnitude of which exceeds any gap between ethnicities or income levels (Carnoy & Garcia, 2017). In eighth-grade NAEP math in 2015, Hispanic ELs scored 1.3 *SD* lower than White students, while Hispanic non-ELs scored 0.4 *SD* lower than White students. Asian ELs scored 0.7 *SD* lower than White students, while Asian non-ELs scored 0.5 *SD* higher than White students. In eighth-grade reading, the gap between White students and Asian ELs was 0.9 *SD*, while the gap between White students and Asian non-ELs was negligible. The gap between White students and Hispanic ELs was 1.1 *SD*, compared with 0.3 *SD* between White students and Hispanic non-ELs.

Research suggests that two school factors are especially important in shaping EL achievement: (1) language services and (2) access to academic content (e.g., Umansky, 2016a; Valentino & Reardon, 2015). Intended to support ELs, intensive English language courses and sheltered content instruction can also have unintended consequences. Most ELs spend between two and four class periods a day taking ELD courses, which reduces the time for ELA classes that students need to satisfy college entrance requirements (Gándara & Orfield, 2012; Lillie, Markos, Arias, & Wiley, 2012; Umansky, 2016a). Similarly, ELs experience restricted access to other core content courses such as math and science, which leaves them unprepared or unqualified to enter four-year colleges (Kanno & Kangas, 2014; Umansky, 2014). Qualitative studies have found that educators tend to have low academic expectations for ELs (Callahan & Gándara, 2004). The intention to protect ELs from difficult academic materials creates an equity trap as English proficiency is equated with intelligence (Ream, 2003). Well-meaning teachers and counselors who view language proficiency as an entrance requirement for rigorous academic coursework may recommend ELs for lower, remedial tracks within their school. As a result, ELs interact mainly with low-achieving peers, which further impedes their academic progress (Callahan, 2005). Such leveled tracking is a major barrier to accessing not only academic content but also high-quality classroom language and discourse (Harklau, 1994; Raudenbush, Rowan, & Cheong, 1993; Umansky, 2016b; Valdés, 2004). Due to fewer incidences of instruction in higher-order thinking in remedial track classes, ELs may lose opportunities to develop problem-solving and critical analysis skills that are crucial to college and career success.

ELs in Secondary School

Academic access inequities become more pronounced in secondary school, where tracking begins and instruction becomes more differentiated. Qualitative and descriptive studies brought equity issues in secondary education to the forefront, showing that EL status may limit high school students' access to rigorous academic content (e.g., Callahan, 2005; Callahan & Humphries, 2016; Callahan & Shifrer, 2016; Estrada, 2014; Kanno & Kangas, 2014; Oakes, 1985). Research on the causal relation between EL status and secondary school access and achievement yielded mixed findings. One study found that marginal students who classified as ELs by barely missing the cutoff score in kindergarten enrolled in fewer core content classes, such as math and science (Umansky, 2016a). Another found that EL classification had weak positive effects on middle and high school academic performance (Shin, 2018). A third, which focused on the effect of reclassification, showed that students who barely missed the cutoff score in 10th grade and remained ELs scored lower on the ACT and had lower high school graduation and immediate college enrollment rates (Carlson & Knowles, 2016). Last, initial EL classification and 8th-grade reclassification were shown to have no significant impact on high school test scores or graduation rates (Johnson, 2019a, 2019b).

California's Assembly Bill 2735, signed in 2018, prohibits secondary schools from excluding EL participation in the schools' standard instructional programs (California Legislative Information, 2018). Research has shown that increases in high school curricular standards can close racial gaps in economic outcomes (e.g., Goodman, 2018). Yet the impact of similar policies on secondary school ELs remains to be explored.

Expanded Learning Opportunities

Some of the barriers to EL academic access stem from tracking, while others are due to resource limitations. Schools face many challenges in simultaneously delivering high-quality academic content and tailored language support to individual students, especially in districts with linguistically diverse student populations (Callahan & Shifrer, 2016). Content teaching and learning in the students' home language may be a promising solution (e.g., Mayer, 2012), but operationalization is difficult when students' home languages differ. In addition, schools continue to face the trade-off between providing specialized services and segregating students by linguistic background (Callahan & Shifrer, 2016). Compared with younger children, students in middle and high school require even more guided practice in academic reading and writing. Given the limited number of instruction hours, it is difficult to master the teaching and learning of both language and academic content during the regular school year. Research thus points to expanded learning opportunities—instruction that occurs after school, on weekends, or during vacations—as a potential for policy interventions (Harris, 2009).

The importance of summer learning to student achievement is well documented (e.g., Atteberry & McEachin, 2016; Gershenson, 2013; Gershenson & Hayes, 2018). A number of studies have explored the effectiveness of summer and after-school programs for fluent English speakers. For instance, there is evidence that mandatory summer school improves students' achievement in math and English (Matsudaira, 2008). Voluntary summer learning has been shown to increase math and, after two summers of participation, ELA achievement among low-income elementary school students (Augustine et al., 2016). Summer math and science remediation has had positive impacts on achievement (e.g., Jacob & Lefgren, 2004; Knox, Moynihan, & Markowitz, 2003). Case studies on online credit recovery also suggest that high school students perceive out-of-school instruction to be a positive and motivating learning experience (e.g., Bostick, 2012; D'Agustino, 2011; Jones, 2011). However, little is currently known about the impact of expanding learning opportunities to summer programs on ELs. Course-taking opportunities will likely become key to expanding secondary school access as the EL population continues to grow. In some states, as many as every one in three ELs is enrolled in 7th to 12th grade (Callahan, 2005; Callahan & Humphries, 2016). By examining the causal impact of summer credit recovery on high school ELs, this article addresses these important gaps in existing research.

EL Summer School

Data for this study come from a large urban school district in California. About a quarter of all students enrolled in the district are ELs. At the high school level, ELs constitute between 12% and 20% of each grade from 9th to 12th and more than one third of students who delay graduation. In the district, students who delay graduation are permitted to continue enrollment. EL Summer School (ELSS) was introduced in the summer of 2013 to provide newcomers with opportunities to earn academic credit, develop English communicative competence, and build supportive communities. ELSS offered free 5-week content courses in ELA, math, science, and social science, which met the district's high school graduation requirements. All courses were taught by certified teachers already employed in the district. Classes met 5 hours a day, 5 days a week. Students typically enrolled in one course per summer and earned credits equivalent to one yearlong course. About 86% of enrollment was in ELD, English literature, or English composition courses—all of which met ELA requirements. Science courses constituted 6% of enrollment, math 5%, and others subjects 3%. The program was designed for newly arrived immigrants who had developed academic literacy in their home language but had low levels of English proficiency. Distinct from longer-term ELs, newcomers are more likely to be prepared for advanced content materials but, as a result of low English proficiency, they take few content courses before the end of 12th grade. The program thus aimed to enable newcomers to recover credit and subsequently enroll in content courses.

To be eligible for the program, an EL student needed to have arrived in the United States less than 3 years prior to the calendar year of the summer program and be a rising 10th, 11th, or 12th grader in the district. Participation in ELSS was voluntary, but a strong first-stage estimate (shown in Appendix Table OA1 in the online version of this article) indicates that eligibility significantly induced enrollment. Registration for each summer took place on a school day during the spring semester. During the first year, 2013, students registered in person at a designated office in their respective high schools. In subsequent years, academic counselors at each high school registered on behalf of the students. Each summer, the program was oversubscribed, and registration priority was given to rising 12th and 11th graders over rising 10th graders. Counselors also targeted ELs with very low academic achievement or English proficiency and permitted some ELs to enroll even after their third year in the United States. This resulted in some noncompliance to the eligibility rule (15.8% of course enrollment was by ineligible ELs).

Sample Selection

I start with an administrative data set containing the demographic, course-taking, graduation, and English proficiency test score data of students who enrolled in any middle or high school in the district between school years 2005–2006 and 2015–2016. From these records, I drop students who entered ninth grade before the fall of 2005 and after the summer of 2013, because the data set does not contain 4 full years of high school course-taking data for these students. I then drop students who did not attend high school in the district. This procedure yields a sample of 40,651 students scheduled to graduate, after 4 years of high school, between 2009 and 2016 (cohorts 2009–2016). There are a total of 17 high schools and a total of 132 cohort-school clusters. Of the 1,371 eligible students, 464, or approximately 34%, took up treatment by enrolling in ELSS. About 86% of the participants enrolled for one summer, and 13% enrolled for two summers.

Table 1, Panel A provides the descriptive statistics for the sample. About 45% of the eligible students were female. About half of the eligible students were Chinese (Mandarin or Cantonese) speakers, and just above a quarter were Spanish speakers. The average age of the eligible students was 18.75 years in June of their cohort's graduation year. Auxiliary regression results, estimated using the difference-in-differences-in-differences (DDD) method described in the next section, are presented in Table 1, Panel B. The estimates indicate that students who were eligible for ELSS were similar to other students in terms of sex, home language, ninth-grade ELA and math GPA, ninth-grade attendance, and eighth-grade math achievement. However, the eligible students were approximately 0.17 years older and had significantly lower (0.68 *SD*) eighth-grade ELA achievement and marginally significantly lower ninth-grade overall GPA (0.31 point).

Outcomes

ELSS intends to improve students' academic outcomes by offering access to core academic content, providing instruction tailored to the needs of newcomer ELs, and facilitating English language development through academic and social interactions. If the program has been effective in expanding access, we would expect participants to enroll in more core academic courses in their first 4 years of high school compared with nonparticipant peers. Since the program provides opportunities to interact with teachers and classmates in English, we might also expect participants to have higher English proficiency levels in the academic year following program eligibility, as measured by the California English Language Development Test (CELDT). To the extent that ELSS builds a cohesive academic community and encourages students to stay in school, participants may show higher levels of engagement, as manifested by a lower probability of dropping out in the subsequent academic years (Balfanz, Herzog, & MacIver, 2007). Ultimately, we would expect participants to graduate from high school at higher rates compared with their nonparticipant peers.

To estimate the program's impact on academic access, I focus on course taking as the primary outcome of interest because it captures both the level and the progression of academic preparation (Callahan & Shifrer, 2016). I measure course taking using two sets of outcomes: the total number of

Table 1
Sample Characteristics

	Non-EL characteristics (N = 20,031)				EL characteristics (N = 20,620)				Eligible students	
	Cohorts 2009–2013 (Post=0)		Cohorts 2014–2016 (Post==1)		Cohorts 2009–2013 (Post=0)		Cohorts 2014–2016 (Post==1)		(Newcomer ELs in Cohorts 2014–2016)	
Panel A: Summary Statistics	Newcomer	Oldcomer	Newcomer	Oldcomer	Newcomer	Oldcomer	Newcomer	Oldcomer	Participants	Nonparticipants
Female	0.486	0.492	0.568	0.500	0.448	0.480	0.447	0.476	0.430	0.454
Age (years, at the end of 12th grade)	18.142	18.260	18.016	18.161	18.687	18.192	18.752	18.149	18.790	18.737
Chinese/Cantonese	0.274	0.028	0.216	0.026	0.521	0.140	0.507	0.211	0.522	0.501
Spanish	0.125	0.016	0.099	0.021	0.255	0.200	0.252	0.201	0.266	0.246
Ninth-grade GPA	2.780	2.404	3.402	2.689	2.431	2.585	2.851	2.789	2.562	2.906
Ninth-grade ELA GPA	3.066	2.902	3.685	3.183	2.976	3.057	3.439	3.238	3.326	3.476
Ninth-grade math GPA	3.507	2.933	4.000	3.128	3.000	3.155	3.241	3.299	3.027	3.271
Ninth-grade %attended	94.827	88.191	98.240	92.599	94.106	92.544	96.600	95.611	97.315	96.468
Eighth-grade ELA test	−0.976	−0.090	0.181	0.320	−1.240	−0.157	−1.161	0.080	–1.446	−1.112
Eighth-grade math test	0.450	0.368	1.027	0.491	0.487	0.719	0.788	0.692	0.206	0.889
N	376	13,093	111	6,451	3,251	10,181	1,371	5,817	464	907

Panel B: DDD Auxiliary Regression Results

	Female	Age (years)	Chinese/ Cantonese	Spanish	Ninth-Grade GPA	Ninth-Grade ELA GPA	Ninth-Grade Math GPA	Ninth-Grade %Attended	Eighth-Grade ELA test	Eighth-Grade Math test
EL × newcomer × post	–0.072	0.169**	–0.025	–0.001	–0.308*	–0.063	–0.189	0.590	–0.675***	0.113
	(0.064)	(0.085)	(0.057)	(0.051)	(0.177)	(0.253)	(0.347)	(1.968)	(0.167)	(0.201)
N	40,651	40,651	40,651	40,651	34,717	6,628	4,185	35,532	26,391	26,391

Panel C: Post/Pre EL Newcomer Covariate Balance

	Female	Age (years)	Chinese/ Cantonese	Spanish	Ninth-Grade GPA	Ninth-Grade ELA GPA	Ninth-Grade Math GPA	Ninth-Grade %Attended	Eighth-Grade ELA Test	Eighth-Grade Math Test
Post/pre difference	−.001	.064	−.014	−.003	.420	.462	.242	2.494	.078	.301
t-Test p value	.932	.024	.390	.828	.000	.000	.000	.000	.082	.002

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. GPA, attendance, and test score means are reported for students with no missing data. GPA is calculated on a 4.0 scale. "%attended" refers to the number of classes attended divided by the total number of classes offered. Eighth-grade test scores are standardized using the state mean and the standard deviation. Eighty-five, or 1.46% of, oldcomer ELs in the post period participated in the summer program. GPA = grade point average; EL = English learner; ELA = English Language Arts; DDD = difference-in-differences-in-differences.
*p < .1. **p < .05. ***p < .01.

courses taken and the completion of courses that meet University of California and California State University A–G entrance requirements (hereafter "UC requirements"). The total number of courses measures overall access to academic content, while completion of California 4-year college entrance requirement courses captures access to and preparation for rigorous college-preparatory courses. I focus on ELA, math, science, and social science courses, which correspond to the subjects for Categories A–D of the UC requirements.[3] I also look at all other courses as a pooled group ("electives") and report the results in the online appendix.

To create outcomes that are comparable across all students, I include courses in which students enrolled during their first 4 years of high school. For total course count within a subject, I count the first attempt at unique courses in each subject, regardless of the final grade. This is a measure of students' exposure to academic content, unconditional on a student's own performance. For completion of UC requirements, I count the number of unique courses in each subject for which students received 1 year of credit.[4] As an alternative measure, I create an indicator for having completed at least one UC requirement course in each subject. Finally, I create an indicator for having completed at least one UC requirement course in all four subjects and another indicator for having completed all four UC A–D requirements.[5]

ELSS can affect access by allowing students to take classes in the summer and freeing up class periods during the subsequent academic years. If students enroll in an ELSS math class during the summer in addition to math classes during the subsequent years, then the net effect on math enrollment would be positive. However, if students enroll in summer math and then replace what would have been a math class in their subsequent academic year schedule with a nonacademic elective, the net effect on math enrollment would be zero. To distinguish between the effects on enrollment during the four summers and the four academic years, I also analyze these two outcomes separately.

In addition to course taking, I examine graduation and dropout rates and English proficiency. Graduation is an indicator that takes on a value of 1 for having graduated before September of the intended year. Prior literature (see Murnane, 2013, for a review) focuses almost exclusively on 4-year graduation rates. In the context of this district, however, newcomer ELs are expected to graduate within 5 years. Therefore, I include 5-year graduation as an outcome.[6] As a measure of disengagement from school, I also estimate the probability of dropping out during any year following the cohort's program eligibility. The dropout indicator comes from students' graduation records and takes a value of 1 if a student is confirmed to have left the district without continuing her or his education elsewhere. Dropouts differ from students missing graduation records in that dropout status is verified while students' missing records could have been transferred but lacked a verified status.

A common concern for credit recovery programs aimed at raising graduation rates is the potential for social promotion without learning gains. To address this concern, I test if the program led to higher levels of English proficiency. The development of language and cognitive skills during the teenage years differs considerably from that in earlier grades (Berman, 2004). Yet few studies have examined the effect of interventions on secondary school ELs' language proficiency. Program impact on English proficiency has important labor market implications. Human capital theory asserts that additional schooling results in higher-level skills, which can be exchanged for higher earnings (Becker, 1962). Consistent with this theory, extant research (e.g., Gándara & Callahan, 2014) shows that English proficiency is positively associated with labor market outcomes among Spanish-speaking workers. If the intervention results in higher levels of English proficiency, it could potentially impact students' future earnings through these skill gains, in addition to affecting students' probability of getting a high school diploma. For these reasons, I examine English proficiency as a measure of human capital accrued to ELs as a result of the intervention.

English proficiency, as measured by CELDT performance in the year following program eligibility and on reclassification during any year following eligibility, provides suggestive evidence for program impact on student learning. CELDT scores are standardized using state grade-level means and standard deviations from the test year 2006–2007. In order to account for initial differences in proficiency level, I control for students' CELDT score in the year prior to program eligibility in the regression with the posteligibility CELDT score as the outcome. Test score data were unavailable for some students in the sample because of CELDT attrition. Once classified as ELs, students took the CELDT annually until they are reclassified and exit EL services. Thus, CELDT scores are only available for students who remain ELs.[7] Since the vast majority of newcomers remain ELs until their third or fourth year in high school, the results are still informative despite this data limitation. To examine the program's effect on students with relatively higher English proficiency, I also look at the probability of reclassifying after the first summer of program eligibility.

Research Design

Since participation in ELSS was voluntary, directly comparing the subsequent academic outcomes of participants with those of nonparticipants may produce biased results due to unobserved factors. In fact, academic counselors reported targeting students most in need of support during the spring registration process. Compared with their peers, these lower-performing students may have achieved lower levels of academic outcomes even in the absence of the program. Estimation that does not account for unobserved differences may result in biased findings. To address this potential bias, I estimate the intent-to-treat (ITT) effect, employing a DDD design. The magnitude of the DDD estimate informs us of the expected impact of implementing a similar summer program at take-up rates similar to that of ELSS.

The DDD design seeks to identify the causal impact of ELSS by differencing the levels of student outcomes observed before and after ELSS was implemented and accounting for secular trends. This quasi-experimental approach is built on a difference-in-differences (DiD) framework that mimics an experiment. In a randomized experiment, subjects are randomly assigned to the treatment group or the control group; causal impact can be estimated by taking the difference between the outcomes of the two groups as long as pretreatment characteristics were equivalent across the groups. The DiD approach allows the analysis of panel data in a way that is analogous to an experimental design by using subjects' program eligibility and their data from time periods prior to program implementation and after program implementation. By interacting program eligibility with observations in the postprogram period, we interpret the interaction effect as the causal impact of the program when preprogram outcome trends are parallel between the eligible and ineligible groups (Angrist & Pischke, 2008).

Eligibility rules for ELSS lend nicely to the construction of student groups to compare with newcomer ELs. Newcomers in the district were eligible to participate in the summer if they were ELs and rising 10th, 11th, or 12th graders. I leverage data on immigrant students who had fluent English proficiency and were not classified as EL on entering the district. These students directly enrolled in monolingual English classes, never received EL services, and were never eligible for ELSS; however, they did enter the United States at the same time, attended the same schools, and were subject to the same district policies as the treated group. Given their shared newcomer status, we would expect immigrant non-ELs to have faced many of the same physical, social, and academic challenges associated with moving to a new country and attending a new school as their EL peers. Newcomer ELs in cohorts 2014 through 2016 were eligible, while immigrant non-ELs and students in cohorts 2013 or earlier (who had graduated before the program was implemented) were ineligible. The intersection of EL status (EL) and postprogram graduating cohort (Post) identifies eligibility within the population of newcomers, which allows me to compare outcomes using the DiD framework by applying the following model to newcomers in the sample:

$$\text{Outcome}_{ics} = \beta_0 + \beta_1 \text{EL}_i + \beta_2 \text{Post}_c + \beta_3 \text{EL}_i \times \text{Post}_c + \boldsymbol{\chi}_i + \delta_{cs} + \varepsilon_{ics}, \tag{1}$$

in which for student i in cohort c starting at high school s,

Post = 1 if the student was in cohorts 2014–2016;
EL = 1 if the student was an EL;
$\boldsymbol{\chi}$ is a vector of student covariates, including sex, age, home language, and prior test score where appropriate;
δ denotes cohort by first high school fixed effects;

ε represents errors clustered at the cohort-school level; and
β_3 is the coefficient of interest providing the effects of ELSS.

The DiD estimate is informative regarding the relative performance within the newcomer group, but it has two shortcomings. First, if characteristics unique to ELs in postprogram cohorts had contributed to differential performance, the DiD design would not properly identify these effects. Second, DiD requires EL and non-EL outcome trends in the years prior to ELSS implementation to be parallel. If this "common trends" assumption is violated, the resulting estimate would be prone to bias (Angrist & Pischke, 2008).[8] One way to address these concerns is to construct a "naive" or placebo DiD using students who were never eligible for the program in any time period (Asim & Dee, 2016). When the common trends assumption holds for this naive DiD comparison, we can take the difference of the true DiD and the naive DiD to get a DDD estimate, which can then be interpreted as the causal effect.

To construct a naive DiD, I leverage data on oldcomers, students who have lived in the United States for 3 or more years. Applying Model 1 described above to oldcomers in the sample, I identify the effect of being an oldcomer in a postprogram cohort. I then obtain a triple-difference estimate by netting out this oldcomer DiD from the newcomer DiD. Figure A1 in the online appendix shows that the preprogram outcome trends for oldcomer ELs and non-ELs are parallel. The DiD estimates for newcomers and oldcomers are reported side by side in Table 2. Nonsignificant oldcomer DiD estimates suggest that the program had no effect on oldcomers, indicating that this is an appropriate naive DiD for the construction of the DDD model.

The DDD approach isolates the ITT effect of ELSS under weaker assumptions than the DiD approach, at the cost of larger standard errors. The DDD design requires that stable unit treatment value assumptions hold. In other words, the composition of both treated and nontreated groups must be stable across time, and there must be no spillover effects. In the context of this study, spillover effects are unlikely as ELs and non-ELs are placed in separate classrooms. Until reclassification, ELs enroll in ELD and sheltered academic courses, while non-ELs take general education courses.

To obtain DDD estimates, I run the following baseline model on data on oldcomer and newcomer ELs and non-ELs:

$$\begin{aligned}\text{Outcome}_{ics} = {} & \beta_0 + \beta_1\text{EL}_i + \beta_2\text{Post}_c + \beta_3\text{Newcomer}_i + \beta_4\text{EL}_i \times \text{Post}_c + \beta_5\text{EL}_i \times \text{Newcomer}_i \\ & + \beta_6\text{Post}_c \times \text{Newcomer}_i + \beta_7\text{EL}_i \times \text{Post}_c \times \text{Newcomer}_i + \boldsymbol{\chi}_i + \delta_{cs} + \varepsilon_{ics},\end{aligned} \tag{2}$$

in which for student i in cohort c starting at high school s,

EL = 1 if the student is classified as EL on entering the district (i.e., not initially English proficient);

Table 2

Difference-in-Differences Estimates for the Effects of ELSS on Course Taking and Graduation

	Total Courses Taken				Graduation		
	(1) ELA	(2) Math	(3) Science	(4) Social Science	(5) 4-Year Graduation	(6) 5-Year Graduation	(7) Dropped Out
Newcomer DiD (EL × Post)	1.134***	0.479***	0.486**	0.309**	–0.042	0.003	0.007
	(0.282)	(0.173)	(0.194)	(0.126)	(0.046)	(0.017)	(0.015)
Observations	5,109	5,109	5,109	5,109	3,513	3,513	5,109
Adjusted R^2	.165	.134	.203	.117	.115	.0895	.153
Oldcomer DiD (EL × Post)	−0.020	0.049	−0.027	−0.020	−0.007	−0.005	0.051***
	(0.065)	(0.053)	(0.061)	(0.045)	(0.007)	(0.006)	(0.007)
Observations	35,542	35,542	35,542	35,542	26,159	26,159	35,542
Adjusted R^2	.171	.242	.218	.195	.160	.108	.090

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are obtained from DiD models (described in Equation 1) using students in cohorts expected to graduate between 2009 and 2016. Samples for columns 1 to 4 and 7 include students with missing graduation outcomes data. Samples for columns 5 and 6 include students with graduation outcomes data. "Graduation" refers to graduating with a regular high school diploma. "Dropped out" refers to confirmed early departure from the district after the year of program eligibility and having a state leave code indicating dropping out. The model includes cohort by first high school fixed effects. EL = English learner; ELSS = EL Summer School; ELA = English Language Arts; DiD = difference-in-differences.

*$p < .1$. **$p < .05$. ***$p < .01$.

Post = 1 if the student is in cohorts 2014–2016;
Newcomer = 1 if the student was a newcomer for the summer students' cohort and became eligible for ELSS;
$\boldsymbol{\chi}$ is a vector of student covariates, including sex, age, home language, and prior test score where appropriate;
δ denotes cohort by first high school fixed effects;
ε represents errors clustered at the cohort-school level; and
β_7 is the coefficient of interest providing the causal impact on student outcome.

The DDD model is applicable to course-taking and graduation outcomes but not to English proficiency. This is because English proficiency is assessed only once for non-ELs, at school entry, and no postprogram outcome is observed. For this reason, to estimate program effects on English proficiency, I apply the following DiD model to newcomer and oldcomer ELs in the sample:

$$\text{Outcome}_{ics} = \beta_0 + \beta_1 \text{Newcomer}_i + \beta_2 \text{Post}_c + \beta_3 \text{Newcomer}_i \times \text{Post}_c + \boldsymbol{\chi}_i + \delta_{cs} + \varepsilon_{ics}. \quad (3)$$

To test the robustness of my findings, I run several alternative model specifications. First, I check if clustered and unclustered standard errors differ (see the online Appendix Table OA8). I then compare the results from models with cohort by school fixed effects with the results from models with school fixed effects (online Appendix Tables OA9–OA13). Finally, I interact cohort with the EL and newcomer indicators to test if treatment effect was different for cohorts 2014, 2015, and 2016.

Prior research suggests that ELs' response to instructional programs may differ by home language and other background characteristics (e.g., Valentino & Reardon, 2015). Since the literature on EL interventions outside the school year is limited, the expected direction of effect heterogeneity is unclear. We might expect boys, who on average have higher high school dropout rates (National Center for Education Statistics, 2019b), to benefit more from ELSS than girls because ELSS provides a credit recovery opportunity that had not been available before. On the other hand, girls may be better positioned to benefit if they are more likely to attend more days of summer instruction. The student demographics in this district are unique in that in addition to a large group of Spanish users, there is also a substantial Chinese user population, who are higher performing than other language subgroups. ELSS could differentially affect high-achieving Chinese users if the curriculum matches their needs especially well. To test for heterogeneity of treatment effects, I interact eligibility with students' sex and home language. I also report subgroup results separately in the online appendix.

In 2008, the district adopted UC requirements as high school graduation requirements, applicable to graduating classes of 2014 onward. Since ELSS was first implemented in the summer of 2013, the class of 2014 was the first cohort to be affected by both the new graduation requirements and ELSS. Changes during the transition year may have differentially impacted newcomer ELs eligible for ELSS. To test the sensitivity of my findings, I also run the above analyses excluding the class of 2014 from my sample (see the online Appendix Table OA18).

Findings

My preferred model to estimate the ITT effect of ELSS on students' academic outcomes is the DDD approach. The pre- and postprogram differences of newcomer EL, newcomer non-EL, oldcomer EL, and oldcomer non-EL students are summarized in the online Appendix Figure OA3. Table 2 reports the DiD estimates for newcomers and oldcomers in separate rows. The online Appendix Table OA17 reports the DiD estimates for ELs only. In the rest of this section, I focus on DDD estimates for course taking and graduation, as well as DiD estimates for CELDT, which are only available for ELs. For each outcome, "post newcomer EL mean" represents the average outcome for newcomer ELs in cohorts 2014, 2015, and 2016. The results shown are from the preferred specification, which includes cohort by school fixed effects, though estimates are robust across model specifications (see the online Appendix Tables OA8–OA18, OA20). I also test for treatment heterogeneity by cohort and report the interaction coefficients when differences are significant.

Course Taking

On average, eligible students enrolled in 4.513 ELA classes, 2.692 math classes, 2.341 science classes, and 2.424 social science classes from 9th to 12th grade, while the district required the completion of 4 ELA, 3 math, 2 science, and 3 social science classes for graduation (Table 3, Panel A). In other words, the average newcomer EL student in post-ELSS cohorts did not even *attempt* the number of math or social science courses needed to meet graduation requirements.

ELSS had significant positive effects on the number of ELA and math courses ELs took during their first 4 years of high school. Eligible students gained an average of 1.156 ELA classes and 0.392 math classes (Table 3, Panel A). When I disaggregate summer and academic year enrollment, I find that ELSS led to an increase of 0.304 ELA classes and 0.074 math classes during the summer sessions (Table 3, Panel B) and an increase of 0.852 ELA classes and 0.317 math classes during the academic years (Table 3, Panel C). This suggests that the gain in access was not limited to program enrollment alone. ELSS afforded students access to more ELA and math courses not only during summer but also during fall and spring. In contrast, newcomer ELs did not enroll in a significant number of additional science or social science courses during summers. However, the increases in academic year and total

Table 3
Estimated Effects on Course Taking (DDD Model, Full Sample)

	ELA (Need 4) (1)	Math (Need 3) (2)	Science (Need 2) (3)	Social Science (Need 3) (4)
Panel A: Total number of classes taken in the first 4 years				
Newcomer × EL × post	1.156***	0.392**	0.425**	0.267**
	(0.266)	(0.157)	(0.167)	0.115)
Post newcomer EL mean	4.513	2.692	2.341	2.424
Adjusted R^2	.179	.231	.245	.192
Panel B: Total number of classes taken during summer				
Newcomer × EL × post	0.304***	0.074***	−0.003	0.022
	(0.061)	(0.026)	(0.021)	(0.014)
Post newcomer EL mean	0.492	0.095	0.034	0.020
Adjusted R^2	.069	.037	.026	.016
Panel C: Total number of classes taken during the academic year				
Newcomer × EL × post	0.852***	0.317**	0.427**	0.246**
	(0.248)	(0.157)	(0.169)	(0.115)
Post newcomer EL mean	4.021	2.597	2.307	2.403
Adjusted R^2	.178	.242	.249	.198
Observations	40,651	40,651	40,651	40,651

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. The number of courses in each subject needed for graduation are given in parentheses in the column heads. Estimates are obtained from DDD models (described in Equation 2) using students in cohorts expected to graduate between 2009 and 2016, including students with missing graduation outcomes data. The model includes cohort by first high school fixed effects. EL = English learner; ELA = English Language Arts; DDD = difference-in-differences-in-differences.
$*p < .1$. $**p < .05$. $***p < .01$.

science and social science enrollment were all substantial and statistically significant.

UC Requirements

ELSS had limited effects on the completion of UC requirements. As shown in Table 4, on average, newcomer EL students in postprogram cohorts completed 2.896 out of 4 ELA classes, 2.071 out of 3 math classes, 1.957 out of 2 science classes, and 1.940 out of 2 social science classes. On the extensive margin, the program had no significant impact on the probability of completing at least 1 UC requirement in any subject after controlling for cohort by school fixed effects (Table 5, Panel A). The estimate on completing at least 1 UC math requirement is a marginally significant 9.1 percentage points. On the intensive margin, eligible students gained 0.643

Table 4
Estimated Effects on the Number of UC/CSU Entrance Requirement Courses Completed (DDD Estimates)

	ELA (Need 4) (1)	Math (Need 3) (2)	Science (Need 2) (3)	Social Science (Need 3) (4)
Panel A: Full sample				
Newcomer × EL × post	0.643**	0.219	0.263	0.169
	(0.173)	(0.145)	(0.165)	(0.104)
Post newcomer EL mean	2.896	2.071	1.957	1.940
Adjusted R^2	.293	.307	.308	.240
Observations	40,651	40,651	40,651	40,651
Panel B: Graduation sample				
Newcomer × EL × post	0.787**	0.359**	0.360**	0.175*
	(0.176)	(0.149)	(0.181)	(0.091)
Post newcomer EL mean	3.584	2.527	2.357	2.270
Adjusted R^2	.424	.221	.224	.185
Observations	29,672	29,672	29,672	29,672

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. The number of courses in each subject needed for graduation are given in parentheses in the column heads. Estimates are obtained from DDD models described in Equation 2. Panel A includes students in cohorts expected to graduate between 2009 and 2016, including students with missing graduation outcomes data. Panel B includes students expected to graduate between 2009 and 2016 with graduation outcomes data. The model includes cohort by first high school fixed effects. EL = English learner; ELA = English Language Arts; DDD = difference-in-differences-in-differences; UC = University of California; CSU = California State University.
$^*p < .1$. $^{**}p < .05$. $^{***}p < .01$.

completed courses in ELA. The effects on the number of UC requirement math, science, and social science courses completed are not statistically significant (Table 4, Panel A). The impact on the probability of having completed at least 1 course in each category and the probability of completing all 11 required courses in the UC A–D categories by the end of the fourth year are also not significant (Table 5, Panel A).

Graduation

Estimates in Table 6 provide suggestive evidence on the effects of ELSS on 4- and 5-year graduation. Approximately 82.5% of program-eligible students in cohorts 2014, 2015, and 2016 graduated in 4 years, and 97.6% graduated in 5 years (Panel A). The impact on the 4-year graduation rate was −6.1 percentage points and not statistically significant. The 5-year graduation rate increased by 2.7 percentage points, which is imprecisely estimated after controlling for cohort by school fixed effects. No newcomer ELs in the

Table 5

Estimated Effects on the Probability of Completing UC/CSU Entrance Requirement Courses (DDD Estimates)

	At Least One ELA (1)	At Least One Math (2)	At Least One Science (3)	At Least One Social Science (4)	Completed at Least One Course in Each Category (5)	Completed All A–D Courses (6)
Panel A: Full sample						
Newcomer × EL × post	0.071 (0.045)	0.091* (0.046)	0.031 (0.052)	0.016 (0.041)	0.043 (0.057)	0.017 (0.043)
Post newcomer EL mean	0.865	0.858	0.845	0.877	0.747	0.232
Adjusted R^2	.181	.204	.188	.145	.225	.161
Observations	40,651	40,651	40,651	40,651	40,651	40,651
Panel B: Graduation sample						
Newcomer × EL × post	0.019 (0.026)	0.086** (0.034)	0.019 (0.043)	0.027 (0.028)	0.036 (0.054)	0.036 (0.055)
Post newcomer EL mean	0.987	0.977	0.936	0.989	0.906	0.307
Adjusted R^2	.045	.078	.065	.040	.097	.189
Observations	29,672	29,672	29,672	29,672	29,672	29,672

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are obtained from DDD models described in Equation 2. Panel A includes students in cohorts expected to graduate between 2009 and 2016, including students with missing graduation outcomes data. Panel B includes students expected to graduate between 2009 and 2016 with graduation outcomes data. The model includes cohort by first high school fixed effects. Dependent variables were assigned a value of 1 if the student completed the courses, 0 otherwise. EL = English learner; ELA = English Language Arts; DDD = difference-in-differences-in-differences; UC = University of California; CSU = California State University.

*$p < .1$. **$p < .05$. ***$p < .01$.

Table 6
Estimated Effects on Graduation and Dropout (DDD Estimates)

Graduation Sample	4-Year Graduation (1)	5-Year Graduation (2)	Dropped Out (3)
Newcomer × EL × post	−0.061 (0.046)	0.027 (0.019)	−0.039 (0.024)
Post newcomer EL mean	0.825	0.976	0
Adjusted R^2	.159	.101	.081
Observations	29,672	29,672	29,672

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are obtained from DDD models described in Equation 2. The model includes cohort by first high school fixed effects. "Graduation" refers to graduating with a regular high school diploma. "Dropped out" refers to confirmed early departure from the district after the year of program eligibility and having a state leave code indicating dropping out. The sample includes students expected to graduate between 2009 and 2016 with graduation outcomes data. EL = English learner; DDD = difference-in-differences-in-differences.

postprogram period dropped out of high school after becoming eligible for ELSS. The estimate on dropping out is negative and not significant. This DDD estimate should be interpreted with caution because the naive DiD for oldcomers was positive and significant.

English Proficiency

DiD estimates on EL students in the sample suggest that ELSS led to significant improvements in English proficiency, as measured by the CELDT overall and section scores. As can be seen in Table 7, on average, newcomer ELs in the postprogram period scored −0.779 *SD* on overall English proficiency. ELSS resulted in a significant increase of 0.127 *SD* in overall proficiency, as well as significant improvements in listening (0.178 *SD*), speaking (0.161 *SD*), and writing (0.113 *SD*). However, ELSS had a small and not significant negative impact on reading and no effect on the probability of reclassification.

Heterogeneity by Cohort, Sex, and Home Language

Program effect heterogeneity was estimated using the preferred DDD specification with cohort by school fixed effects. As shown in Tables 8 to 11, estimated program impact on overall courses taken and completion of UC requirements differed little by cohort, sex, and home language. Estimates are presented separately for boys, girls, Chinese users, and Spanish users in the online Appendix Tables OA4 to OA7 and are qualitatively similar to those reported above. However, there is considerable heterogeneity in the effect on graduation rates by cohort. Both 4- and 5-year graduation declined for the cohort of 2014, while the interaction effects on

Table 7
Impact on CELDT Scores (Difference-in-Differences Estimates)

	Standardized CELDT Scores					Reclassified
	Listening	Speaking	Reading	Writing	Overall	Posteligibility
Newcomer × post	0.178**	0.161***	−0.070	0.113**	0.127**	0.004
	(0.069)	(0.050)	(0.055)	(0.046)	(0.055)	(0.030)
Post newcomer EL mean	−0.675	−0.873	−0.492	−0.521	−0.779	0.291
Adjusted R^2	.515	.711	.542	.539	.698	.198
Observations	8,518	8,518	8,518	8,518	8,518	9,461

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are obtained using a difference-in-differences model, described in Equation 3. The model includes pre-eligibility test score controls and cohort by first high school fixed effects. Data include scores of students with overall, listening, speaking, reading, and writing scores in the year before eligibility and the year after eligibility. "Reclassified posteligibility" refers to having been reclassified between the fall after the cohort would have become eligible for ELSS and high school graduation. The reclassification sample includes students who were ELs as of the summer cohort and would have become eligible for ELSS. CELDT = California English Language Development Test; EL = English learner; ELSS = EL Summer School.
$^*p < .1$. $^{**}p < .05$. $^{***}p < .01$.

the cohorts of 2015 and 2016 were positive and significant (Table 10, Panel A). The differential effects on Spanish users' English proficiency and reclassification were also negative and significant (Table 11, Panel D).

Robustness Checks

To test the sensitivity of my results to model specifications, I compare estimates from the preferred model with cohort by school fixed effects and those from a model with school fixed effects. Estimates and significance are stable. The results are reported in the online Appendix Tables OA9–OA17.

Since the 4- and 5-year graduation indicators are only available for 73% of the sample, I also run the estimations on a restricted sample that has graduation outcomes data (Tables 4–6, Panel B; online Appendix Tables OA10–OA13). Students in this restricted sample enrolled in slightly more courses on average than those in the full sample, which is to be expected since they stayed enrolled in the district. Coefficients on the graduation sample are slightly higher in magnitude but similar to those on the full sample.

Finally, I test the sensitivity of my results to the exclusion of the cohort of 2014, who had only one summer of program eligibility. Results are similar to those obtained using the full and graduation samples (online Appendix Tables OA17 and OA18).

Table 8

Effect Heterogeneity on Course Taking, by Cohort, Sex, and Home Language

	Total Number of Classes				Summer Classes				Academic Year Classes			
DDD Interaction	ELA	Math	Science	Social Science	ELA	Math	Science	Social Science	ELA	Math	Science	Social Science
Panel A: Cohort												
Newcomer × EL	1.947***	−0.168	0.219	0.032	0.407***	0.126***	0.039	0.038***	1.539***	−0.294	0.180	−0.006
	(0.373)	(0.251)	(0.295)	(0.190)	(0.113)	(0.043)	(0.040)	(0.011)	(0.293)	(0.263)	(0.294)	(0.194)
Newcomer × EL × 2015	−0.051	0.445	−0.188	0.160	0.048	−0.060	−0.013	−0.028	−0.099	0.505	−0.175	0.188
	(0.444)	(0.334)	(0.378)	(0.218)	(0.158)	(0.058)	(0.042)	(0.023)	(0.387)	(0.348)	(0.380)	(0.222)
Newcomer × EL × 2016	−0.656	0.318	−0.049	0.124	−0.160	−0.111**	−0.058	−0.029**	−0.496	0.429	0.009	0.153
	(0.446)	(0.307)	(0.335)	(0.237)	(0.122)	(0.049)	(0.043)	(0.013)	(0.378)	(0.318)	(0.333)	(0.242)
p value (2014 = 2015 = 2016)	.154	.398	.853	.764	.132	.070	.098	.067	.349	.298	.809	.698
Panel B: Cohort (pooled 2015/2016)												
Newcomer × EL	1.946***	−0.168	0.219	0.032	0.407***	0.126***	0.039	0.038***	1.539***	−0.294	0.181	−0.006
	(0.373)	(0.251)	(0.295)	(0.190)	(0.113)	(0.043)	(0.040)	(0.011)	(0.293)	(0.263)	(0.294)	(0.194)
Newcomer × EL × 2015/2016	−0.372	0.384	−0.109	0.144	−0.063	−0.086*	−0.036	−0.029*	−0.309	0.470	−0.072	0.174
	(0.417)	(0.287)	(0.331)	(0.209)	(0.132)	(0.050)	(0.041)	(0.016)	(0.342)	(0.299)	(0.331)	(0.213)
Panel C: Sex												
Eligibility (sex—main)	1.027***	0.105	0.293	0.159	0.188**	0.058	−0.025	−0.003	0.839***	0.047	0.318	0.162
	(0.335)	(0.197)	(0.215)	(0.172)	(0.073)	(0.037)	(0.035)	(0.018)	(0.317)	(0.199)	(0.216)	(0.171)
Eligibility × female	0.182	0.516*	0.195	0.137	0.204**	0.031	0.036	0.050**	−0.023	0.485	0.158	0.087
	(0.424)	(0.292)	(0.350)	(0.248)	(0.088)	(0.055)	(0.037)	(0.025)	(0.404)	(0.297)	(0.349)	(0.247)
Panel D: Home language												
Eligibility (language—main)	0.981***	0.528**	0.497**	0.307*	0.333***	0.079**	0.005	0.036*	0.648**	0.449**	0.492**	0.271*
	(0.282)	(0.214)	(0.244)	(0.159)	(0.071)	(0.033)	(0.029)	(0.019)	(0.259)	(0.211)	(0.243)	(0.163)
Eligibility × Chinese	0.150	−0.606	−0.614	−0.288	−0.170	−0.029	−0.007	−0.007	0.320	−0.577	−0.608	−0.280
	(0.541)	(0.421)	(0.472)	(0.275)	(0.126)	(0.076)	(0.046)	(0.034)	(0.503)	(0.413)	(0.467)	(0.275)
Eligibility × Spanish	0.301	−0.828*	0.132	0.029	−0.088	−0.037	0.082	0.026	0.389	−0.792*	0.050	0.003
	(0.801)	(0.469)	(0.581)	(0.584)	(0.196)	(0.124)	(0.058)	(0.053)	(0.742)	(0.473)	(0.569)	(0.586)
p value (Chinese = Spanish)	.834	.644	.176	.560	.714	.958	.118	.598	.918	.656	.227	.598
Observations	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are based on DDD models described in Equation 2. Class of 2014 is the omitted category in Panels A and B. Languages other than English, Chinese, and Spanish are grouped as the omitted category in Panel D. The model includes cohort by first high school fixed effects. "Eligibility" refers to newcomer ELs in postprogram cohorts. Additional separate subgroup analyses are reported in the online appendix. EL = English learner; ELA = English Language Arts; DDD = difference-in-differences-in-differences.

**p* < .1. ***p* < .05. ****p* < .01.

Table 9

Effect Heterogeneity on Completion of UC A–D Requirements, by Cohort, Sex, and Home Language

	Completed at Least One Course				Number of Completed Courses					
DDD Interaction	ELA	Math	Science	Social Science	ELA	Math	Science	Social Science	Completed One Each of A–D	Completed All, A–D
Panel A: Cohort										
Newcomer × EL	−0.043	−0.125	−0.068	−0.018	0.657***	−0.335	−0.182	−0.057	−0.108	0.056
	(0.075)	(0.077)	(0.102)	(0.081)	(0.238)	(0.211)	(0.243)	(0.168)	(0.097)	(0.053)
Newcomer × EL × 2015	0.061	0.146	0.045	−0.030	0.176	0.521**	0.002	0.103	0.093	0.064
	(0.099)	(0.096)	(0.131)	(0.092)	(0.319)	(0.256)	(0.320)	(0.192)	(0.124)	(0.071)
Newcomer × EL × 2016	0.005	0.082	0.075	−0.026	−0.429	0.084	0.215	0.146	0.065	−0.100
	(0.089)	(0.092)	(0.114)	(0.096)	(0.326)	(0.299)	(0.327)	(0.207)	(0.118)	(0.085)
p value (2014 = 2015 = 2016)	0.750	0.310	0.795	0.948	0.135	0.069	0.732	0.778	0.750	0.124
Panel B: Cohort (pooled 2015/2016)										
Newcomer × EL	−0.043	−0.125	−0.068	−0.018	0.657***	−0.335	−0.182	−0.057	−0.108	0.056
	(0.075)	(0.077)	(0.102)	(0.081)	(0.238)	(0.211)	(0.243)	(0.168)	(0.097)	(0.053)
Newcomer × EL × 2015/2016	0.031	0.113	0.061	−0.029	−0.142	0.298	0.116	0.126	0.079	−0.022
	(0.086)	(0.086)	(0.112)	(0.088)	(0.293)	(0.251)	(0.287)	(0.184)	(0.110)	(0.072)
Panel C: Sex										
Eligibility (sex—main)	−0.020	0.067	−0.017	−0.025	0.502**	0.104	0.10	0.115	−0.052	0.061
	(0.051)	(0.057)	(0.059)	(0.058)	(0.229)	(0.203)	(0.203)	(0.149)	(0.071)	(0.056)
Eligibility × female	0.145*	0.032	0.068	0.065	0.214	0.167	0.229	0.053	0.162	–0.088
	(0.076)	(0.082)	(0.082)	(0.087)	(0.308)	(0.284)	(0.309)	(0.212)	(0.105)	(0.066)
Panel D: Home language										
Eligibility (language—main)	0.102*	0.122**	0.065	0.028	0.714***	0.235	0.361	0.221	0.081	–0.028
	(0.056)	(0.058)	(0.070)	(0.052)	(0.225)	(0.197)	(0.234)	(0.140)	(0.079)	(0.054)
Eligibility × Chinese	−0.125	−0.040	−0.103	−0.050	−0.208	−0.198	−0.560	−0.233	–0.128	0.095
	(0.097)	(0.101)	(0.101)	(0.088)	(0.427)	(0.399)	(0.448)	(0.240)	(0.128)	(0.099)
Eligibility × Spanish	−0.027	−0.074	−0.049	0.079	−0.666	−0.452	−0.233	−0.043	–0.111	−0.019
	(0.180)	(0.200)	(0.200)	(0.190)	(0.580)	(0.432)	(0.604)	(0.532)	(0.192)	(0.145)
p value (Chinese = Spanish)	.577	.869	.790	.491	.414	.591	.552	.692	.929	.398
Observations	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651	40,651

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are based on DDD models described in Equation 2. Class of 2014 is the omitted category in Panels A and B. Male is the omitted category in Panel C. Languages other than English, Chinese, and Spanish are grouped as one, which is the omitted category in Panel D. "Eligibility" refers to newcomer ELs in postprogram cohorts. Additional separate subgroup analyses are reported in the online Appendix A. EL = English learner; ELA = English Language Arts; DDD = difference-in-differences-in-differences.

*$p < .1$. **$p < .05$. ***$p < .01$.

Table 10
Effect Heterogeneity on Graduation, by Cohort, Sex, and Home Language

DDD Interactions	4-Year Graduation	5-Year Graduation	Dropped Out
By cohort			
Panel A (3 separate cohorts)			
Newcomer × EL	−0.227***	−0.093***	0.013*
	(0.028)	(0.021)	(0.008)
Newcomer × EL × 2015	0.148**	0.085***	−0.010
	(0.068)	(0.024)	(0.008)
Newcomer × EL × 2016	0.114***	0.104***	−0.017**
	(0.037)	(0.022)	(0.008)
p value (2014 = 2015 = 2016)	.004	.000	.061
Panel B (pooled 2015/2016)			
Newcomer × EL	−0.227***	−0.093***	0.013*
	(0.028)	(0.021)	(0.008)
Newcomer × EL × 2015/2016	0.131***	0.095***	−0.013*
	(0.044)	(0.022)	(0.008)
By sex and home language			
Panel C			
Eligibility (sex—main)	−0.054	0.052**	−0.034*
	(0.046)	(0.024)	(0.020)
Eligibility × female	−0.019	−0.051	0.011
	(0.062)	(0.034)	(0.034)
Panel D			
Eligibility (language—main)	−0.077	0.026	−0.048
	(0.057)	(0.027)	(0.029)
Eligibility × Chinese	−0.048	−0.043	0.044
	(0.065)	(0.043)	(0.043)
Eligibility × Spanish	−0.004	0.009	0.059
	(0.170)	(0.071)	(0.052)
p value (Chinese = Spanish)	.781	.520	.787
Observations	29,672	29,672	40,651

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are based on DDD models described in Equation 2. "Graduation" refers to graduating with a regular high school diploma. "Dropped out" refers to confirmed early departure from the district after the year of program eligibility and having a state leave code indicating dropping out. The sample for 4- and 5-year graduation includes students expected to graduate between 2009 and 2016 with graduation outcomes data. The sample for dropout includes all students expected to graduate between 2009 and 2016. Class of 2014 is the omitted category in Panels A and B. Male is the omitted category in panel C. Languages other than English, Chinese, and Spanish are grouped as one, which is the omitted category in Panel D. "Eligibility" refers to newcomer ELs in postprogram cohorts. Additional separate subgroup analyses are reported in the online Appendix A. EL = English learner; DDD = difference-in-differences-in-differences.
$^{*}p < .1$. $^{**}p < .05$. $^{***}p < .01$.

Table 11

Effect Heterogeneity on English Proficiency, by Cohort, Sex, and Home Language

DiD Interactions	Overall	Listening	Speaking	Reading	Writing	Reclassified Posteligibility
Panel A (3 separate cohorts)						
Newcomer (2014)	0.083 (0.065)	0.061 (0.081)	0.039 (0.044)	−0.130** (0.061)	0.003 (0.058)	−0.027 (0.043)
Newcomer × 2015	−0.015 (0.100)	−0.018 (0.141)	−0.026 (0.059)	0.054 (0.117)	−0.068 (0.073)	−0.098* (0.054)
Newcomer × 2016	0.164* (0.092)	0.141 (0.109)	0.173** (0.076)	0.084 (0.085)	0.090 (0.086)	−0.061 (0.065)
p value (2014 = 2015 = 2016)	.124	.344	.026	.608	.131	.197
Panel B (pooled 2015/2016)						
Newcomer × EL	0.083 (0.065)	0.061 (0.081)	0.039 (0.044)	−0.130** (0.061)	0.003 (0.058)	−0.027 (0.043)
Newcomer × EL × 2015/2016	0.075 (0.085)	0.063 (0.106)	0.075 (0.065)	0.069 (0.084)	0.012 (0.072)	−0.079 (0.052)
Observations	8,518	8,518	8,518	8,518	8,518	9,461
Panel C (by sex)						
Eligibility (sex—main)	0.065 (0.070)	0.101 (0.084)	0.156** (0.062)	−0.146* (0.081)	0.053 (0.062)	−0.029 (0.035)
Eligibility × female	0.134* (0.070)	0.165* (0.093)	0.009 (0.064)	0.165* (0.095)	0.132* (0.072)	0.077* (0.040)
Panel D (by home language)						
Eligibility (language—main)	0.111 (0.083)	0.194* (0.110)	0.065 (0.074)	−0.054 (0.110)	0.127 (0.084)	0.022 (0.045)
Eligibility × Chinese	0.145 (0.088)	0.111 (0.119)	0.184** (0.085)	0.129 (0.120)	0.077 (0.091)	0.063 (0.060)
Eligibility × Spanish	−0.246* (0.130)	−0.375** (0.174)	−0.034 (0.132)	−0.273* (0.144)	−0.218* (0.126)	−0.140** (0.062)
p value (Chinese = Spanish)	.000	.000	.023	.000	.006	.022
Observations	8,518	8,518	8,518	8,518	8,518	9,461

Note. Robust standard errors are in parentheses, clustered at the cohort-school level. Estimates are based on DiD models described in Equation 3. Models include pre-eligibility test score controls and cohort by first high school fixed effects. The sample includes all students expected to graduate between 2009 and 2016 and who had overall, listening, speaking, reading, and writing scores in the year before eligibility and the year after eligibility. "Reclassified posteligibility" refers to having been reclassified between the fall after the cohort would have become eligible for ELSS and high school graduation. The reclassification sample includes students who were ELs as of the summer cohort and would have become eligible for ELSS. Class of 2014 is the omitted category in Panels A and B. Male is the omitted category in Panel C. Languages other than English, Chinese, and Spanish are grouped as one, which is the omitted category in Panel D. "Eligibility" refers to newcomer ELs in postprogram cohorts. Additional separate subgroup analyses are reported in the online Appendix A. EL = English learner; DiD = difference-in-differences; ELSS = EL Summer School.

*$p < .1$. **$p < .05$. ***$p < .01$.

Discussion

This study estimates the causal impact of summer credit recovery on high school newcomer ELs' academic outcomes using a DDD approach. Building on Umansky (2016a), I extend the literature on EL access to secondary education opportunities by examining program impact on ELs' exposure to academic content, as measured by the cumulative ELA, math, science, and social science courses taken during the first 4 years of high school. In addition to general and UC requirement course counts, I look at English proficiency as a measure of skills gained. This study also contributes new evidence to research on high school completion by focusing on the newcomer EL population.

This article reports three main findings. Summer credit recovery resulted in significant increases in the number of ELA, math, science, and social science classes that newcomer ELs took. The increase applied to general high school courses in all four subjects and college-preparatory ELA. Despite these gains in course enrollment, the program did not significantly affect 4-year or 5-year graduation. Increases in CELDT scores provide suggestive evidence of improvements in English proficiency.

ELSS significantly increased newcomer ELs' access to four core subjects. Course completion measures indicate that newcomer ELs were not only taking more college-preparatory ELA classes but also passing and receiving credit. This first finding suggests that expanded learning opportunities may effectively address the EL equity trap, starting with ELA (Callahan & Shifrer, 2016). With appropriate support, newcomer ELs can and do succeed in rigorous ELA courses, even prior to reclassification. This is contrary to the belief that English proficiency must precede exposure to advanced academic material (Harklau, 1994).

This result is important, given the predictive power of high school curriculum intensity on postsecondary outcomes (Adelman, 2006). Nationally representative survey data show that 38% of students who received language services in high school did not attend college at all, while another 38% attended 2-year colleges; 4-year college enrollment rate, on the other hand, was less than half that of native English speakers (Callahan & Humphries, 2016). Expanding ELs' access to college-preparatory classes addresses this 4-year college access gap. Opportunities to take academically challenging courses in high school extend access to 4-year colleges and momentum toward bachelor's degree completion. When high schools give ELs a chance to take and do well in advanced classes, they also give ELs a better chance at attending and finishing college.

Completing more college-preparatory ELA is an important first step. Since most ELSS participants enrolled in ELA classes during the summer, the large and statistically significant gains in college-preparatory ELA completion is not surprising. Increasing participation in summer math, science, and social science classes may contribute to similar increases in the completion of college-preparatory credits in these subjects.

Given its substantial impact on core subject enrollment and college-preparatory ELA completion, summer credit recovery seems to be a promising approach to expanding academic access for ELs. Yet for my sample, this increase in exposure to academic content did not translate into higher graduation rates. Studies on math credit recovery have shown that despite high rates of recovery, credit gains led to on-time graduation only for a small fraction of students (e.g., Heppen et al., 2016). The few additional credits earned in one summer may be insufficient to steer severely off-track students back on track to graduate in 4 years. The cohort of 2014 in my sample, who only had one summer of ELSS eligibility before their scheduled graduation date, may have faced this limitation.

Another potential explanation for the null effect on graduation is the district's adoption of UC/CSU A–G completion as its graduation requirements for cohorts of 2014 and later. Prior research suggests that adoption of more stringent graduation requirements may adversely affect students who enter high school with the weakest academic preparation (e.g., Jacob, Dynarski, Frank, & Schneider, 2016). Eligible students in my sample may have experienced a substantial reduction in graduation rates for this reason. Due to their recent arrival in the United States, newcomer ELs in program-eligible cohorts had very little time to prepare to meet the demanding new requirements. As we might expect, newcomers in the cohort of 2014 suffered the most. Having become eligible for the program just before their senior year in high school, this group may have been too far off track for one summer to remedy. The 2015 and 2016 cohorts may have fared better because they had the opportunity to enroll during multiple summers, and many did.

Still, more than 97% of ELSS-eligible students in the graduation outcome sample (including the cohort of 2014) graduated within 5 years. For the cohorts of 2015 and 2016, the 5-year graduation rate was more than 99%. These rates suggest the potential for improved outcomes. Earlier and repeated interventions may further reduce time to completion. For instance, expanding program eligibility to include rising ninth graders and encouraging program veterans to enroll every summer will afford ELs additional opportunities to complete academic course requirements within 4 or 4.5 academic years.

A third notable finding is indications of improvement in overall English proficiency, as well as higher CELDT listening, speaking, and writing scores for the pooled EL sample. Annual CELDT testing took place during the first 2 months of the fall semester. ELs who attended the summer program were using academic English 5 hours a day, 5 days a week. This practice likely contributed to maintaining or advancing their English proficiency, which is then reflected in their early-fall test scores. Since ELSS focused on developing ELs' communicative competence, improvements in English listening and speaking are not surprising. We might also expect eligible students to perform better on the holistically scored writing task as their speaking skills improve. On the other hand, ELSS did not appear to help students develop reading skills or reclassify.

The program's emphasis on oral communication and its short duration might be a reason. ELSS uses student-centered pedagogy, which allocates most of class time to discussions. As a result, students may not have had much time to practice reading in class. The null effect on reclassification could also be explained by this. In the context of this district, ELs must meet score thresholds on both the CELDT and the California standardized ELA test, as well as approval through school and parent consultation before reclassification. Since the program did not improve reading skills, it was unlikely to move ELs through the end of the intricate, multistep reclassification process.

This is not to say that reading development as a goal is completely out of reach for summer programs like ELSS. According to theories in second-language acquisition, reading skills tend to develop gradually (Grabe, 1991). ELs may not be able to make much progress in terms of actual reading score gains during the 5 weeks of ELSS. However, even 5 short summer weeks could be leveraged to improve reading skills through development of good reading habits. For example, teachers can facilitate the formation of reading groups at the beginning of summer so that students can keep one another accountable. By incorporating regular reading group check-ins throughout the 5 weeks of instruction, teachers can demonstrate the implementation of peer accountability. As students part for the rest of the summer vacation and the upcoming academic year, the network of peer readers formed during ELSS could then motivate them to continue reading outside of school.

I did not find significant differential impact by sex or home language on course taking or graduation. However, DiD estimates suggest that ELSS affected the English proficiency of Spanish and Chinese users differently. It is possible that some aspects of the program did not work as expected for some students. When designing and implementing summer programs, districts should pay attention to student differences in sex and home language and carefully monitor student progress during and after program participation.

Concluding Remarks and Limitations

This article augments research on ELs by focusing on newcomers in high school. Using a DDD design, I report causal evidence on the impact of summer credit recovery. I find that the program significantly increased ELs' access to ELA, math, science, and social science courses, both at the high school graduation and the college-preparatory levels. Impact on 5-year graduation rates was positive but imprecisely estimated. CELDT scores also provide suggestive evidence for improved English proficiency among a subset of ELs.

A few limitations of the study warrant caution in interpretation of the results. First, the sample for this study was drawn from a single school district in California. Findings may only be generalizable to similar urban districts with large EL enrollments.[9] Second, longitudinal achievement data suitable for the DDD analysis were not available.[10] Thus I am not able to estimate the effect of summer credit recovery on students' math, science, or language arts skills. It is not clear whether the additional courses students took had any impact on achievement in those subjects. Third, complete data were only available for a few background characteristics (female, age, home language), which restricted the types of alternative analyses, such as propensity score matching and falsification tests, that could be performed. Fourth, I used initially fluent English–proficient immigrant students as a comparison group against ELs in the newcomer DiD analysis. This non-EL group was relatively small and may not have provided an ideal comparison, but it was the closest peer group available in the data. Last, incomplete data on graduation rates and test scores may introduce bias in the estimates for these outcomes. Findings on these outcomes should be interpreted as suggestive rather than strong evidence.

Even with these limitations, this study shows that summer credit recovery bears potential for increasing EL high school completion and academic achievement. The program described in this study is the first of its kind to provide ELs, the majority of whom are from low-income backgrounds, with summer opportunities for linguistically supported academic development. As demonstrated by recent summer learning research on native English users (e.g., Augustine et al., 2016; Augustine & Thompson, 2017), low-income students suffer more summer learning loss than higher-income students, largely because low-income students have very limited summer learning options. Summer programs that offer 5 or more weeks of academic instruction are not commonly available to low-income urban youth (Augustine & Thompson, 2017). ELs face even more access barriers than other low-income students because summer academic programs with appropriate language support are even rarer. To minimize summer learning loss, states and districts need to provide equitable access to summer academic opportunities to ELs. ELSS is one model of extending learning opportunities from which educators in other contexts can draw inspiration. I hope the results from this study will help initiate more discussions on ELs' access to educational opportunities in and out of school.

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Notes

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[1]In this article, credit recovery is broadly defined as the opportunity to earn credits toward graduation. Students need not have failed one or more courses previously to participate.

[2]According to the Center for Applied Linguistics (2017) Database of Newcomer Programs, most programs list the simultaneous acquisition of English skills and academic content as their primary objective; yet few report using English proficiency or achievement outcomes to assess program quality. Less than 30% of the programs report tracking students' test scores or grades over time. Only one program utilizes data from similar students in other schools to evaluate the program in a difference-in-differences design. The rest either rely only on postprogram outcomes or conduct no evaluation at all.

[3]Newcomers are eligible for world language waivers on demonstrating proficiency in another language. On average, eligible students only took 0.5 Category E (world languages) classes during their first 5 years of high school. Therefore, I only report results on Categories A–D. Categories F (visual and performing arts) and G (electives) are beyond the scope of this article.

[4]A student who enrolled in but failed biology during 9th grade, enrolled in and passed chemistry during 10th grade, passed physics in 11th grade, and took no additional science classes receives a total science course count of 3, one for attempting each class. For UC science, the person receives 2 counts for completing chemistry and physics but no point for biology.

[5]UC/CSU entrance requires completion of 4 years of ELA, 3 years of math, 2 years of lab sciences, and 2 years of social sciences.

[6]Graduation data are available for 73% of the sample. Missingness is balanced across eligible and ineligible students.

[7]Attrition is balanced across eligible and ineligible students.

[8]Outcome trend graphs (course taking, graduation, and CELDT) for newcomer ELs, oldcomer ELs, newcomer non-ELs, and oldcomer non-ELs are shown in the online Appendix Figures OA1 and OA2.

[9]As of the 2011–2012 school year, the 25 school districts with the largest EL enrollment served 21% of all ELs in the United States.

[10]From 2009 to 2013, the California Standards Test math test was administered to students according to the math course in which they were enrolled. High school science is not tested in consecutive years.

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