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# Dual Identification? The Effects of English Learner Status on Special Education Placement

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ABSTRACT

This study examines the effects of English Learner (EL) status on subsequent Special Education (SPED) placement. Through a research-practice partnership, we link student demographic data and initial English proficiency assessment data across seven cohorts of test takers and observe EL and SPED programmatic participation for these students over seven years. Our regression discontinuity estimates consistently differ substantively from results generated through regression analyses. We find evidence that the effect of EL status on SPED placement was either null or tied to slight under-identification. Our results suggest that under-identification occurred two years after EL classification. We also find that EL status led to under-identification for Spanish speakers and proportionate representation for Mandarin/Cantonese speakers and speakers of all other languages.

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## Introduction

The effectiveness of educational support services holds tremendous influence on the academic success of millions of public school students in the U.S. (Thurlow et al., 2006). When such services meet students' needs for linguistic or disability support, indicators of student success can improve (Berkeley et al., 2010; Morgan et al., 2010; Wang & Lam, 2017). Conversely, when student needs are unmet, student success can falter (Morgan et al., 2010). Persistent achievement gaps between student populations who are eligible for accommodations and those who are not suggest that there is substantial room for improvement nationwide (Albus et al., 2013; Pasternack, 2014). Furthermore, some students rely not solely on English Learner (EL) services or Special Education (SPED) services, but rather on supports for *both* a learning disability and developing English proficiency. We refer to these students as *dually identified* students (i.e., identified for both EL and SPED) and they are a central focus of this study (Carnock & Silva, 2019; Umansky et al., 2017).

Despite a substantive body of descriptive and qualitative research on the relation between EL and SPED placement, little is known about the causal link between the two. Prior work has highlighted disproportionate representation of EL students in SPED using descriptive regression and hazard analyses (Morgan et al., 2015, 2017, 2018; Umansky et al., 2017). This work has most frequently found EL students to be under-identified in elementary grades, but over-identified in secondary grades (Umansky et al., 2017). However, a clear evaluation of how EL status *affects* subsequent SPED placement has not yet been conducted. Previous studies on this topic note that to most thoroughly examine disproportionate representation, student-level data are necessary (Morgan et al., 2017).

Through a research partnership with a large, urban school district in California, this study provides credibly causal evidence on the effects of EL status on subsequent SPED placement. We make two critical contributions. First, our application of the regression discontinuity (RD) research design to detailed, student-level administrative data reflects a methodological advancement over prior work in this area. The RD approach is a powerful quasi-experimental design that allows for the unbiased examination of the Local Average Treatment Effect (LATE) by comparing individuals just above and below an arbitrary cut point or threshold that determines assignment to a particular intervention (Angrist & Pischke, 2009; Thistlewaite & Campbell, 1960). A core strength of this methodological approach is the modest number of assumptions that must hold in order for inferences to be considered valid (Angrist & Pischke, 2009). Furthermore, compared with other quasi-experimental research designs (e.g., propensity score matching; interrupted time series designs, etc.), the RD approach stands out because the identifying assumptions can be empirically tested (Calonico et al., 2014; McCrary, 2008).

Second, we generate policy-relevant evidence for practitioners that can facilitate the continuous improvement of SPED identification procedures for ELs. We provide evidence exploring how the *timing* of SPED placement may reflect disproportionate representation. We look specifically at RD results for SPED placement in each year following initial EL classification in Kindergarten (i.e., Grade 1 to Grade 6). Our results offer targeted evidence to practitioners that can inform the ongoing development and refinement of SPED identification procedures. Additionally, we explore effect heterogeneity by primary language to shed light on possible differences that occurred across language groups. To do this, we split our sample into three subgroups (i.e., students speaking Spanish as the primary language; students speaking Cantonese/Mandarin; and students speaking any other non-English language) and apply our RD

approach to each subgroup separately. Our findings help to illuminate potential differences in the way EL students were placed into SPED based on the primary language spoken. Such distilled information can aid district leaders to develop more targeted approaches for students in each of the three distinct subgroups. Therefore, this study's findings are highly relevant for district stakeholders and decisionmakers.

Our reduced-form RD findings indicate that EL status largely led to proportionate representation or slight under-identification (i.e., by 2 percentage points or less) of EL students in SPED placement in this district. In contrast to results from Ordinary Least Squares (OLS) regression analyses, we find no evidence of a positive association between EL status and SPED placement. Furthermore, across a wide range of bandwidths, we reject the null hypothesis that our RD estimate is statistically the same as our OLS estimate, highlighting the novel information generated by the RD approach. These results, based on the pooled sample, suggest that the district is effectively identifying a close-to-proportionate number of EL students for SPED placement. We also find evidence that slight under-identification of EL students for SPED placement occurred during Grade 2. Furthermore, we observe evidence of heterogeneous effects by primary language group: Spanish-speaking students were under-identified for SPED; Chinese-speaking students and students speaking other languages were proportionately represented in SPED. In combination, these results broadly validate district approaches, but also suggest areas for adjusting and improving the SPED identification process for ELs. Further, these findings elucidate the unique value of research-practice partnerships: our results simultaneously inform and advance the existing literature and provide nuanced details that can inform district decision-making.

## Context

### English Learners

ELs are students between the ages of 3 and 21 who need additional support to improve their English language listening, speaking, reading and writing abilities to be able to succeed in academic courses where English is the language of instruction (U.S. Department of Education (DOE), 2016). Also referred to as students with limited English proficiency (LEP) or as emergent bilingual/multilingual students, EL students have been a protected class of students since the 1974 Supreme Court decision in *Lau v. Nichols* (Hakuta, 2011; Martínez, 2018). Title III of the *Every Student Succeeds Act* (ESSA) provides state educational agencies with substantial latitude in how EL students are to be identified, but most commonly the process involves a home language survey being sent to newly enrolled students. Students whose families indicate that a language other than English is spoken at home are then given a formal assessment to determine if the student qualifies for EL classification (Carnock & Silva, 2019). In California, for example, the California English Language Development Test (CELDT) was the formal assessment used to evaluate a student's English language skills from 2001 to 2017. Students who classify as EL (i.e., as opposed to English-proficient) are entitled to educational services for English language development (U.S. DOE, 2016).

Approximately 10 percent of the total US student population was classified as EL in 2015 (Carnock & Silva, 2019; U.S. DOE, 2014). More than three quarters of the "current EL" population (i.e., students with active EL classification in 2015) at that time identified as Latino/a, yet the current EL population overall was extremely diverse with regard to race, ethnicity, nationality and languages spoken (U.S. DOE, 2014). The ten districts enrolling the highest proportions of current ELs were located in California, Alaska, New Jersey, Arizona and

Washington (U.S. DOE, 2014).[3] Of the current EL population, approximately 15 percent qualified for SPED (Carnock & Silva, 2019).

**Special Education Students**

Students in SPED receive services to enable them to access a free and appropriate public education (FAPE). Since 1975, federal law and related judicial rulings have required that all children aged 3-21 nationwide have access to a FAPE. This means that any student with special needs required due to a disability are to receive individually tailored instructional strategies. The *Individuals with Disabilities in Education Act* (IDEA) part B covers students aged 3-21 and requires schools to provide services in the least restrictive environment (Carnock & Silva, 2019). As a result, schools must provide necessary educational accommodations while also ensuring the student is not unnecessarily diverted from typical educational settings. IDEA defines 13 distinct disability categories.[4] When a child aged 3-21 is identified as having a disability in any of these 13 categories, the student is entitled to SPED services and an Individualized Education Program (IEP) is established.

Since 2007-08, between 13-14 percent of all students nationwide have been placed in SPED after being identified as having a disability within one of the 13 categories specified in IDEA. This translates to more than six million students annually receiving SPED services under IDEA (U.S. Department of Education, 2013, Table 204.30). Students identified with either a "specific learning disability" or "speech or language impairment" made up more than half of all disability classifications.[5]

**Dually Identified Students**

Students identified as eligible for both EL and SPED services are one of the most vulnerable student populations, and their unique intersection of needs for educational services

calls for greater study and evaluation (Carnock & Silva, 2019). The provision of both EL and SPED services are required by federal law but implemented at the local level. Federal appropriations for SPED have historically only covered a limited portion of the actual costs to provide such services to districts (National Council on Disability, 2018). In recent years, federal appropriations for SPED have provided just over 15 percent of the actual cost districts experience when implementing these services. For EL services, real funding levels (i.e., adjusted for inflation) have recently (i.e., since at least 2015) dropped below the per pupil amount appropriated in 2002 (Carnock & Silva, 2019). The lack of sufficient funding for both EL and SPED programs makes concerns regarding the provision of services for dually identified students who rely on both programs even more stark.

Around 700,000 students nationwide in 2014-15 were dually identified when using a current-EL framework. Further, current-EL students were more likely to be identified for either the "specific learning disability" or "speech or language impairment" disability categories than non-EL students (U.S. DOE, 2014). However, these statistics mask substantial complexity in defining the presence of EL and dually identified students nationwide by failing to account for students who were ELs at one time but have since reclassified to English-proficient. In contrast, an "ever-EL" framework encompasses a broader set of students who are either current ELs or students who have reclassified out of EL services. A major strength of using the ever-EL framework is that the underlying sample remains consistent over time, retaining all students who ever are identified for EL status regardless of reclassification status (Umansky, 2016b). Using the ever-EL framework is especially important for studying dually identified students because it helps to compare SPED placement rates for students who were quite similar at baseline (i.e., prior to any EL intervention) (Umansky et al., 2017).

## Prior Literature

Existing research suggests that EL classification can affect later student placement in SPED (Burr et al., 2015; Burr, 2019; Hibel & Jasper, 2012; Umansky et al., 2017). To the extent disproportionate classification occurs, SPED placement may be a key moderator of the effect of EL classification on students' short- and long-term outcomes. Extant evidence on this topic is mostly associative in nature and documented using regression or hazard analyses. Our study contributes meaningfully to existing literature by investigating the causal link between EL status and SPED placement. In particular, we study the likelihood of SPED participation between one and six years after being designated for EL status for students who scored only marginally differently on the CELDT initial assessment during Kindergarten.[6] In this review of existing literature, we discuss the challenging work of disentangling disabilities from language needs, the prior focus on disproportionate representation, how both under-identification and over-identification of EL students into SPED can be harmful, and recent methodological advances in work exploring long-term effects of EL classification.

### Disentangling Disabilities from Language Needs

Prior studies highlight the challenge of differentiating student disabilities from English language developmental needs. Poorly designed language assessments with weak psychometric properties, for example, can create problems for discerning between language needs and disability needs (Macswan & Rolstad, 2006). Additionally, an early study noted that a disproportionate number of Latino/a students were labeled as having a learning disability solely due to limited English proficiency (Ortiz & Polyzoi, 1986). More recent literature suggests that difficulty differentiating between a disability and language need continues to challenge

educational institutions and staff (Carnock & Silva, 2019). This can be especially true for students in the early grades and frequently results in diagnoses for a language need earlier than a disability (Burr, 2019; Carnock & Silva, 2019). Policies pertaining to district SPED identification processes may be particularly relevant and important to consider relative to this phenomenon (Burr, 2019).

**Disproportionate Identification**

Multiple studies highlight the issue of a potential disproportionality (i.e., either underrepresentation or overrepresentation) of EL student participation in SPED. Crucially, under-identification of EL students in SPED can be harmful for students academically. Specifically, under-identification suggests that EL students with learning disabilities are not receiving necessary services (Greenberg Motamedi et al., 2016). Such a phenomenon could be occurring as the result of delayed testing for EL students (Samson & Lesaux, 2009). A delay may be stimulated by some form of explicit or implicit bias against EL students (Figueroa & Newsome, 2006). An alternative explanation is that EL students may somehow be more difficult to identify for SPED due to difficulty differentiating between language needs and a disability (Burr, 2019; Carnock & Silva, 2019). Regardless, under-identification of EL students with a disability suggests that some students with needs for SPED accommodations are failing to access essential support services.

On the opposite end of the spectrum, over-identification of EL students for SPED can also be harmful to students (Burr et al., 2015; Burr, 2019). Over-identification could indicate that EL students are unnecessarily being designated for additional services that might limit their inclusion in general education classrooms (Samson & Lesaux, 2009). A key component of federal law establishing protections for both EL and SPED students dictates that students must be

placed in the *least* restrictive educational environment (Carnock & Silva, 2019). Accordingly, placement in SPED without a need for accommodations can stymie a student's ability to participate in general education class settings, which may be essential for that student's growth and development. Furthermore, prior work has highlighted that SPED participation can lead to stigmatization, which can be harmful for the student (Shifrer, 2013).

Proportionate representation, therefore, would indicate that services are being adequately provided with students still having access to less restrictive classroom settings. This represents the appropriate middle ground that districts are seeking to reach (Burr et al., 2015).

Umansky and colleagues (2017) used discrete-time hazard analyses and an "ever-EL" framework to examine the likelihood that a student subsequently participates in SPED (i.e., becomes dually identified). They found that ever-EL students were less present in SPED overall and within most disability categories (Umansky et al., 2017). However, an important limitation of this study was that a causal link was not identified. In other words, it is unclear whether participation in EL services *led* students to be under-classified in SPED or not. As such, outstanding research questions about whether EL services *cause* disproportionate classification for SPED remain.

Other existing work on the intersection of EL classification and SPED classification also suggests that ELs tend to be both disproportionately identified for most disability categories and identified later than non-ELs for SPED services (Hibel & Jasper, 2012; Morgan et al., 2015; Samson & Lesaux, 2009). Notably, however, these three studies did not report findings for the subgroup of students that were initially classified for EL, but rather examined samples of students who either spoke another language at home or were identified as children of immigrants (i.e., students who *may or may not* have been eligible for EL services). Still other work has

looked exclusively at subgroups of *current EL* students to analyze disproportionality (e.g., Artiles et al., 2005; Sullivan, 2011, Sullivan & Bal, 2013, Wagner et al., 2005). However, as has been previously noted in the literature, a key shortcoming of these analyses is the inability to account for reclassification. In other words, the results reported in these articles do not consider that the sample of EL students changes as students exit EL status (i.e., when a student scores at the reclassified fluent English proficient level). Retaining reclassified students in the sample is appropriate as it enables a full and consistent comparison of students over time. Failing to account for these students may lead to inaccurate evaluations of SPED placement rates. A further limitation to some extant literature is the reliance on repeated cross section data instead of panel data (e.g., Klingner et al., 2005; Morgan et al., 2015; Samson & Lesaux, 2009). Such methodologies inhibit the ability to precisely identify if observed relationships were due to policy interventions or changes in the underlying sample. In sum, a substantial amount of prior research has emphasized the importance of understanding the disproportionality of EL students in SPED. However, to date, the empirical methods applied to this topic, while consistently becoming more advanced, have been unable to explore a causal link. Our study, combining panel-based research designs, the ever-EL definition and fine-grain student-level EL and SPED participation data, allows for a rigorous quantitative analysis of this topic.

**Regression Discontinuity Evidence on EL Classification**

Up to this point, the application of regression discontinuity (RD) designs to study how EL status affects SPED placement has not yet occurred. However, recent advances in the literature pertaining to how EL status affects later educational outcomes illustrates the value of this approach for studying outcomes of EL students. In particular, several recent studies have leveraged student scores from the CELDT to employ RD designs that estimate the effect of EL

status on academic achievement and attainment (Johnson, 2019; Shin, 2018; Umansky, 2016a, 2016b). Johnson (2019), for example, used a binding score RD framework to examine the effect of initial EL classification on outcomes such as high school graduation and college attendance. Initial EL classification and later reclassification were both found to have limited effects on high school completion and college enrollment. This evidence aligned with other recent RD work that considered later student achievement via school grades in a different district context (Shin, 2018). In general, Shin (2018) found weak positive effects of initial EL classification. Two other RD studies that use initial student CELDT scores as the forcing variable found that EL classification was in fact harmful to the likelihood of taking rigorous academic coursework and student achievement on standardized tests (Umansky, 2016a, 2016b). Key commonalities across these articles were the application of the RD design, the use of student-level data, and the reliance on an initial CELDT assessment score forcing variable. Our study advances the literature examining disproportionate representation of EL students in SPED by applying these three key components (i.e., an RD design, student-level data and reliance on an initial CELDT assessment forcing variable) to important SPED placement outcomes.

## Data

We partner with a large urban school district and leverage its longitudinal data from the California Longitudinal Pupil Achievement Data System (CALPADS). The data include four essential sets of information: (a) SPED program participation, (b) EL classification; (c) the official results (overall and by domain) that students obtained during their *initial* CELDT assessment; and (d) students' demographic characteristics. Our study is based on panel data for SPED participation, EL classification, and demographic characteristics from SY 2006-07

through SY 2018-19. Results from the initial CELDT assessment are available from SY 2006-07 through 2016-17.[7]

To understand the effect of EL status on a consistent set of SPED outcomes, our sample focuses exclusively on students that took the initial CELDT assessment during their Kindergarten year.[8] The students in our sample, therefore, were those whose families reported speaking a language other than English at home and entered the district during their Kindergarten year.[9] Critically, our analytical sample includes all students who took the CELDT, whether they were classified as EL or English-Proficient.[10] The presence of both sets of students in our analytical sample is essential for our quasi-experimental research design.

In order to gain clear insight into the SPED placement outcomes across the elementary school timespan, we follow 7 cohorts of initial CELDT assessment takers for 7 years (i.e., during the initial CELDT assessment year and in the 6 subsequent years). Figure 1 provides an illustration of the cohorts included in our sample. For the main analyses, we keep cohorts of initial CELDT takers from SY 2006-07 through SY 2012-13 (N=12,607).[11] For each of these cohorts, we observe SPED placement for students in each subsequent year (e.g., in Grade 3).[12] Table 1 provides summary statistics.

Our principal outcome variable is an indicator that the student was placed in SPED between Grades 1 and 6 after the initial CELDT assessment in Kindergarten. Students that were identified for SPED in the *same* year (Kindergarten) as the initial CELDT assessment are not flagged by this outcome. This is because we do not observe the precise start date (day and month) of SPED participation during Kindergarten and cannot identify whether SPED participation started before or after the CELDT test. Also, many students enter Kindergarten having been flagged as needing SPED services through an early childhood education program

(e.g., Head Start; Pre-Kindergarten, etc.). Therefore, our analysis focuses on those students identified for SPED *following* initial CELDT assessment so that we can directly understand the influence of EL status. It is important to note that while SPED participation was somewhat rare (i.e., 7 percent), our uniquely large analytic sample includes 883 students who were identified as SPED between Grades 1 and 6. With this level of variation and sample size, we are able to examine the effects of EL status on SPED placement.

We also explore how EL status affects SPED placement in each subsequent year (i.e., SPED placement in Grade 1, SPED placement in Grade 2, etc.). A strength of this approach is that it enables us to identify when, if ever, EL status had an effect on rates of SPED placement.

In addition, our data include baseline demographic characteristics that make up our student-level controls. We include a flag for whether the student identifies as female. Further, we have race/ethnicity-based flags for individuals that identify as (a) Hispanic, (b) Chinese, or (c) Decline to State their Race/Ethnicity. Our data also include measures of the highest level of education received by the students' mother or father. We synthesize this information into a flag for whether the highest-educated parent had at least a high school diploma. Our final baseline student characteristic approximates the student's age at the time of the initial assessment.[13]

Our data from the initial CELDT assessment also include information about the primary language spoken by the student. More than 40 different languages were represented in the sample, with large groups of students speaking either Spanish or Chinese (i.e., Mandarin or Cantonese). As shown in Table 1, approximately 32 percent of students taking the initial CELDT assessment indicated speaking Spanish. Another 36 percent of students spoke Chinese, and about 33 percent of students indicated speaking another non-English language. Using these language flags, we explore effect heterogeneity by language groups. This analysis was of interest because

the district has this assessment available in English, Spanish and Chinese, but not the other languages. As a result, we consider the possibility of differential experiences across languages.

Finally, our data identify the school that each student attended and the calendar year of initial assessment. Using this information, we create school-cohort groups within our data. These groupings allow us to implement school-cohort fixed effects that help control for common variation experienced by students in particular schools at particular times.

## Methods

One approach to understanding the relationship between EL status and SPED placement is to simply regress EL status on our SPED outcome variables of interest. Unfortunately, such regression analysis sheds light only on the correlation between the two variables rather than the causal relationship. Prior work on this topic has considered the likelihood of SPED placement based on being an EL, which is a roughly analogous approach. A key shortcoming of this type of evaluation is an inability to determine how EL status *affects* SPED placement unless extensive and likely invalid assumptions (i.e., selection on observables) about the relationship are made. Table 2 presents OLS regression results for the relationship between EL status and SPED placement. Across specifications with and without student-level controls and school-cohort fixed effects, the relationship in our context is significant and positive. The model with all controls suggests that being assigned EL in Kindergarten is associated with a 3.4 percentage point increase in SPED placement between Grades 1 and 6. This suggests that EL students were *over*-represented in SPED in this district.

A key contribution of our study is the application of a more advanced quasi-experimental research design that can more rigorously estimate how EL status affected subsequent SPED placement. Leveraging CELDT scale score data, we differentiate between intent-to-treat and

control groups by determining whether the student was assigned to EL status. To do this, we apply an RD research design to estimate the LATE of EL status on SPED placement. Those students assigned to EL status based on their CELDT score are the intent-to-treat group. Those students that were not assigned to EL status (i.e., English-proficient students) based on their CELDT score are the control group.

Using this approach, we leverage a core underpinning concept from the RD literature—the idea that students near the EL threshold were quite similar to one another in expectation and provide a strong counterfactual group (Thistlewaite & Campbell, 1960). Assuming that students were unable to precisely manipulate their score (an assumption that appears to hold based on numerous empirical tests presented in Appendix Table A1 and Appendix Figures A1, A2 and A3 in the Supplemental Materials), EL status can be considered as good as randomly assigned for ranges of the sample near the threshold. Leveraging this natural experiment that occurs near the arbitrary EL threshold enables a stronger counterfactual comparison group than other quasi-experimental research designs, such as propensity score matching (Angrist & Pischke, 2009; Thistlewaite & Campbell, 1960)

Kindergarten students needed to meet a predetermined cut score across multiple language domains in order to be classified as English-proficient. Students with an Overall Scale Score below the "Beginning Advanced" level, a Listening Scale Score below the "Intermediate" level, or a Speaking Scale Score below the "Intermediate" level were classified as ELs. The initial step to implement the RD approach in this context is to construct a "binding score" forcing variable that accounts for these three ways that a Kindergarten student could have been classified as an EL (Papay et al., 2011; Porter et al., 2017; Reardon & Robinson, 2012). To do this, we create variables for the overall scale score and the listening and speaking scale scores that are centered

around the cut scores for each domain and based on *initial* CELDT assessment results in each particular year. This allows us to put scores from each Kindergarten cohort on the same scale despite minor adjustments to the CELDT assessment year to year. For each student, we then take the minimum value of these three variables to create the binding score forcing variable for student *i* in school *s* and cohort *c*:

$$BindingScore_{isc} = MIN\{CenteredOverall_{isc}; CenteredListening_{isc}; CenteredSpeaking_{isc}\}$$

As reported in Table 1, for 92 percent of students, the binding section was the overall score. For 6 percent of students, the binding section was listening. For the remaining students, speaking was the binding section.

With the binding score forcing variable established in this way, we test for manipulation around the cutoff and proceed to apply standard RD methods.[14] First, we use the binding score forcing variable to define the point at which we expect there to be a discontinuous jump in the probability of treatment:

$$Below_{isc} = \mathbf{1}(BindingScore_{isc} < 0)$$

We then apply an RD model that flexibly allows for parametric and non-parametric estimation of the causal relationship:

$$Y_{isc} = \alpha Below_{isc} + f(BindingScore_{isc}) + \lambda_{sc} + \boldsymbol{\beta X_{isc}} + \epsilon_{isc}$$

In this specification, $\alpha$ signifies the discrete jump at the cutoff for EL assignment and is our coefficient of interest. The indicator $Below_{isc}$, flags observations that were below the cutoff based on the binding score forcing variable. The $f(BindingScore_{isc})$ term represents a flexible function of the binding score forcing variable. We implement this as both non-parametric local linear regression and as specifications that include both linear and quadratic splines of the forcing variable.[15] $\lambda_{sc}$ represents school-cohort fixed effects, which allows us to remove variation that is consistent across groups of students testing from the same school-cohort combination. $\boldsymbol{X_{isc}}$ is a vector of student-level covariates, including an approximation of the student's age at initial assessment and indicators for being female, Hispanic, Chinese, having declined to state race/ethnicity, and having the most educated parent being at least a high school graduate. $\epsilon_{isc}$ is the mean-zero error term.

As a critical specification check, we test to see how our binding score forcing variable influenced the probability of actual EL assignment. Table 3 provides point estimates of the first stage relationships across a variety of specifications. Across the columns of Table 3, we observe large and statistically significant relationships between our discontinuity parameter, $\alpha$, and EL assignment. Importantly, this indicates that in most cases our binding score forcing variable is effectively flagging students that ultimately entered EL status. We observe this further by noting that the F-statistic for our main instrument, $Below_{isc}$, is over 1000 for each reported specification.[16] Still, these results also highlight that our binding score forcing variable does not perfectly identify treatment. In some cases, a student may have scored below the threshold but was classified as English-proficient or scored above the threshold but was still classified as EL. Therefore, we are applying a fuzzy RD specification. Figure 2 illustrates the likelihood of EL

classification based on the CELDT binding score. Panel A provides binned averages for the full analytical sample while Panel B focuses on a narrower range around the threshold. In both graphs, the number of students within each bin is reported just above the plotted point. Since the first stage relationship is so strong for both the main sample and sub-samples based on the primary language spoken by students, we privilege reduced form Intent-To-Treat (ITT) effect estimates in our main analysis.[17]

## Results

Table 4 presents our main RD results examining the reduced form effect of EL classification on subsequent SPED placement between Grades 1 and 6. We present three versions of our main specification. In all models we account for heteroskedasticity of the error term by reporting Eicker-Huber-White robust standard errors. Column (1) provides results from our main RD specification that includes a linear spline of the forcing variable, but no other controls. Column (2) reports a specification that retains the linear spline of the forcing variable and includes a set of baseline student-level demographic controls. Column (3), our preferred specification, retains the linear spline of the forcing variable and the student-level controls and adds in school-cohort fixed effects. Due to the increased set of controls incorporated into this model, which modestly aid our precision and control for other relevant factors, we focus our discussion of subsequent results on this specification. The preferred main sample finding is a marginally significant -1.5 percentage point estimate.

A potential concern for the validity of the RD design is the choice of bandwidth and functional form. To address this for each variation of our RD specification, Table 5 reports estimates of the discontinuous jump at the threshold across multiple bandwidth samples. Table 5, row (1) reports results for the full sample (i.e., +/- 180 points). Row (2) reports results using a

sample of students whose scores were within +/- 150 points of the centered cutoff (i.e., a binding score between -150 and 150). Row (3) presents results for a bandwidth +/- 100 points; row (4) presents results for a bandwidth of +/- 50 points; and row (5) presents results for a bandwidth of +/- 34 points around the threshold. Row (5) represents the optimal RD bandwidth suggested by the Calonico, Cattaneo and Titiunik (2014) approach.

For bandwidths of the full sample, +/- 150 and +/- 100, we observe at least marginally significant estimates (i.e., $p<0.1$) of between -0.015 and -0.021 across specifications. This suggests that students identified as EL were less likely to be placed in SPED between Grades 1 and 6. Figure 3 presents a graphical illustration of this main result for the full sample (Panel A) and for a narrow sample +/- 50 from the cutoff (Panel B). However, negative point estimates become slightly larger in magnitude but become statistically insignificant at the +/- 50 bandwidth. Results for all models at the CCT optimal bandwidth similarly indicate that EL status had no effect on SPED placement between Grades 1 and 6 after initial EL classification in Kindergarten. These results for narrower bandwidths suggest that EL status led to proportionate representation in SPED during subsequent years. Examination of both the visual relationships and the regression results indicates that students classified EL were just as likely as or *less* likely than their English-proficient peers to be identified for SPED between Grades 1 and 6. This result stands in stark contrast to the positive association reported through the naïve regression analysis.

We further probe the robustness of our main results across a range of bandwidth samples using our preferred specification. Figure 4 presents the point estimates and confidence intervals. The point estimate is somewhat volatile and confidence intervals are wider at relatively small bandwidth samples. This is what we would expect given the smaller sample sizes. As our bandwidths increase, confidence intervals narrow and point estimates stabilize around -0.015

(i.e., a decrease of 1.5 percentage points). Importantly, our point estimates are largely indistinguishable from zero (i.e., the blue dotted line), indicating that we have a precisely estimated null result. However, these estimates are statistically different from the regression analysis estimates (i.e., the red dotted line). For all bandwidth samples presented, we reject the null hypothesis (when $\alpha$=0.05) that our regression discontinuity model coefficient of interest is equal to our OLS regression analysis coefficient.

Table 6 reports reduced form RD results for our preferred specification (i.e., with a linear spline, student-level controls, and school-cohort fixed effects) for SPED placement in each particular grade after the initial CELDT assessment. Column (1) replicates the RD result for our main outcome, SPED placement between Grade 1 and 6. Column (2) reports the RD estimate for SPED placement in Grade 1. Column (3) presents the RD estimate for SPED placement in Grade 2. Columns (4)-(7) follow in cognate form with Column (7) presenting RD estimates for SPED placement in Grade 6. In identical form to Table 5, the rows of this table correspond to different bandwidth samples (i.e., the full sample, +/- 150, +/- 100, +/- 50 and +/- 34).

Results from column (2) indicate no major difference in SPED placement probability for students classified as EL and English-proficient in Grade 1. Results from column (3), however, consistently show point estimates of about -0.01, which are statistically significant (i.e., $p<0.05$) in bandwidth samples +/- 100 and larger. This provides compelling evidence that EL status was leading to modest under-identification in Grade 2. Columns (4) and (5) report point estimates very close to zero that are not statistically significant. Therefore, we observe no discernable disproportionality emerging in Grade 3 or 4. In column (6), we observe larger negative point estimates in narrow bandwidth samples that become smaller in the larger bandwidth samples.

This provides suggestive evidence that under-identification may have also been occurring in Grade 5. Column (7) indicates that there was not a disproportionate relationship in Grade 6.

In Table 7, we examine heterogeneous effects by students' primary language for our main outcome of interest.[18] Column (1) presents results for Spanish speaking students. Here, we observe quite consistent point estimates, all between -0.043 and -0.055, that are not statistically significant at the narrowest bandwidths reported but become at least marginally significant for bandwidths larger than +/- 100 points. These results indicate that Spanish speakers classified as ELs were between 4.3 and 5.5 percentage points less likely to be placed in SPED between Grades 1 and 6. To contextualize these results, about 10.5 percent of Spanish speakers in the sample were placed in SPED between Grades 1 and 6. Our RD estimates suggest that EL status reduced SPED identification by between 41 and 52 percent for this subgroup. Column (2) of Table 6 presents RD results for the subsample of students who primarily spoke Mandarin or Cantonese. Point estimates are close to zero and not statistically significant across all reported bandwidths. This indicates that there was proportionate representation of these EL students placed in SPED between Grades 1 and 6. Column (3) of Table 6 shows point estimates for students who spoke languages other than English, Spanish, Mandarin, or Cantonese. Here, point estimates for all bandwidths of +/- 50 points around the cutoff or larger are quite consistent. The point estimates range from -0.007 to -0.017, but none of them are statistically significant.

Figure 5 provides graphical illustrations of these heterogeneous results: Panel A for Spanish speakers; Panel B for Chinese speakers; and Panel C for speakers of all other languages. Based on the point estimates and graphical results, we find evidence of under-identification for Spanish speaking students and no evidence of disproportionate representation for Chinese speakers and speakers of all other languages.

In addition to these results of the main analytic sample, we also present supplemental analyses in Appendix Tables in the Supplemental Materials. Appendix Table A2 illustrates the OLS estimate of the relationship between EL status and SPED placement after a varying number of years. Appendix Table A3 provides main specification estimates for differently structured analytic samples (e.g., 8 cohorts observed for 6 years each; 9 cohorts observed for 5 years each, etc.). In Appendix B, we conduct cognate analyses for students who took the initial CELDT assessment in Grades 1-5. Results are similar to the Kindergarten sample but are imprecise due to a far smaller sample size. Future work can explore whether these results are similar for students who took the CELDT assessment in a year after Kindergarten for a longer panel of data.

## Discussion

For students with special needs, timely and appropriate placement into educational support services is essential (Burr, 2019). Delayed identification or misidentification can be academically and psychologically harmful for students (Carnock & Silva, 2019). Prior research has documented concerns specifically about EL students' being misidentified for SPED services and focused the analyses on disproportionate representation of EL students in SPED (Morgan et al., 2015, 2018; Umansky et al., 2017). However, largely due to data restrictions, these efforts have mainly resulted in descriptive findings. This study is the first to directly explore the causal link between EL status and subsequent SPED placement.

Using a rigorous quasi-experimental research design, we provide compelling new evidence about the effect of EL status on SPED placement. In clear contrast to positive correlations suggested by regression analyses, our main RD results indicate a null or slightly negative overall effect of EL status on SPED placement between Grades 1 and 6 after initial EL classification in Kindergarten. For nearly all bandwidth samples, our RD estimates differ

statistically from the positive correlations estimated through OLS. This clear result poignantly emphasizes the critical distinction between correlation and causation. Our credibly causal results indicate that differences are modest when comparing SPED placement rates for students who barely reach and students who barely miss the English proficiency threshold in Kindergarten. In fact, EL status led to proportionate or slight under-identification of EL students for SPED. This finding suggests that qualitative analyses about the districts' identification protocols and practices would be informative and valuable.

Furthermore, our methodological approach allowed us to explore when disproportionate representation might appear. Our results indicate that under-representation by 0.9 to 1.1 percentage points arose during Grade 2. We also found suggestive evidence of slight under-representation by 0.5 to 0.9 percentage points in Grade 5. One potential explanation for under-identification of ELs in Grades 2 and 5 is the preparation for academic transition. Students typically transition from learning to read to reading to learn as they enter Grade 3. It is possible that to prepare for this transition, school staff are paying extra attention to students' learning challenges and needs. Teachers may be more likely to refer students who are native English users or initially English-proficient to be assessed for disabilities. In contrast, EL students may be less likely to be referred for a disability assessment if the teacher believes the challenges EL students experience with learning or literacy are due to developing language proficiency and not due to a potential disability. District administrators affirmed this possibility and shared that schools tend to closely students for SPED needs during Grade 2; ELs on the other hand, are usually given "more time" before they are referred for assessment. A similar scenario might explain the slight under-identification of ELs for SPED placement in Grade 5. As teachers prepare students for transition to middle school in Grade 6, native and fluent users of English may be more likely to

be referred for a needs assessment, since any academic challenges they experience are not likely to be attributed to English language proficiency.

We were also able to examine effect heterogeneity by primary language category. Doing this, we found important results: Spanish speakers were under-identified by a statistically significant 4-5 percentage points; Chinese speakers and speakers of all other languages were proportionately identified. These heterogeneous findings by primary language merit further discussion.

First, practices for identifying special needs used by the district factor prominently into understanding these results. In our partner district, many tools for assessing needs for disabilities are provided in English, Spanish, Cantonese, and Mandarin. In other words, when ELs who are Spanish and Chinese speakers are assessed for special needs, the assessments can often be conducted in their primary language, while ELs who speak other languages are assessed in English. We would expect the availability of primary language assessment to lead to more accurate placement that matches students' needs. But this could also either increase or decrease the rate of actual SPED placement relative to using an English assessment. Although both Spanish speaking and Chinese speaking ELs had access to primary-language SPED assessment, we find that the Spanish speaking ELs were under-identified for SPED relative to Spanish speaking English-proficient students while Chinese speaking ELs were proportionately identified. We do not observe the language of the assessment administered to each student and are unable to analyze the effect of using primary-language assessment. The contrast in SPED placement between Spanish and Chinese speaking ELs raises the possibility of future research investigating the role of primary-language assessment in SPED placement.

Second, given the estimated magnitude of the under-identification of Spanish-speaking EL students for SPED, it is important to further evaluate this result. By considering results by language group, we demonstrate the value of *within* group comparisons as opposed to *across* group comparisons when considering the issue of disproportionate representation of EL students in SPED. Our results suggest that it was possible that Spanish speaking ELs were referred for assessment at lower rates compared to Spanish speaking English-proficient students. Alternatively, among students whose primary language is Spanish who were assessed for special needs, ELs and English-proficient student may have been assessed for SPED in different languages (e.g., ELs in Spanish; English-proficient students in English). Such difference in the language of assessment may have contributed to differential rates of SPED placement. Regardless, under-identification warrants attentions because as noted by prior research (e.g., Greenberg Motamedi et al. 2016), it can suggest that students needing services may not be accessing them. We are unable to directly explore the factors that may have led to differential placement rates by EL status, as we do not observe data on student referral for special needs assessment, the assessments given, or results from those assessments.[19]

This study effectively demonstrates the power of research-practice partnerships in generating research that directly informs education practice and policy. Having discussed the findings with the research team, the district has begun multiple initiatives. First, both SPED and EL departments have set out to analyze qualitative data on SPED identification, with a focus on Grade 2 students and Spanish speaking ELs. Second, the partnership plans to examine additional data to analyze EL pathways through SPED identification and programs. This study has thus motivated a series of mixed-methods inquiries aimed at developing equitable practices around SPED identification and services.

## Limitations

A few limitations to this study merit consideration. First, the RD design exploits assignment to treatment that is as good as random at the cut score; thus, results may not be generalizable to students who scored far above or far below the cut score. Also, in the presence of treatment heterogeneity, our estimates would only be defined for the compliers, who took up the EL status assigned by their kindergarten CELDT score. Second, the data come from one district with a long history of serving a large, diverse EL student population and a mature research-practice partnership with a large research university, so the findings may be particularly influenced by this context. Third, the data did not support a robust standalone analysis of ELs who entered the district between Grade 1 and 5. Future research should consider differential effects of later district entry on dual identification when more data are available. Finally, this study was not able to identify the precise mechanisms driving the overall null effect of kindergarten EL classification or the source of heterogeneous treatment effect by students' primary language. Detailed mixed methods inquiry into this topic could prove valuable for identifying such mechanisms in future research.

## Concluding Remarks

Our results offer a causal assessment of how EL status in Kindergarten affected subsequent SPED placement in our partner district. Such results make a substantial contribution to existing literature in at least two ways. First, they represent a methodological advancement in the consideration of disproportionate representation of EL students in SPED. Our results demonstrate the viability of the RD approach in this context and suggest that ongoing research can use this applied framework to better understand the interaction of these two major educational service programs. Second, we shed light on *when* and *for whom* disproportionate

representation occurred. Such information is valuable to our partner district as they look to improve policy pertaining to the SPED placement process for EL students in the district. Consideration for the timing of SPED placement following EL classification and for potential differences among student subgroups is also relevant to the design of SPED placement procedures across the nation.

This combination of scholarship that advances existing literature and provides directly usable results for practitioners highlights the uniquely valuable contributions of research conducted through research-practice partnerships. Furthermore, the application of the RD methodology to this topic sets the stage for ongoing research studying the interaction of EL and SPED services in other contexts.

## Endnotes

[1] University of Hawai`i at Mānoa

[2] NWEA

[3] Overall, during the 2014-15 school year, the western and southwestern regions had substantially larger current EL populations than most other parts of the US (U.S. DOE 2014).

[4] Specifically, the categories are: (1) autism; (2) deaf-blindness; (3) developmental delay; (4) emotional disturbance; (5) hearing impairment; (6) intellectual disability; (7) multiple disabilities; (8) orthopedic impairment; (9) other health impairment; (10) specific learning disability; (11) speech or language impairment; (12) traumatic brain injury; and (13) visual impairment (including blindness) (Carnock & Silva, 2019).

[5] The next most commonly experienced categories of disability were “other health impairment” and “autism”, which together accounted for just under one quarter of all disability classifications. The next three most prominent categories have tended to be “developmental delay”, “intellectual

disability" and "emotional disturbance". Together, these three categories made up approximately 18 percent of all disabilities classified. Finally, hearing impairments, multiple disabilities, orthopedic impairments, traumatic brain injury and visual impairments consistently made up less than 5 percent of all disabilities classified. (U.S. DOE, 2013, Table 204.30) In addition to learning disabilities, approximately 10 percent of SPED students also classified as being an EL (National Council on Disability, 2018).

[6] From this point forward, we refer to one year after being designated for EL status as Grade 1, two years after as Grade 2, and so on. However, it is possible that a student was retained in a particular grade for a second year. Due to missingness in the grade level variable in our dataset, we cannot exactly estimate the frequency of this occurrence. The district reports a low level of retention overall, however, suggesting that it was quite infrequent.

[7] After 2016-17, California began implementing the English Language Proficiency Assessments for California (ELPAC) and discontinued the same level of reliance on the CELDT.

[8] In supplemental analyses (i.e., Appendix Table B1 and Appendix Table B2), we consider an additional sample of students who took the initial CELDT assessment between Grades 1 and 5. We privilege the Kindergarten sample in our main analysis because students who enter the district and are assessed at Kindergarten entry are more comparable to one another than students who enter the district in later grades. Of the students taking the initial CELDT assessment between Kindergarten and Grade 5, more than 75 percent of them were assessed during the Kindergarten year. The focus on Kindergarten CELDT takers is also consistent with other recent RD studies relying on the CELDT forcing variable (e.g. see Umansky 2016b; Shin 2018).

[9] We exclude students that scored the minimum score Overall or on Speaking or Listening domains because the CELDT assessment simply gives these students the lowest raw score and

does not differentiate between abilities at these levels. We also exclude students with missing outcome or covariate data.

[10] California refers to these students as Initially Fluent English Proficient (IFEP); in other contexts, the term "English-proficient" is more common.

[11] A critical tradeoff in our sample construction was between the number of cohorts to include and the length of time for which we would observe their outcome. We chose this analytical sample in an effort to study the most relevant time window with the greatest statistical power.

[12] In supplemental analyses (i.e., Appendix Table A3), we consider more cohorts of students across shorter periods of time. We also consider fewer cohorts of students across longer periods of time.

[13] Since our data only provide the birth month and birth year of each student, we necessarily approximate student age at the initial CELDT assessment.

[14] In the supplemental materials, Appendix Figures A1, A2 and A3 and Appendix Table A1 provide evidence pertaining to the continuity of the forcing variable. Appendix Figure A1 presents raw histogram of the forcing variable for full and narrow samples. Appendix Table A2 shows results from the McCrary (2008) density test using bin widths of 10 and 2. Appendix Table A3 illustrates results from the Cattaneo, Jannson and Ma (2018) density test. Appendix Table A1 checks covariate balance across the threshold. The combined evidence does not suggest a violation of the continuity assumption. In addition to these checks, we conducted density tests for the forcing variable by different primary language subsamples (i.e., Spanish, Chinese and all other languages). For these subsamples, we similarly observe no evidence of a discontinuity at the threshold.

[15] While we examined models that incorporated quadratic splines of the forcing variable, the Akaike Information Criteria (AIC) indicated that linear specifications should be privileged. As a result, we principally report specifications with linear splines of the forcing variable.

[16] We also test the first stage relationship for three primary language subsamples (i.e., Spanish, Chinese, and all other languages) and find quite consistent results across language groups. In these instances, we observe large and statistically significant jumps at the threshold (i.e., all greater than 0.7) with the first bin to the right of the threshold exhibiting the largest rate of non-compliance, with a likelihood of EL classification between 0.2 and 0.3. The character of the observed fuzziness is quite similar across subsamples.

[17] Results for 2SLS specifications that scale our treatment effect estimates by levels of compliance are available upon request. These 2SLS estimates represent our Treatment on the Treated (TOT) estimates.

[18] Note that each language subgroup sample is only about one-third of the main analytic sample, which reduces the precision of our estimates.

[19] One other possible scenario is that concern about over-identification in an equity-focused district, triggered by the fact that there was a higher SPED identification rate overall for Spanish speaking ELs as compared to Chinese speaking ELs or ELs that speak of all other languages, actually led to lower identification rates for Spanish speaking ELs. Reliance on cross-group comparisons could have guided staff toward reducing identification of Spanish speaking EL students for SPED in order to ensure they were not overrepresented.

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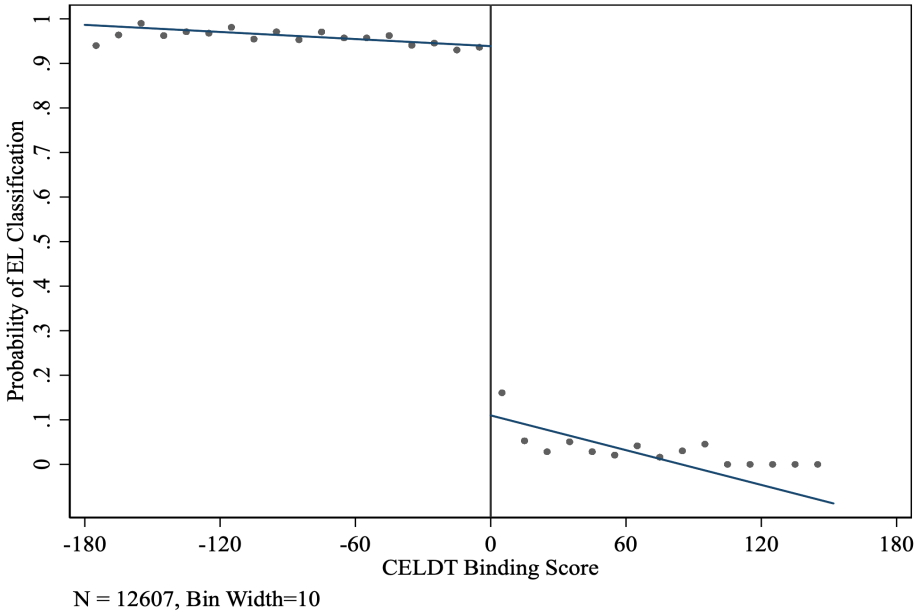
Figure 1. CELDT Cohort Data Visual

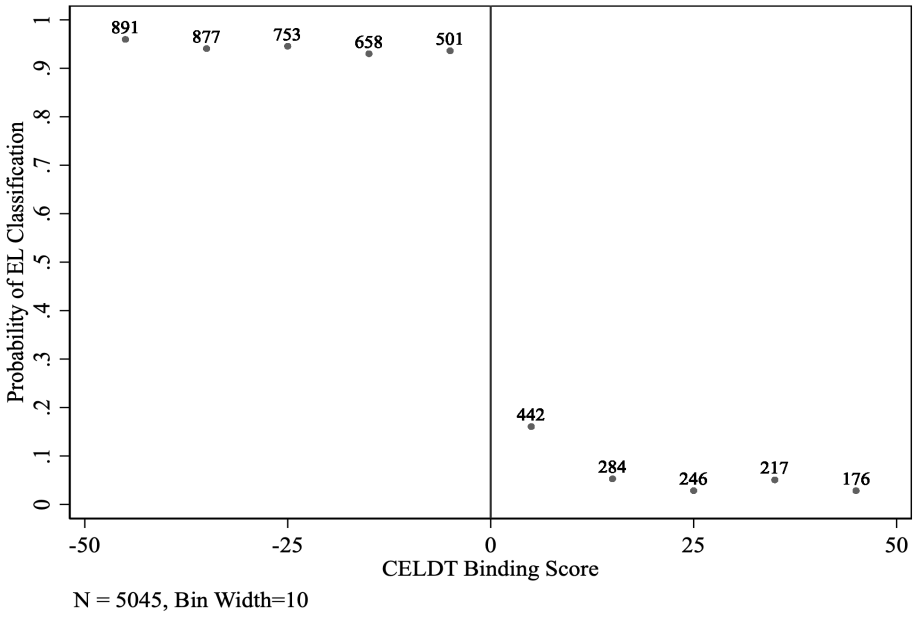
| Kindergarten Cohort | *t=0* | *t=1* | *t=2* | *t=3* | *t=4* | *t=5* | *t=6* | *t=7* | *t=8* | *t=9* | *t=10* | *t=11* | *t=12* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 2006-07 | m | m | m | m | m | m | m | s | s | s | s | e | e |
| 2007-08 | m | m | m | m | m | m | m | s | s | s | s | e | |
| 2008-09 | m | m | m | m | m | m | m | s | s | s | s | | |
| 2009-10 | m | m | m | m | m | m | m | s | s | s | | | |
| 2010-11 | m | m | m | m | m | m | m | s | s | | | | |
| 2011-12 | m | m | m | m | m | m | m | s | | | | | |
| 2012-13 | m | m | m | m | m | m | m | | | | | | |
| 2013-14 | s | s | s | s | s | s | | | | | | | |
| 2014-15 | s | s | s | s | s | | | | | | | | |
| 2015-16 | s | s | s | s | | | | | | | | | |
| 2016-17 | e | e | e | | | | | | | | | | |
| 2017-18 | e | e | | | | | | | | | | | |
| 2018-19 | e | | | | | | | | | | | | |

*Notes:* Student-level data for individuals who took their initial CELDT assessment in Kindergarten are from CALPADS for SY 2006-07 through SY 2018-19. Our main analysis includes cohorts who took the initial assessment from SY 2006-07 through SY 2012-13, observed annually during the assessment year and the six subsequent years. Our supplemental analyses include cohorts who took the inital assessment from SY 2006-07 through SY 2015-16 observed in three to ten subsequent years. We exclude data from cohorts after SY 2015-16 cohorts and we exclude data after the 10th subsequent year for SY 2006-07 and SY 2007-08 cohorts. In the graphic above, "m" indicates that the data were used in the main analysis; "s" indicates that the data were used in supplemental analyses, and "e" indicates that the data were excluded.

Figure 2. Probability of EL Classfication by Binding Score, Graphical Results.



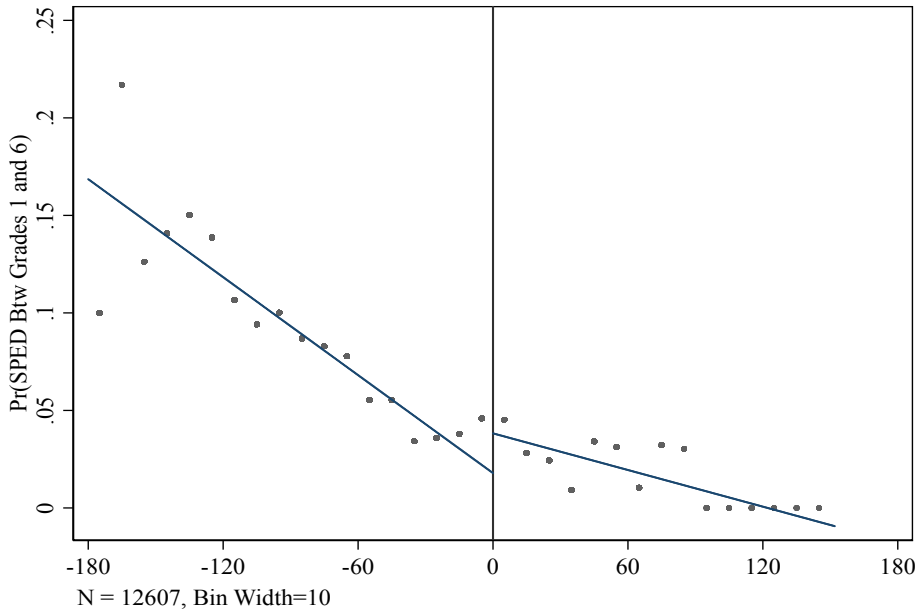

Panel A: Full Analytic Sample



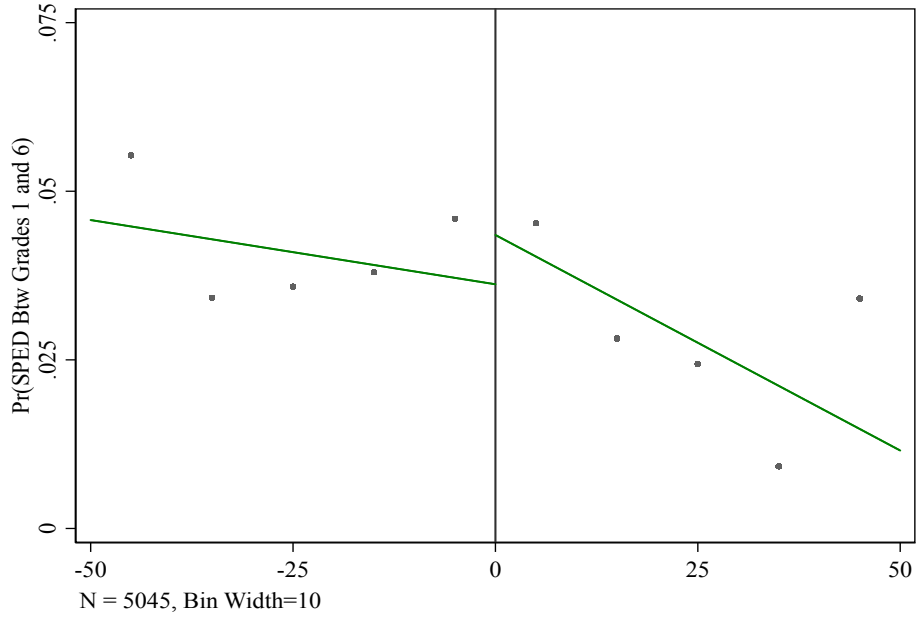

Panel B: Narrow Analytic Sample

*Notes:* Graphs of EL classifiction by the Binding Score forcing variable for full (i.e., all students scoring between -180 and 180) and narrow (i.e., all students scoring between -50 and 50) analytic samples. Bin width: 10. In Panel B, the number of students in each bin is labeled above each binned average.

Figure 3. Probability of SPED Placement Between Grades 1 and 6, by Binding Score Forcing Variable.



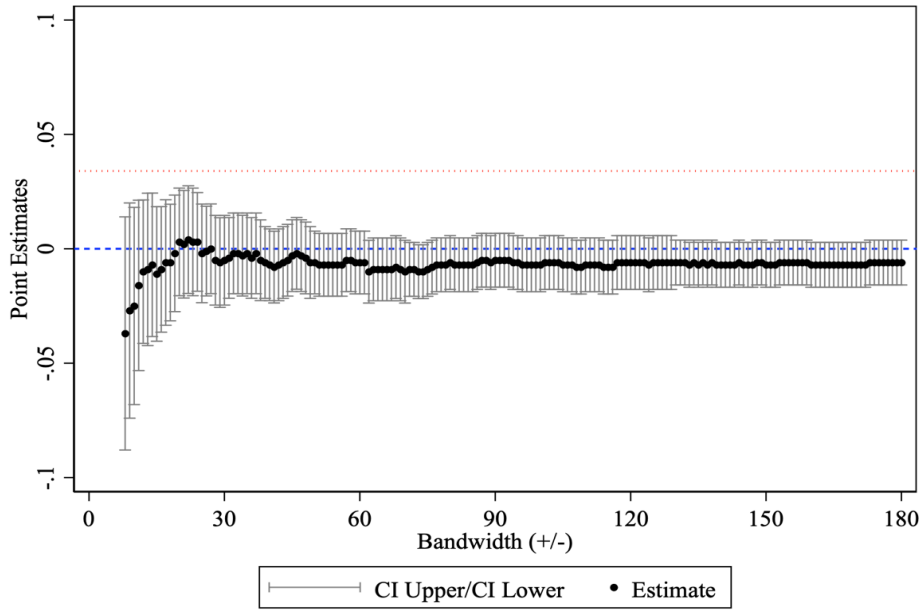

Panel A: Full Analytic Sample




Panel B: Narrow Analytic Sample

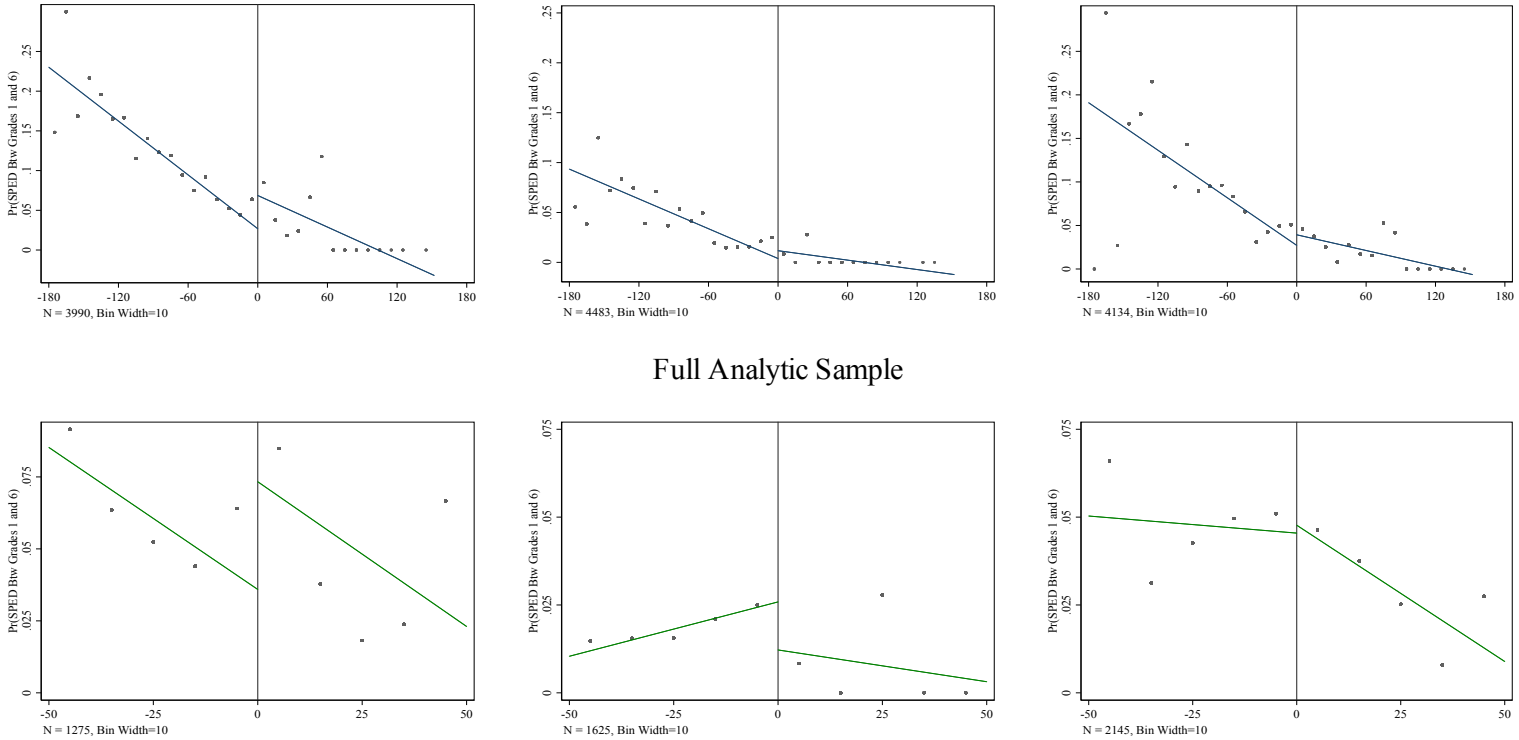
*Notes:* Graphs of EL classifiction by the Binding Score forcing variable for full (i.e., all students scoring between -180 and 180) and narrow (i.e., all students scoring between -50 and 50) analytic samples. Bin widths: 10 for both full and narrow analytical samples.

Figure 4. Sensitivity of Effect Estimates Across Bandwidths.




*Notes:* The dependent variable is a flag for being placed in SPED between Grades 1 and 6. All models include a linear spline of the forcing variable, school-cohort fixed effects and the following student-level controls: an indicator for female; an indicator for the student's most educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; an indicator for declining to state race/ethnicity; and the student's age at initial assessment. We exclude point estimates for samples with bandwidths less than or equal to +/- 7. The 95 percent confidence interval around each estimate is also graphed. The blue line is 0 and the red dotted line is the full analytic sample OLS benchmark estimate of 0.034.

Figure 5. Probability of SPED Placement Between Grades 1 and 6, by Binding Score Forcing Variable and Primary Language.




Panel A: Spanish Speakers | Panel B: Chinese Speakers | Panel C: All Other Language Speakers

*Notes:* Graphs of EL classifiction by the Binding Score forcing variable for full (i.e., all students scoring between -180 and 180) and narrow (i.e., all students scoring between -50 and 50) analytic samples by primary language. The two left most graphs (i.e., Panel A) are for Spanish speakers; the two middle graphs (i.e., Panel B) are for Chinese speakers; and the two rightmost graph (i.e., Panel C) are for speakers of all other non-English languages. Bin widths: 10 for both analytical samples.

Table 1. Summary Statistics

| Variable | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| *Student Characteristics in Year of Initial Assessment* | | | | |
| Female | 0.50 | 0.50 | 0 | 1 |
| Hispanic | 0.35 | 0.48 | 0 | 1 |
| Chinese | 0.38 | 0.48 | 0 | 1 |
| Decline to State Race/Ethnicity | 0.04 | 0.20 | 0 | 1 |
| Parent's Highest Education Level >= High School Diploma | 0.49 | 0.50 | 0 | 1 |
| Age at Initial Assessment (approximation) | 6.4 | 0.3 | 4.6 | 7.4 |
| Primary Language: Spanish | 0.32 | 0.47 | 0 | 1 |
| Primary Language: Mandarin/Cantonese | 0.36 | 0.48 | 0 | 1 |
| Primary Language: Other | 0.33 | 0.47 | 0 | 1 |
| *California English Language Development Test* | | | | |
| Binding Score | -56 | 52 | -176 | 151 |
| Centered Overall Scale Score | -55 | 52 | -176 | 151 |
| Centered Listening Scale Score | -18 | 56 | -147 | 161 |
| Centered Speaking Scale Score | 3 | 60 | -156 | 225 |
| Binding Section: Overall | 0.92 | 0.27 | 0 | 1 |
| Binding Section: Listening | 0.06 | 0.25 | 0 | 1 |
| Binding Section: Listening | 0.01 | 0.12 | 0 | 1 |
| *English Learner and Special Education* | | | | |
| Assigned EL by District | 0.836 | 0.37 | 0 | 1 |
| Below (Binding Score < 0) | 0.863 | 0.34 | 0 | 1 |
| Special Education Between Grade 1 and 6 | 0.070 | 0.26 | 0 | 1 |
| Special Education in Grade 1 | 0.016 | 0.12 | 0 | 1 |
| Special Education in Grade 2 | 0.012 | 0.11 | 0 | 1 |
| Special Education in Grade 3 | 0.014 | 0.12 | 0 | 1 |
| Special Education in Grade 4 | 0.012 | 0.11 | 0 | 1 |
| Special Education in Grade 5 | 0.010 | 0.10 | 0 | 1 |
| Special Education in Grade 6 | 0.006 | 0.08 | 0 | 1 |

*Source:* California Longitudinal Pupil Achievement Data System (CALPADS), SY 2006-07 through SY 2018-19.

*Notes:* The full sample includes students from this district who took the California English Language Development Test (CELDT) for the first time in Kindergarten between SY 2006-07 and SY 2012-13, had valid covariate and outcome data, and scored above the minimum score Overall and in Listening and Speaking domains (N=12,607). Excluded from the sample are students who took the initial CELDT assessment for the first time in 1st grade or later. The intent-to-treat group includes students who scored below the cutoff (N=10,880). The control group includes students who scored above the cutoff (N=1,727).

Table 2. OLS: Estimated Association Between EL Assignment and SPED Placement

| | Dependent Variable: Placed in SPED between Grades 1 and 6 | | |
|---|---|---|---|
| Independent Variable: | (1) | (2) | (3) |
| Assigned EL by District | 0.027*** | 0.023*** | 0.034*** |
| | (0.005) | (0.006) | (0.006) |
| Controls | no | yes | yes |
| School-Cohort FE | no | no | yes |
| $R^2$ | 0.001 | 0.026 | 0.077 |

*Notes:* Robust standard errors are in parentheses. Each model reports the OLS relationship between being Assigned EL by the District and placement in SPED between Grades 1 and 6. The analytic sample (N=12,607) includes students that took the CELDT initial assessment in Kindergarten between SY 2006-07 and SY 2012-13, had covariates and outcome measures available, and scored above the minimum score Overall and in Listening and Speaking domains. The model in column (2) includes the following student-level controls: an indicator for female; an indicator for the student's most educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; an indicator for declining to state race/ethnicity; and the student's age at initial assessment (coefficients suppressed). The model in column (3) includes the same set of student-level controls and adds school-cohort fixed effects (coefficients suppressed). The OLS estimate in column (3) is used to compare to subsequent Regression Discontinuity estimates.

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 3. The Effect of English Learner Eligibility on being Assigned EL by District, Full Sample

| | Dependent Variable: Assigned EL by District | | |
|---|---|---|---|
| Independent Variable: | (1) | (2) | (3) |
| $I(BindingScore_i < 0)$ | 0.829*** | 0.829*** | 0.829*** |
| | (0.012) | (0.012) | (0.012) |
| N | 12,607 | 12,607 | 12,607 |
| $R^2$ | 0.681 | 0.685 | 0.734 |
| Controls | no | yes | yes |
| School-Cohort FE | no | no | yes |

*Notes:* Robust standard errors are in parentheses. Each coefficient represents the results from a separate regression discontinuity model of the effect of the intent-to-treat on treatment status. Results for the three types of models for the +/- 34, +/- 50, +/-100, +/- 150 bandwidth samples were qualitatively similar to the results for the full sample (i.e, reflecting a large, statistically significant jump at the threshold).

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 4. Reduced Form Effect of EL Classification on Subsequent SPED Placement, Full Sample

| | Dependent Variable: Placed in SPED between Grades 1 and 6 | | |
|---|---|---|---|
| Independent Variable: | (1) | (2) | (3) |
| $I(BindingScore_i < 0)$ | -0.020** | -0.020** | -0.015* |
| | (0.008) | (0.008) | (0.008) |
| Student-Level Controls | no | yes | yes |
| Cohort-School FE | no | no | yes |

*Notes:* Robust standard errors are in parentheses. All models include a linear spline of the forcing variable and rely on the full sample (N=12,607). Each cell represents a separate regression. Models reported in columns (2) and (3) include the following student-level controls: an indicator for female; an indicator for the student's most educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; an indicator for declining to state race/ethnicity; and the student's age at initial assessment (coefficients suppressed). The model reported in column (3) also includes school-cohort fixed effects (coefficients suppressed).

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 5. Reduced Form Effect of EL Classification on Subsequent SPED Placement, by Bandwidth

| | Dependent Variable: Placed in SPED between Grades 1 and 6 | | | |
|---|---|---|---|---|
| Bandwidth Sample | (1) | (2) | (3) | N |
| Full Sample | -0.020** | -0.020** | -0.015* | 12,607 |
| | (0.008) | (0.008) | (0.008) | |
| +/- 150 | -0.021*** | -0.021** | -0.017** | 12,261 |
| | (0.008) | (0.008) | (0.008) | |
| +/- 100 | -0.018** | -0.018** | -0.018* | 9,999 |
| | (0.009) | (0.009) | (0.009) | |
| +/- 50 | -0.007 | -0.009 | -0.010 | 5,045 |
| | (0.012) | (0.012) | (0.012) | |
| +/- 34 | -0.002 | -0.002 | -0.007 | 3,267 |
| | (0.014) | (0.014) | (0.015) | |
| Student-Level Controls | no | yes | yes | - |
| Cohort-School FE | no | no | yes | - |

*Notes:* Robust standard errors are in parentheses. All models include a linear spline of the forcing variable. Each cell represents a separate regression. The +/- 34 bandwidth is the CCT optimal bandwidth. Models reported in columns (2) and (3) include the following student-level controls: an indicator for female; an indicator for the student's most educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; an indicator for declining to state race/ethnicity; and the student's age at initial assessment (coefficients suppressed). The model reported in column (3) also includes school-cohort fixed effects (coefficients suppressed). The point estimate for our preferred specification, model (3), is statisically different ($p<0.01$) from the OLS point estimate (i.e., from Table 2, column (3)) for all bandwidth samples.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 6. Reduced Form Effects on Other SPED Placement Outcomes, by Bandwidth

| | Dependent Variable: Placed in SPED: | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Between Grades 1 and 6 | During Grade 1 | During Grade 2 | During Grade 3 | During Grade 4 | During Grade 5 | During Grade 6 | |
| Bandwidth Sample | (1) | (2) | (3) | (4) | (5) | (6) | (7) | N |
| Full Sample | -0.015* | -0.003 | -0.010** | 0.001 | 0.003 | -0.005 | -0.001 | 12,607 |
| | (0.008) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | |
| +/- 150 | -0.017** | -0.005 | -0.010** | 0.003 | 0.000 | -0.005 | -0.001 | 12,261 |
| | (0.008) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | |
| +/- 100 | -0.018* | -0.003 | -0.011** | 0.003 | 0.002 | -0.006* | -0.002 | 9,999 |
| | (0.009) | (0.004) | (0.005) | (0.004) | (0.003) | (0.004) | (0.003) | |
| +/- 50 | -0.010 | -0.004 | -0.009 | 0.008 | 0.004 | -0.009** | -0.001 | 5,045 |
| | (0.012) | (0.005) | (0.006) | (0.006) | (0.005) | (0.005) | (0.005) | |
| +/- 34 | -0.007 | 0.001 | -0.011 | 0.009 | 0.006 | -0.008 | -0.005 | 3,267 |
| | (0.015) | (0.007) | (0.007) | (0.007) | (0.006) | (0.005) | (0.006) | |

*Notes:* Robust standard errors are in parentheses. Each cell represents a separate regression. The +/- 34 bandwidth is the CCT optimal bandwidth. All models include a linear spline of the forcing variable, student-level controls and school-cohort fixed effects (coefficients suppressed). The student-level controls include the following: an indicator for female; an indicator for the student's most educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; an indicator for declining to state race/ethnicity; and the student's age at initial assessment.

*** p < 0.01 ** p < 0.05 * p < 0.1

Table 7. Heterogeneous Reduced Form Effects, by Bandwidth and Language Category

| | Dependent Variable: Placed in SPED Between Grades 1 and 6 | | | | | |
|---|---|---|---|---|---|---|
| | Spanish | | Mandarin or Cantonese | | All Other Languages | |
| Bandwidth Sample | (1) | N | (2) | N | (3) | N |
| Full Sample | -0.043* | 3,990 | -0.002 | 4,483 | -0.007 | 4,134 |
| | (0.023) | | (0.010) | | (0.014) | |
| +/- 150 | -0.051** | 3,826 | -0.001 | 4,363 | -0.012 | 4,072 |
| | (0.023) | | (0.010) | | (0.014) | |
| +/- 100 | -0.054** | 2,948 | -0.003 | 3,487 | -0.017 | 3,564 |
| | (0.026) | | (0.011) | | (0.016) | |
| +/- 50 | -0.055 | 1,275 | 0.008 | 1,625 | -0.009 | 2,145 |
| | (0.037) | | (0.014) | | (0.020) | |
| +/- 34 | -0.053 | 785 | 0.006 | 1,044 | -0.001 | 1,438 |
| | (0.050) | | (0.019) | | (0.026) | |

*Notes:* Robust standard errors are in parentheses. Each cell represents a separate regression. The +/- 34 bandwidth is the CCT optimal bandwidth. All models include school-cohort fixed effects and the following student-level controls: an indicator for female; an indicator for the highest educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; the student's age at initial assessment (coefficients suppressed). The mean SPED placement rate between Grades 1 and 6 after the initial CELDT assessment for key subgroups are as follows: 0.105 for Spanish speakers overall; 0.111 for Spanish speakers scoring below the threshold; 0.047 for Spanish speakers score above the threshold; 0.034 for Cantonese or Mandarin speakers overall; 0.038 for Cantonese or Mandarin speakers below the threshold; 0.007 for Cantonese or Mandarin speakers above the threshld; 0.060 for speakers of all other languages overall; 0.069 for speakers of all other languages below the threshold; and 0.024 for speakers of all other langauges above the threshold.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

## Appendix A

Appendix Figures A1-A3 all provide graphical evidence pertaining to assessing the continuity of our underlying binding score forcing variable. While the RD design relies on far fewer assumptions than most other quasi-experimental research designs, it is critical to thoroughly interrogate the validity of the assumption that underpins this approach.

Appendix Figure A1 presents two histograms of the binding score forcing variable. Using bin widths of 5 points, Panel A depicts the density of the binding score forcing variable for the full sample. Panel B presents the density of the binding score forcing variable using bin widths of 2 for a narrower sample, +/- 50 points from the cutoff. Visual inspections of both histograms do not suggest manipulation (i.e., heaping) of the forcing variables at the cut score.

Appendix Figure A2 presents visual results from two iterations of the McCrary (2008) density test. These tests look to identify evidence of manipulation of the binding score forcing variable. Panel A offers the results when using the full sample and a bin width of 10 points. For this test, the discontinuity estimate reported is -0.027 with a standard error of (0.059). This result is not statistically significant and the corresponding visual suggests smoothness through the threshold. Panel B reports results when using the full sample and a bin width of 2 points. Again, our discontinuity estimate is not statistically significant. The point estimate is -0.048 and the standard error is (0.058). A visual inspection of the graph again shows no evidence of a jump at the threshold. These tests provide no evidence of a discontinuity in the forcing variable.

Appendix Figure A3 presents graphical results for the Cattaneo, Jansson and Ma (2018) test for manipulation at the threshold. Here, we again don't see clear evidence of a discontinuity, though the robust p-value reported is marginally significant (p=0.078). A key component of this density test is the use of the CCT optimal bandwidth, which may partially explain why the

results appear somewhat different from the McCrary (2008) density test. Based on the results from Appendix Figures A1-A3, we find no clear evidence that suggests our continuity assumption is invalid.

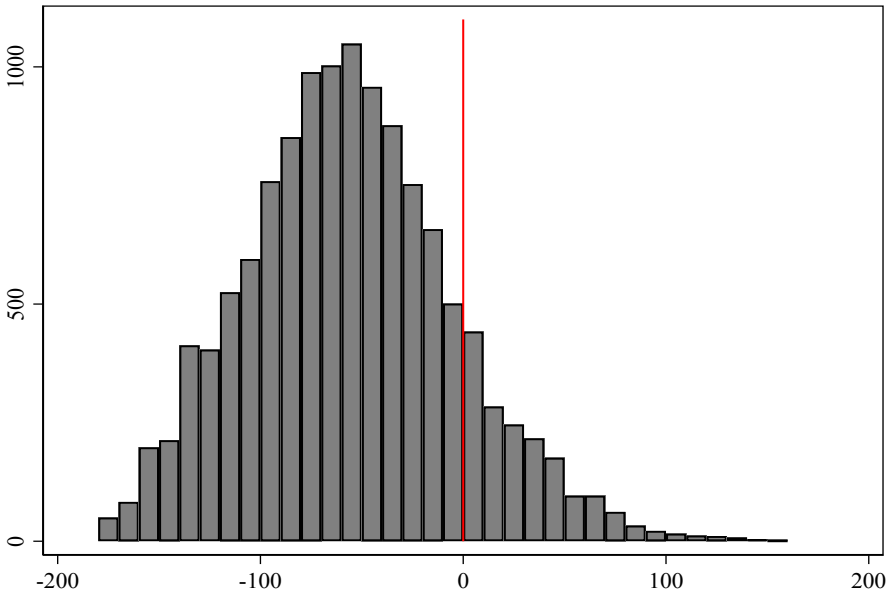
Appendix Table A1 provides an additional check on our continuity assumption by interrogating the pretreatment covariates in our sample. If the threshold cannot be precisely manipulated, we expect that the density of our covariates through the threshold would be smooth. Appendix Table A1 presents evidence about the continuity of covariate balance across the cutoff. In particular, we employ a two-step process for evaluating whether our covariates appear to be imbalanced across the threshold. First, we regress our "SPED placement between Grades 1 and 6" outcome variable on our set of student-level controls. Then we obtain the predicted values, $\hat{Y}$, from this regression. In the second stage, we regress $\hat{Y}$ on the "SPED placement between Grades 1 and 6" outcome variable and a linear spline of the forcing variable. We retain school-cohort fixed effects in each step. Results are presented for the same set of selected bandwidth samples reported in Tables 5-7 of the main analysis. A statistically significant result would indicate imbalance of our baseline covariates. However, across all bandwidths, we do not observe statistically significant results. Therefore, we do not find evidence of covariate imbalance at the threshold for any of the bandwidth samples reported.

Appendix Table A2 reports results from regression analyses that include all student-level controls and school-cohort fixed effects. These analyses look to delineate the OLS relationship between EL status and SPED placement. Different from the main analytic sample, where we look for SPED placement between 1-6 years after EL classification, here we adjust the period of time over which we observe SPED placements. Column (1) shows SPED placement between 1-3 years after EL classification. Each subsequent column adds one year. Column (8) shows SPED

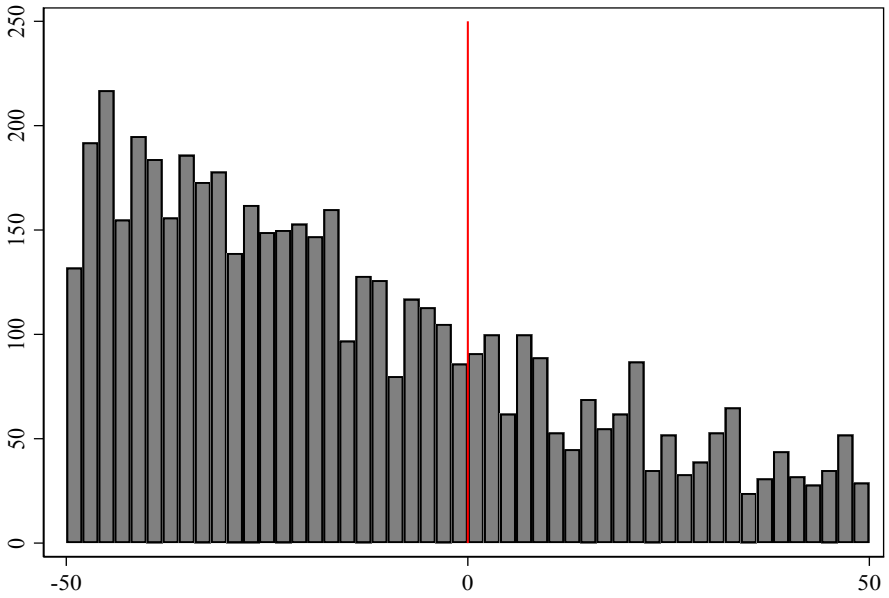
placement between 1-10 years after EL classification. The underlying sample of each regression is changing. In Column (1), we include the largest number of cohorts but observe them for the shortest amount of time. In Column (8), we include the fewest number of cohorts but observe them for the longest amount of time. Interestingly, the relationship is always positive and the point estimates are quite stable, hovering between 2.1 and 3.5 percentage points.

Appendix Table A3 reports results from RD specifications that include a linear spline of the forcing variable, student-level controls, and school-cohort fixed effects. This table presents RD results as we adjust the length of time we observe each student and the number of cohorts we include in the sample. In Column (1), we observe the most cohorts for the shortest amount of time. In Column (8), we observe the fewest cohorts for the longest amount of time. While many of the results are not statistically significant, there is clear consistency in the point estimates. We observe negative point estimates for all bandwidths except the CCT optimal bandwidth. The positive point estimates we observe are less than or equal to 0.007. This set of evidence offers additional validation to our main finding of null or slight underrepresentation of EL students in SPED.

Appendix Figure A1. Histogram of the Forcing Variable



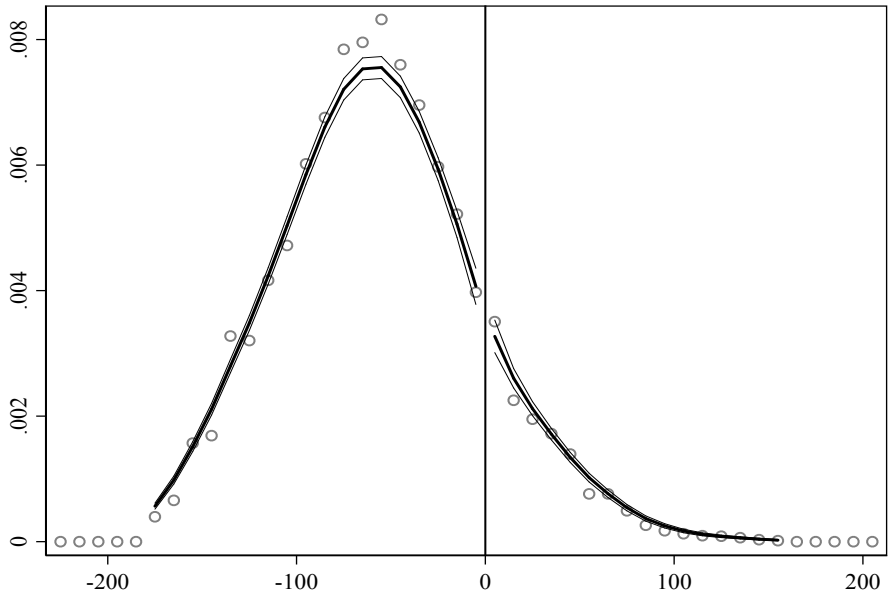

Panel A. Full Analytic Sample



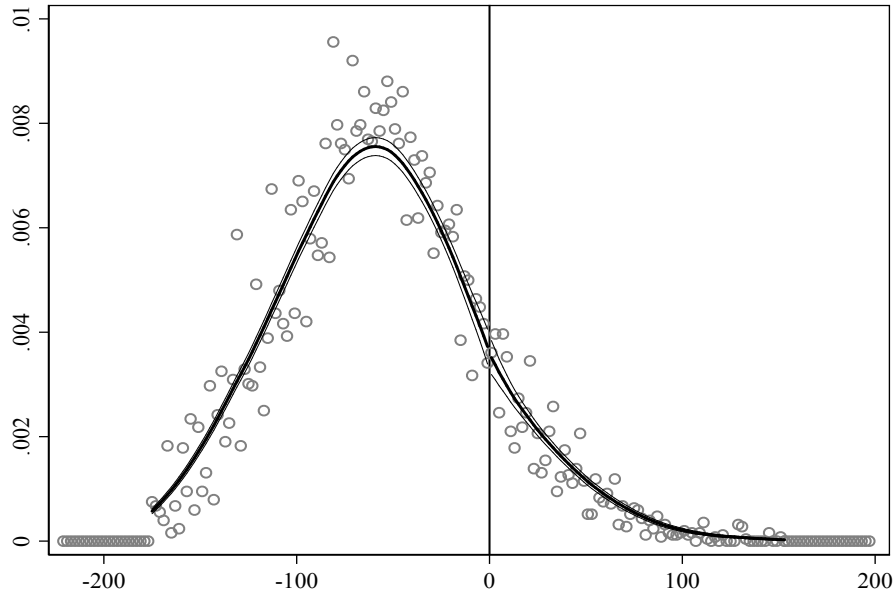

Panel B. Narrow Sample

*Notes:* The full analytic sample presented in Panel A includes students who took the CELDT for the first time in Kindergarten between SY 2006-07 and SY 2012-13, had valid covariate and outcome data and scored above the minimum score Overall and in Listening and Speaking Domains (N=12,706). Panel A graphs frequencies using bin widths of 5. The narrow sample presented in Panel B includes those students that received a binding score between -50 and 50 (N=5,045). Panel B graphs frequencies using bin widths of 2.

Appendix Figure A2. McCrary (2008) Manipulation Tests



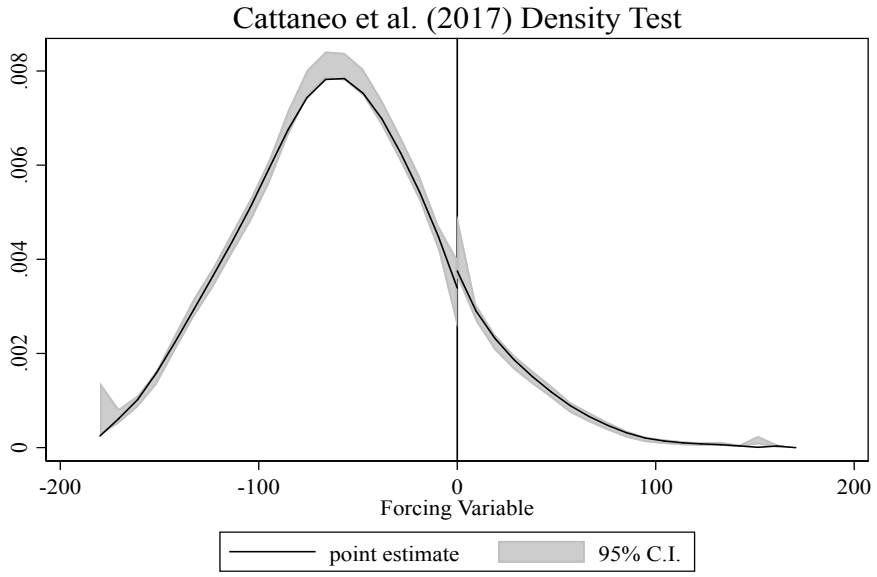

Panel A: Full Analytic Sample, Bin Width of 10




Panel B: Full Analytic Sample, Bin Width of 2

*Notes:* Panel A provides graphical results of the McCrary (2008) test for forcing variable manipulation using bin widths of 10. For this panel, the discontinuity estimate is not statistically significant, with a point estimate of 0.032 with a standard error of 0.070. Panel B provides graphical results of the McCrary (2008) test for forcing variable manipulation using bin widths of 2. For this panel, the discontinuity estimate is not statistically significant, with a point estimate of -0.010 and a standard error of 0.069.

Appendix Figure A3. Cattaneo, Jannson and Ma (2017) Manipulation Test




*Notes:* These graphical results are for the Cattaneo, Jansson and Ma (2017) manipulation test at the threshold. The p-value of the robust discontinuity estimate is 0.0776, indicating that we fail to reject the null hypothesis ($\alpha$=0.05) that the forcing variable is continuous through the threshold.

Appendix Table A1. Auxiliary RD Estimates of Baseline Covariate Balance, by Bandwidth Sample

| | Dependent Variable: $\hat{Y}$ (Placed in SPED Between Grades 1 and 6) | |
|---|---|---|
| Bandwidth Sample | (1) | N |
| Full Sample | 0.00208 | 12,607 |
| | (0.00150) | |
| +/- 150 | 0.00227 | 12,261 |
| | (0.00147) | |
| +/- 100 | 0.00170 | 9,999 |
| | (0.00147) | |
| +/- 50 | 0.00202 | 5,045 |
| | (0.00155) | |
| +/- 34 | 0.00035 | 3,267 |
| | (0.00161) | |

*Notes* : Robust standard errors are in parentheses. Each cell shows the estimate from a two-stage regression. In the first stage, the "Placed in SPED Between Grades 1 and 6" indicator is regressed on all baseline covariates and a predicted index is generated. In the second stage, the predicted index is regressed on the "Placed in SPED Between Grades 1 and 6" indicator and a linear spline of the binding score forcing variable. The +/- 34 bandwidth is the CCT optimal bandwidth. School-cohort fixed effects are included in each step of each model.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Appendix Table A2. OLS: Estimated Association Between EL Status and SPED Placement Across Time Horizons

| | Dependent Variable: Placed in SPED Between Grade 1 and Grade *t* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | *t* = 3 | *t* = 4 | *t* = 5 | *t* = 6 | *t* = 7 | *t* = 8 | *t* =9 | *t* =10 |
| Independent Variable: | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Assigned EL by District | 0.021*** | 0.029*** | 0.033*** | 0.034*** | 0.034*** | 0.035*** | 0.033*** | 0.026** |
| | (0.004) | (0.005) | (0.005) | (0.006) | (0.007) | (0.007) | (0.009) | (0.011) |
| N | 17,714 | 15,983 | 14,280 | 12,607 | 10,777 | 8,932 | 7,239 | 5,069 |
| $R^2$ | 0.067 | 0.071 | 0.074 | 0.077 | 0.079 | 0.081 | 0.081 | 0.089 |

*Notes:* Robust standard errors are in parentheses. Each model reports the OLS estimate of EL status on subsequent Special Education placement across a different time horizon. All models include a full set of student-level covariates and school-cohort fixed effects (coefficients suppressed). The underlying sample includes students who took the initial CELDT assessment in kindergarten between SY 2006-07 and SY 2015-16 and scored above the minimum possible score Overall and on Listening and Speaking domains. The sample size of the panel declines as the length of the time horizon increases. Cohorts with incomplete data are dropped (see Figure 1).

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Appendix Table A3. Reduced Form Effect of EL Classification on Subsequent SPED Placement over Longer Time Horizons, by Bandwidth

| | Dependent Variable: Placed in SPED between Grades 1 and *t* | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *t=3* | | *t=4* | | *t=5* | | *t=6* | | *t=7* | | *t=8* | | *t=9* | | *t=10* | |
| Bandwidth Sample | (1) | N | (2) | N | (3) | N | (4) | N | (5) | N | (6) | N | (7) | N | (8) | N |
| Full Sample | -0.006 | 17,714 | -0.003 | 15,983 | -0.011 | 14,280 | -0.015* | 12,607 | -0.016* | 10,777 | -0.015 | 8,932 | -0.018 | 7,239 | -0.010 | 5,069 |
| | (0.005) | | (0.006) | | (0.007) | | (0.008) | | (0.009) | | (0.010) | | (0.012) | | (0.015) | |
| +/- 150 | -0.006 | 17,282 | -0.005 | 15,584 | -0.012* | 13,901 | -0.017** | 12,261 | -0.018* | 10,455 | -0.017 | 8,627 | -0.019 | 6,959 | -0.015 | 4,827 |
| | (0.005) | | (0.006) | | (0.007) | | (0.008) | | (0.009) | | (0.010) | | (0.012) | | (0.015) | |
| +/- 100 | -0.007 | 14,242 | -0.004 | 12,788 | -0.012 | 11,375 | -0.018* | 9,999 | -0.014 | 8,556 | -0.010 | 7,029 | -0.017 | 5,630 | -0.014 | 3,832 |
| | (0.006) | | (0.007) | | (0.008) | | (0.009) | | (0.011) | | (0.012) | | (0.014) | | (0.017) | |
| +/- 50 | -0.006 | 7,414 | -0.002 | 6,576 | -0.010 | 5,793 | -0.010 | 5,045 | -0.009 | 4,320 | -0.006 | 3,600 | -0.020 | 2,874 | -0.038* | 1,823 |
| | (0.008) | | (0.009) | | (0.011) | | (0.012) | | (0.014) | | (0.016) | | (0.019) | | (0.022) | |
| +/- 34 | -0.002 | 4,882 | 0.006 | 4,322 | -0.002 | 3,783 | -0.007 | 3,267 | -0.002 | 2,799 | 0.007 | 2,356 | -0.007 | 1,894 | -0.024 | 1,183 |
| | (0.009) | | (0.011) | | (0.012) | | (0.015) | | (0.018) | | (0.020) | | (0.023) | | (0.026) | |

*Notes:* Robust standard errors are in parentheses. Each cell represents a separate regression. The +/- 34 bandwidth is the CCT optimal bandwidth. All models include a linear spline of the forcing variable, cohort-school fixed effects, and the following student-level controls: an indicator for female; an indicator for the student's highest educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; an indicator for declining to state race/ethnicity; and the student's age at initial assessment (coefficients suppressed). Cohorts with incomplete data are dropped (see Figure 1).

*** p < 0.01 ** p < 0.05 * p < 0.1

## Appendix B

An important component of our current study is the focus on students who took the CELDT assessment during the Kindergarten year. In an effort to ensure common comparisons over time, we restrict our analytic sample to this particular subset of test takers. However, it is important to note that this subset accounted for 56 percent of all initial CELDT assessments administered by the district. The remaining 44 percent were spread across students transferring into the district and taking the initial assessment at other grade levels. Importantly, these numbers were quite evenly distributed with no other grade level having more than 8 percent of the initial CELDT assessments overall. After Kindergarten, the next most common grade levels for initial assessments were grade 9, grade 1, grade 10, and grade 2.

An additional RD analysis on students taking the initial CELDT assessment at other grade levels is warranted but makes interpretation of findings less straightforward. In this Appendix we provide initial results for the sample of students that took the initial CELDT assessment in Grades 1-5 (i.e., excluding our main cohorts of Kindergarten initial CELDT assessment takers). Appendix Table B1 reports summary statistics for this additional sample. Here, we notice several key points that differentiate this sample from the set of students who enter the district in Kindergarten. First, students taking the initial CELDT assessment in later grades were less likely to speak either Spanish or Chinese and more likely to speak another non-English language as their primary language. Second, these students were more likely to have a parent with at least a high school diploma. Third, students in this subsample were less likely to be classified as ELs than students who took the assessment in Kindergarten. Fourth, SPED identification rates were slightly higher for this subset of students.

We also explore the results from our main RD specification on this subsample of students. Importantly, the sample size is smaller than the sample included in the main panel, somewhat reducing our statistical power. Appendix Table B2 presents results from these specifications. Column (1) reports RD results for being identified as a SPED student between 1 and 6 years after the initial CELDT assessment. In Column (2), we report RD results for the same outcome, but including individual-level controls. In Column (3), we retain these individual-level controls and also add school-cohort fixed effects. In cognate form to the results presented in Tables 5-7 of the main analysis, we report our results across multiple bandwidth samples.

Across bandwidth samples and specifications with and without controls, our results are not statistically significant. In Column (1), we observe a slightly positive point estimate for the full sample and for the CCT optimal bandwidth. For all other bandwidth samples, our estimates are small and negative. A similar pattern is observed when we include student-level controls in Column (2). In Column (3), we see that the full sample result remains slightly positive, but the point estimate for the CCT optimal bandwidth sample becomes slightly negative. Largely, these estimates all suggest that the representation of EL students who took the initial assessment in Grades 1-5 was fairly close to proportionate. While this result is quite similar to what we found for the Kindergarten sample, a more detailed interrogation of the differences between these subsets across a longer period of time is worth exploring in future work.

Appendix Table B1. Summary Statistics for the Grade 1-5 Initial CELDT Taker Sample

| Variable | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| *Student Characteristics in Year of Initial Assessment* | | | | |
| Female | 0.46 | 0.50 | 0 | 1 |
| Hispanic | 0.32 | 0.47 | 0 | 1 |
| Chinese | 0.24 | 0.43 | 0 | 1 |
| Decline to State Race/Ethnicity | 0.04 | 0.19 | 0 | 1 |
| Parent's Highest Education Level >= High School Diploma | 0.60 | 0.49 | 0 | 1 |
| Age at Initial Assessment (approximation) | 8.21 | 1.90 | 5.2 | 12.8 |
| Primary Language: Spanish | 0.26 | 0.44 | 0 | 1 |
| Primary Language: Mandarin/Cantonese | 0.21 | 0.41 | 0 | 1 |
| Primary Language: Other | 0.53 | 0.50 | 0 | 1 |
| *California English Language Development Test* | | | | |
| Binding Score | -40.72 | 69.83 | -179 | 148 |
| Centered Overall Scale Score | -40.04 | 69.90 | -179 | 148 |
| Centered Listening Scale Score | 44.92 | 73.42 | -178 | 197 |
| Centered Speaking Scale Score | 56.32 | 81.36 | -177 | 284 |
| Binding Section: Overall | 0.96 | 0.20 | 0 | 1 |
| Binding Section: Listening | 0.03 | 0.17 | 0 | 1 |
| Binding Section: Listening | 0.01 | 0.12 | 0 | 1 |
| *English Learner and Special Education* | | | | |
| Assigned EL by District | 0.597 | 0.491 | 0 | 1 |
| Below (Binding Score < 0) | 0.704 | 0.456 | 0 | 1 |
| Special Education Between 1 and 6 Years Later | 0.086 | 0.280 | 0 | 1 |
| Special Education 1 Year Later | 0.023 | 0.151 | 0 | 1 |
| Special Education 2 Years Later | 0.020 | 0.139 | 0 | 1 |
| Special Education 3 Years Later | 0.019 | 0.136 | 0 | 1 |
| Special Education 4 Years Later | 0.010 | 0.099 | 0 | 1 |
| Special Education 5 Years Later | 0.008 | 0.089 | 0 | 1 |
| Special Education 6 Years Later | 0.006 | 0.080 | 0 | 1 |

*Source:* California Longitudinal Pupil Achievement Data System (CALPADS), SY 2006-07 through SY 2018-19.

*Notes:* The full sample includes students from this district who took the California English Language Development Test (CELDT) for the first time in Grades 1-5 between SY 2006-07 and SY 2012-13, had valid covariate and outcome data, and scored above the minimum score Overall and in Listening and Speaking domains (N=2,502). Excluded from the sample are students who took the initial CELDT assessment for the first time in Kindergarten. The intent-to-treat group includes students who scored below the cutoff (N=1,762). The control group includes students who scored above the cutoff (N=740).

Appendix Table B2. Reduced Form Effect of EL Classification on Subsequent SPED Placement for the Grade 1-5 Initial CELDT Taker Sample, by Bandwidth

| | Dependent Variable: Placed in SPED between 1 and 6 Years After EL Classification | | | |
|---|---|---|---|---|
| Bandwidth Sample | (1) | (2) | (3) | N |
| Full Sample | 0.015 | 0.006 | 0.013 | 2,502 |
| | (0.017) | (0.017) | (0.021) | |
| +/- 150 | 0.000 | -0.006 | 0.009 | 2,339 |
| | (0.018) | (0.018) | (0.021) | |
| +/- 100 | -0.018 | -0.021 | -0.013 | 1,886 |
| | (0.020) | (0.020) | (0.025) | |
| +/- 50 | -0.012 | -0.015 | -0.007 | 1,101 |
| | (0.028) | (0.027) | (0.039) | |
| +/- 34 | 0.024 | 0.026 | -0.001 | 774 |
| | (0.033) | (0.032) | (0.047) | |
| Student-Level Controls | no | yes | yes | - |
| Cohort-School FE | no | no | yes | - |

*Notes:* Robust standard errors are in parentheses. All models include a linear spline of the forcing variable. Each cell represents a separate regression. The +/- 34 bandwidth is the CCT optimal bandwidth. Models reported in columns (2) and (3) include the following student-level controls: an indicator for female; an indicator for the student's most educated parent being at least a HS graduate; an indicator for being Hispanic; an indicator for being Chinese; an indicator for declining to state race/ethnicity; and the student's age at initial assessment (coefficients suppressed). The model reported in column (3) also includes school-cohort fixed effects (coefficients suppressed).

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$