Tags and a Leaky Pipeline in School Districts' Allocations to Students

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Abstract

A variety of social policies use "tags"–labels that mark a class of recipients as deserving of extra resources– to direct resources towards individuals. In the case of children and education, important tags include those indicating the child qualifies for Free or Reduced Price Lunch (FRPL), English Language Learners (ELL), or students with disabilities (IEPs), and more. A wealth of research studies how tags structure *between*-district resource allocations. Researchers investigate how state legislatures, often under pressure from school finance litigation, change the weights they attach to certain tags like poverty status to distribute state budget allocations between districts; researchers then study how these changes affect student outcomes. Much less research studies how tags structure *within*-district resource allocations. The present paper asks: is there a "leaky pipeline" where resources intended for students with certain tags "leak out" to students with a tag accompanied with stronger legal tools to influence allocations? The analysis proceeds in two steps. After showing how the tag of disability status (IEP) is associated with stronger legal tools for parents to influence district resource allocations than other tags, we use a unique exogenous increase to funding allocated to students with disabilities– the American Recovery and Reinvestment Act (ARRA) causing a one-time doubling of federal allocation to districts to cover the costs of disability services– to show that the legal tools attached to the disability tag are primarily used to influence district resource allocations. After establishing that 1) the disability tag is associated with stronger legal tools than other tags, and 2) the legal tools are used primarily to influence district resource allocations, we investigate the consequences for between-student allocations. We use a regression discontinuity design (RDD) caused by a threshold in a new California grant where districts above a threshold for grant receipt receive resources intended for students with tags *other than* disability status. We find that despite this targeting of resources towards students with other tags, some of the resources appear to "leak out" to students with the disability tag as shown by decreases in parent complaints over services. These reduced complaints indicate less contention over resources and the money "leaking out" from its intended recipients (students with other tags like ELL) to special education students, a tag with stronger legal tools.

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1. Introduction

A variety of social policies use "tags" to outline which recipients should receive the benefits from a policy. These tags–the elderly; veterans; mothers with dependent children; those with disabilities–are legally-binding constraints on who receives a resource. Researchers have noted the prevalence of tags for both cash and in-kind transfers in U.S. social policy (2, 10). When used to structure social policy allocations, many tags share a common structure: rather than granting different sizes of transfers based on a continuous measure of need, the tags often involve binary distinctions where those who qualify for the tag get some quantity of the resource[1] and those who fail to qualify for the tag get nothing.

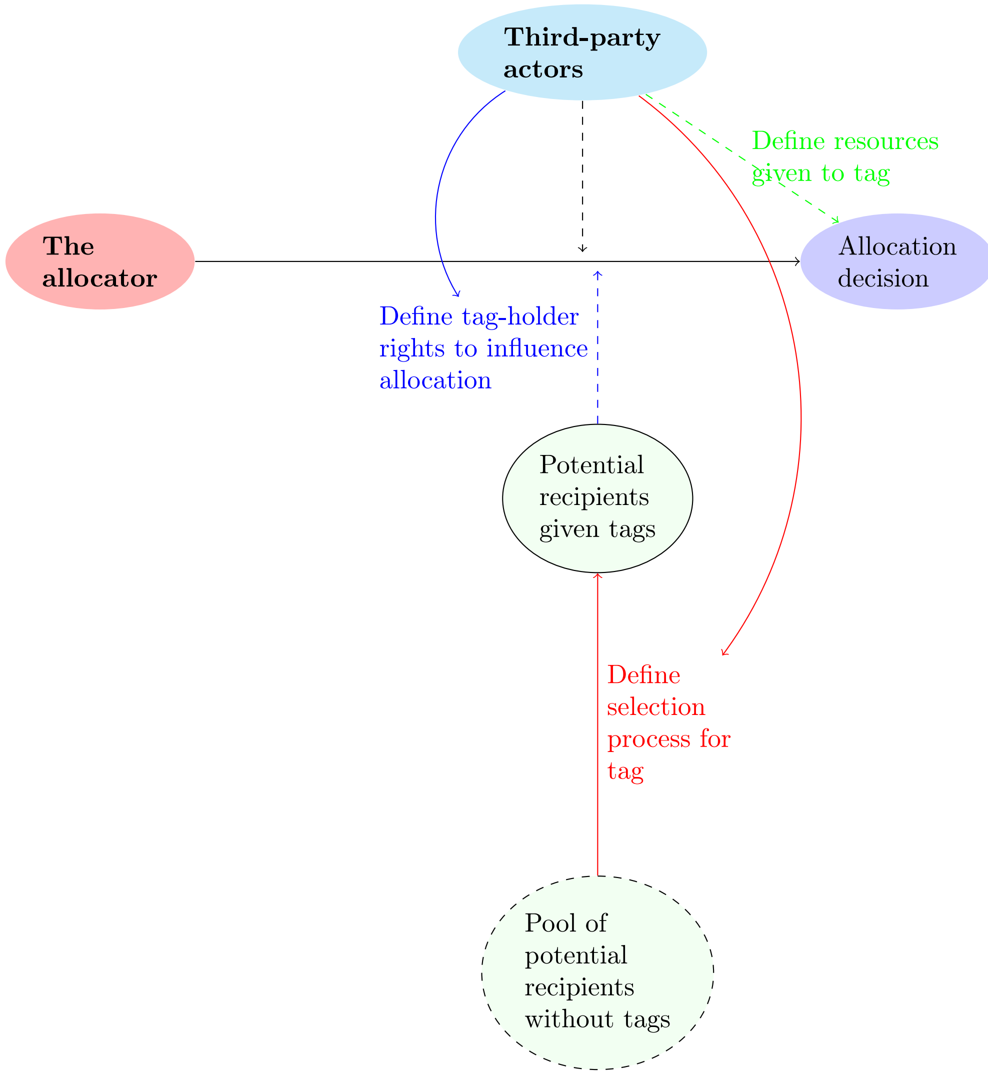
Figure 1 highlights how tags constrain resource allocation decisions and the role that third-party actors like the federal government play in defining who qualifies for the tag, which resources tag-holders receive, and what rights the tag-holders have to shape those resource allocations. The figure also highlights three ways in which tags vary.

The first way that tags vary is in how tags filter a pool of potential recipients into tag-holders given rights to access a benefit. Which criteria are used to select tag-holders who have rights to tag-associated resources? For instance, is the the tag defined by an easy-to-assess category like age or a more difficult-to-assess category like disability status? The second way in which tags vary are the resources attached to a tag. For instance, do Medicare benefits include prescription drugs? Which foods and beverages can individuals receiving Supplemental Nutrition Assistance Program (SNAP) benefits purchase (green arrow in Figure 1)? The third way in which tags vary, and the focus in the present paper, are in the rights that tag-holders have to influence allocations (blue arrow), either by protesting a denial of an allocation or, once allocated, protesting its termination. In other words, what rights do tag-holders have to appeal an unfavorable allocation decision?[2]

[1] This quantity can vary in magnitude or be equal across tagged recipients.

[2] To make the concepts in Figure 1 more concrete, consider how the shift from Aid to Families with Dependent Children (AFDC) to Temporary Assistance to Needy Families (TANF) as part of the 1996 welfare reform re-configured each of the components. The first shift was in how the federal government defined the tag (red arrow). The federal government not only adopted a more restrictive tag of its own that added work requirements to income ones, but also weakened its influence on the tag (weaker red arrow). The government allowed states to define tags more restrictive than the federal ones, such as setting more stringent work or time limits or counting asset exemptions differently (13). The second shift was in the resources tag-holders were entitled to (green arrow), though this shift was more minor than the other two. States continued to have latitude in setting the generosity of benefits associated with the tag, though the block grant financing allowed states to spend portions of the grants not on direct cash transfers to individual tag-holders but on broader supports useful for the *class* of tag-holders like childcare initiative or expanded pre-K (11, 23). Finally, commentators argue that the shift from AFDC to TANF not only

Figure 1: Actors involved in resource allocations: the role of tags



The present paper focuses on this third aspect of tags, asking: when clients of the same organization are given tags with different rights, how do these disparities shape resource allocations? The remainder of the paper proceeds as follows. Section Two outlines the role of tags in school districts and shows how

changed which individuals had access to the tag and the depth of resources tag-holders were promised, but also changed the appeal rights available to tag-holders (blue arrow)(14). In particular, in 1970's *Goldberg v. Kelly*, the Supreme Court ruled that although the U.S. constitution lacks a positive right to welfare, welfare benefits are a form of property and, as a result, holders of the benefit are entitled to the "due process protections of prior notice and a hearing" before the government terminates their benefits. These due process rights are grounded in a recipient's "legitimate expectation of an entitlement." Yet later case law clarified that this legitimate expectation is contingent upon the process used to allocate the benefit. Cases like *Eidson v. Pierce* asserted that individuals lack property rights, and thus lack due process protections, for benefits like housing vouchers where parties involved in the allocation process have substantial discretion in deciding who accesses the benefit. (14) argues that the partial privatization that accompanied welfare reform–states contracting out essential functions of benefits allocation like eligibility terminations and terminations for non-compliance to private firms–might have undermined due process protection for benefit holders by undermining the benefit's entitlement status.

the tag of disability is accompanied by strong formal tools to shape resource allocations. Section Three uses two sources of evidence–qualitative evidence on what disputes are about; quantitative evidence on how disputes respond to resource influxes–to establish that when parents use these tools, they are using them to try to shape decisions about resource allocations. In particular, the quantitative portion of the Section investigates: when resources become more plentiful, do we see a drop in parent complaints about special education services? If we observe a drop, we can be more confident in claiming that parents are using the tools predominantly to protest the denial of resources to their child. Section Three establishes that complaints *fall* when resources to students with the tag of disability *increase*. Section Four uses a threshold for a state grant in California to investigate whether complaints *fall* when resources intended for students with other tags increase, providing evidence of a "leaky pipeline."

The results have implications for between-child welfare. In particular, tags reflect social policy decisions about how different classes of students should be prioritized in resource allocations. For instance, when a state uses a weight of 1.5 for each student living in poverty, it roughly intends for that student to receive 50% more resources than students without the tag. While other research has studied cross-subsidization–how funds directed towards *general education* students without tags end up being diverted towards students with disabilities (8)–the present project studies a particular form of diversion of funds intended for students with other higher-need tags.

2. The specific case: school districts allocating scarce resources between students with different tags

School districts are a unique example of the tag-based allocation processes that Figure 1 depicts due to three features. First, rather than tag-holders receiving resources directly as soon as they are disbursed, resources intended for tag-holders are allocated first to school districts who must then allocate the resources to tag-holders. This creates the potential for a "leaky pipeline" where resources disbursed to a district and intended for one tag-holder are diverted to a different student. Second, students with different tags have different formal rights to stem this leaky pipeline and to influence the resources attached to the tag. Third, the tag to which the federal government attaches the strongest rights to influence resource allocations–the disability tag–receives little funding from the branch of government that defines the scope of those rights. The present paper investigates whether the intersection of these three features creates a "leaky pipeline" where resources intended for students with one tag are diverted to students with other tags.

2.1. Feature one: the potential for a leaky pipeline between resources intended for students with a tag and these students

Many social welfare programs give resources fairly directly to individuals with a tag: for instance, the Social Security Administration gives resources directly to individuals with the tag of elderly; the U.S. Department of Agriculture gives resources directly to individuals who meet the income tags required for the Supplementary Nutrition Assistance Program (SNAP).[3] But allocations to students with tags that entitle the student to extra resources go through two distinct allocation processes that Figure 2 depicts.

First, federal, state, and local governments allocate funds to districts that are adjusted upward for the presence of students with various tags (green box). These tags have the strongest influence on state budget allocations, which compose roughly 45% of funding to districts (with local sources making up another 45% and the federal government roughly 10%).[4] State legislatures specify formulas where they start with a base allocation calculated using the number of students in a district and then add extra "weights" to "direct additional funding to students who require additional resources"(Center). These weights are attached to tags: most prominently English Language Learners (ELL), recipients of Free or Reduced Price Lunch (FRPL) or the updated allocations of Free or Reduced Price Meals (FRPM), and students with disabilities receiving an IEP. Some tags also operate at the district or school level through aggregation of individual student tags–for instance, "Title I" schools are those where over 40% of the students qualify for free or reduced price meals. States vary in which tags they use–as a recent review summarizes, 37 states use at least one student multiple in the allocations they make, and some states use many. Oregon, for instance, has multiples for not only English language learners, free or reduced-price lunch students, and students with disabilities, bur also

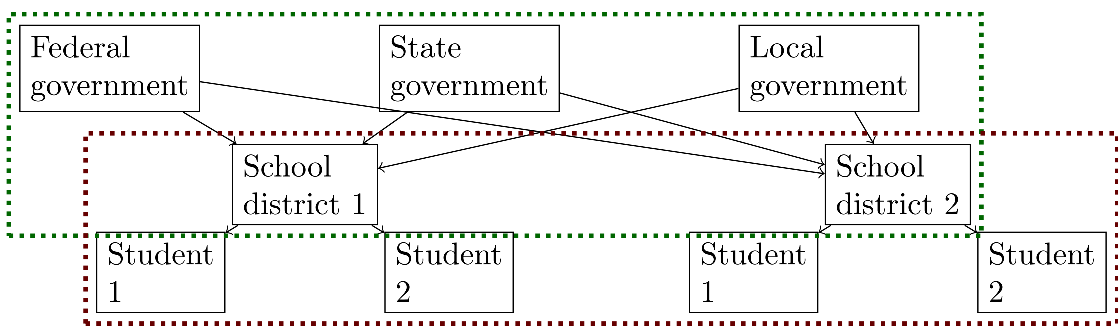
[3] One could object, citing (17), that front-line workers in local bureaucracies–for instance, bureaucrats in "Food Stamp" field offices–are involved in allocating even these more "direct" goods. But while these offices are involved in *administering* the program, they give the resource to all individuals with the tag as opposed to having a resource that they must allocate between tagged and non-tagged individuals.

[4] These proportions vary substantially across districts, with lower-SES districts receiving a higher proportion of funding from state and federal sources.

multiples for those who are pregnant or parenting, classified as "delinquent," and students in foster care.

The majority of research on tags and schooling focuses on how tags influence this first level of allocation. In particular, researchers study court-ordered school finance reform (SFR) where, as a result of litigation and a successful settlement or ruling, a state legislature agrees to change its formula to attach a new set of weights to a tag like free or reduced price lunch status.(5, 9, 16, 15).[5] The research then examines the effects of this more redistributive spending scheme on outcomes like per-pupil state aid and socioeconomic gradients in achievement; recent research documents significant effects on each, with new formulas leading to increases in state aid and reductions in achievement gaps between students in lower and middle-SES districts (16).

Figure 2: **Two levels of resource allocations**. The green box describes allocations *to* school districts; the red box describes allocations *within* school districts (the focus of the dissertation). Depending on the size of the district, there is another layer–district allocations to schools within the districts–with disparities emerging in larger districts that operate many schools.



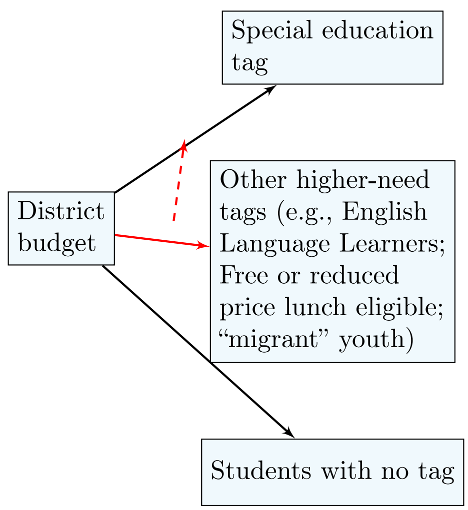
The first set of research thus examines the green box in Figure 2: how do tags influence state aid to districts and how do court-ordered changes to the tags affect student outcomes? Yet once a formula is changed and resources from state sources enter the "black box" of school districts (19), what shapes whether extra resources intended for students with a tag reach their intended beneficiary? The red box in Figure 2 highlights this level of allocation. And the tools that affect allocations at this level are not broad class-action suits such as *Abbott v. Burke*, *Serrano v. Priest*, and other pivotal cases that cause court-ordered school finance reform, but instead are tools that parents use to shape *within*-district resource allocations between students.

[5] After a failure in *San Antonia Independent School District v. Rodriguez* to ground these school finance cases in the equal protection clause of the U.S. constitution, the private enforcement strategy in school finance litigation was to use provisions in state constitutions to make arguments against the link between local property wealth and per-pupil financing.

2.2. Feature two: federal government grants different formal rights to tag-holders

In turn, the formal tools that parents can use to shape district and school resource allocations vary widely between three sets of students. In contrast to other entitlements, where tag-holders may receive different *magnitude* of resources in proportion to characteristics like household size but all individuals have the same *formal rights* to shape aspects of benefit allocation like appealing a termination of benefits, school districts are unique in that they serve populations with different formal rights to shape resource allocations. Figure 3 depicts this structure, where district budgets allocate resources between students whose tag comes with strong formal rights to shape allocations (students with the tag of disability), students whose tag comes with weaker formal rights to shape allocations (tags like ELL), and students without a tag. This configuration can create the potential for a "leaky pipeline" where resources intended for tag-holders with weaker rights to shape allocations are diverted to tag-holders with stronger rights.

Figure 3: A potential leaky pipeline of resource allocations



The students with the strongest formal rights to shape resource allocations are those who qualify for a disability tag, now defined under the Individuals with Disabilities Education Act (IDEA). The federal government plays a strong role in defining the tag and its associated rights and resources. The IDEA's precursor, 1975's Education for All Handicapped Children Act (EAHCA), was passed during a historical era characterized as a "rights revolution" marked by increased demands for legislatures and courts to protect the rights of various minority groups (12). Particularly important for special education's advent and the contours of its private enforcement system was 1954's *Brown v. Board of Education*. Disability rights advocates recognized that *Brown v. Board*, by extending the constitution's "equal protection clause" to argue that the federal government should take proactive action to provide minority students with equal opportunities to majority students, could be used as a template to argue that the federal government should take proactive action to provide a specific type of "minority"–students with disabilities–equal opportunities to non-disabled students within the public school system (24). The final legislation, modeled after two successful class action suits that disability rights advocates had filed in Pennsylvania and D.C., granted federal and state administrative agencies some power to enforce the statute's provisions: for instance, the Department of Education's Office of Special Education Programs (OSEP) was granted the power to initiative investigations into school districts flagged through various means as being non-compliant.

Yet the main feature of the IDEA's precursor was not a strong *administrative agency* enforcement system but a strong *private enforcement* system that grants parents litigable rights to resources. At the cornerstone of this private enforcement system is the IEP, an individualized contract between a student and a school district that only that particular student's parent had legal standing to enforce (20). IEPs result from a negotiation between parents and a school district where a parent or teacher can request that a child be evaluated, experts weigh in on whether the child's difficulties fall into one of thirteen federally-recognized disability categories, and the district decides whether the IEP should be issued.

In turn, IEPs come with rights to not only influence *whether* a student receives an allocation of a fixed size but also the *magnitude* of that allocation. For instance, a parent or advocate for the child may propose 60 minutes of speech therapy a week, a district counters with 20 minutes, and the IEP ends up somewhere in between (6). Thus, while tags for entitlements like Medicaid and SNAP are accompanied by some rights to influence allocations–for instance, the right to a hearing to challenge an inappropriate denial of access to the resource; the right to a hearing to challenge a termination–the tag of disability is accompanied by strong rights to influence not only the *presence or absence* of an allocation but also the *magnitude* of these allocations.

The formal rights that accompany the disability tag compare favorably not only to the rights attached to other entitlements but also to the rights to resources held by two other classes of students: students with other tags (e.g., ELL; FRPL) and students without tags. Tags like ELL come with some rights to influence resource allocations. Yet those rights are largely limited to parents submitting complaints to the Department of Education's (DOE) Office for Civil Rights (OCR) or the Department of Justice's (DOJ) Educational Opportunities Section of the Civil Rights Division, asking either department to investigate a district for non-compliance via inadequate service provision or accommodations for ELL's. The burden of proof is much higher in these complaints than complaints parents can file on behalf of children with disabilities–the investigation must conclude that there is a *district-wide* pattern of non-compliance as opposed to non-compliance for a particular student (22)–and potentially as a result, these complaints are rare: systematic data on the number of complaints are not available, but one article reports 475 complaints submitted nationwide regarding ELL issues between 2011 and January of 2015 (Brown). If we examine complaints about services for students with disability over a smaller time span from the 2009/2010 school year to the 2011/2012 school year, there were 41,944 due process complaints submitted nationwide.[6] This stronger enforcement system is not a historical accident. Instead, it results from greater political successes of parents with children with disabilities than parents of children with needs like ELL to strengthen legal tools through measures like 1986's fee-shifting provision.[7]

Meanwhile, parents of students with no tags have few legal tools to shape resource allocations by districts, leading to other forms of "voice" like attendance at school board meetings or complaints to the school principal.

This situation of a school district allocating resource it receives *for* students with certain tags to a mix of students–tag-holders with stronger rights to influence allocations; tag-holders with weaker rights; non tag-holders–raises questions about how these tools shape resource allocations.[8] More specifically, focusing

[6] Although investigations occur at the district level, complaints could theoretically be submitted by multiple parents or interest groups associated with the same district, making the comparison to due process complaints potentially submitted by multiple parents in the same district more appropriate. Unfortunately, a direct comparison of years is not possible due to when the data on due process hearing ends. OCR

[7] However, we see some retrenchment of this private enforcement regime with the 2004 IDEA amendments. Most notably, the amendments added a provision where parent's attorneys could be held liable for the district's attorney fees if the case is deemed "frivolous, unreasonable, or without foundation" or filed "for any improper purpose, such as to harass, to cause unnecessary delay, or to needlessly increase the cost of litigation"(*IDEA Sec. 300.517 Attorneys' Fees.* Though a detailed account of the political history behind these revisions remains to be written, reviewing the Congressional Record discussions around the provisions shows that some legislators were motivated by concerns from constituents about inappropriately high litigation—e.g, Senator Charles Grassley, "I have heard from many Iowa educators that the Federal IDEA law is too litigious." Senator Edward Kennedy, a notable disability rights advocate, supported the fee provision, saying that "no one wants to see our courts abused by frivolous cases and everyone wants to see less IDEA litigation," but also argued that he could not "stand by and listen to a debate that unfairly characterizes the majority of parents and the majority of attorneys as eager to sue schools." Congressional Record Online, May 12, 2004, Pages S5349-S5351.

[8] We can think of this as delving into some of the mechansims behind cross-subsidization. In the school district case, Conlin

on *within*-district allocations, we ask: is there a "leaky pipeline" where resources intended for students with certain tags "leak out" to students with a tag accompanied with stronger legal tools to shape resource allocations? To do so, we use a regression discontinuity design (RDD) that compares two sets of school districts. First are districts that fall right above a threshold for grant receipt–these treatment districts receive money meant for students with tags like ELL, migrant youth, and others associated with weaker legal tools for influencing resource allocations. Second are districts that fall right below the threshold for grant receipt. To investigate whether the money was going for its intended recipients or "leaking out" to other students, we examine whether districts in the first category experience fewer parent complaints over inadequate special education resources compared to that same district before the grant. These reduced complaints indicate less contention over resources and the money "leaking out" from its intended recipients (students with other tags like ELL) to special education students, a tag with stronger legal tools for enforcement.

Broadly, the RDD finds that when an organization experiences a resource influx, parents with stronger legal tools can potentially use these tools to divert those new resources away from children of parents with weaker tools. As a note, this evidence is indirect: it rests on the assumption that resources *not intended* for students with the tag of special education *should have no effect* on complaints related to this tag. Therefore, it indirectly assumes that if we *do see* that the new resources decrease complaints, *at least some portion* of the resources are leaking out to students with disabilities. Alternately, for students with multiple tags–for instance, a student with disabilities who is also an English Language Learner–the resources are leaking out from one intended use (uses related to English integration) to another intended use (uses related to disability). Before investigating this leaky pipeline, Section 3 establishes that when parents make complaints related to the disability tag, these complaints often focus on resources rather than other considerations (e.g., shielding a child from inappropriate school discipline).

and Jalilevand (2015) have studied how revenues intended for *general education* students leak out to students with disabilities. The present paper studies how revenues intended for other tag-holding students leak out to students with disabilities.

3. Do parents use legal tools attached to the disability tag to influence district resource allocations?

Before examining whether the legal tools attached to the tag of disability create a "leaky pipeline," it's important to establish that when parents *use* these legal tools–most notably, filing a due process complaint with a state administrative hearing system–they are trying to influence resource allocations. The present section first presents *qualitative* evidence that complaints are largely disputes about resources. Then, we exploit an exogenous increase in resources for students with disabilities to present *quantitative* evidence that complaints fall when resources are more plentiful.

3.1. Qualitative evidence that complaints focus on resources

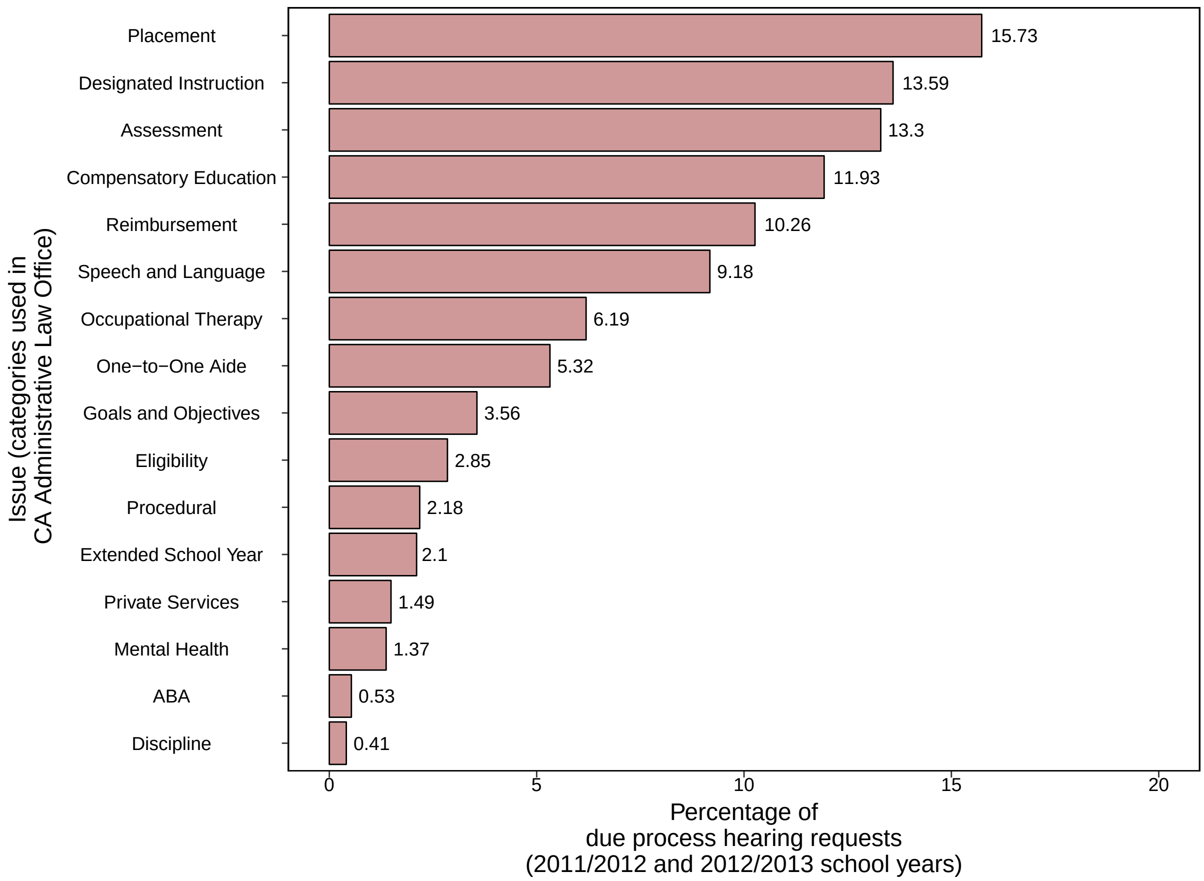
California, the state whose parents complaints we analyze in the next section, summarizes the topics of those complaints in quarterly administrative reports. Figure 4 depicts the breakdown of complaints in school years before the concentration grant goes into effect and for which there is available data.[9] The Figure shows that the issues that generate the highest frequency of contention are those involving "placement," which generally refer to disputes about where the child receives specific resources. Nearly as frequent were complaints about "designated instruction". As defined in the guide to hearings, this captures services that the student is entitled to on account of his or her disability:

> Services required to assist an individual with disabilities to benefit from special education, including but not limited to: transportation, occupational therapy, physical therapy, speech and language therapy, mental health services, and medical care.

The third and fourth most-frequent categories also refer to resource issues– "assessment" refers to the denial of an IEP, Meanwhile, "compensatory education" is the IDEA's equivalent of damages in the tort context; while the IDEA does not offer parents monetary damages to compensate for previous violations of the child's rights, the IDEA instead offers parents services like "summer school, additional therapy hours," and other measures.

[9] The California Office of Administrative Hearings only began publishing the case topics beginning in the 2011/2012 fiscal year: `http://www.dgs.ca.gov/oah/SpecialEducation/Resources/SEReportArchive.aspx`

Figure 4: Breakdown of topics of requests for due process hearings in two school years preceding the concentration grant (2011/2012 and 2012/2013). The percentages sum to over 100% because each request for a hearing can be filed under multiple issues.



The topics also illustrate that complaints about issues *not* related to school resources–most notably, about whether a child was inappropriately disciplined given his or her disability–are rare, with complaints about discipline composing less than half a percent of all complaints. The breakdown provides suggestive evidence that parents file complaints to protest resource-related decisions by districts. As a result, when Section Four analyzes these complaints without regard to their specific topic, we can be more confident that the majority of complaints are focusing on resource issues.

3.2. Quantitative evidence that complaints focus on resources

Another way to establish that complaints focus on resources, and thus that a drop in complaints likely illustrates that resources are reaching students with disabilities, is to show that the *quantity* of complaints drops when there is an exogenous increase to school resources for children with disabilities. One such exogenous increase came as part of the stimulus package following the 2008 financial crisis. The American Recovery and Reinvestment Act (ARRA), signed into law on February 17th, 2009, not only provided money to states to correct for state budget shortfalls that would affect education financing (the "state fiscal stablization funds"), but also resulted in a one-time doubling of the federal allocation to states for special education services. In particular, the previous year's federal allocation for the disability tag was $11 billion dollars, while ARRA allocated an additional $11.7 billion dollars to that existing funding.[10] Half of the funds were disbursed on April 1st, 2009; the other half of the ARRA funds were disbursed in September of 2009.

We can use this large exogenous increase in the money districts had available to allocate to disability services to investigate whether this sharp increase in resources led to decreases in parents of students with disabilities filing complaints against districts. If we *do* observe decreases, it helps establish that parents are using the legal tools associated with the tag to direct district resources towards the tag.

All states and districts were treated by the ARRA funds, meaning we cannot use a difference-in-differences design to examine pre-ARRA versus post-ARRA trends in complaints. Instead, we use an interrupted time series design with state fixed effects to investigate whether there was a drop in complaints during the ARRA grant years (the 2008-2009 school year; the 2009-2010 school year) relative to non-ARRA years. Because time-varying confounders could cause this drop–for instance, a general drop in litigation during the ARRA years due to families struggling financially during the Recession–we test the robustness of these findings using the placebo outcome of trends in child-focused malpractice litigation.

3.3. Data

3.3.1. Dependent variable

The dependent variable is the count of disputes per 10,000 students receiving an IEP. There are two reasons why we measure disputes over *special education students* rather than *all students*. First is that there is significant variation between states in rates of special education placement, so the variable as constructed is designed to pick up variations in disputes *net* of these placement differences. Second is that we if think of a rate as events over a count of those at risk of generating an event, most of the cases are generated by

[10] More precisely, ARRA allocated $11.7 billion for IDEA Part B, which serves students aged 3-21. Additional funding was allocated to IDEA Part C, which serves younger children.

disputes among students *with* IEPs–with the dispute then focused on the content of those IEPs–making a count of those with IEPs the relevant risk pool for generating a case. However, we also test the robustness of the results to examining the rate of disputes over *all students* rather than students with an IEP.

Data for the numerator (count of disputes) are from the Department of Education's (DOE) Office of Special Education Programs (OSEP), available at this link: IDEA data products. Title 1, Part A, Subsection 618 of the IDEA mandates that states submit data on their count of disputes each year, and the present paper uses data from the IDEA Part B reporting, which covers children ages 3 to 21 in special education.[11] The variable is constructed as the sum of three counts: due process complaints, requests for mediation, and written signed complaints. There were no missing data for dispute counts. The denominator–the count of IEPs–comes from the Common Core of Data (CCD) count of all students having an IEP under Part B. Table A1 provides more details on data sources. Observations missing from the CCD data were filled in using the IDEA Section 618 child counts or by contacting state Departments of Education.

3.3.2. Break in complaints

The temporary change in the trend in complaints corresponds to the school years where ARRA grants were disbursed: the 2008/2009 school year (affecting complaints filed later in the academic year) and the 2009/2010 school year (affecting complaints that arise during the fall renewals/re-negotiations of IEPs).

3.3.3. Main covariate: financing formulas

The ARRA grants may have larger effects on complaints in some states than in others. And the main covariate is a state's special educating financing formula. In particular, there are two sets of states. States that use *weighting financing* weight each special education student the district reports serving as a multiple of a general education student for the purposes of the state allocation to the district. For instance, New York uses a single pupil weighting system where each special education student, regardless of his or her disability, is weighted as 1.41 times that of a general education student; so if a district received \$10,000 from the state for each general education student, it would receive \$14,100 for each special education student (1, p. 36). In South Carolina, a multiple pupil weighting system, students classified under autism are

[11] IDEA Part C, which covers early intervention services from birth to age 2 generates close to zero disputes

weighted as 2.57 times that of a general education student, while the "emotionally disabled" are weighted as 2.04 times, which would translate into \$25,700 versus \$20,400 respectively compared to a general education student allocated \$10,000. In states with *capitation financing*, which is also sometimes called *block granting*, a district's allocation is largely not sensitive to the number of special education students the district reports serving.

In turn, we would predict larger drops in complaints during the ARRA stimulus years for states that employ block grant financing than weighting financing, since the former are more likely to have persistent shortfalls in *state* allocations to districts that prompt parent complaints about resource denials.

3.4. Results

3.4.1. Descriptive trends

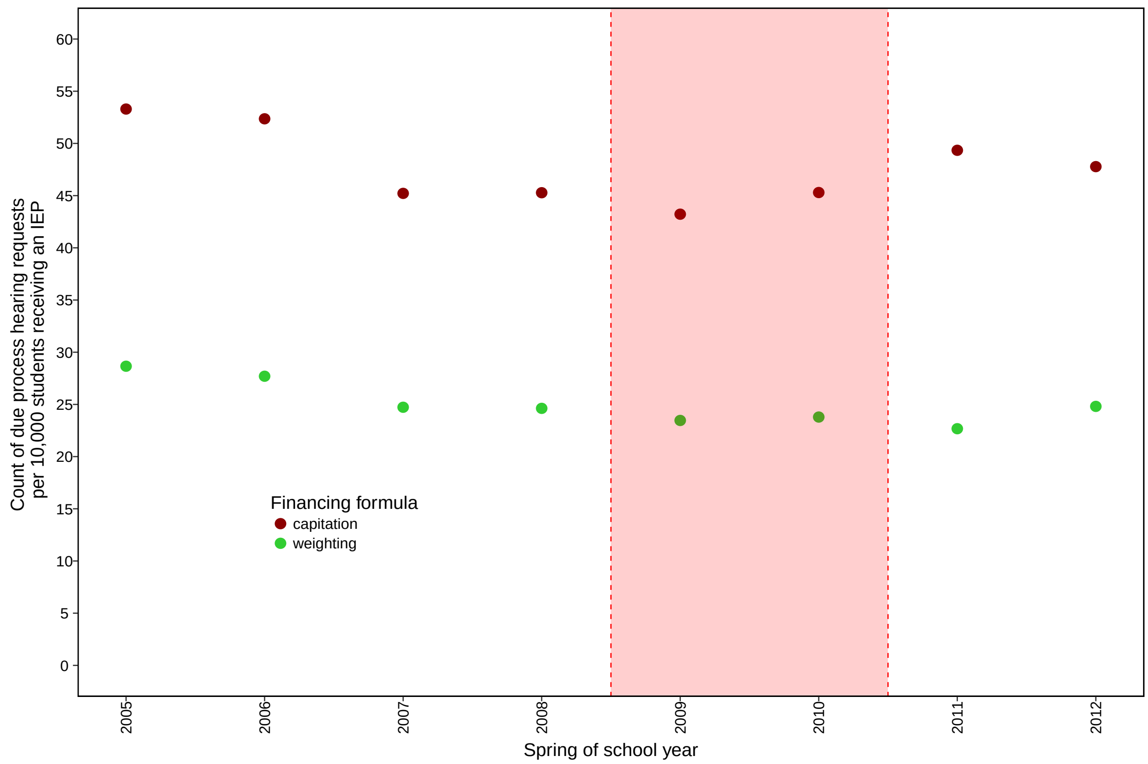
Figure 5 presents the trends in due process rates per 10,000 students receiving an IEP, presenting the mean rates by school year for the two types of states.[12] Two patterns stand out. First, states with the more restrictive capitation financing for special education services tend to have higher rates of disputes throughout the entire period than states with the less restrictive weighting financing. However, there could be unobserved differences between the two types of states that cause both adoption of more restrictive financing of special education services and more disputes between parents and districts. Second, both sets of states appear to experience a drop in disputes during the two ARRA grant years (the 2008-2009 and 2009-2010 school years), with this drop potentially more pronounced in states with more restrictive capitation financing. Appendix Figure A.2 and Figure A.3 present the results separately by state, and show that much of the variation in dispute rates is between-state rather than within-state over time, highlighting the importance of state fixed effects in the subsequent section.

3.4.2. Analytic results

We estimate the following model. y_{it} represents the rate of disputes in state i in year t, α_i represents a state fixed effect that controls for time-invariant unobserved differences between states that affect disputes,

[12] The Figure restricts the sample to the 44 states with no changes in financing formulas during this period. The states that do change *are* included in the analytic models that follow.

Figure 5: Trends in disputes associated with tag of special education



and X_{it} contains a vector of controls by state and year that are plausibly correlated with how much the state benefits from the ARRA grant and dispute rates. In terms of which controls to include, the size of the ARRA stimulus grant to state i for special education followed the same formula that the federal government typically uses to allocate funds to states for special education:[13] the government allocates an amount equal to what the state received in fiscal year 1999, with any extra funds allocated based on the state's number of children in the age range for IDEA Part B (3-21), with a slight upward adjustment for the percentage of those children living in poverty. Therefore, we control for the percentage of a state's children living in poverty and also for the annual unemployment rate, which may reflect poverty not yet captured in the Census definition. The second set of controls are for factors that might lead to variation in the quality of services a school district provides and subsequent disputes. One variable examines, for each state, the share of overall education-directed revenue that comes from federal sources rather than state and local forces, since for states that rely on more federal revenue, the ARRA grant might have had a smaller effect on disputes.

[13] More formally, IDEA Part B.

Another variable captures the expense-revenue ratio in the state's budget, which might affect a state's ability to continue to allocate towards education expenses during the recession.[14] Table A1 outlines data sources for this analysis and that of placebo outcomes.

$$
\begin{aligned}
y_{it} =& \beta_1 \times \text{ARRA year } (1 = \text{yes})_{it} + \beta_2 \times \text{weighting finance } (1 = \text{yes})_{it} + \\
& \beta_3 \times \text{ARRA year}_{it} \times \text{weighting finance}_{it} + \psi X_{it} + \alpha_i + \epsilon_{it}
\end{aligned} \tag{1}
$$

The main coefficient of interest is β_1 (the effect of the ARRA grant on disputes between parents and districts), with the effect allowed to vary across financing regimes.

Table 1 presents the results. The results, which control for time-invariant unobserved features of states associated with disputes, suggest that the ARRA grant years were associated with statistically and substantively meaningful drops in disputes between parents and districts tied to the tag of disability. In particular, a move from a non-ARRA grant year (0) to an ARRA grant year (1) is associated with an average drop in disputes of 3.77 per 10,000 children with an IEP, which is roughly 12% of the average dispute rate of 30.4 disputes per 10,000 children with an IEP. The results also show an association between more generous state financing and lower levels of disputes, identified off of states that shift between financing regimes. Finally, the results are suggestive (but not statistically significant) of a positive interaction effect where states with more generous existing financing experienced a smaller drop in disputes during the ARRA years than states that use block grants to allocate to school districts.

[14] For variables measured using calendar years rather than school years, the variable is matched to the fall of the school year. For instance, the unemployment rate in year 2004 is linked to disputes in 2004-2005 school year.

Table 1: Effect of ARRA grant on disputes over special education

	β (SE) p	
ARRA grant year (1 = yes)	-3.769 (1.58) 0.02	-3.08 (1.22) 0.01
Weighting financing (1 = yes)	-7.259 (2.68) 0.009	-7.846 (2.85) 0.008
ARRA grant year (1 = yes) × weighting financing (1 = yes)	2.005 (1.68) 0.23	1.998 (1.51) 0.19
Observations (50 states × 8 years)	400	400
State fixed effects?	Yes	Yes
Clustered SE by state?	Yes	Yes
Other controls?	No	Yes

3.4.3. Robustness check: defining disputes over all students rather than over students with an IEP

The results in Table 1 define the rate of disputes as the count of complaints over the count of students receiving IEPs:

$$\frac{\text{count disputes}_{it}}{\text{count IEP students}_{it}}$$

But districts that have extra resources as a result of the ARRA stimulus might use these funds in two ways. First is to allocate more IEPs to students–in effect, giving more students the tag of disability and associated rights. Second is that, conditional upon the same count of IEPs, districts may offer more generous services in those IEPs and face fewer disputes from parents.

We can capture these dynamics by looking at the count of disputes over *all students*, which is a function of both the disputes per IEP students and changes in the number of IEPs. We can then analyze this second type of dispute rate–disputes over all students–to make sure that the results above are not fully driven by the *same* count of disputes in the numerator but a higher *number* of IEPs in the denominator (leading to a lower rate).

$$\frac{\text{count disputes}_{it}}{\text{all students}_{it}} = \frac{\text{count disputes}_{it}}{\text{count IEP students}_{it}} \times \frac{\text{IEP students}_{it}}{\text{all students}_{it}}$$

Appendix Table A2 presents the results of the same model in Equation 1 applied to the different rate of the count of disputes over all students, which reflects both drops in disputes in the numerator and any increases in IEPs in the denominator. It shows that the ARRA grant led to significant decreases in disputes defined using this alternate way, though the weaker results suggest that some of the extra funding's impact came through its role in increasing the count of IEPs in addition to increasing the resources (and thus decreasing complaints about) attached to each IEP.

3.4.4. Placebo test: patterns in child-focused malpractice claims

Since the federal government distributed ARRA funds to all states and distributed these funds at the same time, there is no control group of states untreated (or treated at a different time) by the ARRA grant. This lack of control group leads to concerns about time-varying confounding. In particular, there could be unobserved factors occurring during the stimulus years that result in fewer disputes between parents and districts independent of the stimulus funds. If so, this weakens the claim that the drop in cases we observe in the ARRA grant years is caused by the grant itself and weakens our claim that we can use these patterns to show that disputes tied to the tag of disability status are often about school resources.

One type of time-varying confounder are dynamics during the Great Recession that lead to fewer disputes in general between families and professionals who serve their children. For instance, families facing negative income shocks and unemployment during the Recession may have fewer monetary and time resources to devote to litigation in general. We investigate whether this form of confounding might be present by re-estimating the model depicted in Equation 1 with a placebo outcome of rates of child-focused malpractice litigation.

We constructed this rate as follows. First, using data from the National Practitioner's Database (NPDB) of malpractice claims, we filtered the claims to those filed on behalf of children (all claims where the patient age is 19 or under) and estimated the count per state and year.[15] Then, we constructed a rate of child-focused claims per 10,000 children using ACS data on the child population size under 18 in each state.

We hope to see two patterns in this placebo outcomes analysis. First, Figure 5 showed the descriptive pattern of more disputes in states where financing for special education from state sources is more scarce (capitation states). These differences could be related to unobserved features of a state or its parents associated with both the adoption of more restrictive financing regimes and the likelihood that parents dispute decisions by professionals. If child-focused malpractice claims show few patterns by special education financing regime, this supports this interpretation. Appendix Figure A.4 presents the trends in child-focused malpractice claims by state and year. It shows no clear pattern where states with more restrictive financing for *special education services* have different malpractice claims rates.

Second, the figure also suggests that the ARRA stimulus years that *were* marked by drops in disputes over special education services *were not* marked by drops in child-focused malpractice disputes. Examining this more formally, Appendix Table A3 presents the results of the same model depicted in Equation 1 and Table 1, but where the outcome variable is the placebo outcome of the rate of child-focused malpractice claims. The results show no significant effect of the ARRA grant in decreasing malpractice claims against professionals. This begins to help rule out that one form of confounding–a general drop in legal claims during the Recession years coinciding with the ARRA stimulus funds–biases the results from the previous section.

4. Does use of these tools create a "leaky pipeline" of district resource allocations?

The previous section used qualitative and quantitative evidence to show that parents use tools associated with the tag of disability to shape district resource allocations. The present section investigates: do these tools contribute to a "leaky pipeline" in school resource allocations, where resources intended for students without a tag of disability leak out to students with that tag?

[15] Rather than giving a continuous value for age, NPDB classifies age in buckets and the final bucket is those aged 10-19.

5. Methods

5.1. Data sources

5.1.1. Dependent variable

The data for district-level counts of disputes comes from California's Office of Administrative Hearings (OAH)'s Special Education Division. California was chosen for two reasons. First, the state experienced a reform to school financing that involved a sharp administrative threshold that allows us to disentangle the effect of district resources from the effect of district demographics. Second, while several states publish all the hearing decisions from due process requests that go to trial, California is one of the few states to publish counts of all *requests* for hearings by district, which overcomes selection issues in which requests go to trial versus end in settlements.

The analysis draws upon a count of all due process hearing requests by district, which were extracted using macros from Attachment B of the Special Education Quarterly Reports.[16] These counts are the primary dependent variable.

5.1.2. Predictor variables

The main predictor variable–whether a district was eligible for a extra boost in state resources, and how far a district was from the eligibility threshold–is described below when I outline my analytic strategy. Other district-level predictor variables, used for controls and sensitivity checks, are outlined in Appendix Table A4.

5.2. Analytic strategy

To isolate the effect of a resource boost intended for students *with tags other than* special education, I take advantage of a reform of the financing system in California that introduced an administrative threshold for allocating additional resources to districts. In particular, I use a sharp regression discontinuity design (RDD) where the running variable is a district's *unduplicated pupil percentage* (UPP). The UPP is composed of the percentage of a district's students falling into at least one of the following categories of need, categories that *do not* include special education students: 1) English Language Learner (ELL); 2) *eligible* for Free

[16] http://www.dgs.ca.gov/oah/SpecialEducation/Resources/SEReportArchive.aspx. California's hearing system is constructed so that when parents file a request for a due process hearing, the parents also must agree to mediation before a scheduled hearing; if a settlement is not reached at the mediation, the case proceeds directly to the hearing stage.

or Reduced Price Meals (FRPM) based on family income; or 3) in the foster care system (confirmed via administrative database).[17]

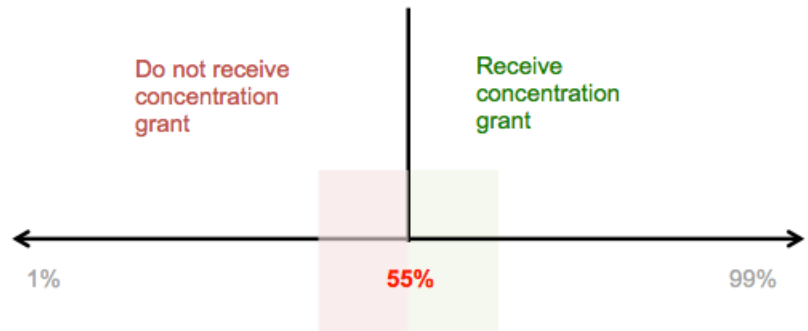
The UPP is used by the state of California to determine whether a district receives or does not receive an extra boost of state aid called a *concentration grant.* This extra boost of state aid was delivered in the context of a broader overhaul of the way that California distributes money to school districts, the Local Control Funding Formula (LCFF), which went into effect beginning in the 2013-2014 school year. LCFF created three grants for the distribution of state aid to districts, and the last grant involves a discontinuity in the UPP (of Education):

1. Base grant: all districts receive based on Average Daily Attendance (ADA), with some adjustment based on grades served by district
2. Supplemental grant: all districts with non-zero UPP receive base grant x 0.20 x ADA x district's UPP
3. **Concentration grant**: at or above a threshold of 55% UPP, district receives grant equal to base grant x 0.50 x ADA x district's UPP

The last category of grants, *concentration grants*, are disbursed according to a sharp threshold at 55% UPP for which districts receive versus do not receive this grant. For instance, a district with a UPP of 58% receives a concentration grant where a district with a UPP of 52% does not. Around this threshold is a subset of districts with similar observed characteristics that might affect the likelihood of a case but districts to the right of the threshold receive a grant that explicitly earmarks resources for students other than those in special education, while districts to the left of the threshold do not receive a grant. Below is a stylized diagram of the relationship between district UPP and the concentration grants.

More formally, in the present context, we are interested in the following, where Y is the rate of litigation, X is the unduplicated pupil percentage, i indexes a district, $Y(1)$ indicates the outcome under receipt of the treatment (a concentration grant), while $Y(0)$ indicates the outcome under no receipt (no concentration grant):

[17] The unduplicated refers to the fact that students who qualify under multiple categories–e.g., an English Language Learner who also qualifies for free or reduced price lunch-only counts once towards a district's UPP, such that the UPP is constrained to fall between 0 and 1.



$$Y_i = \begin{cases} Y_i(0) & \text{if } X_i < 55\% \\ Y_i(1) & \text{if } X_i \geq 55\% \end{cases} \tag{2}$$

Since the average treatment effect of the grant for districts around the threshold is identified by examining the difference in enforcement rates for districts in this subset around the threshold, a major decision in the RDD analysis is how large that subset of districts should be (the bandwidth around the threshold). Rather than choosing a bandwidth *a priori*, I use the optimal bandwidth selector proposed by (4) (CCT for short), implemented in R using *rdrobust*. More specifically, the bandwidth selector fits separate local linear regressions on either side of the threshold ($<$ 55% UPP for no grant; $\geq$ 55% UPP for grant) that assign smaller weights to districts further from the threshold but still within the bandwidth. CCT use a triangular kernel estimator for this local linear regression, that takes the following form, where $|u| \leq 1$ indicates that a district falls within the bandwidth around the threshold:

$$K(u) = \begin{cases} 1 - |u| & \text{if } |u| \leq 1 \\ 0 & \text{if } |u| > 1 \end{cases} \tag{3}$$

The bandwidth is then selected via iterating through different bandwidth choices and then selecting a bandwidth that minimizes the approximated mean squared error around the threshold. The standard error adjusts for bias created by this smoothing (CCT's robust bias-corrected confidence intervals).

Important for fitting the sharp RDD in this case is knowing two features of a district: its unduplicated pupil percentage, which influences whether a district is above versus below the concentration grant threshold, as well as its distance from the threshold, and knowing whether the district *did indeed* receive a concentration grant, which affects whether we can analyze receipt of the concentration grant as a deterministic function of a district's UPP or whether the UPP only increases the probability that a district will receive the concentration grant treatment. The data source used for this analysis–California's LCFF funding snapshot data for fiscal year 2013-2014–reports the precise unduplicated pupil percentage and concentration grant status/amount for all districts. All analyses focus on the 2013-2014 school year, the first year of the concentration grant's implementation, using data on the same districts from the 2012-2013 to control for pre concentration grant trends.

Finally, concentration grants are important because state aid is an important source for per-pupil expenditures in California districts. Though data on the percentage of per-pupil expenditures financed by state revenue sources *during* the concentration grant year are not available, data from the previous school year show that the mean state contribution to per-pupil expenditures is 48.4% of expenditures, while the local contribution averaged 42.4% and the federal contribution averaged 9.2%. This highlights that state aid, and by extension concentration grants, are an important component of how districts allocate resources between students.

5.3. Results

5.3.1. Sample used for regression discontinuity

For the analytic sample, we first exclude 81 districts with zero IEPs in the 2013-2014 school year (all of which had zero cases)–the RDD makes two further restrictions on the sample of districts used for the analysis. First, California has a non-negligible number of administrative units classified as school districts that are only composed of charter schools. These charter districts are present in the case data, but there is not systematic data available on those districts' count of students with an IEP. Since this variable is important for ruling out the idea that a drop in cases after concentration grant receipt stems from a drop in IEPs that might generate a case rather than a drop in cases, these charter districts were excluded, as were any districts that were missing a count of IEPs for the 2012-2013 school year (the year before the concentration grant) or the 2013-2014 school year. Second, the RDD design does not generally use covariates, and a district's likelihood of having any case and count of cases is heavily influenced by the count of IEPs (a count controlled for in the random effects model). In addition, even after excluding districts with zero IEPs, there is significant heterogeneity in what counts as a school district within California, its size, and the resulting count of IEPs. Appendix Figure A.5 highlights the count of districts associated with each IEP. The range once districts with zero IEP's are excluded are nine districts with one student with an IEP (e.g., Coffee Creek Elementary District, a district with average daily attendance of 58 students located in a rural northern county close to the Oregon Border) to Los Angeles Unified School District, which had over 81,000 students with an IEP in the 2013-2014 school year. Third, there are many districts with zero cases, which creates difficulty in creating this rate since it means something different for a district with 400 students with an IEP to have zero cases than for a district with 4 students with an IEP to have zero cases.

The significant heterogeneity between districts in size and IEP counts generates a relationship where above a certain count of IEPs, a district always has at least one case. More formally, if we fit the following logistic regression:

$$log(\frac{P(case)}{1-P(case)}) = \alpha + \beta * IEP\ count_i \tag{4}$$

Appendix Figure A.6 shows that districts above a certain IEP count always have a case, while if we restrict districts to those below a certain IEP count, the curve better resembles a logistic distribution. This would not pose problems for the RDD if the count of IEPs was equivalent for districts above versus below the threshold within a range, but the fact that the unduplicated pupil percentage is based on categories like ELL or being in foster care that are rarer among students means that larger districts are at higher risk of being above the threshold and of also having a higher count of IEP's that generate the cases. Therefore, the second restriction limits the analytic sample to districts whose IEPs are in the range of a medium to high probability of generating any case ($0.4 \leq Prob < 0.9999$), which results in the exclusion of 403 districts. In sensitivity checks, we iterate through different sample restrictions to highlight the range of windows for which the results are valid, and these iterations will include some of the 403 districts excluded based on the IEP threshold.

In sum, these exclusions result in an analytic sample of 221 districts in this middle range of predicted case probabilities based on size that are listed in Appendix Table A.7. These include a mix of elementary school districts and unified school districts.

5.3.2. Sharp or fuzzy RD?

The following table shows the percentage of districts receiving a concentration grant in the full sample of districts with a non-zero IEP count in 2012-2013 and 2013-2014 school year and the average distance from the threshold, showing a similar distance on either side:

	No concentration grant	Concentration grant
Unduplicated pupil percentage $\geq$ 55%	0%	100%
Distance from threshold	-0.22	0.23

The table highlights that we can treat a district's unduplicated pupil percentage as fully determining whether or not the district receives a concentration grant (sharp RDD).

5.3.3. RDD results

Unobserved characteristics of districts–for instance, the density of legal advocacy organizations in a local area–may be an important factor behind cases. This suggests that in addition to a standard RDD that looks at an outcome in a given year, we can improve the RDD by exploiting the fact that we not only observe enforcement activity in districts in the school year *of* the concentration grant, but also enforcement activity in districts in the school year prior to the grant. If unobserved characteristics of a district remain relatively constant across these school years, we can focus on *within-district* estimates that examine how enforcement activity in a district changes *relative to itself* in the year prior to the grant. In particular, when examining the binary outcome of whether a district had a case, there are four combinations, with the ones of interest highlighted in red:

		2012-2013 school year (before grant)	
		No case	Yes case
2013-2014 school year (after grant)	No case	Case neither year	No case before grant but case after
	Yes case	Case before grant but no case after	Case both years

More concretely, the RDD examines four outcomes outlined in Table 2. The *leaky pipeline* hypothesis predicts that the resources from the concentration grant, instead of solely reaching their intended recipients, may "leak out" to reach students with the tag of special education. If this diversion occurs, then the extra resources may *decrease* litigation aimed at securing resources for special education students. Table 2 shows predictions under the *leaky pipeline* hypothesis. The table shows that two of the three *within-district* measures are statistically significant at the $p < 0.05$ level. Districts that *did not* have a case in the year prior to a grant are *less* likely to newly have a case if they receive a concentration grant and districts that had at least one case in the year prior to the grant having *fewer* cases the year the districts receivied the grant. The signs on the other two coefficients are also consistent with the leaky pipeline hypothesis.

Importantly, the results also rule out the idea that this lower likelihood of having a case and lower count of cases in the concentration grant districts is due to a lower amount of students placed in special education: there is no difference between districts receiving a grant and those not receiving a grant in whether there's a drop in IEP's. Important to remember is that the RDD estimates are valid for districts around the concentration grant threshold, and thus should not be generalized to districts with low unduplicated pupil percentages (the wealthiest school districts) or high unduplicated pupil percentages (the poorest school districts).

Table 2: RDD results

Within-district measures	Prediction	RD estimate	Std. error	P-value	Bandwidth
No case year before grant; case after grant (1 = yes; 0 = no)	-	-0.71	0.35	p = 0.04*	0.09
Case year before grant; no case after grant (1 = yes; 0 = no)	+	0.076	0.06	p = 0.21	0.18
For districts with > 0 cases in prev. year, drop in cases (1 = yes; 0 = no)	+	0.47	0.23	p = 0.04*	0.12
Between-district measure					
Any case (1 = yes; 0 = no)	-	-0.13	0.11	p = 0.25	0.13
Check for alternate mechanisms					
Lower % of IEPs in grant year than previous year (1 = yes; 0 = no)	No effect	0.17	0.31	p = 0.56	0.17

5.4. Robustness checks

To investigate the robustness of the present results, we perform the following robustness checks.

5.4.1. Sorting around threshold

The main identifying assumption behind RDD is that units cannot manipulate their position around the threshold–in this case, districts near the cutoff of 55% UPP for the concentration grant cannot manipulate their student characteristics in ways that help the district cross the 55% threshold. We examine this assumption in three ways.

First is via a visual inspection of the distribution of unduplicated pupil percentages in the analytic sample of 221 districts. Figure A.8 highlights the distribution of percentages across districts, and also focuses on the distribution around the threshold. While there are peaks at 56 and 57%, there are also peaks at 46 and 50-51%; the visual inspection does not provide clear evidence of sorting. Second, we can perform a statistical test that checks for discontinuities in the density of observations around the threshold. In particular, we use a sorting test developed by (18). The test is useful for the case where the manipulation around the threshold is *monotonic*–that is, districts either all want to have UPP's above the threshold or districts all want UPP's below the threshold, rather than some districts wanting to be above and other districts wanting to be below the threshold. Since it is most plausible in this case that districts want to monotonically manipulate their percentages to be *above* the concentration grant threshold, the failure to reject the null of no discontinuity is useful in this case. The test fits a local polynomial on either side of the threshold, and then tests the null hypothesis that the limit of the probability density function is the same as you approach the threshold from either side ($lim_{x^+} f(x) = lim_{x^-} f(x)$), where x is the running variable (in this case, the unduplicated pupil percentage) versus the alternative hypothesis that densities are not equal ($lim_{x^+} f(x) \neq lim_{x^-} f(x)$). The McCrary test examines the logged difference between this right and left limits to construct the test statistic ($\hat{\theta} = ln(\hat{f}^+) - ln(\hat{f}^-)$). As a result, failing to reject the null illustrates continuity in the unduplicated pupil percentage; rejecting the null highlights a discontinuity. With a 10% bandwidth, the test yields: $Z = 0.496, p = 0.62$, a failure to reject the null that lends more evidence to the absence of a discontinuity in the threshold in a district's unduplicated pupil percentage, lending greater weight to the idea that districts did not successfully manipulate their percentages to qualify for a grant.

Third, we can exploit the fact that we observe two "ingredients" for the UPP– a district's percentage of students eligible for free or reduced price lunch and a district's percentage of Limited English Proficiency (LEP) students–in two years: the year *directly prior* to the concentration grant receipt and the year *of the* concentration grant receipt, allowing us to investigate whether districts that received a grant seemed to experience greater increases in these categories of need than districts that did not receive a grant.[18] Figure 6 shows the distribution of changes in two characteristics used to determine whether a district is eligible for a grant– FRPL and ELL–between districts that did versus did not receive a concentration grant and that are within the 10% bandwith. A Kolmogorov-Smirnov (K-S) test then tests the null hypothesis that the concentration grant spread of changes and the no concentration grant spread of changes was drawn from the same distribution. In particular, if districts manipulated these percentages to be *over* the threshold, we might expect that for the concentration grant districts, the distribution of change in limited english profi-

[18]English Language Learner (ELL) and Limited English Proficiency (LEP) are used interchangeably; here, I refer to these students as LEP because that is the designation provided by the Common Core Data

ciency (LEP) and free or reduced price lunch students is shifted rightwards compared to the districts that just missed qualifying for a grant. Figure 6 shows the density of changes in the concentration grant districts versus no grant districts near the threshold. For each, the K-S test supports the null of no difference in the distributions, but the change in students classified as having limited english proficiency is closer to failing to reject the null ($p = 0.12$) than the change in FRPL percentage ($p = 0.51$).

5.4.2. Applicability of treatment to districts with different risk pools

Earlier, we discussed how the analytic sample is restricted to districts with roughly similar probabilities of generating a case based on the mechanical relationship between a higher count of IEPs and a higher probability of generating a case. To test the robustness of the results to the lower threshold used for the IEP count, we iterate from a lower IEP threshold of 10 IEPs to a lower threshold of 1000 IEPs in increments of five to show the range of sizes for which the treatment estimate is most valid for the two significant outcomes: the positive coefficient on the same district having fewer cases in the year of the grant than the year before the grant and the negative coefficient on a district without a case in the year before the grant getting a new case in the year of the concentration grant. Figure 7 shows the range of IEP lower thresholds for which the results are most robust. The coefficients are all in the same direction regardless of the threshold–that is, they show that districts that receive a concentration grant have a lower likelihood of a new case and a higher likelihood of fewer cases–but the figures also show that each treatment effect is strongest for districts with at least 200 IEPs (which corresponds to districts with at least 1650 total students in the district).

5.4.3. Placebo test of RDD

Finally, another way to examine the robustness of the results is to: 1) randomly draw placebo thresholds from a uniform distribution of possible thresholds;[19] 2) re-estimate the regression discontinuity, 3) examine the distribution of treatment effects identified in the discontinuity, which should peak at zero given that there was no actual discontinuity in state grants at those placebo thresholds. Again focusing on the two significant results–whether there is a new case in districts that did not have a case in the previous year and whether there is a drop in cases–Figure 8 shows the results. The figures confirm that the treatment

[19] In this case, potential thresholds are from 0 to 1, but the test was conducted between 0.1 and 0.9 to have sufficient data points to estimate the RDD

effect peaks at zero for these randomly drawn thresholds, lending weight to the fact that the threshold used to grant or withhold districts extra resources for students *not in* special education has a valid effect on litigation against those districts.

Figure 6: Distribution of changes in limited english proficiency and FRPL percentages: 10% bandwidth, districts with and without concentration grant

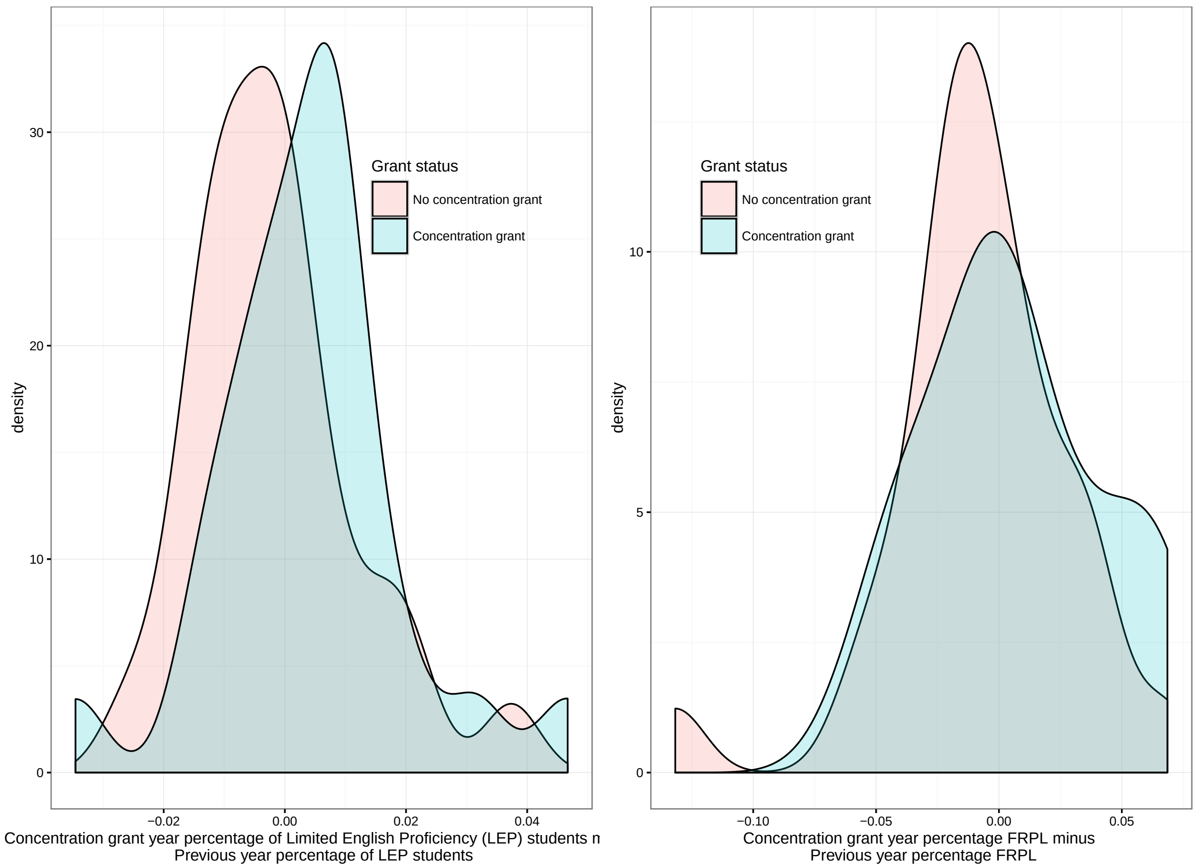


Figure 7: Treatment effects across range of lower thresholds for count of IEPs. *Red dashed line* indicates the treatment results presented

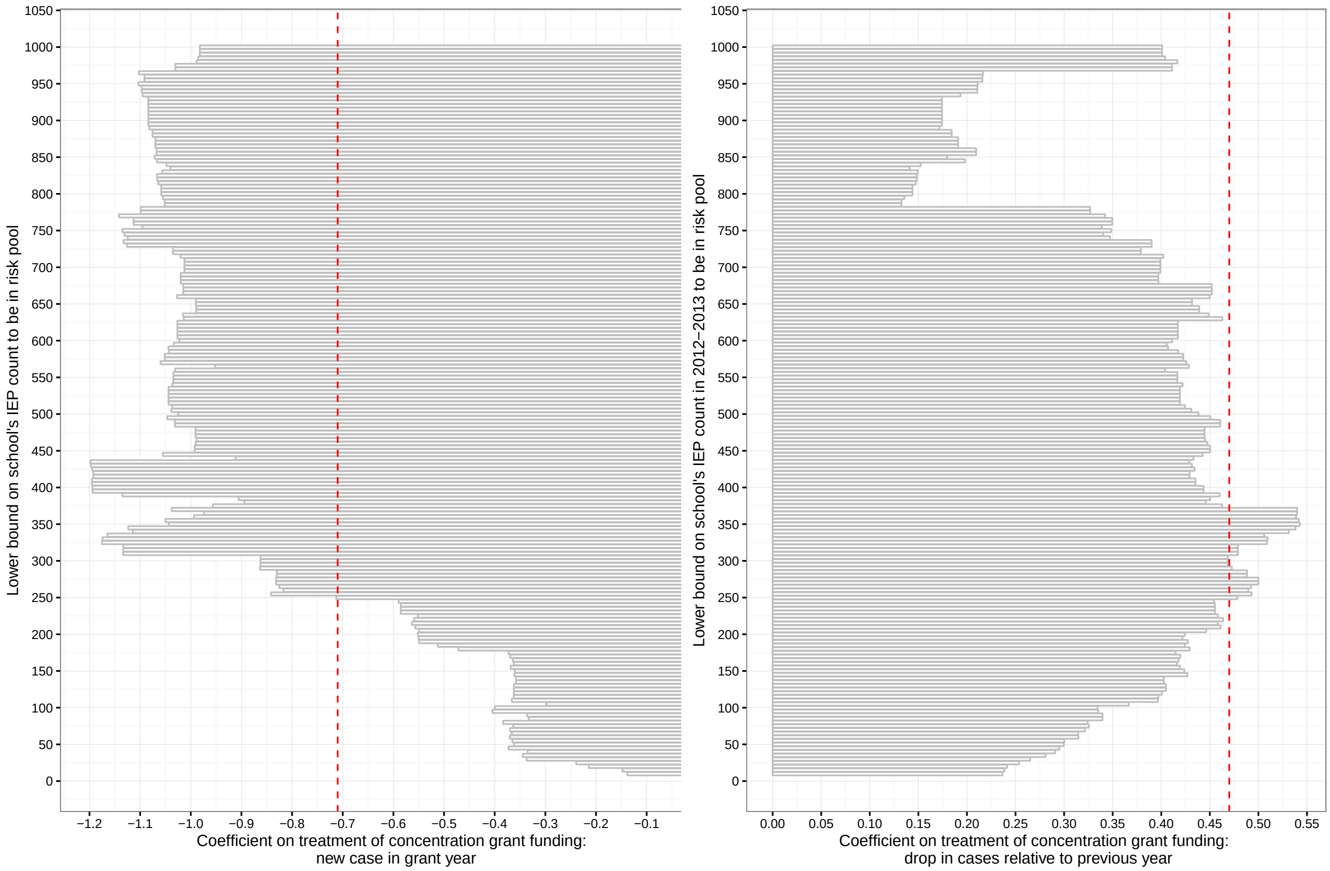
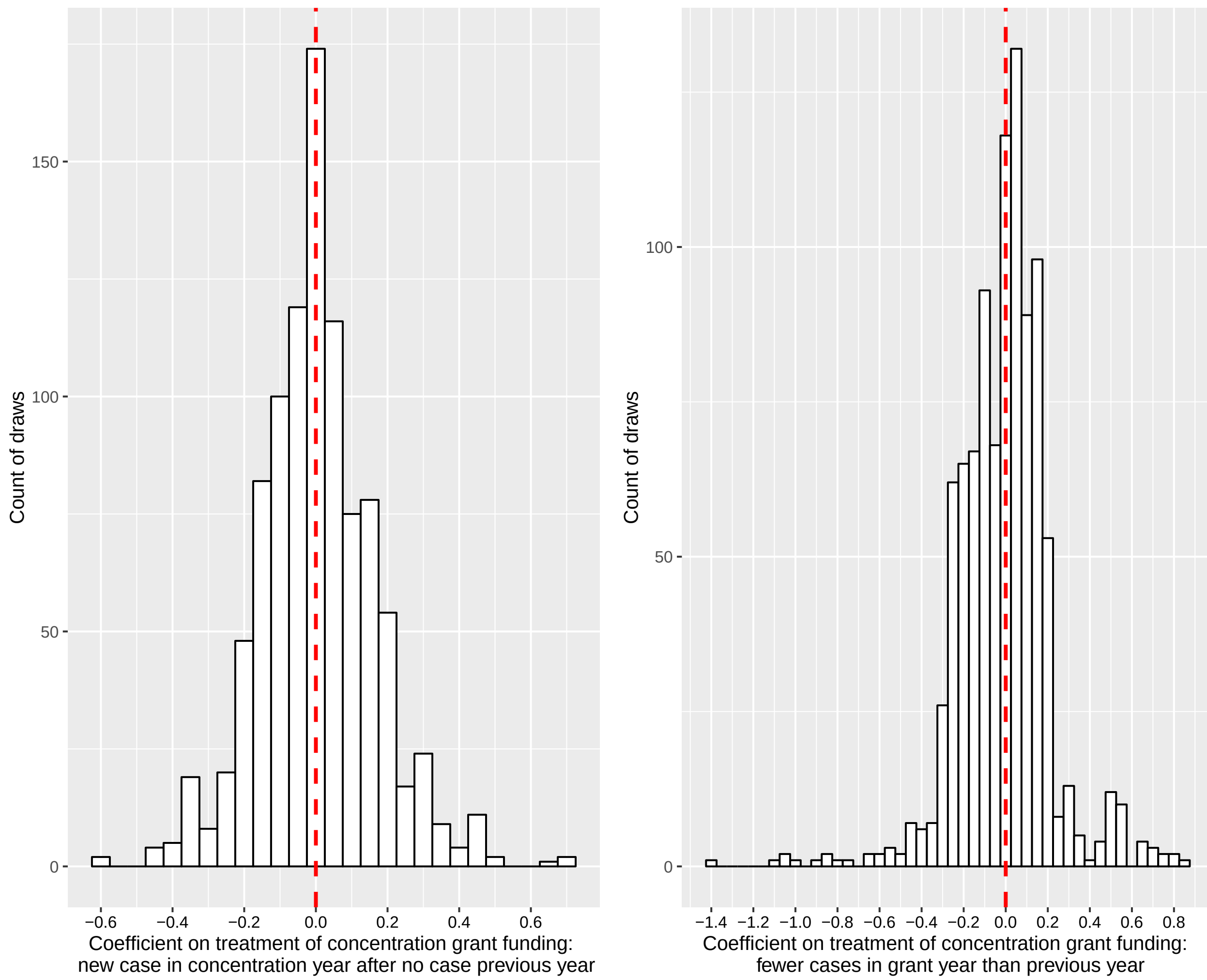


Figure 8: Placebo draws of thresholds: distribution of coefficients (red dashed line shows that it should peak at zero)



6. Discussion

The present paper began with a general framework for how tags structure social policy allocations: tags first filter a pool of potential recipients into tag-holders. Then, tag-holders have both a right to access resources and various tools they can use to shape these allocations.

The paper then highlighted two unique features of how tags function in school districts. First is a multi-level funding structure: state governments use tags to adjust allocations *to* school districts but then it is unclear how school districts distribute these state budget allocations between students with different tags and non-tagged students. Second is that depending on whether a student has a tag and which tag he or she has, parents have different formal tools to shape a district's allocation of resources.

These two features may contribute to a “leaky pipeline” where resources intended for students with one set of tags “leak out” to students with a different tag. One force behind this leaky pipeline are legal tools that give parents of students with one tag–disability status–a contract they can use to shape resource allocations by protesting a district's denial of resources. These strong tools contrast with weaker and less frequently-utilized tools given to parents of students with other tags. Section Three exploits an exogenous shock to resources given to students with the disability tag caused by ARRA to show that parents use these strong tools to try to shape resource allocations. Section Four then uses an RDD to show that a policy change that should have *no effect* on complaints over special education resources leads to decreases in complaints that provides evidence of a leaky pipeline. This leaky pipeline highlights the way that laws that attempt to prioritize certain individuals for resource allocations can increase inequality if accompanied by different legal tools to shape allocations.

7. Appendix

Table A1: Data sources for analysis of ARRA's effect on parent complaints about disability services

Dependent variables: all by state/year from 2004/2005 to 2011/2012 school year

Real outcome	
Disputes per 10,000 special education students	Numerator: IDEA Part B (3-21) Section 618 dispute counts Denominator: CCD with, IDEA Part B Section 618 counts used for missing CCD values
Robustness check outcome	
Disputes per 10,000 students	Numerator: same as above Denominator: CCD
Placebo outcome	
Child-focused malpractice claims per 10,000 children	Numerator: NPDB public-use data file count of claims in 4 age categories: fetus, < 1 year, Age 1-9, Age 10-19. States were coded using recommended logic of first using the practitioner's work state, and then, if missing using the practitioner's home state

Independent variables: all by state/year from 2004/2005 to 2011/2012 school year[20]

ARRA financing years (2008/2009 and 2009/2010)	Legislation
Financing formula	Parrish et al. (2003) and Ahearn (2010) 50-state surveys of financing formula (conducted by partnership between Department of Ed. Office of Special Education Programs (OSEP) and National Association of State Directors of Special Education (NASDSE)
Controls	
% children living in poverty	Census; ACS
Annual unemployment rate	BLS
% educ. revenue from federal sources	CCD Public Education Financial Survey
State expense-revenue ratio	Pew state fiscal health database, derived from state Comprehensive Annual Financial Reports

[20] Data for non-education variables collected yearly rather than by school year was attributed to fall of that school year- so for instance, 2005/2006 school years were matched with data from 2005 year

Figure A.1: Correlation between outcome and predictors: state-level ARRA analysis

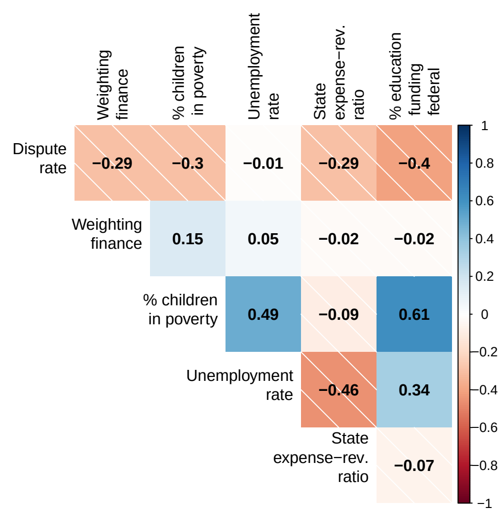


Figure A.2: Trends in disputes in individual states (states ordered alphabetically)

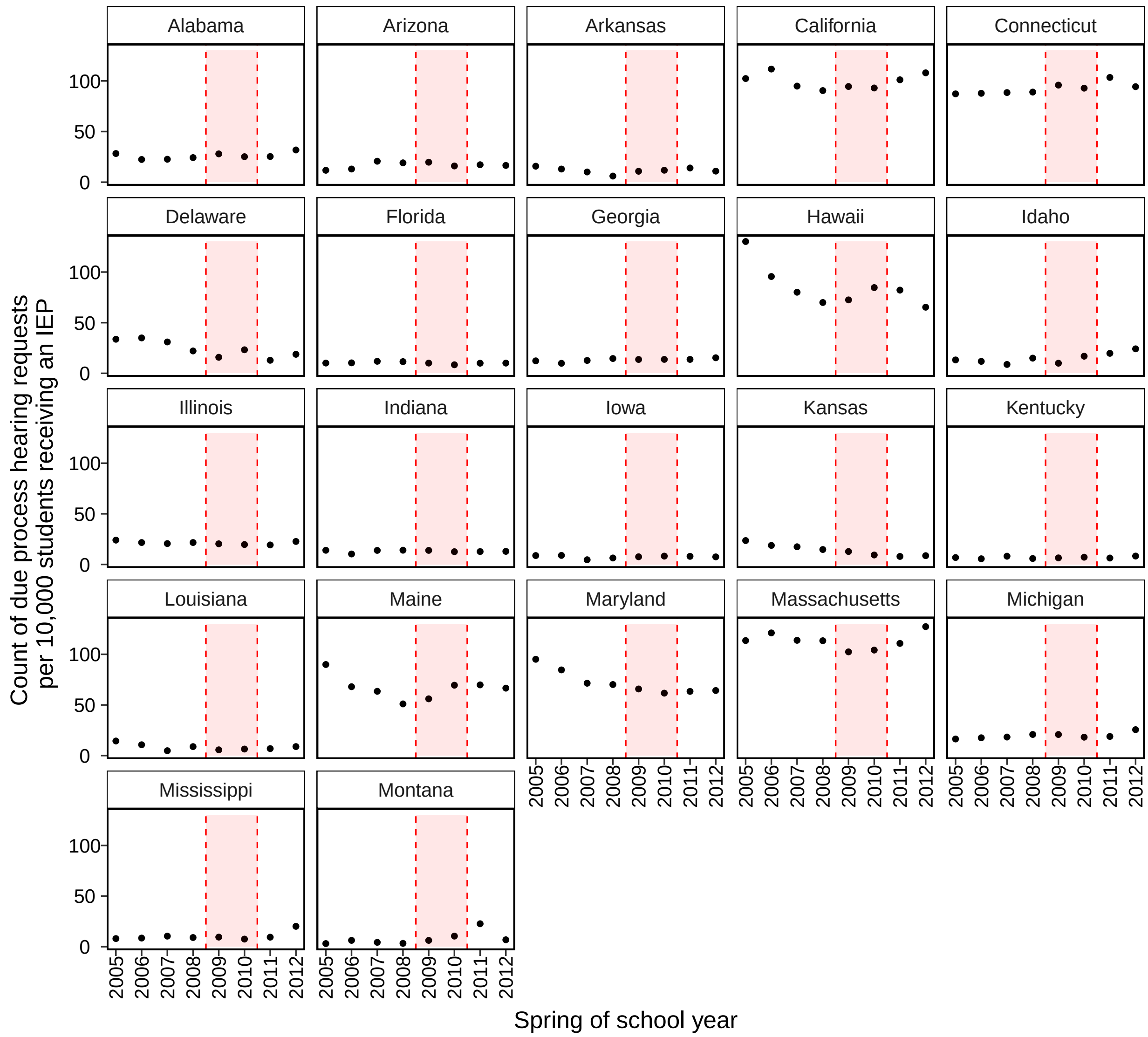


Figure A.3: Trends in disputes in individual states (states ordered alphabetically)

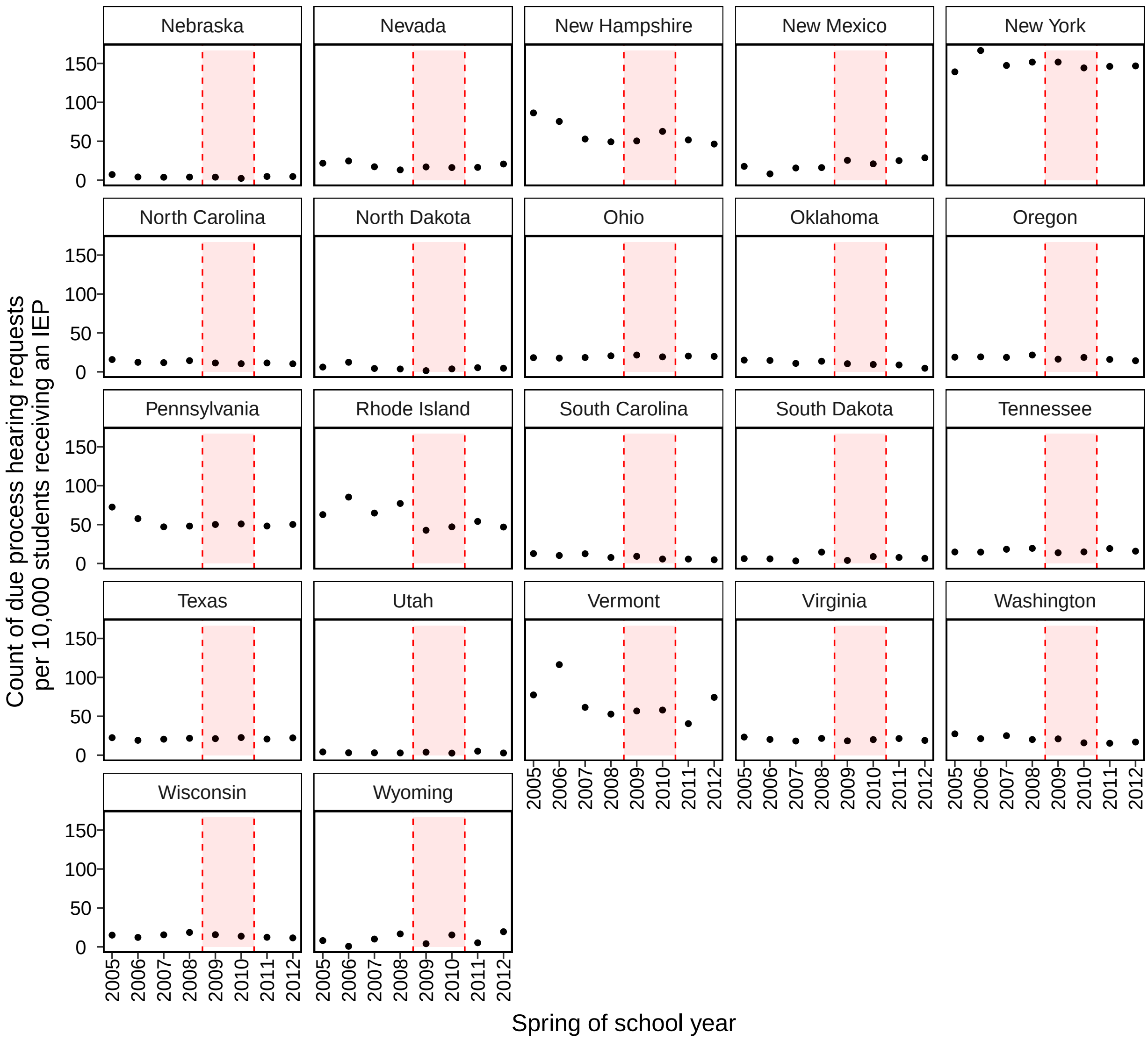


Figure A.4: Placebo analysis of child-focused malpractice claims: trends across states and years

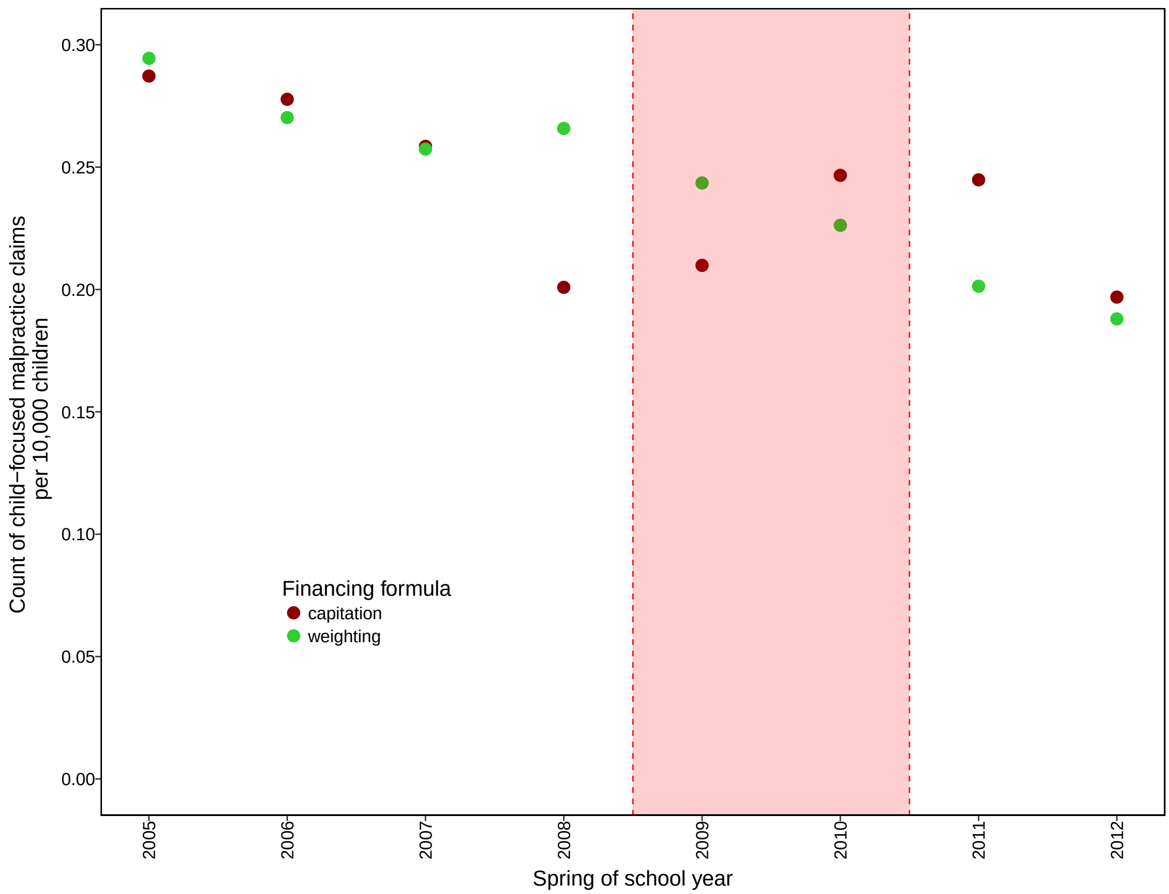


Table A2: Robustness check: effect of ARRA grant on alternate definition of dispute rate ($\frac{\text{count disputes}}{\text{all students}}$ rather than $\frac{\text{count disputes}}{\text{IEP students}}$)

	β (SE) p	
ARRA grant year (1 = yes)	-0.538 (0.27) 0.04	-0.424 (0.21) 0.04
Weighting financing (1 = yes)	-0.606 (0.29) 0.04	-0.695 (0.28) 0.01
ARRA grant year (1 = yes) × weighting financing (1 = yes)	0.285 (0.28) 0.31	0.290 (0.25) 0.25
Observations (50 states × 8 years)	400	400
State fixed effects?	Yes	Yes
Clustered SE by state?	Yes	Yes
Other controls?	No	Yes

Table A3: Placebo test: effect of ARRA grant on child-focused malpractice claim rates

	β (SE) p	
ARRA grant year (1 = yes)	-0.011 (0.01) 0.39	-0.012 (0.02) 0.42
Weighting financing (1 = yes)	0.059 (0.03) 0.05	0.042 (0.02) 0.07
ARRA grant year (1 = yes) $\times$ weighting financing (1 = yes)	0 (0.02) 0.99	0 (0.02) 0.99
Observations (50 states $\times$ 8 years)	400	400
State fixed effects?	Yes	Yes
Clustered SE by state?	Yes	Yes
Other controls?	No	Yes

Table A4: Data sources: predictor variables for district-level analysis

Variable	**Data Source**
Main predictor	
Unduplicated pupil % used in funding threshold	California Dept. of Ed. LCFF Source Data
Controls	
Count and % of students with IEP	NCES Common Core Data
Pre-grant per-pupil expenditures	NCES Common Core Data
Student demographics in district (race; FRPL ELL)	NCES Common Core Data

Figure A.5: IEPs versus count of districts

Figure A.6: Count of IEPs versus predicted probability of having a case. **Left**: shows the predicted probability of case across the range of IEP counts in the data– shows that all districts with IEPs $>\approx 5000$ have a case; **Right**: restricts the window for the graph to districts with below a 0.9999 probability of having a case

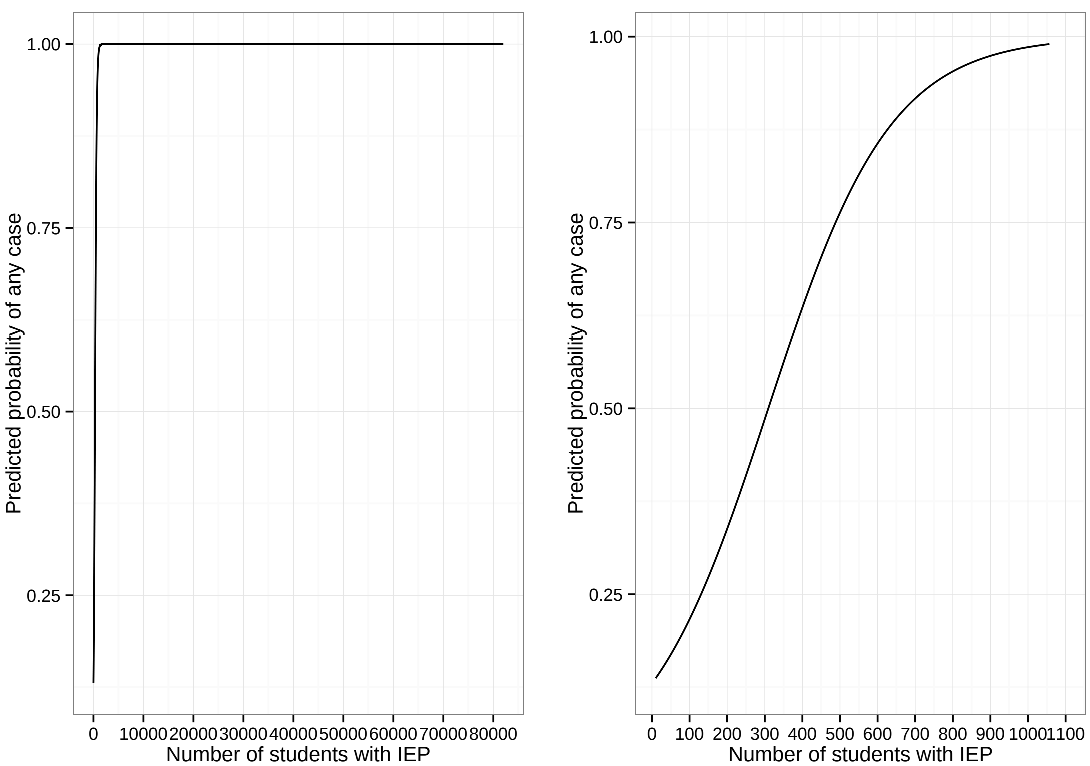


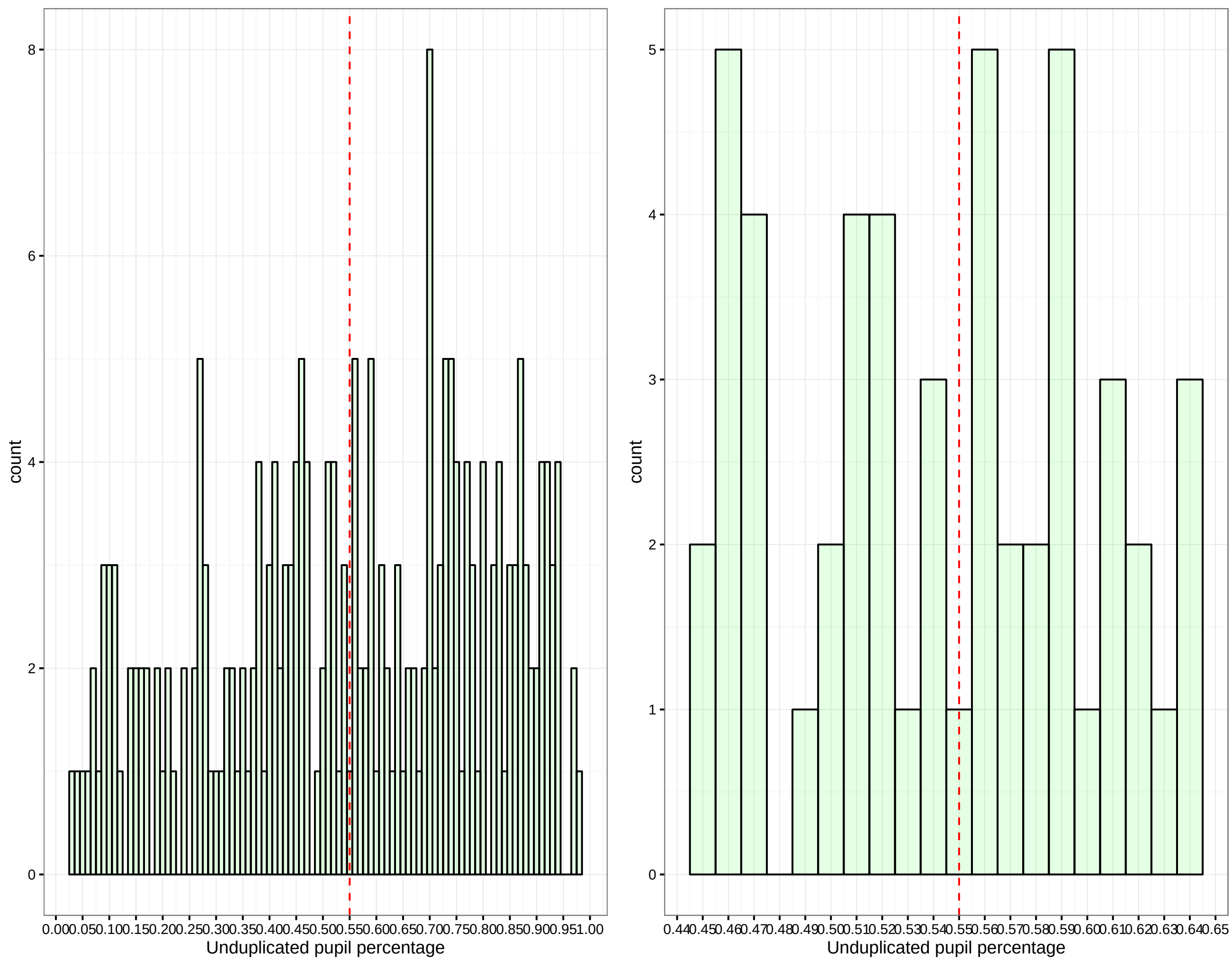
Figure A.7: School districts in analytic sample for RDD

	District
1	Acalanes Union High
2	Adelanto Elementary
3	Alameda Unified
4	Albany City Unified
5	Alhambra Unified
6	Alisal Union
7	Alta Loma Elementary
8	Amador County Unified
9	Apple Valley Unified
10	Arcadia Unified
11	Atwater Elementary
12	Azusa Unified
13	Bellflower Unified
14	Belmont-Redwood Shores Elementary
15	Berkeley Unified
16	Berryessa Union Elementary
17	Beverly Hills Unified
18	Bonita Unified
19	Brawley Elementary
20	Brea-Olinda Unified
21	Buena Park Elementary
22	Burbank Unified
23	Calaveras Unified
24	Campbell Union
25	Campbell Union High
26	Carlsbad Unified
27	Castro Valley Unified
28	Center Joint Unified
29	Centinela Valley Union High
30	Central Elementary
31	Central Unified
32	Central Union High
33	Charter Oak Unified
34	Claremont Unified
35	Coachella Valley Unified
36	Corcoran Joint Unified
37	Coronado Unified
38	Cotati-Rohnert Park Unified
39	Covina-Valley Unified
40	Culver City Unified
41	Cupertino Union Elementary
42	Cypress Elementary
43	Davis Joint Unified
44	Del Mar Union Elementary
45	Delano Joint Union High
46	Delano Union Elementary
47	Delhi Unified
48	Dixon Unified
49	Dry Creek Joint Elementary
50	Duarte Unified
51	Dublin Unified
52	Eastside Union Elementary
53	El Monte City
54	El Rancho Unified
55	El Segundo Unified
56	Empire Union Elementary
57	Encinitas Union Elementary
58	Enterprise Elementary
59	Escondido Union High
60	Etiwanda Elementary
61	Eureka City Unified
62	Fallbrook Union High
63	Fillmore Unified
64	Fountain Valley Elementary
65	Franklin-Mckinley Elementary
66	Fremont Union High
67	Fullerton Elementary
68	Fullerton Joint Union High
69	Garvey Elementary
70	Gilroy Unified
71	Glendora Unified
72	Goleta Union Elementary
73	Hawthorne Elementary
74	Hilmar Unified
75	Hollister
76	Huntington Beach City Elementary
77	Huntington Beach Union High
78	Inglewood Unified
79	Jefferson Elementary (Daly City)
80	Jefferson Elementary (Tracy)

	District
81	Jefferson Union High
82	Julian Union Elementary
83	Keppel Union Elementary
84	King City Union
85	Kings Canyon Joint Unified
86	La Canada Unified
87	La Mesa-Spring Valley
88	Lafayette Elementary
89	Lancaster Elementary
90	Las Virgenes Unified
91	Lawndale Elementary
92	Lemon Grove Elementary
93	Lennox
94	Liberty Union High
95	Lincoln Unified
96	Little Lake City Elementary
97	Los Alamitos Unified
98	Los Altos Elementary
99	Los Banos Unified
100	Lowell Joint Elementary
101	Lucia Mar Unified
102	Lynwood Unified
103	Magnolia Elementary
104	Manhattan Beach Unified
105	Martinez Unified
106	Menifee Union Elementary
107	Mill Valley Elementary
108	Modesto City High
109	Monrovia Unified
110	Monterey Peninsula Unified
111	Moreland
112	Morgan Hill Unified
113	Mountain View Elementary (El Monte)
114	Mountain View Whisman
115	Mountain View-Los Altos Union High
116	Mt. Pleasant Elementary
117	National Elementary
118	Natomas Unified
119	Nevada Joint Union High
120	New Haven Unified
121	New Jerusalem Elementary
122	Newark Unified
123	Newhall
124	North Monterey County Unified
125	Novato Unified
126	Oak Grove Elementary
127	Oak Park Unified
128	Oakley Union Elementary
129	Ojai Unified
130	Orcutt Union Elementary
131	Oroville City Elementary
132	Oroville Union High
133	Oxnard Elementary
134	Oxnard Union High
135	Palo Alto Unified
136	Palos Verdes Peninsula Unified
137	Paramount Unified
138	Paso Robles Joint Unified
139	Patterson Joint Unified
140	Perris Union High
141	Petaluma City Elementary
142	Petaluma Joint Union High
143	Piedmont City Unified
144	Pittsburg Unified
145	Placer Union High
146	Pleasanton Unified
147	Ramona City Unified
148	Ravenswood City Elementary
149	Redondo Beach Unified
150	Redwood City Elementary
151	Rim Of The World Unified
152	Rio Elementary
153	Robla Elementary
154	Rocklin Unified
155	Romoland Elementary
156	Rosedale Union Elementary
157	Roseville City Elementary
158	Roseville Joint Union High
159	Rowland Unified
160	Salinas City Elementary

	District
161	Salinas Union High
162	San Bruno Park Elementary
163	San Carlos Elementary
164	San Dieguito Union High
165	San Gabriel Unified
166	San Jacinto Unified
167	San Leandro Unified
168	San Lorenzo Unified
169	San Luis Coastal Unified
170	San Marino Unified
171	San Mateo Union High
172	San Mateo-Foster City
173	San Rafael City Elementary
174	San Ysidro Elementary
175	Santa Cruz City Elementary
176	Santa Cruz City High
177	Santa Maria-Bonita Elementary
178	Santa Monica-Malibu Unified
179	Santa Rita Union Elementary
180	Santa Rosa Elementary
181	Santa Rosa High
182	Santee
183	Saugus Union
184	Selma Unified
185	Sequoia Union High
186	Silver Valley Unified
187	Snowline Joint Unified
188	Solana Beach Elementary
189	Soledad Unified
190	Sonoma Valley Unified
191	South San Francisco Unified
192	South Whittier Elementary
193	Spencer Valley Elementary
194	Stanislaus Union Elementary
195	Sulphur Springs Union
196	Sunnyvale
197	Sylvan Union Elementary
198	Tahoe-Truckee Unified
199	Tamalpais Union High
200	Temple City Unified
201	Tracy Joint Unified
202	Turlock Unified
203	Ukiah Unified
204	Upland Unified
205	Vacaville Unified
206	Vallejo City Unified
207	Ventura Unified
208	Victor Elementary
209	Victor Valley Union High
210	Walnut Valley Unified
211	Washington Unified (West Sac)
212	Waterford Unified
213	West Covina Unified
214	Western Placer Unified
215	Westminster
216	Westside Union Elementary
217	Whittier City Elementary
218	Whittier Union High
219	Woodland Joint Unified
220	Yuba City Unified
221	Yucaipa-Calimesa Jt. Unified

Figure A.8: Distribution of districts in analytic sample by percentage unduplicated. **Left**: all percentage unduplicated; **Right**: focusing on districts within 10% bandwidth of threshold



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