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Choice, Information, and Constrained Options: School Transfers in a Stratified Educational System

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Abstract

It is well known that family socioeconomic background influences childhood access to opportunities. Educational reforms that introduce new information about school quality may lead to increased inequality if families with more resources are better able to respond. However, these policies can also level the playing field for choice by equalizing disadvantaged families' access to information. This study assesses how a novel accountability system affected family enrollment decisions in the Chicago Public Schools by introducing new test performance information and consequences. We show that a substantial proportion of families responded by transferring out when their child's school was assigned "probation." Poor families transferred children to other schools in the district, but at a lower rate than non-poor families, who were also more likely to leave for another district or enroll in private school. Most striking, we show that despite family response to the probation label, access to higher-performing schools changed very little under the new policy; students who left probation schools were the most likely of all transfer students to enroll in other low-performing schools in the district. Although new information changed families' behavior, it did not address contextual and resource-dependent factors that constrain the educational decisions of poor families.

Keywords

education, social stratification, segregation, urban sociology, public policy, school choice

Racial and economic segregation in U.S. cities has long been acknowledged as a major structural force perpetuating inequality (Massey and Denton 1993; Sharkey and Faber 2014; Wilson 1987). The concentration of disadvantage in urban neighborhoods—shaped by historical forces of racial discrimination, municipal disinvestment, white avoidance, and economic inopportunity—presents a substantial challenge for policymakers attempting to break intergenerational cycles of poverty (Sharkey 2013).

This challenge is particularly salient in the arena of education, where socioeconomic resources strongly determine access to educational opportunities. Households with greater financial, human, and social capital have greater access to higher-performing school districts and school assignment zones, and they have the ability to pay costly private-school tuition (Saporito 2003). Family sorting

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has contributed over time to a system of separate and unequal schools (Logan, Minca, and Adar 2012), reinforced by the dynamic choices of families (Goyette and Lareau 2014; Reardon and Owens 2014).

Various education reforms have attempted to decouple the link between family background and educational opportunity, either by improving low-performing schools or expanding disadvantaged families' access to high-performing alternatives. Over the past 20 years, school accountability policies have emerged in various districts and nationally with passage of the No Child Left Behind Act (NCLB). School accountability policies censure, re-staff, or close low-performing schools, producing pressure for educators to improve their services. In many cases, these reforms also systematize information about school quality, assigning performance labels or A through F letter grades based on student testing levels.

The introduction of school accountability policies allows us to understand more about the dynamics of school enrollment sorting. When first implemented, accountability reforms provide new information about school testing outputs and about which schools may face punitive consequences for underperformance. This information could prompt families to exit schools labeled as low-achieving. Yet access to high-quality schools depends not only on new information, but also on family resources and the quality of viable school alternatives (Lauen 2008). A central question is thus whether and how families will respond to new information about school quality when their enrollment decisions are bounded by financial, social, and geographic constraints (Orfield and Frankenberg 2013). In a contemporary policy climate that often favors choice-based levers to address systemic inequalities, it is critical to understand the impact of new information on the *actual* sorting behavior of families whose school selections are embedded within stratified contexts.

We address this question with an analysis of student enrollment during a novel pre-NCLB accountability policy in the Chicago Public Schools (CPS) that was designed and implemented locally. The CPS "probation" policy publicly identified and censured schools with low levels of reading proficiency on standardized tests. A list of probation schools was reported to local media and updated each summer (Bryk 2003; Luppescu et al. 2011). Because Chicago already had an open-enrollment system in place, the probation policy did not change the rules of school choice, but it did add relevant information and potential consequences for families to consider. Using difference-in-differences event history analysis, we assess whether families exited probation schools in response to the policy when it was first implemented, and also how responses varied by race and socioeconomic status.

Our analysis yields two important findings. First, we find that poor families transferred within the district to other schools, but at a lower rate than non-poor families, who were more likely to transfer schools, leave CPS for another district, or enroll in private school. Second, within-district policy response led to students switching from probation schools to non-probation schools that still performed in the *bottom 50 percent* of all CPS schools. We situate these findings in the context of durable urban inequality in Chicago (Sampson 2012), where neighborhood disadvantage and low-performing schools are geographically clustered (Bryk et al. 2010). Even though the accountability policy provided new information to families, it did not change the fact that access to high-performing schools is constrained by residential location, family resources, and the available supply of nearby school options.

BACKGROUND

School Choice: Pathways, Preferences, and Constraints

In the United States, residential location plays a primary role in the assignment of children to local K to 12 public schools. Geographic boundaries define school district jurisdiction,

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and within most districts, local catchment zones determine which neighborhood elementary, middle, and high school students will attend. Families can influence school enrollment by choosing to live within the district or local school zone that suits their preferences (Goyette and Lareau 2014). Of course, residential choices are resource-dependent, and public school districts in high demand tend to have higher housing costs (Bogart and Cromwell 1997). Families unable or unwilling to pay the cost of entry into such attractive school districts may pursue other pathways—either opting for private schools or seeking more attractive schools located in other neighborhoods within the same district (Saporito 2003).

Several school choice events occur over a child's educational career. The first choice typically begins with initial enrollment into kindergarten, but families must choose again at various grade-structure transition points, such as promotion from 8th grade to high school. Families also make schooling choices when they relocate between regions, cities, or distant neighborhoods; in fact, residential relocation is the most common reason for non-structural school transfers (Rumberger 2003). In many cases, families do not deliberate over which school to attend, so default neighborhood assignment rules make the choice for them (Rhodes and DeLuca 2014).

In this article, we direct attention to a more deliberate school choice event: decisions to voluntarily leave a school. Unlike exiting at structural grade transition points or for circumstantial reasons such as job relocation, voluntary school-exit decisions signal school dissatisfaction relative to alternative options (Hirschman 1970). These intentional exits demonstrate trade-offs in action as families weigh their level of satisfaction with the schools their children currently attend against the promise of something they believe will be better, whether within the same district, in another district, or in the private sector. These decisions are made under family resource constraints and are necessarily relative to the universe of choices a family is able to consider. When deciding whether to stay or go, some families may also weigh additional costs and benefits, such as social disruption and curriculum discontinuity (Hanushek, Kain, and Rivkin 2004).

Districts vary in the flexibility of their school assignment rules. In Chicago, the site of this analysis, an open-enrollment policy has been in place since the 1980s in compliance with a school-district desegregation court order (Lauen 2008). Under the open-enrollment policy, students are assigned to default local neighborhood schools but have the option to transfer to any other school in the district provided there are seats available. Approximately 3 in 10 Chicago elementary students (grades K to 8) opted out of their neighborhood schools in the 1980s and early-1990s, although the practice was slightly more common among high school students (de la Torre and Gwynne 2009).

Chicago's open-enrollment system is one form of school choice policy that weakens the link between residence and school enrollment, at least within district boundaries. Many educational reformers argue for this weakened link as well as a broader array of policies that increase the supply of school options, such as expansion of magnet or charter schools that draw from neighborhoods throughout a district, or use of tax-supported vouchers to subsidize the cost of private-school tuition (Gill et al. 2007). School choice advocates typically also acknowledge the need for wider availability of information about school academic performance so that families can make decisions as informed consumers (Le Grand 2007).

Accountability, Information, and School Quality Standards

School accountability policies attach student performance outcomes to rewards and punishments and are intended to motivate educators to improve the quality of their instruction. In the mid-1990s, school accountability reform emerged in a small number of states and urban school districts; by 2002, the

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federal No Child Left Behind Act (NCLB) required all states to adopt accountability policies. Accountability systems publicly report performance metrics and assign categorical labels to schools: for example, A to F letter grades, high- versus low-performing, and probation. Although primarily used for assigning consequences to schools, categorical labels also simplify for families an otherwise complex comparison across schools while institutionalizing a dimension of quality defined by test performance levels (Favero and Meier 2013; Ladd and Loeb 2013). NCLB even included a school choice provision that sent home letters notifying families if their child's school failed to make Adequate Yearly Progress (AYP), and informing them of their eligibility to transfer to other schools that did make AYP.

When national or district-level accountability reforms sort and label schools, we might expect families will respond. If families are unaware of the learning conditions at their children's school, for instance, the information about test performance relative to other local options could lead to a transfer (Hastings and Weinstein 2008). Accountability labels also communicate which schools face penalties—possible closure, re-staffing, or curricular interventions—should they fail to improve. Some families might transfer to preemptively avoid unwanted turmoil or to avoid possible social stigma associated with enrollment in a negatively labeled school.

Despite the prediction that families will react to accountability labels, few families actually do so by exiting schools. In the first three years of NCLB, less than 1 percent of eligible families in the United States took advantage of the NCLB school choice option (Stulich, Eisner, and McCrary 2007). Choice transfers were likely muted in various districts due to delayed notifications, confusion about the AYP label, or even resistance by district officials (Fusarelli 2007). These patterns do not appear to be limited to NCLB; Henderson (2010) found a similar lack of response under Florida's statewide school A to F grading system.

The remarkably low rate of transfers under NCLB may be a story of supply constraint rather than preference. When NCLB was implemented in Chicago in 2002 to 2003, for instance, 75 percent of schools failed to make AYP. Although a high number of families applied for the NCLB choice program, there were so few alternative options available that most were denied (Lauen 2008). This raises the possibility that the proportion of schools targeted by an accountability system, and their spatial distribution, could interact with family background to affect the likelihood of school exit.

Family Background, Place, and Responses to New Information

Voluntary school-exit decisions are informed not only by family schooling preferences but also by resource constraints and the relative supply of viable alternatives. Thus, if families are affected by a low-quality ranking of the school their children attend, we would expect the likelihood of a responsive school exit and the pathway of that response—private school, district relocation, or within-district transfer—to vary based on family resources and the quality of local options. By considering school choice at the intersection of these individual and contextual factors, our analysis draws together neighborhood, educational, and stratification literatures. We focus on two key points.

First, because families differ in their financial resources, they also vary in whether leaving the school district (whether for private school or for another school district) is truly part of their choice set. These family resources interact with structural features of the local context—the degree of socioeconomic and racial segregation in the housing market, features of the local labor market, and the cost of private schools in the area—that determine the districts and private schools to which students could transfer (Lauen 2007). The degree of social network stratification in the local context may also play a role as parents attempt to process the meaning of the new information and assess their options (Sampson and Sharkey 2008).

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Second, the options for transferring to another public school *within* a district are also shaped by the interaction between family background and structural constraints. Families prefer to attend schools that require limited commuting time, and they choose geographically proximate schools in the context of school choice programs (Hastings and Weinstein 2008; Nathanson, Corcoran, and Baker-Smith 2013). When children are young and cannot travel to school alone, a longer commute time can act as a significant tax on scarce family time and resources, such that a "good school" and a "close school" become synonymous. As a result, families who do try to upgrade may have limited success. New information may lead to the desire to exit one's school, but the spatial distribution of school quality within the district can limit the quality of viable alternatives.

To this latter point, the uneven spatial distribution of school quality in Chicago is of critical importance, and, we believe, is essential to understanding the heterogeneous effects of school choice by family background more generally. In Chicago, decades of stark racial and socioeconomic segregation have led to racially isolated clusters of low-performing schools in neighborhoods with high concentrations of disadvantage (Bryk et al. 2010). In such a spatially stratified context, policies intended to be background-neutral will likely have heterogeneous effects. Given the clustering of disadvantaged schools in Chicago, new information about performance may, for some families, only reveal the dearth of high-quality local alternatives.

For this reason, we believe Chicago provides an important case study for understanding heterogeneity in school choice. In many U.S. cities, educational disadvantage is unevenly concentrated and movement across neighborhoods is difficult (and was intentionally designed to be so) (Massey and Denton 1993). Much of the existing school choice literature fails to recognize that districts do not come to new educational reforms with a clean slate. As Neckerman (2007:vii) notes in her history of school reform in Chicago, "We are trying to do a new thing with an old institution. Urban schools were never designed to produce equality of achievement. . . . When an institution is created with one purpose in mind, it is not easy to redirect it."

To date, no studies have considered the joint impact of family resources and structural constraints on the likelihood of enrollment response to new information about school quality. In part, this is because a low response rate nationally has made it empirically challenging to detect variation among smaller student population subsets. Yet the failure to address variation in response to information about school quality and conditions has led to a social-context-free set of policy predictions about how school choice works in the presence of new information. We address this oversight by turning to an early reform period in Chicago—six years before NCLB—when a school accountability system, designed and enforced locally, was first implemented.

Probation Policy in the Chicago Public Schools

In February 1996, Paul Vallas, the CEO of Chicago Public Schools (CPS), announced his intent to create a new accountability system. The system would be consistent with existing state standards and provide transparent information about school performance to families and educators. After a seven-month period of speculation and internal revision, the new policy was officially implemented on September 30th, 1996, when the district announced that approximately 20 percent of Chicago's public schools (109 of the 541 schools) were placed on "probation" as a result of poor test scores. The list of schools on probation was printed in the two major newspapers in Chicago. On October 1st, 1996, under the title "Here Are the Public Schools Placed on Probation," the *Chicago Tribune* presented an alphabetical list of schools with their street addresses. The *Chicago Sun-Times* included a list under the headline "Troubled Schools," with a subtitle

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stating, "Chicago schools on academic probation." Unlike the *Tribune,* this list did not include addresses, but schools were divided into high school and elementary school categories.

The official policy stated that schools with 85 percent or more of their students falling below the national reading norms (the 50th percentile) on the Iowa Tests of Basic Skills (ITBS) would be placed on probation. To get off the probation list, schools could improve testing performance over consecutive years. Other schools could also be assigned to the list in successive years should their performance decline (Williams 1997). The system is still in effect today, although the probation criteria have been revised.

It is important to consider what kind of information families had about the educational implications of probation, as the desire to avoid future punitive actions could drive school exit as much as the desire to attend a higher-performing school. When first announced in September 1996, the implications of probation assignment were described as a visit from a district assessment team that would be followed by a team of retired principals and teachers working with the school to develop an improvement plan. No deadline for improvement was set, but it was clear that principals and teachers could be removed from their jobs and the Local School Council, the group of parents and residents involved in school decision-making, could be dissolved. Vallas described school closings at this time as "an absolute last resort" and explained, "[w]e will do what's necessary to move schools forward" (Terry 1996).

The accountability plan was a substantial intervention when first implemented, and a source of stigma for struggling schools. A *Chicago Tribune* article, for instance, reported that after the first year of the policy "many students and officials expressed embarrassment—even anger—that their schools were being singled out for the bureaucratic equivalent of a trip to the principal's office" (Poe 1997). One year after implementation, several high schools on probation were re-staffed, sending a message that the CPS Board had the resolve to enforce its accountability policy and that other schools on probation could face similar consequences for failure to improve (Martinez and Poe 1997).

As we will show empirically, a tangible portion of students attending elementary schools on probation at the time of initial implementation transferred to other schools within the district or left CPS altogether. To identify this family-level response to the policy and to further investigate response variation by race and socioeconomic status, we draw on several salient features of accountability implementation in Chicago.[1]

First, Chicago already had an open-enrollment policy in place, which did not change before or after the accountability policy was implemented. The district did not provide transportation, so families without cars needed to rely on Chicago Transit Authority buses and trains or transfer to schools within walking distance. Unlike NCLB, which legislated change in school assignment policies by requiring that districts allow transfers to a school making AYP, the Chicago accountability plan allowed families to continue making school choices just as before, only now with a new system of school performance labels publicly available.

In addition, families knew little about which schools would be assigned probation prior to September 30th, 1996. In the seven months leading up to the release of the probation list, Vallas made multiple announcements that revised whether elementary schools—or only high schools—would be included on the list, how many schools (as few as 12) would face probation, and which standardized test would be used to measure performance—the Illinois Goal Assessment Program (IGAP) or the Iowa Test of Basic Skills (ITBS). Not until September 6th, 1996, did CPS hint at a higher number of schools on probation (80 to 100), still without providing specific school names or clear criteria (Martinez 1996).

Low-performing schools were at greater risk of probation than high-performing schools, but even among low-performing

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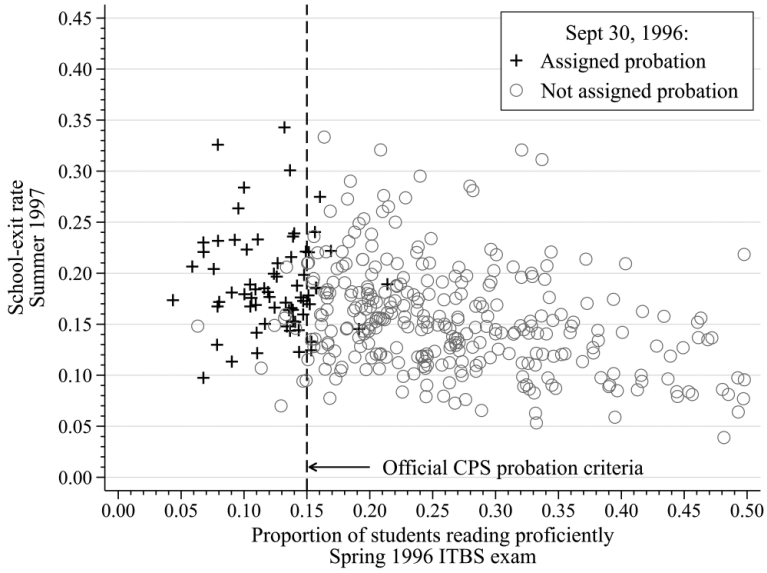

Figure 1. School Reading Performance and School-exit Rate, by Initial Probation Assignment

Source: CPS administrative dataset, aggregated to the school level.

Note: Each open circle or cross represents a single school with at least 100 total students, no fewer than 10 students in grades 1, 2, 4, 5, or 7, and fewer than 35 percent of its student population assigned to special education (as of Fall 1996). The school-exit rate is the proportion of students expected to enroll in the next term who voluntarily transfer schools or leave the district; students required by grade promotion to switch schools are not counted. This scatterplot excludes 54 schools along the right tail with more than half of their students reading at grade-level proficiency (and an average Summer 1997 school-exit rate of .089). The Pearson correlation coefficient between student outflow and student reading proficiency is –.549 ($p < .001$).

schools, there were considerable surprises when the first official list was released (Williams 1996). To illustrate this, Figure 1 presents a scatterplot of Chicago elementary schools, where the *x*-axis measures the proportion of students reading proficiently on the ITBS in each school, and the *y*-axis measures the unadjusted proportion of students voluntarily leaving the school (the school-exit rate) in the summer of 1997. Schools represented with an open circle were not assigned probation; schools represented with a cross were assigned probation.

Figure 1 illustrates the fact that although the CPS Board announced an official criterion for probation assignment (indicated by the dashed vertical line), this definition was by no means precise. Schools on both sides of the performance threshold were assigned probation, and, in several cases, schools performing under the threshold avoided probation in the first year.[2] Figure 1 also reveals between-school performance stratification in Chicago; along the *x*-axis, there is a wide range of academic proficiency levels, with most schools clustering below the 40 percent threshold. Indeed, CPS had a notoriously low rate of academic proficiency, which was a key factor in motivating political support for the probation policy (Bryk et al. 2010). Figure 1 also demonstrates that in the first summer after policy implementation, the rates of school exit were, on average, higher for probation than non-probation schools. Yet, whether this pattern is explained by the probation policy or, rather, by a correlation between lower academic performance and higher student transience, requires a longitudinal approach that can address the influence of individual student characteristics.

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The student population attending Chicago Public Schools is predominantly black and Hispanic, and over 80 percent of students qualify for free or reduced price lunch. CPS is the third largest school district in the United States, serving approximately 400,000 K to 12 students annually. Prior scholarship shows that within-district transfers in Chicago are frequent, and that schools experience a relatively high annual rate of student turnover (Kerbow 1996). Table 1 depicts the characteristics of two cohorts of CPS elementary students, stratified by whether they attended schools that were assigned probation on September 30, 1996. As we will discuss, our analytic sample includes a Fall 1996 cohort that was exposed to active probation policy and a Fall 1994 cohort that serves as a placebo policy counterfactual. Among the 1996 policy-active cohort in Table 1, we see that exposure to school probation was more frequent for poor students qualifying for free or reduced price lunch and for students living with non-family guardians (a proxy measure for foster care). There is also a stark contrast in exposure to school probation by race. Although black students make up 54 percent of CPS enrollment overall, they account for 84 percent of the students attending schools that were assigned probation. By contrast, Hispanic students account for 31.5 percent of CPS enrollment but only 15.4 percent of the students exposed to school probation. Most striking, white, Asian, and Native American students were almost completely absent from probation schools.

As discussed earlier, the broader context of urban inequality in Chicago is a critical feature of how probation policy was implemented, which may have influenced how families responded to new probation assignments. Schools in Chicago are highly segregated by race, and a majority of schools assigned to probation had student bodies that were between 85 and 99 percent black; nearly three in four students exposed to probation were enrolled in these segregated black schools. Probation was also spatially clustered, as shown in Figure 2, a map of elementary school probation assignment in the first year of implementation. The clusters of schools assigned to probation were located in segregated black neighborhoods with high levels of poverty. More advantaged neighborhoods further from the central business district ("the Loop") were generally unexposed to probation assignment.

In summary, the context of racial and economic stratification in Chicago and geographic clustering of disadvantaged and underperforming schools provide important conditions for evaluating how families may have differentially responded to new information about school quality and conditions. When choosing where to enroll in the fall of 1996, families did not know which schools would be assigned probation or the immediate consequences of the policy.

DATA AND METHODS

Data

We analyze school exit with a panel dataset of CPS administrative records. These data include a student record for every fall and spring semester of enrollment between the 1993/1994 and 1998/1999 academic years. By observing students' enrollment over consecutive semesters, we can identify whether students (1) stay at their same school, (2) switch to another CPS school, or (3) leave the district.

Under a restricted-use data agreement, school names were replaced with a randomly generated identifier, so the locations are unknown.[3] However, each student-semester record specifies the student's race, gender, free or reduced price lunch status (FRL), parent/guardian relationship, special education status, bilingual program participation, standardized test scores in math and reading (taken each spring), and school probation status.

Policy Treatment and School Exit

The CPS dataset spans a period before and after introduction of the school probation policy. The timing of policy implementation is critical to our analytic approach. At the start of the 1996/1997 academic year, students

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Table 1. CPS Student Sample, by Cohort and Probation Status

		Fall 1994 Cohort		Fall 1996 Cohort	
	Overall	Non-probation	Probation	Non-probation	Probation
Students (*n*)	325,493	137,298	23,962	140,701	23,532
Free or Reduced Price Lunch (FRL)	.823	.790	.896	.825	.928
Race/Ethnicity					
Asian	.029	.034	.001	.035	.001
Black	.540	.491	.830	.488	.840
Hispanic	.315	.337	.163	.347	.154
Native American	.002	.003	.000	.002	.001
White	.113	.135	.006	.128	.005
Female	.492	.491	.491	.494	.488
Special Education	.091	.088	.082	.095	.091
Bilingual Program (active)	.198	.213	.101	.218	.091
Bilingual Program (former)	.142	.157	.044	.160	.051
Legal Guardian Not a Parent/Relative	.039	.031	.052	.040	.070
Within-District School Transfer in Past Two Semesters	.143	.143	.164	.136	.162
Returning after District Absence	.004	.000	.000	.009	.007
Single-Semester Record Lapse[a]	.006	.005	.008	.006	.006
Repeating Grade	.025	.022	.026	.026	.035
Skipping Grade	.003	.003	.005	.003	.004
Grade Level[b]					
1st grade	.223	.213	.237	.224	.253
2nd grade	.209	.202	.220	.211	.230
4th grade	.197	.198	.211	.193	.201
5th grade	.190	.195	.190	.188	.178
7th grade	.181	.192	.142	.185	.138
School Attended by Student					
Median Math Score[c]	−.069	.008	−.534	.014	−.549
Median Reading Score[c]	−.073	−.001	−.504	.001	−.494

Source: Chicago Public Schools student administrative file.
Note: Probation status for Fall 1994 and Fall 1996 cohorts based on the initial probation announcement made on September 30, 1996. All covariates are measured as binary indicators and captured in the fall semester except where noted. Reported statistics are means unless otherwise noted.
[a]Lapses are potential recording errors when students had no administrative record in the second of three consecutive semesters. Enrollment information is filled in for the missing semester. Findings do not change when lapses are treated as district exits.
[b]Students in 3rd and 6th grade were exposed to a grade-retention policy, so they are excluded from the analytic sample.
[c]School median test scores are based on the prior spring term. Units are standardized within grade-level and year. The analytic sample excludes 3,576 students who attended schools without prior-year elementary testing history.

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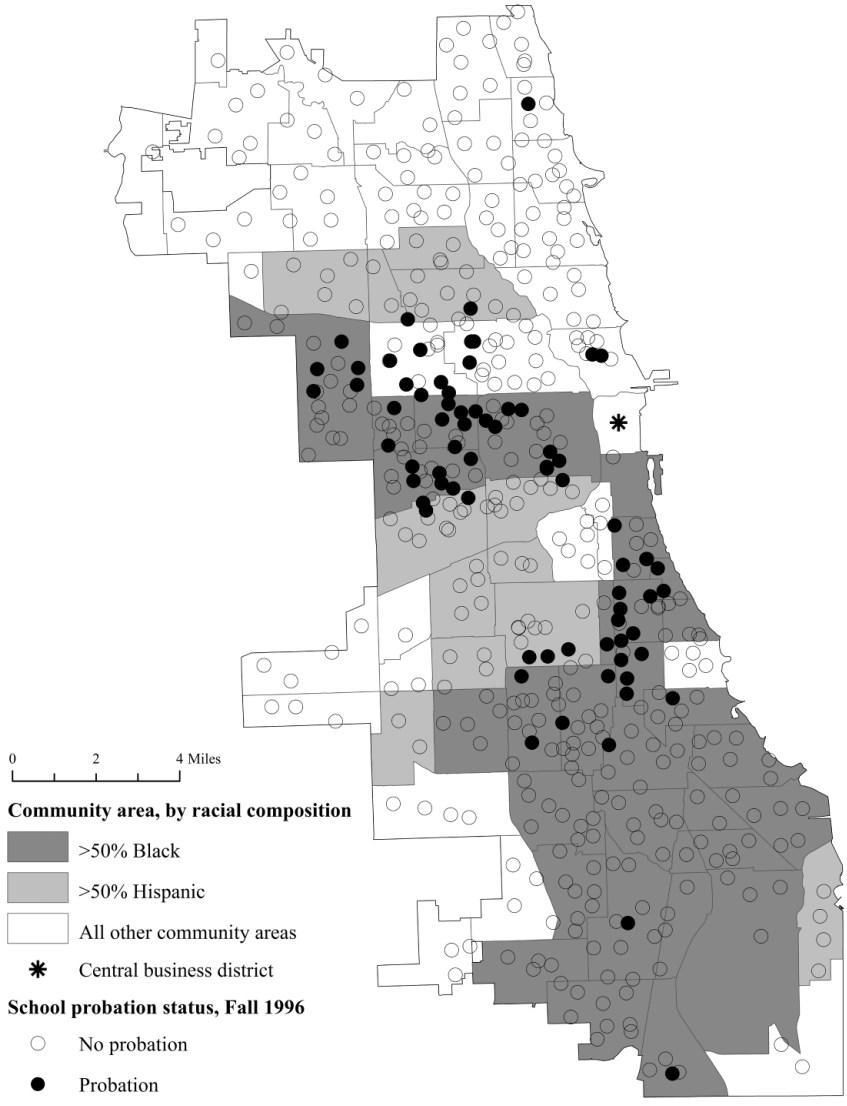

Figure 2. Location of Elementary School Probation Assignment in Chicago, Fall 1996
Source: Author's compilation of publicly available data (see the online supplement [http://asr.sagepub.com/supplemental]).
Note: This spatial sample excludes high schools, special education schools, and schools with fewer than 100 students enrolled in 1996/1997. Elementary schools in Chicago typically offer grades K through 8. A Moran's I statistic of .232 (inverse distance criteria) indicates significant spatial clustering of probation status ($p < .001$).

were enrolled in schools without knowledge of probation status, but on September 30th the status was publicly announced.[4] The primary empirical task of this article is to identify if probation assignment *caused* students to transfer out from these probation schools, and whether this effect varied across different student populations.

Figure 3 shows unadjusted, cross-sectional rates of school exit among elementary school students attending probation and non-probation schools before and during policy

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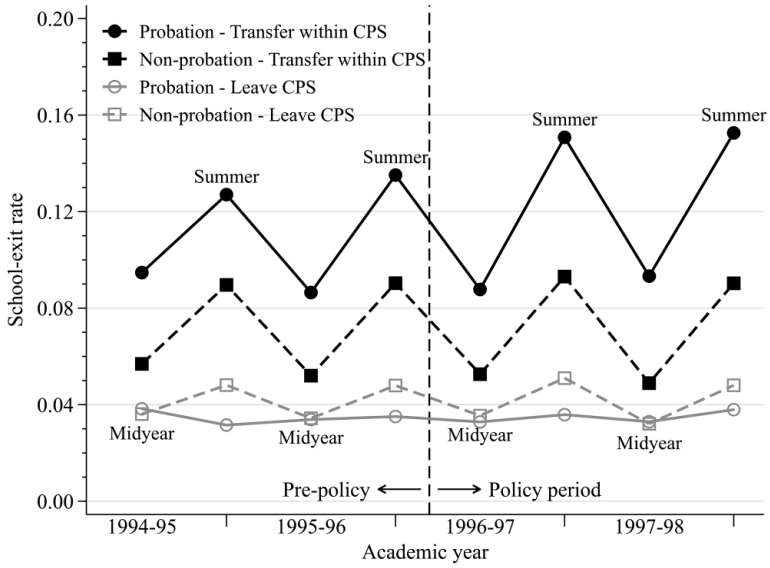

Figure 3. Trends of School Exit among Students Attending Probation versus Non-probation Schools

Source: CPS administrative dataset.

Note: School exit (transfers within CPS and district exits) does not include outflow due to students completing the school's terminal grade. Midyear exits are detected as enrollment changes between fall and spring semesters of the same academic year; summer exits are detected between spring and subsequent fall semesters across academic years. School probation assignment across all time points is based on the initial assignment on September 30, 1996.

implementation. In the pre-policy period, we assign schools to placebo probation statuses based on the future active assignment of those schools on September 30th, 1996. In contrast to Figure 1, which collapsed all forms of exit and reported only summer exits, Figure 3 shows that the social process we model is somewhat more complicated. Families can exit schools in the middle of the school year as well as between school years. Moreover, families can exercise at least two forms of exit: transferring to a different school within the district or leaving the district altogether. A notable limitation of the administrative data is that we have no record once a student leaves the district, so it is not possible to identify whether a district exit led to private school enrollment or residential relocation to another district.

In Figure 3, the saw-tooth trend lines demonstrate that families are substantially less likely to exit during the school year than during the summer. We also see that probation schools experience higher rates of within-district transfers than do non-probation schools. However, the rate of district-exit mobility is higher at non-probation than probation schools in the summer months. The trends appear to change slightly between the pre-policy and policy period, especially in the summer transfer rates of students attending probation schools.

Identification Strategy

The unadjusted comparison of school-exit rates among students attending probation versus non-probation schools cannot reliably measure the effect of the policy because initial school selection—and thus exposure to probation—is not randomly assigned. Unobserved student characteristics, such as family income and homeownership status, influence both the likelihood of attending a low-performing school (at risk of probation) and the likelihood of switching schools (Rumberger 2003).

We use a difference-in-differences (DD) approach to address school selection bias by

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comparing the difference in exit rates between probation and non-probation schools in the pre-policy period with the same difference during the policy period. Assuming that no other changes differentially affected exit rates from probation versus non-probation schools (an assumption of parallel trends), a DD identification strategy measures the unique impact of the policy. To formulate the DD comparison, we define a pre-policy cohort that includes all elementary students enrolling in 1st, 2nd, 4th, 5th, and 7th grades in the fall semester of the 1994/1995 school year, whom we compare to a policy-active cohort that includes a set of 1st, 2nd, 4th, 5th, and 7th graders enrolling in the fall semester of 1996/1997 on the eve of the probation status announcement. All students in the pre-policy cohort are assigned a school probation status based on the eventual September 1996 assignment to which they would have been exposed had the policy been active. Student-semester observations from the pre-policy and policy-active cohorts are pooled together in a single regression model:

$$Y_{ij} = \alpha + \delta P_{ij} + \lambda C_i + \zeta P_{ij} C_i + \varepsilon \qquad (1)$$

Equation 1 indexes student i at school j, where P_{ij} is a dummy variable indicating that the student attends a probation school (treatment), C_i is an indicator for students in the Fall 1996 policy-active cohort (active), and $P_{ij}C_i$ is an interaction term (treatment x active). The δ coefficient captures the difference in outcome Y_{ij} among students at probation versus non-probation schools for the pre-policy period; the λ coefficient captures the difference between pre-policy and policy-active student cohorts attending non-probation schools. Most importantly, the ζ interaction coefficient identifies the unique policy effect as the difference in outcome Y_{ij} between pre-policy and policy-active periods for students attending probation versus non-probation schools.

Table 1 summarizes the pooled analytic sample of 325,493 students. We exclude entering 3rd- and 6th-grade students from the analysis because they were exposed to a grade-retention policy implemented at the same time as the probation policy (Jacob and Lefgren 2004). Students in these benchmark grades could be forced to attend summer school or to repeat a grade for low test-score performance. Their inclusion in the analysis could upwardly bias probation policy effect estimates because low-performing students—who were more highly concentrated in probation schools—may have reacted to the threat of repeating a grade.

Modeling the Effects of School Probation Policy

We estimate the effect of probation on school exit with a discrete-time, competing-risks hazard regression model that includes a modified form of the DD approach defined in Equation 1.[5] The hazard model accounts for two time-related concerns inherent to school exit.

The first concern is possible delayed response to the policy. To adjust for this, we observe student-semester records over four consecutive semesters for the 1994 and 1996 cohorts, spanning a midyear, summer, second midyear, and second summer transition period during which students may have transferred or left the district. Some families may have moved in the summer of 1996, anticipating the impending probation announcement. If so, the Fall 1996 cohort would be slightly selective of families who were not preemptively responsive, which would have a downward bias and make our policy effect estimates conservative.

The second concern is that the likelihood of school exit in one semester may be influenced by prior decisions to stay or leave that have potentially contributed to cumulative inertia or dissatisfaction (Huff and Clark 1978). A hazard regression adjusts for time-dependency by transforming the school-exit outcome of interest into the rate of exit, at each transition period t, only for the population that has not left the school prior to transition t. In other words, the clock starts for each

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student cohort in the initial fall semester (1994 or 1996), and we observe the rate of exit in each subsequent semester only for students who have not already left (i.e., the hazard rate). We censor students if they face structural conditions that compel them to transfer, such as promotion to a grade level not offered at their school, and we track students only up to their first exit event. By transforming school exit into a hazard rate and by conditioning the estimation on a baseline hazard function, this approach adjusts for time-dependent processes that would otherwise confound the estimates (Scott and Kennedy 2005; Singer and Willett 2003).

To address variation in school-exit pathways, we estimate competing-risk hazard rates of transferring within CPS ($k = 1$) or leaving the CPS district (either district relocation or private school enrollment; $k = 2$).[6] The hazard model takes the form of a multinomial logistic regression that jointly estimates the log of the ratio of each school-exit type ($k = 1, 2$) versus staying ($k = 0$). The model is defined in Equation 2, which includes a set of discrete-time indicator variables ($\boldsymbol{A}_{ti}$) for each of four sequential observed semesters:

$$log\left[\frac{h(k)_{tij}}{h(0)_{tij}}\right] = \boldsymbol{\alpha}_k \boldsymbol{A}_{ti} + \boldsymbol{\delta}_k \boldsymbol{P}_{tij} + \lambda_k C_i + \boldsymbol{\zeta}_k \boldsymbol{P}_{tij} C_i + \boldsymbol{\beta}_k \boldsymbol{R}_i + \boldsymbol{\gamma}_k \boldsymbol{S}_{ij} + \varepsilon \quad (2)$$

In Equation 2, a set of DD parameters similar to those in Equation 1 identify the effect of probation policy. However, instead of using a single indicator for probation status, the model has separate indicators for probation in each of the four event periods (indexed by t) to test whether response varied in each semester.[7] Equation 2 also adds a set of student ($\boldsymbol{R}_i$) and school ($\boldsymbol{S}_{ij}$) covariates to control for observed differences between students that could be mutually related to probation exposure and school exit; these covariates (see Table 1) are held constant at baseline to avoid controlling for residual effects of the policy, such as school-level record-keeping changes.

Importantly, probation status was "live" and updated three times in the period of our study: the initial announcement in the fall of 1996, the first summer update in 1997, and a second summer update in 1998. Schools experienced different sequences of on/off probation that may have sent mixed signals about declining or improving quality. We tested five specifications of probation treatment that address this sequence variation, and the results are all remarkably similar (see the online supplement, Table S6 [http://asr.sagepub.com/supplemental]). For purposes of exposition, we only report estimates from one specification that defines policy treatment as the initial 1996 school probation assignment as it persisted over time.[8]

Results from the school-exit analysis are presented in the hazard ratio metric, capturing the hazard rate of each type of school-exit event divided by the hazard rate of staying. Because the primary DD parameters are estimated as interaction terms and presented in hazard ratio units, they convey the multiplicative effect of probation assignment on the baseline hazard ratio of school exit had the policy never been implemented (Buis 2010). Throughout the Results section, we present only policy effect estimates; Table A2 in the Appendix reports a full set of parameter estimates that inform the preferred model.

In addition to evaluating a main effect of the policy on student exits, we measure response variation by race and by free or reduced price lunch status with fully interacted models. We then analyze the likelihood that students transferring within CPS were more likely to trade up to higher-performing schools once probation policy—and the information about school quality it conveyed—became active.

RESULTS

Response to School Probation Assignment

We find that many families responded to probation assignment by transferring schools or by leaving the CPS district altogether. While several factors influence student transfers and district exits, here we focus only on the key

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regression parameters that measure the effect of active probation policy. Table 2 presents selected interaction coefficients in hazard ratio units (i.e., exponentiated logits), which express the multiplicative effect of the active probation policy on the hazard ratio of school exit relative to a baseline counterfactual hazard ratio estimated from the pre-policy period. We display effect estimates across four consecutive periods for two different types of school exit—within-district transfers in the top panel and CPS district exits in the bottom panel.

Model 1 is included in Table 2 as a naïve regression. The coefficients describe the ratio of exit hazard rates between students who attended probation versus non-probation schools. Across all transition periods (1 through 4), the odds of making within-district transfers were significantly higher for students attending probation schools; the odds of leaving the district were equal or higher for students attending non-probation schools. This finding is consistent with unadjusted mobility rates shown in Figure 3, and it confirms a well-known finding that student turnover is higher in low-performing schools (Kerbow 1996). Because the likelihood of school exit in Model 1 may be biased by school selection, the parameters do not measure the causal effect of the CPS accountability policy.

Models 2 through 5 include difference-in-differences (DD) parameters that address this concern of selection bias. The reported estimates capture the policy effect by measuring the difference between attending probation versus non-probation schools during the active policy era compared to this same probation versus non-probation difference in the pre-policy era. The estimated policy effect on school transfers over the summer is quite consistent across Models 2 through 5, although the addition of baseline student covariates (Model 3), school composition characteristics (Model 4), and school performance measures (Model 5) improves the overall model fit.

In Model 5, our preferred model, we estimate that school probation assignment increased the hazard ratio of transferring schools by 19.3 percent ($p < .01$). The magnitude of the policy effect was similar for the hazard ratio of leaving the district, which increased by 16.1 percent ($p < .05$). For both school-exit pathways, the effect of probation assignment was significant in the summer immediately following the first year of probation assignment and at no other semester. Instead of responding immediately, students waited until the end of the academic year to switch schools or leave the district. We see a slight decreased effect of probation on district exits ($p < .05$) in the initial midyear period after announcement, which might be explained if families took a "wait and see" approach, or if selectively sensitive families left the district in the summer of 1996 in anticipation of the new probation policy.

We do not include individual student math and reading test scores as covariates in Models 1 through 5. Because test performance is negatively correlated with the likelihood of school exit generally, we conducted a range of sensitivity tests verifying that the policy treatment effect estimates are very stable whether or not test scores are explicitly modeled (see the online supplement, Table S3). Exclusion of these covariates allows us to generalize to a broader elementary student population, such as 1st and 2nd graders with no prior test data and also newcomers to the district.

The distribution of school academic performance in Chicago is wide, so the effect of probation policy may be relevant only to the local set of schools near the official 15 percent reading proficiency cutoff. In Table 3, Models 6 and 7 present estimates from models identical to the preferred Model 5 but that limit the analytic sample to students attending schools with fewer than 35 or 25 percent students reading at grade level, respectively. While the coefficients decline slightly in these reduced samples, the models lead to substantively similar conclusions.

Up to this point, there is still reason to interpret the probation effect estimates in Model 5 with caution. The DD identification strategy assumes that if not for probation policy, the differential rates of school exit between students attending probation versus

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Table 2. Effect of School Probation on Student Exit: Selected Estimates from Hazard Regression Models

	Naïve	Difference-in-Differences Estimation			
	1	2	3	4	5
Exit Type: Transfer to Another CPS School					
Probation[a] at time 1 (midyear)	1.704***	.950	.932	.942	.935
	(.049)	(.035)	(.035)	(.036)	(.035)
Probation at time 2 (summer)	1.598***	1.222***	1.192***	1.205***	1.193***
	(.056)	(.055)	(.055)	(.055)	(.055)
Probation at time 3 (midyear)	1.966***	1.142*	1.110	1.123	1.112
	(.083)	(.072)	(.071)	(.072)	(.071)
Probation at time 4 (summer)	1.514***	1.130	1.085	1.099	1.085
	(.066)	(.071)	(.069)	(.071)	(.069)
Exit Type: Leave CPS District					
Probation at time 1 (midyear)	1.045	.874*	.882*	.889*	.888*
	(.033)	(.053)	(.053)	(.053)	(.053)
Probation at time 2 (summer)	.734***	1.134*	1.159*	1.162*	1.161*
	(.029)	(.071)	(.076)	(.073)	(.074)
Probation at time 3 (midyear)	1.051	.925	.963	.970	.970
	(.046)	(.075)	(.081)	(.081)	(.081)
Probation at time 4 (summer)	.741***	1.021	1.072	1.067	1.066
	(.038)	(.094)	(.103)	(.099)	(.100)
Student-Semester Observations	1,009,830	1,009,830	1,009,830	1,009,830	1,009,830
Students	325,493	325,493	325,493	325,493	325,493
Log Likelihood	–398,199	–398,164	–380,862	–376,976	–376,219
BIC	796,619	796,687	762,609	754,975	753,516
Model Components[b]					
Baseline hazard	Y	Y	Y	Y	Y
Diff-in-diff parameters		Y	Y	Y	Y
Student covariates			Y	Y	Y
School composition covariates				Y	Y
School test performance covariates					Y

Note: Each column is a multinomial logistic regression model comparing the conditional likelihood of transferring schools or of leaving the district to the conditional likelihood of staying at the same school (baseline). Selected outcome-specific hazard ratios (exponentiated logits) are reported with standard errors clustered by school and grade in parentheses. The analytic sample includes all students from Table 1 observed for up to four consecutive records. Student-semester records are censored after the first school-exit event or when a student is compelled to leave due to grade promotion. See Appendix Table A1 for hazard rates and Appendix Table A2 for a full set of Model 5 parameter estimates.

[a]Probation is the school-level policy treatment assigned in the fall of 1996 as it persists over time. If a school probation status changes after summer updates in 1997 or 1998, surviving student-semester records are censored. Model 1 coefficients are from discrete indicators for school probation status at each analysis time; there are no other covariate controls. Models 2 through 5 have difference-in-differences parameters, so the coefficients are estimated from interaction terms between cohort and probation status at each analysis time (Equation 2).

[b]Student and school covariates are described in Table 1. All covariates are held constant as measured in the first fall semester of the student's cohort year.

*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

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Table 3. Robustness Checks

	School Reading Proficiency[a]		Outlier Censoring[b]		Retention[c]
	6	7	8	9	10
	<35% Proficient	<25% Proficient	Less Restrictive	Not Restrictive	Also Includes Grades 3, 6
Exit Type: Transfer to Another CPS School					
Probation at time 1 (midyear)	.927*	.906*	.948	.959	.916**
	(.035)	(.035)	(.035)	(.036)	(.028)
Probation at time 2 (summer)	1.184***	1.158**	1.205***	1.214***	1.205***
	(.055)	(.055)	(.056)	(.056)	(.048)
Probation at time 3 (midyear)	1.104	1.080	1.120	1.206**	1.140*
	(.070)	(.069)	(.071)	(.080)	(.062)
Probation at time 4 (summer)	1.078	1.057	1.119	1.242**	1.127*
	(.069)	(.068)	(.070)	(.085)	(.060)
Exit Type: Leave CPS District					
Probation at time 1 (midyear)	.874*	.852*	.890*	.890*	.905
	(.053)	(.054)	(.053)	(.053)	(.047)
Probation at time 2 (summer)	1.149*	1.130	1.175*	1.166*	1.106
	(.075)	(.078)	(.074)	(.073)	(.060)
Probation at time 3 (midyear)	.967	.955	.982	.991	.965
	(.084)	(.086)	(.081)	(.081)	(.069)
Probation at time 4 (summer)	1.069	1.064	1.066	1.094	1.086
	(.103)	(.106)	(.098)	(.099)	(.091)
Student-Semester Observations	767,581	518,270	1,012,732	1,018,462	1,408,758
Students	253,350	174,058	325,493	325,493	450,854

Note: Selected results reported in the hazard ratio metric with standard errors clustered by school and grade reported in parentheses. See Table 2 notes.
[a]Models 6 and 7 limit the analytic sample to students attending schools with fewer than 35 and 25 percent, respectively, of students proficient in reading.
[b]Hazard regressions throughout the analysis censor student-semester observations at schools when they experience a single-semester increase in school-exit rate by more than 10 percent of its average over time. Model 8 relaxes this criterion from 10 to 15 percent, and Model 9 relaxes all outlier restrictions.
[c]Model 10 includes a sample of students enrolled in grades 1 through 7 in the Fall 1994 and Fall 1996 cohorts. Students in grades 3 and 6 faced the threat of repeating a grade based on a new retention policy that was implemented at the same time as probation. The preferred model (Table 2, Model 5) includes only students in non-retention grades: 1, 2, 4, 5, and 7.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

non-probation schools would otherwise be the same for the Fall 1994 and Fall 1996 student cohorts. This is not completely testable, but we can address two known changes during this time period that may have differentially affected students at low-performing schools.[9]

First, the Chicago Housing Authority (CHA) and the U.S. Department of Housing and Urban Development began closing housing developments in Chicago in the mid-to-late 1990s. The bulk of the closures did not begin until after 1999, when the CHA implemented the Plan for Transformation (Popkin et al. 2002). Still, the initial closures displaced a nontrivial number of students (de la Torre and Gwynne 2009; Jacob 2003), and

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many of these students attended low-performing schools assigned probation. Because the CPS dataset does not include student residence or school location, we use a school-level proxy measure of CHA closure. We detect, for each school, any semester in which the rate of school exit spiked over 10 percent above the average school-exit rate over eight semesters. These uncharacteristically high exit spikes reflect outlying circumstances that are possibly related to abrupt displacement of housing development residents, so we censor students attending those schools when the spike event occurs (and for all subsequent observations). The results we have presented thus far already include this conservative correction, and we present alternative specifications in Table 3. Model 8 relaxes the censoring criteria to apply only to schools experiencing a 15 percent spike, and Model 9 drops this censoring rule altogether. Estimates from these less restrictive models match the direction of those presented in the main analysis but are stronger. Thus, excluding the outlying cases, as in Table 2, does not change the conclusions drawn in this analysis, but it does provide a more conservative estimate that assuages our concern that the findings are driven by housing development closures.

Second, the CPS Board implemented a grade-retention policy, in step with probation, that required students in 3rd, 6th, and 8th grade to score above a cutoff point on their standardized exams to advance to the next grade. Low-performing students who were forced to repeat—and who were also more highly concentrated in probation schools—may have responded by leaving the district to avoid the promotion policy or by switching schools. We account for this alternative explanation by limiting the analysis to students in non-benchmark grades (1st, 2nd, 4th, 5th, and 7th). If we expand the sample to include all students in grades 1 through 7, as shown in Table 3, Model 10, the estimated policy effect on within-district transfers is stronger but the effect on district exits is slightly weaker. While the direction and magnitude of estimates in Model 10 yield similar conclusions as those in Model 5, we believe that the restricted non-benchmark-grade sample presented throughout the article provides a more careful test of the probation policy effect.

Given the robustness to alternative explanations, we are confident that the summer-transfer and district-exit effects measured in Model 5 capture tangible evidence of family response to probation. That we detect a response at all is a notable divergence from scholarship showing families' muted response to accountability under NCLB and in other districts. This raises the interesting question of whether aspects of probation implementation in Chicago—such as local control or a manageable proportion of affected schools—explain this differential policy effect. An explanation is beyond the reach of our data, but overall family responsiveness in Chicago invites two important empirical questions to which we now turn.

Did Response Vary by Race and Socioeconomic Status?

School choice is shaped by local context and constraints, so family resources and the supply of viable alternative schools might influence the likelihood of response to probation policy. We test for the possibility that some student populations were more responsive than others using hazard regressions that replicate Model 5 (Table 2) but that are stratified by race and by family income subgroups. Table 4 reports these results. Note that pooled, fully-interacted models were used to formally test for statistically significant differences between subgroups; they are reported in the "Sig." columns of the table.

We begin by examining differences by student racial and ethnic background. Referring back to Table 1, it is evident that black students were disproportionately exposed to probation assignment. Over 80 percent of all students attending probation schools were black, and almost none were Asian, Native American, or white. Policy response may have varied by race because schools assigned to probation were clustered in segregated,

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Table 4. Variation in Probation Response by Student Race and Free or Reduced Price Lunch Status

	By Race/Ethnicity			By Free or Reduced Price Lunch		
	11	12		13	14	
	Black	Hispanic	Sig.	FRL	Not FRL	Sig.
Exit Type: Transfer to Another CPS School						
Probation at time 1 (midyear)	.920*	.779**		.926*	1.148	
	(.036)	(.075)		(.035)	(.207)	
Probation at time 2 (summer)	1.200***	1.016		1.203***	1.707***	*
	(.053)	(.157)		(.058)	(.238)	
Probation at time 3 (midyear)	1.034	1.354		1.132*	1.144	
	(.071)	(.213)		(.071)	(.265)	
Probation at time 4 (summer)	1.074	1.030		1.073	2.044**	*
	(.072)	(.245)		(.070)	(.509)	
Exit Type: Leave CPS District						
Probation at time 1 (midyear)	.962	.538***	***	1.037	1.189	
	(.064)	(.071)		(.264)	(.116)	
Probation at time 2 (summer)	1.219**	.856	*	1.014	2.406***	***
	(.090)	(.123)		(.070)	(.408)	
Probation at time 3 (midyear)	.996	.838		.945	.915	
	(.097)	(.141)		(.080)	(.211)	
Probation at time 4 (summer)	1.087	.840		1.064	.605	
	(.116)	(.174)		(.103)	(.229)	
Student-Semester Observations	537,234	318,971		846,971	162,859	
Students	175,779	102,610		267,838	57,655	
Students Exposed to Probation	22.5%	7.3%		16.2%	7.3%	

Note: Selected results presented from four models that are identical in specification to Model 5 (Table 2) but restricted to the indicated subgroup. Results reported in the hazard ratio metric with standard errors clustered by school and grade reported in parentheses. The Sig. column reports whether the ratio of effect estimates between subgroups significantly deviates from 1; this is tested with fully-interacted models estimated for the unstratified sample.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

high-poverty black neighborhoods (see Figure 2). To test for racial response variation, we divide the student sample into black and Hispanic subgroups. White, Asian, and Native American students were extremely underexposed to school probation and were omitted from the racial heterogeneity analysis.[10]

Results from Model 11 of Table 4 show that black students responded to probation by transferring and exiting. In contrast, Hispanic students (Model 12) were more likely to stay in response to probation in the first semester after the policy was implemented, and were possibly more likely to transfer to other CPS schools in a later semester, although this latter result is not statistically significant. The racial variation in Table 4 invites further examination. One potential explanation is that some Hispanic families may have delayed exiting due to difficulty finding alternative schools that meet their bilingual-program needs. It is also possible that many black households, with a longer history of being underserved by the Chicago education system (Neckerman

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2007), had little loyalty compelling them to stay at local schools placed on probation and were thus more immediately responsive to the policy. A more cautious interpretation—and one we favor—is that the policy effect estimates for the Hispanic student population are simply measured with more noise. CPS has high levels of school segregation by race, and only 13 of the 249 schools with less than 85 percent black students were initially assigned to probation. This means many of the Hispanic students exposed to probation were clustered at a small number of schools; estimates of their response are more sensitive to idiosyncratic events that could violate the parallel trends assumption required by our estimation method.[11] Because our data-use agreement with CPS does not allow us to identify schools by name or location, we are unable to address this issue. The speculative accounts we suggest here thus warrant further empirical scrutiny with data better suited to illuminate the mechanisms leading to racial variation in probation policy response.

Table 4 also reports a subgroup analysis by family socioeconomic background. Free or reduced price lunch status (FRL) serves as a rough proxy for household economic status, as students qualify for the program by living within 185 percent of the poverty line. FRL students come from households with fewer resources to respond to a negative school label, even if they would prefer to transfer out. Private-school tuition, financial barriers preventing relocation to nearby high-demand school districts, and transportation concerns may constrain the alternative enrollment options available to low-income households. Poor households may also have had less exposure to the public announcement of the policy change (e.g., the list of probation schools published in the newspaper) or simply been less concerned with its implications.

Model 13 in Table 4 estimates that poor (FRL) students were more likely to transfer schools in the first summer after implementation, but there is no evidence of response by leaving the district. Estimates in Model 14 for non-poor (not FRL) students, by contrast, show a very strong response to probation by transferring, and an even stronger response by leaving the district in the first summer period. Although non-poor students were much less likely to experience probation, their exit response is strongly estimated at 2.41 times the baseline likelihood of leaving ($p < .001$) and is significantly different from poor students ($p < .001$). Non-poor students who stayed after initial probation assignment and through the first summer were more likely to later transfer but not more likely to later leave the district. Although we cannot precisely comment on the destination of non-poor students who left the district, some likely enrolled in nearby private schools; in the city of Chicago, around 15 percent of families exercise this option (Saporito and Sohoni 2007).

Did Students Leave for Higher-Performing Schools?

The impact of the CPS probation policy may extend into the destination choices of families making school transfers. Some school-choice reform advocates believe that families trapped in low-performing schools will be liberated by the ability to choose freely among a set of higher-performing alternatives (see Archbald 2004). The case of probation implementation in Chicago does not strictly adhere to this projection, because families already had an open-enrollment school choice system in place. However, the introduction of a new institutionalized definition of school quality—probation status—may have prompted marginal families to "upgrade" to higher-performing destinations.

In this stage of the analysis, we assess whether the probation policy increased student transfers from low- to high-achieving alternative schools. We again use a difference-in-differences (DD) identification strategy (Equation 1), but we limit the analytic sample to students from the school-exit analysis who transferred to other schools within CPS. We exclude students who left the CPS district because their transfer outcomes are unobservable. Table 5 presents these results, with tests

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Table 5. School Transfer Attainment by Definition of Receiving School Quality

	Receiving School on Probation (binary)		Reading Percentile Rank of Receiving School	
	15	16	17	18
	Constant[a]	Time-Varying	Constant	Time-Varying
Sending school on probation	1.295***	1.345***	–4.051***	–4.466***
	(.072)	(.075)	(.523)	(.509)
Fall 1996 cohort	.929*	.856***	–.118	.024
	(.028)	(.026)	(.265)	(.282)
Probation x Fall 1996	.968	.949	.575	1.734**
	(.056)	(.055)	(.546)	(.539)
Observations (transfers)	65,534	66,229	65,534	66,229

Note: Selected results from four regression models limited to students in the main analytic sample (Table 1) making their first transfer to another CPS school. Transfers occur over two consecutive semesters, where students leave a "sending" school and enroll in a "receiving" school. Models 15 through 18 include the same covariates and baseline time indicators used in Model 5 (Table 2), but estimate an overall probation effect across semester transitions. Models 15 and 16 are logistic regressions reported in odds ratio units; Models 17 and 18 are OLS regressions. Reading percentile rank is based on the school-level percentage of students reading proficiently, measured among all testing schools within the district in the same year. Standard errors in parentheses are clustered by school and grade.

[a]"Constant" models hold receiving school quality outcome measures constant at the initial fall semester of each cohort. "Time-varying" models measure school quality based on the most recent probation announcement and the most recent spring test scores reported before students attended their new receiving school. There are fewer observations in Models 15 and 17 because test and probation information for some receiving schools was not available at baseline.

*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

of two different definitions of school quality that align with the standards of the policy.

Models 15 and 16 are logistic regressions predicting whether students transferred to receiving schools that were assigned probation. In Model 15, the probation status of the receiving school is held constant at the initial baseline semester for each cohort (Fall 1994 or Fall 1996), and Model 16 allows the receiving school status to vary based on the time of the student transfer. DD parameters are reported as odds ratios.

Results from Model 15 reveal a strikingly durable system of unequal school selection in the Chicago Public Schools. In the pre-policy cohort, students who left probation schools had 29.5 percent higher odds of enrolling in another probation school than did students leaving non-probation schools ($p < .001$). If new information about school quality can level the school selection playing field, then students attending probation schools should have upgraded to non-probation schools at a higher rate than other students once the policy was introduced. Yet the key test of this policy effect—the interaction term—is not statistically distinguishable from one. This means that the relative odds of cycling from one probation school to another *did not change* under the new policy.

Importantly, the null interaction effect in Model 15 depicts a story of relative system-wide intransigence to the policy, because it compares students leaving probation schools

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versus those leaving non-probation schools. This does not mean the odds of transferring to a probation school remained stable over time. In fact, the Fall 1996 cohort coefficient indicates that the odds of a transfer from non-probation to probation schools decreased by 7.1 percent during the policy era ($p < .05$). Thus the policy led to *overall* sorting away from schools assigned to probation, but it did not change relative inequality among students leaving probation versus non-probation schools. This same pattern emerges if probation status is measured at the time of student transfers (Model 16).

When we further consider the impact of probation policy on a continuous measure of school quality, we find that students upgraded to only marginally higher-performing schools.[12] Models 17 and 18 in Table 5 report DD parameter estimates from ordinary least squares regressions that evaluate the reading proficiency of transfer students' receiving schools, defined as a within-year, within-district school percentile rank. Percentile rank characterizes school quality as a relative good in the distribution of available options, allowing for a consistent comparison between pre-policy and policy-active periods even as absolute testing levels changed across the district.

Before reviewing results from Models 17 and 18, it is important to clarify what test scores and percentile ranks indicate about school quality during the probation policy implementation period in Chicago. Jacob (2005) shows that although absolute scores on the high-stakes ITBS exam rose in CPS schools, students did not improve on the comparable low-stakes IGAP exam. His study concludes that any test-related measurement of school quality over the implementation period may conflate actual improved learning conditions with the consequences of high-stakes testing, that is, changes in student effort, teacher gaming, educational triage, or administrative resource adjustments (see also Bryk 2003). To address this complication, our analysis tests two measures of school quality that present different assumptions about the meaning of changes in percentile rank during the policy implementation period.

Model 17 measures the quality of receiving schools with their percentile rank held constant at an initial baseline (Spring 1994 or Spring 1996). This approach imposes an assumption that school quality did not meaningfully change under active policy conditions, and pre-policy performance is a more reliable indicator of actual school quality. These results reveal that students attending probation schools in the pre-policy period transferred to schools that were 4.1 percentile ranks lower in the distribution of all schools in the district ($p < .001$), consistent with prior scholarship showing circulation among low-performing schools (Kerbow 1996). Once again, the interaction term testing a policy effect on students leaving probation schools versus those leaving non-probation schools is not statistically significant. This supports our claim of durable selection patterns before and after the probation policy was introduced.

In Model 18, school percentile rank is measured at the time of transfer. This alternative to Model 17 considers changes in rank to reflect true changes in service quality compared to other schools in the district. There is modest evidence of transfer upgrades. Students who left probation schools in the policy-active period enrolled in schools that were, on average, 1.7 percentile ranks higher in the overall distribution of schools ($p < .01$). However, we are hesitant to conclude from these findings that probation policy substantially reduced inequality in school selection. The null interaction effects in Models 15, 16, and 17, and the positive interaction effect in Model 18, illustrate a localized impact of the policy on school sorting patterns: students transferred from probation schools to relatively low-performing alternative schools that had slightly improved under the new policy conditions. These localized school quality upgrades do not support an interpretation of liberated school choice.

We further explicate the findings from Table 5 with a transition matrix. The top and bottom panels of Table 6 compare sending

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Table 6. Districtwide Transfers by Sending and Receiving School Characteristics

Cohort	Sending School Quality	Receiving School Quality (row percentages)[a]						Student Transfers
		A	B	C	D	E	Unknown	
Fall 1994	A. Quartile 1, probation	28.5	16.9	29.4	17.4	4.4	3.4	7,157
Pre-policy	B. Quartile 1, non-probation	21.8	16.3	28.7	21.7	7.1	4.4	5,128
	C. Quartile 2	17.0	14.1	29.3	26.5	11.4	1.6	8,979
	D. Quartile 3	9.8	10.6	24.8	32.8	21.0	1.1	9,225
	E. Quartile 4	5.0	6.1	13.2	28.1	46.1	1.4	3,789
Fall 1996	A. Quartile 1, probation	25.6	17.8	30.5	17.5	4.6	4.1	6,312
Policy-active	B. Quartile 1, non-probation	19.2	18.7	28.7	21.3	7.3	4.8	4,268
	C. Quartile 2	14.5	15.6	31.7	23.6	10.0	4.6	10,878
	D. Quartile 3	9.3	12.3	23.5	30.6	21.3	3.0	8,729
	E. Quartile 4	4.5	5.4	12.9	24.7	47.3	5.3	3,972

Note: School quartiles (within year and district) are based on the school-level percentage of students reading proficiently. Sending school quartiles are measured in the initial fall semester for each cohort; receiving schools are measured at the time of transfer (time-varying). Receiving schools in the "unknown" category did not have elementary test data from the prior spring semester (see Table 5 notes).

[a]Row percentages add to 100. Cells are shaded in gray to note horizontal transfers between schools in the same category. Cells above the gray diagonal are upgrades; cells below are downgrades.

versus receiving schools for student transfers in the pre-policy and policy-active periods, respectively. The rows of each matrix report the percentage of students moving from a common sending school category (measured in the baseline semester) to each type of receiving school (measured at the time of transfer).

The distribution of flows between the Fall 1994 and Fall 1996 cohorts confirm the overall conclusion of durable systemwide selection inequality. Despite the introduction of newly available information and evidence of policy-responsive exits from probation schools, the proportion of transfers from low-achieving to high-achieving schools changed very little.

Focusing first on the pre-policy period (top panel), 28.5 percent of students transferred from one probation school to another, and students leaving all other schools transferred to probation schools at lower rates. Only 5 percent of students attending top-quintile schools transferred to probation schools. Most mobile students transferred to schools within one quartile range of their own school. For instance, 15 of every 20 students who left probation schools transferred to destination schools on probation or in the 1st or 2nd quartile, whereas 15 of every 20 transfer students from top-quartile schools moved to schools in the 3rd or 4th quartiles.

The bottom panel in Table 6 reports the transition matrix for transfers made by the Fall 1996 student cohort. There is indeed evidence of a modest decrease in repeat probation enrollment, from 28.5 percent in the pre-policy period to 25.6 percent in the implementation period. This change is not reflected in the DD interaction parameter estimates from Models 15 and 16 (Table 5), because students transferring from non-probation schools also avoided probation schools at higher rates. Strikingly, even in the active policy period, students leaving probation schools still tended to enroll in low-performing non-probation schools. Only 22.1 percent of probation students transferred to schools in the top two quartiles, compared with 21.8 percent in the pre-policy period. These patterns changed very little after introduction of the probation policy.

Overall, the attainment models and transition matrices cast considerable doubt on the prospect that the new information and accountability conditions in Chicago liberated families from low-performing schools. Transfers did not lead to significant upgrades in school quality, with only a small proportion of families switching from probation to non-probation schools, and with even fewer enrolling in the two highest quartiles of schools in the district. Conclusions from these tables are necessarily limited, because they draw on only the first two years of probation implementation, they exclude students who voluntarily left the district, and they use only two definitions of school quality based on the policy itself. Nonetheless, the evidence that school upgrades were limited despite a period of increased turnover and awareness about school performance suggests that enrollment choices are constrained by much more than access to information. This finding is consistent with qualitative evidence in Chicago (Pattillo, Delale-O'Connor, and Butts 2014) that although families state strong academic preferences for their children's schools, they are often forced to choose the "lesser of evils" among their limited local options. Our findings demonstrate this to be a districtwide phenomenon.

DISCUSSION

In this study, we investigated how enrollment changed in response to new information about school quality in the Chicago Public Schools. Our analysis of student administrative panel data yields three clear findings. First, students attending schools that were assigned probation under the policy had 19.3 percent higher odds of transferring schools and 16.1 percent higher odds of leaving the district altogether in the summer following initial assignment. Second, both poor and non-poor families transferred to other schools in the district, but non-poor families were

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more likely to respond and also more likely to leave the CPS district and relocate or enroll in private school. Third, school transfers induced by new information did not lead to a substantial increase in the proportion of students upgrading to higher-quality schools.

Our results complicate the ongoing debate over expanded school choice. Proponents suggest that accountability and choice programs together stimulate family demand for better schools, which compels schools to improve service quality or risk loss of enrollment and funding (Le Grand 2007). Yet almost all other studies of responses to school accountability labels find no evidence of the type of individual response implicit in this theory of change (Henderson 2010; Lauen 2008; West and Petersen 2006). By merely demonstrating that families responded, our results suggest that accountability and choice initiatives could stimulate changes in how families consume public education under certain conditions.

However, opponents warn that school choice reforms could generate higher levels of inequality through consumer-driven segregation (Orfield and Frankenberg 2013), and our results add validity to this concern. Exiting a school requires some combination of human, social, and financial resources, and it is well-known that these resources are not uniformly distributed across families. The fact that non-poor families transferred within the district at higher rates than poor families, and that they were much more likely to leave the district altogether, demonstrates the enduring impact of family background on educational access. It also leaves open the possibility that an underserved population of poor families would have opted out of probation schools if they had the means to do so.

We do not want to overstate the impact of probation policy on the distribution of students in Chicago by income level. CPS already serves a disproportionate concentration of low-income children, because many Chicago residents—particularly white and middle-class students—attend private schools. Of the non-poor students still selecting into CPS, only 1 in 20 attended a probation school. However, the response behavior shown here supports the position that policies that rely on family resources to access higher-quality services may lead to sorting that privileges families with higher socioeconomic status.

The response to probation in Chicago must also be understood in a broader historical and spatial context. When CPS implemented its accountability program in 1996, the rules governing probation assignment were applicable to all schools across the district. Yet policy neutrality is anything but neutral when it is implemented in a spatially segregated city, shaped by historical forces of racism, exclusion, and disinvestment. The map in Figure 2 brings this spatial inequality into sharp relief, illustrating that schools assigned to probation in 1996 were heavily clustered in poor, black neighborhoods on the South and West sides of Chicago. The accountability policy almost exclusively affected schools in these neighborhoods, where there is a well-known link between concentrated poverty and student learning (Bryk et al. 2010; Roderick, Jacob, and Bryk 2002; Sampson, Sharkey, and Raudenbush 2008). As a result, black students were severely over-exposed to the policy. Over 80 percent of students attending probation schools were black, even though they comprise only 54 percent of the districtwide student population. Moreover, the spatial clustering meant that families attending probation schools had fewer nearby alternative non-probation transfer options.

Given the segregated context in Chicago, we considered whether race played a significant role in the likelihood of response. Here our analysis yielded a finding that merits future empirical investigation: black students were most responsive in the summer after probation assignment, whereas Hispanic students were more likely to stay early on and possibly make transfer decisions after multiple semesters. We offered speculative explanations for this variation, such as different bilingual-program needs, variation in the historical relationships between local schools and racially segregated communities, and possible statistical noise due to limited

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Hispanic exposure. These accounts require additional empirical scrutiny that extends beyond the scope of this project.

Evidence of family response to the probation accountability label in Chicago enabled us to investigate an aspect of school choice rhetoric that has, until now, remained empirically untested: given the clustering of disadvantaged schools, what do accountability labels add to school decision-making processes, and how *should* we expect families to respond? We believe the school transfer attainment analysis and transition matrices tell a particularly important story of constraint. We showed that many students in Chicago transfer from one probation school to another, and this hardly decreased once the policy was introduced, especially compared to other students across the district. The overall flow of transfers remained very stable. In the implementation of probation policy, the new information and conditions may have influenced school preferences, but the policy did not change the local supply of alternative options in the short- to medium-term. Throughout the study, we observe a response under constrained conditions, and we suspect the response may have been even stronger if families had broader access to high-quality choices without the need to travel long distances.

We are careful to distinguish this finding from an evaluation of whether probation—and school choice policies more generally—can raise the overall supply of higher-quality schools through informed quasi-market competition (Le Grand 2007). We leave this possibility untested because we cannot disentangle the effect of probation labels and market pressure on test performance from the simultaneous impact of accountability-based educator incentives and student grade retention in Chicago (Jacob 2005). However, our results suggest that even if families respond to accountability assignment, their preferences are constrained by the *existing* supply of higher-quality schools.

There is also little reason to assume these relationships will be consistent across all contexts and all dimensions of family disadvantage. The social geography of cities varies substantially. Some cities are more economically and racially segregated than others, and cities differ in population size and area, both of which may affect the number of schools from which to choose and the difficulty involved in accessing them. Rather than making universal claims about the effects of school choice reforms across contexts, as has been typical in this literature, we argue that features of local spatial contexts interact with family background to produce choice outcomes when new information is available.

Nonetheless, the findings presented here should limit our expectation that information and choice alone will address spatially unequal access to high-quality schooling, or that they will equalize opportunity for children trapped in inferior schools. In Chicago, the structural forces that constrain choice persisted both before and after the accountability policy was implemented. The policy brought new information and conditions that certainly affected schools, teachers, and, as we have shown, families. But without addressing the broader social and economic determinants that shape the quality of options available to families, the expansion of choice will do little to empower families in their educational decisions.

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APPENDIX

Table A1. Hazard Rates of Student Outflow within Four Semesters by Type, Probation Status, Cohort, and Student Characteristics

	Transfers within CPS				Exits from CPS District			
Probation Status:	No Probation		Probation		No Probation		Probation	
Fall Cohort:[a]	1994	1996	1994	1996	1994	1996	1994	1996
Overall	.251	.247	.350	.439	.147	.144	.116	.131
By Race/Ethnicity								
Asian	.140	.156	[b]	[b]	.206	.188	[b]	[b]
Black	.289	.300	.352	.433	.133	.132	.108	.124
Hispanic	.239	.217	.337	.492	.128	.128	.151	.194
Native American	.241	.176	[b]	[b]	.207	.176	[b]	[b]
White	.163	.154	.314	[b]	.224	.215	.168	[b]
By Free or Reduced Price Lunch								
FRL	.281	.274	.366	.461	.094	.094	.068	.074
Not FRL	.135	.129	.216	.229	.345	.364	.506	.679

Note: N = 255,372 students. Transfers and district exits are measured as the first mobility event over a four-semester period. The denominator of each cell includes students who transferred, exited, or stayed all four semesters; censored students are excluded. See the online supplement (http://asr.sagepub.com/supplemental) for a complete frequency table.

[a]The Fall 1994 cohort was not exposed to the probation policy (see Table 1 notes).

[b]Fewer than 50 students contribute to cell denominator.

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Table A2. Preferred Main Effects Hazard Regression Model, Full Results

	Transfer v. Stay		Leave CPS v. Stay	
	Coeff.	SE	Coeff.	SE
Baseline Hazard				
Time 1 (midyear)	.033***	(.003)	.027***	(.002)
Time 2 (summer)	.055***	(.004)	.039***	(.003)
Time 3 (midyear)	.020***	(.002)	.020***	(.002)
Time 4 (summer)	.051***	(.004)	.035***	(.003)
Difference-in-Differences (DD) Parameters				
Fall 1996 cohort (policy-active)	.976	(.013)	.988	(.014)
Probation assignment				
Probation, time 1	1.102**	(.037)	1.047	(.045)
Probation, time 2	.901*	(.042)	.668***	(.036)
Probation, time 3	1.141*	(.067)	1.074	(.067)
Probation, time 4	.890*	(.049)	.728***	(.048)
Probation x Fall 1996 cohort				
Probation, time 1 x Fall96 cohort	.935	(.035)	.888*	(.053)
Probation, time 2 x Fall96 cohort	1.193***	(.055)	1.161*	(.074)
Probation, time 3 x Fall96 cohort	1.112	(.071)	.970	(.081)
Probation, time 4 x Fall96 cohort	1.085	(.069)	1.066	(.100)
Student Covariates				
Free or reduced price lunch (FRL)	1.094***	(.019)	.131***	(.004)
Race/ethnicity (ref. = Hispanic)				
Asian	.832***	(.044)	1.112*	(.046)
Black	1.187***	(.038)	.959	(.036)
Native American	1.219*	(.122)	1.330**	(.142)
White	1.077**	(.029)	1.174***	(.035)
Female	.964***	(.008)	.961***	(.011)
Special education	1.047**	(.017)	1.151***	(.025)
Bilingual program (active)	.822***	(.020)	.801***	(.025)
Bilingual program (former)	.873***	(.021)	1.153***	(.031)
Legal guardian not a parent/relative	1.207***	(.024)	1.207***	(.037)
Within-district school transfer in past two semesters	2.341***	(.028)	1.346***	(.024)
Returning after district absence	1.956***	(.118)	2.322***	(.176)
Single-semester record lapse	.938	(.041)	1.194**	(.078)
Repeating grade	1.274***	(.033)	1.151***	(.044)
Skipping grade	1.337***	(.090)	1.413***	(.136)
Grade level (ref. = 4th grade)				
1st grade	1.102***	(.028)	1.090***	(.027)
2nd grade	1.066**	(.026)	1.064*	(.027)
5th grade	1.005	(.032)	.925**	(.025)
7th grade	.844***	(.036)	.751***	(.023)
School Covariates				
Proportion FRL	1.627***	(.136)	7.701***	(.688)
Racial composition				
Proportion Asian	3.333***	(.576)	2.220***	(.362)
Proportion black	.870***	(.036)	1.041	(.046)
Proportion Native American	4.533	(6.017)	1.542	(2.122)
Proportion white	.517***	(.052)	1.390***	(.111)
Median math score	.857*	(.060)	.967	(.063)
Median reading score	.570***	(.050)	.697***	(.052)

Note: Full results from the preferred hazard regression model presented in Table 2 (Model 5). Coefficients are reported in the hazard ratio metric. Standard errors are clustered by school and grade in parentheses. N = 1,009,830 student-semester observations (325,493 students). See Table 2 notes for additional information.
*p < .05; **p < .01; ***p < .001 (two-tailed).

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Data

The authors analyze restricted data compiled by the University of Chicago Consortium on Chicago School Research and made available by Chicago Public Schools under terms of a license between Jennings and the University of Chicago. The authors thank both institutions for making the data available.

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Notes

1. The CPS accountability policy was implemented in step with a new grade-retention policy for 3rd-, 6th-, and 8th-grade students (Jacob and Lefgren 2004). We discuss this in the Data and Methods section.
2. The fuzzy assignment of probation in September of 1996 raises the concern that academic performance alone did not determine probation assignment; indeed, some schools applied for waivers and this may be systematically related to unmeasured schooling characteristics that also affect the likelihood of school exit, such as community engagement. This informed our decision not to use a regression discontinuity design for the empirical analysis.
3. The map in Figure 2 was constructed with separate, publicly available data that were not linked to student administrative records.
4. Fall semester records are captured on the 20th school day since the start of the academic year; spring records are captured in the following May. September 30th was the 20th school day of the 1996/1997 academic year.
5. Regressions report standard errors clustered by school and grade to account for the multilevel data structure.
6. Table A1 in the Appendix reports hazard rates.
7. This estimates duration-dependent effects of probation, which relaxes the assumption of a proportional probation effect on school exit across time points (Singer and Willett 2003).
8. This is modeled in the hazard regression by coding school probation status changes in 1997 or 1998 as censoring events, at which point students attending these schools drop from the risk set. Because the counterfactual Fall 1994 student cohort is not subjected to this censoring, the rule could pose a validity threat to the DD estimation. Sensitivity tests confirm this is not a concern in this case.
9. We do not consider supply-side changes as a factor here, because no new elementary charter schools opened in CPS during the analysis period.
10. Even when combining white, Asian, and Native American categories into a composite subgroup, cell sizes were very small. Table S1 in the online supplement reports cross-tabulated frequencies.
11. This is not a concern for the free/reduced price lunch (FRL) subgroup analysis, because FRL and not-FRL students were more evenly distributed across schools.
12. Measuring receiving school quality using math test performance levels yields similar conclusions.

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