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Joshua D. Angrist
Victor Lavy
Jetson Leder-Luis
Adi Shany

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ABSTRACT

We use the discontinuous function of enrollment known as Maimonides Rule as an instrument for class size in large Israeli samples from 2002-2011. As in the 1991 data analyzed by Angrist and Lavy (1999), Maimonides Rule still has a strong first stage. In contrast with the earlier Israeli estimates, however, Maimonides-based instrumental variables estimates using more recent data show no effect of class size on achievement. The new data also reveal substantial enrollment sorting near Maimonides cutoffs, with too many schools having enrollment values that just barely produce an extra class. A modified rule that uses data on students' birthdays to compute statutory enrollment in the absence of enrollment manipulation also generates a precisely estimated zero. In older data, the original Maimonides Rule is unrelated to socioeconomic characteristics, while in more recent data, the original rule is unrelated to socioeconomic characteristics conditional on a few controls. Enrollment manipulation therefore appears to be innocuous: neither the original negative effects nor the recent data zeros seem likely to be manipulation artifacts.

Joshua D. Angrist
Department of Economics, E52-436
MIT
77 Massachusetts Avenue
Cambridge, MA 02139
and IZA
and also NBER
angrist@mit.edu

Victor Lavy
Department of Economics
University of Warwick
Coventry, CV4 7AL
United Kingdom
and Hebrew University of Jerusalem
and also NBER
v.lavy@warwick.ac.uk

Jetson Leder-Luis
Department of Economics
MIT
77 Massachusetts Avenue
Cambridge, MA 02139
jetson@mit.edu

Adi Shany
Department of Economics
University of California, San Diego
ashanyrotem@ucsd.edu

1 Introduction

The Maimonides Rule research design for estimation of class size effects exploits statutory limits on class size as a source of quasi-experimental variation. As first noted by Angrist and Lavy (1999), Israeli schools face a maximum class size of 40, so that, in principle, grade cohorts of 41 are split into two classes, while slightly smaller cohorts of 39 may be taught in one large class. This produces a distinctive sawtooth pattern in average class size as a function of total grade level enrollment, a pattern seen in Israeli data on enrollment and class size as well in data from school districts around the world.

Analyzing data on class average scores for the population of Israeli 4th and 5th graders tested in June 1991, Angrist and Lavy (1999) found a substantial return to class size reductions – on the order of that found in a randomized evaluation of class size for US elementary grades (discussed by Krueger 1999). Many applications of the Maimonides Rule research design in other settings also report statistically significant learning gains in smaller classes (see, e.g, the Urquiola 2006 results for Bolivia). Other studies exploiting Maimonides Rule, however, find little evidence of achievement gains from Rule-induced class size reductions (as in the Angrist, Battistin and Vuri 2016 study of Italian schools).

This paper revisits the class size question for Israel with more recent data and a larger sample than that used in Angrist and Lavy (1999). Specifically, we look at a large elementary school sample drawn from national exams taken by Israeli 5th graders between the school year ending spring 2002 and the school year ending spring 2011. Our empirical update uncovers two clear findings. First, an econometric analysis paralleling that in Angrist and Lavy (1999) generates robust, precisely estimated zeros. Second, the new data reveal substantial sorting at Maimonides enrollment cutoffs: there are too many schools with enrollment values that produce an additional class. This juxtaposition of findings raise the possibility that the lack of an enrollment effect since 2002 is an artifact of systematic enrollment manipulation.

Our investigation of enrollment patterns suggests a simple explanation for enrollment manipulation, and allows a straightforward remedy. A recent memo from Israeli Ministry of Education (MOE) officials to school leaders admonishes headmasters against attempts to increase staffing ratios through enrollment manipulation. In particular, schools are warned not to move students between grades or to enroll those who are overseas so as to produce an additional class. This reflects Ministry concerns that school staff adjust enrollment (or enrollment statistics) when these values are close to cutoffs so as to produce smaller classes (e.g., by driving enrollment in first grade from 40 to 41, and thereby opening a second first-grade class). School leaders might care to do this because educators and parents prefer smaller classes. The tendency to favor smaller classes is also accentuated by MOE rules that set school budgets as an increasing function of the number of classes.

School leaders appear to manipulate enrollment by flexibly applying age-at-entry rules and moving students between grades. We therefore resolve the problem of enrollment manipulation by constructing an alternative version of Maimonides Rule that is largely unaffected by manipulation. The alternative rule pools data on students in 4th-6th grade and uses information on their birthdays to construct a new enrollment variable, which we refer to as "birthday-based imputed enrollment," or just "imputed enrollment." This variable is computed by applying the official (Chanukah-based) birthday cutoff for 5th grade enrollment to a sample that includes all students in 4th-6th grade with birth dates that make them eligible for 5th grade. Imputed enrollment also generates a strong first stage for class size, but with no evidence of sorting around *birthday-based* Maimonides cutoffs. Importantly, class size effects estimated using the statutory rule are also small, precisely estimated, and not significantly different from zero. Consistent with the absence of evidence of imputed enrollment manipulation, Maimonides Rule constructed from imputed enrollment is unrelated to socioeconomic status.

Finally, we return to the 1991 data analyzed by Angrist and Lavy (1999). As first noted by Otsu, Xu and Matsushita (2013), these data show evidence of sorting around the first Maimonides cutoff.[1] As in the more recent data, however, enrollment sorting in the original Maimonides sample does not appear to be highly consequential for class size effects. In particular, we show that the original formulation of the rule (constructed using November enrollment) is unrelated to students' socioeconomic status. More recent data generate small estimated effects of Maimonides Rule on socioeconomic status, but these effects disappear (while becoming more precise) when estimated with a few school-level controls.

The birthday-based imputation used to eliminate sorting in recent data cannot be applied

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[1] Figure 2 in Otsu, Xu and Matsushita (2013) appears to exaggerate this; we report corrected estimates of the 1991 sorting pattern, below.

in the older data because birthdays and individual test scores are unavailable for the earlier period. But other simple corrections, such as "donut" estimation strategy that discards observations near the first cutoff, leave the original results substantively unchanged.[2] The discrepancy between the old and new class size effects therefore seems more likely to be due to a change in the Israeli education production function rather than a sorting artifact. In light of the 2002-2011 results, the evidence for a large, externally valid class size effect in Angrist and Lavy (1999) also seems weaker in hindsight.

It also now seems more relevant that estimates for a 1992 sample of 3rd graders reported in Angrist and Lavy (1999) likewise show no evidence of achievement gains in smaller classes. The 1999 paper argued that the tests given in 1992 may have been compromised. In particular, the score distribution in 1992 appears to reflect a nationwide test preparation effort focused on weaker performers in 1991. This effort, which appears to have continued in later years to one degree or another, may have reduced the information the test scores contain about learning and causal effects. On the other hand, though the test preparation effort has varied in intensity and mostly focused on weaker schools, our results are consistent in showing no causal effects of class size over time and when interacted with school SES. Finally, use of a more modern cluster adjustment in place of the parametric Moulton correction for clustering used in the original Maimonides Rule study also increases the uncertainty associated with the original estimates.

The next section reviews institutional background on the Israeli school system. We then document the Maimonides first stage in our more recent sample, explain how our birthday-based Maimonides instrument is constructed, and show that birthday-based imputation shows no evidence of running variable manipulation. Section 4 reports two-stage least squares (2SLS) estimates using the two alternative Maimonides' instruments and Section 5 looks again at the 1991 and 1992 samples. We conclude by discussing possible explanations for changes in class size effects.

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[2] Barreca et al. (2011) appears to be the first to use the donut strategy to examine the consequences of sorting near regression discontinuity cutoffs.

2 Background and Context

Israeli Schools

Schooling in Israel is compulsory beginning in first grade, starting around age 6. Israeli students attend neighborhood schools, which serve catchment areas determined by a student's home address. Israel runs three public school systems: Jewish secular, Jewish public religious, and Arab (roughly 20% of Israel's population is Arab). There is also a smaller publicly funded but essentially independent ultra-orthodox system. Our analysis focus on secular and religious students in Jewish public schools, the group that constitutes the bulk of public school enrollment.

Public schools are administered by local authorities, but funded centrally by the MOE. Maimonides Rule, which caps class sizes at 40, has determined official practice for purposes of class assignment and school budgeting since 1969. The rule is well-known among school administrators and teachers. Other than through switching sectors, unappealing to most families, most parents have few options by way of school choice other than to move. We therefore expect any manipulation of enrollment to reflect the behavior of educators and school administrators rather than parents.

Related Work

Maimonides-style empirical strategies have been used to identify class size effects in many countries, including the US (Hoxby 2000), France (Piketty 2004 and Gary-Bobo and Mahjoub 2006), Norway (Bonesronning 2003 and Leuven, Oosterbeek and Ronning 2008), Bolivia (Urquiola 2006), and the Netherlands (Dobbelsteen, Levin and Oosterbeek 2002). On balance, these results point to modest returns to class size reductions, though mostly smaller than those reported by Angrist and Lavy (1999) for Israel. A natural explanation for this difference in findings is the relatively large size of Israeli elementary school classes. In line with this view, Woessmann (2005) finds a weak association between class size and achievement in a cross-country panel covering Western European school systems in which classes tend to be small. Recently published regression estimates for Israeli using 2006 and 2009 data show no evidence of a class size effect; this study documents the vigorous debate over class size in

Israel and policies meant to bring about a reduction (Shafrir, Shavit and Blank, 2016).[3]

A number of studies look at data manipulation and how this might compromise attempts to estimate causal class size effects. Urquiola and Verhoogen (2009) uncover evidence of sorting around Maimonides cutoffs in a sample from Chilean private schools. Angrist, Battistin and Vuri (2016) show that estimates from Maimonides style experiments in southern Italy probably reflect increased manipulation of test scores by teachers in small classes. As noted above, Otsu, Xu, and Matsushita (2013) report evidence of sorting around the first Maimonides cutoff in the Angrist and Lavy (1999) sample; we return to this finding below. In related literature, Jacob and Levitt (2003) document manipulation of test scores in Chicago public schools.

Methodological investigations of sorting in a regression discontinuity (RD) running variable originate with McCrary (2008), who introduced the statistical test for sorting used here. Barreca, Lindo and Waddell (2016) show that manipulation and nonrandom heaping of the running variable can bias regression discontinuity estimates. Barreca et al. (2011) explore manipulation of the birthweight data used by Almond et al. (2010) to identify the causal effects of neonatal health care. Gerard, Rokkanen and Rothe (2016) derive bounds on causal effects estimated using regression discontinuity designs that are built on running variables which have been compromised by sorting.

3 Data and First Stage

Data and Descriptive Statistics

The test scores used in this study come from a national testing program known as Growth and Effectiveness Measures for Schools, or GEMS. Starting in 2002, fifth graders in half of Israeli schools have been sampled for participation in GEMS.[4] Tests are given in math, native language skills (Hebrew or Arabic), science and English. We standardized GEMS test scores to have zero mean and unit variance in each subject and year. The appendix describes the raw GEMS data and our standardization process further.

Data on test scores were matched to administrative information describing schools, classes,

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[3] Results in Sims (2008) suggest class size reductions obtained through combination classes have a negative effect on students' achievement.

[4] GEMS also tests eighth graders.

and students. The unit of observation for most of our statistical analyses is the student. School records include information on the enrollment figures reported by headmasters to the ministry of education each November. This enrollment variable, henceforth called "November enrollment", is used by the MOE to determine school budgets. We also have data on class size collected at the end of the school year, in June. We refer to the end-of-year class size variable as "June class size". Individual student characteristics in the file include gender, parents' education, number of siblings, and ethnicity. Schools in the GEMS samples are identified as secular or religious. Each school is also associated with an index of socioeconomic status (SES index).[5]

Our statistical analysis looks at fifth grade pupils in the Jewish public school system, including both secular and religious schools. Our analysis excludes students in the special education system, since these students do not take GEMS tests. Special education students attend regular schools but mostly receive instruction in separate classes.

Our analysis covers data from 2002 through 2011. We start with 2002 since this was the first year of the GEMS tests. In 2012, the MOE began implementing a national plan to reduce class size, rendering Maimonides' Rule less relevant (Vurgan 2011). We focus on math and (Hebrew) language; results for other subjects are reported in Appendix Tables A1 and A2.

The matched analysis file includes 243,213 fifth grade students from 8,944 classes. The data structure is a repeated cross-section; the sample of GEMS schools changes from year to year. As can be seen in Table 1, which reports descriptive statistics for classes, students, and schools in the estimation sample, the average elementary school class in our data has about 28 pupils, and there are roughly 59 pupils per grade with roughly two classes on average. Ten percent of classes have more than 35 pupils, and 10 percent have fewer than 21 pupils. Median class size is also 28. Demographic data show that 89 percent of students are Israeli-born. Many in the sample are the children of immigrants; 16 percent are the children of immigrants from the former Soviet Union, for example (this variable is labeled "former USSR ethnicity"). Although our statistical analyses use standardized scores, Panel C shows the

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[5] The school SES index is an average of the index for its students. Student SES is a weighted average of values assigned to parents' schooling and income, economic status, immigrant status and former nationality, and the school's location (urban or peripheral). The index ranges from 1-10, with 1 representing the highest socioeconomic level. Schools with more disadvantaged students (high SES index) receive more funding per student. We observe only the school average SES.

distribution of raw scores.

The Maimonides First Stage

Maimonides' Rule reflects MOE regulations requiring that classes be split when they reach the statutory maximum of 40. Strict application of the rule produces class sizes that are a non-linear and discontinuous function of enrollment. Writing f_{jt} for the predicted 5th grade class size at school j in year t, we can write rule-based enrollment as

$$f_{jt} = \frac{r_{jt}}{\left[int\left(\left(r_{jt}-1\right)/40\right)+1\right]}, \tag{1}$$

where r_{jt} is November grade enrollment at school j in year t, and $int(x)$ is the largest integer less than or equal to x.

Figure 1 plots actual average June class size and rule-based predictions, f_{jt}, against November enrollment. Plotted points show the average June class size at each level of enrollment. The fit here looks similar to that reported using 1991 data in Angrist and Lavy (1999). Maimonides Rule fits actual class size better for enrollments below 100 that at larger values, a pattern that reflects more deviations from the rule at large values and few schools with such large enrollments. Predicted discontinuities in the class size/enrollment relationship are also rounded by the fact that many classes are split before reaching the theoretical maximum of 40.

The first-stage effect of f_{jt} on class size is estimated by fitting

$$s_{ijt} = \pi f_{jt} + \rho_1 r_{jt} + \delta_1 X_{ijt} + \gamma_t + \varepsilon_{ijt} \tag{2}$$

where s_{ijt} is the June class size experienced by student i enrolled in school j and year t; X_{ijt} is a time-varying vector of student and school characteristics, f_{jt} is as defined above, and ε_{ijt} is a regression error term. The student characteristics in this model include a gender dummy, both parents' years of schooling, number of siblings, a born-in-Israel indicator and ethnic-origin indicators. School characteristics include an indicator for religious schools, the school SES index, and interactions of the SES index with year dummies.[6] We also include year fixed effects (γ_t) and control for alternative functions of the running variable, r_{jt}, as

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[6] The interactions of the SES index with year dummies control for changes in the weights and the components of the index implemented in 2004 and 2008.

described below.

Estimates of π in Equation (2) are remarkably stable at around .62. This can be seen in Table 2, which reports first stage estimates using a variety of running variable controls, including linear and quadratic functions of enrollment and the piecewise linear trend used by Angrist and Lavy (1999). This trend function picks up the slope on the linear segments of the rule. Specifically, the trend is defined on the interval [0,200] as follows:

$$\begin{array}{rl} r_{jt} & r_{jt} \in [0,40] \\ 20 + r_{jt}/2 & r_{jt} \in [41,80] \\ 100/3 + r_{jt}/3 & r_{jt} \in [81,120] \\ 130/3 + r_{jt}/4 & r_{jt} \in [121,160] \\ 154/3 + r_{jt}/5 & r_{jt} \in [161,200] \end{array}$$

The constants here join the Maimonides linear segments at the cutoffs.[7]

Sorting Out Enrollment Sorting

The budget for Israeli primary schools comes from local municipal authorities and the national MOE. The local authority funds administrative costs, while the MOE funds teaching and other educational activities. The MOE's budget for instruction time is based on the predicted number of classes determined by the November enrollment figures reported to the MOE (Ministry of Education 2015*a*). This generates an incentive to manipulate enrollment, either directly by moving students between grades or through false reporting.[8]

As first noted by McCrary (2008), running variable manipulation should be evident in the running variable distribution. Figure 2 shows the histogram of November enrollment in our 2002-11 sample, tabulated using a 1-student bin size. Vertical lines indicate Maimonides cutoffs. The figure shows a clear spike in enrollment just to the right of the cutoffs at 40 and 80, with apparent holes in the distribution to the left.

The forces producing these spikes are hinted at in MOE memoranda on enrollment reporting distributed at the end of the school year. These memoranda remind headmasters of

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[7] For example, $a_3 + \frac{80}{3} = a_2 + \frac{80}{2}$ implies $a_3 = \frac{100}{3}$.

[8] Funding rules for 2004-7 were revised so as to make total enrollment the major funding determinant rather than the number of classes. This reform was never fully implemented. In 2007, the MOE returned to the class-based funding rule (see Lavy 2012 and Vurgan 2007 for details).

the need for appropriate enrollment reporting for funding determination the following year. The 2015 circular cautioned headmasters against enrollment manipulation. In particular, schools were warned not to move students between grades, to enroll a student in more than one school, or to enroll students residing overseas so as to produce an additional class. Since 2016, the MOE has been auditing enrollment data in an effort to prevent this type of manipulation, though it's not clear how or whether schools submitting bad data will be sanctioned (Ministry of Education, 2015*b*).

Interestingly, Figure 2 offers further evidence of financially-motivated enrollment manipulation in the spike at a class size of 20. While budgetary rules set funding as a function of the number of classes, classes with enrollments below 20 are generated allotted half as much funding as any larger class.

Although the incentive for headmasters to artificially push enrollment across Maimonides cutoffs seems clear, the question of whether this produces misreporting or actual movement between grades is less easily addressed. Real enrollment changes can be accomplished by skipping students a grade ahead or through grade retention at any grade. A further especially likely channel is flexible age at entry in first grade. Although the official start age policy specifies a Chanukah-based birthday cutoff (detailed below), in practice, school headmasters have some discretion as to when children may start school.

Appendix Figure A1 suggests that at least some of the enrollment changes resulting from manipulation are real and persistent, rather than purely on paper. This figure reports the histogram of the number of 5th graders present for the GEMS tests in our sample. The evidence here is strongest for bunching around the first Maimonides cutoff, with somewhat weaker evidence of missing mass to the left of 80. Missing data in the number tested for values below the second cutoff might be explained by the fact that roughly 10% of students enrolled miss the test.

Our primary concern is with possible selection bias resulting from enrollment manipulation, rather than with the nature of the manipulation. We might expect, for example, that more sophisticated school leaders understand the value of moving enrollment from just below to just beyond Maimonides cutoffs. And sophisticated school leaders may also teach higher-SES students, on average, producing a spurious achievement increase at the point where rule-based predicted class size drops.

We mitigate selection bias from enrollment manipulation by constructing a version of Maimonides Rule from birthday-based imputed enrollment. Most manipulation appears to result from single-grade retention or advancement relative to birthday-based enrollment, either as a result of delayed or accelerated school entry or a shift since first grade. Data on a sample of 4th, 5th, and 6th graders therefore includes all or almost students who should be in 5th grade. In particular, Israel's compulsory attendance laws specify rules for student enrollment in first grade according to whether a child's 6th Hebrew birthday falls before or after the last day of Chanukah (the 8-day Jewish holiday that typically comes in December). Students born after the last day of Chanukah are too young for first grade and must wait an additional year to start school.

Figure 3 extrapolates Chanukah-based school entry into fifth-grade, showing the grade enrollment determined by application of this rule to birth dates 11 years earlier. For example, students born between December 18, 1990 (the last day of Chanukah in the Fall of 1990) and December 8, 1991 (the last day of Chanukah in the Fall of 1991) should have been enrolled in first grade in the school year ending Spring 1998 and, assuming no grade repetition or skipping, been seen in 5th grade in the school year ending Spring 2002. Students born after December 8, 1991 should have had to wait for first grade until the school year ending in 1999 and therefore been in 5th grade one year later, in the school year ending Spring 2003.

Applying the Chanukah-based birthday rule to June enrollment data for the sample of 4th-6th graders in each school, we construct an imputed enrollment variable for 5th graders that is unlikely to reflect manipulation by school officials. The birthday imputation uses enrollment data for all students seen in June in the relevant grades in sampled schools, not just the 5th graders who took GEMS tests.[9] Specifically, imputed enrollment is computed using data on enrollment and birth dates for all 4th-6th graders in each school in the same year we observe that school's 5th graders taking GEMS tests. Figure 4, which plots the imputed enrollment histogram, suggests that enrollment imputed in this manner is indeed unmanipulated. The figure shows a reasonably smooth distribution, with no evidence of spikes to the right of Maimonides cutoffs. Likewise, the imputed enrollment distribution shows no evidence of a spike at 20.

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[9] The extrapolation uses June enrollment data as only these are available at the student level, showing individual students' dates of birth.

The McCrary (2008) style density plots in Figure 5 are also consistent with the view that imputed birthday-based rule eliminates sorting in the November enrollment data. The upper panel of the figure plots empirical and fitted densities for November enrollment, allowing for a discontinuity at the the first and the second Maimonides cutoffs. Here, the jumps at 41 and 81 seem clear enough. By contrast, Panel B, which shows the same sort of plot for imputed enrollment, suggests the imputed enrollment distribution is smooth through these cutoffs.[10]

First stage estimates computed using imputed enrollment instruments are a little over half the size of those constructed using November enrollment. This can be seen in Table 3, which reports estimates of the first stage regression on Maimonides rule using the birthday-based enrollment figures. As when estimating November data, key first stage parameters are precisely estimated and largely insensitive to the nature of the running variable control.

4 Class Size Effects: 2002-2011

Our two-stage least squares (2SLS) framework models y_{ijt}, the standardized GEMS score of student i enrolled in 5th grade at school j in year t, as a function of 5th grade class size, running variable controls, year effects (μ_t), and additional controls, X_{ijt}. Second-stage models with a linear running variable control can be written:

$$y_{ijt} = \beta s_{ijt} + \rho_2 r_{jt} + \delta_2 X_{ijt} + \mu_t + \eta_{ijt}, \tag{3}$$

where β is the causal effect of interest and η_{ijt} is the random part of potential achievement. The first stage for 2SLS estimation of equation (3) is equation (2).

2SLS estimates of β in equation (3) suggest class size has no causal effect on achievement. Estimates of effects on language, reported in cols 2-4 of Table 4, are zero to 3 digits (with an estimated standard error a little under 0.002). The corresponding 2SLS estimates of class size effects on math scores, reported in columns 6-8, are are also remarkably close to zero. Interestingly, OLS estimates of a version of equation (3), reported in columns 1 and 5 on the table, are also small, though positive and marginally significant.

It seems fair to say that the estimated education production function identified by Mai-

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[10] These plots were constructed using DCdensity (`http://eml.berkeley.edu//~jmccrary/DCdensity/`), which generates a graph of estimated densities with standard error bands, allowing for a single discontinuity, as described in McCrary (2008). Dots in the figure are histograms in an one-unit binwidth.

monides Rule in more recent data differs from the estimated using similar specifications for 1991. The 1991 results are replicated in Table 5, with the modification that the replication reports standard errors computed using a cluster adjustment rather than the Moulton formula used by Angrist and Lavy (1999).[11] In contrast with the small effects found for 2002-2011, use of Maimonides Rule instruments in the 1991 sample with linear running variable controls generates an estimated effect of -.277 for 5th grade language (with a standard error of .076) and an estimated effect of -.231 for 5th grade math (with a standard error of -.099).[12] The estimates for 4th graders are smaller and only that for language with linear enrollment controls is (marginally) significantly different from zero.

Perhaps the new findings showing zero class size effects are an artifact of running variable manipulation. This question is explored in Table 6, which reports a set of 2SLS estimates paralleling those in 4, but computed in this case using a version of Maimonides rule derived from birthday-based imputed enrollment. Like Table 4, the results here show little evidence of achievement gains in smaller classes. In the 2002-2011 data, therefore, the lack of a class size effect appears unrelated to school leaders' efforts to open an additional class by pushing enrollment across Maimonides Rule cutoffs.

In an investigation of possible treatment effect heterogeneity that might be hiding behind an overall zero effect on average, we also estimated models where the effect of class size on test scores is interacted with SES index. The instruments in this case are f_{jt}, and $f_{jt} * SES_{jt}$, where SES_j is the SES index for school j at year t. These results likewise show no evidence of class size effects or SES interactions. An analysis of class size effects by year also generates null effects. This weighs against the hypothesis that the absence of a class size effect reflects extensive test preparation, since the extent of preparation varied from year to year.

Gerard, Rokkanen and Rothe (2016) note that sorting around RD cutoffs is innocuous when manipulated units are similar to those unaffected by sorting. To check for possible discontinuities in school characteristics induced by sorting, we regressed the school-by-year SES index (increasing from 1 to 10 as SES declines) on Maimonides rule in a version of equation (2) fit to school-year averages. As can be seen in Panel A of Table 7, when Maimonides Rule is constructed from November enrollment data, schools with larger predicted class size have

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[11] Standard errors are clustered on school.

[12] As in Angrist and Lavy (1999), 1991 test scores are measured as a composite percentile, ranging from 0-100, with means around 70 and standard deviations around 8-10.

somewhat higher SES. For example, the estimates in column 2 suggest that a 10 student increase in predicted class size is associated with a reduced disadvantaged index (that is, higher SES) of about 0.2. That seems like a modest relationship, amounting to less than one-tenth of a standard deviation of the index. Importantly, the estimates in columns 4-6 of Table 7 show that this relationship disappears when Maimonides Rule is constructed using birthday-based imputed enrollment.

Although encouraging for the thesis that imputed enrollment is uncompromised by sorting, the results in Panel A of Table 7 suggest we should worry about non-random enrollment manipulation when working with November data. But Panel B of the table shows that the association between November-based Maimonides Rule and SES disappears in models that control for a pair of school average covariates (fathers' schooling and family size), while these zeros are estimated at comparable levels of precision. Moreover, Maimonides Rule computed using imputed enrollment is unrelated to SES with or without these controls. Since the findings on class size are consistent using both sources of enrollment data and when estimated with and without covariates, it seems unlikely that non-random sorting across Maimonides cutoffs in the November enrollment data is an important source of bias.[13]

5 Earlier Estimates Explored

The evidence of running variable manipulation apparent in 2002-2011 data naturally raises questions about manipulation in the older data used to compute the estimates in Angrist and Lavy (1999). Figure 6 plots estimated enrollment histograms and densities for the Angrist and Lavy samples of 4th and 5th graders tested in 1991. This figure shows evidence of a gap in the enrollment distribution below the first Maimonides cutoff of 41. The figure also reports estimates of the associated densities, allowing for a discontinuity at 41. Here too, we see evidence of a jump.[14] Appendix Figure A2 presents the enrollment histogram for the sample of 3rd graders tested in 1992; this figure shows a somewhat more modest enrollment jump to the right of the first cutoff.[15]

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[13] 2SLS estimates of class size effects from models without covariates other than running variable controls are, like those in reported those in Tables 4 and 6, insignificant and zero to two decimal places.

[14] The discontinuity at 81 (the split from 2 to 3 classes) in the 1991 data is not statistically significant

[15] The discontinuity at 41 in the 1992 data is statistically significant; the discontinuity at 81 is not.

Otsu, Xu and Matsushita (2013) includes figures similar to our Figure 6. These earlier plots, however, appear to count the 1991 enrollment distribution in terms of classes rather than schools. Because many grade cohorts are indeed split into additional classes at or near 40, the number of classes in schools with enrollments just above 40 jumps with or without sorting. The Otsu, Xu and Matsushita (2013) discontinuity check therefore confounds the density discontinuity induced by sorting with the causal effect of Maimonides Rule on the number of classes. This concern notwithstanding, however, Figure 6 shows evidence of sorting around the first Maimonides cutoff in 1991.

Additional analyses of the older data (not shown here) suggest sorting is less pervasive in 1991 and 1992 data, with little evidence of enrollment discontinuities beyond the first Maimonides cutoff. Even so, in view of the discontinuity in the 1991 enrollment distribution seen in Figure 6, it's worth asking whether enrollment manipulation is likely to be a source of omitted variables bias. Table 8 reports estimates from a regression of school-level SES on Maimonides Rule using 1991 data, similar to the estimates reported in Table 7. As in the more recent data (with covariates), we see little evidence of a relationship between Maimonides Rule and school-level SES. The negative associations estimated for 5th graders are not significantly different from zero, while the sign flips to (insignificant) positive for 4th and 3rd graders.[16]

The additional enrollment data required for a birthday-based imputation of 1991 enrollment are unavailable. We turn therefore to an alternative check on the replicated results that omits observations near the first Maimonides cutoff.[17] The results of this further exploration of the consequences of sorting in 1991 data are reported in Table 9. For example, the estimated class size effect of -0.234 in column 1 of Table 9 was computed using a sample omitting schools with 5th grade enrollments between 39 and 41. This can be compared with the full-sample estimate of $-.277$. Although somewhat less precise, the donut estimates in Table 9 differ little from those for the full sample estimates reported in Table 5.

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[16] The 1991 SES index is scaled as "percent disadvantaged."

[17] Barecca et al. (2011) appear to be the first to propose this simple adjustment for sorting, sometimes referred to as an RD "donut".

6 Summary and Conclusion

Application of the Maimonides Rule identification strategy for class size effects produces precisely estimated zeros in large Israeli samples pooling test scores from 2002-2011. These samples also show clear evidence of manipulation of November enrollment around Maimonides class size cutoffs, likely reflecting the desire of school leaders to open an additional class when enrollment is close to a cutoff. But enrollment imputed using all students with grade-eligible birthdates looks to be unaffected by manipulation, while 2SLS estimation based on imputed enrollment produces similarly small class size effects. Maimonides Rule constructed using birthday-based imputed enrollment is also unrelated to a school-level measure of SES. This constellation of findings suggests the more recent results are unaffected by enrollment manipulation.

We find only weak evidence of *systematic* enrollment sorting: more recent data generate small estimated effects of the November-based Rule on socioeconomic status, but these effects disappear after conditioning on a few covariates. Since the November estimates of class size effects are also estimated to be zero with or without controls, these results reinforce our conclusion that the finding of a null class size effect in recent data is not a manipulation artifact. The estimates of zero class size effect in more recent data contrast with the substantial negative class size effects reported byAngrist and Lavy (1999). We also see some evidence of manipulation around the first Maimonides cutoff in the older data analyzed byAngrist and Lavy (1999). But the absence of a relationship between Maimonides' Rule and school average SES, and results from a donut strategy that omits data near the cutoff, suggest these estimates too are unaffected by enrollment manipulation near cutoffs.

The contrast between the old and new class size effects may be due to a change in the Israeli education production function. The fact that Israeli class size has fallen (from a median of 31 in 1991 to 28 in the more recent sample) may have contributed to this. At the same time, given that the 1991 results are not very precise, the earlier findings may also have been a chance finding. The original estimates are strongest for 5th graders, but less impressive for 4th graders, for whom only estimates for language are significantly different from zero, and only in one specification. The original Angrist and Lavy study also failed to find class effects in a sample of 3rd graders from 1992, a result attributed in the original write-up to (documented) extensive test preparation and changes in testing protocols. These forces may still be at work, especially in weaker schools. But our analysis uncovers no evidence of significant class size/SES interactions or changes in class size effects over time that might be linked to changes in preparation. From today's vantage point, it seems fair to say that the 1991 results are unusual in showing strong class size effects, while the null effects in 1992 have emerged as more representative of the causal relationship between class size and learning in Israel.

Table 1: Descriptive Statistics (2002-2011)

			Quantiles				
Variable	**Mean**	**S.D.**	**0.10**	**0.25**	**0.50**	**0.75**	**0.90**
Panel A. Class Level Data							
June class size	28.0	5.88	21	24	28	32	35
Number Tested in Language	25.7	6.29	18	22	26	30	33
Number Tested in Math	25.5	6.59	18	22	26	30	33
Number of classes				8,944			
Panel B. School Level Data							
June enrollment	58.0	27.2	25	39	54	76	95
November enrollment	58.8	27.4	25	41	55	77	96
Birthday-based enrollement	63.6	28.7	29	42	60	82	103
SES index	5.20	2.45	2.05	3.13	5	7	9
Number of 5th grade classes	2.07	0.78	1	2	2	3	3
Religious school	0.31	0.47	0	0	0	1	1
Number of schools				4,322			
Panel C. Student Level Data							
Language score [N=227,849]	72.0	17.4	48.3	63.3	75.5	84.6	91.0
Math score [N=229,491]	68.1	20.6	37.9	55.3	72.3	84.4	91.5
Father's years of education	11.7	5.04	0	11	12	15	17
Mother's years of education	12.1	4.68	6	12	12	15	17
Number of siblings	1.75	1.24	0	1	2	2	3
Boy	0.50	0.50	0	0	1	1	1
Native	0.89	0.31	0	1	1	1	1
Israeli ethnicity	0.58	0.49	0	0	1	1	1
Ethiopian ethnicity	0.03	0.18	0	0	0	0	0
Former USSR ethnicity	0.16	0.36	0	0	0	0	1
Asia-Africa ethnicity	0.12	0.32	0	0	0	0	1
Europe-America ethnicity	0.11	0.31	0	0	0	0	1
Number of students				243,213			

Notes: This table reports descriptive statistics for a representative sample of the population of Israeli 5th graders and their schools and classes in 2002-2011. The sample includes all fifth grade students in Jewish state elementary schools who participated in the GEMS tests in math and/or language in 2002-2011. Means and standard deviations for class level data are computed using one observation per class. Means and standard deviations for school level data are computed using one observation per school. Means and standard deviations for student level data are computed using one observation per student.

Table 2: First Stage Estimates Using November Enrollment Instruments (2002-2011)

	Language			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
f_{jt}	0.627***	0.614***	0.620***	0.626***	0.613***	0.619***
	(0.0185)	(0.0188)	(0.0184)	(0.0185)	(0.0188)	(0.0184)
November enrollment	0.028***	0.073***		0.027***	0.073***	
	(0.0031)	(0.0093)		(0.0030)	(0.0092)	
Enrollment squared/100		-0.028***			-0.028***	
		(0.0055)			(0.0055)	
Piecewise linear trend			0.0619***			0.0624***
			(0.0061)			(0.0061)
R^2	0.522	0.526	0.524	0.523	0.526	0.524
F_{st}	1154.0	1068.7	1131.0	1148.4	1064.5	1127.0
N		227,849			229,491	

Notes: Standard errors reported in parentheses are clustered at the school and year level. This table reports estimates of equation (2) in the text. The dependent variable is June class size. Covariates include student characteristics (a gender dummy, both parents' years of schooling, number of siblings, a born-in-Israel indicator, ethnic-origin indicators), year fixed effects, indicator for religious school, socioeconomic index and interactions of the socioeconomic index with year dummies. Maimonides Rule f_{jt} is computed using November enrollment. The sample includes all fifth grade students in Jewish state elementary schools who participated in the GEMS tests in math and/or language. The sample covers the years 2002 through 2011.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: First Stage Estimates Using Birthday-based Imputed Enrollment (2002-2011)

	Language			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
f_{jt}	0.330***	0.308***	0.310***	0.330***	0.307***	0.309***
	(0.0177)	(0.0180)	(0.0179)	(0.0176)	(0.0179)	(0.0178)
Birthday-based enrollment	0.055***	0.136***		0.055***	0.135***	
	(0.0030)	(0.0110)		(0.0030)	(0.0110)	
Enrollment squared/100		-0.046***			-0.045***	
		(0.0063)			(0.0062)	
Piecewise linear trend			0.139***			0.140***
			(0.0065)			(0.0065)
R^2	0.349	0.359	0.360	0.351	0.361	0.361
F_{st}	348.7	291.1	298.9	350.8	292.8	300.7
N		227,849			229,491	

Notes: This table reports estimates of equation (2) in the text. The dependent variable is June class size. Standard errors reported in parentheses are clustered at the school and year level. Covariates include student characteristics (a gender dummy, both parents' years of schooling, number of siblings, a born-in-Israel indicator, ethnic-origin indicators), year fixed effects, indicator for religious school, socioeconomic index and interactions of the socioeconomic index with year dummies. Maimonides Rule f_{jt} is computed using birthday-based enrollment. The sample includes all fifth grade students in Jewish state elementary schools who participated in the GEMS tests in math and/or language. The sample covers the years 2002 through 2011.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Class Size Effects Using November Enrollment Instruments (2002-2011)

	Language				Math			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Class size	0.0018*	-0.0002	-0.0006	-0.0006	0.0022*	0.0018	0.0012	0.0011
	(0.0010)	(0.0018)	(0.0019)	(0.0019)	(0.0011)	(0.0021)	(0.0022)	(0.0021)
November enrollment	0.00006	0.00024	0.00117		-0.00005	-0.00002	0.00113	
	(0.00021)	(0.00025)	(0.00083)		(0.00024)	(0.00029)	(0.00091)	
Enrollment squared/100			-0.00056				-0.00068	
			(0.00046)				(0.00050)	
Piecewise linear trend				0.00073				0.00033
				(0.00055)				(0.00064)
N	227,849				229,491			

Notes: This table reports OLS and 2SLS estimates of equation (3) in the text. The endogenous variable is June class size; Maimonides Rule is constructed using November enrollment. Standard errors reported in parentheses are clustered at the school and year level. The dependent variable is the student's standardized test score by year and subject with zero mean and one unit variance in each year and subject (on tests taken from 2002-2011). Covariates include student characteristics (a gender dummy, both parents' years of schooling, number of siblings, a born-in-Israel indicator, ethnic-origin indicators), year fixed effects, indicator for religious school, socioeconomic index and interactions of the socioeconomic index with year dummies.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Replication of 1991 Results

	Language			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Fifth Grade Data			
Class Size	-0.277***	-0.263***	-0.190	-0.231**	-0.264**	-0.205
	(0.0758)	(0.0937)	(0.122)	(0.0985)	(0.123)	(0.145)
November Enrollment	0.0223**	0.0131		0.0410***	0.0631*	
	(0.00912)	(0.0262)		(0.0117)	(0.0355)	
Enrollment Squared/100		0.00417			-0.0100	
		(0.00996)			(0.0138)	
Piecewise Linear Trend			0.137***			0.194***
			(0.0359)			(0.0430)
N	2019	2019	1961	2018	2018	1960
			B. Fourth Grade Data			
Class Size	-0.133**	-0.0739	-0.147*	-0.0497	-0.0328	-0.0982
	(0.0608)	(0.0683)	(0.0887)	(0.0747)	(0.0845)	(0.0990)
November Enrollment	0.00461	-0.0396*		0.0198**	0.00719	
	(0.00794)	(0.0218)		(0.00926)	(0.0274)	
Enrollment Squared/100		0.0210**			0.00601	
		(0.00947)			(0.0124)	
Piecewise Linear Trend			0.100***			0.130***
			(0.0260)			(0.0290)
N	2049	2049	2001	2049	2049	2001

Notes: This table reports 2SLS estimates using 1991 data. These results use f_{jt} as an instrument for class size. Standard errors have been updated to use the Stata cluster command, and are clustered by school. The piecewise linear control in columns (3) and (6) in each panel omit enrollments over 160, to match Angrist and Lavy 1999. Both panels include controls for the school level index of socioeconomic status.

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

Table 6: Class Size Effects Using Birthday-based Imputed Enrollment (2002-2011)

	Language				Math			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Class size	0.00169*	0.00008	-0.00053	-0.00040	0.00171	-0.00106	-0.00223	-0.00224
	(0.00101)	(0.00354)	(0.00389)	(0.00383)	(0.00115)	(0.00438)	(0.00442)	(0.00437)
Birthday-based enrollment	0.00012	0.00025	0.00100		0.00015	0.000374	0.00181	
	(0.00021)	(0.00035)	(0.00108)		(0.00023)	(0.00040)	(0.00119)	
Enrollment squared/100			-0.00040				-0.00078	
			(0.00050)				(0.00054)	
Piecewise linear trend				0.00075				0.00130
				(0.00089)				(0.00102)
N	227,849				229,491			

Notes: This table reports OLS and 2SLS estimates of equation (3) in the text. The endogenous variable is June class size; Maimonides Rule is constructed using birthday-based enrollment. Standard errors reported in parentheses are clustered at the school and year level. The dependent variable is the student's standardized test score by year with zero mean and one unit variance in each year (on tests taken from 2002-2011). Covariates include student characteristics (a gender dummy, both parents' years of schooling, number of siblings, a born-in-Israel indicator, ethnic-origin indicators), year fixed effects, indicator for religious school, socioeconomic index and interactions of the socioeconomic index with year dummies.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Maimonides Rule Effects on Socioeconomic Status (2002-2011)

	Using November Enrollment			Using Birthday-Based Enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without additional controls						
f_{jt}	-0.0223***	-0.0199**	-0.0200**	-0.0104	-0.0079	-0.0066
	(0.00824)	(0.00846)	(0.00846)	(0.00816)	(0.00833)	(0.00835)
Enrollment	-0.0218***	-0.0290***		-0.0217***	-0.0296***	
	(0.00215)	(0.00669)		(0.00202)	(0.00662)	
Enrollment squared/100		0.00504			0.00518	
		(0.00413)			(0.00391)	
Piecewise linear trend			-0.0403***			-0.0432***
			(0.00421)			(0.00420)
R^2	0.164	0.165	0.162	0.159	0.160	0.158
Panel B: With additional controls						
f_{jt}	-0.0047	-0.0050	-0.0049	0.0016	0.0013	0.0026
	(0.00727)	(0.00742)	(0.00743)	(0.00748)	(0.00751)	(0.00756)
Enrollment	-0.0152***	-0.0143**		-0.0144***	-0.0136**	
	(0.00202)	(0.00621)		(0.00194)	(0.00616)	
Enrollment squared/100		-0.00065			-0.00055	
		(0.00394)			(0.00370)	
Piecewise linear trend			-0.0269***			-0.0276***
			(0.00393)			(0.00401)
R^2	0.317	0.317	0.315	0.315	0.315	0.312
N		4,322			4,322	

Notes: This table reports OLS estimates of the effect of Maimonides Rule on a school-level index of socioeconomic status. Columns 1-3 report estimates for a Rule using November enrollment; columns 4-6 report estimates for a Rule using birthday-based enrollment. Standard errors reported in parentheses are clustered at the school level. Panel A include controls for religious school dummy and year fixed effects. Panel B also includes school averages of the following students covariates: father's years of schooling and number of siblings.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Maimonides Rule Effects on Socioeconomic Status in 1991 Data

	Fifth Grade			Fourth Grade			Third Grade		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
f_{jt}	-0.0592	-0.0686	-0.0709	0.0532	0.0636	0.0718	0.0400	0.0444	0.0581
	(0.0784)	(0.0848)	(0.0865)	(0.0810)	(0.0878)	(0.0894)	(0.0845)	(0.0906)	(0.0910)
November Enrollment	-0.0598***	-0.0475		-0.0691***	-0.0824*		-0.0940***	-0.101*	
	(0.0152)	(0.0448)		(0.0155)	(0.0461)		(0.0165)	(0.0534)	
Enrollment Squared/100		-0.00684			0.00761			0.00410	
		(0.0235)			(0.0248)			(0.0307)	
Piecewise Linear Trend			-0.101***			-0.133***			-0.180***
			(0.0350)			(0.0349)			(0.0358)
N	1002	1002	990	1013	1013	1003	989	989	982

Notes: This table reports OLS estimates of the effect of Maimonides Rule on a school-level index of socioeconomic status. The unit of analysis is the school. The third grade sample is limited to those schools which appear in the fourth and fifth grade sample, to create the religious school indicator and limit the sample to non-Arab schools. The piecewise linear control in columns (4), (8), and (12) omits enrollments above 160, to match Angrist and Lavy 1999.

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

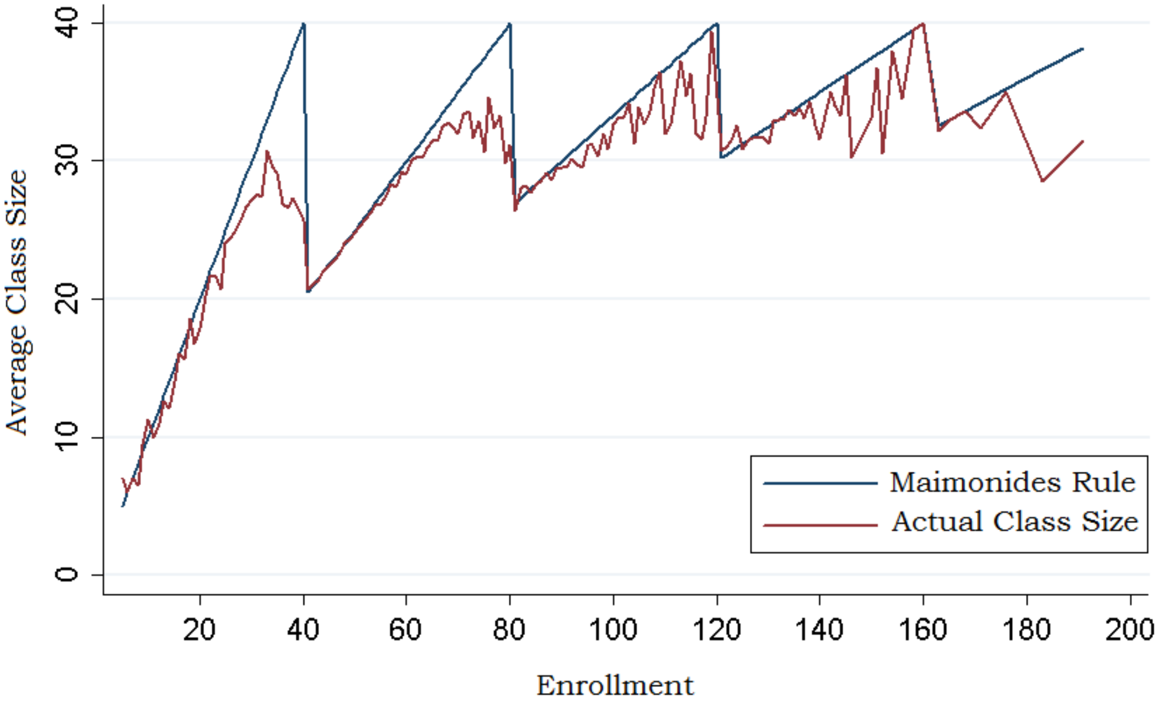
Table 9: 2SLS Donuts Using 1991 Data

	Language		Math	
	(1)	(2)	(3)	(4)
	A. Fifth Grade			
Donut:				
[39, 41]	-0.234***	-0.201**	-0.195*	-0.214
	(0.0762)	(0.0954)	(0.102)	(0.131)
[38, 42]	-0.241***	-0.207**	-0.200*	-0.221
	(0.0776)	(0.0987)	(0.104)	(0.137)
[37, 43]	-0.215***	-0.170*	-0.193*	-0.202
	(0.0777)	(0.0991)	(0.105)	(0.139)
	B. Fourth Grade			
Donut:				
[39, 41]	-0.127**	-0.0581	-0.0544	-0.0353
	(0.0612)	(0.0690)	(0.0749)	(0.0858)
[38, 42]	-0.119*	-0.0431	-0.0438	-0.0208
	(0.0632)	(0.0719)	(0.0775)	(0.0899)
[37, 43]	-0.117*	-0.0390	-0.0467	-0.0227
	(0.0649)	(0.0743)	(0.0794)	(0.0927)
Controls:				
Percent Disadvantaged	X	X	X	X
Enrollment	X	X	X	X
Enrollment Squared /100		X		X

Notes: This table reports 2SLS estimates of class size effects omitting data in the intervals indicated, using the 1991 data analyzed by Angrist and Lavy (1999). Standard errors are clustered by school.

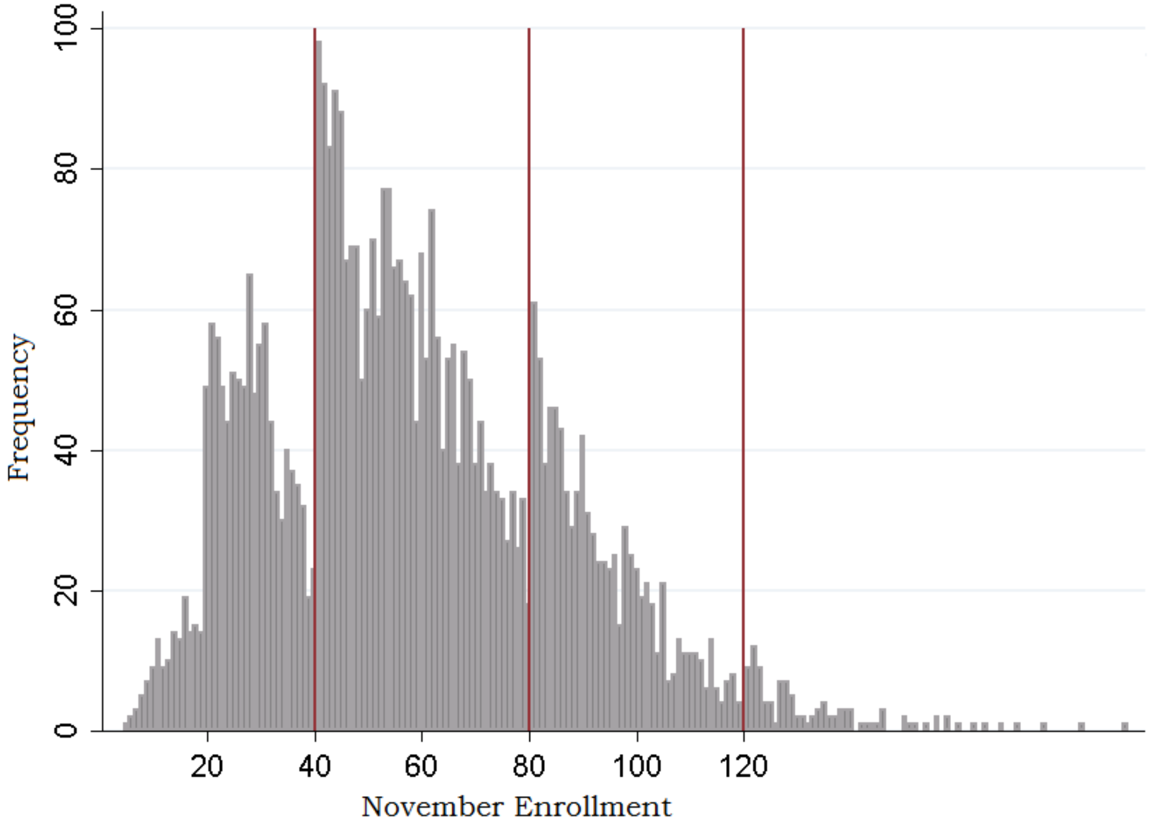
* $p < .1$, ** $p < .05$, *** $p < .01$

Figure 1: June Class Size in 2002-2011, Conditional on November Enrollment



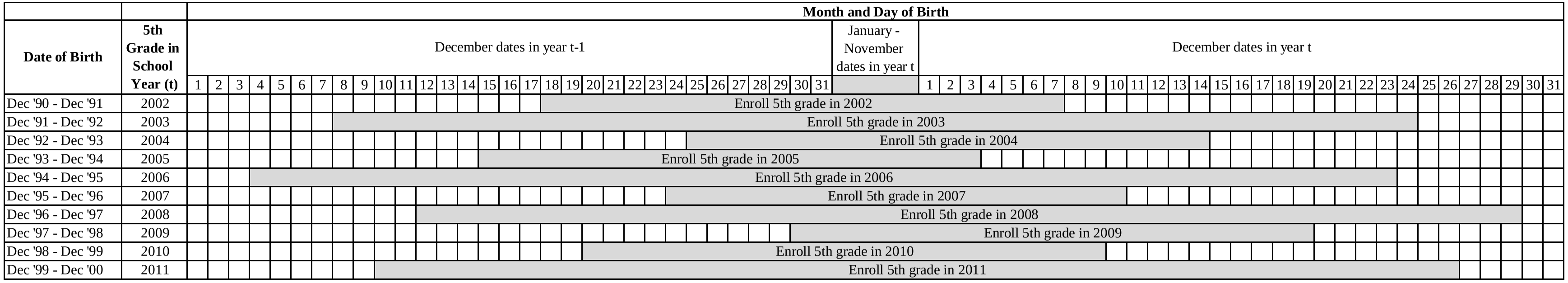
Notes: This figure plots unweighted average (June) class size and Maimonides Rule forecasts conditional on November enrollment in a sample containing averages for each school and year. The underlying data include an average of 570 schools each year between 2002-2006 and 290 schools each year between 2007-2011.

Figure 2: The 5th grade Enrollment Distribution Reported in November 2002-2011



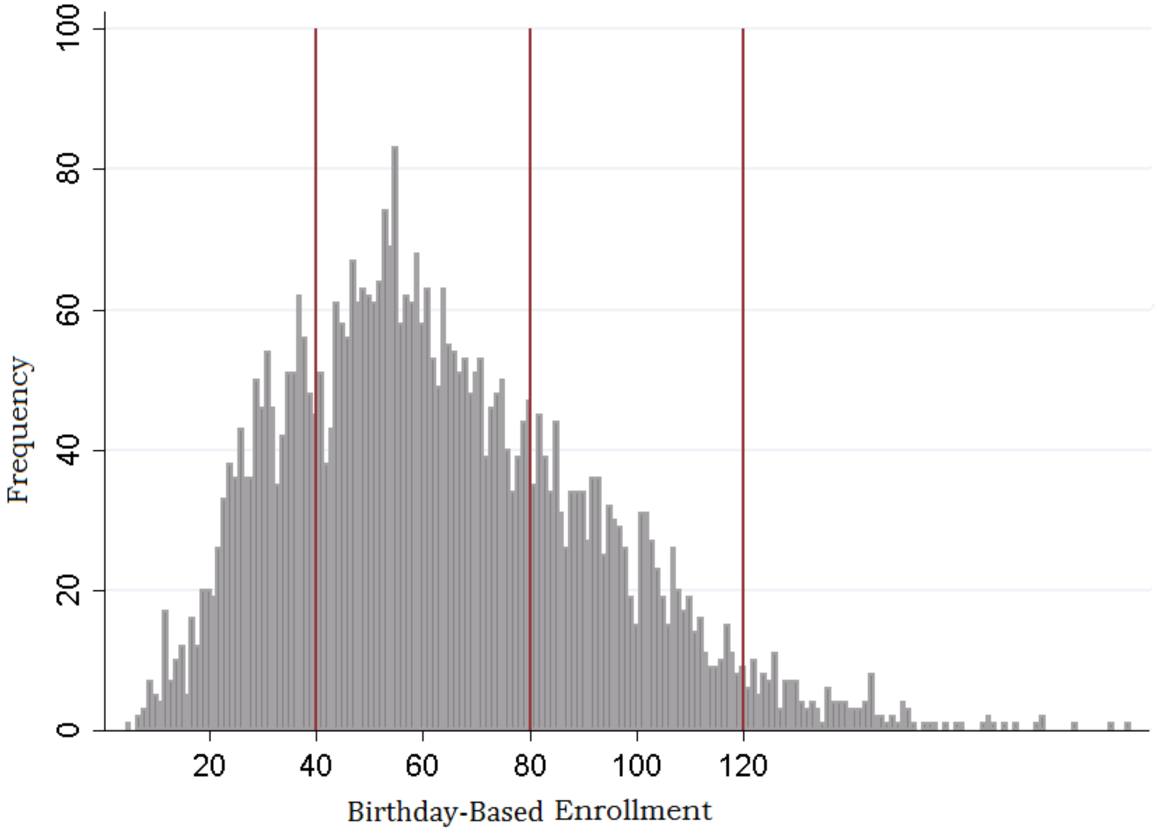
Notes: This figure plots the distribution of 5th grade enrollment values reported by school headmasters in November. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added.

Figure 3: Chanukah-Based Birthday Cutoffs for 5th Grade Enrollment



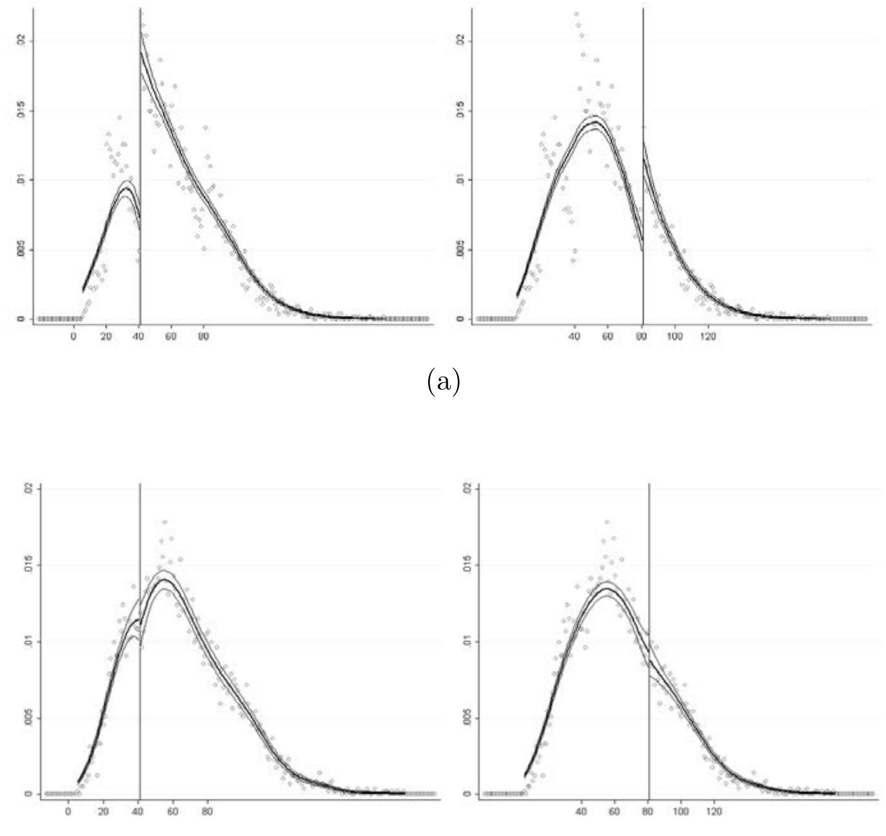
Note: This sketch shows birthday cutoffs for school entry, and their implications for 5th grade enrollment in a pure birthday-based world with no grade repetition or skipping. For example, students born between Dec 18 1990 and December 7 1991 should be enrolled in 5th grade in Spring 2002. Our predicted enrollment variables for 2002-2011 applies these rules to the birthday distribution of children observed enrolled in 4-6th grade in Spring of each year.

Figure 4: 5th Grade Birthday-based Imputed Enrollment Distribution (2002-2011)



Notes: This figure plots the distribution of our 5th grade birthday-based imputed enrollment values by school. Birthday-based imputed enrollment is based on the birthday distribution of students observed in 4- 6th grade in June of each year. The birthday rule counts 4-6th graders born from Chanukah 11 years to Chanukah 10 years before the current school year. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added.

Figure 5: Density Discontinuity Tests (2002-2011)



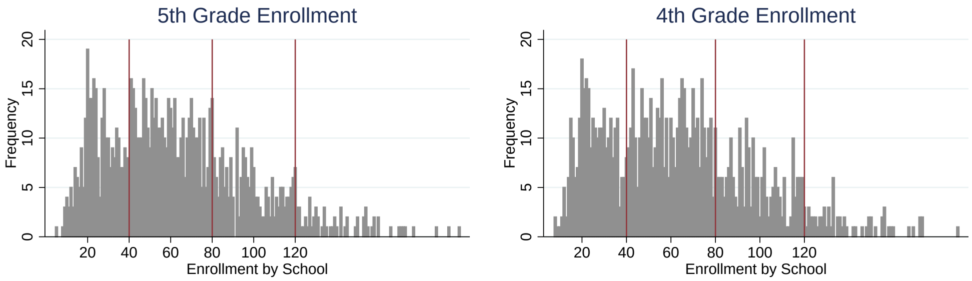
(a)

(b)

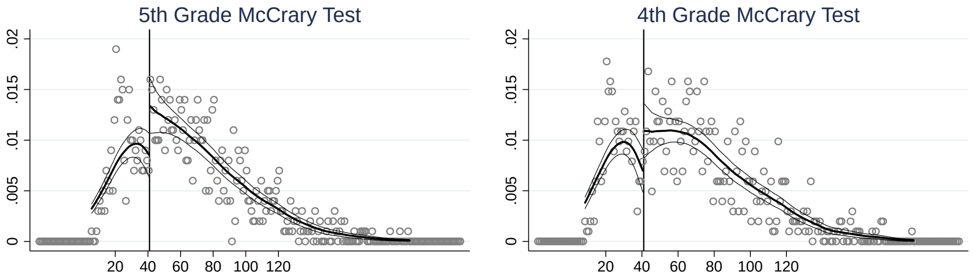
Notes: This figure plots empirical and fitted densities of November enrollment (Panel a) and birthday-based imputed enrollment (Panel b). The figure shows discontinuities at 41 (left) and 81 (right) with bin size of 1. Bandwidth, standard errors, and the density plot were produced using McCrary's DCdensity package, following McCrary (2008).

Figure 6: Density Discontinuity Tests in 1991 Data

A. Histograms



B. McCrary Tests



Notes: Panel A plots the distribution of 4th and 5th grade enrollment values reported by school headmasters in November 1990, for the 1990-91 school year. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added. Panel B plots the densities underlying McCrary (2008) tests for discontinuities at 41 with bin size 1 using the same data.

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Data Appendix

The data used here are from the 2002–2011 Growth and Effectiveness Measures for Schools (GEMS) testing program. GEMS is administered by the National Authority for Measurement and Evaluation in Education. GEMS scores are used to assess school progress. Individual GEMS scores are not released to students or schools administrators.[18]

GEMS tests are usually given some time between mid-March and mid-June (with the exception of the 2004-2006 school years, when the tests were given in October-November). The GEMS test-takers are drawn from a representative 1-in-2 sample of all elementary and middle schools in Israel, so that each school participates in GEMS once every two years. GEMS tests fifth-graders (primary school) and eighth-graders (middle school) in math, science, native language skills, and English. In principle, all students except those in special education classes are tested; in practice, the proportion of students tested is above 90 percent.

We focus on math and language tests given in Jewish elementary schools, as in Angrist and Lavy (1999). GEMS scores are reported on a 1-to-100 scale that we standardized by year and subject. Between 2002 and 2006, participating schools were tested in four subjects. Since 2007, only two subjects at a time are tested, either math and language or science and English. Our ten year sample includes the math and language scores of 243,213 fifth graders. Between 2002-2006, this sample covers an annual average of 570 Jewish public (secular and religious) schools and 1180 classes per year. Between 2007-2001, the sample covers an annual average of 290 Jewish public schools and 600 classes per year.

We linked the GEMS data to MOE administrative records covering all Israeli 5th graders. Student records include gender, parents' education, number of siblings, country of birth, and parents' country of origin. We also collected MOE data on dates of birth for the population of 4th-6th graders (not just those who participated in the math and language GEMS).

The school-level data used in this study are derived from MOE records reporting enrollment, school sector (religious etc), and a school's index of socioeconomic status (SES). We obtained two enrollment variables: November enrollment, reported by school headmasters to the MOE; a June enrollment variables computed by summing Spring class sizes. The November and birthday based imputation generate our instruments, while the June data provide

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[18] The Division of Evaluation and Measurement website, http://cms.education.gov.il/EducationCMS/Units/Rama/Meitzav/ provides additional background.

the endogenous class size variable that gets instrumented.

The 1991 and 1992 samples are those used by Angrist and Lavy. The 1991 data are posted at https://economics.mit.edu/faculty/angrist/data1/data/anglavy99. These are class-level averages.

Appendix Tables and Figures

Table A1: Class Size Effects on English and Science Scores Using November Enrollment Instruments (2002-2011)

	English				Science			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Class size	0.00248**	0.00369	0.00311	0.00328	0.00185	0.00253	0.00283	0.00282
	(0.00124)	(0.00226)	(0.00236)	(0.00232)	(0.00115)	(0.00206)	(0.00218)	(0.00213)
November enrollment	-0.00013	-0.00024	0.00100		0.00025	0.00019	-0.00044	
	(0.00025)	(0.00030)	(0.00094)		(0.00024)	(0.00028)	(0.00098)	
Enrollment squared/100			-0.00074				0.00038	
			(0.00050)				(0.00052)	
Piecewise linear trend				-0.00028				0.00025
				(0.00067)				(0.00064)
N	224,405				225,933			

Notes: See notes to Table 4.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

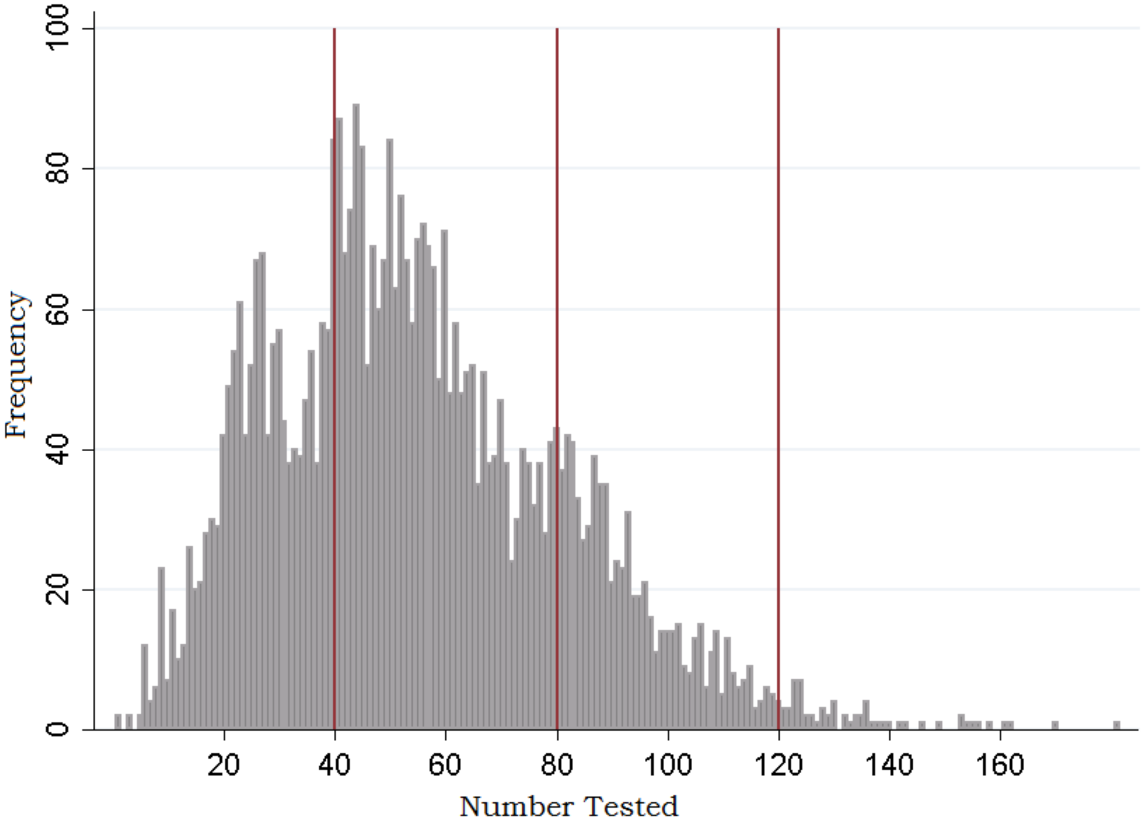
Table A2: Class Size Effects on English and Science Scores Using Birthday-based Enrollment Instruments (2002-2011)

	English				Science			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Class size	0.00247**	0.00345	0.00284	0.00303	0.00184	-0.00316	-0.00314	-0.00286
	(0.00124)	(0.00398)	(0.00439)	(0.00432)	(0.00115)	(0.00384)	(0.00426)	(0.00418)
Birthday-based enrollment	-0.00012	-0.000204	0.000521		0.000245	0.00065*	0.00062	
	(0.00024)	(0.00039)	(0.00118)		(0.00023)	(0.00038)	(0.00120)	
Enrollment squared/100			-0.00039				0.000017	
			(0.00053)				(0.00054)	
Piecewise linear trend				-0.00029				0.00132
				(0.00099)				(0.00097)
N	224,405				225,933			

Notes: See notes to Table 6.

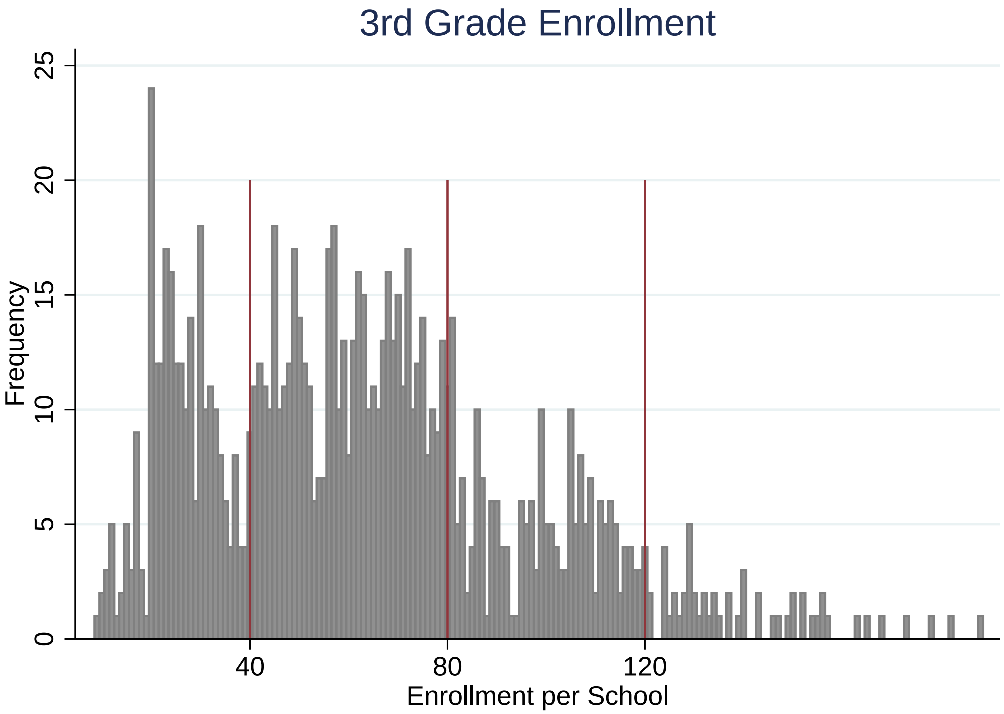
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A1: Enrollment Distribution for Tested Fifth Graders (2002-2011)



Notes: This figure plots the distribution of the number of 5th graders tested (given by the larger of the number tested in math and the number tested in Hebrew)

Figure A2: Enrollment Distribution for Third Graders (1992)



This figure plots the distribution of 3rd grade enrollment values reported by school headmasters in November 1991, for the 1991-92 school year. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added.