

## Maimonides' Rule Redux<sup>†</sup>

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*We use Maimonides' rule as an instrument for class size in large Israeli samples from 2002–2011. In contrast with Angrist and Lavy (1999), newer estimates show no evidence of class size effects. The new data also reveal enrollment manipulation near Maimonides cut-offs. A modified rule that uses birthdays to impute enrollment circumvents manipulation while still generating precisely estimated zeros. In both old and new data, Maimonides' rule is unrelated to socio-economic characteristics conditional on a few controls. Enrollment manipulation therefore appears to be innocuous. We briefly discuss possible explanations for the disappearance of Israeli class size effects since the early 1990s. (JEL C38, H52, I21, I28)*

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 grade-level enrollment, a pattern seen in Israeli data on enrollment and class size as well as in data from school districts around the world.

Analyzing data for the population of Israeli fourth and fifth graders tested in 1991, Angrist and Lavy (1999) reported 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 find little evidence of achievement gains from

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<sup>†</sup>Go to <https://doi.org/10.1257/aeri.20180120> to visit the article page for additional materials and author disclosure statement(s).

rule-induced class size reductions (as in the Angrist, Battistin, and Vuri 2017 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 sample of Israeli fifth graders tested between the school years ending spring 2002 and spring 2011. This update uncovers two findings. First, an econometric analysis paralleling that in Angrist and Lavy (1999) generates robust, precisely estimated zeros. Second, the new data reveal enrollment manipulation at Maimonides cutoffs: there are too many schools with enrollment values that produce an additional class.

Our investigation of enrollment patterns suggests a simple explanation for enrollment manipulation, and allows a straightforward remedy. A memo from Israeli Ministry of Education (MOE) officials to school leaders cautions 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 abroad in order to produce an additional class. This reflects MOE concerns that school staff adjust enrollment (or enrollment statistics) close to cutoffs so as to produce smaller classes (e.g., by driving enrollment from 40 to 41, and thereby opening a second class). School leaders might want to do this because educators and parents prefer smaller classes. MOE rules that determine school budgets as an increasing function of the number of classes also reward this sort of manipulation.

We address this problem by constructing an alternative version of Maimonides' rule that is largely unaffected by manipulation. The alternative rule imputes fifth grade enrollment by applying the official birthday cutoff for fifth-grade enrollment to a sample that includes all students in fourth to sixth grade with birth dates that make them eligible for fifth grade, while ignoring actual grade enrolled. Imputed enrollment generates a strong first stage for class size, but shows no evidence of sorting around birthday-based Maimonides cutoffs. Moreover, class size effects estimated using the statutory rule are also small, precisely estimated, and not significantly different from zero. Consistent with our claim that imputed enrollment cannot be manipulated, 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. As in the more recent data, however, enrollment sorting in the original Maimonides sample does not appear to be 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 show small correlations between Maimonides' rule and socioeconomic status, but these disappear when estimated with a few school-level controls.

The birthday-based imputation used to eliminate enrollment sorting in recent data cannot be applied in the older data because birthdays and individual test scores are unavailable for the earlier period. But other simple corrections, such as a "donut" estimation strategy that discards observations near the first cutoff, leave the original results substantively unchanged.<sup>1</sup> The discrepancy between the old and new class

<sup>1</sup> Barreca et al. (2011) appears to be the first to use the donut strategy to examine the consequences of sorting near regression discontinuity cutoffs.

size effects therefore seems more likely to be driven by a change in the Israeli education production function than by 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 now seems noteworthy that estimates for a 1992 sample of third graders reported in Angrist and Lavy (1999) show no evidence of achievement gains in smaller classes. Use of a more modern cluster adjustment in place of the parametric Moulton correction used in the original Maimonides' rule study also increases the estimated sampling variance of the original estimates.

The next section describes the Israeli school system. We then document the Maimonides first stage in our more recent sample, explain our birthday-based enrollment imputation, and show that birthday-based imputed enrollment generates no evidence of running variable manipulation. Section III reports two-stage least squares (2SLS) estimates constructed using the two alternative Maimonides instruments, and Section IV looks again at the 1991 and 1992 samples. The conclusion considers possible explanations for changing class size effects.

## I. Background and Context

### A. *Israeli Schools*

Schooling in Israel is compulsory beginning in first grade, starting around age six. Israeli students attend neighborhood schools, which serve catchment areas determined by a student's home address. Our analysis focuses on students in secular and religious 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 guided class assignment and school budgeting since 1969. The rule is well-known among school administrators and teachers. Most parents cannot change schools unless they move. We therefore expect any manipulation of enrollment to reflect the behavior of teachers and school administrators rather than parents.

### B. *Related Work*

Maimonides-style empirical strategies have been used to identify class size effects in many countries, including the United States (Hoxby 2000), France (Piketty 2004 and Gary-Bobo and Mahjoub 2013), Norway (Bonesrønning 2003 and Leuven, Oosterbeek, and Rønning 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 the estimates reported by Angrist and Lavy (1999) for Israel. A natural explanation for this difference in findings is the large size of Israeli elementary school classes. In line with this view, Woßmann (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 students using 2006 and 2009 data show no evidence of a class size effect (Shafir,

Shavit, and Blank 2016); this study also documents the vigorous debate over class size in Israel.<sup>2</sup>

A number of studies look at data manipulation and discuss how this might compromise attempts to estimate causal class size effects. Urquiola and Verhoogen (2009) uncover evidence of sorting around Maimonides-style cutoffs in a sample from Chilean private schools. Angrist, Battistin, and Vuri (2017) show that estimates from Maimonides-style experiments in southern Italy reflect increased manipulation of test scores by teachers in small classes rather than increased learning. 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 work, Jacob and Levitt (2003) document manipulation of test scores in Chicago public schools.

Methodological investigations of sorting in regression discontinuity (RD) running variables originate with McCrary (2008), who introduced the statistical test for running variable manipulation used here. Barreca, Lindo, and Waddell (2016) show that manipulation and nonrandom heaping of a running variable can bias RD 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 (2018) derive bounds on causal effects estimated using RD designs that are built on running variables compromised by sorting. Arai et al. (2018) introduce a test for validity of fuzzy regression discontinuity designs based on the joint distributions of treatment status and observed outcomes at cutoffs, applying this to the Angrist and Lavy (1999) sample. This test suggests manipulation of 1991 enrollment is not a source of bias in the original Angrist and Lavy estimates.

## II. Data and First Stage

### A. 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 were sampled for participation in GEMS (which also tests eighth graders). Tests are given in math, native language skills (Hebrew or Arabic), science, and English. GEMS test scores are reported on a 0–100 scale, similar to the scale used in Angrist and Lavy (1999). Math scores average around 68, with standard deviation of about 11 for class average scores; language scores average around 72, with a standard deviation around 8 for class averages. Student-level standard deviations are roughly double the standard deviations of class means. These statistics appear in online Appendix Table A1. The online Appendix also describes the GEMS data further.

Data on test scores were matched to administrative information describing schools, classes, 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 MOE each November. This enrollment variable,

<sup>2</sup>Results in Sims (2008) suggest class size reductions obtained through combination classes have a negative effect on students' achievement.

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 this variable as “June class size.” Individual student characteristics include gender, parents’ education, number of siblings, and ethnicity. Schools in the GEMS samples are identified as secular or religious. Each school is characterized by an index of socioeconomic status (SES index).<sup>3</sup>

Our statistical analysis looks at fifth-grade pupils in the Jewish public school system. We focus here on math and (Hebrew) language exam results. The analysis excludes students in the special education system, who do not take GEMS tests. Our analysis covers data from 2002 through 2011 (2002 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).

The matched analysis file includes 240,310 fifth-grade students from 8,823 classes. The data structure is a repeated cross section; the sample of GEMS schools changes from year to year. Table A1 reports descriptive statistics for classes, students, and schools in the estimation sample. The mean and median elementary school class has about 28 pupils, and there are roughly 58 pupils and 2 classes per grade. Ten percent of classes have more than 35 pupils, and 10 percent have fewer than 21 pupils. Demographic data show that 90 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.

### B. *The Maimonides First Stage*

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

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

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

Online Appendix Figure A1 plots actual average June class size and rule-based predictions,  $f_{jt}$ , against (previous) 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). Predicted discontinuities in the class size/enrollment relationship are diminished by the fact that many classes are split before reaching the theoretical maximum of 40.

<sup>3</sup> A school’s SES index is the average of the index for its students. The student index 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 receive more funding per student.

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

$$(2) \quad s_{ijt} = \pi f_{jt} + \rho_1 r_{jt} + \delta'_1 X_{ijt} + \gamma_t + \varepsilon_{ijt},$$

where  $s_{ijt}$  is the June class size experienced by student  $i$  enrolled in school  $j$  during year  $t$ ;  $X_{ijt}$  is a time-varying vector of student and school characteristics,  $f_{jt}$  is as defined above, and  $\varepsilon_{ijt}$  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 dummies for the 2002–2003 period and the 2008–2011 period.<sup>4</sup> The model includes year fixed effects ( $\gamma_t$ ) and controls for alternative functions of the running variable,  $r_{jt}$ .

Estimates of  $\pi$  in equation (2) are remarkably stable at around 0.62. This can be seen in online Appendix Table A2, which reports first-stage estimates computed 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 mirrors the slope on the linear segments of Maimonides' rule. Specifically, the trend is defined on the interval  $[0, 200]$  as follows:

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

The constants here join the Maimonides linear segments at the rule's cutoffs.

### C. 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 (Israeli Ministry of Education 2015a). This generates an incentive for administrators to manipulate enrollment counts, either by moving students between grades, or through false reporting.<sup>5</sup>

<sup>4</sup> Interactions of the SES index with dummies for these two periods control for changes in the weights and the components of the index implemented in 2004 and 2008.

<sup>5</sup> Funding rules for 2004–2007 were revised to make total enrollment the major funding determinant rather than the number of classes, but this reform was never fully implemented. In 2007, the MOE returned to the class-based funding rule (Lavy 2012; Vurgan 2007).



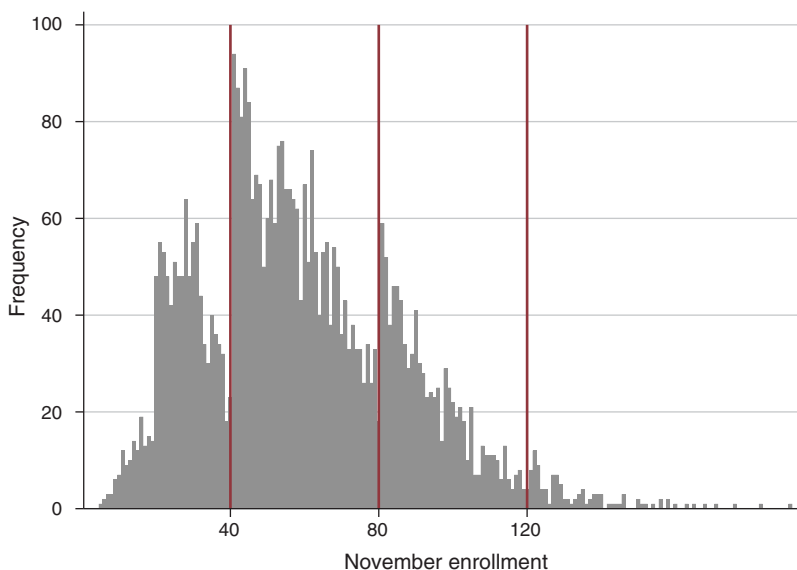


FIGURE 1. THE FIFTH-GRADE ENROLLMENT DISTRIBUTION REPORTED IN NOVEMBER (2002–2011)

*Notes:* This figure plots the distribution of fifth-grade enrollment as reported by school headmasters in November. Reference lines indicate Maimonides' rule cutoffs at which an additional class is added.

As first noted by McCrary (2008), manipulation of a running variable may be revealed by discontinuities in the running variable distribution. Figure 1 plots the histogram of November enrollment in our 2002–2011 sample. 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 the need for accurate enrollment reporting to determine funding. The 2015 circular also 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 abroad in order to produce an additional class. In 2016, the MOE began auditing enrollment data in an effort to prevent this type of manipulation, though sanctions were left unspecified (Israeli Ministry of Education 2015b). Interestingly, Figure 1 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 generally allotted half the regular funding.

Although the incentive for headmasters to push reported enrollment across Maimonides cutoffs seems clear, the question of whether this reflects misreporting or actual movement between grades is less easily resolved. Real enrollment changes can be accomplished by skipping students a grade ahead or through grade retention. A further likely channel is flexible age at entry for first graders. 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 start school.

Online Appendix Figure A2 suggests that at least some of the enrollment changes resulting from manipulation are real and persistent, rather than misreported. This figure plots the histogram of the number of fifth 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 for values below the second cutoff might be explained by the fact that roughly 10 percent of students enrolled miss the test.

Our primary concern is the possibility of selection bias resulting from enrollment manipulation. We might expect, for example, that more sophisticated school leaders understand the budgetary value of moving enrollment from just below to just beyond Maimonides cutoffs. And schools led by sophisticated leaders may also enroll 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 that uses birthday-based imputed enrollment in place of reported November enrollment. Israel's compulsory attendance laws specify rules for student enrollment in first grade according to whether a child's sixth Hebrew birthday falls before or after the last day of Chanukah. Students born after the last day of Chanukah are too young for first grade and must wait an additional year to start school. 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 advancement since first grade. Data on a sample of fourth, fifth, and sixth graders therefore includes almost all students who should be in fifth grade and can therefore be used to reconstruct the enrollment numbers that would be observed in a world where school staff follow official rules.

We apply the Chanukah-based birthday rule to June enrollment data for all fourth to sixth graders enrolled in schools sampled to take GEMS tests. This produces an imputed enrollment variable for fifth graders that is unlikely to reflect manipulation by school officials. Figure 2, which plots the imputed enrollment histogram, suggests that enrollment imputed in this manner is indeed manipulation-free. The figure shows a reasonably smooth distribution, with no evidence of spikes to the right of Maimonides cutoffs or at 20.

The McCrary-style (2008) density plots in online Appendix Figure A3 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 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.<sup>6</sup>

Online Appendix Table A3 reports estimates of the first-stage regression of class size on Maimonides' rule when the latter is computed using imputed birthday-based enrollment. These estimates are about half the size of those constructed using

<sup>6</sup>These plots use 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 plot histograms in a one-unit bin width.



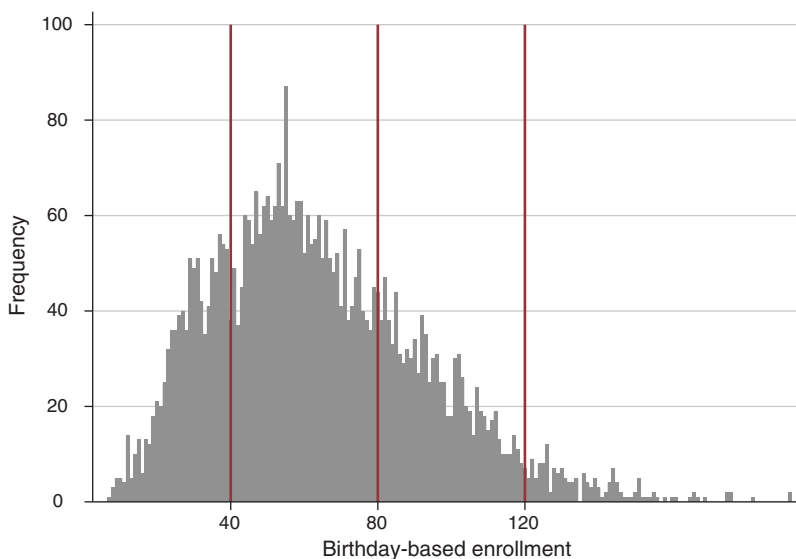


FIGURE 2. THE FIFTH-GRADE BIRTHDAY-BASED IMPUTED ENROLLMENT DISTRIBUTION (2002–2011)

*Notes:* This figure plots the distribution of birthday-based imputed enrollment for fifth graders by school. Birthday-based imputed enrollment is computed from the birthday distribution of students enrolled in fourth to sixth grade in June of each year. The birthday rule counts fourth to sixth graders born between Chanukah 11 years before and Chanukah 10 years before the current school year. Reference lines indicate Maimonides' rule cutoffs at which an additional class is added.

November enrollment. As when estimating November data, however, key first-stage parameters are estimated precisely and are largely insensitive to the details of the specification used for running variable control.<sup>7</sup>

### III. Class Size Effects: 2002–2011

Class size effects are estimated using a two-stage least squares (2SLS) procedure that models  $y_{ijt}$ , the GEMS score of student  $i$  enrolled in fifth grade at school  $j$  in year  $t$ , as a function of fifth-grade class size, running variable controls, year effects ( $\mu_t$ ), and additional controls,  $X_{ijt}$ . Second-stage models with a linear running variable control can be written:

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

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

2SLS estimates of  $\beta$  in equation (3) suggest class size has no causal effect on achievement. Estimates of effects on language and math scores, reported in columns

<sup>7</sup>Online Appendix Figure A4 plots actual average June class size against birthday-based predicted enrollment, comparing the birthday-based first stage to the first stage constructed using actual November enrollment. Consistent with the smaller birthday-based first stage, actual class size follows birthday-based predictions less closely, with smoother size changes at Maimonides cutoffs. The nonlinear and non-monotonic relationship between enrollment and class size remains in the imputed data, however.

TABLE 1—CLASS SIZE EFFECTS ESTIMATED USING NOVEMBER ENROLLMENT INSTRUMENTS (2002–2011)

	Language				Math			
	OLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	OLS (5)	2SLS (6)	2SLS (7)	2SLS (8)
Class size	0.0091 (0.0173)	−0.0220 (0.0314)	−0.0299 (0.0329)	−0.0288 (0.0322)	0.0492 (0.0229)	0.0283 (0.0422)	0.0158 (0.0436)	0.0137 (0.0429)
SES index	−0.4268 (0.0602)	−0.4312 (0.0602)	−0.4281 (0.0603)	−0.4286 (0.0603)	−0.3660 (0.0800)	−0.3688 (0.0801)	−0.3640 (0.0802)	−0.3635 (0.0801)
November enrollment	0.0025 (0.0037)	0.0052 (0.0043)	0.0226 (0.0144)		−0.0008 (0.0048)	0.0010 (0.0058)	0.0285 (0.0183)	
Enrollment squared/100			−0.0103 (0.0081)				−0.0163 (0.0101)	
Piecewise linear trend				0.0147 (0.0094)				0.0100 (0.0128)
Observations		225,108				226,832		

*Notes:* This table reports OLS and 2SLS estimates of equation (3). 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 a math or language test score. Additional covariates include student characteristics (a gender dummy, parents' years of schooling, number of siblings, a born-in-Israel indicator, and ethnic-origin indicators), year fixed effects, an indicator for religious schools, the school socioeconomic index, and interactions of the socioeconomic index with dummies for the 2002–2003 and 2008–2011 periods. The reported SES coefficient is for 2004–2007.

2–4 and 6–8 of Table 1, range from  $-0.03$  to  $0.03$ , with standard errors around  $0.03$  to  $0.04$ , and are not statistically different from 0. These reasonably precise statistical zeros contrast with the Angrist and Lavy (1999) estimates around  $-0.25$ . Interestingly, OLS estimates of  $\beta$  in equation (3), reported in columns 1 and 5 of the table, are also small, though positive (indicating bigger classes improve test scores) and significant for math scores. The large precisely estimated negative SES effect reported in Table 1 implies that a 1 standard deviation increase in the school-wide SES index (that is, reduced SES) is associated with about 0.1 standard deviation lower language and math scores. In estimates not reported in the table, we also see large coefficients on ethnicity and parental schooling. This suggests that our dependent variables are informative measures of student achievement, and bolsters the case for seeing small insignificant class size effects as true zeroes.

The education production function identified by Maimonides' rule in more recent data differs markedly from that estimated using similar specifications in 1991 data. The Angrist and Lavy (1999) results for 1991 are replicated in online Appendix Table A4, with the modification that the replication reports “Stata clustered” standard errors (clustered on school) rather than standard errors clustered using the Moulton formula as in Angrist and Lavy (1999). In contrast with the small effects found for 2002–2011, Maimonides' rule instruments in the 1991 sample, deployed with linear running variable controls, generate an estimated effect of  $-0.277$  for fifth-grade language (with a standard error of  $0.076$ ) and an estimated effect of  $-0.231$  for fifth-grade math (with a standard error of  $0.099$ ).<sup>8</sup> Estimates for fourth

<sup>8</sup> 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 of 8–10.

TABLE 2—CLASS SIZE EFFECTS ESTIMATED USING BIRTHDAY-BASED IMPUTED ENROLLMENT (2002–2011)

	Language				Math			
	OLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	OLS (5)	2SLS (6)	2SLS (7)	2SLS (8)
Class size	0.0070 (0.0172)	0.0012 (0.0608)	−0.0089 (0.0666)	−0.0061 (0.0657)	0.0386 (0.0231)	−0.0366 (0.0814)	−0.0623 (0.0889)	−0.0616 (0.0878)
SES index	−0.4254 (0.0602)	−0.4263 (0.0610)	−0.4246 (0.0609)	−0.4252 (0.0609)	−0.3570 (0.0799)	−0.3680 (0.0809)	−0.3636 (0.0810)	−0.3644 (0.0809)
Birthday-based enrollment	0.0033 (0.0035)	0.0038 (0.0060)	0.0163 (0.0184)		0.0037 (0.0046)	0.0099 (0.0081)	0.0418 (0.0239)	
Enrollment squared/100			−0.0068 (0.0086)				−0.0173 (0.0109)	
Piecewise linear trend				0.0113 (0.0151)				0.0318 (0.0203)
Observations		225,108				226,832		

Notes: This table reports OLS and 2SLS estimates of equation (3). The endogenous variable is June class size; Maimonides' rule is constructed using birthday-based imputed enrollment. Standard errors reported in parentheses are clustered at the school and year level. The dependent variable is a math or language test score. Additional covariates include student characteristics (a gender dummy, parents' years of schooling, number of siblings, a born-in-Israel indicator, and ethnic-origin indicators), year fixed effects, an indicator for religious schools, the school socioeconomic index, and interactions of the socioeconomic index with dummies for the 2002–2003 and 2008–2011 periods. The reported SES coefficient is for 2004–2007.

graders are smaller; only the estimate for language with linear enrollment controls is (marginally) significantly different from zero.

Perhaps the new findings showing no class size effects in recent data are an artifact of running variable manipulation. We explore this possibility in Table 2, which reports a set of 2SLS estimates paralleling those in Table 1, but computed using a version of Maimonides' rule derived from birthday-based imputed enrollment. Like the estimates in Table 1, the results in Table 2 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 add classes by manipulating enrollment.

We also estimated models where the effect of class size on test scores is interacted with the SES index, thereby allowing for the possibility that class size matters most for disadvantaged students. The instruments in this case are  $f_{jt}$  and  $f_{jt} \times SES_{jt}$ , where  $SES_{jt}$  is the SES index for school  $j$  at year  $t$ . These results show no evidence of class size effects or SES interactions. Online Appendix Figure A5 shows that estimating class size effects separately for each year also generates small, mixed positive and negative, and (with one exception), insignificant effects. Because Israeli media reports suggest test preparation efforts have intensified over time, this weighs against the hypothesis that the absence of a class size effect reflects extensive test preparation in more recent data (extensive coaching might equalize achievement in large and small classes).

Gerard, Rokkanen, and Rothe (2018) 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 SES index (increasing from 1 to 10 as SES declines) on Maimonides' rule in a version of equation (2) fit to school-year averages. Panel A of online

Appendix Table A5 reports these results when Maimonides' rule is constructed from November enrollment data, showing that schools with larger predicted class size have 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. This seems like a modest change, amounting to less than one-tenth of a standard deviation of the index. Moreover, the estimates in columns 4–6 of Table A5 show that this relationship disappears when Maimonides' rule is constructed using birthday-based imputed enrollment.

Although encouraging for the thesis that imputed enrollment data are uncompromised by systematic sorting, the results in panel A of Table A5 suggest we might worry about nonrandom enrollment manipulation when working with November enrollment. 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). Maimonides' rule computed using imputed enrollment is unrelated to SES with or without additional covariate controls. Since our findings on class size are consistent using instruments derived from either enrollment variable and when estimated with or without covariates, it seems unlikely that nonrandom sorting across Maimonides cutoffs in the November enrollment data is an important source of bias.<sup>9</sup>

#### IV. Earlier Estimates Explored

The evidence of running variable manipulation in 2002–2011 data naturally raises concerns about the results reported in Angrist and Lavy (1999). Online Appendix Figure A6 plots estimated enrollment histograms and densities for the Angrist and Lavy samples of fourth and fifth 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.<sup>10</sup> Online Appendix Figure A7 presents the enrollment histogram for the sample of third graders tested in 1992; this figure shows a somewhat more modest enrollment jump to the right of the first cutoff.<sup>11</sup>

Otsu, Xu, and Matsushita (2013) includes figures similar to our Figure A6. 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. Figure A6, which looks at the distribution of schools rather than classes, indeed shows evidence of sorting around the first Maimonides cutoff in 1991.

Additional analyses of the older data (not reported here) suggest sorting was less pervasive in 1991 and 1992 than in more recent data, with little evidence of

<sup>9</sup>2SLS estimates of class size effects from models without covariates other than running variable controls are small and positive, and marginally or not significantly different from zero.

<sup>10</sup>The discontinuity at 81 (the split from 2 to 3 classes) in the 1991 data is not statistically significant.

<sup>11</sup>The discontinuity at 41 in the 1992 data is statistically significant; the discontinuity at 81 is not.

TABLE 3—MAIMONIDES' RULE EFFECTS ON SOCIOECONOMIC STATUS IN 1991 DATA

	Fifth grade			Fourth grade			Third grade		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$f_{jr}$	−0.0592 (0.0784)	−0.0686 (0.0848)	−0.0709 (0.0865)	0.0532 (0.0810)	0.0636 (0.0878)	0.0718 (0.0894)	0.0400 (0.0845)	0.0444 (0.0906)	0.0581 (0.0910)
November enrollment	−0.0598 (0.0152)	−0.0475 (0.0448)		−0.0691 (0.0155)	−0.0824 (0.0461)		−0.0940 (0.0165)	−0.101 (0.0534)	
Enrollment squared/100		−0.0068 (0.0235)			0.0076 (0.0248)			0.0041 (0.0307)	
Piecewise linear trend			−0.1010 (0.0350)			−0.1332 (0.0349)			−0.1796 (0.0358)
Observations	1,002	1,002	990	1,013	1,013	1,003	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 schools appearing in the fourth and fifth grade samples. The piecewise linear control in columns 3, 6, and 9 omits enrollments above 160.

manipulation beyond the first Maimonides cutoff. Even so, in view of the discontinuity in the 1991 enrollment distribution seen in Figure A6, it's worth asking whether enrollment manipulation is likely to be a source of omitted variables bias in the older estimates. Table 3 therefore reports estimates from a regression of school-level SES on Maimonides' rule using 1991 data, similar to the estimates reported in Table A5. As in the more recent estimates (with covariates), we see little evidence of a relationship between Maimonides' rule and school-level SES. The negative associations estimated for fifth graders are not significantly different from zero, while the sign flips to (insignificant) positive for third and fourth graders.<sup>12</sup>

The individual student 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. The results of this further exploration of the consequences of sorting in 1991 are reported in online Appendix Table A6. For example, the estimated class size effect of −0.234 in column 1 of Table A6 was computed using a sample omitting schools with fifth-grade enrollments between 39 and 41. This can be compared with the full-sample estimate of −0.277. Although somewhat less precise, the donut estimates in Table A6 differ little from those for the full sample estimates reported in Table A4.

## V. Summary and Conclusion

The Maimonides' rule identification strategy for class size effects generates precisely estimated zeros in large Israeli samples for 2002–2011. These samples also show clear evidence of enrollment manipulation around Maimonides class size cutoffs, likely reflecting school leaders' desire to open an additional class when enrollment is close to a cutoff. But enrollment imputed using information on grade-eligible birthdates appears unaffected by manipulation, and 2SLS estimates derived from imputed enrollment instruments generate similarly small class size

<sup>12</sup>The 1991 SES index is scaled as “percent disadvantaged.”

effects. Maimonides' rule constructed using birthday-based imputed enrollment is also unrelated to a school-level measure of SES.

We find only weak evidence of *systematic* enrollment sorting in recent data: correlation between Maimonides' rule and socioeconomic status disappears after conditioning on a few covariates. The fact that estimated class size effects are similar whether Maimonides' rule is constructed using November or birthday-based enrollment reinforces 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 recent data contrast with the substantial negative class size effects reported by Angrist and Lavy (1999).

We see some evidence of manipulation around the first Maimonides cutoff in the older data analyzed by Angrist 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 are unaffected by manipulation near cutoffs. This conclusion is likewise supported by specification test results reported in Arai et al. (2018).

The disappearance of Israeli class size effects may reflect changes in the Israeli education production function. The fact that Israeli class size has fallen from a median of 31 in 1991 to 28 in more recent samples may be relevant. Yet Figure A5, which plots 2SLS estimates by year, shows no evidence of declining effects over the period 2002–2011. It may be relevant that, since the early 2000s, some schools have hired additional teaching staff, an expense covered mostly by parents in high SES schools (Vurgan 2014). Weighing against the importance of these changes, our analysis fails to show significant class size/SES interactions or significant effects in earlier years.

We briefly explored changes in other education inputs that might explain the absence of class size effects in recent data (data on inputs are from an analysis reported in Blass, Tsur, and Zussman 2012). Regressions of total hours of instruction provided by school staff and others on predicted class size show small, marginally significant increases on the order of 0.5 percent for each additional student. We also see small, marginally significant increases in the share of class time going to small group instruction. Per-pupil spending, however, falls about 2 percent for each additional student. In future work, we hope to identify causal effects of these additional inputs, and to quantify possible interactions with class size in education production.

Finally, it seems noteworthy that the 1991 estimates reported in Angrist and Lavy (1999) are strongest for fifth graders, but less impressive for fourth graders, for whom only estimates for language are significantly different from zero. The original Angrist and Lavy study also reported zero class effects in a 1992 sample of third graders, a result attributed in the original write-up to extensive test preparation and changes in testing protocols. These forces may be at work in the more recent GEMS data analyzed here as well. Some analysts have suggested schools are increasingly and effectively teaching to GEMS tests (e.g., Klieger 2009). Here too, however, there is no smoking gun for mediating interactions: our analysis uncovers no changes in class size effects over time that might be linked to changes in test preparation.<sup>13</sup> In

<sup>13</sup> In view of the unusually early administration of GEMS tests in 2004–2006, online Appendix Table A7 reports estimates analogous to those in Tables 1 and 2, computed in a sample omitting data from 2004–2006. This change in sample changes estimated class size effects little.



retrospect, it seems fair to say that the 1991 results are unusual in showing strong class size effects, while the null effects reported for 1992 are more representative of the causal relationship between class size and test scores in Israel.

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