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ABSTRACT

New Evidence on Classroom Computers and Pupil Learning[*]

The question of how technology affects learning has been at the center of recent debates over educational inputs. In 1994, the Israeli State Lottery sponsored the installation of computers in many elementary and middle schools. This program provides an opportunity to estimate the impact of computerization on both the instructional use of computers and pupil achievement. Results from a survey of Israeli school-teachers show that the influx of new computers increased teachers' use of computer-aided instruction (CAI) in the 4th grade, with a smaller effect on CAI in 8th grade. Although many of the estimates are imprecise, on balance, CAI does not appear to have had educational benefits that translated into higher test scores. OLS estimates show no evidence of a relationship between CAI and test scores, except for a negative effect on 8th grade Math scores in models with town effects. IV estimates for 4th graders show lower Math scores in the group that was awarded computers, with smaller (insignificant) negative effects on language scores.

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> "That small miracle can be replicated in every school, rich and poor, across America . . . Every child in American deserves a chance to participate in the information revolution." President Clinton, at the East Somerville Community School, June 5, 1998.

> "We could do so much to make education available 24 hours a day, seven days a week, that people could literally have a whole different attitude toward learning." Newt Gingrich talking to the Republican National Committee (quoted in Oppenheimer, 1997).

> "Netanyahu explained to a group of politicians and computer professionals how he wanted to provide a quarter-million of his country's toddlers with interconnected computers." Recounted by MIT computer scientist Michael Dertouzos, September 1998.

Politicians, educators, parents, and researchers have long looked to technology to improve schools. One of the earliest advocates for technology in the classroom was Thomas Edison, who predicted in 1922 that motion pictures would revolutionize education and "be an epoch in the common school" (Israel, 1998). Edison himself funded educational films, though he also complained about lack of teacher interest and high production costs. In the 1950s Psychologist B.F. Skinner published a series of papers predicting that "teaching machines" would make learning dramatically more efficient (see, e.g., Skinner, 1954, 1958). Skinner's writing reflects a modern-sounding emphasis on "the constant interchange between program and student" and the value of "home instruction." Recent years have seen renewed and even more intense interest in classroom computer use, including interest in the use of computers in schools in less-developed countries (see, e.g., Anandakirichnan, 1988).

The educational use of computers generally falls under two broad headings. The first is computer skills training (CST), which teaches students <u>how to use</u> computers. The second is computer-aided instruction (CAI), which "<u>uses computers to teach things</u> that may or may not have any relation to technology" (President's Committee of Advisors on Science and Technology, 1997). CST is essentially vocational, and includes keyboarding skills, instruction in the use of word processing, database management, the use of spreadsheets and other software tools, and the study of computer programming.

Basic familiarity with CST skills seems undeniably useful, just as typing was a useful skill taught in American high schools earlier in the 20th century. But most of the recent interest in the educational use of computers focuses on CAI and not CST. This focus is reflected in the report of the President's Committee of Advisors on Science and Technology Panel on Education Technology, in Apple Computer's "Classrooms of Tomorrow" project (Baker, Gearhart, and Herman, 1993), and in the growing interest in "distance learning" in schools and universities. In contrast with the apparent consensus regarding the value of at least some level of computer literacy, the role of CAI remains controversial. Skinner's claims notwithstanding, the theoretical case for CAI is not well-developed, and there are good reasons to believe that computers can actually be a diversion. One widely-cited proponent of this negative view is Stoll (1995), who compared computers to the children's television program Sesame Street, arguing that (p. 147) "Both give you the sensation that merely by watching a screen, you can acquire information without work and discipline."[1]

The question of CAI effectiveness is of much more than academic interest since CAI infrastructure is expensive and may take resources from other educational uses.[2] Perhaps the most important shortcoming in the case for further investment in CAI infrastructure is the fact that the evidence for effectiveness is both limited and mixed. Although CAI has been around for decades, there are few empirical studies that meet a rigorous methodological standard. Many studies are qualitative, gathering impressions from participants in demonstration projects, or quantitative but with no real comparison group. The results of those studies that do attempt to compare outcomes between CAI-trained pupils and other pupils are hard to assess. A recent review by Kirkpatrick and Cuban (1998) catalogs both individual studies and meta-analyses that find wide-ranging effects.[3]

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[1]Oppenheimer (1997) surveys critical assessments. See also Cuban (1986).

[2]In 1998, for example, Massachusetts schools bought 40,000 computers, and the State Department of Education urges schools to replace one-quarter of them annually at a cost of $250-$400 per pupil (Seltz, 1999). Oppenheimer (1997) identifies some school districts where expenditure on computers appears to be crowding out expenditure on music, art, and traditional shop programs.

[3]Economists have looked at CAI in their own discipline. An early reference on CAI in economics teaching is Booms and Kaltreider (1974). Porter and Riley (1992) argue that CAI has not been shown to be effective in economics. A recent study by Wenglinsky (1998) using nationally representative samples finds both positive and negative effects. For other examples and surveys, see,

In this paper, we provide new evidence on the educational consequences of CAI. Our study exploits an episode in Israel that facilitates controlled comparisons. In 1994, the Israeli State lottery, which uses lottery profits to sponsor various social programs, funded a large-scale computerization effort in many elementary and middle schools. By June 1996, about 10 percent of the country's elementary school pupils and about 45 percent of the country's middle schools pupils had received new computers as a consequence.[4] We begin the empirical analysis by using this episode to estimate the effect of the new technology on both teachers' use of CAI and their pupils' test scores. Following this "reduced form" estimation of program impacts, we put the pieces together with two-stage least squares (2SLS) by using Tomorrow-98 deployment as an instrument for the effect of CAI intensity on pupil achievement.

A variety of unique data sources facilitate our analysis of computers in schools, and allow us to estimate the effects of CAI effects using a number of statistical methods. In addition to OLS estimates of the effect of CAI, we use a dummy for program receipt as an instrument for CAI intensity, and we develop a non-linear instrumental variables estimator that exploits information about applicants' priority ranking for program funding as determined by local authorities. These methods show that the influx of new computers in 1994 and 1995 led to a substantial increase in the use of CAI in elementary schools, with smaller effects on usage in middle schools. There is no evidence, however, that increased educational use of computers actually raised pupil test scores. OLS estimates show no relationship between CAI and achievement except for a negative effect on 8th grade math scores in models with town effects. And the IV results show a (marginally) statistically significant decline in the test scores in 4th grade Math classes, where the new computers had the largest impact on instructional techniques.

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e.g., Knight, et al (1981), Kulik and Kulik (1991), Liao (1992), and Cuban (1986, 1993).

[4]Much of the software used in the program was from the *Center for Educational Technology (CET)*, a private company that accounts for most of the educational software market in Israel. The *CET* sells educational software in the US and Europe though a number of well-known foreign partners.

I. Data and background

A. The Tomorrow-98 program

As in the US, many Israeli schools have long had some sort of computer equipment for instructional use, but the Tomorrow-98 program (in Hebrew, "Mahar") allowed for a significant upgrade. The main focus of this program was on the "computerization of the education system", accomplished by ". . . creating a supportive environment that can integrate information technologies in a range of activities within the school," "training teachers to integrate computers in teaching," and "equipping schools with hardware and software, and replacing outdated incompatible equipment" (Israel Ministry of Education, Culture and Sport, 1994, p. 36). The program included significant funding for teacher training as well as hardware and software. Between 1994 and 1996, the first three years of the program, 35,000 computers were installed in 905 schools. In 1994, 474 schools received computers and training. In 1995, schools received 16,000 computers through the program. In 1996, more computers were installed and 2,100 primary-school Math teachers received training in CAI (Israel Ministry of Education, Culture, and Sport, 1996) . The target student-computer ratio was 10:1, to be achieved by 1998, the fifth and final year of the program. Most of the funding came from the Israeli State Lottery, with additional money from the Ministry of Education and local authorities.

Funds for Tomorrow-98 were distributed through an application process. Individual towns and regional authorities applied for funds by submitting a list of elementary and middle schools to be computerized, ranked according to the municipalities' assessment of the schools ability to make good use of the computers. This generally meant the schools had some sort of pre-existing computer infrastructure and some "need" and "ability" to make use of the computers. The Ministry of Education used a set of guidelines to distribute the project money to schools in towns that applied. Priority was given to towns with a high proportion of 7th and 8th grade enrollment in stand-alone middle schools (as opposed to combined 1-8 schools). After high-priority municipalities received an allocation for their middle schools in a 1:10 computer:pupil ratio, equipment was distributed down the municipalities priority list. In this process, each town received money to computerize their elementary and 1-8 schools in a 1:10 ratio up to a ceiling. The ceiling was determined by the municipal grade 1-8 enrollment as a proportion of national grade 1-8 enrollment. The first computers funded by Tomorrow were delivered in September, 1994.[5]

B. Data

The main data source for this study is a test given to pupils attending a random sample of elementary and middle schools in June 1996. Schools from different sectors (Arab/Jewish) and types (religious/secular) were sampled, but we look only at Jewish schools (including religious and secular schools). The total number of Jewish schools sampled was about 200, but only 122 of these applied for Tomorrow-98 program money. The test was designed and conducted by the National Institute for Testing and Evaluation (NITE), which runs college admissions testing in Israel.

Test score data were collected as follows: in each sampled school with at least one 4th grade class (i.e., an elementary school or a 1-8 school), one class was chosen to take a test in Math and one class was chosen to take a test in Hebrew. Similarly, in schools with 8th grade classes, one class was chosen to be tested in Math and one class was chosen to be tested in Hebrew. Schools having both 4th and 8th grades (i.e., 1-8 schools) contribute test scores for both grades. If there were more than two classes in a grade, two classes were chosen for testing at random, with the subject assignment also randomized. The pupil data consists of individual records with either a Math or Hebrew score, and pupil demographic data from school records. The demographic data include age, sex, immigrant status, and special-education status. The tests are grade-normed achievement tests, with scores measured as percent-right.

The NITE data on test scores is combined with data from a brief survey (also designed by NITE)

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[5]In 1998, there were roughly 2000 primary and 500 middle schools in Israel, of which %36 received program computers. Most of the computers were installed in a special classroom or computer lab. Classes used the lab, according to a schedule, that allowed for both computer skills training and computer-aided instruction.

given to all the teachers of each sampled class. The teachers survey and pupil testing was done at the same time. Because each 4th or 8th grade class is potentially taught by a number of teachers for a range of subjects (i.e., Math, Hebrew, Science, Bible), we attempted to identify the principal Math and Hebrew teacher for each class. Our analysis file uses data on these teachers only; that is, our analysis of Math scores includes information for a teacher we identified as the main Math teacher for the class.

The teachers' survey collected information about how teachers teach, including their use of technology in the classroom, and their views on a variety of issues related to technology, teacher training, and instructional methods. Data on CAI were collected in the following question:

Which of the following do you use when teaching?

a. xeroxed worksheets
b. instructional booklets
c. games
d. computer software or instructional computer programs
e. TV programs
f. Other audio-visual materials

Teachers responded to each item using a 4-point intensity scale:

not at all (0)
sometimes (1)
frequently (2)
almost always (3)

The response to item (d) is our measure of CAI. In addition to these survey responses, we asked the Ministry of Education to collect data on teacher demographic characteristics in a follow-up survey in Spring 1997.

The third component of our data base consists of information on Tomorrow implementation schedules and computer infrastructure in schools collected for the purposes of our evaluation. In 1998, the Ministry of Education obtained information from the contractors who installed the Tomorrow computers, with verification and additional information collected from school principals. This information includes the date of receipt of new equipment from Tomorrow, the extent and type of pre-1994 computer resources, and information about non-program computers received between 1994 and 1996. Pre-existing computers are described as either “sophisticated” (IBM XT or better), or “non-sophisticated” (Commodore-type machines). Schools may have had no computers, non-sophisticated machines, or both types.

The fourth component of our data base contains information about schools in 1996 and 1991. The 1996 data come from Ministry of Education files, and includes the Israeli Pupil Disadvantaged (PD) index and school size. The PD index is an important summary statistic used to categorize schools and to make school funding decisions in Israel. The 1991 school-level data comes from the data set used in the Angrist and Lavy (1999) study of class size. This data set provides information on lagged test scores. In the analysis of 4th grade scores, we use the 1991 school average Math and Hebrew scores in 4th grade to control for possible differences in performance across schools. In the analysis of 8th grade scores, we use a less direct control for lagged scores since we have no early information on 8th grade scores. For 1-8 schools, the 8th grade lagged scores are those of 4th and 5th graders in these same schools in 1991. For each 7-9 school, the lagged scores are the averages of the 1991 4th and 5th grade scores from the elementary schools that feed that school. A data appendix describes the procedures used to match the various data sources in greater detail.

II. Descriptive statistics and OLS estimates

A. Descriptive statistics

Descriptive statistics are reported in Table 1 for three samples, separately for each combination of grade and subject. The first sample for 4th grade Math scores consists of 4,779 pupils in 181 schools. This is the full sample of Jewish 4th graders for whom we have 1996 Math scores. The second sample is limited to pupils in schools that applied for Tomorrow funds, and includes 3271 pupils in 122 schools. The third sample is the subset of the applicant sample for which we were able to obtain 1991 score data. This includes 2891 pupils in 107 schools. The three samples for other grades and subjects are organized similarly.

The average 4th grade test score ranges from 67-69 with a standard deviation of around 20. The average 8th grade test score ranges from 57-66, also with a standard deviation around 20. There is little evidence of differences in test scores across subsamples in any grade/subject category. Other variables described in the table include an indicator for any use of CAI, and the computer-use intensity ranking, with a mean of around .8 for 4th graders and .4 for eight graders. This ranking is the main regressor of interest. The next line in the table shows the mean proportion of applicants that received Tomorrow program funding. This proportion is .14-.17 for 4th graders and around .5 for 8th graders. The difference by grades reflects the higher priority given to program funding for middle schools. Descriptive statistics for control variables and lagged test scores are also shown in the table. The pupil disadvantage (PD) index is measured on a standardized scale.

In addition to being more likely to get program funding, 8th graders also had the use of program computers for longer: an average of 13 months versus about 9 months for 4th graders. Still, on average, 4th graders had the use of computers for a full school year as of the test date in 1996. It is also noteworthy that almost half of 4th grade and almost two-thirds of 8th grade pupils had access to some sort of computer technology before the Tomorrow program.

Pupils in schools that use computers for instruction differ in a variety of ways from those that have little or no usage. This can be seen in Table 2, which reports variable means by computer-use intensity and Tomorrow program status.[6] For both grades, pupils in schools with more intense use of CAI tend to be from somewhat more disadvantaged backgrounds, though these differences are not all significant. This may reflect a general tendency in the Israeli school system to direct resources and programs to schools on a remedial basis (Lavy, 1995). Among 4th graders, heavier computer users are also more likely to have had some (relatively) sophisticated computer equipment before 1994. Eighth graders tested in Math were less likely to have had sophisticated computers but more likely to have had unsophisticated computers. There is no

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[6]The standard errors for differences in means in Table 2 and the regression estimates in Tables 3-6 are corrected for school-level clustering using equation (1) in Moulton (1986).

relationship between the presence of previous computer equipment and computer use for 8th graders tested in Hebrew. This may be because 8th grade schools were already relatively well-equiped, though it should also be noted that the "previous computers" measures are retrospective reports by principals that may not be very accurate. Lagged test score differences by CAI status are not significant.

Since our primary identification strategy uses Tomorrow-98 as a source of exogenous variation in computer use, differences by Tomorrow-98 program status are more important for our purposes than differences by computer-use. Fourth grade program participants are more likely to be disadvantaged, but this difference is significant only for schools tested in Hebrew. Moreover, this is reversed for 8th graders. These relationships are broadly consistent with features of the selection process for Tomorrow-98 funding that were described to us. Among 8th graders, middle schools received priority over 1-8 schools; in Israel these schools tend to be located in better areas. Among 4th graders, some preference was given to schools with a higher proportion of disadvantaged students. In any case, it is clear that control for pupil background and school type may be important when attempting to estimate the effect of the program. Another noteworthy difference is an increased likelihood of having pre-program access to relatively sophisticated computers among program participants, both in 4^{th} and 8^{th} grade.

Among 4th graders, there is little evidence of a difference in 1991 test scores by Tomorrow-98 program status, while for 8th graders the differences are positive and somewhat larger. Except for the scores of 8th graders tested in math, however, none of the contrasts in lagged scores by program status is significant. The similarity of lagged test scores between program and non-program groups increases the likelihood that post-treatment differences in test scores are actually caused by T-98.

B. CAI and test scores

The estimation framework is based on a regression model, meant to capture the causal effect of computer use for those whose usage was affected by the Tomorrow program. For the *i*th student in school

s, we assume that potential test scores with alternative levels of CAI are given by:

$$y_{is} = W_s'\gamma + X_i'\beta + c_{js}\alpha + \eta_s + \varepsilon_{is} \qquad (1)$$

where y_{is} is the test score for pupil i in school s, W_s is a vector of school characteristics, and X_i is a vector of pupil characteristics. The regressor of interest, c_{js}, is either a dummy indicating whether the level of computer-use intensity is greater than or equal to j (j=1,2,3), or the intensity ranking itself, which we denote c_s. The intensity ranking is coded from our teacher survey. Since all pupils tested in the same subject and grade have the same teacher, in practice c_{js} and c_s vary only with s. The other school characteristics, W_s, include the proportion of disadvantaged pupils in the school and the school's priority ranking in the Tomorrow-98 allocation process. The pupil characteristics, X_i, include sex and immigrant status. The error term η_s is an i.i.d. random school effect that is introduced to parameterize within-school correlation in scores. The remaining error component, ε_{is}, is specific to pupils. The coefficient, α, is the parameter of primary interest. The empirical analysis uses test scores in standard deviation units, so the estimates of α have an 'effect size" interpretation.

Fourth graders in schools where teachers report using more CAI have slightly higher Math scores, but there is less evidence of an association between CAI and 4th grade Hebrew scores. This can be seen in Table 3, which reports OLS estimates of the relationship between CAI intensity and test scores for applicants, for applicants with test score data, and for a sample of pupils in large towns. This last sample is used to control for town fixed effects, and includes any pupil (whether or not their school applied for Tomorrow funds) living in a town with at least two schools.[7] Each row in the table shows results from a different regression, according to whether the regressor of interest is a dummy variable or the intensity

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[7]Estimates for 4th graders control for sex, immigrant status, special education status, school enrollment, the pupil disadvantage index, whether schools had simple or sophisticated computers before 1994, and the school priority ranking in the Tomorrow-98 allocation process. Estimates for 8th grade Hebrew scores include these controls plus dummies for school type. Estimates for 8th grade Math scores omit controls for immigrant and special education status. Towns with only one school are dropped from the sample when town effects are included and the town-effects sample is not limited to applicants.

ranking itself. For example, 4^{th} grade applicants with CAI$\geq$1 (some use of CAI) have scores .045 above those with no use of CAI, while the model with an ordinal regressor shows that a one unit increase in intensity is associated with .047 higher scores. But the positive effects for 4th grade Math scores are not statistically significant in the applicant samples, and control for town effects reduces the CAI effects for 4th graders essentially to zero.

OLS results for 8th graders in the two applicant samples show little evidence of a relationship between CAI intensity and test scores in either subject. In the town-effects sample for 8th grade Math scores, however, there are marginally significant negative score effects for two out of three dummies and using the ordinal ranking. Except for the Hebrew scores of 4th graders, Table 3 also shows a pattern of declining effects as the models included larger sets of controls, i.e., progressing from a specification for applicants, to applicants with lagged test scores, to control for town effects. This suggests that part of the positive association in column 1 is due to omitted variables that are positively associated with test scores and computer use. For example, since private fund-raising for public school activities is common in Israel, schools in more prosperous neighborhoods probably have greater access to parental resources to fund education technology. This possibility motivates the 2SLS estimation strategy discussed in the next section. The 2SLS estimates exploit Tomorrow-98 program status as a source of exogenous variation in CAI intensity.

III. Instrumental-variables estimates

A. Reduced-form program effects

We begin with a reduced-form analysis of program impacts since this does not require commitment to a particular endogenous variable capturing all possible channels for the impact of CAI intensity. The first four columns of Table 4 report the relationship between CAI intensity and the Tomorrow-98 program. CAI intensity is measured using a series of dummies for levels of the ordinal ranking and with the ranking variable itself. Estimates are reported for models with and without control for lagged scores, and the same covariates as in Table 3. All of the estimates show that 4^{th} grade pupils in schools that received funding from the Tomorrow program were more likely to be exposed to CAI when studying both Math and Hebrew than pupils in schools that did not receive funding. The estimates for Math show a shift at all levels of intensity while those for Hebrew show a shift only from "no use" to "some use" of CAI (i.e., an effect on CAI$\geq$1 or c_{1s}). Of course, these shifts may reflect pre-program differences, but controls for the presence of computers in the school before the program should mitigate pre-program differences. In contrast with the results for 4^{th} graders, program funding had relatively little effect on 8th grade teaching methods in either subject. The difference in program impact on CAI across grades is consistent with the fact that CAI is generally less widely used in upper grades.[8]

In addition to estimating program effects on CAI intensity, we used the teachers' survey to explore the relationship between program status and other aspects of the school environment. In particular, we used equation (1) to estimate the effect of program status on class size, subject coverage, hours of instruction, frequency of teacher training, use of non-computer audio-visual or TV equipment, and teacher satisfaction with the level of training and class size. None of these variables were related to program status, so the Tomorrow-98 program appears to have increased the use of CAI in 4^{th} grade, without otherwise changing the observed school environment.

The reduced form estimates of program effects on test scores are reported in the last column of Table 4. For 4th graders, there is a substantial and at least marginally significant negative relationship between Tomorrow program status and test scores, with pupils in the Tomorrow group scoring .2 to .25 standard deviations lower than other pupils. Fourth grade Hebrew scores and 8th grade Math scores are also lower in the program group; these differences are not significant. Eighth-grade Hebrew scores are slightly higher for program participants, though here too the difference is not significant. Thus, while there is clear evidence that computers funded by Tomorrow-98 led to an increase in CAI at least in 4th grade, there is no evidence

[8]Rotin (1999) also concludes that the Tomorrow-98 program had an impact on the prevalence of CAI, though he does not present separate estimates for elementary and middle school grades.

that this translated into higher test scores. The only statistically significant test score difference is the negative effect on 4th grade Math scores, and two out of three of the other groups show negative effects.[9]

B. 2SLS

The reduced-form effects on test scores capture program impacts without specifying the specific channel whereby new computers affect scores. But it is also of interest to scale these reduced-form effects into the effects of an increase in CAI. For the purposes of 2SLS estimation, we focus on models treating the ordinal ranking variable as the single endogenous regressor of interest. One reason for focusing on the ranking is that it seems most natural to think of Tomorrow-98 program award status (T_s) as providing a single instrument for c_s. Models with more than one endogenous regressor (i.e., multiple intensity dummies) would require more than one instrument.[10] Moreover, in spite of the fact that c_s is ordinal, conventional 2SLS estimates of the effect of c_s using a single binary instrument can be interpreted as estimating the *average* effect of a unit increase in the intensity ranking for those whose intensity was increased by the program (Angrist and Imbens, 1995; Theorem 1). Finally, note that the reduced form estimates show the program shifted the intensity distribution at more than one point in the intensity distribution.. This implies that 2SLS estimates replacing c_s with a single dummy variable for, say, any computer use (c_{1s}), will be "too big" in the sense that they over-estimate the causal effect of interest (Angrist and Imbens, 1995; p. 436). These considerations, discussed in greater detail in the appendix, lead us to treat c_s as the endogenous variable in a 2SLS setup.

We report 2SLS results for the 4th grade sample only. 2SLS results for 8^{th} graders are omitted since

[9]Similar results are obtained when the dummy for Tomorrow-98 is replaced with a variable measuring the number of months Tomorrow-98 computers were in schools. The absence of a significant reduced effect on eight grade scores can be seen as a specification check since there are no first-stage effects on CAI intensity for eight graders.

[10]We also briefly explore specifications using dummies for months of program exposure as multiple instruments. In practice, this approach is not powerful enough to identify the effects of multiple dummies.

there is no significant reduced form effect in the 8th grade sample. The sign of the 2SLS estimates is necessarily the same as the sign of the reduced-form estimate in Table 4; the only change from the reduced form is a re-scaling. The first-stage equation for this procedure is

$$c_s = W_s'\pi_1 + X_i'\pi_2 + T_s\pi_0 + \xi_{is}, \qquad (2)$$

where π_0 is the first-stage effect. Estimates of π_0 in this equation were reported in column 4 of Table 4 (the standard errors in that table allow for school-level clustering in ξ_{is}). The list of control variables is the same as for the OLS and reduced-form estimates reported in Tables 3 and 4, and includes schools' Tomorrow-98 priority ranking.

The results of 2SLS estimation using samples of all applicants and samples of those with lagged test scores, reported in columns 1-4 of Table 5, suggest that increasing the intensity of CAI by one unit reduces the Math test scores of 4^{th} graders by about .3 or .4 standard deviations. Not surprisingly given the reduced-form results, only the Math estimates are significant.[11] The negative effects for 4th grade Hebrew scores are on the order of .25 standard deviations. Importantly, the contrast between columns 1 and 3 and columns 2 and 4 shows that the 2SLS results are not sensitive to control for towns' priority ranking in the Tomorrow-98 allocation process.

Table 5 also reports the results of three simple checks on the basic 2SLS specification. First, the estimates in columns 5 and 6 use samples composed entirely of pupils in schools that received Tomorrow-98 funding and for whom we have data on 1991 scores. As before, the instrument in this case is a dummy indicating whether the pupil is in a school that received funding before June 1996. But here the comparison group consists solely of pupils who received Tomorrow-98 computers after June 1996 (and before the end of December 1997, the last date we have information for). This strategy controls for the possibility that Tomorrow-98 winners differ in some unobserved way from Tomorrow-98 losers, thereby biasing 2SLS

[11]The t-statistics for 2SLS estimates are lower than the corresponding t-statistics for the reduced-form effects because the 2SLS residuals are more highly correlated within schools than are the reduced-form residuals.

estimates of program effects. In fact, results using the "T-98/will-get-T-98" sample are remarkably similar to those in the full sample.

Second, columns 7 and 8 report the results of adding controls for the instructional *use* of computers (as opposed to possession of hardware) by 4th graders in 1991. This school-level variables provides an additional control for pre-existing differences between program winners and losers. The data on lagged computer use come from the same source as lagged test scores. Only a subset of schools have this information, which consists of the school average of indicators for whether teachers in the relevant grade in the school used computers for instruction. Control for lagged computer use has little effect on the estimates of the impact of computer use on 4th grade test scores.

Finally, columns 9 and 10 of Table 5 reports the results of replacing a single T-98 dummy with up to 20 dummies indicating the number of months T-98 computers were used (the number of dummies depends on the subject and grade). The idea here is that the more time a school had access to the Tomorrow-98 computers, the more of an impact should be expected on CAI intensity and test scores. Moreover, if the instruments satisfy the exclusion restriction motivating 2SLS estimation, this specification should generate similar but more precise estimates than those generated by the basic single-dummy specification. Results using months dummies as instruments are considerably more precise than estimates using a single dummy, though somewhat smaller than results from the basic specification. The differences in estimates across models is not statistically significant, however.

C. Assessing instrument validity

The Tomorrow-98 instrument arises from a funding process that involved a number of bureaucratic guidelines and idiosyncratic elements. As we noted earlier, the most important factor determining resource allocation was town ranking of schools, modified to some extent by central government intervention in cases where Ministry of Education officials felt local assessments were biased by political considerations. A second consideration was grade structure and school organization, with priority given to those towns having more stand-alone middle schools. Although these factors were certainly not randomly assigned, Table 2 shows little evidence for a systematic association between Tomorrow-98 award status and either pupil characteristics or schools' average test scores in 1991, three years before the program. This supports a causal interpretation of the IV estimates.

Important additional evidence for instrument validity comes from the pattern of 2SLS results. If computers were especially likely to have been awarded to low-achieving schools, we might have expected lower test scores in award schools for all subjects and grades. The results instead show a significant negative association only for the grade/subject combination where Tomorrow-98 awards were associated with a change in computer use. Thus, the first- and second-stage estimates are consistent with a causal chain linking program computers to changes in computer use and, ultimately, to changes in achievement. Of course, it is impossible to prove that the 2SLS estimates have the interpretation we would like. As a further specification check, we therefore turn to a modified 2SLS strategy that exploits the Tomorrow-98 allocation mechanism directly. This strategy is robust to some of the sources of omitted variables bias that may affect the estimates in Table 5.

D. Non-linear instrumental variables

The 2SLS estimates discussed above may be biased if schools that received Tomorrow-98 computers differ in some way from those that didn't, even after controlling for observed covariates. As a further check on the previous results, we explored an instrumental variables strategy related to the regression-discontinuity method used recently by Angrist and Lavy (1999) to estimate the effects of class size on test scores. This method exploits the fact that, within towns, priority for Tomorrow-98 funding was determined largely on the basis of the towns' ranking of applicant schools. Although there is no sharp discontinuity in the relationship between ranking and funding, we can use the fact that funding is a nonlinear and non-monotonic function of rank to construct instruments for computer use while controlling for parametric functions of rank.

To motivate this approach, let r_s denote the school s rank on the list for the town where this school is located. That is, r_s=1 if the school is first on the priority list in the town, r_s=2 for the second school in the town, and so on, up to N_s, the number of schools on the town list. To adjust for the fact that the likelihood of being highly ranked may have varied with the number of applicants, we work with a normalized rank:

$$R_s = ((N_s+1-r_s)/N_s). \qquad (3)$$

Note that not all schools were ranked, i.e., some schools were deemed ineligible for program funds by the towns. For schools ineligible for funding, we set $r_s=N_s+1$ so $R_s=0$.[12] Thus, R_s ranges from 0 (ineligible for funds) to 1 (highest priority for funding). R_s provides a potential instrumental variable that can be used to identify the effects of Tomorrow-98 computers or CAI on outcomes. The identification in this case turns on the fact that $E[T_s| R_s]$ is a highly nonlinear and non-monotonic function of R_s. We can therefore control for linear and even polynomial functions of R_s while using $E[T_s| R_s]$ as an instrument for c_s.

What sort of omitted variables bias does this strategy mitigate? A concern with the 2SLS estimates discussed in the previous section is bias from correlation between T_s and unobserved school-level characteristics, represented by the error term, η_s. T_s can be viewed as determined by town rank, R_s, town size, N_s, and other school-level random factors, denoted by ν_s, that are likely correlated with η_s. These other (random) factors include the town-specific ranking threshold and anything else used by the town or central authorities to make allocation decisions. For example, the assignment mechanism could be modeled as $T_s = 1[h(R_s) > \nu_s]$. Note that necessarily, we have

$$R_s \perp (\eta_s - E[\eta_s| R_s, N_s]),$$

by iterated expectations. The town rank is therefore available as a potential instrument after controlling for

[12]We determined N_s by counting applicants in Tomorrow-98 program data provided by the Ministry of Education. The town ranking of schools is also reported in this file. In some cases the maximum rank recorded in the data falls short of the apparent number of applicants, probably because schools were incorrectly grouped or identified. In such cases we set schools deemed ineligible for funding (i.e., ranked by the town at 0) to have r_s = max(rank recorded for the town)+1. R_s is the ranking variable included as a control in the OLS and 2SLS estimates.

$E[\eta_s | R_s, N_s]$. This requires sufficient variation in the relationship between R_s and T_s. We therefore make the following identifying assumption:

A1. (i) $E[\eta_s | R_s, N_s] = g_p(R_s) + \delta_0 N_s$, where $g_p(R_s)$ is polynomial function of order p; (ii) The matrix formed from the columns $\{W_s\ g_p(R_s)\ N_s\ E[T_s | R_s]\}$ is of full column rank.

Given A1, the effect of interest is identified even if unobserved components of program award status (ν_s) are correlated with unobserved school-level determinants of test scores (η_s).

A natural estimator given assumption A1 is 2SLS using a modified version of equation (1), where the term $W_s'\gamma$ is augmented by inclusion of N_s and the control function, $g_p(R_s)$, which we take to be quadratic.[13] The resulting equation is:

$$y_{is} = W_s'\gamma + \delta_0 N_s + \delta_1 R_s + \delta_2 R_s^2 + X_i'\beta + c_s\alpha + \tilde{\eta}_s + \varepsilon_{is}, \tag{4}$$

where $\tilde{\eta}_s \equiv \eta_s - E[\eta_s | R_s, N_s]$. The quadratic function of R_s controls for possible effects of the ranking that operate through mechanisms other than the likelihood of receiving new computers.

Implementation of the non-linear IV strategy requires an estimate of $E[T_s | R_s]$ since this is unknown. Following an idea developed by Hahn, Todd, and van der Klaauw (2001) for a related problem, we use local linear regression to estimate this conditional expectation function nonparametrically. Hahn, Todd, and van der Klauuw (2001) incorporate prior information on the location of discontinuities in their nonparametric estimates. Since there are no discontinuities in our case, $E[T_s | R_s]$ was modeled using the entire support of R_s. In particular, we used the Cleveland (1979) local linear regression smoother to construct and estimate $\hat{E}[T_s | R_s]$, for every R_s.[14]

The population of Tomorrow-98 applicants was used to construct $\hat{E}[T_s | R_s]$, so the first-step fitted value can be treated as known for inference purposes. On the other hand, an important source of uncertainty

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[13]Results using linear and third-order polynomial controls were similar. As the degree of polynomial control increases, identification breaks down and the estimates become increasingly imprecise.

[14]The Cleveland (1979) estimator is called LOWESS (see, e.g. Fan and Gijbels, 1995). We use the version of this estimator implemented in Stata.

is the appropriate amount of smoothing when constructing fitted values. Because there is uncertainty about bandwidth, we experimented with a number of choices.

The estimated $\hat{E}[T_s | R_s]$ is plotted in Figure 1 for elementary schools and Figure 2 for middle schools. Both figures show estimates for bandwidth choices of .2, .3, and .4. As the bandwidth gets wider, the estimated $\hat{E}[T_s | R_s]$ gets smoother. At the other extreme, very narrow bandwidths lead to an estimator that interpolates every point. The points themselves, zeros and ones since T_s is binary, also appear in the figure.

Figures 1 and 2 both show that schools with normalized rank below about .7 were much less likely to receive Tomorrow-98 computers than schools with higher rankings. For ranks of .7 and higher, the likelihood of receiving computers increases steeply with rank, though it flattens out below ranks of .9 for elementary schools. Interestingly, schools given a very low ranking by municipal authorities (i.e, below about .2) are more likely to have been given computers than schools with ranks between .2 and .6. This is probably because Ministry of Education authorities over-ruled some low town-based rankings, apparently out of concern that towns' preferences over schools were influenced by local political considerations.

Figures 3 and 4 show local linear regression estimates of the relationship between the normalized town ranking and test scores, parallel to those in Figures 1 and 2 (using a bandwidth of .4). The top half of Figure 3, for 4th grade Math scores, exhibits a pattern that is in some respects the mirror image of Figure 1. In particular, test scores begin to fall with rank for towns with ranks above about .7. Although there is some evidence of a decline for 4th grade Hebrew scores, the pattern is less clear cut than for Math scores, consistent with the insignificant but negative estimates for Hebrew scores in Tables 4 and 5. For 8th graders, however, the only semblance of a pattern is slightly lower scores for low ranked schools and slightly higher scores for highly ranked schools. Both groups were more likely to receive Tomorrow-98 computers, so this pattern may be due to chance. Neither figure shows strong up or down "trend variation" in scores with rank.

The non-linear instrumental variables estimates are broadly consistent with the 2SLS estimates reported in Table 5. This can be seen in Table 6, which reports estimates for 4th graders using three bandwidth choices in the first-stage. As before, the clearest results are for 4th grade Math scores, with estimates ranging mostly around .2 standard deviations in samples of applicants and applicants with lagged scores. One of the estimates in column 2 is marginally significant. Estimates for 4th grade Hebrew scores are also mostly negative, though none are significant. The estimates in column 3 of the table are based on a sample limited to pupils in schools that had a normalized rank above .5. These estimates involve a comparison that exploits variation in $\hat{E}[T_s | R_s]$ close to the level where the probability of receiving computers sharply increased. This limited sample may lead to better control for any omitted R_s effects. In practice, these results are larger in magnitude and less precise than the other results.

IV. Conclusions

Israel's Tomorrow-98 program provides a unique opportunity to assess the short-run consequences of increased computer technology in schools. The program had a clear impact on the use of computers in elementary school instruction, with a much weaker effect on teaching methods in middle schools. This is in spite of the fact that program operators hoped to promote the use of CAI at higher grade levels, where it is generally less pervasive. The results reported here do not support the view that CAI improves learning, at least as measured by pupil test scores. Using a variety of estimation strategies, we find a consistently negative and marginally significant relationship between the program-induced use of computers and 4th grade math scores. For other grades and subjects, the estimates are not significant, though also mostly negative. And simpler OLS strategies generate only one significant estimate for the relationship between CAI and test scores, a negative effect of CAI on 8th grade math scores in models with town effects.

A possible explanation for our findings is that CAI is no better and may even be less effective than other teaching methods. Alternately, CAI may have consumed school resources or displaced educational activities which, had they been maintained, would have prevented a decline in achievement. Our teacher survey included questions that we used to explore possible program-related changes in teaching methods and educational inputs. As noted earlier, we found no evidence of a significant change in educational inputs, instructional methods, or teacher training in Tomorrow-98 schools. This suggest there was no displacement.

On the other hand, while tomorrow-98 included a training component, CAI strategies implemented with a large increase in teacher training may prove to be more effective than the Tomorrow-98 program, though also more costly.

Another possible explanation for the findings reported here is that the transition to CAI is disruptive, and any benefits of CAI take time develop. The schools in our sample had Tomorrow-98 computers for an average of one full school year. This may not be long enough for any benefits to appear. Also relevant for an overall assessment are any spillovers from the use of CAI on computer skills for which there is a direct payoff. The computer-skills benefit may not be reflected in Math and language scores. It should be emphasized, however, that the results reported here show that enough time had passed by the test date for the new computers to have had a large and statistically significant impact on instructional methods for 4th graders. Although other issues are also important, the short-term impact of this change is of immediate policy interest. At a minimum, this short-run decline in test scores is an extra hurdle to overcome if the transition to CAI is ultimately to be justified by pupil achievement.

Finally, an important feature of Israel's computerization program, and an element that is by no means unique to Israel, is the large cost of a broad move to CAI. As noted in the introduction, the Tomorrow-98 program deployed about 35,000 computers in the first three years of the program. The Ministry of Education budgets this deployment at $3,000 per machine, include the cost of hardware, software, and set-up (but not including wiring or other physical infrastructure). Program schools received an average of about 40 computers, for a cost of $120,000 per school. In Israel, this amount would pay the wages of up to 4 teachers. Assuming a depreciation rate of 25% on hardware and software and ignoring any training costs, the flow cost of the computers is about one teacher per year per school.

Recent years have seen similarly ambitious computerization efforts in US schools, where education technology is thought to have cost 5.2 billion dollars in 1998, and the proportion of elementary school classrooms with internet access jumped from 30 percent in 1994 to 75 percent in 1997 (National Center for Education Statistics, 1998). The question of future impacts remains open, but this significant and ongoing expenditure on education technology does not appear to be justified by pupil performance results to date. In addition to the evidence presented here, our skeptical view of the value of expenditure on education technology is reinforced by our earlier findings using Israeli data (reported in Angrist and Lavy, 1999, 2001) suggesting traditional inputs -- reductions in class size and increased teacher training -- do have substantial achievement benefits. Although the labor market consequences of educational expenditures is difficult to assess, these results have clear implications for education production isoquants. On balance, it seems, money spent on CAI in Israel would have been better spent on other inputs.

APPENDIX

1. Data

A. Test score data

Four data files from the Ministry of Education contain the pupil's characteristics and test scores (in Math and Hebrew, for 4th and 8th grade) from the June 1996 national testing program. These files were given to us by the Ministry of Education. Our analysis is limited to the Jewish schools in the sample. The 4th grade Math sample included 213 schools (5584 pupils). The 8th grade Math sample included 177 schools (4172 pupils). The 4th grade Hebrew sample included 209 schools (5466 pupils). The 8th grade Hebrew sample included 176 schools (4695 pupils).

B. *Computer-use intensity data*

The June 1996 testing program included a brief survey given to all teachers of each sampled class. This survey included a question on the intensity of computer use in the classroom. Teachers are identified as Math or Hebrew teachers. Fourth grade pupils were assigned the answers of their (unique) teacher. For the 8th Hebrew sample, there are up to four different teachers who taught the same class different Hebrew-related subjects. In such cases we assigned pupils the answers of their reading teacher.

Teachers' answers on the intensity of computer use were non-missing for 183 schools (4833 pupils) in the 4th grade Math sample, 142 (3290 pupils) schools in the 8th grade Math sample, 166 schools (4180 pupils) in the 4th grade Hebrew sample and for 140 schools (3675 pupils) in the 8th grade Hebrew sample. The observation counts were further reduced to those in Table 1 because of missing data on other variables.

C. *Data on Tomorrow-98 applicants*

The Ministry of Education provided a file containing information on the 1994 applicants to the Tomorrow-98 program and their ranking within municipalities. This file was merged with pupil test scores data using the school id. The 4th and 8th grade applicant Math samples with test score data included 146 schools. The 4th and 8th grade applicant Hebrew samples with test score data included 144 and 140 schools, respectively.

The Ministry of Education also provided files with information on the Tomorrow implementation schedules and existing computer infrastructure (collected for the purposes of this evaluation), along with other school level variables, such as the Pupil Disadvantage index, school size, town code and type of school (secular or religious). We were able to match all of the pupils in the table above to this school-level data.

D. *Lagged test score and computer use data*

Lagged scores for 4th grade Math and Hebrew scores were obtained from the 1991 national test program data used by Angrist and Lavy (1999). Lagged scores were available for 189 schools in the 4th grade Math sample (of which 131 were program applicants), for 130 schools in the 8th grade Math sample (110 program applicants), for 188 schools in the 4th grade Hebrew sample (150 program applicants) and for 119 schools in the 8th grade Hebrew sample (97 program applicants). The estimates controlling for lagged computer use in Table 5 also rely on matched data for a subsample of schools from the 1991 testing program. The data come from a survey of teachers that was done along with the 1991 testing. The lagged use variable in our analyses measures the proportion of teachers at each school in 4th grade using computers for instruction in 1991. The 8th grade lagged use variable is the average lagged use dummy for 4th grade in elementary schools that feed the relevant middle schools.

2. 2SLS estimates of ordinal-response models

To simplify notation, we drop subscripts indexing individuals and schools, and use the upper case to denote random variables with the same distribution as for a randomly chosen pupil or school. Suppose that a pupil would have average test score Y_j when exposed to intensity level j, where j can take on values 0-3. Y_j is a potential outcome; that is, we imagine that for each pupil all of the elements of $\{Y_0, Y_1, Y_2, Y_3\}$ are well-defined, though only one is ever observed. The average causal effect of increasing intensity by one unit is $E[Y_j - Y_{j-1}]$. We could learn about these average effects in an experiment where pupils are randomly exposed to different intensities. Similarly, let C_t be the potential intensity that would be realized when the binary instrument T equals t, for t=0,1 The difference in means, $E[C|\ T=1]-E[C|\ T=0] = E[C_1-C_0]$, is the average causal effect of T on CAI intensity in a randomized trial.

The empirical work is motivated by a model where potential outcomes vary with intensity according to a linear model that is the same for all pupils, but this is almost certainly not an accurate description of the causal effect of changing computer use. Angrist and Imbens (1995) discuss the interpretation of linear IV estimators in models where the underlying causal response function is both heterogeneous and nonlinear. The simplest characterization is for a Wald estimator, i.e., using T as an instrument for a regression of Y on C with no covariates. Extensions are conceptually straightforward, though the notation is more involved.

The Wald estimator using T as an instrument can be written in terms of potential outcomes as:

$$\frac{E[Y|\ T=1] - E[Y|T=0]}{E[C|\ T=1] - E[C|T=0]} = \frac{\sum E[Y_j - Y_{j-1}|\ C_1 \geq j > C_0]P[C_1 \geq j > C_0]}{\sum P[C_1 \geq j > C_0]} , \quad \text{(A2.1)}$$

where the summation is from j=1 to j=3. Formula (A2.1) describes the sense in which 2SLS captures an average causal response. This interpretation applies to Wald estimates of causal effects for any ordered treatment of variable intensity, provided the intensities satisfy $C_1 \geq C_0$ for all pupils ("monotonicity").

Table A1 reports the marginal distribution of CAI intensity for Math and Hebrew. The weighting function underlying the average causal effect in (A2.1) is described in text Table 4, which reports the impact of T on the distribution of C. For example, the effect on the probability CAI $\geq$ 1 is an estimate of $P[C_1 \geq 1 > C_0]$. Using 4th grade Math data, the table shows significant positive weights for effects of this kind at j=1, 2, and 3, for 4th grade Hebrew data, the intensity distribution is shifted only for j=1.

Suppose now that instead of using C as the endogenous regressor, we use a single dummy indicating CAI intensity greater than j as the endogenous regressor. Denote this regressor by $d(j) \equiv 1[CAI \geq j]$. Using the fact that $P[C_1 \geq j > C_0]$ is a difference in CDFs, we have:

$$E[C|\ T=1] - E[C|\ T=0] = \textstyle\sum_j P[C_1 \geq j > C_0].$$

Now, since

$$E[d(j)|\ T=1] - E[d(j)|\ T=0] = P[C_1 \geq j > C_0],$$

it follows immediately that IV estimates using *d(j)* as the endogenous will generally be "too big" in the sense that they over-estimate the causal effect of a unit increase in intensity. The scaling factor is,

$$\{\textstyle\sum_j P[C_1 \geq j > C_0]\} / \textstyle\sum_j P[C_1 \geq j > C_0] \equiv \varphi \geq 1.$$

This equals 1 only if the instrument shifts the distribution of CAI intensity at a single point (as appears to be true for the impact on CAI use for Hebrew). For fourth grade math scores, however, estimates using *d(1)* as the endogenous regressor can expected to be approximately 2-3 times as large as the 2SLS estimates treating C as the endogenous regressor. This is confirmed in Table A2, which reports estimates using alternate dummy-variable specifications, comparable to those reported in Table 5.

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Table 1: Descriptive Statistics

Variables	4th Grade						8th Grade					
	Math			Hebrew			Math			Hebrew		
	Full	Applicants	Applicants with lagged scores	Full	Applicants	Applicants with lagged scores	Full	Applicants	Applicants with lagged scores	Full	Applicants	Applicants with lagged scores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Average scores	69.0	68.2	68.1	68.0	67.3	67.2	57.6	57.1	57.5	65.1	65.5	64.8
	(19.9)	(20.2)	(20.1)	(19.8)	(20.2)	(20.2)	(20.0)	(19.9)	(20.0)	(19.1)	(18.9)	(19.2)
Any Computer use	.494	.556	.534	.402	.422	.408	.283	.300	.286	.242	.264	.306
(CAI≥1)	(.500)	(.497)	(.499)	(.490)	(.494)	(.352)	(.450)	(.459)	(.452)	(.428)	(.441)	(.461)
Computer-use ranking	.851	.929	.898	.775	.837	.791	.437	.440	.413	.424	.456	.539
(CAI intensity)	(.970)	(.941)	(.950)	(1.03)	(1.08)	(1.05)	(769)	(.743)	(.728)	(.834)	(.848)	(.906)
Tomorrow-98 (T-98)	.115	.168	.181	.092	.139	.145	.445	.523	.501	.448	.530	.495
	(.319)	(.374)	(.385)	(.290)	(.346)	(.352)	(.497)	(.500)	(.500)	(.497)	(.499)	(.500)
Months with T-98	-	9.36	9.57	-	8.87	9.16	-	12.7	13.1	-	12.5	12.9
	-	(6.95)	(6.68)	-	(6.68)	(6.84)	-	(4.56)	(4.48)	-	(4.50)	(4.29)
Female	.498	.502	.503	.521	.536	.537	.521	.533	.567	.535	.544	.547
	(.500)	(.500)	(.500)	(.499)	(.499)	(.499)	(.499)	(.499)	(.496)	(.498)	(.498)	(.498)
Immigrant	.056	.063	.062	.054	.063	.063	-	-	-	.044	.038	.042
	(.231)	(.242)	(.240)	(.227)	(.242)	(.244)	-	-	-	(.207)	(.192)	(.200)
Pupil disadvantage	-.007	.084	.103	-.062	.010	.016	.073	.067	.060	.060	.034	.031
index	(.558)	(.569)	(.582)	(.514)	(.543)	(.553)	(.638)	(.673)	(.663)	(.633)	(.654)	(.662)
Special education	.131	.135	.140	.128	.135	.140	-	-	-	.091	.092	.096
	(.337)	(.342)	(.347)	(.334)	(.342)	(.347)	-	-	-	(.287)	(.289)	(.294)
Verbal scores 91	-	-	71.5	-	-	72.8	-	-	70.8	-	-	71.2
	-	-	(7.79)	-	-	(7.19)	-	-	(6.81)	-	-	(6.57)
Math scores 91	-	-	67.5	-	-	68.9	-	-	67.6	-	-	68.2
	-	-	(8.28)	-	-	(8.18)	-	-	(6.94)	-	-	(6.64)
Early computers	.443	.446	.469	.440	.453	.476	.602	.612	.633	.590	.601	.615
(sophisticated)	(.496)	(.497)	(.499)	(.496)	(.498)	(.500)	(.489)	(.487)	(.482)	(.491)	(.490)	(487)
Early computers	.078	.097	.110	.077	.092	.104	.057	.048	.038	.054	.037	.022
(simple)	(.268)	(.296)	(.313)	(.267)	(.289)	(.305)	(.232)	(.214)	(.192)	(.226)	(.190)	(.145)
N	4779	3271	2891	3689	2464	2194	3196	2620	2145	3182	2593	2135

Notes: The average score is percent right. Computer use intensity ranking =0 if teacher never uses computer, =1 if sometimes, =2 if often, =3 if always. T-98=1 if the school received computers through the Tomorrow project. Months with T-98 is only for those schools that participated in the Tomorrow project. Pupil disadvantage index (mean zero, standard deviation=1) is a weighted average of parental schooling, family size, family income, percent immigrant students, distance of school from a large Urban area (larger is worse). Standard deviations are reported in parenthes

Table 2: Differences by Computer Use (C1) and Tomorrow-98 (T-98) program status

Variables	4th Grade						8th Grade					
	Math			Hebrew			Math			Hebrew		
	Mean (1)	Dif. by CAI ≥ 1 (2)	Dif. by T-98 (3)	Mean (4)	Dif. by CAI ≥ 1 (5)	Dif. by T-98 (6)	Mean (7)	Dif. by CAI ≥ 1 (8)	Dif. by T-98 (9)	Mean (10)	Dif. by CAI ≥ 1 (11)	Dif. by T-98 (12)
A. All Applicants												
Any Computer Use	.556	-	.240	.422	-	.451	.300	-	.003	.264	-	.020
(CAI≥1)	(.497)	-	(.122)	(.494)	-	(.139)	(.459)	-	(.090)	(.441)	-	(.097)
Computer-use ranking	.929	-	.558	.837	-	.708	.440	-	-.090	.456	-	-.023
(CAI intensity)	(.941)	-	(.225)	(1.08)	-	(.318)	(.743)	-	(.142)	(.848)	-	(.184)
Female	.502	-.027	.056	.536	.078	.047	.533	-.076	-.046	.544	.008	.061
	(.500)	(.039)	(.050)	(.499)	(.039)	(.057)	(.499)	(.055)	(.053)	(.498)	(.063)	(.054)
Immigrant	.063	.005	-.012	.063	.002	.019	-	-	-	.038	-.014	-.004
	(.242)	(.012)	(.017)	(.242)	(.014)	(.020)	-	-	-	(.192)	(.012)	(.012)
Pupil disadvantage	.084	.112	.150	.010	.297	.358	.067	.262	-.389	.034	.138	-.203
index	(.569)	(.102)	(.133)	(.543)	(.107)	(.156)	(.673)	(.135)	(.121)	(.654)	(.178)	(.140)
Special education	.135	.031	-.001	.135	-.017	.040	-	-	-	.092	-.018	-.035
	(.342)	(.016)	(.022)	(.342)	(.021)	(.030)	-	-	-	(.289)	(.021)	(.019)
Early comp/sophisticate	.446	.157	.115	.453	.295	.180	.612	-.246	.211	.601	-.007	.222
	(.497)	(.090)	(.121)	(.498)	(.104)	(.150)	(.487)	(.097)	(.094)	(.490)	(.106)	(.101)
Early comp/simple	.097	.010	-.051	.092	.032	-.025	.048	.064	-.012	.037	-.013	-.042
	(.296)	(.055)	(.061)	(.289)	(.065)	(.091)	(.214)	(.043)	(.038)	(.190)	(.040)	(.040)
N	3271			2464			2620			2593		
B. Applicants with lagged scores												
Verbal scores 91	71.5	-1.78	.367	72.8	-1.34	-1.6	70.8	-.213	3.53	71.2	1.40	2.10
	(7.79)	(1.48)	(1.93)	(7.19)	(1.51)	(2.16)	(6.82)	(1.59)	(1.40)	(6.57)	(1.58)	(1.50)
Math scores 91	67.5	-1.46	-.260	68.9	-1.46	-2.10	67.6	2.08	2.10	68.2	2.01	1.30
	(8.28)	(1.59)	(2.08)	(8.18)	(1.81)	(2.51)	(6.94)	(1.59)	(1.46)	(6.64)	(1.64)	(1.54)
N	2891			2194			2145			2135		

Notes: See notes to Table 1 for variable definitions. The columns labeled "dif. By C1" show differences in covariate means by whether computers are used at all for instruction. The columns labeled "dif. by T-98" show differences in covariate means by whether Tomorrow-98 computers were received. Standard deviations reported in parentheses for levels. Standard errors are reported in parentheses for differences. The standard errors for differences are corrected for school-level clustering.

Table 3: OLS Estimates of the Effect of CAI Intensity

Grade	Regressor	Math			Hebrew		
		Applicants	Applicants with lagged scores	Town fixed effects: Full sample with lagged score	Applicants	Applicants with lagged scores	Town fixed effects: Full sample with lagged score
		(1)	(2)	(3)	(4)	(5)	(6)
4th	CAI ≥ 1	.045	.069	-.005	-.012	-.018	.031
		(.068)	(.072)	(.056)	(.063)	(.067)	(.056)
	CAI ≥ 2	.105	.080	-.010	-.008	-.0004	.003
		(.072)	(.076)	(.074)	(.066)	(.068)	(.059)
	CAI ≥ 3	.194	.193	.187	-.142	-.126	-.077
		(.174)	(.168)	(.137)	(.100)	(.109)	(.285)
	CAI Intensity	.047	.047	.007	-.016	-.007	.009
		(.035)	(.038)	(.034)	(.028)	(.031)	(.030)
	N	3271	2891	2947	2464	2194	2237
8th	CAI ≥ 1	.037	-.055	-.267	.072	-.017	-.063
		(.092)	(.100)	(.138)	(.073)	(.073)	(.062)
	CAI ≥ 2	.168	.176	-.111	.037	-.008	-.064
		(.133)	(.147)	(.182)	(.094)	(.086)	(.077)
	CAI ≥ 3	-.396	-.873	-.715	.205	.203	.281
		(.356)	(.338)	(.254)	(.163)	(.149)	(.143)
	CAI Intensity	.039	-.0014	-.136	.038	.006	-.014
		(.059)	(.064)	(.070)	(.039)	(.037)	(.032)
	N	2621	2145	1883	2593	2135	1910
Other included controls							
Pre-existing computers		X	X	X	X	X	X
Basic controls		X	X	X	X	X	X
1991 test scores			X	X		X	X
Town effects				X			X
Town Rank		X	X		X	X	

Notes: Row entries are for separate models, each with the covariates listed. Basic controls: Female, Immigrant, Special education, Pupil disadvantage index, total school enrollment. Models for 8th graders also include controls for school types (grades K-8, 7-9). Lagged test scores: For 4th graders, these are school average of scores for 4th grades in 1991. For 8th graders, these are the average of 4th and 5th grade scores in 1991 for the elementary schools that feed these middle schools. The samples used for columns 3 and 6 are not limited to applicants. These samples include all pupils in towns with at least two schools and with data on lagged test scores. Standard errors are reported in parentheses. The standard errors are corrected for school-level clustering.

Table 4: Reduced-Form Program Effects

Grade	Subject	Controls	CAI Indicators: CAI ≥ 1 (1)	CAI ≥ 2 (2)	CAI ≥ 3 (3)	CAI Intensity (4)	Score (5)
4th	Math	Basic Controls	.234	.282	.083	.599	-.204
			(.121)	(.116)	(.044)	(.224)	(.089)
		With Lagged Score	.228	.252	.083	.563	-.241
			(.120)	(.115)	(.049)	(.227)	(.088)
	Hebrew	Basic Controls	.335	.116	-.005	.446	-.052
			(.134)	(.136)	(.094)	(.310)	(.088)
		With Lagged Score	.285	.052	.015	.352	-.079
			(.131)	(.134)	(.087)	(.291)	(.088)
8th	Math	Basic Controls	.118	.015	-.014	.118	-.080
			(.098)	(.069)	(.022)	(.152)	(.095)
		With Lagged Score	.104	.001	-.018	.087	-.051
			(.103)	(.069)	(.022)	(.157)	(.096)
	Hebrew	Basic Controls	.043	-.068	.071	.046	.055
			(.102)	(.082)	(.043)	(.400)	(.072)
		With Lagged Score	.080	-.056	.101	.125	.070
			(.111)	(.097)	(.053)	(.224)	(.072)

Notes: Basic Controls and lagged test scores: as in Table 3. The standard errors are corrected for school-level clustering.

Table 5: 2SLS Estimates of the Effects of CAI Intensity for 4th Graders

	Applicants		Applicants with lagged scores		T-98 / will get T-98 with lagged scores		Control for lagged computer use		Use dummy instruments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Math										
CAI intensity	-.340	-.341	-.427	-.435	-.417	-.427	-.309	-.317	-.236	-.244
	(.214)	(.212)	(.252)	(.245)	(.251)	(.251)	(.187)	(.184)	(.106)	(.106)
Over-id test(df)									8.8 (12)	8.8 (12)
N	3271		2891		2035		2430		2891	
B. Hebrew										
CAI intensity	-.116	-.134	-.224	-.265	-.208	-.284	-.064	-.079	-.104	-.128
	(.208)	(.194)	(.307)	(.279)	(.214)	(.255)	(.168)	(.139)	(.086)	(.085)
Over-id test(df)									15.2 (9)	14.7 (9)
N	2464		2194		1496		1823		2194	
Other included controls										
Pre-existing computers	X		X		X		X		X	
Basic controls	X		X		X		X		X	
1991 test scores			X		X		X		X	
Computer usage in 1991							X			
Town Rank	X		X		X		X		X	

Notes: The endogeneous regressor is the 0-3 CAI intensity ranking. Basic controls and lagged test scores as in Table 3. The samples in columns 3 are limited to pupils in schools that received T-98 funding, including those that received funding after the June 1996 test date (as of 1998). The instrument for all columns except 5 is a T-98 program dummy. The instruments in columns 5 are up to 14 dummies for months of program operation. Standard errors are reported in parentheses. The standard errors are corrected for school-level clustering.

Table 6: Non-linear IV Estimates for 4th Graders

		Sample		
	Bandwidth	Applicants (1)	Applicants with lagged scores (2)	Town rank>.5 with lagged scores (3)
A. Math				
CAI intensity	0.2	-0.151	-0.266	-0.588
		(.131)	(.170)	(.262)
	0.3	-0.121	-0.214	-0.629
		(.121)	(.142)	(.310)
	0.4	-0.142	-0.212	-0.572
		(0.119)	(0.125)	(.263)
N		3271	2891	1550
B. Hebrew				
CAI intensity	0.2	-0.088	-0.202	3.024
		(.189)	(.329)	(18.248)
	0.3	-0.074	-0.153	2.930
		(0.145)	(.248)	(12.310)
	0.4	-0.060	-0.118	-4.401
		(.112)	(.165)	(27.243)
N		2464	2194	1281
Other included controls				
Pre-existing computers		X	X	X
Basic controls		X	X	X
1991 test scores			X	X

Notes: The table reports IV estimates using the predicted probability of receiving T-98 program support as an instrument. The predicted probability is a nonparametrically estimated function of the normalized town rank for funding. Estimates use the bandwidth shown. All models control for a quadratic function of the normalized rank and for the number of applicants in the town. Basic controls: as in Table 3. The samples in columns 3 are limited to pupils in schools with normalized town ranking for Tomorrow-98 funding above .5. Standard errors are reported in parentheses. The standard errors are corrected for school-level clustering.

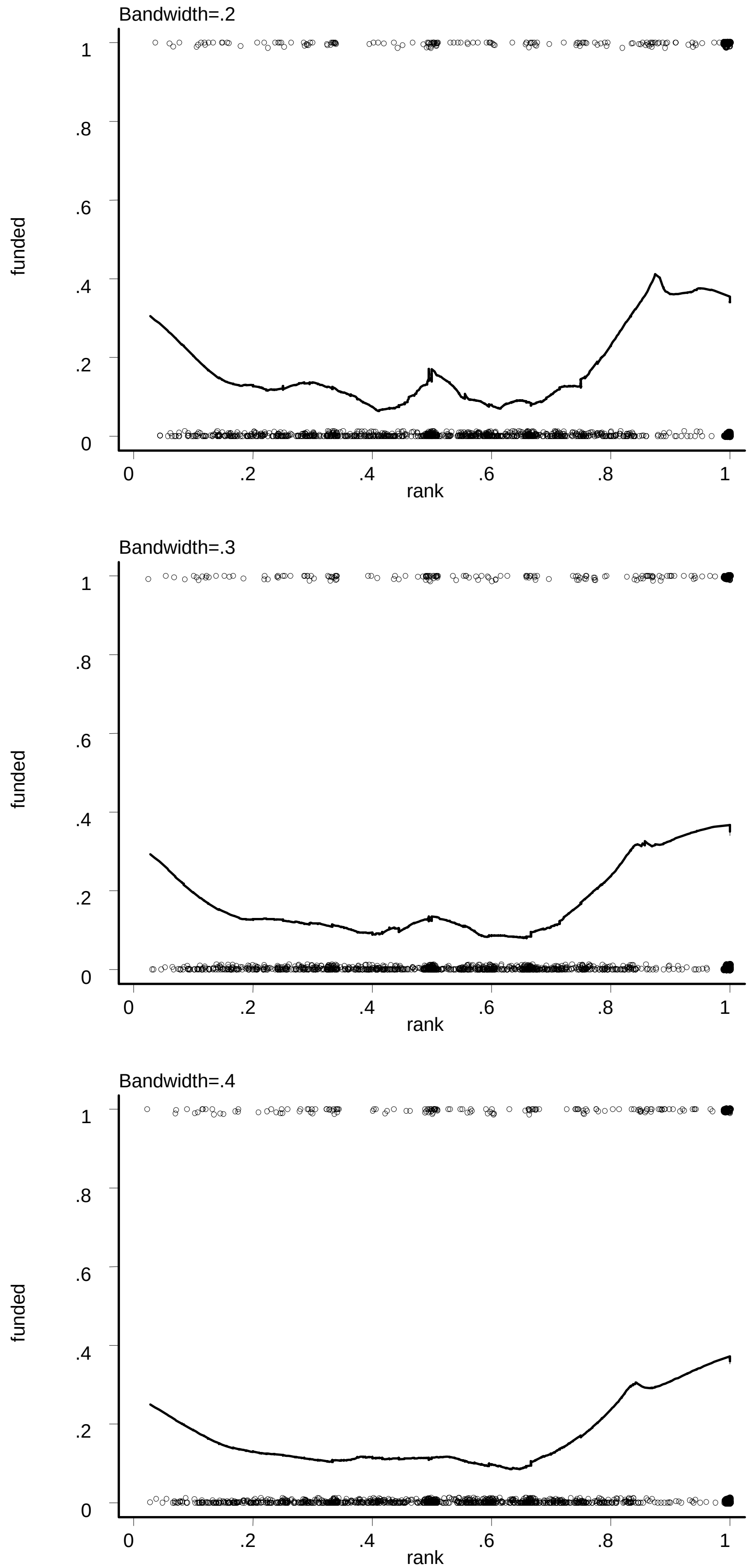


Figure 1. The relationship between within-town rank and the probability of funding for elementary schools.

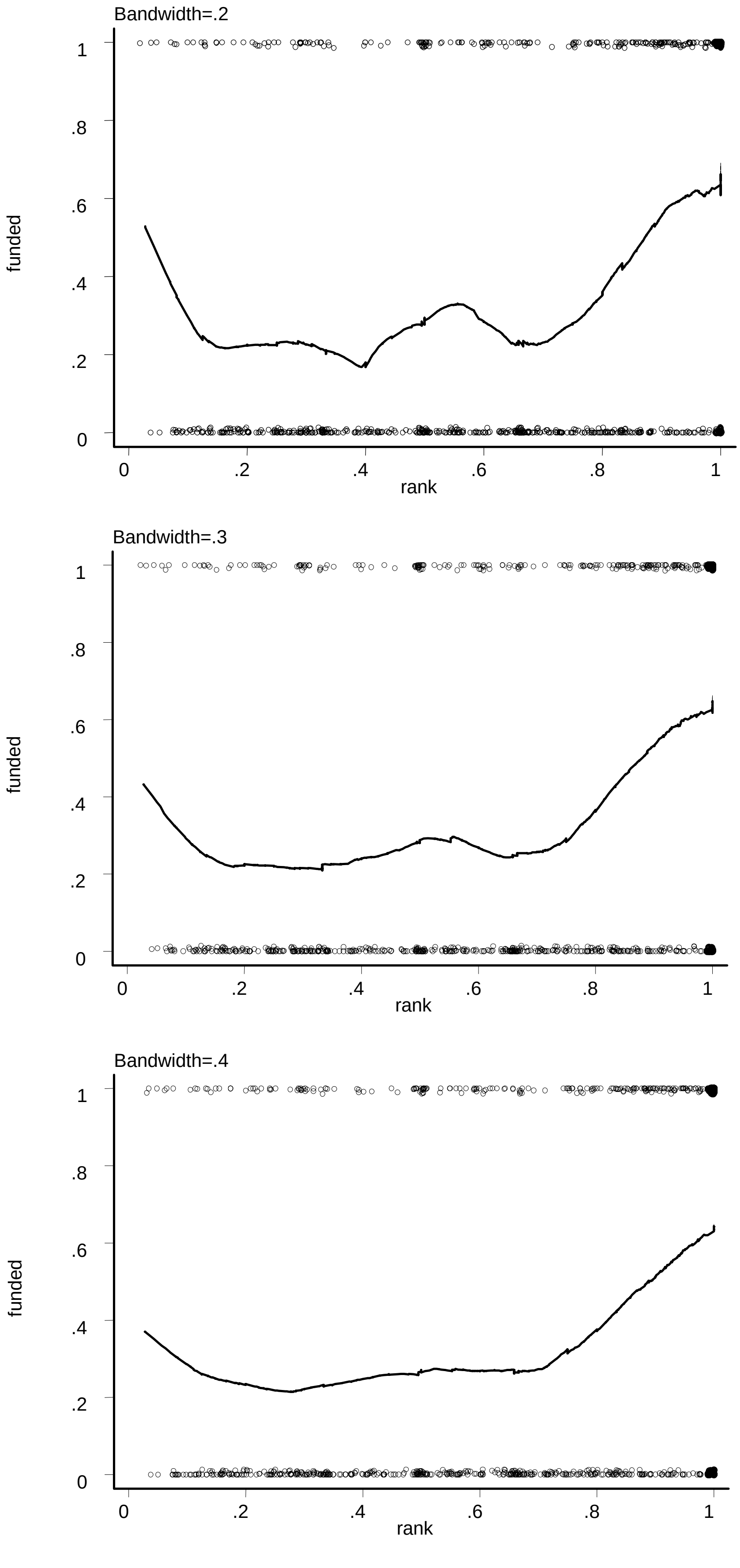
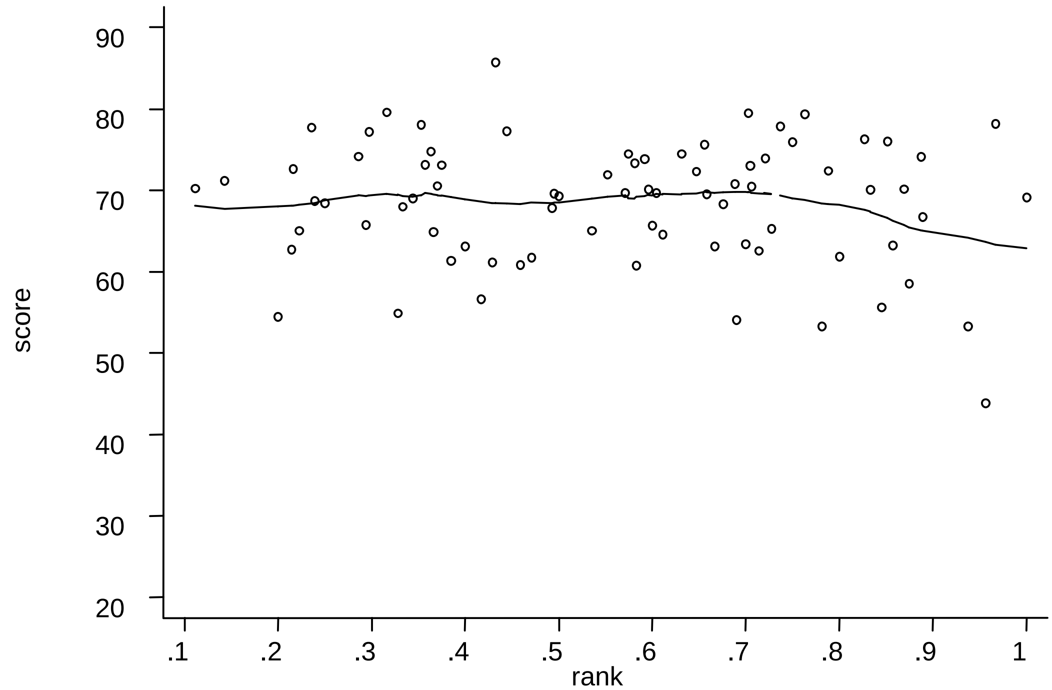


Figure 2. The relationship between within-town rank and the probability of funding for middle schools.

4th Grade Math



4th Grade Hebrew

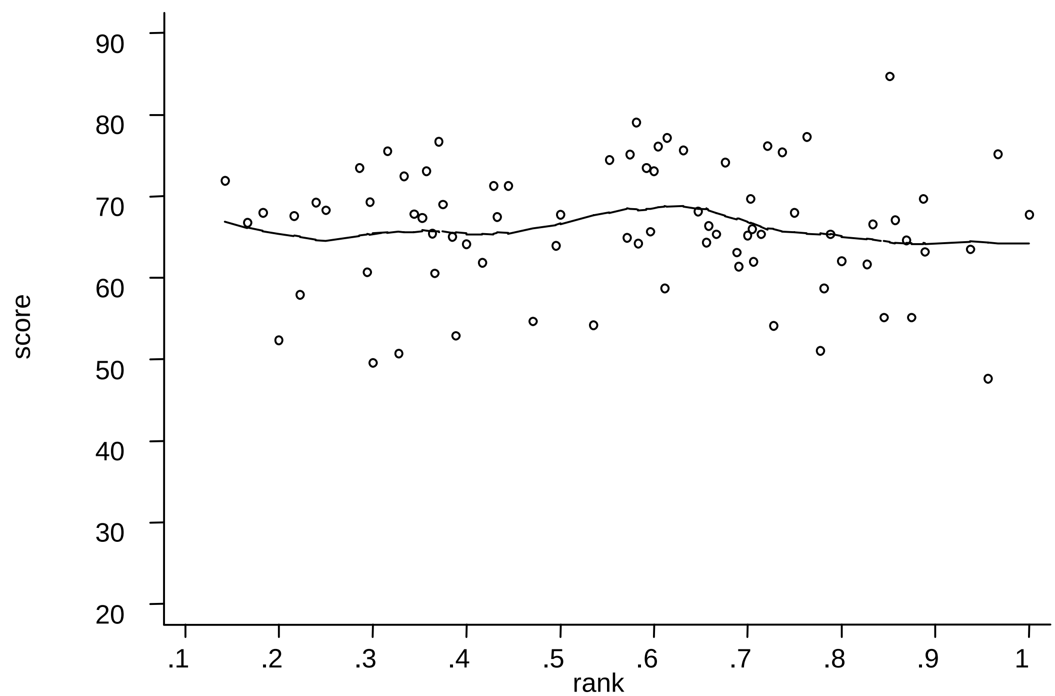
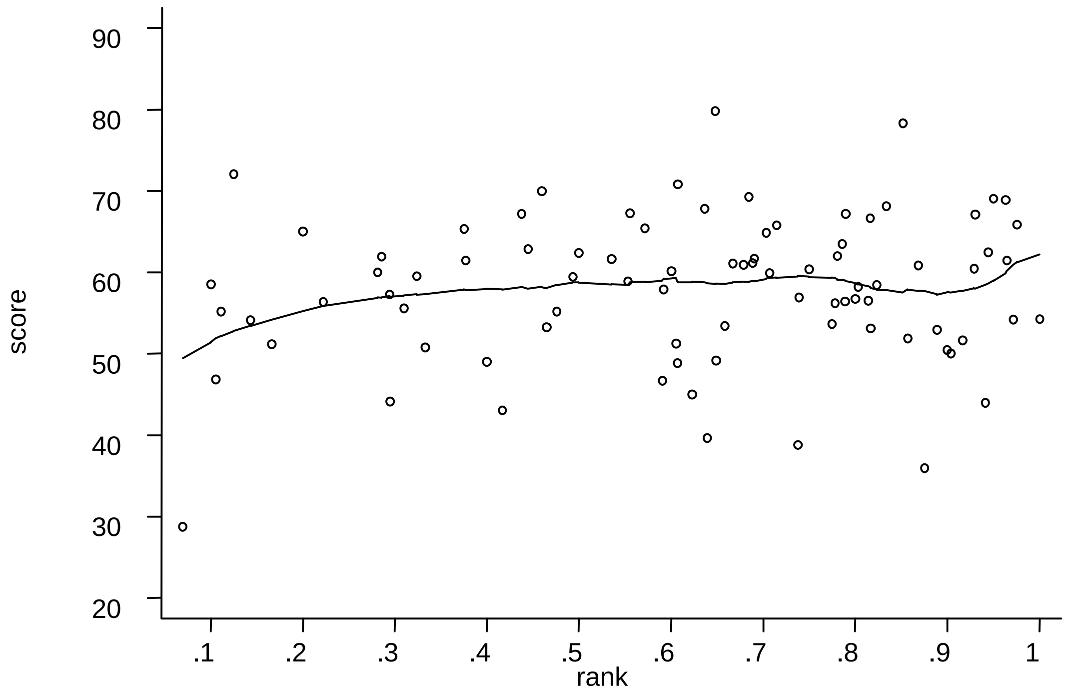


Figure 3. The relationship between within-town rank and test scores for 4th graders, bandwidth=.4.

8th Grade Math



8th Grade Hebrew

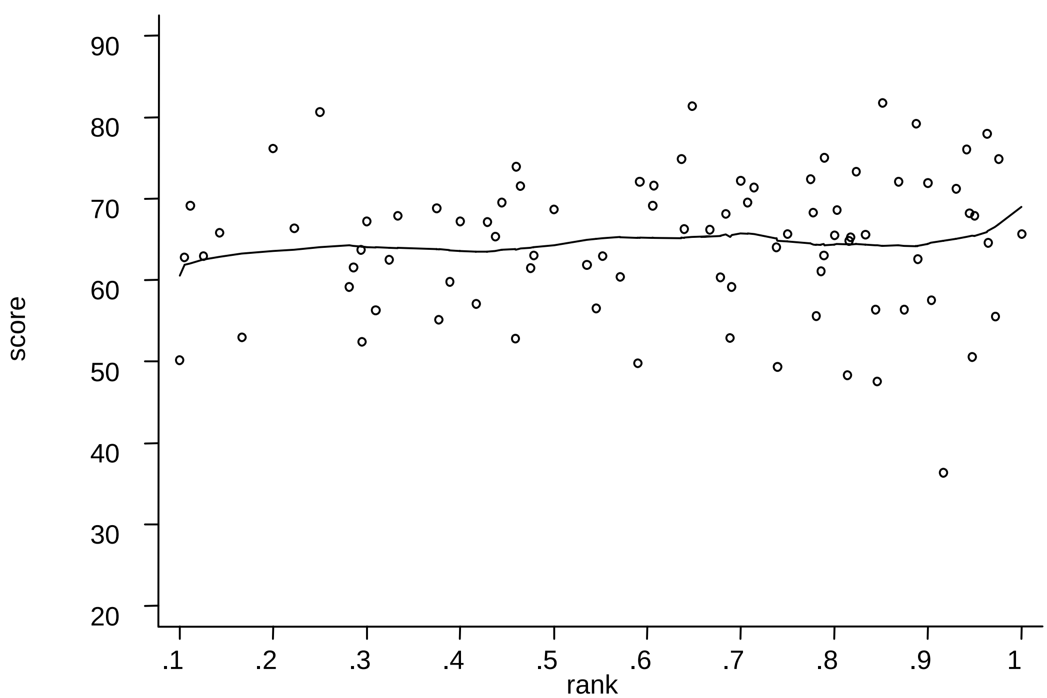


Figure 4. The relationship between within-town rank and test scores for 8th graders, bandwidth=.4.

Table A1: Computer Use Intensity Ranking (CAI) Distribution

Sample	Math				Hebrew			
	CAI = 0	CAI = 1	CAI = 2	CAI = 3	CAI = 0	CAI = 1	CAI = 2	CAI = 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
4^{th} Grade Applicants	44.3	21.8	30.1	3.61	57.8	10.8	21.1	10.2
4^{th} Grade Applicants with lagged scores	46.6	21.0	28.3	4.08	59.2	11.0	21.3	8.52
8^{th} Grade Applicants	67.0	17.4	11.3	1.34	73.6	11.6	10.5	4.36
8^{th} Grade Applicants with lagged scores	71.4	17.5	9.46	1.63	69.4	12.6	12.7	5.29

Notes: The table reports the percent distribution of the intensity ranking.

Table A2: OLS and 2SLS Estimates of the Effects of the CAI Intensity on 4th Grade Math Scores

Estimate	CAI Indicators		
	CAI ≥ 1 (1)	CAI ≥ 2 (2)	CAI ≥ 3 (3)
A. Applicants			
OLS	.045	.105	.194
	(.068)	(.072)	(.174)
2SLS	-.871	-.723	-2.457
	(.621)	(.471)	(1.742)
B. Applicants with lagged scores			
OLS	.069	.080	.193
	(.072)	(.076)	(.168)
2SLS	-1.050	-.954	-2.883
	(0.735)	(0.595)	(2.016)

Notes: Standard error are reported in parentheses. The standard errors are corrected for school-level clustering.

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