Expanding School Resources and Increasing Time on Task:

Effects on Students' Academic and Non-Cognitive Outcomes*

August 2018

Victor Lavy

University of Warwick, Hebrew University of Jerusalem, and NBER

Abstract

This paper uses a natural experiment in Israel to assess the impact of school teaching resources and how it is used, 'time-on-task', on academic achievements and non-cognitive outcomes. It exploits variation induced by a change in the funding formula that reduced instructional resources funding for some schools and increased them for others. The results suggest that increased school resources and students' spending more time at school and on key tasks all lead to increased academic achievements with no behavioral costs. Separate estimations of the effect of increasing subject-specific instructional time per week also show positive and significant effects on math, science, and English test scores and small and non-significant effects on Hebrew test scores. However, there are no cross effects of additional instructional time across subjects. This evidence is robust to using different identification strategies. The evidence also shows that a longer school week increases the time that students spend on homework without reducing social and school satisfaction and without increasing school violence.

msvictor@huji.ac.il
v.lavy@warwick.ac.uk
JEL No. I21,J18,J24

* Special thanks go to Adi Shani, Asaf Kott, Genia Rachkovski and Boaz Abramson for outstanding research assistance. I benefited from comments and suggestions by Arthur Blouin, S. Dynarski, Brian Jacob, Karen Brandon, Michael Freedman, Daniele Paserman, Yona Rubinstein, Tali Regev, Analia Schlosser, four anonymous referees of this journal and seminar participants at Hebrew University, ITAU conference in Rio de Janeiro, LSE, Spain Ministry of Education in Madrid, National University of Singapore, Paris School of Economics, Tel Aviv University, University of Barcelona, and University of Warwick. I acknowledge financial support for this project from the European Research Council through ERC Advance Grant 323439, CAGE at Warwick and the Falk Institute.

1. Introduction

Questions about how students' academic achievements are affected by school instructional resources, such as the amount of time they spend in school and the amount of time they devote to specific subjects, are of compelling interest for policymakers. Research on these questions is important since marginally increasing instructional time is relatively simple to do, and the potential to make such changes would seem to be possible in many countries. This is reflected in international statistics on the annual number of school days and on the distribution of weekly instructional time across subjects which reveal large differences among and within countries.[1] For example, one of the main educational strategies of the "No Excuses" charter schools in New York, Boston and other places in the US is to emphasize the importance of increased instructional time (Angrist et al 2011, Dobbie and Fryer 2011).[2] In addition, evidence on these questions is valuable for improving the efficiency of school resource allocation, in particular the instruction budgets of schools across various subjects and activities.

[1] The OECD Program for International Student Assessment (PISA) 2003, 2006, and 2009 data reveal the great extent of these differences among the more than 60 countries that participated in this project. The reaction to these differences suggests that political leaders do pay attention to the results. For example, President Barack Obama cited the gap between the length of the school year in the United States and other countries as one reason he advocates the expansion of U.S. schools' instruction time among his key educational policy goals (March 10, 2009, at a speech to the U.S. Hispanic Chamber of Commerce).

[2] "No Excuses" schools are loosely defined as schools that emphasize strict discipline, extended time in school, and an intensive focus on building basic reading and math skills. Thernstrom and Thernstrom (2004) and Whitman (2008) argue that the "No Excuses" schools are more effective due to more instructional time, a zero tolerance disciplinary code, high academic expectations for all students, and an emphasis on teaching basic math and reading skills. However, these impressions are based on correlative analysis which does not permit causal interpretation.

While an extensive literature exists regarding the effects of school resources on student outcomes, much of the evidence is inconclusive.[3] For example, regarding the impact of school resources, Hanushek (2006) notes in an extensive survey that "the evidence – whether from aggregate school outcomes, econometric investigations, or a variety of experimental or quasi-experimental approaches – suggests that pure resource policies that do not change incentives are unlikely to be effective. Importantly, the results appear similar across both developed and developing countries". In addition, specific evidence on time use in schools is limited largely to the effect of the number of school days per year on student's achievements.[4] Thus, this evidence does

---

[3] For instance, in a French program that allocates extra financial resources to schools in disadvantaged zones, Benabou et al. (2009) find that this allocation has no significant impact on student outcomes. Häkkinen et al. (2003) uses the dramatic changes in the school spending caused by the 1990s recession in Finland and finds that changes in teaching expenditure did not have a significant effect on the test scores. Hanusheck (2003) report that inputs based schooling policies in the US failed to improve students' test scores. Lavy (2002) finds that providing resources to high schools led to significant gains in test scores and lower dropout rates. Focusing on labor market outcomes, Card and Krueger (1996) find positive effects of school resources on earnings, whereas Heckman et al. (1996) do not find significant effects, and Betts (2001) finds mixed results.

[4] For example, Grogger (1996), and Eide and Showalter (1998) estimated the effect of the length of the school year in the United States and found insignificant effects, perhaps due to selection and omitted variables. Card and Krueger (1992) and Betts and Johnson (1998) used state-level data in the United States to examine the same effect and found positive and significant effects on earnings which converge to zero once school quality is added as a control in the regression. Pischke (2007) used a natural experiment in West Germany and found that a shorter school year increased grade repetition and lowered enrollment in higher school tracks, but it had no effect on earnings and employment later in life. Based on school day cancelations due to snow, Hansen (2008) reports that more instructional time increases student performance, and Marcotte and Hemelt (2008) find that years with substantial snowfall are associated with lower pupil performance.

not tell us much about the effect of the number of hours children spend in school every week; or the time they spend in specific activities, such as math or reading; or whether there is a complementarity or a spillover effect across subjects.

In this paper, I investigate the causal effect of school instructional resources – the total teaching budget per class – on pupils' achievements.[5] I then examine how this instructional budget is used in terms of the number of hours that children spend weekly in school and the amount of time devoted to core subjects, and how these inputs affect cognitive and non-cognitive outcomes of children in school. For these objectives I exploit a unique school finance policy experiment that changed the formula used to determine the teaching budget for primary schools in Israel. Until 2003, almost 90% of schools' funding was based on the number of classes, irrespective of class size, and an additional 10% was based on schools' socio-economic deprivation index. In September 2004, the funding rules were changed and from then on schools received funding per student enrolled where a modified deprivation index was used to determine the amount of each "student voucher," with more money channeled toward students from the lowest economic and educational backgrounds.[6] This experimental reform generated a sharp and exogenous change in the teaching budget of many schools. Some schools gained resources while other schools experienced no change or even a decline in resources. Naturally, schools with a high proportion of students from a deprived

Goodman (2014) suggests that the mechanism through which weather affects achievements is students' absences and not school closures.

[5] Therefore, this paper is more in the spirit of the education production function literature that looks at the effects of specific inputs, such as class size and teacher quality on outcomes and not about what happens when schools are given more money and are allowed to spend it as they will (in the spirit of the "does money matter" literature).

[6] In 2009, the funding rules were changed again to a system very similar to that used prior to September 2004.

background or with large classes were the main beneficiaries from this reform. Given that the instructional budget is in 'kind' and not in money implies that the only thing schools could do in the short term is adjust instruction time, lower the student teacher ratio, or 'buy' in-school teachers' training. However, in the next section I provide evidence that schools did not change class size, or cut/expand extracurricular programs and in-school teachers' training but I show significant and positive "first stage" relationship between the change in instructional resources and length of the school week and time on tasks of core subjects.

For estimation, I use data from 2002-2005 for fifth-grade pupils for all the schools in the country. I observe each school twice, either in 2002 and 2004 or in 2003 and in 2005. The key feature of these data is that they present an opportunity to observe each school under the two different funding systems. This allows one to estimate the resulting changes in the instructional time budget per class, the length of the school week, and weekly instructional time by subject.

I employ three identification strategies in the paper. The first identification strategy compares schools in the years before and after the reform. The advantage of this school fixed effect strategy is that it guarantees that no other fundamental changes occurred in schools during this period. Based on this method I estimate the effect of total teaching budget and total teaching budget per class on the average scores in math, science, English and Hebrew and also the effect on this outcome of two additional key schooling inputs: the length of the school week and the instructional time of math, science, English and Hebrew. The second identification strategy uses the predicted change in funding as an instrumental variable for the actual change in funding. Since the exact pre- and post-reform funding formulas are not known, I compute the instrumental variable based on an approximation of the funding formula in each period and use for prediction the pre-reform rank of schools in terms of the deprivation index and the pre-reform school enrollment so as to neutralize any school level demographic changes or school size between the two periods.[7] I use this IV method to re-estimate the impact of instructional budget per class on the average score of the four main academic subjects. The IV method yields positive and similar size estimates which are however much less precise. The third method of identification exploits variation in the instructional time of different subjects, permitting therefore to get alternative estimates of the effect of instructional time of these four subjects. I compare the various relevant estimates based on the three identification methods, show that they are consistent, and discuss the implications for the quality of identification of each of these estimates. In terms of identification I distinguish between the well identified estimates of the effect of school teaching resources and the estimates of time on task which rely on additional assumptions for identification. I also evaluate the effect of instructional time on students' homework time allocation, on their overall satisfaction with school, on their social satisfaction in class, on their involvement in violent behavior, and on their fear of school bullying. These topics offer an indication as to whether social factors and student behavior are affected by the length of time that students spend in school during the day and week.

The results presented in this paper show that more school resources and a longer school week has positive and significant effects on students' academic achievements. However, any additional instructional budget per class beyond those used to determine the length of the school week has a much lower effect on students' learning outcomes. Furthermore, additional instructional time spent on the different subjects have positive and significant effects on students' academic achievements. These estimates are in contrast to the "naïve" OLS estimates which are actually

[7] In the pre-reform period funding was a function of the 'predicted' number of classes' (based on the rule that class size cannot exceed 40 and on each grade enrollment rate), of the school decile rank based on its score in the deprivation index and some other discretionary rules. Following the funding reform, a school budget was a function of its enrollment (instead of number of predicted classes) and of its decile rank based on its score in the deprivation index. All other funding rules were eliminated. More on this in the data section.

negative, reflecting a negative selection pattern of allocating higher instructional budgets to potentially low achieving schools (low SES). This is the first paper to show that the biased estimated effects of school resources and instructional time are reversed from negative to positive once potential selection and endogeneity of school resources are fully accounted for.[8] The estimates of the effect of the length of the school week and of instructional time in each subject are mutually consistent and yield very similar elasticities of test scores with respect to these measures of instruction time at school. The estimates show that the boost in test scores is modest. Allowing for treatment heterogeneity, the growth in test scores is similar for boys and girls and not systematically different for pupils from different socioeconomic statuses. Overall, the main results presented in the paper are very robust to a variety of robustness checks with respect to their identification assumptions and to threats to their validity. Further, the alternative identification strategy based on pupil fixed effect and on variation in time of instruction across-subjects yields similar results. These estimates are also similar to estimates that I obtained using PISA data of all OECD countries and another sample of all East European countries where the identification strategy was based on pupil fixed effect and variation in time of instruction across subjects (Lavy 2015). The results presented in this paper also suggest small or no effects on homework, satisfaction from school, and the social environment.

The rest of the paper is organized as follows. Section II presents background on the reform. Section III presents the data and section IV presents our empirical strategy and results. Section V presents the conclusions.

[8] Angrist and Lavy (1999) show a similar reversal in sign with respect to the effect of class size. The "naïve" OLS estimates of class size were actually positive, reflecting a negative selection of allocating smaller class size to potentially low achieving schools (low SES). This is consistent with the negative selection pattern of allocating more instructional budget to low achieving schools that I identify in this paper.

2. The 2004 Funding Reform

As noted in the introduction section, I will use three separate identification strategies in this paper, the first two are based on policy induced variation in school resources, time at school and on different tasks. The third strategy is based on variation induced by the degree of flexibility that schools have beyond the compulsory curriculum that dictates minimum standards for time children spend at school and on learning core subjects. In this section I provide the institutional background necessary to understand the variation for both approaches.

The budget for primary schools in Israel comes from two sources: the local municipal authority and the Ministry of Education. The local authority funds all the administrative costs of the school such as the costs of secretaries, school supplies, and building maintenance. The Ministry of Education funds all the teaching and other educational activities in schools. This funding is provided in 'kind' in terms of number of instruction hours, which is measured in units of one hour of instruction per week for the whole school year. It is intuitive to think about this part of the school budget as a voucher that funds all teaching instruction costs, as well as the cost of internal (within school) and external (outside of school) teachers' training. Schools are informed about the size of their voucher during the summer break and they decide on how to allocate the voucher to the different inputs that can be purchased with it. The Ministry of Education then pays the monetary equivalent of the voucher value directly to the providers of these inputs. For example, all primary schools teachers, including schools' principals, are government employees and receive salary directly from the Ministry of Education. Teachers' compensation typically accounts for 80% of the overall voucher of the school's instructional budget. Schools are also allowed to use the instructional voucher to pay teachers for tasks other than classroom teaching such as coordinating a school wide activity such as field trips coordinator or a grade coordinator. The instruction budget also funds in-school teachers' training. Some providers of teachers' training are government employees and are paid by the Ministry of Education through the school instruction voucher. Schools can also 'buy' in-school teachers' training from private providers which is also funded from the instruction voucher and the actual payment is done by the Ministry of Education. Similar arrangements apply for paying with the school instructional time voucher to private providers of all kind of school extracurricular activities such as educational trips, science or other educational clubs, and other educational activities in school that are beyond the compulsory national curriculum. Schools can also use this voucher to fund the addition of a teacher aid in some classes or lessons but it is not usually sufficiently large to reduce class size throughout the school week. However, in recent years the Ministry rules were relaxed to allow the parents association in a school or in a given grade to top the Ministry allocation with additional resources that can permit reducing class size.

In the 2004 school year, the Ministry of Education introduced a school finance reform as a policy experiment that changed the formula used to determine the instruction time budget of primary schools in Israel. Until 2003, schools were funded based on the number of classes, irrespective of class size. The number of classes used for budgeting was predicted based on the rule of maximum 40 students per class and grade specific enrollment rates, and not based on actual number of classes. Schools also received an additional instruction budget based on their score in a deprivation index. The index was a function of students' and schools' average characteristics, in particular average parental education, number of siblings, family income and number of immigrant students. Using the distribution of this index schools were divided into deciles. Schools in higher deciles received more resources. The overall instructional budget of this differential component amounted to about 10% of the overall funding of primary schools in the country but the exact rule that determined the allocation of these resources is not made known to the public. In addition schools received additional instruction budget based on other criterions, for example, in lieu of number of immigrant students in school, or to split classes by gender in religious schools, but the information about these rules is also not available.

In September 2004, the funding rules were changed and from then on schools received funding per student enrolled. In the new system all other sources of funding were cancelled and schools received their central government funding based on one formula. A modified deprivation index featured more prominently in the new funding formula. Schools were divided again to deciles and received instruction time funding per student. The ‘voucher’ for schools in the 10th decile was highest, and it was about 70% higher than the voucher for schools in 1st decile. The needs based deprivation index was again a function of students and school characteristics, with an added ‘national priority’ criteria, prioritizing schools located near the country’s borders, and a ‘periphery’ location criteria. The criteria that formed this index and their (approximate) weights are as follows: mother's years of schooling (15%), father's years of schooling (15%), number of siblings (10%), new immigrant status (20%) and immigrant from developing countries status (10%), national priority status (20%), and periphery location status (10%). Most of the weights were derived from a variance decomposition regression that examines the correlation between students' background characteristics and students' achievement. Since the deprivation index in the post-reform period was very similar to the one used before the reform, almost all schools had their decile rank unchanged.

The new funding formula implied large resource changes for some schools. The Ministry of education decided to converge to the new steady state level of funding gradually, reducing or increasing resources while imposing a maximum to the change that schools experience in a given year. The maximum change, positive or negative, for the first year was set at 15% in the second year at 20%. However, there is no information available about this convergence process to the steady state funding level for each school but the funding that each school received every year is available to us. Even though gradual, this experimental reform still generated a sharp and exogenous change in the teaching budget of many schools. Schools with a large enrollment of students with a high deprivation index and schools with large classes gained resources, while others schools lost resources. The reform was intended to produce no changes in the overall resource distribution among schools in the Jewish public school system. However, the reform was designed to allow for an increase of about 15% to 20% in the overall budget of the Arab schooling sector, as this sector includes a much higher share of students with high deprivation index estimates and is characterized by larger classes. The schools that gained resources had on average lower pre-reform budgets per class while those who lost resources had higher pre-reform budgets per class. This experimental reform lasted until 2008 when it was changed again to a new system that was more similar to the pre-2004 rules.

While there are several inputs that may be affected by a change in a school's budget, I find that the budget per class is highly correlated with the length of the school week and with instructional time of math, science, and English and not with other inputs such as class size, in-school teachers' training or extracurricular activities. For instance, the regression coefficient of budget per class on length of the school week is 0.280 (se=0.017) and it is very similar for schools that gained resources (0.242, se=0.023) and for schools that lost resources (0.324, se=0.019) due to the reform. The regression coefficient of budget per class on instructional time of the four main academic subjects is 0.085 (se=0.009) and it is almost identical for schools that gained resources (0.087, se=0.012) and for schools that lost resources (0.097, se=0.012) due to the reform.[9] These relationships seem stable and yield the same estimates when estimated separately based on the pre- and post-reform samples. At the same time, the estimated coefficient of the budget per class on class size is -0.011 (se=0.034) suggesting that this is not a channel that schools used for spending their teaching voucher. This conclusion is also evident when comparing the estimated effect of the budget per class on weekly hours of instruction or on core subjects instructional time obtained from two sub-samples stratified by actual class size or predicted class size (based on maximum class size of 40, see Angrist and Lavy, 1999). For example, the estimated coefficient of the school budget on the length of the school week is 0.269 in the sample of above the mean predicted class size and it is 0.276 in the sample of schools with below the mean predicted class size. Similar lack of

[9] These results are not presented in the paper and are available upon request from the author.

correlation is obtained when I estimate such relationship for extracurricular activities and for teachers' training in school.

3. Data

The data I use in this study are based on the Growth and Effectiveness Measures for Schools (GEMS - *Meizav* in Hebrew) datasets for the years 2002-2005. The GEMS was administered for the first time in 2002 and it includes a series of tests and questionnaires administered by the Division of Evaluation and Measurement of the Ministry of Education.[10] The GEMS is administered towards the end (from mid-May to mid-June) of each school year to a representative 1-in-2 sample of all elementary and middle schools in Israel, so that each school participates in GEMS once every two years. The GEMS data include test scores of fifth- (primary school) and eighth- (middle school) grade students in math, science, Hebrew, and English. In principle, all students except those in special education classes are tested and the proportion of students tested is above 90%. The raw test scores used a 1-to-100 scale that I transform into z-scores to facilitate interpretation of the results. In this study I use only primary school data since the funding reform only affected primary level schools.

The test scores for the years 2002-2005 are linked to student administrative records collected by the Israel Ministry of Education. The administrative records include student demographics that I use to construct all measures of students' background characteristics. Using the linked datasets, I build a panel for elementary schools with test scores for the years 2002-2005. The sample is restricted to Jewish public schools that follow the same national curriculum and participate in the GEMS national testing. For these reasons I exclude the religious Orthodox Jewish

---

[10] The GEMS is not administered for school accountability purposes and only aggregated results at the district level are published. For more information on the GEMS see the Division of Evaluation and Measurement website (in Hebrew): http://cms.education.gov.il/educationcms/units/rama/odotrama/odot.htm.

schools and the Arab schools. There are 939 elementary schools with test score data. Since every school is sampled once in two years, the panel sample with two observations for each school includes 887 schools. Based on the Ministry rule of limiting the change in school resources to be no larger than 15% in the first year and 20% in the second year, I exclude from the sample 40 schools where the data show change in resources outside these range. This sample restriction practically eliminates from the sample schools with extreme outliers in terms of instruction budget per class which most likely may indicate a measurement error in the data. The final sample for analysis includes 845 schools.

The GEMS also includes interviews with all teachers and the school principal. The questionnaire for 'home class' teachers[11] of all classes included questions about classroom instructional time in each subject and the total instructional time per week. I use teachers' responses to these items to compute the school average for fifth-grade instructional time in each subject. Though there was very little difference between or among fifth-grade classes in a school in these time inputs, I still prefer to use the school-level mean per grade to avoid any biases that might be caused by sorting of students into certain classrooms and setting time allocations for given academic subjects according to those students' particular strengths and weaknesses. In any case, the grade- and class-level measures of these time inputs are very highly correlated.

I also use items from the GEMS student questionnaire that address various aspects of the school and their learning environment. I concentrate on two sections of the questionnaire: the first provides information on student satisfaction in school and on the violent behavior of other students, and the second provides data on student allocation of time for homework by subject. In the first section

[11] A 'home class' teacher in primary school in Israel teaches most weekly sessions of his class, and has additional duties such as taking attendance registers, acting as intermediary in cases of conflict, collating other teachers' impressions of the class and of individual students in preparation for the quarterly report, liaising with parents and various other administrative tasks.

students are asked to rate the extent to which they agree with a series of statements on a six-point scale ranging from "strongly disagree" to "strongly agree". These items include: (1) "There are many fights among students in my classroom"; (2) "Sometimes I'm scared to go to school because there are violent students"; (3) "I am often involved in violent activities in school"; (4) "I feel well-adjusted socially in my class"; and (5) "I am satisfied in school". I transformed students' responses to these items into standardized z-scores.[12] In the second section of the questionnaire, students are asked to report the number of hours per week that they spend at home doing homework in each of three subjects (math, science and English).

In Table 1, I present summary statistics for the variables used in the analysis. Column 1 lists the results for our key variables in the pre-reform period of 2002-2003 and column 2 lists the results for the post-reform period of 2004-2005. Panel A presents the results for the budget per class and instructional time variable, all measured in terms of weekly hours of instruction. According to the table, the mean budget per class in the second period is lower by one hour, about 2% decline, suggesting that the reform had a small negative impact on the mean level of resources for the Jewish secular schools. The length of the school week is on average 35 hours, implying that 76% of the teaching budget of schools is used for classroom instruction. The rest of the teaching budget is used to fund teachers' training, to pay personnel for extracurricular activities in school, and after school remedial education programs. The average instructional time of the four core academic subjects is 19.4-19.7 hours a week, almost a third of it used for math instruction, thirty% of it is used for Hebrew instruction and the rest divided almost equally between the other two subjects. Overall, there seems to be little difference in instructional time in the years before and

[12] I experimented with binary versions of these variables denoting above median answers and also using them linearly and the results were not different, reassuring that the evidence regarding these variables is not a feature of this particular transformation of the data.

after the reform. Panel B presents the means for the average test scores for each subject. Panel C presents the means for student characteristics. Panel D presents the means for school characteristics, which are almost identical over the two periods, respectively.

4. Empirical Strategies and Results

4.1 School Fixed Effect Framework

The effects of unobserved correlated factors usually confound the effect of school budget or instructional time on student outcomes. Such correlations could result if self-selection and sorting of students across schools are affected by school resources, or if there is a correlation between school instructional time and other characteristics of the school that may affect student outcomes. The structure of the GEMS allows me to use an identification strategy that overcomes this potential problem because it is based on observing schools and their students at two points of time: before the funding reform (in 2002 or 2003) and after the funding reform (in 2004 or 2005). I take advantage of this feature and construct a longitudinal dataset at the school level to examine how changes in students' achievements are associated with changes in instructional time. Note that the change in instructional budget can only be due to the funding reform because there is no school choice at the primary schooling level in any school district in Israel and assignment to schools is based on pre-determined rules (mainly the family location of residence). Also note that the reform and the new budget for each school were announced only very shortly before the beginning of the school year which left very short time for parents to consider changing residences. It is also likely that parents of fifth graders will be reluctant to move their children to another school towards the end of primary school. As a result, the potential for selection bias due to sorting of students across schools based on instruction budget is very small in this context. Indeed, I find that the proportion of students in our sample that changed schools between 4^{th} and 5^{th} grade between the 2003 and 2004 academic years was less than two percent and this rate was not different between schools that gained, lost or had no change in instructional resources due to the reform. Also, such mobility was not correlated with observed student's characteristics such as parental education (this evidence is available from the author on request).

To develop the relationships of interest using the panel data, I specify the following standard education production function that links pupils' achievements and their relevant determinants:

$$Y_{ijt} = \alpha_j + \gamma\, W_{jt} + \beta X_{ijt} + \delta S_{jt} + u_{ijt} \tag{1}$$

Where Y_{ijt} is the average achievement of the i^{th} student in math, science and English, in the j^{th} school in period t, W_{jt} is the total budget of instructional time or the budget of instructional time per class in the j^{th} school in period t. X_{ijt} is a vector of characteristics of the i^{th} student, including a gender dummy, both parents' years of schooling, number of siblings, immigration status and ethnic origin indicators based on parental country or continent of birth (Israel, Asia/Africa, Europe/America, Former Soviet Union, Ethiopia, Other). S_{jt} is a vector of time varying characteristics of the j^{th} school, including enrollment and enrollment squared. α_j is a school fixed effect that captures everything about the school that is not observed and does not vary between the two years that each school is observed (2002 and 2004 or 2003 and 2005) and u_{ijt} is the unobserved error term.

Observing schools in more than one time period allows estimating this model with a school fixed effect. The identifying assumption in this model is that the change in W_{jt} is uncorrelated with any elements of the error term u_{ijt} for any reason. This assumption is satisfied if changes in W_{jt} between the two periods for each school could result only because of the change in the funding rules and that the average value of the X's (which are used to compute the deprivation index) remained unchanged. This assumption holds in the current context because the decile rank of the deprivation index of schools remained unchanged after the revision of this index following the funding reform. Therefore, conditional on a school fixed effect, the change in W_{jt} is not correlated with the change in u_{ijt}.

Are the schools that gained or lost resources due to the reform observationally equivalent? Here I test directly whether the students in schools that gained and lost resources due to the reform are statistically indistinguishable in terms of their observed characteristics for two pre-reform cohorts (2002 and 2003), and for the post-reform cohorts (2004 and 2005). Column 1 of Table 2 shows that student background characteristics are very similar in high and low gain (of instructional budget) schools in the sample of schools that gained resources. Column 2 of Table 2 shows that student background characteristics are very similar in high and low loss (of instructional budget) schools in the sample of schools that lost resources. In columns 3 and 4 I present estimates of the indicators of gain and loss (of instructional budget) schools where they are both included in the regression on students' characteristics while using all schools and students in the sample. The estimates present the difference relative to the group of schools that experienced no change in resources. Very few estimates (4 out of 22) are statistically significant and in these cases the estimates have the same sign and are equally significant for both the gain and lost groups. I conclude that students in schools that gained or lost resources due to the funding reform are similar in their mean background characteristics.

Were schools that gained or lost resources due to the reform different in their pre-reform time trends? Since there are no test score data for years prior to 2002, I use instead pre-reform students' characteristics data for grades 1^{st} to 6^{th} in 2002-03 to estimate differential trends in these characteristics for schools that gained resources versus schools that lost resources due to the reform. If there are differential trends in cohort quality between types of schools, then the gaps should widen or narrow as we progress from the youngest to the oldest cohort. The unit of observation in this analysis is the school-grade. I estimate a model that includes a series of grade (cohort) dummies and include in the regression an interaction of each of these grade dummies with the two indicators of increase and decrease of school resources. Main effects of these two indicators as well as for each grade are also included in these regressions. I focus on the three main characteristics: fathers' year of schooling, mothers' years of schooling and number of siblings. These results are presented in Table 3. There are 30 differential trend parameter estimates reported in the table and all of them are small and not statistically different from zero.

Empirical Results - Effect of School Resources: The first row in Table 4 presents the baseline results regarding the relationship between total school instruction budget and students' average achievement in math, science, English, and Hebrew. The second row presents the evidence for the effect of the school instruction budget per class. We use predicted number of classes and not actual number of classes as the denominator of this treatment variable. The estimates are based on equation (1) with varying degrees of control variables. The standard errors are clustered at the school level. The estimates presented in column 1 are from OLS regressions which include only subject and year fixed effects as controls. The estimates presented in column 2 are from regressions that include also school fixed effects. Column 3 also controls for the student characteristics and column 4 controls for the time varying school characteristics. The mean of length of the school week in 2002-2003 measured in weekly instruction hours 34.98 (sd=3.16). Similarly, the mean of instructional budget per class in 2002-2003, also measured in weekly instruction hours, is 46.44 (sd=5.52). The OLS estimate in the first row of column 1 is negative, -0.0001 (se=0.0001), which means that school resources and test scores are negatively correlated though this correlation is not statistically significant. Similarly, the OLS estimate in the second row and column 1 is also negative (-0.0156) and significant (se=0.0017). These two estimates reflect a pattern common in many previous studies upon which a claim is made that school resources have no positive effect on learning outcomes. This are most likely biased estimates since schools with lower potential outcomes receive compensatory resources. The bias could also result from omitted variables that are correlated with student performance.

However, adding the school fixed effect reverses the sign of the estimates to be positive, and in the second row it is even statistically significant. For example, the estimated effect of resources per class on test scores is 0.0085 (se=0.0030). These estimates are unchanged in the other two specifications (columns 3 and 4). This suggests that conditional on the school fixed effects, the instructional budget per class is not correlated with student and time varying school characteristics such as enrollment and enrollment square. This confirms the identification assumption that the school characteristics that form the deprivation index that is used in the budget formula have not changed during the two years between the pre- and post-funding reform. Therefore, we can be confident that the change in the school instructional budget reflects only the change in the weights of the decile rank in the funding rules.[13]

It should be noted that these results estimate the effects of the reform only one or two years after its implementation. Therefore, a valid question is whether the changes we observe in schools and the estimates of the effect of school resources are representative of a longer run effect. By 'long run' I mean that the level of instructional time has been in effect for some time so the adjustment by school, pupil and parents have been completed. Two pieces of evidence suggest that the estimates in the first row of Table 4 do reflect longer term adjustments. First, estimating the effect of school resources separately based on the contrast of 2002-2004 and 2003-2005 yield almost exactly the same estimates, suggesting that the estimated effect based on experiencing one or two years of reform is the same. Second, the results of the third alternative identification strategy which

[13] Note that equation (1) can also be estimated as a regression in changes, which also allows adding pre-program characteristics to account for the possibility that trends in achievement differ by school characteristics. This specification yields very similar results to those reported in Table 4. Equation (1) can also be estimated with the school as the unit of observation where all variables are collapsed to schools means. Using this school level estimation, the effect of the instruction budget is unchanged, 0.0004, and the estimated effect of budget per class is slightly lower, 0.0061, and as expected these estimates are less precisely measured in comparison to the estimates presented in Table 4.

is based on cross section data analysis and reflects long term estimates (see Table 8) are identical to those presented in Table 4.

Another possible concern is whether the results from Table 4 are biased due to the convergence of underachieving schools towards the level of high performing schools. In other words, if schools with a lower than average budget per class (who benefited more from the funding reform and presumably had lower average test scores in the pre-reform period) had a higher improvement rate of test scores due to a convergence effect, such convergence would be positively correlated with the resource gain from the funding reform and therefore will bias upward the estimated effect of the budget per class on test scores. To check the extent of this possible bias, I divided the sample into two groups based on budget per class in the pre-reform period and re-estimated equation (1) in each of these samples separately. The estimated effect of budget per class on the average test score obtained from the sample of schools with above average budget per student is 0.008 (se=0.006). The respective estimate obtained from the sample of schools with below mean budget per student is 0.006 (se=0.003). Stratifying the sample into four groups based on budget per class yields a similar pattern. This evidence suggests that it is very unlikely that the resource effect that we estimated reflects test score convergence. This evidence is also reassuring that the effect of the changes in the allocation of time is not confounded with underlying differential trends in achievement by level of disadvantage.

4.2 Instrumental Variables Approach

The identification assumption in the school fixed effect model described above is that any changes in instructional budget of schools during the study period resulted only from changes in the funding model. The evidence presented in the previous section show that it is unlikely that schools or parents reacted endogenously to the changes in the school budget following the funding reform. However, to further address concerns regarding such potential threats to the identification of the effect of school instructional budget, I employ as an alternative an instrumental variable approach. The instrumental variable is the predicted value of instructional budget per class where predictions are derived based on the new funding formula and *pre-reform* values of its determinants.

The instrument is constructed separately for the pre- and post-reform periods, based on each period funding formula. This construction involves two steps. I first estimate for each period an approximated ('not exact') funding formula using regressions that are specified based on the period's specific funding rules. In a second step I use the estimated parameters from these regressions to predict the instructional budget per school and per class. More specifically, for the pre-reform period I use the estimated parameters of the ***pre***-reform budget regression and the ***pre***-reform values of its determinants. For the ***post***-reform period I use the estimated parameters of the ***post***-reform budget regression and the ***pre***-reform values of its determinants. Using ***pre***-reform values of determinants for the post period predictions guarantee that the incremental variable is not affected by endogenous changes following the reform. As a result, the change in the prediction of schools' instructional budget in the post reform years can result only from changes in the funding rules.

I present below how this approach is actually implemented. The instructional budget prediction regression for the pre-reform period is the following:

$$W_j = \alpha + \pi\, C_j + \Sigma_{1-10}\, \mu_d\, D_{jd} + \lambda_j$$

where C_j is the number of predicted classes in school j and D_{jd} is a dummy indicating the decile rank of the school based on its deprivation index. π is the budget in lieu of each predicted class and μ_d is the additional instruction budget that a school in decile d receives. λ_j captures sources of other funding, some ad hoc. The total budget prediction regression for the post-reform period is the following:

$$W_j = \alpha + \beta\, N_j + \Sigma_{1-10}\, \mu_d\, D_{jd} + \lambda_j$$

Where N_j is the number of students in school j. This equation includes the enrollment and the decile rank of schools as a determinant of funding.

The estimated parameters of these two prediction equations are presented in Table A1 in the online appendix. I note that the R^2 in each of these prediction equations is over 0.8 even though these regression equations are not the exact formulas used by the Ministry of Education. However, the construction of the instrumental variable for each period, and in particular for the post reform period, still satisfies the conditions for a valid instrument because I use for prediction the pre-reform values of N_j and D_j. I use this instrument in the following 2SLS model where the data is at the school level and all variables are obtained from collapsing them at the school-year level:

First Stage: $W_{jt} = \varphi_0 + \varphi_1 W^p_{jt} + \varphi_{3d} N_{jdt} + \varphi_4 X_{jt} + \varphi_5 S_{jt} + \alpha_j + \epsilon_{jt}$

Second Stage: $Y_{jt} = \delta_0 + \delta_1 \hat{W}_{jt} + \delta_{3d} N_{jdt} + \delta_4 X_{jt} + \delta_5 S_{jt} + \alpha_j + \upsilon_{jt}$

Where, W^p_{jt} is the instrumental variable (the predicted budget per class for school j in period t, where t indicates the pre- or the post-reform period), W_{jt} is the actual instruction budget per class of school j in period t, and $\hat{W}_{jt}$ is the instrumented instruction budget per class of school j in period t. The first and second stage regressions include as controls the school means of students' characteristics denoted by X_{jt} and school enrollment and enrollment square denoted by S_{jt}. α_j is a school fixed effect.

Empirical Results - effect of school resources per class: In Table 5 panel A, I present the results from the 2SLS estimation of the effect of resources per class on test scores. The sample includes the same sample of 845 schools that are used in the estimation presented in Table 4. In the first row, column 1 presents the first stage estimates, column 2 the reduced form estimates and column 3 the 2SLS estimates. The first stage estimates show a positive and statically significant relationship between the predicted and actual instructional budget per-class. The estimated effect of the instrument on actual instructional hours per class is 0.5011 with a t-value of about 8. The R^2 in this first stage regression is high, 0.966, largely because it includes fixed effect for each school. The estimated reduced form effects are positive but not significantly different from zero and therefore the 2SLS estimate is also positive and not precisely measured. This estimate, which is almost equal to the respective ratio between the reduced form and first stage estimates, is not too different from the respective estimate presented in Table 4 but it is much less precisely measured. The IV estimate is 0.0072 (se=0.0065) and the OLS school fixed effect estimate of Table 4 is 0.0084 (0.0031).

In panel B of Table 5 I present another 2SLS estimate that I obtain while using current school enrollment instead of pre-reform school enrollment in the equation that predict instructional budget per class. This specification of the post reform prediction equation yields a better first stage equation and a larger reduced form effect. The first stage estimate is now 0.7057 and the reduced form estimate is 0.0053 and therefore the resulting 2SLS estimate of the treatment effect is 0.0075, which is identical to the estimate obtained when using the pre-reform enrollment for predicting the post reform budget. However, this estimate is more precise, almost marginally statistically significant. We note however, that post-reform enrollment might be endogenous and therefore an additional identifying assumption in this case is needed, namely that the changes in enrollment after the reform are uncorrelated with the error term in the regression equation. This assumption however is plausible, especially for the first year of the reform, because schools and parents had no time to adjust enrollment as a response to the reform because the reform was announced to schools just few weeks before the beginning of the school year. Another relevant factor for understanding why the two versions of the instrumental variable yield the same 2SLS estimate is the fact that in primary schooling in Israel enrollment is strictly based on school zoning rules and changing school is only possible with a change in home address. As a result, enrollment of a given cohort in schools is relatively stable from first to sixth grade.

In the next section, I estimate some particular channels through which school instructional resources affect student performance, in particular the length of the school week and classroom study time of core subjects.[14]

4.3 Identification and Estimation of Time on Task

A school's instructional budget is largely spent on the length of the school week. Thus, I estimate equation (2) by replacing the instruction budget per class with the length of weekly school instruction (in terms of hours per week). The weekly instructional time is divided among different subjects. For this reason, I also estimate equation (2) where the sum of weekly hours of instruction of the three core subjects (math, science and English) or of the four core subjects (including Hebrew as well) is the treatment measure. One possible problem with this approach is that these two measures of instructional time are choice variables and, therefore, could be endogenous in equation (2). If the choice made by schools of how much to allocate from the instructional budget to any of these two measures is only a function of fixed characteristics of the school, then the school fixed

[14] An alternative specification of the post-reform funding formula includes 10 interaction terms between the school deciles and school enrollment instead of the ten decile dummies and the enrollment variable. These ten interaction terms are sort of 'informative' dummies: instead of a 0/1 indicator for the ten deciles groups, the predicting regression includes ten 0/enrollment variables and each of the ten estimates present a decile specific 'voucher' per student. This specification is more closely related to the spirit of the post reform funding regime. The estimates of this alternative funding equation are presented in column 3 of Table A1. The funding per pupil increase monotonically from 1.1 hours per student in the first decile to 1.7 hours per student in the 10th decile. Using this equation to predict the post reform budget for schools (instead of that of presented in column 2) yields first stage, reduced form and 2SLS estimates very similar to those presented in Table 5.

effect model will identify the causal effect of any of these two treatment measures. However, if these choice decisions are correlated with the error term in equation (2), our results might be biased. I discuss this issue in greater detail in my Table 6 results.

In the first row of Table 6, I present the estimated effects of the length of the school week (number of weekly hours in school) on the average test score, based on student level regressions. The mean number of weekly hours in school in 2002-2003 is 35.0 (sd=3.2). Similar to the pattern observed in Table 4, the OLS estimate is initially negative (-0.0170) and statistically significant (se=0.0029), and becomes positive (0.0070, se=0.0048) once schools fixed effects are added to the regression. In addition, the estimates in columns 3 and 4 are nearly identical to that of column 2, implying that adding the student and school characteristics as controls has no effect on the estimates and their standard errors. The estimated effect is 0.0078 (se=0.0046), statistically significant at 10% level of significance.

We first note that the estimated effect of the length of the school week is not very different from the two alternative estimates for the effect of the instruction budget per class that I reported in the previous section. The length of the school week is on average 80% of the budget per class and therefore it is again reassuring that the instrumental variable estimate of the effect of the budget per class is not very different from the school fixed effect estimated effect of the instruction budget per class. However, I also note that the estimated effect of the length of the school week and that of the budget per class can be compared only based on their elasticity with respect to the average test score. The elasticity of the instructional budget per class is 0.080, and the elasticity of the length of the school week is also 0.079.[15] This implies that the instructional budget per class has an effect

[15] Since the mean of the standardized test score is zero, I compute the two elasticities based on estimates of equation (2) where the dependent variable is the row grade (scale 1 to 100) instead of the z-score. These estimates, not presented here, show exactly the same pattern that is shown in Table 4. The elasticity of the

on test scores mainly through the increase in length of the school week. A validation of this result is also shown in the second row of Table 6, where I present estimates of the effect of the budget of weekly hours of instruction beyond the length of the school week (simply the difference between the instructional budget per class and the length of the school week) on the average test score. The mean of this measure is 11 weekly hours and its estimated effect presented in column 4 is 0.0037 (se=0.0027). Its elasticity with respect to the average test scores is 0.015, confirming that the effect of class budget on average test scores of the four subjects, beyond what is allocated to the length of the school week, is indeed small.

Another important implication of the similarity of the two estimated effects presented in the first two rows of Table 6 is with regard to the interpretation of the estimated effect of the length of the school week as causal. Given that the change in the instructional budget per class is exogenous, conditional on schools' fixed effects, its estimated effect is clearly unbiased. Therefore, the similarity in the two point estimates and in their implied elasticities is suggestive evidence that the estimated effect of the length of the school week is unlikely to be biased due to selection or endogeneity. A related point is that if the effect of length of the school week was biased, upward or downward, then the effect of the difference between the instructional budget per class and the length of the school week should have been biased in the opposite direction. Instead, we find that this estimate is practically zero.

In the third row of Table 6, I present estimates of the effect of the average weekly hours of instruction on the average score of the four core subjects. The sum of instructional time of these subjects is 19.36 hours per week in the pre-form period and 19.69 hours per week in the post-reform period. The OLS estimate in column 1, third row, is positive but imprecise. However, adding the school fixed effects to the estimated equation triples the estimated coefficients, from 0.0119 to

---

budget per class is computed as [0.127 x (46/70)] while the elasticity of the length of the school week is computed as [0.157 x (35/70)], both equal to 0.079.

0.0360. Remarkably, however, this estimate remains unchanged as I add controls to the school fixed-effect regressions: the point estimate in the third column is 0.0366 (se=0.0207) and it is 0.0370 (se=0.0208) in column 4. This robust estimate implies that adding one hour of instruction in each of the four subjects raises the average score by 0.04 standard deviation.

It is interesting to note that this estimated effects of instructional time is very similar to the estimates that Dobbie and Fryer (2011) obtain from their sample of charter schools in New York City (NYC). They find that schools that add 25% or more instructional time have an annual gain that is 0.059 of a standard deviation higher in math. Note that a one-hour increase in instruction time in our sample is approximately 22% (given that the respective mean is 4.6 hours) and our estimated effect is 0.039, almost identical to the NYC estimate. However, the authors emphasize that their estimates of the relationship between school inputs – including instructional time – and school effectiveness are unlikely to be causal given the lack of experimental variation in school inputs. Moreover, a very similar estimate of effect size of instructional time is reported in Lavy (2015) based on a sample of over 50 countries and data from the 2006 Programme for International Student Assessment (PISA), and in Rivkin and Schiman (2015) who used the 2009 PISA data.

Furthermore, this estimate of 0.04 yields an elasticity of 0.20 which is almost identical to the elasticity of the length of the school week after we adjust for the difference in the means of the two instructional time measures.[16] This result has two important implications. The first is that additional time that children spend in school during the week in pursuits outside of math, science, and English classes, does not affect at all their achievement in these subjects. In other words, the effect of the length of the school week on average test scores is only a reflection of its correlation with the instructional time of these particular subjects. The implication is that whatever skills students acquire during the time in school spent outside of the main academic subjects (60% of

[16] Based on estimates using the raw test score data, the elasticity of this time measure is computed as [1.04 x (13.7/70)].

their total school time) are immaterial to their academic progress in these core subjects, at least as reflected in the short-term test scores. Perhaps we should not be surprised that knowledge in other subjects, such as history, geography and literature, is irrelevant for better achievement in math or science. However, students may acquire and enhance non-cognitive skills, such as socialization, confidence and determination, during longer school weeks. Thus, one might have expected potential spillover effects to surface for a wide array of academic pursuits.

The similarity of the estimated effects of the length of the school week and of the instructional time in the main academic subjects has a second important implication: the effect of instructional time in these subjects is very unlikely to be biased. If the change in instructional time of these subjects between 2002-3 and 2004-5 were determined selectively with respect to potential outcomes in these subjects, we would have expected that the estimated effect of the length of the school week and of weekly hours of instruction of these subjects to be different. Instead, they are almost identical. A validation of this result is shown in the fourth row of Table 6, which presents estimates of the effect on the average test score of the number of weekly hours of instruction in all other subjects and activities in school. This measure of instructional time is simply the difference between the length of the school week and the instructional time of the four core subjects and its mean is 22 weekly hours (19 hours). Remarkably, the point estimates in all columns in these two rows are negative but not significantly different from zero. This result confirms that there are no spillover effects in school in Israel from instruction of all other subjects on achievements in math, science, English and Hebrew. It also confirms that conditional on school fixed effects, the allocation of instructional time to each of the core academic subjects, given the length of the school week, is not correlated with potential outcomes in these subjects. Finally, if the estimated effect of instructional time of the core subject were biased, upward or downward, we should have expected that the effect of all other instructional time during the week to be biased. However, I do not observe such bias as the estimated effect of instruction on non-core subjects on average test scores for core subjects is nearly zero.

4.5 Estimated effect of instructional time in each subject

In this section, I specify and estimate a school fixed-effect model where both the dependent variable and the time of instruction per week are subject-specific, as follows:

$$Y_{kijt} = \mu_j + \rho\ T_{kjt} + \eta\ X_{ijt} + \ \varepsilon\ S_{jt} + u_{kijt} \qquad (3)$$

Where Y_{kijt} is the achievement in the k^{th} subject, in the j^{th} school, of the i^{th} student, and T_{kjt} is instructional time in the k^{th} subject in the j^{th} school. The unobserved error term u_{kijt} is now subject specific. In addition, I estimate an alternative specification that also includes the hours of instruction in each of the other two subjects and the total instructional time of all other subjects as treatments:

$$Y_{kijt} = \mu_j + \ \rho\ T_{kjt}\ +\ \eta\ \ X_{ijt} + \varepsilon\ S_j + \lambda\ T_{2jt} + \ \theta\ T_{3jt} + \sigma\ T_{ojt} + u_{kijt} \qquad (4)$$

Where T_{2jt} and T_{3jt} represent instructional time in the other three subjects and T_{ojt} is instructional time in all other subjects. λ, θ, and σ are the cross subject's parameters. Note that the sum of T_k, T_2, T_3, and T_o is equal to the length of the school week in terms of hours of instruction. By comparing the estimates obtained using equation (4) to those obtained based on using equation (2) and overall instructional time of the four subjects, I hope to strengthen the causal interpretation of the evidence.[17]

In Table 7, I present the results of estimating equations (3) and (4) for each of the four subjects separately. Each parameter presented in the table is estimated in a separate regression. Each regression includes as controls school fixed effects, year dummies, student characteristics, and school characteristics. The four estimates presented in the first row are positive and precisely measured. The effect of an hour of instruction in math is 0.042 (se=0.018), in science it is 0.0344

[17] I also compare the estimates of equation (4) to respective parameters based on a completely different identification methodology (See Table 6).

(se=0.0169), in English it is higher, 0.051 (se=0.012), and in Hebrew it is much lower, 0.0192 (0.0103). The average of the four estimates is 0.037, identical to the estimated effect of the average hours of instruction in these four subject on the respective average score (0.037, se=0.0208)) reported in the third row of Table 6.

Based on the result that the average estimated effect of the four subjects and the respective estimated effect reported in Table 6 are identical, I argue that the allocation of the sum of the total teaching time in all four subjects, to each of the four subjects is not correlated with potential outcomes. Overall, I conclude that the estimates based on separate regressions for each subject are fully consistent with the estimates obtained where the dependent variable is the average test scores of all four subjects and the treatment measure is the average instructional time in these subjects.

In the second panel, I present the results of estimating equation (4) where each regression includes all four subjects' specific weekly hours of instruction as well as the sum of instructional time in all other subjects. The set of four estimates presented in each column is obtained from one regression. The table indicates that the estimates of the effect of instructional time of each subject on the same subject test score are very similar to the respective estimates reported in the first specification. The table also indicates that the cross effects results are almost all positive, though very small and not significantly different from zero. For example, the effects of math and science instructional time on English test scores are, not surprisingly, practically zero (-0.0032 and -0.0074). The largest cross effect is that of math instructional time on science test score, 0.0258. However, this effect is measured very imprecisely (se=0.0285) and therefore not statistically significant from zero. In comparison, the reciprocal cross effect of science instructional time on math is not important as this estimate is 0.0126 (se=0.0137). Interestingly, the table also confirms my earlier finding that instructional time of non-core subjects (representing 60% of the length of the school week) has no effect on achievements in any of the four core subjects: the estimates in the fifth row of the second panel of Table 7 are all positive but very small, and they are not significantly different from zero. This result is also consistent with the evidence presented in Table 6.[18]

Overall, these findings strengthen my previous results, as I did not find any cross effects within subjects, and between each of these subjects and instructional time of non-core subjects. This can be viewed as additional suggestive evidence that the estimated effect of instructional time of each of the subjects is not biased due to selection or endogenous determination of these educational inputs (that is not accounted for by our natural experiment and our school fixed effect difference in differences framework).

E. An alternative identification strategy: between subjects' variation in instructional time

In this section, I present additional estimates to those presented above that are based on an alternative identification method that can account for potential confounding factors in the estimation of instructional time. Here I rely on within-student variations in instructional time across various subjects of study to examine whether differences in student performance in three subjects are systematically associated with differences between subjects in instructional time. The basic identification strategy is that student characteristics and the school environment are the same for

[18] I also examined heterogeneity treatment effect estimates by gender, family education, and by the degree of heterogeneity in student ability in the classroom. The effect of additional instructional time for math is the same for boys and girls, but the effect of increasing science and English instructional time is larger for boys. When the sample is divided by the median value of the sum of one's father's and mother's years of schooling (a proxy for socioeconomic background), the estimated effect of math instruction is larger among children from families with low levels of parent education though this different is not statistically significant. The effect of English hours of instruction is higher for students from higher socioeconomic backgrounds, but this gap is not very large. These results are presented in the online appendix Tables A3-A5.

all three subjects except for the fact that some subjects receive more instructional time. It is important to emphasize that the pupil fixed-effect identification method proposed here does not exploit any variation in instructional time due to the funding reform. The variation in hours of instruction of different subjects across schools comes from the discretion that schools are subject to satisfying the national curriculum requirements of minimum instructional time in core subjects. I use the cross-section variation since I observe students when they are exposed only to one regime of funding. Based on this approach I present within student estimates of the effect of instructional time on individual test scores using the following panel data specification,

$$Y_{kij} = \mu_i + \gamma\, T_{kj} + \beta\, X_{ij} + \delta\, S_j + (\varepsilon_j + \eta_k) + u_{ijk} \qquad (5)$$

Where Y_{kij} is the achievement in the k^{th} subject of the i^{th} student in the j^{th} school, T_{kj} is instructional time in the k^{th} subject in the j^{th} school, X is a vector of characteristics of the i^{th} student in the j^{th} school and S_j is a vector of characteristics of the j^{th} school. ε_j and η_k represents the unobserved characteristics of the school and the subject, respectively, and u_{kij} is the remaining unobserved error term. The student fixed effect μ_i captures the individual's family background, underlying ability, motivation, and other constant non-cognitive skills. Of course, a specification that includes μ_i will not include the term $\beta\, X_{ij}$. Note that the individual fixed effect absorbs also the school fixed effect ε_j. Therefore, exploiting within-student variation allows one to control for a number of sources of potential biases related to unobserved characteristics of the school, the student, or their interaction. One potential source of bias is that students might be placed or be sorted according to their ability across schools that provide more (or less) instructional time in some subjects. For example, if more talented students attend better schools that provide more instructional hours overall in each subject, it would cause γ to be biased downward unless the effect of student and school fixed effects are accounted for. Similarly, the bias will have an opposite sign if the less talented students are exposed to more instructional time. Identification of the effect of instructional time based on a comparison of the performance of the same student in different subjects is therefore immune to biases due to omitted school level characteristics, such as resources, peer composition and so on, or to omitted individual background characteristics, such as parental schooling and income. Equation (5) can be estimated with a single year's cross-sectional data for each school, or it can be estimated using two years of data for each school. The latter allows including in the model a school fixed effect in addition to the pupil fixed effect.

This identification strategy is also subject to several key assumptions. First, a necessary assumption for this identification strategy is that the effect of instructional time is the same for all subjects, implying that γ cannot vary by subject. This restriction seems plausible if applied to the main academic subjects as evident from results obtained from the first identification method that I used: when estimating the treatment effect for these three subjects separately I obtained relatively similar estimates while the effect for Hebrew instruction time was much lower. I will therefore apply the pupil fixed effect estimation model while using only these three subjects. Second, the effect of instructional time is "net" of instructional time spillovers across subjects. This assumption is also supported by the evidence presented in Table 7 which showed that there are no significant cross-subject effects of hours of instruction. Third, this identification strategy does not preclude the possibility that pupils select or are sorted across schools partly based on subject-specific instructional time. For example, the results would be biased if students who have a high ability for math may select or be placed in a school that specializes in math and has more instructional time in math. However, I believe that this concern is not relevant for three reasons. First, the pupils in the sample are in fifth grade of primary school in Israel where admission is based on neighborhood school zones without any school choice. Second, primary schools that specialize in a given subject are very rare in Israel. Third, tracking within schools is not allowed in primary and middle schools in Israel, and Ministry circulars reiterate this issue frequently. Even if some schools overlook this regulation and practice tracking within school, this is not a major concern as I measure instructional time in each subject by the school-level means and not by the class means or even the within school program-level means. Therefore, omitted subject-specific student ability will not be correlated with subject instructional time in a given school.

Table 8 presents the estimates based on the within pupil estimation strategy.[19] Two different specifications are used. The regression results reported in the first column include year fixed effects, pupil demographic controls, and school characteristics. The regression reported in the second column includes also pupils fixed effects. In the first row I present estimates based on pooling together all three subjects. The two estimates are positive and significantly different from zero. The estimate in the second column is lower than the estimate in the first column, suggesting that the OLS estimates are slightly upwardly biased. Significantly, the estimated effect when pooling the three subjects is 0.058 (se=0.008), which is higher but not too different from the average (0.043) of the estimates of the three subjects reported in the first row of Table 7. This is a remarkable outcome since the two estimates are obtained from two very different identification strategies. As noted, in Tables 6-7 the identifying assumption is the comparison of results within schools for two adjacent fifth-grade cohorts where nothing has changed except the funding rules, and, therefore the first difference estimation at the *school level* is appropriate. In Table 8, the identifying assumption is that conditional on pupil fixed effects, hours of instruction of each subject are not correlated with the potential outcome (the error term in equation (5)), and therefore the estimation based on differences of all variables from pupil level means accounts for all potential omitted variables. The similarity in the estimated average treatment effect of instructional time suggests that the short-

[19] Since the treatment variable, instructional time, is measured at the school level, the error term $u_{ijk,}$ is clustered by school to capture common unobservable shocks to students at the same school.

term evidence presented in Table 7 is close to the estimated longer run effects presented in Table 8.

In the other rows of Table 8, I present estimates based on pooling only two out of three subjects. This is possible since all that is needed for this identification strategy is at least two or more observations per student. Remarkably, all three estimates in column 2 are very similar and range from 0.054-0.063, providing additional proof that the effects of instructional time on each of these three subjects are not very different. In addition, the average of the three separate effects is 0.059 which is identical to the estimate (0.058) in the first row that pools all three subjects.[20]

4.6 Effect of school instructional time on pupils' homework effort and satisfaction in school

In order to fully assess the overall benefit of an extended school week program or of interventions that add instructional time to some subjects, it is important to consider other important questions. For instance, do students who "enjoy" a longer school week study harder at home and spend more time doing homework? Are they more satisfied in school? Are they better off socially in class? Do they feel more secured in school and get less involved in violence and bullying?

In Table 9, I estimate the effects of classroom instructional time in each of the subjects on homework time allocation. This estimation is based on the policy variation using equation (2) with different outcomes as the dependent variables. For each subject I report results from a regression specification that includes school fixed effects and student and school characteristics as well as the instructional time in each of the other three subjects. The estimated effect of the latter three

[20] In separate estimation, I consider non-linearity in the marginal productivity of instructional time in each of the three subjects. I estimated regressions where the continuous instructional time measure was converted to dummy indicators, each indicator capturing a range of hours of instruction for each of the subjects. The results from this non-linear specification are reported in the online appendix Table A2. The estimates in the table suggest evidence of a slight increase in marginal productivity as hours of instruction increase.

variables will measure cross effects of instructional time of a given subject on the other three subjects' test scores. Both before and after the reforms, students spent approximately 3.2 hours per week doing math homework, 2.5 hours doing science homework, and 3 hours doing English homework.

The evidence presented in Table 9 suggests that homework time in each of the core subjects increases slightly with the subject's increased instructional time in school. This effect is significant for all three subjects and is largest for math and English though the effect sizes are relatively small. For example, an increase of an hour of school instruction in math or English leads to an average increase of four to five minutes of homework. Considering that students are engaged in homework for 2 to 3 hours per subject, the changes in students' time allocation in each of the three subjects are marginal. The evidence presented in the off-diagonal cells in the first three rows of the table also indicates that the cross subject instructional time estimated effects are very small and not significantly different from zero.[21]

These results imply that added school instructional time for any subject does not crowd out the time that students invest at home for the study of other subjects; to the contrary, it even marginally increases the overall time spent on homework. The implication is that an increase in instructional time at school leads to a net expansion of overall study time at home and at school together. Significantly, the additional time spent on homework is most likely a mechanism for the

[21] I also estimate the effect of instructional time in each of the core subjects on the probability that some students receive additional instruction from privately funded tutors at home. Private tutors work with 14 percent of the students in math, 5 percent of students in science, and 27 percent of students in English. Additional school instructional time in math and science has positive effect on the propensity of getting private tutoring but these effects are small and insignificant for all three subjects. The estimate for math instructional time is 0.003 (se=0.003), for science 0.002 (se=0.001) and for English precisely zero. Results are available upon request.

effect of increased school instructional time on test scores in math and English while for science this effect is not measured precisely to allow such conclusion.[22]

An additional question linked to the school reforms concerns whether additional time spent in school would come at the expense of overall social and school satisfaction. Thus, it is important to assess and measure these effects for a more general equilibrium evaluation of the effect of extending the length of the school week. In Table 10, I present estimates of the effect of increased school instructional time per week on five behavioral outcomes: personal violence in school, mean level of classroom violence, personal fear of violence in school, satisfaction from school, and social satisfaction in school. The regression specification includes school fixed effects and student and school characteristics. Column 1 reports estimates based on the full sample. In columns 2-3, I stratify the sample by gender. In columns 4-5, I present the results for the high and low parental education samples, respectively, and in columns 6-7 for the samples of homogenous and heterogeneous schools samples (as defined in footnote 18). The table indicates that there is no systematic pattern of an effect of the length of the school week on any of the five behavioral measures, and none of the five reported estimates in the first column is significantly different from zero.[23] The conclusion, therefore, is that extending the school week carries no negative

[22] I also examined heterogeneity treatment effect on home work hours estimates by gender, family education, and by the degree of heterogeneity in student ability in the classroom. These results are presented in the online appendix Tables A6-A8.

[23] It should be noted that there are some results that are significant such as the negative estimated effect for boys on school satisfaction. In addition, three other statistically significant parameter estimates in Table 10 are: the negative effect of the length of the school week on classroom violence in heterogeneous school, the positive effect on personal violence in homogenous schools, and the negative effect on school satisfaction in homogenous schools. These however are very small effects and therefore not economically meaningful.

repercussions in terms of satisfaction of students from school, the class social environment, or levels of school violence.

5. Conclusions

In this paper, I estimate empirically the effect of school resources and various measures of increased instructional time in school on students' academic performance and behavior. In particular, I analyze the effects of the total instruction budget in a school, instructional budget per class, the length of the school week, and the instructional time of the main academic subjects on test scores in these subjects and on homework time of students. I also assess students' school satisfaction, social acclimatization, violence, and fear of bullying. I take advantage of a policy reform in Israel that began in 2004 which changed the rules of funding public primary schools. The system changed from funding based on number of classes to a system based on number of students, weighted to take into account their average socioeconomic status. The results based on fifth-grade students' test scores clearly indicate that school resources, total or per class, and the length of the school week have a positive and significant effect on pupils' performance in core subjects. Importantly, the effects of increasing or decreasing school resources or the length of the school week are fully symmetric, as they are identical in absolute terms though opposite in sign.[24] The evidence also consistently shows that increasing the amount of instructional time of math, science, or English positively impacts test scores in that subject. The effect of changing hours of Hebrew instruction is much lower. Here as well, the effect of increasing or decreasing instructional time in the core subjects is symmetric. An alternative identification strategy based on a pupil fixed-effect estimation that exploits variation of time of instruction in different subjects yields exactly the same results. The evidence also strongly suggests that increased achievement in a given subject is the result of increased time on that particular arena of study alone. Results show that cross effects – the

[24] These results are not shown in the paper and are available from the author.

benefits of additional instructional time in one subject upon the achievements in another – are negligible. There is no spillover effect on math and English from increased instructional time in other subjects, and only modest increases in science, with the slight benefit stemming from increased math instructional time. In addition, there are no apparent cross effects from instructional time spent engaging in all other subjects such as history, geography, literature, and social studies on the main academic subjects' test scores. Increasing instructional time in math, science or English actually leads to an increase in the time at home that students spend on homework. Finally, the evidence suggests that expanding the school week does not diminish the school and social satisfaction of students.

This is the first paper that provides such detailed evidence on the causal effects of the school instructional time, in particular the length of the school week, and of instructional time in different subjects on students' academic performance and on important behavioral outcomes. The results are based on a sample that includes all the primary schools in Israel (with the exception of Arab and religious Orthodox Jewish schools). In this sample the means of the length of the school week and the time of instruction in the main academic subjects in Israel are very similar to the respective means of the OECD countries as observed in the PISA data from its various rounds in the previous decade. These two aspects provide an external validity appeal to the evidence presented here because they are relevant to many countries that seek ways to improve their education system. The evidence presented in the paper can also serve as a good benchmark for evaluating the effect and the cost-benefit of many "traditional" school interventions such as reducing class size, increasing teachers' training, and tracking student by ability. It can also serve as a benchmark for evaluating more recent popular "progressive" interventions in schools such as pay for performance for teachers or for students, or using computer added instructions in the classroom.

References

Angrist J., A. Abdulkadiroglu, S. Dynarski, T. Kane, and P. Pathak (2011). "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots", *Quarterly Journal of Economics*, Volume 126, Issue 2, May, Pages 699–748,

Angrist J. and V. Lavy (1999). "Using Maimonides' Rule to Estimate the Effect of Class Size on Children's Academic Achievement." *Quarterly Journal of Economics*, May, 533-579.

Benabou, R., Kramarz, F. and C. Prost (2009). « The French Zones d'Education Prioritaire: Much Ado about Nothing. » *Economics of Education Review*, 28(3), pp. 345-356.

Betts, J. R. and E. Johnson (1998). "A Test of Diminishing Returns to School Spending." mimeograph, University of California San Diego.

Betts, J. R. (2001). "The impact of school resources on women's earnings and educational attainment: findings from the National Longitudinal Survey of young women." *Journal of Labor Economics*, 19(3), pp. 635–57.

Card, D. and A. Krueger (1992). "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *Journal of Political Economy*, pp. 1-40.

Card, D. and A. Krueger (1996). "School Resources and Student Outcomes: An Overview of the Literature and New Evidence from North and South Carolina." *Journal of Economic Perspectives*, 10(4), pp. 31–50.

Dobbie, Will and Roland G. Fryer, Jr (2011) "Getting Beneath the Veil of Effective Schools: Evidence from New York City." Harvard University.

Eide, E. and M.H. Showalter (1998). "The Effect of School Quality on Student Performance: A Quantile Regression Approach." *Economics Letters*, 58, pp. 345-50.

Goodman Joshua "Flaking Out: Student Absences and Snow Days as Disruptions of Instructional Time." NBER Working Paper No. 20221, 2014.

Grogger, J. (1996). "Does School Quality Explain the Recent Black/White Wage Trend?" *Journal of Labor Economics*, 14, pp. 231-53.

Häkkinen, I., T. Kirjavainen and R. Uusitalo (2003). "School resources and student achievement revisited: new evidence from panel data." *Economics of Education Review*, 22(3), pp. 329–335.

Hanushek. E. A (2003) "The Failure of Input Based Schooling Policies." *Economic Journal* 113: F64-98.

Hanushek, E. A (2006). "School Resources", In Eric A. Hanushek and Finis Welch (Eds.), *Handbook of the Economics of Education, Volume 2*, pp. 865-908.

Hansen, Ben (2008). "School Year Length and Student Performance: Quasi-Experimental Evidence." University of California Santa Barbara.

Heckman, J., Layne-Farrar, A. and P. Todd (1996). "Does Measured School Quality Really Matter? An Examination of the Earnings-Quality Relationship." in Burtless, G. (ed.), *Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success*, Brookings Institution Press.

Lavy, Victor (2002). "Evaluating the Effect of Teachers' Group Performance Incentives on Pupils' Achievements." *Journal of Political Economy*, 1286-1317.

Lavy, Victor (2015) "Do Differences in School's Instruction Time Explain International Achievement Gaps in Math, Science, and Reading? Evidence from Developed and Developing Countries." *Economic Journal*, 125 (November), F39 7–F424.

Marcotte, Dave E. and Steven Hemelt (2008). "Unscheduled Closings and Student Performance." *Education Finance and Policy*, 3(3), pp. 316-38.

Pischke, J. S (2007) "The Impact of Length of the School Year on Student Performance and Earnings: Evidence from the German Short School Years." *Economic Journal*, 117(523), pp. 1216-1242.

Rivkin, G. Steven and Jeffrey C. Schiman (2015) "Instruction Time, Classroom Quality, and Academic Achievement." *Economic Journal*, 125 (November), F425–F448.

Thernstrom, Abigail, and Stephan Thernstrom (2004). "No Excuses: Closing the Racial Gap in Learning." Simon & Schuster.

Whitman, David (2008). "Sweating the Small Stuff: Inner-City Schools and the New Paternalism." Thomas B. Fordham Institute.

Table 1: Descriptive statistics: Instruction Time, Test Scores, Students' and schools' Characteristics

	Years	
	2002-2003	2004-2005
A. Instruction time		
Instruction budget per class	46.44	45.48
(in weekly hours)	(5.52)	(5.44)
Length of the school week	34.98	34.65
(in weekly hours)	(3.16)	(3.05)
Weekly instruction hours of math,	13.70	13.89
science and English	(1.64)	(1.71)
Weekly instruction hours of math,	19.36	19.69
science, English, and Hebrew	(2.29)	(2.46)
Weekly instruction hours in:		
Math	6.02	6.13
	(0.79)	(0.80)
Science	3.61	3.61
	(1.07)	(1.04)
English	4.07	4.15
	(0.68)	(0.69)
Hebrew	5.66	5.80
	(1.29)	(1.31)
B. Test scores		
Average test scores (math, science	71.05	75.26
and English)	(15.39)	(14.43)
Average test scores (math, science,	71.37	74.71
English, and Hebrew)	(14.11)	(13.75)
Math	72.41	72.12
	(19.15)	(16.84)
Science	(65.81)	(77.31)
	(17.757)	(15.773)
English	(73.45)	(75.14)
	(20.630)	(21.490)
Hebrew	(71.08)	(72.08)
	(15.936)	(16.675)

Notes: The table presents means and standard deviations for instruction hours, test scores, students' and schools' characteristics. Standard deviations are presented in parenthesis. The sample includes all fifth grade students from 845 Jewish secular and religious state schools. This sample includes over 60 percent of the schools and students in the country. Column 1 presents statistics for fifth grade students in 2002-2003, and column 2 presents similar statistics for fifth grade students in 2004-2005. Panel A presents instruction time means (in hours) for different subjects. Panel B presents average test scores for different subjects. Panels C and D present students' and schools' characteristics, respectively.

Table 1: Continued

	Years	
	2002-2003	2004-2005
C. Student Characteristics		
Boys	0.50	0.50
	(0.50)	(0.50)
Father's years of schooling	12.68	12.66
	(3.62)	(3.78)
Mother's years of schooling	12.91	12.93
	(3.21)	(3.41)
Number of siblings	2.38	2.35
	(1.43)	(1.43)
Immigrant indicator	0.112	0.083
	(0.316)	(0.276)
Native indicator (born in Israel)	0.790	0.797
	(0.407)	(0.402)
Former USSR ethnics origin	0.018	0.024
	(0.132)	(0.153)
Asia and Africa ethnics origin	0.161	0.153
	(0.367)	(0.360)
Europe or America ethnics origin	0.031	0.026
	(0.175)	(0.160)
Ethiopian ethnics origin	0.163	0.106
	(0.369)	(0.307)
Other ethnics origin	0.107	0.087
	(0.309)	(0.281)
D. Schools' Characteristics		
Enrollment	447.05	446.84
	(146.74)	(149.67)
Class size	28.32	28.16
	(3.94)	(3.86)
Religious school	0.21	0.21
	(0.41)	(0.41)
Number of schools	845	845
Number of students	51,043	52,400

Notes: See first page.

Table 2: Balancing Tests of Students' Characteristics with Respect to Whether a School Gained or Lost Instrucion Hours Due to the 2003 Funding Reform

	Sample			
	Schools for whom instruction budget increased	Schools for whom instruction budget decreased	Full Sample	
			Increase Effect	Decrease Effect
	(1)	(2)	(3)	(4)
Male Indicator (1 = Yes, 0 = No)	-0.017	-0.013	-0.017	-0.013
	(0.011)	(0.009)	(0.011)	(0.009)
Father's Years of Education	0.035	0.211	0.043	0.185
	(0.129)	(0.105)	(0.131)	(0.105)
Mother's Years of Education	-0.053	0.167	-0.039	0.141
	(0.122)	(0.097)	(0.123)	(0.097)
Number of Siblings	-0.080	0.023	-0.085	0.020
	(0.055)	(0.046)	(0.056)	(0.046)
Immigrant Indicator	0.003	0.003	0.004	0.003
	(0.008)	(0.006)	(0.008)	(0.006)
Native indicator (born in Israel)	-0.009	-0.001	-0.009	-0.001
	(0.013)	(0.011)	(0.013)	(0.011)
Other ethnics origin	-0.001	0.002	0.000	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Former USSR ethnics origin	0.016	0.000	0.016	0.000
	(0.013)	(0.011)	(0.013)	(0.011)
Ethiopian ethnics origin	-0.005	-0.001	-0.007	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Asia and Africa ethnics origin	-0.017	-0.010	-0.017	-0.009
	(0.007)	(0.006)	(0.007)	(0.006)
Europe or America ethnics origin	0.006	0.014	0.007	0.013
	(0.006)	(0.006)	(0.006)	(0.006)
Number of Observations	20,525	32,207	43,508	43,508

Notes: The table presents balancing tests of students' characteristics with respect to gain or loss of instrucion hours due to the 2002 funding reform. Column 1 presentes the estimates from a sample of schools that gained resources and the determinant is the size of this gain. Column 2 presents the respective estimates in a sample that lost resources follwoing the reform. In colums 3-4, I present estimates based on the full sample and balancing regressions including two determinants, the indicators of gaining or loosing resources. All samples include students from years 2004-2005. Standard errors are presented in parentheses and are clustered at the school level.

Table 3: Estimated Differential Trends in Students' Charactersitics

	Students' Characteristics		
	Father's Average Years of Education	Mother's Average Years of Education	Average Number of Siblings
	(1)	(2)	(3)
Increase	0.356	0.417	-0.045
	(0.204)	(0.194)	(0.083)
Decrease	-0.384	-0.256	0.019
	(0.188)	(0.180)	(0.078)
Grade 2	-0.113	-0.157	0.133
	(0.103)	(0.085)	(0.044)
Grade 3	-0.018	0.032	0.253
	(0.102)	(0.098)	(0.047)
Grade 4	0.075	0.038	0.185
	(0.095)	(0.089)	(0.043)
Grade 5	0.179	0.181	0.255
	(0.094)	(0.100)	(0.044)
Grade 6	0.167	0.162	0.242
	(0.101)	(0.094)	(0.045)
Grade 2 X Increase	-0.062	-0.060	-0.011
	(0.123)	(0.109)	(0.056)
Grade 3 X Increase	0.048	-0.117	-0.126
	(0.126)	(0.121)	(0.057)
Grade 4 X Increase	0.015	-0.072	0.040
	(0.121)	(0.111)	(0.055)
Grade 5 X Increase	-0.146	-0.216	0.024
	(0.119)	(0.122)	(0.058)
Grade 6 X Increase	-0.101	-0.211	0.049
	(0.129)	(0.123)	(0.058)
Grade 2 X Decrease	0.045	0.071	0.032
	(0.113)	(0.095)	(0.049)
Grade 3 X Decrease	0.059	-0.030	-0.037
	(0.111)	(0.107)	(0.052)
Grade 4 X Decrease	-0.020	-0.058	0.060
	(0.105)	(0.096)	(0.048)
Grade 5 X Decrease	-0.056	-0.137	0.000
	(0.104)	(0.109)	(0.050)
Grade 6 X Decrease	-0.044	-0.110	0.019
	(0.113)	(0.106)	(0.051)

Notes: The table presents the estimated cohort trends in students' characteristics and the differential trend by whether a school had experienced an increase or a decrease in instruction hours due to the 2002 funding reform. The student charcteristics used are parental years of schooling and number of siblings. Each column presents the estimates for one of these three student's characteristics. The grade variables are dummies for each relevant elementary school grade. Standard errors are presented in parentheses and are clustered at the school level.

Table 4: Estimated Effect of School Instruction Time on the Average Composite Score

	Year Control Only	School Fixed Effect	School Fixed Effect and Student's Characteristic	School Fixed Effect, Student's and school's Characteristics
	(1)	(2)	(3)	(4)
Instruction budget (in weekly hours)	-0.0001	0.0001	0.0001	0.0004
	(0.0001)	(0.0001)	(0.0001)	(0.0003)
Instruction budget per class (in weekly hours)	-0.0156	0.0085	0.0084	0.0084
	(0.0017)	(0.0030)	(0.0030)	(0.0031)
Number of schools	845	845	845	845
Number of sudents	80,018	80,018	79,509	79,509

Notes: Each estimate presented in columns (1)-(4) is from a different regression. Each row specifies a different independent variable that is included in the regression. The sample is the students' level observations. The dependent variables is the avergae standardized scores in math, science, English and Hebrew. All specifications include year fixed effects. Student characteristics include: gender dummy, both parents' years of schooling, number of siblings, immigration status and ethnic origin indicators. School characteristics include: enrollment and and enrollment squared. Standard errors are presented in parenthesis and are clustered at the school level.

Table 5 : First Stage, Reduced Form and 2SLS Estimates

	First Stage	Reduced Form	2SLS
	Instruction budget per class (in weekly hours)	Average Composite Score	Average Composite Score
	(1)	(2)	(3)
A. Using pre-reform enrollment in predicting the budget per class			
Predicted change in instruction budget per class (in weekly hours)	0.5011 (0.0595)	0.0036 (0.0033)	0.0072 (0.0065)
R^2	0.9660	0.7654	0.7654
B. Using the current enrollment in predicting the budget per class			
Predicted change in instruction budget per class (in weekly hours)	0.7057 (0.0686)	0.0053 (0.0040)	0.0075 (0.0056)
Number of schools	845	845	845
R^2	0.9687	0.7656	0.7653

Notes: The sample includes 845 schools and all variables used in the regressions are school means. The controls included in the regressions include year fixed effects, means of student characteristics (gender dummy, both parents' years of schooling, number of siblings, immigration status and ethnic origin indicators) and school characteristics (enrollment and enrollment squared). Standard errors are presented in parenthesis and are clustered at the school level.

Table 6: Estimated Effect of School Instruction Time Inputs on the Average Composite Score

	Year Control Only	School Fixed Effect	School Fixed Effect and Student's Characteristic	School Fixed Effect, Student's and school's Characteristics
	(1)	(2)	(3)	(4)
Length of the school week (in weekly hours)	-0.0170	0.0070	0.0079	0.0078
	(0.0029)	(0.0048)	(0.0046)	(0.0046)
Difference between instruction budget per class and length of school week	-0.0140	0.0041	0.0038	0.0037
	(0.0020)	(0.0026)	(0.0026)	(0.0027)
Average weekly instruction hours of math, science, English, and Hebrew	0.0119	0.0360	0.0366	0.0370
	(0.0168)	(0.0205)	(0.0207)	(0.0208)
Weekly instruction hours of all other subjects	-0.0165	-0.0012	-0.0007	-0.0007
	(0.0026)	(0.0046)	(0.0045)	(0.0045)

Notes: Each estimate presented in columns (1)-(4) is from a different regression. Each row specifies a different independent variable that is included in the regression. The dependent variables are avergae standardized scores in math, science, English and Hebrew. All specifications include year fixed effects. Student characteristics include: gender dummy, both parents' years of schooling, number of siblings, immigration status and ethnic origin indicators. School characteristics include: enrollment and and enrollment squared. Standard errors are presented in parenthesis and are clustered at the school level. See Table 4 for sample size (number of schools and number of students).

Table 7: Estimated Effect of School Instruction Time by Subject on Test Score

	Test Score			
	Math	Science	English	Hebrew
	(1)	(2)	(3)	(4)
Specification I: *Only own subject hours of instruction is included as treatment*	0.042	0.0344	0.0510	0.0192
	(0.0179)	(0.0160)	(0.0199)	(0.0103)
Specification II: *All four subjects' hours of instruction and total of other subjects' hours of instruction are included as treatments*				
Math instruction hours	0.0444	0.0258	-0.0032	0.0043
	(0.0191)	(0.0285)	(0.0230)	(0.0216)
Science instruction hours	0.0126	0.0354	-0.0074	0.0150
	(0.0137)	(0.0165)	(0.0144)	(0.0144)
English instruction hours	(0.0047)	0.0055	0.0581	-0.0452
	(0.0216)	(0.0215)	(0.0205)	(0.0197)
Hebrew instruction hours	0.0100	0.0088	0.0043	0.0234
	(0.0112)	(0.0122)	(0.0121)	(0.0111)
Other subjects' total weekly instruction hours	0.0044	0.0011	0.0087	0.0055
	(0.0070)	(0.0069)	(0.0075)	(0.0069)

Note: The table presents the estimated effects of various school instruction variables on test scores. All specifications include year fixed effects, student and school characteristics. Student's characteristics include a sex dummy, both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school characteristics are enrollment and enrollment squared. Standard errors are presented in parenthesis and are clustered at the school level. See Table 4 for sample size (number of schools and number of students).

Table 8: Estimated Effect of School Instruction Time on Test Score Based on Within Pupil Regressions and Assuming that the Effect of Instruction Time is the Same for all Subjects

Subjects Combination	OLS regressions with Pupil and School Characteristics	Regressions with Pupil Fixed Effect
	(1)	(2)
Math + Science + English	0.067	0.058
	(0.007)	(0.008)
Math + Science	0.072	0.054
	(0.008)	(0.010)
Math + English	0.079	0.063
	(0.011)	(0.017)
Science + English	0.054	0.060
	(0.008)	(0.013)
Number of schools	845	845

Notes: The table presents the estimated effects of school instruction time of test scores based on within-pupil regressions assuming similar effect of instruction time for all subjects. All specifications include year fixed effects. Student's characteristics include a gender dummy, father and mother years of schooling, number of siblings, an indicator of immigration status and five ethnic origin indicators. The school characteristics are enrollment and enrollment squared. Standard errors are presented in parenthesis and are clustered at the school level.

Table 9: Estimated Effect of School Instruction Time by Subject on Home Work Hours

	Math (mean=3.19, sd=1.54)	Science (mean=2.48, sd=1.54)	English (mean=3.01, sd=1.60)
	(1)	(2)	(3)
Math instruction hours	0.040	0.009	0.011
	(0.023)	(0.022)	(0.024)
Science instruction hours	-0.014	0.026	-0.014
	(0.015)	(0.015)	(0.016)
English instruction hours	-0.005	0.009	0.050
	(0.020)	(0.019)	(0.019)
Other subjects' total weekly instruction hours	-0.008	-0.001	-0.012
	(0.008)	(0.008)	(0.008)

Note: The table presents the estimated effects of school instruction time by subject on home-work hours. All specifications include year fixed effects, student's and school's characteristics. Student's characteristics include a sex dummy, both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school indicators are the enrollment and squared enrollment. Standard errors are presented in parenthesis and are clustered at the school level. See Table 4 for sample size (number of schools and number of students).

Table 10: Estimated Effect of Length of School Week on Violence and Student's Satisfaction.

	Sample						
		Gender		Family's Education		School Homogenity	
	Full Sample	Boys	Girls	High Education	Low Education	Hetrogenous Schools	Homogenous Schools
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Personal violence	0.000	-0.006	0.005	-0.004	0.004	-0.005	0.012
(mean=1.96, sd=1.42)	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)	(0.008)	(0.007)
Class Violence	-0.007	-0.010	-0.005	0.001	-0.010	-0.027	-0.003
(mean=3.62, sd=1.54)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.015)	(0.011)
Fear from violence at School	-0.006	-0.004	-0.008	-0.005	-0.008	-0.005	-0.002
(mean=2.01, sd=1.54)	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.008)	(0.007)
School Satisfaction	-0.005	-0.015	0.006	-0.004	-0.005	0.002	-0.017
(mean=5.18, sd=1.26)	(0.005)	(0.007)	(0.005)	(0.006)	(0.006)	(0.009)	(0.008)
Social Satisfaction	0.000	0.001	0.000	-0.002	0.000	0.001	-0.003
(mean=5.16, sd=1.24)	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)	(0.008)	(0.007)

Notes: The table presents the estimated effects of the length of school week on different measures of violence and student's satisfaction. All specifications include year fixed effects, student's and school's characteristics. Student's characteristics include: gender dummy, both parents' years of schooling, number of siblings, immigration status and ethnic origin indicators. School's characteristics include: enrollment and squared enrollment. Each parameter presented in the table is from a different regression. The values assumed by the violence and student's satisfaction raw variables range between lowest 1 to highest 6. The estimation results presented in columns (1)-(3) are based however on these variables' z scores. Standard errors are presented in parenthesis and are clustered at the school level. See Table 4 for sample size (number of schools and number of students).

On Line Appendix Tables

Not for Publication

Table A1: Prediction Regression of School Instruction Budget

	School Instruction budget (in Weekly hours)		
	2002-2003	2004-2005	
	(1)	(2)	(3)
Predicted number of classes	48.190	-	
	(0.936)	-	
Students' enrollment	-	1.234	
	-	(0.020)	
Decile dummy			Decile dummy X Enrollment
1st	(omitted)	(omitted)	1.07
			(0.02)
2rd	28.390	29.670	1.13
	(10.780)	(9.041)	(0.02)
3rd	40.630	43.950	1.15
	(10.590)	(9.804)	(0.03)
4th	44.670	49.730	1.17
	(10.330)	(9.044)	(0.02)
5th	83.740	82.900	1.24
	(12.240)	(10.500)	(0.03)
6th	112.600	100.500	1.28
	(11.900)	(9.316)	(0.03)
7th	114.600	113.600	1.33
	(11.000)	(9.456)	(0.03)
8th	137.200	132.400	1.38
	(11.590)	(10.240)	(0.03)
9th	157.100	153.700	1.47
	(12.590)	(10.870)	(0.03)
10th	206.300	190.000	1.70
	(17.220)	(15.260)	(0.05)
Constant	79.530	80.740	159.400
	(12.910)	(10.700)	(7.366)
R^2	0.814	0.8397	0.849
Number of schools	845	845	845

Note: Standard errors are presented in parenthesis and are clustered at the school level. Column 1 presents results of the for the regression of hours of weekly school instruction in the years 2002-2003 on the decile dummy of the deprivation index and the predicted number of classes. Column 2 presents results of the for the regression of hours of weekly school instruction in the years 2004-2005 on the decile dummy of the deprivation index and the student enrollment. Column 3 presents results of the for the regression of hours of weekly school instruction in the years 2004-2005 on the intercation between the decile dummy of the deprivation index and the student enrollment. Predicted number of classes is based on the rule of maximum 40 students per class and grade specific enrollent rates.

Table A2: Estimated Non-Linear Effect of School Instruction Time by Subject on Test Score

Test Scores								
Math			Science			English		
Grouping Hours	Proportion in Sample	Estimate	Grouping Hours	Proportion in Sample	Estimate	Grouping Hours	Proportion in Sample	Estimate
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
5 hours or less	20.04%	-	2 hours or less	16.96%	-	3 hours or less	16.05%	-
(mean=4.89)			(mean=1.96)			(mean=2.92)		
6 hours	58.25%	0.061	3 hours	30.69%	-0.007	4 hours	59.99%	0.028
		(0.011)			(0.013)			(0.012)
7 hours	14.98%	0.106	4 hours	33.17%	0.017	5 hours	20.04%	0.049
		(0.017)			(0.014)			(0.015)
8 hours or more	6.71%	0.068	5 hours or more	19.17%	0.059	6 hours or more	3.91%	0.157
(mean=8.21)		(0.023)	(mean=5.39)		(0.016)	(mean=6.14)		(0.024)

Note: The table presents the estmated non-linear effects of school instruction time by subject on test score. All specifications include year fixed effect, student's and school's characteristics. Student's characteristics include both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school characteristics are enrollment and squared enrollment. Standard errors are presented in parenthesis and are clustered at the school level. See Table 4 for sample size (number of schools and number of students).

Table A3: Estimated Effect of School Instruction Time by Subject on Test Score, by Gender

	Test Scores					
	Math		Science		English	
	Boys	Girls	Boys	Girls	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
Specification I:						
Only the subject's hours of instruction are included as treatment	0.046	0.041	0.050	0.018	0.074	0.030
	(0.021)	(0.020)	(0.018)	(0.017)	(0.022)	(0.024)
Specification II:						
Own subject's instruction time and instruction budget per class (in hours per week) are included as treatment						
Subject's instruction hours	0.046	0.040	0.049	0.017	0.070	0.026
	(0.021)	(0.020)	(0.018)	(0.017)	(0.022)	(0.024)
Instruction budget per class (in hours per week)	0.005	0.005	0.008	0.015	0.010	0.012
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Specification III:						
All three subjects' hours of instruction and total of other subjects' hours of instruction are included as treatments						
Math instruction hours	0.053	0.042	0.041	0.016	-0.013	0.010
	(0.022)	(0.021)	(0.031)	(0.030)	(0.025)	(0.025)
Science instruction hours	0.025	0.000	0.054	0.017	0.012	-0.028
	(0.016)	(0.016)	(0.019)	(0.018)	(0.016)	(0.016)
English instruction hours	0.025	-0.011	0.015	0.002	0.080	0.038
	(0.023)	(0.026)	(0.023)	(0.025)	(0.023)	(0.024)
Other subjects' total weekly instruction hours	0.011	0.000	0.006	-0.002	0.010	0.006
	(0.008)	(0.008)	(0.007)	(0.008)	(0.009)	(0.008)
Number of students	41,579	42,191	41,579	42,191	41,579	42,191

Note: Standard errors are presented in parenthesis and are clustered at the school level. All specifications include year fixed effects, student's and school's characteristics. Student's characteristics include both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school's characteristics are enrollment and squared enrollment.

Table A4: Estimated Effect of School Instruction Time on Test Score, by Subject and by Family Education

	Test Scores					
	Math		Science		English	
	High Education	Low Education	High Education	Low Education	High Education	Low Education
	(1)	(2)	(3)	(4)	(5)	(6)
Specification I: *Only the subject's hours of instruction are included as treatment*	0.020	0.059	0.034	0.035	0.059	0.044
	(0.019)	(0.023)	(0.017)	(0.019)	(0.019)	(0.026)
Specification II: *Own subject's instruction time and instruction budget per class (in hours per week) are included as treatment*						
Subject's instruction hours	0.020	0.058	0.033	0.034	0.054	0.040
	(0.019)	(0.023)	(0.016)	(0.019)	(0.019)	(0.026)
Instruction budget per class (in hours per week)	0.005	0.004	0.009	0.011	0.009	0.012
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)
Specification III: *All three subjects' hours of instruction and total of other subjects' hours of instruction are included as treatments*						
Math instruction hours	0.026	0.060	0.011	0.037	0.005	-0.013
	(0.020)	(0.024)	(0.029)	(0.033)	(0.022)	(0.029)
Science instruction hours	0.016	0.016	0.038	0.034	-0.007	-0.012
	(0.015)	(0.017)	(0.017)	(0.020)	(0.014)	(0.019)
English instruction hours	0.000	0.010	0.008	0.005	0.062	0.051
	(0.021)	(0.029)	(0.020)	(0.029)	(0.020)	(0.027)
Other subjects' total weekly instruction hours	0.010	0.001	0.006	-0.002	0.004	0.008
	(0.007)	(0.008)	(0.008)	(0.009)	(0.007)	(0.009)
Number of students	39,468	44,302	39,468	44,302	39,468	44,302

Note: Standard errors are presented in parenthesis and are clustered at the school level. All specifications include year fixed effects, student's and school's characteristics. Student's characteristics include a sex dummy, both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school characteristics are enrollment and enrollment squared.

Table A5: Estimated Effect of School Instruction Time on Test Score in Heterogeneous and Homogenous Schools

	Test Scores					
	Math		Science		English	
	Heterogeneous Schools	Homogenous Schools	Heterogeneous Schools	Homogenous Schools	Heterogeneous Schools	Homogenous Schools
	(1)	(2)	(3)	(4)	(5)	(6)
Specification I:						
Only the subject's hours of instruction are included as treatment	0.032	0.048	0.015	0.043	0.028	0.057
	(0.027)	(0.030)	(0.032)	(0.023)	(0.027)	(0.039)
Specification II:						
Own subject's instruction time and instruction budget per class (in hours per week) are included as treatment						
Subject's instruction hours	0.033	0.047	0.013	0.044	0.013	0.057
	(0.027)	(0.030)	(0.032)	(0.023)	(0.028)	(0.039)
Instruction budget per class (in hours per week)	0.009	0.004	0.024	-0.002	0.017	-0.001
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Specification III:						
All three subjects' hours of instruction and total of other subjects' hours of instruction are included as treatments						
Math instruction hours	0.035	0.042	0.080	-0.022	-0.010	-0.041
	(0.029)	(0.031)	(0.063)	(0.032)	(0.030)	(0.042)
Science instruction hours	0.007	0.005	0.008	0.040	-0.004	-0.027
	(0.023)	(0.023)	(0.032)	(0.024)	(0.024)	(0.023)
English instruction hours	0.012	0.000	0.024	-0.021	0.037	0.057
	(0.030)	(0.041)	(0.037)	(0.033)	(0.029)	(0.040)
Other subjects' total weekly instruction hours	0.007	-0.010	-0.005	-0.005	0.010	-0.005
	(0.013)	(0.010)	(0.014)	(0.011)	(0.011)	(0.013)
Number of students	41,664	42,106	41,664	42,106	41,664	42,106

Note: Standard errors are presented in parenthesis and are clustered at the school level. All specifications include year fixed effect, student and school characteristics . Student's characteristics include a sex dummy, both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school characteristics are enrollment and enrollment squared.

Table A6: Estimated Effect of School Instruction Time on Home Work Hours, by Subject and by Gender

	Home Work Hours								
	Boys								
	Math			Science			English		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Math instruction hours	0.031	0.031	0.025	-	-0.005	-0.007	-	0.023	0.012
	(0.032)	(0.032)	(0.033)		(0.031)	(0.031)		(0.033)	(0.034)
Science instruction hours	-	-0.016	-0.023	0.010	0.009	0.007	-	0.002	-0.010
		(0.020)	(0.021)	(0.020)	(0.020)	(0.021)		(0.022)	(0.022)
English instruction hours	-	-0.026	-0.032	-	0.005	0.003	0.066	0.065	0.054
		(0.026)	(0.027)		(0.026)	(0.027)	(0.027)	(0.027)	(0.028)
Other subjects' total weekly instruction hours	-	-	-0.011	-	-	-0.003	-	-	-0.020
			(0.010)			(0.010)			(0.011)
Number of schools		845			845			845	
Number of students		37,743			37,738			37,715	
	Girls								
Math instruction hours	0.077	0.076	0.071	-	0.016	0.015	-	0.028	0.021
	(0.031)	(0.032)	(0.032)		(0.026)	(0.027)		(0.028)	(0.029)
Science instruction hours	-	0.007	0.001	0.053	0.051	0.051	-	-0.002	-0.011
		(0.019)	(0.020)	(0.018)	(0.018)	(0.020)		(0.019)	(0.020)
English instruction hours	-	0.016	0.011	-	0.025	0.025	0.062	0.061	0.054
		(0.030)	(0.030)		(0.025)	(0.025)	(0.026)	(0.026)	(0.026)
Other subjects' total weekly instruction hours	-	-	-0.009	-	-	0.000	-	-	-0.013
			(0.010)			(0.010)			(0.010)
Number of students		38,562			38,506			38,489	

Note: Standard errors are presented in parenthesis and are clustered at the school level. All specifications include year fixed effects, student and school characteristics. Student's characteristics include both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school characteristics are enrollment and enrollment squared.

Table A7: Estimated Effect of School Instruction Time on Home Work Hours, by Subject and by Family Education

	Home Work Hours								
	High Education								
	Math			Science			English		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Math instruction hours	0.040	0.040	0.034	-	-0.022	-0.025	-	0.017	0.010
	(0.032)	(0.032)	(0.032)		(0.030)	(0.031)		(0.031)	(0.032)
Science instruction hours	-	-0.017	-0.025	0.030	0.028	0.024	-	0.011	0.001
		(0.019)	(0.020)	(0.020)	(0.020)	(0.021)		(0.019)	(0.020)
English instruction hours	-	0.004	-0.002	-	0.022	0.019	0.075	0.072	0.065
		(0.026)	(0.027)		(0.027)	(0.027)	(0.026)	(0.026)	(0.027)
Other subjects' total weekly instruction hours	-	-	-0.011	-	-	-0.006	-	-	-0.014
			(0.010)			(0.010)			(0.011)
Number of schools		845			845			845	
Number of students		36,059			36,036			36,020	
	Low Education								
Math instruction hours	0.064	0.065	0.059	-	0.018	0.016	-	0.027	0.015
	(0.029)	(0.029)	(0.030)		(0.026)	(0.027)		(0.030)	(0.031)
Science instruction hours	-	0.009	0.003	0.028	0.028	0.026	-	-0.016	-0.028
		(0.018)	(0.019)	(0.018)	(0.018)	(0.019)		(0.021)	(0.021)
English instruction hours	-	-0.016	-0.021	-	0.008	0.006	0.049	0.050	0.039
		(0.026)	(0.027)		(0.024)	(0.024)	(0.025)	(0.025)	(0.025)
Other subjects' total weekly instruction hours	-	-	-0.010	-	-	-0.003	-	-	-0.020
			(0.010)			(0.010)			(0.011)
Number of students		40,246			40,208			40,184	

Note: Standard errors are presented in parenthesis and are clustered at the school level. All specifications include year fixed effects, student and school characteristics. Student's characteristics include a sex dummy, both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school characteristics are enrollment and enrollment squared.

Table A8: Estimated Effect of School Instruction Time on Home Work Hours, by Subject and by School Homogenity

	Home Work Hours								
	Hetrogenous Schools								
	Math			Science			English		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Math instruction hours	0.038	0.039	0.030	-	-0.027	-0.023	-	0.006	-0.003
	(0.045)	(0.045)	(0.046)		(0.039)	(0.040)		(0.040)	(0.042)
Science instruction hours	-	0.003	-0.010	0.044	0.042	0.048	-	-0.013	-0.027
		(0.026)	(0.030)	(0.025)	(0.025)	(0.029)		(0.029)	(0.033)
English instruction hours	-	-0.031	-0.043	-	0.018	0.023	0.097	0.100	0.087
		(0.035)	(0.036)		(0.033)	(0.035)	(0.034)	(0.035)	(0.037)
Other subjects' total weekly instruction hours	-	-	-0.018	-	-	0.008	-	-	-0.019
			(0.017)			(0.016)			(0.019)
Number of schools		550			550			550	
Number of students		38,036			38,014			38,000	
	Homogenous Schools								
Math instruction hours	0.029	0.027	0.003	-	-0.007	-0.020	-	0.021	0.001
	(0.040)	(0.040)	(0.041)		(0.040)	(0.042)		(0.042)	(0.044)
Science instruction hours	-	0.009	-0.013	0.044	0.044	0.032	-	0.020	0.002
		(0.021)	(0.022)	(0.025)	(0.025)	(0.026)		(0.027)	(0.026)
English instruction hours	-	0.030	0.018	-	-0.010	-0.017	0.050	0.047	0.036
		(0.043)	(0.042)		(0.039)	(0.039)	(0.038)	(0.039)	(0.038)
Other subjects' total weekly instruction hours	-	-	-0.031	-	-	-0.017	-	-	-0.026
			(0.011)			(0.014)			(0.015)
Number of schools		560			560			560	
Number of students		38,269			38,230			38,204	

Note: Standard errors are presented in parenthesis and are clustered at the school level. All specifications include year fixed effects, student and school characteristics. Student's characteristics include a sex dummy, both parents' years of schooling, number of siblings, immigration status and five indicators of ethnic origin. The school characteristics are enrollment and enrollment squared.