The Impact of Learning Time on Academic Achievement *

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ABSTRACT

As schools aim to raise student academic achievement levels and districts wrangle with decreased funding, it is essential to understand the impact that learning time has on academic achievement. Using regression analysis and a data set drawn from California's elementary school sites, we find a statistically significant and positive relationship between the number of instructional minutes in an academic year and school-site standardized test scores. More specifically, about 15 more minutes of school a day (or about an additional week of classes over an academic year) relates to an increase in average overall academic achievement of about 1.0 percent, and a 1.5 percent increase in average achievement for disadvantaged students, even after controlling for student and school characteristics. This same increase in learning time yields an expected 37 percent gain in the average growth of socioeconomically disadvantage achievement from the previous academic year. Placing this impact in the context of other influences found important to academic achievement, similar increases in achievement only occur with an increase of fully credentialed teachers by nearly seven percentage points. These findings offer guidance regarding the use of extended learning time to increase academic performance. Moreover, they also suggest caution in reducing instructional time as the default approach to managing fiscal challenges.

INTRODUCTION

Given the continued focus on the academic underperformance of primary and secondary public school students in the United States, policymakers continue to explore interventions to raise such performance. Educational leaders often recommend the use of extended learning time (ELT) as such an intervention. President Obama's Education Secretary Duncan expressed support for the use of federal stimulus funds for ELT in public schools (Wolfe, 2009). In addition, many educational reform organizations and think tanks have heavily promoted such an option (for examples see Aronson, Zimmerman, & Carlos, 1999; Farbman & Kaplan, 2005; Little, Wimer, & Weiss, 2008; Pennington, 2006; Princiotta & Fortune, 2009; Rocha, 2007; Stonehill et al., 2009).

While conventional wisdom may expect a positive relationship between additional hours in the classroom and higher standardized test scores, the scholarly evidence from empirical research on this subject is relatively thin. Voluntary after school programs are frequently cited as evidence that extending the learning day raises participants' academic performance (Farbman & Kaplan, 2005; Farland, 1998; Learning Point Associates, 2006). However, the success of after school programs for only those who volunteer to participate in such programs does not necessarily support the mandatory extension of the school day as a policy to raise all student test scores. Worth noting is that little of the existing research has focused on a broad range of schools that exhibit the type of socio-economic diversity present in many public schools in the United States. This is important due to the documented challenges that such diversity presents to raising the overall academic performance of students. Furthermore, school districts struggling to balance budgets during times of fiscal stress, and contemplating a decrease in teaching hours as a way to do it, need to understand the impact of this strategy on academic outcomes. Especially helpful would be how the effect of an expected reduction in learning time compares to the effect calculated for an alternative reduction in other inputs into a school site's academic outcomes.

The State of California offers a contemporary example. As part of the fiscal year 2011-12 state budget agreed upon by California's Governor Brown and the state's Legislature, a budgetary trigger was set in the agreement that if $4 billion in anticipated revenues do not materialize in January 2012, mandated cuts in the current budget year's expenditure go into place. One of the proposed cuts is a reduction of $1.5 billion to state support for K-12 public education made up through seven fewer classroom instructional days (see http://www.cdcan.info/node/340). Such a reduction would be over and above the decrease from 180 to 175 school days allowed by California legislation in 2008, and that most of its school districts had implemented by 2010 to offset continuing imbalances in their budgets (see http://californiawatch.org/k-12/majority-states-largest-districts-shrink-school-calendar-amid-budget-crisis). So what exactly would it mean for the achievement of learning outcomes if California – or for that matter, any state – reduced its required public school days by seven percent (down to 168 days from a previously required amount of 180 in 2008)? The current literature on this topic is unable to offer a reliable prediction.

Accordingly, we provide an empirical examination of how differences in classroom time at a sample of public elementary school sites affects measures of average standardized test scores recorded at these sites. We appropriately measure this impact through a statistical method (regression analysis) that allows us to control for other explanatory factors besides learning time that may cause differences in observed standardized test scores. Our results offer a way to estimate the effectiveness of extended learning time as a strategy to improve student achievement and close the achievement gap. More relevant to current challenges, these results also enable us to predict how student achievement would change if learning time decreases.

Next, we review the relevant literature that seeks to understand how learning time influences academic achievement. Following that, we describe the theory, methods, and data that we use for our empirical examination. Then we share the results of the regression analysis, focusing on the impact of extended learning time on academic achievement. The final section concludes with a discussion of the implications for policy and practice.

LITERATURE REVIEW

Using the economic logic of a production process, the more time spent to produce something (holding the other inputs into the production constant) the greater should be the quantity and/or quality of the output produced. Employing such reasoning, conventional wisdom among many policymakers is that increasing the time that students spend learning offers a simple and obvious way to improve educational outcomes. However, a search of the previous literature on the relationship between learning time and learning outcomes yielded little research that rigorously tests this conventional wisdom. Previous research did consistently indicate that the more time students spend *engaged* in learning, the higher the expected levels of academic outcomes (Borg, 1980; Brown & Saks, 1986; Cotton & Savard, 1981). Yet, the relationship between just the time allocated to learning and student academic outcomes – without controls for the effective the use of that time –

remains unclear. This lack of clarity results from missing or insufficient controls for selection bias and other confounding factors, thereby making causal conclusions from the existing literature on this subject tenuous.. We offer next a review of previous research that aimed to assess how an increased allocation of time devoted to learning effects measures of academic achievement.

Our literature review begins with a description of a meta-analysis whose findings summarize much of the literature in the field. Next, we report upon two studies that have done well in their attempts to deal with these methodological concerns. Later we review a few studies whose reported findings we are less confident in due to methodological concerns.

In a recent meta-analysis, Lauer *et al.* (2006) reviewed 35 different post-1985 studies that focused on whether the voluntary attendance of after-school programs by at-risk students raised their academic achievement relative to a non-attending control group. They found that such studies generally offer statistically significant, but small in magnitude, effects of these programs on the math and reading achievement of at-risk students. For the impact on reading, students who participated in the after-school programs outperformed those who did not by 0.05 of a standard deviation from the mean for the fixed-effects model, and 0.13 standard deviations for the random-effects model. For the impact on mathematics, students who participated in the after-school programs outperformed those who did not by 0.09 standard deviations for the fixed-effects model, and 0.17 standard deviations for the random-effects model.

The Lauer *et al.* (2006) findings offered a general representation of the results reported in nearly all the empirical studies we reviewed. In short, voluntary extended learning programs tended to exert only a small (if any) impact on the measured academic achievement of those participating in them. Such findings make it difficult to predict whether any change in the amount of learning time at a school site would have a measurable impact on the academic outcomes of students at the site. We are also hesitant to place a great deal of confidence in these findings due to methodological concerns present in many of these studies. These concerns include the voluntary, and small in scale, nature of the ELT programs observed, and inadequate controls for other factors that drive differences in academic performance besides learning time. The likely result of using data generated from participants who voluntarily decided to extend their learning time is the inherent "selection bias" of attracting higher achieving (or perhaps more driven to succeed) students to participate in ELT programs. This results in uncertainty as to whether their observed higher achievement after the ELT program is due to the program itself, or non-measured personal characteristics that caused students to enroll voluntarily in the program.

Dynaski *et al.* (2004) offered an experimental (and a quasi-experimental) evaluation of the *21st Century Learning Centers Program*. This large and federally funded program provided extended learning opportunities to students who attempted to improve academic outcomes and offer non-academic enrichment activities. The authors' use of an experimental design to assess effectiveness offered a reasonable way to control for the selection bias of those who voluntarily participated in such a program being on average more engaged in learning that those who did not. However, Dynarski and colleagues were able to use an experimental design and address the problem of selection bias through an unplanned oversubscription to the program, which allowed a random assignment of those wanting to participate as the actual participants. The comparison they used was then between this treatment group and those who wanted to participate, but for whom a spot was not available. Accordingly, the authors' findings only allow us to draw inferences about students who wanted to participate in such a program.

Furthermore, the Dynarski *et al.* study compared the treatment and control groups to see if they were similar in other characteristics. The groups were not significantly different in gender, race/ethnicity, grade level, mother's age, academic traits, or disciplinary traits (with the one exception that the elementary school sample control was less likely to do homework). For elementary school students, the evaluation found no significantly discernible influence on reading test scores or grades in math, English, science, or social studies between those enrolled in the *21st Century Learning Centers Program* and the control group that was not. The authors also examined middle school students, but without a randomly assigned control group. Instead, they used a rebalanced sample based on propensity score matching – matching those who participated to a non-participant based on how alike they are. The treatment and control groups were similar for all characteristics, except the treatment group had lower grades, less-regular homework habits, more discipline problems, and felt less safe in school than the control group. For middle school students, there were again few differences in academic achievement between the extended-learning treatment and control groups. For both elementary and middle school students, across research designs, Dynarski *et al.* found little effect of the afterschool program on students' academic achievement.

Alternatively, Pittman, Cox, and Burchfiel (1986) utilized exogenous variation in the school year to analyze the relationship between school year length and student performance. Such an exogenous variation arose when severe weather led to schools closing for a month in several counties in North Carolina during the 1976-77. During that academic year, students took their standardized test after missing, on average, 20 days of school. The authors made year-to-year and within grade comparisons of individual student test scores for both before and after the shortened school year. Cross-sectional and longitudinal analysis also studied two cohorts of students impacted by the weather. Pitmna, Cox, and Burchfiel reported no statistically significant differences between the academic performances of students in the shortened school year in comparison to other non-shortened years. However, teachers reported that students were more motivated in the year with severe weather, which may have led to increased active learning time in school.

Vandell, Reisner, and Pierce (2007) sought to evaluate the impact of only "high quality" afterschool programs on academic and behavioral outcomes. The researchers whittled down a list of 200 programs to just 35 programs that they deemed as offering "evidence of supportive relationships between staff and child participants and among participants, and on evidence of rich and varied academic support, recreation, arts opportunities, and other enrichment activities" (p. 2). The 35 programs studied were free, offered programming four to five days each week, had strong partnerships with community-based organizations, and served at least 30 students who were largely minority, low-income students in high-poverty neighborhoods. The evaluation of 2,914 students occurred over a two-year period. Only 80 percent of the elementary school sample and 76 percent of the middle school sample remained at the end of the second year of the survey. It is not clearly stated how the control group was chosen and the authors do not compare the groups to ensure that they are similar.

To evaluate the impact of the afterschool programs, Vandell, Reisner, and Pierce used two-level (student and school) random-intercept hierarchical linear models (HLM) which is a form of regression analysis. HLM is useful when studying constructs where the researcher nests the unit of analyses (in this case, a student) in groups (in this case, a school) that are not independent. The authors analyzed elementary and middle school students separately and controlled for a number of background characteristics, including family income and structure, and mother's educational attainment. They found that elementary school students who participated regularly over the two years of the study increased their percentile placement of math test scores from 12 to 20 points (depending on the model) as compared to those who spent their afterschool hours unsupervised. While middle school students who participated regularly over the two years of the study improved their math test score percentile placement by 12 points over those who spent their afterschool hours unsupervised.

Vandell, Reisner, & Pierce (2007) found large, positive impacts of high quality afterschool programming. Their focus on only high quality programs was unique and clarified that only the "best" of the programs may have an impact. However, as noted previously, the issue of selection bias was again present in this evaluation. Students who chose to participate in an afterschool program are likely very different from those who chose not to do so. The authors of this paper did not discuss this issue, nor did the discussion of their model leave the reader feeling that their methods adequately adjusted for these differences. What we can confidently conclude from this study is that students who choose to participate in a *high quality* afterschool program, and do so regularly, have better outcomes than students who do not. We cannot say with any certainty that such *cream-of-the-crop* afterschool program would have the same measured positive academic effects on other types of students.

In another study, Farmer-Hinton (2002) examined a mandatory, two-hour, after-school remediation program and found that after one year (approximately one-month more of learning compared to non-participants), participants had increased math and reading achievement. The authors used HLM and controlled for individual and institutional factors to isolate the impact of the after-school program. These controls included student retention, race, gender, and family income; and school wide student mobility, percent African American, and percent in poverty. The use of such a model allowed the researchers to be more rigorous in assessing causality, but key controls like parental education are still absent. Of further concern is the fact that funds to support the afterschool program were competitive. Unfortunately, Farmer-Hinton offered no discussion of the selection criteria used. This competitive process introduced bias into her findings in at least two ways. First, school principals who applied for the funds are likely more shrewd about getting extra resources for their school. Such shrewdness may translate into other ways they found to increase student achievement. Second, the district could have chosen the school sites that received funds based upon some trait indicating they would be able to garner greater gains from implementing the program.

Frazier and Morrison (1998) examined kindergarteners and found those in a 210-day extended school year exhibited better beginning of first grade outcomes in reading, math, general knowledge, and perceived competence, than kindergartners enrolled in only a 180-day traditional school year. The study used both raw scores and growth rates to measure these academic outcomes, but failed to explain how to interpret both of these metrics. The match between kindergarteners enrolled in the extended school year with kindergarteners enrolled in traditional school years, occurred based on background characteristics and magnet school attendance. While the matched groups look largely the same, one cohort of the extended year students had mothers with statistically significantly more education and with greater employment levels than their matched traditional year peers. Given this, controlling for these variables would have made sense when analyzing differences in outcomes, but the authors simply compared means and score changes with one-way analysis of variance (ANOVA).

Another study by Hough and Bryde (1996) matched six, full-day kindergarten programs with similar half-day kindergarten programs based on location, school size, and student characteristics. The authors then used ANOVA to compare the outcomes of full- and half-day programs and found that full-day students outperformed half-day students on most outcomes. However, it was not clear the size of the performance difference between full-day and half-day kindergarten students, as the authors did not interpret the metrics used to evaluate achievement. Moreover, the authors could have strengthened causal claims by controlling for school, class, student, and family characteristics known to confound the relationship between outcomes and full-day enrollment.

The methodological and data problems in prior studies of the relationship between learning times and academic outcomes, and the inconsistent findings reported from them, clearly indicate a need for further research on this topic. Next, we describe the theory, methodology, and data used in our regression estimation of the influence of learning time on academic achievement.

METHODOLOGY AND DATA

Methodology

We situate our research firmly within the large number of empirical studies that already exist on the causal links between school inputs and academic performance produced at a school site. The consensus among these production-based studies is that student and social inputs (largely out of the control of educators and policymakers) explain more than half of the variation in school scores (Hanushek, 1986 and 2010).

Accordingly, we focus here on how the inputs that a school site has control over (including instructional time) contribute to its academic performance. We concentrate on the effect of differences in learning time at California elementary public school sites (in the form of regular academic hours) in academic year 2005-06 on differences in standardized test performance. The statewide collected Academic Performance Index (API) measures academic performance at a California elementary school site based on state-specified compilation of standardized test scores. In California, a school site's Academic Performance Index (API) ranges from a low of 200 to a high of 1000, with a score of 800 considered proficient. A further description and details on the API calculation for the year used (2005-06) in this study is at http://www.cde.ca.gov/ta/ac/ap/documents/infoguide05b.pdf .

We assess the influences of inputs into academic output as measured by both a school site's overall API score (base) and the change in its API score from the previous academic year (growth). California reports upon these measures for all students at a school site, and for students within specific subgroups (Latino, African American, Asian, White, and Socioeconomic Disadvantaged) for which a significant number of a certain type attends a school site. Though we examined the influence of learning time on all these groups, we only report regression results for the one subgroup (Socioeconomic Disadvantaged) on which learning time exerted a statistically significant influence..

Following Fisher (2007, Chapter 13), we divide the inputs expected to exert an influence into student, social, and school categories. Thus, we model the production of an average standardized API score at school site "i" as:

API$_i$, API Growth$_i$, Socioeconomic Disadvantaged API$_i$, or
Socioeconomic Disadvantaged API$_i$ Growth =

f (**Student Inputs$_i$**, **Social Inputs$_i$**, and **School Inputs$_i$**),

where,

Student Inputs$_i$ = f (Percentage Students African American$_i$, Percentage Students Asian American$_i$, and Percentage Students Latino$_i$),

Social Inputs$_i$ = f (Percentage Students Reduced Price Meals$_i$, Percentage Students Gifted and Talented$_i$, Percentage Migrant Education Program$_i$, Percentage Students English Lang Learners$_i$, Percentage Parents College Educated$_i$, Percentage Parents Grad School Educated$_i$, Percentage Parents Survey Response$_i$),

School Inputs$_i$ = f (Academic Year Teaching Minutes$_i$, Dummy Year Round Calendar$_i$, GradeKto3 Average Class Size$_i$, Grade4to6 Average Class Size$_i$, Percentage Teachers Full Credential$_i$, Percentage District Budget to Teachers$_i$, and Enrollment$_i$).

We realize that the inputs placed in the student and social categories are interchangeable based upon the perspective taken. We base our placement on the viewpoint that student inputs are ones that are inherent to students (race and ethnicity) and unchanged by a student's social environment. We are limited in the number of control variables we can actually measure based on what data are publicly available. That said, the specific ones chosen control for a number of student, social, and school inputs that determine differences in average standardized test scores. Thus, we are optimistic that this model will allow us to capture the independent influence of Academic Year Teaching Minutes on average standardized test scores at a California public elementary school site. We estimate the above model using regression analysis, which allows the calculation of regression coefficients for each explanatory variable. If deemed statistically significant, such regression coefficients measure the expected impact of a one-unit change in an explanatory variable on the dependent variable. The standard errors calculated for the regression coefficients in this analysis are robust to heteroskedastic concerns that are likely to be present. If statistically significant, regression coefficients indicate the expected midpoint effect of a one-unit change in an explanatory variable to the dependent variable, holding all other explanatory variables constant. We offer next a description of how and where the data were gathered for the regression analysis, and descriptive statistics for all variables used in it.

Data

Our study was constrained by the limited amount of information collected on the number of school minutes in an academic year at a California public elementary school site. We were frankly surprised to learn that in California, and even throughout the United States, information on public school learning time is rarely collected. In California, a statewide attempt to assemble data on learning minutes (as measured by "allocated class time") in a school for the state's school sites was last attempted for the 2005-06 academic year as a required element in data submitted to the California Department of Education as part of it School Accountability Report Card Program (for a description see http://www.cde.ca.gov/ta/ac/sa/parentguide.asp). We were further surprised to learn that the required reporting of this data was weakly enforced and therefore not available for all the state's school sites. This is the case even though for the past 13 years it has been a requirement for all public schools to complete and publish their School Accountability Report Card.

We put together a sample of California school sites to include by first gathering a list of the 5,087 elementary schools in California that existed in academic year 2005-2006 and had greater than 500 students enrolled. With a desire to gather a random sample of these schools greater than 500 in number to guarantee an adequate amount of degrees of freedom in our analyses, we then sorted them in order of enrollment and chose every ninth school. This resulted in a shortened list containing 565 sites. This list fell to 546 due to some sites not reporting standardized test scores in the desired years. We then contacted the school district offices for each of these 547 sites to see if they could provide 2004-05 Academic Year Teaching Minutes. Only 166 (or about 30 percent) of these sites had collected the desired information. Because we deemed 166 to be too small a sample size, we then went back to the same school districts that we knew had data for school site instructional minutes for a portion of the original 565 sites. This second effort resulted in a final sample of 310 California school sites for which we had 2005-05 instructional data. For these 310 sites, we next collected the other needed dependent and explanatory variables. We believe the quasi-random approach to gathering the data sample helped us to minimize selection bias. Furthermore, with the exception of some explanatory variables losing their statistical significance due to a smaller sample size, regressions run using only the initial purely random sample of 166 school sites yielded results that were essentially similar to the results we report from all 310 sites.

The four dependent variables used in this study measure: (1) the academic performance of all students at a school site, (2) the average academic performance of those defined as "socio-economically disadvantaged" by the California Department of Education as having both parents without a high school degree and/or the student receiving a reduced-price or free lunch, and (3 and 4) the change in such measures from the previous year. Data on these are all from the 2005-06 academic year. We also tried other group specific (African American, Asian American, and Latino) API base and growth scores for California school sites, but found that Academic Year Teaching Minutes never exerted a statistically significant influence on them.

Student input control variables include the percentage students in the school who were African American, Asian American, and Latino. This accounts for the three major racial/ethnic minority groups in California. Since whites and all other non-white groups are unaccounted for, the regression coefficients on these explanatory variables represent the expected effect of substituting one percent of a site's student population falling into the excluded category, with the respective racial/ethnic category measured. We also include the percentage of surveys returned to a school site after sent home to parents to inquire as to their education level. This response rate measure is included as an explanatory variable because we believe it can act as a measure of the broad "responsible" nature of parents at the school. Such responsibility may translate into parenting approaches that yield higher standardized test scores.

The social input categories include ones that account for the degree of poor families, students enrolled in a "gifted and talented" program, students whose parents are migrant farm workers and enrolled in a special program for them, students classified as English language learners, students who have at least one college educated parent, and students who have at least one graduate school educated parent. The regression coefficients on these explanatory variables represent the expected effect of a one percentage point increase of a school site's student population in a respective category from a one percentage point decrease in an unaccounted for social input category.

The school inputs account for factors over which school administrators have greater control. Besides teaching minutes at the site, this includes whether a school is on a multiple-track-year-round calendar, and average number of students in a class by grade. The regression coefficient on Dummy Year Round Calendar thus measures the amount that this form of scheduling either adds or detracts from a site's API. The regression coefficients on average class size for combined K-3 and 4-6 measure the expected influence on API if average students at either of these two grade categories increase by one. The regression coefficients calculated for Percentage Teachers Full Credential and Percent District Budget to Teachers, respectively, measure how a one-percentage point increase in fully-credentialed teachers and a one percentage point increase in the district's total budget allocated to teachers, change the site's academic performance. Number enrolled at the site is also included to account for any diseconomies of scale that can happen in the production of a site's test score, as its student body grows larger. We also tried the inclusion of a quadratic measure of enrollment to account for the possibility of first increasing and then decreasing returns to scale. We estimated that the quadratic measure to be insignificant (perhaps because our sample of school sites was never less than 500) and subsequently dropped from the final regression specification.

The explanatory variable of most interest is the calculated measure of Academic Year Teaching Minutes. We created this variable from information provided on the number of minutes at a school site for students in each grade between Kindergarten and Sixth. We combined these seven observations into one weighted measure for instructional average minutes at a site based on the minutes for each grade multiplied by the fraction of students at site in that grade, all summed together for all seven grades. The regression coefficient on this explanatory variable tells us the exact information wanted: ***How does a minute of additional average teaching time at a school site affect the site's overall API score after holding other explanatory factors constant?***

REGRESSION FINDINGS

Table I includes details on the source of each variable, number of valid observations, and descriptive statistics that include its mean, standard deviation, maximum, and minimum values. Table 2 offers a record of our regression findings. The cells in Table 2 contain the calculated regression coefficient and directly below that the standard error of the regression coefficient in parenthesis. The ratio of these two values yields a t-statistic that indicates whether the reported effect is as any different from zero (or non-existent) at greater than a 90 percent level of confidence. We indicate this at various levels by the presence of asterisks. As described earlier, our data set began with 310 school site observations, but as noted at the bottom of Table 2, the regression estimations used less than all of these observations due to the exclusion of school sites that lacked information for one or more of the explanatory variables.

Table 1: Source, Valid Observations, and Descriptive Statistics

Variable Name	Source	Valid Obs.	Mean	Std. Dev.	Max	Min
Dependent						
API	2005-06 API*	310	753.9	89.3	952	553
API Growth	2005-06 API	309	11.4	21.7	118	-41
Socioeconomic Disadvantaged API	2005-06 API	262	703.2	61.8	919	547
Socioeconomic Disadvantaged API Growth	2005-06 API	258	10.2	26.1	126	-55
Independent						
Percentage Students African American	2005-06 API	310	9.1	12.0	94	0
Percentage Students Asian American	2005-06 API	310	8.1	11.6	68	0
Percentage Students Latino	2005-06 API	310	49.6	31.0	100	0
Percentage Students Reduced Price Meals	2005-06 API	310	62.4	33.7	100	2
Percentage Students Gifted and Talented	2005-06 API	310	6.8	7.6	47	0
Percentage Migrant Education Program	2005-06 API	310	4.5	11.7	58	0
Percentage Students English Lang Learners	2005-06 API	310	34.8	24.9	92	0
Percentage Parents College Educated	2005-06 API	310	17.1	12.6	52	0
Percentage Parents Grad School Educated	2005-06 API	310	10.1	12.0	79	0
Percentage Parents Survey Response	2005-06 API	310	79.3	21.2	100	0
Academic Year Teaching Minutes	**Described in Text**	**306**	**51,836.6**	**2,589.4**	**63,063.0**	**43,489.7**
Dummy Year Round Calendar	2005-06 API	310	0.142	0.350	1	0
GradeKto3 Average Class	2005-06 API	306	19.5	1.7	31	13
Grade4to6 Average Class Size	2005-06 API	303	28.9	3.0	36	13
Percentage Teachers Full Credential	2005-06 API	310	97.5	4.3	100	67
Percentage District Budget to Teachers	SARC**	310	39.4	4.0	50.8	28.4
Enrollment	2005-06 API	310	406.2	174.8	1159	30

* 2005-06 API from California Department of Education, Base and Growth API Data Files by year, http://www.cde.ca.gov/ta/ac/ap/apidatafiles.asp.

** (SARC) School Accountability Report Cards contain this information, http://www3.cde.ca.gov/sarcupdate/clink.aspx for more recent years. This historical data provided by the California Department of Education's School Accountability Report Card Team at sarc@cde.ca.gov .

Table 2: Regression Findings

Dependent Variables Explanatory Variables	API (Mean=753.9)	Socioecon Disadv API (Mean=703.2)	API Growth (Mean=11.4)	Socioecon Disadv API Growth (Mean=10.2)
Constant	381.2115*** (125.0118)	214.1038 (145.2758)	-114.7702** (53.3239)	-152.4235** (75.2563)
Percentage Students African American	-0.6368*** (0.2950)	-0.1533 (0.3503)	0.0050 (0.1210)	0.1425 (0.1790)
Percentage Students Asian American	0.3428 (0.3481)	1.1038** (0.4571)	-0.1622 (0.1368)	0.0639 (0.1790)
Percentage Students Latino	-0.30329 (0.2583)	0.1324 (0.3467)	-0.1065 (0.1242)	0.0177 (0.1657)
Percentage Students Reduced Price Meals	-0.6817** (0.2650)	0.2371 (0.2573)	0.0965 (0.1048)	0.1509 (0.1377)
Percentage Students Gifted and Talented	1.9146*** (0.3231)	2.2874*** (0.5450)	-0.0882 (0.1768)	0.1055 (0.2730)
Percentage Migrant Education Program	-0.9673*** (0.2683)	-0.8737*** (0.2937)	-0.0550 (0.1500)	0.0189 (0.1607)
Percentage Students English Language Learners	-0.5720** (0.2886)	-0.8362** (0.3433)	-0.1318 (0.1380)	0.0810 (0.1566)
Percentage Parents College Educated	1.1821** (0.4986)	1.7701** (0.7157)	-0.0312 (0.2022)	-0.0691 (0.3984)
Percentage Parents Grad School Educated	0.8909*** (0.2722)	-0.6191 (0.7109)	0.1019 (0.1307)	0.2863 (0.5155)
Percentage Parents Survey Response	0.2736 (0.1815)	0.3186 (0.2009)	0.4291 (0.3776)	0.0720 (0.0983)
Academic Year Teaching Minutes	0.0031*** (0.0012)	0.0042*** (0.0016)	0.0016** (0.0005)	0.0015** (0.0007)
Dummy Year Round Calendar	18.433** (8.384)	24.3877*** (9.45237)	11.1223** (4.764)	13.4309** (5.2409)
GradeKto3 Average Class Size	0.2364 (1.809)	0.1343 (1.9870)	0.4744 (0.9183)	0.9162 (1.0734)
Grade4to6 Average Class Size	0.3469 (1.1240)	0.07556 (1.4667)	-0,2939 (0.5958)	0.2442 (0.7012)
Percentage Teachers Full Credential	1.7257** (0.6864)	1.5467* (0.7943)	0.2501 (0.3006)	0.2625 (0.3667)
Percentage District Budget to Teachers	1.5570** (0.7239)	2.0507** (0.9134)	0.4291 (0.3776)	0.6235 (0.5204)
Enrollment	-0.0338* (0.0197)	-0.0457* (0.0254)	-0.0293 (0.0094	-0.0387*** (0.0132)
Number of Observations	299	253	298	249
R-Squared	0.840	0.513	0.102	0.098

*** = 99% or greater confidence that regression coefficient is statistically significant from zero in a two-tailed test (p<0.01); ** = 95% to less than 99% confidence (p<0.05); and * = 90% to less than 95% confidence (p<0.10).

A general overview of these four different regression results reveals that our choice of explanatory variables do a much better job at explaining a school site's overall academic performance (as measured by API and Socioeconomic Disadvantaged API) than the change in these values from the previous academic year (as measured by API Growth and Socioeconomic Disadvantaged API Growth). As measured by R-Squared, these regressions respectively explained 84 and 51 percent of the variation in the school sites' API and Socioeconomic Disadvantaged API scores from the mean values across our sample of California elementary school sites. This level of explanation fell to around 10 percent when attempting to explain the change in these measures from the previous academic year.

Before turning to a specific explanation of the statistically significant influence of teaching minutes, it is important to point out that the results derived for the other explanatory variables included in the API and Socioeconomically Disadvantaged API regression reasonably matched *a priori* expectations. The following variables were associated with positive academic performance: Year Round Dummy, Percentage Students Gifted and Talented, Percentage Parents College Educated, Percentage Teachers Full Credential, and Percentage District Budget to Teachers. The positive influence to a school site's academic performance by using a year-round, multi-track school attendance calendar for its students is particularly noteworthy. The regression results show that this raises API scores by 18.4 points (with a 95 percent confidence interval that the affect falls between increases of 1.9 to 34.9 points) and Socioeconomically Disadvantaged API scores by 24.4 points (95 percent confidence interval of increases of 5.8 to 43.0 points). These expected midpoint increases are respectively 2.4 and 3.5 percent of the averages of these scores observed for all school sites. Furthermore, the regression results using API Growth and Socioeconomically Disadvantaged API Growth as dependent variables also show that the use of a year-round, multi-track calendar yields a positive influence on the change in a school's measured academic performance. The regression results show that a year-round calendar raises API Growth scores by 11.1 points (95 percent confidence interval of 1.7 to 20.5 points) and Socioeconomically Disadvantaged API Growth scores by 13.4 points (95 percent confidence interval of 3.1 to 23.8 points). These calculated midpoint increases are respectively 97.4 and 131.4 percent of the averages of the changes in these scores observed for all school sites. Although not the focus of this study, these results point to the potential payoff in higher academic scores from moving to a school calendar that involves the accommodation of more students and shorter periods off from study (see www.cde.ca.gov/ls/fa/yr/guide.asp for details on year-round education in California K-12 public schools.)

While in the opposite direction, Percentage Migrant Education Program, Percentage Students English Language Learners, and Enrollment all exert a negative influence on both API and Socioeconomically Disadvantaged API scores. Furthermore, Percentage Students African American and Percentage Students Reduced Price Meals only lower overall API scores, and not those calculated for the socioeconomically disadvantaged. Alternatively, Percentage Students Asian American only influenced the measure of a school site's socioeconomically disadvantaged academic score in a positive direction.

Turning now to the results of most interest for the topic of this investigation, we find that one additional minute of Academic Year Teaching Minutes yielded a 0.0031 rise in a California public elementary school site's overall API score. The same rise in teaching minutes yielded the larger 0.0042 increase in the site's API score calculated from only its socioeconomically disadvantaged students. The influences of Academic Year Teaching Minutes on change in these two measures of academic performance from the previous year are 0.0016 and 0.0015, respectively.

To help put these calculated influences in perspective, recall from the introduction that if anticipated state revenues do not materialize in California by January 2012, the governor is required by the previously agreed upon budget to institute a funding cut to public K-12 education that could be dealt with by cutting the remaining public school academic year by seven days. Assuming a six hour or 360 minute teaching day, such a cut would result in 2,520 (360 x 7) lost minutes of teaching. Multiplying this loss in teaching minutes by the regression coefficients calculated for Academic Year Teaching Minutes, yields the expected loss in overall API of 7.8 points and loss in socioeconomically disadvantaged API of 10.6 points. These are midpoint estimates, using the 95 percent confidence interval of these two effects, these predicted losses could range from 2.0 to 13.4 points for overall API, and 2.5 to 18.4 points for the socioeconomically disadvantaged API. Given that the average overall and socioeconomically disadvantaged APIs in the 2005-06 academic year samples were 754 and 703, the midpoint expected losses for these two scores respectively represent 1.0 and 1.5 percentage losses from the averages.

At first thought, an expected loss of 10.6 API points for the socioeconomically disadvantaged after reducing school days by seven days may not seem large, but an examination of the other inputs found to influence overall API scores should be used to place it in relative perspective. For instance, this expected loss of 10.6 API points is the same as if the percentage of college educated parents at a school site fell by 6.0 percentage points (10.6 / 1.77). Alternatively, a loss of 10.6 API points for the socioeconomically disadvantaged is expected if the Percentage Teachers Full Credential or Percent District Budget to Teachers respectively fell by 6.8 percentage points (10.6 / 1.55) and 5.2 percentage points (10.6 / 2.05).

Moreover, remember that these findings also work in the direction of forecasting the expected effect of increasing teaching days in the typical California public elementary school site when other factors expected to influence API scores held constant. With a desire to raise the socioeconomically disadvantaged API score for the typical California public elementary school site by 10.6 points, we suspect the cost is less for adding a week of learning time as opposed to raising the percentage of teachers full credential by 6.8 percentage points, or increasing the percent of a districts budget devoted to teaching by 5.2 percentage points.

Multiplying a loss in teaching minutes of 360 by the regression coefficients calculated for Academic Year Teaching Minutes in the API Growth and Socioeconomic Disadvantaged API Growth, yields the respective expected losses of 4.0 and 3.8. These are midpoint estimates, using the 95 percent confidence interval of these two effects, these predicted losses could range from 1.3 to 6.8 points for API Growth, and 0.5 to 7.1 points for the Socioeconomically Disadvantaged API Growth. Given that the average overall and socioeconomically disadvantaged APIs in the 2005-06 academic year samples were the much smaller 11.4 and 10.2, the midpoint expected losses for these two scores respectively represent the much larger 35.1 and 37.3 percentage losses from the averages.

CONCLUSION

We find that greater allotted instructional time has a statistically significant and positive impact on a school's average academic achievement after controlling for other student and school factors expected to influence achievement. Our hope is that these findings provide guidance for education administrators and policymakers in thinking about how to improve and/or maintain student achievement. The maintenance of student achievement is especially relevant in tight fiscal times when the conventional wisdom seems to be that cutting school time is the obvious and perhaps simplest way to ease budget constraints.

Our findings also yield important implications for the achievement gap. Finding that the impact of changes in learning time is greater for disadvantaged students than their more advantaged peers indicates that cutting school time would disproportionately affect the neediest students, potentially widening the achievement gap that already exists between the affluent and socioeconomically disadvantaged. Unlike disadvantaged students, more advantaged students likely have educational resources outside of school they can draw on to fill in for the lapse in learning time that occurs when public schools cut it back.

While our study provides rigorous, empirical evidence of the importance of instructional time, we were not able to study what schools did during those instructional minutes. Previous research on the use of class time indicates that what is done in class is at least as important as how much class time there is (Aronson, et al., 1999; Borg, 1980; Brown & Saks, 1986; Cotton & Savard, 1981). The well-established research finding on this topic is that engaged students, who actively participate in class, learn more than those who are not. As such, policymakers seeking to extend learning time should also make an effort to ensure that teachers are using the extended learning time effectively – generally meaning to ensure that active teaching and learning are happening in this time. Alternatively, policymakers seeking to cut learning time in schools may be able to minimize the negative consequences of reduced learning time by working with educators to ensure that a greater proportion of remaining instructional minutes are engaged learning time – it may be even possible to cut instructional minutes without cutting engaged learning time.

A final consideration is that schools that have greater instructional minutes may be different from those that do not in ways that impact academic achievement beyond the number of minutes in a school day. It is likely that communities that value education and can afford to have more school time are also those that have longer school days and higher levels of academic achievement. The higher levels of academic achievement may be partly a result of the extended learning time, but also may be a result of the value placed on education and greater financial resources that manifests itself in other ways. These ways could include parents working on homework with their kids at home, a community's ability to recruit teachers and principals that are more effective, and/or teachers' willingness to work longer days. While our regression analysis aims to control for many confounding factors, we clearly cannot control for everything.

In summary, we find that more time allotted for instruction results in higher academic achievement, especially for disadvantaged students, and supports extended learning time as a way to improve student outcomes. Realizing that states, districts, and schools face tight budgetary constraints, we urge policymakers to think critically before cutting school time and if they must do so, to invest in ensuring that schools use the time remaining in the most effective ways – in engaged and active learning time.

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