(Comments Welcome)

Class Size and Student Achievement in Developing Countries: Evidence from Bangladesh

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Abstract

This paper examines the effect of class size on student achievement in Bangladesh using data from a recent survey of secondary schools. We exploit a Ministry of Education rule regarding allocation of teachers to secondary grades to construct an instrument for class size and report a variety of OLS and IV estimates of the class size effect. This rule causes a discontinuity between grade enrolment and class size thereby generating exogenous variation in the latter. In such a quasi-experimental set up, researchers can effectively purge the effect of class size from the effects of other unobserved variables (such as ability) that are correlated with achievement. We find that all the OLS and IV estimates of class size effect have perverse signs: both the naïve and IV estimates yield a positive coefficient on the class size variable. Our results suggest that reduction in class size in secondary grades is not efficient in a developing country like Bangladesh. This finding also holds for various school types (e.g. public and aided schools, urban schools and poorer rural schools) and for schools that tend to have a monopoly in the local education market. Lastly, as by product, we find some evidence suggesting that greater competition among schools improve student achievement.

Key Words: Class size, Instrumental Variable, Student Achievement, School Competition.

I Introduction

A well-known puzzle in the economics literature on the educational production function is what factors matter most in educational production. It is a common perception that increased school inputs such as higher per student expenditure, higher teacher pay and, smaller class size improve student learning in school, i.e. school resources have a positive impact on student achievement. However, research shows little agreement on this issue. Starting from the 1966 Coleman congressional report to later studies done in the last two decades, most conclude that school inputs do not systematically impact outcomes. For example, 77 studies reviewed by Fuller (1986) for Developing countries, 147 studies reviewed by Hanushek (1986) for Developed countries, 96 studies reviewed by Hanushek and Harbison (1992) for developing countries and, more recently, another review and synthesis of studies for developing countries by Hanushek (1995), all fail to find any 'resource effect' in educational production. Not surprisingly, Hanushek (1995) notes:

"….research demonstrates that the traditional approach to providing more quality - simply providing more inputs - is frequently ineffective."

These reviews suggest that educational production is a black box, both in developing and developed countries[1]. Such dismal findings renewed further interest among social scientists, particularly economists, to look into the phenomenon of the absence of a school resource effect in the educational production process. In a review of this research, Kremer (1995) aptly explains why so many past studies have failed to identify any school resource effect. Most of the older studies are plagued by problems of omitted variable bias and the endogeneity of school inputs. Most studies, Kremer argues, are not based on randomised variation in resources. Hence inferences drawn may be false. The problem is that school inputs are frequently correlated with other unobserved determinants of educational outcomes. As such, school resources and student outcomes may be jointly determined, making it difficult to observe any resource effect in cross-sectional data[2].

Following these observations, economists have revisited the issue of the impact of school resource on student achievement[3]. These post-Hanushek studies exploit exogenous variation in school resources to identify causal resource effects and provide more reliable evidence on the issue. The most prominent school resource that has been at the centre of the debate is class size. Past studies, Hanushek (1995) claims, have been almost equally divided in their findings on the effect of class size on student achievement: there are as many studies

[1] However, Krueger (2002) disagrees with the earlier reviews of the literature on school resources by Hanushek. His analysis indicates that resources (and class size) are systematically related to achievement, when the individual studies reviewed are weighted carefully.
[2] The problem is complicated further by the fact that the direction of endogeneity and omitted variable bias in the naïve estimates is largely unpredictable. We discuss this issue later in the paper.
[3] Further motivation for such research comes from the fact that higher school quality (measured by increased spending) is found to have a positive effect on other outcomes such as labour market earnings, irrespective of the relation between school resources and test scores. For example, Case and Yogo (1999) find that the school quality in a respondent's magisterial district of origin has a large and significant effect on the rate of return to schooling for black men in South Africa. Similar prominent studies for the USA are Card and Krueger (1992) and Betts (1996) and the literature is reviewed in Card and Krueger (1996) and Betts (1999). However, Betts (1996) fails to identify the factors that could explain the effect of school quality on subsequent earnings of students in the labour market.

reporting a negative coefficient on class size as there are studies that find a positive coefficient. However, new research employing experimental or quasi-experimental data overcomes the methodological limitation by taking into account the endogenous nature of class size in educational production. Three most prominent recent studies that have succeeded in recovering the true "resource effect" find significant positive impact of smaller class size in student learning.

Krueger (1999) uses data from the STAR (Student Teacher Achievement Ratio) Project in the USA, a natural experiment of class sizes, where students and teachers were randomly assigned to classes of different sizes. Such randomised variation was then exploited to identify school input (i.e. smaller class size) effect. Case and Deaton (1999) use data from apartheid South Africa, where restrictions on residential choices of the Black households and their inability to influence the resource allocation pattern in schools of their own locality under the apartheid regime led to marked differences in the distribution of educational inputs by race. Such policies implied that school resources observed in black schools were not subject to parental choice. Hence resources were exogenous for black children and the observed relation between input and output can be taken to correspond to the true causal effect. The third paper is by Angrist and Lavy (1999) which applies the so-called "Maimonides' rule" regarding maximum class size in schools in Israel to identify the class size effect. All three papers identify a negative class size effect: students in smaller classes perform better[4].

Although these papers appear to provide convincing evidence in favour of a significant positive school resource effect, the literature is yet to arrive at any consensus in this regard. First, evidence from some of these papers is debatable[5]. Second, some of the recent evidence from natural experiments contradicts the findings from the other new papers: Hoxby (2000) fails to find an effect of class size on student achievement using data from the USA. Third, the new literature has examined the benefits of small class sizes mostly in the early years (of school education)[6]. For secondary grades, the effect is not adequately researched. Further motivation for a study on class size arises from the fact that the issue is not adequately research for developing countries. Developing country data from Bangladesh is interesting due to much larger (than that for most developed countries) average class size[7]. Many of the developed country studies on class size fail to find an effect probably because reduction in class size does not help (in developed countries like USA and in some developing countries like Bolivia) where classes are already small enough. So long as the range in which class size effect studied matters, Bangladeshi data offers a good prospect for re-examination of the issue.

The objective of this study is to look at the effect of school resources on student achievement in a developing country. Using recent national level micro data from various

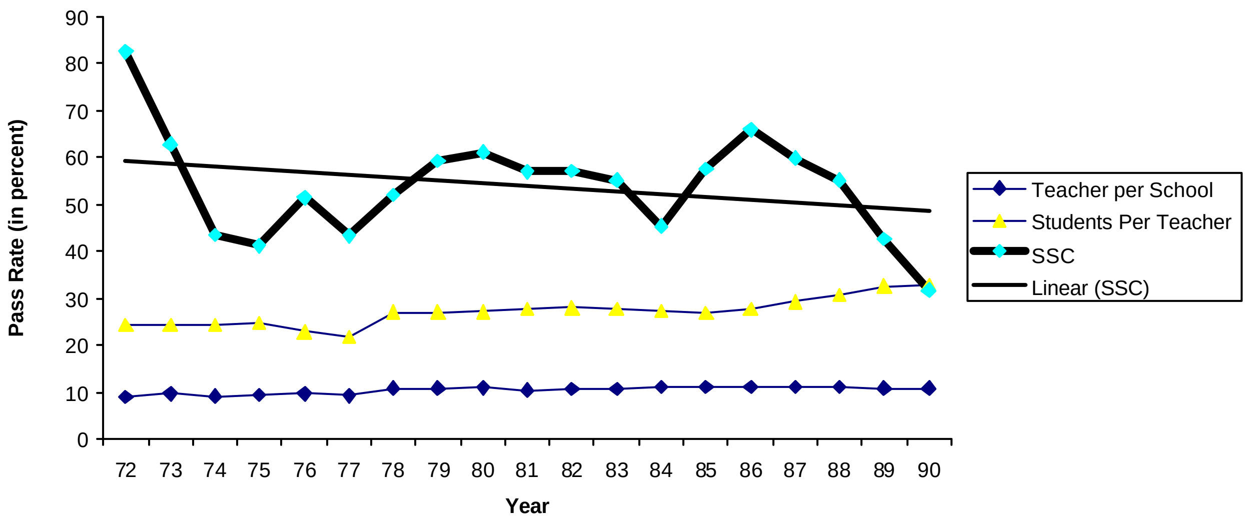
[4] Another recent paper (i.e. Urquiola, 2001) using teacher allocation rule as an instrument, also finds a negative class size effect for rural schools in Bolivia.
[5] For example, project STAR could be subject to two criticisms. The first reservation arises due to the explicit experimental nature of project where individuals were aware about their participation in an experiment and it is possible that this would have led to a modification in their level of efforts. Second, Krueger identifies a class size effect only when a large reduction (one third of the existing regular class size) in class size occurs.
[6] For example, recent studies such as Angrist and Lavy (1999), Krueger (1999), Urquiola (2001) and Iacovou (2001) all examine the class size effect only in the primary grades.
[7] However, class size in developing country is not necessarily always larger: Urquiola (2001) reports an average class size of 30 for Bolivia.

household and school surveys in Bangladesh, we look particularly at the effect of *class size* on student achievement in secondary schools.

Identifying school resources that boost student achievement is very important because of the poor performance of students in Bangladesh. Figure 1 reveals the persistence of a low pass rate in the Secondary School Certificate (SSC) examination, a nationwide public examination in Bangladesh, since its independence in 1971. Although research indicates that external efficiency of secondary education is low[8], the causes behind such poor performance in secondary education have not been adequately researched. For example, the issue of class size reduction in relation to secondary student achievement has not been looked at in Bangladesh. Figure 1 suggests no simple pattern over time between class size (student teacher ratio) and student achievement. Aggregate data analysis shows an increasing trend in class size (measured by student-teacher ratio) and no particular trend over time in student achievement score as measured by percentage passed in the SSC examination[9]. The concern about increasing class size in secondary education prevailed even in the pre-independence years when Bangladesh was part of Pakistan. A quotation from a government report nicely summarizes this concern:

"The academic and moral training of students depends largely upon a reasonable ratio between teachers and students and while the trend in other countries is to reduce it, the ratio in Pakistan has been increasing." (Government of Pakistan, 1960)

Figure 1: Trends in School Input & Output



Data Source: Bangladesh Bureau of Statistics (BBS).

8 For instance, Alam (1994) finds a high unemployment rate among secondary school completers in Bangladesh.

9 If anything, there appears to be a downward trend in achievement score.

Despite such increasing trend in class size, no study has till date examined the impact of (reduction in) class size on secondary school achievement. Clearly, little information is available to policy makers which could guide them to influence the process of resource allocation and boost learning in secondary schools. However, there is huge scope for such intervention in Bangladesh. Despite private ownership of the majority of secondary schools in Bangladesh, most (96%) of these schools are government aided (henceforth aided); the remaining 4% of the schools are either public or private unaided (henceforth private) schools (Hossain, 2000). A particularly interesting feature of the Bangladeshi education system is the provision of public aid to pay for teachers in aided schools: around 80% of the teacher salary in aided schools is public financed. Thus, public financing of the majority of secondary schools provides a potential means to influence the distribution of school resources in much of secondary education in Bangladesh.

The remaining part of the chapter is organized as follows. In section II, we discuss the estimation strategy. Section III discusses data. Section IV reports main results. Section V and VI discuss additional results. Section VII concludes.

II Estimation issues

Studies that estimate a student achievement function usually employ the following reduced form equation of the achievement function model[10]:

$$P_{ijk} = f\{H_i, T_{jk}, C_{ij}, S_j, R_j, \varepsilon_{ijk}^*\} \qquad (1)$$

where, P_{ijk}= Test Score of i-th individual in k-th class of j-th school; H_i= individual characteristics of the i-th student (e.g. ability, parental background etc.); C_{ij}= Characteristics of peer students in k-th class of j-th school; S_j= Characteristics of j-th school (e.g. school type, location etc.); T_{jk}= Vector of average characteristics of teachers (e.g. education, experience, training etc.) teaching the k-th class in j-th school; R_j = Vector of School resources (per student expenditure, class size, teacher pay etc.) in j-th school; ε_{ijk}^* = unexplained variation in P_{ijk} with mean zero and constant variance. However, some of the inputs contained in R_j are potentially endogenous e.g. “Class size (CS)”, “Teacher Pay (TP)” etc. Hence, OLS estimate of equation (1) does not have a causal interpretation. Since at the heart of estimating the educational production function is the issue of endogeneity of school resources, it is worth revisiting this issue. Two of the most popular school reforms i.e. ‘class-size reduction’ and ‘higher teacher pay’ revolve around inputs that are potentially endogenous. These variables are likely to be correlated with achievement score via various omitted variables. Naïve estimates would thus mask the true causal effects.

It is a common perception that class size reduction is good for student learning: students in smaller classes means greater per capita instruction time/teacher attention. Also, such classes require less teacher time to be devoted to disciplinary matters. In such a setting, one expects the coefficient on class size to be negative in achievement regressions. But, small classes are often also observed in schools that serve higher socio-economic status (SES) students as well as hostile, difficult-to-teach students leading to an ambiguous effect of class size. Parents of higher SES children may choose schools with smaller classes for their children.

[10] However, Hanushek (1971) notes that learning in school is a cumulative process and hence proposes a value added formulation of the achievement function where control for past school inputs is allowed.

Naïve estimates would then measure a mixture of "family background/SES" effect & "class size" effect and likely to be biased upwards. Even if SES is adequately observed and controlled, the problem of endogeneity remains if parents who care more about education send their children to schools with smaller class sizes. Furthermore, low ability students may be sorted out and placed in smaller classes by school authority. In this case, the coefficient on class size will be positive. Indeed Lazear (2001) shows that optimal class size is larger for better-behaved students[11].

Similar issues surround the effect of teacher pay in the educational production function. Higher teacher pay is argued to be good for learning. Better pay can help schools to choose superior teachers by attracting a larger pool of applicants to choose from. Teachers may also work harder when paid a superior salary as argued in the efficiency wage literature. But, once again, higher pay is often observed in schools serving students of higher SES; it is difficult to disentangle the true effect of teacher pay from the influence of SES of the students. In addition, if pay is tied to student performance, it gives rise to reverse causality in an achievement function using student test scores. These problems are at the heart of the puzzle that exists about the effect of teacher pay on student achievement. While researchers recognize that teachers and teacher quality are important in student learning (Hanushek, 1998; Behrman et al., 1997; Flyer and Rosen, 1997) others struggle to find a significant impact of teacher pay on learning in school. Thus Hoxby (1999) aptly notes: "it is hard to find evidence that teacher salary matters (to student achievement)....".

To sum up, endogenous variation in school resources - particularly class-size and teacher pay - means that the causal effect of these resources on output (student achievement) may remain unidentified. As highlighted by the recent research of Krueger (1999), Angrist and Lavy (1999), Hoxby (2000) and Case and Deaton (1999), the first best strategy is to use exogenous variation in these arguably endogenous variables in order to recover estimates of the underlying causal effect. In an experimental context such as Krueger (1999) and quasi-experimental setting such as Case and Deaton (1999), class size and teacher pay could be argued to be exogenous so that equation (1) would suffice as the correct specification of the underlying educational production function. Otherwise, one must explicitly treat class size as endogenous and adopt an instrumental variable approach (such as the one in Angrist and Lavy (1999) to identifying the true causal effect on achievement. In the latter case, the correct specification of the educational production function would be:

$$P_{ijk} = f\{ T_j, C_{ij}, S_j, R_j(\hat{CS}, \hat{TP}), e_{ijk}\} \qquad (2)$$

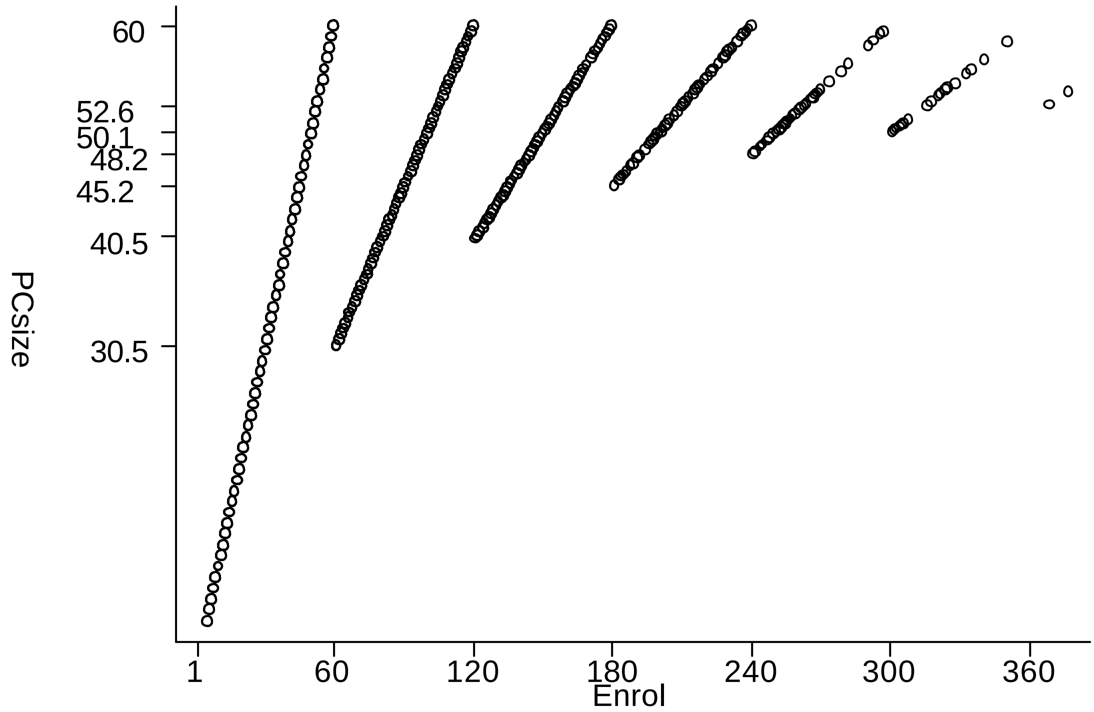
where capped CS and TP represent instrumented versions of endogenous regressors i.e. class size and teacher pay respectively.

However, finding economically sensible and statistically valid instruments is the key challenge. This requires identifying school reforms, government regulations etc. which would generate exogenous variation in school resources. In the presence of such variation, one can arguably sever the link between these resources and other omitted variables that causes selective assignment of students to teachers and/or classes. We have attempted to proceed in this line by closely studying the existing government policies that rule resource allocation across schools in the secondary education sector in Bangladesh. We were not able to identify

[11] We discuss Lazear (2001) in detail later.

any policies which discriminate students on the basis of their SES/race (as was the case in South Africa), and could generate exogenous variation in inputs allocated in schools attended by SES/race of students. Neither do we have any existing random experiment (like the project STAR in the USA) in Bangladesh which could generate the required data. However, in quest of a quasi-experimental setting, we identify a government rule which could serve as a potential source of exogenous variation in class size.

Figure 2: "Saw Tooth" Relationship Between Enrolment & Class Size Under the Teacher Allocation Rule in Bangladesh



Notes: PCsize = class size predicted by teacher allocation rule; Enrol = total enrolment in grade 10.

This rule is similar to the Maimonides' rule (a 12th century biblical rule governing class size) cited in Angrist and Lavy (1999) for Israel where schools seek additional teachers when enrolment in a grade exceeds 40 students. In Bangladesh, a Ministry of Education (MoE) circular maintains that registered secondary schools can recruit a new teacher if class enrolment exceeds 60 (Mia, 2001). Such a teacher allocation rule results in an abrupt drop in class size whenever observed grade enrolment exceeds 60 or an integer multiple of 60. The resulting distribution of class size predicted by this rule generates a saw tooth pattern when graphed against total grade enrolment. Figure 2 shows this graph for grade 10 enrolment data in our sample where average class size drops sharply at the corners of the class size function.

From Figure 2, it is obvious that students in schools with similar grade enrolment i.e. grade enrolment around 60 (or around multiples of 60) would experience different class-sizes. Class size equals grade enrolment till the total grade enrolment is less than or equal to 60. Once grade enrolment exceeds 60, say becomes 61, average class size drops to 30.5. This implies that in such a setting, class-size can be defined as a discontinuous (non-linear) function of grade enrolment and predetermined by the MoE rule. The true causal effect is recoverable if one uses the class size predicted by the teacher allocation rule as an instrument for actual (observed) class size in the achievement function.

While the MoE data that we use provides information on total grade enrolment, it does not collect information on actual class size. Hence despite the presence of a quasi-experimental context that generates the necessary information for identifying the true class size effect in Bangladesh, we are somewhat restricted in our analysis by MoE survey design: we cannot directly adopt the instrumental variable approach as adopted by Angrist and Lavy (1999). As proxy for class size, we use the school's average student-teacher (STR) ratio. The justification for this is that the literature routinely substitutes STR as a proxy for class size[12]. As elaborated later, we observe a strong correlation between class size predicted by the teacher allocation rule and the STR in our multivariate analysis. We are thus able to obtain IV estimates of our achievement functions using predicted class size as an instrument for STR.

For the other endogenous input, i.e. teacher pay, we searched extensively for variables that could serve as good instruments but were unable to find any suitable variable. In our dataset, one arguably exogenous source of variation in teacher pay in aided schools is 'school age' as the amount of government aid varies across schools by their years of operation (Mia, 2001). However, such variation exists only for the first 5 years from the time that the school registers with the government and receives aid. With the registration age exceeding 5 years, all schools receive same amount of aid for a given total of student enrolment, ceteris paribus. Since 99% of the schools in our sample are over 5 years of age, we cannot use this variation as an instrument. In the absence of a genuine source of exogenous variation, a common practice is to find variables which could at least serve as statistically valid instruments for the potentially endogenous variable [13]. But there is an emerging consensus in the literature that the IV estimates are unlikely to yield meaningful results unless there is a genuine experiment or quasi-experiment (Case, 2001). Hence, being unable to treat teacher pay as endogenous, we have excluded it from our main analysis and hence estimate only a reduced form achievement equation[14].

We report the IV estimates along with OLS estimates of achievement function, as the OLS estimates form a useful benchmark for the IV estimates. Further, heterogeneity in the data and substantial sample size allow us to check the robustness of our estimates for a variety of subpopulations. In particular, we are able to present the IV and OLS estimates by expenditure quintiles, teacher pay quintiles, school types (public and private aided), school locations (rural and urban) etc. These sub-sample estimates also offer a crude way to test for predictions made in Lazear (2001).

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[12] The educational production function literature routinely uses class size and STR as almost synonymous. For example, Case and Deaton (1999) uses district average of student teacher ratio as a proxy for class size.

[13] For example, Kingdon and Teal (2001) instrument teacher pay using teaching experience, teacher sex, union membership status of teachers etc. since these variables turned out to be insignificant determinants of student achievement but were well correlated with teacher salary.

[14] However, we report estimates of the model with control for teacher pay in the appendix treating pay as exogenous. As is shown later, exclusion of teacher pay variable does not make a significant difference to the coefficient on class size variable. Another endogenous variable excluded from our analysis is control for per student expenditure.

In his model on class size, Lazear argues that a little increase in the probability of disruption in classroom education can have a disproportionately negative impact on learning. If p is the probability that any given student is not an initiator of disruption (i.e. not interfering with his own or other's learning at a point in time), the probability that all students in the class of size n is behaving can be defined as p^n. In this setting, disruption occurs $1 - p^n$ times. The profit maximization objective of the school is:

$$\max_{n} \left[ZVp^{n} - Wm \right]$$

where, Z = total students in school; V = value of a unit of learning; n = class size (which is in turn equal to Z/m where m is the number of classes in school); W = per class expenditure (which includes class teacher salary and other class specific costs).

Lazear shows that, for a profit maximizing school, it is optimal to reduce class size when p is low (i.e. students are less well-behaved). Clearly, $n = f(p)$. Then, to the extent high p and low p students are sorted by school types, we are more likely to observe a class size effect in school types that educate a greater proportion of low p students. Potential examples of such schools could be low expenditure schools, schools with low teacher pay, schools located in remote rural areas etc.[15] Teachers in these schools (with large low p students) perhaps devote greater time in disciplinary matters. Reduction in class size would then help teachers reduce disciplinary time in favour of more instructional time, thereby leading to a positive effect of smaller class size.

In addition to the conventional inputs (such as class size and school types), we have attempted to model an important factor in educational production i.e. competition between schools. It is argued that competition among schools increases parental choice, obliging school managers to compete for students. Managers may alter the pattern of resource allocation within school and improve the quality of education in order to attract student[16]. Thus, the level of school inputs and the extent to which they are used effectively may depend on the extent of school competition within a given geographical area. For example, Hoxby (1994) shows that the presence of private school in an area improves the efficiency of nearby public schools that must compete for students from the same geographic area. We explore this possibility by including a "competition index" in our achievement function that records the additional number of schools that serve children within the union in the sample[17]. A union is a magisterial unit within a division. It consists of several villages but smaller than a thana (which consists of several unions) and hence, also smaller than a district.

[15] However, in a developed country context, such assumption is unlikely to hold where schools serving difficult-to-teach students and those with special learning needs are not necessarily poorly resourced and located in remote areas.

[16] Hoxby (1999a) finds that in the USA, schools operating in metropolitan areas where parents can choose more easily among school districts exhibit more challenging curriculum and more discipline oriented environment.

[17] The literature uses a measure of school concentration similar to the Herfindahl index frequently employed in the literature on market concentration (e.g. Hoxby, 1994; Marlow, 2000). However, we are unable to use such index in that we do not have data on number of students attending a given school within a given geographical area.

For the sample of aided schools, we also look at the effect of competition from the public schools by including a dummy for the presence of public school in the union[18]. This permits us to look at both competition within and between school types[19]. However, the interpretation of the meaning of the competition variable will not necessarily be unambiguous. To the extent that increased competition is correlated with population density and population density is correlated with SES, our measure of school competition may also partly control for unobserved SES effect. The analysis is essentially carried out in this paper is at the school level as we use a school's aggregate high school pass rate as measure of achievement. This is because we do not have achievement data at the individual student or class level but only for grade 10 as a whole. Thus, our estimation strategy can be summarized in equation (3) below.

$$P_j = \alpha + \phi Comp_j + \delta E_{j10} + \beta_{IV} \hat{CS}_{j10} + \sum_{ij} F_i\{SchType_{ij}\} + \varepsilon_j \qquad (3)$$

where, P_j = aggregate pass rate in SSC examination in j-th school (i.e. fraction of grade 10 students passing the examination by securing more than 60% marks)[20]; $Comp_j$ = competition index (total number of other secondary schools operating in the same magisterial union with j - th school); E_{j10} = Total enrolment in grade 10; $\hat{CS}_{j10}$ = instrumented class size for grade 10 in j-th school; $SchType_{ij}$ = i-th type (Public, private aided, girls, boys, co-education, double shift etc.) of j-th school; ε_j = between school unexplained variation in P_j.

Clearly, $CS_{j10} = f_c(E_{j10})$ as $CS_{j10} = E_{j10}/n_{10}$ where n_{10} = number of classes in grade 10. Central to the identification strategy is to obtain $\hat{CS}_{j10}$ as a discontinuous function of E_{j10}. We obtain $\hat{CS}_{j10}$ from the following first stage regression which uses P_Csize_j as the identifying instrument:

$$CS_{j10} = a + \lambda(P\_Csize)_j + bComp_j + cE_{j10} + \sum_{ij} d_i\{Sch\ Type_{ij}\} + u_j \qquad (4)$$

P_Csize_j is obtained from equation (5) which predicts class size (following the MoE rule regarding maximum class size) as a discontinuous function of E_{j10}:

$$(P\_Csize)_j = E_{j10}/\{integer[(E_{j10} - 1)/C^{max}]+1\} \qquad (5)$$

[18] Though there is some possibility that children could be out-migrating from their own neighbourhood to attend schools in other areas, this is more likely to be a problem at the village level rather than at the union level.

[19] In addition, we intend to explicitly identify schools as competitive or non-competitive in terms of our competition index. However, the threshold value of the index, which could result in such a regime shift, is currently unknown. For example, it is not known whether competition becomes effective when there are two schools in the union or whether there is a threshold minimum number of schools needed in order for there to be effective competition. We will report estimates of the achievement functions splitting the sample by competitive and non-competitive schools if we successfully test for a threshold effect in the competition index. The procedure is detailed in Hansen (2001).

[20] We explain the justification for using this measure of achievement later in the paper.

where, C^{max} = maximum class size (which is 60 for Bangladesh). Hence, our primary parameter of interest is $\hat{b}_{IV}$ obtained from equation (3) and the naïve estimate $\hat{b}_{OLS}$ which is the OLS estimate of class size effect obtained from the regression

$$P_j = \alpha + \phi Comp_j + \delta E_{j10} + \beta_{OLS} CS_{j10} + \Sigma_{ij} ?_i\{SchType\} + \varepsilon_j * \qquad (6)$$

For P_Csize_j to be a valid instrument in equation (4), it must be that $\hat{l}_{OLS} \neq 0$. In our discussions of the results, we thus report the 1st stage regression (equation 4) along with OLS and 2SLS regression results.

A priori, one would expect that $\hat{b}_{OLS} < 0$ and $\hat{b}_{IV} < 0$ i.e. a reduction in class size improves student achievement. However, as discussed earlier, non-random assignment of students to classes by teachers and parents may result in perverse sign on class size variable, making it difficult to predict the signs of $\hat{b}_{OLS}$ and $\hat{b}_{IV}$ in cross-sectional data. Given a positive relation between CS_{j10} and E_{j10}, the direction of bias in $\hat{b}_{OLS}$ depends on: (a) correlation of E_{j10} with unobserved characteristics of the students contained in ε_j and (b) the correlation between these unobserved characteristics and P_j. In general, one would expect the negative coefficient $\hat{b}_{IV}$ to be smaller than $\hat{b}_{OLS}$ [21]. But if CS_{j10} is measured with error, attenuation bias may dominate resulting in a larger $\hat{b}_{IV}$.

Lastly, some caveats on the goodness of our instrumentation strategy. First, for the teacher allocation rule to serve as a valid instrument, the rule must be binding on all schools. In this regard, examination of the underlying incentives facing the public, aided and private secondary schools is useful. The MoE also makes it a point that schools recruiting additional teachers (not guided by the MoE rule) will bear all the expenses arising from such additional un-authorised recruitment of teachers (Mia, 2001). This implies that public schools cannot deviate from the MoE rule in their decisions to recruit teachers. While aided schools can deviate in theory, in practice there are financial disincentives for them in doing so. Clearly, private schools have no explicit incentive to abide by the MoE rule. Hence, we have restricted our analysis only to the sample of public and private aided schools. Secondly, the discontinuity in the relationship between grade enrolment and class size (as defined in equation 5) is the source of our identifying information. $\hat{b}_{IV}$ is probably more precisely determined on or around the points of discontinuities generated by the MoE rule. On other ranges of enrolment (i.e. enrolment figures away from points of discontinuities), $\hat{b}_{IV}$ may be less precise. However, we are unable to test for this possibility due to a lack of degrees of freedom. Thirdly, additional selection biases may arise in analysis of between school comparisons of student performance. For example, studies using data from the USA well recognizes the problem where schools

[21] For example, children of superior SES are more likely to perform better in school. If their parents opt for schools with smaller class sizes, in a cross sectional study, $\hat{b}_{OLS}$ will be biased towards zero.

differ in student composition due to parental choice of schools and residential mobility. However, such concern is less serious in a Bangladeshi context. Parents are often restricted in their school choice due to sparse distribution of schools in a given residential neighbourhood, particularly in rural communities where most children attend the nearest secondary schools. However, such sample selection problem may recur when we carry out the analysis by school types. If parents choose systematically between public, private and aided schools, ideal strategy would be to control for probability of selection into various school types. However, this is impossible in analysis that employs school level data. That said, we discuss the data for achievement analysis in section 2. That said, we discuss the data for achievement analysis in section 2.

III Sample Data Description

The data for our analysis comes from a MoE survey for the year 1999. This is complemented by a recent household survey data - the Household Income and Expenditure Survey or i.e. HIES 2000. These datasets are described below.

i. MoE Data

The Bangladesh Bureau of Educational Information and Statistics (BANBEIS) collects data on all the secondary schools in Bangladesh that are registered with the MoE which results in a near census of existing secondary schools in the country. Such data was collected for the year 1999 which is summarized in the MoE post-primary national education survey report 1999. For our analysis of student achievement, we use test score data which relates to percent of students passing the secondary school certificate exam (SSC), a national level public examination. The SSC examination evaluates students in 10 separate papers. Any student appearing in the SSC test belongs to one of the three distinct groups: science, commerce or arts. The minimum requirement for passing the examination is that the student (belonging to any of the three groups) should obtain at least 33% marks in each of the ten papers. Students who obtain an average percentage mark of 60 or more pass in the first division. Passing the SSC examination in the first division is essential for securing admission in post-secondary education and leads to better labour market prospect[22].

Thus we use as our school-level achievement measure the percentage of grade 10 students of the school that passed the SSC examination in first division in the year 1999. The MoE survey data also contain detailed information on school resources, school types (i.e. public, private or aided) location (urban and rural), teacher characteristics etc. for all the public, aided and most of the private (unaided) secondary schools in Bangladesh.

We construct our main sample by extracting official records on some 2745 secondary schools in seven districts (which includes all the six Divisional head districts) in Bangladesh for the year 1999[23]. Such selection of schools by magisterial districts was unavoidable; the MoE was only willing to provide information on a subset of schools in their database[24]. Our sample includes all three types of secondary schools that exist in the country. Since we focus on achievement in the SSC examination, we drop all the 307 junior secondary schools from our analysis. Of the remaining 2438 schools, SSC score and enrolment data are missing for some schools. After ignoring those schools and dropping the private schools (as the MoE rule is unlikely to be binding for them), we arrive at a sample of 2165 schools for the purpose of our analysis[25]. Our sample consists of 17% of all the secondary schools, 22% of all the public

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[22] It is not uncommon to use matriculation examination pass rate as a measure of achievement (e.g. see Berg, 2002; Alam, 1994). The ideal measure is individual cognitive achievement score, something not available in our dataset.

[23] There are altogether 64 magisterial districts in a total of 6 divisions in Bangladesh.

[24] Part of the unwillingness on their part is explained by the fact that these data are not recorded in a format which allows easy distribution among researchers. It involved substantial computing time for the MoE officials to convert data into portable formats. We also had to undertake further measures to transform it into a format fully suitable for research and analysis.

[25] As such, there is a loss of approximately 9% of our original sample schools. This may create some selection issue if non-respondent schools were systematically different from those for whom responses are

schools and 17% of all the private secondary schools that existed in 1999 in Bangladesh. These schools are distributed in 828 unions over 91 Thanas in our 7 districts.

Table 1: Distribution of Secondary Schools in the Sample and Population for the Year 1999

	Public (Pb)	Private (Pr)	Total
Sample Size	71	2094	2165
Population Size	317	12297	12614

Source: National Education Survey (Post Primary, 1999), MoE.

ii. MoE Data combined with HIES 2000

The national Household Income & Expenditure Survey (HIES) 2000 is used to obtain data on student average family background at the union level. This data is merged with the MoE sample data to construct a matched sample. A unique id was created at the regional level (i.e. union) for merging HIES data with school data. Household variables (e.g. average household size, average per capita income and educational attainment) are aggregated at the union level in order to provide a control for student socio-economic status (SES). This sample includes information on 376 secondary schools. The reduction in sample size with control for SES is because, the national level HIES 2000 sample matched with only 376 unions in the seven districts (out of the 809 unions) over which our sample schools are distributed.

The descriptive statistics of the variables used in our analysis are presented in Appendix Tables 2 to 4a, with the variable definitions provided in Appendix Table 1. We observe a good deal of variation in our key variables of interest, namely test score and student teacher ratio (proxy for class size) across various sub-samples. Mean test score in public school is more that twice that in private aided schools. Public schools also have a considerably smaller student teacher ratio (35) than aided schools (46). Similarly, urban schools have a better average test score than rural schools, though student teacher ratio is almost similar (42 and 46 in urban and rural schools respectively). However, as discussed earlier, simple comparison of unconditional means is not informative when variations in means are driven by many observed covariates at a time and other unobserved factors. Thus we present the results from multivariate analysis in the next section.

known. However, we could not find any particular factor, which could generate the missing observations in a non-random fashion.

IV Main Results

Table 2 below presents the main results of our analysis. The corresponding descriptive statistics for the variables used are reported in the Appendix Table 2. The first four columns (set 'a') correspond to regression with union fixed effects (i.e. including 827 dummies for unions of locations). Regressions in the last 4 columns (set 'b') of Table 2 simply control for district fixed effects. In general, all the regressions reported later include district dummies wherever appropriate (i.e. when they are jointly significant). This specification allows inclusion of the competition index which is defined at the union level. In addition, in all the Tables presented, test scores (i.e. dependent variables) are formed by dividing the test score by the standard deviation of the score. As such, reported (size of the) coefficients show what proportion of a standard deviation change in test scores occurs due to a change in the independent variables. Such standardization facilitates comparison across studies that otherwise differ in their measures of student achievement i.e. test scores.

Table 2: Estimates of Achievement Regressions for Full (pooled) Sample

	(a)				(b)			
	OLS	Reduced form	1st Stage	2SLS	OLS	Reduced Form	1st Stage	2SLS
	Test Score	Test Score	Class Size	Test Score	Test Score	Test Score	Class Size	Test Score
Competition Index	-	-	-	-	0.065	0.065	-0.671	0.087
	-	-	-	-	(5.91)**	(5.89)**	(2.65)**	(5.52)**
Class Size	0.005	-	-	0.070	0.002	-	-	0.033
	(2.18)**	-	-	(3.4)**	0.88	-	-	(2.61)**
Grade Enrolment	0.002	0.001	0.078	-0.004	0.002	0.001	0.078	-0.001
	(3.03)**	(2.03)**	(9.97)**	(1.95)*	(4.06)**	(3.00)**	(10.44)**	-1.04
Pb_sch	1.348	1.216	-9.519	1.969	1.412	1.39	-12.529	1.805
	(6.79)**	(6.46)**	(4.5)**	(6.38)**	(9.42)**	(9.51)**	(6.96)**	(8.34)**
Sch_Sp_shift	-0.012	-0.045	-3.447	0.228	0.032	0.039	-4.701	0.195
	-0.070	-0.26	-1.54	0.93	0.25	0.31	(2.74)**	1.29
Sch_boys	0.212	0.181	-6.257	0.637	0.218	0.21	-7.69	0.464
	1.55	1.46	(3.79)**	(2.87)**	(2.05)*	(2.01)*	(6.79)**	(2.84)**
Sch_girls	-0.048	0.000	-0.585	0.031	-0.124	-0.105	-1.76	-0.047
	-0.710	0.010	-0.570	0.300	(2.14)*	-1.8	(1.97)*	-0.61
P_Csize	-	0.011	0.156	-	-	0.006	0.178	-
	-	(4.71)**	(4.97)**	-	-	(2.91)**	(6.54)**	-
Union Fixed Effects	Yes	Yes	Yes	Yes	No	No	No	No
District Dummies	No	No	No	No	Yes	Yes	Yes	Yes
Observations	2165	2165	2165	2165	2165	2165	2165	2165
R^2	0.60	0.59	0.65	0.25	0.23	0.23	0.34	0.07

Note: (1) T statistics are robust to clustering effect within unions and Huber corrected for hetroscedasticiy. (2) * significant at 5 percent level; ** significant at 1 percent level. (3) Omitted district dummy is Chandpur. (4) Base category for Pb school dummy is "PrA school". Base category for Sch_Sp_shift , Sch_boys and Sch_girls is "co-education".

The first and fifth columns in Table 2 reports the OLS estimates of achievement function whereas the third and sixth columns produce results from first stage regressions. In both sets 'a' and 'b' (i.e. with control for union and district fixed effects respectively), P_Csize (i.e. class size predicted by the MoE rule) is a strong determinant of actual class size, significant at 1 percent level in the first stage regressions. The resulting IV estimates are reported in the fourth and eight columns. Both naïve and IV estimates have the 'wrong' signs in specifications 'a' and 'b'. The size of the coefficient is small. For example, in specification 'b' a 10 percent change in class size changes test score only by .2 percent of a standard deviation in the OLS specification. However, the IV estimate is much larger than the OLS estimate. In addition, while the OLS estimate of class size effect is insignificant, the IV estimate is statistically significant at 1% level (in specification 'b').

Lastly, for 144 schools (6 percent) in the sample, a 'zero' test score (i.e. none passing in first class) was reported resulting in a clustering of test scores at the bottom. We thus estimated our model using a tobit specification (not reported in the text). However, the OLS results (reported in Table 2) are very stable to such alternate (i.e. tobit) specification. For example, the coefficient on class size in the tobit specification remained unchanged yielding a coefficient equal to .002 which is also statistically insignificant at 10 % level.

V Robustness of the Findings

We are interested in finding out whether the positive effect of class size is robust across schools of different types e.g. rural and urban, public and private. For example, it may be the case that class size reduction matters in schools which educate students from lower SES who may be first generation learners and therefore more difficult to teach. Does the provision of smaller classes in poorer areas - such as rural areas - boost student achievement? The results are reported in Table 3. Mean statistics are reported in the Appendix Table 4. The IV estimates of the coefficient on class size variable are .06 for urban schools and .05 for rural schools, though the estimate for rural schools is insignificant at 5 percent level. Thus there appears to be no difference in class size effect across schools by their geographic locations.

The Table 3 also reports regression estimates for public and aided school samples (Mean statistics are reported in Table 3 in the appendix.). This disaggregation is important given the commonly perceived differences in the quality of secondary education by school types in Bangladesh. Public secondary schools in Bangladesh are considered to be of superior quality, maintaining smaller classes and offering teachers a better pay. Hence, there may exist significant differences in the way students in public and aided schools benefit from larger classes. Perhaps, public schools employing superior teachers and resources are better able to teach students in smaller classes. However, the estimated class size effect continues to have a perverse positive sign in both public and aided school samples. Results for public school sample are statistically insignificant, unsurprising given the small sample size. For aided schools, the OLS estimate is statistically insignificant. While the IV counterpart is significant at 1 percent level, the effect is still small. A 10 percent change in class size raises student achievement score by less than 1 percent of a standard deviation.

Table 3: Estimates of Achievement Regressions for Sub-samples

	Urban School Sample			Rural School Sample			Public School Sample			Aided School Sample		
	OLS	1st Stage	2SLS	OLS	1st Stage	2SLS	OLS	1st Stage	2SLS	OLS	1st Stage	2SLS
	Test Score	Class Size	Test Score	Test Score	Class Size	Test Score	Test Score	Class Size	Test Score	Test Score	Class Size	Test Score
Competition Index	0.053	-0.205	0.063	0.036	-0.808	0.078	-0.006	-0.384	0.043	0.069	-0.66	0.093
	(3.37)**	-1.14	(3.40)**	(1.97)*	(3.69)**	(2.68)**	-0.12	-0.72	0.47	(7.61)**	(5.17)**	(7.13)**
Class Size	0.006	-	0.065	0.002	-	0.053	0.003	-	0.115	0.002	-	0.037
	1.77	-	(2.67)**	1.17	-	1.96	0.22	-	1.13	1.28	-	(3.08)**
Grade Enrolment	0.002	0.05	-0.002	0.001	0.127	-0.006	0.008	0.013	0.004	0.001	0.082	-0.002
	(2.85)**	(6.24)**	-1.26	1.38	(11.43)**	-1.68	(5.00)**	0.68	1.09	(3.99)**	(15.00)**	-1.71
Pb_sch	1.452	-10.1	2.028	0.705	-8.433	1.109	-	-	-	-	-	-
	(8.26)**	(4.84)**	(6.40)**	(2.56)*	(2.19)*	(2.85)**	-	-	-	-	-	-
Sch_Sp_shift	-0.106	-2.085	-0.003	0.016	-6.265	0.378	-1.363	2.862	-1.547	0.078	-5.282	0.283
	-0.63	-1.21	-0.02	0.09	-0.88	0.94	-1.9	0.38	-1.37	0.85	(4.05)**	(2.27)*
Sch_boys	0.233	-8.113	0.704	0.101	-6.551	0.43	-0.027	-2.418	0.344	0.148	-7.951	0.432
	-1.4	(4.48)**	(2.65)**	0.86	(3.59)**	1.8	-0.06	-0.56	0.48	1.6	(6.10)**	(3.05)**
Sch_girls	-0.076	-3.754	0.141	-0.182	0.542	-0.185	-0.841	1.24	-0.853	-0.101	-1.781	-0.014
	-0.6	(2.63)**	0.84	(3.13)**	0.52	(2.46)*	(2.05)*	0.29	-1.34	-1.88	(2.32)*	-0.21
P_Csize	-	0.241	-	-	0.095	-	-	0.292	-	-	0.171	-
	-	(4.12)**	-	-	(3.27)**	-	-	1.47	-	-	(6.49)**	-
District Dummy	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Observations	627	627	627	1538	1538	1538	71	71	71	2094	2094	2094
R^2	0.26	0.24	-	0.11	0.41	-	0.38	0.18	-	0.15	0.33	-

Note: (1) T statistics are robust to clustering effect within unions and Huber corrected for hetroscedasticity. (2) * significant at 5 percent level; ** significant at 1 percent level. (3) Omitted district dummy is Chandpur. (4) District dummies were excluded for Pb school regressions as they were not jointly significant at 5 percent level. (5) The 1st stage regression for the Public school sample has a very low F- statistics i.e. 1.34. An exogeneity test rejects the null only weakly at 7% indicating that endogenous regressor's effect on the estimates may be meaningful. So caution is required in interpreting reported estimates for Pb schools. (6) Base category for Pb school dummy is "PrA school". Base category for Sch_Sp_shift , Sch_boys and Sch_girls is "co-education".

Table 4 reports further estimates of the class size effect, splitting samples by expenditure quintiles. As such, these results may be considered as further estimates of class size effect in poorer and well off schools. Evaluation of class size effect for the bottom expenditure quintile sample is particularly important for two reasons. First, as discussed earlier, Lazear (2001) argues that it is optimal to reduce class size when students are less well-behaved. So long as the "less well-behaved" students attend poorly resourced schools, one may expect to find a class size effect in the sample schools with lower expenditure and teacher pay quintiles[26]. Second, there is some empirical evidence suggesting this possibility. For example, Krueger (1999) in his analysis of project STAR data finds that the effect of class size is largest in the case of minority students and those in inner-city areas who presumably have greater disciplinary problems (and attend schools that are poorly resourced).

The first four columns in Table 4 correspond to sample split by per student expenditure whereas the next 4 columns report results for observations split by average expenditure on teacher pay. The coefficient on the class size variable in the 2SLS estimate is .026 and is significant at 1 percent level of the bottom 50 percent of the expenditure quintiles. Similarly, class size has a positive (though the 2SLS estimate is insignificant) effect on achievement in schools that are located in the bottom 50 percent of the sample in terms of average teacher pay. All these results confirm our earning findings from Table 2 suggesting that class size policy does not matter even in the poorer schools i.e. schools with lower per student and per teacher expenditure.

In order to further explore impact of smaller classes in schools that are more likely to experience greater disciplinary problems, we estimated the class size effect for the sample of boys schools, girls schools, co-educational schools and schools operating in double shifts. However, we could not find a negative coefficient on class size variable for any of these four samples.

[26] In addition, given that teacher pay is the principal cost component of class size, class size effects are most likely to be observed when teachers are relatively inexpensive.

Table 4: Further Estimates of Achievement Regression for Sub-samples

	Bt 50% Exp		Top 50% Exp		Bt 50% Teacher Pay		Top 50% Teacher Pay	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	Test Score	Test Score	Test Score	Test Score	Test Score	Test Score	Test Score	Test Score
Competition Index	0.07	0.084	0.068	0.077	0.068	0.087	0.069	0.106
	(4.13)**	(5.47)**	(4.20)**	(5.74)**	(4.20)**	(3.84)**	(5.00)**	(4.05)**
Class Size	0.004	0.026	0.009	0.033	0.005	0.057	-0.001	0.039
	(2.26)*	(2.18)*	(3.58)**	(4.50)**	(2.08)*	1.39	-0.3	1.63
Grade Enrolment	0	-0.003	-	-	0.001	-0.006	0.001	-0.002
	-0.86	-1.54	-	-	1.28	-1.05	(2.74)**	-0.85
Pb_sch	1.533	2.059	1.729	1.834	1.761	2.623	1.666	2.22
	(3.76)**	(5.34)**	(10.13)**	(11.55)**	(3.50)**	(3.28)**	(9.33)**	(5.95)**
Sch_Sp_shift	0.474	0.462	0.031	-0.027	0.331	0.374	-0.037	0.221
	(2.49)*	(2.99)**	0.2	-0.21	1.42	1.78	-0.24	1.07
Sch_boys	0.306	0.314	0.129	0.189	-0.039	0.214	0.106	0.462
	1.11	1.72	1	1.64	-0.2	0.72	0.8	1.87
Sch_girls	-0.167	-0.215	-0.137	-0.066	-0.122	0.013	-0.122	-0.058
	(2.39)*	(2.76)**	-1.63	-0.76	-1.66	0.09	-1.5	-0.58
District Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1064	1064	1066	1066	1021	1021	1026	1026
R^2	0.21	0.1	0.22	0.17	0.16	-	0.26	0.02

Note: (1) T statistics are robust to clustering effect (standard errors are corrected for intra-union correlation) and Huber corrected for hetroscedasticiy. (2) * significant at 5 percent level; ** significant at 1 percent level. (3) Omitted district dummy is Chandpur. (4) Base category for Pb school dummy is "PrA school". Base category for Sch_Sp_shift , Sch_boys and Sch_girls is "co-education".

Further refinements of our results are presented in the Appendix Table 5 where we report regression results using the matched sample data which allows additional control for SES. i.e. including average educational attainment of household members in the union and average household size. Mean statistics are reported in the Appendix Table 4a. Clearly the OLS estimate reported earlier (in Table 2) are unchanged. The class size effect is once again positively signed and it is statistically insignificant at the 5 percent level. Same is true for the IV estimates though there appears to be fall in size of the IV estimate of class size effect as we control for SES. Also, it is no more significant at even 10 percent level.

In addition, in order to assess the validity of the IV estimates, it is important to examine whether control for effects of the variable that generates the discontinuity is adequate. Therefore, we also include additional control for enrolment by including square of total enrolment in grade 10 in the list of regressors. These results are reported in Appendix Table 6. Our results still hold though the significance of the excluded instrument (i.e. P_Csize) in the first stage regression is somewhat reduced. The IV estimate of class size effect in a model without (Table 2) and with (Table 5 in the Appendix) additional control has the same positive sign though the latter estimate is insignificant at the 5 percent level.

The distribution of schools across magisterial unions allows us to construct a unique sample of schools for which variation in school resources could be arguably less endogenous.

The rationale is that in the absence of any additional school in one's own locality (i.e. union), each school in the magisterial union is likely to enjoy monopoly power over educational services. In such a setting, parents facing non-competitive/monopoly schools would be limited in their school choice[27]. Hence, observed variation in inputs such as class size would mostly reflect variation in school policies towards resource utilization. The results are reported in the Appendix Table 7 and are consistent with earlier findings (Table 4a in the Appendix contains the mean statistics). The coefficient on class size is positive but very insignificant. Table 7 also reports estimates for a reduced sample of monopoly schools (excluding schools located in a Metropolitan area). Though the class size effect for this specification is negative, it is once again insignificant. Thus, we do not find a significant effect of class size effect on student achievement in Bangladesh.

Lastly, we re-estimate the specification reported in Table 4 with an additional control for average teacher pay (treating teacher pay as exogenous). Such a control may be important if class size and teacher pay are related. For example, Teacher pay is likely to be a good measure of teacher quality, and omission of teacher pay would bias the coefficient on class size though only upwards if schools assign better quality teachers to larger classes. The results are reported in the Appendix Table 9. Our earlier conclusions (reported in Table 4) remain unchanged. If anything, there is a slight reduction in the size of the coefficient on the class size variable in the IV specifications when we control for teacher pay. In addition, the IV estimates turn out to be insignificant when expenditures per teacher are held constant. This is true both for the full sample (where we control for missing observations on teacher salary, introducing a dummy) and restricted sample (where we ignore observations for which teacher salary data is missing).

VI Other Findings

Apart from analysis of the effect of class size, the regressions discussed so far yield some additional results. First, we find a positive effect of competition among schools (measured by the number of additional schools with which a school competes in same locality i.e. magisterial union). Schools that compete with others over the provision of educational services in a neighbourhood are more likely to allocate resources efficiently (Marlow, 2000). Such efficient resource utilization would then result in a positive achievement effect. In general, the majority of the results reported suggest that increased competition among schools has a statistically significant and positive effect on student achievement. The effect size is quite comparable to that for class size. For example IV estimates reported in Table 2 show a 0.6 percent increase of a standard deviation in test score for a 10 percent increase in competition among schools. In addition, 1st stage regression reported in Table 2 indicates that competition between schools has a negative significant effect i.e. schools facing higher competition tend to keep class sizes small. However, this may also be due to the fact that schools that face greater competition also operate in high population density or high demand regions. Hence, imperfect control for total enrolment may bias the estimated effect of competition[28]. We (imperfectly) check for this possibility in the

[27] Parents could still choose schools by migrating away from unions with no school choice. Though a common phenomenon in developed countries, educational migration is not very frequent in developing countries, particularly in Bangladesh. However, the possibility of fostering of children still remains.
[28] Another source of bias may result from any unmeasured differences in other union characteristics.

Appendix Table 6. After controlling for square of total enrolment in grade 10, our results still hold.

We further explore the effect of competition between school types. That is, we examine how achievement in the aided schools is affected due to competition arising from the presence of public schools in the same magisterial unions. In our sample of 2094 aided schools, a 7 percent of them operate in unions with at least one public school. An IV model (Appendix Table 8) that includes a dummy indicating such presence of a public school reports a 1.7 percent of a standard deviation increase in average test score in aided schools due to presence of a public school[29]. This effect is also statistically significant at 5 percent level. The reported coefficient on the competition index is .08 (significant at 1 percent level). Also interesting is the highly significant negative coefficient on the public school dummy in the 1st stage regression of class size. Presence of a public school in the same union reduces average class size in the aided schools by 2.8 percent.

Lastly, we find a strong school type effect. IV estimates indicate that test score is higher for public schools in comparison to aided schools by 18 percent of a standard deviation (see Table 2). This result is robust to all the alternate specifications reported in this section. This finding is in stark contrast with others for South Asia though consistent with earlier studies for Bangladesh (e.g. see Alam, 2000)[30].

[29] However, public school enrolment may be endogenous to aided and private school quality. For example, if low aided school quality raises the demand for public school, resulting coefficient on the public school dummy could be biased. However, this is unlikely to be a problem in our data in that such endogeneity would bias the coefficient on public school downward, assuming that unmeasured quality of private and aided schools is negatively correlated with public school enrolment.

[30] For example, Kingdon (1994) finds that private (unaided) schools outperform public and private aided schools in India.

VII Conclusion

In this paper, we have attempted to examine the effect of class size on student achievement in secondary schools in Bangladesh using cross sectional school data. In particular, we attempt to exploit a MoE rule regarding allocation of teachers to grades to construct an instrument for class size and report OLS and IV estimates of the class size effect for a variety of sub-samples and specifications. Such a rule causes a discontinuity between grade enrolment and class size, thereby enabling researchers to purge the effect of class size from other unobserved factors that are correlated with achievement. Application of the MoE rule to construct instruments yields strong first stage regressions in most of our sub-samples and specifications. Indeed R^2 for the 1st stage regressions reported in this paper are quite comparable to other studies such as Angrist and Lavy (1999). The survey design of the MoE however forced us to make some significant compromises in analysis; we use student teacher ratio as a proxy for actual class size. Further, we were unable to check whether our results hold exclusively for the discontinuity sample as well. With these limitations in mind, the following results are obtained from our analysis.

All the OLS and IV estimates of class size effect in our models have perverse signs: both the naïve and IV estimates yield a positive sign on the class size variable. On an average the naïve estimates of the effect of change in class size is approximately equal to zero. Though the IV estimates are somewhat larger (ranging from .02 to .07), in half of the specifications reported, they are statistically insignificant. All these results suggest that reduction in class size is not efficacious in a developing country like Bangladesh. This finding appears to be robust to changes in specifications or changes in the sample under study. We find that class size policy does not matter even in the poorer rural schools, schools with lower per student expenditure or lower average teacher salary, which tend to serve students from relatively poorer SES. Like Hoxby (2000), our findings thus indicate that the new literature that has claimed to identify a negative effect of class size on student achievement should be interpreted with care.

The differences in findings across these studies perhaps also reflect the fact that schools across countries operate in distinct ways, thereby resulting in different optimum class sizes. In addition, our study explores the class size effect only for secondary grades. Given that children in earlier (primary) grades are relatively more difficult to teach, smaller classes may still have a beneficial effect in the early years of primary education in Bangladesh. Average class size in primary school in Bangladesh is much larger than that in secondary grades. Indeed in a recent study on student achievement in primary schools in Bangladesh, Alam (2000) reports a negative coefficient on class size for students in grade 1. However, for students in grade four, all the coefficients have perverse (i.e. positive) sign indicating that class size reduction is perhaps inefficient in later primary school grades[31]. Third, benefits of class size may vary across subjects[32]. In such a setting, students tested for (relatively difficult) science subjects may benefit more from smaller classes than those examined for commerce and/or social sciences in the SSC examination. In this paper, we have not able to

[31] However, the negative coefficient is significant only for boys who were tested in Mathematics. In addition, all the estimates are from OLS regressions and hence likely to be unreliable for reasons discussed earlier in this chapter.

[32] Indeed Case and Deaton (1999) report a significantly larger class size effect for the maths test score in comparison to the comprehension scores .

test for this possibility due to unavailability of dis-aggregated data on test score. As such our data may have masked any beneficial effect of class size that exists for certain group of students (i.e. those taking difficult subjects).

To conclude, governments in developing countries like Bangladesh, where there already exist policies that generate quasi-experimental data, should be more rigorous in their survey design and data collection. This will allow more studies of superior quality based on randomised variation in school inputs in a developing country like Bangladesh.

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Appendix:

Table 1: Definition of Variables

Dependent Variable	
Test Score	Aggregate Pass rate in the SSC exam
Independent Variables	
Competition Index	Total No of additional schools in the union
Class Size	Measured by students per teacher
Grade Enrolment	Total student in grade 10
P_Csize	Class size Predicted by the MoE rule
Teacher Salary	Log of Average Monthly Salary Per Teacher
School Type Dummies	
Pb_sch	Dummy
PrUA	Dummy
Sch_Sp_shift	School operating in double shift (Dummy)
Sch_boys	Dummy
Sch_girls	Dummy
SES backgrounds	
Edu_attain	Average grade attainment in the union
LnHH_tot	Log of average household size in the union
District Dummies	
Dhaka	Dummy
Chittagong	Dummy
Sylhet	Dummy
Rajshahi	Dummy
Khulna	Dummy
Barisal	Dummy

Table 2: Descriptive Statistics of the Variables for Full Sample

Variable	Obs	Mean	Std. Dev.
Test score	2165	0.23	0.20
Competition Index	2165	2.92	2.26
Stu_Teacher	2165	45.13	14.89
P_Csize	2165	40.35	12.87
Teacher Salary	2074	8.39	.393
Grade Enrolment	2165	81.67	67.41
Pb_sch	2165	0.03	0.18
Sch_Sp_shift	2165	0.06	0.24
Sch_boys	2165	0.06	0.24
Sch_girls	2165	0.16	0.36
Dhaka	2165	0.19	0.39
Chittagong	2165	0.22	0.41
Sylhet	2165	0.09	0.29
Rajshahi	2165	0.15	0.36
Khulna	2165	0.11	0.31
Barisal	2165	0.15	0.35
Chandpur	2165	0.10	0.30

Table 3: Descriptive Statistics of the Variables by Sample of Pb & PrA Schools

	Public Schools			*Private Aided Schools*		
Variable	**Obs**	**Mean**	**Std. Dev.**	**Obs**	**Mean**	**Std. Dev.**
Test score	71	0.56	0.26	2094	0.22	0.19
Competition Index	71	3.41	2.46	2094	2.91	2.25
Stu_Teacher	71	35.84	11.09	2094	45.44	14.90
P_Csize	71	47.93	8.30	2094	40.09	12.92
Grade 10 Enrolment	71	154.75	88.34	2094	79.19	65.20
Pb_sch	71	1	0	2094	0	0
Sch_Sp_shift	71	0.04	0.20	2094	0.06	0.24
Sch_boys	71	0.45	0.50	2094	0.05	0.21
Sch_girls	71	0.38	0.49	2094	0.15	0.36
Dhaka	71	0.35	0.48	2094	0.18	0.38
Chittagong	71	0.17	0.38	2094	0.22	0.41
Sylhet	71	0.07	0.26	2094	0.09	0.29
Rajshahi	71	0.17	0.38	2094	0.15	0.36
Khulna	71	0.13	0.34	2094	0.11	0.31
Barisal	71	0.03	0.17	2094	0.15	0.36
Chandpur	71	0.08	0.28	2094	0.10	0.30

Table 4: Descriptive Statistics of Regression Variables for Sample of Urban & Rural Schools

	Urban Schools			*Rural Schools*		
Variable	**Obs**	**Mean**	**Std. Dev.**	**Obs**	**Mean**	**Std. Dev.**
Test Score	627	0.32	0.26	1538	0.19	0.16
Competition Index	627	4.03	3.14	1538	2.48	1.58
Stu_Teacher	627	42.95	15.82	1538	46.02	14.41
P_Csize	627	43.18	11.99	1538	39.19	13.04
Grade Enrolment	627	112.02	89.87	1538	69.30	50.79
Pb_sch	627	0.09	0.29	1538	0.01	0.09
Sch_Sp_shift	627	0.19	0.39	1538	0.01	0.10
Sch_boys	627	0.14	0.35	1538	0.02	0.16
Sch_girls	627	0.24	0.43	1538	0.12	0.33
Dhaka	627	0.45	0.50	1538	0.08	0.27
Chittagong	627	0.17	0.37	1538	0.24	0.42
Sylhet	627	0.04	0.20	1538	0.12	0.32
Rajshahi	627	0.10	0.31	1538	0.17	0.37
Khulna	627	0.10	0.31	1538	0.11	0.31
Barisal	627	0.07	0.26	1538	0.18	0.38
Chandpur	627	0.06	0.24	1538	0.12	0.32

Table 4a: Descriptive Statistics of Regression Variables for Matched Sample & Monopoly School Sample Data

	Matched Sample Data			*Monopoly School Data*		
Variable	**Obs**	**Mean**	**Std. Dev.**	**Obs**	**Mean**	**Std. Dev.**
Test Score	388	0.29	0.25	180	0.22	0.18
Competition Index	388	3.94	2.83	-	-	-
Edu_attain	388	6.89	1.29	-	-	-
LnHH_tot	388	1.75	0.16	-	-	-
P_Csize	388	42.93	12.27	180	42.09	11.96
Stu_Teacher	388	43.70	16.46	180	49.59	15.27
Grade Enrolment	388	102.50	86.77	180	93.14	74.41
Pb_sch	388	0.05	0.22	180	0.02	0.15
Sch_Sp_shift	388	0.16	0.37	180	0.08	0.28
Sch_boys	388	0.08	0.28	180	0.03	0.18
Sch_girls	388	0.20	0.40	180	0.05	0.22
Dhaka	388	0.45	0.50	180	0.22	0.41
Chittagong	388	0.15	0.35	180	0.29	0.46
Sylhet	388	0.01	0.11	180	0.17	0.37
Rajshahi	388	0.13	0.33	180	0.08	0.28
Khulna	388	0.13	0.34	180	0.08	0.28
Barisal	388	0.12	0.32	180	0.07	0.26
Chandpur	388	0.02	0.13	180	0.08	0.28

Table 5: Estimates of Achievement Regression for the Matched Sample Data

	OLS	1st Stage	2SLS
	Test Score	**Class Size**	**Test Score**
Competition Index	0.086	-1.251	0.111
	(3.72)**	(4.10)**	(2.74)**
Edu_attain	0.113	-1.416	0.14
	1.94	(2.03)*	1.97
LnHH_tot	-0.117	-7.863	-0.003
	-0.3	-1.46	-0.01
Class Size	0.002	-	0.023
	0.57	-	0.98
Grade 10 Enrolment	0.002	0.05	0
	(2.11)*	(3.51)**	0.22
Pb_sch	1.339	-14.782	1.633
	(4.30)**	(4.75)**	(4.14)**
Sch_Sp_shift	0.122	-0.107	0.125
	0.54	-0.04	0.54
Sch_boys	0.356	-3.748	0.429
	1.27	-1.29	1.27
Sch_girls	-0.005	-0.338	0.005
	-0.03	-0.16	0.03
P_Csize	-	0.281	-
	-	(4.08)**	-
District Dummy	Yes	Yes	Yes
Observations	388	388	388
R^2	0.28	0.30	0.22

Note: (1) t-statistics are robust to clustering effect within unions and Huber corrected for hetroscedasticiy. (2) * Significant at 5 percent level; ** significant at 1 percent level. (3) Omitted district dummy is for the district of Chandpur. (4) Base category for Pb school dummy is “PrA school”. Base category for Sch_Sp_shift , Sch_boys and Sch_girls is “co-education”.

Table 6: Revised Estimates of Achievement Regression for the Full Sample (with additional control for enrolment)

	OLS Test Score	1st Stage Class Size	2SLS Test Score
Competition Index	0.066	-0.577	0.14
	(5.93)**	(2.50)*	(2.81)**
Class Size	0.002	-	0.132
	0.78	-	1.62
Grade Enrolment	0.002	0.188	-0.025
	1.81	(9.86)**	-1.48
(Grade Enrolment) sq	5.65e-07	.0003	.00004
	0.18	(5.39)**	1.56
Pb_sch	1.409	-13.203	3.137
	(9.37)**	(7.44)**	(2.85)**
Sch_Sp_shift	0.032	-4.8	0.673
	0.25	(2.87)**	1.52
Sch_boys	0.216	-8.186	1.293
	(2.04)*	(7.27)**	1.86
Sch_girls	-0.122	-1.491	0.089
	(2.13)*	-1.67	0.52
P_Csize	-	0.05	-
	-	1.69)***	-
District Dummy	Yes	Yes	Yes
Observations	2165	2165	2165
R^2	0.23	0.37	-

Note: (1) t-statistics are robust to clustering effect within unions and Huber corrected for hetroscedasticiy. (2) * Significant at 5 percent level; ** significant at 1 percent level; *** significant at 9 percent level (3) Omitted district dummy is for the district of Chandpur. (4) The test for exogeneity shows that class size effect can be treated as exogenous i.e. OLS estimates are valid (p value is .2869). (5) Base category for Pb school dummy is “PrA school”. Base category for Sch_Sp_shift , Sch_boys and Sch_girls is “co-education”.

Table 7: Estimates of Achievement Regression for the Monopoly School Sample

	Full Sample			Reduced Sample		
	OLS Test Score	1st Stage Class Size	2SLS Test Score	OLS Test Score	1st Stage Class Size	2SLS Test Score
Class Size	0.002	-	0.013	-0.003	-	-0.008
	0.42	-	0.45	-0.4	-	-0.18
Grade Enrolment	-0.001	0.069	-0.002	0	0.118	0.001
	-1.34	(2.54)*	-0.8	-0.06	(4.51)**	0.1
Pb_sch	1.471	-7.903	1.556	0.746	-21.055	0.64
	(3.49)**	-1.68	(3.17)**	1.21	(4.10)**	0.49
Sch_Sp_shift	-0.011	-11.761	0.125	-0.168	-28.84	-0.316
	-0.04	(2.60)*	-0.28	-0.52	-1.85	-0.23
Sch_boys	0.734	-13.527	0.882	0.508	-17.751	0.425
	1.41	(3.33)**	1.62	0.56	(5.06)**	0.48
Sch_girls	0.574	-7.855	0.676	1.158	-1.074	1.149
	1.45	-1.59	1.66	(2.15)*	-0.49	(2.95)**
P_Csize	-	0.21	-	-	0.17	-
	-	(2.13)*	-	-	(1.8)***	-
District Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Observations	180	180	180	146	146	146
R^2	0.2	0.38	0.17	0.19	0.47	0.18

Note: (1) t-statistics are robust to clustering effect within unions and Huber corrected for hetroscedasticiy. (2) * Significant at 5 percent level; ** significant at 1 percent level; *** significant at 8 percent level (3) Omitted district dummy is for the district of Chandpur. (4) Reduced sample is obtained after dropping 34 schools that are located in Metropolitan area. (5) Base category for Pb school dummy is "PrA school". Base category for Sch_Sp_shift , Sch_boys and Sch_girls is "co-education".

Table 8: Estimates of Achievement Regression for the Sample of PrA schools (including a dummy for Pb_sch presence)

	OLS	1st Stage	2SLS
	Test Score	Class Size	Test Score
Competition Index	0.067	-0.589	0.088
	(7.25)**	(4.53)**	(6.98)**
Class Size	0.002	-	0.037
	1.33	-	(3.08)**
Grade Enrolment	0.001	0.082	-0.002
	(3.94)**	(15.07)**	-1.72
Union_pb	0.075	-2.814	0.175
	0.98	(2.59)**	1.89
Sch_Sp_shift	0.071	-4.991	0.264
	0.77	(3.82)**	(2.16)*
Sch_boys	0.137	-7.519	0.405
	1.48	(5.73)**	(2.92)**
Sch_girls	-0.104	-1.655	-0.022
	-1.94	(2.16)*	-0.33
P_Csize		0.171	-
		(6.50)**	-
District Dummy	Yes	Yes	Yes
Observations	2094	2094	2094
R^2	0.15	0.34	-

Note: (1) t-statistics are robust to clustering effect within unions and Huber corrected for hetroscedasticiy. (2) * significant at 5 percent level; ** significant at 1 percent level; *** significant at 8 percent level (3) Omitted district dummy is for the district of Chandpur. (4) Union_pb indicates presence of a Pb school in the same union where the PrA school is located. (5) Base category for Pb school dummy is PrA school dummy. Base category for Sch_Sp_shift , Sch_boys and Sch_girls is “co-education”.

Table 9: Estimates of Achievement Regression with Control for Teacher Pay

	Full Sample			Restricted Sample		
	OLS	**1st Stage**	**2SLS**	**OLS**	**1st Stage**	**2SLS**
	Test score	**Class Size**	**Test score**	**Test score**	**Class Size**	**Test score**
Competition Index	0.064	-0.677	0.08	0.07	-0.705	0.083
	(5.75)**	(2.58)**	(5.54)**	(6.21)**	(2.53)*	(5.42)**
Class Size	.0004	-	0.023	0.001	-	0.019
	0.24	-	1.7	0.46	-	1.29
Teacher Salary	0.384	3.297	0.292	0.395	3.714	0.313
	(4.94)**	(3.46)**	(3.18)**	(5.03)**	(3.84)**	(3.17)**
No data on Teacher Salary	3.048	27.7	2.29	-	-	-
	(4.68)**	(3.57)**	(3.01)**	-	-	-
Grade Enrolment	0.001	0.073	-0.001	0.001	0.074	-0.001
	(2.48)*	(10.01)**	-0.75	(2.03)*	(9.92)**	-0.54
Pb_sch	1.387	-12.953	1.674	1.629	-14.576	1.892
	(9.70)**	(7.20)**	(7.66)**	(10.04)**	(7.57)**	(7.15)**
Sch_Sp_shift	0.027	-4.794	0.143	0.069	-6.018	0.184
	0.22	(2.78)**	0.97	0.51	(3.75)**	1.1
Sch_boys	0.152	-8.154	0.339	0.071	-7.921	0.216
	1.45	(7.03)**	(2.05)*	0.64	(6.39)**	1.25
Sch_girls	-0.149	-2.059	-0.089	-0.141	-1.986	-0.095
	(2.58)**	(2.30)*	-1.17	(2.41)*	(2.09)*	-1.25
P_Csize	-	0.16	-	-	0.15	-
	-	(5.63)**	-	-	(5.06)**	-
District Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2165	2165	2165	2047	2047	2047
R^2	0.25	0.35	0.17	0.25	0.35	0.2

Note: (1) t-statistics are robust to clustering effect within unions and Huber corrected for hetroscedasticiy. (2) * significant at 5 percent level; ** significant at 1 percent level; *** significant at 8 percent level (3) Omitted district dummy is for the district of Chandpur. (4) Base category for Pb school dummy is PrA school dummy. Base category for Sch_Sp_shift , Sch_boys and Sch_girls is "co-education". (5) The variable "No data on Teacher Salary" is a dummy taking '1' if data on teacher salary is missing and '0' otherwise.