Has the "Ambition Success Networks" Educational Program Achieved its Ambition? *

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Abstract

This paper evaluates the French RAR program (Réseaux Ambition Réussite or Ambition Success Networks), a junior high school program started in 2006 which intended to concentrate means and funds on well-chosen disadvantaged junior high schools. We use the criteria of eligibility to estimate a regression discontinuity model.

For evaluated junior high schools, the increase in per-pupil teaching hours, and the decrease in class size are disappointing. Our results also suggest that the program may have had a negative effect on teacher and pupil enrollment. Both the proportion of older teachers and the proportion of poorly qualified teachers have increased and the pupils' achievement has decreased in Grade 9, the final year of junior high school. The RAR program has increased the disparity of teachers' characteristics and of pupils' ability between schools.

Keywords: educational policy, school-oriented compensation policy, regression discontinuity.

JEL Classification : I28, I24, C21.

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Education pursues two goals: offering equal opportunities to all, and promoting success for each student. In France, however, as in other countries, large achievement inequality has persisted since the 60's, mainly due to heterogenous parental backgrounds.(see for instance Coleman et al. (1966) for the USA and Plowden & the Central Advisory Council for Education (1967) for the UK). As a result many countries have set up compensatory education programs to foster equality between pupils (van der Klaauw (2008), Heckman & al. (2010)).

In France the main compensatory policy - the Politique d'Education Prioritaire, or Preferential Educational Policy - has targeted schools rather than students since its introduction in 1982.[1] This school-based program, *Educational Priority Zones* (ZEP), was overhauled twice and eventually replaced in 2006 by the *Réseaux Ambition Réussite* (RAR) policy, or *Ambition Success Network* program, the focus of this paper.

The ZEP policy came under criticism for spreading itself too thinly: too many schools were given too few funds. As a result, many evaluations of the ZEP policy conclude that this costly program had no positive effect on student achievement (see Benabou et al. (2009), and Meuret (1994)) but a negative signaling effect instead. Hence the RAR program was intended to concentrate its funds on fewer schools: 249 junior high schools in 2006-2007 against 900 for the ZEP policy.[2]

In this paper we provide a first evaluation of the recently implemented RAR program.

Any evaluation of school-targeting policies must deal with several methodological issues. First, treated schools are not randomly selected: they differ greatly from non-selected schools. We overcome this issue by using a regression discontinuity design that allows us to find valid counterfactual schools for both groups of treated schools. Second, it is not without interest to describe some aspects of the actual policy. Each school was supposed to get supplementary teachers. However, the real and announced policies may differ: we therefore need to study the number of teachers in treated and untreated schools. Third, the RAR program may have had negative signaling effects on both teachers and students. Labeling a junior high school as a RAR school may induce poor-quality teacher assignment

[1] Though pupil- or class-oriented policies are more widespread in developed countries, a few noticeable educational policies target schools. In the United States, Title I of the Elementary and Secondary Education Act funded schools and school districts with a high percentage of disadvantaged students in 1965. In the United Kingdom the *Education Priority Areas* (EPA) were launched in 1967, disappeared at the end of the 70's but following the victory of New Labour in the 90's were reborn in two new programs: *Education Action Zones* and *Excellence in the Cities*. They both targeted schools (see Machin & Vignoles (2005)).

[2] A network was made up of a junior high-school (from 6th to 9th grade) and a few primary and/or nursery schools (from 1st to 5th grade, or before 1st grade).

and the departure of the best students. Therefore we study the program effect on both pupil-school and teacher-school matchings. Finally, we also compare final exam results between treated and non-treated schools. This mixes two effects: the change in teaching team efficiency, and the change in sorting on unobserved characteristics of pupils across schools.

We find negligible or adverse effects of the policy on the two groups of treated schools for which we are able to find credible counterfactuals: the first is centered on the disadvantaged student proportion threshold, the second on the repeating student rate threshold. For the first group of treated schools, the policy has had no effect on the teacher per pupil ratio, nor on teacher and pupil characteristics, but it has had a negative effect on pupil achievement. For the second group, we find an increased number of teachers in 6th and 7th grades (but not for other grades). This effect comes from an increase in the proportion of older teachers, and teachers with an unusual degree. Pupil structure has also been modified in these schools: we find an increase in the proportion of children from blue-collar backgrounds, counteracted by a decrease in the proportion of children with self-employed parents. Finally, for both groups of treated schools, final junior high-school scores have worsened in RAR assigned schools.

The rest of this paper is organized as follows. Section 2 describes the RAR program. Section 3 develops the identification and estimation strategies. Section 4 presents the results and Section 5 concludes.

1 RAR program: design and background

1.1 A brief history

In 1982 a new program - *Educational Priority Zones* (ZEP) - was conducted in France. This policy's main objective was to increase efforts in unsuccessful zones to reduce inequalities. These new education priority zones aimed to set up an educational project that would provide support to underachieving students. In 1985, its main focus was redirected to address deficiencies in 'core learning' such as reading and French. Afterwards successive reforms were introduced, each one expanding the number of schools concerned, but without increasing further inputs in already treated schools.

Benabou et al. (2004) draw three main conclusions concerning these Education Priority Zones. First they argue that the subsidies were divided between too many schools and mainly given to teachers via supplementary wages without any overtime teaching. Hence

the actual per capital allocation of funds to pupils was scarce. Secondly, the authors find that the treated junior high schools experienced a decrease in their total number of students and an increase in the proportion of socially disadvantaged pupils. Teachers also migrated from these schools. Their turnover increased after the assignment of these schools to priority zones. Finally, Benabou et al. (2004) found no significant effect on different measures of student achievement, or on high-school graduation. These disappointing results suggest a restructuring of the ZEP program that better targets efforts and funds.

1.2 The Ambition Success Network policy

As a consequence, the reform introduced in 2006 pursued the goal of better targeting funds and efforts. The education priority map was reshaped, and the resources were given to a smaller number of schools (249 junior high schools in 2006-2007 against around 900 with the previous policy). New zones were defined and named "Réseaux Ambition Réussite" (Ambition Success Networks). These networks are made up of one junior high-school (6th to 9th grade) and some primary and/or nursery schools (1st to 5th grade, or before 1st grade). These entities share a common project under the guidance of a committee composed of the heads of the junior high-schools and some representatives of the primary or nursery schools. At the beginning of the 2006-2007 school year, 249 networks were created, consisting of 249 junior high-schools and 1,715 elementary schools. It represented 126,000 pupils in junior high schools: one junior high school student in twenty was enrolled in a RAR targeted school.

The Ministry of Education recommended to its regional heads ("recteurs") that each network get three to four supplementary teachers, some teaching assistants and at least one full-time nurse. On a national level, this amounted to 1,000 supplementary teachers, and 3,000 teaching assistants. However considerable discretion was left to regional heads. Although the list of "RAR" junior high-schools was decided in a concerted way, the list of primary and nursery schools, and the resources devoted to each network were chosen by the regional heads. Moreover the type of educational services provided was left to the discretion of the junior high schools and regional heads, with no requirement for accountability.

The number of RAR networks has evolved since their introduction. From 249 in 2006-2007, there were 254 public junior high-schools at the beginning of the 2008-2009 school year and 118,000 junior students. There are large regional discrepancies: the proportion of "RAR" junior high-schools ranges from 0.4% for the regional area of Grenoble to 13% for the regional area of Aix-Marseille. There are also a few private "RAR" junior high schools

(11 in 2008-2009). Networks or zones that were not targeted by the new policy in 2006 constitute a different category of networks called "networks of school success" ("Réseaux de Réussite Scolaire", RRS). In 2007-2008, of the 253 RAR junior high schools, 238 had previously been "ZEP". The previous denomination "ZEP" (theoretically) disappeared in September 2008.

1.3 How were the RAR schools selected?

Internal notes of the Statistical Service of the Education Ministry provide information about the selection process of secondary schools. RAR networks were chosen on the basis of three main criteria evaluated for the 2004-2005 school year. First, the proportion of socially disadvantaged[3] students in 6th grade had to be equal to or above 67%. Second, either of the two following criteria had to be met: the proportion of students who have repeated two grades or more when they entered 6th grade had to be at or above 10%; or the school average score at the entrance evaluation of 6th grade had to be at or below 47%. Additional criteria that were used to define the final list of RAR junior high schools included the local unemployment rate and the proportion of people benefiting from social assistance. Instead of the 164 initially chosen with the three criteria previously mentioned, 249 were finally selected after an agreement between the French Ministry of Education and its regional heads.

1.4 Expected results

If we assume that 1,000 supplementary teachers were uniformly assigned across new RAR junior high schools, then we would expect an 8% increase in the total number of teachers in RAR schools versus non-RAR schools. As mentioned before however, regional head masters have considerable discretion. For instance, even if the Ministry of Education gives them additional teachers that must be specifically assigned within the RAR junior-high-schools, the regional head officials can assign them as they see fit. In particular, teachers already working in a RAR school may be transferred to a non-RAR school to ensure the stability of the total number of teachers both in RAR and non-RAR schools. This eviction phenomenon is credible if regional head officials are reluctant to treat similar

[3]A child was classified as 'disadvantaged' when their referring parent was either a blue collar worker, or retired from a blue collar or white collar occupation, or out of work. This criterion was calculated from the occupation covariate coded on two digits in the data files.

schools differently: e.g. two schools just below and above the selection thresholds were alike before program assignment.

Some parents may have interpreted the RAR assignment as a negative signal, as a consequence the number of pupils in treated schools may have decreased. Benabou et al. (2004) and Benabou et al. (2009) have discussed such a decrease for ZEP-schools during the 90's. In that case, the teacher-pupil ratio may increase more than expected.

If the teacher-pupil ratio increases enough, the average class size may decrease. The effect of class-size on pupil achievement, using the seminal method of Angrist & Lavy (1999), is often reported as negative (see Angrist & Lavy (1999), Leuwen et al. (2008), and for the French case, Gary-Bobo & Mahjoub (2006) and Piketty & Valdenaire (2006)). Another group of estimates comes from the Tennesse STAR class size experiment. Word et al., (1990), Finn & Achilles (1990), Krueger (1999), and Krueger & Whitmore (2001) all found that smaller class sizes have a significant and lasting impact on academic achievement and educational attainment.

Another issue relates to the teacher quality in RAR schools. First, supplementary teachers can have some specific characteristics. Previous studies on ZEP policies show that the new teachers in treated schools are often younger and have less experience. It can be argued that the assignment of schools to the RAR-program acts as a negative signal for the teachers as well, who would then prefer to be migrate to non-treated schools. However, teacher mobility and tenure are closely related: the less experience they have, the lower their bargaining power to move. Moreover in the previous ZEP program, teachers willing to work within ZEP schools were promoted quicker to encourage them to teach in such schools. However bigger promotions ease the transfer to another school if requested. And it was noticed indeed that the turnover of the teaching teams was higher in ZEP schools, which can be harmful to school management. Ly (2010) observed that in 1999, the ZEP assignment change induced the mobility of oldest teachers from ZEP to non-ZEP schools.

Finally the policy can modify the sorting of pupils across schools. Benabou et al. (2004) and Benabou et al. (2009) found that school discrepancy between treated and untreated schools increases after assignment: they measure school discrepancy through the proportion of pupils not enrolled in the school cafeteria. Other studies show that parents take into account school choice in their decision to move or relocate (Fack & Grenet (2010) for the case of Paris). More generally, sociologists have reported the existence of parental strategies concerning the schooling of their children (François & Poupeau (2004)) and economists have tried to quantify the valuation of schools (Black (1999), Fack & Grenet (2010), Gibbons & Machin (2003)). All these parental strategies are based on available

information about schools and assignment to the RAR program can affect parental choice. Even if one can not *a priori* exclude the possibility that parents give a higher valuation to RAR-schools because such schools get higher resources, empirical results in the literature suggest that school-based discrimination increases segregation. Should there be such an increase in segregation the effect on average test scores in the final exam is muddled: the policy may increase the efficiency of treated schools, but such an increase in efficiency may be compensated for by a sorting effect. Our data only allow us to compare average results across schools. This is a clear limit of this paper and the reader must bear in mind that the estimates on the final score exam mix the two effects.

2 Data and some descriptive statistics

To analyze the effectiveness of the RAR program, we use school-level data collated from various administrative sources of the Ministry of Education:

- The first dataset is an exhaustive pupil-level cross-sectional dataset (*Scolarité*) for every student in junior or secondary high school. This data provides cross-sectional information about age, nationality, residence location, main parent's occupation, class, languages and other options, school lunch status, and the same variables for the previous year.

- The second dataset is an exhaustive teacher-level panel dataset (*Relais*) for every teacher in junior or secondary high school. This provides information on the total number of hours taught by each teacher, for each subject they teach, in each school, along with their age and degree.

- We supplement the pupil-level data with a third, exhaustive, pupil-level dataset that contains their national exam scores, (*Brevet des Colleges*), taken at the end of the 9th grade. We are unable to fully merge the two pupil-level datasets due to the absence of a unique student identification number that could combine them both. We are however able to provide a distribution of test scores for every school and for every year by combining these two datasets.

- The fourth dataset is an exhaustive panel dataset of French junior and secondary high schools that includes information on their Educational Priority policy status and their RAR eligibility criteria.

We construct a school-level dataset for school years 2003/2004 up to 2008/2009. We restrict our attention to public junior high schools in mainland France. This is because private schools are almost never assigned to treatment, and because in French overseas departments and territories, junior-high schools are almost always assigned to treatment. Moreover private schools, which represent around 20% of pupils, differ substantially from public schools on various dimensions. This is also true for overseas schools. We thus obtained an exhaustive panel of around 5000 public junior high schools in metropolitan France. Among these schools, 206 were affected to the RAR program.

We exclude educationally disadvantaged students who have severe and long-running problems with core learning as they belong to special classes called "Segpa" (*section d'enseignement général et professionnel adapté*). Special funds are dedicated to them, but they do not interfere with the resources allocated to the educational priority programs. Similarly, when we evaluate the average number of pupils per class, or the social structure of pupils moving up into 6th grade, we exclude these students.[4]

Finally, for final exam test scores, we focus our attention on formal written literacy and maths tests. Every year from 2003 to 2009, final exams took place in June for pupils in 9th grade. For each school, we calculate average and quantiles of test score distribution. For these computations, we exclude disabled pupils[5] and pupils following vocational curricula.

Table 1 presents pre-treatment descriptive statistics for the whole sample of public junior high schools in mainland France. RAR public junior high schools have a higher proportion of pupils entering 6th grade after repeating a grade. A junior entering 6th grade is usually 11 years old. Four pupils out of 10 in RAR schools had repeated at least one grade when entering their 6th grade in September 2005, only two out of 10 were in this case in non-RAR schools. This discrepancy is roughly the same after 2006, even though the proportion of students having repeated a grade has fallen since 2003 due to a no repeating grade policy in the French educational system. In the 6th and 9th grades, socially disadvantaged pupils are overrepresented in RAR schools. Only 2.1% of juniors entering the 6th grade in RAR schools belong to a family whose head[6] is an executive. This proportion is to 15.9% for non-

[4]Schools have indeed specific resources for SEGPA classes and we're interested in the extra resources allocated to schools by the RAR program that are not allocated to SEGPA students. SEGPA pupils are within junior high schools but in different teaching structures, in separate classes, with different teachers. Moreover the proportion of SEGPA pupils has the same distribution below and above both considered thresholds. Hence excluding SEGPA students from teaching, score, and resource indicators does not lead to biased estimates.

[5]The way they take the national exam is different from the non-disabled.

[6]In the data, only the occupation of one of the two parents is available. This parent, who is usually the father, is called the family head.

RAR schools. Unlike 'executive-children', 'unemployed-children' and 'blue-collar-children' are over-represented in RAR schools. In grade 6 in 2006/2007, over 27% of children in RAR schools have an unemployed parent compared to non-RAR schools where this figure is approximatively 9%.

Table 1 also presents proxies of average expenditure per pupil, namely average class size and average weekly hours of teaching per pupil. One year before treatment, average class size was around 21 in RAR schools and 24 in non-RAR schools. Similarly, average per-pupil weekly teaching hours was about 12% higher in RAR schools (1.41 versus 1.24 in non-RAR schools). In non reported results we find that this figure holds for all grades: many RAR schools previously benefited from the ZEP program, hence from a higher teacher-pupil ratio.

Teachers are less qualified and are younger in RAR junior high schools. The proportion of physical education teachers was larger in RAR schools.

Table 1 provides a comparison between RAR and non-RAR schools in maths and literacy scores as obtained in the final national exam. Each student at the end of his/her 9th grade takes written tests in maths and French. Scores range between 0 and 40. For each junior high school, we computed average test scores in maths and French. The comparison of the average scores highlights the huge discrepancy between RAR and non-RAR schools prior to treatment. For the exam taken in 2006, the average test score in French for RAR schools was 14.32 against 19.01 for non-RAR schools. The same holds in maths. Comparison of the quantiles (not reported here) shows that this result holds for the entire distribution of test scores.

The descriptive statistics show that a naive comparison between the treated and non-treated schools is unsatisfactory to test efficiency of the RAR program. Conditioning on some important observable covariates may mitigate bias but even in this case, the interpretation is likely to be misleading: assignment to treatment may have been driven by some unobserved variables. To overcome this issue, one difference-in-difference strategy may be adopted using the panel dimension, but this would be valid only if temporal trends are the same between treated and non-treated schools. Simple panel-data regressions reported in Table 2 show that temporal trends between 2003 and 2005 differ significantly (at the 5% level) for many variables.[7] At baseline, we find that the gap in the proportion of 13-year-olds enrolling into grade 6 falls. This however is not true for children whose parents are executives or blue-collar workers. The gap in average class size between RAR

[7] To be more precise, trends of treated and untreated schools differ significantly for at least one modality of every categorical variable considered: parental profession, teacher characteristics and age of pupils at beginning of 6th grade.

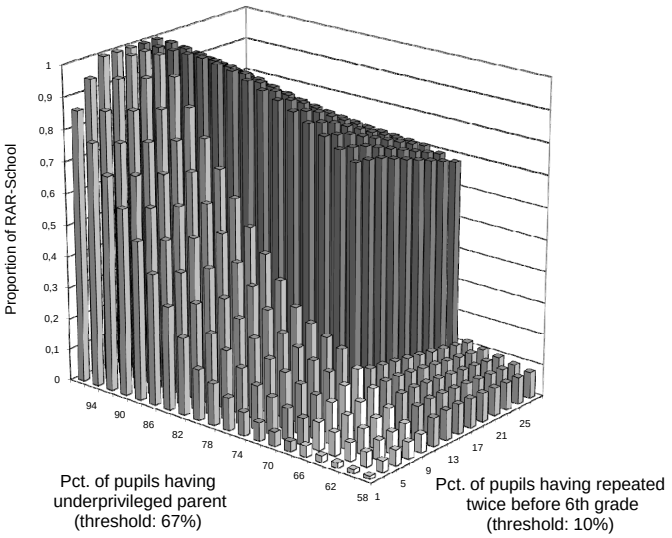
and non-RAR schools also increases, and structures of teacher qualifications and age also evolve differently in the treated and non-treated schools before the beginning of the policy. These different evolutions rule out the use of a difference-in-difference estimation strategy.

3 Identification and estimation strategy

3.1 A fuzzy regression discontinuity design

As explained in the previous section, the assignment to RAR treatment is based on thresholds of some predetermined variables. These key features of the RAR-policy allow us to use a fuzzy regression discontinuity design to estimate the causal effects of the RAR program on different outcomes such as means, but also signaling and sorting. Such a discontinuity is clearly supported by Figure 1.

Figure 1: RAR recipiency rate in 2006-2007



This discontinuity in the probability of being treated implies that local average treatment effects ($LATE$) are nonparametrically identified (Hahn et al. (2001)). The basic idea is

Table 1: School Characteristics in September 2005, for treated and non-treated schools

School Characteristics	non-RAR	RAR	t-stat
ZEP before 2006	0.13	0.99	36.36
Number of pupils	461.46	428.07	-2.63
Pupils entering sixth grade			
% of male	50.73	50.87	0.35
% 10 years old or younger	2.95	1.48	-9.53
% 11 years old	76.85	58.30	-30.62
% 12 years old	19.15	36.72	29.71
% 13 years old or older	1.05	3.50	19.93
Farmer	2.95	0.17	-7.62
Self-employed	8.66	3.78	-13.57
Executive	15.88	2.08	-15.46
Intermediate	14.68	5.44	-20.49
Employee	15.77	11.21	-9.01
Blue collar	29.57	41.26	12.05
Retired	1.22	3.63	19.68
Unemployed	8.60	27.31	36.89
Pupil supervision			
Hours of teaching per pupil	1.24	1.41	17.75
Class size	24.00	20.96	-20.16
Teaching Staff (% of hours dispensed by)			
Highest teaching degree	0.04	0.04	-2.74
Qualified teacher	0.72	0.72	-0.56
PE teacher	0.10	0.11	2.21
Other teacher	0.13	0.14	1.17
Teacher under age 30	0.15	0.27	14.73
Teacher between 30 and 40	0.31	0.37	7.27
Teacher between 40 and 55	0.35	0.25	-11.87
Teacher over 55	0.44	0.27	-16.59
Average results at the Brevet exam (Grade 9, June 2006)			
French Score	19.01	14.32	-24.47
Maths Score	18.32	11.85	-18.93
Number of schools	4,795	205	

Table 2: Temporal trend of school characteristics before September 2005, for treated and non-treated schools

School Characteristics	non-RAR	RAR	t-stat
Number of pupils	-10.00	-11.80	-1.56
Pupils entering sixth grade			
% of male	-0.06	0.16	0.80
% 10 years old or younger	0.16	0.08	-0.84
% 11 years old	0.41	0.74	1.42
% 12 years old	-0.48	-0.39	0.45
% 13 years old or older	-0.09	-0.44	-5.33
Farmer	-0.07	-0.04	0.30
Self-employed	0.24	0.15	-0.55
Executive	0.31	-0.17	-2.55
Intermediate	-0.14	-0.06	0.42
Employee	-0.02	0.33	1.59
Blue collar	-0.39	-1.00	-2.16
Retired	0.04	0.00	-0.59
Unemployed	0.06	0.91	4.71
Pupil supervision			
Hours of teaching per pupil	0.00	0.00	0.10
Class size	0.02	-0.09	-2.30
Teaching Staff (% of hours dispensed by)			
Highest teaching degree	0.00	0.00	-2.23
Qualified teacher	0.01	0.02	2.78
PE teacher	0.00	0.00	-0.08
Other teacher	-0.01	-0.02	-1.78
Teacher under age 30	-0.01	-0.01	-0.60
Teacher between 30 and 40	0.01	0.01	2.31
Teacher between 40 and 55	-0.02	-0.01	2.26
Teacher over 55	0.00	0.00	-1.17
Average results at the Brevet exam (Grade 9, before June 2005)			
French Score	-0.45	-0.59	-1.85
Maths Score	-0.46	-0.64	-1.53
Number of schools	4,795	205	

to compare the outcomes of junior high schools just above and just below the thresholds used to assign treatment. Multiple assignment criteria allow us to identify several parameters. We can identify and estimate nonparametrically the LATE for junior high schools having around 67% of socially disadvantaged pupils and having more than 10% of pupils having repeated a grade twice before Grade 6. We can also identify the LATE for junior high schools having around 10% of repeating pupils and with more than 67% of socially disadvantaged pupils.

To describe the identification approach used in this paper, let T_i be an indicator equal to 1 if school i is treated. $Y_i(0)$ denotes the potential outcome if the junior high school i was non-treated ($T_i = 0$), and $Y_i(1)$ if it was treated ($T_i = 1$). Then the actual and observed outcome is $Y_i = Y_i(0) + T_i(Y_i(1) - Y_i(0))$. Let Z_i^F be the percentage of pupils coming from a disadvantaged family and Z_i^L be the percentage of pupils in 6th grade having repeated a grade at least twice. Discontinuities of the conditional regression function of the treatment T_i on the running variables (Z_i^F, Z_i^L) imply the existence of some complying schools for each running variable. This means that some schools are not treated when the values of the running variables are under thresholds, but are treated otherwise. For these complying schools, if Z_i^F and Z_i^L cannot be manipulated, the rules of assignation to the treatment generate an "as-good-as random assignment". We denote by the dummy C_L (respectively C_F) the dummies of being a complying school for the threshold $Z^L = 10\%$ (respectively for the threshold $Z^F = 67\%$). Assuming continuity of conditional regression functions of potential outcomes on running variables, and monotonicity of treatment in a neighborhood of discontinuities (see Imbens & Lemieux (2008)), we can identify the following local average treatment effects:

$$LATE_{F67} = \mathbb{E}\big(Y(1) - Y(0) | Z^F = 67\%, Z^L \geq 10\%, C_F = 1\big)$$

$$LATE_{L10} = \mathbb{E}\big(Y(1) - Y(0) | Z^L = 10\%, 80\% \geq Z^F \geq 67\%, C_L = 1\big)$$

We impose the restriction $80\% \geq Z^F$ on the second parameter because discontinuity vanishes when $Z_L = 10\%$ and Z_F close to 100% (see Figure 1).

3.2 Can the threshold be manipulated ?

To get consistent estimates, the conditional regression functions of potential outcomes on the running variable (see Imbens & Lemieux (2008)) have to be continuous. This assumption may be violated if some agents, for instance headmasters or regional heads, are able to manipulate the running variables Z^L or Z^F. This is highly unlikely for the

following reasons.

First, the statistical service of the Education Ministry collects family information about each pupil in junior high schools. The running variables Z^L and Z^F are calculated by aggregating this information at the school level and are not available within junior high schools.

Second, the statistical service has had considerable autonomy in choosing the eligibility criteria and the threshold values. It is unlikely that headmasters had access to or were aware of the various selection criteria chosen by the ministry. Moreover, the Education Ministry had ordered the release of the RAR list in 2006 based on information collected in September 2004. Therefore, in 2004, headmasters, collecting individual information about pupils, could not have anticipated the introduction of a policy two years beforehand, especially as the RAR program was the first one to be threshold-based. If by any chance they had heard of a future policy, the eligibility thresholds were not known in September 2004. As a consequence, we can be confident that manipulating parents' occupation, or entrance results to be assigned to RAR treatment was not feasible.

Finally, following the approaches of Saez (2010) and McCrary (2008), we studied the densities of Z^L and Z^F near the thresholds. Indeed, a monotonic manipulation of the running variable density-for instance if some schools just under the threshold manipulate the data and report being just over the threshold- must display a local minimum and a local maximum on either side of the threshold. Without any manipulation, there is no reason to see any local extremum around the threshold.[8] Figures 2 and 3 provide strong evidence that potential manipulations of running variables are not troublesome in our case.

3.3 Advantages and drawbacks of the identification strategy

Some former evaluations of the ZEP program relied on the assumption that the treatment was exogenously assigned given a set of observable covariates. Some articles, such as Benabou et al. (2009), stand out because the authors use a difference-in-difference approach. Unless the treated and non-treated schools's different outcomes share common trends, this strategy is not valid. This assumption is indeed questionable: treated schools can be located within deprived areas. In France, the increasing geographical segregation during last the two decades has been demonstrated by many researchers (see, for instance, Maurin (2004)). We test the assumption of common trends before the RAR policy and,

[8]The absence of local extremum can also occur if upward manipulations compensate for downward manipulations. However in our case, such a coincidence seems very unlikely.

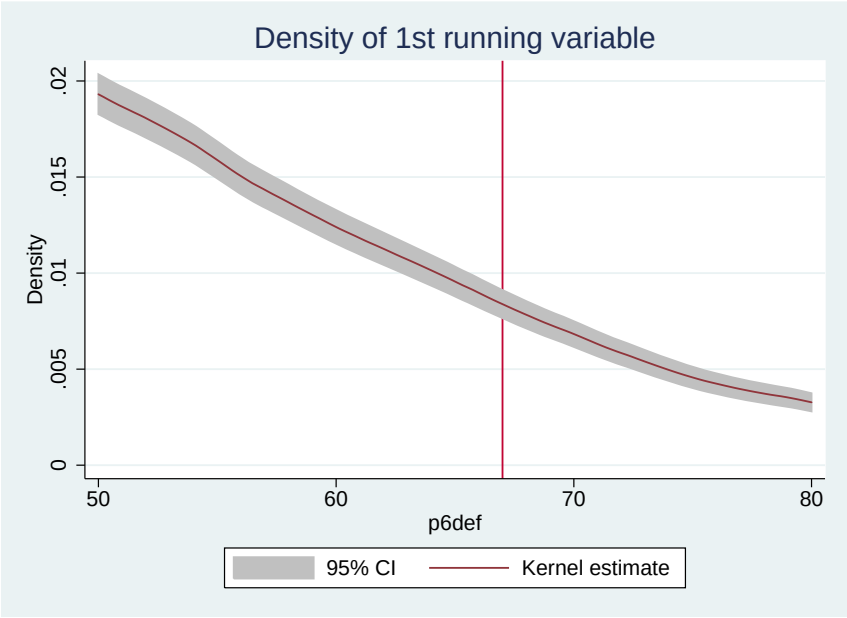


Figure 2: Density of the running variable Z^F around the threshold (67%)

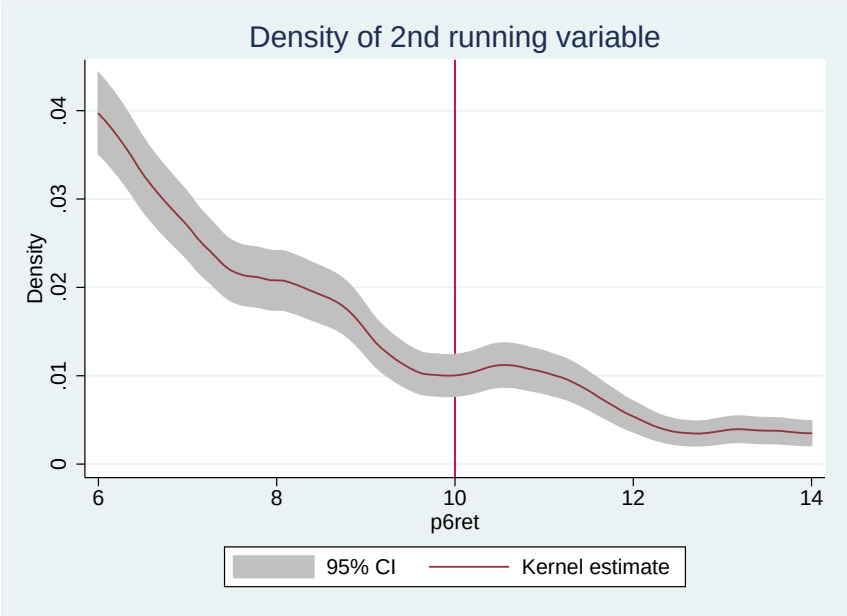


Figure 3: Density of the running variable Z^L around the threshold (10%)

as mentioned in the previous section, Table 2 clearly rejects the assumption of common trends between treated and non-treated schools. Hence, as noticed by Gurgand in Benabou et al. (2004), though the difference-in-difference estimation may reduce bias, it cannot be measured.

Unlike the difference-in-difference approach, the regression-discontinuity method does not

rely on such an assumption of common trend. Nonetheless the regression discontinuity estimates cannot be extrapolated to the whole population of treated schools, and they may be not precise enough. This is a concern when the effects we want to measure have a low magnitude, or when few observations are located near the threshold. To compensate for the lack of precision, we can use observations further away from the threshold, but at the cost of an increased bias.[9] Our strength is the panel data at hand. We thus add school fixed effects and time dummies, and they turn out to explain more than 75% of the variance of the outcomes we studied. As a result, we are able to statistically measure small effects. However, we need to check whether complying schools around the thresholds display common trends in the absence of treatment to ensure our approach is valid. We tested this assumption using observations before the RAR program was set up, from 2004 to 2006: no trend difference protrudes around both thresholds before September 2006.[10]
We estimate the LATE by a two stage panel least squares (TSLS) regression after selecting our data around both thresholds. This is equivalent to using a uniform kernel for the local linear regression, as suggested by Hahn et al. (2001). For observations such that $Z^L \in [10-h, 10+h]$ and $80\% \geq Z^F \geq 67\%$ and with $c = 10\%$, and for observations such that $Z^F \in [67-h, 67+h]$ and $Z^L \geq 10\%$ and with $c = 67\%$, we compute the following TSLS estimates:

$$Y_{it} = \alpha_i + \beta_t + \gamma T_{it} 1\{t \geq 2006\} + \delta' V_{it} + \varepsilon_{it} \tag{3.1}$$

where $T_i.1\{t \geq 2006\}$ is the endogeneous covariate, $V_{it} = \begin{pmatrix} 1\{Z_i < c\}1\{t \geq 2006\}(Z_i - c) \\ 1\{Z_i \geq c\}1\{t \geq 2006\}(Z_i - c) \end{pmatrix}$ are the exogenous covariates and $1\{Z_i \geq c\}1\{t \geq 2006\}$ is the instrument.
The regressors V_{it} are introduced to avoid asymptotic bias in the estimates (Hahn et al. (2001), Imbens & Lemieux (2008)). Standard tests remain asymptotically valid when the regressors V_{it} are added to regressions.
It is important to notice that we have not induced any estimation bias by selecting our sub samples having checked that the repeating rate distribution was continuous around 67% disadvantaged threshold, (resp. the disadvantaged student proportion around 10% repeating threshold). The estimates we obtained are only local average treatment effects and we cannot answer for the efficiency of the global policy since we would have to impose unrealistic parametric assumption to identify the ATE on the whole public school set.

[9] Adding covariates in the regressions decreases the variance of the estimates by the proportion of the explained variance of the outcomes by covariates.

[10] Common trend tests around both thresholds are available upon request.

3.4 The outcomes

We provide an assessment of RAR treatment along different dimensions:

1. First, we consider outcomes that proxy for expenditures, to estimate the intensity of positive discrimination: average class size and weekly per-pupil teaching hours at the school level and for different grades (from Grade 6 to Grade 9).

2. A second set of outcomes considered are observable characteristics of pupils at the beginning of Grade 6. Hence, we examine pupil sorting across treated and non-treated schools. These are: family head occupation[11], dummy for attending cafeteria at lunch and the total number of pupils.

3. A third set of outcomes examines changes to the teaching structure. We focus on teacher-related outcomes such as the percentage of teaching hours per qualification, and the teacher age structure. We distinguish between four types of qualification: physical education (PE) teachers, post-graduate teachers ("agrégation", the top competitive examination), junior high school teachers ("certified" teachers recruited by a more open competitive examination) and finally, other teachers that have not been recruited by the usual competitive examinations (a priori less qualified). We also distinguish teachers by age, as it proxies tenure: less than 30, between 30 and 39, between 40 and 54, 55 and over.

4. The last set of outcomes is related to the academic achievement of pupils: we study the means and quantiles (Q10-Q25-Q50-Q75-Q90) of French and maths score distributions, at the final exam in Grade 9.

Studying these outcomes will provide us with an answer about any potential negative signaling effects of RAR treatment.
For all these outcomes, we test sensitivity to bandwidth choice h. We select the observations around both thresholds. For the percentage of disadvantaged pupils entering 6th grade, results are reported for $h = 4$, 6 and 8. For percentage of grade repeaters, the results are reported for $h = 2$, 3 and 4. Our results are robust to the choice of some alternative bandwidth values.

[11]We distinguish 9 main occupations: farmers, self-employed occupations (artisans, shopkeepers, company managers), executives, intermediate occupations, administrative, sales or service occupation, blue collar, retired parents, out of the labor force

4 Results

4.1 Per pupil expenditure

We begin by reporting per pupil expenditure across RAR and non-RAR schools. If 1,000 supplementary teachers had been uniformly assigned across new RAR junior high schools, we would expect an 8% increase in the total number of teachers in RAR schools versus non-RAR schools. Tables 3a and 3b test this claim and present two types of indicators for different grades: the number of pupils per class and the number of teaching hours per week divided by the number of pupils (denoted resp. P/C and H/P, where P stands for *pupil*, H for *teaching hour*, and C for *class*).

We actually see that the 1,000 supplementary teachers had not been uniformly distributed across RAR junior high schools: the recommendation that each RAR junior high school receive 4 supplementary teachers was not strictly followed by each regional head. It can be inferred that teacher assignment was conducted on an ad-hoc basis, driven partly by regional heads and teachers' preferences. Furthermore regional heads and headmasters of junior high schools have discretion on how to allocate additional teachers to different grades, which is why it is important to study the effect of RAR assignment on school resources, since we do not clearly know whether the policy has been followed by regional heads and school headmasters.

Average class size and per-pupil teaching hour estimations do not match political commitments. Specifically, results differ only very slightly across the two discontinuities, implying that treatment has only a weak effect on resources if any. For schools where $Z^F = 67\%$ and $Z^L \geq 10\%$ in 2004, we notice a small effect of the policy on class size reduction but the magnitude of this effect drastically decreases when estimation is made using a larger bandwidth (see Table 3a). The robustness of this measured effect is therefore questionable. A more robust and significant result concerns per-pupil teaching hours for Grades 6 and 7. The magnitude of this effect is more stable across bandwidth changes (see Table 3a). For schools with $Z^L = 10\%$ and $67\% \leq Z^F \leq 80\%$ in 2004, we find no significant effect on per-pupil teaching hours or on class size (see Table 3b). Per-pupil teaching hours decrease less than expected for all grades.[12] This may be explained by the preference of headmasters for concentrating compensatory policy on the most disadvantaged schools, that are schools

[12]For the number of hours per pupil, one can expected an increase of 1.4*8%=0.11. The corresponding estimates only range between -0.09 and 0.09 (with strong variations depending on the grade). For the number of pupils per class, estimates range between -1.38 and 1.09 for an expected value close to -20*8%=-1.6.

far from both thresholds.

To sum up, we can conclude that the treatment has only a weak effect on per-pupil teaching hours and class size in borderline schools.

What therefore has happened for schools away from the thresholds? Though we do not have a reliable identification strategy to evaluate the RAR effects on these schools, we can nonetheless provide some descriptive statistics. Graphs 4 and 5 support that extra resources may have been allocated to severely disadvantaged schools. Per-pupil teaching hours increased after RAR assignment in September 2006. The average class size was also affected though moderately. However, the literature suggests that such a decrease in class size would have only small effects on achievement : for junior high schools Piketty & Valdenaire (2006) found that dividing the class size by 2 increases the scores of pupils in the National Exam in Grade 9 by only 10% of a standard deviation (see also Gary-Bobo & Mahjoub (2006)).

Figure 4: Pupils per Class (All Grades)

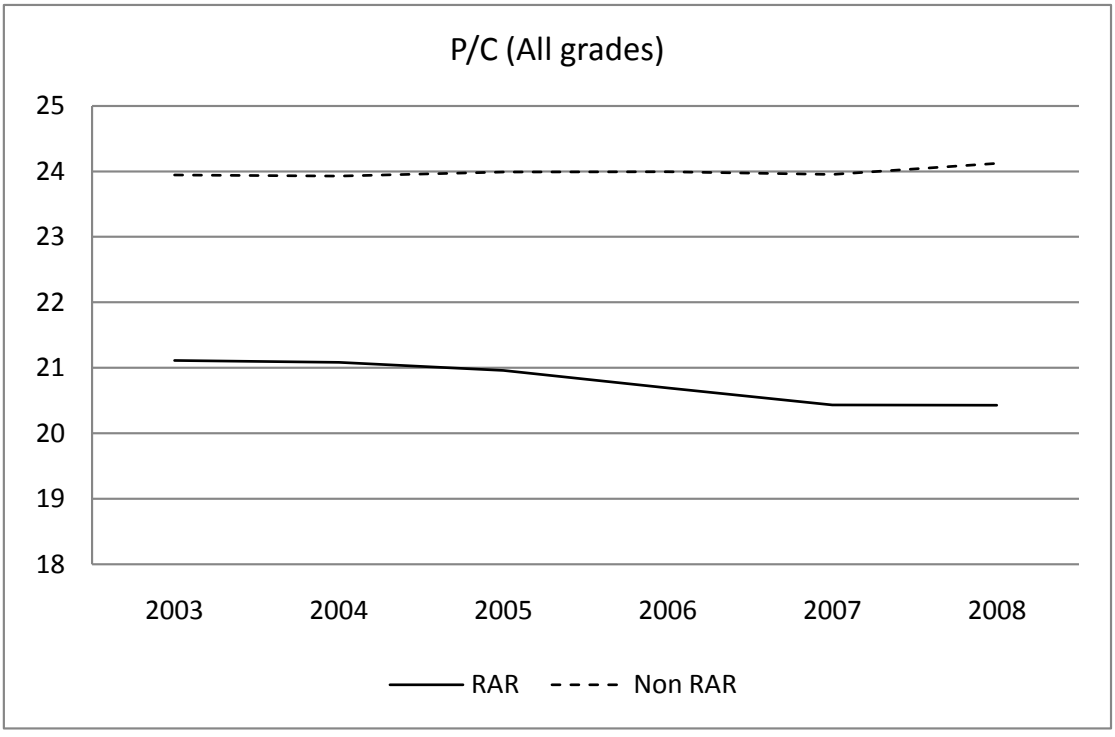
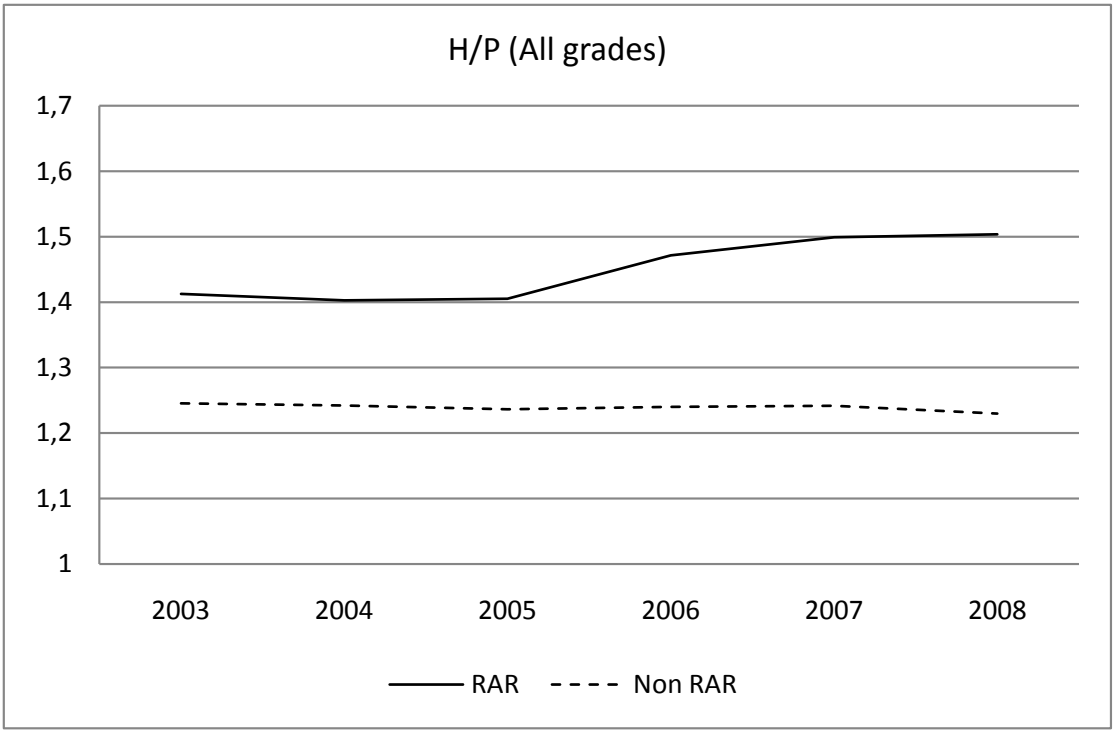


Figure 5: Per-pupil Teaching Hours (All Grades)



Though the program may have had no effect on the resources of borderline schools, it may anyway have induced some teachers to relocate, or some parents to withdraw their children from the treated schools, and this would impact on school achievement.

4.2 Pupils entering Grade 6

We now present results on student enrollment. Before September 2007, pupils were assigned to junior high schools on the basis of their residence. Parents could avoid this assignment by either choosing a private junior high school, or a specific language or option not available in the assigned school when their child entered Grade 6.[13] Previous studies suggest that

[13]In France, in Grade 6, pupils have to choose options that they did not follow before, usually a language. If a pupil wants to avoid his or her assigned junior high school, he or she can ask for learning Russian or Chinese.

Table 3: RAR effect on pupils per class (P/C) and per-pupil teaching hours (H/P) indicators

(a) Disadvantaged discontinuity: $Z^F = 67\%$ and $10\% \leq Z^L$

	h= 4		h= 6		h= 8	
P/C (all grades)	-1.38 *	[1.02]	-0.03	[0.85]	1.09	[0.83]
P/C (9th grade)	-2.81 *	[1.95]	-0.60	[1.70]	-0.89	[1.59]
P/C (8th grade)	0.09	[2.00]	0.96	[1.75]	3.95	[1.72]
P/C (7th grade)	-0.43	[1.89]	0.60	[1.59]	1.60	[1.56]
P/C (6th grade)	-0.82	[1.87]	0.11	[1.60]	0.60	[1.55]
H/P (all grades)	0.07	[0.07]	0.07	[0.06]	-0.02	[0.06]
H/P (9th grade)	-0.10	[0.13]	-0.11	[0.13]	-0.04	[0.12]
H/P (8th grade)	0.03	[0.12]	0.01	[0.11]	-0.16	[0.10]
H/P (7th grade)	0.13	[0.13]	0.14 *	[0.10]	0.06	[0.10]
H/P (6th grade)	0.16	[0.14]	0.21 **	[0.12]	0.05	[0.14]
N° Schools	29		52		76	
N° Obs.	174		312		456	

(b) Repeating discontinuity: $Z^L = 10\%$ and $67\% \leq Z^F \leq 80\%$

	h= 2		h= 3		h= 4	
P/C (all grades)	-0.68	[2.19]	-0.45	[3.98]	0.10	[1.06]
P/C (9th grade)	-1.55	[4.62]	-1.85	[8.15]	0.71	[2.25]
P/C (8th grade)	0.04	[4.82]	1.83	[8.27]	1.26	[2.10]
P/C (7th grade)	-0.44	[4.96]	5.15	[9.50]	1.06	[2.12]
P/C (6th grade)	-3.21	[4.39]	-5.81	[8.98]	-1.96	[2.00]
H/P (all grades)	0.01	[0.15]	0.09	[0.27]	-0.09	[0.09]
H/P (9th grade)	0.08	[0.34]	-0.14	[0.61]	-0.22	[0.17]
H/P (8th grade)	-0.07	[0.31]	-0.16	[0.54]	-0.17	[0.14]
H/P (7th grade)	-0.10	[0.31]	-0.33	[0.64]	-0.11	[0.15]
H/P (6th grade)	0.33	[0.39]	1.15	[1.12]	0.14	[0.18]
N° Schools	33		50		77	
N° Obs.	198		300		460	

Estimated treatment effect, standard error in bracket, unilateral test of equality between treated and non-treated schools. Level: $*$ 10%, $**$ 5%, $***$ 1%

parents are sensitive to junior high school districts when choosing their residence (Fack & Grenet (2010), Black (1999), Gibbons & Machin (2003), Maurin (2004)). Since September 2007, in the aftermath of the Presidential election, the number of exemptions to assignment has increased and it has been easier for parents to choose their children's junior high school. In this context, that of parental junior high school choice, the "RAR" label could have had mixed consequences. On the one hand, it could act as a positive signal for parents - additional school inputs or resources per child- or as a negative signal - indicating severe difficulties at the treated school. For instance, Benabou et al. (2004, 2009) found that the proportion of students who have lunch at school decreases when junior high schools receive the "ZEP" label (Education Priority Zones). This evolution may indicate an increase in segregation of ZEP and non-ZEP junior high schools: poorer kids do not go to the cafeteria for school lunch. The authors also found mixed effects on the total number of pupils in ZEP junior high schools: for junior high schools treated in 1989, the total number of junior pupils decreased relative to non-ZEP schools. However for those treated in 1990, they did not find any effect. For our part, we did not find any significant difference between RAR and non-RAR schools near the second discontinuity concerning the total number of pupils entering Grade 6 (see Table 4b), while for the first discontinuity, we found an increase in pupils entering Grade 6 (see Table 4a). Furthermore, we found no significant effect of treatment on the proportion of pupils entering Grade 6 who have lunch at school (see Tables 4a and 4b).

We also use parental occupations at the beginning of Grade 6 to test whether social segregation increases in "RAR" junior high schools. Results differ along both discontinuities.

For schools around the 'disadvantaged' discontinuity, we found a small negative effect in the enrollment of children whose parents are self-employed, and to some extent, on enrollment of children whose parents are involved in intermediate occupations. This reduction goes along with an increase in the proportion of blue collar parents (see Table 4a). For schools near the 'repeating' discontinuity, we do not observe such an effect (see Table 4b).

We therefore conclude that treatment effects are heterogeneous, since we get different results on both discontinuities. For the first one, RAR policy has had an adverse effect, while for the second no significant effect is detected.

4.3 Teaching structure

In this subsection, we report results on another set of outcomes related to teaching structure. The outcomes we consider are teacher mobility, their seniority and their qualifica-

Table 4: RAR effect on Pupils entering Grade 6

(a) Disadvantaged discontinuity: $Z^F = 67\%$ and $10\% \leq Z^L$

	h= 4		h= 6		h= 8	
Parents' occupation (%):						
Farmer	1.47	[1.27]	1.43	[1.34]	0.31	[1.14]
Executive	-9.62 ***	[3.26]	-8.41 ***	[2.76]	-8.42 ***	[2.52]
Manager	0.62	[2.42]	0.03	[2.14]	-1.09	[1.91]
Intermediate	-6.78 *	[3.50]	-6.26 **	[3.09]	-3.52	[3.04]
Employee	3.60	[4.68]	4.24	[4.28]	2.69	[3.99]
Worker	11.96 **	[5.89]	10.67 *	[6.14]	12.16 **	[6.16]
Retired	1.23	[1.50]	0.61	[1.36]	0.87	[1.33]
Unemployed	0.30	[5.31]	-2.89	[4.58]	-5.95	[4.59]
Not known	-2.78	[4.93]	0.58	[4.23]	2.94	[4.03]
Lunch at school	1.17	[6.61]	2.45	[5.86]	-1.79	[5.73]
Tot. Entering	6.94	[12.51]	20.14 *	[11.92]	34.80 ***	[12.80]
N° Schools	29		52		76	
N° Obs.	174		312		456	

(b) Repeating discontinuity: $Z^L = 10\%$ and $67\% \leq Z^F \leq 80\%$

	h= 2		h= 3		h= 4	
Parents' occupation (%):						
Farmer	0.38	[4.22]	-2.90	[6.42]	-0.90	[1.56]
Executive	-6.72	[6.70]	-9.99	[13.48]	-4.31	[2.91]
Manager	5.41	[5.49]	3.39	[8.82]	3.64	[2.61]
Intermediate	2.33	[7.90]	-3.08	[14.40]	0.29	[3.77]
Employee	7.34	[11.13]	-23.10	[25.60]	-4.21	[4.86]
Worker	-3.40	[17.39]	68.61	[67.20]	10.32	[8.26]
Retired	-1.70	[3.98]	-2.00	[6.91]	0.77	[1.78]
Unemployed	2.53	[14.10]	8.21	[24.59]	3.76	[6.23]
Not known	-6.17	[10.78]	-39.14	[39.45]	-9.36 *	[4.86]
Lunch at school	-0.56	[10.80]	6.23	[16.71]	-2.35	[7.08]
Tot. Entering	-6.26	[25.59]	-27.84	[58.00]	13.11	[15.59]
N° Schools	33		50		77	
N° Obs.	198		300		460	

Estimated treatment effect, standard error in bracket, bilateral test of equality between treated and non-treated schools. Level: $*$ 10%, $**$ 5%, $***$ 1%

tions. For instance, some incentives, such as faster access to promotion, were implemented to encourage teachers to teach longer within RAR schools.

For the ZEP program, these issues were studied by Benabou et al. (2004, 2009) with a different identification strategy. The authors found that the incentives were insufficient to cap the high turnover of teachers in treated schools. Ly (2010) found that the reform of the ZEP program in 1999 had had an adverse effect on the age and the experience of teachers in treated schools. To study this issue, we compare the structure of age and qualifications of teachers in schools close to the discontinuities. Tables 5a and 5b report regression discontinuity estimates. These results are novel with respect to the Preferential Educational Policy litterature and differ from the results obtained by Benabou et al. (2004, 2009) and Ly (2010).

For schools where $Z^F = 67\%$, the proportion of teachers over 55 has increased significantly with the introduction of the RAR program. For these schools, the proportion of highly qualified teachers ("agrégation") has decreased while the proportion of teachers having a non-standard qualification in junior high schools has increased. This may be due to the fact that RAR policy has encouraged primary school teachers to work in junior high schools. The new teachers assigned to treated junior high schools may more often have been primary school teachers, and often the oldest, being the most experienced, are more likely to have been selected. It could also be explained by lower mobility among older teachers who would like to leave their newly assigned RAR school, but cannot for family reasons. For schools where $Z^L = 10\%$, not a single treatment effect can be displayed on the teaching structure. Once again, such a difference between the two discontinuities highlights the heterogeneity of treatment effects.

4.4 Maths and Literacy scores

Finally, we present results on student achievement. Table 6 reports estimates of treatment effects on junior high school score distributions. We find that treatment has a negative effect on scores, when significant. This result is more pronounced for schools where $Z^F \approx 67\%$ and $Z^L > 10\%$. For these schools, the treatment effect differs for maths and French (Tables 6a). The negative effect of treatment on scores is mainly concentrated at the bottom of the French distribution, whereas, for maths, it is at the top of the distribution. For schools where $Z^L \approx 10\%$ and $80\% \geq Z^F \geq 67\%$, estimates are often insignificant (especially in French) but other than that, these effects are quite large and negative. For these schools, treatment effects are mainly visible in the middle of the Math score distribution.

Table 5: RAR effect on teachers' characteristics

(a) Disadvantaged discontinuity: $Z^F = 67\%$ and $10\% \leq Z^L$

	h= 4		h= 6		h= 8	
Tot. of hours dispensed	68.45	[56.68]	135.08 ***	[49.33]	167.77 ***	[53.47]
% of hours dispensed by :						
Highest teaching degree	-0.03	[0.02]	-0.03 *	[0.02]	-0.03 *	[0.02]
Qualified teacher	-0.02	[0.05]	-0.04	[0.04]	-0.04	[0.04]
PE teacher	0.01	[0.01]	0.00	[0.01]	0.00	[0.01]
Other teacher	0.04	[0.05]	0.07 *	[0.04]	0.07 *	[0.04]
Teachers under age 30	-0.07	[0.05]	0.01	[0.05]	0.01	[0.05]
Teachers between 30 and 39	-0.01	[0.05]	-0.03	[0.05]	-0.07	[0.05]
Teachers between 40 and 55	0.02	[0.05]	-0.02	[0.05]	0.04	[0.04]
Teachers over 55	0.12 **	[0.05]	0.09 **	[0.04]	0.11 **	[0.04]
N° Schools	29		52		76	
N° Obs.	174		312		456	

(b) Repeating discontinuity: $Z^L = 10\%$ and $67\% \leq Z^F \leq 80\%$

	h= 2		h= 3		h= 4	
Tot. of hours dispensed	-35.91	[106.86]	154.44	[283.97]	62.58	[63.67]
% of hours dispensed by :						
Highest teaching degree	0.01	[0.05]	0.01	[0.09]	-0.01	[0.02]
Qualified teacher	-0.09	[0.11]	-0.24	[0.25]	0.00	[0.05]
PE teacher	0.04	[0.03]	-0.03	[0.05]	0.00	[0.01]
Other teacher	0.04	[0.10]	0.26	[0.25]	0.02	[0.05]
Teachers under age 30	-0.04	[0.13]	-0.08	[0.23]	0.01	[0.06]
Teachers between 30 and 39	0.02	[0.14]	0.36	[0.40]	0.04	[0.06]
Teachers between 40 and 55	0.04	[0.13]	-0.26	[0.33]	-0.07	[0.06]
Teachers over 55	-0.12	[0.13]	-0.01	[0.19]	0.06	[0.05]
N° Schools	33		50		77	
N° Obs.	198		300		460	

Estimated treatment effect, standard error in bracket, bilateral test of equality between treated and non-treated schools. Level: $*$ 10%, $**$ 5%, $***$ 1%

How can we interpret these results on the change in pupil achievement as a result of treatment? The results may be due to a combination of two distinct effects: a potential increase in the sorting of pupils across schools based on ability and/or parental schooling choice, and the potential inefficiency of the educational policy within treated schools. Our results imply the existence of at least one of these two effects. It cannot be excluded that the policy has had a positive effect on pupil achievement for those that remain in treated schools, but this effect might be compensated for by an increase in a pupil selection effect across schools. Moreover, we note that we can only observe the results of pupils up until 2009. Therefore observed final scores are relevant to pupils who began junior high schools before the beginning of the RAR program. As a consequence this result can differ from the effect of policy on pupils having a full scholarship into treated schools.
A referee suggested that the test results should not be pooled to assess the program impact on score results. We have thus estimated the effect of RAR treatment with an unequal length of treatment:

$$Y_{it} = \alpha_i + \beta_t + \gamma_{2007} T_{it} \mathbb{1}_{\{t=2007\}} + \gamma_{2008} T_{it} \mathbb{1}_{\{t=2008\}} + \gamma_{2009} T_{it} \mathbb{1}_{\{t=2009\}}$$

where T_{it} equals 1 if the junior high school i enters the RAR program at date t. These variables $T_{it}\mathbb{1}_{\{t=j\}}$ are instrumented by $\mathbb{1}_{\{Z_i \geq c\}}\mathbb{1}_{\{t=j\}}$. We obtain similar results which are robust to a differentiating length treatment effect.

5 Conclusion

In this paper, we have evaluated the effect of the RAR educational policy, introduced in French public junior high schools in September 2006. To do so, we used two strong discontinuities in assignment to the treatment. For schools close to these two discontinuities, no substantial desired treatment effects stand out. Precisely, resources allocated to schools were disappointing around the thresholds. Second, we found that the policy worsens social segregation across schools when measured by parental occupation. This may induce the reduced achievement we observed in treated schools. The RAR assignment would therefore appear to have had a strong negative signaling effect, encouraging better students to move from their junior high school. An alternative, but discouraging, explanation would be that the policy has had a negative effect on achievement in treated schools. Our results on teachers indicate an increase in the proportion of older and less qualified teachers. This could be explained by a reassignment of older primary teachers to RAR schools, in order

Table 6: RAR effect on school level distribution of scores at the National Exam in Grade 9

(a) Disadvantaged discontinuity: $Z^F = 67\%$ and $10\% \leq Z^L$

	h= 4		h= 6		h= 8	
French - Mean	0.74	[1.29]	-1.79	[1.25]	-2.03	[1.18]
French - Q10	-0.14	[1.55]	-1.75	[1.46]	-2.18 ***	[1.41]
French - Q25	0.14	[1.41]	-2.01	[1.34]	-2.48 ***	[1.28]
French - Median	-0.05	[1.47]	-2.32	[1.47]	-2.71 *	[1.39]
French - Q75	1.90	[1.66]	-1.11	[1.51]	-1.13	[1.42]
French - Q90	3.23 *	[1.86]	-0.66	[1.70]	-0.62 *	[1.58]
Maths - Mean	-1.13	[2.23]	-2.60	[1.94]	-4.75 *	[1.92]
Maths - Q10	1.20	[2.19]	-0.17	[1.88]	-1.02	[1.68]
Maths - Q25	0.70	[2.56]	-1.86	[2.20]	-4.49	[2.11]
Maths - Median	-1.69	[2.64]	-2.88	[2.27]	-5.40 **	[2.29]
Maths - Q75	-3.59	[2.70]	-4.68 *	[2.41]	-6.95	[2.46]
Maths - Q90	-3.09	[2.82]	-3.93	[2.52]	-6.47 **	[2.54]
N° Schools	29		52		76	
N° Obs.	174		312		456	

(b) Repeating discontinuity: $Z^L = 10\%$ and $67\% \leq Z^F \leq 80\%$

	h= 2		h= 3		h= 4	
French - Mean	-6.77	[4.80]	-0.37	[5.63]	-0.49	[1.52]
French - Q10	-0.97	[3.72]	4.67	[7.50]	0.06	[1.76]
French - Q25	-2.43	[3.95]	1.56	[6.54]	0.02	[1.71]
French - Median	-7.24	[5.38]	0.02	[6.38]	-0.84	[1.73]
French - Q75	-13.23 *	[7.75]	-4.72	[7.78]	-1.02	[1.84]
French - Q90	-11.01	[7.01]	0.18	[7.94]	1.13	[2.13]
Maths - Mean	-7.74	[6.23]	-5.59	[9.52]	-3.61	[2.42]
Maths - Q10	-5.76	[5.23]	-5.29	[8.81]	-1.91	[2.10]
Maths - Q25	-4.13	[5.43]	-8.99	[11.77]	-5.32 *	[2.81]
Maths - Median	-8.14	[6.92]	-13.21	[15.00]	-6.27 **	[3.13]
Maths - Q75	-11.59	[8.69]	-3.24	[11.20]	-2.63	[2.99]
Maths - Q90	-7.65	[8.09]	9.70	[14.51]	-0.04	[3.10]
N° Schools	33		50		77	
N° Obs.	198		300		460	

Estimated treatment effect, standard error in bracket, bilateral test of equality between treated and non-treated schools. Level: ∗ 10%, ∗∗ 5%, ∗∗∗ 1%

for them to obtain promotion more easily.

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