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The French zones d'éducation prioritaire: Much ado about nothing?

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ABSTRACT

We provide an assessment of the French ZEP (Zones d'Education Prioritaire), a program started in 1982 that channels additional resources to schools in disadvantaged areas and encourages the development of new teaching projects. Focusing on middle-schools, we first evaluate the impact of the ZEP status on resources, their utilization (teacher bonuses versus teaching hours) and key establishments characteristics such as class sizes, school enrolments, teachers' qualifications and experience, and student composition and mobility. We then estimate the impact of the ZEP program on four measures of individual student achievement: obtaining at least one diploma by the end of schooling, reaching 8th grade, reaching 10th grade and success at the Baccalauréat (the national examination at the end of high school). We take into account the endogeneity of the ZEP status by using both difference in differences and instrumental variables based on political variables. The results are the same in all cases: there is no impact on student success of the ZEP program.

1. Introduction

Many developed countries have adopted compensatory education policies that direct extra resources to disadvantaged schools. In the United States, this has been the case since 1965 with the Title I program. In Great Britain, the Education Priority Areas experiment of the early 1970s followed the Plowden report, a thorough review of primary education in England which had a considerable impact. This program re-emerged in 1997–1998 as the Education Action Zones and Excellence in Cities. A number of other countries, such as Portugal and Belgium, have similar programs.

In France, the idea of priority zones first spread among teacher unions in the 1970s and then became part of the political platform of the Socialist party, which came to power in 1981. Thus, in 1982, a new program, the "Zones d'Education Prioritaire" (Priority Education Zones, henceforth ZEP) was launched, under which selected schools received extra resources such as funds, teacher hours, etc. These zones were originally meant to be temporary, but the program instead became permanent and was substantially extended in successive "waves" throughout the 1980s and 1990s. Initially, the main goal of the ZEPs was to foster new educational projects and partnerships with local actors that would help improve academic achievement. Gradually, decreasing class size also became an important objective. The amount and nature of the extra resources given to the ZEPs were never specified, however, nor the actual procedure by which priority status was to be determined. Perhaps most tellingly, to this day the Ministry of Education's budget still contains no specific line item for "priority education".

As the first program to target schools and local areas rather than provide individual financial aid to poor students (usually very modest stipends), the ZEPs remain highly controversial. Some argue that any form of "positive discrimination" is contrary to the national ideal, others that it serves both fairness and efficiency and should get much more resources, others yet that it has just been a waste of money. Most recently, in the wake of the riots which

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occurred in the poor suburbs of large cities in November 2005, the French government put forth a set of new measures intended to promote greater equal opportunity through subsidized housing projects and reforms of the education system and labor market regulation. The flagship decision in the education field is to be a new expansion and reform of the ZEP program.

In spite of the central place it occupies in French education policy and the political debate, there has been no systematic evaluation of the impact of the ZEPs on schooling outcomes. In this paper, we provide an assessment of the ZEPs along three dimensions: the resources effectively deployed, the mobility response of students and teachers, and the overall impact on academic achievement. Due to data limitations we study the period that covers only the first phase of the policy (1982–92) and focus on its implementation in lower secondary schools.

This evaluation presents particular challenges. In particular, the precise nature of the intervention is not well known: priority status is allocated according to a rather opaque, erratic administrative procedure and it translates into unspecified extra resources that may vary from zone to zone, from year to year, or be used differently by different schools. Together with the lack of budgetary data, this leads us to devote the first part of the paper to a kind of "detective work" aimed at determining what it meant, *in practice*, for a school to become a ZEP—in terms of inputs such as class size, weekly teaching hours, qualifications and seniority of the teachers, and size and composition of the student body. Then, in the second part of the paper, we examine how the granting of ZEP status to a junior high school affected its students' educational outcomes, both in those grades and beyond. The fact that the priority status may – in particular through a "labeling" effect – induce a mobility response on the part of teachers or students' families (moving to another district, sending their children to private school, etc.) is another source of complexity in evaluating the ZEPs. At the same time, studying the elasticity of such responses and assessing educational outcomes from a program in which they potentially operate is both interesting and important, because any intervention that exceeds the scale of small controlled experiments is likely to trigger such behaviors.

Our analysis combines very rich student survey data with administrative files on all French schools and teachers. The first source consists of two panels of students that provide detailed information on their family background, early educational history, and major academic outcomes throughout secondary schooling. The second source is an original panel of schools that we constructed, using exhaustive establishment files from the Ministry of Education. We unfortunately had to restrict attention to secondary schools because of the available data, even though the ZEP program also concerns primary schools.

We address the endogeneity in the allocation of ZEP status using two methods, namely difference-in-differences (or establishment fixed effects) and instrumental variables. These techniques are applied with four different measures of academic achievement. Our instrumentation strategy relies on the fact that national political forces interfered in the process by which priority zones were designated, and in particular in the timing of the choice of ZEPs. We show that the vote shares obtained in different French regions by the major parties in the national (legislative) elections of 1981 and 1988 provide a plausibly exogenous source of variation that we can use to identify the ZEP effect.

Our results on the *nature* of the ZEP "treatment", at least in term of quantifiable inputs, show that this treatment translated into a continuous but extremely slow decrease in class sizes (−0.2 students per year on average) and increase in teaching hours per student (+1.2% per year); meanwhile, the experience of ZEP teachers declined slightly, in spite of the bonuses offered. Our results on the *change* in the composition of the student body show a large reduction in enrollments following a school's classification as ZEP. This reduction did not come from an increase of the number of the students leaving these schools but from a decline in arrivals, and it did not lead to an obvious deterioration in the socio-economic background of the students. Turning finally to the *effects* on student achievement of the overall ZEP treatment (including financial resources, the more qualitative aspects of the zones' educational projects and the effort and mobility responses of students and teachers), the results from both our methodologies lead to the same conclusion: during our sample period, the impact of the ZEPs on the subsequent academic achievement of pupils in the 6th and 7th grade (age 11 and 12) is nil.

The paper is organized as follows. In Section 2, we describe the general educational context and the main features of the ZEP program. The data are described in Section 3. Changes in schools' characteristics when they become ZEP are then examined in Section 4, and the impact of priority zone status on student achievement is analyzed in Section 5. Section 6 offers a brief summary of our main findings and their policy implications.

2. A brief description of the Education Priority Zones ("Zones d'Education Prioritaire")

First established in 1982, the ZEPs, or education priority zones, include both primary (1rst to 5th grades) and junior-high establishments (6th to 9th grades), plus a small number of high schools (10th to 12th grades). Initially, the ZEPs covered approximately 8% of junior-high students in the country. The program was originally meant to be temporary, with the zones established for a limited statutory term of 4 years. Over time, however, the program was not only maintained but substantially expanded, with many new zones created in 1989, 1990, 1994 and 1999. It is now the main policy in France directed at helping students from disadvantaged backgrounds.

The operating principle of the ZEP program is to provide additional resources to schools in the most disadvantaged zones and allow them to develop specific initiatives and educational methods tailored to their students' needs. In 1982 the regional heads of the education administration were thus asked to select zones according to some vague criteria on the student population.[1] In later years many new

[1] Heads of the administration were conduced to use indicators such as parents' social and professional backgrounds, parents' rate of unem-

zones were created, according to a procedure that left considerable discretion to the regional heads of the Education Ministry administration ("recteurs") in making decisions that were loosely based on indicators such as the shares among parents of blue-collar workers, unemployed workers, and high-school dropouts, the fraction of families with at least one non-European member, and 3rd grade test scores in a national student evaluation.

In 1997 nearly 700,000 students in primary schools and 400,000 in junior high schools benefited from "priority" treatment, representing respectively 11% and 15% of the respective totals.[2] Only very few high schools are ZEPs. The vast majority of ZEP students live in major urban centers. A good indicator of poverty is the fraction of students *not* enrolled in the school's cafeteria's plan, because this generally denotes that their family cannot afford to pay for these lunches.[3] This fraction is indeed higher in the priority zones (69%) than outside (40%), and reaches 80% in those ZEPs located in major urban centers. Similarly, the proportion of non-French students is typically higher, exceeding 35% in more than 10% of the ZEP schools. Concerning educational attainment, in 1995 37% of students entering the 3rd grade (age 8) in ZEP schools did not possess "basic reading competencies", which is double the proportion found in non-ZEPs (18%).

The ZEP status is associated with extra resources for the selected schools, mostly in the form of additional hours of instruction and bonuses for teachers and other personnel. Here again, considerable discretion was given to the regional heads of the education administration with respect to the amount and nature of the resources allocated to ZEP schools. The French education system is extremely centralized and the ZEP program was the first one whose aim was to give relative freedom of action to local authorities. One of the drawbacks was that the information about the extra resources was not collected at the centralized level. The first year for which *any kind* of budgetary information is available is the school year 1998–1999. Extra resources directed to the ZEPs that year amounted to €400 million, of which €110 million took the form of bonuses paid to all employees of the schools – mostly teachers – while the rest was used to increase total teaching hours and reduce class sizes.[4] Every ZEP employee also benefited from a relative advantage in administrative promotion criteria. As far as non-wage financial resources are concerned, there was no priority for ZEP schools (Jeljoul, Lopes, & Degabriel, 2001). In particular, regional and local subsidies were not higher in ZEP schools.

The €400 million represented 1.2% of total expenditures on teaching activities in primary and junior high schools, and were directed to 12% of the total student population in those grades. This means that a school's classification as ZEP translated in 1998–99 into an extra 10% more resources per student. Looking more appropriately at instruction costs (leaving aside fixed operating costs, etc.), which represent 77% of total costs on average, the increase was 13%.[5] No such data is available for the earlier period that corresponds to most of our student sample. We can, however, use some of our estimation results to come up with a reasonable ballpark estimate. Based on the establishment data, we estimate that teaching hours per student increased by about 1.2% per year following the granting of ZEP status in the 1989 and 1990 waves (relative to non-ZEP schools). We also know that the teacher bonus in 1990–91 was approximately equal to 2% of the average teacher wage. Thus we can estimate that the ZEP label brought an extra 4.4% in the teaching budget that year, with the allocation between bonuses (2%) and hours per student (2.4%).

By comparison, the resources devoted to the EIC program in the United Kingdom amounted to £300 million in 2003 (around €440–460 million) and covered about a third of secondary school pupils (Machin, McNally, & Meghir, 2004). In the United States, Title I programs provided $11.7 billion in 2003 to schools with high concentrations of poverty and this funding represented roughly 5% of these schools' total budget (Van der Klaauw, 2008). In terms of overall financial scope, these three different national programs are thus more or less comparable. They differ markedly, on the other hand, in how the additional resources are used. In the United Kingdom, the program includes very specific components aimed at extending learning opportunities through additional teachers, support units, or extra help for the most disadvantaged children. In the United States, funds are explicitly targeted to disadvantaged children inside the school.[6]

In France, the nature of the intervention was left to each school's discretion: the idea of the ZEP program was just to impulse new educational projects and partnerships with local authorities. Existing evaluations are based on simple comparisons of average achievement between ZEP students and non-ZEP students (Meuret, 1994; Caille, 2001). Our paper thus differs from these earlier analyses through the use of econometric techniques to address the endogeneity and selection-bias problems that are of primary concern in

ployment, fraction of students who are not native French speakers and fraction of students having repeated a grade (a very large fraction of French students repeated at least once in those years).

[2] The statistics presented in this paragraph are taken from the Ministry of Education's Note n° 98-15, "Les Zones d'Education Prioritaires en 1997–1998".

[3] In France, lunches are subsidized for everybody but there are no free-lunch programs. Some of the students who do not have lunch at school have lunch at home because the family income is large enough that the mother (or father) does not work. But the majority of students who do not have lunch at school are those whose family cannot afford to pay for the lunches.

[4] Thus, a ZEP bonus of 1,046 Euros was paid to 96,000 teachers, accounting for 100 out of the 110 million.

[5] We can decompose this 13%: 13*(110/400) = 3.6% went to bonuses and the remaining 9.4% to increased hours of teaching per student. As a check on this calculation, direct computations based on the data we obtained show that the bonus in that year was approximately 4% of the average teacher salary.

[6] Machin et al. (2004) find a positive, albeit small, improvement in pupil outcome measures for Mathematics (but not for English) and a strong reduction in absences within EiC schools. On the contrary, Van der Klaauw finds that the Title I program does not improve student achievement. He interprets this finding as related to the manner in which funds are spread thinly across a large number of schools and to the apparent ineffectiveness of many Title I funded remedial education programs.

the literature on program evaluation, such as the nonrandom allocation of the ZEP status.

3. The data

3.1. The FSE datasets

The FSE administrative files (Fichiers Standards Enrichis) of the Ministry of Education constitute our main source of school-level data. Every year, all school principals fill out a detailed questionnaire on the characteristics of each grade in their establishment. This includes information on class size, nationalities of students, number of students having repeated each grade, number of students having lunch at the school's cafeteria, and language courses chosen. Each statistic is measured by grade. These data are exhaustive for the period 1987 to 1992 and cover both public and private schools; we shall focus here on public-sector junior high schools (6th to 9th grades).[7]

The FSE dataset can be extended to the period 1994–1999 by another data source, the IPES (Indicateurs pour le Pilotage des Etablissements Scolaires). Unfortunately, not all the variables reported there are compatible in their definitions with those available for the earlier period. We will therefore mainly present the results based on the FSE dataset and report more briefly on those obtained for the longer period, which are essentially identical.

We also link these two establishment datasets with files from the Education Ministry's information system for the management of teachers. This source provides us with statistics on teacher characteristics by establishment, such as the number of young or experienced teachers, the diplomas held, etc. Finally, another set of files from the Ministry of Education identify which establishments are located in a priority zone.[8]

3.2. The panel datasets

The main sources of individual data used in our analysis are two panels of students collected by the Ministry of Education.

3.2.1. The 1980 panel

This panel includes 20,961 students who entered 6th grade (age 11) in 1980, and constitutes a representative sample of 1/40 of all students entering junior high school in France that year. The sampling scheme has two levels. First, establishments were stratified according to city size, establishment size and sector (public or private school). One in five junior high schools was thus selected. Second, one out of eight students in those schools was selected and then followed across establishments until the completion of his or her secondary schooling. The attrition rate was 9%, reflecting departures abroad, deaths, and (in 80% of cases) "unexplained" school departures.

3.2.2. The 1989 panel

This sample includes 24,455 students who entered 6th grade in 1989. The sampling scheme here has only one level: the heads of all junior high schools were asked to include in the panel all students entering 6th grade in 1989 who were born on a specific day in each month (1/30th). These students were observed until the end of their schooling, including higher education. The attrition rate was similar to that observed in the 1980 panel.

These two panels provide extensive information about the students. First, we have family background data: date of birth, sex, nationality, country of birth, number of siblings, birth rank, occupation of the head of the family, parent(s) legally responsible for the child, and number of years spent in nursery school and in primary school. Second, we have schooling information for each year from 6th grade on: grade, class size, foreign languages studied, lunch at the school's cafeteria or not, financial aid received. For each observation (student-year), we also know the identification number of the schooling establishment.

The only available characteristic on classes is class size. The information on schools consists of the establishment's identification number, the educational administrative region and whether it is a private or public establishment. Thanks to the identification numbers, we are able to match the student panels with the time-varying ZEP or non-ZEP status. Given our sample period, we had students in each of the three "waves" of ZEPs—1982, 1989 and 1990. We also computed school-level variables by averaging students' characteristics by establishment in each panel.

We now turn to measures of academic achievement. The panels unfortunately do not contain information on the grades received in national exams or national evaluations (the only national evaluation in high school is in 6th grade). We do know, however, whether the student passed or failed any exam that she or he took. We also know, for each year, whether he or she moves up to next grade, repeats the grade, or exits to the vocational track. More precisely, the four measures of academic achievement that we use are:

- Completion of school years with at least one degree (versus finishing schooling without any diploma).
- Moving up to 8th grade (versus switching to a vocational track at the end of 7th grade).
- Moving up to 10th grade (versus switching to a vocational track after the 7th or the 9th grade).
- Success at the "Baccalauréat", the French national exam at the end of high school, i.e. 12th grade (versus going to a vocational track that does not lead to the Baccalauréat, or failing the Baccalauréat).

These choices are motivated by the following observations. First, reducing the number of students who leave the school system without any degree was the main target of the Ministry of Education in general and of the ZEP program in particular.[9] More generally, this measure captures the bottom part of the achievement distribution. Second,

[7] Throughout the paper we will use "year" instead of the "school year"; for instance, year 1987 corresponds to the school year 1987–1988.

[8] Descriptive statistics are available in Benabou, Kramarz, & Prost (2005), BKP hereafter.

[9] Among students entering 6th grade in 1980, 23% had no degree at the end of their schooling years.

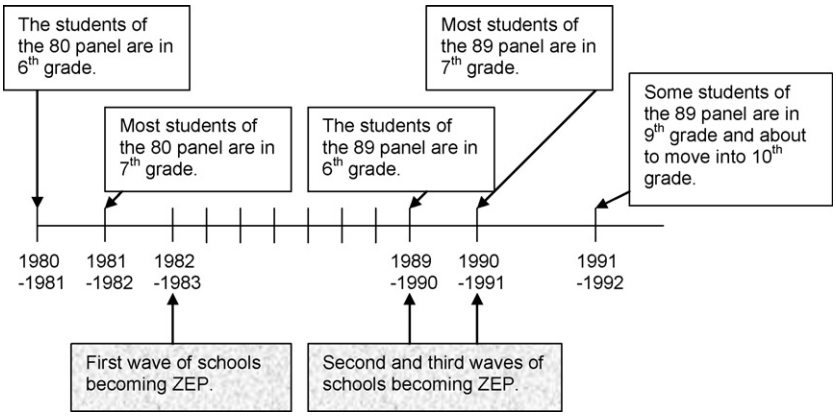

Fig. 1. Chronology.

in France many students are virtually forced to switch to a vocational track (seen as much less desirable and even stigmatizing) at the end of 7th or 9th grades. For instance, only 46% of those entering 6th grade in 1980 continued their education in the regular track all the way to 12th grade. Reaching the 8th and 10th grades are therefore important milestones in the schooling process. Finally, success at the Baccalauréat is key because it conditions entry into the university system and the "grandes écoles" (selective colleges).

Fig. 1 describes how the time structure of our panels meshes with the chronology of the ZEP program. In 1981, just before the launching of the policy, most students of the 1980 panel were in 7th grade (except those who repeated 6th grade). By contrast, for the students in the 1989 panel, entry into 6th grade took place as 1989 wave of ZEPs was ongoing. And one year later, when most of them were in 7th grade, the 1990 wave was launched. Because conditions prevailing in the 6th and 7th grades strongly affect students' future schooling outcomes, and because mobility between establishments is low, the ZEP indicator we shall use is equal to 1 if the student's establishment in his or her 7th grade is located in a ZEP, and equal to 0 if not.[10] Note that this indicator is time-varying, since some students in the 1980 panel do their 7th grade in schools that will become ZEPs only in 1982, 1989 or 1990, while some in the 1989 panel do their 7th grade in the very same schools after they have acquired priority status.

3.3. The political dataset

The last set of data that we use is political variables, which will provide instruments to address the potential selection bias in the determination of the priority zones. The shares of the vote received by each political party in every parliamentary election are collected at the regional level ("département") by the CEVIPOF ("Centre d'Etudes de la Vie Politique Française"). To reflect the main features of the French political system we aggregated them into "Extreme Right", "RPR and UDF" (the two main right-wing parties), "Other Right", "Greens", "Communist Party", "Socialist Party", and "Other Left". We grouped RPR and UDF together, since these two parties formed a coalition (single list) during some of the parliamentary elections in the 80s. Voting in French parliamentary elections involves two rounds, and as in Bertrand and Kramarz (2002) we used only the first round. Finally, we focused on the years that precede 1982, 1989 and 1990 (when new ZEPs were implemented), that is, on the elections that took place in 1981 and in 1988. Our variables thus consist of the fraction of votes received by the various political groups in every "département", in each of these two elections.

4. The impact of ZEP status on school characteristics

4.1. Number of students and class size

Using the FSE data for every year from 1987 to 1992, we analyze the changes in school characteristics that occur when the ZEP status is granted. We distinguish the impact of this event on both levels and trends, by estimating the following model

$$\begin{aligned} Y_{j,t} = {} & \delta_t + \delta_j + 1_{j\in zep89}[\gamma^{zep89} 1_{t\geq 88} + \gamma^{zep89}_{change} 1_{t\geq 89} \\ & + \gamma^{zep89}_{trend} T_{89}] + 1_{j\in zep90}[\gamma^{zep90} 1_{t\geq 88} + \gamma^{zep90} 1_{t\geq 89} \\ & + \gamma^{zep90}_{change} 1_{t\geq 90} + \gamma^{zep90}_{trend} T_{90}] + \varepsilon_{j,t}, \end{aligned} \quad (1)$$

where $Y_{j,t}$ corresponds to characteristics for school j in year t, δ_t is an indicator for year t (year fixed effect), δ_j an indicator for the establishment (school fixed effect), $1_{j\in zep89}$ is equal to 1 if the school j becomes ZEP in 1989, $1_{t\geq\tau}$ is equal to 1 if the year t is greater or equal to τ, T_{89} is a linear trend starting in 1989, and ε is an i.i.d. residual. The variables in the second line of (1) are defined similarly.

Since year dummy variables control for evolutions common to all establishments and school dummy variables control for any difference in levels between them that already existed in 1987, the coefficients γ^{zep89}_{change} and γ^{zep90}_{change} capture the change in the level of $Y_{j,t}$ specifically associated to a change in status. Similarly, γ^{zep89}_{trend} and γ^{zep90}_{trend} capture the linear trends specific to ZEP establishments

[10] Our results are robust to using instead an indicator equal to 1 if the student was in a ZEP in either the 6th or 7th grade, due to the low inter-establishment mobility between these two classes.

Table 1
Students, teachers and ZEP.

	Number of students	Number of students per class	Number of teachers	Number of teachers per student	Number of weekly hours per student	Share of young teachers	Share of non-certified teachers
Zep89*1988 dummy	−6.7 (4.7)	0.2* (0.1)	−0.6 (0.4)	0.000 (0.001)	0.008 (0.010)	−0.006 (0.008)	−0.002 (0.005)
Zep89*1989 dummy (zep effect, level)	−3.9 (5.3)	−0.2 (0.2)	−0.3 (0.5)	0.000 (0.001)	0.012 (0.012)	−0.014* (0.009)	0.010* (0.006)
Zep89*Trend starting in 1989 (zep effect, trend)	−6.5** (1.5)	−0.2** (0.0)	0.1 (0.1)	0.001** (0.000)	0.011** (0.003)	0.013** (0.002)	−0.003* (0.002)
Zep90*1988 dummy	0.3 (4.0)	0.3** (0.1)	0.1 (0.4)	0.000 (0.001)	−0.003 (0.009)	0.000 (0.007)	0.005 (0.004)
Zep90*1989 dummy	−5.2 (4.0)	−0.2 (0.1)	−0.3 (0.4)	0.000 (0.001)	0.008 (0.009)	0.008 (0.007)	0.004 (0.004)
Zep90*1990 dummy (zep effect, level)	−6.1 (5.1)	0.0 (0.2)	−0.2 (0.5)	−0.001 (0.001)	−0.004 (0.011)	−0.014 (0.008)	−0.007 (0.005)
Zep90*Trend starting in 1990 (zep effect, trend)	0.5 (2.0)	−0.2** (0.1)	0.4* (0.2)	0.001** (0.000)	0.015** (0.004)	0.009** (0.003)	0.012** (0.002)
R-Square:	0.97	0.75	0.96	0.82	0.79	0.66	0.63

Source: FSE files, 1987–1992, 4743 establishments per year. Standard Errors in parentheses (*: significant at the 10% level, **: −5% level). Establishment fixed effects and year fixed effects.

after they became ZEP. Because a ZEP-specific trend could pre-exist, the coefficients γ_{88}^{zep89}, γ_{88}^{zep90} and γ_{89}^{zep90} capture any changes in $Y_{j,t}$ that started in 1988 (respectively, in 1988 or 1989) between the establishments that became ZEP in 1989 (respectively, in 1990) and the others.

In Table 1 we only report γ_{88}^{zep89}, γ_{88}^{zep90}, γ_{89}^{zep90}, γ_{change}^{zep89}, γ_{change}^{zep90}, γ_{trend}^{zep89} and γ_{trend}^{zep90}, together with their estimated standard deviations. The estimation is carried out over 4743 junior high schools per year. Among those, 138 establishments became ZEP in 1989 (we shall refer to them as ZEP-89) and 365 others acquired the status in 1990 (ZEP-90).

In 1987, ZEP establishments are on average 7–9% larger than others. Table 1 reveals that the average number of students decreased by 7 per year in those schools that became ZEP in 1989. The most likely explanation is that of an avoidance strategy on the part of some families, even before the status change, a view that find further support in an analysis of student mobility, which confirm that the reduction in the number of students was primarily due to reduced entry into those establishments, rather than to increased exit.[11]

The coefficients for the ZEP-90 are generally not significant, but this is not surprising given that the FSE data ends in 1992, making it difficult to identify structural breaks. And indeed when we analyze the full 1987–1999 period using both FSE and IPES data, the previous results are largely confirmed. The number of students in ZEP-89 schools continued its gradual decrease, relative to the general trend, until 1999. Schools that became ZEP in 1982 lost about 4 students per year between 1987 and 1999, and those became ZEP in 1990 lost about 6 students per year between 1995 and 1999. Again, the decrease in the number of students is essentially concentrated on 6th and 7th graders.[12]

The second and important result in Table 1 is that the reductions in *class size* associated to ZEP status were quite small: on average, a decrease of 0.2 students per class per year. On the other hand, class size in 1987 was already smaller in schools that became ZEP in 1990 (24.0 versus 24.4 for non-ZEP). These schools were thus probably already identified and benefiting from extra resources even before acquiring priority status. The analysis over the longer period 1987 to 1999 shows a continuation of this modest and slow decrease. Over the course of 10 years, the average class size decreased by 2 students for the ZEP-89 and by about 1.5 students for the ZEP-1982 and ZEP-90, while the average class size in non-ZEPs remained stable.

Our results are thus consistent with the Ministry's estimate that ZEP junior high schools in the late 90s had 2 students less per class than non-ZEP,[13] but at the same time they reveal a much less well-known fact: the underlying process was extremely slow and actually began before the granting of priority status. In any case, even the ten-year reduction in class size in the priority zones was at best modest, especially in light of the intended goal of improving educational conditions of students living in socially disadvantaged areas.

4.2. Size and composition of the teaching staff

Table 1 shows that the number of teachers in ZEP schools did not increase more than in other establishments, except for a small positive trend in the ZEP-90.[14] Since in the ZEP-

[11] The analysis of mobility, not shown here, was done through linear probability models of leaving a school, using the student panels to analyze an individual's probability of leaving his or her school for another one in the same educational administrative region (see BKP).

[12] The results for the period 1987–1999 are available from the authors upon request.

[13] See for 1997, Ministry of Education, note n° 98-15.

[14] Over the longer period 1987 to 1996, the number of teachers actually decreased by one per year for the ZEP-1982 and ZEP-1989, and remained

Table 2
Social composition of the schools and ZEP.

	Occupation of the Head of the Family:		Nationality: African, Asian	Schooling information: No lunch at the canteen of the school
	White-collar worker	Skilled blue-collar worker		
Establishment became ZEP in 1982	−0.0226 (0.0244)	0.0355 (0.0279)	0.0888** (0.0142)	0.0228 (0.0326)
Establishment became ZEP in 1989	−0.0154 (0.0344)	0.0596 (0.0394)	0.0600** (0.0201)	0.0526 (0.0460)
Establishment became ZEP in 1990	−0.0618** (0.0294)	0.0901** (0.0336)	0.0454** (0.0172)	0.0466 (0.0393)
R-square:	0.1279	0.1648	0.2252	0.2929

Sources: 1980 and 1989 panels. 28,713 observations. Regressions include 3200 establishment effects. The relevant school is the one where the student is in his or her 7th grade. Standard Errors in parentheses (*: significant at the 10% level, **: −5% level).

89 the number of students went down slightly more than elsewhere, the number of teachers per student rose slightly after 1989. The annual rate of increase was again small, which is consistent with the results on class size.

It is worth noting that the extra teaching hours attributed to ZEP schools would not necessarily have translated into reductions in class size. These additional man-hours (or woman-hours) can also be used to divide classes into subgroups for some disciplines, or to add remedial classes given in small groups. One way of capturing such uses is to compute the weekly number of hours per teacher and per student. This ratio increased when a school acquired priority status, by 0.011 h per week in the ZEP-89 and by 0.015 h in the ZEP-90. Thus, once again, the increase was very slow and modest—in percentage terms, 0.8% and 1.2% per year respectively. Since the reduction in class size was 0.8% per year, it thus accounted for most of the increase in hours per student, with only a much smaller fraction used in other ways.

While class size and hours show only very modest changes, the significant resources allocated to raising *teacher pay* in ZEPs could have led to an improvement in the qualifications of the professorial staff. In fact, our estimates reveal exactly the opposite. One standard measure of quality is experience: Table 1 shows that the fraction of young professors (less than 30 years old) in ZEP schools goes up slightly shortly after the status change (the increase appears in 1990 for the ZEP-89). An optimistic interpretation of this evolution would be that young teachers are more dynamic or better able to relate to the children, even though they have less professional experience. Unfortunately, our results on professional qualifications show that the fraction of teachers without tenure and holding lesser diplomas ("maîtres-auxiliaires", who do not have the regular teaching certificate) also increased slightly in schools that became ZEP in 1990.[15] It is worth noting that in France, schools do not hire their teachers. Job assignment are instead determined at the national level, through a system in which teachers express preferred choices and priority is roughly determined according to seniority (tenure length). Hence, more experienced teachers are able to choose the schools they want, whereas younger ones tend to end up in less "desirable" establishments.

The results established so far lead to two main conclusions. First, the extra resources allocated to ZEP schools in terms of additional teacher slots and extra hours were quite limited. Second, the more substantial bonuses and promotion incentives granted to ZEP teachers (independently of the performance of their students) did not help in stabilizing the teaching staff or improving its skill composition. In the "market" for teachers (internal to the Education civil service), the ZEP wage premium was not nearly sufficient to compensate for the adverse "hedonic" and signaling characteristics associated with teaching in such schools.

4.3. Social composition of schools

We now compare the social composition of ZEP establishments to that of non-ZEP ones, both before and after the former's change in status. The evolution of the social mix is of interest both *per se* and because of its possible impact on student performance through peer effects.[16]

The estimation is similar to the previous one, but now on the panels of individual student data. Only two years are compared: 1981, when students from the Panel 1980 are in their 7th grade and 1990, when students from the Panel 1989 are in that same grade. These students are the ones who will be used in Section 5 to estimate the ZEP effect.

The model is:

$$1_{characteristic}(i) = \delta_{i \in 80panel} + \delta_{j(i)} + \gamma_{82} \cdot 1_{j(i) \in zep82} \cdot 1_{t(i) \geq 82} + \gamma_{89} \cdot 1_{j(i) \in zep89} \cdot 1_{t(i) \geq 89} + \gamma_{90} \cdot 1_{j(i) \in zep90} \cdot 1_{t(i) \geq 90} + \varepsilon_i$$

where $1_{characteristic}(i)$ is a dummy variable for a student characteristic (for example, occupation of the head of the family = executive) of student *i; j(i)* and *t(i)* denote respectively the establishment and year where and when the student was in his or her 7th grade; $\delta_{j(i)}$ is an indicator for the establishment where the student was in his or her 7th grade and $\delta_{i \in 80panel}$ an indicator equal to 1 if the student

stable for the ZEP-90. As explained earlier, these results are not reported here but are available from the authors.

[15] Other measures of skills, such as the proportion of those with the highest teaching diploma ("agrégation"), remained virtually unchanged (results not reported here).

[16] In Section 5, we will estimate the impact of ZEP status on student achievement with a difference-in-differences approach. Thanks to the panel datasets, we will compare students being in a ZEP school to those being in a non-ZEP school, with school variables and school fixed effects controlling for stable differences between establishments. But if there is deterioration in the social composition of ZEP schools relative to the others, our school variables may not be sufficient to control for that and the estimated ZEP effect may underestimate the "pure" impact of the ZEP treatment (effect of the extra resources and educational projects).

belonged to the 1980 panel. Indicators $1_{j \in zep82}$, $1_{t \geq 82}$, etc., are defined in the same way as before.

Table 2 presents the results. It seems that the composition of the students did not change much in the ZEP schools, except regarding students' nationalities. In 1982, the ZEP schools already had a larger share of African and Asian students than the other schools. This share increased more than elsewhere between 1982 and 1990 (Table 2). But it is worth noting that according to several studies,[17] children from immigration have in France similar or better achievement than other children, all other factors being equal. Our results on the achievement regressions, in Section 5, lead to the same conclusions.

There is no strong evidence of deterioration of the social composition in ZEP schools. Concerning the parents' occupations, the only significant change is more skilled blue-collar workers and less white-collar workers in the schools that became ZEP in 1990. There is also no change in the fraction having lunch at the school cafeteria.[18] As mentioned earlier, in France children who are enrolled in the cafeteria plan typically come from more advantaged backgrounds than those who do not.

5. The impact of ZEP status on individual schooling achievement

Our results so far show that the financial support given to ZEP schools was far from negligible, but also that these funds were sprinkled across many establishments, without any apparent targeting towards the potentially most efficient inputs or towards those students most likely to benefit from these extra inputs. Thus the decrease in class size was small and progressive, the number of teachers hardly increased and their qualification remained at best unchanged.

The ZEP "treatment", however, is a potentially much more complex object than a simple change in financial resources or teaching hours. First, the official goal of the ZEP program was also to provide the means for schools to create *new educational projects* and connect more closely with local institutions such as municipalities. These projects were supposed to have a positive impact on the academic achievement of ZEP students. Second, the sizeable bonuses and career improvements offered to teachers in ZEP schools could have contributed to improving their motivation,[19] or allowed the Education Ministry to select teachers for these schools from a higher quality pool of applicants (in ways not reflected by seniority and tenure indicators). Third, on the negative side, an adverse signaling effect (stigmatization) could have discouraged effort by both professors and students, leading to deterioration in school performance. Finally, if teacher experience improves student attainment, the increase in the fraction of younger teachers could have lowered the educational achievement of some or all the students.

To assess the value of the ZEP program, one therefore needs to quantify the *overall* impact of granting priority status to a school on students' academic achievement. This is the paper's second main objective, to which we now turn.

5.1. Estimation strategy

It would clearly be inappropriate to simply regress individual student performance on personal characteristics plus a ZEP indicator, and thus compare mean outcomes between ZEP and non-ZEP students. Indeed, the ZEP variable certainly is endogenous, even given all our individual controls. For instance, priority status could have been preferentially granted to those establishments where schooling outcomes were the worse or deteriorating the most rapidly, or conversely to those among the "difficult" zones deemed the most likely to succeed. To deal with this problem, we use both difference-in-differences (implemented through establishment fixed effects) and instrumental variables.

The first method exploits the fact that, thanks to our two panels, we can compare students who went through the same grade in the same school, but with some attending before it became a ZEP and others nine years later, after it had acquired priority status. The idea is then to subtract from the deviation between ZEP and non-ZEP schools estimated on the 1989 panel the corresponding deviation estimated on the 1980 panel, but with the ZEP indicator replaced there by a "future ZEP" dummy, equal to 1 if a school was part of the ZEP "wave" of 1989 or 1990. This difference in differences approach controls for any unobserved factors affecting student performance in the priority zones (relative to non-ZEP ones) that already existed prior to the status change, and therefore yields an unbiased estimate of the reform's impact (assuming stability of the unobserved heterogeneity and distribution of errors.) In the linear model, this method is implemented both very simply and more generally by running a single regression that includes establishment-specific fixed effects, which control for unobserved stable heterogeneity across *all* schools.

Our second method for dealing with potential selection biases uses instrumental variables, described in subsection 3.3.

In all these cases, our estimates differentiate between the three "waves" of ZEPs, thus capturing a potential "duration effect": for instance, some establishments that were ZEPs in 1990 acquired that status in 1982, whereas others received it only in 1989.

5.2. Difference-in-differences estimates

In this section we estimate the effect of the "ZEP treatment" using the difference-in differences technique in a linear probability model with fixed effects. Thus, a student's

[17] See for instance Caille and Vallet (1995) and Gary-Bobo, Prieto, and Picard (2006).

[18] This information is also available in the FSE data. The same estimation as in 3.1 and 3.2 shows that the fraction of students having lunch at the school cafeteria decreased slightly in the ZEP schools after 1989 and 1990 compared to 1988 and to the other schools (see BKP). This decrease may have followed an increase between 1982 and 1988.

[19] Since these bonuses were not conditioned on student achievement or any other performance measure they could not have had any standard incentive effect. They could still, however, have enhanced teachers' "intrinsic motivation" by eliciting feelings of reciprocity or professional pride.

academic achievement is modeled as follows:

$$1_{achievmt}(i) = \alpha X_i + \delta_{i\in 80panel} + \delta_{j(i)} + \gamma_{82}\cdot 1_{j(i)\in zep82}\cdot 1_{t(i)\geq 82} + \gamma_{89}\cdot 1_{j(i)\in zep89}\cdot 1_{t(i)\geq 89} + \gamma_{90}\cdot 1_{j(i)\in zep90}\cdot 1_{t(i)\geq 90} + \varepsilon_i \quad (3)$$

where $1_{achievmt}(i)$ is an indicator for achievement (moving up to 8th grade, to 10th grade, getting at least one degree, success at the Baccalauréat) of student i; $j(i)$ and $t(i)$ denote respectively the establishment and year where and when the student was in his or her 7th grade; $\delta_{j(i)}$ is an indicator for the establishment where the student was in his or her 7th grade and $\delta_{i\in 80panel}$ an indicator equal to 1 if the student belonged to the 1980 panel. The vector X_i contains the student's individual and family characteristics as well as establishment variables, measured as the average of the individual variables over those students in the panel studying in the same school $j(i)$. This aggregation is done separately for each panel, so these establishment variables are time-varying. Finally, the indicators $1_{j\in zep82}$, $1_{t\geq 80}$, etc., are defined in the same way as before.

Eq. (3) makes clear how the coefficients γ_{82}, γ_{89} and γ_{90}, which capture the effects of a change to ZEP status, are identified by using our two panels simultaneously. For instance, for a given establishment that became ZEP in 1982 the indicator function $1_{j(i)\in zep82}\cdot 1_{t(i)\geq 82}$ goes from 0 for the students in the 1980 panel who attended that school to 1 for those in the 1989 panel who followed them there nine years later.

The regressions corresponding to reaching 8th grade are carried out over 17,279 students enrolled in 2099 establishments in the 1980 panel and 11,435 students enrolled in 3031 establishments in the 1989 panel.[20] The identification of the ZEP coefficients relies on the 1944 establishments present in both panels, with 93 of these becoming ZEP in 1982, 40 in 1989 and 62 in 1990. The reference establishment is that of the first 7th grade of each student. When estimating the other three equations – for obtaining a degree, moving up to 10th grade and success at the Baccalauréat – the sample consists of 16,816 students enrolled in 2051 establishments for the 1980 panel and 11,016 students enrolled in 3009 establishments for the 1989 panel. The identification of the ZEP coefficients now relies on 1891 establishments common to both panels, with 93 establishments becoming ZEP in 1982, 40 in 1989 and 61 in 1990. In these cases, the reference establishment is that of the last 7th grade of each student.[21]

[20] We excluded the observations from Corsica from our sample, in order to make the regressions more comparable to those run later with instrumental-variables, in which the political variable was not available for that region (due to the fact that a single "département" was later split into two). Leaving in Corsica has no effect on the results, however.

[21] The idea here was to minimize the time interval between the 7th grade ZEP/non-ZEP treatment and the 10th grade or Baccalauréat outcome. Using the first 7th grade instead makes no difference to the results, however.

Table 3
Linear model with establishment fixed effects: ZEP variable.

	Getting one degree		Moving up to 8th-grade	
	Coefficient	Stderr	Coefficient	Stderr
ZEP in 1982	−0.0428	0.0299	−0.0052	0.0293
ZEP in 1989	0.0068	0.0426	0.0339	0.0393
ZEP in 1990	−0.0030	0.0364	−0.0126	0.0364
R2	0.2077		0.2556	
Nb of obs	27831		28713	
	Moving up to 10th-grade		Success at baccalauréat	
	Coefficient	Stderr	Coefficient	Stderr
ZEP in 1982	−0.0046	0.0330	−0.0200	0.0338
ZEP in 1989	0.0561	0.0497	0.0212	0.0457
ZEP in 1990	−0.0171	0.0432	−0.0494	0.0443
R2	0.3272		0.3179	
Nb of obs	27831		27831	

Sources: 1980 and 1989 panels. Regressions include about 3200 establishment fixed effects, in addition to the individual characteristics and school variables (averages of individual characteristics by school on each panel). The relevant school is the one where the student is in his or her 7th grade. Standard Errors in parentheses (*: significant at the 10% level, **: −5% level).

Turning to the variables of central interest,[22] we see from Table 3 that the ZEP coefficients are never significantly different from 0, irrespective of the measure of achievement used. These non-significant ZEP effects show that the granting of "priority" status to their school did not help students, once pre-existing differences between establishments are controlled for. This is our second main finding.

5.3. Instrumental variables estimation

To address the endogeneity of the ZEP status, we also estimate the model using instrumental variables, based on the shares of the vote received by the various parties (or coalitions) in the first round of the 1981 and 1988 parliamentary elections. Several elements reveal that the granting of ZEP status was indeed influenced by political considerations, on top of educational ones. First, establishments in a designated geographical zone could opt out and, in the first years of the program, the Communist Party gave instructions to its mayors to refuse the ZEP status in their cities, as it saw it as stigmatizing. Second, and surprisingly, priority zones were initially concentrated in only a handful of regions, especially in Seine Maritime and Aquitaine, which are far from being particularly poor. By contrast, there were no priority zones in Marseilles until the "wave" of 1990, even though it is France third-largest city and includes some of its most disadvantaged areas. And even then, there were odd priorities: as many as 29% of the junior high schools located in the Nièvre "département" were granted ZEP status, as were 19% of those in the Ariège "département"—two rural regions with obviously much less need for this type of program. Analysts duly noted that the Nièvre was president Mitterand's elec-

[22] Results of individual and establishment-level variables are shown in BKP.

Table 4
Instrumentation of the ZEP indicators.

	Zep in 1989		Zep in 1990	
	Coef.	Sterr	Coef.	Sterr
Votes during parliamentary elections:				
Extreme-right	−0.0948**	0.0373	0.4238**	0.0432
RPR-UDF	−0.0213	0.0421	0.0944*	0.0487
Other right	0.1609**	0.0476	−0.1986**	0.0550
Other left	−0.3355**	0.0793	−1.1475**	0.0916
Communist party	−0.3632**	0.0458	−0.1434**	0.0529
Greens	0.0783	0.0977	0.9149**	0.1129
Extreme-left	0.1846**	0.0590	0.3613**	0.0682
F-stat for the significance of the instruments	25.24		63.97	
(*p*-value)	($p < 0.01$)		($p < 0.01$)	

Sources: 1980 and 1989 panels. 27,831 observations. Regressions include about 3200 establishment fixed effects, in addition to the individual characteristics and school variables (averages of individual characteristics by school on each panel). The political data are Cevipof data, for the years 1981 and 1988. The estimation is done with a linear probability model. The Fisher statistics is the significance test of the political variables. The relevant school is the one where the student is in his or her 7th grade. Standard Errors in parentheses (*: significant at the 10% level, **: −5% level).

toral stronghold, and Ariège that of Lionel Jospin, Minister of Education between 1988 and 1992. Even Lionel Jospin recognized that the attribution of the ZEP status was linked to political considerations: in a June 1998 speech at the "Assises nationales des ZEP" in Rouen, when he was now Prime Minister, he criticized the fact that 39% of junior high school students in Nièvre were in a ZEP school against only 19% of students in Seine-Saint-Denis, a very poor "département" where the riots started in November 2005.

Our instrumentation strategy relies on two hypotheses. First, political factors must affect the determination of where ZEP schools are located—as we just confirmed. Second, the political variable must be uncorrelated with the (differential) performance students in ZEP schools, conditional on all the other exogenous regressors. To understand why this is a plausible assumption, it is important to note that: (i) the political variable is measured at the level of a "département", which is a much larger entity than that of school districts (the level at which the ZEP/non-ZEP classification operates), where educational outcomes and political conditions could be quite correlated; (ii) the control variables include school fixed effects, which will absorb in particular any fixed differences in the population composition of a "department" that could affect both its political outcome and the (relative) performance of its most disadvantaged schools.

The first-stage regression corresponds to the linear probability model

$$1_{i\varepsilon zep89} = X_i'\beta + \gamma P_{j,t(i)} + \delta_{i\varepsilon 80panel} + \delta_j + \varepsilon_i \quad (4)$$

where $Pj,t(i)$ denotes the shares of the different parties in the parliamentary elections that took place in 1981 (resp. in 1988) if the student belonged to the 1980 panel (resp. the 1989 panel), the share of the Socialist Party being the omitted variable. The inclusion of the establishment fixed effects is again allowed by the fact (with the estimation performed on both panels of students) both the ZEP variables and the political instruments are time-varying.

Table 5
Linear model with instrumental variables.

	Getting one degree		Moving up to 8th-grade	
	Coefficient	Stderr	Coefficient	Stderr
ZEP in 1989	0.1622	0.5195	−0.0546	0.5177
ZEP in 1990	0.0106	0.3079	−0.1541	0.2915
R2	0.1001		0.1354	
Nb of obs	27831		28713	
$\chi 2$ over-identification test (*p*-value)	3.2728 ($p = 0.86$)		2.4089 ($p = 0.93$)	
	Moving up to 10th-grade		Success at baccalauréat	
	Coefficient	Stderr	Coefficient	Stderr
ZEP in 1989	0.0062	0.6199	0.3879	0.6293
ZEP in 1990	−0.0608	0.3674	−0.5339	0.3730
R2	0.2161		0.1798	
Nb of obs	27831		27831	
$\chi 2$ over-identification test (*p*-value)	2.3147 ($p = 0.94$)		5.9496 ($p = 0.55$)	

Sources: 1980 and 1989 panels. Regressions include about 3200 establishment fixed effects, in addition to the individual characteristics and school variables (averages of individual characteristics by school on each panel). The ZEP variable is instrumented by the school variables and the political variables. The relevant school is the one where the student is in his or her 7th grade. Standard Errors in parentheses (*: significant at the 10% level, **: −5% level).

The first-stage results are presented in Table 4. They show that the priority zones were located in "départements" in which the Extreme Left and the Other Right did better in 1988 than in 1981, relative to the Socialist Party, and the opposite for the Extreme Right and especially for the Communist Party and Other Left. By contrast, the schools that became ZEP in 1990 tended to be located in regions where either extreme wing (right or left), as well as the Green Party, were stronger in 1988 than in 1981 (relative to the Socialists) and where the Other Right, Other Left and the Communist Party were relatively weaker. The F-statistics for the significance tests of the instrumental variables, given in Table 4, are high enough to confirm our intuition that these political instruments are of sufficient quality.

The results of the second-stage (instrumented) regressions are presented in Table 5. They are virtually identical to those obtained using establishment fixed effects. In particular, the ZEP impact on students' academic achievement is never significantly different from zero, no matter what measure of achievement is used. The χ^2 over-identification tests, presented for each of the regressions, support the validity of our instrumentation strategy: they do not reject the null hypothesis of orthogonality of the IV residuals to the instruments.

6. Conclusion

Three main results can be derived from our analysis of the impact of the ZEP program that was put into place in French junior high schools in the 80s and early 90s.

First, the overall resources involved were relatively important but were allocated to a large fraction of the school population: approximately 10% of all students in primary and junior high schools belonged to a ZEP, and in 1990, the extra resources amounted to an extra 5% in expenditures per pupil. During our sample period, about one half of these resources were used for teacher bonuses and the other half for extra hours of teaching. The resulting decrease in class size was quite small and very progressive.

Second, our results suggest that the signaling effect of the ZEP status was negative for teachers. Despite the bonuses offered and additional career incentives, the teaching staff saw no improvement in qualifications or turnover and actually became less experienced over time. Moreover, the ZEP status led to a decrease in the number of students enrolled. On the other hand, there is no clear evidence of deterioration in the socio-economic background of students, at least between 1982 and 1990.

Finally, and most importantly, the ZEP "treatment" had no discernable effect on any of our four measures of students' academic achievement: obtaining at least one degree by the end of schooling, reaching the 8th or 10th grade, and success at the Baccalauréat. Perhaps most notable is the absence of impact at the lower end of the achievement distribution (exiting school without any degree), which was the intended target of the policy. These results mean that the combination of the increase in measured teaching inputs and the more "qualitative" dimensions of the ZEP program (which was meant to spur new educational projects, teaching methods, etc.) had no effect on academic achievement. This outcome could in part be the result of an adverse effect due to the small deterioration of the teaching staff. But it seems quite consistent with the small decrease in class size and with the large variance in the nature of ZEP-educational projects, which has led frequent concerns about their average effectiveness. Some of these projects may have been effective, but on the other hand it is known that some ZEP schools did not manage to develop any new educational project at all. Moreover, because of the lack of overall coherence in the ZEP program, there was no clear mechanism by which successful projects could spread to other schools (even assuming that successful projects were identified successful in spite of the lack of systematic evaluation).

We should, on the other hand, emphasize that the negative results found here for junior high schools cannot be generalized without additional studies to other aspects of the overall ZEP policy, which in particular also covered primary schools. Thus, recent studies (Bressoux, Kramarz, & Prost ,2007; Piketty, 2004) find a strong effect of class size on 3rd grade test scores, especially for pupils from disadvantaged backgrounds. The fact that these children are much younger than those we study is in line with the general consensus in the economics of education literature that interventions are most effective the earlier they occur (e.g., Heckman, 2000). Targeting the decrease in class size in primary schools could thus lead to much more of an improvement in pupil achievement. Had the same overall budget been more carefully targeted, the Ministry of Education could have, for instance, allocated an extra expenditure of 25% to 2% of the students. Even without altering the teacher share (which would have required going up against very powerful unions), this would have allowed a much more significant decrease in class size, of 6 students on average. The diffuse sprinkling that our study brings to light may have been related to the political difficulties of giving up the deeply ingrained idea of "equality of treatment", as well as to the pressures from most local constituencies to receive their share of the national budget. It is interesting to note that in the United States and the United Kingdom, similar compensatory education programs cover an even much larger share of the students.

Our results also show, however, the difficulties that similar interventions targeted at "poor" schools are likely to face, in particular when the budgets involved are not clearly known (in this instance, neither by the public nor, more surprisingly, by the education authorities themselves) and when powerful professional and political interests come into play. Future programs that target aid to schools or students in under-privileged zones should incorporate these findings. They should also be continuously evaluated, in particular because of the potential general-equilibrium effects that cannot be monitored in controlled experiments.[23]

[23] For examples of how general-equilibrium effects of education policy interventions can be very different from partial equilibrium ones, due in particular to the endogenous sorting of students (across schools) or households (across neighborhoods), see, e.g., Benabou (1996) on the theoretical side and Hsieh and Urquiola (2006) on the empirical side.

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