**Preschool Enrolment, Mothers' Participation in the Labour Market,**

**and Children's Subsequent Outcomes**

Dominique Goux (DARES) et Eric Maurin (PSE)

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Abstract:

French children start public school either the year they turn two or the year they turn three. We provide the first comprehensive evaluation of this unique early schooling policy. Using discontinuities in enrolment rates generated by eligibility rules, we show that pre-elementary school availability has a significant employment effect on lone mothers, but no effect on two-parent families. Also, we show that substituting one year of pre-elementary school for one year of alternative modes of childcare has no significant effect on two year olds' subsequent educational outcomes. Overall, pre-elementary school is shown to be more cost-effective than existing alternative modes of childcare for two-year olds in France.

Given many recent policy proposals to extend pre-school programs across developed countries, it is important to understand what the consequences of these policies will be. The first expected outcome of such policies is to enhance child development and improve their subsequent adaptation to elementary school. Another expected outcome is to provide families with free child care and to make it easier for parents with young children to participate in the labour market. However, despite the importance and cost of such policies, very little is known about their effectiveness in achieving their goals.

Within this context, France represents a very interesting case since it introduced universal pre-elementary school for *three*-year olds in the early eighties. A significant fraction of French children start public school even earlier, at the age of two[1]. French pre-elementary schools are genuine part of the primary school system. The curriculum is defined at the national level and teachers are certified primary school teachers. The program consists of 28 hours per week during the same academic year as elementary schools. The annual cost per pupil is about 4,700 euros.

The French system provides a unique opportunity to study the causal effects of pre-school access on mothers' participation in the labour market and on their children's subsequent outcomes. To identify the effect on labour market participation, we make use of the fact that pre-elementary school eligibility depends on year of birth and not on exact date of birth. This system generates very significant discontinuities in school enrolment probability between children born in December and children born at the beginning of January of the following year.

[1] According to European Commission (1995), France and Belgium are the only countries where children can start school as early.

The first basic question is whether we observe similar discontinuities in the variation of mothers' labour market participation with respect to their children's exact date of birth, which would suggest a causal relationship between access to preschool and mother's labor force participation. Importantly, we find no discontinuities for two-parent families, but significant ones for single-mothers. Further investigations reveal that this effect varies significantly across sub-populations of single mothers. In particular, the estimated effect of preschool availability is much larger in regions where alternative child care arrangements are the least developed and where preschool enrolment capacity is the highest. In addition, the estimated effect is larger when the child is three years old than when she is only two years old, which is consistent with the fact that non-working mothers lose their eligibility to family benefits when their child turns three.

All French pupils start elementary school in September of the year they turn six. As a consequence, children who benefit from an early start in pre-elementary school actually spend *four* years in pre-elementary school whereas the normal starters spend only *three* years. The second objective of this paper is to analyse the causal effect of this additional year in pre-elementary school on children's subsequent outcomes, that is, the effect of substituting one year of early school for one year of alternative child care on subsequent outcomes. To identify this effect, we build on a different feature of early admission regulations, which is the priority given (within each birth cohort) to the oldest children by the start of the academic year. In regions with relatively low early enrolment capacity, we show that this regulation generates very significant difference in early enrolment probability between children born before the summer period and children born either during or after the summer period. In regions with relatively high enrolment capacity, the same regulation generates very significant difference in enrolment probability between children born either before or during the summer period and children born after the summer period. Interestingly enough, we find that these variations in early enrolment probability across periods of birth and regions do not coincide with any significant variations in children's subsequent outcomes. In particular, we do not find any variation across regions in the difference across periods of birth in test scores in primary school nor in the probability of early drop out from school. These findings suggest that the substitution of one year in pre-elementary school for one year at home or in alternative child care institutions has no significant effect on children's subsequent educational outcomes.

The first section of the paper provides an overlook of the related literature. The institutional context and the data used are presented in the second section. The conceptual framework is presented in section III and sections IV and V show the empirical results. The last section uses our findings to provide a simple cost-benefit analysis of the French early schooling policy.

## I. Related Literature

Several recent studies exploit variations in the presence of public preschools across geographic areas over time in order to identify the effect of these programs on maternal employment (see Cascio, 2006, Baker, Gruber and Miligan, 2005, Schlosser, 2005, Berlinski and Galinani 2005). For example, Cascio (2006) argues that the introduction of kindergartens for children aged five in the US public schools during the 1960s and 1970s raised very significantly the labour supply of single mothers with eligible children, but had no effect on other mothers. In a related paper, Baker et al. (2005) show that the extension of full-time kindergartens to all five year olds (and the provision of childcare at a price of 5 dollars per day to all children aged 4 or less) in the Canadian province of Quebec in the late nineties coincides with an increase in maternal labor supply and a decline in children's outcomes in this province (compared with the rest of Canada). One issue with this evaluation is that there were several other changes to the benefits paid to families, both in the province of Quebec and the rest of Canada in the late 1990s (as well as other specific reforms in other provinces).

Berlinski and Galiani (2007) analyze the impact of a program of construction of pre-elementary schools in Argentina. Using the difference in the timing of construction across regions, they find a significant effect of the program on both preschool enrolment en maternal employment. According to Berlinski, Galiani and Gertler (2006), this program also had a positive effect on pupils' outcomes. Schlosser (2005) evaluates the impact of a reform conducted in 1999 in Israel which increased the availability of free preschool for children aged 3 and 4 in the poorest towns of the country. Building on the difference in the timing of the reform across municipalities, she finds that free preschool availability increased both preschool attendance and maternal labour supply in Arab municipalities.

In a very different contribution, Gelbach (2002) shows that the quarter of birth of five year olds affects maternal employment and children's preschool attendance in a parallel way. Using quarter of birth as an instrument, his analysis suggests that preschool availability has a significant effect on maternal employment. It is not clear, however, whether the effect of quarter of birth on mothers' behaviour is due to free child care availability only. Children born later in the year are also younger at each point of time, which may, as such, be an explanation for the lower participation of their mother in the labour market.

Finally, the recent paper of Fitzpatrick (2008) examines how universal preschool availability for 4-year olds affects mothers' labour supply in Georgia and Oklahoma. Comparing children born just before and just after the eligibility cut-off (i.e., September 1st), she does not find any robust impact on mothers' behaviour, except some minor changes in rural areas.

The second objective of this paper is to evaluate the effect of substituting time spent in pre-elementary school for time spent in alternative childcare arrangements on children's subsequent outcomes. This contribution is related to the long standing literature in developmental psychology and economics that has attempted to evaluate the effect of time spent in school holding age at test constant. One important strand of this literature compares outcomes of children born before the school entry cut-off date to those born after and interprets the difference in subsequent outcomes at different points in time as the effect one additional year spent in the school system (see e.g. Cahan and Cohen, 1989). One issue with this approach is that it relies on assumptions on the effect of age on children's outcomes that are difficult to motivate. A recent contribution of Cascio and Lewis (2006) overcomes this problem by using variation in school entry cut-off dates across states in the US. They find a significant effect of time spent in school on military test scores for minority groups. In a related paper, Crawford, Dearden and Meghir (2007) use the fact that school starting age varies across educational district in Britain to separately identify the entry age from the age at test effect on pupils' outcomes. A limitation with these strategies is that pupils born just below the cut-off tend to be the youngest of their year group whereas pupils born just above tend to be the oldest of the following year group. Hence these evaluations capture a 'relative age' effect on top of the time in school effect.

In another recent contribution, Dhuey (2007) uses the variation across US states in the timing of adoption of publicly funded kindergarten (over the 1960-1980 period) to estimate the effect of enrolment on later outcomes. She finds significant effects on individuals with a low socioeconomic background. In contrast, Fitzpatrick (2007) finds that the introduction of universal pre-kindergarten programs in the state of Georgia had little effect on students' subsequent performance in primary school. The effects are concentrated among low-income students, however. Leuven, Lindahl and Oosterbeek (2006) uses the specific timing of school holidays in Dutch primary schools to provide estimate of the pure effect of time in school on 4 years olds'outcomes, holding age constant. They find positive effect on disadvantaged pupils only.

## II. French Institutional Context and Data

Before starting pre-elementary school, a minority only of French families have access to publicly-funded childcare (see e.g. Blanpain, 2002). According to a survey conducted in 2002 by the French Ministry of Social Affairs, only about 12% of one-year olds attend a public childcare centre (called *crèche*) whereas only about 22% benefit from a registered child-minder (called *assistante maternelle agréée*). The demand for these highly subsidized childcare options exceeds by far the supply and the vast majority of families have either to stay at home with their children or to find an informal solution[2]. Within this context, the availability of free pre-elementary school increases very significantly the proportion of families that can benefit from subsidized childcare and our first basic research question is to identify whether it affects maternal labour supply. Our empirical strategy makes use of the specific admission rules of the French pre-elementary school system and this section begins by describing these rules.

### II.1 . Eligibility rules

French academic year begins in September and children start pre-elementary school either in September of the year they turn three (normal start) or in September of the year they turn two (early start). Early starters (about one third of a birth cohort in the late nineties) spend four years in pre-elementary school from year 1 (called *Très Petite Section*) to year 4 (called *Grande Section*). The other children spend only three years in pre-elementary school from

[2] A very small minority only (about 2%) can afford to have an employee at home to care about the children.

year 2 (*Petite Section*) to year 4. All children start elementary school in September of the year of their sixth birthday[3].

Within this system, the school enrolment rate observed in March of any calendar year *t* is close to zero for children born in *t-2*. They were too young by the start of the current academic year (September of year *t-1*), even for an early enrolment. In contrast, the enrolment rate is close to 1 for children born in *t-4*. They have all benefited either from a normal start by the beginning of the current academic year or from an early start by the start of the previous academic year. Lastly, the enrolment rate lies somewhere between 0 and 1 for children born in *t-3*. A fraction only has benefited from an early start at the beginning of the current academic year. Overall, at the beginning of any calendar year, eligibility rules generate two potential discontinuities in school enrolment rates, one between children born in late *t-4* and children born in early *t-3* and one between children born in late *t-3* and children born in early *t-2.* The basic questions asked in this paper will be whether these discontinuities are actually perceptible in our dataset and whether they coincide with discontinuities in the variation of maternal participation in the labour market. Small variations in date of birth generate significant variation in the availability of free child care and the issue is whether it affects labour supply.

Another important feature of the French system is that it generates significant inequalities in early enrolment across regions. There are about 18,000 pre-elementary schools in France and the enrolment capacities of these schools (and the number of teachers allocated to them by the administration) are designed in such a way that any child can benefit from a normal start in the pre-elementary school of her district of residence. In practise, the capacity of most schools exceeds the number of 'normal' preschool children living in the district. It is the basic reason

[3]In France, pre-elementary schools and elementary schools belong to the same public system of *primary* education. The curricula of both pre-elementary and elementary schools are defined at the national level by the same administration. Teachers ("professeurs des écoles") are the same in both types of schools. Any given *professeur des école* can teach either in pre-elementary or in elementary schools and move from one type of schools to the other.

why school heads can always accept a significant number of early starters at the beginning of each academic year. But the proportion of such early starters varies a lot across districts and regions. Specifically, it is relatively small in regions where the overall enrolment capacity of schools is weak relative to the number of preschool children (for example in the *Alsace* region) whereas the proportion of early starters is relatively high in regions where the capacity of schools is large compared to the number of preschool children (*Bretagne* region is an example). Lastly, official regulations ask to give priority to the oldest children by the start of the academic year, i.e., children born earlier in the year[4]. As discussed below, this rule generates (within each region) highly discontinuous variation in early school attendance across children born at different periods of the year and provides a way to identify the effect of early schooling on subsequent outcomes.

### II.2 Data

Our analysis of mothers' labour market participation makes use of the general census of the population, conducted in March 1999. It provides us with detailed information on the labour market and family situation of the total population of French mothers as well as on the date of birth, number and sex of their children. In March 1999, children born in 1997 are still too young to have benefited from an early enrolment at school, whereas a fraction only of those born in 1996 have benefited from an early start. To analyse the enrolment and participation discontinuities generated by French preschool regulations, we will mostly focus on the population of mothers whose youngest child was born either in December 1995-January 1996 or in December 1996-January 1997.

[4] See *Circulaire* n°91-124, June 1991 and *Circulaire* n°92-216, July 1992.

Our analysis of children's subsequent outcomes uses the results at the national evaluation of French pupils conducted at the beginning of 3rd grade for a representative sample[5] of pupils born between 1991 and 1994 (*N=8797*). The available dataset provides information on the year of birth, the month of birth and the region of residence of pupils as well as the results at tests in French and in Math. Importantly, we also know whether the child benefited from an early start at school (i.e. at the age of two). The last section of the paper will also make use of the very large samples of 17-year, 18-year and 19-year olds that can be constructed from the 1999 census. We have information on exact date of birth, region of residence and school attendance. This dataset makes it possible to measure the variation across date of birth in the difference across regions in the rate of early dropout from school (school is compulsory until the age of 16).

## III. Preschool Availability and Mothers' Employment : a Conceptual Framework

This section develops a very simple analytical framework that makes it possible to explore why the effect of pre-elementary school availability on mothers' labour market participation is potentially ambiguous and may be very different across subgroups of mothers. Before moving on to the empirical analysis, it is important to define as precisely as possible the effect that we want to identify.

To begin with, let us consider a standard labour supply model where the mother maximizes the value of a utility function $U=U(C,M)$ where $C$ represents the consumption of the household and where $M$ is the number of hours of non-paid activities of the mother (her 'leisure'). For the sake of simplicity, we assume that $M$ can written $M=G+N$ where $G$ is the

[5] The available dataset provides the results of a sample representative of the pupils who take their exam between 1998 and 2003 (N=*14699*). These pupils were born between 1990 and 1995. For pupils born in 1990, the dataset does not provide the results of those who are one year ahead. Symmetrically, for pupils born in 1995, we do not have the results of pupils who have been held back a grade. Overall, we have a representative sample of results for cohorts born between 1991 and 1994 only (*N=8797*).

number of hours of child-care provided by the mother and *N* is the number of hours of other 'non-paid' activities. If *L* denotes the number of hours of paid work of the mother, the time budget of the mother can be written (after normalization),

$$L+M=1,$$

whereas the budget constraint is,

$$C=wL-qP+R,$$

where *P* is the number of hours of non family child-care, *q* the cost per hour of *P* and *R* the other family income and *w* the potential hourly wage. Finally, the time budget of the child is,

$$G+P+E=1,$$

where *E* is the number of hours of free child care provided by public pre-elementary school[6]. Note that -in this first model- pure 'leisure' (*N*) does not provide more utility than family childcare (*G*), but implies additional child care costs (*q*). Hence, pure leisure is never optimal for the mother and the hours of non-paid activities are all spent with the child (i.e, $N=0$).

Within this context, let us first consider the case where pre-elementary school is not available ($E=0$). In such a case, every hour of paid work requires an hour of non-family child-care and the net hourly wage is $w-q$. As a consequence, denoting $t(R)$ the marginal rate of substitution of an hour of paid-work for an hour of non-paid[7] (i.e., the value of an hour of non-paid activity when alternative income is *R*), a mother participates in the labour market if and only if $w-q$ is greater than $t(R)$. In contrast, when free pre-elementary school is available ($E=1$), non-family child-care is not required anymore and the net hourly wage is *w*. A mother participates in the labour market if and only if *w* is greater than $t(R)$. Hence, in this simple setting, pre-elementary school availability is simply equivalent to an increase $+q$ in the net hourly wage of the mother. It generates an unambiguous increase in maternal propensity to

[6] In this simple model, it is always optimal for the mother to use all the child-care provided by public school.

[7] We have $t(R)=U'_2(R,1)/ U'_1(R,1)$. The marginal rate of substitution increases with *R*, but decreases with the elasticity of substitution of non-market activities for market ones (which characterizes *U*). If the elasticity is very low, mothers participate in the labour market regardless of whether preschool is available or not and preschool availability has no effect on participation.

participate in the labour market. Any mother whose potential hourly wage *w* lies between *t(R)* and *t(R)+q* increases her participation in the labour market when *E* moves from 0 to 1. Note that this simple model predicts a stronger impact of free school availability in regions where it is more difficult to find alternative public child care arrangement (i.e., in regions where *q* is higher). Also it predicts a different effect on mothers in single parent families (*R* and *t(R)* relatively low) and on mothers in two-parent families (*R* and *t(R)* relatively high).

One issue with this basic model is that hours spent in non-paid activities *M* are all assumed to have the same impact on a mother's well-being, regardless of whether she has to provide child care or not. Put differently, child care provided by mothers is equivalent to pure 'leisure'. The predictions become somewhat different when this assumption is relaxed. For example, assume that mothers' utility depends on *N* only, i.e. *U=U(C;N)*. In such a case, the hours of child-care provided by the mother (*G*) are not equivalent to 'leisure' anymore, but to unpaid work, which increases a lot the incentive to work when preschool is not available. As a consequence, it reduces a lot the likelihood of a positive effect of preschool availability on participation in the labour market.

The real world is plausibly somewhere in between a model where child care provided by mothers is equivalent to pure 'leisure' and a model where it is equivalent to pure 'work'. Under this assumption, the effect pre-elementary school availability on maternal labour supply is plausibly a combination of a positive price effect (net wage increases) and a negative income effect (the direct or indirect costs of child care decreases and, consequently, net disposable income increases). The only simple prediction is that the price effect is more likely to dominate when the other family income (*R*) is low and leisure has little value for the mother.

Another issue with the basic labour supply model discussed above is that it does not take family benefits into account. In 1999, French mothers who do not participate in the labour market are eligible to a benefit of about 530 euros (more than half the minimum wage), provided that their youngest child is two years old or younger. Hence, for any eligible mothers, the participation decision is not simply a matter of comparing her net hourly wage with the value of an additional hour of non-paid activities, it is also a matter of whether the family benefits alone do not provide her family with a better standard of living than the one that can be reached through labour market participation and paid-work. If family benefits are relatively high compared to potential labour earnings, the availability of public school may have no effect on a mother's labour supply even when it increases net hourly wage above the value of an additional hour of non-paid activity. Overall, in the French context, the effect of public school availability on mothers' participation in the labour market is plausibly more significant when children are three years old than when they are only two years old and when mothers who chose to stay at home are still eligible to benefits. For two-year olds, we may also speculate that some parents do not believe that school is a good option.

## IV. Empirical Analysis

This section begins by providing some graphical evidence on the relationships between exact date of birth, preschool attendance and maternal participation in the labour market, as measured in March 1999 in the general census of the population. We focus on mothers whose youngest child was born during either the month before or the month after the cut-off entry date (i.e., 1996, January, 1$^{st}$). Also, we provide a separate analysis for two-year olds and three-year olds as well as for mothers in two-parent families and single-parent families. Single-parent families are faced with financial and time constraints that are much more severe than those of two-parent families and are likely to be much more directly affected by free public school availability.

To begin with, Figures 1a and 1b show the relationship between exact date of birth and pre-elementary school attendance in March 1999 for mothers whose youngest children were born between December 1995 and January 1996. These children are three years old by the time of the census. Figure 1a focuses on mothers in two-parent families and Figure 1b on single-parent families. Comfortingly, the graphs reveal a very significant drop in pre-elementary school attendance rate between late December 1995 and early January 1996. This pattern is consistent with the eligibility rules where pupils can start either in September of the year of their third birthday (normal start) or in September of the year of their second birthday (early start). The vast majority of children born in late 1995 have benefited from either a normal start (in September 1998) or an early start (in September of 1997) whereas a fraction only of those born in early 1996 have benefited from an early start (in September 1998).

Figures 2a and 2b focus on the same samples of mothers and show the relationship between children's exact dates of birth and mothers' labour market participation. Interestingly, Figure 2a does not show any discontinuity between December 1995 and January 1996 for two-parent families. In contrast, Figure 2b reveals a clear discontinuity for single mothers (about 4 points). As discussed above, pre-elementary school availability increases potential net wages, but the effect on labor market participation is perceptible for lone mothers only.

Figures 3a and 3b show the relationships between exact date of birth and pre-elementary school attendance in March 1999 for children born between December 1996 and January 1997. These children are two years old by the time of the census. Unsurprisingly, the graphs show a drop in pre-elementary school attendance between late December 1996 and early January 1997 which is almost as significant as the drop observed in Figures 1a and 1b. The attendance rate is close zero for children born in early 1997 whereas it is above 15% for children born in late 1996. Children born in early 1997 were too young in September 1998, even for an early start, whereas a significant fraction of those born in 1998 have actually benefited from an early start.

Figures 4a and 4b focus on the same samples of mothers and show the relationships between children's exact dates of birth and mothers'labour market participation. Figure 4a does not show any discontinuity for two-parent families, whereas Figure 4b reveals a discontinuity for single mothers (about 2 points). Hence, the pattern is similar to Figures 2a-2b, even though the single-mother discontinuity is less clear-cut. As discussed above, the smaller effect for mothers of two-year olds is consistent with the fact that those who do not work are still eligible to family benefits.

Overall, the graphs suggest that the same discontinuities exist in the relationships between single-mothers labor market participation and their children's dates of birth as in the relationships between early school enrolment and children's dates of birth. The next section analyses the conditions under which this may be interpreted as reflecting a causal effect of early school availability on labour market participation.

### IV.2. Identifying assumptions

This section provides the assumptions under which the discontinuity in the relation between children's date of birth, enrolment at school and mothers' participation in the labour market can be used to identify the causal effect of pre-elementary school availability on maternal labour supply.

For child $i$, we denote $D_i$ the exact date of birth and $S_i$ a dummy indicating actual school enrolment. Also, following Imbens and Angrist (1994) and Hahn, Todd and Van der Klaauw (2001), we denote $S_i(d)$ the potential school enrolment status of child $i$ given any date of birth $d$. We assume that $S_i(d)$ is independent of $D_i$ and non-increasing in $d$ for each $i$. Lastly we define $L_{i1}$ the potential labour market participation of $i$'s mother if $i$ has already started school and $L_{i0}$ the same potential outcome if $i$ has not yet started school.

We want to evaluate the distribution of the causal effect $L_{i1}$-$L_{i0}$ of school enrolment on mothers' participation in labour market and the fundamental issue is that we never observe simultaneously $L_{i1}$ and $L_{i0}$. Our identifying strategy exploits the discontinuities in the probability of enrolment observed at $d_c$=January 1st 1996 (or at $d_c$=January 1st 1997).

To be more specific, we have the usual relation between the actual labour market participation of $i$'s mother (denoted $L_i$) and her potential outcomes $L_{i0}$ and $L_{i1}$,

(1) $$L_i = (1-S_i)L_{i0} + S_i\, L_{i1}=L_{i0} +S_i\, (L_{i1}-L_{i0}),$$

which can be rewritten,

(2) $$L_i = L_i(d_c) + (S_i-S_i(d_c))\, (L_{i1}-L_{i0}),$$

where $L_i(d_c)= (1-S_i(d_c))L_{i0} + S_i(d_c)L_{i1}$ represents the participation of $i$'s mother that would be observed if the school enrolment of her child was kept the same as at the cut-off point $d_c$. Using this decomposition, the actual participation rate of mothers whose children were born at $d$ can be written for any $d$,

(3) $$\mathrm{E}(L_i/D_i= d) = \mathrm{E}(L_i(d_c)/D_i= d)+ \mathrm{E}[(S_i(d)-S_i(d_c))(L_{i1}-L_{i0})/\, D_i= d],$$

where $\mathrm{E}(L_i(d_c)/D_i= d)$ represents the participation rate that would be observed if enrolment status were kept the same as at $d_c$ whereas $\mathrm{E}[(S_i(d)-S_i(d_c))(L_{i1}-L_{i0})\; D_i= d]$ captures the pure effect of changes in enrolment status between $d$ and $d_c$.

Interestingly enough, Equation (3) shows that, for any $d< d_c$, the variation in actual participation rates between $d$ and $d_c$ (i.e., $\mathrm{E}(L_i/D_i=d)$-$\mathrm{E}(L_i/D_i=d_c)$ can be decomposed into [a] the variation that would be observed if school enrolment was held constant (i.e., $\mathrm{E}(L_i(d_c)/D_i= d)$-$\mathrm{E}(L_i(d_c)/D_i= d_c)$ and [b] the variation due to changes in enrolment status only (i.e.,$\mathrm{E}((S_i(d)-S_i(d_c))(L_{i1}-L_{i0})/D_i=d)$). Hence, the variation in the actual participation rates in the neighbourhood of $d_c$ identifies the pure effect of changes in enrolment status if and only if the first term of the decomposition tends to zero when $d$ tends to $d_c$, that is if and only if the potential participation rate $E(L_i(d_c)/D_i=d)$ is continuous at $d_c$. Under this identifying assumption, the variation in participation rate at $d_c$ can be written,

(4) $\lim_{d\downarrow dc}$ [E($L_i/D_i= d$)- E($L_i/D_i=d_c$)]= $\lim_{d\downarrow dc}$ [E(($S_i(d)$-$S_i(d_c)$)($L_{i1}$-$L_{i0}$)/$D_i=d$)].

Under the maintained assumption that $S_i(d)$ is non increasing and independent from $D_i$, the right hand side of Equation (4) can be rewritten,

$$\lim_{d\downarrow dc} [E(L_{i1}-L_{i0}/S_i(d)-S_i(d_c)=1;D_i =d)] \times \lim_{d\downarrow dc} [E(S_i /D_i= d)-E(S_i /D_i= d_c)].$$

Hence, denoting $\Delta L_c=\lim_{d\downarrow dc}[E(L_i/D_i=d)-E(L_i/D_i= d_c)]$ the actual discontinuity in participation rate observed at $d_c$ and $\Delta S_c=\lim_{d\downarrow dc}[E(S_i/D_i=d)-E(S_i/D_i= d_c)]$ the corresponding discontinuity in school enrolment rate, we have $\Delta S_c>0$ and the ratio $\gamma=\Delta L_c/\Delta S_c$ identifies the average causal effect of school enrolment on families whose enrolment status change at $d_c$ (i.e., $\lim_{d\downarrow dc}[E(L_{i1}-L_{i0}/S_i(d)-S_i(d_c)=1;D_i =d)]$).

In substance, our identifying assumption means that the participation rates would be continuous at $d_c$ if enrolment status did not change at $d_c$ (so that any discontinuous shift in actual participation rates isolate the causal effect of change in school enrolment status). Hahn, Todd and Van der Klaauw (2001) provide a simple setting where this assumption is satisfied. Their model amounts assuming that the potential outcome $E(L_{i0}/D_i=d)$ is continuous at $d_c$ and that the causal effect $L_{i1}$-$L_{i0}$ is independent from date of birth $D_i$. In such a case, the potential participation rate $E(L_i(d)/D_i=d)$ depends on date on birth only through $E(L_{i0}/D_i=\mathrm{d})$ and is continuous at $d_c$ by construction. There exist other possible settings. For example, it is possible to make use of the fact that $E(L_i(d_c)/D_i= d)$ can be decomposed into,

(5) $E(L_{i0}/D_i=\mathrm{d},S_i(d_c)=0)\times \Pr(S_i=0/D_i= d_c)+ E(L_{i1}/D_i=d,S_i(d_c)=1)\times \Pr(S_i=1/D_i= d_c)$,

which shows that a sufficient condition for $E(L_i(d_c)/D_i= d)$ to be continuous at $d_c$ is the continuity at $d_c$ of both $E(L_{i0}/D_i=d,S_i(d_c)=0)$ and $E(L_{i1}/D_i=d,S_i(d_c)=1)$. Put differently, identification only requires the continuity of the potential participation rates for the group who has already started school at $d_c$ as well as for the group who has not yet started school at this date.

Note that these continuity assumptions would plausibly be violated if mothers were able to manipulate exact date of birth in order to modify the school entry age of their children. Also our continuity assumptions would be problematic if the subsequent fertility of a mother (as well as her probability of becoming a single-parent mother) was affected by her child's exact date of birth (i.e., by whether the birth took place just before or just after the school enrolment cut-off). One simple way to test these assumptions is to test whether there are discontinuities in the distribution of youngest children's date of birth at the cut-off dates. We have checked that it is not the case. Figure A1 and A2 in the Appendix show that there are no discontinuous variations in the number of single-parent families across youngest children's date of birth. We have also checked that when we focus on families with one child born between Dec. 1995 and Jan. 1996 (or Dec. 1996 and Jan. 1997), the probability of being a single-parent family and the probability of having a younger child were both continuous at the cut-off dates (not reported). These results clearly rule out the assumption that mothers' fertility (or family composition) is affected by whether the exact date of birth is above or below the cut-off date.

With respect to estimation, we need to estimate the ratio of two differences, $\gamma=\Delta L_c/\Delta S_c$. As discussed by Hahn et al. (2001) or Imbens and Lemieux (2008), the simplest way is to use local linear regressions. It amounts selecting the observations within a distance $h$ on either side of $d_c$ and estimating the effect of $S_i$ on $L_i$ by two-stage least squares method with the discontinuity dummy $1[D\geq d_c]$ as the excluded instrument and with a spline function of $D$ (with a knot at $d_c$) as a control variable. With respect to the bandwith size $h$, we will use

$h$=one month as a minimal benchmark and test the robustness of our results to alternative (larger) bandwith choices.

### IV.3 Basic Regression Results

To begin with, let us consider mothers whose youngest child was born between early December 1995 and late January 1996. All these children are three years old in March 1999 (census date) but those born in late 1995 are in the year of their fourth birthday whereas those born in early 1996 are still in the year of their third birthday. As a consequence, all those born in late 1995 have already started school whereas a significant fraction of those born in early 1996 have still not started. Table 1 shows the result of regressing children's enrolment at school (column 1) and mothers' participation in the labour market (column 2) on a dummy indicating whether the children were born in 1996 (i.e., $1[D\geq d_c]$), where $d_c$=January 1$^{st}$, 1996) and a spline function of exact date of birth with a knot at $D= d_c$ (i.e., holding $D$ and the interaction between $D$ and $1[D\geq d_c]$ constant). As expected, the first stage analysis confirms that there is a very significant discontinuity in school enrolment probability at $d_c$. The discontinuity is almost as large for single mothers (14 percentage points) as for mothers in two-parent families (17 percentage points). The reduced form equation does not show any significant discontinuity at $d_c$ in labour market participation for mothers in two-parent families, but reveals a 3.6 percentage point discontinuity for single mothers. When we focus on single mothers, the ratio of the participation and enrolment discontinuities provides an estimation of the average effect of school enrolment at age 3 on maternal participation of about .25 (significant at standard levels).

A similar pattern emerges from the analysis of the sample of mothers whose youngest child was born between early December 1996 and late January 1997 (Table 1, columns 3 and 4). All these children are two years old in March 1999, but those born in 1996 are in the year of their third birthday whereas those born in 1995 are still in the year of their second birthday. As a consequence, none of those born in early 1997 are at school whereas a significant fraction of those born in late 1996 have benefited from an early start in pre-elementary school.

As expected, the first stage analysis confirms that there is a significant discontinuity in school enrolment probability at $d_c$ =1996 January 1st. It is almost as large as the discontinuity at $d_c$=1995 January 1st. Also the reduced form analysis confirm that no discontinuity in maternal participation exists at $d_c$ for two-parent families, whereas a 1.4 percentage point discontinuity is perceptible for single mothers. It is not significant at standard levels however. The ratio of the participation and school enrolment discontinuities provides an estimation of the average effect of school enrolment at age 2 on single mothers' participation of about .11 which is not significant at standard levels either. As discussed above, one explanation for this less significant elasticity at age 2 lies in the fact that mothers who do not work are still eligible to family benefit when their youngest child is still below the age of three.

Overall, Table 1 confirms the graphical evidence and suggests that availability of free public school has a significant effect on single mothers, especially after their youngest child turns 3, whereas it has no perceptible effect for mothers in two-parent families, regardless of whether we focus on families with two-year olds or with three-year olds[8].

## IV.4. Variation in the Effect across Regions

[8] As a robustness check, we have replicated this analysis using the sample of lone mothers whose youngest child was born in November-December 1995 (or October-December 1995) and December 1st (or November 1st) as cut-off. These placebo regression yields non-significant regression-discontinuity estimate of the elasticity of mothers' labor supply. The same result holds true when we use the January-February 1996 (or the February-March 1996) sample and the February 1st cutoff (or March 1st cutoff).

There exists very significant variation across regions in the availability of child care arrangements that are alternative to early schooling. Also, there exists some evidence that alternative child care arrangements tend to be less developed in regions where the enrolment capacity of pre-elementary schools is the most important. Within this context, we speculate that the effect of school enrolment varies a lot across regions and tend to be more significant in regions where early enrolment is the most developed.

To test this assumption, Table 2 focuses on the same sets of single mothers as the panel B of Table 1, but provides separate analysis for the sub-sample of mothers living in regions where early enrolment rates are relatively high (i.e., above the median) on the one hand and, on the other hand, on the sub-sample of mothers living in regions where the overall early enrolment rates are relatively low (below the median).

Panel A shows the results for the 'high enrolment' sample. Comfortingly, first stage regressions show a discontinuity in preschool enrolment between children born in December 1995 and children born in January 1996 which is relatively modest compared to what we observed on the full sample of single mothers (7 percentage points). Most interestingly, the reduced form regression reveals that the corresponding shifts in maternal labour supply is actually *larger* than what we observe in the full sample. The estimated shift in maternal participation between children born in December 1995 and children born in January 1996 is about 3.2 percentage points, whereas it is only 2.6 percentage points in the full sample. As a consequence, the regression discontinuity (RD) estimate of the effect of preschool availability for children aged 3 is much larger in this 'high-enrolment' sample than in the full sample of regions. This is consistent with the assumption that alternative child care arrangements are relatively rare in these regions. The estimate suggests than a 10 percent point increase in public school availability increase the participation of single mothers by about 4.6 percentage points.

With respect to children born between December 1996 and January 1997 (the group of two-year olds), the discontinuity in enrolment probability is by construction relatively strong (15.9) compared to what we observe in the full sample of single mothers. The reduced form regression shows that the corresponding discontinuity in maternal participation in the labour market is larger than in the full sample too (+2.8 percentage points). The corresponding RD estimate of the effect of preschool availability for children aged 2 is about 0.24, which is twice as large as the estimated elasticity for mothers of 2-year olds in the full sample (but twice as small as the estimated elasticity for mothers of 3-year olds in the 'high-enrolment' sub-sample).

Panel B replicates the analysis for the low-enrolment regions. Comfortingly, the magnitude of the discontinuities in preschool enrolment are symmetric to those observed in the high-enrolment regions, i.e., they are relatively large between children born in December 1995 and children born in January 1996 and relatively modest between children born in late 1996 and children born in early 1997. Interestingly enough, the discontinuities in maternal labour supply are much less significant in these regions than in the high-enrolment regions. The discontinuity is marginally significant for mothers whose youngest child is three years old and negligible for mothers whose youngest child is two years old.

Overall, our analysis confirms the very high level of heterogeneity of the effect of preschool availability across regions. It is very strong and significant for single mothers who are living in high enrolment regions and whose children aged three. It is negligible for single mothers living in low enrolment regions or in two-parent families. It confirms that the effect of early school availability on a mother's participation in the labour market depends hugely on the existence of child care arrangements that are alternative to early school and on the level of resources that are available to the mother when she does not work.

### IV.5 Discussion

French pre-elementary school is free, available to all families (regardless of income) and represents a good childcare option for working women. It runs four days a week (plus Saturday morning) for the length of school year and lasts 6 hours per day (from 8.30 to 11.30 in the morning and 13.30 to 16.30 in the afternoon). Also, most schools offer extended hours after 16.30 and during the lunch time (lunch being highly subsidized). Overall pre-elementary schools care for children for a large part of the week and represent a very significant subsidy for childcare. Given this reality, it is not clear why the employment effect on two-parent families is negligible. The recent evaluations of the 1994 reform of family benefits suggest that it is not because the elasticity of female labour supply is low in France (see e.g. Piketty, 2003). Before 1994, mothers with three children or more received about 550 euros per month (about half the minimum wage) provided that they do not work and that their youngest child is below the age of three. The 1994 reform extended the program to two-child mothers and Piketty (2005) shows that it was followed by a huge drop in the labour market participation of these women. Figure 5 provides new evidence on this issue. Specifically we use the 1999 census and compare the labour market participation of mothers in two-parent families depending on whether their youngest child is just above or just below the 3 years old threshold. The graph reveals a very clear discontinuity of about 4 percent points at the cut-off[9]. A detailed analysis of this effect is below the scope of this paper but it clearly rules out the assumption that the elasticity of female labour supply is negligible in France. Overall, the

[9]We have checked that the discontinuity is as large for single-parent families as for two-parent ones.

most plausible explanation for the weak employment effect of preschool on two-parent families is simply that it crowds out alternative non-family childcare. Two-parent families are much less exposed to poverty and have larger social networks than lone mothers[10]. As a consequence, they have a much larger access to non-parental modes of childcare (formal and informal) and are likely to use preschool more often as a mere substitute for these non-parental childcare. It is plausibly why their labour supply is much less affected than those of lone mothers.

## V. The Effect of Early School Enrolment on Children's Subsequent Outcomes

French early schooling policy affects mothers' labour supply, but it also increases the time spent in pre-elementary school by young children. This section asks whether it has an effect on children's subsequent outcomes. The basic problem is to identify changes in early enrolment probabilities that are unrelated to changes in unobserved determinants of performance at school. To address this issue, we make use of the fact that French regulations generate highly discontinuous variations in early enrolment probability across children's month of birth and that the cut-off months of birth vary across regions. In other words, French institutions create discontinuous variations in time spent in pre-elementary school across regions and months of birth which are plausibly uncorrelated with pupils' unobserved characteristics and which can be used for identification.

### V.1 Graphical Evidence

[10] According to the French National Institute, the poverty rate for single-parent families is about 30% whereas the poverty rate for two-parents families is about 10% (see Goutard and Pujol, 2008).

To begin with, Figures 6a and 6b show the relationships between early schooling and month of birth, using the survey on national tests taken at the beginning of 3rd grade. For the sake of clarity, we still distinguish two types of regions only, according to whether regions are above or below the median of the regional distribution of overall early enrolment rates. Also we use December (month 12) as a reference.  Figure 6a shows a very clear negative shift in early enrolment rate for pupils born just *before* the summer period in low-enrolment regions whereas Figure 6b shows that a similar decrease occurs only for pupils born *after* the summer period in high-enrolment region.  Overall, being born *during* the summer period rather than *before* is accompanied by a significant decrease in early schooling probability in low-enrolment regions, but not in high-enrolment ones. Symmetrically, being born *after* rather than *during* the summer period implies a significant decrease in early schooling in high-enrolment regions, but not in low-enrolment ones. Given these facts, the issue is whether we observe a relative decline in test scores in low enrolment region for pupils born during the summer period and, subsequently, a relative decrease in high-enrolment region for those born just after the summer period. Figure 7 suggests that this is not the case. It compares high-enrolment and low-enrolment regions with respect to the scores obtained at the national evaluations in Math and French conducted at the beginning of the third grade. The scores obtained in high-enrolment regions are on average slightly higher than in low-enrolment regions, but the difference is almost exactly the same for pupils born during the summer as for pupils born either at the beginning or the end of the year (about 1 point=6% of a SD). This result clearly suggests that substituting early schooling for alternative child care does not really matter for subsequent educational outcomes.

Table 3 goes beyond simple visual evidence and provides an explicit statistical evaluation of the variation in early schooling and test scores according to whether birth took place before, during or after the summer period (defined as June to September). Panel A confirms that the decline in early enrolment probability occurs mostly before the summer period in low-enrolment regions (-7.0 percent point), but after the summer period in high-enrolment regions (–6.4 percent points). These shifts generate a very significant decrease in the relative enrolment rate in low-enrolment regions (–8.2 percent points) just before the summer cut-off, and a symmetrical increase in the same variable (+7.1 percent points) just after the summer cut-off (Panel A, column 3). Panel B confirms that these changes in the relative enrolment rates of children living in low-enrolment regions do not generate any significant shifts in their relative performances in primary school (Panel B column 3). There is a significant decline in average test scores between pupils born before the summer period and pupils born during this period. Also, there is another significant decline between pupils born during the holiday period and pupils born after the holiday period. But these declines reflect pure maturity effects and have almost exactly the same magnitude in high-enrolment and low-enrolment regions. Overall, Figures 6 and 7 and Table 3 suggest that early schooling has, as such, no significant effect on children's subsequent educational achievement.

As additional piece of evidence, Panel C shows that there is no significant variation in family socio-economic background before and after the summer cut-off, regardless of the region. This finding is consistent with the assumption that being born before, during or after the summer period is related to changes in parental resources that are similar in low and high enrolment regions (and actually negligible in both regions). Under this assumption, the discontinuous variations in relative enrolment rates observed just before or just after the summer period make it possible to identify the effect of school enrolment on subsequent outcomes. Specifically, the ratio of the discontinuity in relative outcomes and the discontinuity in relative enrolment rates provides an estimate of the impact of enrolment on outcomes. The next section develops a simple regression model to estimate this impact.

### V. 2. Regression analysis

Before moving on to the regression analysis, we introduce some concepts and notations. For any child, $D$ will represent the date of birth, $D_k$ the first of September of the year of the *k-th* birthday, and $D_E$ the date of entry into pre-elementary school (with $D_E=D_3$ or $D_2$ depending on whether the child is an early or a normal starter). The time spent in school before entry into the first normal year of pre-elementary school is $S=D_3-D_E$, the time spent in alternative child care arrangement at the same date is $F= D_E-D$ and we assume that educational outcomes observed after $D_3$ (denoted $Y$) can be written,

(6) $$Y= \alpha S + \beta F + \gamma R + \theta X + \delta + u,$$

where $\alpha$ represents the effect of early time spent in pre-elementary school (i.e., between $D_2$ and $D_3$) and $\beta$ the effect of time spent in alternative child-care during the same period. The dummy variable $R$ represents region of residence ($R=1$ if high-enrolment region) whereas $X$ represents family socio-economic background, $\delta$ and intercept and $u$ the unobserved determinant of performance in primary school. Using $F=D_E-D=D_3-S-D,$ Eq. (6) can be rewritten,

(7) $$Y= (\alpha-\beta)S - \beta D + \gamma R + \theta X + \lambda + u.$$

where $\lambda=\delta+\beta D_3$ is a new intercept. The issue is to evaluate $(\alpha-\beta)$ the effect on $Y$ of substituting some $S$ for some $F$ and the basic problem is that early schooling $S$ is not randomly distributed across pupils. The unobserved factor $u$ is potentially correlated with $S$ and $(\alpha-\beta)$ cannot be identified through standard regressions. To overcome this issue, we assume that the difference across regions in the variation in enrolment rates across periods of birth does not coincide with significant difference in the variation of unobserved $u$. Formally, it amounts assuming that the interaction variables $Z_1$ ='living in a high-enrolment region and being born after the summer period' and $Z_2$='living in a low-enrolment region and being born in the summer period or after' are uncorrelated with $u$ and can be used as instrumental variables[11]. We use region of birth (high vs. low-enrolment regions), period of birth (before, after or during the summer period), month of birth (linear specification) and a full set of family socio-economic background dummies and cohorts, as control variables.

The first stage regression confirms that being born after the specific cut-off of ones' region has a significant negative effect on time spent in pre-elementary school (Table 4, model 1). A Fisher test ($F$=6.8) clearly rejects the joint nullity of the effects of our two instruments at standard level. In contrast, the reduced form analysis shows that being born after the cut-off date has no significant effect on test scores, neither in high-enrolment nor in low-enrolment regions. The corresponding IV estimate of $(\alpha-\beta)$ is small, negative and not significantly different from zero (Table 4, model 3), which is consistent with our graphical analysis.

One potential issue with this basic model is that the effect of time spent in alternative child-care institutions may vary across high-enrolment and low-enrolment regions. One cannot exclude that the quantity and quality of alternative child-care vary in response to schools' early enrolment capacity. In such a case, the discontinuity in early enrolment rates observed before the summer period in low-enrolment region does not identify the same effect as the discontinuity after the summer period in high-enrolment regions. Under this assumption, the previous IV estimate is an average of the effect in high-enrolment region and the effect in low-enrolment regions. It may be close to zero even though the effect is significantly positive in one type of regions and significantly negative in the other ones. One very simple way to test this assumption is to test the internal validity of our two instruments $Z_1$ and $Z_2$. If the effect of substituting time spent in early schooling for time in alternative child-care is not the

[11]This identifying assumption implies that the difference in average potential outcomes across regions is continuous at the cut-off months of birth, i.e. the same type of continuity assumptions as those used in the first part of the paper for identifying the employment effect of preschool. Let us emphasize that our strategy does not rely on the assumption that period of birth is unrelated to unobserved characteristics (such as family resources). We only assume that the difference in family resources across regions does not vary significantly before and after the cut-off dates.

same in high-enrolment and low-enrolment regions, the two instruments $Z_1$ and $Z_2$ identify different effects and are not jointly valid. Comfortingly, standard over-identification tests do not reject the assumption of the joint validity of $Z_1$ and $Z_2$.

Another simple approach consists in using our two sources of identification to evaluate separately the effect of early schooling in low-enrolment and high-enrolment regions (or alternatively, to evaluate separately the effect in one type of region and the difference between the two types of regions). Denoting $\beta_0$ the effect of time spent in alternative child-care in low-enrolment region and $\beta_1$ the same effect in high-enrolment region and assuming that the two effects are potentially different, Equation (7) can be rewritten,

(8) $$Y= (\alpha- \beta_0)S + (\beta_1-\beta_0)FxR - \beta_0 D + \gamma R + \theta X + \lambda + u.$$

By construction, $Z_1$=($1$-$R$)x1($D$>$May$) is correlated with $S$ but not with the interaction term $FxR$. It identifies $(\alpha- \beta_0)$. In contrast, $Z_2$=$R$x1($D$>$September$) is correlated with both $S$ and $FxR$. It provides an additional instrument for identifying $(\beta_1-\beta_0)$ on top of $(\alpha- \beta_0)$. The last column of Table 4 shows the results obtained with this augmented specification. The estimated $(\beta_1-\beta_0)$ is small and not significantly different from zero, which confirms the result of our over-id test. The estimated $(\alpha- \beta_0)$ is negative, but not significantly different from zero, which is consistent with our initial IV analysis.

Finally, we have checked that the IV results are similar when we use the interaction between 'living in a high-enrolment region and being born in the summer period' as sole instrument and add an interaction between region of residence and a variable indicating the maturity of the child by the start of the academic year[12] (as measured by our period of birth index, before summer, summer, after summer). In this specification, we do not assume anymore that the potential variations in the unobserved determinants of performance at school across periods of

[12] Formally, it amounts using ($Z_1$+$Z_2$) as sole instrument and using ($Z_1$-$Z_2$) as additional control variable.

birth are the same in low and high enrolment region, but only assume that these variations are the same across the first and second cut-off[13].

### V.3. The Effect of Early School Enrolment on Early Dropout Rates

Overall, national tests conducted at the beginning of 3rd grade suggest that early schooling has no significant effect on performance in primary school. The general census of the population makes it possible to analyse another very basic educational outcome - the probability of early dropout from school – using much larger samples. In France, school is compulsory until the age of 16 and the census provides a direct measure of the proportion of individuals who dropout from school at age 17, 18 or 19 as well as a measure of the variation in these proportions across regions and exact dates of birth. Do we observe any significant increase in early dropout rates for individuals born just before the summer period in low-enrolment regions and for individuals born just after the summer period in high-enrolment region? Figure 8 suggests that it is not the case. It shows that the early dropout rates are slightly larger in low enrolment regions, but the difference between high and low enrolment regions is about exactly the same for students born during the summer period as for students born either before or after this period. In both regions, the early dropout rates declines in a highly parallel way from about 9 percentage points for pupils born in January to about 5 percentage points for pupils born in December, without any clear fluctuations in the regional difference across dates of birth within the year.

Table 5 provides a regression analysis of the effect of early enrolment on early dropout from school using census data and the same specifications as Table 4. One issue is that we have to

[13]We have also replicated our regression analysis separately on the sub-sample of pupils with a high socio-economic background and on the sub-sample of pupils with a low socio-economic background. We have been unable to detect significant difference in the estimates obtained with the different sub-sample.

use different censuses for the first-stage and second stage analysis. Specifically, the census conducted in 1999 provides information on the relationship between date of birth and early dropout rates for cohorts born in the early eighties and late seventies (second-stage), but not on the relationship between date of birth and early schooling probability for these cohorts (first-stage). Symmetrically, the census conducted in 1982 provides information on the relationship between date of birth and early schooling for cohort born in the late seventies, but not on the relationship between date of birth and rates of early dropout from school for these cohorts. To overcome this problem, we proceed in two steps. First we perform the first-stage analysis with the 1982 census. Secondly, we use the results of this initial regression to construct predicted early enrolment rates with the 1999 data and we perform the second-stage analysis with these predicted values[14], i.e., using the Two-Sample Instrumental Variable (TSIV) technique developed by Angrist and Krueger (1992).

To begin with, the first model of Table 5 shows the results of regressing early schooling on exact date of birth, region of residence and interactions between these two variables using the 1982 census and the same specification as model 3 in Table 4. Comfortingly, the results are consistent with our previous analysis. We find a negative shift in early enrolment probability for children born before the summer period in low-enrolment regions and for children born after the summer period in high-enrolment regions. In the late seventies and early eighties, a priority was already given to young children born earlier in the year, so that the difference in early schooling rates between high and low enrolment regions was already much larger for pupils born during the summer period than for pupils born either at the beginning or the end of the year [15]. The second model of Table 5 provides a reduced form analysis of the impact of

[14] The 1982 census provides the first stage analysis for the cohort born in 1979. With respect to dropout rates at age 19, the 1999 census provides the reduced form analysis for the same 1979 cohort. For dropout rates at age 18 or 17, the information is for cohort born in 1981 or 1980. In such a case, our TSIV estimates assume that the first-stage estimates would be similar for cohort 1980 (or 1981) as for cohort 1979.

[15]The census conducted in March 1982 makes it possible to analyse the full distribution of early enrolment rates across regions and date of birth within the year for the cohort 1979. This analysis (not reported) confirms that the

exact date of birth on dropout rates at age 17 using the 1999 census and the same specification as model 3 of Table 4. We find no significant variation in relative dropout rates either before or after the summer cut-off. A Fisher test (*F=1.21*) does not reject the joint nullity of the reduced-form effects of the two instruments at standard level. The third model shows the result of the corresponding Two-Sample Instrumental Variable estimation. It confirms that early schooling has a no significant effect on early dropout rates. Models 4 and 5 replicate this analysis with dropout rates at age 19 as dependent variable. Model 4 does not reject the joint nullity of the reduced form effect of our instruments on outcomes (*F=2.1*) and the corresponding TSIV estimate remains non significant at standard level. Overall, we have an array of results suggesting that the long term effect of early enrolment at school is small and statistically non-significant.

## VI. Discussion : The Costs-Effectiveness of French Early Schooling Policy

Early school availability increases lone mothers' participation in the labour market and has no significant adverse effect on children's subsequent outcomes. This section makes use of these results to provide a simple back-of-the-envelope evaluation of the cost-effectiveness of this policy. What would be the costs and benefits of substituting early school for alternative modes of childcare for the population of two-year olds?

As discussed above, before starting pre-elementary school, French children are either in a collective childcare centre (*crèches*, about 12% of children) or with a registered child-minder *(assistante maternelle*, about 22% of children) or at home with one of their parents[16] (about

difference in early enrolment rates across high and low enrolment regions is already about twice as large for pupils born in June (30 percentage points) than for pupils born in late December (15 percent points) early January (19 percent points).

[16] In fact, a significant proportion of parents who do not use formal non-parental childcare (*crèche* or *assistante maternelle agréée*) and who respond that they provide childcare themselves, plausibly use informal modes (non-registered child-minder for instance).

two-third of the cases). Given various public aids, the total costs per child and year are about 15,000 euros for *crèches*, 10,000 euros for *assistantes maternelles* and 5,000 euros for parental childcare. Hence, the different alternative arrangements are on average more costly for the society than pre-elementary school (which total costs is about 4,700 euros per child and year). The average gain of substituting one child in pre-elementary school for one child in alternative childcare institutions is actually about +2,600 euros (12%.15,000 + 22%.10,000 + 66%.5000 – 4,700). With respect to childcare, pre-elementary school represents for society a technology which is actually much more productive than existing alternative modes. We have only about six children per adult in *crèches* and only three children (maximum) per *assistante maternelle*, whereas the average class size in pre-elementary school is about twenty five.

With respect to benefits for society, we have shown that an increase in preschool availability increases very significantly single mothers' labour supply. Given that single-mothers represent about 12% of the population of mothers and that the elasticity of their labour supply to pre-elementary school is about 0.25, the employment effect of substituting pre-elementary school for alternative childcare is on average about +1200 euros per child and per year[17] ($0.25\times 0.12 \times 40{,}000$, where 40,000 represents the average annual value of one job in the French economy).

Overall, substituting early pre-elementary school for existing modes of childcare in France generates a net benefit for society of about 3,800 euros per child and year (2,600 + 1,200), without having significant adverse effects on children's subsequent outcomes. Pre-elementary school is often presented in the French debate as a relatively costly policy option. It is actually much more cost-effective than the existing alternative options[18].

[17] We should also take into account the discounted effect on mothers' careers. Long disruption have a very significant negative effect on subsequent wage mobility (Bayet, 1996).

[18] It does not mean that it is necessarily more cost effective than any other comparison policy. If the only goal is to increase maternal employment, it would be plausibly even more cost effective to eliminate the current subsidy to mothers not working (or to combine this elimination with the development of pre-elementary schools).

## VII. Conclusion

French children start pre-elementary school either the year the turn two or the year they turn three. This paper provides the first comprehensive evaluation of the effect of this unique early schooling policy on both maternal labor supply and children's educational outcomes. To begin with, we find that pre-elementary school availability has significant employment effect on single-parent families, but no effect on two-parent families. Two-parent families have a larger access to alternative non-parental modes of childcare and it is likely that pre-elementary school crowds out these non-parental alternatives. It is also highly plausible that the substitution of free pre-elementary school for alternative (non-free) modes of childcare increases family net resources and well-being. Further research is needed in this area to better understand the benefits of free pre-elementary schools for families.

With respect to children's educational outcomes, we find that substituting one year of pre-elementary school for one year of alternative modes of childcare has no significant effect on two year olds' subsequent outcomes. There exist very significant variations across months of birth in the differences in early school enrolment rates across regions, but these variations do not coincide with any significant changes in either test scores in elementary school or early dropout rates from school at age 17.

Taken together, our results suggest that pre-elementary school is more cost-effective than existing alternative modes of childcare for two-year olds in France. It does not mean that it is the most cost-effective institution one can possibly conceive. Further research is needed to explore this issue and help designing more effective preschool institutions.

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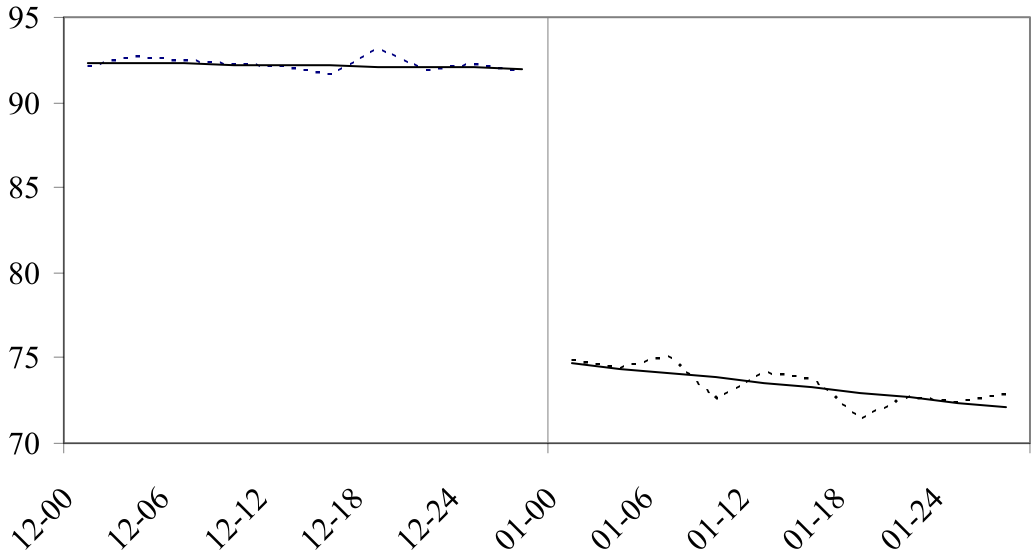
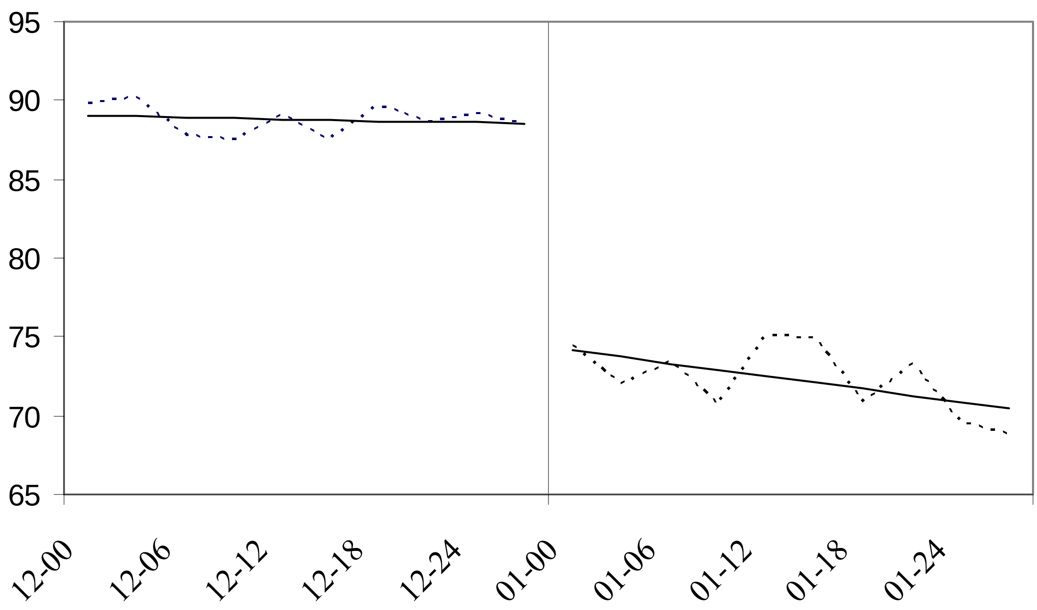
Figure 1a: School enrollment, two-parent families (1995-96 cut-off, age 3)



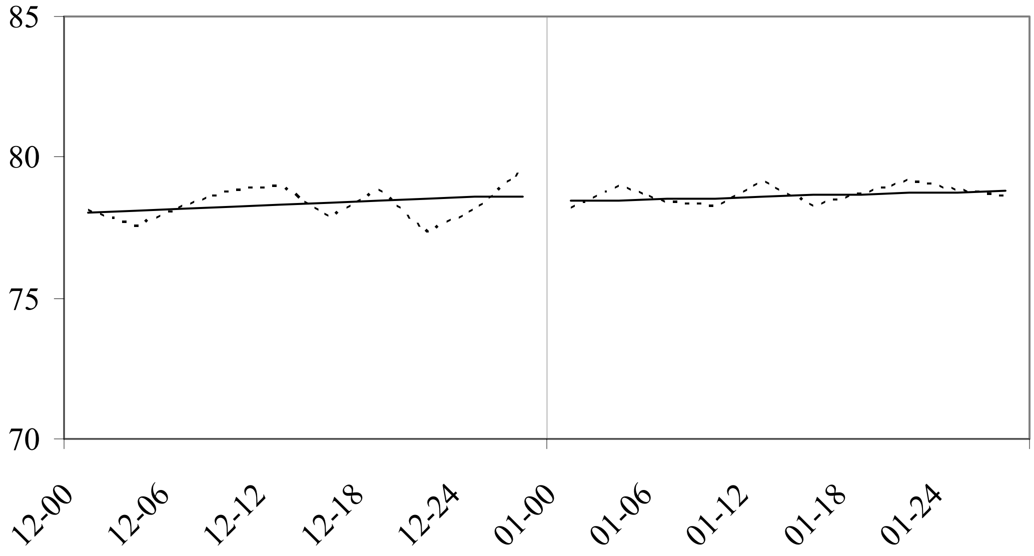

Source: 1999 census.
Sample: Two-parent families whose youngest child was born between Dec. 1995 and Jan. 1996.
Reading: The enrolment rate is above 90% for children born in Dec. 1995, it is below 75% for children born in Jan. 1996

Figure 1b: School enrolment, single mothers (1995-96 cut-off, age 3)



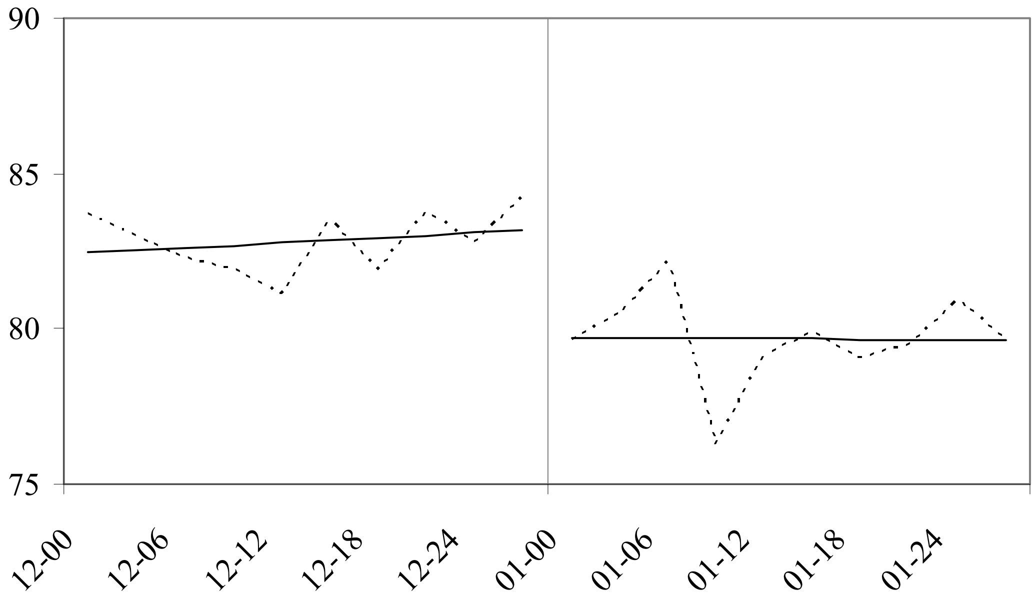

Source: 1999 census.
Sample: Single-parent families whose youngest child was born between Dec. 1995 and Jan. 1996.

Figure 2a : LM Participation, two-parent families (1995-96 cut-off, age 3)



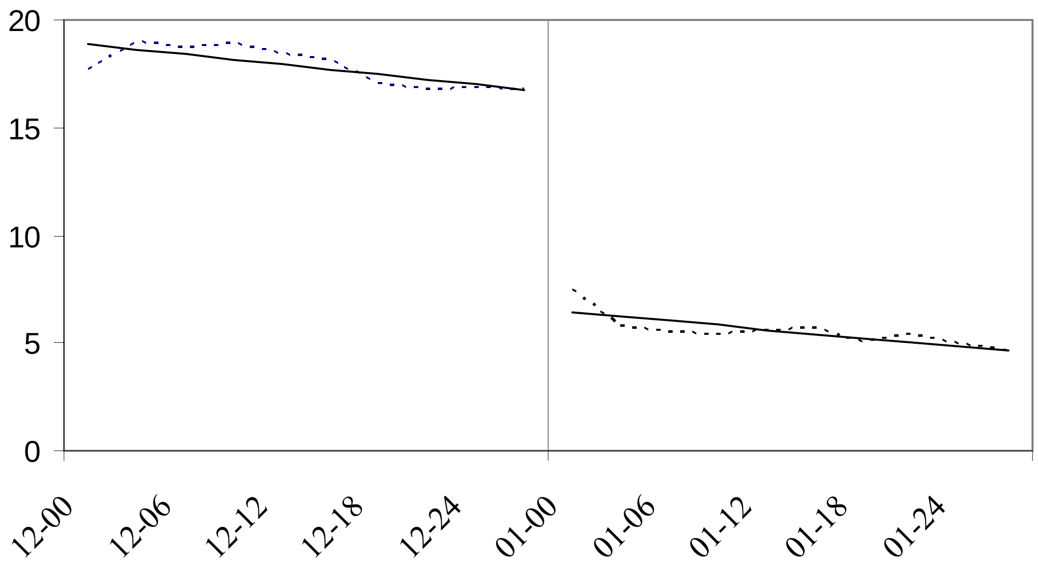

Source: 1999 census.
Sample: Two-parent families whose youngest child was born between Dec. 1995 and Jan. 1996.
Reading: The proportion of mothers participating in the LM is about 78% for mothers whose youngest child was born in Dec. 1995 as well as for those whose youngest child was born in Jan. 1996.

Figure 2b: LM Participation, single mothers (1995-96 cut-off, age 3)



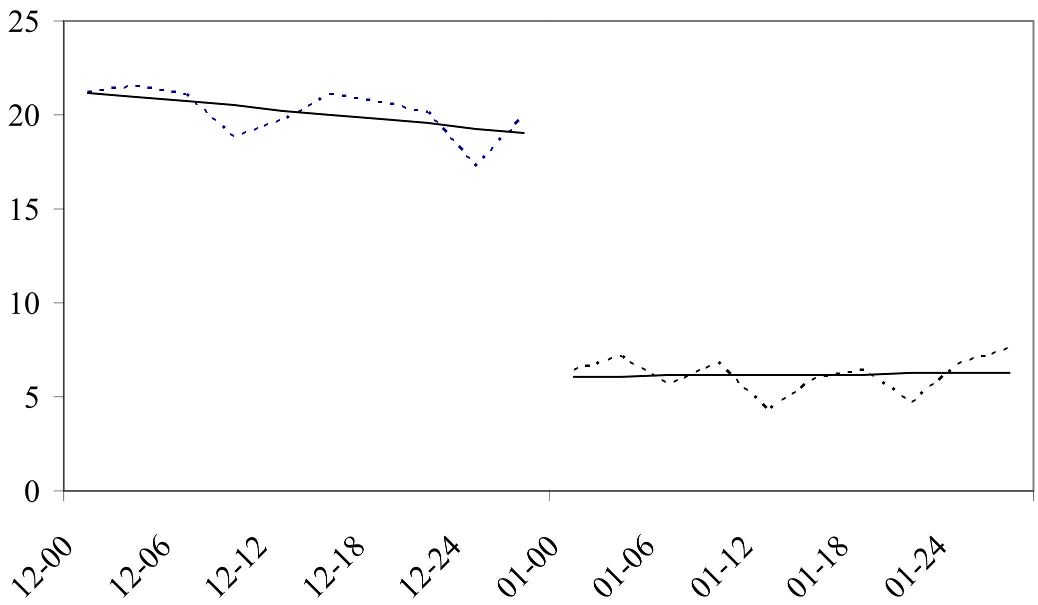

Source: 1999 census.
Sample: Single-parent families whose youngest child was born between Dec. 1995 and Jan. 1996.

Figure 3a: School enrolment, two-parent families (1996-97 cut-off, age 2)



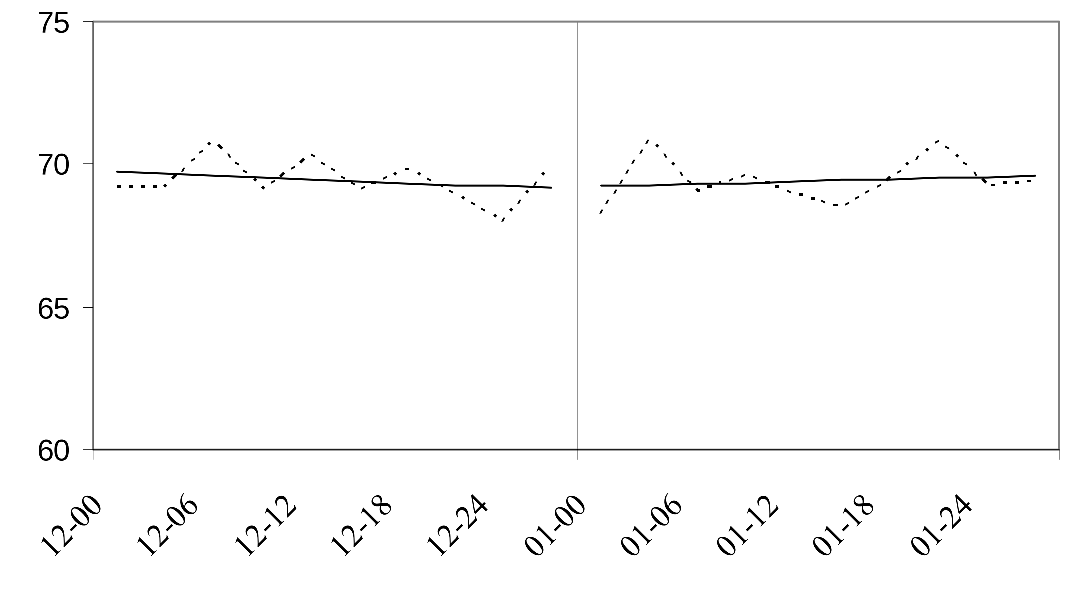

Source: 1999 census.
Sample: Two-parent families whose youngest child was born between Dec. 1996 and Jan. 1997.

Figure 3b: School enrolment, single mothers (1996-97 cut-off, age 2)



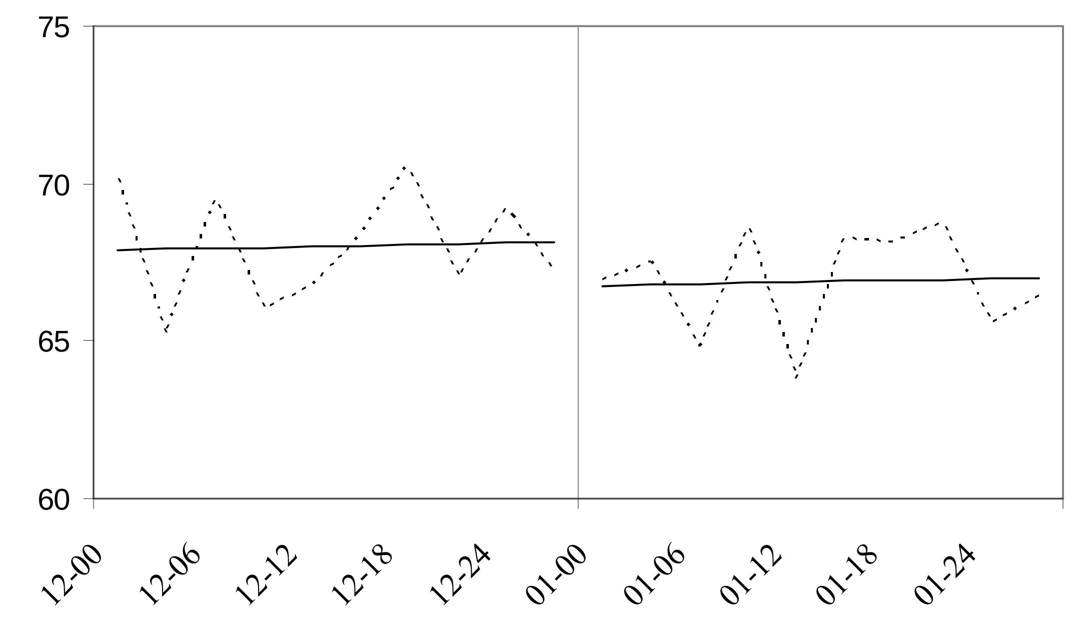

Source: 1999 census.
Sample: Single-parent families whose youngest child was born between Dec. 1996 and Jan. 1997.

Figure 4a: LM Participation, two-parent families (1996-97 cut-off, age 2)



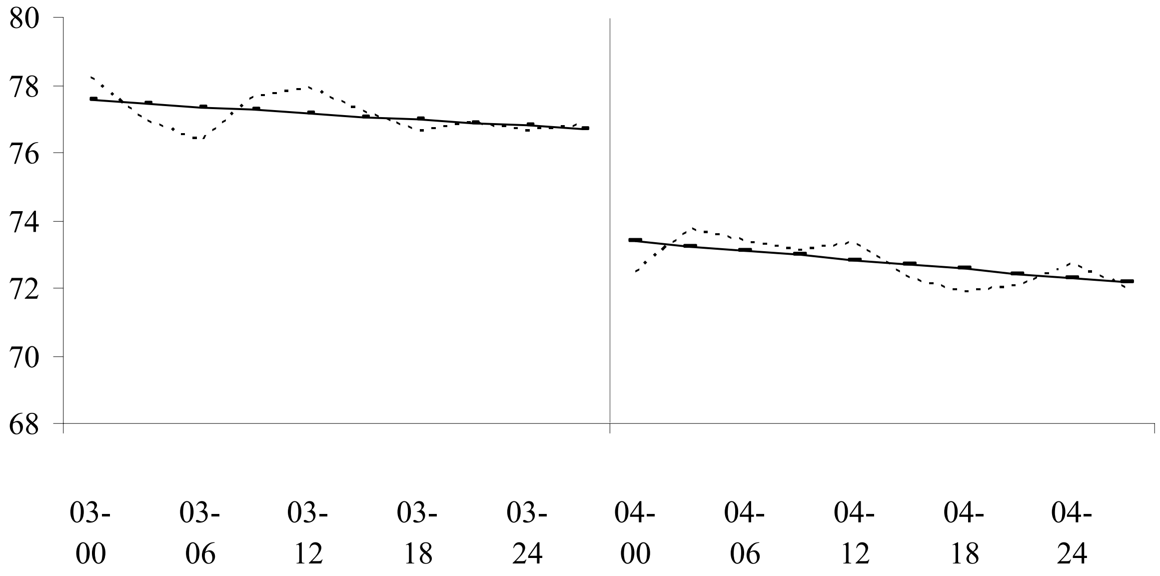

Source: 1999 census.
Sample: Two-parent families whose youngest child was born between Dec. 1996 and Jan. 1997.

Figure 4b: LM Participation, single mothers (1996-97 cut-off, age 2)



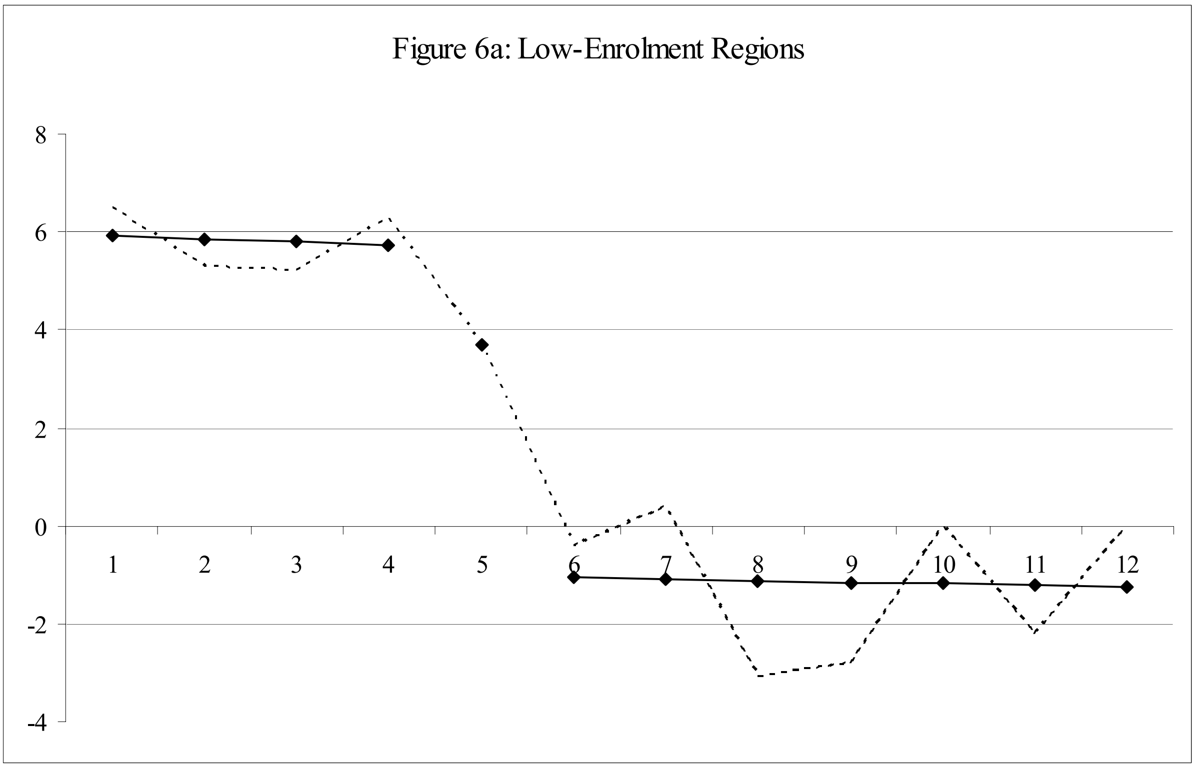

Source: 1999 census.
Sample: Single-parent families whose youngest child was born between Dec. 1996 and Jan. 1997.

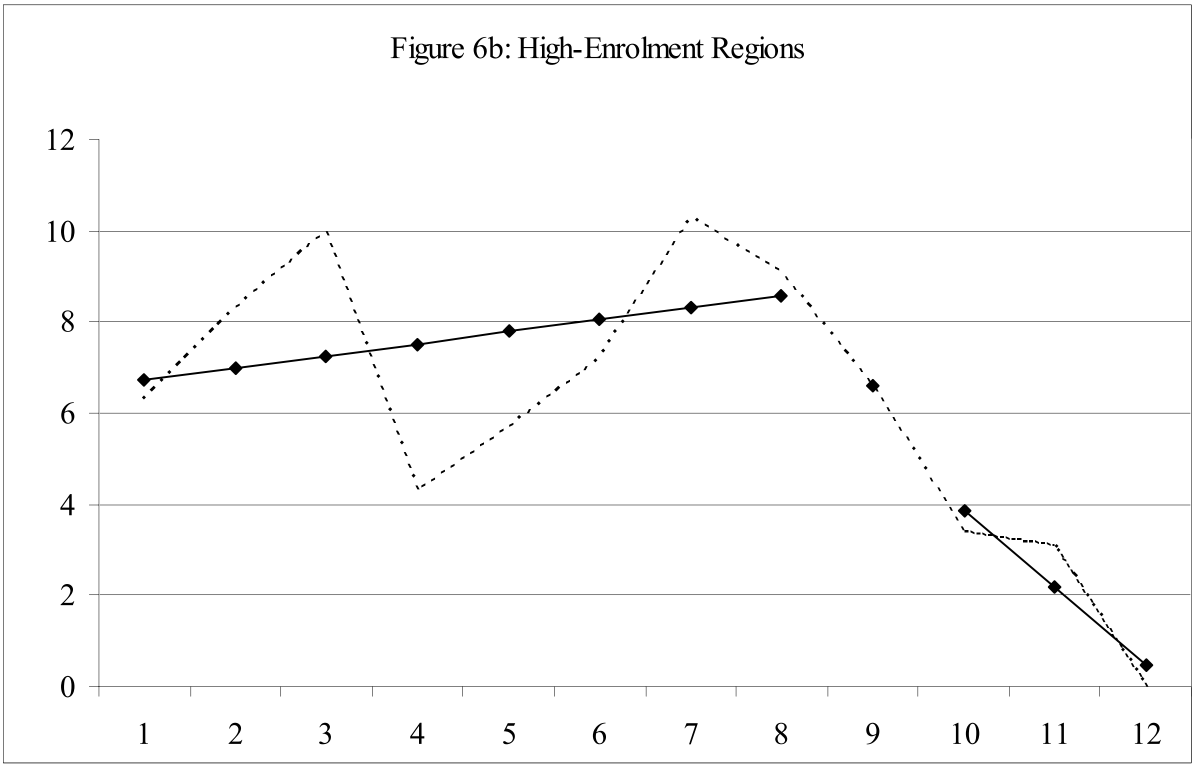
Figure 5: LM participation, two-parent families, (1996 March/April family benefits cutoff)




Source: 1999 census.
Sample: Two-parent families whose youngest child was born between March 1996 and April 1996.

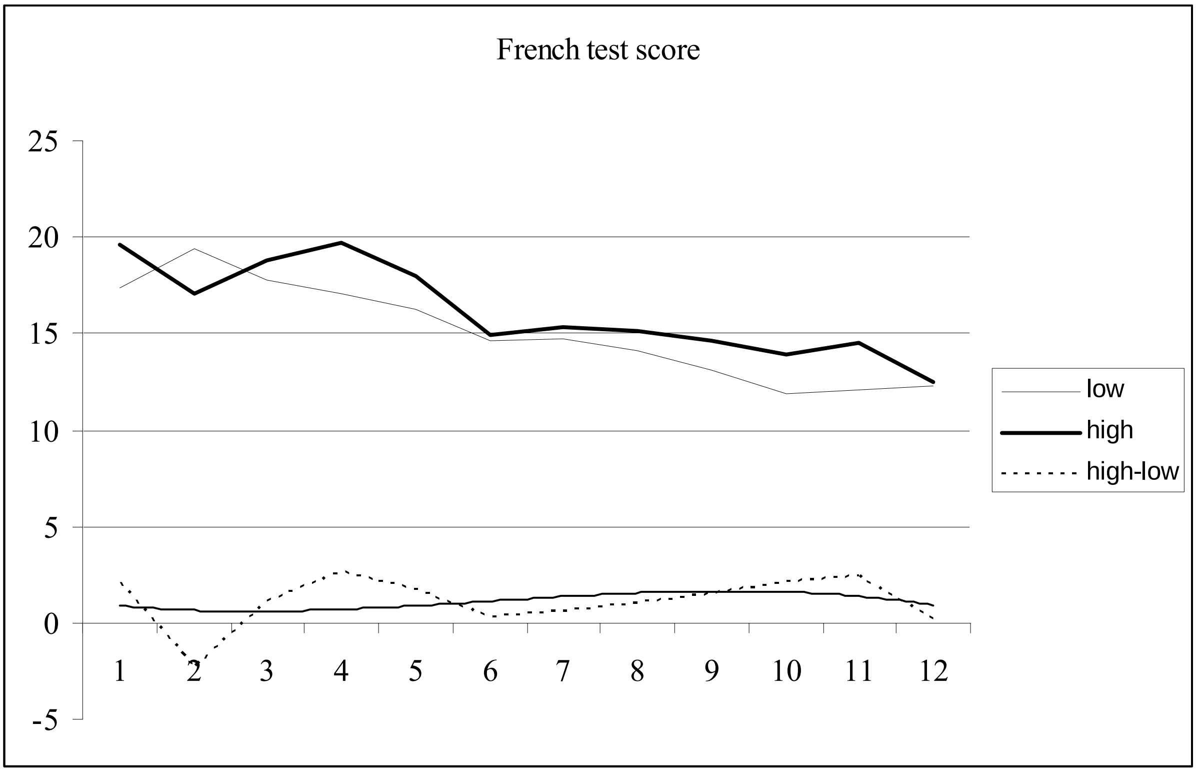
Figure 6: Variation across Months of Birth in Early Enrolment Probability

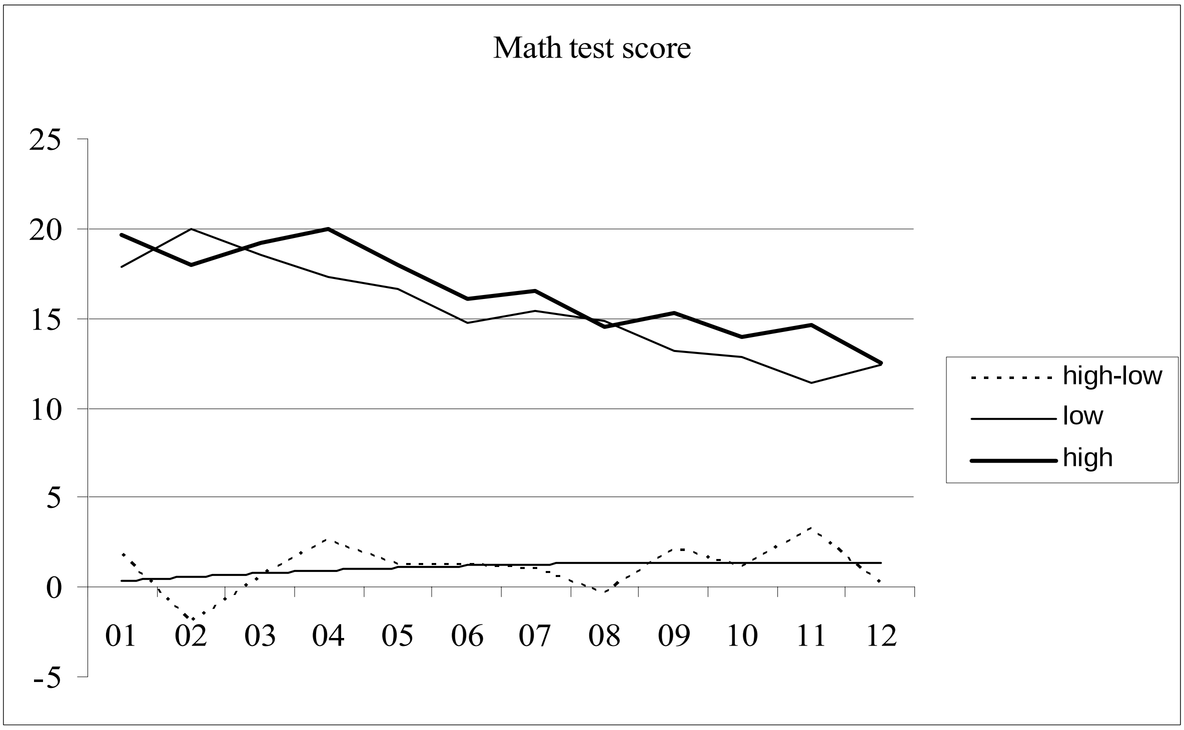



Source: National survey on tests taken at the beginning of 3rd grade, cohorts 1991-1994.
Reading: In low enrolment regions, the proportion of pupils who started school at the age of 2 is about 6 percent points higher for pupils born in April (month of birth=4) than for pupils born in June (month of birth=6). December is taken as reference.

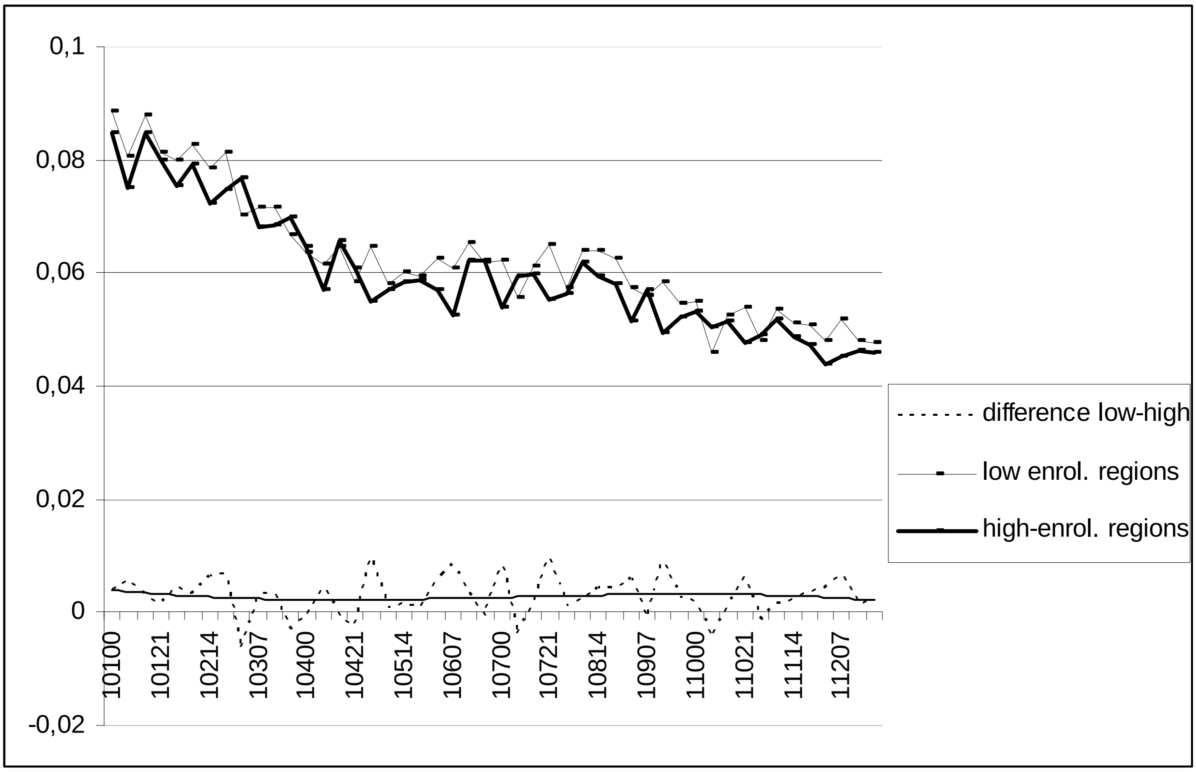
Figure 7: Test Scores in High and Low Enrolment Regions, by Months of Birth




Source: National survey on tests taken at the beginning of 3rd grade, cohorts 1991-1994.

Figure 8: Early Dropout Rates in High and Low-Enrolment Regions, By Months of Birth.




Source: 1999 census. Sample: Children born in 1981.

**Table 1:** The Effect of Pre-elementary School Enrolment on Mothers' Labor Supply: Regression-Discontinuity Estimates, by Age Group and Family Type.

| Panel A | Two-parent families | | | |
|---|---|---|---|---|
| | Children born between Dec. 1995, 1 and 1996, January, 31 | | Children born between Dec. 1996, 1 and 1997, January, 31 | |
| | School Enrolment (first stage) | Mother's Participation (reduced form) | School Enrolment (first stage) | Mother's Participation (reduced form) |
| Intercept | 0.938 (0.023) | 0.754 (0.026) | 0.262 (0.018) | 0.709 (0.026) |
| Year of birth=1996 | -0.171 (0.005) | -0.004 (0.006) | 0.101 (0.004) | 0.001 (0.006) |
| Day of birth | -0.051 (0.022) | 0.028 (0.024) | -0.078 (0.017) | -0.014 (0.025) |
| Day of birth ×1996×100 | -0.075 (0.030) | -0.013 (0.034) | - | - |
| Day of birth ×1997×100 | - | - | 0.014 (0.024) | 0.029 (0.035) |
| R-Square | 0.062 | 0.000 | 0.037 | 0.000 |
| Number of observations | 73,493 | 73,493 | 88,843 | 88,843 |
| IV Estimate Z=[Year of birth=1996] | | 0.025 (0.036) | | 0.005 (0.062) |
| **Panel B** | **Single-mother families** | | | |
| | Children born between Dec. 1995, 1 and 1996, January, 31 | | Children born between Dec. 1996, 1 and 1997, January, 31 | |
| | School enrolment (first stage) | Mother's Participation (reduced form) | School enrolment (first stage) | Mother's Participation (reduced form) |
| Intercept | 0.905 (0.065) | 0.797 (0.066) | 0.285 (0.055) | 0.668 (0.078) |
| Year of birth=1996 | -0.143 (0.015) | -0.036 (0.015) | 0.131 (0.013) | 0.014 (0.018) |
| Day of birth | -0.016 (0.060) | 0.029 (0.061) | -0.078 (0.052) | 0.011 (0.073) |
| Day of birth ×1996×100 | -0.110 (0.086) | -0.026 (0.086) | - | - |
| Day of birth ×1997×100 | - | - | 0.095 (0.073) | 0.002 (0.103) |
| R-Square | 0.044 | 0.002 | 0.043 | 0.000 |
| Number of observations | 10,188 | 10,188 | 10,471 | 10,471 |
| IV Estimate Z=[Year of birth=1996] | | 0.253* (0.109) | | 0.111 (0.142) |

Source: 1999 Census.
Sample: Families whose youngest child was born between Dec. 1995 and Jan. 1996 (two first columns) or between Dec. 1996 and Jan. 1997 (two last columns).

**Table 2:** The Effect of Pre-elementary School Enrolment on Single-Mothers' Labor Supply: Regression- Discontinuity Estimates, by Age Group and Region of Residence

| Panel A | High enrolment regions | | | |
|---|---|---|---|---|
| | Children born between Dec. 1995, 1 and 1996, January, 31 | | Children born between Dec. 1996, 1 and 1997, January, 31 | |
| | School Enrolment (first stage) | Mother's Participation (reduced form) | School Enrolment (first stage) | Mother's Participation (reduced form) |
| Intercept | 0.927 (0.007) | 0.790 (0.008) | 0.423 (0.008) | 0.648 (0.010) |
| Year of birth=1996 | -0.070 (0.011) | -0.032 (0.013) | 0.159 (0.012) | 0.028 (0.015) |
| Day of birth | -0.025 (0.010) | 0.020 (0.012) | -0.154 (0.011) | 0.004 (0.014) |
| Day of birth ×1996×100 | -0.093 (0.018) | -0.034 (0.022) | - | - |
| Day of birth ×1997×100 | - | - | 0.122 (0.021) | 0.023 (0.025) |
| R-Square | 0.041 | 0.001 | 0.112 | 0.001 |
| Number of observations | 17,243 | 17,243 | 17,329 | 17,329 |
| IV Estimate Z=[Year of birth=1996] | | 0.457* (0.188) | | 0.242* (0.095) |
| **Panel B** | **Low enrolment regions** | | | |
| | Children born between Dec. 1995, 1 and 1996, January, 31 | | Children born between Dec. 1996, 1 and 1997, January, 31 | |
| | School enrolment (first stage) | Mother's Participation (reduced form) | School enrolment (first stage) | Mother's Participation (reduced form) |
| Intercept | 0.906 (0.008) | 0.847 (0.007) | 0.190 (0.006) | 0.732 (0.009) |
| Year of birth=1996 | -0.208 (0.012) | -0.014 (0.011) | 0.085 (0.009) | -0.015 (0.014) |
| Day of birth | -0.024 (0.011) | -0.003 (0.010) | -0.043 (0.009) | -0.024 (0.013) |
| Day of birth ×1996×100 | -0.223 (0.021) | -0.040 (0.019) | - | - |
| Day of birth ×1997×100 | - | - | 0.031 (0.016) | 0.004 (0.023) |
| R-Square | 0.156 | 0.003 | 0.034 | 0.000 |
| Number of observations | 17,913 | 17,913 | 18,015 | 18,015 |
| IV Estimate Z=[Year of birth=1996] | | 0.069 (0.054) | | -0.175 (0.161) |

Source: 1999 Census.
Sample: Single-parent families whose youngest child was born between Dec. 1995 and Jan. 1996 (two first columns) or between Dec. 1996 and Jan. 1997 (two last columns).

**Table 3**: Early Schooling, Test Score (3rd grade) and Family Background, by Date of Birth and Region of Residence.

| Panel A | | Early schooling | |
|---|---|---|---|
| Variation in Date of Birth | Low-enrol. region (a) | High-enrol. region (b) | *Diff. (a) – (b)* |
| During Summer /Before Summer | -7.0* (1.4) | +1.2 (1.9) | *-8.2* (2.4)* |
| After Summer /During Summer | +0.7 (1.7) | - 6.4* (2.1) | *+7.1* (2.7)* |

| Panel B | | Test score | |
|---|---|---|---|
| Variation in Date of Birth | Low-enrol. region (c) | High-enrol. region (d) | *Diff. (c) – (d)* |
| During Summer /Before Summer | -4.1* (0.6) | -3.6* (0.6) | *-0.5 (0.8)* |
| After Summer /During Summer | -1.6* (0.7) | -1.5* (0.6) | *-0.1 (0.9)* |

| Panel C | | High Socioeconomic Family Background | |
|---|---|---|---|
| Variation in Date of Birth | Low-enrol. region (e) | High-enrol. region (f) | *Diff. (c) – (d)* |
| During Summer /Before Summer | +1.1 (1.3) | +0.6 (1.5) | *+1.7 (2.0)* |
| After Summer /During Summer | -0.8 (1.5) | -1.3 (1.7) | *+0.5 (2.2)* |

Source: National survey on tests taken at the beginning of 3rd grade, cohorts 1991-1994. The number of observations is 3601 in low-enrolment region (first column) and 3588 in high-enrolment ones (second column). Reading: In low enrolment regions, we observe a -7.0 percent difference in early schooling probability between children born during the summer (Jun. to Sept.) and those born before the summer (Jan. to May). * indicates statistical significance at the 5% level. Standard errors are in parenthesis.

**Table 4**: The Effect of Early Schooling on Test Scores: IV Estimates.

| | Early school (First-stage) | Test score at entry 3rd grade. | | | |
|---|---|---|---|---|---|
| | | Reduced-form | IV | OLS | IV |
| | (1) | (2) | (3) | (4) | (5) |
| Early school | - | - | -1.59 | +1.82* | -5.8 |
| | | | (8.6) | (.37) | (9.5) |
| R x (1-Early school) | - | - | | | -1.56 |
| | | | | | (1.44) |
| $Z_1$=R x (D>Sept.) | -.073* | .78 | - | - | - |
| | (.027) | (.89) | | | |
| $Z_2$=(1-R) x (D>May) | -.085* | -.11 | - | - | - |
| | (.024) | (.76) | | | |
| (D>May) | .041 | -1.94* | -1.95* | -1.94* | -1.98* |
| | (.026) | (.73) | (.73) | (.73) | (.74) |
| (D>Sept.) | .030 | .02 | .00 | 02 | .01 |
| | (.025) | (.64) | (.65) | (.64) | (.66) |
| D | -.06 | -.43* | -.43* | -.41* | -.52* |
| | (.04) | (.14) | (.14) | (.14) | (.17) |
| R | .33* | 1.66* | 2.50* | 1.26* | 8.27 |
| | (.02) | (.51) | (3.11) | (.35) | (6.22) |
| Test $Z_1$=$Z_2$=0 | | | | | |
| F-value | 6.8* | .62 | - | - | - |
| (P) | (.001) | (.53) | | | |
| Sargan stat. | - | - | .28 | - | - |
| (P) | | | (.75) | | |
| Nb. Obs. | 7190 | 7190 | 7190 | 7190 | 7190 |

Source: National survey on tests taken at the beginning of 3rd grade, cohorts 1991-1994.
Note: R is a dummy indicating that the pupils live in a high-enrolment region whereas D indicates exact date of birth. All regressions include a full set of family social background dummies and cohort dummies as additional control variables. * indicates statistical significance at the 5% level. Standard errors are in parenthesis.

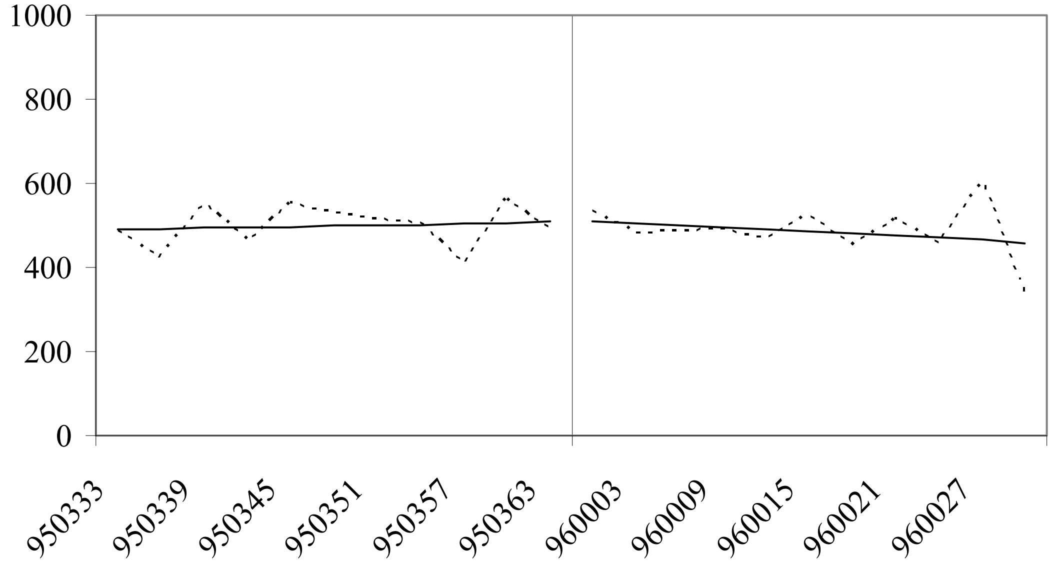
**Table 5**: The Effect of Early Schooling on Early Dropout Rates : TSIV Estimates

| | Early school (First-stage) | Dropout from school at age 17 | | Dropout from school at age 19 | |
|---|---|---|---|---|---|
| | | Reduced-form | TSIV | Reduced-form | TSIV |
| | (1) | (2) | (3) | (4) | (5) |
| Early school | - | - | -.012 (.018) | - | .065 (.034) |
| $Z_1$=R x (D>Sept.) | -.079* (.003) | .0016 (.0015) | - | -.0058* (.0028) | - |
| $Z_2$=(1-R) x (D>May) | -.012* (.002) | .0019 (.0013) | - | -.0024 (.0025) | - |
| (D>May) | -.156* (.002) | .0067* (.0013) | .0056 (.0030) | .016* (.0003) | .025* (.006) |
| (D>Sept.) | -.133* (.002) | .0050* (.0013) | .0037 (.0032) | .005* (.002) | .014 (.006) |
| D | .00049* (.00001) | -.0140* (.0007) | -.0140* (.0007) | -.0225* (.0014) | -.0225* (.0014) |
| R | .257* (.002) | -.0020* (.0008) | .0002 (.0044) | -.0032 (.0017) | -.0193* (.0083) |
| Test $Z_1$=$Z_2$=0 F-value (P) | 408* (<.0001) | 1.21 (.30) | - | 2.10 (.12) | - |
| Nb. Obs. | 681,892 | 755,866 | 755,866 | 679,119 | 679,119 |

Source: 1982 census (models 1) and 1999 census (models 2 to 5).
Sample: Children born in 1979 (models 1, 4 and 5) and children born in 1981 (models 2 and 3).
Note: R is a dummy indicating that the pupils live in a high-enrolment region whereas D indicates exact date of birth. * indicates statistical significance at the 5% level. Standard errors are in parenthesis.

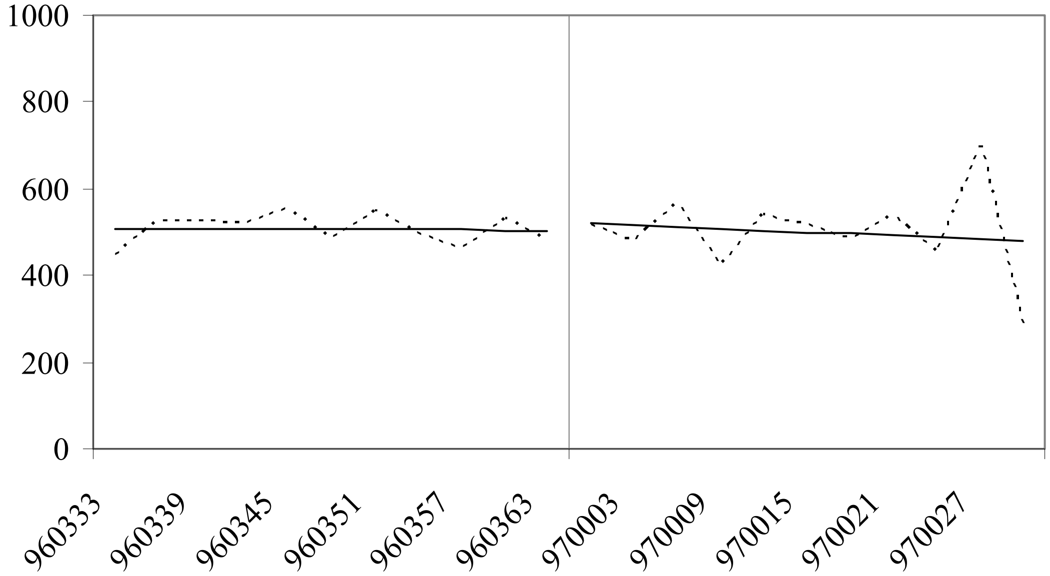
Appendix A

Figure A1 : Number of Single-Parent Families, By Youngest Child Date of Birth (95-96 cutoff)




Source: 1999 census.
Sample: Single-parent families whose youngest child was born between December 1995 and January 1996.

Figure A2 : Number of Single-Parent Families, By Youngest Child Date of Birth (96-97 cutoff)




Source: 1999 census.
Sample: Single-parent families whose youngest child was born between December 1996 and January 1997.