

Demand for Education and Labour Market Outcomes: Lessons from the Abolition of Compulsory Conscription in France

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Abstract

Before 1997, education was a way for young French men to avoid military service in the army. After the abolition of compulsory conscription in 1997, this incentive to stay on in education disappeared. We show that the decrease in the benefit of pursuing education for men was followed by a fall in their educational achievement relative to women and by a decrease in their relative entry wages. These results suggest that high-school dropout rates could be reduced by policies increasing the immediate benefits of pursuing education and that it would yield a substantial improvement in early labour market outcomes.

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I. Introduction

Before 1997, young French male adults were subject to a ten-month military service at the age of 18. However, in practice, the vast majority were reluctant to do their military service at that age. It was perceived as a loss of time and was associated with various costs: living in a barrack, sharing a room with five other men, participating in military training and drills. They preferred deferring their service for several years, mostly through pursuing education.

Education was indeed a way to avoid immediate military service in the army and to increase the probability of being selected into milder forms of the national service, that is, in technical assistance services, in the police or overseas. After the abolition of the national service, these specific incentives to stay on in education disappeared.

The first contribution of this paper is to show that this decrease in the benefits of pursuing education for men was followed by a significant decrease in their relative demand for education. When we compare behaviours before and after the reform, we observe a clear fall in the educational outcomes of men relative to women. Specifically, the reform was followed by a significant decrease in men's propensity to pursue education after the age of 16 or 17 and a decrease in their relative probability of high-school graduation. A large fraction of French male students (about 50 percent) are held back a grade or more in primary or junior high-school and are at the margin of dropping out from school when they finish junior high school, at the age of 16 or 17. The vast majority come from a low socio-economic background and our analysis suggests that their educational outcomes are significantly affected by changes in the benefits of pursuing secondary education.

The second contribution of the paper is to use the variation in men's demand for education induced by the reform to identify the impact of education on wages at the entry into the labour market. When we focus on men with a low socioeconomic background, we find that

the reform has had a significant negative impact on both relative education and relative entry wages. In contrast, we find no impact on the relative education nor on the relative entry wages of those with a higher socioeconomic background. These findings are consistent with the assumption that the national service, as such, had no direct impact on entry wages and affected the earning capacity of young workers only through its impact on their educational choices. Under this assumption, we find that the causal effect of education on entry wages is as large as the differences observed in cross section between the entry wages of educated and non educated workers. To the best of our knowledge, this paper is the first to provide an evaluation of the causal effect of additional education on wages at the moment of entry into the labour market.

Lastly, the reform is used to identify the impact on subsequent wages of pursuing post-compulsory education rather than accumulating early labour market experience. The impact of substituting formal education for early labour market experience is identified by comparing the relative outcomes of male workers coming from a low and a high socioeconomic background, before and after the reform, within each age group. This analysis suggests that each additional year of formal education causes a 13 percent increase in wages at each given age. It is comparable to the upper bound in studies which rely on reforms that increased the length of compulsory schooling in the fifties and the sixties (see Hamon and Walker 1995; Oreopoulos 2006).

Interestingly enough, the reform analysed in this paper is distinct in several ways from the institutional reforms often used to estimate the causal effect of education on wages (see for example Aakvik, Salvanes and Vaage 2003; Harmon and Walker 1995; Lemieux and Card 2001; Meghir and Palme 2005; and Gurgand and Maurin 2006). First, most of the existing literature relies on reforms which took place in the fifties or sixties. There may be some doubt about whether the returns to education that are identified through these old reforms are still

valid and could provide a framework for predicting the outcomes of further expansion of the educational system. Our paper contributes to this debate by identifying relatively high returns using a more recent reform. The question remains open, however, whether the effects on early labour market outcomes identified in this paper will persist over the life cycle.

Most importantly, the existing literature on the returns to schooling has focused mostly on supply-side sources of variations in schooling, attributable to such features as the minimum school-leaving age or the geographic proximity of schools (see for example Card 1995; Oreopoulos 2006). The policy reform used in this paper is different as it did not affect schooling outcomes through an expansion of the educational system and a general increase in the supply of education, but through a decrease in the benefits of attending school for a specific fraction of the population (i.e., men from a lower social background). Put differently, our paper is not the first to identify relatively important returns to additional years of education, but it uses a source of identification which is more recent than, and different in nature from most existing studies.

Finally, our paper provides potentially important lessons for countries considering the abolition of compulsory conscription. In Germany for instance, there is a long-standing debate regarding the abolition of the military service and the creation of a professional army. The same debate is taking place in Switzerland, a country with a large army and a long military tradition. Professionalising the army is also being considered (and planned for in the near future) in Croatia, Bulgaria, and Greece. In many countries (such as Greece, Croatia, Switzerland and Norway) education operates in similar ways as in France, in that it provides a means to defer the military service. This paper may thus be useful in providing some insights into the unexpected consequences of the abolition of compulsory conscription in countries where it is being considered.

The paper is organised as follows. The next section describes the 1997 reform of the national service. Section 3 shows evidence on the impact of the reform on education and section 4 on starting wages. In addition section 4 compares our findings with those of the existing literature on the effect of veteran status on workers' earning capacity. Section 5 proposes an evaluation of the causal effect of education on wages using the reform as a natural experiment. Section 6 provides some interpretations and discusses the policy implications and Section 7 concludes.

II. Data and Background

The data used in this paper come from the annual Labour Force Surveys (LFS) conducted each year from 1991 to 2002¹ by the French Statistical Office (hereafter, INSEE, Institut de la Statistique et des Etudes Economiques). The annual LFS sample is a large representative sample of the French population aged 15 or more (N=150,000 ; sampling rate=1/300). The survey provides information on the date of birth, sex, and educational level of each respondent, as well as the occupation of their fathers. It also provides information on the wages and activity status of each respondent, that is, whether s/he is employed, unemployed, still in education, on the military service or without specific activity. We also use a supplement of the LFS conducted by INSEE in 1997. Each year, a supplement (Enquête Complémentaire) is actually directed at a subsample of the respondents of the LFS. The supplement conducted in 1997, called "Jeunes et Carrières", focuses on about one third of the LFS respondents aged 19-45 (i.e., one third of those born between 1952 and 1978, N=20770) and provides additional information on their educational and occupational career. Interestingly enough, it provides us with information on whether and when the respondent did the national service, and on the type of the national service (for more details on the survey see Herpin and Mansuy 1995).

A. The French Military Service

Before 1997, all French male adults (and only males) had to do a 10-month military service at the age of 18. In practice, they had the right to defer their service until the age of 22 without a specific justification. According to the 1997 LFS supplement, only 10 percent of conscripts did their national service at the age of 18 or 19. The vast majority of conscripts deferred the service by pursuing education.

Before the reform, staying on in education was one way to avoid immediate national service and to increase the probability of accessing milder forms of the national service outside the army. The 1997 LFS supplement confirms that the proportion of conscripts doing a non-standard national service (that is, outside the army, either in the police or in technical assistance services or overseas) was significantly larger for high-school graduates (38 percent) than for high-school dropout (24 percent, see Table 1). Also, more educated conscripts had access to higher responsibilities during the service, regardless of whether they did their service in the army or not.

There was another incentive to delay the national service through pursuing education, rather than entering into the labour market. This is because getting a job before doing the national service would imply looking for a job twice: once before and once after the service. About 75 percent of the young men who did the national service and were employed before that, had to change employers and search for a new job upon return (see Table 1). Overall, data from the LFS show that about two thirds of individuals doing their military service at t , were still in education at $t-1$ whereas about 20 percent were unemployed, 7 percent held a temporary contract and only 8 percent a permanent one. These figures confirm that the vast majority were either in education or waiting for their call-up by the military administration, with weak links to the labour market.

B. The 1997 Reform

The discussion about the potential reform of military service in France was initiated by Jacques Chirac in 1996. The difficulties encountered by the French army during the Gulf War convinced him of the necessity of a reform. During the Gulf War the French force Daguet was stigmatised by its lack of mobility, its lack of autonomy vis-à-vis the American forces. It became clear that the large number of conscripts made the French army much too static and unable to defend French interests overseas. With French borders no longer under threat, the need for a large army had weakened and the country had thus to replace its conscripts by a smaller, more mobile and better performing professional army.

These concerns were potentially enough to reignite an old debate between a great share of the French left still attached to maintaining a democratic army (perceived as an inheritance of the French Revolution) and a fraction of the right which promoted the idea that defence had to be left in the hands of professionals². In fact this debate never took place. The reform was never discussed as a political choice of decisive and strategic character (Cluzel and Thibault 2005; Paquetteau 1997). Even the parties of the left, a priori the most hostile ones towards the idea of a professional army (especially the communist party), did not really defend the retention of the national service and they all favoured a deep reform of the compulsory conscription.

In fact compulsory military service was broadly perceived by the public as a pure waste of time. This year off - away from civilian and social realities - was all the more difficult to accept as it took place at a crucial moment of one's life, between the end of education and the entry into the labour market. At the beginning, the replacement of military service by civil service was considered, but the idea was abandoned. Conscription was finally suspended in 1997 and the military administration stopped using conscripts in August 2001. According to

the military administration, the number of conscripts was stable in the early nineties, about 200,000 which implies that about 40 percent -50 percent of each birth cohort was called up by the administration and actually did the national service. After Chirac's initiative in 1996, the number of conscripts declined rapidly from 202,000 in 1996 to almost zero in 2001 (see Figure 1).

III. Impact of the Reform on Education

The French LFS conducted between 1991 and 2002 makes it possible to construct representative samples of French people aged 17 to 23 for cohorts born between 1974 and 1979. This section builds on these data to compare the probability of being in education of males and females aged 17 to 23, before and after the reform of the military service. The issue is to test whether the reform actually increased men's incentives of early entry into the labour market relative to pursuing post-compulsory education³.

To begin with, Figure 2 shows the changes in the probability of being in education at age 17-23 across cohorts, by gender. It reveals a clear fall in the male relative probability for cohorts born after 1977. The relative probability of being in education at age 17-23 is about 2.5 percentage points lower for cohorts born in 1974-1977 relative to those born after 1977. Moreover we do not see any smooth relative declining trend for those born before 1977. Men born in 1978 were only about 18 years old in 1997 when the reform was introduced and when the risks of being called by the military administration began to decline rapidly. Figure 2 suggests that they were indeed the first significantly affected by this institutional change⁴.

Table 2 explores whether the shift in men's relative probability of being in education varies across the different age groups. Specifically, it shows the results of regressions comparing post-reform cohorts (1978-1979) and pre-reform cohorts (1974-1977) for four age groups: 16-17, 18-19, 20-21 and 22-23. It reveals a significant decrease in men's relative probability of

being in education for those aged 18-19 and 20-21, and even for the 16-17 years old⁵. In contrast, there is no significant shift for the older ones (22-23). After the reform, a significant fraction of young men leave school between the age of 16 and 18 rather than between the age of 21 and 23. Table A1 in the appendix provides a separate analysis of the proportion of men and women in education by age groups. It confirms that the proportion of men in education at the age of 18-19 or 20-21 decreased by about 2.5 percentage points after the reform, whereas it remained stable for women. As it turns out, the reform has mostly affected the behaviours of would-be early school-leavers.

A large fraction of French male students (about 50 percent) are held back one grade or more in primary or junior high-school (see for example Goux and Maurin 2005). They finish junior high-school at the age of 16 or 17 and have then to choose between direct entry into the labour market or additional secondary education either in a vocational or a general track. Those who choose additional secondary education have to study for three or four years in order to obtain some high-school degree⁶. The military service was providing them with incentives to pursue some secondary education and to complete a high-school degree. These incentives disappeared after the reform and thus their relative probability of being in secondary education between 18 and 21 decreased.

Table 3 shows the results from regressing three measures of educational level on a complete set of cohort-gender interactions and a complete set of cohort, age, and gender dummies as controls. The three educational measures used in these regressions consist of: a dummy which equals one if "still in education", a dummy indicating "high-school graduation", and a continuous variable measuring the years of education. This analysis confirms a significant decrease in men's relative probability of pursuing post-compulsory education after the reform and reveals that this decrease was accompanied by a significant shift in their relative probability of high-school graduation⁷. After the reform, pursuing

education does not provide a means to defer the military service or gain access to a better military service anymore and thus men's relative demand for education has decreased, as a result. Our findings are in line with those in Card and Lemieux (2001) who use differences in the risk of induction during the Vietnam War across cohorts of men, to estimate the effect of draft avoidance behaviour on rising college attendance rates in the mid-1960s in the US. Their results suggest that men facing higher induction risk, had also high rates of college enrolment and they were more likely to hold a college degree.

IV. Impact of the Reform on Entry Wages

We have just shown that, after the reform, a significant fraction of men leave school before the age of 18 rather than between 18 and 23. In this section, we ask whether this educational shift has affected their earning capacity at the entry into the labour market. To address this issue, we focus on the sample of LFS respondents who are observed leaving school between 16 and 23 and we compare their wages at the entry into the labour market, before and after the reform⁸.

Specifically, Table 4 shows the results of regressing their number of years of education, and their entry wages on a full set of dummies for their sex and birth cohorts⁹, and on an interaction of a sex dummy and a dummy indicating whether they were born just before (cohorts 1975-1977) or just after the reform (cohorts 1978-1979). The first regression confirms a significant decline in men's relative years of education for the cohorts born after the reform (-.35 of a year). Interestingly enough, the second regression shows that this decline was accompanied by a significant decline in their relative hourly wages at the entry into the labour market. Specifically, it shows that men's relative entry wages are 12.5 percent lower after the reform than before.

In the appendix (Table A2), we replicate Table 4 dropping 1978 from the set of post-reform cohorts and replacing 1977 by 1974 in the set of pre-reform cohorts. We obtain a very similar set of findings (see the first two columns of Table A2). These results still hold true when we use either only 1974-1975 or only 1975-1976 as control group (Maurin and Xenogiani 2005). Also we have replicated Table 4 using three supplementary pre-reform cohorts (from 1972 to 1974) and controlling for potential pre-reform trends in gender differentials (see Table A3). This analysis shows the same significant negative shift in male relative education and male relative wages at the entry into the labour market for post-reform cohorts as Tables 4 and A2. Figure 3 plots the effects of birth cohort on male relative education and (log) wages at the entry into the labour market, as estimated in Table A3. It makes it visually explicit that they followed a very parallel pattern across pre- and post-reform cohorts.

Overall, the reform was followed by a simultaneous decrease in the relative education and entry wages of male workers. This result may be interpreted as interesting new evidence on the causal effect of education on the earning capacity of individuals at the entry into the labour market. Another interpretation is that the national service had itself a significant positive effect on earning capacity, which disappears for those men who were affected by the reform.

A simple test of this hypothesis uses the fact that the reform has mostly affected men coming from a low socioeconomic background. One of the interesting features of the French Labour Force Survey is that it provides us with information on the occupation of the respondents' father. Specifically, it is possible to distinguish between respondents whose father was a manual worker (that is, relatively low socioeconomic background) and other respondents. Column 3 (5) of Table 4 shows the impact of the reform on the relative education of men, for individuals coming from a low socioeconomic background (for

individuals coming from a high socioeconomic background). Most interestingly, it shows that the reform had a very strong impact on the relative education of low socioeconomic background men (-0.47 of a year), but no significant impact on the relative education of men coming from a high socioeconomic background (-0.05 of a year). The reform has affected students who were at the margin of pursuing secondary education at the end of compulsory education and our data confirm that the vast majority of these students came from the poorest strata of the population.

These results provide a means to test whether the abolition of the national service, as such, has an effect on entry wages. If this assumption was true, we should observe a shift in the relative entry wages of men within the group of children coming from a high socioeconomic background, even though their relative education has not changed, following the reform. Column 6 of Table 4 shows no such impact. In contrast, the data confirm the existence of a significant and strong decline in men's relative hourly wages (-17 percent) within the group of those coming from a low socioeconomic background (column 4). We end up with similar results when we drop 1978 from the set of treated cohorts and replace 1977 by 1974 in the set of pre-reform cohorts (see Table A2, columns 3 to 6). Also, the findings are similar when we further drop either 1976 or 1974 from the control group (not reported here, see Maurin and Xenogiani 2005).

Hence, the reform affected neither the relative education nor the relative entry wages of men coming from a high socioeconomic background, whereas it affected strongly and negatively both the relative education and relative entry wages of low socioeconomic background men. These results are consistent with the assumption that the national service affected men's relative entry wages mostly through affecting their relative education. Under this assumption, the interaction between the sex of workers and their birth cohort (before/after

the reform) provides us with an instrumental variable to analyse whether the relation between education and entry wages is a relation of cause and effect.

The differences in entry wages across educated and non-educated workers are actually very large in France. Standard OLS estimates show that each additional year of education is associated with a 21 percent average increase in entry wages (see Table 5, column 1). The relationship is even stronger for early school-leavers, especially those with a low socioeconomic background. For example, when we focus on male workers who left school before the age of 19 and whose father is a manual worker, we find that each additional year of education is associated with a 36 percent increase in entry wages (Table 5, column 2). Column 3 of Table 5 uses the interaction between the sex of the worker and his/her birth cohort (post/pre-reform) as an instrument to explore whether this very strong relationship between entry wages and education is causal. Interestingly enough, the IV estimate is almost identical to the OLS estimate for early-school leavers¹⁰ (36 percent). Again, Table A4 shows similar findings when we drop 1978 from the treated group and replace 1977 by 1974 in the control group. The IV estimates remain significant and large when we use a different control group (i.e., 1974-1975 or 1975-1976). Also, our results are not significantly affected when we use additional pre-reform cohorts and when we control for a trend in the relative outcomes of men (see columns 3 and 4, Table A3). Overall, these findings suggest that the large difference observed between the entry wages of educated and non educated workers do not reflect differences in unobserved ability or circumstances, but the causal effect of additional education¹¹.

Before moving on to the analysis of the causal effect of education on subsequent wages, it is worth discussing whether our assumption about the effect of the military service on earning capacity is consistent with the existing literature. Angrist (1990) uses the introduction of the draft lottery during the Vietnam War as a natural experiment in order to get an estimate of the

causal effect of veteran status on earnings. His results suggest that the veteran status does affect earnings mostly because it constitutes a poor substitute for civilian labour market experience and only marginally because of its direct effect on individuals' earnings capacity. Angrist and Krueger (1994) exploit the fact that after 1942 men were drafted in chronological order of birth and they use quarter of birth as an instrument for veteran status. They find a negative effect of WWII veteran status on earnings and argue that the positive estimates of the WWII veteran premium found in the literature are entirely due to non random selection into the army. Angrist (1998) uses aggregate data to estimate the effect of voluntary military service on earnings. He finds that the long run gain in terms of civilian earnings is negative for whites and is negligible for non whites. Generally speaking, the impact of veteran status on earnings in this literature is difficult to interpret since it combines the pure effect of war, the loss of civilian labour market experience and the effect of the educational assistance program (G.I. Bill) designed to help members of the Armed Forces to adjust to civilian life after separation from service¹².

The compulsory national service under consideration in this paper does not render eligible to such specific programs and does not imply going through such a critical test as war¹³. In that sense, the study on Dutch data by Imbens and Van der Klaauw (1995) is closer in spirit to our work than existing studies on veteran status or the effect of WWII. They estimate the effect of Dutch military service on earnings, using aggregate data for the Netherlands. They use the variation in aggregate military enrolment rates induced by policy variation to deal with the issue of endogenous selection into the Dutch military service (due to both medical and psychological tests and the way exemptions from the army operate). Their results suggest a negative impact of the army on earnings, and suggest that these earning losses can be interpreted as the consequence of the corresponding losses in labour market experience.

All in all, the existing literature suggests that periods of military service as such have no significant impact on the individuals' earning capacity, the main effect on individuals' earnings being through losses of labour market experience. By comparing the effects of the abolition of the military service on different subgroups of young men (that is, sons of white versus blue collar), we end up with a very similar conclusion, meaning that the military service, as such, has no significant impact on the earning capacities of male workers. Put differently, the existence of the national service affected labour market outcomes mostly because it affected the incentives to pursue education¹⁴ and it is precisely the reason why its abolition provides us with an interesting tool for identifying the effect of post-compulsory education on labour market outcomes¹⁵.

V. An Evaluation of the Returns to Education

The reform of the national service has modified the distribution of education and entry wages in a way which is consistent with the assumption that education increases significantly the earning capacity of young workers at the entry into the labour market. It does not necessarily mean, however, that education increases earning capacity more than early experience into the labour market. This is nevertheless a key policy issue: is it really beneficial for young workers to substitute post-compulsory education for early labour market experience?

To be more specific, let us consider a student of age a and denote (i) w_0 his potential entry wage at age a , (ii) w_0+b his potential entry wage at age $a+1$, after one additional year of education and (iii) w_0+c his potential wage at age $a+1$ after one year of experience into the labour market (i.e., wage at $a+1$ after leaving school at age a). The parameter b is the impact

of one additional year of education on entry wages, the parameter c is the impact of one additional year of early labour market experience on wages and parameter $b-c$ represents the impact on wages of substituting one year of education to one year of early labour market experience holding age constant. The previous section provides an estimate of b and the issue is now to provide an estimate of $b-c$. It is still not clear whether the substitution of education for early labour market experience really increases wages within each age group.

The reform of the national service provides an interesting natural experiment to address this issue, because it has mostly affected the demand for education of men coming from a low socio-economic background. All men, regardless of their socio-economic background, have been affected by the suppression of the national service and the associated mechanical increase in labour market experience within each age group. However, men coming from a low socio-economic background have, on top of that, been affected by a specific additional substitution of early labour market experience for education. In such a context, the difference between the variations of male relative wages for workers coming from a high and a low socio-economic background provides an estimate of the effect of the substitution of early labour market experience for post-compulsory education that has specifically affected men coming from a low socio-economic background.

Table 6 focuses on the sample of all workers (not necessarily new entrants) and provides an analysis of their number of years of education¹⁶ and wages as a function of their sex, age, cohort of birth (before/after the reform), family background (children of low/high background), and all the possible interactions between these explanatory variables. The first column of Table 6 confirms that the reform was followed by a significant decrease in the relative number of years of education of men coming from a low socio-economic background (-0.6). The national service provided these students with an incentive to pursue education at a turning point of their schooling career, i.e., at the stage where they had to decide between

entering into the labour market and pursuing higher-secondary education. Most interestingly, the second column reveals that this educational shift has been followed by a significant decrease in their relative wages (-7.9 percent). Assuming that the effect of the reform on relative wages comes from its effect on relative education only, these reduced-form results suggest that the substitution of one year of secondary education for one year in the labour market increases wages by about 13 percent (that is, $0.079/0.60$). The last column of Table 5 shows a regression of wages on years of education using a dummy interacting sex, social background, and birth cohort as an instrumental variable. The IV estimate of the returns to one year of education is significant at the 6 percent level and (unsurprisingly) identical to our initial Wald estimate (13 percent). It is also very close to the OLS estimate of the return to education for male low-background early school-leavers (Column 4).

We obtain similar results when we drop cohort 1978 from the set of post-reform cohorts and/or replace cohort 1977 by cohort 1974 in the set of pre-reform cohorts (see Table A5 in the Appendix). We have checked that the IV estimates were similar when we further drop either 1976 or 1974 from the pre-reform cohorts (Maurin and Xenogiani 2005). Also the results are not significantly affected when we add an interaction between the sex of the respondent and a cohort trend as additional control variables.

Finally, Table 7 shows the results when we use an alternative measure of education, i.e., high-school graduation. The first-stage regression confirms the significant impact of the reform on the probability of high-school graduation (-0.13). The IV regression suggests that high-school graduation increases subsequent wages by about 61 percent at each given age. Given that it takes about four years of post-compulsory education to obtain this degree, this IV results suggest a 15 percent net impact for each additional year of secondary education, which is consistent with the evaluation in Table 6. We have also performed IV regressions

using occupational status rather than wages as dependent variable (not reported). They confirm a significant impact of education on early occupational status.

VI. Discussion

The existing literature on the returns to post-compulsory education relies on reforms which took place in the fifties or the sixties and consisted in general increases in the supply of education. Our paper contributes to this literature by identifying relatively high returns, using a more recent reform and a very different source of identification.

Generally speaking, our results suggest that the average gains from additional secondary education are very significant among individuals at the margin of pursuing post-compulsory education. Specifically, assuming a discount rate of 5 percent and a return to labour market experience of 3 percent, a 13 percent effect on wages at each age corresponds to a gain in permanent income which exceeds foregone earnings by a factor of about seven (i.e., $0.13/(0.05-0.03)$). Given this calculation, how can we account for the persistence of dropout behaviours? How can one reconcile high returns to education with low propensity to pursue education?

One possible explanation is that pursuing education is, as such, very costly for would-be dropouts. This cost can be both in terms of psychological pressure and financial cost. In fact in France, the vast majority of high-school dropouts have repeated one or two grades in elementary or junior high school. They are considered as second class students and may want to leave school as soon as possible, even at the expense of losing a significant amount of future money. Furthermore, the high cost of education combined with the financial constraints faced by poor families can possibly explain the dropout behaviour of young students. Poor families cannot borrow off of higher expected flows of income to help finance education for their children. Even access to banking services (such as checkbooks, cards,

credit) is very selective in France. A recent report published by the French National Observatory of Poverty reveals that the poorest families are much more likely to have their bank account opening or credit request denied (Gloukoviezoff 2004). Furthermore, even those who do have a bank account may have limited access to bank drafts. Another report, from the Ministry of Health, highlights the importance of access to credit in the fight against child poverty in France (Hirsch 2005).

Alternatively, young students may have low subjective discount rates, placing a low weight on the importance of education in determining earnings and thus be more reluctant to pursue further education. Moreover, as discussed in Oreopoulos (2006), they may ignore or mispredict the potential impact of education. Others may think that pursuing education is too uncertain as an investment. Finally cultural and peer pressure may play some role in encouraging dropout behaviour.

Regardless of the deep causes of dropout behaviour, one lesson from the recent French reform is that it is possible to reduce the numbers of dropouts by increasing the immediate benefit of pursuing education. It is difficult to be more specific without evaluating the utility gap (say, $v_E - v_S$) between pursuing education and doing immediately the national service. The first order impact of military service on the incentives to pursue education is indeed equivalent to that of a loan that would increase current utility by about $(v_E - v_S)$. Assuming that utility v is a function of the number of hours worked and of wages, we can set $v_{E=0}$ as a normalization and write $v_S = v(w_S; T_S)$ where T_S is the number of hours worked during national service and w_S the corresponding compensation. The conscripts received only some symbolic remuneration, but they were provided free medical care, food, and accommodation. According to the military administration, the cost of these various provisions was about 40,000 FF (about €6,000) per conscript in 1996, which amounted to half the French minimum wage. Put differently, the situation of a conscript can be thought of as roughly equivalent to

that of someone in low-skill employment, but receiving only half the corresponding wage. Hence, assuming that the conscripts did not suffer from costs other than the missing wages, the cost of the military service for each conscript was about half the minimum wage, meaning about 6,000 euros. Within this framework, the possibility of deferring the national service is equivalent to the possibility of obtaining a loan of 6,000 euros at age 18 in order to pursue education. Our data suggest that such a possibility diminished the proportion of high-school dropouts by about 12 percent.

These findings are in line with the recent evaluation of the Education Maintenance Allowance (EMA) introduced in September 1999 in 15 Local Education Authorities (LEA)¹⁷ in England. This policy aimed to raise participation, retention, and achievement in post-compulsory education among 16 to 18-year-olds. It consists of a means-tested allowance given to 16 to 18-year olds from lower income families (paid either directly to them or to their parents)¹⁸. An extended evaluation of the EMA was undertaken by a consortium of research organisations (Centre for Research in Social Policy, the National Centre for Social Research, the Institute for Fiscal Studies, and the Institute for Employment Research, 2002). The main conclusion is that the policy increased post-16 education participation by 3.8 percentage points on average (5.9 percentage points among the eligible). The maximum weekly allowance ranges between 30 and 40 pounds per week. If the full amount is awarded, this is equivalent to 1030 French Francs per month, that is, one sixth of the French minimum wage. Hence, we end up with the same significant impact on education by providing an annual allowance equivalent to about one sixth of the minimum wage or by providing a three years loan equivalent to one half of the annual minimum wage.

In France there already exist several types of financial help for young individuals who wish to continue their education. However, these are usually much lower than the amount calculated above. In particular, allowances for secondary education (*bourses du second degré*)

are provided to about 23% of students in junior high school, but only offer a maximum of 393 euros a year. Given our results, this seems much less than the minimum amount that would be necessary to have a significant effect on education participation. Thus, financial help should be more substantial, and be more focused on youngsters from disadvantaged backgrounds.

VII. Conclusion

Compulsory conscription in France was abolished in November 1997. Before the reform, staying on in education was a means to defer the national service and to get access to more interesting forms of the national service. After the reform, these incentives to pursue education disappeared. We find that the reform was followed by a significant decline in the relative proportion of men in education at age 18-21 and in their probability of high-school graduation. These findings suggest that a significant fraction of men born after the reform would have left school later if they were born some years earlier.

Additional investigations show that the reform had an impact mostly on students coming from a low socioeconomic background. Specifically, we find that the reform was accompanied by a significant decline in the relative education and the relative entry wages of men coming from a low socioeconomic background. In contrast, it had no significant impact on either the relative education or the relative entry wages of men coming from a high socioeconomic background. These findings are consistent with the assumption that the suppression of the national service affected workers' earning capacity only insofar as it affected their education.

We build on this assumption to provide an IV analysis of the causal effect of substituting education for early labour market experience on subsequent wages. We end up with estimates suggesting a 13 percent net impact for each additional year of post-compulsory education on wages. The reform was followed by a 0.6 decline in the relative number of years of education

and an 8% decrease in the relative hourly wages of men coming from a low socioeconomic background. Interestingly enough, the treated group in our paper is composed of persons at the margin of dropping out from school at the end of compulsory education, i.e. a group that would plausibly be affected by policies targeted at increasing the benefits of post-compulsory education.

One important lesson from the reform is that high-school dropout rates could actually be reduced through an increase in the immediate benefits of post-compulsory education. A significant fraction of adolescents living in poor families do not pursue post-compulsory education, even though pursuing education would be highly beneficial in the long run. These young unskilled workers have difficulties finding good and stable jobs and this represents a key social issue in France, as in most western countries. Any policy improving the psychological and material constraints for disadvantaged adolescents during their secondary education could significantly reduce their likelihood of schooling failure.

1 We cannot rely on the LFS conducted after 2002. This is because they are different from the surveys of previous years, which generates spurious shifts in the distributions under consideration. The new LFS take place every three months rather than yearly. They are mostly through telephone rather than personal interview and the new questionnaire differs from the old one. In particular the activity status is not identified through the same questions nor coded with the same variables. These changes have been implemented in order to improve the conformity of the French survey with the European standards.

2 See for instance the book written by De Gaulle in 1934, "Towards a professional army", (*Vers l'armée de métier*).

3 Compulsory schooling goes until the age of 16 and LFS confirms that about 99 percent of French youngsters are in school at the age of 16. Generally speaking, the results are very similar when we include respondents aged 16 in the analysis.

4 The LFS does not provide accurate information on the proportion of conscripts by cohorts since it is conducted in ordinary housing only (and not in military barracks). The LFS confirms nonetheless that the proportion of men who did the national service is almost zero for cohort 1979 and only about 19 percent for cohort 1978. In contrast, the LFS shows that the proportion of male respondents doing the military service was about 40-45 percent for cohorts 1975 and 1976, which is close to the administrative numbers. Cohort 1977 lies in-between with a proportion of about 30 percent.

5 The impact on the 16-17 year group suggests that a small fraction of people who would have left school after the age of 18 before the reform, are leaving school at the age of 16 or 17 after the reform (that is, without even pursuing post-compulsory education).

6 The Labour Force Survey suggests that the median age of high-school graduation is actually about 19-20.

7 Note that the different evaluations of the effect of the reform on education given in Table 3 are compatible with each other. For the sake of clarity, assume that the effect of the reform boils down to a three percentage points increase in male relative probability of leaving school at age 17 and a three percentage points decrease at age 22. In such a case, the average effect on enrolment probability between 17 and 23 is a little less than $3 \times (6/7) \approx 2.6$ percentage points whereas the average effect on respondents' years of education at age a is $0.03 \times (a-17)$. This yields an average effect of about $0.03 \times (7/2) \approx 0.11$ when age varies between 17 and 23. This is actually close to what Table 3 shows.

8 In this analysis, the school leaving age is defined as $t-1$ for workers observed at t who were in education at $t-1$ and as $t-2$ for workers who were in the military service at $t-1$.

9 Note that age cannot be included in this analysis since age at the entry into the labour market is simply another measure of the number of years spent in the educational system.

10 Comfortingly, this TSLS estimate is identical to the ratio between the relative decline of men relative wage after the reform (-12.5 percent) and the relative decline in their number of years of education (-0.35), i.e., the standard Wald estimate.

11 Notice that the effect of education on entry wages is different from the effect of substituting education for labour market experience and, consequently, it is not comparable to usual estimates of the returns to education.

12 See Angrist and Krueger (1994) or Bound and Turner (2002) for a discussion on this.

13 A related paper by Bauer et al (2006) evaluates the impact of military service in Germany.

14 The idea that the draft might affect earnings via channels other than the veteran status is not new. Angrist (1990) says that "it may be that the draft has affected education attainment and other career choices along with its effect on the military service". Baskir and Strauss (1978) suggest that during the Vietnam War men went to college to avoid the draft. Angrist and Krueger (1994) mention that "there is evidence that during the Vietnam War, college educated men from wealthy families managed to avoid the military service, whereas less educated- low income men were unable to do so".

15 Recent papers using large scale natural experiment for identifying the returns to education include Ichino and Winter-Ebmer (2004) and Oreopoulos (2006).

16 In this analysis, "years of education" are measured using available information on educational credentials. High-school dropouts is taken as reference (zero years), high-school graduation corresponds to four years, some college to six years and college graduation to eight years of schooling.

17 The selection of LEA areas to participate in the EMA pilots was not random. These were urban areas with relatively high levels of deprivation, low post-compulsory education participation and low levels of attainments in year 11. Other LEA's with similar characteristics were chosen as the control areas.

18 There were four different variants in the weekly amount young people received as a function of family income, the amount of retention and achievement bonuses and to whom the allowance was paid (young person or his parents).

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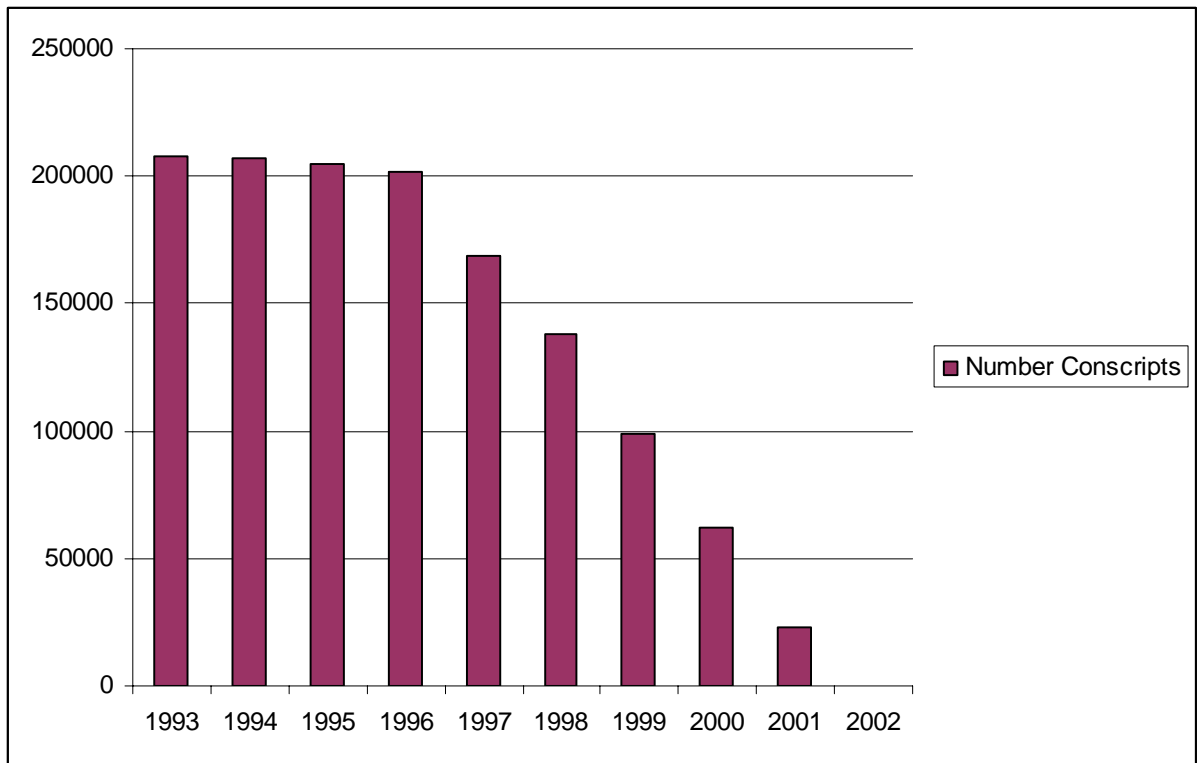


Figure 1

Decline in the Number of Conscripts after 1996

Source: French Ministry of Defence.

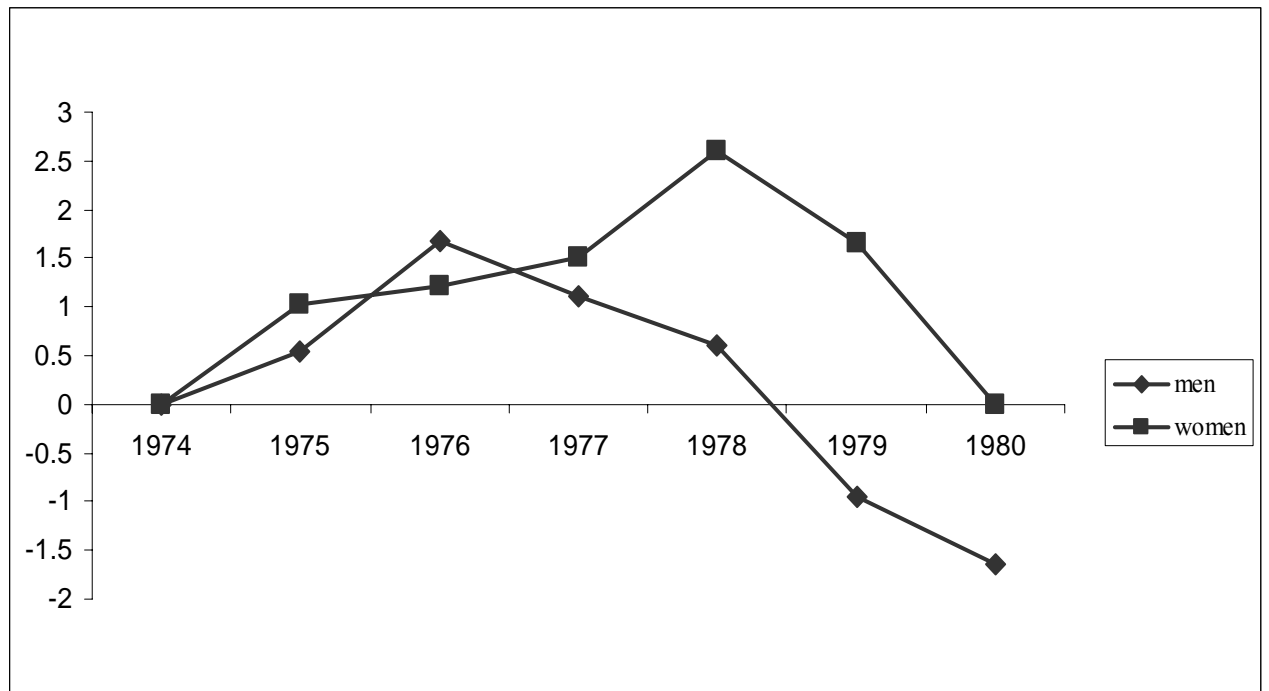


Figure 2

Changes in the Proportion of Men and Women Still in Education at the Age of 17-23 across Birth Cohorts

Source: Labour Force Surveys, 1991-2002.

Reading: For each cohort c between 1974 and 1979, the graph reports the differences between cohort c and cohort 1974 in the proportions of men and women still in education at age 17-23. For cohort $c=1980$, we report the difference in the proportions observed at age 17-22 (cohort 1980 is not observed at age 23).

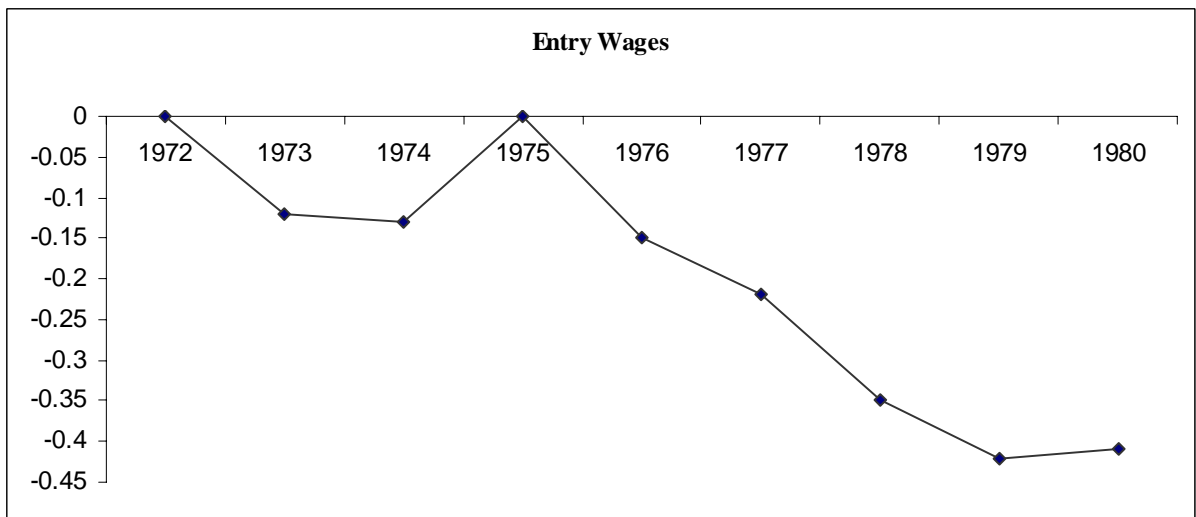
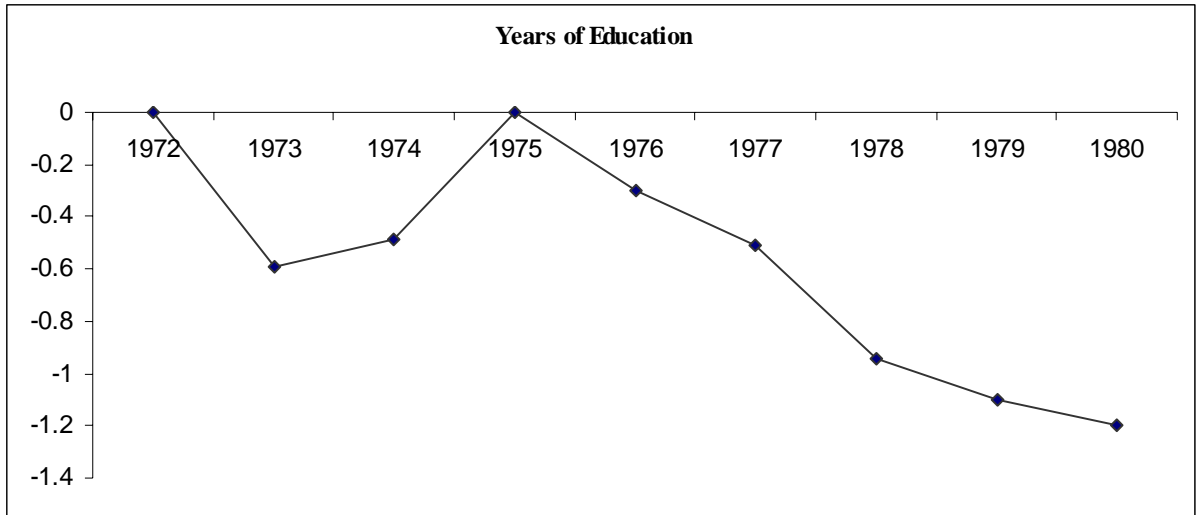


Figure 3

Cohort Effects on Male Relative Number of Years of Education and Wages at the Entry into the Labour Market

Source: Labour Force Surveys, 1991-2002.

Note: Cohort effects on male relative number of years of education and (log) entry wages. The detailed estimates can be found in Table A3.

Table 1

Basic Features of the French Military Service, by Education Group

	High-school (or more)	High-school dropout	All
Proportion of exemptions	0.30	0.29	0.29
Proportion of conscripts outside the army	0.38	0.24	0.28
Proportion of conscripts who did the service at the age of 20 or after	0.90	0.51	0.64
Proportion of conscripts who did not go back to previous job (among those who had a job)	0.80	0.72	0.75

Source: 1997 Supplement of the French Labour Force Survey (Enquête Jeunes et
Carrières, 1997)

Sample: Men aged 26-45.

Table 2

The Effect of Being Born After the Reform on Educational Outcomes, by Age Group

	Dependent Variable: Being still in education = 1			
	Age 16-17	Age 18-19	Age 20-21	Age 22-23
Male=1 *(cohort=78-79)	-0.011* (0.005)	-0.027* (0.009)	-0.024* (0.012)	-0.005 (0.013)
Additional controls: full set of sex, age and cohort dummies	yes	yes	yes	yes
R-squared	0.03	0.02	0.03	0.02
Observations	29585	29932	28990	27218

Source: Labour Force Surveys, 1991-2002.

Sample: Individuals born between 1974 and 1979.

Standard errors in brackets.

Table 3

The Effect of the Reform on Three Different Educational Outcomes

	In Education=1	High-school graduation= 1	Years of Education
Male*74	Ref.	Ref.	Ref.
Male*75	-0.004 (0.009)	0.004 (0.009)	-0.01 (0.05)
Male*76	-0.001 (0.009)	-0.006 (0.009)	-0.05 (0.05)
Male*77	-0.009 (0.009)	-0.004 (0.009)	-0.05 (0.05)
Male*78	-0.019 (0.009)	-0.015 (0.009)	-0.11 (0.05)
Male*79	-0.026 (0.009)	-0.020 (0.009)	-0.11 (0.05)
Additional controls: full set of cohort, sex and age dummies	yes	yes	yes
R-squared	0.20	0.18	0.28
Observations	101,021	101,398	101,398

Source: Labour Force Surveys, 1991-2002. Sample: Individuals born between 1974

and 1979, aged 17-23. Standard errors in brackets.

Table 4

The Reduced-Form Effects of the Reform on Male Relative Education and Wages at the Entry into the Labour Market

	All		Low socio-economic background		High socio-economic background	
	Years education	Entry Wages (log)	Years education	Entry Wages (log)	Years education	Entry Wages (log)
Male=1 *(cohort=78-79)	-0.35 (0.12)	-0.125 (0.036)	-0.47 (0.15)	-0.17 (0.04)	-0.05 (0.20)	-0.02 (0.06)
Additional controls: full set of sex and cohort dummies	yes	yes	yes	yes	yes	yes
R-squared	0.02	0.01	0.02	0.01	0.02	0.01
Observations	3935	3935	2868	2868	1067	1067

Source: French Labour Surveys (1991-2002)

Sample: New entrants into the labour market (private sector) who have left school between the age of 16 and 23 and were born between 1975 and 1979. Standard errors in brackets.

Table 5

The Effect of the Number of Years of Education on Entry Wages: an Evaluation

Using the Interaction between Post-Reform Cohorts and Gender as an Instrumental

Variable

	OLS (full sample)	OLS (subsample male early starters from low-background)	IV ^a
Years of education	0.21 (0.03)	0.31 (0.01)	0.36 (0.09)
Additional controls: full set of sex and cohort dummies	yes	Yes	yes
R-squared	0.49	0.58	0.23
Observations	3935	1111	3935

Source: French Labour Surveys (1991-2002)

Sample: New entrants into the labour market (private sector) who have left school between the age of 16 and 23 (except column 2, aged 19 or less) and were born between 1975 and 1979. Standard errors in brackets.

a. In column 3, the effect of years of education is identified using an interaction between a male dummy and a 1978-1979 cohort dummy as an instrumental variable.

Table 6

The Effect of Years of Education on Wages: an Evaluation Using the Interaction between Post-Reform Cohorts, Gender and Low Socioeconomic Background as an Instrumental Variable

	First stage	Reduced-form	OLS (full sample)	OLS (subsample male early starters from low- background)	IV
	Years of education	Wages (log)	Wages (log)	Wages (log)	Wages (log)
Years of education	-		0.020 (0.002)	0.125 (0.03)	0.13 (0.07)
[Male*Low*(1978-1979)=1]	-0.60 (0.26)	-0.079 (0.37)	-	-	-
Additional controls : cohort, age, sex, family background dummies and interactions ^a	yes	yes	yes	yes	yes
R-squared	0.23	0.58	0.59	0.41	0.33
Observations	8579	8579	8579	999	8579

Source: French Labour Surveys (1991-2002)

Sample: Full time private sector wage earners, aged 23 or less (except column 4, aged 19 or less) and born between 1975 and 1979. Standard errors in brackets.

a. A full set of pair wise interactions between cohort, gender and family background are used as controls.

Table 7

The Impact of High-School Graduation on Subsequent Wages: an Evaluation Using the Interaction between Post-Reform Cohorts, Gender and Low Socio-economic Background as an Instrumental Variable

	First stage	Reduced-form	OLS (full sample)	OLS (subsample Male early starters from low-background)	IV
	High-School graduation	Wages (log)	Wages (log)	Wages (log)	Wages (log)
High-School Graduation	-		0.09 (0.01)	0.74 (0.22)	0.62 (0.33)
[Male*Low*(1978-1979)=1]	-0.13 (0.04)	-0.079 (0.37)	-	-	-
Additional controls : cohort, age, sex, family background dummies and interactions ^a	yes	yes	yes	yes	yes
R-squared	0.17	0.58	0.59	0.41	0.44
Observations	8579	8579	8579	999	8579

Source: French Labour Surveys (1991-2002)

Sample: Full time private sector wage earners, aged 23 or less (except column 4, aged 19 or less) and born between 1975 and 1979. Standard errors in brackets.

a. A full set of pair wise interactions between cohort, gender and family background are used as controls.

Table A1

Proportions of Men and Women still in Education After and Before the Reform, by
Age Groups

Year of Birth	Age Groups			
	16-17	18-19	20-21	22-23
Panel A: Men				
a. 1974-1976	93.6 (0.3)	79.4 (0.5)	59.6 (0.6)	36.1 (0.6)
1977	93.6 (0.5)	80.4 (0.8)	58.7 (1.0)	34.6 (1.0)
b. 1978-1980	93.5 (0.3)	77.4 (0.5)	56.9 (0.6)	35.7 (0.6)
[a-b]	-.1 (0.4)	-2.0 (0.7)	-2.7 (0.7)	-.4 (0.7)
Panel B: Women				
c. 1974-1976	96.5 (0.2)	86.6 (0.4)	67.2 (0.6)	41.6 (0.6)
1977	97.3 (0.3)	88.9 (0.6)	67.1 (1.0)	40.6 (1.0)
d. 1978-1980	97.6 (0.2)	88.1 (0.4)	66.5 (0.6)	41.1 (0.6)
[c-d]	+1.1 (0.4)	+1.5 (0.7)	-0.7 (0.7)	-0.5 (0.7)
[a-b]-[c-d]	-1.2* (0.5)	-3.5* (1.0)	-2.0* (1.0)	-0.1 (1.0)

Source: Labour Force Surveys, 1991-2002.

Sample: Individuals born between 1974 and 1980.

Table A2

The Effect of the Reform on Male Relative Education and Wages at the Entry into the Labour Market (Alternative Specification: 1974-1976 versus 1979)

	All		Low socio-economic background		High socio-economic background	
	Years education	Entry Wages	Years education	Entry Wages	Years education	Entry Wages
Male=1 *(cohort=79)	-0.31 (0.15)	-0.14 (0.04)	-0.46 (0.18)	-0.195 (0.05)	-0.07 (0.25)	-0.02 (0.06)
Additional controls: full set of sex and cohort dummies	yes	yes	yes	yes	yes	yes
R-squared	0.02	0.02	0.02	0.02	0.02	0.01
Observations	2868	2868	2087	2087	1067	1067

Source: French Labour Surveys (1991-2002)

Sample: New entrants into the labour market (private sector) who have left school between the age of 16 and 23 and were born in 1974-1976 or in 1979.

Standard errors in brackets.

Table A3

The Effect of Doing the Military Service on Wages for Pre-reform Cohorts

	Reduced Form		OLS	IV ^a
	Years of Education	Entry Wages		Entry Wages
Years of education			0.20 (0.003)	0.24 (0.043)
Male=1 *Trend	0.16 (0.07)	0.053 (0.02)	-0.01 (0.004)	-0.01 (0.004)
Male=1 * (Cohort=72)	Ref.	Ref.		
Male=1 *(Cohort=73)	-0.59 (0.19)	-0.12 (0.06)		
Male=1 *(Cohort=74)	-0.49 (0.18)	-0.13 (0.05)		
Male=1*(Cohort=75)	Ref.	Ref.		
Male=1 *(Cohort=76)	-0.30 (0.23)	-0.15 (0.07)		
Male=1 *(Cohort=77)	-0.51 (0.28)	-0.22 (0.08)		
Male=1 *(Cohort=78)	-0.94 (0.33)	-0.35 (0.10)		
Male=1 *(Cohort=79)	-1.10 (0.40)	-0.42 (0.12)		
Male=1 *(Cohort=80)	-1.25 (0.46)	-0.41 (0.14)		
Additional controls: full set of sex and cohort dummies	yes	yes	yes	yes
F-statistic (P)	2.8 (0.04)	4.7 (0.003)		
R-squared	0.04	0.02	0.43	0.41
Observations	6400	6400	6400	6400

Source: French Labour Force Surveys, 1991-2002 Sample : New entrants into the labour market (private sector) who have left school between the age of 16 and 23 and were born in 1972- 1980. Standard errors in brackets.

a. The Fisher statistics tests the joint significance of the three post-reform interaction variables. These three variables are used as instruments in the IV regression.

Table A4
 The Impact of the Number of Years of Education on Entry Wages: an Evaluation
 Using the Interaction between Post-Reform Cohorts and Gender as an Instrumental
 Variable
 (Alternative Specification: 1974-1976 versus 1979)

	OLS (full sample)	OLS (subsample male early starters from low-background)	IV
Years of education	0.19 (0.004)	0.325 (0.011)	0.44 (0.16)
Additional controls: full set of sex and cohort dummies	yes	yes	yes
R-squared	0.45	0.435	0.23
Observations	2868	1128	2868

Source: French Labour Surveys (1991-2002)

Sample: New entrants into the labour market (private sector) who have left school
 between the age of 16 and 23 and were born in 1974-1976 or 1979.

Standard errors in brackets.

Table A5

The Impact of Years of Education on Wages: an Evaluation Using the Interaction between Post-Reform Cohorts, Gender and a Low Socio-economic Background as an Instrumental Variable (Alternative Specification: 1974-1976 versus 1979)

	First stage	Reduced-form	OLS (full sample)	OLS (subsample male early starters from low- background)	IV (1)	IV (2)
	Years of Education	Wages	Wages	Wages	Wages	Wages
Years education	-		0.020 (0.002)	0.06 (0.03)	0.115 (0.068)	0.088 (0.047)
[Male*Low* (Cohort 1979=1)]	-0.80 (0.31)	-0.090 (0.046)	-	-	-	
Additional controls : cohort, age, sex, family background dummies and interactions ^a	yes	yes	yes	yes	yes	yes
R-squared	0.23	0.58	0.54	0.41	0.33	0.46
Observations	6680	6680	6680	752	6680	8473

Source: French Labour Surveys (1991-2002)

Sample: Full time private sector wage earners, aged 23 or less (except column 4, aged 19 or less) born in 1974-1976 or 1979. Standard errors in brackets.

a: A full set of pair wise interactions between cohort, gender and family background are used as controls