Vive la Révolution! Long-Term Educational Returns of 1968 to the Angry Students

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The famous events of May 1968, starting with student riots, threw France into a state of turmoil. As a result, normal examination procedures were abandoned, and the pass rate for various qualifications increased enormously. The lowering of thresholds at critical stages of the education system enabled a proportion of students to pursue more years of higher education than would otherwise have been possible. For those on the margin of passing their examinations, additional years of higher education increased future wages and occupational levels. Interestingly, the effect is also transmitted across generations and is reflected in the educational performance of children.

I. Introduction

In May 1968 a conflict between students and the university authorities in Paris precipitated a series of events that would lead to mass student protests, the biggest national strike in the history of France, and the

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dissolution of Parliament. By mid-June, the revolution was over, and normal life resumed. However, there were important consequences for those taking examinations in that year. In the university context, exams became a central aspect of the bargaining process between students and the authorities, with the former successfully bargaining for exams to be less stringent than usual so as “to avoid harming students who have spent a lot of time struggling for a better university.”[1] There are numerous examples of delays and modifications to university examinations taking place in that one year. The important examination taken for the *baccalauréat* (success at which guarantees access to university), only involved oral tests in that year. As a result the pass rate for various qualifications increased enormously. We show that the lowering of thresholds at an early (and highly selective) stage of the higher education system enabled a significant proportion of persons born between 1947 and 1950 (particularly in 1948 and 1949) to pursue more years of higher education than would otherwise have been possible. This was followed by a significant increase in their subsequent wages and occupational attainment, which was particularly evident for persons coming from a middle-class family background. Using birth in an affected cohort as an instrumental variable (IV), we find that each additional year spent in higher education increases wages by about 14%. Finally, returns were transmitted to the next generation. We see a significant decrease in the probability of grade repetition for the children of the affected cohorts.

This article has similarities to several other studies that use a “natural experiment” affecting particular birth cohorts as an IV strategy to estimate the labor market returns to schooling (e.g., Harmon and Walker 1995; Lemieux and Card 1998; Aakvik, Salvanes, and Vaage 2003; Meghir and Palme 2005; Oreopoulos 2006) and to identify the relationship between parental education and that of their children (Chevalier 2004; Black, Devereux, and Salvanes 2005; Oreopoulos, Page, and Stevens 2006).[2] Unlike all other papers in this literature, the intervention is one-off, unexpected, and temporary: it has no consequences for cohorts coming after the 1968

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[1] For example, see the debates reported in the French newspaper *Le Monde*, May 17, 1968.

[2] Many studies have used an IV approach to estimate the returns to education, especially following Angrist and Krueger’s (1991) landmark study. See Card (2001) for a review of the methodological issues in this literature and many of the important studies. This article also has similarities to Currie and Moretti (2003), who examine the effects of maternal college education on infant health.

events, and the incentive structure of the education system is unchanged.[3] Most important, the experiment under consideration in this article consists of a widening in access to higher education. In contrast, most other papers that use a similar identification strategy (cited above) focus on interventions that affect years of compulsory schooling.[4] In fact, this is the first study to use an intervention occurring at a later stage to investigate consequences for the intergenerational transmission of human capital.

Where changes to compulsory school-leaving laws are used for identification, the treatment group (and hence those for whom returns to education are identified) are those who leave the education system with relatively low-level qualifications. In contrast, the treatment group in our article is composed of those on the margin of the higher education system. Interestingly, we find large returns to an additional year of higher education for this group—comparable to the upper bound in studies that rely on an intervention affecting the length of compulsory schooling.

Widening access to higher education is on the political agenda in many countries. The British government has recently set a target of 50% for the number of young people entering higher education by 2010, while the French government has set a target of 50% with respect to the number of university graduates by 2015. However, these policies remain highly controversial. One important question is whether the returns for those induced by a reform to get more higher education (i.e., the "marginal group") would be as significant as the (very large) observed wage gap between college and high school graduates. The effects of the events of 1968 on education and wages suggest that the answer to this question is not necessarily negative. This is a case where easier entrance examinations to the higher education system actually yielded substantial returns to "marginal" students and, subsequently, to their children. To our knowledge, this is the first study that estimates the returns to education of parents on their own outcomes and those of their children within the same framework.

This article has the following structure: in Section II, our descriptive section, we provide further details of the consequences of the events of

[3] It has been shown that reforms to the compulsory school-leaving age can affect the educational decisions of those not directly affected by the rule change (e.g., Lang and Kropp 1986).

[4] Card and Lemieux (2001) focus on the effect of a program designed to facilitate the further education of post–World War II veterans in Canada. Hence, this intervention also directly affects years of postcompulsory schooling. Aakvik et al. (2003) use multiple reforms in Norway to estimate returns to different levels of education (including higher education). There is a literature using college proximity as an IV approach to estimate returns to years of education (Card 1995). The main problem is that living near a college is potentially endogenous (Card 2001).

1968 for the examinations undertaken in that year, and we use our data to provide descriptive evidence of the cohorts affected. In Section III, we present our estimation strategy and provide estimates of the effect of higher education on wages and occupational status using two identification strategies: the first uses the fact that particular birth cohorts were affected by the relaxation of examination standards in 1968, and the second uses the fact that the impact was strongly linked to students' family background. Also, we provide estimates of the effect of parental education on that of their children using the same natural experiment. In Section IV, we discuss different possible interpretations of our results and argue that they are likely to reflect the human capital value of higher education rather than its signaling value. Finally, we draw together conclusions from the analyses.

II. Descriptive Section

A. The 1968 Events and the Examinations

In 1967 and 1968, a wave of student protest movements broke out across Europe, Japan, and the United States. This was the era of mass protest against politics and social values, about which much has been written. However, prior to May 1968, there was no mass student movement in France (Touraine 1971), and the events of that month took everyone by surprise. This is well encapsulated by the now famous editorial written in a French national newspaper in March of that year: "The French are bored. They are not taking part either directly or indirectly in the great convulsions which are shaking the world."[5] The spark that set off the dramatic sequence of events in France was the decision to close the University of Nanterre in Paris on May 2 (following months of conflict between students and the university authorities) and the heavy-handed response of the police to the subsequent protest at the Sorbonne. The protests quickly snowballed across the whole of France in university towns. Then workers became involved: "It was only in France that the revolt of the students got a response from the workers . . . that precipitated the biggest general strike in French history, paralyzing the economy" (Singer 2000, 133). Over 10 million French workers were involved in the strikes—roughly two-thirds of the French workforce. De Gaulle dissolved the national assembly and called for new parliamentary elections in June. These actions effectively ended the brief, dramatic "revolution."

In the midst of these events, university examinations became a central feature of the bargaining process between the administration and the students. The latter exerted considerable pressure for exams to be less stringent than usual. There was extensive coverage in the French newspaper

[5] *Le Monde*, March 14, 1968.

Le Monde of the general controversy surrounding the examination procedures of that year and notice of the numerous delays and modifications to procedures in different institutions throughout the country. The following quotations come from student representatives: "Exams do not have to be abolished: it would harm students who need their diploma"; "Exams do not have to be as hard as usual: it would damage the students who have spent a lot of time struggling for a better university."[6] The authorities seem to have given way to student demands regarding exams in that year, so as not to deny the value of the student movement.[7]

The examination for the *baccalauréat* in 1968 provides one very interesting and important example of the consequences of these decisions. The *baccalauréat* is "the symbolic national diploma both crowning the successful completion of secondary education and providing a passport for entry into higher education."[8] It can be seen as the first examination within the French system of higher education. It has a long and distinguished history from its foundation by Napoleon in 1808. Normally it involves several oral and written examinations and takes place over several days. Students must obtain the *baccalauréat* to advance within the system of higher education. Indeed, this qualification gives recipients an automatic right to attend university. In May 1968, it was announced that the examination for the *baccalauréat* would be postponed,[9] but this was superseded by a later announcement, which stipulated that there would only be oral examinations, to take place on the same day. Candidates were informed of their results on the day of taking the examination, thus implying that examiners from different places could not consult with one another before making a final decision over the grades (there is normally a national commission to harmonize marking procedures). As a result, the number of persons obtaining the *baccalauréat* in 1968 was about 30% larger than in the adjacent years, whereas the size of the corresponding birth cohorts was fairly stable over this period (see fig. 1).[10]

[6] Students attending the *commission des examens* at the Sorbonne, as reported in *Le Monde*, May 17, 1968.

[7] After the 1968 events, various university reforms were proposed by the new minister of education, Edgar Faure. The aim was to allow some restructuring of universities, give them more autonomy, and ensure a greater representation of academics and students on university and departmental councils. According to Ball (1979), nothing was done administratively and financially to facilitate the application of reforms, although he notes some success with regard to the decentralization of decision making.

[8] Quoted from the Embassy of France in the United States, http://www.info-france-usa.org/atoz/edu_fr.asp.

[9] This decision was made on May 24, the day the minister of education, Alain Peyrrefites, resigned from his post.

[10] An examination for entry to *terminale*, the year of preparation for the *baccalauréat*, was abolished in 1966. This led to an unusually high number of can-

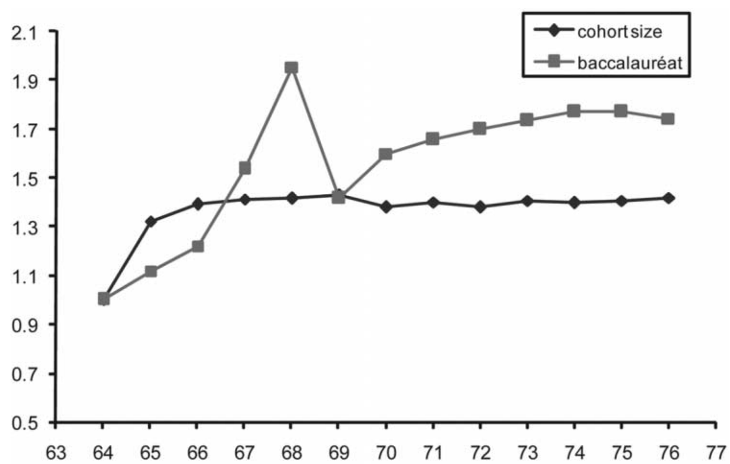

FIG. 1.—Trends in the number of *bacheliers* and in cohort size. The size of the cohort for year *t* corresponds to the number of persons born at $t - 19$ (19 is the median age of candidates). The two series are normalized to one in 1945. Source: French Ministry of Education (number of *bacheliers*) and the French Statistical Office (cohort size).

The lax examination procedures of 1968 were also important at the most selective stages of the university system, typically for students in the first 2 years (premier cycle). These first 2 years make it possible to obtain a university diploma (usually an upper-level technician diploma) and are essential in order to pursue a university degree (1 more year) or a masters (2 more years). We have made a systematic investigation of newspaper archives, and they are full of examples showing the difficulties faced by university administrators in organizing exams to be held at that time. In almost every university, it was not possible to organize regular examinations without protracted delays and adaptations resulting from the bargaining process between the students and the administration. For example, the *brevets de techniciens supérieurs* (upper-level technician diploma) was granted to students without any specific examination in 1968. Instead, the decision to confer this qualification was made on the basis of the candidate's work throughout the 2 years.[11]

B. The 1968 Events and Educational Credentials

The potential consequence of easier examinations is an increase in the final educational outcomes of the beneficiaries. Each year, a certain proportion of students (usually those who have already been held back a

didates for the *baccalauréat* in 1967, which explains the increase in the number of *bacheliers* in that year. Other changes to the *baccalauréat* are discussed in app. A, with reference to the Labor Force Survey.

[11] *Le Monde*, June 25, 1968.

grade) fail examinations in the early stages of higher education and drop out of the education system, whereas they could have stayed if the examinations had been easier. The effect of a significant relaxation of examinations on any given birth cohort is important if it contains a high proportion of such students at relevant stages of education in that year. Hence, we can speculate that the relaxation of the examinations in 1968 primarily affected students born between 1947 and 1950, that is, students who were at an early stage of higher education at that time.

In our analysis we choose 1946 and 1952 as comparison cohorts as they are less likely to be affected by the relaxation of examination standards in 1968.[12] Cohorts born in 1945 and earlier are less suitable for the comparison group since cohort sizes during the period of World War II were much smaller (whereas they were stable between 1946 and 1952). Also, they had to do a longer military service during the war in Algeria, which could have affected their demand for education and their subsequent labor market outcomes. Cohorts born in 1953 and after may have been affected by the Berthoin reform, which increased the compulsory school-leaving age by 2 years. However, in appendix B, we show that our basic findings are robust to alternative choices of comparison cohorts.

Educational and labor market outcomes for cohorts considered here (1946–52) can be illustrated using the French Labor Force Survey (LFS) for the years 1990, 1993, 1996, and 1999. These 4 years were selected because the sample rotates every 3 years, and we wish to observe each person only once. We only use surveys from 1990 onward because before that time information was not collected on wages. The LFS is a large representative sample of the French population of age 15 and above. There are around 10,000 respondents per cohort in our pooled sample. We focus on outcomes for male workers, leaving an analysis of the impact of the 1968 events on female outcomes for a separate paper. In appendix A, we provide a description of the LFS (with relevance to this article), in particular in relation to educational qualifications and destinations. Table 1 provides some descriptive statistics on the sample of male workers born between 1946 and 1952. We could have chosen the population census to do a more limited analysis of the effects of 1968. The census does not have information on wages or the social background of individuals. Furthermore, it is a lot less reliable than the LFS for measuring individual characteristics (notably education and date of birth). However, in appendix C, we show that a similar story is revealed with respect to available

[12] In contrast, broader effects of 1968 (e.g., being part of the student culture in which "revolutionary" activities occurred) might be expected to affect the comparison cohorts as much as the treated cohorts. However, as discussed below, we develop an identification strategy that does not rely on this assumption.

Table 1
Descriptive Statistics

Variable	Mean	Standard Deviation
Cohort dummy:		
1946	.128	.33
1947	.140	.35
1948	.145	.35
1949	.148	.35
1950	.145	.35
1951	.145	.35
1952	.148	.35
Education dummy:		
Less than *baccalauréat*	.718	.45
Baccalauréat only	.096	.29
University diploma (*bac* + 2)	.074	.26
University degree	.111	.31
Years of higher education	1.440	2.47
Wage (log)	9.170	.49
Middle-class family background	.246	.43
N	26,371	26,371

SOURCE.—Labor Force Survey 1990, 1993, 1996, and 1999.
NOTE.—Sample is male wage earners born between 1946 and 1952.

measures using the census. The larger sample sizes in the census allow much greater precision.

A detailed breakdown of the effect of 1968 is provided in table 2, which shows the percentage of male wage earners in each birth cohort within the following categories of educational attainment: the *baccalauréat* but no higher, any university qualification, a 2-year university diploma (*bac* + 2), and at least a degree-level university qualification (> *bac* + 2). A number of interesting facts emerge from this table. First, the percentage of workers with the *baccalauréat* only remains stable across cohorts. In particular, it is very similar in 1946, 1949, and 1952. In contrast, the percentage of workers with at least a university qualification is significantly higher for students born in 1949 than for students born in 1946 or 1952. Generally speaking, the majority of workers in the LFS with the *baccalauréat* subsequently achieve a university qualification, but the percentage is significantly higher for students born between 1947 and 1950 than for students in adjacent years. Taking the trend between 1946 and 1952 as a reference, the increase in the percentage of students with a university qualification is as follows: 1.3 in 1947, 1.5 in 1948, 2.7 in 1949, 0.8 in 1950, and 0.2 in 1951. Cumulatively, this is about 6.5% of a cohort (which is consistent with the increase in the number of *bacheliers* in fig. 1, taking the trend between the early sixties and the early seventies as a reference).

Thus, there is a significant shift in the percentage of workers with a university diploma (*bac* + 2), with a peak for the 1949 cohort (i.e., those age 19 at the time of the 1968 events). For those with a university degree

Table 2
Distribution of Education across Male Workers, by Year of Birth (%)

	Baccalauréat Only (1)	Greater than *Baccalauréat*: All (2)	Diploma (*Bac* + 2) (3)	Degree+ (> *Bac* + 2) (4)
1946	9.9 (.4)	17.4 (.6)	6.3 (.3)	11.0 (.5)
1947	9.0 (.4)	18.8 (.6)	7.1 (.4)	11.8 (.5)
1948	9.1 (.4)	19.0 (.6)	6.9 (.4)	12.1 (.5)
1949	9.8 (.4)	20.3 (.6)	8.6 (.4)	11.6 (.5)
1950	9.8 (.4)	18.4 (.6)	8.0 (.4)	10.4 (.5)
1951	9.8 (.4)	17.9 (.6)	7.5 (.4)	10.4 (.5)
1952	9.8 (.4)	17.8 (.6)	7.3 (.4)	10.4 (.5)

Source.—Labor Force Survey 1990, 1993, 1996, and 1999.
Note.—Sample is male wage earners. Standard deviation is in parentheses.

or higher (> *bac* + 2), the peak is for the 1948 cohort. Taken together, the most affected cohort is 1949, that is, those who were 19 years old in 1968.

In theory, students who are of "normal" age pass the *baccalauréat* at age 18. In practice, the majority of students repeat at least one grade when in primary or secondary school and pass the *baccalauréat* at age 19. This means that a significant proportion of the 1949 cohort was actually studying for the *baccalauréat* in 1968.[13] Specifically, about 10% of the 1949 cohort was at that stage in 1968. Given that the pass rate for the *baccalauréat* increased by about 30%, it is clear that the events of 1968 enabled a significant fraction of these students to cross the threshold for the *baccalauréat*, allowing them to pursue further years of education and potentially attain a university diploma.[14] It is also likely that another fraction of these students crossed the *baccalauréat* threshold earlier than they would have crossed it (i.e., in 1968 rather than 1969) and, because of this shortening of the time spent in high school, were subsequently able to achieve a university diploma rather than a *baccalauréat* only.[15]

There is also a group of students born in 1949 who were already at university in 1968. This smaller group, who passed the *baccalauréat* at age 18 or younger, constitutes about 7% of the cohort. The events of

[13] The French survey Formation et la Qualification Professionnelle (1993) confirms that the median age of candidates for the *baccalauréat* is 19 and shows that students studying for the *baccalauréat* in 1968 comprised about 36% from the 1949 cohort, 26% from 1950, 18% from 1948, 7% from 1947, and 8% from 1951. This analysis is available on request. See also app. 3 in Maurin and McNally (2005). The same survey shows that 55%–62% of these cohorts repeated at least one grade at school.

[14] A pass rate of about 30% applied to 10% of the 1949 cohort suggests that about 3% of the cohort would have been affected. This order of magnitude is consistent with that implied by col. 1 of table 2.

[15] This happens if the opportunity cost of remaining in the education system becomes more binding, the longer a person takes to complete the *baccalauréat*.

1968 made it easier for this group to achieve their university diploma and to subsequently obtain a university degree.

It is important to note that a negligible effect of the 1968 events on the proportion receiving the *baccalauréat* only does not mean that all those who would otherwise have failed the *baccalauréat* automatically earned a university qualification. For example, if the effect of the 1968 events on the 1949 cohort is to push 3% of the cohort from no *baccalauréat* to *baccalauréat* only and another 3% from *baccalauréat* only to university diploma (*bac* + 2), then we will observe no net change in the proportion of the cohort with *baccalauréat* only and will only see a positive net effect on those with a university diploma.

In the econometric approach, we largely focus on outcomes for the 1949 cohort as compared to the outcomes for the comparison cohorts of 1946 and 1952, who are unlikely to have been affected by the impact of the 1968 events on examinations. This approach will provide an evaluation of the average effect on wages of all the changes in educational achievement experienced by affected students who belong to this cohort.

C. Within-Cohort Effects of 1968

The cohorts born between 1947 and 1950 have been more affected by the 1968 events because they were more likely to take important examinations at this time and hence more likely to be at a highly selective stage of the education system, where the probability of failure is high. However, it is not the case that the members of these birth cohorts were exposed equally to the events of 1968. Specifically, only "marginal" students—those who would not have been able to pass the examinations if they had taken them 1 year before or after the 1968 events and who would not have been able to pass if they had repeated the exam year—will have been affected. These marginal students can be better identified by exploring the distribution of educational credentials by social background.

In the sixties and early seventies, most children from lower socioeconomic groups left school before taking the exam for the *baccalauréat*, whereas children from higher socioeconomic groups were likely to pass the *baccalauréat* and enter university.[16] Middle-class children were more likely than the former group to persist in the secondary school system but less likely than the latter group to pass the exams for entry into tertiary education.[17] For example, using the LFS, one can observe that the sons of middle-class fathers (*employés* or artisans and *commerçants*) were the most likely to pass the diploma taken at the end of junior school (*brevet*

[16] Only 10% of the sons of manual workers passed the *baccalauréat*, compared to about two-thirds of the sons of *cadres* (i.e., an upper-level white-collar position).

[17] As in other countries, there is evidence that educational attainment is strongly linked to family background in France (see, e.g., Maurin 2002).

Table 3
Percentage of Male Workers in Each Birth Cohort with at Least a University Diploma, by Social Background

	Social Background						
	Middle Class			Other Social Class			
Birth Cohort	Upper Level	Lower Level	All	Low (Manual Worker and Farmer)	High (White Collar)	All	Relative Odds Ratio
1946	21.8	15.7	18.2	8.8	47.2	17.1	1.00
1947	25.9	20.9	22.9	9.1	49.8	17.4	1.30
1948	25.6	22.5	23.8	10.1	44.1	17.4	1.37
1949	29.1	23.1	25.7	10.2	49.2	18.5	1.40
1950	27.4	17.9	21.7	9.6	45.4	17.4	1.21
1951	26.4	19.1	22.1	8.6	47.3	16.6	1.31
1952	22.9	20.2	21.3	9.3	44.9	16.6	1.12

NOTE.—Upper-level middle class corresponds to artisans and *commerçants* (i.e., shopkeepers, craftsmen, self-employed, etc.), whereas lower-level middle class corresponds to *employés* (i.e., nonmanual routine workers and clerks). If q_t denotes the probability of holding a university diploma for workers born in year t and coming from a middle-class family and p_t denotes this probability for workers from other social classes, then the relative odds under consideration can be written as $[q_t(1-p_t)/p_t(1-q_t)]/[q_{46}(1-p_{46})/p_{46}(1-q_{46})]$. The last column shows the changes in these relative odds across cohorts.

d'etudes du premier cycle) without subsequently passing the *baccalauréat*.[18] Hence, it follows that the marginal student is less likely to be from a lower socioeconomic group (who leave school without taking the *baccalauréat*) or a higher socioeconomic group (who have less difficulty in passing the exams). The marginal student is more likely to be middle class. Table 3 provides support for this hypothesis since the effect of the 1968 events is much more pronounced for male workers from a middle-class family background than for workers from other social groups. The difference in the log odds of holding a university diploma between workers from a middle-class family background and workers from any other type of family background increased by about .40 between 1946 and 1949 and then decreased by about .28 between 1949 and 1952 (see the last column of table 3). In other words, there is a notable peak in the log odds differential for the cohorts most affected by the 1968 events, which is consistent with these events having been particularly important for students from a middle-class family background.[19] Thus, our second econometric strategy will focus on variations in the educational gap between the sons of middle-class fathers and the sons of fathers from other socioeconomic

[18] The corresponding table is available on request. See also Maurin and McNally (2005).

[19] It should be acknowledged that the educational attainment of children from the other social classes increased slightly as well. Hence, evaluation relying on the change over time in outcome differences between middle-class children and other children involves making an assumption that the effect of education does not vary significantly by social background.

groups across birth cohorts and test whether these variations have been accompanied by similar variations in their labor market outcomes.

As a final robustness check, we use graduates from the top institutions of higher education (*grandes écoles*) as a comparison group since such students are well above any threshold of entry to higher education (whether stringent or not). In France, there are two distinct types of higher education institutions: universities and the system of *grandes écoles*. University entrance is open to all those with a *baccalauréat*, whereas access to the *grandes écoles* is much more selective. For entry to the *grandes écoles*, students must first be accepted into preparatory classes (*classes préparatoires aux grandes écoles*). Once admitted, the students undertake 2 or 3 years of work in an intensely competitive environment (half are eliminated and transfer to university before the end of that initial selection period). At the end of this preparatory stage, each remaining student takes several entrance examinations. The Labor Force Survey makes it possible to use a restrictive definition of *grandes écoles* (the top ones, about 0.7% of a cohort) and a less restrictive one (about 4% of a cohort). One key feature of this system is that the number of places available in *grandes écoles* is a *numerus clausus*, which does not change across cohorts. The proportion of each cohort attending *grandes écoles* remained almost the same across cohorts born between 1946 and 1952, regardless of whether we use a more or less restrictive definition. In such a context, our strategy will simply be to test whether the events of 1968 generated an increase in the relative labor market outcomes of those who were not selected into top institutions within affected cohorts. If the increase in labor market outcomes observed for the 1949 cohort is really due to the impact of the events on the educational achievement of students who were at the margin of passing the *baccalauréat*, then this increase should only be perceptible for people who did not attend the system of *grandes écoles*.

III. The Long-Term Educational Returns from 1968

A. The Estimation Strategy

We write the labor market outcomes (w_i) of worker i from cohort c_i at age a_i as follows:

$$w_i = \alpha n_i + \beta a_i + \gamma c_i + u_i, \tag{1}$$

where n_i represents years of education and u_i represents unobserved determinants of wages (such as ability). In what follows, w_i will represent either the wage earned or occupational status of i. Holding age constant, the cohort trend is equivalent to a time trend and captures the effect of a general increase in productivity on wages and occupational levels. We assume that years of education (n_i) vary in a nonlinear way across cohorts:

$$n_i = d_{47}C_{i47} + d_{48}C_{i48} + d_{49}C_{i49} + d_{50}C_{i50} + d_{51}C_{51} + \theta c_i + v_i, \quad (2)$$

where the C_{it} variables ($t = 47, \dots, 51$) represent cohort dummies and v_i is a random variable that captures the influence of unobserved ability on years of education. The d coefficients reflect the effect of the 1968 events on the minimum levels of ability that are required to pass the exams (see Maurin and McNally [2005] for the corresponding model of transitions in the school system). This effect varies across the different cohorts depending on the stage they were at in the education system in 1968. The potential correlation between v_i and unobserved factors affecting wages (as reflected in u_i) is the reason why estimation of equation (1) may be misleading without the benefit of the natural experiment, which is provided by the 1968 events.

In our empirical strategy, we first estimate the reduced-form effect of birth cohorts on labor market outcomes by substituting equation (2) into (1). If our understanding of the 1968 events is correct (and if α is positive), dummy variables for the 1948 and 1949 cohorts should have a significant positive effect in addition to the cohort trend. Second, we restrict our sample to the most affected cohort (1949) and two cohorts that appear to be unaffected (1946 and 1952), and we identify the return to education using C_{i49} as an IV.

This approach assumes that C_{i49} does not affect labor market outcomes other than through an individual's educational attainment. It might be argued that some students benefited directly from the experience of being involved in the student protests. If the benefits were different for students of age 19 (born in 1949) than for older or younger students (i.e., born in 1946 or 1952), the events might have generated differences in labor market outcomes across cohorts.

To account for the possibility that there is a direct effect of birth cohort on wages, we develop a second identification strategy that does not rely on cohort effects for identification. Given that the effects of the 1968 events are expected to vary by family background for the reasons discussed above, our second identification strategy involves using the interaction between birth in the affected cohort and coming from a middle-class family background. Hence, we augment equations (1) and (2) by including a cohort dummy for year of birth and a dummy for family background. Specifically, the wage of worker i from cohort c_i and family background f_i at age a_i is written:

$$w_i = \alpha n_i + \beta a_i + d_{c_i} + e_{f_i} + u_i, \quad (3)$$

where d_c represents cohort fixed effects, e_f is family background fixed effects, and the effect α of n_i on w_i is identified using the interaction

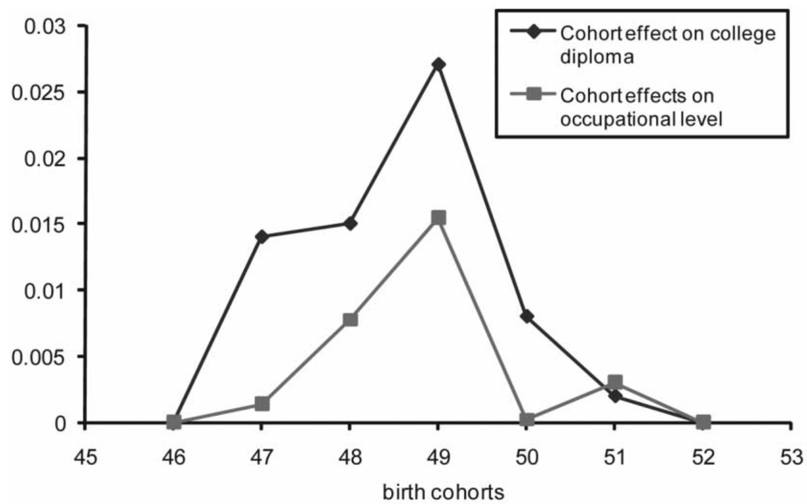

FIG. 2.—Net effects of birth cohort on the probability of holding at least a university diploma and on the probability of holding an upper-level white-collar position (*cadre*).

between a dummy variable indicating that the worker was born in 1949 (C_{i49}) and a dummy variable indicating that the worker comes from a middle-class social background (f_i = middle class) as the IV. This interaction term enables identification while also allowing for cohort effects to exert a direct influence on wages.

B. The Effects of 1968 on Education and Labor Market Outcomes

The essence of our results can be simply illustrated. Figure 2 shows the impact of birth cohort on the probability of obtaining a university diploma and the probability of holding an upper-level white-collar position (*cadre*) in ordinary least squares (OLS) regressions in which we control for a cohort trend and an age trend.[20] The birth cohorts are observed in LFS surveys between 1990 and 1999. Hence the occupation of survey participants reflects their position when in their forties. The pattern in the data shows a marked similarity for the two different effects and provides good prima facie evidence of the relationship between educational and labor market outcomes, with a pronounced upward shift for persons born in 1949.

A more comprehensive summary of the impact of birth cohort on the education and labor market outcomes of workers is provided in table 4.

[20] About 15% of the population of male workers in our sample hold these positions. Their wages are, on average, twice as high as the wages of other workers. In the French context, being a *cadre* may be interpreted as a proxy for permanent income.

Table 4
Impact of Birth Cohort on the Education and Labor Market Outcomes of Male Workers

	Baccalauréat Only (1)	At Least University Diploma (*Bac* + 2 or More) (2)	At Least University Degree (*Bac* + 3) (3)	Years of Higher Education (4)	Log Wage (5)	*Cadre* (Upper-White-Collar Occupation) (6)
1947	−.009 (.006)	.014 (.008)	.008 (.006)	.060 (.050)	.006 (.010)	.001 (.008)
1948	.007 (.006)	.015 (.008)	.012 (.006)	.080 (.050)	.031 (.010)	.008 (.008)
1949	−.001 (.006)	.027 (.008)	.009 (.006)	.150 (.050)	.021 (.010)	.016 (.008)
1950	−.001 (.006)	.008 (.008)	−.002 (.006)	.030 (.050)	.005 (.010)	.000 (.008)
1951	−.005 (.006)	.002 (.008)	−.001 (.006)	.010 (.050)	.003 (.010)	.003 (.008)
Trend	−.000 (.001)	.001 (.008)	−.001 (.001)	.005 (.010)	.010 (.002)	−.005 (.001)
Age	−.000 (.001)	.001 (.008)	.000 (.001)	.004 (.005)	.023 (.001)	.003 (.001)
N	26,370	26,370	26,370	26,370	26,370	26,370

Source.—Labor Force Survey 1990, 1993, 1996, and 1999.
Note.—Sample is male wage earners born between 1946 and 1952. Coefficients for the worker's cohort dummy are relative to the comparison cohorts of 1946 and 1952. Standard deviation is in parentheses.

Columns 1–3 show the probability of obtaining various educational qualifications for each birth cohort (relative to a reference group born in 1946 or 1952), controlling for a cohort trend and the age of the worker. The educational qualifications are *baccalauréat* only, at least a university diploma (*bac* + 2 or more), and at least a university degree (*bac* + 3 or more). We have converted qualifications to the equivalent number of years in higher education,[21] and this is the dependent variable in column 4. Then in columns 5 and 6, we report analogous regressions where the dependent variables are labor market outcomes—log wages and the probability of holding an upper-level white-collar position (*cadre*).

The results show a significant positive impact of belonging to cohorts particularly affected by the 1968 events (1949 and 1948) in terms of the probability of obtaining a university qualification or higher years of education and in terms of labor market outcomes. There is a wage premium of 2%–3% from having been born in 1948 or 1949. There is also a higher probability of obtaining a high-status occupational position (*cadre*) for those born in 1949.

In table 5, we show results from estimating the earnings equation and the probability of being in a high-level occupation (*cadre*) using OLS (cols. 1 and 3) and using birth in 1949 as an IV for years of higher education (cols. 2 and 4). In this case, only the cohort born in 1949 and the two comparison cohorts (1946 and 1952) are included in the analysis.

[21] We have constructed the years of higher education variable with a value of three for workers with *baccalauréat* only (the 3 years of upper-secondary education), 5 = 3 + 2 for workers with *bac* + 2, and 7 = 5 + 2 for workers with more than *bac* + 2.

Table 5
Evaluation of the Return to Education Using 1949 as an Instrumental Variable

	Log Wage		Upper-White-Collar Occupation (*Cadre*)	
	OLS (1)	IV (2)	OLS (3)	IV (4)
Years of higher education	.0940 (.0020)	.1400 (.0600)	.0970 (.0010)	.1030 (.0410)
Cohort trend	.0100 (.0020)	.0100 (.0200)	−.0056 (.0015)	−.0057 (.0017)
Age	.0230 (.0010)	.0230 (.0020)	.0034 (.0009)	.0033 (.0011)
N	11,171	11,171	11,171	11,171
R^2	.25	. . .	.36	. . .

Source.—Labor Force Survey 1990, 1993, 1996, and 1999.
Note.—OLS = ordinary least squares; IV = instrumental variable. Sample is male wage earners born in 1946, 1949, or 1952. Standard deviation is in parentheses.

The results indicate that each additional year spent in higher education increases wages by about 14 percentage points and increases the probability of holding a high-status position by 10 percentage points.[22] In appendix B, we show estimates obtained using different possible comparison groups. The results are very similar. Results are also robust to the inclusion of quadratic cohort trends.

When looking at the probability of holding a high-status position (*cadre*), point estimates from the IV and OLS approaches are very close. Moreover, the estimate is almost exactly the same (and estimated with greater precision) when the same approach is applied to census data (see app. C). In the earnings equation, the point estimate for years of higher education is higher in the IV regression than in the OLS regression, indicating a possible downward bias in the latter case (although it is not statistically different). A higher estimate in the IV specification is a fairly typical finding in the literature estimating the wage returns to education (Card 2001). It may be due to errors in the measure of education. Another explanation is that the effect of education varies across individuals or across grades and that IV and OLS estimators capture different aspects of the distribution of this effect. In our case, the IV strategy provides us with an estimate of the average causal response (ACR) as defined by Angrist and Imbens (1995), where there are three possible elementary changes in the intensity of the treatment: an increase from below to above the *baccalauréat*, an increase from below to above a university diploma,

[22] The results are qualitatively similar when we measure education by whether the worker holds at least a university diploma.

and an increase from below to above a university degree.[23] When the 1949 cohort is used as an instrument, the ACR captures a weighted average of causal responses to these elementary changes for the persons born in 1949 whose educational attainment has been affected by relaxation of the examination standards in 1968. As shown by Angrist and Imbens (1995), the ACR weights can be simply estimated from the difference between the empirical CDF of the educational attainment of persons born in the treated cohort (1949) and the nontreated cohorts (1946 and 1952). In our case, this difference is provided by table 2. It shows that the main effect of 1968 has been to shift about 2.5% of the 1949 cohort from below to above the *baccalauréat* and another 2.5% from below to above a university diploma (*bac* + 2). Given that the two elementary shifts are of equal magnitude, our IV estimates are close to the average effect of the two corresponding elementary changes.

We have replicated the analysis of table 5 using the 1948 cohort as an instrument (not reported). The effect of the 1968 events on the educational attainment of this cohort is different from their effect on the 1949 cohort since they have induced an increase in the proportion of persons with a university degree (> *bac* + 2) but not a change in the proportion of persons with a university diploma (*bac* + 2). Also, the effect on the proportion of those with a university degree is not very large (+1.3%). The reduced-form analysis shows that this educational shift was accompanied by a significant 3% increase in wages paid to persons from the 1948 cohort. This is consistent with the existence of a large effect of obtaining a university degree on the wages of workers born in 1948 who were at the margin of dropping out of university after the *baccalauréat*, even though it is not possible to obtain very precise IV estimates (details of this analysis are available on request).

C. The Effects of 1968 within Cohort

As discussed above, there may be a concern that cohort dummies should enter directly into the wage equation, if the 1968 events had a direct effect on wages (as well as through years of higher education) that varies across the treatment and comparison cohorts. As shown above, students born in 1949 from a middle-class family background were considerably more

[23] The ACR is a generalization of the local average treatment effect (Imbens and Angrist 1994), where the treatment is not binary. We must also assume that the treatment is a monotone function of the instrument. In our case, this amounts to an assumption that people born in 1949 complete at least as much schooling as they would have completed had they been born in 1946 or 1952. This identifying assumption is not verifiable but has the testable implication that the empirical cumulative distribution function (CDF) of the educational attainment of people born in 1949 does not cross (and actually dominates) the CDF of people born in 1946 or 1952. Table 2 shows that this is true.

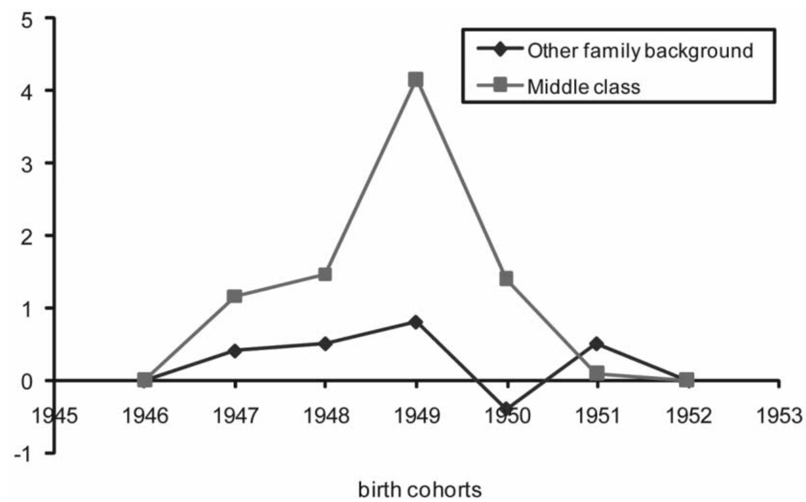

Fig. 3.—Net effects of birth cohort on the probability of holding an upper-level white-collar position (*cadre*) by family background.

affected by the consequences of the 1968 events on examinations than students born in the same year but from another social background. Given this fact, a simple test of our basic identifying assumption is to examine whether this is reflected in labor market outcomes. This is very simply illustrated in figure 3 (OLS regressions—by family background—where we control for birth cohort, a cohort trend, and an age trend). This shows that the peak for the 1949 cohort in terms of occupational status is much stronger for workers coming from a middle-class family background than for workers from other social groups.

Hence, we implement our second IV strategy, which uses the interaction between birth in 1949 and coming from a middle-class family background to identify the return to an additional year of higher education and thus enables the birth cohort dummy to enter directly into the wage equation. Regression results are shown in table 6. The first column shows the first-stage regression, where years of higher education are regressed against age, birth cohort, social background, and interactions between cohort and coming from a middle-class family background. The interaction term between birth in 1949 and coming from a middle-class family background is shown to have a positive and significant effect. Columns 2 and 3 show reduced-form regressions, where the dependent variables are log wages and holding an upper-level white-collar occupation (*cadre*), respectively. Again, positive and significant effects are shown for having been born in 1949 and coming from a middle-class family background. In columns 4 and 5, we present IV estimates for the two labor market outcomes using

Table 6
Effect of Years of Higher Education on Wages and the Probability of Holding an Upper-Level White-Collar Position (*Cadre*)

	First Stage	Reduced Form		IV	
	Years of Higher Education (1)	Log Wage (2)	Upper-Level White-Collar Occupation (3)	Log Wage (4)	Upper-Level White-Collar Occupation (5)
Years of higher education	. . .	. . .	. . .	.170 (.070)	.100 (.050)
Cohort:					
Middle × 1949	.340 (.120)	.060 (.024)	.034 (.020)	. . .	. . .
1949	.120 (.060)	.045 (.013)	−.005 (.010)	.020 (.020)	−.017 (.013)
Middle × 1952	.270 (.120)	.004 (.025)	.007 (.020)	−.040 (.020)	−.019 (.015)
1952	.040 (.070)	.074 (.015)	−.025 (.012)	.070 (.020)	−.029 (.010)
Age	.013 (.006)	.024 (.001)	.004 (.001)	.022 (.002)	.003 (.001)
Social background:					
High (upper level)	3.420 (.130)	.530 (.030)	.460 (.020)	−.070 (.260)	.120 (.170)
High (lower level)	1.680 (.120)	.350 (.030)	.210 (.020)	.060 (.130)	.040 (.090)
Middle (upper level)	.720 (.150)	.220 (.030)	.110 (.020)	.090 (.070)	.040 (.050)
Middle (lower level)	.360 (.140)	.190 (.030)	.050 (.020)	.120 (.050)	.010 (.030)
Farmer	−.030 (.120)	.030 (.020)	−.040 (.020)	.040 (.030)	−.030 (.020)
Manual worker	−.310 (.110)	.050 (.020)	−.050 (.020)	.100 (.030)	−.020 (.020)
N	11,171	11,171	11,171	11,171	11,171

Source.—Labor Force Survey 1990, 1993, 1996, and 1999.
Note.—IV = instrumental variable. Sample is male wage earners born in 1946, 1949, or 1952. The excluded category for socioeconomic background is "unknown." Standard deviation is in parentheses.

the interaction between 1949 and middle-class family background as an IV. The results are remarkably similar to those using the first identification strategy. In this approach, an additional year of education raises wages by 17 percentage points and the probability of holding an upper-level white-collar position (*cadre*) by 10 percentage points.

As discussed above, the events of 1968 have not modified the selectivity of the system of *grandes écoles* across the cohorts under consideration. Given this fact, a simple robustness check of our within-cohort analysis is to make sure that cohort effects are not perceptible for *grandes écoles* graduates. Interestingly, our data are consistent with this prediction. We have estimated the impact of being born in 1949 rather than 1946 or 1952 on labor market outcomes, separately for *grandes écoles* graduates and non-*grandes écoles* graduates (not reported). This analysis confirms a significant positive effect of being born in 1949 for people who are not *grandes écoles* graduates. However, it does not show any positive effect for *grandes écoles* graduates. This result holds true, regardless of whether we adopt a more or less restrictive definition of *grandes écoles*.

Generally speaking our estimates of the effect of an additional year of higher education on workers' outcomes are close to those reported in studies that use reforms to the French system of secondary education or exogenous changes in the benefits of pursuing secondary education in France as a source of identification (see Gurgand and Maurin 2006 or Maurin and Xenogiani 2007). This suggests that the return to an additional year of higher education may be at least as large as the return to an additional year of secondary education. The next question is whether this gain in education and earnings for the affected cohorts had any impact on the outcomes of their children.

D. The Effects of 1968 on the Next Generation

An important policy issue is whether providing additional education to parents (and resources more generally) is transmitted to the next generation. We have an opportunity to consider this issue by comparing the educational outcomes of children of cohorts affected by the impact of the 1968 events on examinations (1948 or 1949) with outcomes of children of the adjacent unaffected cohorts (1946 or 1952). Our empirical approach is similar to that described above in relation to labor market outcomes.

We consider the sample of 15-year-olds who are observed in the Labor Force Survey between 1990 and 2001. We have information on their date of birth and their grade and the date of birth, education, and occupation of their father. Our primary measure of school performance corresponds to children's educational advancement at age 15. It describes the difference between the actual grade of the student and the normal grade for his or her age group (i.e., the ninth grade). This takes a value of one when the student is 1 year ahead (tenth grade), zero when of "normal" age, −1 when 1 year behind (eighth grade), and −2 when 2 years behind (seventh grade). The distribution for children in this sample is as follows: 54.7% are at normal age, 3.1% are 1 year ahead, 29% are 1 year behind, and 13.2% are 2 years behind. We will also present results when using a dummy variable to indicate whether the 15-year-old has been held back at least two grades at school.[24]

Similar to Oreopoulos et al. (2006), the educational outcomes we consider are related to grade repetition. Grade repetition is a widespread phenomenon in many countries and is correlated with other measures of educational achievement. The Programme for International Student Assessment conducted by the Organization for Economic Cooperation and Development shows that 15-year-old French students who have repeated a grade are likely to obtain much lower scores in math, reading, or science

[24] The results are qualitatively similar when we use a dummy indicating that the child is 1 year ahead or a dummy indicating that he or she is at least 1 year behind.

Table 7
Relationship between the Father's Cohort of Birth and His Resources or His Child's Performance at School

	Father's Resources		Children's Performance	
	Years of Higher Education (1)	Upper-Level White Collar (*Cadre*) (2)	Actual − Normal Grade (3)	Two Grades Behind (4)
Dummy for father's cohort:				
1947	.170 (.100)	.000 (.020)	−.006 (.030)	−.006 (.013)
1948	.290 (.090)	.043 (.014)	.097 (.027)	−.043 (.011)
1949	.220 (.080)	.022 (.013)	.070 (.025)	−.031 (.011)
1950	.120 (.080)	.016 (.013)	.034 (.024)	−.010 (.010)
1951	.090 (.080)	.010 (.013)	.052 (.025)	−.010 (.010)
Father's cohort trend	−.060 (.020)	−.013 (.002)	−.003 (.005)	.000 (.002)
Child's date of birth	.070 (.010)	.007 (.001)	.026 (.002)	−.011 (.001)
Male	.010 (.050)	.010 (.010)	−.170 (.010)	.056 (.006)

Source.—Labor Force Survey 1990–2001.
Note.—Sample is adolescents age 15 and fathers born between 1946 and 1952. Coefficients for the father's cohort dummy are relative to the comparison cohorts of 1946 and 1952. Standard deviation is in parentheses.

than those who progress to this point without grade repetition. In terms of standard deviations, the difference is equivalent to about 1.14, 1.26, and 1.17 in math, reading, and science, respectively (Murat and Rocher 2003). In the French context, this outcome can be considered as an appropriate measure of school performance.

The first two columns of table 7 show the effect of the birth cohort of the father on parental resources, where the measures of parental resources are the father's years of higher education (col. 1) and whether the father is in an upper-level white-collar occupation (*cadre*; col. 2). In columns 3 and 4, we estimate the effect of the father's birth cohort on children's educational advancement. The results show a strong positive relationship between birth in an affected cohort and the father's resources—whether this is measured by his years of higher education or his occupational status.[25] As before, the strongest effects are found for the cohorts born in 1948 or 1949. In columns 3 and 4, we show that this translates into better school performance for their children (see fig. 4). The continuous measure of educational advancement (in col. 3) suggests that children of the affected cohorts are more likely to be on target or ahead in the school system at age 15 in comparison with the two cohorts that have been unaffected by the impact of the 1968 events on examinations (1946 and 1952). In column 4, results show that children of fathers born in these years are less likely to repeat two grades by 4.3 percentage

[25] For the subsample of fathers, we obtain similar results as for all males when birth in an affected cohort (1948 or 1949) is used as an instrument to estimate the causal impact of years of higher education on occupational outcomes.

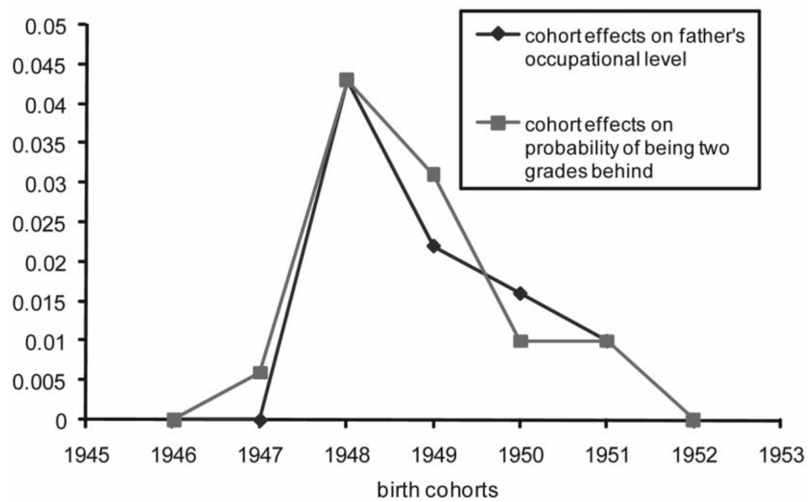

FIG. 4.—Effects of birth cohort on fathers' probability of holding an upper-level white-collar position and on children's probability of being two grades behind.

points and 3.1 percentage points, respectively.[26] Thus, it would appear that the benefits of the 1968 events were transmitted to the children of the affected students.

In table 8, we report regressions where cohort dummies denoting whether the father was born in 1948 or 1949 are used as IVs for the father's years of higher education. The dependent variable is educational advancement, as measured by the difference between the child's actual grade at age 15 and the normal grade for this age group. We report the results from two IV strategies, where birth in 1948 is used as the instrument in the first approach (col. 4) and birth in 1949 is used in the second approach (col. 5). The IV regressions consist of those cohorts born in 1946, 1952, and either 1948 or 1949, depending on the instrument used. In column 1, we report results from the first-stage regression, where the father's years of higher education are regressed on birth cohort dummies, trends for his own birth cohort and that of his child, and the gender of his child. As before, there is a strong positive relationship between years of higher education and birth in a cohort affected by the impact of the 1968 events on examinations (1948 and 1949), relative to birth in the comparison cohorts (1946 and 1952). In column 2, we show the reduced-form regression where the child's educational advancement at age 15 is the dependent variable (similar to table 7). Then in column 3, we report

[26] The average probability of being at least two grades behind at age 15 is about 11% for the children of fathers from cohorts 1946–52.

Table 8
Evaluation of the Impact of the Father's Education on Children's Performance at School

	Father's Education	Actual − Normal Grade			
	First Stage (1)	Reduced Form (2)	OLS (3)	IV ($Z = 1948$) (4)	IV ($Z = 1949$) (5)
Father's education	. . .	. . .	.076 (.004)	.330 (.120)	.320 (.150)
Father's cohort = 1948	.290 (.090)	.097 (.027)	. . .	. . .	. . .
Father's cohort = 1949	.220 (.090)	.071 (.025)	. . .	. . .	. . .
Father's cohort trend	−.060 (.020)	−.004 (.005)	.004 (.005)	.010 (.010)	.010 (.010)
Child's cohort trend	.004 (.005)	.030 (.003)	−.023 (.003)	.006 (.012)	−.001 (.014)
Gender of child (male)	.004 (.005)	−.160 (.020)	−.160 (.020)	−.160 (.030)	−.200 (.030)
N	5,087	5,087	5,087	3,710	3,804

SOURCE.—Labor Force Survey 1990–2001.
NOTE.—IV = instrumental variable. Regressions include all children whose fathers were born in 1946, 1948, 1949, and 1952 (first-stage, reduced-form, and ordinary least squares [OLS] regressions in cols. 1–3); 1946, 1948, and 1952 (col. 4); and 1946, 1949, and 1952 (col. 5). When $Z = 1948$ (1949), the 1948 (1949) cohort dummy is used as an instrument for the father's education. Standard deviation is in parentheses.

the OLS regression, before showing regressions using the two IV strategies in columns 4 and 5.

The regression results suggest a strong causal relationship between fathers' educational resources and the school performance of their children. Most interestingly, the size of this effect is almost exactly the same whether we use 1949 or 1948 as an IV.[27] Results are qualitatively similar when we use other measures of educational advancement, such as the probability of being 2 years behind at school. Results are qualitatively similar to Oreopoulos et al. (2006) in that larger effects are estimated when using the IV approach. Finally, similar results are obtained if we use the interaction between cohort 1949 and whether the father comes from a middle-class family background as an IV.[28]

[27] One potential issue is that cohort effects potentially capture the effect of the father's age as well as a time trend. This may be a problem if the effect of the father's age is nonlinear and not fully absorbed by the cohort trend. We have checked that our results are robust to the inclusion of a set of dummies indicating the age of the father (inclusion of age dummies does not affect the cohort dummies but absorbs the negative cohort trend in the father's education equation). Also, we obtain almost exactly the same results when we replicate the analysis on the subsample of children whose father is 44–49 years old. Given our window of observation (1990–2001), this sample is representative of children born to men between the ages of 29 and 34 for all the cohorts (1946–52). By construction, there is no correlation between the father's cohort and the father's age within this sample (detailed results available on request).

[28] However, in this case, the results are less well estimated. They are available upon request. Also, a falsification test focuses on the subsample of fathers who graduate from *grandes écoles* (as discussed in Sec. II.C). In this case, we do not

It is likely that fathers' education is not the only family resource whose distribution across children has been affected by the 1968 events. Since similarly educated men and women are more likely to marry each other, the distribution of mothers' education has plausibly been affected, and our IV estimate should be interpreted in this light. We have checked that children of fathers born in 1948 or 1949 are more likely to have highly educated mothers than children born in the comparison cohorts of 1946 and 1952 (not reported; see Maurin and McNally 2005). Given this fact, the estimated impact of the father's years of higher education reflects both the direct effect of his resources and the indirect effect of his wife's resources on the educational performance of children.[29] When years of higher education of mothers and fathers are entered into the regression separately, OLS estimates suggest a similar order of magnitude for the two effects. These estimates will have a similar endogeneity bias if the unobserved attributes of children are similarly correlated with the education levels of both parents. Under this hypothesis, the true causal effect of paternal and maternal education is close in value and can be identified using the average level of education of mothers and fathers as a measure of family resources. When we use this measure, the IV estimate is close in value to that estimated when fathers' education is used. However, in this case the gap between the OLS and IV estimates is reduced.

IV. Interpretation of Findings

An important issue is whether the higher wages paid to more educated workers reflect the effect of education on human capital and productivity or simply the fact that education acts as a signal of the productive ability of workers. If we assume that employers are able to distinguish between the productive contribution of workers born in a given year and the contribution of those born 3 years earlier or 3 years later (because they do not have the same age and may not be at the same stage of their career), then the wage effect of being born in 1949 rather than in 1946 or 1952 cannot be interpreted as reflecting the signaling value of additional education received by persons born in 1949. In such a case, the evaluation of the return to education relying on the comparison between the 1949 cohort and the 1946 or 1952 cohorts plausibly captures the effect of

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find any differential effect of being born in the 1949 cohort on fathers' educational attainment (or on the probability of grade repetition of their children), which is consistent with our identifying assumption.

[29] If α_f denotes the true effect of fathers' education and α_m denotes the true effect of mothers' education, our IV estimates actually correspond to $\alpha_f + \alpha_m\gamma/\pi$, where γ and π are the effects of the instrument on mothers' and fathers' education, respectively.

education on human capital.[30] Put differently, if education were only a signal providing information on the rank of workers within the ability distribution of their cohort, we would not observe any difference between the average wage of workers born in 1949 and that of workers born in 1946 or 1952.

We also produce estimates that rely on comparisons between subgroups born the same year but coming from different family backgrounds. If we assume that employers are not able to distinguish the contribution of workers born in the same year, even when they come from a different family background, these within-cohort evaluations potentially capture both the signaling and the human capital effects of education. Under this assumption, the fact that within-cohort and between-cohort evaluations provide us with similar estimates of the return to education suggests that signaling effects are small.[31] This conclusion is consistent with the recent analysis provided by Chevalier et al. (2004) for the United Kingdom. However, research is needed to explore the extent to which employers are actually able to identify the productive contribution of workers of different ages and whether this information is used for decisions about hiring and wages.

As discussed above, one interesting feature of our natural experiment comes from the fact that the shift was unexpected and short-lived. In particular, the incentive structure of the educational system remained unchanged (which would not have been the case if we had studied a reform leading to a permanent increase in the supply of higher education). Hence, the difference in labor market outcomes between workers born in 1949 and workers born in 1946 or 1952 can be interpreted as the pure effect of additional years at university on workers' human capital, that is, effects that have not been mitigated by changes in student or teacher behavior.[32] These estimates do not allow an evaluation of what would have been the

[30] Another argument in favor of the human capital interpretation is that employers plausibly had good information on the educational consequences of 1968 (the ease of exams in that year was well known). In such a case, there is no reason for the average labor market outcomes to be higher for the 1949 cohort except because of an increase in their human capital.

[31] Another interpretation is that employers are actually able to identify the contribution of different subgroups within cohorts and that the within-cohort evaluation also provide us with an estimate of the effect of education on human capital.

[32] Assuming a discount rate of 5% and a return of 3% for each year of labor market experience, a 15% effect on wages corresponds to a gain in permanent income that exceeds private costs (foregone earnings) by a factor of about seven (i.e., .15/[.05 − .03]), and that exceeds the social costs of 1 year of education by an even larger factor. In France, the annual per student cost of higher education is about $8,000. This is lower than annual earnings for a worker earning the minimum wage.

outcome of a permanent relaxation of exam pass requirements (since such a permanent relaxation may affect incentives and behavior) but an evaluation of the large human capital effect of enabling the marginal university entrant to gain years of higher education at this time. Although one cannot use this article to say how we should improve access to higher education in today's context, one can certainly use our findings to say that the group of people who may benefit from higher education is potentially larger than the social norm.

V. Conclusion

The famous short-lived "revolution" of 1968 had consequences for those undertaking important examinations in that year. The protracted delays, modifications to exam procedures, and deliberate intention that any exams taking place should be less stringent than usual led to higher pass rates than would otherwise have been the case. While for some students this would have made no difference or only modified the timing of their progression through the education system, there is a sizable group for whom these events made a real difference to the years of higher education obtained. This group consists of birth cohorts where many would have been in very selective stages of the higher education system at the time of the 1968 events. The 1949 cohort was particularly affected. The importance of 1968 for this cohort is mainly due to the effect of the 1968 events on the examination for the *baccalauréat*. We also show that the "marginal" student is more likely to be from a middle-class family background, which may be explained by the fact that students from lower socioeconomic groups are much less likely to progress to this level of education and that students from higher socioeconomic groups are more likely to pass the examinations in any given year.

The consequence of 1968 for the affected group was that it became easier to progress to a further stage of higher education and thus to obtain more years of higher education than would otherwise have been the case. We use this exogenous shift in years of higher education to implement an instrumental variables strategy in which we estimate the labor market return to years of higher education and analyze whether these additional years have any causal impact on the educational outcomes of the next generation. We obtain very similar results regardless of whether we compare affected and unaffected groups of students across cohorts or within cohorts.

The regression results show large labor market returns to a year of higher education and show that returns are transmitted across generations. The wage return is higher than in many other studies in this literature and suggests that the return to an additional year of higher education may be at least as large as an additional year of secondary schooling (around which most experimental studies in this literature are based). In fact, the returns to schooling for those induced to get more education by the easy exams are as important as the (very large) wage gap observed between college and high school graduates. This supports a policy of expansion in the higher education system, at least in contexts similar to that described in this study.

Appendix A

The Labor Force Survey and Educational Qualifications

The annual Labor Force Survey (l'Enquête Emploi) is the largest household survey conducted by the French Statistical Office, INSEE (sampling rate = 1/300; 150,000 respondents). It is the survey used to measure the rate of unemployment. Also, it provides a measure of the distribution of educational attainment across the French population that is used as a reference by INSEE when conducting smaller surveys. These measures are subject to particular care. The surveyor poses a series of three questions to determine (*a*) the highest qualification obtained in *l'enseignement secondaire général* (general secondary education), (*b*) the highest qualification obtained in *l'enseignement secondaire technique* (vocational secondary), and (*c*) the highest qualification obtained in *l'enseignement supérieur general ou technique* (tertiary education). There are additional questions about educational specialization.

With regard to the *baccalauréat*, there are different routes that can be followed (in terms of subjects chosen). The most common route is *baccalauréat général* (i.e., academic). However, there are also vocational options—technical *baccalauréat* and equivalent technical diplomas. Table A1 shows the percentage of male workers in cohorts 1946–52 who obtain a *baccalauréat général* and the percentage who obtain one of the technical diplomas that make it possible to pursue higher education.[33] The percentage of male workers with a *baccalauréat général* is about 18.3% for cohorts 1946–52, with a peak of 20.2% for the 1949 cohort. It is only slightly higher than that suggested by administrative data when we divide the number of persons who obtain the *baccalauréat général* at date t by the size of the cohort born at date $t - 19$. The percentage of male workers with some form of technical diploma is about 6.9% and very stable across cohorts. This suggests that it has not been affected by the 1968 events. Thus, the shift in the distribution of university credentials observed in

[33] The rise in *baccalauréat technologique* (taken for the first time in 1969) was accompanied by a decline in *brevets* of the same magnitude. Hence, the percentage of workers with a technical diploma is fairly constant over these years. The introduction of *baccalauréat technologique* was not followed by an increase in the probability of obtaining a university diploma for persons with a nongeneral *baccalauréat*.

the LFS is driven by a shift in the percentage of those with the *baccalauréat général*. Further analysis (not reported) confirms that the shift is very strong for those from a middle-class family background (+4 points) and only very modest for other social classes (+1 point).

It is possible to identify the type of institutions from which university qualifications were obtained: (*a*) a qualification from technological institutes and other specialized institutes (*institut de technologie*, *écoles d'infirmiers*, *d'instituteurs*, *école d'architecture*, etc.), (*b*) general university (from *premier*, *deuxième*, or *troisième* cycle), and (*c*) *grandes écoles* (the elite institutes of higher education). Table A2 shows that the increase in the percentage of workers obtaining the *baccalauréat général* for the 1949 cohort has been followed by an almost equivalent increase in the percentage of workers graduating from vocationally oriented and other specialist institutes. In summary, the events have shifted a fraction of the 1949 cohort from below the *baccalauréat* to above the *baccalauréat*, and the data suggest that the vast majority of these students have used the opportunity to obtain a upper-level technician diploma from one of the many specialist institutes in France.

Table A1
Percentage of *Bacheliers* among Male Workers, by Cohort and Type of *Baccalauréat*

		Technical *Baccalauréat* and Other Technician Diplomas			
	General *Baccalauréat*	All	*Baccalauréat de Techniciens* and *Baccalauréat Technologiques*	Diplomas Equivalent to *Baccalauréat Technologiques* and *Baccalauréat Techniciens*	*Brevets* from Technical Schools
1946	17.2	6.9	2.5	2.4	2.0
1947	18.2	6.5	2.8	1.9	1.8
1948	18.2	6.7	3.3	2.4	1.0
1949	20.2	7.0	3.6	2.7	.7
1950	18.2	7.0	4.6	1.8	.6
1951	17.8	7.0	4.7	2.2	.2
1952	18.4	7.1	4.9	1.7	.3
All	18.3	6.9	3.8	2.2	.9
N	26,371	26,371	26,371	26,371	26,371

NOTE.—Data are for male wage earners born between 1946 and 1952.

Table A2
Percentage of Workers with a University Diploma or Above, by Type of Higher Education Institute

	Institutes of Technology and Other Specialized Institutes	General University (First, Second, and Third Cycles)	*Grandes Écoles*
1946	8.4	7.3	.8
1947	8.7	8.5	1.0
1948	8.7	8.5	1.0
1949	11.1	7.9	.8

Table A2 (*Continued*)

	Institutes of Technology and Other Specialized Institutes	General University (First, Second, and Third Cycles)	*Grandes Écoles*
1950	9.4	7.4	1.0
1951	9.2	7.2	.7
1952	8.8	7.8	.7
All	9.2	7.8	.8
N	26,371	26,371	26,371

NOTE.—Data are for male wage earners born between 1946 and 1952.

Appendix B

Robustness Check Using Different Possible Comparison Groups

Table B1 shows IV estimates of the effect of years of higher education on log wages, using alternative specifications for the comparison cohorts. The treated cohort is 1949. Similar results are obtained to those reported in the text (Sec. III.B) when we use comparison cohorts that are closer to the treated cohort (i.e., 1947 and 1951) or further from the treated cohort (i.e., 1945 and 1953). Also, we obtain similar results when we rely only on the difference between the treated cohort and cohorts born later (i.e., 1950–53) or cohorts born earlier (i.e., 1944–47). Finally, results are similar when we use more comparison cohorts (i.e., 1944–47 and 1950–53) and a quadratic specification for the cohort effects.

Table B1
Instrumental Variable Effect of Years of Education: A Reevaluation Using Alternative Specifications

	1947, 1949, and 1951	1945, 1949, and 1953	1944–47, 1949, and 1950–53	1944–47 and 1949	1949 and 1950–53
Years of higher education	.14 (.06)	.16 (.06)	.13 (.06)	.16 (.11)	.18 (.08)
N	11,427	10,292	31,520	16,145	19,262

NOTE.—Sample is male wage earners. Data are results of regressions of (log) wages on years of higher education, controlling for age and a cohort trend. Results are reported for five different sets of comparison cohorts, using the cohort dummy 1949 = 1 as an instrumental variable. Standard deviation is in parentheses.

Figure B1 shows an extension of figure 2 to include cohorts born between 1943 and 1955. It shows that labor market and educational outcomes kept on following a similar pattern after 1952 (with a peak in 1953, plausibly reflecting the reform of compulsory education). In contrast, the pattern is somewhat different for the cohorts born during World War II. One possible explanation is that cohorts 1944–45 were the last cohorts exposed to the war in Algeria and the corresponding 12 additional months of military service (the total duration being 16 + 12 = 28 months; abolished in late 1965). This may explain the relatively low outcomes of these cohorts (less experience in the labor market at each age) and also their relatively high level of education: some members may have pursued education to obtain a deferment of their obligation to join the army up until the 1965 reform—exactly the same phenomenon as observed just before the abolition of conscription in France in 1997 or during the Vietnam War. See Maurin and Xenogiani (2007) for a study of the former issue and Card and Lemieux (2001) for a study of the latter.

Fig. B1.—Net effects of birth cohort on the probability of holding at least a university diploma and on the probability of holding an upper-level white-collar position (*cadre*), for cohorts 1943–55.

Appendix C

A Robustness Check Using Census Data

The population census is much less suitable than the LFS for analysis of the 1968 events because it does not have information on wages. However, it is of interest to see whether similar effects can be found for the probability of obtaining a higher-level qualification and an upper-level white-collar position (*cadre*). In the census conducted in 1999, 19.6% of men born in 1949 state that they have a higher-level qualification than the *baccalauréat*. This compares to 18.6% of those born in 1952 and 18.2% of those born in 1946. A similar pattern is found with previous censuses, in 1982 and 1990. Also, whichever census is used, the proportion of persons with the *baccalauréat* but no higher is stable over these cohorts. Hence, the distribution of education across cohorts has a similar profile in the census as in the LFS for cohorts born between 1946 and 1952, even though the increase observed for the 1949 cohort is lower in the census. The lower peak in the census may reflect the lower quality of information collected in the census and hence the effect of measurement error.

While the French census does not provide information on wages, some information is collected on occupation. Therefore, we have been able to test whether the increase in qualifications observed for the 1949 cohort translates to an increase in the proportion of white-collar positions (*cadre*). Using the 1990 census, one observes an increase of .7% for men born in 1949 who have this occupational status, relative to the comparison cohorts of 1946 and 1952. Similar to that observed for educational qualifications, this increase is less strong than in the LFS. The same type of pattern is observed in previous censuses. Overall, both outcome measures show a similar peak in the censuses and the LFS. Only the magnitude of the 1949 effects differs between these data sources. It is interesting to observe that the difference between the magnitude of the peaks in the census and the LFS is almost identical with respect to both occupational status (*cadre*) and educational attainment. As a result, the return to education—that which can be identified using the relationship between the increase in occupational status (*cadre*) and the increase in qualifications—is very similar using both data sources. Using the 1949 cohort as an IV, the effect of education on the probability of obtaining a higher occupational status is 9.9% (table C1), which is almost exactly the same as that found with the LFS.

Table C1
Effect of Years of Higher Education on the Probability of Being a *Cadre*: A Reevaluation Using 1982 Census Data

	Educational Level			Occupational Status			
	Baccalauréat Only	At Least University	Years of Higher Education	*Cadre*	*Cadre* or Technician	*Cadre* (OLS)	*Cadre* (IV)
Years of higher education	. . .	. . .	. . .	. . .	. . .	.0820	.0990
						(.0002)	(.0230)
1949 = 1	−.0007	.0072	.0420	.0042	.0106		
	(.0012)	(.0013)	(.0080)	(.0012)	(.0017)	. . .	. . .
Cohort trend	.0006	.0001	−.0013	−.0086	−.0130	−.0085	−.0085
	(.0002)	(.0003)	(.0016)	(.0002)	(.0003)	(.0002)	(.0002)
N	328,916	328,916	328,916	328,916	328,916	328,916	328,916

Source.—Census 1982 (25% random sample).
Note.—OLS = ordinary least squares; IV = instrumental variable. Sample is men born in 1946, 1949, or 1952. Standard deviation is in parentheses.

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