Teachers' careers and students' paths in higher education: three essays on public policy evaluation

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Three essays on public policy evaluation

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Remerciements

Alors que ma thèse touche à sa fin, il est temps de jeter un regard en arrière et d'apprécier le chemin parcouru. En un sens, limiter à ce manuscrit l'effort consenti pendant ces 4 années serait réducteur, car les trois articles présentés ici ne sont qu'un échantillon minime de la multitude de sujets qui m'intéressent en économie. C'est pourquoi j'ai toujours considéré cette thèse comme une étape de ma carrière, un marchepied qui me permettrait d'accumuler suffisamment de méthodes et connaissances techniques afin de pouvoir traiter dans le futur n'importe lequel de mes sujets de prédilection.

Quand on fait face, tôt ou tard, à la fatidique question du pourquoi de la thèse, je ne peux nier qu'une croyance personnelle a joué un grand rôle dans ma motivation. C'est l'idée selon laquelle la capacité d'apprentissage ralentit avec l'âge, et que si je ne souhaite pas être limité plus tard, je dois investir dès maintenant. La fameuse «S-shaped learning curve» est souvent associée aux rendements marginaux décroissants de l'éducation. Et si l'âge, par un canal biologique, jouait également un rôle et accélérait l'arrivée au plateau final du S ? C'est pourquoi entreprendre une thèse, c'est également courir contre le temps, afin d'arriver le plus haut possible en termes de compétences.

Je tiens donc à utiliser cet espace pour mettre en avant quelques personnes qui m'ont aidé à réaliser mes objectifs. Tout d'abord je remercie Denis Fougère, mon directeur de thèse et coauteur de deux des articles présentés ici, dont les connaissances économétriques m'ont grandement permis de progresser. Sa patience et sa minutie ont aussi clarifié nombre de mes idées qui jaillissent parfois de manière brouillonne. J'ai également eu la chance de travailler comme «teaching assistant» d'Etienne Wasmer, mon second directeur de thèse. Non seulement il m'a permis de m'investir dans l'enseignement de la microéconomie, mais il m'a également grandement sensibilisé à l'intérêt de l'interdisciplinarité, moteur de recherche du LIEPP, le laboratoire qui m'a financé durant mes 4 années de thèse. Etienne m'a aussi accueilli au sein d'un projet qu'il menait depuis de nombreuses années avec Mame-Fatou Diagne, ce qui m'a permis de produire le dernier article de cette thèse. J'exprime ici toute ma gratitude à leur égard, car la gentillesse de leur accueil a fait de notre période de coopération une expérience très enrichissante.

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J'adresse également un clin d'oeil à mon professeur de microéconomie de l'Université de Montréal, Yves Richelle, dont le cours a fondamentalement modifié ma perception de l'économie. Je me souviens du regard critique et original qu'il portait sur tout, et les discussions que nous avons pu avoir ont sans aucun doute grandement influencé l'économiste que je suis devenu.

Enfin, je ne peux conclure cette section sans souligner la coïncidence selon laquelle je délivre cette thèse l'année où Bob Dylan reçoit le prix Nobel de Littérature. J'espère que cet artiste, dont les chansons ont rythmé nombre de nuits de travail, m'inspirera encore longtemps. Que je continuerai de m'étonner, d'être curieux, de découvrir bref, «*to stay forever young*».

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General introduction

From problem identification to policy evaluation, political scientists have distinguished several overlapping stages in the public policy process. This project focuses on the last one, namely evaluations of public policies, in order to assess impacts of public interventions. Sometimes defined as an interdisciplinary field, policy evaluation attracts researchers from different academic disciplines collaborating, and sometimes competing, with each other.[1] This study aims at contributing to this general framework of analysis by using econometric modeling on evaluating public policies within the world of education.

In modern societies, a more systematic evaluation process of public intervention has emerged as a result of two trends. First, voters have been increasingly requiring accountability related to public spendings. This transparency concern has progressively arisen as post WWII welfare states where shrinking the scope of their intervention. In the 80's for instance, many governments launched structural reforms or adopted austerity measures (e.g., in the US under Reagan, in the UK under Thatcher, even in France under François Mitterrand and the third Mauroy's government, or in developing countries in the 90's as imposed by the IMF as Structural Adjustment Policies). Second, while government try to spend less, they seek to maximize the impact of every unit of currency spent. In a context of scarcity of resources, this comes down to a search for efficiency.

In France, yet, some policy experts[2] still further criticize a lack of culture of evaluation, as if the process of measuring the governance quality was a step behind compared to the others industrialized countries. Historically, the French Court of Auditors[3] was in charge of the evaluation. More recently though, the LOLF[4] strengthened in 2001 the control power of the Parliament regarding the definition and the follow up of new policies, while professionals structured themselves as the French Society of Evaluation since 1999.

However, this multiplicity of actors does not guarantee sound evaluation. On the one hand, each of them claims its own private preserve, specific tools and databases in terms of evaluation: financial equilibrium for the Court of Auditors, democratic dimension for the Parliament, and efficiency for researchers. On the other hand, they do not coordinate with each other most of the time; therefore,

[1] see for instance Jacob (2008) or Wasmer (2014) for a more detailed discussion on the topic
[2] see for example the report on the "First days of evaluation for French-speaking countries"
[3] in French, "Cour des Comptes"
[4] in French, "Loi organique sur les lois de finance"

mistrust has sometimes arisen among them. For instance, public administrations have long been a monopoly in the exploitation of their data, and the transition to open data and a broader access granted to researchers have carried out at a low pace.

This is why France Strategy published the following recommendations (2014) to improve evaluation in France. The creation of an independent institution organizing the knowledge of evaluation would foster the coordination among actors and the setting of a global research agenda. This would ensure the comprehensiveness of evaluation in terms of topics and methodologies, and further improves the visibility and credibility of results. More important, the new agenda recenters evaluation as the key element of policy process rather than a tool among others instruments of New Public Management[5] (namely the implementation in public services of management techniques borrowed from the private sector). To do so, evaluators should participate very early on in the identification of public issues and in the design of policies, in order to make those policies intrinsically easy to evaluate. Thus, the evaluation would not only be a control occurring essentially at the end of the policy process, but a driving reforming force.

Unfortunately, since it is not yet the case in France, researchers mostly rely on ex-post evaluation and natural experiments. Therefore, this study falls into three parts, each of them offering the ex-post evaluation of the impact of a reform, or program, in the world of education. Each chapter can be read independently, and is intended to contribute to the interdisciplinary framework of evaluation by bringing econometric modeling and estimation to measure the magnitude of the policy impact.

The first chapter focuses on the effects of the 2003 reform of the French national pension scheme on secondary school teachers. This reforms aimed at postponing the retirement of civil servants, by giving them financial incentives (penalty for those leaving early, and premium for those contributing longer). Due to the progressive implementation of the reform, a direct reduced-form approach could not be used. Instead, this first analysis proposes an evaluation based on the estimation of a structural model, namely the Stock and Wise option value model (*Econometrica*, 1990). Results of such an estimation can, not only characterize parameters of interest for the population (discount factor, risk aversion, disutility of labor), but also simulate what would have happened if the reform schedule has been different. Ultimately, it evaluates, under specific conditions, what will be the impact of the reform in its final stage in 2020.

[5] in French, "Nouvelle gestion publique"

The second chapter analyzes the main feature of the 2010 reform, namely the increase of the minimum legal retirement age and its impact on short term sick leaves before retirement. A first background (theoretical) model is built, in order to explain why this feature of the reform could have an impact on health of high-school teachers. Two groups of teachers are then constituted: a control group consisting of cohorts not affected by the increase in the minimum legal retirement age, namely those born between 1948 and 1950; and a treatment group, which consists of cohorts born between 1951 and 1953. Whereas the control group can still retire at 60 years old, the treatment group must retire after 60 years old and 4 months (at least). The effect of increasing the minimum retirement age on short sick leaves is identified by comparing, across the two groups, probabilities to take at least one sick leave at every age between 55 and 59 years old. Linear and nonlinear estimates of an autoregressive panel data model evaluate the magnitude and the robustness of this effect.

Ultimately, the third article evaluates the impact of a French affirmative action on overall access to higher education at the school level in higher education. This analysis exploits a natural experiment in which high-schools from deprived neighborhoods are granted a special admission procedure to Sciences Po, one of the most selective higher education establishment in France. To the contrary of typical affirmative action in the US, the program does not use racial statistics but rely on geographic localization and socioeconomic status. By defining three different control groups, following Diagne *et al.* (2016), it is possible to estimate the impact of the program using differences in differences, and to check the robustness of this estimation across the different control groups. Unfortunately, this program has not been thought since the beginning from the point of view of an evaluator: in fact treated high-schools were the one applying to the program, and the unbiased estimation of the impact of the program rely on the (strong) assumption that these schools are identical to the control ones. This last article is another piece of evidence that keeping in mind future potential evaluation since the beginning of the policy or program design can facilitate the obtainment of credible results.

Chapter 1

The effects of financial incentives and disencentives on teachers' retirement decisions: the case of the 2003 French national pension reform

Denis Fougère and Pierre Gouëdard

Using a sample of 12,463 high-school teachers, we evaluate the impact of the 2003 reform of the French national pension scheme. Considering the progressive implementation of the reform, we cannot use a reduced-form approach. Consequently, we estimate an option value model à la Stock and Wise (Econometrica, 1990). Structural estimates suggest that teachers are slightly risk averse, that their quarterly discount factor is close to unity and that their preference for leisure is comparable to the one found by Stock and Wise (1990). Simulations imply that teachers respond significantly to monetary incentives offered to those who continue working after the legal retirement age. Our cost-benefit analysis shows that the reform has progressively increased the average retirement age up to 61. This shift in the retirement age distribution should result in year 2020 in a 12% decrease of total public spendings associated with high-school teachers' pensions.

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1.1 Introduction

In many developed countries, the combination of two trends, namely the aging of the population and the decrease in labor participation of older workers (see, for instance, Gruber and Wise, 1998), has undermined the solvency of retirement systems.[1] To cope with this challenge, much academic work has been undertaken to examine how financial schemes shape individual retirement decision. A first generation of empirical studies tried to explain and to reproduce the sharp increase in labor force exits observed at age 62 and 65 in the U.S. (Gustman and Steinmeier, 1986; Stock and Wise, 1990; Blau, 1994; Lumsdaine, Stock and Wise, 1995). Later, a research stream initiated by Gruber and Wise (2004) broadened the analytical framework by studying how social security wealth may influence retirement decisions in twelve different countries.

From an econometric point of view, we may distinguish two types of approaches. On the one hand, dynamic programming (hereafter DP) aims at modelling the retirement decision as the solution of a finite-horizon stochastic problem. Relaxing progressively simplifying assumptions, models became more complex. For instance, Rust (1989) and Rust and Phelan (1997) incorporate environment uncertainty by introducing into their models multiple state variables (such as health, employment and marital status). Berkovec and Stern (1991) take into account individual heterogeneity by allowing for job-specific effects, whereas French (2005) develop the very first model which allows for savings but not for borrowing.

On the other hand, reduced-form models have progressively been refined, from standard OLS to dynamic panel data analysis. For instance, Asch *et al.* (2004) and Belloni and Alessie (2009) use peak value and accrual (namely, the variation in social security benefits) as the main drivers of exit behavior. Fitzpatrick (2013) uses a differences-in-differences strategy to assess whether introducing health insurance for public school employees affects their labor supply.

In between lies the option value model proposed by Stock and Wise (1990). At first considered as a suboptimal solution of the dynamic programming rule, it rapidly gained a great popularity among economists to the point where it is often used as a regressor in reduced-form models (see, for instance, Chan and Stevens, 2004). More flexible than DP models, the option value model relies on less ad hoc assumptions. Moreover, its predictive validity is at least as good as the one of DP models while

[1] As Börsch-Supan *et al.* (2014) notice, in particular, France, Germany, and Italy "have labor markets characterized by low participation rates of young women and individuals aged 55 and over. In spite of these structural problems, France, Germany, and Italy have been remarkably resistant to labor market and pension reforms".

it relieves their computational burden (see Lumsdaine, Stock and Wise, 1992). To some extent, all mentioned articles show the importance of financial incentives on retirement decisions.

In our study, we use the option value model in order to evaluate the impact of the 2003 French national pension reform on high-school teachers' retirement decisions.[2] Due to the complexity of this reform, which modified progressively several parameters of the pension system, we cannot use a reduced-form methodology, such as a differences-in-differences strategy or a regression discontinuity method.

Unlike reduced-form models, the option value model allows to identify several structural parameters of interest, such as the degree of risk aversion and the preference for leisure. In our preferred specification, the estimated quarterly discount factor is equal to 0.97 and the risk aversion coefficient is approximately 0.5: theses values are standard in the econometric literature.

The model fits well our data: estimated retirement rates are close to the observed ones. Some alternative scenarios show that financial incentives to continue working impact the retirement age more than financial disincentives associated with early exits (even if this conclusion must be qualified). Finally, we run a cost-benefit analysis that permits an ex-ante evaluation of the reform. The design of the reform is such that parameters of the pension system are modified each year until 2020. We then predict the evolution of the average retirement age as well as that of the total public cost (defined as the sum of wages and pensions paid by the State) for each setup of the reform.

Our study contributes to the literature on pension reforms in several ways. First of all, no structural estimation of a pension reform in France has been undertaken so far. For instance, Bozio (2008) uses a differences-in-differences approach for estimating the effects of the 1993 reform which concerned workers in the private sector only. Depending on their birth year and their length of contribution at age 60, workers were differently affected by this reform ; this feature of the 1993 reform allows Bozio to define control and treatment groups. He estimates a quarter elasticity of 0.54, meaning that requiring an additional contribution quarter in order to get the full pension rate, leads to an increase of 0.54 contributed quarter. Aubert (2009) pursues this approach with better data and estimates elasticities of 0.7 for men and 0.6 for women.

Benallah (2011) focuses on a specific feature of the 2003 reform, namely the "*surcote*", which

[2]Except estimates contained in the seminal article by Stock and Wise (1990), our paper is, at our best knowledge, the second empirical study that provides a full estimation of the conditional multiple-years model introduced by Stock and Wise (1990), the first being the article published by Belloni and Alessie (2013). However, our study is the first to use this model for estimating the effects of a complex pension system reform on individual retirement decisions.

consists in a premium paid for each extra contributed quarter. She compares two cohorts (those born in 1938 and 1944), the former being the control group, the latter the treatment one. Using a propensity score matching method, she estimates that this premium increased the average retirement age by 2 months and the probability to stay in activity after age 60 by 12%. Baraton, Beffy and Fougère (2011) consider younger cohorts, but cannot estimate elasticities, because some workers are still employed at the end of their observation period. Nonetheless, they quantify the impact of the reform by using a regression discontinuity design. They find that the probability to retire between 60 and 61 years old decreased by 9 percentage points between 2004 and 2007. They also document the heterogeneity of this effect: for teachers with a few missing quarters, the reform would barely affect their retirement age.

Second of all, previous studies about the 2003 reform were limited by the fact that the first targeted cohorts were not fully retired when these studies were conducted. To circumvent this issue, Benallah (2011) reduced her treatment group to the 1944 cohort, whereas Baraton *et al.* (2011) weighted some observations. By contrast, our sample is composed of fully retired cohorts. Moreover, simulations based on our structural estimates offer an outlook of alternative scenarios. Our results illustrate what would have happened with a different reform setting.

Next sections depict the French institutional framework (Section 1.2) and our database along with our identification strategy (Section 1.3). Section 1.4 is dedicated to the presentation of the Stock and Wise model adapted to the framework of the 2003 French pension reform. Estimates and simulations are then commented in Section 1.5. Finally, we conclude our study by evoking some research perspectives (Section 1.6).

1.2 The institutional framework

1.2.1 The French context

In year 2014, Marco Buti, Director-General for Economic and Financial Affairs at the European Commission, underlined the impossibility to undertake at the same time sound fiscal policies, sustainable welfare regimes and ambitious structural reforms. This inconsistent trinity may be solved by giving up one of these three objectives, in order to ensure political and social viability.

The French pension system, is known to be highly complex. During the last two decades, two major reforms of the French pension system have been implemented, one in 1993, under the government of the then Prime Minister Edouard Balladur, and one in 2003 under the government of the then Prime Minister Jean-Pierre Raffarin. According to Blanchet (2015), "none of these two reforms will be sufficient to fully ensure equilibrium for the pension system, but both of them have had, or should have, very significant impacts".

The 1993 reform concerned only the private sector. It increases the number of years required for getting full pension. This number was previously equal to 37.5 years. After the reform, it increased by one quarter each year until 2003, in order to reach 40 years in 2003. Moreover, the 1993 reform changed the number of years on which individual past wages are averaged in order to compute the reference wage used for calculating the pension rate. From 1993 on, this number has increased from 10 to 25 years. Finally, before 1993, past wages and pensions were discounted according to GDP growth, while since the 1993 reform, they are only indexed on inflation. Once again, according to Blanchet (2005), "this new rule considerably strengthens the impact of having shifted from an average of wages over the 10 best years to the average over the 25 best years of one's career".

E. Balladur, head of French Government in the early 90's, witnessed unprecedented deficits and undertook a structural pension reform, in order to save the French welfare model by bridging financial gaps. The reform directly affected private sectors employees, basically by reducing their pensions and increasing the length of contribution.

Logically, this step should have been closely followed by the extension of the reform to the public sector. Measures aiming at doing so, implemented by the Juppé Government in 1995 were not as successful. Numerous strikes paralyzed the country (more than three weeks in the transport sector), and tide demonstrations took place in large cities (2 millions of protesters were counted in France the 12th December 1995). Three days later, the government withdrew the reform. Under such circumstances, how can we explain the 2003 reform project was achieved, where the "Plan Juppé" failed?

1.2.2 An optimal agenda for structural reform?

This situation raises the question of an optimal agenda for structural reform. What does improve the probability of success for a reform, or more specifically, are there incentives for a politician to reform? The literature on the topic agrees on one hand that political leadership (Harberger - 1993, Sachs - 1994, Haggard and Williamson - 1994) or crises accelerate reform. This is especially true when the cost of waiting is increasing, persuading opposing groups to commit together on unpopular measures.

On the other hand, uncertainty may hold back reforms that would have been accepted *ex post* otherwise (Fernandez and Rodrik, 1991). When “winners and losers” are not clearly identified, a greater share of the population might consider themselves as “losers” and therefore militate against the reform, according to some status quo bias. In addition, France system is well known for its political fragmentation, which multiplies the number of veto points that could block reform legislation (Alesina and Drazen, 1991). Bonoli (2000) identifies pension reform as more difficult in such countries, requiring from their government more sophisticated strategies.

The reform we are studying was voted in 2003 and is roughly the same as the one proposed in 1995. How can we explain one succeeded where the other failed? Pierson (1994) details how obfuscating strategies lower political costs. In the case of an inherently complex pension reform, use of technical jargon may conceal real benefit cuts. Besides, timing might also be crucial as pointed out by Haggard and Webb (1994), where a newly elected government benefits from the public indulgence, and can trade costs of adjustment against political gains. This effect, known as the “honeymoon effect” could have played a role when we consider that the “Plan Juppé” was implemented at the very end of Miterrand’s term whereas the Fillon’s reform (the 2003 reform) was proposed only 10 months after the election of Jacques Chirac at the presidential office in 2002.

Moreover, Orenstein (2000) distinguishes 3 steps in the reforming process. During the first one, the reformer must gather as much political and expert agreement as possible, in order to build a strong political basis to cope with future negotiations. This is the commitment step. Then, by disseminating the spirit of the reform, communicating about it, the reformer tries to gain acceptance of it: the coalition step. In this respect, F. Fillon (minister in charge of the reform) attended the French TV broadcast “100 Minutes pour convaincre”, only 2 months after submitting his reform project to the Parliament. He declared on this occasion “the reform is the only one possible”. Despite this pedagogical try, resistance was still vivid among unions. Echoing the timing argument previously mentioned, the reform was finally submitted to the government during summer, when the opposition is often the least virulent.

Finally, one last explanation of the reform “success” maybe lies in the last step described by

Orenstein: the implementation. Sturzenegger and Tommasi (1998), and Lora (2000) underline the importance of the speed, appropriate bundling and tactical sequencing in order to properly achieve a reform. In our case, a classical parametric reform, crucial coefficients, that determine the level of pension, are smoothly but steadily increased. This sequencing implies that cohorts closed to retire are not much affected, but younger cohorts are impacted by the growing power of the device. The very design of the reform, by spreading its effect over time, dilutes immediate opposition, because the stronger the effect is, the later it will come into force.

This brings us to the interest of the structural estimation of such a reform. Due to the difficulty of pushing through unpopular reform, progressive designs are implemented, taking advantage of the sequencing argument from Sturzenegger and Tommasi. Unfortunately, they rarely allow a simple identification of mechanisms: either several parameters are shifted together, or only one is modified, but during such a small temporal window that it substantially reduces the sample of interest. With structural estimation, we can evaluate the total effect of the public policy, on the whole population, and then study the sensitivity to the shift of one parameter, all other things being kept equal.

1.2.3 The 2003 reform

The 2003 pension reform is an extension of the 1993 reform to the French public sector. At that time, three main arguments were highlighted to justify the new reform. The first one was related to fairness: the objective was to bridge the gap between private and public sectors in terms of required years of contribution. Oddly, by offering to civil servants the indexation of pensions on prices, rather than on wages as previously, the reform helped to maintain their purchasing power over time since public wages have been frozen for several years during the last decade.

The second argument was the demographic concern. Since 1970, life expectancy in France is increasing approximately by one quarter each year.[3] Moreover, generations born after World War 2 were expected to massively retire around 2005. It implies that active workers have to contribute more intensively, and for a longer period, to pension funding. Namely, their effort should progressively shift from 2.2 workers for 1 pensioner in 2002 to only 1.5 workers in 2020.

The third justification was the fiscal requirement triggered off by the demographic issue. Without any reform, the system would have faced a deficit of 43 billions of Euros in 2020, endangering even

[3]Source : Insee, statistiques de l'état civil et estimations de population, http://www.insee.fr/fr/ffc/figure/NATnon02229.xls

more the solvency of public accounts. The 2003 reform could bring up to 42% (18 billions of Euros) of the forecasted 2020 deficit.

1.2.4 Main features of the 2003 reform

As noticed by Blanchet (2005), the 2003 pension reform has three main features:

- the first objective was to ensure a convergence of conditions for access to a full pension rate in the private and public sectors. Consequently, in the public sector, the number of required years of contributions has been raised from 37.5 to 40 years between 2004 and 2008;
- after 2008, the number of required years has increased by one more year by 2012 in the two sectors; it is then expected to reach 41.75 years in 2020;
- in compensation, the penalty for early exits has been reduced, and a financial incentive to postpone retirement has been introduced; it consists of a 3% bonus for each supplementary year.

A key concept in the computation of the pension is the so-called "year for opening rights". It corresponds to the year in which a teacher can legally retire. For cohorts considered in our study (those born between 1940 and 1947), the year for opening rights is the year in which a teacher reaches the age of 60. For instance, a teacher born in 1942 (i.e., who turns 60 in 2002) gets a pension calculated on the basis of the pre-reform scheme, whereas a teacher born in 1947 must comply with the 2007 setup (i.e., when she turns 60).

It must be noted that for the considered cohorts, the year for opening rights always occurs at age 60. This means that teachers can still retire at this age. If they delay retirement, they are eligible to the bonus (see above). We therefore want to estimate the sensitivity of teachers' retirement decisions to these mechanisms. To sum things up, for the cohorts we consider, the reform has introduced four main changes:

1. a gradual increase in the number of quarters required to get the full pension (this number is hereafter denoted *Length FR*),
2. the introduction of two mechanisms, namely a penalty (*"decote"*) for those who retire before getting the required quarters of contribution, and a premium fo those who postpone retirement

(*"surcote"*):

- the penalty was implemented from 2006 onwards; it increased since then; for instance, it reduced the pension by 0.125 pp. per missing quarter in 2006, and by 1.250 pp. in 2015;

- the premium was implemented from 2004 onwards; it also increased since then; for instance, the pension was increased by 0.75 pp. per extra quarter in 2004, and by 1.250 pp. in 2015;

3. an increase in the age at which the penalty rate is cancelled (the so-called "age limit")

4. a change in the formula for the pension calculation:

$$P = \tau \times Ind \times Val \times \min\left\{1; \frac{\text{Length PS}}{\text{Length FR}}\right\}$$

where

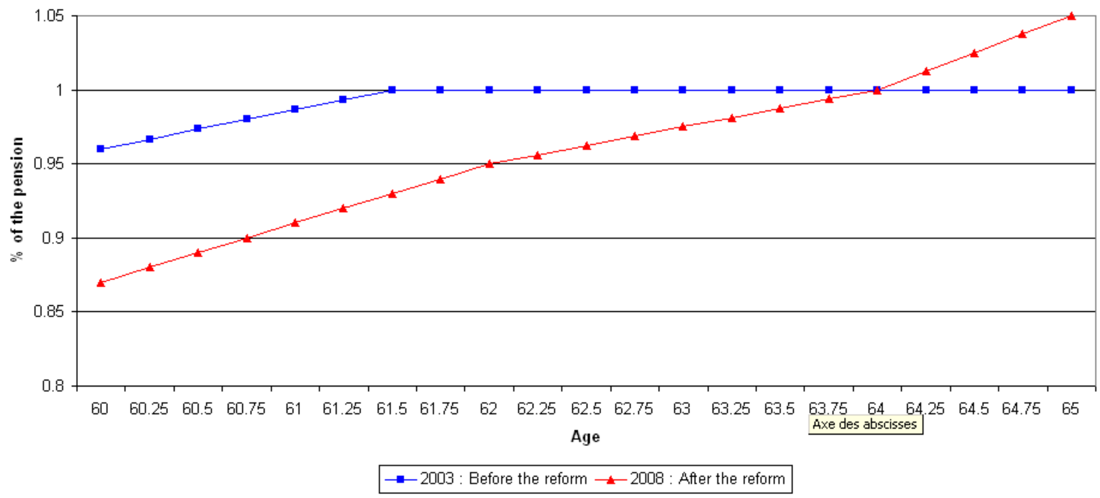
- τ is the pension rate,
- Ind is the index of the wage that is reached at least 6 months before the retirement date,
- Val is the value (in Euros) of one point for the wage index,
- *Length PS* is the number of quarters spent in the public sector,
- *Length FR* is the number of quarters required to get the full pension rate.

Before 2003, τ was equal to 0.75 while *Length FR* was equal to 150. After 2003, τ became equal to $0.75 \times (1 - \text{penalty} + \text{premium})$, while *Length FR* increased.

These measures have introduced financial incentives in order to maintain teachers in activity after the legal retirement date. Table 2.1 sums up the schedule of the evolution of coefficients and Figure 1.1 illustrates how the reform works.

On Figure 1, one can see that a teacher who contributed for 144 quarters at age 60 in 2003 almost gets the full pension rate at that age. If she contributes a few more quarters (6 supplementary quarters in fact), she gets 100% of her pension at age 61.5 because *Length PS* is now equal to *Length FR*. Beyond that age, she has no incentives to work any more, since she cannot increase her pension.

Figure 1.1 – Pension for a teacher who contributed for 144 quarters at age 60



After the reform, the pension profile of a teacher who is similar to the previous one, except she was born in 1948, undergoes a shift downwards because of the increase in the number of quarters required to get the full pension rate and because of the penalty of 0.375% imposed for each missing quarter. The kink at age 62 corresponds to the "age limit", when the penalty does not apply any more. The pension rate is only reduced because *Length PS* is lower than *Length FR*. By contrast, the teacher can now expect to increase her pension rate beyond 100%, thanks to the premium (i.e., bonus) mechanism.

Table 2.1 shows that the reform has been progressively implemented since 2003. For instance, each extra quarter contributed between 2004 and 2009 increases the pension by 0.750 pp., while after 2009, this premium doubles. The penalty rate is also increasing over the period. In fact, for the youngest cohorts, the cost of each missing quarter is higher, but each additional quarter yields more money.

Table 2.1 illustrates why a structural model of retirement behavior is preferred to a reduced-form model. One can see that each year, 3 or 4 parameters are varying together, which prevents us to identify precisely the effect of one specific feature of the reform by using either a differences-in-differences procedure or a regression discontinuity method. This is why, using a regression discontinuity method, Baraton *et al.* (2011) can only estimate the overall effect of the reform on the probability to delay retirement between 2004 and 2007. A solution would be to compare cohorts 1944 and 1945, since there is only one parameter that changes in 2005, namely the number of quarters required to get the

Table 1.1 – The 2003 reform's schedule: quarters of contribution and coefficients

Year of opening rights	Number of quarters required	penalty rate per missing quarter %	Age limit[a]	Premium rate per extra quarter % [b]
before 2004	150	0.000	60	0.000
2004	152	0.000	60	0.750
2005	154	0.000	60	0.750
2006	156	0.125	61	0.750
2007	158	0.250	61.5	0.750
2008	160	0.375	62	0.750
2009	161	0.500	62.25	1.250
2010	162	0.625	62.5	1.250
2011	163	0.750	62.75	1.250
2012	164	0.875	63	1.250
2013	164	1.000	63.25	1.250
2014	165	1.125	63.5	1.250
2015	166	1.250	63.75	1.250
2016	166	1.250	64	1.250
2017	166	1.250	64.25	1.250
2018	166	1.250	64.5	1.250
2019	167	1.250	64.75	1.250
2020	167	1.250	65	1.250

[a]The age at which the penalty rate is cancelled
[b]This rate does not depend on the year of opening rights but on the year in which the quarter is completed

full pension rate (see Table 2.1). However, we would not be able to estimate the premium or the penalty effects on teachers' retirement decisions in year 2005.

1.3 Data

The administrative datasets we use are hosted by the *"Département de l'Évaluation de la Prospective et de la Performance"*, which is the Department for Statistics and Evaluation of the French Ministry of Education. In order to observe wage and pension profiles, we use two files. The first one (*"Fichier des Dossiers d'Examen des Droits à Pensions"*, 1940-1955) gathers information about teachers during the year their rights are opened (i.e., when they are around 58 years old). The second one (*"Fichiers des bénéficiaires d'une pension civile du Ministère de l'Éducation Nationale"*, 1998-2012) collects information concerning teachers who are already retired. These two files allow us to observe wages and pension benefits from age 60 to age 63, in each quarter and for any teacher, but they contain no information about teachers' consumption and savings. Finally, we use mortality tables for professional and intellectual occupations built by the National Institute for Demographic

Studies (*"Institut National des Études Démographiques"*") in order to calculate the expected flows of revenues, whatever the retirement age.

1.3.1 Descriptive statistics

Descriptive statistics are presented in Table 2.3. Our sample consists in 12,463 teachers, from seven birth cohorts (1940-1947). The 1943 cohort was removed because we do not know whether these teachers were aware of the forthcoming reform or not. They aged 60 in 2003. The first year they could legally retire is also the year the reform was adopted by the Parliament. As we do not really know if option values they calculated were based on the former scheme or on the new one, we withdraw them as a precaution.

The number of observations is larger for later cohorts, due to a sharp increase in recruitment after the World War II. We focus here on two groups of teachers, the first consisting of certified teachers (who hold the *CAPES*[4] certificate), the second consisting of physical education teachers. Women are more numerous, just as certified teachers. Certified teachers represent the majority of teachers in secondary schools. Physical education teachers share exactly the same characteristics, so we decided to include them as well. Unlike more qualified teachers, these two categories of teachers usually do not teach in post-secondary education institutions, which grants they are not cumulating two positions at the end of their career. Our administrative data are exhaustive and individual wage profiles are reliable.

At the date of retirement, wage indices are similar across cohorts.[5] In order to avoid difficulties associated with inflation, we use wage indices instead of nominal wages. For instance, for a teacher born in 1947, a wage index equal to 783 in 2007 corresponds to a monthly gross nominal wage of around 3,550 Euros. In our sample, the median number of years of contribution is approximately equal to 37 years (see Table 2.3).

The proportion of part-time teachers at the date of retirement is very low (see Table 2.3). Because the decision to be part-time employed is difficult to formalize, we have excluded part-time teachers from our study sample. There is a specific rule for female teachers with three children or more. After the 2003 reform, they were still eligible to the pre-reform pension scheme (which was more generous) as long as they were employed for fifteen years as a civil servant and in addition their third child was

[4] *Certificat d'Aptitude au Professorat de l'Enseignement du Second degré*

[5] Remember that the index is the basis on which the wage is calculated

born before 2003. Their pension rate was then equal to the one in effect the year their third child was born. Due to the absence of information on that year, we were unable to calculate pension rates of these women. For this reason, they are excluded from our sample, and the maximum number of children is two.

Table 1.2 – Descriptive statistics

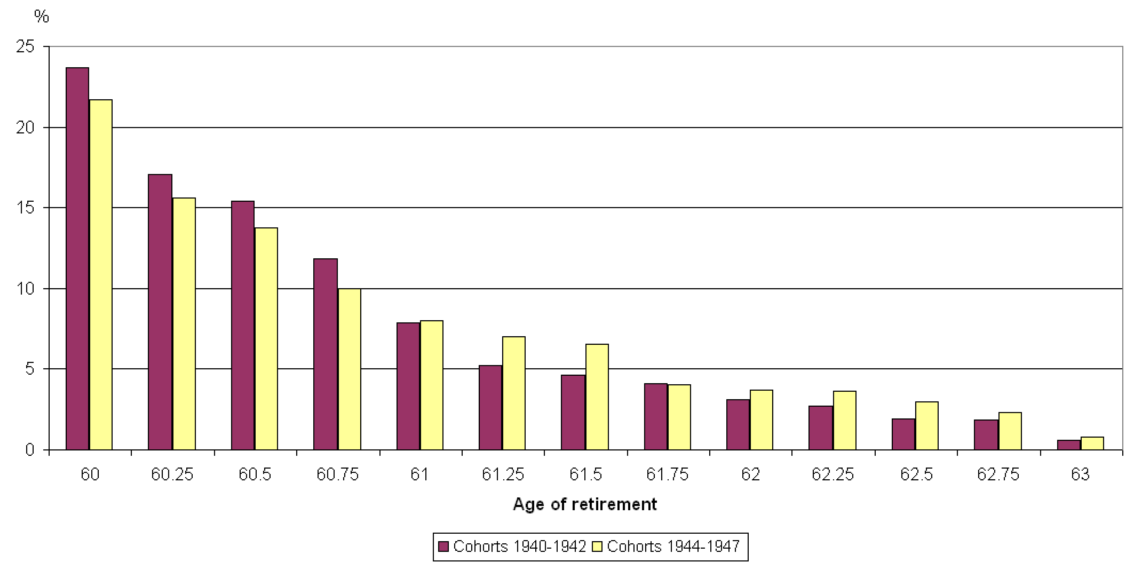
birth cohorts	1940	1941	1942	1944	1945	1946	1947	Total
number of observations	629	773	1,055	1,495	1,817	2,987	3,707	12,463
proportion of women	57.39	58.73	57.54	56.12	58.28	57.21	60.16	58.24
proportion of men	42.61	41.27	42.46	43.88	41.72	42.79	39.84	41.76
proportion of certified teachers	95.07	92.5	92.23	91.17	90.59	91.43	91.42	91.59
proportion of P.E. teachers	4.93	7.5	7.77	8.83	9.41	8.57	8.58	8.41
median wage index	783	741	741	741	783	783	783	772
average wage index	737	734	732	729	730	734	737	734
median number of years of contribution	36.5	36.5	36	36	37	37	37	36.5
median age of entry in the P.S.	27.9	27.3	27.3	27.3	27.6	27.1	26.5	27.1
proportion of part-time teachers at the retirement date	3.5	3.6	2.9	2.9	3.9	4.9	5.7	4.2
average number of children	1.31	1.31	1.35	1.33	1.36	1.35	1.39	1.36

Abbreviations: P.E. for Physical Education, P.S. for Public Sector

1.3.2 Identification strategy

The 2003 reform introduced an exogenous variation in the way pensions are calculated. Since the reform, entitlement to the full pension rate depends on the number of contributed quarters and on the teacher's birth year. Figure 1.2 illustrates the fact that younger cohorts, which are directly affected by the reform, are less likely to retire at age 60. By contrast, we observe a shift towards age 61, as shown by Baraton *et al.* (2011).

Figure 1.2 – Distribution of the retirement age according to the birth cohort



Somehow, one could consider that 1940-1942 cohorts may constitute the control group whereas the 1944-1947 ones may be the treated group. Table 1.3 shows that the probability to retire between 60 and 61 is reduced by 8 percentage points if the teacher belongs to a cohort affected by the reform.

Table 1.3 – Distribution of retirement ages for treated and control groups (in percentage)

retirement age	cohorts 1940-1942	cohorts 1944-1947
$[60; 61[$	68	61
$[61; 62[$	22	26
$[62; 63]$	10	13
Total	100	100

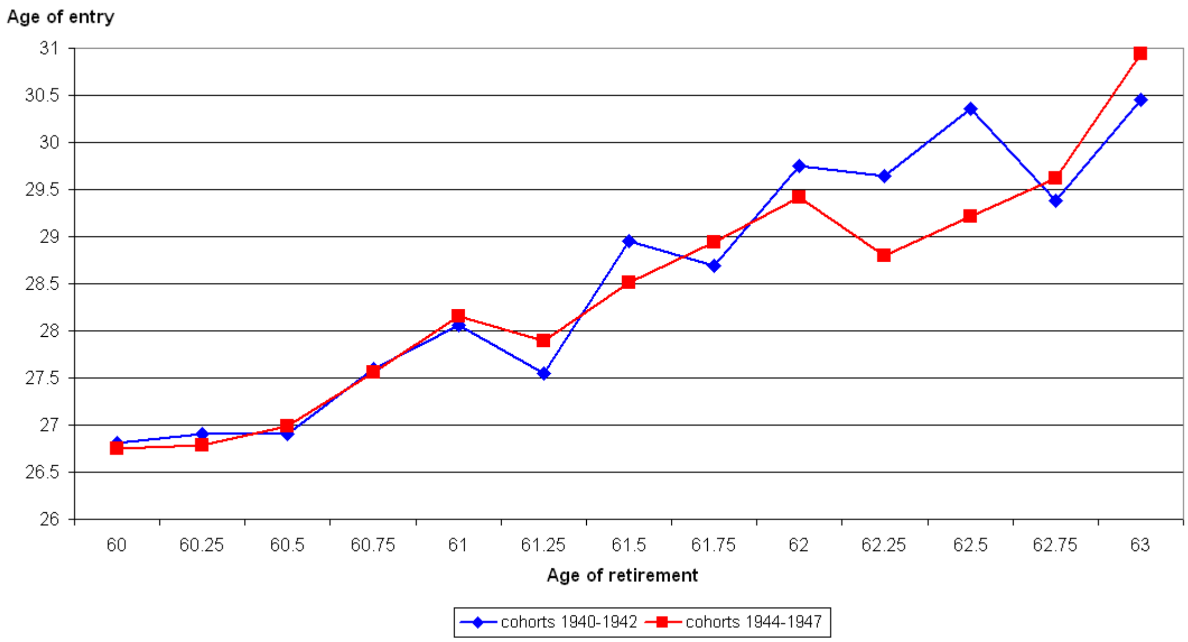
Could this increase in the age of retirement be a consequence of an increase in the age of entry into the public sector? Figure 1.3 shows that the retirement age increases with the age of entry into the public sector, but this relationship is the same for pre- and post-reform cohorts.

Figure 1.4 shows that the 2003 reform, which increased the number of quarters required to get the full pension rate, induced teachers to declare more frequently their past employment spells in the private sector in order to increase the length of their contribution period.[6]. The upward shift of the baseline curve shows the average difference between our two groups. For instance, for teachers retiring at age 60, there is a non negligible difference of 5 quarters of contribution.

After the 2003 reform, declaring these quarters of employment in the private sector has two types

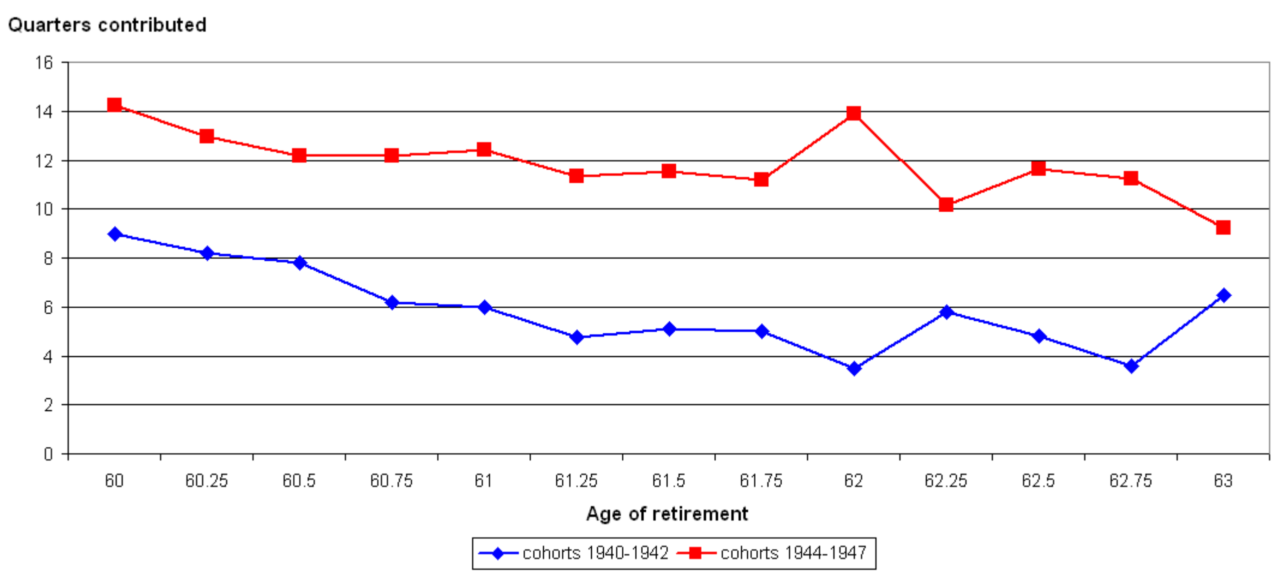
[6]This fact was already noticed by Bozio (2008)

Figure 1.3 – Mean age of entry in the public service according to the retirement age



of advantages. First, for those teachers who have the required number of quarters of employment in the public sector and who can thus get the full pension rate, these supplementary quarters give entitlement to the premium rate (see Table 2.1). Second, those for whom the number of employment quarters in the public sector is insufficient, may benefit from such supplementary quarters, which help them to get the full pension rate more rapidly.

Figure 1.4 – Quarters contributed outside Public Service depending on the age of leave



Finally, one could argue that the time span between the first and the last cohorts is too large, and that the decision to retire is not only affected by the reform, but also by macroeconomic conditions. For instance, the extreme cohorts (1940 and 1947) can retire from 2000 and from 2007, respectively. Meanwhile, the situation has dramatically evolved, especially because of the 2002 recession. How to be sure that changes in behavior are not linked to changes in the macroeconomic environment, in which case our identification strategy could be blurred? A solution would be to limit the sample to only two close cohorts, say 1942 and 1944. Nevertheless it is not necessary, since we assume that teachers are facing a certain and secure environment (i.e., a deterministic wage progression, the quasi-impossibility to be dismissed) that makes them less likely to be affected by macroeconomic shocks.

To sum things up, the 2003 reform (implemented from 2004) modified the calculation of pensions. Cohorts born between 1940 and 1942 were only affected by one feature of the reform, and in a negligible way. Namely, teachers of these cohorts who have not retired in 2004 could claim the premium for their extra quarters of contribution. In our sample, since nobody retires after age 63, this implies that the maximum number of quarters teachers born between 1940 and 1942 could claim is 4 (i.e., a teacher born in 1942 is 62 in 2004 and can get a maximum of 4 quarters when she retires at 63 years old). This deadweight effect is therefore minor, because it only concerns the teachers born in 1942 who are still working at age 62. By contrast, cohorts born between 1944 and 1947 faced at an earlier age the financial incentives introduced by the reform; in addition, they were potentially affected by a penalty per missing quarter and by an increase in the number of quarters required to get the full pension rate.

We think that the exogenous modification of pension calculation is the cause behind the distortion of the retirement age as depicted by Figure 1.2. Since we reject the hypothesis stating that younger cohorts entered the public sector later (Figure 1.3), we interpret their delayed retirement as a distortion of their option value profiles.

1.4 The model

The option value model, which has been introduced by Stock and Wise (1990), has been used several times in the empirical literature. In the so-called reduced-form option value model, economists use the option value as a crucial regressor in the retirement decision equation. For instance, Samwick

(1998), Coile and Gruber (2000), and Baker *et al.* (2003) calculate the option value from parameters estimated by Stock and Wise (1990) and then plug it into their regressions. Such a method substantially reduces the computational burden since it avoids estimating directly parameters of the individual utility function.

Sometimes, all parameters of the utility function are set equal to 1, transforming the utility function into a simple function depending linearly on financial components. This is the solution proposed, e.g., by Asch and Warner (1999) who try to identify determinants of the retirement decision. Coile and Gruber (2001) use the same simplifying assumption to estimate the peak value variable.

Here we intend to estimate the full structural option value model developed by Stock and Wise (1990). Such an estimation is not an easy task, as mentioned for instance by Samwick (1998).[7] At our best knowledge, our study is the second paper providing a full estimation of the conditional multiple-years model introduced by Stock and Wise (1990), the first being the article published by Belloni and Alessie (2013). In the next subsections, we summarize the Stock and Wise model and we explain how we adapt it to our framework.

1.4.1 The utility function and the decision rule

Let us consider a teacher working at age t:[8] her future wage at age s, which is assumed to be perfectly known,[9] is denoted Y_s. If she retires at age r, her pension is equal to $B_s(r)$. Her indirect utility stems either from her wage, or from her pension.[10] In the first case, her utility is denoted U_w, in the latter it is denoted U_r. The teacher chooses her retirement age by maximizing the mathematical expectation of the following value function with respect to r:

$$V_t(r) = \sum_{s=t}^{r-1} \beta^{s-t} U_w(Y_s) + \sum_{s=r}^{S} \beta^{s-t} U_r(B_s(r)) \tag{1.1}$$

This value function depends on future earnings and retirement pensions, and, above all, on the retirement age r. In this expression, β denotes the discount factor and S is the maximum age of death, which is assumed to be the same for all individuals (in our application, $S = 100$). The earlier

[7]Samwick (1998) did not succeed in estimating simultaneously the parameter representing the preference for leisure and the discount rate.

[8]In our application, age is measured in quarters.

[9]This assumption is not restrictive for teachers. As civil servants, they cannot be laid off, except in the case of a serious professional misconduct, and they can forecast with certainty their future wages which essentially depend on their seniority.

[10]The model does not take into account individual savings and financial wealth, see subsection 1.4.5.

the retirement age, the higher the weight assigned to U_r.

At each age t, the teacher is supposed to solve this maximization problem by comparing the expected value of retiring at age t with the greatest among expected values of retiring at any future age r. Then the optimal retirement age r^* is the one which maximizes the function:

$$G_t(r) = E_t V_t(r) - E_t V_t(t)$$

namely,

$$r^* = \underset{\{t+1,t+2,\dots,S\}}{\operatorname{argmax}} E_t V_t(r)$$

Consequently a teacher stops working when there is no older age r such as $G_t(r)$ is positive. In other words, the decision to retire is taken as soon as $G_t(r^*) \leq 0$. This means that the teacher continues working as long as:

$$G_t(r^*) = E_t V_t(r^*) - E_t V_t(t) > 0 \tag{1.2}$$

It is worth noting that this process is incremental: at each age r, a new r^* is calculated, which can differ from the one computed in the previous period $r-1$. Unlike a more "sophisticated" rule that requires a one-shot resolution by backward induction, based on a Bellman equation (see Rust, 1987, for instance), the Stock and Wise model is solved one step at a time. However, as shown by Stock and Wise (1990, p. 1166), if all future revenues are known with certainty, which is the case for secondary school teachers at the end of their career in the public sector, the two rules are equivalent (see Appendix A for a formal proof).

1.4.2 Functional forms

Under the assumption of a constant relative risk aversion (CRRA), the above utility functions become:

$$U_w(Y_s) = \frac{Y_s^{1-\gamma}}{1-\gamma} + \omega_s \qquad\qquad U_r(B_s) = \frac{(kB_s(r))^{1-\gamma}}{1-\gamma} + \xi_s$$

where γ measures the degree of relative risk aversion ($1/\gamma$ is the elasticity of inter-temporal substitution) and k is a coefficient representing the preference for leisure.

Errors are assumed to follow a first-order autoregressive process, with an individual random effect whose persistence is represented by the parameter ρ ($0 < \rho < 1$):

$$\omega_s = \rho\omega_{s-1} + \varepsilon_{\omega s} \qquad\qquad \xi_s = \rho\xi_{s-1} + \varepsilon_{\xi s}$$

where $E_{s-1}(\varepsilon_{\omega s}) = E_{s-1}(\varepsilon_{\xi s}) = 0 \quad \forall s = t+1, \cdots, S$, and $\varepsilon_{\omega s}$ and $\xi_{\omega s}$ are i.i.d. $\forall s = t, \cdots, S$.

These specifications of utility functions and of errors are then plugged into equation (1). Thus $G_t(r)$ becomes:

$$\begin{aligned} G_t(r) = {} & E_t \sum_{s=t}^{r-1} \beta^{s-t} \frac{Y_s^{1-\gamma}}{1-\gamma} + E_t \sum_{s=r}^{S} \beta^{s-t} \frac{(kB_s(r))^{1-\gamma}}{1-\gamma} - E_t \sum_{s=t}^{S} \beta^{s-t} \frac{(kB_s(t))^{1-\gamma}}{1-\gamma} \\ & + E_t \sum_{s=t}^{r-1} \beta^{s-t} (\omega_s - \xi_s) \end{aligned}$$

which can be written under a more compact form as:

$$G_t(r) = g_t(r) + \phi_t(r)$$

where

$$g_t(r) = E_t \sum_{s=t}^{r-1} \beta^{s-t} \frac{Y_s^{1-\gamma}}{1-\gamma} + E_t \sum_{s=r}^{S} \beta^{s-t} \frac{(kB_s(r))^{1-\gamma}}{1-\gamma} - E_t \sum_{s=t}^{S} \beta^{s-t} \frac{(kB_s(t))^{1-\gamma}}{1-\gamma}$$

and

$$\phi_t(r) = E_t \sum_{s=t}^{r-1} \beta^{s-t} (\omega_s - \xi_s)$$

If we assume that the conditional survival probability at age s given that the teacher is alive at age t ($s > t$) depends neither on the stream of revenues, nor on the individual disturbance, we can rewrite these two last functions as:

$$\begin{aligned} g_t(r) = {} & \sum_{s=t}^{r-1} \beta^{s-t} \pi(s|t) E_t \left(\frac{Y_s^{1-\gamma}}{1-\gamma} \right) + \sum_{s=r}^{S} \beta^{s-t} \pi(s|t) E_t \left(\frac{(kB_s(r))^{1-\gamma}}{1-\gamma} \right) \\ & - \sum_{s=t}^{S} \beta^{s-t} \pi(s|t) E_t \left(\frac{(kB_s(t))^{1-\gamma}}{1-\gamma} \right) \end{aligned}$$

and

$$\phi_t(r) = \sum_{s=t}^{r-1} \beta^{s-t} \pi(s|t) E_t(\omega_s - \xi_s)$$

Assuming that errors are AR(1) results in:

$$\phi_t(r) = \sum_{s=t}^{r-1} \beta^{s-t} \pi(s|t) E_t(\omega_s - \xi_s) = \sum_{s=t}^{r-1} (\beta\rho)^{s-t} \pi(s|t)(\omega_t - \xi_t) = K_t(r)\nu_t$$

where

$$K_t(r) = \sum_{s=t}^{r-1} (\beta\rho)^{s-t} \pi(s|t) \qquad \text{and} \qquad \nu_t = \omega_t - \xi_t$$

Finally, we get:

$$G_t(r) = g_t(r) + K_t(r)\nu_t \tag{1.3}$$

The gain function breaks down into two parts, namely a deterministic component $g_t(r)$ which only depends on future revenues, and a stochastic one ν_t. In equation 1.3, $K_t(r)$ is a global deflator which evaluates at time t the future value of the random components. When the retirement age r increases, $K_t(r)$ increases too, and a greater weight is put on the random part of the gain function.

1.4.3 The likelihood function

Let us first consider a single period (quarter). Recall from equation 1.2 that a teacher continues to work if $G_t(r^*) = g_t(r^*) + K_t(r^*)\nu_t > 0$.
Assuming that $\nu_t \sim \mathcal{N}(0; \sigma_\nu^2)$, the probability to retire at time t is given by:

$$Pr(R = t) = Pr\left(\nu_t \leq -\frac{g_t(r^*)}{K_t(r^*)}\right) = 1 - \Phi\left(\frac{1}{\sigma_\nu}\frac{g_t(r^*)}{K_t(r^*)}\right)$$

where Φ is the c.d.f. of the standard normal distribution $\mathcal{N}(0;1)$. The individual contribution to the likelihood function is therefore:

$$\mathcal{L}_i = Pr[R=t]^{\mathbb{1}(R=t)} \times [1 - Pr[R=t]]^{\mathbb{1}(R>t)}$$

$$= \left[1 - \Phi\left(\frac{1}{\sigma_\nu}\frac{g_t(r^*)}{K_t(r^*)}\right)\right]^{\mathbb{1}(R=t)} \times \Phi\left(\frac{1}{\sigma_\nu}\frac{g_t(r^*)}{K_t(r^*)}\right)^{\mathbb{1}(R>t)} \tag{1.4}$$

Now consider multiple periods (quarters). The probability of retirement at age τ is now:

$$Pr[R=\tau] = Pr\left[\nu_t > -\frac{g_t(r_t^*)}{K_t(r_t^*)}\ ;\ \ldots\ ; \nu_{\tau-1} > -\frac{g_{\tau-1}(r_{\tau-1}^*)}{K_{\tau-1}(r_{\tau-1}^*)}\ ;\ \nu_\tau \leq -\frac{g_\tau(r_\tau^*)}{K_\tau(r_\tau^*)}\right]$$

Considering in addition that random terms ν_s are not independent since $\nu_s = \rho\nu_{s-1} + \varepsilon_s$ (see Appendix B for further details), this probability does not break down into a product of simple components. The individual contribution to the likelihood function is therefore given by:

$$\mathcal{L}_i = \int_{-\frac{g_t(r_t^*)}{K_t(r_t^*)}}^{+\infty} \cdots \int_{-\frac{g_{\tau-1}(r_{\tau-1}^*)}{K_{\tau-1}(r_{\tau-1}^*)}}^{+\infty} \int_{-\infty}^{-\frac{g_\tau(r_\tau^*)}{K_\tau(r_\tau^*)}} f(\nu_t, \cdots, \nu_{\tau-1}, \nu_\tau)\ \mathrm{d}_t \cdots \mathrm{d}_{\tau-1}\mathrm{d}_\tau \tag{1.5}$$

where f is the p.d.f. of a normal multivariate distribution with a mean of 0 and covariance matrix Σ of dimension τ. At this point, we depart a little bit from Stock and Wise model, because we add an assumption of second-order stationarity, so that the variance does not tend to infinity when t increases. Thus the covariance matrix has the following convenient form:

$$\Sigma = \frac{\sigma_\varepsilon^2}{1-\rho^2}\begin{bmatrix} 1 & \rho & \rho^2 & \cdots & \rho^{\tau-1} \\ \rho & 1 & \rho & \cdots & \rho^{\tau-2} \\ \vdots & \vdots & & & \vdots \\ \rho^{\tau-2} & & \cdots & 1 & \rho \\ \rho^{\tau-1} & \rho^{\tau-2} & \cdots & \rho & 1 \end{bmatrix}$$

The second-order stationarity assumption reduces the number of parameters to be estimated. Since $Var(\nu_s) = \sigma_{\nu s}^2 = \sigma_\nu^2\ \forall s = t+1, \cdots, S$ and because this variance depends only on ρ and σ_ε, we do not estimate directly σ_ν^2. Instead we estimate separately ρ and σ_ε.

1.4.4 Seasonality of retirement

French teachers are more likely to retire in early September, first because the school year generally ends after summer vacation, second because teachers are paid during summer months, even if they are not present at school. In order to take into account this phenomenon, we add to the threshold of the decision rule (given by equation 1.2) a shift function indicating that the month of September belongs to the quarter in which the teacher retires.[11] Then a teacher decides to retire at age r^* if:

$$G_t(r^*) \leq 0 + \psi \times \underbrace{\mathbb{1}(sept \in quarter)}_{=z}$$

which implies that:

$$g_t(r^*) + K_t(r^*)\nu_t \leq \psi z$$

or equivalently:

$$\frac{g_t(r^*)}{K_t(r^*)} - \frac{\psi z}{K_t(r^*)} \leq \nu_t \tag{1.6}$$

Consequently, in the single-period case, the individual contribution to the likelihood function is:

$$\mathcal{L}_i = \left[1 - \Phi\left(\frac{1}{\sigma_\nu\ K_t(r^*)}(g_t(r^*) - \psi z)\right)\right]^{\mathbb{1}(R=t)} \times \Phi\left(\frac{1}{\sigma_\nu\ K_t(r^*)}(g_t(r^*) - \psi z)\right)^{\mathbb{1}(R>t)} \tag{1.7}$$

while, in the multiple-periods case this contribution becomes:

$$L_i = \int_{\frac{-g_t(r_t^*)+\psi z_t}{K_t(r_t^*)}}^{+\infty} \cdots \int_{\frac{-g_{\tau-1}(r_{\tau-1}^*)+\psi z_{\tau-1}}{K_{\tau-1}(r_{\tau-1}^*)}}^{+\infty} \int_{-\infty}^{\frac{-g_\tau(r_\tau^*)+\psi z_\tau}{K_\tau(r_\tau^*)}} f(\nu_t, \cdots, \nu_{\tau-1}, \nu_\tau)\ \mathrm{d}_t \cdots \mathrm{d}_{\tau-1}\mathrm{d}_\tau \tag{1.8}$$

[11] Let us recall that in our application, the time unit is the quarter.

1.4.5 A model without savings

Because of the lack of relevant information in our dataset, our empirical model abstracts from personal savings. Indeed it is likely that individual assets and wealth could affect the retirement decision. One could yet argue that teachers who have almost the same length of service within the public sector and have benefited from a similar wage progression during their career should roughly own the same amount of savings. However this argument is somewhat weak, and several factors, like the spouse's earnings, the household composition, home ownership and family bequests, may invalidate it.

Nonetheless, as noticed by Belloni and Alessie (2013), "earlier reduced-form studies based on UK and USA data (see, e.g., Blundell *et al.*, 2004; Hausman and Wise, 1985) show that financial wealth has at most a small impact on retirement choices once one controls for pension wealth. [...] In particular Blundell *et al.* (2004) report that retirement choices in the UK are not significantly partially correlated with financial wealth."

Moreover, in order to show that omitting savings in the Stock and Wise model has not serious consequences, Belloni and Alessie (2013) re-estimate this model on a subsample of Italian blue-collar workers who hold much less wealth than other workers. In fact they find that "the model for blue-collar workers generates very similar actual and predicted retirement rates to those of the overall model" (Belloni and Alessie, 2013, p. 524). Thus, they conclude that the effects of omitting savings are not that dramatic.

In the light of these previous studies, we can reasonably think that abstracting from personal savings should not significantly modify the accuracy of the model predictions for a relatively homogenous group of workers like the French secondary school teachers. The good fit of our estimates (see next section) strengthens this argument.

1.5 Results

1.5.1 Parameter estimates

The adapted Stock and Wise model is estimated using our sample of 12,463 secondary school teachers from cohorts 1940-1942 and 1944-1947. Likelihood functions represented by equations (2.4) and (2.5) are maximized in order to get parameter estimates of the static and dynamic models. Our estimation of the static model is based only on the quarter in which a teacher reaches the age of 60. In theory, the dynamic model should take into account all the quarters between 60 and the age of retirement. Considering that all observed retirement ages are comprised between 60 and 63, this implies that the full dynamic model would include individual likelihood contributions involving 13-dimensional integrals. Consequently, this model should be estimated by simulated maximum likelihood or other simulation techniques (like simulated moments or MCMC procedures). In order to circumvent the computational burden associated with the estimation of the full dynamic model, we set at 3 the maximum number of successive periods, which was the set-up adopted by Stock and Wise (1990). For those teachers retiring either at 60 or 60.25 (0.25 representing one quarter after the 60th birthday), the individual likelihood contribution still consists in one or two components.

Parameters estimates of the two models are reported in Table 1.4. In each model the preference for leisure is assumed to be a linear function of the spouse's age and of the presence of at least one child less than 20 in the teacher's household. This implies that coefficient k has the following functioning form:

$$k = k_0 + k_1 \mathbb{1}(\text{spouse is over 60}) + k_2 \mathbb{1}(\text{have a child under 20})$$

By doing so, we aim at introducing some observed heterogeneity in the models. Parameter k_1 is expected to be positive: in fact, it is intended to capture the higher utility resulting from the fact that a spouse who has more than 60 years old could also be retired, which should increase the teacher's preference for leisure. When a teacher has at least one child less than 20, she has to cover expenditures associated with this child's care. She has consequently higher incentives to continue working in order to increase the household's income. Thus, parameter k_2 is expected to be negative.

Table 1.4 shows that parameter estimates are all highly significant and have a theoretically credible order of magnitude. We nonetheless focus on estimates of the dynamic model for several reasons. First, the quarterly discount factor is estimated to be equal to 0.92 in the static model, which implies an annual discount factor of 0.78. Although this value is close to the one found by Stock and Wise (1990), it seems somewhat low. We prefer the one obtained with the dynamic model specification which gives a quarterly discount factor equals to 0.97, corresponding to an annual discount rate of 0.91. This latter value is, for instance, close to those found or calibrated by Eckstein and Wolpin (1999) and Keane and Wolpin (2001) with discrete choice dynamic models.

According to the risk-aversion classification proposed by Holt and Laury (2002), teachers of our sample are estimated to be risk averse within the multiple-period model,[12] while they are found to be risk-neutral within the one-period model. From this point of view, the dynamic model is clearly more credible than the static one. The difference between the estimates of relative risk-aversion coefficients may be explained by the fact that, in the one-period model, the teacher chooses only once the optimal date of retirement, which leaves no room for potential substitution across periods, and then could result in a lower β. On the contrary, in the multiple-period model, the decision rule is repeated up to three times: each time a teacher delays the retirement decision, this choice increases her expected stream of revenues (if not, she would have retired).

Apart from this, the AR(1) coefficient ρ, which is estimated to be 0.60 within the multiple-period model, ensures that exogenous shocks may have long lasting effects. We do believe that this error structure is more adapted to our data because it captures the effect of some unobserved variables, such as health for instance.[13] Of course, further research using individual health status (which is missing in our dataset) would be worthwhile.

The coefficient associated with the dummy variable indicating that the month of September belongs to the current quarter is highly significant, which means that seasonality plays a crucial role in the decision to retire. Including this variable improves substantially the fit of the model: omitting it would have biased upwards other parameter estimates.

The coefficient associated with the preference for leisure, denoted k_0, is estimated to be 1.23 for single teachers or teachers whose spouse is less than 60 years old and who have no child under 20. This value implies that one euro of pension benefit has a higher weight than one euro of wage. This parameter might as well be considered as a measure of work disutility, since its value implies that 0.82 euros of pension provides the same utility level than one euro of wage. Parameters k_1 and k_2 modify this value. A teacher whose spouse is over 60 but who has no child under 20 only needs 0.75 euros of pension benefit to compensate for one euro in wages. This value increases to 0.87 euros if she has a child under 20 and a spouse below 60.

[12] The estimated relative risk-aversion coefficient, equal to 0.49, implies that in a lottery where a teacher either wins 10,000 euros with probability 0.5 or wins 20,000 euros with the same probability, her certainty equivalent is 14,562 euros, which is lower than her expected gain of 15,000 euros.

[13] Let us remark that in the multiple period model, the standard error of the random term ν is significantly reduced from 32.8 to 21.8. In other terms, this model restricts the relative magnitude of the random component.

Table 1.4 – Parameter estimates of the Stock and Wise model

Parameters	Definition	One-period model	Multiple-period model
σ_ν	standard error of ν	32.8 (0.47)	21.8 (calculated)
σ_ε	standard error of ε		17.4 (6.8e-5)
ρ	AR(1) coefficient		0.60 (6.8e-7)
γ	relative risk aversion	0.09 (4.2e-9)	0.49 (7.0e-7)
β	discount factor	0.92 (2.8e-9)	0.97 (7.0e-7)
ψ	September	38.6 (0.82)	55.8 (6.9e-5)
k_0	preference for leisure	1.10 (5.9e-9)	1.23 (7.0e-7)
k_1	spouse over 60	0.08 (1.7e-8)	0.11 (6.9e-7)
k_2	child under 20	-0.04 (1.5e-8)	-0.08 (6.8e-7)
Number of observations		12,463	12,463
Log-likelihood value		-5,434	-12,601

Remark: standard errors are presented between brackets

1.5.2 Simulations

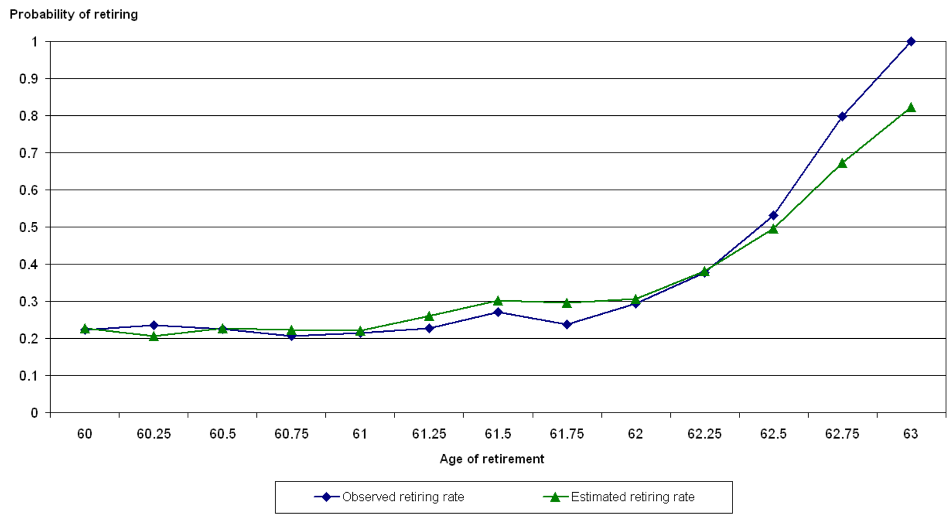
The model fit can be illustrated by plotting simulated retirement rates, compared to the observed ones. This exercise is presented on Figure 1.5. Using estimates from the multiple-periods model, we simulate retirement rates at each age by using those teachers who are still working at this age.

The most important discrepancies between observed and estimated retirement ages are at ages 61.75 (i.e., 61 years and 3 quarters), 62.75 and 63. At the oldest ages, these gaps may be explained by the low number of observations (see Figure 1.2).[14] At all other ages, especially between 60 and 61 years old, estimates are quite accurate. The peak at age 61.5 is well accounted for, but the model overpredicts the retirement rate at age 61.75. Let us remark that at this exact age, there is no difference in the proportions of retirements in the two groups of cohorts, namely the ones having 60 before the reform (1940-1942) and those having 60 after the reform (1944-1947).[15]

[14] Stock and Wise (1990, p. 1169) underpredict also retirement rates at the oldest ages. Since the retirement rate is essentially a hazard rate, a single retirement might heavily affect this rate when the number of teachers still working is low. This mathematical definition of the retirement rate makes the upper tail of the curve less reliable.

[15] See Figure 1.2.

Figure 1.5 – Conditional probability to retire depending on the teacher's age



Since the model fits the data conveniently, we can now develop counterfactual scenarios, which may be simulated by using the estimated structural parameters. The 2003 French reform was aimed at delaying retirement. This goal was expected to be reached through three mechanisms, namely the increase in the number of quarters required to get the full pension rate, a financial incentive (the "premium") and a financial disincentive (the "penalty").[16]

The following simulations focus on these two last mechanisms. In particular, we analyze the sensitivity of retirement rates to changes in coefficients determining the percentage that is applied to any missing or extra quarter of contribution. These simulations are run on the cohorts 1944-1947, since they are those directly impacted by the reform.

Figure 1.6 reports what would be (according to the parameter estimates of the structural model) the effects of cancelling the penalty and those associated with a penalty sets at its final (maximal) level, i.e., 1.25% for each missing quarter. This changes in the penalty rate do not affect retirement rates. At first glance, this could be interpreted as an efficient measure allowing to reduce pensions, without having any impact on teachers' retirement behavior. This statement has to be qualified though. In fact, in our sample, only 14.5% of teachers are subject to a small penalty when they effectively retire.[17] Given that they can only at 60 and above and that the age at which the penalty is cancelled (the so-called "limit age") is 61, the maximum number of quarters subject to the penalty

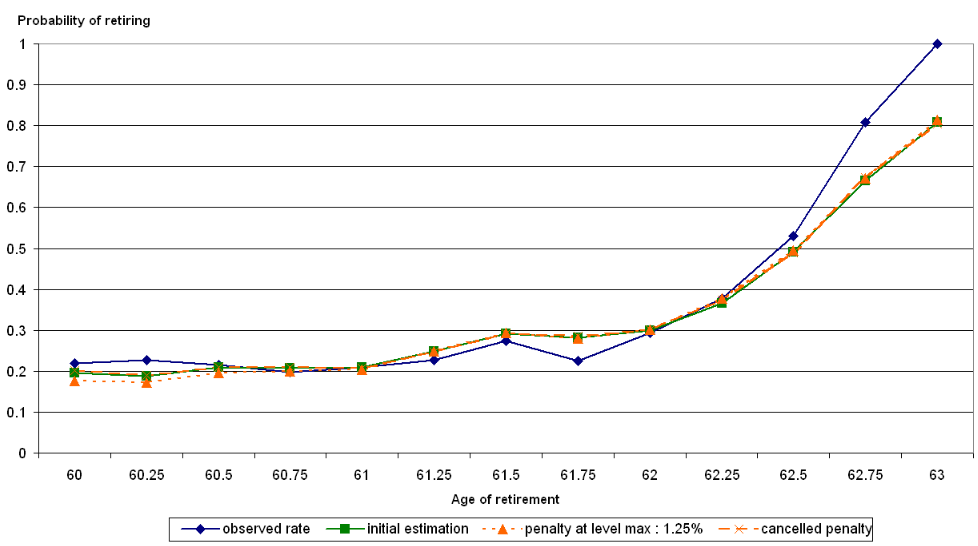
[16]See section 1.2.4.

[17]For instance, the penalty rate was equal to 0.125% for a teacher born in 1946 and to 0.25% for one born in 1947; cf. Table 2.1)

is 4. In other words, the maximum penalty rate that can be applied to one's pension rate is equal to 1%, which is very small.

Therefore it is not surprising that this mechanism has not decreased retirement rates. On the one hand, our sample is constituted by teachers that are barely concerned by the penalty. On the other hand, the magnitude of the penalty coefficient is too small to affect teachers' decision. Starting from 0.125% per missing quarter in 2006, the penalty rate reaches the initial level of the premium rate (0.75%) five years later, and its maximum level (1.25%) in 2015 when all the teachers of our sample will be retired. Finally, the existence of a limit age, which is really close to the age at which teachers can legally retire in the very first cohorts affected by the reform, worsens even more the ineffectiveness of the financial disincentive (i.e., the penalty rate) in our sample.

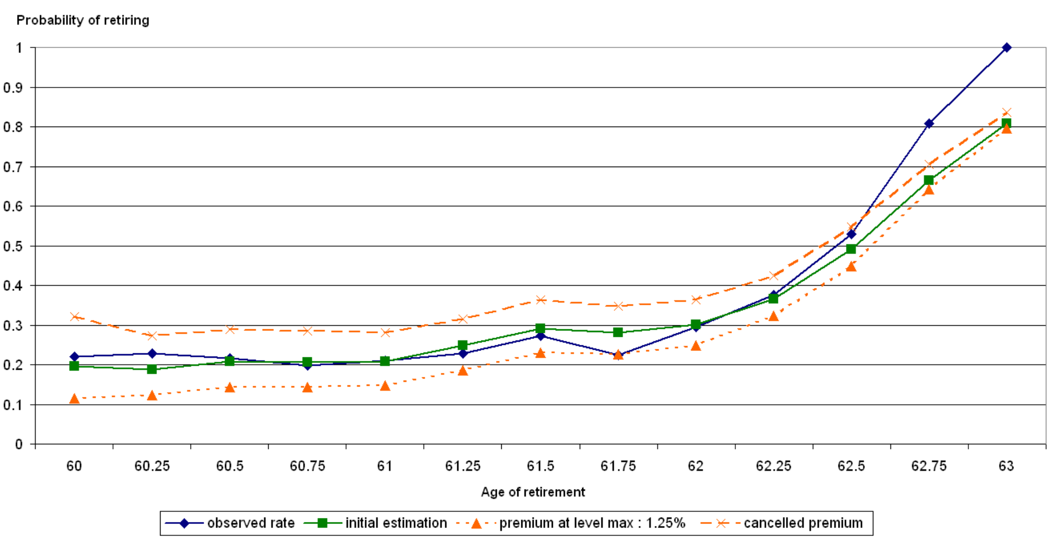
Figure 1.6 – Conditional probability to retire depending on the teacher's age and on the level of the penalty rate (cohorts 1944-1947)



More interesting is the simulation concerning the premium mechanism. Figure 1.7 shows that doubling the legal premium rate divides approximately by two retirement rates between 60 and 61 years old. Above 61, and especially after 62 years old, the decrease in the retirement rate is much lower. On the opposite, cancelling the premium rate would have increased by more than 50% the retirement rate between 60 and 61 years old. After 61, the increase in the retirement rate would have been less important. Another interesting feature lies in the fact that dashed lines converge towards the initial estimation line, showing that as times goes by, teachers are less and less sensitive to changes in the level of the premium rate. This argument is consistent with a labor disutility (or a preference for leisure) that increases with age, meaning that teachers are less sensitive to financial incentives as they get older.

Once again, this result must be qualified. Changing the financial incentive (i.e., the premium rate) affects a wider proportion of the sample, since 57% of teachers were indeed eligible for at least one extra quarter when they retired. In our scenario, changing the premium rate directly affects these teachers, but also those who have contributed for a number of quarters close to the legal requirement and who may decide to continue working to benefit from the premium rate. However, the behavioral responses to the two scenarios, namely cancelling or doubling the premium rate, is asymmetric: cancelling the premium has an higher effect (in absolute value) than doubling it.

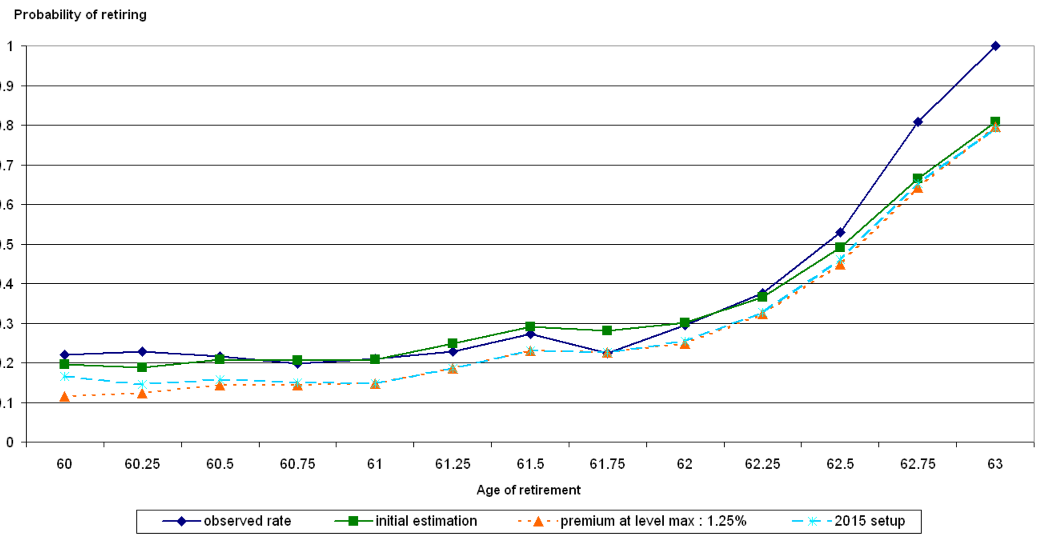
Figure 1.7 – Conditional probability to retire depending on the teacher's age and on the level of the premium rate (cohorts 1944-1947)



Aside from these scenarios, one could wonder what would have occurred if the cohorts 1944-1947 would have been immediately subject to the maximum premium and penalty rates implemented from 2015 onwards. In this scenario, the premium and penalty rates are both equal to 1.25%, the number of required quarters is equal to 166 and the limit age is 63.75 years old. Figure 1.8 plots the result.

This simulation is close to the doubled premium scenario (which is equivalent to set the premium rate at its maximal level), but conditions to get the full pension rate are now hardened. This means that in the 2015 setup, the budget cost should be lower than in the doubled premium scenario. This point addresses the question of budgetary effectiveness of the reform: at similar retirement rates, some reform setups may be fiscally preferable, as will be tackled in the next subsection.

Figure 1.8 – Conditional probability to retire depending on the teacher's age with the 2015 setup (cohorts 1944-1947)



Unfortunately these results do not allow to conclude definitively about the relative efficiency of these two incentive mechanisms, namely the premium (*the carrot*) and the penalty (*the stick*). For the first cohorts affected by the reform (cohorts 1944-1947), the premium coefficient was 6 times larger than the penalty one. In addition, the premium was introduced two years before the penalty, which explains that many more teachers were eligible to the premium within these cohorts.

Thus, our simulations may be depending on the composition of our sample. Their replication using more recent cohorts, whose structure in terms of gender, marital status, number of children and number of contributed quarters is likely to differ, could yield alternative results.

However, even if we have to be prudent when generalizing our results, the previous simulations illustrate the fact that the premium rate played a crucial role for the first cohorts affected by the reform. Offering a bonus to teachers that have contributed for the required minimum number of quarters is a strong incentive for postponing retirement. It is also a political strategy in order to increase the acceptability of the reform, at least in the short term.

1.5.3 Cost-benefit analysis

It can also be interesting to assess the effects of the 2003 pension reform on the State budget. Reforming the pension system should indeed increase teachers' mean retirement age. However, the relative effect of the 2003 reform both on wages and on pensions paid to teachers is *a priori* unclear since the schedule of this reform (see Table 2.1) is quite complex.

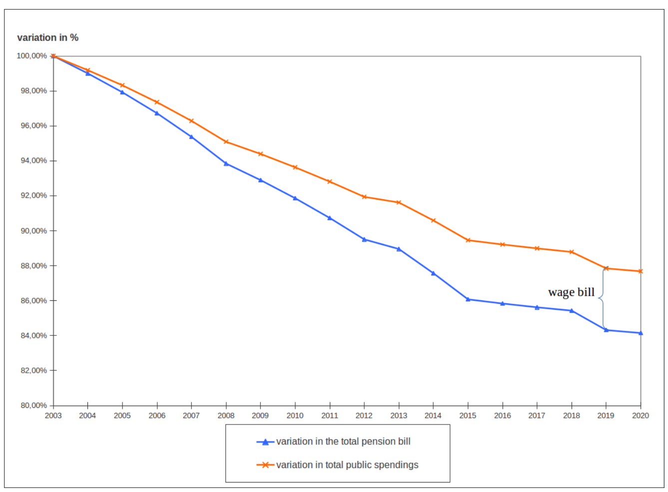
In order to evaluate, at least approximately, the budget effects of this reform we run the following exercise. We pool the four cohorts (1944-1947) impacted by the reform and we assume that all these teachers reach age 60 in a given year (namely, 2003, 2004, ..., 2020). First we simulate their individual retirement age according to the pension scheme in effect before the reform. We then calculate both the total wage and pension bills, and the average retirement age. These estimations are our baseline (counterfactual) values for the year 2003. We then repeat this simulation for each annual setup of the reform schedule (see Table 2.1). In this exercise we assume 1) that the survival probabilities remain unchanged over the period 2003-2020, and 2) that the statutory retirement age is always equal to 60 years old. These two assumptions are probably too strong and they should somewhat restrict the validity of this last exercise.

Evolutions of the mean retirement age and of the total wage bill are reported on Figure 1.9. From 2003 to 2015 we observe a steady rise in the mean retirement age, which is mechanically mirrored by the increase in the total wage bill, before a stabilization on a plateau around age 61, corresponding to the stabilization of the pension parameters from 2015 onwards. After 2015, the mean retirement age does not increase any more: at that time, the penalty and premium rates have reached their maximum values, and the total number of quarters required to get the full pension rate increases only by one quarter over these final years.

Figure 1.9 – Evolutions of the mean retirement age and of the total wage bill

Figure 1.10 shows how the total pension bill decreases when the reform schedule comes closer to the end of the period of study. Two mechanisms may explain this result. First, the increase in the mean retirement age tends to lower the total pension bill. Second, since conditions to get the full pension rate have been toughened over these years, teachers are more likely to retire before reaching the increasing number of quarters required to get the full pension rate (because of their high disutility of labor; see the estimated value of parameter k_0 in Table 1.4). Therefore they prefer to incur the penalty, which is relatively small, than continuing to work. This explains why their average retirement age increases by less than one year between 2003 and 2020, while the number of quarters required to get the full pension rate rises by 17 quarters over the same period. Consequently, a continuously decreasing proportion of teachers getting the full pension rate induces a reduction in the total pension bill. Moreover, the premium, which could counterbalance this reduction, is progressively more difficult to obtain.

Figure 1.10 – Evolutions of the total pension bill and of total public spendings



However, the improvement of public accounts is not as strong as expected. In fact, under the 2020 setup, the estimated decrease in the total pension bill is around 15% (see Figure 1.10), but this has to be corrected by the increase in the total wage bill of 135% (see Figure 1.9). The overall effect is then a 12% reduction in total public spendings, which means that the reduction in the total pension bill overcomes the increase in the total wage bill.

In the very first years of the reform (2004-2006), the variation in the total pension cost corresponds roughly to the variation in total public spendings, meaning that the increase in the total wage bill is negligible. The positive but small impact on public accounts is mainly driven by the toughening in conditions imposed to get the full pension rate. But as the reform strengthens, the gap between the two curves widens because the mean retirement age does not increase quickly enough. A significant mandatory increase in the statutory retirement age, say up to 63 years old, would probably reduce the improvement in public spendings because teachers would then be obliged to work longer without being able to obtain the number of quarters required to get the full pension rate. A way to improve the public budget would be to increase significantly the penalty rate, which should have not much consequences on the retirement rate (see Figure 1.6), because teachers have a relatively high disutility of labor.

1.6 Conclusion

Using a unique sample of 12,463 secondary-school teachers, we have evaluated the effect of the 2003 French pension reform on the retirement rate of teachers. To do so, we have used the Stock and Wise option value model (1990).

Estimates show that teachers are risk averse (the relative risk aversion coefficient is approximately 0.5) and have a quarterly discount factor close to unity (equal to 0.97). The coefficient representing their preference for leisure implies that 0.8 Euros of pension generates as much utility as 1 Euro of salary. This preference for leisure increases if the spouse is over 60 years old, and decreases if the household still includes at least one child less than 20 years old.

Simulations based on estimated structural parameters highlight the high sensitivity of teachers' retirement rates to financial incentives (namely, premiums) granted to teachers who continue working beyond the legal duration. This can result from the fact that our simulations are based on the first cohorts affected by the reform. Those cohorts have easily benefited from this premium because the increase in the legal duration of contribution was still limited.

Exploiting successively the annual reform setups, which have continuously changed from 2004 onwards, we have conducted a cost-benefit analysis with the 1944-1947 cohorts (those cohorts being directly impacted by the reform). That exercise shows how toughening the conditions required for obtaining the full pension rate affects the average retirement age. This age should progressively increase in order to reach 61 years old in 2020. Moreover, in year 2020, this reform should reduce by 12% the total public spending associated with the payment of wages and pensions of secondary school teachers above 60 years old. This reduction is essentially driven by three causes, namely *i*) the dramatic increase in the number of quarters required to get the full pension rate, *ii*) by the teachers' relative high disutility of labor, and *iii*) the penalty imposed to teachers who retire before reaching the legal duration of contribution.

Some important research issues should still be examined. A first interesting extension would be to incorporate the teachers' health status into the analysis. For instance, French (2005) has set forth a model with a utility function that depends on health, which is a more refined framework than ours relying on the AR(1) structure of errors in order to capture the persistence of health shocks. In fact, the individual health status at older ages is an important issue. Inducing teachers who would have otherwise retired, to continue working can generate additional costs for the public accounts: a higher frequency of sick leaves could deteriorate the budget impact of the reform. Last but not least, integrating data on working conditions could be of some interest. For instance, in some disadvantaged areas, financial incentives could be ineffective since teachers would like to leave as early as possible because of a higher disutility of labor.

Appendix A: Equivalence of decision rules in the dynamic programming and the option value models when future revenues are certain

The decision rule used by Stock and Wise (1990) differs from the dynamic programming decision rule, deduced from a Bellman equation. Here we show that the two rules are equivalent when future revenues are certain. Let us simplify the problem by setting $\gamma = k = \beta = \pi(s|t) = 1$, and by limiting the time span to three periods. Then the teacher can work during periods t and t_1, but retirement is mandatory in period t_2.

In the option value model, a teacher decides to work in period t if retirement in a later period yields a higher utility. In other terms, the teachers works in period t if:

$$\max[E_tV_t(t+1); E_tV_t(t+2)] > E_tV_t(t)$$

Using the analytical expression of the value function in each state, we get:

$$\begin{aligned} E_t(Y_t + w_t) + \max[E_t(B_{t+1}(t+1) + \xi_{t+1}) \ ; \ E_t(Y_{t+1} + \omega_{t+1})] > {} & E_t(B_t(t) + \xi_t) \\ & + E_t(B_{t+1}(t) + \xi_{t+1}) \end{aligned} \tag{1.9}$$

Conversely, the dynamic programming rule implies that the teacher works in period t if the expected utility stream induced by postponing retirement is greater than the one associated with current retirement:

$$E_t(Y_t + \omega_t) + E_t \max[V_{t+1}(t+1) \ ; \ V_{t+1}(t+2)] > E_tV_t(t)$$

which is equivalent to:

$$\begin{aligned} E_t(Y_t + \omega_t) + E_t \max[(B_{t+1}(t+1) + \xi_{t+1}) \ ; \ (Y_{t+1} + \omega_{t+1})] > {} & E_t(B_t(t) + \xi_t) \\ & E_t(B_{t+1}(t) + \xi_{t+1}) \end{aligned} \tag{1.10}$$

The two rules differ because the expected value of the maximum of two random functions is not the maximum of their expected values. Since the former is usually greater than the latter, it means that the left hand side of equation 1.10 should be greater than the left hand side of equation 1.9. Thus the option to delay retirement is underweight in the option value model. Under the assumption stipulating that survival rates are equal to one in each period, the uncertainty results from the stochastic part of the revenues, namely ξ_{t+1} and ω_{t+1}. If we consider in addition that revenues are perfectly known, the stochastic part disappears and calculating expectations of deterministic revenues becomes null and void. Therefore, equation 1.9 and equation 1.10 become equivalent and their common expression is:

$$Y_t + \max[B_{t+1}(t+1)\ ;\ Y_{t+1}] > [B_t(t) + B_{t+1}(t)]$$

A teacher who decided to retire in period t stays out of the labor market in period $t+1$ (i.e., retirement is an absorbing state). By contrast, a teacher who worked in period t has the choice either to work or to retire in period $t+1$. Since retirement is compulsory in period $t+2$, the decision in period t rule is given by:

$$E_{t+1}V_{t+1}(t+2) > E_{t+1}V_{t+1}(t+1)$$

which is equivalent to:

$$E_{t+1}(Y_{t+1} + w_{t+1}) > E_{t+1}(B_{t+1}(t+1) + \xi_{t+1})$$

In this case, there is no need to impose certainty of future revenues: if there is only one period left, the model comes down to a one-shot decision. Option value and dynamic programming decision rules match because there is no maximization any more. The teacher only considers what she would get in the two different states, and choose either to work or to retire accordingly.

In period $t+2$, there is no choice left, everyone retires.

Increasing the number of periods would not change the result. The following equations show how incorporating the work option in period t_2, before imposing mandatory retirement in t_3, increases the dimension of integration in order to compute the dynamic programming rule.

More precisely, the decision rule in a four-periods option value model is:

$$E_t(Y_t + w_t) + \max \left\{ \begin{array}{ll} E_t(B_{t+1}(t+1) + \xi_{t+1}) + E_t(B_{t+2}(t+1) + \xi_{t+2}) & ; \\ E_t(Y_{t+1} + w_{t+1}) + E_t(B_{t+2}(t+1) + \xi_{t+2}) & ; \\ E_t(Y_{t+1} + w_{t+1}) + E_t(Y_{t+2} + w_{t+2}) & \end{array} \right\} >$$

$$E_t(B_t(t) + \xi_t) + E_t(B_{t+1}(t) + \xi_{t+1}) + E_t(B_{t+2}(t) + \xi_{t+2})$$

while the decision rule in a four-periods dynamic program model is:

$$E_t(Y_t + \omega_t) + E_t \left[\max \left\{ \begin{array}{ll} B_{t+1}(t+1) + \xi_{t+1} + B_{t+2}(t+1) + \xi_{t+2} & ; \\ Y_{t+1} + \omega_{t+1} + B_{t+2}(t+1) + \xi_{t+2} & ; \\ Y_{t+1} + \omega_{t+1} + Y_{t+2} + \omega_{t+2} & \end{array} \right\} \right] >$$

$$E_t(B_t(t) + \xi_t) + E_t(B_{t+1}(t) + \xi_{t+1}) + E_t(B_{t+2}(t) + \xi_{t+2})$$

Nonetheless, in an environment with certainty, the stochastic part of revenues is left aside, and only the determinist term remains. The expectation of the maximum and the maximum of the expectations coincide and the two rules are equivalent.

Appendix B: Structure of errors

We assumed that $\nu_{t+1} = \rho\nu_t + \varepsilon_{t+1}$ with $\varepsilon_t \sim \mathcal{N}(0, \sigma_\varepsilon)$. Similarly, we assume that the initial distribution of ν_t is a mean-zero normal distribution, i.e., $\nu_t \sim \mathcal{N}(0, \sigma_\nu)$ and that ν_t is independent of ε_s for $s = t+1, ..., S$.

Then we get:

$$\begin{aligned}
\nu_{t+1} &= \rho\nu_t + \varepsilon_{t+1} \\
\nu_{t+2} &= \rho^2\nu_t + \rho\varepsilon_{t+1} + \varepsilon_{t+2} \\
\nu_{t+3} &= \rho^3\nu_t + \rho^2\varepsilon_{t+1} + \rho\varepsilon_{t+2} + \varepsilon_{t+3} \\
&\vdots \\
\nu_\tau &= \rho^{\tau-t}\nu_t + \sum_{j=0}^{\tau-t-1} \rho^j \varepsilon_{\tau-j} \\
\nu_{\tau+1} &= \rho^{\tau-t+1}\nu_t + \sum_{j=0}^{\tau-t} \rho^j \varepsilon_{\tau+1-j}
\end{aligned}$$

Because of the stochastic independance between ν_t and ε_s for $s = t+1, ..., S$, the variance of ν_τ is given by:

$$\begin{aligned}
Var(\nu_\tau) = E(\nu_\tau^2) &= \rho^{2(\tau-t)} E\left(\nu_t^2\right) + \sum_{j=0}^{\tau-t-1} \rho^{2j} E\left(\varepsilon_{\tau-j}^2\right) \\
&= \rho^{2(\tau-t)}\sigma_\nu^2 + \sum_{j=0}^{\tau-t-1} \rho^{2j}\ \sigma_\varepsilon^2 \\
&= \sigma_\nu^2 + (\tau - t)\sigma_\varepsilon^2 \quad \text{if } \rho = 1
\end{aligned}$$

and the covariance between ν_τ and $\nu_{\tau+1}$ is:

$$\begin{aligned}
Cov(\nu_\tau, \nu_{\tau+1}) = E(\nu_\tau \cdot \nu_{\tau+1}) &= \rho^{2(\tau-t)+1}\sigma_\nu^2 + \rho \sum_{j=0}^{\tau-t-1} \rho^{2j}\ \sigma_\varepsilon^2 + \underbrace{\varepsilon_{\tau+1} \sum_{j=0}^{\tau-t-1} \rho^j \varepsilon_{\tau-j}}_{=0} \\
&= \rho \left[\rho^{2(\tau-t)}\sigma_\nu^2 + \sum_{j=0}^{\tau-t-1} \rho^{2j}\ \sigma_\varepsilon^2 \right] \\
&= \rho\ Var(\nu_\tau)
\end{aligned}$$

For example, in a three-periods setup, the covariance matrix is the following:

$$\Sigma_3 = \begin{bmatrix} \sigma_\nu^2 & \rho\sigma_\nu^2 & \rho^2\sigma_\nu^2 \\ & \rho^2\sigma_\nu^2 + \sigma_\varepsilon^2 & \rho^3\sigma_\nu^2 + \rho\sigma_\varepsilon^2 \\ & & \rho^4\sigma_\nu^2 + (1+\rho^2)\sigma_\varepsilon^2 \end{bmatrix}$$

If the process ν_t is second-order stationary, namely the diagonal elements should be equal, which implies that the variance of ν_t is constant over time. Under that assumption, the covariance matrix becomes:

$$\Sigma_3 = \frac{\sigma_\varepsilon^2}{1-\rho^2} \begin{bmatrix} 1 & \rho & \rho^2 \\ \rho & 1 & \rho \\ \rho^2 & \rho & 1 \end{bmatrix}$$

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Chapter 2

Slow down before you stop: The effect of the 2010 French pension reform on older teachers' sick leaves

Denis Fougère and Pierre Gouëdard

The 2003 reform of the French public pension system introduced financial incentives to delay retirement of teachers. Seven years later, a second reform also increased the minimum legal retirement age from 2010 onwards. Using a sample of 38,652 high-school teachers, we find that teachers are more likely to take short sick leaves before their retirement after 2010. For that purpose, we define a control group consisting of cohorts not affected by the increase in the minimum legal retirement age, namely those born between 1948 and 1950. These cohorts can still retire at 60 years old. The treatment group, which consists of cohorts born between 1951 and 1953, must retire after 60 years old and 4 months (at least). We identify the effect of increasing the minimum retirement age on short sick leaves by comparing probabilities to take at least one sick leave between 55 and 59 years old across the two groups. Linear and nonlinear estimates of an autoregressive panel data model show that teachers affected by the reform have an increased probability to take short sick leaves before retirement, and that this discrepancy increases with age. Moreover, the effect seems to be stronger for younger cohorts.

Contents

2.1 Introduction

Many developed countries are currently encouraging their workers to postpone retirement. In a context of increased life expectancy, fiscal stability of the pension system is jeopardized if no additional contributions compensate for pensions benefits that have to be paid over a longer period for each retiree. This is especially true in France, where the system is of the pay-as-you-go kind (i.e., a contributory pension scheme). This means that the equilibrium directly depends on the worker-retiree ratio. As baby boomers massively retire since the end of the 90's, the French government has launched several reforms aiming at keeping people at work in order to ensure the fiscal sustainability of the system.

A lot of work has been dedicated to study the effect of later retirement on individual well-being and health. Main pitfalls consist in selection bias and endogeneity of the retirement decision. In fact, if people in bad health retire first, in other words if they self select out, then the impact on health of postponing retirement would be minimized, or a positive correlation between work and health might even appear (see, for instance, Kalwij and Vermeulen, 2008). Besides, health might affect work and vice versa. If it is obvious that bad health might induce early retirement, it is also true that staying employed helps maintaining a certain level of health. Some articles have indeed shown that the retirement decision is traumatic, and can trigger off a quick decline in the health status. Overall, many unobserved factors may blur the relationship between health and retirement and make it difficult to infer causation.

Most of these studies are comparing employees and retirees, in order to assess the impact of work on health. On the one hand, work is supposed to induce a stress detrimental to health, as shown by Ekerdt *et al.* (1983). For the US, Coe and Lindeboom (2008) find a temporary increase in the self-reported measure of health after retirement, their results being confirmed by Coe and Zamarro (2011) with European data. Similarly, Neuman (2008) provides evidence that retirement maintains health according to subjective measures, but finds no effect on objective health variables. Finally, Bound and Waidman (2008) show that retirement has a temporary positive effect on health of males (only) in the UK. On the other hand, retirement might be considered as a stressful event, after which retired people lack the physical and mental activity that is associated with work. As work increases activity, income and social networking, some other studies present alternative results. Bonsang *et al.* (2010), but also Rohwedder and Willis (2010), show how retirement induces a negative effect on memory. Dave *et al.* (2006) document how earlier retirement is associated with poor physical and mental health after retirement.

In this study, in order to compare health among similar workers facing different minimum legal age of retirement, we use the exogenous French reform of the public sector pension system that was implemented in 2010. Instead of health indicators, we use information on teachers' individual sick leaves provided by the "Département de l'Évaluation, de la Prospective et de la Performance", a department of the French Ministry of Education. We focus on secondary-school teachers, who, because of the reform, face different minimum legal retirement ages depending on their birth cohort. Prior to the 2010 reform, secondary-school teachers had the option to retire as soon as they reached 60 years old, even though some financial incentives to postpone retirement has been implemented after 2003.

However younger cohorts (namely those who reached 60 after 2011) were subject to a further constraint: the minimum legal retirement age increased from 60 to 61 and 2 months between 2011 and 2015. Therefore, before 60 years old, differences in the frequencies of short sick leaves taken by teachers of the same age but subject to different minimum legal retirement ages, might either be interpreted as voluntary sick leaves, or as the direct impact of the reform on teachers' health, provided that teachers from different cohorts are indeed similar. Thus the reform could have had an adverse effect in the sense that after its implementation, teachers were more likely to take short sick leaves, in order to restore their level of health, or to compensate the increase in the total disutility of work associated with the constraint to work longer.

Consequently, our study lies at the interface between two streams of the literature: the health-work relationship, and the issue of monitoring absenteeism. In fact, as soon as the concept of *voluntary sick leave* is mentioned, one can think of a way to prevent it. A large literature was dedicated to it, focused on the impact of sick-leave policy or disability insurance on worker absenteeism (see, e.g., Winkler, 1980, Meyer and Vescusi, 1995, Gruber, 2000, Neuheuser and Raphael, 2004, and Karlsson and Ziebarth, 2010). Unsurprisingly, the more generous the system, the more absenteeism. Other studies enumerate procedures which may reduce absenteeism, by aligning workers' and employers' objectives' (see, e.g., Bowers and Mc Iver, 2000, Bowers, 2001, Clotfelter *et al.*, 2009, and Ivatts, 2010).

Exploiting the increase in the minimum legal retirement age, which was implemented since 2011, we show how teachers in cohorts affected by this reform took more often short sick leaves than similar teachers of pre-reform cohorts. This increase in absenteeism propensity, that was hardly predictable by the legislator, has nevertheless a non negligible impact on the public budget, and may hinder fiscal gains of the reform. Over and above, if it appears that it is an opportunistic behavior that drives up sick leaves, some monitoring could be implemented to deter it, because, as Clotfelter *et al.* (2009) wrote, "whatever the importance of strong training, classroom experience, or advanced pedagogical methods for the scholastic development of students, these factors can have scant effect on a day a teacher is away from school."

The next section presents the institutional framework of the French reform. Section 2.3 develops a background model predicting how disutility of work and absenteeism increase before retirement when the minimum legal retirement age augments. Administrative data are detailed in Section 2.4. In Section 2.5, we present econometric models and we comment the results. Section 2.6 concludes.

2.2 Institutional framework

The 2003 pension reform of the French public sector is an extension of the 1993 reform of the private sector, which aimed at increasing the length of contribution by introducing a 10% pension penalty per missing year. Ten years after, F. Fillon (French Minister of Social Affairs) broadens the reform by extending it to the French public sector. In 2010 though, another layer is supposed to reinforce the financial incentives previously implemented. The progressive increase of the minimum legal age of retirement mechanically postpones the retirement decision of teachers.

Fougère and Gouëdard (2016) detail how the parameters of the pension calculation have been modified with the 2003 reform, and how new financial incentives have been introduced in order to shape the retirement decision of teachers. In this study though, we focus on the feature of the 2010 reform which consists in the increase of the minimum retirement age, as summed up in Table 2.1 (column 2). Two groups are therefore established: teachers born before 1950, who can still retire at 60, and the others who have to work longer. It has to be noted that all of these cohorts are in fact also impacted by the 2003 reform, since the number of quarters they have to contribute in order to get a full pension is increasing (it was set at 150 before the 2003 reform, see Table 2.1, column 4). We nonetheless label the 1948-1950 cohorts as the control group, since they still can retire at age 60.

Data concerning retirement were only available until the year 2013. However, if data are somehow

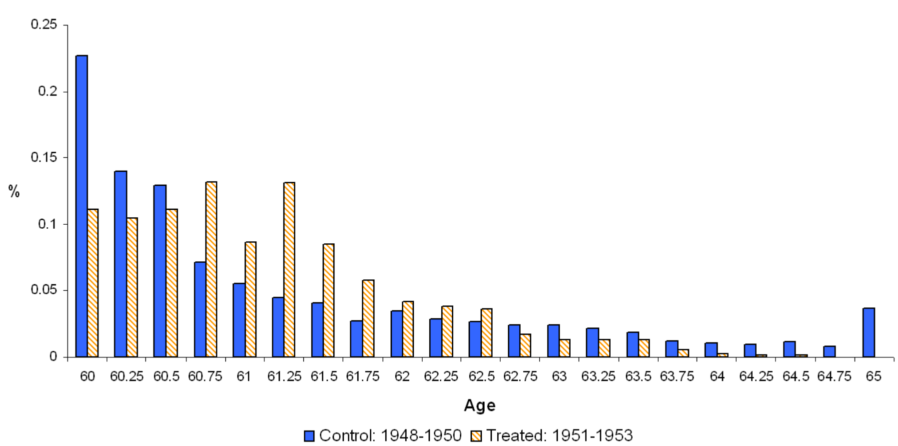
Table 2.1 – Reform's schedule

<table>
<tr><th>Year of birth</th><th>Minimum age of retirement</th><th>Year of opening rights</th><th>Number of quarters required</th></tr>
<tr><td>1948</td><td>60</td><td>2008</td><td>160</td></tr>
<tr><td>1949</td><td>60</td><td>2009</td><td>161</td></tr>
<tr><td>1950</td><td>60</td><td>2010</td><td>162</td></tr>
<tr><td>from Jan to Jun 1951</td><td>60</td><td rowspan="2">2011</td><td rowspan="3">163</td></tr>
<tr><td>from Jul to Aug 1951</td><td rowspan="2">60 and 4 months</td></tr>
<tr><td>from Sep to Dec 1951</td><td>2012</td></tr>
<tr><td>from Jan to Mar 1952</td><td rowspan="2">60 and 9 months</td><td>2012</td><td rowspan="2">164</td></tr>
<tr><td>from Apr to Dec 1952</td><td>2013</td></tr>
<tr><td>from Jan to Oct 1953</td><td rowspan="2">61 and 2 months</td><td>2014</td><td rowspan="2">165</td></tr>
<tr><td>from Nov to Dec 1953</td><td>2015</td></tr>
</table>

censored (younger cohorts are not all fully retired), the Figure 2.1 shows the impact of the reform on the retirement behaviour. To take into account the fact that teachers have not yet retired, we define a mass point at their age when they exit the sample. For instance, for teachers born in 1952 and not retired in 2013 (last year of the sample), a mass point is defined at age 61.

Unsurprisingly, the sharp drop observed in the retirement probability at age 60 is due to the increase in the minimum legal age of retirement for a teacher. The peak at 65 years old corresponds to the limit age: a teacher cannot work after. One can also observe a shift in the distribution towards the age of 61, that proves the reform indeed delays retirement.

Figure 2.1 – Distribution of the age of retirement



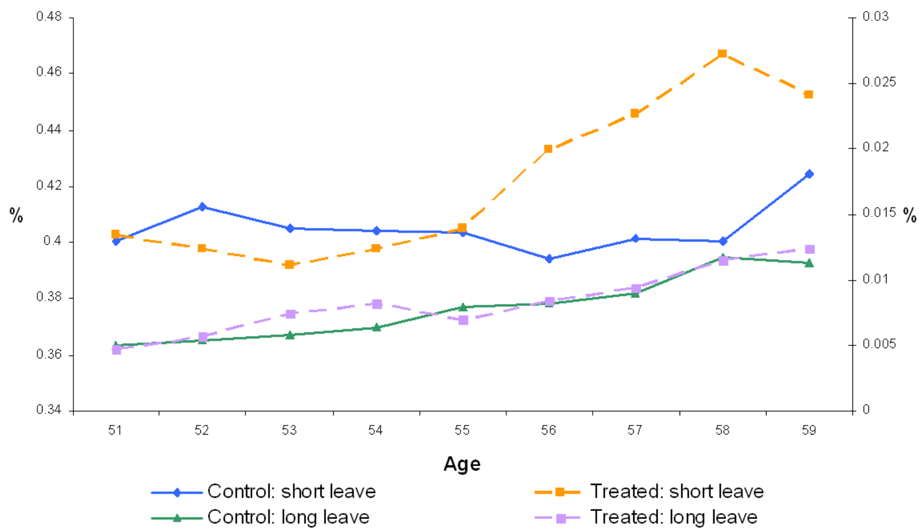
If we observe what happens before the age of 60 in Figure 2.2, different patterns arise. We can see at first there is no significant difference between control and treatment group concerning the probability of taking a long term sick leave. Conversely, a discrepancy appears between treated and control teachers for short term sick leaves. For instance, the probability to take a short term sick leave is 6.7 percentage point higher at age 58 if a teacher is in the treated group (born after 1951).

The hypothesis we would like to test is the following: does the increase in the minimum legal age of retirement increases the likelihood of taking a sick leave before 60 years old, namely before the minimum legal age of retirement? As previously stated, two narratives could explain why teachers would be more likely to take sick leaves.

First, we can consider that the increase in the minimum legal age of retirement has a direct impact (psychological for instance) on teachers' health, especially for those who were planing on retiring as early as possible (at age 60). Second, we can consider that the reform has an indirect impact, revealing an opportunistic behavior. As long as teachers were only *incited* to stay at work, teachers were still able to retire at age 60, eventually with reduced pension benefits. But as soon as they have been *constrained* to postpone retirement, they started to compensate the disutility inherent to a longer work spell by taking more short term sick leaves. One could consider that the reform, as unpopular as it was, decreased for teachers the threshold above which they take a sick leave. The fact that the probability to take long sick leaves does not seem modified (cf. Figure 2.2) highlights this potential opportunistic behavior, as long term leaves are far less manipulable than short ones. This approach was already proposed by Winkler (1980), who was studying absenteeism and focused on short-term absences, assuming they are less likely to be due to illness than long-term absences.

In the next section a background model is developed. It explains how modifying the minimum retirement age increases the global disutility of a teacher and might induce her to take more sick leaves. This model follows the second hypothesis previously formulated.

Figure 2.2 – Probability to take at least one sick leave during a year



2.3 A background model

Let us consider an optimization model with perfect smoothing consumption and with disutility of labor. In period t, the net utility of work is thus given by the difference between the utility resulting from consumption $U_W(C_t)$ allowed by wage W_t and the disutility of work $D_t(\tau)$, which is assumed to depend on the retirement date τ. The disutility is a stock variable that is discounted in each period of time, and that is increasing with age and with the time interval between the current date and the retirement age. Traditional disutility functions such as $D(t) = D_0 e^{\rho t}$ imply that disutility grows at a constant rate ρ. Here, the disutility function exhibits a growth rate $\frac{D'(t)}{D(t)} = \frac{\rho}{e^{t-s}-1}$ decreasing with age t. As documented in the behavioral literature, this accounts for the fact that the same absolute amount of time represents a smaller and smaller percentage of one's life span, while people tend to value less the future as they get closer to death (Green *et al.*, 1999; Reimers *et al.*, 2009).

We assume that the occurrence of a short sick leave, which may correspond to some kind of absenteeism, is an increasing function of the disutility of labor. The utility of retirement is denoted $U_R(C_t)$. It stems from the consumption allowed by pension benefits R_t. The lifetime utility of a representative teacher is then a value function depending on the retirement age τ, the age of death denoted T, and the consumption at time t denoted C_t. The intertemporal budget constraint stipulates that the total amount of consumption cannot exceed the flow of discounted revenues. The global discount factor is denoted ρ.

The value function is thus defined as:

$$V(\tau) = \max_{\tau, C_t} \int_s^\tau e^{-\rho(t-s)} (U_W(C_t) - D_t(\tau))\,\mathrm{d}t + \int_\tau^T e^{-\rho(t-s)} U_R(C_t)\,\mathrm{d}t$$

$$\text{w.r.t. :} \begin{cases} \int_s^T e^{-\rho(t-s)} C_t\,\mathrm{d}t \le \int_s^\tau e^{-\rho(t-s)} W_t\,\mathrm{d}t + \int_\tau^T e^{-\rho(t-s)} R_t(\tau)\,\mathrm{d}t \\ D_t(\tau) = \int_s^t e^{\rho(\tau - v)} D_0\,\mathrm{d}v \end{cases}$$

where D_0 represents the baseline (constant) value of work disutility, and $D_0(\tau) = 0$ when the teacher has not started to work. If λ denotes the Lagrange multiplier, the FOC with respect to C_t is given by:

$$\frac{\partial V(\tau)}{\partial C_t} = \int_s^\tau e^{-\rho(t-s)} U'_W(C_t)\,\mathrm{d}t + \int_\tau^T e^{-\rho(t-s)} U'_R(C_t)\,\mathrm{d}t - \lambda \int_s^T e^{-\rho(t-s)}\,\mathrm{d}t = 0$$

If $t < \tau$, $U_R(C_t) = 0$ for each t. Then the FOC implies that:

$$\int_s^\tau e^{-\rho(t-s)} U'_W(C_t)\,\mathrm{d}t = \int_s^\tau e^{-\rho(t-s)} \lambda\,\mathrm{d}t \qquad \text{hence} \qquad U'_W(C_t) = \lambda \text{ because } U'_W(C_t) > 0.$$

If $t \ge \tau$, $U_W(C_t) = 0$ for each t, and the FOC implies that:

$$\int_\tau^T e^{-\rho(t-s)} U'_R(C_t)\,\mathrm{d}t = \int_\tau^T e^{-\rho(t-s)} \lambda\,\mathrm{d}t \qquad \text{hence} \qquad U'_R(C_t) = \lambda \text{ because } U'_R(C_t) > 0.$$

These results are usual: they mean that, in every period, the optimal choice of consumption is the one such as the marginal utility of consumption is equal to the Lagrange multiplier set at its optimal value. The Lagrange multiplier is then the marginal utility obtained if the budget constraint were loosened by one unit. In other words, it measures the marginal utility of income.

The FOC with respect to τ is given by:

$$\begin{aligned} \frac{\partial V(\tau)}{\partial \tau} &= U_W(C_\tau) - U_R(C_\tau) - D_\tau(\tau) - \int_s^\tau e^{-\rho(t-\tau)} D'_t(\tau)\,\mathrm{d}t \\ &\quad + \lambda \left[(W_\tau - R_\tau) + \int_\tau^T e^{-\rho(t-\tau)} R'_t(\tau)\,\mathrm{d}t \right] \\ &= 0 \end{aligned}$$

Let us denote by $\Gamma_\tau = \lambda \left[(W_\tau - R_\tau) + \int_\tau^T e^{-\rho(t-\tau)} R'_t(\tau)\, \mathrm{d}t\right]$ the future increase in intertemporal utility associated with postponing retirement by one year. Then the optimal retirement age τ verifies:

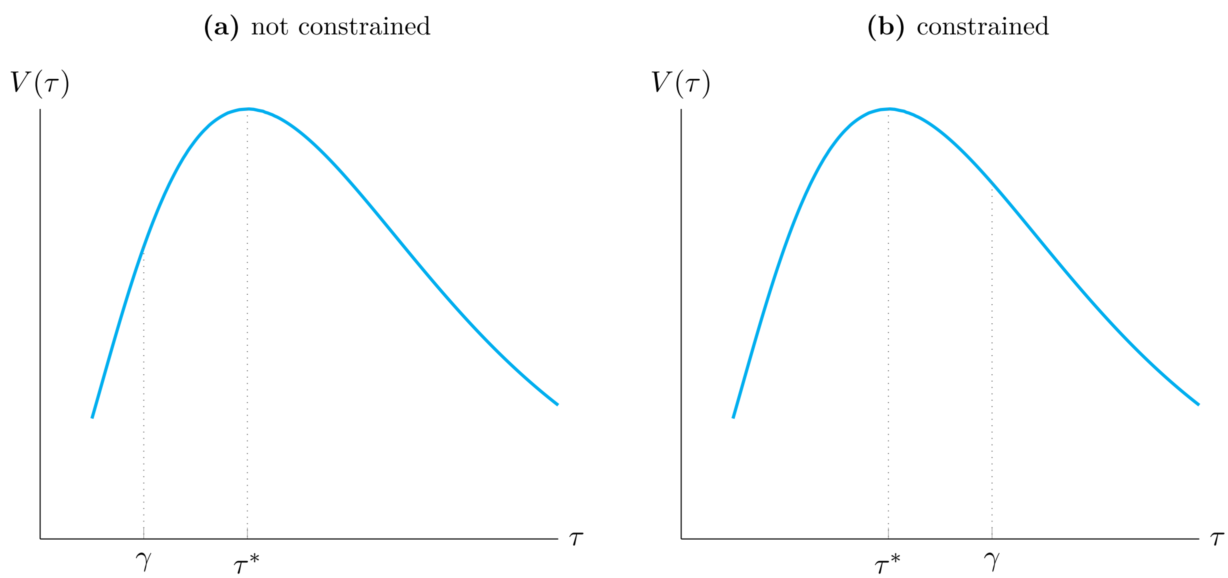
$$U_W(C_\tau) - D_\tau(\tau) - \int_s^\tau e^{-\rho(t-\tau)} D'_t(\tau)\, \mathrm{d}t = U_R(C_\tau) - \Gamma_\tau \tag{2.1}$$

where the left hand side of the above equation represents the marginal utility gain of working an additional year, whereas the right hand side corresponds to the marginal (opportunity) cost. We should also note that, if $D_\tau(\tau)$ is the direct disutility induced by an additional working year, $\int_s^\tau e^{-\rho(t-\tau)} D'_t(\tau)\, \mathrm{d}t$ represents the increase in disutility for all the worked years, since the functional form of D implies that this integral depends directly on the retirement age.

Retirement for unconstrained teachers, $\lambda > 0$:

According to Karush–Kuhn–Tucker conditions, the intertemporal constraint is saturated. Under the assumption that the value function is single peaked (see Figure 2.3), then a teacher whose optimal date of retirement τ^* occurs after the minimum legal age of retirement γ (say 60 years before the reform) can retire at τ^* and maximizes its utility (left panel). Condition 2.1 is respected.

Figure 2.3 – Single-peaked value function of a teacher



Retirement for constrained teachers, $\lambda = 0$:

In this case, the intertemporal constraint is not saturated. If a teacher's optimal date of retirement τ^* occurs before the minimum age of retirement γ (right panel), she has to wait until γ before retiring. These teachers are constrained by the minimum legal retirement age. For them, staying at work decreases their lifetime utility stream, and they retire as soon as they can, namely at time γ. This is in fact a corner solution, because the condition 2.1 is not verified. Still assuming that the value function $V(\tau)$ is single peaked, there exists a whole range of date j comprised in $\tau^* < j \leq \gamma$ such as:

$$
\begin{aligned}
& U_W(C_j) - D_j(j) - \int_s^j e^{-\rho(t-j)} D_t'(j)\,\mathrm{d}t < U_R(C_j) - \Gamma_j \\
& U_W(C_j) - D_j(j) - \int_s^j e^{-\rho(t-j)} D_t'(j)\,\mathrm{d}t < U_R(C_j) \qquad \text{since } \lambda = 0
\end{aligned}
\tag{2.2}
$$

For those constrained teachers abiding the equation 2.2, the functional form of the disutility in the optimization problem is modified. The disutility function does not depend on τ anymore, since it would result in an age not accessible for retirement, but on the state variable γ, the earliest age of retirement available to them. This yields a new form of the disutility function $D_t(\gamma) = \int_s^t e^{\rho(\gamma-v)} D_0\,\mathrm{d}v$. For these teachers, the optimization problem becomes trivial: they retire as soon as they reach age γ, and their consumption path is determined accordingly.

Impact of the change in legislation on the value function for constrained teachers

Now, let us compare the lifetime utility stream of two constrained teachers (i.e., their optimal retirement age is lower than the minimum legal retirement age): the first is affected by the reform and her minimum legal retirement age is γ', whereas the second is not affected and her minimum legal retirement age is γ, with $\gamma < \gamma'$. We can show that the level of disutility, at a given age, is greater for the teacher affected by the reform since :

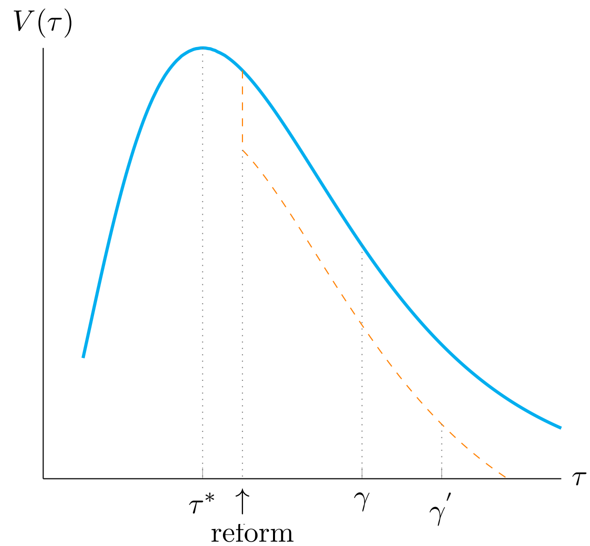
$$
D_t(\gamma') = \int_s^t e^{\rho(\gamma'-v)} D_0\,\mathrm{d}v > D_t(\gamma) = \int_s^t e^{\rho(\gamma-v)} D_0\,\mathrm{d}v \tag{2.3}
$$

which implies that the lifetime utility stream of the constrained teacher affected by the reform will be smaller than the one of the teacher not affected. Since we assume that the occurrence of a short sick leave is somewhat correlated with disutility, the following proposition holds:

Proposition 1 *At any age, the higher the minimum legal retirement age, the more likely a teacher takes short sick leaves.*

Figure 2.4 gives an illustration of this effect, using a standard single-peaked function modelizing $V(\tau)$. The plain line represents the stream of utility for a teacher who is not affected by the reform, but constrained by the minimum legal retirement age, i.e., for whom $\tau^* < \gamma$. Now, let us assume that the reform occurs unexpectedly when the teacher's age is between τ^* and γ years old[1]. This reform, which increases the minimum legal retirement age from γ to $\gamma^{'}$, modifies through the disutility channel the teacher's value function, $V(\tau)$, which is now represented by the dashed line. The reform induces a downward shift of the teacher's lifetime stream of utility. In that situation, if wages are not affected by absenteeism, and this is mostly the case for public teachers, the increase in disutility should trigger off more short term sick leaves.

Figure 2.4 – Illustration of Proposition 1



Further, we can study how the difference between these disutilities should evolve through time, and therefore how value functions will change as well. The result is given by the sign of the derivative of the difference between the disutilities of a teacher impacted by the reform and a teacher who is not:

$$\frac{\partial\left(D_t(\gamma^{'}) - D_t(\gamma)\right)}{\partial t} = D_0(e^{\rho(\gamma^{'}-t)} - e^{\rho(\gamma-t)}) > 0. \tag{2.4}$$

Once again, building on our assumption that disutility is correlated with short sick leaves, we formulate our second proposition:

Proposition 2 *The difference between the probabilities of teachers' short sick leaves after and before the reform increases with age.*

[1] it could also happen at another age: as long as it is before γ, results are unchanged

Figure 2.5 – Illustration of Proposition 2

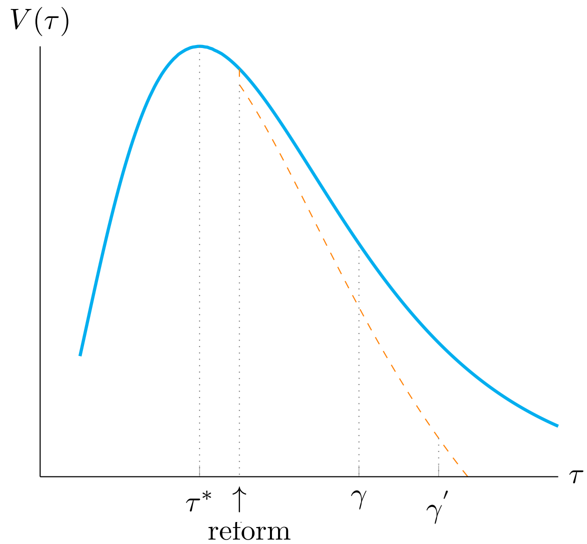


Figure 2.5 illustrates Proposition 2. Once again, using a standard single-peaked function, we illustrate the shape of the curve modelizing $V(\tau)$. After the reform, the gap between the post-reform and the pre-reform value functions increases with age, since the disutility grows faster for the teacher impacted by the reform according to equation (2.4). In other words, it means that a teacher affected by the reform is more and more likely to take short sick leaves as she gets closer to her retirement age.

We should also notice that the reform affects teachers whose ages are more or less distant from the minimum legal retirement age. Teachers close to the retirement age are marginally affected, while, for younger cohorts, changes in the pension scheme are more substantial and they occur at an earlier age, which leads to Proposition 3.

Proposition 3 *The earlier a cohort is affected by an increase in the legal minimal age of retirement, the sooner the frequency of its short sick leaves is shifted upward.*

Figure 2.6 illustrates the situation for two different cohorts. The older cohort can retire at time γ', whereas the younger one has to wait until γ''. So, the younger the cohort, the earlier the distortion of the sick leave profile.

Figure 2.6 – Illustration of Proposition 3

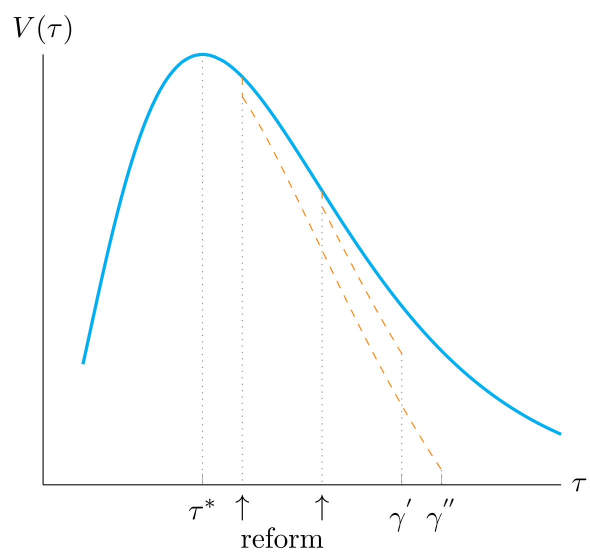
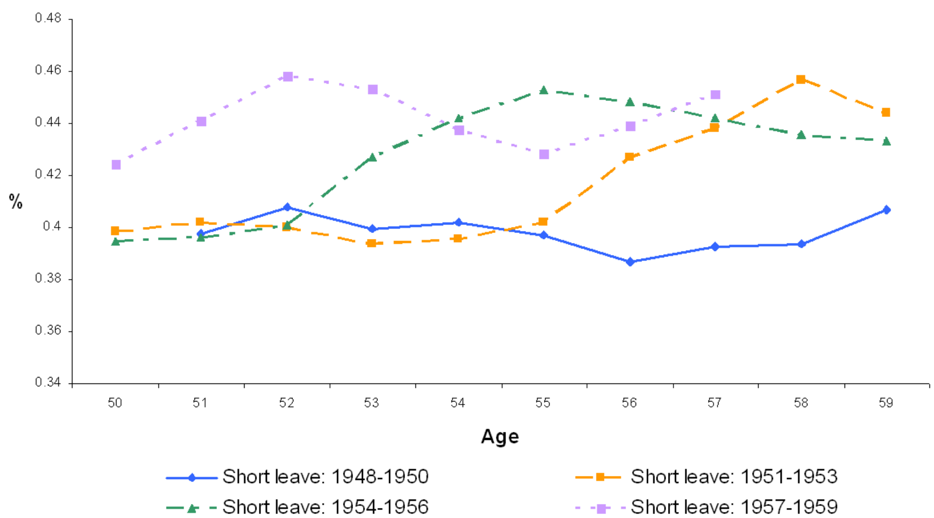


Figure 2.7 provides evidence supporting this last proposition. It shows how the different cohorts of our sample have reacted to the reform. The frequency of short sick leaves *(i)* increases from age 55 for the first cohorts affected by the reform (i.e., cohorts 1951-1953), *(ii)* increases sooner, namely from age 52, for younger cohorts, those born between 1954 and 1956, and *(iii)* still sooner for those

born between 1957 and 1959.

Unfortunately, unlike Propositions 1 and 2, Proposition 3 cannot be tested with our sample. In fact, as our data run from 2001 to 2013, teachers born in 1955 (or 1956) for instance, have only 58 (or 57) years old in year 2013. In the youngest cohorts, the proportion of right-censored observation is mechanically higher. That is why this third proposition is left aside for further research, when more complete data will be available.

Figure 2.7 – Observed frequencies of short sick leaves according to the teacher's age and to her birth cohort



2.4 Data

The administrative data that we use are hosted by the *"Département de l'Évaluation de la Prospective et de la Performance"* (DEPP, French Ministry of Education). We have constructed an original database by merging several distinct files. First, information concerning teachers' sick leaves is provided by annual administrative files which are pooled to follow teachers' sickness history over time. Then, by using teachers' identifiers, we have merged these files with administrative datasets providing information on some individual characteristics of teachers including their birth date, their gender, their marital status, their spouse's birth date, their number of children, and the identifiers of the high schools to which they are assigned.

By merging these two files we finally get a panel data set including cohorts born between 1948 and 1953, and containing information on all sick leaves taken by high school teachers followed from age 50 up to 60. However we only know the duration, but not the reason, of each sick leave spell.

2.4.1 Descriptive statistics

Our sample contains 38,652 teachers: 22,320 were born between 1948 and 1950 (this is our control group), 16,332 were born between 1951 and 1953 (this is our treatment group). As we have access to health files for school years 2000-2001 to 2012-2013, the age of teachers in the control group runs from 51 to 65 years old, whereas the age of those in the treatment group goes from 48 to 62 years old.

Table 2.3 presents some descriptive statistics on teachers who are 53 years old, which is the youngest age available in all considered cohorts. Seniority measures the total number of years spent in the public sector. Besides, the salary grade informs on the teacher's career achievement on a scale from 1 to 12. As it directly determines the teacher's wage level, the higher the salary grade, the higher the wage. For instance, a teacher with a grade of 9 would earn approximately 2100 euros (net) per month, whereas the highest grade corresponds to a monthly net wage of 2800 euros (in 2015). 90% of the teachers are employed full time at age 53. The average number of working hours per week is equal to 18 hours, which is also the mode of the distribution of the number of hours.

Other covariates reported in Table 2.3 describe some features of the working environment. The average class size is calculated as the ratio of the total number of students over the total number of teachers assigned to the school. However, as some teachers can split their working time between several schools, the total number of teachers in a school may be overestimated, and the ratio may be artificially low: in France, teachers in high schools are usually facing no less than 25 students in a classroom. This measure is nonetheless a proxy of the effective number of students a teacher is facing.

The average salary grade and the proportion of teachers above 40 in the school are proxies for the quality and the experience of the educational staff of the school. Besides, the priority education area variable indicates if a teacher is working in an area that benefits from additional means from the government, in order to cope with social and educational issues. In this type of schools, working conditions are supposed to be more difficult, and one can expect that teachers working there are more likely to take sick leaves.

Except for full time status, we observe some discrepancies between the control and treatment groups. It is interesting to consider that at the same age, the treatment group has a higher seniority, but a lower salary grade. In any event, this difference represents only a value of 0.05 on a scale varying from 1 to 12, which roughly corresponds to 10 euros a month. On top of that, since the average number of working hours per week in the treatment group is lower, while the proportion of full time teachers is the same, one can expect that teachers from the treatment group work less intensively than those in the control group (at age 53).

Whereas the proportion of married male teachers is comparable in the two groups, the proportion of married female teachers is slightly higher in the control group. However, this information must be carefully considered. In fact, the marital status is declared by the teacher to the administration: by default, it is set at single, and teachers have to produce administrative documents to update it. Usually, they do it if the change in their marital status confers them some advantages, for instance, some tax deductions, or a geographical mobility closing the distance between spouses. The discrepancy between these two ratios might as well reflect an administrative change, and does not necessarily jeopardize our identification strategy.

Since Table 2.3 presents descriptive statistics at age 53, it only reflects a snapshot of the reality. Appendix D depicts the evolution of mean differences from age 55 up to age 59. One can observe that for treated cohorts, the average salary grade increases slower than for control cohorts. Moreover, teachers in treatment cohorts are more likely to work full time, and their average number of working hours per week is higher. They also face larger class sizes. Conversely, both groups work equally in priority education areas.

2.4.2 Data reliability and waiting period

Research on health depends crucially on data quality. On the one hand, there is some controversy about the best indicators intended to measure health status: objective or subjective variables, weighted mean of different indicators... On the other hand, authors wonder how to get unbiased estimates, when data seem either "unreliable" (which is often the case for subjective indicators) or plagued with errors (like in administrative datasets). For instance, Bowers (2000) and Ivatts (2010) underline how the greater or lesser degree of rigor in the treatment of information ultimately affects the analysis quality.

In France, when a teacher is absent, the school reports it to the board which is in charge of the schooling district (there are 26 different districts on the territory). By doing so, the school is directly asking either for a replacement teacher or for a financial compensation if another teacher in the school has to take the additional workload. There is a direct incentive for the school to notify the absence because some assistance will be set. At the end of the year, once the board has collected all the data concerning its district, it transfers them to the statistical department of the French Ministry of Education (the DEPP). This decentralized process works if schools are indeed reporting efficiently absenteeism, which should be the case given their incentives. Conversely, one cannot really control if the encoding process is really accurate (the true reasons of sick leaves, the right number of days,...).

Another point that should be highlighted is the reform of the waiting period ("*délai de carence*") that occurred on January 1st, 2012. Before that reform, French teachers were paid from the first day they were absent from school. The reform introduced a waiting period before being compensated; in other words, the first day of absence was unpaid. This reform was finally cancelled two years after (on January 1st, 2014), when the government noticed that, when sick, teachers were taking more than one day off, or were not taking any day off until their health had so worsened that they had to take a long sick leave. Because the waiting period was fiscally not advantageous and politically unpopular, the government went back soon thereafter.

Therefore, one could imagine that during this period, the probability to take a short sick leave would drop. The reform was indeed intended to discourage this behavior. Consequently, the estimates of the impact of the 2003 retirement reform on absenteeism due to short sick leaves could be underestimated. However, it only concerns the two last years of the panel (which corresponds only to 13% of the number of teachers in our sample).

2.5 Empirical model and results

2.5.1 The econometric model

In order to test predictions of the theoretical model introduced in section 2.3, we propose to estimate the following linear probability model:

$$sick_{it} = \alpha + \beta sick_{it-1} + \gamma' X_{it} + \sum_{s=55}^{59} \delta_s \mathbb{1}(age = s) + \delta_{s\_ref} \mathbb{1}(age = s, birth > 1950) + \nu_i + \varepsilon_{it} \quad (2.5)$$

where $sick_{it}$ is a binary variable taking the value 1 if teacher i takes at least one short sick leave during the schooling year t (0 otherwise), ν_i is an individual fixed effect and ε_{it} is an error term potentially correlated over time. We assume that the model is autoregressive, which means that it accounts for state dependence. In fact, it is common to assume that previous health status influences the current one. The vector X_{it} includes controls presented in Table 2.3 (seniority, salary index, marital status,...). The set of binary coefficients δ_s captures the effect of age on the probability to take a short sick leave. These coefficients should increase with age: the older a teacher, the more likely she takes a sick leave. Coefficients of main interest are δ_{s_ref}, which represent interaction terms between the treatment (namely, being affected by the reform) and age dummies. If these coefficients are positive and statistically significant, it means that postponing the minimum legal retirement age increases the probability of taking short sick leaves before being allowed to retire.

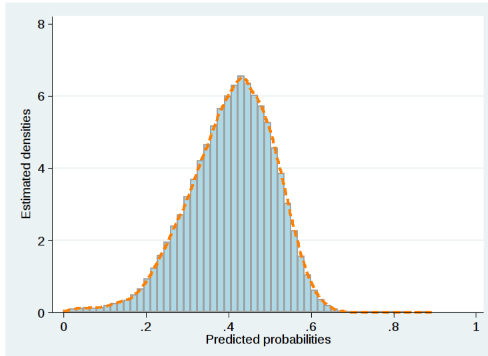
Several estimation procedures are possible, ranging from OLS to GMM techniques for AR(1) panel data. In the next subsection, we present OLS estimates before reporting results for autoregressive models.

2.5.2 Non-autoregressive models

First, we assume β to be null (see equation 1). The estimation of a linear probability model (LPM) raises several well known issues: if the distribution of errors is not uniform, estimated outcomes might exceed 1, which means they are biased and inconsistent. However, if the distribution of predicted outcomes is not extreme, essentially in the [0.3;0.7] range, the conditional expectation of the binary outcome may be assumed to be linear, and a LPM is usually estimated. In addition, Horrace and Oaxaca (2006) show that if only a few (or none) predicted probabilities fall outside the [0;1] interval, then the LPM estimates may be considered to be unbiased and consistent. Figure 2.8 presents the estimated density function of the conditional predicted probabilities (bars) given teachers' covariates. The short dashed line is the kernel estimator of this density. Our estimation shows that only 0.85% of our observations do no meet the [0;1] boundary constraint. The LPM is therefore a first reliable benchmark.

Beyond simple OLS estimation or pooled panel procedures, some individual-specific effects may be added to the model, either random or fixed (i.e., potentially correlated with the regressors). Under the assumption that true individual-specific effects are random, random-effect (RE hereafter)

Figure 2.8 – Density of LPM estimated probabilities



coefficients are efficient but inconsistent otherwise, whereas fixed-effect (FE hereafter) estimates remain consistent, but potentially inefficient. The first three columns of Table 2.4 report OLS, RE and FE parameter estimates of the linear model 3.3.2 with $\beta = 0$. The last three columns of Table 2.4 present average marginal effects derived from the estimation of three nonlinear models: a panel logit model with random effects, a conditional logit model with fixed effects, and a panel probit model with random effects.

Estimated coefficients associated with seniority and salary grade are often statistically significant. It is worth noting that the sign of the coefficient associated with salary grade is reversed when individual fixed effects are introduced. This could be due to an omitted variable bias. The positive bias affecting the salary grade coefficient advocates for a positive correlation between unobservable characteristics and salary grade. One can think that teachers with the highest salary grade are those who worked the hardest during their career, committing themselves into administrative tasks and developing projects within the school on top of their own teaching activity. Once corrected for individual fixed effects, which may capture the unobserved individual level of effort, one observes that an increase of one scale point in the salary grade decreases by 0.2 percentage point (pp hereafter) the probability of taking at least one short sick leave during the school year.

Coefficients associated with the interaction between gender and marital status cannot be estimated by using models with individual fixed effects because marital status is essentially time-invariant at these ages (55-60 years old). If we consider RE models, we find that, compared to married males, single males are more likely to experience at least one short sick leave during the school year: their probability is 2.6 pp higher. For single (respectively, married) females, it is 15.2 (respectively, 10.8) higher. Thus, women tend to take more often short sick leaves than men. One can suppose that women either take more care of their health status, or, as their spouse or parents get older, take more care of their elderly relatives. Notwithstanding the gender, married teachers take less often short sick leaves, which is in line with the literature stating that married people are happier, less often sick, and less likely to indulge in risky behavior (see, e.g., Verbrugge, 1979, Umberson, 1987, Goldman *et al.*, 1995).

The probability to take at least one short sick leave during the school year decreases both with the proportion of teachers above 40 and with the average salary grade of the teachers in the same high school. One can assume that the more homogeneous the educational staff, the more bonded: in that case, peer pressure could lift the threshold over which a teacher decides to take a short sick leave. Moreover, teachers whose working conditions are more demanding are more likely to be sick. The class size matters: when the teacher faces a larger class, her working conditions are more difficult because she has to maintain discipline, to speak louder, to face a higher level of stress,... For similar reasons, it seems that working in a priority education area also affects health status: in that situation, the probability to take a short sick leave during the year is increased by around 3 pp.

Finally, the probability to take at least one short sick spell during the year is slightly increasing with age, from age 55 to age 59. The increase is rather smooth, except at age 59, where the probability more than doubles. For a given age, it is also possible to compare teachers affected by the reform with those in the pre-reform cohorts. For instance, at age 56, the probability to take a short sick leave increases by 3.5 pp after the reform (according to the RE model). At any age above 55, the probability to take a short sick leave is increased after the reform, whatever the estimation method. This result is in line with Proposition 1 of the theoretical model presented in Section 2.3.

In addition, examining the estimated effect of age for the post-reform cohorts gives us an idea of the effect of the reform. Did it only shift the probability to take a short sick leave at any age, or did it also distort the age profile of these probabilities? If we consider the logit RE estimates, a teacher affected by the reform does not take more often a short sick leave at age 55. However, her probability to be sick at age 56 is increased by 5.4% pp, and by 5.7% percentage point at age 57. This increasing gap backs up Proposition 2 of the theoretical model (see Section 2.3). The only exception is that at age 59 the interaction term between age and the post-reform dummy decreases. This might be due to the sudden increase at age 59 in the probability to take a short sick leave by teachers in the pre-reform cohorts: therefore, the gap between control and treatment groups is smaller, and the estimated coefficient measuring the impact of the reform at age 59 is smaller, although positive and statistically significant.

2.5.3 Autoregressive models

Now we assume that the current health status in year t may depend on the previous one (in year $t-1$). As it is well known, allowing for state dependence leads us to some econometric concerns. The error term, that includes the individual-specific effect α_i, is positively correlated with $y_{i,t-1}$ which leads to inconsistent (and upward biased) OLS estimates. Similarly, in the demeaned regression of $(y_{it}-\bar{y}_i)$ on $\Delta y_{i-1} = (y_{it-1}-\bar{y}_{i-1})$ and $(x_{it}-\bar{x}_i)$, the error term $(\varepsilon_{it}-\bar{\varepsilon}_i)$ is correlated with Δy_{i-1} and the procedure yields (downward) biased estimates. Inconsistency also affects RE estimates, which are a linear combination of between (BE hereafter) and FE estimates.

Other estimation methods must then be implemented. The starting point is the first-difference model:

$$y_{it} - y_{it-1} = \gamma(y_{it-1} - y_{it-2}) + (x_{it} - x_{it-1})'\beta + (\varepsilon_{it} - \varepsilon_{it-1})$$

which is also inconsistent.[2] To tackle this problem we successively use the Arellano and Bond (1991) estimator (hereafter, AB) and the Blundell and Bond (2000) estimator (hereafter, BB).

These two estimators are obtained by GMM methods, which rely on the assumption that y_{i1} is uncorrelated with future disturbances $\varepsilon_{i2},...,\varepsilon_{iT}$. However, in nonlinear cases, we must use a maximum likelihood method, which may be crucially affected by the way initial conditions are treated. In this work, we consider individual-specific effects to be either purely random or somehow correlated with the observed regressors. In the former case, y_{i1} is considered to be exogenous, and thus does not depend on the individual-specific effect. The contribution of the initial condition to the likelihood function is therefore multiplicative, and it can be separated from the integrand calculated with respect to the distribution of the individual-specific effect. This assumption corresponds to the well-known RE probit model. Assuming exogenous initial conditions is probably not valid when considering health status at the end of a teacher's career, because health is mostly correlated with individual characteristics and personal history.

There are mainly three ways to take into account endogenous initial conditions. For instance, Heckman (1981) has proposed to specify a reduced form equation. This equation should be a func-

[2]Because the lagged regressor $(y_{it-1} - y_{it-2})$ is correlated with the error term $(\varepsilon_{it} - \varepsilon_{it-1})$.

tion of some excluded regressors (IVs). Orme (2001) writes the individual-specific effect as a function of another time invariant effect, which is by construction uncorrelated with the initial status. By regressing the initial status on predetermined regressors in a first step, he then gets estimated residuals, which are in turn used as regressors in a RE probit model. Finally, Wooldridge (2005) treats the unobserved heterogeneity term as a function of the mean of the covariate vector (like in Mundlak, 1978) and of the initial status of the dependent variable. In the latter approach, initial conditions are not specified: the estimator is obtained by a conditional maximum likelihood procedure, since the distribution of the dependent variable is conditional on the regressor and on the initial status. In our application, we use successively three specifications for estimating the non-linear autoregressive model: the RE probit model, the Orme procedure and the Wooldridge estimator.[3]

All estimates are reported in Table 2.5. The last three columns report average marginal effects (AME) estimated with the three specifications that we consider for the nonlinear autoregressive model. Introducing the lagged dependent variable increases greatly the explanatory power of the regression, since the R^2 coefficient goes up from 0.05 to 0.13. However, as previously stated, OLS and FE estimates suffer from upward and downward biases, respectively. Yet, these estimates set bounds for the true value of the coefficient of the lagged dependent variable. After taking into account the endogeneity of the lagged dependent variable (by using GMM methods) and of the initial condition (by using nonlinear models), the estimated coefficient associated with the lagged dependent variable decreases significantly: it is estimated to be approximately equal to 0.05 in the GMM approach, and to 0.07 with a nonlinear method. This implies that state dependence is rather limited, and that the endogeneity issue mostly results from individual-specific effects.

Estimated coefficients associated with seniority and grade salary are again statistically significant. Conversely, being single and/or a woman, teaching more hours, facing larger classes or working in a priority education area increases the probability to take at least one short sick leave during the schooling year. Likewise the FE estimator, the AB estimator cannot to estimate coefficients associated with time-invariant marital status.

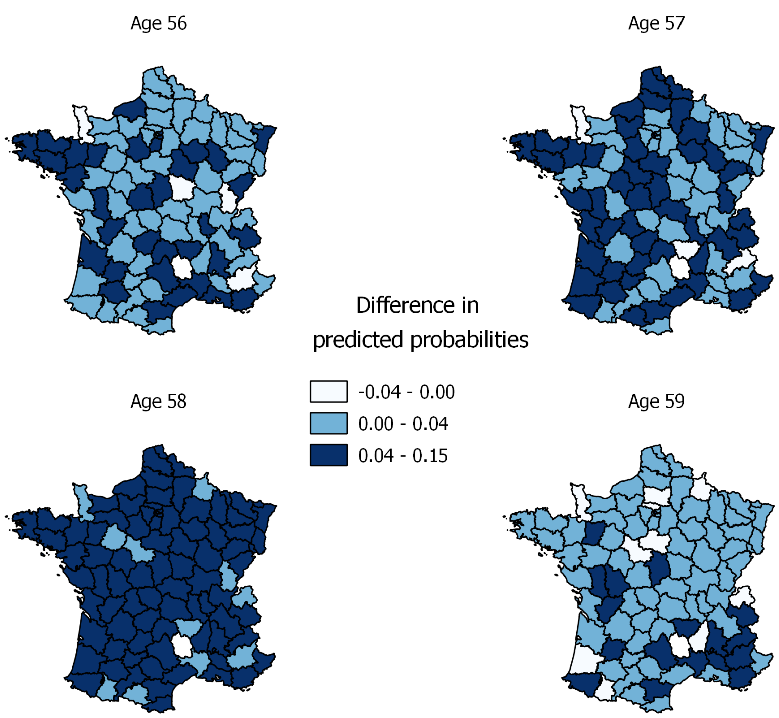
Finally, the most interesting result is that the introduction of the lagged dependent variable has not cancelled all the age effects (see Table 2.5). We still observe that the probability to take a short sick leave during the school year increases slightly with age. But most of all, age dummies

[3]The Heckman estimator, the most computationally intense one, is not presented because the statistical software failed to calculate the likelihood gradient.

for teachers affected by the reform are all significant, confirming once again Proposition 1 of our theoretical model (see Section 2.3). Furthermore, interaction terms between age and post-reform dummies are still increasing with age, in line with Proposition 2 of our theoretical model. For instance, if we consider the BB estimation, for a teacher affected by the reform, the probability to take a short sick leave is increased by 3.2 pp at age 56, by 3.6 percentage point at age 57 and by 5.4 percentage point at age 58.

Figure 2.9 presents the geographical distribution of differences in predicted age effects. At any age, the probability to take at least one short sick leave during the year is computed, both for pre- and post-reform cohorts. Differences are then plotted on the France map. One can see that these differences increase with age, except at age 59. However, no geographical pattern can be detected.

Figure 2.9 – Spatial distribution of the age effects



2.5.4 Elasticities

We have found that teachers affected by the reform are more likely to take short sick leaves. For the policy designer, or the national accountant, a crucial point is to estimate the elasticity of the probability to take short sick leaves with respect to the increase in the minimum legal retirement age. As shown in Table 2.2, the minimum retirement age has increased for cohorts born between 1951 and 1955 (and after). It is then possible to estimate the variation in the probability of taking a short sick leave for each cohort.

Table 2.2 – Minimum retirement age for cohorts born between 1948 and 1955

Year of birth	Minimum retirement age	Number of extra months
1948 - 1950	60	0
1951 (until June)	60	0
1951 (from July)	60 years 4 months	4
1952	60 years 9 months	9
1953	61 years 2 months	14
1954	61 years 7 months	19
1955 +	62 years	24

Table 2.2 reports the number of extra months that teachers must spend working depending on their birth cohort. Focusing once again on the 1948-1953 cohorts, the variation in the number of additional months allows us to estimate the model described by the following equation:

$$sick_{it} = \alpha + \beta sick_{it-1} + \gamma^{'} X_{it} + \sum_{s=55}^{59} \delta_s \mathbb{1}(age = s) + \delta_{s\_ref} \mathbb{1}(age = s) \times \text{extra\_month} + \nu_i + \varepsilon_{it} \quad (2.6)$$

This specification assumes that the effect of the reform is constant: if the minimum age of retirement increases by one month, the probability to take a short sick leave at age s augments by δ_{s_ref}, regardless of the effective number of additional months required. By construction, the estimated coefficient δ_{s_ref} is a weighted average of coefficients for teachers differently affected by the reform. A way to disentangle these effects, namely to allow for heterogeneity across cohorts, would be to add interaction terms between age dummies and dummies corresponding to the different number of extra months (4, 9 or 14 months).

Table 2.6 reports the variation in the probability to take a short sick leave for the different cohorts.[4] The coefficient δ_{s_ref} corresponds to the elasticity of the probability to take a short sick leave, it is reported in the first column of Table 2.6. All these elasticities, except the one at age

[4]Detailed results are presented in Table 2.9 in Appendix E

59, are statistically significant. For instance, a one-month increase in the minimum retirement age increases the probability to take at least one short sick leave during the year by 0.45 percentage point at age 56. Columns 2-4 in Table 2.6 multiply these elasticities by the number of extra months imposed by the reform, in order to get the effect for each cohort. Thus, for a teacher born in 1952, the effect of a one-month increase is 9 times the elasticity (according to Table 2.2), which implies an increase in the probability by 4.1 percentage point.

So far, our results shed light on the extensive margin: after the reform, teachers take more often short sick leaves. The impact of the reform on the intensive margin is more complex to identify and cannot be assessed by a simple difference: since the extensive margin is modified, groups of teachers affected or not by the reform are different. Being affected by the reform can be interpreted as the combination of two effects, namely by the change in the cumulated number of days out for teachers taking short sick spells (i.e., the intensive margin) and by the variation in the probability to take a short sick leave (i.e., the extensive margin). A type-I Tobit model, in which the dummy variable *sick* is replaced in equation 2.6 by the duration of the spell, allows us to estimate the overall effect. Parameters estimates of this model are presented in Table 2.9 and a summary is plotted in Figure 2.10.

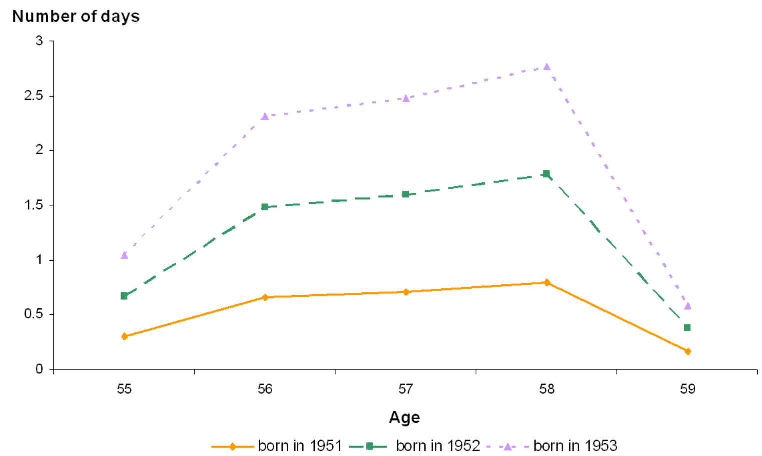
Figure 2.10 plots for every age, the difference in cumulated days of short sick leaves taken by teachers affected by the reform (namely, those born between 1951 and 1953), compared to teachers who are not affected by the reform (namely, those born between 1948 and 1950). All the values are statistically significant at the 1% level, except at age 59, where the estimates are only significant at the 10% level. For instance, one can observe that, at age 57, teachers born in 1952 take on average 1.6 more days out during the school year, whereas teachers born in 1953 take on average, at the same age, 2.5 more days out compared to the control group.

2.5.5 Robustness checks

In order to test the validity of one of our hypothesis assumptions, namely the fact that short sick leaves are under the control of teachers, we run a first additional regression whose main results are summed up in Table 2.7.[5] Recall that we suppose increasing the minimum legal retirement age should increase work disutility, and induce teachers to take more often short sick leaves. This implies

[5]Detailed results are presented in Appendix F.

Figure 2.10 – Changes in the duration of sick spell depending on the birth cohort



that the threshold above which a teacher would take a short sick leave is lowered by the reform. This hypothesis should hold for short sick leaves, but not for long sick leaves, which are less manipulable by teachers and more exogenous (due to more serious diseases).

Therefore, we first estimate the probability for a teacher to take a long sick leave, by replicating equation 3.3.2. Results show that there are no differences between pre- and post-reform groups, since age dummies for teachers born after 1951 are never statistically significant. This backs up the idea that long sick leaves are not manipulable, and that their frequency stay constant over time regardless of the reform, contrary to short sick leaves.

Second, we replace the post-reform cohorts by "placebo" cohorts, in order to check whether our control group is valid or not. Age dummies labelled as ref now captures the increase in the probability to take a short sick leave for teachers born between 1948 and 1950, compared to teachers born between 1945 and 1947 (namely, the placebo cohorts). It is worth noting that these two groups of teachers face the same minimum legal retirement age, i.e., 60 years old. Once again, none of the age dummies are statistically significant, which means that the control group is not different from previous cohorts, and results found on the post-reform cohorts are not a statistical artefact. Nevertheless, there is a positive, and statistically significant, difference at age 59, which implies that teachers born between 1948 and 1950 took more often short sick leaves at this age. This explains why we find a smaller difference with teachers born between 1951 and 1953 (the post-reform group), which is slightly in contradiction with Proposition 2.5.

2.6 Conclusion

The 2003 reform of the French pension system introduced financial incentives in order to postpone the retirement age. The 2010 reform increased the minimum legal retirement age after 2011. First cohorts affected by the reforms (in our case, teachers born between 1943 and 1950) were only concerned by financial incentives, while those born in 1951 and after were both subject to the increase in the minimum legal retirement age and to financial incentives. In order to identify the impact of increasing the minimum legal retirement age on the frequency of short sick leaves, we compare teachers born between 1948 and 1950 who can still retire at age 60, and teachers born between 1951 and 1953, who have to work up to the age of 60 and 4 months at least.

Since we consider sick leaves *before* age 60, namely when no teachers have already retired, we eliminate the potential selection bias that plagues studies comparing workers and retirees. In fact, one can consider the change in the probability to take a short sick leave as the impact of the reform on teachers: either directly on their health, or on their disutility, both leading to a higher probability to take sick leaves. In order to back up this second rationale, we have proposed a model of retirement decision in which teachers maximize their utility by choosing their level of consumption and their date of retirement.

This model shows that, for teachers whose optimal date of retirement is lower than 60 years old, their discounted stream of work disutility is an increasing function of the minimum legal retirement age. In addition, the gap between the utility streams of a teacher affected by the reform and a teacher who is not, is increasing with age. This implies that in order to compensate for their utility loss, teachers have increased the frequency of paid short sick leaves before retiring.

Parameter estimates of reduced-form econometric models are unambiguous: regardless of the methodology, teachers affected by the reform exhibits a higher probability to take short sick leaves. The effects of the reform are found to be statistically significant at the 1% level. The occurrence of a short sick leave during the school year increases by 3.2 percentage points (pp hereafter) at age 56, 3.6 pp at age 57, 5.4 pp at age 58 and 1.5 pp at age 59.

Further research should consider younger cohorts in order to assess if these effects are long lasting. One could imagine that teachers far away from their retirement age feel less concerned by these issues, suffer less from the psychological effect of a later minimum legal retirement age, and would be less likely to *manipulate* their short sick leaves. In addition, if we consider the impact of the reform being due to the revision of agents' anticipations, we can wonder if this impact, measured among teachers close to retire at the moment of the reform, is more than an entitlement effect.

Besides, this effect of the reform raises the question of the issue of a better monitoring for short sick leaves. This was originally the aim of the "waiting period" mechanism, which was intended to be a disincentive for opportunistic behavior, but that appeared to be poorly designed and was rapidly withdrawn. This leaves room for proposing a policy which deters more efficiently shirking behavior. Finally, an extension of this work, maybe the most interesting one for the policy maker, would be to assess the cost of keeping older teachers at work, as well as the quality of service they provide. Usually, teachers in bad health should self select out as soon as they can. Nevertheless, keeping them within the labor force could imply a higher cost for social security accounts, this cost potentially exceeding fiscal benefits implied by the retirement pension reform.

Table 2.3 – Summary statistics for teachers aged 53

	Mean for cohorts 1948-1950 (Control)	Mean for cohorts 1951-1953 (Treatment)	Difference
seniority	14.89	15.63	0.74 ***
	(0.07)	(0.07)	
salary grade	9.02	8.97	-0.05 ***
	(0.012)	(0.014)	
full time	0.90	0.90	0.00
	(0.002)	(0.002)	
working hours per week	18.02	17.77	-0.25 ***
	(0.016)	(0.02)	
married male	0.27	0.26	-0.01 **
	(0.003)	(0.003)	
single male	0.09	0.10	0.01 ***
	(0.002)	(0.002)	
married female	0.42	0.40	-0.02 ***
	(0.03)	(0.004)	
single female	0.22	0.23	0.01 ***
	(0.003)	(0.003)	
average class size in the school	12.43	12.27	-0.18 ***
	(0.02)	(0.02)	
proportion of teachers over 40 in the school	0.60	0.58	-0.02 ***
	(0.001)	(0.001)	
salary grade of teachers in the school	7.12	7.02	-0.10 ***
	(0.004)	(0.005)	
priority education area	0.05	0.07	0.02 ***
	(0.001)	(0.002)	
number of observations	22,320	16,332	

Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: Calculations by the authors from administrative files (Ministry of Education, Paris)

Table 2.4 – Parameter estimates of non-autoregressive models

	Linear			Non Linear: AME		
	OLS	RE	FE	Logit-RE	Logit-FE	Probit-RE
seniority (in years)	-0.00230***	-0.00251***	-0.000410	-0.00341***	-0.000683	-0.00328***
	(0.000105)	(0.000171)	(0.000752)	(0.000224)	(0.000973)	(0.000216)
salary grade	0.00549***	0.000901*	-0.00167***	0.000923	-0.00220***	0.000975
	(0.000502)	(0.000525)	(0.000540)	(0.000674)	(0.000715)	(0.000652)
number of hours per week	0.00333***	0.00675***	0.00961***	0.00938***	0.0121***	0.00907***
	(0.000330)	(0.000398)	(0.000455)	(0.000514)	(0.000660)	(0.000495)
single male	0.0265***	0.0219***		0.0222***		0.0213***
	(0.00327)	(0.00520)		(0.00699)		(0.00673)
single female	0.153***	0.149***		0.188***		0.181***
	(0.00263)	(0.00441)		(0.00563)		(0.00545)
married female	0.110***	0.115***		0.147***		0.142***
	(0.00224)	(0.00370)		(0.00493)		(0.00476)
share of teachers over 40 in the school	-0.0434***	-0.0517***	-0.0543***	-0.112***	-0.0708***	-0.107***
	(0.0118)	(0.0141)	(0.0161)	(0.0171)	(0.0202)	(0.0166)
average salary grade in the school	-0.0162***	-0.0111***	-0.00336	-0.0151***	-0.00455	-0.0146***
	(0.00237)	(0.00264)	(0.00286)	(0.00328)	(0.00373)	(0.00317)
average class size in the school	0.0109***	0.00760***	0.00210***	0.0109***	0.00258***	0.0103***
	(0.000448)	(0.000652)	(0.000800)	(0.000745)	(0.000974)	(0.000699)
priority education area	0.0349***	0.0203***	0.00705	0.0273***	0.0101	0.0264***
	(0.00438)	(0.00536)	(0.00605)	(0.00665)	(0.00760)	(0.00644)
age_55	0.00908**	0.00632*	0.000565	0.00700	0.000918	0.00667
	(0.00370)	(0.00330)	(0.00370)	(0.00444)	(0.00474)	(0.00430)
age_56	0.00483	0.00490	-0.000843	0.00532	-0.000659	0.00516
	(0.00369)	(0.00337)	(0.00409)	(0.00451)	(0.00525)	(0.00437)
age_57	0.0160***	0.0166***	0.00911**	0.0212***	0.0124**	0.0206***
	(0.00373)	(0.00346)	(0.00455)	(0.00456)	(0.00600)	(0.00442)
age_58	0.0191***	0.0204***	0.0111**	0.0263***	0.0152**	0.0253***
	(0.00375)	(0.00352)	(0.00507)	(0.00463)	(0.00673)	(0.00448)
age_59	0.0446***	0.0471***	0.0371***	0.0617***	0.0484***	0.0596***
	(0.00384)	(0.00366)	(0.00564)	(0.00470)	(0.00821)	(0.00454)
age_55 post-reform	-0.0000657	0.00701	0.00969*	0.0115*	0.0126*	0.0108*
	(0.00522)	(0.00479)	(0.00515)	(0.00648)	(0.00671)	(0.00628)
age_56 post-reform	0.0348***	0.0388***	0.0394***	0.0532***	0.0505***	0.0512***
	(0.00525)	(0.00489)	(0.00517)	(0.00648)	(0.00729)	(0.00628)
age_57 post-reform	0.0380***	0.0413***	0.0423***	0.0559***	0.0537***	0.0539***
	(0.00530)	(0.00496)	(0.00519)	(0.00649)	(0.00732)	(0.00629)
age_58 post-reform	0.0581***	0.0615***	0.0636***	0.0822***	0.0802***	0.0795***
	(0.00534)	(0.00502)	(0.00521)	(0.00650)	(0.00793)	(0.00629)
age_59 post-reform	0.0194***	0.0232***	0.0258***	0.0316***	0.0325***	0.0304***
	(0.00539)	(0.00511)	(0.00525)	(0.00653)	(0.00697)	(0.00630)
Intercept	0.210***	0.205***	0.276***			
	(0.0187)	(0.0241)	(0.0644)			
R^2	0.0438	0.0428	0.00688			
Number of observations	288,635	288,635	288,635	288,635	232,727	288,635

Notes: Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. AME: average marginal effect.

Source: Calculations by the authors from administrative files (Ministry of Education, Paris)

Table 2.5 – Parameter estimates of autoregressive models

	Linear				Non Linear: AME		
	OLS	FE	AB	BB	Probit-RE	Wooldridge	Orme
short sick leave$_{t-1}$	0.300***	-0.0869***	0.0334***	0.0455***	0.0854***	0.0606***	0.0689***
	(0.00190)	(0.00221)	(0.00326)	(0.00286)	(0.00273)	(0.00251)	(0.00260)
seniority (in years)	-0.00157***	-0.00142	-0.00522***	-0.00237***	-0.00297***	-0.00138	-0.00377***
	(0.000105)	(0.000929)	(0.00114)	(0.000170)	(0.000204)	(0.000986)	(0.000186)
salary grade	0.00380***	-0.00136**	-0.00113	0.00333***	0.00185***	-0.00151**	0.00610***
	(0.000497)	(0.000616)	(0.000878)	(0.000595)	(0.000666)	(0.000675)	(0.000635)
number of hours per week	0.00354***	0.00936***	0.0101***	0.00556***	0.00800***	0.0109***	0.00622***
	(0.000324)	(0.000524)	(0.000696)	(0.000427)	(0.000495)	(0.000569)	(0.000465)
single male	0.0188***			0.0242***	0.0211***	-0.00516	0.0250***
	(0.00322)			(0.00525)	(0.00636)	(0.0127)	(0.00586)
single female	0.106***			0.146***	0.164***	0.0602***	0.219***
	(0.00262)			(0.00438)	(0.00512)	(0.0114)	(0.00474)
married female	0.0761***			0.107***	0.126***	0.0806***	0.147***
	(0.00223)			(0.00365)	(0.00446)	(0.00406)	(0.00410)
share of teachers over 40	-0.0352***	-0.0609***	-0.0273	-0.0487***	-0.110***	-0.0641***	-0.117***
	(0.0116)	(0.0187)	(0.0263)	(0.0155)	(0.0165)	(0.0199)	(0.0155)
average salary grade	-0.00935***	-0.00258	-0.00427	-0.0122***	-0.0126***	-0.00314	-0.0296***
	(0.00234)	(0.00326)	(0.00437)	(0.00295)	(0.00321)	(0.00356)	(0.00304)
average class size	0.00736***	0.00210**	0.00278***	0.00876***	0.00984***	0.00219**	0.0149***
	(0.000421)	(0.000958)	(0.00103)	(0.000667)	(0.000689)	(0.000991)	(0.000643)
priority education area	0.0264***	0.00603	0.0128	0.0261***	0.0279***	0.0111	0.0565***
	(0.00419)	(0.00696)	(0.00895)	(0.00564)	(0.00636)	(0.00750)	(0.00596)
age_55	0.00577	0.00215	0.000297	0.00438	0.00522	0.000891	0.0243***
	(0.00358)	(0.00380)	(0.00435)	(0.00354)	(0.00437)	(0.00458)	(0.00418)
age_56	0.000882	0.00144	0.00323	0.00380	0.00237	-0.000325	0.0220***
	(0.00358)	(0.00440)	(0.00496)	(0.00351)	(0.00444)	(0.00506)	(0.00424)
age_57	0.0134***	0.0117**	0.0158***	0.0155***	0.0178***	0.0129**	0.0388***
	(0.00361)	(0.00500)	(0.00569)	(0.00361)	(0.00448)	(0.00562)	(0.00428)
age_58	0.0128***	0.0156***	0.0198***	0.0190***	0.0211***	0.0153**	0.0424***
	(0.00364)	(0.00562)	(0.00654)	(0.00367)	(0.00454)	(0.00628)	(0.00433)
age_59	0.0371***	0.0433***	0.0468***	0.0443***	0.0543***	0.0465***	0.0747***
	(0.00373)	(0.00641)	(0.00751)	(0.00380)	(0.00460)	(0.00700)	(0.00438)
age_55 post-reform	0.00223	0.0108**	-0.000101	0.00190	0.0104*	0.0127**	0.0106*
	(0.00498)	(0.00526)	(0.00616)	(0.00502)	(0.00622)	(0.00636)	(0.00592)
age_56 post-reform	0.0343***	0.0413***	0.0273***	0.0323***	0.0504***	0.0473***	0.0501***
	(0.00502)	(0.00551)	(0.00627)	(0.00504)	(0.00622)	(0.00636)	(0.00592)
age_57 post-reform	0.0269***	0.0481***	0.0311***	0.0360***	0.0503***	0.0483***	0.0486***
	(0.00506)	(0.00562)	(0.00638)	(0.00517)	(0.00623)	(0.00637)	(0.00593)
age_58 post-reform	0.0462***	0.0698***	0.0505***	0.0537***	0.0751***	0.0724***	0.0725***
	(0.00511)	(0.00570)	(0.00652)	(0.00525)	(0.00624)	(0.00638)	(0.00593)
age_59 post-reform	0.000829	0.0338***	0.0121*	0.0144***	0.0245***	0.0265***	0.0229***
	(0.00517)	(0.00583)	(0.00668)	(0.00531)	(0.00626)	(0.00641)	(0.00595)
R^2	0.131	0.00163					
Number of observations	268,477	268,477	240,675	268,477	268,477	268,477	268477

Notes: Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. AB: Arellano-Bond estimator, BB: Blundell-Bond estimator. AME: average marginal effect.

Source: Calculations by the authors from administrative files (Ministry of Education, Paris)

Table 2.6 – Changes in the probabilities to take short sick leaves depending on the birth cohort

	elasticity	born in 1951	born in 1952	born in 1953
age 55	0.00196***	0.00784	0.01764	0.02744
age 56	0.00453***	0.01812	0.04077	0.06342
age 57	0.00479***	0.01916	0.04311	0.06706
age 58	0.00478***	0.01912	0.04302	0.06692
age 59	0.000424	0.001696	0.003816	0.005936

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7 – Robustness check

	Impact on long sick leaves			Comparison to other cohorts		
	OLS	SE	Wooldridge	OLS	SE	Wooldridge
age_55_ref	-0.00122	-0.000636	-0.000429	-0.00659	-0.00692	0.0553
	(0.000920)	(0.00102)	(0.000374)	(0.00787)	(0.00810)	(0.0452)
age_56_ref	0.000210	-0.000690	0.0000980	-0.00385	-0.00246	0.0630
	(0.000966)	(0.000922)	(0.000358)	(0.00631)	(0.00656)	(0.0465)
age_57_ref	0.000254	-0.000682	0.000105	0.00527	0.00800	0.0727
	(0.00103)	(0.000942)	(0.000343)	(0.00585)	(0.00607)	(0.0465)
age_58_ref	-0.000617	-0.00157	-0.000108	0.00148	0.00549	0.0693
	(0.00114)	(0.00103)	(0.000318)	(0.00581)	(0.00602)	(0.0465)
age_59_ref	0.00109	0.000999	0.000372	0.0436***	0.0451***	0.119**
	(0.00134)	(0.00125)	(0.000342)	(0.00582)	(0.00604)	(0.0464)
Observations	264161	264161	264161	150918	150918	150918

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C: Solving the classical model of retirement decision

Let us consider an optimization model with perfect smoothing consumption and where work is costly through disutility. The net utility of work is given by the difference between the utility issued from consumption $U_W(C_t)$ allowed by wages and the disutility of work $D(t)$. The functioning form of the disutility is such as it increases at a constant rate with age. The utility of retirement is noted $U_R(C_t)$ and stems from the consumption allowed by pension benefits. The lifetime utility of a representative teacher is then a value function depending on τ the date of retirement, T being the date of death, and C_t, the consumption at time t, with respect to a resources constraint : the total amount of consumption cannot exceed lifelong earnings (terminal condition of non-negativity of assets).

$$V(\tau) = \max_{\tau, C_t} \int_s^\tau e^{-\rho(t-s)}(U_W(C_t) - D(t))dt + \int_\tau^T e^{-\rho(t-s)} U_R(C_t)dt$$

$$sc : \begin{cases} \int_s^T e^{-\rho(t-s)} C_t dt = \int_s^\tau e^{-\rho(t-s)} W_t dt + \int_\tau^T e^{-\rho(t-s)} R_t(\tau) dt \\ D(t) = D_0 e^{\rho t} \end{cases}$$

With λ the Lagrange multiplier, the FOC with respect to C_t is given by:

$$\begin{aligned} 0 &= \frac{\partial V(\tau)}{\partial C_t} \\ &= \int_s^\tau e^{-\rho(t-s)} U_W'(C_t)dt + \int_\tau^T e^{-\rho(t-s)} U_R'(C_t)dt - \lambda \int_s^T e^{-\rho(t-s)}dt \end{aligned}$$

If $t < \tau$:

$$\int_s^\tau e^{-\rho(t-s)} U_W'(C_t)dt = \int_s^\tau e^{-\rho(t-s)} \lambda \ dt \qquad \text{hence} \qquad U_W'(C_t) = \lambda \text{ because } U_W'(C_t) > 0$$

If $t \geq \tau$:

$$\int_\tau^T e^{-\rho(t-s)} U_R'(C_t)dt = \int_\tau^T e^{-\rho(t-s)} \lambda \ dt \qquad \text{hence} \qquad U_R'(C_t) = \lambda \text{ because } U_R'(C_t) > 0$$

These results are classic, and mean that at every period, the optimal choice of consumption is the one such as the marginal utility of consumption is equal to the Lagrange multiplier at its optimal value. The Lagrange multiplier is then the marginal utility obtained if ever the resource constraint were loosen by one unit, it measures the marginal utility of income.

The FOC with respect to τ is given by :

$$
\begin{aligned}
0 &= \frac{\partial V(\tau)}{\partial \tau} \\
&= e^{-\rho(\tau-s)}(U_W(C_\tau) - D(\tau)) - e^{-\rho(\tau-s)} U_R(C_\tau) \\
&\quad + \lambda \left[e^{-\rho(\tau-s)}(W_\tau - R_\tau(\tau)) + \int_\tau^T e^{-\rho(t-s)} \frac{\partial R_t(\tau)}{\partial \tau} dt \right] \\
&= U_W(C_\tau) - D(\tau) - U_R(C_\tau) + \lambda \left[(W_\tau - R_\tau(\tau)) + \int_\tau^T e^{-\rho(t-\tau)} \frac{\partial R_t(\tau)}{\partial \tau} dt \right]
\end{aligned}
$$

Let us set $\Gamma_\tau = \lambda \left[(W_\tau - R_\tau(\tau)) + \int_\tau^T e^{-\rho(t-\tau)} \frac{\partial R_t(\tau)}{\partial \tau} dt \right]$ the future gain of utility to postpone retirement by one year. The condition that determines the optimal τ for the retirement decision is thus the following :

$$
U_W(C_\tau) - D(\tau) = U_R(C_\tau) - \Gamma_\tau
$$

when the marginal utility of working an additional year equals the marginal (opportunity) cost of retiring for the year concerned.

Appendix D: Evolution of mean differences between pre- and post-reform cohorts

Table 2.8 – Mean differences between post- and pre- reform cohorts

	Age				
Variable	55	56	57	58	59
seniority (in years)	0.87 ***	0.76 ***	0.74 ***	0.72 ***	0.71 ***
salary grade	-0.06 ***	-0.14 ***	-0.32 ***	-0.47 ***	-0.45 ***
full time	0.01 *	0.05 ***	0.06 ***	0.06 ***	0.06 ***
number of working hours per week	-0.05 *	0.69 ***	0.95 ***	0.91 ***	0.87 ***
married male	-0.01	-0.01	-0.01	-0.01	-0.01 *
single male	0.01 **	0.01 ***	0.01 ***	0.01 ***	0.01 ***
married female	-0.03 ***	-0.03 ***	-0.03 ***	-0.03 ***	-0.02 ***
single female	0.02 ***	0.03 ***	0.02 ***	0.02 ***	0.02 ***
average class size	0.15 ***	0.15 ***	0.24 ***	0.41 ***	0.54 ***
proportion of teachers over 40 in the school	0.00 ***	0.00 ***	0.00 *	0.00 ***	0.01 ***
salary grade of teachers in the school	0.13 ***	0.10 ***	0.01	-0.04 ***	-0.08 ***
priority education area	0.02 ***	0.00 *	0.00	0.00	0.00

Notes: Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: Calculations by the authors from administrative files (Ministry of Education, Paris)

Appendix E: Elasticity estimates

Table 2.9 – Elasticity models

	Probabilities		Duration	
	SE	Wooldridge	OLS-duration	Tobit
lagged sick leave	0.0458***	0.0608***		
	(0.00286)	(0.00251)		
lagged duration (in days)			0.188***	0.0394***
			(0.00310)	(0.00420)
seniority (in years)	-0.00236***	0.000283	-0.0265***	-0.122***
	(0.000170)	(0.000943)	(0.00253)	(0.00848)
grade	0.00328***	-0.00177***	0.0862***	0.0755***
	(0.000595)	(0.000677)	(0.0120)	(0.0276)
number of hours	0.00568***	0.0109***	0.00201	0.250***
	(0.000426)	(0.000569)	(0.00775)	(0.0206)
single man	0.0240***	-0.00532	0.357***	0.908***
	(0.00525)	(0.0127)	(0.0740)	(0.267)
single woman	0.146***	0.0603***	1.798***	6.503***
	(0.00438)	(0.0114)	(0.0633)	(0.216)
married woman	0.107***	0.0809***	1.027***	4.884***
	(0.00365)	(0.00406)	(0.0515)	(0.189)
share teachers over 40	-0.0470***	-0.0604***	-0.787***	-4.570***
	(0.0155)	(0.0199)	(0.285)	(0.680)
average salary grade	-0.0125***	-0.00343	-0.301***	-0.587***
	(0.00294)	(0.00357)	(0.0570)	(0.132)
average class size	0.00885***	0.00251**	0.133***	0.433***
	(0.000668)	(0.000994)	(0.00992)	(0.0289)
priority education area	0.0263***	0.0110	0.400***	0.971***
	(0.00564)	(0.00751)	(0.105)	(0.258)
age_55	0.000976	-0.00259	0.267***	0.446***
	(0.00321)	(0.00398)	(0.0770)	(0.165)
age_56	0.00605*	0.000267	0.291***	0.639***
	(0.00319)	(0.00448)	(0.0783)	(0.167)
age_57	0.0180***	0.0113**	0.551***	1.396***
	(0.00328)	(0.00507)	(0.0814)	(0.168)
age_58	0.0298***	0.0224***	0.720***	2.077***
	(0.00336)	(0.00574)	(0.0828)	(0.170)
age_59	0.0511***	0.0466***	1.138***	3.429***
	(0.00348)	(0.00648)	(0.0876)	(0.173)
age_55_duration	0.00109**	0.00196***	0.00554	0.0747***
	(0.000482)	(0.000552)	(0.0112)	(0.0245)
age_56_duration	0.00336***	0.00453***	0.0238**	0.165***
	(0.000491)	(0.000549)	(0.0116)	(0.0243)
age_57_duration	0.00369***	0.00479***	0.0296**	0.177***
	(0.000500)	(0.000552)	(0.0119)	(0.0242)
age_58_duration	0.00342***	0.00478***	0.0420***	0.198***
	(0.000507)	(0.000552)	(0.0124)	(0.0242)
age_59_duration	-0.000411	0.000424	-0.00228	0.0415*
	(0.000511)	(0.000555)	(0.0128)	(0.0244)
Observations	268477	268562	268476	268476

Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix F: Robustness checks

Table 2.10 – Impact of the reform on long sick leaves

	OLS	SE	Wooldridge
lagged long sick leave	0.0812***	0.167***	0.00256***
	(0.00705)	(0.0446)	(0.000574)
seniority (in years)	-0.0000357*	-0.0000194	-0.0000177**
	(0.0000208)	(0.0000189)	(0.00000829)
grade	0.000565***	0.000242**	0.000200***
	(0.0000976)	(0.000102)	(0.0000408)
number of hours	-0.0000718	0.000371***	0.0000119
	(0.0000599)	(0.0000905)	(0.0000239)
single man	0.00107*	0.000824	0.000483*
	(0.000596)	(0.000576)	(0.000278)
single woman	0.00492***	0.00372***	0.00180***
	(0.000539)	(0.000549)	(0.000254)
married woman	0.00195***	0.00202***	0.000894***
	(0.000409)	(0.000397)	(0.000199)
share teachers over 40	-0.000528	-0.000878	-0.000295
	(0.00236)	(0.00273)	(0.000796)
average salary grade	-0.000966**	-0.000607	-0.000211
	(0.000492)	(0.000566)	(0.000163)
average class size	0.000128	0.0000707	0.0000526*
	(0.0000782)	(0.0000881)	(0.0000304)
priority education area	-0.0000503	0.000298	0.0000884
	(0.000864)	(0.00104)	(0.000297)
age_55	0.00164**	0.00102	0.000649**
	(0.000675)	(0.000732)	(0.000265)
age_56	0.00166**	0.000938	0.000691***
	(0.000684)	(0.000667)	(0.000267)
age_57	0.00280***	0.00172**	0.00113***
	(0.000723)	(0.000684)	(0.000271)
age_58	0.00536***	0.00441***	0.00199***
	(0.000813)	(0.000747)	(0.000291)
age_59	0.00561***	0.00326***	0.00213***
	(0.000826)	(0.000759)	(0.000297)
age_55_ref	-0.00122	-0.000636	-0.000429
	(0.000920)	(0.00102)	(0.000374)
age_56_ref	0.000210	-0.000690	0.0000980
	(0.000966)	(0.000922)	(0.000358)
age_57_ref	0.000254	-0.000682	0.000105
	(0.00103)	(0.000942)	(0.000343)
age_58_ref	-0.000617	-0.00157	-0.000108
	(0.00114)	(0.00103)	(0.000318)
age_59_ref	0.00109	0.000999	0.000372
	(0.00134)	(0.00125)	(0.000342)
Observations	264161	264161	264161

Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11 – Impact of the reform on the placebo cohorts

	OLS	SE	Wooldridge
lagged short sick leave	0.303***	0.0449***	0.0538***
	(0.00255)	(0.00392)	(0.00348)
seniority (in years)	-0.00115***	-0.00177***	-0.000872
	(0.000136)	(0.000208)	(0.00139)
grade	0.00314***	0.00314***	-0.00147
	(0.000663)	(0.000797)	(0.000986)
number of hours	0.00247***	0.00433***	0.00896***
	(0.000517)	(0.000666)	(0.000962)
single man	0.0195***	0.0260***	0.0198
	(0.00432)	(0.00676)	(0.0211)
single woman	0.109***	0.152***	0.0647***
	(0.00348)	(0.00544)	(0.0194)
married woman	0.0768***	0.108***	0.0820***
	(0.00293)	(0.00449)	(0.00494)
share teachers over 40	-0.00186	0.00198	-0.0418
	(0.0161)	(0.0211)	(0.0301)
average salary grade	-0.0120***	-0.0168***	-0.00412
	(0.00316)	(0.00392)	(0.00507)
average class size	0.00762***	0.00928***	0.00156
	(0.000594)	(0.000931)	(0.00149)
priority education area	0.0315***	0.0323***	0.0178*
	(0.00599)	(0.00764)	(0.00947)
age_55	0.00941	0.0116	-0.0511
	(0.00741)	(0.00775)	(0.0449)
age_56	0.00124	0.00360	-0.0623
	(0.00574)	(0.00618)	(0.0462)
age_57	0.00714	0.00739	-0.0555
	(0.00524)	(0.00568)	(0.0462)
age_58	0.00986*	0.0134**	-0.0500
	(0.00520)	(0.00566)	(0.0463)
age_59	-0.00157	0.00470	-0.0617
	(0.00519)	(0.00567)	(0.0464)
age_55_ref	-0.00659	-0.00692	0.0553
	(0.00787)	(0.00810)	(0.0452)
age_56_ref	-0.00385	-0.00246	0.0630
	(0.00631)	(0.00656)	(0.0465)
age_57_ref	0.00527	0.00800	0.0727
	(0.00585)	(0.00607)	(0.0465)
age_58_ref	0.00148	0.00549	0.0693
	(0.00581)	(0.00602)	(0.0465)
age_59_ref	0.0436***	0.0451***	0.119**
	(0.00582)	(0.00604)	(0.0464)
Observations	150918	150918	150918

Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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Chapter 3

Free your mind: The impact of geographically-targeted affirmative action on access to higher education

Pierre Gouëdard

Market failures in education can lead to a suboptimal number of students accessing higher education, and/or a mismatch in resource allocation. Some students may either under-invest in the skill accumulation process, or simply self-select out of it. This analysis exploits a natural experiment to assess the impact of a French affirmative action, the "Convention d'Éducation Prioritaire" (CEP) program, in which high-schools from deprived neighborhoods are granted a special admission procedure to Sciences Po, one of the most selective higher education establishment in France. To the contrary of typical affirmative action in the US, the CEP program does not use racial statistics but rely on geographic localization and socioeconomic status. Using differences-in-differences (following Diagne and Wasmer, 2013), I estimate that the program increased by 3 percentage points the access to higher education for treated high-schools, and has a knock-on effect on other students not accessing Sciences Po. At the same time, none of the statistical tests indicate that the program modified the composition of targeted schools, which could have blurred the causal link I unveil. These findings suggest a non negligible role for pre-market discrimination in explaining a lower access to higher education in underprivileged high-schools in France.

Contents

3.1 Introduction

Modern economies are affected by a pervasive skill biased technological change which strengthens inequalities. If technological progress can foster growth, rising inequalities, aside from its impact on social cohesion, has been proven to be harmful for long-term growth. In this respect, the OECD report “In it together” presents an econometric study on OECD countries over the last thirty years. More precisely, the report estimates that 4.7% of cumulative growth has been lost between 1990 and 2010, due to the rise of income inequality between 1985 and 2005.

Skill biased technological change triggers off both income and wage inequalities. On the one hand, income inequalities stem from the hypothesis that technological change is labor reducing and will increase unemployment[1]. On the other hand, skill biased technology rewards more labor skilled workers. Rising inequalities can there be understood as differences between skills groups in their abilities to attain educational levels keeping pace with technological change.

A credible transmission channel between inequalities and growth might be the stock of human capital. Canonical models of growth recognize human capital as the main determinant of the growth potential of an economy. Besides, educational attainment and especially access to tertiary education, is linked to income. Therefore, as the gap between the lower income households and the rest of the population rises, students from the most disadvantaged households struggle to access higher education. It jeopardizes not only social mobility, but also deprives the country of a vast pool of human capital.

Ensuring equity in the access to higher education is thus a crucial issue, because it allows to optimize the pool of human capital in a country, reduces inequalities and paves the way for long term growth. There is then room for public policy action when market failures lead to a suboptimal number of students accessing higher education and/or a mismatch in resource allocation. Such market failures are issued either by imperfect funding systems for students or pre-market selection. The goal of correcting public actions is then double sided. First, they can offer funding in order to lift out financial constraints for the least wealthy students. Second, they may try to counterbalance a more insidious consequence of pure discrimination, which is the pre-market selection. In France, where tuitions are low and public funding is widespread, the impact of measures targeting pre-market

[1] everything else kept equal: in general equilibrium, one can think that new opportunities may arise and compensate this harmful effect on employment

discrimination is likely to be more significant.

Dedicated literature historically attributed discrimination in the labor market to taste (Becker, 1957) or imperfect information leading to statistical discrimination (Arrow, 1973). More recently though, some authors (Heckman, 1998; Neal et al., 1996) assessed that most of the differences between groups of workers in the United States stem from the differences in the skills that various groups bring to the market. This is the so called pre-market discrimination.

In fact, if individuals expect lower future returns to skills because of the anticipation they will be discriminated against on the labor market, they may either under-invest in the skill accumulation process, or simply self select out of it. In that case, discrimination as measured by a Mincerian equation is underestimated. Firstly, because of the selection bias at stake: only the most successful representatives of a minority will be present in the sample. Secondly, minority members, who have not performed the optimal level of effort during the skill accumulation process, might reinforce the employer's idea they are less qualified. This vicious circle could maintain discrimination over time, as ex-ante beliefs of minorities become self-fulfilling (Arrow, 1973; Loury et al., 1993).

A wide range of policy interventions has been implemented in order to tackle pre-market discrimination. Hickmann (2010a) summarizes some of their objectives: a) narrow the enrollment gap, that is raise social or ethnic diversity ; b) narrow the achievement gap, post-college admission, and/or preserve the quality of admits ; c) preserve incentives, that is, raise or at least maintain effort both pre and post admission. These goals rely on the explicit criterion of "race". Generally speaking, conventional positive action follows two paths: either the policy is explicitly targeted on race, in which case we can refer to it as "colored" policy (for instance with a quota rule) ; or it is only correlated with it, what can be called "apparently colorblind" policy (for instance with relative quota rules).

Affirmative actions take a different form in France, which targets at socioeconomic status instead of race or ethnicity. This paper thus offers to evaluate a different kind of affirmative action, namely a geographically-targeted program based on socioeconomic determinants and close in mind to "colorblind" policies. After courts in the US started questioning the efficiency of racial affirmative action, "colorblind" measures begun to thrive. Such programs, which aim at increasing social diversity without invoking racial statistics, are yet prone to debate. Theoretically, increasing the probability to access higher education for discriminated groups may reduce the level of effort they provide.

This paper is designed as an extension of the Diagne and Wasmer (2013) study, which examines the impact of an affirmative action that rewards effort among high-school students in socioeconomically disadvantaged areas in France, on graduation rates (Baccalauréat results), and therefore level of effort. For that purpose, they use an unique experiment ran by Sciences Po, one of the top French higher education establishments (details of French higher education will be offered in the next section), which has been signing agreements (called Conventions Education Prioritaire or CEPs) with high-schools in deprived areas to admit their highly achieving students through a special procedure since 2001. The interests of the program lies in the absence of explicit quota and a special admission procedure in Sciences Po remaining highly selective. In fact, the CEP does not restrict the number of admission for each school. However, only one or two top students per high-school are admitted every year. This feature of the CEP program makes it then unlikely to create effort disincentives.

In this work, I extend Diagne and Wasmer's work on graduation rate to higher education access. I aim to show that the CEP program has a knock-on effect on students in treated high-school, since the increase in the access rate for treated high-schools remains, even after removing students accessing Sciences Po via the special admission procedure. This confirms that market failure in education is a sizable issue, and that France is depriving itself of potential human capital.

The narrative explaining the success of the program is the pre-market discrimination in the broader sense, namely any mechanisms, aside financial constraints, preventing French students from a general high-school track from choosing a higher education track, the expected follow-up in the French system. The underlying goal is to show that bringing one of the French elite school closer to underprivileged high-schools can reduce information bias, social fatalism and incentivize students to access higher education.

Implicitly, entry into tertiary studies rely on aspirations previously developed by students. Breen and Garcia-Penalosa (2002) have shown how career choices differ between men and women even with the same preferences, due to past discrepancies in beliefs. Segregation could then carry on over time between students from different neighborhoods, despite a program such as CEP. Guyon and Huillery (2016) enumerate at least 6 factors likely to influence aspirations: knowledge of existing tracks, current academic achievement, perception of current academic achievement, anticipation of future academic achievement, personal taste and anticipation of returns to education. However, due to the design of the CEP and available data, it is not possible to disentangle the differentiated impact of the program on the several factors influencing aspirations. The ambition of this work is then to determine if the affirmative action has any impact, and to cast some light on potential factors explaining a lower access to higher education in underprivileged high-schools.

Within the same framework as Diagne and Wasmer (2013), I use differences-in-differences estimates to quantify the impact of the program on the probability to enter tertiary education as a measure of my broad interpretation of pre-market discrimination. I show that for enrolled schools, the percentage of students pursuing their studies increased by around 3 percentage points, and that this effect is borne by high-schools students following the economic track. Stratified regressions do not allow to detect any differentiated effect according to gender, or socioeconomic background. Finally, regressions checking if the program induced any change in the school composition as potential confounders do not exhibit any significant result. These findings suggest a non negligible role for pre-market discrimination in explaining a lower access to higher education in underprivileged high-schools in France.

The paper is organized as follow: Section 3.2 describes the CEP program as well as the French context and Section 3.3 presents data and the corresponding descriptive statistics. Section 3.4 displays differences-in-differences estimates, then robustness checks are shown in Section 3.5. Finally, Section 3.6 concludes.

3.2 The CEP program, answer to a tense French context?

The "Conseil national d'évaluation du système scolaire" (CNESCO), a French national committee gathering 22 teams of researchers, recently released a report (September 2016) underlining the inability of the French educational system to reduce inequalities at school. One of the biggest failure pointed out in the report is a public policy launched more than 30 years ago and called "Priority Education" (a.k.a. ZEP). To sum things up, the report shows that certain areas labelled as "Priority Education" with additional resources triggered off strong adverse effects: concerned high-schools were even more segregated, as more privileged family were moving away from them and the social diversity went decreasing. In fact, as stated by the 2012 PISA report, France has become the OECD country where the social economic status has the strongest correlation with academic achievement: an increase in one unit of the PISA index of economic, social and cultural status triggers off an increase in the mathematics score of 57 points in France, the highest variation among OECD countries (average at 39 points).

The CEP program, which started in 2001, answers a double sided challenge. First, it aims at increasing social and geographic diversity in Sciences Po, at a time when a social divide was exacerbated between rich and poor or wealthy urban centers and deprived suburbs. As noted by Delhay (2007), French "Grandes Écoles" train the vast majority of future business leaders or politicians whereas their pool of students only represent 15% of the whole population. On top of that, Sciences Po statistics were showing that an increasing share of students coming from the highest fractiles of the wage distribution was admitted. Besides, whereas the proportion of students whose parents' occupation belongs to the lowest socioeconomic category (unemployed, unskilled workers and clerks) accessing Universities was approximately 30% in 2000, it only reached 3% in Sciences Po.

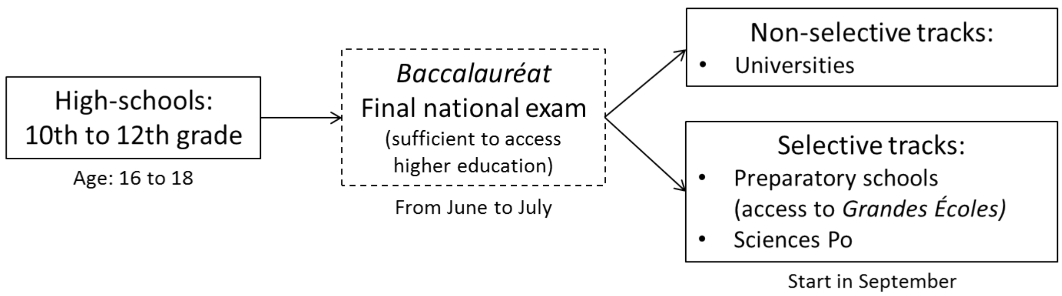
Second, the CEP program tackles in general the issue of pre-market selection, by developing incentives for students to invest in skill accumulation process in underprivileged high-schools. In these areas, students are indeed suspected to self-select out, because they anticipate the future discrimination they will be victim of. Aside this pure pre-market discrimination effect, other common reasons for students from these high-school to not access higher education include financial constraints (especially when considering the opportunity cost of long studies), imperfect information but also social bias in career guidance and aspirations.

3.2.1 Access to higher education in France

In France, at the end of high-school, students pass a national exam called Baccalauréat in order to graduate. This diploma is mandatory to access higher education, and sufficient to enter Universities. To the contrary of Anglo-Saxon schemes where higher education revolves around Universities, the French system is dual (see Figure 3.1). On the one hand, Universities claim limited tuition (around 400 euros a year) and do not practice any explicit selection on undergraduates students, except from their place of residence. On the other hand, selective tracks gather "Grandes Écoles": engineer schools (e.g. Polytechnique), business schools (e.g. HEC) or generalists school (e.g. "École Normale Supérieure", Sciences Po).

The traditional way to access a "Grande École" is to enter a preparatory school after the "Baccalauréat". This kind of intensive program prepares student during two years, before they take ranking exams in order to enter the "Grande École". A wide range of tuition is observed, depending on the status of the school: public ones are free and students are paid to attend classes (Polytechnique, ENS) whereas private ones usually claim high fees (HEC, Edhec), around 15,000 euros per year. Although Sciences Po is part of "Grandes Écoles", admission take place just after the "Baccalauréat", and do not require the 2 years of preparatory school. As a private institution, Sciences Po requires higher tuition than Universities, but they are negatively correlated to the income of the student's parent (ranging from 0 to 10,000 euros per year).

Figure 3.1 – Accessing higher education in France



3.2.2 Admission at Sciences Po under the CEP program

Before the CEP program, admission at Sciences Po was possible through two channels:

- students who obtained highest honors at the Baccalauréat ("Mentions Très Bien") could directly pass an oral examination (22% admit, 78% reject)
- other students laureates of the Baccalauréat can try the September entrance exam (12% admit, 88% reject).

The CEP program is geographically targeted. It aims at giving deprived areas (generally poor urban suburbs) a special selection procedure in order to increase social diversity at Sciences Po. In fact, students from these areas are more likely to be rejected in an oral examination since they lack social and cultural capital compared to students from more privileged neighborhood. Besides, as the CNESCO's report stated, teachers in Priority Education areas spend less time teaching (because they have to maintain discipline in the classroom), and they use teaching techniques less sophisticated (for example, students do not learn "how to learn"). In that regard, traditional admission procedures seems especially unfair.

High-schools that have signed an agreement with Sciences Po (a CEP) can prepare their students for the special admission procedure. First, an information session about the program takes place in the high-school. Interested students who have attended this meeting start preparing during 6 weeks a presentation on a theme of their choice. Then a first oral examination, led by a panel of teachers and professionals, determines if they can enter the program or not (pre-admitted), and access the special admission procedure. Second, at the end of the year, and conditional of their graduation (getting the Baccalauréat at their first try), they pass the special oral examination organized by Sciences Po that ultimately states if they are admitted in Sciences Po or not. The final admission decision is based on this last oral exam and their high-school grades. The crucial point here is that the competition to enter Sciences Po takes place *between* students from deprived neighbourhood, and restore to some extent fairness in the admission procedure.

Yet, the process is still highly selective. Even though the admission threshold to Sciences Po is lower for students from the CEP program than other students undertaking the normal procedure, the concern that CEP agreement could decrease optimal level of effort among participants is unlikely. In fact, in the CEP track in 2013, 15% of candidates are admitted and 85% are rejected, and only one or two students are accepted in Sciences Po every year. To the contrary of conventional affirmative action, there is no cap regarding the number of CEP students to admit. Students from the CEP program are therefore incentivized not to spare their efforts in order to be admitted in Sciences Po.

Figure 3.2 presents the evolution of the number of schools enrolled in the program and the number of admits in Sciences Po. In 2001, CEP admits were only representing 3% of an entry cohort, but reaches 10% in 2013. However, the dramatic increase in the size of cohorts over the last decade do not let us suppose that the CEP procedure crowded out other admission procedures, thus reducing the risk of any challenges associated with individual affirmative action policies.

3.2.3 Selection of high-schools

Started in 2001, the CEP program was progressively expanded. During the first year, 7 enrolled high-schools allowed 17 students to be admitted in Sciences Po, whereas in 2013, 100 high-schools sent 149 students. In total, 1151 accessed Sciences Po through the special procedure over the period.

However, eligible high-school for the CEP program must meet at least one of the following requirements:

- belongs to a “Priority Education” area
- have a proportion of students whose parents belongs to disadvantaged socio-professional cate-

gories exceeding the national average by 70%

- have a proportion of 10th grade students coming from junior high-schools belonging to a "Priority Education" area greater than 60%

Once the board of Sciences Po adopted and started promoting the CEP program, it was yet the responsibility of schools principals to get in touch with Sciences Po in order to sign an agreement. Sciences Po officials report though that every eligible high-school that expressed interest signed a partnership.

3.2.4 What does happen in treated high-schools?

It is worth noting that the treatment unit in the CEP program is high-schools. Therefore, the program might have a knock-on effect on all students, regardless of their individual level. Sessions of information given by high-school teachers or Sciences Po current students involve all students of a high-school. In twelfth grade, interested high-school students are invited to spend a day in Sciences Po, where they can meet students, attend courses and a special information session with a panel of professionals (private sector executives, journalists, judges and high-ranked civil servants).

The program has therefore several implications. First, it reduces asymmetries of information for students whose social capital does not allow them to even "think" about higher education. Either because no one in their neighborhood attended a tertiary education institution (lack of information), or because it seems out of reach (bounded aspirations). Second, it shows students from deprived areas they are not excluded and it is possible for them to reach such an elite institution. This can restore somehow their confidence in the future, which is a crucial determinant of the level of effort and motivation they exhibit at school. Anecdotal evidence gathered from teachers in CEP high-schools indicates a decrease in absenteeism and an increase in effort among students, who strive to pass the Baccalauréat exam with honors and be selected at Sciences Po or started considering pursuing their studies in higher education.

Besides, high-school teachers obtained from Sciences Po the exclusive role of selecting worthy applicants, which empowered them. Van Zanten (2010) explains that by detecting and coaching potential future members of the elite, high-school teachers reconnect with a symbolic dimension of their role, which has been steadily diluted with mass education. Just as for students, the program may have a strong motivational effect on teachers, which can indirectly strengthens the impact on students' aspirations, and launch a virtuous circle.

Now a word about students admitted through the special procedure. They benefit at the same time from academic assistance (there is a two-week integration camp during summer prior to the beginning of classes and an individual tutor who monitors them) and from economic support in order to lift financial constraints (which includes tuition free, a fellowship, a special access to housing and free books). Some partner firms are even involved in their recruitment after graduation. As a result, even if academic performance of CEP students during the first year is still below the one of other students, there is no significant discrepancy by the time they graduate (5 years).

Students valuate a Sciences Po degree because it grants them 4 years of high quality course, a Master level, and its signalling effect on the labor market as "Grande École" laureates. Among 2013 graduates, 80% of the students who decided to enter the labor market found a job within 6 months, and earn an average annual salary of 41,000 Euros for jobs in France. These results have to be compared for instance with those gathered by the APEC[2] in their survey of the working status in 2013 of 2012 Bac+5 (Master level) graduates. After 6 months, only 58% of University graduates found a first job, and respectively 70% and 66% for engineer schools and business schools. In terms of salary range, the gap is even wider: University laureates earn as little as 23,200, whereas graduates of engineer or business schools perform a bit better, with respectively 32,100 Euros and 26,800 Euros.

3.3 Data

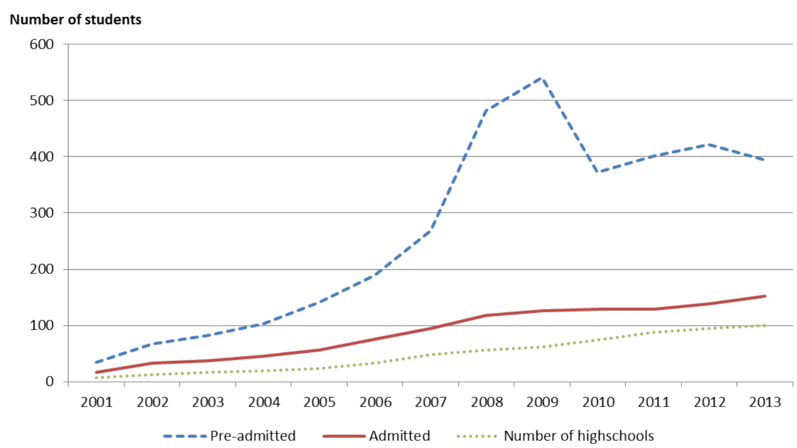
3.3.1 Different Ministries, different data sets

Data used in this work are issued by different institutions. The French Ministry of Education made available "Indicateurs pour le Pilotage des Etablissements du Second degré" (IPES) and "Aide au Pilotage et à l'Auto-évaluation des Établissements" (APAE), two datasets that include information from 1998 to 2013 on school contextual characteristics (notably on students and teachers). The extraction of data gathers all secondary schools (public or private with government supervision) on the 4 main school districts (Académies) of France where CEPs were experimented: Lille, Créteil, Nancy-Metz and Versailles.

[2] "Association pour l'Emploi des Cadres"

These two data sets include the population data and are matched with data from Sciences Po, which signed during a first wave 25 agreements between 2001 and 2006 in the four first Académies under consideration (7 in 2001, 6 in 2002, 4 in 2003, 2 in 2004, 2 in 2005 and 4 in 2006). Many agreements followed afterwards, reaching a total number of 100 agreements in 18 Académies. Figure 3.2 depicts how the program progressively rolled on. As the number of partner high-schools steadily increases over time, so does the number of admits, reaching a cumulative number of 1151 in 2013. In addition, the share of students admitted through the special CEP procedure rose from 3% in 2001 to 10% in 2013, with an abnormally high number of pre-selected students during two years before reverting to the trend in 2010.

Figure 3.2 – Number of admitted students, of pre-selected students and of high-schools in the program



In order to test the hypothesis of pre-market selection, and to measure if students' aspiration are curbed to some extent, we need to follow students after the Baccalauréat. The "Système d'Information de Suivi des Étudiants" (SISE) tracks students' paths in higher education since 2005. The SISE database is hosted by the French Ministry of Higher Education and Research, and gathers several files, one for each higher education institution (such as universities, engineer schools, management schools etc). An original database has then been built by appending all the SISE files, and by merging them with the file concerning secondary education characteristics. So far, this methodology has never been implemented: students are followed either through secondary education, or through higher education, but the gap between them has never been bridged since the different Ministries do not allow individual merge across databases.

However, since the treatment unit of the CEP program is the high-school, the merge has been allowed by Ministries. For each high-school, I counted in the SISE file how many students where choosing each higher education track, after they obtained their “Baccalauréat”. The resulting measure is a rate of access to higher education, and is the most comprehensive possible (some of the higher institutions are still off the SISE database, for instance: private schools). Under the assumption that students from CEP and non CEP high-schools randomly access these “shadow” institutions, my estimates of access to higher education should still be unbiased.

3.3.2 Identification strategy and modelling

As previously mentioned, the methodology employed to assess the level of pre-market discrimination is a differences-in-differences. According to Bertrand *et al.* (2004), the estimation procedure is the following :

$$Y_{ict} = A_t + TREATED_c + \delta \text{TREATPOST} + \eta \text{TREATPOST} \times \text{year\_cep} + \gamma X_{ict} + \varepsilon_{ict}$$

where Y_{ict} is the outcome of interest (for instance the access rate to higher education) for the high-school i, in the group c, at time t. A_t and $TREATED_c$ are fixed effects, representing respectively years and groups, and X_{ict} is a set of covariates that controls for contextual characteristics of the school. The variable of interest TREATPOST is a dummy variable indicating if at time t, the group c was treated, and δ, the interest parameter, captures the impact of the CEP program on the dependent variable. An additional variable TREATPOST $\times$ year_cep captures the heterogeneity of the treatment, by taking into account if the program has a different impact according to the year of the CEP agreement.

It is well known that the reliability of the estimation of the parameter delta by differences-in-differences rely on the assumption that trends of the access rate to higher education would have been the same between treated and non treated in the absence of treatment. To ensure so, the control group is usually selected on the basis of characteristics that are as close as possible as the ones for the treated group.

In this estimation, I propose 3 different control groups (following the methodology of Diagne and Wasmer, 2013), in order to check both the significance and the magnitude of estimated effects. The first control group is composed by “eligible” high schools in Académies where the CEP program was implemented, or high-schools who meet the program requirements but did not apply (see subsection 3.2.3). An alternative control group also gathers all eligible high school, but in Académies where the program was not implemented. Finally, the third control group consists in late CEP partners, or high-school who signed after 2008. Besides, I select high-schools in control groups for which the size of the cohort passing the Baccalauréat is at least 82, namely the smallest cohort found in treated high-schools.

Lastly, becoming a CEP high-school depends greatly on the motivation of the principal and of the teaching staff, since it was the principal's responsibility to approach Sciences Po to request a partnership. Early members of the program are likely to be a self-selected group of dynamic high-schools. In this regard, estimates based on the following regressions may offer an upper bound of the real impact of the program.

3.3.3 Descriptive statistics

Table 3.2 presents some of the main characteristics of high-schools in our sample at baseline in 2005. The share of disadvantaged students, the share of students coming from ZEP junior high-schools as well as the share of high-schools labelled as ZEP are the criteria listed in Section 3.2.3. Meeting these criteria is the requirement to be eligible for the CEP program. Other control variables such as the share of students repeating final grades, or the share of teachers with the highest qualification[3] or over 50 years old, complete the information available on high-schools.

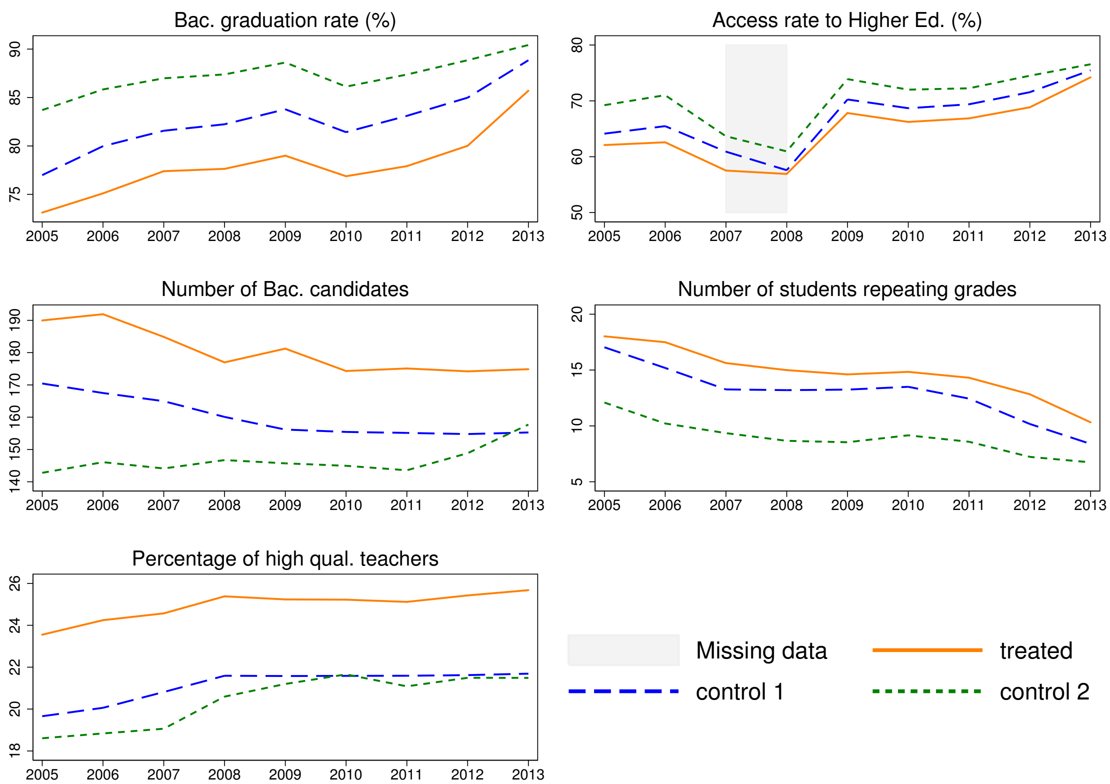
Column (3), (5) and (8) display t-tests on the equality of means for treatment and control groups. We can see that the alternative control group (4) differs in more dimensions than our main control group. Conversely, the last control group (7) is the closest one, as we expect future CEP partners to be really comparable to early ones. Regarding control groups 1 and 2, treated high-schools seems to have a higher number of Baccalauréat candidates, a lower access to higher education, younger teachers and more students coming from priority education areas. On the opposite, their share of high qualified teachers is higher.

Figure 3.3 illustrates the evolution of some important variables. A methodological note has to be pointed out though. The drop we can observe in the access to higher education for years 2007 and

[3]Most of teachers are certified after passing a national competitive examination called "CAPES". However, it is also possible for them to undertake a more competitive examination, namely the "Agrégation". Agrégés teachers benefit from a higher ranking in the civil servant hierarchy: they have a higher salary, can work less hours and can often choose before other teachers in which high-school they will work.

2008 is linked to a procedural error in the Ministry of Higher Education and Research. In fact, some individual identifiers were lost for those two years, so when computing the ratio of a smaller number of students than in reality, over a stable number of Baccalauréat candidates, we find this small drop. Under the assumption this attrition hits both controls and treated group the same way, estimates should still be unbiased. Robustness checks performing regressions excluding those two years do not exhibit any change in the results (see section 3.5).

Figure 3.3 – Evolution over time of some selected variables



To fully discuss the identifying assumption of common trends, it would be useful to run a placebo regression before the start of the program, namely before 2001. Such a regression would check whether the improve in access to higher education for treated high-schools is indeed due to the program, or to a pre-existing trend. Unfortunately, as stated in Section 3.3, the follow up of students in higher education only started in 2005. It is then impossible with available data to run placebo tests in order to strengthen the identification strategy.

Other descriptive statistics at the 2005 baseline, regarding the distribution of tracks Baccalauréat laureates choose after graduation, are displayed in Table 3.3. In average in our sample, the number of students getting their Baccalauréat is around 100 per high-school. The vast majority of them choose a track to University (around 70%), whereas 6% choose a preparatory school and 6% opt for a technical school. Regardless of the group considered (treatment of control), around 15% of a cohort of laureates is exiting education after the Baccalauréat. These figures are far below those presented by the Ministry in governmental publications where on average in France, only one or two percent of Baccalauréat laureates (coming from Scientific, Economic or Literature tracks) are not accessing higher education. This fact is not surprising when recalling characteristics of under privileged high-schools in our sample. The CEP program is thus all the more relevant, because there is a significant space for improving access to higher education for students from these high-schools.

3.4 Results

3.4.1 Access to higher education

Tables 3.4, 3.5 and 3.6 present differences-in-differences estimates of the effect of the CEP program on the access rate to higher education. Each table is considering a different control group, namely eligible schools in CEP *Académies*, eligible schools in non-CEP *Académies* and future CEP schools. For the last control group, the sample size is considerably reduced since in order to compare early to future CEP partners, regression are only run over periods 2005-2007.

Results are unambiguous, estimated effects of the program are positive and significant, regardless of the control group or of the number of covariates. For instance in Table 3.4, column 3, the estimate indicates that a school, that signed a CEP, increased by 3 percentage point (pp. hereafter) its access rate to higher education. Adding controls (column 2) to the simple regression (column 1) logically reduces the raw estimate, but controlling for the year a school signed a CEP increases the estimated effect of the program. This means there is heterogeneity within partners high-schools: the "Treatpost $\times$ year_cep" coefficient controls not only for the year the CEP was signed, but also for the duration a high-school has been treated (the higher, the less time the school was enrolled in the program). Since the estimated coefficient is negative, it means that the program has a smaller impact on late CEP partners.

Recalling Table 3.2 (column 8), we see that the second wave of CEP partners has, in average, a lowest share of students coming from ZEP and a lowest share of ZEP high-schools. This might be due to the opening, after 2007, of the CEP program to overseas territories or areas outside of Île de France (which is the territory around Paris). This advocates for the use of the interaction term between the effect of the program and the year a school signed the CEP, to control for potential heterogeneity. The fact that the parameter estimates of this term, namely Treatpost $\times$ year_cep, is negative but only statistically significant with the first control group (Table 3.4, column 3), suggest somehow that first treated high-schools are the one where the impact of the program might be the strongest. This could either stems from the role model hypothesis (the longer a high-school is enrolled in the program, the higher the impact, see section 3.4.2), or from the fact that the second wave of high-school partners includes more rural *Académies* and have principals and teaching staffs less motivated than those from the first wave. In rural *Académies*, CEP can be less efficient because the supply in higher education is less dense, which might induce extra costs (inadequate training offered or financial burden to study far away). Moreover, because high-schools from the second wave have less under-privileged students, the program may play a less crucial role in addressing them to higher education.

In general, there is no correlation between the number of Baccalauréat candidates and access to higher education. As expected, the share of disadvantaged students and the share of students repeating the final grade are negatively correlated to access to higher education. Only the latter is significant though, and is somehow mechanical: if more students repeat their final high-school grade, the number of students allowed to apply for higher education is lower. Concerning the educational staff, there are some evidence that the share of highest qualified teachers and the share older teachers are positively correlated with the access to higher education. However, the parameter associated with the share of teachers over 50 years old is never statistically significant, whereas the parameter corresponding to the share of highest qualified teachers is only statistically significant with the first control group.

Theoretically, the CEP program can improve the access to higher education by two channels, either directly by bringing higher education closer to high-schoolers, or indirectly by increasing the Baccalauréat pass rate. In fact, if more students obtain the Baccalauréat, more students can then apply for higher education institutions. This indirect effect has been the core of the article written by Diagne and Wasmer (2013). Previous estimates (Table 3.4-3.6) of the impact of the CEP program are thus estimates of a mix of these direct and indirect effects, because the dependent variable is the ratio of students accessing higher education over all the Baccalauréat candidates.

However, by narrowing down the measure to the ratio of students accessing higher education over Baccalauréat laureates, we get an estimate of the direct effect of the CEP program on pursuing in higher education. This ratio measures how the retention rates for higher education of partners high-schools are impacted. As shown in the descriptive statistics in Table 3.3, no less than 15% of the laureates in our sample do not enter higher education. Table 3.7 shows that the program increased by 3 pp. (column 1) the probability that a Baccalauréat laureate would access higher education. For the average treated school at baseline 2005, it represents 3.5 students. Unsurprisingly, the coefficient estimates of students repeating grade is close to zero and not significant anymore, since population of interest is Baccalauréat laureates and not candidates. Moreover, we observe this time a positive and statistically significant correlation between the share of disadvantaged students and access to higher education. Puzzling at first, this result might be explained by the widespread scholarship system existing in France and its strong link between secondary and tertiary education. As seen in the previous paragraph, the unconditional share of students accessing higher education is negatively correlated (but not significantly) with the share of disadvantaged students. Conversely, when we condition on the Baccalauréat obtainment, the correlation becomes positive. A potential explanation would be that the Baccalauréat graduation operates as a filter. In high-schools where the share of disadvantaged students is higher, students who graduate are the most motivated ones, and the most likely to enter higher education.

Aside the question of granting access to higher education, lies the issue concerning the *quality* of education. We have seen the CEP program has a volume effect, because it increases the number of students pursuing their studies after the Baccalauréat. However, has the program modified the tracks students would normally choose? In other words, by offering to students from deprived area a special admission procedure to one of the most selective school in France, does the CEP program broadens the scope of potential applications these students would do? To test this hypothesis, I built a new ratio, based on the number of students accessing selective tracks after the Baccalauréat (namely engineers schools, business schools, preparatory schools and SCiences Po), over the total number of Baccalauréat candidates. Results are displayed in Table 3.8 and show that high-school treated send their students to a selective track with a higher probability of around 2 pp. In that regression though, the correlation between the share of disadvantaged students and the dependent variable is negative and significant. There is then a link between the socioeconomic background, and the propensity to access a selective track, which was not the case when I aggregated all different tracks. Even though it would require to control for school achievement level, this kind of evidence reinforces the idea that pre-market selection plays a role in France, where students from the unprivileged socioeconomic backgrounds self-select out of the most prestigious selective tracks.

3.4.2 Role models

As stated in the previous section, controlling for the year the high-school signed a CEP improves the model performance. By doing so, we take into account either the heterogeneity between different waves of partners (for instance early high-schools compared to later ones) or the elapsed time since the beginning of the CEP program. Estimates of the program impact are thus averaging these two effects. One way though to disentangle them, and try to assess the dynamic impact of the program, is to develop a role model.

A role model states that the current number of students accessing higher education depends on the past number of students from the high-school who successfully entered Sciences Po thanks to the CEP program. Formally presented in Diagne and Wasmer (2013), it consists in regressing the rate of access to higher education, on the cumulative number of past admits in Sciences Po and other control covariates. The model implicitly assume that if a high-school performs well by sending students to Sciences Po, it gathers "success stories" that spread among students and give them incentives to try to enter higher education.

Table 3.9 presents role model estimates, for each of the three control groups. All estimates are positive and significant, and validate the theory according to which the more students sent to Sciences Po in the past years, the more students from the high-school will access higher education during the current year. For instance, if a high school sent 10 students in the past years, the ratio of access to higher education is increased by 1.1 pp. (column 1). In addition, if the share of the past admitted students over the total number of Baccalauréat candidates increases by 10 pp., then the access rate would increase by 2 pp. (column 4).

3.4.3 Stratification of the sample

In order to refine the results, and to better understand where the impact of the CEP program stems from, I stratify the sample. Because the unit of analysis is the high-school, by stratification I intend to focus on subgroups in each high-schools, which will not reduce the sample size. For instance, if a high-school is composed by two subgroups, let us say x and y, a new dependent variable for regression would be the ratio of members from x who accessed higher education, over the total number of x. The following subsections apply this strategy.

According to Baccalauréat track

A first intuition is to consider if the CEP program had a differentiated impact according to the track students are following in high-schools. Because this work focus on general Baccalauréat tracks, only three majors are considered: Economic, Scientific and Literature tracks.

Table 3.10 presents the results of these regressions. It appears that the access to higher education is only significant for students from the economic track (increase of 3.8 pp., column 1), compared to the scientific or literature tracks (no statistically significant impact). To better understand this result, we have to dive into the high school black box, and the mechanism at stake when teachers from CEP high schools are selecting their students to be part of the program. Three hypothesis are plausible. Firstly, during the oral examination in the high-school, teachers do not select in priority letter or scientific students. This might be due either because these students are less interested and then under represented, or because they are less likely to meet the requirements set by the examination panel. This explanation is validated by statistical evidence exposed by Le Chapelain (2015) : in average, 60% of CEP students in high schools are from the econ track, 25% from the sciences track and only 15% from the literature track. Secondly, the preparation students follow to be admitted in the program (see section 3.2.2) empowers them with skills that are more in line with the curricular of the econ track. Therefore, the program may increase the number of Baccalauréat graduates, and mechanically increase the access rate to higher education. Finally, there exists an implicit hierarchy between tracks in high-school in France, according to which good students follow the Science track while lower achiever students attend the Literature track. In that case, econ students are those who are the most likely to marginally react to any kind of incentive.

According to gender

Another aspect worth exploring is the gender dynamic within the program. Do girls perform better than boys, or the opposite? Following the seminal work initated by Kling *et al.* (2005), who documented mixed results by gender on youth outcomes following the Moving to Opportunity program, I run regressions focusing alternatively on boys and girls.

Regressions of the number of boys (girls) accessing higher education over the total number of boys (girls) Baccalauréat candidates on the program variable of interest and other control variables are displayed in Table 3.11. Alternative dependent variables such as 1) the number of boys (girls) accessing higher education over the total number of Baccalauréat candidates and 2) the ratio of girls over boys accessing higher education (not presented here but available upon request) exhibit the same results. No significant impact is detected, neither for girls, nor for boys, and thus, no differentiated impact can be detected according to gender.

This result is important though, especially because some concerns, related to the skills developed during the preparation of the admission procedure, have arisen. High-school teachers supervising students explained that training not only consisted in academic work, but also in empowering students by boosting their confidence, improving their oral expression, and fostering their quickness to respond. The development of these non-cognitive skills may however be unequal between boys and girls, and some may have feared that boys, more confident at the start, would have diverted resources and attention away from girls.

In line with these concerns, annual reviews of sociology such as Buchmann *et al.* (2008) and Xie *et al.* (2015) report how the accumulation process of non-cognitive skills could affect academic performance. However, results presented here show there is no statistical evidence that girls and boys in treated high-schools accessed higher education differently. Other regressions focusing on the specific access to selective tracks (not presented in this work, but available upon request) do not exhibit any gender bias in the impact of the CEP program.

According to socio-economic background

The French Ministry of Education gathers data concerning occupations of students' parents. This information is then organize according to 4 categories, which take into account both the level of the household income and the academic performance of students according to their socio-economic status (SES hereafter). As an example, children of teachers are sorted in the highest class ("highly privileged") because they compensate a lower level of revenues by over-performing in average during standardized tests.

Following the same methodology as before, I run regressions for each of the 4 categories of SES, to detect if either the program benefits in particular to a certain subgroup of the population or if there is a crowding effect of some SES category against another. Table 3.13, 3.14, 3.15 and 3.16 present some of these results. In Table 3.13 for instance, the number of highly privileged students over the total number of highly privileged Baccalauréat candidates, accessing higher education in treated high-school is not statistically different than elsewhere. Not only I do not find a differentiated impact according to the SES, but the same regressions run for the access to selective track show the same results.

Although there is a line of research more critical and somehow suggesting CEPs may increase inequalities, my findings are more optimistic. A first layer of analysis focuses on inequalities *between* schools. Oberti (2013) underlines the fact that if the CEP program indeed increases the social diversity in the selection process of Sciences Po, the selection of high-schools partners remains unequal. In fact, he criticizes the spatial heterogeneity the program induced: whereas some districts have many high-school enrolled in the program, the adjacent area may have none. However, a second layer of analysis targets inequalities *within* schools, what this article is digging in, and results show no evidence the program is increasing inequalities at this level. More research could be done though to connect these two sides of the analysis.

Interpretation of the stratification

The absence of significant results with the stratifications relative to gender and SES does not mean there is not a differentiated impact, but rather if there is one, it is not detected with the design of the study. In fact, since the unit of analysis is not reduced, the sample size does not decrease and therefore the initial statistical power is preserved. We would then expect to estimate one significant parameter in a subgroup at least. Yet, when the variable of interest is changed, the minimum detectable effect is altered, since it is directly correlated with the variance of the outcome. The minimum required sample size is modified, according to equation 3.1:

$$n_1 = \frac{r+1}{r}\sigma^2_{\hat{Y}}\left[\frac{Z_{\alpha/2}+Z_\beta}{MDE}\right]^2 \times VIF \tag{3.1}$$

where r is the ratio of the larger group n_2 (control high schools) over n_1 (treated high schools) and $n = n_1 + n_2$ is the minimum required sample size, $\sigma_{\hat{Y}}$ the standard deviation of the outcome, $Z_{\alpha/2}$ the probability of type I error set at 5%, Z_β the probability of type II error set at 20% and VIF the variance inflating factor taking into account the different clusters.

Table 3.1 – Minimum required sample size to get an MDE of 3pp.

dependant variable	r	$\sigma_{\hat{Y}}$	$Z_{\alpha/2}$	Z_β	VIF	n
Access rate to high. ed.	1.8	9.6	1.96	0.84	5.01	1770
Access rate to high. ed. for boys	1.8	12.9	1.96	0.84	5.01	3163
Access rate to high. ed. for girls	1.8	13.7	1.96	0.84	5.01	3568

Table 3.1 displays for different parameters the minimum required sample size to get an MDE of 3pp (baseline result of the study) for the first control group. The increase in the standard deviation of the outcome inflates the minimum required sample size and can explain why no significant effect is detected when focusing on a subgroup.

3.5 Robustness checks

3.5.1 Integrity of the database

As pictured in Figure 3.3, a small gap in trends of access to higher education is observed for years 2007 and 2008. Since the new database concerning the follow-up of students after the Baccalauréat started in 2005, some protocols were not fully functional and some identifiers were lost during the aforementioned years.

In order to ensure that results are not driven by a differential attrition between control and treatment groups, I run regressions using the same methodology as before but I withdraw years 2007 and 2008 from the sample. Let us for instance imagine that the attrition affected more heavily the control group than the treated one, then mechanically the treated group would perform better, and this event would act as a confounder for the causality I try to unveil.

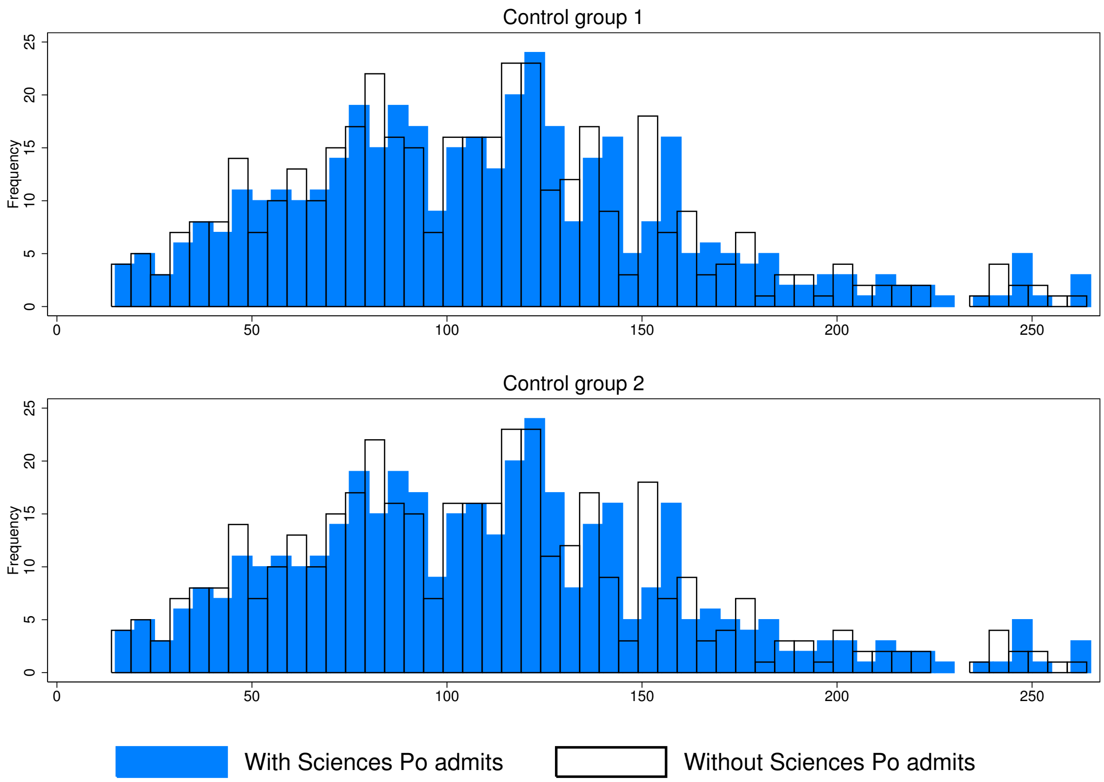
Table 3.17 presents results of such regressions and estimated coefficients are of the same magnitude as the one in the corresponding tables (third column of Tables 3.4, 3.5 and 3.6). Once again, I found that in treated high-school, the ratio of Baccalauréat candidates accessing higher education increases by approximately 3 pp.

3.5.2 Access rates without Sciences Po admits

Another concern that could arise is whether or not the impact of the program is solely driven by students admitted in Sciences Po. In average, 1.5 to 1.9 students per high school are accepted in Sciences Po every year, and this has to be compared with an average effect of 3 additional Baccalauréat laureates accessing higher education (estimated effect of the program, around 3 percentage points, applied to the average number of laureates, namely 100). Figure 3.4 plots the distribution of the number of students accessing higher education, with and without CEP admits. Unsurprisingly, distributions overlap since only a tiny share of students accessing higher education achieve to enter Sciences Po.

A more systematic check is to run regressions without students entering Sciences Po, in others words to compute a rate of access taking only into account all others tertiary tracks. Table 3.18 summarizes these results and we can see that even if the magnitude of the estimated coefficient of the impact of the program is less than 3 pp., it is still statistically significant, and above 2.5 pp for 5 regressions out of 6. The estimated coefficient relative to the second control group is not significant any more in the regression on the Baccalauréat candidates accessing higher education. This group is however the least close to the treated group in terms of descriptive statistics (see Table 3.2) and the one where the identification is the weakest and estimated impacts of the program the smallest (see Table 3.5). Overall, these regressions represent evidence of pre-market selection, since some students entered higher education without benefiting from a special admission procedure while they would not in the absence of the affirmative action. The program has indeed on knock-on effect on the high-school students.

Figure 3.4 – Distribution of the number of students accessing higher education, in treated high schools



3.5.3 School composition and diversity

Before the launch of the CEP program in 2001, a vivid debate took place within Sciences Po, but also beyond its walls. For many, this kind of positive action was undermining the republican meritocracy. Some of the former Sciences Po graduates withdrew their financial support to the school, while some of the current students were outraged that specific students can benefit from a special admission procedures. Delhay (2007) recalls some of the concerns that fed the controversy, and in particular the fact that the program would not increase the diversity but rather select "the bourgeois from the suburbs".

Although descriptive statistics concerning the program have shown the program indeed increased social diversity within Sciences Po, this argument offers another concern when it comes to the evaluation of the policy. Estimates of the regression are interpreted while keeping everything else equals, but some may argue that high-schools have evolved during the considered time span. In fact, becoming a Sciences Po partner for a high-school from a deprived area might be the opportunity to keep the best students, or attract better teachers. Both the composition of the teaching staff and of high-schoolers are then confounding factors when it comes to the evaluation of the impact of the policy.

In order to test the hypothesis that the treatment unit (the high-school) remained stable over time, I run several regressions summarized in Table 3.19. Each line corresponds to the regression of a specific dependent variable and each column to a specific control group. The coefficient reported is corresponding to an interaction of the treatment dummy and an indicator for periods after the beginning of the treatment.

An important result of this robustness check is that none of the statistical tests implemented indicate that the signature of a CEP have modify the school composition and diversity. Regardless of the identification strategy (the control group chosen), neither the proportion of 10th grade students originating from a ZEP (priority education) junior high-school, nor the share of disadvantaged students, nor the number of Baccalauréat candidates have been impacted by the CEP program. Regarding the teaching team, estimations indicate that the share of high skill teachers, the share of teachers over 50 or under 40 years old and the share of female teachers have not been statistically significantly impacted. This final table reinforces the conclusion that the improvement of performance in access to higher education for CEP high-schools is neither correlated to a change in their audience nor their teaching staff, but in fact due to the CEP program, via a decrease in pre-market discrimination.

3.6 Conclusion

Sciences Po CEP program is an example of affirmative action in the French style. To the contrary of the US, where racial statistics are available, the use of such statistics in France is prohibited. The CEP program is then close in mind to “colorblind” affirmative action, that try to increase diversity without targeting a particular ethnicity. Offering a special admission procedure to high-schools from deprived neighborhood is a way for Sciences Po to increase social diversity and to fight against pre-market selection. The program is designed in a way that gives incentives to students to invest in the skill accumulation process and reduces the self-selecting out of potential good students.

In this work, I build on the analytic framework of Diagne and Wasmer (2013) to analyze the effect of the program on the access rate to higher education. Three main channels may explain a potential impact of Sciences Po affirmative action. Firstly, the program offers to students from CEP high-schools a brand new opportunity they are willing to seize, which gives them incentives to raise their level of effort. Secondly, students in partners high-schools benefit from a far better information concerning higher education, and the program may motivate them to even dig further. Last but not least, bringing within reach an elite school to students may help them to build different aspirations for their future. Actually, in underprivileged areas, some students have not *even* thought accessing higher education was possible.

The econometric analysis consists in a differences-in-differences estimation. To assess the robustness of the results, three different control groups are successively used, namely “eligible”, but not partner, high schools in Académies where the CEP program was implemented, “eligible” high schools in Académies where the CEP program was not implemented and future CEP partners, or high-school who signed after 2008. Results are robust: the percentage of students pursuing their studies is increased by around 3 percentage points in treated high-schools and this effect is carried out by students following the economic track. Other regressions stratifying the sample do not exhibit any statistically significant differentiated impact of the program according to gender or socioeconomic status (parents’ occupation).

The increase for high-school students in the higher education ratio shows the importance of market failures in education, and casts some light on potential pre-market discrimination in France. Yet, some limits exist that could jeopardize these results. If, for instance, the composition of the treated high-school were changed during the period, then the effect of the program would be confounded with other factors. Opportunistic behavior of wealthier parents trying to enrol their children in partner high-schools, or high qualified teachers more willing to work in this environment are potential confounders. However, robustness check regressions do not show any impact of the program on the social composition or on staffing team characteristics of treated high-schools.

When it comes to impact evaluation, the Hawthorne effect concern cannot be easily dismissed. It would imply that my estimates set the upper bound of the real impact of the program. Besides, it would be interesting to disentangle more precisely through which channels the program transmits its effect. Especially when it comes to aspirations, and the potential social bias when they are build. In that regard, further qualitative work could be enlightening.

Finally, these findings suggest a non negligible role for pre-market discrimination in explaining a lower access to higher education in underprivileged high-schools in France. In the US however, the end of affirmative action reduced admission probabilities for minorities students without changing necessarily the application behavior of highly qualified minorities to elite schools. This shift in the US policy admission has been partly motivated by the crowding out effect of hiring minorities. In Sciences Po though, the dramatic increase in the number of admissions over the years has limited this side effect. The question of the opportunity to launch such "colored" policy is still raised though, when it will only benefit to the elite minority, already well informed of strategies to develop in order to enter higher education. Targeting geographical areas, based on economic resources, might have a stronger impact, because it decentralizes the positive action to the high-school level, and reaches out students that would not feel concerned otherwise. The underlying argument here is whether the race, or the socioeconomic environment, is more important when it comes to address inequalities. Further work attempting to answer this question, would allow to help designing better public policy against discrimination.

Table 3.2 – Descriptive statistics: school characteristics at baseline (2005)

	Eligible			Not-CEP Academies		Future CEP		
	(1) Treatment	(2) Control	(3) Diff.(T-C)	(4) Control 2	(5) Diff.(T-C2)	(6) Treat01-07	(7) Treat08-11	(8) Diff. (T01-07 - T08-11)
Number of Bac.	162.0	126.1		111.6		158.3	167.0	
candidates			35.83***		50.40***			-8.711
			(8.994)		(9.522)			(14.28)
Access to Higher Ed,	61.42	63.67		69.91		61.77	60.94	
%			-2.251*		-8.487***			0.836
			(1.236)		(1.231)			(1.840)
Disadvantaged	44.56	45.74		39.47		45.66	43.03	
students, %			-1.189		5.090***			2.622
			(1.308)		(1.609)			(2.611)
Students repeating	18.11	17.78		11.67		17.83	18.49	
final grades, %			0.330		6.445***			-0.656
			(1.036)		(0.965)			(1.495)
Teachers with	22.18	18.29		17.75		22.22	22.12	
highest qual., %			3.889***		4.423***			0.0942
			(1.046)		(1.284)			(1.988)
Teachers over 50, %	25.73	27.98		30.14		24.52	27.40	
			-2.256*		-4.409***			-2.886
			(1.154)		(1.428)			(2.088)
ZEP students(%)	37.97	24.17		24.36		43.41	30.23	
			13.80***		13.61***			13.18***
			(2.879)		(4.128)			(4.954)
School in ZEP, %	15.05	4.265		9.722		20.37	7.692	
			10.79***		5.332			12.68*
			(3.244)		(5.247)			(7.480)
Number of CEP	12.08	0		0		18.13	3.692	
admits, 2001-2013			12.08***		12.08***			14.44***
			(0.874)		(1.389)			(2.260)
Observations	93	216	309	86	179	54	39	93

Columns (3), (5) and (8) show t-tests on the equality of means for treatment and control groups.

Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3 – Descriptive statistics: repartition of students in higher education (2005)

	Eligible			Not-CEP Academies		Future CEP		
	(1) Treatment	(2) Control	(3) Diff.(T-C)	(4) Control 2	(5) Diff.(T-C2)	(6) Treat01-07	(7) Treat08-11	(8) Diff.
% Bac. graduates	72.47	75.88		84.22		71.53	73.77	
			-3.415**		-11.75***			-2.246
			(1.368)		(1.366)			(2.053)
% preparatory school	5.705	7.540		8.070		5.833	5.529	
			-1.835***		-2.365***			0.303
			(0.677)		(0.719)			(0.715)
% university	70.99	68.90		67.27		72.30	69.17	
			2.088**		3.719***			3.138*
			(1.034)		(1.131)			(1.590)
% engineer school	0.880	0.686		1.090		1.043	0.653	
			0.194		-0.210			0.391
			(0.169)		(0.225)			(0.329)
% management school	0.283	0.198		0.284		0.223	0.366	
			0.0849		-0.00118			-0.142
			(0.0683)		(0.118)			(0.129)
% private school	0.0974	0.217		0.449		0.135	0.0448	
			-0.120*		-0.351**			0.0906
			(0.0627)		(0.162)			(0.0711)
% technical school	6.392	6.403		6.008		6.055	6.859	
			-0.0105		0.384			-0.803
			(0.576)		(0.613)			(0.984)
% sciences po	0.545	0		0		0.939	0	
			0.545***		0.545***			0.939***
			(0.0862)		(0.137)			(0.249)
% exiting education	15.11	16.06		16.83		13.47	17.38	
			-0.946		-1.721*			-3.916***
			(0.806)		(0.923)			(1.218)
Observations	93	216	309	86	179	54	39	93

Columns (3), (5) and (8) show t-tests on the equality of means for treatment and control groups.
Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4 – DD estimates of the impact of CEPs on the probability to access higher education (Main) control group: Eligible high schools in CEP *Académies*

	(1)	(2)	(3)
DD effect of CEP	2.334***	1.838***	3.060***
	(0.894)	(0.670)	(1.082)
Treatment	-2.101**	-0.668	-0.669
	(1.012)	(0.746)	(0.747)
Treatpost x year cep			-0.269*
			(0.147)
Number of Bac. candidates		-0.003	-0.003
		(0.003)	(0.003)
Disadvantaged students, %		-0.025	-0.027
		(0.030)	(0.030)
Students repeating final grades, %		-0.686***	-0.682***
		(0.031)	(0.031)
Teachers with highest qual., %		0.065**	0.067**
		(0.029)	(0.030)
Teachers over 50, %		0.028	0.029
		(0.023)	(0.023)
Constant	70.239***	71.960***	72.043***
	(4.921)	(2.451)	(2.411)
Observations	2752	2751	2751
R^2	0.391	0.586	0.588

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.5 – DD estimates of the impact of CEPs on the probability to access higher education (Alternative) control group: Eligible high schools in non-CEP *Académies*

	(1)	(2)	(3)
DD effect of CEP	2.516***	1.775**	2.435*
	(0.935)	(0.696)	(1.439)
Treatment	-9.465***	-1.114	-0.999
	(0.830)	(1.873)	(1.854)
Treatpost x year cep			-0.125
			(0.190)
Number of Bac. candidates		-0.004	-0.004
		(0.005)	(0.005)
Disadvantaged students, %		-0.059*	-0.059*
		(0.032)	(0.032)
Students repeating final grades, %		-0.677***	-0.675***
		(0.045)	(0.045)
Teachers with highest qual., %		0.043	0.045
		(0.040)	(0.041)
Teachers over 50, %		0.051	0.051
		(0.034)	(0.034)
Constant	66.305***	72.809***	72.731***
	(0.618)	(1.688)	(1.698)
Observations	1597	1597	1597
R^2	0.512	0.643	0.643

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.6 – DD estimates of the impact of CEPs on the probability to access higher education (Alternative) control group: schools that signed CEPs in 08-13

	(1)	(2)	(3)
DD effect of CEP	3.530*	3.598**	5.286***
	(1.860)	(1.497)	(1.868)
Treatment	-1.579	-2.489	-2.399
	(2.338)	(1.710)	(1.732)
Treatpost x year cep			-0.593
			(0.397)
Number of Bac. candidates		0.004	0.004
		(0.008)	(0.008)
Disadvantaged students, %		-0.051	-0.048
		(0.053)	(0.052)
Students repeating final grades, %		-0.677***	-0.668***
		(0.085)	(0.086)
Teachers with highest qual., %		-0.030	-0.032
		(0.068)	(0.068)
Teachers over 50, %		0.028	0.023
		(0.059)	(0.057)
Constant	57.132***	72.804***	72.570***
	(3.267)	(4.042)	(4.150)
Observations	280	280	280
R^2	0.340	0.558	0.564

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.7 – DD estimates of the impact of CEPs on the probability to access higher education (for Baccalaureat laureates)

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
DD effect of CEP	3.063***	2.755**	3.976***
	(0.896)	(1.173)	(1.178)
Treatment	0.353	4.796***	1.141
	(0.574)	(1.334)	(1.427)
Treatpost x year cep	-0.280**	-0.217	-0.755**
	(0.114)	(0.145)	(0.304)
Number of Bac. candidates	-0.004	-0.005	-0.002
	(0.003)	(0.004)	(0.007)
Disadvantaged students, %	0.130***	0.110***	0.176***
	(0.021)	(0.029)	(0.047)
Students repeating final grades, %	0.000	0.039	0.010
	(0.022)	(0.033)	(0.067)
Teachers with highest qual., %	0.100***	0.061**	0.061
	(0.020)	(0.027)	(0.060)
Teachers over 50, %	0.005	0.039	0.012
	(0.019)	(0.030)	(0.054)
Constant	76.894***	73.919***	79.145***
	(1.618)	(1.512)	(4.404)
Observations	2751	1597	280
R^2	0.494	0.560	0.563

Robust standard errors (clustered by highschool) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.8 – DD estimates of the impact of CEPs on the probability to access higher education
Accessing selective tracks

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
DD effect of CEP	2.708***	1.881***	2.275***
	(0.558)	(0.544)	(0.767)
Treatment	-1.043**	0.769	0.979
	(0.443)	(0.738)	(0.712)
Treatpost x year cep	-0.271***	-0.180**	-0.419***
	(0.075)	(0.078)	(0.127)
Number of Bac. candidates	-0.003	-0.007***	0.001
	(0.002)	(0.003)	(0.003)
Disadvantaged students, %	-0.065***	-0.069***	-0.040*
	(0.015)	(0.021)	(0.022)
Students repeating final grades, %	-0.153***	-0.139***	-0.112***
	(0.019)	(0.020)	(0.029)
Teachers with highest qual., %	0.053**	0.020	-0.045*
	(0.022)	(0.023)	(0.026)
Teachers over 50, %	-0.002	0.008	0.021
	(0.014)	(0.016)	(0.026)
Constant	14.882***	8.414***	9.247***
	(3.227)	(0.864)	(1.450)
Observations	2751	1597	280
R^2	0.425	0.492	0.478

Robust standard errors (clustered by highschool) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to a selective track. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.9 – Impact of the number of past CEP admits on the probability to access higher education

	(1) C1	(2) C2	(3) C3	(4) C1	(5) C2	(6) C3
Cumulative number of CEP admits	0.110***	0.104**	0.334***			
	(0.034)	(0.043)	(0.102)			
Cum. num. of CEP admits,% 12Gr. studts				0.201***	0.211***	0.579***
				(0.056)	(0.068)	(0.137)
Treatment	-0.486	-1.228	-2.568	-0.496	-1.449	-2.511
	(0.693)	(1.819)	(1.661)	(0.685)	(1.880)	(1.653)
Treatpost x year cep	0.092	0.184**	0.468	0.082	0.191**	0.452
	(0.082)	(0.082)	(0.323)	(0.084)	(0.085)	(0.322)
Number of Bac. candidates	-0.004	-0.006	0.002	-0.003	-0.003	0.006
	(0.003)	(0.005)	(0.007)	(0.003)	(0.005)	(0.007)
Disadvantaged students, %	-0.021	-0.053*	-0.043	-0.023	-0.057*	-0.046
	(0.030)	(0.032)	(0.049)	(0.030)	(0.032)	(0.051)
Students repeating final grades, %	-0.683***	-0.670***	-0.673***	-0.688***	-0.677***	-0.687***
	(0.031)	(0.044)	(0.084)	(0.032)	(0.045)	(0.083)
Teachers with highest qual., %	0.069**	0.048	-0.032	0.066**	0.051	-0.036
	(0.029)	(0.041)	(0.064)	(0.029)	(0.041)	(0.067)
Teachers over 50, %	0.028	0.051	0.037	0.030	0.052	0.039
	(0.023)	(0.033)	(0.053)	(0.023)	(0.034)	(0.054)
Constant	72.059***	72.896***	65.361***	77.044***	72.804***	64.982***
	(2.444)	(1.691)	(4.198)	(2.399)	(1.704)	(4.194)
Observations	2751	1597	280	2744	1597	280
R^2	0.588	0.645	0.570	0.589	0.646	0.573

Robust standard errors (clustered by highschool) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In all columns, the dependent variable is the access rate to higher education.

All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.10 – DD estimates of the impact of CEPs on access to Higher Education (conditional on the track)

	Econ			Sciences			Letter		
	(1) C1	(2) C2	(3) C3	(4) CI	(5) C2	(6) C3	(7) C1	(8) C2	(9) C3
DD effect of CEP	3.775***	3.472*	5.134**	1.146	0.441	2.751	-1.961	-1.894	2.934
	(1.429)	(1.854)	(2.235)	(1.145)	(1.490)	(2.484)	(2.032)	(2.649)	(3.379)
Treatment	-1.562	-3.971*	-2.360	-0.673	1.015	-1.887	0.565	-4.445	-4.655*
	(1.097)	(2.341)	(2.442)	(0.767)	(1.589)	(2.144)	(1.192)	(3.810)	(2.775)
Treatpost x year	-0.342*	-0.230	-0.594	-0.106	0.013	0.056	0.109	0.278	-0.342
cep	(0.184)	(0.244)	(0.507)	(0.156)	(0.185)	(0.555)	(0.281)	(0.352)	(0.729)
Number of Bac.	-0.001	-0.003	0.001	0.002	0.001	0.016	-0.005	-0.000	-0.006
candidates	(0.005)	(0.006)	(0.010)	(0.004)	(0.005)	(0.011)	(0.006)	(0.008)	(0.012)
Disadvantaged	-0.044	-0.069	-0.168**	0.012	-0.066*	-0.008	-0.043	-0.007	0.037
students, %	(0.039)	(0.046)	(0.080)	(0.034)	(0.039)	(0.075)	(0.055)	(0.058)	(0.097)
Students	-0.688***	-0.671***	-0.610***	-0.672***	-0.642***	-0.646***	-0.689***	-0.702***	-0.857***
repeating final grades, %	(0.045)	(0.061)	(0.117)	(0.035)	(0.055)	(0.113)	(0.063)	(0.087)	(0.177)
Teachers with	0.079*	0.073	0.074	0.054*	0.010	-0.117	0.032	0.020	-0.026
highest qual., %	(0.041)	(0.055)	(0.112)	(0.031)	(0.042)	(0.089)	(0.044)	(0.068)	(0.117)
Teachers over	-0.015	-0.006	-0.159*	0.052*	0.069*	0.078	0.033	0.018	0.115
50, %	(0.032)	(0.046)	(0.085)	(0.027)	(0.035)	(0.076)	(0.042)	(0.055)	(0.094)
Constant	77.683***	78.583***	80.171***	76.747***	77.000***	71.890***	74.008***	75.352***	69.165***
	(3.151)	(2.549)	(7.123)	(2.283)	(1.996)	(5.409)	(4.133)	(5.516)	(6.844)
Observations	2377	1451	280	2723	1591	280	2121	1321	243
R^2	0.400	0.448	0.382	0.497	0.540	0.412	0.291	0.338	0.458

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
C1, C2 and C3 stand for the different control groups employed : respectively eligible high schools, eligible high schools in non-CEP (*Académies*), and future members of the CEP program.
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment.
All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.11 – DD estimates of the impact of CEPs on the probability to access higher education: gender stratification

	Boys			Girls		
	(1) C1	(2) C2	(3) C3	(4) C1	(5) C2	(6) C3
DD effect of CEP	1.609	1.439	2.897	1.147	0.371	3.691*
	(1.189)	(1.555)	(2.666)	(1.192)	(1.595)	(2.024)
Treatment	-1.174	-0.236	-2.100	-0.389	-1.848	-2.673
	(0.844)	(1.879)	(2.394)	(0.788)	(2.094)	(1.779)
Treatpost x year cep	-0.125	0.015	-0.038	-0.132	0.008	-0.508
	(0.158)	(0.204)	(0.505)	(0.163)	(0.210)	(0.468)
Number of Bac. candidates	-0.003	-0.005	0.003	0.001	-0.000	0.010
	(0.004)	(0.005)	(0.011)	(0.004)	(0.005)	(0.008)
Disadvantaged students, %	-0.010	-0.048	-0.034	-0.039	-0.067*	-0.077
	(0.035)	(0.039)	(0.076)	(0.033)	(0.036)	(0.053)
Students repeating final grades, %	-0.749***	-0.696***	-0.697***	-0.638***	-0.657***	-0.645***
	(0.039)	(0.055)	(0.116)	(0.033)	(0.050)	(0.092)
Teachers with highest qual., %	0.032	0.036	-0.091	0.081***	0.055	0.011
	(0.035)	(0.045)	(0.081)	(0.030)	(0.043)	(0.074)
Teachers over 50, %	0.027	0.053	0.038	0.015	0.041	-0.013
	(0.028)	(0.039)	(0.075)	(0.026)	(0.036)	(0.062)
Constant	73.590***	71.363***	73.257***	76.614***	74.192***	72.648***
	(2.713)	(1.902)	(4.711)	(2.501)	(1.967)	(4.877)
Observations	2751	1597	280	2736	1589	280
R^2	0.491	0.557	0.431	0.464	0.523	0.495

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
C1, C2 and C3 stand for the different control groups employed : respectively eligible high schools, eligible high-schools in non-CEP (*Académies*), and future members of the CEP program.
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment.
All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.12 – DD estimates of the impact of CEPs on access to Higher Education
Accessing selective tracks according to gender

	Boys			Girls		
	(1)	(2)	(3)	(4)	(5)	(6)
	C1	C2	C3	C1	C2	C3
DD effect of CEP	1.443**	0.952	0.108	0.802	-0.177	1.010
	(0.684)	(0.617)	(0.921)	(0.534)	(0.540)	(0.663)
Treatment	-1.263**	2.488**	1.734*	-0.850**	-0.221	-0.107
	(0.564)	(1.062)	(0.940)	(0.381)	(0.624)	(0.623)
Treatpost x year cep	-0.249***	-0.193**	-0.148	-0.069	0.045	-0.153
	(0.092)	(0.095)	(0.151)	(0.075)	(0.075)	(0.115)
Number of Bac. candidates	-0.001	-0.005*	0.001	0.001	-0.004	0.005*
	(0.003)	(0.003)	(0.005)	(0.002)	(0.003)	(0.003)
Disadvantaged students, %	-0.063***	-0.074***	-0.054*	-0.048***	-0.051***	-0.053***
	(0.020)	(0.026)	(0.030)	(0.014)	(0.018)	(0.019)
Students repeating final grades, %	-0.199***	-0.179***	-0.132***	-0.118***	-0.106***	-0.083***
	(0.024)	(0.026)	(0.044)	(0.017)	(0.021)	(0.026)
Teachers with highest qual., %	0.050*	-0.005	-0.082*	0.048**	0.037*	-0.000
	(0.027)	(0.027)	(0.044)	(0.019)	(0.019)	(0.023)
Teachers over 50, %	-0.008	0.006	0.029	-0.003	0.003	-0.013
	(0.018)	(0.022)	(0.038)	(0.012)	(0.016)	(0.022)
Constant	18.061***	11.035***	12.826***	12.217***	5.493***	7.148***
	(3.457)	(1.171)	(2.726)	(2.841)	(0.808)	(1.227)
Observations	2751	1597	280	2736	1589	280
R^2	0.348	0.436	0.390	0.289	0.333	0.418

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
C1, C2 and C3 stand for the different control groups employed : respectively eligible high schools, eligible high-schools in non-CEP (*Académies*), and future members of the CEP program.
In all columns, the dependent variable is the access to a selective track. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment.
All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.13 – DD estimates of the impact of CEPs on the probability to access higher education: Highly privileged SES

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
DD effect of CEP	2.015	1.648	2.142
	(1.407)	(1.712)	(2.914)
Treatment	-1.066	-18.652***	2.131
	(1.064)	(2.045)	(2.481)
Treatpost x year cep	-0.168	-0.069	-0.258
	(0.184)	(0.241)	(0.613)
Number of Bac. candidates	0.001	0.002	0.007
	(0.004)	(0.007)	(0.013)
Disadvantaged students, %	-0.012	-0.032	0.040
	(0.044)	(0.056)	(0.095)
Students repeating final grades, %	-0.584***	-0.472***	-0.565***
	(0.048)	(0.078)	(0.145)
Teachers with highest qual., %	0.050	0.002	-0.100
	(0.040)	(0.059)	(0.125)
Teachers over 50, %	0.001	0.067	0.123
	(0.032)	(0.047)	(0.098)
Constant	73.371***	87.649***	68.310***
	(3.013)	(3.517)	(7.145)
Observations	2726	1586	279
R^2	0.298	0.300	0.218

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.14 – DD estimates of the impact of CEPs on the probability to access higher education: Privileged

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
DD effect of CEP	1.896	0.366	4.007
	(1.509)	(1.961)	(3.004)
Treatment	-0.792	-3.722	-2.354
	(1.064)	(4.139)	(2.953)
Treatpost x year cep	-0.211	0.006	-0.485
	(0.204)	(0.264)	(0.577)
Number of Bac. candidates	-0.003	-0.003	0.030**
	(0.005)	(0.007)	(0.014)
Disadvantaged students, %	-0.002	-0.030	0.011
	(0.040)	(0.046)	(0.106)
Students repeating final grades, %	-0.663***	-0.761***	-0.660***
	(0.049)	(0.071)	(0.145)
Teachers with highest qual., %	0.096**	0.066	0.029
	(0.040)	(0.053)	(0.102)
Teachers over 50, %	0.056	0.061	-0.116
	(0.036)	(0.050)	(0.106)
Constant	68.946***	81.352***	63.054***
	(3.602)	(2.828)	(7.770)
Observations	2725	1586	279
R^2	0.254	0.301	0.280

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years (1998-2012).

Table 3.15 – DD estimates of the impact of CEPs on the probability to access higher education: Average

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
DD effect of CEP	0.673	-0.373	2.458
	(1.353)	(1.731)	(2.149)
Treatment	-1.126	0.689	-3.707
	(0.991)	(1.935)	(2.685)
Treatpost x year cep	-0.003	0.175	-0.047
	(0.162)	(0.197)	(0.502)
Number of Bac. candidates	-0.002	-0.004	-0.008
	(0.005)	(0.006)	(0.011)
Disadvantaged students, %	0.010	-0.016	-0.009
	(0.037)	(0.048)	(0.075)
Students repeating final grades, %	-0.705***	-0.649***	-0.566***
	(0.041)	(0.058)	(0.141)
Teachers with highest qual., %	0.034	-0.005	-0.119
	(0.037)	(0.052)	(0.095)
Teachers over 50, %	0.015	0.043	0.047
	(0.029)	(0.042)	(0.086)
Constant	79.170***	70.971***	71.584***
	(2.864)	(2.333)	(7.424)
Observations	2741	1594	280
R^2	0.345	0.390	0.296

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.16 – DD estimates of the impact of CEPs on the probability to access higher education: Under privileged

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
DD effect of CEP	1.712	1.358	5.636**
	(1.281)	(1.690)	(2.564)
Treatment	-0.357	-3.961*	-4.996***
	(0.839)	(2.008)	(1.751)
Treatpost x year cep	-0.215	-0.097	-0.437
	(0.188)	(0.237)	(0.583)
Number of Bac. candidates	-0.006	-0.005	0.003
	(0.004)	(0.005)	(0.009)
Disadvantaged students, %	-0.001	0.002	-0.042
	(0.036)	(0.042)	(0.065)
Students repeating final grades, %	-0.679***	-0.707***	-0.709***
	(0.038)	(0.057)	(0.090)
Teachers with highest qual., %	0.114***	0.103**	0.065
	(0.034)	(0.048)	(0.089)
Teachers over 50, %	0.004	0.021	-0.052
	(0.030)	(0.041)	(0.073)
Constant	70.082***	76.726***	70.345***
	(3.182)	(2.015)	(4.834)
Observations	2749	1596	280
R^2	0.434	0.461	0.430

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.17 – DD estimates of the impact of CEPs on the probability to access higher education
Robustness check: without years 2007 and 2008

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
DD effect of CEP	3.266***	3.354**	5.989**
	(1.150)	(1.501)	(2.692)
Treatment	-1.000	-5.380**	-1.797
	(0.780)	(2.649)	(1.752)
treatpostXyearcep	-0.392**	-0.324	-1.061
	(0.191)	(0.249)	(0.713)
Number of Bac. candidates	-0.005	-0.005	0.003
	(0.004)	(0.006)	(0.010)
Disadvantaged students, %	-0.043	-0.061	-0.044
	(0.031)	(0.041)	(0.070)
Students repeating final grades, %	-0.686***	-0.712***	-0.728***
	(0.035)	(0.063)	(0.095)
Teachers with highest qual., %	0.063**	0.023	-0.043
	(0.031)	(0.049)	(0.086)
Teachers over 50, %	0.033	0.049	0.054
	(0.026)	(0.045)	(0.076)
Constant	72.843***	77.415***	71.633***
	(2.681)	(3.309)	(4.271)
Observations	2134	813	186
R^2	0.526	0.644	0.588

Robust standard errors (clustered by highschool) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment. All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.18 – DD estimates of the impact of CEPs on access to Higher Education (without students accessing Sciences Po)

	Candidates			Laureates		
	(1)	(2)	(3)	(4)	(5)	(6)
	CI	C2	C3	C1	C2	C3
DD effect of CEP	2.515**	1.843	4.705**	2.743***	2.413**	3.702***
	(1.092)	(1.452)	(1.909)	(0.897)	(1.176)	(1.184)
Treatment	-0.656	-0.924	-2.503	0.361	4.824***	1.054
	(0.748)	(1.864)	(1.733)	(0.574)	(1.335)	(1.429)
Treatpost x year cep	-0.228	-0.080	-0.504	-0.258**	-0.194	-0.714**
	(0.148)	(0.191)	(0.403)	(0.114)	(0.145)	(0.303)
Number of Bac. candidates	-0.003	-0.005	0.004	-0.004	-0.005	-0.002
	(0.003)	(0.005)	(0.008)	(0.003)	(0.004)	(0.007)
Disadvantaged students, %	-0.028	-0.060*	-0.055	0.131***	0.111***	0.173***
	(0.030)	(0.032)	(0.052)	(0.021)	(0.029)	(0.047)
Students repeating final grades, %	-0.682***	-0.677***	-0.667***	0.000	0.039	0.012
	(0.031)	(0.045)	(0.086)	(0.022)	(0.033)	(0.067)
Teachers with highest qual., %	0.066**	0.046	-0.030	0.100***	0.062**	0.064
	(0.030)	(0.041)	(0.068)	(0.020)	(0.028)	(0.060)
Teachers over 50, %	0.029	0.052	0.020	0.005	0.039	0.010
	(0.023)	(0.034)	(0.056)	(0.019)	(0.030)	(0.054)
Constant	72.082***	72.778***	66.217***	76.913***	73.935***	68.544***
	(2.413)	(1.704)	(4.181)	(1.625)	(1.518)	(4.540)
Observations	2751	1597	280	2751	1597	280
R^2	0.589	0.646	0.565	0.493	0.558	0.563

Robust standard errors (clustered by school) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C1, C2 and C3 stand for the different control groups employed: respectively eligible high schools, eligible high schools in non-CEP (*Académies*), and future members of the CEP program.

In all columns, the dependent variable is the access rate to higher education. The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment.

All regressions include dummies for academic regions (*Académies*) and school years.

Table 3.19 – DD estimates of the impact of CEPs on the school composition

	Control:		
	(1) eligible	(2) not-CEP academies	(3) future CEPs
Disadvantaged students, %	2.299	0.486	3.182
	(2.403)	(3.156)	(5.840)
Gr10 students from a ZEP, %	3.711	5.738	2.545
	(6.623)	(9.083)	(12.088)
Number of Baccalauéat candidates	20.444	-4.442	-10.950
	(18.605)	(18.254)	(38.412)
Teachers with highest qual., %	-0.834	-2.330	-0.438
	(2.291)	(2.769)	(3.585)
Teachers over 50, %	-2.408	-0.904	3.697
	(2.261)	(2.305)	(3.701)
Teachers below 40, %	-1.256	-1.709	-4.514
	(2.758)	(3.084)	(4.626)
Female teachers, %	4.740**	4.684	4.455
	(2.259)	(3.000)	(3.346)

Robust standard errors (clustered by highschool) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Each line reports the coefficient of the DD effect on the dependent variable (left column).
The coefficient reported for DD effect of CEP is the coefficient on the interaction of the treatment dummy and an indicator for periods after the beginning of treatment.
All regressions include dummies for academic regions (*Académies*) and school years.

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