CHAPTER 5

RISING EDUCATIONAL ATTAINMENT AND OPPORTUNITY EQUALIZATION: EVIDENCE FROM FRANCE

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ABSTRACT

Educational policies are widely recognized as the means par excellence *to equalize opportunities among children with different social and family backgrounds and to promote intergenerational mobility. In this chapter, we focus on the French case and we apply the opportunity equalization criterion proposed by Andreoli, Havnes, and Lefranc (2019) for evaluating the effect of rising compulsory schooling requirements in secondary education. Our results show that such education expansion has a limited redistributive effect on students' earnings distribution. Nonetheless, we provide evidence of opportunity equalization among groups of students defined by family background circumstances.*

Keywords: Equality of opportunity; education; inverse stochastic dominance; economic distance; income distribution; policy evaluation

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INTRODUCTION

Equality of opportunity (EOP) has gained popularity, in scholarly debates as well as among policymakers, for defining the relevant objective for distributive justice. Nowadays, public policy often explicitly seeks to level the playing field among citizens and to equalize opportunities for a broad range of individual social and economic outcomes (e.g., education, health, income). In this regard, educational policies are often considered by economists and policy makers as the means *par excellence* to equalize opportunities among children with different social and family backgrounds and to promote intergenerational mobility.

This chapter examines whether increasing educational attainment allows equalizing opportunities for earnings acquisition. In line with modern theories of social justice, this chapter focuses on inequalities that stem from unfair sources of advantage, such as parental background, while taking a neutral stance with respect to other factors, such as effort. Following Roemer (1998) and subsequent literature, we use distributions of earnings conditional on circumstances of origin to measure opportunities, and use gaps in these distributions to assess how unequally opportunities are distributed.[1] Our analysis focuses on the French Berthoin reform, which increased the mandatory schooling age, and assesses the effect of this reform on EOP using the empirical criteria developed in Andreoli, Havnes, and Lefranc (2019). A policy widens accessibility to the secondary education when it provides additional years of schooling to those who would have otherwise dropped out of the schooling system. If dropout students, who are more likely to benefit from increasing high school access, are the ones raised in more disadvantaged families, we expect policies aimed at raising educational attainment to improve the earnings prospects of students experiencing less advantaged backgrounds, relative to other more advantaged groups.

A vast body of evidence has been collected about the effect of expanding educational attainment on adult earnings. Braga, Checchi, and Meschi (2011) provide a detailed account of the effects of competing reforms affecting duration of education at various stages. A large part of this literature makes use of reforms affecting the minimum schooling leaving age as an exogenous source of variation to identify the relevant causal effect of education on earnings. The implementation time of the reform may serve as an instrument to assess the direct effect of rising educational attainment on the earnings of compliers. Brunello, Fort, and Weber (2009) exploit cohort and country variation in the changes in minimum school leaving age to identify the effect of education on earnings, assuming homogeneity of the effect across countries. Nonetheless, the returns associated with an increase in the compulsory schooling age are found to be heterogeneous across countries. For example, there is no evidence of beneficial effects in terms of earnings from reforms implemented in Germany (Pischke & von Wachter, 2008), France (Grenet, 2013), the Netherlands (Oosterbeek & Webbink, 2007), and Poland (Liwinski, 2020). However, other papers document a positive effect, between 3% and 7%, associated with Swedish (Meghir & Palme, 2005) and British reforms (Devereux & Hart, 2010; Dolton & Sandi, 2017; Grenet, 2013). A positive effect (6–8%) is also found for the German reform by Cygan-Rehm (2018), who considers in her analysis a different sample and more institutional aspects than Pischke and von Wachter (2008). Lastly, larger effects ranging from 10% to 15% are found in Canada, the USA, and the UK by Oreopoulos (2006, 2007).

Studies of reforms increasing the minimum schooling leaving age have not been restricted to monetary outcomes, but also examine non-monetary dimensions, for example, risk aversion (Jung, 2015), trust (Yang, 2019), civic participation (Milligan, Moretti, & Oreopoulos, 2004), anti-immigration attitudes (Cavaille & Marshall, 2019), and health (Courtin et al., 2019; Kemptner, Juerges, & Reinhold, 2011, Lager & Torssander, 2012; Silles, 2009). Evidence about the intergenerational consequence of large educational reforms is limited, and mostly concerns the intergenerational transmission of human capital (Black, Devereux, & Salvanes, 2005; Pekkarinen, Uusitalo, & Kerr, 2009), of socioeconomic advantage (Meghir & Palme, 2005), and of health (Meghir, Palme, & Simeonova, 2018). Evidence on the distributive impact of education policies on earnings opportunities is also lacking (exceptions are Aakvik, Salvanes, & Vaage, 2010; Brunello et al., 2009), albeit own education is the most relevant mediating channel in the generation of unfair income inequalities (Palomino, Marrero, & Rodriguez, 2019).

The objective of this chapter is to evaluate the opportunity equalizing effect of rising attendance to secondary education on the group of students who would otherwise drop out of education too early. To do so, we first estimate the effects of attending additional years of secondary education on earnings for this selected group. We exploit the context of the *Loi Berthoin* as a quasi-natural experimental setting. This reform, enacted in 1959, raised minimum school living age from 14 to 16 years for cohorts born in 1953 and after and can be used as an instrument for educational attainment. We use estimated distributional effects of rising access to secondary education, heterogeneous both across groups defined by parental background and across levels of earnings, to simulate the effects of exogenously rising marginally educational attainment on those born in pre-reform cohorts who dropped out of education exactly at mandatory age. We use actual data and simulated counterfactual data to test the global implications of rising educational attainment on inequality of opportunity (IOP) for earnings acquisition among cohorts born in the 1950s in France.

To assess the impact of this reform, we rely on the opportunity equalization testing procedure developed by Andreoli et al. (2019). This procedure is robust in the sense that it draws on social consensus in assessing if the unfair earnings advantage enjoyed by any given circumstance group with respect to the rest of the distribution in a given context (for instance, before the *Loi Berthoin*) shrinks by effect of a given policy change (for instance, in the simulated distribution of earnings). The test makes use of distribution gap curves, obtained by differentiating earnings distributions conditional on circumstances in each policy regime, to test consensus. Applying this testing procedure to actual and counterfactual earnings distributions in France suggests that raising secondary education attainment has a weak yet positive impact on improving earnings opportunities. We also find weak evidence of equalization of opportunities across most socioeconomic groups: individuals from less advantaged family background tend to gain more out of the increase in minimum school leaving age than those from more advantaged backgrounds. These results are aligned with the patterns of effects discussed in Meghir and Palme (2005) for the Swedish education reform and Pekkarinen et al. (2009) for the Finnish reform, but the equalization potential of rising mandatory schooling seems significantly smaller than the effect recorded for education expansions taking place as early as kindergarten level (Andreoli et al., 2019).

The rest of the chapter is organized as follows. Section 2 describes the policy framework, defines the simulation procedure, and discusses the equalization criterion proposed by Andreoli et al. (2019). Section 3 presents the data and describes the empirical strategy. Section 4 discusses the results, and Section 5 concludes.

2. TESTING THE OPPORTUNITY EQUALIZING EFFECT OF EDUCATIONAL EXPANSION

The core objective of this chapter is to determine whether raising mandatory schooling age equalizes the distribution of opportunities between individuals with different circumstances, such as family background. In a nutshell, this entails assessing whether the distributions of individual outcomes, here earnings, come closer together as a result of the educational expansion.

This assessment requires combining three distinct ingredients. The first ingredient is an estimation of the causal effect of the reform on the distribution of earnings of treated individuals. The second ingredient is a simulation of the entire earnings distribution that results from the reform, reflecting both the effect of the reform on treated individuals and the distribution of treatment status in the population. The third element is a formal criterion to judge whether opportunity sets are equalized or not. We now discuss how each of these ingredients can be developed.

2.1. Estimating the Returns to Educational Expansion

Our first concern is to estimate the causal effect of the educational expansion on the affected population, that is, the earnings impact of a rise in mandatory age on the individuals who would otherwise have dropped sooner. This corresponds to the treatment on the treated impact and is addressed by resorting to an instrumental variable (IV) approach. Besides, given our distributional concern, we need to go beyond the computation of *average treatment effects* (Angrist & Krueger, 1991; Card, 1993) and allow for heterogeneous effects. This is addressed using quantile treatment effect (QTE) estimates.

To elaborate on our IV approach, let D be an indicator variable capturing the educational attainment, where $D = 0$ corresponds to individuals who drop school at the (pre-reform) mandatory schooling age and $D = 1$ indicates individuals with some post-compulsory education (spending at least 1 year in the educational system from age 15). The purpose of the reform is to increase the educational attainment of these individuals who leave school at the mandatory schooling age from $D = 0$ to $D = 1$.

The simple differences in earnings conditional on D do not measure the causal effect of an increase in educational attainment, since educational outcomes may reflect observable and unobservable earnings determinant, notably ability or family background characteristics. However, educational reforms shifting minimum school living age can be used as an instrument for education in this context, as used for instance in Harmon and Walker (1995). In this chapter, we exploit the quasi-natural experimental setting induced by the Berthoin reform (*Loi Berthoin*) in France as an instrument for educational attainment.

The Berthoin reform passed the French parliament vote in 1959. The reform raised the mandatory schooling age from 14 to 16 years for all children born after January 1, 1953.[2] Following Grenet (2013), the Berthoin reform can be used to define an instrument Z for the educational attainment of individuals constrained by the mandatory schooling age, with $Z = 1$ if born in or after 1953 and $Z = 0$ for earlier cohorts. The rationale for using the reform as an instrument in the estimation of the effect of increased education on earnings is that the distribution of potential earnings profiles is independent on shift in education induced by the reform (Card, 2001), at least for those cohorts born around the reform enforcement date. The instrument is independent of the type of unobserved heterogeneity that we would like to control for: ability, family background effects, "hard" and "soft" skills and parent investments are likely to be similarly distributed across adjacent cohorts, while these factors are likely to differ substantially for people self-selecting into different schooling attainment levels. A second condition for identification is that the IV has a causal impact on schooling attainment. This is granted by the universal coverage of the Berthoin reform.

Using this identification strategy, Grenet (2013) estimates the average returns to years of education, β for early school dropouts, at the discontinuity when the reform is introduced. Estimates of the average returns from education reveal that age left full-time schooling has a small and statistically insignificant impact on earnings, on average. We move beyond average treatment effects to estimate the distribution of treatment effects among individuals affected by the reform. We apply the approach developed by Abadie, Angrist, and Imbens (2002) that allows estimating the heterogeneous treatment effects $\beta(p)$ for any quantile of the earnings distribution. Our estimates are obtained from a sample of French male employee who has taken at most some years of secondary education without completing it.

Reducing the estimating sample to this group allows to identify the effect of a *marginal* change in education within an IV framework.

2.2. Simulating the Effect of Educational Expansion on the Earnings Distribution

The next step requires to assess the effect of the educational reform on the distribution of earnings in the population at large. Our concern is to assert what the earnings would have been in the pre-reform group, had it been affected by the reform. We let $F(y)$ denote the earnings distribution and $Q(p)$ denote the associated quantile function.

Comparing the earnings distribution between cohorts unaffected by the reform ($F(y|Z = 0)$ and cohorts affected ($F(y|Z = 1)$) fails to provide a consistent estimator of the effect of the reform on the distribution of earnings. In fact, only a small fraction of the population is directly impacted by the mandatory school age and its reform across cohorts. This corresponds to individuals who reported dropping out of school at the mandatory age ($D = 0$). At the same time, various shocks might have been at work between the pre- and post-reform cohorts that would have affected the overall earnings distribution, independently of the reform effect. Hence, the distribution $F(y|Z = 1)$ does not provide a relevant counterfactual for what the earnings distribution of group $Z = 0$ would have been if this group had experienced the increase in the mandatory schooling age.

To obtain a credible counterfactual distribution, we combine the observed earnings distribution in the pre-reform with estimates of the QTEs of the reform in the following way. We first assume that only individuals who dropped out of school at the mandatory age would have been affected by the reform. This amounts to rule out the possibility of spillover effects. For individuals who left school at the mandatory age, we assume that their educational attainment increases by the rise in the mandatory age (i.e., 2 years) and adjust their earnings by the estimated QTEs, conditional on their observed quantile. Formally, letting $\hat{Q}$ denote the counterfactual quantile function, we assume: $\hat{Q}(p|D = 0, Z = 0) = Q(p|D = 0, Z = 0) + \beta(p)$, where $\beta(p)$ is estimated following the identification strategy presented in Section 3.2. Inverting this quantile function provides the counterfactual distribution among the group $D = 0$, $\hat{F}(y|D = 0, Z = 0)$. The overall counterfactual in the whole population results from the mixing of the counterfactual distribution in both groups $D = 0$ and $D = 1$ and is given by:

$$\hat{F}(y \mid Z=0) = p_0\,\hat{F}(y \mid D=0, Z=0) + (1-p_0)F(y \mid D \neq 0, Z=0), \qquad (1)$$

with $p_0 = p(D = 0|Z = 0)$.

Using the same approach, we can also compute the counterfactual distributions for subgroups of the population defined by their circumstances c, $\hat{F}(y|Z = 0, c)$ that are required to assess equalization of opportunities, as we now explain.

2.3. Testing Equalization of Opportunity

EOP theories draw a distinction between fair inequality, arising from differences in individual *effort*, and unfair inequality arising from differences in individual *circumstances*, which comprises the determinants of success for which society deems the individual not to be responsible. In this setting, EOP requires that individuals face similar opportunities for outcome, regardless of their circumstances. This corresponds to the compensation principle (e.g., Fleurbaey, 2008; Roemer, 1998).

Following Lefranc, Pistolesi, and Trannoy (2009) (henceforth LPT), the earnings opportunities offered to individuals with circumstances c can be characterized by the conditional distribution function of earnings, given circumstances, $F(y|c)$, where c is a vector of observable circumstances. EOP is then said to prevail in the distribution of earnings if for any pair of possible circumstances (c, c') with $c \neq c'$ the following condition prevails: $F(y|c) = F(y|c')$ for any y.

This condition is very demanding and likely to be violated in many empirical contexts. However, situations where outcome distributions differ across types, that is, among individuals sharing similar circumstances, do not necessarily imply that one type is unambiguously advantaged over the others. LPT propose to single out cases where such an advantage unambiguously exists, by resorting to stochastic dominance tools. For instance, when $F(y|c)$ dominates $F(y|c')$ by first-order stochastic dominance, the expected utility of the opportunity set is larger for type c compared to type c'. When the distributions can be ranked according to second-order stochastic dominance, all risk-averse preferences will prefer the opportunity set offered to type c compared to type c'. These two situations obviously correspond to strong deviations from the EOP principle.

Combining these different notions of EOP and IOP allows to characterize a given distribution of outcomes from the point of view of EOP. The perspective of the present chapter departs from this concern, in the sense that instead of assessing a given situation, from the EOP perspective, we wish to assess the effect of a policy reform on the extent of IOP. This requires comparing two distributions: the distribution that prevails before the implementation of the reform and the counterfactual distribution that incorporates the effect of the reform. Accordingly, the distributions we want to compare can be indexed by a social state variable π which indicates whether a given policy is implemented ($\pi = 1$) or not ($\pi = 0$).[3] With these notations $F_\pi(y|c)$ defines the opportunities offered to individuals with type c in social state π. Our objective is thus to compare the distribution of these opportunity sets, across types, between the two social states and to assert whether implementing the reform (i.e., moving from social state $\pi = 0$ to social state $\pi = 1$) equalizes opportunity.

Our assessment of equalization of opportunity (EZOP) rests on the criterion developed by Andreoli et al. (2019) (henceforth AHL). The EZOP equalization criterion of AHL stems from the notion that opportunities are equalized if the advantage enjoyed by the advantaged types decrease when society moves from social state $\pi = 0$ to social state $\pi = 1$. To substantiate this notion, the EZOP criterion makes use of cardinal indices of advantage to measure the expected welfare of a lottery with cumulative distribution F. The notion of economic advantage offered by circumstance type c (with distribution F) relative to type c' (with distribution F') requires to compare the welfare index across circumstance: if welfare is larger for type c than for type c' then the value of the opportunity set is greater for type c than for type c'. Equalization occurs when there is agreement in a relevant set of evaluation functions that unfair disadvantage is reduced due to implementation of the policy.

AHL provide the minimal empirical conditions that need to be imposed on the set of distributions $F_0, F_0' F_1, F_1'$ in order to ensure that equalization is satisfied for all preferences in the class of Yaari's (1987) rank-dependent model (denoted $\mathcal{R}$) and within the Von Neumann expected utility model. More specifically, let $\Gamma(F, F', p) = F^{-1}(p) - F'^{-1}(p)$ denote the *cumulative distribution gap* between F

and F', then a necessary condition for EZOP is that the cumulative distribution gap should be smaller, in absolute value, at any percentile, under $\pi = 1$ than under $\pi = 0$. The graph of $\Gamma\left(F, F', p\right)$ against p is referred to as the *Gap curve*. As a consequence, if EZOP is satisfied on the set of preferences $\mathcal{R}$ then for all $p \in [0, 1]$, we have: $\left|\Gamma\left(F_1, F_1', p\right)\right| \leq \left|\Gamma\left(F_0, F_0', p\right)\right|$.

When there is agreement on the ranking of types, gap curve dominance provides a necessary and sufficient condition for EZOP. When gap curves cross, then agreement on the effect of a policy on IOP cannot be reached. In some cases, types cannot be ranked. AHL derive a condition allowing to endogenously identify a restricted set of preferences of $\mathcal{R}$ over which unanimity might be reached regarding EZOP. Unanimity within this class of functions can be tested by virtue of inverse stochastic dominance analysis (Andreoli, 2018; Muliere & Scarsini, 1989). More specifically, let $\Lambda_\pi^k(p)$ (respectively $\Lambda_\pi'^k(p)$) the integral of order $k-1$ of the inverse distribution functions of F_π (respectively F_π') evaluated at fractional rank p, and let $\Gamma\left(\Lambda_\pi^k, \Lambda_\pi'^k, p\right) = \Lambda_\pi^k(p) - \Lambda_\pi'^k(p)$, the cumulative distribution gap integrated at order $k-1$. Therefore, EZOP will be satisfied in the set $\mathcal{R}$ only if $\left|\Gamma\left(\Lambda_0^k, \Lambda_0'^k, p\right)\right| \geq \left|\Gamma\left(\Lambda_1^k, \Lambda_1'^k, p\right)\right|$ for any $p \in [0, 1]$.

This condition is also sufficient whenever for all π the distribution F_π dominates distribution F_π' for order-κ inverse stochastic dominance ($F_\pi \succ ISD_k F_\pi'$).

If unambiguous dominance of one type with respect to another cannot be established under both policy regimes, then gap curve dominance does not allow to conclude. In this context, lack of gap curve dominance can still be used to conclude against EZOP.

Distributional effects of the policy can also be used to assess *opportunity amelioration*: a simple test would require to assess if the distributional effects expand opportunity profiles and/or reduce earning differences at the extremes of the distribution. That is, for a given circumstance c, $\Gamma\left(\Lambda_1^k, \Lambda_0^k, p\right) \geq 0$ for every p and for some order κ.

In the rest of the chapter, we use the EZOP criterion and identification conditions to assess whether the educational expansion that resulted from the Berthoin reform allowed to equalize opportunities between individuals from different parental background. Social state $\pi = 0$ corresponds to the status quo pre-reform distribution; social state $\pi = 1$ corresponds to the counterfactual distribution where individuals dropping out of school at the mandatory age would be treated by the reform. The next sections provide the details of our estimation and testing procedures and present the results.

3. DATA AND ESTIMATION PROCEDURE

3.1. Data

We use data from the French Labor Force Survey (LFS, *Enquête Emploi* collected by INSEE) for the years 1990, 1993, 1996, 1999, 2004, 2006, 2008, and 2010. The sample is a rotating panel, therefore, we select only particular years of the survey to preserve exclusively the cross-sectional information.[4] The LFS is a large representative sample of the French population of age 15 and above.

There are on average 15,000 respondents per cohort in our pooled sample. Our sample is restricted to French male employees with full-time jobs in the private sector, born between 1950 and 1955, for a total of 26,421 observations, equally distributed across cohorts. The LFS database reports, for each individual, information on monthly earnings after taxes, which we use to measure earnings opportunities. Data also report information on the socioeconomic status (SES) of the respondent's father during childhood, measured at the end of compulsory education, and father's citizenship.

Based on these measures of parental background, we partition our sample into four types: *Circumstance 1* gathers individuals whose father is non-French, nearly 6% of the overall sample. The remaining population, with a French father, is split into three groups according to father's SES: *Circumstance 2* if father was a farmer or a manual worker; *Circumstance 3* if the father was an artisan or non-manual worker; *Circumstance 4* if the father was executive or professional. Data also report information on nationality, gender, age when leaving education, years of education, and highest degree obtained, job status (self-employed, employed, and in public sector) and information about family status.

We split observations into two groups defined by their exposure to the Berthoin reform: those born 1950–1952 (reform exposure indicator variable $Z = 0$) and those born after the implementation of the reform in cohorts 1953–1955 ($Z = 1$). As motivated by Grenet (2013), the Berthoin reform induced a significant increase of roughly 1 year in age left full-time schooling for cohorts born after 1953, with respect to older cohorts. This is implied by the raise in mandatory schooling age from 14 to 16 years. This result is also illustrated in Table 1, where differences in education and age of leaving school are significantly different between groups born before and after the Berthoin reform.

The *treatment* variable takes value $D = 1$ if spending at least 1 year of secondary education beyond mandatory schooling age (i.e., above age 16 (14) for post- (pre-)reform cohorts) but without completing it. The variable identifies a conservative group of those staying in secondary education beyond mandatory schooling age, without completing it. In our full sample, when excluding those completing secondary education or attaining some tertiary education, about 50% of the individuals spend at least some years in secondary education (i.e., receive the treatment, $D = 1$). The impact of the Berthoin reform on education emerges clearly from the table. For pre-reform cohorts ($Z = 0$), the proportion of individuals spending at least some time in secondary education was 43.2%, significantly rising by 10.8% in the post-reform cohorts.

In order to isolate the causal effects of attending some additional years in higher education, we focus on the group of respondents who did not complete secondary education. This group constitutes what we call the *trimmed sample*, as respondents are selected on the level of years of education they report. The effect identified on this sample is a lower bound estimate of the implications of the Berthoin's reform, net of spillover effects of the reform on own education, insofar as some people treated with the reform may have pursued their studies beyond secondary education, while they would not have done so without the reform (by lowering costs of human capital accumulation but rising opportunity costs of

Table 1. Descriptive Statistics: Covariates Before and After the Reform (Z).

	After Reform ($Z = 1$)		Before Reform ($Z = 0$)	
	1		2	
Individual characteristics:				
Wage, monthly, in Euro	1,676.578	[2,876.4]	1,737.303	[3,246.8]
Prizes	0.511	[0.5]	0.525	[0.5]
Weekly working hours	40.120	[9.4]	40.338	[9.7]
Self-employed	0.022	[0.1]	0.026	[0.2]
Employed in the public sector	0.244	[0.4]	0.251	[0.4]
Education, years	12.116	[3.3]	11.903	[3.6]
Age, in years (above 15)	43.984	[6.5]	46.165	[6.0]
Marriage status	0.758	[0.4]	0.790	[0.4]
Number of children below 18	1.034	[1.1]	0.907	[1.1]
Socioeconomic conditions of the father:				
Circumstance 1	–	–	–	–
Father without French nationality	0.066	[0.2]	0.060	[0.2]
Circumstance 2	0.539	[0.5]	0.533	[0.5]
Farmers	0.113	[0.3]	0.119	[0.3]
Manual worker	0.456	[0.5]	0.443	[0.5]
Circumstance 3	0.220	[0.4]	0.242	[0.4]
Artisans	0.101	[0.3]	0.109	[0.3]
Non-manual workers	0.140	[0.3]	0.151	[0.4]
Circumstance 4	0.174	[0.4]	0.165	[0.4]
H-grade prof.	0.075	[0.3]	0.075	[0.3]
L-grade prof.	0.115	[0.3]	0.104	[0.3]
Age of leaving education	18.116	[3.3]	17.903	[3.6]
$(cob - 1953)^2$	1.667	[1.7]	4.559	[3.3]
$(cob - 1953)^3$	3.002	[3.6]	−11.658	[10.9]
$(cob - 1953)^4$	5.672	[7.3]	31.634	[34.5]
Target group for simulation	–	–	0.268	[0.4]
Stay in school beyond mandatory age ($D = 1$)	0.540	[0.5]	0.432	[0.5]
Δ *treatment*		0.108***	(0.006)	
Proportion in trimmed sample	0.672	[0.5]	0.676	[0.5]
Of which stay beyond mandatory age ($D = 1$)	0.804	[0.5]	0.638	[0.5]
Of which exit at mandatory age ($D = 0$)	0.196	[0.5]	0.362	[0.5]

Source: Labor Force Survey 1990, 1993, 1996, 1999, 2004, 2006, 2008, and 2010.
Notes: Sample reduced to French male earners where circumstances have been recorded, cohorts 1950–1955. Standard deviations in brackets. Differences in covariates between before and after the reform are not significant at 5%. Variable *cob* identifies the cohort of birth. The group of longer staying is given by those spending some year in the secondary school beyond mandatory schooling age (14 years old for pre-reform cohorts and 16 years old for post-reform cohorts). Trimmed sample size refers to the sub-sample of those who attended some secondary education but without completing it. The target group, used to simulate policy intervention, refers to students born before the reform, dropped out the educational system just at (or before) the mandatory schooling age pre-reform (14 years old). *** indicates significance at 1%.

foregoing higher wages). Nonetheless, the effects are appropriate for our simulation study, insofar as it can be used to simulate earnings of the units who would have dropped out early from the system in the absence of the reform. Over a using sample of 26,421 units, the trimmed sample we employ in estimation of the relevant effects consists of all individuals who have at most attended some secondary education but do not hold a secondary education diploma, and amounts to 17,779 observations, that is, about 67% of the original sample in pre- and post-reform cohorts. Within this sample, the Berthoin reform raises attendance to some secondary education (i.e., the share of $D = 1$ group in the trimmed sample) from 63.8% to 80%. We can thus define a *control* group in the trimmed sub-sample, corresponding to those for which the treatment is $D = 0$. Estimates of the difference in earnings between the groups $D = 1$ and $D = 0$ within the trimmed sub-sample are not causal. Our identification strategy retains instead that differences in earnings across treatment and control that are related to the implementation of the *Loi Berthoin* identify the causal effect of interest. Table 2 illustrates the composition of the sample across the different subgroups identified by D and Z, and it depicts the distribution of incomes by quantile relative to each of the groups. We use a conditional model to estimate such effects.

To simulate the effect of the reform, we focus on a *target* group of individuals that we treat with additional years of education. The target group corresponds to those individuals born before the reform ($Z = 0$), that exit formal education system at or before their 14th year of life, that is, the pre-reform mandatory school age (hence, $D = 0$). As of Table 1, the target group is 26.8% of the population in the pre-reform cohorts. In the simulation exercise, we assume that this group is treated by the Berthoin reform and we simulate the resulting increase their earnings, based on estimates of the effect of the reform. Individuals outside the

Table 2. Descriptive Statistics: Earnings by Reform Exposure (Z) and Treatment (D).

Earnings (Monthly)	Overall	After Reform ($Z = 1$)		Before Reform ($Z = 0$)	
	(1)	$D = 1$ (2)	$D = 0$ (3)	$D = 1$ (4)	$D = 0$ (5)
Q5%	426.9	387.2	416.0	450.0	472.6
Q10%	762.2	762.2	686.0	800.4	731.8
Q25%	985.6	990.9	911.1	1,067.1	914.7
Q50%	1,219.6	1,250.0	1,092.8	1,311.1	1,112.9
Q75%	1,550.0	1,585.0	1,402.5	1,676.9	1,402.5
Q90%	2,058.1	2,000.0	2,200.0	2,134.3	1,900.0
Q95%	2,500.0	2,400.0	3,000.0	2,500.0	2,591.6
Mean	1,395.0	1,383.9	1,436.6	1,458.5	1,285.4
	[2,160.3]	[1,977.2]	[3,771.0]	[2,185.2]	[886.9]
Trimmed sample size	17,779	7,357	1,785	5,513	3,124

Source: Labor Force Survey 1990, 1993, 1996, 1999, 2004, 2006, 2008, and 2010.
Notes: Trimmed sample reduced to French male earners where circumstances have been recorded, cohorts 1950–1955. Trimmed sample size refers to the sub-sample of those who attended some secondary education but without completing it. Income quantiles are measured in Euro. Standard deviations reported between brackets.

target group are not treated in the simulation. We then combine the target and non-target groups to recover the simulated changes in the overall earnings distribution, based on the whole sample of 26,421 individuals, that would result from this selective treatment and assess how it would change the extent of IOP.

3.2. Identification and Estimation

Distributional effects of additional years of secondary education on earnings can be assessed using quantile regression methods applied to the trimmed sample. Let $y(p)$ be the quantile of the earnings distribution, we assume linearity of the treatment effect on the earnings distribution, implying:

$$y(p) = y(c, D, X, \varepsilon) = \sum_c \alpha_c(p) + \beta(p)D + X\gamma(p) + \varepsilon, \tag{2}$$

where D measures educational attainment and is an indicator variable equal to 0 for individuals leaving school at the (pre-reform) compulsory age and is equal to 1 for individuals attending some post-compulsory secondary education without graduating. The QTE of interest, $\beta(p)$, and the marginal effects of the observables, $\gamma(p)$, are calculated at percentile p. In our specification, we use birth cohort and year fixed effects and cohort trends as controls. We assume that circumstances of origin have an intercept effect on the earnings quantile function, while treatment effects do not vary across circumstance types. If the treatment were assigned randomly conditional on observables (implying $\varepsilon \perp (D, X)$) then QTE could be estimated by comparing quantiles of the conditional earnings distributions by treatment status: $\beta_c(p) = Q_{y|D=1,X,c}(p) - Q_{y|D=0,X,c}(p)$.

Unobservable ability affects both the decision to accumulate further education, as well as the earnings distribution, making the QTE estimator biased due to endogeneity of the treatment D. IV methods provide powerful tools for identifying causal estimates of QTE under endogeneity. We consider the Berthoin reform indicator Z as an instrument, which affects the potential treatment status of an observation. Our identifying assumptions are that of linearity and the fact that given X (the observable covariates), potential outcomes, and potential treatment status (i.e., the counterfactual incomes and education level one unit would achieve if $Z = 1$ or if $Z = 0$) are jointly independent of the Berthoin reform assignment. The credibility of the assumption rests on the fact that we compare individuals born in very close cohorts who differ only from assignment to the Berthoin reform. In this situation, variations in the IV identify the causal effect of the treatment status on the outcome quantiles, while potential outcomes should not be directly affected by the IV. Abadie et al. (2002) showed that this assumption implies that in the population of *compliers* (those whose potential treatment assignment status changes by effect of changes in the IV), comparisons by D conditional on X have a causal interpretation.

Using the quantile regression procedure outlined in Abadie et al. (2002) we identify the QTE $\beta(p)$ on the groups of compliers:

$$Q_{Y|X,D_1>D_0,c}(p) = \sum_c \alpha_c(p) + \beta(p)D + X\gamma(p). \tag{3}$$

We estimate linear model (2) on the trimmed sample at a finite number of points corresponding to the vingtiles of the earnings distribution in the trimmed sample. Our preferred specification reports treatment effects calculated on the entire trimmed sample, while we also report estimates conditional on the circumstance group as a robustness check.

We then select significant estimated quantiles, $Q\hat{T}E(p)$, to treat the baseline distribution of earnings of the target group (denote it $\tau = 1$) in the using sample, after purging earnings from year, cohort fixed effects, and trends. Let the detrended distribution of earnings opportunities be $\hat{F}_{\pi=0}(y \mid c, \tau = 1) = \hat{F}_{D=0}(y \mid c, \tau = 1)$, the *simulated counterfactual* distributions are obtained by setting $\hat{F}^{-1}_{\pi=1}(p|c, \tau = 1) = \hat{F}^{-1}_{D=0}(p|c, \tau = 1) + \beta(p)$ for any p.

The underlying assumption is that all units in the target group would receive the QTE identified for the group of compliers. Effects are then rescaled to the overall population to determine the counterfactual distribution of earnings for the treatment group. This leads to distributions $\hat{F}_{\pi=0}(y \mid c)$ and $\hat{F}_{\pi=1}(y \mid c)$.

EZOP tests are performed on these distributions. Following AHL, the EZOP test involves estimating vectors of gap curves coordinates based on empirical distributions at a fixed number of quantiles (5%–95%). Null hypothesis setting equality or inequality constraints on these vectors allow to estimate equality or dominance in gap curves. To assess equalization, we first assess inverse stochastic equality and dominance null hypothesis across distributions for any degree $k = 1$ up to 5, and determine the implicit ranking of circumstances. Denote $\kappa(c, c', \pi)$ the minimum degree of dominance at which two distributions $\hat{F}_{\pi}(y \mid c)$ and $\hat{F}_{\pi}(y|c')$ can be ranked. The gap dominance test can be performed over the class $\kappa(c, c', \pi = 1)$. We test equality and dominance in gap curves $\Gamma\left(\Lambda_0^{k(c,c',1)}, \Lambda_0'^{\,k(c,c',1)}, p\right)$ and $\Gamma\left(\Lambda_1^{k(c,c',1)}, \Lambda_1'^{\,k(c,c',1)}, p\right)$ for any $p \in \{5\%, 10\%, \ldots, 95\%\}$, and report the outcome of such test. If the ranking of earnings distributions conditional on circumstances is stable in control and simulation settings, then differences in gap curves defined on the basis of such ranking provide a necessary and sufficient empirical condition to conclude on robust opportunity equalization.

Opportunity amelioration refers instead to the direct comparison of earnings distributions across policy regimes. Amelioration is verified when, for every circumstance group, the corresponding simulated distributions of earnings dominate at a certain ISD order the observed distributions.

Inverse stochastic dominance at order $k = 1, 2$ is estimated as in Beach and Davidson (1983), while for $k \geq 3$ tests are constructed by following Andreoli (2018). Asymptotic test statistics for gap curve dominance tests and opportunity amelioration tests are based on bootstrapping methods as in AHL.

4. RESULTS

Table 2 shows the earnings quantiles of treated and non-treated observed individuals within the trimmed sample born before and after the implementation of the Berthoin reform. The differences between columns (2) versus (3) and (4) versus

(5) are sizeable. However, these differences are remarkably similar across reform assignment, indicating that QTE of the policy are low in size. Table 3 reports QTE estimates of $\beta(p)$ from model (2) and their standard error at selected quantiles for the overall population within the trimmed sample (model (1)), as well as for the sub-samples defined by background circumstances (models (2)–(5)). Results suggest that gains associated with the Berthoin's reform are concentrated in the middle of the earning distribution, as it is not possible to identify a significant effect of the educational indicator for population percentiles that range out of the 30% to the 75% quantiles intervals. However, this effect could be interpreted as a lower bound estimate of the reform's effect. By focusing only on individuals within the trimmed sample, that is, those who attended some secondary education without finishing it, we are sure to rule out possible snowball or spillover effects of the reform associated with the choice of tertiary education and to focus on the effects of accumulating only few years of education after mandatory schooling age.

Table 3. Quantile Treatment Effects – Trimmed Sample, IV Estimator.

Independent Variable: Earnings	Overall	Conditional			
		Circ. 1	Circ. 2	Circ. 3	Circ. 4
	(1)	(2)	(3)	(4)	(5)
Treatment Q5%	49.5	−0.8	48.7	24.7	53.7
	(76.9)	(248.9)	(86.5)	(133.6)	(254.3)
Treatment Q10%	57.5	−47.2	53.4	76.2	1.5
	(61.7)	(245.3)	(63.4)	(139.0)	(243.1)
Treatment Q25%	90.6	−93.8	76.2	104.2	135.1
	(59.1)	(674.5)	(59.0)	(147.8)	(437.1)
Treatment Q50%	142.3**	45.7	126.4**	157.2	7.7
	(58.8)	(720.8)	(62.4)	(162.9)	(333.0)
Treatment Q75%	167.7*	−187.3	179.9	155.8*	−152.4
	(88.3)	(697.5)	(100.3)	(188.1)	(542.8)
Treatment Q90%	167.7	−759.6	228.7	228.7	−457.3
	(165.5)	(1,978.8)	(174.3)	(321.4)	(1,035.8)
Treatment Q95%	157.4	−1,021.4	167.7	213.4	−643.8
	(306.9)	(1,409.6)	(278.5)	(640.9)	(1,145.9)
Controls (reported at Q50%)					
$(cob - 1953)^2$	11.0 (29.7)	−48.0 (319.3)	4.8 (30.2)	29.4 (81.2)	−18.0 (243.0)
$(cob - 1953)^4$	−0.8 (2.8)	2.2 (29.2)	−0.4 (2.8)	−2.7 (8.1)	0.2 (25.3)
Circumstance 1	−0.0 (179.0)				
Circumstance 3	52.6 (45.0)				
Circumstance 4	116.9 (144.4)				
Survey year (FE)	Yes	Yes	Yes	Yes	Yes
Trimmed sample size	17,779	981	11,351	3,720	1,727

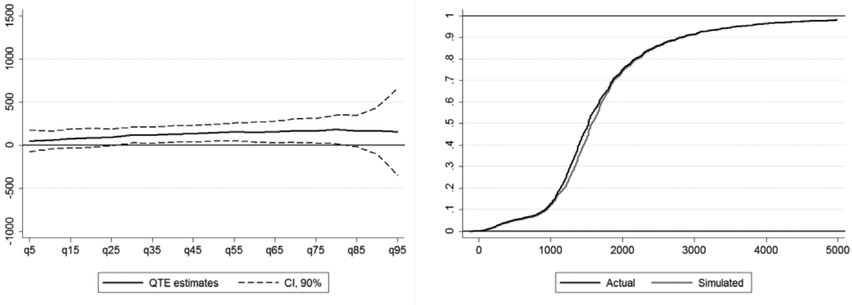
*$p < .10$, **$p < .05$, ***$p < .01$ (one-tailed).
Source: Labor Force Survey 1990, 1993, 1996, 1999, 2004, 2006, 2008, and 2010.
Notes: Trimmed sample reduced to French male earners where circumstances have been recorded, cohorts 1950–1955. Trimmed sample size refers to the sub-sample of those who attended some secondary education but without completing it. The dependent variable measures earnings in 1999, once the year effect has been eliminated. Robust standard errors are reported in parenthesis.

Furthermore, looking at the conditional earning distribution, the QTE estimates remain significant exclusively for the groups Circumstance 2 (farmer or manual worker father) and Circumstance 3 (artisan or non-manual worker father).

The full impact of the access to secondary education on the earning distribution is illustrated in Fig. 1(a). Overall, the reform has a positive, but relatively modest, effect on the entire earning distribution, which tends to increase as we move up the earning distribution. Effects on yearly earnings are about 150 Euro, that is, about 1.4% of median income of the pre-reform comparison group (1,112.9 Euro).

The estimated marginal effects in Fig. 1(a) are used to simulate the implications of a policy change on the earnings of the target group, using the position occupied in that group to assign the corresponding QTE to the individual income observation to all people on the same 5% percentile range. The target group includes students in the age interval 11–14 year olds who exit the formal education system in the pre-reform cohorts exactly at age 14. This group consists of 26.8% of the 26,421 individual observations of the using sample. Fig. 2(a) illustrates the density of the target group, as well as the density of other educational groups, across *population* income quantiles. Most of the individuals targeted by the simulation are concentrated at the bottom of the distributions and receive a zero treatment effect from an educational expansion. The share of target group decreases substantially over the distribution of earnings quantiles.

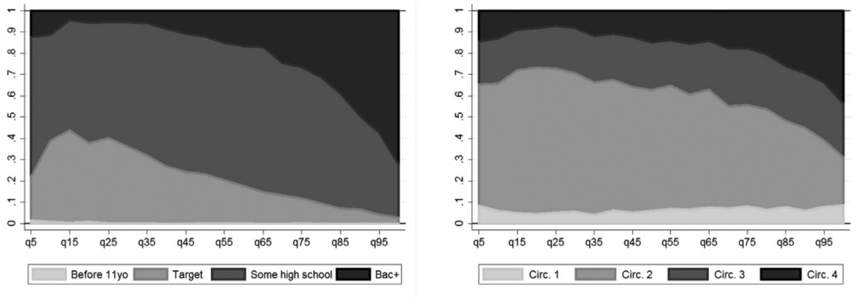
QTE estimates are rescaled according to the target group composition at each quantile of the main sample to simulate the overall effect of a policy change. Fig. 1(b) reports the actual distribution of earnings from pre-reform control group ($\pi = 0$) from the using sample and the simulated distribution of earnings ($\pi = 1$).



(a) QTE, by 5% quantiles (b) *cdf*s before/after treatment

Fig. 1. QTE of the Impact of Access to Secondary Education on Earnings.

Notes: Estimates based on the trimmed sample of cohorts 1950–1955 of French male earners. In panel (a), QTEs are computed at 5% income intervals (IV estimator), the CI at 90% is computed with robust standard errors. Controls: cohort trends, year of survey, a quartic polynomial of the gap between the year 1953 and last year spent in school, and circumstance dummies. Empirical *cdf*s in panel (b) are obtained for detrended earnings data (actual) and by providing policy treatment by quantile of earnings for the *target students* (simulated).



(a) By educational achievement (b) By circumstances

Fig. 2. Composition of the Population Occupying Each of the 5% Tranches of Earnings Quantiles, Where Groups are Defined by Educational Achievement (a) and Circumstances (b).

Notes: Scores have been calculated from a multinomial logit model assigning to each 5% share of population, arranged by increasing income, the probability of belonging to each of the groups (these probabilities add up to 1 for every 5% revenue tranche).

Overall, there is evidence that the simulated effects induce an improvement in the distribution of earnings, which is concentrated mostly around the median earnings level. The distribution of the simulated effects may vary across circumstance groups for two reasons: first, because circumstances groups are unevenly distributed across deciles of the pre-reform distribution, as clarified by Fig. 2(b); second, because the target group is not equally represented across all circumstance groups.

Table 4 reports selected quantiles of the earnings distributions for $\pi = 0$ and simulated earnings for $\pi = 1$ distributions in the using sample, in the target group, and then breaking down observations by parental background. Differences in average earnings across actual and counterfactual distributions are very narrow, reflecting the small and selected effect of the treatment of interest. Nonetheless, the simulated distributions display lower inequality (as measured by the Gini index) than the actual distribution, highlighting that most of the simulated effects of exogenously rising secondary school attainment have a redistributive effect.

We use these data, the observed/actual data, and the simulated counterpart, to draw the relevant distributions under analysis. Empirical earnings *cdf* in the actual setting are estimated from the using sample data to obtain conditional distribution under $\pi = 0$ and from the simulated sample to obtain conditional distributions under $\pi = 1$. These distributions are reported in Fig. 3. We use the implied conditional quantile functions to draw gap curves and differences in gap curves. We use the underlying samples to bootstrap quantile functions 200 times and obtain variance-covariance matrices for these quantiles. We make use of these curves estimates as well as the associated covariance matrices to run joint tests of opportunity equalization, following Andreoli and Fusco (2019).

Fig. 4 presents the differences (in gap curves) for each pair of circumstances. These differences are obtained by differentiating for each pair of circumstances

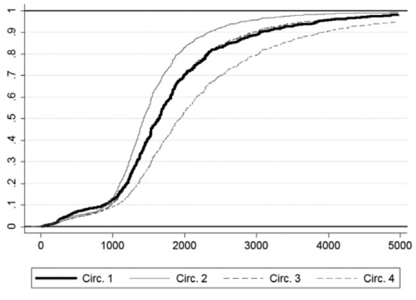
Table 4. Earnings Distributions by Cohorts for Selected Quantiles, Actual (Before Policy Implementation) versus Simulated Data.

Quantiles	Overall	Target	Circ. 1	Circ. 2	Circ. 3	Circ. 4
	(1)	(2)	(3)	(4)	(5)	(6)
Actual data (before policy implementation)						
Q5%	499.1	618.2	394.0	474.8	606.3	569.1
Q10%	944.6	883.6	883.6	914.7	975.1	1,066.5
Q25%	1,226.7	1,097.0	1,269.4	1,173.3	1,275.2	1,448.3
Q50%	1,534.3	1,305.6	1,638.4	1,427.6	1,620.6	1,934.4
Q75%	2,011.7	1,529.1	2,164.2	1,808.7	2,134.3	2,748.7
Q90%	2,825.0	1,840.1	3,049.0	2,316.6	2,935.4	3,876.1
Q95%	3,535.4	2,147.0	3,841.1	2,779.7	3,665.4	4,976.7
Mean	1,825.7	1,378.5	1,940.3	1,597.6	1,875.4	2,431.4
	[3,026.9]	[2,102.1]	[3,868.8]	[2,378.3]	[2,270.0]	[4,785.9]
Gini	0.303	0.204	0.330	0.256	0.287	0.352
	(0.006)	(0.015)	(0.022)	(0.008)	(0.010)	(0.018)
Sample size	26,421	5,585	1,682	14,134	6,103	4,502
Simulated policy implementation						
Q5%	499.1	618.2	394.0	474.8	606.3	569.1
Q10%	944.6	883.6	883.6	914.7	975.1	1,066.5
Q25%	1,264.3	1,097.0	1,290.2	1,219.6	1,310.5	1,473.3
Q50%	1,574.9	1,447.9	1,656.3	1,493.4	1,656.3	1,934.4
Q75%	2,011.7	1,676.1	2,164.2	1,808.7	2,134.3	2,748.7
Q90%	2,825.0	1,840.1	3,049.0	2,316.6	2,935.4	3,876.1
Q95%	3,535.4	2,147.0	3,841.1	2,779.7	3,665.4	4,976.7
Mean	1,842.5	1,458.3	1,950.0	1,621.0	1,888.0	2,436.3
	[3,024.7]	[2,102.7]	[3,867.5]	[2,376.5]	[2,267.6]	[4,784.9]
Gini	0.299	0.197	0.326	0.251	0.284	0.351
	(0.006)	(0.014)	(0.022)	(0.008)	(0.010)	(0.018)
Sample size	26,421	5,585	1,682	14,134	6,103	4,502

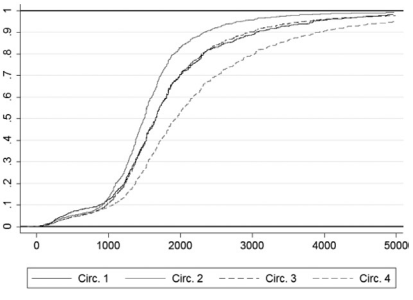
Source: Estimates from Labor Force Survey 1990, 1993, 1996, 1999, 2004, 2006, 2008, and 2010.
Notes: Earnings quantiles for earnings distribution detrended by the age effect. Sample size refers to the overall sample of French male earners where circumstances have been recorded, cohorts 1950–1955. Earnings after policy implementation are obtained by assigning quantile treatment effects estimated by model (1) in Table 3 to the target group. Standard deviations reported in brackets. Gini index is reported for each subgroup's earnings distribution. Standard errors in parentheses are calculated by bootstrapping 100 replications of the Gini index.

the differences in earnings (D, the gap curves), in the *Generalized Lorenz* (GL) curves (D2, the integrals of the gap curves) and in the integrals of the GL (D3, the double integrals of the gap curves), computed at each percentile of the actual (for $\pi = 0$) and simulated (for $\pi = 1$) earning distribution.

Figs. A1 (for $\pi = 0$) and A2 (for $\pi = 1$) in the Appendix provide a graphical account of the extent of disadvantage in the actual and simulated data respectively. The patterns of the differences in the GL curve ordinates and their integrals are positive along the earning percentiles domain for all pairs of circumstance in both social states, except for the comparison between Circumstance 1 and Circumstance 2. For this pair of circumstances, indeed, the



(a) Actual data ($\pi = 0$)



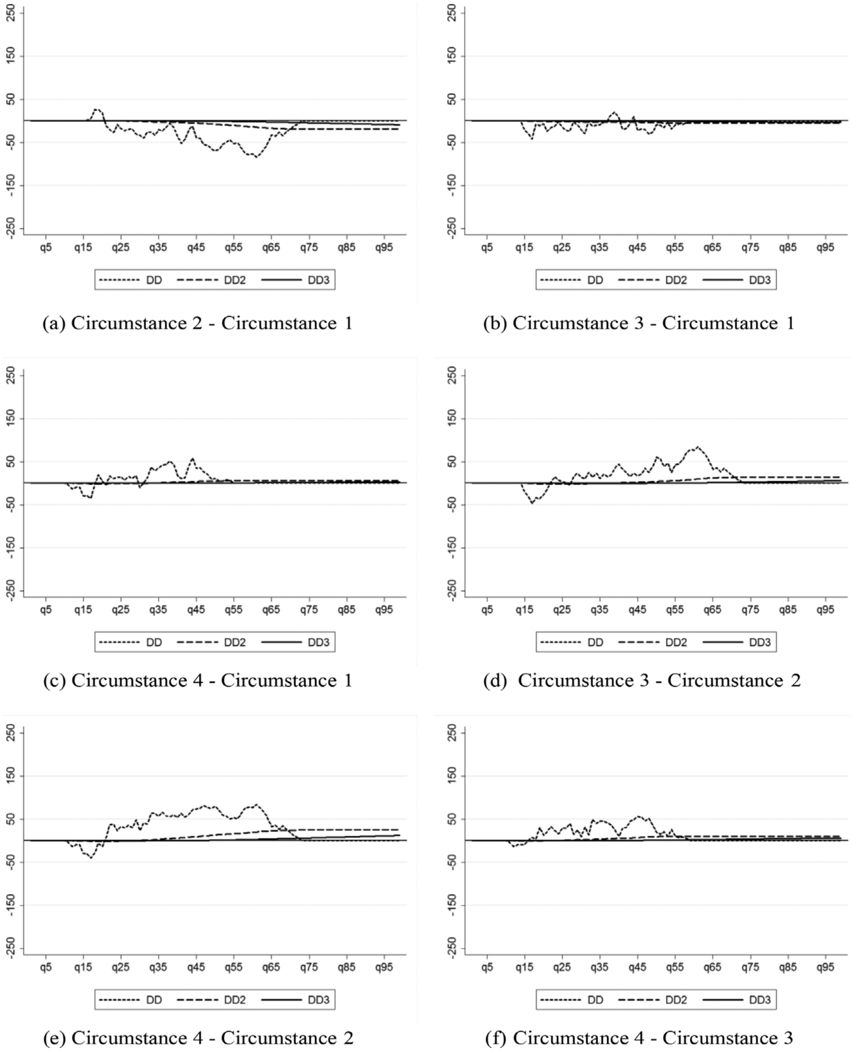
(b) Simulated data ($\pi = 1$)

Fig. 3. Earning Distribution Before and After Simulating the Expansion of the Secondary Education System.

policy implementation reverts the direction of the advantage as measured by the differences in the GL curve ordinates and their integrals. Therefore it is possible to rank these circumstances only according to ISD at order $\kappa = 3$. Graphical evidence is that almost all pairs of circumstances are clearly ranked by the extent of advantage they display under both policy regimes, and the ranking of advantage is stable across policy regimes, except for comparisons involving the least advantaged group. The differences of these curves, which serve at identifying the gap dominance, are little conclusive as the curves fluctuate around the zero line (Fig. 4), thus providing inconclusive evidence about opportunity equalization across all circumstance pairs. We now use joint tests to determine the extent to which some of these intersections in gap curves can be ruled out in favor of a weak form of gap curves dominance. The outcome of these joint tests is presented in Table 5.

In columns (1) and (2) of Table 5, we report, for each policy and for each pair of circumstances, the direction of dominance and the minimal order at which ISD cannot be rejected by the data at a 5% confidence level. For instance, one has to read the first dominance relation in (1) as $Circ.1 \succ ISD_1\, Circ.1$ $Circ.2$ (but not the inverse) under $\pi = 0$. The joint tests for EZOP confirm that the direction of the advantage as measured by ISD is unaffected by policy implementation for all pairs of circumstances, except for Circumstances 1 and Circumstances 2. Furthermore, the comparison between Circumstance 1 and Circumstance 3 cannot be ranked according to ISD1. For these two circumstances, indeed, it is necessary to test dominance up to the order three, which is verified both before and after policy intervention. It is nevertheless possible to rank unambiguously the Circumstances 2, 3, and 4 (French father, different socioeconomic classes) according to ISD1 both before and after policy simulation. This result shows that the policy has no impact in reducing agreement over the direction of the disadvantage, nor on changing the direction of disadvantage itself.

Results for the distance comparison and for the gap curve dominance tests are reported respectively in columns (3) and (4) of Table 5.[5] Gap curve dominance



(a) Circumstance 2 - Circumstance 1

(b) Circumstance 3 - Circumstance 1

(c) Circumstance 4 - Circumstance 1

(d) Circumstance 3 - Circumstance 2

(e) Circumstance 4 - Circumstance 2

(f) Circumstance 4 - Circumstance 3

Fig. 4. Difference in Differences in Quantile Functions (D), *GL* Curves (D2) and Integrals of the *GL* Curves (D3) Computed at each Percentile of the Actual and Simulated Earnings Distributions.

Notes: Values on the horizontal axis refer to percentiles of the earnings distribution. Values on the vertical axes express the difference across policies in the differences between earning gaps, *GL* curves gaps, and gaps in the integrals of *GL* curves associated with pairs of circumstances, in Euros. Earnings differences in differences trimmed at 250 and −250 Euro.

Table 5. Equalization of Opportunity Test: Ordinal and Distance Criteria for High School Expansion Policies.

Circ. c vs Circ. c'	Actual ($\pi = 0$)	Simulated ($\pi = 1$)	Opportunity Equalization Test	
			$\Delta_W(F_0^c, F_0^{c'}) - \Delta_W(F_1^c, F_1^{c'})$	Gap Dominance Order k (Tested Model)*
	(1)	(2)	(3)	(4)
Circ. 2 vs Circ. 1	$\prec_{ISD3}$	$\succ_{ISD3}$	$\geq 0\ \forall W \in \mathcal{R}^3$	≥ 0 for $k = 3$ (12–21)
Circ. 3 vs Circ. 1	$\succ_{ISD3}$	$\succ_{ISD3}$	$= 0\ \forall W \in \mathcal{R}^3$	$= 0$ for $k = 1$ (31–31)
Circ. 4 vs Circ. 1	$\succ_{ISD1}$	$\succ_{ISD1}$	$= 0\ \forall W \in \mathcal{R}^1$	$= 0$ for $k = 1$ (41–41)
Circ. 3 vs Circ. 2	$\succ_{ISD1}$	$\succ_{ISD1}$	$\geq 0\ \forall W \in \mathcal{R}^1$	> 0 for $k = 1$ (32–32)
Circ. 4 vs Circ. 2	$\succ_{ISD1}$	$\succ_{ISD1}$	$\geq 0\ \forall W \in \mathcal{R}^1$	> 0 for $k = 1$ (42–42)
Circ. 4 vs Circ. 3	$\succ_{ISD1}$	$\succ_{ISD1}$	$= 0\ \forall W \in \mathcal{R}^1$	$= 0$ for $k = 1$ (43–43)
		Opportunity Amelioration Test		
		Δ Policy impact		
Overall		After ($\pi = 1$) $\succ_{ISD1}$	Before ($\pi = 0$)	
Circ. 1: Non-French father		After ($\pi = 1$) $\sim_{ISD1}$	Before ($\pi = 0$)	
Circ. 2: Farmer or manual worker		After ($\pi = 1$) $\succ_{ISD1}$	Before ($\pi = 0$)	
Circ. 3: Artisan or non-manual worker		After ($\pi = 1$) $\sim_{ISD1}$	Before ($\pi = 0$)	
Circ. 4: Executive or professional		After ($\pi = 1$) $\succ_{ISD1}$	Before ($\pi = 0$)	

Source: Estimates from Labor Force Survey 1990, 1993, 1996, 1999, 2004, 2006, 2008, and 2010.
Notes: Earnings distribution corrected by the age effect. Sample size refers to the overall sample of French male earners where circumstances have been recorded, cohorts 1950–1955. EZOP tested at 5% significance level on a selected sample of twenty quantiles. Both inverse stochastic equality and dominance null hypothesis have been separately tested for any degree $k = 1$ up to 5. Only the minimal degree of dominance $\kappa(c, c', \pi)$ is reported. The notation $c \succ \boldsymbol{ISD}_k\ c'$ as in the Table, means that the earnings distribution of circumstance c ISD at order κ the earnings distribution of circumstance c'. The distance test is defined over the class $\kappa(c, c', \pi = 1)$. Direction of the Gap curves dominance is reported, along with information on the direction of distance for the model verifying gap dominance. ISDk for $k = 1, 2$ is estimated as in Beach and Davidson (1983), while for $k \geq 3$ tests are constructed by following Andreoli (2018).
*For model ($ij - kh$) we tested the gaps curve of circumstances i vs j in $\pi = 0$ minus the gaps curve of k vs h in $\pi = 1$, exclusively for configurations $k = i$ and $h = j$ or $k = j$ and $h = i$.

relations are tested at 5% significance level. The tested model, reported in brackets, gives the order of differentiation of circumstances' earnings distributions under each policy regime, which allows to conclude in favor of dominance in gap curves. For instance, the model associated to circumstances Circumstance 2 and Circumstance 1 is (12–21), which means that to find dominance in Gap curves at order three it is necessary to take the difference of the integral of GL of Circumstance 1 minus the integral of GL of Circumstance 2 under policy $\pi = 0$ and the inverse under policy $\pi = 1$. Otherwise, alternative models for gap dominance always reject the null hypothesis of equality or dominance even at higher orders of inverse stochastic dominance. This specific ranking is consistent with the fact that gap curves in panel (a) of Fig. 4 always lie below the horizontal line.

Altogether, there is agreement on the order of ranking in each social state, and there is agreement on the fact that the ranking switches across social states, implying that dominance in gap curves identifies agreement on the reduction of unfair disadvantage. For the other pairs of circumstances, we find evidence that the gap curve dominance at the first order cannot be rejected at the 5% confidence level for the pairs {3, 2} and {4, 2}. The gap curve dominance tests are coherent with the direction of advantage measured by ISD under both policy regimes, although for many comparisons the change in distance is statistically zero (i.e., the gap curve coincides with the zero line). This result is coherent with the fact that the simulated policy has no sizeable impact on the earnings distribution of Circumstances 1 and 3. The distance between Circumstance 2 and the Circumstances 3 and 4 is reduced by effect of policy simulation, while the distance between Circumstances 1 and 4 remains unaffected. This result is consistent with the fact that an expansion of the secondary education system provides benefits for students coming from more disadvantaged backgrounds. The policy does not have a statistical impact on the distribution associated with Circumstance 1.

We conclude that under the assumption of the rank-dependent model for preferences, the ex-ante EZOP criterion is validated by the data, although there is only weak evidence of consensus that the simulated increase in education attainment reduces unfair earnings gaps in France. To quantify the opportunity equalizing effect of the reform Berthoin we compute the Gini opportunity index ($GO(\pi)$) of Lefranc, Pistolesi, and Trannoy (2008), using the information reported in Table 4 about the average earnings, the Gini index and the relative sample size of the overall population and each circumstance/background groups. The GO index provides a cardinal evaluation of the extent of IOP within each policy regime.[6] We find that after the policy implementation the unfair inequality reduces from $GO(0) = 0.0385$ to $GO(1) = 0.0360$, that is, a reduction of about 6.5%, comparable in magnitude to the estimates by AHL in the evaluation study of the Norwegian Kindergarten Act. We conclude that a policy aimed at granting to drop out students the possibility to spend at least some additional years in the secondary education system equalizes opportunities in the sense of ex-ante EZOP, this effect being strong only for comparisons involving Circumstance 2, 3, and 4.

Differently from what has been found by AHL, our results reject potential trade-offs between opportunity equalization and amelioration. The opportunity amelioration tests we report in Table 5 conclude that none of the groups has lost, in terms of earnings, by effect of the educational expansion, while some groups, notably the sons of French farmers and manual workers and the executives and professionals, have gained from the reform. Equalization follows from the fact that more disadvantaged groups are catching up, by effect of a change in education, with the earnings of the privileged groups, this effect being robust across the distribution of effort.

5. CONCLUSIONS

In this chapter, we provide an illustrative application of the criterion proposed by Andreoli et al. (2019) to assess the degree of EZOP achieved by alternative public policies.

We evaluate if an educational policy, widening the access to the secondary education system, fosters EZOP. Obtained results suggest that this policy has a very mild impact on future students' earnings. The gains associated with this policy mostly affect those in the center of the distribution, while leaving unchanged the tails of the earnings distribution. However, we find that this allocation of gains promotes opportunity equalization in the sense of the EZOP criterion. The expansion of the secondary education seems to provide benefits for students coming from more disadvantaged backgrounds, with the circumstance groups 2 and 3 experiencing a narrow gap with respect to circumstance 4 by effect of the policy. At the same time, the policy seems to have any statistical impact on the distribution of the group of individuals with non-French father.

We speculate that the increase in accessibility to the educational system is more effective in equalizing opportunities if the adequate reforms take place early in the students' careers. For instance, AHL find strong opportunity equalization potential of kindergarten expansion, albeit the policy has insignificant average returns. International evidence suggests that school education reforms comparable to the *Loi Berthoin* have high average returns. We provide evidence of only mild equalization effects. This result suggests that EZOP objectives do not contrast efficiency motivations in public provision of educational services. There is growing evidence (Cunha & Heckman, 2007; Cunha, Heckman, & Lochner, 2006) that it is cheaper and more efficient for the society to compensate disadvantaged individuals/groups early in their educational career rather than to provide late intervention measures.

We leave to further investigations the assessment of the opportunity equalizing impact of other types of policies that can take place earlier or later (such as expanding university attendance) on the opportunities of the treated. Research in this field would provide additional information on hidden benefits of such policies that are often overlooked by traditional cost-benefit methods for policy evaluation.

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NOTES

1. Empirical approaches to inequality of opportunity are summarized in Checchi and Peragine (2010), Ferreira and Gignoux (2011), Lefranc et al. (2008), and Ferreira and Peragine (2016). The approach we pursue is distributional, as in Lefranc et al. (2009).
2. See Grenet (2013) for a detailed description of the reform.
3. More generally, the two social states may also correspond to two periods or two countries that one would like to compare (Andreoli & Fusco, 2019).
4. The panel rotation frequency was of 3 years before 2003 and earnings information are available only after 1990. This explains the choice of the years 1990, 1993, 1996, and 1999. Moreover, the rotation frequency after 2003 changed to one year and a half (i.e., one-sixth of the sample is replaced every trimester). Picking up information every 2 years allows to deal with a renewed sample, as in years 2004, 2006, 2008, and 2010. The year 2002 is not exploited due to imperfections in the data collected.
5. For a given model, we report the minimal order at which it is not possible to reject, with a confidence of 5%, that the gap curve generated by that model is either statistically equal to zero, or it always lies above the zero line for all the considered quantiles.
6. Let μ and G denote, respectively, the average earning and the Gini index, while s indicates the population share represented by one of the k-specific circumstances groups, which are indexed by i or j, then the GO index is defined as

$$GO(y) = (1/\mu)\sum_{i=1}^{k}\sum_{j>1} s_i s_j (\mu_j (1-G_j) - \mu_i (1-G_i)).$$

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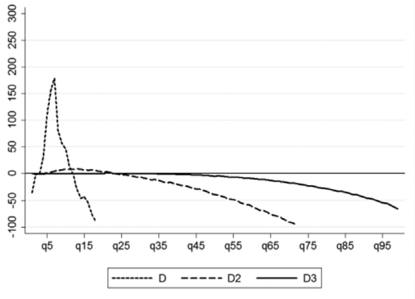
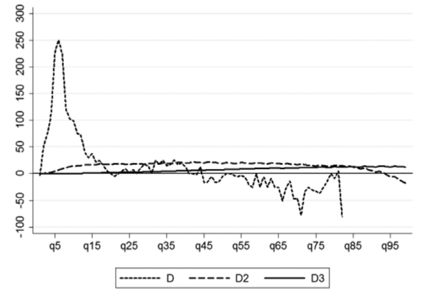
APPENDIX

Additional Graphs



(a) Circumstance 2 - Circumstance 1



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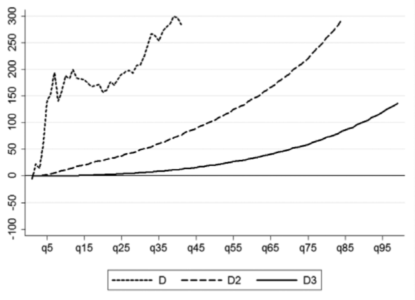
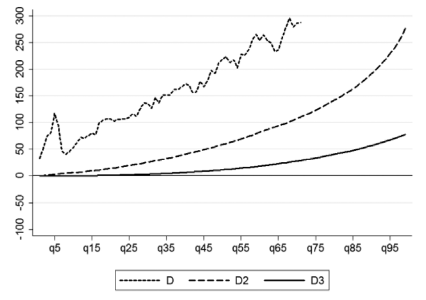
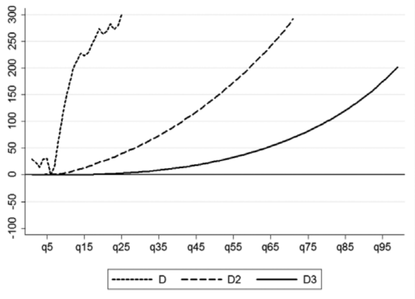


(c) Circumstance 4 - Circumstance 1



(d) Circumstance 3 - Circumstance 2



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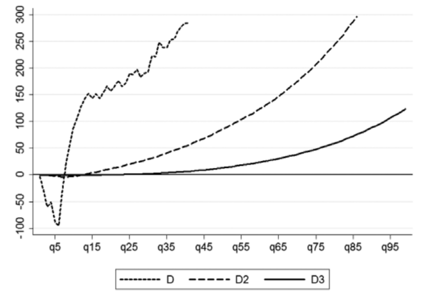

(f) Circumstance 4 - Circumstance 3

Fig. A1. Differences in quantile functions (D), *GL* curves (D2), and integrals of the *GL* curves (D3) computed at each percentile of the *actual* earnings distribution without policy treatment.

Notes: Values on the horizontal axis refer to percentiles of the actual earnings distribution. Values on the vertical axes express the difference between curves, in Euros. The curves represent the differences between the prospect of the outcomes associated with two distinct circumstances, for a total of six comparisons. Earnings differences are trimmed at 300 and −100 Euro.

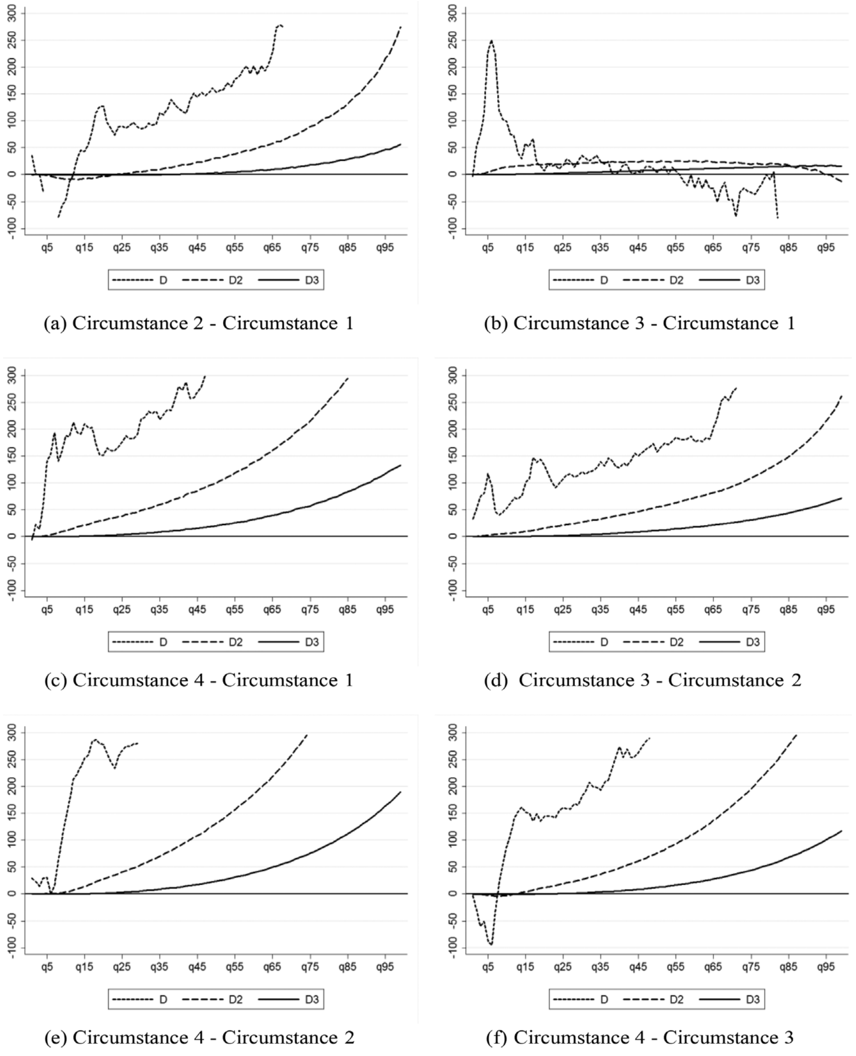

Fig. A2. Differences in quantile functions (D), *GL* curves (D2), and integrals of the *GL* curves (D3) computed at each percentile of the *simulated* earnings distribution with policy treatment.

Notes: Values on the horizontal axis refer to percentiles of the simulated earnings distribution. Values on the vertical axes express the difference between curves, in Euros. The curves represent the differences between the prospect of the outcomes associated with two distinct circumstances, for a total of six comparisons. Earnings differences are trimmed at 300 and −100 Euro.