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Abstract

While many studies look into the effect of primary school quality on labor market outcomes in developing countries, little is known about the effects at higher education levels. In this paper, we use the experiment provided by a large-scale education reform launched in Senegal in 2000 to examine the effects of quality improvements at the university level on the labor market outcomes of young high-skilled workers. Our estimates from a difference-in-difference model suggest that young university-educated workers have an employment rate about 12 percentage points higher after the reform as compared to older workers. In addition, we find that they are more likely to find employment in the service industry and in public or private enterprises. These estimates, which are robust to a host of robustness checks, support the idea that improvements in the quality of education reduce the mismatch between the quality of labor demanded and supplied in the high-skilled market.

Keywords: Higher education, employment, impact analysis, quality mismatch.
JEL Classification: I21, O15, O55.

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1 Introduction

Education is widely considered a key issue in the economic and social development of a country. Given the high rates of illiteracy in developing countries, most policies focus on improving access to education and most evaluation studies of the education sector in developing countries focus on access, particularly to primary education (e.g., Duflo, 2001, 2004). However, school quality is slowly emerging as an important issue in developing countries. Recent evidence suggests that cognitive skills rather than school attainment are strongly associated with individual outcomes and with economic growth (see, for example, Hanushek and Woessmann, 2008, and the papers cited therein). Moreover, there seems to be a direct link between school quality and attainment. For instance, Hanushek et al. (2008) find that Egyptian children were more likely to drop out of primary schools of lower quality, while Harbison and Hanushek (1992) find that improved school quality reduces the repetition rate among Brazilian primary school students. Finally, Behrman et al. (2008) find that the return from investing in quantity (access) might be smaller than the return from investing in quality with respect to schooling in rural Pakistan.

Not surprisingly, the research on quality of education in developing countries focuses exclusively on primary and secondary schools and on the effects on both achievement and labor market outcomes (e.g., Harbison and Hanushek, 1992; Glewwe, 1999; Bacolod and Tobias, 2006; Handa and Simler, 2006; Behrman et al., 2008; Hanushek et al., 2008; Hanushek, 2009). Economic development, however, seems to depend not only on the average level but also on the distribution of human capital or cognitive skills, particularly at the upper tail (Castelló and Doménech, 2002; Hanushek and Woessmann, 2008). Still, the quality of higher education is largely overlooked both in economic research and in policy design.

The purpose of this paper is to estimate the effects of improvements in the quality of

higher education on the labor market outcomes of highly-educated individuals. In particular, we focus on the short-term effects of an education reform in Senegal in the early 2000s, the Development Program for Education and Training (*Programme de développement de l'éducation et de la formation*, *PDEF*). We study the short-term effects of this reform because its short-run objective was a rise in the quality but not in the quantity of education at the university level. Because of data limitations, we can only analyze the effects on employment and not on other labor market outcomes such as wages or job quality. This is still an interesting outcome since previous studies found that, unlike in developed countries, a higher-level degree in a developing country does not necessarily lead to better employment prospects (e.g., Pritchett, 2001; Fares et al., 2005; Pauw et al., 2008; Boudarbat, 2006, 2008). In particular, unemployment seems to be positively correlated with educational attainment in sub-Saharan Africa (Fares et al., 2005). If the higher unemployment rate of highly-educated workers is due to a mismatch between the quality of labor demanded and supplied in the high-skilled labor market (Pauw et al., 2008), then improvements in the quality of higher education can lead to higher employment rates.[1]

Senegal is an appropriate setting for this exercise because its labor market exhibits this pattern of high unemployment rates for highly-educated individuals. Table 1 shows the relationship in 2002 between the unemployment rate and education for individuals living in Dakar, Senegal's largest city. Although household heads with university training have the lowest unemployment rate, the situation is exactly the opposite for dependents. In the short term, the individuals most likely to benefit from the reform are the highly-educated young, who (as we will show later) are also almost exclusively dependents, suggesting that unemployment is a severe problem among those most likely affected by the reform.

[1] In the rest of the paper, we will use the terms "high-skilled," "highly-educated" and "university-trained" to refer to individuals who obtained at least some education at the university level.

We use data from two surveys of the Senegalese population, one conducted right before the implementation of the reform (in 2001) and one five years later (in 2006), to estimate a difference-in-difference model. Because we cannot precisely identify in our data the individuals who benefit from the reform, we restrict our sample to individuals 20–39 years old and we use age to proxy for exposure to the reform. This leads to a trade-off between the size of the treated group and the contamination of both the treated and the control group: a wider age interval includes in the treated group a larger fraction of the individuals educated (at least partially) after the start of the reform, but also a larger number of individuals who completed their education before the first year of the reform. Taking this contamination into account, our results are generally robust to the definition of treatment. Our preferred definition is the 20–26 year-olds, a group that seems to offer the most balanced combination of size and contamination.

Our estimates suggest that the reform had strong positive short-term effects on the employment rate of all groups of young highly-educated workers. For our preferred definition of reform exposure, 20–26 year-olds, we find an increase of about 12 percentage points in their employment rate as compared to older highly-educated workers. We verify the robustness of these results to several specification checks. First, we confirm that our estimates are not driven by age effects by means of a triple-difference model that adds 20–39 year-old high school-educated individuals as an additional control group. Second, we estimate two placebo specifications, one among individuals with only high school education and one among older high-skilled individuals. The estimated employment changes in both placebo tests are small and insignificant, suggesting that the results are not caused by general shifts in labor demand. While in another specification check we do find a slight reduction in the labor force participation of young high-skilled individuals, this seems to be mostly due to higher

university enrollment. Moreover, a triple-difference model shows that this effect is entirely due to age effects. We also interpret these results as reduced dropout due to the reform. Finally, we do not find any significant changes in the fraction of highly-educated individuals, suggesting that our results are not due to reductions in the high-skilled labor force. Taken together, these results suggest that there is indeed an increase in the employment rate of young highly-educated workers after the education reform.

Several studies of the labor market in developing countries (e.g., Boudarbat, 2008; Fares et al., 2005) posit that the high rate of unemployment among the young highly-trained individuals is due to a "waiting queue" for jobs in the formal sector. According to this scenario, a rise in the employment rate of high-skilled workers is due to an expansion of the formal sector. However, our robustness tests do not suggest that this is the case. In addition, in the last part of the paper we investigate the sources of the employment growth found. Because of data limitations, we look at the choice of industry and of employer by young high-skilled workers. We find an increase in the fraction of individuals employed in services (to the detriment of manufacturing and of other industries) as well as a slight increase in employment in private or public enterprises (accompanied by a reduction in and in self-employment). These suggest that high-skilled individuals are better able to find jobs that pay better than the outside option (self-employment) after the reform, and that they mostly work in the service sector for private enterprises and somewhat for the government. These results provide suggestive evidence in favor of our hypothesis that the mechanism explaining the observed employment growth is the reduction in the quality gap between labor supplied and demanded due to the reform.

Our study has several limitations. First, we only examine the short-term effects of the reform. However, proof of immediate results is essential for the continued implementation of

a reform in volatile political environments such as in much of the developing world. Second, we cannot determine the effect of the reform on the quality of employment or on wages because of data limitations. We do not believe that this is a major concern since none of our robustness tests suggests the presence of some factor that pushes younger high-skilled workers to accept lower quality jobs after the reform. Moreover, we interpret the reduction in self-employment as suggestive evidence that individuals are able to find better paying and presumably better quality jobs after implementation of the reform. Third, we cannot distinguish between full or partial exposure to the reform. However, this implies that our estimates are likely a lower bound of the short-term effects of the reform. Finally, similar to many other studies using data from Sub-Saharan Africa, we have a relatively small sample, mostly due to the size of the high-skilled workforce in the Senegalese economy. While this is likely to generate imprecision in our estimates, the effects are robust to several specification checks and generally statistically significant.

The rest of the paper proceeds as follows. The next section presents the institutional background and the educational reform in Senegal. The empirical strategy is described in section 3 and the data used in section 4. The results from the baseline specification and from the various robustness tests, as well as the choice of industry and employer are discussed in section 5. Section 6 concludes.

2 Institutional background

The higher education system in Senegal is based on the French model. For the period under study, this means that university coursework generally starts at around age 19 for a regular trajectory through the education system. Most subjects require four years of study for a successful completion, but degrees are awarded along the way and students can quit their

studies after obtaining one of these diplomas. The first degree comes after the first two years and represents general university training (*diplôme d'études universitaires générales*, *DEUG*), followed by another degree after a third year of more specialized studies (*licence*) and the final degree after a fourth year of specialized studies (*maîtrise*). During the late 1990s, a majority of students quit their studies after obtaining a *DEUG*, the first and most basic of these degrees, and seemed to have some difficulties in the labor market due to differences between the knowledge acquired and that desired by employers (Samb et al., 1999).

Against this backdrop, Senegal included some specific goals for higher education in the major reform of its education system undertook in 2000 as part of the Millennium Development Goals. The Development Program for Education and Training (*Programme de développement de l'éducation et de la formation*, *PDEF*) had as main objectives to increase access to basic education, to improve the quality of learning and to make system management more efficient, affecting all education levels over a period of ten years from 2000 to 2010 (Ministère de l'éducation, 2003).[2] Although the emphasis was on primary and to some extent secondary education, the reform included actions and objectives targeted specifically to the higher education. Moreover, higher education expenditures did not fall during the first half of the program. Panel A of table 2 shows that the share of higher education in total expenditures on education was constantly above the planned levels and that it did not fall below 23 percent throughout the entire period. Given that public spending on education increased during the same period as shown in panel B of table 2, this implies that spending on higher education actually increased at a steady pace between 2000 and 2005.[3]

[2]The initial name was of the program was the Ten-Year Program for Education and Training (*Programme décennal de l'éducation et de la formation*), with the same acronym.

[3]The cost per student is also much higher in higher education. For example, the unit cost associated with a student at the university level can be between 5 and 20 times higher than that of a secondary and primary

The two main objectives of the reform with respect to higher education concerned improved access and better quality of instruction. The first objective involves the construction of regional university centers to reduce the pressure on the country's two main universities, Cheikh Anta Diop in Dakar and Gaston Berger in Saint Louis (Ministère de l'éducation, 2003). However, the first of these new institutions of higher education opened in 2007 (Direction de la planification et de la réforme de l'éducation, 2009), making this aspect of the reform not relevant for our study as we restrict our attention to the short-term effects of the reform and particularly the period 2001–2005, as we will detail in section 4.

The second goal of the reform and the one we focus on in this paper was to improve quality. This objective was deemed "imperative" and several quality indicators were identified and targeted, such as the dropout rate, the distribution of students across technical and non-technical fields, and the match between university training and employment (Ministère de l'éducation, 2003). During the first phase covering the years 2001–2003, the budget allocated to quality improvements represented 42% of operating expenditure and 50% of new investment at the tertiary level (Ministère de l'économie et des finances du Sénégal, 2001). The actions undertaken involved improvements in the technology used in instruction and in laboratories, enhancements to libraries and information systems, the allocation of additional research funds, and a realignment of fields of specialization to follow closer the demands of the labor market (Ministère de l'éducation, 2003).

Some suggestive evidence on the results of these actions is presented in table 3. Panel A of this table shows the actual enrollment in 2000 and in 2007 at Université Cheikh Anta Diop in Dakar, the main university in Senegal, as well as the predicted enrollment for the period 2000–2007 based on a prediction model developed by the Ministry of education. Panel B

student (Direction de la planification et de la réforme de l'éducation, 2008).

lists the projected required faculty size at the same university. Based on the 2000 levels and the objectives of the reform, the Ministry of education projected a general decline in enrollment, particularly in humanities, accompanied by a rise in the number of faculty. In reality, enrollment in all fields increased by 2007 by a factor of at least 2. Since capacity constraints did not change during this time, the most plausible explanation for this trend is that of lower dropout rates, i.e., students pursued their education until obtaining a *licence* or *maîtrise*. Note that although the additional enrollment could cause lower quality of education for students obtaining the more advanced degrees due to a higher student-to-teacher ratio, it would presumably increase the quality of the overall graduating class by increasing the share of students finishing with the more advanced degrees.[4]

Finally, the reform can also increase the quality of high-skilled labor through positive selection. A perceived improvement in the quality of higher education and in the labor market prospects post graduation can cause some higher-ability high school graduates to pursue higher education instead of entering the labor market immediately. In addition, some of the high school graduates that seek higher training abroad can choose to obtain it in Senegal. As a result, the quality of both the student pool and of the highly-educated labor force improve and, to the extent that there are positive peer effects in learning, the quality of higher education is even further improved.

[4]Other potential explanations are an increased demand for education following the announcement of the reform and its objective or general trends in the demand for higher education. However, these explanations would hold only if higher education was underutilized before the reform. This hypothesis is unlikely to be true, based on the fact that part of the reform was to *add* capacity to the higher education system. In any case, a higher labor supply lowers the employment prospects of young university-trained individuals and reduces the effects of the reform in the labor market, an aspect to which we return in section 5.2.

3 Empirical strategy

The empirical strategy exploits the fact that the education reform was an event exogenous to the labor market experience of any particular individual. As such, we use a difference-in-difference approach where treatment status is given by exposure to university training after the implementation of the PDEF. As in any difference-in-difference model, the estimation is based on a pre-post comparison of mean outcomes between treated individuals (i.e., those who obtained at least part of their education after the implementation of the reform) and control individuals. Since the reform was not targeted at a particular demographic group, controlling for observed socio-economic characteristics of the individuals should not change the estimates, but it can improve the explanatory power and the precision of the estimation. We start by restricting the sample to university-trained individuals because individuals who obtained higher education before the reform are likely to be the best comparison group for those who obtained their higher education after the reform. The difference-in-difference model estimated in this sample has the following form:

$$P(Y_{it} = 1|T_i, P_t, X_{it}) = \Phi(\beta_0 + \beta_1 T_i + \beta_2 P_t + \beta_{12} T_i P_t + \delta X_{it}), \tag{1}$$

where Y_{it} is a binary measure of labor market performance, T_i is a dummy variable indicating whether at least part of the education of individual i was obtained after the reform, P_t is an dummy variable for period t being after the implementation of the PDEF (or, equivalently, the observation coming from the ESPS survey), and $\Phi(\cdot)$ is the standard normal distribution function. We estimate the model by probit and we correct the standard errors for correlations within regions (e.g., due to local labor market conditions) by clustering them the region level.[5]

[5]Senegal is divided into ten regions: Dakar, Ziguinchor, Diourbel, St-Louis, Tamba, Kaolack, Thiès, Louga, Fatick and Kolda.

The causal effect of interest is the effect of the education reform on university-trained individuals, which is an average treatment effect on the treated. To the extent that the control group provides an appropriate counterfactual for the average outcome of the treated group in the absence of the reform, Puhani (2012) shows that the treatment effect can be calculated as:

$$\begin{aligned}\widehat{ATT} &= \big(E[Y|T_i=1,P_t=1]-E[Y|T_i=1,P_t=0]\big)\\ &\quad-\big(E[Y|T_i=0,P_t=1]-E[Y|T_i=0,P_t=0]\big)\\ &= E[\Phi(\beta_0+\beta_1+\beta_2+\beta_{12}+\delta X_{it})]-E[\Phi(\beta_0+\beta_1+\beta_2+\delta X_{it})]\end{aligned}$$

As we will detail in the next section, we cannot precisely identify individuals who obtained their higher education after the PDEF. Instead, we proxy exposure to the reform with age and define the treatment group as the set of university-trained individuals in a certain age group. This creates two potential problems. First, it is possible that the differential evolution of the outcome of interest between the treated and control groups is due to "age effects," time-varying unobservable characteristics that are correlated with both the employment probability and the age of individuals. To the extent that these age differences are similar across different groups in the population, we can use high school trained individuals to eliminate them. Specifically, we estimate difference-in-difference-in-difference (triple-difference) models in which we include individuals with high-school education in the same age groups as an additional control group. The triple-difference model is:

$$\begin{aligned}P(Y_{it}=1|T_i,P_t,X_{it}) = \Phi(&\beta_0+\beta_1T_i+\beta_2P_t+\beta_3U_i+\beta_{12}T_iP_t\\ &+\beta_{13}T_iU_i+\beta_{23}U_iP_t+\beta_{123}T_iP_tU_i+\delta X_{it}),\end{aligned} \tag{2}$$

where U_i is an indicator for individual i being university-trained and the effect of the reform is determined by the triple interaction β_{123}. As before, the standard errors are clustered at the region level to control for correlations within regions.

The second potential problem is related to the imperfect identification of individuals exposed to the reform. Since it is unlikely that all individuals in an age group obtained their education after the reform or that all remaining individuals obtained their education before the reform, this leads to a potential contamination of the treated group, of the control group or both.[6] Formally, suppose that exposure to the reform is imperfectly observed, with T_i^* an indicator of unobserved actual exposure, and suppose that a fraction δ_T of the treatment group did not obtain their education after the PDEF ($T_i^* = 0$ in the post-reform period), while a fraction δ_C of the control group did ($T_i^* = 1$ in the post-PDEF period). For simplicity, suppose also that the effect of PDEF on the outcome studied is constant across all individuals, Δ, and that there is no trend in the outcome among the unaffected individuals:

$$E[Y|T_i^* = 1, P_t = 1] - E[Y|T_i^* = 1, P_t = 0] = \Delta,$$
$$E[Y|T_i^* = 0, P_t = 1] - E[Y|T_i^* = 0, P_t = 0] = 0.$$

In this case, the estimated effect of the reform is:

$$\begin{aligned}\widehat{ATT} &= (1 - \delta_T)\big(E[Y|T_i^* = 1, P_t = 1] - E[Y|T_i^* = 1, P_t = 0]\big) \\ &\quad - \delta_C\big(E[Y|T_i^* = 1, P_t = 1] - E[Y|T_i = 1, P_t = 0]\big) \\ &= [1 - (\delta_T + \delta_C)]\Delta,\end{aligned}$$

[6] Hotz et al. (1997, 2005) present a thorough analysis of contamination of the control group in a different setting.

which underestimates in absolute value the true effect, and the size of the bias depends on the cumulative fraction of contamination in the treated and in the control group.

4 Data

The data comes from two household surveys, the *Deuxième enquête sénégalaise auprès des ménages* (*ESAM-II*), conducted in 2000–2001, and the *Enquête de suivi de la pauvreté au Sénégal* (*ESPS*), conducted in 2006. These surveys had the same main objective of determining the poverty profile in Senegal and they both used questionnaires based on extensions of the Core Welfare Indicators Questionnaire. As a result, they are rather similar, as pointed out by several other studies (e.g., Mesplé-Somps and Robilliard, 2010; Fall, 2010).

The ESAM-II survey was conducted in two waves: June–August 2000 and February–April 2001. Individuals not in school who already obtained university training by the time of the survey must have done it before the implementation of the PDEF, which came into effect during the 2000-2001 academic year. Thus, this survey provides a snapshot of the situation right before the implementation of the education reform. The second survey, ESPS, was conducted in one wave between December 2005 and April 2006 so some of the individuals included acquired part or all of their university training after the implementation of the reform (partial or full exposure to the reform). In addition, since the survey was conducted before any of the additional higher education institutions opened, the only aspect of the reform in effect during this period is the quality improvement. To reduce the effects of seasonality in the labor market when comparing the two surveys, we only use the second wave of the ESAM-II, which was conducted at almost the same time of the year as the ESPS. To obtain nationally-representative figures, we combine the sample weights used in the two surveys based on the assumption that the structure of the population did not change

significantly in the period between the two surveys and provide weighted statistics and regression results throughout the paper.

Several data limitations dictate our choice of variables. First, both surveys include information on the labor force participation and employment status of surveyed individuals but not on their wages or occupation, which limits our choice of outcome variables. Our main dependent variable is employment status, but we also study the labor force participation decision and the industry of employment. Second, the ESAM-II survey does not distinguish between the different higher-education degrees (*DEUG*, *licence* or *maîtrise*). To ensure comparability across surveys, we define high-skilled individuals as persons who finished at least one year of university studies.[7] Finally, in most specifications we include a set of socio-economic variables that influence labor market participation and job-search intensity, such as marital status and the relationship to the household head, as well as variables describing the local labor market, such as the region of residence and the degree of urbanization of the place of residence.

We restrict the sample to highly-educated individuals in the 20–39 age group. The minimum age in the sample, 20 years, represents the age after one year of university coursework when following a regular path in the education system. By setting the maximum age to 39 years, we keep only recent graduates and young workers for a more comparable sample. Finally, in the baseline specification we restrict the sample to individuals in the labor force, working (employed and self-employed) or unemployed, and exclude persons still in school or out of the labor market for other reasons, although we relax this restrictions in some specifications.

As mentioned before, we proxy reform exposure with the aid of an age group and use the

[7] A similar situation applies to high-school educated individuals.

rest of the sample as the control group. We start by providing a general picture of exposure to the reform in the ESPS survey in order to determine the relevant age group. First, note that individuals under 24 must have obtained at least part of their higher education after the reform simply because of the total number of years required. For some of the older individuals, we can infer if they were exposed to the reform based on their degree (e.g., a 26 year-old who obtained a 4-year degree must have followed some university courses after the implementation of the PDEF). Finally, we use the questions on school attendance during the survey year and during the previous year included the ESPS survey.

Figure 1(a) shows the fraction of highly-educated individuals identified as exposed to the reform for at least one year, calculated as described above.[8] The figure shows that the only non-contaminated treatment group is the 20–24 year-olds and that the degree of contamination of the treatment group increases as age increases. Figure 1(b) shows the cumulative distribution of reform exposure by age, again based on our imperfect measure of exposure. Even with this underestimate, a relatively large fraction of exposed individuals are excluded from the treated group if treatment is defined as the 20–24 year-old bracket, suggesting an inverse relationship between the two contamination proportions δ_T and δ_C. However, note that an increase in the age group used to define treatment, at least for small treated groups, leads to more overall contamination due to the relative size of the treated and control groups. For example, suppose we increase the treated group from 20–24 to 20–25 year-olds. This means that we shift 25 year-olds from the control group to the treated group. The amount of contamination in the control group does not change by much, since 25 year-olds do not represent a large part of the 25–39 year-old group (the number of highly-educated

[8]Note that this approach gives us a lower bound of the fraction of individuals affected by the reform, since we cannot identify all the individuals older than 24 who had at least part of their university training after the reform, but before 2005. Note also that this approximation is farther from the true fraction as age increases.

individuals in each age cell is generally increasing with age simply because people can obtain more education over time). However, the contamination of the treated group increases by more because 25 year-olds are a relatively large part of the 20–25 year olds.[9]

In addition, the size of the treated group varies directly with the width of the age interval used to define exposure to the reform. Therefore, we have a trade-off between contamination of the two groups and size of the treated group. Our preferred definition of treatment is the 20–26 age group because figure 1(a) shows that a minimum of approximately 80 percent of individuals in each age cell are exposed to the reform and 1(b) indicates that about 80 percent of all individuals identified as exposed to the reform are included in this age group.[10]

Table 4 presents the descriptive statistics for this definition of treatment, separately for each period (survey). The table shows that most of the highly-educated individuals are male and live in the urban areas of Dakar. While there are some difference with respect to the other demographic characteristics between the individuals in the treated and in the control group, these tend to be mostly similar in the two periods. For instance, the control group tends to have higher fractions of married individuals in both periods, as well as higher fractions of household heads. Also, employed individuals in the control group are more likely to work for the government or in the "other industry" category, but again these differences tend to persist in both periods. Both these difference and the characteristics of each of the two groups are generally similar between the two periods. This suggests that the difference-in-difference strategy should eliminate these differences and controlling for socio-economic characteristics in the regressions should not influence our estimates, which would be expected if the control group provides a valid counterfactual for the treated group.

[9]Indeed, our crude measure of overall contamination is strictly increasing with the width of the age interval used to define exposure to the reform between 20–25 and 20–29.

[10]These numbers should be taken as suggestive because of the issues mentioned earlier.

More importantly, the table shows a significant increase in the employment rate of 20–26 year-old individuals compared to a relatively stable employment rate for the 27–39 year-old individuals. This is a preview of our main results, suggesting that the reform led to better employment prospects for the individuals likely to be affected.

We should note at this point the relatively small size of our sample. As it will become apparent when we include high school-educated individuals in the analysis, this is due to the small fraction of the Senegalese population that acquires higher education. This is a problem that would likely appear in a study of other developing countries as well and that could be solved only by using large data sets such as from a national census, which is unfortunately not possible in our case.

5 Results

5.1 Baseline specification

Table 5 contains the marginal effects corresponding to the effect of the reform estimated from the baseline empirical strategy, both as a difference-in-difference and as a triple-difference.[11] As mentioned before, all specifications are weighted using sample weights and allow for within-region correlations in errors. We start in panel A with the effect of the PDEF for our preferred definition of treatment, 20–26 year old individuals. The estimate in column 1 indicates that the employment rate of 20–26 year old individuals increased after the reform by approximately 12 percentage point as compared to the employment rate of 27–39 year old individuals. This represents a 21 percent increase in their employment rate as of 2000.

In the next column, we add controls to eliminate a potential bias due to differences

[11] The results from linear models, available upon request, are very similar to the marginal effects reported in the table.

in demographic characteristics between the two groups and their effect on labor market outcomes. The estimated effect drops slightly to 9 percentage points (a 16 percent increase from the employment rate in 2000) but remains significant. Finally, we estimate the triple-difference specification in order to eliminate any potential age effects that could drive our estimate. As before, we estimate both a specification without controls (in column 3) and one with controls (in column 4). The coefficient estimates are both very similar to the ones in the first two columns, though an increase in standard errors reduces their significance level.

A small sample size can cause problems in terms of precision but also in terms of bias due to the sampling scheme. To alleviate this concern, we bootstrap the marginal effects calculated above and provide the bias-corrected 90 percent confidence interval after 1,000 replications. Although the confidence intervals are somewhat larger, we cannot reject relatively large effects of the reform. In addition, the estimated bias is small, of the order of 1 percentage point in all cases. All of these make us confident that our estimates are reliable.

In panel B, we study the sensitivity of our estimates to different definitions of exposure to the reform. We vary our treated age group from 20–25 to 20–29 year-olds (in all cases, the rest of the 20–39 year-olds form the control group).[12] As before, adding the controls to either the difference-in-difference specification or to the triple-difference specification produces little change in the estimates. Similarly, age effects do not seem to be of concern as the effects in the difference-in-difference model are similar to those in the triple-difference model. More importantly, the effects are monotonically decreasing with the size of the treated group, just as predicted by an increasing degree of overall contamination of the treated and of the control group. Using our crude measure of contamination, we can provide some back-of-the envelope calculations of the true effect. With the exception of the 20–25 year olds, all the

[12] We estimated all the other specifications in the paper for these additional definitions of treatment. The results, available upon request, are similar to those for the 20–26 year-old definition.

effects corrected for contamination are between 6.9 and 12.8 percentage points.[13]

Our estimates are likely lower bounds of the true effect, implying that the education reform had significant employment effects among young high-skilled workers. If this effect is due to a mismatch in the quality of education demanded and supplied in the high-skilled labor market that is reduced after the implementation of the PDEF, the only increase in employment once this mismatch is completely eliminated comes from new jobs. As a result, the medium and long-term effects of the reform are presumably different from the short-term effects of the reform estimated in this paper.

5.2 Robustness checks

The identification of effects in difference-in-difference models is based on two hypotheses: that the control group provides an appropriate counterfactual for the treated group in the absence of the intervention, and that the only factor influencing the outcome is the intervention. Any violation of these hypotheses produces biased estimates. We already showed in table 5 that there our results do not seem to be driven by age effects as the triple-difference model yields results very similar to the difference-in-difference model. This suggests that older workers are indeed a good counterfactual for young workers, our treated group. In the rest of this section, we will provide evidence in support of the second hypothesis, that our estimates capture only the effect of the reform. We focus particularly on scenarios that would bias our coefficients upward.

If the economy expanded during this period such that the demand for high-skilled labor increased, our estimation strategy would incorrectly attribute to the reform the rise in the

[13]The effects corrected for contamination are, in order from 20–25 and 20–29: 0.331, 0.128, 0.118, 0.086, and 0.069. The true effects in these samples are likely even closer to each other since our underestimation of overall contamination is presumably increasing with the age group used to define exposure to the reform.

employment rate of young workers due to this expansion. It is likely that an expansion of the economy would affect high-skilled workers of all ages. In panel A of table 6, we provide the results from a placebo test based on a specification identical to our baseline difference-in-difference model but estimated in the sample of 30–49 year-old university-educated workers where treatment is defined as 30–36 year old individuals. We find a small (1.5 percentage point) and statistically insignificant reduction in the employment rate of 30–36 year-olds as compared to older workers. This confirms that our results are not driven by an expansion of the economy during this period.[14]

These results also provide evidence against a "substitution effect." If employers have a limited number of jobs, they might substitute younger workers for the already-employed older workers if the quality of younger workers is higher. This would cause the employment rate of younger workers to go up and that of older workers to go down, artificially increasing the estimated effect of the reform. To the extent that the quality of a potential employee is based on their academic training and on-the-job learning and experience, we would expect this type of substitution to be more prevalent for individuals of closer age to our treated groups (in other words, younger workers are probably not good substitutes for more experienced workers). We can then obtain some some suggestive evidence on the presence of substitution effects by comparing the employment rate of individuals slightly older than our treated group to that of even older individuals. The results in panel A indicate that slightly older individuals experienced no worse labor market outcomes than older and more experienced high-skilled individuals, providing evidence against substitution effects driving our results.

[14] One scenario which would reconcile both these results and our baseline estimates is if almost all older workers were already employed and the expansion of the economy has to be met by increased employment of young workers. However, this is unlikely to be the case as the labor force participation rate of workers in our control group is around 74 percent in both periods, indicating that there is still excess labor supply in this age group.

To further investigate the possibility that our results are driven by other factors not controlled for in equation (1), we conduct another placebo experiment where we estimate equation (1) on the sample of 20–39 year-old high school educated individuals, using the same treatment definition as before. The short-term objectives of the PDEF with respect to high school education were almost exclusively related to access, such as the construction of new schools and new classrooms. These effects would take some time to be seen and therefore we should not see any effect of the reform in this sample. Indeed, the estimated effects shown in panel B of table 6 are very small (2.3 percentage points) and statistically insignificant, suggesting again that our estimates are not capturing other factors such as an expansion of the economy.

Another potential concern is that the results are driven by sample selection, such as if the reform influences labor force participation. Our results are upward biased if young high-skilled individuals are more likely to leave the labor force, thus reducing the number of individuals actively looking for a job. In this case, we would find a higher employment rate even though there is no real positive effect of the PDEF on employment. To test for this scenario, we estimate our equation (1) using labor force participation as the dependent variable in the sample of high-skilled individuals in and out of the labor force and we report the estimated reform effect in panel C of table 6. We find a relatively larger (9.5 percentage point) decline in the labor force participation of young workers as compared to older high-skilled workers, although still statistically insignificant. However, this decline could simply be due to "age effects": we would see a reduction in the share of labor force participants among younger workers if the general trend is for younger people to obtain more education. Panel D of the same table presents the result from equation (1), the triple difference model that eliminates age effects, estimated in the sample of high school and university educated

individuals both in and out of the labor force and using labor force participation as the dependent variable. In this case, we do not find any evidence of reduced labor force participation among younger high skilled workers as the coefficient is almost zero (0.6 percentage points). This suggests that, indeed, the reduction in labor force participation found initially is due entirely to age effects. To further confirm this, we estimate our baseline difference-in-difference model in the sample of high-skilled workers in and out of the labor force, but with school enrollment as the dependent variable. We find that young individuals are 6.9 percentage points (panel E of table 6) more likely to be enrolled in university after reform than older individuals. This pattern is consistent with lower dropout rates, delaying the labor market entry for a certain fraction of the increase in enrollment documented in table 3. Moreover, the magnitude of the coefficient explains almost entirely the reduction in labor force participation found earlier.

If the reform changes incentives to obtain higher education, this can cause another type of sample selection. Similar to the previous case, our estimates are upward biased if individuals are less likely to obtain higher education after the implementation of the reform and downward biased in the opposite case since that would increase the labor supply.[15] To test for changes in the decision to pursue university coursework, we estimate a specification based on equation (1) in the sample of high school and university trained labor market participants, but with U_i (the indicator for whether the individual has university training) as dependent variable. The estimated effect listed in panel F of table 6, although positive (3.7 percentage points), is small and insignificant. This is also consistent with the fact that the capacity constraints in higher education did not change in the short term.

[15] One exception to this is if the reform changes the ability distribution of the university-educated workforce (if, for instance, returns to education increase). In this case, the number of employed individuals could go up even if labor supply increases as the gap between labor quality demanded and offered shrinks. However, this is an effect of the reform we would probably want to include in our estimation.

Finally, our estimates are upward biased if the education reform encourages high-skilled workers to accept (temporary) jobs that require less education. This is unlikely to be the case, however, as Serneels (2007) finds in the Ethiopian context that high-skilled unemployed individuals do not generally take on temporary employment after leaving university because social networks are usually more effective when the applicant is unemployed and because employment of poor quality can damage the reputation of the individual and thus reduce the probability of obtaining a more desirable job in the future. We will return to this issue in the next section.

Taken together, the tests conducted in this section suggest that our estimates capture the effect of the PDEF reform on the employment rate of young high-skilled workers. Moreover, as we do not find evidence of an expansion of the Senegalese economy in terms of employment of older high-skilled individuals or of high school educated individuals, this supports our initial hypothesis that the effect of the reform was to reduce the gap between the quality of labor demanded and supplied in the high-skill labor market.

5.3 Possible sources of employment growth

In this section, we investigate some potential sources for the additional employment among young high-skilled workers. Unfortunately, we do not have information on occupation or on the employer side, which would allow us to directly test if job quality improved after the reform. Instead, we use the industry of employment and the employer type as proxies for changes in the labor market prospects of university-trained workers.

We start by estimating a multinomial probit specification similar to equation (1) for the choice of industry on the sample of working high-skilled individuals. We combine the

different industries into three groups: manufacturing, services, and other industries.[16] Panel A of table 7 shows the marginal effects of the reform effect, which are the difference between younger workers and older workers in the probability of choosing the corresponding industry as compared to the other two groups after the reform.[17] The coefficients indicate that younger workers are significantly more likely to work in a services industry (19 percentage points) and less likely to work in manufacturing (8 percentage points) or another type of industry (11 percentage points). In a study of the Senegalese economy in 2003, Echevin and Murtin (2009) find that the returns to education are among the highest in the trade and services sectors. Therefore, our results suggest that young high-skilled workers are able to find jobs that better reward their education level after the reform.

Panel B of the same table shows the results of a multinomial probit similar to the previous one but where the dependent variable is a variable indicating the employer of the individual: self-employment, government, or a public or private company. The regression is estimated again on the sample of working high-skilled individuals. The results indicate that young workers are much less likely to be self-employed (by 14.6 percentage points) and marginally less likely to be employed by the government (by 2.7 percentage points), but they have a higher probability of working for a public or private enterprise (17.3 percentage points). Although all the effects are statistically insignificant, they do point to a pattern of finding better-quality jobs to the extent that self-employment is a last-resort activity.[18]

Since private employers are the ones most likely to respond to changes in the quality of

[16] "Manufacturing" groups mining, manufacturing and other public utilities. "Services" includes trade, tourism, transportation and communication, finance, insurance, and real estate, and personal services. "Other industries" includes the other industries of employment.

[17] Therefore, the sum of the three marginal effects is 1. Because of convergence issues, the results are reported from models that do not include the control variables.

[18] One caveat to this interpretation is if the reduction in self-employment is due to tighter credit constraints. In this case, individuals could choose lower paying jobs. However, this situation cannot explain our previous results that individuals tend to work more in service industries, which tend to reward education better.

the workforce, these results provide additional evidence that the effect of the reform was to improve the quality of high-skilled workers and thus their employment prospects. As a result, the individuals most likely affected by the reform see an increase in their employment in private or public enterprises and in the sectors where their education is best rewarded.

6 Conclusions

In this paper, we analyzed the short-term effects of an education reform targeting higher education on the labor market outcomes of high-skilled workers. Since in the very short term the reform changes only the quality of, but not access to, higher education, we estimate the effect of an improved quality of education on the employment prospects of university-trained individuals. We find that the reform led to significant increases in the employment rate for various definitions of exposure to the reform and that these estimates are robust to a host of specification checks. We also find that young workers are better able to find jobs in the services sector and in public or private enterprises, all of which are presumably associated with better returns to education.

Our results contribute to the existing literature on education in the developing world. In particular, to our knowledge, this is the first study of the effects of quality at the higher education level on the labor market in developing countries. Our findings indicate that quality improvements in higher education could have significant positive effects not only on the labor market outcomes of university-trained individuals, but also on the dropout rate and on university attendance. In addition, as individuals are able to find jobs that better exploit their training, this also creates the potential for long-term economic growth. Finally, our results provide evidence in support of the hypothesis that developing countries such as Senegal experience significant differences between the quality demanded and offered in the

high-skilled labor market. The immediate impact of a reform improving the quality of higher education is most likely to fulfill the backlogged demand for better trained workers, leading to a relatively sharp increase in the employment rate of young workers. It is likely that the longer-term effect of the reform will be lower, even as employers adjust to the expected influx of better quality workers.

There are several limitations to our study. First, as mentioned above, we can only examine the very short-term effects of the reform. However, in volatile political environments such as in much of the developing world, having proof of immediate results encourages the continued application of the reform and the implementation in other countries. Second, we do not have information on wages or job quality so we cannot determine whether the increase in employment is associated with better jobs. We do not consider this a major concern, as we do not find any evidence in our robustness tests of factors pushing younger high-skilled workers to accept lower quality jobs after the reform. On the contrary, we find evidence that young individuals tend to work more in services industries, which were previously found to offer one of the highest return to education (Echevin and Murtin, 2009). Third, we cannot distinguish between full or partial exposure to the reform, which means that we are likely estimating lower bounds for the short-term effects of the reform. Finally, we have relatively small samples, mostly due to the shortage of high-skilled workers in the developed world. While this is likely to lower the precision of our estimates, the estimated effects are robust to a host of specification checks and generally statistically significant.

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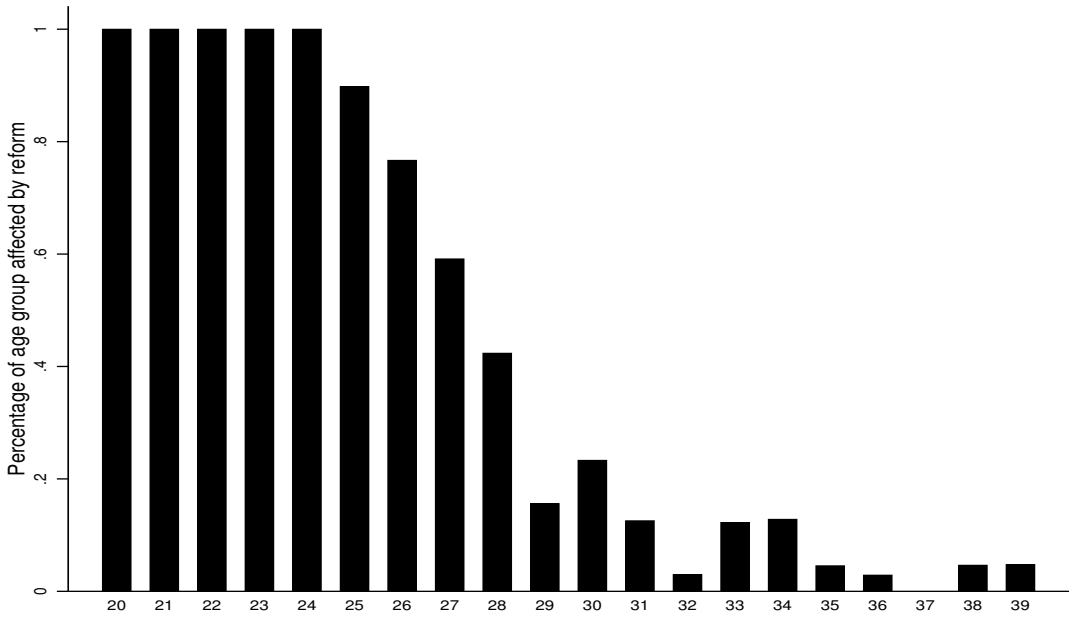
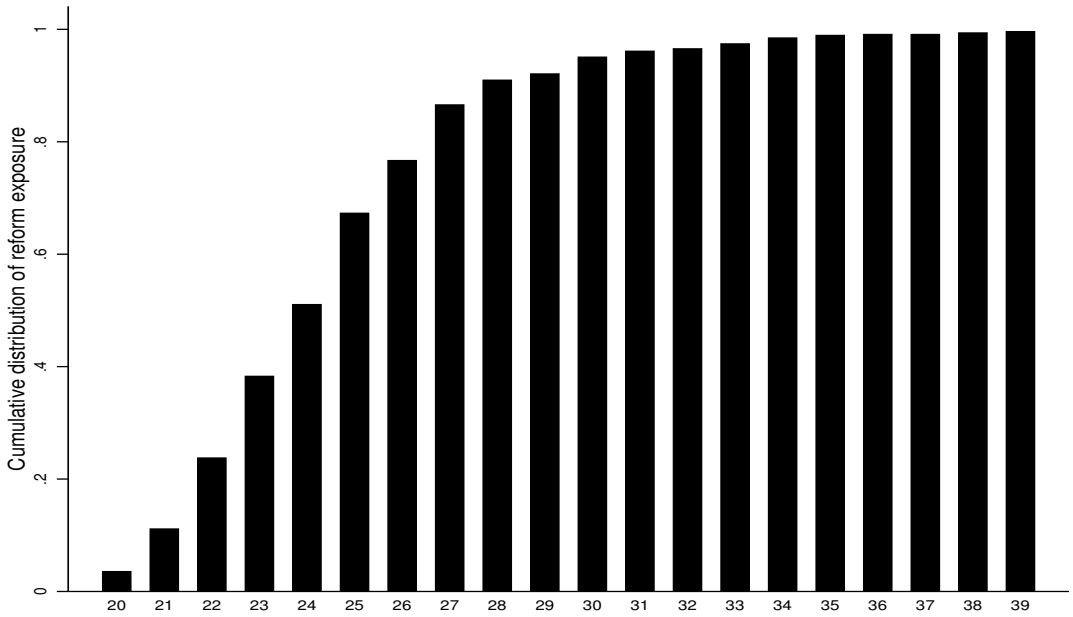


(a) Proportion of university graduates affected by PDEF, by age



(b) Cumulative distribution of PDEF exposure, by age

Figure 1: An estimate of contamination in the treated and in the control group

Table 1: Unemployment rate in Dakar, by education (2002)

	No education	Primary	High school	University
Head of household	0.061	0.068	0.071	0.027
Dependent	0.097	0.133	0.179	0.238

Source: Agence Nationale de la Statistique et de la Démographie (2004).

Table 2: Education expenditures

	2000	2001	2002	2003	2004	2005
A. Higher education expenses (% of education spending)						
Projected	—	19.90	19.25	25.47	24.41	23.39
Actual	25.60	26.10	25.30	23.80	26.10	23.80
B. Public spending on education						
Percent of GDP	3.16	3.29	3.39	3.51	3.86	5.15

Source: Direction de la planification et de la réforme de l'éducation (2008), World Development Indicators.

Table 3: Actual and projected enrollment and faculty staffing at Université Cheikh Anta Diop

	Actual	Projected							Actual
	2000	2001	2002	2003	2004	2005	2006	2007	2007
A. Enrollment									
Engineering and sciences	3,742	3,301	3,200	3,100	2,999	2,999	2,999	2,999	6,825
Humanities	7,777	7,700	6,500	5,401	4,200	4,200	4,200	4,200	22,850
Economics and Business Administration	2,607	2,801	2,700	2,501	2,400	2,400	2,400	2,400	6,842
Medical School	2,146	2,320	2,320	2,320	2,320	2,320	2,320	2,320	5,379
Political Studies and Law	3,820	3,820	2,400	2,299	2,200	2,200	2,200	2,200	10,821
B. Number of faculty									
Faculty members	1,091	1,146	1,206	1,270	1,339	1,412	1,490	1,576	

Source: Projection model of the Ministry of Education.

Table 4: Sample characteristics, treatment defined as 20-26 year-old

	Pre-intervention (ESAM-II)		Post-intervention (ESPS)	
	Treated 20–26 year old (1)	Control 27–39 year old (2)	Treated 20–26 year old (3)	Control 27–39 year old (4)
Employment rate	0.575	0.925	0.737	0.898
Age	23.988	32.868	24.008	32.635
	(0.415)	(0.358)	(0.333)	(0.263)
Male	0.742	0.840	0.812	0.639
Married	0.092	0.449	0.228	0.572
Relationship to household head				
Household head	0.000	0.260	0.063	0.283
Spouse	0.000	0.016	0.000	0.071
Child	0.608	0.357	0.707	0.465
Other	0.392	0.367	0.230	0.182
Industry				
Manufacturing	0.071	0.040	0.050	0.072
Services	0.143	0.218	0.374	0.247
Other	0.361	0.667	0.312	0.578
Employer				
Self-employed	0.264	0.249	0.134	0.167
Government	0.193	0.414	0.253	0.405
Company	0.118	0.262	0.350	0.326
Urbanization				
Urban Dakar	0.600	0.654	0.712	0.660
Other cities	0.118	0.202	0.141	0.245
Rural	0.281	0.145	0.147	0.096
Observations	19	115	61	443

Note: All statistics are weighted using sample weights. Married includes polygamous and monogamous marriages.

Table 5: Baseline results

	Difference-in-difference (N = 638)		Triple-difference (N = 4183)	
	No controls	With controls	No controls	With controls
	(1)	(2)	(3)	(4)
A. Preferred definition of exposure to PDEF (20–26 year old)				
Effect of PDEF	0.120**	0.090**	0.130	0.112
	(0.049)	(0.045)	(0.079)	(0.073)
	[−0.025, 0.269]	[−0.080, 0.229]	[−0.113, 0.384]	[−0.108, 0.350]
B. Alternative definitions of exposure to PDEF				
20–25 years old	0.234***	0.247***	0.317***	0.318***
	(0.068)	(0.051)	(0.102)	(0.083)
20–27 years old	0.081*	0.060	0.115	0.116
	(0.047)	(0.051)	(0.079)	(0.079)
20–28 years old	0.048	0.039	0.055	0.067
	(0.045)	(0.045)	(0.076)	(0.073)
20–29 years old	0.034	0.026	0.035	0.039
	(0.039)	(0.038)	(0.065)	(0.065)

Notes: Each cell represents marginal effects from a separate probit regression. All specifications use sample weights and include dummy variables for the post-intervention period and for the control group. In addition, columns 2 and 4 include dummies for sex, residence (rural, urban other than Dakar, reference category urban Dakar), region (10 regions, reference category Dakar), married (mono- or polygamous) and relationship to household head (spouse, child, or other, reference category head of household). Robust standard errors clustered at the region level in parentheses, biased-corrected 90 percent confidence interval (1,000 replications) in brackets. *p<.1, **p<.05, ***p<.01.

Table 6: Robustness checks

	Estimate	Observations
A. Age groups shifted by ten years (treated = 30–36, control = 37–49)	−0.015	763
	(0.030)	
B. Employment rate among the high school (HS) educated	0.023	3545
	(0.031)	
C. Labor force participation as dependent variable	−0.095	968
	(0.069)	
D. Labor force participation as dependent variable (sample including HS educated)	0.006	8271
	(0.054)	
E. University enrollment as dependent variable	0.069	968
	(0.067)	
F. University education as dependent variable (sample including HS educated)	0.037	4183
	(0.023)	

Notes: Each cell represents marginal effects from a separate probit regression similar to equation (1). The specifications in panels A, B and F are estimated among labor force participants, the other panels use all available observations. All specifications use sample weights and include dummy variables for the post-intervention period and for the control group, as well as dummies for sex, residence (rural, urban other than Dakar, reference category urban Dakar), region (10 regions, reference category Dakar), married (mono- or polygamous) and relationship to household head (spouse, child, or other, reference category head of household). Robust standard errors clustered at the region level in parentheses.

Table 7: Possible sources of employment growth

	Estimate
A. Industry (N = 573)	
Manufacturing	−0.080***
	(0.022)
Services	0.192*
	(0.113)
Other	−0.112
	(0.125)
B. Employer (N = 573)	
Self employment	−0.146
	(0.119)
Government	−0.027
	(0.195)
Private and public enterprises	0.173
	(0.108)

Notes: Each panel represents a different multinomial probit regression with dependent variable indicated in the panel title. Each cell represents the marginal effect of the reform calculated for the corresponding outcome. All specifications use sample weights and include dummy variables for the post-intervention period and the control group. Robust standard errors clustered at the region level in brackets. *p< .1, **p< .05, ***p< .01.