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Long-Term Effect of Television Usage in the Classroom: Evidence from the PETV Program in Côte d'Ivoire

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Abstract

This research examines the long-term effect of Côte d'Ivoire's *Programme d'Education Télévisuelle* (PETV), a country-wide educational television program implemented between 1971 and 1982, on education and labour market outcomes. Using data from the 2018-2019 Living Standards Measurement Study (LSMS), the research exploits the abrupt, nationwide termination of the program in 1982, to compare individuals exposed to the PETV program with those who were not. Individuals exposed to the PETV program are estimated to be 10 percentage points (approximately 0.21 standard deviations) less likely to attain lower secondary education, with no meaningful difference in upper secondary schooling and tertiary education. Heterogeneity analysis by gender reveals no differential effect on educational attainment. In the labour market, the research finds that PETV had no meaningful effect on different employment types considered. Overall, results suggest that substantial investments into the PETV program did not produce gains that stand out in comparison to traditional deliveries of education. The findings contribute to the literature on the long-term effect of educational technology in low- and middle-income countries, offering valuable insights for policymakers seeking to improve the quality of education and address educational disparities in resource-constrained environments.

Keywords: Educational Television, Long-term Effect, PETV Program, Côte d'Ivoire, RDiT

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1 Introduction

Many low-income countries face a dual challenge of expanding access to education while ensuring that students acquire meaningful skills (World Bank, 2018). In developing countries, rapid increases in enrolment have not translated into commensurate improvements in learning outcomes, partly due to severe teacher shortages and limited instructional quality. In response, governments have experimented with scalable instructional technologies, including classroom-based educational television. One of the earliest such interventions was the *Programme d'Education Télévisuelle* (PETV), implemented in Côte d'Ivoire in 1971, which broadcast lessons by high-performing teachers to primary school classrooms nationwide (Egly, 1986).

Despite its scale and technological ambition, the program was discontinued in 1982 due to fiscal and political pressures rather than any evidence of ineffectiveness (Koné & Jenkins, 1990). This makes the PETV a rare case: to our knowledge, it is the only nationally comprehensive classroom television program in Africa whose termination was driven by factors unrelated to its educational outcomes. The abrupt, nationwide end of the program in 1982 generates plausibly exogenous variation in exposure across cohorts, providing a clean institutional experiment for identifying its long-term causal effects.

Hence, we investigate the following research question: what is the long-term effect of the PETV program on learning outcomes and labour market outcomes? The long-term effect of the PETV program is theoretically ambiguous. Positive effects could emerge if televised instruction improved foundational learning and complemented the work of underqualified teachers. Negative or null effects would be consistent with implementation constraints such as linguistic diversity, limited teacher training and weak state capacity, undermining the program's pedagogical potential. The analysis of the long-term effect of the PETV program extends beyond educational attainment to encompass labour market outcomes. From a human capital perspective, investments in education can shape individuals' occupational trajectories by increasing skills, productivity and access to higher-quality jobs over the life cycle (Becker, 1964; Mincer, 1974). The empirical literature documents positive returns to education especially in low and middle income countries where schooling is associated with higher earnings than in high income countries (Montenegro & Patrinos, 2014).

To assess the long-term effect of the PETV program, this research mainly uses data from the 2018-2019 Living Standards Measurement Study (LSMS). This dataset provides information on educational and labour market outcomes in Côte d'Ivoire for the first and second generations of individuals exposed to the program. The empirical analysis employs a regression discontinuity in time (RDiT) design to estimate the effect of the program. This approach exploits the fact that

the implementation of the PETV program was terminated nationwide in 1982, by comparing individuals who started primary school before and after this cutoff.

Regression results show that individuals exposed to the PETV program are less likely to attain lower secondary education, reducing the likelihood of attaining lower secondary education by about 0.21 standard deviations. The effect on upper secondary schooling and tertiary attainment is negligible. For labour market outcomes, consistently show no meaningful effect on high-skill occupation, self-employment or government employment. Robustness checks using narrower bandwidths confirm these patterns, though estimates become less precise. Placebo tests at false cutoffs strengthen confidence in the validity of the main estimates. "Donut" regressions (Hausman and Rapson, 2018), comparing individuals away from the cutoff, reinforce the plausibility of a negative effect of the PETV on education outcomes, with limited effects on labour market outcomes. Heterogeneity analysis by gender shows that the program had no differential effect on educational attainment for boys and girls, but reveals a gender-specific pattern in labour market outcomes, with exposed females more likely to access government employment relative to exposed males. Finally, second-generation results show no evidence that parents' exposure to PETV affected their children's likelihood of attending formal education.

Overall, the findings imply that the substantial resources invested in the initiative did not generate gains superior to conventional education systems of the 1970s and 1980s, in contrast to findings from similar programs implemented outside the continent, which are discussed in the next paragraph. However, the estimated negative effect on secondary education should be nuanced. A likely explanation is an exam mismatch, whereby the skills developed through PETV did not align well with the content or format of certification exams governing progression to secondary education. As a result, students may not have been adequately prepared to succeed in these high-stakes assessments, despite potential improvements in underlying learning. This mechanism has been documented in the literature (Crépeau, 1982). At the same time, during the PETV period, education spending expanded substantially, raising the possibility of internal displacement within the sector. Even without direct diversion of funds from secondary to primary education, resources devoted to PETV could otherwise have been used to expand secondary schooling. Since secondary education lacked sufficient places to meet increased demand during the PETV period, part of the negative effect on lower secondary attainment may also reflect binding capacity constraints.

This research contributes to the growing body of literature on the long-term effects of using educational technology (edtech) in classrooms to improve teaching quality and reduce rural-urban disparities in low- and middle-income countries (LMICs).

In Mexico, Fabregas & Navarro-Sola (2025) investigate the long-term effect of the *Telesecundarias* program, which has been broadcasting secondary school lessons via television to remote and underserved areas since 1968. The study examines the long-term education and

labour market effects of this program, finding that students exposed to Telesecundarias were 8 percentage points more likely to graduate from secondary school, had an additional 0.4 years of education on average and experienced an 8% increase in hourly earnings. These findings suggest that television-based educational interventions can have significant positive effects on educational attainment and labour market outcomes, particularly for students in rural areas where traditional schooling options may be limited[1].

It is worth noting key similarities and differences between the Telesecundarias and the PETV programs, the latter having implications for generalizability of results. In terms of implementation, Telesecundarias share several key features with the PETV: lessons are delivered via television broadcast to a classroom where a local generalist teacher is present, the programs target underserved rural communities facing teacher shortages, and both were scaled nationally. However, two institutional differences are worth noting. First, while Telesecundarias targeted secondary school students (grades 7-9), the PETV was a primary education intervention (grades 1-6), a distinction that matters because returns to education differ by the level of education[2]. A second difference is state capacity. The PETV was heavily reliant on foreign personnel to compensate for limited local institutional capacity, a constraint that Mexico did not seem to face to the same extent when implementing Telesecundarias. Naturally, large-scale programs requiring national coordination in content production, equipment distribution, teacher training and simultaneous cross-country broadcast logistics are sensitive to such capacity limitations.

In the context of China, Bianchi et al. (2022) conducted a large-scale study by on the long-term effects of a computer-aided learning (CAL) program - combining television and computers (the PETV program relied exclusively on television) – which provides further evidence of the potential benefits of educational technology. The authors find that exposure to the CAL program improved students' academic achievement, labour market performance and computer usage up to ten years after the intervention. The program, which connected China's best teachers to rural students via satellite, also helped reduce the rural-urban education gap. These findings highlight the potential for low-cost, television-based interventions that are relatively adapted to LMICs to yield long-term educational and labour market benefits.

In the African context, between the mid-1960s and early 1980s in Africa, educational television initiatives were introduced in Côte d'Ivoire, Niger and Senegal (Egly, 1986). These programs were driven by a common goal: to improve educational systems. Côte d'Ivoire's PETV

[1] In contrast to the long-run labor market effects studied by Fabregas & Navarro-Sola (2025), Borghesan & Vasey (2024) analyze the short term effect of attending Telesecundarias on students' math and Spanish test scores, after one year of exposure. Using a marginal treatment effects framework, they find that attending a Telesecundaria results in significant improvements in academic performance. The authors conclude that Telesecundarias are not necessarily inferior to traditional schools, as is often assumed, and that they can be an effective tool for reducing educational inequality.

[2] Returns to secondary schooling tend to be lower than those to primary and tertiary education (Montenegro & Patrinos, 2014).

was the largest of these experiments, covering the entire country and reaching nearly 900 000 students. In contrast, the educational television programs in Niger and Senegal were more localized and experimental in scope. In addition to the historical experiments, there have been some recent efforts to use television in African classrooms, although these have generally been smaller in scale. For instance, in Ethiopia, the government launched a nationwide Satellite Plasma TV Project in 2003 to improve secondary education. The project aimed to overcome the shortage of qualified teachers by broadcasting pre-recorded lessons via plasma screens, with local teachers facilitating classroom discussions. While the project initially seemed promising, studies have found that it faced significant challenges, including a lack of stakeholder consultation and language barriers (Barasa, 2021). In Ghana, a recent, lower-scale, interactive satellite-transmitted instruction program tested the use of live, two-way videoconferencing to connect 70 rural schools with teachers in the capital, Accra. The program, which targeted math and English instruction, demonstrated significant improvements in student performance. However, the program faced high implementation costs, raising questions about its scalability and cost-effectiveness (Johnston & Ksoll, 2022). Rodriguez-Segura (2022) provides a systematic review of EdTech interventions in developing countries, including Africa, finding that instruction-focused and self-led learning technologies yield the largest learning gains. Notably, however, rigorous evidence from Africa remains scarce in this literature and the PETV program examined here fills this gap, representing one of the earliest and largest-scale instructional television initiatives on the continent.

It is important to distinguish between edutainment programs broadcasted at home, such as Sesame Street-type shows in countries like South Africa, Nigeria and Tanzania (Mares & Pan, 2013), and classroom-based TV instruction. During the COVID-19 pandemic, there was a global surge in the use of ICT in education, particularly radio and television in the context of Africa (Barasa, 2021), but these at-home broadcasts are not the same as in-class TV lessons. At home, children are learning quite independently, at least without the presence of peers or a teacher. In contrast, when television is used in the classroom, the teacher plays a critical role in maintaining discipline, explaining the material and facilitating discussions, echoing broader findings that teacher engagement is a key mediating factor in the effectiveness of educational technology (Di Pietro & Castaño Muñoz, 2025). Evidence from Brazil during the COVID-19 pandemic illustrates the risks of unstructured remote learning. Lichand et al. (2022) document dramatic learning losses and a sharp rise in dropout risk when students were shifted to remote instruction without adequate support. In contrast, the PETV program combined televised instruction with in-person classroom facilitation. This distinction is important, as Beg et al. (2022) show through two RCTs in Pakistani middle schools. The research finds that integrating brief, expert-led, curriculum-based videos into classrooms improved math and science test scores by 0.3 standard deviations, among the highest impacts of education interventions in the literature (Angrist et al, 2025), after four months of exposure, and student and teacher attendance as well as exam pass

rates improved. However, providing similar content on personal tablets decreased test scores by 0.4 standard deviations, underscoring the critical role of teacher engagement in leveraging educational technologies effectively.

Overall, relative to the existing African evidence, this study makes several contributions. First, it provides evidence on the long-term effects of a large-scale national television education program using a quasi-experimental design, whereas existing studies focus on short-run outcomes and smaller-scale interventions. Second, the PETV program differs from other African initiatives in its scale and technological simplicity, relying on nationwide broadcast of recorded lessons rather than more complex or interactive technologies such as the Ghanaian intervention. Third, unlike edutainment programs such as Sesame Street, PETV was embedded within the formal school curriculum, allowing us to assess its effect within the core education system.

While the positive findings from Mexico, Pakistan and China establish that television-based instruction can yield meaningful gains in education and in the labour market, several contextual differences limit how directly these results generalise to Côte d'Ivoire. Mexico, Pakistan and China implemented their programs within relatively more developed institutions and in a different linguistic environment. The heavy dependence of the PETV on foreign personnel raises questions about local ownership and sustainability that did not characterise Mexican, Pakistani and Chinese programs to the same degree. In addition, Côte d'Ivoire in the 1970s faced over sixty spoken languages and a severely underqualified teaching workforce, conditions that likely undermined the effectiveness of French-language televised content at the classroom level (De Landsheere, 1981; Crépeau, 1975). Taken together, these differences suggest that the institutional fragility related to the human capital necessary to manage such a program, and linguistic diversity, make Côte d'Ivoire a relatively harder environment for educational television to succeed. Therefore, the results from the PETV should be interpreted as reflective of these contextual factors rather than as a simple contradiction of international evidence.

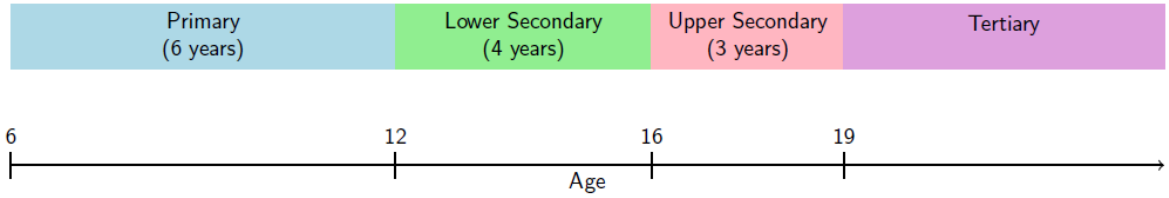
With the increasing use of information and communication technology (ICT) in education, particularly in response to the COVID-19 pandemic (Barasa, 2021), this research revisits a major television education program that deserves the attention of policymakers interested in televised instruction and, more broadly, in edtech. The findings from this study provide an informative comparison with cases where positive effects have been documented.

The rest of the paper is structured as follows. Section 2 provides a detailed background on the PETV program, including its origins, objectives and implementation. Section 3 presents the empirical strategy, followed by a description of the data in Section 4. Section 5 presents the main results, robustness checks, heterogeneity analyses, and second-generation effects. Section 6 concludes.

2 The PETV program: context and implementation

The education system of Côte d'Ivoire is structured into four main levels, which still exist today.[3] Primary education lasts six years, covering ages 6 to 12. It is followed by lower secondary education, which spans four years, from age 12 to 16. Upper secondary education then extends over three years, from age 16 to 19. After completing upper secondary schooling, students proceed to tertiary education (Figure 1).

Figure 1: Structure of education in Côte d'Ivoire



Notes: The figure shows that education in Côte d'Ivoire follows a 6-4-3 structure: six years of primary education, four years of lower secondary, and three years of upper secondary, before entry into tertiary education. *Source* : Adapted from UNESCO, UIS (2025).

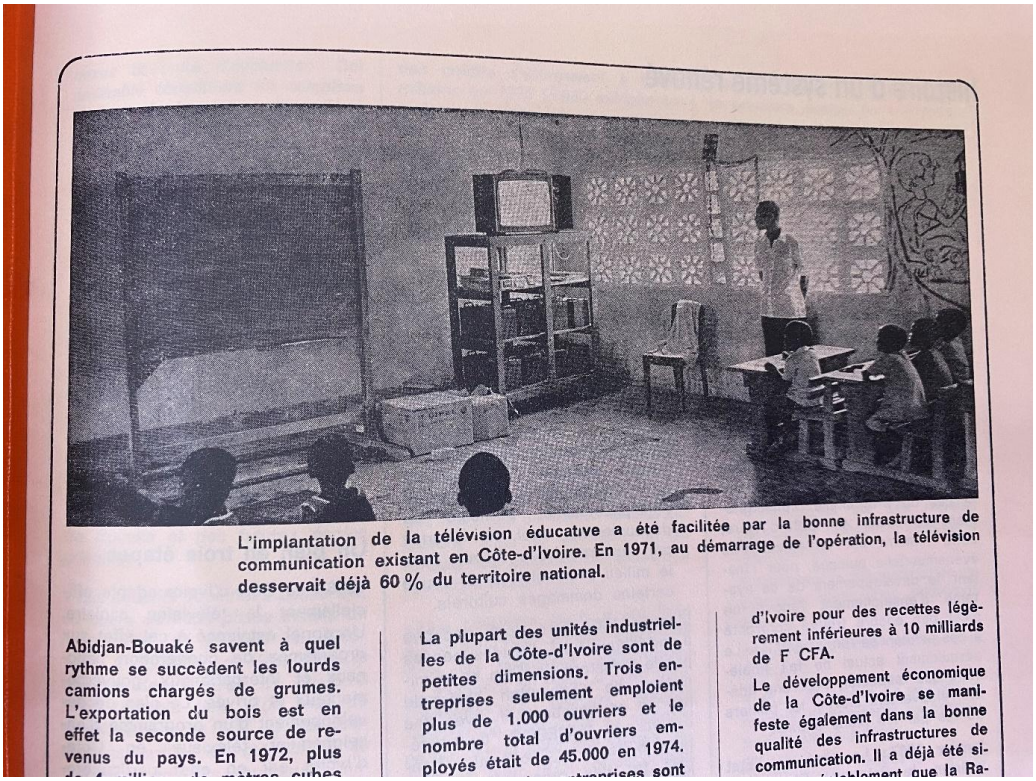
The *Programme d'Education Télévisuelle* (PETV) was a large-scale educational television intervention implemented in primary education in Côte d'Ivoire between 1971 and 1982. The PETV aimed to improve primary education by integrating televised lessons in classrooms across the country, as illustrated in **Figure 2**. Although it was eventually abandoned after a decade of implementation, at least from its scale, the PETV program remains a unique example of an attempt to leverage technology for education in the African context.

At the time PETV implementation, education suffered from numerous divides: North/South, with the poorer North having much lower enrolment rates than the South; urban/rural; girls/boys (De Landsheere, 1981). In addition, the education system faced significant linguistic inequalities, as more than sixty languages were spoken and teachers' proficiency in French varied considerably, alongside substantial differences in the quality of instruction across schools.

In the 1960s, the Ivorian education system faced a significant shortage of qualified teachers (Crépeau, 1975). In 1964, only 9.5% of teachers held the title of *instituteur*, having completed lower secondary education, 52% were assistant teachers (*instituteurs-adjoints*) with only an elementary teaching aptitude certificate, 27% were monitors and 11% were assistant monitors. With a primary school enrolment rate of 55% in 1970 (WDI, 2024), Côte d'Ivoire set an ambitious goal of achieving universal schooling by 1980-1985 (Crépeau, 1975). To address the

[3] In addition to pre-primary education.

Figure 2: Television usage in a classroom in Côte d'Ivoire for the PETV program



L'implantation de la télévision éducative a été facilitée par la bonne infrastructure de communication existant en Côte-d'Ivoire. En 1971, au démarrage de l'opération, la télévision desservait déjà 60 % du territoire national.

Abidjan-Bouaké savent à quel rythme se succèdent les lourds camions chargés de grumes. L'exportation du bois est en effet la seconde source de revenus du pays. En 1972, plus

La plupart des unités industrielles de la Côte-d'Ivoire sont de petites dimensions. Trois entreprises seulement emploient plus de 1.000 ouvriers et le nombre total d'ouvriers employés était de 45.000 en 1974.

d'Ivoire pour des recettes légèrement inférieures à 10 milliards de F CFA.

Le développement économique de la Côte-d'Ivoire se manifeste également dans la bonne qualité des infrastructures de communication. Il a déjà été si-

Notes: Source: Crépeau (1975)

teacher quality issue, the country leveraged regular television broadcasting to provide training for educators.

Origins and Objectives of PETV program

The PETV program was conceived at a time when Côte d'Ivoire, like many newly independent African nations, was grappling with the challenge of building an education system accessible to the masses. In response to the challenges of low primary enrolment and underqualified teachers, the Ivorian government, with significant foreign assistance, launched the PETV program in 1971, after three years of preparation. The objectives of the program were to (1) to standardize education across Côte d'Ivoire by reducing disparities between the North and South, urban and rural areas, and between boys and girls, (2) to address the critical shortage of teacher training, providing a solution to the lack of adequately prepared educators, (3) to improve educational outcomes through the modernization of curricula and teaching methods, notably by modernizing mathematics teaching, increasing the time dedicated to oral expression in French and introducing a new course called *Etude du milieu* (the study of the environment), a combination of history, geography, natural sciences and civic education (De Landsheere, 1981). French was not only the language of instruction but was also viewed as an essential tool for integrating Côte d'Ivoire into the global economy. Hence, in addition to addressing educational challenges,

Koné & Jenkins (1990) suggest that the PETV program implicitly aimed to consolidate French cultural and economic influence in the post-colonial period.

Implementation and Foreign Involvement

The PETV program was implemented by the Ministry of Education of Côte d'Ivoire, which created a specialized department responsible for primary and television education. However, from the outset, the program was heavily dependent on foreign expertise while funding was mainly borne by the Ivorian government since the beginning of implementation in 1971. The French played the main role in staffing and managing the program. For instance, in 1977, close to 90% of the program's foreign personnel were French nationals. Other countries, including Canada, Belgium, West Germany and the United States contributed technical and financial assistance, while international organizations like United Nations Educational, Scientific and Cultural Organization (UNESCO), the World Bank, the International Labour Organization (ILO) and the United Nations Development Programme (UNDP) were actively involved in providing additional support. The World Bank, in particular, funded the Bouaké Complex, which served as the production facility for the educational content used in the televised lessons, employing around 500 persons (Egly, 1984). Specifically, the Complex was responsible for producing films and videos that were used for classroom instruction. The videos were recorded in a studio at the Bouaké Complex while films, which were relatively expensive, were shot in the studio and/or outside.

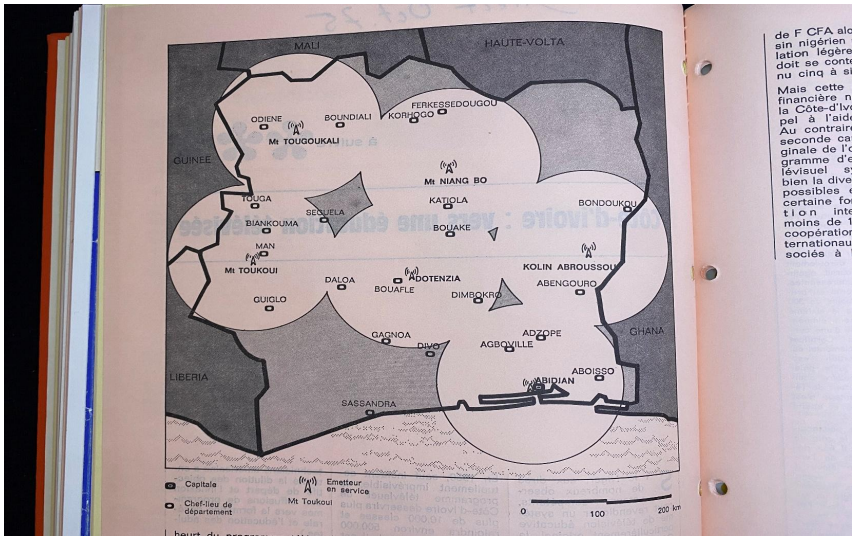
Gradual coverage of the PETV program

The PETV program was implemented across Côte d'Ivoire gradually, expanding its reach over the course of the decade of implementation. When the program launched in 1971, the television broadcasting network covered approximately two-thirds of the country and served around three-quarters of the population, as shown in **Figure 3**. By 1973, 28% of primary school classrooms were equipped with black-and-white televisions for viewing the educational broadcasts (Koné & Jenkins, 1990).

After four years of experimenting with the innovation in the field, the PETV program extended in 1975 to all first-year primary classrooms. The program was practically generalized in 1978 given that close to 80% of schools were within the PETV broadcast coverage zone (Crépeau, 1982). By 1980, the PETV program had expanded to cover 88% of the Ivorian population, reaching over 15 635 classrooms and more than 651 743 students (Egly, 1984). This gradual implementation allowed the program to scale up while working through logistical challenges such as providing specially designed television sets and addressing the issue of power supply in remote areas through the use of alkaline batteries or solar-powered ones. It should be noted that private schools were not covered by the program. The share of children in private primary schools was estimated at approximately 23% in 1970 and 14% in 1981 (Egly, 1984).

Lesson sequences

Figure 3: Television coverage during PETV implementation



: Transmitter in service : Department capital

Notes: **Source:** Crépeau (1975).

The PETV program's teaching sequences were aired in short segments throughout the school day for lessons intended for students and at the end of the day for teacher training sessions (Crépeau, 1982).

For students, the lessons were divided into short segments. Each broadcast was about 10 minutes long. These sequences were broadcast throughout the school day, with multiple time slots: from 8:00 to 10:00 AM, 10:15 to 12:00 PM, 3:00 to 4:00 PM and again from 4:15 to 5:00 PM, four days a week, plus a session on Saturday mornings. The lessons covered all subjects including French, mathematics and "the study of the environment" (*Etude du Milieu*).

For teachers, there were dedicated sequences aimed at helping them prepare lessons but also to train them. A 30 minute broadcast, called the "Journal du Maître," was aired at the end of the school day, four days a week. This program discussed the educational broadcasts planned for the following day and offered guidance on how to integrate them into the classroom teaching. In addition, teachers had access to a 90 minute weekly program, broadcast on Thursday mornings, which alternated between mathematics and *Etude du Milieu*.

Both teachers and students were given accompanying printed materials to complement the broadcasts.

Immediate Successes of the Program

In terms of immediate educational outcomes, the PETV program appears to have contributed to significant changes. Between 1970 and 1980, the number of primary school students increased sixfold, rising from 135 000 to 900 000 by 1982, representing 75% of the school-aged population (Crépeau, 1982). Moreover, the proportion of female students increased from 26% in 1970 to 40% in 1982, reflecting notable success in bridging gender disparities in education.

A descriptive evaluation conducted at the end of the program highlighted notable improvements in educational outcomes (De Landsheere, 1981). The PETV program appears to have contributed to reduced grade repetition and improved retention. For example, between 1966 and 1970, only 9.3% of students reached the last year of primary school without repeating a year. This percentage rose to 53.5% between 1971 and 1975. However, this progress was partially attributed to the introduction of automatic promotion[4] policies rather than solely to the effectiveness of televised lessons. PETV also appeared to contribute to reducing the urban-rural education gap. Only 7% of performance variation among first-year primary students was attributable in 1980 to differences between schools, a proportion comparable to that of Sweden, indicating a high level of homogeneity between schools.

The PETV provided an effective way for children to learn French, the language of instruction, in regions where teacher quality was a significant barrier. This likely explains why comparisons between PETV and non-PETV students showed superior performance by the former in French oral expression, reading and writing, with no significant differences in mathematics. Positive results were also documented in spontaneity and inquiry, skills that were not appreciated by traditionalist parents but which may have positively impacted the students' labour market outcomes.

Challenges and Criticisms

Despite these successes, the PETV program faced substantial resistance from various segments of Ivorian society, which ultimately contributed to its downfall. First, the program was perceived as too costly, as the Ivorian government gradually took on a larger share of the financial burden, increasing its contribution from less than a quarter of the total cost at the program's outset to around 90% by the end (Koné & Jenkins, 1990), while management largely remained in the hands of foreigners. The education ministry's budget share increased from 18% in 1975 to 40% in 1980, rising from 3% of GDP in 1965 to 9% in 1982 (Crépeau, 1982). Primary education received around 45% of this budget, with PETV accounting for roughly half (See **Table A 1**).

One reason the program's heavy reliance on foreign personnel became a source of contention relates to the timing of implementation. In the years following independence in 1960, there was growing suspicion of foreign influence, particularly from France, the former colonial power. Many Ivorians felt that the program represented a form of neo-colonialism, with rumors circulating that the country's children were being used as "guinea pigs" for low-quality educational experiments (Koné & Jenkins, 1990). Additionally, the fact that the televisions used in the program were black and white, at a time when color television technology was available, only fueled these suspicions.

[4] Historical accounts indicate that automatic promotion began during the period of PETV implementation, without specifying a precise year. However, this does not pose a problem for identification, as the policy remained in place after the end of the PETV program.

Teachers and parents, in particular, expressed dissatisfaction with the televised lessons. Teachers complained that the broadcasts moved too quickly, preventing them from thoroughly explaining the material or addressing students' difficulties. Perhaps more critical were parents' complaints. For them, primary education was expected to lead to secondary education, and then to jobs outside of agriculture. However, only 25% to 35% of PETV students were admitted to secondary school, in part because the entrance exam did not account for certain skills acquired in the PETV program (Crépeau, 1982), but also because the secondary education system did not have enough places to meet the demand, despite the barrier posed by the entrance exam (Egly, 1984). As a result, each year, around 110 000 students were completing primary education with no hope of entering secondary school.

End of the Program

The combination of high costs, foreign dependency and domestic opposition eventually led to the official abandonment of the PETV program in 1982 (Koné & Jenkins, 1990). The program is a prime example of projects that are abandoned, not because they don't work, but mainly because of implementation challenges. Its ambitious scope seems unique on the African continent, which calls for an investigation of its long-term effect in order to inform educational policy in developing countries.

3 Empirical strategy

The empirical strategy exploits the termination of the PETV program in 1982 to estimate its effect on later-life outcomes. Since the program was implemented nationally between 1975 and 1982 and then officially discontinued, exposure to PETV varies depending on the year in which individuals entered primary school. The nationwide termination of the program naturally lends itself to a regression discontinuity in time (RDiT) design, where the running variable is the year an individual began primary school and the cutoff corresponds to the program's termination (see Hausman and Rapson, 2018)[5]. Compared to a traditional RD, an RDiT design cautions against using observations that are too far from the cutoff and requires careful consideration of potential issues related to autoregression in the outcome variable.

Individuals who started primary school before 1982 were potentially exposed to PETV, whereas those who started school after 1982 were not[6]. The year when an individual started primary school is inferred from a combination of the information of when they reported to have started school and the year of birth. If they started school at 6 years or older, the individual is considered to have started primary education at that age. It should be noted that pre-primary

[5] This RDiT is similar to RD designs involving age as a running variable (See Lee and Lemieux, 2010).

[6] The PETV program ended mid-1982, at the close of the school year. Individuals who started primary school in 1982 were therefore not exposed.

school enrolment was less than 1% before 1988 (WDI, 2025). Since the data used in this study was collected 30-44 years after the relevant school entry decisions, recall error may introduce measurement error in the running variable. Classical measurement error would attenuate the estimated discontinuity toward zero, suggesting that estimates represent a lower bound on the true effect. Crucially, for such error to threaten identification, it would need to be systematically concentrated at the 1982 cutoff, requiring respondents to misreport their school-start year in a discontinuous manner around that threshold. This is implausible given that respondents had no reason to be aware of the analytical relevance of 1982.

Based on historical accounts discussed in Section 3, the analysis restricts the exposure window to 1975-1981 for treated cohorts and 1982-1988 for untreated cohorts. This symmetric seven-year period, which I reduce in robustness checks, addresses the need for comparability of adjacent cohorts in a bandwidth which is not too wide (Hausman and Rapson, 2018). The choice of a symmetric seven-year window around the 1982 cutoff reflects a trade-off between including sufficient observations for statistical power and maintaining comparability of cohorts close to the threshold[7]. Earlier cohorts than 1975 are excluded to avoid periods before the full implementation of the PETV program, and using a symmetric window ensures balanced comparisons on both sides of the cutoff.

Compared to a traditional RD, an RDiT design raises specific concerns related to serial correlation in the outcome variable across adjacent cohorts (Hausman and Rapson, 2018). In this context, outcomes for individuals who start school in nearby years may be correlated due to shared macroeconomic conditions, education policies, or other cohort-level shocks, which could bias inference if not properly accounted for. In this setting, however, this concern is mitigated by the nature of both the data and the outcomes considered. First, the outcomes are measured at the individual level in cross-section many years after exposure, rather than as a time series observed repeatedly over time. Second, the outcomes (such as educational attainment) do not follow a dynamic process in which past realizations mechanically determine current outcomes. Instead, they reflect discrete transitions completed long after the treatment period. Finally, inference is conducted with standard errors clustered at the cohort level, which accounts for potential within-cohort correlation. Taken together, these features make serial correlation unlikely to pose a threat to identification in this context.

Assignment to treatment is not perfectly sharp. Although the probability of exposure increases as cohorts approach the 1982 cutoff from below, it does not reach one because, in practice, not all schools were equipped with television sets, and some cohorts may have experienced only partial exposure. As a result, the empirical design is best interpreted as a fuzzy RDiT. In

[7] This choice also limits potential contamination from broader changes in the education system during the late 1980s and early 1990s, a period marked by fiscal adjustments discussed below. While cohort trends absorb gradual changes over time, the restricted window further reduces exposure to broader structural changes that may not be well captured by parametric controls.

the absence of individual-level data on actual exposure, I estimate reduced-form effects corresponding to the intention-to-treat estimate of PETV exposure based on cohort. These estimates therefore capture the effect of program availability rather than the effect on individuals who were exposed. Consequently, the estimated coefficients may understate the magnitude of the effect on treated students. If individual-level PETV coverage data were available, a fuzzy RDiT with an IV approach could recover the Local Average Treatment Effect (LATE), i.e. the effect among those schools and students who were actually exposed.

The empirical model estimates the change in outcomes at the 1982 cutoff using the following specification:

$$y_i = \alpha + \rho T_i + \gamma \, School\ start\ year_i + \delta \, T_i \times School\ start\ year_i + X_i\beta + \mu_k + \varepsilon_i \quad (1)$$

$$\text{with } T_i = \begin{cases} = 1 \ if \ 1975 \leq School\ start\ year_i < 1982 \\ = 0 \ if \ 1982 \leq School\ start\ year_i \leq 1988 \end{cases}$$

y_i captures education and labour market outcomes. T_i is an indicator of potential exposure to the program, i.e. a dummy which equals 1 if an individual started primary school before 1982 (when PETV was being implemented) and 0 if they started school after 1982 (when the program had officially ended). *School start year* is a running variable that captures the year the individual began primary school, centered at 1982 to facilitate coefficient interpretation. In the baseline specification, I do not include the interaction term $T_i \times School\ start\ year_i$, and include it in the full specification to allow different cohort trends before and after 1982[8]. X is a vector of control variables measured at the individual, household or locality level. μ_k represents regional fixed effects and ε_i is a stochastic error term. Standard errors are clustered at the cohort level (School-start year) to address the problem of serial dependence in the residuals[9]. The parameter of interest is ρ, which represents the reduced form (intent-to-treat) treatment effect of the PETV program.

Identification relies on the assumption that, in the absence of PETV termination, potential outcomes would have evolved smoothly across the 1982 cutoff. In the context of this RDiT design, this assumption requires that, absent the termination of PETV, adjacent cohorts would have experienced similar underlying trends in educational and labour market outcomes, without any other policy change or macroeconomic shock generating a discrete break at the 1982 cutoff. While gradual changes in economic conditions may affect outcomes over time, they do

[8] Results are similar when we allow for quadratic cohort trends.

[9] Since the running variable takes only 14 distinct values, cohort-level clustering yields a small number of clusters. We therefore complement our clustered standard errors with wild cluster bootstrap p-values (Cameron, Gelbach, and Miller, 2008), which are more reliable in settings with few clusters.

not threaten identification as long as they do not induce a sharp discontinuity precisely at the threshold.

Côte d'Ivoire experienced a period of economic stagnation in the 1980s (**Figure A 1**) following the collapse of cocoa and coffee prices, which led to the implementation of structural adjustment programmes (SAPs) supported by the IMF and the World Bank. These reforms involved fiscal consolidation and reductions in public expenditure, including in the education sector (World Bank, 1994; Berthélemy and Bourguignon, 1996). However, historical analyses emphasize that both the economic crisis and the associated policy adjustments unfolded progressively over several years rather than as a discrete shock coinciding with PETV termination. In particular, fiscal tightening and sectoral reforms were implemented gradually throughout the 1980s, with no clear evidence of a sharp policy break coinciding exactly with the termination of the PETV programme. As such, while SAPs may have contributed to broader cohort trends in educational attainment, they are unlikely to generate a discontinuity at the 1982 cutoff. This supports the plausibility of the smoothness assumption underlying the RDiT design. To further mitigate this concern, the empirical specification flexibly controls for cohort trends, and the results remain robust to allowing for non-linear (quadratic) cohort dynamics.

A key assumption of the RDiT design is that individuals cannot precisely manipulate the running variable, here the year of school entry, around the cutoff. While the calendar time itself cannot be manipulated, behavioural responses may arise if the termination of PETV was anticipated. In particular, parents could have delayed or advanced school entry in response to expectations about the programme's continuation. To assess this concern, I examine the distribution of age at school entry around the cutoff and find no evidence of discontinuity (See **Figure 4**, Balance of Year of Birth variable). This suggests that families did not systematically adjust school entry timing in response to the programme's termination.

I further assess the plausibility of the identification strategy by examining the continuity of predetermined covariates around the cutoff. For this purpose, I focus on characteristics that are fixed prior to treatment and cannot be affected by PETV exposure : gender, year of birth, year of birth, religion, ethnicity and nationality. The results, presented in **Table A 3**, show no evidence of discontinuities in these variables at the cutoff, supporting the absence of compositional changes across adjacent cohorts.

A potential concern in a regression discontinuity design is differential cohort composition around the cutoff. Unlike standard cross-sectional RD settings, individuals cannot manipulate their school-start year, but compositional changes across cohorts may still arise over time. To assess this, I examine whether the number of observations varies discontinuously at the 1982 cutoff.

An additional concern is that parents who opposed the PETV, mainly urban parents from the bourgeoisie (Egly, 1984), might have enrolled their children in private schools. However,

this does not appear to be a major issue, as the share of children in private primary schools actually decreased during the PETV implementation period, falling from approximately 23% in 1970 to 14% in 1981, which aligns with the surge in public primary school enrolment during that time - primary school enrolment increased from 55% in 1955 to 79% in 1981 (WDI, 2024). Nevertheless, controlling for urban/rural area of residence should partially account for this effect.

Given the program's national scale, individuals in untreated cohorts may still have been indirectly affected, for example, through older siblings or neighbours exposed to PETV, or through increased teacher quality due to the program. Such indirect effects imply that the estimates should be interpreted as reduced-form effects that may combine direct exposure and indirect channels, rather than isolating the pure instructional effect of PETV. However, these indirect effects do not threaten the validity of the identification strategy.

An important feature of the PETV program is that it may have affected not only learning outcomes but also the allocation of education resources across levels. As stated earlier, improvements in primary education driven by the PETV program increased demand for secondary education without a corresponding increase in available places. This creates the possibility of a resource displacement mechanism, whereby increased investment in primary education is associated with limited expansion, or even relative contraction, of secondary school supply. In such a setting, observed educational attainment outcomes reflect both the direct effects of instructional improvements and contemporaneous changes in access to higher levels of education.

The main specifications are estimated using a Linear Probability Model (LPM), given the binary nature of the outcome variables. The LPM offers a straightforward interpretation of coefficients as changes in probability and is standard practice in regression discontinuity settings, where it provides consistent estimates of local average treatment effects. While non-linear models such as logit or probit impose functional form assumptions, I verify the robustness of the results by estimating these alternative specifications and reporting marginal effects at the cutoff.

I implement several other robustness checks. I verify that cohort size evolves smoothly around the 1982 cutoff, consider narrower bandwidths and placebo cutoffs. I also estimate a donut regression excluding cohorts closest to the cutoff, explore the local randomization approach, re-estimate the main specifications using survey sampling weights, and augment the baseline specification with macroeconomic controls. Together, these checks ensure that any estimated discontinuity reflects the effect of PETV exposure.

4 Data

4.1 Data description

The empirical analysis primarily relies on the Living Standards Measurement Study (LSMS) 2018-2019 conducted by the Institut National de la Statistique (INS) of Côte d'Ivoire, with support from the World Bank and the West Africa Economic Monetary Union (WAEMU) Commission. The survey is nationally representative with almost 13 000 households across all regions, in both urban and rural areas. The household questionnaire essentially gathers information on demographics, education, health, employment, income sources, consumption, assets and shocks. Education is measured at the individual level through indicators such as school attendance, age at school entry, type of schooling (formal and non-formal) and academic achievement. Labour market outcomes are captured in the employment module, which includes records of individuals' activity status over the previous seven days, and characteristics of primary and secondary employment over the past 12 months, including sector of activity, occupation and employment status. These modules allow us to construct the main variables of interest, namely educational attainment and labour market outcomes, using nationally representative microdata. Below, I explain the rationale for selecting this survey data to track long-term effects on outcomes of interest in this study, i.e. education and labour market outcomes.

If I consider for instance, individuals who began primary school in 1975, assuming they did not repeat any school years[10], they would have completed secondary school by 1988, given that primary school in Côte d'Ivoire lasts six years and secondary school seven years. Since enrolment in post-secondary education was very low at the time[11], the vast majority of those who "finished" school in 1988 would have entered the workforce. For individuals who repeated one or more grades, their school completion would have occurred a few years later than 1988. For all these individuals, I capture information on their labour market situation after 1988.

The earliest nationwide survey data containing all the information needed to capture both education and labour market outcomes is the 2018-2019 LSMS survey. The study focuses on the specific people who were in primary school around 1982, resulting in a sample of 2955 individuals.

A particularly valuable aspect of this data is that it enables us to capture first-generation and second-generation outcomes. For example, an individual who started primary school in 1975 and was born six years earlier would be 49 years old in 2018. This means they would have

[10] Approximately 22% of students repeated primary school between 1975 and 1985 (WDI, 2024).

[11] Gross enrolment in tertiary education was estimated at 2.6% in 1992 (WDI, 2024).

had time to have children, allowing us to observe not only their education and labour market outcomes but also their children's education[12].

5 Summary statistics

Table 1 shows outcomes used in this study, i.e. three education outcomes and three labour market outcomes. The education outcomes include achievement of lower secondary education, achievement of upper secondary education and achievement of tertiary education.

Table 1: Outcome and control variables

Variable	Obs	Mean	Std. dev.	Min	Max
Outcome variables					
Lower secondary	2,948	0.490	0.500	0	1
Upper secondary	2,948	0.244	0.430	0	1
Tertiary	2,948	0.076	0.265	0	1
High-skill	2,955	0.125	0.331	0	1
Work for own account	2,955	0.275	0.447	0	1
Work for the government	2,955	0.269	0.443	0	1
Control variables					
Gender (=1 if male)	2,955	0.646	0.478	0	1
Year of birth	2,955	1975.747	4.218	1960	1985
Religion	2,955	2.040	1.013	1	5
Nationality	2,955	3.053	0.893	1	11
Ethnicity	2,668	27.107	21.878	1	71
Father's education	2,768	1.677	1.156	1	5
Mother's education	2,666	1.229	0.634	1	5
Health (Hospitalization Prev 30 days)	2,955	1.691	0.462	1	2
Rural/Urban	2,955	1.487	0.500	1	2
Region	2,955	13.963	10.552	1	33

Notes: This table presents summary statistics for the dependent and control variables used in the analysis. The dependent variables are binary indicators of educational attainment and labour market outcomes, while the controls include demographic characteristics, parental background, health status, and geographic variables. Differences in the number of observations across variables reflect missing values. *Data source*: LSMS 2018-2019

The labour market outcomes focus on the type of employment individuals hold. In many African contexts, including the sample used here, most adults have a job. Specifically, about

[12] Other candidates for datasets exploitable for this study are Demographic and Health Survey data collected in 1994 and 1998-1999 but these do not contain information on the year an individual started primary education, a critical variable for the identification strategy we use in this study. Furthermore, second-generation effects cannot be captured using data collected in the 1990s.

98% of the adults in our sample are employed. Therefore, the key labour market question is not about employment status but rather the nature of the jobs people occupy. One labour market indicator is whether an individual works in a high-skill or low-skill occupation. Following the International Standard Classification of Occupations (ISCO-08), which defines skill levels on the basis of the complexity of tasks performed and the level of formal education typically required (ILO, 2012), I classify the first three occupational categories in the survey (Intellectual and scientific professions, Intermediate professions, and Administrative employees – managers) as high-skill, corresponding to ISCO skill levels 3 and 4. The remaining categories, i.e. Farmers and skilled agricultural workers, Craftsmen and skilled craft workers, Plant and machinery operators, Unskilled workers and employees, Army and security, Other trades and professions, and No profession and unspecified profession, are classified as low-skill. **Table A 2** provides the full distribution of occupations in the sample. The other labour market indicators are: working for one's own account and working for the government.

Table 1 also provides summary statistics of the control variables used in the empirical analysis. These variables are drawn from the LSMS dataset and are included both because they are consistently available for the cohorts under study and because the literature identifies them as important determinants of educational outcomes and labour market performance. Gender, year of birth, religion, nationality, ethnicity, parental education, area of residence, and region are standard covariates in education and labour studies, and they span personal, parental and community characteristics known to correlate with both education and labour market outcomes. Their inclusion helps account for sources of heterogeneity that could otherwise confound the estimated effect of PETV exposure.

Gender is included because boys and girls historically faced different schooling opportunities and labour market trajectories in many African contexts (Anyanwu and Augustine, 2013), with boys typically attaining higher levels of formal education and greater access to wage employment. The year of birth captures broad cohort effects in education system expansion, macroeconomic conditions, and policy reforms. Religion, nationality and ethnicity proxy for cultural norms, migration patterns and community characteristics that have been shown to influence household decisions about schooling and labour supply (See, for instance, Byfield, 2008). Parental education and health (proxied by hospitalization in the past 30 days) are important predictor of children's human capital outcomes (Hanushek and Woessmann, 2012), consistently associated with higher educational attainment and improved employment prospects. Urban residence is included because urban households generally benefit from better school infrastructure, more qualified teachers and stronger labour markets, leading to higher schooling attainment and greater access to skilled jobs. Finally, region fixed effects absorb differences in economic development, school quality and public service provision across geographic areas.

The outcome variables indicate that 49% of individuals attained lower secondary education, 24% completed upper secondary, and 8% reached tertiary education. In the labour market, 29% hold high-skill occupations, 28% work on their own account, and 26% are employed in the government sector. The control variables show that 65% of the sample are male, with an average year of birth of 1975 and cohorts ranging from 1961 to 1984. Parental education levels are modest, particularly for mothers, and approximately half of the sample resides in urban areas. The data also display considerable variation across religion, nationality, ethnicity, and region, justifying the inclusion of these characteristics as controls in the empirical specifications. Full categories of pre-determined categorical variables are shown in balance **Table A3.**

6 Results

6.1 Covariate balance

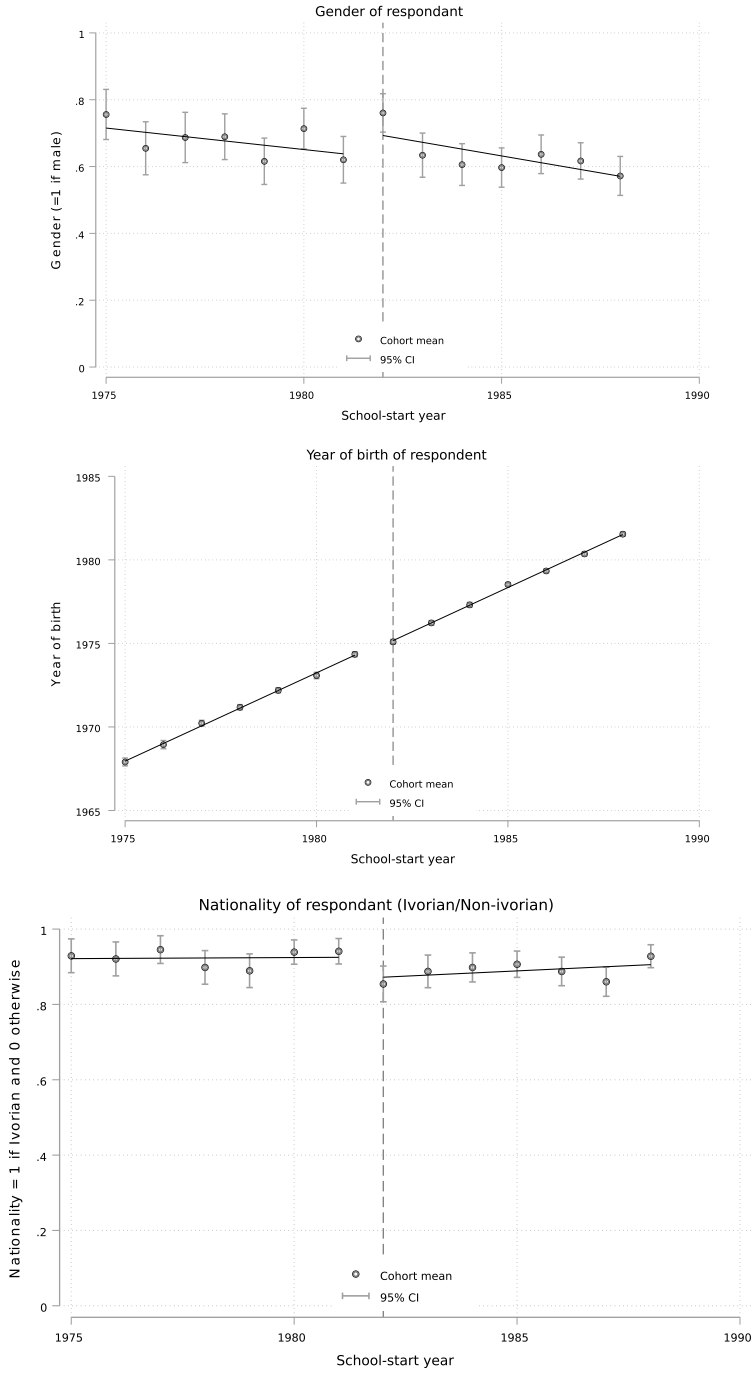
I present covariate balance graphically for variables with few categories or which are continuous in **Figure 4**. Other variables are presented in **Table A 3**. The purpose of covariate balancing is to verify whether observable characteristics are comparable between the treatment and control groups. It allows us to assess whether individuals exposed to the PETV program differ systematically from non-exposed individuals in terms of predetermined characteristics. If treatment and control groups are similar with respect to observable covariates, differences in outcomes can be more credibly attributed to the program rather than to pre-existing differences. Conversely, if covariates are not balanced, it suggests potential compositional differences between groups.

In **Figure 4**, the y-axis plots gender, year of birth[13] and nationality (Ivorian or non-Ivorian), while the x-axis is the year when an individual started primary school. The absence of sharp jumps at the 1982 cutoff supports the assumption that individuals exposed and non-exposed to the PETV program are comparable. In **Table A 3**, while most predetermined covariates are balanced across the cutoff, there are statistically significant differences in gender, some ethnic groups and some nationalities. In particular, the share of male respondents differs by approximately 4.6 percentage points across cohorts, and there are noticeable shifts in the distribution of nationality groups. These differences likely reflect gradual cohort trends rather than discrete changes at the cutoff. For instance, gender composition may vary across cohorts due to differential survival or migration, while nationality shares may evolve over time in response to broader demographic and migration dynamics in Côte d'Ivoire. Importantly, visual inspection of these variables around the cutoff in **Figure 4** does not indicate a sharp discontinuity at 1982, supporting the interpretation that the statistically significant differences reported in **Table A 3** reflect

[13] It is important to check for balance in year of birth to assess whether individuals delayed entry into school because of the PETV program.

gradual cohort trends rather than sorting around the threshold. For gender, the share of male respondents follows a gradual downward trend on both sides of the threshold. Similarly, the share of Ivorian nationals trends similarly on both sides of the cutoff, with no sharp break at 1982.

Figure 4: Balance of pre-determined control variables (Gender, Year of birth, and Nationality) around the 1982 cutoff



Notes: The figure shows the balance of pre-determined control variables *Gender, Year of birth, and Nationality* around the 1982 cutoff (vertical dashed line) using the full sample. Dots represent binned sample means by year when an individual started school, with vertical bars indicating 95 percent confidence intervals. Solid lines correspond to first-order polynomial fits estimated separately on each side of the cutoff. Gender is coded as an indicator equal to 1 if male and Nationality is equal to 1 for Ivorians and 0 for other nationalities. Balance of *Ethnicity* is shown in **Table A 3** because of its multiple modalities.

7 Main regression results

Table 2 reports the estimated effect of exposure to the PETV program on educational attainment using different measures of education outcomes. Columns (1) to (3) present estimates from the baseline specification, while columns (4) to (6) include an interaction between exposure and school-start year to allow for differential cohort trends before and after the 1982 cutoff.

In the baseline specification, exposure to PETV is associated with a statistically significant reduction (at the 1% level) in the probability of attaining lower secondary education. The magnitude of the coefficient indicates that individuals exposed to the PETV program are about 10 percentage points less likely to attain lower secondary education than comparable non-exposed individuals. For upper secondary education, the estimated effect is statistically insignificant, suggesting no detectable relationship between exposure and the probability of reaching this level. For tertiary education, the coefficient is also not statistically significant.

When allowing for differential cohort trends by including the interaction between exposure and school-start year (columns (4) to (6)), exposure to PETV remains associated with a statistically significant reduction (at the 1% level) in the probability of attaining lower secondary education. The magnitude of the coefficient remains similar, indicating a reduction of about 10 percentage points in the probability of reaching this level, corresponding to approximately 0.21 standard deviations[14]. For upper secondary and tertiary education, the estimated effects remain statistically insignificant. Overall, the results suggest that exposure to the PETV program is associated with a lower probability of attaining lower secondary education, with no evidence of an effect on upper secondary or tertiary education. These estimates should be interpreted as intention-to-treat effects, reflecting the effect of assignment to PETV exposure and potentially understating the effect on individuals who were fully exposed.

Table 3 reports the estimated effect of exposure to the PETV program on labor market outcomes. The outcomes considered are employment in high-skill occupations, working for one's own account, and working for the government. In the baseline specification (columns (1) to (3)), exposure to PETV is associated with a statistically significant increase (at the 10% level) in the probability of working in a high-skill occupation. However, when allowing for differential cohort trends by including the interaction between exposure and school-start year (columns (4) to (6)), the coefficient is not statistically significant[15], similarly to working for one's own account and working for the government. Overall, the evidence does not support the hypothesis that PETV exposure improved labour market outcomes, at least as measured along these dimensions.

[14] The 0.21 SD figure comes from dividing 0.103 by the outcome standard deviation (0.500 from Table 1): 0.103/0.500 = 0.206 ≈ 0.21.

[15] This result is robust to using a narrower first two-category definition for high-skill occupation (**Table A 12**).

Table 2: Effect of the PETV program on education outcomes

VARIABLES	(1) Lower Secondary	(2) Upper Secondary	(3) Tertiary	(4) Lower Secondary	(5) Upper Secondary	(6) Tertiary
Exposed	-0.104***	-0.011	0.004	-0.103***	-0.008	0.004
	(0.029)	(0.031)	(0.012)	(0.026)	(0.028)	(0.013)
	[0.013]	[0.735]	[0.756]	[0.012]	[0.77]	[0.800]
School-start year	-0.049***	-0.028***	-0.016**	-0.050***	-0.030***	-0.016**
	(0.010)	(0.008)	(0.005)	(0.011)	(0.009)	(0.006)
Exposed*School-start year				0.002	0.004	0.000
				(0.005)	(0.006)	(0.005)
Constant	-81.330***	-64.113***	-29.217***	-81.358***	-64.169***	-29.220***
	(16.323)	(16.966)	(7.910)	(16.303)	(16.947)	(7.926)
Observations	2,296	2,296	2,296	2,296	2,296	2,296
R-squared	0.220	0.218	0.143	0.220	0.218	0.143
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). Numbers in brackets are wild cluster bootstrap p-values (Webb weights, 9 999 replications). *** p<0.01, ** p<0.05, * p<0.1

The marginal effects obtained from logit specifications evaluated at the cutoff are quantitatively very similar to the baseline LPM estimates (Tables **Table A 8** and **Table A 9**). For educational outcomes, exposure to PETV remains associated with a statistically significant reduction in the probability of attaining lower secondary education, with magnitudes close to those reported in the main specifications. Effects on upper secondary and tertiary education remain statistically insignificant across all models. For labour market outcomes, the logit results also mirror the LPM findings: the estimated effects are generally not statistically significant, with only weak evidence of a positive association with high-skill employment in the baseline specification. Overall, these results confirm that the main findings are not driven by the linear functional form imposed by the LPM and are robust to non-linear estimation.

I re-estimate the main specifications excluding control variables that may not be predetermined : parental education, health, are of residence and region of residence. The results, reported in Tables **Table A 8** and **Table A 9**, remain virtually unchanged in both magnitude and statistical significance across all outcomes. This suggests that the main findings are not driven by the inclusion of these controls and this is consistent with the regression discontinuity design, where identification relies primarily on local variation around the cutoff rather than on covariate adjustment.

Table 3: Effect of the PETV program on labour market outcomes

VARIABLES	(1) High skill	(2) Work for own account	(3) Work for the government	(4) High skill	(5) Work for own account	(6) Work for the government
Exposed	0.028*	-0.010	-0.001	0.027	-0.002	-0.002
	(0.014)	(0.036)	(0.021)	(0.017)	(0.033)	(0.020)
	[0.112]	[0.821]	[0.960]	[0.303]	[0.958]	[0.937]
School-start year	-0.017**	0.003	-0.008	-0.017*	-0.001	-0.008
	(0.008)	(0.008)	(0.010)	(0.008)	(0.010)	(0.010)
Exposed*School-start year				-0.001	0.011	-0.001
				(0.005)	(0.007)	(0.005)
Constant	-42.970***	4.631	-26.525	-42.958***	4.484	-26.514
	(13.714)	(17.295)	(17.202)	(13.732)	(17.386)	(17.222)
Observations	2,301	2,301	2,301	2,301	2,301	2,301
R-squared	0.142	0.134	0.258	0.142	0.134	0.258
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: High skill=1 if an individual has a high-skill job and =0 otherwise. Work for own account=1 if an individual works for their own account and =0 otherwise. Work for the government=1 if an individual works for the government and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). Numbers in brackets are wild cluster bootstrap p-values (Webb weights, 9 999 replications). *** p<0.01, ** p<0.05, * p<0.1

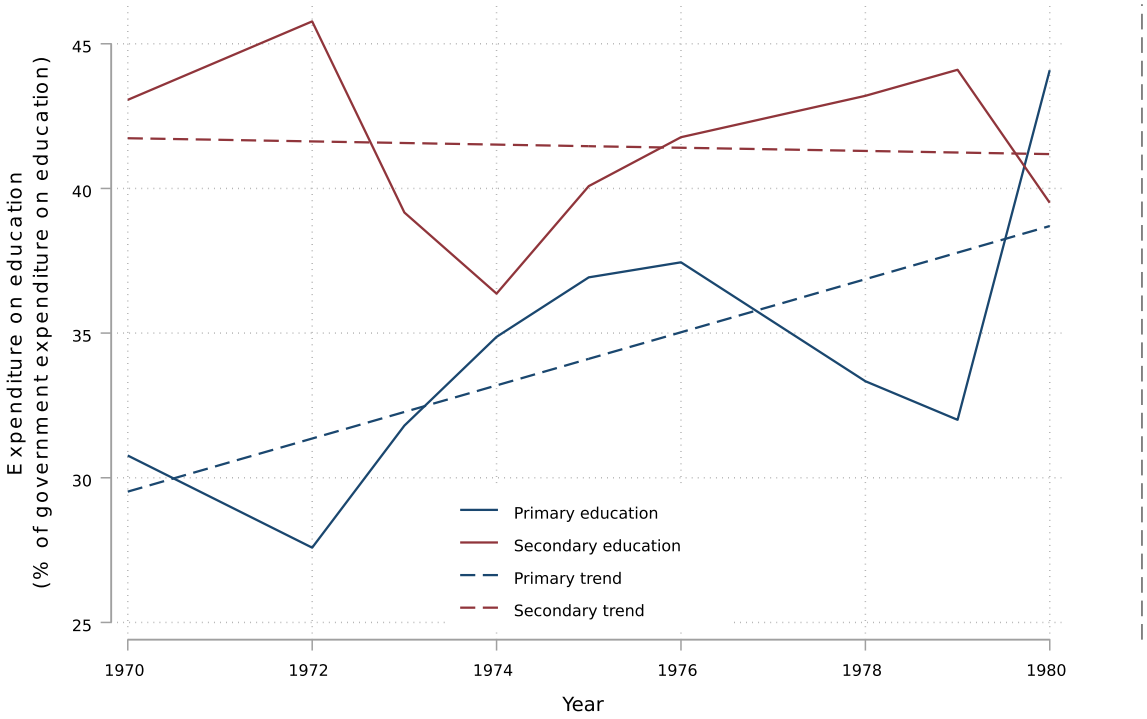
It should be noted that, after the termination of the PETV, teachers had already acquired training through television broadcasting. So if the underlying effect of PETV instruction is actually positive, the comparison being made would be attenuated by teacher training. Inversely, if the full direct effect is negative, teacher training reduces the magnitude of the estimated coefficient.

Recall that the education ministry's budget share increased from 18% in 1975 to 40% in 1980, rising from 3% of GDP in 1965 to 9% in 1982 (Crépeau, 1982). In theory, this allocation choice could have caused displacement from secondary to primary education in two ways: (1) some of these additional resources could have been allocated to secondary education in the absence of PETV; (2) resources may have been diverted away from secondary education toward primary education. I did not find any historical account suggesting the second type of displacement. However, the first mechanism still places a caveat on the interpretation of the local average treatment effect. In fact, historical accounts indicate that the secondary education system did not have enough places to meet demand, despite the barrier posed by the entrance examination (Egly, 1984), suggesting the first type of displacement.

Figure 5 provides available descriptive evidence on the allocation of public education expenditure across levels during the period surrounding the PETV program. The figure shows a

relative increase in resources allocated to primary education over time, while relative expenditure on secondary education stagnates over the period 1970-1980. This pattern is consistent with historical accounts suggesting that the expansion of primary education during the PETV period may not have been matched by a proportional increase in secondary school capacity. As a result, cohorts benefiting from improvements at the primary level may have faced tighter constraints when transitioning to secondary education.

Figure 5: Evolution of public expenditure allocated to primary and secondary education: 1970-1980



Notes: Source: WDI (2026) The figure displays the evolution of the share of public education expenditure allocated to primary and secondary education over the period 1970-1980, corresponding to the years for which data are available prior to the termination of the PETV program in 1982. Data for the period 1981 to 1991 is not available.

In this situation, the estimated negative effect of PETV exposure on lower secondary attainment may not only reflect the pedagogical effectiveness of televised instruction at the primary level, but may also captures a supply-side contraction at the secondary level, such as increased overcrowding, reduced infrastructure investment, or stagnation in teacher recruitment.

However, I further examine whether the negative effect of PETV on secondary attainment is driven by limited secondary school capacity using a more direct test. To do so, I control for a proxy of secondary school capacity, defined as the ratio of secondary to primary students, which captures the availability of secondary places relative to cohort size. As shown in Columns (4) to (6) of **Table A 10**, the inclusion of this measure leads to only a modest reduction in the estimated effect on lower secondary attainment, which remains large and statistically significant. This suggests that while capacity constraints may play a role, they are unlikely to be the primary

mechanism underlying the observed negative effect. Instead, the findings are consistent with an exam mismatch mechanism mentioned in the literature (De Landsheere, 1981; Egly, 1984), whereby the skills developed through PETV may not align well with the content or format of secondary school entrance exam. Such a mismatch could lead to lower transition rates despite potential improvements in underlying learning.

8 Robustness checks

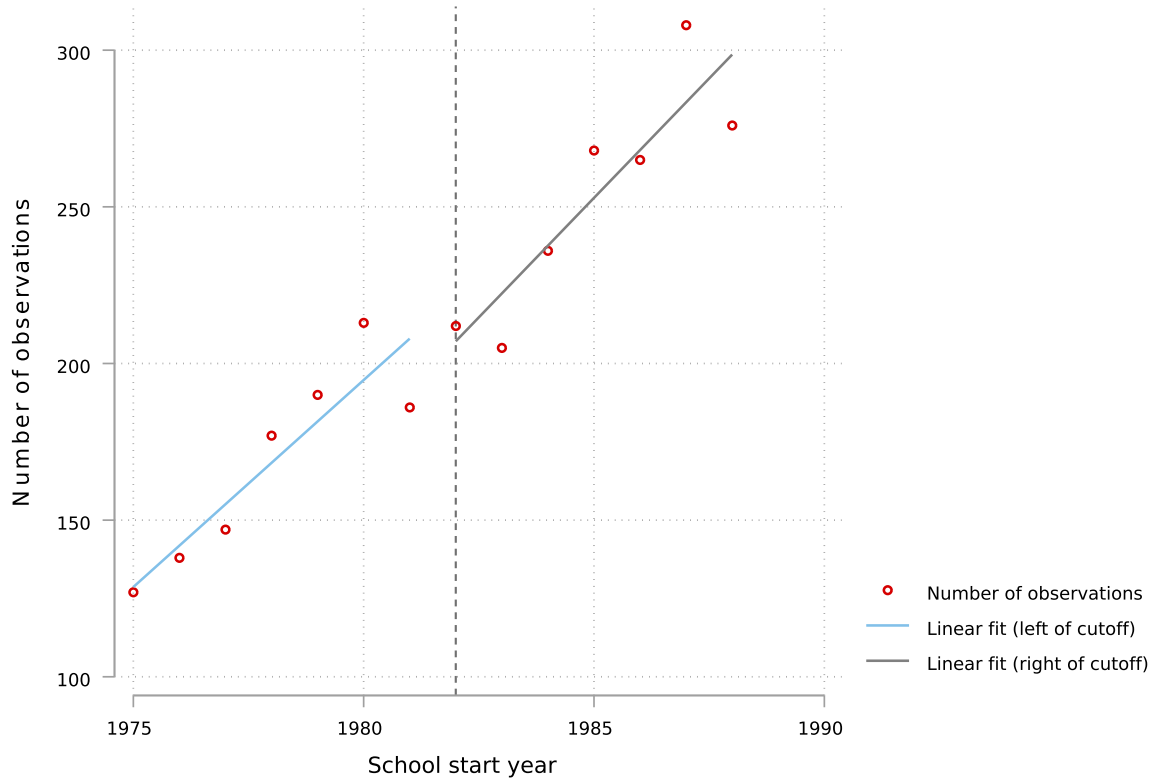
In this subsection, I perform seven robustness checks: (1) I verify that cohort size evolves smoothly around the 1982 cutoff, with no discontinuity in the number of observations at the threshold; (2) I consider narrower bandwidths to assess the sensitivity of the main estimates to the choice of comparison window; (3) I estimate the main specification at every pseudo-cutoff year from 1977 to 1987 to verify that the discontinuity detected at 1982 is not spurious; (4) I consider the intensity of treatment using a donut regression, which excludes cohorts immediately surrounding the cutoff to focus on individuals with the most clearly defined exposure status; (5) I explore the local randomization approach as an alternative identification framework; (6) I assess robustness of the main specifications to using survey sampling weights; and (7) I augment the main specification with macroeconomic controls to verify that the estimated discontinuity is not driven by contemporaneous economic conditions coinciding with the 1982 cutoff.

9 Cohort Density

Figure 6 plots the number of observations by school-start year around the 1982 cutoff. The figure shows an upward trend in cohort size over time, reflecting broader demographic dynamics. Importantly, there is no visible discontinuity at the 1982 threshold, with cohort sizes evolving smoothly across the cutoff. While there is some variation in slope before and after the threshold, this reflects gradual changes rather than a discrete break. Overall, the graphical evidence does not suggest any abnormal drop or spike in cohort size at the cutoff.

Additionally, I formally test for a discontinuity in cohort size by estimating a regression discontinuity specification using the number of observations as the outcome variable. **Table 4** reports the results of a regression discontinuity test using cohort size as the outcome variable. The coefficient on the exposure indicator, which captures the discontinuity at the 1982 cutoff, is statistically insignificant. This indicates that there is no evidence of a discrete change in the number of observations at the threshold. Combined with the graphical evidence in **Figure 6**, these results suggest that cohort size evolves smoothly across the cutoff.

Figure 6: Cohort size around the 1982 cutoff



Notes: This figure plots the number of observations by school-start year around the 1982 cutoff. Each point represents the cohort size for a given year, and the solid lines show separate linear fits estimated on either side of the cutoff. The vertical dashed line indicates the cutoff year (1982).

Table 4: Test for Discontinuity in Cohort Size at the 1982 Cutoff

VARIABLES	(1) Number of observations
Exposed	-4,048.821
	(8,752.088)
School-start year	13.214***
	(3.123)
Exposed*School-start year	2.036
	(4.417)
Constant	-25,969.571***
	(6,177.720)
Observations	14
R-squared	0.930

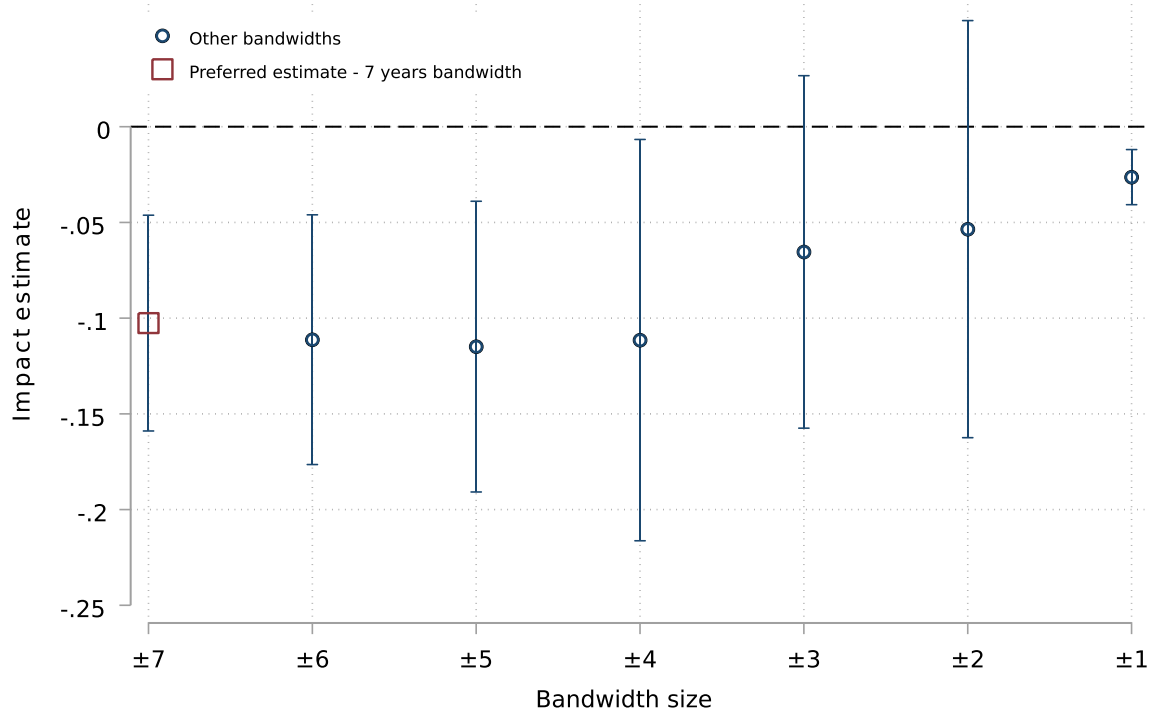
Notes: Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

10 Reduction of the bandwidth

To assess the robustness of the results to bandwidth selection, I estimate the treatment effect across all feasible symmetric windows around the 1982 cutoff, ranging from ±1 to ±7 years. Figures **7** to **12** report the estimated coefficients and corresponding 95% confidence intervals for each outcome. This approach is particularly appropriate in this setting, as the running variable is discrete and takes only a limited number of values. In such cases, data-driven bandwidth selectors designed for continuous running variables are less informative, and robustness to alternative bandwidth choices provides a more transparent assessment of sensitivity.

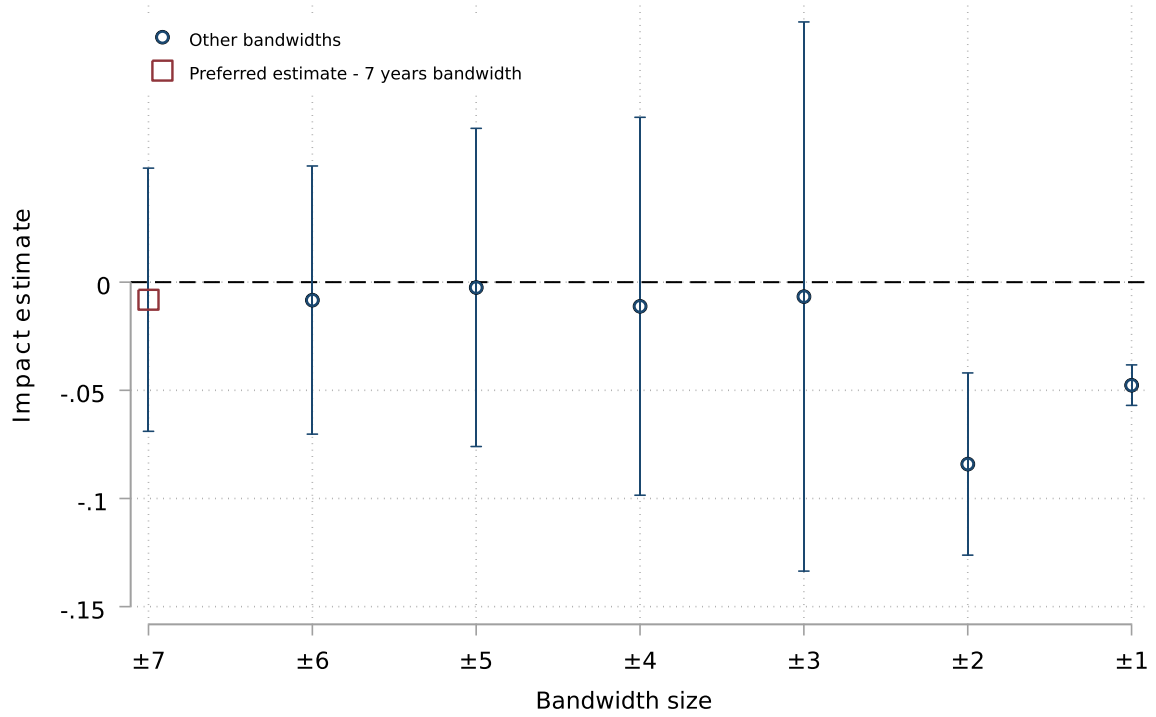
Across all outcomes, the estimates are stable in sign and broadly similar in magnitude across a wide range of bandwidths. The preferred specification using a ±7-year window balances precision and comparability across cohorts and yields estimates that are representative of the broader pattern observed across bandwidths. In particular, the negative effect on lower secondary education remains consistent across all specifications. Estimates based on very narrow bandwidths (e.g. ±1 or ±2 years) are less precise and exhibit greater variability, reflecting the limited number of cohorts available in these specifications. Overall, the consistency of results across bandwidths indicates that the main findings are not driven by the choice of comparison window.

Figure 7: Sensitivity of estimated effects to bandwidth choice: lower secondary education



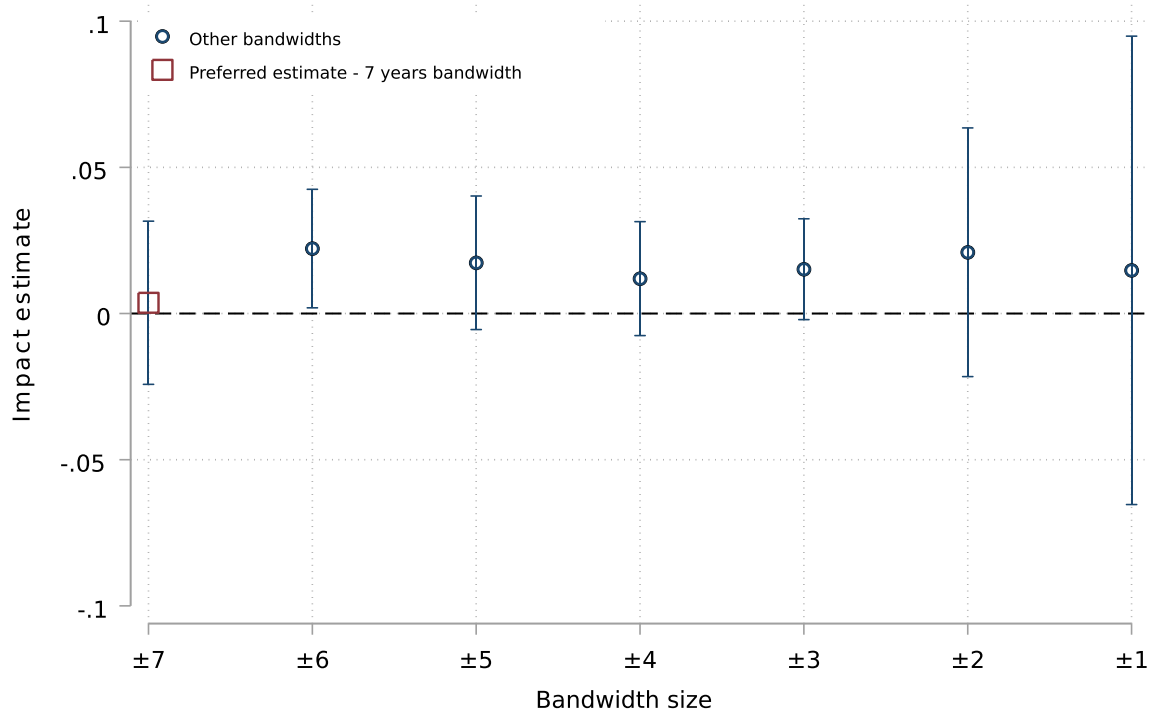
Notes: This figure reports the estimated effect of PETV exposure on lower secondary education across symmetric bandwidths ranging from ±1 to ±7 years around the 1982 cutoff. Each point represents the coefficient on the exposure indicator from a separate regression, and vertical bars denote 95% confidence intervals. The red square highlights the preferred specification using a ±7-year bandwidth.

Figure 8: Sensitivity of estimated effects to bandwidth choice: upper secondary education



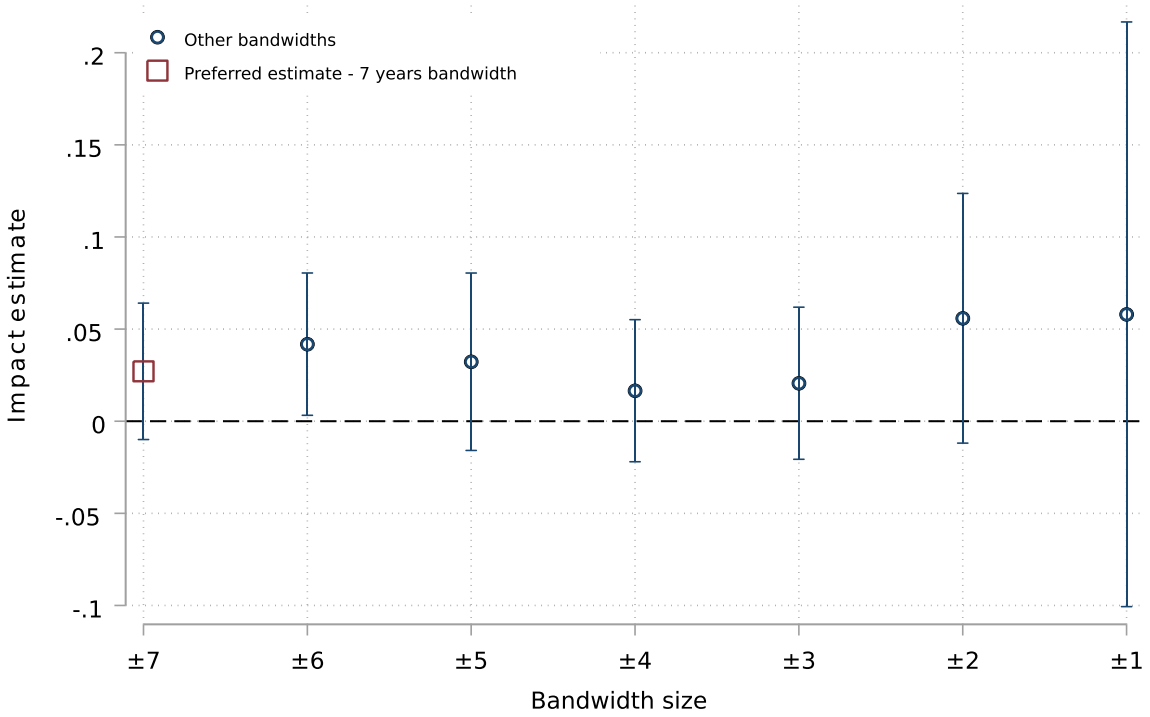
Notes: This figure reports the estimated effect of PETV exposure on upper secondary education across symmetric bandwidths ranging from ±1 to ±7 years around the 1982 cutoff. Each point represents the coefficient on the exposure indicator from a separate regression, and vertical bars denote 95% confidence intervals. The red square highlights the preferred specification using a ±7-year bandwidth.

Figure 9: Sensitivity of estimated effects to bandwidth choice: tertiary education



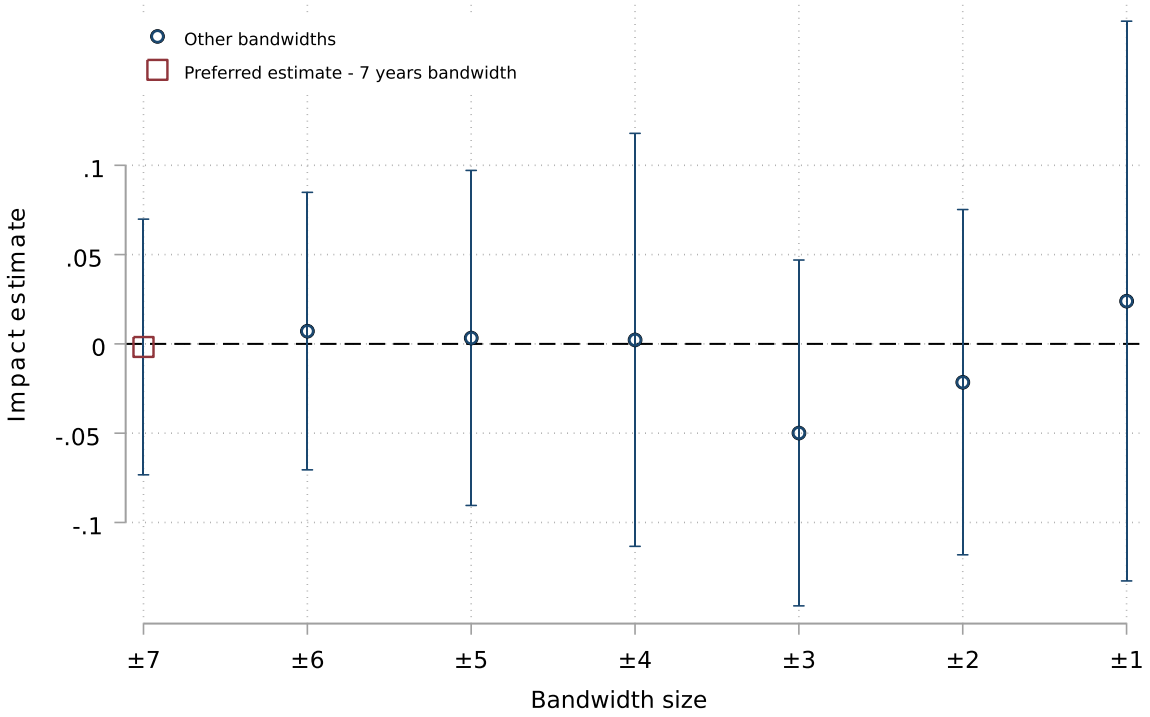
Notes: This figure reports the estimated effect of PETV exposure on tertiary education across symmetric bandwidths ranging from ±1 to ±7 years around the 1982 cutoff. Each point represents the coefficient on the exposure indicator from a separate regression, and vertical bars denote 95% confidence intervals. The red square highlights the preferred specification using a ±7-year bandwidth.

Figure 10: Sensitivity of estimated effects to bandwidth choice: high skill occupation



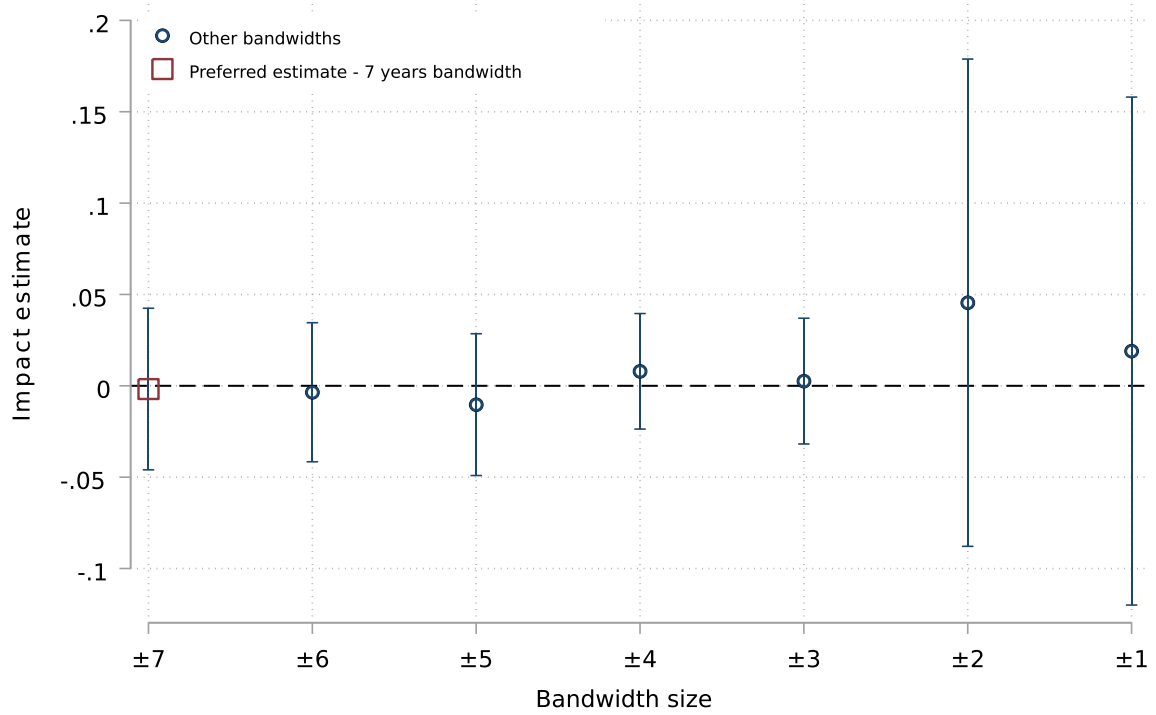
Notes: This figure reports the estimated effect of PETV exposure on high skill occupation across symmetric bandwidths ranging from ±1 to ±7 years around the 1982 cutoff. Each point represents the coefficient on the exposure indicator from a separate regression, and vertical bars denote 95% confidence intervals. The red square highlights the preferred specification using a ±7-year bandwidth.

Figure 11: Sensitivity of estimated effects to bandwidth choice: work for own account



Notes: This figure reports the estimated effect of PETV exposure on working for own account across symmetric bandwidths ranging from ±1 to ±7 years around the 1982 cutoff. Each point represents the coefficient on the exposure indicator from a separate regression, and vertical bars denote 95% confidence intervals. The red square highlights the preferred specification using a ±7-year bandwidth.

Figure 12: Sensitivity of estimated effects to bandwidth choice: work for the government



Notes: This figure reports the estimated effect of PETV exposure on working for the government across symmetric bandwidths ranging from ±1 to ±7 years around the 1982 cutoff. Each point represents the coefficient on the exposure indicator from a separate regression, and vertical bars denote 95% confidence intervals. The red square highlights the preferred specification using a ±7-year bandwidth.

11 Placebo test

As a further validation of the identification strategy, I estimate the main specification using every pseudo-cutoff year from 1977 to 1987 and plot the resulting coefficients with 95% confidence intervals for each outcome (Figures **A 2** to **A 7**). This approach assesses whether the estimated discontinuity at 1982 stands out from the distribution of estimates that would be obtained at alternative, arbitrary cutoffs.

For lower secondary attainment (**Figure A 2**), the 1982 estimate is the most negative across all pseudo-cutoffs tested, and it is the only one whose confidence interval lies below zero. Estimates at all other cutoffs are smaller in magnitude and statistically indistinguishable from zero, but generally become more negative as the pseudo-cutoff approaches 1982.

For outcomes other than lower secondary attainment, a small number of pseudo-cutoffs yield estimates that are statistically significant, scattered across different years and outcomes without a consistent pattern. This is expected given the large number of implicit comparisons involved (66), with eleven pseudo-cutoffs tested across six outcomes, some false positives are anticipated under conventional significance thresholds.

I apply a Bonferroni correction across the 11 pseudo-cutoffs tested per outcome (adjusted threshold: $0.05/11 \approx 0.005$). The estimated effect on lower secondary attainment at the true

1982 cutoff passes the Bonferroni correction and remains the most robust finding. However, three pseudo-cutoff estimates remain significant after this correction: work for own account at 1979, upper secondary education at 1984 and tertiary education at 1986. The pseudo-cutoff at 1979 for work for own account and upper secondary education at 1984 fall within the PETV implementation period (1975-1982), meaning that they do not constitute clean placebo tests. The significant results at these cutoff are therefore consistent with genuine variation in exposure rather than a spurious detection by the empirical strategy. The significant pseudo-cutoff estimate tertiary education at 1986 is acknowledged as a limitation of the placebo exercise. I note, however, that tertiary education does not show a significant effect at the true 1982 cutoff in the main results. I interpret this latter finding as an isolated anomaly in the data rather than evidence of a systematic threat to identification, and acknowledge that I cannot rule out the presence of unobserved factors affecting tertiary attainment at this particular year.

12 Donut regression

Next, I estimate a "donut" RDiT by excluding observations in the three cohorts immediately before and after the 1982 cutoff (i.e. school-start years 1979-1984), to mitigate concerns about anticipation and to examine whether similar patterns emerge for individuals who were most exposed to the PETV program. The choice of a three-cohort exclusion window reflects a trade-off inherent to donut designs. On the one hand, excluding observations close to the cutoff helps address concerns related to transition dynamics and low intensity exposure. On the other hand, removing too many cohorts would substantially reduce the sample size and weaken statistical power. Excluding three cohorts on each side of the cutoff therefore represents a balanced and pragmatic choice, allowing us to focus on individuals with more clearly defined exposure status while retaining a sufficient number of observations to obtain precise estimates.

Results are reported in **Table A 9**. In the full specification (Columns (4) to (6)), exposure to PETV is associated with a statistically significant reduction in the probability of attaining lower secondary education, upper secondary education and tertiary education. The estimated coefficients indicate reductions of about 9.7 percentage points for lower secondary education, 6.5 percentage points for upper secondary education and 8.1 percentage points for tertiary education. This stands in contrast to the main specification, where effects on upper secondary and tertiary education are statistically non-significant. The broader effects observed in the donut sample likely reflect the fact that individuals near the cutoff had only partial exposure to PETV, attenuating the estimated effects in the full sample. The donut results therefore suggest that the full impact of sustained PETV exposure may extend beyond lower secondary attainment. Negative effects also appear for self-employment and working for the government (**Table 6**), but they are statistically less pronounced than those for education outcomes.

Table 5: Effect of the PETV program on education outcomes – Donut regression

VARIABLES	(1) Lower Secondary	(2) Upper Secondary	(3) Tertiary	(4) Lower Secondary	(5) Upper Secondary	(6) Tertiary
Exposed	-0.085**	-0.050*	-0.040	-0.097***	-0.065***	-0.081**
	(0.028)	(0.022)	(0.035)	(0.025)	(0.015)	(0.031)
School-start year	-0.049**	-0.037**	-0.018*	-0.049**	-0.037**	-0.016
	(0.015)	(0.013)	(0.009)	(0.015)	(0.013)	(0.009)
Exposed*School-start year				-0.003	-0.004	-0.010
				(0.005)	(0.003)	(0.008)
Constant	-85.764**	-76.782**	-26.609*	-85.840**	-76.873**	-26.855*
	(24.719)	(24.142)	(13.453)	(24.714)	(24.103)	(13.482)
Observations	1,294	1,294	1,294	1,294	1,294	1,294
R-squared	0.256	0.265	0.146	0.256	0.265	0.146
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. The regression removes three years around the cutoff. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of the PETV program on labor market outcomes – Donut regression

VARIABLES	(1) High skill	(2) Work for own account	(3) Work for the government	(4) High skill	(5) Work for own account	(6) Work for the government
Exposed	0.009	-0.016	-0.011	-0.002	-0.062**	-0.059*
	(0.032)	(0.026)	(0.030)	(0.036)	(0.024)	(0.029)
School-start year	-0.021	0.008	-0.012	-0.020	0.010	-0.010
	(0.014)	(0.007)	(0.008)	(0.014)	(0.007)	(0.007)
Exposed*School-start year				-0.003	-0.011	-0.012**
				(0.009)	(0.006)	(0.005)
Constant	-47.424*	15.072	-32.295*	-47.488*	14.815	-32.559*
	(23.946)	(14.031)	(14.922)	(23.958)	(14.100)	(14.964)
Observations	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.200	0.189	0.305	0.200	0.189	0.306
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: High skill=1 if an individual has a high-skill job and =0 otherwise. Work for own account=1 if an individual works for their own account and =0 otherwise. Work for the government=1 if an individual works for the government and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. The regression removes three years around the cutoff. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

13 Local randomization approach

As an additional robustness check, we explored the local-randomization approach of Cattaneo, Titiunik, and Vázquez-Bare (2016). This method relies on identifying a window around the cutoff within which treatment assignment can be considered as if random based on covariate balance tests.

In this setting, no symmetric window around the 1982 cutoff satisfies standard covariate balance criteria. This finding is consistent with the graphical evidence (**Figure 4**), which shows gradual changes in covariates across cohorts but no discontinuity at the cutoff. Taken together, these results suggest that a regression discontinuity framework is more appropriate than a local randomization approach in this context.

14 Robustness to Survey Sampling Weights

The LSMS 2018-2019 survey follows a sampling design with sampling weights. While the main analysis relies on unweighted regressions, I assess the robustness of the results using the survey design features of the data.

The weighted estimates obtained using the survey design are very similar to the baseline results for education outcomes (**Table A 6**). In particular, exposure to the PETV program remains associated with a statistically significant reduction in the probability of attaining lower secondary education, with a magnitude close to the unweighted estimates. The coefficients for upper secondary and tertiary education are also very similar to the baseline results and remain statistically insignificant. These findings indicate that the education results are not driven by differential sampling probabilities.

For labor market outcomes, no statistically significant evidence of an effect on labour market outcomes is found across all specifications (**Table A 7**). This lack of statistical significance reinforces the conclusion that the program did not have detectable long-run effects on labor market outcomes.

15 Macroeconomic controls

As discussed in the empirical strategy section, a potential threat to identification is that the 1982 cutoff coincides with the onset of a broader economic downturn in Côte d'Ivoire, driven by the collapse of international cocoa and coffee prices and the subsequent implementation of structural adjustment programmes. Although historical accounts suggest this crisis unfolded gradually over the course of the 1980s rather than as a discrete shock at 1982, as illustrated in **Figure A 1**,

I provide a more direct empirical test by augmenting the baseline specification with cohort-level macroeconomic controls.

Specifically, I assign to each individual the GDP per capita of Côte d'Ivoire and the international commodity prices for cocoa, arabica coffee and robusta coffee corresponding to their school-start year. By indexing these variables to school-start year rather than survey year, I capture the macroeconomic environment experienced by each cohort at the time of school entry.

Results are reported in **Table A 11**. The estimated effect of PETV exposure on lower secondary attainment remains negative, stable in magnitude and statistically significant at the 1% level across all specifications. The coefficient on Exposed moves only marginally from -0.104 in the baseline to -0.089 when macroeconomic controls are included, and the commodity price and GDP per capita coefficients are themselves largely statistically non-significant. These results confirm that the main finding is not confounded by the macroeconomic environment prevailing at the time of school entry, and that the estimated discontinuity at the 1982 cutoff reflects the termination of the PETV programme rather than a concurrent economic shock.

16 Heterogeneity with respect to gender

I explore whether the effect of the PETV program varies by gender, assessing whether the program may have varied by male and female subgroups, even if average effects are small or absent.

Across all education measures, interaction terms between exposure and being male are statistically insignificant (**Table 7**). This indicates that the program had no detectable differential effect on schooling for boys and girls. The male dummy is positively associated with all three educational outcomes, consistent with known gender gaps in schooling patterns, but PETV exposure does not appear to have amplified or reduced these differences. The labour market results show one gender-specific pattern (**Table 8**). The interaction with being male is negative and statistically significant for government employment, suggesting that males are slightly less likely to work for the government than females who were exposed to the program. For high-skill occupations and self-employment, the interaction coefficients are statistically insignificant, indicating minimal gender-based differences in PETV effects.

A natural dimension of heterogeneity to explore would be area of residence (urban vs. rural) and region of origin (North vs. South), given that reducing these divides was an explicit objective of the PETV program. However, the LSMS 2018-2019 records area and region of current residence rather than residence at the time of schooling. Since residential location in 2018-2019 may itself reflect post-schooling migration decisions influenced by educational attainment, using current residence in lieu of residence at the time of school would produce an endogenous effect. We therefore do not pursue these heterogeneity analyses and acknowledge them as a limitation of the available data.

Table 7: Heterogeneity with respect to gender - education outcomes

VARIABLES	(1) Lower Secondary	(2) Upper Secondary	(3) Tertiary	(4) Lower Secondary	(5) Upper Secondary	(6) Tertiary
Exposed	-0.098***	0.013	0.011	-0.097***	0.015	0.011
	(0.030)	(0.026)	(0.018)	(0.028)	(0.025)	(0.019)
Exposed_male	-0.009	-0.037	-0.011	-0.008	-0.036	-0.011
	(0.035)	(0.024)	(0.019)	(0.035)	(0.023)	(0.018)
School-start year	-0.049***	-0.028***	-0.016***	-0.050***	-0.029***	-0.016**
	(0.010)	(0.008)	(0.005)	(0.011)	(0.009)	(0.006)
Exposed*School-start year				0.002	0.003	-0.000
				(0.005)	(0.006)	(0.005)
Gender (Male=1)	0.278***	0.239***	0.097***	0.278***	0.239***	0.097***
	(0.014)	(0.021)	(0.012)	(0.014)	(0.021)	(0.013)
Constant	-81.642***	-64.678***	-29.406***	-81.662***	-64.714***	-29.406***
	(16.493)	(16.934)	(7.880)	(16.470)	(16.919)	(7.892)
Observations	2,296	2,296	2,296	2,296	2,296	2,296
R-squared	0.220	0.218	0.143	0.220	0.219	0.143
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table 8: Heterogeneity with respect to gender - labour market outcomes

VARIABLES	(1) High skill	(2) Work for own account	(3) Work for the government	(4) High skill	(5) Work for own account	(6) Work for the government
Exposed	0.036*	-0.004	0.058*	0.036	0.003	0.057*
	(0.020)	(0.048)	(0.030)	(0.022)	(0.046)	(0.029)
Exposed_male	-0.013	-0.010	-0.090**	-0.013	-0.007	-0.091**
	(0.032)	(0.041)	(0.034)	(0.032)	(0.041)	(0.033)
School-start year	-0.017**	0.003	-0.008	-0.017*	-0.001	-0.007
	(0.008)	(0.008)	(0.010)	(0.008)	(0.010)	(0.010)
Exposed*School-start year				-0.001	0.010	-0.002
				(0.005)	(0.007)	(0.005)
Gender (Male=1)	0.109***	-0.177***	0.318***	0.109***	-0.178***	0.318***
	(0.019)	(0.024)	(0.020)	(0.018)	(0.024)	(0.020)
Constant	-43.116***	4.518	-27.534	-43.105***	4.408	-27.508
	(13.769)	(17.269)	(17.427)	(13.793)	(17.372)	(17.475)
Observations	2,301	2,301	2,301	2,301	2,301	2,301
R-squared	0.142	0.134	0.260	0.142	0.134	0.260
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: High skill=1 if an individual has a high-skill job and =0 otherwise. Work for own account=1 if an individual works for their own account and =0 otherwise. Work for the government=1 if an individual works for the government and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

17 Second generation effects

I present second generation effects, i.e. effects of the PETV program on children of exposed adults, in **Table 9**. Across all specifications, the results show no evidence that parental exposure to PETV translated into higher or lower rates of formal schooling[16] among their children. In the simplest specification (column 1), children of exposed adults are 5 percentage points more likely to have ever attended formal education, and this estimate is statistically significant at the 1% level. However, once one accounts for basic demographic structure by controlling for year of birth (column 2), the coefficient is reduced in size and becomes statistically insignificant, suggesting that the initial difference is essentially explained by age/cohort rather than parental exposure itself, making the controlled specification the relevant one for causal interpretation. Adding the full set of controls (column 3) further reduces the magnitude of the effect, which is again statistically insignificant. Overall, the results indicate that intergenerational spillovers on children's formal schooling appear absent once appropriate controls are introduced.

Table 9: Effect of the PETV program on – Second generation effects

VARIABLES	(1) Formal education	(2) Formal education	(3) Formal education
Adult_exposed	0.050***	0.008	-0.003
	(0.012)	(0.011)	(0.012)
Year of birth		-0.022***	-0.019***
		(0.001)	(0.001)
Constant	0.749***	45.913***	39.866***
	(0.008)	(1.971)	(2.046)
Observations	5,025	4,907	4,322
R-squared	0.004	0.100	0.183
All Controls	NO	NO	YES
Region Fixed Effects	NO	NO	YES

Notes: Notes: Formal education = 1 if he/she has he done, or is he currently doing, studies in a formal school. Standard errors in parentheses : *** p<0.01, ** p<0.05, * p<0.1

[16] Data on secondary and tertiary attainment is not available for children.

18 Conclusion

This study examines the long-term effect of the *Programme d'Education Télévisuelle* (PETV), a large-scale, understudied educational television intervention implemented in Côte d'Ivoire in the 70s and 80s, on education and labour market outcomes.

The main results of the analysis show that exposure to the PETV program is consistently associated with lower attainment of lower secondary education, with no meaningful effects on upper secondary schooling and tertiary education. For labour market outcomes, the analysis finds no significant effect of PETV exposure on high-skill employment, self-employment or government employment. Robustness checks using narrower bandwidths, a placebo cutoff, and "donut" regression, confirm these patterns, though precision decreases in very small samples. Heterogeneity analyses indicate slightly better outcomes for exposed urban individuals, while gender differences are insignificant. Finally, there is no evidence of intergenerational effects: parental exposure does not significantly influence children's formal school attendance. Hence, despite substantial investments from the government and international donors, the PETV program did not yield clearly better outcomes than the traditional classroom method of teaching, in a context where the education system was arguably not very advanced. However, these estimates should be interpreted as intention-to-treat effects, capturing the effect of cohort-based exposure to PETV and likely attenuating the effect on those who were actually treated.

It should be noted that the weak performance of the PETV may partly reflect the relatively underdeveloped state of the technology at the time. By contrast, a recent study in Pakistan shows that a similar intervention led to substantial improvements in educational outcomes, suggesting that both the maturity of the technology and the quality of implementation play a crucial role (Beg et al, 2022).

The findings carry several policy lessons for the design and evaluation of large-scale educational technology interventions. First, the contrast between the results reported here and positive findings from comparable programs in Mexico and Pakistan underscores that the effectiveness of classroom television education is context specific. Such comparative analysis is not the focus of the present study but would be a valuable direction for future research. Second, in the Ivorian context, PETV may have improved foundational learning at the primary level without equipping students with the skills required to succeed in secondary school entrance examinations, suggesting that primary-level edtech investments should be accompanied by careful alignment between primary curricula and transition requirements. Third, the absence of detectable long-run gains from a decade-long, heavily resourced intervention points to the importance of rigorously evaluating the opportunity costs of large-scale education innovations before committing to their adoption at scale, particularly in contexts where alternative investments, such as expanding secondary school capacity or improving teacher quality, may yield higher returns.

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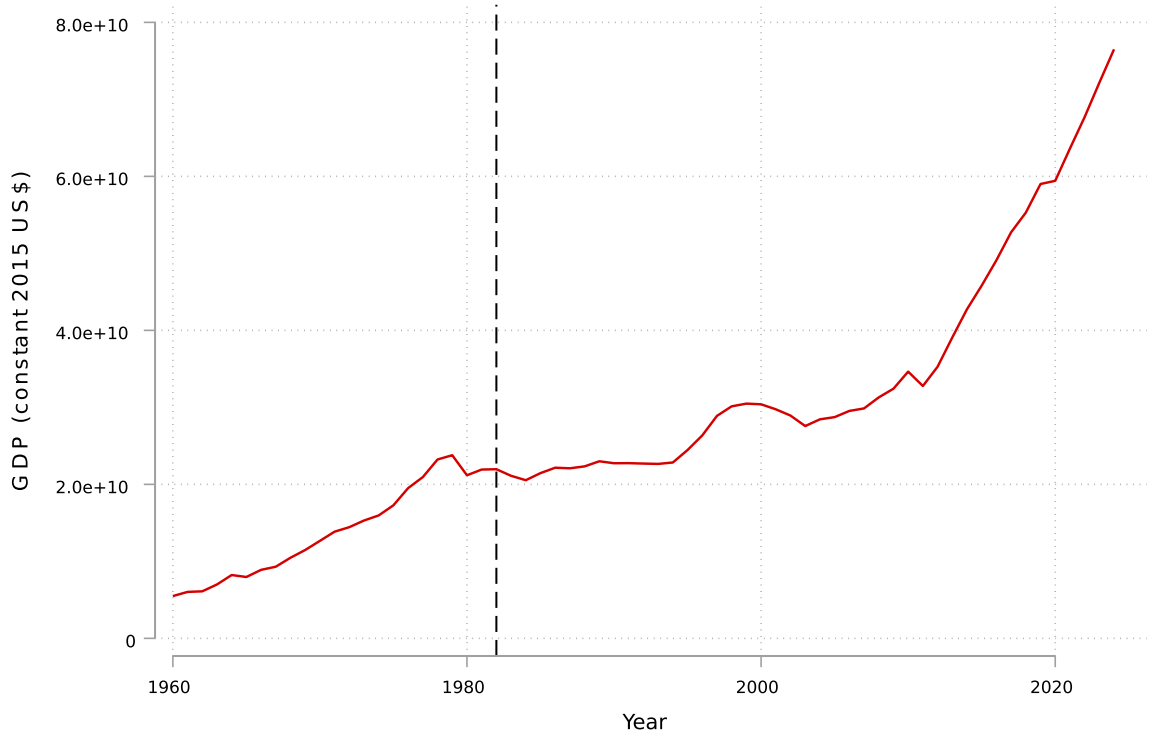
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Appendix A: Additional tables and figures

Figure A1: GDP of Côte d'Ivoire : 1960-2025



Notes: Source: WDI (2026) The figure plots GDP in constant 2015 US dollars for Côte d'Ivoire from 1960 to 2023. The series shows steady growth from independence through the late 1970s, followed by a period of slight decline and stagnation during the 1980s and early 1990s. The vertical dashed line marks 1982, corresponding to the end of the PETV program.

Table A3: Covariate balance

		(1)		(2)		(1)-(2)
		0		1		Pairwise t-test
Variable	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
Pre-determined variables						
Gender (=1 male)	1775	0.628	1180	0.674	2955	-0.046**
		(0.011)		(0.014)		
Muslim	1775	0.257	1180	0.242	2955	0.015
		(0.010)		(0.012)		
Christian	1775	0.622	1180	0.626	2955	-0.004
		(0.012)		(0.014)		
Animist	1775	0.035	1180	0.042	2955	-0.007
		(0.004)		(0.006)		
Other religion	1775	0.011	1180	0.009	2955	0.002
		(0.003)		(0.003)		
No religion	1775	0.075	1180	0.081	2955	-0.006
		(0.006)		(0.008)		
Ethnicity_1	1579	0.032	1089	0.037	2668	-0.004
		(0.004)		(0.006)		
Ethnicity_2	1579	0.011	1089	0.008	2668	0.003

(continued)

		(1)		(2)		(1)-(2)
		(0.003)		(0.003)		
Ethnicity_3	1579	0.008	1089	0.004	2668	0.004
		(0.002)		(0.002)		
Ethnicity_4	1579	0.018	1089	0.016	2668	0.003
		(0.003)		(0.004)		
Ethnicity_5	1579	0.015	1089	0.012	2668	0.003
		(0.003)		(0.003)		
Ethnicity_6	1579	0.083	1089	0.088	2668	-0.005
		(0.007)		(0.009)		
Ethnicity_7	1579	0.003	1089	0.002	2668	0.001
		(0.001)		(0.001)		
Ethnicity_8	1579	0.002	1089	0.005	2668	-0.003
		(0.001)		(0.002)		
Ethnicity_9	1579	0.006	1089	0.007	2668	-0.002
		(0.002)		(0.003)		
Ethnicity_10	1579	0.063	1089	0.058	2668	0.005
		(0.006)		(0.007)		
Ethnicity_11	1579	0.006	1089	0.004	2668	0.002
		(0.002)		(0.002)		
Ethnicity_12	1579	0.003	1089	0.003	2668	-0.000
		(0.001)		(0.002)		
Ethnicity_13	1579	0.003	1089	0.001	2668	0.002
		(0.001)		(0.001)		
Ethnicity_14	1579	0.256	1089	0.263	2668	-0.006
		(0.011)		(0.013)		
Ethnicity_15	1579	0.048	1089	0.042	2668	0.006
		(0.005)		(0.006)		
Ethnicity_16	1579	0.001	1089	0.001	2668	-0.000
		(0.001)		(0.001)		
Ethnicity_17	1579	0.000	1089	0.001	2668	-0.001
		(0.000)		(0.001)		
Ethnicity_18	1579	0.020	1089	0.028	2668	-0.009
		(0.003)		(0.005)		
Ethnicity_19	1579	0.035	1089	0.024	2668	0.011
		(0.005)		(0.005)		
Ethnicity_20	1579	0.005	1089	0.006	2668	-0.000
		(0.002)		(0.002)		
Ethnicity_21	1579	0.009	1089	0.007	2668	0.002
		(0.002)		(0.003)		
Ethnicity_22	1579	0.001	1089	0.001	2668	0.000
		(0.001)		(0.001)		
Ethnicity_23	1579	0.001	1089	0.001	2668	0.000
		(0.001)		(0.001)		
Ethnicity_24	1579	0.006	1089	0.003	2668	0.003
		(0.002)		(0.002)		
Ethnicity_25	1579	0.001	1089	0.000	2668	0.001
		(0.001)		(0.000)		
Ethnicity_26	1579	0.003	1089	0.004	2668	-0.001
		(0.001)		(0.002)		
Ethnicity_27	1579	0.005	1089	0.006	2668	-0.001
		(0.002)		(0.002)		
Ethnicity_28	1579	0.033	1089	0.026	2668	0.007
		(0.004)		(0.005)		
Ethnicity_29	1579	0.030	1089	0.031	2668	-0.001

(continued)

		(1)		(2)		(1)-(2)
		(0.004)		(0.005)		
Ethnicity_30	1579	0.001	1089	0.000	2668	0.001
		(0.001)		(0.000)		
Ethnicity_31	1579	0.002	1089	0.001	2668	0.001
		(0.001)		(0.001)		
Ethnicity_32	1579	0.001	1089	0.000	2668	0.001
		(0.001)		(0.000)		
Ethnicity_33	1579	0.004	1089	0.001	2668	0.003
		(0.002)		(0.001)		
Ethnicity_34	1579	0.016	1089	0.016	2668	0.000
		(0.003)		(0.004)		
Ethnicity_35	1579	0.002	1089	0.006	2668	-0.004
		(0.001)		(0.002)		
Ethnicity_36	1579	0.001	1089	0.000	2668	0.001
		(0.001)		(0.000)		
Ethnicity_37	1579	0.019	1089	0.016	2668	0.003
		(0.003)		(0.004)		
Ethnicity_38	1579	0.001	1089	0.001	2668	-0.000
		(0.001)		(0.001)		
Ethnicity_39	1579	0.008	1089	0.002	2668	0.006**
		(0.002)		(0.001)		
Ethnicity_40	1579	0.005	1089	0.006	2668	-0.001
		(0.002)		(0.002)		
Ethnicity_41	1579	0.013	1089	0.017	2668	-0.005
		(0.003)		(0.004)		
Ethnicity_42	1579	0.033	1089	0.045	2668	-0.012
		(0.004)		(0.006)		
Ethnicity_43	1579	0.004	1089	0.004	2668	0.000
		(0.002)		(0.002)		
Ethnicity_44	1579	0.002	1089	0.005	2668	-0.003
		(0.001)		(0.002)		
Ethnicity_45	1579	0.000	1089	0.002	2668	-0.002*
		(0.000)		(0.001)		
Ethnicity_46	1579	0.000	1089	0.001	2668	-0.001
		(0.000)		(0.001)		
Ethnicity_47	1579	0.001	1089	0.001	2668	0.000
		(0.001)		(0.001)		
Ethnicity_48	1579	0.001	1089	0.000	2668	0.001
		(0.001)		(0.000)		
Ethnicity_49	1579	0.087	1089	0.088	2668	-0.001
		(0.007)		(0.009)		
Ethnicity_50	1579	0.008	1089	0.016	2668	-0.008**
		(0.002)		(0.004)		
Ethnicity_51	1579	0.003	1089	0.002	2668	0.001
		(0.001)		(0.001)		
Ethnicity_52	1579	0.003	1089	0.003	2668	0.000
		(0.001)		(0.002)		
Ethnicity_53	1579	0.015	1089	0.019	2668	-0.004
		(0.003)		(0.004)		
Ethnicity_54	1579	0.061	1089	0.057	2668	0.004
		(0.006)		(0.007)		
Ethnicity_55	1579	0.000	1089	0.001	2668	-0.001
		(0.000)		(0.001)		
Ethnicity_56	1579	0.005	1089	0.006	2668	-0.000

(continued)

		(1)		(2)		(1)-(2)
		(0.002)		(0.002)		
Ethnicity_57	1579	0.001	1089	0.000	2668	0.001
		(0.001)		(0.000)		
Nationality_1	1775	0.011	1180	0.006	2955	0.005
		(0.002)		(0.002)		
Nationality_2	1775	0.072	1180	0.039	2955	0.033***
		(0.006)		(0.006)		
Nationality_3	1775	0.890	1180	0.923	2955	-0.033***
		(0.007)		(0.008)		
Nationality_4	1775	0.001	1180	0.002	2955	-0.001
		(0.001)		(0.001)		
Nationality_5	1775	0.008	1180	0.008	2955	-0.001
		(0.002)		(0.003)		
Nationality_6	1775	0.007	1180	0.005	2955	0.002
		(0.002)		(0.002)		
Nationality_7	1775	0.001	1180	0.000	2955	0.001
		(0.001)		(0.000)		
Nationality_8	1775	0.005	1180	0.003	2955	0.002
		(0.002)		(0.001)		
Nationality_9	1775	0.002	1180	0.004	2955	-0.002
		(0.001)		(0.002)		
Nationality_10	1775	0.003	1180	0.007	2955	-0.004
		(0.001)		(0.002)		
Nationality_11	1775	0.003	1180	0.003	2955	-0.001
		(0.001)		(0.002)		
Other control variables						
Father's education	1647	1.766	1121	1.545	2768	0.221***
		(0.030)		(0.032)		
Mother's education	1594	1.282	1072	1.149	2666	0.133***
		(0.018)		(0.016)		
Health (Hospitalization Prev 30 days)	1775	1.700	1180	1.676	2955	0.024
		(0.011)		(0.014)		
Rural/Urban	1775	1.475	1180	1.504	2955	-0.029
		(0.012)		(0.015)		
Region	1775	13.767	1180	14.258	2955	-0.490
		(0.250)		(0.307)		

Notes: The table reports summary statistics separately for treated and control cohorts, where treated individuals are those who started school before 1982. Pre-determined variables are presented first, followed by other variables used in the analysis.

Table A1: Annual cost of PETV program and sources of finance (in thousands of 1980 CFA Francs)

Sources of finance	1969–70	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	Total
Ivory Coast	469,262	1,427,872	1,962,539	2,832,962	3,144,142	4,475,457	5,179,683	5,702,584	6,488,892	7,270,449	6,474,585	45,129,407
France	1,101,240	721,864	1,185,620	1,212,392	1,218,392	965,160	1,088,741	931,852	811,933	455,243	401,450	10,094,458
Unesco	434,845	362,960	426,804	352,759	246,197	269,517	267,517	228,296	178,444	137,939	69,750	2,975,345
Canada		2,373	99,050	463,436	679,112	665,560	357,984	386,791	164,308	192,535	140,577	3,269,726
UNICEF	42,684	57,607	78,056	38,104	40,488	16,269	6,199	4,299	8,292	20,266	29,250	325,494
US Aid				15,832	25,424							51,246
F.R. Germany					39,226							108,882
Belgium						50,949	29,938	91,476	75,200	71,505	28,000	347,066
Subtotal Foreign Aid	1,578,749	1,144,804	1,786,530	2,083,093	2,266,838	2,047,360	1,750,379	1,642,715	1,335,177	867,487	669,027	17,172,161
Total cost	2,048,011	2,572,676	3,749,069	4,916,055	5,410,980	6,823,817	6,930,062	6,745,279	7,824,069	8,137,936	7,143,612	62,301,566

Notes: Data source : Koné & Jenkins (1990)

Table A2: Distribution of adults by occupations

Occupation	Freq.	Percent	Cum.
Intellectual and scientific professions	165	6.12	6.12
Intermediate professions	94	3.49	9.61
Administrative employees - managers	113	4.19	13.81
Service and sales personnel	477	17.71	31.51
Farmers and skilled agricultural workers	1,101	40.87	72.38
Craftsmen and skilled craft workers	158	5.86	78.25
Plant and machinery operators	62	2.3	80.55
Unskilled workers and employees	191	7.09	87.64
Army and security	50	1.86	89.5
Other trades and professions	229	8.5	98
No profession and unspecified profession	54	2	100
Total	2,694	100	

Notes: Data source : LSMS 2018-2019 The first three categories (Intellectual and scientific professions, Intermediate professions, and Administrative employees-managers) are classified as high-skill in the baseline specification, corresponding to ISCO-08 skill levels 3 and 4 (ILO, 2012). Service and sales personnel, assigned to ISCO skill level 2, are classified as low-skill. The remaining categories are classified as low-skill.

Table A4: Effect of the PETV program on education outcomes – Removing pre-determined controls

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Lower Secondary	Upper Secondary	Tertiary	Lower Secondary	Upper Secondary	Tertiary
Exposed	-0.092***	-0.007	0.007	-0.093***	-0.004	0.009
	(0.029)	(0.024)	(0.009)	(0.030)	(0.021)	(0.010)
School-start year	-0.075***	-0.056***	-0.029***	-0.074***	-0.057***	-0.029***
	(0.009)	(0.007)	(0.004)	(0.009)	(0.006)	(0.004)
Exposed*School-start year				-0.002	0.003	0.002
				(0.006)	(0.004)	(0.003)
Constant	-137.610***	-123.311***	-58.416***	-137.597***	-123.329***	-58.427***
	(15.726)	(12.554)	(6.639)	(15.765)	(12.522)	(6.654)
Observations	2,661	2,661	2,661	2,661	2,661	2,661
R-squared	0.115	0.107	0.063	0.115	0.107	0.063
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table A5: Effect of the PETV program on labour market outcomes – Removing non-predetermined controls

VARIABLES	(1) High skill	(2) Work for own account	(3) Work for the government	(4) High skill	(5) Work for own account	(6) Work for the government
Exposed	0.029**	-0.000	-0.002	0.029*	0.005	0.001
	(0.013)	(0.037)	(0.023)	(0.016)	(0.037)	(0.024)
School-start year	-0.033***	-0.000	-0.031**	-0.033***	-0.003	-0.032***
	(0.008)	(0.007)	(0.010)	(0.008)	(0.009)	(0.010)
Exposed*School-start year				-0.000	0.006	0.003
				(0.005)	(0.008)	(0.005)
Constant	-76.056***	-0.998	-74.474***	-76.054***	-1.029	-74.492***
	(14.532)	(15.977)	(18.472)	(14.542)	(16.071)	(18.430)
Observations	2,668	2,668	2,668	2,668	2,668	2,668
R-squared	0.055	0.086	0.110	0.055	0.086	0.110
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: High skill=1 if an individual has a high-skill job and =0 otherwise. Work for own account=1 if an individual works for their own account and =0 otherwise. Work for the government=1 if an individual works for the government and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table A6: Effect of the PETV program on education outcomes – with sampling weights

VARIABLES	(1) Lower Secondary	(2) Upper Secondary	(3) Tertiary	(4) Lower Secondary	(5) Upper Secondary	(6) Tertiary
Exposed	-0.095**	-0.077	-0.014	-0.093*	-0.079	-0.015
	(0.047)	(0.048)	(0.037)	(0.049)	(0.049)	(0.037)
School-start year	-0.049***	-0.046***	-0.021**	-0.050***	-0.045***	-0.021**
	(0.017)	(0.011)	(0.009)	(0.017)	(0.012)	(0.010)
Exposed*School-start year				0.003	-0.002	-0.001
				(0.012)	(0.012)	(0.009)
Constant	-77.910***	-82.266***	-32.662**	-78.154***	-82.120***	-32.603**
	(28.607)	(19.000)	(14.390)	(28.484)	(18.987)	(14.413)
Observations	2,296	2,296	2,296	2,296	2,296	2,296
R-squared	0.255	0.260	0.191	0.255	0.260	0.191
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table A7: Effect of the PETV program on labour market outcomes – with sampling weights

VARIABLES	(1) High skill	(2) Work for own account	(3) Work for the government	(4) High skill	(5) Work for own account	(6) Work for the government
Exposed	0.022	0.006	0.013	0.020	0.008	0.010
	(0.042)	(0.050)	(0.049)	(0.043)	(0.051)	(0.051)
School-start year	-0.025**	0.007	-0.002	-0.023**	0.006	-0.001
	(0.010)	(0.012)	(0.016)	(0.011)	(0.013)	(0.016)
Exposed*School-start year				-0.003	0.003	-0.004
				(0.011)	(0.013)	(0.012)
Observations	-53.934***	2,301	2,301	-53.739***	2,301	2,301
R-squared	(17.551)	0.139	0.278	(17.522)	0.139	0.278
Controls		YES	YES		YES	YES
Region Fixed Effects	2,301	YES	YES	2,301	YES	YES

Notes: Notes: High skill=1 if an individual has a high-skill job and =0 otherwise. Work for own account=1 if an individual works for their own account and =0 otherwise. Work for the government=1 if an individual works for the government and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table A8: Effect of the PETV program on education outcomes (Logistic regression – Marginal effects)

VARIABLES	(1) Lower Secondary	(2) Upper Secondary	(3) Tertiary	(4) Lower Secondary	(5) Upper Secondary	(6) Tertiary
Exposed	-0.096***	-0.015	0.001	-0.094***	-0.011	-0.001
	(0.022)	(0.031)	(0.018)	(0.019)	(0.028)	(0.020)
Observations	2,281	2,264	2,069	2,281	2,264	2,069
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table A9: Effect of the PETV program on labour market outcomes (Logistic regression – Marginal effects)

VARIABLES	(1) High skill	(2) Work for own account	(3) Work for the government	(4) High skill	(5) Work for own account	(6) Work for the government
Exposed	0.025*	-0.010	-0.007	0.025	-0.000	-0.006
	(0.014)	(0.036)	(0.021)	(0.017)	(0.032)	(0.020)
Observations	2,238	2,262	2,280	2,238	2,262	2,280
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: High skill=1 if an individual has a high-skill job and =0 otherwise. Work for own account=1 if an individual works for their own account and =0 otherwise. Work for the government=1 if an individual works for the government and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table A10: Effect of the PETV program on education outcomes – Controlling for secondary school capacity

VARIABLES	(1) Lower Secondary	(2) Upper Secondary	(3) Tertiary	(4) Lower Secondary	(5) Upper Secondary	(6) Tertiary
Exposed	-0.104***	-0.014	0.003	-0.089***	-0.021	0.001
	(0.030)	(0.029)	(0.012)	(0.026)	(0.025)	(0.022)
School-start year	-0.048***	-0.033***	-0.016**	-0.046***	-0.034***	-0.017**
	(0.010)	(0.009)	(0.006)	(0.010)	(0.009)	(0.007)
Exposed*School-start year				0.013	-0.006	-0.002
				(0.013)	(0.014)	(0.011)
Secondary school capacity	-0.268	0.981	0.133	-1.766	1.731	0.373
	(0.674)	(0.847)	(0.626)	(1.770)	(2.058)	(1.452)
Constant	-81.225***	-64.496***	-29.269***	-80.816***	-64.701***	-29.334***
	(16.313)	(16.923)	(7.943)	(16.192)	(16.930)	(7.909)
Observations	2,296	2,296	2,296	2,296	2,296	2,296
R-squared	0.220	0.218	0.143	0.220	0.218	0.143
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

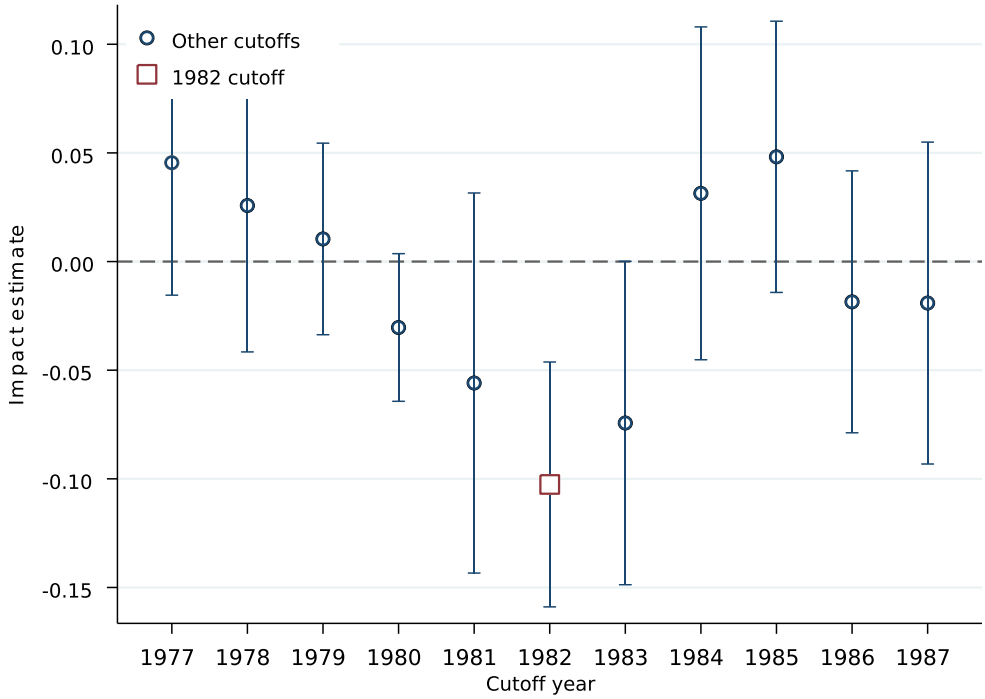
Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Table A11: Effect of the PETV program on education outcomes – Adding macroeconomic controls

VARIABLES	(1) Lower Secondary	(2) Upper Secondary	(3) Tertiary	(4) Lower Secondary	(5) Upper Secondary	(6) Tertiary
Exposed	-0.089***	0.013	0.018*	-0.087***	0.019	0.018**
	(0.029)	(0.026)	(0.009)	(0.027)	(0.025)	(0.008)
School-start year	-0.045***	-0.022**	-0.012*	-0.046***	-0.025**	-0.012*
	(0.010)	(0.009)	(0.006)	(0.010)	(0.010)	(0.006)
Exposed*School-start year				0.002	0.005	0.000
				(0.005)	(0.006)	(0.005)
Cocoa price	-0.040	-0.092**	-0.048*	-0.038	-0.087**	-0.048
	(0.040)	(0.036)	(0.025)	(0.042)	(0.037)	(0.027)
Coffee price-arabica	-0.039	-0.023	-0.020	-0.037	-0.018	-0.020
	(0.038)	(0.034)	(0.022)	(0.038)	(0.032)	(0.023)
Coffee price-robusta	0.063	0.078	0.049	0.061	0.071	0.048
	(0.058)	(0.053)	(0.029)	(0.058)	(0.050)	(0.032)
GDP per capita	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-82.003***	-65.444***	-29.957***	-82.004***	-65.448***	-29.957***
	(16.559)	(17.062)	(8.064)	(16.558)	(17.069)	(8.066)
Observations	2,296	2,296	2,296	2,296	2,296	2,296
R-squared	0.220	0.219	0.143	0.220	0.219	0.143
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

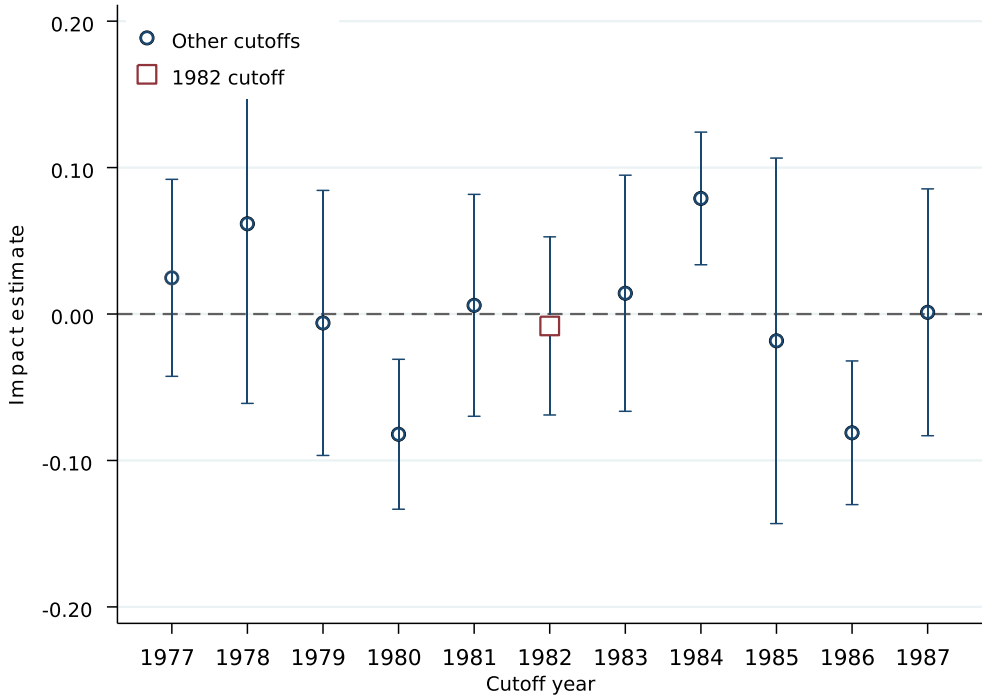
Notes: Notes: Lower Secondary=1 if an individual attained lower secondary education and =0 otherwise. Upper Secondary=1 if an individual attained upper secondary education and =0 otherwise. Tertiary=1 if an individual attained tertiary education and =0 otherwise. Exposed=1 for individuals who started school between 1982 and 1984 and =0 for individuals who started school from 1985 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1982 and 1988. Standard errors are clustered at the cohort level (School-start year). *** p<0.01, ** p<0.05, * p<0.1

Figure A2: Sensitivity of estimated effects to cutoff choice - Lower secondary education outcome



Notes: This figure displays the sensitivity of the estimated effect of the PETV program on lower secondary education attainment to alternative cutoff years. Each point represents the estimated treatment effect obtained using a given cutoff year, with vertical bars indicating 95% confidence intervals. The dashed horizontal line denotes zero effect. The hollow square highlights the true cutoff, corresponding to the end of the PETV program in 1982.

Figure A3: Sensitivity of estimated effects to cutoff choice - Upper secondary education outcome



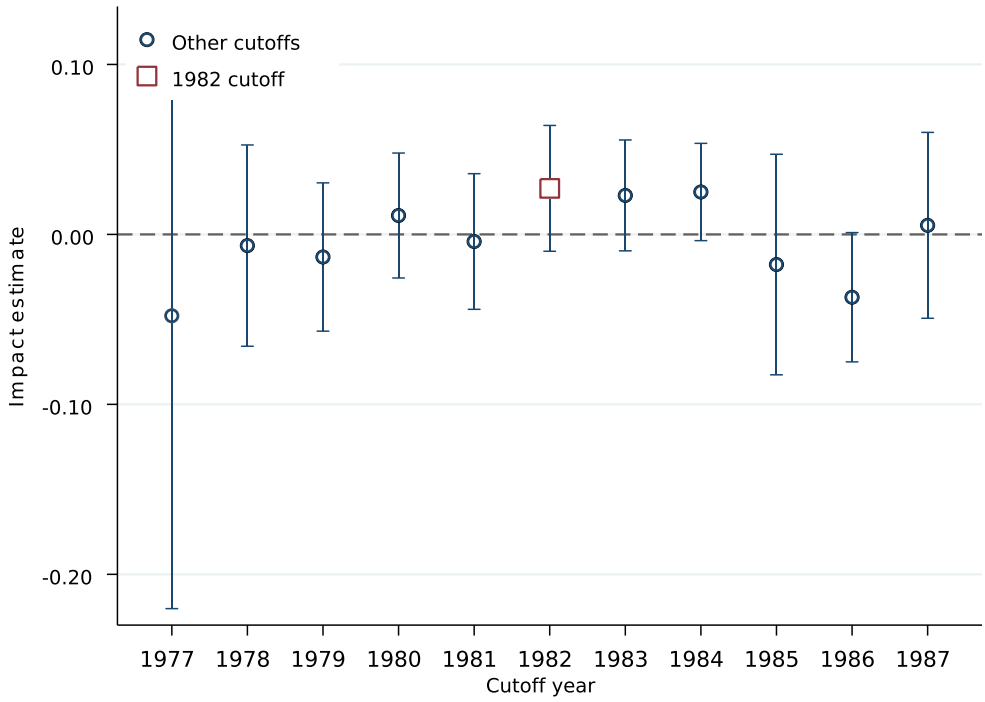
Notes: This figure displays the sensitivity of the estimated effect of the PETV program on upper secondary education attainment to alternative cutoff years. Each point represents the estimated treatment effect obtained using a given cutoff year, with vertical bars indicating 95% confidence intervals. The dashed horizontal line denotes zero effect. The hollow square highlights the true cutoff, corresponding to the end of the PETV program in 1982.

Figure A4: Sensitivity of estimated effects to cutoff choice - tertiary education outcome



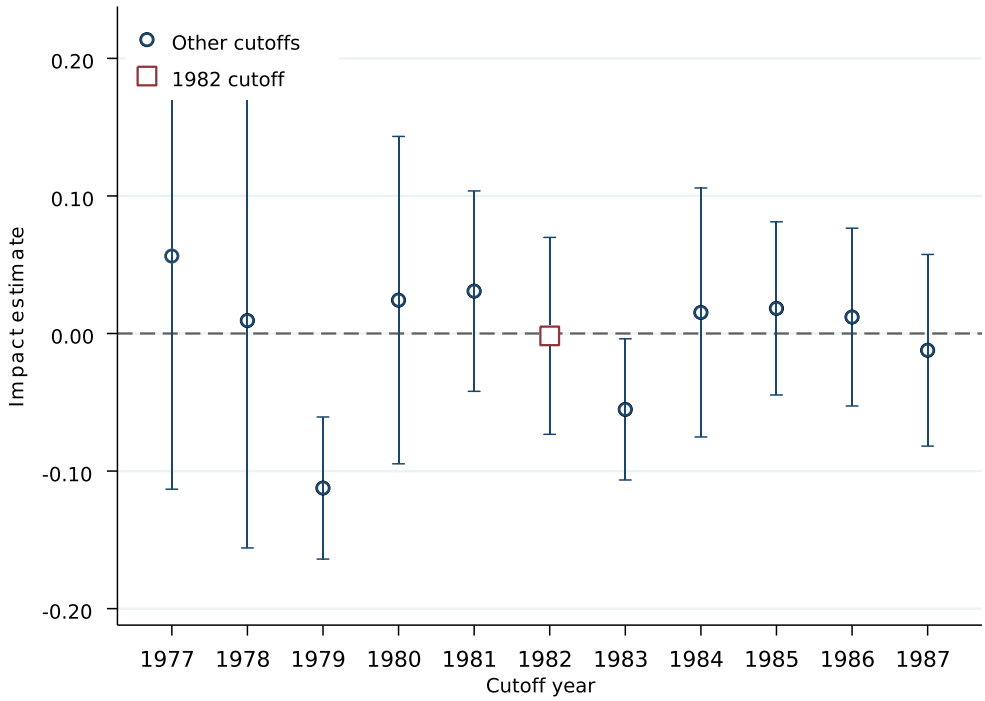
Notes: This figure displays the sensitivity of the estimated effect of the PETV program on tertiary education attainment to alternative cutoff years. Each point represents the estimated treatment effect obtained using a given cutoff year, with vertical bars indicating 95% confidence intervals. The dashed horizontal line denotes zero effect. The hollow square highlights the true cutoff, corresponding to the end of the PETV program in 1982.

Figure A5: Sensitivity of estimated effects to cutoff choice - high skill outcome



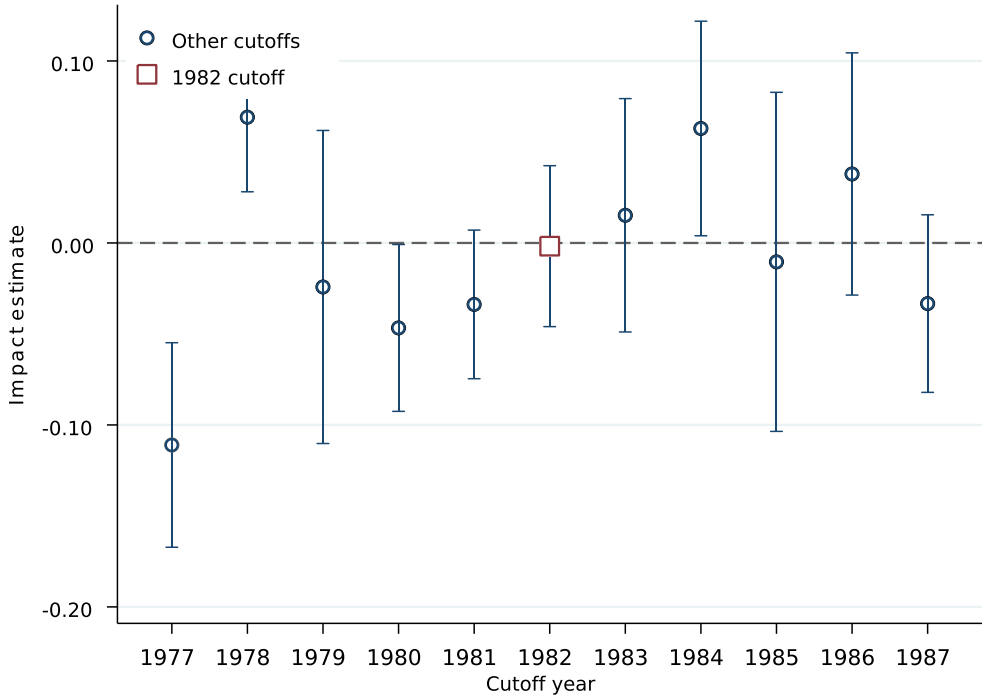
Notes: This figure displays the sensitivity of the estimated effect of the PETV program on high-skill occupation to alternative cutoff years. Each point represents the estimated treatment effect obtained using a given cutoff year, with vertical bars indicating 95% confidence intervals. The dashed horizontal line denotes zero effect. The hollow square highlights the true cutoff, corresponding to the end of the PETV program in 1982.

Figure A6: Sensitivity of estimated effects to cutoff choice – work for own account outcome



Notes: This figure displays the sensitivity of the estimated effect of the PETV program on working for own account to alternative cutoff years. Each point represents the estimated treatment effect obtained using a given cutoff year, with vertical bars indicating 95% confidence intervals. The dashed horizontal line denotes zero effect. The hollow square highlights the true cutoff, corresponding to the end of the PETV program in 1982.

Figure A7: Sensitivity of estimated effects to cutoff choice – working for the government outcome



Notes: This figure displays the sensitivity of the estimated effect of the PETV program on working for the government to alternative cutoff years. Each point represents the estimated treatment effect obtained using a given cutoff year, with vertical bars indicating 95% confidence intervals. The dashed horizontal line denotes zero effect. The hollow square highlights the true cutoff, corresponding to the end of the PETV program in 1982.

Table A12: Effect of the PETV program on labour market outcomes – High-skill occupation restricted to first two categories

VARIABLES	(1) High skill	(2) Work for own account	(3) Work for the government	(4) High skill	(5) Work for own account	(6) Work for the government
Exposed	0.011	-0.010	-0.001	0.007	-0.002	-0.002
	(0.014)	(0.036)	(0.021)	(0.018)	(0.033)	(0.020)
School-start year	-0.017**	0.003	-0.008	-0.015**	-0.001	-0.008
	(0.007)	(0.008)	(0.010)	(0.006)	(0.010)	(0.010)
Exposed*School-start year				-0.005	0.011	-0.001
				(0.005)	(0.007)	(0.005)
Constant	-39.384***	4.631	-26.525	-39.314***	4.484	-26.514
	(10.864)	(17.295)	(17.202)	(10.896)	(17.386)	(17.222)
Observations	2,301	2,301	2,301	2,301	2,301	2,301
R-squared	0.125	0.134	0.258	0.126	0.134	0.258
Controls	YES	YES	YES	YES	YES	YES
Region Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Notes: High skill=1 if an individual has a high-skill job and =0 otherwise. Work for own account=1 if an individual works for their own account and =0 otherwise. Work for the government=1 if an individual works for the government and =0 otherwise. Exposed=1 for individuals who started school between 1975 and 1981 and =0 for individuals who started school from 1982 to 1988. School-start year is the running variable capturing year when an individual started primary education between 1975 and 1988. Standard errors are clustered at the cohort level (School-start year). Numbers in brackets are wild cluster bootstrap p-values (Webb weights, 9 999 replications). *** p<0.01, ** p<0.05, * p<0.1