DID UNIVERSAL PRIMARY EDUCATION IMPROVE LABOUR MARKET PERFORMANCE IN NIGERIA?

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ABSTRACT

This study examines causal impact of education on wealth status of Nigerians using the 2008 Nigeria's Demographic and Health Survey (NDHS). This study exploits the quasi-natural experiment offered by the large-scale government investment in Universal Primary Education between 1976 and 1981. Results from the Differences-in-Differences technique established that the UPE programme had positive impact on schooling attainment. To estimate the labour market impact of the UPE programme, we use both OLS and IV. OLS results show that a year of education increases well-being by 8.5 per cent, with lower and upper limits being 7.9 percent and 8.53 percent respectively. Econometric test of selection on both observed and unobserved variables indicate that OLS results are not driven by omitted variable bias. Furthermore, we use IV technique to correct for reverse causality and measurement error by using three instruments: years of exposure to free UPE programme, intensity dummy of UPE at the level of Local Government Area (LGA) and the interaction of exposure to UPE and UPE intensity dummy. Our IV results reveal the UPE programme has significant labour market consequences. On the average, IV results show that a year of schooling increases wellbeing by 15 percent, which is nearly twice as high as the OLS estimate**.** Our estimates are robust to a number of tests such as specification test, exogeneity or over-identification test, falsification tests, addition of a number of control variables, state fixed effects and cohort fixed effects.

Key Words: Schooling, **OLS, IV,** economic well-being

1.0 Introduction

The early stage of the economic growth debate was about physical capital as the driving force of accelerated economic growth. So important was the subject of physical capital accumulation that capital-poor countries were told that their only route to rapid economic growth lies in the importation of capital, since domestic savings were barely adequate for economic development.[2] In the context of the Harrod-Domar growth model[3], savings transformed into physical capital was the only means of raising the rate of economic growth. By the middle of last century, the fact that skills or capital embodied in humans could be an important means of rapid economic growth has entered into the growth debate. In nearly six decades, interest in the subject of human capital and economic growth has not abated. The various channels by which skills, abilities and knowledge embodied in humans could directly or indirectly translate to economic growth have been explored.

Though human capital can be acquired and augmented via education[4], it is also important to note that human capital can be enhanced through on-the-job training, learning by doing, mobility, home training and health-care investment (Mincer, 1989).

Human capital acquired through formal schooling helps to raise labour productivity and hence the earnings of labour. A survey paper by Psacharoupoulos (1985) finds considerably high rates of return to investments in education. It is as high as 26% for primary education in sub-Saharan Africa. The rates are 27 % and 13% respectively primary and higher education in Asia, while Latin America and the Caribbean's rates are 26% and 16% for primary and higher education respectively. Education by boosting labour productivity helps to accelerate economic growth.

A macroeconomic study by McMahon (1987) shows that increase in GDP per worker is due to expenditure on primary and secondary education as well as higher education. He found the rate of return to be 20% for Africa[5]. Education has also assisted in reducing poverty considerably (Okojie, 2002; Olaniyan, 2002; Oyelere, 2003). The absence of sufficient human capital, in terms of formal education and training, also explains why poor countries receive little foreign capital and technology (Lucas, 1990). Hanson (1996) cross-country study has given this theoretical proposition empirical validity.

This study uses microdata to examine the causal impact of education in long term indicator of economic well-being: this indicator is computed from individual possession of durable physical assets and the specific characteristics of housing conditions into a wealth index. Principal component analysis is used to compute this index. The fourth wave of Demographic

[2] Check Lewis (1954) for classic treatment of the role of physical capital in economic growth and development.
[3] Harrod (1939) and Domar (1946) made seminal contributions to this debate.
[4] For the purpose of our study, words like education, schooling and human capital are used interchangeably, though they do not exactly have similar meanings. Education covers both formal and informal learning activities while schooling is specifically referring to the formal aspect of learning. Human capital does not only embody education but also on-the-job training, learning by doing, migration and investment in healthcare.
[5] Other studies have similar findings. Baum, Blackman an Wolff (1989), revealed that growth in output per capital for 103 countries for the period 1960-1985 is due to the growth in the initial GDP per capita as well as the enrollment rates in primary and secondary schools, fraction of GDP spent by government, investment ratio, price distortion, fertility and other indicators of political and social instability. For Mankiw, Romer and Weil (1992) growth in output for the same period of 1960-85 can be partly explained by the average proportion of the labour force with secondary school education.

and Health Survey household data collected in 2008 in Nigeria is used for this analysis (NDHS). Against the background of the 1976 UPE, this study turn out to be both an evaluation of the effect of the programme on labour market performance of beneficiaries relative to non-beneficiaries and the determination of the causal impact of schooling on economic well-being. The UPE programme presents an opportunity to overcome the identification problems associated with estimating the impact of schooling on individual economic and non-indicators of well-being.

Previously, both Osili and Long (2008) and Oyelere (2010) used UPE programme to estimate the impact of schooling on fertility and individual earnings respectively. Though both studies showed that UPE programme has significant impact on individuals years of schooling attainment, Osili and Long (2008) also found out UPE reduced fertility significantly through its impact on schooling attainment while Oyelere (2010) found that its impact on individual earnings was minimal. Instead of using individual earnings, which are often reported with large errors because of large number of informal sector workers and are inappropriate for sundry other reasons, our study uses wealth index as defined earlier.

While we retained the Oyelere (2010) measurement of exposure to years of UPE, we rejected Osili and Long (2008) and Oyelere (2010) subjective definition of intensity of UPE. To construct our measure of UPE intensity, we at first relied on Duflo (2001) objective definition of programme intensity[6]. First, we determine the proportion of school age children in the 2006 Nigerian Census for each of the 774 Local Government Areas (LGAs) and used it estimate the number of children as at 1976. Second, the regress the number of schools constructed during the UPE programme, obtained from Nigerian School Census database, on the estimated number of school age children in 1976. Third, residuals were obtained from the bivariate regression. LGAs with positive residuals were regarded as high intensity LGAs and those with negative residuals are regarded low intensity areas. However, our estimates of school age children are accurate to the extent that the demographic composition of the 774 LGAs had not changed between 1976 and 2006. This assumption is too strong and empirical results turn out to be implausible.

To overcome this problem, we develop two alternative measures of UPE intensity. Using Nigerian School Census database, which contained the names of all primary and secondary schools in Nigeria and the school year of establishment, we calculated the number of schools established during the UPE programme per square kilometre. Second, we calculated the percentage increase in the number of schools during the programme period per square kilometre. Both indicators were normalized, having a mean of zero and standard deviation of one. LGA with positive values are high intensity areas with value 1 allotted to them and those with negative values are low-intensity areas with 0 assigned to them. Both measures provided more plausible results for virtually all of our econometric regressions. Similar results are obtained from the two indicators of UPE programme intensity. For this study, we report

[6] She regressed the number of schools on the number of school age children in each district of Indonesia. Thereafter, she obtained the residuals. Districts that yielded positive are high intensity districts and those that yielded negative residuals are tagged low intensity districts. Value of 1 is allocated to high intensity districts and 0 to low intensity districts.

results with the dummy for the number of schools established during the UPE programme per square kilometre.

Using a Differences-in-Differences approach, we showed that years of exposure to UPE, UPE intensity and the interaction of both have significant and positive impact on years of primary, secondary and total schooling attainment. Results are robust to the inclusion of control variables as well as time invariant and time varying variables which might confound our estimates of UPE programme on schooling. Not only is exposure to UPE leading to higher schooling attainment, high intensity LGAs achieved greater increase in schooling attainment than low intensity LGAs.

Next, we present OLS results of the impact of schooling on wealth index, our indicator of economic well-being. Our results show that a year of schooling raises economic well-being by 8.8 per cent. A year of education increases well-being by 8.5 per cent, with lower and upper limits being 7.9 percent and 8.53 percent respectively. These estimates are robust to the inclusion of several variables which might affect our indicator of wealth status. Because we are not sure that omitted variables that might bias upward OLS estimates, we draw on the insight of Altonji, Elder and Taber (2005) technique of selection on observed and unobserved variables. The logic of their technique is to estimate how much greater the impact of unobservables would have to be, compared to observables, to account fully for the positive relationship between schooling and well-being. Results reinforce our conviction that schooling impact positively and significantly on well-being. Thus, unobservable variables such as health characteristics of individuals, years of vocational training, migration, natural ability, religion, ethnic identity, political attributes of LGAs and many more might not be driving our results.

However, OLS technique cannot fully care of endogeneity and measurement error problems. Therefore, we use IV identification strategy, using years of exposure to UPE, UPE intensity and the interaction of both variables as instruments for schooling. This approach, in part allows us to evaluate the impact of the 1976 UPE programme on economic well-being. Beyond that, it allows to estimate the causal impact of schooling on long term indicator of economic well-being. The F-statistic from the first stage regression shows that our instrument set is very strong.

One major source bias for OLS and IV estimates is selective migration. The 2008 NDHS household data has no information on migration at all much less on the more precise information on location(s) of schooling and work at different stages of the life cycle. The anecdotal evidence suggests that Nigerians are non-migrants when it comes to crossing their states of birth. According to Oyelere (2010), more than 95 per cent of Nigerians never leave their locations of birth. Much of the movement is restricted to moving from one part of the state of birth to another. If sufficient variation exists in different parts of the same state, our estimates of schooling variable will still be biased. If we assumed that individuals can make the greatest gains by moving to the capital of the state of birth, then we can estimate the loss associated with being further away from the state capital. We use the great circle great formula in estimating the distance of the individual from the state capital. Introducing this

distance variable in the econometric model indicates that greater distance from the state capital reduces significantly economic well-being. However, schooling variable remains significant, even at 1 per cent. Second, if an alternative instrument could be found, which is not confounded by migration, then we can be sure that migration is not driving our results. In this study, we use the year of birth, year at six and year at twelve as alternative instruments. These instruments pick up the reduction in schooling costs are more schools are constructed nationwide. Schooling estimates from this IV strategy yields nearly the same results as the previous instruments. This instrument is both strong and exogenous. Thus, migration might not be driving our results. Excluding respondents from Lagos state, Abuja and other more urbanized states of the South-West, which are known to be the destination points for people born in other states, could create its own bias. Dropping these locations allows us to determine how sensitive our results are to the inclusion of outliers. Our results did not change as the schooling coefficient is still significant at 1 per cent.

To address concerns about validity of our instruments, we developed two other placebo UPE laws and performed some falsification tests. The false instruments turn out to be positively and insignificantly related to well-being indicator. Nigeria is probably one of few countries that introduced UPE programme in the mid-1970. Thus, if we have similar DHS data from other African countries and assumed falsely that they implemented UPE in 1976 and operated it nationwide until, the relationship between the false UPE instrument and wealth index should be zero and insignificantly different from zero. The instruments turn out to be insignificant. Using alternative instruments such as year of birth, year at six and year at twelve, our over-identification tests fail to reject the exogeneity of main instruments used in this study. Our IV result shows that a year of schooling increases wellbeing by 15 percent, which is significantly higher than the OLS estimate. In fact, the IV estimate is nearly twice as high as the OLS estimate. While the IV estimate remain significant at 1 percent under a number of robustness tests, the estimate reduced to as low as 12.7 percent.

While this study re-investigates the issue of private returns to education as done by Okuwa (2004), Aromolaran (2004, 2006) and Oyelere (2005 and 2009), we differ from these studies in terms of methodological approaches that improves the precision of our estimates of the causal impact of schooling on indicator of labour market performance. Unlike Okuwa (2004) and Aromolaran (2004, 2006), we use *Differences-in-Differences* technique to obtain estimates of the causal impact of UPE on schooling attainment. Instrumental Variable (IV) technique is used to take care of potential problems of measurement error, endogeneity and omitted variable bias often associated with schooling when OLS method is employed to determine the causal impact of schooling on individual wealth status. Though Oyelere (2010) adopted the same instrumental variable technique (IV), unlike her study, we use an indicator of welfare not subject to non-classical measurement error as would individual earnings. More importantly, we use more recent advancements in the econometrics of exclusion restriction testing to provide a firmer basis for the validity of our instrument set. The adoption of a fairly objective definition of programme intensity and the use of 2008 School Census Survey data allow to identify high and low intensity LGAs. Furthermore, we could determine whether high intensity LGAs recorded faster schooling attainment than those low intensity LGAs.

The rest of this paper is divided as follows. Section two gives an overview of the UPE programme in Nigeria. Section three gives descriptive analysis of the relevant aspects of our data, describing the cohort by cohort progression in schooling attainment and wealth status nationwide and for the 6 geopolitical zones. The cohort by cohort trend in gender differences in schooling attainment and wealth status are presented. In section four, we presented the DD identification strategy and outline the econometric model and results of the impact of UPE programme on schooling attainment. Both sections five and six present the OLS and IV results respectively. In section seven, we address concerns about instrument validity using variety of additional econometric strategies. The paper is summarized in section eight and conclusions drawn.

2.0 Background to the Study

2.0.1 Universal Primary Education (UPE) in Nigeria

In the immediate pre-independence years, the major regions in the South started the implementation of free Universal Primary Education. Lagos, then the Federal Capital Territory, was not left out. However, the programme started in the western region of Nigeria. The implementation started in January 1955 (Bray, 1981; Fafunwa, 1974). Lagos as the Federal Capital Territory outside the control of South-West started its UPE programme in January 1957 (Bray, 1981; Fafunwa, 1974). A month later, the Eastern region launched its own UPE programme (Bray, 1981; Fafunwa, 1974). The Northern section did introduce conditional UPE programme in 1958 given that it should implemented by localities when qualified teachers are available (Bray, 1981; Fafunwa, 1974), it is actually difficult to say whether what was implemented could be the equivalent of a UPE programme. For whatever it is worth, UPE programme in the North was nothing in magnitude to what was put implemented in the various parts of the South. Table 1.0 reveals that schooling attainment is significantly lower for various parts of Northern Nigeria relative to the Southern parts for beneficiaries of UPE programme (44-53 and 54-63 cohorts), except for the North-Central which previous exposure to highly subsidized missionary education.

The federal government of Nigeria later initiated the programme in 1974 and began its implementation in 1976 (Bray, 1981; National Education Policy, 1981; Obasi, 1997; Ozigi and Ocho, 1981). The objective of Nigeria's UPE program was to provide tuition-free universal primary school education for six years. A school entry age of six was stipulated in the official UPE programme of the federal government. Previous UPE programmes at the regional levels came with its specifications. This is partly a reflection of the intense political competition among the major parties in control of the various regions. Thus, each political party had specific criteria for the implementation of its educational programme. The Action Group which started the programme in 1955 in the South-West Nigeria (Bray, 1981; Fafunwa, 1974) originally recommended eight years of free and compulsory primary school education (Bray, 1981). Because pupils enrolments exceeded official estimates, the government of South-West reduced years of primary education to 6 from 8 and dropped the compulsory aspect of its UPE programme. The financial resources for the implementation of its programme given the huge enrolment rate were simply beyond what the region could afford at the time. In addition, primary education of eight years could lower the school completion rate. Thus, reducing years of primary education could increase the proportion of pupils completing primary education (Bray, 1981). In order to cope with the fiscal constraints imposed on the regional government because of the programme, some other levies were

introduced. These include textbook fees, building levies, and examination fees. They were introduced on a school-by-school basis (Bray, 1981: 29). Though tuition fees were never reintroduced, the other levies were charged to raise resources required for school maintenance.

The UPE scheme in South-West Nigeria eliminated school fees for all years of primary education, though other expenses were borne by parents of pupils. The elimination of tuition had dramatic impact on pupils' enrolments. The Western Region UPE scheme accomplished much in its early years, raising enrolments from 35 percent of the 5-14 age group in 1954 to 61 percent in 1955, and 90 percent in 1960 (Bray, 1981: 29). The rise in children's school enrolment is seen as one of the most important accomplishments of the UPE program in western Nigeria.

A number of problems accompanied the implementation of the UPE program in Western Nigeria. For instance, teacher quality was a major concern. In many of the schools, unqualified teachers and staff were engaged to teach the children and administer the school. The general impression is that employed teachers never had more than a fourth grade education, or probably less (Bray, 1981). Basic teaching facilities and materials such as blackboards, chalk, erasers, and building infrastructure that are important for learning were insufficient in most schools. For a region already spending about 40 percent of its recurrent expenditure on UPE programme, the problems of poor school buildings and lack of teaching materials reflect the financial constraints faced by the governments of the region at the time (Bray, 1981; Fafunwa, 1974).

Lagos as the Federal Capital Territory had the most successful UPE programme, partly because of its greater financial resources as the centre of commercial activities in the country and much more so because of its smaller size. Its financial resources are considerably higher than that of the Western and Eastern regions combined (Bray.1981). Its 1957 plan stipulated that primary school would be eight years in length. The downside to its programme implementation was because of substantial increase in school aged children's enrolment and shortage of school buildings. To cope with the problems, the region created multiple-shift teaching system in order to accommodate the number of children who wanted to attend school (Bray, 1981).

The UPE programme in South-East Nigeria was dramatically different from what obtains in the Western region and Lagos. The National Council for Nigeria and the Cameroons (NCNC) was the political party which dominated the Eastern region proposed that primary school in the region would initially be eight years. Subsequently, it would be reduced to six years as teaching efficiency is radically improved (Bray, 1981).

An important part of the UPE scheme in Eastern Nigeria was the initial proposition includes government funding the first four years of primary school education while the local community would have to pay the cost of the last four years of primary school education (Bray, 1981). In the same proposal, UPE programme will not be made compulsory until the local governments in the South-East have the resources to enforce such a rule (Bray, 1981). When Action Group, the party in charge in the Western region introduced tuition-free UPE for the first six years, eastern region made significant changes in its UPE programme. An important part of the modification involved the provision of tuition-free primary education

eight years (1981). This change would rival the UPE programme of Action Group in Western Nigeria.

The Eastern region also had to recruit essentially unqualified teachers for the implementation of its programme (Bray, 1981). Nevertheless, enrolment rates rose considerably after UPE implementation. With initial enrolment of 518,900 pupils in 3, 500 primary schools across the region in 1952, the total pupils' population had risen to 1,194,400 in 6,700 schools in 1957 this had expanded (Bray, 1981: 30-31).

In response to challenge from the regional governments of Southern Nigeria, the North of Nigeria introduced its Universal Primary Education (UPE) in 1958 (Bray, 1981; Fafunwa, 1974). Its UPE proposal stipulated seven years of primary school education (Ozigi and Ocho, 1981). The UPE program in Northern Nigeria also emphasized the importance of local communities in the administration of primary education.

3.0 Descriptive Analysis

The 2008 Nigerian Demographic & Health Survey (NDHS) is the main data used in this study, with extra information drawn from 2006 Nigerian Census, 2008 School Census and 2005/2006 Labour Force Survey. From these data sources, we estimate the causal impact of schooling on individual economic well-being. This is done using a combination of variables that cover personal, family and community level characteristics of individuals.

While the 2008 NDHS is collected from men aged 15-59, women within 15-49 cohorts, children, couple and household, this study depends essentially on household data. The household data cover individuals aged 0 to 96 years. There are 156,809 individuals in the household dataset with only 80,464 falling in the age group 15-65, 66,712 in 20-65 and 54,769 in the 25-65 cohorts and 55864 in 30-65 cohorts.

The average year of schooling for individual is **4.35**. As is observable, the average years of schooling rises progressively as we move down the age-cohort shown in table 4.0. While the initial rise in average schooling attainment is barely significant, a more noticeable increase is observed at some point, probably coinciding with period of large-scale nationwide expansion in primary schooling in 1976.

When the data is further disaggregated into the size informal geographic zones of Nigeria, the trend is made clearer. Zones in northern Nigeria apparently seem to have made more noticeable progress in schooling attainment for cohorts likely to have benefitted from the 1976 Universal Primary Education (UPE). One related fact is that average number of schools constructed between 1974 and 1981 for each Local Government Area (LGA) per square kilometre is considerably higher than what obtains in the south[7]. This is line with evidence provided Osili and Long (2008) study budgeted expenditure per capita at the state level.

The logarithm of wealth index, which is our proxy for individual economic wellbeing, shows noticeable improvements as we move down the age cohort, excluding individuals in the 0-19 category. At the level of the geopolitical zone, the wealth index rises sharply. The rise, as is the case with average years of schooling attainment, becomes dramatic at some point. The

[7] This was done to bridge the educational gap caused by unequal exposure of the North and the South to pre-colonial and colonial missionary education.

age cohort that had the highest years of schooling also has the greatest wealth index. What cannot be inferred from some measure of confidence is whether the relationship is causal, and whether the direction of causation runs from schooling to wealth?

At the level of geographical zone, the wealth variable rises sharply just as the years of schooling, more so for urban than rural dwellers (male versus female). As the gap in average years of schooling between similar cohort across geopolitical zones declines as we move from older to younger cohorts, so did wealth. In sum, it does appear the inequality in wealth distribution across geopolitical zones over time, though the difference is still significant.

To get a better idea of the impact of UPE programme on schooling attainment and wealth status, we separated the data into age-cohort that benefitted from the UPE programme (34-43 the treated group) and the cohorts who did not (19-33 & 44-96 as control groups). At the national and zonal levels, the treated group has both higher levels of average schooling and wealth status relative to the control groups. The differences for both variables for both treated and control groups are larger for the zones of the North than those of the South. The tentative, but inconclusive evidence from this analysis is that the UPE programme might be indirectly increasing individual economic wellbeing by increasing their schooling attainment.

A similar picture emerges when we compare cohorts from treated and control groups across geopolitical zones, from previous implementation of regional UPE programme in South-West and South-East of Nigeria. For instance, those who range from 49 to 64 in 2008 in both the South-West and South-East Nigeria and benefitted from exposure to free primary education have higher average years of schooling compared to similar cohorts from Northern Nigeria who did not. Thus, the initial advantage due to early exposure to missionary education, which created an educational gap between Northern and Southern Nigeria, was further accentuated by the implementation of large-scale free primary education at the regional level by zones in Southern Nigeria.

This regional implementation of UPE increased enrolment by improving access to schools and reducing the costs of schooling. The 1976 UPE seeks to bridge the zonal gap in educational inequality by raising both access and reducing costs of schooling in the Northern part of Nigeria while eliminating only tuition costs in south states.

Table 1.0 Summary Statistics

Panel A	**Average Schooling Attainment (S) and Wealth Status (W)**													
	national		**SW**		**SS**		**SE**		**NC**		**NE**		**NW**	
Age Range	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**
<3	0	0	0	0	0	0	0	0	0	0	0	0	0	0
4-13	2.52	-	2.36	2.88	2.28	2.74	2.04	2.71	1.62	2.4	0.971	1.84	0.9412	2.11
14-23	6.66	**4.22**	9.09	2.95	9.07	2.8	9.04	2.83	7.06	2.5	4.038	1.893	3.788	2.22
24-33	**7.06**	**3.53**	10.15	3.02	10.03	2.9	9.98	2.91	7.532	2.54	3.858	1.927	3.602	2.18
34-43	**6.33**	**3.18**	**9.48**	**3.01**	**9.17**	**2.85**	**8.40**	**2.84**	**6.92**	**2.55**	**3.436**	**1.88**	**3.016**	**2.11**
44-53	4.68	2.96	7.26	2.90	7.27	2.75	6.30	2.76	4.92	2.52	2.00	1.76	2.210	2.07
54-63	3.04	1.9	4.59	2.77	4.95	2.6	4.04	2.71	2.92	2.40	0.978	1.7	1.052	2.01

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64-73	2.19	1.67	3.05	2.70	4.03	2.6	3.00	2.72	1.45	2.29	0.6864	1.73	0.4568	1.95
74-83	1.29	1.77	1.87	2.66	2.83	1.59	1.58	2.71	0.708	2.19	0.3014	1.71	0.2109	1.98
84-93	1.00	1.54	1.53	1.75	1.3	2.66	1.74	2.83	0.268	2.38	0.2424	1.72	0.3492	2.09
>93	1.07	1.25	0. 53	2.45	1.7	2.53	1.26	2.73	0	2.47	0.800	1.65	0	1.88
Panel B	**Mean Differences in Schooling Attainment & Wealth Status by Gender**													
	national		**SW**		**SS**		**SE**		**NC**		**NE**		**NW**	
Age Range	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**	**S**	**W**
<3	0		0		0		0	0	0	0	0		0	0
4-13	0.07		-0.06	-0.05	-0.15	-0.15	-0.06	-0.04	0.04	-0.05	2.96	-0.01	0.23	0.03
14-23	1.16		0.33	-0.02	0.04	-0.04	-0.09	0.04	1.22	0.06	2.54	0.01	2.34	0.06
24-33	1.26		1.36	0.05	0.00	0.00	0.64	0.14	2.83	0.04	2.64	0.07	3.25	0.11
34-43	1.64		1.98	0.09	2.26	0.10	1.47	0.15	3.48	0.07	1.76	0.05	2.87	0.06
44-53	**1.9**		**3.01**	**0.09**	**4.68**	**0.11**	**3.6**	**0.15**	**4.96**	**0.15**	**1.37**	**0.06**	**2.63**	**0.06**
54-63	1.24		3.58	0.12	4.13	0.14	3.02	0.06	3.08	0.12	1.29	0.03	1.29	0.02
64-73	1.03		2.7	-0.03	3.57	0.09	3.49	0.05	1.59	0.06	0.86	0.04	0.52	-0.14
74-83	0.42		1.99	-0.07	3.39	0.05	2.42	0.07	0.51	-0.15	0.61	-0.26	0.06	-0.18
84-93	0.37		1.92	0.05	1.05	0.02	2.89	-0.02	0.71	-0.15	0.32	-0.34	0.51	-0.12
>93	0.23		0.83	-0.03	2.11	-0.33	2.0	-0.11	0.72	0.13	0.05	-0.65	0.00	-0.13

One noticeable trend a cross the 6 geopolitical zones is the progressive rise in years of schooling as you more from older to younger cohorts except for the youngest two cohorts whose members are principally with in the school age category and may not have completed schooling activities. But the rise has been marginal in nearly the 3 geopolitical zones of one north for the first 6 older cohorts that is those falling within 96–44 age category. While exposure to subsidized missionary education led to a steady rise in schooling attainment in the 3 southern zones a dramatic rise occurred for those who fall within 48–65 cohorts. Majority of 44–53 and 54–63 cohorts' mere beneficiaries of regional UPE implemented within 1955 to 1966. The mere fact that schooling attainment rose faster in the South than in the North this caused the educational gap to get wider between the two zones. The divergence is caused by imperceptibly progress made by both the North-East and North-West, while North Central, which limited exposure to Christian missionary education, has made some modest progress.

In the entire zones of the north, increase in the years of schooling was more remarkable the time of the UPE programme. This is noticeable as we move from cohorts 44–53 to 34–43.

The later cohorts are the beneficiaries of the 1976 UPE programme. The jump is 2 year(s) of schooling for beneficiaries in North-Central, 1.44 for those in North-East and 0.81for North-West. Because greater number of schools was built in the North during the UPE programme, substantial reduction in individual costs of primary education came through school fee elimination and reduction in distance to school. The latter was probably more important for the North than fee elimination because with considerably lesser number of schools than the South and with 70 percent of the country land mass getting to the nearest school was costly. Currently, costs of reaching schools may still be substantial (Lincove, 2009). However, combination of tuition fee elimination and reduction in access costs to primary did not have as much a dramatic effect on schooling attainment as much as tuition fee elimination had on schooling attainment in the South. For each of the three zones in the South, the least increase in years of schooling attainment is 1.9 for the South-South.

Through the 1976 UPE programme formally ended in 1981 there was no dramatic drop in years of schooling through attainment for all regions, but there was no drastic improvement either. The two subsequent cohorts (24-33) had to pay tuition fee in many parts of the country outside the southwest, but had the added advantage of closer schools and lower access costs when compared to the pre-UPE cohorts. The results show to some extent that UPE programme raised years spent in school for the affected cohort (34 - 43) more than any other cohorts.

Panels B of table 1.0 help to break the aggregate figure in panel A into their gender components. In all the three zones of southern Nigeria, there is slight rise in schooling attainment and wealth status, particularly after the implementation of regional UPE programmes in the Western and Eastern parts of the South. This is in line with Nwachukwu (1985:145) finding that general school enrolment rose about 40 percent in 1966 in the South-West from 20 percent enrolment in 1954.

In the lower panel of table 1.0, the difference in the average years of schooling for male and female respondents in the sample is shown. For the zones in Southern Nigeria, the difference in average schooling attainment for male and female declined from older to younger cohorts with more dramatic reduction noticed for cohorts who benefited from regional and national UPE programme. Before the UPE programme, the gender difference in schooling attainment rose considerably from older to younger cohorts (above 93 to 44-53) and drop sharply thereafter.

The northern zones present a slightly different picture. . Generally, the initial gap in male-female average schooling attainment did not decline for those older than 34–43 cohorts. North-Central states recorded a modest drop in gender differences in schooling attainment and barely perceptible rise for North-Eastern states. States in North-Western Nigeria actually had a rise in gender difference in schooling attainment. All cohorts who were too old to benefit from 1976 UPE did not show any noticeable progress in schooling attainment and wealth status. This is much true for North-West and North-East though North-Central made modest progress with some decline in the gender schooling attainment gap. It is likely that some cultural and religious factors may be hindering female child education in the far north crape all 2003.

Overall, the ANOVA (Not shown) analysis of average schooling attainment for whole sample, male sample and female sample across one 6 zones indicate significant differences in schooling attainment across the zones and for all cohort. Though the F-statistic, a measure of the significance of means of schooling attainment declined sharply from older to younger

cohorts, it is still significant for the younger cohort. This is even more striking for cohort 34–43, who benefited from the nationwide implementation of UPE programme and for 4–13 cohorts who had benefited from the 1999 Universal Basic Education (UBE) programme. Thus, convergence might still take a fairly long time to achieve. Hopefully the reintroduction of the programme in 1999, which is now compulsory and has been extended to the first nine years of basic education, convergence might not take a much longer time. This is because the youngest cohort (4–13) has the smallest F-statistic for our ANOVA results, though it is still significant.

In the following sections, we present empirical models and results showing the causal impact of schooling on economic well being. The following sections present our econometric model as well as OLS results. Because of a number of factors that confound OLS identification strategy, the IV econometric strategy is adopted and results are presented to provide greater confidence in the OLS estimates of the impact of schooling on individual economic wellbeing.

While 2008 NDHS data combination of information on personnel, household and community level variables, it does not have information on the income of individuals included in the sample. In addition, 2008 NDHS data have no direct information on migration. Further econometric robustness checks show that migration may not be driving our results.

4.0 Identification Strategy

Our empirical strategy intents to show the strong correlation between school attainment and economic welfare, as graphically illustrated in the introductory section is due to the implementation of one UPE programme, which was announced in 1974 but commenced in 1976 before being brought to an end in 1981.

We use the Difference-in Differences (DID) technique which incidentally is also the first stage of our IV regression, this technique exploits the fact that some people benefited from this programme (the treatment group) and others did not (the control group). Furthermore depending on where people resided when they were qualified for primary school education, members of the treatment group witnessed varying intensity of exposure to the UPE programme. Because of initial disadvantage in school attainment some areas had more resources released for the expansions of primary education. While tuition fee was abolished nationwide larger numbers of schools were in many districts of northern Nigeria, and in a few areas in the south. In the absence of disaggregated at the LGA level, previous studies (Osili and Long 2008; Osili, 2008; Oyelere 2010) could not properly identify the high and low intensity areas. While Oyelere highly defined intensity states as those in the Northern part of the country, Osili and Long definition accommodation states outside of the southwest of Nigeria.

Though, Osili and Long (2008) used capita fund disbursed at the level of the 12 states when the programme was first announced as an objective measure of UPE intensity this is not without its own shortcomings. One macro and micro studies have shown the weak relationship between public expenditure and specified outcome indicators (Easterly and Levire, 2002 : Reinikka and Svensson). Added to this is the fact that 10 out of 12 governors

or administrators who started this programme were found to be corrupt when the Gowon was overthrown in 1975. The administration that succeeded it has no better record of transparency and accountability. One other shortcoming is that the per capita fund disbursed did not fully reflect the spending variation within the states. Within high intensity state located in the North, some LGAs had fairly large number of schools constructed while others had just a few. For instance urban areas had more schools prior to the UPE programme than rural areas.

Te get around this problem, we used the 2008 School Census data to determine the number of schools built across the 774 LGAs in Nigeria between 1774 and 1981. Following Duflo (2001), we regress the numbers of school constructed within 1974–1981 on the population of children in each LGA. Areas that yielded positive residuals ware tagged intensity LGAs and those with negative residuals are called low intensity LGA. A dummy variable value1 is given high intensity LGAs and 0 to low- intensity LGAs.

Unfortunately, we do not have any data on the number of school children at the LGA level in 1976, to derive a school age population data for each LGA in our study; we assume the demography structural composition of the 1976 population is basically the same as that of the 2006 population for which we have data. The percentage of school age children was determined in the 2006 census data, and then mapped into the aggregate 1976 estimated national population. While the overall age distribution of the population may not have changed dramatically between 1976 and 2006, it is most unlikely that other things have remained the same. Some areas have become more rapidly urbanized than others, and have witnessed higher growth rates. Thus our indicator of intensity of UPE implementation will be subject to substantial measurement error, producing bias estimates of the impact of UPE on school attainment. In our empirical analysis, our UPE-intensity variable turns out to be negative and significant at 1 percent (table2.0 panel A).

To overcome this short coming, we use two other alternative indicators of UPE intensity. One is the percentage increase in the number of schools within this period. Two we use the number of schools constructed (within 1976 to 1981) per square kilometer. Aside the abolition of tuition fee the increased in the number of schools per square kilometer expanded access to primary education. The impact was more dramatic in high intensity LGA than in others. Second, we constructed the percentage increase in the number of schools constructed within this period. Both indicators were normalized to mean 0 and standard deviation 1. If normalisation, high intensity LGAs are those with positive values and are assigned value 1 and low intensity LGAs are those with negative values and are assigned value 0. Panels B and C of table 2.0 report empirical results using these indicators.

To measure exposure to UPE by age, treatment group included those who were within 2 –11 age bracket, with those aged 6 and 7 exposed to five full years of UPE programme. Years of exposure declines as push towards 2 and move upward towards 11. Alternative econometric specifications also used dummy variable for exposure to UPE programme, assigning 1 to those in 2-11 age bracket as at 1976 and 0 to others . When the school entry age is raised to 7years due to significant number of reported over age enrolment the same results are

obtained.[8].The same results are obtained if we include those who are three years older than 11years as at 1976 from the control group.

The baseline econometric model which incorporates UPE exposure, UPE intensity and other important covariates is presented as equation one:

$$S_{ik} = \alpha_1 + \alpha_2 UPE\_E_{ik} + \alpha_3 UPE\_I_k + \alpha_4 (UPE\_E)(UPE\_I)ik + \alpha_5 X \quad (1)$$

Where S_{iyk} is years of schooling of individual ***i*** living in LGA k, UPE_E_{ik} is years of exposure to UPE programme of individual i in LGA k and UPE_I_k is the measure UPE intensity at LGA k while X represents a set of other important explanatory variables such age, age squared, gender dummy, location, cohort fixed effect, state fixed effects, marital status dummy etc. The 2008 NDHS household dataset does not have variables on religion, and therefore we are not able to control for it. However, the introduction of state fixed effect dummies should be a large extent, mitigate their non–inclusions since both are to a large extent time invariant in nature.

UPE exposure measures whether individual years of exposure to UPE programme impact on years of schooling attainment, and intensity dummy variable coefficient (α_3) measures the intensity of UPE implementation impacted on years of schooling of individual ***i***. The coefficient (α_3) captures the impact of UPE given that it was implemented with greater intensity in some LGAs than in others. Because it is represented by a dummy variable, it reveals the difference in the average schooling attainment of high-intensity and low-intensity LGAs. Given that high intensity LGAs are assigned 1 and low-intensity 0, there should be greater impact of UPE programme in LGAs with larger number of constructed schools. Therefore, the expectation is that α_3 will be positive. Co-efficient α_4 measures the extent to which high-intensity LGA achieve more or less years of schooling relative to low-intensity LGA conditional on years of exposure to the UPE programme. The a priori expectation is that it should be significant and positive.

The baseline econometric model depicted by equation 1 also represents the first stage regression in an IV strategy that seeks to measure the impact of schooling attainment on a long term indicator of individual well being. It is used to resolve the identification problems often associated with the determination of causal relationship between schooling and economic well being. Ordinary Least Squares (OLS) is incapable of generating an unbiased estimate when the key explanatory is measured with error, one or more explanatory variables are measured endogenous and the model is misspecified. The DD technique allows us to generate predicted values of endogenous schooling variable that are plausibly exogenous, and can be used to determine the causal impact of schooling on individual long term indicator of well–being.

[8] the 1999 multiple indicator cluster survey report indicates that out of 977females enrolled into primary one, 4% are age 4, 19% are age 5, 31% are age 6, 21% are age seven and 15% are above age seven. For 1976, over age enrolment is likely to be more pervasive than is reported in the 1999 MICS).

Our chosen instrument is the years of exposure to UPE programme given the intensity of the UPE programme implementation at LGA level. This instrument, as we would soon see, is highly correlated with the endogenous schooling variable and plausibly exogenous. Our chosen identification strategy allows us to evaluate the UPE programme apart from estimating the causal impact of schooling on economic well being.

The two baseline econometric equations (2) represent the implementation of IV strategy

$$W_{ik} = \beta_1 + \beta_2 S_{ik} + \beta_3 X + \varepsilon_{ik} \quad (2b)$$

$$S_{ik} = \alpha_1 + \alpha_2 UPE\_E_{ik} + \alpha_3 UPE\_I_k + \alpha_4 (UPE\_E)(UPE\_I)ik + \alpha_5 X \quad (2a)$$

W_{ik} is the long term proxy of individual (i) economic well–being who is resident in LGA k. It is computed from household ownership of durable consumer goods, quality of housing and toilet facilities and other characteristics that are reasonable proxies of household socio – economic status.

There are large numbers of negative W_{ik} values in our sample because the welfare index is computed by principal component analysis and the resulting asset scores standardized in relation to a standard normal distribution with mean of zero and standard derivation of one. With negative values, the Mincerian framework with semi – logarithmic specification can not be used. There are no logarithmic values for negative observations. Thus, the W_{ik} observations were transformed in order to eliminate negative values. To do this, we found the smallest value of W_{ik} and then added this value plus one to all W_{ik} observations[9]. The logarithm of these transformed observations is their obtained. Thus, our dependent variable is the logarithm of transformed W_{ik} observations. X is a set of control variables mentioned previously. However, S_{ik} is now the predicted values of endogenous schooling variable purged of its endogenous component in first stage regressions represented by 29. The predicted values of S_{ik} are used in second stage regressions depicted by 2b.

4.01: 1976 UPE and Schooling Attainment: The Differences-in-Differences Approach

In this section of the study, we present result on impact of UPE on schooling attainment. Later, we will present OLS and IV results of UPE impact on our proxy of long term labour market performance. Results for 5 age cohorts are shown in table 2. The baseline cohort is 15-96, assuming the working age starts at 15. We extended beyond the official terminal working age of 65 because we also want to see the impact of UPE on post working life welfare. For comparison we use 15–65 age cohorts, which is limited to the working age bracket (Aromolaran 2006). Since schooling is unlikely to have been completed for a substantial number of persons, we progressively raised the age labour market entry to 20,

[9] If for instance the smallest value is –x, we added x+1 to all W_{ik} observations. Qualitatively similar results are obtained if fractional values of all W_{ik} observations are first obtained and the value 1 is later added to the transformed values to change into non-negative values.

then to 25 and 30, while limiting the age of labour market exit to 65. As in Oyelere (2010), those who are in 2–11 age cohort as at 1976 benefitted from the UPE programme. There beneficiaries were 34–43 year old when the fourth wave of NDHS was done in 2008. Those who are 6 to 7 years old in 1976 had five full years of UPE programme. Years of exposure to UPE declined as you move towards the lower and upper limits of 2-11 age brackets. Though the treatment and control groups were constructed assuming that school entry age is 6, raising it to 7 year did not change our results[10]. Those below 34 year old and those above 43 years old as at 2008 are in the control group. Because selected number of those above 43 years could have benefitted from previous regional UPE programmes, we control for years of exposure to previous regional UPEs in some of our estimations. To account for substantial over-age enrolment when the UPE programme, stated in 1976, we excluded from our control groups those who are 3years older than 11 in 1976 or 34 in 2008.

Table 2 has three panels. Each panel uses each of the three measures of UPE programme intensity at the LGA level. As shown in panel **A** of table 2, UPE exposure has significant and positive impact on years of schooling even at 1per cent. However, UPE intensity has significantly negative effect on years of schooling. This implies the larger the number of schools the lower the years of schooling. This result could be due the way the children population as at 1976 was computed. Panels B and C of table 2.0 report results using two alternative indicators of UPE programme intensity. As in panel A, the results are similar UPE

Table 2.0 UPE Programme: Impact Evaluation on Schooling Attainment

Panel A	**UPE Programme: Impact Evaluation on Schooling Attainment**		
Independent Variables	**Dependent Variable : Years of Schooling**		
UPE_Exposure	**0.921787*** **(0.017237)**	**0.900543*** **(0.016677)**	**0.9021643*** **(0.016444)**
UPE_Intensity	**-2.533291**** **(0.035467)**	**-2.5291888**** **(0.033489)**	**-2.5366634**** **(0.033221)**
(UPE_Exposure)(UPE_Intensity)	**-0.488739***** **(0.0257839)**	**-0.476893***** **(0.0222559)**	**-0.4739321***** **(0.0221118)**
Individual control variables	**Yes**	**Yes**	**Yes**
Previous UPE dummies	**Yes**	**Yes**	**Yes**
State FE	**No**	**Yes**	**Yes**
Cohort FE	**No**	**Yes**	**Yes**

[10] However, raising school entry age to 8 years produced a very insignificant impact of UPE on schooling attainment, and using this for our IV estimation will result in the usual weak instrument problem.

UPE-Enrol	**No**	**No**	**Yes**
Log (1 + number of dams)	**No**	**No**	**Yes**
R-Squared	**0.5269**	**0.5360**	**0.5472**
No of observations	**85, 580**	**85, 476**	**85, 476**
Panel B: UPE Programme: Impact Evaluation on Schooling Attainment			
UPE_Exposure	0.6290198* **(0.0152659)**	0.6265538* **(0.0152432)**	0.617297* **(0.005782)**
UPE_Intensity	0.01438855* **(0.0247713)**	0.1446984* **(0.247595)**	0.1426562* **(0.246424)**
(UPE_Exposure)(UPE_Intensity)	0.4093089* **(0.023078)**	0.4077504* **(0.0233932)**	0.394779* **(0.0232425)**
Individual control variables	**Yes**	**Yes**	**Yes**
Previous UPE dummies	**Yes**	**Yes**	**Yes**
State FE	**No**	**Yes**	**Yes**
Cohort FE	**No**	**Yes**	**Yes**
UPE-Enrol	**No**	**No**	**Yes**
Log (1 + number of dams)	**No**	**No**	**Yes**
R-Squared	**0.5269**	**0.5360**	**0.5472**
No of observations	**85, 580**	**85, 476**	**85, 476**
Panel C: UPE Programme: Impact Evaluation on Schooling Attainment			
UPE_Exposure	0.6290198* **(0.0152659)**	0.6265538* **(0.0152432)**	0.617297* **(0.005782)**
UPE_Intensity	0.01438855* **(0.0247713)**	0.1446984 * **(0.247595)**	0.1426562* **(0.246424)**
(UPE_Exposure)(UPE_Intensity)	0.4093089* **(0.023078)**	0.4077504* **(0.0233932)**	0.394779* **(0.0232425)**
Individual control variables	**Yes**	**Yes**	**Yes**

Previous UPE dummies	**Yes**	**Yes**	**Yes**
State FE	**No**	**Yes**	**Yes**
Cohort FE	**No**	**Yes**	**Yes**
UPE-Enrol	**No**	**No**	**Yes**
Log (1 + number of dams)	**No**	**No**	**Yes**
R-Squared	**0.5269**	**0.5360**	**0.5472**
No of observations	**85, 580**	**85, 476**	**85, 476**

****Significant at 1 percent, ** significant at 5 percent and *** significant at 10 percent. Individual control variables include age, age-square, sex dummy, sector dummy and marital status dummies. Cohort fixed effects use 10 dummies based on the age categories presented in table 1.0. State fixed effects include a set of 36 state dummies using the Federal Capital Territory (F.C.T) as the base dummy variable. The UPE-Enrol variable is constructed at the local government level. It is the LGA enrolment rates prior to the outset of the 1976 UPE programme.***

exposure has significant and positive impact on years of schooling attainment. LGAs were the UPE was implemented with greater intensity record oh higher schooling attainment compared to those who lower UPE intensity. A year of UPE programme raised individual attainment by 0.34year for treatment group relative to control group while higher intensity LGA on average has 0.25 more year of individual schooling attainment compared to these in lower intensity LGAs. For Osili and Long (2008) exposure to UPE programme raised years of schooling attainment by 1.54 for women in the treatment group relative to those in the control group. For both sexes, Oyelere (2010) reports that a year exposure to UPE raised schooling attainment by 15 percent every year. From Duflo (2001) seminal study, the implementation of similar programme in Indonesia had significant impact on years of completed schooling. For instance, each primary school constructed per 1000 children resulted into an average increase of 0.12 to 0.19 years of schooling.

Our results are robust to the inclusion of various control variables such as age, age–square, marital status dummy, sex dummy and location dummy. In fact variables of UPE exposure, UPE intensity and the interaction of UPE exposure and UPE intensity are still significant at per cent.

However, there is still plausible reason to believe that unobserved variables at the state level and at the level of age cohorts might be driving our results. Thus, we introduced state fixed effects to account for unobserved state level heterogeneity. This should deal with bias resulting from time invariant, unobserved state level heterogeneity. For instance, the socio-economic indicators were most likely lower in LGAs which had greater number of schools during the UPE programme. Thus, introducing state fixed effects (Abuja is base dummy) absorbs the impact of initial differences in LGA socio-economic level of development. Our regressions also control for the fact that UPE cohorts were exposed to other government programmes, policies and events relative to pre – and post UPE cohorts which affect schooling attainment. Thus, cohort fixed effect variables were incorporated into the first econometric model. While the estimates of the UPE exposure, UPE intensity and the

interaction of both on years of schooling attainment dropped, they remain significant at 1 per cent.

Our identification assumption could be biased upward by unobservable time varying factors specific to states and LGAs in the country. Apart from the fact that more schools were built in areas with lower schooling attainment during the UPE, drastic change were also made to the federal revenue sharing formula, principally for promoting even development across states and regions (Wantchekon, Asadurian and Nnadozie, 2005). Thus, other public projects could have been implemented with greater intensity in poorer LGAs than richer ones. It is important to note that a significant fraction of public projects were implemented at the same time as the UPE programme. Due to comparative in agriculture, a significant number of dams and water projects were established in the North. The introduction of variables capturing this could change the statistical significance of previous estimates. The implication is that we would be overstating the impact of UPE on schooling attainment because of the presence of confounding time varying factors. A promising approach is to use the number of dams established to provide electricity, water and irrigation services at the time the UPE programme was being implemented. The logarithm of 1 plus the number of dams created during UPE period at the state level was added to our economic model. This did not fundamentally change our results. Unfortunately, we do not have enrolment data for LGAs prior to the period of UPE implementation. To get an idea of the enrolment rate prior to the introduction of UPE programme, we check the 2008 NDHS data for the total number of school-age (6-12) respondents per LGA that reported positive years of schooling and the total number of school-age respondent. The ratio of the two is our measure of school enrolment rate at the outset of the UPE programme[11]. The inclusion of this variable did not alter the statistical significance of our estimates. There is some confidence that the increase in schooling attainment is due to the UPE.

If our DD strategy is valid we should not find exposure to UPE having positive and significant effects on cohorts younger or older than 34-43 cohorts. The least we expect that it could be modestly significant for cohorts slightly below 33 or a tad above 43 to take care of underage or overage enrolment. For cohorts older than 46 years or younger than 30, exposure to UPE should not have any significant effect on schooling attainment. Further tests (NOT SHOWN) reveal moderate relationship between schooling attainment and UPE exposure dummy for those three years older than 43 in 2008. Beyond 46 years, the coefficients of UPE dummy are positive but insignificant.

In comparison with the treated cohort (34 -43), UPE had modest impact cohorts aged 44-53, who are 12 years and above when the programme started in 1976 but nevertheless benefited from the programme.

5.0 Econometric Model and Empirical Results

5.01. OLS Estimates

[11] In 1976, those in primary schools should be within the ages of 6 and 12, extending to say 14 years to account for over-age enrolment.

We begin by estimating the relationship between the female years of schooling the two indicators of well-being. Our baseline estimating equation is:

$$W_{ik} = \beta_1 + \beta_2 S_{ik} + \beta_3 X + \delta + \theta + \varepsilon_{ik} \qquad (3)$$

where all symbols retain previous meaning. In addition, we have δ as a set of state fixed effects variables picking unobserved variables driving outcomes aside schooling attainment and included control variables. θ is cohort fixed effect to pick up the possibility that different cohorts might face different government policies that also affect outcome variables independently of other variables included in the econometric model.

From the first part of table 2.0, female schooling attainment has strong and positive impact on economic status of all. On the average, an extra year of female education increases farm profit 7.31. This is significant at 1 percent. This is much higher than the overall estimates reported by Aromolaran (2004, 2006) for persons with secondary education and less. It is considerably higher than the IV estimates of Oyelere (2010) as average returns to education in Nigeria. In most of the OLS specifications, age and age-squared variable assume the expected signs, with wealth status first increasing with age before declining.

Table 3: OLS RESULTS (DEPENDENT VARIABLE : WEALTH INDEX)

SAMPLE	All Women Sample	All women sample minus Lagos & Abuja	Minus Southwest	Minus Southwest & Abuja
Schooling years	***0.0853** (0.0003207)	***0.0835** **(**0.0003298)	***0.0832** (0.0003617)	***0.0798** (0.0003874)
Schooling years coefficients after controlling for Log of 1 +plus no. of dams and sanitation projects*	***0.0851** (0.0003102)	***0.0822** **(**0.0003298)	***0.0855** (0.0003367)	***0.0792** (0.0003845)
Adj R2	0.5760	0.5874	0.6046	0.6076
Observations	4372	4123	3148	3069
Prob > F	0.000	0.000	0.000	0.000
F (Statistic)	2878.47	2968.36	2989.27	2978.25

****Significant at 1 percent, ** significant at 5 percent and *** significant at 10 percent. Individual control variables include age, age-square, sex dummy, 3 ethnic dummies, English speaking dummy, sector dummy and marital status*

dummies are included in the regression. Cohort fixed effects, State fixed effects and UPE-Enrol variable is constructed at the local government level are also added. Standard errors are in parenthesis.

However, different parts of the country have varying degree of urbanization. Thus, locations such as Lagos and Abuja have greater urban representations. Similarly, all states from the south-west of Nigeria have greater urban locations in NDHS data than other locations. Since amenities and opportunities are not equally distributed across rural and urban locations in Nigeria, it is expected that OLS estimates will be biased by this spatial inequality. To correct for this potential source of bias, we excluded Lagos and Abuja locations from our sample. The estimate of schooling variable diminished slightly to 7.1 percent but remained significant at 1 percent. If all locations in the Southwest are removed from the sample, an extra year of female education raises wealth by 7 percent. When locations from southwest and Abuja are dropped from sample, the impact of schooling is reduced to 6.9 percent. When rural and urban location samples are estimated separately, the estimate is 7.5 for rural locations and a unit lower for urban locations.

4.0.2 Estimating Causal Relationships

The positive correlation between schooling attainment and economic status that is documented in the previous sub-section is consistent with our expectation that schooling augment skills that raise long term wealth status. However, the correlation could also be explained by omitted variables that are correlated with selection into schooling activities and with subsequent outcome variable wealth index. For example, extensive irrigation and water supply facilities, important for outcome, are provided with significant variations across the country. For the northern part of the country with comparative advantage in agricultural activities, disproportionately large number of large-scale irrigation facilities was established. Second, unobserved variables, for example ability, might also matter for outcome.

To take care of these possibilities, we pursue two strategies to assess whether the correlations documented to this point are causal. First, we control for observable characteristics such that the logarithm of dams per LGA and a set of dummies that capture the presence of water and sanitation facilities across the LGAs. Table 3 reports schooling coefficients after controlling for the selective placements of dams and water projects. For the various regressions carried out, estimates range from 7.9 to 8.6.7 percent for extra year of schooling. However, all estimates are statistically significant.

Though we control for observable factors, such as magnitude of irrigation and water-supply cum sanitation facilities across the states in Nigeria, our estimates may still be biased by unobservable factors correlated with selection into the schooling investment, and also correlated with welfare indicator of the respondents. In the final part of this subsection, we assess the likelihood that the estimates are biased by unobservables. The strategy that we exploit the strategy suggested by Altonji, Elder and Taber (2005) that selection on observables can be used to assess the potential bias from unobservables. Their work provides a way of measuring the magnitude of the likely bias arising from unobservables: how much stronger selection on unobservables, relative to selection on observables, must be to explain away the full estimated effect (Nunn and Wantchekon, 2011).

In our study, we constructed and estimated two regressions with restricted set of control variables, and another two with a full set of controls. In one restricted set, we have just the schooling variable in a bivariate regression model. In the second restricted set, we have individual control variables added to the schooling variable. In the regressions with full set of

controls, one had 36 state fixed effects dummies to complement variables in the restricted set and another included 10 cohort fixed effect dummies in addition to variables in the restricted set and state fixed effects variables. Panel B in table 4.0 reports results obtained after applying the formula suggested by Altonji, Elder and Taber. We find that the influence of unobservable factors would have to be between four and ten times greater than observable factors. Therefore, it is unlikely that our estimates can be fully attributed to unobserved variables.

Table 4 : Testing for Potential Bias from Unobserved Variables.

Controls in the restricted set	Controls in the full set	Income per month	Asset index
None	All Controls stated in equation one	**4.59**	**5.21**
None	All control variables in the equation plus state fixed effects (36 dummies) and cohort fixed effects (10 dummies)	**9.46**	**7.78**
Age, Age squared , sector, family size.	All controls stated in equation 1	**4.28**	**4.87**
Age, Age squared, Marital Status, Sector, input index, cropping method dummy.	All control variables in the equation 1 plus state fixed effects (36 dummies) and cohort fixed effects (10 Dummies)	**10.23**	**7.51**

6.0 Instrumental Variable Strategy

6.01 Instrumental Variable (IV) Results

Though the previous section proves somewhat convincingly that omitted variables might not be driving our OLS results, endogeneity of schooling variable and measurement error are still important sources of biases. Our final strategy is to use of instrumental variables to resolve the remaining source of biasness. This requires an instrument that is correlated with the female years of schooling and affects outcome variables only through years of schooling attainment. We use exposure to 1976 UPE programme, the variation in intensity of UPE program and the interaction of both as instruments for schooling. As previously discussed, the program was implemented to raise the schooling attainment of the people, and more specifically increase enrolment rates in the Northern part of the country. As our subsequent analysis will reveal, the instrument turns out to be strong and valid.

The instrument is constructed using age of respondents in the 2008 Nigeria DHS to determine who benefitted from the UPE program and the degree of exposure in years to the UPE program. Assuming 6 years as age of school entry, we calculate that only those within 34-43 age brackets benefitted from the program. Younger or older cohorts did not. Because the program lasted for 5 years, the maximum years of exposure are five for some and less than 5 for others. To obtain a measure of the intensity of program implementation, we use the 2008 School Census Survey data, which contain the names of all primary schools and their year of establishment, to construct the number of schools built during the program for each LGA in our sample. After estimating the mean number of schools built per LGA and standardizing it so that it has a mean of zero (0) and standard deviation one (1), LGAs with zero and positive mean values are regarded as high-intensity areas, those with negative are regarded as low-intensity. This is better that the arbitrary definition of program intensity adopted by Osili and Long (2008) and Oyelere (2010).

Table 5 : IV ESTIMATES

SECOND STAGE **DEPENDENT VARIABLES:**	**WEALTH INDEX**
Schooling years	***0.1509171** (0.00670192)
R. Squared (Adjusted)	0.5109
Overidentification test (P – Values)	0.3365
FIRST STAGE : DEPENDENT VARIABLE: SCHOOLING YEARS	
Exposure to UPE (IN Years)	***0.368647** (0.0067766)
UPE Implementation Intensity	**0.18911125** (0.0146618)
(UPE_ Exposure)(UPE_ Intensity)	***0.382544** (0.00722898)
Year at birth	***0.0543427** (0.0056745)
Year at six	***0.0748857** (0.0492648)
Year at twelve	***0.1966125** (0.0169854)
F- Statistic of excluded instrument.	2986.54
R- Squared (Adjusted)	0.5387

****Significant at 1 percent, ** significant at 5 percent and *** significant at 10 percent. Individual control variables include age, age-square, sex dummy, 3 ethnic dummies, English speaking dummy, sector dummy and marital status dummies are included in the regression. Cohort fixed effects, State fixed effects and UPE-Enrol variable is constructed at the local government level are also added. Standard errors are in parenthesis.***

Compared with the rule-of-the-thumb value of 10 for F-Statistic (Bound, Jaeger and Baker, 1996) all our first stage regressions show that instrument set is relevant. The important issue is whether our instrument set is uncorrelated with factors, other than schooling attainment, that may affect female welfare outcomes such profit and wealth index–for example, nationwide water and sanitation programs implemented about the same time as the UPE program, which may have affected individual schooling attainment, as well as its subsequent economic status. Our IV strategy account for this possibility.

We report IV estimates for each of the two measures of economic welfare in table 5.0. To save space, the individual control variables as well as state and cohort fixed effects are not shown on the table. Similarly, variables for log the number of irrigation and sanitation projects at the LGA level are not shown. Table 5 lower panel shows the impact of our instrument on female schooling attainment. A year of exposure to UPE increases schooling attainment by about 0.39 years, with high-intensity LGA having 0.19 year of more schooling than the low-intensity LGA. The F-Statistic of 550.41 shows that the instrument is relevant, and that is about the least for most of our IV regressions. Thus, it passes the benchmark of 10 for weak instrument. Thus, the program helps to bridge the educational gap between the North and South of Nigeria.

In the second-stage regression, estimates show a positive and highly significant effect of the schooling attainment on wealth index. As reported in some studies (Card 1995), the magnitudes of the estimates are noticeably higher than the OLS estimates. In fact, in all specifications, the Durbin-Wu-Hausman test rejects the null hypothesis that the IV and OLS estimates at the five percent level or lower are the same. These results suggest that measurement error might be biasing downward our estimates more than endogeneity is raising it.

7.0 Instrument Validity Problems

Our choice of instrument set is complicated by the fact that the state of the economy affects the demand and supply of education. The state can expand the supply of educational services during period of rapid economic growth and reduce supply when there is a downturn. For the same reason, demand for education by the households and individuals could be high during periods of rapid economic growth and low during downturns. Thus, if UPE was introduced because the economy was growing rapidly and stopped because of a downturn, then our IV approach will produce bias results from failure to satisfy the exclusion restriction condition. According to Oyelere (2010), this is not likely to be the case because crude oil price was yet to reach an all-time high of about $40 per barrel it assumes in 1979. UPE was not introduced by general demand but arose largely from the preference of commander. Also reassuring is the fact that while the percentage of government expenditure spent on education fell after SAPs were established, the school enrolment of children of primary school age increased between 1970 and 1995. In 1970, the primary school enrolment was 3.5 million. The figure increased to 14.6 million in 1983 and it drop slightly to 14 million in 1990. By 1995, it has risen to 17 million (Geo-Jaja and Mangum, 2003). Data from UNESCO (2008) data also revealed that there has been an upward trend in the primary school gross enrolment ratio (GER) of children in Nigeria. Furthermore, it shows the country's primary school enrolment ratio in the post-SAP years, 1999-2002. In 1999, primary school enrolment rate in the country was 88 percent. This increased to 91 percent in 2002 (UNESCO, 2008).

This information demonstrates that, even under the conditions of SAPs and the decrease in government spending on education, households in Nigeria have continued to send their children to school, presumably bearing an increased cost of their children's education (Geo-Jaja and Mangum, 2003). In contrast, in other developing and sub-Saharan African countries, research finds that adjustment efforts resulted in sharp declines in school enrolment (see Cornia, 1987; Geo-Jaja and Mangum, 2003; Lockheed and Verspoor, 1991). However, scholars such as Geo-Jaja and Mangum (2003) state that Nigerian education quality degenerated and suffered in the midst of the rising enrolment and reduction in government spending. In specific states of the country, the school enrolment of poor children and girls, particularly at secondary and tertiary levels, plunged during the post-adjustment period (Obasi, 1997). Nigeria stands out in terms of enrolling children in school under SAPs. However, the quality of education fell drastically. Extensive tests in Oyelere (2010) show that school quality may not a serious problem. More reassuring is the selection on observable test implemented in the previous section. Thus, we should have some measure of confidence in the validity of our instruments, which we suspected might be correlated with the state of the economy.

In spite of this, there is a still concern about instrument validity. Some have expressed the role oil wealth played in initiation and spread of UPE programme in Nigeria. The wealth from Nigeria's petroleum in the 1970s provided the country with solid monetary resources to carry out its UPE plan on a national scale (Bray, 1981). Given that finance was not of concern during the planning and implementation stages of UPE in 1976, the country pursued an educational agenda that promoted the importance of formal education over informal or vocational education for its people, in particular children (Bray, 1981). It saw formal education and children as vital parts in its objective to expand its education system. In addition, the planners of Nigeria's UPE program envisioned that primary education would not be an end in itself for the people. Rather, it would act as the initial medium through which people would be encouraged to move to post primary school education and beyond. The wealth from oil revenue in the 1970s played a significant role in Nigeria's decision to pursue formal education rather than an informal education agenda (Bray, 1981). There is another potential source of bias, one coming from selective migration of UPE beneficiaries. In the remainder of this section, we provide more evidence in favour of the validity of chosen instruments.

7.0.1 Bias from Selective Migration

Migration is just one important reason why doubt exists as to whether exclusion restriction could be fulfilled. If selective migration is prevalent, then it is likely that IV estimates will be biased. Yet, the manner in which our instruments are constructed is crucially dependent on the absence of mass migration. Mass migration may undo the affect of the UPE programme (Duflo, 2004). Worse still the 2008 NDHS data dose not have any information on respondent migration history, much less on specifics such location of birth and locations of schooling and works. Estimate of private or external returns to schooling investment will be incorrect when migration occurs (Schultz, 1988). When individuals educated in the rural area move to urban locations due to rural–urban wage differentials, wage returns are partly associated with education and migration, while returns to schooling in the rural sector is under–estimated. A study in Colombia, reported in Schultz (1988) on private returns to schooling discovers substantial difference in return to schooling between rural and sector are as when migration was accounted for. If individuals' current location is different from locations they attended schools, then our instrument might turn out to be weak, and invalid. The exclusion restriction

condition is violated because our instrument is highly correlated with unobserved migration variable in the error term.

In this study, we explore three approaches in accounting for the possibility that bias from selective migration is responsible for the results obtained here. One, evidence from the literature on internal migration in Nigeria has not revealed migration taking place on a massive scale yet (Osili and Long, 2008; Osili 2008; Oyelere, 2010). Across the state migration is limited. Analysis of the 2004 Nigeria Living standard survey (NLSS) as well as the 2005 Nigerian Labour survey data reveals that about 10 percent the people line outside their L G A of birth, and about 2 percent outside their state of birth. However, the labour survey data reveals that about 40% of Ogun State indigenes reside. Osili and Long report that more than 80% of Lagos state residents were born outside of Lagos.

The literature on migration does not give us much cause to worry, because long distant inter-state or inter-regional migration is barely significant. Migration at a fairly massive scale is essentially within states, or best within regions, with locations sharing similar characteristics. However, the NDHS data cover all states of the federation, including states like Lagos, and to a small extent the Federal Capital Territory (F.C.T), with significant urban representations. Thus, while the bulk of the data are collected from rural locations where most agricultural activities take place, certain states of the federation, particularly those of the South-West, are more urbanised than other states of the federation. Other states in South-western Nigeria have urban households outnumber those in rural households. Thus, modest migration might undo our estimates of schooling variable. To tackle this problem, we drop Lagos and Abuja samples, and re-run the regression. Our results are robust to the exclusion of this sample, though the estimates diminished slightly. Second, we dropped all states in the South-West, and implemented our econometric strategy. Our results are still robust to the exclusion of South-Western states. Similar results are obtained if Abuja and all South-Western states are dropped, and irrespective of which welfare indicators are selected. Panels A & B of table 6 summarize these results. One added boost to our results is that returns to schooling are statistically the same between migrants and non-migrants (Panel C of table 6).

Table 6 : Testing for sample selection in Migration in Regressions

Panel A.

	Dependent Variable is wealth index.			
	Whole Sample	Whole Sample minus Lagos & Abuja	Whole Sample minus southwest	Whole Sample minus southwest & Abuja
Schooling Years	***0.142447** (0.0060708)	***0.14187354** (0.00645928)	***0.13889921** (0.00581353)	***0.1246559** (0.00561228)
Schooling Years after controlling for distance from state capital.	***0.121237** (0.0060708)	***0.128539** (0.00645928)	***0.1146888** (0.00581353)	***0.1126784** (0.00561228)

PANEL C : DEPENDENT VARIABLE IS INCOME PER WEEK		
	Migrants **(IV Estimates)**	Non-Migrants **(IV Estimates)**
Schooling Years	***0.14433679** (0.0060728)	***0.1428564** (0.00645928)
PANEL D: DEPENDENT VARIABLE IS INCOME PER WEEK		
`	*Instruments constructed from LGA of birth*	*Instrument constructed from current LGA of residence.*
SCHOOLING YEARS	***0.1058908** (0.0088848)	***0.1057941** (0.0077773)
PANEL E		
	Dependent Variable: Log (Wage Migrant in)-Log (Wage Non-Migrant)	**Dependent Variable: Log (Wage Migrant out)-Log (Wage Non-Migrant)**
$UPE_{intensity}$	**0.00489 (0.09865)**	**0.00706 (0.03884)**
Number of Cells	**774**	**774**

Significant at 1 percent, ** significant at 5 percent and *** significant at 10 percent. Individual control variables include age, age-square, 3 ethnic dummies, sex dummy, sector dummy and marital status dummies. Cohort fixed effects use 10 dummies based on the age categories presented in table 1.0. State fixed effects include a set of 36 state dummies using the Federal Capital Territory (F.C.T) as the base dummy variable. The UPE-Enrol variable is constructed at the local government level. It is the LGA enrolment rates prior to the outset of the 1976 UPE programme.

One potential way out is to look for another instrument, whose construction is not affected by migration. If people segregate into locations by educational attainment (Cutler and Glaeser, 1997), they are also likely to do so by household. Thus, average educational attainment of household members who are 21 years and above could be a useful instrument. Estimates from the causal impact of schooling on economic well-being using this instrument could be compared to previous IV estimates. Better still, the availability of another instrument could permit the implementation of over-identification test. Thus, instrument exogeneity can at least be established. Table 5 reports estimates from using a suite of three extra variables; year at birth, year at six and year at twelve. Over-identification tests reinforce confidence in near exogeneity of chosen instruments.

Though, there is evidence that across the state migration is not of a considerable magnitude to affect our schooling estimates, there are still concerns that within state migration might confound our schooling estimates from IV regressions. Nigerian urbanization literature has reported a massive increase in the number of urban areas in the last three decades. If rural-urban migration within states is considerable schooling estimated will be exaggerated in the model that fails to account for migration.

For nearly all states, the capital is the best place for maximum gains to migration. It is probably the most developed part of the states for self-or paid-employment. Since we have no

information on individual migration history, we constructed a variable measuring the distance between individual's current location and the state. A prior, we expect the distance variable, constructed using the Great Circle Formula (GPS visualizes facility of the internet www. Gpsviualizer.com is utilized for this purpose), to be negatively related to our dependant variable, wealth index. Individuals further away from the state capital should have lesser economic status. If movement to the state capital is the upper limit of potential gains from migration, the introduction of the distance variable should absorb possible bias from the exclusion of migration variable. Thus, schooling variable should yield estimates closer to its true value. In panel c, we report the results from addition of distance variable to our model. While estimates (table 6A) from schooling may have been reduced they are nevertheless significant, even at 1 percent. There is some assurance that migration might not be biasing our schooling estimates.

7.02 Sample selection Bias

Duflo (2004) identified two sources of sample selection bias. One, there is the possibility of selective migration along unobserved characteristics. This occurs if low productivity old people moved towards the region of the program, or high productivity old people quit living in the program region. Two, because the program affected the segment of the sample population for whom specific outcomes are observed, the probability of selection in the sample is affected by the instruments. There is a similar possibility of bias of program beneficiaries quit to leave in better neighbourhoods. The bias is more serious when beneficiaries move farther afield, and the sending and receiving regions are fundamentally different from each other. This happens when the movement is massively from rural locations to thriving urban centres. According to **Schultz (1988)** and Duflo (2004), selective migration might bias coefficients of schooling attainment variable towards zero. Worse still, the 2008 NDHS data do not indicate individual's migrant status, and I do not have any income measure for individuals at all. We thus need other sources of information to shed light on this issue.

To be sure migration is not biasing our estimates, we looked for another data for added information. From the 2005 Labor Force Survey (LFS) we get information on respondent current LGA of residence and LGA of birth. First we can estimate separately for migrants and non-migrants. If any significance difference exists between the two estimates, then selective migration will be biasing our estimates. Panel D of table 6 shows no statistical difference between the two estimates, not even at 10 percent. In addition, we can assume that respondents had their primary education in the LGA of birth before moving to their current locations, we can estimate the impact of schooling attainment on weekly real income of workers for both locations of birth and residence. While exposure to UPE is assumed to be the same for similar cohorts across, UPE implementation differs from one LGA to the next. Thus, if migration is from high to low intensity LGA, and is on a massive scale, the returns to schooling should be significantly different between the two locations. The difference between the two IV estimates should reflect the extent to which migration might be significant. Beyond this, this approach mitigates potential bias from rural-rural migration which is said to be going on a modest pace in Nigeria (Mberu, 2005). The same set of instruments is used in this case, and the results are reported below in table 6. The two IV estimates are not significantly different from each other.

TABLE 6B: ADDITIONAL TESTS OF SAMPLE SELECTION BIAS

PANEL A/DEPENDENT VARIABLE: PROFIT	OLS	IV	MLE	HECKMAN2
Second Stage Results				
Schooling years	***0.0737(0.000407)**	***0.127121 (0.00976242)**	***0.127080 (0.0097444)**	***0.127006 (0.009711224)**
PANEL A/DEPENDENT VARIABLE: WEALTH INDEX	**OLS**	**IV**	**MLE**	**HECKMAN2**
Second Stage Results				
Schooling years	***0.0737(0.000407)**	***0.142447 (0.0060708)**	***0.1429988 (0.0060205)**	***0.14244524 (0.0060242)**

****Significant at 1 percent, ** significant at 5 percent and *** significant at 10 percent. Individual control variables include age, age-square, sex dummy, 3 ethnic dummies, English speaking dummy, sector dummy and marital status dummies. Cohort fixed effects use 10 dummies based on the age categories presented in table 1.0. State fixed effects include a set of 36 state dummies using the Federal Capital Territory (F.C.T) as the base dummy variable. The UPE-Enrol variable is constructed at the local government level. It is the LGA enrolment rates prior to the outset of the 1976 UPE programme.***

The approach suggested by Duflo (2004) is equally useful in this context. The 2005 LFS is used to measure whether productivity differences exist between migrants and non-migrants that are correlated with the UPE program. To implement, we estimated for each LGA the difference between the logarithm of the hourly wage of the migrants and that of the non-migrants for UPE non-beneficiaries older than 46. The estimated difference is regressed on the variable indicating the number of UPE schools built per square kilometre in each LGA. The coefficient on the number of schools is actually positive (but insignificant), which suggests that there is no downward sample selection bias. Alternatively, constructing the difference between the wage of those who migrated out of their region of birth and those who stayed, and regressing the resulting estimate on the number of schools per square kilometre produced similar results. This difference is not correlated to the level of the UPE program[12]. Panel **E of table 6B** shows the results of the Duflo approach.

The second type of bias could arise in many African countries where agriculture is labour-intensive. Nigeria is no exception. Given the high level of physical strength required in farm work and other farm related activities, it is expected that only physically strong people with minimal education will self-select into this kind of activity. To test the potential bias arising from this kind of self-selection, we adopt the Maximum Likelihood Estimation (MLE) and the Heckman 2-step approaches. The estimates obtained are compared with previous OLS & IV estimates. As shown in Panels A & B of table 6B, the estimates of schooling attainment do not differ significantly from the IV estimate. The mere fact that the estimates of the two techniques are closer to the IV estimate than it is to the OLS estimates could be because the IV technique, to a large extent, partially correct for self-selection.

7.0.3 Falsification Tests

[12] Results are available but not shown.

In our reduced form specification we find a strong positive relationship between the UPE instrument and welfare indicator. The first- and second-stage IV regression estimates reported in tables 4 are pointers to this fact. Our IV identification strategy is reliable to the extent that the schooling attainment is the only channel through UPE instrument affects welfare indicator. If this is true, then no positive relationship between false UPE instruments and our indicators of welfare: wealth index. If there is, then the validity of our true UPE instrument is in doubt. The luck of draw simply favoured our instrument as it is unlikely to be valid.

From the implementation of the Difference-in-Differences (DID) and IV strategies, we discover a strong and positive relationship between UPE instruments and schooling attainment. Not only is exposure to UPE programme significantly related to total years of schooling attainment, empirical evidence presented before now suggest that the impact of one UPE programme is stronger in LGAs where greater number of schools were built. The estimates from the first-stage IV and second-stage IV reported in… bears testimony to this. However, our IV strategy will only be accepted if the only channel through UPE instruments will affect economic status is through its impact on schooling attainment. If UPE instruments yield results presented in our IV regression, placebo instruments should not generate the same results. Otherwise, our instrument may be spurious, or might simply capture trends in enrolment rates, without affecting schooling attainment (Oyelere, 2010) Bertrand et al (2004) ran placebo regressions with false instruments. They found significant impact of placebo interventions. Previously, Bound, Jaeger and Baker (1995) have shown that generating instruments that are random, even they are not relevant, could produce results similar to those reported in Angrist and Krueger (1991) which used season of birth and compulsory laws as instruments for schooling attainment.

Table 7: Empirical Relationship between instruments Wealth index

Table 6 : Empirical Relationship between instruments & Wealth index						
Panel A	**Dependent variable**: **Wealth index**.					
	1966 – 1970 Period	**1971 – 1975 Period**	Ghana		Cameroun	Lesotho
Exposure to UPE (in Years)	**0.4302207** (0.0633461)	**0.0490207** (0.084137)	**0.0884286** (0.44013)		**0.0397784** (0.0400416)	**0.0455667** (0.8872112)
Log of 1 +plus no. of dams & sanitation projects*	**YES**	**YES**	**YES**		**YES**	**YES**
R- Squared	**0.3351**	**0.3426**		**0.2426**	**0.2829**	**0.2989**

Panel B	**Dependent Variable: Wealth index**				
	1966 – 1970 Period	**1971 – 1975 Period**	Ghana	Cameroun	Lesotho
UPE Exposure	***0.00466912*** (0.0039124)	**0.0004989** (0.000351)	**0.0050937** (0.015989)	**0.0060626** (0.1892758)	**0.00604904** (0.0189125)
Log of 1 +plus no. of dams & sanitation projects*	**YES**	**YES**	**YES**	**YES**	**YES**
R- Squared	**0.4122**	**0.4430**	**0.2924**	**0.3127**	**0.3124**

****Significant at 1 percent, ** significant at 5 percent and *** significant at 10 percent. Individual control variables include age, age-square, sex dummy, sector dummy and marital status dummies. Cohort fixed effects use 10 dummies based on the age categories presented in table 1.0. State fixed effects include a set of 36 state dummies using the Federal Capital Territory (F.C.T) as the base dummy variable. The UPE-Enrol variable is constructed at the local government level. It is the LGA enrolment rates prior to the outset of the 1976 UPE programme.***

To be sure our instruments are not spurious, we chose two separate periods in which there was no UPE programme and constructed two placebo instruments from these time periods. We constructed instruments based on UPE programs that were never inaugurated regionally or nationwide for three specific time periods: 1966-1970 and 1971-1975. A second set of placebo regressions were run. The variation in the implementation of UPE across Africa provides the appropriate platform to implement this test. A select number of African countries of with no history of UPE implementation were chosen for the implementation of placebo regressions with false instruments. The countries include Ghana, Cameroun and Lesotho. The false instruments turn out to be positively and insignificantly related to well-being indicator. Nigeria is probably one of few countries that introduced UPE programme in the mid-1970. Thus, if we have similar DHS data from other African countries and assumed falsely that they implemented UPE in 1976 and operated it nationwide until, the relationship between the false UPE instrument and wealth index should be zero and insignificantly different from zero. The instruments turn out to be insignificant.

7.0.4 Near Perfect Exogeneity Test

Although our falsification tests do reinforce evidence in favour of the validity of our instrument, we recognize that the requirement of perfect exogeneity is in reality unlikely to hold exactly. The question is what is the permissible limit of correlation between and instrument and unobserved factors in the error term which does not undermine confidence in the unbiasedness and consistent estimates of our key independent variable of interest? To determine the robustness of our IV estimates, we assume some correlation between our instruments and the error term so that we know the upper and lower limits of estimates. To implement this strategy, we follow the suggestion of Conley, Hansen and Possi (2008) that allows the instrument to be incorporated into the second-stage regression of IV regression model. Their strategy accepts plausible rather perfect exogeneity, and implements econometric strategy under assumption of less than perfect correlation between instrument

and the unobservables in the error term. Instrument of UPE exposure was incorporated into the second stage of the IV regression model, we can determine if the coefficient of schooling will include a value of zero for a specified confidence interval. In the specific case of this paper (results not shown), coefficient of instrument is negative and insignificantly different from zero. The interpretation is that is the bound on the strength of schooling coefficient is further away from zero compared to schooling coefficient originally obtained under the IV regression strategy. There is no upward bias in the original IV estimate of the causal impact of schooling on wellbeing. The mere fact that the coefficient is negative though insignificant indicates a downward bias relative to the original IV estimate but not serious enough to lose confidence in the reported IV estimate.

7.0.5 Over-identification Tests

With one instrument it is impossible to implement the exogeneity test. If more than one instrument could be found, then we can perform exogeneity test on our UPE exposure instrument. To do this, we added three more instruments to test the joint validity of the UPE instrument as well as other instruments. Year of birth, year at six and year at twelve are added to the first stage of the regression, and subsequently exogeneity tests are implemented on each instrument in turn. Table 4 shows results of our over-identification test. Because more than one instrument for used for our endogenous input index, it is also possible to execute over-identification for our instruments. Table 5 reports the results of over-identification test. This approach is useful because it is a direct test of exclusion restriction (Acemoglu, Johnson and Robinson, 2001). Results show that instruments for both female schooling attainment and input index are valid. In all specifications, our results fail to reject the exclusion restriction condition by a wide margin. Because the results from tables 4 and 5 show that $\rho \succ 0.05$ by a considerable margin, we do not reject the null hypothesis, and can in fact conclude that the overidentifying restriction is valid. While this test is not definitive on the question of validity, it is at least assuring when taken alongside the results of other tests.

8.0 Summary and Conclusion

In this study attempt to estimate the impact of schooling attainment on an important indicator of economic welfare: wealth index. OLS technique yielded an increase of 8 percent when economic welfare is defined in terms of wealth. OLS results are robust to test of omitted variables. IV technique that takes care of endogeneity and measurement error problem associated with schooling variable produced significantly higher estimates for both indicators of well-being. A year of schooling raises wealth by 15 percent. Econometric tests prove that instruments are valid. Additional falsification tests, plausibly exogenous test and over-identification test are proofs that of instrument are valid. A number of econometric strategies implemented indicate that selective migration is not biasing our results. In spite of the range of tests performed in this study, it has not taken general equilibrium effects into consideration nor is the political institutions that inhibit or accelerate the mobility of factors fully accounted for (Acemoglu, 2010). It is possible the range of estimates obtained may change substantially when other important factors, not necessarily restricted to the educational sector, are taken into consideration. The various feedback mechanisms, when they take their full turn, may significantly alter the results obtained in this study. Since this study is also an evaluation of the UPE program, the results may change considerably as the scale of the program expands. Thus, it is possible that the re-introduction of UPE as Universal Basic Education (UBE), which makes basic education free and compulsory for the first nine years and has been

implemented over a longer period may produce results radically different from what we have here.

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