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Schooling Capital and Economic Well-Being in Nigeria: Empirical Results from New Welfare Indicator?

Musiliu Adewole

For additional information please contact:

Name: Musiliu Adewole
Affiliation: Covenant University, Nigeria

Email Address: ade.adewole@covenantuniversity.edu.ng

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Schooling Capital and Economic Well-Being in Nigeria: Empirical Results from New Welfare Indicator?[1]

MUSILIU ADEOLU ADEWOLE*[2]

DEPARTMENT OF ECONOMICS

SCHOOL OF SOCIAL SCIENCES

COLLEGE OF DEVELOPMENT STUDIES

COVENANT UNIVERSITY.

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ABSTRACT

This study examines causal impact of education on economic status of individuals in the urban sector of Nigeria. This study exploits the quasi-natural experiment offered by the large-scale government investment in Universal Primary Education between 1976 and 1981 to confront the identification problems associated with this kind of study. Results from the Differences-in-Differences technique established that the UPE programme had positive impact on schooling attainment of beneficiary urban residents. 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 wellbeing

[1] **This is part of my PhD work.**

[2] gold_soad@yahoo.com or ade.adewole@covenantuniversity.edu.ng

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.[3] In the context of the Harrod-Domar growth model[4], 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[5], 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[6]. Education has also assisted in reducing poverty considerably in Nigeria (Okojie, 2002; Olaniyan, 2002; Oyelere, 2003; Anyanwu, 2005). 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 micro data to examine the causal impact of education in long term indicator of economic well-being: this indicator is computed from individual possession of durable

[3] Check Lewis (1954) for classic treatment of the role of physical capital in economic growth and development.
[4] Harrod (1939) and Domar (1946) made seminal contributions to this debate.
[5] 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.
[6] 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.

physical assets and the specific characteristics of housing conditions into a wealth index[7]. Principal component analysis is used to compute this wellbeing indicator. Higher values imply better economic well-being. The fourth wave of Demographic and Health Survey (NDHS) household data collected in 2008 in Nigeria is used for this analysis. Against the background of the 1976 UPE, this study turned out to be both an evaluation of the effect of the programme on wellbeing. The UPE programme presents an opportunity to overcome the identification problems associated with estimating the impact of schooling on individual economic situation. We depend on the exposure to the UPE programme, the regional variation in the intensity of programme and the interaction of both to resolve the identification problem. To avoid the confounding effects of previous UPE programmes on causal estimates of 1976 UPE programme, we limit our sample to those in 15-48 age brackets. Except for those residents in Lagos from birth or at early schooling age, those in 15=48 age brackets either benefitted from the 1976 UPE programme or did not benefit from any UPE programmes at all.

To construct our measure of UPE intensity, we at first relied on Duflo (2001) objective definition of programme intensity[8]. 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 of urban residents. 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 Ordinary Least Squares (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 wellbeing 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 indicator. Results reinforce our conviction that schooling impact positively and significantly on economic wellbeing. Thus, unobservable variables such as health characteristics of individuals, years of vocational training, migration, natural ability, religion, political attributes of LGAs and many more might not be driving our results.

[7] Within the DHS data, there is an alternative poverty ranking: richest, rich, middle-class, poor and very poor given the value of 1, 2, 3, 4 and 5. Using ordered probit and probit instrumental variables techniques yield strikingly similar results as OLS and IV methods. We stick to the latter two techniques because of easy of interpretation.

[8] 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.

However, OLS technique cannot fully take 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. 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. The IV estimate remains significant at 1 percent when a number of robustness tests were implemented, and IV estimate was never below 12.7 percent. The F-statistic from the first stage regression shows that our instrument set is very strong. This study also shows that selective migration does not constitute a major source of bias for OLS and IV estimates.

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 in Sub-Sahara Africa (SSA) 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 1981, 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. The paper also shows that the UPE programme did not have any negative impact on non-beneficiaries, eliminating concerns that general equilibrium effects of the programme might be substantially negative.

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 DID 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. In section eight, we test for whether general equilibrium effects might undo our estimates in addition to addressing concerns about the use of assets as indicator of labour market performance. The paper is summarized in section nine and conclusions drawn.

2.0 Background to the Study

2.0.1 Universal Primary Education (UPE) in Nigeria[9]

[9] This section has drawn extensively on the contributions of Fafunwa (1974) and Bray (1981) on the history of education 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. Lagos as the Federal Capital Territory outside the control of South-West started its UPE programme in January 1957. A month later, the Eastern region launched its own UPE programme.

The Northern section did introduce conditional UPE programme in 1958 given that it should implemented by localities when qualified teachers are available. Thus, 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 (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. Urban areas had most schools constructed at the initial stage of the programme (Ozigi and Ocho, 1981).

3.0 Descriptive Analysis

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

While the 2008 NDHS was collected from men aged 15-59, women within 15-49 cohorts, children, couples and households, 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 15-65 cohort, 66,712 in 20-65 cohorts 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[10]. This is line with evidence provided by Osili and Long (2008) study which used budgeted expenditure per capita at the state level for classifying states into high and low-intensity states.

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 age cohort that had the highest years of schooling also has the greatest wealth index. What cannot be inferred with 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 declined over time, though inter-regional inequality is still high.

To get a better idea of the impact of the 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 schooling attainment.

A similar picture emerges when we compare cohorts from treated and control groups across geopolitical zones, from previous implementation of regional UPE programmes in South-West and South-East Nigeria. For instance, those who range from 49 to 64 group in 2008 in both South-West and South-East Nigeria and benefited 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 cost 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 southern states.

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

[10] 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.

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.

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	1.95	4.59	2.77	4.95	2.65	4.04	2.71	2.92	2.40	0.978	1.79	1.052	2.01
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

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.

Panel B of table 1.0 helps 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 welfare. 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. For the purpose of econometric estimation, selected sample include only those in the 34-43 age brackets. As previously stated, this removes the confounding effects of previous UPE programmes

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 status is due to the implementation of one UPE programme.

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 established 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 defined intensity states as those in the Northern part of the country, Osili and Long definition accommodate 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, 2004). 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 Administration was overthrown in 1975. The administration that succeeded it was no more transparent and accountable. 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, thus they were allocated less schools.

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 1974 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 value 1 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 demographic structure 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. After 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 the UPE programme. Years of exposure declines as we move downwards towards 2 and move upward towards 11. Alternative econometric specifications also used dummy variable for exposure to the UPE programme, assigning 1 to those in the 2-11 age bracket as at 1976 and 0 to others . When the school entry age is raised to 7 years due to the significant number of over age pupils enrolled into the UPE programme, the same results are obtained.[11].The results are the same if we include those who are three years older than 11 years 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:

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

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.

[11] 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).

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.

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 in addition to estimating the causal impact of schooling on economic well being.

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

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

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

W_{ik} is the long term indicator economic wellbeing of individual **i** who is resident in LGA k. It is computed from her ownership of durable consumer goods.

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 for it to have 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[12]. The logarithm of these transformed observations is then 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 the results of 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 are shown in table 2. The baseline cohort is 15-48, assuming the working age starts at 15. For comparison we use 25–48 age cohorts, which are limited essentially to those who might completed schooling and started working. The results from using the two sets of cohorts are essentially the same. Thus, we stick to our original sample, 15-48 cohorts. As in Oyelere (2010), those who are in 2–11 age cohort as at 1976 benefited from the UPE programme. There beneficiaries were 34–43 year old when the fourth wave of NDHS was conducted 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[13]. 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 1 per 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 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

[12] 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.

[13] 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.

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 1 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 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 advantage 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.

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
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	25, 580	25, 476	25, 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.*

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 LGA 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 respondents in that LGA. The ratio of the two is our measure of school enrolment rate at the outset of the UPE programme[14]. 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 but not above 48years, the coefficients of UPE dummy are positive but insignificant.

In comparison with the treated cohort (34 -43), UPE had modest impact on cohorts aged 44-45, who are 12 years and above when the programme started in 1976 but nevertheless benefited from the programme. In the next section, OLS and IV regression results are presented and discussed.

5.0 Econometric Model and Empirical Results

5.01. OLS Estimates

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 3.0, female schooling attainment has strong and positive impact on economic wellbeing. On the average, an extra year of female education increases wealth status 8.53 by the same rate. 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.

[14] 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.

Table 3: OLS RESULTS (Dependent Variable: Wealth or Poverty Index)				
SAMPLE	All Sample	All 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	25,580	12932	17148	15762
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 urbanisation 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 8.35 percent but remained significant at 1 percent. If all locations in the Southwest are removed from the sample, an extra year of education raises wealth by 8.32 percent. When locations from southwest and Abuja are dropped from sample, the estimate is further reduced to 7.98 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 UPE programme augments individual schooling attainment, and schooling attainment in turn raises 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. 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 8.51 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 engage is suggested by Altonji, Elder and Taber (2005) which argues 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.

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 (age, age-squared, marital status, location, ethnic dummies) 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 6 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

6.0.1 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 Variable:**	**Wealth or Poverty Index**
Schooling years (one instrument)	*__0.1509171__ (0.00670192)
Schooling years (multiple instruments)	*__0.15295347__(0.0009321)
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 additional important issue is whether our instrument set is uncorrelated with factors, other than schooling attainment, that may affect welfare outcomes such 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.

In the second-stage regression, estimates show a positive and highly significant effect of the schooling attainment on wealth index. It can be seen that a year of schooling increases wealth by 15.1 percent. This is nearly twice as high as OLS estimates. 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).

In spite of this, there is a still concern about instrument validity. Some have expressed the role oil wealth played in the initiation and spread of the UPE programme in Nigeria. The wealth from Nigeria's petroleum in the 1970s provided the country with considerable 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.

Other potential sources of instrument validity exist if even oil wealth did not fuel the introduction and expansion of UPE programme. For instance, selective migration of UPE beneficiaries across LGAs might undo our estimates. It is also possible that any other kind of instruments, even if irrelevant, could have produced the same results presented in this study. Given that there are no instruments that are perfectly correlated with suspected endogenous variable, our instrument might not be exogenous enough to warrant placing too much confidence in our IV estimates. As you would soon see, a number of robustness checks to address these and similar concerns about our IV estimates did not undermined confidence in our IV estimates.

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 do not have any information on respondent migration history, much less on specifics such as locations of birth, schooling and work experience. Estimate of private or external returns to schooling investment will be incorrect when migration occurs (Schultz, 1988). According to Schultz (1988) and Duflo (2004), selective migration might bias coefficients of schooling attainment variable towards zero. 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 origin communities are under–estimated. A study in Colombia on private returns to schooling discovers substantial difference in return to schooling between rural and sector when migration was accounted for (Schultz, 1988). If individuals' current location is different from locations where 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 not 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 of the people live outside their LGA 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. Urban-to-urban migration is also prevalent (Mberu, 2005). However, the NDHS data cover all states of the federation, including states like Lagos, and to a small extent the Federal Capital Territory with greater level of urban development. The same argument applies to the South-West, which is the most urbanised region in Nigeria. 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 barely the same when estimated for all residents and for all residents minus migrants (Panel B 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 B : DEPENDENT VARIABLE IS INCOME PER WEEK				
	All Urban Residents **(IV Estimates)**		Non-Migrants **(IV Estimates)**	
Schooling Years	***0.14433679** (0.0060728)		***0.1428564** (0.00645928)	
PANEL C: DEPENDENT VARIABLE IS INCOME PER WEEK				
`	*Instruments constructed from LGA of birth*		*Instruments constructed from current LGA of residence.*	
SCHOOLING YEARS	***0.1058908** (0.0088848)		***0.1057941** (0.0077773)	
PANEL D				
	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)**	

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. Results from over-identification tests reinforce confidence in near exogeneity of chosen instruments. IV estimate from using one instrument is not significantly different from estimate obtained from employing multiple 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 urban-to-urban migration within states is considerable schooling estimates 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 capital. A priori, 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 poorer 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 A of table 6.0, we report the results from addition of distance variable to our model. While IV estimates 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.

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 all urban residents, and then for non-migrants. If any significance difference exists between the two estimates, then selective migration will be biasing our estimates. Panel B 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 across cohorts, UPE implementation intensity 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 reveals potential bias from urban-urban 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 6C. The two IV estimates are not significantly different from each other.

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 this strategy, 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 (Table 6.0D). 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[15].

The second type of bias could arise in situations where people with different educational endowment self-select into various types of activity. To test the potential bias arising from

[15] Results are available but not shown.

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 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.

TABLE 6B: ADDITIONAL TESTS OF SAMPLE SELECTION BIAS

Dependent Variable: Schooling years	**OLS**	**IV**	**MLE**	**HECKMAN2**
First Stage Results				
UPE Instruments	***0.0737(0.000407)**	***0.127121 (0.00976242)**	***0.127080 (0.0097444)**	***0.127006 (0.009711224)**
Dependent Variable: Wealth or Poverty 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.*

7.0.3 Falsification Tests

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 table 7.0 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) had shown that generating instruments that are random, even they are not relevant, could produce results

similar to those reported in Angrist and Krueger (1991) that 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 were 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 Additional Tests

8.0.1 Testing for General Equilibrium Effect

In spite of the range of tests performed in this study, it has not taken general equilibrium effects into consideration. 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 (Acemoglu, 2010). Since this study is also an evaluation of the UPE program, the results may change considerably as the scale of the program expands. In fact the 1976 UPE programme could have had significant and negative impact on non-beneficiaries by reducing their incomes or forcing them out of the formal sector. The magnitude of the loss may actually be more than the volume of gains made by UPE beneficiaries. 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.

To be sure general equilibrium effects are not of the magnitude that will offset the OLS and IV positive estimates obtained in this study, we regressed our indicator of individual economic wellbeing against the LGA years of schooling alongside all variables used in our previous regressions. LGA years of schooling is the same the average years of schooling of UPE beneficiaries in every LGA selected in 2008 Nigerian Demographic and Health Survey. To test the general equilibrium effects formally, we exclude cohorts who benefitted from the UPE programme from our sample. In the second set of regressions, we excluded cohorts who had not benefitted from the programme, leaving only the beneficiaries in our regression sample. OLS and IV regression results are reported for the two categories of individuals. While the estimates for the non-beneficiary sample are an order of magnitude less than the estimates for beneficiary sample, they are nevertheless significant at 5 percent. (Table 5.0b)

8.0 Testing for General Equilibrium Effect

Dependent Variable: Wealth Index	**OLS Estimates**	**IV Estimates**
LGA Average Years of Schooling Estimates (Beneficiaries)	***0.0974** (0.0034632)	***0.1205629** **(**0.005954872)
LGA Average Years of Schooling Estimates (Non-Beneficiaries)	***0.0674** (0.00395631)	***0.0805629** **(**0.00467699)

Values in parentheses are clustered robust standarderrors with an individual as a unit of observation.
*Significant at the 1 percent level
**Significant at the 5 percent level
***Significant at the 10 percent level

8.0.2 Wealth Index as Dependent Variable

The use of 2008 wealth index as dependent comes with a number of concerns. Household assets assigned to all individuals within the household may be owned exclusively by the head of the household, and in some cases jointly owned with the spouse. It is also possible that household assets could have been received as gifts, and thus unrelated to the actual economic status of individuals. To deal with these concerns, a number of alternative specifications of our econometric models were estimated with OLS and IV identification strategies. First, we run regressions exclusively for household heads. A full set of explanatory variables are included in all regressions. Second, we run regressions for the household heads and spouse. Schooling coefficients are highly significant in OLS and IV regressions. To be sure that wealth index is a good proxy for wellbeing indicator, we estimated the LGA average income using the 2008 General Household Survey (GHS) and LGA average wealth from the 2008 NDHS. If the two indicators of wellbeing are highly and positively correlated, then gifts might not be a significant proportion of total wealth assets. Over time, individuals might be translating earned income to assets. Since the lower age limit for our estimated sample is 15 years and the upper age limit is 48 years, it is most unlikely that persons in the age brackets will be receiving substantial parts of their assets as gifts. Correlation between the two variables is positive and significant, exceeding 0.8. If gifts are likely to come from long

distance migrants, which are resident outside the LGA, the result of correlation analysis attest to the fact gifts are not likely to be a significant source of assets. Able 9.0 Panel A reports the results of the first three sets of regressions. Finally, we run OLS regressions with 2008 GHS data, using real income per hour as dependent variable. Nearly all explanatory variables used in previous regressions are used in OLS and IV regressions. Results are qualitatively similar to those reported previously. Panel B of table 9.0 reports that schooling attainment effect on income per capita is significant and positive.

Table 9.0 Testing Appropriateness of Wealth Index as an Dependent Variable

PANEL A					
Key Independent Variable	**Dependent Variables: Wealth Index**				
	All Sample	**Household head**	**Spouse**	**Household Head & Spouse**	**Correlation between Wealth Index & Income**
Schooling Attainment (OLS)	***0.0853 (0.0003207)**	***0.1153 (0.0059347)**	***0.0685 (0.0019321)**	***0.0853 (0.0003207)**	***0.85634**
Schooling Attainment (IV)	***0.15099171 (0.00670192)**	***0.15099171 (0.00670192)**	***0.15099171 (0.00670192)**	***0.15099171 (0.00670192)**	
PANEL B					
Key Independent Variable	**Dependent Variables: Individual Real Income Per hour**				
Schooling Attainment (OLS)	***0.123754 (0.00974526)**				
Schooling Attainment (IV)	***0.186745 (0.01657822)**				

9.0 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 of instrument validity. A number of econometric strategies implemented indicate that selective migration is not biasing our

results. While wealth index as a proxy for individual wellbeing comes with its own limitations, a number of checks on this proxy justify its appropriateness in this context.

General equilibrium effects, not often considered in studies of this kind, but nevertheless important for projects of this magnitude were estimated. This is important because the re-introduction of the programme as Universal Basic Education (UBE) in 1999 have further extended from years of tuition-free education from six to nine years in addition to making the programme compulsory. 1976 UPE impact on non-beneficiaries are positive and significant.

Fears that the overall social effects are negative and substantial are not borne out by the empirical results of this study. While this not an exhaustive examination of a general equilibrium effects, it is at least reassuring. Though we have partially accounted for general equilibrium effects in our regressions, we have not investigated what the likely impact of economic and political institutions on outcome will be. In agriculture, fallowing for sufficient period of time allows agricultural lands to recover lost nutrients and support crop growth. However, Goldstein and Udry (2008) found that fallowing in Ghana was grossly insufficient because those without local political power could lose their lands during fallowing period. Another study by Acemoglu, Johnson, Robinson and Querubín (2008) shows the limitations of the attempt to control inflation by promoting the independence of the Central Banks in societies. Policymakers in institutionally weak societies constrained by the monetary policies of an independent Central Bank might resort to running large fiscal deficits to undo the favourable effects of monetary policy.

Generally, the central contribution of this paper is to add to the ongoing effort at evaluating the 1976 UPE programme. Before now, rigorous econometric evaluation of the programme has not been implemented. The initial attempt at evaluating this programme effect on female schooling, child mortality and fertility has produced controversial results (Osili and Long, 2008: Osili, 2008: Palmer-Jones, 2008). Oyelere (2010) did not specifically evaluate the 1976 UPE programme, but basically used all UPE programmes implemented before political independence of Nigeria and 1976 UPE programme as IV identification strategy. While the collective impact of the programme is substantial in terms of raising schooling attainment, it is small in terms of overall effects on individual income. From Oyelere study, we cannot determine the individual effects of all UPE programmes on schooling attainment and income. Thus, her study was in no way an evaluation of the UPE programme, but rather an attempt to use all UPE programmes to construct a valid instrument useful in resolving the identification problems associated with studies trying to derive causal estimates. Therefore, her study cannot be considered as evaluation of the 1976 UPE programme. It is at least fair to conclude that all things being equal, large-scale public sector investment in schooling is capable of increasing economic wellbeing of Nigerian residents considerably.

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