

Education and Development: a journey through time and space

Angela W. Little

Lecture delivered to the BAICE student conference, held at the University of Sussex, June, 2008

Thank-you for the invitation to address this BAICE student conference 'Adventures in Methodology, ethics and fieldwork: the research student experience and beyond'. You have kindly given me the opportunity to reflect on my research career in Education and Development, an indulgence I have not been offered before. I thank you for that.

My talk is titled *Education and Development: a journey through time and space*. Like many of you my career has been punctuated by a series of professional identities developed in multiple contexts. From a primary and secondary education on a small independent island in the middle of the Irish sea, the Isle of Man, I took a major step (for me) to become a university student in England where I studied social sciences, majoring in sociology and psychology. From university student in England I journeyed to Nigeria

as a volunteer teacher of mathematics. From teacher in Nigeria I journeyed back to my island via North Africa and Europe and wondered where my next journey would take me. I landed somewhat serendipitously across the road at the Institute of Development Studies with a one month contract as a research assistant working on the analysis of agricultural productivity data from the state of Bihar in India. One month was extended to four months as I worked with Ron Dore on his book *The Diploma Disease* and then to nine as I worked alongside Ron and others at IDS to support the establishment of the first M.Phil course in Development Studies launched in September 1973. I thought of applying to the course but moved instead to London to train as a primary school teacher. While training as a teacher the IDS launched a long term research programme on ‘Labour Market Backwash and the quality of education in developing countries’. I returned to IDS as a research officer and stayed another 13 years before moving on to the Chair of Education in Developing Countries at Institute of Education at the University of London where I have been based for 21 years. During my time at IDS I worked on a number of research projects all which in one way or another were designed to explore one or more of the many, some rather controversial, propositions embedded within the complex *Diploma Disease* thesis. Many of you will be

familiar with that thesis and related research and I don't propose to explore it in any detail today.

However my time at IDS also generated the inspiration and opportunity for the two pieces of research which I do wish to share with you today.

The Development of the Child's Understanding of the Causes of Academic Success and Failure

Since most of you are research students the first piece I have chosen is my DPhil thesis from the University of Sussex where I was enrolled in Social Psychology. My thesis title was the Development of the Child's Understanding of the Causes of Academic Success and Failure. It carried a long subtitle 'Studies of English and Sri Lankan schoolchildren'. It is always difficult to pinpoint where a journey starts and ends but I think there were three sets of ideas and experiences that started me on this journey.

The first was my earlier experience as a teacher in a Muslim girls high school in Nigeria, a former British colony. At first, living and working in Nigeria felt a little strange. I remember thinking how my first week there felt

like sitting in a cinema watching a film. Soon the strange began to feel a little more familiar. On the surface there were many similarities between the Nigerian system of education and that found in much of Britain. The system was structured in a similar way. The rhythm of the school day was similar. The textbooks were similar. But there were also many differences. The secondary classes were mixed ability, they were not streamed. Though English was the medium of instruction in secondary it was the mother tongue of no girl. Gradually I came to learn more about the system I was working in – and the system with which I was most familiar – that of the Isle of Man - became gradually more strange. I learned much about Nigerian girls' aspirations and expectations for the future. Compared with the students I had known when I was at school, these students had uniformly high aspirations for their futures. They wanted to be doctors, teachers, lawyers. In terms of the pecking order of examination success of the schools in the Nigerian town where I worked, my school was at the bottom. It did not matter. These girls were attending secondary school as a way of reaching the top. Their aspirations were high, their motivation strong and they and their parents believed that if they studied and tried hard enough they could make it.

Teaching in two primary schools in England provided the second spring of inspiration. In the first I introduced a game for seven years olds to assist in the development of concept formation. Children play in groups of four. I start off the game to give them the general idea and ask John to select from a large box of objects all those which were smooth. He looked at and felt many of the objects and worked quickly and well. I then asked Martin to select all the triangles. Martin started well but after some time John started to try to help him. Martin became frustrated and insisted that it was 'his turn'. Gradually the others in the group begin to chant in unison 'we can see another one'. Martin cannot 'see another one' and his frustration grows. Finally he spots the last triangle in the box. Extremely relieved he exclaims 'see, I am clever after all'. That sequence raised a number of questions in my mind

From where and from whom did Martin develop the idea that he is clever?

What does seven year old Martin understand by the term clever?

What are the implications of Martin's understanding for his future motivation to learn?

Martin was from a middle class family as were most of the children in this particular school. In the second school I meet Mark, a poorly dressed, eight year old, sniffly and without a handkerchief and a tie knotted some way down his chest. His parents had separated and he and his mother live with her parents, his grandparents. He is a low achiever and a loner. During the lunchbreak he prefers to be with me in the classroom rather than running around in the playground outside with friends. One day as he finishes off some of his morning work he says 'you know, I'm always last'. I say nothing. Cheering up a little he remarks 'still, I quite like being last 'cos it means I have to catch the others up'. That raised another set of questions in my mind.

How did Mark understand and explain his apparent 'failure' to himself?

Did other children in his class perceive him to be a failure? If so, how did they describe and explain his failure?

When did he become aware that he was ‘always last?’ Would he always be last? Would his understanding of why he was last endure or change over time?

The third spring of inspiration for my D.Phil research was a series of ideas embedded in Dore’s original formulation of the Diploma Disease. You may remember that in Chapter 13 of his book titled ‘some modest proposals’ Dore sets out a series of suggestions of how to mitigate the worst effects of the diploma disease. Among these are the use of lotteries and of achievement tests which ‘cannot be, or cannot much be, specifically coached or crammed’. He was thinking here of so-called aptitude tests and intelligence tests which are less susceptible to the effects of practice and effort than conventional achievement tests. The point here is not to debate those proposals but to see what ideas they stimulated in me. How would learners respond to selection systems based on criteria beyond their control, I wondered? What impact would a system based on random luck or on inherited personal characteristics have on the motivation to learn, to try hard in school and to persevere? How would my Nigerian secondary school students have interpreted and reacted to such a system? How would my English primary students have interpreted such a system? This led me to ask

the larger research question which began to form in my mind: How do young people attribute the causes of academic success and failure in school in different cultures and do these understandings change over time? (note that this is quite a crisp way of expressing what came to be the overarching theme of my research. I doubt it was so crisply expressed at this very early stage of the research)

How did my research idea link with existing theory and empirical research and how did theory help me to frame the research better? The ‘causes’ of academic success and failure had long been a subject of interest for sociologists and psychologists. Douglas (1964), Coleman (1966) and Husen (1967) had underlined the importance of the home background. Burt (1975) and Jensen (1969) had underlined the role of native intelligence. Rosenthal and Jacobsen (1968) and Pidgeon (1970) had explored the role of teacher expectations. McClelland (1953 – see how very dated some of my reference sound now!) had written of the ‘need for achievement’ while Bowles and Gintis (1976) had attributed success and failure to systemic characteristics of the school system. Yet almost all these explanations had one thing in common. They failed to include in their explanations the explanations held by the subjects they studied. This I felt, a priori, was an important omission.

Around this time I chanced upon a new book written by Bernard Weiner and colleagues in California titled *Perceiving the Causes of Success and Failure* which introduced the Theory of Attribution. Attribution theory seeks to provide a meta knowledge of lay people's understanding of the causes of success and failure. If you like, attribution is a theory about lay people's theories. Would this theory be useful in understanding children's understandings I wondered. I set about reading the literature on attribution. It was a demanding literature. I warmed to it as I had warmed a couple of years earlier as an undergraduate to the writings of George Kelley and others on Personal Construct theory. It took seriously the idea that people's own constructions of the social world were important determinants of their subsequent behaviours. I noted down theoretical propositions, I described experiments and methods, I created lists of antecedents and consequences of attributions of different types. After a year or two some articles began to appear on developmental changes in attributions. This literature drew heavily on Piaget's developmental psychology, specifically on operational thought and on causal schemes for understanding the physical world. I explored this literature avidly and identified one causal scheme that had not been investigated to date (or if it had been the work had not been published). This was the scheme of compensatory causality – the tool of operational

thought that enables one to compensate the relative presence of one cause for the relative absence of another. So for example if one is asked about two children who achieve the same outcomes on the same task and one child is trying harder does one compensate for this greater effort of one by reasoning that the other probably has greater ability. I set about developing hypotheses and an empirical procedure – sets of stories - with children of different ages. So far so good. My research would begin to explore this unresearched scheme of compensatory causality. I had set out my initial problem/issue. I had reviewed the empirical literature and identified gaps. I had combined key concepts from Attribution theory and Piagetian theory. I developed hypotheses and I designed a series of closed-ended stories to enable me to detect the extent to which the scheme of compensatory causality was used by children of different ages. If I had stopped there my lot as a D Phil student life would have been relatively easy.

But I did not stop. Some aspects of the attribution research were intriguing and troubling me. Up to the mid 1970s much of the empirical work on attribution had been framed by four main attributions - ability, effort, task difficulty and luck - and two organizing dimensions – locus (internal vs external) and stability (stable vs unstable).

Figure 1

	Internal	External
Stable	Ability	Task Difficulty
Unstable	Effort	Luck

Evidence suggested that success tends to be attributed to stable attributions like ability or task ease, while failure tended to be attributed to unstable attributions like effort and luck. Behaviour inconsistent with past performance tends to be attributed to luck and effort. And the greater degree of prior success or failure, the greater the likelihood that repeated success and failure will be attributed to high or low ability.

The reported research seemed to assume that

- (i) young children's attributions were the same as those of adults
- (ii) young children's concepts of social causality were the same as those of adults

- (iii) the types and range of attributions made by children were the same as those made by adults.

- (iv) attributions made by children and adults in different cultures were the same

By this stage I had been working in Sri Lanka for some time in connection with the IDS research programme. I had become aware of the intense pressure on Sri Lankan children to succeed in education and of their participation in two parallel systems of education – the formal public system by day and the private tuition system by evening and night. Children and their parents invested huge amounts of effort and cash in academic success, much greater than in England. This observation, combined with a growing unease about some of the implicit assumptions of the empirical work running alongside the development of attribution theory, gave rise to a very different strand of my D.Phil research. This second strand was much more open ended and was guided by research questions in which the answers were not predicted in advance. It contrasted with the first strand in which hypotheses were derived from theory, findings predicted in advance,

evidence collected and hypotheses disconfirmed/confirmed. Three questions formed this second strand of the research

What are the types of explanation used by children to explain academic success and failure?

Do children's attribution change over time?

How do English school children's attributions compare with those of school children in Sri Lanka where the school is wedded to a non-industrialised economic and social system?

To address these research questions my main field work consisted of tape-recorded interviews with 149 children in the five to fourteen age range from two schools in Newhaven, England and 180 children from two schools in Sri Lanka (urban Colombo and rural Pilagoda). In England I personally interviewed all the children and transcribed every interview from tape – a painstaking task. In Sri Lanka I worked through two assistants who interviewed children in Sinhala. Before the interviews could start much preliminary work was undertaken. In Sri Lanka pilot questions were back-

translated from English to Sinhala by an independent translator and back again from the translated Sinhala to English by a second independent translator. The research assistants transcribed the interviews into a Romanised form of Sinhala which we developed together (Sinhala has its own script) and I was able to work on this Romanised script with them. From the transcribed English and Sinhalese interviews in both contexts I identified content and themes and developed a code book in which all possible answers to all questions could be recorded. This phase of work was qualitative. I worked with text. I sought meanings lying behind words. I sought to classify text into attributional categories. Because the data were qualitative and because I was the person developing the codes I used a panel of independent judges to assist me in moving from the text to the coded data – this helped me to establish the inter-judge reliability of the codes assigned. In the 1970s this procedure was considered to be essential in order to avoid the inevitable biases of researcher subjectivity.

Once the data were coded into exclusive categories I was able to move on to analyse them by type and by age and by cultural context. At this point the research became more quantitative. I don't have the time to share with you the detailed findings of this research – if you are interested the thesis is

sitting on a shelf across the road in the library – but I do want to share with you a major obstacle that I faced in the quantitative analysis of some of this qualitative data and an obstacle that became so oppressive that I almost gave up in despair – several times.

The problem was this. You will recall that I was interested in the types of attributions children of different ages used to explain instances of success and failure. Through a series of questions children were asked to think about children they knew in their class who were working well/not so well, working better/worse than others, or achieving more in reading, maths and art than others, and to suggest reasons for why this might be so. This generated an enormous volume of qualitative data. After extensive analysis I classified the attributions into seventeen types – starting with three types of ability statement. (The first was what I called performance ability. This type of ‘explanation’ was less of an explanation and more of a description of the thing to be explained. This was a very common response among the youngest children. So, for example, in response to the question ‘what is the reason why she is working well?’ the child responds ‘because he does nice writing’. Or ‘what is the reason why Darren is working better than Trevor’ the child responds ‘it’s because he never gets his sums wrong’. Then there

were what I called specific competence ability attributions. In these, children combine a ‘dispositional’ term like ‘know’ or ‘can’ with the achievement outcome to be explained. So, for example, in response to the question ‘why is he on a higher maths book?’ the child responds ‘ he knows how to do sums’. This is to be distinguished further from what I termed general competence ability attributions. If specific competence links a person’s characteristics with his or her achievement outcome then general competence delinks the person from those specific outcomes. Achievement outcomes are explained neither by a redescription of the outcome nor by a combination of predisposition and outcome but solely by a personal disposition e.g. he is brainy, she is clever, she is intelligent, he is thick.

Alongside these different types of ability attribution were a preponderance of effort attributions – he tries harder, she gets on with it etc. In all I identified 17 types of attributions used freely by children.

The statistical obstacles I faced in the quantitative phase arose from the nature of the open-ended free response interview style. My questions were designed to elicit from children of different ages as many attributions as possible and I did this through at least seven questions, worded slightly

differently. While it is pretty straightforward to explore which attributions are used most frequently and to see whether specific questions elicit different types of attribution it is much more difficult to explore whether age makes a difference to the types of attributions offered by children. At one level one can simply use regressions of age against use or non-use of the attribution. Linear and quadratic trends (in which the middle age groups use an attribution more or less than both the older and younger groups) yielded a number of clear trends. In the English data for example there was a strong negative age effect for the use of performance ability attributions and a strong positive effect for specific and general competence, effort and the influence of other persons. Some attributions showed curvilinear or quadratic trends (e.g. bad behaviour, time spent on the task and speed).

But one must be careful in reading too much into these trends, especially the linear positive trends. This is because this was a free response exercise and if you think for just a moment, it is fairly obvious, that older children are likely to produce more responses – to be ‘more verbal’ – than younger children. The fact that there is a strong positive age trend for effort may simply be due to the fact that older children produce more of most types of response. The trend may not necessarily show an age preference for a particular type of

response. An analysis of the number of responses produced per age group and per question did indeed showed in general an increase in the number of responses produced per question by age.

There were several ways in which I explored this further. The one which gave me the greatest headache was the attempt to compare the profiles of responses produced by children of different age groups. If older children are producing more responses in general then the shape of the profile of use across the age groups would be similar. The profile of the older children would simply sit higher up the y-axis. (Use PPT slide here).

On the other hand if older children are producing more responses – but of qualitatively different types – then the shapes of the profiles would be different. The shapes would be independent of each other, disconfirming the hypothesis that age effects arise from the fact that older children are more verbal. (Use PPT slide here)

Graph profiles can be tested for their independence from each other through a multi-variate analysis of variance (Morrison, 1976). The test for independence involves a comparison of a matrix of between-age variances

with a matrix of within-age variances. But in order to compare the matrices one needs to know the ‘determinant’ of the matrix. The problem arises when the determinant of the within –age matrix is zero. In several cases – especially in the least used attributions - there were empty cells for some age groups. The determinant could not be computed and the test kept failing. An additional constraint was imposed by the computer infrastructure we were using at that time in Sussex. This was the late 1970s before the days of PCs and we worked on main frames, usually with the help of a computer operator or statistician. Because the Sussex capacity was relatively small, many of our jobs were sent ‘up the line’ (I presume a telephone line but I am not sure) to Manchester University, processed and sent back. For reasons I forget the probability of sending a job up the line and receiving a completed job back was only one in three. So a job sometimes had to be sent three times in the hope of getting a procedure completed. For a time we did not know whether the tests were crashing because of the matrix constraints or the hardware. Eventually I decided to restrict the analysis to most frequently produced attributions. I reduced the data in the profile test from 17 to 9 – and hey presto – my tests of profile independence could now be computed. My tests showed that the profiles were indeed independent. But my goodness was it a frustrating process. How many times did I consider

collecting up the mounds of paper on my kitchen floor and putting them in the dustbin?

I have dwelt on this aspect of the research at some length because I wanted to demonstrate three things. The type of research question you pose guides you to your methods. Questions about types of attributions used by children of different ages in grounded situations require an open-ended qualitative interview approach. Questions about the frequency with which attributions are used by older and younger children require a quantitative approach. Both methodological approaches can be employed within the same piece of research - but there are optimal and suboptimal ways of doing this. I am a firm supporter of mixed methods approaches to educational research. I believe that the choice of methods derives from one's research question - and that research questions in turn derive variously from theoretical, policy and practice concerns. I have shared with you some of my frustrations because I do not think I combined my qualitative and quantitative methods optimally. I would have done better to have separated the research more clearly into phases or stages. After the initial qualitative stage I should have confined the quantitative analysis of the qualitative response data to an analysis of frequency and dominance. Once I had ascertained the most

frequently used attributions for different age groups I should probably have moved to a series of stories or questions that required more closed-ended response – asking children to choose the most probable explanation from a specified list. This would have had the advantage of focusing on the mostly salient attributions while generating the possibility of sophisticated and sensitive analysis of age trends. One lives and learns.

Labouring to Learn: towards a political economy of plantations, people and education in Sri Lanka

The second piece of research I wish to share was also inspired in part by my work at IDS, but in a very different way. Early in the 1980s I undertook an evaluation of an education development proposal of an integrated rural development programme aimed at poverty reduction in a rural district in Sri Lanka. The district was the Badulla district which, in the early 1980s, had a very diverse population of whom 25% were Indian Tamils living inside tea plantations. A community marginalized from the mainstream, their schools had recently been taken over by the state from private ownership. Their pupil:teacher ratios were the highest in the district and their achievements the lowest. The proposal I was asked to evaluate ignored this educationally

disadvantaged community almost completely. I faced a dilemma. Do I ignore or highlight what seemed to me to be a striking omission in the choice of target groups? I chose the latter course. I was young, I was curious, I was becoming gradually more aware of the politics of ethnic relations in Sri Lanka, and, in retrospect it seems, I was not afraid to speak my mind. After all this was a country which at the time – the early 1980s - was being hailed by the international community for enormous achievements in educational provision for all. My report, written I admit in a rather strident style, suggested that the target groups for the proposal be revised to include the plantation Tamil community. This was met with a very negative reaction from the Ministry. I was not invited to be present to explain my case and were it not for a personal connection between the then Head of the Ministry and IDS fellows on the one hand and the fact that the Deputy Minister of Education's electorate stood to gain from a revised definition of target groups and schools for development, that would have been that, my report would probably have been placed in a drawer and forgotten, another consultant appointed or the potential programme axed.

To cut a long story short, the target communities – the 'beneficiaries' as we termed them then - were redefined, the programme went ahead and began to

generate results. Over time what had started as a sub-district programme run from a regional office became a national programme extending to six districts and run from the national ministry with support from the Ministry of Education and the Swedish International Development Authority. I continued to be associated with these programmes over a very long period of time – from 1982 to about 1994 – managing much of the project planning, monitoring and internal evaluation work. My role was that of a development project worker. And, looking back on the short period in which my initial report was rejected and resisted, I suppose my role had been that of a development activist, contesting Ministry definitions of who the most needy beneficiaries of a project might be. For a period of about 12 years I went on to work inside development projects in Sri Lanka – not continuously since I had a full time job at IDS and subsequently in London – but a long term commitment to a team of Sri Lankan colleagues and a programme of work in which I was both actor in as well as a privileged participant observer of how schools improved, of how schools related to educational administration, of how a Ministry worked and of how a Ministry interacted with an aid agency. I learned a great deal about the politics of ethnic relations and about how these were played out in the capital as well as in far away districts. Over

time my role as development worker began to diminish and my direct involvement in the projects came to an end.

But this end of my role as development worker was also the start of my systematic research on education in the plantations. It was clear that the education programmes in the plantations had had some impact on educational access and progression among the estate community in most of the areas where we worked. As I retreated from the day to day action, I began to see what we had been doing in a much broader context. I began to feel that the apparent ‘success’ the projects had much deeper explanations that reached far beyond the project itself. Growth in enrolment and enrolment ratios was not confined to the so-called project schools alone. They were occurring elsewhere. It was also clear that the Ministry’s stance on education in the plantations had changed quite significantly – from bureaucratic resistance to bureaucratic tolerance. Why and how was this occurring? What were the broader political, economic and social forces at work?

As I spoke about these things with a colleague – a political scientist - he casually said why not write a book about it. Suddenly I had one of those

gestalt moments. And so began a programme of personal research into the bigger picture of educational change in the plantations. This was not a story about the projects with which I had been involved. The ethics of that would have been rather complex. My underlying motivation was rather different. I had learned an enormous amount from my Sri Lankan colleagues working inside these projects. The literature on the contemporary story of education in the plantations of Sri Lanka was sparse. I held a privileged position at a privileged and well known university. Perhaps I could use this other identity to give something back to the community from which I had learned so much. The book which emerged some five years later – *Labouring to Learn: towards a political economy of plantations, people and education in Sri Lanka* – addresses a question of educational access: why and how has educational participation among the plantation community increased? The book starts with a day in the life of Vickneswari, a young girl attending a school inside a tea plantation. It works outwards to her household and school to schools in her district, province and in the country more generally. The book charts change and tries to explain it.

This research journey took me back many years – to 1832 and the establishment of plantations in Sri Lanka. It took me to many parts of the

world – not personally, but in terms of the geographical scope of explanation. Archival material transported me to the Caribbean and the abolition of slavery, to China and the possible importation of Chinese labour to work on the plantations, to the Baptist and Anglican missionaries in London and Madras, and to the Houses of Parliament in London and the debate about the education of children in plantations in the early 1900s. It was the first piece of history I had ever tried to write - and I was loving it. It was not funded by an external funding agency and hence was not time-limited. It was informed by theoretical work – by Marx on class and social reproduction, by Myron Weiner’s work on the political economy of mass education in India and by some of Margaret Archer’s work on the politics of educational change. But I did not tie myself too tightly to any particular theory of social change. I wanted to hold open the possibility that the relative importance of the economic, the political, the social and the cultural in the explanation of educational change – both progress and decline - might change over time. During my journey I sought to make connections between what we now refer to, sometimes glibly, as the ‘local’, the ‘national’ and the ‘global’. At the local level I was interested in parent attitudes, teacher backgrounds and identities and the attitudes of estate superintendents to labour and the education of labourers’ children. Beyond the local I needed to

learn about developments within the colonial state, and, after independence in 1948, the national state. I needed to find out about educational legislation, about legislation on citizenship, about the electoral process, political enfranchisement and disenfranchisement, about trade union pressure on employment law. Internationally/globally I learned about global markets for coffee, tea and rubber, the global economic depression in the 1930s, the slump in the world demand for rubber, the retrenchment of labour and the return of many families to India, the decline in enrolments in plantation schools.

As well as analyzing archive texts I interviewed students, teachers, parents, estate superintendents, education officials, Ministers, trade union leaders, politicians and Presidents. I faced three methodological challenges – language, country-wide evidence on literacy and participation rates and time. My inability to speak Tamil constrained some of the primary evidence I was able to glean. But I worked very closely with Tamil speaking research assistants and much of the work could be undertaken personally in English. The second challenge was to substantiate my hunch that educational participation rates among all plantation communities country-wide had increased markedly and that there was something tangible, real and objective

that was worth explaining. I am not a social scientist who believes that reality resides solely in social construction. Just because people tell you that there has been positive change or decline or that ‘everyone believes there has been change’ does not mean that either is true. I always wish to find further substantiation and evidence. At the same time I am a social scientist who is very aware of the distortions of reality that official statistics can create. I already knew from the evidence of large data from the projects with which I was involved that enrolments had been increasing quite dramatically over time. I knew from Ministry statistics that there had also been increases (not quite so great) in schools in non project areas. However, I did not know and nor did the Ministry know how these figures compared with natural increases or decreases in population, nor did we know how many children from the estate community might be attending schools beyond the estates. To answer these questions we needed the latest population census. The problem was that a civil war had been and still is raging in various parts of the country. The last full population census was conducted in 1981. I was assured year after year in the mid 1990s by the Census Department that a new census was about to be launched, but time and again a full census was prevented by the war. In the book I pieced together evidence on literacy rates as best I could from various sources. I have still not been able to

construct a sound time trend in educational participation rates and have only quite recently (after the publication of the book) have been able to locate evidence on trends in literacy rates between 1986 and 2003. Fortunately these confirm my assertions. In the seventeen years between 1986 and 2003 literacy rates in the country as a whole grew from a high of 88.6% to 92.5%. In the estate sector the growth was from 68.5% to 81.3% with a stronger growth among females, from a low of 58.1% to 74.7%.

Table 1 Literacy rates by sector and by male/female

	Rural	Estate	All sectors
1986-7	89.5	68.5	88.6
Male	92.8	80.0	92.2
Female	86.5	58.1	85.2
1996-7	92.3	76.9	91.8
Male	94.4	87.2	94.3
Female	90.4	67.3	89.4
2003-4	92.8	81.3	92.5
Male	94.7	88.3	94.5
Female	91.1	74.7	90.6

Source: CFSES report, 2003-4, cited in World Bank, Sri Lanka Poverty Assessment, 2007

My third challenge was time – or a surfeit of it. Because this was personal research unfunded by an external agency I was not bound by quarterly and annual reports, deadlines and grant termination dates. This was liberating – but also constraining in a perverse way. I began to dawdle along the routes of this journey. I did not stick to the main roads or even the B roads – but I allowed myself to be distracted down the alley ways, the lanes or, as we would say here in East Sussex, the twittens. I was enjoying this research perhaps a little too much. There came a point when I decided I needed a bit of self-imposed ‘external regulation’. So I applied for and won a small grant and employed a research assistant for about two months. Now if you employ a research assistant you have to ask them to do something and this requires a certain amount of organization of the work done to date and work needed to be done. This proved to be a great help – and eventually I reached a point where I could submit a manuscript to a publisher.

The resulting narrative is historical and political. The underlying explanation of increases in educational access is not singular. Rather it is a combination

of economic, political and socio-cultural conditions and structures on the one hand and of powerful agents for change – politicians, bureaucrats and teachers. In the little time that remains I can provide only a glimpse of the web of explanations of expansion over the most recent period 1977-1994 explored in the book.

OHP

- Direct state intervention through the take-over of schools
- Indirect state intervention through the granting of citizenship to stateless persons
- Growth of a labour surplus in the plantations
- Growth of foreign aid for social sector development in the plantations
- The agency of politicians in the context of civil war
- The agency of education officials of plantation origin
- The agency of critical mass of ambitious young teachers of plantation origin

Concluding remarks

To conclude. I have shared with you just two pieces of my research selected from over thirty years of work in this field. They are very different. The first

was a cross sectional study of young children designed to generate new knowledge about age and context-dependent trends in their understandings of the causes of academic success and failure. In this study the unit of analysis is the individual learner. The second was a historical study designed to generate new knowledge about how a system of education becomes successful in extending provision to hitherto marginalized populations. In this study the unit of analysis is ethnic group educational participation. Because the units of analysis – the learner and the ethnic group - are quite different I have drawn on rather different theoretical literatures – in the first Attribution theory and Piagetian development theory; in the second Marx and social reproduction, and Archer and Weiner on the politics of education and political economy. While both studies employed abstract concepts drawn from relevant theory their initial impetus derived from critical reflections on experience – in the first case as a teacher and in the second as a development worker. And because the studies are addressing different types of research question the methods used to elicit evidence were different. But in both cases I used a mixed methods approach in which both qualitative and quantitative methods played a role. At the end of the day what matters to me is the question, the puzzle, the issue, not the method. I am one those researchers interested in *why* and *how* things happen – why learners and

social groups move forwards, backwards, sideways or sometimes stay still. I am interested in explanations, in causes, influences and effects and I will use almost any idea, technique or paradigm to help me in that quest.