

# J. CLYDE MITCHELL

James Clyde Mitchell

21 June 1918 – 15 November 1995

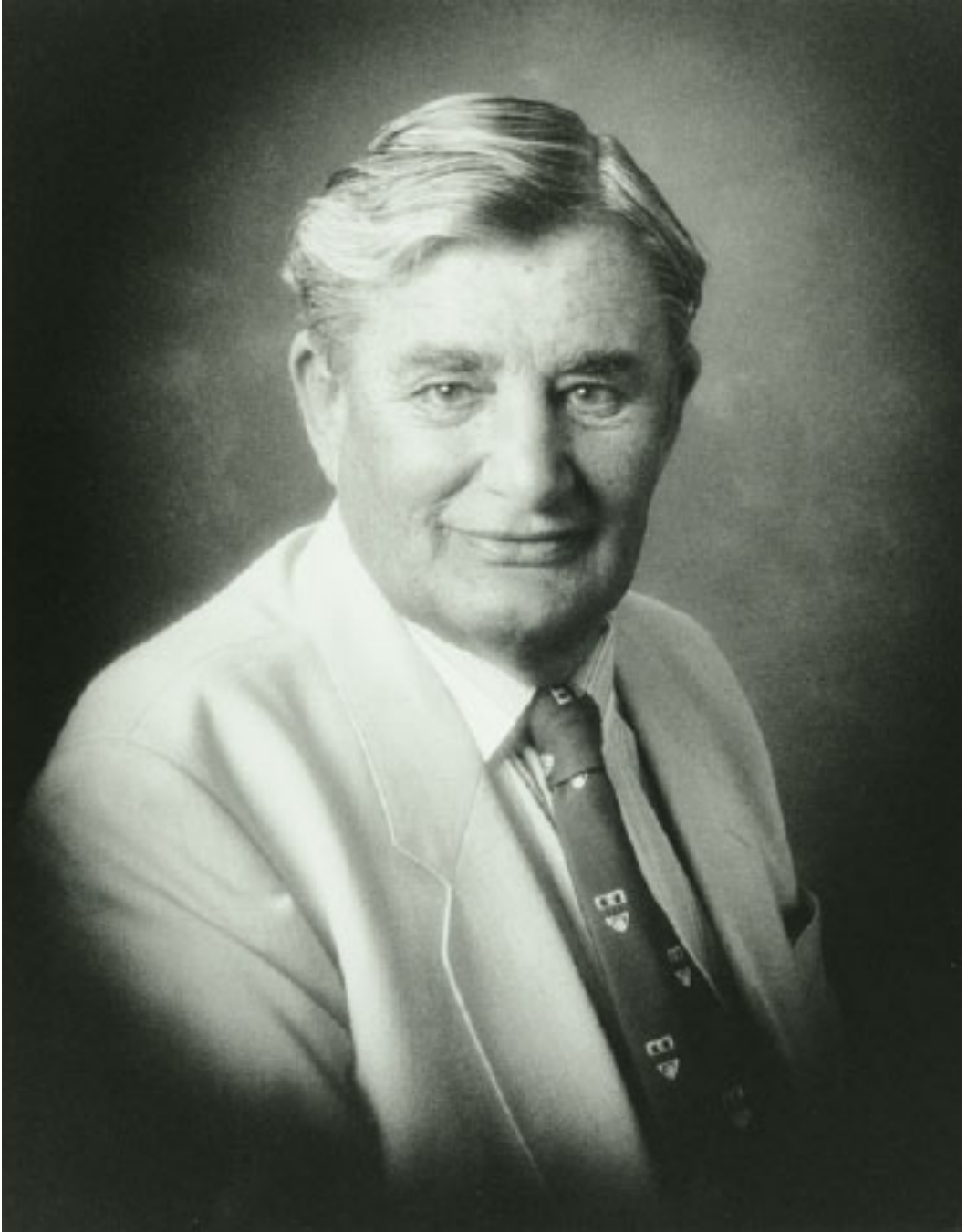
elected Fellow of the British Academy 1990

by

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*Fellow of the Academy*

J. Clyde Mitchell was one of the finest anthropologists of his generation. An early recruit to the Rhodes-Livingstone Institute, he became its fourth Director in 1952, overseeing a remarkably productive period and a turn to urban research. An advocate for case and situational analysis, he also had a feeling for numbers that was unique for the time. His anthropological ‘standards’ include *The Yao Village* and *The Kalela Dance* (both published in 1956), while a fascination with human relations and agency made him a pioneer of social network analysis. From 1955, Mitchell occupied the first Chair in African Studies at the University College of Rhodesia and Nyasaland (now the University of Zimbabwe). A decade later, the Unilateral Declaration of Independence in Southern Rhodesia propelled him to the UK, where he renewed his association with ‘The Manchester School’, before completing his career in Oxford, where he was the first anthropologist elected to the fellowship at Nuffield College.



J. CLYDE MITCHELL

## Introduction

*'Social anthropology is disappearing as a discipline ...'*<sup>1</sup>

James Clyde Mitchell (Clyde to those who knew him) was one of the finest anthropologists of his generation. Born in Pietermaritzburg in 1918, the fourth of seven children (all boys) of a Scottish rail worker, he moved with his father's job from place to place in what was then Natal Province (now KwaZulu-Natal). Growing up in the shadow of colonialism, Clyde spoke Zulu from childhood. He felt keenly the winds of political change and anchored his life's work on how they shaped the course of African urbanisation.

Initially struggling both to afford, and to win, access to a university education, Clyde settled into life as a civil servant, as a hospital clerk monitoring the cost of treating infectious diseases. To break out of this, which he felt he must, he pursued a part-time degree in social studies (by evening class) mounted in 1938 by the University of Natal, then a College of the University of South Africa. Apparently he had social work in mind as a new career. However, he excelled in sociology and psychology, and this opened up a whole new world. Intellectually, he found his niche as an anthropologist, though he was ambivalent about the label and eschewed the disciplinism it implied.<sup>2</sup>

By the time Clyde graduated in 1941, the Western world was at war, and he joined the air force, initially as a pilot. It is rumoured that he flew round the Mediterranean with sociological texts propped up on his map table.<sup>3</sup> Whatever the reason, he soon discovered that his *métier* lay in navigation and it is tempting to organise his intellectual journey—his myriad projects and his, for the time, prodigious output of twelve books and seventy or so articles—in similar geographical vein.

Clyde's academic career began in Africa, at the then-Rhodes-Livingstone Institute (RLI),<sup>4</sup> after a brother stationed in Durban saw an advert at the local university. Clyde

<sup>1</sup>In a letter to A. L. (Bill) Epstein in the early 1950s, Mitchell wrote 'I think that social anthropology is disappearing as a discipline and that the future lies in modern studies' (28.5.1951). At the time, ironically, he was busy transforming the subject in ways that, in the end, would help secure its future.

<sup>2</sup>In the earlier part of his career Clyde, like a number of his colleagues, notwithstanding their disciplinary credentials, used the appellation 'sociologist'. This gave their subject a more 'scientific' feel while signalling affinity to a style of anthropology designed explicitly to escape the charge of 'Othering' (Robert Gordon, pers. comm. 28.8.2018).

<sup>3</sup>Clyde spoke little about his distinguished war record (despite keeping his log books for many years). He reputedly flew without doors, enabling swift, and therefore safe, take-off immediately after landing. His passengers included wounded men, refugees, and emissaries, one of whom was apparently a lieutenant sent by Tito for discussions with Churchill about the future of Yugoslavia (John Goldthorpe, pers. comm. 29.8.2014).

<sup>4</sup>Established in 1937 in Livingstone as the first social research institute in Central Africa, the RLI became the Institute for Social Research/Centre for African Studies between 1966 and 1971, and the Institute for

accepted the position of Research Officer (assistant anthropologist) in 1946 and studied for a doctorate under the supervision of Max Gluckman. He stayed for nine years, assuming the role of Senior Sociologist from 1950, and becoming the Institute's fourth Director from 1952 (working from a field site in Luanshya, before moving with the Institute from Livingstone to Lusaka the following year).<sup>5</sup>

His next move was to Salisbury (Harare) in 1955 into the first Chair in African Studies at the University College of Rhodesia and Nyasaland (UCRN, now the University of Zimbabwe) where he stayed for a decade before being propelled to Britain by Rhodesia's Unilateral Declaration of Independence (UDI) in 1965. From 1966, he occupied the Chair in Urban Sociology at Manchester University, making his final move to Nuffield College Oxford in 1973, which had never before elected an anthropologist. As if to underline this disciplinary homecoming he simultaneously accepted an honorary fellowship in the Department of Social Anthropology at University College London. He nevertheless remained in Oxford for the rest of his life, retiring in 1985, just a decade before his death.

If this implies a singular journey, however, it is an illusion. Clyde's DPhil was registered at the University of Oxford where he was a student at St Catherine's College, living in the precincts for the requisite three terms. Throughout that period, he was continuously employed at the RLI, interleaving even that with lengthy fieldwork excursions to Nyasaland (Malawi). From 1953 and throughout his Directorship of the RLI he was also a Senior Research Fellow at Manchester University, straddling two institutions and narrowing the gap between them with a sense of common purpose. All the while, his work ranged back and forth in space and time; indeed, his final authored book, *Cities, Society, and Social Perception* (1987) made new sense of his earliest research at the RLI, and his final published paper (Mitchell, 1994) was a reworking of data that first appears in the book of his DPhil thesis (Mitchell, 1956a). Mitchell may have journeyed extensively during a highly productive career, but all the time he was steeped in Oxford anthropology, integral to the Manchester School, and spliced to his African roots.

In the end, neither Mitchell's work nor his life fit neatly into geographical, historical or even intellectual categories, and I feel sure he would not have wanted to compartmentalise them in that way. My guess is that he would have preferred to think of himself as part of an extensive interdisciplinary network, spanning three-quarters of a century and stretching across the globe. He would not have placed himself at the heart

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African Studies from 1971 to 1996. Since 1996 it has been known as the Institute for Economic and Social Research (INESOR) at the University of Zambia.

<sup>5</sup>After Godfrey Wilson (1938–41: the first Government-appointed anthropologist in the region), Max Gluckman (1941–7) and Elizabeth Colson (1948–51).

of it nor willingly turned a spotlight on the corner that he anchored. This may be why, despite the radical originality of his contribution, his ideas are less explicitly acknowledged or profiled than those of some predecessors, peers and successors.<sup>6</sup> In truth, however, without his influence an extensive and enduring web of anthropological and sociological scholarship would be far less intellectually remarkable than it is; it would be less dense, less coherent, less vibrant, and would lack the warmth and generosity of spirit that Clyde infused into its life and work.

Bearing all this in mind, I have chosen to profile, in no special order, five facets of Clyde Mitchell's intellectual contribution. They are his unswerving commitment to empirical research (it is probably fair to say that all his key ideas were wrested from close encounters with human subjects); his fascination with numerical techniques; his methodological and conceptual innovations around case, situation and network analysis; his substantive contributions to debates on African urbanisation; and his highly personal political position. These items are in no special order, because in Clyde's work, and indeed in his life, they were inseparable and interleaved.

### Empirical foundations *'the laboratory in the field'*<sup>7</sup>

As a research student Clyde was swept into a unique anthropological collaboration anchored by the RLI, under its second Director, Max Gluckman. Mitchell's doctorate, which he embarked on in 1946 and secured in 1950, slotted into an ambitious multi-centre seven-year research plan which aimed to document the changing times of Central Africa, spanning what are now the national territories of Zimbabwe, Zambia and Malawi (Gluckman, 1945).<sup>8</sup> The scholars Gluckman assembled regarded their work as an exercise in empirical observation, their field as a 'laboratory'. Schumaker (2001, 84) writes engagingly about this collaboration, noting that 'the concept of the field laboratory helped to structure the research and standardize

<sup>6</sup> Reflecting on the legacy generally of the RLI-based south-central African anthropologists, Werbner (1984, 190) observes that notwithstanding the enduring relevance of their work, it is too often underestimated 'or even neglected as if it were hopelessly out of date'. Referring specifically to Clyde's position, in the foreword to Mitchell's last, capstone, book, Kapferer (1987a, xv) explains: 'He is generous with ideas in the extreme and in the most marvelous way. Thus while he generates ideas in others he always seems to impart the impression that these ideas are those of his students or colleagues rather than his own.'

<sup>7</sup> This phrase, used as a chapter heading by Schumaker (2001), alludes to an essay published by Max Gluckman (1946) in a short-lived monthly magazine; it positioned Central Africa as a laboratory for those 'scientific' studies of social life referred to in the RLI statement of aims.

<sup>8</sup> A second seven-year plan, running from 1950, was developed by the Institute's third Director, Elizabeth Colson, who also facilitated the RLI's move from Livingstone to Lusaka (Schumaker, 2001, 119).

the researchers' individual approaches ... [it also] aided the emergence of a shared work culture among the team members, not unlike the unique styles that develop in physics and biology laboratories'. In the end, the RLI programme rolled out in two ways.

On the one hand, Mitchell himself inspired a series of social surveys, whose design and execution owe much to the skills he brought to the group. He in fact drove this part of the agenda. Colleagues embarking on rural ethnographies were persuaded to carry census cards with them, while Clyde himself directed the larger-scale 'sociographic' surveys of the towns.<sup>9</sup> He saw this style of fieldwork as essential for its conceptual value ('a way to refine and deepen' generalisations, or 'bring to light regularities which might otherwise have escaped notice'); equally he promoted it for pragmatic reasons, because census type materials were not, on the whole, yet available in the African countries (Mitchell, 1966, 39). Others, including Max Gluckman, who had previously tried to recruit a demographer to the RLI, embraced this quantitative turn for the scientific credibility it brought to the group.<sup>10</sup>

The main survey project (reported in Mitchell, 1987) was, moreover, truly innovative for its time. A carefully stratified sample of around 12,000 people, interviewed over five years in all the major Copperbelt towns, answered a range of questions that went far beyond a simple census to create a rounded 'social profile of the people'. The schedule covered quality of life in urban versus rural settings, occupational status, perceptions of regionalism and ethnicity, and more. Administering it, however, was a drawn-out process. In a series of letters to John Barnes in the early 1950s, Mitchell writes (20.12.1951) 'whatever [i]nthusiasm I have had for the Copperbelt has died from marasmus. The sociographic survey drags its uninspired feet month after month—nothing emerges from it;' later he seems resigned to failure, noting that: 'The history of the Copperbelt study was a sad one' (22.11.1952). Yet, the survey kept going, and although Clyde was dreading having to check and prepare the data, the results have endured thanks mainly to his own dogged determination not only to complete the work at the time, but also to transfer the data onto 80-column punched cards once computers were installed in universities—a challenge that apparently took five years to complete (Mitchell, 1987, xvi). Notwithstanding sample bias and other challenges highlighted retrospectively by Mitchell (1987) and others (e.g.

<sup>9</sup>Thanks to Richard Werbner (pers. comm. 31.7.2018) for this insight, and for the observation that because Gluckman, though much-influenced by Mitchell's convictions, was not wedded to social surveys (and perhaps did not have the skills to handle them), the quantitative programme stood somewhat apart from the RLI's anthropological tradition except through the persona of Mitchell himself.

<sup>10</sup>Robert Gordon (pers. comm. 28.8.2018) pointed this out and goes as far as to suggest that Gluckman saw statistics as a kind of 'magic bullet' in this respect.

Peil, 1988), it was the most systematic contemporary data resource assembled around the zenith, and in the wake, of colonialism in Africa.

Max Gluckman's overarching plan, on the other hand, which is more widely documented, was anchored in a comprehensive programme of detailed ethnographies whose completion rested on a unique division of intellectual labour. This equally systematic effort formed the core anthropological project of what was later known as the Manchester School (whose impact is profiled by Werbner, 1984, and forthcoming). Although it was (thanks largely to Clyde's powers of persuasion) de facto interleaved with the survey effort it was the area of work in which Clyde initially felt least at home. In the end, however, he would take a lead, conceding that intensive approaches—or 'anthropological methods'—tend ultimately to generate the most 'fruitful hypotheses' (Mitchell, 1966, 42). He was, indeed, Director of the RLI when some of the more important 'Manchester' fieldwork was done (Kapferer, 1987a).

Gluckman's original idea was to assign each researcher to a different 'tribe' or ethnic group,<sup>11</sup> in settings strategically selected to illuminate particular questions or problems.<sup>12</sup> While the programme—with its ambitious aim of gaining a comprehensive, even total, understanding of the region—was never (and probably never could have been) completed, the result was series of complementary monographs and articles, much as had occurred under the tutelage of Robert Park in Chicago a quarter of a century before. The early African effort was initially and in part collected as *The Seven Tribes of British Central Africa* by Colson and Gluckman (1951). As time went on, however, most studies turned into at least one book and the collaboration became, as Hannerz (1980) has observed, a 'school' whose size and scope—with the arguable exception of its Chicago counterpart—no other single localised complex of ethnographies has matched.

The cumulative, comparative and collaborative character of the fieldwork was established from the start. In that sense, it stood in marked contrast to the more individualised model of anthropological endeavour that prevailed at the time.<sup>13</sup> Clyde has described in person and in print the way he, John Barnes, Elizabeth Colson and Max Marwick formed the 'early team' under Max Gluckman, their common approach informed by an initial training programme and followed up from time to time when

<sup>11</sup> Mitchell later distanced himself from the label 'tribe' though it was in common use during the 1940s and 1950s; see footnote 35.

<sup>12</sup> For example, Chewa (Max Marwick), Lozi (Max Gluckman), Luapula (Ian Cunnison), Mambwe (William Watson); Ndembu (Victor Turner), Ngoni (John Barnes), Shona (Hans Holleman), Lakeside Tonga (Jaap van Velsen), Plateau Tonga (Elizabeth Colson), and so on. Mitchell's study was of the Yao villages of Nyasaland (Malawi).

<sup>13</sup> Though in practice and as time went on, neither the RLI project nor the Manchester School more broadly were (as Werbner, 1984, points out, citing Mitchell as a source) as unified internally as they may have seemed from the outside.

they were called in (often from sites hundreds of miles distant) to spend a week discussing progress and problems.

The seminars thus spawned are legendary. Everyone would present a paper covering the data they had secured, their first attempts to make sense of it all, their thoughts about fieldwork and their ideas of how to go on. Such meetings were critical in helping those who struggled with fieldwork—Mitchell very much among them.<sup>14</sup> By providing an opportunity to exchange ideas, work out differences, develop a structured approach to fieldwork and a shared understanding of the importance of detailed documentation, the seminars wrested synergies from the group and were ‘the crucial method of building us into a team’ (Mitchell, interview with Bernard, 1990). Maps, censuses and systematic record-keeping were, at Gluckman’s insistence, essential ingredients of the process, whose field notes (which Bruce Kapferer, pers. comm., 8.2.2016, describes as ‘frightening’ in their meticulous detail) had to be accessible to others, and were eventually lodged in the library.

In short, the ethnographic programme at the RLI called for high standards and painstaking documentation. Some seminars, apparently, are still archived, as are the reams of correspondence that culture of openness and exchange encouraged. This not only enabled team members to travel (Barnes to Norway, Epstein to Papua New Guinea, and so on) without losing a sense of connectedness but produced vigorous exchanges of letters around research ideas and achievements that kept the group in close (if uneven) contact with one another and added to their collective conceptual edge. In Clyde’s case there is a particularly notable exchange of long, inspiring letters in the late 1940s and early 1950s with Gluckman (whose supervisory role soon evolved into collegiality and friendship), Barnes (a close friend, fellow mathematician, and ally among peers),<sup>15</sup> and Epstein (a research student-turned-partner in a collaboration that Richard Werbner, pers. comm. 31.7.2018, suggests forms ‘the very heart of Mitchell’s Copperbelt contributions and project’).<sup>16</sup>

Mitchell’s earliest original (doctoral) contribution to this collaborative effort was a study of the Yao, a Bantu-speaking matrilineal Muslim community in rural Nyasaland (now Malawi) with whom he worked in two phases from September 1946 to September 1947, and from September 1948 to June 1949, with an interlude in

<sup>14</sup>This struggle is scattered across Mitchell’s correspondence, for example in his letter to Max Gluckman on 25.3.1949, reporting that ‘fieldwork languishes as usual’.

<sup>15</sup>On 20.3.1949, for example, Clyde sent seven pages of closely typed notes to Barnes offering a critique and elaboration of his marriage paper (later a book).

<sup>16</sup>Interestingly, but not especially unusually for the time, they rarely published under joint names, except perhaps for pragmatic reasons. Mitchell, for example, reflecting in a letter to Epstein on a rare joint work (Mitchell and Epstein 1959) indicates: ‘I am glad we have got the stuff out—I hate to see material lying around not made use of’ (15.1.1956).



Oxford from May 1947 to fulfil the DPhil residence requirement. The work adopted a now classic, then innovative, mixed methods approach, combining historical analysis, a village-by-village census and survey, mapping exercises, case studies, use of local tax records and more.

Such work was fraught with problems. Some were practical (the challenge of living in tents, with few amenities, far from home), others were financial (Clyde reports living from hand to mouth when his children were young). Many hurdles were bureaucratic, and most, of course, were political (see later), including being treated with suspicion by all parties. As an exercise in anthropology, moreover, working from his nomadic base in a tent, he struggled to find a niche: ‘Each day I set out with good intentions ... and each day end up by discussing witchcraft’ (letter to MG 25.3.1949) (a topic he felt was already well explored by Evans-Pritchard and Gluckman himself).

Although Clyde’s CV is peppered with outputs from this project (whose substantive significance is considered later), the book of his thesis remained unpublished until long after he had left the Copperbelt for Salisbury. As he notes in the opening chapter: ‘the hardest part of fieldwork is writing it up’ (Mitchell, 1956a, 7). This is another lament that he carried throughout his working life. My own notes, having read a cross-section of his letters, read ‘JCM spends his whole career struggling to write everything up ...’.<sup>17</sup> So it was September 1954 before he reported to Barnes that ‘the Yao Book is posted off’, and 22 June 1956 before the published version—*The Yao Village*—came out, just in time for Clyde’s proud father (to whom, together with the memory of his mother, it is dedicated) to see it before he died.

It is interesting to note that so pervasive was the collaborative ideal, that in the acknowledgements even of this most individualised of treatises—his doctorate—Mitchell (1956, ix) wrote ‘the book is as much my colleagues’ as my own’. He also acknowledged that his first wife, Edna, had ‘the major share’ in the book’s preparation. They refused to be separated by the demands of the project and she shared with him the difficulties of life in the field. Indeed, hers and their first (at the time only) son Donald’s presence clearly helped Clyde integrate into village life. Among professional colleagues, he acknowledges Max Gluckman as the person to whom he owes the most. Gluckman, at the same time (while lamenting that more of the ethnographic material had not been packaged into it), described the pre-publication

<sup>17</sup>Though it did not prevent him building up a remarkable CV. For example, by 1954, while Director of the RLI, Clyde reported being ‘thoroughly browned off and doing nothing at all in the way of getting any of my stuff out’ (letter to Barnes 6.1.1954) yet by September he had managed to post off the manuscript of the book of his DPhil (*The Yao Village*). Similarly, a year later, he wrote, ‘I have got nowhere with writing anything except letters’ (to Epstein 16.8.1955), even though just months later *The Kalela Dance* was published, again to great acclaim.

manuscript as ‘magnificent, a notable contribution, well-argued and very interesting to read ... a great book’ (letter 9.11.1955); later, on 24.1.1957, Gluckman wrote of the book itself: ‘I have just been lecturing on it for two hours, and it is perfect. It left me feeling not only humble but also envious.’ The work is described by a contemporary reviewer as ‘an outstanding example of the recent attempt by social anthropologists working in Africa to document their findings statistically (Fallers 1957, 731). As for one or two cooler reviews, Gluckman’s observation (writing to Mitchell 28.5.1957) was that: ‘It was too good for the present state of Anthropology.’ Later, of course, it would become a classic; perhaps the first original use of the extended case study in the discipline.

If *The Yao Village* was a classic of one type, exemplifying the systematic survey strand of the RLI endeavour, Clyde’s other classic—*The Kalela Dance*, possibly the best, most original piece of contemporary anthropology—embraced and enlarged the ethnographic tradition that the Manchester School was best known for. It was a fortuitous encounter; an accident of location described both in interview with Russell Bernard (1990) and by Shumaker (2001). Based with his family in Luanshya in a house provided by the RLI, Clyde was drawn by the noise of the drums on a Sunday afternoon to the nearby municipal African township. There he witnessed a tribal, or ethnic, dance which—unlike its counterparts further South—was not ablaze with extravagant costumes and exotic colour but was performed by young African men dressed in European suits ‘shuffling around in a circle’. More notable still, given that ‘tribal’ dances were generally attracting rather small audiences in urban settings, the Kalela was ‘packed thick’ with spectators. Clyde’s curiosity was piqued and, together with field assistant Sykes Ndilila (who also translated the song), he embarked on what was to become one of anthropology’s seminal works, tracing out in minute detail the structure and form of the dance, together with the fourteen-stanza song embedded in it.

Clyde’s engagement with practical fieldwork was in some senses (and measured by the scale of the project) cut short when he moved to the Chair at the University College of Rhodesia and Nyasaland (UCRN). Like so many scholars steeped in fieldwork, the teaching and administrative burdens he encountered as his career developed, together with the urge to write up the data he already had, might well have curbed his travel. He also had a large and growing family (a daughter and three sons—Gillian, Donald, Keir and Alan), all under the age of ten when he moved to Salisbury, where he also lost their mother, his first wife Edna, to leukaemia. By then, however, he had assembled as much material as any anthropologist of his generation and more than enough to complete his career, the majority of which was devoted to analysing and publishing his Copperbelt materials. There might also have been a shift in his thinking about the nature of empirical research and analysis

around this time; one of his sons recalls a growing interest in numerical techniques and a renewed determination to define his subject as ‘sociology’ (D. Mitchell, pers. comm. 22.5.2018).

In her assessment of the work of the Manchester School, Shumaker (2001, 256–7), concludes that the book of the Copperbelt project—a much hoped for and talked about ‘final jointly written volume on the industrial revolution in central Africa’—was never realised. It is possible, however, that Clyde’s last book (Mitchell, 1987) was his own attempt to see this through, albeit drawing, by then, primarily on his own materials.<sup>18</sup> It was not the last word, of course: the impact of the Manchester School, and in particular of Max Gluckman as the individual most associated with it, continues to fascinate scholars (its momentum most recently assured by Gordon, 2018, and Werbner, forthcoming). That, however, is (mostly) another story.

These empirical underpinnings stayed with Clyde throughout his life, as did the collaborative spirit they nurtured and which, for Mitchell, was one of the defining features of the Manchester School. He certainly took this ethos to heart when he, in turn, became director of the RLI, using it as a platform from which to launch a new and wide-ranging programme of self-consciously urban research. Always hungry for new material, he had even hoped to apply some elements of the RLI model when he moved to Manchester University and was involved in a programme of linked studies funded by the ESRC to chart the social impacts of the growing conurbation.<sup>19</sup> He certainly remained a stickler for regular contact with students in the field to stimulate ideas and reduce isolation. As he notes in an afterword to a collection of essays assembled in his honour: ‘One lives through the research experience of one’s students as they are conducting their enquiries ... I required them to write to me once a month even if they had little to report because I know how lonely anthropological research can be’ (Mitchell, 1995, 335).

<sup>18</sup>As early as the mid-1950s, Mitchell wrote to Barnes indicating that he had taken on the mantle of writing a final work, noting that he was spending ‘much of my time now working on the Copper Belt material which promises to be [a] huge volume’ (JCM to JB June 1955). Much later, in the early 1980s, I recall Clyde speaking to me, and possibly to others, about the responsibility he felt to bring to fruition the ideas and materials that he and his colleagues had assembled. He apparently held a contract for such a book for many years (though not with the publisher he eventually used for *Cities, Society and Social Perception*).

<sup>19</sup>This is referred to in his papers, though not contained in the e-searchable ESRC archives (which do not go back that far). Richard Werbner has some memory of a project on social stratification (pers. comm. 31.7.2018) and there may therefore be records in Manchester (e.g. among the papers of Max Gluckman) or in hard copy reports to what would then have been the SSRC.

Feeling for numbers  
*‘The only person I knew who had unbounded enthusiasm  
 for my data and statistical problems’<sup>20</sup>*

In an interview in July 1990 in the garden of his Oxford home, Mitchell, speaking to Russell Bernard, describes in passing how he spent his spare time while training for the air force in the 1940s. He took the opportunity to calculate chi-square values for illegitimacy and religion among the Zulu on a slide rule! This self-defining moment sums up a key element of his intellectual legacy: an insatiable appetite for using formal descriptive tools to illuminate the substance of social life.

Clyde’s fascination for numerical techniques anticipated, embraced and survived the so-called quantitative revolution of the 1960s, just as his systematic collation and organisation of empirical data of all kinds presaged the advent of cumulative social science and the turn to ‘big data’. Throughout his career his receptiveness to—indeed demand for—new empirical material was more than matched by his passion for making the most of it analytically. Moreover, his early sense that there would always be new analytical possibilities—a feeling for numbers perhaps—influenced the way he approached even his ethnographic data collection. His early recognition that prestige was a salient social marker, for example (see later), drove him not only to observe and describe the deference the Yao villagers paid to the ‘Headman’ but equally to ‘make a special effort to collect as much quantitative data relating to these indicators of prestige that I could’ (Mitchell, 1994, 267).

Clyde’s letters over the years are peppered with this appetite for numbers. When Max Gluckman sent him a correlation chart in the late 1940s, he described it as a masterpiece. ‘I am’, he writes, ‘hanging it in the house in place of my Cézanne’, following up, wryly, with the observation: ‘There is an arithmetical approach to the same problem that would have taken up less space’ (JCM to MG 18.5.1947). His letters are full of phrases like ‘I think I have worked out a survival table for Yao marriages’ (JCM to MG 25.3.1949); or that he had done ‘a little algebra’ on Epstein’s PhD data on the decline in Union membership ‘for fun’ (JCM to AE, 16.8.1955). It is no surprise to find that when Max Gluckman tried to tempt Clyde to move to Manchester in the early 1950s, he wrote as a PS to his letter of 6.5.1949: ‘I should add that Manchester is stiff with calculating machines! and that in our faculty statistics are taught!’ Neither is it unexpected to find that Clyde’s last published paper (whose proofs he checked during his last admission to hospital) contains a reanalysis of data collected during

<sup>20</sup>Part of a tribute from the late Professor Ceri Peach read at a memorial service for J. Clyde Mitchell held at Nuffield College (1996).

his doctoral fieldwork in a Yao village ‘using techniques which have become available since’ (Mitchell, 1994, 268).

In the intervening years, Clyde devoted a great deal of time and many publications to exploiting the new calculating power that pre-computers and then a generation of mainframe machines brought to his fingertips. He thought it important, and he delighted in it. He initially managed to analyse his early census work at the RLI, for example, by securing the use of a Hollerith machine owned by Anglo-American Copperbelt mines; he was also drawn to the mining companies’ own punch-card database of staff records. During a gap with no director at the RLI in 1951, when Mitchell was the heir apparent and the wheels were grinding slowly, he was consoled by writing about ‘indices of urbanisation’.

In September 1954 his dismay at the plodding role of the Directorship he had now assumed at the RLI was temporarily alleviated by news that the Trustees had allowed him to buy a Powers-Samas tabulating machine (a device that read punch-cards mechanically). By the following year, he had embraced the move to Salisbury, with the caveat that ‘The University are taking so long to let me have a calculating machine that I can’t get out the short statistical pot-boilers I have almost ready’ (letter to Epstein 16.8.1955). One of these potboilers was apparently a factor analysis of the mining staff records mentioned above; this would have been among the earliest substantive applications of the technique in the social sciences.<sup>21</sup>

As the years went by, Clyde was increasingly fascinated by the range and complexity of the numerical techniques available to social research, and he played a key role in stimulating their use in social anthropology. His interest and impact grew through the 1970s and culminated in a wide-ranging edited collection (Mitchell, 1980), which did ‘a fine job of demonstrating the substantive value of mathematical approaches’ (Robbins, 1983). Notwithstanding the inscription he wrote in my copy—‘some dull reading for a light afternoon’—and belying its paltry twenty-three Google Scholar citations, it is a path-breaking collection with an engaging editorial introduction, introducing some less well-known but substantively illuminating approaches that have since become standards.

While ‘Numerical techniques’ is perhaps his best-known intervention in this area, it is notable that, in response to a request I sent to colleagues for memories of Clyde and his work, his feeling for numbers was a recurring theme.<sup>22</sup> Commenting on his

<sup>21</sup> I have not been able to track down every paper on Clyde’s publication list, but as far as I can see, this analysis was never published. His landmark round-up of the causes of labour migration (Mitchell, 1959) contains phrases and conceptualisations that are entirely appropriate to a factor analytic approach but it is based on secondary sources.

<sup>22</sup> Even his family reported this to me. For example, in the early years, he had a hand calculating machine that one of his sons, recalling his childhood, describes as ‘a combination of levers and number barrels

time at Nuffield College, his friend and colleague John Goldthorpe noted, ‘One remarkable thing was how much quantitative analysis Clyde did in those days—when computers were still difficult and user non-friendly beasts—on a hand calculator. I recall him once telling me that in fitting a particular statistical model he had to carry out twenty-seven iterations of an algorithm in this way before getting a satisfactory convergence!’ (pers. comm. 29.8.2014). Peter Jackson reflecting on the 1980s noted that ‘his enthusiastic engagement with each project he supervised’ extended in his, Jackson’s, case to ‘undertaking a smallest space analysis of some of my housing data’ (pers. comm. 27.8.2014). Vaughan Robinson, writing to Clyde’s wife Jean shortly after his death, told the story of how, at a particularly tedious College meeting, Clyde appeared to be dozing, but was in fact leafing surreptitiously through a wedge of computer printout—balanced on his knees under a table—containing the results of his latest efforts at multi-dimensional scaling.

I am guessing that practically all his students, and most of his colleagues, have a story of this kind to tell. Eleanor Kelly’s work on the length of stay of homeless families in temporary accommodation is a case in point. Rather than simply setting out the characteristics of the sample and speculating on the variable (always slow) rate of rehousing into the social sector triggered by various priority needs, Mitchell found a way to quantify the data and conduct a multivariate analysis to show that, ironically and counter-intuitively, length of stay was longest for those *most* eligible for rehousing (Kelly et al., 1990).

Even the co-supervisor (with Clyde) of my own DPhil was drawn into this pattern, when Clyde spotted that by using multidimensional scaling techniques to classify indices of similarity, and logistic regression techniques to analyse the results, it would be possible to resolve a longstanding debate around segregation and intermarriage. Peach and Mitchell (1988) thus established that spatial separation reduced the odds of intermarriage between ethnic groups in San Francisco (in 1980) to a greater extent than either educational difference or social distance. This numerical imperative did not, of course, flow in just one direction. Across the top of an offprint he gave me of a paper using generalised procrustes analysis to cast light on processes of social stratification (Mitchell and Critchley, 1985), Clyde has written ‘[a] very useful technique this, that Frank introduced me to’.

Mitchell could, in short, be relied on to draw everything possible from the fragments of social life that fieldwork captures from the chaotic *melée* that makes up

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driven by something similar to a sewing machine handle’. From time to time, around bedtime, ‘teddy’ might fall into this machine and be shredded into a thousand tiny bears. Clyde would apparently recount with mirth and pleasure how the micro-bears spread anxiously in a swarm searching for a suitable integration function enabling them to reassemble into the original toy (Don Mitchell, pers. comm. 22.5.2018).

the world. He had a feeling for numbers—an intuitive understanding of what quantitative techniques could achieve for knowledge. To be clear, however, his fascination with the numerical was not with the power of technique or the clever manipulation of statistics for their own sake (he describes having to teach the methods course at Manchester as ‘absolute hell’). He was concerned rather with what the advent of new ways of handling, manipulating, and analysing social and anthropological data added to his conceptual and substantive interests. The only restriction he placed on contributors to his book on numerical techniques (Mitchell, 1980, 2–3), for example, was ‘that each paper should relate to a substantive ethnographic or anthropological problem’.

In short, Mitchell was an advocate of numerical reasoning in the broadest sense, not of number crunching as an end in itself. Craig Calhoun describes it as the difference between ‘caring about what people do in their lives and what happens in their lives’, on the one hand, and ‘relatively abstract intellectual puzzles’, on the other (pers. comm. 29.11.2015). Kapferer (2010, 7) puts it more formally: ‘The study of statistical analysis was, in Mitchell’s view, thoroughly dependent on ethnographic work that was alive to social variation and its situated production.’ This is more than evident in the ideas we turn to next.

### Epistemological energies

*‘In Manchester School anthropology, fieldwork materials and conceptualisation shape and use one another, producing recurrent epistemological surprise’<sup>23</sup>*

The RLI model of working produced broadly comparative studies across the so-called Copperbelt. This early leaning to comparability in qualitative as well as quantitative observation underpinned the development of two epistemological ideas that Clyde would play a major part in advancing across his career. Both challenged dominant wisdoms relating, first, to generalising from unique occurrences, and second to the conceptualisation of social life itself. They are, case and situational analysis on the one hand, and network analysis on the other.

### Case and Situation

*‘It is always good to advance theory and empirical analysis at the same time, but in practice very difficult to move on both fronts at once’.<sup>24</sup>*

The case study approach which Clyde encountered through his early social work training became central to the operation of the Manchester School. Its epistemological

<sup>23</sup> Evans and Hendelman (2006, ix).

<sup>24</sup> Memorable advice offered by Clyde to his research student, Craig Calhoun (pers. comm. 29.1.2015).

significance is set out in a seminal paper (Mitchell, 1983) that was once envisaged as a book. The appeal of the case study—an abstraction from a wider situation made to illustrate a theoretical point—is that it offers a means by which the rigour of qualitative methods can be established in its own right, rather than set against (or positioned as lacking in relation to) standards applied to test the significance of quantitative social and scientific research.<sup>25</sup> To that end, Mitchell (1983, 192) defined the concept of ‘case study’ quite tightly, and certainly as very much more than an exercise in bounded description. He positions it, rather, as ‘a detailed examination of an event (or series of related events) which the analyst believes exhibits (or exhibit) the operation of some identified theoretical principle’. Mitchell’s main interest was in the possibility to use such material to generalise, not by analogy with statistical inference (which would imply standard questions administered to random samples), but by invoking a logical inference in which ‘the validity of the extrapolation depends not on the typicality or representativeness of the case but upon the cogency of the theoretical reasoning’ (p. 207). His point is that what happens in one case study may (probably will) be unique; but that does not mean that it will not allow general principles about particular social phenomena to be drawn out.

Logical inference is a serious undertaking which demands intensive knowledge of the context from which case materials are drawn. Its value for Clyde was that ‘it forced one to appreciate the complexity of behaviour of people even when operating within, say, the framework of a lineage system’ (Mitchell, 1986a, 17). So, case study research is integral to, and inseparable from, the situational approach that is more particularly seen as a hallmark of the Manchester school.

‘The starting point in situational analysis’, for Mitchell (1987, 8), ‘is the assumption that social behaviour exists as a vastly complex set of human activities and interactions about which any one observer can appreciate only a limited part.’ Characteristically, in developing this idea, a great many of the most pertinent ideas were exchanged and sharpened up in an exchange of letters. A letter from Clyde to Max Gluckman on 2.11.1948 following the death the previous year of American sociologist W. I. Thomas remarks: ‘He is the person that I get the situational approach from and it is an approach which I think holds quite a lot of answers to our difficulties.’ He was especially engaged by Thomas’ interest in ‘defining the situation’, a conversation he took up with Barnes in correspondence early the following year (e.g. Barnes to Mitchell 20.3.1949). It retained his fascination through to the conclusion of his last book: ‘The importance of this idea was that it located the reality of norms and customs in the perceptions of the actors in a social situation’ (Mitchell,

<sup>25</sup>This is an interesting take on the scientific aims of the RLI: the ‘laboratory in the field’ established its own rules of the game for securing generalisations from ethnographic data that had their own validity.



1987, 289). This is a further reminder of the lasting influence the early American sociologists, especially those based in Chicago, had on the Manchester School. It is a signal too that while it is easy to attribute a set of ideas to a single person—Gluckman, for example, is generally credited, not least by Mitchell himself, as the originator of the situational approach developed at the RLI, and his idea, in turn, was much-influenced by Evans-Pritchard (Kapferer, 1987b)—the truth about scholarly ideas is that they arise in a community of interest.

To epitomise the situational perspective he advocated, Mitchell often referred—even in his later years—to Gluckman's (1940) essay on the opening ceremony for a bridge in 'Zululand'. In the mid-1950s he urged Gluckman to reprint the paper 'because I feel there is a certain freshness in the approach which I think you will never be able to capture again with this material' (2.11.1956). Mitchell wrote an introduction to the reprinted paper which Gluckman found 'not only flattering, but also most charming—it touched me deeply' (28.5.1957). Mitchell went on, however, to publish a paper that, for some, would eclipse this classic, and would in its turn become what Kapferer (2005, 101) describes as 'one of the best examples of the situational approach'. It is an in-depth study of one set of enactments of the then-widely performed Kalela dance. The impetus this gave to the qualitative research tradition in the RLI was noted earlier, and its substantive significance is considered later. Epistemologically, however, this thoroughgoing case study is at the heart of a research tradition that 'loses sight neither of the complexities of social life through time nor the importance of theorizing these' (Evans and Hendelman, 2006, ix). It is an important precursor of what would later be described as an anthropology of generic moments (Meinert and Kapferer, 2015; Kapferer, 2010).

There is a sense in which advancing the case for situational sociology could be seen as Mitchell's core project. The first chapter of his last book is an extended, thirty-three-page, essay on the topic. It is a theme he returned to time and again to argue not just for the practice but also for the absolute necessity of 'the intellectual isolation of a set of events from the wider context in which they occur in order to facilitate a logically coherent analysis' (Mitchell, 1987, 7). That is, of course, a contestable position, especially in the light of more recent innovations in participatory research, but its potential for making sense of complexity, and especially for understanding that analytical concepts (such as 'class struggle') are not necessarily recognisable in everyday life, has endured. As Kapferer (1987, x) puts it in his forward to the book: it is 'a method whereby the meanings in use can be systematically unravelled, their perceptual texture peeled away, and the social processes which generated them examined concretely'. Most importantly, Mitchell's situational analysis embraced both 'a practice of structure' and 'a structure of practice' (p. viii); it recognised that a prevailing political-economic order can be unsettled by actors who have agency and creativity,

even if neither the potential nor the limitations of that are immediately obvious in the lived experience of daily life.

It is easy to argue (and indeed has been argued) that the situational perspective lacks critical, structural edge, but it is hard to find a basis for this in a close reading of Mitchell's work. From very early days, Mitchell spoke and wrote of the distinction between, and interleaving of, structural, categorical and personal relations (e.g. his letter to Epstein dated 12.2.1958) and, as he himself points out (Mitchell, 1987, 313), a situational perspective is impossible without reference to, and understanding of, a wider structural setting. So what Mitchell's project achieves—with its insistence on tackling the naivete of some styles of qualitative research—is certainly not reductionist. In truth, it offers a radical departure in anthropological reasoning; a way of organising and interpreting social data that Max Gluckman among others described as key for the next generation.

### **Linked in**

*'The founder, if anyone, of social network analysis'*<sup>26</sup>

Clyde Mitchell was a central figure among a group of scholars who, during the 1950s, were dissatisfied with anthropological functionalism, wary of the turn to structuralism, unhappy with institutionalism, and deeply interested in the articulation of micro-social processes with the political-economy of urbanisation. Mitchell was particularly concerned with a disconnect between theoretical expectations about structural change, on the one hand, and the fruits of ethnographic observations on the other. This drove his search for alternative—or more properly complementary—frameworks to account for the extent to which, under colonialism and industrialisation, traditional institutions were breaking down, while newer ones struggled to establish themselves. The case study approach, with its emphasis on complexity, was a step in the right direction, but Mitchell soon realised that he 'needed some other method of formal analysis to understand what was going on in a systematic way' (Mitchell, 1986a, 17).

That 'other method' had to do with wresting order from the character, form and content of social networks. 'It was', observed Mitchell, speaking in 1990 about his fascination with the concept, 'the minutiae ... people doing things in relation to one another which fascinated me.' The process-orientated concept of a web, mesh or network of social relations was attractive from the start. It probably has its origins in 1930s psychology, or even earlier (Freeman, 2004), and the broad idea quickly found

<sup>26</sup> Discussing Mitchell's influence by telephone Barry Wellman, then co-Director of Toronto University's NetLab (<http://groups.chass.utoronto.ca/netlab/barry-wellman/>) identified Mitchell as '*the founder, if anyone, of social network analysis*' (pers. comm. 31.3.2016).

its way into a wider literature during the decade that followed. Mitchell's *côterie*, however, were the first to use networks conceptually and analytically in the social sciences more broadly, and they were publishing on it at a time when few others were.

Barnes (1954) is usually credited with the first anthropological use of networks as more than metaphorical—as both a conceptual and an analytical tool. Mitchell (1969, 4) himself drew a distinction between earlier studies informed by formal questionnaires, and Barnes' interest in working with networks 'based predominantly upon participant observation'. These early experiments were nevertheless part and parcel of the collective that was the Manchester School. That is what helped Elizabeth Bott (1957) reshape her research on marital relations in London into what Mitchell (1990, in interview) regards as the first publication in which the idea of network was used substantively as an analytical tool to explain behaviour.

Thinking with networks offered a way to lay bare the array of linkages—the flows of information and ideas, of goods and services, of beliefs, values and expectations, of power and influence—that underpin, indeed help realise, the structures of social life; structures which may or may not fit within conventional abstractions such as norms, institutions, class or ethnic divides. The appeal of this is apparent in Mitchell's own earliest writings. In a letter to Epstein (12.2.1958), for example, he talks of social relationships being ordered in three kinds of ways 'depending on the intimacy or face-to-face contact required of them': these are—categorical 'where the contact is superficial and people react to symbols or uniforms'; 'structural' 'whereby people interact consistently within an institutional framework'; and 'network type relations' which applied to the more personal relations of kinship and friendship which 'are unique for each person and ramify across the community'. By the end of his inaugural lecture at UCRN Mitchell (1960, 30) had arrived at a conception of societies as 'complex reticulations of social relationships in which people are linked and cross-linked by numerous ties and bonds', whose contents and characteristics were key to understanding the conflicts and continuities of plural societies.

It is hardly surprising that when, during the mid-1960s, Mitchell convened a field-work seminar (continuing a longstanding tradition) for a new generation of urban anthropologists at the UCRN, networks were high on the agenda. The result was an edited collection (Mitchell, 1969) that one contemporary reviewer regards as 'the first major work to explore systematically the utility of network analysis of sociological field materials' (Aronson, 1972, 476), and another describes as 'a pioneering work of theoretical significance to social anthropology in any ethnographic context' (Gulliver, 1971). It includes an editorial introduction that Carrigan and Scott (2011) recognise to be 'one of the earliest summaries of a formal social networks methodology'. This lengthy essay sets out the concept and use of social networks and puts flesh on the bones of morphological descriptors like anchorage and reachability, and of

interactional features like content and durability. In subsequent years, as well as taking on the associate editorship of the journal *Social Networks* (launched in 1978), Mitchell made numerous interventions in this interdisciplinary paradigm shift, laying the foundations of what would become a new anthropology of complex systems.

As might be expected, Clyde warmly embraced the quantitative turn that some of this literature took, often writing his own computer programs, and securing research council grants to facilitate this.<sup>27</sup> However, he was also wary of a growing disparity between ‘the underlying assumptions and therefore the characteristics of networks taken to be significant by those interested in network analysis algorithms’ on the one hand, and matters of concern to ‘those interested in substantive issues’ on the other (Mitchell, 1979, 438). Although he ventured at least one essay on the untapped potential of key techniques for which data in appropriate formats had yet to be collected, more usually he was worried that innovations in the numerical were outstripping both theoretical and substantive advances, and the fieldwork required to operationalise them (Mitchell, 1974, 279). He thus used a keynote lecture in the USA (Mitchell, 1986a) to insist on the merits of fieldwork—of securing qualitative, ethnographic data—in network research, arguing that while formal analytical procedures may be essential, it is the quality of the observational data driving them that is key. The real objective of such work was, he urged, to illuminate social life not push the boundaries of statistical or mathematical technique. That, he believed, required analysts not only to ‘draw on more extensive information about the people involved’ but also to become familiar with ‘the overall social context in which these people happen to be located’ (Mitchell, 1986b, 91).

That is probably what prompted Kapferer (2005, 112) to recognise that a key merit in Mitchell’s approach to network analysis was ‘to attend to individual agency without losing the significance of larger structural forces’. It was also a way to explore the impact of large-scale social processes without losing their connection with lived experiences. It could indeed be said that Mitchell’s aim in invoking the concept of networks was to explore the ongoing realisation of the social, of humanity itself; and this, as Kapferer (2014b) recognised much later, is one of a number of links and complementarities between what he describes as ‘Mitchellian’ and ‘Latourian’ notions of networks. There are, to be sure, gaps that neither of them fill: Kapferer talks about values, and Strathern (1996) about lengths, cuts and stopping points; the literature has moved on. Yet, it might still be said that Mitchell’s take on performativity was prescient of Latour’s, and that it is Mitchell who spearheaded the approach which, in Boissevain’s (1979, 392) words, ‘opened a door to permit the entry of interacting

<sup>27</sup>One of the doctoral students funded in this way, Martin Everett, became co-founder in 1977 and President of the International Network for Social Network Analysis (INSNA).

people engaged in actions that could alter and manipulate the institutions in which they participated’.

It is not surprising in light of all this that, in 1986, Mitchell received the Simmel Award for major advances and achievements in the study of social networks; it may indeed have been the flourishing of this strand of work that finally (and belatedly) sealed his election in 1990 as a Fellow of the British Academy. There is, however, something of a discontinuity running through the world of networks. On the one hand, Clyde’s books and articles on networks account for by far the majority of his citations.<sup>28</sup> As Hannerz (1980, 181) observed nearly forty years ago, Mitchell’s approach to networks probably inspired ‘the most extensive and widely applicable framework we have for the study of social relations’; morphological and interactional qualities together providing ‘an idea of what is potentially knowable and what would be needed for something approaching completeness in the description of relationships’. On the other hand, given the wide-ranging contemporary impact of his ideas, and their resonance with the cutting edge of post-social science, Hannerz has more recently observed that ‘as network analysis has spread in the social sciences, the foundational work of Clyde and his colleagues is seldom given the recognition it deserves’ (pers. comm., 25.8.2014).

Where this oversight occurs, it partly reflects the massive growth of the field in recent years and its appeal to very many different disciplines, each with their own histories and traditions. It also reflects the enormous potential of the idea, its rapid spread in popularity,<sup>29</sup> and a certain disciplinary ‘stickiness’ in some areas. It may also reflect the fact that few contemporary commentators are either as generous as Kapferer who, when billed as ‘one of the founders of social network analysis’, used that keynote specifically to describe a movement inspired ‘most notably by Clyde Mitchell’, or as vocal as Barry Wellman, who not only featured Clyde’s influence in his later works (Rainie and Wellman, 2012; Wellman and Berkowitz, 1988) but identified Mitchell as ‘*the* founder, if anyone, of social network analysis’ (pers. comm. 31.3.2016), who ‘did more than any other person to put network analysis on the map (Wellman, *Ties and Bonds*, n.d., 13).

Certainly in his later years Clyde was most in demand for this aspect of his work and was, thanks to his third wife, Jean, able to travel and teach on it in retirement

<sup>28</sup> Alan and Don Mitchell estimate that 75 per cent of Clyde’s citations and licensing (ALCS) fees are accounted for by networks.

<sup>29</sup> Rogers and Vertovec (1995), for example, introducing a collection of essays assembled essentially as a festschrift for Clyde, identify four ways in which the idea could transform even one field of urban studies: documenting the rural-urban spectrum empirically; exploring the interleaving of the social with the spatial; charting the flow of information and resources; and capturing the agency involved in social change.

despite various challenges to his health.<sup>30</sup> As time went on, then, networks were what tied Clyde most explicitly into the ongoing international, interdisciplinary and intellectual adventure that had been his life's work. Viewed in the round, it is the positioning of networks as an analytical tool that is arguably the most influential of his intellectual legacies. It is fitting, then, that the UK's world-leading centre for social network analysis is based at Manchester, rooted in the work of the Manchester School and 'named after and dedicated to Clyde, in memory of his foundational role for the development of social network analysis'.<sup>31</sup>

### Substantive challenge

*'the best anthropologist of the new generation'*<sup>32</sup>

From his earliest work at the RLI to his final few publications, Clyde Mitchell was pre-occupied with understanding the political-economy and cultural politics of African urbanisation. This forced him to think about the drivers of urban-industrialism, the residualisation of rural life, and the processes of labour migration that linked the two.<sup>33</sup> The context was time- and space-specific and his substantive project was, at heart, about getting to grips with the lived experience of colonialism or, as he put it, of 'colonial social orders of different kinds' (Mitchell, 1987, 312). His spotlight fell on the process and practicalities of social change: the conflicts and alliances inspiring it, the networks and mobilities that channelled it, and the identities, beliefs and behaviours of those least able to control it. He thereby illuminated many substantive themes, often—albeit fortuitously—challenging the intellectual status quo.<sup>34</sup>

Initially, like his peers, Mitchell worked in a rural setting and was engaged in an intense study of a single 'tribe' or people.<sup>35</sup> His main substantive interest, after an

<sup>30</sup> Clyde lived for many years with diabetes.

<sup>31</sup> <http://www.socialsciences.manchester.ac.uk/mitchell-centre/about-us/clyde-mitchell/>.

<sup>32</sup> In an exchange of letters with Mitchell about the prospects of him moving to Manchester, Max Gluckman referred to the mix of 'intellectual quality' and 'sociological insight' that positioned Mitchell as 'the best anthropologist of the new generation' (14.7.1959).

<sup>33</sup> The details of Clyde's work on labour migration merit more space than this memoir allows; his early paper (Mitchell, 1959) on that theme has been described as 'a classic contribution to which all subsequent scholars of African migration have been indebted' (Cohen, 1990, 609).

<sup>34</sup> Mitchell was not drawn to scholarly combat; quite the opposite, as he wrote to Max Gluckman in the late 1950s: 'one wants to seek happiness and contentment and peace with life and fly in the face of ambition doing it' (letter to MG 7.7.1959).

<sup>35</sup> In the third printing of his first book, Mitchell substituted the appellation 'Malawian People' for 'Nyasaland Tribe' in the subtitle both to reflect the creation of the independent state of Malawi in 1964 and to accommodate the fact that the label 'tribe' had taken on 'social and political connotations I did not imply when the book was first published' (Mitchell, 1956a: 1971 reprint, x). In this vein I have also,

early fascination with kinship<sup>36</sup> and a tactical eschewal of witchcraft, was in social stratification.<sup>37</sup> He regarded the Yao Villages that were the subject of his doctoral project as internally differentiated material and social structures; concentrations of huts occupied by people ‘who recognise their social identity against other groups’ (p. 3). He located each such assemblage as ‘a unit in a larger field of political relations’ (p. 2), shaped by the imposition of a colonial administration. Amid the tension between these scales or orders he identifies myriad struggles for recognition. He documents, for example, the changing character of the ‘Chiefdoms’, the jostling for position (or rank) of Headmen, the cross-cutting effects of kinship and clanship, and the complex confrontations of matrilinearity, uxurilocality<sup>38</sup> and patriarchy occasioned by the catastrophe that colonial rule (and some earlier dislocations) had inflicted on Malawian rural life.

In this way, *The Yao Village* posed a challenge to grand theories of the exploitation of labour by capital; not because such exploitation was of marginal interest, but because, for Mitchell, demonstrably in the empirical world so much more was implied by, and required to appreciate, the impacts of colonialism. This drew him not only to scrutinise the social trappings of production and consumption but also to embrace the struggle to control symbolic as well as material rewards. Even viewed from the rural edge of what Mitchell would eventually cast as a process of urban change, the messiness of the real world drew him to regard prestige or status as a crucial modality through which the contradictions of colonialism were lived.

In this, he nodded towards Max Weber,<sup>39</sup> for whom the acquisition of status has to do with struggles over life chances and resources that are occasioned or mediated by ‘a positive or negative social estimation of honour’ (Weber, 1968, 187). The idea of status fascinated Mitchell because it enabled him to engage with the heterogeneity of African societies, attending to the complexities of a political-economy held together by *multiple* oppositions and confrontations, including a ‘competition for various symbols of prestige’ (Mitchell, 1956a, 76). This possibility that the social and political structures of colonial societies were powerfully expressed through status is the Ariadne’s thread linking Mitchell’s DPhil thesis to his wider lexicon. It runs from an early much laboured-over paper on occupational prestige in the late 1950s (Mitchell

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except in direct quotations, tended to use alternative labels—village, ethnic, kin-group and so on—for Mitchell’s references to subjectively understood communities of descent in publications from that time.

<sup>36</sup> A topic that Max Gluckman persuaded him to set aside in the late 1940s.

<sup>37</sup> See, for example, his letter to Epstein on 15.1.1956.

<sup>38</sup> Systems in which a married couple resides with or near the wife’s parents.

<sup>39</sup> Clyde never cited Weber’s work, though he talked about his ideas and was interested in Schutz’s elaboration of them (as well as Simmel’s input to them); the point here is that Mitchell was aligned with a body of thought that recognised the importance of a variety of subjectivities as the basis or framework for action.

and Epstein, 1959) to his final full paper, which in the mid-1990s revisited the ordering of a wider range of symbolic markers (Mitchell, 1994).

There is, however, a second game-changing thesis embedded in the Malawi study. Already, in this, Mitchell was adopting what Norman Long might term a ‘processional view of village politics’ to explore the encounter of traditional and modern forms of political authority under colonialism. This encounter, to Mitchell’s eye, testified not just to the resilience of traditional patterns of social life (itself a radical idea, in the face of colonial rule) but also to their vitality—to their creative adjustment to external shocks and influences.<sup>40</sup> The study thus offered important early insights into the truism that people are never bound into fixed categories but are linked into boundary-crossing networks that can draw them in different directions. In his 1959 inaugural lecture as Professor of African Studies at the University College of Rhodesia and Nyasaland in Salisbury, Mitchell positioned this manoeuvrability as key to social change under colonialism (Mitchell, 1960, 30). He gave two examples of what that key could unlock, both highlighting the actancy of African people caught in the contradictions of colonialism.

First, he tells the story of an individual (William) who to all intents and purposes had long abandoned traditionalism in favour of the trappings—diet, dress, habits and appearance, at home and at work—of a Western-oriented townsman. Professionally, however, William was a *ng’anda*—a traditional medical practitioner whose success depended not on the rational application of scientific principles (though he was not averse to invoking these when required) but on the veracity of magic. Magic featured in his printed brochures, worked for his regular patients, and won him office in a professional association of *ng’andas*. In short, he readily operated in a setting in which ‘quite disparate systems of belief may co-exist and be called into action in different social situations’ (Mitchell, 1960, 19). William was inventive, and in that sense powerful, eliding the traditional with the modern to define his professional niche.

A second example picks up on a foundational interest of the RLI—the labour migrations required to support the Northern Rhodesian copper mines. This was the project of founding director Godfrey Wilson, who struggled with authority and bureaucracy to pursue it (Morrow, 2016, chapter 8). For many years, labour negotiations were funnelled through a system of consultation with village elders. This broke down over time, in favour of more conventional systems of wage bargaining. Wilson interpreted this as an aspect of ‘detribalisation’ and lamented the loss of cultural

<sup>40</sup>The novelty of this is signalled in a contemporary review by Mary Douglas (1957); in later years (the early 1970s) she went further, expressing considerable admiration for Mitchell’s work and explicitly attributing the processual turn in the study of African local politics to *The Yao Village* (Richard Fardon, commenting on a lecture he attended in 1972–3, pers. comm. 31.7.2018).



heritage it represented. Mitchell, however, argued that the shift was more complex, and was not entirely (or even) about the proletarianisation of Africans who had severed their rural ties. In fact, Mitchell's account shows that the mineworkers were able actively to embrace modernity as industrial employees without sacrificing more traditional (ethnic) relationships and (village-specific) orientations in other areas of their lives. That is, two seemingly intermeshed spheres—one rooted in rural life, the other a route to urban industrialism—flourished without either precluding, much less subsuming, the other.

These examples offer one illustration of how traditional identities dovetailed with the world of work through a period of political and economic upheaval. Both question the idea that the colonial order inspired a steady process of 'detrribalisation', and in this they enlarge on the findings of Mitchell's earlier more widely cited study of the Kalela Dance (Mitchell, 1956b). This foundational piece is, however, the touchstone for myriad other innovative theses.

In an era when *urban* anthropology was at best in its infancy, at worst marginal to the discipline, the Kalela Dance presaged the comprehensive urban research programme that the RLI embraced under Mitchell's directorship. That programme drew attention to the lived experience of crisis and change 'at a time in the history of anthropology when such topics were more an afterthought than the major focus of interest' (Kapferer, 2006, 86). It also recognised, from the start, that there was little value in essentialising towns or cities; that the focus should not be on what Mitchell (1966, 44) called 'historic change' as labour migrants adapt their behaviours to immutable urban institutions, but rather on 'situational change' effected through urban encounter as 'new institutions and patterns develop out of old'. *The Kalela Dance* also established Mitchell as a pioneer in the anthropology of ethnicity—a term he favoured as time went on to refer to people's affinity with (in Max Weber's terms) a 'subjectively believed community of descent' (Weber, 1968, 309).

Although, as noted earlier, Mitchell was indebted to Weber for his interest in the status order, he had a different view about how ethnicity—as a particular realisation of the distribution of power—fitted into this. While Weber regarded ethnicity as a very specific, somewhat irrational and probably transient, element of the status order, Mitchell positioned it more centrally. He recognised it to be as powerful and enduring a force as occupational prestige, and a crucial mediator of social relations in public life. In this, he was influenced by the Urban Sociology of post-depression USA, which set the scene for a round of empirical urban research sensitive to markers of difference other than class (Hannerz, 1980). In the US context these cleavages were generally labelled race or ethnicity, the first applying to African-Americans, the second to European immigrants. Both were obstinate in the face of expectations around integration and assimilation. This resilience might have shaped Clyde's thinking as he

approached the Kalela Dance, but by the time the work was complete, he had laid the foundations of a much more radical theory of society.

Mitchell used the Kalela dance—enacted before a crowd of onlookers by Bisa labour migrants in the suburbs of Luanshya—as a lens through which to view the structure of social relationships (the ‘whole social fabric’) among Africans on the Copperbelt. As an exercise in case and situation analysis, a first important observation was that its location, its segregated setting, and other features of its performance—that it was limited to Sundays and holidays in the absence of European officials, drew large crowds because the drums reached easily across a swathe of densely packed municipal housing, and so on—testified to the way *all* social relations in the Copperbelt were powerfully framed by colonialism; by ‘the general system of Black-White relationships in Northern Rhodesia’ (Mitchell, 1956b, 1).

Drawn to the Kalela partly because of this, Mitchell found, nevertheless, that the dance was not primarily about those oppositions; indeed it did not directly reference them at all. That social life on the Copperbelt would be enmeshed by colonial regulation (laws enacted to circumscribe the lives of town-dwellers) and industrial transformation (economic imperatives that unsettled traditional social ties) was, both for Mitchell and for those performing the Kalela Dance, axiomatic. Dancers and author alike might have been enmeshed in the ‘Othering’ that a binary opposition between ‘Western’ and ‘African’ implies, but the Kalela Dance was, in practice and in performance, about very much more.

Mitchell’s starting point, in fact, is a paradox: the Kalela Dance—by far the most popular traditional dance enacted across the Copperbelt—was ostensibly about ethnic distinction (Bisa identity), yet it eschewed all the trappings of a conventional ‘tribal’ dance. To be sure, it gathered dancers—primarily young unskilled male workers—from a variety of regional backgrounds, but they did not disrupt the world of colonial urbanism with exotic costumery, flamboyant gesture or rural reference. On the contrary, the team of nineteen youths was dressed in ‘well-pressed grey slacks, neat singlets and well-polished shoes’, and mounted a performance that was surprisingly unobtrusive, ‘almost prosaic’. It took the form of a shuffle rather than a display of athleticism or authenticity, made no reference to traditional themes, was far from ostentatious and included none of the key roles (e.g. village headman, or elders) that a Bisa village might contain. Rather it revolved around a committee with a chairman, secretaries, treasurers and other officials, who ‘conduct their business on the same lines as any European association does’. It seemed wholly ‘ethnic’ in spirit yet was strikingly ‘modern’ in look and feel. For Mitchell, therefore, the defining qualities of the Kalela were ‘drawn from an urban existence’—an act of self-definition that was in and of a space that many believed to be structured specifically to submerge traditional identities rooted in villages, kin and clan.

Eager to account for this and attentive to every nuance of lived experience, Mitchell turned to the songs. Sung in Bemba using an urban lingua franca that would have been lost on an outsider, they were full of witty, topical verses ‘composed in towns for the amusement of people in towns’. In performance the songs served primarily to define and underline the unity of one ethnic group, the Bisa, and set it against a heterogeneity of others, but there was nothing to suggest that village ties and the identities rooted in them were homogenised, suppressed, dissolved or absorbed by the transformations of colonial-industrial life. Rather key points of commonality and distinction were underlined through the use of Bisa self-praise (a method of identification), and by the construction of stereotypes to depict and categorise ‘rival’ groups, mainly through the vehicle of ridicule. In fact, the whole dance, as Mitchell recognised, could be cast as a kind of ‘joking relationship’.<sup>41</sup> It thus testified to the veracity and diversity of ethnic affiliation, yet was a parody of tradition rather than a means of enacting or preserving it.

This, in a sense, is what resolved the paradox. The Kalela was neither about ‘detrribalisation’ nor was it a manifestation of traditionalism *sensu stricto*. It was positioned somewhere between the two. It neither facilitated the assimilation of labour migrants into a colonial urban order, nor transplanted ‘a complete tribal system’ (of kinship, clanship or village membership) into the city. The categories used to organise lived experience—in a setting where the economic order was broadly individualising—could reasonably be described as ethnic or ‘tribal’, but they were not primordial or ‘essential’. In the suburbs of Luanshya, where labour relations were not so structured as around the mines, the message the Kalela delivered was about the *creation of something new*.

As Mitchell put it himself, ‘the set of relationships among a group of tribesmen in their rural home is something very different from the set of relationships among the same group when they are transposed to an urban area’ (Mitchell, 1956b, 44). Labour migrants’ social lives were structured through colonial-industrialism, but with inventiveness and imagination African peoples resisted the fracturing of identity—the anomie or alienation—this might imply. Performing (and witnessing) the Kalela thus offered both incumbents and audiences a way to understand self and categorise others amid the multiple and mutable social orders of the city. In that sense, the dance might reasonably be cast as a political resource, or at least as a point of resistance against the idea that heterogeneity was on the wane and that entire social structures would yield to the values and organisational requirements of colonial industrialism.

<sup>41</sup> A new generation of urban anthropologists would show more explicitly how ‘joking relationships’ masqueraded for (but by no means detracted from) the dynamic interplay of politics with culture (e.g. Cohen 1993).

Certainly Bisa people, like others inhabiting a new urban order, sought to, and did, shape their own way of life even under the conditions of high colonialism that Mitchell positioned at the heart of his analysis.

It was probably insights such as this—and Clyde spent a great deal of time labouring over them—that secured him the award of the Rivers Memorial Medal by the Royal Anthropological Institute in 1960.<sup>42</sup> He continued to develop the ‘performativity’ thesis (though he would never have called it that) across a quarter century or more, recognising the absolute irreducibility of ethnicity to traditional institutional or structural forms (kinship, class and so on) and using his last substantive writings explicitly to conceptualise its emanant qualities, casting ethnicity as ‘not a pervasive element in social relationships but one which emerges in particular social situations’ (Mitchell, 1987, 241).

There are two other themes from the Kalela that Mitchell took up in later years. First, for a work so attentive to process and a thesis so wedded to futures still-to-be-made, the Kalela Dance makes surprisingly frequent reference to what might be called a ‘categorical imperative’. Although Mitchell roundly resisted the formal classification of social structures and their gathering into bounded functional wholes, he did recognise that social life is shot through with alliances and oppositions which encourage social life to settle out into recognisable if mutable shapes. Ethnicity is a case in point, arising out of what Mitchell thought of as ‘the alignments and interests of the actors in specified situations in which [ethnic] cues and signs *take on* meanings and are used to define the stances that actors adopt to one another in that interaction’ (Mitchell, 1987, 241).

In urbanising Africa, for example, Mitchell observed that many lines of cleavage, such as clanship, which might have been important in the villages, were downplayed in favour of visually recognisable characteristics, mainly for pragmatic reasons: ‘a way of simplifying or codifying behaviour in otherwise ‘unstructured’ situations’ (Mitchell, 1966, 53). A whole section of *The Kalela Dance* is devoted to this point. Describing the dilemma facing a stream of labour migrants, Mitchell wrote ‘their own ethnic distinctiveness which they took for granted in the rural areas is immediately thrown into relief by the multiplicity of tribes with whom they are cast into associations. Its importance to them is thus exaggerated and it becomes the basis on which they interact with all strangers.’

Second, while this kind of boundary-building might, through the Kalela Dance, have been played out with jocular nuance, in essence it is a method of confrontation.

<sup>42</sup> Clyde was the 40<sup>th</sup> recipient of the Rivers Medal, which was first awarded in 1924; among anthropologists associated with the RLI he was preceded in winning this honour by Audrey Richards, Monica Wilson, Max Gluckman and John Barnes, and succeeded by Victor Turner and Elizabeth Colson.

Mitchell's analysis of the Kalela Dance is one nudge in a direction—inspired not least by the work of Georg Simmel—that inclined him very distinctly towards a conflict theory of society. He tilted at this in his 1959 inaugural lecture at UCRN observing that consensus exists 'only among those people who happen to be acting jointly in one particular situation, in other circumstances there may well be conflicting valuations and hence dissent among the same people' (Mitchell, 1960, 30). As his ideas developed, he was increasingly conscious of how readily shared cues could turn into divisive markers; that social life was structured through oppositions as well as alliances; and that networking could be as much about enacting difference as building consensus. This set him apart from prevailing (acculturationist, assimilationist and integrationist) views of African urbanisation and positioned the status order that preoccupied him as just one realisation of an irresolvably uneven distribution of power in urban settings shaped by colonial rule.

Finally, it is worth noting that although Mitchell's empirical eye inhabited a space outside categories like capital, labour and class, he readily acknowledged their veracity as structuring principles. He did not write extensively about them, but he did engage with them. He recognised, for example, the complexities of articulating the structures that constrain lived experience (the impact of urban industrialism in a colonial setting, for example) with the agency he observed in the field (the capacity of central African people to shape their lives and futures). He also understood the significance of a disconnect between lay meanings and experiences—ideas that people could articulate and engage with—and the operation of wider, structural, forces which might not be accessible to, or appreciated within, the conduct of everyday life. This is as far in the direction of 'grand theory' that he ventured, but he did take some steps to formalise his position.

In a mid-1970s paper on perceptions of ethnicity and ethnic behaviour, Mitchell (1974, 2) voiced a concern that analysts tend to use ethnicity 'either as a structural category, that is, as a general principle that illuminates the behaviour of persons in specified social situations', or 'as a cultural phenomenon, that is, as a set of attitudes, beliefs and stereotypes that people hold about persons identified by some appropriate 'ethnic' label'. The elision is common today, and Mitchell's worry was that it prevented the relationship between these two sets of ideas being properly conceptualised, understood or acted on (whether by politics, policy, or publics).

Addressing this, Mitchell collated a range of material from the RLI projects to illustrate, graphically and quantitatively (by way of an hierarchical cluster analysis), the four 'levels of abstraction' that he felt could account for the different ways that actors and analysts might conceive of the world. It was a slightly unwieldy formulation,<sup>43</sup> but

<sup>43</sup>It differentiated (broadly speaking) between, first, commonsense perceptions of cues embedded in

his typically painstaking attention to detail does explain why ‘structural’ and ‘interpretative’ approaches to the same data might lead analysts to different, seemingly irreconcilable, conclusions. Later he simplified the argument, identifying two orders of data (Mitchell, 1987, 243): ‘The first relates to the way in which the actors see the situation’ (and thus how they account for and rationalise what they think and do); ‘The second relates to the abstract structural or morphological characteristics of the settings which derive from the theoretical perspectives adopted by the analyst’ (and which the average actor is unlikely routinely to engage with, and may never easily relate to).

There are many ways of unpacking this but the important point is that Mitchell embraced the challenge that the critical theorists were starting to pose, and sought to locate his own work within that frame. The labels ‘first’ and ‘second’ when referring to ‘orders’ of data are, nevertheless, to my eye significant. Whatever else they achieve, these essays surely are Mitchell’s way of signalling that while it is essential to understand how colonialism defined the parameters of African urbanism, analysts could and should also trace out and valorise the agency of subjugated peoples as they struggle to create and shape their lives and futures. That conviction was the heart of Mitchell’s own life’s work.

### The personal and the political *‘a man of fathomless courage’<sup>44</sup>*

Clyde Mitchell was, as noted above, an early recruit to the Rhodes-Livingstone Institute where he spent the first decade of his academic career. The period (the mid-1940s to mid-1950s), the place (‘Rhodesia’), and the name of the institution, all position him at the lip of British imperialism. A left-leaning liberal, Clyde’s politics were lived rather than written, but his work and his life were powerfully sculpted by the struggles of the time.

The idea for the RLI was mooted by the then-Governor of Northern Rhodesia, Hubert Winthrop Young, who, in the wake of a mineworkers’ strike, and with the world economy in recession, recognised that the pace of urban-industrial change in the Copperbelt had outstripped the administration’s ability to handle it. An Institute tasked to secure a systematic, independent, anthropologically informed, understand-

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everyday life, and behaviours associated with these cues, and second, analysts’ constructs of cultural variability (based on common sense perceptions) and analysts’ interpretation of structural differentiation (based on observed behaviours).

<sup>44</sup> In a tribute written for his memorial service, Kapferer (1996) observes that ‘Clyde was a thoroughly tolerant man and a man of fathomless courage’.

ing of Africa and its people was the response. It was viewed with suspicion from the outset by a variety of constituencies, not least for its engagement with social change and its problem-solving brief (Morrow, 2016). Even its name tapped into a more complex mix of local sentiments in the run up to the Rhodes Jubilee and Livingstone Centenary than might be imagined (Schumaker, 2001). In its later years the Institute might, for a time, have become a front for government interests but in the early days it operated with a distinctly radical edge.

Every early Director insisted on the political and commercial independence of the Institute's research agenda;<sup>45</sup> all were resistant to the racially exclusionary appointment policies favoured by its Trustees; without exception they assembled teams that were self-consciously pro-African and, for the most part, they were not afraid to act accordingly. Wilson, for example, resigned when the Trustees tried to limit fieldwork if it involved mixing with, and visiting the homes of, African mineworkers: 'it is said you have been sitting on a box with a native on a chair' (cited in Morrow, 2016, 190).<sup>46</sup> His successor, Max Gluckman, favoured the same anti-racist, anti-colonialist tradition, to the disappointment of the provincial commissioners (Musambachime, 1993). Elizabeth Colson, who followed, sought funds for African researchers, though her tenure was short.<sup>47</sup>

When Mitchell took over, the scene was set fully to involve properly paid and trained African researchers in both quantitative and qualitative research. Mitchell was keen for African scholars to advance and to publish, and arguably his directorship marked 'the first time in the history of anthropology that a large number of indigenous researchers worked together for a lengthy period of time doing studies of their own communities and society' (Shumaker, 2001, 152). That their main involvement initially was in the administration of surveys is an important caveat (Richard Werbner, pers. comm. 31.7.2018). However, Shumaker goes on to trace the life paths and careers of some of these scholars, demonstrating their engagement in the co-production of postcolonial knowledges, and documenting their involvement in renewed debate about tribalism, nationalism, and indigenous identity in Zambia.

Recruitment practices were one element of a more sweeping trajectory. Not only did the RLI stand out in this period as one of the few non-racial institutions in the Federation (that was Shumaker's point, above), but by the early 1950s it had become, and was generally seen to be, 'the most politically critical branch of a discipline otherwise not especially noted for its radicalism' (Kapferer, 2014b, 148). At its zenith,

<sup>45</sup> Partly reflecting their confidence in social *scientific* (impartial, objective) inquiry.

<sup>46</sup> Wilson may equally have resigned because the Trustees objected to his pacifism.

<sup>47</sup> During her short Directorship, Colson was instrumental in relocating the RLI from Livingstone to Lusaka, but left for health reasons in 1951.

under Mitchell (who presided over its most productive period), the RLI had, far from acting as an agent of colonial rule, become thoroughly Africanised both by virtue of the mixed constituencies that shaped its fieldwork and ‘through its adaptation to the landscape of Africa itself and to the material constraints and opportunities it found there’ (Shumaker, 2001, 6-7). Its pragmatic, even anti-intellectual, edge was widely recognised and reflected in the pride its male members took in being dubbed ‘the cloth cap boys’ of academia (Epstein, in Yelvington, 1997).

Mitchell did not greatly enjoy the Directorship, shot through, as it was, with politics and bureaucracy. His tenure was correspondingly short. ‘It is impossible’ he wrote to Barnes ‘to describe the inanities I have to commit as Director of this show’ (22.11.1952); and two years later: ‘Life out here is even more bloody than usual ... it is a long and dismal tale of an uphill struggle against obscurantism’ (6.1.1954). He was particularly candid in his correspondence with Max Gluckman, with whom he shared the frustration and the stress of ‘running a liberal research institute in an illiberal atmosphere’ (JCM to MG, 9.2.1955). By June that year (1955), Mitchell was ready to leave. He and Gluckman were increasingly disaffected with the Institute, its governance and its productivity as an academic centre. Before long, their doubts about its direction, independence and critical edge were leading them to sever all remaining ties. ‘The R.L.I. as we knew it’, wrote Mitchell (17.6.57) ‘no longer exists’; ‘it seems quite clear to me’ replied Gluckman two months later, ‘that the RLI is going to become an adjunct to government’ (8.8.1957). Deliberating at what point her own account of the work of the RLI should stop Schumaker (2001, 227) summed it up: ‘One could end with Mitchell’s resignation and use that endpoint to stress that academically minded anthropologists no longer controlled the Institute and its research agenda.’<sup>48</sup>

Political frustration did not, of course, ease with Mitchell’s move to Salisbury (Harare) and the Chair at UCRN, where he was initially happy, maintaining the critical, problem-oriented spirit of the RLI despite duties that prevented him working on the Copperbelt materials.<sup>49</sup> One such duty was teaching, which was not his first love.<sup>50</sup> At the time, however, it was proving increasingly difficult for Africans to obtain higher

<sup>48</sup> Shumaker did not, in the end, conclude her analysis with Mitchell’s move to Salisbury, partly because she felt his influence endured, partly because she was interested in the ongoing role of the RLI as a field centre, and mainly because she wanted to recognize the first appointment of an African director, Philip Nsugbe, in 1968.

<sup>49</sup> Werbner (1984, 161) identifies this emphasis on ‘relevance of the problems to the people themselves’ as a dominant strand of the Manchester School, writing in the preface to his own contribution that ‘Clyde Mitchell, my fieldwork supervisor at the University College of Rhodesia and Nysaland, did his best to get me to appreciate the impact of state intervention on the people’s lives—he urged me to study current social problems’ (Werbner, 1991, vi).

<sup>50</sup> Though, ironically, he was always sought after and valued for his teaching, especially by his research students.



education, and Clyde saw the opportunity to change this by creating a learning environment that taught African Studies without objectifying African people. As he notes in interview with Russell Bernard, with the help of grants from the Ford Foundation he transformed a dominantly white degree programme into one with more balanced (50/50) participation.<sup>51</sup>

As an academic administrator his work was, nevertheless, increasingly compromised. Although he successfully attracted scholars like Kingsley Garbett and Jaap van Velsen (who extended the radical implications of some of his work) to join him as colleagues, together with PhD students such as Bruce Kapferer, David Boswell, Peter Harries-Jones and Richard Werbner—all resistant to the principle of white rule—his attempts to create opportunities for African scholars were often thwarted<sup>52</sup> and his bid to employ A. L. (Bill) Epstein was blocked.<sup>53</sup> When Epstein wrote on 10 February 1956 declaring that anyway he felt himself shrinking increasingly away from the field situation, Mitchell replied (28.2.56) ‘I appreciate very keenly, of course, your concerns about staying in Africa. Who with any conscience has not had them?’ These concerns quickly increased, and as the 1960s gathered pace the pressures became intolerable.

Among Mitchell’s papers in the Bodleian library is a news clipping from the *Sunday Mail* of 13 September 1964. The headline is ‘Smith’s great gamble’.<sup>54</sup> It rules out ‘one man, one vote’ and promises instead to consult ‘people who have made a lifetime study of African custom and African law’ to find out how best to take into account the views of the African people. This explicit attempt by Ian Smith to hijack professional anthropology to legitimise his decision to disenfranchise black Africans was the last straw for Mitchell who (as he later wrote in response to a 1975 inquiry from UNESCO), as the senior anthropologist in the country at that time, could categorically state that: ‘Mr. Smith had not consulted me or any of my colleagues’ (letter to Mrs. O’Callaghan 2.5.1975). Within a week, Mitchell had assembled a group of scholars to argue publicly for the democratic rights of Africans. Their position, set out

<sup>51</sup> He took a number of initiatives in this respect, including sending a life changing letter to Gordon Chavunduka who, after working with Mitchell as a sociologist in Salisbury/Harare, studied for two degrees (at UCLA and Manchester University) and later became Vice Chancellor of the University of Zimbabwe (<http://www.colonialrelic.com/biographies/dr-gordon-chavunduka/>, accessed 1.8.2018).

<sup>52</sup> In a personal communication (31.7.2018) Richard Werbner has described how a potential African research assistant had to run for his life in the night, after leading an anti-government protest; Werbner himself later became a prohibited immigrant under the Smith regime.

<sup>53</sup> Epstein had tried to study the social and organisational life of migrant African labourers in the towns of the Copperbelt and was cast as pro-Union and subversive: he had, as Rew (1999) writes in his obituary, ‘dared to act as an anthropologist in towns rather than in a rural location’.

<sup>54</sup> This refers to Ian Smith’s bid for Rhodesian independence to prevent the transition from colonial rule, and a shift to full enfranchisement.

on page 1 of the 22 September 1964 edition of the *Guardian* was that ‘No other method [than the right to vote] can give valid results’. ‘We are’, the group is quoted as saying, ‘utterly opposed to the idea that there is something peculiar to Africans that makes it impossible to test their opinions by normal procedures.’ For this they were roundly attacked by the government, with several signatories to the *Guardian* letter (including Mitchell’s second wife, Hilary Flegg-Mitchell) apparently banned from re-entering the country.

Undeterred, Mitchell kept a close eye on the censoring of academic freedom, and in 1965 wrote another letter to the *Rhodesia Herald* complaining of ministerial interference—in this case the removal of items from a reading list at the Teachers Training College. Elsewhere among Mitchell’s papers—files he must have kept for years—is a list of Council members for the University College of Rhodesia and Nyasaland dated 30 March 1965; he has written at the bottom ‘No African member of College Council since break-up of federation (1963)’. Speaking to Russell Barnard (1990) he tells of his growing outrage as the government populated the lay memberships of University committees with its own sympathisers, and of his fears for the future. In his initial response to the UN inquiry mentioned above, he describes this process as spanning the entire period 1957-65 in which ‘there was a slow infiltration of people with Rhodesian Front sympathies into lay positions on the various administrative councils and they began to make their influence felt in many ways particularly in respect of the political activities of members of staff and students of the College’ (letter to Mrs O’Callaghan, 14.3.1975).

In early December 1965, shortly after the declaration of UDI, Mitchell wrote to the Principal of the University setting out the principles of operation of an independent University. Independence rests, he argued, ‘squarely on the freedom of the scholars who constitute it—staff and students alike—to be able to criticise current social, economic, political, religious, philosophical, ethical, scientific or any other type of thought’. This was impossible, he felt, ‘where government is exercised by a minority whose justification to govern rests upon ideologies which cannot withstand the cold analysis of trained minds’. He then lists the ways in which the University could respond to maintain its independence. It is a lengthy, thoughtful and constructive letter; the reply from the then-principal is a single, platitudinous line. On 21 December 1965, Mitchell left Africa for the UK.

There is a scattering of materials in Mitchell’s papers testifying to the extent to which he remained engaged in post-UDI politics, including letters from 1967 that suggest he had raised funds to meet the defence costs of African political detainees and prisoners. He was so steeped in all this that when in the mid-1970s he was approached by a UNESCO enquiry into how social scientists like him had seen their work in Rhodesia, it was only in the follow up correspondence that he was forced to

make the blindingly obvious statement that underpinned his entire life's work: 'The major impact on African social structure was undoubtedly their military conquest by Europeans' (to Mrs O'Callaghan, 2.5.1975). The fact that he regarded this as self-evident—as the catastrophe at the heart of every anthropological inquiry or social study he engaged in—was not always appreciated.

For two decades, Mitchell's work had been located at the centre of a 'perfect (political) storm' whipped up by colonialism, dispossession, racism and segregationism. He saw these forces as central to the lived experience of everyday life, and he resisted such oppressions with every fibre of his being. He was devastated, therefore, when both his role and his work were roundly criticised by Bernard Magubane (1969, 1971), who cast anthropology in Africa as a handmaiden of colonialism. Mitchell's and Epstein's ethnographic work came under particular scrutiny, first for taking the colonial system for granted (i.e. assuming that its general characteristics were known) and second for dwelling on the trivial materialities of dance and fashion (symbols of 'acculturation') rather than on the fundamentals of oppression and inequality. More generally, the Copperbelt anthropologists were charged with ethnocentrism, bias and 'a pragmatic propagandisation of certain ideals in the guise of sociological analysis' (Magubane, 1971, 430).

There followed robust debate (with all positions represented in the twelve replies to the 1971 critique published by *Current Anthropology* (volume 12 (4)).<sup>55</sup> It was not resolved then (though it was much-debated by subsequent RLI directors) and is unlikely to be concluded now. In part, it was about a much wider post-colonial critique of traditional anthropology, which was quickly politicised and soon embraced geography, sociology and more. In part it reflects an enduring intellectual struggle between grand theory and small stories, between structure, agency and more. Either way it informed a new generation described by Shumaker (2001, 230) as 'largely non-African anthropologists and historians' who were involved 'in a territorial move for the displacement of British social anthropology from the African field'.

Whatever the wider ramifications of this exchange,<sup>56</sup> as a specifically political debate with Mitchell, Magubane's intervention, though thoroughly distressing, and unsettling (notwithstanding the fact that Clyde was well aware of his awkward

<sup>55</sup> Mitchell was not enamoured by the cut and thrust of the academic mainstream. Explaining in a letter to Max Gluckman (7.7.1959) why he could not move to Manchester at that time he wrote 'Heaven knows, I am insecure enough as it is in intellectual circles—to face up to hostility (as I am absolutely certain I shall have to) would knock me up completely.' Rightly or wrongly, he experienced Magubane's critique as a personal attack, all the more upsetting because his demonstrably critical opposition to the regime was not recognised (Kapferer, pers. comm. 24.7.2018).

<sup>56</sup> It is certainly the case, as Werbner (1984, 159) observed, that, for a variety of reasons, during the 1970s 'the great stream of fieldwork and fieldworkers, primarily in Zambia, dried up to a trickle, as did the stream of monographs'.

positionality), was less than convincing even in its own terms. A central concern that ‘the colonial social order worked to limit every aspect of African life’ (Magubane, 1971, 420) is one that Mitchell clearly shared; and the argument that more should have been done explicitly to conceptualise, document and resist that system will always be true. Mitchell was mortified by Magubane’s tendency to leap from reasonable critique (e.g. ‘there is no image of the colonial social structure’) to sweeping conclusions (e.g. that implicitly, therefore, those anthropologists believed ‘in the rightness of the white conquest of the African’). As a scholar, moreover, Mitchell could not agree that studies of ethnicity and social status could be reduced to the analysis of class.<sup>57</sup> He was concerned that Magubane misread his use of ‘tribalism’, criticising him for arguing that tribal values from rural areas were relevant in town when Clyde was suggesting the opposite, namely that tribalism in towns was, as Kapferer puts it, ‘a radical construction within modernity’ (pers. comm. 24.7.2018). Finally, Mitchell never accepted that ‘taking for granted’ the extent to which African urbanisation in the post war years was shaped by a colonial order—that is, actively positioning colonialism as the context or setting for a series of studies—amounted to condoning or supporting that regime.

Ironically, Mitchell himself felt that, during his time in Africa, his role as an anthropologist was justifiably viewed with suspicion—not because he was courting government, but because of the challenge his team posed to the regime by virtue of the fact that ‘we were all supporting the blacks against the whites’ (interview with Russell Bernard, 1990). To be sure, some—a lot—of his written work might reasonably be construed as apolitical, reflecting his interest in the scientific credibility of social research, and his attentiveness to small-scale social processes. But taken together, and as works of their time, his research and publications are distinctive for the extent to which they recognise and harness the agency of African subjects. As Schumaker (2001, 7) puts it: ‘As terrible in its consequences as colonialism was ... [in this programme of research] colonial actors never exercised complete domination and colonial subjects never behaved solely as passive victims.’

On balance, it is hard to see how a close reading of Mitchell’s lexicon as a whole, much less an assessment of his life ‘in the round’, could cast him or his scholarship as in any way uncritical of colonialism. Indeed, many regard his contribution as explicitly anti-colonial (see Brown, 1973; Hannerz, 1980, 157–62; Shumaker, 2001, 239–41). The cautionary tale this inspires is elegantly recounted by Jeffrey Prager (1982, 99). Charting the course of racisms and oppressions through time, Prager notes that what

<sup>57</sup> Nor did he alter a view expressed in his inaugural lecture at UCRN that anthropological teaching and intervention were important because ‘it is a subject which deals in abstract terms with custom and belief, and these are perhaps the first aspects of African life to understand’ (Mitchell, 1960, 6).

is radical and progressive is itself situated in time and place. To look back and recognise yesterday's mistake is not always to assume a position of strength tomorrow. Each generation, observes Prager, is inclined to label the work of their predecessors with phrases like 'Theirs was a racist reaction; ours enlightened'. Mitchell—undoubtedly radical, for his time enlightened, and always a modest, generous scholar—entirely avoided that conceit.<sup>58</sup>

### *Acknowledgements*

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I am indebted to the Bodleian Library for help in accessing relevant collections and in particular to Senior Archivist, Lucy McCann. Any errors or oversights are, of course, my own. I hope, given Clyde's own struggle to write up, that he would not be too alarmed—and might even be amused, in a frustrated kind of way—that this

<sup>58</sup> Many of his students will have notes and letters like those he wrote to Epstein (29.1.1951), about a lecture he is giving in Kitwe 'I have suggested that you might attend—if you want some light amusement'.

memoir is published so long after his death, and so long after even I (not the first person to take it on) had aimed to deliver it. As one of his many research students, I knew him well for a while, yet I am not sure that I ever properly thanked him for the extraordinary lengths he went to in order to support my career. Above all, I am grateful that Clyde shared with me, as he did with countless others, what his own friend and mentor Max Gluckman describes as ‘a quality of imaginative adventurousness’ (letter to JCM, 15.1.1951) that is undiminished by the passage of time.

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