

Transcending the academic/public divide in the transmission of theory: Raglan, diffusionism,
and mid-century anthropology

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‘With the adoption of . . . functionalist anthropology an almost complete rupture was
created. . . The theoretical framework . . . of the diffusionists was dead’

(Barth 2005: 28).

‘After Malinowski, there was no more of the old armchair anthropology in British anthropology. It really was like turning a switch. That was the end of it in Britain’

(Kuper 2006: min. 48).

Decades ago Stocking demonstrated that the conventional history of anthropology was Whiggish, scripted to a narrative of theoretical progress. According to him, the Whigs portrayed anthropological development as a process of supposed improvement, where the successful proponents of new theoretical approaches pushed aside those till-then dominant (Stocking 1987). While Stocking and others following him showed in a usually convincing manner that the past of anthropology was best studied in historicist rather than presentist terms (Stocking 1968: ch.1; Trautmann 1992), what they did not attend to was the continued existence of purportedly vanquished theories.

The validity and significance of this last point has recently become ever clearer for several reasons. First, today, an ever-growing proportion of anthropologists with doctorates do not get lectureships and start to ascend the university hierarchy, but choose instead to practise anthropology along career paths outside the academy: e.g. in public service, NGOs, or commercial organisations (MacClancy 2017). Second, associated with this shift is a rising move within Academe towards a more ‘critical’, ‘public’, ‘collaborative’, or ‘engaged’ anthropology: i.e., the reintroduction of our subject to debates and topics broader than

those which only interest colleagues (e.g., Marcus 1998; Eriksen 2006; Borofsky 2011). The common concern to these variously named initiatives is to exploit the potential power of anthropology to inform issues of the day in a knowledgeable, analytical, revelatory manner; at the same time, they vaunt the promise of edging our fieldwork towards moral equity, grounded on greater participation between the parties involved in a research project. Third, these parallel processes—the occupational shift in the anthropological workplace and the ‘turn to the public’—have led to an increasing recognition that anthropology may be profitably practised not just by academics, but by amateurs and others who do not hold tenured positions as well, and that the non-academics’ contribution to a broadly conceived anthropology may be wide and multifarious. If these present-day developments have led to a re-envisioned future for our subject, they also enable a revised past for it, one more open and plural than the singular versions propounded by the institutionally powerful (MacClancy 2013).

A similar argument can be made within Stocking’s own terms, for he characterised anthropology as the ‘boundless discipline’ (Stocking 1995). Haddon had made the same point in the early 1900s, noting ‘This lack of definiteness adds a charm to the subject and is fertile in the production of new ideas’ (Haddon 1903: 11). Sixty years later Wolf called anthropology ‘a discipline between disciplines’; it can also be viewed as an evolving assortment of activities coasting among disciplines (Wolf 1964; MacClancy 2013: 3). A scholarly chronicle of anthropological practice is thus not one of steady development, or even salutatory evolution, but of constant dispute where rival camps seek to persuade others (colleagues, students, funders, the public) of the value of their distinctive approach. These intellectual propounders work to exploit anthropology’s vagueness, by seeking to fill

it with the content or style they deem most promising. In this account, the historical course of anthropology should be viewed, not as a simple-minded scholarly progress over time, rather a stormy muddling through a learned terrain whose terms, divisions, and destination change as we seek to move on (MacClancy 2013: 4). In other words, I wish to argue against the crudely dichotomous, overly academic view perpetuated by the anthropologists in my epigraphs.

In this paper, I contribute to the redressing of the imbalance created by Stocking and his acolytes, and to exemplify the longevity of purportedly moribund theory, by opening up to a plural conception of anthropology, where the extra-mural or public is not ignored or prematurely denigrated for the sake of academic trends of the moment. Rather than precipitately confine study to debates by academic anthropological participants, I wish to appreciate the popular as part of the life-course of anthropology, especially when some popularly presented ideas disparaged in their time by the academically mainstream have since been re-evaluated, positively. Mass Observation, for instance, presented itself as popular anthropology, i.e. anthropology of the people, for the people, by the people. Though it was strongly criticized by anthropologists of its day, it has since become a respected and established part of British academia, utilised by, among others, social anthropologists (MacClancy 1995).ⁱ In this paper, I thus strive to give equal weight to untenured anthropologists who also weighed in or opined publicly as much as to their academic counterparts. I emphasize that I am not trying to re-centre the previously marginalised for the jejune sake of arcane historiographical end, a near-obsessive filling out of a footnote or two in the conventional chronicle of anthropology. Instead, as I have already argued for other popular anthropologies (MacClancy 2013), I hope that re-

evaluation of slighted protagonists or theories may nudge us to reconsider the contexts and nature of anthropology more broadly.

My ethnographic example is the mid-century British anthropologist, the 4th Baron Raglan, who actively engaged in anthropological debate from the 1930s to the 1960s. The last of the gentleman scholars in anthropology Raglan, who never held a university post yet became the President of the Royal Anthropological Institute (RAI) in the 1950s, consistently transcended any academic/popular divide. Examination of his life, besides enriching our understanding of the course of British anthropology during his period, serves to underline that research into the interface between the popular and the academic may uncover a somewhat different, much more extensive version of anthropology extant in universities than the narrowly focussed one promoted by those who would be hegemonic.

Raglan, an anthropologist of quixotic profundity, has been disregarded because he spent much effort promoting with vigour a side-lined paradigm. This interested exclusion shunts out of sight the suggestive contributions he did make. It is true he was an unrepentant diffusionist in times of structural-functionalist hegemony, but there is more to the man than that, as this paper demonstrates.

I do not wish to be misunderstood. I do not here argue that an overly speculative hyperdiffusionism is of intrinsic interest. Nor do I question that the empirically-grounded monographs of the structural-functionalists are justly famed for the ethnographic riches and unexpected insights they provided. What I do argue is that a dismissive, general crabbing of diffusionism for reasons which include the hegemonic threatens to occlude the strengths of a modulated diffusionism, particularly its concern with cultural transmission, and to pass over the blindspots of orthodox structural-functionalism, especially its marginalizing of

historical factors and often rigid concept of culture. Acknowledging the intellectual strengths of the victors should not entail ignoring those of their opponents.

The structure of my argument follows a four-step logic, with each step cumulative. First, I sketch Raglan's life and characterise his general anthropological approach. Second, I analyse his popularity and its flip-side: assessment of his work, particularly by academics. Third, I consider the intellectual legacy of Raglan and other diffusionists, and argue for the remarkably un-noticed persistence of diffusionist styles in a broadly conceived anthropology. Fourth, consideration of the exaggerated rivalry between diffusionists and structural-functionalists leads to general remarks about the life of theory and the historiography of anthropology.

A noble life

Fitzroy Richard Somerset, born in 1885, was educated at Eton and Sandhurst. From 1913 he acted as District Commissioner in southern Sudan, where he worked hard on the language of the local Lotuko. In 1919 he transferred to Transjordan, with the brief of keeping the peace in volatile districts. On his father's death in 1922, he resigned from the Army and established himself at the family seat in Monmouth.

According to his younger son, Raglan had 'a terrific sense of duty' (G.Somerset, pers. comm. 11 x 2014). Once a lord, he became highly active in local affairs, and held a long series of public posts. His letters give a flavour of his daily routine: a full schedule of agricultural tasks, public addresses, official entertaining, scholarly gatherings, committee meetings, and shooting parties.

Beyond Monmouth, Raglan was popularly known as an anthropologist and witty polemicist. An accomplished linguist, he had first learnt Arabic in the Sudan, then 'set to work to learn all I could of the local language and customs'. In Transjordan, he 'visited a great number of villages and as I spoke Arabic fluently and always spent the night in the sheikh's guest-room, I got to know the country and people pretty well'. When Charles Seligman, professor of ethnology at the LSE, saw an article by Raglan in *Sudanese notes and records*, he encouraged him to join the RAI. 'And when I had married and settled down at my ancestral home, I put myself through a course of anthropological literature' (Raglan 1959: 1-2). Within ten years he was elected President of Section H (Anthropology) of the British Association for the Advancement of Science. After the war he became President of the Folklore Society (1945-47), and then of the RAI (1955-58).

Raglan remained remarkably active in anthropology throughout his post-military life: in his final eighteen months, when he was approaching 80, he broadcast a piece on 'The origin of folklore' for the BBC, wrote the cover story for the Xmas issue of *New Society*, the leading weekly in popular social science, and saw his last book through to publication (Raglan 1963; 1964).

Sharp thought, diffuse patterns

Raglan's theoretical approach was a variable blend of ritualism, diffusionism, and neo-Frazerianism, combined with a remarkably developed sense, for his time, of the structuring power of the symbolic. Like the hyperdiffusionism then espoused by Grafton Elliot Smith and William Perry at University College London, Raglan believed that civilization arose, benefitting all humankind, in one specific place and time. Unlike them, he did not indulge in Egyptocentricity, but claimed Sumeria c.4000 BP was the fount of today's developed world. A further difference was Raglan's relative lack of interest in the geographic details of cultural transmission. He was usually far more concerned with varieties of social organisation, especially its cosmological dimensions. For him, the most acceptable mode for explaining social phenomena was the social, supplemented by a speculative historicism.

In his first book *Jocasta's crime* (Raglan 1933a), Raglan asked why Jocasta, unwittingly made pregnant by her son Oedipus, fears the cosmic, not the congenital consequences of her action: she is not worried by inbreeding, but the plague she has brought on Thebes. Why, he asks, should a personal transgression lead to general disaster? Sweeping aside arguments appealing to the role of instinct or analogy from animal behaviour, Raglan notes the near-universality of the incest taboo, magical prohibitions, and exogamy among 'savage societies'. He thus argues the latter two legitimate the present-day upholding of the taboo, while its historical justification is provided by creation myths,

themselves based on creation rituals, many of which have similar internal structures. Finally, he imagines a primal scene of his own, whose pattern and logic were diffused around the globe.

The core of his second, and most successful anthropological book, *The hero*, is a comparative survey of European legends and tales of exemplars. Contrary to the widespread assumption that old stories are 'historical unless . . . proved fictitious', he argues an opposite thesis. On the basis of his survey, he elucidates twenty-two motifs typical of heroic figures, then applies his list to twenty-one heroic lives, from Classical, Old Testament, Javanese, Nilotic, Nordic, Celtic, and English folkloric sources. In each case the chosen character exhibits a majority of the motifs (Raglan 1936). In other words, the hero is a polythetic concept, i.e. a broad set of criteria, where no particular criterion is necessary or sufficient; any example of the concept must simply manifest a certain minimal number of the criteria (Needham 1975). For Raglan, the important consequence is that heroic tales he should be regarded as above all structured according to a loose but common narrative: this tells more about modes of thought than historical realities (Raglan 1936g). The corollary is that all these heroes are mythical. Finally he argues it is probable that these traditional narratives derive from ritual drama.

In *How came civilization?* (Raglan 1939), he examines the archaeological and ethnographic evidence for the geohistorical spread of key items of civilization. Confounding contemporary opinions by exposing their fragile presumptions, he argues the probable ritualistic origin of all his itemised list. Thus, animals were first domesticated, not for meat, but for sacrifice; the first use of the plough was to symbolically fertilise the soil; bows and

arrows symbolically destroyed distant enemies; and so on. The cumulative consequence is that civilization originated in ritual, in Ancient Sumeria.

In *Death and rebirth* (Raglan 1945), Raglan engages in a cross-cultural ethnographic survey, to argue that initiations, coronations, marriages, and funerals are all variants of the same ceremony, as in each the central figure undergoes a symbolic death and rebirth. Here openly neo-Frazerian, he uses his survey material to suggest that many widespread and familiar beliefs about the future life, especially forms of rebirth, derive from a single source: the cult of the Divine King. Again, he considers this religion comes ultimately from prehistoric southwest Asia.

Four years later, in *The origins of religion* (Raglan 1949a), he contends that a religion is an organisation for the performance of symbolic activities, i.e. rites. The object of rites is to secure the life and prosperity of the members of the religion, first in this world, later in the next one. These ritual activities symbolized the unity of the group, and its strength through that unity. Thus religious proselytizing and wars are to be viewed as but attempts to form stronger, more effective groups. He counters Frazerians by arguing that thought proceeds from the concrete to the abstract, not vice versa. Therefore religious innovators were not proto-theologians; they did not win over others by saying, 'This is what I think; it may contain an element of truth' rather, 'Do what I tell you and you will be rewarded'. Raglan thought it 'Time that the practice of speculating about imaginary speculators was abandoned, and that it was realized that religion is not what individuals think, but what groups of people do' (Raglan 1949a: 128).

In his final work *The temple and the house* (Raglan 1964), he argues that houses derive historically from palaces, in turn derived from temples, where the most important rite of early religion was held: the annual marriage of the Earth and the Sky, performed to ensure prosperity and fertility. In the palatial version of this ceremony, the Sky and the Earth are replaced by the monarch and his queen; following Frazer, Raglan argues both figures are understood to be divine, and the king symbolically dies and is reborn in the ritual course. These rites, he suggests, are the legacy of a religious system which appeared in the Ancient East as part of the Neolithic Revolution (Raglan 1964: 195-6).

How to be a popular anthropologist

Raglan worked hard, via a variety of means, to be a successful, popular anthropologist.

First, he wrote a lot. Besides authoring books, he produced anthropological articles, papers and reviewed profusely in a variety of prestigious periodicals, both popular and academic. For decades he was a regular reviewer for *The Times Literary Supplement*; many single issues of *Man*, the monthly RAI journal, include several reviews by him. The list of journals to which he contributed range from the leading popular science journal *Discovery* to *The Monthly Record* of the Ethical Society (Raglan 1934a), from *The Listener* (Raglan 1934b, c) to *The Rationalist* (Raglan 1949b, 1950). When, in 1938, a new current affairs monthly, *Query?*, was launched, contributors to the inaugural issue included Oswald Moseley, H.G.Wells, and Hilaire Belloc, with Raglan providing the lead article

Second, he wrote in a style both scholarly and lively. His measured playfulness did not undercut the seriousness of his arguments. The ludic did not win out over the learned, and he kept to the point. Malinowski and his acolytes tended to write brick-sized tomes dense with the imponderabilia of daily life, their persuasiveness as texts grounded on the weight of detail they provide. In contrast Raglan produced mostly short books (several are only c.45,000 words long), tight of argument, where the level of detail is relatively light and used only to assist the author towards the next step in his sequential contention. Though he makes clear the breadth of his learning, he wished his thoughts to be accessible. For example in *Jocasta's crime*, his ethnographic examples are profuse, his sentences short, his argumentative style relentless. The Major takes no prisoners, his opponents given no quarter. To reproduce his own military metaphors, he fires away at his predecessors' theories,

Firstly, because some of them are too deeply entrenched to be driven out without a regular assault; and secondly, because in levelling their works I shall at the same time be preparing the foundations for what I believe to be a more permanent and defensible fortress (Raglan 1933: 5).

He confesses his own answer to the question is speculative but, 'having criticized (so many others) it would be would be cowardly not to give other people a chance to criticize me' (Raglan 1933: 96). At least, he argues, his answer takes better account of the known ethnography than those of his predecessors.

Publishers were well aware of the power of his prose. His first publisher accepted *Jocasta's crime* immediately:

The book is highly provocative and as most prominent anthropologists in the country are made at one place or another in it to look somewhat ridiculous it ought to have a good circulation in academic circles! Apart from this however, I think both its subject and the humorous way in which you have treated it will attract a somewhat wider circle of readers (Rieu to Raglan, 21 vii 1932, 1, GA).

Indeed, Raglan's literary style was considered so exemplary that an extract from *The hero* was included in a postwar anthology of good writing (Steinman 1964).

Third, he maintained a widespread, lively correspondence. He communicated regularly with Oxbridge dons, and academics throughout the UK and the English-speaking world: e.g., anthropologists, folklorists, archaeologists, palaeontologists, classicists, Romance scholars, literary critics, psychologists, museum curators, students of comparative religion. The range of his archived letters suggests the vast majority of his anthropologically-oriented correspondence was with the university-tenured, not with fellow popularizers, whether in anthropology or neighbouring fields. They also tended to be drawn from the ranks of the already distinguished, some of whom first contacted Raglan, not vice versa, for example, George Devereux, S.H.Hooke, Ananda Coomaraswamy, E.O.James, Gilbert Murray, F.R.Leavis. He also dispatched offprints to the interested, and had many stay at his seat. Raglan not only maintained a lively network, he also 'did much to promote personal contact among scholars and to encourage work in which he was interested' (Forde 1964). As his old

friend the UCL anthropologist Daryll Forde pointed out, before the war this kind of contact was all the more important as 'anthropological studies were less organised and contributions from outside the universities played a larger part' (Forde 1964).

Fourth, he liked being a public figure. He intervened sporadically in debates of his time: raising issues in the House of Lords; writing letters to *The Times* and articles for other national periodicals, some of which were syndicated worldwide.ⁱⁱ His collected correspondence shows he was a frequent speaker at university departments, anthropology clubs, learned societies, and on a host of other public platforms. He was also ready to air his views on BBC radio. As his son admitted, Raglan did not shy from the limelight: he was pleased that journalists responded in print to his views; that people listened to his words, invited him to speak on various platforms, and requested he accept public office. Some were well aware of this aspect of his personality: as one reviewer stated, 'Lord Raglan vastly enjoys expounding his principles' (*South Wales Argus* 13 vi 1934). In letters to his mother, he usually enumerates the audience at his talks; in one letter, he even refers to himself as a 'local celebrity'. One sign his efforts to become a public intellectual paid off was when Raglan attacked the colonial system of Indirect Rule its architect, Lord Lugard, wrote an extensive reply (Lugard 1932). Similarly, when he criticized Bertrand Russell in *Man* for misapplying anthropology, the philosopher bothered to respond, admitting his error (*Man* July 1951: 104).

Fifth, his pleasure at being a public figure dovetailed with his delight in creating controversy. While most of his books are in anthropology, a few are deliberately polemical essays on central issues of the day. Raglan enjoyed propounding a contrary position, often

in stark terms, to excite reaction and stimulate debate (e.g. Brown 1943). He liked to prod people into reflecting on cherished assumptions, to provoke them deliberately, 'as he thought, once you'd got them puce with rage, they might well say something worth listening to' (G. Somerset, 11 x 2014). Some of his work in current affairs, he openly classed as 'thought exercises', both for himself and for readers. In sum, as his son opined, there was 'a touch of arrogance' to Raglan: definitely 'a bit of the bolshie about him' (G. Somerset, 11 x 2014).

Raglan's various calls—to sterilize the unfit, banish the Irish, replace fairy tales with historical narratives in the raising of children—and his bold statements, e.g. the lack of successful barriers to 'racial fusion' (Raglan 1934c), brought him notoriety, and also some attention when he turned to more strictly anthropological matters. The publicity given to these polemics lent light to his more scholarly endeavours.

Unlike most of his academic contemporaries, Raglan was ready to attack fellow anthropologists in print, without remorse. A deft writer, he was intolerant of sloppy prose; as an analytical scholar, he enjoyed exposing others' ill-grounded assumptions or claims. He liked to direct his critical eye at alternative styles of explanation: in particular, biological reductionism, functionalism, psychologism, scientism, environmental determinism, Eurocentric utilitarianism, and Freudianism. Malinowski, who was then busy trying to 'ruthlessly shut out the old guard' (Kuper 2010; 141), especially diffusionists, was a particular bugbear for Raglan, whose books are peppered with sharp comments about his opponent's brand of functionalism. However, Malinowski was prepared to goad as good as he got. For he was as much as, if not more a showman-polemicist than Raglan:

I was delighted to get your letter and find that you again violently disagree with my views on primitive law. You are, of course, entirely wrong and misguided, but still, it is always amusing to discuss and to disagree. Why not put your views into print and force me to a reply? (Malinowski to Raglan, 11 iii 1935, 2, GA).

Sixth, Raglan enjoyed a highly productive friendship with the publisher of all his prewar books: E.V.Rieu, the renowned classicist and managing director at Methuen. Indeed, after accepting Raglan's first book, Rieu commissioned his next two, on current affairs, and hired a publicity team to promote one of them (Rieu to Raglan 24 viii 1933, 1, GA). Rieu also tried to persuade his friend to write a book on anthropology 'for the general reader' but it seems Raglan's work on *The hero* and then on *How came civilization?* took precedence (Rieu to Raglan 29 x 1937, *Hero* envelope, 2, GA). Thanks to Rieu's efforts, his books were very broadly reviewed: Raglan's archive contains cuttings from 17 periodicals for *Jocasta's crime*, 51 for *If I were dictator*, 66 for *The hero*, and 11 for the *Civilization* book. These journals ranged from the local (*Derbyshire Advertiser*) to the national (*Daily Mirror*, *Daily Telegraph*), to the international (*Peiping Chronicle*, *Boston Evening Transcript*); and from the intellectual but general (*Nature*) to the more specific (*Eugenics Review*, *World Service and Psychic Review*).

How Raglan was assessed

For anthropologists who regard their subject in conventional historiographic terms, Raglan can be seen as a mere transitional figure, an amateur who achieved prominence at the very time others were striving to professionalise their scholastic pursuit. This is an impoverishing view from an interested position.

In the eyes of the professionalisers, Raglan championed an increasingly unfashionable approach. Moreover he did so energetically and in an uncompromising manner. His copious book reviews in and letters to *Man* are often bold in tone and unsparing in criticism. He repeatedly chides colleagues for shoddy scholarship, logical inconsistencies, and poorly grounded speculation. At times his dedication to keeping debates alive forced *Man's* editor to foreclose correspondence on the respective issue (e.g. *Man* July 1938: 120).ⁱⁱⁱ Further, unlike his tenured opponents, his anthropological position does not appear to evolve. His 1957 Presidential address to the RAI is a re-declaration of diffusionist principles and functionalist criticisms: 'To sum up, my quarrel with the anti-diffusionists is not merely that they are wrong. Many errors are harmless, but theirs are dangerous' (Raglan 1957: 146).

If Raglan's aim was to excite reaction, he succeeded, though some of that response was negative, and a bit of it ill-tempered. For instance, in 1942, a communication he gave at the RAI on 'The future of anthropology', was followed in *Man* by lengthy, critical comments from five Fellows (Raglan 1942). His diffusionist paper about the cruck truss led to a similarly drawn-out correspondence with architectural historians, among others (Raglan 1956). But

the most extended example of his ability to initiate and maintain a debate was the 'Webs of fantasy' correspondence, which he started, and a series of Fellows kept up in the journal for over two years in the mid-1950s. The key issue was the role of history in anthropological explanation, but the sometimes acid tone of exchange also served to mark out the differences between the structural-functionalists and others. John Beattie, as self-appointed representative of the professionalisers, treated Raglan's argument as 'fantastic', and tried to divorce the diffusionists from social anthropologists by classifying their 'hypotheses as somebody else's problem' (Beattie 1954: 35). In turn, Raglan thought the functionalists blinkered as they allowed for diffusionism in European prehistory but not for other places in contemporary times (Raglan 1955: 48).

Raglan's election in 1955 to the Presidency of the RAI is further evidence of how divided opinion of him was. At the initial meeting of the Committee, no elector raised any objection to Raglan as a candidate, other than that, as a controversial figure, 'Council might be divided about him' (124/6/14, RAI). Raymond Firth, who had succeeded to Malinowski's chair at the LSE, was ambivalent. He feared that selection of Raglan for the presidency might be viewed as the Institute 'giving support to a very unorthodox—and some might think—semi-scholarly type of work' (124/6/14, RAI). Thanks primarily to Forde's critiques of other candidates, agreement was finally reached over Raglan. Among other reasons, electors recognised the years of service he had quietly given to the RAI. Firth opined, 'I know there will be surprise in some quarters about the choice, partly because of Raglan's unorthodox work in some fields of anthropology and his provocative style of attack' (124/6/19, RAI).

Raglan was not a lone protagonist. Others at the same time were also striving to maintain a broadly based anthropology. For instance, in 1956 the former colonial administrator and professor of anthropology at Cambridge, J.H. Hutton, argued that the 'professional academic discipline. . .is liable to suffer (because) it *is* academic'; he praised 'the great multitude of ethnographers not trained as professional social anthropologists' (Hutton 1956, orig. ital.) Further, during Raglan's presidency of the RAI, William Fagg, then its Secretary, campaigned in an agitated manner against Leach's proposed merging of *Man* and the *Journal of the RAI*, with a single editor appointed by the ASA. Fagg thought that tantamount to the surrender of the Institute to the 'specialists' (Fagg to Raglan 16 x 1955; 21 xii 1955, 1, GA).

Within the UK, and beyond academia, assessment of Raglan's anthropological work was much more positive. Generally, reviewers of his books appreciated his cross-cultural ability to remind readers how arbitrary English customs of the day were; they valued his crisp delivery, sly asides, impatience with sloppy thinking, and fearless approach to big themes, (e.g. *Granta* 13 iv 1933; *Week End Review* 22 iv 1933; *The Observer* 30 iv 1933; *Morning Post* 15 ix 1936). According to one, *The hero* 'will set thoughtful people thinking afresh, irritate many more, and amuse that big public which delights in seeing idols knocked down' (*Daily Telegraph* 19 v 1936).

Reviewers' praise could be qualified: for some, his arguments were initially plausible, but carried too far (e.g. *London Quarterly & Holborn Review* I 1940). Some interpreted his claim that legendary figures were mythical to mean there was no creditable history until the

early modern period (*Manchester Guardian* 18 ix 1936; *The Nation* 21 viii 1937). Others rubbished his assertion that ‘folk-memory’ was historically ungrounded, and at least one argued Raglan misunderstood myth: its point was not to provide historical certainty but moral exemplars (*The Observer* 29 ix 1936).

At times his debunking was deeply unpalatable, his provocations taken as insults. Raglan’s claim the oldest British lineages were fiction upset greatly some nobles who claimed descent from the Normans; his interpretations of Biblical narrative irritated censorious clerics; racial apologists crabbed his critical comments on ‘purity of breeding’ (*Friend*, Bloemfontein, 17 ix 1933; *Ceylon Observer* 26 xi 1933; *The Christian* 21 xii 1933; *The Times* 4 viii 1936; *Ipswich Evening Star* 5 viii 1936).^{iv} Welsh nationalists were outraged by his statement that their language was not worthy of support: he ignored their calls for him to stand down as Lord Lieutenant (*The Times* 30 x 1958; *Western Mail* 27 xi 1958. See also *South Wales Argus* 29 xii 1933). As one renowned writer admonished Raglan, ‘I read (*Jocasta’s crime*) with attention, edification, delight—and infuriation. There are times when it seems to me you are the most wilfully wrongheaded anthropologist I know, a singular if bad eminence’ (Shanks to Raglan, 5 vi 1951, 2. GA).

Raglan’s work was well publicized throughout the Commonwealth. But he was particularly appreciated in North America. The editor of *The New Yorker* was eulogistic:

You have among the member of the staff of this magazine half a dozen fervent admirers. We read ‘The Hero’ over and over—at least, I do and have enormously

enjoyed 'The origins of religion'. They are the biggest little books ever written (Gill to Raglan, 24 viii 1949, 1, GA).^v

Some US scholars of literature and myth recognized the power of his caveat against reading history into legends, and the 'enduring value' of his thematic analysis in *The hero*, as well as its innovative proto-structuralist approach (Fontenrose 1966: 25; Scholes 1975: 68; Miller 2000: 66). Some, however, worried about his lauding of myth at the expense of history; others criticized the vagueness and rigid, static nature of his ritualism, which, they argued, did not stand up to further cross-cultural analysis (*New York Herald Tribune* 25 vii 1937; *New York Times* 28 vii 1937; *Saturday Review*, New York, 14 viii 1937; Thompson 1955; Weisinger 1956; Bascom 1957). His work in this general area is often compared with that of the far more successful Joseph Campbell, whose volumes on comparative mythology started to appear in the late 1940s (e.g. Lederman 1979; Van Spanckeren 1989; Bell 1997: 16; Gottschall 2005; Segal 2013). Postwar publishers in the USA were very keen to push Raglan's books. In 1952 the UK-based Rationalist Press was ready to sell a thousand copies each of *The hero* and *The origins of religion* to an American firm at less than cost (Letter to Raglan 29 v 1952, 1, GA). In 1955 Vintage Press funded an 'ambitious' campaign to promote *The hero* in north America; over 10,000 copies were sold (Knopf to Raglan, 28 ii 1955, GA; Hyman to Raglan 27 xii 1955, 1, GA). In 1959 Harper considered republication of *How came civilization?* in paperback (Methuen to Raglan 16 v 1959, 1, GA). Five years later, when Raglan was nearing 80, Alan Dundes asked to include his 1934 paper 'The hero of tradition' in an anthology of classic papers in folklore, ranking it as 'one of the most important studies in the field' (Dundes to Raglan 3 vii 1964, 2, GA). Several weeks before Raglan died, the American publisher of *The temple and the house* wrote of his 'greenest curiosity about the

subject of your next book and its probable date of completion' (Brockway to Raglan, 16 vii 1964, 1, GA).

In the 1960s, in an unexpected twist, the American atheist activist Madalyn Murray O'Hair adopted his *Origins of religion*. She said it showed people were reliant on the mystique of rituals: what they needed was less ceremonial, and more meaning in their lives (Le Beau 2005: 224-5). A trawl through the Net reveals Raglan's work continues to be used as grist to the atheist mill.^{vi}

Raglan's legacy?

During his lifetime, assessment of Raglan's work, within academic anthropology and beyond, was variable. In the years following his death the British anthropologists who wrote the history of their discipline were much more uniform in opinion about the diffusionism he espoused. They thought it a mistake.

Evans-Pritchard had early set the tone. In a single sentence, he claimed diffusionism had had 'little lasting influence', partly because it is 'as conjectural and unverifiable' as the work of evolutionists (Evans-Pritchard 1951: 47). His students kept to his line in the definitive textbooks they wrote: Beattie chided diffusionists for 'going far beyond the evidence' while Godfrey Lienhardt ignored them altogether, moving without interruption in

his intellectual genealogy straight from Frazer to Radcliffe-Brown (Beattie 1964: 8; Lienhardt 1964: 28). Mair, like Evans-Pritchard a student of Malinowski's, was equally dismissive: 'Not much need really be said about the war of the diffusionists and evolutionists. . . The whole discussion now seems rather unreal' (Mair 1965: 18). These postwar anthropologists come across as mainly concerned to distance themselves as much as possible from their immediate predecessors.^{vii} The epigraphs heading this paper, by distinguished anthropologists of the succeeding generation, suggest how little attitudes had changed, even fifty years later.

The postwar neo-Durkheimians were so ready to denigrate diffusionism because it clashed with their structural-functionalist portrayal of societies as coherent wholes coasting in an ethnographic present. Societal integrity was valued, cultural borrowing was not. History was a complicating factor they usually wished to do without. In this constraining context, Raglan's sustained latter-day defence of diffusionist development and appeals for the incorporation of historical dimensions into anthropological studies were unwanted. As an independent scholar, he had the freedom 'to voice muted discontents more widely felt within the profession at large' (Vincent 1994: 257). However, as an amateur in an increasingly professionalised university context, he had no power to change, only to irritate.

Ideological factors were very likely at play here as well. Though few postwar anthropologists were avowed left-wing radicals, most did espouse liberal critique of Western values. British diffusionists tended to be more conservative. Anti-evolutionists, they held to a mainly static view of society: development was not brought about by tempered change, but unpredictably, provoked by the innovations of especially talented

individuals or the interventions of conquering peoples. This view dovetailed with the heightened imperialism of the era (Kuklick 1991: 256-65). Raglan's *The science of peace* is an exemplar here: he argues a ritualist origin for armed conflict, later exacerbated by nationalist excess, in turn only tempered by wars of empire. Imperialist campaigns pacified aggressive peoples, spread knowledge of arts and crafts, and humanized indigenous religions (Raglan 1933b). Evans-Pritchard and his cohort may have wished to train colonial officer cadets in anthropology (and so justify the funding of their own positions), but the deployment of anthropology in a vigorous defence of Empire was taboo.

It is relatively easy to demonstrate this postwar attitude of methodological dismissiveness is as misrepresentative as it is interested. From about 1910 to 1930 diffusionists had in fact enjoyed an extended popularity, their fundamental concerns about migration and culture contact being shared by many of their anthropology contemporaries. Kuklick argues it is unsurprising that these phenomena were then of interest, given the considerable movements of populations at the time (Kuklick 2008: 68). Moreover, during this interwar period, diffusionism was also popular beyond the academy, as much of its protagonists' writings were aimed at a general readership, who 'responded enthusiastically' (Kuklick 1991: 12).

Furthermore, within British academic anthropology of the postwar period, diffusionism managed to live on, albeit in a minor key and shorn of its excesses. Its central figure was Raglan's friend, Daryll Forde. A geographer-turned-anthropologist, he established the UCL department, and headed it for twenty-four years. His landmark work, *Habitat, economy and society* (Forde 1934), which M.G. Smith ranked 'high as a classic

demonstration of comparative method in social anthropology', is a sustained exercise in correlating features of social organisation, ecology, and the environment (Smith 1969: xxv). This textbook, aimed at older school pupils and undergraduates, was reprinted over thirteen times in its first thirty years: in 1973 Ioan Lewis claimed it was 'still going strong' (Lewis 1973: 57). Of course, prewar UCL had been the base for Elliot Smith, and his approach was upheld there through into the 1970s by N.A. Barnicot, who became the country's first professor of physical anthropology (1960-1975).^{viii} Though Forde's aims shifted away from diffusionist concerns, for several decades UCL still acted by default as the academic homebase for British diffusionism.

Diffusionism was also kept alive, to a certain degree, by Rodney Needham at Oxford. Looking for precursors of structuralist concerns, he came to laud, in a discriminating manner, the work of Maurice Hocart. Introducing a reissue of Hocart's *Kings and councillors* (Hocart 1936), he decried the structural-functionalists' unfair shunting of diffusionism into 'a limbo of unregenerate error' (Needham 1970: lxviii):

We can surely try to encourage a more liberal and fertile style of investigation in social anthropology by turning students, or at least those who are not already too set or malformed in their ideas, away from the banalities and shuffling compromises of the textbook. . . , and more in the direction of those adventures of the mind such as Hocart exemplified (Needham 1970: xcvi-vii).

Needham was assiduous in trying to revive interest in Hocart and collaborated with his literary executor, Raglan, to that end (Needham pers comm 2000). In his review of *The temple and the house*, Needham acknowledged the shakiness of some of Raglan's

diffusionist claims, but argued they left 'untouched most of the central issues which Lord Raglan forcefully delineates' and which were worthy of 'respect and interest' (Needham 1964).

Similarly Edmund Leach, as renowned as Needham for his individual approach, greatly appreciated the imaginative acuity of Raglan's symbolic equations. Leach thought colleagues might class *The temple and the house* a display 'of amateurism and of senility'; he considered it 'very shrewd'. For him, the more hidebound had been too distracted by Raglan's quixotic manner to perceive his 'element of true genius' (Leach 1965; 1982: 209). Francis Huxley, again a very independent anthropologist of this general period, was equally appreciative of Raglan's work (Huxley 1974).

If we shift from a general intellectual focus to a more ethnographic one, it becomes clear that diffusionism also did not go away in the study of specific geographic areas. Perhaps the most illuminating case is that of Oceania: a collection, after all, of physical isolates (islands), which yet show many striking social and cultural similarities. To the question, 'How might this be?', a ready answer in the early twentieth century was diffusionism via migration and trade. The exemplary, early text here was Rivers' two-volume *History of Melanesian society* (Rivers 1914), whose pioneering insights were still being lauded by Melanesian comparativists seventy years later (e.g. Brunton 1989). In the following decades this diffusionist approach was upheld by, among others, Deacon and Layard in their ethnographies (Deacon 1934, Layard 1942). This regional maintenance of an explanatory style was not by chance, as the concept of diffusion is particularly apt for Melanesian societies, given their customary propensity for internally generated

transformation. In Vanuatu, for instance, change was stimulated by aspirant big-men's desires to awe their ritual constituency, which dovetailed with an inter-island trading network of ritual elements, undergirded by local notions of individual creativity and cultural copyright (Allen 1981b). In this context, the modernization movements once known as 'cargo cults' were simply the most evident of a long-grounded series of renovatory initiatives (Guiart 1956, 1958; MacClancy 1980). It is not surprising therefore, that even in the 1980s anthropologists of the area had no difficulty about deploying the concept of diffusion within their analyses (e.g. Allen 1981a, b).

Diffusionism also survived because of its patent interdisciplinary potential. From anthropology, the first discipline to develop diffusionism in a sustained manner, it diffused through to other fields of study. A key figure here is the geographer Carl O. Sauer who, inspired by the work of his Berkeley colleague Alfred Kroeber (Duncan 1970; Solot 1986: 510-13), developed with his students a diffusionist style of 'cultural history'. Within geography this approach was both highly influential and enduring, until well into the 1960s. Similar statements can be made about archaeology, with *fin de siècle* practitioners reacting against evolutionist approaches while borrowing from the anthropology of the time both the diffusionist approach and the concept of 'culture', to produce the paradigm of 'culture history'. Dominant in the British branch of the discipline in the first half of the last century, culture history was only marginalized in the 1970s by the rise of processual approaches, though it has remained a necessary constituent of archaeological investigation (Trigger 1991; Michaels 1996; Shennan 2000). Anthropologists also influenced diffusion research traditions in other fields, such as history, education, and communication studies (Hugill and Dickson 1988) but 'especially early sociology and rural sociology' (Rogers 2003: 49-50). In

these various fields, diffusionists often attended to ideas more than technological items, to innovation (and denovation) as much as diffusion, and to the role of the adoption environment.

Moreover, several anthropologists have noted that diffusionism, in a transmogrified form, has recently re-emerged onto the centre-stage of anthropology, within the burgeoning study of globalisation (e.g. Fabian 1998: 107, n.23; Friedman 2003: ix-x; Friedman 2013: 335; Hahn 2008; Leal 2011). Though globalisation theory is much more than a re-packaging of diffusionism, both share a common orientation: a focus on flows, undermining a rigid concept of culture, and highlighting its localist tendencies in the process. As Appadurai opined, 'Diffusionism, whatever its defects and in whatever guise, has at least the virtue of allowing everyone the possibility of exposure to a world larger than their current locale' (Appadurai 1988: 39).

The burden of historiography

Diffusionism did not stutter into inactivity in the late 1930s, despite what some wish to claim. As evidenced here, it remained a live anthropological concern, though one outside the UK mainstream, through to contemporary times, while the recent emergence of globalisation theory has led to the beginning of its reappraisal and some initial questioning of postwar dismissiveness.

It is now clear that at a certain level of abstraction, diffusionism and structural-functionalism may be seen as flip-sides of each other, one stressing synchrony and cultural boundedness, the other diachrony and cultural permeability. Evans calls this sort of serial rise and fall of apparently opposed theories 'the continual ebb and flow of academic orientations' (Evans 2006: 94). Abbott, in a geometric turn, argues that common oppositions, functioning at all levels of theory, organise disciplinary knowledge: thus diffusionism and structural-functionalism can be viewed, like fractals, as mutual reflections of their own distinctions (Abbott 2001). In a more dynamic vocabulary, Barrett characterizes their association as the 'pendulous' or 'oscillating' nature of anthropological knowledge, which operates as an unproductive closed circle (Barrett 1984). Alternatively, if we apply an anthropological distinction to scholarly activity, we might see the dovetailing caricatures deployed by protagonists of the two approaches as a process of mutual mimicry, with the functionalists trying to turn symmetrical schismogenesis into its complementary version. For Evans, Abbott, and Barrett, it's as though, for a discipline, there are a limited number of explanatory styles, whose academic prominence is rhythmic, or otherwise patterned. To turn dramatic for a moment, they present the available range of theories like a limited cast of actors on a revolving stage, periodically reappearing before being returned backstage for a while. Theoretical activity as repertory theatre?

One could choose other, more optimistic metaphors which, also, fit with more plural conceptions of disciplines. We might speak instead of the 'after-life' of theories, of paradigms which long continue to survive, albeit in minor mode, once the initial potency of their explanatory potential has started to decline. To take a different tack: without too much stretching of Deleuze and Guattari's original characterization (Deleuze and Guattari

1980), we can class diffusionist thought rhizomic, supposedly withered at source but pushing up shoots in unexpected places time after time, making unforeseen connections in the process. Whether we plumb for metaphors tidal, mathematical, mechanical, Batesonian, physical or biological (and each, by definition, has its limitations), I hope it is now clear that at least one consequence remains the same: theories do not die just because powerful opponents declare so, and manage to get that repeated in the key textbooks.

Raglan may be viewed as a victim of this divisive process, as one made to suffer the common fate of unfashionable radicals: more lampooned than lauded, more dismissed than discussed. Too often, his desire to be bold, to not appear cowardly, was the major's undoing. The lord wanted to cut a dash. Instead he was usually just cut, marginalised as a late stalwart of a noble stereotype, the English eccentric. Given his sustained work in the symbolic and spiritual dimensions of social life, it is ironic this clever but idiosyncratic scholar is today best remembered by religious sceptics.

Bendix has recently classed work into the history of anthropology as 'overabundant' (Bendix 2015). Godfrey Lienhardt used to say the same to me, about Stocking's *History of anthropology* annual. He thought a discipline as small as ours could not bear the weight of so much historical analysis (Lienhardt pers comm 1989). My hope is that the evidence of this paper might suggest otherwise. For overviewing the intellectual trajectory of Raglan exemplifies, in a multi-contextual manner, the work done by diffusionists. Also, it serves to illuminate a key dimension of British anthropology's 'golden age', an element of that period as misrepresented as it is neglected. Moreover, Raglan's interstitial status between the academic and the popular helps clarify the multi-headed nature of anthropology over the

mid-century decades. If, for that extended period, Robert Graves can be regarded as the greatest populariser in the UK of neo-Frazerian anthropology (MacClancy 2013: 110-134), then Raglan may be viewed as his diffusionist counterpart. Their example reminds us how much work is done in the name of our discipline beyond its academically defined borders. Furthermore, as some were well aware, Raglan's defiant defence of diffusionism could mask his genuinely pioneering, protostructuralist work: the symbolic equation linking incest to exogamy; the structure of mythical-hero narratives; the common symbolic logic to a variety of rites, and to housing structures; the organizing role of symbolism in religious rites. In these senses, Raglan's protostructuralist equivalences strove to keep symbolic concerns to the fore at a time when neo-Durkheimians concentrated on elucidating social structures. Finally, Raglan may also be regarded as an upholder of fundamental concerns in anthropology. Needham, for one, thought Raglan an exemplar of what anthropologists should be doing: not just comprehending alien categories, but subjecting our own categories to 'a comparative and sceptical appraisal' as well (Needham 1964).

More generally, scrutinising the ways diffusionism was slighted by its opponents yet continued to be practised within academic anthropology enables us to adumbrate the demonstrable life-course of theories, and not just perpetuate the self-serving representations of hegemon and their epigones. Despite the disciplinary success of the structural-functionalists, diffusionism did not turn into some kind of zombie-theory but was kept alive in ways unexpected by more strait-jacketed colleagues.

If this were the case for diffusionism, how many other theories, once past their heydays, have been similarly neglected by historians of anthropology?

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Archives

GA Baron Raglan and Family Estate Records, Gwent Archives, Ebbw Vale, Wales

RAI Presidency Committee, Royal Anthropological Institute, London

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Endnotes

ⁱ See also the Mass Observation Archive website, University of Sussex:

<http://www.massobs.org.uk/>. Accessed 1 xi 2017. For the contemporary use of Mass

Observation and its methods by anthropologists, see for example: Stewart 2013;

<https://www.ucl.ac.uk/anthropology/anthropology-news/mystreet;>

[http://www.lancaster.ac.uk/library/news-training-and-events/news/2014/mass-](http://www.lancaster.ac.uk/library/news-training-and-events/news/2014/mass-observation-online/)

[observation-online/;](http://www.lancaster.ac.uk/library/news-training-and-events/news/2014/mass-observation-online/) [https://leweshistory.org.uk/2014/03/10/an-anthropology-of-](https://leweshistory.org.uk/2014/03/10/an-anthropology-of-ourselves-exploring-mass-observation-for-creative-projects-march-april-2014/)

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ⁱⁱ Raglan made his most noteworthy interventions in the Lords in the years immediately after ascending to the peerage: on the negotiations in Transjordan, the protection of British imperial interests, colonial administration, the passport system, the vocational training of soldiers, and Justices of the Peace.

ⁱⁱⁱ The editor might also pass sly comment on a polemical aside by Raglan, in order to undercut his point (e.g. *Man* February 1940: 32).

^{iv} His archives include an insulting letter, by '1066', to his mother, complaining about his 'vile insinuations on the Aristocracy of England' (GA).

^v The renowned literary critic Stanley Edgar Hyman, who also wrote for *The New Yorker*, was equally generous with his praise (Hyman to Raglan, 22 xi 1955, GA).

^{vi} E.g. <http://confessionsofadoubtingthomas.blogspot.co.uk/2011/09/archetypal-hero.html>;
<http://www.rationalskepticism.org/general-faith/lord-raglan-on-mythic-heroes-t2089.html>;
<http://www.rediff.com/news/column/does-heroism-come-with-a-sell-by-date/20130311.htm>; <http://3rdchimp.blogspot.co.uk/>. All accessed 30 iii 2015. His 'hero' pattern is also fitted to modern comic-heroes, e.g. Son of Goku, <http://thedragonballblog.blogspot.co.uk/2012/05/son-goku-and-lord-raglans-hero-pattern.html>. Accessed 20 vii 2015. Jack Goody states he came to anthropology, while reading English at Cambridge, by via F.R.Leavis's references to Raglan (http://www.alanmacfarlane.com/DO/filmshow/goody_fast.htm, min.14. Accessed 20 vii 2015.)

^{vii} Lévi-Strauss could indulge in the same tactic. In the opening chapter to his *Elementary structures of kinship*, he dismisses Raglan's historicism but fails to mention the intellectual congruence between Raglan's equation of incest and exogamy and his own (Lévi-Strauss 1969 [1949]: 23). See also MacClancy 2010: 269, 271n.10

^{viii} <http://www.ucl.ac.uk/museums/ethno/about/history>. Accessed 21 vii 2015