

Narratives of the making of academic hegemony: Mary Douglas's ethnography in the British Africanist field

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Short Abstract:

Drawing on the case of Mary Douglas's less known ethnography, I intend to shed some light upon the constitution of the British Africanist field to understand how some monographs are made canonical while others remain aside, considering the interaction between Anthropology and the colonial context.

Long Abstract:

Mary Douglas is far from a minor figure in the History of Anthropology and in the theoretical basis of the discipline, may that be in the British academy or even in other countries. Nonetheless, her ethnographic contribution, consisting of several articles, reviews and books written until *Purity and Danger* (1966), is fairly unrecognized as relevant to the Africanist field, with the exception of its use in her own theoretical work when in comparison with other contexts. Douglas's work among the Lele of the Kasai, in the end of the 1940's and the beginning of the 1950's, have become one of the cornerstones of her analysis of the relation between social structure and symbolic representation of the values embedded in institutions. A study of her field notes at Evanston's Northwestern University Archives, along with letters from the period of her anthropological maturity and documents from fellow ethnographers and colonial actors that were part of the British effort to understand Africa, held at the International African Institute's archives, provides a different account of the colonial enterprise that is now related with the discipline's history, by bringing to light the experience of one of Anthropology's major contributors in one of its important chapters – African colonialism prior to its territories independence. At the same time, it can help explain the importance of the ethnographic material in Mary Douglas's

work and the complex structure of British Africanism field that excludes this period of the anthropologist's career from hegemonic narratives of its history.

During the year of 2007 I had the opportunity to meet the late Mary Douglas, and discuss with the notable anthropologist some insights I had while reading Richard Fardon's intellectual biography on her (Fardon, 1999). While one could be struck with Fardon's assertion that Mary Douglas, during the period of her Africanist writings, remained fairly unrecognized by the leading scholars from the field¹, a study of her intellectual dialogues with such anthropologists, along with some cautious reading of her personal material held at various institutions, provides some information to the historian of the discipline on how to understand some of the constraints to the making of her professional anthropological career in the United Kingdom.

In fact, Douglas provided some corroboration to the idea of her marginality in the British Africanist field, commenting that she had some reservations with Max Gluckman and the anthropologists from the Rhodes-Livingstone Institute in Northern Rhodesia and also at Manchester, who became the core of the British Africanism in the second half of the last century. It was clear that this was at least an emic issue. As it was also apparent that her own perception of her work stated the importance of the ethnography of the Lele of the Kasai – the matrilineal society she studied while doing fieldwork in the Belgian Congo – to the development of her anthropological imagination, a study of these hypothesis of marginality were worth investigating.

In studying the opportunities offered to Douglas during her anthropological training, but also the difficulties encountered along the way, the historian of anthropology is also led, inevitably, to analyse British colonialism in Africa and its relation with the consolidation of the field. In doing so, it is possible to inquire as to

¹ As Fardon noted, Douglas writings prior to the publication of *Purity and Danger* (Douglas, 1966) were focused on her African material: "With the benefit of hindsight, Mary Douglas's writings between 1950-1965 can be seen to foreshadow interests that were to dominate her later years. But, had she ceased to write in 1965, the profile of her published works would have been indistinguishable from that of a Professional Africanist ethnographer" (Fardon, 1999, p. 48).

whether it is relevant to analyse Douglas more theoretical work, after the publication of her seminal *Purity and Danger* (Douglas, 1966), through the lenses of her Africanist articles and books. It might also be arguable that it would be premature to dismiss the continuity of the Oxford trained anthropologist's entire intellectual project, by separating her Africanist findings from her later thinking – what a statement of marginality could lead to.

The relevance of an ethnographical monograph during this now classical period of the discipline (that coincided with Douglas's intellectual maturity) is product of a myriad of factors, some of which exceed the research done in the field and the writing of the results. Critics of the ethnographical knowledge produced have long denounced the impossibility of science's neutrality (see, for example, Marcus and Fisher, 1986; Marcus, 1999; Marcus and Clifford, eds, 1986). Nonetheless, we are still reviewing the benefits of a study of the academic field, the particular strategies available to the anthropologists in the making of their careers, the importance of intellectual collaborations and of the research and publication funding, in order to understand the scholar's trajectory and the appreciation of their works.

I shall briefly describe a few episodes of Mary Douglas's early attempts to join the Africanist ranks in Britain after her fieldwork in Congo (during 1949-1950 and again in 1953), in order to propose an alternative analysis of the importance of her ethnographical writings and delineate some hypothesis about the reasons of her leaving the Africanist field (while at the same time refusing to abandon it completely). I will draw mostly on a review written by Douglas, but also on her personal material, as well as of some of the key anthropologists at the time, held at the archives of the Oxford University (where she studied and presented her doctoral thesis), at the archives of the International African Institute (that is currently held at the London School of Economics, and that funded Douglas's fieldwork), and at the archives of the Northwestern University at Evanston, IL (where she spent some time during the 1980's and to where she donated much of her letters and field notes). I hope it will bring elements to assert the place of her Lele findings in her theoretical project, but also to inquire about the strategies of the Africanist field that prevented her from establishing in it.

The leading experts of African ethnography in Great Britain after the 1950's (at least in the African central area where Douglas did her research) were the

anthropologists working with Max Gluckman at Manchester University (most of them were also at the Rhodes-Livingstone Institute in Northern Rhodesia, where Gluckman acted as director for several years). Himself an Oxford collaborator at some point², Gluckman was known as a brilliant professor who was deeply involved with his students' training in the field, but also as a rather difficult man when it came to defend his theoretical perspectives. Schumaker (2001) wrote that the members of the Rhodes-Livingstone shared more than intellectual connections and interests, but also a common fieldwork experience, shaped by a particular colonial political setting (and that opposed the malinowskian pragmatic approach of African colonialism). Indeed, even if Mary Douglas wasn't a stranger to the group (she joined the Ethnographical Survey of Africa project under the supervision of Gluckman himself³), recognized the relevance of the Manchester School's innovations⁴, and got some responses from the Rhodes-Livingstone and the Manchester anthropologists to some of her articles and letters, her major Africanist monograph (Douglas, 1963) remained largely unrecognized by them (even though she received positive reviews from anthropologists outside the Manchester School⁵).

In fact, even if Douglas appreciated the School's modern approach to the colonial problems, she might have considered non-theoretical factors when it came to evaluate Gluckman and his colleagues. For instance, the Oxford anthropologist was a little offended with some remarks made by Gluckman about Malinowski. She was

² While still at Oxford, Gluckman was already a respected anthropologist. Mary Douglas remembers his time at the University: "When in 1947 Max Gluckman, director of the Rhodes-Livingstone Institute joined, we could not fail to know that we were small fry into whose midst a very big fish had swum" (Douglas, 1999b, p. 4).

³ In 1947, Douglas (then Mary Tew), was responsible for the Nyasaland portion of the Ethnographical Survey while at Oxford, after receiving funding from the International African Institute and also from the Rhodes-Livingstone Institute, granted with the permission from the Colonial Office and with the help of Daryll Forde. See, for example, the letters from Forde to the Colonial Office staff at the end of 1947, in the International African Institute archives (IAI 16/1).

⁴ Fardon (1999) noted that Douglas analysis was so tuned with the Manchester theories that it seemed to address them explicitly. Werbner, referring to the importance of the School in British anthropology, stated: "Perhaps the earliest outside recognition that a new school had emerged came in a review by Douglas, an Oxford-trained anthropologist familiar with and yet marginal to both the main area and the original working group of the School" (Werbner, 1984, p. 158). As a matter of fact, it seems that much of the School's focus on the politics and conflict in colonial context is described as inseparable from the fact that the researchers were, themselves, deeply involved in the everyday struggles in the colonies, something that they had to address in the academic politics when in touch with the colonial administration.

⁵ For example, Gulliver (1964), Cabot (1963), Lienhardt (1964), Vorbichler (1964a, 1964b), Geluwe (1964) and Lystad (1963). Vansina (1964a, 1964b) and Kuper (1963) even predicted that the book would become a classic in the field. Others, like Luc de Heusch, made later attempts to use Douglas ethnographical data in their own studies (de Heusch 1975, 1981).

particular taken aback with an article written in 1947 by the South African in which he criticised the Polish anthropologist. It must have been, Douglas thought, a work written by a man who could not “be good”, as it was so harsh with the senior professor. She even referred to two of the main Gluckman’s collaborators (John Barnes and Clyde Mitchell, who seemed to attend all of Gluckman’s lectures) as his “bodyguards”⁶. Fardon also stated that Douglas publicly criticised Gluckman, who she thought was “an unattractive and domineering personality – a view he seems to have reciprocated”⁷.

Malinowski had a great influence in the collaboration between the British Academy and the Imperial Government, represented by the Colonial Office. During its most productive years the IAI established tight links with the Empire, with other colonial governments in Europe and even with some American and African institutions, which were responsible for the funding of the International African Institute directed by Daryll Forde in London. As a matter of fact, Gluckman was also deeply interested in articulating the delicate game of producing theory and doing fieldwork, by obtaining funding and forming intellectual networks in prestigious institutions. But it was clear that the dichotomy between the groups of British anthropologists at the second half of the last century made it difficult for someone who was not a “cloth cap boy”⁸ to enter their circle. In 1952, Douglas (Douglas, 1952) wrote a review of the book edited by Gluckman and Elizabeth Colson (Gluckman and Colson, eds, 1951). Even though Douglas praised the work done at the Rhodes-Livingstone and its researchers, she criticised the excessive delay in publishing the results, and also the book’s organization: she thought that its objective, apparent in the title itself (*Seven Tribes of British Central Africa*), wasn’t achieved, since the volume lacks a comparative exercise that had to be done. She argues that the editors might have assumed that geographical criteria would have maintained the cohesion of the works. By not relating in a productive way with each other, she thought the published papers didn’t add anything to the students of anthropology, eager to read the research. It wasn’t, to add to the criticism, useful to colonial

⁶ Douglas, personal communication with the author. Douglas stated the same information in an interview given to Peter Fry (Douglas, 1999a).

⁷ Fardon (1999, p. 51).

⁸ As Anthony Epstein put to Schumaker when describing the class division between the anthropologists from the United Kingdom and the ones from the colonial territories, where the Manchester School members were identified (Schumaker, 1999, p. 110).

administrators – a kind of question the anthropologists in Britain were particularly interested in (despite the discussion about the scientific relevance of such pragmatic matters by some who championed “pure theory”), and that led to later criticism under the idea of paternalism. Gluckman rather proposed an integrate analysis of black and white groups in the colonies.

The response didn't take long to be published (Gluckman and Colson, 1951). The authors didn't quite focus on the allegations that the book lacked internal organization, rather that Douglas worked on incorrect and imprecise assumptions about the mode the Rhodes-Livingstone functioned. To some of Douglas's criticism Gluckman and Colson stated, for example, that Ian Cunnison's work wasn't published because he hadn't returned from the field in time; that Douglas didn't have the knowledge on the costs of publishing; about the delay in releasing the research; and also about the desire that the findings could have been made accessible to anthropology students in advance (the editors remembered that Douglas had access to the manuscripts while working at the Ethnographical Survey). They list and challenge each of Douglas's points by this premise: that an promising yet un-established anthropologist was being unjust in criticising the selection of the works, for she lacked the proper understanding of the particular dynamics of the Institute, since she wasn't familiar with it and wasn't considered to be a part of the established group.

It seemed that it was a common strategy to assert the understanding of the anthropological literature, while criticising the published authors and proposing complementary analysis of the issues in question, to establish oneself in the field. Others had done so with less or more success. Nonetheless, Mary Douglas knew her criticism caused some uneasiness even among the IAI staff. In a letter to Beatrice Wyatt, secretary of the Institute, while making reference to another review she was commissioned to do (of the book *Good Company*, by Monica Wilson), she also mentioned the review on Gluckman and Colson's book, and apologized for any inconveniences she might have caused: “Here is a review of ‘Good Company’ at last. I am sorry I got you into trouble with my review of ‘Seven Tribes’ and have tried to be more tactful this time”⁹.

⁹ Douglas to Wyatt, February 22nd, 1952. Archives of the International African Institute. IAI 38/27.

Even though Douglas's letter suggests that Miss Wyatt could have taken some of the heat caused by the review, the first took the hardest hit. It was not the first time Douglas was caught in some kind of embarrassment due to unfamiliarity with the academic rules and procedures. She got a reprimand from Forde when returning from the field, because she had agreed with William Fagg, who she met in the Congo, to talk about her ethnography in a Royal Anthropological Institute meeting (later things were amended when a joint meeting was settled)¹⁰. She also caused worry to the IAI director when she applied for a lecturer position at the Oxford University while still under contract from the IAI (later Forde agreed that she could apply and use the remaining amount from her grant for a second trip to the Congo, given she could present a report of her research). It took a considerable amount of negotiation between Forde, Evans-Pritchard (who acted as Douglas's supervisor at Oxford) and the staff from the institutes to set things right¹¹.

But, being fair with Douglas, she did have the consent from Forde to publish the referred review. In a letter from August 28th, 1951, Douglas sent a first draft of the text. She seemed aware of reactions her criticism could stir, but she also followed Forde's advice and didn't refrain from doing it: "Here is my promised review of 'Seven Tribes'. You see I have taken you at your word, and not pulled any punches and I am afraid none of them will be very pleased. But now I will try to do a kinder review for 'Man'"¹². Forde answered on September 3rd, stating: "I share your opinion with regard to the planlessness of the Seven Tribes volume, and feel that you are right in calling attention to it. I am suggesting one or two verbal alterations, not to tone it down, and therefore enclose your typescript to know if they are acceptable"¹³.

¹⁰ See the whole negotiation in the letters: Fagg to Forde, April 14th, 1950; Forde to Fagg, April 17th, 1950; Forde to Tew (Douglas), June 16th, 1950; Forde to Tew, June 19th, 1950; Tew (Douglas) to Forde, June 23rd, 1950. Archives of the International African Institute. IAI 38/27.

¹¹ See the exchange of letters in: Tew (Douglas) to Forde, November 11th, 1949; Forde to Tew (Douglas), November 21st, 1949; Evans-Pritchard to Forde, November 24th, 1949; Evans-Pritchard to Forde, November 30th, 1949; Forde to Evans-Pritchard, December 5th, 1949; Evans-Pritchard to Forde, December 7th, 1949; Forde to Evans-Pritchard, December 8th, 1949; Forde to Holdsworth (secretary for the Colonial Studies Committee), November 30th, 1949. Archives of the International African Institute. IAI 38/27.

¹² Douglas to Forde, August 28th, 1951. Archives of the International African Institute. IAI 38/27.

¹³ Forde to Douglas, September 3rd, 1951. Archives of the International African Institute. IAI 38/27. Emphasis in the original.

Douglas wrote to Forde the next day: “I am glad of most of the amendments suggested for the review of ‘Seven Tribes’”¹⁴.

What one could infer from the whole episode is that although Douglas was ultimately responsible for the review, Forde shared her criticism. The fact that it was up to Douglas to deal with the public response from Gluckman and Colson wasn’t entirely because it was her name under the review, but it could also be that she was an inexperienced anthropologist, who was yet to defend her DPhil and showed some ignorance about the proceedings of the anthropology’s field. She was the obvious target.

It turned out that Douglas never really managed to establish an intellectual collaboration with the Manchester School. She remained in touch with several of its scholars, but even future relations weren’t deprived from acid remarks. Another example of how were the relations between Douglas and Gluckman can be seen in reading the Northwestern University’s archives. In a long letter from February 1957, Gluckman commented on an article draft she posted to him and Victor Turner, another Manchester anthropologist at the time. It is possible to perceive, in the letter, a considerable effort from the two authors, even if the criticism was harsh and denoted some considerations about another factor raised by Fardon for Douglas’s marginalization: gender. Gluckman wrote that he thought the argument of the text was confused and needed better refining. He suggested, quite elucidating, that she ought to read some of the Manchester’s works, like of Clyde Mitchell and Colson. Gluckman wrote and I quote:

Herewith comments from myself and Vic Turner on your manuscript. I’m sorry for the delay; but you are largely responsible. I read the paper a long time ago, and saw at once it would take a longer time to work over. For old friendship sake, I’ve done this. I hope you won’t think I’ve been too severe, or that Vic has. We have caught at every point we could: for this seemed to your advantage.

Briefly, I have enjoyed reading the paper, especially the section on raffia and the last 6 pages. But frankly the mss. reads like the product of the mother of three small children – who has written down her good ideas in offs and ends of time, never had the chance to undertake a continuous stretch of writing and thinking on the problem, not had time to ‘scissors-and-past’, or to index the order of points and re-write. You have my sympathy!

This has made it difficult to follow the main development of the analysis; and in the end I went at it pars. by pars., and scribbled notes in the margin of my comments¹⁵.

¹⁴ Douglas to Forde, September 4th, 1951. Archives of the International African Institute. IAI 38/27.

It is important to mention, given these few examples of Douglas's early attempts to establish herself in the British Academy, in general, and in the Africanist field, in particular (with an additional remark: that the Africanist group she wasn't able to join was the hegemonic Manchester School, since she did have a good relationship with other notable scholars in Britain and overseas), that the "silence" Fardon referred to was mostly about her 1963 monograph, from the group of anthropologists working close to Gluckman. Douglas managed to express her points in her articles, articulating the anthropological theory at hand and stimulating responses from her peers.

What is more relevant is that at an early stage of her work Douglas was already interested in relating the description of the forms of classification and social organization with the structures that ensure solidarity and its functioning. She was deeply concerned with Lele symbolic thought and the way social structure was maintained through rules of avoiding misfortune. While still struggling with her ethnographical material she wrote letters to Religious Societies asking for references about Biblical passages that dealt with notions of purity and "matter out of place", which she tried to relate to Lele categories of taboo, expressed in witchcraft conceptions¹⁵. Her later work, starting with *Purity and Danger*, was an ongoing attempt to articulate symbolic thinking about impurity and its avoidance with cultural assumptions shared by individuals and society's institutions that enforce them. The reading of Douglas's Africanist work allow us to highlight its importance in her

¹⁵ Gluckman to Douglas, February 1957. Northwestern University Archives, Series 11/3/2/2. Box 1, Folder 2. It seems Evans-Pritchard wasn't pleased with Douglas marriage and her moving to London. Her supervisor showed some of the difficulties women anthropologists had in post-war ethnography when deciding that Douglas needed to choose a safe location to do research, since a negative experience could prevent other women to do fieldwork. The Lele territory in Kasai was relatively safe and isolated (Fardon, 1999).

¹⁶ At least in her second trip to the field (1953), Douglas showed interests in subjects she later analysed in *Purity and Danger*. In March 25th, 1954, C. P. O'Donovan, secretary of the Bellarmine Society, sent references on purification rituals described in the Pentateuch. In May 7th, 1955, P. Whithall, also from the Bellarmine Society, helped Douglas with some information about food interdictions and the expected restrictions of the clergy as described in the Scriptures. She wrote letters to Louis Dumont, who spent some time teaching at Oxford, about the relation between food and dangerous food, and to Jan Vansina, the Belgian anthropologist who did his fieldwork in Bushong territory, close to the Lele country, inquiring about indigenous conceptions on "forbiddenness". In fact, there is a significant amount of letters about the subject, and her field notes have abundant examples of her analysis of Lele ideas about cleanness and dirt, which she tried to relate to Biblical passages and also to Franz Steiner's studies about taboo. Steiner was a brilliant professor who taught at Oxford while Douglas was there, and his influence on her thinking was clear (Douglas, 1999b). See O'Donovan to Douglas, March 25th, 1954; Whithall to Douglas, May 7th, 1955; Dumont to Douglas, June 25th, 1954; Vansina to Douglas, February 8th, 1954. Northwestern University Archives, Series 11/3/2/2. Box 5, Folder 9.

theory, but knowing it is seldom studied in anthropological disciplines and is hardly considered by critics of her theoretical texts, it seems that we could fail to apprehend the attempt to establish a coherent intellectual project, and the relevance of this “unbalance” between Africanist writings and theoretical work to Douglas. It might have guided, more than we think, the manner with which Douglas compared systems of thought in different societies. In her own perception of the importance of her insights of Lele society in the entirety of her thinking, her ethnographical monograph, ignored by the Manchester group, was supposed to end her life’s work (she did intend to return to Lele material, after the publication of her last book about religious analysis¹⁷).

In the preface of *How Institutions Think* (Douglas, 1986), writing about how a theory of institutions might amend the sociological view of human cognition, Douglas stated that this book should be the first to have been written after her fieldwork in Africa. Instead, she wrote *Purity and Danger*, as an attempt to generalize her African material. She later published her risk studies books, which should have been written before other attempts to analyse society’s beliefs, even outside Africa. *The Lele of the Kasai*, Douglas thought, should have appeared after these publications. One could only guess what Douglas’s work appreciation might have been, had she done just that. Much is yet to be said about the intellectual connections, its relation with the British anthropological field and its particular rules of establishment, the importance of personal networks of collaboration, and the effects social origin have in articulating these factors. A study of the links between canonical works, as well of the political struggles behind them, could give an alternative perspective on Douglas’s ethnography. It must, of course, be comprehended in the context of these hegemonic groups in British anthropology, in the framework of colonialism. These factors might be just as important as theoretical influences in Douglas’s career.

¹⁷ Communication with the author. Douglas died in May of 2007, not being able to pursue her intentions.

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