IN THE WILD

IN RELATION: AN INTERVIEW WITH MARILYN STRATHERN
EN RELACIÓN: UNA ENTREVISTA CON MARILYN STRATHERN

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ABSTRACT: We are delighted to mark the change of name of our journal with an interview with Marilyn Strathern, one of the most original and influential anthropologists of the last fifty years. The text is in three parts. The first part offers a short historical and institutional background to Strathern's academic life. The second part is an introduction to her work. The third and final part is an interview that Alberto Corsín Jiménez conducted via email with Marilyn Strathern between September and November 2018.

KEYWORDS: Marilyn Strathern; Anthropology; Ethnography; Kinship; Feminism; Melanesia.

RESUMEN: Nos complace celebrar el cambio de nombre de nuestra revista con una entrevista a Marilyn Strathern, una de las antropólogas más originales e influyentes de los últimos cincuenta años. El texto consta de tres partes. La primera parte ofrece una panorámica del recorrido académico e institucional de Strathern. La segunda parte presenta una introducción a su obra. En la tercera y última parte se ofrece una entrevista que Alberto Corsín Jiménez realizó por correo electrónico a Marilyn Strathern entre septiembre y noviembre de 2018.

PALABRAS CLAVE: Marilyn Strathern; Antropología; Etnografía; Parentesco; Feminismo; Melanesia.

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We are delighted to mark the change of name of our journal with an interview with Marilyn Strathern, one of the most original and influential anthropologists of the last fifty years. It is a doubly special occasion for us since Strathern's works are regretfully not readily available in Spanish, there being no translations of her major works into our language. We are therefore taking the opportunity to accompany the interview with a brief overview and introduction to her work in the hope that this will inspire our readers to further engage with the corpus of Strathern's scintillating writings.

This piece is in three parts. The first part offers a short historical and institutional background to Strathern's academic life. The second part is an introduction to her work. This introduction is not intended as a reassessment or analysis of Strathernian anthropology, of which there is a growing and very insightful body of scholarship (for example Gell 1999; Deiringer and Lebner 2008; Holbraad and Pedersen 2009; Edwards and Petrović-Šteger 2011; Street and Copeman 2014; Lebner 2017b). Instead the text aims to offer a provisional introductory framework for new readers wishing to engage with her work, as well as placing in context the interview material that follows. The third and final part is an interview that Alberto Corsín Jiménez conducted via email with Marilyn Strathern between September and November 2018.

I.

Marily Strathern studied archaeology and anthropology as an undergraduate and research student in Girton College at the University of Cambridge in the early 1960s, at a time when both Meyer Fortes and Edmund Leach were lecturers there and engaged in a fiery debate over the structure and organisation of kinship groups. In 1963, under the supervision of Esther Goody, Strathern left to carry out doctoral fieldwork in the New Guinea Highlands, a region that had only recently opened up for anthropological fieldwork. As she has often noted, their choice of field site (for she left with her then husband Andrew Strathern) was partly prompted by New Guinea 'not being West Africa' (Strathern 2009b), because much anthropology, and specially Cambridge anthropology in the 1950-1960s, was dominated by debates inspired by Africanist ethnography. Upon returning from the field, Strathern was offered the post of assistant curator of the Museum of Anthropology and

Archaeology in Cambridge, where she was involved in revamping the New Guinea collection (Strathern 2009b).

In preparing her doctoral fieldwork in New Guinea, Strathern had originally intended 'to study the effect of sibling order on cash-cropping achievement' (Czegledy and Strathern 1992: 2). Upon arriving in the field, however, her attention was quickly drawn to the public life of ceremonial exchanges and dispute settlement arenas. Many of these disputes involved litigation over the role and behaviour of women, which in time became the subject of her PhD and first book, *Women in Between* (Strathern 1972), as well as spawning her long-time interest in gender studies.

Between 1969 and 1976 Strathern lived between Canberra and Papua New Guinea, where her then husband Andrew Strathern had been offered a university professorship (Viveiros de Castro, Fausto, and Strathern 2017: 41). Her stay away from Cambridge these years meant that upon their return in 1976 she had a fresh encounter with a number of schools of thought and intellectual sensibilities that had taken off during this time, including Marxist anthropology and feminism. As she has sometimes commented, her disengagement from these debates up to that point enabled her to enter these conversations from an original perspective of partial detachment (ibid: 42). It was also during this time that Strathern first read Roy Wagner's *The Curse of Souw* (1967) and *The Invention of Culture* (1975), which had a lasting influence on her thinking (ibid: 44).

During her time as an undergraduate student in Cambridge, Strathern was one of a batch of students enlisted by Audrey Richards to collect family histories and genealogies in the village of Elmdon as part of their training in field methods. In the late 1970s, Richards asked Strathern for help organizing and writing up the Elmdon materials that had accumulated over the years, a project which, in time, Strathern took upon herself to complete and turned into the monograph *Kinship at the Core* (1981b).

These two books — *Women in Between* and *Kinship at the Core* — lay out some of the central concerns that have stayed with Strathern over the years. As she has often put it, both *The Gender of the Gift* (1988) and *After Nature* (1992a) were conceived in part as attempts to 're-write' the arguments of, respectively, *Women in Between* and *Kinship at the Core*

(Carsten and Strathern 2014: 278): in the first case in response to developments in feminist scholarship in the late 1970s and early 1980s; and in the second case in light of the cultural shifts propitiated by the entrepreneurial ethos of Thatcherite Britain. The public debate surrounding the Bill for the Human Fertilisation and Embryology Act that was passed in 1990 further prompted Strathern to think about cultural reconfigurations of kinship in the advent of new reproductive technologies, in works such as *Reproducing the Future* (1992b) or *Technologies of Procreation* (Edwards et al. 1999).

In 1985 Strathern was appointed Professor of Social Anthropology at Manchester University, her first departmental job, which required her piloting the department through the government's first full Research Assessment Exercise (a nation-wide, multi-year audit of the quality of universities' research outputs). In 1993 Strathern was appointed to the William Wyse Professorship in Social Anthropology at the University of Cambridge (where she is today Emeritus Professor). Concurrently with her professorship in anthropology at Cambridge, from 1998-2009 Strathern was also Mistress of Girton College. Strathern has often commented that she has an 'interest and love of institutions' (Strathern 2009b), both the demands and possibilities that organised life makes on us, and it is from these various institutional postings that she developed in the 1990s lucid analyses about the looming dangers of the audit turn and the instrumentalization of knowledge in the academy, in works such *Audit Cultures* (Strathern 2000a) and *Commons and Borderlands* (Strathern 2004a).

Over the years, Strathern's perceptive analyses of the socio-cultural transformations of kinship brought to her attention the crucial role that 'relations' have played as both figures of description and figures of analysis in anthropology—for example, when speaking of relationships between kin, friends or strangers (relationships as descriptions) or when addressing relations between the parts and wholes of structures or patterns (relations as analytics). Her inaugural lecture as William Wyse Professor, appositely titled *The Relation: Issues in Complexity and Scale* (Strathern 1995), first alerted her to this curious epistemology of the anthropological relation, but her interest in the playful capabilities of relations have stayed with her since, playing an important role in *Kinship, Law and the Unexpected* (2005), where she reassessed the impact of biotechnology and changing regimes in in-

tellectual property on new family arrangements and indigenous culture. *Relations* (Forthcoming), her latest book, sees Strathern further develop her interest in the relation by exploring its historical and epistemological flights, examining in particular the expositional strategies that have enabled relations to expand the anthropological imagination.

Professor Strathern was made a Fellow of the British Academy in 1987 and a Dame Commander of the British Empire in 2001. She was awarded the Rivers Memorial Medal from the Royal Anthropologist Institute in 1976, the Viking Fund Medal from the Wenner-Gren Foundation for Anthropological Research in 2003, the Huxley Medal in 2004, and the 30th Anniversary Independence Medal from Papua New Guinea in 2005. In 2018 she was awarded the Balzan Prize in Social Anthropology from the International Balzan Prize Foundation.

II.

Strathern's style of anthropological argumentation has sometimes been described as analogous to a 'musical composition' (Benthall 1994: 15), to a silent drawing of diagrams (Gell 1999) or to gardening (Street and Copeman 2014). Some authors have attributed to it a unique 'slowness' (Street and Copeman 2014: 12; Viveiros de Castro and Goldman 2017), an attentive care of detail that throws its elements of description into mesmerizing suspension, into a 'deep hesitation' (Holbraad and Pedersen 2009: 387), during which time they display their singular but also many-faceted potentialities, only to thereafter trigger an accelerating and exhilarating flight of interpretative effects. Strathern's is an artful composition of description that holds the suspension of description as, itself, an analytic effect.

This short introduction to Strathern's writings is offered in the spirit of such deep hesitation. In a well-known analysis of Strathern's work, Alfred Gellonce noted that

> there are three distinct but intertwined aspects to her academic persona: there is Strathern the contemporary cultural critic concerned explicitly with feminism and rather less explicitly with postmodernism; secondly there is Strathern the meta-anthropologist, concerned with knowledge practices and the author of an anthropology of anthropology; and finally there is Strathern the straight anthropologist (Gell 1999: 29).

Whilst the identification of three Strathernian personas offers me, indeed, grounds for hesitation, the triangulation between cultural criticism, the anthropological canon, and the flights of a meta-anthropology, seems, on the other hand, quite a productive space for enabling descriptive suspension. What follows, then, takes such triangulation as a point of departure.

THE STRAIGHT ANTHROPOLOGIST

The straight anthropologist, as Gell calls her, would be Strathern the Melanesian ethnographer and fieldworker, as well as Strathern the student and analyst of contemporary kinship formations.

Melanesia

Strathern is widely recognized today as one of the most sophisticated interpreters of Melanesian social and symbolic practices. *The Gender of the Gift* (1988, hereafter *GOG*) is undoubtedly the work that set Strathern apart as a magisterial analyst of Melanesian ethnographic materials. The book inaugurated a new epoch for Melanesian ethnography, thereafter known as the 'New Melanesian Ethnography' (Josephides 1991). But *GOG* is also, by some accounts, the most important intervention in anthropological theory since the days of Lévi-Strauss, so let me clear some ground before returning to this magnum opus.

Prior to the publication of *GOG* Strathern had already an established reputation as an interpreter of Melanesian ethnographic materials. There are two aspects of that early work that I would like to emphasize here: her writings on the sociology of women, including early interventions in the famous inequality and gender debates of the late 1970s, and her interests in aesthetics and form.

As regards Strathern's engagement with feminist scholarship, this goes in fact all the way back to her first monograph, *Women in Between* (1972). The book offered a detailed sociological analysis of married women's ambiguous position in the Mount Hagen area in the New Guinea Highlands. Women were at once outsiders to their husbands' families (until they borne progeny) and 'outside' their own clan (unless they proved valuable for furthering future exchanges between clans). They were twice outside

and therefore in-between. As Van Baal noted at the time, the book offered 'a perfect analysis' (Van Baal 1973: 518) of a complex kinship tension, and it did so steeped in empirical data.

The afterlife of *Women in Between* had a profound influence in shaping the future directions of Strathern's thought. In 1976 Annette Weiner published *Women of Value, Men of Renown* (1976) where she engaged critically with *Women in Between*. According to Weiner, Strathern's book should be welcomed for being the first study on women in Papua New Guinea, although, she added, Strathern had fallen into the 'traditional male trap' (Weiner 1976: 13) of not taking women's actions seriously. For Weiner, Strathern's book was a study on women written from a male perspective. Strathern has often noted how it took her a few years to come to terms with Weiner's criticism (Czegledy and Strathern 1992: 6), which was eventually published as 'Culture in a Netbag' (Strathern 1981a), but also how grateful she remains to Weiner for supplying her with a 'rapid education' into feminism (Carsten and Strathern 2014: 273).

The richness of the empirical materials showcased in *Women in Between* is rehearsed anew in Strathern's follow-up research report, *No Money on Our Skins* (1975a), a study of young male Hagener migrants to Port Moresby. This book stands somewhat uneasily along Strathern's other works, in that it covers materials and analytics (urban migration and economy, ethnic solidarity, social change) that she has seldom taken-up later. Notwithstanding, we can think of *No Money on Our Skins* as a structural inversion of *Women in Between*: young males who seek adventure in Moresby and in so doing carve for themselves a space of 'autonomy' outside traditional kinship ties. The young urban migrant is in a structural position analogous to the woman in-between. The 'notion of [migrants'] autonomy', writes Strathern there, 'makes an ethic out of instability' (Strathern 1975a: 149), a statement that strikes a chord with her earlier description of women's position of 'in-betweenness' as itself an ethic of instability, one that guarantees an illusion of autonomy and self-sufficiency to the wider kinship system. The feeling for the 'in-between' is a concern that will stay with Strathern in much of her later writings.

What distinguishes both *Women in Between* and *No Money on Our Skins* is Strathern's early reputation as a formidable ethnographer of Melanesian social life. 'The ethnography is brilliant', wrote Nancy Bow-

ers in her review of *No Money on Our Skins*; 'the analysis of the material', wrote Alfred Gell (1972: 684) in reviewing *Self-decoration in Mount Hagen* (1971 co-authored with her then husband, Andrew Strathern), 'suffers from too unremitting a concentration on specific ethnographic detail'. Strathern's past as an overwhelmingly meticulous ethnographer is a point sometimes lost on readers of her later, more analytically ambitious works.

An important point of inflection in Strathern's articulation of ethnographic materials is marked by the publication of *Nature, Culture and Gender*, which she co-edited with Carol P. MacCormack (MacCormack and Strathern 1980). In her contributions to the volume Strathern makes a powerful case for understanding ethnographic complexes not as 'culture done otherwise' but as 'analysis done otherwise', a perspective that was partly inspired by her recent reading of Roy Wagner's *The Invention of Culture* (Viveiros de Castro, Fausto, and Strathern 2017: 44). For the first time she puts forward the figure of Melanesia not simply as an ethnographic region, a casebook for comparison, but as an analytically productive artefact in its own right: Melanesia is not simply a place where sex and gender are done differently but can in fact teach us how 'difference' is itself differently articulated. The book quickly became a source of inspiration for feminist scholarship, whose approach to the sex-gender complex had until that day been very much bogged down by the comparative and structural study of cultural formations.

I noted above that a second area of ethnographic interest in Strathern's early oeuvre is her work on aesthetics and form. *Self-decoration in Mount Hagen* is perhaps her best-known work in this respect. Although Strathern only sporadically has taken up again 'visual culture' as an object of direct study (although see Strathern 2013), I believe her work may be described in fact as an aesthetics *tout court*, for it is indeed towards the elaboration of an analytical language capable of making visible how 'forms' and 'effects' are elicited by social life that much of her work has been dedicated to. There is no better rendering of this aesthetic analytic that Strathern's own definition of it in *Partial Connections* (1991) as 'the persuasiveness of form, the elicitation of a sense of appropriateness' (Strathern 2004b [1991]: 10). In this light, one of the legacies that Strathern the Melanesianist has bequeathed to anthropology is an art of ethnographic redeployment, such that, as Ashley

Lebner (2017a) has noted, the careful composition of re-description becomes an analytical effect in its own right. Analysis, in other words, as the tactical rediscovery of display.

Kinship

One of Strathern's lasting contributions to anthropology has been to rekindle interest in the study of kinship, which had gone into dormancy in the late 1970s following various proclamations about its death as an object of study. We may distinguish two types of interventions on this front: (i) a first intervention marked by the appearance of *Kinship at the Core* (1981b); and (ii) what is undoubtedly one of her most outstanding and innovative contributions to social theory at large: the analysis of the rise of new reproductive technologies in the late 1980s and their impact on the reconfiguration of Euro-American conceptions of consanguinity, biological descent and family ties — as well as their larger influence in shaping contemporary assumptions about 'choice' or 'accountability' (Strathern 1992b).

Kinship at the Core is an extraordinary book, 'a miracle', as Lucy Mair put it in her book review for the *British Journal of Sociology* (Mair 1983: 268). In it Strathern revisited the field notes on kinship ties that Audrey Richards and her Cambridge undergraduate students had produced between 1962 and 1974 as part of their university fieldwork methods course in the village of Elmdon, Essex. Strathern used these materials to analyse how a group of 'core' village families represented to themselves their identity as 'real Elmdon' people. The study brought out the complex displacements afforded by idioms of genealogical and residential identity, and their entanglement in processes of geographic mobility, property relations, class dynamics, rank, and the very notion of 'change' as something that is internally negotiated by people as a 'kinship' factor. In other words, how kinship had become a central operator of modernity: a device for making intelligible the contrast between tradition and change.

Kinship at the Core developed an analytical sensibility that Strathern would later refine into a wholesale anthropological technology: the way anthropologists use (social) relationships to uncover (analytical) relationships. The relationships of kinship became a tool for eliciting descriptive and conceptual relations

for analysis. The purchase of relationality as a 'duplex', as Strathern would come refer to it (Strathern 2005: 7 and passim), allowed her to explore the cultural shifts of late-twentieth-century English culture from a radically novel position.

Published in 1992, *After Nature* testifies to the radical originality of the anthropological technology discovered by Strathern: a book that uncovers the role of kinship as perhaps the most powerful tool available to twentieth century English society for conceptualizing and reconceptualizing its historical predicaments. Kinship, she argues in the book, has worked as a 'merographic' connector, a term used to explain how Euro-American ideas move between domains, now a part to this whole, now a part to a different whole: for example, how kinship individuates the person as a natural kind but simultaneously collectivises it as a relational kind; how it enables discourses about biological life but likewise facilitates and supports parlance of cultural love and affect. The 'keeper of… partial analog[ies]' (Strathern 1992a: 183), kinship has worked thus as the anchor of Euro-American 'plurality'.

Moreover, *After Nature* further delineated a paradigm shift brought about by the arrival of new reproductive technologies, in particular the way they undermined the merographic function played by kinship. No longer just a cultural enabler of overlapping pluralities, kinship has also become now an option for plural choices: when to have children, with what genetic make-up, etc. It has become one amongst many options for 'enterprising' one's life. In this light, *After Nature* describes also the epistemic grounds of our new 'post-plural' society.

Meta-anthropology

As noted above, in her contributions to *Nature, Culture and Gender* (1980) Strathern first put Melanesia under the spotlight as an analytical artifice. Reflecting on Melanesian symbolic and relational practices Strathern manoeuvred to turn Euro-American theoretical reflections inside-out as symbolic conventions. This turning 'inside-out' (Strathern 1979: 253 and passim) of Melanesian and Euro-American knowledge practices would become over time one of her analytical trademarks. For example, in the 'Introduction' to the edited volume *Dealing with Inequality* (1987b), Strathern developed an agenda for feminist scholarship that took care in examining its own internal construction as an anthropological epistemology. Dealing with inequality in gender and sexual relations in the Melanesian region (or elsewhere) became, not a critical routine for delineating hierarchical relations (of power, class, capital, what have you), but a means for analytical recursion *in social theory itself*: a making explicit of 'how we make known to ourselves that inequalities exist' (Strathern 1987b: 2). Such a strategy made visible how far parlance of in/equality presupposed a levelling or re-balancing of domains that up to that very moment were perceived as being discrete or discontinuous: for example, the 'inequality' (the difference) between subject and object, male and female, public and domestic, culture and nature. Moreover, that these various categories are perceived as suitable for an analogical exercise, that they have been thought as capable of cross-pollinating each other, speaks more about our analytical predispositions than those of the ethnographic subjects we are describing: 'Our own concepts provide a structure so pervasive that when we come across other cultures linking, say, a male-female contrast to oppositions between the domestic and wild or society and the individual, *we imagine they are parts of the same whole*' (Strathern 1980: 216 emphasis added). Our analytical equipment, therefore, presupposes already a theory of relations and actions between domains and concepts. The first inequalities to deal with, then, are those that are inscribed in social theory's own analytical perspectives.

In *GOG* Strathern takes up her interest in gender and social analysis afresh and uses the relational/ethnography duplex to mount nothing less than a wholesale challenge to the basic tenets of Euro-American social theory. The book was written in response to ongoing debates in feminist scholarship in the 1980s, in particular what Strathern perceived as an impasse in the configuration of gender and gender identity as qualities owned by bodies and people, qualities, moreover, defined and shaped by the 'commodity logic' — possessive, individualistic, arithmetic — of Euro-American ideology (Strathern 1988: 176-177 and passim). We have reached a cul-de-sac by organizing gender under the logic of the commodity, Strathern expounded, so let's imagine what the gender of the gift might look like.

To this effect, Strathern constructs a methodology and delineates a strategy that drops 'conventional social science understandings' in favour of imagining 'what an indigenous "analysis" might look like if we

took seriously the idea that these islanders might be endorsing their own theory of social action' (Strathern 1988: 150). She does this by owning to three debates deliberately construed to mediate her analysis: an exchange of perspectives between Melanesian anthropology and feminist anthropology; between the 'West' and 'Melanesia'; and between so-called gift and commodity economies.

I can hardly do justice to the wealth of provocations and insights that the book opened-up for anthropology and for social theory at large. 'This is a brilliant, subversive, anticomparative analysis… [that] challenges the universal applicability of categories central to social science', wrote Deborah Gewertz in the pages of American Anthropologist (Gewertz 1990: 797). A 'tour de force', wrote Mary Douglas (1989: 17) in the pages of the London Review of Books, in a review that was otherwise partly critical of Strathern's text.

Perhaps one of the most noted contributions of the GOG is Strathern's critique of the 'individual vs. society' dichotomy. Here Strathern propounds an alternative theory of personhood organized around the notion of the Melanesian 'dividual', a term she borrows from McKim Marriott (1976: 111) to describe how bodies and persons constantly call upon and divide one another. For example, a focus on dividuality helps signal that persons do not 'have' bodies; rather bodies are made visible (they 'have' us) at specific times and places through the claims and relations exerted or demanded from us. Our bodies are divided by, but also sustained through, the relations we have we others. They are relational forms. Similarly, persons do not have gender either; gender is modulated and made concrete (in male, female or otherwise forms) through the different claims or impositions that we make on others and others make on us. We are constantly divided by these appearances and demands. As such, the role that division plays as a radical constant in social life challenges many of the sociological categories — say, labour, property or exchange — that we traditionally resort to for understanding how bodies and capacities act on one another. Thus, Strathern outlines a model of Melanesian sociality where social efficacy is premised upon the aesthetic elicitation of relations — a model that stands on its own accord as an original theory of social action.

GOG unsettled the very foundations of both the anthropological project and, more narrowly, the descriptive artifices of Melanesian ethnography. It was not a book likely to go unnoticed, and it did of course provoke heated debates and reactions. Just as the book spawned a 'New Melanesian Ethnography', there was soon also a body of 'New Melanesian History' calling ethnographers to care for the historicity of their accounts as much as their epistemological persuasions (Foster 1995). Some feminist scholars had also difficulty coming to terms with Strathern's conceptual dismissal of Marxist approaches to inequality, and gender inequality in particular (Josephides 1991).

Throughout the 1990s Strathern's work was also criticized for its orientalisation of Melanesia (e.g. Thomas 1991; Carrier 1992a) and its occidentalisation of Euro-America (e.g. Carrier 1992b). I believe these critiques misunderstood the purpose of Strathern's project, for hers was not an attempt at offering a comprehensive 'representation' of Melanesian social life, let alone of Western societies. Rather, as we have seen, Strathern set out to contrive an analogy between Melanesian and Euro-American symbolic and practical knowledge-systems in order to uncover the epistemological artifices — say, comparison, scale or contextualisation — through which social theory is organized as a technology of interpretation. As Alfred Gell put it, for Strathern '"Melanesia" stands for an intellectual project rather than a geographical entity' (Gell 1999: 34).

The theory of relations drawn out in GOG was radicalised and taken almost to its limit in what is, in my view, one of Strathern's most challenging and inspiring works, Partial Connections (2004b [1991]). If GOG tested the limits of the comparative method by unveiling the symbolic conventions underpinning Euro-American social theory, Partial Connections charts out an itinerary for an anthropology based not on 'comparison' but on 'compatibility' (Strathern 2004b [1991]: 35, 38, 54): where orders or domains of knowledge are not made commensurable but analogous to one another. We can hardly make any piece of knowledge 'inhabit' an epistemic domain, suggests Strathern, for the closer we scrutinize it the more likely we will find further epistemes growing under our feet. One can zoom-in into a problem—say, by moving from country to region, or city, or household—but such operations will never reduce the order of complexity we encounter at any one level, which will simply recompose to remain constant throughout. Such fractal qualities of knowledge require us abandoning any hope for comparability

(comparisons between countries, say, or between households) and search instead for a working model of compatibility. Borrowing from Donna Haraway's 'cyborg' figure (Haraway 1985), Strathern builds a 'composite' theory of ethnographic and intellectual praxis where the challenge is not to 'connect' disparate pieces of evidence, but to elaborate on the (partial) connections already existing. There is no point trying to seek for an external reference point from where to judge the fitness of an intellectual judgment. Rather, judgments seem to work by way of the partiality of connections — the capacity to produce analogies — through which they fit themselves to action.

CULTURAL CRITICISM

Feminism and critiques of the audit and knowledge-management cultures of academia have been two of Strathern's most active areas of cultural criticism. In parallel to this latter interest in the institutionalisation of knowledge-forms, Strathern has developed also an interest in the cultural asymmetries between intellectual property law regimes and indigenous claims to traditional knowledge. Since I have already commented on Strathern's contribution to feminist scholarship, I shall limit myself here to her contribution to the scholarship on audit cultures and intellectual property rights.

As Strathern has often pointed out, her interest in the political rhetorics of persuasion in the academy was a result of her appointment as Professor and Head of the Department of Social Anthropology at Manchester University in 1985, in time to pilot the department's bureaucratic journey through the first Research Assessment Exercise. By the time of her appointment as William Wyse Professor in Social Anthropology at Cambridge in 1993 the entanglement of the professorial 'office' and its 'persona' in regimes of bureaucratic process had already awakened her critical gaze (Strathern 2009a). Some of these concerns were first aired in *After Nature*, where she essayed a critique of 'enterprise' as a cultural discourse about life choices and change in 1980s Britain. In 2000 Strathern edited *Audit Cultures*, which examined the travels of enterprise into the academy and in particular its institutionalisation in ethics boards and administrative regimes of accountability. Building on her distinctive analytical strategies, Strathern compared modalities of 'making things visible' in Mela-

nesian and audit cultures, in particular as they regard cultures and practices of transparency (Strathern 2000b). The book itself opened up new fields of inquiry in the anthropology of organisations, including innovative ethnographic studies on interdisciplinarity or science-society collaborations.

Her work on intellectual property rights may likewise be described as at once synthesizing an established corpus of scholarship (anthropological work on political struggles over traditional knowledge claims and property rights) and inaugurating an original academic field in its own right: the study of intellectual property as a duplex for Euro-American social theory, where law, knowledge, and social relations become ambiguous and contradictory descriptors for talking about the origins or sources of cultural creativity and its transactional regimes and registers (Hirsch and Strathern 2004). Her work in this field has inspired novel anthropological enquiries into, for example, emerging property forms in biomedicine, the study of clinical trials in developing countries, bio-prospecting programmes or community compensation frameworks.

BEYOND ANTHROPOLOGY

Strathern's oeuvre, we have seen, has shaped profoundly and opened new areas of work in all fields of social and cultural anthropology: economic anthropology, material culture, kinship studies, feminist scholarship, the anthropology of organisations, the anthropology of science and technology, etc. But the influence of her work reaches well beyond the confines of the discipline, having been picked up across the social sciences and humanities. Her work on relational theory has made her an indispensable interlocutor for actor-network theorists (Law 1999); her writings on new reproductive technologies, interdisciplinarity and intellectual property have had a profound influence on social studies of science and technology (Biagioli and Galison 2002; Law and Mol 2002) and legal studies (Pottage and Sherman 2010); whilst her work on accountability and knowledge-management has likewise stimulated new lines of work among organisational scholars (Munro 2005) and in the field of education studies (Stronach 2011).

Moreover, Strathern's intellectual contributions have also reached outside the academy, helping shape public debates and policymaking on a number

of topics both in the UK and Papua New Guinea. Throughout the early 1970s Strathern completed a number of research reports for a variety of Papua New Guinean administrations, such as a report on *Villagers' attitudes towards Corrective Institutions* (Strathern 1975c) or a *Questionnaire relating to sexual offenses in the criminal code* (Strathern 1975b).

Between 2000-2006, Strathern sat on the Nuffield Council on Bioethics, and again between 2010-2011 when she chaired the Working Party that authored the report *Human bodies: donation for medicine and research* (Nuffield Council on Bioethics 2012), offering policy guidance on ways to promote altruism in organ donation, whilst making considerations for non-altruistic-donation interventions (financial incentives, benefits in kind) under certain circumstances.

We hope this introduction to Strathern's oeuvre, as well as the interview that follows, will invite readers to relate to her work, for indeed in some respects none other has been the purpose of her writings than *relating* — narrating, connecting, and analysing — the generative effects of the anthropological imagination.

Acknowledgements

III. INTERVIEW WITH MARILYN STRATHERN

Alberto Corsín Jiménez (ACJ): The first question I want to ask is about *The Gender of the Gift* because this year (2018) is its thirtieth anniversary. You have often noted that you wrote the book as a solution to a specific problem, namely, the way feminist debates in the late seventies and eighties were configuring gender as something 'owned' by bodies and persons. Could you tell me a little bit about the situated nature of those debates then, but also about how you reflect on both the book and the development of feminist scholarship and activism these past thirty years?

Marilyn Strathern (MS): Thank you for reminding me of that. I have more recently returned to other problems and solutions with respect to certain usages in that book, for instance the 'dividual' person; this was a (conceptual) solution, as you note, to what I saw as problems for analysis posed by the theoretical and vernacular antithesis of individual and society (Strathern 2018). But I haven't been thinking about the place of *GOG* in the ongoing feminist debates of the time, and it is that larger question you are asking about.

Among other things, the book came out of an impatience with some of the language that seemed to have sedimented after more than a decade of widespread debate. Notable was the language of rights, and the reiteration of subjectivity in terms of 'a person in his or her own right'. That said, the irritation was really directed towards the applicability of such concepts (rights, etc.) to anthropological analysis: the politics to which they spoke was spot on. This was a period when domination was critically articulated in terms of possession, and subject-object relations. To conceptualize oppression as objectification, the notion that one was being treated as a possessible object (rather than in one's own right), circulated as an indigenous Euro-American intervention (looking back to the impetus from Simone de Beauvoir) that gave expression to conditions vividly felt. All this made political sense. When it came to the canard that 'biology is destiny', here anthropologists joined others in criticizing the alleged naturalness of certain dispositions and ways of behaving, which was being discussed in the 1960s as a background to assumptions about inequality. No one at that point saw a solution of sorts in the radical multiplication of 'gender identity' that made nothing an either/or question. *GOG*, as you know, profited from a much wider debate in anthropology over nature and culture, but here it found the vernacular concept of identity an impediment to analysis.

Feminist writing was of course nothing if it was not heterogeneous. The burgeoning number of influential writers in the States, who through the 1970s and 80s were the backbone of what one could collectivize as 'feminist anthropology', could be divided into at least three theoretical persuasions. However I want to make particular mention of a really very avant-garde body of women writers, and pay tribute to Australian colleagues, anthropologists and otherwise, who seemed so ahead of the game then. Looking back on it, I wonder if some of their sophistication did not

come from interdisciplinary reading in (for example) an emergent critical studies, including what was locally called 'critical philosophy'. I am forever grateful to Margaret Jolly (ANU), activist and critic, in this respect. In my head, Olivia Harris (LSE/Goldsmiths) played a similar role in the UK, as did years later Barbara Bodenhorn (Cambridge). I recall being open to criticism (that is not the same as saying I was) partly because of the glimpses of other horizons, partly because those intellectual interchanges helped guard against the limitations of my experience.

In personal terms, I refer particularly to the period during and following an exhilarating year at the Australian National University with a study group (composed largely of anthropologists) on gender relations in the southwestern Pacific, subtitled 'ideology, politics and production' (1983-1984). That was about the time that the journal *Australian Feminist Studies* was getting off the ground. The triangulation that you mention at the beginning of *GOG*, between three sets of relations with distinct social sources in social anthropology, feminist studies and Melanesian ethnography, seems all there in the paper I published in its first issue (Strathern 1985). (This was an early version of an article that appeared in the US feminist journal *Signs,* on the awkward relationship between feminism and anthropology). It received trenchant criticism from Vicki Kirby (1989), a Sydney graduate who was then studying at Santa Cruz.[2]

More reliable than my impressions is the record that actually exists of the impact (on my thinking) of the early years of feminist scholarship and conceptualizations of gender relations. Some of the great bubbling of intellectual excitement, starting with the Women's Liberation Movement of the 1970s, was captured at the time in *Before and After Gender* (Strathern 2016), published as you know in 2016 but written in 1973-1974. You can see there how anthropological commentary was speaking to debates of the time, not least their questions about sex and the (apparent) immutability of biology. Looking back, I see for myself how that book, displaced and put out of mind in the mid-1970s, appeared in some respects — albeit in different form and with different materials — in *GOG* in the mid-1980s. By then I was hoping to reconfigure some of the concepts that informed anthropological analysis. Now Judith Butler wrote a very

generous afterword to *Before and After Gender*, and it was of course her radical re-thinking of the language of gender that changed its performance and opened up quite new trajectories for feminist debate. Meanwhile feminist scholars at large were extending the former trio of concerns linking gender, race and class into involvement with technology, science studies, human-nonhuman relations, and so forth, contributing among other things to subaltern standpoints and de-colonizing the academy. It is as though an original critical impulse, at once social and intellectual, had indeed been a founding moment of what the early Women Libbers had always hoped, spear-heading a fundamental reconfiguration of the conditions of existence and/or knowledge. I am reminded of the ASA Decennial meetings of 1983: while walking to a one-off session on gender relations organized by Shirley Ardener (Oxford), I heard Edmund Leach remark words to the effect, 'Now for something interesting!'.

ACJ: You did doctoral fieldwork in Mount Hagen, in the New Guinea Highlands, in 1963-1964, a region that had only recently opened up for anthropological fieldwork through the work of, for example, Marie Reay and Dick Salisbury. You have often said that your choice of field site responded to New Guinea 'not being West Africa'. I suspect this is an image — the energy derived from escaping and finding liberation from particular intellectual frameworks — that resonates to many anthropologists. Of course, over the years, as we come to know our field sites better, there is the likelihood that this energy will be tamed and domesticated — that it will become like 'West Africa' — not least through our own very work. I wonder if you could say something about this tension, between cultivating interest and cultivating surprise, and the risks that loom from expecting too much from either — in the field but also in writing.

MS: This is a lovely question. There is of course a paradox in cultivating surprise, until one opens out the notion of 'cultivation' into a broad sense of discipline, the notion that one can be trained (for instance) to develop certain kinds of self-awareness. To allow oneself to be surprised — that is, available to the possibility — speaks to a particular disposition or way of attending to events as though they were moments appearing athwart what is already known or already registered in terms amenable to existing categories of analysis.

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2   To which I responded briefly (Strathern 1989a), and then at greater length in the same journal (Strathern 1989b).

The anthropological models of society and culture that I grew up with seemed to be very good at presuming what it was one saw — almost anything could be pressed into a 'social' or 'cultural' frame, as Niklas Luhmann (1990) argued was true for the autopoetic systems of Western institutions (just as 'educational' systems see their environment in terms of what is amenable or otherwise for 'educational' purposes). But then, again, within the framework of a critical enterprise, it was also part of anthropological training to learn how to criticize given categories of analysis (there was nothing not amenable to critical scrutiny). Being able to 'see' (critically) what one is doing touches of course on your work on the devices that social theory uses to reveal its insights (Corsín Jiménez 2013). Here I think you might reserve 'surprise' for what you manage to find in the traps you carefully construct, such as those to deal, for example, descriptively with anthropological description. Only after the trap is sprung, so to speak, will one know what one knows.

But there are other snares along the way to do with what one apparently knows — notably the cluster of reactions to what seems out-of-the ordinary or unusual or unexpected. They do an excellent job of jolting one out of complacency, but they also announce themselves as already knowing what is interesting. Yet it is often the usual or the mundane, what one thinks one already knows, that yields surprise. A lesson for me here was the way audit practices made wholly illuminating the history of a professional practice I had always taken for granted: examinations (as in administering to students thereof).

Then there is what happens when everything is turned into 'knowledge'. You point to the routinization — domestication — of energy released by escaping from existing frameworks, which includes the realization that what one thought one knew is not known after all. One must be prepared 'not to know', and possibly stay in that state for quite a while, a disposition that goes beyond the ignorance that an investigator may assume in order to elicit people's responses to what they might have thought you already knew. (And some people have quite demanding standards about what one is supposed to be able to retain: when, on a visit to Mt Hagen in 2015, I enquired about the identity of people in various photographs I had taken many years earlier, my queries were sometimes met with bemusement. I had been there — the photograph was the evidence of that — so why was I asking?). Surprise is not of course

limited to work in the field, and in the vocabulary of observation and analysis I tried, at one point, to capture the oscillation between the understood (what is analysed at the moment of observation) and the need to understand (what is observed at the moment of analysis) as an 'ethnographic effect' (Strathern 1999: 6 et seq.). While the relation between what is already apprehended and what seems to demand apprehension is infinitely regressive, there are also moments that retain a kind of dazzle beyond their original revelatory force. In other words, they refuse domestication. In fact I wonder if 'Melanesia' does not continue to work that effect on me. Although supposedly the more one knows, the more familiar things become, Melanesian ethnography — whether I am writing or reading others' accounts — seems to maintain its capacity to surprise.

That said, 'West Africa' has now become quite strange and interesting, to the point that I occasionally find it a bit uncanny to encounter phenomena that seem (sometimes) rather 'Melanesian'-like. But then the original impetus was less West African ethnography than what it stood for in terms of anthropological authority at that particular moment of departmental development. If there is a question about the extent to which one is playing back one own's creations (thoughts, images, analyses), then that can only be part of a more general question about how 'authentic' surprise ever is. I am not sure that it matters.

ACJ: In the late 1970s you worked with the archives that Audrey Richards and her students (yourself included) had collected on Elmdon family genealogies and histories. This work culminated in the publication of *Kinship at the Core*. Can you tell us about your experience of working with ethnographic materials collected by others, about the relationship between the archival and ethnographic gazes in that context?

MS: The archival gaze has acquired specific connotations in postcolonial social theory, in terms of its power to authorize specific forms of the future, and so forth. That could also apply to the perpetuation of certain stereotypes, such as might be found in connotations of the English village (e.g. its 'traditional' nature). It would be interesting to consider what stereotypes and assumptions are written into what survives of the Elmdon material (*its* archives), by contrast (say) with what has actually been written about the village — in my own case I treated any notion of 'tradition' as a claim not a descriptive. But I take your

question as one about the kinds of materials at one's disposal.

In fact at the time of 'writing' the Elmdon book I was acutely aware of one dimension that set it off from an analytical or ethnographic venture as ordinarily conceived. (Writing in quotation marks, because although I authored the narrative, it was not only drawn from the notes of many other hands but was a proxy for what Audrey Richards had intended to write). The dimension in question can be simply stated. In the field (whatever or wherever that is), gathering more information than seems relevant at the time, one is exposed to the repetition of apparently trivial phrases. The repetition may in fact be more illuminating than it first seems. I think I was particularly aware of this having recently (early seventies, in Port Moresby) worked with migrants from Mt Hagen who went to the coast (Moresby), ostensibly to earn money. At that time (in Papua New Guinea) a vast national survey was being undertaken by geographers and economists to determine the economic reasons for internal migration of this kind. I wasn't part of it, and wondered what a very different kind of study would reveal. So I would ask Hagen acquaintances about their reasons for coming to the city. The responses were, it seemed to me, invariably trivial — generally a variant on 'because I wanted to'. When I first heard that phrase I thought I was not asking the right questions, and was simply failing to elicit anything interesting; but after having heard it again and again, I realized that it might hold more than was first apparent. (And indeed went on to think about the expression of autonomy that was inscribed in the very act of leaving Hagen). It was just such repetitions that were impossible to glean from the notes on Elmdon that others had made. Not that there weren't echoes, but I missed that exposure to the randomness of information. Less a matter of comparing an ethnographic and an archival gaze, than lacking an ethnographic ear.

That said, the notebooks compiled by various members of the project yielded one recurrent phrase — 'real Elmdon'. The issue was who did or did not belong to a real Elmdon family. And that had come from Audrey's own experience of living as a long term resident of the village; she must have heard it with something of that persistent randomness to which field study exposes one. But as far as I recall it was put explicitly to us students as a research problem. That was partly bound up with the fact that Edmund

Leach, who joined Audrey Richards at the start of the project, had his Sri Lankan village in mind (Leach 1961), which had led him to argue that kinship was an ideological framework for land tenure/land interests. (He made extensive notes on Elmdon inheritance of land holdings).

As to the archives entailed, there were the various notes that Audrey had kept as a resident, as well the notes, essays, surveys, sketches, and so forth, that many others had done, and left with her. I should add that when what expanded from an offer to help her write up a chapter turned into me undertaking the whole volume, she generously allowed me to take it over analytically and theoretically. In the meanwhile she and a colleague Jean Robin produced an account of some Elmdon families, complete with genealogies, having other residents in mind. Robin herself wrote a companion book to 'mine' from a historical perspective.[3] This drew very extensively on archives proper, that is, parish registers and other records, work which partly fed into my own account through Marianne Leach [no connection with Edmund Leach], who was systematizing an original survey for Audrey and undertook primary archival work on family histories. So that material, largely demographic, was filtered through a certain amount of historical expertise, and I treated it much as I would have treated the kind of statistical or survey material I compiled in Hagen, albeit having to take it on trust. Almost everything that went into *Kinship at the core* was in some sense or other second if not third hand — most of multi-authorship was of a collaborative kind, I should add, and as one would emphasize today.[4]

ACJ: My next question builds from the previous one. Anthropologists seem to have an awkward relationship with ethnographic archives. Some anthropologists have long been calling for making ethnographic archives publicly available (subject to the necessary ethical provisions), others are much more wary. Today, new mandates for data governance are adding additional pressures on how ethnographers manage

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3 It might be of some interest to note that I record how she and I diverged in our usages of the terms 'Elmdoner' and 'villager': it was imposed by the material, I say, Jean dealing with residence as it appears in past records and I intending these terms to indicate notions of identity that can only be known from the present (Strathern 1981b: xxxiii).

4 Very much taken for granted then (with of course the liability of being altogether overlooked).

and archive their data. The calls for openness and transparency are problematic, we know that. However, it seems to me that there is a specific issue for anthropology here, not so much about how, when or where to archive (our) ethnographic materials, but about the archival nature of the ethnographic project itself: how we keep records during fieldwork, in whose companionship, in what registers, etc. Many of the people we work with (doctors, scientists, indigenous activists) are keeping and designing archives themselves, sometimes enlisting ethnographers in these tasks. Therefore, I wonder if the archival is not the site where the ethnographic effect is undergoing transformation today. As someone who has been a museum curator, who has worked with other people's archives, as well as kept her own archives, do you have a view on the question of the archival nature of ethnography?

MS: Yes, I really like your question about the archival as site for anthropological transformation. I have had some indirect experience of this, as it were, in the project Eric Hirsch and I undertook on intellectual and cultural property rights (Hirsch and Strathern 2004). I make the connection because as soon as one creates artefacts (notebooks, photographs, transcriptions) one creates a question about how over time these objects re-emerge as property, as records, and records as property. At least, property is among the relations that Euro-American authors have to what they produce, and objects as property demand attention. The older they get the more these artefacts acquire the historical patina of being records as well. All the intellectual property rights arguments that go into how many hands lie behind a scientific article, or the cultural-political arguments about the relationship between author-possessor and those who are the subjects of what has been authored, become acute in the case of ethnographic records. It opens up questions about who perceives 'rights' in or lays claims to specific artefacts, at numerous levels, including questions about the language of rights, as well as about the conditions of original authorship or appropriation, who can speak for whom, and the entanglements of repatriation.

There is no settlement of these issues. To the contrary, there is a good case for arguing that keeping them open to changing conventions and interpretations over time is exactly what makes this area — the ethnographic project — a site of transformation. Every moment of resolution (I have recently made my Hagen photographs available to the Papua New Guinea National Museum in Port Moresby) only meets the perceived demands of the moment, and will have its own repercussions and irresolutions (what about Hagen people wanting to have reproductions?), which in turn may well demand, say, institutional intervention (should they be freely accessible and if so who will pay to make them so?), and local/international conventions concerning restricted and open access.

For what are simultaneously changing of course are recording techniques as such.[5] If electronic devices make the photographic issues easier than in the past, they also reverse the question of accessibility (can anyone have access to them and does that make them infinitely reproducible?). The law (copyright; freedom of information; third party privacy) has ongoing answers that change as society-technology changes, in fact the law's changing goalposts make it a kind of alter-ethnographer: keeping track in order to refine its definitions. Because of the public aspects to creating 'property' out of records, it is no surprise that there are all kinds of protocols that compel institutions, including research funders and professional associations, to put various protections in place. This leads to an *anticipation* of the conversion of notes into records: the future artefact is folded into the making of the present one. Such a process is itself transformative, and no doubt ethnographers these days have to have multiple audiences in mind. I am not sure I have anything to add to the debate — in which I know you have been involved — concerning the multiple obligations of anthropologists in particular. All I shall mention is that, in practical terms, the point where records are often most useful is the very point at which their future status as archives can be problematic.

I am thinking of the humdrum task of analysis, especially when there is a need to compute something. In mind is the way in which I worked out the divorce

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5   There has of course been a lot of anthropological experimentation with digital archives, which open up all kinds of possibilities. For example, where protocol about who can and cannot have access to specific types of information or image is part of the knowledge being conveyed, then protocol can be written into the technical steps of access itself. Equally of course, such procedures open themselves up to infinite regress, e.g. is 'conveying knowledge' what it should be about in the first place? I am afraid I can see the problems more easily than having much insight into the potentials here!

'rates' of two regions in Hagen, based on population details of clan members, including incoming and married-out women, including both living and deceased, and above all including adjudications as to what counted as a marriage. A decision had to be taken in each individual case, which then fed into a globalized tabulation, a routine statistical procedure. I very quickly discovered that it was fatal not to record each decision taken — fatal for the enumerations in hand (not counting people twice, finding out new or apparently contradictory information, and so on). But what is stark in a statistical exercise also applies for any generalizing statement as against the particulars that feed it. One needs such records to guard against impressions, of which orders of magnitude are a prime example; others might need them if ever they wanted to check. At the same time, the personal details of all kinds of people with no direct say in the matter, remain therein.

However this is probably all wrapped up in the prelude to your question, whereas you were asking about the archival nature of the ethnographic project itself. I suspect much of the politics repeats itself, and confine myself to two responses.

First, thinking of the gratitude with which as a museum curator I would fall on any scrap of information (one kind of archive) about artefacts (another kind of archive), was of course to turn both into material for a knowledge regime of a specific kind and for a specific purpose. The appropriateness of those purposes, that is, the use to which the material is put, is open to challenge, that is, to finding new usages for it. We know this from local scholars and activists, among others, and all the 'tacit knowledge'/'indigenous knowledge' arguments. Anthropologists do not have any option but to be interested in those challenges.

Second, there is perhaps a lesson to be learnt from a century or more of ethnographic work that is applicable to any information-gathering exercise concerning persons and their milieux. One might think that the more the world changes, the more remote previous epochs become, not least those of seemingly distant cultures and societies that initially figured in anthropological theorizing. Indeed, anthropologists are acutely aware that their studies engage specific time horizons, and this is the point. It is not necessarily the case that the particular present that researchers bring with them furnishes the most appropriate tools for understanding the materials they

work with. Usually this is expressed in terms of developing paradigms or modes of analysis, so that what appears to be changing is the anthropology, the scholarly theories. But there is another factor altogether, which is to do with the kind of time that researchers live in, or at least the kind of time or epoch that they imagine, and which influences their sensibilities. As the anthropologist moves through time, he or she may find new sources of comprehension. I take an example from legal anthropology. Mid twentieth century debates about dispute settlement in societies indigenously lacking centralized administration had to deal with mechanisms that seemed alien to the metropolitan judicial distinction between criminal and civil law: e.g. forms of mediation between disputing parties, or attention to — even compensation of — injured parties in cases involving assault or death. By the late twentieth century, something analogous to these practices had become formalized at least in British judicial procedure, namely the importance of mediation in domestic matters, and an increasing visibility of and attention to victims. In turn, those remote practices now start looking far less alien; today they would require a different approach. There is a provocation here to reflect upon the correspondence, or slippage, between the epoch of the researcher and what is being researched, and here a century of work archived in the 'ethnographic record' provides food for thought.

ACJ: Your first departmental appointment was at the University of Manchester, where you arrived in 1985 as Professor of Social Anthropology, in time to pilot the department through the government's first full Research Assessment Exercise (RAE). As you well know, the RAE and other metric- and impact-driven initiatives have put a lot of pressure on academics to obtain research grants and publish. It is my impression that in anthropology this can have a particularly perverse effect, since the demands of ethnography call for us leaving for rather long periods of fieldwork, leading in turn to the appointment of younger colleagues in temporary and often precarious positions. I want to ask you about the relationship between anthropology and the university, about how to create virtuous loops between our research culture and our university work—that is, about how to care for the culture of anthropology as an institutional culture?

MS: The issue about how to care for the culture of anthropology certainly exercised me a lot, as it has others of course. There are many forms of precarity,

and perhaps I could refer briefly to one — basic support for doctoral students. All kinds of possibilities that the pre-audit university allowed, simply by being fairly relaxed over the mean by which students made it through a PhD programme, have been gradually cemented over into hard little pathways.

You raise the issue of the time needed for first hand ('field') research. When time was largely unaudited there was some slack in the system, which meant that the exceptionally lengthy periods that students in social anthropology, by contrast with other disciplines, needed to be away from their universities was regarded as a local idiosyncrasy, or as something to be negotiated through the department's own understanding of what it required. (It was assumed it had the expertise). Of course such a degree of trust — and trust of students, let alone staff — could not go unchecked in an institution whose processes were to be 'managed'! So when the Research Councils (Economic & Social Research Council, ESRC, for UK social sciences) began demanding that doctorates be completed within three years, with one year's grace, the local institution in the processes of managerializing itself, responded rapidly to such demands. Thus (in Cambridge, where it became acute) the Board of Graduate Studies also made that time frame an internal requirement. And the pragmatic reason, to do with the number of years for which students received funds and paid fees, became a sign of a new spirit of rational regulation.

Over the 1990s and 2000s I watched how time became ever more tightly linked to money, so the two became perceived as isomorphic. It was no longer possible to play the one off against the other. What had been a reasonably permissive system, with niches for all kinds of arrangements for enabling research students to, or at minimum not preventing them from being able to, keep themselves going and complete a piece of work, was gradually concreted over by the way time and money were locked into each other. Social anthropologist research students had always, for example, been able to rely on the fact that they did not have find fees for the time they were away (on fieldwork, for which there was an — albeit limited — amount of separate support), and that counted against the full fee-paying period. It made the total package much more within reach where funds were sparse. Having to demonstrate that fees were available upfront removed any sense of manoeuvre. The same thing applied to expectations of the time with-

in which a doctorate had to be completed: what had been taken for granted as inevitable delays or interruptions to the whole process, now appeared as yet more evidence that anthropologists' completion rates were problematic, both for the university and for its graduate programme. The Department tried to keep a sense of proportion, and where it could pursued internal funding to assist doctoral students with fieldwork and writing-up costs (this came from diverse bodies within the wider University: while funds were earmarked for social anthropology, they had to be applied for in each individual case). I shall never forget, year after year, the struggle it was to convince the administrators of such funds that a 'fourth year' of funding — following fieldwork of 12-15 months plus preparation and return-time [6] — was not evidence of slackness (on the part of either student or department), but crucial to the whole anthropological enterprise. The prejudice endured. It was dispiriting. I hope it is better now.

All this is recalled for an era now in the past, and I am not up to date on current arrangements. I am not praising that era, for the ambiguities in the system that enabled students to get by (and many wonderful ones!) was also a time of precarious living for them. The Department also put in place a scheme to offer some funding for extra studentship support in addition to Research Council studentships and the like. This was limited, but kept everyone's spirits up. That said, it became increasingly harder for students of uncertain means — in terms of either time or money — to get through a doctoral programme without formal support. The story would have been very different if the government's aim had been to reduce the initial precarity and provide more ample funding in absolute terms so that the aspirant student body could be supported properly. As it was, I gradually saw loopholes closing up. Not good ecology.

I am not sure this quite answers your query about how we best take care. Bypassing the time = money nexus might be a start. One of the implications that

6   Four years was incredibly tight, given the fact that the first year was course work and field preparation, and by contrast with many other disciplines thesis-writing did not begin until after the return from the 'field'. Moreover that return could not possibly be a matter of the student picking up where he or she had left off: fieldwork was *intended* to be transformative of the research project. In any case, wherever the student had been, re-entry was a complex matter.

business-derived models of universities as enterprises never seemed to grasp is that competition among academics is not well imagined as market competition. This does not just apply to anthropologists of course (indeed the well being of anthropologists in the academy has to be part of everyone's well being). Scholars *need* their 'rivals' — for ideas to circulate, for works to be read, for arguments to have bite. Time spent in such communications is not time at the expense of the home institution.

And one other point. It seems to me that there is much more project funding available these days — which does not just take staff away temporarily and encourage a culture of fixed term contracts in lieu, but more positively often also brings into the system funds for PhD students. One of the virtuous loops one might think of is to try to get the university to appreciate that participation in the administration of external (including international) funding bodies is not time (and thus money) taken away from the institution. And that this has nothing directly to do with finding resources for one's own institution, or even country; rather, supporting anthropology anywhere is also supporting anthropology full stop. Without a global view, there is no local one either.

ACJ: I have long been drawn to the generosity of your writing style. For me, your writing performs scholarship through a *response-ability* that is at once attentive and gracious. You rarely respond in a critical vein to other people's writings; instead you work out the terms for an enabling response. You make your texts grow by enabling the growth of other texts. In my mind you perform a style of engagement that is expressive of a particular feminist sensibility, as well as at perpendicular angles with the citational politics of impact and metrics. I wonder if you could say a little about the politics of this kind of responsibility in writing and scholarship, and in particular if you remember specific contexts or situations (or authors or readings) that may have shaped or modulated how you relate to your writing?

MS: Thank you for how you put it! Not everyone, as you know, sees my writing style like that. Whether it is writing style or argumentation (or both) is not quite clear, but there is a wonderful criticism of the 1986 Frazer Lecture, 'Out of context' (Strathern 1987a), from Tyler and Marcus (1987), which suggests I am not in writerly control of my ambivalences and hesitations — and if the need is to change the reception of anthropological work (opening up how we

read it), writers need to see themselves self-consciously *as* writers. I would like to make a momentary virtue of not being in control. While their goal is to open up an environment to the multiple readings of anthropological texts, I think — to a similar end — that I prefer to leave my writing open to allow the unforeseen to make its way in, not least in writings from the hands of others. Only that sounds as though I have a choice! Out of writerly control, I do not.

The politics of acknowledging some of the many hands (minds) in a composition, like the politics of multiple authorship as such, is highly significant, but I am not sure there is a great deal of depth to it in the abstract. Many of the arguments — a couple or more decades ago — about intellectual 'property' held, I think, plain lessons for how ethnographers might begin to think of the manifold degrees and scales of their responsibilities and of the collaborations that their writing implied. I am sure there are imaginative solutions being undertaken these days, including examining the very assumption that having their co-authorship made evident (say) is the most meaningful way in which another's contribution can be recognized. The politics of recognition make that highly equivocal.

As to what has shaped my style, here a black box descends over my head (with extensions for hands, since the words are there too). I have no idea.[7] I suspect this is a knowing ignorance, that is, it keeps self-consciousness at bay. Of course I vary according to audience, but I think I have to feel that I am writing the best I can (whatever the purpose) — another dimension altogether from either the sensibilities of engagement or citational impact metrics. 'Best' has no measure, and certainly wont show up on the reader's screen. In fact that orientation in writing is altogether different from what one brings to reading (I don't read the best I can!).

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7   This is a tad disingenuous since there are some conscious aspects, for example in composition or structure. These include introducing significant terms before they are discussed for their significance so the reader will already have encountered them; deploying enough 'ethnographic' material en passant for the reader to be able to follow things within the compass of the text; interleaving stretches of argument because while one may embark on A before one can discuss AB, one also need to know something about B as well, that is, one comes back to A with B in mind. Of course I do not know if these really work.

ACJ: In your writing you have resorted to the notion of 'Euro-America' as a heuristic device that you deploy tactically to uncover specific epistemological artifices in the organisation of social theory. One of the interesting things about writing in Spanish (or from a Spanish-speaking location) is that Euro-America invokes a very different heuristic space, namely, the geographical history of Spain and Latin America, including its troubled history of empire and colonialism. Re-describing the (conceptual, political, world-making) work that Euro-America has done in this and other cases is part of what the turn to ontology aims to accomplish. However, it is not about such a turn that I wish to ask you about. Following Ashley Lebner's cue (2017a) I would like to ask you instead about 're-description', about the covert work that re-description does and that not always gets explicitated. I wonder if you could comment on the importance of re-description in your work.

MS: Sorry about Euro-American! I shall probably make it worse by saying that the ugliness of the term, and the inadequacy shown up vis-à-vis diverse usages of it, is meant as a deliberate irritant. No-one could be fooled into imagining it did not need to be questioned. (In this it is rather like 'Melanesia'). At the same time one requires some kind of specificity to remind anthropologists, among others, self definitely included, of what we probably don't see very easily, which is just how parochial the place is from which we write. Which is why I wanted something more specific than 'Western'. The debates have moved on now, but that is how the formulation began.[8] As you say, it needs constant re-describing.

I am very much aware that some of your own forays into the Spanish Baroque have been a springboard for thinking about the position of the 'middle third' from which the embellisher of forms realizes that he is re-forming them (Corsín Jiménez 2015). Velázquez was caught between his painting and his choreography of the royal household's interior decoration (he was appointed curator), painting those very interiors in his depictions of royalty. You have him suddenly realize that between 'the pictorial resources employed in the description of insides and the ornamental means with which outsides have traditionally been furnished', he had the possibility of re-embellishing (in the paintings) what he had already embellished as curator. This gave him a perspective on the act of re-embellishment itself, and an insight into re-description. ('Re'-description because it knowingly goes back on, recursively, what has already been described). I think you imply that this same experimentation is refracted though another relation, the friction set up between artwork and spectator. The only thing I would add is that, when re-description is realized in the verbalizations of scholarly work, it also matters what 'description' is there in the first place (what has already been written and is to be re-written). Qua anthropology, in my own usage the concept of description came to be the exposition that encompasses both analysis and theorizing: it is not the beginning but the end-result of such interventions. As a consequence 'description' is displaced from its frequent location as somehow unadorned; as a further consequence, in my usage, re-description is not theorizing-up data or analysing some pre-analysed bit of information. Rather, re-description entails a 'total' (after Mauss) re-configuration of the end result of intellectual work: so the impetus to re-describe has to come from a somewhere else, imagined as another perspective or world, or whatever.

As Lebner observes,[9] anthropological redescription may be accomplished indirectly, through for example the juxtaposition of one description with another. Various possibilities follow that are not open to the household embellisher. If it is an attempt to 'read' one socio-cultural description refracted through another, the two may remain (analogically) in suspension; or one perspective may displace or obviate another. The latter is very evident in the academic context of higher education. Part of the power of audit 're-description', as applied to the scrutiny of academic performance, lies in the nature of the invitation to the participants. They are invited to offer a 'self-description' (of their institution; of their academ-

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8 The best brief 'definition' I have given is in Strathern (2005: 163, n. 1). Edwards subsequently traces the use back to David Schneider's influence, an unconscious adoption on my part of what was probably also in the air at the time (Edwards 2009: 6).

9 I do not go further into her account, compelling and provocative as it is. It is among the stimulating re-descriptions, and critical interventions, colleagues have explicitly offered of my 'work', which you mention at the beginning. I have learnt a huge amount, but would find it too solipsistic to respond with further commentary (responding in the course of this or that argument is another matter); otherwise put, I prefer to keep the relations open. But the present exercise does afford an opportunity to express my numerous thanks.

ic outputs), as though this were an evaluation of themselves, and of their academic accomplishments, rather than a valorization of an exercise that has already rendered them and their institution auditable, accountable, measurable, and so forth, and displaced their aspirations thereby. That they now have new ones, or additional ones, may be welcomed or excoriated.

ACJ: You have long been interested in the 'relation' as an anthropological descriptor. If I am correct, your inaugural lecture as William Wyse Professor first articulated in print your interest in the topic. Do you recollect if there was a specific context prompting your turning attention to the anthropological epistemology of relationality at that particular moment in your career?

MS: The inaugural lecture did indeed tackle 'relations'. But that was partly because it seemed the most obvious thing to do. It was certainly reinvigorated on that occasion by my coming across the philosopher Locke's exemplification of relations through the enigmatic birds from South-East Asia, [10] cassowaries, newly on (live) display in seventeenth century London. They were for him a perfect example for showing that one could have a clear idea about relations (as between a dam and a chick) even if one's idea about the entities involved were indistinct. But that was because I was already looking for relations!

Maybe something that had happened years before has really got under my skin. I certainly think I absorbed from my training at Cambridge, in the early 1960s, the sense that everything was about relations. This was when the concept of 'social relations' was riding high in British Social Anthropology: kinship relations formed a good part of the substance through which they were to be thought, and discriminations between different kinds of relations were the mark of critical acuity. Finding relations, or arriving at a demonstration of interrelationship, was evidently a goal of anthropological enquiry. Of course in retrospect one can see that there was something tautologous about attributing social formations to relations — whether interpersonal or epistemic (that is, to do with conceptual or logical relations) — since those were what one started with. Nonetheless there was

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10 Large flightless birds, cassowaries were familiar to me from Papua New Guinea, but these specimens would not have come from there.

much analytical or theoretical mileage to be gained from showing how particular relations might affect others — whether one was studying totemism or witchcraft or kingly succession, one could show the effects of these particular relations against those… It was all so self-evident in a way, and hardly the prerogative of the Cambridge School to point out. But that understanding is also what tripped me up.

The occasion I recall was my third year (written) examinations as an undergraduate, and a question I thought was just made for answering (fatal that!). I do not recall the actual wording, but it was about the 'importance' of relations in social life. I had a lot of Australian ethnography in my head I seem to recall. The problem was that I did not see the tautology clearly enough to be able to appreciate it, and remember floundering horribly in what at the time seemed the most obvious of questions. All there was to say was 'Yes'! Of course in retrospect the tautology is fascinating, and could do with some cultural unscrambling; my memory is of simply being astonished, in the examination hall, at myself, for not being able to make a good narrative out of the obvious. Maybe I have been trying to make up for it ever since. It certainly seemed an appropriate topic for a lecture that was not just for the Cambridge community and its ancestors but also was for me a kind of re-entry into it.

ACJ: In your recent work you have spoken of the relation as a 'companion concept' (Strathern 2014: 8), because it is an idiom that indexes, especially in the English language, our care for describing how we know ourselves as well as how we get to know about the world around us. The image of companionships is one with considerable hold over social theory today, for example in Haraway's work (2003), but also in idioms of entanglement, assemblages, messiness, meshwork, etc. You have also recently turned to exploring certain expressions of caring companionship, for instance in horticulture and the work of 'transplantation' (Strathern 2017). In your work, however, relations are as much about the work of division and cutting as they are about connecting, accompanying, and eliciting. I wonder if you could say a little bit about how you see the futures of the anthropological relation, the limits and affordances of its modes of companionship today.

MS: Relations can of course mean all things to all people, whether we are dealing with social relations, propositional relations or whatever. So they can be

coloured by context, as for example when I was conversing with Haraway. I wanted to make a connection with what she was arguing, and the companionate nature of making connections sprang from her own vocabulary. (Bricolage!) The idea of 'companion' species has become a way of infusing a certain sense of co-dependence into present-say acknowledgement of symbiosis in its myriad forms, and acknowledgement may lead to an intuition of care. It is precisely because such strings of associations (themselves relations) show how relations can be coloured by context that I am interested in the extent to which specific traditions of thought habitually invest the concept with certain values. These values may serve as default positions, unless the relations in question are specified otherwise.

I think that English language usage, for instance, is prone to positive or benign connotations of relations, which gives the idea of a link or connection something of value in itself, as against disconnection, the very negative giving the game away. So attachment carries positive resonances that may have to be deliberately qualified (as in avoiding over-attachment), and that in turn also make the concept of detachment one for which positive connotations have to be deliberately stated (as in the detachment of an independent third party, such as a judge, from whom one seeks impartiality). This cluster of idioms simultaneously depends upon and changes according to context, all the time, *and* appears to be imbued with qualities that makes neutral usage very difficult. I emphasize this as a difficulty inherent within writing through English: sometimes in relation to what one wants to say, it feels almost as though one were having to translate, adapt, specific English forms. (Describing gender relations made me aware early on of another kind of pull of linguistic expression, what it meant to talk of men doing this or women doing that; I do not think it would apply today in quite the same way, but then the bias in English was not so far from certain Hagen idioms either).

There is a further issue to relation. While the English terms carry positive connotations of tying or bonding, that is, connection, as opposed to disconnection, an anthropologist might point out that the oppositional pair also exists in a hierarchical relation, such that the concept of connection (relation) contains both connection and disconnection. So any kind of 'disruptive' relation is also a kind of relation, and the same applies to division (think of the connota-

tions of 'divisive'), separation and so forth — these are all relations. Emphasizing metaphors of cutting may have been one way to comment on the positive bias that, from an English language point of view, one often has to spell out. This is not insignificant, because these usages affect how we enact interpersonal relations too. At one point Jeanette Edwards and I pondered on the (exclusive) concept of belonging in this regard, which led us to the point that kinship is as much about disconnection as about connection (Edwards and Strathern 2000).

As to the future of the anthropological relation, I am not sure if you mean what at one point I called 'anthropology's relation' (2005), the dovetailing of epistemic and interpersonal relations already mentioned. There is already an extension of it, in a sense, with its interpersonal side figured in inter-species relations, precisely when we think of the beings so related as companions. I imagine that diverse anthropologies will find their own 'anthropology's relation' in diverse languages, drawn from social media perhaps or from ecological crisis. But I guess that behind the logical (epistemic) versus social (interpersonal) duplex was a particular contrast or congruence between a narration (relation) of events and their own dovetailing (relations) with one another: as long as *that* relation is in place, I see no reason for the anthropology's relation to disappear.

As to the immediate future, the bias that might make the English language use of this less than neutral term analytically difficult is also a bias we may wish to exploit. Exactly because the relation gathers so much up into itself, it may be politically very useful. I am thinking about diplomacy across barriers, about the differences we erect into disqualifications, about the rotten political language of self-interest: anything that can give a positive gloss to the notion of interdependence is worth a try.

ACJ: This is my last question. One of the things I have found hugely enabling of your recent incursions into the historical epistemology of the relation is the trouble you take to situate and describe its origins as a duplex descriptor in seventeenth century England (Strathern 2014). I say I find this enabling because for someone like me, who is situated between Spanish and English as both languages and traditions of anthropological description, your move opens up new possibilities for thinking with and across these positions. In a somewhat related way, I was amused to read Alice Street and Jacob Copeman's recent char-

acterisation of your work as displaying a 'quintessentially English' humour (Street and Copeman 2014: 32), which echoed Janet Carten's description of you as a 'quintessentially English' anthropologist (Carsten and Strathern 2014: 264). I wonder if you would care to comment for some of our non-English readers on the work that England/English may be doing in both emplacing and enabling your thinking.

MS: Your last question nicely turns into an expansive possibility from what I fear has a rather narrow base. I am deeply appreciative of non-English readers and colleagues who are themselves fluent in the language, especially when I cannot return the compliment. It is not of course trivial that an international language (for scholarly purposes especially) also has very specific, parochial roots, and as I have already hinted some of my present work is directed at elucidating aspects of the biases that brings.

However, I suspect that you would also find in my writings unexamined references to English(ness) (and implying more than the language). Sometimes they are simply no more signs that I do not want to claim too much. On other occasions they may offer a gloss that cries out for refinement by context, locale, generation or whatever, and especially social class. Or they may draw attention to the fact that given there are so many speakers of English, there are many different Englishes: American English, for instance, has a special place in the world of English English. At other times it signals what self-ascribed 'English' people tell themselves is 'English'. *After Nature* is probably most explicit as regards the latter. It might be worth adding that, although the book appears to range over very diverse pieces of information, there was an idiosyncratic rigour to the selection in that (as far as I recall, and with one exception, a book I borrowed from my mother) everything I used was already in my possession. That included my professional library of course, but also materials that I had otherwise acquired in circumstances that had nothing to do with planning the book. In short, they comprised a 'naturalized', random perhaps, background of sorts.

I should probably be blushing at this point, since that is an admission that I was taking myself as a register for some kind of 'Englishness' (not simply English-born since that would have to include Welsh). I hope I more or less left it at that. I certainly did not go the whole hog and use myself as a sounding board in lieu of investigation. As to the traits that people gather under that head, they are probably best read as stereotypes — and it may or may not be helpful to be rolling them out.

So perhaps I could come back to your readers. I have huge respect for all the work, reading or writing, that is entailed for those for whom English is a second language, and huge admiration for those who make it possible for someone like myself to communicate 'internationally'. Just know that I do not take it for granted.

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