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An interview with Naoki Kasuga

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Professor Naoki Kasuga is the editor of the *Anthropology as critique of reality*. He has worked at Hitotsubashi University since 2010, when he moved from a position at Osaka University. In many ways Kasuga is a unique figure in Japanese anthropology. He is the author of a series of experimental and highly divergent works, and he was one of the translators of *Writing culture* into Japanese. This interview weaves together a discussion of Kasuga's own trajectory with a story of some broader transformations in Japanese anthropology that have led to current explorations of ontology.

CASPER BRUUN JENSEN (CBJ): Professor Kasuga, I should like to begin by asking about your own intellectual history and trajectory. Perhaps you can start by telling me a bit about the path that took you to anthropology?

NAOKI KASUGA (NK): I would have declined to talk about my own intellectual history, say, ten years ago. However, the intellectual situation is transforming more radically than I expected, and by explicating some of the changes I underwent, I hope to impart to younger anthropologists an experiential sense of these changes. In fact, Japanese intellectuals in the 1970s were much more familiar with and knowledgeable about their predecessors' works. For example, in high school I was moved by Tetsuro Watsuji's¹ *Climate and culture* (Eng. transl. 1961), which described a contrast between the West and the East; by Hideo Kobayashi's² *The*

1. A moral philosopher and cultural historian (1889-1960). Several of his books are available in English.

2. A central figure in establishing Japanese literary criticism (1902-1983).

pathos of things (Motoori Norinaga), and by philosopher Kitaro Nishida's *An inquiry into the good* (on 'pure experience') (Eng. transl. 1992). However, I was most fascinated by the novels of people like Osamu Dazai, Ango Sakaguchi, and Yukio Mishima.³ For me, November 25, 1970, the day when Mishima killed himself by hara-kiri, became unforgettable. I was deeply shocked, and stopped reading and studying for months. Perhaps one reason was because I was very embarrassed by the disaccord between his writing and his action. His act seemed absurd, so far from the elegance and intelligence exhibited in his writing. Modifying my favorite essay by Tsuneari Fukuda, I can express my sentiment towards Mishima's action in the following way: "human, an unintelligible being." In that state of mind, I turned to the natural sciences, which seemed much more "loyal" to their readers. But things turned out to be not so simple, for I could not give up on the human and the social. What then would I study? Most disciplines seemed to me suspect in theoretical terms. I began studying economics because of its mathematical rigor. Within a year I became weary of looking for reality in neoclassical theory, and drifted to institutional economics. Gradually, I became interested in the Japanese Marxist economist Uno Kozo, who had reorganized *Capital* by dividing it into theory and history. From there I moved to Karl Polanyi and his readings of Malinowski and Mauss. That is how anthropology entered the picture. The field seemed to me only loosely theoretical and unnecessarily descriptive; still, I felt as if I had no other choice when I advanced to graduate school in 1978.

CBJ: It is a fascinating trajectory, moving from an interest in literature to neoclassical economics and on to anthropology. I would like to pick up on your final observation about anthropology. Could you tell me a bit about the Japanese anthropological climate at the time? What were the central approaches? And why did you consider them "loose" and overly descriptive?

NK: Japanese anthropology, in those days, was just beginning to develop. Especially, the emergence of Lévi-Strauss's new work created very high expectations to the field among intellectuals in general. Anthropology seemed to embody something totally new for those who were fed up with Marxism, historicism, modernism, existentialism, and so on. Say, even if revolution turned out to be hopeless, we could still have a fete!⁴ Well-known anthropologists, whose writings were widely circulated, such as Masao Yamaguchi and Tamotsu Aoki (my then-supervisor who later became a Commissioner of the Agency for Cultural Affairs 2007-9) were very much aware of this and often quoted Lévi-Strauss. But as interesting as their work was, it still seemed to me insufficiently theoretical. In general, we might say that there were two categories of anthropologists: those who had studied abroad and those who prided themselves of remaining 'traditional.'

3. All are major Japanese novelists who published major works between the 1930s and 1960s. Many are translated. Dazai committed suicide in 1948 and Mishima in 1970.

4. This formulation refers to Victor Turner's argument that categorized revolutions and fetes under the same rubric of anti-structure.

Both types, to my mind, were at once ‘unnecessarily descriptive’ (as if staying at the level of recording facts ensures scientificity and interest) and too ‘loosely theoretical.’ The reason, of course, was that both their Japanese and Euro-American predecessors had been overly descriptive and insufficiently theoretical too. Thus, I became bored with lengthy descriptions followed by very modest conclusions holding only for specific cases. Of course, we were well aware of leading anthropological theorists such as Mary Douglas and Victor Turner. They presented interesting models, but their level of abstraction and generality, and their seeming applicability to cases here, there, and everywhere, diminished their explanatory power and theoretical appeal.⁵ Edmund Leach, Marshall Sahlins, and Pierre Clastres all attracted me, but I could not find a way to develop their arguments in novel directions. The two great exceptions were Clifford Geertz and Lévi-Strauss. Those two dazzled me, though they left me, and still leave me, wondering how to actualize their brilliance in my work.

All in all, my problem was that I was too theoretical to be an anthropologist, but too empirical to be a sociologist. Ever since, I have been coping with this situation, which has left me as an anthropologist, in institutional terms, but not in substantive ones. Yet, what other discipline would have been more suited for engaging in social and human studies?

CBJ: Perhaps you can continue to tell a bit about what you *did* as an anthropologist? I am thinking both of empirical focus and theoretical interest here.

NK: I alluded to the fact that I chose anthropology in order to understand humans in their unintelligibility. At first, I imagined that I might explore humanity, drawing on ideas formulated in books like *Argonauts* and *The gift*. Of course, once again, things were not so simple. I had chosen to work in Fiji for various contingent reasons, but also because of a particular conjunction of inspirations. In the Fijian context, Sahlins intersected with Rodney Needham [author of the amazing introduction to *Imagination and proof: Selected essays of A. M. Hocart* (1987)], and with Malinowski as a Melanesianist.

However, during my anthropological training, I felt disorganized both in empirical and theoretical terms. In general, anthropologists were heavily empirically oriented but in a way that seemed always to modify and domesticate the empirical to fit their theoretical preoccupations. My own sense of self-disorganization progressed during my fieldwork. In order to regain a measure of control, I decided to engage in the classical descriptivist effort: to measure and record as many things as possible in one village. Perhaps you imagine my resulting

5. Lest this seems contradictory, consider the two statements “An apple dropped because that is what God wanted” and “An apple dropped in this way because $f=mv^2$.” The former type of “universal explanation” is applicable to any imaginable setting, whereas the latter has built-in constraints in terms of the systems it can be used to characterize. Anthropological and other social scientific attempts at general theoretical explanation are mostly closer to the first type.

first paper on Fiji produced with the tools of SAS (Statistical Analysis System), especially regression analysis.

Later, in the 1990s, I came to practice three styles of writing, each of which could be seen as attempts to deal with the aforementioned self-disorganization. The first was a work of literary critique and deconstruction, dealing with Osamu Dazai, one of the novelists, who moved me a lot in my teens and twenties. It was published in 1998, at a time of increasing confluence between ethnography and literary criticism, which, however, seemed always to produce the same results. On the one hand, ethnographies nourished literary critics, who used them as materials on which to apply their own tools. On the other hand, anthropologists learned from literary critique to become increasingly reflexive and self-critical. But I did not feel this kind of approach was conducive to anthropology. Thus, my approach, was different, in fact almost inverse: I attempted to analyze Dazai's fictions in terms of cultural theories.

The second experimental style I utilized came out of the admirable works by the historian Carlo Ginzburg, who had distinguished between possibilities and proofs. I borrowed from this approach in my work on the most influential socio-economic and religious-political movement in Fijian history, the Viti Kambani movement (2001). My third attempt at experimental style was inspired by Walter Benjamin's work on passages in *The arcades project* (1999), which I used in an effort to produce novel forms of subtle, intertextual effects.

Paradoxically, I was a quite productive, even ardent anthropologist; but I had survived thanks to a random series of somewhat nonanthropological works, including fiction. I could not get rid of a feeling of deception and disaccord in relation to anthropology itself. Hence, I became obsessed by the aporias inherent in attempts to distinguish between the empirical and the non-empirical, the logical and the illogical.

CBJ: Your explanation is quite intriguing because it defines your anthropological quest in paradoxical terms—as an effort to understand the unintelligibility of humans! Yet, qua anthropologist, your effort is presumably still to somehow bridge such differences. However, your answer evokes a sense of continued futility. Perhaps you can speak a bit to how you did move on with your experimental works?

NK: Let me see how far I can get with a response. I believe that what is required to deal with the situation is, in a word, *awareness*. It is an awareness that entails questioning the very way of posing anthropological questions. To modify Wittgenstein, before we ask “yes” or “no,” we need to explore whether it concerns anything intelligible. It is more appropriate to accept the possibility that a question may be inherently unanswerable than to presume that it must be answerable. It is even more adequate to consider how one's questions can be transformed into other questions. I admit this sounds banal. I suppose everyone has similar experiences; even if we believe in The Beatles' idea to “take a sad song and make it better,” we may be unable to actualize it. My sense of actualization was enabled through an exploration of the way in which humans, as self-conscious beings, rely

on *delay*. It is based on a sense that the “I” who thinks can only catch the “I” who was thinking there-and-then, but not the “I” who is thinking here-and-now. My feeling of disaccord could thus be transformed into this issue: there was an irresolvable disaccord between what I *thought* as the empirical and the logical; between how I *felt* the empirical and the logical. This is the disaccord between the ‘I’ who thought and the ‘I’ who is thinking. In newer writings, I argued that a sense of delay underlies and influences our thinking but remains largely underexplored. I attempted to elucidate this situation by tracing two prominent lines of contemporary philosophy (i.e., Kant-Deleuze and Heidegger-Derrida), making the argument that each line defines a different style of grappling with delay. I developed this argument by weaving “delay” into my work, in the form of seemingly unrelated phenomena and writings such as Hamlet, Polanyi, Dazai, Viti Kambani, Japanese freeters,⁶ and so on. The addition of Japanese thinkers to this palette would have enriched these analyses, but I did not do so, with the exception of Hideo Kobayashi, whose impact wouldn’t leave me. Other Japanese scholars offer distinctive ways of thinking, which, though moved by Western thinkers, definitely present other ideas of delay.

These explorations were related to my emerging interest in the precursors of “the ontological turn,” such as Marilyn Strathern, Roy Wagner, and Eduardo Viveiros de Castro. While my book *Delayed mind* (Okure no Shiko) (2007), was in print in 2006, I began reading Strathern intensively. I knew from two old friends of mine who also worked on Fiji—Hirokazu Miyazaki and Annelise Riles—that Strathern’s work was very powerful, but I had not found a way to really *enjoy* it. However, while I was writing *Delayed mind*, I came to realize that her writings since *The gender of the gift* (1988) exemplified how unanswerable questions can yet be turned into other (unanswerable) ones. I was especially struck by the introduction to *The gender of the gift* where Strathern described her ambition to break through the stalemate in anthropology set by deconstructionism, feminism, and empiricism. Her dissolution of one form of analysis by means of another form impressed me, and induced what I call a ‘Strathernian transformation.’

Unlike Lévi-Strauss, Strathern refused to resort to any essentialism, while resolutely staying in the orbit of the undecidable distinction between fiction and nonfiction. She reminds me of the genius mathematicians who demonstrated the completely unexpected possibility of transforming one equation (or function) into another, thereby generating a set of new equations. In this analogy, the equal signs are partial connections. Equations are questions that call for solution. Strathern sets up new equations, and probably even proves them; while also leaving them unsolved. This ‘Strathernian transformation’ is an extraordinary accomplishment!

Subsequently, I tried to locate her work in relation to Bruno Latour, Michel Foucault, and Gilles Deleuze, and eventually this brought me in contact with the volume *Thinking through things* (Henare et al. 2007). Through this work, I came across the writings of Eduardo Viveiros de Castro.

6. In the *White Paper on the National Lifestyle*, published by the Japanese government, “freeter” is defined as young (aged 15–34 years) part-time workers and the unemployed, who are neither students nor housewives—i.e., people without “the will” to work.

CBJ: In the Euro-American context, the scholars you have just mentioned are of course prominent inspirations for Henare et al.'s "quiet ontological revolution." But it strikes me that the guiding force in your story does not sound *prima facie* "ontological"; that is, in classical terms, as concerned with the "nature of being," but rather as a way of dealing with the intractable relations between the conceptual and the empirical, which would often be seen as epistemological. I ask not least because you refer to Strathern and, as far as I am aware, she has never spoken of ontology. Therefore I would like to ask you about your grouping together of Deleuze, Latour, Strathern, and Viveiros de Castro. In which sense do they belong to the same territory in your reading?

NK: You are right that the guiding force which led me to these people can be described as unanswerable questions about the conceptual and the empirical and also, let me add, about thinking and feeling, and even language and sense. This is where normal philosophical distinctions between epistemology and ontology lose their meaning. It is where the accordance between what we think, what we know, and what is there collapses. All efforts to identify, denote, and represent become senseless. This is where the Strathern transformation works and where Deleuzian differentiation proceeds. It is also the space from where Latour advocates nontheorizing through a flat ontology. And it provides the impetus for Viveiros de Castro's aim to direct anthropology toward "ontological realms" (that is, realms, such as Amazonia, where the nature of being differs from the West or Japan) thereby *inserting a difference* into our sameness. In this sense, these authors share a similar situation that each attempts to break through in his or her own way. For me, it was striking that Strathern is the one British anthropologist among this group of French (or French-inspired) thinkers. According to orthodoxy, she locates her anthropology as more empirically oriented (which, in fact, I believe it is), and this made me pause to consider Strathern's work with special attentiveness. Of course, French scholars such as Tarde, Foucault, Deleuze, and Latour have been much more explicitly linked to ontology, in spite of their differences. Viveiros de Castro is the crucial link, since he was the one who brought this French lineage into Anglophone anthropology.

Yet, I would like to emphasize that we should be careful not to simplify Strathern and Viveiros de Castro. Both begin their writings under the very condition where the factual and the fictional, the conclusive and the interrogative, lose their distinct features. And yet their writings are not amorphous: they present something positive. They argue "A and B and C and . . ." instead of "not A, not B, not C, not . . ." Thus, their analyses bring something *into being*. This, too, offers a sense in which their work can be called "real" or "ontological." But their writings are, of course, notoriously unsuitable to reduce to formulas. While I found such terms as the "ontological turn" and the "quiet revolution" fabulous, and also expect them to work as banners, it is not easy to delineate exactly what they stand for. Thus we need to keep in mind how the scholarly portraits of these two figures are framed and modified, and for what purposes; especially for the work conducted under the banner of the "ontological turn."

CBJ: This takes us directly to the publication of your recent edited volume, *Anthropology as critique of reality*, which introduces the “ontological turn” in the Japanese context. What kind of “quiet revolution,” if that is indeed what it is, do you hope that these ideas can do for Japanese anthropology? What are they a banner for, and what is it wielded against?

NK: The origin of the book can be traced back to 2007, to the time when I was still wondering how to develop a Strathernian kind of anthropology in Japan. When I came across the striking phrases “quiet revolution” and “ontological turn,” I felt that I “got it.” These phrases elegantly described things occurring in anthropology since the late 1980s. Yet, I was not certain how they could be put to use in Japanese anthropology. Over a three-year period, between 2007–2010, I organized four annual seminars, and the *Anthropology as critique of reality* volume is the outcome of this endeavor. Needless to say, this was a collective process, which would have been impossible without the encouragement and support of many colleagues.

In particular, I worked with a group of young Japanese anthropologists who were well versed in science and technology studies (STS). They were familiar with Bruno Latour and also had an interest in Marilyn Strathern and Alfred Gell. I don’t think we reached a clear-cut conclusion or consensus. However, we did gain some confidence that we were moving in an exciting direction. Whether it is the right direction remains unknown, of course. So the book presents our various ethnographic and anthropological arguments relating to the works of Strathern, Latour, Viveiros de Castro, and so on. Obviously, each reflects the specific intellectual history of its author. Perhaps you already have a rough idea of what our banner was wielded “for” and “against.” It was raised simply in order to create a field different from the one that has cultivated such familiar concepts as “glocalization,” “essentialization,” “appropriation,” “strategy,” etc.

As a final note about Japanese anthropology, I should also highlight the impact of *Writing culture*. To this day the impact of that book has remained so strong that I felt it necessary to elucidate our direction in contrast with so-called postmodern anthropology. This is one of the points I make in the introduction to the volume.

CBJ: Maybe I can pursue the difference between this new work and postmodern anthropology by asking about the title *Anthropology as critique of reality*. Contrary to *Anthropology as cultural critique* (Marcus and Fischer 1999), the critique in your title is not concerned with cultural representations of reality, but with reality itself. Even so the notion of critique is surprising, since this terminology is not typically affiliated with Euro-American explorations of ontology. Could you reflect on what is implied by critiquing reality? To what ends should reality be critiqued? And how does this differ from the critical position of postmodern anthropology?

NK: I attach strategic as well as substantial meanings to the word “critique.” Strategically, “critique” marks an important difference from postmodern

anthropology in the Japanese anthropological scene. Somewhat boldly, my introduction posits three historical stages through which anthropology has moved since the late nineteenth century: roughly divided, 1860s–1920s, 1920s–1980s, and 1980s–. The first stage was characterized by research into distinctive spheres of cultures and the natural historical construction of a new science of humanity. The second can be depicted as the flourishing of monolithic analytical wholes, and the claim to explain sociocultural and economical-political complexes by analogically modeling them after the idea of nation-states. What we are witnessing in the third period is the proceeding meltdown of these existing spheres; a decomposition that requires us to reconsider *becoming* based on topics rather than anthropological divisions and subdisciplines like economic or political anthropology. The postmodern anthropology of the 1980s reflects the transitional aspects of this world and our discipline from the second to the third period very well. It developed its own critique against the second period, despite sharing similar images and concepts. In the title of *Anthropology as critique of reality*, the word “critique” is meant to evoke the possibility of differentiating the effort of the book from postmodern writings. In this sense, my use of critique follows the common usage in philosophy—i.e., a methodological practice of doubt. Such a “critical” perspective needs to be retained in order to emphasize and address the abyssal situation into which the anthropology of today has been thrown. And, in that regard, it is particularly incumbent that any superficial interpretation of ontology is avoided. So by ontological I do not mean anything transcendental. The notion of ontology, in my view, is fruitful precisely in situations where our generic distinctions between the theoretical and the empirical have become, or are becoming, fundamentally problematic. This problem extends to the aporia that arise whenever one attempts to differentiate ontology (What is in the world? What is the nature of being?) and epistemology (How do we know?). This is why an anthropological approach has to be at once critical (i.e., exert constant vigilance and methodological doubt) and ontological while retaining, as ethnographic, the question of what is the nature(s) of being(s). In other words, reality cannot be adequately anthropologically elicited without being ontological and critical at once, since an ontological orientation necessarily implies questioning what the object is *while* also asking who (or what) is the subject attempting to grasp it.

CBJ: Thanks so much Professor Kasuga. Here at the end, I wonder if you have any parting thoughts about where these new tendencies are taking us, or ought to be taking us. What would be your future aspirations for anthropology?

NK: Who could possibly know the future of anthropology, or any other human and social science? These fields of inquiry have been precarious enough to discourage any future predictions, not to mention strong hopes! Just as no one could predict how Nietzsche would turn out to be exploited by Deleuze, it is almost impossible to say how “partible personhood,” for example, will be or ought to be taken up by coming generations. Ideally, I would keep on making these tendencies as self-elusive as possible so that they can trigger fruitful engagements in the future, no matter what form they might take. In this sense, I am ready for their ineluctable deviation.

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Un entretien avec Naoki Kasuga

Résumé : Le Professeur Naoki Kasuga a dirigé l'ouvrage *Anthropology as critique of reality*. Après avoir enseigné à l'Université d'Osaka, il travaille depuis 2010 à l'Université de Hitotsubashi. Il est une figure relativement unique au sein de l'anthropologie japonaise, auteur de travaux expérimentaux et hautement exploratoires, et l'un des traducteurs de *Writing culture* en japonais. Cet entretien associe à la discussion de la trajectoire de Kasuga une histoire de certaines des transformations importantes de l'anthropologie japonaise qui ont abouti aux explorations actuelles de la notion d'ontologie.

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Casper Bruun JENSEN is Associate Professor at the *Technologies in Practice* group, IT University of Copenhagen. He has published in *Common Knowledge*, *Critique of Anthropology*, *Science, Technology and Human Values*, and *Social Studies of Science*. He is the author of *Ontologies for developing things: Making health care futures through technology* (Sense, 2010) and *Monitoring movements: Recursive partnerships and aid infrastructures* (MIT Press, 2013) with Brit Ross Winthereik. He is the coeditor (with Kjetil Rødje) of *Deleuzian intersections: Science, technology and anthropology* (Berghahn, 2009). He is also currently part of the Environmental Infrastructures project, conducting ethnography among Japanese hydrologists.