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Peter Schröder

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Theodor Koch-Grünberg with Macúna, Yabaána and Yaúna warriors at the lower Río Apaporis. Photograph by Otto Schmidt, 1905. (Credit: Nachlass Theodor Koch-Grünberg, Ethnographische Sammlung der Philipps-Universität Marburg, Germany, No. KH\_H\_II, 88)

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# Caçando os devoradores. Agência, “meninas indígenas” e enquadramento neocolonial

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José Miguel Nieto Olivar

🏠 *Universidade de São Paulo, Faculdade de Saúde Pública, Departamento de Saúde,  
Ciclos de Vida e Sociedade | São Paulo, SP, Brasil*  
✉ *escreve.ze@gmail.com*

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## RESUMO

Este artigo é resultado de uma pesquisa antropológica sobre gênero e territórios de fronteira conduzida desde 2010 na Amazônia brasileira. Aqui me aproximo da *violência* através de uma análise das relações entre “indígenas” e “brancos” no enquadramento da construção de uma cidade amazônica, como expressões de políticas corporais (neo)coloniais. Focando na perspectiva de mulheres indígenas que habitam a cidade, presto atenção a seus envolvimento conjugais, sexuais e econômicos com o “mundo dos brancos” (incluídos os corpos, a cidade e o Estado). Estes envolvimento são entendidos em termos de *agenciamentos* indígenas e generificados: a capacidade dessas jovens de lidar com, resistir, sofrer e se apropriar dos bens, dos presentes e dos corpos do projeto colonial.

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## PALAVRAS-CHAVE

Gênero, sexualidade, violência, exploração sexual, Amazônia, fronteiras.

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HUNTING THE DEVOURERS. AGENCY, “INDIGENOUS GIRLS” AND THE NEOCOLONIAL FRAME

**ABSTRACT**

This paper is a result of the anthropological research on gender and borderlands that I have conducted since 2010 in the Brazilian Amazon. Here, I will address violence through an analysis of the relations between “indigenous” and “white” peoples through the frame of a fabricated Amazon city, as expressions of (neo)colonial body politics. Drawing from the perspective of urban indigenous women, I will focus on their conjugal, sexual and economic engagement with the “white man’s world” (the bodies, city and the State included). This engagement is understood in terms of gendered indigenous agency: the capacity of these young women to deal with, to desire, to resist, to suffer, and to appropriate the goods, the gifts and the bodies of the colonial project.

**KEYWORDS**

Gender, Sexuality, Violence, Sexual Exploitation, Amazon, fronts/borderlands.

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*El movimiento de la imagen o del texto fuera del confinamiento es una especie de “evasión”, de manera que, aunque ni la imagen ni la poesía puedan liberar a nadie de la cárcel, detener una bomba ni, por supuesto, invertir el curso de una guerra, sí ofrecen las condiciones necesarias para evadirse de la aceptación cotidiana de la guerra y para un horror y un escándalo más generalizados que apoyen y fomenten llamamientos a la justicia y al fin de la violencia.*

Judith Butler (2010: 26-27).

*La poésie antillaise sera cannibale ou ne sera pas.*  
Suzanne Césaire, 1942 (apud Mascot, 2015: 110)

**INTRODUÇÃO**

Era manhã do dia 14 de novembro de 2016. Antes das 7h eu já estava tomando café da manhã no Mercado Municipal da cidade e conversando com as minhas interlocutoras. De repente, um rebuliço de gente foi se formando em frente a um terreno desabitado, a alguns metros dali, na avenida principal. No meio do matagal, foi achado o cadáver de uma jovem. Nas vozes que rapidamente elaboravam e transmitiam a notícia, e nas fotos que os transeuntes tiravam e depois circularam por *WhatsApp* ou *Facebook*, aparecia o corpo nu de uma adolescente indígena, machucado, ferido, com a farda e o material da escola jogado a seu lado. Sarah, de 17 anos, proveniente do trecho alto de um dos rios que formam a Bacia do Rio Negro, foi “estuprada” (segundo todo mundo) e assassinada por “asfixia, estrangulamento e agressão” (segundo registro da Secretaria Municipal de Saúde)<sup>1</sup>.

<sup>1</sup> Elimino aqui os nomes e outras marcas de identificação ou sobrepesquisas de pessoas e lugares. Contudo, os procedimentos de proteção de identidades nesse trabalho, com as implicações éticas e de segurança inclusive para o autor, não são nada fáceis, pois envolvem especificidades locais e regionais, dados de conhecimento público (seja no plano da informação local, seja da mídia regional e nacional), bem como diversas escalas e âmbitos de leitura.



Mais uma, pensei. Mais uma, falaram algumas pessoas. Essa morte, que até 2017 ocupava um lugar nebuloso nos registros, ou, melhor, que nos registros oficiais havia sido novamente reconfigurada pelo apagamento do estupro, juntava-se a tantas e tantas histórias de abuso, violência e morte. Ouço tais relatos nesta cidade desde 2010, vindos de mulheres indígenas habitantes dela sobre si mesmas ou sobre amigas, vizinhas, parentes<sup>2</sup>. Sobre essas violências, ou sobre as relações marcadas por nós e outros atores com o termo violência, era comum ouvir que “sempre aconteceram”.

Na forma e persistência dessas mortes e violências, e dos julgamentos morais e temores atrelados, é possível ver a explicitação de um *frame* (em termos de Judith Butler, 2010), de uma gramática (Das, 2007) que coloca nos corpos destas moças os fazeres e destruições do projeto “civilizacional” e são efeito da materialização performativa do *Estado, da governamentalidade*<sup>3</sup> e seus “processos de fronteirização” (Grimson, 2003; Albuquerque, 2015). A primeira parte do artigo visa *enquadrar* essas violências no marco de invenção e fabricação desta cidade como parte de um *embate colonial* (McClintock, 2010) específico.

Compreender propriamente essa condição de *embate* requer ir para além da compreensão do *quadro* e da denúncia das violências que o constituem. Assim, meu objetivo neste artigo é começar a compreender os agenciamentos<sup>4</sup> femininos-indígenas, especialmente juvenis, que emergem no e contra o marco gramatical de medo, morte e segregação através do qual forças de poder específicas produzem e governam a cidade. Dito em termos de Butler (2010), interessa-me compreender formas através das quais estas moças se relacionam com um *frame* que não faz suas vidas apreensíveis enquanto dignas de luto, mas as coloca naquele matagal em que morte violenta e reprodução biológica e social definem a estrutura e o cotidiano dos seus corpos e perspectivas.

Levando em consideração uma bibliografia sobre *sexual economies* (Cabezas, 2009; Piscitelli, 2016; Olivar e Garcia, 2017), bem como o espaço simbólico ocupado nesta cidade pela “exploração sexual”, argumento, na segunda parte do artigo, que as práticas de intensificação dos fluxos e circulações sexuais e econômicas entre mulheres indígenas e homens brancos são especialmente férteis para pensar estes agenciamentos. Esse argumento fica mais claro à luz do trabalho de Anne McClintock (2010), que evidencia como o espaço de encontro entre sexualidade, gênero, racialização e negociações econômicas constitui o coração operacional, moral e estético do *frame* colonial. Por fim, espero mostrar como determinados agenciamentos, para além de *resistências*, têm a possibilidade de reorganizar as relações na longa duração do ordinário (Das, 2007) e, inclusive, atrever-se a formas de contraefetuar ou *canibalizar* a “devoração” colonial de que são objeto.

2 Entre 2010 e 2013 realizei pesquisa pós-doutoral sobre os mercados do sexo na fronteira urbana entre o Brasil o Peru e a Colômbia; entre 2013 e 2015 participei de um projeto coordenado pela Dra. Adriana Piscitelli, sobre as políticas de combate ao tráfico de pessoas, incluindo no Norte do Brasil; e entre 2014 e 2017 fui responsável por um projeto Jovem Pesquisador que visava aprofundar a compreensão das fronteiras Amazônicas em termos de gênero e de sexualidade e das articulações entre dinheiro e Estado (Fapesp: 2013/26826-2). Todo esse processo de pesquisa esteve institucionalmente ancorado no Núcleo de Estudos de Gênero – Pagu/Unicamp, e, na cidade site deste artigo, contou com a parceria cada vez mais ativa de organizações sociais locais/regionais, principalmente, de mulheres.

3 Sigo as elaborações sobre poder, Estado e governamentalidade de autores como Foucault (1980, 1995, 2008), Butler (2004, 2009), Sharma e Gupta (2006), e Das e Poole (2004).

4 Ahearn (2001) entende “agência” como uma ideia aberta: a capacidade socialmente mediada de agir. Porém, prefiro noções mais, digamos, fortes, seguindo as elaborações de Montgomery (2001) – no marco do estudo de “crianças que se prostituem” –, de Ortner (2006) e de Strathern (1990). Particularmente, seguindo essa última autora, agência não é uma espécie de capacidade individual, mas um efeito relacional que diz respeito à capacidade [de relações] de “impactar” [to impinge on] em “âmbitos de eficácia” [ranges of efficacy] imaginados como específicos e diferenciadores (Strathern, 1990: 93). Sobre agência e gênero em pesquisas sobre a guerra e a memória, ver Theidon (2011).

## ENQUADRANDO. CIDADE, VIOLÊNCIA E GRAMÁTICA COLONIAL

*São Gabriel  
Dos meus sonhos és formosa  
Eras tranquila na lembrança de um passado  
E hoje entre o progresso que passa  
Marcham no ritmo viril acelerado.*

*As cachoeiras que apreciam agradáveis,  
Por entre as pedras águas a passar  
Seus murmúrios infinitos e saudáveis,  
Vejo as espumas brancas engrossar.  
(...)*

*São Gabriel formosa e radiosa  
De jovens valentes  
De praia tamanhas,  
De belos pássaros  
E lindas montanhas.  
Terra hospitaleira,  
Vibrando em progresso  
E esperança mil,  
Um provir te espera  
Na integração do nosso Brasil.*  
Hino de São Gabriel da Cachoeira.  
Letra: Terezinha Violeta de Jesus.  
Música: Irma Sal. Rosa Godoy Quintão.

São Gabriel da Cachoeira (SGC) está localizada na região conhecida como Alto Rio Negro, no noroeste amazônico. A maior parte da região corresponde a terras indígenas demarcadas de forma contínua, além do Parque Natural do Pico da Neblina (ISA, 2011). Dos 37.896 habitantes do município em 2010 (estimativa de 43.831 para 2016), 29.017 se autodeclararam indígenas no último censo (76,57%), configurando-se como o município com a maior quantidade de indígenas no país. Segundo essas autodeclarações, do total da população municipal em 2010, cerca de 50% era população residente urbana (19.054 pessoas) e, destes, 11.016 (57,8%) eram indígenas<sup>5</sup>. A cidade é tipicamente apresentada como uma cidade indígena, e a “urbanização” e a relação entre “comunidade” e “cidade” são centrais na produção antropológica e nas discussões políticas indigenistas (Lasmar, 2006; Andrello, 2006; FOIRN/ISA, 2005; Lubel, 2015; Marques, 2015). Essa centralidade da cidade está diretamente relacionada com a forma particular como

5 Fontes: IBGE (2012); ISA (2011); IBGE cidades, disponível em: <http://www.cidades.ibge.gov.br/xtras/perfil.php?lang=&codmun=130380&search=amazonas|sao-gabriel-da-cachoeira>, acesso em 17/01/2015.

essa cidade foi inventada e fabricada no final do século XX<sup>6</sup>.

A cidade costuma ser narrada como tendo raízes numa “maloka de índios Passés” e na implantação, por parte de Portugal, do Forte de São Gabriel em meados no século XVIII. A partir daí, dá-se a lenta gestação de um povoado atrelado à presença colonial - forte, freguesia, posto comercial-, que em 1861 chega a 33 casas (César, 2015: 30, 32). Criado como município em 1891, no início do século XX foi descrito pelo etnologista e explorador alemão Koch-Grunberg como “um ninho miserável, quase sem habitantes...” (*apud* Ibid: 36). É a partir de 1916, com a chegada da Missão Salesiana à região, que são implantadas as condições de possibilidade para o gradual crescimento da cidade e para sua posterior fabricação política e militar.

Com uma forte vocação para a educação e para a gestão do trabalho, os salesianos implantaram um sistema de internatos indígenas no Alto Rio Negro (nove entre 1916 e 1968, sendo o de São Gabriel o primeiro) onde concentraram crianças e jovens em “campos de estudo e trabalho” (César, 2015: 80) agrícola e urbano. Esses assentamentos missionais deram a forma definitiva ao processo de “civilização e catequese” que vinha sendo frustrada desde os séculos anteriores (Wright, 2005; César, 2015) e representaram o primeiro investimento sistemático de construção de cidades (indígenas) na região. A partir daí, São Gabriel da Cachoeira intensificou sua posição como povoado comercial, de serviços e de intercâmbios diversos, fornecendo, por exemplo, indígenas “civilizados” e “educados pelos padres” para serem soldados, trabalhadores, pilotos, pedreiros, babás e empregadas domésticas dos grandes comerciantes da região.

As missões salesianas, em cujo centro estava a produção de cidades, de economias, de terras e de corpos “civilizados”, foram agradecidas com orgulho nas palavras do ex-governador do Estado do Amazonas, Artur César Ferreira Reis, no livro “*De Tupan a Cristo*” – autobiografia do sucesso salesiano:

*As construções que levaram a termo, de tijolo, pedra e cimento, modificando a paisagem material, eram uma lição às populações para que se orientassem na conquista de um bem-estar menos precário, ao lado dos templos, residências, hospitais, educandários. Nas malocas indígenas, alteração total das condições de vida, desde a moradia, que deixou de ser taba rude para ser a casinha de madeira, à organização social, moral e espiritual elevada, dignificada e preparada para o ingresso na vida civilizada à sistemática de trabalho, à utilização do gênio industrial e artístico que revela a generalidade do gentio amazônico (César, 2015: 71).*

Contudo, seria apenas nos anos 1970 e pelas intervenções do governo militar brasileiro – no caminho do “progresso” e da “integração” – que a cidade de São Gabriel começaria a se tornar o que hoje é. A Lei Federal 5449 de 1968 “enqua-

6 Sobre a história da ocupação da Bacia do Rio Negro ver Wright (2005), Azevedo (2003) e Andrello (2006).

drou” São Gabriel da Cachoeira como área de segurança nacional (Ibid: 44), entrando no mapa do projeto governamental de construção de infraestrutura e das grandes estradas que abririam definitivamente os territórios da floresta e dos indígenas e expandiriam a fronteira agrícola brasileira até seus últimos limites nacionais – como de fato aconteceu no centro-oeste do país e no sul da Amazônia (Velho, 1972; Torres, 2005; Galetti, 2012). O projeto foi a construção dos subtrechos da estrada BR-307, entre São Gabriel e Cucuí, e da estrada Perimetral Norte, BR-210, que chegaria à cidade de Benjamin Constant, na tríplice fronteira do Brasil com a Colômbia e o Peru.

Assim, entre 1973 e 1974, o 1º Batalhão de Engenharia e Construção (BEC) foi inteiramente transportado da cidade de Caicó, RN, no nordeste brasileiro, para construir não apenas estradas, mas um assentamento militar permanente e uma cidade “escoadouro” (César, 2015: 207). Junto com o BEC, chegaram as famosas “firmas” construtoras: a Empresa Industrial Técnica –EIT, a Queiróz Galvão, além do Departamento Nacional de Estradas e Rodagens, a LASA, entre outras. Entre 1970 e 1980, a população passou de 785 para 3.102 habitantes, sendo que entre 1974 e 1976 foram registradas seis mil pessoas residindo na cidade (Santos, 1988; apud Lasmar, 2005).

*Máquinas e homens entraram mata adentro, **rasgando a selva amazônica, anunciando o progresso e o desenvolvimento** (...) com a certeza absoluta do cumprimento da missão (minhas ênfases, César, 2015: 209).*

A missão (das rodovias) não foi cumprida. Em 1977, a Perimetral foi abandonada, enquanto a BR-307, de aproximadamente 200 km, foi concluída sem pavimentação e somente quinze anos depois do início das obras (César, 2015). Porém, a outra missão foi plenamente cumprida: inventar e construir uma cidade, um assentamento de fronteira, através do estabelecimento de uma força militar para o controle e a produção territorial e populacional, que tem o efeito de atualizar em performances cotidianas a mitologia da *fronteira* (Serje, 2005) e do *sertão* (Galletti, 2012). Essa geografia do “vazio” e da segregação atravessou a própria construção da cidade, pois a propriedade da terra foi redefinida, orientando a organização urbanística atual. O Decreto-lei No. 86, de 30 de novembro de 1977 destina para o uso do Ministério do Exército as terras que foram “doadas” à União (César, 2015: 203). Diversos moradores antigos da cidade lembram com precisão como as melhores terras do povoado, que ainda não tinham sido colonizadas pela Missão, foram “apropriadas” pelo exército e pelas firmas na sua chegada. Nelas, o exército construiu suas vilas, seus clubes, as casas-grandes dos altos oficiais, suas sedes de treinamento, etc.

Seguindo a escola salesiana, a cidade se abria como um dispositivo de cria-

ção de novas paisagens, de novas relações e de novos corpos, num enlaçamento entre transformação física, substancial e econômica. Sexualidade e alimentação, e marcas corporais de raça/etnia, cruzavam-se com racionalidades monetárias intensificadas para produzir o novo mundo do “progresso”. A memória de um comerciante e ex-funcionário do BEC citado por César ajuda-nos a ilustrar as formas da fabricação e da inserção alienígena, produzindo não uma bolha econômica paralela entre “firmas” e exército (como acontece em alguns enclaves de extração), mas uma nova razão de socialidade em *embate*:

*O Batalhão quando aqui chegou instalou logo um supermercado. Trouxe de tudo para vender, com exceção de ovos, frangos, verduras. Era tudo enlatado, carnes enlatadas, naquele tempo já traziam cerveja para vender pra gente, refrigerantes, tudo descartável. A nossa alimentação quando chegamos aqui era tudo na base do enlatado, era bordon, quitute, sardinha, feijão, arroz, essas coisas, mas o que a gente mais gostava era do charque (César, 2015: 202).*

Esses milhares de soldados e trabalhadores eram fundamentalmente homens não-indígenas e sem conjugalidade, que fabricaram a cidade como materialização etnoracializada e generificada do progresso, da integração e da civilização. Uma forma de compreender essa noção de cidade é elaborada por Lasmar (2005) em termos de oposições de raça, etnia e gênero, e faz referência à concorrência de jovens mulheres indígenas por potenciais maridos brancos. De um lado, aquelas com melhor capital no mercado matrimonial seriam as habitantes de bairros mais tradicionais, imaginadas como “menos índias”, mais “misturadas” ou “caboclas”, de etnias mais “civilizadas” ou de famílias cuja relação com a cidade é mais antiga. Do outro lado, aquelas dos bairros mais recentes, procedentes de rios ou regiões mais distantes, meninas “mais indígenas”, de famílias recém-chegadas. Nestes últimos bairros, imaginados como periferias, em que com tanta facilidade se falam as línguas das cabeceiras dos rios Uaupés, Tiquié ou Içana, os “brancos” chamam a atenção e a experiência social do “racismo” se faz de obrigatória expressão<sup>7</sup>.

Então a cidade foi e é produzida como espaço performativo de lógicas de socialidade e de poder baseadas na articulação assimétrica do gênero, da raça/etnia, da procedência regional e nacional, da posição no sistema de colonização e, gradualmente, do conhecimento do dinheiro e de outros saberes e objetos dos “brancos” (Andrello, 2006) ou da política indigenista (incluído o saber dos “projetos”, como as pessoas em São Gabriel rapidamente identificam). Uma verdadeira “fazenda de domesticação” (Leirner, 2012). Nos termos de Taussig (1993), como centro de escambo e “escoadouro” comercial – também da “exploração sexual” –, essa cidade seria o espaço em que a ficção do comércio e o fetiche da dívida se

7 A relação entre cidade e raça/racismo foi marcante na experiência de campo. Uso aqui a palavra “racismo” como o termo êmico através do qual uma mulher de etnia tukano me interpelava uma noite quando, junto a algumas amigas indígenas, eu bebia cerveja nos bares de um bairro da periferia. Para ela, um “branco, bem branco” como eu não deveria estar aí. “Você não sente nojo de estar aqui?”, disse ela. “Nojo? Por que sentiria nojo?”, respondi. “Pelo... racismo”, disse ela concluindo.

materializariam. Conforme McClintock (2010), seria uma versão muito particular (amazônica, de pequena escala e alto impacto regional) de um espaço colonial de exposição fetichista das mercadorias (de uns e outros) e seus efeitos mágicos (de uns e outros...), como referência civilizatória para toda a região rionegrina<sup>8</sup>.

É na fabricação e gestão desta cidade, no “rasgar da selva amazônica” que se localiza a base material, o sentido e a memória mais recente daquilo que “sempre aconteceu”. Vejamos.

Um ex-soldado do BEC comenta para César que, na época da chegada das “firmas”, os moradores tinham medo dos soldados. A Igreja havia avisado os pais que “estava chegando um bocado de **devoradores** na cidade” (minha ênfase, César, 2015: 202). “Nós fomos discriminados inicialmente”, diz ele (Ibid). É fascinante como se reproduz com extrema facilidade a celebração da missão civilizadora do “progresso”, e como nela a assimetria e a violência das relações são borradas com igual facilidade por uma retórica de emoções masculinas individuais (pioneirismo), associada ao mito-conceito da *fronteira* (Serje, 2005): “nós também tínhamos medo”, “ouvimos falar sobre os índios que comiam gente”, “ouvíamos de colegas que morreram com flechas envenenadas”, “elas [as índias] eram loucas pra casar com a gente...”. Essa retórica, do outro lado, é atualizada pela Igreja, importante agente colonial e civilizacional, para inicialmente se diferenciar de seu novo aliado-e-concorrente – “os devoradores” – a partir da gestão moral do medo sobre o consumo/gozo dos corpos femininos indígenas.

Nas palavras de D. Inácia, indígena de etnia tukano na casa dos 70 anos e moradora antiga da cidade, é possível ver as formas e traduções da violência (mas também, e já nos conectando com toda outra dimensão, *os deslizamentos “doidos” do desejo*):

*Nesse tempo chegaram outras pessoas da firma (...). Chegaram cinco turmas. Tinha muita gente brancos. Ficava cheio no caminho: “vamos dançar, vamos dançar”, de cabelos compridos, enrolados... e eu ficava com medo, ficava muito com medo deles... E eu não saía mais... Mas depois sim... depois **fiquei doida e queria era fugir do restaurante** [em que trabalhava]. (...)*  
*...e tinha agora várias casas de festas. Era cheio de pessoal nas festas... e não tinha indígenas, só de fora e pessoal daqui que já morava. Polícia andava **caçando** as mulheres que saíam nas festas e as que andavam muito nas festas eram chamadas para ir morar no puteiro<sup>9</sup>, por isso me deu tanto medo e eu não saía mais. Elas aguentavam um mês, dois meses e não aguentavam mais... elas morriam. (...) E assim alguns [“peões”, soldados] trabalhavam e iam embora, voltando pras cidades deles. Engravidaram pessoal daqui e foram embora... (...)*  
*No tempo que as firmas chegaram, houve muitas mortes, estupravam, principalmente as meninas que saíam fora de hora. Abusavam, levavam na estrada. Até no*

8 Para melhor compreensão desta organização do poder estatal-militar, veja Leirner (2012), e na sua relação com família, esposas de militares e vilas militares, veja Silva (2016). Para uma discussão sobre a condição urbana e a retórica da “hierarquia”, veja Leirner (2014), e para melhor compreensão da organização política civil e com participação indígena mais recente, veja Lubel (2015).

9 D. Inácia e seu marido contam da existência de dois prostíbulos associados à presença das “firmas” em que trabalhavam fundamentalmente moças vindas de fora: o P1 e o P2.

*cu delas eles faziam... elas acabavam morrendo sem ser enterradas, na estrada...  
(...) É assim que aconteceu naquele tempo, que eles engravidavam, mataram...  
E são os filhos que estão agora praticando esse tipo de crime<sup>10</sup>.*

A cidade, pois, não apenas foi fabricada por tijolos e comidas enlatadas, mas também a partir da cofabricação prática de corpos que assim se tornavam generificados e etnoracializados. Nessa fabricação, para os corpos das mulheres (apenas indígenas, nas memórias e nas narrativas), sobrava esse lugar que vamos aqui descrevendo. Dona Inácia, casada há mais de trinta anos com um homem indígena, teve (ao menos) dois filhos de homens de fora, que também tiveram filhos com outras mulheres locais, exemplificando a operação polinizadora da “Integração Nacional”.

Desde os anos 70 até hoje, essas relações se estenderam e tomaram, primeiro, a forma de violência sexual e gravidez indesejada (ou não reconhecida) e, depois, de exploração sexual e estupro. “Em 1994, a FOIRN [Federação das Organizações Indígenas do Rio Negro] encaminhou uma carta-queixa ao Comandante do 5º BIS (Batalhão de Infantaria de Selva), responsabilizando os militares brancos e solicitando providências” (Lasmar e Azevedo, 2004: 5). Mas não se tratava apenas de militares. Lasmar (2005) e Lasmar e Azevedo (2004) identificam também o lugar dos taxistas (não-indígenas e vindos de fora) nestas narrativas já no final dos anos 90 e, em todas as minhas idas à cidade (2010, 2014 e 2016), eles ocupavam um lugar privilegiado nas narrativas sobre assédios e estupros<sup>11</sup>.

Em 2010, soubemos que todos os integrantes da Polícia Militar da cidade estavam sendo investigados por crimes sexuais. Também foram abundantes as narrativas sobre “abusos”, “orgias” e “explorações” de garotas indígenas menores por parte de militares e, principalmente, de comerciantes locais poderosos, que vinham sendo denunciadas desde 2008.

Esse conhecimento veio a público de forma espetacular em 2013. Na época, uma investigação da Polícia Federal (PF) chamada Operação Cunhatã revelou “uma suposta rede de exploração sexual de menores indígenas” (Polícia Federal, 2013). Nela teriam sido ouvidas 16 mulheres adolescentes e pré-adolescentes na faixa dos 13 aos 16 anos e foram capturadas nove pessoas: três comerciantes de uma mesma família, um militar, um ex-vereador e duas mulheres indígenas que seriam as aliciadoras das “garotas” (Severiano, 2013). Apesar do espetáculo e do aparente sucesso da operação, em 2016 os três grandes comerciantes estavam de volta às suas atividades sem que nenhuma grande represália jurídica ou social tivesse acontecido. Nada, fora a própria Operação Cunhatã e as imagens associadas, parecia exceder ou quebrar uma gramática conhecida. Nas palavras de um representante da Igreja Católica, foi a captura desses três irmãos que mais impressionou na cidade, pois sobre suas práticas, “todo mundo sabia”. Nos

10 Minhas ênfases. Entrevista realizada em 5 de outubro de 2016 na casa de D. Ignácia, em São Gabriel da Cachoeira, e traduzida posteriormente do tukano para o português por uma professora da mesma etnia (mantenho o anonimato também da tradutora).

11 O transporte massivo mais comum na cidade são “lotações”: taxis particulares que pegam pessoas na rua até completar a lotação e as deixam no lugar que cada um quiser. O valor entre 2014 e 2017 era de R\$ 3,00 (U\$ 1,00). Os motoristas eram tipicamente homens “de fora”. Em 2016, em conversas realizadas com mulheres indígenas entre os 13 e os 17 anos, estudantes de uma escola pública na periferia da cidade, todas elas, sem exceção, manifestaram grande medo e desconfiança sobre estes homens.

termos que o caso nos foi explicado em 2014 por diversas mulheres locais, essas violências “sempre aconteceram”.

Além das moças ouvidas durante a investigação policial e das muitas que me contaram em 2016 ter vivido assédios desses mesmos “velhos”, houve uma narrativa que mudou inteiramente minha percepção da escala destes “crimes”. Uma mulher indígena, por volta dos 45 anos, habitante da cidade e vinculada à luta pelos direitos humanos, explicava-me como ela própria, na sua adolescência, havia sido assediada por pelos irmãos quando caminhava entre a escola e sua casa.

– Os mesmos?

– Os mesmos – respondeu ela, e complementou afirmando que nunca aceitou os convites ou foi forçada, mas que amigas e vizinhas dela foram.

– Queres dizer que há mais de 20 anos esses mesmos irmãos vêm assediando, abusando e explorando garotas indígenas na cidade?

– Pelo menos um deles, sim.

Os grupos dominantes da política e do comércio são conformados majoritariamente por antigos soldados e trabalhadores das “firmas”, bem como por seus herdeiros e seus principais clientes: os militares vindos de fora. Os três maiores centros de comercialização de alimentos, de bebidas e de outros bens de consumo básico, assim como outros comércios (como a distribuição de gás) e diversas propriedades, pertencem aos três irmãos vindos de fora, “brancos”, principais acusados na Operação Cunhatã.

A história destes três irmãos, que é publicamente conhecida e não é excepcional, serve para se fazer uma ideia das formas gramaticalmente aceitas em que o poder é exercido em SGC, das possibilidades de atuação dos agentes político-comerciais e das noções de “comércio” em jogo (em uma declaração que ecoa os termos de Taussig, um delegado da PF, na ocasião da Operação Cunhatã, descreveu os acontecimentos como um “comércio”). Famílias de comerciantes de longa data têm ocupado quase de forma hereditária o poder executivo local, bem como cargos legislativos. O comércio da cidade lucra dos salários dos funcionários públicos, principalmente militares, e, seguindo uma tradição que remonta ao século XVII (Wright, 2005), da exploração dos corpos, produtos, necessidades e dinheiros indígenas. Pontualmente, no desejo e na dificuldade para acessar determinados alimentos, ferramentas, bebidas alcoólicas e combustível, o endividamento dos indígenas com os comerciantes, inclusive de comerciantes indígenas com comerciantes brancos (Andrello, 2006), implica uma forma nada excepcional de relação e de exercício do poder. Dívidas, pagamentos, dádivas e terror se encontram na composição moral da pessoa no marco dessas economias coloniais, e no cruzamento de fetiches sobre o mundo dos brancos.



*Tudo, nesse sistema, repousa na aparência de um comércio, no qual o devedor não é nem escravo, nem trabalhador assalariado, mas um comerciante, sujeito à férrea obrigação de pagar adiantadamente. (...) Com a mesma frequência com que esse relacionamento era colocado no primeiro plano – um relacionamento entre comerciantes –, transformando-se em algo difuso, que desembocava na escravidão, da mesma forma a terminologia está sujeita a saltos mortais. Nesta semiose desordenada, quem teria condições de afirmar quem era credor e quem era devedor, para não falar daquilo que tornava um homem devedor e daquilo que transformava a dívida em um homem? [...ou em uma mulher...?] (Taussig, 1993: 79).*

Essa lógica de exercício do poder-comércio, que é generificada e etnoracializada na origem, tem então uma dimensão geracional em torno da sexualidade. Pelo visto, na Operação Cunhatã e nos seus desdobramentos, esta forma de relação de poder de “brancos” sobre “índios” e de algumas etnias sobre outras, tem um correlato de gênero e geração na assimetria violenta de transações sexuais em que corpos de meninas indígenas são tomados pelos homens “brancos” à força ou sob a ficção de um pagamento ou de um dom. Essa ficção abre para sempre um espaço de complexidade e de ambiguidade, que Taussig descreve através de uma pergunta: “Mas se a força bruta era aconselhável [possível, diríamos no nosso caso], porque eles se incomodavam em dar presentes e persistiam na ficção da ‘dívida’?” (Taussig, 1993: 44).

Por fim, antes de passar à seguinte seção, gostaria de arriscar alguns elementos analíticos, ainda preliminares, sobre estas violências. Do que estão nos falando essas violências se, como Segato (2003), pensamos na sua dimensão semântica e comunicativa? Ou, seguindo Gregori (1993), o que essas violências produzem na sua dinâmica mais relacional? Devido à evidente falta de dados oficiais e etnográficos, podemos afirmar mais tranquilamente algo que essas violências não são. Elas não têm o efeito principal de “limpezas étnicas” e, apesar da tenaz militarização, não correspondem exatamente a estratégias de uma guerra<sup>12</sup>. O povoamento, parte da estratégia militar de colonização, se alterna com a simples corporificação temporária – generificada (Strathern, 1990) – do *poder de morte* (Foucault, 2008) ou do poder de endividar, atrelado a formas ambíguas do “endoidecer” feminino (como nos lembrará D Inácia sempre). Apesar de ter componentes de punição sexual/moral (como na história dos “puteiros” que D. Inácia menciona) e apesar de realmente implicar uma transformação étnico-racial que pode tender ao “mundo dos brancos”, não há como afirmar que se trata de um programa de estupro punitivo, de branqueamento biológico do inimigo, de abortos obrigados, de *war brides* (Cho, 2008) ou de apropriação de crianças para a guerra ou a indústria.

**12** Existe uma extensa literatura, especialmente posterior aos anos 1990, sobre guerras étnicas e formas de violência sexual e de gênero com interpretações feministas de orientações diversas. Algumas partem da correlação entre homens e violência, e sexo e opressão, e outras questionam tais vínculos e usam a ideia de “gênero”. Veja por exemplo: McKinnon (1993), Olujic (1998), Bell e Narayanaswamy (2003), Eriksson e Stern (2009), Milillo (2006), Segal (2008), Peres (2011). Veja também Segato (2003, 2013) sobre violação, morte e gênero. Em outro trabalho abordei essa questão (Olivar e Pacheco 2012), que não é foco principal da análise neste texto.

O primeiro apontamento, então, é dizer que aquilo que “sempre aconteceu” marca práticas diversas em escala massiva (gravidezes indesejadas ou não reconhecidas, estupros, assédios, abusos e “explorações sexuais”) e que a enunciação “Isso sempre aconteceu” não é uma metáfora, uma referência ao argumento do patriarcado universal, ou uma sensação imprecisa de *opressão*. Trata-se mais de uma afirmação referente a relações específicas, com corpos, raças/cores/etnias e até nomes próprios, que persistem no tempo, estão enlaçados nas estruturas comerciais e políticas da cidade, e são hereditárias<sup>13</sup>.

O segundo elemento que deve ser mencionado (talvez como um programa de estudos por vir) é que não apenas os homens “brancos” aparecem como atores destas violências. No estudo antes referido de Lasmar e Azevedo (2004), homens jovens indígenas, pobres, habitantes da periferia da cidade, foram indicados por adultos indígenas, homens e mulheres, como agentes de violência urbana. Igualmente, em baixo volume e partindo de fontes especializadas, houve sempre a informação de estupros, em ocasiões coletivos e continuados através de filmagens em redes sociais, que responderiam às hierarquias indígenas rionegrinas. Por outro lado, são abundantes as queixas de mulheres indígenas sobre a violência (agressões, insultos, espancamentos, xingamentos) exercida por seus maridos ou por outros homens sob efeito de bebidas alcoólicas. De início, seguindo Theidon (2011), na ritualização dispersa dessas violências, é possível pensar que os homens (em relações do mesmo sexo com tolerância para etnias cruzadas) constroem suas alianças, o reconhecimento mútuo como “devoradores” e seus desejos cruzados, no marco da assimetria mítica e estrutural<sup>14</sup>. E nessa reiteração performativa se atualiza a assimetria socialmente compartilhada e a relação colonial no contexto urbano – base das demais transações e constituinte dos fetiches cruzados<sup>15</sup>. Devido à variação de sentidos e de contextos, de relações e de planos cosmológicos, morais e políticos, resulta muito delicado atravessar essa fronteira analítica sem dados suficientes. Por tal motivo, tenho me concentrado nos rastros destas relações violentas que colocam uma polaridade gramatical entre homens “brancos”, vindos de fora e, principalmente, adultos, e mulheres indígenas, principalmente em fases de juventude e crescimento. Contudo, é importante lembrar que estes atravessamentos e conexões existem e que pousam de forma particular sobre os corpos das “meninas indígenas”.

Seguindo o movimento, sabemos que não é apenas através dos homens que a violência contra as mulheres se reproduz<sup>16</sup>. Sugiro, então, que esses gestos performativos, reiterados massivamente por décadas (aquilo que “sempre aconteceu”), não dizem respeito à “prostituição” e nem apenas à linguagem da aliança ou da violência como excepcionalidade. Deles não derivam represálias públicas ou grandes mobilizações políticas de mulheres<sup>17</sup>. No conjunto, seu efeito tem a ver, justamente, com a atualização compartilhada de princípios de socialidade,

13 “Filhos de comerciantes” aparecem recorrentemente como agentes de violência sexual contra garotas indígenas.

14 Um homem liderança indígena se aproximou de mim no alto de uma noite de caxiri, veio me fazer um reclamo. Segundo ele, eu, que estava o tempo inteiro com “suas” mulheres, deveria agenciar para eles – homens lideranças – aquelas “brancas” minhas amigas. Em outra ocasião, na mesma noite de cerveja em que aconteceu a enunciação do “racismo” antes referida, um homem que conheceu recentemente, “metade índio, metade branco”, despediu-se de mim contando que ia atrás de uma “bocetinha de 12 anos que me ofereceram”. Nessas ocasiões, forma-se uma aliança presumida entre nós, baseada em gênero e na heterossexualidade, que temporariamente passa por cima da *etnia* e de qualquer outra diferença.

15 Na verdade, se levamos em consideração o conhecimento sobre parentesco na região do Rio Negro, devemos pensar que gênero e geração são também dispositivos estruturantes do sistema social de hierarquia, de marcadas assimetrias de poder “antes” ou “à margem” da relação intensificada com “o mundo dos brancos” (Hugh-Jones, 2001; Andrello, 2006; Lasmar, 2005). Para uma análise de gênero do Vaupés colombiano, atravessada por uma leitura crítica à construção masculina do pertencimento étnico, veja Rossi (2016). Veja também Oliveira (2016), para perspectivas femininas e centradas em casas. Por outro lado, a bibliografia rionegrina exhibe a reprodução das narrativas míticas de origem do mundo baseada na “hierarquia”, em que “os brancos” assumem de início um lugar de alteridade absoluta, dominante e fetichizada; uma

gênero e parentesco, intensamente carnais e libidinais, afirmados no processo colonial republicano. Trata-se de uma atualização que viabiliza de forma particular relações cruzadas ou contínuas de gênero, geração e etnia; isto é, que possibilita a versão rionegrina das relações de *contato* e de reorganização cosmo-política (não contabilizando com facilidade estas práticas violentas – sexuais e de gênero – na pauta da política indigenista, inclusive feminina).

É justamente nessa torção argumentativa que devemos buscar as formas de *agência* e de apropriação por parte destas mulheres jovens.

## E AS MULHERES?

### MILLENA, 22

31 de outubro de 2013 às 04:12

*Sou moradora da cidade, e melhor do que ninguém, sei que esta notícia está bem mascarada, não estou falando que os acusados são santos, **mas, acredite, as meninas envolvidas são bem menos santas que eles.** Ao contrário do que dizem, que são pobres coitadinhas e inocentes, (...) **estas meninas estão em casas de festas, na beira da praia ou em qualquer outro local se oferecendo, geralmente bêbadas, com microroupas, no intuito de engravidar e ganhar uma boa pensão como geralmente acontece. Se não acredita, pergunte a qualquer mulher de militar** (...) todos conhecem a fama dessas garotas, que até o presente momento ainda **estão se prostituindo na cidade, e não em troca de bombons ou guloseimas como disse o Dr. Delegado** (...).<sup>18</sup>*

O comentário acima é a resposta de uma “moradora da cidade” a uma das notícias sobre a Operação Cunhatã. Ele funciona como uma passagem clara para o mundo destes agenciamentos que me interessa privilegiar. Vemos nele uma reelaboração moral feminina dos eventos, dos corpos e das relações envolvidas na Operação. O comentário aponta para as práticas noturnas e sexuais de jovens mulheres indígenas e como implicam uma ameaça para as “esposas dos militares”. O que apresenta nesta seção, portanto, é um mergulho nesse espaço intersticial e ambíguo no qual essas jovens performam seus corpos, seu gênero, suas sexualidades através da gramática descrita. Através das explicações de Rosa, uma jovem indígena, e em conexão diacrônica com as memórias de juventude de D. Inácia, avanço na compreensão destas formas de *agência*, suas possibilidades e limites.

Neste artigo, meu foco não está colocado nas *reações* e *resistências* organizadas perante os eventos mais espetaculares de violência. Interessa-me mais um plano de agenciamentos, que diz respeito ao modo como esses *quadros* de não legibilidade destas vidas – de suas vidas, sob a ameaça e o exercício histórico e ordinário dos “devoradores” e dos “exploradores” –, essas mulheres mantêm a vida viva

mitologia considera por Eduardo Viveiros de Castro como “deriva ‘arborescente’ em direção à cosmogonia e à teologia, ao logos monárquico – o mito do Estado” (2015: 196, nota 8).

**16** Diversas mulheres indígenas com quem falei em São Gabriel e que me contavam de estupros ou assédios contra mulheres próximas, mencionavam essa dimensão moral dos julgamentos no âmbito doméstico, por parte das outras mulheres e, inclusive, do indigenismo. Igualmente, pensando nos seus próprios filhos em casos específicos, mulheres com quem falei se colocam como agentes “educadoras” de homens violentos com outras mulheres.

**17** Veja o contraste, por exemplo, com as mobilizações de mulheres em Altamira (PA) a partir dos casos dos meninos emasculados e, depois, contra a Usina Hidrelétrica de Belo Monte (Lacerda, 2012 e 2017).

**18** Minhas ênfases. *Comentário de leitor* sobre notícia da Operação Cunhatã (Farias, 2013: s/p). Sobre “mulheres de militar” em SGC, veja Silva (2016).

(Das, 2007; Butler, 2010). Melhor ainda, como se apropriam destes *quadros*, consomem-nos e os empurram até o limite para fazer mais que sobreviver?<sup>19</sup>

Pergunto a D Inácia pelas “reações” dela ou dos parentes perante os assédios ou as violências sofridas no tempo das “firmas”. Ela diz que não havia nada. D Inácia continua:

– Por isso é que eles andavam de lá pra cá soltos e iam embora...

– Nem os padres, a Igreja fazia nada?

– Nada.

– E os patrões?

– Eles ficavam na casa deles: “se vocês querem ficar tranquilas, fiquem no Internato, vocês que decidem... se querem essa vida, saem...”. Por isso que eu nunca saía, eu ouvia a voz dos patrões... se eu sáísse eu já teria cinco filhos.... (...) Depois que conheci o Sargento ele me levava sábado e domingo pra passeio, mas os patrões esperavam a minha chegada... Depois que os patrões foram embora **aí que endoidei, doida mesmo... queria experimentar. Encontrei um nordestino de olhos azuis que queria me levar, mas o patrão não deixou...**<sup>20</sup>

19 A ese respeito, escreve Theidon (2011: 73): “De ese modo, una sensibilidad de género se centraría menos en estrategias para que las mujeres hablen de ‘sus violaciones’ y más en el desarrollo de nuevas formas de escuchar lo que ellas dicen sobre la guerra, y como lo dicen. (...) Y también está lo que las mujeres callan”.

20 As ênfases na fala de D. Inácia são minhas.

Através de D. Inácia, sigo a intuição de Lasmar (2005). No seu livro pioneiro, Cristiane Lasmar se aproxima das relações de gênero e interétnicas em São Gabriel da Cachoeira e suas transformações pelo caminho do casamento entre mulheres indígenas e homens “brancos”, e seu efeito de atualização de uma tendência “cultural” a se orientar para o “mundo dos brancos”. Apesar da sua importância, a análise de Lasmar ignora a violência e foca no regime moral da domesticidade, da mitologia e do parentesco (genealogia/aliança) como plano último de relação e de existência social.

Nesse sentido, é importante levar em consideração o chamado de atenção de Cho sobre as análises acerca das *war brides* coreanas, casadas com soldados estadunidenses e migradas a esse país, que seriam

*sanctified by giving her family in Korea [nas comunidades indígenas?] an opportunity to participate in the American dream [o “mundo dos brancos”?]. The war bride, as the pionner of Korean migration to the United States [migração indígena à cidade?], then operates as a figure for the disappearance of geopolitical violence into the realm of the domestic (2008: 14).*

Contudo, Cho nos leva diretamente para a retórica da produção de *traumas* e vítimas, que devemos também evitar porque, como vimos, há sexo e desejo, agenciamentos e caçadas *para fora* da “exploração” e dos estupros.

Isto é, definitivamente não é sobre casamento (e nem sobre *trauma*) o que as

relações entre este “endoidar” e “experimentar”, entre estas “microrroupas”, estes corpos jogados no mato, estas meninas exploradas pelos “velhos” comerciantes estão nos comunicando. É sobre não-casamento; é sobre sexo, juventude, desejo, rua, noite, bebida e dinheiro, sobre violências cotidianas, medos e os julgamentos morais, que operam como desdobramentos do quadro<sup>21</sup>.

Minha abordagem, portanto, orienta-se pela evidência empírica da importância destes fluxos e transações entre sexos, dinheiros, conjugalidades, afinidades e gênero (“endoidar”), e pela minha própria trajetória de pesquisa sobre prostituição e *economias sexuais*, em diálogo com autoras feministas nesse campo, tais como McClintock (1995), Montgomery (2001), Kempadoo (2004), Cabezas (2009) e Piscitelli (2013, 2016). Essas autoras têm destacado as capacidades e formas de agenciamentos femininos interseccionais em, e através, das *economias sexuais* em contextos de relações interétnicas e de mobilidades sociais e territoriais.

Vamos à etnografia. Coincidindo com o tempo em que Adriana Piscitelli e eu estivemos na cidade em 2014, houve um grupo de oficiais da Força Aérea Brasileira fazendo práticas de voo: “os aeronáuticos”. Eram, na sua maioria, homens brancos, altos, de corpos cultivados pelo exercício físico, que exibiam roupas e acessórios de marcas reconhecidas e gastavam festivamente dinheiro todas as noites da semana nos bares da cidade. Ao redor deles havia sempre muitas mulheres de diversas idades, incluindo indígenas adolescentes e adultas, e mulheres brancas locais ou forasteiras, adultas, vinculadas a serviços de saúde, educação, etc. As moças jovens indígenas estavam em grupos que, por sua vez, mantinham conexões com moços indígenas. Elas iam e voltavam entre um grupo e outro, paqueravam os jovens militares, bebiam com eles manifestando algo como um tédio mútuo, soltavam-se das capturas físicas que estes tentavam e, alternativamente, dançavam e curtiam muito com seus pares, que esperavam a alguns metros, dentro ou fora do bar.

Em nenhuma dessas noites vimos moças indígenas, no âmbito público, beijando-se ou “se agarrando” com nenhum desses “brancos”, tampouco investindo em performances de dança ou de gestos mais eróticos para seduzir ou ganhar a atenção deles. Aliás, o maior investimento na realização de danças eróticas públicas não foi realizado por nenhuma mulher, mas pelos próprios “aeronáuticos”. Em uma das últimas noites, num local de festas muito frequentado, dois dos “aeronáuticos” mais jovens, altos, brancos e de corpos muito trabalhados, subiram no palco para se exhibir dançando a coreografia “lepo-lepo”, em estilo *striptease*, com direito a levantada da camiseta para expor seus abdominais malhados. Apesar da euforia do público (principalmente feminino), no percurso da noite, nenhum deles conseguiu “ficar” com as moças com quem tentaram. É claro, nada sabemos sobre os arranjos privados que começam com trocas de números de celular e continuam em veículos, festas particulares, piscinas e casas (também oficiais).

21 Há um agenciamento feminino indígena muito importante que interpela diretamente a estrutura do *frame* de forma mais classicamente política e explícita. Refiro-me às organizações de mulheres indígenas da região do Rio Negro. Este importante agenciamento não é objeto deste artigo porque demandaria uma análise detida e completa, e porque seu âmbito de relação é fundamentalmente fora da cidade, nas comunidades.

O que me interessa destacar, fora o desespero narcisista dos colonizadores, é a intensa alegria das jovens indígenas quando, circulando entre os homens brancos e seus pares, encontravam-se e dançavam forró com estes últimos. A dança entre eles, aliás, era preciosa e irrepetível, e sem dúvida se constituía num espaço quase ritual de exegese do mundo, de encontro, de alimentação da alegria, da beleza e de um mundo que colocava aos “brancos” no seu lugar (à margem do movimento, da cumplicidade e da alegria). Não havia nenhum homem que não fosse indígena, principalmente jovem, que dançasse com elas daquele jeito precioso, cheio de giros e de leveza, com um balançar permanente de braços como ondas ou correntezas<sup>22</sup>.

A essa dinâmica geral agenciada pelas adolescentes, minha vizinha Rosa deu o nome de “putaria e saca”. Indígena de 17 anos e proveniente de uma dupla mobilidade (do alto de um dos rios para a cidade e da periferia para o centro), ela morava “amigada” com seu namorado militar em um pensionato do centro da cidade. Conversando na área da lavanderia, ela me explicava que se trata de uma prática entre amigos e amigas pares, “sem homens e sem mulheres”, dizia ela, referindo-se à importância da pura amizade, da ausência de envolvimento amorosos nessa prática de grupo. O centro da jornada noturna para eles e elas não é o sexo, apesar do que a expressão poderia sugerir, mas a bebida e a amizade. A turma se desloca à noite dos bairros de periferia para o bairro da Praia, onde está a maior parte dos bares frequentados por “brancos”, ou para algumas casas de festas do momento. O alvo da missão são “velhos” “brancos”. As moças, que se produzem belamente, intensificando os olhos puxados e seus cabelos pretos e compridos, colocam-se no campo visual dos “velhos” como disponíveis para paqueras e convites, mas sem realizar qualquer gesto direto de sedução – em alguns casos, dizem, “um primo arruma homens pra gente”. O “velho” convida a moça para uma bebida e, um tempo depois, alguns dos amigos ou amigas vão chegando casualmente, sendo convidados como requisito para o “velho” continuar gozando da companhia da ou das moças bonitas. Nessa coreografia, o “velho”, então, tenta seduzir e avançar com suas mãos, sendo sempre interrompido e sabendo que em público nada pode acontecer. Na reconstrução de Rosa, o “velho” perguntaria se no final da noite poderiam “ficar”: “pode, pode” – “putaria”. Quando a noite vai chegando ao fim, as jovens pediriam licença para ir ao banheiro e, aos poucos, um atrás do outro, os amigos iriam se afastando – “sacanagem”. Em outra versão, Rosa me conta que o homem dá a ela um dinheiro para adquirir bebidas, e ela compra menos que o valor poderia pagar e guarda o troco, repetindo essa prática por toda a noite com o mesmo ou diversos “velhos”. Nessa lógica, diz ela, “uma vez arrumamos 600 reais [U\$ 200,00] numa noite!”.

Vi e vivenciei estas cenas muitas noites<sup>23</sup>. Inclusive, fiz parte de uma muito especial, em uma posição semelhante à dos amigos pares (sem ser capaz de

**22** A dança, aliás, como gesto cultural de longa duração, é analisada por Marco Tobón (2016) junto a outros gestos como o humor, como um dos veículos por meio dos quais grupos indígenas colombianos (vizinhos do Rio Negro) enfrentam e “humanizam” a guerra e suas feras.

**23** Tive experiências parecidas em cidades como Altamira (PA) e Tabatinga (AM). Nesta última, um amigo gay de 16 anos, que fazia “babado” (programa), acusava essa prática de “exploração”.

dançar bem). Essa cena me ensinou a consistência social da “putaria e saca”, pois as protagonistas não eram garotinhas pobres da periferia. Eram mulheres locais (três indígenas e uma branca), já na casa dos 25 a 30 anos, profissionais e empregadas, que operavam sobre “velhos” militares conhecidos por elas procedimentos semelhantes aos descritos por Rosa. No lugar da cerveja, bebíamos whisky e, no final da noite fugimos deles, não de lotação ou caminhando, mas no carro próprio de uma das amigas.

Nesse jogo, a contradádiva sexual não é “necessariamente” concedida e nem estritamente cobrada, conforme o relato de Rosa. Raramente o dinheiro gasto pelo homem se transforma, de fato, na “devoração”: “eu não gosto de velho... não tenho preconceito, mas não gosto de velho”. Putaria e Saca. Contudo, é claro, como em toda relação canibal (Fausto, 2002; Viveiros de Castro, 2002), na “sacanagem” diversas portas ficam abertas para o exercício do “predador”/“devorador” (pressões, convites posteriores, encontros casuais, dádivas maiores, etc.).

Por outro lado, não é possível perder de vista que, *para fora* da violência, o sexo existe, tanto em termos de transações econômicas, mais ou menos sistemáticas, como em termos de buscas libidinais, mais vinculadas aos afetos e ao prazer sexual. Rosa, como muitas das garotas indígenas das periferias de São Gabriel com quem conversei, tinha vínculos próximos com algumas das moças envolvidas em um caso que ficou famoso por ter sido publicamente denunciado. Algumas delas, como a moça de 17 anos que fez a denúncia, porque “o velho mexeu com a irmãzinha dela”, não foram nem forçadas e nem enganadas, e teriam nesses “velhos” não-indígenas, nessa “prostituição” ontologicamente assimétrica, uma fonte segura de dinheiros e de alguns bens.

Apropriando-se da minha pronúncia hispano-americana do “s” como “ç” quando perguntei sobre “casar com militares”, Rosa disse-me, rindo, que as moças gostam mesmo de “caçar” e que estão sempre sabendo o tempo e o bar onde, como objeto de caça ou como fruto de árvore, “está dando muito militar!!”. Esses militares são os “aeronáuticos” – objetos perfeitos de “putaria e saca” ou da “prostituição” –, mas também são os “recrutinhas”, “novinhos e bonitinhos”, como o *marido* de Rosa, com o qual se tecem diversos vínculos de afetos e transações.

Em vários sentidos, a prática da “putaria e saca” se opõe termo-a-termo às dinâmicas relacionais coloniais antes narradas, principalmente da “exploração sexual”, bem como às narrativas de vitimização moral sobre elas construídas. Tal oposição não é de contestação ou resistência, mas de contraefetuação, e passa fortemente por noções como “devorar”, “caçar”, “comer”, consumir, comercializar, “putaria e sacanagem”. De um lado, temos a imagem dos carros dos “velhos” rodando pelos bairros da periferia, perto das escolas, assediando as moças que transitam em paz. Do outro, observam-se relações de agenciamento coletivo, com moças arrumadas e noturnas, aventurando-se para sair dos seus bairros e

penetrar ora o coração da beleza postal e da balada “branca” de São Gabriel – o bairro da Praia –, ora os circuitos mais mal-falados de noitada, com o intuito de “caçar” militares e de “arrumar” homens. Se, em ambos os casos, há uma forma-predação (Fausto, 2002), em que caçar e consumir fora tem o efeito de produzir relação e afetos dentro, de um lado a relação é orientada pela obstinação instrumental e destrutiva do “devorador” com o sexo e com a substância dos corpos das garotas indígenas; do outro, a orientação é a amizade e os afetos entre pares, a bebida e a dança, colocando a busca por esses homens como um mero percalço e implicando, também, a dívida. De um lado, a eficácia do agenciamento está na autoridade, no uso da força, no isolamento, na escuridão, na redução étnica e sexual generificada, e na escassez econômica do outro. Do lado contrário, a eficácia descansa na beleza, em toda uma poética da sedução dissimulada e fugidia, na estética cuidada da dança, dos cabelos e das roupas, na apropriação algo paródica ou fantástica do código de etnicização erotizada. Por fim, se de um lado há a tentativa da dominação pela força ou pela dívida civilizatória (a ficção do comércio), no outro há o claro exercício de ser objeto de dívida e se agenciar no sexo, na “caça” e na administração da gramática. Impossível dizer que há uma reformulação do quadro, mas quiçá uma canibalização da dívida e dos desejos?

### **CONSIDERAÇÕES FINAIS**

Este artigo começou com uma cena de assassinato e estupro e terminou com uma tensa festa libidinal e econômica. Entre as cenas, encontramos uma cidade neocolonial na Amazônia, a “exploração sexual” e as lembranças de juventude de D. Inácia. O objetivo deste artigo foi localizar formas específicas de agenciamentos de mulheres jovens indígenas nessa cidade, contra, através e no meio da paisagem barroca de violências sexuais. Estas violências são efeito das configurações do *poder* e do *Estado* na cidade, que emerge como um “escoadouro” comercial e como uma fabricação religioso-militar. Nesta fabricação, em nome de uma compreensão muito específica do “progresso” e da “integração nacional”, e em tortas relações com a ideologia da “defesa”, produz-se um mundo de absoluta insegurança e desintegração para sujeitos indígenas, particularmente para as jovens. Nesse sentido, as violências aqui analisadas avançam na atualização performativa de um *frame* (Butler, 2010) que coloca as moças indígenas na permanente ida e volta entre a morte e a reprodução biológica, entre a “exploração” e o casamento, entre o estupro e o assassinato – o peso histórico e mitológico dos (des)fazer do projeto “civilizacional” nas suas particularidades rionegrinas.

A prática da “putaria e saca”, como descrita por minha vizinha Rosa, ofereceu-nos algumas ideias para imaginar formas possíveis em que estas moças *lidam*



com o enquadramento. A “putaria e saca”, enquadrada, remete-nos a duas formas diferentes e articuladas de agenciamentos. Uma se situa no plano da *evasão* estética e a outra, talvez mais englobante, no plano do que desde certa antropologia poderíamos chamar de contraefetuação canibal. No primeiro plano, localizo as formas poéticas por meio das quais estas mulheres se implicam no *quadro*: a produção da sua própria beleza, da sedução, da dança, da alegria etílica, do gozo sexual. Aqui, é central a inspiração de Butler quando analisa as possibilidades da poesia e das imagens produzidas em contextos de guerra. São atos poéticos e estéticos que não têm pretensão ou sequer possibilidade de acabar a guerra, porém,

*(...) tienen claras consecuencias políticas: surgidos de escenarios de sojuzgamiento extremo, son la prueba fehaciente de una vida tenaz, vulnerable, abrumada, la vida propia y la no propia, una vida desposeída, airada, perspicaz. Como **red de afectos transitivos**, los poemas—su escritura y su divulgación—son actos críticos de resistencia, interpretaciones insurgentes, actos incendiarios que, en cierto modo e increíblemente, viven a través de la violencia a la que se oponen, aún cuando no sepamos todavía de qué manera van a sobrevivir dichas vidas (minhas ênfases. Butler, 2010: 94).*

A chave pois, está na coexistência da *evasão*, do afrouxamento dos mecanismos de controle e, isso é fundamental, das *novas trajetórias de afetos* (idem: 27) e das *redes de afetos transitivos*. Além dos namorados e “colegas” de rua ou de escola, os “recrutinhas”, “soldados” e outros militares jovens parecem ser ótimos parceiros para encontros sexuais e para abrir, agora sim voltando a Lasmar (2005), possibilidades de vínculos afetivos mais duradouros: porque, além de “dar coisas”, são “novinhos e bonitinhos”. Esses jovens militares de hoje são, então, uma função daqueles desejos “doidos” da juventude de D Inácia e da sua relação com “o Sargento”, e, lado a lado com a dança das moças e com o “lepo-lepo” dos “aeronáuticos”, lembram-nos irremediavelmente de um conjunto relacional extremamente mais complexo que a pura dominação e que as acusações morais sobre a sexualidade das moças ou sobre os desejos dos colonizadores. Contudo, os afetos novos, transitivos e em movimento, bem como os afrouxamentos do *frame*, marcam também as novas alianças: mulheres pesquisadoras, funcionárias de ONGs, jornalistas, colegas de militância indígena que, pouco a pouco, vão acolhendo e acarinhando essas outras existências.

Nesse sentido, para imaginar as possibilidades de reenquadramentos e eficácias, é importante se perguntar pelo espaço conectivo entre Rosa e Sarah, entre a morte violenta, a “exploração sexual” e a “putaria e saca”. É possível para as mulheres jovens evitar a morte? A “putaria e saca” representa um risco especial para estas mulheres?

De início, há uma analogia possível. D Inácia conta das punições que a polícia exercia, “caçando” e obrigando as jovens indígenas que frequentavam a rua e a noite a serem prostitutas. Elas, não aguentando, morriam nos prostíbulos. Há uma analogia com o tempo presente: essas moças da “exploração”, da “putaria e da saca”, que “caçam” militares, são transformadas discursivamente em “prostitutas” e depois estupradas e assassinadas na saída de um “Club” ou no trânsito noturno de uma cidade que já não tem prostíbulos. Para evitar assédio e estupro, as mulheres da cidade, principalmente as mais jovens, evitam certas ruas, horários e taxis desconhecidos; para evitar a morte, D. Inácia permanecia em casa.

Assumindo o risco, de forma semelhante a Rosa, D. Inácia “saiu”, ficou “doida”, namorou, engravidou e não morreu. O caso da Sarah é, de alguma forma, a reflexão negativa da inevitabilidade da morte. Foi o único caso de corpos de mulheres indígenas estupradas e assassinadas encontrados na rua em 2016 (entre pelo menos três), capaz de criar sentimentos e expressões públicas de luto e dor, sendo lembrado com detalhes ainda em 2018. O fato de ela não ter sido assassinada numa casa de festas, ou vestida com “microrroupas”, de madrugada ou bêbada, após na saída de uma danceteria, fez com que a sua moralidade pública fosse resgatada, abrindo espaço para a condenação da imoralidade da sua morte.

Esta *não-causalidade* é importante para pensar as possibilidades que estes agenciamentos têm quando impactam o *frame*. Afinal, não-causalidade é a marca da incerteza plena e da fácil expansão do medo entre mulheres que habitam e visitam a cidade. Em consequência, tem fortes efeitos na manutenção das assimetrias relacionadas à *precariedade* de certas vidas (não as das jovens da “putaria”, mas, virtualmente, de todas as mulheres jovens indígenas no percurso de gerações).

Por outro lado, a “putaria e saca” – transformação sofisticada desse “endoidar” da festa de que D. Inácia falava –, pode ser lida como uma contundente contraefetuação dos regimes morais, corporais, urbanos e econômicos majoritários. Isto é, talvez a “putaria e saca” seja uma versão possível de *canibalizações* contracoloniais, no sentido de poéticas, políticas ou filosofias nativas de socialidade – na chave da caça e devoração –, a partir das quais se traduzem e reconfiguram substâncias, corpos e gramáticas (Mascat, 2015; Viveiros de Castro, 2015)<sup>24</sup>. Trata-se, então, de “sacanear o *frame*”, conforme as palavras de Rosa. É na intensificação e administração da alegria, da potência erótica, noturna e juvenil, produzida de forma coletiva, dançante e fortemente estética, que essas moças *sacaneiam* parcial e temporariamente o processo de *devoração* colonial e de sujeição moral de que são objeto, ou se reinscrevem nele desde a linguagem da *putaria* e do *canibal*: por isso o risco permanente de ser objeto efetivo da devoração (necropolítica).

Perante um sistema sólido de colonização que tem em destruições físicas, endividamentos e explorações sexuais um centro estrutural, a “putaria e saca” se

24 Há algum tempo venho colocando em diálogo noções êmicas sobre “caçar”, “comer” e “devorar” nas relações entre sexualidade, gênero e dinheiro (Olivar, 2011, 2013) com uma literatura da antropologia indígena brasileira que fala de “canibalismo” e “predação” (Fausto, 2002; Viveiros de Castro, 2002). Mais recentemente, a noção de “canibalismo” encontra o pensamento descolonial por caminhos e em sentidos diferentes: vinda da história intelectual e feminista do Atlântico Negro (Mascat, 2014) e pela especulação filosófica-política a partir de elaborações antropológicas do pensamento ameríndio (Viveiros de Castro, 2015). A primeira tem como base uma noção contracolonial, feminina e poética de canibalismo dos anos 1940; a segunda, como se sabe, com forte influxo do modernismo antropofágico brasileiro. Desde o início desse esforço, eternamente inacabado e parcial, entendo que não se trata de “transportar” conceitos entre campos culturais ou acadêmicos com fronteiras, digamos, ontológicas, mas de, como sugere Goldman (2008: 9), “usar os conceitos de forma propriamente conceitual (...) como modos de organização e formas de criação”.

apresenta como um mecanismo de tradução da história e da genealogia familiar, o controle possível do “endoidar” e da experimentação. Uma tecnologia que fissa a *frame*, que o quebra entre “velhos” e “jovens”, recombina possibilidades de relação étnica, insere distinções em seu interior, permitindo compreender e separar as fontes dos perigos, dos prazeres e das satisfações, e, insisto, possibilita a composição de novas trajetórias de afeto. Uma tecnologia através da qual estas moças tensionam publicamente sua existência – fazendo-se assim existir como vidas *tenazes* – no marco, agora *sacaneado*, da função sexual prevista por cosmologias nativas e pelo processo colonial. “Putaria e saca” é a tradução feminina, indígena, urbana e juvenil da intrincada rede de desejos que conecta os “velhos” e seus avanços, as vontades de beber, as necessidades de dinheiro, a potência do sexo, as fantasias conjugais, a amizade, “o mundo dos brancos”.

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**José Miguel Nieto Olivar** é comunicador social e antropólogo, professor na Faculdade de Saúde Pública da Universidade de São Paulo.

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# Hematologia mura: os caminhos por onde o *sangue* escorre

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## Fernando Augusto Fileno

🏠 Universidade de São Paulo | São Paulo, SP, Brasil

✉ fernando\_fileno@hotmail.com

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### RESUMO

O *sangue* existe no mundo como fonte da vida, inerente aos seres que dele usufruem, mas ele também atua independente dela como um *corpo*. O *sangue* que corre pelas veias, veículos em nossa cosmologia cientificista de um conjunto celular de natureza diversificada é, para nossos interlocutores, *espírito*. Os Mura entendem o *sangue*, para além de sua classificação biológica, como um plano da cultura. Em uma palavra, o *sangue* é relação. No contexto-mundo dos Mura, habitantes das comunidades do rio Igapó-Açu, município de Borba (AM), o *sangue* é um meio de comunicação transespecífico e transespacial. Nossa intenção neste artigo é apresentar uma hematologia que dê conta das múltiplas possibilidades que o *sangue* engloba. Se, à primeira vista, o *sangue* ressoa aspectos da vida privada, poderemos ver como, em um segundo momento, ele passa a ser objeto de interesse público e, principalmente, cosmológico.

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### PALAVRAS-CHAVE

Mura, sangue, ontologia, cosmologia, corporalidade, Amazônia.

**MURA HEMATOLOGY: THE PATHS THROUGH WHICH BLOOD FLOWS****ABSTRACT**

*Blood* exists in the world as the source of life, inherent in the beings that utilize it, it also acts independently of it as a *body*. The blood that flows through the veins, vehicles in our scientific cosmology of a cellular set of diversified nature is, for our interlocutors, *spirit*. The Mura understand *blood*, beyond its biological classification, as a plan of culture. In a word, *blood* is relationship. In the world-context of the Mura, inhabitants of the communities of the river Igapó-Açu, city of Borba (AM), *blood* is a trans-specific and trans-spatial communication medium. Our intention in this article is to present a hematology that accounts for the multiple possibilities that blood encompasses. If at first glance the *blood* resounds aspects of private life, we will be able to see how in a second moment it happens to be object of public interest and, mainly, cosmological.

**KEYWORDS**

Mura, Blood, Ontology, Cosmology, Corporality, Amazonia.

Quando a gente morre, vai que é o sangue... Com essa afirmação, uma conhecida pajé do rio Igapó-Açu tencionava responder à minha pergunta sobre o destino do espírito após a morte do corpo. Não era a primeira vez que sangue despontava como componente das explicações que deslindavam o tema da sociedade e do cosmos mura. Como tal, o *sangue*<sup>1</sup> surgia como um componente do corpo, mas igualmente como um *corpo*, marcando presença ativa tanto dentro, como fora do seio da carne. Existindo aquém e além da pessoa, o *sangue* a conectava à vida, mas igualmente às possibilidades de social. Traçar os caminhos pelos quais o *sangue* escorre é também revelar os múltiplos caminhos pelo quais o social se espraia. Apesar de sua relevância para entender as relações que envolvem a pessoa mura, assim como as relações entre os Mura, o *sangue* existe apenas nas entrelinhas, subtendido na rotina das precauções do dia a dia, subsumido aos elementos do corpo. Apresento a seguir mais uma peça ao quebra-cabeça transcultural que tange às definições de sangue para as populações indígenas na Amazônia (Belaunde, 2006). A hematologia mura, que ganhará registro nestas páginas, oferece um quadro das definições sobre o *sangue* com relação à noção de pessoa, ao gênero e à cosmologia, dando a ver a centralidade do *sangue* para a existência dos Mura.

Os Mura a quem dou conta habitam o rio Igapó-Açu, pertencente ao interflúvio do rio Madeira, dentro da Terra Indígena Cunhã-Sapucaia, localizada no município de Borba (AM). Este artigo é um desdobramento de um dos capítulos de minha dissertação, que se dedicou a essa população indígena de língua mura que posteriormente adotou o nheengatu e hoje é falante do português (Fileno, 2016). Subentendido entre referências pontuais, recolhidas em distintos contextos relacionais, essa etnografia registrou qualidades atribuídas ao sangue que

1 Daqui em diante a grafia do termo sangue sublinhará a diferença existente entre a substância composta por plasma e glóbulos vermelhos e brancos, o líquido espesso que circula pelo sistema vascular e aquela referida pela definição que os Mura fazem dele. Por isso, quando o termo for grafado em itálico estaremos nos remetendo ao conceito local, quando não, a palavra remeterá à definição da ciência biológica.

nada têm a ver com a definição da fisiologia que o entende como um conjunto celular de natureza diversificada. O *sangue* para os Mura antes, participa das distintas etapas de maturação de uma pessoa (nascimento, idade adulta e morte), concedendo vida ao corpo, bem como sendo ele próprio vida além do corpo. Conectando as pessoas entre si, o *sangue* igualmente guarda como potencial a capacidade de unir mundos separados.

Desde o artigo seminal de Anthony Seeger, Roberto DaMatta e Eduardo Viveiros de Castro (1979), tem-se em conta como a noção da corporalidade e do social se fazem juntas à fabricação do corpo. O sangue e seu valor nesse processo não passariam despercebidos, como inúmeras etnografias sobre a região amazônica sinalizaram. Atinente à construção do corpo, às distinções de gênero, bem como às definições cosmológicas, o sangue surge como artífice de operações relacionais. Refletindo através de casos comparativos, apresento as bases de definição do *sangue*, ou seja, o sangue a partir da concepção mura. Minha descrição, logo, é tanto um registro etnográfico, como mais uma referência para definir o escopo das qualidades e potencialidades do sangue na Amazônia indígena.

Os Pirahã, pertencentes à mesma família linguística dos Mura, habitantes dos rios Maici e Marmelos, guardam um lugar fundamental para o sangue em sua cosmologia. Essência do corpo (*ibiisi*), assim como uma das autodenominações utilizadas por esse grupo, ele é um dos componentes de quase todos os seres do cosmos (Gonçalves, 2001). Para os Yanomami, o sangue é a substância biológica fundamental, associada à imagem vital *bei a ne utubi*, que encerra a essência metafísica (Albert, 1985: 348). Assim, o sangue também é de suma importância para os Apinayé, pois sua quantidade no corpo representa um dos marcadores do estágio da vida (DaMatta, 1976: 85). Crianças e velhos têm pouco sangue, porque estão situados nos extremos dessa escala sanguínea: de um lado, os pequenos ainda estão muito próximos à natureza e, por isso, necessitam crescer morosamente a quantidade de sangue em seu corpo; de outro, os velhos se aproximam cada vez mais do fim da vida (*Ibid*). Ainda no Brasil Central, para os Bororo, o sangue, assim como seus derivados, sêmen e o leite materno, são considerados como veículos do *raka*, a força fundamental que anima os seres. É aquilo que permite aos homens e às mulheres realizar suas atividades cotidianas, como caçar, pescar, plantar e cozinhar (Crocker, 1985: 41). Quando tratamos dos princípios cosmológicos *aroe* e *bope*, cuja dialética anima a existência bororo, vemos como é também através do *raka* que se realiza sua intermediação com os homens. Essas menções pontuais revelam, através de sua proximidade lógica, uma preocupação pan-amazônica em relação ao sangue e que começa dentro do corpo.

Para os Mura, o *sangue* é um reconhecido elemento volátil, as preocupações e cuidados que o envolvem transcendem a esfera do corporal e, fora do corpo, colocam-se na pauta das medidas de importância social. Longe de ser um assunto

de cunho privado, as atenções em torno dessa substância respondem aos limites entre planos cosmológicos contíguos, ou, como acertaria o senso comum: o interesse de ver *cada qual em seu lugar*. O problema, portanto, não reside no lugar do sangue, mas as condições que ele cria para mudar-se de *lugar*.

## DENTRO DO CORPO

Em seu percurso bibliográfico pela Amazônia, que resultou no livro *El recuerdo de luna – Genero, sangre y memoria entre los pueblos amazónicos*, Luisa Elvira Belaunde sustenta como hipótese a ideia de que o sangue seria o “principal veículo tanto da diferenciação como da unidade entre os gêneros ao longo da vida” (2005: 18, tradução minha). Sobre esse ponto, a concepção e o nascimento mura não divergiam da mesma conclusão. Ao nascer, diziam que a mulher derramava seu *sangue* pela criança que dera à luz e que, como consequência tanto ou menos grave que a perda do sangue em si, lhe seria destinado um tempo de resguardo. Ao homem recomenda-se a evasão do excesso de esforço físico, trabalho pesado com madeira ou do roçado. Desacatar esse princípio forçaria o umbigo da criança, *pondo sangue*, perturbando o delicado equilíbrio sanguíneo do infante em sua fase mais crítica, deixando-o inflamado e mesmo inchado. Um dos remédios ensinados contra esse prejuízo era colocar o crânio de tracajá (*Podocnemis unifilis*) no umbigo da criança prejudicada e comprimi-lo para que ele se recolhesse como a cabeça do quelônio o faz quando se sente ameaçado. Se, por um lado, a mulher *derramava o sangue* pelo filho gerado, o pai, inversamente, colocava o *sangue* no filho nascido. Essa circulação não atendia um fluxo de reposição, a mãe deveria recuperar o seu *sangue* perdido, enquanto o pai preocupava-se com o *sangue* que não deveria ser depositado em excesso.

O descompasso sanguíneo na concepção poderia apontar também a uma divergência na gestação. Ao que parece, os Mura não possuíam qualquer teoria sobre a concepção que contrastasse com aquela oferecida pela equipe médica que recorrentemente os atendia e pela ciência biomédica. Ainda assim, poderíamos vislumbrar como essa diferença entre perda e ganho de *sangue* remeteria ao processo de formação do feto dentro do útero, fecundando um ser composto pelo *sangue* paterno e reunido no seio materno<sup>2</sup>. Em vez de o sêmen ser a substância vital, como geralmente é apontado para a condução desse processo, o *sangue* se destacaria com esse papel. Seja como for, o laço físico existente entre os pais e a criança ratifica a presença do *sangue* como intermediário da diferença e da unidade entre os genitores de um novo ser.

Idioma focal igualmente quando o tema do parentesco vinha à tona, o *sangue* demarcava a diferença entre os parentes gerados pelo nascimento e aqueles contraídos pela *criação*. Para os Mura, então, existiriam parentes de

2 Formulada a partir da etnografia, essa explicação não deixa de ser influenciada pelas teorias de concepção agnática, como aquela já registrada entre os Tumpinambá e que segue ainda sendo registrada entre os grupos de língua tupi. Para eles, a mãe não possuía nenhuma participação como genitora, servindo como receptora do material depositado pelo pai a partir da relação sexual (Fernandes, 1963).

*sangue* e parentes de *criação*, uma diferença que se fazia presente no cotidiano e que sublinhava a valoração dos últimos como parentes verdadeiros (Fileno, 2016). Presente antes de nascer, o *sangue* não somente demarcava as relações construídas ao redor da pessoa, como era um índice importante no processo de sua maturação. O *sangue* era algo diferenciava os gêneros a partir das condições sobre o seu controle. Preservado no interior do corpo, este atuaria como um limite de segurança, assegurado entre os homens e que se via interrompido diante da primeira menstruação e das que seguiriam desde o momento que a mulher estivesse *formada*. Tratava-se de uma diferença entre corpos que, se assimilada a um fato da natureza, deveria ser contornada por uma postura de retidão moral<sup>3</sup>. A condição de fazer-se *moça* surgia como um dos marcos mais significativos da passagem que assinalava a responsabilidade da mulher consigo mesma e com os outros. Por isso, os adultos, quanto a essa regra, diziam que mulheres não deveriam sair quando *não estão podendo*.

O sangue menstrual parece ser o principal fator da diferença entre homens e mulheres. Essa diferença não se sustenta nas atividades cotidianas, que ganham nas aldeias do Igapó-Açu uma grande fluidez, haja vista que, se a princípio homens caçam e pescam, enquanto mulheres cuidam e cozinham, isso não se determina como regra, dado que essas posições podem chegar a se reverter. Ao passo que o *sangue* diferencia tanto quanto aproxima, conforme sua existência é uma questão para mulheres e para homens, sendo, no limite, um problema da comunidade.

Os efeitos do *sangue* do catamênio operam no interior do corpo da mulher, bem como fora dele. A mãe da menina recém-*formada* deveria tomar precauções, aparentemente miúdas, para abrigar a filha da amplitude que todos os seus sentidos (audição, visão, etc.) ganhavam naquele período. Enquanto durasse o período de resguardo, a mãe mantinha sua filha dentro do quarto às janelas fechadas, com a cabeça enrolada e certificando-se que também os ouvidos estivessem protegidos da alusão ao mundo exterior. A alimentação e os banhos deveriam ser tomados dentro do quarto, alheios à vista de qualquer um. A mãe explicava que sua cabeça necessitava ser enrolada quando trovejava, porque o barulho do raio perturbaria o cérebro. Do mesmo modo, não era aconselhável olhar para a cigarra quando ela cantava, já às seis da manhã no terreiro, porque, caso contrário, ela *cortaria a vista da pessoa*. O que estava posto para as meninas recém-formadas e para as mulheres não é uma renovação corporal ou uma troca de pele, como seria o caso para alguns grupos de língua tukano do Noroeste Amazônico. A exemplo dos Barasana e dos Makuna, a menstruação é vista como uma fonte do poder reprodutivo feminino (Hugh-Jones, 2011 [1979]; Arhem, 1981). Para os Mura, a menstruação está relacionada à potencialidade agentiva que o *sangue* tem sobre a experiência vivida, despertando, com sua vazão menstrual, qualidades nas meninas e mulheres no decorrer de suas vidas.

3 Os Mura, até onde sei, não elaboram uma explicação mitológica para a existência do fluxo menstrual das mulheres, ao contrário do que é amplamente verificado na Amazônia (Belaunde, 2005; Hugh-Jones, 2011 [1979]; Rodrigues, 2008). Não existem narrativas que elucidam esse fato atual a partir da agência social entretecida em tempos míticos, o que parece sempre ter existido é a ameaça que esse componente reativo carrega consigo para a coletividade. Conta o mito de Cantagalo, por exemplo, que toda uma aldeia foi para o fundo do rio em razão de uma menina que estava em seus dias de resguardo, ao som da derradeira noite de festa estavam todo no fundo, inclusive os animais, em um interminável burburinho (Fileno, 2016: 133).

A expansão das possibilidades comunicativas e perceptivas é o meio de abertura do cotidiano para a concretude de outros tempos e espaços cosmológicos (Belaunde, 2006: 207). Desde a menarca e consecutivamente durante a parte do mês na qual a mulher estaria naqueles dias em que *não pode* consigo, o resguardo em regime cerrado era exigido dentro da aldeia. Uma precaução que previa os efeitos que o *sangue* desencadeava sobre agência alheias.

Dentro do corpo, o *sangue* se comporta como substância, fora da descrição do processo de maturação, o *sangue* é também um dos componentes da pessoa mura, um princípio material que anima o substrato corpóreo. Tal como a *sombra* projetada sobre o chão, o *sangue* existe como um *corpo*, um dos termos da constituição física, mas que também existe como entidade independente. O destino *post mortem* ameríndio já foi amplamente descrito na literatura, seu início manifesta-se através da dispensa de princípios vitais que ganham caminhos diversos. O *sangue* e a *sombra* mura parecem se assemelhar às narrativas tupi-guarani que postulam a existência de até dois desses princípios (Fausto, 2002; Garcia, 2012; Gallois, 1984/5; Viveiros de Castro, 1986). Para os Araweté, por exemplo, aquilo que para nós traduzir-se-ia como alma segue após a morte uma divisão: parte dele (*i-*) ascende ao céu, já sua contrapartida (*ta'o we*), permanece na terra, manifestando-se como uma ameaça canibal, um perigo constante para os viventes (Viveiros de Castro, 1986: 515). O que parece, contudo, monopolizar a narrativa mura é a dispensa desses princípios vitais enquanto há vida no corpo que habitam, pois o perigo reside exatamente em uma ausência prolongada de seus componentes.

A mesma pajé citada no início deste artigo havia ressaltado para mim que o nosso corpo era formado por outros *corpos*. Alinhados ao primeiro de carne e ossos, *sangue* e *sombra* manifestam o caráter multicorporificado da pessoa. Explicava ela, através de uma sutileza, sobre a posição de nossa *sombra* com o sol a pino, dizendo que nesse horário estávamos pisando em cima de nossas próprias cabeças. Tal definição, à primeira vista fenomenológica, parece estar relacionada à necessidade de um corpo para se experimentar o mundo. Logo, *sangue* e *sombra* não poderiam existir além de seu invólucro primitivo de outra maneira que não fosse como *corpos*. Ser e estar no mundo não estão desassociados, pois a pessoa vive e sente a existência concreta através de seu corpo, ela é o que o seu corpo sente. Assim sendo, tanto o *sangue* como a *sombra* só poderiam existir autonomamente a partir do mesmo modelo sensível. A livre experiência e a independência desses princípios são relegadas pela teoria nativa à atividade de especialistas, entretanto, qualquer indivíduo tem ao seu alcance a chave ou o drama dessa potencialidade.

O perigo iminente da decomposição física e metafísica involuntária da pessoa está diretamente relacionado à condição de *corpo aberto*, a mais importante definição de vulnerabilidade conhecida na região do rio Igapó-Açu. O *corpo*



*aberto* não é um estado adquirido, tampouco se resume a uma consequência fisiológica, apesar de inflectido por ela, ele existe como um intervalo entre o nascer e o formar-se, persistindo contudo, no decorrer da idade adulta até a morte. Todos têm o seu *corpo aberto*, uma constante que deve ser progressivamente constrangida até aproximar-se do seu grau zero. Em seu trabalho junto aos Javaé, pertencentes à família linguística karajá, Patrícia de Mendonça Rodrigues mostra como a existência para esse povo que vive às margens do rio Araguaia emparelha-se entre duas possibilidades: aquela associada aos corpos abertos, marcada por uma perda excessiva de substância – como o sangue –, e a aquela referida aos corpos fechados, entre seres que não guardam relação entre si (2008: 258). Tal diferença explica mesmo a condução da história e se estende do plano terrestre, onde o autodenominado “povo do meio” (*Itya Mahãdu*) habita, até os outros que coexistem paralelamente (2008). De importância igualmente cabal para os Mura, a abertura e o fechamento do corpo estão enraizados no solo da vida cotidiana. Deixando de operar como um modelo de referência para a construção do cosmos, como parece ser o caso javaé, essa condição, ao mesmo tempo natural e cultural, põe à vista uma passagem comunicacional entre mundos contíguos, que deve ser restringida a todo custo.

O *corpo aberto* é uma categoria ampla que não se limita ao estado reativo do *sangue* e da *sombra* que habitam a pessoa mura. A mitigação da porosidade comunicacional do corpo é um processo contínuo e necessário, o qual se faz para reduzir a permeabilidade do *corpo aberto* aos afetos e agências estrangeiras. Ao nascer, a pessoa precisa ser moldada e seu corpo protegido, um longo e delicado processo envolve a infância de práticas preventivas. O banho de folhas, amplamente disseminado entre as comunidades do rio Igapó-Açu, é o principal instrumento profilático e curativo utilizado (Fileno, 2016: 112). Ter o corpo aberto significa estar e deixar à mercê aqueles que menos têm condições de se proteger, como sugere ser o caso das crianças. Débeis, não por escolha, as crianças têm o *corpo aberto* por não contarem com meios de “fechá-lo” por si mesmas. A preocupação e acompanhamento dos adultos dura enquanto a criança não possui ciência do mundo e autonomia suficientes para cuidar de si. Mesmo os adultos se desviavam da segurança que os mantinham resguardados da ameaça externa. *Sangue* e *sombra* eram reféns em um corpo desguarnecido da reza, dos bons pensamentos e do cuidado e atenção para com os semelhantes. Tomado pelo *remorso*, agravo das más relações que o separam do grupo, o corpo quando aberto segue provocado por uma profunda perturbação, da qual todo o seu organismo se ressentido. Nessas paragens, o fisiológico, o ético e o moral caminham juntos.

Diferente da *sombra*, a existência extracorpórea do *sangue* não se limita aos efeitos da ação maléfica de entidades estranhas ao corpo da pessoa. Para a *sombra*, existe sempre o risco de sequestro perpetrado pelos seres que abundam

o cosmos mura. Uma preocupação que costuma colocar os pais em estado de alerta, mas que também constrói os adultos. Apesar de *sangue* e *sombra* serem partes da pessoa, não existem como partes iguais. Sobre a última, há uma série de especificidade cujo escopo deste artigo não cabe contemplar, por isso concentramos nossa atenção aqui na descrição do primeiro. O *sangue* existe como sonho. A atividade onírica criava um contexto de exceção, excluído o corpo físico, reinante durante a condição de vigília, o *sangue* existia para além da tênue fronteira da pele como *espírito*. Mais uma tradução que nossos interlocutores mobilizavam para dar conta qualidades semânticas e sensoriais que compreendiam essa substância. Como outro pajé concertaria:

*Sonhando, você não sonha que você vai pra sua terra? Vai pro seu canto, às vezes até o seu amigo conhece, né? É mesmo assim, né? Sonho é o sangue da gente, o sangue da gente depois que sair... A nossa carne, nós fala a nossa carne porque o sangue tá dentro, depois que sair, meu irmão, o nosso corpo fica igual a um bicho que morre. O sangue que é o espírito da gente, né? Você sonha brigando, ou você sonha pegando mulher, cada um tem um sonho, ou lendo, ou tocando... (Entrevista, aldeia do Jutai, 2015).*

O *sangue*, portanto, era mais do que aquele líquido espesso e vermelho que circula pelo sistema vascular; ele era um elemento compósito, porém autônomo e dotado de intencionalidade. O *sangue* é aquilo que, em suma, anima o corpo. Sem ele, nada distingue a nossa carne da carne de uma “presa qualquer”. Fonte anímica, sua existência separa a vida da morte, um princípio vital que existe como potência para todas as criaturas. O corpo depende dele para existir, e não o contrário. Enquanto o sangue corre dentro das veias, o indivíduo é capaz de interagir com o mundo à sua volta. Esse parece ser o desdobramento lógico por trás da expressão concreta que define o corpo, quando separado de um de seus elementos compósitos, como um reles animal morto. A imagem de uma presa abatida sobre o chão da floresta é tanto a figuração do *sangue* coagulado em seus condutores venais e derramado através de feridas fatais, como é um juízo sobre um ser que foi destituído de sua capacidade de agir sobre o mundo. Asserções distintas, mas que igualmente apontam para uma mesma consequência: o movimento. Dentro ou fora do corpo, o *sangue* é expressão de movimento, ação e causa.

O *sangue* parece ser a chave para abrir os diferentes caminhos pelos quais a pessoa se distribui. Para os Jívaro, segundo a descrição de Michael Harner, o sangue também é espírito, o “verdadeiro” ou “ordinário” espírito (1972). Nesse registro, a pessoa jívaro poderia ainda contar, além do sangue, com outros dois espíritos, o *arutam wakani* e o *muisak*, desde que fossem adquiridos de modos tradicionais. Nascendo com ele, o sangue (*nekás wakani*) poderia esvair-se da

pessoa, como se ela estivesse em um processo de perda de espírito (*Ibid*: 149). Sua presença, contudo, não cumpria papel importante no sistema de crenças local, situação adversa ao que é o *sangue* no cotidiano do rio Igapó-Açu. Como princípio genérico, o *sangue* ganha identidade ao animar o corpo em que habita. Responsável pelo deslocamento de seu suporte físico, ao mesmo tempo, ele é movimento em si, pois dele se diz estar em diferentes lugares quando o corpo em seu estado pleno não está. Aliado de sua locação originária, ele ainda segue com uma identidade definida. O *sangue* é o sonho, assim como o *espírito* de uma pessoa. Por ser sonho, ao *sangue* é outorgado a tangibilidade das experiências conduzidas durante o sono, pois elas não existem apenas como plano do inconsciente, mas geram efeitos concretos. Realizam-se para um dos *corpos* da pessoa, regalando-lhe as consequências deletérias ou benéficas que esses novos contatos possibilitam. Ao mesmo tempo, por ser *espírito*, o que se satisfaz no plano onírico apenas sutilmente está ligado à pessoa da qual se originou aquela abertura. O que se concretiza no plano dos sonhos é tão real quanto aquilo que se dá durante a vigília, efetivando-se como experiências e histórias levadas a cabo pelo *espírito*. Tudo se passa como se a pessoa atuasse igualmente em dois estados distintos, o de vigília e o de letargia: os mundos podem ser os mesmos, mas os *corpos* em questão são outros. O desalinhamento dos *corpos*, como traduzimos, a separação dos compósitos que sustentam a pessoa neste mundo, não existe sem efeitos colaterais. As viagens encabeçadas pelo *espírito* desguarnecem seu invólucro, criando um estado semimorto. Controlar plenamente as faculdades do sonho garante àquele que as conduz unir em seu favor o conhecimento entre mundos distintos.

O *sangue* corre pelas veias assim como corre pelos mundos, conduzindo tanto experiências como possibilidades. O sangue na Amazônia indígena é comumente associado à pura agencialidade, como fluido corporal, delimita o seu invólucro, distribuindo gênero, ideias e força à pessoa, permitindo a condução do conhecimento para todo o corpo (Belaunde, 2006: 207). De um modo geral, se o sangue parece consubstancializar o pensamento, para os Mura ele é relação. Diziam meus informantes que a cabeça é a guia dos pensamentos, depósito dos bons e maus desejos que estão envolvidos na tarefa de coesão e manutenção da comunidade, assim como de sua dispersão, mas, como *sangue*, o pensamento transmitia-se transversalmente, ou melhor, transcorporealmente. Afinal, os pensamentos eram mais uma maneira de traduzir a relação, a experiência pessoal que conecta um aos seus outros. Sabemos, por meio da literatura etnológica, como na Amazônia a valoração do conhecimento diz respeito à autoridade daquele que o presenciou. Quando vinculado a outrem, sua validade é proporcionalmente questionada à medida da extensão da cadeia de transmissão que o conduziu (Gow, 1991: 151). Nesse sentido, o *sangue* é outra maneira de tornar o

conhecimento possível. Sendo ciência do corpo vivo, ele é também a habilidade de comunicação com outras pessoas em outros mundos. Tanto aquela ativada pela prática xamânica, na qual os *mestres* do pajé lhe transmitem a receita que servirá para atender os males de seus pacientes, como aquela entretida involuntariamente pelas pessoas comuns.

As narrativas em torno do tema dos sonhos exemplificavam os modos distintos em que uma relação poderia se estabelecer. Em uma roda de conversa, uma mulher anunciou certa vez como *sonhava feio* sempre que dormia com as luzes apagadas<sup>4</sup>. Em outra oportunidade, um homem contava-me como, enquanto dormia em sua rede no porto em Borba, era *amassado* por uma entidade que queria levá-lo. Traduzindo: queriam levar sua *sombra* para longe do convívio humano. Sonhar *feio* é uma alusão a maus encontros em contextos oníricos, um estado em que não se é completamente dono de si e se está cercado por estranhos. Esse estado indefenso do corpo a bordo da pequena morte diária que nos acomete a cada início de noite era um anúncio claro da associação do movimento do corpo ao *sangue*. A inércia corpórea era o resultado óbvio da ausência do *sangue*. Não de sua totalidade, afinal, o indivíduo não estava morto de todo, ainda respirava e estava ali para aqueles que o queriam em caso de necessidade. Parte do *sangue* estava ausente, uma parcela fragmentada de um de seus *corpos* atuava em paragens distantes. Desguarnecido de um de seus princípios vitais, restava ao corpo subsistir com sua *sombra*, aberto à intervenção alheia. A aventura do *sangue* é a infelicidade da pessoa, um ensejo para o desvio da *sombra*. O sonho era um exemplo de abertura do corpo – um canal que se abria em mão dupla – exercendo o potencial de comunicação resguardado pelo invólucro físico, ao mesmo tempo que exaurindo o daquilo que fincava sua existência naquele lugar e naquele momento. Afinal, se é possível deparar-se com amigos enquanto se sonha, igualmente é provável deparar-se com inimigos ou não amigos.

A pessoa mura era fragmenta, bem como as parcelas que a compunham também o eram. Haviam dito isso igualmente sobre a *sombra*, o desvencilhamento de um mero “pedaço” de *sombra* era o suficiente para pôr risco à saúde e à vida de qualquer um. O mesmo valeria para o *sangue*, passível de ser derramado: sua perda poderia resultar em desdobramentos reversíveis. Certo era que a pessoa não subsistia com a ausência completa do *sangue* ou da *sombra*, no limite, a consequência primeira era a morte. Não sabemos ao certo de que é composto o *sangue*, uma constituição específica jamais foi aludida quando o assunto era questionado. Certo é que se dizia que algumas pessoas teriam *sangue forte*, o que não necessariamente seria oposto a ter *sangue fraco*. Ter o *sangue forte* significa estar sujeito fisicamente à sua latência, de modo que cortes e outras feridas tardam a cicatrizar-se. Erupções cutâneas também são justificadas por um caráter hematológico especial. Uma constatação que se averigüe desde a

4 A esse respeito, os Kaxinawá dizem que a luz é importante para afastar o *yuxin* (Lagrou, 2007). Em suas casas, costumam dormir com as luzes acesas, condição peremptória após o falecimento de uma pessoa, quando a aldeia se torna mais vulnerável às visitas daqueles que não estão mais vivos (*ibid.*: 336). Para os Mura, a luz é igualmente importante para afastar entidades estranhas e afins ao sequestro de *sombras*. Em casas de recém-nascidos ainda não batizados sempre se deixa ao menos uma luz de lanterna, como precaução, para vencer o breu da noite.

infância, diretamente relacionada à saúde do corpo, igualmente se relaciona à condição do *sangue* como um elemento de constituição autônoma e inconstante. Um indivíduo não apenas nasce com o *sangue forte*, mas a própria alimentação pode contribuir para que a pessoa “fortaleça” seu *sangue* – o café, por exemplo, era um dos produtos recriminados com esse efeito. Da força do *sangue* não se extrai, por conseguinte, a força do indivíduo, como qualidade, destaca-se como um atributo êmico do próprio *sangue*. Uma dedução lógica para uma substância que impera como entidade de agência individuada.

Não era para pouco então que sangramentos deviam ser contidos a todo custo. Mesmo a primeira menstruação de uma mulher era pensada como uma espécie de hemorragia a ser contida, afinal, a circunscrição da moça já *formada* dentro de sua residência durante o período de resguardo equiparava-se à contenção do escoamento de *sangue* para fora dos vasos sanguíneos. Existiam tratamentos locais tradicionais para a cicatrização de feridas, entre eles a banha de *sucuriju* (*Eunectes murinus*). Sua aplicação originava-se de uma propriedade do próprio animal que, afirmavam, voltava a se recompor caso fosse partido. Outra medida bem comum para o estancamento de hemorragias costumava ser o uso do próprio pó de café, o mal e a cura desse sistema vital. Derramar *sangue* equivale a perder a agência e a subjetividade que define a carne como viva e que conduz o corpo pelo mundo em que ele foi moldado. Ocasiona-se, dependendo das circunstâncias, um acesso pelo qual o mundo se abre, ameaçando o idílio de segurança sobre o qual ele foi construído. Põe-se o risco de perder o caminho de retorno à morada, o lugar onde a pessoa sabe quem são seus pares. Ao morrer, o corpo mura perde por completo o seu espírito (*sangue*) que, diferente do que os Jívaro contam sobre o “espírito verdadeiro”, não se transforma em névoa ou nuvens (Harner, 1972: 151), mas ascende ao céu como um pássaro. Todos esses elementos parecem ser facetas que envolvem a condição de vivente. Perder *sangue* registra-se, então, como uma faca de dois gumes: por um lado, afasta a vida do corpo e, por outro, aproxima a vida de *outro* mundo.

#### FORA DO CORPO

Circulando no interior do corpo, o *sangue* existe como um dos bastiões que asseguram a propriedade da vida. Fora dele, sua existência, que se traduzia como um movimento ordenado e seguro, desvelar-se-á como um movimento desordenado e imprudente. Exaurindo-se do corpo até o limite mortal, o *sangue* como substância agentiva guarda novos efeitos quando associado à pessoa a partir de um vetor externo. Se, quando está acobertado pela carne, ele governa a relação entre a vida e a morte, quando é eximido de sua função corpórea, o *sangue* sanciona a relação entre o ser e o não ser. Para os Pirahã, o sangue é a substância

a partir da qual é possível transmutar a forma (Gonçalves, 2001). Capacidade semelhante é conferida ao *sangue* pelos Mura, uma definição que se vê atualizada em dois contextos modelares de relação com o *outro*: a caça e a pajelança. Já tendo aludido ao *sangue* como relação, passamos agora a analisá-lo não mais em um contexto de semelhança, mas de alteridade. A posição do *sangue* como condutor de transformações assevera para esses dois contextos relacionais a constante disputa de posições (Lima, 1996; Viveiros de Castro, 1996). Em um mundo onde há uma plena dispersão de perspectivas (Viveiros de Castro, 2015), o controle e a ciência sobre as chaves dessas transformações resguardam a pessoa de perder-se do convívio com seus semelhantes.

Falar da prática da caça, no rio Igapó-Açu, não se limita à descrição de uma atividade econômica relativa à subsistência daquelas populações. Aproximarmos-nos dos ímpetos que a governam traz à tona um *ethos* particular e perigoso. O caráter escuso e paradoxal do personagem caçador, imbuído da tarefa de garantir parte da alimentação que compõe a dieta de sua família, revela-se a partir do *sangue*. A arte da caça é a experiência do matador. Adentrando as sendas que se abrem pela mata, ele afasta-se pouco a pouco não apenas da aldeia, mas alija-se também do ideal que lá se acomoda. Ganhando ressonância, em muitos contextos ameríndios, com a guerra (Viveiros de Castro, 1986; Garcia, 2012; Lima, 1996; Descola, 2006 [1993]; Maizza, 2012), a caça para os Mura não deixa de estar associada à morte iminente, mas dela também emerge o risco da transformação. Quanto mais distante do terreiro da aldeia e mais próximo do *centro* da mata, mais se assevera a mudança que se abre com as possibilidades que aquele novo contexto social oferece. Adentrar o espaço da socialidade animaléscica demanda uma nova postura, um comportamento que ganha igual distância daquela que separa o caçador de sua casa, que o alija da condição humana.

*A onça todo tempo ela é sagica, duro [sic], não é todos os bichinhos que andam com ela não, ela carrega um touro. Mesmo assim é a gente, gente que come macaco, que come rã, sucuriju, essas coisas, que não têm outras coisas, têm que comer tudo que a gente vê na mata aí fica forte, fica umas pessoas duro [sic]. Pra adoecer é muito difícil... (Entrevista, aldeia Fé em Deus, 2014).*

Nesse excerto, a menção da onça não se faz despropositadamente. Desde sua mirada, vislumbramos o protótipo do caçador. Como arquétipo, ela é a referência para as condições adversas que vigoram na floresta. Subentendida, ainda, ocultando-se atrás da figura da onça, está uma qualidade subjetiva associada ao *sangue* e que parece moderar os ânimos daqueles que vivem na floresta: a raiva. As virtudes elencadas pelo informante a respeito da onça (dureza, força, invencibilidade) convergem para um mesmo fim quando orientadas pela raiva: a pre-

dação. Alimentar-se de *sangue* é alimentar-se *como* uma onça, em certo sentido, ser uma onça e, por isso, estar imbuído das mesmas qualidades que o predador. A saída de uma pessoa para a caça é marcada mais do que pela ausência em sua residência, ela sela também um processo provisório de mudança. Deixando de ser cônjuge ou progenitor, a pessoa abre-se para novas relações. Segue por um caminho pelo qual é esperado saber retornar; como *foi*, deve *voltar*. Novamente, o *sangue* surge como intermediário do risco de transformação, por essa razão, a presença da onça na fala de nosso informante se justifica. Quando o *sangue* está sob o julgo, a onça, ou jaguar, surge como referência para conjugá-lo: “O sangue (como seu odor), funciona como indexador sensível de agência transformativa” (Fausto, 2002: 18), sua potência reside na capacidade de ingestão das “qualidades subjetivas” (*Ibid.*) daquele tomado como presa.

Como modelo de atuação no plano da floresta, agir como uma onça está associado ao ato de comer cru e, como tal, versa sobre a possibilidade de apropriação de qualidades anímicas de uma vítima. Alimentar-se de *sangue* é tanto nutrir a raiva, como ingerir uma força que ultrapassa o suporte carnal, estar além de si e, por isso, ser mais poderoso que outros que estão aquém de si. Podemos ver como, no discurso local, enunciado em contexto de caça, sangue (qualidade) e cru (condição) se configuram:

*Nós somos viciados nas coisas que têm sangue, a gente, pra nós, hoje em dia que não comer um negócio de um porco botando sangue não tá bom não, tem que tá com aquela força. Tem que tá sangrando, comer o bicho meio cru pra poder dar sustância. A gente comer um bicho bem assado, bem... Não tem sustância porque sai a sustância (Entrevista, aldeia Fé em Deus, 2014).*

A onça era apenas um dos modelos da predação mobilizados pelo imaginário e pela experiência mura, outro bem conhecido e constantemente citado, era a figura do índio bravo. Imagem do canibalismo, ao índio bravo – habitante das cabeceiras dos rios – era atribuída a prática de comer os próprios filhos. Baseando sua dieta no comer cru, existia, tanto quanto a onça, como um devir animalesco e possível. Ambos, a onça e o índio bravo, têm em comum o fato de serem *bravos*, ou seja, movidos pela raiva, representam inimigos e perigos temerosos<sup>5</sup>. Um modelo de agência a ser evitado, bem como emulado. Por essa razão, o *meio cru* da enunciação acima faz toda a diferença, pois o que se representa nessa cadeia alimentar omófaga é um contínuo de transformação<sup>6</sup>. Comer *meio cru* é ser quase canibal, estar a meio caminho de sê-lo ou não ser de fato.

Afastando o que está completamente cru de seu cardápio, o caçador age pela manutenção da sua condição humana. Uma postura que o assegura dentro dos limites da humanidade, como garante a segurança daqueles que esperam o

5 A imagem do *índio bravo* era comumente associada ao mundo do centro, o lugar onde só poderia imperar uma socialidade animalasca. As descrições que tinham essa figura como tema, costumavam atribuir a ela qualidades de bom caçador, como o olfato aguçado e o gosto pela carne. Quando os Mura aproximavam-se desse modelo, através de comportamentos que lembravam os modos de viver antigos, era dito que estavam *que nem mourão* (Fileno, 2016: 164).

6 Há outros modos de modular essa condição meio crua da comida, como me disseram: “água e sal, boto lá, já é o bastante”. O sal atua como elemento exógeno e modificador que realinha o alimento dentro dessa cadeia, retirando-o do cru absoluto. Entretanto, o sal também é uma marca da oferta do mundo branco e, por isso, atrai para o outro polo da cadeia de transformação. Uma experiência que os Karitiana, entre outros exemplos, suspeitam como a mudança do corpo para um cada vez mais semelhante ao do branco (Vander Velden, 2008).

retorno do dono da casa. É por essa razão que se tinha com reservas aquele que acabava de chegar da mata, recém-saído de uma condição de predador temporária. Ele representava ainda uma ameaça para aqueles não contaminados com a bagagem volátil do estar sob as regras de outro parâmetro de socialidade. Desde as *Mitológicas*, esses operadores lógicos são discutidos, como nos mostrou Lévi-Strauss, em um triângulo culinário. Para o alimento bruto, desenrolam-se duas vias de possibilidade: a transformação cultural provocada pelo cozimento e a transformação natural desencadeada pelo apodrecimento. Como polo não marcado, o cru conteria em si a condição de pura potencialidade (Lévi-Strauss, 2004 [1968]: 431). Consequentemente, para as comunidades indígenas do rio Igapó-Açu, o que está em questão é também a pura potencialidade do cru, viabilizada pela condição agentiva do *sangue*.

Como qualidade indispensável ao caçador que se embrenha pelas sendas da morada alheia, a raiva e todo o comportamento relacionado a ela deixa de ser apropriada no contexto da comunidade. Assim como o caçador deve se despir do semblante de matador, tudo que adentra a vida na aldeia, para que dela faça parte, necessita passar por um processo de amansamento. Desde os animais, entre uma grande variedade de espécies, para se tornarem animais de criação, até as próprias crianças, que ao nascerem são modeladas à face dos valores exigidos pela convivência comum. Diz-se, comumente, que é necessário tirar *enjoio de criança*, qualidade antissocial por excelência, daquele que há pouco está presente no mundo, não pode ser *bravo* e não pode estar *bravo*. Lembro, por exemplo, de um avô que questionava seu neto sobre seu humor: *tá bravo? Tá querendo comer gente?* Não era a primeira vez que esse temperamento era associado ao desejo por carne humana. A volatilidade inerente à raiva destaca-se nessa afirmação, pois, a despeito do tom jocoso do adulto dirigido à criança, sublinha-se uma alta gama de variedades em torno da raiva. Essas variações sondavam desde o mau humor de uma criança até o risco de canibalismo. Para os Airo-Pai da Amazônia peruana, a raiva também era pensada a partir de uma dinâmica particular. Elemento deletério, ela oferecia um empecilho para se viver bem no interior da comunidade (Belaunde, 2001). As crianças, cuja vulnerabilidade era uma constante preocupação para os pais, seguiam um longo processo de apaziguamento que, iniciado pelo batizado, deveria ensiná-los a pensar (*coalsaye*), ou seja, “escutar com propósito’ as palavras de conselho (*iyehuoye*) de seus antigos, internadas em seu coração (*joyó*), circulam em seu corpo, tornando-o hábil” (Belaunde, 2005: 157, tradução minha). Mais uma vez, vemos como o equilíbrio das relações sociais depende do frágil equilíbrio das relações fisiológicas, pois os afetos que tocam o corpo igualmente influenciam as relações que a pessoa do corpo entretém.

Já vimos como o *sangue* é um disparador, responsável por perfazer o caminho



entre o cru e o podre. Uma das versões dos polos que se abrem para o movimento de ser Mura e o deixar-se de ser. Uma abertura corpórea e igualmente ontológica. Afastar a condição humana está diretamente relacionado ao consumo desmesurado de *sangue*, como um desdobramento. A ação confirma a aquisição de um novo *habitus* e, dessa maneira, angaria um novo sentido sobre as coisas e sobre suas relações. A trilha de sangue deixada por um caçador também é signo da cadeia de evoluções que o conduziram a deixar as fileiras da humanidade. O *sangue*, logo, existe em um primeiro plano como um operador lógico de classificação, satisfazendo a necessidade de encontrar ordem no mundo. Do mesmo modo, exprime-se em um segundo plano à maneira de um reagente, dando espaço à transformação dos termos que existiam de antemão estáveis dentro de um sistema.

Das transformações involuntárias desencadeadas e conduzidas pelo *sangue* existem também aquelas operadas a partir de seu poder de atração e de sedução. Ademais do gosto do *sangue*, seu cheiro também se efetua como uma possibilidade de transformação ontológica. Essa situação era exemplificada no caso das mulheres que entravam em seu ciclo catamênico. Nesses contextos, o estado de alerta era acionado, como já salientamos. O *sangue* é um “operador de perspectivas” (Belaunde, 2006: 232), a ele se imputa uma capacidade transformacional e, sobretudo, uma função comunicacional. Sua ação transcende a função fisiológica para manifestar um efeito sociocosmológico. Para as meninas que alcançavam a condição de maturidade, os mais velhos costumavam afirmar que o *sangue rescende com o cheiro de abacaxi* para o boto, como explicaram: quando se corta um abacaxi, seu odor se sente de longe, para onde o vento bafejar sabe-se que um abacaxi foi cortado. Índice de um alerta constante e silencioso, a menção ao boto vigorava entre narrativas míticas e experiências cotidianas. Servia de sobreaviso, anúncio de um risco que poderia atingir a qualquer um. O perigo do boto, na verdade, era a ameaça das *gentes* que habitavam outros mundos. Sob a pele de boto, ocultava-se uma *gente* não como qualquer outra, mas da qualidade daquelas que viviam no *fundo* do rio. Para o boto, o *sangue* da mulher, caso ela estivesse menstruada e próxima à beira do rio, sentir-se-ia de longe como uma fragrância que exala desejo e convite. Sobre o abacaxi, também afirmavam que ele era perfume, sublinhando com essa tradução o fator sedutor e atrativo do *sangue*. Tal influência envolvia mormente as entidades pérfidas que habitavam outras paragens e que mantinham como único interesse a atração dos humanos que viviam em terra. Se, no *centro* da mata, o *sangue* era um estímulo para alimentar a cólera, no *fundo* do rio, ele era um disparador para incitar a atração. De um lado e do outro, o *sangue* era mobilizado pelos Mura para nos ajudar a pensar a relação.

A aldeia vê-se como ponto nodal de um amplo sistema que engloba troca de informações, comunicação e transporte; seu acesso, contudo, não se faz

livremente. Por ser demasiadamente aberto, ele é capaz de desintegrar-se. A abertura das vias comunicacionais da aldeia para a floresta e da aldeia para o rio existem como possibilidades relacionais das quais se depreendem os perigos da mudança de posição, da perda da sanidade e, finalmente, da desumanização. Desguardadas, as divisas dessas passagens seriam abertas pela inobservância dos tabus e pelo descuido dos desprevenidos e, principalmente, pela desproteção inata do recém-nascidos. É nesse ínterim que a atividade de pajelança encontra seu principal espaço de atuação. O pajé é o responsável por estabelecer relações comunicacionais entre mundos. Sua ciência está sumariamente submetida a uma habilidade que foi tanto concedida como originada de um dom nascido com sua pessoa (Fileno, 2016: 209). Nas aldeias do Igapó-Açu, a prática-xamânica existia como um sistema social de saúde (Langdon e Wiik, 2010). Realizando suas atividades em conjunto com outras especialidades, o pajé exercia a tarefa de intermediador entre mundos, agindo em favor dos seus pares para obter a cura no local onde o mal teria sido desencadeado.

Para exercer a sua atividade, os pajés contam com o amparo de entidades auxiliares, *seus homens*, que agem em e para o seu nome, pelo mesmo respeito que se deve ter em relação aos próprios pais. *Seus homens*, os *caboclos do fundo*, existem como linhas que se descortinam em todas as direções, andando em linhas retas. Eles ignoram completamente o traçado curvilíneo do rio para atingir os seus fins com o máximo de eficiência possível. Por isso, também são cunhados como *linhas*, porque estão lá ao mesmo tempo em que seguem andando por diferentes lugares. São vias de acesso que possibilitam o contato direto ao corpo do pajé com os *corpos* das entidades, seus filhos, que residem no *fundo* do rio. Esta descrição nos traz à memória os “finos fios de algodão” dos xamãs waiãpi (Gallois, 1996), mas, em vez de algodão, aqui os finos fios das conexões multicorpóreas são possibilitados pelo *sangue*. Como descreve Dominique Gallois, os especialistas waiãpi têm todo o tronco e os pulsos envoltos por longos fios, partindo em direções diversas, tornando possível a conexão entre o xamã e os mestres dos seres que habitam o mundo (*Ibid.*: 41). Para os Mura, o que poderia ser descrito como finos fios igualmente se determina como *sangue*, levando o pajé às suas entidades auxiliadoras – um laço que vincula a proximidade que existe entre a pessoa do pajé e a experiência de viver em outros mundos. Os processos desencadeados por esses especialistas descrevem-se como fluxos de pensamentos, *corpos*, substâncias – a matéria bruta da definição de relação – que terminam por sublinhar a tangencialidade desses contratos invisíveis.

Para realizar o seu *trabalho*, diz-se que o corpo do pajé *se atua*, recebendo a visita de outras entidades que o tomam como instrumento para falar. Na aplicação reflexiva do verbo atuar, o que está em questão é a encenação do papel de si pelas mãos de *outro*. Através do corpo do pajé, *seus homens* apresentam-se à au-

diência cativa que aguarda notícias do *fundo* do rio sobre o enfermo, o pleiteante de cura e atendimento – ele é mais um dos que acompanham o cerimonial. Assumindo a posição do *outro*, sendo-se Outro, o pajé torna possível a ajuda aos Seus.

Os pajés com quem conversei frisavam constantemente que aquele era o seu corpo, mas aquela não era mais sua pessoa. A cisão daquele ser representa a ausência e a presença de seu corpo a um só tempo. Logo, todos os atos ali praticados diziam respeito ao discernimento de outro ser, mesmo a comida e a bebida consumidas dirigiam-se ao estômago de *outro*. Para além do movimento, o *sangue* é a própria referência de consciência sobre o mundo e sobre os seus parentes. À diferença das pessoas comuns, o pajé guarda a habilidade de controlar o movimento desordenado do *sangue* quando desenlaçado de seu suporte carnal. Quando o pajé *se atua*, o *espírito* sai, e é nesse momento o *sangue* inicia sua viagem por paragens longínquas. Algumas vezes, o pajé visita parentes seus que estão longe, mas, na maioria das vezes, sai para apreciar as belezas que se encontram no mundo das *gentes* do *fundo* e do *centro*, lugares de origem das entidades que estão sob o seu comando. Através do dom e da posse do conhecimento adquirido, o pajé manipula as particularidades extracorpóreas do *sangue*, dirigindo seus pensamentos aos seus *outros* parentes, com quem guarda um contínuo contato, assim como recebendo pensamentos que por eles são enviados.

Todo o *trabalho* sucede-se dentro da Seara, o espaço de comunhão entre humanos e não humanos regrado pelas leis do *convite* e da *confiança*<sup>7</sup>. Quando os *caboclos do fundo* visitam a Seara, gostam de encontrar tudo em silêncio – explicavam que *eles* não acham *bonito* que se façam gracejos e não consentem que o público presente cruze as pernas. Pernas e braços cruzados *trançam as linhas*, dificultando seu transcurso. O que se vê nesse ambiente é o estabelecimento de vias comunicacionais que aproximam aquele quarto dos mundos ao seu redor. Essas vias de acesso devem estar desobstruídas, sua interrupção ou corte dificulta a comunicação e obstaculiza a própria passagem pela qual se aproximam as entidades que participarão do *trabalho*. Por essa razão, as mulheres que estivessem em seus dias de resguardo não podiam passar atrás do pajé, sua condição exposta abriria um novo caminho, que cortaria os anteriores, interrompendo o fluxo sanguíneo que conectava os donos a seus auxiliares. Uma linha de *sangue* em sentido perpendicular cortava as outras. Acidentes como esse poderiam significar a perda momentânea ou permanente da conexão que o pajé mantinha com *seus homens*, bem como a abertura de ofertas a contragosto. Trata-se de uma relação de *sangue* e conhecimento sobretudo.

A Seara é um espaço de compartilhamento e de relação entre humanos e não humanos, uma agremiação cujos critérios de associação se baseiam em requisitos metafísicos. Olhando para o centro da Seara através de uma “teoria generalizada de fluxos” (Deleuze e Guattari, 2010 [1972/1973]: 348), vemos como

7 *Convite e confiança* existem, respectivamente, como instituição e valor importantes dentro das relações estabelecidas. O *convite* é mediador de visitas intercomunitárias, trabalhos coletivos (*ajuri*) e mesmo das relações entre afins, contudo ele opera a partir de um precedente. As relações de cuidado, criação e convívio são a matéria da qual a *confiança* é forjada, fabricando o caminho pelo qual os *convites* são endereçados (Fileno, 2016).

as relações são organizadas em torno da sua direção e de sua origem, pois de onde vêm e para onde vão devem ser condições conhecidas de antemão. O caos das relações generalizadas, estendidas para além dos circuitos da aldeia, é controlado no espaço da Seara visando um mesmo fim. A direção e a origem desses fluxos podem ser pensadas como modulações do *convite* e da *confiança*. O que se espera dos caminhos traçados é que eles sigam por um mesmo sentido, sem interrupções ou desvios, pois o fazem demonstrando a validade da confiança depositada naquele que os convidou. O receio depositado na presença de mulheres no período de resguardo e nos braços e pés cruzados da audiência é índice de uma abertura que deve ser feita de forma controlada. Uma passagem que deve ser aberta e fechada rapidamente para garantir a conclusão do trabalho e a salvaguarda da sanidade dos presentes e do pajé.

Muitas mulheres trabalhavam com pajelança no Igapó-Açu. Recebendo um dom que lhes fora transmitido pela família, elas intercediam de pura vontade pelo restante da comunidade. Contudo, a condição de maturidade feminina colocava um obstáculo ao exercício de sua tarefa. Explicava-se que, enquanto sobreviessem os períodos de menstruo, ao *trabalho* eram antepostas pausas irremediáveis. Nessas ocasiões, as pajés recusavam atuar em qualquer tipo de *trabalho*. Períodos de interrupção que obedeciam aos ciclos menstruais, interregnos que continuariam até que a pajé chegasse à menopausa, angariando a condição de homem, como uma delas sentenciou. À diferença das mulheres, os homens não se expunham e não ofereciam aos outros os riscos mensais que os fluxos involuntários de *sangue* criavam. A chegada à menopausa para os Macuna era tratada de maneira semelhante, cuidada com coca e tabaco pelo xamã. A mulher desde ali representaria um homem e estaria afeita mesmos aos riscos provocados pelo sangue menstrual que qualquer outra pessoa (Arhem apud Belaunde, 2005: 135). Os pajés mura demonstravam sua condição de alerta logrando até mesmo enxergar a condição colocada pela menstruação das mulheres, bastando que essas estivessem às suas vistas. Esse fato atestava a habilidade desses especialistas de divisar o invisível.

Os fluxos controlados são os caminhos privilegiados dos *caboclos* que pertencem à *mesa* de *trabalho*, os quais não deixavam de ser igualmente usufruídos pelas entidades que lhe eram estranhas. Em certo sentido, os caminhos eram os próprios *corpos*, que produziam um rastro atrás de si, liberando, fosse mão de ida ou de volta, uma passagem possível. Assegurando que *seus homens* seguissem o percurso de seus calcanhares, os pajés eram afiançados pela segurança que eles ofereciam, negando oportunidade para o desmedido atrevimento daquelas entidades que eram *confiadas*. Moderar o fluxo do *sangue* próprio e alheio, independente do gênero, logo, era o principal meio de dominar as passagens sempre imprevistas e invisíveis que existiam entre os mundos.

As conexões sanguíneas são a razão das possibilidades comunicacionais se estenderem para além de onde a vista é capaz de chegar, bem como são vias por meio das quais o conhecimento pode ser alcançado e transmitido. Os pajés conservavam um contato pleno e contínuo com suas entidades auxiliares, sentiam o que elas sentiam e sabiam o que lhes passava.. Narravam com propriedade as aventuras e desventuras de *seus homens*, os momentos de briga e namoro, bem como tinham notícia da morte de um ou de outro quando essa ocorria. A condição extracorpórea voluntária do pajé, condicionada aos contextos de sua atuação, apresenta as consequências máximas a que se pode chegar no entendimento do *sangue* como movimento, relação e ciência. O *sangue* é mais um indício de como a abertura ao Outro também é passível de ser tecida por canais sensíveis e invisíveis.

### CONSIDERAÇÃO FINAIS

O *sangue* existe no mundo como plena possibilidade relacional, seu fluxo abre-se como vias de conhecimento, mas também de transformação. Matéria plena, volátil e divisível, ele também abunda no plano do invisível por diferentes caminhos. O *sangue*, quando está dentro do corpo, é visível, circula de maneira ordenada sob a pele e a carne, e gera um movimento ordenado e bem calculado pela cabeça que o guia. Fora do corpo, o *sangue* é invisível. Nesse estado, ele age como pleno movimento desordenado, sem orientação de seu respectivo dono, transformando-se em agente de relações que levam a pessoa cada vez mais distante da atenção que a deve fincar entre os seus pares. Passível de ser alimentado pela raiva, impinge-se em um movimento cada vez mais descentrado de sua origem. Os caminhos pelos quais o *sangue* escorre atingem planos e distâncias indiscerníveis pelos humanos. Criam extensões do desejo de estar com alguém que vive longe, assim como dão ensejo aos múltiplos devires que residem no corpo. Tudo se passa como se a corrente sanguínea, ademais de distribuir nutriente por toda extensão do corpo, abrisse possibilidades de relações por toda a extensão dos mundos desconhecidos.

Para os Mura, o que está em questão é o problema da abertura e do fechamento excessivos, um risco para si e para o social do qual a pessoa faz parte. Subentendido como uma variante do tema da instabilidade dos corpos na Amazônia (Taylor, 2012 [1996]; Viveiros de Castro, 2002; Vilaça, 2005), revela-se para as gentes do Igapó-Açu como um problema que transpassa o público e o privado, o sociológico, bem como o cosmológico. Fator de diferenciação entre os gêneros, ele também é uma referência de moral. Costumavam me dizer, falando desse entre outros assuntos, que cada qual tinha o seu lugar. Mudar esse esquema de ordem era obrar sobre a compleição do mundo e também dos seres que o habi-

tam. O *sangue* é o motor dessa operação, pois converge, para uma mesma ocasião e lugar, mundos que existem contiguamente. Colocando modos de operar distintos e opostos sobre um mesmo plano, ele faculta a mudança de um pelo outro.

Ao referenciar o *sangue* em suas múltiplas possibilidades, os Mura estão traduzindo, através de uma gramática sensível, a experiência do indizível, do invisível, do inexplicável. O que em nossa tradição ocidental é particionado entre grandes divisores encontra no idioma do corpo mura um único caminho, atualizando experiências díspares pela sua concretude. Planos distintos, logo, podem ser atravessados por um contínuo que impõe à relação a exigência da proximidade e do contato para que ela se efetive. Existindo dentro e fora do corpo, manifestando-se nos dois lados da divisa, o *sangue* apresenta-se como uma dobradiça tripla entre a natureza, a cultura e a sobrenatureza. Uma passagem que existe menos como um caminho único do que como um indício para repensar divisões que não fazem sentido em certos rincões deste planeta.

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**Fernando Augusto Fileno** é mestre pelo programa de pós-graduação em Antropologia Social da Universidade de São Paulo. Possui graduação e licenciatura em História pela Universidade de São Paulo (2012), atualmente realiza atividades junto ao CEStA/USP, Centro de Estudos Ameríndios, do qual faz parte como pesquisador.

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# O *si indelegável* como aposta contra a inércia e a apatia na contemporaneidade

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**Sara R. Munhoz**

📍 Universidade Federal de São Carlos | São Carlos, SP, Brasil  
✉ [sararmunhoz@gmail.com](mailto:sararmunhoz@gmail.com)

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*Désobéir*, publicado em 2017 pela Abin Michel e recentemente traduzido por Celia Euvaldo, é conduzido inteiramente por um desassossego, uma urgência frente ao intolerável. Diante dos escândalos

obscenos da desigualdade social (justificada pela matemática e seus efeitos de realidade), do sufocamento da Natureza (incapaz, pelas sucessivas intervenções técnicas à que foi exposta, de recuperar seus ciclos de renascimento) e do processo contemporâneo de criação de riquezas (cujas engrenagens fundamentais são a dívida e especulação), os criadores irresponsáveis desse mundo silenciam. O escândalo da inércia movimenta o trabalho de Gros, e uma indagação mais apaixonada que rigorosa funciona como ponto de partida de um debate ético, capaz de borrar os binarismos entre sujeito e sistema, entre ação individual e coletiva.

A disposição do texto expõe o leitor à sensação constante de deslocamento, de mudança de rumo. Passamos por capítulos breves, que adicionam sempre novas possibilidades de definir e compreender a obediência e a dificuldade profunda de desobedecer. Eles sequenciam-se, adensando o argumento, mas, ao mesmo tempo, podem ser encarados como pequenos excertos avulsos. Gros esmiúça as nuances entre submissão, subordinação, conformismo, consentimento, obrigação; e também entre rebelião, resistência, transgressão, desobediência civil e dissidência cívica. Trata como variações de estilos éticos o que é comumente reunido sob o binarismo obedecer-desobedecer: cinco tipos de obediência, com seus cinco espelhos inversos. Dialogando com autores como Dostoiévski, Arendt, Kant e Foucault, e transitando com presteza entre literatura, acontecimentos históricos (em especial os relacionados ao nazismo e às indagações que dele se deprenderam) e debates filosóficos, finca sua questão

nos seguintes termos: “a partir de que relação consigo mesmo se respeita ou se transgride a lei pública?” (: 35).

O desfecho do livro confirma o que seu próprio arranjo já sinalizava: a aposta da filosofia (desde Sócrates, passando por Kant, Nietzsche, Foucault), compartilhada por Gros, está no deslocamento que o pensamento é capaz de provocar. Aí a humanidade, aí a possibilidade de ativação de qualquer tipo de dissidência, de resistência. Aposta filosófica “insensata, tenaz” (: 218) de que o *si indelegável*, inquieto, impelido a julgar, a pensar, possa, na covibração com tantos outros *sis* insubstituíveis, desencadear “a verdadeira política, a das desobediências” (: 218).

O caminho até o clamor final é longo, ainda que desembaraçado, ligeiro. E a voz do autor, colocando-se timidamente, quase sempre atravessada por outros autores ou por narrativas cuidadosamente compostas, se fará ouvir claramente nas últimas páginas do livro. *Desobedecer* parte de um incômodo indignado e termina com uma esperança singela, simples e, ainda assim, revolucionária. Mas até que cheguemos à asserção pela humanidade absoluta – porque singular – somos conduzidos por um trajeto que, pela obediência, nos expõe à possibilidade de inspecionar uma nova estilística, a do que desobedece. Logo de saída, a democracia crítica e a dissidência cívica aparecem como possibilidades da insurreição enquanto um regime filosófico do pensamento, um esforço contra o conformismo e uma vitória sobre si. Para expor seu argumento, Gros não recorre a descrições analíticas de movimentos sociais contemporâneos (ainda que eles não sejam ignorados), mas interroga as condições éticas do sujeito político que o impelem ou impedem de desobedecer.

Obedecemos porque somos submissos e uma relação de forças nos subjuga e aliena. A submissão expõe o paradoxo de indivíduos que são ativos em sua passividade; o submisso é aquele que nada inicia, que não pode pagar o preço da desobediência. Desmistifica, ainda, a política, ao tornar nítidas as imagens de guerra, de injustiça e de violência que nos espreitam fora do pacto social (e que nos acompanham, em alguma medida, também nele). Gros desmembra esse tipo de obediência em “submissão deferente”, que conserva, apesar das reverências ao senhorio, o julgamento crítico e a “propensão mais perigosa” de o submisso, ele mesmo, explorá-la como “alavanca de justificação” que o desresponsabiliza.

Obedecemos, por outro lado, porque nos subordinamos, idolatramos, somos agradecidos. Indagação aristotélica que sugere a existência de desníveis naturais, hierarquias inatas. A subordinação tem a relação parental como modelo de autoridade e se impõe sem coação nem violência. Obediência dócil, natural. Justificada na utopia política cristã pelas qualidades do dirigente, a subordinação admitiria, inclusive, a dissolução entre comando e obediência. A ordem, fim último da política, coloca até mesmo o dirigente à serviço, e expurga como loucura, irracionalidade, crime, qualquer possibilidade de desobedecer.

Obedecemos também porque nos conformamos ao hábito, à inércia passiva. Obediência sem verticalidade, que se ancora na consistência da ideia de “pessoas em geral” (: 98). O “seguidismo cego”, produz o alinhamento passivo dos pensamentos, comportamentos, desejos e identidades. Obediência das democracias liberais, das sociedades de consumo e de produção em massa, que contrapõe a uniformidade à ameaça do anarquismo cacofônico, do dismorfismo.

Obedecemos, enfim, porque consentimos, ainda que em um passado mítico imemorial, ainda que a possibilidade de escapar não nos tenha sido apresentada. Obediência política, do cidadão. Consentir implica um ato inicial de renúncia: aceitar depender de outro, constituindo-se como passividade. Trata-se da servidão voluntária, centro da relação política entre os contratualistas. Consentimos porque os custos da desobediência nos apavoram, porque o que faz de nós civilização é justamente a decisão de não mais decidir.

O autor tipifica os estilos de obediência assinalando com clareza suas diferenças sutis. Não obstante, são os entrelaçamentos hábeis entre os capítulos, que recolhem tudo o que escapa às classificações originais, que arrebatam o leitor. Com uma erudição desenvolta, apresenta diante de cada estilo de obediência suas possibilidades de superação. A partir de La Boétie e Hannah Arendt, incita-nos abandonar a superobediência, alicerce do poder político e da tirania. Demonstra como a liberdade exige uma revolta miúda, ainda que custosa: disposição ética a uma submissão ascética, que calcula insistentemente a possibilidade de obedecer *a minima*, que desencoraja em si toda chance de adesão, que desmascara o fascínio pelo Um, a lealdade idólatra aos que nos submetem. Resistir às comunidades de obediência massificadas com a “máquina de guerra” (: 62) da amizade, da pluralidade, da dispersão, das discussões ferozes sem ódio. Contraposição ao Um das tiranias com o “todos uns” – singulares, críticos, íntegros. Mais adiante, Antígona subversiva escancara o risco da desobediência, em sua beleza e sua intimidação: com suas recusas e revoltas, é ela quem “estremece a própria ideia de uma ordem” (: 90), desencadeando algo de selvagem, de incontrolável. A desobediência de Antígona não substitui o senhor, não invoca legitimidades superiores. Ela carrega em si toda a potência da transgressão pura.

Resistir ao conformismo como cético ou como cínico, mantendo o livre exercício do juízo ou ladrando as hipocrisias do sistema e a estupidez social, requer que nos coloquemos contra o que trata as diferenças como imorais, vulgarizando a humanidade. Exige uma assimilação do potencial totalitário das democracias liberais, tal qual apresentavam as distopias de Huxley e Orwell. Mas Gros apresenta ainda uma terceira possibilidade de resistência, por um desvio, um retorno aos gregos (via Foucault e Deleuze): o protesto da diferença, “da raridade do belo, dificuldade do verdadeiro, lirismo da erudição” (: 108). A exigência radical de elevação, a “recusa do maciço, da covardia, da humildade” (: 108), que

concebe a igualdade como igualdade de exigências, como capacidade de rivalizar. Universalização externa ao reducionismo normatizador.

Para inspecionar as possibilidades de desobediência civil, Gros coteja Hobbes e Rousseau descrevendo como o contrato social, que fez humanidade, implica justamente, entre eles, o consentimento às leis. Para Locke, ao contrário, a obediência política seria secundária, deriva do consentimento primeiro, horizontal, de “fazer sociedade”. Nesse cenário, a desobediência civil, longe de ser causa irresponsável das desordens ou atenção egoísta às convicções minoritárias, seria um retorno à essência viva do contrato. É a democracia crítica que exige para si mesma a desobediência e a legitima enquanto um processo que perpassa a todos exigindo mais liberdade, igualdade, solidariedade.

O nono capítulo de *Desobedecer* é um ponto de inflexão na obra, e dos mais belos. Nele aparecerá o *sujeito indelegável*, que acompanhará até as últimas páginas o argumento do autor. Gros caminha com Henry David Thoreau e esmiúça as fronteiras entre o conceito de desobediência civil e de resistência, ou dissidência. Por outra via, Gros retorna aqui à crítica à superobediência. Descreve a importância do fazer da vida um atrito ao “aparelho enorme e frio” do governo (: 153) que, quando bem lubrificado, torna-se perigoso. Não há vínculo entre moral e política e não há, ainda, a possibilidade da obediência no interior da reflexão crítica, como Kant sugerira.

Diferentemente dos submissos deferentes e dos conformados céticos, a defesa de Thoreau é pela desobediência como dever de integridade espiritual, sinceridade moral e superioridade ética (: 154). Desprezo radical, no limite, pelos próprios procedimentos democráticos que geram a obediência passiva de individualidades isoladas. A “ascese da desobediência” de Thoreau conduz Gros a uma dissolução dos limites entre o indivíduo e a coletividade, entre a ação e a passividade, entre a crítica e a transformação. Porque o sujeito indelegável se distingue brutalmente do individualismo, do relativismo e do subjetivismo. É a experiência da dissidência cívica, uma postura ética “a partir da qual a desobediência civil, como composição de potências, autentica-se e desencoraja antecipadamente qualquer recuperação politiqueria” (: 157), é a “urgência de repelir nossa inércia, de nos descobrir solidários e finalmente de nos sublevar” (: 158).

Depois da caminhada com Thoreau, dissidência cívica, obrigação ética e responsabilidade sem limites formam o tripé que arremata a reflexão de Gros. Com Sócrates, ele apresenta a experiência do intolerável e a impossibilidade consequente de obedecer. Dissidência cívica como reflexo invertido da submissão: “compreensão abrupta de que a obediência era uma sucessão indefinida de negações interiores” (: 168) tornando impossível a manutenção da inércia. Mas essa impossibilidade, essa experiência de urgência, não abstém o sujeito do exame constante e da coragem da verdade. É preciso, como afirma Kant, liberar-

-se das “delícias soberanas da desresponsabilidade” (: 161). É preciso ousar saber, desejar emancipar-se.

Em Aristóteles Gros encontra a caracterização da obrigação ética. A democracia ateniense exigia a dissolução entre o ativo e o passivo, entre dominante e dominado. A obrigação, ali, demandava um confinamento ético particular: na política, eu me obrigo, comando a mim mesmo, comprometo-me. A obediência política cidadã é voluntária, designa uma ativação de vontade (não uma abdicação) e opõe-se ao consentimento. Essa forma carrega em si “a possibilidade geral de desobedecer no interior da própria forma ética da obediência” (: 177). Porque a obrigação ética exige que nos mantenhamos amigos de nós mesmos e nos tornemos objeto de cuidado estético, constante e exclusivo: *epimeleia heautou, cura sui*. Com Foucault, Arendt, Ricardo III e Lorenzo Milani, Gros diferencia a via ética em relação à moral. “A condenação da injustiça não se faz aqui em nome de princípios transcendentais” (: 179), tampouco trata-se de uma resposta a um si egoísta, um *eu* íntimo. A obrigação ética é a “alavanca da desobediência”, a partir do qual eu me autorizo a aceitar ou recusar tal ordem, tal decisão, tal ação. É ela que, pela “bricolagem tenaz de nossas inquietações éticas” (: 182) nos faz desobedecer. Não o narcisismo complacente (erro dos que não compreendem Arendt ou Foucault), mas o exame dialético interminável de Sócrates, o cuidado de si; a coragem da verdade e o pensamento kantiano que não cessa de julgar.

O “pensamento pensante” (: 183) nos faz desobedecer, e o faz a partir do princípio da responsabilidade indelegável. Constituição de um si “obrigado de um outro, dos outros, de causas e de valores que ultrapassam o indivíduo” (: 184). A experiência do sujeito indelegável soma a obrigação ética à dissidência cívica e torna possível a insubmissão coletiva como “covibração de numerosos sis indelegáveis” (: 184), quando a urgência de reagir e a necessidade de desobedecer contaminam. Desobedecemos, em conjunto, “quando cada um faz a experiência da impossibilidade de delegar a outros o cuidado do mundo” (: 184).

O problema da responsabilidade permeia todo o livro. Em La Boétie, na descrição do julgamento de Adolf Eichmann, nos experimentos psicossociais que revelam o potencial torturador que há em cada um de nós. Mas é já nas últimas páginas do livro que Gros sistematiza essa noção a partir de uma pergunta muito simples: até que ponto desobedecer é responsável? Gros tipifica as responsabilidades infinita, integral, absoluta e global, que embora sejam “extremos irrespiráveis da moral” (: 192) funcionam como marcadores de uma provocação necessária às consciências. A responsabilidade doméstica “os núcleos éticos pelos quais nos devemos ao menos nos deixar atravessar”. A experiência da responsabilidade que se contrapõe à má-fé e à boa consciência, é capaz fazer surgir aquele que desobedece.

Uma última caminhada, com Sócrates, e um último alerta: se o pensamento

é a possibilidade contínua de desobedecer a si mesmo, é importante ressaltar que esse *si* como consistência, como reduto soberano, não existe. É um dois-em-um (de Arendt), uma relação consigo mesmo (como em Foucault). É a organização, em nós, das hierarquias e paixões que nos compõem. É a paixão de se descobrir insubstituível quando a serviço do outro, pela introdução de uma “defasagem irreduzível” de si para si. No retorno à Sócrates, encerra com um clamor insurgencial: “pensar é se desobedecer”, deslocar as certezas, os confortos, os hábitos. Pensamento combatente. Para Gros, é isso, afinal, que nos fazendo mancar, faz de nós humanidade.

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**Sara R. Munhoz** é doutoranda em Antropologia Social pelo PPGAS/UFSCar. Autora do livro *O governo dos meninos: liberdade tutelada e medidas socioeducativas*, resultado da etnografia desenvolvida em seu mestrado. É pesquisadora do grupo *Hybris* e tem experiência nas áreas de antropologia dos documentos, da família e do direito.

# SPECIAL ISSUE | Introduction: The German Tradition in Latin American Anthropology

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Han F. Vermeulen, Cláudio Costa Pinheiro and Peter Schröder

▲ Max Planck Institute for Social Anthropology | Halle (Saale), Germany,  
Universidade Federal do Rio de Janeiro | Rio de Janeiro, RJ, Brasil,  
Federal University of Pernambuco | Recife, Brazil

✉ [vermeulen@eth.mpg.de](mailto:vermeulen@eth.mpg.de), [c.pinheiro@sephis.org](mailto:c.pinheiro@sephis.org), [pschroder@uol.com.br](mailto:pschroder@uol.com.br)

The devastating fire in the 200-year-old Museu Nacional in Rio de Janeiro on September 2, 2018, demonstrates the vital importance of the historiography of sciences and the arts. As most collections have vanished, it is left to the history of science to pick up the pieces and present past developments in their complexity and global entanglements. In this special issue we aim to do this for the work of German and German-speaking anthropologists in Latin America, with a special focus on Brazil, carried out between the 1880s and 1945.<sup>1</sup>



**Figure 1**

Adolf Bastian on an engraving by Adolf Neumann of Presidents of the Berliner Gesellschaft für Erdkunde in the magazine *Die Gartenlaube – Illustriertes Familienblatt*, 1878

This period was important in the history of anthropology as ethnography, its most enduring incarnation, led to a strong and varied research tradition in the German-speaking countries, including Germany, Austria, and Switzerland. This tradition has a long and chequered history, going back to the early eighteenth century (Vermeulen, 2015), but it came to fruition in the work of Adolf Bastian (1826-1905), director of the Ethnological Museum in Berlin, and his associates Karl von den Steinen (1855-1929) and Franz Boas (1858-1942) (Figures 1-3).

The articles assembled here discuss various forms of ethnographic research and writing, the institutionalization of anthropology in Latin America, and the impact of German or German-speaking anthropologists on the trajectory the discipline took in Europe and elsewhere. They emphasize how these scholars con-

**1** It is an honor and a privilege to publish this special issue in the *Revista de Antropologia*, celebrating its 65th anniversary in 2018. Many of the names and ideas presented in this issue were first discussed on its pages. The guest editors are grateful to Prof. Dr. Laura Moutinho, editor-in-chief of *Revista de Antropologia*, and this journal's Board for embracing our proposal, the authors for their confidence in our project, the peer-reviewers for carefully evaluating the manuscripts, and the archives and institutes in South America, North America and Europe for making available 40 illustrations.

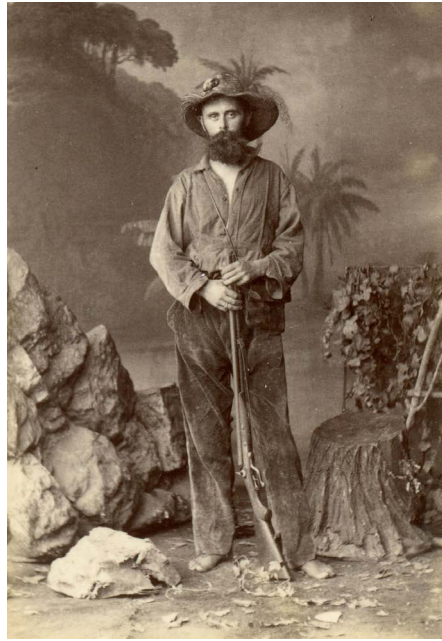


ducted research by means of peer-working with colleagues from museums and institutes based in German-language countries as well as with scholars and intellectuals in Latin America. Our focus on transnational networks demonstrates the strong and longstanding relations between scientific communities in Western and Central Europe with scientific communities in the Americas. Many of these scholars became “brokers” – *passeurs culturels* (Sanjad, 2015: 77) – between these communities.

The present issue is opened by Renate Brigitte Viertler, who presents von den Steinen’s ethnographic research in Brazil conducted during expeditions to the Xingu River in 1884 and 1887-88.<sup>2</sup> Diego Ballesterio analyzes the anthropological and ethnological studies carried out by Robert Lehmann-Nitsche in Patagonia, Argentina, 1898-1919. Peter Rohrbacher discusses astronomy, mythology, and Ancient Mexican Studies in Austria between 1910 and 1945, i.e., until the end of World War II. Michael Kraus places Max Schmidt’s ethnographic research pertaining to his doctoral thesis, *Die Aruaken* (The Arawak, 1917), in the context of contemporary German ethnology.<sup>3</sup> Erik Petschelies focuses on Theodor Koch-Grünberg, a “field ethnologist” who stood in close contact with Brazilian intellectuals.<sup>4</sup> Finally, Peter Schröder’s article about Curt Nimuendajú as a collector and researcher for ethnological museums in Germany discusses an episode in the history of German and Brazilian anthropology during the Weimar Republic, 1928-1930.<sup>5</sup>

These articles result from meetings of the History of Anthropology Network (HOAN) as part of a larger effort to provide more visibility to anthropological traditions that influenced the development of anthropology and ethnography beyond the hegemonic tradition of the English-speaking parts of the world (Ribeiro, 2006, 2014), in this case in Latin America. Five of the articles were originally presented during panels at the EASA conference in Milan (2016), the GGA conference in Berlin (2017), and the IUAES World Congress in Florianópolis (2018). They have been extensively rewritten and edited for the present purpose. The sixth article, by Michael Kraus, is the slightly expanded English version of an article that will soon also appear in Portuguese; it is published here for the first time.

Anthropology is understood here as a generic term for a group of studies including ethnology, ethnography, social anthropology, cultural anthropology, folklore studies, prehistoric archaeology, linguistics, biological anthropology, etc.



**Figure 2**

Karl von den Steinen posing in Rio de Janeiro, 1884. Photograph by Alberto Henschel & Ca. (Courtesy of Reimar Schefold, Amsterdam, The Netherlands)

**2** On von den Steinen, see e.g., Baldus (1954-68), Oberacker (1955), Schaden (1955, 1956, 1981, 1990), Coelho (1993), Schefold (2001), Hermannstädter (2002), Hemming (2003), U. von den Steinen (2010), Petschelies (2018).

**3** On Max Schmidt, see e.g., Baldus (1951a-b), Susnik (1991), Bossert and Villar (2013, 2015).

**4** On Koch-Grünberg, see e.g., Schaden (1953), Kraus ed. (2004), Kraus (2002, 2010), Guerra (2015). Mário de Andrade based his novel *Macunaíma* (1928) partly on Koch-Grünberg’s *Vom Roraima zum Orinoco* (Sá, 2004).

**5** On Nimuendajú, see e.g., Baldus (1945, 1946, 1954-68), Schaden (1946, 1967-68), Viveiros de Castro (1986, 1987), Hemming (2003), Schröder (2013), Schröder ed. (2015), Welper (2018).

A selection hereof is included in anthropology as defined in the US American “four-field approach”: cultural anthropology (previously referred to as ethnology), linguistics, archaeology, and biological anthropology. While the boundaries between these fields vary between national traditions in Europe, the UK, the USA, Latin America, or Asia, “anthropology” serves best to classify these specialties under a general label. For historical purposes, however, it is vital to study these traditions from a historicist perspective and use the terms practitioners applied to the fields they worked on. In the articles presented here, these comprised ethnology, linguistics, archaeology, and anthropology, i.e., biological anthropology. Whereas the four-field approach was first formulated in the Anthropological Society of Washington’s statutes of 1879 to include “Archaeology, Somatology, Ethnology, and Philology” (Vermeulen, 2015: 8, 422), the three-field Berlin Society for Anthropology, Ethnology, and Archaeology was founded in 1869.

**Figure 3**

Franz Boas posing for a figure in the diorama titled “Hamats’a coming out of secret room” at the United States National Museum, c. 1895. (Credit: National Anthropological Archives, Smithsonian Institution, Washington, DC, USA, Negative No. MNH 8302)

### TRAVELERS AND EXPLORERS

Travelers, explorers, missionaries, and scholars from present-day Austria, Germany, and Switzerland have visited parts of South and Central America and the Caribbean either on voyages of exploration or individually for at least 300 years. They have produced a significant body of knowledge concerning nature and indigenous groups of the southern and central parts of the Americas, including Brazil. Learning about nature often depended on the knowledge accumulated by native groups, translated through European expressions of curiosity.

German-speaking travelers were not the only ones. The first reports describing Brazil and its inhabitants were written by the Portuguese chroniclers Pero Vaz de Caminha (c.1450-1500) and Pero Cândavo (c.1540-1580), the Spanish Jesuits José de Anchieta (1534-1597) and Christóbal de Acuña (1597-c.1676), the Portuguese Jesuit Manuel da Nóbrega (1517-1570), the Portuguese explorer Gabriel Soares de Souza (c.1540-1591), the French Franciscan priest André Thevet (1502-1590), and the French Reformed pastor Jean de Léry (1536-1613).

The earliest German “reporter” was Hans Staden (c.1525-c.1576), a soldier and mariner who made two journeys to South America in Portuguese ships. On his second voyage, Tupinambá warriors captured Staden in 1552, assuming him to be Portuguese. Knowing Tupi after a 3-year sojourn in Brazil, Staden managed to sur-

vive for months among his captors before finally escaping. During his captivity he observed many aspects of their culture, which he recorded in a two-part narrative: *Warhaftige Historia* (printed at Marburg, 1557). A recent translation into English is titled *Hans Staden's True History* (2008).<sup>6</sup>

During the seventeenth century, information on Brazilian Indians was acquired by the French Franciscan Claude d'Abbeville (c.1590-1632) and Capuchin Yves d'Evreux (1570-1630); the Dutch merchant Joannes de Laet (1581-1649); the Dutch theologian and historian Caspar Barlaeus (1584-1648); the Flemish interpreter and ambassador of the West Indian Company Roulox Baro (see Albuquerque, 2006); the Spanish Jesuit Antonio Ruiz de Montoya (1585-1652); and the Portuguese auditor Maurício de Heriarte.

During the Dutch interlude (1637-1644), the German naturalist and astronomer, Georg Marcgraf (1610-1644), served as a member of Johan Maurits van Nassau-Siegen's mission to Recife in northeastern Brazil (Disney, 2009: 225). The published work of Marcgraf and Willem Piso (1611-1678), the Dutch physician and naturalist on this mission, included zoological, botanical, and ethnographic accounts as well as notes on medical plants, the first astronomical observations, and a lexicon including notes on Tupi and other languages: *Historia naturalis Brasiliae* (1648); the book was richly illustrated with woodcuts by the Dutch artist Albert Eckhout (c.1610-1665).

Another German naturalist active in Latin America was Maria Sibylla Merian (1647-1717). This illustrator traveled to Surinam in 1699-1701 on presumably "the first European voyage exclusively for scientific fieldwork" (Valiant, 1993: 470). Merian pioneered the metamorphosis of natural species, especially insects, as well as the classification of tropical fauna and flora. Her work influenced biologists such as Linnaeus and Darwin, but somehow fell out of the canon (Valiant, 1993: 475). Recently "re-covered," she stands out as a major figure in the fields of biology and anthropology, and is especially noted for her ethnographic manner of interviewing Native Americans and Africans about their uses of plants for healing purposes, thereby anticipating future ethnobotanical research (Valiant, 1993; Todd, 2007).

Two Austrian missionaries from Bohemia, Martin Dobrizhoffer (1717-1791) and Florian Paucke (1719-1779), worked in Paraguay from 1748 to 1768 and contributed to the ethnography of the Gran Chaco (Kitzmantel, 2004).

During the eighteenth century (systematic) ethnography and ethnology were developed in the Russian Empire, the Academy of Sciences in St. Petersburg, the universities of Halle and Göttingen, and in Vienna. German explorers such as the naturalist Daniel Gottlieb Messerschmidt (1685-1735), the historian Gerhard Friedrich Müller (1705-1783), and the naturalist Johann Georg Gmelin (1709-1755) described indigenous groups in Siberia. Müller developed an ethnographic program for the description of all Siberian peoples and their comparison among each other and with peoples of other continents. His influence extended to all later explorers

<sup>6</sup> Another early traveller was Ulrich Schmidl (1510-1581), who undertook an expedition to Argentina in 1534. He lived in South America until 1552 and also travelled through Southern Brazil where he met Guarani Indians.

of the Russian Empire as well as to historians such as August Ludwig Schlözer (1735-1809) in Göttingen and Adam František Kollár (1718-1783) in Vienna, who coined the terms *Völkerkunde* (1771) and *ethnologia* (1783), respectively (Vermeulen, 2015, 2019).

One of Schlözer's students, Alexander von Humboldt (1769-1859), set off on an epoch-making expedition through South America between 1799 and 1804. Together with the botanist Aimé Bonpland and Carlos de Montúfar, von Humboldt carried out a 5-year investigation of the interrelations between nature and the populations of Mexico, Colombia, and of districts bordering on the Orinoco and Amazon Rivers, i.e., not in Brazil.

Subsequent expeditions included the journey of Prince Maximilian zu Wied-Neuwied (1782-1867), a zoologist and disciple of the anatomist Johann Friedrich Blumenbach in Göttingen, who arrived in Brazil in 1815 and acquired ethnographic and anthropological material among the Botocudo Indians residing in the provinces of Bahia and Rio de Janeiro; his monograph avoided the prejudices of his time (Baldus, 1954-68, 1: 12).

In 1817, the zoologist Johann Baptist von Spix (1781-1826) and the botanist Carl Friedrich Phillip von Martius (1794-1868) arrived in Brazil as members of a group of Austrian naturalists to accompany Maria Leopoldina of Austria. Departing from São Paulo in 1820, both men reached Maranhão to then travel up the Amazon River coming across Indians who were culturally influenced by colonization. Although Martius held views on "degenerated Indians" that are now unacceptable,<sup>7</sup> he proposed a linguistic classification that proved useful to Karl von den Steinen's and Paul Ehrenreich's work on languages almost a century later (Baldus, 1954-68, 1: 12-13). Martius's essay titled "How the history of Brazil should be written" (1845), awarded by the recently inaugurated Institute for Brazilian History and Geography in 1843, influenced the historiography of Brazil as developed by Francisco Varnhagen as well as the sociological program carried out by Gilberto Freyre and others.<sup>8</sup>

Another naturalist accompanying the Archduchess Leopoldina, Johann Natterer (1787-1843), acquired ethnographic objects in Southern, Central, and Northern Brazil between 1817 and 1835, which are now kept at the Vienna Museum of Ethnology (Augustat, 2012).

Georg Heinrich von Langsdorff (1774-1852), a German-Russian physician who served as the Russian consul in Rio de Janeiro from 1813 on, carried out an expedition between 1824 and 1829. Organized by Tsar Alexander I, Dom Pedro I, and José Bonifácio de Andrada e Silva this expedition aimed at promoting commercial relations between the Brazilian and Russian empires. At that time, Bonifácio, who was also a mineralogist, invited his friend Alexander von Humboldt to come to Brazil in order to assist in increasing the yield of gold mines. Declining this invitation, von Humboldt travelled to Russia in 1827, at the invitation of Tsar Nicholas I, to study the gold and silver mines of Central Asia.

<sup>7</sup> On the problem of "degenerated Indians," see Pacheco de Oliveira (1987: 164 ff), Sallas (2010), Welper (2018).

<sup>8</sup> See Martius (1845), Salgado Guimarães (1988), Contijo (2010).

**Figure 4**

Karl von den Steinen during his second expedition to the Xingu River in 1887-88. Standing, from left to right: Januarío, Peter Vogel, Karl von den Steinen, Luis Perrot, Antonio; sitting, from left to right: Wilhelm van den Steinen, Paul Ehrenreich. (Credit: Ethnologisches Museum, Staatliche Museen zu Berlin, Germany, No. VIII E NIs 779)

## EXPLORERS AND FIELDWORKERS

The articles in this issue take up the discussion with two research expeditions led by Karl von den Steinen to Brazil during the 1880s (Figure 4). Inspired by the ethnologist Adolf Bastian,<sup>9</sup> von den Steinen conducted the first systematic ethnographic research in Brazil, following rigorous methodological steps German ethnographers had established during the preceding century. In analyzing von den Steinen's work, as well as that of his contemporaries and successors, it is not so much the countries (Germany, Austria, Switzerland), but the language area (German) that interests us. Some of the topics discussed in the articles are: (1) How did their anthropology reflect the authors' own experiences or wider representations of Latin America? (2) Which political, religious, social, and cultural factors influenced their research and reports? (3) What is the legacy of German-language anthropology in Latin America?

At the time of von den Steinen's sojourn in Brazil, the economic frontier to the hinterland was rapidly expanding. The colonizing Brazilian and non-Brazilian population deemed the Indians as having occupied too much land (a view again cherished by the current administration), that they were "lazy" and "good to be killed." In 1884, Dom Pedro II, Emperor of Brazil, assisted von den Steinen in exploring the possibilities of navigating along the Xingu River from Mato Grosso to Pará and then search for a fluvial connection that could benefit the regional economy.<sup>10</sup> Although the latter plan failed, the Upper Xingu region was opened up to ethnographic research. Accompanied by two other scientists, two Brazilian assistants and a group of Brazilian soldiers, von den Steinen set off on his first Brazilian expedition to this isolated and relatively unknown region, where he met Bakairí, Kustenuau, Trumai, Suyá, Manitsauá, Juruna, Nahukuá, Mehinako,

<sup>9</sup> On Bastian, see e.g., Steinen (1905), Baldus (1968), Koepping (2001), Fischer et al. (2007), Kraus (2007).

<sup>10</sup> Petscheli (2018) argues that Dom Pedro II was interested in the scientific results but also in maintaining his status as an intellectual. A fluvial connection to northern Brazil was especially in the interest of regional elites.



Yawalapiti, Aweti and Kamaiurá Indians. He revisited some of them during a second expedition (1887-88) when he also encountered Paresí Indians in Cuiabá and stayed among the Bororo residing in the Teresa Cristina colony along the São Lourenço River. These indigenous groups were already engaged in direct contact with the non-indigenous population of the Mato Grosso region. The extermination of the Indian population intensified in such a way that an Indian Protection Service was founded in 1910, aimed at transforming Brazilian Indians into good, hard-working citizens.<sup>11</sup>

The period between 1880 and the end of World War I can be considered a Great Age of German-language anthropology in Latin America. A series of expeditions, field research, and ethnographic monographs from this period have become classics in Americanist ethnology. Well-known representatives, in addition to von den Steinen, are Paul Ehrenreich (1855-1914),<sup>12</sup> Konrad Theodor Preuss (1869-1938), Theodor Koch-Grünberg (1872-1924), Max Schmidt (1874-1950), Felix Speiser (1880-1949), Fritz Krause (1881-1963), and several other anthropologists (see Kraus, 2004). The Brazilian anthropologist of German descent Curt Nimuendajú (1883-1945) worked in the same tradition, although he in later years of his career was influenced by North American anthropologists of the Boas school such as Robert H. Lowie (1883-1957) (Figures 5-9).

Other scholars of this era were the zoologist Hermann von Ihering (1850-1930), the botanist Ernst Heinrich Georg Ule (1854-1915), the archaeologist Friedrich Max Uhle (1856-1944), the Swiss naturalists Emil August Goeldi (1859-1917) and Jacques Huber (1867-1914), as well as the anthropologists Robert Lehmann-Nitsche (1872-1938), Wilhelm Kissenberth (1878-1944), Paul Kirchhoff (1900-1972), and Hermann Trimborn (1901-1986). Female scholars included the Austrian ethnologists Wanda Hanke (1893-1958), who studied indigenous groups in Brazil, Bolivia, Argentina, and Paraguay to acquire important collections of artifacts she presented to museums in Europe, and Violetta Becker-Donner (1911-1975), who worked for the Ethnological Museum of Vienna and conducted fieldwork in Africa as well as in Central and Northern Brazil (Sombrio and Lopez, 2011; Sombrio, 2014). The latter's husband, Hans Becker (1895-1948), worked on Indian studies in the Chaco Boreal (Becker, 1941).

In 1884, Goeldi disembarked in Rio de Janeiro, after being invited to work at the

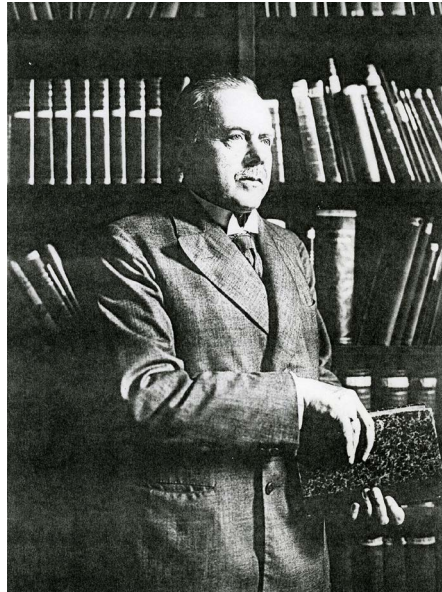
**11** The Serviço de Proteção aos Índios e Localização dos Trabalhadores Nacionais (SPILT) was the first official name of the Indian Protection Service (Serviço de Proteção aos Índios, SPI), created on June 20, 1910 by Decree no. 8.072. The SPI was Brazil's first federal agency charged with protecting indigenous peoples against all kinds of violence and the predecessor of the current National Indian Foundation (Fundação Nacional do Índio, or FUNAI), established on December 5, 1967.

**12** On Ehrenreich, see among others Hempel (2015).



**Figure 5**  
Paul Ehrenreich, 1890.  
Photograph by P. Grunder, Berlin  
(Credit: Staatliche Museen zu Berlin, Ethnologisches Museum, Archiv)

Museu Imperial e Nacional, now the Museu Nacional (sadly consumed by fire in 2018). During his formative years, Goeldi travelled between Switzerland and Germany (Leipzig and Jena) and carried out research near Lake Biel and the Gulf of Naples (Sanjad and Güntert, 2015: 27-28). Having spent five years at the museum in Rio he was dismissed in 1890; three years later, the governor of Pará requested him to reorganize and chair the Pará Museum of Natural History and Ethnography, founded at Belém in 1866. Upon his arrival, Goeldi employed a team of Austrian, German, and Swiss zoologists and botanists to study human interactions with nature, which led to innovations in the fields of botanical geography, plant sociology, and ethnobotany (Sanjad, 2015). In his honor, the museum was renamed the “Museu Paraense Emílio Goeldi” in 1902.

**Figure 6**

Konrad Theodor Preuss, c. 1909  
(Credit: Staatliche Museen zu Berlin, Ethnologisches Museum, Archiv)

Two ornithologists were of significance to the ethnography of Brazil. Emilie Snethlage (1868-1929) made several research trips to the Xingu River, lived among the Xipaya and Kuruaya, and acquired artifacts for the Museum of Ethnology in Berlin. She later directed the Museu Goeldi in Belém and supported Nimuendajú's research. Emile-Heinrich Snethlage (1897-1939) followed his aunt to Belém and undertook two expeditions in Brazil, the first of which for the Field Museum in Chicago; he later worked at the Berlin Museum.<sup>13</sup>

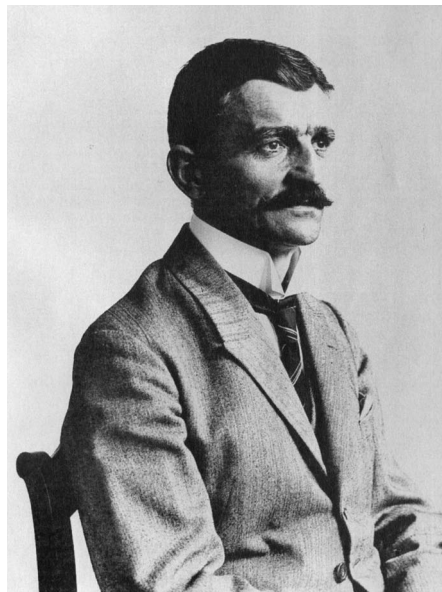
### THE GERMAN TRADITION

Anthropologists such as von den Steinen, Ehrenreich, Preuss, Koch-Grünberg, Schmidt, Kirchhoff, Trimborn, and the archaeologist Uhle conducted research in all regions of the subcontinent: Mexico (Preuss, Kirchhoff), Colombia (Preuss), the Andean region (Uhle, Trimborn), Patagonia (Lehmann-Nitsche), Chaco and Pantanal (Schmidt), and Amazonia (von den Steinen, Ehrenreich, Koch-Grünberg, Schmidt, Nimuendajú). German and Austrian museums supported their expeditions with clear objectives: to collect information on lesser-known regions and peoples, organize new ethnographic collections or complete existing ones and, in so doing, salvage testimonies of cultural diversity. In Imperial Germany, this financial support was nourished by a rivalry between ethnological museums, which were frequently maintained by donations from the local bourgeoisie (Penny, 2002). Thus, the opportunity to exhibit major ethnographic collections

**13** Other travelers in Brazil include the French botanist Auguste de Saint-Hilaire (1779-1853), the German teacher who lived in Brazil Erich Freundt (1905-?), the Czech botanist Alberto Vojtěch Fric (1882-1944), the Canadian ethnologist Kalervo Oberg (1901-1973), and the Russian ethnographer Henrich Manizer (1889-1917). In the second half of the nineteenth century the Italian painter Guido Boggiani (1861-1902) visited the Kadiwéu and documented their body paintings; the Brazilian Antonio Manuel Gonçalves Tocantins (d. 1876) wrote on the Munduruku (1877); José Vieira Couto de Magalhães (1837-1898) obtained information on tribes residing near the Araguaia River in 1863; the botanist João Barbosa Rodrigues (1842-1909) reported on tribes in the Amazon Valley; Telemaco Morosini Borba (1840-1918) described the Kaingang and Guarani of southern Brazil; and Alfredo Maria Adriano d'Escagnolle Taunay, the Viscount of Taunay (1843-1899), worked among the Terena, Guaikuru, Guaná, and Kaingang.

was part of a competition aimed at obtaining prestige.

These anthropologists shared thematic focuses: an interest in material culture, art, religion, mythology, and indigenous languages. These subjects were not studied in isolation, but understood as interconnected. At that time, the interest in material culture was related to the museum background of the majority of German and Austrian anthropologists, whereas the study of indigenous languages can be attributed both to linguistic instruction and to the idea that language could be the key to understanding the spiritual worlds of other people. As Vermeulen (2015) has demonstrated, the German-speaking ethnographers shared a strong linguistic tradition which, in principle, was not judgmental. In this connection, one may recall that two of Boas' students, Alfred L. Kroeber (1876-1960) and Robert H. Lowie, who were of German and Austrian descent, respectively, also received extensive linguistic training.



**Figure 7**

Theodor Koch-Grünberg, 1924.  
(Credit: Nachlass Theodor Koch-Grünberg, Ethnographische Sammlung der Philipps-Universität Marburg, Germany)

The primary interest of German-speaking ethnographers was in so-called *Naturvölker* (natural peoples), i.e., peoples closely depending on nature. Until 1945, German-language anthropology in Latin America was almost a synonym of studying indigenous peoples, then called “Indians,” from an Americanist point of view. This implied that above all indigenous cultural “traits,” regarded as “original” or less influenced by non-indigenous societies, should be investigated in order to reconstruct cultural histories and illuminate the development of “people without history” (Wolf, 1982). This approach survived in the Boasian tradition of North American historical anthropology during the first half of the twentieth century.

Both traditions placed a strong emphasis on empirical work in the field, combined with efforts to catalogue as many details as possible. Whereas the research undertaken by von den Steinen, Ehrenreich or Krause consisted of research expeditions comprising sojourns of a few days in most indigenous villages, Schmidt, Koch-Grünberg, Preuss, and Nimuendajú were forerunners of modern anthropological fieldwork with its stationary character (Kraus, 2004: 254-56, 288-90) – even if Malinowski (1922) was more successful in propagating the principles of anthropological fieldwork. It is important to underline that ethnologists such as Koch-Grünberg actively reflected on fieldwork conditions and their implied subjectivities, contrary to current suppositions



about that generation. However, they did this primarily in private correspondence, not in scientific publications, as Kraus (2004) has shown.

In addition to scholars, an educated middle-class (*Bildungsbürgertum*) in the German-speaking countries took an interest in exhibitions of ethnographic objects in museums and in reading ethnological literature written for a wider public. Therefore, von den Steinen, Koch-Grünberg, and Schmidt not only published articles and monographs according to the scholarly standards of their time, but also popular editions about their research, often full of lively descriptions of their expeditions and the people they had encountered.<sup>14</sup>

Regarding their theoretical assumptions, the majority of German-speaking ethnologists of the period under discussion were influenced by Adolf Bastian's ideas about a comprehensive ethnology conceived as a "Universal Archive of Humankind" (Fischer et al., 2007). Whereas Alexander von Humboldt had emphasized the unity of nature, Bastian stressed the "psychic unity of mankind." In the early twentieth century, ideas from the Vienna "culture circle" school led by Father Wilhelm Schmidt (1868-1954) were added to the "cultural history" program developed in Leipzig by Friedrich Ratzel (1844-1904) and in Berlin by Fritz Graebner (1877-1934) and Bernhard Ankermann (1859-1943). The concept of *Kulturkreise* was inspired by Ratzel, named by Leo Frobenius (1873-1938) in 1897-98, formalized for Africa and Oceania by Ankermann and Graebner in 1905 and 1911, and further developed by Wilhelm Schmidt and his associates in the 1920s and 1930s (Eidson, 2017: 55); collectively these approaches are known as diffusionism. By contrast, many ethnologists conducting research in the South American lowlands, above all Max Schmidt, criticized the "culture circle" theory in their publications. A common trait of German-language anthropology was the idea to map various cultures and even



**Figure 8**

Max Schmidt (Credit: SUSNIK, B., Prof. Dr. Max Schmidt. *Su contribución y su personalidad*. Asunción, Museo Etnográfico "Andrés Barbero" / Editora Litocolor, 1991)

**14** The principal periodicals for publishing were *Zeitschrift für Ethnologie* (Berlin, since 1869), *Petermanns Geographische Mitteilungen* (Gotha, 1855-2004), *Globus. Illustrierte Zeitschrift für Länder- und Völkerkunde* (Braunschweig, 1861-1910), *Mitteilungen der Anthropologischen Gesellschaft Wien* (Vienna, since 1870), *Internationales Archiv für Ethnographie* (Leiden, 1888-1947), *Anthropos* (first published at Mödling near Vienna, from 1906 on; now at Sankt Augustin near Bonn), and *Baessler-Archiv* (Berlin, from 1910 on).



**Figure 9**

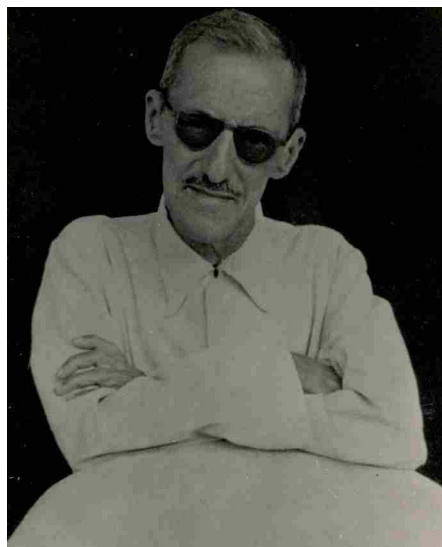
Curt Unckel Nimuendajú visiting Gothenburg in the summer of 1934. Photograph by Stig Rydén, 1934. (Credit: National Museums of World Culture—Museum of World Culture Gothenburg, Sweden. Inventory No. 006843)

the cultures of humanity in a kind of synthesis of Bastian's ethnology and Ratzel's anthropogeography (Santini, 2018).

Distinctive for the German tradition in Latin American anthropologies was the practice of systematic empirical data collection. The aim of carrying out systematic, comprehensive ethnography by focusing on material and spiritual culture, on art, languages, myths, songs, folklore, etc., was a common denominator of this research tradition. The call for empirical work, and the ethnographic practice of German-language anthropology in both the eighteenth and the nineteenth century, offered a stance against speculative, evolutionist tendencies predominant in other national contexts, especially during the nineteenth century. Empiricism as an epistemology and a scientific practice was so relevant to German ethnology that Boas succeeded in transferring it to the American academic environment.

To illustrate the importance of this kind of research: Martin Gusinde SVD (1886-1969) undertook four research trips to Tierra del Fuego between 1918 and 1924, accompanied by Wilhelm Koppers SVD (1886-1961) on the third. Gusinde worked as a teacher in Santiago de Chile from 1912 to 1922, and as a volunteer at the Museo de Etnología y Antropología de Chile (at first as an associate of Max Uhle). Concluding his research in 1922-24 (Bornemann, 1970), Gusinde's ethnography of three "tribes" of "Feurland-Indianer" is highly valued as these groups are now extinct or nearly extinct and because he, in documenting their cultural richness, refuted the idea of "uncivilized savages" common in Chile and Argentina at the time (Rabinovich, 1985: 397-398).

As von den Steinen cited Bastian in his obituary: it was vital to "salvage" items of material and spiritual culture "before it will be too late" (*Rettet! rettet! ehe es zu spät ist*. Steinen, 1905: 248). German-speaking ethnologists formed a small community in which almost everyone knew everybody else; depended on local assistants, interpreters, governors, missionaries, and state authorities; and cooperated with Latin American intellectuals. To cite only a few examples of such transnational interactions: Antonio Bakairí supported von den Steinen (Viertler, this issue); Juan Salva Marinau was a Mapuche informant who helped Lehmann-Nitsche (Ballestero, this issue); Max Schmidt's Brazilian travel partner André is often cited in his accounts of the expedition in the Upper Xingu region (Kraus, this issue);



**Figure 10**

Carlos Estevão de Oliveira.  
Date and photographer  
unknown. (Credit: Acervo Museu  
do Estado de Pernambuco/  
FUNDARPE, Brazil)

João Capistrano de Abreu, Teodoro Fernandes Sampaio, and Affonso d'Escragnoille Taunay were important academic interlocutors for Koch-Grünberg (Petschelies, this issue); and Carlos Estevão de Oliveira was not only one of Nimuendajú's best friends, but also one of his most important financial supporters and political allies (Schröder, this issue) (Figure 10).

Thus, identifying scholars exclusively on the basis of national origins would be misleading. A focus on transnational networks is more useful for studying the connections between intellectual agendas, especially regarding the role of Brazilian academic institutions and individuals in joining scholars from different academic backgrounds.

### ANTHROPOLOGICAL TRADITIONS IN BRAZIL

Herbert Baldus (1899-1970) and Emilio Willems (1905-1997) continued the tradition in São Paulo (Figures 11-12). Key players in the institutionalization of anthropology in Brazil, Willems and Baldus trained the next generation. From 1936 on, Willems held the chair of sociology at the Free School of Sociology and Politics (ELSP), founded in 1933, and at the University of São Paulo (USP), founded in 1934. Baldus accepted the chair of Brazilian ethnology at the Free School in 1939. That same year, Willems launched the *Revista de Sociologia*, the most important sociological and anthropological journal in Brazil for years. In 1941, Willems helped consolidate the teaching of anthropology by accepting a chair in anthropology at the Faculty of Philosophy, Sciences, and Literature of the USP. In 1947, *antropologia* was finally made a specialization in the diploma of the social sciences and one year later became a compulsory subject in courses on social sciences, geography, and history (Borges Pereira, 1994). Baldus coordinated the ethnological section of the *Revista de Sociologia*, through which new studies and translations of foreign authors circulated. This section included "suggestions for ethnographic investigations," which helped define trends in anthropology and inspired younger scholars to find



**Figure 11**

Herbert Baldus. Date and photographer unknown. (Credit: National Museums of World Culture – Museum of World Culture Gothenburg, Sweden. Inventory No. 014428)



**Figure 12**

Emilio Willems in the late 1930s, when he served at the Free School of Sociology and Politics and the Faculty of Philosophy, Sciences, and Literature of the University of São Paulo (Credit: CEDOC/FESPSP) – before founding the *Revista de Sociologia* in 1939.

topics for their graduate studies (Borges Pereira, 1994).

Willems and Baldus influenced the next generation of Brazilian-born or immigrant social scientists, who would follow their professional agenda. Most of them were graduate candidates, research assistants, or students who had attended their courses. This generation included Oracy Nogueira (1917-1996), Gioconda Mussolini (1913-1969) and Virgínia Leone Bicudo (1915-2003), who earned their master's degrees in 1945; Lucila Hermann in 1946, Florestan Fernandes (1920-1995) in 1947, Fernando Altenfelder Silva (1916-1993) in 1949; Levy Cruz in 1951; Sérgio Buarque de Holanda (1902-1982) in 1958. Other students of future relevance to Brazilian anthropology were Darcy Ribeiro (1922-1997), Juarez Brandão Lopes (1925-2011), Cândido Procópio Ferreira de Camargo (1922-1987), Alfonso Trujillo Ferrari, Egon Schaden (1913-1991), João Baptista Borges Pereira (b. 1930), David Maybury-Lewis (1929-2007), Roberto Cardoso de Oliveira (1928-2006), Harald Schultz (1909-1966), and Thekla Hartmann (b. 1933) among others.<sup>15</sup> Thus, Schaden, Hartmann, and Fernandes – some of the biggest names of the first generation of social scientists in Brazil – were former students of Willems and Baldus. When Willems left for Vanderbilt University in Nashville (USA) in 1949, his assistant Egon Schaden, born in Brazil, succeeded him as *professor catedrático de Antropologia* (full professor of anthropology) in 1952. Schaden founded the *Revista de Antropologia* at São Paulo in June 1953.<sup>16</sup>

Up to the 1930s, the German-language tradition of ethnography and ethnology was central to structuring the development of Latin American anthropology. From the 1930s until the 1950s, however, when most Brazilian and Latin American research and teaching institutions were established, German-speaking anthropologists in Brazil had to compete with French and North American colleagues. Fernanda Peixoto (2001: 478) drew a sharp distinction between these disciplinary traditions: “Brazil chooses French scholars as masters. The North Americans select Brazil as their ‘object.’” The growing influence of French and North American scholars and academic institutions on the formation of the social sciences in Latin America coincided with the rise of National Socialism in Germany (1933-45).<sup>17</sup>

French cultural missions staffed institutions with scholars, especially teaching the humanities.<sup>18</sup> Claude Lévi-Strauss (1908-2009) held a visiting professorship in sociology at the University of São Paulo between 1935 and 1939. His first wife, Dina Dreyfus (1911-1999), worked as a visiting professor of ethnology at the same university. Together, they undertook an expedition to the Mato Grosso, studying the Kadiwéu and Bororó. The acquired artifacts were exhibited at the Musée de l'Homme in Paris: “Indiens du Mato-Grosso (Mission Claude et Dina Lévi-Strauss)” in 1937. The following year, they carried out a second expedition to the Nambikwara and Tupi-Kawahib, which lasted more than 6 months. During

**15** On the institutionalization of ethnology, sociology, and anthropology in Brazil, see e.g., Melatti (1983), Cardoso de Oliveira (1988), Schwartzman (1991), Borges Pereira (1994), Sampaio-Silva (2000), Peirano (2018).

**16** Baldus (1945, 1946, 1951, 1966), Schaden (1946, 1953, 1955, 1971, 1976, 1980, 1981, 1990), Hartmann (1977), and Horch (1969) published about their predecessors and colleagues. Their work consisted mostly of articles but also included reference works such as the *Dicionário de etnologia e sociologia* (Willems and Baldus, 1939) and the *Bibliografia crítica da etnologia brasileira* (Baldus, 1954-68; vol. 3 edited by Hartmann, 1984).

**17** For the impact of National Socialism on ancient Mexican studies in Austria, see Rohrbacher, this issue.

**18** On the French academic and cultural missions, see Peixoto (1991, 2001), Ferreira (1999), Miceli (2001).



**Figure 13**

Emilio Willems in the late 1930s, when he served at the Free School of Sociology and Politics and the Faculty of Philosophy, Sciences, and Literature of the University of São Paulo (Credit: CEDOC/FESPSP) – before founding the *Revista de Sociologia* in 1939.

these expeditions Lévi-Strauss turned into an ethnologist and collected material for his first dissertation (1948b); his photographs were later published in *Sau- dades do Brasil* (1994). (Figure 13)

North American funding organizations, such as the Rockefeller, Ford, and Carnegie Foundations, exerted a growing influence on the social sciences that were established in Latin America from the 1930s on (Miceli, 1993). Brazil and other parts of Latin America became a site for developing research, particularly concerning topics associated with racial relations and community studies. Of key significance was Donald Pierson (1900-1995), Robert E. Park's Ph.D. student at the University of Chicago. Pierson conducted fieldwork in Bahia (1935-37) as part of the "Race and Cultural Contact" program supervised by Robert Redfield, Louis Wirth, Herbert Blumer, and Park. Park's project of a comparative sociology pertaining to the integration of blacks and migrants in communities in the United States as well as globally, led members of the Chicago school to select Bahia as a research site, hereby resonating Rudiger Bilden's 1929 characterization of Brazil as a "laboratory of civilization" (cited in Chor Maio, 1999: 142). After defending his thesis titled *Cruz das Almas: A Brazilian Village* at Chicago in 1939, Pierson returned to São Paulo to later officiate as professor at the Free School of Sociology and Politics as well as at the Faculty of Philosophy, Sciences, and Literature of the USP, where he served until 1959. Working alongside Willems and Baldus, Pierson played an important role in training Brazilian social scientists through courses, fieldwork, and by co-supervising graduate researchers (Silva, 2012; Chor Maio and Costa Lopes, 2017).

After 1945, Bahia continued to be a major research site. Due to the atroci-

ties committed during World War II, the problem of “race” and race relations became a serious subject within the social sciences. From 1950 on, UNESCO commissioned a series of researches in Bahia concerning race relations and the integration of minorities. The Brazilian social scientist Arthur Ramos (1903-1949) delineated the guidelines of this UNESCO Project, which included research in several cities and regions in Brazil. The initial purpose, as summarized by Charles Wagley (1952: 1), was “to determine the economic, political, and psychological factors which influence such relations.” After the demise of Ramos, Alfred Métraux (1902-1963), having studied in Paris and Gothenburg and carrying out fieldwork among the Argentine Chaco, continued the coordination of the division “Racial Relations” in UNESCO’s Department of Social Sciences. In the following years, Brazilian and North American researchers published monographs in Portuguese, French or English dealing with case studies that continued this project, committed to display a country that remained “a lesson in racial democracy for the rest of the world.”<sup>19</sup>

In 1967, Egon Schaden “prematurely and hastily” resigned from his post at USP in order to occupy a chair in ethnology at the University of Bonn, where he had been a visiting professor several times (Figure 14). “Personal and family matters prevented him from accepting this position” (Borges Pereira, 1994: 253). At the same time, Lévi-Straussian structuralism was attracting most scholarly attention. “By the late 1960s, social anthropology had become the dominant, hegemonic orientation within anthropology” in Brazil (Peirano, 2018).

**19** This project was funded by *Fundação para o Desenvolvimento da Ciência da Bahia*, the Viking Fund (New York), the Council for Research in the Social Sciences at Columbia University (New York), and the Department of Social Sciences of UNESCO, in collaboration with the Department of Anthropology of Columbia University, see Baldus (1963), Chor Maio (1999).



**Figure 14**  
Egon Schaden at his desk, c. 1990, by an unknown photographer (Courtesy of Schaden Institute). Schaden founded the *Revista de Antropologia* at São Paulo in 1953.

## DECLINE OF THE GERMAN TRADITION

From the 1960s on, German-language anthropology suffered from a lack of interest to such an extent that references to German or German-speaking anthropologists have almost vanished from the canon of social sciences in Latin America. The Brazilian anthropologist of German descent, Renate Brigitte Viertler, who spent her career studying the Bororo and relating her fieldwork data to German, French, Italian, and English references, recently published a book on the foundations of German anthropology, which she opens with the statement that ethnological treatises written in German are no longer acknowledged in the majority of works on the history of anthropology written in English or French (Viertler, 2018: 15). Egon Schaden still discussed German-speaking ethnographers during Viertler's first year of studies at the University of São Paulo in 1960-61, but these references gradually came to a halt. Today, in Brazil, German-language contributions to anthropology and ethnology are infrequently discussed in scholarly works or in lectures on anthropology.

Evidence of this amnesia can be found in the *syllabi* (programs) for anthropology courses in Brazil, Argentina, Mexico, etc. A study of *syllabi* for the Graduate Program for Social Anthropology at the Museu Nacional in Rio de Janeiro, one of the first and most prestigious graduate schools in anthropology in Latin America, inaugurated in 1968, indicates that the number of references to works by German-speaking ethnologists is extremely low. The only exceptions are a few works by Willems and some of his students, such as Schaden or Gioconda Mussolini, offered in courses on "migration and community studies" and "ethnology of inter-ethnic contact." Without being exhaustive, two observations can be made. First, works by von den Steinen, Nimuendajú, Max Schmidt, and Koch-Grünberg, or by Baldus and other ethnologists are notably absent, although they carried out important field research and contributed to the institutionalization of anthropology in Brazil. Second, German-speaking ethnologists such as Bastian and those who pursued fieldwork in Latin America are never referred to in courses on "Anthropological Theory" or "Ethnology."

These findings are remarkable, considering that ethnography, one of anthropology's most enduring roots, has been a strong research tradition, as the case of German-speaking ethnographers and ethnologists in Latin America presented in this issue demonstrates.<sup>20</sup>

Yet another Brazilian anthropologist, Mariza Peirano, commented on this amnesia in 2004 and raised critical questions about the legacy of the German tradition in Brazil. In a paper presented at the "Anthropology of Anthropology Seminar" in São Paulo, she looked back on her study days in Cambridge, MA, and contrasted the loss of visibility of German ethnographers in Brazil with the high regard for their compatriots in North America:

**20** These findings are even more striking when compared to the positive reception of German-language sociology in Brazil (Villas Bôas, 1997). In general, graduate students in the social sciences and philosophy in Latin America are well aware of the names of sociologists and philosophers such as M. Weber, G. Simmel, F. Tönnies, J. Habermas, N. Luhmann, N. Elias, K. Marx, R. Luxemburg, P. Lazarsfeld, etc., and less able to recognize the names of German-speaking anthropologists.

*It was then [in 1977] that George Stocking Jr. spent a semester at Harvard. [...] It was during his classes that I raised the fateful question that would lead me to the thesis I finally wrote [Peirano, 1981]: If the German ethnographers who went to North America left behind a Franz Boas, why had we not gained an equivalent legacy from the ethnologists who came to Brazil as part of the same project? Why do our intellectual lineages so rarely go back to the German ethnographers from the late 19th century (except for Baldus, and Schaden, for example)? Why, after all, did the long-term ethnographic style never “catch on” in Brazil as it did in the United States? (A contemporary version of the same question might lead us to query why some currents never make it here [in Brazil], while others catch and stick, as obligatory “musts”.) (Peirano, 2004: 4 – our emphasis)*

Peirano's questions disclose a striking paradox: If we accept the fact that Brazil has been a major fieldwork destination for German-speaking scholars and a space in which writings that impacted mainstream anthropological theory have been developed, why does the German anthropological tradition experience such restricted visibility in Brazilian academia? The same can be said when referring to other Latin American academic contexts in which mainstream narratives tend to reinforce the relevance of British, North American or French anthropologies to the detriment of German-language anthropological contributions. Following Peirano's suggestion, further interrogations could be added: *why* did the German-language anthropological tradition fall into oblivion in Latin America and *how* did that happen?

Of course, the amnesia and exclusion are not only due to the language barrier and the limited availability of the original publications in libraries around the world. Cultural and political factors play a role here, too. After both world wars (1914-18, 1939-45), German-speaking scholars suffered from a loss of academic prestige, the German language lost its position as an international language of science, and the number of scholars familiar with publications in German has dwindled in the century since 1918 (Eidson, 2017: 49).

The decline of this research tradition began with the defeat of the German and the Austro-Hungarian Empires in the course of World War I. After the Great War both empires suffered from economic crises and their academic institutions were downsized. The ensuing hyperinflation of the Weimar Republic (1918-33) affected the funding of anthropological research in Latin America, rendering it almost impossible for many years and even influenced anthropological periodicals. After World War I, only *Anthropos* continued to be regularly published. Curt Nimuendajú was forced to find financial support outside his usual networks and received assistance from Lowie in Berkeley, who sponsored his research from 1935 on (Schröder, this issue; Welper, 2018).



In addition to financial and institutional challenges, German and Austrian anthropology witnessed unfortunate theoretical orientations. The majority option, which favored diffusionist approaches, resulted in a theoretical dead end. Although some German-speaking anthropologists maintained their international contacts and applied various approaches to their research, the majority of German and Austrian anthropologists became isolated, lost contact with their colleagues abroad, and failed to acknowledge the importance of British and French innovations. British functionalism, for example, which had developed from the 1920s on, was introduced into the German ethnological curriculum only after 1945.

Internal factors, intrinsic to ethnology in the German language area, contributed to what Andre Gingrich (1997: 84) called the “decline of the German language ... seen in the comparatively weak resonance” in the international literature. Gingrich cited a 1995 conference report by the Austrian ethnologist Walter Dostal (1928-2011), who had argued that this decline was not only “caused by the events of the Second World War” but also “a result of developments in the post-war era in which German-language anthropology lost its importance due to exhausting directional quarrels, an increasing deficit of theories, and the ‘crisis of meaning’ which resulted from it” (cited in Gingrich, 1997: 84). Accordingly, Michael Kraus (2004: 488-89) concluded that German-language Amazonia studies remained strong until 1929; this tradition could only be continued in the late 1960s.

In an irony of history, non-mainstream German ethnologists such as Fritz W. Kramer (b. 1941) and Hans-Peter Duerr (b. 1943) anticipated the reflexive turn of Anglo-American anthropology during the 1970s and 1980s. Needless to say, their publications were not received in international academia as they were written and published in German.

## CONCLUSION

From the 1960s on, German and German-speaking anthropologists again carried out research in almost all Latin American countries, although regional focuses on Mexico and the Andean countries became apparent. Their publications escaped international attention if written in German. Thus, the works of Mark Münzel (b. 1943), Udo Oberem (1923-1986), Hanns J. Prem (1941-2014) or Gerhard Baer (1934-2017) were perceived in Brazil, Ecuador, Peru, and Mexico only when published in English, Spanish or Portuguese.<sup>21</sup>

Brazilian scholars such as Edgar Roquette-Pinto (1884-1954) and Sérgio Buarque de Holanda (1902-1982) continued to refer to German ethnographers and ethnologists. Their work was important for Buarque de Holanda in his renowned *Caminhos e Fronteiras* (1957).<sup>22</sup> Lévi-Strauss, for one, was well aware of the importance of German ethnographers and cited their work in his early

**21** On the history of German anthropology, see Gingrich (2005, 2017), Haller (2012), Eidson (2017), Kohl (2018).

**22** On Buarque de Holanda, see Françaço (2004; 2007).

publications (e.g. 1948a). Currently, however, for most Brazilian anthropologists this research tradition remains a *terra incognita*, even though many German-language books have been translated into Portuguese.<sup>23</sup>

Yet these later developments spring the timeframe of the present issue. Our overview of a rich but fragmented research tradition closes in 1945 with the demise of Nimuendajú, the most dedicated fieldworker of them all. In an obituary published in *Boletim Bibliográfico* and *American Anthropologist*, Herbert Baldus stated:

*On December 10, 1945 occurred the death of the great ethnologist Curt Nimuendajú. Like Theodor Koch-Grünberg, he died among the Indians to whom he was so greatly attached. No one perhaps has contributed more to the study of Brazilian aborigines than these two men of German origin. (Baldus, 1946: 238, 1945: 91)*

Baldus listed 35 “explorations” Nimuendajú had undertaken between 1905 and 1945, many of which at his own expense, as well as 33 publications resulting from these journeys, five of which written either in cooperation with or translated by Robert Lowie in 1937-45. Baldus hailed Nimuendajú’s ethnographic and archaeological collections that “enriched museums in Germany, Sweden, and Brazil,” praised his efforts to protect the Indian tribes against violence, and called him “perhaps the greatest Indianista of all time” (Baldus, 1946: 241).

It is hardly an exaggeration to conclude that the German-language empirical tradition, focused on ethnography, has been constitutive and exerted an important influence on Latin American academia, at least until the 1930s. From the 1960s on, the influence of Austrian, German or Swiss scholars on scholarly research in Latin America faded away and today is hardly acknowledged in the master narrative of disciplines such as anthropology.

To help remedy this situation, the present special issue offers fresh contributions based on extensive archival research, accumulated discussions, and primary publications. It aims to introduce hypotheses, on-going research questions, and recent research findings that address the relevance of ethnographic research to both world anthropology and indigenous peoples. In a recent study of *German Ethnography in Australia*, the editors, Nicolas Peterson and Anna Kenny (2017), emphasize the relevance of “early ethnographic accounts of Aboriginal life” to native title and statutory land claims in Australia – a vital issue also acute in Brazil today.

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**Cláudio Costa Pinheiro** is Professor of Asian and African Studies at the Institute of History, Rio de Janeiro Federal University, and International Scholar at the University of Cologne (2017-2019). He chairs the Sephis Programme and has

**23** Following translations during the 1930s, 1940s, and 1950s, recent translations into Portuguese include:

KOCH-GRÜNBERG, Theodor. *Zwei Jahre unter den Indianern. Reisen in Nordwestbrasilien (1910-1911)*. Translation: *Dois anos entre os indígenas: viagens ao noroeste do Brasil (1903-1905)*. Manaus, EDUA e FSDB, 2005.

KOCH-GRÜNBERG, Theodor. *Vom Roraima zum Orinoco (vol. I, 1916)*. Translation: *Do Roraima ao Orinoco. Observações de uma viagem pelo norte do Brasil e pela Venezuela durante os anos de 1911 a 1913*. São Paulo, Editora da UNESP, 2006.

KOCH-GRÜNBERG, Theodor. *Südamerikanische Felszeichnungen (1907)*. Translation: *Petróglifos sul-americanos*. Manaus, Universidade Federal do Amazonas, 2009.

KOCH-GRÜNBERG, Theodor. *Anfänge der Kunst im Urwald (1905)*. Translation: *Começos da arte na selva: desenhos manuais de indígenas colecionados por Dr. Theodor Koch-Grünberg em suas viagens pelo Brasil*. Belém, Museu Goeldi; ISA, 2010.

NIMUENDAJÚ, Curt. *Five German texts published in Anthropos 14-24 (1919-1929)*. Translation: SCHRÖDER, Peter (ed.) *NIMUENDAJÚ, Curt, Os índios Xipaya: cultura e língua: textos de Curt Nimuendajú*. Recife, Editora Universitária da UFPE, 2015 (e-book) and Campinas, Editora Curt Nimuendajú, 2019 (printed version).

SCHMIDT, Max. *Die Aruaken (1917)*. Translation: PETSCHLIES, Erik; SCHRÖDER, Peter (eds.)

SCHMIDT, Max, *Os Aruaques: uma contribuição para o problema da difusão cultural*. Recife, Editora Universitária da UFPE, forthcoming.

been a visiting professor and visiting scholar at universities in Austria, Germany, Brazil, India, and Japan. He works on the interfaces between history, anthropology, and sociology and conducted anthropological fieldwork and historical archival research along two main axes: (1) knowledge production and circulation, and (2) structures of power, slavery and forms of dependency, both of which consider the impact and durable effects of colonialism in Western and non-Western societies. He is the author of “Unhomely Afterlives: The Works and Lives of Rabindranath Tagore” in WILLIAMS, James and HENTSCHKE, Felicitas (eds) *To be at Home: House, Work, and Self in the Modern World* (De Gruyter, 2018); “Modernity and the Artifices of Place-making. The Relevance of the BRICS to contemporary Social Sciences” in *AEGS—Annual of European and Global Studies*, no. 4, June/July 2017. E-mail: c.pinheiro@sephis.org

**Peter Schröder** is an associated professor in the graduate program for anthropology (PPGA/Pós-Graduação em Antropologia) of the Anthropology and Museology Department (DAM/Departamento de Antropologia e Museologia), Universidade Federal de Pernambuco (UFPE), Recife, Brazil. Having obtained his Dr. phil. in Ethnology from the Rheinische Friedrich-Wilhelms-Universität Bonn, Germany, in 1993, he conducted post-doctoral research in southern Ceará State, Brazil, in 1995–97 and at Leipzig University, Germany, in 2010–11. His research interests are indigenous societies in South America, development anthropology, economic anthropology, history of anthropology. Carrying out research on Curt Nimuendajú’s life and work from 2009 on, he translated five of Nimuendajú’s texts on Xipaya culture into Portuguese: *Os índios Xipaya cultura e língua* (Recife, 2015; Campinas, 2019). E-mail: pschroder@uol.com.br

**Han F. Vermeulen** is a researcher at the Max Planck Institute for Social Anthropology in Halle (Saale), Germany, specializing in the history and theory of anthropology. He is editor or co-editor of seven books, including *Fieldwork and Footnotes: Studies in the History of European Anthropology* (1995), *Treasure Hunting? Collectors and Collections of Indonesian Artefacts* (2002), and *Tales from Academia: History of Anthropology in the Netherlands* (2002). His research interests are the history and theory of anthropology, the study of race and ethnicity, and the anthropology of Africa, Asia, and the Americas. His latest book, *Before Boas: The Genesis of Ethnography and Ethnology in the German Enlightenment* (2015), was listed by the *Süddeutsche Zeitung* as one of the most important books of 2016, awarded the ICAS Book Prize Social Sciences during the International Convention of Asia Scholars in 2017, and released as a paperback in 2018. E-mail: vermeulen@eth.mpg.de

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# Karl von den Steinen's Ethnographic Research among Indigenous Peoples in Brazil, 1884-1888

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## Renate Brigitte Viertler

🏠 *Universidade de São Paulo | São Paulo, SP, Brazil*  
✉ *renatebv@hotmail.com*

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### ABSTRACT

This article analyses and contextualizes the life and work of the German ethnologist Karl von den Steinen (1855-1929), who conducted ethnographic research in Brazil in 1884 and 1887-88. With a solid education in sciences and the humanities, von den Steinen, a physician and psychiatrist, set out to study social institutions, material and spiritual culture among “stone age” tribal societies along the Amazon. Arriving in Brazil in 1884 he studied the Upper Xingu and the Bororo Indians in the Mato Grosso province. In the Upper Xingu area he obtained a rich ethnographic collection and gathered detailed ethnographic data that continue to be useful for modern-day anthropologists. From a theoretical point of view, von den Steinen endorsed the theory of multilineal development proposed by his mentor Adolf Bastian (1826-1905). Despite many difficulties during his stay among the Bororo Indians, he was able to acquire valuable data to be used by those interested in Bororo social institutions and ceremonial life.

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### KEYWORDS

Bororo Indians,  
Upper Xingu Indians,  
Brazilian Ethnology,  
History of Anthropology,  
Indigenous Peoples

## INTRODUCTION

The European expansion to the East and West Indies during the sixteenth century furthered the discovery of Brazil by Portuguese explorers. The first reports were written by Portuguese, Spanish, Swiss, and German authors; describing the new land and its inhabitants. Europeans arrived on the Brazilian coast in search of new resources, such as *pau-brasil* (redwood for dyeing fabrics), tropical plants, spices, precious metals, and stones. The Brazilian indigenous population



**Figure 1**

Karl von den Steinen and his Travel Companions. Left: Dr. Otto Clauss, Center: Dr. Karl von den Steinen, Right: Wilhelm von den Steinen. Wood cut by Richard Bren D'Amour. (Reproduced from: STEINEN, Karl von den, *Durch Central Brasilien. Expedition zur Erforschung des Schingú im Jahre 1884*. Leipzig, F.A. Brockhaus, 1886, Frontispiece)

was enslaved, as more labour was required,. Direct contact between indigenous peoples and Europeans led to epidemics, the extinction of many indigenous societies in the coastal areas and in the Amazon basin, and a dramatic decrease of the Brazilian indigenous population. The increase of gold prospecting and other resources in the Brazilian hinterland, furthered by the Portuguese monarchy, required the establishment of settlements in indigenous territories, leading to interethnic conflicts and changes in the territorial distribution of the indigenous population.

Ethnography and ethnology were developed in the Russian Empire, at the Imperial Academy of Sciences in St. Petersburg; the German universities of Halle, Göttingen, Leipzig and Jena; and at the court library of Vienna (Austria); during the eighteenth century (Vermeulen, 2015, 2016). In Brazil, ethnology had to wait until the end of the nineteenth century. It was preceded by contributions made during expeditions led by German naturalists such as Alexander von Humboldt (1769-1859), Maximilian Prince zu Wied-Neuwied (1782-1867), Georg Heinrich Langsdorff (1774-1852), Carl Friedrich Philipp von Martius (1794-1868), and Johann Baptist von Spix (1781-1826).

When Karl von den Steinen (1855-1929) undertook his Brazilian researches in 1884 and 1887-88, he developed new standards for ethnographic research. Inspired by the ethnologist Adolf Bastian (1826-1905), and following rigorous methodological steps that had already been established in German ethnography, he started what has recently been named as the “beginnings of systematic ethnography in Brazil” (Petschelies, 2018).

At the time of von den Steinen's sojourn in Brazil, a rapid expansion of the economic frontier to the hinterland occurring. The neo-Brazilian population considered the indigenous population occupying too much land; that they were “lazy” and “good to be killed”.

In 1884, Dom Pedro II, Emperor of Brazil, helped von den Steinen with the project of exploring the possibilities of navigating the Xingu River, starting from Mato Grosso to Pará and to search for a fluvial connection that might benefit regional economy. The latter plan failed, but the Upper Xingu river basin was opened up to ethnographic research. In the same year, von den Steinen began his first Brazilian expedition to visit this completely isolated and unknown area with the intention of meeting Bakairi, Custenaó, Trumai, Manitsauá, Juruna, Nahuquá, Mehinaku Iaulapiti, Auetö, and Camaiurá Indians. He revisited them during a second expedition in 1887-88 when he also met Pareci Indians in Cuiabá and stayed among the Bororo of the Teresa Cristina colony at the São Lourenço River. It should be noted that these two tribes were already engaged in direct contact with the neo-Brazilian population of Mato Grosso province.

1 The extermination of the Indian population intensified in such a way that an Indian Protection Service was founded in 1910 with the aim of civilizing Brazilian Indians by transforming them into good working citizens.

## LIFE AND WORK

Born in Mühlheim as the son of a physician, Karl von den Steinen attended school in Düsseldorf and concluded his school days at the early age of 16. Despite his humanistic background, he studied medicine at the universities of Zurich, Bonn, and Strasbourg, at the Physiological Institute where he obtained his PhD in 1875. After specializing in psychiatry at the University of Vienna in 1878-79, he worked as a medical assistant at the famous Charité hospital in Berlin (Thieme, 1993: 41).

At that time, Berlin was an important center of innovations in science and the arts. After the fall of Napoleon Bonaparte and the Restoration policies, the city had been chosen as the capital of the Prussian monarchy; after the German unification of 1870-71, it served as the capital of the Second Empire. Its university, the *Friedrich Wilhelm Universität*, later called the *Humboldt Universität*, counted among its staff the pathologist and physical anthropologist Rudolf Virchow (1821-1902) and the physician and ethnologist Adolf Bastian (1826-1905), director of the *Königliches Museum für Völkerkunde* in Berlin who made nine research and acquisition trips around the world.

In 1879, Karl von den Steinen began travelling around the world. From Bremen he sailed to New York, Cuba, Mexico, the United States, Polynesia, Java, China, Japan, India, and Egypt, to study various treatments of mental diseases. In Polynesia, on Honolulu, he met Bastian, the well-known traveller, who suggested for him to engage in ethnological researches and collect ethnographic material.

It was a time of great innovations and experiments in psychology. Apart from studies of the “unconscious,” psychophysics, and the *Völkerpsychologie* (psychology of peoples) of Moritz Lazarus (1824-1903) and later Wilhelm Wundt (1892-1920), there was the work of the psycholinguist Johann Friedrich Herbart (1776-1841). Herbart considered that psychological analysis is not limited to the individual but also encompasses society, a view adopted by the ethnologist Bastian.

Trained as a physician and skilled in psychiatry and neurology, Adolf Bastian developed modern and surprising reflections about phenomena such as possession, exorcism, and the psychological contagion associated to religious life and mythology of *Naturvölker* (“peoples living in nature”), relating them to manifestations observed in psychiatric clinics. Bastian endorsed the principle of the psychic unity of humankind by proposing the concept of “elementary ideas”, universals that can be found among all human societies.

Returning from Southern Georgia in 1883, von den Steinen passed Montevideo (Thieme, 1993: 42) and began planning an ethnological expedition. Interested in “stone age people” living in Central South America, he first planned to visit the *Chiriguano* of the western Chaco. Ultimately, he explored the Upper Xingu, an uncharted river of interest to Brazilian provincial authorities since, if navigable, it would allow access to the international market in Amazonia (Thieme, 1993: 49).



Encouraged and materially assisted by the Brazilian emperor Dom Pedro II, Karl von den Steinen, Wilhelm von den Steinen a designer, a designer and draughtsman, and Otto Claus, a geographer and astronomer, started to explore the province of Mato Grosso for the first time in 1884. They departed from Cuiabá and reached the Upper Xingu area. During a second expedition to the same area, carried out in 1887-88, Karl von den Steinen and his cousin Wilhelm were accompanied by Paul Ehrenreich, an ethnologist, and Peter Vogel, a geographer (Schaden, 1993: 111).



**Figure 2**

Karl von den Steinen posing in Montevideo, 1883. Photograph by Chute & Brooks. (Courtesy: Reimar Schefold, Amsterdam, The Netherlands, Family Archive)

After his two expeditions in Brazil, Karl von den Steinen went to the Marquesas Islands and to Polynesia in order to study the material culture, plastic arts, and tattoos of their inhabitants and to obtain new ethnographic collections (Thieme 1993: 4) and returned to Germany in 1898

In 1889, Karl von den Steinen married Rosa Eleonore Herzfeld and they went to live in Berlin-Steglitz, having eight children. It was also in 1889 that he presented his habilitation thesis to the University of Berlin. Entitled *Erfahrungen zur Entwicklungsgeschichte des Völkergedankens* (Experiences regarding the Developmental History of the Thinking of Peoples), this study connected his observations to Bastian's cultural theories. Subsequently, he started lecturing at the University of Berlin. After a year he obtained a chair in ethnology at the University of Marburg where he intended to elaborate his ethnographic data. However, he remained in Marburg for only two years and returned to Berlin in 1893 to resume his activities at the university and at the Museum of Ethnology (Thieme, 1993: 83).

There are three books relating to von den Steinen's two expeditions in Brazil, which are *Durch Central Brasilien* (Through Central Brazil), 1886; *Unter Naturvölkern Zentral Brasiliens* (Among Peoples of Nature in Central Brazil), 1894; and *Die Bakairi-Sprache: Mit Beiträgen zu einer Lautlehre der karaischen Grundsprache* (The Bakairi Language: With Contributions to a Phonetics of the Carib Basic Language), 1892.

At the funeral service of Adolf Bastian in 1905, Karl von den Steinen read his mentor's obituary in which he praised him and summarized his theoretical ideas. After retiring from official duties in 1906, von den Steinen went on editing the *Zeitschrift für Ethnologie* (Ethnological Journal) and the review *Das Ausland* (The

Exterior) until 1911. He continued to develop activities at the *Deutsche Gesellschaft für Anthropologie, Ethnologie und Urgeschichte* (German Society of Anthropology, Ethnology and Prehistory) founded by Virchow and Bastian in 1869, until 1928.

Von den Steinen had become friends with the Swedish prehistorian and ethnologist Erland Nordenskiöld (1877-1932) from the Etnografiska Museum of Göteborg; they corresponded about subjects such as spinning techniques of the Bororo and Bakairi Indians, Polynesian art, palisades or wooden drums of the Kamayurá and the Bakairi Indians (Thieme, 1993: 93).

Apart from ethnographic subjects, von den Steinen and Nordenskiöld exchanged personal information about his deceased son, poverty, or disgust against the conditions of the Versailles peace treaty imposed on Germany at the end of World War I.<sup>2</sup> With rising inflation after the first World War the family was obliged to move to a more modest home in Berlin-Wilmersdorf. Von den Steinen could no longer afford a secretary and gave up his library in exchange for food. In 1928 von den Steinen invited Nordenskiöld to join him at his new residence in Kronberg (Taunus) but he died a short time after, on 4 November 1929 (Thieme, 1993: 101).

**2** Besides all his problems Karl von den Steinen also suffered from cataract in both eyes.

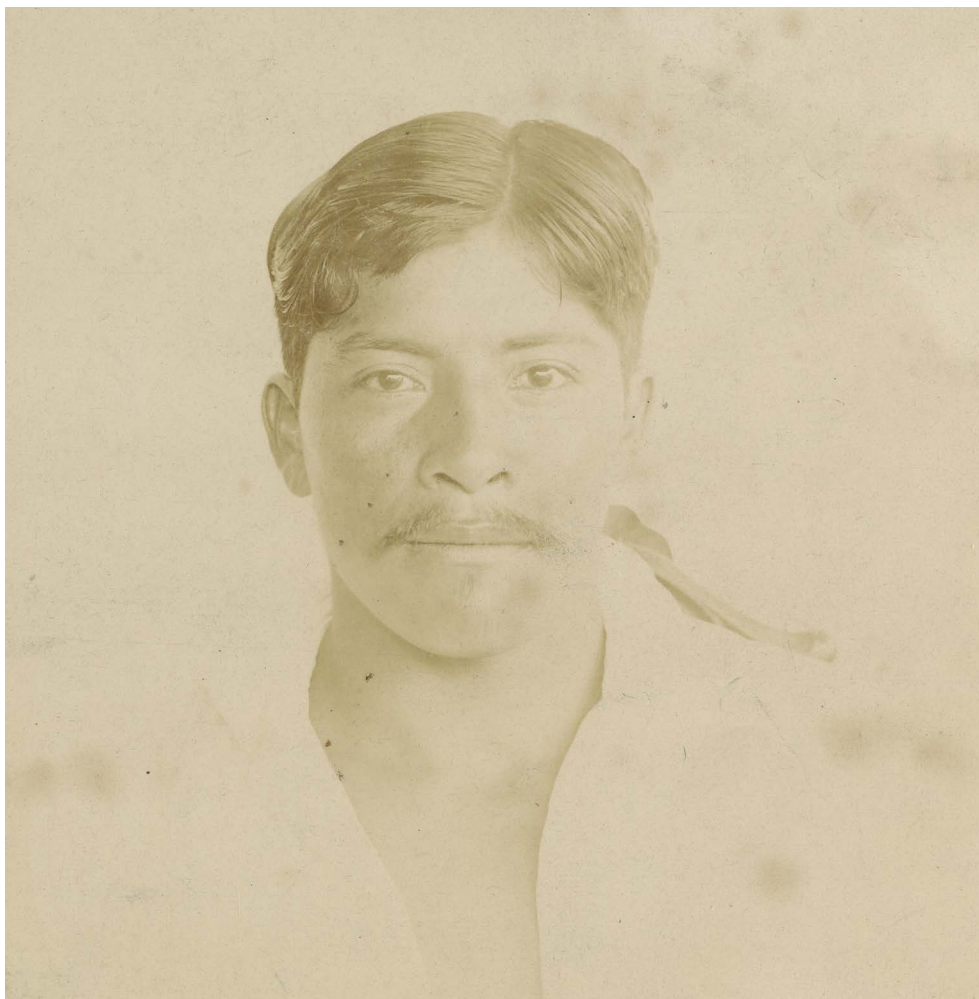
## THEORETICAL PRINCIPLES

In Germany, ethnologists believed that the best way of obtaining solid knowledge about phenomena was to start by describing them in the most detailed way possible. This principle had been introduced by historians and philosophers in the eighteenth century and became a tradition in Germany that was followed by nineteenth-century ethnologists such as Theodor Waitz (1821-1864), Gustav Klemm (1802-1867), Friedrich Ratzel (1844-1904), and Adolf Bastian. This was criticized as “excessive” by Paul Mercier (*História da Antropologia*. Rio de Janeiro, Livraria Eldorado Tijuca, 1974) and other English- and French-speaking ethnologists. “German empiricism” was a response to the general lack of trustworthy ethnological data confirmed by Anthropological Associations at that time. Differing from British and French ethnology, German ethnology or *Völkerkunde* stressed the importance of linguistic data and material culture. Each artifact found its place in an ethnographic collection that, by means of esthetical patterns, codified cultural values and ideas pertaining to human social life.

While English- and French-speaking ethnologists proposed a universal large-scale process of socio-cultural evolution, von den Steinen was influenced by the theoretical approach of Bastian, who viewed regional socio-cultural developments as activated by the interplay of the human psyche: *Elementargedanken* (elementary ideas), based on the psychological unity of human beings despite their physical differences; *geographische Provinzen* (geographical provinces) exerting pressures on the regional habitat; and *historical relations* between individuals or groups pertain-

ing to different geographical provinces. The conjunction of these three dimensions may or may not have stimulated the growth of human cultures he defined as *Völkergedanken* (thoughts of peoples), a concept that corresponds to the concept of culture, introduced by Gustav Klemm in the middle of the nineteenth century and endorsed by Edward Burnett Tylor (1832-1917) from 1871 onwards.

From a methodological point of view, aside from bibliographical research and direct observations in the field, von den Steinen also used iconographic information from the designs of Amadeo Adriano Taunay and Hercules Florence, former members of the Langsdorff expedition. Since human "language" represents an indispensable tool for understanding human "thinking," von den Steinen conducted extensive linguistic research with the help of his main informer, Antonio, chief of the Bakairi Indians. In terms of material culture, he obtained an ethnographic collection of artifacts that, according to his mentor Bastian, may be conceived as a creative fusion between "ideas" and raw materials from a specific habitat. Without the help of photographic resources human types, houses, ornaments, body paintings, and scenes of social activities had to be registered by means of drawings made by



**Figure 3**  
Antonio, the Bakairí Indian from Paranatinga, who was Karl von den Steinen's main informant during his stay in the upper Xingu area. Photograph by Paul Ehrenreich, October 1887. (Courtesy: Ethnologisches Museum, Staatliche Museen zu Berlin, Germany, No. VIII E N1s 108). Reproduced by Karl von den Steinen in his book *Die Bakairí-Sprache* (1892).

Karl's cousin Wilhelm von den Steinen. Karl von den Steinen, who also made drawings, offered iron tools in exchange for verbal informations and artifacts.

### IN THE UPPER XINGU AREA

Following the German tradition, von den Steinen tried to collect the largest possible number of ethnographic data relating to the material and spiritual culture of the Upper Xingu Indians. Basing himself on the comparative linguistics that had been developed by Wilhelm von Humboldt (1767-1835), von den Steinen started to employ language as an important criterion for ethnographic classification. He regarded Upper Xingu society as an isolated group of representatives from four linguistic families of Brazilian Indians – Carib, Aruak, Tupi, and Gê – each represented by one or more villages.

Although Martius saw indigenous people as inferior or degenerated, a view that was unacceptable to von den Steinen (Schaden, 1977: 4 apud Thieme, 1993: 46), he had proposed a linguistic classification that was useful for both von den Steinen's and Paul Ehrenreich's linguistic classification, proposed almost one century later (Balduz, 1954: 12-13). Von den Steinen redefined Martius's linguistic classification by means of the comparison of linguistic elements that he correlated with cultural and ethno-historical data relating to migrations through the South-American continent. He classified the Bakairi language as belonging to the Carib linguistic family and proposed that there had been Carib migrations starting from the region of the Upper Tapajós and the Xingu rivers. He did not agree with Alexander von Humboldt, who had suggested that Carib migrations originated from northern South America or the Antilles (Schaden, 1993: 58-59). Von den Steinen's hypothesis, based on inferring migrations, routes and origins departing from linguistic data, was criticized by Gerhard Baer (1934-2017); although Egon Schaden (1913-1991) felt that nobody was able to entirely refute it (Schaden, 1956: 123).

Apart from establishing that the language spoken in the various Bakairi villages belonged to the Carib linguistic family, von den Steinen also considered



**Figure 4**  
The Xingu River amid the Amazon River basin. (Adapted from Wikipedia.org) URL: [https://en.wikipedia.org/wiki/Xingu\\_River#/media/File:Xingurivermap.png](https://en.wikipedia.org/wiki/Xingu_River#/media/File:Xingurivermap.png)



the Mehinaku and Iaualapiti part of the (Nu)aruaque linguistic family, the Aueté and Kamayurá part of the Tupi family, the Araueté linguistically representing a mixture of Tupi and (Nu)aruaque, and the Trumai belonging to an isolated linguistic family. His linguistic research, required by German ethnography, was important because linguistic knowledge represented a vital tool for understanding the spiritual and material culture of Upper Xingu Indians.

Despite their linguistic differences and some material specialties related to intertribal trade, Upper Xingu Indians showed many uniformities such as: the slash-and-burn cultivation of manioc and other crops, the preparation of food, stone axes, teeth- and shell-tools, fishing nets, the lay-out of the villages, architecture, cotton hammocks, big round and small ceramic pots, wooden zoomorphic stools, shell necklaces, feather ornaments, *uluri* (women's pubic attire), gourds, the use of tobacco, body paintings, musical instruments, weapons and social institutions. According to von den Steinen, the Upper-Xingu cultural traits and patterns seemed to have originated through diffusion due to frequent intertribal visits during funerals and festivities that had stimulated exchanges (von den Steinen, 1940: 428-429). Although he did not yet employ the concept of "intertribal acculturation," von den Steinen assumed that this "leveling" process (or cultural diffusion) in the Upper Xingu area was still occurring (op.cit.).

As a psychiatrist, von den Steinen showed a tendency to discuss problems relating to the Upper Xingu in psychological terms and he intended to describe the peculiarity of the Indians' mentality. Despite their horticultural activities, von den Steinen argued that they reacted as hunting and gathering societies (Schaden, 1993: 115-116) and belonged to the "stone age" in view of their rudimentary technology, such as stone axes, and the lack of generic concepts or higher abstractions that characterize human societies of a higher cultural level. The cultural homogeneity of the Upper Xingu had been explained as the outcome of a history of peaceful alliances among Indian populations of different cultural origins furthering a process of intertribal acculturation. According to Schaden,

*One of the most intriguing aspects of the problem continues to be the existence of distinct manifestations material and non-material cultural elements of originally heterogeneous ways of life. The innovations are marked in such a way that each tribe continues to maintain its language and does not give up certain cultural specifics (Schaden, 1993: 116).*

Despite the cultural uniformity of the area as the result of past and on-going regional cultural-historical processes, von den Steinen found that there was a certain unevenness among the tribes showing that the levelling (i.e., acculturation) was still occurring and allowed for culture-historical inferences and the

recognition of regional processes of cultural development. Hence, he proposed an interpretation of cultural developments based upon a multilineal approach.

Besides the cultural diffusion furthered by frequent intertribal relationships, von den Steinen also intended to explain the origins of certain cultural traits in terms of independent evolution, a process of cultural growth by means of new ideas or “thoughts” based on psychological associations. Studying the data he had collected on spiritual and material culture, he concluded that in the Upper Xingu area, there was an ongoing evolutionary process from “inferior” to “superior,” from “practical” to “aesthetic” – exemplified by the Indian practice of “rubbing their body with *urucu* to protect their skin” evolving to “*urucu* body paintings” as an esthetic expression; and from “concrete” to “abstract” – exemplified by conceiving a fish in the bottom of a fishing-net as having inspired the *merexu* or “decorative motive” painted on artifacts, such as masks, pots and stools, that originated from certain visual aspects of animal species in the area. Von den Steinen tried to formulate the intrinsic logic of the “origins” of concrete cultural manifestations in terms of “thoughts growing out of combinations and interactions among sensations, images, and ideas,” a theory formulated by the psycholinguist Herbart, who had also inspired Bastian's theory of the *Völkergedanken* (Viertler, 2018: 206).

According to Herbert Baldus, “Karl von den Steinen's discoveries and subsequent studies furnished material of perennial value to the cultural history of the continent. They completed and modified the ethnographic map and familiarized us with Indian life as no other previous study, and few other subsequent ones, have done” (Baldus, 1954: 30). However, ethnographers of the Upper Xingu, such as Paul Ehrenreich, Herrmann Meyer, Fritz Krause, Mark Münzel and Rolf Krusche among others, realized that their rapid journey through the area would entail negative consequences. After the German visitors had left, several flu epidemics reduced the indigenous population in the area. In addition, during his fieldwork, von den Steinen himself had been responsible for the distribution of large quantities of iron axes and knives that, despite being eagerly desired by the indigenous peoples, caused the rapid decay of traditional techniques, while migrations and changes in the spatial distribution of indigenous communities increased the social pressures on Brazilian regional society.

#### **AMONG THE BORORO OF THE SÃO LOURENÇO RIVER**

While the Upper Xingu area was still isolated at the end of the nineteenth century, the Bororo Indians had been exposed to intense social pressures since the early eighteenth century, at a time when some inhabitants of São Paulo, called *bandeirantes*, formed armed expeditions guided by indigenous slaves and left the coastal areas for Mato Grosso and Goiás in search of Indians, precious met-

als, and precious stones. These expeditions, called *bandeiras*, were controlled by the Portuguese crown with the purpose of increasing its political hegemony over the Brazilian territory against Spanish invasions.

The first neo-Brazilian settlements of Mato Grosso were established in Bororo Indian's territories who retaliated. During these interethnic conflicts some Bororo were reduced to slavery and obliged to work in the gold mines or on the plantations in Mato Grosso and São Paulo. Others were forced to become soldiers of the *bandeirantes*, such as Antonio Pires de Campos, who fought against the Kaiapó, the Akroá, and the Xakriabá Indians apart from the Paiguá indigenous communities, who were attacking the newcomers and hindered regional commerce.

From 1725 to 1727, Mato Grosso province suffered a severe food supply crisis while slaves escaped and miners left Cuiabá in search of new gold repositories. Around 1750, escaping from famine and epidemics, several groups of Bororo crossed the Brazilian border and settled in Bolivian territories, creating the *Bororo Ocidentais* (western Bororo). They engaged in hostilities against the neo-Brazilians, stealing iron tools but were later subdued and obliged to work in exchange for metal tools, clothes, and liquor.

Meanwhile, the *Bororo Orientais* (eastern Bororo) continued their hostilities against neo-Brazilians. Between 1771 and 1775 large farms were established along the Cuiabá River and the expanding economic frontier invaded and occupied further Bororo territories while their traditional owners retired to isolated areas near the São Lourenço River.

During the nineteenth century the Bororo of the São Lourenço river continued to all cruelties perpetrated by the "civilized" in a never-ending cycle of hostilities. It was only in 1886 that the president of Mato Grosso province finally ordered the pacification of the Bororo living on the margins of the São Lourenço River. This difficult task had to be accomplished by soldiers established in two colonies along the São Lourenço River.

Since the eighteenth century, suspicious and afraid of the "fierce Bororo," neo-Brazilians offered them liquor obtained from many distilleries established around the lower Cuiabá River. This practice was intensified after the Bororo pacification of 1886 and reinforced by regional politicians, who used to enter Bororo Indian reservations before elections, and by the Bororo themselves.<sup>3</sup>

In 1888, leaving the peaceful and bucolic atmosphere of the Upper Xingu, Karl von den Steinen went to visit the Bororo living in the Teresa Cristina colony who received systematic liquor distributions from the colony administrators. The aim was to control them and keep them inside the colony in order to prevent Bororo hunting and gathering expeditions that might get in the way of the Brazilian economic expansion (Viertler, 1993: 185).

**3** Even today the Bororo buy liquor from their pensions.



Soon after his arrival, the two hundred Bororo Indians living in promiscuity with more than fifty neo-Brazilian soldiers and a few corrupt military administrators in concubinage with Indian women shocked Von den Steinen. Afraid of the Bororo (who had been pacified only two years before), the administrators of the colony offered them gifts such as clothes, food, and sugar-cane liquor without any work or pay. This situation caused indignation among the soldiers who were obliged to pay for everything they needed. Besides, they also had to work hard in fields that were systematically plundered by the Bororo. Von den Steinen explained this plundering in terms of the Bororo's stage of socio-cultural evolution since, as hunters and gatherers, they seemed to lack any knowledge of horticulture. However, during his stay in Teresa Cristina, he was told about a Bororo village downstream that was producing various species of gourds, corn, tobacco, and cotton yet von den Steinen still classified the Bororo as being hunters and gatherers. Later research showed that the Bororo did have horticultural knowledge at the time of von den Steinen's visit and seem to have refused to "work" for neo-Brazilians who they considered as *subordinates and therefore entitled to work for the Bororo*. This interpretation may explain why eventual gifts of iron tools offered by von den Steinen would not be well accepted by the Bororo of Teresa Cristina.

Based on ethnographic and iconographic material, von den Steinen concluded that despite some differences, the Bororo of the São Lourenço river belonged to the same tribe as the Bororo da Campanha and the Bororo Cabaçais near the Paraguai river. He rejected Martius's hypothesis that the Bororo were the result of a *colluvies gentium*, a cluster of people of different origins. According to von den Steinen, the division of the Bororo into Occidental and Oriental may be seen as the result of diffusion through migrations of various Bororo groups originat-

**Figure 5**  
Dwellings in the Colônia Teresa Cristina. Drawing made by Wilhelm von den Steinen on March 27, 1888. (Courtesy: GRASSI Museum für Völkerkunde zu Leipzig, Staatliche Kunstsammlungen Dresden, Germany).



ing from the area of the lower São Lourenço River (von den Steinen, 1940: 568).

Still marveled by his experience in the Upper Xingu, von den Steinen was disgusted by the humiliating conditions in which the Bororo lived in Teresa Cristina. He could not help but opening his heart by means of a sarcastic description of the drunk Diapocuri. He was frustrated by the impossibility of doing systematic research of the Bororo language, which he considered pertaining to an unknown linguistic family (later identified as the Macro-Gê or Otuké family). However, despite all difficulties, he obtained precious information about social institutions and funerary ceremonies.

In Teresa Cristina, despite the daily turmoil caused by drunken Bororo and a funeral ceremony – usually a time of much drinking (traditionally of fermented palm tree sap) and social tensions – von den Steinen was able to obtain rich ethnographic data about many aspects of Bororo material and spiritual culture, confirmed by later ethnological research. He considered Bororo social organization of the utmost ethnological interest because it displays unknown social institutions: (1) the institution of the “women of the men’s house”; (2) the “blessing ceremonies” over certain animal and vegetal species before they are consumed; and (3) the “representation of the deceased’s souls.” Von den Steinen explicitly considered that the first two institutions were linked to food shortage and, as was stated later, the third institution, being associated with hunting and fishing expeditions, was

**Figure 6**

Dwellings in the Colônia Teresa Cristina. Drawing made by Wilhelm von den Steinen on March 27, 1888. (Courtesy: GRASSI Museum für Völkerkunde zu Leipzig, Staatliche Kunstsammlungen Dresden, Germany).



also linked to periodical food distributions in the village (Viertler, 1991).

According to von den Steinen, there was a clear social division between older married men living in the houses of their wives and youth, and bachelors living in a central men's house (*baito*). The bachelors only had sexual access to a small number of young unmarried women (*aredo baito*), who were forbidden to procreate. After intercourse, the bachelor was obliged to offer ornaments or arrows to the older male relative of the woman he had had intercourse with. Von den Steinen interpreted this institution as a way of restricting demographic growth, of limiting the population size and, therefore, as a social strategy to reduce the need of food (Thieme, 1993: 79) in a very harsh environment. This idea was later confirmed by ecologists who characterized the Bororo territories in Central Brazil as a habitat of strong seasonal resources fluctuations.

Von den Steinen studied the rigorous patterns of food distribution. There were taboos and rules for consuming certain animal and vegetal species only after they had been ceremonially blessed and divided by a *bari*, a shaman who, by means of his spirits, punishes anyone who breaks the rules with death. In contrast to the chaotic food distributions organized by the administrators of the colony, food was traditionally prepared and distributed in an orderly and peaceful manner. This was mainly, as was found out later, during collective meals honouring the souls of the deceased.

The Bororo men's house was also an artisanal space for producing beautiful feather ornaments, spinning cotton, or singing before hunting and fishing expeditions required by ceremonial life. Von den Steinen did not explicitly mention the institution of the "representation of the deceased" by a Bororo hunter designed as a "new soul" (*aroe maiwu*), who was tasked to revenge the deceased by hunting a jaguar. Despite aversions and frustrations, von den Steinen made such an accurate description of this particular character that we may identify the drunk Diapocuri as one of those ritual hunters who, painted in black and wearing a string made from human hair, had been chosen as the "representative" of one of the two deceased Bororo whose funeral was celebrated at the time of his visit. Later ethnologists have supported that the "representative of the deceased", besides hunting, offering trophies of the dead animal to the mourners and dancing during the funeral ceremonies they were also obliged to provide food for the mourning family as long as they lived; hence, another original social strategy for guaranteeing food to completely deprived members of the village.

In the middle of the turmoil in Teresa Cristina, von den Steinen successfully carried out a very difficult line of research.<sup>4</sup> Bororo ceremonial chiefs demand total obedience of their orders, an inversion of the political hierarchy of everyday life that may obstruct ethnological research. However, despite all these difficulties, von den Steinen managed to follow Bororo funeral etiquette. He accurately

**4** Present-day ethnologists feel it is also difficult, sometimes even impossible, to continue their research, mainly with Bororo Indians directly involved in funerals, a time of much drinking and latent hostilities against foreigners.

described the mourners's weepings, collective huntings, funerary dances, hunting chants, and the incineration of the deceased's property; a practice he interpreted as a way of eliminating the accumulation of material property. With this interpretation, von den Steinen foreshadowed later economic anthropologists such as George M. Foster who formulated the law of "the limited good" among non-capitalist societies.

At the end of the funeral cycle, after much collective singing and dancing, the *Bororo* performed - and still do - the *aije* ceremony forbidden to non-initiated women and children. The *aije-doge* or *bull-roarers* ceremony is associated with the washing of the rotten corpse, a gruesome task done by the representative of the deceased's soul. Von den Steinen described the *bull-roarers* being whirled in circles and buzzing in the middle of the village, frightening women and children locked in their houses. During such a tense ceremony he managed to register the bull-roarers's decorative motives – bands, circles and squares – which he interpreted as having originated from the vision of the deceased's bones, skulls, and the square funerary baskets that had to be blessed, painted, and feathered (von den Steinen, 1940: 634). Therefore, as proposed for the Upper Xingu, Karl von den Steinen explained the origins of Bororo decorative motives – bands, circles and squares – by ideas departing from concrete visual experiences.

Ethnographically comparing the Upper Xingu Indians with the Bororo of the São Lourenço, von den Steinen suggested that the latter being at a less advanced cultural stage, saw the *bull-roarers* as a way of communicating with the dead. In contraposition, the Upper Xingu Indians, at a more advanced stage, used them as toys and in dances. However, according to him, the variability of their meaning in the context of different tribes required careful analysis in each case (von den Steinen, 1940: 633).

Von den Steinen, always interested in establishing the "origins" of cultural traits and patterns, also interpreted Bororo women's belts having broad grey stripes originating from the practice of carrying heavy baskets on their back during collecting activities – the stripes functioning to support the basket and at the same time to protect the skin. He also interpreted the custom of decorating whole parts of the human body with feathers as a form of medicine (Thieme, 1993: 81).

Apart from details relating to the institutional aspects and material culture of Bororo Indians, von den Steinen also discovered many aspects of their spiritual life which he described by means of "thoughts." These included, "the equivalence of men and animals," or "if a *Bororo* kills an animal its' spirit will kill the Bororo," or "funerary hunting and fishing as expeditions represent a way of communicating with the dead" – exemplifying Bororo ideas composing a particular *Weltanschauung*. In the twentieth century this procedure was also employed by American configurationists from the culture and personality school.

He did not frame his ethnographic data in terms of social morphology concepts such as “moieties,” “clans,” or “kinship,” sociological concepts later employed by the Salesian missionaries. In this respect he followed the principle of Herbart that sociological and psychological aspects cannot be separated. Accordingly, he did not see social relations as an autonomous aspect of human social life that can be detached from spiritual and material cultural data. However, he succeeded in distinguishing the social function of the *bari*, a shaman connecting to the spirits of animals and plants, from that of the *aroe etawara aro*, a shaman connecting to the souls of the deceased.

The numerous and accurate ethnographic data obtained by Karl von den Steinen in the short time he spent among the Bororo, attests to his sensitivity as a social researcher working in the German tradition of obtaining the most reliable and best ethnographic information. His ethnographic data relating to the Bororo give priority to social institutional and ceremonial aspects, while those obtained in the Upper Xingu river basin reveal an emphasis on material culture and linguistic data. In the Upper Xingu area he was able to develop linguistic research and to obtain a rich collection of artifacts (approximately 1.322 items) now held at the Ethnological Museum in Berlin. According to Günther Hartmann (1993), only 349 artifacts were obtained from the Bororo in Teresa Cristina. We may consider that, at the time of von den Steinen's visit, many epidemics were already hitting the Bororo, requiring more funerals and incinerations of traditional artifacts that had belonged to the deceased. Besides, the growing shortage of raw material caused by the loss of tribal territories and the negative ecological impact of the neo-Brazilian expansion made it more difficult, if not impossible, to produce certain traditional artifacts.<sup>5</sup>

## CONCLUSION

The careful compilation of ethnographic data is one of the main characteristics of the ethnographic tradition created by German ethnographers working in Russia and elsewhere during the eighteenth and nineteenth centuries. Differing from British and French ethnology, German *Völkerkunde* stressed the importance of linguistic data and material culture, as did von den Steinen. Opening the way for Brazilian ethnology (Petschelies, 2018: 544), he compiled accurate ethnographic data that continue to be valuable for new research.

According to Baldus (1954) the two Xingu expeditions of Karl von den Steinen were the most important events in nineteenth-century Brazilian ethnology and inspired new research for German ethnographers. These included Paul Ehrenreich (1855-1914), who had done independent research in Brazil in 1884, who accompanied von den Steinen during the latter's second expedition

**5** During the last decades, although traditional artifacts are still being used during ceremonies, sometimes borrowed from other Bororo villages, the Bororo are engaged in producing non-traditional artifacts in order to make money and buy goods in Brazilian cities.



to the Upper Xingu and Bororo Indians and stayed on to visit the indigenous population along the Purus and Araguaia rivers; Herrmann Meyer (1826-1909), who visited the Upper Xingu area in 1896 and 1899 accompanied by Karl Ranke (1870-1926) and Theodor Koch-Grünberg (1872-1924) (Schaden, 1993: 111); Fritz Krause (1881-1963), who besides studying the material culture of the Upper Xingu Indians visited the Karajá of the Araguaia river and the Kayapó in 1908 and published ethnographic data on the Upper Xingu and the Karajá (Baldus 1954: 14); and Max Schmidt (1874-1950) visiting the Upper Xingu area four times from 1900 onwards. Later research was conducted, for example, by Gerhard Baer (1934-2017), Mark Münzel (b.1943), Günther Hartmann (1924-2012), and Rolf Krusche (b.1938).

In other Brazilian regions, the German tradition was continued by Herbert Baldus (1899-1970), who migrated to Brazil in order to escape the Nazi regime and did important research among the Tapirapé and several other Brazilian Indian tribes; Curt Nimuendajú (1883-1945), who worked among Gê- and Tupi-speaking Indians producing an ethnographic map and publishing many articles and books; and Egon Schaden (1913-1991), son of a German immigrant who published important studies on Tupi-speaking Guarani and several other indigenous tribes. According to Schaden, German tradition consisted of obtaining solid empirical data that, more than illustrating theories, are gathered to prove them.

The work of Karl von den Steinen may be considered the beginnings of systematic ethnological fieldwork, and goes on inspiring new research among Brazilian Indigenous tribes.

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**Renate Brigitte Viertler** was born in Germany, and lives in Brazil where she did her studies in social sciences at the University of São Paulo. Starting as a teacher at the Anthropology Department of the same university, she engaged in ethnological fieldwork among the Bororo Indians of Mato Grosso. Obtaining her PhD in 1973 and becoming Associate Professor, she published books and articles on Bororo social organization besides lecturing and teaching at foreign universities. The last few years she spent studying the theoretical foundations of German ethnology and anthropology. Her latest book *Os Fundamentos da Teoria Antropológica Alemã: Etnologia e Antropologia em Países de Língua Alemã (1700-1950)* was published in São Paulo by Editora Annablume in April 2018.

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# “Savages at the End of the World” – Robert Lehmann-Nitsche and his Studies of the Indigenous Peoples of Patagonia, 1898-1919

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## Diego Ballester

🏠 *Universidad Nacional de la Plata | La Plata, Argentina*

✉ *dballest@uni-bonn.de*

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### ABSTRACT

One of the first research interests of the German anthropologist and ethnologist Robert Lehmann-Nitsche (1872-1938), upon assuming the position as head of the Department of Anthropology at the La Plata Museum in Argentina (*Sección de Antropología del Museo de La Plata*), concerned the indigenous peoples of Patagonia. This article explores Lehmann-Nitsche's anthropological, ethnological, and linguistic studies among the indigenous peoples of Patagonia (Selk'nam, Gennakenk, Mapuche, Aónikenk, Kawésqar, and Yaghan) conducted between 1898 and 1919. These studies included research into pathology, craniology, anthropometry, archaeology, ethnology, linguistics, and mythology. He aimed at contributing to contemporary theories of German scholars on the development and characterization of “natural peoples” (*Naturvölker*), as opposed to “cultural peoples” (*Kulturvölker*), asserting a basic division of humanity.

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### KEYWORDS

Anthropological Practices, South American Indigenous Peoples, Patagonia, Language Studies, Myths, Robert Lehmann-Nitsche (1872-1938), Argentina, Museo de La Plata.

*If we ... accept the principle that the modern barbarian world has preserved to a fair degree the culture of humanity's adolescence, we may legitimately go a step farther and look to the modern savage world for some clue to the culture of humanity's childhood.*

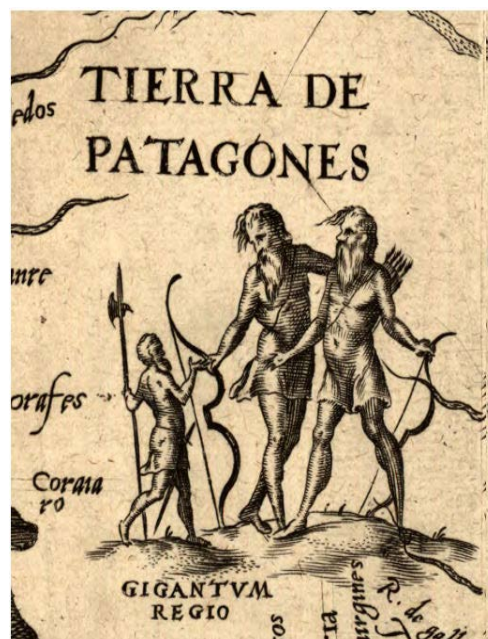
Cooper, 1917: vi

Since the early sixteenth century, Patagonia has been linked, in the European imaginary, to a region where a race of giants inhabited the distant and uncharted southern borders of the Spanish empire<sup>1</sup> (Fig. 1). Despite the increase of geographic, cartographic, and ethnographic knowledge, this narrative persisted until the second half of the nineteenth century. From then on, “Patagonian giants” were also inserted into anthropological evolutionary accounts. In the case of German ethnology, they were classified along with Africans, Pacific Islanders, and other indigenous peoples of the Americas, as “natural peoples” (*Naturvölker*), who differed from “cultural peoples” (*Kulturvölker*). In this theoretical approach, that distinction constituted a basic division of humanity (Waitz, 1859).

“Natural peoples” supposedly lacked history and were separated from narratives of Western civilization; being attributed a greater proximity to nature. Supposedly isolated from the world around them and evolution in general, these groups seemed promising in order to reach back deeper in time; offering a source of information for understanding the evolutionary path of European populations and human history’s underpinnings (Gould, 1988; Rudwick, 2014; Stocking Jr., 1987; Wolf, 2010).

In the last third of the nineteenth century, innovations and technological modifications in the fields of communication and transport contributed to an increasingly “networked” pattern of politics, economics, and social life, thereby endangering the very indigenous peoples that ethnology sought to investigate. Scholars studying them believed in their imminent and inevitable extinction (Penny, 2002; Zimmerman, 2001). By the 1860s, Adolf Bastian (1826-1905), a German physician, world traveler, and director of the Royal Museum of Ethnology in Berlin, had begun

**1** In 1523, Maximilianus Transylvanus (1490-1538) published the first descriptions of Patagonian indigenous people, which were characterized as being of “extraordinary height [...] clothed in the skins of wild beasts and seemed darker” (Pigafetta, 1874: 190).



**Figure 1**  
“Gigantic Regio” (Region of Giants). Detail from the 1562 Map of America by Diego Gutiérrez and Hieronymus Cock. Source: Gutiérrez and Cock (1562)

his large-scale empirical project of obtaining materials from a range of rapidly vanishing “natural peoples” in Africa, Australia, and South America. To this end, he had established extensive international networks of collection, collaboration, and exchange in order to obtain ethnographic and osteological material. Bastian, like other scholars, affirmed that the elements of contemporary peoples’ material culture and the material traces of earlier peoples were fundamental in understanding the evolution and development of humankind in a direct and “objective” way.



**Figure 2**  
Robert Lehmann-Nitsche (ca. 1899)  
Source: Lehmann-Nitsche legacy, Ibero-American Institute (Berlin)

Robert Lehmann-Nitsche’s (1872-1938) studies of the inhabitants of Patagonia were developed within this international scientific context (Fig. 2). These studies included the establishment of multiple registers of physical, anatomical, and ethnological characteristics in order to salvage what he believed were the essential elements of these rapidly vanishing “natural peoples.”

Arriving in Argentina on 10 July 1897, Lehmann-Nitsche assumed the position as the head of the Department of Anthropology (*Sección de Antropología*) of the Museo de La Plata in Buenos Aires, which had been left vacant by the Dutch anthropologist Herman ten Kate (1858-1931). Lehmann-Nitsche had studied in Freiburg, Berlin, and Munich, where he obtained two doctorates at the Ludwig-Maximilian-University, the first in natural sciences (1894) and the second in medicine (1897). Once settled in La Plata, he soon began forging ties with local and foreign scholars, particularly with the Germans, which gave him access to instrumental and bibliographical resources for his research. In this matter, the German community residing in Argentina was also crucial as its members provided him with an important logistical and material infrastructure needed for his anthropological expeditions undertaken between 1900 and 1926. His research interests included folklore, linguistics, archaeology, paleoanthropology, ethnology, and mythology.

In 1903, he taught the first Argentinean university courses in physical anthropology at the Universidad de Buenos Aires. With his appointment as the university’s Professor of Anthropology in 1905, the first South American university professorship in physical anthropology was created. In 1906, he also began to teach anthropology at the Universidad de La Plata. At both universities, he supervised several doctoral dissertations related to physical anthropology and paleoanthropology.



He was a member of numerous scientific societies in Argentina, France and Germany. After his retirement in 1930, he returned to Berlin to work as a lecturer for South American cultures (*Kulturen Südamerikas*) at the University of Berlin until his death in 1938.

Lehmann-Nitsche's original idea had been to stay in Argentina for no longer than six years, a time span he considered sufficient in order to investigate the important Museo de La Plata's osteological collections and to write a series of monographs on them. His aim was to gain academic visibility in order to obtain a workplace in the competitive German scientific context (Ballesterio, 2014).<sup>2</sup> However, he stayed in the country much longer and dedicated more than 20 years in the study of Patagonia's indigenous peoples, focusing on subjects ranging from paleoanthropology to linguistics.

### TREPANNED SKULLS AND ILL BONES

Lehmann-Nitsche's first research on Patagonia's indigenous peoples should be understood in the light of the research opportunities that catered to his personal needs. The "access" to important osteological collections was seen as an opportunity to establish a reputation among scholars doing research on the indigenous peoples of southern Argentina. For scholars, skeletal remains, in comparison to results provided by "subjective" written sources, were supposedly more impartial, objective, and accurate when studying humankind's history and evolution (Daston and Galison, 2010; Fabian, 1983; Lucas, 2005).<sup>3</sup> Therefore, scholar's demands for skeletal remains increased significantly during the second half of the 19<sup>th</sup> century. In a competitive global market, where European institutions conducted an aggressive policy of acquisition,<sup>4</sup> Lehmann-Nitsche took advantage of his unhindered and exclusive access to the Museo de La Plata's osteological collections.

Following the subject developed in his second doctoral thesis, Lehmann-Nitsche began a comparative study of racial pathology and craniology (Lehmann-Nitsche, 1904a).<sup>5</sup> In the beginning, he focused on injuries and surgical interventions, such as trepanations and perforations, remarking that the Patagonian skulls provided irrefutable evidence of the antiquity of surgical interventions in South America's southern region (Lehmann-Nitsche, 1898b; 1902a). Based on the cranial characteristics and the visible marks, he established ethnographic parallels between the operative and post-operative processes of Neolithic populations in Europe and contemporary indigenous peoples from South America and other so-called "primitive" peoples, such as the "Aboriginal Tasmanians" and the "Torres Strait Islanders" (Lehmann-Nitsche, 1898b; 1916a).

**2** This career plan was influenced by the Swiss anthropologist Rudolf Martin (1864-1925), who negotiated Lehmann-Nitsche's appointment as head of the department of anthropology of the Museo de La Plata with its director, the Argentinian Francisco Moreno (1852-1919). Martin assured Lehmann-Nitsche that La Plata's Museum was merely a repository of anthropological collections with no scientific production. According to him, these valuable collections were wasted due to the lack of a professional anthropologist. With the exception of ten Kate's work, Martin assured Lehmann-Nitsche that the scientific results produced at the Museo de La Plata were poor and inconsequential (Martin, 1896).

**3** For Lehmann-Nitsche, skeletal remains provided crucial information when it was useless to "dig in written history's old books" (Lehmann-Nitsche, 1898a: 28). Unless specified otherwise, all translations from German, French and Spanish are mine.

**4** In his research on the supposed existence of pre-Columbian leprosy in America, the American physician Albert Ashmead (1850-1911) pointed out "The Leipzig authorities when collecting specimens even killed a Guayaquí Indian in South America to obtain his skull!" (Ashmead, 1903: 383).

**5** To this end, he assembled a "pathological cabinet" with 300 skulls, 19 skeletons, and almost 2.000 isolated bones from Patagonia and began writing a series of brief essays on racial pathology and craniology. According to ten Kate (1897), these skeletal remains were ideal if one intended to undertake

Lehmann-Nitsche also studied the lesions present in the long bones of the arms and legs, formulating hypotheses about the probable pre-Columbian existence of osteological infectious diseases and the chronic joint disease found in contemporary Patagonia's indigenous people.<sup>6</sup> For him, the observable pathological conditions resulted directly from the nomadic lifestyle of the indigenous peoples of Patagonia. Observing how Patagonia's indigenous people affected with chronic joint disease could endure their nomadic lifestyle, he stated that "primitive peoples, like prehistoric peoples, were clearly characterized by a great resistance to infection and a less pronounced sensitivity to pain than modern civilized peoples" (Lehmann-Nitsche, 1902b: 29).

Lehmann-Nitsche noted that the structural, economic, and institutional limitations of being a scholar in Argentina restricted his research. In order to resolve these unsatisfactory local working conditions, he implemented different strategies. One strategy was to travel to Europe in order to acquire anthropological instruments and specialized literature,<sup>7</sup> visit institutions and museums, and attend scientific congresses. There he had the opportunity to discuss his findings and compare them with the views of other German scholars (Lehmann-Nitsche, 1900b; 1902b; 1904c).<sup>8</sup> Another strategy was to develop a collaborative network with other scholars involved in craniological studies.<sup>9</sup>

## INDIGENOUS BODIES

Together with the studies on osteology, Lehmann-Nitsche studied living indigenous people of Patagonia. In 1898, he measured and photographed two indigenous families being exhibited at the National Industrial Exhibition in Buenos Aires; he also gathered a vocabulary and made drawings of them.<sup>10</sup> Similar studies were conducted at the Museo de La Plata's anthropological laboratory in 1905 on a group of 7 individuals returning from being exhibited at the Louisiana Purchase Exposition (United States). On this occasion, he made one of the first phonographic recordings of South American indigenous music (Lehmann-Nitsche, 1908; 1916a).<sup>11</sup>

This research allows us to reflect on the nature of "fieldwork" in the early twentieth century. The "field" was not a physically defined place; rather an abstract, timeless, and transitory space constituted through specific practices, technologies, and actors (Latour and Woolgar, 1986; Lynch, 1991). In both circumstances, the physical space in which the studies were conducted was irrelevant to Lehmann-Nitsche, as for him the priority consisted in approaching a study object that, as he pointed out, was becoming extinct: "Our South American tribes are destined to disappear, so we need to take urgent action and save what still exists" (Lehmann-Nitsche, 1899: 124).

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an objective and scientifically important anthropological research in South America, a view also propounded by one of the founders of French physical anthropology, the French physician Paul Broca (1824-1880).

**6** Specifically he studied the occurrence of leprosy, syphilis, lupus, rheumatoid arthritis, rickets, avascular necrosis, periostitis and osteitis. The results were compared with those known from contemporary European populations.

**7** Lehmann-Nitsche remarked prior to his first journey to Europe in 1900 that his main objective of this trip was to "carry out osteological studies in specialized laboratories, which cannot be done in Argentina due to the lack of equipment and specialized instruments." Therefore, he bought new anthropometric instruments and photographic equipment in order to overcome "deficient instruments" used during his first research studies (Lehmann-Nitsche, 1900: 3; 1901b: 4).

**8** In 1900, he brought 5 skulls and 4 jaws with artificial injuries to be exhibited in meetings of the Berlin Society for Anthropology, Ethnology, and Prehistory; they were subsequently examined by the German physicians Rudolf Virchow (1821-1902) and Curt Strauch (1868-1931). In 1904, he exhibited purportedly syphilitic long bones in the International Congress of Americanists meeting in Stuttgart (Germany).

**9** One example was the collaboration with the German palaeontologist Hermann von Ihering (1850-1930) in order to develop a

Together with the studies carried out in urban spaces, Lehmann-Nitsche conducted fieldwork in the southern part of Argentina and Chile in 1902, carrying out studies in places such as mission stations, police posts, and private landholdings, where a significant number of indigenous people were concentrated. These places provided Lehmann-Nitsche with physical spaces where he could carry out his research, procure food, and find rest. The possibility of access to these places also reveals the impact of the shared sociability of scholars, but also of chance. Prior to travelling the Tierra del Fuego region, Lehmann-Nitsche received several letters from German philologist Rudolf Lenz (1863-1938), who was living in Chile since 1899, providing him with instructions on how to travel in Chile and a list of members of the German community that could lodge him. Similarly, access to the police stations spread throughout Tierra del Fuego was made possible by Eduardo Holmberg, responsible for an exploratory expedition commissioned by the Argentinean Agriculture Department, whom Lehmann-Nitsche coincidentally met on board the steamboat to Tierra del Fuego (Ballesterro, 2014).

Working in remote locations, far from the closed space of the laboratory and the commodity of the academy, required the collaboration of individuals with little or no academic training in anthropological observation. Lehmann-Nitsche received help by local police officers and the Salesian missionaries from the Misión Salesiana de la Candelaria (Fig. 3 and 4). They actively helped him by making notes for his linguistic and anthropometric studies, taking photos of indigenous persons, and subsequently offering them to him (Lehmann-Nitsche, 1902). These “invisible technicians,” generally omitted from publications and official reports, played an active role and were often indispensable in the creation of scientific knowledge (Shapin, 1989).

craniological cartography of South American's indigenous peoples (von Ihering, 1903; 1910). Unfortunately, like in other cases, this cooperation was restricted solely to private correspondence and never materialized in any concrete project.

**10** The families brought by Lieutenant Colonel Pedro Godoy (1858–1899), governor of Tierra del Fuego, consisted of two couples, the men aged 18 and 22, the women 20 and 16, and two children aged 8 years and 6 months.

**11** Lehmann-Nitsche's main objective was to collaborate with the musical cartography projects promoted by the Berliner Phonogramm-Archiv.



**Figure 3**  
Patagonians in the Tierra del Fuego police station (1902)  
Source: Lehmann-Nitsche's legacy, Ibero-American Institute (Berlin)

Towards the first decade of the twentieth century the influence of physical anthropology and the anatomical construction of “races” based on measurements of body parts, especially the skull and the long bones, declined. This led to an increasing interest among scholars in collecting myths, vocabulary and phonographic recordings. According to Lehmann-Nitsche it was necessary to complement biological studies with other research that could penetrate indigenous peoples’ psyche. In this regard, throughout the first two decades of the twentieth century, he published extensively on linguistics and mythology, synthesizing in this way a vast body of data gathered from 1897 on.



**Figure 4**  
Patagonians in the Salesian mission of Tierra del Fuego (1902)  
Source: Lehmann-Nitsche’ legacy, Ibero-American Institute (Berlin)

#### **ARIADNE’S THREAD**

By 1915, Lehmann-Nitsche described the situation of linguistic studies in Argentina as formless and chaotic. Meanwhile, documenting and analyzing indigenous peoples’ languages constituted a consistent and continuous part of his research during his stay in Argentina. His linguistic studies covered a number of subjects: the gathering of vocabularies, the determination of linguistic correlations between Patagonian indigenous peoples and other South American regions, the publication of early linguistic reference sources, and finally the preparation of a schematized linguistic cartography of Patagonia.

As with his craniological and anthropometric studies, Lehmann-Nitsche benefited from a preexisting large body of data gathered throughout the second half of the nineteenth century by scholars, missionaries and military. Previous comparative studies were confined to the relations between languages, emphasizing similarities and differences between them. By contrast, Lehmann-Nitsche proposed to considering the linguistic classification as “Ariadne’s thread” for the ethnographic classification of American indigenous peoples (Lehmann-Nitsche, 1918: 324). In this sense, he pointed out that the linguistic approach “has proven its effectiveness in cataloging the vast number of indigenous peoples” (Lehmann-Nitsche, 1922a: 21). As he claimed, anthropology was not only physical anthropology; it could not and should not be limited to the biological study of humankind (Lehmann-Nitsche, 1904b).

The opening up of the Spanish and Portuguese domains in South America,

at the beginning of the nineteenth century, coincided with the start of European scientific expeditions set in order to remedy the general lack of knowledge about the “New World.” Scholar-travelers such as the German Alexander von Humboldt (1769-1859), Carl Friedrich Phillip von Martius (1794-1868) or the French Alcide d’Orbigny (1802-1857) documented a large number of unknown indigenous languages, contributing to the initial assessment of linguistic diversity in South America.<sup>12</sup> Based on this vast collection, scholars in France, the United States, and Germany promoted an ethnolinguistic shift in regards to the study and classification of South American indigenous peoples during the last decades of the nineteenth century (Campbell and Grondona, 2012).

Works such as *The American Race* (1891) by the American ethnologist Daniel Brinton (1837-1899) or *Principes et dictionnaire de la langue Yuracaré* (1893) by the French linguist Lucien Adam (1833-1918) proposed the first taxonomic classification of South American indigenous languages. For both of them, previous geographical, phenotypical and racial classifications had not yet produced convincing scientific results; only a linguistic approach could remedy this situation (Adam, 1893; Brinton, 1891). Referring to these works, the Argentinian linguist Samuel Lafone Quevedo (1835-1920) stated, “the philological derivation correlates with the results of archaeology, craniology, anthropology, geology, and common sense” (Lafone Quevedo, 1893: 127).

Lafone Quevedo was one of the most active Argentinian scholars in the systematic study of Argentinian indigenous languages. Like Lehmann-Nitsche, he was a strong critic of previous work conducted by individuals without an appropriate academic background. Between 1896 and 1915, he published vocabularies and original manuscripts from the region of Gran Chaco (Argentina), which he complemented with studies realized by him. With auspicious eyes, Lafone Quevedo saw what he considered as the beginning of linguistic studies in Argentina; which would not only contribute to the development of ethnology and anthropology at a local level but would also support linguistic studies carried out in Europe.<sup>13</sup>

If Brinton and Adam were the main influences on Lafone Quevedo’s linguistic studies, for Lehmann-Nitsche’s studies it was the work of German ethnologists such as Paul Ehrenreich (1855-1914), Karl von den Steinen (1855-1929), Franz Boas (1858-1942), Konrad Theodor Preuss (1869-1938), Theodor Koch-Grünberg (1872-1924), and Rudolf Lenz (1863-1938). Using slightly different approaches, these scholars questioned the racial classification based exclusively on morphological or osteological features by advocating a philology-based methodology, which would lead them to establish interethnic relations and migration patterns.

Giving special importance to the geographical distribution of cultural elements, these scholars considered that anthropology should adopt a linguistic approach in order to increase knowledge about South American indigenous

**12** Von Humboldt registered important ethnographic data and vocabulary lists of the languages spoken in the northern Caribbean, Northwestern South America, and the Middle Andes. Von Martius published an extensive vocabulary from 72 ethnic groups of the Amazonia region collected by the Austrian naturalist Johann Natterer (1787-1843). D’Orbigny collected languages from the Southern part of South America and Southern Brazil.

**13** In this sense Lafone Quevedo pointed out, “The Europeans complain about the lack of data on South American languages... Here then, we offer them the first part of data...to fill the void” (Lafone Quevedo, 1892: 373-374).

peoples. Reflecting on their classification, Ehrenreich pointed out: “A reasonable orientation, in view of the confusion caused by the many small tribes, can only be made with a reliable linguistic foundation” (Ehrenreich, 1904: 42). A few years before, Lafone Quevedo had stated, “common lexical roots could help explain craniological community” (Lafone Quevedo, 1893: 127). As Lehmann-Nitsche would later mention, the similarity or difference in some physical characteristics could not determine belonging to a “race,” because no single data on its own, either craniological or ethnological, provided objective criteria for such an affiliation. Physical, ethnological and geographical data should be considered together in order to establish membership of an ethnic group (Lehmann-Nitsche, 1899).

According to Ehrenreich, if the future of anthropology rested on linguistic studies, these were subject to work carried out by German scholars in the Americas. In this sense, he affirmed: “Even here abroad, if German scholars are in the frontline, then we can expect that Germany will prevail over the United States as the leader in this interesting field of ethnology” (Ehrenreich, 1904: 75). Despite this statement, Ehrenreich called to advance these studies, since to him, South America was still a “terra incognita” from a linguistic point of view (Ehrenreich, 1904: 75). The work of von den Steinen, Boas, Preuss, Koch-Grünberg, Lenz, and Ehrenreich helped clarify methodological considerations, as well as defined instrumental resources, strategies, and spaces where fieldwork should be conducted. These aspects of scientific practice were echoed by Lehmann-Nitsche, guiding his linguistic studies (Ballesterro, 2014).

Considering the above-mentioned, Lehmann-Nitsche’s documentation practice of the languages of Patagonian indigenous peoples differed from previous researches carried out by Daniel Brinton, Lucien Adam or Lafone Quevedo, among others, in the way that he gave equal importance to the analysis of phonetics and grammar while seeking assistance from bilingual indigenous informants to ensure that the gathered linguistic data and their phonetic transcriptions were correct. In addition, he conducted surveys, noted down specific information with respect to the context of collecting, cataloged the interaction time span with the indigenous, and the use of unpublished missionary, military, and governmental sources.

#### **COMPILING LANGUAGE SPECIMENS**

Shortly after arriving to the region, Lehmann-Nitsche began the linguistic registration process. During the previously mentioned National Industrial Exhibition in Buenos Aires of 1898, he had compiled a vocabulary dictated by two Selk’nam, called Tschoskiai and Kíótomen and the translator who had accompanied them.<sup>14</sup> A year later, he transcribed two short stories from a young Mapuche

**14** The vocabulary lists included body parts, elements and nature, utensils, animals, numbers, and pronouns (Lehmann-Nitsche, 1898).



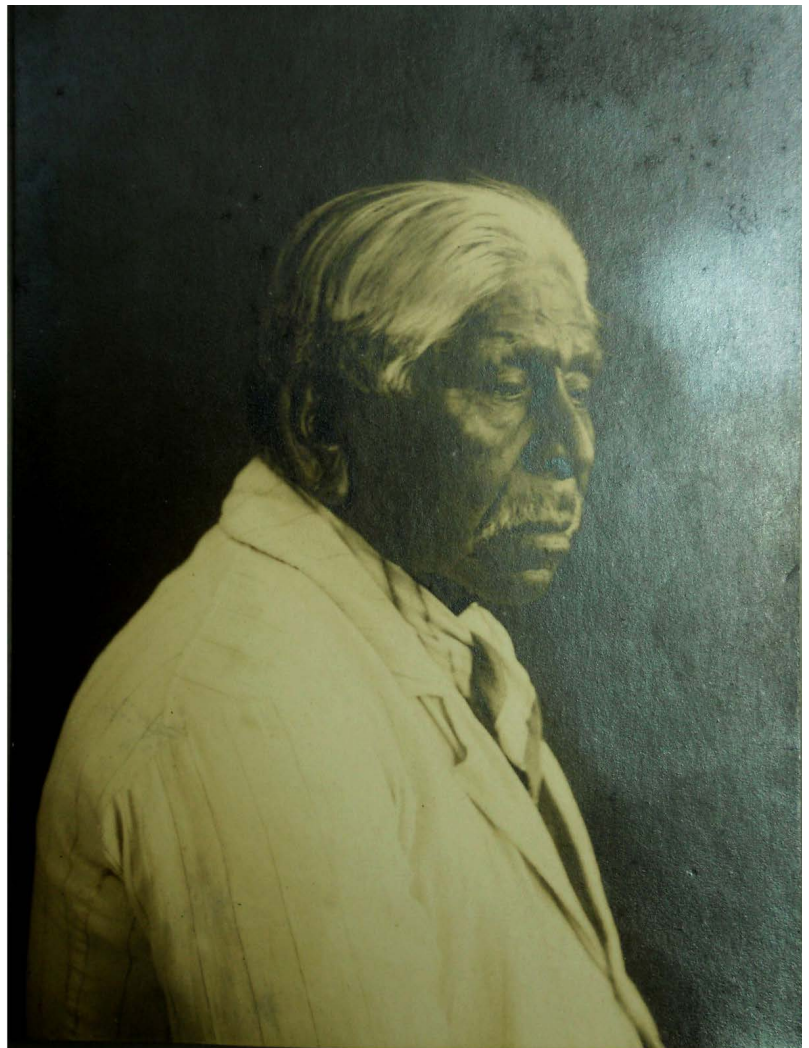
named Lemudeu, who worked as a firefighter in the city of La Plata.

These primary vocabulary lists did not comprise a coherent and systematic plan of study, but like his craniological and anthropometric studies were shaped by his belief in the inevitable physical extinction of indigenous peoples. Lehmann-Nitsche remarked, “The professional philologists, unfortunately, do not appreciate the scientific value of the systematic study of primitive languages that are still spoken today. Especially in South America, precious material disappears every day without philologists documenting it, with only a few exceptions such as Lenz, von den Steinen, Ehrenreich, Lafone Quevedo, etc.” (Lehmann-Nitsche, 1905: 31). Faced with this inexorable reality, Lehmann-Nitsche began with an exhaustive data collection in La Plata

city. This had strategic value for accessing his subject, as La Plata was the epicenter for displaced indigenous people resulting from military campaigns and the subsequent annexation of the Patagonia region.

Very quickly, Lehmann-Nitsche established a network of Mapuche informants in La Plata city (Fig. 5). Some of them, such as Lemudeu, Katrúlaf and Nahuelpi, were important nodes of information, as they facilitated Lehmann-Nitsche’s access to the local Mapuche community and enabled meeting other informants. This collaboration network allows us to single out a few essential points about the role each participant played.

Indigenous living in urban spaces greatly facilitated the scholars’ fieldwork, as they were readily available and there was no need to travel to indigenous communities in the hinterland. At the same time, indigenous informants took an active part in the scholars’ research, using their “indigeneity” in exchange for financial remuneration or special favors by the scholars. Lehmann-Nitsche’s indigenous informants corrected his transcriptions and interviewed other indigenous people in order to gather more information, requesting payment for the



**Figure 4**  
Patagonians in the Salesian mission of Tierra del Fuego (1902)  
Source: Lehmann-Nitsche’s legacy, Ibero-American Institute (Berlin)

hours worked (González, 1902).

In other cases, they benefited from Lehmann-Nitsche's social status as a professor and a scholar in order to intergrate into the Argentinean society of the twentieth century. For example, on his return to the province of Neuquén, Nahuelpi asked Lehmann-Nitsche to recommend him to be reintegrated into the local army battalion. Similarly, in 1908, Katrülaf, who was about to be released from prison, requested 10 or 12 pesos from Lehmann-Nitsche. As Katrülaf remarked, he was going to be released from jail, a situation that left him alone, far away from home, and with no financial compensation (Castro, 1908; González, n.d.).

Between 1899 and 1907, Lehmann-Nitsche gathered large amounts of Mapuche linguistic material, including 2,410 pages organized in a manuscript entitled *Textos Araucanos*, which was never published.<sup>15</sup> For this manuscript, he followed the research protocol described in Lenz's *Estudios Araucanos*, which specified the selection of informants, the transcription of oral texts, the implementation of recording instruments, and related procedures.<sup>16</sup> Following this protocol, he intended to ensure, on the one hand, the standardization of the collected linguistic data in order to compare them; while on the other, to gather control elements for his anthropometric and visual data. Part of the collected linguistic data was systematized and analyzed by Lehmann-Nitsche in a series of articles discussing the alleged existence of Quaternary animals. Thanks to these collected stories, he identified them as otters. Another article discussed the relationship between the stories mentioned above, the European folkloric narrative about animals, and Hansel and Gretel<sup>17</sup> (Lehmann-Nitsche, 1902d; 1905).

#### FIELDWORK IN BUENOS AIRES AND NORTHERN PATAGONIA

One of the most important benefits that Lehmann-Nitsche obtained from working with indigenous people in urban spaces was the possibility of expanding this collaboration network to include friends, acquaintances, and family members of the network's participants living in provincial territories. This dynamic multi-agent structure underpinned the success of Lehmann-Nitsche's fieldwork in the Northern part of the province of Buenos Aires and in Northern Patagonia between 1911 and 1925, offering him a vast body of information; especially, in spaces that were used as strategic logistical centers to rest, obtain supplies, and process the collected data.

The selection of places where fieldwork was conducted was not accidental. They corresponded to geographical spaces included in previous linguistic studies conducted by "armchair scholars", as well as in compilations made by eighteenth and nineteenth-century travelers.<sup>18</sup> Hence, through data collected in situ, Lehmann-Nitsche aimed to ascertain the validity and empirical value of

**15** Throughout 1901 and 1902, Lehmann-Nitsche worked intensively with Nahuelpi, Katrülaf, and Kolüngür in La Plata city. Between 1906 and 1907, he worked with Juan Salva Marinau and Antonio Coron in the cities of La Plata and Buenos Aires. The "Textos Araucanos" were organized in three volumes: Volume 1: Dialogues in Mapuche dialect. Stories about Animals. Mythical stories. Short stories; Volume 2: Historical Stories; Volume 3: Songs.

**16** Contact with Lenz began in 1897. Since that time, they established a fluid correspondence exchange, in which Lenz recommended literature about linguistic studies to Lehmann-Nitsche and helped him in contacting other scholars researching the same subjects (Ballestero, 2014).

**17** Lehmann-Nitsche compared the stories he collected with the following European fairy tales: The Rabbit and the Hedgehog (*Der Hase und der Igel*), Henry the Wolf and his Lions (*Heinrich dem Wolfen und seinem Löwen*), Hansel and Gretel (*Hänsel und Gretel*), and the Town Musicians of Bremen (*Die Bremer Stadtmusikanten*).

**18** He conducted fieldwork in Los Toldos, Antonio Oeste, Ramos Mexía, Aguada Cecilio, Valcheta, General Conesa, Carmen de Patagones, Pringles, Sauce Blanco, Valcheta, Primera Angostura, Segunda Angostura, Tapilique, Aguada Cecilio, Colonia Frías, and Bariloche (Ballestero, 2014).



such preliminary sources in the field as well as to formulate new hypotheses and carry out analyses against the historical-comparative approach based only on written sources.

In particular, he referred to the tradition established by the English Jesuit missionary Thomas Falkner (1707-1784) in his work *A Description of Patagonia and the adjoining parts of South America, with a grammar and a short vocabulary, and some particulars relating to Falkland's Islands* (1774). In 1915, during his first fieldwork in the province of Rio Negro, Lehmann-Nitsche pointed out that it “was necessary to consult Falkner’s book in situ. I have a copy, as I will travel next year, for the same purpose, from Carmen de Patagones to Colonia Frias, being able to consult Puelches and Araucanos about many details of Falkner’s book, which were doubtful and important at the same time” (Lehmann-Nitsche, 1922a: 12).

For gathering linguistic data, Lehmann-Nitsche used the 1892 *Handbuch zur Aufnahme fremder Sprachen* (Handbook to Collect Foreign Language Specimens) by the German linguist Hans von der Gabelentz (1840-1893). Primarily intended to be used by colonial officials, missionaries, and individuals dealing with indigenous languages, this handbook included a number of technical and methodological guidelines on how to interact with the informants, how to gather and structure the linguistic data into a series of grammatical categories, and how to analyze them.<sup>19</sup> The use of this handbook by Lehmann-Nitsche corresponded to his methodological and strategic premises. On the one hand, it gave coherence and continuity to the collection process, since Lehmann-Nitsche had previously used this handbook to collect vocabularies and short texts amongst the Selk’nam (1898) and Aónikenk (1903). On the other, it enabled him to order, analyze, and classify information from several sources, which regardless of the collection methods, were rendered into uniform, standard, and comparable data.

During his series of fieldwork periods, Lehmann-Nitsche acquired an important compendium of linguistic elements, which were integrated in and articulated with those obtained in the urban areas of La Plata and Buenos Aires between 1917 and 1927; as well as from glossaries compiled by other scholars and from unpublished manuscripts provided through private correspondence. Based on this synthesis, Lehmann-Nitsche was able to draw a general linguistic map of Patagonia’s indigenous peoples, which he also conceived as a potentially viable instrument used to elucidate ethnical and archaeological subjects.

Studying a group of indigenous women in Tierra del Fuego, Lehmann-Nitsche stressed that their physical similarity made it difficult to determine their ethnological and regional identity based only on anthropometric and morphological characteristics. Nevertheless, using the complementarity of their linguistic differences as a criterion for classification, he concluded that one of them was Yaghan, two were Kawésqar, and four were Selk’nam (Lehmann-Nitsche, 1916a;

**19** The grammatical categories were: pronouns, numbers, nouns, adjectives, adverbs, prepositions and verbs (Gabelentz, 1892).

1916b; 1916c). Similarly, alleging the supposed inherent and structural integrity of the language over time, Lehmann-Nitsche used the collected linguistic data as empirical evidence to address the core issue of the origins, relations, and interethnic correlation of archaeological objects such as engraved axes, labial buttons, and auricular discs<sup>20</sup> (Lehmann-Nitsche, 1909a; 1909b; 1916d; 1916e).

In his general conclusions on Patagonia's linguistic cartography, Lehmann-Nitsche stated that the fact that the ethnic classification of South American indigenous peoples was primarily based on geographical particularities, a tradition he traced back to Falkner's work from the eighteenth century, was one of the problems of South America's anthropology. According to him, the main problem was the artificiality of geographical boundaries, which moreover, presented the increasing tendency to change over time, other than the criterion of language. In this sense, he proposed to use gloss-ethnic<sup>21</sup> classification to bring together the regional languages of Patagonia and Tierra del Fuego under the name "Tshon," an indigenous word meaning "human and people."

Based on this nominal syntagma, he determined geographical/ethnical regions according to specific linguistic particularities,<sup>22</sup> rejecting the linguistic stock proposed by Daniel Brinton and the Canadian anthropologist Alexander Chamberlain (1865-1914). Brinton suggested three linguistic stocks for Patagonia and Tierra del Fuego: "Alikuluf," "Ona," and "Yahgan," which were composed of 12 indigenous dialects.<sup>23</sup> On his part, Chamberlain proposed the inclusion of various indigenous dialects spoken from the province of Rio Negro to the Straits of Magellan in a single linguistic stock he named "Tsonekan or Tehuelchean" (Chamberlain, 1911).

For Lehmann-Nitsche, the classificatory errors committed by Brinton and Chamberlain resulted from the exclusive use of secondary data sources for their work. Lehmann-Nitsche remarked that many of these sources were vocabularies collected by individuals without any academic training and therefore comprised of numerous misconceptions and misunderstandings. In this sense, he drew attention to the work written by armchair scholars while advocating for increased fieldwork (Lehmann-Nitsche, 1913). Finally, Lehmann-Nitsche suggested distinguishing Falkner's "Puelche people" from the outer northwestern Patagonian territories into 4 linguistic groups<sup>24</sup> (Lehmann-Nitsche, 1922a; 1925). This led him to propose the existence of the "Het linguistic group," an indigenous word meaning "people," to which he devoted several publications between 1918 and 1930 (Lehmann-Nitsche, 1918; 1922a; 1925; 1930a).

According to Lehmann-Nitsche, Falkner's work and other colonial manuscripts were sufficient authoritative evidence required to make such a proposal. Decades later, it was found that the vocabulary collected by Falkner, on which Lehmann-Nitsche's proposal relied, could be considered Mapuche or Tehuelche

**20** Lehmann-Nitsche characterized Patagonian archaeological culture as "poor" because of the local degeneration and the incipient level of local art. These series of studies were principally based on data and archaeological material provided by private collectors.

**21** The term "gloss-ethnic" refers to the practice of inferring that all members of an ethnic group share a common set of culture-related characteristics, such as language.

**22** Lehmann-Nitsche divided Patagonia into the following areas: North (Tehuelche), South (Aónikenk), and southwest (Tä'nöshun). Tierra del Fuego was divided according to the traditional way of life of the indigenous into "land people" and "canoeist people". The former were divided into the following areas: Center (Selk'nam) and Southeast (Mánekenk). In the case of the latter, the areas were South (Yaghan) and Southwest (Kawésqar) (Lehmann-Nitsche, 1913; 1921).

**23** The Alikuluf linguistic stock contained the following dialect: Alikulufs and Karaikas. The Ona linguistic stock: Onas, Huemuls, Iress, Oensmen, Pescheress, and Yacanas. The Yahgan linguistic stock: Kennekas, Takanikas, Yahgans, and Yapoos (Brinton, 1891).

**24** The four linguistic groups were: 1) Che language (Moluche, Puelche, and Araucanians people), 2) Künnü language (Tehuelkünnü and Tuelche people), 3) Kün'k language (Tehuelche and Yacana-künnü people), 4) Het language (Chechehet and Divihet people) (Lehmann-Nitsche, 1922).

(Casamiquela, 1956; Escalada, 1949). Although he was wrong to propose a new linguistic group, his publications about the “Het linguistic group” were a significant contribution to the linguistic studies in those days, being based on the geographical and ethnical identification method that in principle was valid.

### **THE CYCLE OF THE MYTHS**

Among the linguistic data he collected, Lehmann-Nitsche found a considerable number of indigenous stories associated with climatic and astronomical phenomena. According to him, the study of these elements would not only allow an understanding of indigenous cosmogony and worldview, but more importantly, a study of their “primitive soul” (Lehmann-Nitsche, 1916f: 28). As the French philosopher Lucien Lévy-Bruhl (1857-1939) pointed out in the early twentieth century, indigenous stories were conceived as a material means in which their “primitive” soul materialized, making it possible to register and accept them as ethnological evidence (Lévy-Bruhl, 1927).

For Lehmann-Nitsche, the development of an ethnographic map of South America would be possible only through the articulation of indigenous linguistic, mythological, and astronomical elements in a single explanatory model, which would identify geographical areas in which indigenous peoples live as well as zones of interaction (Lehmann-Nitsche, 1930b; 1937). This led him to emphasize that until that moment the study of human’s physical characteristics had not yet produced useful scientific results, whereas the study of the “psychic field” proposed by ethnology promised to revert this situation (Lehmann-Nitsche, 1922b: 22).

Following the ideas of Rudolf Martin and the German ethnologist Emil Schmidt (1837-1906), Lehmann-Nitsche defined anthropology as “the physical and psychological study of humankind through comparison” (Lehmann-Nitsche, 1906: 5). He divided this study into two interrelated and complementary fields: physical anthropology, which for him was anthropology per se, and psychic anthropology, which comprised both ethnology and related disciplines such as linguistic and mythology (Lehmann-Nitsche, 1906). He also remarked that at the beginning of the twentieth century the study of South American indigenous mythology had been conducted by persons without professional training, except for the studies by Ehrenreich of South American indigenous myths and legends that provided the first scientific approach to this subject (Lehmann-Nitsche, 1916g).

In 1905, Ehrenreich proposed an integrated model to explain the thematic similarities between American myths and to establish relations with those of Europe. Using a geological analogy, he suggested the existence of an old “layer” of myths and stories originating from the American continent, subsequently

registering an infiltration and dissemination of European isolated mythological elements. Ehrenreich stated that only the convergence of data provided by physical anthropology and ethnology would allow for the reconstruction and study of humankind's historical evolution; and furthermore, to determine patterns of migration and social interaction. Noting the absence of such studies in South America, he urged scholars working in this region to initiate these studies in order to ensure that their work would complement the research carried out in North America<sup>25</sup> (Ehrenreich, 1905).

Lehmann-Nitsche's studies of indigenous mythology and astronomy responded to Ehrenreich's request to determine "mythological cycles" for the region of Gran Chaco and Patagonia in order to develop an inter-American comparative mythology (Lehmann-Nitsche, 1937). For this purpose, he conducted an extensive review of the literature and began exchanging letters with scholars working on similar issues in Chile and Peru.<sup>26</sup> In this way, he was able to gather a large quantity of information on terrestrial phenomena (earthquakes, volcanoes), atmospheric phenomena (rain, thunder), and cosmic phenomena (solar and lunar eclipses).

This information was related to that collected during his fieldwork in northern Patagonia (1915 and 1916) and data provided by his informants in La Plata. According to him, this should have allowed him to verify the information and more importantly, to overcome possible misconceptions resulting from working only in museum cabinets. Lehmann-Nitsche pointed out that his research intended to surpass the "misconceptions born in the cabinet and in the spirit of those people who never worked with representatives of primitive humans" (Lehmann-Nitsche, 1922b: 25). At the same time, following recommendations of Konrad Theodor Preuss, he contrasted the historical information about cosmic phenomena with astronomical records in order to rectify errors resulting from the alleged subjectivity of written sources. In this sense, he asked astronomers for astronomical records of the position of stars at specific dates and instructions on how to calculate these<sup>27</sup> (Ballesterro, 2014).

From these sources, Lehmann-Nitsche addressed themes that, according to him, were preferred by scholars working on comparative mythology, namely eclipses and the universal flood. He speculated that this predilection responded to the influence exerted by the Christian religion on the intellectual world. Although Lehmann-Nitsche respected the religious choices of other scholars, he warned that these should not influence the choice of the studies topics. Regretting such influence, Lehmann-Nitsche remarked, "The predilection of the mythologist for these subjects can be explained, I believe, by the secondary and unconscious effect of the Biblical tradition that dominated and continues to dominate – much longer than is admitted – the scientific world on their conception of the world and the universe" (Lehmann-Nitsche, 1916h: 206).

**25** He specifically referred to Franz Boas' research on the mythology of the indigenous peoples of the Northwest Pacific coast.

**26** For Chile, he entered into correspondence with the English civil engineer and ethnologist Richard E. Latcham (1869-1943) and the Chilean physician Aureliano Oyarzún (1858-1947). For Peru, he contacted the Ecuadorian historian Jacinto Jijón y Caamaño (1890-1950) and the American ethnologist Stansbury Hagar (1869-1942).

**27** He consulted the Argentinean astronomer Félix Aguilar (1884-1943), director of La Plata's Astronomical Observatory, and the American astronomer Charles Dillon Perrine (1867-1951), director of Córdoba's Astronomical Observatory.

Lehmann-Nitsche's selection of subjects was also due to technical reasons. Most stories he collected had these themes as central core, with the exception of only three (about the tiger as a mythical animal, the leadership granted through the magical characteristics of the axe, and a stone giant). At the time of his analysis, Lehmann-Nitsche had difficulty acquiring recent literature, already the norm within Museo de La Plata's research dynamics, but even worse after the First World War.

The Argentinean academic community, especially scientific societies, subjected German scholars residing in Argentina to intimidation and discrimination during and after the Great War of 1914-18. In addition, German scholars had problems acquiring anthropological instruments or recent literature from Europe, due to the precarious social and economic situation in which Europe found itself. In view of this situation, and following the example of Franz Boas in the United States, individual initiatives and assistance committees were organized in Argentina by German scholars in order to compensate the shortages in scholarly material (Ballestero, 2014).

In such a situation, the network constituted by German scholars working on the same issues proved to be a valuable resource, providing Lehmann-Nitsche with the necessary references derived from the literature. Together with the scholars mentioned in footnote 24, those who helped Lehmann-Nitsche were the German archaeologist and ethnologist Max Uhle (1856-1944); the Brazilian historians Basílio de Magalhães (1874-1957) and João Fernandes (1860-1934); the Chilean botanist Gualterio Schalleberg (1898-1982); the German linguist Carl Bezold (1859-1922); and the German ethnologists Walter Lehmann (1878-1939) and Alfred Maass (1863-1946).

Following Ehrenreich's theoretical and methodological proposals, Lehmann-Nitsche proceeded to systematically arrange and compare all versions of mythological explanations of climatic and astronomical phenomena. Based on this comparative summary, he identified characteristic details from which he established similarities and differences. The reduction of the large number of variables to a minimum allowed Lehmann-Nitsche to specify convergence, isolation, and exchange zones (Lehmann-Nitsche, 1916f, 1930b). Far from being a novelty, this methodological approach had earlier been implemented by the German geographer and ethnologist Richard Andree (1835-1912) and the Austrian ethnologist Moriz Winternitz (1863-1937) in order to isolate the essential components of universal myths and elaborate a common explanatory matrix (Ballestero, 2014).

For Lehmann-Nitsche the repetition of these elements provided important evidence in support of the "unity of the human mind," a hypothesis formulated by Adolf Bastian, director of the Royal Museum of Ethnology in Berlin. At the same time, the repetition of these elements was used to suggest that the evolution

of the human mind could be studied by using the empirical, inductive method of the natural sciences, transcribing the subjective and abstract concepts of the immediate experience into measurable, observable and comparable elements.

Convinced that humankind's physical unity had already been established, Bastian focused on establishing the psychic unity of thought because for him the history of humanity was the history of the human mind. The way to study it was not through subjectively written sources, but by examining and comparing material culture from the perspective of geography and history, which he believed would reveal that the same psychological elements circulate "through the heads of all peoples, in all times and places" (Bastian, 1860: 9).

The only reference to this kind of approach for South America was the research by the Austrian priest and ethnologist Wilhelm Schmidt (1868-1954) on the development from monotheism to polytheism<sup>28</sup>. Using Bastian's concept of "elementary ideas", the cultural migration theory by the German geographer Friedrich Ratzel (1844-1904) and the concept of "cultural circles" by the German ethnologist Leo Frobenius (1873-1938), Schmidt proposed a relational analysis of the cultural history of South American indigenous peoples, establishing three "cultural circles" for South America: a circle of nomadic hunter-gatherer peoples, a circle of sedentary peoples, and a circle of high-culture peoples (Schmidt, 1913).

Based on this theoretical-methodological matrix, Lehmann-Nitsche established a series of "scientific analogies between geographically distant peoples," identifying "mythological regions" and suggesting a number of intracultural and intercultural relations between the Old World and the New (Lehmann-Nitsche, 1922b: 50). The identification of common and recurrent elements allowed Lehmann-Nitsche to argue that there was not a common origin of the diluvial myth, but rather various and chronologically different origins. In contrasting these elements with zoological information (in cases in which animals appeared) and with historical information presented in colonial documents, Lehmann-Nitsche was able to geographically and temporally identify those places that could be considered points of dispersion of the myth's different versions. In the specific case of the diluvial myths from Argentina, Brazil, Paraguay, Bolivia, Peru, and Chile, he concluded that they were strongly related, the latter being the confluence and dispersion point of the myth's different versions (Lehmann-Nitsche, 1916f; 1916g; 1916h).

With regard to solar and lunar eclipses, he proposed to define zones according to the gender assigned to the sun and the moon. In this way, the anthropomorphization of the latter into man and woman respectively made it possible to accurately delimit a western region that, across the Andes Mountain range, extended from Ecuador to Tierra del Fuego. At the same time, the anthropomorphization of the sun and the moon into human beings delimited an eastern re-

**28** Schmidt was a key and prominent actor in the further development of the cultural circle concept or "Kulturkreis", which was the central core of the so-called Vienna school of ethnology. Schmidt's main contribution was to incorporate the criteria of cultural stratum; continuity and relatedness, extending the cultural circle concept into a cultural complex that included material culture, economy and religion.

gion that stretches from Brazil to northern Patagonia (Lehmann-Nitsche, 1916g, 1922b, 1937). Finally, Lehmann-Nitsche defined “mythological cycles” for each “mythological region,” which, following the approach of the German psychologist Wilhelm Wundt (1832-1920), allowed him to identify the “ethnic psychology” of each group and establish mutual cultural correspondences with Central Europe and Asia (Lehmann-Nitsche, 1937).

## CONCLUSION

Over the course of two decades, Lehmann-Nitsche employed a range of human, financial, instrumental, and physical resources in order to establish multiple registers of anatomical, ethnological, linguistic, and mythological features of Patagonia’s inhabitants. On the one hand, this variety of topics corresponded to the changing characteristics of the German anthropological tradition. In this sense, Lehmann-Nitsche adapted to these changes in order to use the indigenous inhabitants of Patagonia as “cultural capital” for lubricating patronage networks and build his career. On the other, for him a complete understanding of indigenous peoples was only possible through an integral study that included both physical anthropology and ethnology.

As he explained in a private letter to his family, the publication of these results might not be relevant, but the important task was to contribute to international discussions with data collected from indigenous in Patagonia, something few European scholars could do. Moreover, continuously publishing academic papers was a way for Lehmann-Nitsche to accumulate sufficient academic prestige for him to be able to return to Germany and obtain an academic position there (Ballesterro, 2014).

Lehmann-Nitsche’s studies on Patagonia’s inhabitants took place in an international, political, and academic context, in which, also due to the values that shaped the policies of anthropological practices, the inhabitants of Patagonia were study subjects with a high “scientific value,” a value that increased for epistemological and practical reasons. On the one hand, they were considered to be one of the most “primitive” indigenous peoples of the world and therefore a key subject for studying human history. In turn, their alleged condition of “primitiveness” condemned them to an inexorable passage of time and thereby, an almost certain physical extinction. Therefore, the recording of these living remnants of the past became imperative.<sup>29</sup> On the other, travelling from Europe to Patagonia during the second half of the nineteenth century involved a considerable investment of time and money that not all scholars could or were willing to afford.

As other scholars studying “natural peoples” in Africa, Australia, and South America, Lehmann-Nitsche established and participated in extensive interna-

**29** This widespread anthropological idea, known as the “salvage paradigm,” had, since the second half of the nineteenth century, been associated with the “notion of an ethnographic—indeed a scientific—mission, not to stem the tide of civilization’s advance but to preserve that which was about to be destroyed” (Gruber, 1970: 1294).

tional networks of collection, collaboration, and exchange in order to obtain osteological and ethnographic materials from the inhabitants of Patagonia. These materials were articulated into the broader comparative series that scholars argued would allow them to empirically analyze and understand the long and complex sequences of human history and the development of the human species itself.

In the pursuit of his academic and professional future, Lehmann-Nitsche collected and analyzed skulls, long bones, language samples, music, and mythological beliefs from Patagonian indigenous people. They contained the past that scholars found interesting and that was threatened by the passage of time. Lehmann-Nitsche codified them as indices, cranial measurements, wax cylinders, vocabularies or stories written into his field notebooks, in order to insert them into international anthropological networks. In so doing, he granted the indigenous peoples of Patagonia temporal transcendence and disseminated their culture in different fields of knowledge.

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**Diego Ballesterro** is an Argentinean Anthropologist and Doctor on Natural Sciences by the National University of La Plata (Argentina). He is currently guest lecturer at the Institut für Archäologie und Kulturanthropologie (Bonn University, Germany). His thematic research interests are the history of anthropology, transculturation process in historical perspective and its transcontinental relation, and the construction and representation of ethnic identity in museal context.

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# Encrypted Astronomy, Astral Mythologies, and Ancient Mexican Studies in Austria, 1910-1945

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## Peter Rohrbacher

🏠 *Institute for Social Anthropology, Austrian Academy of Sciences | Vienna, Austria*

✉ [peter.rohrbacher@oeaw.ac.at](mailto:peter.rohrbacher@oeaw.ac.at)

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### ABSTRACT

This paper examines the beginnings of Austrian studies on ancient Mexico by analyzing the work of Damian Kreichgauer and Friedrich R6ck in the early twentieth century. Both argued that a priest elite intentionally “coded” astronomical data in ancient Mexican manuscripts. The first section of the article sheds light on the theoretical background of this interpretation. The main section, based on numerous archival sources, is dedicated to the deciphering procedure elaborated by R6ck, the first director of the Ethnological Museum in Vienna (today *Weltmuseum Wien*). Since R6ck’s method seemed to revolutionize the discipline, it gained a great deal of attention from German Nazi authorities. The final section deals with R6ck’s student Karl Anton Nowotny, who elaborated an ethnographic approach of ancient Mexican studies in Austria after World War II. This study provides new insights into the historical background of post-war ancient Mexican studies in Austria.

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### KEYWORDS

Ancient Mexican Studies, History of Anthropology, Diffusionism, Codex Vindobonensis I, National Socialism.



Ancient Mexican studies has been an academic discipline in Austria since 1925. Albeit without a proper chair, Austrian ancient Mexican studies have been firmly established within the international scientific community with the work of Karl Anton Nowotny (1904-1978) and his student Ferdinand Anders (born 1930).<sup>1</sup> The following article deals with the beginnings of this academic discipline in Austria and seeks to trace the historical foundation on how Ancient Mexican studies became a subdivision of post-war anthropology. Shortly before the First World War, Ancient Mexican studies evolved in Austria with a focus on astronomy and the calendar. Its foundations were laid by Damian Kreichgauer (1859-1940) and Friedrich Röck (1879-1953). Both advocated a diffusionist approach, arguing that Pre-Columbian Mexican manuscripts contain highly complex astronomical knowledge that was “coded” by a priest elite. In 1936, Röck introduced a new method for deciphering ancient Mexican manuscripts, which was widely accepted outside academia by the authorities of the Nazi period. The present article is based on archival documents not previously taken into consideration, as well as documents from an extensive collection of Röck’s unpublished manuscripts.<sup>2</sup>

#### **ANCIENT MEXICAN STUDIES IN THE CONTEXT OF THE ASTRAL MYTH SCHOOLS**

Prior to the beginning of the twentieth century, mythologies had for a long time been ignored as a source genre by prevailing historicism. A change took place when a series of disciplines – led by classical studies, folklore studies, and ethnology – began to use myths, regarded as humanity’s oldest sources, for the study of history (Frazer 1906-1915). In 1906, the German historians Ernst Siecke und Georg Hüsing established a Society for the Comparative Study of Myths in Berlin. This society, with its book series “Mythological Library” (1907-1916) created the institutional framework for an astral interpretation of myths and thus distanced itself from the predominantly philological approach of the time. In Vienna, a group of mythologists formed around Leopold von Schroeder, an Indologist from Latvia, became known as the Viennese School of Mythology and cooperated closely with folklore studies, which was not yet university based (Bockhorn, 1994: 488-495).

The astral myth interpretation, which can be traced back to the eighteenth century,<sup>3</sup> was based on the idea that all mythology around the world was exclusively concerned with processes in the sky and the movements of the stars, especially with the movements of the sun, the moon, and Venus in relation to the twelve constellations of the zodiac. In this doctrine, the numbers of orbital periods of the planets and their relations to each other played a special role and they were seen as the origin for time divisions and calendar systems.

Against this theoretical background, the Catholic missionary Damian

**1** Nowotny (1961); Anders; Jansen; García (1992); Boone (2007: 161-162). This article is the result of an extensive study of Friedrich Röck and other scholars in the period of Nazi domination. A separate version will be published as “Verschollene Kulturzusammenhänge: Der Altorientalist und Altamerikanist Friedrich Röck und seine Stellung in der NS-Zeit”. In: Gingrich, Andre and Rohrbacher, Peter (eds.) *Völkerkunde zur NS-Zeit aus Wien (1938-1945): Institutionen, Praktiken und Biographie-zentrierte Netzwerke*. Wien: Österreichische Akademie der Wissenschaften (forthcoming). This study was supported by the Faculty of Social Sciences at the University of Vienna. For suggestions to improve this article I thank Han F. Vermeulen.

**2** Röck’s private papers and handwritten manuscripts are housed in more than boxes and are preserved at Vienna University Archive (henceforth abbreviated as VUA), partially evaluated by Peter Linimayr (Linimayr, 1993/2: Q33-Q157; Linimayr, 1994: 87-116).

**3** Buchwald and Josefowicz (2010: 28-31; 47-69).

Kreichgauer gave a celebrated lecture, “Les Mythologies et les calendriers de l’ancien Mexique”, at Louvain (Belgium) in September 1913 during the “Religious-Ethnological Weeks” organized by Pater Wilhelm Schmidt. His contribution to the conference also formed the prelude to the academic profession of ancient Mexican studies in Austria (Kreichgauer, 1914: 315-333). As a trained physicist, Kreichgauer had first been a research assistant to Hermann von Helmholtz in Berlin-Charlottenburg, but in 1892 joined the Steyler Missionaries and moved to Austria (Kraus, 1962: 31). As a missionary priest, he combined his extraordinary mathematical knowledge with cultural studies and applied these to the pre-Columbian cultures of America. Working as an autodidact in this field, Kreichgauer based his work on that of Eduard Seler, the German founder of ancient American studies in Berlin. Seler’s pioneering astronomical interpretations of the Codex Borgia (Seler 1904-1909) formed the starting point for Kreichgauer’s investigations of the Codex Zouche-Nuttall. In 1916 Kreichgauer concluded that this ancient Mexican picture manuscript contained highly complex astronomical knowledge, which had intentionally been encrypted by a priest elite. Unlike Seler, Kreichgauer believed that mythological images served the sole purpose of distracting from the actual astronomical data in order to protect it from misuse by outsiders (Kreichgauer, 1915-1916: 1). For example, Kreichgauer interpreted images as disguised arithmetic operations, such as a “spider web” standing for “subtraction” (Ibid.: 11-12). He argued that Seler had ignored this “skill in secrecy” (Ibid.: 2) and Kreichgauer continued his research by studying the Codex Vindobonen-

**Figure 1**  
Kreichgauer interpreted images of the Codex Zouche-Nuttall as disguised arithmetic operations, such as a “spider web” (Spinnennetz) standing for “subtraction” (Kreichgauer 1915-1916: 11)

Die Astronomie des Kodex Nuttall.

11

Zeichen für die Subtraktion fehlt. Wir erhalten also in zweiter Annäherung  $72^{\circ} 113^{\circ} + 9^{\circ} 80^{\circ} = 81$  Jahre 193 Tage. In der Reihe treten aber noch zwei weitere Korrekturen auf. Zunächst finden wir hier zum zweitenmal eine Anzahl einzeln stehender, unbeschäftigter Götter. Es sind die sieben Personen auf Seite 23 links oben über dem Spinnennetz, und die zwei unten in der Mitte befindlichen Gestalten, zwischen denen ein zweites Spinnennetz gezeichnet ist. **Das Spinnennetz schreibt die Subtraktion vor.** Dazu kommt eine ganz neue Korrektur, die wir aber später noch zweimal antreffen werden, nämlich 20 Punkte um einen Jaguarkopf auf Seite 24. Überall, wo diese letztere Korrektur vorkommt, muß sie subtrahiert werden. Wir kommen somit schließlich zu dem Resultate 81 Jahre 164 Tage, genau wie in Reihe A.



Abb. 6.

Reihe D.

Ohne deutliches Kennzeichen beginnt am Ende der Seite 6 eine dritte Reihe über die mittlere Dauer von 50 Venusperioden. Das normale Ausgangsdatum fehlt; wenn man es ergänzt, ist sie zusammengesetzt wie folgt:

a	1. Jahr	209. Tag	[ 1 XIII, 1 I]
b	43. Jahr	37. Tag	[ 4 III, 1 XIX]
c	62. Jahr	278. Tag	[10 XVIII, 1 XV]

Das rohe Resultat beträgt 61 Jahre 69 Tage. Daran sind zwei Arten von Korrekturen anzubringen, von denen die eine schon in den Reihen C und L aufgetreten ist. Hinter den auf Seite 6 zur Reihe C gehörigen einzelnen Götterfiguren, die alle nach rechts gewendet sind, beginnt nämlich eine neue Gruppe von ganz entsprechenden zwölf Einzelgöttern, aber mit der Blickrichtung nach links. Das Spinnennetz zeigt an, daß die Anzahl dieser Figuren vom rohen Resultat abgezogen werden muß; diese Operation führt uns zu der Zahl  $61^{\circ} 57^{\circ}$ . Die zweite Korrektur ist neu; aber auch sie wird, wie alle die vielen anderen Arten im Laufe der Untersuchung wieder auftreten. Auf Seite 8 sieht man einen sorgfältig gezeichneten Tempel des zunehmenden Mondes mitten im Texte und ohne Beziehung zu seiner Nachbarschaft (Abb. 7). Ein solcher hat als Korrektionsglied den Wert von 30 Tagen (Dauer des synodischen Monates). Auf Seite 7 links oben steht aber ein Spinnennetz mit einer Hand, die auf den Tempel hinzeigt. Da wir schon aus mehreren Fällen das Netz als Zeichen der Subtraktion kennen, so ziehen wir 30 Tage von  $61^{\circ} 57^{\circ}$  ab und gelangen zum Schlußresultat 61 Jahre 27 Tage.

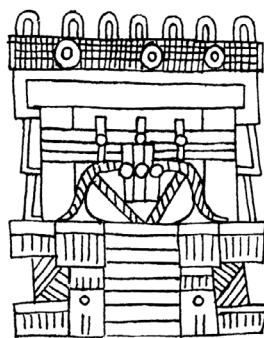


Abb. 7.

Um den Zusammenhang mit den beiden vorigen Reihen mehr hervorzuheben, verwandeln wir die Tage in Bruchteile des Jahres und finden für A und M  $81 \cdot 45$  Jahre, und hier  $61 \cdot 07$  Jahre. Es ist aber auch  $81 \cdot 45 \times \frac{3}{4} = 61 \cdot 09$ .

sis I. His studies reaffirmed that “astronomer priests”, “had written all necessary data into the interstices of a series of mythological images” (Kreichgauer, 1917: 7). Kreichgauer published his results in 1917 at the Imperial Academy of Sciences, yet the work, while being well promoted, was ignored for the next decade.

#### **PAN-BABYLONISM, ELAMISM AND THEOSOPHY**

The followers of “pan-Babylonism”<sup>4</sup> were an important subgroup of astral mythologists that regarded ancient Babylon as the culture-historical starting point of the astral system. According to them, this system was fully developed by around 3,000 BC and first spread across the ancient Orient, then finally around the entire globe. Pan-Babylonists argued that religion comprised specialized astronomical secrets passed on to the “common people” in the form of myths. This theory was mainly based on the astronomical knowledge of the precession of the Earth’s axis with its long cycle of about 26,000 years (“Platonic Year”). The history of astronomy teaches, however, that the Greek mathematician Hipparchos of Nicaea discovered precession in the second century BC and thus this phenomenon could not have been known in ancient Babylon (Hoskin, 1999: 18-47). For this reason, pan-Babylonists were rightly exposed to heavy criticism from the beginning.<sup>5</sup>

From an early age, Friedrich Röck was in close contact with the pan-Babylonists. Born on July 14, 1879, in Imst, Röck came from an educated middle-class family in Tyrol.<sup>6</sup> Nevertheless, in June 1904 he graduated late at the age of 25 from the Austrian Gymnasium Brixen. Subsequently, Röck studied Babylonian-Assyrian and Egyptian philology at the University of Innsbruck, as well as the history and cultural history of the ancient Near East. Röck’s most important academic teacher was Thomas Friedrich, who had taken over the chair in Ancient Oriental Antiquities and History of the Ancient Near East at Innsbruck in 1908.<sup>7</sup> Friedrich also supervised Röck’s dissertation, which culminated in the writing of “Studies on Babylonian-Assyrian Mythology”, with which he graduated from the University of Innsbruck in March 1911 (Röck, 1911).<sup>8</sup>

However, Röck’s research interests were less influenced by the Assyriologist environment of the University of Innsbruck, than by astral-mythological schools of thought and pseudo-scientific theosophical circles. As a student, for example, he was looking for evidence to support the pan-Babylonist theory in ancient India, and in an article published in 1910 he argued that, “the entire Indian Yuga system” [...] was built on the ‘Platonic number’” (Röck, 1910: 324). After graduation, Röck came under the influence of the “Elamists.” Like Röck, they were *völkisch*-minded<sup>9</sup> Assyriologists who attributed the emergence of the first written culture to ethnic “Aryans.” Accordingly, they ascribed the origins of astral

**4** On the history of “pan-Babylonism”, see Weichenhan 2016: 19-72.

**5** One of the best-known opponents of pan-Babylonism was Franz Xaver Kugler SJ, a German Assyriologist and astronomy historian, whose arguments were adopted by Father Wilhelm Schmidt and Leopold von Schroeder (Kugler, 1907: 38-50; Schmidt, 1908: 77).

**6** Kofler (1975/I: 3) (Karl Röck, CV of our father Hermann Röck, Oct 1, 1950).

**7** See <https://www.uibk.ac.at/alte-geschichte-orient/institutsgeschichte/kurzgeschichte-altorientalistik.html> (accessed Oct 12, 2017).

**8** According to the Archive of the University of Innsbruck, Röck’s unpublished dissertation is lost.

**9** The term *völkisch* has no direct English equivalent, but it could be rendered as hyper-nationalist sentiment allied with anti-Semitism.

theory not to ancient Babylon but to the even older Elam Empire, whose capital, Susa, is in present-day Iran. Among the most influential representatives among them were Ferdinand Bork and Georg Hüsing, who knew each other from their student days in Königsberg (now Kaliningrad).<sup>10</sup>

Bork was able to prove the rotation period of Venus was 225 days in the ancient Mexican calendar in 1910 (Bork, 1910: 83-105). According to a diffusionist approach, the origin of this knowledge was to be found in the Old World. "It's up to research," Bork stated, "to find the paths that lead from Elam to Mexico" (Bork, 1910: 102). With his pan-Elamite research, Ferdinand Bork opened a field of research to which Röck dedicated his entire life.

In an unpublished manuscript from 1912, Röck stated: "The evidence that Elamite cultural influence extended across almost all the world in prehistoric times is compelling."<sup>11</sup> He showed that the ancient zodiac was not a Babylonian invention, as had previously been assumed, but much older, going back to ancient times. Röck expounded that the twelve-part zodiac was derived from an older zodiac system that distinguished eight constellations corresponding to the Elamite "system of eight" described by the *völkisch* mythologist Wolfgang Schultz.<sup>12</sup> Just like the aforementioned pan-Elamists, Röck believed that "a significant portion of ancient Elam's culture can be traced back to Aryan tribes."<sup>13</sup>

Röck's younger brother Karl<sup>14</sup>, wrote extensive diaries, which are stored at the Research Institute Brenner-Archive in Innsbruck.<sup>15</sup> These reveal a hitherto unknown side of the future museum director: According to his brother, Friedrich Röck was a devout theosophist, i.e., a supporter of mystical and occultist philosophy. For example, on December 24, 1919, Karl Röck noted: "Fritz is an active member [of the Theosophical Society]"<sup>16</sup> and on December 11, 1920: "In the evening [I attended] the lecture of my brother Fritz at the Theosophical Society of Agrippa von Nettesheim on numerical symbolism."<sup>17</sup> The Friedrich Röck papers contain a number of manuscript excerpts from works by Helena Petrovna Blavatsky and Annie Besant.<sup>18</sup> These fragments show that Röck probably adopted the theory of the "Aryan zodiac," with its "immeasurably" old age, from Blavatsky's "Secret Doctrine," the most important work of nineteenth-century Western theosophy (Blavatsky, 1899/I: 713).<sup>19</sup>

#### THE INSTITUTIONALIZATION OF ANCIENT MEXICAN STUDIES IN VIENNA

In November 1920, Röck became a research assistant at the Ethnographic Department of the Natural History Museum in Vienna. The head of this department was Viktor Christian, who liked the fact that Röck was a trained Assyriologist. However, Röck was assigned to the Americas section in this department and from that moment on he increasingly focused his studies on Mexico. During the next

**10** Ferdinand Bork was a schoolteacher at the Löbenicht Realgymnasium in Königsberg. Not affiliated to any university, he developed into the leading Elamist until the post-war period (Weidner, 1963: 304).

**11** VUA, FRP, 131.104.21; Röck, Jackele does not want to shake pears, pears do not want to fall, 1912: 1-16, here 15, unpublished manuscript.

**12** Ibid., here 15; Röck (1913: 147-176); see Schultz (1910: 101-150) and Bork (1914: 66).

**13** Ibid., here 16.

**14** Karl Röck was an employee of the literary magazine Brenner and was one of Georg Trakl's closest friends. In 1925 after Trakl's death, he published the first complete edition of Trakl's poems.

**15** For this note I thank Ildikó Cazan, Vienna. Karl Röck's diaries are already published (Kofler 1975/I-III).

**16** Kofler (1975/I: 317) Karl Röck, diary entry, Dec 24, 1919.

**17** Kofler (1975/I: 334) Karl Röck, diary entry, Nov 4, 1920; see also Röck (1915); Nowotny 1967).

**18** VUA, FRP, 131.104.21; excerpts to Helena P. Blavatsky "The Secret Doctrine" (Engl. orig. 1877) and Annie Besant "The Seven Principles of Man" (Engl. orig. 1892).

**19** Theosophists believe that Aryans descended from the sunken continent Atlantis.



**Figure 2**

Director Friedrich Röck (center) with employees of the Ethnological Museum Vienna, 1930 (Private Archive Bettina Hainschink, Vienna)

decade, Röck was able to build a considerable academic career. In March 1925 he received permission from the University of Vienna to teach the subject “Ethnology with Special Attention to American Linguistics and Antiquity”. When Christian moved to the University of Vienna, Röck took over leadership of the Ethnographic Department at the Natural History Museum in December 1925. In this capacity, he supervised the relocation of the ethnographic collections to the Neue Burg Palace in 1927 and the following years. He became the first director of the Ethnological Museum in Vienna in 1928, a position he held until his discharge in 1945. In June 1931, Röck was appointed associate professor at the University of Vienna.<sup>20</sup>

Röck’s professional transformation from Assyriologist to ancient Mexicanist was remarkable. He could not claim to have had a relevant education, but in one of his CVs stated that he in 1907 had “privately” studied “Ethnography and American studies.”<sup>21</sup> Like Kreichgauer, Röck was largely self-taught. His first step into serious ancient Mexican studies took place in 1917 when Röck set up a “Toltec star map” identifying “twenty-eight Toltec lunar houses.” He interpreted them as an “ancient Mexican calendar scripture.”<sup>22</sup> On the one hand, the result was based on the professional calculations of the Viennese astronomer Friedrich Karl Ginzler, whose star map showed “lunar stations around 4000 BC” for the northern hemisphere.<sup>23</sup> On the other hand, however, Röck had merely followed the unproven thesis that the German mythologist Eduard Stucken had presented

**20** VUA, PH PA 3.052.

**21** VUA, S 304.1056; Röck’s extensive student notebook for the academic year 1906/07 does not yet contain any entries to America (PA Anders; Röck, student notebook, 1906/07, 264 pages).

**22** Röck designed this star map on Apr 25, 1917 (Röck, 1922: 134).

**23** Cf. the star map in Ginzler (1906/I: appendix), Röck (1922: 134), and Röck (1925/I: 147). For this note, I thank Ferdinand Anders, Klosterneuburg. See also Steinle (1995: 133); Luksch (2015: 119-120).



in his book “The Origin of the Alphabet and the Lunar Stations” (Stucken, 1913). Like Stucken and many other astral mythologists, Röck believed the “origins of Scripture” to be “in the sky” (Röck, 1919-1920: 1095).

In 1922, Röck argued in his habilitation thesis that elements of the Venus calendar as well as scripture in ancient Mexico “have their exact counterparts in the Old World” (Röck, 1922: 132). As evidence, he offered “traces of an old Venus calculation” on the basis of the Elamite “octagonal system,” which he in the available codices interpreted as “octagram,” “octagonal *fickfack*,” and “octagonal radial star.”<sup>24</sup> Röck summarized the main results of his habilitation thesis as follows: “I therefore believe that the calendar systems, especially the Venus calendar, the entire belief in stars [...] of the ancient Mexicans are remnants of lost cultural relations with the Old World” (Röck, 1922: 132).

When Röck submitted his habilitation to the University of Vienna on March 8, 1923, the College of Professors at the Faculty of Philosophy had difficulty in its assessment; the first session was fruitless.<sup>25</sup> Subsequently, an expert opinion was requested from Walter Lehmann, Director of the Ethnological Research and Education Institute of the Ethnological Museum of Berlin. He was Eduard Seler’s most important student and gained his habilitation on the subject “American Language, Folklore and Antiquity Studies” in 1915. Lehmann’s judgment was devastating. In a letter to the dean he stated that Röck advocated views “for example, about a planet order [...] with which neither my teacher Seler nor I can agree.”<sup>26</sup> Lehmann’s extremely unfavorable report was read on February 8, 1924 in the second commission meeting; however, it was ignored. “For personal reasons [I support Röck],” countered art historian Josef Strzygowski, an extreme German nationalist. Thus, common agreement was reached among the College of Professors<sup>27</sup> and Röck’s application for the “*Venia legendi*” was accepted with five votes in favor and two abstentions.<sup>28</sup> The protocol of the habilitation committee shows clearly that Röck, right from the start, enjoyed strong support from the Faculty of Philosophy of the University of Vienna, which was based less on professional competence, than on German-national networks.

### **RÖCK’S “DECIPHERING PROCEDURE” IN THE CONTEXT OF NATIONAL SOCIALIST SCIENCE POLICY**

Röck’s preoccupation with ancient Mexican manuscripts received a new impetus when, in 1929, the Austrian National Library published the first complete facsimile edition of the famous *Codex Vindobonensis I*. However, not Röck but the aforementioned Walter Lehmann wrote the “technical introduction” (Lehmann, 1929: 13-42). Lehmann could not accept the evidence that Kreichgauer had presented for his astronomical “camouflage theory.” In his introductory commentary,

**24** Ibid.; see also Röck (1919-1920: 1092).

**25** VUA, PH PA 3.052, folio (f.) 17; protocol of the College of Professors, Jan 24, 1924. See also the evaluation of Ranzmaier (2013: 238-239).

**26** VUA, PH PA 3.052, f. 21; Lehmann, Feb 1, 1924, to Dean Franz Eduard Suess.

**27** VUA, PH PA 3.052, f. 20; protocol of the College of Professors, Feb 8, 1924.

**28** VUA, PH PA 3.052, f. 11; Schmidt, Feb 8, 1924, commission report to the dean; Paul Kretschmer abstained because Lehmann’s letter contained no judgment on Röck’s linguistic works (Ranzmaier, 2013: 238).

Lehmann noted that Kreichgauer's views were "not based on fixed facts," but merely "assumptions" (Ibid.: 19). Despite intensive studies, Lehmann admitted that he too, could not "comment exhaustively" on the "Viennese manuscript" (Ibid.). At this point, Röck began his ambitious seven-year engagement in order to discover the unknown meaning of the "Old Indian Picture Book," as he initially titled it (Röck, 1935: 193).

The deciphering technique developed by Röck was built on the work of Kreichgauer. However, Röck focused on the images and after a while he found "hidden ratios" in them that Kreichgauer had not seen. Using the rules of camouflage Röck identified "number pictures," as well as arithmetical operations of astonishingly precise astronomical calendar data (Ibid.: 199). He concluded that ancient Mexican astronomer priests had already been able to correctly calculate the orbital periods of the planets Venus, Mars, Mercury, Jupiter and Saturn on three and four decimal places. Consequently, Röck made an impassioned case for the recognition of the "Viennese Picture Manuscript" as a masterfully coded scientific book (Ibid.: 198).

In February 1936, Röck presented his results at the Institute of Ethnology at the University of Vienna. The title of his lecture was "Cipher Art and Astronomy in the Great Viennese Picture Manuscript from Mexico" (Röck, 1936a: [8]-[10]). At this ethnological session, Röck might have been quite satisfied with the feedback. He then contacted the Viennese press. On February 23, 1936, the Sunday edition of the *Neues Wiener Journal* carried a column entitled: "Mystery of Mexican Pictorial Art Solved" (Anonymous, 1936: 11). Some weeks later, Röck published a similar article in the weekend edition of the *Neue Wiener Tagblatt* under his own name (Röck, 1936b: 7-8). Due to these sensational articles, the "Union of Correspondence from



Figure 2 Director Friedrich Röck (center) with employees of the Ethnological Museum Vienna, 1930 (Private Archive Bettina Hainschink, Vienna)

the Foreign Press” became aware of the Viennese museum director. In June 1936, Röck was invited to the Ambassador Hotel in Vienna in order to present his scientific discoveries.<sup>29</sup> Among the audience was a consul from the German embassy in Vienna. His name is not mentioned in the archival documents, but it was he who invited Röck to provide a proposal of his research results.<sup>30</sup>

Röck applied to the consul to publish his “deciphering findings and explanatory notes to the *Codex Vindobonensis mexicanus* 1 of approximately 1,200 printed pages in folio format” and to finance a “Research Institute for American Language and Antiquity” under his leadership in Vienna.<sup>31</sup> Although Röck did not provide details of the costs, his application was discussed with Franz von Papen, the German ambassador in Vienna. In this manner, contact was established with the Foreign Office in Berlin. The German Legation in Vienna saw the potential for a “great propaganda effect” in the publication project, which should be used for the foreign policy “interests of the Reich.”<sup>32</sup>

However, the German SS research facility *Ahnenerbe* (Ancestral Heritage) intervened immediately. Created by Heinrich Himmler in July 1935, it also set up a department for the “examination of literature”. Its scientific director, Walter Wüst, read Röck’s article “Unraveling the Mexican Picture Manuscript” in the German periodical *Neue Woche* of 1936.<sup>33</sup> However, Wüst did not initiate a review procedure and instead expressed his personal disapproval.<sup>34</sup>

The German Legation ignored Wüst’s negative statement and urged the Ibero-American Institute in Berlin (IAI) to accept this “cultural-political issue.” As a non-university research institution, the IAI, led by Wilhelm Faupel, was involved in Nazi foreign policy and maintained close diplomatic relations with Spain, Mexico, and Argentina (Gliech, 2003: 17-66). At the end of August 1936, Faupel suggested to the Reich Education Ministry (REM)<sup>35</sup> that he would “release” the aforementioned publication, “if the costs of printing could be provided.”<sup>36</sup> As an interim measure, the German Research Foundation (GRF) initiated a review procedure. The first request for an expert review was made to Georg Christian Thilenius, the long-standing director of the Ethnological Museum in Hamburg.<sup>37</sup> In contrast to that of Wüst, his opinion was extremely positive.<sup>38</sup>

Surprisingly, the GRF could find no other experts willing to submit reviews despite repeated requests by the Reich Education Ministry. This was all the more significant, as Thilenius was not a specialist in ancient Mexican studies, but rather an expert on Oceania. The whole control procedure seemed to stall. Even when the Ibero-American Institute in Berlin invited Röck to give a lecture at the University of Berlin’s Romance Seminary in May 1937,<sup>39</sup> which was attended by the most renowned scholars of ancient America in Germany, nothing changed (Röck, 1937: 356). Only when Max Uhle, “old master of American studies,” intervened did Röck’s German colleagues finally agree to take a stand.

**29** BAB, R73/14017, f. 45-48; Röck, Jun 3, 1936, lecture at the Ambassador Hotel, entitled “The Ancient Mexican Picture Manuscript of the Vienna National Library”.

**30** BAB, R73/14017, f. 42-44; Röck, Jun 18, 1936, proposal (exposé) to the consul.

**31** Ibid.

**32** BAB, R73/14017, f. 41; Heinz, Jun 27, 1936, to the Foreign Office in Berlin. Hans Rüdiger von Heinz was a German embassy counselor in Vienna from 1934 to 1938 under von Papen.

**33** BAB, NS21/2116, fs. 756-758; Friedrich Röck, Die altmexikanische Bilderhandschrift der Wiener Nationalbibliothek. *Neue Woche*, 25, p. 30 (Jun 17, 1936) (Röck, 1936c).

**34** BAB, NS21/2116 Röck, f. 750; Wüst, Jul 1, 1936, handwritten note.

**35** Officially called “German Ministry of Science, Education and Popular Education”.

**36** BAB, R73/14017, f. 40; Faupel, Aug 21, 1936, to the REM.

**37** BAB, R73/14017, f. 38; GRF, Oct 23, 1936, to Thilenius.

**38** BAB, R73/14017, fs. 35-36; Thilenius, Nov 2, 1936, to the GRF.

**39** Officially called “German Ministry of Science, Education and Popular Education”.



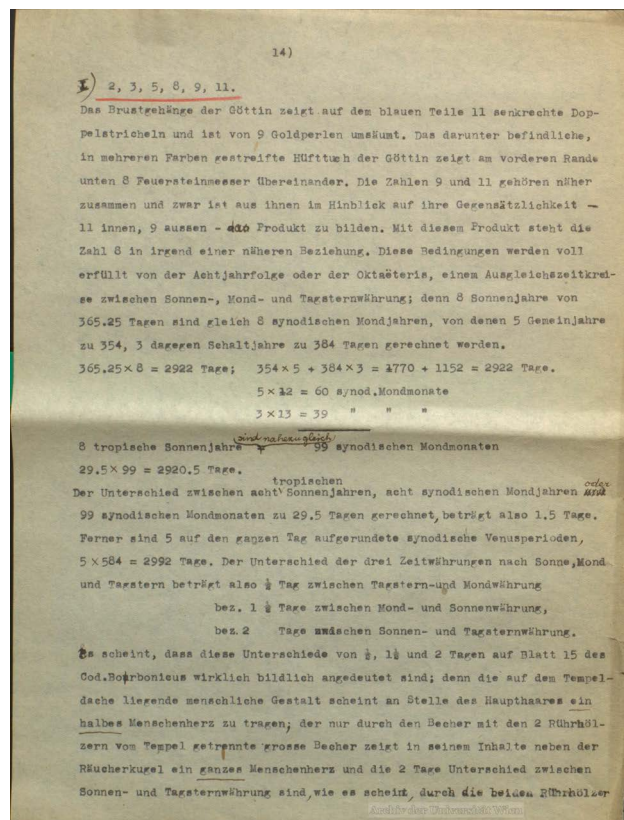
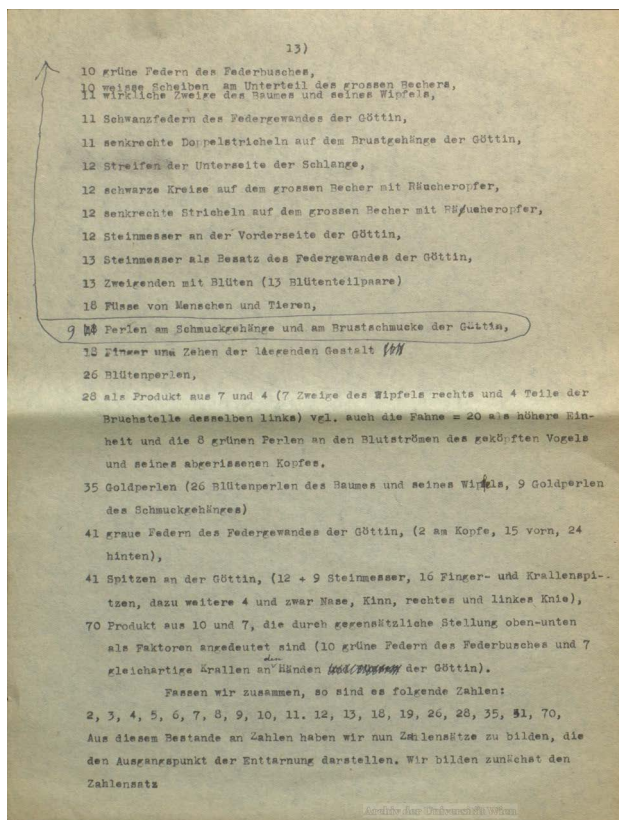


Figure 4

Details from Rökk's manuscript for his lecture in Berlin, May 31, 1937, demonstrating his deciphering procedure to count material items (VUA, FRP, 131.104.21)

On October 18, 1937, Uhle presented all reviews he had managed to gather to the German Research Foundation, partly compiled by phone calls.<sup>40</sup> As all reviewers had reported doubts and concerns, their judgments were devastating. Konrad Theodor Preuss, who had led the North and Central America Department of the Ethnological Museum of Berlin since 1920 and had also attended Rökk's lecture in Berlin, commented on his decoding technique in the following way: "The manner of his research did not immediately make sense to many listeners including myself. In particular, the argumentation of his number series, of seemingly irrelevant details, in the pictures often appears arbitrary."<sup>41</sup> Even Paul Schellhas, a Berlin lawyer and Maya expert who had spent decades deciphering the Mayan hieroglyphs, could not agree with Rökk's findings: "On the contrary, I think that they are very doubtful and sometimes downright imaginary."<sup>42</sup> Schellhas regarded Rökk's assumption that "priests" had "disguised" their astronomical knowledge in the manuscript as "completely arbitrary and in no way proven."<sup>43</sup> He convincingly argued that "camouflage" was "superfluous" as at that time there were "hardly any copies of such precious manuscripts."<sup>44</sup> His judgment had weight as it was published in the prestigious *Zeitschrift für Ethnologie* (Ethnological Journal) shortly thereafter in the context of a serious historical essay on Mayan research (Schellhas, 1937: 382). Negative statements of a similar kind were also given by the German ancient Americanists F.W. Hans Ludendorff, Walter Lehmann,

40 BAB, R73/14017, f. 24; IAI, Oct 18, 1937, to the GRF.

41 BAB, R73/14017, f. 24; IAI, Oct 18, 1937, to the GRF.

42 BAB, R73/14017, f. 24; IAI, Oct 18, 1937, to the GRF.

43 Ibid.

44 Ibid.

Robert Lehmann-Nitsche, and Max Uhle. There is no evidence of Nazi ideological influence in any of these reviews.

Röck's reputation as an ancient Mexicanist was already badly damaged, even before the incorporation of Austria into Nazi Germany on March 12, 1938, known as the *Anschluss*. In order to salvage his scientific reputation, he appointed doctoral students to verify his deciphering procedure. All of them were members of the Nazi Party, which prior to 1933 had been banned in Austria. Röck himself was ideologically close to the Nazi Party, he even tried to join, but he never officially became a member. According to Johann Leopold Ehm and Adolf Anton Plügel, Röck's indoctrination of his students was successful.<sup>45</sup> However, his strategy ultimately failed with Karl Anton Nowotny, a postal official from Hollabrunn, who studied anthropology at the University of Vienna since the beginning of 1935 and devoted himself to "ancient American cultural problems."<sup>46</sup>

#### **NOWOTNY DISTANCED HIMSELF FROM RÖCK'S "DECIPHERING PROCEDURE", 1939**

Röck forced Nowotny to write a commentary on the Codex Laud as a dissertation topic. However, Nowotny distanced himself from Röck's method of deciphering, as the following passage shows: "It is therefore necessary to point out once again that the following, second part of the work is most strongly committed to Prof. F. Röck and that it is based on the methods of Prof. F. Röck, not those of the author" (Nowotny, 1939a: 166).

Nowotny was the one who clarified Seler's astronomical speculations during the post-war period (Nowotny, 1961: 122). He drew an ethnographic analogy with modern ritual practices in Guerrero in Mexico in order to interpret the codex pages as protocols for rituals (Boone, 2007: 161-162) and showed that the religious codices had no occult astronomical content. However, Nowotny never carried out ethnographic fieldwork in Mexico, but his mature work played "a central role in elaborating a new paradigm" in ancient Mexican studies (Jansen, 2017: 443; cf. Everett; Sisson, 2005: xix-xxi).

In his report on Nowotny's dissertation, Röck ignored the criticism of his student and emphasized that the candidate had carried out the description and explanation of the image section of the Codex Laud "as well as the deciphering of its astronomical content on the basis of the deciphering procedure [...] set up by the supervisor" done "with scientific meticulousness."<sup>47</sup> Nowotny would probably not have agreed with this verdict. Röck used his comments on Nowotny's dissertation as an opportunity to justify his approach in the face of his German colleague's attacks.

Against this background, it is relevant to explain Nowotny's position on National Socialism. Research on his early biography has received little attention

**45** See Ehm (1938); VUA, PH RA 13.922; Röck, dissertation report, Jun 17, 1938; see Plügel (1939); VUA, PH RA 14.992; Röck, dissertation report, Jun 6, 1939.

**46** NARA II/T580/A3359; NS Personalfragebogen, K. A. Nowotny, Traun, May 21, 1938. I thank Andre Gingrich who has made available this entire archival record.

**47** VUA, PH RA 14.855; Röck, dissertation report, Apr 11, 1939.

so far (Gohm-Lezuo, 2014: 84; Jansen, 2017: 443) but archival documents clearly show that Nowotny was a radical supporter of Nazi ideology. He joined the Nazi Party in 1932 and lost his job in 1934 after the ban on National Socialism in Austria.<sup>48</sup> He mentioned his “participation in a demonstration” as well as “a series of anonymous ads for Nazi occupation at the post office in Stockerau [Lower Austria]” as a reason for this.<sup>49</sup> In June 1935 he interrupted his university studies and fled to Berlin. Initially, he received “temporary” support from the Nazi Refugee Relief Organization and later, according to him, was active “in General Göring’s regiment.”<sup>50</sup> The exact time of his return to Vienna is unknown. After the *Anschluss*, he acted as the “chief executive officer” at Hollabrunn.<sup>51</sup> Nowotny carried out several months of research for the German Ancestral Heritage (SS *Ahnenerbe*) and received a monthly grant of 140 Reichsmark from November 1938 until March 1939.<sup>52</sup> He published his findings in the journal *Germanien*, which was also edited by the SS *Ahnenerbe* (Nowotny, 1939b: 218-225). At the same time, he contributed the article “Myth or Magic in Germanic Antiquity?” to the *Nationalsozialistische Monatshefte*, a political Nazi magazine published by the *Amt Rosenberg*.<sup>53</sup> This article concluded with a favorable quote from one of Adolf Hitler’s key speeches (Nowotny, 1939c: 248).

After graduating from the University of Vienna in April 1939, Nowotny used his connections in the Nazi state in order to gain a respectable career. At the outbreak of World War II he moved to Krakow, capital of the General Government (now Poland).<sup>54</sup> There, he held a leading position in the Department of Science and Education for Museums.<sup>55</sup> After the war ended, Nowotny was incarcerated and he subsequently returned to Vienna in 1946. With a letter of discharge from Jan Tadeusz Kowalski, secretary of the Polish Academy of Sciences, Nowotny did not have to undergo “denazification”<sup>56</sup> and in 1947 he was given a job as a research assistant at the Ethnological Museum in Vienna (Gohm-Lezuo, 2014: 84-85).

#### **ANCIENT MEXICAN STUDIES AND THE “CULTURE-COMPARATIVE EXHIBITION” OF 1942**

After the *Anschluss*, despite his attempt to join the Nazi Party, Röck was subjected to fierce attacks as the director of the Ethnological Museum. The Nazi cultural authority Reich Government III intended to replace Röck with the Austrian anthropologist Hugo Adolf Bernatzik.<sup>57</sup> However, Röck successfully defended his post and was supported by the personal network he had built long before the *Anschluss*. His newly strengthened position enabled him to organize a major exhibition in order to demonstrate his deciphering procedure to the public.

On March 2, 1942, the “culture-comparative exhibition,” curated by Röck, opened at the Vienna Ethnological Museum in the presence of representatives of the Nazi Party, the German state, and the army (*Wehrmacht*). Röck had in-

**48** Elsewhere Nowotny gave the date of his entry as “December 1933” (see NARA II/T580/A3359; NS Personalfragebogen, Nowotny, May 21, 1938).

**49** VUA, PH RA 14.855; Karl Anton Nowotny, CV, no date [1939].

**50** NARA II/T580/A3359; NS Personalfragebogen, K. A. Nowotny, Traun, May 21, 1938. Nowotny’s task within this regiment cannot be precisely determined due to illegibility.

**51** VUA, PH RA 14.855; Nowotny, CV, no date [1939].

**52** BAB, NS21/1984, f. 648; Wüst, Oct 21, 1938, to Nowotny, Hollabrunn. Nowotny contributed the topic “The Birch” for the SS research project “Forest and tree in the Aryan-Germanic history of ideas and culture”.

**53** This was an official body for cultural policy and surveillance within the Nazi party, headed by Alfred Rosenberg (see Bollmus, 1970).

**54** The General Government was a German zone of occupation established after the joint invasion of Poland by Nazi Germany and the Soviet Union in 1939 at the onset of the Second World War.

**55** Hansen 1994: 589. His exact field of responsibility in Krakow has not yet been investigated.

**56** ÖStA, AdR, K 19/77 Präsidium; Kowalski, Kraków, Jul 7, 1947.

**57** ÖStA, AdR, BKA-Inneres, Reichsstattalterei III, 78.220/1939; Otto Koller, Jun 9, 1938, to the Reich Commissioner for Cultural Affairs.



vested greatly in the exhibition, as it gave him the first opportunity to present his life's work in the field of astral calendars. The topics ranged from calendar systems and ornamental signs of orientation (*Ortung*) to star constellations and astronomical "camouflage" (Röck, 1942: 12-15).

The major aim of the exhibition was to restore Röck's reputation in ancient Mexican studies. The highlight, presented at the end of the tour, was the *Codex Vindobonensis I*, renamed *Codex Kreichgauer*. Undoubtedly, the last hall demonstrated Röck's "deciphering procedure" as a grand finale. Röck had organized twelve blackboards to explain the rules of encrypted astronomy to the ultimate detail.<sup>58</sup> The first step of this method was based on counting various symbols. Then, abstract number series were generated and, finally, after applying the basic types of mathematical calculation, the astronomical data was produced. This sophisticated procedure was exemplified in almost all of the fifty-two codex sheets. Special attention was given to sheet fifty-one, since according to Röck, it contained the exact number of the synodic orbital period of Saturn with a duration of 377.75 days.<sup>59</sup> The extremely text-heavy display boards, with their elaborate arithmetic operations, probably only served to reconfirm to visitors what Röck's colleagues had already identified in their reviews in 1937: his deciphering method was based on number acrobatics, which were incom-

<sup>58</sup> VUA, FRP, 131.104.2; photographs for the blackboards in room 6.

<sup>59</sup> Ibid.



**Figure 5**  
A slide page of the famous Codex Vindobonensis I (ÖNB/Wien, Cod.mex.1, pag. 31) Röck misinterpreted these temples as "twenty-eight Toltec lunar houses" (VUA, FRP, 131.104.2)

prehensible to anyone but him. Ancient American scholars simply ignored the exhibition. Not a single scientific review of the “culture-comparative exhibition,” which ran until September 1944, is known.

On June 7, 1945, Röck was discharged from his posts by the Austrian authorities and had to undergo a “denazification” procedure. Röck had wanted to join the Nazi party in 1938, but ultimately, he simply remained a party candidate. He was judged free of National Socialist sentiments.<sup>60</sup> Since Röck had already passed the age limit of 65, he retired. Nevertheless, he persisted in claiming the veracity of his deciphering method into old age.<sup>61</sup> All the more tragic is the fact that since World War II, the astral interpretation of myths in ancient Mexican studies has generally been regarded as a scientific aberration.<sup>62</sup> In 1949, the Mexican archaeologist Alfonso Caso began to work on a genealogical-historical interpretation (Caso, 1949: 145-186), which Karl Anton Nowotny and his student Ferdinand Anders joined in developing.<sup>63</sup> Nowotny based this new interpretation on the ethnographic observations of the German zoologist and linguist Leonhard Schultze-Jena, who between 1929 and 1931 visited a number of Mesoamerican communities and recorded oral traditions (Jansen; Pérez; Jiménez, 2017: 444).

## CONCLUSION

Eduard Seler from Berlin laid the foundations for astronomical and calendar-oriented ancient Mexican studies in Austria during the First World War. The Steyler missionary Damian Kreichgauer worked in St. Gabriel near Mödling on the thesis that the ancient Mexican manuscripts were intentionally “coded” by a priest elite. Although the Seler School in Berlin rejected this reorientation, Friedrich Röck drew on Kreichgauer’s work. Röck built his scientific career in an environment of *völkisch*-minded ancient Assyriologists who wanted to establish a “pan-Aryan Orient” comparable to the previous “pan-Babylonism.” Röck adopted the astral-Elamite approach of the Prussian schoolteacher Ferdinand Bork and transferred it to ancient Mexican studies. He developed a trans-Pacific approach that would show “lost cultural connections between the Old and the New World” (Röck, 1925: 142-152). Based on his theosophical-mystical worldview, Röck elaborated an opaque deciphering system for ancient Mexican manuscripts. In June 1936, he received an offer from the German Legation in Vienna, who wanted to exploit Röck’s results for Nazi science policy, but this never came to fruition because German ancient Mexican scholars rejected Röck’s method as unscientific. Karl Anton Nowotny was an early critic who distanced himself from “Röck’s method” in his own dissertation, which was supervised by Röck. Thus, Nowotny created an important basis for the genealogical-historical and ethno-

**60** ÖStA, AdR, BMfU, PA Röck, f. 134; Resolution of the Special Commission of the First Instance of the State Office for Volksaufklärung, for Education and Cultural Affairs: “Assessment according to §21 of the Prohibition Act”, Nov 15, 1945.

**61** In the post-war period, Röck published his deciphering procedure in the periodical *Sternbilder*, edited by the Austrian sci-fi writer Erich Dolezal (Röck 1948: 104-115).

**62** Kudlek 1972: 431; Anders/Jansen/García 1992; Anders 2005: xi; see also Steinle 1995: 74.

**63** An important Nowotny student is also Johanna Broda (born 1943), who graduated from the University of Vienna in 1967 (Broda de Casas 1967). With her longstanding professorship at the National Autonomous University of Mexico (UNAM), she is by far one of the most well-known experts in Archaeoastronomy, Calendars and Sacred Geography in ancient Mesoamerica.

graphic interpretation of Mexican codices in the post-war period. The fact that Nowotny belonged to the Nazi Party as early as 1932 and had accepted research assignments from the German Ancestral Heritage (SS *Ahnenerbe*), after the 1938 *Anschluss*, has previously been unknown and is perhaps one of the most surprising findings of this study. A former Nazi modernized ancient Mexican studies in Austria. Since the retirement of Ferdinand Anders, this previously well-established discipline at the University of Vienna is currently no longer represented.

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**Peter Rohrbacher**, PhD in Vienna 2001, is a postdoctoral researcher at the Institute for Social Anthropology of the Austrian Academy of Sciences in Vienna. His work focuses on the history of anthropology and on African studies. At the moment, he is co-editor of the book project “Viennese Ethnology in the Nazi Period” (in collaboration with Andre Gingrich, forthcoming).

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# Beyond the Mainstream: Max Schmidt's Research on "The Arawak" in the Context of Contemporary German Ethnology

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## Michael Kraus

🏠 University of Göttingen | Göttingen, Germany

✉ [mkraus@sowi.uni-goettingen.de](mailto:mkraus@sowi.uni-goettingen.de)

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### ABSTRACT

Analyzing the work of Max Schmidt (1874-1950), especially his 1917 book *Die Aruaken. Ein Beitrag zum Problem der Kulturverbreitung* [The Arawak: A Contribution to the Problem of Cultural Dissemination], this article deals with methodological and theoretical trends among German ethnologists carrying out expeditions in the Amazon region at the turn of the nineteenth century. The approaches outlined are placed in the context of the institutionalisation of ethnology as a separate academic discipline in Germany. The focus is on the development of modern fieldwork methods; the critique of diffusionism by Schmidt and other South America researchers; and the specific approaches of Max Schmidt who, in spite of the contemporary emphasis on "material" and "intellectual" culture, also considered sociological issues in his analysis.

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### KEYWORDS

Max Schmidt,  
German ethnology,  
Arawak, Amazonia,  
(financing of)  
expeditions,  
fieldwork, (critique  
of) diffusionism

## INTRODUCTION<sup>1</sup>

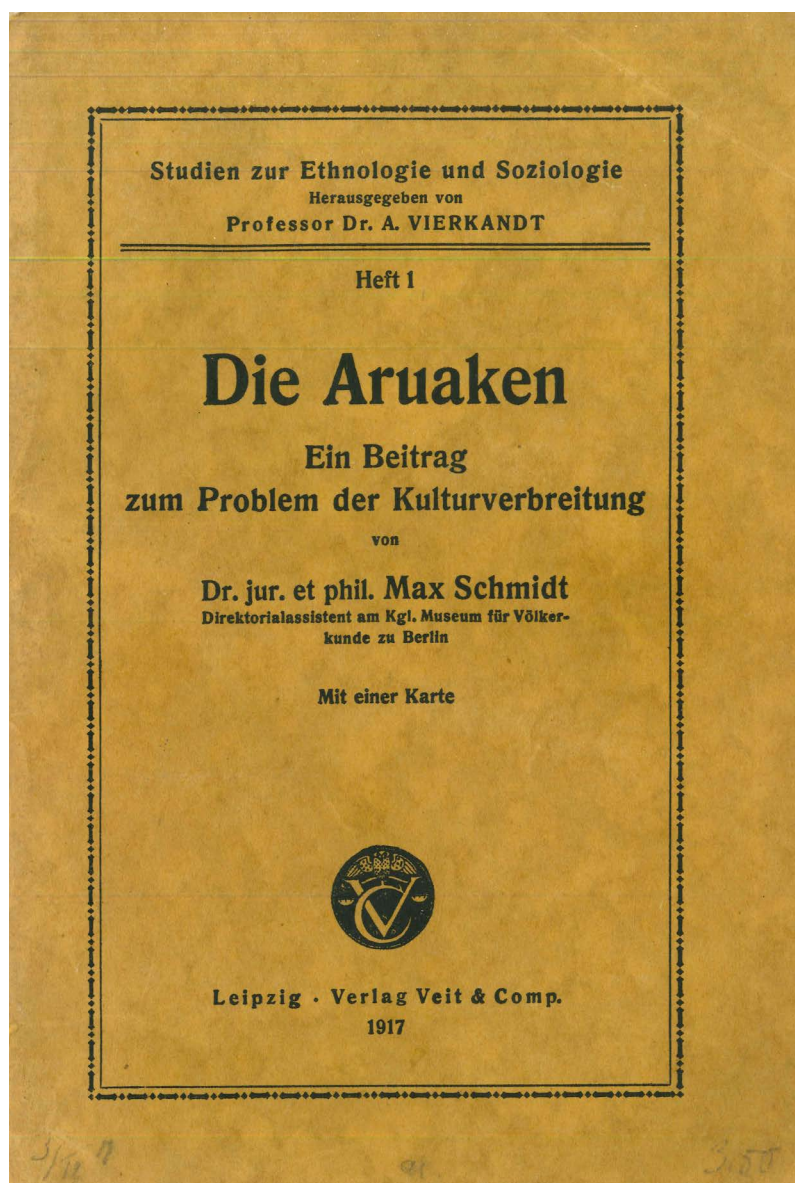
"Max Schmidt laid stone upon stone in the great structure of Brazilian ethnology. He was an honest and industrious worker. Not everyone can be an architect. Max Schmidt could not be compared with the brilliant and powerful personality reflected in the works of Karl von den Steinen, nor did he have the fanatic will with which Curt Nimuendajú integrated himself into the life of the Indians to the extent that he could understand the most difficult problems of their culture better than anyone ever before. But as long as studies about the native peoples of Brazil continue, Max Schmidt will be thought of as the researcher of important ergological, economical, and juridical questions, and as an indispensable source of knowledge about many tribes in Matto Grosso" (Baldus, 1951a: 303).

With these words Herbert Baldus (1899-1970) ended his obituary of the ethnologist Max Schmidt (1874-1950), who had died under tragic circumstances in Paraguay one year earlier. Baldus did not write a hymn of praise, but rather attempted to pay an objective and critical tribute to the scholar who grew up in relatively well-to-do surroundings, had first studied law and then, following his own inclinations, devoted his life's work—as well as his private means—to the study of indigenous cultures in the South American lowlands.

As Baldus correctly mentioned, Schmidt's work already during his lifetime stood in the shadow of other studies and personalities that would mark the young discipline of "ethnology" far longer than Schmidt's endeavors. Yet, the analysis of Max Schmidt and his work is of interest today not merely for the sake of historical inclusiveness. Schmidt not only contributed to a better knowledge of the ethnography of the Amazon area, but also to the

<sup>1</sup> This is a revised and updated version of an unpublished paper, encouraged by Augusto Oyuela-Caycedo and Manuela Fischer several years ago and translated from the German by Emily Schalk. I am grateful to Han Vermeulen for valuable comments. A Portuguese version will be published in Petscheli and Schröder (eds.).

**Figure 1**  
Cover of Max Schmidt's book *Die Aruaken* (1917)



establishment of field research. In his work, he repeatedly contested research traditions and contemporary debates in his own discipline. Aside from his critical standpoints, he endeavored to integrate perspectives and approaches, which until then were dealt with only marginally within the dominant discussion, into scholarly debate, and thus stimulated new theoretical views. In addition, he was intensively concerned with the collections stored in the *Königliches Museum für Völkerkunde* (Royal Museum of Ethnology) in Berlin and through generally comprehensible summaries tried to contribute to a record of contemporary ethnological knowledge; providing an orientating guide for delineating the boundaries of the discipline at that time. Moreover, in his writings he repeatedly called for an end to "European" arrogance and condescension when dealing with foreign cultures. However, Schmidt saw no contradiction in combining a critique of "civilized people" and calling for an "unbiased judgment" of non-European people, on the one hand, and noting the potential use of such knowledge in colonial work, on the other hand (Schmidt, 1920-21, vol. 1: 2, 8).<sup>2</sup>

Max Schmidt did not start his professional life as an ethnographer. At first, he studied medicine, but after one semester changed his subject and continued with legal studies. In 1899 he obtained his doctorate at the University of Erlangen with a thesis about *Beiträge zur ratio juris im römischen Recht* (Contributions to the ratio juris in Roman law). For a short time, he worked at the local court of Blankenese, but soon moved to Berlin where he enrolled at the university in both ethnology and anthropology, also starting work as a volunteer at the Royal Museum of Ethnology in Berlin. From 1900 to 1901, he undertook his first expedition to Central Brazil. Upon his return, he landed a job as an assistant of the director at the museum in Berlin. In the museum he repeatedly ascended the ranks, becoming head of the department of South America in 1918.

In 1910, 1914, and from 1926 to 1928, Schmidt again went to South America to study the culture of indigenous societies. In 1917 he was awarded his second doctorate, this time about *Die Aruaken. Ein Beitrag zum Problem der Kulturverbreitung* (The Arawak: A Contribution to the Problem of Cultural Dissemination) at the University of Leipzig (see Fig. 1). In 1918 he obtained the title of "professor", and from 1921 onwards he also worked as associate professor ("außerordentlicher Professor") at the Friedrich-Wilhelms-Universität of Berlin (today: Humboldt-Universität). In 1929, after his fourth research trip to South America, Schmidt resigned from his work in Berlin. He first went to Brazil and then to Paraguay, where he continued his fieldwork and contributed to the development of Paraguayan ethnology. Seriously ill and without financial means, he died in Asunción in 1950.<sup>3</sup>

In the following, I shall discuss the achievements of Max Schmidt within the framework of contemporary developments in German ethnology, and of research of the South American lowlands in particular. A few initial remarks on the

**2** The argumentation in an earlier contribution is ambiguous. On the one hand, Schmidt refers to the lack of legal protection for Brazilian Indians and, with view of the German colonial policies in Africa, emphasizes that indigenous law should be studied as well, in order that it could be considered in given cases in colonies. On the other hand, he emphasizes the advantage of this knowledge for an "effective control of the economic configurations" (Schmidt 1907: 462, 475).

**3** For more detailed biographical information, see Baldus (1951a-b), Bossert and Villar (2013), Schmidt (1955) and Susnik (1991).





**Figure 2**

Caetano, chief of the Guató, serves as a guide for Max Schmidt, 1910. (Courtesy: Ethnologisches Museum, Staatliche Museen zu Berlin, Germany, No. VIII E 2680)

history of expeditions may illustrate the slow but steady formation of modern research practices in fieldwork. These are followed by two sections that elucidate Schmidt's thematical-theoretical approach in agreement with or differing from topics of discussion in contemporary ethnology. A conclusion summarizes my view on Max Schmidt as an often ignored, but remarkable innovative thinker and an industrious fieldworker of his era.

#### **RESEARCH EXPEDITIONS AND FIELDWORK**

During the thirty years before the outbreak of World War I, sometimes retrospectively designated as the "age of the great expeditions" (Illius, 1992: 108), a comparatively large number of German scientists traveled to the South American lowlands to study the cultures of indigenous ethnic groups living there.

When viewing these travels, particular aspects are striking: Even in its seminal years, ethnology in Germany was not purely "arm-chair" anthropology. As early as the nineteenth century, numerous persons, who not only considered themselves as "data collectors", but also as scientists, traversed foreign regions to undertake research. This research was given an enduring structure by the influence of museums, which were the central institutions for the discipline during the nineteenth and the early twentieth century.<sup>4</sup> As a rule, the financial support provided by museums was connected with the obligation to acquire compre-

**4** In a seminal study, Han F. Vermeulen (2015) analysed the genesis of ethnography and ethnology as terms, methods, and research programs in the work of German-speaking Enlightenment historians. As an independent academic discipline, with its own institutions (the first ethnographic museums), permanent jobs for persons calling themselves "Völkerkundler/Ethnologists" and the possibility to habilitate at an university – the first time achieved by Adolf Bastian in 1866 (for "Ethnographie"), and second by Karl von den Steinen in 1889 (for "Ethnologie") – Ethnology was established in Germany in the second half of the nineteenth century.

hensive collections, a precondition that could force an early departure from one area in order to acquire new ethnographic objects in another. Nonetheless, during this period, a tendency from extensive travels in an extended region to more intensive stays with one specific ethnic group can be discerned.<sup>5</sup>

The history of German travelers in the Amazonas region, who understood themselves explicitly as ethnologists, seeing science as their profession and who in their own country at some time worked in a museum or at an university where they evaluated and published the compiled data and collections, begins with Karl von den Steinen (1855-1929).<sup>6</sup> In 1884 von den Steinen and his companions traversed the area of the Alto Xingu (see Chapter 1). They were provided with a military escort on the part of the Brazilian authorities, whose number von den Steinen attempted to keep to a minimum. This undertaking did not result in anything of relevance for the economic development of the country (Steinen, 1885: 228). Instead, von den Steinen emphasized that the province of Mato Grosso would hold greater treasures than gold and diamonds (Steinen, 1886: 327). The fact that he returned to the Rio Xingu basin again, in order to devote himself once more in greater detail to the study of the indigenous people in that region began during the first expedition, clearly shows the real interest of this scholar: the study of the culture of people, who until then had been largely ignored in academic research, viewed as a contribution to an universal science of humanity.<sup>7</sup>

During his second expedition, von den Steinen left his companions for a short time; he became aware that their appearance as a group had a negative influence on the contact situation. In his book *Unter den Naturvölkern Zentral-Brasiliens* (Among the Native Peoples of Central Brazil), published in 1894, he emphasized that the relationship with the Indians improved significantly when he resided as a guest, alone in their village. Referring to the wholehearted behavior of his hosts in those days, he wrote: "none too little of this [behavior] disappeared when the larger party came; the complete impartiality that I as an individual person was shown, did not remain, and the behavior came to resemble more of the known pattern that tends to be described in books" (Steinen, 1894: 56; 100).<sup>8</sup>

In von den Steinen's footsteps, Herrmann Meyer (1871-1932) undertook two expeditions to the Xingu region of central Brazil in 1896 and 1899. The second expedition in particular, which in view of those times was equipped in downright luxury, disintegrated quickly due to internal controversies, barely navigable rivers, as well as hunger and illness among the participants.<sup>9</sup>

The after-effects of this expedition were reported even by Max Schmidt, who arrived in Cuiabá, the capital of Mato Grosso, not quite two years after Meyer's second expedition. The grand outfitting of Meyer's expedition had heightened expectations, which Schmidt, much more modestly equipped,

**5** See in detail Kraus (2004a; 2014). On the emergence of ethnological museums in Germany as well as on the general situation of ethnology during the German Kaiserreich, see also Penny (2002), Bunzl and Penny (2003).

**6** Karl von den Steinen himself called Carl Friedrich Philipp von Martius (1794-1868) the founder of Brazilian ethnography (Steinen, 1894: 397). That being said, this famous researcher, whose collections still count among the most outstanding holdings of the ethnological museum in Munich today (Museum Fünf Kontinente), cannot be designated a professional representative of an independent academic discipline. Contrarily, Karl von den Steinen, educated as a physician and psychiatrist, achieved the *habilitation* in "Ethnologie" (ethnology) after the two Xingu expeditions in 1889 in Berlin. In 1891 he was given the title of professor in Marburg. He later worked in the Royal Museum of Ethnology in Berlin until 1906. Paul Ehrenreich (1855-1914) traveled throughout Brazil at about the same time as von den Steinen, and in 1887 together with von den Steinen. Ehrenreich was also in close contact with the Berlin Museum. In 1911 he received a professorship; on von den Steinen, see Coelho [ed.] (1993); Hermannstädter (2002); Kraus (2004a); on Ehrenreich cf. Kraus (2004a) and Hempel (2015).

**7** Cf. Fischer, Bolz and Kamel (2007); with reference to research in the Amazon region, see Kraus (2007).

**8** The accompaniment of a large team was not of foremost interest to von den Steinen, who noted that the military escort in Brazil was more of a necessary evil than his own wish. Other ethnologists, such as Fritz Krause

could not meet (Schmidt, 1905: 24, 40-41.). Furthermore, Schmidt noted changes in the structure of indigenous society. Accordingly, Antonio Bakairi, who had accompanied von den Steinen as well as Meyer on their expeditions, had become a wealthy and well-armed leader in the region of the Indians as a result of the pay that he had got for his support of their expeditions (Kraus, 2004a: 362-371; 2014: 42-46).

Schmidt's plan in 1901 was first long-term research in a village on the Rio Xingu. Contrary to the preceding expeditions, he wished to rely solely on a Brazilian assistant as well as the alternating support of Indians. However, he was unable to fulfill this plan, which now seems quite modern. After a few weeks Schmidt became seriously ill and had to return to Cuiabá under dramatic conditions. He had not even reached the village of the Kamayurá in the Alto Xingu area, where he intended to carry out what would have been the first stationary fieldwork in the area. En route to this village, Bakairi, Nahukwá, and Aweti Indians took possession of all goods Schmidt had brought along for exchange, so that he found himself forced to retreat.

It is noteworthy that Schmidt published his experiences on the Xingu river in all openness, without seeking the fault for the failure of his plan in others – contrary to, for example, Herrmann Meyer.<sup>10</sup> Despite his sometimes painful experiences Schmidt did not change, neither in his positive attitude towards the Indians nor in his general endeavor and personal intention to carry out fieldwork. Thus, subsequent to his more or less unsuccessful first Xingu venture, Schmidt, after a short recuperation in Cuiabá, visited the Guató in the border area of Brazil and Bolivia, still in 1901 (Schmidt, 1905). As mentioned above, prior to his final emigration to South America, he had departed from Germany for a total of four field trips; in later journeys he again visited the Guató and also returned to the Bakairi (see Fig. 2, 3).<sup>11</sup>

Furthermore, Schmidt's travels – and in a sense, especially the failure of his first expedition – are an impressive example of a new research strategy with the explicit aim to avoid a position of superiority. As the aim was to become acquainted with the indigenous population, and not to dominate them, traveling researchers increasingly relied upon their native hosts. Thus, the at times, rather indiscriminate assumption that conditions of power in the field were generally to the advantage of the white researchers, or were almost completely manipulated to suit their own interests, is quite erroneous.<sup>12</sup>

With more success than Max Schmidt, Theodor Koch-Grünberg (1872-1924) carried out research in the South American lowlands during the following years (see Chapter 5). Similarly, Koch-Grünberg was accompanied by only one white assistant and otherwise relied on the help of indigenous individuals.<sup>13</sup> He traversed relatively large areas – out of interest in ethnographic-geograph-

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from Leipzig, rejected such an escort, referring explicitly to von den Steinen's reports on the negative experience with such entourages (Krause, 1911a: 28).

**9** Koch-Grünberg (2004). Meyer's expeditions represent a special case in German explorations of the Xingu. Financially independent, Herrmann Meyer endeavored, amongst others, to shed the shadow cast by his older brother Hans Meyer, a renowned colonial geographer and specialist on East Africa. Thus, his journey was directed more towards attaining prestige than towards achieving scientific results. With its pompous furnishings and the authoritarian behavior of its leader, his journey almost seemed like transferring an expedition in colonial Africa to the context of Brazil (Kraus, 2004b: 474 ff.). Meyer was the only one of the explorers mentioned above who became economically engaged after the expeditions. He invested in the establishment of colonies for German emigrants in southern Brazil. Following the second expedition his involvement in ethnology was more like that of a patron. See in more detail, Hermannstädter (2004).

**10** Meyer never published the planned comprehensive monograph. Only a few articles appeared subsequent to his second expedition, in which he mainly attributed the failure of his fieldwork to the difficult geographical conditions and to mistakes made by his accompanying team (see Hermannstädter 2004, Kraus 2004b).

**11** See also Schmidt's critique on the lack of "enthusiasm for fieldwork" in Felix Speiser's descriptions of his travels (M. Schmidt, 1926). For a more detailed analysis of this

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ical pioneer studies but also compelled by the need to acquire comprehensive museum collections; thereby, the actual duration of his stay in different villages varied greatly. Nevertheless, he spent several weeks or even months with certain ethnic groups. The itinerary of Konrad Theodor Preuss (1869-1938), for instance, was marked by even longer stationary sojourns.<sup>14</sup>

The description of these travel experiences, which were still standard in the work of the ethnographers mentioned above, did not necessarily serve as an opportunity for self-esteem or for heralding one's own deeds, as many (post-)modern analyses of travel reports often imply.<sup>15</sup> To the contrary, their travel journals in South America imparted the aspect of early discussions on the methods employed. Through their accounts of their individual course of travel, the forms of contact that took place, and of concrete individual problems and impressions, these scientists formulated deliberations about the conditions of travel and research in the field of a foreign culture. On the one hand, these observations made a precise contextualization and assessment of the data collected possible; on the other, they were of use for later travelers in their preparations and possible approaches for abating actual difficulties in field research (Kraus, 2004a: 204-222).

This aspiration was explicitly formulated in the work of the Leipzig ethnologist Fritz Krause (1881-1963), who had traveled through parts of the area of the Rio Araguaia in 1908, where he had come into contact foremost with Karajá Indians. In the beginning of his book, *In den Wildnissen Brasiliens* (In Brazil's Wilderness), published in 1911, Krause justified the structure of his description that follows the common contemporary division into "travel experiences" and "travel results." He stated:

*I commence with a report on the expedition itself in the first part, which precedes the actual results. I consider this highly important, for the results of the expedition can only be appraised correctly when the manner of their attainment is known, that is, the conditions under which the studies took place (Krause, 1911a: iii).*

Accordingly, Krause's aim was not to show the life of the Indians, but rather life

*with the Indians, as from only this manner of presentation can possibilities for research, especially the manifold constraints one is subjected to out there, much against one's will, be recognized. [...] Thus, this report includes, in short, an overview of the geographical conditions in the areas traversed, the manner of travel, the cultural situation of the country, and, hence, will offer much that is new and can provide important pointers for future travelers (Krause, 1911a: iv; italics by the present author).*

expedition, see Kraus (2004a: 317-326). Short summaries are presented in Hemming (1995 [1987]) and Kraus (2000). More recently, Bossert and Villar (2013; 2015) have provided a deep and sympathetic analysis of Schmidt's travels.

**12** See for example Niekisch (2002: 8). During his visit to the Aparai of northeastern Brazil in 1924, the Swiss ethnographer Felix Speiser made the following interesting observation: "Now of course we could have behaved differently among the Indians, that is, as white masters who made demands of the Indians; and after our return we were ridiculed for not doing just that. But this reproach could only have been voiced by those who did not know our goals. We wanted to conduct ethnographic studies and were therefore dependent on the Indians viewing us as friends whom they trusted, to whom they would gladly communicate what we wanted to know" (Speiser, 1926: 125).

**13** Nevertheless, it should be noted that Koch-Grünberg simultaneously suffered and benefited from the system of structural violence that had been imposed on the area by rubber collectors (Kraus, 2018).

**14** On Preuss, see Fischer (2007); Kraus (2004a; 2007); Valdovinos (2013); Reyes (2017). See also the online exhibition of the Deutsche Digitale Bibliothek: <http://ausstellungen.deutsche-digitale-bibliothek.de/preuss/exhibits/show/kolumbien-preuss/konrad-theodor-preuss> (last accessed 19 May 2018).

**15** The travel description by Herrmann Meyer was an exception, as already noted (see Kraus, 2004b: 476-483).

By contrast, the second part of Krause's book, the actual ethnography, was intended

*more as a reference book. A strict distinction is made between those events that had been observed just once or several times and those about which information had been solely reported. This distinction cannot, in my view, be strict enough, if errors are to be avoided that afterwards are passed on and on. Some reports that seemed uncertain to me are dealt with in the footnotes, as a sign to later researchers that they should be investigated (Krause, 1911a: iv).<sup>16</sup>*

Subsequent to this research trip, Krause made several plans aimed at exploring the area between the Rio Araguaia and the Rio Xingu. In the preserved records he reached out to his colleague Theodor Koch-Grünberg for discussion on the subject. Krause called for the assignment of up to four scientists, who were to study the respective language as well as the course of festivities during the yearly cycle and who would remain up to one year with a specific ethnic group; which, during that time meet up occasionally to exchange experiences.<sup>17</sup> Yet, the execution of this project was above all thwarted by the outbreak of World War I, which largely disrupted the German tradition of research in Amazonia.

What left a lasting mark on these early expeditions – aside from other factors, such as the behavior of the indigenous people who were visited, the regional political and geographical constellations encountered, or the personality of the respective ethnographers – was the collaboration with museums. These institutions both supported research and constrained it at the same time. Thus, the sale of collections acquired was one of the most important financial resources for ethnologists and their expeditions. In many cases researchers had to carry the financial risk alone. For example, when Karl von den Steinen arrived in South America for the first time in 1883, neither the exact route of travel nor the financing of the expedition had been clarified. It was only on site that von den Steinen decided to traverse the Xingu region, hitherto unknown to scientists. The 1,500 German marks from the Berlin Museum, mediated by the German consul in Belém, and the additional sum of 4,500 marks for the sale of his collection to the Berlin Museum were paid only *after* termination of his travels. Prior to his expedition von den Steinen had been assured funds of only 1,000 marks by Adolf Bastian (1826-1905), the then director of the Royal Museum of Ethnology in Berlin.

Other scientists also worked without financial security initially. Theodor Koch-Grünberg accompanied Herrmann Meyer in 1899, who alone assumed the costs for transportation, equipment, food, and accommodation, a situation that corresponded with the widespread model for young researchers traveling without any remuneration (Koch-Grünberg, 2004: 21). Aside from funds from the Berlin Museum, Koch-Grünberg was able to carry out his famed Rio Negro

**16** That the introductory passages were actually used in the way mentioned is attested in various documents in the archives. For instance, Koch-Grünberg explicitly recommended the first chapter in Krause's work ("Zweck und Ziel der Expedition, Plan und Ausrüstung" (Purpose and goal of the expedition, plan and equipment)) when preparing for travel to South America. Letter by Koch-Grünberg to Hintermann, 30 Oct. 1923, VK Mr. A.35. In his review of Erland Nordenskiöld's book *Forschungen und Abenteuer in Südamerika* (Research and Adventures in South America), Wilhelm Koppers (1923/24: 1102) noted: "This publication is also intended for larger circles. But this does not render it indispensable for the specialist. For here he not only sees each stage of travel, but also the individual conditions under which the various results were gained."

**17** These plans, made in 1910, 1914 and 1916, are now preserved in VK Mr. G.I.1, see also Kraus (2004a: 106-07; 264-65).

journey above all through the financial support from his future mother-in-law. He later reimbursed this sum through the sale of collections.<sup>18</sup>

Max Schmidt, who like Karl von den Steinen came from a relatively wealthy family, financed his first journey to Brazil in 1900-01 with private funds. Only after his return from the Xingu Indians in Cuiabá did he receive financial support from the *Ethnologisches Hilfskomitee* (Ethnological Auxiliary Committee),<sup>19</sup> which enabled him to continue his travels to the Guató. Likewise, during his later expeditions Schmidt often paid the travel costs and only later, whether through state support or the sale of the acquired collection to the Berlin Museum, was he partially reimbursed. The possible risks involved in attempts to acquire large numbers of ethnographic objects are already apparent in Schmidt's first trip: Due to illness, in 1901 he had to leave his collections behind in the Xingu region. However, ultimately one year later some Bakairi brought the objects to Cuiabá, from where they embarked upon a short odyssey on various steamships before arriving in Corumba. There the ownerless crates were discovered by chance by the German consul Hesslein, who ordered their shipment to Berlin, where they were delivered to Schmidt, who to his great surprise received them three years after his return from Brazil (Schmidt, 1905: xiv).

Thus, collecting in the field was of an ambivalent character, too. On the one hand, it reflected the scholarly interest in material culture;<sup>20</sup> on the other, it proved to be an economic necessity that determined other aspects of fieldwork. The demand to take large numbers of exchange goods to the Indians and bring back comprehensive collections, made journeying a logistically tedious undertaking and restricted other research interests.<sup>21</sup> This becomes clear, for example, in a letter by Koch-Grünberg, dated 1916, in which the researcher regrets having to relinquish the pursuit of his scientific interests because of the need to acquire large collections of ethnographic objects. He wrote: "When the present irrational time [World War I] is over, and the Ocean is open again, I shall probably journey once more to the upper Rio Negro, to accomplish what I could not carry out as I had wished for at that time because I had to collect for museums. There I shall record the rich mythology of the Arawak tribes and their folk magic" (Letter of Koch-Grünberg to A.V. Frič, 13 April 1916, StA Lu (EL 232, Büschel 333), see also Kraus (2004a: 109-114)).

In the following years, the wish to travel once more to the upper Rio Negro with enough time to study mythology, which his collecting obligations during the expedition between 1903 and 1905 had prevented, often appears in Koch-Grünberg's letters. Thereby, he also expressed his impression – and with regard to the work of Max Schmidt, this should be emphasized – that the Arawak influence in South America was "certainly far greater than imagined until now, in mythology as well" (Letter of Koch-Grünberg to Walter Lehmann, 7 December 1920, VK Mr. A.29). However, Koch-Grünberg was unable to fulfill

**18** On the Rio Negro journey, see Ortiz (1995); Kraus (2004c; 2018). On financial resources for research in general, see Kraus (2004a: 108-129).

**19** In existence from 1881 to 1925, the *Ethnologische Hilfskomitee* (until 1902: *Hilfscomité für Vermehrung der Ethnologischen Sammlungen der Königlichen Museen*) was established under the decisive participation of Adolf Bastian. Its members provided capital (at least 3,000 marks per person) for the purchase of collections or the financing of journeys. Upon receiving the collection, the Museum paid this sum back to the committee (Westphal-Hellbusch, 1973: 65-68).

**20** In Bastian's view, collections had the same function for ethnologists as manuscripts for philologists. Within the canonical scientific disciplines they also served to delineate ethnology from other fields that were likewise concerned with non-European cultures but specialized mainly on written sources (see Kraus, 2007: 142-144; 2014).

**21** The difference between researchers and traders was the preparedness of the former to pursue their research interests, the scientific preparation and the evaluation and publication of the collected data and material, even though only the accompanying costs were covered. Financially independent researchers often donated their collections to museums.



his plans. He died of malaria on the 10<sup>th</sup> of October, 1924, at the beginning of his fourth research trip – this time as a member of the US American expedition of Alexander Hamilton Rice (1875-1956) to northern Brazil and Venezuela. Shortly before his departure to Brazil, Koch-Grünberg wrote to his friend, the Swedish ethnologist Erland Nordenskiöld (1877-1932), about his plans: "We want to take the route from the Sierra Parima to Casiquiare, if possible, over the still unknown course of the Siapa. Then, if my health allows, I want to spend some time in São Felipe on the upper Rio Negro, to record Arawak myths" (Letter of Koch-Grünberg to Nordenskiöld, 5 May 1924, VK Mr. B.I.4.).

### CONTEMPORARY DEBATES IN GERMAN ETHNOLOGY

After this short history of expeditions to the South American lowlands, some aspects of the scholarly work of Max Schmidt can now be considered. Here, I aim at illuminating his work in the context of the developing discipline of ethnology rather than as an entity of its own. The manifold themes and deliberations found in Max Schmidt's work can be taken up in two lines of discussion, which are also present in his book "The Arawak," published in 1917. They clearly indicate the position of his work within the wider ethnological discussion at the end of the nineteenth and the beginning of the twentieth century. By following these lines, it is first necessary to introduce the most important debates and protagonists of that time.

Questions on the provenance and genesis of peoples and cultures were a principal component of ethnological research during the aforementioned time span. One particular controversy in Germany at the end of the 1900s is associated with the names of Adolf Bastian and Friedrich Ratzel (1844-1904). Without refuting processes of diffusion,

Bastian propounded his concept of "universal ideas" (*Elementargedanken*, elementary ideas), which developed within a "geographical province" of each cultural unit into "folk ideas" (*Völkergedanken*). He advocated the idea of a regionally independent development of cultural traits, at least as long as no external influences were evident. Ratzel presented the opposite argument. For him independent inventions of cultural phenomena were an exception, which first had to be



**Figure 3**  
Bakairí at the  
Paranatinga river, 1927.  
(Courtesy: Ethnologisches  
Museum Berlin, Germany,  
No. VIII E 4850)

proven. Ratzel held that cultural similarities could not be explained as multiple and independent formations and, thus, as a parallel course in evolution; instead, they should be seen far more as a singular formation and as the result of subsequent migration, dissemination, and adoption (Koepping, 1983: 60-68; Petermann, 2004: 525-547).<sup>22</sup>

At the start of the twentieth century Bernhard Ankermann (1859-1943) and Fritz Graebner (1877-1934) attracted attention with two articles in which they attempted to identify "cultural circles" (*Kulturkreise*) and "cultural layers" (*Kulturschichten*) in Africa and Oceania (Ankermann, 1905; Graebner, 1905). A few years later, Graebner and Father Wilhelm Schmidt (1868-1954), founder of the Vienna school of "cultural circles" (*Wiener Schule der Kulturkreislehre*), applied this approach in a rather schematic way to South America (Graebner, 1909: 1013-1024; W. Schmidt, 1913).<sup>23</sup> Thereby, they closely followed the work of Ratzel as well as that of Leo Frobenius (1873-1938), among others.<sup>24</sup>

Apart from ethnographic literature, the material culture present in museum collections formed the principal basis of data. By analyzing the geographical dissemination of individual elements of culture, or of entire cultural complexes, attempts were made to deduce historical connections in the form of wanderings and adaptations. The differentiation of individual layers of these complexes was intended to enable the construction of at least a relative chronology for the culture historical development, even for those regions where no written sources existed. According to Graebner's work of 1911, *Methode der Ethnologie* (Method in Ethnology), two principal criteria were available as evidence of culture historical connections: "the criterion of form, that is, the correspondence of characteristics, which without necessity comes from the essence of the object, and the criterion of quantitative correspondence" (Graebner, 1911: 108; see also Ankermann, 1911: 159ff.).

Whereas Bastian, who played a seminal part in establishing ethnology as an independent academic discipline in Germany (Fischer; Bolz; Kamel, 2007), had designed this discipline as a strictly inductive, empirically directed science, oriented toward the natural sciences, the persons mentioned above tried to situate ethnology as part of the historical sciences (Foy, 1911; Graebner, 1911; Graebner, 1923; see also Ankermann, 1911, 1926; Bossert; Villar, 2013: 8-13).<sup>25</sup>

Although a number of the issues called for by "culture historians" - as, for example, a critique of the speculated sequence in the stages of evolution; an attempt to increase the use of museum holdings for research purposes by developing a methodology that takes a critical view on sources; and a demand for historical depth in research on cultures that are "non-literate" yet not "without a history"; - could well have agreed with the existing approaches, a fundamental conflict in this line of thinking soon evolved.

As it is not uncommon in scientific debates, the protagonists made ever fewer

**22** A third approach, which besides explaining cultural similarities as many independent formations on the one hand, and adaptations on the other, was the concept of convergence, borrowed from biology. As stated by Paul Ehrenreich (1910: 263), this was understood to mean that "under the influence of the same environment and the same cultural conditions, items of different origins can take on similar forms."

**23** Even though Father Wilhelm Schmidt, who regularly referred to Graebner's writings, repeatedly tried to minimize the contradictions in their respective approaches, the relationship between Graebner and Wilhelm Schmidt was not without tensions (see, for example, W. Schmidt, 1911).

**24** In his *Methode der Ethnologie*, Graebner, educated as a historian, frequently refers to the *Lehrbuch der historischen Methode und der Geschichtsphilosophie* (Handbook of the Historical Method and of the Philosophy of History) by Ernst Bernheim (1889, fifth edition 1908). Interestingly, at the conference of 1904 in which Ankermann and Graebner introduced their ideas to the public, Leo Frobenius withdrew from his earlier position, to which both Ankermann and Graebner made reference.

**25** For a comparison between corresponding concepts of exhibitions by Bastian on the one hand and Graebner/Foy on the other, as well as the turn from emphasizing a "psychic unity of mankind" (Bastian) in emphasizing the differences between peoples, see Penny (2003).



efforts to learn from one another and to find common ground. Instead, they strove towards establishing their own positions with the use of sometimes rather coarse and, above all, one-sided promulgations of their respective views at the cost of others. The participants' attempt to expand their claim of superiority and to defy criticism with theoretical arguments shaped the discussions. This narrowed the view of the actual object of study and caused the methods employed to become more and more static, construed, and empirically questionable.<sup>26</sup>

While the portrayal of theories about "diffusionism" and "cultural circles" was the main concern in historical overviews of ethnology among German speakers during the first quarter of the twentieth century,<sup>27</sup> it should not be overlooked that these approaches were already a topic of intense debate among contemporaries. Accordingly, almost all of the aforementioned ethnologists, who had specialized in research on South America, held a critical position towards the theses of Graebner, Foy, Ankermann, and Wilhelm Schmidt.<sup>28</sup> In analogy to Bastian's position in the controversy with Ratzel, the German Americanists rejected the version of diffusionism afore mentioned, yet not the investigation of processes of diffusion as such. During their own research trips, they had observed and described the migration and adoption of various cultural elements. The theories of culture historians, based on unreliable data and premature classifications while increasingly becoming generous in drawing connections between continents, did not comply with the results of the Americanists' detailed regional studies.

After a lecture presented by Ankermann in 1911, in which he once again explained the "theory of cultural circles," and also expressed his agreement with Graebner's "Method" (Ankermann, 1911), both Paul Ehrenreich (1855-1914) and Fritz Krause spoke out critically. Referring to earlier works by Graebner and Foy, and using individual examples from America, Krause contested the accuracy of the purported proof of the dissemination of specific elements of culture. Namely, in the view of regional specialists this evidence was not compatible with the ethnographic data at hand; instead, the various clues had been selected to suit the consistency of theory and methods. Had the investigation begun in America and not in Oceania, whence it was attempted to transfer the achieved results, then – according to Krause – the results would have been completely different (Krause, 1911b, in the appendix to Ankermann, 1911). Two years later, during a discussion about Wilhelm Schmidt's attempt to apply the culture historical method to South America, Krause once again criticized the fact that from the entire ensemble of ethnographic contexts only those elements that seemed to comply with the proposed view had been selected, while other examples were left out of consideration. Furthermore, he emphasized that he himself had studied under Friedrich Ratzel; there he had realized the limitations of the possibilities of geographical cognitions and for this reason he could *not* agree with the culture his-

**26** This applies above all to Foy, later works by Father Wilhelm Schmidt, and to Graebner, albeit somewhat more moderately. Ankermann's formulations are clearly more deliberated; he defended the culture historical direction without rigorously doubting other approaches in ethnology or declaring them out of date. The extent to which polemics and distorted versions had already determined the controversy between Ratzel and Bastian is described by Koeppling (1983: 60-68).

**27** Works of the cultural circles' theorists aroused international attention as well. Thus, in his *Argonauts of the Western Pacific*, Malinowski (1922: 516) distanced himself from previous theories, although he named Ratzel, Foy, Graebner, and F. Wilhelm Schmidt, among others, as the "ethnological school"; other approaches in the German-speaking world were not mentioned. Lowie (1937) discussed the "German diffusionists" during the period in question, but ignored their German-speaking opponents.

**28** On the rejection of the theory of "cultural circles" by German researchers on South America, see the detailed account in Kraus (2004a: 469-481). For criticism in general, among others by Franz Boas and later Robert Lowie in the USA, see – despite the imprecise facts about Paul Ehrenreich and Max Schmidt – Petermann (2004). Likewise, the directors of the Berlin Museum, Felix von Luschan (1854-1924), and the Hamburg Museum, Georg Thilenius (1868-1937), were at least skeptical about the new method (Laukötter, 2007: 67-85).

torical "Method in Ethnology." In his comment he went on to state that, "So many geographical and historical conditions, dispositions, level[s] of culture, economic form[s], among others, of the people in question, play such an important role in shifts and adoptions of culture, that no generally applicable guidelines can be found. Hence, one is forced to decide case to case. But this again opens the door to the run-around with hypotheses, although the method claims to present absolutely objective results that are free of hypotheses. For this reason, I wish to warn against an over-estimation of geographical diffusion with reference to its use in historical investigations; for I am convinced that it is impossible to distinguish the historical layers in geographical distribution, according to generally valid guidelines" (Krause, 1913: 1126).<sup>29</sup>

Paul Ehrenreich, who likewise raised his voice in 1913, emphasized among other points, that although the material culture of South American Indians was relatively well studied, the sociological conditions had been investigated far too little, and that, therefore, the application of concepts developed elsewhere served more for the spread of catch-phrases than for the elucidation of local developments. Some years later, Konrad Theodor Preuss laconically noted in the controversy with Father Wilhelm Schmidt that the latter's theories "stated far more than the Americanists knew." He also concluded that "it appears that when a few elements of a cultural circle are found somewhere, one can reckon with all that otherwise belongs to them; yet this is not at all permissible. Indeed, [the concept of] cultural circles demands most of Americanists, for – in spite of all admissions – things in America often will not correspond" (Preuss, 1927: 146-47). Americanists further criticized that with the aforementioned approaches, methods and theories developed at home (that is, in the museum) would gain greater weight than their own field studies (see Kraus, 2004a: 479ff.).<sup>30</sup>

Max Schmidt was one of the vehement opponents of diffusionism and the theory of cultural circles. Yet, unlike his colleagues, he was not only critical,<sup>31</sup> but with his work "The Arawak" he undertook an attempt to formulate his own model of "the problem of cultural dissemination" in South America. As he had no comprehensive data at his disposal, he could at least fall back on his own experiences and observations in South America (see Fig. 4). In several places in "The Arawak," he substantiated his rejection of the approach of Graebner and Wilhelm Schmidt in detail (Max Schmidt, 1917: 2-3; 23; 92-96; 104). In other writings, he referred again and again to what he saw as the one-sided erroneous view of German-speaking advocates of the cultural circles theory.<sup>32</sup>

As far as his theoretical direction is concerned, Max Schmidt stood quite close to Adolf Bastian; however, he constantly strove towards corrections and augmentations. In an early article about the "Ableitung südamerikanischer Geflechtmuster aus der Technik des Flechtens" (Derivation of South American

**29** The discussion including the critical comments of Krause and Ehrenreich is printed at the end of Wilhelm Schmidt (1913).

**30** Like Malinowski, Graebner was unable to leave Australia due to the beginning of World War I in 1914; however, he did not use the five years of his compulsory stay for comparable field studies. Anckermann journeyed through the Cameroon Grasslands already in 1907-09. Father Wilhelm Schmidt traveled to the USA in 1935, then to China and Japan, but did not conduct fieldwork. Nevertheless, through the assignment of missionaries working as ethnologists Wilhelm Schmidt stimulated a comprehensive research program.

**31** Fritz Krause in Leipzig worked on an alternative program. Although in the course of time, his attitude towards the idea of the approach became more positive, he remained critical. In the early 1920s Krause published short articles in which he outlined an approach that in principle was structural-functionalist in outlook (Kraus, 2004a: 478-79.). After 1933 Krause's image was tarnished by his compliance with national socialist powers.

**32** See, for example, Max Schmidt (1918: 13-15, 24-28, 37; 1919: 349, 352, 357; 1922: 441; 1920-21, vol. 1: 9-10; 1923: 20).

weaving patterns from the technique of weaving), he already concluded that many patterns were simply determined by the material utilized and that a large number of widespread ornaments derived from the technique of weaving. Therefore, a decisive factor for the emergence of decorations was the material and the intended function of the object. For this reason, he advocated – pro Bastian and contra Ratzel – a manifold, independent emergence of specific weaving patterns everywhere, “where palm trees grow and where people use its leaves to weave their domestic utensils” (Schmidt, 1904: 512; see also 1905: 330-397). For solving queries in ethnology, he advocated an inductive approach applied as consistently as possible. Accordingly, he wrote in 1919, “In ethnology as a discipline of the natural sciences only ethnological data determined through sensory perception must form the basis for ethnological conclusions; and as an independent science with its own methods, this science can therefore acknowledge as fully valid only those research results that fulfill this necessary requirement of all ethnological studies” (Schmidt, 1919: 369). Furthermore, he criticized that ethnological examples often had to be shown as illustrations for principles that had long since been gained through deductive thought (Schmidt, 1919: 371).

Not concerned with preferring one theoretical variant over the other, Max Schmidt favored the objective verification of individual cases. Thus, he accepted the deductive approach as a necessary supplement to the inductive approach. Of importance to him was the concept of ethnology as a “science of experience,” the sensory perception is decisive in the process of recognition (Schmidt, 1920-21: vol. 1: 19-20; 1924: 30). The extent to which this should become established practice is shown by his remark that in order to understand weaving patterns, it is important “to have plaited something oneself and to be acquainted with the manner in which a plaited pattern appears during plaiting” (Schmidt, 1905: 374).

In Schmidt's view, the question Graebner had formulated as the basic problem in ethnology after Ratzel – “whether a culture historical association or an independent appearance can be assumed by formal correspondences between objects from different peoples” – could not be solved in this general form as it was “stated falsely”: “In individual cases the question concerned can only reach the extent to which the formal correspondence between objects can be traced back to one of them and how far to the other of both of these often juxtaposed effective powers. Especially the form of objects from spatially separated tribes can only be explained in that way, that all of those factors are drawn forth equally, through which the form of the individual objects can be determined. But a whole series of such determining formal factors have to be considered, of which one exerts its force more in the direction of borrowing or transferring, the other more in the direction of an independent emergence and development” (Schmidt, 1918: 13-14).<sup>33</sup>

Schmidt rejected evolutionism as much as the cultural circles theory

**33** Here Schmidt referred to Graebner (1911: 94); see also Schmidt (1920-21, vol. 1: 17).

(Schmidt, 1924: 40-44; also, 1920-21, vol. 1: 8-20). However, his corresponding statements were not without contradictions. For example, the idea that non-literate cultures could give insights into European prehistory was anchored so firmly in Western thinking that it appears repeatedly, even in the work of opponents of evolutionism (Schmidt, 1923: 3; 108-09).<sup>34</sup>

#### GERMAN TRENDS IN RESEARCH ON THE SOUTH AMERICAN LOWLANDS

Despite a general consensus among German Americanists to dissociate themselves from diffusionism and the theory of cultural circles, differing views did arise within the framework of individual regional research. Therefore, Max Schmidt's position should be analyzed from this aspect as well.

German ethnologists who traveled to Amazonia at the turn of the nineteenth to the twentieth century were concerned with a myriad of different themes. Although not all of them pursued the same research interests, certain focal points can be recognized.

On the one hand, they shared a concern with material culture – as mentioned for both scholarly and financial reasons. They also shared the aim to circumscribe the life of the people encountered during their travels as comprehensively and multifaceted as possible. Moreover, in addition to "material culture" a concern with the so-called "intellectual culture" (*geistige Kultur*) predominates, with the documentation of the language, psychological development, mythology, and art.<sup>35</sup> Although data on economic strategies was compiled, it was nevertheless largely descriptive. Sociological issues were scarcely dealt with, as Robert Lowie and Herbert Baldus had already noted (Baldus, 1970 [1954]: 30-33; 214; 347; 351; 774; Lowie; 1937: 6; Lowie on Nimuendajú, 8 August 1938, cited in Dungs, 1991: 291).

In the latter respect, the work of Max Schmidt was an exception as he repeatedly called for a more detailed involvement with sociological and economical questions (Fig. 5).<sup>36</sup> As early as his first research trip to the Xingu river in 1901, Schmidt was concerned with collecting genealogical data in order to connect this information with questions about the economy and the legal situation. His interests extended to the daily life he observed, as well as to the underlying structures. Therefore, Schmidt – probably through his juridical schooling – not only called for the use of uniform terms, but also pointed out the necessity of explaining which of the indigenous concepts actually corresponded with the terms employed. For example, in an early article about the "Guaná" (= Chané, see Susnik [1991: 16]) he remarked:

"It has already been mentioned that in view of the linguistic material at hand the subdivisions of the Guaná tribe listed in the following table [...] are, from a

**34** Interestingly, similar statements can be found in Ankermann 1911: 156. Conversely, Schmidt's application of categories employed in European prehistory, such as "Stone Age" (1923: 108ff.; 1924: 291) had already been criticized by von den Steinen (1894: 203, 212). Like Koch-Grünberg, von den Steinen was strongly influenced by evolutionist ideas, although the personal experiences of both during fieldwork had a relativizing effect, see Kraus (2004a: 397ff.; 421-432).

**35** See in greater detail Kraus (2004a: 399-418). Here the most important protagonists are Karl von den Steinen, Paul Ehrenreich, Theodor Koch-Grünberg, and Konrad Theodor Preuss.

**36** Another exception was Fritz Krause, who in 1924 presented a short study on economic ethnology and worked on the study of "structural-functional theory" (see also footnote 31). The most significant socio-ethnological research was undertaken by Curt Unckel Nimuendajú (1883-1945), who emigrated to Brazil in 1903, and thus despite continuous contact, was no longer firmly institutionally based in Germany.

linguistic standpoint, at most dialectically different. The basis upon which this differentiation of different groups within the population with specific names rests, whether linguistic, economic, juridical or local, cannot be stated with certainty due to the uncertainty in sociological aspects that unfortunately exists in the literature [...] as well as in a large number of travel reports. Camaño speaks of 4 'tribus' of the 'nacion Guana'. According to Hervas the Guaná are divided into 7 'poblaciones ó tribus', according to Azara in 'seis parcialidades, que se gobiernan sin dependencia unas de otros', by Aguirre in '5 naciones', by Castelnau in 'tribus principales' and finally by Taunay in 'ramificações'. This mayhem of designations for one and the same social form shows quite clearly how much ethnology suffers from a lack of uniform terms, especially in the fields of sociology and law" (Schmidt, 1903: 326).<sup>37</sup>

Regarding his work on "The Arawak," the location of the publication is already indicative of Schmidt's sociological interests. His book was published in the series issued by Alfred Vierkandt (1867-1953), *Studien zur Ethnologie und Soziologie* (Studies in Ethnology and Sociology).<sup>38</sup> In his introductory text Vierkandt emphasized that the books in the series should deal foremost with the border area between ethnology and social theory, "in that they examine ethnological themes from a sociological point of view." A strict delimitation on one or the other subject was not intended, as psychological questions should also be considered. By contrast, "the old method of boundless comparisons" was excluded; "instead, only the facts within relatively uniform cultural areas, especially within the so-called ethnographic provinces, should be compared with one another." The themes foreseen in the series – Vierkandt included "family and education; law and customs; self-help and war; political organization and classes; clans and men's societies; community and exchange in nutrition; landowning and ban on land;" and also "the emergence and principles of the class state as well as the mechanisms and social achievements of morality" – should be examined in an "inductive way" in the planned publications (Vierkandt, 1917).

With his sociological, juridical, and economic interests, Max Schmidt introduced new lines of inquiries in the discussions in German research on South America. In addition, he strove towards interdisciplinary connections for the young discipline. Moreover, he took a critical stand towards existing trends in Americanistics research. As early as 1907, Schmidt criticized the "stepmotherly" treatment of economic conditions as compared to "ornament and mythology" (Schmidt, 1907: 461).<sup>39</sup> His attempts to bring approaches of different disciplines together are among others attested in his *Grundriß der ethnologischen Volkswirtschaftslehre* (Outline of Ethnological Economic Theory) (1920-21).

Although clearly distanced from the dominating variant of the theory of cultural circles, German Americanists also attempted to reconstruct the affinity between Indian ethnic groups as well as the origins and historical wanderings of

**37** Max Schmidt also criticized the manifold lists of words pointing to the two "separate worlds of terms" of Europeans and Indians. Terms such as "family," "house" or "tribe," that Europeans take for granted were – according to Schmidt – often insufficiently defined. The actual contextual meaning of a simple designation, as translated by indigenous persons was, therefore, dubious: "In order to come closer to [the meaning of] the term 'house', the question arises first about the extent to which this term actually exists among the Indians, [that is] the extent to which there is a term in their vocabulary that corresponds to our word 'house'. Unfortunately, most vocabularies that we have collected from native peoples and especially those that focus on material goods, are still insufficient in that they present for the most part a more or less arbitrary translation of our European terms, upon which they are focused far too much. Thus, just any word for 'house' is often given in vocabularies without more precise information concerning the specific kind of house that is meant in the respective case" (Schmidt, 1922: 444). As important as this recognition was, it did not save Schmidt from making mistakes in his own translations (see Münzel, 2004: 443).

**38** Vierkandt, who had inter alia studied under Friedrich Ratzel and Wilhelm Wundt in Leipzig, achieved his *habilitation* in 1896 with a study of *Naturvölker und Kulturvölker* (Natural Peoples and Cultural Peoples). He was one of the founders of the *Deutsche Gesellschaft für Soziologie* (German Society of Sociology) in 1909 (Petermann, 2004: 775-76; Strenge 1991).

**39** A later remark, that

these groups. The different fields and subjects that Americanists tried to combine were at first foremost geography (regional division, environmental influences), anthropology (body form, appearance; in the German custom of speech at the time, "anthropology" as a rule meant "physical anthropology") as well as linguistics and ethnology (that is, the study of material and intellectual culture; sometimes aspects of linguistics were also assigned to ethnology).

However, researchers on Amazonia soon turned away from investigations in the field of (physical) anthropology.<sup>40</sup> For them the decisive criterion was language, or membership of a language family. Thus, Karl von den Steinen, who at that time proposed a theory about the "primeval homeland" of the Caribs (that was later rejected), already underlined the methodologically leading role of linguistics (Steinen, 1886: 325; see Kraus, 2004a: 403-08; 2007).<sup>41</sup> In an article published in 1891, Paul Ehrenreich maintained: "The ethnographic division of an ancient population like the Brazilians, who have not come to a differentiation according to nationality, nor to a formation of state, can only be achieved on a linguistic basis, for the sole reason that the individual peoples are distinguishable only through their language" (Ehrenreich, 1891: 85).<sup>42</sup>

As his use of the term "Arawak" demonstrates, Schmidt agreed with this criterion of a common classification of specific ethnic groups; however, in his view this was only a hypothetical starting point for further investigations. Linguistic relationships alone, according to Schmidt, did not allow any secure deductions about the historical origins or genesis of a people (Schmidt, 1917: 8-9; 15-16; 19-20; 72). Instead, he emphasized that "individual cases of the repression of Arawak dialects need not be connected with the suppression of Arawak cultures as well. This is far more the case of foreign languages being learned and employed specifically for the purpose of expanding the sphere of power over foreign influences" (Schmidt, 1917: 21).

With his *Überschichtungshypothese* (hypothesis of superimposition, see Münzel, 2004: 438) Schmidt thus tried, among other things, to differentiate the framework of analysis for the (re-)constructed developments in South America, while the dominant German research tradition on the Lowlands at that time, as mentioned above, was oriented towards other factors: regional distribution, language, material culture (as well as increasingly myths), and physique, with a focal point on the determination of the geographical distribution of linguistically related ethnic groups. In this context, Schmidt endeavored to pay more attention to sociological and economical aspects (according to him: specific rules of filiation and marriage as well as other cultural patterns that served to achieve the goals of land occupation, the acquisition of work forces and the protection of the means of production).<sup>43</sup> Yet, in his work about "The Arawak," these factors took on decisive significance for Schmidt in the dissemination and change of cultures – in the case under study, particularly the Arawak, as well

during a stay on a larger expedition the normal way of life was disturbed and that the economic life came to a pause, for which reason most observers scarcely noticed it, was possibly a jab at von den Steinen (Schmidt, 1922: 442).

**40** Educated as physicians, both von den Steinen und Ehrenreich were initially concerned with physical anthropology; however, in the course of time they distanced themselves ever more from this subject. The study of skulls and body measurements in no way was the determining subject of the times (see Kraus, 2004a: 399-418). A development away from physical anthropology to art-ethnology is discernible elsewhere as well, as shown by Christian Kaufmann with the case of the Oceania specialist Felix Speiser (1880-1949). Speiser also undertook a journey to Brazil in 1924.

**41** On the use of these categories in contemporary discussions, see for example von den Steinen 1886: 323-329; Ehrenreich 1891, Ehrenreich 1897; Vierkandt 1897. For an early critique on von den Steinen's theories, see Baer 1965; for a critique on Schmidt, see in addition Münzel 2004: 436ff.; for current views on settlement history in the Xingu region, see Franchetto and Heckenberger (eds.) 2000.

**42** Therefore, the works of the Americanists mentioned here contradict the widely quoted argument of the US-American historian Andrew Zimmerman (2001: 3; 20; 49-61) that in the early years of ethnological study in Germany indigenous peoples were considered peoples "without culture and history" and that philological and linguistic methods played no decisive role in their study.



as the ethnic groups influenced by them. Against the approaches that focus foremost on language, Schmidt attempted, on the one hand, to consider cultural differences among the different Arawak-speaking ethnic groups and, on the other, to distinguish between the "motifs," the "means" and the "actual nature" of the dissemination.

The fact that Schmidt in his writings, was quick to use interesting initial observations for far-reaching and sometimes questionable conclusions, is shown by another example of how he first detached himself from predominating trends in ethnology in a constructive manner. Theodor Koch-Grünberg rejected any deeper meaning in the Indian rock carvings he discovered on the upper Rio Negro and interpreted them as "playful expressions of a naïve perception of art" (Koch-Grünberg, 1907: 68; 78-79).<sup>44</sup> By contrast, Max Schmidt pointed out a mythological content in the carvings of the Guató and Paresí and presented observations that had possibly gone unnoticed by Koch-Grünberg elsewhere in Brazil. Yet Schmidt went beyond the informative value of his results by attempting to enhance the symbolic contents of these drawings as the discovery of a "primitive pictographic script" (Schmidt, 1917: 70-71) (see Fig. 6).<sup>45</sup>

**43** Sociological concepts are found in places among advocates of the cultural circles theory; yet these theorists made less use of empirical data like strictly observed marriage rules and more of complex abstract constructs like "two-class culture" or "totemism in connection with paternal rights," whose supra-regional use was already strongly criticized by Ehrenreich among others. See also Lowie (1937: 180-185).

**44** Thereby, this theory initially received great acceptance. See Ehrenreich

**Figure 4**

Max Schmidt with a group of Paresí in Hanauinahrtigo at the Juruena river, 1910. (Courtesy: Ethnologisches Museum, Staatliche Museen zu Berlin, No. VIII E 2744)





## CONCLUSION

The extent to which Max Schmidt's theory on the dissemination of culture can still act as a stimulus for research must be judged by current experts on Arawak culture(s).<sup>46</sup> A detailed historical analysis of his book "The Arawak," with respect to his peculiar thoughts on a "drive towards acquisition and subjugation," on the concept of a class of "masters" and "slaves" or on economic inequality as decisive factors for cultural development – as well as the political consequences of such views – is still lacking but warrants a comprehensive study.<sup>47</sup> Certainly noteworthy is his emphasis on a slow and step-by-step process of cultural change in contrast to theories proposing great waves of migrations as well as the concept of cultural mixing as a condoned if not fostered co-product of economic colonialism.

The aim of this article is to place Max Schmidt's work in the context of contemporary ethnological trends in Germany and thereby, alongside the introductory remarks on then developing field research prior to Malinowski, to place his position through a two-fold contextualization in a historical context, markedly reflected in all his works, but most clearly in his monograph, *Die Aruaken. Ein Beitrag zum Problem der Kulturverbreitung*: (1) a critique of diffusionism and cul-

## Figure 5

Plantation of chief Makazore in Hanauinahrtigo at the Juruena river, 1910. (Courtesy: Ethnologisches Museum, Staatliche Museen zu Berlin, No. VIII E 2747)

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(1906) and Vierkandt (1908). For a comparison of contemporary theories on the emergence of art in South America, see Kraus (2000-2001).

**45** Schmidt first formulated his theory in a previous publication (Schmidt, 1914b: 282-83). On the interpretations of paintings on wooden posts and house posts, see the details in Schmidt (1914a: 231-237) and Koch-Grünberg (1967 [1909-10], vol. 2: 240-245). Furthermore, in his critique of evolutionism

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tural circles theory, ever more dominant in the German-speaking world whereby Schmidt, as shown above, was in agreement with other researchers on South America; and (2) the controversy among specialists about the prevailing research focuses on the South American Lowlands. In both respects, Schmidt tried to formulate his own position and move beyond the mainstream. At the same time he consistently strove towards interdisciplinary ties.

This originality - as well as the fact that his great personal engagement in fieldwork was rather unspectacular in comparison to the famed expeditions of his times, due at the very least to the recurrent illnesses during his journeys, - was potentially contributing to the fact that during his lifetime Schmidt was one of the figures on the periphery of German ethnology. The decline of the Americanist research tradition in Germany during both World Wars, as well as Schmidt's emigration, likely fostered this development even more.

Nonetheless, with regard to the time-dependent perception of a scholar – which furthermore must take into consideration his participation in establishing ethnology in Paraguay – no concluding judgment can be made on the originality of his concepts. In sum, as this essay illustrates, the methodical approach, the thematic direction, and the internal discussion of ethnologists at the time, were much more multifaceted than is explicated in many an overview that concentrates on the dominant schools of thought. Andre Gingrich distinguished the group of "historical diffusionists" and the group of "moderate positivists" among ethnologists in the German-speaking world at the beginning of the twentieth century, whereby he assigned Max Schmidt to the latter group. Gingrich concludes: "The moderate positivists did not become as famous, but in retrospect I regard them as the far more interesting group of scholars. In their time, however, they became increasingly marginal as the historical diffusionists gained new hegemony in anthropology in the German language zone" (Gingrich, 2005: 91). For the history of science this reminds us that in historical research, in which one is not only interested in summarizing reviews of dominant contemporary concepts but also in finding stimulating questions, it is well worth the effort to take a fresh look at former investigations beyond the mainstream discussions of a time period.



Schmidt opposed the concept followed by Koch-Grünberg (1905), among others, that in their historical development [adult] Indian drawings could be compared with the drawings of European children (Schmidt, 1920-21, vol. 1: 14; 1924: 40-41.).

**46** For an appreciation of Schmidt's analysis, see Bossert and Villar (2013: 27), who, among other things, conclude that his "hypothesis was ahead of its time because it conceived of the Arawak as a hybrid conglomeration of intermingled, essentially mestizo societies. As such, it departed from the ideal equation according to which: one ethnic group = one territory = one language = one culture (an equation which, be it said in passing, has since been impugned by modern social sciences)." For a critique of Schmidt's theses, see Münzel (2004: 436ff); for a comparative overview of studies on Arawak peoples, see Hill and Santos-Granero (2002).

**47** Here one important line of study is surely the influence of Alfred Vierkandt. Schmidt cites Vierkandt in *Die Aruaken* four times (see his footnotes 60, 96, 163 and 186), whereby he mostly refers to *Die Stetigkeit im Kulturwandel. Eine soziologische Studie* (The Constancy in Cultural Change: A Sociological Study) of 1908. See also footnote 38.

**Figure 6**  
Max Schmidt.  
(Courtesy: Archivo fotográfico Museo Etnográfico Dr. Andrés Barbero in Asunción, Paraguay)

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**Michael Kraus** is curator of the ethnographic collection at the Georg-August-Universität of Göttingen. He studied ethnology, comparative religious studies, and sociology at the universities of Tübingen, Guadalajara, and Marburg. After completing his studies, he worked in various positions at the Philipps-Universität in Marburg, the Deutsches Historisches Museum, the Humboldt-Universität, and the Ethnologisches Museum in Berlin as well as at the Department for the Anthropology of the Americas at the Rheinische Friedrich-Wilhelms-Universität of Bonn. His research focuses on the history of anthropology, the indigenous cultures of Amazonia, visual anthropology, museum studies and curatorial practices, and material culture studies.

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# Theodor Koch-Grünberg (1872-1924): A “Field Ethnologist” and his Contacts with Brazilian Intellectuals

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## Erik Petschelies

🏠 *Universidade Estadual de Campinas | Campinas, SP, Brazil*

✉ *erik.petschelies@gmail.com*

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### ABSTRACT

The German ethnologist Theodor Koch-Grünberg (1872-1924) became one of the world's leading Americanists of his era after having successfully concluded two expeditions to Amazonia. Between 1903 and 1905 he studied indigenous peoples inhabiting the regions of the rivers Rio Negro, Vaupés, and Japurá in northwestern Brazil; between 1911 and 1913 he traveled through northern Brazil and Venezuela investigating local Amerindian communities. He contacted dozens of indigenous peoples, studied their mythology, material culture, and languages. Koch-Grünberg maintained a scientific correspondence with some of the best-informed anthropologists of his time, including Adolf Bastian, Franz Boas, Arnold van Gennep and Paul Rivet. He also exchanged letters with Brazilian colleagues such as João Capistrano de Abreu (1853-1927), Teodoro Sampaio (1855-1937), and Affonso d'Escragnolle Taunay (1876-1958). Through an analysis of primary sources – the correspondence held at the Theodor Koch-Grünberg Archive of the Philipps-Universität Marburg in Germany – this article aims at contributing both to the history of Brazilian social thought and the history of German ethnology by contextualizing these relations within the broader context of social exchanges. Therefore, the history of anthropology should be written in the same way as Koch-Grünberg imagined ethnology: as an international science, based on humanistic principles and grounded on social relations.

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### KEYWORDS

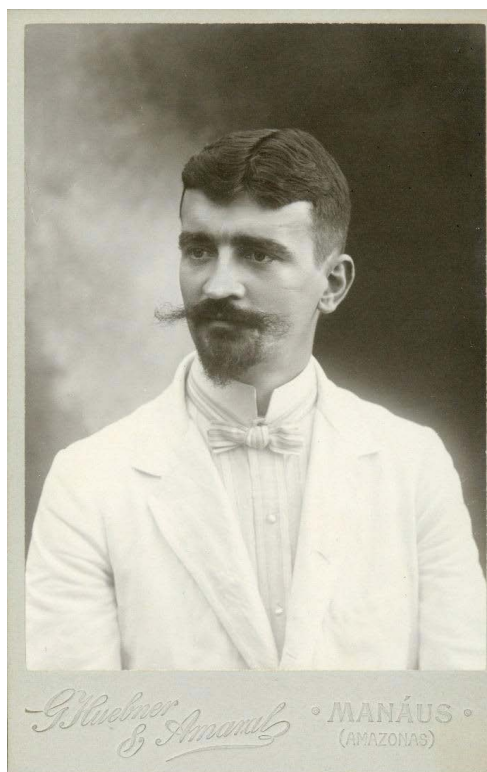
Affonso  
d'Escragnolle Taunay,  
Correspondence,  
João Capistrano  
de Abreu, Teodoro  
Sampaio

## INTRODUCTION<sup>1</sup>

Christian Theodor Koch was born on April 9, 1872 in Grünberg (Germany). He studied classical philology at the University of Tübingen and in 1896 began to teach Latin, Greek, history, and German at a secondary school in his home state Hessen. Two years later, he interrupted his work in order to participate as a volunteer “scientific companion” during the fourth German ethnological expedition to the Xingu river basin in Brazil, led by Herrmann Meyer (Kraus, 2004a: 454-456). In 1901, he resigned from his job as a schoolteacher and became a volunteer research assistant at the Royal Museum of Ethnology in Berlin, under the direction of the most prominent Americanist of his time, Karl von den Steinen (1955-1929). In the following year he earned a doctoral degree at the University of Würzburg with a thesis on Guaicuru languages, based both on linguistic material collected by himself during his Xingu expedition and on that provided by his director, von den Steinen. Between 1903 and 1905 (Fig. 1), he undertook a very successful expedition to the rivers Negro, Içana, Aiari, Japurá, and Uaupés in northern Brazil, where he studied several indigenous peoples, particularly the Baniwa, Tukano, Cubeo, and Yahúna (Kraus, 2004b: 35). After this travel, he added the name of his hometown to his own surname (Kraus, 2004b: 35). Koch-Grünberg worked as a research assistant at the Ethnological Museum in Berlin until 1909, the same year he obtained a habilitation degree from the University of Freiburg. Between 1911 and 1913, he traveled from Mount Roraima, in Venezuela, to the river Orinoco, in northwest Brazil, researching local indigenous groups, such as the Pemon and the Yek’wana. In 1915, he took over the position as scientific director at the Linden Museum in Stuttgart and in 1924, joined the American explorer Hamilton Rice (1875-1956) on an expedition to the Orinoco river basin. At the beginning of this trip, on October 8, 1924, Koch-Grünberg died of malaria in the Brazilian town of Vista Alegre (Kraus, 2004b: 36).

Unlike Karl von den Steinen, Koch-Grünberg had neither been to

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**Figure 1**  
Theodor Koch-Grünberg in  
Manaus (Brazil), 1903.

São Paulo nor Rio de Janeiro, cities home to the Brazilian intellectual elite.<sup>2</sup> Von den Steinen, who undertook pioneer expeditions to the Xingu river basin in 1884 and 1887-88, not only met with governmental authorities but also with Brazilian intellectuals, such as the historian João Capistrano de Abreu (1853-1927) (von den Steinen, 1886: VII) and the engineer, politician, and novelist, Viscount Alfredo d’Escagnolle Taunay (1843-1899) (von den Steinen, 1894: 2). On account of the fact that he only been to northern Brazil, Koch-Grünberg’s sojourns in the country had always been distant from the Brazilian *intelligentsia*.

His first contact with a Brazilian intellectual was in 1913, when Koch-Grünberg sent João Capistrano de Abreu several of his publications.<sup>3</sup> At that time, Koch-Grünberg had already traveled to Brazil three times and maintained a regular exchange of letters with European colleagues who lived in Brazil, such as natural scientists Hermann von Ihering (1850-1930), Emilio Goeldi (1859-1917), Emilie Snethlage (1868-1929), Jacques Huber (1867-1914), and Ernst Ule (1854-1915). Nevertheless, until 1913, his correspondence with Brazilians was limited to traders or to the staff he hired for his expeditions.

The present article is essentially supported by unpublished primary sources, namely, the letters that Koch-Grünberg exchanged with some of his professional colleagues between 1913 and 1924. This correspondence is held in the *Nachlass Theodor Koch-Grünberg* [Theodor Koch-Grünberg Archive] at the Philipps-Universität Marburg (Germany). The main goal of this article is to understand the relationships of Koch-Grünberg with three Brazilian scholars, João Capistrano de Abreu (1853-1927), Teodoro Sampaio (1855-1937), and Affonso d’Escagnolle Taunay (1876-1958), situating these in the context of dramatic social changes, i.e., World War I (1914-1918) and the ensuing economic depression. As a result, two other aims will be set: on one hand, to provide material that contributes both to the history of Brazilian social thought and the history of German speaking ethnology; on the other, to contribute to the discussion of the archive as an ethnographic field by paying attention to the inference of supra-individual events on social relations (Cunha, 2004: 291).

## THE NACHLASS

The Theodor Koch-Grünberg Archive is composed of his personal papers, which include thousands of pictures, field notes, diaries, manuscripts, book reviews, various kinds of drafts, and his “scientific correspondence” comprising of more than 7,500 letters. The German word for this kind of collection is *Nachlass*. There is no perfect translation in English conveying its meaning. Koch-Grünberg’s heirs donated the *Nachlass Theodor Koch-Grünberg* to the Department of Ethnology of Philipps-Universität Marburg in 1999. The ethnolo-

**2** Koch-Grünberg to João Capistrano de Abreu, 6 Jan. 1920, VK Mr A.28, file C.

**3** João Capistrano de Abreu to Theodor Koch-Grünberg, 8 June 1913, VK Mr A.16, file C.

gist himself organized part of it. He separated the correspondence into private and professional sections, and put it in a chronological order. A project led by Mark Münzel and Michael Kraus cataloged the entire *Nachlass*<sup>4</sup> and, in the end, I made an inventory of the scientific correspondence, the *Briefnachlass*, between 2016 and 2018.

The correspondence not only reveals all of Koch-Grünberg’s professional partners and the kind of relationships he had, but also many other aspects, such as ordinary life events like births, marriages, diseases, financial difficulties, travels; but also failed research projects, all types of professional tips concerning expeditions, publications, and Amerindian ethnology, and finally, an abundance of gossip, intrigues, and conflicts.

Therefore, if one takes the *Briefnachlass* as a collection, it is possible to think of it as something like a comprehensive orblike spider web. Every silk line is a social link tied to the center of the web: Koch-Grünberg’s subjectivity. The same way an orb spider web is built from its center, encompassing comprehensive circles and aiming at certain finality, the web of social relations expressed in the archive’s classificatory order is produced from a central point of view. In this sense, by comparing the *Nachlass* to a spider web, the center of which being a specific subjectivity composed by a set of relations, the analytical stance hereby proposed differs from Tim Ingold’s approach. According to Ingold, the threads of a spider web are the material extensions of its body, transforming the spider and its environment into a fluid space in which forces compose all things (Ingold, 2001). In other words, Ingold (2001) proposed an anthropological approach in which connections do not exist, there are only lines. The spider and the web share the same essence. In the present article, however, by taking the *Nachlass* as a relational web, the approach is historical and sociological: the connections do exist and are the main concern.

By thinking of the *Briefnachlass* as a web, the focus of the present analysis migrates from a single set of relations between two correspondents to a group or to patterns of relations, such as those that Koch-Grünberg established with Brazilian intellectuals. Letters are therefore the materialization of social relations seen from a personal, subjective point of view. Letters are also marked by a centripetal force, attracting symbols and expressions of the writer’s subjectivity. There are hundreds of correspondents, which necessarily constitute the *Nachlass* as a polyphonic field, but the voices should be understood according to the relation they had to Koch-Grünberg. This requires that the ethnologist or historian take a critical approach that carefully examines the discourses produced from the center of the web of relations. By taking the *Briefnachlass* as a web of relations and meanings built according to a central perspective, this article aims to contribute to approaches that

<sup>4</sup> Available at:  
[https://www.uni-marburg.de/fb03/ivk/vk/forschung/vkprojekte/koch/index\\_html](https://www.uni-marburg.de/fb03/ivk/vk/forschung/vkprojekte/koch/index_html). Accessed on 15 July 2018.

consider archives as ethnographic fields.

## THE ENCOUNTER

In the first letter João Capistrano de Abreu wrote to Koch-Grünberg, in 1913, he told him that two Amerindians were teaching him their language.<sup>5</sup> Capistrano de Abreu was probably referring to his two informants, the Kaxinawá Indians Tuxinĩ and Borô, with the help of whom he wrote his famous book *Rã-txa hu-ni-kuĩ*, published the following year (Christino, 2006: 123).

Born in Maranguape, Ceará (Brazil), on October 23, 1853, Capistrano de Abreu studied humanities and law in Recife, but never earned a degree (Fig. 2). In 1875, he moved to Brazil’s capital Rio de Janeiro, where he began to work as a journalist and librarian at the *Biblioteca Nacional* (National Library). He dedicated his lifework to the study of colonial Brazil, based on rigorous source investigation and critical reviews of contemporary historians (Gontijo, 2010: 16-19). In the 1890s he began to study indigenous languages, a task he carried out for the rest of his life (Gontijo, 2010: 23). *Rã-txa hu-ni-kuĩ – a língua dos caxinauás do rio Ibuacu, afluente do Murú (Prefeitura de Tarauacá)* [rã-txa hu-ni-kuĩ – The Language of the Kaxinawá from the Ibuacu River, Tributary of the Murú River (Municipality of Tarauacá)] consists of an interlinear translation of sentences from this Pano language into Portuguese, as well as a grammatical and linguistic analysis. His book was highly influenced by Karl von den Steinen’s book on a Carib language, *Die Bakairi-Sprache* (The Bakairi Language) (1892). Abreu knew von den Steinen personally, read his books and even translated some of his writings. As Beatriz Christino has shown, Capistrano de Abreu was definitely part of a European Americanist network (Christino, 2006: 77).

Their next contact happened only in the following year, a few weeks before World War I began.<sup>6</sup> The First World War was devastating to German science, as Koch-Grünberg’s correspondence with his colleagues reveals. In addition to the human suffering imposed by the war, science was



**5** João Capistrano de Abreu to Theodor Koch-Grünberg, 8 June 1913, VK Mr A. 16, file C.

**Figure 2**  
João Capistrano de Abreu.

**6** João Capistrano de Abreu to Theodor Koch-Grünberg, 4 June 1914, VK Mr A. 16, file C.



severely affected by the lack of money,<sup>7</sup> personnel,<sup>8</sup> and even paper,<sup>9</sup> as well as censorship.<sup>10</sup>

The war also interrupted communication between colleagues living in countries against which Germany was at war,<sup>11</sup> such as Paul Rivet (1876-1958) in France, but also those living overseas, such as Curt Unkel Nimuendajú (1883-1945) in Brazil, due to the interruption of transatlantic communications caused by the war.<sup>12</sup>

In a letter to Capistrano de Abreu, Nimuendajú, a Brazilian ethnologist of German descent requested copies of writings from his Brazilian colleagues. Nimuendajú also inquired about expeditions led by Cândido Mariano da Silva Rondon (1865-1958), a well-known military officer, who was part of the *Comissão Telegráfica* (Telegraphic Commission), an expedition to the Brazilian hinterland tasked with building telegraph lines and contacting indigenous peoples.<sup>13</sup> A few weeks later, the correspondence between Koch-Grünberg and Theodoro Sampaio began.

Theodoro Fernandes Sampaio was born on January 7, 1855 as the son of an enslaved woman with a local priest (Fig. 3). His father took him to Rio de Janeiro, where he studied engineering and obtained a post as draftsman at the National Museum (Costa, 2007: 38-40). He participated in two expeditions through the Brazilian hinterland (1878-1880 and 1886) and was involved in the investigation of Brazilian history and indigenous ethnography and linguistics. In his correspondence with Koch-Grünberg, Sampaio stated that he was interested in Koch-Grünberg’s studies of the indigenous languages spoken in the region of the Orinoco and Amazon rivers, since he himself was studying “Brazilian languages”.<sup>14</sup> He made it clear that he contacted Koch-Grünberg by Abreu’s mediation. Sampaio told Koch-Grünberg that he was writing an article called *Os naturalistas viajantes dos séculos XVIII e XIX e do progresso da ethnographia indígena no Brasil* (The Traveling Naturalists of the 18th and 19th Centuries and the Progress of Indigenous Ethnography in Brazil), which Koch-Grünberg immediately requested a copy in his response. In that letter, Koch-Grünberg also praises another written work from Sampaio, *Os Kraôs do Rio Preto no Estado da Bahia* (The Krahô of the Preto River in the State of Bahia), which he had read in the *Revue de la société des Américanistes* that Rivet had sent him enclosed in one of the last letters that they exchanged during the war. Koch-Grünberg confided to his Brazilian colleague that, “the connections with my Brazilian friends and colleagues are now especially valuable to me, since this horrible war has interrupted so many of our beautiful science’s international bonds.”<sup>15</sup> Capistrano de Abreu played a key role in Koch-Grünberg’s connections with scholars in South America.

In a letter Koch-Grünberg sent to his colleague, Walter Lehmann (1878-1939), the following statement is even clearer in this respect:

**7** Theodor Koch-Grünberg to Alberto Frič, 13 April 1916, VK Mr A. 20, file F.

**8** Theodor Koch-Grünberg to Erland Nordenskiöld, 3 Dec. 1915, VK Mr A. 19, file N.

**9** Eugen Dietrichs Verlag to Theodor Koch-Grünberg, 18 Dec. 1916, VK Mr A. 20, file D.

**10** Theodor Koch-Grünberg to Hermann Schmidt, 10 May 1917, VK Mr A23, file Sch.

**11** Theodor Koch-Grünberg to Erland Nordenskiöld, 8 Dec. 1919, VK Mr A. 27, file NO.

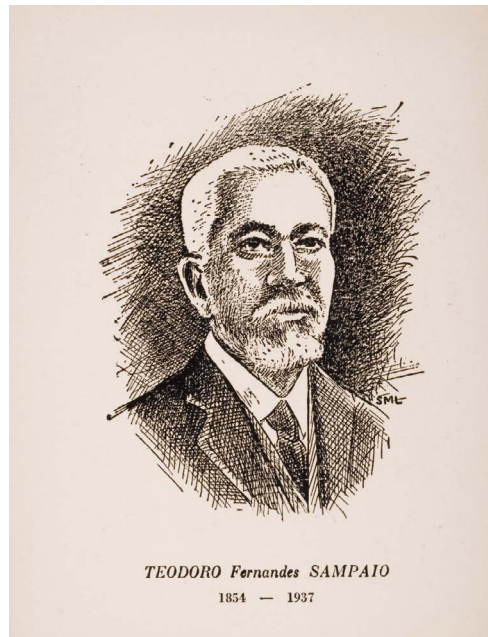
**12** Theodor Koch-Grünberg to Alberto Frič, 13 April 1916, VK Mr A. 20, file F.

**13** João Capistrano de Abreu to Theodor Koch-Grünberg, 4 June 1914, VK Mr A. 16, file C.

**14** As Sampaio wrote in Portuguese: “línguas Brasileiras.” Theodoro Sampaio to Theodor Koch-Grünberg, 1 Aug. 1914, VK Mr A. 17, file S.

**15** In German: “Die Verbindung mit meinen brasilian. Freunden und Kollegen ist mir jetzt besonders wertvoll, da der furchtbare Krieg so viele internationale Beziehungen in unserer schönen Wissenschaft unterbrochen hat.” Theodor Koch-Grünberg to Theodoro Sampaio, undated, VK Mr A. 17, file S.

*I now have more connections to South American colleagues than before the war without an effort from my part. Almost every parcel from there brings me valuable books and writings and friendly letters, especially from Brazil, Argentina, Paraguay, and Ecuador. It is different with the USA. The gentlemen there behave straightforwardly stupid.*<sup>16</sup>



**Figure 3**

Teodoro Sampaio.

At the end of his first letter to Sampaio, Koch-Grünberg expressed hope in finding more intellectuals in Brazil, such as Sampaio and Capistrano de Abreu, “dedicated to the interesting study of the natives, before it is too late.”<sup>17</sup> This sentence conveys one of Koch-Grünberg’s main concerns, namely the disappearance of indigenous peoples, either due to the massacres, diseases, or by the damaging contact with European culture. By means of genocide and through contact with Europeans and Brazilians, indigenous societies would disappear and with them, their cultural properties. Ever since Adolf Bastian (1826-1905), the physician, anthropologist, traveler, founder of the Museum of Ethnology in Berlin, and “founding father of modern ethnology in Germany” (Vermeulen, 2015: 242), German ethnologists were actively engaged in so-called “salvage ethnography.” Therefore, ethnologists sought for the most isolated native peoples possible, in order to not only to study their culture but to also form large ethnographic collections. Bastian had stated that it would be possible to deduct the *Elementargedanken* (elementary thoughts) of the people that made ethnographic objects. The main goal of Bastian’s ethnological program was the formation of an inventory of the elementary thoughts of all peoples of all times, the *Gedankenstatistik* (thought statistics) (Koepping, 1983: 84-88). Thus, contact with colleagues living in South America was not only important for supplying him with articles or books that he could not obtain in Europe during a time of war, but also for acquiring firsthand information, such as ethnographic notes and indigenous vocabularies. Koch-Grünberg’s correspondence with Curt Nimuendajú and Father Constant Tastevin (1880-1962), among many others, reveals this in detail. In this connection, the epistemological value of Abreu’s work also shows itself: its reading is complicated, but there are “magnificently original things.”<sup>18</sup>

Between 1915 and 1920, Koch-Grünberg received only two letters from Brazil-

**16** “Mit den südamerikanischen Kollegen habe ich jetzt mehr Verbindung als vor dem Krieg, ohne mein Zutun. Fast jede Post bringt mir von dort wertvolle Bücher und Schrifte und freundliche Briefe, so besonders aus Brasilien, Argentinien, Paraguay, Ecuador. Anders ist es mit den USA. Die Herren dort benehmen sich direkt blödsinnig.” (Theodor Koch-Grünberg to Walter Lehmann, 7 Dec. 1920, VK Mr A. 29, file L).

**17** “Möchten sich doch in Brasilien recht viele Gelehrte finden, die sich, wie Sie und Dr. J. Capistrano de Abreu, dem interessantesten Studium der Ureinwohner widmen, bevor es zu spät ist.” Theodor Koch-Grünberg to Teodoro Sampaio, undated, VK Mr A. 17, file S.

**18** “Es sind prächtig ursprüngliche Sachen, aber man muss sich erst hineinlesen, da es fast nur Interlinearübersetzungen gibt.” Theodor Koch-Grünberg to Walter Lehmann, 24 March 1919, VK Mr A. 27, file L.

ian intellectuals, both written by Teodoro Sampaio. On January 16, 1915, Sampaio thanked Koch-Grünberg for the articles he had sent him and informed him that communication with European colleagues was becoming more and more difficult because of the war.<sup>19</sup> On July 15, 1915, Sampaio thanked Koch-Grünberg for the articles about Pemon Indians’ magical speeches and expressed his desire to proceed with his studies of non-Tupi indigenous peoples, “but there are many difficulties and I cannot travel all the time to contact those savage peoples.”<sup>20</sup>

During World War I, an anti-German sentiment flourished in countries within and outside Europe, including Argentina and Brazil. As historians H. Glenn Penny and Stefan Rinke note about the German Brazilians, “because of the power of that anti-German discourse, many ethnic Germans saw the professional and economic advantages they had gleaned from their ethnicity before 1914 transformed into serious disadvantages” (Penny and Rinke, 2015: 185). Koch-Grünberg received news about the anti-German hatred that his friends in Brazil were suffering even after the war. In 1915 the German ethnologist and folklorist Robert Lehmann-Nitsche residing in La Plata (Argentina) wrote to Koch-Grünberg that the “German hatred is producing wild effects here.”<sup>21</sup> Curt Nimuendajú also reported the rising prejudice against Germans in Brazil<sup>22</sup> and Mrs. Huber, Jacques Huber’s wife, suspected that English civilians set up Brazilians against Germans.<sup>23</sup> Hence, the anti-German sentiment that part of Brazilian society manifested became an important issue in the Brazilian-German relations.

Although opposing the war and any kind of nationalism,<sup>24</sup> Koch-Grünberg was recruited in August 1917, and served as a cartographer in Ulm.<sup>25</sup> There Koch-Grünberg suffered a severe malaria attack and was taken to a field hospital and transferred to a hospital in Stuttgart, where he stayed until November. In January 1918, Koch-Grünberg was ordered to go to Stuttgart and began to work on military maps, “a type of work that was more appealing” to him “than the spirit-killing barrack service.”<sup>26</sup> As he was 45 years old, the father of three children, suffered from chronic malaria and had an important post as scientific director at the Linden Museum in Stuttgart, Koch-Grünberg used his contacts in order to be relieved from military service. He appealed to the board of the *Württembergischer Verein für Handelsgeographie* (Württemberg Association for Trade Geography), the institution managing the museum. Through the influence of the director’s brother, Count Karl von Urach (1864-1928), head of the house of Württemberg (Claußen, 2015: 208) and highly influential in Germany – during World War I von Urach almost became Prince of Albania and King of Poland – Koch-Grünberg was dismissed from military service in May 1918 and resumed his post at the Linden Museum.<sup>27</sup>

In 1920, Koch-Grünberg recommenced his correspondence with Brazilian colleagues. To his friend, Clemens Brandenburger (1879-1947), Koch-Grünberg confided:

**19** Teodoro Sampaio to Theodor Koch-Grünberg, 16 Jan. 1915, VK Mr A. 19, file S.

**20** As it states in Portuguese: “[...] mas dificuldades são muitas, e eu nem sempre posso viajar até me por em contacto com aquella gente selvagem.” Teodoro Sampaio to Theodor Koch-Grünberg, 15 July 1915, VK Mr A. 17, file S.

**21** “Der Deutschenhass treibt hier tolle Blüten.” Robert Lehmann-Nitsche to Theodor Koch-Grünberg, 21 April 1915, VK Mr A. 19, file L.

**22** Curt Nimuendaju to Theodor Koch-Grünberg, 28 Dec. 1921, VK Mr A. 33, file N.

**23** Frau Huber to Theodor Koch-Grünberg, 1 June 1920, VK Mr A. 33, file N

**24** Theodor Koch-Grünberg to Hermann Schmidt, 1 June 1920, VK Mr A. 29, file Sch.

**25** Theodor Wanner to Theodor Koch-Grünberg, 21 Aug 1917, VK Mr A. 23, file W.

**26** “[...] einer Arbeit, die mir mehr zusagt als der geisttötende Kasernendienst.” Theodor Koch-Grünberg to “Euer Excellenz,” 5 Feb. 1918, VK Mr A. 24, Akte B.

**27** Theodor Koch-Grünberg to Fürst Carl von Urach, 21.05.1918, VK Mr A. A12, file *Briefentwürfe an Herzog Wilhelm und Fürst Carl von Urach u.a.*

**28** “Mit dem trefflichen Capistrano de Abreu stehe ich seit Jahren vor dem Krieg und neuerdings wieder in eifrigem Briefwechsel. Sein Werk über die Kaschinawa ist hervorragend und fast eigenartig für

*I have been in assiduous correspondence with the excellent Capistrano de Abreu for years before the war and lately again. His work on the Kaschinawa is outstanding and almost unique to South America.*<sup>28</sup>

That same year, Koch-Grünberg and Capistrano de Abreu exchanged articles and books authored by themselves<sup>29</sup> and by other Brazilian intellectuals, like the physical anthropologist from Rio de Janeiro, Edgar Roquette Pinto (1884-1954).<sup>30</sup> They also discussed Rondon’s expedition and its results,<sup>31</sup> as well as their own works.<sup>32</sup>

In 1920, Koch-Grünberg published a book about indigenous mythology, *Indianermärchen aus Südamerika* (Fairy Tales from South America), which contains some myths from the Kaxinawá that Koch-Grünberg extracted and translated from “the excellent and extensive work by the Brazilian Capistrano de Abreu on the fairy world of the Kaschinawa, an Indian tribe in western Brazil” (Koch-Grünberg, 1920: III).<sup>33</sup> Koch-Grünberg sent this book<sup>34</sup> to Capistrano de Abreu with two others as a sign of his “admiration and respect.”<sup>35</sup> He recommended Abreu’s book to his colleagues,<sup>36</sup> advised research institutes to invite Abreu to collaborate,<sup>37</sup> and published a book review of Abreu’s Bakairi study in the journal *Anthropos*.<sup>38</sup> Few intellectuals were as highly estimated in Koch-Grünberg’s *Briefnachlass* as Capistrano de Abreu. Nevertheless, Abreu’s work was recognized by German ethnologists, not only because of its epistemological value (residing in the interlinear translation, the presentation of myths and tales in Kaxinawá language, and the grammatical analyses), but also because it reflected the extension of the German ethnological program in South America. Capistrano de Abreu applied Karl von den Steinen’s method in his own writings, translated articles written by von den Steinen and Ehrenreich, and acted as a promoter of German ethnology. This meant that, by praising Abreu’s work, Koch-Grünberg was acknowledging the success of German Americanist ethnology of which he was a main agent, at least after the publication of the book *Zwei Jahre unter den Indianern Nordwestbrasilens* (Two Years among the Indians of Northwest Brazil), detailing his travel experiences between 1903 and 1905.

During the war, Koch-Grünberg began to express a desire to return to Brazil. He wanted to study the mythology of the Baniwa Indians of the Rio Negro,<sup>39</sup> to travel to Rio de Janeiro, and to visit his friends.<sup>40</sup> The suffering imposed by the war had a very negative influence on Koch-Grünberg’s evaluation of European culture, since he was a staunch pacifist.<sup>41</sup> He deemed the war to be an “insanity”<sup>42</sup> and shared Alberto Frič’s “contempt for the haughty and in reality so cultureless white humanity.”<sup>43</sup> The ordinary German population impoverished rapidly due to the severe economic crisis and monetary inflation resulting from the war, which according to Koch-Grünberg had “demoralized them and squeezed them like a lemon.”<sup>44</sup>

Koch-Grünberg’s salary during the 1920s corresponded to only 30% of the

Südamerika.” Theodor Koch-Grünberg to Clemens Brandenburger, 13 July 1920, VK Mr A. 28, file B.

**29** Capistrano de Abreu to Theodor Koch-Grünberg, 20 Oct. 1920, VK Mr A. 28, file C.

**30** Theodor Koch-Grünberg to Capistrano de Abreu, 25 Nov. 1920, VK Mr A. 28, file C.

**31** Theodor Koch-Grünberg to Capistrano de Abreu, 25 Nov. 1920, VK Mr A. 28, file C.

**32** Theodor Koch-Grünberg to Capistrano de Abreu, 13 Feb. 1920, VK Mr A. 28, file C.

**33** In German: “[...] das ausgezeichnete, umfangreiche Werk des Brasilianers Capistrano de Abreu, über die Märchenwelt der Kaschinawa, eines Indianerstammes im westlichen Brasilien [...]”

**34** Theodor Koch-Grünberg to Capistrano de Abreu, 19 Aug. 1920, VK Mr A. 28, file C.

**35** As it states in the German source: “Bewunderung und Verehrung.” Theodor Koch-Grünberg to Capistrano de Abreu, 06 Jan. 1920, VK Mr A. 28, file C.

**36** Erland Nordenskiöld to Theodor Koch-Grünberg, 11 June 1919, VK Mr A. 27, file NO.

**37** Theodor Koch-Grünberg to Dr. B. Schädel, 09 Nov. 1915, VK Mr A. 19, file H.

**38** Theodor Koch-Grünberg to Clemens Brandenburger, 13 July 1920, VK Mr A. 28, file B.

**39** Theodor Koch-Grünberg to Alberto Frič, 13 April 1916, VK Mr A. 20, file F.

value he had earned before the war.<sup>45</sup> Daily life became harder and harder, so much that at several points Koch-Grünberg needed to accept donations from friends living abroad, such as the Swedish ethnologist Erland Nordenskiöld (1877-1932)<sup>46</sup> and the Dutch navy officer and cartographer C.H. de Goeje (1879-1955),<sup>47</sup> who provided him with coffee, chocolate, cheese, butter, sugar, milk, peas, and other groceries. In a letter thanking for a package he had received from Robert Hoffert, Koch-Grünberg wrote: “Concerning this excellent coffee, you have probably thought rather of me the ‘Old Brazilian,’ who was too often deprived of this stimulating beverage during the long period of war.”<sup>48</sup>

In order to improve his income, Koch-Grünberg undertook a series of lecture tours: in 1922 and 1923 he went to Switzerland and between March 20 and April 16, 1924, Koch-Grünberg gave 16 talks in various cities in central Germany.<sup>49</sup> Delivering lectures in scholarly associations was an ordinary intellectual practice, but doing it at this intensity and especially at this frequency only began after the war.

Moreover, after World War I Koch-Grünberg realized that the means obtainable in Germany were too scarce for him to organize another expedition or even to publish the last two of his five-volume monograph *Vom Roraima zum Orinoco* (From Roraima to Orinoco). Therefore, he tried to use his network of scientific contacts in order to obtain foreign funding. In addition to the Swiss lecture tour, Koch-Grünberg attempted to organize another through Sweden<sup>50</sup> and applied for funding from the *Emergency Society for German & Austrian Science and Art*, established by Franz Boas in the USA, in order to publish two books.<sup>51</sup> Koch-Grünberg encouraged his friends to request funding outside Germany. When, for instance, Nimuendajú wrote to him that he was working for a Swedish museum, Koch-Grünberg advised: “Make sure that you are well paid for your affairs! The Swedes can do it with their krona.”<sup>52</sup>

Therefore, one should understand the relationships with Brazilian intellectuals within this contextual paradigm shift: in less than forty years, the German Americanist ethnology saw its apogee and its decline. Especially during the 1920s, German Americanists were struggling to put in practice the research program they had established before the war. However, this program depended on abundant funds that could no longer be found anywhere in Europe and was based on theoretical problems that most anthropologists outside of Germany had already rejected.

In 1922, the Brazilian government celebrated the centenary of independence from Portugal with several events: the *Museu Paulista* in São Paulo obtained new collections concerning Brazilian history, Rio de Janeiro hosted the world exposition as well as the International Congress of Americanist, and numerous civic commemorations took place. Being aware of his leading role in the study of Brazilian Amerindians, Koch-Grünberg cherished the hope of finally returning

**40** Theodor Koch-Grünberg to Capistrano de Abreu, 6 Jan. 1920, VK Mr A. A28, file C.

**41** Theodor Koch-Grünberg to Hermann Schmidt, 1 June 1920, VK Mr A. 29, file Sch.

**42** “Es ist wirklich höchste Zeit, dass dieser Wahnsinn aufhört.” Theodor Koch-Grünberg to Richard Karutz, 24 April 1917, VK Mr A. 23, file K.

**43** In German: “Ihre Verachtung für die hochmütige und in Wahrheit doch so kulturlose weisse Menschheit kann ich vollkommen nachfühlen!” Theodor Koch-Grünberg to Alberto Frič, 13 April 1916, VK Mr A. 20, file F.

**44** As Koch-Grünberg wrote: “[Krieg], der das Volk entnervt, demoralisiert und ausgepresst hatte wie eine Zitrone.” Koch-Grünberg to Hermann Schmidt, 1 June 1920, VK Mr A. 29, file Sch.

**45** Theodor Koch-Grünberg to Alfredo Jahn, 17 Dec. 1923, VK Mr A. 35, file IJ.

**46** Erland Nordenskiöld to Theodor Koch-Grünberg, 20 May 1919, VK Mr A. 27, file NO.

**47** C.H. de Goeje to Theodor Koch-Grünberg, 28 Dec. 1920, VK Mr A. 30, file G.

**48** “Bei dem ausgezeichneten Kaffee haben Sie wohl in erster Linie an mich alter Brasilianer gedacht, der diesen anregenden Trank während der langen Kriegszeit oft genug schmerzlich entbehren musste.” Theodor Koch-Grünberg to Robert Hoffert, undated 1919, VK Mr A. 26, file H.

to Brazil,<sup>53</sup> after receiving an official invitation and funds from the Brazilian government in order to participate in these commemorations.<sup>54</sup>

Koch-Grünberg thought that because of the centenary festivities the Brazilian government would bring indigenous peoples to Rio de Janeiro.<sup>55</sup> As Christino argued that if had been the case, Koch-Grünberg could have undertaken linguistic studies even by merely sojourning in the city (Christino, 2007: 57). Subsequently, Koch-Grünberg wrote to his friend Father Adalbert Kaufmehl, who lived in Rio, about the possibilities of obtaining an official invitation from the Brazilian government.<sup>56</sup> Thus, Koch-Grünberg mobilized his network in Brazil to help him obtain funding. He paid for the subscription and even received the membership card (Fig. 4), still expecting financial aid from the Brazilian government.<sup>57</sup>

As Paul Rivet stated in a letter to Koch-Grünberg that no European ethnologist had the means to travel to Rio in order to participate in the Americanist Congress,<sup>58</sup> therefore Brazilian funds were the only possibility for attending the event. Eight months later, Kaufmehl wrote to Koch-Grünberg that himself, Capistrano de Abreu, and Affonso d’Escragnolle Taunay (1876-1958) were seriously engaged in obtaining financing for Koch-Grünberg. “Capistrano Abreu,” reported Kaufmehl, “has personally had several meetings with ministers and other officials of the federal government; however, the answer was always: ‘there are no funds available for this.’”<sup>59</sup>

Furthermore, neither Koch-Grünberg nor any other German ethnologist was invited to the congress.<sup>60</sup> Koch-Grünberg complained to his friend Clemens Brandenburger, quoting Nordenskiöld: “In Rio they don’t seem to have any idea about our existence.”<sup>61</sup> Nordenskiöld was surprised when he learned that he had received an invitation and Koch-Grünberg had not,<sup>62</sup> since, in his opinion, no one had done more for the investigation of Brazil than Koch-Grünberg.<sup>63</sup> Koch-Grünberg certainly thought that the influence of his few intellectual friends and local prominent citizens in far northern Brazil would be enough to convince the national government to pay for his trip. However, what Koch-Grünberg did not realize was that the centenary celebration intended to present a positive image of Brazil, and that a European intellectual who exposed cruelties committed against indigenous peoples, criticized regional political elites and Western civilization in general, would not be very welcome. Nimuendajú understood Koch-Grünberg’s rejection in the context of the anti-German sentiment in Brazil: “I believe that the Brazilians have



**Figure 4**  
Koch-Grünberg’s membership card.

**49** Gustav Hennig to Theodor Koch-Grünberg, 1 Feb. 1924, VK Mr A. 37, file QR.

**50** Theodor Koch-Grünberg to Erland Nordenskiöld, 14 Aug. 1922, VK Mr A. 33, file N.

**51** Franz Boas to Theodor Koch-Grünberg, 09 April 1921, VK Mr A. 30, file B.

**52** As it stands in the German source: “Lassen Sie sich die Sachen nur recht gut bezahlen! Die Schweden können es mit ihren Kronen.” Theodor Koch-Grünberg to Curt Nimuendajú, 19 Sept. 1922, VK Mr A. 33, file N.

**53** Theodor Koch-Grünberg to Don Germano Garrido y Otero, 27 Nov. 1920, VK Mr A. 28, file G.

**54** Theodor Koch-Grünberg to Paul Rivet, 27 Nov. 1920, VK Mr A. 29, file R.

**55** Theodor Koch-Grünberg to Paul Rivet, 27 Nov. 1920, VK Mr A. 29, file R.

**56** Theodor Koch-Grünberg to Adalbert Kaufmehl, 31 Aug. 1920, VK Mr A. 29, file K.

**57** Mitgliedskarte. VK Mr A. 32, file A.

**58** Paul Rivet to Theodor Koch-Grünberg, 20 Nov. 1920, VK Mr A. 31, file R.

**59** “Capistrano Abreu hat persönlich mit mehreren Ministern der Bundesregierung Rücksprache gehabt, allein aller Bescheid war immer: ‘não



no interest in the participation of German scholars in the conference.” After Germany’s defeat in the war, to which Brazilians believed they contributed to, “people have gotten used trampling on us.”<sup>64</sup>

At about the same time, another possibility of travel to Brazil arose. In 1920, Koch-Grünberg managed to establish communication with his Brazilian colleagues and among them being Affonso d’Escragnolle Taunay (1843-1899) (Fig. 5). Taunay’s biography contrasted with that of Teodoro Sampaio and Capistrano de Abreu. Sampaio and Abreu had been free black men during the slavery regime in Brazil. Abreu was an autodidact and Sampaio had to buy the freedom of his brothers. By contrast, Taunay was the son of the Viscount Alfredo d’Escragnolle Taunay (who had met Karl von den Steinen) and a member of a wealthy French family that had migrated to Brazil in 1816. Like his father, Affonso d’Escragnolle Taunay studied engineering before turning to the humanities. Abreu was a friend and his mentor: he encouraged Taunay to study the history of São Paulo province (Oliveira Junior, 1994: 10-14).

In 1917, Taunay became director of the *Museu Paulista* and was the main intellectual in charge of transforming it from a naturalist institution into a historiographical museum. In this process, he and Koch-Grünberg exchanged materials for approximately one year, beginning in February 1920.<sup>65</sup> In April 1921, Taunay asked Koch-Grünberg if he would accept to undertake an expedition to the Doce River; the *Museu Paulista* would cover the expenses.<sup>66</sup> This territory was not entirely *terra incognita*, as German ethnologists would have preferred, but the last German expedition to this region had been in 1884, led by Paul Ehrenreich (Ehrenreich, 1887). In almost 40 years, no German ethnologist had studied the Botocudo Indians, so this trip could provide meaningful material. Koch-Grünberg joined the project with enthusiasm. Since he had to quit his job, he needed 6,000 Mark monthly in order to support his wife and children in Germany, as well as funds to buy equipment, and cover travel expenses.<sup>67</sup> In June 1921, Taunay answered that the Brazilian government would transfer these resources to Koch-Grünberg’s bank account in Germany and that he had reported to the Minister of the Interior about the ethnologist’s decision.<sup>68</sup> In September, Taunay informed him that the Minister had decided that the expedition should wait until a better financial moment<sup>69</sup> and in December, he wrote saying that the expedition needed to be postponed until 1923, due to the preparations for the independence celebrations.<sup>70</sup> Six months later the project was definitely abandoned, burying Koch-Grünberg’s hopes for traveling to Brazil in 1922.<sup>71</sup>

Koch-Grünberg’s contacts with other Brazilian intellectuals and institutions were not successful either. He tried to engage in a relationship with them by sending books, articles, and requesting their publications. He sent, for

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ha verba para isso.” Adalberto Kaufmehl to Theodor Koch-Grünberg, 10 April 1921, VK Mr A. 30, file K.

**60** Franz Heger to Theodor Koch-Grünberg, 15 Oct. 1923, VK Mr A. 35, file H.

**61** “Man scheint in Rio von unserer Existenz gar keine Ahnung zu haben.” Theodor Koch-Grünberg to Clemens Brandenburger, 27 Dec. 1921, VK Mr A. 29, file V.

**62** Erland Nordenskiöld to Theodor Koch-Grünberg, 15 May 1922, VK Mr A. 33, file N.

**63** Erland Nordenskiöld to Theodor Koch-Grünberg, 23 May 1922, VK Mr A. 33, file N.

**64** “Ich glaube es liegt den Brasilianern auch gar nichts an der Teilnahme deutscher Gelehrter am Congress.” “Jetzt hat man sich daran gewöhnt uns mit Füßen zu treten.” Curt Nimuendaju to Theodor Koch-Grünberg, 28 Dec. 1921, VK Mr A. 33, file N.

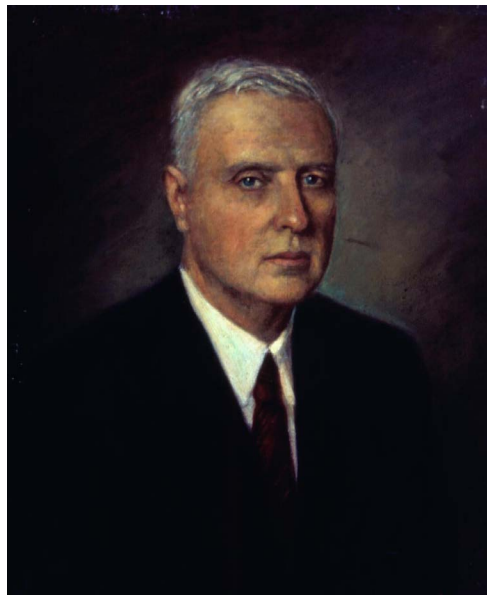
**65** Affonso d’Escragnolle Taunay to Theodor Koch-Grünberg, 20 Feb. 1920, VK Mr A. 29, file T.

**66** Affonso d’Escragnolle Taunay to Theodor Koch-Grünberg, 7 April 1921, VK Mr A. 31, file S.

**67** Theodor Koch-Grünberg to Clemens Brandenburger, 23 May 1921, VK Mr A. 31, file S.

**68** Affonso d’Escragnolle Taunay to Theodor Koch-Grünberg, 25 June 1921, VK Mr A. 31, file S.

instance, some of his material to Roquette Pinto, Mariano Rondon, and the *Instituto Histórico e Geográfico* (Geographical and Historical Institute); but never received an answer from them.<sup>72</sup> He wondered whether they had received his parcels, so he wrote to them several times. He asked his Brazilian friends to obtain news that never came.<sup>73</sup> After realizing that the Mausean *triple obligation* he had mobilized many times in order to establish relationships with intellectuals from other nationalities and indigenous peoples was no longer working in Brazil, he felt that the authorities and institutions, as well as many scholars, were treating him unfairly, especially after he had dedicated 25 years of his life studying that country. Koch-Grünberg openly confided his frustration in a letter to his colleague Dr. O. Quelle:



**Figure 5**

Affonso d'Escragnolle Taunay.

*I even own two copies of “Rondonia,” not from the author [Roquette-Pinto], to whom I wrote in vain at the time, but from my old friend Dr. João Capistrano de Abreu, a very serious Brazilian scholar, and my colleague in São Paulo, Dr. Affonso d'Escragnolle Taunay, director of the Museu Paulista. Other than that, I have been treated in a rather negligent way by the Brazilians, whom I have been working for many years.*<sup>74</sup>

In May 1923, the north-American geographer Hamilton Rice invited Koch-Grünberg to participate in an expedition to Brazil<sup>75</sup>; two months later, they began to plan the trip.<sup>76</sup> In January 1924, Koch-Grünberg was dismissed from his post as scientific director at the Linden Museum in Stuttgart due to the financial situation of the association funding the museum.<sup>77</sup> During this month, he tried to obtain a professorship in ethnology at Heidelberg University, where he had been giving lectures since 1915,<sup>78</sup> but he did not obtain this position.<sup>79</sup> Nevertheless, Koch-Grünberg was set to land a position in Berlin, either as general director of the Museum of Ethnology or as professor at the University of Berlin or maybe even both.<sup>80</sup> His position would be discussed with the government after Koch-Grünberg's return from the Rice expedition.<sup>81</sup> Sadly, at the beginning of this travel, Koch-Grünberg died of malaria in Vista Alegre, northern Brazil, on October 8, 1924 (Kraus, 2004b: 36).

**69** Affonso d'Escragnolle Taunay to Theodor Koch-Grünberg, 14 Sept. 1921, VK Mr A. 31, file S.

**70** Affonso d'Escragnolle Taunay to Theodor Koch-Grünberg, 17 Dec. 1921, VK Mr A. 31, file S.

**71** Theodor Koch-Grünberg to Affonso d'Escragnolle Taunay, 21 June 1922, VK Mr A. 33, file T.

**72** Theodor Koch-Grünberg to Capistrano de Abreu, 8 May 1920, VK Mr A. 28, file C.

**73** Theodor Koch-Grünberg to Capistrano de Abreu, 13 Feb. 1920, VK Mr A. 28, file C.

**74** “Von der ‘Rondonia’ besitze ich sogar zwei Exemplare, nicht vom Verfasser, an den ich mich seinerzeit vergeblich wandte, sondern von meinem alten Freunde Dr. João Capistrano de Abreu, einem sehr ernstem brasilianischen Gelehrten, und von meinem Kollegen in São Paulo, Dr. Affonso d'Escragnolle Taunay, Direktor des Museu Paulista. Sonst werde ich von den Brasilianern, für die ich doch auch seit vielen Jahren arbeite, recht stiefmütterlich behandelt.” Theodor Koch-Grünberg to Dr. O. Quelle, 16 July 1923, VK Mr A. 35, file PQ.

**75** Hamilton Rice to Theodor Koch-Grünberg, 1 May 1923, VK Mr A. 35, file R.

**76** Theodor Koch-Grünberg to Hamilton Rice, 23 July 1923, VK Mr A. 35, file R.

**77** Württembergischer Verein für Handelsgeographie to Theodor Koch-Grünberg, 8 Jan. 1924, VK Mr A. 37, file W.



## CONCLUSION

Koch-Grünberg witnessed the transformation of social structures, which he reported on in a subjective manner in his correspondence. He began his career during the pompous German Empire; but when he died, Germany was an impoverished republic, devastated by the First World War and ravaged by an economic crisis. Koch-Grünberg’s correspondence evinces how World War I and the economic depression shaped social relations and how European Americanists mobilized their relationship networks within this context. These events imposed social, economic, and political restrictions on social relations. Yet, within this context Americanists still exerted enough agency in order to engage in social relations, find solutions to problems that did not exist before this shift, and signify that context by interpreting reality.

From a historiographical perspective, by taking the archive as an ethnographic field, it was possible to invert the formula that imposes the impact of supra-individual events upon common people. Instead, it is possible to notice how ordinary people were trying to understand their realities, expressing worldviews and signifying the context in which they were involved. Therefore, instead of looking at the events in order to understand the changes they produced in the social structure, this article’s aim is to comprehend the understanding that ordinary people had of those events. Moreover, to understand the worldview of the Other – be they Amerindians, the Nuer, scientists or field ethnologists – is an anthropological endeavor.

Koch-Grünberg also observed acute transformations in anthropology: the procedure of field research changed from “extensive scientific expeditions towards intensive fieldwork” (Kraus, 2004a: 469), the theoretical problems changed, as well as the aspects of social life investigated by scholars. Koch-Grünberg saw Germany losing its leading role within museology and ethnology. Glenn Penny has argued that by the end of the nineteenth century, German ethnological museums, especially in Berlin, had acquired so many ethnographic objects that their directors and curators were no longer able to exhibit them in meaningful displays (Penny, 2002: 162ff.). The fast-growing collections worsened the situation so that, when World War I began, German ethnological museums had already lost some of their influence in international museology. German ethnological museums were considered exemplary in the late 1880s and 1890s, but in less than 30 years they became out of date.

Koch-Grünberg was also aware of the anthropological turn his generation was facing. He knew the classics of British anthropology –citing E. B. Tylor in his work about South American animism (Koch-Grünberg, 1900) – but like his German colleagues, opposed social evolutionism because of its lack of empiri-

**78** Ministerium des Kultus und Unterrichts to Theodor Koch-Grünberg, 2 Oct. 1915, VK Mr A.1, file K6-01K2.

**79** Ministerium des Kultus und Unterrichts to Geheimrat Hofrat, 22 Jan. 1924, VK Mr A. 37, file H.

**80** Theodor Koch-Grünberg to Alfred Hettner, 30 Jan. 1924, VK Mr A. 37, file H.

**81** Theodor Koch-Grünberg to Alfred Hettner, 7 Jan. 1924, VK Mr A. 37, file H.

cal grounds and its deductive method. As a rigorous supporter of the inductive methodology based on empirical investigation, or as a “field ethnologist,” as he once labeled himself,<sup>82</sup> Koch-Grünberg ignored French scientific literature, since at this time French social thought was more concerned with theories than with providing primary ethnographic data, as Robert Parkin (2005: 159-160) has argued. Paul Rivet’s work was an exception: it was in accordance with German epistemological premises and therefore highly esteemed by Koch-Grünberg and his compatriots. The impact of North-American ethnology on German-speaking ethnology was more profound. At the same time that German academia was passing through a major crisis due to a lack of resources and personnel as well as unemployment, the institutionalization of anthropology in the USA, led by Franz Boas and based on the four-field approach (physical and cultural anthropology, linguistics, and archaeology), developed rapidly. As Han Vermeulen (2015: 435) summarized the latter development:

*The departmentalization of anthropology is generally regarded as having launched anthropology as a profession in the United States. The PhD program in anthropology set up at Columbia enabled Boas and his students to acquire major positions in American academia, which led to a higher degree of professionalization earlier than in Europe.*

North-American cultural anthropology shared some of its philosophical roots with German ethnology, including the work of the philosopher Johann Gottfried Herder (1774-1803) and the linguist Wilhelm von Humboldt (1767-1835). As Matti Bunzl (1996: 71-73) pointed out, the modern concept of culture, to which Boas contributed substantially to its constitution, can be traced back to Bastian and Humboldt, “although not without a paradoxical and portentous residue of conceptual and ideological ambiguity, to the Herderian ideal of *Volksgeist*” (ethnic spirit).

Moreover, both traditions shared at least three characteristics: “the focus on language, the historical relations between peoples, and the empirical or descriptive approach to the study of human diversity” (Vermeulen, 2015: 435). This means that German ethnologists did not reject North-American cultural anthropology, as they did with French and British approaches. Koch-Grünberg knew Boas’ *Indianische Sagen von der Nord-Pacifischen Küste-Americas* (1895) (Indigenous Legends from the North Pacific Coast of America)<sup>83</sup> and possessed a copy of *Kultur und Rasse* (1914) (Race and Culture) that Boas donated him,<sup>84</sup> which the German Africanist Leo Frobenius (1873-1938) also studied carefully.<sup>85</sup> Fritz Krause (1881-1963) undertook a study of the indigenous groups of California, which was mainly grounded on North-American authors, such as Franz Boas, Alfred Kroeber (1876-1960), and the German-born linguist and anthropologist Edward Sapir (1884-1939) (Krause,

**82** “Feldethnologue”. Theodor Koch-Grünberg to Karl Weule, 20 April 1921, VK Mr A. 31, file W.

**83** Ernst Wasmuth Verlag to Elsa Koch-Grünberg, 5 Sept. 1912, VK Mr A. 13, file W.

**84** Theodor Koch-Grünberg to Franz Boas, 22 Sept. 1921, VK Mr A. 30, file B.

**85** Leo Frobenius to Theodor Koch-Grünberg, 5 July 1923, VK Mr A. 34, file F.

1921: 95-96). Apart from the growing theoretical influence of cultural anthropology, North-American universities and museums had a huge advantage over German institutions, because of the funds at their disposal to publish books, to increase ethnographic collections by purchasing objects and pictures, as well as to send ethnographers to the field and to hire skilled personnel to guide them there.

Koch-Grünberg followed Americanist literature produced in the United States, such as the work of William Curtis Farabee (1865-1925) as well as North-American expeditions to the Amazon. He published a small article on the expedition that Hamilton Rice undertook in 1907 (Koch-Grünberg, 1908) and declined a request from the newspaper *New York World* to comment critically on the results of the scientific expedition led by Theodore Roosevelt and Mariano Cândido Rondon in 1913-14.<sup>86</sup> He was in touch with research centers such as the Smithsonian Institution, the Field Museum of Chicago, the University Museum of Philadelphia, as well as with scholars like Franz Boas and his former student Frank G. Speck (1881-1950). As previously mentioned, Koch-Grünberg died during the expedition led by Hamilton Rice. However, Koch-Grünberg remained reticent towards American scholars and institutions. For instance, he would have regretted if the financial situation forced his friend Nimuendajú to work for North-American museums, especially if this concerned linguistic material: “I would be sorry, if this intellectual work done by the Germans fell right into American hands.”<sup>87</sup>

Finally, Koch-Grünberg noticed that German ethnologists could no longer concur with their North-American colleagues. In a letter to his colleague Walter Lehmann, he summarized the situation that German Americanists were facing: they were “handing over” their discipline “entirely to the Americans, with whom we can no longer compete even in South America, thanks to their tremendous resources.”<sup>88</sup>

Hence, in Koch-Grünberg’s correspondence with his Brazilian colleagues two institutional histories converged: the rise and fall of German Americanist ethnology from the perspective of one of its leading experts and the attempt by three Brazilian scholars to forge an international network of scientists dedicated to the study of Brazil. Such an attempt took place even before the institutionalization of the social sciences in their country, which only occurred during the 1930s and 1940s (Candido, 2006: 271). A third institutional history arises indirectly from the relationships between Koch-Grünberg and his Brazilian colleagues: the professionalization of anthropology in the United States and its growing influence on other national ethnological traditions.

From a historic and ethnographic perspective, by taking the *Nachlass* as a unity, it is possible to perceive how certain patterns of social relations changed during the period. The network of relations comprises a *socioplasticity* capable of adapting to external changes, absorbing the contextual impact, and removing,

**86** Theodor Koch-Grünberg to the *New York World*, 31 May 1914, VK Mr A. 17, file N.

**87** “[...] es wäre mir leid, wenn diese Geistesarbeit der Deutschen gerade in amerikanische Hände fiel.” Theodor Koch-Grünberg to Curt Nimuendaju, 7 Dec. 1915, VK Mr A. 1, file N.

**88** “[...] wir liefern sie [die Amerikanistik] ganz den Amerikanern aus, deren Konkurrenz wir, dank ihrer ungeheuren Mittel, auch in Südamerika nicht mehr begegnen können.” Theodor Koch-Grünberg to Walter Lehmann, 30 June 1921, VK Mr A. 31, file L.

remodeling, creating or recreating social relations. Lastly, by considering the *Nachlass* as a *socioplastic network*, it becomes possible to glimpse how Koch-Grünberg’s connections to Brazilian scholars were impacted by the contextual shift and remodeled by him in order to adapt to the newly created social arrangement. Yet, this always from his personal perspective, grounded on the premise that ethnology can only exist as an international science guided by humanistic ideals.

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**Erik Petschelies**, after studying social sciences at the State University of Campinas (Brazil), earned his master’s degree in social anthropology in 2013. He is currently a PhD candidate in social anthropology at the same university and was a guest researcher at the Philipps-Universität Marburg (Germany) between 2016 and 2017.

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A.1 – A. 37: Scientific correspondence

H.: Photographic collection

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#### **ILLUSTRATION CREDITS**

**Figure 1:** Theodor Koch-Grünberg in Manaus (Brazil), 1903 (VK Mr KG\_H\_VIII).

**Figure 2:** João Capistrano de Abreu (Photographic collection of the Archive of the Museu Paulista of the State University of São Paulo).

**Figure 3:** Teodoro Sampaio (Photographic collection of the Archive of the Museu Paulista of the State University of São Paulo).

**Figure 4:** Koch-Grünberg’s membership card (VK Mr A. 32, file A).

**Figure 5:** Affonso d’Escragnolle Taunay (Photographic collection of the Archive of the Museu Paulista of the State University of São Paulo).

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# “Three Long Rows of Empty Shelves” to Fill: Curt Nimuendajú as Collector and Researcher for Ethnological Museums in Germany, 1928-1930

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Peter Schröder

🏠 *Universidade Federal de Pernambuco | Recife, Brazil*

✉ *pschroder@uol.com.br*

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## ABSTRACT

In 1928-29 and 1930, the German-Brazilian anthropologist Curt Nimuendajú was contracted twice by German ethnological institutions (above all, museums) for organizing ethnographic collections and carrying out anthropological research among indigenous peoples, principally Jê-speaking peoples in various regions of today's Maranhão and Tocantins states in Brazil. This is not only a lesser-known part of Nimuendajú's biography, but also an example of a kind of academic cooperation difficult to imagine nowadays. The collections, partly destroyed during World War II, are still stored in the ethnological museums of Hamburg, Leipzig, and Dresden, along with a great number of mostly unpublished letters and other documents linked to them. The history of these expeditions offers an opportunity for reflections about the implicit theories involved in contemporary collecting, fieldwork methods, and the style of anthropology practiced. The research reveals influences from German ethnology on the academic environment in Brazil, which later became increasingly independent from this input.

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## KEYWORDS

Nimuendajú,  
German ethnology,  
Ethnographic  
Collections,  
Ethnological  
Museums, Jê-  
speaking Peoples

## INTRODUCTION

Today, the German-Brazilian anthropologist Curt Nimuendajú is considered one of the pioneering figures in the history of Brazilian anthropology in its pre-institutional period. In 1928-29 and 1930, he was contracted by German ethnological institutions for organizing ethnographic collections and carrying out anthropological research among indigenous peoples in various regions of the current Maranhão and Tocantins states. This article is about the partial results of a research project regarding Nimuendajú's relationships with German museums. As a contribution to the history of Brazilian anthropology and of German ethnology, it deals with the history of two field expeditions in Brazil and the ethnographic collections that resulted from them.<sup>1</sup>

I discovered this subject in 2009 by reading a popular biography of Nimuendajú (only two biographies of him exist, both are non-scientific<sup>2</sup>), published in the former GDR, by a journalist and *Heimatforscher* (regionalist amateur researcher) in 1979. In his book *Nimuendajú – Brother of the Indians*, Georg Menchén, who died in 1989, published part of the correspondence between Nimuendajú and his interlocutors in Germany. The main problems with this book are that Menchén's selection of sources was one-sided, biased, and his narrative is not trustworthy because the author substituted a lack of information with imagined episodes, often with romantic traits.

That same year, I initiated e-mail contact with German ethnological museums in order to find out if the collections organized by Nimuendajú still existed and if the documents cited by Menchén could be consulted; the answers were positive. In 2010-11, during a seven-month period of post-doc research in Germany,<sup>3</sup> I carried out a survey regarding the collections organized by Nimuendajú for German ethnological museums and the documentation related to them (Schröder, 2011). Research was done, among other place, in the following places and institutions:

- ▶ The Grassi Museum in Leipzig;
- ▶ Archives of the Institute of Ethnology at Leipzig University;
- ▶ The Museum of Ethnology in Dresden;
- ▶ The Museum of Ethnology in Hamburg;
- ▶ The Ethnological Museum in Berlin;
- ▶ The Ethnological Museum in Munich (now Museum Fünf Kontinente).

Indeed, in Hamburg, Dresden, and Leipzig, the majority of ethnographic and archeological objects collected by Nimuendajú for German museums can still be found, confirming the information found in two former publications (Becher,

**1** This article is a revised and extended version of a paper originally presented in the Panel 060 “Themes in the History of Anthropology and Ethnology in Europe” during the 14th Biennial Conference of the European Association of Social Anthropologists (EASA), held at the University of Milano-Bicocca in Milan, Italy, 20-23 July 2016. The presentation of the paper was made possible by a grant from CNPq (Conselho Nacional de Desenvolvimento Científico e Tecnológico) of the Brazilian Ministry of Science, Technology, Innovations and Communications (MCTIC) (AVG facility, process n. 451989/2016-3).

The author also expresses gratitude to CNPq for supporting studies about the work and life of Nimuendajú and his contacts with German ethnology by having granted a special scholarship for distinguished productivity called *Produtividade em Pesquisa* (PQ modality; processes n. 307304/2013-2 [2014-2017] and 306550/2016-4 [2017-2020]).

**2** A second biography of Nimuendajú was completed in 2014 in Bochum (Germany) by the Brazilian journalist Salvador Pane Baruja and was made available online in 2015 (Pane Baruja, 2014). Although his treatise, in comparison with Menchén's book, has the evident merit of being extensively based on Brazilian and German documentary and image sources, his analysis in many parts suffers from a lack of scientific rigor. The author, for example, explains Nimuendajú's thematic preferences in a certain field research by referring to specific psychological constellations of his biography instead of trying to relate them to prevailing tendencies in German ethnology at that time.

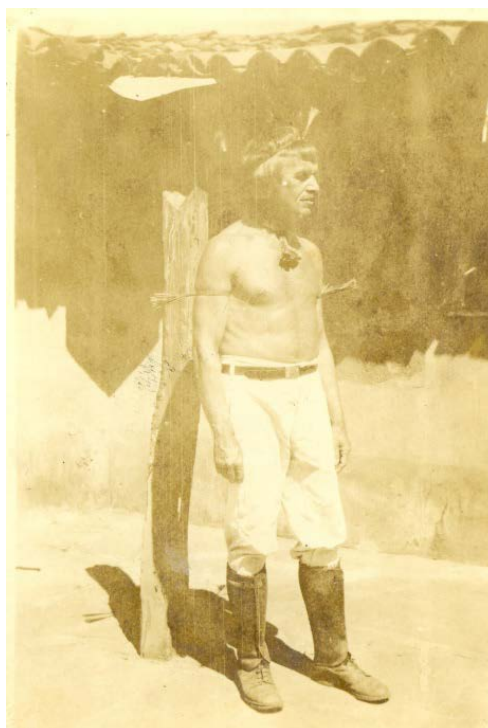
1955; Staatliches Museum für Völkerkunde Dresden, 1983) about the existence of the collections. The hook for this article is that a detailed evaluation of new information contained in original sources only partially analyzed, allows us to draw some interesting conclusions about the collector; as well as about the historical context of his activities.

### THE MAIN ACTORS

Among the main actors involved in the story of the two expeditions done for German ethnographic museums – Nimuendajú, Fritz Krause, and Otto Reche – Nimuendajú is certainly the best known in the international anthropological community.

Curt Unckel was born on April 17, 1883, in Jena, Thuringia, Germany and immigrated to Brazil in 1903. From 1905 to 1907, he spent two years with a Guarani group in a village along the Batalha River in the hinterland of São Paulo state, where he was adopted by an indigenous family and received his name Nimuendajú. He later registered Nimuendajú as his family name in 1926 when opting for Brazilian citizenship. In 1913 he moved from São Paulo to Belém, where he maintained as his permanent address until his death in December 1945 in a Ticuna village, in São Paulo de Olivença municipality, in the Upper Solimões region of the Amazon (Figure 1). The cause of his death is still unclear, although the hypothesis that he was murdered is the most accepted (Oliveira, 1999).<sup>4</sup> More than four decades of work dedicated to the ethnology of indigenous peoples earned him, already during his lifetime, the reputation of being one of the best experts on indigenous peoples of Brazil in the first half of the century (Grupioni, 1998: 164) and, according to some, the greatest of all (Kraus, 2004: 44-45).

It is interesting to note that Nimuendajú is not only attributed to one national tradition in anthropology; however, this depends on one's point of view. In Brazilian anthropology, he is unequivocally seen as a Brazilian anthropologist, sometimes with the addendum “of German descent,” but generally he is not con-



**Figure 1**  
Curt Nimuendajú during fieldwork, around 1934 (unknown photographer; Acervo Instituto Martius-Staden, São Paulo)

**3** Financed by CNPq. The scholarship was a PDE facility (process n. 200455/2010-9). Hereby, I again express my gratitude to CNPq for having made possible this research.

**4** In a recent article Elena Welper offers a kind of genealogy of the different versions of Nimuendajú's death. See Welper (2016).

sidered a representative of German ethnology. On the other hand, in Germany, things look very different. For example, among the short biographical portraits of the portal *Interviews with German Anthropologists* one can find an entry about “Curt Nimuendajú (Unckel).”<sup>5</sup> Furthermore, in 2013 I was invited by the Historical Commission of the Bavarian Academy of Sciences to write a dictionary article on him for volume 26 of *Neue Deutsche Biographie* (New German Biography), an important reference which was finally released in 2016 (Schröder, 2016), though Curt is listed as Unckel, not as Nimuendajú.

Fritz Krause (1881-1963) was a German ethnologist principally known among Americanists for his 1908-09 expedition to the Araguaia region in Central Brazil. He found employment at the Ethnological Museum in Leipzig from 1912 onwards, serving as its director since 1927. During this time he also taught as a professor at Leipzig University from 1925 to 1945 (Figure 2). Krause developed a distinct approach to ethnology, which he labelled “ethnological structuralism.” Although having strong psychological associations, it was very different from the later French structuralism of Lévi-Strauss. Krause’s intellectual effort was aimed at finding a new theoretical path among the mainstream tendencies of German ethnology at that time (see Wolfradt, 2011 for Krause and his structuralist approach). As a co-founder of the German Ethnological Society (*Deutsche Gesellschaft für Völkerkunde – DGV*) in 1929, Krause held a strong position in the institutional arrangements of German ethnology at that time, but after World War II his arrangements and engagements with National Socialism gave him the status of a person hardly tolerated in Eastern Germany under the communist regime. Nevertheless, his political sympathies did not appear in his correspondence with Nimuendajú (for an obituary of Krause, see Damm, 1966).

Otto Reche (1879-1966), an ethnologist and physical anthropologist, was one of the most prominent exponents of Nazi anthropology. Being a tenure track professor of (physical) anthropology and ethnology at Leipzig University since 1927, he was director of its Institute for Anthropology and Ethnology, later renamed “Institut für Rassen- und Völkerkunde” (Institute for Racial



5 Available at: <http://www.germananthropology.com/short-portrait/curt-nimuendaj-unckel/193>, access in July 13, 2018.

**Figure 2**  
Fritz Krause (Reproduced from Herbert Baldus, *Bibliografia Crítica da Etnologia Brasileira*, vol. 2., Hannover: Kommissionsverlag Münstermann-Druck, 1968; unknown photographer)

Studies and Ethnology) (for the academic vita of Reche, see Geisenhainer, 2002). Again, as in the case of Krause, his ideological and political ideas did not appear in his correspondence with Nimuendajú.

Other German scientists were directly and indirectly involved in the history of the two expeditions. Their roles were secondary in comparison to the three main actors mentioned above because they never were direct interlocutors with Nimuendajú, exchanging letters only with Krause or Reche. Among these “secondary” actors we should include Georg Thilenius (1868-1937), director of the Hamburg Museum of Ethnology, and Arnold Jacobi (1870-1948), zoologist and director of the Dresden Museum of Zoology and Ethnology; both were involved in financing the expeditions. Their correspondence with Krause, and on a smaller scale with Reche, above all reveal the financial and logistic preoccupations and less an interest in the circumstances of Nimuendajú’s fieldwork. However, this idea may be the result of the letters’ subjects, because they received copies of all letters sent by Nimuendajú to Krause from the fieldwork areas.

Thus, the main axis of communication existed between Krause and Nimuendajú. However, there was another direct interlocutor for Nimuendajú, who must be mentioned: Carlos Estevão de Oliveira (1880-1946) of the Museu Paraense Emilio Goeldi (Figure 3), one of his most important allies in the Brazilian institutional scene and a personal friend for the rest of his life. All the letters from Nimuendajú to Carlos Estevão, held at the State’s Museum of Ethnology and Archaeology of São Paulo, have been published by Thekla Hartmann (see Nimuendajú, 2000), and some of them provide accounts of his fieldwork activities for the German museums.



**Figure 3**  
Carlos Estevão de Oliveira with his daughter Lygia in the Zoobotanical Park of the Museu Paraense Emilio Goeldi in Belém (Acervo Museu do Estado de Pernambuco/FUNARPE; unknown photographer)

#### THE SOURCES

The primary sources of this research are letters and complementary documents, lists of objects, maps, photos, and ethnographic objects, are all archived and deposited in the ethnological museums of Leipzig, Dresden and, on

a minor scale, Hamburg. In the Ethnological Museum at the Grassi Museum Leipzig the entire documentation of both expeditions was found in near entirety. Visual materials are quite scarce, while the number of well conserved correspondence is remarkable<sup>6</sup>.

Other correspondence was also found; principally between Krause and the Swiss ethnologist Felix Speiser (1880-1949), the Swedish ethnologist Baron Erland Nordenskiöld (1877-1932), director of the Ethnographic Museum of Gothenburg, and the Austrian ethnologist Father Wilhelm Koppers (1886-1961), from the *Anthropos* journal. These letters only represent a minor part of the correspondence regarding the two expeditions.

The documentation archived at the Grassi Museum allows a complete reconstruction of the history of the first expedition. Whereas, in the case of Nimuendajú's second expedition for German museums, such a reconstruction is only possible with the help of the documents found in Dresden. Generally, Nimuendajú's letters are long, detailed, and have minimal spacing. They often contain vivid descriptions of the fieldwork conditions and his research style, together with detailed ethnographic information and numerous critical observations about indigenous politics as well as about the kinds of relationship between the indigenous and non-indigenous population. Frequently, the language of these letters is non-scientific and generally has a mixture of formal German writing and popular expressions. Without exception, all the letters were written in German.

From these letters, a quite different Nimuendajú emerges than the one known by his scientific monographs and articles. By comparison, the language of Krause, Reche, and that of the other German ethnologists, is sober and even bureaucratic, although Krause is the only one among them who sometimes makes ironic comments in some passages, adding a different, more interesting style to his letters.

One example might illustrate this. In January, 1928, Nimuendajú wrote a letter to Krause with two proposals for expeditions which could be conducted by him for German museums<sup>7</sup>:

*An ethnographic travel for collecting, with the center of activities in Boa Vista on the Tocantins River, with the Apinayé as the principal object for studies and an extension of the studies to the neighboring tribes as far as the means can afford. I consider this field of activities by far the best. Very good results can be expected, because I am quite sure of the Apinayés' good intentions.*

[...]

*In Boa Vista, there is a an unrestricted ruler governing, the old Father João, without whose most holy will no one can land firmly on the ground there. Much depends upon his thoughts. As far as I know, at least he is no enemy of the Indians.*

**6** The following original letters were located: 53 letters exchanged between Nimuendajú and Krause about the two expeditions; 12 letters between Nimuendajú and Reche regarding the second expedition; 10 letters between Krause and Reche regarding the second expedition; 29 letters between Krause and Thilenius about the two expeditions; 28 letters between Krause and Jacobi about the first expedition.

**7** Staatliche Ethnographische Sammlungen Sachsen (SES), Leipzig, 1928/39, Krause, January 31, 1928. (translation P.S.)

With reference to this passage Krause comments in a letter to Thilenius from April, 1928<sup>8</sup>:

*Nimuendaju was invited by the Apinayé who had sent a legation to Para to visit them. The Apinayé are waiting for him. The region of Boa Vista, however, is controlled by a Father, though he shall be a friend of the Indians. Yet, without his consent nothing can be done there. Nimuendaju ought to gain the friendship of this Father, what he might succeed by presenting him the Anthropos. [the Anthropos journal is edited by clergymen since its foundation]*

<sup>8</sup> SES, Leipzig, 1928/43, Krause, April 13, 1928. (translation P.S.)

In the depot of the Museum of Ethnology in Dresden, the following letters were found, either as originals or as photocopies: 15 letters between Nimuendajú and Krause; 34 letters between Nimuendajú and Reche; and 14 between Krause and Reche.

On a lesser scale, there are also some letters between Krause, Thilenius, and Jacobi, and between Reche, Thilenius, and Gustav Antze, from the Hamburg Museum. The documents archived in Dresden enable a reconstruction of major aspects of the second expedition. As for the style of the letters, the same observations can be made as in the case of the correspondence archived at Leipzig.

So far, it was not possible to visit the archive at the Hamburg Museum of Ethnology, but this does not represent a major problem. This is because the entire correspondence between the three museums could be localized in Leipzig and Dresden, and there was no direct contact between Nimuendajú and the Hamburg museum staff.

## CONTACT AND CONTRACT

Nimuendajú was a self-educated anthropologist and never had the possibility of an academic education. He had his first contact with the European academic environment when he published his famous ethnographic monograph about Guarani religion and worldview in the well-respected *Zeitschrift für Ethnologie* (Nimuendajú, 1914). He achieved this success through the mediation of the German ornithologist Emilie Snethlage (1862-1929), who had become director of the Goeldi Museum in Belém that year. World War I interrupted this contact with German ethnology but after the war Nimuendajú managed to maintain a regular correspondence with Theodor Koch-Grünberg (1872-1924) for nine years, from 1915 to 1924, terminating with the death of Koch-Grünberg.<sup>9</sup> Having published various articles in periodicals such as *Zeitschrift für Ethnologie* (Berlin), *Petermanns Geographische Mitteilungen* (Gotha), *Anthropos* (Mödling bei Wien), and *Journal de la Société des Américanistes* (Paris), Nimuendajú built up a reputa-

<sup>9</sup> The entire correspondence between Nimuendajú and Koch-Grünberg will be published by the Museu Paraense Emílio Goeldi (MPEG) in a volume edited by Michael Kraus (Göttingen University), Ernst Halbmayer (Marburg University), Nelson Sanjad (MPEG), and the present author.

tion as a specialist in indigenous ethnology and linguistics of Lowland South America in the midst of the 1920s.

But he was short on money, and this seems to have been a permanent cause for concern until the end of his life. At the very least, this regularly is one of the topics in his letters. For this reason, he accepted to carry out archaeological excavations and ethnographic studies for the Ethnographic Museum of Gothenburg, from 1923 to 1927, with the support of the museum’s director, Erland Nordenskiöld (Nimuendajú, 1929a; 2004). But in 1926 the relationship with this sponsor became complicated and the cooperation ended in 1927. Thus, the continuity of Nimuendajú’s studies was at risk.

In this delicate situation, a recommendation by the Swiss ethnologist Felix Speiser was very welcome. Speiser was introduced to Nimuendajú through letters from Koch-Grünberg and later met him in Belém. On February 25, 1927, Speiser wrote a letter to Krause, introducing Nimuendajú as an excellent collector and fieldworker:

*[...] Thus, for Mr. Nimuendajú [sic] it is a question to get a new basis of existence. This could be quite easy for him, retiring himself completely from ethnography, but it will be very hard for him to give up ethnography, which became a way of living for him. It also would be a loss for ethnography itself if it would dispense the collaboration of one of the best experts on Brazilian Indians, as Mr. N. may well be labelled. [...] He knows vast regions of South America, and as he already lives in Brazil he will be able to visit any region you indicate to him as a collecting area with a relatively low budget. Besides, his personal modesty is enormous. In this way, your museum would have an opportunity to obtain valuable original collections and in the same way Mr. Nimuendajú’s continued work for ethnography would be guaranteed.<sup>10</sup>*

**10** SES, Leipzig, 1928/43, Krause, February 25, 1927. (translation P.S.)

Krause’s reaction was positive but, still wary, he preferred to also contact his old friend, the Baron Nordenskiöld, on November 12, 1927:

*Before engaging in such big business we would like to make some things clear. And as you have worked with Mr. N. in the same manner for some years, I would like to ask you for some information, above all about the scientific qualification of Mr. N. After all, I know some of his publications in Zeitschrift für Ethnologie; but they do not show to what extent the respective editors possibly revised them. Thus, we would like to know above all if Mr. N. has the qualifications for pure scientific research, especially for linguistic records, ethnological studies of individual peoples or the investigation of certain ethnological problems that could be selected for him, for example, the social organization of one or more tribes and*



*the like. And what about his talent for collecting ethnological and archaeological objects? Are his collections arranged in such a way that one gets a scientifically satisfying portrait of the culture of the respective tribes?<sup>11</sup>*

It is noteworthy that Krause's questions were not only about collecting objects in a strict term of the sense, but also about the quality of anthropological and linguistic research. Although texts written by self-educated researchers such as Nimuendajú were still accepted at that time, Krause was a professional ethnologist and, moreover, a Prussian style public servant responsible for the utilization of the museum's annual budget. And he even was right with his doubt about the possibility of editorial interferences in Nimuendajú's texts, but not in the way he imagined.<sup>12</sup>

Nordenskiöld replied on November 15 that,

*As for Nimuendajú, I must tell you that I consider him an excellent researcher who managed to carry out very precious work for us with very modest means.*

*[...]*

*The reason I broke off relations with him is that he doesn't know how to deal appropriately with the Brazilians. Probably he is too honest to adulate them when that is necessary. With the Indians, however, he gets on excellently.*

*As for his published articles, they were elaborated without any assistance.<sup>13</sup>*

In the meantime, Nimuendajú and Krause had exchanged several letters trying to come to terms with Nimuendajú's detailed explanations about fieldwork possibilities and the interests of the museum. For example, Nimuendajú prepared a list of research possibilities in various regions of the Amazon and adjacent areas, in a letter dated August 12, 1927.<sup>14</sup> But the decisive factor was the museum's interest in acquiring ethnographic objects from the region between the Middle Tocantins River and the Mearim River, in Maranhão, with the aim “to fill regional gaps” in its collections. Moreover, Krause himself had carried out an expedition to the Karajá in the Araguaia region between January 1908 and February 1909. Thus, he saw in Nimuendajú's field activities a seamless continuation of his own research focus.<sup>15</sup>

Unfortunately, not only Nimuendajú was short on cash, the Grassi Museum was also suffering from the permanent budget crises that hit most museums and scientific institutions in postwar Germany. In order to find a solution for the mutual interests, Krause succeeded, in longwinded negotiations, to convince Thilenius, from the Hamburg Museum of Ethnology, and Jacobi, from the Dresden Museum of Zoology and Ethnology, to support an expedition and share the costs. In return, Nimuendajú had to collect three copies of every object to be redistributed among the three museums. This obligation was even the subject

**11** SES, Leipzig, 1928/43, Krause, November 12, 1927. (translation P.S.)

**12** In one of Nimuendajú's articles about mythology, religion and shamanism of the Xipaya Indians, the ecclesiastical editors of *Anthropos* did not revise any of his often popular expressions, but paradoxically interfered in the mythical text about the origin of the Brazil-nut tree (Nimuendajú, 1919/20: 1019), where one passage originally written in German was substituted by a translation into Latin. The reason? The original text contained some parts with explicit reference to human genitals.

**13** SES, Leipzig, 1928/43, Krause, November 15, 1927. (translation P.S.; underline in the original letter)

**14** SES, Leipzig, 1928/43, Krause, August 12, 1927.

**15** SES, Leipzig, 1929/71, Krause, S. 727-731, November 27, 1929.

of a detailed contract, which Nimuendajú considered unnecessary because, from his point of view, he had to assume all the “real” risks, including his health and financial outcome.

### THE EXPEDITIONS<sup>16</sup>

On September 17, 1928, Nimuendajú wrote a letter to Krause explaining his apprehensions about the expedition’s success:

*Finally, I have arrived at the conclusion, after having made purchases and having organized the exchange goods for the different tribes: Although I considerably exceeded the amount planned for this purpose, I became aware that it will not be sufficient for the complete execution of my program. I am probably not going to reach the Apinayé and the bands located to the northeast. I will have to give up going ahead to the Karaô and the Šerénte in the south, and the Canellas and Guajajára will probably come off somewhat scanty. – If it were only a question of simply illustrating the culture of the different tribes, everything would be very fine, but in my mind I see THREE long rows of empty shelves which I shall fill up all. Notwithstanding that, I believe optimistically that I will succeed to give satisfaction to all three institutes.<sup>17</sup>*

“In my mind I see THREE long rows of empty shelves which I shall fill up.” This sentence seems to be a premonition of the difficulties Nimuendajú would face during the expeditions, even if collecting three copies of each object often was not the main challenge.

The first expedition started in September 1928 and finished in May 1929. Fieldwork was done among the Apinayé, Krĩkateyé, Kreapimkateyé, Pukobyê, Guajajara and Canela (Apanyekrã and Ramkokamekrã) in the current Tocantins and Maranhão states. Nimuendajú described the complicated fieldwork circumstances in his detailed letters to Krause, but also to Carlos Estevão (the latter case being in Portuguese). An example might illustrate the letters’ style.

On October 31, Nimuendajú arrived at the Tocantins river:

*On the very same day I crossed over to Boa Vista on the left side of the Tocantins river in Goyaz. The oars used here have blades in the shape of a lancet. I introduced myself to the village chief, Father João Lima, who manages a severe, patriarchal government for the sake of the place, which had been a real hell of revolutions and crime before this interesting man came into power. Unfortunately, the usual problems have not failed to appear here, too. [...] Finally, Father João lent me a horse which I used for riding to the Apinayé village Bacabal on November 4.<sup>18</sup>*

<sup>16</sup> The complete correspondence between Nimuendajú, Krause and Reche about the two expeditions will be initially published in Portuguese in two volumes.

<sup>17</sup> SES, Leipzig, 1928/43, Krause, September 17, 1928. (translation P.S.)

<sup>18</sup> SES, Leipzig, 1928/43, Krause, November 9, 1928. (translation P.S.)

As can easily be suspected, the original aim of collecting three copies of each object was impossible to be achieved entirely, due to “real life” circumstances in the field, and had to be given up partially. The letters express profound cultural pessimism regarding the survival of the visited groups as culturally distinguished collectivities. This is explicit in the case of the Bacabal village of the Apinayé described by Nimuendajú in the same letter:

*The Apinayé gave me an extraordinarily friendly welcome. Their chief José Dias had prepared his people well over two years ago, when we had agreed upon a visit for the first time. It was as if I had returned to old acquaintances. Notwithstanding that, the Aldea [village] was for me, another one of the deceptions I am almost always used to have in such cases: Unfortunately, the old village with its bachelors' house and mask hut that the chieftain had drawn for me on a paper two years ago was no more. 5 huts in Neo-Brazilian style were arranged around a very badly cleaned, almost circular place; the population numbers less than 50 heads. Two years were enough to change many things: A wave of influenza had carried off the children and the young people. José Dias had undertaken a voyage to Goyaz and São Paulo in order to protest against the occupation of tribal lands by the neo-Brazilians.<sup>19</sup> He had already been declared dead and the Aldea had already disintegrated according to that rumor. The Brazilians just were about entering into his heritage when José Dias suddenly returned carrying a lot of gifts, which caused them a great displeasure. He joined his people again and build, not far away from the old one, the current Aldea da Bacaba [sic]; but the old Indian way is dead and gone. Fortunately, however, it is part of a past so recent that many things can still be recovered. José Dias as well as the old medicine man Nicolau are so friendly and eager that I believe that the voyage was worth the trouble. The circumstances are approximately the same as among Koppers' Yahgan<sup>20</sup>. Here I cannot walk from hut to hut and buy up a magnificent collection within 2 days, as you did among the Kayapó. I will have to dispense completely with triplicate collecting and would be very glad if I can at least succeed to more or less reasonably document the old culture. In about one month I will be able to inform you about the results among the Apinayé from the museum's point of view. There is no need to be too worried: It will not be very splendid, but it won't be a fiasco. The situation now calls for waiting patiently until the old customs and objects occasionally turn up again. The chief insists on that I should stay AT LEAST for a month. Then I would still get to see many interesting ceremonies. It would not be possible without further ado. It stands to reason that we also cannot celebrate the Midsummer Day and Christmas within a week. The old medicine man asked intently if I also had brought along with me enough paper: I ought to have such a high pile so that he could dictate me EVERYTHING; now he also wants to give me his younger daughter in marriage.*

**19** Neo-Brazilians is a word used by Nimuendajú which refers to the non-indigenous population in Brazil. Implicitly, the indigenous peoples were 'Brazilians' or, with other words, the 'real' Brazilians. This kind of classificatory scheme (Brazilians – neo-Brazilians) has its origins in Nimuendajú's version of Americanist Ethnology with its suppositions about the difference between 'original' and 'mixed', 'acculturated' or even 'deformed' cultures. Some traces of German romanticism may even be identified in such a classification. By the way, Nimuendajú's scheme neither became established in Brazilian anthropology nor by South Americanist ethnology. It remained part of a personal vocabulary.

**20** Reference to Koppers (1924) and his ethnography about indigenous peoples in Tierra del Fuego.

Nimuendajú patiently waited until old objects re-emerged, but he also induced the Apinayé to fabricate them again. He tried, for example, to resuscitate the manufacture of masks, but was unsuccessful. In the end, he left the village with some 300 objects. However, he became more enthusiastic after having initiated fieldwork among the Ramkokamekrã whose culture he considered a kind of “gold mine” for ethnographic research:

*Ponto, the village of the Rẽmkókamekra is the biggest Indian settlement I have ever seen: it comprises 31 huts with more than 300 residents. [...]*

*In a word, the Aldea do Ponto is an ethnological treasure: it is also the only Aldea where one nowadays can still study in their entirety the social and religious institutions of the Timbira.<sup>21</sup>*

**21** SES, Leipzig, 1928/43, Krause, March 31, 1929. (translation P.S.)

In his eyes, however, this Canela group was constantly threatened by non-indigenous influences of all kinds or, in his own words, by “the Christians,” “Christianity” or “the neo-Brazilians”:

*Two circumstances render it particularly difficult to take a longer sojourn in this village, which is so interesting: In the first place, the brandy dealers who plague the Aldea every other day; in the second place, the excessive begging already mentioned. Indeed, it is not easy to live for a month among a bunch of 300 beggars and to have to keep them in a good mood!*

*[...]*

*By the way, once again I have been very successful in achieving one thing: The friendship of the Indians of Ponto; and in the same measure as it increased, the begging diminished somewhat. They almost fed me to death; on the central ground of the village they solemnly gave me the name of their deceased principal chief Kukaipó; unlike the case of other tribes, I was the declared favorite of the women and girls who decked me out with adornments like a Christmas-tree and devoted hours to painting me very properly from top to toe; as luck would have it, they did not give me in marriage as the Apinayé. Again and again they said that I should stay, that I should attend the youth initiation ceremony, enter in an age class – Lord, and protect me from my friends! Because, after having first kicked out 3 brandy dealers one after another as if I was entitled to do that, the Christianity on all sides got scared and did not dare any more to appear with brandy.*

Sometimes his letters also include comical observations, for example about the Guajajara, neighbors of the Canela: “Bananal [village] was still more civilized than Lagoa da Pedra. There, I also had the pleasure, among other things, to see Indian wives with bobbed hair (*Bubiköpfe*) dancing tango while being accompa-

nied by accordion, and they pleasantly invited me!”<sup>22</sup>

On the whole, the expedition was highly successful from the point of view of the three museums, but the collector, in his own descriptions, hardly managed to return to Belém because of the hardships in various field situations. He returned home quite ill and without a penny in his pocket, so that the three museums decided to arrange some extra resources in an emergency action between the 5<sup>th</sup> and 12<sup>th</sup> of June, 1929.

One month after Nimuendajú had concluded the expedition, Krause asked in a letter from June 27, 1929, about his future plans and started to suggest another contractual work for German museums and research institutions.<sup>23</sup> In a letter from May 31, 1929, Krause had already set out his plan for “the further fate of Mr. Nimuendajú” to Arnold Jacobi from the Dresden Museum of Zoology and Ethnology.<sup>24</sup> As the museum’s resources would be insufficient, he suggested to involve the Saxony State Research Institute for Ethnology (Staatlich-Sächsisches Forschungsinstitut für Völkerkunde, 1914-1936) of Leipzig University.

Ultimately, the second expedition was financed by the ethnological museums of Leipzig and Hamburg, by the Institute of Ethnology of Leipzig University and with the support of the Emergency Association of German Science (*Notgemeinschaft der Deutschen Wissenschaft*, NDW), founded in 1920.<sup>25</sup> During this expedition, which lasted from February to September 1930, Nimuendajú carried out fieldwork among the Apinayé, Xerente, Krahô and, again, the Ramkokamekrã.

His descriptions of indigenous peoples present a pessimistic view. During the first expedition ethnographic information was only requested for completing the description of the objects collected; during the second expedition the writing of a monograph (with the title *Die Timbira*) was agreed upon. This should be a complementary publication for the planned exhibition of the objects. This monograph, about the Apinayé and the Canela, was the subject of protracted negotiations between Krause and Nimuendajú, but ultimately was not published as originally planned. The financial restraints of German scientific institutions became so critical in the final years of the Weimar Republic that this plan had to be abandoned. Nimuendajú became thoroughly dissatisfied and this led him, among other things, to gradually withdraw from German ethnology and also prepared the ground for his contacts with American anthropologists. *Die Timbira* was finally published in 1939 and in 1946, but not in its original form. *The Apinaye* and *The Eastern Timbira*, two ethnographic studies translated and revised by Robert Lowie, originally planned as two parts of the monograph, are nowadays deposited in two copies (one at the Museum of Ethnology in Dresden, the other at the National Museum in Rio de Janeiro).<sup>26</sup>

It is interesting to note, though not surprising, how the indigenous peoples visited during the expeditions were seen by the parties of the contracts. Already,

**22** SES, Leipzig, 1928/43, Krause, February 15, 1929. (translation P.S.)

**23** SES, Leipzig, 1929/71, Krause, S. 700, June 27, 1929.

**24** SES, Leipzig, 1928/39, Krause, May 31, 1929.

**25** The Emergency Association of German Science (NDW) was founded on October 30, 1920 as a centralized institution with the objective to facilitate raising and distributing of resources for German scientific research. Due to the general financial situation after World War I, NDW had an important role in the general context of decay scientific research was suffering during that period. In 1933, NDW was taken over by the Nazis and ended its activities in 1945. The current DFG (Deutsche Forschungsgemeinschaft/ German Research Foundation) was founded in 1951 in the Federal Republic of Germany as the successor of NDW.

**26** The copy in Dresden is well conserved, but this cannot be said about the copy in Rio de Janeiro, which was probably consumed by the fire which destroyed

a superficial reading of the correspondence permits the conclusion that the German museum staff, including Krause with his own field experience, saw the Amerindians primarily as suppliers of interesting objects for the museum's depots. Nimuendajú, on his part, once having entered “the field,” adopted a very different position, assuming a role that nowadays would be called engaged anthropology.

As in the case of other fieldwork he conducted, Nimuendajú generally played down the results, transforming them into “failures” or at least “small successes” before receiving any written reaction of the addressees of his letters and shipments (with the objects). This seems to have been a kind of preventive strategy, as he was well aware of the high expectations of European museums, where armchair anthropologists often did not have an idea of the difficulties that arose in the field (even if this does not apply to Krause).

#### **IMPLICIT THEORIES AND EXPLICIT METHODOLOGY**

Is the history of the two expeditions only an interesting example of anthropological collecting in the twentieth century, or were there any theoretical and methodological aspects that could be taken from them?

Nimuendajú himself did not have theoretical pretensions, as was noted by several authors (Welper, 2002). His texts are frequently characterized as purely descriptive, and Nimuendajú himself contributed to this image of an ethnographer uninterested in theoretical questions. Indeed, he never wrote any theoretical text, which can be explained by his lack of formal academic training, and consequently by his fear to expose himself unnecessarily in academic circles.

Yet, organizing collections for museums does not happen in a theoretical or methodological vacuum. As in the case of ethnographic studies, a pure description does not exist (neither of objects nor of cultural practices); any description is based on theoretical choices, at least implicitly (Bruck, 1987). Composing museum collections and selecting its objects implies taking decisions based on explicit and implicit theories (Clifford, 1988). The history of Nimuendajú's collections in the Hamburg, Leipzig and Dresden museums confirm this affirmation. In this case, we have to take into account his acquaintance with contemporary German ethnology; that is, his lectures and correspondence with German ethnologists at the time.

In his published texts, as well as in his correspondence we can find references to ethnological historicism, with its methodological principle of a particularizing “research of facts” (Müller, 1980). Alleged theoretical abstinence even formed part of recurrent recitations done by many German and Austrian diffusionists as a discursive effort to banish the vices of speculative classical evolu-

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the museum in September, 2018. Maybe the existence of the copies nowadays might excite the ambition to publish the monograph *The Timbira* in Portuguese, German or English or in a bilingual edition. However, it should be taken into consideration that the mere publication of the manuscript, though tempting in times of neoliberal productiveness, does not make any sense without a careful comparison with the subsequent published monographs of 1939 and 1946. And this is only possible with intimate knowledge of the German language.

tionism. can The present author still observes this discursive practice among the last representatives of German diffusionism during the 1980s.

Although organizing collections for European and Brazilian museums was not Nimuendajú's only activities during the 1920s and 1930s, collecting objects was an essential part of his ethnological practice, and this can only be understood in the historical and theoretical context of contemporary German ethnology. Purchasing ethnographic collections through research expeditions was not merely part of the rivalry between ethnological museums in imperial and post-imperial Germany (Penny, 2002). In addition, research following Adolf Bastian's ideas about collecting ethnographic artifacts included the thematic priority of material culture, as was demonstrated by Michael Kraus (2004) in his brilliant study of German ethnological research in the Amazon region between 1884 and 1929.

Krause's generation regarded the collection of objects as inseparable from the concept of doing ethnography, because the objects were interpreted as sources containing ethnographic information. By that time, Brazil held an important position among the purveyors of objects for foreign museums. Castro Faria (1987) even recommended that Nordenskiöld was a protagonist in the mercantilism of archaeological and ethnographic collections, even if this practice was indeed much older. Nimuendajú's collecting, however, was not merely commercial, as his collections became particularly valuable because of the detailed ethnographic information supplied with the objects. His ethnographic practice had much more to do with contemporary ideals of cultural salvage in German ethnology than with predominant theoretical concerns. From his point of view, it was urgent to record indigenous cultural manifestations before they were abandoned and forgotten. Thus, we can recognize in Nimuendajú a greater affinity with ideas of Adolf Bastian and Franz Boas than the other directions in the anthropology of his time (Fischer et al., 2007; Stocking Jr., 1996).

Other indications of theoretical interest in the history of the two expeditions are Krause's brief comments about the ethnographic collections at Leipzig and Dresden, which would need to be “completed” for two main reasons: (a) the lack of “material” from whole “cultural areas or provinces” (a diffusionist argument) and (b) the idea that cultures can be represented by a sensible and discerning selection of objects for offering a “complete” image of an indigenous culture by its materialized aspects. The second point of view was completely shared by Nimuendajú.

While anthropological theories remained implicit in the correspondence about the expeditions, methodological aspects became quite explicit, above all when one takes into account contemporary standards for reporting fieldwork activities. Nimuendajú's letters from the field are quite detailed, very personal, and direct about the situations in the field and their vicissitudes. We do not find any objective, scientifically weighed discourse, but rather vivid

descriptions about the difficulties and hardships experienced. Father Wilhelm Koppers (1886-1961) from the journal *Anthropos* even decided to publish a private letter sent to him by Nimuendajú from the field about his difficulties in meeting the Canela's constant requests for presents (Nimuendajú, 1929b), but this went too far. After a protest formulated by Krause<sup>27</sup> in the name of the three museums, Koppers felt obliged to publish a formal explanation about his reasons for having published Nimuendajú's letter in *Anthropos* (Nimuendajú, 1929, vol. 24: 1104). Krause had informed him that the directors of the three museums were anxious that Nimuendajú's letter could give the impression they were exploiting the willingness of a cooperative researcher with insufficient means, leaving him destitute and ill after having received rich collections organized in very adverse circumstances; whereas Koppers firmly denied that such a negative interpretation could be imperative. While the three museums and the journal came to terms with the publication of a formal explanation, Nimuendajú never forgave Koppers, seeing his decision as an indiscretion. He broke off contacts with the periodical and never again published a text in *Anthropos*.

Nimuendajú's fieldwork style can be described as radically individualistic, solitary and highly empathetic. In his extensive, but often prolix thesis about Nimuendajú's fieldwork methodology, Dungs (1991) pointed out that Nimuendajú did not only use to “live among the Indians,” but that he preferred to live “like an Indian.” Welper (2002: 127) emphasized his conviction that Nimuendajú needed a kind of cultural and social conversion as a precondition for successful fieldwork. Indeed, “living among the Indians like an Indian” implied much more than the scientific collection of information. It established social relations and implied obligations and moral engagements not intended in contemporary idealized views on scientific work. Thus, Nimuendajú's ideal fieldwork strategy can best be understood as being a mix of a lone wolf (*Einzelgänger*, in German) and a border crosser (*Grenzgänger*) (Figure 4).

**27** SES, Leipzig, 1928/43, Krause, October 19, 1929; partially reproduced in *Anthropos*, 1929, p. 1104 (“Analecta et Additamenta”).

**Figure 4**  
Nimuendajú among Canela Indians, around 1935 (unknown photographer; CELIN/Museu Nacional/UFRJ, Rio de Janeiro)





### THE COLLECTIONS TODAY

The original lists of objects archived at the Grassi Museum in Leipzig indicate 2,478 ethnographic and archeological objects collected by Nimuendajú. In addition to objects from the Apinayé, Canela, Guajajara, Krahô, Kreapimkateyé, Krĩkateyé, Pukobyê and Xerente, there were also a large number of archaeological items.

In addition, Nimuendajú offered the first collection of objects to the museum in November 1927. Although not too much is known about that earlier collection, the number of 2,478 objects deposited remained constant until December 4, 1943, when Leipzig suffered a heavy bombing raid and one bomb hit a wing of the museum where numerous objects had been exhibited. Only 613 objects remained undamaged. In other words, the Nimuendajú collections in Leipzig suffered a 75% loss in just one night of World War II. Thus, the collections at the Grassi Museum were the most affected by war among the three museums (Figure 5).

The Dresden museum, on its part, received 354 objects (Apinayé, Canela-Ramkokamekrã, Guajajara, Kreapimkateyé, Krĩkateyé, and Pukobyê) from the first expedition. Although that collection was not so tragically affected as the one in Leipzig, the total losses were 81 objects, or 23% (KV = *Kriegsverluste* = “lost by war”: 81; other objects are registered as “missing” / *fehlt*: 5). Currently, the Nimuendajú collection at Dresden comprises 273 objects, and some of them were exhibited during an exposition about Amazonian indigenous cultures in 2009-2010 (Kästner, 2009).

According to an Excel list received by e-mail on October 13, 2010, the Hamburg Museum of Ethnology must have received three contingents of objects on three different data. According to the museum’s books of register these include:

- ▶ 346 objects from the Apinayé, Canela (Apanyekrã and Ramkokamekrã), Guajajara, Kreapimkateyé, Krĩkateyé and Pukobyê received on October 26, 1929, as well as four dancing masks from the Ramkokamekrã;
- ▶ 302 objects from the Ramkokamekrã received on February 25, 1931; and
- ▶ 139 objects from the Apinayé, Krahô and Xerente received on April 4, 1932.

By studying these registers and comparing them to another Excel file from Hamburg received in 2011, it becomes clear that the Nimuendajú collections in Hamburg represent the most complete and best preserved of all three col-



**Figure 4**

The Grassi Museum, nowadays  
(Courtesy: GRASSI Museum  
für Völkerkunde zu Leipzig,  
Staatliche Kunstsammlungen  
Dresden; photo: Volkmar  
Henze)

lections resulting from the two expeditions. However, direct access to these collections in Leipzig, Dresden, and Hamburg, may be cumbersome and require a great deal of time for researchers because of the bureaucratic procedures that can be quite extensive.

Yet, Nimuendajú assembled two other collections for German ethnological museums, namely for those in Berlin and Munich. The collection in the Ethnological Museum of Berlin, organized in 1935, originally comprised 228 objects from the Ramkokamekrã detailed in the entry logs and the complementary documentation. However, during a visit to this museum in February 2011, only 135 objects could be located in the cabinets of the enormous depository, where approximately 35,000 objects from the South American lowlands are now preserved. The history of the Berlin collection was the subject of two papers presented during two congresses (Schröder, 2013; 2017) and will be published in a forthcoming article.

By contrast, Nimuendajú did not directly sell the collection of 86 objects from the Ramkokamekrã at the Ethnological Museum of Munich to the museum; but, in August 1933, he had offered it to a German aviator called Otto Meyer who was living in Pernambuco. When Otto Meyer returned to Germany in 1934, he first offered the entire collection to the museum in Munich, but later decided to sell it to Gustav Umlauff, the well-known ethnographic object-trading firm in Hamburg. This firm then resold the collection, with the exception of some objects, to the Ethnological Museum of Munich in October, 1934.

Single objects collected by Nimuendajú that can now be found in other German museums (that of Hannover, for example) were generally acquired by exchange between German and foreign museums (the Ethnographic Museum of Gothenburg, for example).

The histories of the Berlin and Munich collections and of the single objects collected by Nimuendajú available in other German museums are not the subject of the present article, as they resulted from a post-1930 period in Nimuendajú's collecting activities and were not part of the contractual agreements of the two expeditions he carried out in 1928-1930.

## **CONCLUSION**

What kind of anthropology was practiced during the two expeditions of 1928-29 and 1930? Was it German anthropology at the southern fringes of Amazonia, the initial stage of Brazilian anthropology, or something else? This depends on the point of view taken. In Brazilian anthropology, Nimuendajú has long been incorporated as a prominent person in its genealogical tree. From this perspective, the question would be easily answered, although it is difficult to neatly differentiate

a national anthropological tradition in Brazil for the period around 1930. At that time, curious individuals generally practiced Brazilian anthropology with academic backgrounds, lacking formal anthropological training, and there was no intellectual school or tradition that could be followed. Scholars such as Edgar Roquette-Pinto (1884-1954) or Capistrano de Abreu (1853-1927) were exceptions to the rule.

As a self-educated researcher, and an enthusiastic and studious reader of ethnographic studies published by German ethnologists, Nimuendajú repeatedly deplored in his letters that sometimes he had only access to popular (reduced and simplified) versions of German publications about the indigenous cultures of the South American lowlands. Notwithstanding, German ethnology was his main anthropological reference at the time. Thus, around 1930, he was above all a German expatriate living in Brazil, practicing Americanist ethnology without being part of a national academic establishment. Gradually, however, he became part of an international transatlantic network of knowledge interchanged by academic and non-academic anthropologists, archaeologists, and linguists.

And what might have been his own answer? Nimuendajú's correspondence with Koch-Grünberg makes this quite plain: The “we” used in many letters refers not only to a common nationality, but also to the mutual comprehension of a national anthropological tradition. However, the decline of German ethnology's role in Brazil from the 1930s onwards, coincides with Nimuendajú's expeditions. Therefore, the lack of possibilities to continue researching and collecting ethnographic objects for German institutions prepared the ground for an intensive and productive cooperation with American anthropologists, especially with Robert Harry Lowie, and also for systematic research about Jê-speaking indigenous peoples. It is interesting to observe that Nimuendajú received suggestions to read some of Lowie's works in the correspondence with Krause (Lowie, 1920; 1924),<sup>28</sup> which were even ordered for him in London and were included in the budget for the second expedition. Herewith, we can observe a parallelism between a change in Nimuendajú's scientific biography and a macro tendency in the anthropology in Brazil during this period. That is seen as a gradual distance from German ethnology with a simultaneous approximation to North-American anthropology.

As in the case of other ethnographic collections, the history of Nimuendajú's two expeditions financed by German institutions reveals that these collections were more than a set of objects whose organization was based upon some explicit or implicit criteria of the collectors and their financiers. With their objects and their complementary documentation, they offer differentiated information and insights about various aspects of anthropological practices in specific historical contexts. Therefore, the collections and their documentation become sources that “speak” as much about the indigenous producers and users of these objects, as about the anthropologists involved in collecting them.

**28** SES, Leipzig, 1929/71, Krause, S. 736-739, December 12, 1929.

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**Peter SCHRÖDER** is associated professor in the graduate program for anthropology (PPGA/ Pós-Graduação em Antropologia) of the Anthropology and Museology Department (DAM/ Departamento de Antropologia e Museologia), Universidade Federal de Pernambuco (UFPE), Recife, Brazil. Obtaining his Dr. phil. in Ethnology from the Rheinische Friedrich-Wilhelms-Universität Bonn, Germany, in 1993, he conducted post-doctoral research in southern Ceará State, Brazil, in 1995-97 and at Leipzig University, Germany, in 2010-11. His research interests are: indigenous societies in South America, development anthropology, economic anthropology, history of anthropology. He carries out research on Curt Nimuendajú's life and work from 2009 on.

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