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American Hookworm Antiquity (& Response)

Karl Reinhard  
*University of Nebraska-Lincoln*, kreinhard1@mac.com

Adauto Araújo  
*Escola Nacional de Saúde Pública, Fundação Oswaldo Cruz*, adauto@ensp.fiocruz.br

Luis Fernando Ferreira  
*Fundação Oswaldo Cruz, Rio de Janeiro*, ludovico@ensp.fiocruz.br

C. E. A. Coimbra Jr.  
*Escola Nacional de Saúde Pública, Fundação Oswaldo Cruz, Rio de Janeiro, Brazil*

Kathleen Fuller  
*University of Kansas School of Medicine*

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American Hookworm Antiquity

Karl Reinhard¹
Adauto Araújo²
Luiz Fernando Ferreira³
Carlos E. A. Coimbra⁴

Fuller (1997) presents a refutation of the evidence of ancylostomids (hookworms) in the New World. She argues that the life cycle of hookworms limits them to warm, moist environments and, thus, confines their prehistoric distribution to the Old World. Smith (1990) emphasizes that very little is known about the ecological parameters of egg and larval survival and that
eggs and larvae can survive in cold environments. Fuller’s hookworm life cycle is oversimplistic and does not address the diverse array of infection modes available to *Ancylostoma duodenale*. This species is capable of mucosal penetration and, therefore, can cause infection if its larvae are eaten. In other words, *A. duodenale* is a geohelminth, and the host can be an active player in the infection mode. Hypobiosis of *A. duodenale* diversifies infection modes (Schad 1990). The larvae have the capacity to infect through milk, and there is strong evidence that they can also migrate into the fetus. Experimental evidence indicates that the larvae can enter hypobiosis in the muscle of animals and can infect humans who eat the meat of such animals. Thus infection can occur without an extracorporeal phase.

Fuller also refutes the diagnoses of ancylostomids from archaeological sites. The first diagnosis of ancylostomids in the New World was based on the recovery of adult worms from the intestine of a mummified body dated to pre-Columbian times (Allison et al. 1974). Allison et al.’s documentation consists of light microscopy, scanning electron microscopy (SEM), and histology, and they present a diagnosis of *Ancylostoma duodenale*. Fuller’s attempt to refute Allison et al.’s diagnosis is based on the following points: (1) “The southern coast of Peru is totally free of hookworm infection” (Fuller 1997:301, citing Chandler 1929); (2) the mummified adult worms are smaller than worms found in clinical settings (Fuller 1997:302); and

1. Karl Reinhard, Ph.D., is a pathoecologist and parasitologist who is interested in the ecology of prehistoric and modern parasitic disease and has authored 30 publications on the subject at the School of Natural Resource Science, University of Nebraska, Lincoln, NE 68588-0368, USA.
2. Adauto Araújo, M.D., Ph.D., is a paleoparasitologist who is particularly interested in the prehistoric distribution of parasites and the implications of these distributions for understanding prehistoric migrations. He has authored over 30 publications on paleoparasitology at the Laboratório de Paleoparasitolgia, Escola National de Saúde Pública, Fundacao Oswaldo Cruz, 1480 Rua Leopoldo Buhloes, 1480.21041-210, Rio de Janeiro, Brasil.
3. Luiz Fernando Ferreira, M.D., Ph.D., is the founder of paleoparasitology in Brazil and, for 20 years, has actively traced the distributions of parasites, both modern and ancient. He has over 100 publications on the subject, and he works at the Laboratório de Paleoparasitolgia, Escola National de Saúde Pública, Fundacao Oswaldo Cruz, 1480 Rua Leopoldo Buhloes, Rio de Janeiro, Brasil.
4. Carlos E. A. Comibra Jr., Ph.D., is a medical anthropologist who is interested in the ecology of parasitic diseases of indigenous populations of the Amazon and has authored over 50 publications in this area. He is affiliated with the Escola National de Saúde Pública, Fundacao Oswaldo Cruz, 1480 Rua Leopoldo Buhloes, Rio de Janeiro, Brasil.
(3) the eggs found in the intestinal contents of the mummified body are more advanced in embryogenesis than is typically the case for eggs studied in a clinical setting. From these three points Fuller concludes that what Allison et al. diagnosed as hookworm (*Ancylostoma duodenale*) was actually pinworm (*Enferobius vermicularis*).

Fuller’s assertion that the southern coast of Peru is totally free of hookworm infection is simply incorrect. More recent studies than Chandler (1929) show that hookworm infection is endemic in the area (Faust and Russell 1964; Camillo-Coura 1970; Naquira, 1990). As of 1994, there was a documented 1.6% prevalence of ancylostomid infection among the people living in the Andes and a 0.33% prevalence among coastal Peruvians (Elliot and Caceres 1994:36). *Ancylostoma duodenale* is even more common on the Pacific coast. Therefore, Fuller’s first point is invalid.

Fuller’s second point is also invalid. Her use of worm size as a diagnostic criterion reflects her lack of familiarity with parasite taphonomy. Size should never be used as a diagnostic criterion for desiccated adult nematodes (Reinhard et al. 1986). Nematodes have neither an endoskeleton nor an exoskeleton, so the worms shrink as they desiccate. In our experience, only egg size can be used as a diagnostic criterion.

Fuller’s third point relates to the state of larval maturation found by Allison et al. She notes that “fecal hookworm eggs are at the four-cell or eight-cell stage of embryonation when they are expelled from the host... Therefore, it would not be possible for these presumed egg casings to contain larvae.” She fails to recognize that hookworm eggs embryonate in a variety of environments, including distilled water, saline solution, the interior of refrigerators, and even dead hosts. It follows that the state of embryonation of nematode eggs in fresh feces cannot be applied as a diagnostic criterion for determining the nature of paleoparasitological remains.

Fuller’s alternative diagnosis—that is, what Allison et al. actually found was *E. vermicularis*—cannot be accepted. According to Fuller: “Given the size and morphology of the ‘worms’ in Allison et al. (1974) photos, it is probable that they are the larval stage of *E. vermicularis*.” It is impossible to confuse an adult hookworm with a larval pinworm. The animals are in two separate nematode orders (Strongyloidea and Oxyuroidea, respectively). The *A. duodenale* adult possesses a muscular esophagus and four teeth. It feeds by engulfing a section of intestinal epithelium, using its teeth as an anchor and cutting mechanisms, and it attaches itself to the wall of the small intestine. This is precisely what Allison et al.’s photos demonstrate; thus there can be no doubt that they were dealing with adult *Ancylostoma* worms.
Fuller then turns her attention to Ferreira et al.’s identification of ancylostomid eggs, as reviewed by Horne (1985) and Araujo (1987). Ancylostomid work in Brazil is complicated by the fact that only eggs and larvae have been found. Experimental analysis of artificially desiccated eggs indicates that the egg morphology of some species can be used as a diagnostic criterion (for a review, see Reinhard et al. 1986). Brazilian researchers turned to the SEM study of larvae (Araujo 1987; Reinhard et al. 1987), which indicated that the microstructure of ancylostomids preserves and can be used for diagnosis.

Again citing Chandler (1929), Fuller suggests that at least some of these finds were misdiagnosed because the remains from one of the sites come from northeastern Brazil, an area in which hookworm is absent. However, later researchers, whom she does not cite, did encounter hookworms in that area. Camillo-Coura (1970) found an 18% prevalence of ancylostomid infection among the population in northeastern Brazil. As with the eggs found in Allison et al.’s Peruvian mummy, so with the Brazilian eggs: Fuller argues that the fact that they are embryonated negates the possibility that they come from hookworms. She is apparently under the mistaken belief that “hookworm larvae do not develop until the feces come in contact with warm, moist soil” (Reinhard et al. 1986:303). She also believes that moisture level within the cave was insufficient for hookworm embryonation. These opinions reflect a lack of knowledge of both fecal ecology and the variability in hookworm embryonation. Fecal pellets in dry environments undergo surface desiccation, which protects the inside of the feces from drying. Thus, for a period of time, embryonation occurs inside feces that look dry. It follows that it is possible to find unembryonated eggs, embryonated eggs, and free larvae in the same feces. Also, hookworm egg embryonation is not dependent upon coming into contact with warm, moist soil, as Fuller asserts. She presents an alternative diagnosis of *Strongyloides stercomlis* for the Brazilian ancylostomids. This diagnosis is not consistent with the *Strongyloides* life cycle, as *S. stercoralis* lay eggs that hatch in the intestine and are rarely seen in stool.

Fuller’s literature review misses some important points. First, the diagnosis of the Brazilian ancylostomids was long ago debated in the literature (see Horne [1985] for a review). The result of that debate was the acceptance of the ancylostomid diagnosis (Horne 1985; Kliks 1990; Reinhard 1990; Náquira 1990; Merbs 1992). Her discussion of the Brazilian finds, based as it is on a 1929 reference, is hopelessly dated; she simply does not seem to
realize that the parasitological community at large accepts our diagnosis. Fuller presents no actual diagnostic basis for altering previous conclusions, and she neglects a number of ancylostomid-egg finds in North America that have been discussed fully by Reinhard (1990).

References


Response

Kathleen Fuller

Based on the analyses presented in the preceding two rebuttals of my 1997 article, it would appear that these researchers have proven that the

1. Kathleen Fuller, Ph.D., is a biological anthropologist interested in the interaction between human phenotypes and the environment, and the impact this has on health and disease. She has authored several articles on a variety of topics in this area. She is the Associate Director of the Center for the Study of Race and Ethnicity in Medicine, and Research Assistant Professor of Preventive Medicine at the University of Kansas School of Medicine, 3901 Rainbow Blvd., Kansas City, KS 66160, USA.
eggs, worms, and larvae they found are, indeed, those of human hookworm. While I would like to unconditionally accept these conclusions because doing so would make the issue of hookworm in the Americas much more intriguing, I still have a number of concerns related to environment. While individual worms may be able to survive in an inhospitable environment, this does not guarantee that a population of worms could flourish in one.

I am flattered that my brief comment on Faulkner’s find—a single egg containing an unidentifiable larva—generated a major research project; although I do not see how the impetus for such a project could be attributed to me. I am fully aware of the historical evidence for hookworm in miners who worked moist European caves. Of the environmental regions discussed here, Tennessee is the only one for which I could most readily accept the evidence of pre-Columbian hookworm—had Faulkner had more evidence than a single egg (Faulkner 1991).

Between Chandler’s (1929) publication and Faust and Russell’s (1964) publication there was adequate time for the microclimate of certain areas of coastal Peru and/or northeastern Brazil to change due to a dramatic increase in population density and altered sanitation conditions. This changed local environment could indeed be conducive to an influx of hookworms (or other helminths), carried via infected humans. In this way, hookworm could become established in a formerly inhospitable environment (Reinhard 1988).

Northeastern Brazil is environmentally similar to the Southwest U.S. The hot, arid climate of the Southwest provides excellent conditions for the preservation of human fecal material. Analysis of coprolites from one Archaic (6900-4800 BC) and two Anasazi (AD 500-1250) sites found no evidence of parasitic worms in the Archaic foraging population but did find parasitic worms (primarily pinworm) in the more densely populated, permanently settled agricultural sites. There was no clear evidence of hookworm (Reinhard et al. 1987; Reinhard 1988) either here or in the other putative hookworm sites in North America (Reinhard 1990). Neither Chandler (1929) nor Faust and Russell (1964) found any evidence of hookworm in the Southwest. The Brazilian sites seem to be foraging sites (7230 ±80 BP; 3610-3370 BP) although one is possibly a postcontact agricultural site (AD 1450-1590); it is difficult to say because the timespan given for one of the sites is so broad (Fuller 1997). It is hard to understand how a presumably small forager population, living in impermanent villages in a hot,
arid region of Brazil, could have had a high enough incidence of hookworm to leave signs of it—particularly when a dense and settled population living in the environmentally similar Southwest and suffering from parasitic worms did not.

For anyone considering Hawdon and Johnston's (1996) Trans-Berengian hypothesis involving hypobiosis and maternal transmission of hookworm via breastmilk, I suggest re-reading my critique of the Trans-Berengian route (Fuller 1997), focusing on the word “endemic.” In order for their hypothesis to work, hookworm must be endemic in the region. If migrants somehow brought hookworm with them, it must become endemic in the region for hypobiosis to continue. Both Chandler (1929) and Faust and Russell (1964) agree that hookworm was/is non-existent from the Siberian coast of Asia, through Berengia, and well past the Northwest Coast of the Americas. Furthermore, Faust and Russell (1964) indicate that the hookworm species predominating in the Americas (especially the Southeast U.S., Central America and the Caribbean, and northeastern Brazil) is *N. americanus*, which does not undergo hypobiosis (Schad et al. 1973).

Extraordinary claims—that hookworm was found in regions outside its normal environmental distribution during the pre-Columbian era—require extraordinary levels of documentation. In the conclusion of my article (Fuller 1997), I suggested doing DNA analysis of the putative Peruvian hookworms. (Essentially, the Tennessee and Brazilian material consists of a few eggs the researchers would probably be loath to sacrifice.) Genetic information on both types of human hookworm is published (e.g., Monti et al. 1998), and DNA from mummies in the same region was sequenced for presence of *Treponema* (Rogan and Lentz 1994). Therefore, it appears that DNA analysis of the Peruvian worms could be conducted and would conclusively resolve the issue. If it turns out that it is indeed *A. duodenale*, then we have a very intriguing situation. Given that the Trans-Berengian and Trans-Pacific routes are precluded, from where could hookworm have travelled to the Americas?

References


