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Worms, slugs and humans: the medical and popular construction of an emerging infectious disease

Vermes, caramujos e humanos: as construções médica e popular de uma doença infecciosa emergente

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Abstract

The identification of the worm *Angiostrongylus costaricensis* parasitizing land snails and humans in Southern Brazil suggests under-diagnosis and under-notification of patients with abdominal angiostrongyliasis. This article analyzes how the concept of abdominal angiostrongyliasis was constructed in different ways in Costa Rica and Brazil and how these changes affected the understanding of its clinical and epidemiological diagnosis. The research shows that abdominal angiostrongyliasis is, *de facto*, a sociocultural construct, although the parasites and vectors are real. The analisys also shows the importance of an interdisciplinary approach for understanding disease.

Keywords: medical knowledge; public understanding of disease; emerging infectious disease; abdominal angiostrongyliasis; normal and pathological.

Resumo

A identificação do verme Angiostrongylus costaricensis *em humanos e caramujos, no sul do Brasil, sugere a ocorrência de subdiagnóstico e subnotificação de pacientes com angiostrongilíase abdominal. O artigo analisa as diferentes construções do conceito de angiostrongilíase abdominal na Costa Rica e no Brasil e a influência dessas variações em seu diagnóstico clínico e epidemiológico. Demonstra que a angiostrongilíase abdominal é,* de facto, *um* constructo *sociocultural, embora parasitas e vetores sejam reais. Também destaca a importância da abordagem interdisciplinar para a compreensão da doença.*

Palavras-chave: conhecimento médico; entendimento público da doença; doença infecciosa emergente; angiostrongilíase abdominal; normal e patológico.

In 1995, a land slug identified as *Sarasinula linguaeformis* (Thomé, Arruda, Silva, 2007) invaded areas planted with corn, beans, and soy between the municipalities of Nova Itaberaba and Planalto Alegre in the State of Santa Catarina, in southern Brazil. 307 individuals belonging to 74 families lived in the area. Besides agricultural losses, residents were concerned about the presence of large numbers of slugs in their residences, gardens, pigsties, and chicken coops, which constituted their main sources of income. Several farmers decided to plant other crops and three families abandoned their properties.

In 1999, public officials from Nova Itaberaba asked the Agricultural Research Company (Epagri) of Santa Catarina to investigate the problem. The company sought advice from experts from the Institute of Biosciences, Pontifical Catholic University of Rio Grande do Sul [Instituto de Biociências da Pontifícia Universidade Católica do Rio Grande do Sul], and the possibility that the local human population could be harboring an emerging parasitic infection was raised.

Graeff-Teixeira (1986, 1991) had previously shown that a land slug belonging to the family Veronicellidae could harbor a helminth parasite of man. *Sarasinula linguaeformis* was found in Nova Itaberaba, and is described in the medical literature as an intermediate host of the nematode *Angiostrongylus costaricensis*. It was also known that during its infectious phase, the parasite is found in the mucus secreted by the slug and that people could possibly be infected through contaminated food, especially salads. Moreover, retrospective studies brought to light previous information on anatomopathological findings indicating the presence of infected individuals in the general area (Luzzi, Newmann, 1989). In this way, an agricultural pest became a public health problem.

A medical doctor and an agronomist conducted a field study in order to collect and examine slugs and to screen the human population at risk. Four blood samplings were performed, in August 2000, December 2000, April 2001 and August 2001, in which between 173 and 127 individuals were tested. From the 54 individuals that participated in all four tests, 41 were found positive in at least one test. Of the mollusk specimens sent to the experts at the Institute of Biosciences in July 1999, 86% were found to be infected with the nematode *Angiostrongylus costaricensis*.

In each visit to the area, the routine was the same: a short talk by a medical doctor and an agronomist, followed by delivery of the results of the blood tests, a clinical-epidemiological survey of those individuals who had tested positive, and further blood sampling.

At that time, the press gave ample coverage, highlighting the number of people infected and the names of the properties where slugs had been found. This succeeded in alerting the population to the risk of contracting a disease but also showed the social relevance of research in order to raise the necessary funds and attention from the authorities concerned. On the other hand, the news created further problems. As one researcher said: "People from the town would come up to us saying, 'I saw on the TV' or 'I read in the paper that the slug can transmit a worm that produces such and such symptoms. Well, at our house we show the same symptoms, so now what are we supposed to do?'" (Epagri technician, 2002). Furthermore, the farmers living in those localities became stigmatized. As one interviewee said: "Nobody comes to our festivities anymore, and they also refuse to buy our products."

This article explores the social construction of a new disease both from the point of view of the medical profession and from that of the affected population. We intend to show how the concept of abdominal angiostrongyliasis was constructed in different ways in Costa Rica and Brazil and how these differences had an impact on the understanding of its clinical and epidemiological diagnosis, as well upon the definition of normal and pathological by both the doctors, and the families affected. Abdominal angiostrongyliasis is, *de facto*, both a scientific and popular construct, one whose definition kept changing as a result of the different diagnostic methods used by various researchers. However, while we recognize the sociocultural construction of diseases, we must consider parasites, slugs, wild reservoirs, lesions and organic dysfunctions as objective and real, existing regardless of whether they were known at the time, and of their scientific or popular taxonomic status. They could have existed even before the appearance of mankind.

We must clarify our usage throughout the text of the expression social construct. First and foremost, it applies to the concept of disease (Avila-Pires, 2008). Diseases are constructs in the sense that they are collective entities, their definition being the result of a combination of signs and symptoms gleaned from individual observations of many patients. Hardly any patients will show all the signs and symptoms described as characteristic or diagnostic of a given disease. Evidently, the construct depends on the sociocultural context in which the researcher works.

Secondly, it applies to the public interpretation of those signs and symptoms, and also to how people translate the scientific/medical discourse on them.

The construct of disease from a medical point of view

The present study is one of a disease in which the symptoms are vague, for which there is no cure and in which, in the majority of cases, a diagnosis is only made when the parasite is seen and isolated through biopsies, or detected through serological tests when the result suggests the presence of an infection. There may be crossed reactions with other simultaneous or past parasitic infections and treatment with an anti-helminthic may result in the parasite migrating to other organs. Clinical manifestations occur in sites where the symptoms can be misinterpreted as those of other diseases such as appendicitis or abdominal tumors.

The eggs of *A. costaricensis* are not expelled in the feces of the human host (Benenson, 1983; Acha, Szyfres, 1986; Kazura, 2007; Morera, 1973; Graeff-Teixeira, 1986; Graeff-Teixeira, Camillo-Coura, Lenzi, 1991). Furthermore, abdominal angiostrongyliasis is considered to be under-diagnosed due to the lack of a specific, sensitive serological test; and to the failure to submit surgical samples for anatomopathological analysis, especially in cases clinically diagnosed as appendicitis, or, if samples are sent, to their being discarded in a short period of time; as well as to the general lack of knowledge about the disease among clinicians.

These considerations did not come easily or by chance; they are the result of years of research and reformulations. The study of the history of this pathology allows us to demonstrate how the 'truth' was constructed as we know it today, even if it is provisional.

The formulation of the concept of abdominal angiostrongyliasis required the establishment of four investigative processes: (1) clinical and anatomopathological research; (2) identification of the parasite and its life cycle, which led to the identification of the definitive hosts (rodents) and the intermediary ones (mollusks); (3) epidemiological study; (4) the diagnostic methods available, especially the serological tests to be performed on the population at risk.

Following the description of the parasite, the initial studies were complemented or reformulated by other study groups in various countries on the American continent. It is important to highlight, however, that the discovery of the disease, the parasite and its hosts occurred at about the same time.

In 1967 in Costa Rica, Céspedes et al. published an article describing a clinical case of a new pathology. In the same year, Morera (1967) suggested the presence of a parasite. In 1970, the intermediate and definitive hosts were identified and in 1971, the entire set of etiology, biological cycle, pathology and clinical and anatomopathological characteristics were described in three articles (Morera, Céspedes, 1971; Morera, 1970; Morera, Ash, 1970).

In Brazil, Agostini, a medical pathologist, described the first findings of the parasite in 1983 and also described the characteristic intestinal lesions. He was instrumental in awakening interest in studying the emergent disease in Brazil. His former student and collaborator, Graeff-Teixeira, became the chief investigator and leader of the Itaberaba Project, which investigated the outbreak of angiostrongyliasis in the region of Nova Itaberaba, in Santa Catarina State.

The first two processes (anatomopathological study and identification of the parasite) were conducted by several groups, with an emphasis on the Costa Rican and Brazilian doctors. The Brazilians were the first to conduct long-term epidemiological research, followed by a study conducted in Honduras in 1988. The diagnostic methods, as well as serological and other tests, were carried out by different researchers. A detailed account of these studies can be found in Grisotti, 2003.

In order to understand the process of construction we analyzed works published in Costa Rica and other Latin American countries from 1967, the year when the pathogenic lesions and the life cycle of the parasite were described, to 2002. Instead of a chronological analysis, we conducted a typological one, covering the identification of the parasite and the elucidation of its life cycle, anatomopathological study, the epidemiology of the disease and the methods of diagnosis, with an emphasis on serological tests. In each of those categories we highlighted the most important sociological questions that emerged. Apart from surveying the literature, we participated in a seroepidemiological field study carried out in Nova Itaberaba from August, 2000 to August, 2001 and interviewed doctors and fifty local families out of the original 74. We sought answers to the following preliminary questions: (1) When and why was diagnosis through the identification of the parasite and anatomopathological studies replaced by serology? Which factors made this transition possible and why was it not accepted by all researchers? (2) How did the concept of an asymptomatic disease replace that of a serious acute disease, and how were cases cured without surgery explained? (3) Is abdominal angiostrongyliasis due to *A. costaricensis* a new disease or an old and under-diagnosed one? (4) Did the re-description of the parasite

cycle imply continuity or rupture? (5) What is the current significance of serological tests, and how are borderline individuals diagnosed as normal or diseased?

In the following paragraphs we try to answer each of the queries listed above.

(1) An extensive review of the literature shows how diagnostic methods changed over the years. At the beginning, as established by Morera and Céspedes in Costa Rica, diagnosis was made by visual observation or isolation of parasites, larvae or eggs through anatomopathological studies. Suspected cases were discarded in the absence of parasitological confirmation. Serological tests used by them (latex) were too problematic and unreliable. Between the 1970s and the 1990s, the identification of typical lesions and/or the results of serological tests as proposed by Graeff-Teixeira, Agostini, Camillo-Coura and Cruz (1997) became acceptable in Brazil.

According to our interpretation, the fact that Morera in Costa Rica and Agostini et al. (1984) and Rodriguez (1997) in Brazil were pathologists influenced their favoring diagnosis through anatomopathological findings and observation of the parasite. Graeff-Teixeira, being an epidemiologist and immunologist, adopted both intestinal histopathological lesions and what he called the morphological tripod (eosinophilic infiltrate, eosinophilic granulomae, and vascular alterations) as acceptable evidence for a diagnosis. As we see it, scientific background and experience may explain the difference in their preferences and consequently in the results they obtained.

(2) As to the change in categorization from a severe acute disease to a common asymptomatic illness, analysis of the literature also provides an explanation. While Morera (1986, 1988) warned about the severity of this disease, several authors described the occurrence of asymptomatic cases. According to Morera, the determination of the geographical distribution of angiostrongyliasis depends upon the presence of medical doctors who know about the disease. For Morera, all registered cases were diagnosed through an anatomopathological study of lesions detected during surgery, while mild infections in cases where surgery was not indicated went unrecognized.

In Brazil, Graeff-Teixeira (1991) reviewed previous findings, the biological cycle of the parasite, its epidemiology with emphasis in Southern Brazil, clinical aspects of the disease, differential diagnosis, treatment and prophylaxis. Concerning the differential diagnosis, that author recommended that in endemic areas, medical doctors should always suspect angiostrongyliasis when a patient presented abdominal symptoms followed by leucocytosis and eosinophily. By that time, Graeff-Teixeira accepted the possible existence of asymptomatic cases or the spontaneous remission of symptoms. These hypotheses would be confirmed later through seroepidemiological studies (Graeff-Teixeira et al., 1997).

In endemic areas, Graeff-Teixeira obtained information that suggested the occurrence of a considerable number of cases that evolved to complete remission. Cases where intestinal obstruction and/or perforation occurred were relatively rare and were the only instances when a diagnosis was made through anatomopathological confirmation. Here again, the explanation for this change in the concept of the disease is found in the analysis of the different diagnostic procedures. While Morera identified severe cases of disease through an anatomopathological exam, Graeff-Teixeira identified infection, or contact with the parasite, in the absence of clinical disease through serology. On the other hand, the results of

serological tests performed by Graeff-Teixeira and witnessed by us in western Santa Catarina, where 86% of the slugs collected were infected, posed a new series of unanswered questions. How to explain the immunological response of individuals who showed successive positive and negative responses in serological tests? Were the parasites eliminated? In what way? We must bear in mind that the early literature showed the degeneration of worms could cause thrombosis, among other problems.

(3) As to the possibility of abdominal angiostrongyliasis being a new disease, Graeff-Teixeira in his doctoral thesis pursues the initial topic of his masters' dissertation: is it a new human parasitic disease? Is it under-diagnosed? Graeff-Teixeira's conclusion was that this is not a new disease in Southern Brazil, and that there is no evidence of association with cases due to environmental changes, like those caused by the widespread use of pesticides in Rio Grande do Sul, as suggested by Agostini et al. (1983). Graeff-Teixeira stated in several of his articles that the disease was not new and that it was under-diagnosed.

Graeff-Teixeira's field research revealed that human infection does occur and is commonly asymptomatic. His findings differ from those published in Costa Rica, where the disease seemed to be common. While for Morera, lack of medical knowledge was the main obstacle preventing demonstration of the high prevalence of the disease, for Graeff-Teixeira, in Southern Brazil, infection is frequent, but the disease is rare, and for the most part asymptomatic. These differences in opinion derive from the different approaches followed by those authors. Morera reviewed individual case studies and anatomopathological results of tests, while Graeff-Teixeira (1991) relied upon seroepidemiological investigations in populations exposed to contamination in areas where intermediate hosts were present, using a test developed by himself. This type of population study was not reproduced elsewhere. In spite of Graeff-Teixeira's conclusions about the possibility of under-diagnosing of the infection and the disease, we cannot admit the existence of something that does not exist, but we also cannot deny the possibility of its presence, undetected, because of a lack of investigation.

Graeff-Teixeira's research indicated the need to revise the accepted diagnosis of the disease as described by Morera and the prophylactic procedures to be adopted. For Graeff-Teixeira there is no correlation between abdominal pain and positive serology, and those who persistently test positive represent a minority.

As to the recommendation for not treating patients with anti-helminthic drugs, Graeff-Teixeira, Camillo-Coura, and Lenzi (1991) based their opinion on experimental models which showed that the use of drugs was associated with further deterioration of the lesions.

Regardless of the differences of opinion between Graeff-Teixeira and Morera, both agree that angiostrogyliasis is not a new disease, but it is under-diagnosed.

(4) As to the question of the redescription of the biological cycle of the parasite in Brazil and of whether it constituted a rupture (discontinuity) or a reformulation (continuity) in the history of disease, we ought to recognize that, ever since the early description of the disease in Costa Rica, the possibility of the existence of mollusks and rodents in its cycle was recognized. As a parasitologist himself, Morera was aware that all species of *Angiostrongylus* passed through mollusks and rodents during their life cycles. Together with Ash (1993), he searched endemic areas and discovered a large number of infected specimens of *Vaginulus*

(*Sarasinula*) *plebeius* Fischer. They examined domestic and wild mammals captured around houses and found the parasites in two species of rodents, *Sigmodon hispidus* and *Rattuts rattus*. In this case, we see continuity in the history of discoveries, as analyzed by Canguilhem (1984), since the authors used theories already accepted in parasitology to further their knowledge – the identification of the parasite's biological cycle. Eighteen years later, Mota and Lenzi's redescription of the parasite's life cycle did not differ from the basic plan proposed by Morera but raised important questions that Morera missed: (a) Why is the *A. costaricensis* cycle so different from other metastrongylides worms, which pass through the pulmonary circulation after leaving the lymphatic system? (b) How to explain Morera's hypothesis of the passage from the lymphatic to the arterial system by penetrating the mesenteric muscular arteries from the adventitia side, a phenomenon not observed in any kind of parasitic disease? (c) How to explain the presence of larvae in the lungs and liver, especially in the portal veins? (Mota, Lenzi, 1995, p.706).

The question of continuity/discontinuity in the history of science as discussed by Bachelard (1972) and Canguilhem (1984) helps us to understand the meaning of the re-description of the life cycle of *Angiostrongylus* larvae. In the case of the construction of Chagas disease, Delaporte (1999) showed how discontinuities, mistakes and epistemological ruptures permeated the process whereas in the present case there was continuity, with the accumulation of new knowledge.

(5) In 1991, Graeff-Teixeira, in his PhD thesis, developed an immunoenzymatic test for the detection of antibodies that was 86% sensitive and 83% specific, and was applied in two seroepidemiological studies prior to its use in the case of Nova Itaberaba. As Graeff-Teixeira remarked, the test is an important way of detecting healthy carriers with sub-clinical infections and no symptoms of the disease. It was of the utmost importance for the planning of preventive public health measures.

In the case of angiostrongyliasis, there is no possibility of performing coprological studies, as eggs are not found in the feces. The isolation of the parasite is only possible through a surgical procedure and so it is innapropriate for a parasitological survey. In a seroepidemiological survey we must contend with a number of borderline individuals, but clusters of positives and negatives are sufficient to provide an epidemiological picture. Borderlines are important for the clinician, but not for the epidemiologist. Evidently, the occurrence of a number of borderline individuals had an impact on the population's view of the disease, which will be described under the item public understanding of the disease.

Although Graeff-Teixeira's Elisa test is far more specific than others, he showed certain reservations concerning its sensitivity and specificity due to the unavailability of specific purified components and antigens against *Angiostrongylus costaricensis*, which could result in cross reactions with other helminthiasis. However, the number of high responses to the test indicating contamination showed its value for epidemiological studies, although borderline cases were important from the clinical point of view.

In many instances serology is the only available test used for epidemiological surveys. Tuberculosis and neurocisticercosis are good examples. At present, researchers in Brazil are developing a test for neurocisticercosis and Chagas disease, in order to be able to determine

rates of infection in human populations. Actual known rates for angyostrongiliasis are deduced from anatomopathological specimens.

As to the question of classifying individuals as healthy or normal versus diseased we must turn to Canguilhem (1966), as he discussed the arbitrary nature of this decision, which presupposes universal validity.

The results of the first serological tests performed during the epidemiological study showed a wide variation from the established parameters chosen to define infection, together with other signs and symptoms. Since a large number of individuals were clustered around the cut-off line, Graeff-Teixeira decided to revise it in order to accommodate those people who showed an adjoining titer of 0.9 to 1.1 when a titer above 1 was considered positive. Furthermore, a review of the literature showed also a wide degree of variation in the number of leucocytes and eosinophiles, one of the three diagnostic signs of the infection.

Some important theoretical and practical questions were then raised. In what way did the different percentiles of eosinophiles and the changes that resulted from the raising of the cut-off line change the status of the normal (negative) and the pathological (positive)? What was the sociocultural impact of those changes? Specifically, what was the impact of the change in the cut-off line for medical knowledge and for the individuals concerned? Although in terms of scientific research, those changes in the percentiles were merely technical measures of adjustment, this was not so for the population concerned, to whom they represented a renewed source of subjective worries and objective socioeconomic problems. As the cut-off line was raised by the researcher, a few of the former positive individuals tested negative or healthy. In the event, it increased the degree of confidence in relation to the individuals with a high titer.

Apart from the specific outcomes of this study we should consider some other issues.

In order to understand the epidemiology of this disease it is necessary to use correct methodology to estimate the actual prevalence of the infection in mollusks. Bias in collecting procedures may easily falsify the results, leading us to overestimate or underestimate this important epidemiological factor.

An extensive review of the literature in addition to the results of empirical research shows that, until now, the surest way to reach a clinical diagnosis was through a biopsy of surgical specimens. Identification of the parasite will only take place during a surgical procedure or a necropsy, and only if the pathologist is looking for it, and trained to do so. In daily practice a pathologist usually concentrates on confirming a suspicion of cancer, disregarding other possibilities. This is one of the limitations that we believe results in this disease being under-diagnosed.

A great many of the medical papers on this subject that have appeared in Latin American literature are repetitive and contribute little to the history of the construction of the disease. In some instances, erroneous information appearing in an early paper is repeated subsequently, without verification. The construction of a disease is made through a composite description of individual case histories, but in the parasitological textbooks the more striking cases are chosen as illustrative. This leads to the generalization of such rare instances distorting the concept of a pathology. An example is the repetition of the first

findings of angiostrongyliasis in the Hospital de Niños de Costa Rica (Loria-Cortés, Lobo-Sanahuja, 1980), suggesting that the disease was a pediatric illness.

In spite of several references to cases occurring in the United States (Hulbert, Larsen, Chandrasoma, 1992; Liacouras, 1993; Ubelaker, Hann, 1979), there is a noticeable absence of references to abdominal angiostrongyliasis in the analyses of cases of negative appendectomies or unspecific acute abdominal pain in periodicals such as *The American Journal of Surgery*, the *British Medical Journal* and the *British Journal of Surgery* during the 1980s and 1990s.

Public understanding of the disease

Abdominal angiostrongyliasis is considered an emerging disease in medical thinking but it is a completely new illness for the population of the region affected. To the people at risk interviewed by us, it was a new disease, and one that caused a number of social and psychological problems.

In order to assess the public understanding of the 'slug disease,' as it became locally known, we took into consideration the warnings of Radley and Billing (1996), for whom many a narrative that disregards the context in which health and disease are understood and fails to consider how things are being said by focusing only on what is being said, ends up by naturalizing perceptions and behaviors of individuals, as if their simple answers during interviews translated their actual opinions and behavior.

In Nova Itaberaba and Planalto Alegre, 24% of the individuals who were tested in the first round of serological exams were found positive. These results were not subsequently reconfirmed for some individuals, causing widespread concern, and raising doubts in the population as to the reliability of the diagnosis.

As we have described, widespread acceptance of serological tests among the population since the early days did not dispel a feeling of apprehension. The recurring question was: "If there is no treatment, why do we need to undergo repeated blood tests?" They understood the need for one blood test, but not four. "Why so many times?" All the more so when they learned that the tests did not lead to a cure.

Also, the reaction of test subjects as they received the results of their blood tests and. as the news spread among the community was one of anxiety and nervousness, since people did not understand why they did not all become sick, as they all lived in the same house and ate the same food.

One of the most important problems stressed during the interviews concerned the publication of press reports on the contamination of slugs and humans in that area causing the real estate and symbolic value of their lands and of some agricultural products to fall. Several nurses from the health units and supermarkets from the neighboring towns banned the acquisition of vegetables from areas where infection had been detected.

Not only were there economic losses, but also the residents themselves were stigmatized. Several people we interviewed reported that residents from neighboring areas avoided participating in gatherings and celebrations held at the community center for fear of contamination. Many among them said that the slugs could stick to the wheels of their

cars and thus be brought home. Furthermore, when the press announced that the first slugs had arrived in 1993, with the introduction of fruit trees imported from São Paulo, the 'culprit' was readily identified. In an interview, a resident confirmed that he had suffered considerable anxiety on that account.

In the search for answers and interpretations we identified a wider scenario for the 'slug disease.'

Although at the beginning of the research into angiostrongyliasis the main concern of the population was with the disease, that soon changed to the prejudice and stigma generated by its presence in the region, infecting slugs and people.

Graeff-Teixeira recognized that, as far as the population was concerned, abdominal angiostrongyliasis was a disease he had himself created. It already existed for science, but not for the local residents. According to him, after the third blood test, the results indicated that angiostrongyliasis should not be considered a public health problem. However, slugs persist as an agricultural pest, as do the social and economic repercussions of the episode.

One of the lessons to be learned is that epidemiological investigations of this sort should be accompanied by sociologists and anthropologists, preferably with some training in the medical/health aspects of their disciplines, to prevent or reduce the negative impact of the problems we witnessed and described.

The construction of abdominal angiostrongyliasis: empirical evidence and a theoretical problem

Diagnosis of abdominal angiostrongyliasis in western Santa Catarina confirmed the hypothesis of under-diagnosis and under-notification of the illness due either to a lack of knowledge concerning disease in general or to deficiencies in epidemiological monitoring by official health services. It is possible that the illness was known to science, but unknown to a large number of doctors and to epidemiological surveillance services.

From a detailed analysis of the sociophilosophical literature, it is clear that this hypothesis – so widely accepted in biology, medicine, and to a certain extent in epidemiology – had become extremely controversial for sociologists. The central question is: how can we confirm the existence of an illness about which little is known?

Latour's argument (1998) concerning the death of Pharaoh Ramses II due to tuberculosis is illustrative. For him, Ramses could not have died of tuberculosis 3,000 years ago, as the bacillus was only discovered by Koch in 1882. The answer to this question given by historians is that the object (the bacillus) was present when Ramses was alive and that scientists only discovered it later. For Latour, this explanation suggests that illnesses exist naturally, independently of the observer and of the context of analysis. But it also suggests that it was necessary to wait until 1976 to give a cause of death for the pharaoh. For Latour (1998, p.35), to admit that the pharaoh died of tuberculosis is an anachronism: "We cannot project into the past a modern invention."[1] For him, therefore, there is a history of the discovery of the world by scientists, but there is not a history of the world itself.

Latour and Woolgar (1979) argued that scientists, through the results of their laboratory research, speak in the name of nature reaffirming the socially constructed character of

scientific facts. However, their argument does not solve or help to answer the question of how to deal with unknown objects and phenomena (either still unclassified or scientifically constructed) such as certain types or organisms or 'non-human' beings (in the expression used by Latour) that lead us to face real situations, such as illness and death.

The studies of Latour and his followers led to the development of analyses that began with the identification of a set of appropriate techniques whose object of investigation was the very process of scientific production. However, contrary to their intention of offering a new vision of the production of scientific facts, they revived the old controversies found in the works of Foucault (1969) and Canguilhem (1984), and more recently, Delaporte (1999). The relationship between the discovery by scientists of an artifact or of the etiology of an illness and the previous existence of non-human reservoirs and vectors of illnesses and their natural sylvan or enzootic cycles has always been and still is polemical.

In his *Études d'histoire et de philosophie des sciences* (Studies of history and philosophy of sciences), Canguilhem (1984) used the example of crystallography and the crystal to illustrate the difference between the historicism of a scientific discussion (history of science) and the object of the science. The first belongs to a scientific discourse, constructed through objective propositions subjected to verification, concerning the crystal as a natural object, while the second – the crystal – has, in relation to science and the discourse for which it is an object of knowledge, a certain degree of independence, which allows it to be considered a natural object. However, Canguilhem, in a footnote, states that a natural object is not essentially natural, but is an object of the experience and the perception of a culture.

This same situation, where a historical nature independent of the observer does not exist, is found in some fragments of the works of Latour (2000) and of Delaporte (1998, 1999) as we can see in the following quotes: "These actors are the microbes, that, before Pasteur, had no existence for man" (Latour, 2000, p.302)[2]; "If we didn't see [the disease] it is not due to a faulty observation, but because it was invisible" (Delaporte, 1999, p.182)[3]; "Trypanosomiasis, as it appeared at the time of the conquest, is an authentic new disease. But the trypanosomiasis as described by Chagas at the beginning of the twentieth century is a false new disease, as it is four centuries old" (Delaporte, 1998, p.24).[4]

Bearing in mind the differences shown by Canguilhem between the history of scientific discourse and the object of the science, we can return to the polemic regarding the mummy of Ramses II which we described earlier. Avila-Pires (2008) considers that when we say that Ramses II died of tuberculosis we actually mean, in abbreviated form, that the remains found by archeologists and attributed, on the basis of independent evidence, to Ramses II, exhibited physical evidence that paleopathologists were recently able to diagnose as being compatible with those caused by bacterial infection – in Pasteur's sense – of a bacillus described by Koch in 1882 which may be identified by DNA tests. For Avila-Pires, the scientific definition of the disease and the subjective symptoms of the patient are intellectual and social constructs. The disease may be an abstraction but the lesions and the parasites (and other microorganisms) are concrete objects.

What changes, therefore, is the social history of disease. Its object is the investigation of reports, documents and testimonial evidence of the perceptions of a certain time period, place and set of people. These descriptions are influenced by subjectivity and result in a

construct. Therefore, according to Avila-Pires, while the concept of a disease, either on the individual level or on the epidemiological level, varies in time and place, the lesions found in the sick person are constant and objective, often indelible, surviving the person's death.

The polemic triggered by Latour needs, therefore, to be properly reformulated. We recognize that all scientific knowledge is the product of a construct which is dependent on a sociocultural context. What we question is the tendency to overlook the distance between scientific constructs that we willingly use at the present time, and the need for new explanations about the diversity and complexity of life, including all living organisms, not just the human species.

The greatest human and scientific challenge that results from those facts is to learn how to deal with the uncertainties and complexities of living organisms, as we realize the limited and abstract character of scientific constructs in the face of the immense complexity, diversity, and environmental variety in which we are immersed and which we strive to understand. Besides providing an awareness of the complexity of life, what other benefit would be gleaned from the statement that facts are scientifically (socially) produced if, to paraphrase Gould (1996), ants continue to ruin our picnic and bacteria to take our lives?

In our study we demonstrated that abdominal angiostrongyliasis is, *de facto*, a sociocultural construct, whose medical definition kept changing as a result of the different diagnostic methods used. It is important to remember that, initially, in Costa Rica, the disease was known as *granuloma eosinofílico parasitario intestinal* or intestinal parasitic eosinophilic granuloma (Robles et al., 1968). However, while we recognize the sociocultural character of the disease, nevertheless – and here we distance ourselves from Latour (1999) – we accept that the parasites and the vectors involved are real, and exist regardless of whether they were known at the time and of their scientific or popular taxonomic status.

The construction of this disease arose from a combination of investigations performed in Costa Rica and in Brazil, using different, complementary methods and techniques of research and diagnosis.

The different circumstances in Brazil (Nova Itaberaba) and Costa Rica led researchers to adopt different methods of diagnosis. First, those methods arose from the abundance of surgical cases in Costa Rica and the need for the development of a method to be used in epidemiological surveys in Brazil. Second, the backgrounds of Morera and Graeff-Teixeira differed, since Morera was primarily a pathologist and surgeon and Graeff-Teixeira an immuno-epidemiologist. Third, as knowledge is not static, the medical construction of a disease changes over time. In order to describe the parasite and to investigate its biological cycle Morera depended upon previous research on parasitology and zoology carried out in other places, in different circunstances and time periods. Also, serological tests were based upon earlier knowledge and they continue to be improved.

Another factor is the territorial size of both countries. In Costa Rica, Morera was able to convince people of the importance of pathological investigation in all cases of suspected appendicitis and of abdominal tumors, while in Brazil anatomopathological studies were only performed by a handful of medical doctors with a special interest in parasitology.

Both methods have limitations and advantages, and they had a different impact on the affected population. The anatomopathological method has led to the isolation of the

parasite, then being the more evident, but it is restricted to surgical cases and to clinical research, while the serological test, which is still not 100% specific, supplemented by studies of the parasite cycle in non-human hosts and vectors, provides sufficient information for understanding the dynamics of the disease. As we saw, the serological test has a greater impact upon the population, particularly as regards the risks and uncertainties surrounding the means of contamination.

Although both methods have limitations, they both contribute to our understanding of the pathology and the epidemiology of the constructed disease, making comprehensible the interrelationships between human beings, slugs and parasites.

The understanding of an emergent disease depends not only on its medical construction but also on anthropological and social analysis of how it is understood by the public and, therefore, this study highlights the importance of interdisciplinary work.

NOTES

[1] "*On ne peut pas faire rétroagir sur le passé une invention du présent.*" In this and other citations of texts from non-English languages, a free translation has been provided.

[2] "*Ces acteurs, ce sont les microbes qui, avant Pasteur, n'avaient pas beaucoup d'existence pour les hommes.*"

[3] "*Si on ne la voyait pas [the disease] ce n'est pas en raison d'une observation défectueuse, mais parce qu'elle était invisible.*"

[4] "*La trypanosomiase, telle qu'elle apparaît au moment de la conquête, est une authentique maladie nouvelle. Mais que la tripanosomiase telle que la décrit Chagas au début du XXe siècle est une fausse maladie nouvelle, puisqu'elle est vielle de quatre siècles*."

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