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Some Elements of the Regime of Management of Irrelevance in Science

Fabrício Monteiro Neves

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Abstract In contexts of scientific production deemed peripheral, knowledge produced is depicted in a condition of inferiority relative to that produced in other contexts; the daily practice of science is then guided by values and procedures, be they conscious or not, of peripheralization. This research note discusses the constitution, reproduction and generalization of peripheralization into what I call a regime of management of irrelevance in science: a scientific process with its own pragmatic and value content, whose elements will be presented in this work. These elements were identified during field research in laboratories and interviews with key interlocutors (research leaders) of biotechnology research teams in Brazil. What matters here is instead of taking the center/periphery dichotomy as an objective structure of the scientific system—a common approach in science and technology studies—it is shown as expecta-tions with practical repercussions.

Keywords Center and periphery ▪ peripheralization ▪ science and technology studies ▪ scientific hierarchies

Alguns elementos do regime de gestão da irrelevância na ciência

Resumo Em contextos de produção científica declarados periféricos, o conhecimento produzido é representado como inferior ao produzido em outros contextos. A prática da ciência é então guiada por valores e procedimentos, conscientes ou não, de periferização. Esta nota de pesquisa discute a constituição, reprodução e generalização da periferização no que eu chamo de regime de gestão da irrelevância na ciência: um processo científico com seu próprio conteúdo

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Fabrício Monteiro Neves
Department of Sociology (SOL), University of Brasilia (UNB), Brazil
email: fabriciomneves@gmail.com

pragmático, cujos elementos serão apresentados neste documento. Estes elementos foram identificados durante pesquisas de campo em laboratórios e entrevistas com interlocutores-chaves (líderes de pesquisa) de equipes de pesquisa em biotecnologia no Brasil. O que é importante aqui é que, em vez de reproduzir a dicotomia centro/periferia como uma estrutura objetiva do sistema científico - uma abordagem comum nos estudos sociais da ciência e da tecnologia - é mostrada como uma coleção de expectativas com implicações práticas.

Palavras-Chave Centro e Periferia ▪ Perifericalização ▪ Estudos de Ciência e Tecnologia ▪ Hierarquias científicas

Algunos elementos del régimen de gestión de la irrelevancia en la ciencia

Resumen En contextos de producción científica considerados periféricos, el conocimiento producido se representa en una condición de inferioridad respecto al producido en otros contextos; la práctica cotidiana de la ciencia se guía entonces por valores y procedimientos, conscientes o no, de periferización. Esta nota de investigación discute la constitución, reproducción y generalización de la periferización en lo que llamo un régimen de gestión de la irrelevancia en la ciencia: un proceso científico con un contenido pragmático y valórico propio, cuyos elementos serán presentados en este trabajo. Estos elementos fueron identificados durante una investigación de campo en laboratorios y entrevistas con interlocutores clave (líderes de investigación) de equipos de investigación en biotecnología en Brasil. Lo importante aquí es que en lugar de tomar la dicotomía centro/periferia como una estructura objetiva del sistema científico -un enfoque común en los estudios de ciencia y tecnologíase muestra como expectativas con repercusiones prácticas.

Palabras Clave Centro y Periferia ▪ Periferización ▪ Estudios de la Ciencia y la Tecnología ▪ Jerarquías Científicas

Editors' Note In order to ensure the widest dissemination of this article on the (ir)relevance of knowledge produced in peripheral areas, this article is published, in open access, by *East Asian Science, Technology and Society* and will also be published simultaneously and identically, in open access, in *Tapuya: Latin American Science, Technology and Society.* The article was peer-reviewed by colleagues from East Asia and Latin America in an effort by both journals to bring the argument developed by the author into dialogue with research traditions in the social studies of science and technology in both regions. The author retains copyright of the article, as per the open access guidelines of both academic journals.

1 Introduction

This research note seeks to list several elements of the regime of management of irrelevance, the scientific order of supposedly peripheral contexts of knowledge production. These elements were identified in the course of field research in laboratories and interviews with key interlocutors (research leaders) of biotechnology

teams in Brazil.[1] The rationale for choosing biotechnology lies on my research experience of over 15 years in this field alongside renowned researchers. The choice is also justified by the fact that Brazil was conferred international prominence in biotechnology mainly for research on tropical agriculture and neglected diseases, which has endowed the country an important role in the related publishing scenario (Bound 2008). From these experiences, I began questioning myself: "How is it that, despite their notoriety, they depreciate their science so much?"

In contexts of scientific production imagined as peripheral, the produced knowledge is reduced to a condition of inferiority relative to that produced elsewhere. In these contexts, the daily practice of science is then guided by values and procedures, should they be conscious or not, of peripheralization, a scientific process with its own pragmatic and value content, whose constituent elements will be presented later in the text.

The imagery currently associated with contexts of scientific practice takes for granted a geopolitics of knowledge reinforcing hierarchies related to "good and bad science," "advanced and backward science," and "center and periphery." To do science is also to establish a more or less accepted hierarchical order of taken for granted epistemological and methodological procedures that pervade scientific institutions across the globe. This order legitimizes itself by assigning either a peripheral or central status to the various spaces where science is practiced independently and before considering the quality of the study's results and its possible impact in the construction of knowledge.

In order to be effective, peripheralization must become routine in the spaces of science production. It is not necessarily conscious, nor would awareness of it allow for the redirecting of practices, due to entrenched contextual resistances reinforced daily. The reinforcement of local politico-economical interactional processes accounts for the difficulty in overcoming this state of affairs.[2] A self-deprecating and stable peripheralized scientific order is thus formed, a regime of management of irrelevance.

The process of legitimizing hierarchies in science, as I shall argue, rests on a practical, routine, disciplined, encouraged, and funded construction process that reinforces expectations as to how science should be and what it should produce. This very practice, in turn, echoes those hierarchical expectations which affect the judgment of projects, relevant objects, and science knowledge itself. This paper

[1]The following institutions were visited: Federal University of Minas Gerais (UFMG, Belo Horizonte), Federal University of Rio Grande do Sul (UFRGS, Porto Alegre), Catholic Pontifical University of Rio Grande do Sul (PUC-RS, Porto Alegre), Catholic Pontifical University of Paraná (PUC-PR, Curitiba), Embrapa's National Center for Genetic Resources and Biotechnology (EMBRAPA/CENARGEN, Brasília), Oswaldo Cruz Foundation (FIOCRUZ, Rio de Janeiro), Federal University of Viçosa (UFV, Viçosa, Minas Gerais), Federal University of Juiz de Fora (UFJF, Juiz de Fora, Minas Gerais), Federal University of Pernambuco (UFPE, Recife), and Federal University of Ouro Preto (UFOP, Ouro Preto, Minas Gerais).

[2]Interactions with colleagues in the laboratory, in congresses and thesis committees, policies for science and technology, private financing—all this finds a local existence embedded with values and practices in scientific activity.

discusses those hierarchical expectations, their content and consequences for everyday scientific practices.

In addition to exposing and further analyzing the empirical findings of the study, I will endeavor to shed light on the values and elements that—intentionally or not—constitute the process of peripheralization.

This paper unfolds in four parts, aside from this introduction and the conclusion. The next section discusses the hierarchical differentiation of center-periphery in the framework of the theory of social systems, seeking primarily to articulate it to the practical dynamics of the interaction systems. The aim is to show how a regime of management of irrelevance stabilizes and generalizes itself even at the level of in interactional processes. Later on, the previous discussion will be addressed from the social studies in science and technology perspective, mainly emphasizing that these studies examine science through its contextual, localized practice. This "contextuality" of science is fundamental to think through the implications of the hierarchical differentiation of the scientific processes of knowledge production. Following this, the study details the dynamics of hierarchical differentiation, here termed contextualization so to characterize what we call processes of peripheralization and centralization: practical substrates of the regimes of management of irrelevance and relevance. in the last part, we arrive at the heart of the argument, where we present several elements of the scientific order of supposedly peripheral contexts of science: Core of the regime of management of irrelevance.

2 Center/Periphery

The center/periphery differentiation theories gained notoriety when it was addressed in its geographical, economic or political dimensions, common in classical approaches to the problem[3] which were typically structuralist, as, for instance, in the theories of modernization and dependence of the Economic Commission for Latin America and The Caribbean (ECLAC). This hierarchical perspective was characterized by a macro-sociological analysis of the "global capitalist economy" and the "political and territorial influence," which assumed a heuristic based on markers of intensity, time-line, and divergent cultural patterns. Theoreticians spoke of advanced and backward capitalism, metropolis and colony, and central and peripheral modernity. This criterion was also used to differentiate regions such as the West and the East, the South and the North, indicating deterministic geographic content.[4]

Social studies of science in Latin America have also considered, from their origins, the center/periphery difference as a structural dimension of scientific

[3]See Shils (1992) for an influential approach, and Cueto (1989), aiming at a discussion on the concept of science.

[4]It is in this intellectual and political context that the pioneering studies of the so-called Latin American thought on science, technology and society (PLACTS) arise. Through its several phases, they were based on the most varied theoretical strands and empirical models, which are difficult to systematize in the space of this article. So, for the purposes indicated here, the synthesis offered by Dagnino, Thomas, and Davyt (1996) is still useful. The works of Keim (2008, 2011) and Alatas (2003) are also inspired by Latin American dependency theory and could offer an interesting approach to the center/periphery difference problem.

dynamics in approaches on imperialism, in proposals for scientific and technological modernization, as well as in the search for original scientific traditions (Herrera 1971; Sábato 1975; Sagasti 1983). They take for granted the existence of a superior science that was not developed in the geographical regions considered peripheral, mostly regions that were not in North America or Europe.

For Hebe Vessuri (1984), for example, at the level of knowledge, periphery of science means "normal science," i.e. solving puzzles proposed at the center; regarding themes, the periphery would be characterized by the application of central sciences; and at the institutional level, the periphery would be characterized by fragile institutions, subject to more general institutional ruptures. Schwartzman (2001) uses the same principle when approaching problems of hierarchy in science. He considers that in the "periphery," only "normal science" would be produced in comparison to the "center," which would be characterized by "revolutionary" science. Kreimer (2006) shares Vessuri's assumption when he mentions a mega-network that has emerged in the contemporary internationalization of science. His perspective reproduces the division of labor between global and local scientific groups, central and peripheral groups. It is from these geographical divisions that Kreimer builds his approach on scientific hierarchies.

The important works of the Latin American tradition of social studies of science voice only a structural and objectivist sense for thinking about scientific hierarchies. I argue that such hierarchies should be assessed complementarily through the expectations of scientists. Expectations are traversed by hierarchical values that manifest themselves as effective binary differences (good and bad science, cutting edge and backward science).

Since hierarchical perception develops into expectations, better than taking it as a concrete structure, it is important to understand the practical consequences this has for the construction of research. The identification of oneself or others with one side of the hierarchical difference center/periphery produces identity (central or peripheral) and, consequently, induces the practical direction of actions, peripheralizing or centralizing them. In this sense, one can speak of central or peripheral science without introducing territorial, national, or geopolitical markers.

De Giorgi (2017), within the framework of systems theory, speaks of "peripheralization" in reference to the macro-dynamics of this process. Of more interest to us here is De Giorgi's reference to floating peripheries and centers that do not have geographical limits of any order. Whereas it is salutary, theoretically, to rethink the center/periphery difference starting from its "delocalized, floating dematerialized" production (De Giorgi 2017: 44), this is not sufficient for the understanding of the construction of centers or peripheries in the dynamics of contemporary society. I want to introduce the concept of self-attribution processes of peripheralization or centralization when I refer specifically to the social system of science.[5] I also refer, unlike most systemic theorists, to a specific level of systemic-social formation: the interactions. On this level, self-attribution works in the context of a relational

[5]For a discussion on center/periphery difference in science operating from Luhmann's systemic framework, see Neves (2009), Neves (2014), and Neves and Costa Lima (2012).

logic that necessarily involves attributing to the other, simultaneously, one of the positions in the center/periphery difference.

Luhmann conceptualizes interactions, in contrast to organizations and societies, as simple social systems characterized by the physical presence of interlocutors engaged in communication; "They include everything that can be treated as being present, and may, in certain cases, decide among those present who should and should not be treated as present" (Luhmann 2016: 467). Luhmann uses binary code to denote distinctions that operate in the sphere of communication and that consist of self-excluding values, positive and negative which direct the communicative flow. Everything that is selected as positive in communication leads to ignoring the opposite side of the pair and any other possible course. This dynamic of ignorance relates to the process of "attention" according to the phenomenological discussion of Zerubavel (2015). For this author, attention works as a search light, which selects what is in its range and ignores everything that is left out. For Zarubavel and for Luhmann, the dynamics of attention and ignorance is socially constructed, indicating what is more or less valued by a specific cultural context.

It is important to note that such selections operate on a case-by-case basis, that is, there is no guarantee that selections made earlier may have validity for later moments of communication. In these contexts of communicative co-presence (laboratories, congresses, meetings, colloquia), a hierarchical form of selectivity guides the interactive course of communication toward the side of the binomial selected by those present (center or periphery), stabilizing the subsequent communications in this direction and reinforcing hierarchical expectations. Regimes of Management of irrelevance develop as a result of the generalization of (self) deprecating hierarchical expectations.

3 Science and Context of Practice

Numerous are the images of scientific practice still in circulation and their related values, communities, laboratories, norms, rules, dispositions, languages. A generally reverent set of images depicts the scientist as a rigorously trained person, an expert in a field of knowledge, someone possessing universalist values, an organism adapted to the esoteric conditions of laboratories and of scientific language. All these characteristics are mentioned in parliaments, in the media, in classrooms, international congresses, in books. Such images ignore the location and social conditions that underpin science, the scientist and the laboratory, and which imprint specificity where universality is boasted.

> It seemed that any more comprehensive effort to situate science in the places of its making would be taken as an assault on the integrity and authenticity of scientific knowing. Indeed, the modern invention of the laboratory can be interpreted as a conscious effort to create a "placeless" place to do science, a universal site where the influence of locality is eliminated. Securing credibility and achieving objectivity required "placelessness," and the triumph of the laboratory as the site par excellence of scientific plausibility since the middle of the nineteenth century bears witness to this prevailing conviction. (Livingstone 2003: 3)

The laboratory is the locus where this practice assumes its most widely credited qualities: disinterested, neutral, objective, plausible, and universal. The indication of a

laboratory is a mechanism of guaranteeing credibility and excellence, and of attributing relevance to the knowledge produced, through its scientists, technicians and apparatuses. Laboratories, however, are never assumed in their general sense. On the contrary, one speaks of the molecular genetics laboratory at the University of Cambridge, a laboratory of theoretical physics at the Massachusetts Institute of Technology, the Functional Genetics Laboratory at EMBRAPA. Research centers as these are located in institutions, states and countries, have specific Nobel Prize winners researchers, receive resources from particular agencies and financing companies, have ties with such and such other centers, publish in such and such magazines, with specific impact indicators. These characteristics are habitually used to identify excellence and relevance, and are important for the formation of hierarchies in science, notably those that separate "centers" and "peripheries."

> The image of a concentrated center – and its numerous research efforts – therefore dominates all our analyses of science, whether such analyses occur in the form of scholarly articles, textbooks, or in the shape of more popular science writing. Thus, when we think of science, we usually restrict ourselves to thinking about a center, which is usually thought of as embodied in some European or American scientific community. It is from within such central communities that all groundbreaking research is expected to emerge, including any research that leads to new discoveries. Let us call this image the central community model of science. (Dasgupta 2016: 382)

Science and technology studies (STS), however, point to another image of science, laboratory and scientific practice. They are attentive to the maximum contextuality and situational contingency of scientific practice (Knorr-Cetina 1981); the resulting knowledge reaches universality depending solely on the practical processes of support networks, social and technical ones (Latour 1987). STS not only relativized the cognitive superiority of scientific knowledge in the face of other forms of knowledge (Barry, Bloor, and Henry 1996) but also relativized their models and theories within Western science, considering its controversial and non-consensual, historical and situated nature. As a result of these studies, the validated model of the central science community was questioned to a large extent, considering the contexts of practical interaction within laboratories and their resulting products.

By bringing to light the practical processes of constitution of legitimacy, hierarchy, and universalization, STS leads to a more controversial, parochial and worldly image of science—where one acts, interacts and communicates in the same way as in any other social contexts of practice (Pickering 1992). It has been demonstrated that the "sacred space" of the laboratory is deeply rooted in more general processes of society. The small laboratory network is extended by enlisting spokespersons, who start to create other spaces and enlist other spokespersons. The extension of the network, however, does not only translate theories and methods but also appropriates performances, techniques, cognitive hierarchies, and hegemonic languages. In this way, it inevitably reiterates the places and the practices of places where the knowledge produced in laboratory "x" finds acceptance. Thus, acceptance and resistance to knowledge are rooted in local traditions of research, in interactional dynamics *in situ*.

> The outcome of controversies is frequently shaped by battles of evidence; thus, there is no doubt that a technical, universalistic decision criterion is influential and that the

> world has a kind of agency in decision-making of this sort. However, the ability to produce good evidence is shaped by research traditions that govern its interpretation, access to resources that govern its production, control over what counts as good methods, and the ability to mobilize rhetoric and colleagues to win arguments over the interpretation of data. (Hess 2001: 235)

STS grants us this image of science and of a social world in which local traditions of research, funding, researchers, rhetoric, values, and cultural beliefs interact. An image, as mentioned, very different from the generalized model of the central science community. This image makes it possible to understand the perceived hierarchical difference of center and periphery through the dynamics of network expansion, i.e. as situated practices of legitimation and hierarchization. In these practical processes, the hierarchy is tacitly assumed and starts to operate science with this assumption. It creates a geopolitical imagery of knowledge in the situated interactions with pre-suppositions that are structured as common sense, reproducing expectations of "central science" and "peripheral science," an imaginary with the symbolic effectiveness to influence the scientific practice.

The generalized model of a central community of science rules in practice as an imagery, as a guiding idea, as value, as a premise. It is through this model that scientists attribute value to what they and others do. The way in which such assumptions are contextualized and how they influence scientific practices should, therefore, be taken into account. As Longino points out:

> The role of assumptions in enquiry means that epistemological analysis of scientific theory and enquiry must include analysis of the social and intellectual context in which enquiry is pursued and theories and hypotheses are evaluated. The intellectual context is constituted of background assumptions and investigative resources - instruments, samples, experimental protocols. The social context is the set of institutions and interactions in and through which assumptions and resources circulate, as well as the larger social environment in which institutions and interactions are embedded. (Longino 2002: 176-177)

The consequences of using these hierarchical assumptions are decisive: to operate on one side of this dichotomy, through processes of self-attribution, means to ignore the other. I want to focus on this dynamics of ignorance stemming from the center and periphery differentiation. For Luhmann (1986), the result of a selection between two sides of a binary code that serves as a basis for other selections; when operating from one side of the code, the other side remains an unselected, ignored possibility. Consequently, a blind spot is created, i.e. the unselected side in the process of reproduction of society or, more specifically, in the process of reproduction of social systems, such as science. Many possibilities of truth are ignored, especially those considered peripheral. In this research note, I assume that the ignorance of those other possibilities of truth is due to—at least in part—the ignorance of supposedly peripheral spaces by both the "center" and the "periphery."

4 Contextualization

Social studies of science have a long tradition of considering the social context in which scientific practice takes place. For example, Knorr-Cetina (1981) and

Lynch and Woolgar (1990) assume the contingency and contextuality of scientific action taking into account the dynamics of symbolic processes. I call contextualization to the process of locating oneself or others in the "center" or the "periphery," along with its implications for a given hierarchical position in the system of acknowledgment of science. In particular contexts of scientific practice, contextualization is tantamount to processes of centralization in which the generated knowledge is positively valued, with expectations for publication and circulation; ultimately, with conditions for the universalization of how and what to produce. When knowledge is produced in these "central" spaces, it immediately assumes a positive value, going beyond its space of construction, transcending disciplines, languages, laboratories, and, eventually, countries. Apart from the value that the context (laboratory, research group, university, etc.) is given historically, this also ensues from the academic strategies that emerge from the assumptions inherent in the situation of considering oneself "central." As stated in Karen Knorr-Cetina, relevance is managed.

> The authors have established that they have something relevant to say, given their description of the state of affairs existing prior to their contribution. Thus, their right to insert their statement into the scriptures of a field through publication is legitimate. (Knorr-Cetina 1981: 112)

In other contexts of scientific practice, "contextualization" means "peripheralization." It is not just about the availability of material resources; rather, it is about assigning a negative value to what is done and how it is done, which leads to expectations of nonrecognition, non-publication, and restricted circulation (circulation oftentimes restricted to one's context). What matters in this article is, specifically, peripheralization with its attendant expectations and practices. In these cases, irrelevance is managed by blending the contextual expectations of insignificance, generalizing them. Such practices boil down to a regime of management of irrelevance, which reinforces those expectations through interactions in the same space of local practice, or through cross-linking in global spaces of practice, such as international conferences.

In order to exemplify the aforementioned plots enacted in the process of management of irrelevance, we take the recent case of Brazilian neuroscientist, Suzana Herculano-Houzel, internationally recognized for her research on neurons. After a career in Brazil heading the Comparative Neuroanatomy Laboratory at the Federal University of Rio de Janeiro (UFRJ), the author accepts an invitation from Vanderbilt University in Nashville to work in the United States. Upon leaving the country, Suzana gives an interview that offers a picture of that scientific imagery. She says: "We do science in Brazil in miserable conditions" and "Brazilian reagents and equipment are very expensive and bad" (Herculano-Houzel 2015),

> I have two foreign post-doctoral students in my lab, one is French and one German, and it is embarrassing when I have to say that we ran out of water in the bathroom, or that there is a power outage, or that the internet is intermittent. "I'm sorry, but this is our reality," I say to them. The most I can do is try to look on the bright side: "It's bad, but if you can work under these conditions, it will be wonderful when you return to your home country. You will put on quite a show." Because they've learned to work the hardest way possible. It is embarrassing. (Herculano-Houzel 2015)

"There" and "here" are articulated in such a way as to show a hierarchical framework of science based on linguistic elements such as "expensive and bad," "lack of water and energy," "no Internet." Then, the question that comes to mind is the same as the one asked by the attentive reporter: "And what's the secret to having so many well-recognized research studies, even with all these problems of lack of resources and infrastructure?" The answer indicates possibilities that are often ignored due to atavistic elements in our scientific imagery which reinforces the idea that modest resources are a sign of miserable and low-quality peripheral science.[6]

> It all depends on the type of research you can do, the type of question you pose, and the approach you use. Studying molecular genetics in this country, for example, is unthinkable. In my laboratory, we have been as successful as we have in the last few years because we have discovered a niche of very basic questions and answers in neuroscience - which are therefore striking and interesting to a large number of people - that can be approached with an inexpensive method, invented by me in my laboratory. Only for this reason. If I were to need anything else, our production would be much smaller than what it is today. (Herculano-Houzel 2015)

Inverting the author's argument, we can articulate a paragraph with the same expressions—"very successful," "cheap" method, "impacting"—to indicate a context of extreme importance for science. A global neuroscientific research center: in terms of the knowledge generated, this is what her articles and impact indicators mean. However, another narrative is assumed, which belittles Brazilian science based upon its financial and bureaucratic difficulties. At this point, cognitive and material aspects are mixed and knowledge is measured based on the economic needs of institutes, universities, and research laboratories.

The hierarchical presuppositions that structure the imagery related to science are traversed by a basic center/periphery difference that, through processes of self-attribution, operates in its practice, contextualizing research results. Peripheralizing or centralizing oneself also involves other people interacting in the spaces of science, who are judged on the basis of these hierarchical expectations. These expectations are reinforced in contexts of interaction and are generalized, informing scientific practices and assigning positive (center) or negative (periphery) value to what is or has been done. A regime of management of irrelevance encompasses this process of generalizing expectations and practices oriented by a self- and hetero-attributed negative value.

5 Elements of the Regime of Management of Irrelevance

The following discussion is the result of various researches I have undertaken over the last 15 years. I used the same qualitative research methods, which combined

[6]The difficulties are well documented in the literature of the sociology of science in Latin America and are still widely indicated in research on laboratory infrastructure, funding edicts and bureaucracy. The aim here is not to deny the existence of obvious limitations for scientific research in Latin America, but only to show that the cognitive value of the science done is diminished whenever the criterion of difficulties is used.

semi-structured interviews with key interlocutors (in a total of 63 interviews), systematization of the material through the QSR-NVIVO data analysis program, simple coding, and content analysis. I then cross-referenced objective information —regarding location, funding, international experience, the object of research, place of publication, bibliography used and research agenda, which could be found in the respective CVs on the Lattes Platform of National Council for Scientific and Technological Development (CNPq) at http://lattes.cnpq.br—with the interviews.

The first element to compose the regime is the posterior reference. From the practices of constructing research agendas to the standards for bibliographic citations, everything happens as if they were delayed, tardy. In this way, there is a tendency to refer to the "center" of production as if the legitimate cognitive product, the one worthy of reference must be there, ignoring what is happening in the "periphery." This explains the widespread adherence to global scientific agendas, which circulate through spaces of practice as if they were universal. Peter Burke once wrote that "antecedence makes reference" (Burke 1997). The approach of the present work takes both antecedence and reference as consequences of the practical formation of regimes of management of relevance/irrelevance.

Upon recognizing and assigning antecedence to the center, to the global agendas, a benchmark is created, a reference to be followed. It is from this supposedly international criterion that "peripheralized" scientific competition strategies emerge. In these spaces of practice, the "international benchmark "—or, as mentioned earlier, the idea of a central science community—discourages research on some antecedent themes and objects, prompting investigations into themes and objects neglected by "central" science and which do not form global agendas, therefore having little impact on the international publishing system.

Let us examine, below, the account of a well-known Brazilian researcher in the field of the biotechnology of "neglected diseases":

> We expect that, when working with diseases caused by parasites, *Schistosoma mansoni*, which occur in regions with low levels of development, in tropical and subtropical regions, working with diseases neglected by the pharmaceutical industry, the pharmaceutical market - and this is something that motivates a lot - there is a possibility of maintaining a level of competition with foreign groups, mainly in pharmaceuticals. Diseases like diabetes, Alzheimer's, obesity, coronary heart disease are all intensively researched by the pharmaceutical industry, so it's very difficult for you to enter such competition. On the other hand, by researching neglected parasites you have a little more time and you can do your research without being rushed about. Anyway, developing a drug is something that takes time, it is thirty, twenty years of research; the pharmaceutical industry, with technology and resources, takes around twelve, ten years. (Interview with author number 6)[7]

> If I take a project to analyze and I see ambitious themes, or pretentious ones, that demand time, money, personnel, machinery, I already discourage them. And if they are objects very alien to our reality, I disapprove. I warn my students: "be modest,

[7]Interview conducted at PUC-RS, Porto Alegre, Brazil with a researcher founder of a research laboratory in a private institution after retiring from a public institution.

> you are in Brazil". This is allowed, that is not. One cannot throw money away. (Interview with author 31)[8]

In the above statement, there is the manifest contradiction between the unquestionable relevance of the "neglected" agenda and the term used to characterize it, which indicates lack of synchrony, detachment, alienation. In other words, they are researches assumed with little relevance, since the relevance would be in research on "diabetes, Alzheimer's, obesity, coronary diseases." To assume such agenda is to have "no criteria at all," it would be "throwing money away." Localization mechanisms are evident ("be modest, you are in Brazil") when it is assumed that these neglected themes and objects are of interest to places of "low development level, in tropical and subtropical regions" and, supposedly, only to science in these places. The statement "I already discourage them" results from this locational dynamic. Its practical consequence is the disapproval of projects "alien to our reality," which would relate to global science agendas, attracting more editorial interest from international journals.

Posterior reference leads us to the second element to compose the regime, namely, the election of the object, or, as heard in the interviews, to exoticism. In regimes of management of irrelevance, which usually have few sources of funding, the incentive to do science is focused on objects that serve specific interests. There is a redirection of the research interests, in practical terms as well, ascribing to oneself and others the inability to follow supposedly central research protocols, the inability to reproduce them at a level of excellence and innovation comparable to those of the "center". The criteria for election of the objects must meet expectations about peripheral research; one must be forewarned about the place in which the search is conducted.

> We have criteria here, but some laboratories have no criteria whatsoever. A teacher's lab here got public money, and what did she want to do? She wanted to research breast cancer (sic). Breast cancer genetics is the most studied thing in the world. In general, in this day and age you have to be connected to a hospital, connected to a medical team, everything arranged for the thing to work. She did not think about these details; it was a total failure. It's something beautiful, grounded [just] in theory … (Interview with author number 20)[9]

> [if] You ask me what I'd choose: do badly what others do well, or do my own, do well, say, do something exotic, of interest to fewer people, journal … I do the exotic. Ah, but it's not relevant, nobody cites it. We have to conform; you can't aspire to the Nobel by researching sugar cane. (Interview with author number 33)[10]

> When I came to Brazil, I had to see where I could contribute and from there define my area of activity. [. . .] The research area that EMBRAPA proposed to me ended up going back to what I had done during my doctorate. It was not exactly the same thing, but it

[8]Interview held at UENF, Campos dos Goytacazes, Rio de Janeiro, Brazil. CNPq researcher professor, area of functional genetics.

[9]Interview conducted at UFMG, Minas Gerais, researcher in the field of tropical medicine.

[10]Interview held at EMBRAPA CENARGEN. Researcher Professor CNPq, area of Molecular Genetics and Microorganisms.

> was quite related to the subject of biofuels, renewable chemical compounds. The work that I developed during my doctorate in Sweden was focused on the production of ethanol biofuels, second generation, from wood, what we call biomass. That's what they had to produce from renewable sources. In Brazil, I am interested in doing research with second-generation ethanol, but our main biomass is sugarcane, given the specificities of each country and the industrial structure of each country. [. . .] We have specificities. The entire production of biofuels in Brazil, which began in the 1970s with ethanol, is unique in the world. I have to take into account what the Brazilian problems are the problems of Brazilian industry. But at the same time, from the technical point of view, the best techniques and best strategies, [I have to consider] the world literature. (Interview with author number 18)[11]

In these contexts, scientists find themselves between exoticism and submission, in the latter case ascribing themselves the quality of backwardness, of being out of step, as the first element of the regime suggests. After all, objects that are not in the global agenda are also classified as exotic and would supposedly result in studies that are unable to contribute to the frontiers of "central knowledge." This has practical consequences since this in-between the exotic and the submissive has unclear boundaries, unclear career strategies, and no expectations of recognition—a limbo, especially in editorial terms. In these two cases, the editorial refuge is generally in periodicals with little international circulation.

> There is a colleague who cited a classic case [of an expert who claimed] that the cerrado[12] is such, such, and such, and that it is not such. She [this colleague] had to explain to him what the cerrado was, because he showed a certain lack of knowledge. I assure you, the problem was not the language. The colleague was born in the United States, she was raised in the United States, she was native. You can see that it was a [case of a] prejudiced view. Why was that? "Ah, they are Brazilian researchers, Brazilian institution", and then you receive exaggerated criticism. I think this happens. I have seen this happen in other countries, not only in Brazil. Colleagues have told me "If I receive an article from a certain country, I am always hesitant". We cannot take a whole population and say that every work coming out of there is of questionable quality. (Interview with the author 45)[13]

Exoticism is generally related to a perception of editorial irrelevance. The publication is uncertain since exotic objects do not follow global agendas, coexisting with the low impact of their citation, with low probability of being read or cited later on. As Latour (1987: 40) wrote, "There is something still worse, however, than being either criticized or dismantled by careless readers: it is being ignored. Since the status of a claim depends on later users' insertions, what if there are no later users whatsoever?." Scientific expectations in these contexts, therefore, combine "neglect," "exoticism" and "ignorance," consolidating a space of practice ruled by the sense of irrelevance.

[11]Interview held at EMBRAPA AGROENERGIA. Researcher of Molecular Genetics and Microorganisms.

[12]Cerrado is the name given to the Brazilian savannahs.

[13]Interview conducted at EMBRAPA AGROENERGIA, researcher in the field of Molecular Genetics and Microorganisms.

> You write a good article, your colleagues read it, your PhD students read it, I write well in English, I am fluent, I have never had a problem. Then you send it to that prestigious international journal and you get mediocre reviews from people who do not know the area of research, you know? "Ah, in Brazil it is like that … ", but then "explain this, explain that". You put your hand on your head and say: "So much effort for that? There is prejudice, you know? Yes, there is. They ignore Brazil. Well, I am a reviewer of international journals, my reviews are better than the ones I receive. (Interview with author 41)[14]

The perception of being ignored is one of the elements in the regime of management of irrelevance. The dynamic of ignorance in supposedly peripheral contexts of science—which cannot be simply attributed to deliberate disputes for power and prestige since it also involves issues related to language and proximity—is a fundamental factor in understanding the constitution of scientific hierarchies. A dynamic of this nature is established based on the inattention, often an unconscious prejudice, to scientific products generated in other arenas, deemed as peripheral. Inattention to spaces and products is a result of scientific socialization, which steers the interest toward specific scientific products, neglecting others. Beyond being a condition of our sensory experience, attention is also a deliberate mode of interest toward things that are socially constructed as relevant:

> In short, we notice and ignore things not only as individuals and as human beings but also as social beings. While it is certainly Nature that equips us with our sense organs, it is nevertheless our social environment that so often determines how we actually use them to access the world. (Zerubavel 2015: 52)

Our focus of interest must be understood through an investigation that indicates, at a macro level, those products most valued by particular cultures; on the interactional plane, the dynamics of interest in valued products and processes that are relationally constituted, and on the individual plane, the biography of the observer. The dynamics of scientific ignorance is then constituted from products and processes valued and reproduced in these dimensions, from those considered culturally, relationally, and individually relevant. Everything else is a backdrop, irrelevant, periphery. The dynamic of attention structures the regime of management of irrelevance, forming a "community of attention," and reproduces it whenever the focus continues to be on "central" processes and products in these dimensions.

> Effectively delineating the scope of our attention and concern, it is particular attentional communities that often determine what we come to regard as relevant and to which we therefore attend. Such communities have their own distinctive attentional traditions and therefore also distinctive attentional habits and biases, as manifested in their members' [. . .] It is specific conventions of what is noteworthy, for example, that make the Sistine Chapel and the Coliseum such "must" attractions for visitors to Rome. (Zerubavel 2015: 52-53)

The regime of management of irrelevance as a generalized self-deprecating order constitutes a community of attention. In practice this is manifested in the election

[14]Interview held at EMBRAPA CENARGEN; Researcher Professor CNPq, area of Molecular Genetics and Microorganisms.

of research objects and which of those would be worthy of reference. Whenever the focus on spaces taken as central becomes generalized, a mutual reinforcement of self-derogating expectations is at work.

> It happens that you put [cite] foreign co-authors, from the center, and they guarantee [the publication of] the article. They reinforce their position and we reinforce ours. Well, this keeps everything as it is, the prestige and the backwardness. "Oh, it's someone else writing here"; All right, but that someone is never us. There have been cases of foreign people being called to publish the same article, just to give their name [to attach a foreign author's name to it]. You know, nobody pays any attention. Well, not even us, right? My colleague next door publishes it and I don't even know what it is about. (Author's interview number 12).[15]

The dynamic of ignorance and attention concerning the exchange of researchers accounts for the third element of the regime of management of irrelevance. The intensity of this flow of researchers between spaces of science production plays a fundamental role in legitimizing those very spaces of practice. According to Burris (2004), the traditional view of the academic hierarchy that directly associates departmental prestige to important publications and the acceptance of theories could not explain the perceptions of prestige which were often inconsistent with scientometric data. For this author, departmental prestige is an effect of networks and the exchange of researchers between institutions, that is, an effect of accumulated social capital in certain spaces.[16]

In the dynamics of academic networks, departmental flows, mainly of doctors (PhD's) and post-docs, are expected to move from the less prestigious departments to those of higher prestige, to short courses and to research exchanges (Xie 2014). Thus, the most prestigious departments tend to increase or stabilize their prestige, the opposite being true for those of lesser prestige. This process is well documented by the sociology of science through the Matthew effect (Merton 1968); this "means that eminent scientists receive disproportionately greater recognition and rewards for their contributions to science than lesser-known scientists for comparable contributions" (Xie: 2014, 809). Circulation and prestige go hand in hand in the constitution and reinforcement of attentional dynamics that promote hierarchies in science.

> I did a PhD abroad, post-doc fellowship and everything. If you take my most cited papers, they're the ones I published when I was in my PhD. My PhD in Wisconsin had, by far, the most impact on my career. You're challenged all the time by new topics, different areas … I watched my advisor, a super-famous guy, and saw how science developed in a TOP place. He was a reference. I found the idea of excellence in Wisconsin. There was a seminar room called Howard Temin, the guy who discovered reverse transcriptase, the most widely used biochemistry book in the world was from my university, the Lehninger. They instilled [that] in you. When they were bringing in new students for PhDs, they would get the students who were there at the time to help persuade them to come. There were fantastic people before you, but you had to

[15]Interview held at UFRGS; Researcher Professor CNPq, area of Functional Genetics.

[16]Xie (2014) discussed the effect of academic networks on increasing global inequality in science. It indicates that "central" universities, by attracting more foreign researchers, make these well-trained professionals work for their senior researchers, to whom these partnerships bring still more prestige.

> make sure that other fantastic people would come, so that this [the excellence] was maintained. (Author interview number 46)[17]

Such asymmetry in the flow of researchers leads to ideas of "methodological updating," "theoretical updating," and "modernization," which are very common in the vocabulary of international mobility programs in science offered by countries whose science is deemed peripheral. In Brazil's case, although not only in that country, the United States and Europe are prioritized, tacitly assuming these places as the centers of the relevant scientific production, most of the time by disbursing sums disproportionate to the already scarce amount of resources allocated for science and technology. The flow of students and professors from the "periphery" to the "center" reaffirms hierarchical positions and reproduces the model of the central science community in their countries of origin. It also carries symbolic resources, such as theories and methods that gain immediate visibility and local attention.

Asymmetry in flows takes us to the fourth element of the regime of management of irrelevance: the capacity for translation/diffusion, a process well documented by Medina (2013). The author acknowledges the extensive networks of scientific knowledge diffusion that have been structured through asymmetric flows between spaces of science production. This process bequeathed us the central community model of science, our attentional assumptions, and the notions of cognitive hierarchy structured around the center and periphery difference.

> If, as held by the actor-network theory, an idea can only go as far as the reach of the network that contains it, it can be said that the acceptance of an idea by academics from developing countries depends on the strength, density, and scope of the network which allowed the knowledge to reach places far from where it was produced. (Medina 2013: 9)

On the other hand, the restricted access to global practice spaces and the low editorial impact produce "asymmetric translations" (Medina 2013) of scientific knowledge. Medina is concerned with the hierarchies and particularities that arise when "[. . .] fields, social worlds, or the dependency of a transnational corporation or government, to name but a few areas are unequally equipped in terms of symbolic and material resources" (Medina 2013: 16). These differences matter to the effectiveness of the translations/reception; they point to the resolution, for example, of controversies and to the legitimacy of theories.[18] Some translations are more likely to become legitimate than others. With regards to this, Medina (2013: 17) refers to "asymmetric translations."

> What happens when the areas that condition the actors are different? What happens when actors are unbalanced when it comes to symbolic and material resources? How

[17]Interview held at EMBRAPA BIOENERGIA. Researcher Professor CNPq, Molecular Genetics and Microorganisms areas.

[18]Collins and Pinch (1993) share the same view on enumerating the factors that influenced the closure of the gravitational wave controversy. Doubts about Joseph Weber's experiment involved, among other factors, the prestige of his home university, his integration into scientific networks, and his nationality.

> viable are the translations and what effects do they produce? In order to start clarifying this topic, we will call asymmetric translations those that are produced by actors whose power is not comparable.

It is important to add to these asymmetries indicated by Medina their rooting in spaces of scientific practice, which make them natural, legitimate scientific values. Hierarchies are constructed not only from asymmetric academic flows but also from the legitimacy that scientific hierarchies acquire in the spaces of practice. Attention to the products of others' science, with a concomitant ignorance of one's own is core to the hierarchical value that structures the regimes of management of irrelevance around "center and periphery science." Asymmetric flows and translations reinforce as much as ensue from this regime. Thus, the power to translate (Medina 2013), in the conceptualization sought in this study, refers to the reproduction of the valuational and attentional presuppositions embedded in the regime of management of irrelevance. It is not the legitimacy and validity of the translation that are in question. In such contexts, there are regular "appropriation" and "assimilation" attempts, often delegitimizing conceptual innovations that recurrently emerge. The persistent image used, almost as a moral judgment, to refer to more autonomous processes of scientific construction in such supposedly peripheral context, is that of "backwardness."

6 Some Final Notes

In this work, we investigate the scientific practice in supposedly peripheral contexts, in which the scientific knowledge produced is belittled before that of other contexts. We have shown that the daily practice of science in these contexts is guided by values and procedures, conscious or not, of peripheralization, that is, a scientific process with its own valorative and pragmatic content, whose elements have been discussed earlier. The peripheralization is effective as it becomes routine practice in the spaces of science production, is reinforced through the trajectory of scientists, in everyday interactions and in the material structure that sustains science in its most diverse contexts. These articulated dimensions produce self-depreciation, low self-esteem, peripheralized and stable scientific order, here called the regime of management of irrelevance.

The regime of the management of irrelevance operates by locating such spaces of practice between the submissive and the exotic. This location is a publication limbo for scientists, who perceive themselves between the inattention of the "center" and the disinterest of the "periphery." This attentional dynamic produces asymmetric flows of researchers, who, attentive to the legitimated "centers," tend to prefer them to the detriment of the "periphery." As a consequence of these flows, the hierarchical order in science is reproduced without questioning and the regime of the management of irrelevance is reinforced.

The dynamics between "local" and "global" must therefore be viewed by the social studies of science as rooted in contexts of scientific practice and as forming processes of "centralization" and "peripheralization." Such procedures should not, however, be taken as a function of a national context, nor as being stable. On the one hand, the model of the central science community goes beyond political

boundaries and settles itself even in renowned laboratories; on the other hand, the center and periphery dynamic is constantly changing and building new forms of hierarchy. The images of "center" and "periphery" circulate as an expectation, as value (Luhmann 1986), which guides elections in scientists' daily practice and structures the dynamics of attention. From this orientation emerge the elements of the regime of management of irrelevance. Such findings also apply to the very "center" of STS. The mainstream of the area is recurrently updated with categories, values, theories, and practices that are dear to European and North American contexts (Law and Lin 2015, Lin and Law 2014). There is a complete lack of attention and interest in what has been happening in other contexts of science studies production, despite a rudimentary movement in the opposite direction, arising from supposedly "peripheral" contexts and individuals. Such a movement can produce collaborative dynamics that surpass consolidated hierarchies, over and above particular languages, practices and norms whose consequence is solely the ignorance of one's own and others' contexts of production of science.

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ORCID

Fabrício Monteiro Neves http://orcid.org/0000-0002-2886-0577

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Fabrício Monteiro Neves has a PhD in Sociology, is an associate professor at the Sociology Department of the University of Brasília and a CNPq productivity scholar. His research is on processes of peripheralization in science and technology.