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THE EMERGENCE OF GENETICS IN PORTUGAL: J. A. SERRA AT THE CROSSROADS OF POLITICS AND BIOLOGICAL COMMUNITIES (1936-1952)

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Abstract

This thesis aims to contribute to a better understanding of the history of biology in Portugal. Genetics in Portugal emerged in the 1930s. Why not before and how this took place were the two questions I proposed to answer in the fist part of this thesis. My approach focuses on the dynamics between different biological communities and on the influence other "national traditions" in biology had on a periphery such as Portugal. A historical survey of the first decades of the twentieth century regarding the extant sources on Mendelism is provided and the influence of the French tradition in biology is debated. In the second part, following the scientific life of the leading Portuguese geneticist J. A. Serra (from 1936 until 1952), I analyse the emergence of genetics in the Faculty of Sciences of the University of Coimbra, discussing the influence of German genetics during World War II. Here I add a further dimension by looking into the relation between science and politics, as a reciprocal one. Two main conclusions: 1) the emergence of genetics in Portugal during the 1930s played a crucial role on the migration of experimental practices among different biological communities; and 2) the consolidation of genetics in the university context of Coimbra was shaped and occasionally benefited from the political agenda of the New State.

Key words
Genetics, centre-periphery, national traditions, biological communities, Mendelism, husbandry, experimental practices, fascism, politics

Resumo

Esta tese pretende ser um contributo para um maior conhecimento da história da biologia em Portugal. A genética em Portugal emergiu na década de 1930. Numa primeira parte procurei compreender como é que isso sucedera e porque que não antes. Escolhi como objecto de enfoque as dinâmicas entre as diferentes comunidades biológicas e a influência de outras "tradições nacionais" em biologia num país periférico como Portugal. Numa primeira parte, é apresentado um conspecto histórico de fontes das primeiras décadas do século XX em que o Mendelismo é mencionado, e debatida a influência da tradição francesa em biologia. Numa segunda parte, acompanhando a vida científica do geneticista J. A. Serra (de 1936 a 1952), analisei a emergência da genética na Faculdade de Ciências da Universidade de Coimbra, discutindo a influência da genética alemã durante a Segunda Guerra Mundial e a recíproca relação entre a genética e o regime fascista português. Duas conclusões principais: 1) a emergência da genética teve um papel crucial na migração de práticas experimentais entre diferentes comunidades biológicas; e 2) a consolidação da genética no contexto universitário de Coimbra foi moldada e por vezes beneficiou da agenda política do Estado Novo.

Palavras chave
Genética, centro-periferia, tradições nacionais, comunidades biológicas, Mendelismo, pecuária, práticas experimentais, fascismo, política

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CHAPTER ONE

State of art, goals and methodology

1.1 State of the art: presenting Parts I and II

The history of the life sciences in the twentieth century remains a largely subsidiary subject of the history of science in Portugal, especially when compared to the traditional interest of Portuguese scholars in physics or mathematics.[1] The historiography of Portuguese biology is a fragmented territory of tangential studies and episodic historical accounts.[2] One should highlight the crucial contributions of the zoologist Carlos Almaça[3], whose positivistic approach to the history of science has successfully accomplished to organize several controversies, institutions and personalities, and the work of Isabel Amaral on the history of biochemistry, which unveiled several data and contextual patterns concerning the Portuguese experimental biologists of the beginning of the twentieth century. The historiography of genetics in Portugal, in *strictu sensus,* is composed of chronological accounts of former

[1] Conceição Tavares and Henrique Leitão, *Bibliografia de História da Ciência em Portugal 2000-2004* (Lisboa, Centro de História das Ciências da Universidade de Lisboa, 2006).

[2] A survey of the historiography of biology in Portugal is Maria do Mar Gago, "História da biologia em Portugal (1700-1946): a tradição naturalista, as ciências biomédicas e a emergência da genética" in Gonçalves, M. E. and J. Freire, eds., *Biologia e Biólogos em Portugal. Ensino, Emprego e Sociedade* (Lisboa, Esfera do Caos, 2009), pp. 171-214.

[3] For instance, Carlos Almaça, *Bosquejo Histórico da Zoologia em Portugal* (Lisboa: Museu Nacional de História Nacional, 1993); *O darwinismo e a Universidade Portuguesa (1865-1890)* (Lisboa: Museu Nacional de História Natural, 1999); *Museu Bocage. Ensino e Exibição. Museu Bocage* (Lisboa: Museu Nacional de História Natural, 2000); or "A Sociedade Portuguesa de Ciências Naturais, a Marinha e a Estação de Biologia Marinha", VIII Simposium, "O Mar no Futuro de Portugal", Lisboa, 2003.

geneticists.[4] Exceptions to this state of the art are the recent works of Tiago Saraiva which focus on the crucial role of geneticists in the construction of the New State [Estado Novo].[5]

Since the late 1980s genetics is on the spotlight of the history of science. Traditional historiographies of genetics, which tended to reflect the views of American geneticists and to focus on "transmission genetics"[6], were improved by historical investigations tracking down genetic research programs developed under a non-Mendelian tradition[7] or concerned with developmental and physiological aspects of heredity.[8] This new trend in the history of genetics often arose in association with genetic researches conducted in Germany[9], a country which was the European "centre" of genetics during the first decades of the twentieth century to such an extent that Jonathan Harwood contrasted different "national styles" in genetics in Germany and in

[4] Luís Archer, "Contribuições para a História da Genética em Portugal" *in História e Desenvolvimento da Ciência em Portugal no século XX* (Lisboa: Academia das Ciências de Lisboa, 1980), pp. 1027-1065; Aurélio Quintanilha, "História da Genética em Portugal", *Brotéria* XLIV, 3-4 (1975), 189-208; Miguel Mota Mota, "A Contribuição Portuguesa para o progresso da genética", International conference "Os Portugueses e o Mundo", Porto, 4-7 June 1985; and José Antunes Serra, "Contribuições Portuguesas para o Progresso da Genética. Tentativa de menção cronológica sistematizada", *Brotéria Genética*, 8 (1987), 17-34.

[5] Tiago Saraiva, "Laboratories and Landscapes: The Colonization of Portugal and Mozambique in the Twentieth Century", *HoST*, 3 (forthcoming 2009) and "Fascist Labscapes: Geneticists and the New Landscapes of Fascism in Italy and Portugal", *Historical Studies in the Natural Sciences*, 40 (forthcoming 2010).

[6] For example, Horace F. Judson, *The Eight Day of Creation. The Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979); or Ernst Mayr, *The Growth of Biological Thought* (Cambridge: Harvard University Press, 1980.

[7] Jan Sapp, *Beyond the Gene, Cytoplasmatic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987).

[8] (Harwood, 1987, 1993; Rheinberger, 2000; see also Max Planck project on the Cultural History of Heredity IV (2006), especially the papers of Staffan Müller-Wille and of Christophe Bonneuil).

[9] For example, Jonathan Harwood, "National Styles in Science. Genetics in Germany and the United States between the World Wars", *Isis*, 78 (1987), 390-414; or H.J. Rheinberger, "Ephestia: The Experimental Design of Alfred Kühn's Physiological Developmental Genetics", Journal of the History of Biology, 3 (2000), 535-576.

United States; France also revealed a special "national tradition" in genetics[10] (see chapter three).

The first attempt to rewrite the traditional historiography of genetics took into account how countries appropriated Mendelism differently – for example, in terms of their philosophical backgrounds towards the construction of biological knowledge and the institutional organization of an emergent discipline such as genetics. Part I of this thesis aims to contribute to the historiography of "national traditions" in science offering an original account which takes the point of view of a peripheral country – I plan to unveil which centers, or which genetic traditions, influenced the Portuguese appropriation of Mendelism.[11]

More recently, the history of genetics was again scrutinized by less "internalist" approaches: studies on the interaction between genetics and politics, especially in the context of policies striving at economic autarky[12] or racial hygiene under the Nazi regime[13]; studies on the role of agriculture in the construction of genetic knowledge and institutional emergence of genetics[14]; or studies of material culture following the

[10] Richard Burian, Jean Gayon and Doris Zallen, "The singular Fate of Genetics in the History of French Biology, 1900-1940" *Journal of the History of Biology*, 3 (1988), 357-402.

[11] In the historiography of science this approach has been worked out in the context of a working group dedicated to the European Periphery (STEP). Some works are: Ana Simões, Ana Carneiro, Maria Paula Diogo, eds., *Travels of Learning. A Geography of Science in Europe* (Dordrecht: Kluwer Academic Publishers, 2003); Ana Simões, Ana Carneiro, Maria Paula Diogo, "Perspectives on Contemporary History of Science in Portugal," *Nuncius* 23 (2) (2008), 237-263; Kostas Gavroglu, Manolis Patiniotis, Faidra Papanelopoulou, Ana Simões, Ana Carneiro, Maria Paula Diogo, Jose Ramon Bertomeu-Sánchez, Antonio Garcia Belmar, Agusti Nieto-Galan, "Science and technology in the European periphery. Some historiographical reflections", *History of Science* 46 (2008), 153-175.

[12] S. Heim, "Research for autarky. The Contribution of Scientists to Nazi rule in Germany", Forschunsgprogramm Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus, Ergebnisse 4 (2001); O. Elina, S. Heim and N. Roll-Hansen, "Plant breeding on the front: Imperialism, War and Exploitation, *Osiris*, 20 (2005), 161-179.

[13] Sheila Faith Weiss, "Human Genetics and Politics as Mutually Beneficial Resources: The Case of Kaiser Wilhelm Institute for Anthropology, Human Heredity and Eugenics During the Third Reich", *Journal of the History of Biology*, 39 (2006), 41-88; see 2005 *Osiris* issue.

[14] Some outstanding results can be found in the 2006 issue of the *Journal for the History of Biology.*

"evolutionary history" of model organisms genetically produced by humans.[15] This recent trend was able to establish connections between the work undertaken at laboratory scale and broader contexts such as the global standardization of food production or colonial projects.[16] Part II of this thesis aims at contributing to this new approach (less "internalist") to the history of genetics, looking at the scientific activity of one of the leading Portuguese geneticists, José Antunes Serra (1914-1990), through the scope of the interaction between science and politics.

1.2 Goals, methodology and description of chapters

Part I offers a historical survey of the extant sources regarding Mendelism during the first decades of the twentieth century – by Mendelism I mean both early Mendelism (1900-1914) and its chromosomal version (since 1915). My scope is the construction of genetics knowledge through an epistemological perspective attending to the dynamics between different biological communities. My main goal is to understand, in the framework of national traditions, which "centres" influenced the Portuguese appropriation of Mendelism. I pay special attention to the French (chapter two) and German (chapter three) traditions for their influence in the Portuguese context is more evident.

Chapter two analyze these sources from the standpoint of the ongoing discussion on the international historiography of genetics and point out some of the possible

[15] E. Russell, "Introduction: The Garden in the Machine: Toward an Evolutionary History of Technology", in Schrepfer, S. and P. Scranton, eds., *Industrialing Animals. Introducing Evolutionary History* (Nova Iorque/Londres: Routledge, 2004) pp. 1-16; Sarah Franklin, *Dolly Mixtures. The Remaking of Genealogy* (Londres: Duke University Press, 2007).

[16] Christophe Bonneuil, "Development as Experiment: Science and State Building in Late Colonial and Postcolonial Africa, 1930-1970", *Osiris*, 15 (2001), 258-281.

explanations *why* Mendelism was not appropriated by the Portuguese scientific community during the first decades of the twentieth century. Taking into account recent studies on the role of agriculture in the construction of genetic knowledge and the institutional emergence of genetics, I analyze separately the experimental biology community and the agronomic community.

The goal of chapter three is to understand *how* Mendelism was appropriated in the late 1920s in the university context. The scope of analysis is reduced to the Portuguese experimental biologists and to research at the University of Coimbra. According to historical accounts of Portuguese scholars, I argue that Portuguese genetics emerged in the 1930s. However, only a thorough, painstaking analysis of all scientific production published will be able to confirm whether or not genetic research did not take place prior to 1930, and point to the reasons behind the non-appropriation of Mendelism in the beginning of the century. In the absence of such long overdue joint work of the community of scholars what can be said individually is intrinsically tentative – nevertheless, I hope my preliminary work on the Portuguese appropriation of Mendelism may contribute to future researches on the history of heredity in Portugal.

Part II of the thesis is dedicated to the early scientific life of Serra. The period under analysis starts in 1936, the year Serra graduated from the Faculty of Sciences of the University of Coimbra, and ends in 1952, when he moved to the Faculty of Sciences of the University of Lisbon. My principal goal is to frame Serra's achievement in the context of the international trends in genetics and in the national political context of the New State.

Similarly to part I, I analyze the construction of genetics knowledge through an epistemological perspective attending to the dynamics between different biological communities. I was forced to take a broader view of "biological communities",

including also the veterinarians (chapter five). I follow closely the events in German genetics for their presence during the consolidation period of genetics in the University of Coimbra was very strong during the research period of this thesis. A particularity of Part II is the attention paid to the relationship between science and politics. Following the footsteps of Sheila Faith Weiss[17], I try to understand how Serra used politics as a resource to for scientific activity and how the New State used Serra to fulfil his political agenda.

In chapter four I describe *how* Serra took advantage of the eugenics in the national political context, *where* within the international community he got ideas and support for setting his laboratories and *what* strategy he pursued to accomplish international recognition in a short period of time and with poor laboratorial conditions. This chapter includes the period from 1938 (his first publication) to 1947 (when invitations from two important international biological institutes marked in a very clear way the beginning of his international career). It is important to underline that it was during the war that Serra organized the experimental conditions he needed at the University of Coimbra (in the Anthropological Laboratory, in the Zoological Laboratory and in the Chemical Laboratory).

In chapter five I focus on his collaboration with the National Board of Husbandry, regarding sheep genetics. I aim at understanding *why* the New State needed a "pure geneticist" such as Serra, and *how* did his collaboration with the veterinarians of the national board take place. Moreover I am interested in understanding and following the life of scientist who saw himself as "apolitical" after 1947. This chapter aims to be part of a recent trend in historiography of science that wants to rethink the relation

[17] Sheila Faith Weiss, "Human Genetics and Politics", Ibid. p. 41-88.

between science and politics during the fascist regime in Portugal, essentially viewed in the historiography of Portugal from the point of view of political persecutions.

My original goal was to follow the scientific life of Serra. His internationalization and his interest in epigenetics – a topic nowadays very much in vogue among biologists – were my principal motivation. However, after some investigation, it became clear to me that, in order to understand Serra's decisions and interests, I first needed to understand how genetics had emerged in the University of Coimbra, where Serra studied and conducted his first works on genetics. Moreover, I wanted to understand why genetics had not emerged in Portugal during the first decades of the century. I knew I could not make a systematic investigation on these topics, but I considered important at least to try to make a preliminary historical assessment of such topics. This aim is responsible for the division of my thesis into two parts. A first part consisting of a preliminary study of the appropriation of Mendelism in Portugal and a second dedicated to my original goal, to follow the scientific life of Serra. I limited my research to his early career (from 1936-1952), leaving for a future work the analysis of his epigenetic ideas on biology.

1.3 Historiography of genetics and its problems

The beginning of genetics is conventionally allied to the rediscovery of Mendel's work in 1900. Based on a series of crosses between different varieties of peas, the (Moravian) monk Gregor Mendel proposed in 1865 several general principles that explained the frequency with which several traits appeared among the offspring –

Mendel's laws of heredity.[18] Mendel's work was appropriated by the scientific community only three decades later, when "rediscovered" by the Dutch plant physiologist Hugo de Vries (1848-1935), the Austrian plant breeder Erich von Tschermak (1836-1927) and the German plant physiologist Carl Correns (1864-1933).[19] In 1900, all three claimed that their breeding experiments conducted in the late 1890s had given similar results to Mendel's crosses.

Historiography of genetics has described how within some years after 1900 Mendel's laws became a fundamental program of heredity research and how important was the role of the British biologist William Bateson in establishing a Mendelian agenda for heredity. Drawing on Mendel's theoretical work, Bateson coined the term "genetics" for the emergent research discipline of heredity (and variation) in 1905; Mendelian "factors" would be renamed as "genes" by the Dutch Wilhelm Johannsen in 1909.

The next important event in the history of genetics was the publication of the Mendelian chromosome theory by Thomas Hunt Morgan and his co-workers, based on their work with *Drosophila* at Columbia University, in 1915. This theory explained the mechanism of heredity as Mendel had conceived it based on its material basis

[18] There is a historiographical controversy on the significance that Mendel attributed to his laws. Should we say Mendel's "laws of hybridization" or Mendel's "laws of heredity"? Was Mendel a Mendelian? – asked Robert Olby in his 1979. Staffan Muller-Wille tries to sum up this controversy arguing that Mendel's achievement was a product of normal science and yet a revolutionary step forward. In the end, Muller-Wille concludes that Mendel could be read as a Mendelian when he was rediscovered in 1900 and his work understood as an attempt to understand heredity ("From Linnaeus species to Mendelian factors: Elements of Hybridism, 1751-1870", *Annals of Science*, 64 (2), (2007), 171-215).

[19] Raphael Falk, among others historians of genetics, underlined the fact that these "rediscovers" did not see in Mendel's work the universal laws for heredity. De Vries, for example, was interested in this kind of universality, but he sustained his own theory, so he did not support Mendel's particulate one; Correns, on the other hand, was less ambitious and although he knew he had discovered something new he thought Mendel's work could not be applied universally; finally, Tschermak, also did not have this kind of theoretical ambitions, being several times described in the historiography of genetics as a merely experimenter (Falk, "Mendel's Impact", *A Cultural History of Heredity III: 19th and Early 20th*, Max Planck Institute for the History of Science, Preprint 294, 2005, p. 11).

(chromosome). In terms of the history of scientific disciplines, Morgan's breakthrough marked the beginning of an alliance between genetics and cytology.

From a historical point of view, "genetics" is a moot concept and therefore leaves room for a series of misunderstandings. When Bateson minted the term "genetics", dubbing it the "new science of heredity (and variation)," he had a particular methodology in mind, viz., Mendelism, and based on this belief he introduced a "reductionist research agenda" in the study of heredity.[20] The fact that the beginning of genetics is bound to a particular research program of the study of heredity (Mendelian genetics) is apt to create a first type of ambiguity when we look back at the past, since not all geneticists supported Mendelian methodology.

Indeed, this mist is somewhat dispelled if we choose to focus on a much more innocuous word: "heredity".[21] Thus, according to Jean Gayon, the history of heredity can be divided in three periods: biometrics (end of nineteenth century), Mendel-chromosome (1900-1950) and molecular (second half of the twentieth century).[22] In this thesis, I only deal with the second period and I use the term "Mendelism" to designate either "early Mendelism" or its "chromosomal version". I opt to consider Mendelian chromosome theory more like an event than a conceptual step in the history of genetics, in spite of its historical significance, for example, from the point of view of scientific practices and material cultures.[23]

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[20] Raphael Falk, "Mendel's Impact", Ibid.

[21] Several contributions concerning the historical focus on "heredity" resulted from the workshop "A Cultural History of Heredity III: Nineteenth and Early Twentieth Centuries" that took place at the Max Planck Institute for the History of Science from 13 to 16 of January 2005.

[22] Jean Gayon, "From measurement to organization: A philosophical scheme for the history of the concept of heredity" in Peter Beurton, Raphael Falk and Hans-Jörg Rheinberger (eds.), *The Concept of the Gene in Development and Evolution. Historical and Epistemological Perspectives* (Cambridge: Cambridge University Press, 2000), pp. 69-90.

[23] See for example Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994).

The second type of ambiguity arises from the fact that genetics is not solely concerned with the study of the transmission of characteristics from progenitors to offspring, Bateson's work notwithstanding. Genetics is also concerned with how these characteristics are developed, e.g., how an egg becomes an individual similar to its parents. This was why the German geneticist Richard Goldschmidt felt the necessity of naming his scientific domain "physiological genetics", as opposed to "transmission genetics".

In addition to these two terminological difficulties, the history of genetics, particularly "transmission genetics", gets even more complicated, being constantly crossed by genetics controversies about the best theory of evolution, often creating false fragmentations. The bones of contention are manifold: the general mechanism of evolution, dividing neo-Darwinism and neo-Lamarckism, the former attributing it to natural selection and the latter to the inheritance of acquired characters; the origin of heritable variation, splitting Weismannism and neo-Lamarckism, the first assigning it to random factors, or at least to something internal to the individual, while the latter ascribes it to the "use and desuse of organs", that is, to the environment; the gradualism of evolution separating mutationists (like de Vries) and gradualists, the former defending an evolution by "leaps" (Darwin's view, on the contrary, is that new characters appear by the accumulation of small variations; also for Lamarck evolution is gradual).

Yet another group of genetics controversies focus on the material basis of heredity, and are accordingly particularly connected with the appropriation of the Mendelian chromosome theory. Bateson's rejection of this theory is a well-known instance. This controversy was particularly intense during the 1940's. Shortly, the topic divided geneticists in two: some argued that the gene was something that could be

precisely located in the chromosome (the “particulate” view of the gene, more popular in United States); others, namely the German geneticist Richard Goldschmidt and father of physiological genetics, argued that the whole chromosome should be interpreted as the heredity unit (the “architectural” view of the gene, more popular in Europe).

PART I

A PRELIMINARY STUDY ON THE EMERGENCE OF GENETICS IN PORTUGAL

CHAPTER TWO

The first decades of the twentieth-century in Portugal: Mendelism in the agronomic and biomedical communities

The Portuguese scientific community began to move towards genetics during the late 1920s. In chapter three, I will show how this happened and how Portuguese genetics emerged in the 1930s, both at the research level and at the education level. In this chapter I want to describe the scientific environment established in Portugal in the course of the first decades of the twentieth century and, through the lenses of the international historiography of genetics, try to unveil the reasons behind the non-assimilation of Mendelism during this period. As I said before, by Mendelism I mean both early Mendelism (1900-1914) and its chromosomal version (since 1915). I analyze separately the biomedical community and the agronomic community for it is my belief that they correspond to different scientific cultures, connected to different national traditions.

The sources used in this chapter consist mainly of textbooks, science popularization texts and dissertation theses.[1] I should also underline that the material surveyed in this chapter concerning the first Portuguese accounts on Mendelism (Araújo in 1912, Cortesão in 1913 and Mayer in 1914 – see references in note 1) has already

[1] For instance, Arthur Araújo, *Mendelism in Man (a brief study on heredity)*, dissertation thesis presented to the Faculty of Medicine of the University of Porto in 1912; Ruy Mayer, *Biometric Methods and their applications on thrematology*, dissertation thesis presented to the Superior Institute of Agronomics of the University Technical of Lisbon in 1914; Gonçalves de Sousa "As Leis da Hibridação segundo Mendel e de Vries", *Revista Agronómica*, Ano II, 1 (1904), 149-153; Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Colecção Natura (Lisboa: Biblioteca Nacional, 1922).

been cited by the scholarship of Portuguese genetics[2], although these scholars, mostly former geneticists, did not probe the sources from a *geistgeschichtliche* standpoint on heredity, nor in the context of extant genetics' controversies.

2.1 National traditions and the appropriation of Mendelism

The appropriation of Mendelism has been a subject for the historiography of genetics. The first case studies were based in examples from the United States and Britain, key players in the institutionalization of genetics as a scientific discipline. In both countries, early Mendelism was swiftly appropriated and Mendelian-chromosome theory was soon established between 1920 (in the United States) and 1925 (in Britain).[3]

The case of United States is paradigmatic for the traditional history of science. No country had better institutional conditions for the emergence of the new biological discipline called "genetics", and hence for the appropriation of Mendelism. In 1900, American research institutions were in the middle of an expansion process that had started in 1870 and would last until 1914. Along with this, the university system was moving towards specialization. Both movements (of expansion and specialization) allowed American scientists to dictate the institutional autonomy for their discipline

[2] Four Portuguese geneticists wrote about the history of their discipline: Luís Archer, "Contribuições para a História da Genética em Portugal" *in História e Desenvolvimento da Ciência em Portugal no século XX* (Lisboa: Academia das Ciências de Lisboa, 1980), pp. 1027-1065; Aurélio Quintanilha, "História da Genética em Portugal", *Brotéria* XLIV, 3-4 (1975), 189-208; Miguel Mota Mota, "A Contribuição Portuguesa para o progresso da genética", International conference "Os Portugueses e o Mundo", Porto, 4-7 June 1985; and José Antunes Serra, "Contribuições Portuguesas para o Progresso da Genética. Tentativa de menção cronológica sistematizada", *Brotéria Genética*, 8 (1987), 17-34. The information concerning the first decade of the twentieth century in all four seams based on a historical survey conducted by Armando Cortesão, presented in his dissertation thesis in Coimbra in 1913 (reference see below).

[3] Stephen G. Brush, "How theories became knowledge: Morgan's chromosome theory of heredity in America and Britain", *Journal History of Biology*, 35 (2002), p. 471.

from the onset of the twentieth century.[4] The consequences were momentous: in 1916, for instance, genetics was taught in 51 American colleges and universities. [5]

The history of genetics in Britain is not so well documented from the institutional point of view and has been particularly focused on the figure of Bateson. Early Mendelism was appropriated by British biology via Bateson's research school at the University of Cambridge. Soon after 1900, Bateson initiated experimental research on hybridization and a course on Mendelism, and by 1906 Cambridge had one of the foremost genetic research schools in the world[6]. This school eventually ended, with Bateson's failure to institutionalize genetics at Cambridge (the first chair was taken up by his disciple R. C. Punnet in 1916). In fact, the real interest came from plant and animal breeding, and in 1910 Bateson moved to the John Innes Horticultural Institute[7].

In the last decades, several authors have offered important insights on the traditional historiography of genetics, drawing attention to different patterns of Mendelism's appropriation. Most of these remarks are concerned with the assimilation of early Mendelism, during the first and second decade of the twentieth century. It was chiefly in this period that the history of genetics was influenced by "national styles of science" or "genetic national traditions".

Jonathan Harwood has dwelt on the way Mendelism was appropriated by German scientists and on its singularities when compared with the American case.[8]

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[4] Jonathan Harwood, "National Styles in Science. Genetics in Germany and the United States between the World Wars", *Isis*, 78 (1987), 390-414.

[5] Jonathan Harwood, *Styles of Scientific Thoughts: The German Genetics Community 1900-1933* (Chicago: University of Chicago, 1993), pp. 138-181.

[6] Marsha Richmond, "Women in the History of Genetics. William Bateson and the Newham College Mendelians, 1900-1910", Isis, 92 (2001), p. 56.

[7] Peter J. Bowler and Iwan Rhys Morus, *Making Modern Science. A Historical Survey* (Chicago/London: The University of Chicago Press, 2005), p. 202.

[8] See Jonathan Harwood, *Styles of Scientific Thoughts: The German Genetics Community 1900-1933*, Ibid.; and "National Styles in Science", Ibid.

Also in Germany Mendelism was rapidly accepted, contrary to what traditional historiography of genetics has argued. However, due to philosophical and institutional constraints, Mendelism was appropriated in a less "reductionist" modality, which meant that Mendelian research on heredity was more attuned to the problems of evolution and development. This was to have important consequences in the history of genetics, creating an audience for several critical voices of mainstream genetics during the first half of the twentieth century.

Richard Burian, Jean Gayon and Doris Zallen investigated possible reasons for the strong French resistance to Mendelian genetics. [9] French research on genetics started in 1930, and genetics only began to be institutionalized as a discipline after 1945. Lucien Cuénot is the exception, having produced quite important work on physiological genetics before 1914. The authors attribute the anti-Mendelian positions of French biologists, not only to the activism of the neo-Lamarkians, but also to the pervasiveness of a more physiological approach to heredity, based on the metabolism of the entire cell. These approaches had their roots in two research lines of French biology: physiology and microbiology. As the authors put it, "the country of Lamarck was also the country of Claude Bernard and Louis Pasteur". [10]

A different set of historiographical interpretations tries to escape from the Mendel-centered view of genetics (Harwood may also be included therein). Traditional history of early genetics tends to treat genetics and Mendelism as synonyms. Based mainly on the German case, Jan Sapp accounted for several non-Mendelian genetic

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[9] Richard Burian, Jean Gayon and Doris Zallen, "The singular Fate of Genetics in the History of French Biology, 1900-1940" *Journal of the History of Biology*, 3 (1988), 357-402.

[10] Richard Burian, Jean Gayon and Doris Zallen, "The singular Fate of Genetics", Ibid, p. 358.

research (research on heredity which does not accept Mendel's propositions as valid), [11] specially the ones involved in cytoplasmic inheritance; the author frames the dispute between nucleo-centric and cytoplasmic views as a national affair between Americans and Germans. Also Christophe Bonneuil reflects on the importance of non-Mendelian tradition in French plant breeding.[12]

The role of agriculture in the history of genetics, particularly the role of plant breeding research in the appropriation of Mendelism, is another topic discussed by several authors. Jonathan Harwood or Barbara Kimmelman have both given several examples of how crucial was the role of experimental stations and agricultural colleges in the growth of American genetics.[13] This very same connection between genetics and agriculture has been investigated in the British case.[14] But among Europeans, the best case studies come from the so called scientific periphery: the Scandinavian countries. Olga Helina *et al*.[15] shows how national agricultural plans in these countries promoted the communication between local farmers within a national network of experimental stations, which resulted on a continuous feed-back on the practicability of scientific innovations. Mendelism would be adopted in the breeding methodology of the experimental stations soon after its "rediscovery". The most famous of the Scandinavian stations was the Svalöv Station, a plant-breeding institute created in 1886 at Svalöv,

[11] Jan Sapp, *Beyond the Gene, Cytoplasmatic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987).

[12] Christophe Bonneuil, "Mendelism, Plant Breeding and Experimental Cultures: Agriculture and the Development of Genetics in France", *Journal of the History of Biology*, 39 (2006), 281-308.

[13] See Jonathan Harwood's work (mentioned before) and also *Technology Dilemma. Agricultural Colleges between Science and Practice in Germany, 1860-1934* (Bern: European Academic Publishers, 2005); and Barbara Kimmelman, "Mr. Blakeslee builds his dream house: Agricultural institutions, genetics, and careers 1900-1915", *Journal of the History of Biology*, 39 (2006), 241-280.

[14] For example, Palladino, "Between Craft and Science: Plant Breeding, Mendelian genetics, and British Universities, 1900–1920", *Technology and Culture*, 34 (1993), 300-323.

[15] Olga Elina, Susane Heim and Nils Roll-Hansen, "Plant breeding on the front: Imperialism, War and Exploitation, *Osiris*, 20 (2005), 161-179.

South of Sweden. From 1890 until 1920, the Svalöv Station was the "Mecca" of plant breeding for Americans, Germans and Russians.

2.2 Portuguese accounts on Mendelism: 1912, 1913 and 1914

In 1912, a graduate student of medicine presented his graduation thesis to the Faculty of Medicine of the University of Porto under the title *Mendelism in Man (a brief study on heredity).*[16] According to Artur da Cunha Araújo, "on their vast majority" Portuguese physicians "ignore altogether the subject of this new science."[17] Presenting a thesis about this subject at the Faculty of Medicine of Oporto is for him more a matter of "daring" than of scientific culture.[18] "This School does not answer for the doctrines proposed in the dissertation and enunciated therein" is asserted twice in the first pages.[19] The reception of Araújo's thesis at this faculty does not seem to have been easy – although it is not yet clear if the choice of topic is accountable for such a reception.

Araújo's thesis is the first Portuguese critical account on Mendelism. By "critical" I mean only a minimum discussion of ideas and bibliographic review. Out of

[16] Artur da Cunha Araújo, *O Mendelismo no Homem (Breve estudo sobre a hereditariedade)*, dissertation thesis presented to the Faculty of Medicine of the University of Porto in 1912. This source was first mentioned by Miguel Mota, "A Contribuição Portuguesa para o progresso da genética", Ibid. p. 214.

[17] Artur da Cunha Araújo, *O Mendelismo no Homem*, Ibid. p.39.

[18] "[...] Os trabalhos laboratoriais onde o esforço pessoal tão palpável se torna não me atraíam. Abordar o estafado tema d'esta ou d'aquella doença e repetir a fastidienta lição dos livros, com dois casos clínicos, era o mais logico e mais consentaneo com a insuficiente preparação de quem desperta estremunhado para o amanhecer da vida pratica. Mas d'esta vez ainda, por temperamento, me senti pouco inclinado a usar esse meio cómodo de dar cumprimento à lei. Abalancei-me por caminho mais largo mas, de caminho se diga, sem a minima pretençao de lhe atinjir o fim. Reconheço a minha incompetência para satisfatoriamente desempenhar a tarefa que me impuz e por isso me resignei como o sapateiro de Apéles a 'não me elevar acima do calçado'. O assunto que escolhi é interessante e o aspeto sob que o encaro é completamente novo, pelo menos entre nós, onde nada conhecço escrito a tal respeito. Mas isso longe de me encorajar, deixa-me somente uma maior responsabilidade para uma competência minima. Que me perdoem a audácia. [...]" (preface of *O Mendelismo no Homem*, ibid.)

[19] Artur da Cunha Araújo, *O Mendelismo no Homem*, Ibid. pages without number (the first two).

this category I classified the article of the agronomist J. V. Gonçalves de Sousa[20], written in 1904 to the *Agronomic Journal* [Revista Agronómica].[21] As the author himself explains, the article was "translated and summarized" from another one written by an Italian agronomist, G. Cuboni, published in "Bolletino degli agricoltori italiani" and in "many Italian and French journals".[22]

One year after *Mendelism in Man*, in 1913, Armando Cortesão[23] presented at the Superior Institute of Agronomics of the University of Coimbra a thesis called *The Theory of Mutation and Plant Breeding (A Thrematologic tudy)*[24]. Cortesão does not seem to have been at the center of any discriminatory treatment, as it seems to have happened with Araújo. His erudite style protected him from possible reactions to his complaints about the state of the science of heredity, which was "quasi unknown" in Portugal.[25] Still, it is undeniable that this thesis appears as an isolate work at the agronomic institute of Coimbra for one his final remarks is a call for the creation of a *thermatology* (breeding) discipline. According to him, such an initiative, together with

[20] "As Leis da Hibridação segundo Mendel e de Vries", *Revista Agronómica*, Ano II, 1 (1904), 149-153. All the authors (Archer, Mota, Serra and Quintanilha) did note him, but as far as I could ascertain the first to do it was Armando Cortesão, *A Teoria da Mutação* (see reference note 24).

[21] This was one of the first Portuguese scientific journals founded in 1903 by the Agronomic Sciences Society of Portugal [Sociedade de Sciencias Agronómicas de Portugal]. It epitomizes all the pragmatic and utilitarian view of science held in Portugal at the turn of the century. On its first number, Veríssimo de Almeida introduces the spirit of the new scientific journal as a reaction to the "depressing influence of the late nineteenth century" and in defence of "practical modern life", "A 'Revista Agronomica' e a agricultura", *Revista Agronómica* 1 (1903), 1-4.

[22] "Ao transcrevermos e analysarmos este importante estudo de Sig. G. Ciboni, publicado no *Bolletino degli agricoltori italiani*, e em várias revistas italanas e francesas, julgamos ser uteis aos nossos leitores e mormente aquelles que se dediquem a este genero de trabalhos.", Gonçalves de Sousa, "As Leis da Hibridação", Ibid. p. 155.

[23] Armando Cortesão did not pursue his academic career as an agronomist, but as historian specialized on the Discoveries and cartography.

[24] Armando Cortesão, *A Teoria da Mutação e o Melhoramento de Plantas (Estudo trematológico)*, dissertation thesis presented to the University of Coimbra, in 1913.

[25] Armando Cortesão, *A Teoria da Mutação*, Ibid. p. XVIII.

the foundation of an agronomic station and a better organization of the agrarian services, "would in a few years solve the national economic crisis, which would raise Portugal in the eyes of the world".[26]

One year afterwards, in 1914, another young agronomist presented a thesis also concerned with heredity, called *Biometric Methods and their applications on thrematology,*[27] at the Superior Institute of Agronomics of the University Technical of Lisbon. Contrary to the former, this thesis does not seem an isolate act in this university context. Mayer acknowledged a special indebtedness to his professor Lima de Bastos, who had "initiated this trend in our university education".[28] "He [Lima de Bastos] could be considered to a certain extent *responsible* for the emergence of this work".[29] His goal is "to expound the theoretical foundation and the practical mechanism of the biometric methods of the Anglo-American school,"[30] for he foresees that biometry is to play an important role in biology.[31]

These three theses share among them a high presence of French literature, a pattern that fits in the general paradigm of the Portuguese culture during this period – the French influence has been well documented in the history of science as well as in

[26] Armando Cortesão, *A Teoria da Mutação*, Ibid. p. 205.

[27] Ruy Mayer, *Os Methodos Biometricos e as suas Aplicações em Thremmatologia. Estudo de Mathematica Applicada* , dissertation thesis presented to the Superior Institute of Agronomics of the University Technical of Lisbon in 1914.

[28] Ruy Mayer, *Os Methodos Biometricos*, Ibid. p. 6.

[29] "Elle se pode pois considerar de, até certo ponto, como *responsável* pela aparição d'este trabalho", Ruy Mayer, *Os Methodos Biometricos*, Ibid. p. 6.

[30] Ruy Mayer, *Os Methodos Biometricos*, Ibid. p. 5.

[31]: "[...] no estado da ciência biológica moderna, em que os methodos de observação e de experiência iniciam a sua obra, se antevê como indiscutivelmente oportuna a entrada em acção de um processo que os pode completar e que tem em vista levá-los ao máximo da exactidão. A situação da biometria em face das teorias da evolução fica pois, desde já, ao que se nos afigura, marcada com bastante clareza.", Ruy Mayer, *Os Methodos Biometricos*, Ibid. preface.

arts.[32] The most striking case is Araújo who has a considerable number of bibliographic references coming from communications in the IV international congress of genetics, which took place in Paris, in 1911 – indeed it is plausible to speculate that this dissertation owes its topic to the impact this congress had on the Portuguese student. It is also strange to see how he explain Mendel's laws based on the experiments with mice of Lucien Cuénot, who, as I explained before, was one of the few French biologists working with Mendel methodology at this period. Britain and the United States are mentioned to say that "they are the places in the civilized world where more attention has been given to these lines of work"[33], but there are no bibliographic references in English.

The two agronomists also reveal an acquaintance with a substantial quantity of French books and translations, but contrary to the prospective physician, they do not dismiss the crucial developments genetics had in United States and in the Scandinavian countries. Another difference is that both are much more critical when evolution controversies are concerned – a debate all three feel impelled to enter and to take a stands on. If Araújo accepted very easily the ideas of the French neo-Lamarckist Louis Blaringhem (again presented on the IV international congress of Paris; Blaringhem allied Lamarckism and mutationism), the two agronomists are much more cautious with the explanatory power of de Vries theory regarding evolutionary issues – something de Vries himself would appreciate.[34]

[32] With respect to the French influence on the history of life sciences see Isabel Amaral, *A emergência da bioquímica em Portugal: As escolas de investigação de Marck Athias e Kurt Jacobsohn* (Porto: Fundação Calouste Gulbenkian e Fundação para a Ciência e a Tecnologia, 2006).
[33] Ruy Mayer, *Os Methodos Biometricos*, Ibid. p. 5.

[34] About the historical misunderstandings on de Vries's work and evolutionary ideas see Bert Theunissen, "Knowledge is power: Hugo de Vries on science, heredity and social progress", *BJHS*, 27 (1994), 291-311.

Cortesão accepted it as a theory of evolution but not as a theory of variation. After accepting the occasional occurrence of the inheritance of acquired characters (in plants), he tries to steer clear of the controversy on Darwinism-Lamarckism. For him, it is important for mutationism – the theory which "fully explains in our view the complex phenomena of the origin and evolution of species"[35]– to keep away from the "transcendent melée of neo-Lamarckists and Weissmannists"[36]. As he said, "de Vries came up with the big idea of picking out what is good in Lamarckism and Darwinism, to combine it in a stroke of genius with the discovery of the importance of mutation, thus creating his admirable theory".[37] The critical point is that mutation (variation) is the cause of evolution. The question of whether this is due to internal conditions (de Vries's idea and neo-Darwinism) or to external ones (Lamarckism) remains a secondary problem for Cortesão. Also the idea of evolutionary time or the notion that new characters appear by the accumulation of small variations seem also unimportant. Both topics – variation and evolutionary time – do not seem to bother the young agronomist.

In a very clear way, Mayer explains how Mendelism discarded one of the major obstacles to natural selection, blending inheritance[38], but at the same time its experimental output "doesn't fit" the Darwinian view according to which characters appear by the accumulation of small variations.[39] To him, the theory of mutation of de

[35] Armando Cortesão, *A Teoria da Mutação*, Ibid. p. 96.

[36] Armando Cortesão, *A Teoria da Mutação*, Ibid. p. 77.

[37] Armando Cortesão, *A Teoria da Mutação*, Ibid. p. 96.

[38] "Com a aplicação dos conhecimentos sobre a physiologia da hereditariedade derivada de experiencias de Mendel, desaparece um dos argumentos mais importantes dos antiselecionistas – o que afirmava que um caracter que se manifestasse num individuo corria o risco de em breve se oblliterar pelo cruzamento entre esse individuo e outros que não o possuissem. [...] A tese darwinista, ou melhor, novo-darwinista, achava-se neste ponto fortemente consolidada.", Ruy Mayer, *Os Methodos Biometricos,* Ibid. p. 25.

[39] "Há porem uma ideia fundamental de Darwin que, pela constatação dos casos registados por Mendel e pelos seus successores, experimenta um abalo violento. A theoria darwinista de que os caracteres apparecem por acumulação de pequenas variações não se coaduna com os factos observados nos estudos de hybridação pelo metodo mendelista.", Ruy Mayer, *Os Methodos Biometricos,* Ibid. p. 25.

Vries was also the best available to explain the mechanism of evolution.[40] But again this theory could not solve the problem of the origin of variation. Pointing out plant-breeding experiments, Mayer criticizes Vries's theory arguing that "it never takes into account an environment-caused variation", describing his concept of "latency" as a "clever" explanation, "perhaps even *too clever*".[41] Another criticism concerns evolutionary time: despite the "resounding success" of Mendelism and mutationism, he states that it "is far from being proven that continuous variation, defended by Darwin and Lamarck as the primary influence of phylogenetic evolution, did not had that dominant effect".[42] Nevertheless, and despite criticisms and the theoretical consequences on the theory of evolution, Mayer opts for a pragmatic position: mutationism is too much "constructive" as a scientific theory to be discarded for not achieve to a general theory of biological evolution.[43]

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[40] "A questão da variação descontinua, a que nos conduziu um rapido esboço da influencia das doutrinas mendelistas sobre as theorias da evolução, constitui o apoio fundamental d'uma nova corrente, que, derrubando um postulado de Darwin, veio establecer, no campo da sciencia moderna, uma forte e decisiva orientação, e sobretudo impor a imprescindilidade do methodo experimental, cuja efficacia, de resto, já tinha sido comprovada pelos resultados de Mendel", Ruy Mayer, *Os Methodos Biometricos*, Ibid. p. 27.

[41] Ruy Mayer, *Os Methodos Biometricos*, p. 37.

[42] Ruy Mayer, *Os Methodos Biometricos*, Ibid. p. 38. And he adds: "A variação descontínua pode considerar-se um facto demonstrado, a sua importancia na theoria da evolução é indiscutivel, mas nem por isso somos obrigados a admitir que qualquer variação do que derive um typo novo tenha ou tivesse de ser necessariamente brusca. Mayer, Ibid. p. 38.

[43] "Se a nova doutrina [de Vries's theory of mutationism] não pode ser considerada como uma explicação geral da evolução phylogenetica [...] nem por isso podemos deixar de reconhecer que é d'um admiravel vigor constructivo, e que tem, na obra experimental em que se apoia, a mais firme das fundações. Induzindo o methodo da experiencia no estudo da origem das especies, veio por a disposição dos biologistas, quaesquer que sejam as suas tendencias theoricas, um utensilio efficacissimo, um dos que, com segurança, podem dar às ideias científicas sobre evolução a precisão e a solidez que ainda hoje faltam às generalizações biológicas", Ruy Mayer, *Os Methodos Biometricos*, Ibid. p. 38.

2.3 The Mendelian aspirations of the agronomic community

As far as I could ascertain, agronomic Mendelian research only began in the 1930s, in a regular way, with the foundation of the National Agronomic Station. Until then, and in spite of the enthusiasm and knowledge of agronomists like Mayer, the institutional conditions for agronomic research were not created.[44] Portuguese agronomy had always problems in assuming its authority and status as an area of scientific research. It remains so until this day – and so it was in the beginning. An exceptional period seems to have taken place during the Portuguese dictatorship, when agrarian policies ran parallel to the international autarky movement.

Indignation against Portuguese science policy regarding the agronomic sciences ranges paramount in Cortesão's thesis.[45] According to him, three institutions conducted in 1902 plant breeding experiments, all concerning wheat: Agronomic Institute (Lisbon), National School of Agriculture of Coimbra and National Station of Belém (Lisbon)[46]. In his opinion, the most interesting breeding experiments occurred in the Station of Belém. However, in his opinion, the results were "poor", with lack of scientific and biologic criteria, and also "a notable unawareness for Mendel's Laws"[47]. With the exception of "Belém wheat" ("Trigo de Belém"), all the other innovations

[44] Further investigation is needed in order to understand how the Mendelian chromosome theory was appropriated by the agronomic community.

[45] The second part of Armando Cortesão's thesis is particularly important to the history of Portuguese genetics because it helps us to understand the situation of Portuguese plant breeding during the period prior to 1913. Archer, Quintanilha, Serra and Mota – they all based on this source to describe the situation of plant breeding in the first decade of the Twentieth century.

[46] Armando Cortesão relates these institutions with a set of instructions published in the Govern Diary (Diário do Governo) of 22 November of 1901, "Instruções para os trabalhos de polinização e selecção de trigos, written by Conselheiro Alfredo Le Cocq (General Director of Agricultural) and published in 1902 in a small "folheto" of General Department of Agriculture, *A Teoria da Mutação*, Ibid. p. 193 (note 3).

[47] Armando Cortesão based this judgment on a report of this station presented in 1909 to the "Direcção Geral de Agricultura, *A Teoria da Mutação*, Ibid. p. 195.

came from outside Portugal, and, as he noted, they did not always work since they were adapted to a special environment different from ours.[48]

I can not answer definitely to the question why was not Mendelism appropriated by the agronomists in the two first decades. But I start to eliminate working hypotheses. Cortesão and Mayer (especially the later) are good evidences that one can not apply the peripheral myth of "lack of information" to this community, at least, regarding early Mendelism in the 1910s. Both Portuguese agronomists knew exactly where their "centers" were, where they should look and whose footsteps to follow: the United States and the Scandinavian countries. These countries were their models and rightly so. Both Cortesão and Mayer dedicate several pages of their theses describing how agronomic sciences are conceived in Sweden or in the United States, and how agronomy allied to genetic research can be an economic state tool. Cortesão, more eloquent, describes Portugal as a "commercial colony of England"[49] and dreams with a privately owned or a state agronomic station dedicated exclusively to the breeding of plants which constituted the staples of Portuguese agriculture: "cereals, leguminous, horticulture and fruit-culture"[50]. How would it be done? Easily:

> That Company – or the State- should pick a resolute man, versed in thrematology, surround him with expert auxiliary personnel , to send hum ,since it is found necessary, to the United States or to Sweden to

[48] Further investigation on the scientific production of the Portuguese experimental stations would be needed to understand the totality of external and internal causes regarding the appropriation of Mendelism in the agronomic context.

[49] Armando Cortesão, *A Teoria da Evolução*, Ibid. p. 200-201.

[50] Armando Cortesão, *A Teoria da Evolução*, Ibid. p.198.

personally check the work carried there on breeding, and place him afterwards as head of station, fully equipped.[51]

Also in terms of university education the Portuguese acknowledged the American, together with the German, as the best university example, valuing the importance experimental activities and applied science had in these universities.[52] In fact, in 1915, the Institute of Agronomics of Lisbon sent to the University of Illinois and the University of Wisconsin, two professors (one of them was Mayer) in a mission to study "the general organization of academic agronomy teaching", considered "well in spirit as in material conditions."[53] Mayer came back amazed with the American utilitarian and pragmatic style, but also with the ideology of "freedom" as a foundation of the American pedagogy. He describes how the American university gives autonomy to the student – "giving a free rein, allowing him to take the initiative without checking his individual effort" – providing at the same time all the necessary resources for him to accomplish his goal.[54]

The scenario of Portuguese agronomy during the first decades of the twentieth century seems very similar to the French one, in so far as it is described by Christophe Bonneuill. According to him, French agronomists did not share with the "university biologists" the same preconceived ideas about the new approach to heredity, and the

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[51] "Devia essa companhia, ou o Estado, escolher um homem decidido e bem conhecedor de trematologia, rodeá-lo de pessoal auxiliar bem habilitado, mandá-lo, como se torna necessário, aos Estados Unidos ou Suécia examinar pessoalmente os trabalhos de melhoramento que lá se executam, e colocá-lo depois à frente duma estação com todo o material necessário.", Armando Cortesão, *A Teoria da Evolução*, Ibid. p. 198.

[52] Ruy Mayer , "A Univerisdade Americana. O seu espírito; os seus métodos; influência na sua acção educativa", *Revista Agronómica* (1916), p. 15.

[53] Excerpt from a governement decree of 1915, following a proposal of the Academic Council of the ISA, Ruy Mayer, "A Universidade Americana", Ibid. p. 5.

[54] Mayer, "A Universidade Americana", Ibid. p. 29-30.

same happened in Portugal, as I will explain below, but on the other hand they could not put in practice their interest for Mendelism. Different experimental systems and cultures were at the heart of the problems between agronomists doing Mendelian genetics and plant breeders, who were able to turn theory into field practice. [55] Could it be that the same happened in Portugal? Here is a topic for future work on the history of heredity in Portugal.

2.4 The anti-Mendelian bias of the biomedical community

Let's now focus on the biomedical community. At this point of my research, I can not advocate that Araújo's thesis is representative of the Portuguese biomedical community. But, still, interesting interaction points emerge between his 1912 thesis and other sources (from the 1920s) I will proceed analyze. Again, I am aware I can not answer why this community did not appropriate Mendelism during the first decades of the twentieth century. But I will dare to raise some hypotheses.

The first step is to understand how the rise of experimental biology in Portugal is inextricably linked to the history of the Portuguese biomedical sciences. In Britain, Germany or France, experimental biology was also implemented in the context of the biomedical community.[56] However, in those countries this happened in the second half of the nineteenth century and in Portugal it only took place in the last decade of the 1890s and, in a more vigorous way, during the early twentieth century.[57] This means

[55] Chistophe Bonneuil, "Mendelism, Plant Breeding and Experimental Cultures: Agriculture and the Development of Genetics in France", *Journal of the History of Biology*, 39 (2006), 281-308.

[56] See Peter Bowler and Iwan Rhys Morus, *Making Modern Science. A Historical Survey* (Chicago/Londres: The University of Chicago Press, 2005).

[57] See Maria do Mar Gago, "História da biologia em Portugal (1700-1946): a tradição naturalista, as ciências biomédicas e a emergência da genética" in Gonçalves, M. E. and J. Freire, eds., *Biologia e Biólogos em Portugal. Ensino, Emprego e Sociedade* (Lisboa, Esfera do Caos, 2009), pp. 171-214.

that in Portugal, during the rise of Mendelian genetics, the only experimental biologists were also physicians.

Indeed, the research I have conducted so far suggests that, during the first decade of the twentieth century, there was not a Portuguese experimental zoology or botany[58]. Naturalists had not yet found their bearings in the new modes of experimental practice. The project to found the National Marine Station, for example, accounts for how zoologists yearned at the beginning of the century for acquiring the experimental skills of their physicians' colleagues[59] (other instances will be shown in the next chapter). Let me stress again that further investigation is required to fully confirm this general overview of this period. But, it appears undisputable that the experimental practices during the first two decades of the twentieth century were controlled by physicians. From now on I will treat these "physicians", not as such, but as "biologists" – a designation they surely would prefer.

The two Portuguese foundational traditions in experimental biology are bacteriology and histology (including embryology and physiology).[60] In 1880, Câmara Pestana founded the Bacteriological Institute (Lisbon), and during the first decade of the twentieth century Marck Athias founded (also in Lisbon) several institutes dedicated to the biological sciences. Both Pestana and Athias received their training in Paris: Pestana at the Institute Pasteur and Athias at the laboratory of the histologist Mathias Duval – Pestana following the footsteps of Louis Pasteur, Athias of Claude Bernard. In the first

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[58] One has to keep in mind that Portuguese botanic research had only started in the eighteen century, with the reform of the University of Coimbra (1772), and zoology as an organized scientific activity had only emerged in the middle of the nineteen century; about the history of zoology research see Carlos Almaça, *Bosquejo Histórico da Zoologia em Portugal* (Lisboa: Museu Nacional de História Nacional, 1993).

[59] Carlos Almaça, "A Sociedade Portuguesa de Ciências Naturais, a Marinha e a Estação de Biologia Marinha", VIII Simposium, "O Mar no Futuro de Portugal", Lisboa, 2003.

[60] See Maria do Mar Gago, "História da biologia em Portugal", Ibid.

decade of the twentieth century, bacteriological research would lose its importance but the impact of the research school of Marck Athias continuously grew with the implantation of experimental practices in histology, physiology and embryology.[61] He was a major player in several university departments and scientific institutes, for instance, the Institute of Histology and Embriology, connected to the Faculty of Medicine of the University of Lisbon.

The hegemony of the biomedical research in Portuguese biology during the first decades of the twentieth century becomes even more acute when we realize that these biologists occupied important positions in the several governments of the I Republic, actively participating in science policy reforms. For instance, right after the university 1911 reform[62], the norms regulating medical education and research in the new faculties of medicine were "updated"; as far as I could investigate, the same did not occur in the faculties of science. It was Augusto Celestino da Costa, a disciple of Marck Athias, who adhered more keenly to this educational reform, which extended throughout the 1910s and 1920s. This initiative began in March 1912, when Celestino da Costa, professor of the Faculty of Medicine of the University of Lisbon, presented to the *Conselho Escolar* a proposal of amendment to the 1911 reform; this was eventually introduced in 1918.[63]

One of the major contributions of this reform was a greater autonomy for the faculties of medicine, which afterwards were free to organize their pedagogic program; they could now organize laboratorial teaching as they saw fit, something impossible

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[61] Isabel Amaral, *A emergência da bioquímica em Portugal: As escolas de investigação de Marck Athias e Kurt Jacobsohn* (Porto: Fundação Calouste Gulbenkian e Fundação para a Ciência e a Tecnologia, 2006).

[62] Decreto de lei de 22 February 1911; regulamento de 23 de Agosto de 1911. As stated before, this educational reform ended with the monopoly of the University of Coimbra. Two more universities were created. Each of the new and reformed universities (Coimbra, Lisbon and Porto) had a faculty of sciences and a faculty of medicine.

[63] Augusto Celestino da Costa, "A Reforma do Ensino Médico", Separata de *Medicina Contemporânea*, 31-32 (1918), pp. 3-5.

within the "retrograde aspects" of the 1911 reform.[64] Obviously, the 1918 reform was far from perfect. One negative aspect concerned the stringent formal requirements for the recruitment of new people, which in the words of Celestino da Costa, "even in France have been relented, and only remain so in Portugal and Spain."[65] It is important to note that, like the agronomists, these biologists were very critical of the French university system. If "internally" speaking they were very close to their French peers, from the point of view of the university system, including the models of pedagogical organization and science policy, the German university and scientific institutions were their reference.[66]

Burian, Gayon and Zallen argue that the foundational traditions of French biology were microbiology, physiology and causal embryology, and that their central issues were not easily reconcilable with genetics. "The failure of Mendelism to resolve a variety of legitimate explanatory issues to the satisfaction of serious investigators trained in the dominant French disciplines also contributed to the failure of Mendelism to penetrate French science".[67] The other reason pointed by these authors was the anti-Mendelian polemics "put forward by many of the committed neo-Lamarckians".[68]

Here I highlight some of the Portuguese anti-Mendelian arguments (such as Mendelian characters being "ornamental"; or Mendelian genetics neglecting the

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[64] Augusto Celestino da Costa, "A Reforma do Ensino Médico", Ibid.

[65] But as he also says: "O resto do mundo não as conhece, mas a lei não se fez para o resto do mundo mas para Portugal e para os portugueses. Quer isto dizer que é preciso considerar esta lei não como uma obra definitiva (o que seria absurdo nesta matéria) mas sim como uma *étape*.", Augusto Celestino da Costa, Ibid. p. 8.

[66] See Isabel Amaral, Ibid. and Júlia Gaspar, *A Investigação no laboratório de física da Universidade de Lisboa (1929-1947)*, Tese de mestrado em História e Filosofia das Ciências apresentado na Faculdade de Ciências da Universidade de Lisboa em 2008.

[67] Burian, Gayon and Zallen, "The singular Fate of Genetics", Ibid. p. 401.

[68] Burian, Gayon and Zallen, "The singular Fate of Genetics", Ibid. p. 401.

cytoplasm), also pointed out by Gayon, Burian and Zallen. But I do not think that in the Portuguese case such arguments were manipulated by "committed neo-Lamarckians", like these authors claim to have happened in the French case. It seems that these anti-Mendelian arguments emerged from their empirical works which pointed that the cell should be considered as a whole. Indeed, contrary to the Portuguese agronomists, these experimental biologists from the Portuguese biomedical community did not seem worried with the evolutionary issues genetic knowledge could raise (further investigation is needed, though). Their main preoccupation was of another kind: to explain the consequences of genetics in our understanding of cell's functioning.

This reaction of the Portuguese tradition in histology-embryology against Mendelism can best be grasped in the appropriation of the new knowledge concerning the material basis of heredity. Seven years after the publication of Morgan's work, Roberto Chaves – Athias's disciple, professor at the Faculty of Medicine of Lisbon and one of the key elements of the Lisbon medical school – wrote a long article of popularization of science called "The cytological bases of heredity" in 1922 – the first Portuguese account on this topic I could find within the biomedical community.[69] Chaves has an agenda: to criticize the nucleus-centric view of heredity.[70] He describes how "exaggerated"[71] is the idea that only the nucleus is responsible for heredity and how it so happened that cytologists were forced by the theory to the "belief on the chromosomes individuality."[72] The long hours spent at the microscope convinced him

[69] Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Colecção Natura (Lisboa: Biblioteca Nacional, 1922).

[70] Concerning the theories of evolution, he accepts the ideas of Brachet, who distinguished between general heredity and special heredity; the first dependent from cytoplasm and the second from the nucleus.

[71] Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Ibid. p. 23.

[72] Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Ibid. p. 3.

that the cell behaved as a whole. “On heredity, like on assimilation, nucleus and cytoplasm conserve each other, because they are closely connected”.[73] Mendelian methodology implicated a nucleus-centric view of heredity and that could not be accepted.[74]

For Chaves, more important than to profit from the magnificent experimental power of research programs on nucleus heredity was to prevent misconceptions about the cellular mechanism.[75] But he expressed opinions which were not backed by an extended study on the topic. In this article, Morgan’s work is introduced as “curious discoveries made mainly by American cytologists”, responsible for “linking the determination of one particular character – sex – to the existence of specific chromosomes.”[76] He does not explain, however, the Mendelian chromosome theory nor does he make any remark concerning it.

In conclusion, Portugal presents the same pattern of non-appropriation of Mendelism than France with concern to their main research programs on histology, physiology and embryology. For these biologists genetic knowledge not only could not

[73] Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Ibid. p. 52.

[74] Indeed, after mentioning “one example” of Mendelian characters (in the last page of his book), he hasted to add: “Não concluamos, porém, daqui a existência firme da separação nítida das funções do núcleo e do citoplasma relativamente à hereditariedade. O funcionamento da célula resulta das interacções dos seus diferentes componentes. Vários factos citados comprovam a influência acentuada da constituição citoplasmática sobre o comportamento da cromatina, e, se nas condições habituais determinados elementos da célula, especialmente os cromossomas, se apresentam sempre sob o mesmo aspecto, com a mesma forma, o mesmo número, a mesma estrutura, isto não implica forçosamente que esta aparência de individualidade corresponda a uma autonomia real de tais funções.”, Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Ibid. p. 50.

[75] “A ideia preconcebida [i.e. nucleus heredity] tem na ciencia o grande papel estimulador de pesquisas, mas é pena que a sua influencia vá mais além e obrigue os homens de ciencia a moldarem os factos às teorias e não as teorias aos factos. Esta acção é de tal ordem, as hipóteses penetram tanto determinadas descobertas que muitas destas não podem ser rememoradas sem imporem imediatamente a lembrança da teoria com que andaram associadas. Porém, pouco a pouco, um novo encadeamento se vai formando, ainda que com dificuldade, e já hoje, para falarmos no assunto do nosso artigo, poucos são os biólogos que teimam em conferir ao núcleo o papel exclusivo da transmissão hereditária.”, Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Ibid. p. 51-52.

[76] Roberto Chaves, *As Bases Citológicas da Hereditariedade*, Ibid. p. 26.

help but did not fit their experimental systems. As I described above France was the "training centre" of the two leading figures of the Portuguese experimental biology at the turn of the twentieth century. But is this a sufficient argument? One institutional aspect is also revealing of how Portugal followed France from an epistemological point of view – the autonomy of "histology". As Celestino da Costa and Chaves proudly wrote in 1921:

> With few – if telling exceptions –, there are no private dozent of Histology in German universities. The teaching thereof is borne by Anatomy professors, and the same is the case in America and other nations. In England, physiology professors also teach Histology. Not so, however, in France, in Belgium, Swiss, the Netherlands and Austria: in all of those, Histology chair is independent in all or almost all universities.[77]

The author explains how histology in countries like Germany or England was to constitute most of the activity of anatomy teachers. Could it be that it had also allowed anatomists in these countries to look at biological knowledge with a different experimental background? Could it be that this different institutional organization had stimulated the use of experimental practices in departments of natural history and hence opened the minds of botanists and zoologists traditionally committed to taxonomic approaches to biology? Could it be that such an epistemological switch had played a role in the appropriation of Mendelism in Germany or England? In that case, the

[77] "Salvo raras, ainda que bem significativas excepções, não há nas faculdades alemãs professores privativos de Histologia. Cabe ao respectivo ensino aos professores de Anatomia e o mesmo se repete na América e noutras nações. Na Inglaterra os professores de Fisiologia são-no também de Histologia. Não assim porém na França, na Bélgica, na Suiça, na Holanda, na Áustria onde em todas, ou quasi todas, as Universidades, há cadeiras de Histologia independentes.", Costa, A. Celestino da, and P. R. Chaves, *Manual de Técnica Histológica. Guia de Trabalhos Práticos* (Lisboa, 1921), p. V (preface).

autonomy of histology in countries like Portugal or France could be considered one of the causes for the non-appropriation of Mendelism in these countries.

Further investigation is needed to corroborate this thesis. But, in my opinion, it is precisely when Burian, Gayon and Zallen do *not* make a distinction between the different biological communities that their thesis fails its explanatory potential. It is difficult to understand exactly where the French botanists and zoologists are being placed. Are they also the so called "French biologists"? Or are they a small part of the group not considered by these authors? When they state that the foundational traditions in French biology are microbiology, embryology and physiology are they also talking about the departments of natural history? Or do they restrict themselves to what I call here the biomedical community? Mendelism in the United States was appropriated in very special conditions, with the institutional autonomy of genetics. But in Germany, Mendelism was appropriated even within a plurality of research programs closely related to heredity, many in the domain of physiological genetics. Why did this not happen in France? In my view, Burian, Gayon and Zallen's thesis can not answer this question.

2.5 Conclusion

Further investigation is needed to buttress our understanding of the history of experimental zoology and botany in Portugal. But if we assume that during the first decades of the twentieth century the departments of natural history maintained mainly a descriptive and taxonomic tradition, this means that during the rise of genetics the only "people available" that could appropriate Mendelian genetics were i) agronomists (that

were interested but *probably* did not have the conditions) and ii) the biologists/physicians (that had the conditions but were not interested).

From a "nationalistic" point of view, the crucial question is to understand why the ones that had the conditions were not interested. As I described, in the context of the biomedical community, the non-appropriation of Mendelism in Portugal presents a similar pattern to the one described by Burian, Gayon and Zallen with regard to the French case. Although the anti-Mendelian polemics did not seem to be instigated by "committed neo-Lamarckians", like it seems to have happened in France, the research priorities and programs of Portuguese experimental biology are indeed very similar to French biology organized around histology, physiology and embryology. Now, several problems within these disciplines – where cell should be seen as a whole – were not easily reconcilable with genetics. It is possible that the autonomy of histology in those countries had retarded the emancipation of experimental modes of practice in the departments of natural history. This connection with France is marked by the scientific lives of Câmara Pestana and Marck Athias, the founders of two main domains of Portuguese experimental biology (bacteriology and histology), both trained in this country.

It is often held among biologists that the "belatedness" of Portuguese genetics is due the French influence for Portuguese scientists were "passive" actors looking to the "wrong" country for guidance. The arguments behind this idea are the same used as in many areas of Portuguese culture, taking the shape of an omnipresent France, our older sister, taking care of us since Napoleon. I do agree that Portuguese biology, as many other areas of Portuguese culture, was influenced by a French tradition in biology (by means of bibliographic references, training and university organization of knowledge)

and that was the principal motive of our non-appropriation of Mendelism. But I do not think “passive” the Portuguese biologists of the beginning of the twentieth century.

The point I want to make is that during the first decades of the twentieth century Portuguese experimental biology was reduced to biomedical research, which at the time was interested in histology, physiology and embryology, and regarding these scientific areas France was a “centre” on its own right. Portuguese experimental biologists, almost all graduated in faculties of medicine, were looking to their French colleagues because they were interested in the same subjects. Mendelism did not make part of their French peers’ interests; but nor did they make part of the Portuguese. They were looking to the “proper” place for guidance. I will show in the next chapter that it is only when experimental modes of practice are absorbed into the departments of the faculties of science that the conditions for the emergence of genetics were created. From that moment onwards Portuguese biologists would shift their attention to another country – Germany.

CHAPTER THREE

Interwar period: the emergence of Portuguese genetics in the university context

As I have shown in chapter two, Portuguese biomedical community developed the experimental skills and modes of practice on histology – later generally designated as cytology –, but did not appropriate Mendelism (neither in its early or chromosomal versions) since it did not accept chromosomes as the fundamental material basis of heredity. This happened more or less simultaneously within two distinct biological communities: 1) experimental botany, with the research school of Aurélio Quintanilha in the Botanic Institute of the University of Coimbra; and 2) agronomy, with the research school of António Sousa da Câmara in Lisbon, which began in the Superior Institute of Agronomics [Instituto Superior de Agronomia] and was consolidated in the National Agronomic Station. The first papers on genetics were authored by members of these schools and were published in the 1930s.

Recent historiography of science has tackled the emergence of the Lisbon school.[1] After World War I and the great depression of 1929, countries all over Europe sought in science a way for self sufficiency based on national resources, and Portugal was no exception. It was within this context of "autarky" that was created in 1936 the National Agronomic Station, directed by António da Câmara, a scientist from the

[1] Tiago Saraiva, "Laboratories and Landscapes: The Colonization of Portugal and Mozambique in the Twentieth Century", HoST, 3 (forthcoming, 2009) and "Fascist Labscapes: Geneticists and the New Landscapes of Fascism in Italy and Portugal", Historical Studies in the Natural Sciences, 40 (forthcoming 2010); Júlia Gaspar, Maria do Mar Gago, Ana Simões, "Fields and Labs, groups and research schools: Portuguese geneticists and physicists under the fascist regime (1929-1954)," ICHST 2009, Session S-37 Science, Politics and Development in the 20th century, Budapest, 28 July-2 August 2009.

Superior Institute of Agronomics and the head of the "wheat campaign".[2] Câmara completed his training on genetics first at the Plant Breeding Institute in Cambridge and then in the Kaiser Wilhelm Institute for Breeding Research in Berlin.

In this chapter I focus on the emergence of the genetics school lead by Quintanilha in the University of Coimbra. Contrary to Câmara's school, which resulted from a top-down political decision, Quintanilha's school is a very good example of how scientific knowledge can be constructed in a peripheral context and how epistemological or institutional constraints are to be broken. Before, though, I will address the work of Mathilde Bensaúde, who made important discoveries on the sexuality of fungus – a research line later attacked, genetically, by Quintanilha in Coimbra.

3.1 Sexuality of fungus: Mathilde Bensaúde's legacy

Mathilde Bensaúde (1890-1969) was a member of a highly cultured Jewish family from the high bourgeoisie of Lisbon.[3] Her father, Alfredo Bensaúde, was the son of José Bensaúde, from the Archipelago of Azores and one of the most important industrialists of the island of São Miguel. Alfredo Bensaúde was sent by his father to study in Germany where he concluded high school, graduating from the Mines School of Clausthal and being awarded his PhD at the University of Göttingen. On his return to Portugal, in 1884 he became professor of mineralogy and geology at the *Instituto*

[2] An epistemologically-oriented approach to assess the accomplishments of this school is needed, especially regarding its relation with pro-Mendelian agronomists of the second decade of the twentieth century, such as Ruy Ferro Mayer.

[3] This part of the thesis dedicated to the life of this Portuguese scientist is based on a biographical text written by Aurélio Quintanilha: "Mathilde Bensaúde 23.1.1890 – 22.11.1969", *Boletim da Sociedade Portuguesa de Ciências Naturais*, 13 (5-19), (1970-71), 2ª série, 5-19. This is an unorthodox source since Quintanilha cites Mathilde's words without reference to primary sources.

Industrial e Comercial de Lisboa, but in 1911, after the proclamation of the Republic (1910), he was appointed head of a new institute of engineering, the *Instituto Superior Técnico* of Lisbon. Mathilde Bensaúde's mother, Jane Gabrielle Eleonore Oulman, was French and belonged also to a distinguished and wealthy Jewish family from Paris.

After attending summer-school in Germany for a couple of years, Mathilde Bensaúde went to study in Switzerland. She finished high school in Lausanne in 1909 and in the same year she enrolled at the University of Lausanne to study physics, chemistry and biology. Eventually, though, she interrupted these studies and returned to Lisbon, where she began to work at the *Instituto Câmara Pestana*, the Portuguese institute for bacteriological research. It was in this institute, a branch of the institutional platform of the Lisbon biomedical community described in chapter two, that she decided to become a protozoologist.[4] But this was not to be done in Portugal. In 1913, at the age of 23, she entered the Sorbonne, in Paris, to pursue her dream.[5]

Bensaúde took courses in zoology, histology and embryology-evolution at the department directed by Maurice Caulery, where she met "colleagues that were already 'high-standard scholars'", such as the Canadian entomologist William Thomson and "big Keilin".[6] Following her father's advice – who recommended to complement her Sorbonne studies with "some kind of laboratory work", in order to "avoid the bookish attitude of French education"[7] – Bensaúde went on to work at the Collége de France in

[4] About this decision Bensaúde underlines the influence of "a German" hired to teach his methods to Portuguese technicians. "I learned a lot with that man, and I decided to become a protozoologist", says Mathilde Bensaúde *in* Quintanilha, "Mathilde Bensaúde", Ibid. p. 8.

[5] Choosing Sorbonne was coherent with her aspirations of being a protozoologist; as referred in chapter two, microbiology was at that time a mainstream research line of French biology.

[6] Mathilde Bensaúde in Quintanilha, "Mathilde Bensaúde", Ibid. p. 8.

[7] Mathilde Bensaúde in Quintanilha, "Mathilde Bensaúde", Ibid. p. 8.

the laboratory of histology directed by Jean Nageotte[8]; she also conducted field work on the marine biology station of Wimereux. For two years, Paris was a productive and exciting workplace for Bensaúde, yet this did not last. When the Great War began, Mathilde returned temporarily to Lisbon, her father having forbidden her to stay in Paris. Eventually she convinced him to let her go and at end of 1915 she was back in the French capital. However, everything had changed.

> How all was so changed! The boys of my time were no longer there, and the University was now filled with girls, too shy, often from a modest background. Professors soon assumed the guise of primary school teachers, and I thought myself back in that land full of discipline but bereft of wit Lausanne. Caulery's laboratory, once remarkable, now hosted almost exclusively professor Caulery, a bitter small man. Keilin, Thompson, Perez and several others had already left.[9]

Upset by this new atmosphere, she decided to take some botany courses with Louis Matruchot. With this French mycologist Bensaúde rediscovered the excitement of doing science.[10] Right after her graduation in 1916, she began her PhD research on the

[8] Jean Nageotte (1866-1948) was a French histologist who did considerable research on the histology of the nervous system. At the College de France, he was a tutor in the laboratory of histology between 1903 and 1912, and in 1912 he succeeded Louis Ranvier in the chair of comparative histology. See Jaques Philippon and Jacques Poirier, *Joseph Babinsky. A Biography*, (Oxford: Oxford University Press, 2008), p. 143-144.

[9] "Mas que diferença eu lá achei! Os rapazes já lá não estavam, a Universidade encontrava-se cheia de rapariguinhas de condições modestas, assustadas, demasiado dóceis. Os professores não tardaram a tomar tons de escola primária… e tive a impressão que estava de novo nessa terra de disciplina mas sem espírito: Lausanne. O laboratório de Caulery, tão notável outrora, quase somente continha o professor Caulery, homem pequeno e azedo. Keilin, Thompson, Perez e tantos outros tinham partido.", Mathilde Bensaúde in Quintanilha, "Mathilde Bensaúde", Ibid. p. 8.

[10] "A man full of vitality, zest and kindness as there are few. He was an excellent teacher and, attending his course, I rediscovered that liveliness, that curiosity which has enchanted me in some Zoology and

cytology and reproduction of the Basidiomycetes, studying the mating reactions of single-spore cultures of *Coprinus fimetarius.* One year later, in August 1917, Bensaúde demonstrated the mechanism of sexuality for this class of fungus in a note to the *Comptes Rendus the l'Academie des Sciences de Paris*[11], and in detail in her PhD thesis *Recherches sur le cycle évolutif et la sexualité chez les Basidiomycètes,*[12] published in 1918. The mechanism of sexuality on fungus was demonstrated by Albert Blakeslee in 1904 for the class of Zygomycetes. But until Bensaúde, the reproductive mechanism of Basidiomycetes had remained unknown.[13] This scientific discovery was confirmed two years later, in 1920, by the German botanist Hans Kniep, the authority in the cytology and sexuality of the Basiodiomycetes and professor at the University Würzburg. Because of the Great War, both scientists undertook their research simultaneously and independently: nor Bensaúde had access to the works of the German professor nor Kniep was informed about the results of the young PhD Portuguese student.[14] Nevertheless, Kniep never denied Bensaúde credit for the discovery and always cited her whenever appropriate.[15]

Embriology courses and in Thompson and Kellin's research work" says Mathilde Bensaúde *in* Quintanilha, "Mathilde Bensaúde", Ibid. p. 9.

[11] Mathilde Bensaúde, "Sur la sexualité chez les Champignons Basidiomycètes", 165 (1917), 286-288.

[12] Mathilde Bensaúde, *Recherches sur le cycle évolutif et la sexualité chez les Basidiomycètes* (Nemours: Impremerie Nemourienne, 1918).

[13] See Ainsworth, G. C., *Introduction to the History of Mycology* (Cambridge: Cambridge University Press, 1976) (digitally print version 2009) p. 127; Shu-Ting Chang and Philip G. Miles, *Musshrooms: Cultivation, Nutritional Value, Medicinal Effect and Environmental Impact*, Second. Ed. (CRC Press, 2004), p. 105; In the 4th volume of his book *Recherches on Fungi* Buller included the following dedication: "To Mathilde Bensaude who by means of experimental and cytological investigations on a *Coprinus* first showed that heterothallism occurs in Hymemomycetes". Quintanilha, evidently, always mentions Bensaúde's discovery in "Progressos da Genética dos Fungos e a contribuição dos Portugueses", *Revista de Ciências Biológicas*, Vol. 2, Serie B (1973) and "História da Genética em Portugal", *Brotéria* XLIV, 3-4 (1975), 189-208.

[14] Hans Kniep, "Über morphologische und physiologische Geschlechst Differenzierung (Untersuchungen na Basiodiomyzeten)", *Verh. Phys. Med. Ges. Wurzburg*, 46, 1-18, 1920 *in* Chang, S-T, and Miles, P. G., Mushrooms: Cultivation, Nutritional Value, Medicinal Effect and Environmental Impact, Second. Ed. (CRC Press, 2004), p. 127.

Bensaúde's research work belonged to a biological tradition that aimed to identify the mechanism of sexuality in the life cycle of several biological groups. Sexual reproduction in man implies secondary sexual characters and the production of gametes morphologically different. In the majority of plants it is also possible to differentiate the female and male organs, either in the same individual or in separate individuals. However, in many other groups, for instance in algae and fungus, there are no "external" evidence for sexual reproduction. As Quintanilha quite plainly explained in his *Scientific Foundations of Sexuality*,[16] the dilemma of sexuality became a "cytological" problem during the second half of the 19th century – and shortly afterwards a "cariological" one.[17] Only by analysing the number of chromosomes was it possible to identify the moment of fecundation (or cariogamy) and the moment of chromossomical reduction. If biologists could identify these two phenomena in the life cycle of a certain organism, they could certainly say that sexual reproduction occurred in that particular biological group.

Fungus was a challenging group of organisms. Many were isogamic species (with morphologically identical gametes) hence only crosses of all possible combinations of mycelia could reveal the mysteries of their life cycle. If some crosses produced zigotos and others not, this meant that this particular species had two types of mycelium. Given that one could not talk about "sex" on isogamic species, the signs (+) and (-) were adopted to designate the different types of mycelia. It was along the research line of crossing mycelia experiments, developed by Blakeslee among others,

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[15] Aurélio Quintanilha, Ibid. p. 14.

[16] Aurélio Quintanilha, *Os Fundamentos Científicos da Sexualidade*, Biblioteca Cosmos collection, first chapter. *Biblioteca Cosmos* was a pocket book collection edited by Bento de Jesus Caraça, and part of his agenda of bringing education to all.

[17] Aurélio Quintanilha, *Os Fundamentos Científicos da Sexualidade*, Ibid. p. 10-11.

that Bensaúde prepared her PhD research and eventually discovered heterothallism on Basidiomycetes.

But what was the genetic explanation for heterothallism? How many Mendelian factors were involved in the formation of these two types of mycelia observable on Basidiomycetes? Bensaúde would not pursue these questions. After her PhD she turned her attention to phytopathology and left behind the sexuality of fungus[18]. As Quintanilha reported it was Kniep who first conducted a Mendelian approach to the heterothallism on Basidiomycetes[19]. But had her work any influence in Portuguese biology? In a historical account on Portuguese genetics, the geneticist Luis Archer argued that the first Portuguese creative research work on genetics was due by Bensaúde, but without consequences on Portuguese science since it had not been conducted in Portugal.[20] Was it so? Indeed Quintanilha's PhD subject (on a group of fungus), the progressive importance of the genetics of sexuality and the biological

[18] In 1919 Bensaúde moved to the United States; she had a brother who lived in New York. Tired of laboratory life, she decided to stop her work on the genetics of fungus and became interested on phytopathology..Between 1921 and 1923 she attended several courses from the department of phytopathology, directed by L. R. Jones, of the University of Wisconsin. She worked with G. W. Keit and published with him an article on the different strains of *Cladosporium* that parasite different species of fruit trees. But in 1923, Bensaúde was back in Portugal, in São Miguel, Azores, the Atlantic island where her father was born. She worked as a phytopathologist in an enterprise of pineapples' exportation (*Sociedade Correctora*) and set up a small laboratory of phytopathology where she studied the diseases and plagues on agricultural products for exportation, such as pineapples, melons, peanuts and bananas. In 1928, she became part of the research team of the Rocha Cabral Institute – the first private Portuguese scientific institute dedicated to biological sciences, created in 1925 in Lisbon – and afterwards she initiated her collaboration with the National Agronomic Station. In 1931 (she had 41 years old) a government job was offered to Bensaúde at the Ministry of Agriculture, and shortly after she was directing the Division of the Phytopathologic Service. See Quintanilha, "Mathilde Bensaúde", Ibid.

[19] "Foi Kniep o grande continuador da obra de Bensaúde, com a descoberta de espécies homotálicas e heterotálicas, bipolares e tretapolares, e a interpretação genética destes fenómenos", Aurélio Quintanilha, "Historia da Genética em Portugal", Ibid. p. 193.

[20] "A primeira investigação original realizada por um português na área da genética foi a de Mathilde Bensaúde que […] descobriu o heterotalismo em basidiomicetes. […] Mas este trabalho não teve, na altura, qualquer repercussão na ciência portuguesa. Após o seu doutoramento, Bensaúde passou a dedicar-se a estudos duma área diferente", Luís Archer, "Contribuição para a História da Genética em Portugal", Ibid. p. 1028.

facilities of a material such as fungus explain *per se* his interest in the genetics of sexuality on Basidiomicetes, his lifetime research on genetics. But was that all?

Portuguese scientists had obviously no reputation on genetics research; but in Germany, the European "centre" of genetics, a reputed botanist (Kniep) knew the work of a young and bright Portuguese PhD student (Bensaúde) who had made some discoveries before him. Bensaúde had not done the work in Portugal, but she was Portuguese and Quintanilha was aware of her work – why not start from there? Had this detail of local proximity a role in the construction of genetic knowledge within a scientific periphery such as Portugal? Quintanilha himself never traced any causal relation between Bensaúde's research and his genetic research interests.

3.2 The Go-Between: Aurélio Quintanilha

The emergence of Portuguese research on genetics in the university context began with the group of Aurélio Quintanilha in the 1930s, at the Botanic Institute of the University of Coimbra. The first part of his scientific life could not offer a more striking instance of the end of the hegemonic position of the biomedical communities in what concerned experimental skills and modes of practice in cytology, and also of how this experimental culture slowly permeated departments of faculties of science, new spaces in which conceptual freedom enabled their members to explore the experimental power of Mendelian methodology.

Quintanilha had little funds and so he began his university's studies working at the same time. He concluded the first three years of Faculty of Medicine of Coimbra but he failed the exams since he did not have time to prepare himself. He moved to Lisbon

in 1913 (1912?) and attended the Faculty of Medicine of Lisbon for two years.[21] It was in this context that he worked with the biomedical community I described in chapter two – the Lisbon circle of Athias's disciples.

> My first contact with this group was in 1912, when I enrolled in the Faculty of Medicine, after three years in the Faculty of Sciences in Coimbra. In the meantime the chair of Histology and Physiology was split in two. Athias got hold of Physiology, and relinquished Histology to his disciple Celestino da Costa. I was mainly interested in Celestino da Costa's lectures, and also in the practical classes, of which Roberto Chaves was in charge. I had two outstanding masters.[22]

However, after three years he again failed the exams. He continued to attend the laboratory of histology but he was no longer a university student. One day, Teles Palhinha, who was professor of the Faculty of Sciences of Lisbon, advised him to enter the graduation of Historical-Natural Sciences at the Faculty of Science of Lisbon – the graduation was "faster" and there were more "vacancies for assistants".[23] He followed this advice and in 1917 he passed the exam of botany with the highest mark; he was immediately promoted to second teaching assistant by the professors of the course, Teles Palhinha and Pereira Coutinho. The scientific skills he gained while practicing

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[21] Fernandes, A., "Prof. Dr. Aurélio Quintanilha", Separata do *Boletim da Sociedade Broteriana*, 36 (1962), 2ª série, p. 9.

[22] "O meu primeiro contacto com esse grupo foi em 1912, quando me matriculei na Faculdade de Medicina, depois de três anos na Faculdade de Ciências de Coimbra. Entretanto tinha sido desdobrada a cadeira de Histologia e Fisiologia. Athias ficou com a Fisiologia e cedeu a Histologia ao seu discípulo Celestino da Costa. Interessei-me sobretudo pelas lições de Celestino da Costa e pelas aulas práticas, a cargo de Roberto Chaves, dois mestres admiráveis.", Aurélio Quintanilha, "História da Genética em Portugal", *Brotéria* XLIV, 3-4 (1975), p. 190.

[23] Aurélio Quintanilha, "História da Genética em Portugal", Ibid. p. 190.

within the biomedical community were right away appreciated. “Since I had attended Celestino da Costa’s lab, I had already a good training, both theoretical and practical, on animal citology and histology.”[24] He was put in charge of the practical courses of morphology and physiology of plants, and proved a success among students. In fact, not only among students: “I remember showing for the first time chromosomes and images of mitoses and meiosis to the professors Pereira Coutinho, Palhinha and Carriço, who were thrilled with my preparations.”[25] At the same time, he continued to do research at the laboratory of Celestino da Costa.

After his graduation in 1919, Quintanilha was invited to take up a position in the Botanic Institute of the University of Coimbra. He accepted. The institute was under the direction of Luís Carriço, who had been nominated in 1918 the successor of Júlio Henriques.[26] But the heyday of this charismatic director of the Botanic Institute of Coimbra had long passed and the institute was now but a shadow of older times. Above all, it was nothing compared with the laboratory of the Faculty of Medicine of Lisbon where Quintanilha had given his first steps into laboratory life.

[24] Aurélio Quintanilha, Ibid. p. 191.

[25] Aurélio Quintanilha, Ibid. p. 191.

[26] Júlio Henriques is a very important figure in the history of Portuguese biology and of the University of Coimbra. He is responsible for the reconstruction of Brotero’s herbarium, which was the first Portuguese who described the flora of Portugal (late eighteenth century – early nineteenth). In 1873, Júlio Henriques became the director of the Botanic Garden of the University of Coimbra and in 1880 he founded the *Sociedade Broteriana* (an homage to Brotero), which aimed to rally Portuguese society around the project of making a flora of Portugal. This society was to publish a famous Portuguese scientific journal called *Boletim da Sociedade Broteriana*. See Abílio Fernandes, “História da botânica em Portugal até finais do século XIX” *in História e Desenvolvimento da Ciência em Portugal*, (Lisboa: Academia das Ciências de Lisboa, 1986). Júlio Henriques is also noted for being the first Portuguese to incorporate Darwin’s theory of evolution in the context of his PhD thesis at the University of Coimbra. See Carlos Almaça, *O darwinismo e a Universidade Portuguesa (1865-1890)* (Lisboa: Museu Nacional de História Natural, 1999) p. 21; and Ana Leonor Pereira, *Darwin em Portugal (1865 – 1914). Filosofia, História, Engenharia Social* (Coimbra: Almedina, 2001), p. 68.

> There were no others professors or assistants and in the lab there were no helpers. Not a single good microscope was available, nor a microtome, nor a stove for *inclusões* or cultivation of micro-organisms. It was amidst this dire scarcity that I started to teach botany in Coimbra. I had to secure all practical classes of all courses on botany, and in next year they also put me in charge of the chair of Medical Botany.[27]

If in Lisbon the professors of botany had never seen a chromosome, in Coimbra there was not even a microscope in good conditions. This was the panorama of the Portuguese faculties of science at the beginning of the interwar period. The economic difficulties that had thwarted Quintanilha as a student, had given him the exceptional opportunity of learning with the best professors of the biomedical community, but did not enable him to become one of them. Opting for the "easiest" way and for a career opportunity, Quintanilha placed himself in the context of the botanic departments of the faculties of science, bringing with him the know-how and curiosity for laboratory life. As I will show, in this way the conditions were created for the emergence of research on genetics at the Faculty of Science of the University of Coimbra.

3.3. The genetics group of Aurélio Quintanilha (1926-1935)

During his first years in Coimbra Quintanilha worked towards his PhD, while preparing his courses and equipping the laboratory. In 1925 he completed his PhD on

[27] "Não havia mais professores nem assistentes, e no laboratório não havia nem preparadores nem serventes. Não havia um único bom microscópio, nem objectivas de imersão, nem um micrótomo, nem estufas para inclusões ou para cultura de microorganismos. Foi nesta pobreza confrangedora que comecei a ensinar Botânica em Coimbra. Tinha todas as turmas de trabalhos práticos, de todas as cadeiras de Botânica a meu cargo e, no ano seguinte, deram-me a regência da cadeira de Botânica Médica"Aurélio Quintanilha, Ibid. p. 191.

the life cycle of the fungus *Synchitrium*, and applied for a position as professor with a research work on the cyto-physiology of the digestive glands of the carnivore plants. In 1926, he had already become interested on the genetics of the sexuality of the Basidiomycetes.[28] In 1927 (1926?) he went to Berlin "to personally meet Kniep and to let him know of my enthusiasm for his brilliant work and tell him of my ambition of working with fungus' genetics"[29] The German botanist invited Quintanilha to work with him and in 1928 the Portuguese biologist moved to Berlin to the *Pflanzen-physiologishes Institut* in Dahlem, with a scholarship granted by his own university.

Every two weeks, there was a colloquium at the *Kaiser Wilhelm Institut* for Biology, which Kniep used to attend together with his research students. At that time, the KWI was under the supervision of Carl Correns, with Richard Goldschmidt directing the department of zoology and Max Hartmann the department of botany. "It was then that I had the opportunity of meeting all those 'grosse Kanonen', as we used to call them – Correns, Goldschmidt, Hartmann."[30] Correns was "small, modest, almost shy, with a short white-yellowish beard, he looked as if he were always apologising for being the great Correns himself!", and Goldschmidt was "tall and strong". "His solemn manners made me, at first, a little afraid".[31] He felt greatly indebted to this "remarkable quartet" whose books and papers he had read. He considered himself "deeply influenced and stimulated by discussing fundamental problems with them". Moreover, his research topic crossed all departments. "The genetics of sexuality was, almost everywhere, on the agenda, with Correns dealing with the flowering plants, Goldschmidt with butterflies, Hartmann with algae and Kniep, at the *Pflanzen-physiologishes Institut*,

[28] Aurélio Quintanilha, "História da Genética em Portugal", Ibid. p. 193.

[29] Aurélio Quintanilha, "História da Genética em Portugal", Ibid. p. 193.

[30] Aurélio Quintanilha, "Richard B. Goldschmidt", *Portugaliae Acta Biológica,* Serie A (1951), p. 11.

[31] Aurélio Quintanilha, "Richard B. Goldschmidt", Ibid. p. 11.

busy with algae and fungi."[32] Quintanilha stayed one year at the *Pflanzen-physiologishes Institut*. But in 1929 Kniep died, and Quintanilha, together with another of his colleagues, was invited to work under the supervision of Hartmann at the KWI. After two years of research at this institute, he returned to Portugal in 1931.

Despite his genetic work conducted in Germany, it is interesting to note that he never used the word "genetics" or "Mendelian" in his reports to the Board for National Education. His collaboration with Kniep is described simply as having produced "very important scientific research."[33] The anti-Mendelian positions shared by the Portuguese biomedical community, to which belonged the heads of the Board for National Education, probably inspired Quintanilha to be careful in reporting his activities.[34]

Quintanilha made important contributions concerning the genetics of the sexuality of Basidiomycetes. In 1933 he published "Le problème de la sexualité chez les basidiomycètes"[35] – in his own words, "the first creative work on genetics published in Portugal".[36] In order to do it he received a six-months grant from the National Board of Education [Junta Nacional de Educação] supplemented by another for the acquisition of material from the university (Fund Sá Pinto). Later, during his stay in Paris, he made discoveries concerning the "phenomenon of Buller", twelve years later spotted by Haig Papazian in the United States.

More than a scientific leader, Quintanilha fits better the category of "mentor," awakening his students to the new science of heredity. Lack of time seems to have been

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[32] Aurélio Quintanilha, "Richard B. Goldschmidt", Ibid. p. 11.

[33] *Relatório dos trabalhos efectuados em 1930/31,* Lisboa: Junta de Educação Nacional, 1933, p. 126.

[34] See Júlia Gaspar, Maria do Mar Gago and Ana Simões, "Fields and Labs, groups and research schools", Ibid.

[35] Aurélio Quintanilha, "Le problème de la sexualité chez les basidiomycètes. Recherches sur le genre Coprinus", *Boletim da Sociedade Broteriana,* 8 (1933), 2ª serie, 3-99.

[36] Aurélio Quintanilha, "História da Genética em Portugal", Ibid. p. 194.

the main constraint which accounts for his inability to consolidate a research school on genetics with a coherent agenda. [37] Quintanilha's group of disciples was formed between 1926 and 1935, each approaching cytogenetics in a different way. The first disciple was Abílio Fernandes, who was Quintanilha's student during the academic year 1926-1927[38]. Fernandes was invited to become teaching assistant in 1927, before Quintanilha moved to Berlin. Fernandes began studying the number and form of chromosomes in different species, and later specialized on cyto-systematics. He became the head of an important research programme in this area in the University of Coimbra, where he stayed all scientific life.[39] According to Quintanilha, Fernandes's earliest works – including his thesis published in 1930, called *Study on the chromosomes of Liláceas and Amarilidáceas* – are the first published in Portugal in which chromosomes are considered "carriers of the heritable characters", representing thus "the beginning of a new branch of science among us, cytogenetics".[40]

In my view, Quintanilha is clearly establishing a frontier between the biomedical community and the one he helped to build. This epistemological shift concerning the role of the chromosome was only possible with the migration of this concept to a community in which heredity was not a theoretical affair, but did actually play a vital

[37] In 1935, Quintanilha was expelled from the university during the first university purge of Portuguese fascist regime. From 1936 until 1939 he worked at the Cryptogamic Laboratory of the National Museum of Natural History of Paris, a period that would remember as "the most prolific years of my scientific career". With the onset of World War II, Quintanilha returned to Portugal where he worked clandestinely in the National Agronomic Station, in Lisbon, with the internal support of his colleague António Câmara, who tried to persuade elements from the fascist regime to readmit Quintanilha. However, the only position Quintanilha was able to secure was not in Lisbon but in Mozambique. In 1943, Quintanilha moved to Lourenço Marques to be the director of the Centro de Investigação Científica Algodoeira (Cotton Research Center).

[38] Abílio Fernandes, Prof. Dr. Aurélio Quintanilha, *Separata do Boletim da Sociedade Broteriana*, 36 (1962), 2ª série, p. 14.

[39] According to José de Barros Neves, Abílo Fernandes was the founder of the "school of cyto-taxonomy of Coimbra", *in* "Prof. Doutor Abílio Fernandes, Boletim da Sociedade Broteriana, Vol. LIII, 2ª serie (1980) p. 19.

[40] Aurélio Quintanilha, "História da Genética em Portugal", Ibid. p. 192.

part in the experimental systems designed and manipulated by researchers. This was the main achievement of Quintanilha's genetic legacy, providing the conceptual unity among his disciples' research work.

Fernandes was the sole Quintanilha's recruits before his stay in Berlin. The other disciples came later. A good part of his students during the academic year 1933-1934 in the Morphology and Physiology in Plants chair were to prove his most important followers.

José Antunes Serra was "the brightest among Quintanilha's disciples"[41]. I am not going into details here, since I will dedicate to him the second part of this thesis. I shall only say that Serra started his university studies at the Faculty of Medicine, and it was here that he first met Quintanilha. When Quintanilha returned from Germany, in 1931, he was responsible for two courses in the University of Coimbra: Plants Morphology and Physiology, at the Faculty of Sciences, and Medical Botany at the Faculty of Medicine. It was while attending Quintanilha's lectures in Medical Botany that he decided to change to the Faculty of Sciences. Even as a professor Quintanilha continued to act as a bridge between the biomedical community settled scientifically and institutionally and the unknown land of the biological sciences. Nevertheless, he advised Serra on the difficult challenge involved in his decision. "Man lives not by bread alone", quipped Serra to Quintanilha. The next year, in 1932, Serra enrolled in Biological Sciences. Both departments – botany and zoology/anthropology – invited him to work as a teaching assistant. Serra was closer to the department of Quintanilha, to his research and laboratory, but eventually settled for a career opportunity at the zoological department, where there was an appointment for a professorship. Thus, Serra brought with him to the zoological-anthropological community the genetic knowledge

[41] Abílio Fernandes, "Lembrando o Prof.", Ibid. p. 194.

and the experimental modes of practice in cytology already practiced in the botanic community – much as Quintanilha had done before, moving from the biomedical to the botanic community. Within genetics, Serra developed important work on the field of physiological genetics, starting with the problem of biochemistry of melanin. He did not study abroad. After his PhD in Coimbra he considered doing so, but it was too late - the fascist regime imposed strict restrictions on travelling.

Flávio Resende was another important disciple of Quintanilha. His early scientific career is completely different from Fernandes and Serra. Resende was a high school teacher who came to work at Quintanilha's laboratory. After his degree in Oporto, he took a job (in 1931) as professor at a high school in Guarda, in the middle of Portugal. But in 1932, he was applying to the National Board of Education asking for a scholarship to work in a laboratory of biological research.

> […] After my degree, I was a high-school teacher in Guarda, the only place where vacancies opened. The experience lasted one year. I felt our training was inadequate. I decided to expound my situation to the National Education Board. It so happened that the Board was headed by Professor Celestino da Costa, who tried to solve my case, allowing me to go to Coimbra to work with Professor Quintanilha.[42]

Thus it happened that Resende received a scholarship and came to work with Quintanilha and his students at the laboratory of the Botanic Institute of Coimbra.

[42] […] quando terminei a licenciatura, fiquei professor de Liceu na Guarda, único sítio onde havia uma vaga. Foi uma experiência de um ano. Senti que a preparação que o curso nos dava era insuficiente. Lembrei-me de recorrer à Junta Nacional de Educação expondo o meu problema. Tive a sorte de ser presidente dessa Junta, o Professor Celestino da Costa que tentou resolver o meu caso possibilitando-me ir para Coimbra trabalhar com o Professor Quintanilha.", Flávio Resende, Diário de Lisboa, 2.11.1965 in C. N. Tavares, "Vida e actividade científica", Ibid. p. 3.

Further changes were yet about to come. In 1933, Quintanilha received in his laboratory the offer of a scholarship from the Humboldt Foundation [*Alexander von Humboldt Stiftung*?] to study in Germany. He decided to give this scholarship to Resende who moved to Germany in that same year. Resende finished his PhD in 1937, in the University of Hamburg, under the supervision of Emil Heitz. He started with pure cytogenetics (number and form of chromosomes), like Fernandes, but later he became interested in the expression of sex in plants and how the growth hormones and the genetic factors influenced this expression.[43] Since 1943 Resende's academic career as a professor and researcher took place at the Botanic Institute of the Faculty of Sciences of the University of Lisbon.

3.4 Genetics in the University curricula

The word "genetics" appears only as part of the Portuguese university curricula in 1964 in the context of the course "General Genetics".[44] However, the history of genetics teaching is much older. In 1929, genetics began to be taught in Coimbra, in the context of the recently created course on "Biology". This course was created in 1926, during the first year of the dictatorship in the context of an educational reform of the faculties of science conducted by the minister Ricardo Jorge[45], which also included the

[43] See C. N. Tavares, "Vida e actividade científica do Prof. Dr. Flávio Ferreira Pinto Resende", Separata de *Portugaliae Acta Biológica* (B), 9 (1-2) (1967).

[44] DL 45.840, 31 July 1964. See letter from Abílio Fernandes to Luís Archer, 4 November 1989, *in* Archer, "Contribuições para a História da Genética em Portugal", Ibid. p. 1055.

[45] DL nº 12.687, 17 November 1926. See letter from Abílio Fernandes to Luís Archer, 4 November 1989, Ibid. p. 1054.

splitting of the Group of Biological Sciences (created by the reform of 1911) into two different groups: botany and zoology-anthropology.[46]

In 1929, the Council of the Faculty of Sciences of the University of Coimbra decided that much more important than teaching the generics of biology was to communicate the developments of the science of heredity, considering "the importance genetics was acquiring"[47]. The institutional framework of the course of "Biology" required it to be taught by the two groups in charge of the domain of biological sciences in the Portuguese faculties of science: the botany group and the zoological-anthropological group. In the following year (1930), the University of Lisbon followed suit and Oporto soon afterwards. This is how genetics entered in the Portuguese university curriculum: in the context of a discipline lectured alternately by professors of botany and of zoology/anthropology. This pattern is of the utmost importance, as we shall see.

When this reform was first implemented in the University of Coimbra (1929), Quintanilha was the most qualified professor on genetics. However, he was at that time living in Germany and eventually the Biology course was entrusted to Euzebio Tamagnini, professor of Zoology and Anthropology. Tamagnini chose Fernandes, Quintanilha's first disciple, to be responsible for the practical course. This course included: 1) culture of *Drosophila melanogaster* in banana milieu fermented by beer yeast; 2) morphological study of the wild type and its laboratory manipulation, creation of some pure lines of mutants, specially the ones considered more important: black body ebonite, miniature wings, vestigial wings, white eyes, sepia eyes; 3) experiments of mono-hybridism, di-hybridism, tri-hybridism, heredity linked to sex, repetition of

[46] See Carlos Almaça, *Museu Bocage. Ensino e Exibição. Museu Bocage* (Lisboa: Museu Nacional de História Natural, 2000), p. 36-37.

[47] Letter from Abílio Fernandes to Luís Archer, 4 November 1989, Ibid. p. 1054.

Mendel experiments with peas; 4) statistical study of variation, using principally plants; nuclear observations (mitosis and meiosis in *Narcissus* and *Crepis*) and *Drosophila melanogaster*.[48]

Everything was ready except for one thing: the *Drosoplila* cultures. Where to find them? In 27 November 1929, the German biologist Erwin Bauer came to the Botanic Institute of Coimbra to deliver a conference promoted by the German Institute [Instituto Alemão] of Coimbra. During his stay, Tamagnini and Abílio Fernandes asked him if he could arrange the required *Drosoplila* cultures, which he promptly did on returning to Germany.[49] The course began in the academic year 1929/1930 with Tamagnini and Abílio Fernandes as professors. In 1930/31 the same duo was responsible for the new discipline. Finally, in the year 1931/1932, the course would be coordinated by Quintanilha and his disciple Abílio Fernandes. From 1931 to 1935, the head of the chair would change alternatively, between the Department of Botany (Quintanilha) and the Department of Zoology and Anthropology (Tamagnini).

So far I have been unable to confirm whether Quintanilha interfered in the decision of the Council of the Faculty of Sciences of Coimbra, which decided to use the recently created course of "Biology" to teach genetics. However, one can argue that Quintanilha's genetics group contributed actively to the commitment to teach genetics in Coimbra by being in charge of the totality of the practical courses during the first years; and that it was due to the group's educational initiative that the first cultures of *Drosophila* crossed the threshold of Coimbra university. Finally, is important to note that the institutional constraints imposed on the "Biology" course forced professors of

[48] Letter from Abílio Fernandes to Luís Archer, 4 November 1989, *in* Archer, "Contribuições para a História da Genética em Portugal", Ibid. p. 1054.

[49] Letter from Abílio Fernandes to Luís Archer, 4 November 1989, *in* Archer, "Contribuições para a História da Genética em Portugal", Ibid. p. 1054-1055.

both Departments – Botany and Zoology and Anthropology – to appropriate a minimum of genetic concepts in their teaching.

3.6 Conclusion

I analyzed in chapters two and three the construction of genetics knowledge through an epistemological perspective attending to the dynamics between different biological communities. From this perspective, some conclusions can be drawn about the emergence of genetics within the university context.

The first conclusion is that the migration of experimental modes of practice, particularly in histology, from the biomedical community (associated with the Faculty of Medicine of Lisbon) to the botanic community (Faculty of Sciences of Coimbra) rendered possible the emergence of genetics research in the faculties of sciences. With Quintanilha at the Faculty of Sciences of Coimbra, conditions for an epistemological shift involving the chromosome concept were created and Mendelism appropriated. In the botanic departments, and later in the zoological and anthropological departments, nuclear heredity and Mendel's laws were no longer a theoretical affair as they were for the biomedical community, but became part of the experimental systems designed and manipulated by researchers.

The second conclusion is that, as a consequence, Germany became the scientific "centre" for Portuguese geneticists. Fostered by a long cultural tradition, prevailing since the eighteenth century, which turned the French language and culture an imperative for educated families, France was an appropriate centre for the biomedical community which focused on biological problems of physiology and embryology. The biomedical community had long admired Germany for the pedagogical organization of

their universities, science policies and aspects related to the institutionalization of science – but not for their experimental research on heredity. This situation changed with this generation. Both Quintanilha (between 1928 and 1931) and Resende (between 1933 and 1937) received their genetics training in German institutes (Fernandes and Serra did not study abroad during their early training); and it is through Erwin Bauer that the University of Coimbra first acquired *Drosophila* cultures.

Thirdly, a final conclusion is that career opportunities seams to have been an important motor for the dissemination of the genetic knowledge in a periphery such as Portugal. The lack of material and formal conditions (structure of university careers) to keep a promising young student in a certain institution (university, for example) obliged them to search for a career opportunity in other communities (Quintanilha) or departments (Serra), reproducing their knowledge and enthusiasm in new environments.

PART II

J.A. SERRA AT THE CROSSROADS OF POLITICS AND BIOLOGICAL COMMUNITIES

CHAPTER FOUR

Genetics during the War: Serra from anthropology to cytogenetics

José Antunes Serra (1914-1990) was born in 5 January 1914 in Vela, near Guarda, in the centre of Portugal.[1] He moved to Coimbra in 1931 to study in the Faculty of Medicine, but after the first year he attended the course of Medical Botany lectured by Aurélio Quintanilha, professor of the department of Botany of the Faculty of Sciences, and decided to change to this faculty and to graduate in Biological Sciences in 1935-1936. As I explained in the previous chapter, he was invited by Quintanilha to continue his studies in the Botanic Institute but he opted for an opportunity career at the department of Zoology and Anthropology of the same faculty.[2]

Serra is consensually regarded as one of the leading Portuguese geneticists of the twentieth century. Bereft of the assistance of his former mentor Quintanilha, Serra very rapidly gained international recognition. *How* did this happen? By putting in context his scientific research, I plan to describe how he quickly appropriated the genetics international trends and how he did legitimate his interests within the Portuguese scientific and political community.

[1] Rolanda M. Albuquerque, "Professor José Antunes Serra", *Brotéria Genética*, 12 (1991), 9.

[2] Additional information on his academic life in Coimbra: assistant in 28 November 1936, second assistant in 29 May 1937, first assistant in 2 January 1942, full professor in 12 July 1950; SF-UC (front page of the file). Serra was the director of the Zoological Laboratory and Museum of Coimbra between 1950 and 1953 and of the Anthropological Laboratory and Museum between 1950 and 1952, Rolanda M. Albuquerque, "Professor José Antunes Serra", Ibid. p. 10.

4.1 Coimbra's school of anthropology: the rise of a biological tradition

The first thing to understand about the department of Zoology and Anthropology is that anthropology occupied a hegemonic position in regard to zoology. This must be related to the fact that its director, Tamagnini, was also the head of Coimbra's anthropological school. Indeed, it was at the heart of this anthropological school that Serra started his PhD thesis, under the supervision of Tamagnini. But what were the main research lines and the scientific practices of this school? And how did Serra later legitimate his proposed research program on genetics inside this department? These are the two questions I wish to answer below.

The conditions for the first Portuguese school of anthropology were created in 1885 in the University of Coimbra – at that time the only one in Portugal.[3] Two men lead this school until the end of the first half of the twentieth century: from 1885 until 1907, Bernardino Machado was responsible for its institutionalization, with the creation of the first course[4], museum and laboratory of anthropology in the Faculty of Natural Philosophy of the University of Coimbra; from 1907 until 1950, Eusébio Tamagnini was responsible for the consolidation of the same institutional project. During more than four decades, Tamagnini directed this school taking Portuguese population as its

[3] The first allusions to physical anthropology came from Lisbon, in the end of the 19th century, and in the context of the Lisbon biomedical community, revolving around the figure of the anthropologist and physician Francisco Ferraz de Macedo. It was nevertheless in Coimbra, within the university context, that its institutionalization took place. For more about the history of Coimbra's school of anthropology see Gonçalo Duro dos Santos, *A Escola de Antropologia de Coimbra, 1885-1950: o que significa seguir uma regra científica?* (Lisboa: ICS Imprensa de Ciências Sociais, 2005); Eusébio Tamagnini and José A. Serra, "Subsídios para a História da Antropologia Portuguesa", Congress of the Portuguese Scientific Activity, Coimbra, 1940 (printed in 1942).

[4] About the course of Anthropology, Human Palaeontology and Pre-Historic Archaeology, created in 1885, see Eusébio Tamagnini and J.A. Serra, "Subsídios para a História da Antropologia Portuguesa", Congress of the Portuguese Scientific Activity, Coimbra, 1940 (printed in 1942).

mainstream subject. How he approached this subject, however, would vary considerably over time.

Gonçalo D. Santos classifies the scientific production of this school into two distinctive traditions: the medical and the biological.[5] The first concerns biometry, an area of anthropological research producing work since the beginning of the school. Anatomy and anthropology combined to describe physical characters (particularly in the context of osteometry, the comparative study of individuals based on their bones) in different actual populations, and in relation to the archaic ones.[6] Tamagnini would dedicate himself to these morphological studies on Portuguese population along his entire career; one important goal was to establish the evolutionary hegemonic position of the Portuguese population in relation to the colonies and to the world.[7]

In 1929, Tamagnini became responsible, together with Quintanilha, for the Biology course, whose contents were genetics (see former chapter). In a few years Mendelism would be appropriated and in the beginning of the 1930s Tamagnini's school of anthropology began to move towards human heredity – this new phase of Coimbra's school of anthropology is what Gonçalo D. Santos describes as inscribed in a "biological" tradition.[8] In 1934, Tamagnini was convinced that the mechanism of human heredity functioned "according to the Mendelian scheme".[9] Of course, human heredity had been for decades an anthropological subject in countries which had early

[5] The terms used by Gonçalo D. Santos are *evolutionary anthropology of naturalistic orientation and medical inspiration* and *evolutionary anthropology of naturalistic orientation and biological inspiration*, *A Escola de Antropologia*, Ibid. p. 126.

[6] Gonçalo D. Santos, *A Escola de Antropologia*, Ibid. p. 126.

[7] Gonçalo D. Santos, *A Escola de Antropologia*, Ibid. p. 126.

[8] Gonçalo D. Santos, *A Escola de Antropologia*, Ibid. p. 126.

[9] Eusébio Tamagnini, "Os Problemas da Mestiçagem", I Congresso Nacional de Antropologia Colonial, Exposição Colonial Portuguesa, Porto, 1934, p. 10.

appropriated Mendelism. In Germany, for instance it was around 1914 that the trend in anthropology began to shift "from measuring skulls to an application of Mendelian genetics", regarding the study of human physical, psychological and cultural differences.[10]

Human heredity was considered a promising scientific domain since it was anthropology's breakthrough to the biological sciences.[11] However, serious obstacles were to be considered. Unlike plants or some animals, humans could not be submitted to experimentation; research projects were therefore dependent on genealogic and statistical methods, rendering the process of scientific research a sluggish and difficult one. Projects such as the genealogic study on the families of the region of Coimbra (ongoing in 1940) were predicted to yield reliable results only in long time scale.[12] Indeed, it would not take long until other organisms were adopted in order to conduct experimentation. It was therefore inside the anthropological school of Coimbra that zoological research conquered its scientific legitimacy. As I will show, this rearrangement of the relative importance of zoology and anthropology within this department of the Faculty of Sciences of Coimbra was conducted by Serra while he pursued his works on heredity and physiology of pigmentation. Before, though, let us know the scientific environment Serra found when he accepted Tamagnini's invitation to carry on his studies in the department of zoology and anthropology in 1936.

[10] Sheila F. Weiss, "Human Genetics and Politics", Ibid. p. 49.

[11] See Eusébio Tamagnini and José A. Serra, "Subsídios para a História da Antropologia Portuguesa", Ibid.

[12] Eusébio Tamagnini and José A. Serra, "Subsídios para a História da Antropologia Portuguesa", p. 13.

4.2 Genetics and politics in the 1930s: Human heredity, racial differences and eugenics

The second phase of Coimbra's anthropological school emerged in the 1930s in the international context of eugenics. Anthropology and politics engage at this time in a very lively interaction.[13] Although in Portugal the emergence of eugenics' thought coincides with the rise of the fascist regime, it is nowadays consensual that some eugenic positions during the 1920s and 1930s were generally accepted in Europe and the United States, and had made important legislative leeway in several countries, including sterilization and euthanasia.[14] Hence, eugenic programs in Portugal were not only created by a fascist regime but, and above all, by an international trend in the social and political spheres. Nevertheless, it is possible to describe *how* the Portuguese fascist regime responded to this international context and, by so doing, colonialism emerges as the central issue of eugenic political initiatives in Portugal.

Actively collaborating in the construction of a "new state", Tamagnini became "Salazar's scientific ideologue for the Portuguese colonial empire".[15] Involved in the foundation (in 1930) of the political party behind the New State, National Union [União Nacional], – Tamagnini would be shortly afterwards (1934) invited to be the Minister of Public Instruction [Instrução Pública]. He held this position until 1936, accumulating this function with his university position and responsibilities as head of Coimbra's

[13] Gonçalo D. Santos argues that the "high promiscuity" between the scientific and the political spheres has been pointed out before as a general feature of the emergence of anthropology in Portugal (Ibid, p. 166). According to him, both Machado and Tamagnini – the former an engaged Republican and the later an ideologue of the fascist regime – used the "pedagogic role of scientific activities" as a way to struggle in the political context for the autonomy of anthropology (Ibid, p. 169). Even with I Republic Tamagnini cultivated this institutional dialogue. However, it seams reasonable to claim that from the interwar period ahead, the interaction between Tamagnini and the New State became especially fertile.

[14] Gonçalo D. Santos, *A Escola de Antropologia de Coimbra*, p. 131.

[15] Gonçalo D. Santos, *A Escola de Antropologia de Coimbra*, Ibid. p. 167.

anthropology school. During these two years, in the context of an "imperial isolationism" political agenda, Tamagnini successfully promoted an eugenic program of racial and sexual hygiene. This program purported to protect, especially through the use of pedagogy, Portuguese population from the "threats of degeneration", such as, "degraded, criminals, alcoholics, jews, crazy people, creoles and niggers".[16]

The "scientific" background of this program would be presented in the I National Congress of Colonial Anthropology (in 1934), organized together with Mendes Correia, head of Oporto's anthropology school. Knowing that his audience was not familiarized with the science of heredity[17], Tamagnini wrote a communication with a considerable long didactic introduction to Mendelian genetics. He then used genetics (or "genetic biology" as he says) to demonstrate the negative output of inter-racial crossings, that is, how it was liable to produce creoles. In the end of the communication, his political agenda got clearer. Colonies are the answer for the Portuguese demographic problem, by serving as an alternative territory for the "fixation of population's excess in the continent".[18] However, such a colonial emigration policy should at the same time assure the perpetuation of the Portuguese "superior race".

> Against the idea that we, Portuguese, solely go to Africa in order to rule the black population, one should set an idea that serves more adequately the national interest and the economy of emigrants, viz., the permanent constitution of population nuclei of Portuguese families, where the professional activities play

[16] Gonçalo D. Santos, *A Escola de Antropologia de Coimbra*, Ibid. p. 210, note 55.

[17] To know exactly who was present in this congress see "Circular, comissão de honra, programa e lista das comunicações já anunciadas" of the I National Congress of Colonial Anthropology.

[18] Eusébio Tamagnini, "Os Problemas da Mestiçagem", p. 25-26.

out and are distributed as if they where real metropolitan Portuguese population.[19]

For this to happen the careful selection of settlers was crucial – "intelligent individuals, with strong character and heredity capacities recognised" – and a "radical" change concerning their "sexual approximations", since the Portuguese's "low repugnancy" " concerning individuals of other ethnic groups was well known.[20] Such a eugenic political program could only be done with the contribution of anthropological studies on human heredity (genetics) and racial differences. It is therefore easy to understand why the statutes of the Portuguese Society of Eugenic Studies [Sociedade Portuguesa de Estudos Eugénicos] were drafted during this congress.[21] Presided by Tamagnini, this society was eventually created in 1936, and had, among other responsibilities, to monitor the above mentioned eugenic program of racial and sexual hygiene in the Portuguese colonies.

This very same approach to anthropology – "a combination of genetics, eugenics and the study of racial differences"[22] – had been initiated in 1927 at the Kaiser Wilhelm Institute for anthropology, in what seems to have been a personal project of its director Eugen Fisher. Could it be that Tamagnini was influenced by the events at the KWIA? Could their political activities – both were sympathizers of national socialist

[19] "À ideia de que nós, portugueses, vamos para África apenas para comandar pretos, deve sobrepor-se a ideia de que o que mais convêm ao interesse nacional e à economia individual dos emigrantes, é a constituição definitiva de núcleos populacionais […] de famílias portuguesas, onde as actividades profissionais se exerçam e repartam como se se tratasse de autênticas populações portuguesas metropolitanas", Eusébio Tamagnini, "Os Problemas da Mestiçagem", p. 25.

[20] Eusébio Tamagnini, "Os Problemas da Mestiçagem", p. 26.

[21] About the influence of others European eugenic societies (Germany, England, Sweden and Norway), see Gonçalo D. Santos, *A Escola de Antropologia de Coimbra*, Ibid.

[22] Sheila F. Weiss, "Human Genetics and Politics", p. 4950.

ideology[23] – had influenced the direction of Coimbra's anthropological school? In 1952, Gerhard Koch, a pupil of Otmar Freiherr von Verschuer whom worked with Fisher at the KWIA – wrote:

> […] Portuguese-German relations, so warm and close in the field of anthropological sciences are intimately connected to the names of renowned Portuguese and German researchers such as Eusébio Tamagnini, António Augusto Mendes Correia, Alfredo A. Athayde, Eugen Fischer, Fritz Lenz and Otmar Freiherr von Verschuer.[24]

According to him, it was to "consolidate these relations" that in 1937 Fischer was nominated "doutor honoris causa" of Coimbra's university.[25] Further investigation is yet needed to understand to what extent these relations were privileged or not, either in the political or in the scientific level.

I described the scientific-political environment Serra found when he entered Coimbra's school of anthropology as a graduate student in 1936. On one hand, human heredity had just started, outgrowing exclusive dedication to the skeletons hidden in the anthropological museum. On the other hand, the interaction between the fascist regime and Tamagnini was at its most intense. At that time, within the context of eugenics and

[23] "Toda a gente em Coimbra dizia que ele [Tamagnini] era Nazi. Aliás, até se dizia que Hitler o tinha escolhido para ser o Gauleiter da região de Portugal depois da invasão", Interview with Rolanda Albuquerque, Outubro 2009, Lisboa.

[24] "As relações luso-alemães, bem intimas e amistosas no domínio das ciências antropológicas encontram-se estritamente ligadas aos nomes dos conhecidos dos investigadores portugueses e alemães Eusébio Tamagnini, António Augusto Mendes Correia, Alfredo A. Athayde, Eugen Fisher, Fritz Lenz e Otmar Freiherr von Verschuer […]", Gerhard Koch, "Otmar Freiherr von Verschuer. Vida e Obra", *O Médico*, 38 (1952), p. 3. Gerhard Koch was at the time assistant at the Universidade de Tübingen. This article was written on the occasion of the 55º anniversary of de Verschuer, his professor.

[25] Gerhard Koch, "Otmar Freiherr von Verschuer. Vida e Obra", Ibid. p. 3.

colonialism, Serra's PhD supervisor and the head of the department had put human heredity and racial differences studies at the centre of attentions.

4.3 Heredity and phenogenetics of pigmentation: from man to rabbits

The key word to understand Serra's early scientific activity is pigmentation, the topic he chose for his PhD thesis, published in 1939.[26] This thesis was composed of two parts: an heredity study based on boys and girls from several schools, between 7 and 22 years old, which produced a co-relation between age and pigmentation of skin, hair and eye colour (phenomena of darkness with age), with the purpose of facilitating the "correction of the genealogic data when one studies heredity of pigmentation on man"[27]; and a study on the chemical composition of melanin, which drew on pigments not only from humans but also from rabbits, and aimed at a better understanding of the "phenogenesis" of pigmentation.

The phenomenon of pigmentation was a classical subject in studies of hereditary inheritance. Since the beginnings of genetics, using model organisms from mice to the fruit-fly, geneticists studied the inheritance patterns of pigmentation traits. On the contrary, the mechanism of development of these pigmentation traits (the process that lead genes to produce pigments that would turn into a certain phenotype), long disregarded, was only becoming a mainstream research line in genetics in the late 1930s. This change was due to the works of Alfred Kuhn and his co-workers, who had

[26] José A. Serra, *Estudos de pigmentação melânica. A determinação da pigmentação e escurecimento com a idade. Composição das melaninas*, PhD thesis presented to the Faculty of Sciences of the University of Coimbra in 1939, pp. 173; he had already published in 1938 *A Pelve nos Portugueses. Morfologia da Pelve no Homem* (Coimbra, Tipografia da Atlântida: 1938),

[27] José A. Serra, *Estudos de pigmentação melânica*, Ibid. p. 161.

worked on the flour moth *Ephestia kühniella* between 1925 and 1945,[28] and to the new developmental genetic research line in Morgan's school, which started in 1935 when George Beadle and Boris Ephrussi invented a method of transplanting imaginal disks in *Drosophila*.[29]

As Serra clearly ascertained in his PhD introduction, he aimed a "better understanding and elucidation of heredity and phenogenesis."[30] By choosing these two targets, Serra was acknowledging his interest for the two branches of genetics: 1) genetics of the transmission of heritable characters – a tradition that had just started in the anthropological school of Coimbra(in the framework of human genetics, and particularly in the study of the inheritance of "racial" traits), with the appropriation of basic concepts of Mendelism after the institutionalization of the first genetics core course about it; and 2) developmental and physiological genetics (or "phenogenetics"), in which, as far as I could ascertain, no one in Portugal had conducted work before.[31] His broad view of genetics, tackling both problems of transmission genetics and physiological genetics, along with his engagement in debates in theoretical biology (see below), places Serra in what Harwood once defined as the German "style" of genetics –

[28] Hans-Jörg Rheinberger, "Ephestia: The Experimental Design", Ibid. p. 535. There had been geneticists working on developmental and physiological genetics prior to 1930s. However, it is during late 1930s and 1940s that this domain gets more dominant in the genetic international community.

[29] Robert E. Kohler, *Lords of the Fly*, Ibid. p. 208.

[30] José A. Serra, *Estudos de pigmentação melânica*, Ibid. p. 1-2.

[31] Research is needed to clarify the scientific production of Alberto Xavier da Cunha developed in the department of Zoology and Anthropology of Coimbra. In 1931, this Portuguese was a member of Kuhn's working group on physiological genetics of *Ephestia kühniella*, Hans-Jörg Rheinberger, "Ephestia: The Experimental Design", Ibid. p. 547. In 1940s he was Serra's colleague at the department of Zoology and Anthropology. No correspondence was found between them in SF-DZAC. Xavier da Cunha became director of the Zoological (and Anthropological) Laboratory and Museum of Coimbra in 1953 after Serra moved to Lisbon in 1952.

the American, Harwood qualified as more specialized and especially focused in transmission genetics.[32]

Under the supervision of Tamagnini, Serra comprehensibly had to choose Portuguese population as his main object. But he was also forced to choose another organism – whose races were known in genetics terms and with which it was possible to conduct experimentation – in order to conclude the second part of his PhD.[33] He chose rabbits.

At a first cursory historical glance I may suggest that this kind of research project was a little bit too much for the conservative anthropological school of Coimbra which envisioned Serra as the proverbial "crazy scientist" pursuing his personal interests alone.[34] Yet this assessment is far from the truth. In fact, Serra was deeply influenced by the events in the Kaiser Wilhelm Institute for Anthropology (KWIA), the German renowned research institute of anthropology directed at that time by Eugen Fisher.

It was exactly in the late 1930s that Fisher – inspired by the work at the neighbour Kaiser Wilhelm Institute for biology, and especially impressed with Alfred Kühn's and Richard Goldschmidt's researches – decided to create this new research line, where he could open his institute to experimentation.[35] The word "phenogenetics"

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[32] Jonathan Harwood, "National Styles in Science", Ibid.

[33] José A. Serra, *Estudos de pigmentação melânica*, Ibid. p. 5.

[34] About Serra's scientific work see Carlos Almaça, "As Primeiras Fases da Obra Científica do Prof. J. A. Serra", *Brotéria Genética*, 13 (1992), 33-40; Rolanda M. Albuquerque, "Professor José Antunes Serra", *Brotéria Genética*, 12 (1991), 5-44; and Luís Vicente, "José Antunes serra (1914-1990). A fascinante aventura da genética," in Ana Simões, org., *Memórias de Professores Cientistas* (Lisboa: FCUL, 2001), p. 112-119.

[35] Exactly when did Fischer present this new line using this name is not clear. In her extensive work on Fisher's activity as the director of KWIA, Sheila F. Weiss notes that the first time he proposed "phänogenetik" as "a kind of new frontier for his Institute" was in a 1940 letter to his colleague and friend Verschuer. She also says that he had presented this new research line, in the 1938 meeting of the German Society for Heredity, but at the time under the umbrella of "developmental genetics in humans", Sheila F.

[from whence derives Serra's "phenogenesis"] was the key word of Fisher's new experimentation project for the KWIA[36]. Fisher's "phenogenetics" aimed to study the role of genes during the development of the organism from the embryo to the phenotype. In the phenotype what was due to genes and what was due to the environment? For this project Fisher selected rabbits, an animal used by Nachtsheim on the study of the expression of hereditary pathological traits in the same institute, as his model organism.[37]

One of the results of this new project at the KWIA was Fisher's human heredity analogies based on Nachtsheim's work on rabbits. Now, Serra was extremely critical of these, considering them "very poorly grounded".

> In order to draw valid conclusions from analogy, one has to establish the adequacy of the analogy or at the very least its likelihood. For instance, for the inheritance of pigmentation in man, E. Fischer drew largely on Nachtsheim's results on rabbits, proceeding by analogy- and such analogy is very poorly grounded. One could not ascertain whether the pigments in Rabbit's (coat) have an analogue in man; one could not tell even how many different species of pigments exist in rabbits as chemical compounds. Admittedly, this unawareness didn't stop the discovery and firm validation of pigmentation inheritance, but in

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Weiss, Ibid. p. 63. Since Serra is using it in 1939, I can only conclude that probably he had discussed the idea before (orally), or that the letter of 1940 was not the first time he referred to it.

[36] As Weiss argued, phenogenetics was another word for "physiological genetics", conceptualized by Goldschmidt in his 1927 treatise *Physiologische Theorie der Vererbung*. The author even speculates that regarding the German's political atmosphere in the late 1930s, Fisher preferred to adopt the term phenogenetics, coined by the "Arian" Valentin Haecker, thqn the term proposed by Goldschmidt, a Jewish émigré. The word "phegogenetics" was firstly coined in 1918 by German biologist Haecker, but with a different meaning from what it acquired in the 1930s as described by Sheila F. Weiss, Ibid. p. 63.

[37] Sheila F. Weiss, Ibid. p. 63.

order to be able to lawfully draw analogies, one is required to harness a certain number of basic facts.[38]

According to Serra, in order to make such analogies it was necessary to know the exact composition of melanic pigments (or melanins) and how many of these were involved in man and rabbit. Serra was amazed to realise how these "basic facts" were so poorly studied, even in rabbits.

> Among mammals, rabbits are the ones where pigmentation is better studied, both from the standpoint of its inheritance and physiology, on which there is an extensive literature, mainly American and German. The findings of Nachtsheim and his school, Schulz, Engelmeyer, Daneel and associates, and also those of Castle and associates, have shed light on inheritance, and enabled us to perceive heredity and slightly better the phenogenesis (these latter, though, only in "Russian" rabbits), while the constitution and number of pigments are left to unveil, since the methods employed are ill fit for those objectives.[39]

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[38] "Para que se possam tirar conclusões válidas por analogia, convém demonstrar que realmente a analogia existe ou é muito provável. Por exemplo, para a hereditariedade da pigmentação no homem, E. Fischer partiu, em grande parte, dos resultados de Nachtsheim sobre os coelhos, procedendo por analogia – e esta analogia é muito incompletamente fundada. Não se sabia se os pigmentos existentes no coelho (pêlos) têm os seus homólogos no homem, não se sabia mesmo quantas espécies diferentes de pigmentos existem no coelho como compostos químicos diferentes. É claro que este desconhecimento não impediu a descoberta e firme estabelecimento da hereditariedade da pigmentação no coelho, mas é conveniente que, para se tirarem legitimamente analogias, se conheça um certo número de factos basilares.", José A. Serra, *Estudos de pigmentação melânica,* Ibid. p. 110;

[39] "Entre os mamíferos é no Coelho que está melhor estudada a pigmentação sob o ponto de vista da sua hereditariedade e mesmo da fisiologia, havendo uma literatura numerosa, sobretudo de autores alemães e americanos. Os resultados de Nachtsheim e sua escola, Schulz, Engelmeyer, Daneel e associados, e também os de Castle e associados, aclararam a hereditariedade e um pouco da fenogénese (esta apenas em coelhos "russos") emquanto que a constituição e número de pigmentos ficaram por resolver, pois os métodos empregados são pouco próprios para estes fins.", José A. Serra, *Estudos de pigmentação melânica*, Ibid. p. 4.

In 1940, the year after his PhD presentation, Serra decided to organize a research line on the composition of melanic pigments. Though his ambitions were to contribute to a better understanding of genetics, as he clearly stated on his thesis, Serra knew how to play at home. During that year, in a congress on the science of population held in Porto [Comemorações Portuguesas de 1940], Serra argued that pigmentation constitutes the "indispensable basis" for human racial classifications.[40] He knew that anthropologists did not want to know about the biochemistry of melanins unless he could convince them that such a work could contribute to the study of racial classifications so in vogue during that period of strong eugenic influence.

4.4 Laboratory settings for biochemistry of pigmentation: the German link

In the spring of 1940, the Institute for High Culture [Instituto de Alta Cultura – IAC] – the state organism responsible for scientific grants (the successor of JEN, the Board for National Education) – granted a scientific research scholarship to "be in charge of a course [encarregado de curso]".[41] This scholarship had to be renewed each year. Proper scientific devices and reagents were sponsored on another way. Also in the spring of 1940, Serra got a grant from this university, called *Fundo Sá Pinto*, 18.000 escudos for his "studies on melanic pigmentation".[42] In 1943 he received 6.000 escudos

[40] José A. Serra, "Novos métodos de estudo da pigmentação e sua importância racial", in *Congresso Nacional de Ciências da População, Comemorações Portuguesas de 1940*, Porto (1940), 1.

[41] Rector of the University of Coimbra (António Morais Sarmento) to director of the Faculty of Sciences of the University of Coimbra, 3 June 1940, SF-UC.

[42] Secretary of the University of Coimbra to Serra, 1 May 1940, SF-UC.

for "material acquisition"[43] and, in 1943, 2.000 escudos for "extra funds for the acquisition of a Lumetron photometer".[44] The bulk of Serra's experimental work, where he conducted the experiments on the composition of melanins, took place in the Chemical Laboratory of the Faculty of Sciences of Coimbra.

But how Serra got the needed pure lines of rabbit's hairs? His biochemistry work on the composition of melanins would only be interesting if he could use "pure races" (or pure lines), that is, genetically known races. Only in this case would his results contribute to the discussion on the physiologic genetics and on the analogies of human heredity. But – how to get them in the beginning of World War II? It was the German geneticist Nachtscheim, from the *Institut für bungsforschung der Landwirtschaftlichen Hochschule*,[45] who provided Serra with this experimental material.[46]

In the beginning, Serra and Tamagnini[47] tried a more institutional way. Tamagnini wrote to the dean of the university[48], who on 10 of November 1939 wrote to Veiga Simões, Ambassador of Portugal in Berlin [Ministro de Portugal em Berlin] inquiring if he could get these samples of "rabbit hairs" through Nachtscheim.[49] However, this "diplomatic" route was simply too slow for Serra, who eventually found another way to get in contact with Nachtscheim. Indeed in 31 of January 1940, when

[43] Secretary of the University of Coimbra to Serra, 2 April 1943, SF-UC.

[44] Rector of the University of Coimbra (Maximino Correia) to Serra, 14 April 1943, SF-UC.

[45] The name of this intitute was mentioned in Veiga Simões' letter (see reference in the next note). However, according to Sheila F. Weiss in 1941 he was working at the KWIA ("Human Genetics and Politics", Ibid. p. 62). Was Nachtscheim in these institutes at the same time?

[46] Veiga Simões to Rector of University of Coimbra (António Morais Sarmento), Berlin 31 January 1940, SF-UC.

[47] The director of the anthropological school was always very protective towards Serra's activities, even those more off mainstream.

[48] Rector of the University of Coimbra (António Morais Sarmento) to Eusebio Tamagnini, 14 February 1940, SF-UC.

[49] Veiga Simões to Rector of the University of Coimbra (António Morais Sarmento), Berlin 31 January 1940, SF-UC.

finally Veiga Simões replied, justifying his delay with the "abnormality of postal communications that were not made by plane via Roma", it was to say that when he contacted the German scientist he had already packed the "rabbit's hairs" to Serra.[50] Who had contacted Nachtscheim first? No evidence was found, but I hazard to speculate that Tamagnini used the "friendly relations" between himself and Fisher, or simply, between someone from the KWIA and his school of anthropology.

Adopting the number and constitution of melanic pigments as his scientific target, Serra becomes hence specialized in the composition of melanic pigmentation, that is, in a biochemical issue of great importance not only for a better understanding of phenogenetics, but also for the correction of human heredity analogies. Serra soon realized that biochemistry was probably one of the few possible ways of entering the international arena for a junior scientist like himself, in a peripheral country without a genetics tradition able to organize conveniently human and laboratory resources. If he did not have the conditions to do physiology or development of genetics in a larger sense, why not contribute with small but important scientific discoveries on biochemistry that could afterwards be used by bigger laboratories? Biochemical community was not an easy one to play in. "A life was a zero-sum game: only one person got a prize, the one who got it first", Robert Kohler once put it.[51] But during the first years of Serra's research, this strategy proved to be right on track. International recognition came later: in 1943, with a publication on the reputed European journal *Genetica*, and in 1946 with a short note in *Nature*.[52]

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[50] Veiga Simões to Rector of University of Coimbra (António Morais Sarmento), Berlin 31 January 1940, SF-UC.

[51] Robert E. Kohler, *Lords of the Fly*, Ibid. p. 233.

[52] His publications on the topic covering the period from 1938 to 1952 were: "Estudos de pigmentação melânica. A determinação da pigmentação e escurecimento com a idade. Composição das melaninas", Tese de Doutoramento", *Revista da Faculdade de Ciências da Universidade de Coimbra*, 7 (1939), pp. 235-407; "Novos métodos de estudo da pigmentação e sua importância racial", Congresso Nacional de

Material culture would yet dictate the rules. To pursue his research on the composition of melanins he needed an ultraviolet microscope, an ultracentrifuge or an electroforese, material he could not acquire.[53] A travel to Sweden would have solved this material impasse, but during the 1940s (even before the 1947 purge) going abroad was not an easy step for students with IAC scholarships to work in Portugal. "When will it be that a plain trip to Sweden doesn't prove almost as difficult as a voyage to the moon?", Serra unburdened himself in 1944 to Celestino da Costa.[54] Eventually he felt disheartened and turned his attention to other research lines.[55]

4.5 Zoological Laboratory: from cell to nucleus

In 1943, on a report to IAC, Serra still presented the biochemical approach to melanic pigmentation as his front line of research during the next year.[56] However, he had other ongoing projects. One was a promising collaboration with Queiroz Lopes,

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Ciências da População, Comemorações Portuguesas de 1940, Oporto, 1940; "Estudos sobre pigmentação melânica. Espectrografia dos pigmentos do coelho", *Las Ciencias*, 7 (1941), pp. 904-914; "Sobre a natureza das melaninas", IV Congresso da Associação para o Progresso das Ciências (conjuntamente com XVII Congresso da Associação Espanhola para o Progresso das Ciências), Oporto, 18-24 June 1942, pp. 673-688; "Sur la nature dês mélanines et la mélano-genèse", *Genetica*, 23 (1943), 300-314; "Fenogénese e constituição das melaninas de mamíferos", *Boletim Escola Farmácia de Coimbra*, 4 (1945), pp. 188-298; "Constitution of hair melanins", *Nature*, 157 (1946), 771-772; "Natural melanins. Constitution and Production", *Chemical Products*, Mach-April (1947), 31-37. There is also an isolated work, as far as I could ascertain, published in *Nature* and also related with pigmentation, but with a more genetically oriented approach (and less biochemical): "A possible explanation of the relations between heredity anaemias and depigmentation in the mouse", *Nature*, 159 (1947), 504-505.

[53] "Logo que a ocasião seja melhor, espero ir (ou pelo menos fazer os possíveis por ir, até à Suécia, onde estão lá o Caspersson e o seu microscópio de ultra-violeta, mas também o Svedberg com a ultracentrifugadora e o Tiselius com a electroforese, que muito me interessam para a resolução do problema das melaninas que actualmente me é impossível resolver sem os aparelhos de que lá dispõem porque são muito caros. Mas quando será que uma viagem até à Suécia não é quasi tão difícil como uma viagem à lua?", Letter from Serra to Celestino da Costa, 23 March 1944, SP-DZAUC.

[54] Letter from Serra to Celestino da Costa, 23 March 1944, SP-DZAUC.

[55] Interview with Rolanda M. Albuquerque, September 2009, Lisbon.

[56] Serra to IAC, 13 December 1943, SF-IAC.

another member of the anthropological school of Coimbra. Their aim was to contribute to a better understanding of nuclear physiology. Together they designed several experimental systems to study the behaviour of nucleolus[57] and chromosomes[58] during the mitoses and meiosis. The organisms Serra and Lopes used were snails (*Helix aspersa* and *Tachea nemoralis*), grasshoppers and *Chironomus* (larvae buds). Regarding these latter, interesting for their large saliva chromosomes, there is a nice story of material culture: *Chironomus* were collected every time skeletons of the Anthropological Museum were transported to the terraces of the old university to be washed out. It was during this maintenance routine that Serra and Lopes collected these larvae bugs that appeared floating in the dirty water of the skeletons.[59]

The experimental work was conducted in the Zoological Laboratory. Melanic pigmentation had pushed Serra from the Anthropological Museum to the Chemical Laboratory, but now he returned to his original department to initiate a research line of "basic science". At that time only Lopes and Xavier da Cunha conducted experimental work there.[60] No evidence has been found revealing any efforts to justify his new

[57] "Diekter Nachweis und Lokalisation von basischen Protein in dem Chromosomen und im Nukleolus", *Chromosoma* (Berlin), 2 (1944), 576-595; "Comportamento do nucléolo durante a ovulação em Moluscos", *Boletim Sociedade Broteriana* (Coimbra), 19 (1944), 295-309; "Données pour une citophysiologie du nucléolo. I – L'activité nucléolaire pendant la croissance de l'oocyte chez des Helicidae", *Portugaliae Acta Biologica*, 1 (1944), 51-94; "Chemical Constitution of the Nucleolar Inclusions in Growing Oocytes Cells, *Nature*, 155 (1945), 792.

[58] "Mitose e Meiose. Dados e interpretações sobre a fisiologia da mitose e da meiose" in *Actas I Reunião Citológica* (Lisboa, 1945), vol. I, pp. 47-96; "Contributions to a physiological interpretation of mitosis and meiosis. I-The composition of the resting stage nucleus", *Portugaliae Acta Biologica*, 2 (1947), 25-44; "Contributions to a physiological interpretation of mitosis and meiosis. II-The profase appearing of the cromonemata and spiralization", *Portugaliae Acta Biologica* (1947), 45-90; "Composition of chromonemata and matrix and the role of nucleoproteins in mitosis and meiosis" in *Cold Spring Harbor Symposium of Quantitative Biology*, 12 (1947), 192-210; "The parallelism between the chemical and the morphological changes in the chromosomes during mitoses and meioses", *Acta Physiologica Cellularis* (Proceedings of the 6th International Congress of Experimental Cytology), 8 (1947), 111-122.

[59] Interview with Rolanda M. Albuquerque, September 2009, Lisbon.

[60] Interview with Rolanda M. Albuquerque, September 2009, Lisbon. She also mentioned a German researcher, Ernest Mathess, who started to work there after escaping from Nazism – Mathess is co-author of one of the most popular textbook of practical zoology in Portugal, edited by Almedina, Coimbra.

interest within the anthropological school of Coimbra. Probably he had already the scientific legitimacy he needed to choose by his own his research lines – a sign that zoology was finally emancipating itself in relation to anthropology.

If melanic pigmentation had led him from the individual to the cell, cytogenetics was now taking him further on this biological journey: from cell to nucleus. Serra's scientific agenda was to integrate morphological and chemical data on the nuclear components.

> The changes in the chromosomes during cell division are usually considered separately, either from the point of view of the morphologist or from that of the biochemist. This lack of integration results from the evidence gathered by researchers in different fields, and thus the conclusions reached sometimes seem to contradict one another.[61]

Taking advantage from the modes of practices in biochemistry and experimental apparatuses developed during the melanic pigmentation's project – in fact, he had already anticipated (in 1941) that such methodology could be applied to nuclear physiology[62] – he designed several experimental systems where genetic problems, such as the chemical composition of the nuclear inclusions, the composition of the cronometrata or the role of nucleoproteins, were investigated. During the course of these experiments, Serra made a methodological discovery – he described the histochemical

[61] José A. Serra, "The parallelism between the chemical and the morphological changes", Ibid. p 111.

[62] "Os métodos de ataque a este problema [composição das melaninas] são principalmente os usados em casos semelhantes para as proteínas. Foram efectuadas as análises de preparados em que a separação das queratinas e de outras proteínas dos tecidos era mais ou menos completa e no futuro pretendo [….] aclarar um pouco estas questões. Os resultados obtidos servirão de preliminares no estudo fisiológico que tenciono executar no futuro. Conjuntamente, e com os mesmos métodos, poderão ser estudados outros compostos ou complexos proteicos tais como os nucleoproteidos, assim como alguns problemas de fisiologia nuclear correspondente.", Serra to IAC, 17 February 1941, SF-IAC.

arginine reaction, which could be used in the characterization of the basic and non-basic proteins.[63] Such discovery was described in an article to *Stain Technology* in 1946 – by far his most cited article.[64]

The results of this research line, published in the German journal *Chromosoma* (1944) and in *Nature* in the next year (1945), were very well received by the international community. In 1947 he was invited to attend the Sixth International Congress for Experimental Cytology to be held in Stockholm,[65] and to participate in the Symposium on Quantitative Biology at Cold Spring Harbour. The symposium was dedicated to "Nucleic Acids and Nucleoproteins" and Serra should present a paper on the session on "cytological aspects".[66] In the letter sent by Demerec, he explained that one of his goals was "to stimulate the exchange of ideas between workers in borderline sciences".[67] As I will show in the next chapter, Serra was not authorized to go.

Meanwhile, Serra maintained his collaboration with other departments – he continued to work on morphology and osteometry of the Portuguese population, as well as on its serological chart, projects where he improved his statistical skills,[68] and he

[63] José A. Serra, "Histochemical tests for proteins and amino acids. The characterization of basic proteins", *Stain Technology*, 21 (1946), 5-18.

[64] A search in the ISI Web of Knowledge counted 183 papers which cited it. Then followed a paper presented in Cold Spring Harbor symposium (33 citations) and his *Nature* paper on the composition of melanins (29 citations). This search was done in February 2008 for the time period 1945-1954.

[65] Letter from Hydén to Serra, 15 January 1947, SF-UC.

[66] The other sessions at the symposium were: "structure and nucleic acids", "metabolism", "active biological agents", "viruses as nucleoproteins" and "bacterial nucleoproteins", Letter from Demerec to Serra, 13 February 1947, SF-UC.

[67] Demerec to Serra, 13 February 1947, SF-UC.

[68] Some of his works, published between 1936 and 1952, within the old tradition of the Coimbra's anthropological school are: *A Pelve no Portugueses*, Ibid. (1938); "Morfologia do esterno", Congresso Nacional de Ciências da População, Porto, 1940, pp. 453-471; *O esterno nos Portugueses. Caracteres métricos e morfológicos do esterno no homem* (Coimbra, Tipografiaa da Atlântida, 1941); with L. P. Canêdo de Morais, "Sobre a determinação do índice orbitário e a assimetria da órbita", *Las Ciências*, 3 (1941) 3; with L. P. Canêdo de Morais, *Sobre a determinação do índice orbitário e a assimetria da órbita* (Coimbra: Tipografia da Atlântida, 1941); "Subsídios para a história da antropologia portuguesa" (1942), Ibid.; *As proporções e a assimetria dos membros dos portugueses* (Coimbra: Tiprografia da Atlântida,

collaborated with Fernandes of the Botanic Institute, on a study about the effect of chemical and physical agents on chromosomes, especially on the heterocromatine.[69] He was also responsible for the abstracts of articles published in Portuguese scientific journals to appear in *Biological Abstracts* (from 1940 to 1952) and *Resumptio Genetics* (edited by M. J. Sirks).[70] In 1944, he founded, together with Flávio Resende, *Portugaliae Acta Biologica*, a Portuguese scientific journal which Serra tried to make known abroad through his international publications. During his stay in Coimbra (from 1936 to 1952), 21 of his publications were in international journals (in a total of 70 publications).

4.6 Genetic controversies in the 1940s: a struggle for synthesis

Serra produced a considerable amount of theoretical work in the beginning of his career. In fact, while he published his experimental results on international journals, he expounded in peripheral ones his general interpretations on genetic controversial matters. One of the privileged topics in the 1940s was the *nature of the gene*. The topic divided geneticists: some argued that the gene was something that could be precisely located in the chromosome (the "particulate" view of the gene, more popular among American geneticists), and others, such as the German geneticist Richard Goldschmidt,

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1943); with A. Queirós Lopes, Contribuições para o Estudo da Antropologia Portuguesa. XXI – *Correlações entre a estatura e alguns caracteres osteométricos (*Coimbra: Tipografia Atlântida, 1944);

[69] "Euchromatine et hétérochromatine dans leurs rapports avec noyau et le nucléolo. *Bol. Soc. Broteriana*, 19 (1944), 67-117 and "Sobre a origem das substâncias constitutivas do necléolo durante a ovulação em Moluscos, *Bol. Soc. Broteriana*, 19 (1944), 109-122. Other works: "Sobre a Origem das substâncias constitutivas do nucléolo", *Revista da Faculdade de Ciências da Universidade de Coimbra*, 14 (1944), 109-122;

[70] Rolanda M. Albuquerque, "Professor José Antunes Serra", *Brotéria Genética*, 12 (1991), 15.

the father of physiological genetic, argued that the whole chromosome should be interpreted as the heredity unit (the "architectural" view of the gene).

Serra's participation on this controversy exemplifies his "synthetic" attitude when confronted with controversial matters. Recent observations on the experimental production of mutation, chromosome re-organizations and position effects had contributed to the so-called *crisis of the gene*. Serra analysed the two major hypotheses and proposed his own based on the subdivision of the gene in what he called the "active-zones" and the "linking zones". In this way, he could accept the gene as the unit of heredity but explain at the same time the importance of its position in the chromosome. According to him, the association of "active zones," with different orders of magnitude, represented a gene mutation with possible phenotypic effects. Hence, as he liked to say, the crisis of the gene should be viewed "rather as a growth crisis than as a senesce crisis".[71] His position on this controversy was firstly expounded in "An attempt at a synthesis of the concept of the gene", published in 1944 in the Portuguese journal *Boletim da Sociedade Broteriana*. After discussing each of the opposed theories (particulate and architectural), he states in a Hegelian way:

> This opposition is a conflict between two antitheses and as both theories explain what is evidently an aspect of reality, the solution of this problem will certainly be reached only by a synthesis at a higher level. We are sure that any such synthesis is at present merely provisional [...]. Nevertheless, it is extremely foolish for the partisans of each theory simply to ignore the other merely because

[71] José A. Serra, "An attempt at a synthesis of the concept of the gene", *Boletim da Sociedade Broteriana*, 19 (1944), 362.

it is the antithesis, for there are facts which cannot be explained by any other means than by a kind of blending between the two hypotheses.[72]

Another genetic controversy popular in the 1940s regarded the possibility of cytoplasm having a role in the inheritance of characteristics from progenitors to offspring. The first time Serra deals with the cytoplasm-nucleus controversy is in his textbook of genetics, *Genética Geral e Fisiológica* (1949).[73] The textbook offers a balanced survey of the international literature (up to 1947) both on transmission and physiological genetics.[74]

Serra described one by one each phenomenon explaining the several experiments supporting them, the majority of which were performed by German scientists. For him, cytoplasm inheritance was evident in the cases of maternal predetermination (based on experiments designed by Alfred H. Sturtevant, Carl Correns and M. J. Sirks) and maternal transmission (based on Alfred Kuhn's work). He also accepted the "Dauermodifikationen", with a possible "multiplication power", although under the "control of the genes"[75] (he cites Victor Jollos). For him, all these phenomena, including the plastids, could be explained by the existence of "substances" or "particles" in the cytoplasm.

He became sceptical, though, concerning the "general heredity effects of the cytoplasm" – that is, hereditable characters *apparently* transmitted by the cytoplasm and

[72] José A. Serra, "An attempt at a synthesis of the concept of the gene", Ibid, p. 361.
[73] José A. Serra, *Moderna Genética. Geral e Fisiológica* (Coimbra: Author's edition, 1949), Vol. II. Indeed, in spite of being the first textbook of genetics written in Portuguese, Serra did not obtain external funds to publish it.

[74] As Serra pointed out in the introduction "one should think before accepting the predominant ideas which tend to focus more on the formalistic and Mendelian Genetics (...) and less on Physiologic Genetics.", *Moderna Genética,* Ibid. p. V.

[75] José A. Serra, *Moderna Genética*, Ibid, p. 553.

which are not linked to well individualised particles. However, his scepticism did not prevent him from commanding state of the art literature.[76] Serra criticized both theories but especially the plasmagenes because "similarities between such particles [plasmagenes] and genes, concerning their nature and composition, have still to be largely demonstrated."[77]

In fact, Serra's position on cytoplasm inheritance was very similar to Richard Goldschmidt's. Although Goldschmidt was a partisan of the architectural hypothesis of the nature of the gene, he also sustained a "nucleus-centric" view of heredity. For both geneticists, cytoplasm was only a substratum for the action of the genes. For Ian Sapp, Goldschmidt's position was odd for he was a geneticist working on physiological genetics and development, sustaining an architectural view of the nature of the gene, but denying the possibility of cytoplasmic inheritance. Sapp describes Goldschmidt in *Beyond the Gene* as someone "who was always on the outside of whatever side was in"[78]. Curiously, here, Sapp gets closer to the American traditional history of genetics which has been so critical towards the speculative style of Goldschmidt. In fact, as Michael Dietrich pointed, the German scientist was taken quite seriously in the 1940s, for instance, by Theodosius Dobzhansky, one of the most important figures of the Modern Synthesis.[79] Serra, who did not only respect Goldschmidt as a scientist, but

[76] Indeed he described the works of the Germans Fritz Von Wettstein, Ernst Lehmann, Otto Renner (since 1918) and the reviews of Carl Correns, M. J. Sirks and Peter Michaelis (from 1937 until 1942). The works of Philippe L´Héritier and Tracy Sonneborn were also cited. Serra confronted the two theories in vogue to explain this phenomenon. The theory of plasmagenes, advocated by Hans Winkler (1924), explained cytoplasmic inheritance by hypothetic cytoplasm particles, called the plasmagenes, which were endowed with a nucleus-proteic composition and auto-reproduction capacity; the theory of plasmon, advocated by Fritz Von Wettstein (1937) considered the whole cytoplasm as the hereditary unity.

[77] José A. Serra, *Moderna Genética*, Ibid, p. Ibid, p. 582.

[78] Jan Sapp, *Beyond the Gene*, Ibid. p. 166.

[79] Michael Dietrich, "Goldschmidt's 'Heresies'", p. 433.

shared some of his ideas and admired his adventurous criticism, dedicated to him, in 1949, a volume of a Portuguese scientific journal called *Portugaliae Acta Biologica*.

Serra was a curious mixture of an experimentalist and a theorizer. At the same time he achieved international recognition by the exemplary way in which he conducted experimental work, he theorized and speculated whenever controversies arose. He advocated for several times that speculation should be encouraged in biology. But a pre-requisite to speculation was the survey of all literature. Struggling for synthesis was therefore an obligation for him if he wanted to speculate about controversial issues.[80] His *Genética Geral e Fisiológica*, the first Portuguese textbook on genetics, is the most emblematic example of such commitment.

4.7 Conclusion

"While almost all the world was at war, Prof. Serra, of the University of Coimbra, Portugal, continued to address himself to the chemistry of hair colours". With this sentence, a journalist from *Science Today* (an American journal of popularization of science) opened his article about Serra's work in an issue of 1947.[81] The post-war mood made it hard to contemporize with the narrowness of interests chosen by scientists during the war, especially in a privileged, neutral country such as Portugal. How could this journalist imagine two years after the end of the war that Serra's work was closely associated with the research conducted in one of the German scientific institutes which had benefited the most from the racial hygiene ideas of the Nazi Party? How could he

[80] See Maria do Mar Gago, "Portuguese struggle for synthesis: José Antunes Serra and the genetic controversies in the 1940's", 6th STEP Meeting, Junho 2008, Istambul, Turquia.

[81] *Science Today*, 29 (May 1, 1947), p. 218-219; the article was entitled "Hair colours".

imagine that Serra's peaceful, neutral and dull subject was directly connected to the racial ideology that had marked World War II?

The first conclusion is that the appropriation of genetic knowledge within the anthropological community highly benefited from the political context of colonialism aiming at the implementation of eugenic programs in the Portuguese colonies. As Fisher during the first years of the Nazi regime, Tamagnini was engaged in selling human heredity as a national resource during the mid 1930s, when eugenics programs were to be implemented in the Portuguese colonies. His presentation at the 1934 congress corroborates this view. But, if for the German anthropological community Mendelism was a working tool since 1914, for the Portuguese anthropologists it was something new. Hence, in Portugal eugenic reasonings contributed to the beginning of the appropriation of Mendelism by the anthropological community.

Furthermore, Serra used racial classification to legitimize his scientific goals on phenogenetics after his PhD (defended in 1939). However, Serra's interests were less clear than Tamagnini's. Still, in a publication presented in a congress on the science of population in Porto [Comemorações Portuguesas de 1940], he claimed: "Pigmentation constitutes the indispensable base for racial classifications and this reason is sufficient to justify interest on the study of pigments."[82] Serra was responding to the latest trend in the KWIA, Fisher's attempt to introduce experimentation in anthropological heredity studies. Since this could not be done with humans, other animals, such as rabbits, had to be chosen. It is exactly following the same scientific dilemmas of the KWIA, many times criticizing Fisher's methodologies, that Serra distanced himself from the biometric studies and introduced experimental animal genetics in the department. Basic experimental material was also sent from the KWIA.

[82] José A. Serra, "Novos métodos de estudo da pigmentação e sua importância racial", in *Congresso Nacional de Ciências da População, Comemorações Portuguesas de 1940*, Porto (1940) 1.

But not only American common sense considered exotic Serra's work on the composition of melanin. Almaça's survey of his scientific production during his early career fails to explain the epistemological relationship between Serra's human heredity studies and the "chemistry of the hair colour".[83] He speaks of a "spill over from Anthropology into General Biology". In this chapter I hope to have demonstrated how Serra was trying to respond, not only to the latest genetic trends in general biology (a deep interest on physiologic genetics, in other words, in phenogenetics), but also to the more recent challenges posed to the international anthropological community, especially the German. The key to understand Serra's broad scientific production in Coimbra is pigmentation: pigmentation forced him away from biometry and into biochemistry and through the study of the composition of melanins he set an experimental system that could be used in nuclear physiology; in chapter five, I will show how that it was also pigmentation that led Serra to collaborate with the New State.

The second conclusion is, therefore, the crucial influence German geneticists had on the consolidation of genetics in Coimbra during the war. In this thesis I raise the hypothesis that ideological "affinities" between Tamagnini and Fisher (both conservative and nationalist) could had influenced the direction of Coimbra's anthropological school. Another possible explanation could be related to the supply of international scientific journals in Portugal. In the summer of 1945, Serra wrote to Sewall Wright asking him some reprints and apologizing saying that "due to the war, which has now ended, we have been almost completely without American scientific periodicals."[84] Was there a difference between the supply of American and German journals? Further investigation is needed to corroborate these hypotheses.

[83] Carlos Almaça, "As primeiras fases da obra científica do Prof. J. A. Serra", Ibid. p. 33

[84] Letter from Serra to Sewall Wright, 28 August 1945, SP-DZAUC.

A third conclusion aims to contribute to a future construction of the history of Portuguese genetics. With the help of hindsight, it is interesting to see that similarly to what Quintanilha had done ten years earlier, when he introduced experimental practices and settled laboratory conditions for genetic research in the Botanic Institute of Coimbra, Serra was now doing the same in the Zoological and Anthropological department, including the Anthropological Museum and Laboratory and the Zoological Museum and Laboratory. But it was in the latter that his role – as a scientist and as a research organizer – had more important consequences having in mind the hegemonic position of anthropology within this department. With Serra, zoology – not systematic zoology, but experimental zoology (with rabbits) – could also be sold as a resource to the political regime.

But how did he so rapidly accomplish international recognition? His success is certainly related to both his cognitive capacities (in particularly his theoretical aptitude to struggle for synthesis in every controversial issue) and his way of organizing scientific activity – legitimating his interests in political and scientific contexts, resisting financial adversities, and starting collaborations with colleagues of other departments (of chemistry, especially). However, Serra was also intelligent enough to decide to what areas of genetics he could contribute with novel experimental data. In biochemistry "life was a zero-sum game", as Robert Kohler once put it. But to a junior scientist in a country without genetic tradition, with poor resources for experiment and difficulties of participating in international congresses, a biochemical approach to genetics was above all an opportunity: a chance to comply to international standards and publish in the best journals.

CHAPTER FIVE

Sheep Genetics: Serra in the construction of the New State

At the end of World War II in May 1945 and after the defeat of Hitler and Mussolini regimes, Salazar suspended the General Assembly [Assembleia Geral] and ordered anticipated elections on 18 November 1945. Along with these measures to legitimate his rule, the Portuguese dictator suspended censorship.[1] Taking advantage of this political thaw, some opponents to the fascist regime organized a meeting with elements of the First Republic and of the Portuguese Communist Party, which took place at the Centro Escolar Almirante Reis (Lisbon) in 8 October 1945.[2] The Movement of Democratic Unity (MUD) [Movimento de União Democrática], was born. In the end, the ensuing report was signed not only by the promoters of the initiative but by all attendants, among whom were the Portuguese scientists Bento Jesus Caraça, Ruy Gomes, Torre da Assunção, Manuel Valadares.[3]

"Germany's defeat does not mean the automatic downfall of Salazar! The people must free themselves by their own means", alerted the communist journal *Avante!* in May 1945.[4] But these warnings did not affect many intellectuals who, inspired by the liberation of Europe, were convinced that Portugal would soon have its deserved

[1] António Ventura, "Resistências ao regime ditatorial" in João Medina, eds., *História de Portugal. O Estado Novo. Opressão e Resistência* Vol. XIII (Amadora: Clube Internacional do Livro), p. 170.

[2] File Hélio Vieira Quartim, Process MUD Juvenil, nº 50-46, box 2513, PIDE-DGS, ANTT. During the research for this thesis I had no access to Serra's file in PIDE-DGS, ANTT.

[3] Júlia Gaspar, *A Investigação no laboratório de física da Universidade de Lisboa (1929-1947)*, MSc thesis in History and Philosophy of Science, Faculty of Sciences of the University of Lisbon , 2008, and published as Júlia Gaspar, *A Investigação no laboratório de física da Universidade de Lisboa (1929-1947)* (Braga: CIUHCT, 2009).

[4] Avante!, 76, VI serie, May 1945, p.1 in António Ventura, "Resistências ao regime ditatorial", Ibid.

democracy. Serra was one of these optimistic men who, in 29 October 1945, signed MUD's petition to change the rules for election to the National Assembly [Assembleia Nacional].[5]

5.1 The dark year of 1947

For around two years MUD was tolerated by the fascist regime, yet this situation changed in 1947. Twelve years after the 1935 university purge (which expelled Quintanilha from the university), the government ordered again the expulsion of university and military opponents. Celestino da Costa, Bento Jesus Caraça or Manuel Valadares were some of the scientists ousted from their functions in the June 1947 purge. This repressive action meant partly to punish MUD members, who had organised public sessions and published protesting documents[6]; in January of 1948, the government declared MUD illegal.

Serra was not expelled from the university. But, as many others, he was forbidden to travel abroad. This restriction was particularly harsh in Serra's case, considering he had not yet applied to JEN/IAC for a scholarship to study abroad. After a whole scientific career "made in Portugal", he had finally conquered the international recognition of his peers when suddenly the dictatorship closed all doors around him. On 15 January 1947, Hydén invited Serra for the Sixth International Congress for Experimental Cytology, to be held in Stockholm from 10 to 17 July.[7] One month later,

[5] Interview with Rolanda M. Albuquerque, September 2009, Lisbon; and Serra's letter to Salazar (see reference below).

[6] See Júlia Gaspar, *A Investigação no laboratório de física da Universidade de Lisboa (1929-1947)*, Ibid. pp. 156-169.

[7] Letter from Hydén to Serra, 15 January 1947, SF-UC.

it was the turn of Milislav Demerec who invited him to participate in the Symposium on Quantitative Biology at Cold Spring Harbour, in New York, during 11-20 June. After the symposium all participants were invited to stay for summer research at the Biological Laboratory of this institute, explained Demerec to Serra, and travel and accommodation expenses – "for as long as you can stay after the symposium" – were to be covered by the laboratory.[8]

The response from IAC to Serra's request for authorization for both symposia, took time to arrive. Serra insisted, in March 1947, through a more institutional way[9], and both Tamagnini and the director of Faculty of Sciences of Coimbra intervened on his behalf with a note of information, regarding especially Cold Spring Harbour's invitation.[10] "An honour for us", wrote Tamagnini. On 12 June 1947, Celestino da Costa was perfectly convinced that Serra would get the authorization.

> I trust your trips both to Stockholm and America are definitely settled. At least I hope so from the bottom of my heart. I must admit I regret not going to Stockholm, but alas it cannot be. The main thing, Serra, is that you should go. [11]

But in the same day, IAC wrote Serra the official letter; and the verdict was negative.[12] Celestino da Costa was soon dismissed from IAC and from the University of

[8] Letter from Demerec to Serra, 13 February 1947, SF-UC.

[9] Serra to Director of the Faculty of Sciences of Coimbra (João Pereira Dias), 18 March 1947, SF-UC.

[10] Information notes of Tamagnini, 18 March 1947, and of the Director of the Faculty of Sciences of Coimbra 19 Mach 1947; both in SF-UC.

[11] "Espero que a sua viagem a Stockolm bem como a da América estejam já definitivamente resolvidas. Pelo menos assim espero de coração. Confesso que tenho pena de não ir a Stockolm, mas essa não pode ser de forma alguma, paciência. Vá o Serra, que é o essencial", Letter from Celestino da Costa to Serra, 12 June 1947, SP-DZAUC.

[12] Secretário IAC (Medeiros-Gouveia) to Reitor UC 12 June 1947, SF-UC.

Lisbon. On 25 June 1947, Serra wrote him a quite touching letter where he urged Celestino da Costa to "fight wholeheartedly against any thought or physical state hazardous to your health."[13] "[…]. You are held in great esteem by all of us, your admirers, who wish you to pursue your scientific task undaunted, and to go on with your work."[14]

Celestino da Costa was eventually readmitted in the following year, as well as many other scientists, such as Flávio Resende – the reasons behind this sudden reinstatement are still not clear. The environment in the Portuguese universities would not be the same, however. In December of 1947, Serra thought that the government had refused him the authorization to go to Stockholm and Cold Spring Harbour because of the "tension politique du moment"; he was convinced that in the next year he would be authorized to go abroad.[15] But he soon lost all illusions.

In the end of 1948, he decided to write to Salazar. He saw himself, in the line of Thomas Mann, a non-political man – a self-image presented in all his scientific and non-scientific work – and he could not accept the regime to have a different view.

> […] Whoever should come to Coimbra, a small milieu where everything is easily known, will find that I am not a political man, that I count for nothing in local politics, and that all my minutes are devoted to my scientific work and life as a university professor.[16]

[13] Letter from Serra to Celestino da Costa, 25 June 1947, SP-DZAUC.

[14] Letter from Serra to Celestino da Costa, Ibid.

[15] Letter from Serra to Jean Brachet, 17 December 1947, SP-DZAUC.

[16] "[…] quem vier aqui a Coimbra, meio pequeno onde facilmente se sabe tudo o que se passa, pode dizer que não sou político, que nada represento na política local, e que e trabalhos científicos, e que gasto todo o tempo na minha vida de professor universitário", Letter from Serra to Oliveira Salazar, 20 December 1948, SP-DZAUC.

The motives behind his signature of the petition to change the rules for election to the National Assembly [Assembleia Nacional] are also presented. His demands to the dictator were two: to end the interdiction on travelling abroad and the authorization to apply for the position of full professor.

In the beginning of 1950, the cabinet of the presidency finally authorized the aforesaid application to a full professorship.[17] Serra was invested with the autonomy he had hankered for, chiefly on matters relating to the organization of the Zoological Museum and Laboratory [Museu e Laboratório Zoológico]. Yet the travelling interdiction remained – in fact it was only overcome in the sixties with a change in the political context. For scientists such as Serra, the extent of political oppression was only gradually perceived as it impacted on their scientific careers. Besides, after 1947, the restrictions on travelling abroad where shared by the majority of Portuguese scientists – but not all.

Câmara, the plant geneticist and head of the National Agronomic Station, was one such exception. Two years after the university purge, Câmara was in the University of Missouri, United States, doing research with a team specialized in wheat genetics and working "in ideal conditions, with wonderful and friendly colleagues and employing a material of which I am extremely fond – Triticum."[18] "[…] I surmised that a couple of months of familiarity with these outstanding workers, exchanging ideas and work

[17] Rector of UC to Director of the Faculdade de Ciências da UC, SF-UC. In this same "oficio" it is said that an authorization was denied to his colleague Xavier da Cunha Marques to apply to the very same position, after his expulsion from Legião Portuguesa.

[18] Letter from Câmara to Serra, 24 May 1949, SP-DZAUC.

themes, will allow me to bring back to Portugal something useful", wrote Câmara to Serra.[19]

The motive behind this correspondence, began by Câmara in February 1949, was an invitation for Serra to give a conference at the Superior Council of Scientific Research, in Madrid.[20] The National Agronomic Station was at that time helping the Spanish scientific institution with the "organization of a genetics laboratory".[21] "From time to time, some of us go to Madrid and stay there for short periods, resuming in new laboratories the work we have in hands. Thus, life is gradually infused in the budding centre."[22] Seeing how Serra's work was appreciated by the Spanish colleagues, and wanting them to invite Serra to visit his institute, Câmara had "dared" to intercede and addressed him directly.[23] In May 1949, neither Câmara nor Serra revealed any fear that this trip would not be authorized.[24] Both took for granted the government's authorization and discussed several details. Three months later, Serra had the official answer. He wrote to his colleague of the Spanish institute, José Maria Albareda, saying that "unfortunately" he could not go because he received an official answer to his request stating that "as an economic measure, all voyages abroad are unauthorized." "[…] Personally, I regret this state of things, and that we have to postpone yet further

[19] Letter from Câmara to Serra, Ibid.

[20] Letter from Câmara to Serra, 3 February 1949, SP-DZAUC.

[21] Letter from Câmara to Serra, Ibid.

[22] Letter from Câmara to Serra, Ibid.

[23] Letter from Câmara to Serra, Ibid.

[24] Letter from Serra to Câmara, 6 May 1949, and letter from Câmara to Serra, 24 May 1949, both in SP-DZAUC.

the exchange between Coimbra and Madrid which all of us wish to be as intense as possible."[25]

5.2 Corporatism: the National Board of Husbandry

So far I have described how some *authoritarian* aspects, contained in the 1933 Constitution with which the New State was born, surfaced in 1947, marginalizing Serra from the international community and hence putting obstacles to his scientific goals. Yet this is only part of the story. In 1946, the government also invited Serra to collaborate with the National Board of Husbandry [Junta Nacional de Produtos Pecuários, JNPP]. The aim was for Serra to help the national board's veterinarians to conceive a plan to improve the national wool production.

Portuguese economic interest in wool production during the 1940s can be traced back to the national context of autarky which was consolidated from the institutional point of view during the 1930s. The end of World War I and the great 1929 depression are generally pointed out as the two motives behind western countries search for an agricultural path towards self-sufficiency. In the Portuguese case, it is reasonable to think that the great depression bore only an indirect, not decisive influence since the Wheat Campaign [*A Campanha do Trigo*], inspired in a homologous process conducted by the fascist regime of Mussolini [*La Bataglia del Grano*], started in 1929.[26] But, despite the fact that this campaign became one of the most emblematic examples of

[25] "Infelizmente creio que a ida se malogrou porque actualmente recebemos uma ordem do nosso Governo proibindo, por medida de economia, todas as viagens ao estrangeiro. […] Pela parte que me toca, lamento muito que isto tenha sucedido e que assim mais uma vez tenha que ficar adiado este intercâmbio entre Coimbra e Madrid que todos fazemos votos por que seja o mais frequente possível.", Letter from Serra to José Maria Albareda, Coimbra, 3 August 1949, SP-DZAUC.

[26] Tiago Saraiva, "Laboratories and Landscapes", Ibid. pp. 5-6.

Portuguese autarky, it was during the 1930s that the autarkic trend became institutionally organized following the 1933 Constitution.

Indeed, this Constitution not only served to legitimize the political regime imposed in May 1926, condoning the asphyxia in terms of political rights –, but to re-organize Portugal into a "corporative" state.[27] Autarkic sentiments were therefore translated into a corporative language and several state organisms (like the National Board of Husbandry) were created, revealing a clear and rare policy focus on Portuguese agriculture. The role of techno-scientific elites in the application of these reforms has been recently stressed.[28] Genetics, and in particular plant genetics, could be extremely useful in improving national productivity, and the fascist regime was aware of such possibilities. Recent accounts of the history of the National Agronomic Station and the Cotton Research Centre, directed respectively by two reputed geneticists, António Câmara and Aurélio Quintanilha, compel us to rethink the relationship between scientists – sympathizers of the fascist regime (as Câmara) or not (as Quintanilha) – and New State, invariably described in the orthodox historiography from the perspective of political persecutions.

Although some light has been shed on the relationship between Portuguese fascism and plant genetics, this relationship cannot be extended to animal genetics. Given that Serra was a leading Portuguese geneticist of the twentieth century and JNPP one of the biggest corporative organisms[29], the collaboration between them is therefore a unique case for studying the relationship between animal genetics and Portuguese

[27] I will not discuss the Portuguese version of corporatism, a fractious and contentious issue debated in the historiography, in the context of which it is not even consensual whether Portugal was ever a corporative system. For me, at the moment, it is sufficient to accept *prima facie* the once consensual view that this ideal was present in the 1933 Constitution and its following reviews.

[28] See Tiago Saraiva, "Laboratories and Landscapes", Ibid. and "Fascist Labscapes", Ibid.

[29] See Manuel Lucena, *A evolução do sistema corporativo português*, 2 vol. (Lisboa: Perspectivas & Realidades, 1976).

fascism. What was the role of JNPP? How did Serra begin to collaborate with it? How did this collaboration contribute to the national wool production? How did it locally work and how was scientific work organised? How did it influence Serra's scientific practices and ideas? Here are some of the questions guiding this thesis' research.

Under the New State's corporative ideal, the National Board of Husbandry was conceived as an "organism of economic coordination".[30] With the specific purpose of increasing Portuguese exportation, the fascist regime created special organisms whose mission was to "develop, improve and coordinate" production activities.[31] They included regulatory bodies [comissões reguladoras], institutes and national boards. One may well ask: if Salazar really wanted an economy based on professional corporations (a corporative state), why did he create these type of organisms which presided over the coordination of economic activities and hence interfered in the life of corporations? One may venture to guess that Portuguese society was felt not to be ready for pure corporatism since corporations were not believed to be consolidated. Hence, the first step was for the State to assure this consolidation through these "pre-corporative" organisms,[32] like the national boards, and only afterwards should corporations and market economy take control. This Portuguese version of corporatism has been ironically compared to the mismatch in communist ideology and communist practice of the role assigned to the State.[33] Following the footsteps of Tiago Saraiva, I will demonstrate that it was at the forefront of these "pre-corporative" organisms, meant to transform Portuguese society in a corporative ideal, that scientists were thought to play a crucial role.

[30] This is a juridical designation used from 1936, DL 26.757, 8 July 1936.

[31] Manuel Lucena, *A evolução do sistema corporativo português*, Ibid. p. 307

[32] Manuel Lucena, *A evolução do sistema corporativo português*, Ibid. p. 305

[33] Manuel Lucena, *A evolução do sistema corporativo português*, Ibid.

In the very same year in which these organisms were given a legal standing, in 1936, the National Board of Fruits [Junta Nacional das Frutas] was created, followed in the next year by the National Board of Olive Oil [Junta Nacional do Azeite] and the National Board of Wine [Junta Nacional do Vinho] – in 1939, the National Board of Husbandry was set up.[34] All shared a similar organic structure, although JNPP was more complex since it was divided into three sections – meat, wool and milk –, each of these being endowed with a technical council [conselho técnico].

5.3 Portuguese herds: "confusion" and variability

On 23 January 1946 at the Portuguese Society of Veterinarian Medicine, Mário Coelho Morais presented to an audience of veterinarians, zootechnicians, producers, businessmen and industrialists of this economic sector what he considered to be one of the main reasons behind the "Portuguese wool problem" [Problema Lanar Português]:[35] How to improve the economic wool cycle if among producers, businessmen and industrialists there was not a common terminology for the types and classes of different national wools?[36] Presenting a proposal for a system of classification able to end with such "extraordinary confusion" was therefore the main purpose of this event[37] – in his view, system classification "should be subordinated to industrial criteria." [38]

[34] Manuel de Lucena, "Sobre a evolução dos organismos de coordenação económica ligados à lavoura (I)", *Análise Social*, 56 (1978), p. 855.

[35] Mário C. Morais, "Classificação das Lãs Nacionais", Portuguese Society of Veterinary Medicine, Lisbon, 23 January 1946.

[36] "[…] Tive a necessidade de me aperceber da forma como, em Portugal, eram classificadas as lãs nacionais por produtores, comerciantes e industriais, depressa pude perceber que imperava uma apreciável confusão e, quantas vezes, completo antagonismo entre os tipos e classes de lãs a que entidades diferentes e em diversas regiões davam a mesma designação.", Mário C. Morais, "Classificação das Lãs Nacionais", Ibid. p. 20.

[37] Mário C. Morais, "Classificação das Lãs Nacionais", Ibid. p. 3.

But Morais had another agenda: to subtly introduce his audience to a much bigger plan concerning Portuguese wool and the sheep responsible for its production – a plan to unveil the technological and biological causes behind "national wool defects".[39] Such plan became soon a reality for in this very same year the Plan of Genetics and Laboratorial Studies of the Sheep and Wools of Portugal was launched under his supervision.[40] The constitution of a JNPP technical council specialized in wool production[41] reveals how important this economic sector was to the ideologues of the New State in the beginning of the 1930s. But as we can see, it was only in the post-war period that theory came into practice.

The confusion did not only concern wool terminological aspects. The lack of knowledge about the national races existing in Portugal was profound and the proliferation of different names in different regions was immense. Apparently, selection had always been in the hands of shepherds and no sheep reproduction plan had ever been introduced in Portugal at a national level.[42] Portuguese sheep herds had never been studied from a scientific point of view. How could genetics be of any use in this state of things? The paradoxical answer is that this was not necessarily bad for genetics – and in fact it could be far better.

In countries with a long-standing scientific approach to husbandry – such as England, Germany or Holland – national races were well known and societies had been created in order to protect each "pure race" (in the commercial sense). Since the late

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[38] Mário C. Morais, "Classificação das Lãs Nacionais", Ibid. p. 22.

[39] Mário C. Morais, "Classificação das Lãs Nacionais", Ibid. p. 33.

[40] J. C. Antunes-Correia, "A contribuição fundamental do Professor José Antunes Serra na Genética do melhoramento dos ovinos", *Brotéria Genética*, XIII (1992), p. 16. (in Portuguese, *Plano de Estudos Genéticos e de Laboratório sobre a Ovelha e as Lãs de Portugal*).

[41] The name of this JNPP section was Production and Trade of Wool [Produção e Comércio de Lãs].
[42] Interview with Rolanda M. Albuquerque, September 2009, Lisbon.

eighteenth century, livestock had been the object of inbreeding methodology and selection on the basis of pedigree. The goal was "purity", not in the scientific sense of "pure line" (i.e. an individual homozigotic in respect to a certain character), but in the breeder's sense of "constancy of a distinct commercial brand".[43] These inbreeding procedures, however, brought some surprising results, – unforeseeable diseases such as bovine tuberculosis in cows of the extreme dairy type[44] – and so in the twentieth century they began to be criticized. The complexity of these "field experimental systems" ruled by economic criteria made it especially difficult for geneticists to collaborate with animal breeders.[45]

It was only around the 1940s that both geneticists and breeders agreed that traditional trend of purity was an unsuitable methodology.[46] And though geneticists could not provide livestock breeders with the factorial constitution of their species, they could certainly help them to improve selection methods, for example, determining less obvious cases of dominant/recessive characteristics and, most importantly, contributing to a better understanding of the interaction between multiple hereditary traces. However in Portugal this "purity" trend in husbandry had never occurred. And from a genetics point of view, it became an advantage: cattle in Portuguese farms had more variability

[43] Bert Theunissen, "Breeding Without Mendelism: Theory and Practice of Dairy Cattle Breeding in the Netherlands 1900–1950", *Journal of the History of Biology*, 41 (2008), p. 656.

[44] Bert Theunissen, "Breeding Without Mendelism", Ibid. p 637.

[45] Some reasons behind this complexity are common to other domestic mammals: long periods of time until animals achieve sexual maturity (and during gestation), reduced number of offspring individuals, and difficulty in tracking down mutants along the generations. But in livestock case, this last problem is aggravated. Contrary to the selection of mice, rabbit, cat, dog and horse (where scientific or aesthetic reasons can prevail), sheep selection is normally guide solely by economic reasons, and since the majority of mutations give generally rise to individuals with low viability, mutants are generally eliminated, José A. Serra, "Genétique du mouton", Ibid. pp. 9-10. Mutations behave many times as a recessive gene and that is why they are important to genetic to the genetic knowledge of species.

[46] Bert Theunissen, "Breeding Without Mendelism", Ibid. p 637.

than cattle in countries where agriculture had been traditionally considered a national priority from the economic point of view. For once, periphery was seen with good eyes.

> Not having gone through the stage of pure race and concomitant societies, should not hinder our progress in husbandry, for we may move faster than breeders who have used empiric methods, if one applies the good methods developed at present, and one is endowed with sufficient means.[47]

This was the context of Portuguese husbandry when Serra started to collaborate with JNPP in the Plan of Genetics and Laboratorial Studies of the Sheep and Wools of Portugal, in 1946.[48] JNPP began by looking abroad for expertise on genetics; however, the international community referred back to Serra as the best choice.[49]

> I heard one of my early collaborators stating that it all started when one of the persons in charge of the old JNPP sought abroad for an expert in the area, to participate in the plan of improving the standards of national wools, which the aforesaid institution wanted to launch once the war ended. After consultations, they were informed there was no need to look abroad, since there was in

[47] "O facto de não termos atravessado a época histórica da raça pura e respectivas sociedades de registo não deve impedir o progresso pecuário, pois poderemos, se aplicarmos os bons métodos actuais e com meios suficientes, andar o caminho histórico muito mais rapidamente que o fizeram os ganadeiros pelos seus métodos empíricos.", José A. Serra, *Os Caminhos da Melhoria Pecuária* (Lisboa, Junta Nacional dos Produtos Pecuários, 1958), p. 211.

[48] Rolanda M. Albuquerque, "Professor José Antunes Serra", Ibid. p. 25.

[49] According to Albuquerque, Morais went "to Germany (or Poland?)" to understand how he could solve "the problem of the white wool" in Portugal and it was Nachtscheim who indicated Serra, Interview with Rolanda M. Albuquerque, September 2009, Lisbon.

Portugal a renowned scientist [Serra], with an acknowledged record in that area of expertise and ideal for the job.[50]

5.4 Serra out on the field and inside the laboratory

JNPP was interested in the study of all factors of sheep production. But, as I explained above, wool was the priority. Portuguese wool had four main biological "defects": short fibre, heterogeneity of *velo* (a part of the wool), coarseness of touch and high percentage of "canary".[51] JNPP needed the scientific elite to investigate the rightly breeding methodology to be applied in order to reduce these defects. Moreover, they wanted to know if some of these defects could be eliminated through chemical reactions in the laboratory.

Another problem was wool colour. Portuguese sheep of the *Merino* race (one of the most popular races in Portugal) could be white or coloured, that is, pigmented. Among the later, several colours were found in the fields: black, dark brown, light brown, red or beige [camurça].[52] White wool was more expensive than coloured wool, a commercial situation which led sheep breeders to select white individuals.[53] Could genetics contribute to the "purity" of white in Portuguese sheep? Was this a good

[50] "Ouvi um dos meus primeiros colaboradores dizer que tudo terá começado quando um dos responsáveis pela antiga JNPP procurou no estrangeiro um cientista especializado nessa matéria, para colaborar no plano de melhoramento de lãs nacionais, que aquela instituição queria iniciar a seguir à 2ª Grande Guerra. Consultada essa entidade, terá sido por ela dito, que não valia a pena fazer tal procura no estrangeiro, quando era certo haver em Portugal um cientista de renome, com obra feita no sector e que poderia, melhor que ninguém, executar tal tarefa.", J. C. Antunes-Correia, "A contribuição fundamental do Professor", Ibid. p. 15-16.

[51] José A. Serra, *Os Caminhos da Melhoria Pecuária* (Lisboa, Junta Nacional dos Produtos Pecuários, 1958), p. 218.

[52] José A. Serra, "Aplicações da genética no melhoramento de ovinos. I Parte: Caracteres pigmentares", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 1 (1949), p. 10.

[53] José A. Serra, "Aplicações da genética no melhoramento de ovinos", Ibid. p. 26.

selective strategy in terms of the global productivity of sheep? As I explained in the previous chapter, Serra had specialized on heredity of pigmentation – a specialization that fitted like hand and glove the challenge Portuguese herds were facing.

Therefore, Serra became responsible for the physiological and biochemical approach to problems of sheep wool. His work took place at the Zoological Laboratory. During his stay in Coimbra, he worked on two ongoing projects: the removal of melanins (how to turn pigmented wool into white)[54] and the nature of "canary", a yellowish coloration in white wools only detectable after industrial washing and, as I mentioned before, a factor responsible for the devaluation of Portuguese wool.[55] Helped by his student Rolanda Albuquerque, Serra found that in the majority of cases "canary" was a chemical reaction involving wool keratine, going against authors who argued that it was caused by internal or bacterial pigments.[56]

But his collaboration was not restricted to the laboratory. Serra actively participated in the animal breeding experiments conducted in the Alter Stud and supervised by Morais and Ramos da Costa. Several Portuguese races were inspected such as *Merino*, *Churra Algarvia*, *Churra Lapeira* or *Jardo.*[57] He also participated in the First Sheep raising Study-Enquiry [I Estudo-Inquérito Ovinícola][58], which aimed to overcome the lack of scientific knowledge about Portuguese races.

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[54] José A. Serra, "Ensaios de remoção das melaninas das lãs pigmentadas", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 4 (1953).

[55] José A. Serra and Rolanda M. Albuquerque, "Nature of the canary colouration of wool", *Journal of the Textile Institute*, 7/8 (1951), 329-331; and "Natureza e causas da cor do 'canário' em lãs brancas. II. pH, alcalinidade e sugo", *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 5 (1954), 1-53.

[56] José A. Serra and Rolanda M. Albuquerque, "Natureza e causas da cor do "canário", Ibid. p. 44.

[57] The "jardo" breed was of particular interest to JNPP due to the similarities of his wool to Karacul.

[58] José A. Serra, "Instruções para o Estudo-Inquérito Ovinícola", *Publicações da Junta Nacional dos Produtos Pecuários* (1956).

Alter Stud breeding experiments aimed to isolate some hereditary characters using a pure line selection strategy ("pure line" in genetics terms). As I said before, white colour was more expensive for it served better the textile industry during the wool dyeing process. But at the same time, shepherds and sheep producers argued that pigmented wool sheep produced more milk.[59] Was it so? The problem was that what shepherds and sheep producers considered white, geneticists did not. To produce homozygote individuals was a long time run and to transform Portuguese herds into an "experimental system" was an impossible task.[60] Still, during the first years of his collaboration, some discoveries were made regarding the heredity of wool pigmentation, for instance, the demonstration that in Portuguese races of sheep, whose white wool was always dominant over pigmented, there were also cases of black dominant.[61]

5.5 Co-working with veterinarians

Three different cultures were involved in the Plan of Genetics and Laboratorial Studies of the Sheep and Wools of Portugal: geneticists (Serra and his co-workers) had the scientific responsibility for the physiological work conducted at the Zoological Laboratory and for the design of breeding experiments; shepherds and sheep producers, offered their herds to be subjected to crossings conducted at the Alter Stud and contributed with their empirical knowledge; and the employees of the JNPP section of Production and Wool Trade, mainly veterinarians, were involved in the organization

[59] José A. Serra, "Aplicações da genética no melhoramento de ovinos", Ibid. p. 31.

[60] According to Rolanda M. Albuquerque, in 1974 Serra and his co-workers of JNPP had achieved to breed a totally white herd – but after the revolution of 25 April, two decades of selective work were destroyed.

[61] This discovery was made in collaboration with Guilherme Pereira and the crossing experiments took place in the National Zootechnical Station, José A. Serra, "Aplicações da genética no melhoramento de ovinos", Ibid. p. 58.

and scientific production of all research lines, and ultimately had the final responsibility for the Plan results.

Veterinarians acted therefore as intermediaries between national wool policy and animal genetics. Serra did not consider them as peers. He once dubbed them as mere "technicians".[62] But this does not mean that their work was reduced to the technicalities and organization of this enterprise. In fact, they were also involved in scientific activities, including both breeding and laboratory experiments. With respect to breeding, as I mentioned before, they assured the supervision of crossings. And regarding laboratory work, they had their own laboratory[63] and participated in Serra's work at the university. In a letter to Cuenca, a Spanish veterinarian, Serra makes following comment about Morais:

> I still haven't seen our friend Morais, and therefore I still do not know what your arrangement was. Dr. Morais is more and more absorbed with the Board' bureaucracy and it is difficult to make him turn from those matters to cast an eye on the microscope or to observe the sheep pigments and similar things. I can only hope it does not take much longer for him to surface.[64]

[62] As he once wrote to Roberts Rugh, professor of New York University, applied genetics on cattle was "a kind of work which a professor of pure genetics cannot avoid when the 'technicians' require cooperation with the more academically minded members of the staff of the old pure science Universities.", Letter from Serra to Roberts Rugh, 16 November 1947, SP-DZAUC.

[63] According to Serra, some laboratorial work was conducted in JNPP, José A. Serra, "Aplicações da genética no melhoramento de ovinos", Ibid. p. 31.

[64] "Ainda não vi nosso amigo Morais, pelo que não sei de nada a respeito do que combinaram. O Dr. Morais anda cada vez mais absorvido com a burocracia da Junta, pelo que é difícil arrancá-lo um pedaço para vir dar uma olhadela pelo microscópio ou observar os pigmentos das ovelhas e outras coisas que jandas. Espero que ele não demore muito a dar acordo de si.", Letter from Serra to Carlos L. Cuenca (Faculdade de Veterinária, Universidade de Madrid), 4 July 1949, SP-DZAUC. This letter concerned the translation of "La Genetic du Mouton" and Serra's textbook on genetics.

Further investigation is needed in order to understand the extent of this collaboration in terms of the exchange of experimental modes of practice between the "professor of pure genetics" and the "technicians."[65] Furthermore we need to understand how exactly the latter contributed to the construction of scientific knowledge. A cultural history of the emergence of this biological community is also required.

After several years passed in the Alter Stud and inside the laboratory, some results turned into scientific discoveries. Such was the case of the dominant black or the nature of "canary". But does this sum up Serra's contribution to JNPP? I do not think so. One of his first contributions to JNPP was a review paper, a state of the art account on sheep genetics, "Genétique du mouton. Mise au point critique"[66], published in 1948. Such bibliographic review was not an easy task since the topic was "very badly known" worldwide.[67] A sharp capacity of "struggling for synthesis" was required – indeed his speciality (see chapter four). This work was published by the JNPP and it was the first paper of a prolific publishing activity. There were two kinds of publications: "A" series, a "popularization and practical applications series"; and "B" series, a "scientific and research series". What is intriguing is that during the first decade these two series were filled almost exclusively with Serra's articles, some more practical[68], others more

[65] See note 62.

[66] José A. Serra, "Genétique du mouton. Mise au point critique", *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 1 (1948). Two years after, "La Genétique du Mouton" was translated into Spanish: *Genética* A *Ovina* (Madrid: Sociedad Veterinaria de Zootecnia, 1950).

[67] José A. Serra, "Genétique du mouton", Ibid. p. 11. According to Serra, the sole attempts to deal with sheep genetics had been done by Roberts and Crew (1925) and Vasin (1928). Vasin almost exclusively dedicated himself to pigmentation. He also points out two surveys in the general books of Castle (1940) and Krallinger (1944) – to which he had had access through Douglas Laurie of the Imperial Bureau of Animal Breeding and Genetics (Edimburg) and M. L. Fraser.

[68] José A. Serra, "Aplicações da genética no melhoramento de ovinos. I Parte: Caracteres pigmentares", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 1 (1949); "Aplicações da genética no melhoramento de ovinos. II Parte: Caracteres

theoretical.[69] Common to all publications (especially series A) is Serra's effort to "translate" – in a Peter Galison's sense[70] – the latest scientific discoveries on genetics into an easier language and to put in context the results of Alter Stud breeding experiments. It is my contention that in doing so Serra was (purposefully or not) giving theoretical training on genetics to JNPP employees – the veterinarians.

I do not mean to say, though, that the JNPP publishing activity was a preconceived act aimed at the training of veterinarians. Indeed I tend to see things the other way around. Maybe this was the result of Serra's manoeuvre to "coordinate" with veterinarians his "actions and beliefs", again in Peter Galison's sense. To secure a high ratio between "work time" and "publications" was, as it is today, a cultural belief among scientists. In exchange for his scientific input, JNPP provided Serra with a publishing opportunity. And, as a side effect, a colossal synthetic work on the genetics of sheep and other cattle began taking place. Further investigation is required to corroborate this hypothesis.

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morfológicos. Cromossomas e Hibridação", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 2 (1949); "Aplicações da genética no melhoramento de ovinos. III Parte: Caracteres Fisiológicos e Caracteres de Produção", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 3 (1952); "Ensaios de remoção das melaninas das lãs pigmentadas", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 4 (1953). In 1949, he published together with Mário C. Morais, "Escala de cores das lãs nacionais", Congres. Internac. Allevamento Ovino dei Paesi del Bacino Mediterraneo, Roma, 1949. This was an old project entertained by Morais, who on the 23 January 1946 presented at the Portuguese Society of the Veterinary Medicine (Lisbon) in a conference entitled "Classificação das Lãs Nacionais".

[69] José A. Serra, "Short-cut pratical methods for mounting preparations in balsam, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 2 (1949); (with G. Pereira), "Inheritance of dominant pigmentation in fine wool Portuguese sheep", *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 3 (1950); (with M. C. Morais and E. S. Ramos da Costa), "Hereditariedade do interpolado tipo jardo no carneiro. I, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 4 (1953); (with Rolanda M. Albuquerque), "Natureza e causas da cor do "canário" em lãs brancas. II. pH, alcalinidade e sugo, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 5 (1954). During Coimbra's period, the only publication in an international scientific journal concerning his collaboration with JNPP was "Nature of the canary colouration of wool", *Journal of the Textile Institute*, 7/8 (1951), an article authored together with Rolanda M. Albuquerque.

[70] Peter Galison, "Trading Zone – Coordinating Action and Belief", in Mario Biagioli, ed., *The Science Studies Reader* (New York/London: Routledge, 1999), pp.137-160.

Another feature emerging from the interaction between applied and basic science concerns the entrance of a new material culture into Serra's laboratories.[71] According to Albuquerque, "a lot of material"[72] had entered the Zoological Laboratory since the beginning of Serra's collaboration with JNPP. Scientific material was ordered in Serra's name, and sent to his laboratory "on behalf" of JNPP.[73] This permeability between his duties as professor of a Portuguese university and as a collaborator-researcher of JNPP was criticized by Ricardo Jorge when Serra moved to Lisbon to succeed him as director of the National Museum of Natural History. Serra was accused of trying to occupy the museum installations with "a train load of apparatuses he brought with him from Coimbra, given by the Husbandry Group." Ricardo Jorge, an old enemy of Serra, complained:

> Almost as soon as I retired, wool research began to be conducted in several rooms, transformed into a sort of "branch" of the Husbandry Group. This went on up until 1960 with dividends – or so it seems – vastly superior to the meagre salary of 560 escudos earned by the present Director who often complains bitterly about it […].[74]

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[71] Here I have in mind not only the laboratory in Coimbra (the Zoological Laboratory) but also the one in Lisbon (after 1952).

[72] Interview with Rolanda M. Albuquerque, September 2009, Lisbon.

[73] For example, when Serra received the microfilms sent to him from the Imperial Bureau of Animal Breeding, he asked M. L. Fraser to ship the bill and receipt to "Prof. J. A. Serra – Museu Zoológico. Universidade de Coimbra (Portugal) – Para Estudos sobre lãs pigmentadas nacionais, por conta da Junta Nacional dos Produtos Pecuários"; M. L. Fraser was the scientific assistant of the Imperial Bureau of Animal Breeding, Edinburgn, of which C. H. Waddington was consultant director; Letter from Serra to M. L. Fraser, 7 January of 1948, file "bibliography about pigments", SP-DZAUC.

[74] "Não tardou, porém, logo após a minha jubilação, que, para o estudo das lãs, passassem a aproveitar-se outras dependências, transformadas numa espécie de 'sucursal' dos Serviços Pecuários, cujo pleno funcionamento se prolongou até 1961, com proveitos – ao que parece – incomparavelmente superiores à magra gratificação de 560 escudos de que tanto se chora o actual Director […]", Arthur Ricardo Jorge, *Um Homem Nefasto* (Lisboa, 1963), p. 8.

5.6 Conclusion

This chapter aims to be part of a recent trend in historiography of science to rethink the relation between science and politics during the fascist regime in Portugal, essentially viewed in the historiography of Portugal from the point of view of political persecutions. In the history of genetics in Portugal, this trend began with the recent work of Tiago Saraiva who has focused on the crucial role of geneticists in the construction of the New State. In this chapter I analyzed not only how Serra contributed to the construction of the New State but also how the political agenda for husbandry influenced his scientific practices and ideas. Despite many questions which remain still to be answered, some conclusions can be drawn.

The first conclusion is that the National Board of Husbandry was the platform of the fascist regime created to function as an intermediate between the knowledge on animal genetics, demonstrated by Serra, and Portuguese economy regarding sheep and wool production. As Tiago Saraiva advocates, based on Quintanilha's work on the National Board of Cotton, these "pre-corporative organisms" designed by the New State to foster the transition from a non-corporative to a corporative system, have played a crucial role in the relation between science and politics during the fascist regime in Portugal. Hence, as the author argues, they should be the subject of extended historical research if one wants to study the relation between science and politics during the fascist regime.

The second conclusion concerns the way the New State invested in animal genetics. No "revolutionary" scientific institution especially devoted to applied animal genetics was created contrary to what happened in research on plant genetics with the

foundation of the National Agronomic Station.[75] The practical demands of cattle breeding and the methodological difficulties of geneticists regarding husbandry are some of the reasons behind this state of affairs. Still, in the mid 1940s, following a new international trend in husbandry, the New State took genetics as a crucial step to improve the wool cycle production. In 1946 the Plan of Genetics and Laboratorial Studies of the Sheep and Wools of Portugal was launched. And it counted on Serra's scientific expertise. Serra, at that time the leading Portuguese geneticist internationally renowned, was not invited to become the director of a New State iconic institution, as it happened with Câmara in Portugal or with Quintanilha in the colonies. Instead, he received the New State *inside* his university. In the JNPP context, his collaboration was remembered as the "striking instance of the fertility of the link University-Corporation."[76] In the history of Portuguese genetics, this was a new way of interaction between the New State and scientific elites that was being tested.

The third conclusion focuses on Serra's contribution to the construction of the New State. One of the main priorities of the fascist agenda on husbandry was the wool production cycle. Portuguese wool showed "defects" from the economical point of view and JNPP wanted to overcome these defects both in the field and in the laboratory. Serra's contribution was therefore of two kinds: on the one hand, he offered scientific supervision in the design of sheep breeding experiments aimed at reducing these defects through a proper selective methodology; and on the other hand, he investigated in the laboratory ways to eliminate these wool defects through chemical reactions. Breeding experiments aimed also to tackle another problem: the heredity of white wool, more

[75] A closer look at the scientific production of National Zootechnic Station is yet required.

[76] […] terá sido deste modo que se estabeleceu uma relação entre Professor Serra e JNPP, a qual foi um exemplo bem claro de quanto pode ser fecunda a ligação Universidade-Empresa.", J. C. Antunes-Correia, A contribuição fundamental do Professor José Antunes Serra na Genética do melhoramento dos ovinos, *Brotéria Genética*, XIII (1992), p. 15-16.

lucrative than the pigmented one. With passing years, some results turned into discoveries. But I do not think Serra's contribution to the New State's political agenda on husbandry should be reduced to such scientific achievements. This point leads to the fourth conclusion of this chapter.

In my view one of Serra's crucial contributions was to give theoretical training (mainly on genetics, but not just on it) to the "biological community" that had the power of action inside this "pre-corporative" organism of the New State: the veterinarians. In JNPP publications (especially series A) Serra "translated" complicated genetics concepts and the latest discoveries concerning sheep genetics, at the same time that he put in context Alter Stud experiments. As it had happened with Portuguese anthropology and zoology (chapter four), Serra was again a vehicle of circulation of genetic knowledge – and, as it had happened with anthropology, he acted as a privileged communication channel towards a biological community closely connected to the policies of the fascist regime.

Finally, the model of interaction between JNPP and animal genetics seems to have been really advantageous to Serra. It is my contention that Serra favoured work in a "university-corporation" model by opposition to work within an "institution for applied scientific work" where, as he stated, there was "always (I know of no exception) a peculiar character proper to authoritarian systems."[77] In a way, such a hybrid model of collaboration with the fascist regime fitted better his self-image of an "apolitical" scientist. I suspect that, furthermore, such a scheme gave him the space and freedom to improvise and appropriate JNPP's project material and new experimental practices in his "basic science" research avenues – I plan to investigate this hypothesis in my PhD thesis.

[77] Letter from Serra to Hans Lettré, 6 February 1959, SP-DZAUC

FINAL REMARKS

This thesis addressed the emergence of genetics in Portugal and the role played by Serra in this process. In what follows I will briefly sum up its main conclusions

1) As many areas of Portuguese culture, the emergence of experimental biology in Portugal in the beginning of the twentieth century was influenced by the French tradition, a factor which accounts for the initial non-appropriation of Mendelism. However, I disagree with the statement often advocated by biologists according to which Portuguese experimental biologists were "passive" in relation to France. Regarding their scientific interests, France was a "centre" on its own. In fact, in the beginning of the century experimental biologists were restricted to the biomedical community and within this community problems in biology were centered around histology, physiology and embryology, exactly like in France – likewise, they were looking to the "proper" place for guidance. To fully understand the "delayed" emergence of Portuguese genetics it is therefore imperative to understand why experimental modes of practice were not at first appropriated by botanists and zoologists.

2) Portuguese biology began to move towards genetics during the late 1920s. Through Quintanilha, experimental modes of practice migrated from the biomedical community (Faculty of Medicine in Lisbon) to the botanic community (Faculty of Science of Coimbra), and this migration is associated with an epistemological shift involving the chromosome concept. The conditions for the appropriation of Mendelism were created, and a new community of

3) The appropriation of genetics within the anthropological community highly benefited from the political context of colonialism aiming at the implementation of eugenic programs in the Portuguese colonies. Both Tamagnini, the head of the anthropology school, and Serra, his student, used eugenic policies to pursue their scientific research on heredity. But Serra went further: he used it to emancipate zoology in relation to anthropology, a discipline which held a hegemonic position in the department of anthropology and zoology since the beginning of the century. With Serra, "rabbits" were sold as a resource to the political regime – they were crucial to the study of racial classification.

4) Genetics research in the anthropological community of Coimbra during the 1930s and 1940s was strongly influenced by events taking place at KWIA. Following KWIA latest trends in anthropology (phenogenetics), Serra introduced experimental zoology in the department through his work on pigmentation. And it were the geneticists from this institute who provided the material necessary (rabbits hair) for his research. Pigmentation is the key to understand Serra's broad scientific production in Coimbra: pigmentation forced him away from biometry and into biochemistry and through the study of the

5) Serra contributed to the construction of the New State in the context of the main priorities of the fascist agenda on husbandry – the wool production cycle. Serra's contribution was of two kinds: first, to scientifically supervise the design of sheep breeding experiments, which took place at the Alter Stud, and which aimed at the reduction of defects through a proper selective methodology; and second, to investigate ways to eliminate wool defects through chemical reactions performed in the laboratory. Breeding experiments aimed also to tackle another problem: the heredity of white wool, more lucrative than the pigmented one. While I do not disregard the actual importance of Serra's experimental work, I argue that his crucial contribution was to give theoretical training on genetics to veterinarians, members of a "biological community" infiltrated in this "pre-corporative" organism of the New State. As it had happened with Portuguese anthropology and zoology, Serra was again a vehicle of circulation of genetics knowledge – and, as it had happened with anthropology, he acted as a privileged communication channel towards a biological community closely connected to the policies of the fascist regime.

6) The way the New State invested in animal genetics was very different to what had happened in research on plant genetics, at least in sheep genetics, the topic addressed in this thesis. Serra, at that time the leading internationally renowned Portuguese geneticist, was not invited to become the director of a New State iconic institution dedicated to applied science, as it happened with Câmara in Portugal or with Quintanilha in the colonies. Instead, he received the New State *inside* his university. In the history of Portuguese genetics, a new way of interaction between the New State and scientific elites (a "university-corporation" model) was being tested. In this thesis I suggest that this hybrid model of collaboration with the fascist regime fitted better Serra's self-image of an "apolitical" scientist, giving him at the same time the space and freedom to improvise and appropriate JNPP's project material in his "basic science" research avenues.

ARCHIVES AND BIBLIOGRAFIC REFERENCES

Archives

Arquivo Nacional da Torre do Tombo (ANTT)
Processo MUD Juvenil, nº 50-46, box 2513, PIDE-DGS

Departamento de Zoologia e Antropologia da Universidade de Coimbra (DZAUC)
Personal Papers of José Antunes Serra (SP-DZAUC)

Instituto Camões (IAC)
José Antunes Serra's file (SF-IAC)

Universidade de Coimbra (UC)
José Antunes Serra's file, box 301 (SF-UC)

J.A. Serra's publications

Serra, J. A., *Estudos de pigmentação melânica. A determinação da pigmentação e escurecimento com a idade. Composição das melaninas,* PhD thesis presented to the Faculty of Sciences of the University of Coimbra, 1939.

Serra, J. A., "Novos métodos de estudo da pigmentação e sua importância racial", in *Congresso Nacional de Ciências da População, Comemorações Portuguesas de 1940,* Porto (1940).

Serra, J. A., "An attempt at a synthesis of the concept of the gene", *Boletim da Sociedade Broteriana,* 19 (1944).

Serra, J. A. and A. Queirós Lopes, “Diekter Nachweis und Lokalisation von basischen Protein in dem Chromosomen und im Nukleolus”, *Chromosoma* (Berlin), 2 (1944), 576-595.

Serra, J. A. and A. Queirós Lopes, “Comportamento do nucléolo durante a ovulação em Moluscos”, *Boletim Sociedade Broteriana* (Coimbra), 19 (1944), 295-309.

Serra J. A. and A. Fernandes, “Sobre a Origem das substâncias constitutivas do nucléolo”, *Revista da Faculdade de Ciências da Universidade de Coimbra*, 14 (1944), 109-122.

Serra, J. A. and A. Queirós Lopes, “Données pour une citophysiologie du nucléolo. I – L’activité nucléolaire pendant la croissance de l’oocyte chez des Helicidae”, *Portugaliae Acta Biologica*, 1 (1944), 51-94.

Serra, J. A., “Fenogénese e constituição das melaninas de mamíferos”, *Boletim Escola Farmácia de Coimbra*, 4 (1945).

Serra, J. A. and A. Queirós Lopes, “Chemical Constitution of the Nucleolar Inclusions in Growing Oocytes Cells, *Nature*, 155 (1945).

Serra, J. A., “Mitose e Meiose. Dados e interpretações sobre a fisiologia da mitose e da meiose” in *Actas I Reunião Citológica* (Lisboa, 1945), vol. I, 47-96.

Serra, J. A., “Histochemical tests for proteins and amino acids. The characterization of basic proteins”, *Stain Technology*, 21 (1946), 5-18.

Serra, J. A., “Constitution of hair melanins”, *Nature*, 157 (1946), 771-772.

Serra, J. A., “Contributions to a physiological interpretation of mitosis and meiosis. I- The composition of the resting stage nucleus”, *Portugaliae Acta Biologica*, 2 (1947), 25-44.

Serra, J. A., “Contributions to a physiological interpretation of mitosis and meiosis. II- The profase appearing of the cromonemata and spiralization”, *Portugaliae Acta Biologica* (1947), 45-90.

Serra, J. A., “Composition of chromonemata and matrix and the role of nucleoproteins in mitosis and meiosis” in *Cold Spring Harbor Symposium of Quantitative Biology*, 12 (1947), 192-210.

Serra, J. A., “Genétique du mouton. Mise au point critique”, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 1 (1948).

Serra, J. A., “Short-cut pratical methods for mounting preparations in balsam, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 2 (1949).

Serra, J. A., “Aplicações da genética no melhoramento de ovinos. I Parte: Caracteres pigmentares”, *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 1 (1949).

Serra, J. A., “Aplicações da genética no melhoramento de ovinos. II Parte: Caracteres morfológicos. Cromossomas e Hibridação”, *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 2 (1949).

Serra, J. A., “Aplicações da genética no melhoramento de ovinos. II Parte: Caracteres morfológicos. Cromossomas e Hibridação”, *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 2 (1949).

Serra, J. A., *Moderna Genética. Geral e Fisiológica* (Coimbra: Author’s edition: 1949).

Serra, J. A. and M. C. Morais, “Escala de cores das lãs nacionais”, Congres. Internac. Allevamento Ovino dei Paesi del Bacino Mediterraneo, Roma, 1949.

Serra, J. A. and G. Pereira, Inheritance of dominant pigmentation in fine wool Portuguese sheep, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 3 (1950).

Serra, J. A., *Genética Ovina* (translation to Spanish of "Genetique du Mouton") (Madrid: Sociedad Veterinaria de Zootecnia, 1950).

Serra, J. A. and R. M. Albuquerque, "Nature of the canary colouration of wool", *Journal of the Textile Institute*, 7/8 (1951), 329-331.

Serra, J. A., "Aplicações da genética no melhoramento de ovinos. III Parte: Caracteres Fisiológicos e Caracteres de Produção", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 3 (1952).

Serra, J. A., "Ensaios de remoção das melaninas das lãs pigmentadas", *Publicações da Junta Nacional dos Produtos Pecuários. Série B: Série de Divulgação e de Aplicações Práticas*, 4 (1953).

Serra, J. A., M. C. Morais and E. S. Ramos da Costa, "Hereditariedade do interpolado tipo jardo no carneiro. I, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 4 (1953).

Serra, J. A. and R. M. Albuquerque, "Natureza e causas da cor do "canário" em lãs brancas. II. pH, alcalinidade e sugo, *Publicações da Junta Nacional dos Produtos Pecuários. Série A: Série Científica e de Investigação*, 5 (1954).

Serra, J. A., "Contribuições Portuguesas para o Progresso da Genética. Tentativa de menção cronológica sistematizada", *Brotéria Genética*, 8 (1987), 17-34.

Serra, J. A., *Os Caminhos da Melhoria Pecuária* (Lisboa: Junta Nacional dos Produtos Pecuários. Serviços de Lã e Comércio de Lãs, 1958).

Bibliographic references

Ainsworth, G. C., *Introduction to the History of Mycology* (Cambridge: Cambridge University Press, 1976) (digitally print version 2009).

Albuquerque, R. M., "Professor José Antunes Serra", *Brotéria Genética*, 12 (1991), 5-44.

Almaça, C., "As Primeiras Fases da Obra Científica do Prof. J. A. Serra", *Brotéria Genética*, 13 (1992), 33-40.

Almaça, C., *Bosquejo Histórico da Zoologia em Portugal* (Lisboa: Museu Nacional de História Nacional, 1993).

Almaça, C., *O darwinismo e a Universidade Portuguesa (1865-1890)* (Lisboa: Museu Nacional de História Natural, 1999).

Almaça, C., *Museu Bocage. Ensino e Exibição* (Lisboa: Museu Nacional de História Natural, 2000)

Almaça, C., "A Sociedade Portuguesa de Ciências Naturais, a Marinha e a Estação de Biologia Marinha", VIII Simposium "O Mar no Futuro de Portugal", Lisboa, 2003.

Amaral, I., *A emergência da bioquímica em Portugal: As escolas de investigação de Marck Athias e Kurt Jacobsohn* (Porto: Fundação Calouste Gulbenkian e Fundação para a Ciência e a Tecnologia, 2006).

Almeida, J. V., "A 'Revista Agronomica' e a agricultura", Separata *Revista Agronómica* 1 (1903), 1-4.

Antunes-Correia, J. C., A contribuição fundamental do Professor José Antunes Serra na Genética do melhoramento dos ovinos, *Brotéria Genética*, XIII (1992).

Araújo, A. C., *Mendelism in Man (a brief study on heredity)*, Dissertation thesis presented to the Faculty of Medicine of the University of Porto in 1912.

Archer, L., "Contribuições para a História da Genética em Portugal" *in História e Desenvolvimento da Ciência em Portugal no século XX* (Lisboa: Academia das Ciências de Lisboa, 1986), pp. 1027-1065.

Bensaúde, M., "Sur la sexualité chez les Champignons Basidiomycètes", *Comptes Rendus the l'Académie des Sciences de Paris*, 165 (1917), 286-288.

Bensaúde, M., *Recherches sur le cycle évolutif et la sexualité chez les Basidiomycètes* (Nemours: Impremerie Nemourienne, 1918).

Bonneuil, C., "Development as Experiment: Science and State Building in Late Colonial and Postcolonial Africa, 1930-1970", *Osiris*, 15 (2001), 258-281.

Bonneuil, C., "Plant Breeding and Experimental Cultures: Agriculture and the Development of Genetics in France", *Journal of the History of Biology*, 39 (2006), 281-308.

Bowler, P. J. and I. R. Morus, *Making Modern Science. A Historical Survey* (Chicago/Londres: The University of Chicago Press, 2005).

Brush, S. G., "How theories became knowledge: Morgan's chromosome theory of heredity in America and Britain", *Journal History of Biology*, 35 (2002), 471-535.

Chang, S-T, and P. G. Miles, *Mushrooms: Cultivation, Nutritional Value, Medicinal Effect and Environmental Impact* (CRC Press, 2004) , 2nd ed..

Chaves, P. R., *As Bases Citológicas da Hereditariedade*, Colecção Natura (Lisboa: Biblioteca Nacional, 1922).

Costa, A. Celestino da, "A Universidade Portuguesa e o problema da sua reforma", Conference of the Federação Académica Portuguesa de Lisboa, 19-22 April 1918.

Costa, A. Celestino da, "A Reforma do Ensino Médico", Separata *Medicina Contemporânea*, 31-32 (1918).

Costa, A. Celestino da, and P. R. Chaves, *Manual de Técnica Histológica. Guia de Trabalhos Práticos* (Lisboa, 1921).

Elina, O., S. Heim and N. Roll-Hansen, "Plant breeding on the front: Imperialism, War and Exploitation, *Osiris*, 20 (2005), 161-179.

Falk, Inicial do primeiro nome???? "Mendel's Impact" in *A Cultural History of Heredity III: 19th and Early 20th* (Max Planck Institute for the History of Science: Preprint 294, 2005), 9-29.

Fernandes, A., "Prof. Dr. Aurélio Quintanilha", Separata do *Boletim da Sociedade Broteriana*, 36, 2ª série (1962).

Fernandes, A., "História da botânica em Portugal até finais do século XIX" *in História e Desenvolvimento da Ciência em Portugal*, (Lisboa: Academia das Ciências de Lisboa, 1986).

Fernandes, A., "Lembrando o Prof. Doutor Aurélio Quintanilha", *Brotéria Genética*, IX (1988), 135-150.

Franklin, S., *Dolly Mixtures. The Remaking of Genealogy* (Londres: Duke University Press, 2007).

Gago, M. M., "História da biologia em Portugal (1700-1946): a tradição naturalista, as ciências biomédicas e a emergência da genética" in Gonçalves, M. E. and J. Freire, eds., *Biologia e Biólogos em Portugal. Ensino, Emprego e Sociedade* (Lisboa, Esfera do Caos, 2009), pp. 171-214.

Gago, M. M., "Portuguese struggle for synthesis: José Antunes Serra and the genetic controversies in the 1940's", 6th STEP Meeting, Junho 2008, Istambul, Turquia.

Galison, P., “Trading Zone – Coordinating Action and Belief” in Mario Biagioli, ed., *The Science Studies Reader* (New York/London: Routledge, 1999), pp. 137-160.

Garcia Belmar, Antonio, and José Rámon Bertomeu-Sánchez, Manolis Patiniotis, Anders Lundgren, eds., Special Issue, *Science and Education*, 15 (7-8) (2006), pp.657-880.

Gaspar, J., *A Investigação no laboratório de física da Universidade de Lisboa (1929-1947)*, MSc Dissertation in History and Philosophy of Science , Faculdade de Ciências da Universidade de Lisboa, 2008.

Gaspar, J., *A Investigação no laboratório de física da Universidade de Lisboa* (*1929-1947*) (Braga: CIUHCT, 2009).

Gaspar, J., and Maria do Mar Gago, Ana Simões, “Fields and Labs, groups and research schools: Portuguese geneticists and physicists under the fascist regime (1929-1954),” ICHST 2009, Session S-37 Science, Politics and Development in the 20th century, Budapest, 28 July-2 August 2009.

Gavroglu, Kostas, and Manolis Patiniotis, Faidra Papanelopoulou, Ana Simões, Ana Carneiro, Maria Paula Diogo, Jose Ramon Bertomeu-Sánchez, Antonio Garcia Belmar, Agusti Nieto-Galan, “Science and technology in the European periphery. Some historiographical reflections”, *History of Science* 46 (2008), 153-175.

Gayon J. and D. Zallen., “The singular Fate of Genetics in the History of French Biology, 1900-1940” *Journal of the History of Biology*, 3 (1988), 357-402.

Gayon, J., “From measurement to organization: A philosophical scheme for the history of the concept of heredity” in Peter Beurton, Raphael Falk and Hans-Jörg Rheinberger eds., *The Concept of the Gene in Development and Evolution. Historical and Epistemological Perspectives* (Cambridge: Cambridge University Press, 2000), pp. 69-90.

Harwood, J., “National Styles in Science. Genetics in Germany and the United States between the World Wars”, *Isis*, 78 (1987), 390-414.

Harwood, J., *Styles of Scientific Thoughts: The German Genetics Community 1900-1933* (Chicago: University of Chicago, 1993).

Harwood, J., *Technology Dilemma. Agricultural Colleges between Science and Practice in Germany, 1860-1934* (Bern: European Academic Publishers, 2005).

Heim, S. "Research for autarky. The Contribution of Scientists to Nazi rule in Germany", Forschunsgprogramm Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus, *Ergebnisse* 4 (2001).

Judson, Horace F., *The Eight Day of Creation. The Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979).

Kimmelman, B., "Mr. Blakeslee builds his dream house: Agricultural institutions, genetics, and careers 1900-1915", *Journal of the History of Biology*, 39 (2006), 241-280.

Koch, G., "Otmar Freiherr von Verschuer. Vida e Obra", Separata *O Médico,* 38 (1952).

Kohler, R. E., *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994).

Lucena, M., *A evolução do sistema corporativo português, O Salazarismo* (vol. 1) (Lisboa: Perspectivas e Realidades, 1976)

Lucena, M., *A evolução do sistema corporativo português, O Marcelismo* (vol. 2) (Lisboa: Perspectivas e Realidades, 1976)

Lucena, M., "Sobre a evolução dos organismos de coordenação económica ligados à lavoura (I)", *Análise Social,* XIV (56) (1978-4º), 817-862.

Mayer, R., *Biometric Methods and their applications on thrematology,* dissertation thesis presented to the Superior Institute of Agronomics of the University Technical of Lisbon in 1914.

Mayer, R., “A Univerisdade Americana. O seu espírito; os seus métodos; influência na sua acção educativa”, Separata da *Revista Agronómica* (1916).

Mayr, E., *The Growth of Biological Thought* (Cambridge: Harvard University Press, 1980).

Morais, M. C., “Classificação das Lãs Nacionais”, Portuguese Society of the Veterinary Medicine, Lisbon, 23 January 1946.

Mota, M., “A Contribuição Portuguesa para o progresso da genética”, International conference “Os Portugueses e o Mundo”, Porto, 4-7 June 1985.

Müller-Wille, S., “From Linnaeus species to Mendelian factors: Elements of Hybridism, 1751-1870”, *Annals of Science*, 64 (2) (2007), 172-215.

Neves, J. B., “Prof. Doutor Abílio Fernandes”, Separata do *Boletim da Sociedade Broteriana*, LIII, 2ª serie (1980).

Palladino, P., “Between Craft and Science: Plant Breeding, Mendelian genetics, and British Universities, 1900–1920”, *Technology and Culture*, 34 (1993), 300-323.

Papanelopoulou, Faidra and Agusti Nieto-Galan, Enrique Perdiguero, eds., Popularizing Science and Technology in the European Periphery, 1800-2000 (Oxon: Ashgate, 2009).

Pereira, A. L., *Darwin em Portugal (1865 – 1914). Filosofia, História, Engenharia Social* (Coimbra: Almedina, 2001).

Philippon J. and J. Poirier, *Joseph Babinsky. A Biography* (Oxford: Oxford University Press, 2008).

Proctor, R., “From Anthropology to Rassenkunde in the German Anthropological Tradition” in George W. Stocking Jr., ed., *Bones, Bodies, Behavior, Esssays on Biological Anthropology* (University of Wisconsin Press, 1988), pp. 138-179

Quintanilha, A., “Le problème de la sexualité chez les basidiomycètes. Recherches sur le genre Coprinus”, *Boletim da Sociedade Broteriana*, 8 (2ª série) (1933), 3-99.

Quintanilha, A., “Richard B. Goldschmidt”, *Portugaliae Acta Biológica*, Serie A (1951), 11-16.

Quintanilha, A., “Mathilde Bensaúde 23.1.1890 – 22.11.1969”, *Boletim da Sociedade Portuguesa de Ciências Naturais*, XIII (2ª série) (1970-71), 5-19.

Quintanilha, A., “Progressos da Genética dos Fungos e a contribuição dos Portugueses”, *Revista de Ciências Biológicas*, 2 (Série B) (1973)

Quintanilha, A., “História da Genética em Portugal”, *Brotéria*, XLIV (3-4) (1975), 189-208.

Rheinberger, H.J., “Ephestia: The Experimental Design of Alfred Kühn's Physiological Developmental Genetics”, *Journal of the History of Biology*, 3 (2000), 535-576.

Richmond, M., “Women in the History of Genetics. William Bateson and the Newham College Mendelians, 1900-1910”, *Isis*, 92 (2001), 55-90.

Russell, E., “Introduction: The Garden in the Machine: Toward an Evolutionary History of Technology”, in Schrepfer, S. and P. Scranton, eds., *Industrializing Animals. Introducing Evolutionary History* (New York/London: Routledge, 2004) pp. 1-16.

Santos, G. D., *A Escola de Antropologia de Coimbra, 1885-1950: o que significa seguir uma regra científica?* (Lisboa: ICS Imprensa de Ciências Sociais, 2005).

Sapp, J., *Beyond the Gene, Cytoplasmatic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987).

Saraiva, T., “Laboratories and Landscapes: The Colonization of Portugal and Mozambique in the Twentieth Century”, *HoST*, 3 (forthcoming 2009).

Saraiva, T., "Fascist Labscapes: Geneticists and the New Landscapes of Fascism in Italy and Portugal", *Historical Studies in the Natural Sciences*, 40 (forthcoming 2010).

Schrepfer, R. S., "Afterword" in Schrepfer, S. and P. Scranton, eds., *Industrialing Animals. Introducing Evolutionary History* (New York/London: Routledge, 2004) pp. 261-266.

Simões, Ana, and Ana Carneiro, Maria Paula Diogo, eds., *Travels of Learning. A Geography of Science in Europe* (Dordrecht: Kluwer Academic Publishers, 2003).

Simões, Ana and Ana Carneiro, Maria Paula Diogo, "Perspectives on Contemporary History of Science in Portugal," *Nuncius* 23 (2) (2008), 237-263.

Sousa J. V. Gonçalves de, "As Leis da Hibridação segundo Mendel e de Vries", *Revista Agronómica*, Ano II, 1 (1904), 149-153.

Tamagnini, E., "Os Problemas da Mestiçagem", I Congresso Nacional de Antropologia Colonial, Exposição Colonial Portuguesa, Porto, 1934.

Tamagnini, E. and J. A. Serra, "Subsídios para a História da Antropologia Portuguesa", communication presented at the Congress of the Portuguese Scientific Activity, Coimbra, 1940 (printed in 1942).

Tavares, C. N., "Vida e actividade científica do Prof. Dr. Flávio Ferreira Pinto Resende", Separata de *Portugaliae Acta Biológica* (B), IX (1-2) (1967).

Theunissen, B., "Knowledge is power: Hugo de Vries on science, heredity and social progress", *British Journal for the History of Science*, 27 (1994), 291-311.

Theunissen, B., "Breeding Without Mendelism: Theory and Practice of Dairy Cattle Breeding in the Netherlands 1900–1950", *Journal of the History of Biology*, 41 (2008), 637–676.

Ventura, A., “Resistências ao regime ditatorial” in João Medina, eds., *História de Portugal*. *O Estado Novo. Opressão e Resistência* (Amadora: Clube Internacional do Livro), Vol. XIII.

Weiss, S. F., “Human Genetics and Politics as Mutually Beneficial Resources: The Case of Kaiser Wilhelm Institute for Anthropology, Human Heredity and Eugenics During the Third Reich”, *Journal of the History of Biology*, 39 (2006), 41-88.