A thesis submitted to the Department of Environmental Sciences and Policy of Central European University in part fulfilment of the Degree of Master of Science

The presence and perception of Lysenkoism and Michurinist biology at the Genetic Institute of the Hungarian Academy of Sciences, 1948–1966

Tamás SZABÓ

July, 2014 Budapest Notes on copyright and the ownership of intellectual property rights:

(1) Copyright in text of this thesis rests with the Author. Copies (by any process) either in full, or of extracts, may be made only in accordance with instructions given by the Author and lodged in the Central European University Library. Details may be obtained from the Librarian. This page must form part of any such copies made. Further copies (by any process) of copies made in accordance with such instructions may not be made without the permission (in writing) of the Author.

(2) The ownership of any intellectual property rights which may be described in this thesis is vested in the Central European University, subject to any prior agreement to the contrary, and may not be made available for use by third parties without the written permission of the University, which will prescribe the terms and conditions of any such agreement.

(3) For bibliographic and reference purposes this thesis should be referred to as:

Szabó, T. 2014. The presence and perception of Lysenkoism and Michurinist biology at the Genetic Institute of the Hungarian Academy of Sciences, 1948–1966. Master of Science thesis, Central European University, Budapest.

Further information on the conditions under which disclosures and exploitation may take place is available from the Head of the Department of Environmental Sciences and Policy, Central European University.

# Author's declaration

No portion of the work referred to in this thesis has been submitted in support of an application for another degree or qualification of this or any other university or other institute of learning.

Tamás Szabó

# CENTRAL EUROPEAN UNIVERSITY

#### **ABSTRACT OF THESIS** submitted by:

Tamás SZABÓ

for the degree of Master of Science and entitled:

The presence and perception of Lysenkoism and Michurinist biology at the Genetic Institute of the Hungarian Academy of Sciences, 1948–1966

Month and Year of submission: July, 2014.

After agronomist T. D. Lysenko gained supreme power in Soviet agricultural matters in 1948, his controversial Michurinist biology was ardently exported to satellite states, including Hungary. In the midst of this centralized purging of modern genetics and evolutionary biology, the Genetic Institute of the Hungarian Academy of Sciences inexplicably seemed to resist the ideological, "pseudoscientific" push until Lysenko's fall in 1964-65.

In my thesis, to understand the history of plant genetic research at the Genetic Institute between 1948 and 1966, I show the extent, and the involved scientists' perception of Lysenkoism and Michurinist biology. By analyzing archival sources through interrelated conceptions of relationships (environment and organisms; humans and society; politics and science) of the rival scientific discourses, a representative image is drawn about the perceived order of things. This is interpreted using the works of Latour, and Lewontin and Levins, emphasizing a science that is in the collective making; merging context and contents; historical contingency; interpenetration and mutual constitutiveness.

It is found that though operating with different sets of closed concepts, both discourses share approximately the same stance in their relationships toward the natural and social environment; the role of humans; and the interaction of politics and science. The same underlying attitude of modernity is uncovered, stressing the idea of a consciously designed of society with Promethean men, and the subordination of nature to their will; devoid of political and historical self-reflection, promoting the often still prevailing, yet false idea of neutral science in the sole service of society.

**Keywords:** biology, genetics, agriculture, politics, science, pseudoscience, Lysenko, socialism, modernity, discourse analysis

# Acknowledgements

I would like to thank my supervisor, Guntra Aistara; Zsófia Szatmári-Margitai; Ádám Fülöp; Diána Hay; and Béla Novák for their help and support.

1. Introduction	1
2. Methodology	5
3. Theoretical Framework	10
3.1. Latour: Black boxes, science in the making, merging context and contents	10
3.2. Lewontin and Levins: Dialectics, contingency; interpenetration, mutual constitutivenes	s13
4. Literature Review	16
4.1. The popular opinion	16
4.1.1. Neo-Lamarckian pseudoscience	16
4.1.2. A political dogma	17
4.2. Enter history	18
4.3. Complex explanations	20
4.3.1 And their details	21
4.4. Critique	22
4.4.1. The lack of environment and organisms	23
4.4.2. The lack of humans and society	24
4.5. Conclusion	26
5. Findings	28
5.1. RQ1: How did ideology and politics influence plant genetic research?	28
5.1.1. Introduction; and a short summary of history	28
5.1.2. The establishment in the early fifties; and the Institute	29
5.1.3. The dissenting voices and turning point of 1953-54	31
5.1.4. 1956: "We are liberated from the oppression of dogmatism"	32
5.1.4.1. Critique	33
5.1.5. The 1958 Genetic Debate: Restoration	35
5.1.6. The comparative dialectical materialism of the early sixties	38
5.1.7. 1965-66: "The switch from 'Lysenkoist genetics' will be considerably difficult"	40
5.1.7.1. Critique	41
5.1.7.2. "This fight of worldviews did not restrict any biologist"	42
5.1.8. After 1966; and a one-time reminder	44
5.1.9. Conclusion and critique: Discourses and Scientization	45
5.2. RQ2: How were the environment and organisms conceptualized by plant scientists?	49
5.2.1. Introduction	49

5.2.2. The inauguration of the early fifties	. 49
5.2.3. The restoration of the early sixties	51
5.2.4. The geneticists' side	53
5.2.5. Conclusion and outlook	54
5.3. RQ3: What was the role of humans and society in the natural and social environment?	56
5.3.1. Introduction	56
5.3.2. Living the 11th thesis	. 56
5.3.3. Gramsci: Man is the ensemble of social relations, the conqueror of material forces	57
5.3.4. Designing humans and society; controlling nature	58
5.3.5. Modernity as communism: The ultimate subordination of nature to humans	59
5.3.6. High Modernism: The point of convergence	. 60
5.3.7. Legibility; and history in the making	60
5.3.8. Conclusion and outlook	61
6. Conclusion	. 64
6.1. Motivation, aims, objectives, research questions, and methodological overview	64
6.2. Summary of the history of plant genetics at the GI between 1948 and 1966	65
6.3. RQ1: How did ideology and politics influence plant genetic research?	65
6.4. RQ2: How were the environment and organisms conceptualized by plant scientists?	67
6.5. RQ3: What was the role of humans and society in the natural and social environment?	67
6.6. Summary and Outlook	. 69
7. References	70
7.1. Academic Literature	. 70
7.2. Archival Sources	72

# List of Tables

Table 2.1. Archival sources used in this study	6
Table 2.2. Classification of archival sources used in this study	.9

# 1. Introduction

On January 25, 1960, the Soviet agronomist Trofim Denisovich Lysenko (1898–1976), famous for his vernalization experiments<sup>1</sup> and various other biological practices, reminiscent of Jean-Baptiste Lamarck's,<sup>2</sup> gave a speech at the Hungarian Academy of Sciences (HAS) in front of a full house. By that time, Lysenko has already fallen out of grace once after his great patron, believed to be under his spell by some, Stalin himself, died in 1953. Nevertheless, during the years since the late thirties, on the surface he inexplicably gained enormous political influence in Soviet agriculture by systemically eradicating modern genetics; and sometimes having the curious luck to see his scientific enemies suddenly disappear from among the living, while positioning his school of thought, named after horticulturalist I. V. Michurin, as the only biology and genetics. Nonetheless, his time was soon up but not yet, so that year when he came to Hungary, a satellite state of the Soviet Union where his teachings were ardently exported, his mission was to preach some more by answering the more than two hundred questions he received during the previous days. He, however, made the impression of a humble and simple man when he began talking:

I have said it multiple times in Hungary that during my whole life, I have striven to learn, and I feel that I know very little. I am sure that the comrades gathered here, who welcome Soviet comrades with such great love, know more than me in many aspects. ... I promise to you that I will use every effort in order to answer the numerous questions raised on behalf of the Academy of Sciences. ... I would like to emphasize that I literally prepared for this speech the whole night, and I am still trembling a bit, I am still afraid. (Lysenko 1960, 1-2)

In spite of this, Sándor Igali (2002), a then contemporary scientist, drew different conclusions: "The anticipated effect turned against itself. The audience was listening to the familiar phraseology in awk-ward embarrassment ... [and] then the fiasco became evident."

In 1965, twelve years after the discovery of the structure of the DNA, a substance whose existence or at least function he denied, Lysenko finally and irrevocably fell out of favor. His teachings were replaced by rapidly advancing modern genetics, and Soviet agriculture was keen on to make up for the lost time in developing agriculture to ensure food security; the "Lysenko affair," as it is often called, was over.

<sup>1</sup> Vernalization is the "acquisition or acceleration of the ability to flower by a chilling treatment" (Chouard quoted in Amasino 2004, 2554). Lysenko claimed that such properties can be inherited, therefore vernalization would dramatically reduce the speed of grain production compared to the meticulous process of selective breeding informed by genetics.

<sup>2</sup> The biologist Jean Baptiste-Lamarck (1744–1829) is generally known for basing evolution on natural laws, and his theory on the inheritance of acquired characteristics, for which Darwin and his followers did not find evidence.

But back in 1949, the year after the infamous VASKhNIL session where modern genetics based on Mendel and Morgan was formally banned by the Soviet Communist Party, and Lysenko rose to supreme power, enormous energy was spent in frenetic excitement on propagating Lysenko's teachings both in the Soviet Union and outside of it in the satellite states including Hungary, where the same official purge took place from books through institutional positions to academic education (Ibid.). Nevertheless, the provocatively named Genetic Institute (GI) of the HAS, whose director at the time was geneticist, biochemist, plant physiologist Barna Győrffy, apparently resisted the push all the way from 1948 to 1965, while engaged in, among other things, plant breeding research, a field in need of, therefore controversial because of it, modern genetic knowledge.

In my thesis, by analyzing archival sources never before researched, including programmatic statements, internal records, and personal communication, I intend to show how this influence from the Soviet Union affected the way the GI conceived the environment and organisms; humans and society; and politics and science which, in turn, altered the direction of Hungarian plant genetic research.

Therefore, my aim to understand the history of plant genetic research at the GI between 1948 and 1966 through two objectives:

- by examining the extent of the presence of Lysenkoism and Michurinist biology at the GI and the HAS
- 2. by examining the plant scientists' personal perception of Lysenkoism and Michurinist biology

Accordingly, I propose three interrelated research questions that, contrary to most research done so far, start the investigation at the epistemological foundations through which an implicit discursive ontology could be drawn. These research questions are the following. During this period (1948–1966) at the GI,

- 1. how were the environment and organisms conceptualized by plant scientists?
- 2. what was the role of humans and society in the natural and social environment?
- 3. how did ideology and politics influence plant genetic research?

The subsequent Literature Review and Findings will be interpreted through these questions. In my Findings, each question will receive its own subchapter, and beyond actual results, they will be sup-

plemented by my analyses and reflections to make comprehension more practical. What follows, is a brief presentation of the contents of these subchapters.

The first subchapter, regarding the concepts of the environment and organisms of plant scientists, will deal with the history of the GI, the changes in its operations, policies, and ideologies due to the influence of Lysenkoism and Michurinist biology. This history would be told, however, through the lens of the environment and organisms, by highlighting and analyzing certain events and programmatic statements, such as internal debates and the five-year plans.

The reader will understand not only how plant breeding and genetics research of the era clashed with Lysenkoism and Michurinist biology, but also to grasp a more theoretical, philosophical idea of what the environment and organisms were imagined to be. This subchapter already deals with politics and science, the topic of the third research question and subchapter, but only on the historical, descriptive level, as there will be no analysis of their relationship.

The subchapter will lead to the next one by asking where humans are in relation to that environment.

The second subchapter, regarding the role of humans and society in the natural and social environment, will deal with concrete and abstract concepts of humans and those environments. There will be concrete, descriptive examples of how humans should act in those environments due to their social, political or scientific responsibility, so that an image of how people engage with "their" environment will be drawn. There also will be a more theoretical, philosophical analysis of what this relationship means in the history of human–environment relationship and modern civilization.

The reader will understand how the particular dynamics of the environment and organisms in plant science of the first subchapter was part of a larger whole, the one of humans' relationship with it.

The subchapter will lead to the next one by asking how that role of humans is created and molded; of all the things that affect it, politics is of great importance, as humans live in a social and cultural reality informed by politics, besides their biological one.

The third and last subchapter of my Findings, regarding the influence of ideology and politics on plant genetics, will deal with how scientists reacted to ideology and politics "threatening" the often mentioned neutrality of science. I will deliver an analysis of ideology and politics, and science in the case of the GI through the comparison of official programmatic statements; actual applications for research plans, and their later reports; and personal opinions, representing progressive milestones in time. By infusing the chapter with references to the Theoretical Framework about politics, society, and science, I will demonstrate to the reader the contingency, non-linearity, non-narrative nature of the history of plant genetics at the GI of the period. Before that, however, I will introduce my Methodology, Theoretical Framework, and Literature Review.

### 2. Methodology

While researching and writing my thesis, I followed an interpretivist method as I was interested in understanding what Lysenkoism and Michurinist biology meant uniquely and individually for the people at the GI.

This interpretivist method is based on "the theoretical belief that reality is socially constructed and fluid. Thus, what we know is always negotiated within cultures, social settings, and relationship with other people."

It has a relativist ontology, which assumes "that reality as we know it is constructed intersubjectively through the meanings and understandings developed socially and experientially."

Its epistemology is a transactional or subjectivist epistemology, which "assumes that we cannot separate ourselves from what we know. The investigator and the object of investigation are linked such that who we are and how we understand the world is a central part of how we understand ourselves, others and the world."

From this perspective, "validity or truth cannot be grounded in an objective reality, [and] ... meanings are emergent from the research process. Therefore, [all] interpretations are based in a particular moment. That is, they are located in a particular context or situation and time" (Cohen and Crabtree 2006).

This means that my thesis is founded on a qualitative, discourse analysis approach similar to the one in Michel Foucault's *The Archaeology of Knowledge* (1969). This discourse analysis maintains that the "systems of thought and knowledge (epistemes or discursive formations, in Foucault's terminology) are governed by rules, beyond those of grammar and logic, that operate beneath the consciousness of individual subjects and define a system of conceptual possibilities that determines the boundaries of thought in a given domain and period" (Gutting 2013). Its mission is to uncover those axiomatic rules.

For this, I conducted my research at the Archives of the Hungarian Academy of Sciences (H-1051 Budapest, Széchenyi István tér 9. Ground Floor) between May 7 and 28. When choosing the place, I assumed that since GI was part of HAS at the period, it would probably have all the documents that I need to understand the perceptions of people and the Institution then.

I was greatly assisted by head of department Diána Hay in my search for broad, related topics (plant breeding and genetics, Lysenkoism, Soviet biology); the period (from the late forties to 1965); and key people (Lysenko, Barna Győrffy). Archival auxiliary Béla Novák helped me in administrative issues, and photocopying my chosen documents. While probing the database of the Archives, I as-

sumed that such topics and the activities of key people restricted to that certain period would yield significant amount of useful data, so as to enable me to narrow down my focus.

I collected 71 unique documents (542 A4 pages) out of eight archival boxes originating from between 1947 and 1969. Out of these, I recognized ten types of documents. There are 7 programmatic papers (statements or missions), 4 resolutions or decrees, 12 research plans, reports or documents, 23 internal reports or opinions, 7 records of meetings or sessions, 3 typed lectures, 5 articles, 4 internal letters, 1 handwritten note, and 5 classified as other. The following two tables (Tables 2.1 and 2.2) summarize the titles and types of documents that I found.

Box and folder	Title	Year
Elnökségi Iratok 226/4	T.D. Lysenko: The present questions of Michurinist biology (HAS lecture)	1960
VIII. Biológiai Tudományok Osztálya 5/9	The five-year plan of Hungarian biological research	1950/51?
VIII. Biológiai Tudományok Osztálya 5/10	Calendar of biological events	?
VIII. Biológiai Tudományok Osztálya 8/3	The five-year plan of Hungarian scientific research	1950/51?
VIII. Biológiai Tudományok Osztálya 8/3	T.D. Lysenko: The questions of species formation	?
VIII. Biológiai Tudományok Osztálya 8/9	Records of the September 2, 1953 meeting's resolutions concerning the 1954 research plan of Martonvásár Institute	1953
VIII. Biológiai Tudományok Osztálya 8/9	Records of the November 9, 1953 meeting of the Biology Committee	1953
VIII. Biológiai Tudományok Osztálya 8/9	Records of the December 14, 1954 meeting of the Biology Committee	1954
VIII. Biológiai Tudományok Osztálya 8/9	The Biology Committee's problems and work done so far	pre. 1953
VIII. Biológiai Tudományok Osztálya 72/3	Records of the October 9, 1958 Genetic Debate	1958
		40/0
4/2/1	iv.A. waksimov and P.A. Genkeij: The theory of stage development	1949
Győrffy Barna Iratai 4/2/4	Barna Győrffy's handwritten notes on Lysenko's work	1951
Győrffy Barna Iratai 4/2/5	V.S. Dimitriev: The further development of Darwin's teachings on the formation of species in the work of academician T.D. Lysenko	1952

Table 2.1. Archival sources used in this study. Source: Archives of the Hungarian Academy of Sciences.

Győrffy Barna Iratai 4/2/11	A.N. Studitsky: For the creative development of species formation	1953
Győrffy Barna Iratai 4/5/5	T.D. Lysenko: The new definition of biological race (Szabad Nép (originally Pravda), November 24 1950)	1950
Győrffy Barna Iratai 7/1/1	Barna Győrffy's lecture on the state of Hungarian plant breeding	1947
Győrffy Barna Iratai	The current situation regarding the genetic debates	1965
Győrffy Barna Iratai	Barna Győrffy's lecture: The ways of scientific experimentation	1947
Győrffy Barna Iratai	Notes on the situation of the education of genetics	1965
Győrffy Barna Iratai 7/1/3	Proposal for the reorganization and development of our plant breeding	1949
Győrffy Barna Iratai 7/1/4	The five-year plan of agricultural plant breeding	1949
Győrffy Barna Iratai 7/1/4	Notes on the future research and education fields of genetics	1965
Győrffy Barna Iratai 7/1/5	Letter to Barna Győrffy on the role of education of genetics in animal husbandry	1965
Győrffy Barna Iratai 7/1/6	The most significant achievements of genetics in the past 15 years	1965
Győrffy Barna Iratai 7/2	The draft report of the Section of Biological Sciences on the state of genetics	1965
Győrffy Barna Iratai 7/2	Barna Győrffy's comment on the draft report of the Section of Biological Sciences on the state of genetics	1965
Győrffy Barna Iratai 7/2	Barna Győrffy's letter to Brunó Straub F. on the report by the	1966
Győrffy Barna Iratai 7/2	Resolution proposal based on the report	1966
Győrffy Barna Iratai	The history, state, tasks of our plant breeding (proposal)	1965
Győrffy Barna Iratai 7/?	Barna Győrffy's opinion of the proposal	1965
Győrffy Barna Iratai 7/2/1	The main problems of the scientific five-year plan of agriculture and forestry (Preliminary plan)	1950
Győrffy Barna Iratai 7/2/2	Barna Győrffy's letter to Imre Biacsi on the five-year plan 1.	1950
Győrffy Barna Iratai 7/2/2	Barna Győrffy's letter to Imre Biacsi on the five-year plan 2.	1950
Győrffy Barna Iratai 7/2/3	The five-year plan of Hungarian biological research (The general principles of the plan's elaboration)	1950
Győrffy Barna Iratai 7/2/3	The main aspects of the five-year plan's main tasks' development	1950
Győrffy Barna Iratai 7/3/8	Barna Győrffy: Notes on the relationship between biological, medical, and agricultural sciences	1953
Győrffy Barna Iratai 7/3/10	The main tasks of the VIII. Agricultural Sciences Department's 1953 research plan	1953
Győrffy Barna Iratai 7/3/17	The perspective plan of experimental botanical research (The main tasks of the second five-year plan)	1954
Győrffy Barna Iratai 7/3/21	Barna Győrffy's opinion on the second half of the 1955 report by Martonvásár	1956?
Győrffy Barna Iratai 7/3/22	Barna Győrffy's opinion on the 1954 report by the HAS Botanical Research Institute	1955
Győrffy Barna Iratai 7/3/23	Barna Győrffy's opinion on the 1954 work of the Sopronhorpács Plant Selection and Plant Breeding Research Institute	1955
Győrffy Barna Iratai 7/4/1	The notes of Barna Győrffy on the 1957 research work of Martonvásár Genetics Department	1958
Győrffy Barna Iratai 7/4/3	The notes of Barna Győrffy on the genetic aspects of the 1958 report by Martonvásár	1959

Győrffy Barna Iratai 7/5/1	Planned research topics directed by Barna Győrffy	1960?
Genetikai Intézet	Sándor Sárkány: The history of the Institute 1939-1948	1948
Iratai 3/1	Sandor Sankary, menistory of the instruct (555-15-16	1510
Genetikai Intézet Iratai 3/2	The operation of the Genetic Institute of the Hungarian Academy of Sciences	1959?
Genetikai Intézet Iratai 3/2?	The 4.008/1949. (91.) Gov. Decree of the Government of the Republic of Hungary on the creation of Agrobiological Institute	1949
Genetikai Intézet Iratai 3/2?	The 8.062/8/1949. (119.) MoA. Decree of the Minister of Agriculture on the creation of Agrobiological Institute	1949
Genetikai Intézet Iratai 3/2?	Hungarian Academy of Sciences Agrobiological Institute: Genetic Department	1950?
Genetikai Intézet Iratai 3/2?	The description of the tasks of the Genetic Institute 1939-1949	1951?
Genetikai Intézet Iratai 3/3	The Genetic Institute of the Hungarian Academy of Sciences	1965
Genetikai Intézet Iratai 3/5/1	Records of the June 1, 1956 meeting of the Biological Group	1956
Genetikai Intézet Iratai 3/5/1	Barna Győrffy's comment on the meeting	1956
Genetikai Intézet Iratai 3/5/2	The state of Hungarian plant genetics research (without micro- evolution)	1966
Genetikai Intézet Iratai 3/8	Appendix, The most significant achievements of the progress of genetics in the last 15 years	1965?
Genetikai Intézet Iratai 3/8/2	The 25 years of genetics in Hungary	1969
Genetikai Intézet Iratai 3/8/2	The achievements of domestic genetic research	1956
Genetikai Intézet Iratai 3/8/?	Hungarian plant genetics research (without micro-evolution) 1946-1965	1965?
Genetikai Intézet Iratai 3/8/3	The report by the Section of Biological Sciences on the state of genetics	1966
Genetikai Intézet Iratai 3/8/3	Resolution no. 14/1966. on the state of genetics by the directorate of the HAS	1966
Genetikai Intézet Iratai 79/1/1	Topic sheet? for the 1961 research plan "The analysis and criticism of the molecular genetic concept" 1.	1960
Genetikai Intézet Iratai 79/1/1	Topic sheet for the 1961 research plan "The analysis and criticism of the molecular genetic concept" 2.	1960
Genetikai Intézet Iratai 79/1/2	Topic sheet for the 1961 research plan "The analysis and criticism of the molecular genetic concept" 3.	1960
Genetikai Intézet Iratai 79/2/1	Topic sheet for the 1962 research plan "The analysis and criticism of the molecular genetic concept"	1961
Genetikai Intézet Iratai 79/2/2	Report? Of "The analysis and criticism of the molecular genetic concept"	1962
Genetikai Intézet Iratai 79/3/3	Topic sheet for the 1963 research plan "The philosophical analysis of biological quantitative and qualitative changes"	1962
Genetikai Intézet Iratai 79/4/1	Curriculum plan with five topics	1964?
Genetikai Intézet Iratai 79/4/1?	Topic sheet for the 1964 research plan "The philosophical analysis of biological quantitative and qualitative changes"	1963
Genetikai Intézet Iratai 79/4/1	Topic report on the 1964 research plan "The philosophical analysis of biological quantitative and qualitative changes"	1965
Genetikai Intézet Iratai 79/4/3	The philosophical analysis of biological quantitative and qualitative changes	1964
Genetikai Intézet Iratai 79/4/7	Opinion of the opponent of the 1964 research "The philosophical analysis of biological quantitative and qualitative changes"	1965

Туре	Box number								
	226	5	8	72	4	7	3	79	TOTAL
Programmatic papers		1	1			5			7
Resolutions, decrees						1	3		4
Research plans, documentations						2		10	12
Reports, opinions			1			14	8		23
Records			3	1		1	2		7
Lectures	1					2			3
Articles			1		4				5
Letters						4			4
Notes					1				1
Other		1					3	1	5
TOTAL	1	2	6	1	5	29	16	11	71

Table 2.2. Classification of archival sources used in this study. Source: Archives of the Hungarian Academy of Sciences.

After data collection, I read and analyzed them through the perspectives of my research questions, while paying attention to two dimensions: the historical events or event-like documents, and the unique, personal wording of remarks at other cases. While writing my thesis, I assumed that comparing these would shed light on the many turns and dead ends of plant genetics (biological science) and history in the making. For the three research questions and the factual history, I used four differently colored pens and examined the documents by hand, looking for any occurrence that is broadly related to "environment," "organisms," "humans," "society," "ideology," and "politics," and the history of the Institute. After this, I drafted paper outlines, listing every entry that I found. These then were selected and ordered into the final outline that formed the basis of this paper. This process was done according to the priorities of the Theoretical Framework, and my reflections.

The limitations of this study originate from time constraints; and the nature of the archival research itself, the fact that the veracity and the representativeness of the sources cannot be completely ensured.

## 3. Theoretical Framework

In my thesis; to analyze and evaluate the presence and perception of Lysenkoism and Michurinist biology at the GI, regarding the relationship of ideology and politics, and science; I will use a particular theoretical framework, a set of interconnected thoughts based on sociologist of science Bruno Latour's *Science in Action* (1987), along with the chapter "A Reasonable Skepticism" in evolutionary biologist, geneticist Richard Lewontin's *Biology as Ideology* (1995),<sup>3</sup> and various other chapters in *Biology Under the Influence* (2007) by Richard Lewontin and mathematical ecologist Richard Levins.

# 3.1. LATOUR: BLACK BOXES; SCIENCE IN THE MAKING; MERGING CONTEXT AND CONTENTS

In his seminal book, Latour (1987) follows scientists and engineers through history and society to understand how hypotheses turn into facts, then into machines or other small parts of a larger whole. He investigates how eventually all scientific facts or machinery get black boxed; that is, being too complex and entangled in other theories and applications, scientists and engineers abandon understanding the internal logic of them after a while, and draw a box around it, and tend only to the inputs and outputs. A common example for a black box is a computer, or the fact of climate change.

Latour's (Ibid., 21) fundamental approach stems from the realization that understanding how science is created is simpler before the black boxes close and become black than looking for social influences and biases later on. Realizing that eventually all accepted scientific facts are black boxes, he (Ibid., 25) notes about scientific literature: "*By itself a given sentence is neither a fact nor a fiction; it is made so by others, later on*. You make it more of a fact if you insert it as a closed, obvious, firm and packaged premise leading to some other less closed, less obvious, less firm and less united consequence." Even though he writes about scientific publications, this thought is applicable to the whole of science as well, as science can be conceptualized as an ever-improving production, a continuous build-up of working ideas about the world; and build-ups are only possible if their bases are kept in peace, in other words, black boxed. This means that science is a collective process (Ibid., 29), therefore the "fate of the statement, that is the decision about whether it is a fact or a fiction, depends on a sequence of debates later on," and "*the status of a statement depends on later statements*. It is made more of a certainty or less of a certainty depending on the next sentence that takes it up" (Ibid., 27).

<sup>3</sup> This book unfortunately lacks page numbers, therefore I also omitted them.

From these foundational thoughts, it is now possible to outline Latour's theoretical outlook, or ontology and epistemology of science and society.

It is worth noting now that, contrary to a first reading, Latour is not an advocate of the extreme postmodern social constructivism which claims that we can never know the world, that there is only subjectivity, plurality, and relativity of facts and opinions alike, and science or law-like facts are only mental constructions. This stance typically results from a misunderstanding or mistranslation of de-constructionist philosopher Jacques Derrida's famous 1967 assertion that *il n'y a pas de hors-texte*, that "there is no outside-text." Most proponents of postmodern thought understood it to mean *il n'y a rien en dehors du texte*, that "there is nothing outside the text." The latter indeed suggests an ultimate relativist epistemology, and maybe even ontology, toward the world that Latour does not intend in his book.<sup>4</sup>

Thus, the whole of Latour's arguments presume a sociocultural truth or reality situated right after the "material," or "objective" truth and reality, but before us, humans. Therefore this social reality filters "true reality," the state of "how things really are" for us; as a medium, it differs from that objective truth, yet we can only know that truth through this mediation. This is why science is *in the making* in Latour's thought, not just *is*.

When a hypothesis becomes a scientific fact, scientists or engineers tend to unconsciously agree that the social reality mediates something in a clean form from the objective reality; there is a wish for making this medium invisible. What actually happens is that the hypothesis is black boxed partly with old, partly with newly accepted biases, hence the apparent invisibility. This means that, as I mentioned before, science is a process with many actors, factors, and contingency. Scientific facts are always in the making, even if it is the nature of black boxes to keep us ignorant about their inner workings. By being apparently solid blocks carved out of one thing, they keep us away from realizing that no matter how old and accepted they are, they are still composed of a multitude of very different, ever-uncertain but just no longer questioned parts.

This theoretical position relativizes science, since now it is no longer seen as a quantized, that is, a delineated entity out of historical and cultural context, but as an ever-expanding, directionless process or "development." We can see the past, present, and possible future of a scientific fact or machinery; we can, at least intellectually, accept that there was a time when it was not science, and there might be a time when it is no longer science. Scientists actually know that science is based on falsifiability: its whole mission of producing absolute knowledge about the material world is based on a search for mistakes, the showcasing of errors, the public shaming of fellow scientists, and the debunk-

<sup>4</sup> For the radical textualist approach, cf. Where Are You?: An Ontology of the Cell Phone (2014) by Maurizio Ferraris.

ing of false theories just a day or hour before accepted as truth. Latour (Ibid., 52) notes that because of its high internal, discriminative standards, "it is hard to popularise science because it is designed to force out most people in the first place." Forcing people out is what is called convincing in science, and in the case of scientific literature, this convincing is "not just a matter of throwing words about. It is a race between the authors and the readers to control each other's moves" (Ibid., 58).

When some people, usually those with conflicts of any interests as we will see later in the main part of this thesis, claim the contrary, that whatever the state of the world might be, science does have a "development," and furthermore, an "internal" one, then that is the denial of historical, cultural, and political contexts and contingency, in other words, the existence and nature of black boxes. Then, the thinking that science has "development" presumes a value difference between two states, usually the past is bad or wrong, the future is good or correct, yet in Latour's historicist approach, there is no place for such arguments, since human values, no matter how primeval they might be, are also filtered through that sociocultural medium. Saying therefore, that science "develops" indicates the lack of historical reflection and foresight.

Therefore, looking at science from a historicist perspective, we can see that it is continuously and always in the making, since many "external" things influence the medium of social reality. The "social consciousness" is constantly changing, yet we can only arrive at discovering natural laws or scientific facts through this. Claiming that science is a separate, independent, objective entity is the effort to conceal or disavow the medium, usually done in order to sustain the interests of the present social or-der, no matter how small that privileged society may be. It is done out of fear for one's passing.

Notwithstanding, in reality, according to Latour (Ibid., 5), context and contents cannot be separated before the creation of a black box. When discussing the exciting discovery of the molecular structure of DNA quoting words from Watson, he (Ibid., 6) writes:

"Suspense," "game," "tone," "delay of publication," "awe," "six weeks delay" are not common words for describing a molecule structure. This is the case at least once the structure is known and learned by every student. However, as long as the structure is submitted to a competitor's probing, these queer words are part and parcel of the very chemical structure under investigation. Here again context and content fuse together.

It is only when things are black boxed by society, that knowledge and its context separate in an attempt of the black box to appear neutral, without contingency, in one word: strong (Ibid., 13).

As to the black boxing of statements into tacit knowledge, then incorporation of that into instruments (Ibid., 44), or more generally, how the fusion of context and contents, and other similar structures; such as whole and parts, environment and organism, and so on; can be explained in a more applied way, let us look at Lewontin. He, along with Levins, also writes about the socialization of the environment (Lewontin and Levins 2007b, 37) which is the main link between Latour and them. While Latour gives a historicist overview of science in the making, Lewontin and Levins engage in the making of that science, and while preaching about contingency, they themselves are also aware of their own contingency.

# 3.2. LEWONTIN AND LEVINS: DIALECTICS; CONTINGENCY; INTERPENETRATION, MUTUAL CONSTITUTIVENESS

Lewontin's (and also Levins') approach has been called different names even by himself (themselves), including Marxist dialectics, materialist dialectics, dialectical materialism or simply dialectics. Nevertheless, he (they) only represent one particular way of applying dialectic thought informed by a Marxist background. While being aware the consequent limitations, I do not intend to criticize dialectical materialism as a whole, nor do I would like to suggest that this approach is the best or without faults, nor that the apparent partiality toward materialism, as opposed to idealism, represents my view on the things discussed in this thesis. It will be an interesting attempt, however, to apply dialectics to self-proclaimed dialectical arguments in my Findings, and see how they hold up.

Therefore, instead of starting out from political motivations, I intend to use the underlying thought structure which presumes, in the words of Young (1978), "interpenetrations and mutual constitutiveness rather than simple causalities and mechanical interactions," as "an alternative world view to that of the positivism of the integrated conceptions of capitalism and its science and technology." Hence, when referring to my approach, I will use "dialectics" throughout in this thesis to get rid of most of the direct political associations of these terms, and focus on the movement of thought professing a sort of simultaneous oneness and difference.

Lewontin (1995) very early gives the position of science within society and culture by considering science as a productive activity, and as such, it is "a social institution completely integrated into and influenced by the structure of all our other social institutions. The problems that science deals with, the ideas that it uses in investigating those problems, even the so-called scientific results that come out of scientific investigation, are all deeply influenced by predispositions that derive from the society in which we live."

But above this level, "science is molded by society because it is a human productive activity that takes time and money, and so is guided by and directed by those forces in the world that have control

over money and time." In summary, it is "a supremely social institution, reflecting and reinforcing the dominant values and views of society at each historical epoch."

After discussing how during history science became the chief legitimating force in society replacing religion, then giving examples where (part and whole, cause and effect) the current mainstream view of science fails in providing a complete understanding of the world, he concludes that this view is untenable:

We have become so used to the atomistic machine view of the world that originated with Descartes that we have forgotten that it is a metaphor. We no longer think, as Descartes did, that the world is like a clock. We think it is a clock.

The popular alternative is to "go back to nature and the good old ways, ... to return to a description of the world as an indissoluble whole that we murder to dissect. [For people engaged in this,] there is no use in trying to break anything down into parts because we inevitably lose the essence, and the best we can do is treat the world holistically." Finally, he outlines the problem and simultaneously provides a definition for dialectics:

The problem is to construct a third view, one that sees the entire world neither as an indissoluble whole nor with the equally incorrect, but currently dominant, view that at every level the world is made up of bits and pieces that can be isolated and that have properties that can be studied in isolation. Both ideologies, one that mirrors the premodern feudal social world, and the other that mirrors the modern competitive individualist entrepreneurial one, prevent us from seeing the full richness of interaction in nature. In the end, they prevent a rich understanding of nature and prevent us from solving the problems to which science is supposed to apply itself.

### So what is a dialectical way of making science? That science stresses

(1) the historically contingent nature of wholes; (2) the qualitative differences among kinds of wholes such as organisms, ecosystems, and societies, each with its own origins and dynamics; (3) the ontological equality of part and whole, and their reciprocal determination; (4) the absence of any universal organizing principle. Rather, the way to understand systems is to identify the opposing processes that allow its persistence and those that eventually transform it. (Lewontin and Levins 2007c, 128)

Moreover, they (2007d, 187) emphasize processes more than things, "regarding things as snapshots of process. When we change our focus from objects to processes, we ask two fundamental questions: Why are things the way they are instead of a little bit different? Why are things the way they are instead of very different?"

Identifying the opposing processes of a system, and these two questions above allude to the same phenomenon. Thinking dialectically then, is to be aware of the simultaneously open and closed state of black boxes, in other words, being able to switch between different temporal and methodological perspectives, to speed up time or slow it down in order not to miss important events which would otherwise be neglected on the long run. Such principles can also be utilized in a *critique* of any science that fails to adhere to these. In my Findings, I will continuously refer back to these in the analysis and reflection parts.

This approach especially effective, since, as a summary, "[b]ecause every historical phenomenon has its own particular locus in a particular sociocultural constellation with its own concrete and particular time and history, there is no one transhistorical law or generality that can explain the dynamics of all historical change" (Fracchia and Lewontin 2007, 276).

When looking at the interface ideology and politics, and science, I believe it is pertinent to attempt to take up a pre-black box, process-based dialectical perspective in the case of Lysenkoism and Michurinist biology at the GI where, as I presumed through my interconnected research questions, a particular idea of the environment and organisms, an image of humans and society, as well as ideology and politics somehow must have penetrated science. My assumption is that it happened so much that it is not possible anymore to say that they "penetrated science," since "science" as an independent, objective entity *alone* is no more, there is nothing that could be "penetrated." Science simultaneously *is* and *is not* the penetration itself; the idea that the interaction assigns the roles of the object and subject<sup>5</sup> is neglected in conventional thought.

Let us see in the Literature Review now, how others perceived and analyzed Lysenkoism and Michurinist biology worldwide.

<sup>5</sup> Cf. Dolphijn, and van der Tuin 2012.

## 4. Literature Review

The simplest question to describe the main question of this Literature Review would be: What were Lysenkoism and Michurinist biology? Who was this man? An evil charlatan, a cunning magician, a barefoot peasant scientist, or a ruthless political careerist? All such are implied in literature.

It turns out that this is not the correct question, for two reasons. Firstly, my goal is neither to understand and evaluate the scientific merits of Lysenkoism and Michurinist biology; and secondly, my goal is nor even to completely understand the historical events. These are unnecessary burdens, as my true goal with this Literature Review is to find the gaps in how people before me interpreted the "Lysenko affair:" those promising omissions. Therefore, a better formulation to describe the direction taken in this review is: What was the perception of the rise and fall of Lysenkoism and Michurinist biology?

Nevertheless, finding those gaps is only possible if I use a different perspective by rendering this literary narrative through my three research questions outlined in the Introduction in a slightly different form. I will look for the occurrence of the "environment and organisms," "humans and society," and "ideology and politics, and science" in the broadest sense to see where they lead me.

#### 4.1. THE POPULAR OPINION

#### 4.1.1. Neo-Lamarckian pseudoscience

The still prevailing, general mainstream opinion of others of what Lysenkoism and Michurinist biology were is easy to come across.

Firstly, they must have had something to do with Jean-Baptiste Lamarck and his theory on the inheritance of acquired characteristics or it is simply pseudoscience. Nabham (2009, 463) writes that "Lysenko and his colleague Michurin built a house of cards on the already discredited theories of Jean-Baptiste Lamarck." A very similar description is of deJong-Lambert's (2007, 1), who writes that "... Lysenko wielded absolute authority in Soviet biology to promote his agricultural techniques, premised primarily upon belief in 'Lamarckism,' or the inheritance of acquired characteristics." Flitner (2003, 179) briefly mentions Lysenko as an "infamous neo-Lamarckian leader." When discussing how Lysenko was assisted by Prezent, an ambitious, "cunning philosopher," to develop his ideas into a coherent whole, Russian science historian Mark Popovsky writes: "Prezent set about providing him [with] a philosophical program [based on] Lamarckism. ... [The Soviet regime already] demanded

that scientists 'revolutionize the life of plants and animals.' [Prezent and Lysenko believed that] Lamarck indicated how it could be done" (Nabham 2009, 464).

Turning now to contemporaries of Lysenko, according to prominent developmental biologist L. C. Dunn, "... Lysenko's popularity was due to the appeal of Lamarckism, and the tendency of the general public to 'grasp any straw that seems to confirm their almost innate desire to have evolution interpreted in this way" (deJong-Lambert 2007, 12). Another figure, geneticist and evolutionary biologist Theodosius Dobzhansky; known for as one of the initiators of the modern evolutionary synthesis of Mendelian inheritance and Darwinian natural selection; wrote to Dunn that "Lysenko was a 'contemptible cheat' who had obtained backing for 'prescientific and at best 19th century ideas'" (Ibid., 15). Another contemporary, though more removed, playwright George Bernard Shaw, in defending Lysenko, opined something similar in meaning, that, Lysenko was a vitalist (Ibid., 16).

Miklós Müller, after giving a brief memorial of Lysenko's 1960 speech at the HAS, finishes Lysenko's legacy describing it as a "false doctrine" (Müller 2011, 1358). Another Hungarian having experienced Lysenkoism, Sándor Igali, uses figurative language: "[Lysenko] poured dialectical sauce on the mystical core, and tried to feed it to the people with force" (Igali 2002).

#### 4.1.2. A political dogma

Secondly, according to the popular opinion, the rise of Lysenko was due to political influence; and Lysenkoism and Michurinist biology were nothing more than the typical example of totalitarian ideology (Marxism, Leninism, Stalinism, etc.) devastating science (genetics and biology). As deJong-Lambert and Krementsov (2012, 378) summarized,

historical accounts (particularly memoirs) of these campaigns in separate countries almost universally resorted to decidedly simplistic–Cold War inspired–explanations of events as being steered by the "hand of Moscow," forcing Lysenko's doctrine upon passive victims of the Soviet regime (particularly its satellites in the Eastern bloc). The "Lysenko affair" was portrayed as a heroic struggle of Western (and occasionally Eastern) "true" science and scientists against the "pseudoscience" espoused by Lysenko.

As an example, evolutionary biologist, and first general secretary of the United Nations Educational, Scientific and Cultural Organization (UNESCO), Julian Huxley, wrote in 1949 that "the major issue at stake was not the truth or falsity of Lysenko's claims, but the overriding of science by ideological and political authority" (Roll-Hansen 2008, 168). Similarly, during the summer of 1953, Dobzhansky expressed at a conference "how the Bolshevik party and the Soviet state had subjugated science to their ends. If anyone had set out to undermine Soviet agriculture they could not have done a better job than Lysenko and his associates" (deJong-Lambert 2007, 21).

Another contemporary, Nobel laureate geneticist Herman J. Muller said (Ibid., 23) in a 1958 speech that

[i]n this crucial area of biology the Russian system of absolute authoritarianism has in fact proved fatal. It has literally killed off their great leaders in genetics, deprived their rank and file geneticists of the opportunity of doing further researches along their own lines, expurgated their curricula, textbooks and periodicals of any treatments of the subject, and brought up a whole generation on totally false biological doctrines.

It was Igali (2002) again who went into figurative details in his memoir when he wrote that "[i]n the blood-showered 20th century of mankind, ideology and politics encroached the natural sciences in a way and extent never before experienced." As to the details, "lacking reliable experimental proof, they cooked up ideological explanations." Justifying his motivation to write this text, he says that he wanted to warn the next generations that "how certain interest groups tried to substitute established scientific knowledge by force with a false and damaging doctrine driven by hidden ideological, political, and economic agendas."

Lastly, in a more cooler tone, science historian Loren Graham observed that Lysenkoism "appealed not only to communist functionaries and the agricultural establishment but also to peasants working the fields," therefore it "was socially constructed both in terms of Lysenko's ideological and personal biases and in terms of its supporting constituencies in the Soviet Union" (Hughes 2000, 350).

The problem with all these reflections is, firstly, they are very eager to take up a reductionist Lamarckism out of context as an explanation for Lysenko's doctrines, whereas Lamarck's theories are more complex and coherent than conventional commentators make it seem by highlighting only one aspect of it. Secondly, these opinions, almost without exception and at least implicitly, expose science as the independent search for truth contaminated by ideology and politics, and only by those; in this way, science and history becomes inherently ahistorical and apolitical, devoid of any connections to the "outside" world (Ferguson 2011, 9). That how false this view is, becomes clear when we consider more elaborate studies on the rise and fall of Lysenkoism and Michurinist biology.

#### 4.2. ENTER HISTORY

Starting from the more simple analyses, firstly, Joravsky (1959, 104) contends that "to regard the texts of Marx and Engels as the chief determinant of this involvement is very much mistaken," that is, the rise of Lysenkoism and Michurinist biology is not due to their inherently political nature, they did not embody Marxist dialectical materialism at the beginning for the sake of asserting themselves: initially, Lysenko was *not* supported by the Party *because* his ideas were in perfect harmony with the distorted Stalinist concepts of Marxism. Instead, "it was the program of 'cultural revolution' that launched Soviet Marxism on its quarrelsome search of an appropriate theory for 'red specialists' in biology; and it was the drive for collectivized agriculture, Stalin's cataclysmic 'revolution from above'" (Ibid.), that produced an opening for what we know now as Lysenkoism and Michurinist biology.

Even though Joravsky details the political ascendency of Lysenko; thereby refuting the comfortable, Western arguments that implicitly claim an unexplainable, irrational insanity in the Soviet leadership for the sole reason of supporting a "madman;" his argument still maintains high political decision-making as the primary factor in Lysenko's rise, which alone, as I will show it later, cannot explain the whole as a historical phenomenon.

A similar account is of Krementsov's, who tells the Lysenko story in front of a Cold War backdrop, so that the mentioned solidification of Lysenkoism and Michurinist biology at the 1948 VASKhNIL conference had, in fact, almost nothing to do directly with their actual scientific merits, nor with their ideological, political suitability for the regime. He (1997, 159) writes: "... by far the most important factor in Stalin's decision to intervene on Lysenko's behalf in July 1948 was the escalating Cold War. Stalin used the competition between geneticists and Lysenkoists as a convenient pretext to announce a new party line in domestic and foreign policy: the final establishment of two opposing camps, Soviet and Western." This intervention "was intended to advance the image of the USSR as the only force for world progress, and to reassert the ultimate authority of the party agencies over Soviet science, expanding this authority into the cognitive content of science itself." This might explain the great effort taken to export Lysenko's teachings to the satellite states (Ibid., 181).

It also sheds a different light on Lysenko, who now turns out to be another puppet used in the show, especially if we take into account how his opening address was carefully edited by Stalin himself, for with the VASKhNIL meeting, "the party agencies had a broader agenda than just the denunciation of genetics: they wished to completely reshape the system of relationships between the scientific community and the party" (Ibid., 168). They wanted "to establish complete control over the community, expanding their power from 'external' (political, practical, and ideological) to 'internal' (intellectual and cognitive) aspects of scientific activity" (Ibid., 183).

Roll-Hansen (2008, 169) draws other conclusions from a later work of Joravsky. According to him, Joravsky concluded that Lysenkoism and Michurinist biology "rebelled against science altogether. Farming was the problem, not theoretical ideology. Not only genetics but all the sciences that impinge on agriculture were tyrannically abused by quacks and time-servers for thirty-five years."

This explanation opens up the field of discussion, as the idea emerges that there might be at least one other type of science, so that even if science is the search for truth, and truth is independent; there is a better, still scientific but different in kind, way toward it, which implies the failure of Western science to maintain its integrity and universal applicability.

#### 4.3. COMPLEX EXPLANATIONS...

Later post-Soviet or other, less mainstream authors, however, started to realize that, in the words of Roll-Hansen (Ibid., 170), the "complex integration of scientific and political establishments implied an intimate two-way relationship rather than simple subordination of science to politics." Kojevnikov goes as far as to say that "under mature Stalinism, the rules of intraparty democracy were extended to science, and scientific issues decided accordingly." (Ibid., 170) This idea, for the first sight, makes the whole "Lysenko affair" seem almost innocent, since, in a sense, it suggests that only radical political democracy is what happened, nothing more.

A more fruitful approach is of Lewontin and Levin's, who, taking Marxism and dialectical materialism seriously, wrote in 1976 that

this contest between the effete middle-class intellectuals, and the close-to-the-soil practical agronomists was subtly extended to include a conflict between theory and practice, a vulgarisation of Marxism. In every aspect the conflict in agriculture was a revolutionary conflict, posing the detached, elite, theoretical, pure scientific, educated values of the old middle classes against the engaged, enthusiastic, practical, applied, self-taught values of the new holders of power. That is why Lysenkoism was an attempt at a cultural revolution and not simply an "affair". (Young 1978)

Even though the exact veracity of class-struggle being the main driver behind this phenomenon can be hardly evaluated today, they are right to denounce the "affair" denomination, since that imposes an ever-conservative perspective looking at new developments, a failure to comprehend history in the making. More importantly, at another place Lewontin and Levins note that "Lysenkoism, like all non-trivial historical phenomena, results from a conjunction of ideological, material and political circumstances, and at the same time is the cause of important changes in those circumstances" (Ibid.). As opposed to the Soviet Marxist type, this truly dialectical understanding; exemplified here; is, according to Young (Ibid.), "a philosophy of nature, persons and society with labour at the heart of its ontology, while the conception of dialectical processes (interpenetrations and mutual constitutiveness rather than simple causalities and mechanical interactions) is an alternative world view to that of the positivism of the integrated conceptions of capitalism and its science and technology."

In this sense, Lysenkoism and Michurinist biology cannot be explained by ideology, politics, Cold War, science, society alone, there must be other factors as well. Let us see now some parts that contribute to this whole.

#### 4.3.1. ... And their details

Roll-Hansen lists numerous events in history which contributed to the rise of Lysenkoism and Michurinist biology. One of them is a July 1931 Soviet government decree that demanded new varieties of grain to be produced in four to five years instead of the former ten to twelve years, which gave impetus to Vavilov's interest<sup>6</sup> in Lysenko's vernalization technique (Roll-Hansen 2008, 174), thus enhancing Lysenko's influence unwillingly. Another event is the introduction of planned economy that demands accountability and reliability even more quickly so (Ibid., 175), thus increasing the pressure on geneticists like Vavilov. Next, in time genetics became enmeshed with Nazi eugenics in the Lysenkoist rhetoric, further contributing to the demise of the belief in Western science (Ibid., 177). Then the Great Terror of 1934-40 removed Vavilov from the presidential chair of VASHKhNIL, putting Lysenko to the top in 1938 (Ibid., 180). This list could go on and on, but we can already see that a great many things contributed to Lysenko's ascendancy.

Ferguson (2011, 12-13) lists other points as well, such as the fact that Lysenko was able to "get things done," whatever this may mean; the circumstance that collectivization and state ideology brought many into high positions; that budgetary constraints fostered the institution of "peasant scientists" as "trained agronomists were not able to be placed at every village" so that "by 1929 there were 23,000 participant peasant scientists working in 'hut labs' throughout the Soviet Union."

<sup>6</sup> The botanist and geneticist N. I. Vavilov is best known for identifying the centers of origins of cultivated plants while personally collecting large amounts of seeds from around the world for the Leningrad seed bank. At this time, Vavilov was the director of VASKhNIL. Later he was arrested on a countryside expedition, then tried and tortured. He died of starvation in prison, and was buried in a mass grave.

Young (1978) lists the following contributing historical factors: "(1) revolution from above, (2) the perceived need for rapid industrialisation, (3) collectivisation, (4) cultural revolution, (5) need to overcome the attitudes and wrecking of kulaks and bourgeois experts, (6) 'on the grain front', (7) war scare of 1927, (8) cold war, (9) xenophobia."

Although these keywords are enigmatic now, taken out of context, their amount and nature implying long, now compressed expositions make us think more deeply, instead of readily accepting the simplistic "self-conscious" and "self-congratulatory" myth about "Lysenkoism as a cautionary tale about the intrusion of the alien values of politics and ideology into the domain of value-neutral science" (Ibid.).

Lastly, it is again Lewontin and Levins who speculated most impressively on the contributing factors:

First, Lysenkoism developed during a time in Soviet society receptive to radical proposals due to the pressing needs of Soviet agriculture. Second, there were strands of non-academic agricultural traditions and practices along with discredited Lamarckian conceptions such as the transmission of inheritance through acquired characteristics, from which to draw intellectual content. Third, due to high literacy and the popularization of science, the debates over theory and method was made a public affair. Fourth, a budding cultural revolution put tension between the youth and exacerbated the view of an elitist academy. Finally, there was a widespread belief in the relevance of philosophical and political issues which kept discourse at the most general level. In addition, these circumstances were nested in a larger, international political context of competition with a rival superpower ... and a repressive and dogmatic bureaucratic and administrative apparatus. (Ferguson 2011, 10)

#### 4.4. CRITIQUE

These lists are certainly impressive and correct to an extent, but evaluating their actual veracity is beyond my abilities and this paper's aims and goals. Despite or because of this, they are meant to represent the sheer complexity of the topic. A perfectly sound justification for my not engaging in thorough criticism of these, besides the just given reasons, is that they all are very similar in one respect: they only talk about politics and history. They perfectly deliver answers to my third research question, but not to the first and the second. Almost none of the publications I reviewed mentioned anything about the environment, nor about humans' and society's relation toward it. This is a problem, and I believe there is a reason for it.

The reasons for being silent about the "environment and organisms", and "humans and society" in the broadest possible terms could be many things. Either the authors did not consider these issues important for further reasons, or they did not consider themselves knowledgeable enough for further reasons to take up these aspects as well. Nevertheless, there might be another answer, that this silence is the silence of the shared discourse; the silence of the approval of the definition and place of the environment and organisms, and humans and society in relation to each other and many other entities. By keeping silent on these issues, almost all the authors unwillingly or unwittingly forestalled the discussion of them, and elevated the discourse to a higher, cultural level. Examining what has not been discussed hugely contributes to the aim of this paper, as this is the justification for my three research questions.

#### 4.4.1. The lack of environment and organisms

Firstly, by looking at the conception of the environment and organisms in the whole phenomenon of Lysenkoism and Michurinist biology, only a few mentions appear, and mostly on the level of Lamarckism and vitalism. As I have quoted before, many authors and contemporaries of Lysenko connected Lysenkoism and Michurinist biology to Lamarck's idea of the inheritance of acquired characteristics so much, that some of them, considering it to be central to the phenomenon, tried to entertain a specific train of thought. As Dunn wrote to Dobzhansky, the "part of the problem was the impossibility of proving acquired characteristics are *not* inherited. This would require demonstrating a universal negative—and such disproof could only ever approach completeness" (deJong-Lambert 2007, 14). Regarding Shaw's defense of Lysenko with vitalism, deJong-Lambert (2012, 174) also implies something similar when discussing the biographical background of Lysenko:

In the winter when the ground was hard and the weather was cruel days might be spent asleep on the stove. Few people could read, or needed to. Life was unpredictable and full of magic. Fairies, water nymphs, demons and devils filled the forests, and everyone feared the evil eye. The season to reap or sow was determined by signs like, "when the tree gets dressed," or a certain bird arriving in the village. Tools and techniques—plows or weeding—brought from the city, caused suspicion or ridicule; worse than evil was being laughed at.

More deliberate accounts mention the environment as a historical factor influencing Lysenko's career in some way or other. Roll-Hansen (2008, 173) notes about vernalization that it had "a serious economic background. Grain was the key agricultural product, and harsh winters that killed the seedlings of winter sowings became a critical problem. This happened extensively in both 1927 and 1928, on the eve of the first five-year plan." Furthermore, though connected to politics, Roll-Hansen (Ibid., 183) writes that: "Like many left-leaning politicians and intellectuals, Stalin had a soft spot for Lamarckian ideas about the malleability of heredity under environmental influence. The mechanistic and somewhat inhumane deterministic taste of classical genetics did not suit his romantic and holistic tendencies. Stalin was also a passionate hobby gardener who felt he, not unlike Lysenko, had an intimate practical knowledge of plants."

Ultimately it was the environment, including living organisms that, as a factor, contributed to the fall of Lysenko, as by the early sixties it was obvious that cluster planting, based on the idea that intraspecific competition does not exists, was not working, and is a waste of resources (Ibid., 186).

On the highest level of discussion, the environment appears closely intertwined with philosophy and political theory. As Young (1978) writes:

Stalinism had within it three congruent struggles which are central to the construction of socialism: the rejection of bourgeois economistic fatalism (which was at the centre of Marx's *Capital* and rightly at the heart of socialist planning), the rejection of biologistic fatalism, and the removal of the recalcitrant experts whose scientism retarded socialism. In this sense, Lysenkoism points to a problem which must be faced by any revolutionary movement which is attempting to socialise knowledge and dismantle the hi-erarchical division of labour ...

One such problem could very well be the presumed independence of science.

4.4.2. The lack of humans and society

Looking at how the concepts of humans and society feature in Lysenkoism and Michurinist biology; even before the advent of Lysenkoism and Michurinist biology, dismissing Morganism and Lamarckism alike; the Communist Academy's newly established Institute of Biology announced a new theme in 1933, "man as a factor of evolution":

... Without considering what man does in various socio-historical formations, without considering what the possibilities are of influencing the animal and plant world in the conditions of a planned socialist economy, it is not possible either to utilize effectively the accomplishments of world science (and above all of evolutionary theory), or to design new researches consonant with a planned socialist economy. (Joravsky 1959, 103)

This means that any biological and genetic theory is *flawed* unless it considers explicitly the cultural and social dimension of human life.

This follows from Marx's 11th thesis on Feuerbach:

Philosophers have sought to understand the world. The point, however, is to change it.

Despite the fact that "modification and intervention in the evolution of organisms for human purposes fit well with the normative conceptions of Marxism, ... [in the case of Lysenkoism and Michurinist biology however,] rather than applying dialectical materialism to produce them, the tenets were born from experience in field and promotion of false results. Lysenkoism was a Marxist science in name only" (Ferguson 2011, 15).

This belief in human power to modify the natural and social environment was the reason, according to Zirkle, why Lysenko and his followers, by pointing to Mendel, "could claim that genetics was a plot by the Catholic church to imply that humans were helpless to alter nature and heredity" (deJong-Lambert and Krementsov 2012, 374).

This idea of humans and their place in the scheme of things served as a perfect posterior opportunity to legitimize Lysenkoism and Michurinist biology as valid Marxist science; even more so, since after a time this tenet became one of the basic qualities of the new socialist man: "Lysenkoism (as Joravsky convincingly shows) was not derived from Lamarckian ideas of the inheritance of acquired characteristics. But it was compatible with them, while they were very attractive in some respects to Soviet creators of a new humanity" (Young 1972).

Just as we have seen in the quick definition of Stalinist socialism earlier, environment and politics formed a linkage. Now here is another linkage, this time between humans and society, and the environment: transforming (human) nature. Lysenko recited Michurin's words: "It is possible, with man's intervention, *to force* any form of animal or plant *to change more quickly and in a direction desirable to man*. There opens before man a broad field of activity of the greatest value to him" (Ibid.). As Graham notes, "by the time of Lysenko's ascendancy in 1948, the slogan 'the transformation of nature' became the basis of a whole programme" (Ibid.).

As Young (Ibid.) notes, the 1949 Chinese revolution's slogan, "Throw off Nature's insolent yoke!" expresses a remarkably similar sentiment, so much that Lysenko's opening address at the 1948 VASKhNIL conference included these lines: "Only on the basis of the teachings of Marx, Engels, Lenin and Stalin can science be fully reconstructed. ... Man is a part of nature, but he must not merely outwardly contemplate this nature. ... The philosophy of dialectical materialism is an instrument for changing this objective world; it teaches how to influence this nature and to change it" (Ibid.).

What are we to make of all this? What was Lysenkoism and Michurinist biology after all? As a matter of fact, for the purposes of this thesis, it does not matter gravely. What truly matters is the validity of the synthetic approach of the three aspects pertaining to science: the environment and organisms, humans and society, and ideology and politics, corresponding to my research questions. These are able to represent this paper's historical phenomenon as it really happened, with all its, in the words of Young (Ibid.), "interpenetrations and mutual constitutiveness." A very different image of science emerges from this perspective, contradicting the general wishful opinion: that science may not be the only way of knowing: not *the*, but *a* truth; and it may not be independent at all while engaged in that.

As J. T. Desanti wrote as early as 1950 on Lysenkoism:

That there is a bourgeois science and a fundamentally contradictory proletarian science means above all that science too is a *matter of class struggle, a party matter*. ... If science is the product of a class, how is one to understand the objectivity of its content? How is one to understand the undoubted *unity of its development*? ... Science is the fruit of human labour and in this labour man determines nature as it is in itself. To transform the thing in itself into a thing for us means to attack brute nature with tools forged in contact with it and to learn by this labour to master it. Now, this transformation is not the work of man in isolation; it uses tools, it is achieved in labour. Hence it is the fruit of the whole social edifice; and hence also the interests of the class whose social activity promotes the productive forces and sustains the form of organisation of labour. ... This explains how the content of science can be objective and yet express the viewpoint of the rising or ruling class. (Ibid.)

During this Literature Review we have seen that, as time passed, the perception of Lysenkoism and Michurinist biology slowly changed, and the field of thoughts broadened from the simplistic, general opinion to very complex, detailed historical explanations. From bloody showers; to maintaining, at least implicitly, that the leaders of the Soviet Union suffered from some type of mental illness for allowing Lysenko to roam free; to seeing this phenomenon in front of a Cold War backdrop; to apply rigorous dialectical, historical materialism, resolutely this time; to even asserting that there was a very different kind of democracy working inside the state apparatus: we have seen it all.

The multitude of questions that Lysenkoism and Michurinist biology raised serves as a proof, or at least provides a great further research impetus, that Lysenkoism and Michurinist biology are really not about biology and genetics after all. Instead, they are about the environment and organisms, humans and society, and ideology and politics; and how these three things interfaced with each other and science—and this has not been researched thoroughly until now. For the rest of this thesis, I will examine how it happened in Hungary between 1948 and 1965 at the Genetic Institute of the Hungarian Academy of Sciences, and will try to find answers and meanings of universal significance.

# 5. Findings

#### 5.1. RQ1: How did ideology and politics influence plant genetic research?

#### 5.1.1. Introduction; and a short summary of history

During researching and writing this thesis I was mainly interested in the scientists' personal perception of Lysenkoism and Michurinist biology so that I can compare those to historical events. This meant that, in the spirit of the interpretivist approach, I relied on the documents and the data that I found: I let it lead me toward a conclusion. This is the reason why, contrary to how I outlined it in the Introduction, I will now deviate from that structure by answering the research questions in a different order, first the originally third research question, then the first, then the second. This enables me to frame the discussion in a more complete way by; besides grounding it in the data that I found; arriving at humans and society at the end, where all three strands actually converged. This is why the main conclusion of my findings is at the end of the third subchapter.

Before my findings, I will provide a brief summary of the political history of the GI through important milestones to make my findings easier to follow by familiarizing the reader with what happened. While examining these events, the detailed history of the GI in the period will be also chronologically narrated, which is the main reason why this subchapter is significantly longer than the others. I will insert analyses and reflections at important points following the descriptive paragraphs, to make comprehension more practical; this is the structure I will adhere to through the whole of the Findings chapter.

According to my sources, after tentative wordings in 1949, the next year introduced the The five-year plan of Hungarian scientific research, and the The five-year plan of Hungarian biological research, both of which made the Lysenkoist and Michurinist mission concrete, repressing genetics. In this period, the GI was forced to include vegetative hybridization<sup>7</sup> in its research curriculum. This era lasted until approximately 1953-54, when talks about the insufficient education of breeders mention the lack and need of genetics at universities. Then the meeting of the Biological Group on June 1, 1956 resulted in an open debate about the past repression, and the freedom that the GI can now enjoy. It is not clear what exactly happened, but in 1958 another debate was held, but this time, the wording

<sup>7</sup> Vegetative hybridization was one of the main tools of Lysenko and Michurin; it is "the phenomenon of the mutual influence of the stock and scion upon each other during plant grafting" (The Great Soviet Encyclopedia, 3rd ed., 1970-1979). Accessed July 17. URL: http://encyclopedia2.tthefreedictionary.com/Vegetative+Hybridization
and the outcome were more careful, talking about coexistence at most, rather than "freeing ourselves from the Michurinist doctrine." After this period of uncertainty, the documents of the early sixties were more reserved, factual in tone, then finally in 1965-66 with the fall of Lysenko in the Soviet Union, genetics returned with full force, and Lysenkoism or Michurinist biology is not even mentioned in internal historical reflections anymore. Let us see now what happened in the beginning.

### 5.1.2. The establishment in the early fifties; and the Institute

In April 1949, the Proposal for the reorganization and development of our plant breeding was released, which asserts that planned scientific work is not pursued on the state's 15 plant breeding sites, a reason of which is the "lack of knowledge and application of modern materialist agrobiology<sup>8</sup> and Soviet experiences." Then it is stated, actually repeated, that in order to complete the outlined tasks, plant breeding has to be reorganized and developed by "ensuring that the work of our plant breeders is based on the most advanced agrobiological science and the experiences of the Soviet Union" (Győrffy? 1949, 1).

In November the same year, The five-year plan of agricultural plant breeding was completed by Béla Rieger, a "plant breeding lecturer," in which one of the targets is "sustaining soil productivity, and increased animal husbandry (forage-grass crop rotation)." Forage-grass crop rotation was expounded by V. R. Vil'iams<sup>9</sup>, and his principles were quickly adopted by Soviet agriculture and Lysenko to a certain degree. Among the "Methods" section, however, there is a much clearer expression: "We have to acquire the methods of the new Soviet agrobiology" (Rieger 1949, 1).

Then The five-year plan of Hungarian scientific research and the The five-year plan of Hungarian biological research were released around 1950<sup>10</sup>, both of which make much more explicit statements than the documents mentioned before. Based on these texts, the following sketch of imagined agricultural, biological, and genetic future can be drawn.

<sup>8</sup> Lysenko called his scientific work agrobiology among other things, presumably because his focus was always the metabolic process between the environment and the living; these latter being cultivated plants.

<sup>9 &</sup>quot;V. R. Vil'iams provided scientific and theoretical substantiation of the positive effect exerted by the periodic cultivation of mixtures of perennial legumes and cereal grasses in fields. Forage-grass cultivation improves the structure and the physical properties of soil (water permeability, water retention, aeration); inhibits the growth of weeds among young crops; increases the yields of grains, industrial, and other crops grown after grasses; and controls crop pests and diseases, especially in cotton-growing regions" (The Great Soviet Encyclopedia 1979). Accessed July 25. URL: http://encyclopedia2.thefreedictionary.com/Forage-Grass+Cultivation

<sup>10</sup> These documents are not dated, but since the five-year plans were always developed in advance, they may originate from 1949.

A principle that has to be followed "consistently" in science is the "vindication of the world view and methodology of Marxism-Leninism, and the increased application of the achievements and methodology of Soviet science in research." It is further explained: "In order to successfully realize our five-year scientific plan, we have to rely increasingly more than before on the achievements of the leading-edge science of the Soviet Union. It is already due to e.g. Michurinist biology that plan breeding research work has become productive" (Anon. 1950?a, 1). Similarly, it is mentioned that for articulating the five-year plan for biology, besides the targets of the national five-year plan, it was the "pursuit that the Hungarian biological research should be practiced in the spirit of the leading-edge Soviet biology" that served as a foundation for the plan (Anon. 1950?b, 1).

How it is to be done? "[t]he recognition and application of the achievements of Soviet science promotes the fight against enemy ideologies, primarily cosmopolitanism," (Anon. 1950?a, 2) as one of the goals of the biological five-year plan is that "it should serve as a support for the purge of idealist theories still present in biology" (Anon. 1950?b, 1). To achieve this, "[t]he recognition, application, and further development of the principles and methods of Michurinist biology [is required] as perfectly as possible," (Ibid., 1) since "[t]he task of plant breeding is to produce higher yielding varieties based on Michurinist methods" (Anon. 1950?a, 12). More concretely, this means the introduction of the grass crop rotation system of Vil'iams, and vegetative hybridization studies (Ibid., 11).

This can only succeed, however, if the foundations of biological research will be the "dialectical materialist worldview, and the advancing Soviet biology, the teachings of Vil'iams–Michurin–Ly-senko" (Anon. 1950?b, 1). Since "it was the dialectical materialist approach in genetics that created Michurinist biology, [t]his leading-edge theory has to steer our genetic studies as well" (Ibid., 5).

But where was the Genetic Institute during these events? The predecessor of the GI was originally founded in 1939 as a laboratory at the Seed Monitoring Station (Vetőmagvizsgáló Állomás), then changed its name the next year, then again in 1942, but this time it became an independent body, called Plant Genetic and Breeding Research Institute (Növényörökléstani és Nemesítéstani Kutató Intézet) (Sárkány 1948, 1-2). During the World War II bombings at Budapest, most of the facilities and instruments were destroyed, but a gradual rebuilding soon started (Ibid., 4-5).

In 1949, however, the Government created a new research body replacing three others, the Agrobiological Institute (Agrobiológiai Intézet), whose name indicated the arrival of new times. The new institute was placed under the control of the Agricultural Scientific Centre (Mezőgazdasági Tudományos Központ) at Martonvásár, (Anon. 1949ab) where it remained until next year when it was transferred to the Section of Agricultural Sciences of the Hungarian Academy of Sciences (Győrffy 1959?, 1). Barna Győrffy, its director at the time, already requested in 1948 changing the name of the Institute to Genetic Institute, but due to the mentioned events, it received a different name and tasks, including vegetative hybridization, (Győrffy 1956, 40) since, as Győrffy (Ibid., 3) writes, the superior body, the Agricultural Scientific Centre believed in the "new approach hailing from Moscow". During the 1950 transfer to the HAS, its tasks largely remained the same, as that body also acted according to the five-year plan's scientific policy. Finally, in 1954, the Institute was transferred to the Section of Biological and Medical Sciences, and received its new name, Genetic Institute, along with new tasks (Győrffy 1965a, 1).

### 5.1.3. The dissenting voices and turning point of 1953-54

According to my sources, it was around 1953-54 when the first publicly dissenting voices could be heard. On the November 9, 1953 meeting of the Biology Committee, Barna Győrffy presented his report on the situation and tasks of domestic genetic research. One of the scientists present, Kurt Sedlmayr, after agreeing to the plan outlined by Győrffy, noted that "he also thinks that the fact that genetics is set aside recently causes really great damage" (Anon. 1953, 3).

A year later, on the December 14, 1954 meeting of the Biology Committee, Győrffy was also participating by saying "We have to clarify what we mean by genetics..." and that "[i]t has to be examined that out of genetics, what we should teach at universities, i.e. there is no genetic education at universities at all. [Clarifying this problem as soon as possible is also important] because graduated experimental stock and plant breeders severely feel the lack of it" (Anon. 1954, 10). Another scientist, also the superior and supporter of Győrffy, Imre Törő, also noted that "greater encouragement should be given to researchers on the field of genetics" (Ibid.).

Two years later, in the summer of 1956, Győrffy said in his presentation on the achievements of domestic genetic research, that "[w]hen the HAS in 1954 set out the development of experimental biological sciences, that also enabled the renewed practice of genetics" (Győrffy 1956, 36). Another document, this time from the enthusiastic year of 1966 entitled The state of Hungarian plant genetics research (without micro-evolution), mentions the 1954 transfer to the Section of Biological and Medical Sciences, noting that "[the Section] granted [the Institution] independence again, and approved the continuation of its genetic basic research" (Daniel 1966, 2).

Enthusiasm or not, both the actual records and the subsequent internal presentation and history writing places the turning point around 1953-54, which is altogether not surprising, since it was a

process that could have been influenced by many factors. One such could have been the 1953 discovery of the molecular structure of DNA, which at least on the side of the geneticists, put a decisive end to the discussion of the scientific merits of Lysenkoism and Michurinist biology.

5.1.4. 1956: "We are liberated from the oppression of dogmatism"

Then, it was in the summer of 1956, as I mentioned, when Győrffy was asked to deliver a presentation on the achievements of domestic genetic research before the Biology Committee of the HAS. Until now, the voices of dissent did not mention the other camp, Lysenkoism and Michurinist biology; they only articulated their own need for space in science. Győrffy (1956, 36), however, was harsh in his critique: "Then came a period, and either the total neglect of the up to date achievements of biological and genetic research; or besides the distortion of those, with highlighting late and now already obsolete authorities in biology, restarting genetic research became almost impossible." He (Ibid.) then said:

In recent years, a peculiar new form of biology was created, which is aptly described by the name "citatology." Many facts of biology experimentally proved were set aside. Relying sometimes on new "experiments" and observations of strongly questionable worth, using often vague and cloudy new terminology, this recitation of "authorities" attacked and twisted the genetics of the turn of the century. And in few short years, it succeeded in causing more severe damage to the Hungarian peoples' economy than the supposed Morganist genetics with the forced execution of its ideas (domesticating lemon and cotton, dispensing with animal registry).

Then the comments of fellow scientists followed, of which I will highlight some. Andor Bálint agreed that in the field of plant genetics, "citatology [indeed] caused severe damage" (Anon. 1956, 9). Later Endre Papp, a plant breeder, said (Ibid., 19) that "breeding hybrids could not find favorable conditions when the freedom of genetic research was impeded by dogmatic prejudice, or cult of personality toward certain researchers." This was an important moment, since according to my sources, it is the first time when politics or ideology is openly but indirectly mentioned in a criticism, and in fact, an explanation of Lysenkoism and Michurinist biology. It proved to be contagious, but we might never know whether it was due it being a widely shared sentiment and therefore a perfect statement, or still out of some fear of retribution.

Nevertheless, András Balázs then talked about "damages caused by dogmatism," and the question of the definition of genes that was "starkly penetrated by dogmatism" (Ibid., 22-23). Béla Pozsár also claimed that "due to the cult of personality and dogmatism, contrary to previous conditions, certain

dismissals were present in cytology also" (Ibid., 28). It was Márk Fodor (Ibid., 30) who made it direct by saying:

In 1948, the great Soviet academic debate—now we not only know, but we can openly say it without any drawbacks—ensured not a scientific but primarily a political victory to Lysenko's school against the position of Schmalhausen and his fellows, who then already dared to accept certain unquestionable teachings of Western biology.

Árpád Virág continued with the damages caused by dogmatism (Ibid., 34), then at the end, Rezső Maucha (Ibid., 42), chairman of the debate, closed it with a cheerful note that received great applause:

Due to the shortage of time, I only would like to highlight the thought, the emphasis of which was the aim of this presentation, that due to the resolutions of the XX. Congress pertaining to the cult of personality, we are liberated from the oppression of dogmatism. ...

### 5.1.4.1. Critique

Despite the conclusion's positive, unifying force, there were some voices of dissent from even this side, all of them before Papp's contagious comment about dogmatism and the cult of personality. So now the credibility of the closing statement remains in question.

If now biology and genetics are free from the suppression that caused severe damages, how is it that Andor Bálint said that "during these four-five years, the development of genetics [could not be] seriously hindered, even less so since it is director comrade Győrffy who knows it best that no administrative body could influence the curriculum and work of the Genetic Institute" (Ibid., 9)? Were there damages then or not? Igali (2002) certainly claims it in his paper.

Even more interesting is a third approach that seems to go against the conclusion's definite dismissal of Lysenkoism and Michurinist biology. Bálint also said a bit later that "in essence, it is clear for all of us, that here are two schools, two theories facing each other" (Anon. 1956, 10). Later on, he concluded: "I think it is correct if adherents of both schools starting out from their own theoretical base, but using methodological principles genuinely, mutually accepted, design their experiments, so that these experiments will be verifiable, reproducible, and will satisfy all scientific standards" (Ibid., 12).

If I would be engaging in speculation, I would say that Bálint wanted to save some space for Michurinists, among whom he may have belonged (Ibid., 11), in an embarrassing situation where it was openly discarded. Nonetheless, some other comments also deserve attention, as they shed light on how the particular discourse of Western genetic science imagined itself in relation to other things.

Let us consider Győrffy's statement, in parallel with Bálint's previous one, in his presentation that "domestic vegetative hybrid originating from grafting conforming to the principles of genetic experiments—unfortunately—does not exist today" (Győrffy 1956, 41).

Both of these explicit or implicit claims for mutually accepted, high scientific standards are misplaced, as the point is that such standards do not exist in Lysenkoism or Michurinist biology, therefore they are not reasonable expectations from the geneticists' side. It is not that Lysenkoists simply do not use the scientific method, so they practice bad science, it is more than that: the methods of Western science are *not missing*, but simply *do not exist* in the Lysenkoist discourse, because it is, let us say, a "qualitatively" different *science*. This does not mean that Lysenkoism and Michurinist biology is worth the same as Western genetics in their pursuit of absolute material knowledge abut the world; or their efforts to serve society, when it comes to judgment by practical evidence. It merely means that, as two discourses aiming at the same goal, they were and are on the same level of historical consideration, therefore the geneticists' claim to establish mutually accepted scientific standards is already value-laden, since it is only Western science that has those standards in question.

Another, "pre-dogmatic" comment could be connected to this thought. Béla Faludi (Anon. 1956, 7) said that:

I see the greatest danger ... in that, though starting out from different perspectives, but damaging science anyway, we unhealthily simplify, turn important questions into primitive ones, either in the form of personalized equations, either in treating questions already decided, either in citatology, or [in any other way].

On the one hand, this comment seems rather sober and reflective, but on the other one, this could also be a very diplomatic attempt at a desperate reconciliation between the two schools of thought. Nevertheless, Faludi talks about unhealthy simplification, which is nothing else than a form of Latour's black boxing, since exactly, by "treating questions already decided," we are no longer interested in its internals, only in its inputs and outputs: a (scientific) theory becomes a package we filter the "real world" through, and see if it matches what we already thought about it. This silent acceptance of collective biases is valid for both sides, and, as I have shown, it was very prominent in the geneticists' expectation of mutually accepted scientific standards or methodology.

Furthermore, this comment implicitly assumes that science is a separate entity that people from the outside or from afar can damage by, for instance, simplifying it. The assumption is that science has an ideal form of complexity that should be treated reverently. Approaching from the perspective of my Theoretical Framework, however, this assumption proves to be false. It represents a static, dualistic concept of science, instead of a science that is in the making; and can, yet cannot, be separated from those who are making it, who are in turn, similarly can and cannot be separated from their social and natural environments that are molding them, and that they, in turn, also mold.

Notwithstanding, the year 1956 proved to be a success for Western genetics at the GI, its victory, or at least liberation, was proudly and openly announced after the oppression of "dogmatism and the cult of personality" that granted only, according to those present at the debate, a "political victory" to Lysenko's school. Science; though ridden with its invisible, historically conditioned, contingent biases, and black boxed concepts; triumphed with a capital s.

### 5.1.5. The 1958 Genetic Debate: Restoration

The next milestone in the course of events was the October 9, 1958 Genetic Debate, as it was called in the database of the HAS Archives. The question of Lysenkoism and Michurinist biology was again on the agenda.

On that day, the management of the Biological Group organized a meeting inviting the Genetic Committee, reasoning, that a month ago, on the 1958 September meeting of the Genetic Committee, many questions were raised and forwarded to the Biological Group, which should be cleared up before the upcoming great convention where, among others, the Genetic Institute would present its work in front of the HAS directorate (Anon. 1958, 1–2). After reading the Genetic Committee's and Győrffy's report from the previous occasion, chairman Imre Törő gave the floor to the comments.

I will first highlight the comment of Sándor Rajki, then the director of the HAS Martonvásár Agricultural Institute, who in the meantime returned from the Soviet Union after receiving Lysenkoist education. According to him, "[i]n general, one of the obstacles of the development of genetic research in the period before 1953-54—including Michurinist and the formalist genetic schools—was the one-sided science policy of administrative bodies" (Ibid., 32).

Bruno Straub F. joined to Rajki's opinion when he said: "The particular [directorates of the Academy] committed mistakes for years without doubt when they, let me say this way, spread the Michurinist school with fire and sword, without knowing its substance and experimental proofs; and this includes me as well. I believe that we are still at this point..." (Ibid., 81–82).

A possible old and new solution to this old and new problem arrived from Andor Bálint, who remarked (Ibid., 13–14-20) that:

[There are two schools] ..., and our standpoint is that let everyone work according to his own understanding, and according to that, bring something scientifically new ... [We should] respect everybody's own standpoint and own opinion ... [since] it is possible to interpret, explain [things] from two understandings ... We think that both schools should produce things scientifically as serious as possible, and things as meticulously conforming to standards as possible. As a matter of fact, I already said this to the Academy at the 1956 genetic debate. ... Some have this standpoint, some that one, and there is no point debating this issue now.

Rajki was also expressing a very similar sentiment: "Let us all experiment, let these geneticists experiment, let those ones as well ... because this is the only thing that helps the case of solving problems" (Ibid., 33-40–34-40).

Finally, it was Törő who concluded the discussion: "If we want to summarize … the platforms we agree on, [then it is this]. That there is one genetic science. And then, that ultimately, there is one scientific truth that they try to resolve and solve from different sides … There is one genetic science, and two methodological approaches" (Ibid., 111–112).

So what was different this time, compared to the "liberation" of 1956? Firstly, Rajki was present, and, to an extent, dominated the discussion as we will see soon, but moreover, he already had a very influential position at the country's leading plant breeding institute; he was not afraid of any kind of repression. His infamous moment came early in the debate when he (Ibid., 42.) said that:

It is not the Michurinist genetics, its development that the statement, that there are two principal schools of genetics in genetic science, the straightforward admission of which, will benefit, because we will work independently of whether on the other side they say that it exists, or they say it does not. I, and allow me to speak my own opinion, have not so many doubts about the future. To say a political simile, it would be better for the Americans to leave Taiwan now, and not to wait until they are bundled off. But this is such a thing that will be decided by experiments and facts at the end. So let us experiment, and form our opinions according to the facts.

It was a markedly different atmosphere than in 1956, so much that what happened, and Rajki's particular political simile, haunted Győrffy even a year later when he wrote, also mentioning Rajki before, that: "It is hoped that the currently muddled mood will settle down, and the healthy atmosphere will be created ... Because the same uncertainty spread around about genetic research again that was present until 1954" (Győrffy, B. 1959?, 11).

It was a different stop on the way secondly, because now there was only one scientific truth. Even though the concept of the two schools were articulated again, explicitly uniting them under one truth, and not any truth, but *genetic* truth this time, meant the formal restoration of Lysenkoism and Michurinist biology besides the dominant, "formal" genetics. The latter was not suppressed again, but the aura of scientific freedom was sensibly gone with lifting Lysenkoism and Michurinist biology to the same level of intellectual merits than that of formal genetics'.

It was a significant event when looking at it through the lens of my corresponding research question. Győrffy observed very early in the discussion that: "It has been said many times that genetics is a political science. What about this question? ... It has been said many times that genetics can only be practiced with ideology and politics. We have to look at this question. Because I always denied that this would be the case, but it came up many times" (Anon. 1958, 21).

Rajki was quick to articulate his disbelief: "It should be asked of whom said it" (Ibid.). He continued by saying: "Such a statement was never uttered by me or Professor Bálint" (Ibid., 22).

Even more interesting is, however, what Törő replied: "There is some politics in every science. No science exists without politics. So, without this, meaning worldview by politics, there is no science in which it does not manifest. But of course, saying that in any science, political questions dominate, is not correct" (Ibid.).

Rajki agreed: "Section Secretary comrade Törő is completely right that certain conclusions of a worldview are possible to be drawn. As it is possible from nuclear physics or biochemistry ... But it does not mean that genetics is politics. That genetics is politics, that is a terrible vulgarization, and unscientific, incorrect statement" (Ibid.).

Even though both Győrffy and Rajki seemed to represent the general, mainstream opinion on the relationship between ideology and politics, and science that I have shown in the Literature Review, Törő certainly understood the core of something similar to what I outlined in the Theoretical Framework. Nevertheless, he was not completely right in implying that it is only the worldview that constitutes politics in science. The political is much more, it is an embeddedness probably impossible to circumnavigate. As Lewontin (2007, 229) writes: "So the limitations of our conceptual schemes dictate not only the form of our answers to questions but which questions are allowed to be 'interesting."

The relationship between politics and genetics was not settled, however. Bálint (Anon. 1958, 122) later returned to it:

<sup>[</sup>The point is] that even between geneticists and non-geneticists, the relationship toward the people's democracy is a political question ... Therefore someone getting to the foreground or background is not due to him being Morganist or not Morganist, but the proper stance toward people's democracy is the same requirement in the case of a geneticist, teachers, or other professionals, who represent a responsible position in people's democracy and possess appropriate scope of authority.

This is a more refined approach than Törő's, though delineating the boundaries might prove to be very difficult, if not impossible, in a statist country; yet it finally recognizes the political embedded-ness of people in society.

Just a few sentences later, however, he contradicted himself. In 1957, a year before this debate, Béla Jankó, a doctoral candidate, when attending his dissertation defense, said something harsh along the lines that Lysenkoism is a political or ideological science. Referring back to this event, Bálint (Ibid., 123-130) continued:

The other thing, if someone, [i.e. Jankó] speaks like this about Michurinist genetics in the March of 1957, that is a political question. This is obvious, that after the counter-revolution, making a stand there with such words ... [it is] a stance against people's democracy in that situation and that environment ... If it would be remarked by Barna Győrffy in the Genetic Committee that we should look at these questions, that is a different thing. But to bring this up that way in front of plenary crowd in the March or April of 1957, that is a political utterance.

This quotation proves that it is indeed the people, and the historical circumstances they are situated in and partly constituted of, upon whom the fate, or veracity for that matter, of a statement depends, as Latour (1987, 27) noted. What is very difficult to explain, however, is that if they were already conscious of it, why did not they applied it to their situation? One speculative answer would be that they still believed in an ideal science for its own, or other very noble goal's, sake, and considered their situation a temporary deviation. But it is precisely accepting deviation as part, yet not a part, of "normality," and by temporariness, a different temporal perspective, that is implied by the process-based dialectical thinking. Echoing Lewontin and Levins (2007d, 187): "Why are things the way they are instead of a little bit different? Why are things the way they are instead of very different?" Not accepting deviation as part of normality is still one-sided, dualistic thinking that believes that every whole could be readily assembled from smaller parts; deviation is a part to be discarded as not fitting into the system.

5.1.6. The comparative dialectical materialism of the early sixties

After this temporary and maybe unexpected restoration of Lysenkoism and Michurinist biology that was presumably due to Rajki's influence; and the 1956 Revolution and its consequences pertaining to the political climate, almost right after the proclaimed liberation from the "dark phantom" (Igali 2002)—a different period began. Or, let us say in a dialectical fashion, the new period could have been only as new as much as it was separated from influences of the past and its own present. With the same logic, the 1958 restoration represented only a point of convergence that otherwise ensues all the time. It is significant only because it synchronized the present-times of important social actors, thereby focusing energies on modifying the future more effectively either in the short or long run.

The early sixties therefore give the impression of quiet work with a more official and stern restoration of Marxism and dialectical materialism in its research rhetoric.

I came across in the archival documents a curriculum sheet, presumably from 1960-62, that lists the topics to be researched in the next years under the direction of Győrffy. These include, for instance: "The analysis of the emergence and preservation of heterosis, and the development of methods of prediction"; "The study of heterosis phenomena in polyploids"; "The study of the inheritance of yield quality"; "Mammal zygote cultivation in in vitro conditions"; and "The unity and struggle of opposites in genetics" (Anon. 1960?, 1-2).

With the exception of the last one that is more philosophical, all of them are "regular," that is, Western-type, biological or genetic studies, mainly experimenting with, or explaining, hybrids. Nevertheless, there were other topics listed as well, which might allude to the atmosphere at the GI at the time. These include: "The study of the theoretical foundations of vegetative hybridization"; "Comparative analysis of heterosis phenomenon and the law of segregation in vegetative and generative hybrids"; "The effect of selection and controlled cultivation on the generation of quantitative traits"; "The theoretical and methodological analysis of the two schools of genetics"; and "The philosophical analysis of the biological quantitative and qualitative changes" (Ibid.).

Furthermore, on the separate description sheet of one of the topics called "The analysis and criticism of the molecular genetic concept," in the "Targets" section we can read: "Due to contradictory opinions in connection with the evaluation of the methodology of different genetic schools and their experimental results, it is timely that we criticize and evaluate the fundamental, and from an ideological aspect, often problematic questions of genetics based on dialectical materialism" (Anon. 1960, 1).

What is common in the titles of the topics and the description of the previous topic? Firstly, by treating their subject seriously, at least on the surface; they strive to conduct comparative studies and experiments in order to reach a definite conclusion. Judging only by the titles and some descriptions, this is exactly what has been decided by the Lysenkoist faction at the 1958 meeting: "Let us experiment, and form our opinions according to the facts" (Sándor Rajki, in Anon. 1958, 42).

Secondly, even though vegetative hybridization and the two schools of genetics are mentioned, there is no reference to either Lysenko or Michurin. This suggests a more "objective," that is, less personal, approach toward genetics where only the cool facts and the laws of nature will decide things, not heated, personal arguments as before. The previous topic description, although it did not mention, is based on mutation experiments, something that was forbidden in the early fifties according to Lysenko's teachings. This cool and reserved atmosphere is in spite of the fact that it was in the January of 1960 that Lysenko visited Hungary and gave a lecture at the HAS where he received serious questions. Igali (2002) proposed that it was ultimately Lysenko himself, who disappointed his ardent followers at the lecture.

Thirdly, the theoretical framework they mention to be used is dialectical materialism or, at other cases, Marxism, but not Marxism-Leninism, or the Vil'iams-Michurin-Lysenko approach. This might be explained with the political changes due to the succession of new First Secretaries in both states, Hungary and the Soviet Union: the cult of personality, and the constant reference to personal authorities was perceptibly over this time in science.

### 5.1.7. 1965-66: "The switch from 'Lysenkoist genetics' will be considerably difficult"

The final stop in the history of the GI discussed in this thesis are the years of 1965 and 1966.

On September 27, 1965, Section Secretary Bruno Straub F. asked Győrffy to compile with the Genetic Committee a summary about the current situation regarding the genetic debates in order to present it to the HAS directorate. Lysenko fell along with Khrushchev in 1964, and by 1965 he was removed from his academic position as well, after a joint meeting in September between the directorate of the Soviet Academy of Sciences, and of VASKhNIL, and the College of the Ministry of Agriculture. His ideas were denounced, and the formal restoration of genetics quickly took place to cope with Soviet agriculture lagging behind. Therefore, in my studied period, 1965 and 1966 would be expected to be the times of the most open, yet maybe also victoriously one-sided, speech.

Accordingly, in the summary written by Győrffy and his associates (Győrffy 1965b, 4), we can read: "It generally describes the debate against genetics that against genetic dogmatism, they always threw dogmas to the head of others, and there were some whose skulls were crushed!"

At another place (Ibid., 2): "The second movement of politicogenetics [i.e. the first one was Nazi eugenics] of the recent past ... was represented by Lysenkoism, which, by partly distorting, contrasting in an obsolete fashion, and artificially magnifying, and partly passing the questions to the ideological, political field, resulted in non-professionals recognizing 'two genetic' schools."

Half a year later, on March 29, 1966, the directorate of the HAS held a session discussing the document called The report by the Section of Biological Sciences on the state of genetics. It is written

about Lysenko that "he positioned his school as the only authorized and faithful follower of Darwin, which is factually wrong ... he distorted the opinion of Marx-Engels on Darwin" (Anon. 1966, 4).

It is remarked that "[After the 1948 debate], the acceptance of Lysenkoist teachings became the trial of attachment to social advancement and the Soviet Union. In the conditions of the given period, the genetic debate falsely and unfortunately became a political struggle" (Ibid., 6).

Regarding what people thought, "Even though the media and the official statements asserted it, the not geneticists, yet professionals working on the field of applied genetics did not really adopted it. Most of our geneticists currently stand on the basis of classical genetics" (Ibid.).

Regarding the role of higher political bodies, "In our country, significant restrictive administrative measures pertaining to certain individuals due to their stance on genetic questions did happen in the previous era either, therefore rehabilitation is not needed" (Ibid.).

In summary:

There is no Eastern or Western genetics, there is no reactionist and advancing genetics, idealist or materialist genetics. There is only one genetics, the practitioners of which could have belonged, and still can, to different schools. As much as it was incorrect to proclaim the so called Michurinist school as the only materialist biology, the other understanding is as incorrect, according to which, when interpreting the facts of the classical or even modern genetics, idealist schools cannot appear. The criticism of these still remains the part of our task. (Ibid., 8-9)

About the future, it said that "The switch from 'Lysenkoist genetics' will be considerably difficult ... Therefore, in the new educational material, it cannot be left without commentary, but it should be critically evaluated" (Ibid., 12). Furthermore, "[it is important that from now on,] research should receive support corresponding to its scientific value" (Ibid., 13).

### 5.1.7.1. Critique

Regarding the 1965 summary compiled by Győrffy and his associates, it clearly states that Lysenko and his followers repeatedly strayed from "strictly scientific" questions and diverted the discussion toward statements that could easily have direct ideological or political connotations, and used those as dogmas to justify their inquiry. This statement corresponds to the mainstream, general opinion about Lysenkoism by refusing to admit the possibilities that firstly, there could be more than politics around Lysenkoism and Michurinist biology, and secondly, that there could be something equally deeply biased in their own approach. Not admitting as little at least; that their graduate and doctoral education to become professors and academicians of biology and genetics was deeply conditioned by social circumstances, an environment where space is given to such admittedly high-prestige occupations; is due to them seeing their position as part of the mentioned normality, not deviation. A speck of irony in this is that the summary compares the ideological, politicized nature of Lysenkoism to Nazi eugenics, just as Igali (2002) did, whereas it was Lysenko first in the thirties who made same claim but with genetics (Flitner 2003, 179–180).

The 1966 report reinforces the political character of Lysenkoism and Michurinist biology again by contrasting it with higher, mutually accepted and revered authorities as "Marx-Engels." Furthermore, the oneness of genetics is also stated again after 1958, however, this time in a different modality. As I see it, in 1958, it was used for the restoration of Lysenkoism and Michurinist biology to the same level of intellectual merit of genetics granting legitimacy. This time, however, by explicitly describing what sorts of genetics do not exist, this seems to be an effort to get rid of the perceived ideological and political overtones associated with the *practice* of science. It is important to note, that it is the practice of science that was corrupted by ideology and politics here, not science itself. This is illusory: it makes one wonder what kind of science is the one that is not put into practice some way.

In a curious historical coincidence, Lysenko said almost the same six years before at the HAS lecture, though he clearly made a value judgment between the two schools:

It is clear for all of us, that in the natural sciences there can be no Western or Eastern science. Both in the East and the West, in one room, two different people can interpret facts differently. So it is not about East or West, but about the interpretation, understanding of phenomena, and the explanation given to them. And the understanding of phenomena can vary. It can be such, that we possibly take into account all circumstances and all realities. We call this kind of understanding of phenomena dialectical. There could also be another understanding of the same phenomena ... For instance, looking at a chicken or a plant as if it were some fossilized thing that does not change, that is not altered. This is one-sided understanding. This is metaphysical understanding. (Lysenko 1960, 22–23)

Nevertheless, judging by the sources I acquired, the geneticists still did not understand the discursive difference between their science and that of the Lysenkoists. Even if setting out that "research should receive support corresponding to its scientific value" enables Lysenkoists to continue their work, since they still can receive support if they are deemed worthy, such a statement inevitably blurs the difference between the two sciences that are not compatible with each other, as they do not use the same vocabulary, they do not even operate with the same natural entities. It is again a sign of unifying different contexts and contents into one melting pot, thus eliminating historical contingency, trying to act as if the medium between the "real world" and us, described by Latour (1987), would not exist.

A few months later in June, the directorate of the HAS released Resolution no. 14/1966. on the state of genetics that summarized and made the contents of the 1966 report official. It highlighted the oneness of genetics, the mistake of considering Michurinist biology as the only correct materialist biology, the importance of applying a Marxist perspective to evaluate the ideology of the schools of genetics, and the fact that violent administrative tools were not used to repress genetics (Rusznyák 1966, 1).

Nonetheless, back in February, Győrffy already saw the initial version of the report. In a letter to Section Secretary Bruno F. Straub, dated February 25, 1966, he noted that, contrary to the report's opinion, there were administrative restrictions:

It was a significantly restrictive, and dictatorially administrative measure that, as a consequence of the April 28, 1959 monitoring of the Biological Group, personnel changes occurred in the Genetic Institute due to the inappropriate political and professional standards, and [because] "the Michurinist genetics has to be represented in the curriculum." (Győrffy 1966, 1)

Despite the continuously political nature of the times, when one had to be careful not to write down anything that comes to mind, I would consider this letter as one of the most honest ones, unless it is emotionally driven, the truth of which I cannot verify. If it is indeed honest, then it clearly states what I only alluded to so far, that after 1958 debate, the formal restoration of Lysenkoism and Michurinist biology did in fact happen, and that after the initial period from 1949 until 1953-54, the GI again operated for a time with prescribed research curriculum. It turned out that Győrffy's fears about the "uncertainty" of the times I mentioned earlier proved to be correct.

Győrffy also noted in this letter that it is not correct to talk about two schools of genetics, as Lysenkoism and Michurinist biology only related to certain, narrower aspects of it, and "ultimately, it was a political school" (Ibid.). The problem with the two schools for him was that "[If the 'school' stays,] then we hardly arrive at the required purge of Lysenkoism" (Ibid.). He suggested using the word "concept" instead.

In spite of this passionate defense of genetics against an ideological and political "concept" that used "restrictive, and dictatorially administrative measures" to vindicate its place at least on the same intellectual and institutional level that of genetics, on the bottom of this letter, there is a handwritten sentence by Győrffy that was later completed and typed onto the other side of the paper. It reads:

This fight of worldviews, especially in the beginnings of the 1950s made itself felt here at us also, but rather only on the philosophical plane. Fundamentally, it did not restrict, and maybe did not make impossible for, any biologist not practicing based on the Michurinist concept, to work. (Ibid., 2)

So what about that "significantly restrictive, and dictatorially administrative measure"? One of the already mentioned scientist working under Győrffy, Sándor Igali (2002) would contend such easy-going summary of the events, unless the Genetic Institute was a truly exceptional place in those times.

He (2002) writes:

They did not make it [i.e. Lysenkoism and Michurinist biology] accepted by professional persuasion based on experimental facts, but with the ideological, political, and administrative coercion of the communist party ... The tools of the Lysenkoization of genetics were the pseudoscientific propaganda; and the central planning of publications, education, and research ... They forced the new school of thought into research with plans based on, and approved by, the central principles ... As a result, Lysenkoism became an absolute dogma in the fields of science, education, public education, print media, radio, etc. ... The new Lysenkoist doctrine, as a compulsory ideology, reached every basic body through the communist party's dictatorially centralized apparatus ... Geneticists were threatened, oppressed, silenced, slandered, dragged through the mire, persecuted, dismissed from their jobs ... They only left some internationally renowned professionals as directors who could not be substituted.<sup>11</sup> Lysenkoists were the directors of research institutes, heads of university departments, the secretaries of academic sections, [and] decision-makers at ministries and authorities.

### 5.1.8. After 1966; and a one-time reminder

What happened at the GI and the HAS after 1965-66? Apparently, scientific research "regained" its "lost freedom." An Appendix to an, at least for me, unknown document, presumably from 1965 or early 1966 lists the most significant achievements of the progress of genetics in the last 15 years. There is not a single mention of Lysenko, Michurin, or their followers, nor any about vegetative hybridization, and so on, neither in the international section, nor in the Hungarian. Reading this one document, it seems as if genetics finally and irrevocably triumphed. There is one entry listed, however, that says otherwise: "Vernalization of spring wheats (S. Rajki., submitted for doctoral dissertation)" (Anon. 1965?, 4).

It is enough for us now to know that Rajki's defense in 1966 was an infamous, memorable event often recited later on. It lasted for a whole day provoking furious debate when the academic commission finally decided to end it at one point, which was against regulations. Rajki, along with his wife,

<sup>11</sup> Barna Győrffy could have been such a one. This would explain the exceptionally peaceful operation of the GI, compared to most of the country's institutes.

continued to conduct vernalization experiments until the end of his professional career, 1983, as the director of the Martonvásár Agricultural Institute, even though the failure was already evident in 1962-63 (Balla 1999).

Barna Győrffy lead the Institute until his 1970 death.

### 5.1.9. Conclusion and critique: Discourses and Scientization

To conclude, and give the final evaluation of the relationship of ideology, politics, and science in the case of the GI and Lysenkoism, my stop in 1966 is an arbitrary decision as far as it would be advisable to process the whole history of the presence and perception of Lysenkoism at the GI and the HAS. Due to the shortage of time and my abilities, I did not intend to cover Rajki's defense, the documentation of which alone, if I remember correctly seeing it in the HAS Archives, numbered several hundred pages. I also believe that it would be an unnecessary enterprise, since approximately after that moment<sup>12</sup>, Lysenkoism and Michurinist biology were not important factors anymore in genetics, international or domestic.

Furthermore, as far as process-based dialectics goes, any history writing is arbitrary, as much as it highlights certain things, and leaves others in the obscurity of continuous, directionless convergence and divergence. It therefore differentiates, that is, it creates dualities, often without acknowledging this act; and rather than providing an "honest," "objective," or even "true" account of what happened, it creates a narrative favorable to certain interests. Those interests may be conscious or unconscious. It was precisely the importance of that "unconscious," the sociocultural and historical embeddedness that I emphasized in my Theoretical Framework, and that I now have to apply, or at least accept, to my work as well.

Nevertheless, the image drawn from this subchapter is both surprising and not surprising. It is not surprising, because the way leading figures at the GI and the HAS perceived Lysenkoism and Michurinist biology is very similar to those early reactions mentioned in the Literature Review. As deJong-Lambert and Krementsov (2012, 378) summarized it,

Historical accounts ... in separate countries almost universally resorted to decidedly simplistic–Cold War inspired–explanations of events as being steered by the "hand of Moscow," forcing Lysenko's doctrine upon passive victims of the Soviet regime (particularly its satellites in the Eastern bloc). The "Lysenko affair" was portrayed as a heroic struggle of Western (and occasionally Eastern) "true" science and scientists against the 'pseudoscience' espoused by Lysenko.

<sup>12</sup> Of course, there was no such moment if I take the things written in my Theoretical Framework seriously, since moments are arbitrary constructions.

## Or take another, highly critical opinion of this perception, this time it is of Young's (1978):

[It is the] crude use/abuse model which has characterised the writings on the subject in both East and West. [It is necessary to look] deeper than the liberal-to-reactionary scientific self-consciousness which sees the history of Lysenkoism as a cautionary tale about the intrusion of the alien values of politics and ideology into the domain of value-neutral science. Hitherto everything I have read on the subject has taken this self-congratulatory line.

Indeed, as we have seen, almost everybody in my sources took this position, defending an ideal of science, and treating the slightest departure from the unwittingly socioculturally conditioned, thus obviously expected, as a temporary deviation to be discarded. Their failure to realize their own deeply contingent positions might stem from the static and dualistic perception of science and themselves as *entities* engaged *in study*, who are different from the *studied*, but also from the *study* itself. This is an "impossible" condition, as it alienates the practitioners from the world by creating an objectifying gaze,<sup>13</sup> most of the time devoid of professional and human responsibility; and, to a certain extent, duty as well. Out of all the 542 archival pages I read, none of them contained any attempt, honest or not, to understand what Lysenkoism and Michurinist biology actually was at the time.

What happened at the GI is a good example of a clash between two discourses, neither of which conscious of this fact about itself, but both striving to reach supremacy in some way, yet completely failing to do so due to the incompatible black boxes they operate on a daily basis: the black boxes they themselves have become.

According to the historically more enlightened authors cited in the Literature Review, including Young (1978), and Lewontin and Levins, Roll-Hansen (2008), and Ferguson (2011), despite the popular conception, Lysenkoism and Michurinist biology were not spawned by the "hand of Moscow." It emerged at a particular time of crisis in Soviet history, when the old culture or system was not working "well," that is, as then expected. Later it was exported due to a probably multitude of reasons, that is, it was uprooted, using indeed ideological and political power, then it failed due to the lack of the exact context of its inception and since. What the leading figures at the GI failed to do so, in writing at least, is differentiating between the inception and the export.

It is stimulating to see this through Beck's (1992) perspective of science and society (politics). Even though Beck writes about late postmodern 20th century, his thoughts can be applied to the case of Lysenkoism and the GI, to a certain extent.

<sup>13</sup> Cf. something very similar in Scruton 2009.

In the chapter "Science Beyond Truth and Enlightenment?" of his book *Risk Society*, he (1992, 155) defines two scientization, primary and reflexive:

At first, science is applied to a "given" world of nature, people and society. In the reflexive phase, the sciences are confronted with their own products, defects. ... [When reflexive scientization happens,] "the sciences are now being confronted with their own objectivized past and present – with themselves as product and producer of reality and of problems which they are to analyze and overcome. In that way, they are targeted not only as a source of solutions to problems, but also as a *cause of problems*. (Ibid., 156)

Both Young (1978), and Lewontin and Levins (Ferguson 2011, 10) mention the "attitudes of bourgeois experts" and the "elitist academy" as one of the factors contributing to the rise of Lysenkoism and Michurinist biology. Focusing only on this aspect, Lysenkoism becomes the product of an unsatisfactory primary scientization of tzarist Russia, and emerges as reflexive scientization.

As a consequence of reflexive scientization, two important things happen, among other things. Firstly, "a momentous *demonopolization of scientific knowledge claims* comes about: science becomes more and more *necessary*, but at the same time, *less and less sufficient* for the socially binding definition of truth" (Beck 1992, 156). This demonopolization "*immunizes* socially prevailing ideologies and interested standpoints against enlightened scientific claims, and throws the door open to a feudalization of scientific knowledge practice through economic and political interests and 'new dogmas'" (Ibid., 157).

Secondly, "even the *foundations of scientific rationality* are not spared from the generalized demands for change. It is precisely reflexive scientization that makes the self-imposed taboos of scientific rationality visible and questionable. The suspicion is that 'objective constraints,' ... are themselves *manufactured* and thus are in principle *solvable*. The project of modernity, Enlightenment, is unfinished" (Ibid.).

This is exactly what happened with the rise of Lysenkoism and Michurinist biology. It is interesting, however, that Lysenkoism also had the characteristics of primary scientization. As Beck (Ibid., 155) writes:

Primary scientization gains its dynamism from the contrast of tradition and modernity, of lay people and experts. Only under the conditions of this demarcation can the *skepticism* in the internal relations of science be generalized at the same time as the application of scientific results is advanced in an *authoritarian* fashion in external relations. This constellation of an unbroken faith in science and progress is a characteristic of modernization in industrial society into the first half of the twentieth century ...

The authoritarian execution is certainly true of Lysenkoism, as well as the contrast of tradition and modernity that serves as the source of dynamism; except in this case, it is a contrast turned upside down. Lysenkoism emerges as the dominant force, despite the fact that it drew intellectual content from "non-academic agricultural traditions and practices along with discredited Lamarckian conceptions" (Lewontin and Levins, quoted in Ferguson 2011, 10).

Nonetheless, this unbroken faith in, I shall add, "objective," science is equally true of modern genetics and "Western" science of the period, and we can interpret the resistance toward, and eventual rejection of, Lysenkoism on the part of the GI as a defense of the values of modernity and Enlightenment; as a moment of primary scientization.

Lastly, why did not Lysenkoism and Michurinist biology work out at the GI? *Looking back* on it now, it is undeniably true that it was forced upon the country and, to an extent, and here accounts differ as we have seen, the GI through ideological and political manipulations. Investigating, and experiencing it at the time, however, would have yielded much richer and not so clear-cut answers.

Dialectically speaking, as discourses, Lysenkoism and Michurinist biology need their own context that they can modify, and which can, in turn, modify the discourses (the content) in order for them to flourish. It means, that to implement Lysenkoism, the country would have needed the experience of the things Lysenkoism defined itself against in the first place. Not knowing this, propagators ignored the historical contingency of Lysenkoism and Michurinist biology, and also their own, pretending them to be universal.

In sum, contrary to the popular, isolating, dualistic, mechanistic, positive belief that tends to cultivate the image of individual, unique heroes in history, the success of Lysenkoism and Michurinist biology, and in fact, any school of thought of any discipline, depends even more on the creation of its exact opposites or enemies, the negatives, than merely on the subsequent provision of its claims and needs, and direct belief in it. In times of crisis, when possible futures are perceived to be constrained due to all being so unstable and simultaneously possible, exact opposites may prove to be enough.

An equally important question remains, however: Why did not the Hungarian State intervened, and forced Lysenkoism upon the GI? Answering this, however, is the task of someone else, as it is beyond my aims and competence.

# 5.2. RQ2: How were the environment and organisms conceptualized by plant scientists?

### 5.2.1. Introduction

So far I have written only about the perception of Lysenkoism and Michurinist biology at the GI pertaining to ideology and politics vs. science, and narrated it through historical milestones of the Institute. In contrast, in this subchapter, comprehensive narration is not needed anymore, and this time, the domains of the environment and organisms, and politics and science will inevitably overlap. First I will present the "environment" of the Lysenkoist side, then a dialectical analysis of the restoration period will follow, and finally I will introduce the geneticists' point of view.

In spite of us seeing at the end of the previous subchapter that modern genetics and Lysenkoism, from a discursive angle, shared many things—and I will return to this at the end of this thesis—the popular assumption is that the one thing in which they definitely differed is the conception of the environment and organisms. This is easy to see, the implicit argument goes, since one of them is genetics after all, the other is "neo-Lamarckian pseudoscience," and we all know that Lamarck was also wrong.

Lamarck assigned far greater role to the environment in the interaction between it and the organism. Simplifying it, he proposed that acquired characteristics; for instance, the gradual elongation of the giraffe's neck as it wants to reach the tree's higher leaves; is passed down to the next generation. It was later Darwin and the idea of natural selection that was responsible for an epistemological break, without which further progress in biology would have been impossible (Lewontin and Levins 2007a, 31). As Lewontin (2007, 230-231) wrote, "Darwin's radical difference from Lamarckism was in his clear demarcation of inside and outside, of organism and environment, and his alienation of the forces within organisms from the forces governing their outside world ... [thus] replacing the mystical interpenetration of interior and exterior that was without any material basis."

Therefore, it makes perfect sense to compare the two schools of thought in order to find out whether there was such a difference or not.

### 5.2.2. The inauguration of the early fifties

First, by examining the official programmatic statements of the early fifties promoting Marxism-Leninism, dialectical materialism, and the achievements of advancing Soviet science, it becomes obvious that one of the problems with bourgeois, idealist science is its conception of the environment. Very simply, "it denies the dialectical unity between the living and its environment, and recognizes only 'random variations'" (Anon. 1950?b, 2). As opposed to this, "it was the dialectical materialist approach in genetics that created Michurinist biology, [and t]his leading-edge theory has to steer our genetic studies as well" (Ibid., 5). Adopting this view is absolutely necessary, as the plans can only succeed if the foundations of biological research will be the "dialectical materialist worldview, and the advanced Soviet biology, the teachings of Vil'iams–Michurin–Lysenko" (Ibid., 1).

If taken seriously, this dialectical approach indeed alludes to an alternative understanding of environment and organism, as anyone who starts to read, for instance, Lewontin and Levins will see. Here it is an understanding alternative to Darwinian natural selection that was rejected by Lysenkoism and Michurinist biology as a Malthusian error, even though Darwin was very much appreciated as the "founder of materialist biology" as he was "instinctively a dialectical materialist" (Anon. n.d., 1). Based on my Literature Review, if I do not accept Lysenkoism and Michurinist biology as neo-Lamarckian ventures, then the only way to explain why they were and are called so, remains speculation about the other side's possible unconscious motives in the sense of unwitting sociocultural embeddedness.

One such attempt at that would be to say that Lysenkoism and Michurinist biology are associated with Lamarckism because that is the negative side of the then and still generally accepted, mainstream evolutionary biology discourse, the modern evolutionary synthesis, which informs genetics as well. As I have written in the Conclusion of the previous subchapter, creation, and now I shall add cultivation, of a discourse equally, if not more, depends on the provision of its negative context, the backhand it defines itself out of and against. Lamarckism then is the last point of divergence, and used as to mentally classify Lysenkoism and Michurinist biology as different and simultaneously false, reinforcing the prevailing theory became tacit knowledge<sup>14</sup>.

Calling it pseudoscience or mysticism, and so on, that is, creating an apparently far greater temporal gap, is much less polite and just, as these general descriptive terms, having no inherent stance of their own, cannot defend themselves against a well-formulated theory, such as genetics. The temporal gap is indeed only apparent, therefore illusory, since it seems solid only due to the linear conception

<sup>14</sup> Cf. Latour 1987.

of time of Western modernity<sup>15</sup>, and its consequent inventions, such as "progress" and "development." Moreover, it is probable that these terms are used out of unconscious teleology inspired by the metaphor of biological evolution and its perceived directionality.

Nevertheless, Pseudoscience and mysticism are still alive traditions, as well as science. Rejecting these as "backward" is in fact, usually, the uncritical acceptance of the hegemony of Western positivist science, and its collective sociocultural, that is, deeply political, biases; while, at the same time, usually not admitting it.

Furthermore, as I have already outlined in the Theoretical Framework in connection with falsifiability, at a particular moment, every science is either a kind of pseudoscience, science or speculation, the difference is only due to our mental and intellectual stoppage of the flow of time and ideas, in order to feel empowered, either to do actual work, or on another level, to continue to exist at all.

5.2.3. The restoration of the early sixties

At another period, in the early sixties after the 1958 restoration of Lysenkoism and Michurinist biology, dialectical materialism and Marxism without "-Leninism," gained momentum as a mutually accepted basis to work with. A 1964 research supervised by Győrffy that I have mentioned before, entitled The philosophical analysis of the biological quantitative and qualitative changes was such a one that explicitly attempted to use this framework. The invited opponent frequently criticized it since it failed at crucial points in this endeavor. Nevertheless, there is still some more to criticize, this time the opinion of the opponent itself.

He (Erdélyi 1965, 2) writes at a place: "In the movement of the living world [or nature]<sup>16</sup>, the interaction of external and internal factors emerges, and usually it is precisely the internal factors that have primacy."

If we are to be consistently dialectical, then it would be expected from the author to cautiously note somewhere in his text that it is only through our mental and intellectual investigation that *in-teractions* begin to exist. In true, or some would say radical, dialectics there is only intra-action<sup>17</sup>, the

<sup>15</sup> Cf. next subchapter.

<sup>16 &</sup>quot;Living world" is the literal translation of the Hungarian "élő világ," where *élő* means living, *világ* world. Nevertheless, written in one world, *élővilág* translates to flora and fauna, or nature. I think both translations are correct, I used the former to authentically evoke the vocabulary and feeling of the Lysenkoist discourse of the time, even though the reason for writing the two words separately may only be grammatical convention.

<sup>17 &</sup>quot;And that is partly what I mean by the notion of 'intra-action' as proposing a new way of thinking causality. It is not just a kind of neologism, which gets us to shift from interaction, where we start with separate entities and they interact, to intra-action, where there are interactions through which subject and object emerge, but actually as a new understanding of causality itself" (Karen Barad in Dolphijn, and van der Tuin, 2012, 55).

parts of which nevertheless can be named and examined separately, it is only the precaution and important resulting attitudes that are missing from "interaction."

Unfortunately, however, such lack of precaution can quickly allude to a still dualistic thinking, the one that the "dogmatic geneticists" are supposed to have, and this idea is further strengthened by the second part of the sentence stating the primacy of internal factors. If something has primacy, again, without a careful note on what exactly primacy means, then by assuming the same implicit discourse, the statement becomes dualistic.

It is curious, however, that it cannot be decided which side this sentence plays. It is probable on the hand hand, that this is a definite stance on the side of genetics having the DNA as the sole controller of an organism's life history. But the statement remains equally true from a Lysenkoist and Michurinist point of view as well, since, even though it is the external that induces the change, and therefore has apparent primacy, it is from the inside that this induced change wonderfully manifests itself due to metabolic processes—this is the approximate description of the Soviet Creative Darwinism of Lysenko, as it was called at the time (Cf. Krementsov 1997, 149–155).

Nonetheless, without such notes or comments I can only give the benefit of the doubt, the case of which is further weakened by the knowledge we have now about how Lysenkoism and Michurinist biology was never pursued out of honest belief at the GI. This propels me to consider the stance of the early fifties and this one from 1964 on the same page, thereby connecting back to the things said before.

In the curricula discussion of The five-year plan of Hungarian biological research, under the "Plant genetics" section, it is written: "An important principle of the development of the living world [or nature] is ... variability, which creates new traits as a result of the interaction of living conditions and the living" (Anon. 1950?c, 10[3]). On the next page it is continued as: "The study of the modi-fying effects of the environment ... is significant both on the theoretical and the pragmatic plane" (Ibid., 11[4]).

The wording and context of concepts are so similar that it is very easy to draw the same conclusions, that, firstly, true interaction would be intra-action, as I mentioned before. And secondly, the environment has no "modifying effects" on its own, since first, there has to be something that could suffer those effects, and second, the organism also changes that environment; this is a never ending process or "movement." As Lewontin and Levins (2007a, 34) describes:

A consequence of the codetermination of the organism and its environment is that they coevolve. As the species evolves in response to natural selection in its current environment, the world that it constructs around itself is actively changed ... One cannot make a sensible environmental politics with the slogan "Save the Environment" because, first, "the" environment does not exist, and second, because ev-

ery species, not only the human species, is at every moment constructing and destroying the world it inhabits.

### 5.2.4. The geneticists' side

Finally, when we examine the stance of the other side, we arrive at a similar position.

In the 1956 "liberation" debate, in his response to previous comments, Győrffy (Anon. 1956, 39) said: "According to my knowledge, we do not have vegetative hybrids in Hungary at the moment, however, if we look at the latest literature of genetics, we see everywhere that the environment-effect is in the foreground."

Regarding the 1965 draft version of the Report of the Section of Biological Sciences on the state of genetics, Győrffy (Győrffy 1965c, 1) commented that "...the classics of genetics already highlighted the importance of the environment (with the creation of the phenotype and reaction norm concepts)..."

He (Ibid.) then criticized the Lysenkoists and Michurinists by reciting a comment by Andrei Sakharov about the Soviet situation, that "the roles of these [i.e. the environment and the organism] were astoundingly inverted at here. According to the classics of Marxism, the propulsive forces of every process are the internal factors, the self-movement of matter ... Such an impossible notion as 'the unity of the environment and organism' could only flower on the soil of philosophical and biological myopia."

One of the things we can gather from these is that Győrffy gave due credit to geneticists, who, along with Darwin earlier, did recognize the role of the environment in the "development" and life history of an organism. The second idea is that that even though Győrffy uses Sakharov's comment as his own opinion on the matter—apparently irrevocably condemning Lysenkoism and Michurinist biology on the basis of the mutually accepted basis of Marxism—Sakharov's argument is not as strong as it first appears to be, as we have seen in the brief description of "Soviet Creative Darwin-ism." It is not obvious what and how that enjoys primacy.

A year later in 1966, the final version contains the sentence: "The further progress of genetics will obviously stem from that, after clarifying the basic questions, it will study how the known laws of genetics emerge in the interaction of living organisms and the environment" (Anon. 1966, 3). Underneath Győrffy's handwritten note can be read: "relieved of the 'environment-dogma' of Lysenko." On the next page (Ibid., 4), beside the summary of Lysenko's principles, on the margin of the page, Győrffy noted "=holism." This last comment is very similar in its essence to the ones given by then contemporary American scientists quoted in the Literature Review.

### 5.2.5. Conclusion and outlook

To conclude, these small comments, both the positive ones, and the negative criticisms of Lysenkoism and Michurinist biology, serve as clues to how geneticists, including Győrffy, conceptualized the environment and organisms, and through that, themselves; this latter thought will be the connection to the next subchapter. But staying with the environment and organisms now, after the Literature Review and the previous subchapter, it is not surprising at all that Lysenkoism and Michurinist biology were again condemned as myopic, holistic dogmas.

Two things are, however, surprising. The first of them is the difference between the words used for creatures. The dialectical materialist or Lysenkoist discourse used "the living," while geneticists "organism" or "living organisms." Taking into account everything that I have written until now about discourses, I am convinced that this is not an accident, but it indicates the fundamental difference between the "old" and the "new" regime of sight and thought. On the surface, they refer to the same things, but actually they do not, as both of these are black boxes. "The living" at least have something to do with life, while organisms are embodiments of static text-book definitions<sup>18</sup>, ready to be examined, and experimented with.

The second surprising matter is related to this difference. One of the properties of living things that non-living do not possess, is that they produce movement. Even if its body does not move, the living still has active metabolic processes that could count as movement. No doubt that this is one of the explanations as to why Lysenkoism and Michurinist biology favored "movement" so much.

Nevertheless, based on this, it might signify something deeper as well. It is not entirely impossible that Lysenkoism and Michurinist biology was not vitalism or holism at all, though some "mystical" element certainly was there. I would say that the mysticism was in the beholder, that is, Lysenkoism approached "the living" with a reverence unknown to Western science since at least Darwin, the mentioned point of divergence. It is unknown to it, because, considering the "objective," text-book understanding, it sees the environment and organisms alike at the interface of the laboratory. The actual participation is long gone, everything is mediated through the experiment, yet this mediation is assumed to be invisible; while at the same time, the results of it are strongly believed to be applicable to the "real," unmediated world.

<sup>18</sup> The word *organism* appeared in English first in the 17th century. Its root, *organ* appeared in the 13th, and meant an "instrument, engine or tool, with two derived senses: the *abstract 'instrument' – agency* [emphasis added], and musical instrument." After continuous transformations, in 1923, Bertrand Russel described machines as "[they are] essentially organic, in the sense that [they have] parts which co-operate to produce a single useful result, and that the separate parts have little value on their own account" (Williams 1985, 227-229). This conception of a system with functionally different, separate parts is a very similar to the what the atomistic, reductionist thinking I criticized earlier has to say on living beings.

This is the same professional, objectifying gaze that I have mentioned before, and as we move to the final subchapter, a complementary to this one, by asking how humans (scientists) defined themselves unwittingly through the definition of the environment, we will see that the objectifying gaze is not uniquely reserved for geneticists at all.

## 5.3. RQ3: What was the role of humans and society in the natural and social environment?

### 5.3.1. Introduction

Finally, after discussing how Lysenkoism and Michurinist biology were introduced and practiced at the GI, how scientists interpreted the ideological and political features of their profession, then how the environment and organisms were perceived, we will look at how, in connection with all previous aspects, what scientists thought about humans and society.

There is one peculiarity in this case: out of my sources, only the programmatic statements of the early fifties mention anything about humans and society, after that, there is nothing. I can only speculate on the reasons, through it will not be difficult; indeed, it will seem obvious, after the comprehensive analysis of the fifties.

### 5.3.2. Living the 11th thesis

The Biological five-year plan has merely enough dedication to this subject, which directly alludes to an image of *man* crucial in the discourse of Soviet agriculture. As the plan says (Anon. 1950?b, 2), the problem with bourgeois biology is that "it denies that new plant and animal formations can be created by purposeful [in a planned fashion<sup>19</sup>] human activity." In contrast, the "advancing Michurinist biology cannot be separated from pragmatics, and arms researchers and practical professionals with scientific methods that enable them to change living nature for the good of the people ..." (Ibid.).

In the "Thematic plan" section listing the tasks to be carried out, it is written (Ibid., 5): "It has to be studied ... that with the insertion of appropriate factors, how it is possible to control the development of plants, and how this alteration is inherited."

Another source, an entry of a biological calendar writes (Anon. n.d., 4) about Lysenko: "Sept. 29. 1898. <u>The birth of Lysenko.</u> – Trofim Denisovich Lysenko is the founder of the stage development of plants and the practical methods based thereon, as well as dialectical materialist genetics; based on Michurinist traditions, along with Michurin, he is the purposeful<sup>20</sup> transformer of the nature of plants and animals."

20 Cf. the previous note.

<sup>19</sup> The Hungarian original is *tervszerű*, the literal translation of which would be "plan-like," or "in a plan-like manner".

By now, we have an idea about the man of the age, the key words and expressions are: *can* be created, *plan*-like, *change* living nature, *control, transformer* of the nature. These all follow from, as I mentioned in the Literature Review, Marx's *11th thesis on Feuerbach*: "Philosophers have sought to understand the world. The point, however, is to change it." In this aspect, it is not the reverence at all mentioned that Western science lacks; Soviet, or "Eastern" science lacks it too.

Considering the origin of this later basic tenet of the Soviet discourse that was also exported, at least in its vocabulary, to the satellite states; this image of man was almost predestined to become a political dividing line in any context due to Marxism being one of the foundations of the theory of Soviet socialism. Lysenko and his followers also aligned themselves to this teaching, so much that the original Marxist attitude and theirs became indistinguishable from each other. As it is written in the The report by the Section of Biological Sciences on the state of genetics of 1966: "[After the 1948 debate], the acceptance of Lysenkoist teachings became the trial of attachment to social advancement and the Soviet Union" (Anon. 1966, 6).

### 5.3.3. Gramsci: Man is the ensemble of social relations, the conqueror of material forces

Early 20th century Marxist theoretician, activist, and politician Antonio Gramsci, building on Marx, Engels, and related authors, renders a very similar image of man into an elaborate philosophical manifesto in his *Prison Notebooks* that also offers surprising insights on the image of man at the GI.

After stating that man is "conceived as the *ensemble* of social relations" (Gramsci 1971, 359), he delineates three ways that men create their personality. One of them is precisely "by contributing to modify the *ensemble* of the concrete conditions for realising [oneself] to the extent of one's own limits and capacities and in the most fruitful form" (Ibid., 360).

Now, since "[the idea that] ethical 'improvement' is purely individual is an illusion and an error: the synthesis of the elements constituting individuality is 'individual,' but it cannot be realised and developed without an activity directed outward ..." (Ibid.). Man is therefore imagined to be a confluence point at the nexus of the internal and external, and "[for] this reason one can say that man is essentially 'political' since it is through the activity of transforming and consciously directing other men that man realises his 'humanity,' his 'human nature'" (Ibid.).

Knowing that the human essence is not inside but gained in the, let us simplify it, interaction with others, the conclusion is: "In reality, though, it was implicitly admitted that human 'nature' was

not within the individual but in the unity of man and material forces. Therefore, the conquest of material forces is one way, and indeed the most important, of conquest of personality" (Ibid., 361).

In essence, man's destiny is to transform material forces including nature, and no obstacle is real enough to stop him, since, echoing Marx's Preface to *A Contribution to the Critique of Political Economy*<sup>21</sup>: "no society poses for itself problems the necessary and sufficient conditions for whose solution do not already exist or are coming into being; and ... no society comes to an end before it has expressed all its potential content" (Ibid., 367). The language of mentioned the five-year plans correlates with the implications of these thoughts.

### 5.3.4. Designing humans and society; controlling nature

Other qualities of this Promethean man is collected by Scott (1998, 195) when writing about Soviet collectivization: "The 'new man'—the Bolshevik specialist, engineer, or functionary—came to represent a new code of social ethics, which was sometimes simply called *kultura*. In keeping with the cult of technology and science, kultura emphasized punctuality, cleanliness, businesslike directness, polite modesty, and good, but never showy, manners."

Creating this man was to be achieved by conscious design:

Statistical facts were elaborated into social laws. It was but a small step from a simplified description of society to a design and manipulation of society, with its improvement in mind ...

The scope of intervention was potentially endless. Society became an object that the state might manage and transform with a view to- ward perfecting it ... It was possible to conceive of an artificial, engineered society designed, not by custom and historical accident, but according to conscious, rational, scientific criteria. (Ibid., 92)

This re-design of man and society was accompanied by the re-design, transformation of nature, the same force that propelled Lysenkoism and Michurinist biology as well:

As the principles of a rationally organized, good society (more factories, more machines, more control over nature) were well known and agreed upon, one could proceed directly to usher any society (and particularly a society without factories, without machines, without the capitalists eager to build them, without the workers oppressed and exploited in the process of building) into a state designed by those principles. (Bauman 1992, 166)

<sup>21 &</sup>quot;No social order ever perishes before all the productive forces for which there is room in it have been developed; and new, higher relations of production never appear before the material conditions of their existence have matured in the womb of the old society itself. Therefore mankind always sets itself only such tasks as it can solve; since, looking at the matter more closely, it will always be found that the task itself arises only when the material conditions for its solution already exist or at at least in the process of formation" (Gramsci 1971, 367).

### 5.3.5. Modernity as communism: The ultimate subordination of nature to humans

It is crucial to understand, however, that this was a not a uniquely Soviet phenomenon, which was later exported to satellite states, including Hungary. It was the project of modernity, civilization itself:

Like socialism (and all other staunch believers in the modern values of technological progress, the transformation of nature and a society of plenty), communism was thoroughly modern in its passionate conviction that a good society can only be a carefully designed, rationally managed and thoroughly industrialized society. It was in the name of those shared modern values that socialism charged the capitalist administrators of modern progress with mismanagement, inefficiency and wastefulness. Communism accused socialism of failing to draw conclusions from the charges: stopping at critique, denunciations, prodding—where an instant dismissal of inept and corrupt administrators was in order. (Ibid.)

### Communism,

Socialism's younger, hotheaded and impatient brother, it wholeheartedly shared in the family trust in the wonderful promises and prospects of modernity, and was awe-struck by the breathtaking vistas of society doing away with historical and natural necessity and by the idea of the ultimate subordination of nature to human needs and desires. But unlike the elder brother, it did not trust history to find the way to the millennium. Neither was it prepared to wait till history proved this mistrust wrong. Its war cry was: "Kingdom of Reason—now!" (Ibid.)

In this sense, "[c]ommunism was modernity in its most determined mood and most decisive posture; modernity streamlined, purified of the last shred of the chaotic, the irrational, the spontaneous, the unpredictable" (Ibid., 167). The peculiarity is, of course, that Lysenkoism and Michurinist biology indeed could be described as "chaotic," "irrational," "spontaneous," and "unpredictable"; yet the reasons for their ascendency are probably found among the factors listed by Young (1978) and Lewontin and Levins (Ferguson 2011, 10) in the Literature Review. Nonetheless, in general terms, it was precisely what caused Lysenko's fall later.

Regarding practical implementation, "[it] was a system onesidedly adapted to the task of mobilizing social and natural resources in the name of modernization: the nineteenth-century, steam and iron ideal of modern plenty" (Bauman, 169).

Contrary to Western society and culture that bred the discourse of the modern synthesis genetics also present at the GI,

this was not a society of *growth*..., but a society of *stability* and *equilibrium*, one of a steady, well-balanced economy, catering for all needs of the population—not an economy beefing up and pushing to new limits their consumptive needs and capacities. The goodness of society was to be measured by its productive performance, by the degree of gratification of needs (given, "objective", finite), not by the growing richness and spectacularity of its consumptive display. (Ibid., 167)

In summary, "[t]he communist state, in its own admittedly unprepossessing way, seemed to serve the same ideals of modern era which even its capitalist haters readily recognized as their own" (Ibid., 168). This means that, after all, both being the products of modernity, there is no significant difference between the elementary conceptions and relations of Lysenkoism and Michurinist biology, and genetics. This explains the peculiar silence of geneticists' at the concept of humans and society in my sources; silence is agreement, or ignorance because of the subject's unimportance. Either way, it is approval.

### 5.3.6. High Modernism: The point of convergence

Another characterization of the modernist endeavor is the "High Modernism" of Scott (1998, 89-90):

What is high modernism, then? It is best conceived as a strong (one might even say muscle-bound) version of the beliefs in scientific and technical progress that were associated with industrialization in Western Europe and in North America from roughly 1830 until World War I. At its center was a supreme self-confidence about continued linear progress, the development of scientific and technical knowledge, the expansion of production, the rational design of social order, the growing satisfaction of human needs, and, not least, an increasing control over nature (including human nature) commensurate with scientific understanding of natural laws. High modernism is thus a particularly sweeping vision of how the benefits of technical and scientific progress might be applied ... in every field of human activity. ... [T]he high-modern state began with extensive prescriptions for a new so-ciety, and it intended to impose them.

And finally, connecting the three strands of my research, ideology and politics, and science; humans and society; and the environment and organisms—true both of the Western and (exported) Soviet models:

Although high modernists came to imagine the refashioning of social habits and of human nature itself, they began with a nearly limitless ambition to transform nature to suit man's purposes—an ambition that remained central to their faith. ... This belief that it was man's destiny to tame nature to suit his interests and preserve his safety is perhaps the keystone of high modernism.... (Ibid., 94–95)

### 5.3.7. Legibility; and history in the making

From the point of view of the regime, however, this was *only* about control, that is, to use Scott's (1998) term, *legibility*. In this aspect, High Modernism, with both Lysenkoism and Michurinist biology, and modern genetics situated in it, be it in the Soviet Union or Hungary, was one of the possible routes taken unwittingly through a myriad of socioculturally conditioned, contingent decisions. These created cause-and-effect chains, which in turn strengthened the preference for certain new points of convergence and divergence, some of them with definite connections in content to the past; and this latter quality of historical change is why events, which are arbitrary constructions from the point of view of the future, seemed to be obviously connected in a clearly delineated context. But before the black boxes close, they are continuously questioned, and context and contents are still merged (Latour 1987, 5, 13).

Was it obvious that Lysenkoism and Michurinist biology will be introduced in Hungary? In 1948, when finally rebuilding after war bombings, it certainly did not seem so, since Lysenko was not in absolute power yet. Was it obvious that Lysenko will achieve such a supreme power in August 1948? It was not, as the VASKhNIL conference was initially intended to be only a resolution, which "[i]n the course of ... work, ... was transformed into a twenty-two-page treatise entitled 'On the Situation in Soviet Biological Science'" (Krementsov 1997, 167), then

sometime between July 10 and July 20, the Politburo decided not to issue it. Instead, it was decided to hold a meeting of VASKhNIL with a "report of one of the Marxist biologists," Lysenko, on the same subject—"On the Situation in Soviet Biological Science". On July 23, Lysenko sent Stalin a preliminary draft of his report to the forthcoming meeting. Stalin attentively read the text, edited it, and sent it back with numerous corrections and suggestions. (Ibid., 168)

Was it obvious that after the death of Stalin in March 1953, Lysenko will recover from his first fall with the support of Khrushchev? It was not, though after Khrushchev was mesmerized by the US hybrid maize farms, another high modernist dream come true, it seemed highly plausible that it is Lysenko's turn to move. Was it then obvious that the 1956 Revolution will occur just five months after the great "liberation" debate, ending the manifest liberation until Lysenko's final fall in 1965? Finally, was it obvious that Lysenko will ever fall again? For the hopeful, of course. And that Khrushchev will precede him?

As a conclusion for this whole chapter, a different view emerges from these perspectives that is no longer constituted of the tripartite structure of ideology and politics, and science; humans and society; and environment and organisms. Instead, we can also see these three constellations of relationships and their actors as on the stage of modernity, or unknowingly on strings in a system founded on the assumptions and inferences of world zeitgeists.

If this is true, then the fall of Lysenkoism and Michurinist biology became the absolute, apparently irrevocable source of legitimacy for the hegemonic reign of the modern synthesis and genetics, similarly to the case of communism and capitalism. As Bauman (1992, 177) writes:

It is widely assumed ... that the practical discrediting of communism (construed as "the Other" of *our form of life*, as the *negative* totality which injects meaning into our *positivity*), pre-empts by proxy and disqualifies in advance any doubts about the unchallengeable superiority of the *really existing* regime of freedom and the consumer market; that it discredits, moreover, any suggestion that this regime [i. e. capitalism], even if technically more viable, may be still neither entirely flawless, nor the most just of conceivable orders; that it may be instead in urgent need of an overhaul and improvement.

### Furthermore, the fall of communism (and Lysenkoism) was in fact, the fall of modernity:

The fall of communism was a resounding defeat for the project of a *total order*—an artificially designed, all-embracing arrangement of human actions and their setting, one that follows the rules of reason instead of emerging from diffuse and uncoordinated activities of human agents; it was also the downfall of the grandiose dream of *remaking* nature—forcing it to yield ever more of anything human satisfaction may require, while disregarding or neutralizing such among its unplanned tendencies as could not be assigned any sensible human benefit; it demonstrated as well the ultimate frustration of the ambitions of global management, of replacing spontaneity with planning, of a transparent, monitored, supervised and deliberately shaped order in which nothing is left to chance and everything derives its meaning and *raison d'être* from the vision of a harmonious totality. In short, the fall of communism signalled the final retreat from the dreams and ambitions of *modernity*. (Ibid., 178)

Nevertheless, the legitimacy gained this way is illusory, as by getting rid of communism, or modernity, the West got rid of its own past, and even more importantly, its feeling of responsibility:

What the affluent west is in fact celebrating today is the official passing away of its own past; the last farewell to the modern dream and modern arrogance. ... With communism, the ghost of modernity has been exorcised. Social engineering, the principle of communal responsibility for individual fate, the duty to provide commonly for single survivals, the tendency to view personal tragedies as social problems, the commandment to strive collectively for shared justice—all such moral precepts as used to legitimize (some say motivate) modern practices have been compromised beyond repair by the spectacular collapse of the communist system. No more guilty conscience. No scruples. No supra-individual commitments contaminating individual enjoyment. The past has descended to its grave in disgrace. (Ibid., 180)

This could perfectly explain; though not from the beginning, and this is important; why the aversion from politics "contaminating" science was so strong both on the West and in Hungary in geneticist circles. But the danger of removing the "Other" and responsibility, is a totality that rewrites history and itself, always becoming the one without alternatives:

One aspect of the situation in which the western form of life has found itself after the collapse of the communist alternative is the unprecedented freedom this form of life will from now on enjoy in construing "the other" of itself and, by the same token, in defining its own identity. We do not really know what effects such freedom may bring: we can learn little from history, since it knows of no similar situations. (Ibid., 183)

Without others to watch our every move, and to answer for our actions, there is no need for reflection on our contingent place in history, because our self-definition, deriving its essence from the others' "negative totality," the backhand, ceases to exist. The result is that we have lost our selves by becoming the absolute, hegemonic, genetic totality.

Nevertheless, this does not explain the aversion from politics before the fall of Lysenkoism and Michurinist biology, the answer to which could be the dualistic, reductionist way of thinking originating perhaps from the time of Descartes. This is an understanding of the world that pervades everything in the West, and now the rest of the planet as well to a great degree through "development." It even "[dictates] not only the form of our answers to questions but which questions are allowed to be 'interesting'" (Lewontin 2007, 229).

How to ask a question which cannot ask beyond itself?

## 6. Conclusion

# 6.1. MOTIVATION, AIMS, OBJECTIVES, RESEARCH QUESTIONS, AND METHODOLOGICAL OVERVIEW

This paper was prompted by an interest in Lysenkoism and Michurinist biology, and the realization, that these topics are still worth studied today, as they may serve as warnings for those engaged in creating, and writing about, science.

Correspondingly, this thesis looked at the Genetic Institute of the Hungarian Academy of Sciences in the period between 1948 and 1966, and examined the presence and perception of Lysenkoism and Michurinist biology from the point of view of scientists, from both sides of the "affair."

Therefore, my aim was to understand the history of plant genetic research in the mentioned period through two objectives: firstly, by investigating the presence of Lysenkoism and Michurinist biology at the GI; and secondly, by investigating the scientists' personal perception of it.

In order to arrive at these, I proposed three research questions, which comprehensibly covered the crucial points of the discourses I found myself facing. These pertained to the conception of the environment and organisms; the role of humans and society in the social and natural environment; and the hypothetical manifestation of politics in genetic science.

The aims and objectives were achieved through archival research conducted at the Archives of the Hungarian Academy of Sciences between May 7 and 28, during which I collected 542 A4 pages out of eight archival boxes, a total of 71 unique documents originating from between 1947 and 1969. See Tables 2.1 and 2.2 for more information.

By taking up an interpretivist epistemological approach, and applying discourse analysis similar to Michel Foucault's in the *Archeology of Knowledge* (1969), I set out to uncover history and science in the making; and deconstruct, though not in the Derridian sense, the traditionally dualistic, binary thinking about these by using dialectics. These intentions derived from my Theoretical Framework, in which I drew extensively upon Latour (1987) and various essays by Lewontin, and Levins.

As I wrote in the beginning of the first subchapter of my Findings, I relied on the documents and the data that I found, I let it lead me toward a conclusion, and this resulted in changing the order of research questions investigated, and the order of subsequent subchapters. In the end, this proved to be more productive, as it enabled me to reach a more complete, interconnected conclusion.
After the Introduction, Methodology, Theoretical Framework, and Literature Review chapters, the results in the Findings were presented and merged with corresponding analyses and reflections, so as to make comprehension more practical.

#### 6.2. Summary of the history of plant genetics at the GI between 1948 and 1966

Regarding factual history, Lysenkoism and Michurinist biology were never widely practiced at the GI out of honest conviction. After the 1948 VASKhNIL conference, where Lysenko gained supreme power in agricultural matters, his teachings were exported to satellite states, including Hungary. Nevertheless, despite the dramatic events described by Igali (2002), the GI only had to partly modify its research curriculum by the addition of vegetative hybridization, and smaller, related topics.

This period lasted approximately until 1953-54, when the GI was transferred under the supervision of another Academic Section favorable to genetics. The "liberation" from the "dark phantom" (Ibid.) of Lysenkoism first came in a June 1956 meeting, where the break with the previous years was officially declared. Few months later, however, the 1956 Revolution changed the ideological and political climate for the worse, as it turned out at the 1958 Genetic Debate.

A formal restoration of Lysenkoism and Michurinist biology followed by elevating them to the same level of intellectual merits than that of genetics'. The latter was not banned or restricted anymore, instead, Lysenkoism and Michurinist biology were to have their now rightful, legit place in research curriculum.

With the 20th Congress of the Communist Party in the Soviet Union, and the successive purge of the cult of personality, previously highly personalized concepts, such as the "Vil'iams-Michurin-Lysenko" direction to be followed; espoused by, for instance, the 1950 Five-year plan of Hungarian biological research; disappeared from the discourse, leaving only the mutually accepted Marxism, and a formalistic dialectical materialism in its place.

After Lysenko's second and final fall in 1964-65, liberation indeed happened, with a resolution admitting the mistakes of the past. The exuberant celebration of freedom was, however, not a clearcut and easy matter; the defense of a doctoral dissertation by a life-long Lysenkoist was still to come in 1966, resulting in an outrage. My investigation ended before this point. Regarding how ideology and politics influenced plant genetic research, apart from the factual things just mentioned, the presence of Lysenkoism was not a major issue at the GI.

The scientists' perception of it, however, excluding a few, noted cases, strongly corresponded to the initial international scholarly opinion, which considered Lysenkoism as a "cautionary tale about the intrusion of the alien values of politics and ideology into the domain of value-neutral science" (Young 1978). Lysenkoism and Michurinist biology were convicted as a period of "total neglect of the up to date achievements of biological and genetic research [and] ... the distortion of those, with highlighting late and now already obsolete authorities in biology" (Győrffy 1956, 36). "In 1948, the great Soviet academic debate ... ensured not a scientific but primarily a political victory to Lysenko's school..." (Anon. 1956, 30) Genetics research was "impeded by dogmatic prejudice or the cult of personality" (Ibid., 9).

In sum, Lysenkoism and Michurinist biology were considered to be "the second movement of politicogenetics," (Győrffy 1965b, 2) the first being Nazi eugenics.

In my analyses, I attempted to show how the things that happened at the GI were a good example of a clash between two discourses, neither of which conscious of this fact about itself, but both striving to reach supremacy in some way, yet completely failing to do so due to the incompatible black boxes they operated on a daily basis: the black boxes they themselves became.

I built on Beck's (1992) reflexive scientization concept, whereby "the sciences are now being confronted with their own objectivized past and present – with themselves as product and producer of reality and of problems which they are to analyze and overcome" (Beck 1992, 156).

Examining the consequences of this process, I found that it perfectly fitted what Lysenkoism and Michurinist biology were as scientific phenomena. With reflexive scientization, and the rise of Lysenkoism and Michurinist biology, "a momentous *demonopolization of scientific knowledge claims* [came] about" (Ibid.), along with questioning "even the *foundations of scientific rationality*, ... [as] [t]he suspicion is [that] that 'objective constraints,' ... are themselves *manufactured* and thus are in principle *solvable*. [With these,] [t]he project of modernity, Enlightenment, [remains] unfinished" (Ibid., 157).

Finally, asking why Lysenkoism and Michurinist biology did not work out at the GI, I ventured to state that it was due to the lack of the exact discursive context that gave birth to it, and was later modified by it in a never-ending dialectical process. In summary, by reflecting on the factual history and the discourse of ideology and politics, and science at the GI, I found that the success of Lysenkoism and Michurinist biology depended even more on the creation of its exact opposites or enemies, the negatives, than merely on the subsequent provision of its claims and needs, and direct belief in it.

## 6.4. RQ2: How were the environment and organisms conceptualized by plant scientists?

Regarding how the environment and organisms were conceptualized by plant scientists, I found significantly less mentions. The programmatic statements of the early fifties, inspired by Lysenkoism and Michurinist biology, speak of a "dialectical unity between the living and its environment" (Anon. 1950?b, 2), an understanding in which "we possibly take into account all circumstances and all realities" (Lysenko 1960, 22–23). Darwin was thought of as the "founder of materialist biology," as he was "instinctively a dialectical materialist" (Anon. n.d., 1).

On the other side, after condemning Lysenkoism and Michurinist biology, this time on the basis of their teachings, for being myopic, holistic dogmas; there are only static, factual, text-book-like presentations, from which I drew the conclusion that the environment and organisms alike are seen at the interface of the laboratory; the actual participation is long gone, everything is mediated through the experiment, under the all-knowing, professional, objectifying gaze characteristic of Western science since at least Darwin.

# 6.5. RQ3: What was the role of humans and society in the natural and social environment?

Regarding the role of humans and society in the natural and social environment, I found even less material to work with, though it was more than enough to arrive at a far reaching conclusion, as at this point, the previous research questions also converged together, actively filling the discursive gaps of this question. I decided, therefore, to give the conclusion of the thesis in this subchapter.

It was also again the programmatic statements of the early fifties where I found clues as to what constituted the image of the *man* of the age. The key words and expressions were: *can* be created; *plan*-like; *change* living nature; *control*; *transformer* of the nature. These all followed from, as I mentioned in the Literature Review, Marx's 11th thesis on Feuerbach: "Philosophers have sought to understand the world. The point, however, is to change it." Man appeared to be a Promethean transformer in the imported Soviet discourse, and this image quickly merged with Lysenkoism and

Michurinist biology, therefore the acceptance of them in Hungary "became the trial of attachment to social advancement and the Soviet Union" (Anon. 1966, 6).

In my reflection, I highlighted that having this *man* was a result of deliberate creation, as "[i]t was [now] possible to conceive of an artificial, engineered society designed, not by custom and historical accident, but according to conscious, rational, scientific criteria" (Scott 1998, 92). After establishing, by drawing on Bauman (1992), that at the heart of Lysenkoism and Michurinist biology, as products of a communist regime, lies the project of modernity, among other things; I showed that it was indeed "awe-struck by the breathtaking vistas of society doing away with historical and natural necessity and by the idea of the ultimate subordination of nature to human needs and desires" (Bauman 1992, 166).

Since the reigning Western discourse of the time, the one genetics also embraced, was also a school of modernity, I established the ultimate similarity and kinship between the discourses of Lysenkoism and Michurinist biology, and the modern synthesis genetics; the former being different only in the respect that it was given birth by communism (which it later molded as well) that was "modernity streamlined, purified of the last shred of the chaotic, the irrational, the spontaneous, the unpredictable" (Ibid., 167). This argument stands, in spite of the fact that Lysenkoism and Michurinist biology indeed could be described as "chaotic," "irrational," "spontaneous," and "unpredictable"; yet the reasons for their ascendency are probably found among the factors listed by Young (1978) and Lewontin and Levins (Ferguson 2011, 10) in the Literature Review. Nonetheless, in general terms, it was precisely what caused Lysenko's fall later.

Another characterization of Lysenkoism and Michurinist biology, as it was embedded in the authoritarian Soviet state; and exported to Hungary into, in certain respects, similar conditions; came from Scott (1998). It was called High Modernism, and "[a]t its center was a supreme self-confidence about continued linear progress, the development of scientific and technical knowledge, the expansion of production, the rational design of social order, the growing satisfaction of human needs, and, not least, an increasing control over nature (including human nature) commensurate with scientific understanding of natural laws" (Scott 1998, 89-90).

Finally, at the point of convergence, I established that the ideological and political mission of changing humans and society, and modifying the environment and organisms through science originally started with the transformation of nature. As Scott (Ibid., 94–95) wrote: "[T] hey began with a nearly limitless ambition to transform nature to suit man's purposes—an ambition that remained central to their faith. ... This belief that it was man's destiny to tame nature to suit his interests and preserve his safety is perhaps the keystone of high modernism..."

#### 6.6. Summary and Outlook

Toward the end of my analyses and reflections, a different view emerged, which was no longer constituted of the tripartite structure of ideology and politics, and science; humans and society; and environment and organisms. Instead, it was possible to see these three constellations of relationships and their actors as on the stage of modernity, or unknowingly on strings in a system founded on the assumptions and inferences of world zeitgeists.

This was precisely connected to my Theoretical Framework, through which I endeavored to occupy an awareness of the simultaneously open and closed state of a black box (cf. Latour 1987); in other words, being able to switch between different temporal and methodological perspectives, to speed up time or slow it down in order not to miss important events which would otherwise be neglected on the long run; by identifying the opposing processes of a system, and the relative, historical contingency of the beholder (cf. Lewontin and Levins). When applied to Lysenkoism and Michurinist biology, my intention was to purge in this attempt the dualistic, reductionist way of thinking originating perhaps from the time of Descartes, which "[dictates] not only the form of our answers to questions but which questions are allowed to be 'interesting'" (Lewontin 2007, 229).

### 7. References

### 7.1. Academic Literature

Amasino, R. 2004. Vernalization, Competence, and the Epigenetic Memory of Winter. *The Plant Cell* 16 (October): 2553–2559.

Balla, L. 1999. Reflexió [Reflection]. *Magyar Tudomány* 160 (7): 892–?. Accessed July 20. URL: http://epa.oszk.hu/00700/00775/00007/1999\_07\_17.html

Bauman, Z. 1992. Intimations of Postmodernity. London: Routledge.

Beck, U. 1992. Risk Society: Towards a New Modernity, transl. M. Ritter. London: SAGE.

Cohen, D., and Crabtree, B. 2006. Qualitative Research Guidelines Project. Robert Wood Johnson Foundation. Accessed July 20. URL: http://www.qualres.org/HomeInte-3516.html

DeJong-Lambert, W. 2007. The Cold War Politics of Genetic Research. Paper presented at Annual International Young Researchers Conference: Culture, Practices, and the Memory of the Cold War, 25–7 October, at Havighurst Center for Russian and Post-Soviet Studies, Miami University, Oxford, Ohio, U.S. Accessed July 16. URL: http://www.units.miamioh.edu/havighurstcenter/conferences/ documents/WdJLPaper.pdf

\_\_\_\_\_. 2012. The Cold War Politics of Genetic Research: An Introduction to the Lysenko Affair. New York: Springer.

DeJong-Lambert, W., and Krementsov, N. 2012. On Labels and Issues: The Lysenko Controversy and the Cold War. *Journal of the History of Biology* 45 (2012): 373–388.

Dolphijn, R., and van der Tuin, I. 2012. Interview with Karen Barad. In Dolphijn, R., and van der Tuin, I. *New Materialism: Interviews & Cartographies*, 44–70. Ann Arbor: Open Humanities Press.

Ferguson, M. A. Jr. 2011. The Myth of Progress in Science: Dialectics, Distortion and Lysenkoism in the Soviet Union. *The Hilltop Review* 4 (2): 8–22.

Flitner, M. 2003. Genetic Geographies: A Historical Comparison of Agrarian Modernization and Eugenic Thought in Germany, the Soviet Union, and the United States. *Geoforum* 34 (2003): 175–185.

Fracchia, J., and Lewontin, R. 2007. Does Culture Evolve?. In Lewontin, R., and Levins, R. *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health,* 267–296. New York: Monthly Review Press.

Gramsci, A. 1971. The Study of Philosophy: Problems of Philosophy and History. In *Selections from the Prison Notebooks*, eds., transl. Q. Hoare, and G. N. Smith, 343–377. New York: International Publishers.

Gutting, G. 2013. Michel Foucault. *The Stanford Encyclopedia of Philosophy*, 2013 Summer. Accessed July 20. URL: http://plato.stanford.edu/entries/foucault/

Hughes, T. P. 2000. Lessons From Soviet Science and Technology: Loren Graham's "What Have We Learned about Science and Technology from the Russian Experience?" *Technology and Culture* 41 (2): 348-352.

Igali, S. 2002. A liszenkoizmus Magyarországon [Lysenkoism in Hungary]. *Valóság* 45 (3). Accessed May 23. URL: http://www.valosagonline.hu/index.php?oldal=cikk&cazon=371&lap=0

Joravsky, D. 1959. Soviet Marxism and Biology Before Lysenko. *Journal of the History of Ideas* 20 (1): 85–104.

Krementsov, N. 1997. Stalinist Science. Princeton: Princeton University Press.

Latour, B. 1987. Science in Action: How to Follow Scientists and Engineers Through Society. Cambridge: Harvard University Press.

Lewontin, R. 1995. *Biology as Ideology: The Doctrine of DNA*. 2nd ed. Concord, Ontario, Canada: House of Anansi Press.

\_\_\_\_\_. 2007. Genes, Environment, and Organisms. In Lewontin, R., and Levins, R. *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health,* 221–234. New York: Monthly Review Press.

Lewontin, R., and Levins, R. 2007a. Organism and Environment. In Lewontin, R., and Levins, R. *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health,* 31–34. New York: Monthly Review Press.

\_\_\_\_\_. 2007b. The Biological and the Social. In Lewontin, R., and Levins, R. *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health,* 35–38. New York: Monthly Review Press.

\_\_\_\_\_. 2007c. Aspects of Whole and Parts in Population Biology. In Lewontin, R., and Levins, R. *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health*, 125–148. New York: Monthly Review Press.

. 2007d. Educating the Intuition to Cope with Complexity. In Lewontin, R., and Levins, R. *Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health*, 183–198. New York: Monthly Review Press.

Müller, M. 2011. Liszenko emlékezetes előadása a Magyar Tudományos Akadémián, 1960-ban [Lysenko's Memorable Lecture at the Hungarian Academy of Sciences in 1960]. *Magyar Tudomány* 172 (11): 1355–1359.

Nabham, G. P. 2009. Where Our Food Comes From: Retracing Nikolay Vavilov's Quest to End Famine. Washington, D.C.: Island Press.

Roll-Hansen, N. 2008. Wishful Science: The Persistence of T. D. Lysenko's Agrobiology in the Politics of Science. *Osiris* 23 (1): 166–188.

Scott, J. C. 1998. Seeing Like a State: How Certain Schemes to Improve the Human Condition have Failed. New Haven and London: Yale University Press.

Scruton, R. 2009. The Aesthetic Gaze. In *The Roger Scruton Reader*, comp., ed., introd. Mark Dooley, 137–151. London: Continuum.

Williams, R. 1985. Keywords: A vocabulary of culture and society. New York: Oxford University Press.

Young, R. M. 1978. Getting Started on Lysenkoism. Accessed July 16. URL: http://human-nature.com/rmyoung/papers/getting.html

#### 7.2. ARCHIVAL SOURCES

Anon. n.d. Biológiai eseménynaptár [Calendar of biological events]. Archives of the Academy, VIII. Biológiai Tudományok Osztálya 5/10.

\_\_\_\_\_\_. 1949a. A Magyar Köztársaság Kormányának 4.008/1949. (91.) Korm. Számú rendelete Agrobiológiai Intézet létesítéséről [The 4.008/1949. (91.) Gov. Decree of the Government of the Republic of Hungary on the creation of Agrobiological Institute]. Archives of the Academy, Genetikai Intézet Iratai 3/2?.

\_\_\_\_\_. 1949b. A Földművelésügyi Miniszter 8.062/8/1949. (119) F.M. Számú rendelete Agrobiológiai Intézet létesítéséről [The 8.062/8/1949. (119.) MoA. Decree of the Minister of Agriculture on the creation of Agrobiological Institute]. Archives of the Academy, Genetikai Intézet Iratai 3/2?.

\_\_\_\_\_. 1950?a. A magyar tudományos kutatás ötéves terve [The five-year plan of Hungarian scientific research]. Archives of the Academy, VIII. Biológiai Tudományok Osztálya 8/3.

\_\_\_\_\_. 1950?b. A magyar biológiai kutatások ötéves terve [The five-year plan of Hungarian bio-logical research]. Archives of the Academy, VIII. Biológiai Tudományok Osztálya 5/9.

\_\_\_\_\_. 1950?c. A magyar biológiai kutatások ötéves terve (A terv kidolgozásának általános irányelvei) [The five-year plan of Hungarian biological research (The general principles of the plan's elaboration)]. Archives of the Academy, Győrffy Barna Iratai 7/2.

\_\_\_\_\_. 1953. Jegyzőkönyv a Biológiai Bizottság 1953. november 9-i üléséről [Records of the November 9, 1953 meeting of the Biology Committee]. Archives of the Academy, VIII. Biológiai Tudományok Osztálya 8/9.

\_\_\_\_\_. 1954. Jegyzőkönyv a Biológiai Bizottság 1954. december 14-én megtartott üléséről [Records of the December 14, 1954 meeting of the Biology Committee]. Archives of the Academy, VIII. Biológiai Tudományok Osztálya 8/9.

\_\_\_\_\_. 1956. Jegyzőkönyv, készült a biológiai Csoport 1956. június 1-én délelőtt tartott ülésén [Records of the June 1, 1956 meeting of the Biological Group]. Archives of the Academy, Genetikai Intézet Iratai 3/5.

\_\_\_\_\_. 1958. Jegyzőkönyv, A Genetikai Bizottságnak a Biológiai Csoport vezetőségének részvételével 1958. október 9-én megtartott ülésről [Records of the October 9, 1958 Genetic Debate]. Archives of the Academy, VIII. Biológiai Tudományok Osztálya 72/3.

\_\_\_\_\_. 1960?a. Győrffy Barna által irányított kutatási tervfeladatok [Planned research topics directed by Barna Győrffy]. Archives of the Academy, Győrffy Barna Iratai 7/5.

\_\_\_\_\_. 1960. Témalap 1961-re – A molekuláris genetikai koncepció elemzése és bírálata 2. [Topic sheet for the 1961 research plan "The analysis and criticism of the molecular genetic concept" 2]. Archives of the Academy, Genetikai Intézet Iratai 79/1.

\_\_\_\_\_. 1965?. Melléklet, A genetika haladásának legjelentősebb mozzanatai az utolsó 15 évben [Appendix, The most significant achievements of the progress of genetics in the last 15 years]. Archives of the Academy, Genetikai Intézet Iratai 3/8.

\_\_\_\_\_. 1966. A Biológiai Tudományok Osztályának jelentése a genetika helyzetéről [The report by the Section of Biological Sciences on the state of genetics]. Archives of the Academy, Genetikai Intézet Iratai 3/8.

Daniel, L. 1966. A magyar növénygenetikai kutatások helyzetképe (mikroevolúció nélkül) [The state of Hungarian plant genetics research (without micro-evolution)]. Archives of the Academy, Genetikai Intézet Iratai 3/5.

Erdélyi, L. 1965. Opponensi vélemény a MTA Genetikai Intézet 1964. évi kutatási beszámolójának "A biológiai mennyiségi és minőségi változások filozófiai elemzése" c. témájáról [Opinion of the opponent of the 1964 research "The philosophical analysis of biological quantitative and qualitative changes"]. Archives of the Academy, Genetikai Intézet Iratai 79/4. Győrffy, B.? 1949. Javaslat növénynemesítésünk átszervezésére és fejlesztésére [Proposal for the reorganization and development of our plant breeding]. Archives of the Academy, Győrffy Barna Iratai 7/1.

Győrffy, B. 1956. A hazai genetikai kutatások eredményei [The achievements of domestic genetic research]. Archives of the Academy, Genetikai Intézet Iratai 3/8.

\_\_\_\_\_. 1959?. A Magyar Tudományos Akadémia Genetikai Intézetének működése [The operation of the Genetic Institute of the Hungarian Academy of Sciences]. Archives of the Academy, Genetikai Intézet Iratai 3/2.

\_\_\_\_\_. 1965a. Az MTA Genetikai Intézete [The Genetic Institute of the Hungarian Academy of Sciences]. Archives of the Academy, Genetikai Intézet Iratai 3/3.

\_\_\_\_\_. 1965b. A genetikai viták alapján kialakult helyzet [The current situation regarding the genetic debates]. Archives of the Academy, Győrffy Barna Iratai 7/1.

\_\_\_\_\_. 1965c. Győrffy Barna hozzászólása a Biológiai Tudományok Osztályának a genetika helyzetéről szóló jelentés-tervezetéhez [Barna Győrffy's comment on the draft report of the Section of Biological Sciences on the state of genetics]. Archives of the Academy, Győrffy Barna Iratai 7/2.

\_\_\_\_\_. 1966. Győrffy Barna levele Straub Brunónak a Biológiai Tudományok Osztályának jelentéséről [Barna Győrffy's letter to Brunó Straub F. on the report by the Section of Biological Sciences]. Archives of the Academy, Győrffy Barna Iratai 7/2.

Lysenko, T. D. 1960. A micsurini biológia időszerű kérdései [The present questions of Michurinist biology]. Lecture presented at the Hungarian Academy of Sciences, 13 January. Archives of the Academy, Elnökségi Iratok 226/4.

Rieger, B. 1949. A mezőgazdasági növénynemesítés ötéves terve [The five-year plan of agricultural plant breeding]. Archives of the Academy, Győrffy Barna Iratai 7/1.

Rusznyák, I. 1966. Az MTA Elnöksége 14/1966. számú határozata a genetika helyzetéről [Resolution no. 14/1966. on the state of genetics by the directorate of the HAS]. Archives of the Academy, Genetikai Intézet Iratai 3/8.

Sárkány, S. 1948. Az Intézet története 1939-1948 [The history of the Institute 1939-1948]. Archives of the Academy, Genetikai Intézet Iratai 3/1.