

# *AUTO-CENSURA* (SELF-CENSORSHIP) – AN ORDINARY AND INTEGRAL PART OF RESEARCH LIFE (?) WORK IN PROGRESS IN A FORM OF AN ESSAY\*

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## Abstract

This work-in-progress paper paves future research regarding the practice of *auto-censura* (self-censorship). Starting from auto-biographical experience, the text shifts into a more theoretical reflection about this part of the scholarly work, which is hidden and unstudied. The working definition of the analyzed phenomenon aims to open an academic discussion about this practice. The paper ends with the call for testimonies and participation in the study.

*Keywords: auto-censura, self-censorships, academic writing, research practice, sociology of science, academic freedom*

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It only takes one person to produce speech, but it requires the cooperation of all to produce silence.

(Zerubavel 2006: 47)

## 1. Introduction

### 1.1. *A case study*

On April 18, 2022, Canadian anthropologist Gregory Forth published a controversial opinion piece in *The Scientist*, a magazine aimed principally at professionals in biological sciences. In the article, “Do members of *Homo Floresiensis* still inhabit the Indonesian island where their fossils helped identify a new human species fewer than 20 years ago?” (Forth 2022a), Forth describes how during ethnographic fieldwork on Flores (an Indonesian island) twenty years earlier, he collected tales of humanlike creatures, “some still reputedly alive although very rarely seen” (ibidem). Comparing fossils discovered in 2004 with his informants’ reports, Forth realized that they might be speaking of the same species. Consequently, this could mean that some ‘non-sapiens hominis’ are not extinct, despite the scientific consensus that they are. In May 2022 Forth published a book, *Between Ape and Human*, showing how scientific and popular narratives corresponded to each other.

Forth acknowledged that he had completed his research on the topic in 2018 and it was only then that he felt ready to write about it.<sup>1</sup> At the moment of the publication of the book he was retired. This raises questions as to what Forth was thinking. Why had he kept secret what he considered an important scientific issue? Was he afraid of the reaction

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<sup>1</sup> This sentence is the part of the letter exchange with Gregory Forth. He also published an article on the topic in 2013, in *Anthropology Today*.

of his peers? How did he go through this process, step by step, from the hypothesis through the collection of evidence, and finally to the decision to publish the discovery?

What I found fascinating in Forth's case (apart from the possibility of living 'non-sapiens hominis', which I leave to experts in evolutionary anthropology) is the timing of the discovery process – from the data collection and its processing to the final publication. If Forth prevented himself from publishing his data for several years, being convinced that he had made a discovery and kept it secret, this phenomenon I will call *auto-censura* (self-censorship; see working definition below). It is the core topic of this essay.

I wanted to know more about this case and asked Prof. Forth for an interview. He agreed and we meet virtually on July 20, 2022. It was a recorded interview, focused only on *auto-censura*. Prof. Forth complained that some journalists had simplified the story and interpreted the process of discovery in a one-dimensional way, suggesting that he was afraid of the reaction of his milieu, and this was the reason he had postponed his book's publication.

In fact the story was more complex. Forth said that several factors contributed to the delay in publication:<sup>2</sup> a mix of standard reasons related to his academic career, the timing of the research, and teaching duties. Asked if he experienced self-censorship, Forth recognized that he certainly had, but mostly unconsciously. He underlined the power of peer review, which all scientists have in mind while writing a paper. Then I listen to his explanations regarding the *auto-censura* practices.

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<sup>2</sup> The book provoked some negative reactions. In the interview with Rohini Krishnamurthy for journal Down to Earth, Gregory Forth said: "I am a participant in the culture of academia. I understand the argument I am making is controversial. I have received a few nasty emails from people saying I have done a great disservice to science and anthropology. Some negative reactions were bound to come my way. I also understand this kind of inquiry on mysterious creatures is held in low esteem in academia. It is for these reasons that I felt a little uncomfortable" (Krishnamurthy 2022).

It was a very inspiring conversation, and its analysis will undoubtedly be a part of my book about the phenomenon. For the most part the discussion confirmed my previous working definition, however I add the category of conscious vs. unconscious self-censorship.

## 1.2. *Working Definition*

*Auto-Censura* (AC) could be called self-censorship in English, however I chose to adapt a title closer to the original *Latin* (which is easily understood in several European languages: in French *auto-censure*, in Polish *auto-cenzura*, in Italian and Spanish *auto-censura*)<sup>3</sup>. AC is probably a common practice, a part of the research process, at the stage between data analysis and publication. During data analysis and writing, researchers are selecting their material and the results of their studies. Usually, the most important findings and data are included in the publication. However, it may happen that a researcher decides not to publish findings, despite the high quality of the data or the high relevance of the conclusions (which means that the Author estimated that the data and the relevance of conclusion are of high quality).

How does this process happen? Why does the researcher decide to hide their discoveries? Which data/type of knowledge is covered by the AC practice? What causes the scholar to change their mind and finally publish the hidden discovery? How do the objective and subjective characteristics of the researcher influence this practice? Are some disciplines, subjects, or methods more susceptible to producing higher intensity of such practices? And finally, a simple question about the working conditions – how does the working environment influence AC?

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<sup>3</sup> I chose to say – auto – also because at that time, I worked in Polish and French – so self-censorship (the English variant for calling the phenomenon) was not immediately in my mind as a term associate with this practice.

In this text, I try to reflect on this phenomenon. It is a sociological issue, because auto-censorship is a production of silence, and this is a collective work (Zerubavel 2006: 47)<sup>4</sup>. How do we, as researchers, through auto-censorship practices, participate in this process of the silencing of creation?

I will enter into the topic by recalling my own immersion in it, since it has been a condition of my work since my first research experience.

## 2. Genesis of the Auto-Censura “discovery” – auto-ethnography of the AC practices

### 2.1. *My first fieldwork – virtuoso violinists and their small world*

I have struggled with the problem of choice – what I should write and what I cannot – since my first research experience. The fieldwork that was the basis for my Ph.D. thesis (2006) many years ago was conducted in the music virtuoso environment, which was partially the environment of my own family and professional lives. Doing a study in a familiar setting was compatible with the old Chicago sociological tradition, especially as seen in Everett Ch. Hughes’s pedagogical strategy in the mid-20th century. Hughes, an excellent teacher, encouraged his students to do their fieldwork in their “natural” environment, and this was a successful strategy. Many Chicago sociologists conducted thesis work based on their family occupations, fathers’ jobs, original family culture, or hobbies (see Harper 2017; Chapoulie 2001). When I joined the Ph.D. program in Paris, where we worked following the Chicago sociological tradition (Chapoulie 2001), I was a professional musician, trained in Poland, who had lived in France for ten years.

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<sup>4</sup> I will explain later that some areas of knowledge production are taboo. There are some topics not covered by studies, the result of collective tacit agreement about such forbidden subjects.

I originated from a musical family, my children were trained as musicians, and my partner played music. My social world was one of classical music, in particular the violin virtuoso universe, which at the time (late 1990s) in the Parisian region, was dominated by recent Soviet (and Eastern European) emigrants. As an ethnographer, I did mainly participant observation; rarely I went undercover, playing the role of the mother of a young virtuoso student. At that time, studies under-cover in France were not yet restricted. My Ph.D. program included ethics courses with clear rules: do not harm participants, a fundamental principle for all trained anthropologists. In the first instance this meant not exposing the identity of our participants in our writing. Anonymization, however, could be difficult if the studied individuals and groups were small and easily recognizable, as in the field of musical virtuosi.

When I started writing my Ph.D. thesis, my main problem was that a lot of data (notes from observations, situations, stories) were too specific to be told. The music world and what I observed in my field were not simply an environment of passion for the arts. Intertwined in this professional elite universe are colossal levels of competition, jealousy, pressure, mobbing (bullying), struggles over money and jostling among the candidates for rare places on the front stage. My own children were inside this boxing ring, and I could never write about everything I learned – I had to keep a lot of data to myself. I needed to protect my participants – and my children’s professional life; they were in the middle of this race for places in a highly competitive world. I could not allow them to fail because of my work. As a result, some critical knowledge remained unpublished, and I was not fully satisfied as a sociologist.

My Ph.D. thesis was written in French but never published in that language. I translated it into English and published it in the U.S., hoping that ten years after my study was conducted, many events, people’s stories, and relationships would be forgotten – undetectable. I made

few efforts to publicize this work, possibly out of fear of hurting my participants, even though I had made substantial cuts, my first acts of auto-censorship. I was convinced at that time that I paid this high price because of my familiarity with the field. But speaking with some ethnographers about their fieldwork, I realized that this is a phenomenon much more frequent than I expected.

## *2.2. The privileged discussions with almost retired successful scholars*

My post-doc position took place in Warsaw in January 2007. The transition between my work in French academia (EHESS, where I did my Ph.D.) to the Polish university environment (the University of Warsaw is the largest public university in Poland) went smoothly. I worked as a researcher in the Center for French Culture, in the section of social sciences; the unit was managed from the French side by Sorbonne and EHESS and from the Polish side by the University of Warsaw. The French Ministry of Foreign Affairs provides the primary financial support via the French Embassy in Warsaw. Half of my position was my research project (which focused on laboratory research careers – I was already letting go of the music virtuoso field), while I also organized the visits of eminent French scholars to Warsaw to share their knowledge with young Polish researchers. This was before the EU Council created Erasmus Plus<sup>5</sup> – a program that enabled one-week stays for visiting professors – however, we also hosted French professors for one week. Once a month I spent a few days with the visitors as their immediate host. It was a privileged position from which I could learn a lot about

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<sup>5</sup> Erasmus Plus is an extension of Erasmus program [EuRopean Community Action Scheme for the Mobility of University Students] – Erasmus Plus is the exchange of university teachers – started in 2014.

their careers, the researcher's life, and unique experiences that were only rarely part of their books.

All these backstage stories helped me to understand that these scholars had accumulated a mass of knowledge during their careers (most were retired or semi-retired; we rarely hosted young scholars). The most spectacular guest was anthropologist Jeanne Favret-Saada, who did fundamental work on witchcraft in rural Bocage, in western France (see Favret-Saada 1981; 2009). Her experience revealed the risks of participant observation as those that occur when a researcher loses control of publications related to her subjects. An editor at a leading French magazine twisted the content of an interview with Favret-Saada (the interview had been conducted by another journalist), and betrayed her pledge of anonymization by taking pictures in the village she studied (the journalists had tricked Favret-Saada into providing its location). The article was published without the anthropologist's permission, and it presented the villagers as ridiculous, uncivilized people. Favret-Saada was obliged to stop her fieldwork because her study participants were offended (*Les Possédés et leurs mondes*, 2018 [video])<sup>6</sup>. She shared her experience in detail with the young Polish scholars during her visit, elaborating on the specificity of her work and the results of her studies.

The meeting with Favret-Saada was for me the beginning of a series of considerations about what we ethnographers can publish, and what we cannot. I hadn't yet considered the issue in the context of auto-censorship, a concept that grew as I began questioning all visiting professors who were open to sharing the backstages of their work. I would become convinced that this auto-censoring publication practice was specific to qualitative methods in sociology, anthropology, and all direct

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<sup>6</sup> The backstage of her work was published in her book: *Deadly Words: Witchcraft in the Bocage* (2010), published in French in 1985. See also *Les Possédés et leurs mondes* 2018; Lagrange 2012.

contact with participants. And I asked myself if this practice was also known to life science researchers?

### 2.3. *The observation of laboratories and auto-censorship among life science researchers*

In 2003 I for the first time entered a research laboratory. I had no formal project at the time but was encouraged by a Principal Investigator (PI), a friend who was interested in sociology and had some management issues. He hoped I could help him resolve some minor misunderstandings, and in exchange, he would help me in my work. I was curious how much my knowledge gained among musicians – about their elitist socialization, excessive hard work and training, and career construction processes – could be adapted to this new environment.

After first introductory work in France, I opened my regular fieldwork in Poland in one of the best institutions for molecular biology research (beginning in 2006). There, I shadowed the work of scientists; I was present at their meetings, formal and informal venues, during ordinary laboratory days, and exceptional conferences. On one of these meetings, we (I was a part of the lab) hosted a collaborator from another country, a PI who came on a short visit (the lab and the visitor planned to work in parallel on the same project). During a methodological discussion, the Polish investigators proposed an experiment. However, the visiting PI said it would not work because of a genetic problem. His Polish collaborators were puzzled. There were no publications, by the visiting PI or anyone else, that would explain his position. The visiting PI responded:

In our laboratory, we studied this issue; we had some promising results. However, after a couple of months, we could not replicate the same experience [which

is necessary according to the research protocol]<sup>7</sup>. We lost six months before learning that our supplier exchanged the mice and that the ones we'd gotten were animals with a different genetic provenance, produced by another firm. That is why our experiment stopped working. In such a way, we learned that genetics is a crucial problem in the disease we studied, because the other groups of mice did not get at sick at all with the same treatment<sup>8</sup>. This was a big discovery for us, and we had to do another type of experiments to prove it.

This PI explained that the lab could not publish the first results because they were made in error: the mouse delivery service had failed to respect the contract, and checks were not done as they have been according to the protocol (here the researchers were in fault). They learned a lot, but it was unpublishable. Two years later, the group published the results of its supplementary study.

That was an example of auto-censorship in practice in life science.

There were many other situations that I observed where researchers (individually or in a team) were constrained to auto-censor their publication. (I will develop this question later).

After hearing the story about the error caused by the wrong mice, I searched for publications about auto-censorship.

## 2. 4. *Literature inspirations*

In 2007, the Polish Academy of Sciences reprinted the selected papers of the Polish-Jewish physician, microbiology researcher and sociologist of science Ludwik Fleck (1896-1961). Many of the analyses of scientific work were published only in Polish in prewar scientific and medical

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<sup>7</sup> The comment in square gaps is mine.

<sup>8</sup> Their project was composed from two parts: first was to take healthy mice and made them sick – then try various treatments (this is a typical work of people working on curing drugs).

journals, and in the popularized science press. This revival of Fleck's work offered a unique opportunity to learn about his whole scholarship. This literature was enlightening for my observation work, primarily because of the proximity of our methodology<sup>9</sup> and the context of the researchers' work (basic research, molecular biology, teamwork).

In my opinion, Fleck's perspective was and remains the most appropriate for understanding the cognitive aspects of research work. I found nothing in Fleck's work that specifically dealt with auto-censorship. However, I borrowed from Fleck the idea of communities of thought, and have used it to understand the tacit passage of knowledge not only in a positive sense (as the inspiration for developing knowledge) but as a limitation – the tacit transmission of interdictions, of limits, which prevent people from publishing the results which are not compatible with their "thought collective." While I saw in Fleck and in my own work was, as part of socialization to the scientific world and academic culture, paradigms were to be respected and it was complicated to challenge them (a lot of stories are told about the revolutions that young scholars prepare for changing the paradigms imposed by their mentors). Fleck was one of the first authors in the field, which was later called science of science (naukoznastwo) and which started in Poland in early 20th century (Kokowski 2015).

Another important thinker representing this field was sociologist Stanisław Ossowski (1897-1963). In his inspiring paper published in 1956 under the title *The Researcher's Duty: Obedience in Thinking and*

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<sup>9</sup> I observed researchers who employed microbiology approach; some of them worked in areas similar to Fleck's field. However, this was the 21st century, not the mid-20th century, and the difference in technology in such research areas is critical.

*Social Duty of a Scientist*<sup>10</sup>, Ossowski focused on censorship and its influence on the researcher's work. He also opposed the imposed state Marxist ideology as a unique scientific approach. Censorship was one of the most critical problems of scholars (humanities and social sciences) in the so-called communist states. It was a part of the specific context in which Polish scholars worked from WW2 up to 1989 when official censorship was abolished as a part of democratic changes. Other than in the work of these two authors, which inspired my thinking, I was unable to find other texts responding to my questions about auto-censorship practices, other than those raised in the context of limits on freedom and official censorship.

#### 2.5. *Contextual influence – Official Censorship and Auto-censorship practices in the communist states*

Ossowski wrote about creative workers and their constraints within an imposed ideology. "The attempt to transfer the methods of militant organizations to the field of scientific and artistic creativity has yet another aspect related to the obedient stance in the quest for new truths and new scientific, artistic, or moral values. (...) Disobedient thinking is one of the duties of the research worker." (Ossowski 1998 [1956], 93).

Auto-censorship is a tool that research workers use when practicing obedient thinking. When scholars work in the context of reduced freedom of speech, as in the case of the communist states, they practice auto-censorship constantly. There are some studies about this kind of writing in literature and film, but not as much regarding academic disciplines (Romek, Kałmińska-Chełminiak 2017; Romek 2018).

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<sup>10</sup> The paper published in Polish in 1956, at a time of political thaw, in the journal *Przegląd Kulturalny* 13, under title "Taktyka i Kultura" [Tactics and Culture]. It was partially translated into English and published in *Polish Sociological Review* (1998) in the special issue "Polish Sociology under Communism".

A notable exception occurred in the field of biology in relation to Lysenian genetics, and in history, which in communist states was under the close control of authorities. However, as Polish historian Patryk Pleskot notes, “The subject of censorship in Polish science, although widely known and noticed, has not yet become the subject of a thorough research analysis – at least in the field of historical sciences – being only on the fringes of works on the history of historiography” (Pleskot 2011: 111; cf. other publications about the censorship in history: Romek 2010; Pleskot 2010; Konopska 2007).

This limitation significantly influenced the careers of historians. The Middle Ages was a safe career choice for Polish historians and they had many successes in this area. Twentieth Century history was, of course, always risky (see the example of Romek 2000; Modzelewski 2013).

I was born in 1964, and remember periods (especially under martial law in 1981) when freedom of speech was curtailed and, even as a high-school student, I learned how to read “between the lines.” It was a form of popular art in Poland (and in other countries with restricted freedom) to learn how to read a book or a journal article, or watch a movie, while deciphering each slight allusion. It was at times a sophisticated exercise, at times humorous, but always necessary for understanding what was being said in code.

Poland was not the first state, nor Polish the first language in which authors performed this double language. From Ancient Greece we have the example of Aesop (although he may have been a composite historical figure), who passed into history as a storyteller who bypassed political censorship using animals to personify his fables. His metaphorical language is now described as Aesopian. In Poland, we cultivated this art and this language as a way of avoiding official censorship, and recalling these practices has been helpful in my research.

## 2.6. *History writing as a risk*

I spent seven years working on a biography of Zygmunt Bauman, whose career as a globally known sociologist provided an excellent case study. I wrote the book in English and then translated it into Polish myself. I needed total control over each word to express everything the way that I wanted (translators do not always satisfy authors' choices). The leading theme in this biography is the antisemitism that shaped Bauman's life, and especially the racist rules practiced in the interwar Polish schools, from primary to university. Before, during, and after WWII, antisemitism in Poland was an element of the social norm. Writing about it today is not without risk for authors. As I was working on the book in 2018, the Polish government passed a law forbidding certain kinds of writing about the Polish state and nation. Victimhood of the Nation is a legally protected value in Poland (see Gliszczynska-Grabias & Sledzinska 2018; Wyrzykowski 2018) for which there is a penalty of imprisonment of up to three years.

The law put pressure on authors whose findings would provide evidence of wrongdoing by the Polish state or people, by making such conclusions punishable (see the process of Barbara Engelking and Jan Grabowski in Gessen 2021; Charlish & Wlodarczak-Semczuk 2021). The potential three years in prison were particularly threatening to historians, especially experts in Holocaust studies who were working on studies of the contribution of Poles in the persecution of Jews. The situation was reminiscent of the pre-1989 period, in which the state imposed an interpretation of the attitude of non-Jewish Poles toward their Polish-Jewish neighbors, according to which Poles were courageous and righteous while suffering Nazi oppression at the same level. Between 1990 and 2015, Polish scholars were free of such heavy political pressure, but today, freedom of speech is menaced (Hackmann 2018). This pressure is not of the same degree as it was in the pre-1989

period, yet it may have a powerful influence on some scholars, particularly those in precarious financial situations or those seeking promotions in their careers.

In my work on Bauman's biography in Polish, I didn't make any such cuts. I was careful to be specific and to have documents and proof supporting each conclusion. The Polish version of that book is more extended because I needed to cite all the documents extensively. My auto-censorship practice was limited to the choice of terminology (I avoided some sensitive terms, such as nation, which I replaced with society).

While I refused to adjust my texts to respond to the law, I was curious to see the impact on historians who study Polish-Jewish relationships, and interviewed a few individuals to see if they were practicing auto-censorship. They responded that writing articles and books in their specialties had become challenging, and that some of their colleagues were dealing with lawsuits.

Poland is not the only state in which scholars may feel that their academic is restricted (for the restrictions in Poland see: Kichelewski 2020; Gessen 2021). In France, state security services requisitioned all the materials collected in Corsica by researcher Thierry Dominici, who subsequently was unable to publish an academic text stemming from his work on Corsican nationalism that constituted his Ph.D research (his research material was confiscated by authorities; Laurence & Dominici 2016). Most often, the texts dealing with state control in the work of researchers concern states under dictatorship or run by an authoritarian government (Aldrin et al. 2022).

While government institutions can cause a scholar to self-censor, it is often the activity of a powerful informal group limits the researcher.

### 2.7. *Breaking the silence – Ostracism by the milieu*

Working on the careers of sociologists is not a good idea for a young scholar, especially when the subjects are stars or powerful – for example, heads of large networks, or informal gurus, gifted theoreticians cited by almost everyone in their fields. In 2011 I published in Poland a long article about the career and collaborations of Pierre Bourdieu (Wagner 2012). Several readers in Poland were shocked to learn that the style of collaboration of this French sociologist, who was known for his leftist political orientation and studies devoted to “the oppressed,” acted in his professional milieu similarly to an aggressive corporation or, as some said, “his laboratory worked as the royal court in Versailles.” My paper evoked a part of the history of sociology that is infrequently mentioned, especially by people who belonged to Bourdieu’s network and still admire leader, overlooking the “feudal” relationships that were endured. The publication of such analysis, even if exciting, is risky, because the milieu’s ostracism could block career development. Have an English version of it, and for ten years, following the advice of one of my older friends, I haven’t publish it... yet.

Silence is a collective phenomenon. In order to be efficient we must learn about the spaces covered by silence, and we must obey by keeping silent about such things. For instance, in the above-mentioned case, no one dares to publish a critical analysis of that star sociologist’s career, although the evidence is ready to hand. It’s an “everyone knows,” but no one dares say, phenomenon that enables social and professional control, and it is also a case of *auto-censura* – avoiding a risky topic<sup>11</sup>. In this case, the risk is to career progress of the almost-author. In other

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<sup>11</sup> Bourdieu’s theory is criticized (Verdès-Leroux 1998; Verdrager 2010), however to my best knowledge, there is no academic analysis of his style of managing a team and co-working.

instances, silence is imposed by threats coming from informal groups outside the university.

### 2.8. *Risky fieldworks – Protection of participants and protection of the researcher*

When I started researching forced migration, I became aware of the extremely difficult nature of the field environment. I observed in every case the principle of protecting weaker participants and preserving their anonymity. But during the observation, I was mainly focused on respecting their dignity. In some cases, taking part in an open conflict between two observed groups by defending the abused participants is also a researcher's duty. In highly risky environments, this kind of conflict can be dangerous. I didn't measure my safety before entering this situation, and after a year of fieldwork it became difficult for me to stay in a place where I witnessed several irregularities. I was forced to change my observation settings and collect my data in another place. I also had to be careful with my writings, not only for the participants' protection but also for my own and that of my family. I have been forced to auto-censor this section of my work even now. I am unable to present specific data from that research.

*Auto-censura* that occurs in this sort of "risky" environment, where matters of life and death are at play, is probably unusual in our professional field. So are instances in which the most important findings of a study (and I consider these to be the most important of my professional life) – cannot be published.

Obviously, however, I am not alone in this situation. A couple of months ago, four ethnographers published a fascinating book about the dangers inherent to their research that provoke auto-control and *auto-censura* practices. The work entitled *L'enquête en danger: Vers un nouveau régime de surveillance dans les sciences sociales* [Fieldwork in

danger: toward a new regime of control in the social sciences] by Philippe Aldrin, Pierre Fournier, Vincent Geisser and Yves Mirman (2022) provides cases of difficult and risky fieldwork and discusses the situation of the researchers, their working conditions, and problems related with the publications of the results of the studies. This book is an exceptional contribution and perhaps opens a larger discussion about the *auto-censura* practices.

### 2.9. *Black Box and other strategies*

Over time I accumulated data in a space I started to call “the black box.” What could I do with all this “auto-censored” knowledge? I tried to find the solution to this problem, but I didn’t find it in the literature. A lot was published about the protection of the participants, but few texts analyze the problem of the researcher’s security. There is literature about risky settings, but it concerns the data collection and not the writing stage (see Aldrin et al. 2022 denouncing the lack of the protection of researchers). I could not find reflections on how to deal with the ostracism of the milieu. Nor were there texts with considerations on how to write about a society in states that forbid “slandering the good name of the nation” (see about the law: Gessen 2021). And no one offers advice about how to publish relevant data from fieldwork while living under serious threats. So what could I do?

Often I received the advice: keep quiet and publish after a couple of years; ten years, in the case of the famous sociologist career paper. I also waited almost ten years before publishing my book on virtuosos. Publishing in another language is also a way of changing readers, restricting access to the texts (this strategy is in total opposition to the major strategy of researchers – to get the highest visibility). Publishing in rare languages (such as Polish) could also be a good solution, but not if this is a publication on the internet. Unfortunately, google translate program

gives access to the text quickly. There is also the radical solution of not publishing at all.

Another strategy would be to employ an Aesopian writing style, using a pseudonym, or even publishing outside of the world of academia – not as a scientific text. There are some solutions that I found potentially accessible. But I was still not satisfied. Being in such an impasse, I followed the advice of Charles Wright Mills (1959) and transformed the issue (I cannot publish my important data) into a sociological problem (*auto-censura* in research writing).

My personal constraint of *auto-censura* became my research topic.

#### 2.10. *Processing...*

How do other researchers manage the similar situations I went through in each project? What categories of data/results are they putting under-cover? What happens with the data set in the “black box”? These and other questions passed through my mind constantly. I brainstormed with my closest friends over a couple of months. I had difficulties believing that nobody had ever worked on this phenomenon. Then, I talked with some of my trustful colleagues. I questioned them about the viability of such a project and the pertinence of the question.

*Auto-censura*: How do we do it? When? Why? After a couple of months, I started to present the idea in front of unknown people – speaking about it at two conferences, then during two scholarship interviews. The reactions were, in a large majority, very enthusiastic. I collected a lot of new examples from various disciplines. Sometimes my colleagues expressed modest reservations about methodology, but most often they shared my enthusiasm for the topic. Two of my colleagues were openly skeptical, and others shared their doubts about getting funding for such a project. But here is my proposal:

### 3. AESOP – a research project – work in progress

#### 3.1. *Acronym and objectives*

Whenever I prepared a research project I try to find an attractive acronym, a catchy name for a fascinating issue. In this instance I started with “black box,” but decided instead to celebrate Aesop, and his Aesopian method of coping with censorship. The acronym AESOP stands for Autocensorship Entanglements: Sciences’ Occulted Practices.

The working definition of auto-censura (AC) is the following: a practice that leads to the non-publication of all or part of research results in a conventional form; it is a practice covered by silence. AC is a task that is part of the research process, occurring at the stage after data collection and analysis, and at the beginning of writing.

The objectives of the AC study will be to understand the whole process and to identify the logic of *auto-censura* practices. Also, I will map the spaces covered by these practices with a dynamic and multidimensional approach. The project will consider the intersection of the disciplines and take into account the characteristics of the context (place, politics; history, and language), the professional background of the researcher, their auxiliary characteristics (gender, class, ethnic origin, religion; see Hughes, 1971), and status (stage of career, being “inheritors”, i.e. daughters and sons of the scholars, see Bourdieu 1984).

The project aims to study the process, understand the dynamics, and capture moments in which the researcher breaks with *auto-censura* related material. Moreover, the study will reflect on the trust and spaces of exchange in the academic world, examining the reasons for a lack of studies regarding this practice, and the absence of interest in it. Why is *auto-censura* ignored? Is it perceived as a shameful practice that contradicts the image of academic freedom?

### 3.2. *Theoretical Approach – thinking with Hughes, Fleck and Ossowski*

The fleeting, confusing, secret, difficult-to-study nature of AC probably at least partly accounts for the absence of studies of the phenomenon. A large part of academic writing is an individual, solitary activity and the reasons for hiding essential results are not always the subject of discussion among authors, for the same reason that they are suppressing these pieces of knowledge in the first place. The phenomenon of AC can be more explicit when researchers are working in teams. In both cases (group or individual activity), symbolic interactionism<sup>12</sup> is the better theoretical framework for understanding this phenomenon, enabling us to study the spaces covered by silence and the content suspended from scientific communication. AC is a process in which two parts interact – author(s) and public (peers); the former limits their actions with regard to their concerns about the reception of the content. The publications' consequences are considered in a way that frames the content. Suppose the author believes that the sensitive content will expose themselves or the participants. In that case, the author modifies the content or suspends the sensitive parts – abstention from the publication being the extreme version of this.

The interactions described above belong to the area of sociology of work as applied to scientific occupations. The approach developed for this area by Everett Ch. Hughes provides inspiring tools, and thinking with Hughes helps shape my analysis of AC practices (Hughes 1971). The following elements constitute the focus of Hughes' approach: control over the work results (publication), hierarchical positioning, career advancement and status of a researcher (permanent contract/tenure or

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<sup>12</sup> Symbolic interactionism (SI) is a sociological theory born at the beginning of 20th century at the University of Chicago, with roots in pragmatism (George Herbert Mead), sociologist from so called Chicago School (Herbert Blumer – theorized the SI; other sociologists worked in the framework of SI, among them Everett Ch. Hughes, whose conceptual tools will be employed in this study).

freelancer – grant hunters), the transmission of tacit knowledge, career processes, socialization in research work, the construction of prestige, recognition, and peer influences.

The more specific tools for investigating AC are borrowed from this theoretical approach as applied to creative settings. The work of Robert Faulkner (1983) on creating movies and movie directors' collaborations with composers is especially relevant. Another essential theoretical frame will be the analysis of the concept of the convention. The theory of the change of conventions proposed by Howard Becker (1982) seems fruitful for understanding the breaking moments when an author decides to publish previously auto-censored content.

The epistemic cultures approach may help understand the differences between AC practices in various disciplines and sub-disciplines (and different methodologies). Mainly, I will try to catch the dynamic of tacit knowledge transmission and how it shapes *auto-censura* practices (interdiction learned during socialization).

I plan to focus this project in part on migrating researchers, those who not only have changed their workplace (laboratory, institution, country of work) but changed their conceptual environment by working in the framework of new approaches. This work will be employed in the framework of Ludwik Fleck's thought styles and thought collectives (Fleck 1935; 1970). Based on my preliminary data and initial analysis of the phenomenon, I plan to extend Fleck's approach by adding the component of changes in the scientific environment.

In scientific work, the most important *auto-censura* practices are *auto-censura* of ideas. Fleck (1979: 105) referred to this as the self-containment of thought:

A thought collective consists of many such intersecting circles. Any individual may belong to several exoteric circles but probably only to a few, if any, esoteric circles. There is a graduated hierarchy of initiates, and many threads connecting the various grades as well as the various circles. No direct relation exists between

the esoteric circle and that creation of thought [*Denkgebilde*] but only one mediated esoterically. Thus most of the members of the thought collective are related to the works produced by the thought style [*Gebilde des Denkstiles*] only through trusting the initiated. But the initiated are by no means independent. They are more or less dependent, whether consciously or subconsciously, upon “public opinion,” that is upon the opinion of the exoteric circle. This is generally how the intrinsic self-containment of the thought style with its inherent tenacity arises.

What Fleck calls self-containment of thought is a category of *auto-censura* practice. I am developing Fleck’s approach by looking at situations of the geographical migration (new environments; different work/organizational culture, different types of socialization)<sup>13</sup>. Fleck explained how the proximity to the esoteric center influenced the possibility of concept creation. I will explore here another possibility – not regarding the center of circles but the outsider’s position (migrating/exiled scholar). What happens when a researcher changes the close environments that frame their scientific work? How does this situation influence their emancipation from previously incorporated systems of opinion?

Analyzing the process of concept creation, Fleck (1979: 27) explains that [o]nce a structurally complete and closed system of opinions consisting of many details and relations has been formed, it offers enduring resistance to anything that contradicts it. A striking example of this tendency is given by our history of the concept of ‘carnal scourge’ in its prolonged endurance against every new notion. What we are faced with here is not so much simple passivity or mistrust of new ideas as an active approach which can be divided into several stages. (1) A

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<sup>13</sup> I thank Łukasz Jarnicki for the discussion about Fleck’s theory and its interpretation. For controversy about Kuhn’s inspirations, see Jarnicki 2021. For Fleck’s approach regarding style of thought in the contemporary understanding outside Poland (it is important, while the understanding of Fleck depends from the language of his texts, as Jarnicki has shown, the English translation contributed to the wrong lecture of Fleck’s theory; Fleck wrote in Polish and German and these both versions are considered as original; more about the reception of Fleck see, Braunstein 2003; about erroneous translations, see Jarnicki 2016.

contradiction to the system appears unthinkable. (2) What does not fit into the system remains unseen; (3) alternatively, if it is noticed, either it is kept secret, or (4) laborious efforts are made to explain an exception in terms that do not contradict the system. (5) despite the legitimate claims of contradictory views, one tends to see, describe, or even illustrate those circumstances which corroborate current views and thereby give them substance.

I am interested to learn how *auto-censura* practices are connected to the stages of the process presented above. How do these practices impact scientific knowledge elaboration? We are here in the core of a cognitive process that has not yet been studied in connection to AC practices. I intend to explore the spaces, following Fleck's expression, that appear "unthinkable," "unseen," and "kept secret." I would try to understand how the former interdictions (learned in the process of socialization in the first place) operate when the institutional and/or cognitive structure that was active before migration no longer has control over the researcher's communication? We can easily imagine that a scholar leaving an oppressive state and publishing in a new space will be able to communicate more freely in the new context; however, it will be difficult for them to incorporate the new set of tacit interdictions.

Being in a new place help them leave the former constraints, while at the same time they will not yet have incorporated the new limits (especially those involving the non-verbalized transfer of knowledge). This being the case, are these exiled/migrated researchers more free? Do they experience less of the pressure that results in *auto-censura* practices? When people are changing their routine, when they are changing the workspace and their teams, and especially their thought collectives, this could favor emancipation from previous limitations, making "transgression" easier. By transgression here, I mean the decision to publish previously suspended content. It is important to remember that it is not only the case in repressive regimes' contexts but also in all academic environments that provide a frame for scientific communication.

The final framework to be employed in our analysis of the data comes from the reflection on the production of knowledge in the context of limited freedom (political dictatorship and official-institutional censorship). These imposed ways of scientific thinking are easier to study than the influence of ever-present social norms that shape our way of thinking and our style of writing.

There is a permanent tension over what an author can publish in a text, anticipating the censors' reactions. What can be published without negative consequences, and how far it is possible to go without damaging a scientific career? Stanisław Ossowski's short text, mentioned above, is essential for understanding AC phenomena in this context. In a section of his work titled "compromises with the truth," Ossowski (1956: 96-97)<sup>14</sup> wrote:

Under the conditions of subordinating science to the directives of the political prosperity, over time, scholars develop specific psychological attitudes, elaborate specific methods of conduct and particular norms of moral evaluation. A common phenomenon among party and non-party academics is compromise attempts, which can be formulated in two ways: how far can one go along the line of obedience without losing respect as a scientist? Or – how far can one's own conviction of truth guide one without getting off the line?

Many scientists have faced the question: is it worth compromising the truth in order to be able to conduct socially useful research? But compromises with the truth corrupted, introducing a technique of half-truths, enabling multiple interpretations so that one can defend oneself both against those who attack in the name of defending the political line, and those who attack in the name of the truth. The technique of protective phrases or protective quotes is developing. The attitude of a research worker is replaced by the attitude of an advocate who focuses his attention, not on the correctness of the views but on their defense in the face of a twofold danger. With time, the research becomes accustomed to the

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<sup>14</sup> Translated from Polish into English by the Author.

methods of protection *per fas et nefas*. Compromises with the truth corrupt morally and mentally; they cause indifference to issues. This is not a good foundation for building a new culture.

The sociologist Everett Hughes highly valued what he called “extreme cases.”<sup>15</sup> Ossowski's reflections were born in circumstances that could be considered an extreme case of scholarly activity performed under dictatorship (though pre-1956 Stalinist conditions in Poland were not as confined as those in Nazi Germany or Stalin's USSR). Investigating *auto-censura* practices that shaped knowledge production in such contexts of limited academic freedom seems to be an excellent introduction to this unexplored topic and may be the first part of a larger project.

While AC is a stage of scientific work and a step in the process of knowledge production, the question of its origin should not be avoided. The project must be grounded in the history of science. Following experts who are interested in the sociological questions regarding the scientific work in the past centuries — such as Steven Shapin — it seems necessary to look on the traces of the AC practices in the past. Thanks to the comparison of contemporary and past practices, we should be able to detect not only the impact of the new technologies but also (perhaps especially) the influence of the modification of scientific texts' accessibility (through the Internet, including automated translation services) on the practices of *auto-censura*<sup>16</sup>.

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<sup>15</sup> Hughes showed that in extreme conditions or extreme cases of studied phenomena, hidden processes became more visible, and by consequence easier to observe and to study, see Becker 2010; Helmes-Hayes 2010.

<sup>16</sup> Immediately two problems should be mentioned: 1) some disciplines shifted their communication styles to make them more accessible to the larger public, and the content of the texts is thus more accessible; 2) language of communication is less important than in the past, since thanks to AI and translation programs, it is possible to access the content of the publication even in an unmastered language.

In coherence with the theories mentioned above, we will analyze the data in an inductive way. The following general research questions will drive the project:

- How do *auto-censura* practices occur, and how are they shaping the process of knowledge construction? This question should reveal the process of AC in a step-by-step fashion, how it happens and what the missing pieces say about our societies. For what purposes is this knowledge hidden and made unavailable to the large public? Importantly, the evidence in question remains invisible, which I consider a loss for the global knowledge. However, we know very little about this phenomenon.
- What type/kind of knowledge are we losing in *auto-censura* practices? Sometimes entire research communications are kept secret for years. When the environment exercises robust control over the researcher's publications, *auto-censura* may take an extreme form – non-publication. Zygmunt Bauman mentioned his colleague Edward Lipiński's advice regarding scholarly activity in a time of strong political restrictions: "First of all, do not think (...) if you cannot stop thinking, then don't talk! And if you can't stop talking, don't write. If you cannot stop writing, never, ever publish! Under those circumstances." (Bauman cited in Wagner 2020: 203). In parallel, I should be precise that I do not consider the publication of the results of the studies as an absolute necessity. Sometimes, sensitive content should not be available to the large public or even to the scientific larger community. In such cases, safe spaces may be created in which to communicate previously hidden knowledge. Such spaces may include conferences and professional/research meetings, mainly practicing the oral presentation of data. Their ephemeral character (in comparison to publication) can be considered less risky.

However, due to the increasing competition and the new management organization of academic work (*punctosis*<sup>17</sup> and the publish or perish rule), conferences today are more frequently places where scholars present already published data, because they fear losing ground in their field of expertise if they disclose new content (a discovery) before publishing it. In several disciplines, the rule – there is no second place, only the first one – is maintained, which makes competition extremely aggressive. The following question should be asked – to what extent is it possible to create safe spaces? Is it legitimate to consider creating systems that enable the limited circulation of sensitive knowledge?

### 3.3. *Auto-censura practices*

Based on the auto-ethnographic material<sup>18</sup> and the partial data from my previous projects (which were not focused on auto-censura but contained some data on this phenomenon), I would propose – in a mode of “work in progress” – provisory categorizations of the main AC types.

Moreover, I will add some additional information regarding the importance of the researcher’s capital, which could be employed in the analysis as a variable. Moreover, I focus on the areas of AC that deal with taboos and interdictions present in each discipline.

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<sup>17</sup>This term appeared around 2010 in discussions among Polish academics. It was a derivate of *grantoza* (grant-hunting), employed in the press by Agnieszka Graff. At a 2012 seminar for PhD students and at later events, I presented the following model of academic career:  $punctosis + grantosis = carierosis$ . It was a way of denouncing the perverse effects of an accounting approach to academic and scientific work, see Pilawski 2016.

<sup>18</sup>Auto-ethnographical material – the data which were collected by authors themselves, about their own experience. In sociology, this methodology is called auto-ethnography.

a) Categorizations (introductory)

Firstly, the AC practices could be divided into individual and collective work, based on the organization of the project as well as disciplinary divisions and subdivisions related to specialties and methodology. The latter distinction seem even more pertinent, because the same discipline may contain distinct work organizations (ranging from extensive research teams to individual working scholars). In sociology, for example, a large international comparative survey, in which the authors' list may include many names, contrasts with individually conducted ethnographic research. (According to the classical protocol for ethnography, each stage of the project should be conducted by the same researcher: from fieldwork and long-term data collection through the transcription of the interviews, coding and analysis, to the writing of the final account). These contrasting work organizations are present in other disciplines as well.

The second categorization is based on the nature of the control system: external (such as political/state) and internal (inside of academia). The extreme case of the former would be state dictatorship; the latter includes local schools of thought, informal thought collectives, networks of support and coteries. There is also a need to look closely at the limits imposed by the general context – the system that frames knowledge production. The primary classification reflects the organization of control: centralized and decentralized. In the former, the state performs control via bureaucratic institutions, such as the Ministry of HE<sup>19</sup>. In a decentralized system there is no formal central control and

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<sup>19</sup> Ministry of Higher Education. In Europe, each state has the ministry of HE – a central power that supervises universities and public research institutions (the state finances most research activity and university education in the majority of EU states; access to university training is usually not limited by finances — the fees are not very high and in some countries there are no fees at all for university

control of the scientific activity is exercised by local authorities, private groups, families, businesses, and pressure groups.

Thirdly, AC practices may occur as a response to imposed epistemic cultures (Knorr-Cetina 1999), but also more general features such as the language of communication, particularly cultural, political, and historical contexts that frame the publication process (and publication spaces).

The following categorization, which should be addressed, is related to the researcher's positioning. Their professional background, auxiliary characteristics (gender, class, ethnic origin, religion, family composition, financial background/ownership of the apartment, ability to geographic mobility) and status (stage of career, from "inheritor" to "outsider," tenured professor or freelancer working based on grant support).

Finally, categorization focused on the nature of knowledge transmission and its official and tacit forms. Collectives of thought are formed and maintained during master-disciple and teamwork relationships, and built and maintained over the years. This is a long-lasting process in which interactions may be formal and informal, involving sharing of knowledge through verbal and non-verbal, as well as conscious and unconscious means, as is the case for elements of the socialization processes. The transmission of tacit information regarding AC practices must also be studied as a socialization process.

There will undoubtedly be other ways of categorization; however, before conducting the project, it is impossible to address all categories that will be taken in consideration during the implementation of the formal project.

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education (there are additional fees, such as for library privileges and social security, but the lectures are free).

## b) Specificity – Protecting the Researcher’s ‘Capital’

Preliminary data provide some information on AC practices that appear to result from expectations that publication may bring adverse effects. Sometimes, these negative expectations are related to an atmosphere of heavy competition context, mistrust of peer reviewers and fear of intellectual theft. Scholars self-censor because they worry about losing credit for their research results. This kind of apprehension grows with the conviction that the findings are original, rare, exceptional or groundbreaking. The AC practices may be preventive, not referring to the researcher’s safety but rather their status and ownership of data – the protection of the researcher’s “capital”. Their ideas are the treasure kept behind a veil of silence into the moment when the author will decide that the risk of intellectual property theft is weak, or the possibility of losing the opportunity to be first to announce the discovery is high. At this point the scholar decides to prioritize the discovery and release the auto-censored knowledge.

### 3.4. *Interdictions and Taboos*

The previous section discussed problems of interdiction and taboos imposed by milieus, thought collectives or politico-historical systems as core issues framing the *auto-censura* practice. Studying such invisible/hidden practices is challenging. How do we examine hidden content, secrets and unrevealed spaces of knowledge and information? How do we locate the dangerous contents? How do we study these spheres put in the shadow, kept only by the initiated and the chosen?

Trust is the key to the investigation of such difficult-to-access spaces. Trust in the researcher conducting this project, her understanding of the process, and the anonymizing of collected data. However, how do we access the data if there are taboos and unconscious *auto-censura*

concealing it? It is indeed impossible to give a response to this question before the deep implementation of the project.

The study of exile trajectories may be a key way of investigating such hidden processes. In this study, a privileged place will be given to scholars who changed their thought collective (which is not the same as changing their geography or country). When such a situation occurs, the researcher experiences emancipation – former taboos lose their power in new places, and the restrictions of the new workplace or thought collective have yet to be incorporated. This situation seems particularly stimulating to the liberation from previous limits and writing without previous AC practices. I am particularly interested in researchers who passed through this type of transformation. It is probably easiest to get specific data from such persons because they are no longer acting in a routine normative to their previous environments. Exiled trajectories represent considerable potential in collecting data about emancipation from former self-constraints.

### 3.5. *Limits, risks and extensions*

#### A. When is it not *auto-censura*?

It is essential to not confuse the AC practice with other forms of selection and categorization of data. In the analysis process, each researcher attributes to their data a particular category in regard to its security: hard proof, strong evidence, uncertain data, weak indications, illustrations, unconfirmed information, doubtful testimony, gossip, and fiction. A researcher who does not publish the study because of inadequate data is not practicing *auto-censura*, but rather curation of data based on its value.

To illustrate this case, I will provide an example from the field of 20<sup>th</sup>-century history. In a biographical book based on a long interview conducted by Aleksandra Pawlicka (Gross & Pawlicka 2018), historian Jan Gross explained the background of his research published in the groundbreaking book *Neighbors; The Destruction of the Jewish Community in Jedwabne* (2002). The testimony of Szmul Wasersztajn was fundamental to the work and an inspiration to the author. However, it took four years before Gross realized that Wasersztajn's testimony was a masterful piece of evidence in the Jedwabne massacre and not the weak testimony of traumatized survivor who, as Gross said, "had experienced something terrible and, to put it bluntly, gone mad. As a reader, I was aware that the author of the text [Wassersztajn] had experienced something horrible, but not what he describes there. For a few years, I was convinced that Wasersztajn must have confused things" (Gross & Pawlicka 2018: 137-138).

The first categorization of the source was not strong, because the horror of the reconstructed tragedy was beyond the researcher's capacity to fathom. With time, other testimonies came up to confirm it, and a paradigm change occurred in the practice of Holocaust history. With the modification of the status of Holocaust survivors (which passed from emotional victims to valuable witnesses) the survivors' accounts became analyzed as strong data (about the modification of status of Holocaust survivor's witnesses testimonies in historiography, see Aleksiu 2014; Wagner 2022). These narratives constitute today the basis for the reconstruction of tragic events. Such a process is not a case of *auto-censura*. It would be, if, for some reason (such as fear of a hate campaign by those wishing to suppress evidence of Polish misdeeds during the war), the historian had omitted the Wasersztajn testimony or described him as an illusional person with PTSD [Post-Traumatic Stress Disorder].

To avoid confusion during our study of AC practices we need to consider the whole context for omission of data or research results and understand the author's position. To understand the relationship between the omission and the studied processes requires the *auto-censura* researcher to have deep expertise in the discipline under study.

#### B. Disciplinary Variety and Extensions

The reflection on AC started as auto-ethnographical data collection. I gathered the information from practices in sociology, anthropology and history. Formal and informal discussions with researchers in these fields has enabled me to get to this point. I also have begun gathering data from the life-science researchers, who were my privileged informers during previous projects. Moreover, I have long testimonies, gathered over many years, from the career of an eminent mathematician, who agreed to provide a deeply detailed account of his *auto-censura* practices. It seems evident that there are significant differences in motivation among those who practice AC. Social scientists seem more exposed to general influences (political correctness, imposed social norms, proximity to participants, self-protection). In contrast, natural scientists are more constrained to follow current scientific paradigms. Their peers control them in a way in which radical discoveries would have trouble being published (in the analysis of this phenomenon, Robert Kuhn's theory of scientific revolutions as an extension of Fleck's approach to thought collectives will be helpful; see also Kokowski 2001).

A future project may go beyond the fields of sociology, anthropology and history to include disciplines such as literary criticism, art history, cultural studies, political science, economy, as well as the life science, physics and chemistry.

The project would aim to eliminate the risk of ethnocentric bias, ideally by implementing a similar project in a non-Western society (China,

India, or Northern Africa, for example). However, the political situation may limit researchers' access to the available data. Since trust is the basis of efficient data collection, such gathering will be superficial in areas of restricted academic freedom. To avoid this, the geographical extension of the project must go in parallel with the study of AC in the past. The historical study will eliminate the problem connected to interviewing, since the matter under investigation will not be scholars who are giving testimonies, but traces of research activity from the past. It will be essential to collect all testimonies about AC in the accounts of the scholars working in ancient China, India, and the Islamic Golden Age periods. This very challenging idea of the investigation of AC practices of scholars from the past may bring entirely different kinds of findings from contemporary data. Also, checking in non-European and North American centers of knowledge practice should help avoid ethnocentric bias.

#### 4. Instead of Conclusion – opening a discussion

##### 4.1. *Auto-censura is a shameful practice?*

The practice of *auto-censura* is not the object of open and broadly spread discussions among scientists. There are several reasons for the silence covering this supposedly common practice. The fundamental reason is that censorship as a voluntary act – as opposed to it being imposed, or data withheld because of scientific skepticism – is contrary to the idea of academic freedom, one of the potent myths attached to the scientific work. Scholars themselves seems to rarely challenge this mythology. We are pleased to believe and sometimes fight for academic freedom, the liberty of speech, and independence in thinking. Such values are assimilated during socialization into the academic world and scientific work.

Scholars strongly value freedom, by which they mean the liberty to express ideas even if they oppose current paradigms. Yet when researchers express challenging, unpopular ideas that are contrary to the mainstream, it brings them into opposition to the leading networks and individuals controlling their fields. Is such an attitude well accepted in the working environment, considered one of the most feudal milieus in the 21<sup>st</sup> century? Is the opposition to peers a real option? He certainly had, but at a price, and one that is not the same for an established scholar and a young researcher. The institution's prestige framework is not the same as that of an "independent" scholar. Both situations have their own constraints. Status and power inside a field are important, for those with more robust positions can do more, also in opposition to the established conventions (in the sense proposed by Becker 1982). While the respect for taboos and interdictions, as well as current paradigms, can be approached as conventions, *auto-censura* practices are the tools that help to maintain them. A scholar anticipates peer review by auto-censoring the most controversial parts of their work. It is an act of obedience to those who control the field (those who establish and follow the current paradigms).

However, the idea of *auto-censura* challenges the fundamental myth of a researcher's work – as a way of reaching the truth and not an activity of submission and obedience. The work in science for people outside it seems a strange occupation: too hard, time-consuming and underpaid, a permanent race for success that is almost impossible to achieve. Something needs to balance these problematic working conditions, which is why the freedom myth is so important. As a scholar's community, we are attracted to this idea that we are (perhaps) better than any other members of society because we are "free." We don't like to acknowledge how unfree we are: that we must obey our PI, our professors, reviewers, influential colleagues. We anticipate their objections by

avoiding difficult positions – we delay writing things, waiting for a better moment. We limit our knowledge “production,” waiting to make our position stronger. We hope the future literature will support us, and that our discipline, in the future, will be more accepting. We are subordinate – small pieces in a big machine – peons in a massive hierarchical structure. In Poland, scholars tend to believe that a habilitation degree brings emancipation, but even that dream, once achieved, can sour. The claims about the freedom of academic expression are a powerful illusion.

However, we all need this illusion to advance in our work and push ourselves as much as possible. We need this dream of freedom to pursue our activities. Many of us believe in it, and as Ossowski’s text shows, the researcher’s duty is disobedience, in the name of scientific truth and freedom of academic expression. AC is contrary to this picture. It is a fascinating, hidden phenomenon, that certainly merits study. I am waiting with hope for the “coming out” of researchers, who may contribute through their testimony to the investigation of this “shameful” practice<sup>20</sup>. We need to learn more and understand how it works. Then, perhaps we can improve the advance in knowledge construction without – or with limited – *auto-censura* practices.

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<sup>20</sup> This term came up in several discussions with my colleagues about auto-censorship.

## APPENDIX

### Call for testimonies<sup>21</sup>

If you are a scholar or researcher and you have published, it is likely that for some reason, and contrary to your convictions, you have prevented yourself from publishing part of your research results even though you estimated that they were of high quality. If this is the case, you have practiced *auto-censura*, a part of scientific work that has never been deeply researched. This project is focused on this ordinary but hidden practice. Your testimony would help to study it and add to the knowledge of scholarly work.

How did it happen to you? Which kind of data/results did you put on hold? For what reasons? Finally, did you publish these results later? Do you think you will be able to publish them in the future? What did your discipline lose as a result of your concealment of knowledge?

Please complete your testimony with the information about your situation/positioning: age, gender, class (parent's occupation), material situation (ownership of dwelling), institutional status (Ph.D. student, post-doc, assistant professor, etc.), job category/level of precarity (permanent contract, tenured, free-lancer — grant hunter), ethnicity (language spoken), job experience (geographic mobility, discipline mobility), family status (single, married, children care, elderly parents care),

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<sup>21</sup> The call for testimonies is a strong marker of Polish sociological tradition (Jakubczak 1995). This idea was born in Florian Znanicki's work on Polish immigrants in the U.S. and developed by his student Józef Chałasiński. These researchers organized a contest among Polish emigrants for the best diary before and after WWII. The call also sought writings from the young generation of peasants and women living in the countryside. This method was criticized for its bias (people wrote diaries to win a prize, and the "authenticity" of their testimonies was challenged); however, it was an original and rich source of personal data. My call for testimonies is aimed at collected only the practice of auto-censorship. The target group is scholars, and their depositions may be anonymous or not. There is no prize, competition, or list of winners; the goal is data collection and the progress of knowledge.

positioning in the field (debutant, expert, head of the academic/research institutions, politician of science).

Your testimony may be anonymous; however, some specific data are necessary to understand the process. The publication of the results may include the citations, but they will be anonymized. The first phase of the project will be conducted for three years (2022-2025). I hope that the results will not only be fascinating and groundbreaking, but also help in the elaboration of pragmatic solutions for researchers who would like to share their protected data in the safe spaces. In such cases, *auto-censura* practices will not abort or slow down the progress of science.

Please, contribute to this project by your discovery. You can send testimony to: autocensura.wagner@gmail.com with the title “*auto-censura* account.” I will respond to all letters explaining the details about this call if necessary. Let’s learn together about our work!

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